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

Robert M. Gonzalez*

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Abstract

In August 2025, President Trump invoked Section 740 of the District of Columbia Home Rule Act to deploy National Guard troops across Washington, D.C. This paper examines the short-run effects of the deployment on crime by combining administrative incident data with a triple-differences design that compares changes in crime before and after the August 11 deployment across treated and untreated Police Service Areas (PSAs), relative to the same period in prior years. The intervention led to declines in robberies, thefts, and motor vehicle thefts, while assaults with dangerous weapons increased. Other violent and property crimes were unchanged. Cost–benefit analysis shows total 30-day benefits of $1.7 million against costs of $90 million. For comparison, a similar community-based monitoring program achieved higher crime-reduction benefits at less than four percent of the cost. This underscores the relative inefficiency of large-scale federal deployments as a crime-control strategy.

Keywords: crime, policing, National Guard, Washington DC

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

1 Introduction

In August 2025, President Trump declared a "crime emergency" in Washington, D.C. and 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 U.S. history and effectively placed D.C.'s policing under temporary federal control. Although National Guard deployments have previously occurred in response to civil unrest, this was the first time in which federal authorities proactively assumed command of local law enforcement. Given the unprecedented nature of the intervention, empirical evidence on its effects remains scarce. Moreover, in light of the political rhetoric surrounding its implementation and subsequent federal deployments in other U.S. cities, it is crucial to understand whether such measures are effective in improving public safety. This paper provides the first causal assessment of the 2025 D.C. federal deployment by examining its short-run effects on crime and assessing its efficiency relative to alternative policing strategies.

We use a high-frequency administrative dataset of reported offenses from the D.C.'s Metropolitan Police Department (MPD) covering 2019–2025. We combine these data with media-sourced and independently verified information on the location of federal personnel deployments. We then geocode the incidents and deployment location data to the city's 57 Police Service Areas (PSAs) and aggregate to a PSA-by-week panel. The analysis combines two complementary strategies. First, we estimate a triple-differences (DDD) design that compares changes in crime before and after the August 11 deployment between deployed and non-deployed PSAs in 2025, netting out analogous changes around the same calendar period in previous years. This design isolates the causal effect of the deployment by differencing out PSA-specific seasonality and citywide shocks. Second, to account for policing and crime spillovers within the city, we repeat our DDD design using D.C.-adjacent police jurisdictions in Maryland and Virginia as external control units instead.

We first assess the effects of the deployment within Washington, D.C. across six offense categories: homicide, assault with a dangerous weapon, robbery, burglary, motor vehicle theft, and theft. Our results reveal limited effects of the deployment on crime. For most offenses and specifications, estimated coefficients are statistically indistinguishable from zero. Robberies exhibit declines that reach marginal significance in some specifications. Event-study versions of the triple-difference model show no evidence of systematic pretrends and no consistent post-deployment response. The results remain robust to a wide set of alternative specifications, including using a Poisson estimator, a differences-in-differences (DiD) model, using crime

rates instead of counts as outcomes, aggregation at the police-district level, aggregating to the monthly level, and aggregating to broader violent and property crime outcomes.

We then examine the effect of deployment on crime using control units from Montgomery County, Prince George's County, and the cities of Alexandria and Baltimore rather than relying on non-deployed PSAs within D.C. We harmonize offense definitions across jurisdictions to align with the six crime categories analyzed for D.C. Using these out-of-D.C. controls, we find consistent evidence of declines in robberies, motor vehicle thefts, and, to a lesser extent, thefts. In contrast, we find no effects on homicides or burglaries while documenting a statistically significant increase in assaults with dangerous weapons. These findings are robust across Poisson and difference-in-differences estimators, monthly aggregations, models excluding Baltimore, specifications using crime rates instead of counts, and models that aggregate offenses into broader violent and property crime categories. Results are also robust to a synthetic difference-in-differences estimator that constructs a data-driven counterfactual using weighted donor jurisdictions and yields estimates similar in magnitude to the baseline results. Lastly, a placebo exercise assigning the August 11 cutoff to non-deployment years yields no systematic effects, with placebo estimates generally small and statistically insignificant relative to the 2025 treatment effect.

To better understand the increase in assaults with dangerous weapons, we explore two mechanisms: a disruption channel and an enforcement interaction channel. The disruption channel posits that deployment may disrupt organized criminal activity, potentially triggering conflicts among rival groups as they reallocate activities. This response would likely manifest in increased firearm-related assaults. The enforcement interaction channel suggests that deployment may lead to more physical confrontations between civilians and deployed personnel during enforcement activities, such as arrests, crowd control, or protest policing, which would more likely appear as non-firearm assaults in reported crime data. Consistent with the enforcement interaction channel, we find that the increase in assaults with dangerous weapons is driven entirely by non-firearm incidents, while firearm-related assaults remain unchanged. Interestingly, using novel data on geolocated gunshots detected by a network of acoustic sensors deployed across DC (ShotSpotter alerts), we document a significant increase in detected gunfire following deployment. However, using administrative data on gunshot-related emergency department visits from the DC FASTER firearm injury surveillance system, we find no corresponding increase in firearm-related injuries. Taken together, these results suggest that the rise in reported assaults with dangerous weapons may be explained by increased enforcement-related interactions; while the increase in detected gunfire may capture expressive violence in response to deployment

disruptions rather than violence leading to injuries or reported firearm assaults.

We also test directly for spillovers within the city and find evidence of declines in motor vehicle thefts and thefts and increases in assaults in non-deployed PSAs within DC. Taken together, the results suggest that deployment generated deterrence effects for visible, opportunistic crimes such as robberies, thefts, and motor vehicle thefts while having no effect on homicides and burglaries. This is consistent with a visibility-based deterrence mechanism: offenses that are visible and opportunistic are more responsive to heightened security presence than crimes that occur in less visible settings (e.g., burglaries) or that are driven by impulsive interpersonal violence (e.g., homicides).

Lastly, we test whether pre-existing crime patterns, demographic composition, or geographic proximity to central landmarks predicted where federal personnel were deployed. We find little evidence that pre-existing crime is a strong predictor of deployment. By contrast, proximity to the White House and other central landmarks strongly predicts deployment. This suggests that selection was driven less by local public-safety conditions and more by symbolic or strategic considerations tied to visibility.

We complement these findings with a cost–benefit analysis that monetizes the estimated changes in robberies, thefts, motor vehicle thefts, and assaults using contingent valuation estimates widely used in the literature.[1] Over a 30-day deployment window, the implied social benefits of crime reduction amount to approximately $1.73 million, compared to an estimated $89.86 million in deployment costs. This yields a benefit–cost ratio of roughly 0.02. These cost figures account only for the National Guard component of the operation and therefore likely underestimate total fiscal costs as the deployment also involved multiple federal agencies. Moreover, they omit intangible costs such as reduced commercial activity, lower foot traffic, and disruptions to residents' sense of safety. Taken together, the results indicate that while the deployment may have achieved modest reductions in visible, street-level offenses, these benefits were outweighed by the substantial fiscal and social costs of the intervention.

We benchmark these estimates against Chicago's Safe Passage Program (SPP), a long-running community monitoring initiative designed to deter crime near public schools. Applying the same valuation framework, we estimate that the SPP generated social benefits of approximately $63.87 million at a cost of $3.3 million over a 30-day period, implying a benefit–cost ratio of 19.35. When comparing benefits per dollar spent, the Chicago program was nearly three orders of magnitude more cost effective than the D.C. deployment. This contrast highlights the relative inefficiency of large-scale, symbolic deployments compared to localized, civilian-led de-

[1] The analysis focuses on these three offenses because they are the only ones that exhibit the most consistent declines across specifications and models.

terrence models.

This paper lies at the intersection of research on the effectiveness of police presence and research on military deployments for policing functions. The literature on police presence and manpower has primarily focused on three approaches: hot-spots policing, which concentrates police presence in persistent crime areas (Sherman and Weisburd, 1995; Braga et al., 2019; Braga and Weisburd, 2020); proactive policing, which relies on heightened enforcement and visibility (Zhao and Zhang, 2022; Masera, 2021); and problem-oriented policing, which combines targeted analysis with community engagement (Hinkle et al., 2024; Bullock et al., 2021; Taylor et al., 2022). While evidence on hot-spots and problem-oriented policing points to meaningful and sustained reductions in crime, findings for proactive policing are more mixed, with concerns about diminishing returns and potential legitimacy costs.[2]

By contrast, studies of military or militarized deployments for policing find little evidence of sustained crime reduction and, in some cases, suggest negative consequences for legitimacy and safety (Blair and Weintraub, 2023; Masera, 2021). In the United States, police militarization through the 1033 Program increased use of force and officer injuries without clear public-safety gains (Masera, 2021). In the case of Colombia's *Plan Fortaleza*, Blair and Weintraub (2023) detected no measurable reductions in violence despite intensive army patrols. Such interventions differ from conventional policing in mandate, duration, and community embeddedness, suggesting that visibility alone may not yield deterrence. To date, there is no causal evidence on the use of military personnel for domestic policing in the United States. This paper fills this gap by providing the first causal evidence on a federal military deployment used for domestic policing and expands on existing work by comparing its effectiveness to more community-oriented interventions that emphasize sustained engagement rather than symbolic force.

2 Background and Institutional Context

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

[2]Other papers that study the broader 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 the most recent comprehensive review of evidence on police manpower, redeployments, and deterrence mechanisms, see Chalfin and McCrary (2017).

Metropolitan Police Department (MPD) (Washington Post Staff, 2025).

The deployment was justified by the administration as a response to an alleged crisis of violent crime in the capital. President Trump described D.C. 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 from the prior year, and independent analyses confirmed that overall crime was at its lowest point in three decades. This divergence between rhetoric and data fueled debate over the political and institutional implications of the intervention (Washington Post Staff, 2025; ACLU of the District of Columbia, 2025; Greenblatt, 2025).

Community leaders and local officials criticized the deployment as a political maneuver that undermined D.C.'s limited self-governance and perpetuated longstanding tensions between the federal government and the city's residents (Washington Post Staff, 2025; Horton et al., 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 D.C. 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. Scholars and legal experts noted that the case raised broader questions about the use of crime narratives as justification for federal encroachment on local authority and the implications for democratic accountability in the District (Brennan Center for Justice, 2025; Levitz, 2025; Ducharme, 2025; Forgey and Schor, 2025).

3 Data

We use administrative records of reported crime incidents in Washington, D.C. provided by the Metropolitan Police Department (MPD) through the District's Open Data Portal (District of Columbia Metropolitan Police Department, 2025). Our dataset covers January 1, 2019 to October 31, 2025. The data include incident-level information on the date and time of the offense, the police district, the ward, the Police Service Area (PSA), the offense type, and geocoded coordinates.

Our main crime measures aggregate the data to the PSA–week level. Washington, D.C. is divided into 57 PSAs, which represent the smallest operational units in MPD's organizational structure. We use PSA aggregation because it strikes a balance between geographic precision

and administrative relevance: PSAs are the units where MPD manages deployment, supervises officers, and track crime trends internally. We focus on daily aggregation in order to capture short-run responses to the deployment and to examine whether crime patterns shifted immediately following the intervention. In addition to our main specification, we also conduct analyses using alternative aggregations, including weekly and district-level aggregates. These latter measures allow us to capture medium-run responses.

The dataset includes the following offense types: homicide, assault with a dangerous weapon, robbery, sex abuse, burglary, theft, motor vehicle theft, and arson. During our sample period, there were a total of **135,371** reported incidents. Theft constitutes the largest category, accounting for about **66%** of all incidents, followed by motor vehicle theft (**16%**) and robbery (**8%**). Appendix Table A1 presents these figures.

Figure 3 presents monthly trends in crime rates by offense category and deployment status between 2019 and 2025. The figure plots treated and control PSAs separately, showing broadly similar trajectories across groups for most offenses.[3] Assaults with a dangerous weapon have followed a steady downward trajectory since 2019, reaching some of the lowest levels in the entire sample period before the deployment. Robberies and homicides exhibit a spike in mid-2023 but then decline to the lowest levels in the sample period by mid-2025. Among property crimes, thefts remain relatively stable over time. Burglaries display a gradual decline across the sample period, reaching their lowest levels just prior to deployment. Motor vehicle thefts also surge in mid-2023 and subsequently decline, although they remain higher relative to earlier months in the sample period.

We code the timing of the deployment using dates reported in major media outlets, which documented the announcement on August 11, 2025 and the subsequent arrival of National Guard units and other federal agencies in the city (Washington Post Staff, 2025; Horton et al., 2025). We geolocate the deployment using the same set of 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 by drawing on visual accounts from reporters on the ground and verified videos on social media. We georeferenced the reported locations and matched 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. While media-based geolocation may not capture every deployment site, it provides a credible approximation of where federal personnel were visibly present. This is supported by the conspicuous nature of the deployment—large numbers of uniformed troops,

[3]Theft shows different average levels between deployed and non-deployed PSAs. However, their trajectories over time are largely similar.

military vehicles, and federal agents in public spaces. Figure 1 provides a map of agencies deployed across PSAs.

We complement the deployment geolocation data with a control pool constructed from 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.[4] To enhance comparability with treated PSAs in D.C., we restrict Prince George and Montgomery counties' control pool to beats belonging to police districts that border D.C.[5] Appendix B provides a detailed description of the offense classification and harmonization across the four jurisdictions.

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 donor 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 donor units.

4 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 PSAs that experienced deployments and those that did not. Our DDD design introduces an additional layer of differencing across years. Specifically, it differences out systematic shifts in crime that occur around the deployment date in prior years for both treated and untreated PSAs. In doing so, the DDD design isolates the 2025-specific treatment effect from recurring PSA-level seasonality around the deployment period. This is particularly important in this context, given the inherent seasonality of crime both across space and time.

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

[5] 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.

Formally, we estimate:

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

where y_{its} denotes the number of incidents of a given offense in PSA i, in calendar week t, and year s. Deployed_i equals one if PSA i 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 non-deployed PSAs, netting out analogous differences around the same period in previous years. Fixed effects α_{it} (PSA×calendar week), γ_{is} (PSA×year), and δ_{ts} (calendar week×year) absorb all main effects and two-way interactions among PSA, calendar week, and year. More generally, these fixed effects serve to flexibly account for multiple sources of confounding variation: α_{it} captures PSA–calendar-week specific characteristics that do not vary across years (e.g., crime in downtown versus residential PSAs during summer). γ_{is} absorbs PSA-specific annual trends in crime and other characteristics. δ_{ts} accounts for shocks that are common to all PSAs. Standard errors are clustered at the PSA level to allow for arbitrary serial and spatial correlation within PSAs.[6]

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:

$$y_{its} \;=\; \sum_{k\neq -1} \beta_k\,(\text{Deployed}_i \times \mathbf{1}\{\tilde{t} = k\} \times Y2025_s) \;+\; \alpha_{it} \;+\; \gamma_{is} \;+\; \delta_{ts} \;+\; \varepsilon_{its}. \tag{2}$$

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 these main specifications with several robustness exercises. For comparison, we estimate a simpler *difference-in-differences* (DiD) specification that compares changes in crime across deployed and non-deployed PSAs in 2025 alone:

$$y_{it} = \beta\,(\text{Deployed}_i \times \text{Post}_t) + \alpha_i + \gamma_t + \varepsilon_{it}, \tag{3}$$

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

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

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).

where t indexes weeks within 2025. This design identifies the deployment effect under the assumption that, in the absence of deployment, crime in deployed and non-deployed PSAs would have followed similar trends after August 11. Second, we estimate all models using monthly rather than weekly aggregation to ensure that results are not driven by short-term fluctuations. Third, 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 PSAs; (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.

5 Results

This section presents results from two complementary strategies. First, we analyze data that uses PSAs within DC that report no deployment as control group. The advantage is that the control units are more comparable and not subject to differnet institutional differences since they are all from within the same jurisdiction. The disadvantage is that even if deployments occurred in specific PSAs, potential offenders may have perceived the intervention as a citywide policing surge, generating deterrence spillovers that blur the distinction between treated and control units. In this case, PSAs classified as untreated may experience indirect effects, biasing estimated treatment effects toward zero. Similar attenuation may occur if deployments were mobile or not fully documented, leading to misclassifications in the treatment variable.

Second, we analyze data using the same treatment PSAs from DC but instead use control units from outside DC. This can mitigate the issue of spillovers described above. However, the tradeoff is that we are using control units that are less comparable and have different institutioanl dynamics, and the need to harmonize crime categories across jurisdictions. We complement these results by looking at spillover dynamics within control PSAs in DC. The section concludes by presenting results on the determinants of selection into deployment for treated PSAs.

5.1 Within-DC Analysis

Our main estimates use weekly data and a 24-week period prior to the deployment date. Table 1 reports the triple-differences estimates from Equation (1). The results suggest 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.

Turning to property crimes, burglary and theft display no meaningful changes after deployment. The coefficient for motor vehicle thefts is positive but also statistically insignificant, providing no evidence of a consistent response in this category. Taken together, these results indicate that while point estimates generally align with a small reduction in robberies, there is no robust statistical evidence that deployments affected crime levels across PSAs.

Figure 6 presents the dynamic DDD estimates from Equation (2). 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. No offsetting increases are observed in other offenses.

Overall, the analysis suggests that deployments may have contributed to a temporary decline in robberies, but the estimates are statistically weak, and there is little evidence of systematic changes across other major crime categories.

This conclusion is robust across a wide range of alternative specifications. We obtain the same qualitative results when re-estimating the weekly triple-differences specification using a Poisson pseudo-maximum likelihood (PPML) estimator (Table A2). The findings are also unchanged when using a standard difference-in-differences (DiD) specification as in Equation (3) (Table A3), when using crime rates per 100,000 residents as the outcome (Table A4), and when aggregating the data to the police district level (Table A5).[7] Results are similarly unchanged when we aggregate the data monthly rather than weekly (Table A6), and when collapsing offenses into broader violent (assault with dangerous weapon, robbery, homicide) and property (theft, burglary, MVT) crime categories to assess effects on overall crime (Table A7).

5.2 Using External Controls

Deployments may produce broad deterrence responses extending beyond formally treated PSAs. Potential offenders may perceive the presence of federal personnel as a citywide increase in policing. In this case, PSAs classified as controls may have been indirectly "treated," biasing estimated effects toward zero. Additionally, if patrols moved across PSA boundaries or if some deployments were not fully documented in public sources, our deployment variable may contain measurement error that further attenuates estimated effects.

To assess whether deployment spillovers or misclassification may have attenuated our within-DC estimates, we re-estimate our triple-differences model comparing all D.C. PSAs relative to control units outside D.C. Specifically, we use control units from surrounding jurisdictions:

[7] 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 ($G = 7$).

Prince George's County, Montgomery County, the City of Alexandria, and the City of Baltimore.[8]

We harmonize offense categories across jurisdictions for the six categories available in the initial DC analysis.[9] In addition, we limit the control group to units located in districts adjacent to the D.C. boundary to ensure geographic comparability and reduce unobserved heterogeneity across jurisdictions. The resulting panel covers 274 external control units observed weekly over a one-year pre-treatment window.[10] Figure 5 shows units across DC and control areas.

We begin by estimating Equation (1) using treated PSAs in Washington, D.C. and control units from neighboring jurisdictions—Prince George's County, Montgomery County, the City of Alexandria, and the City of Baltimore. This specification excludes all non-treated D.C. PSAs to mitigate potential spillovers within the city. We also exclude 2023 from the main analysis, as it represents an atypical year for crime in D.C., particularly with respect to violent offenses. As shown in Figure 3, robberies exhibit a pronounced spike during 2023.[11] Panel A of Figure 5 depicts the estimation sample.[12] Table 2 reports the corresponding DDD estimates, and Figure 7 shows the dynamic event-study coefficients.

Results indicate statistically significant declines in robberies, thefts, and motor vehicle thefts (MVT) in treated D.C. PSAs relative to external controls, alongside a statistically significant increase in assaults. For all crime categories, the event-study coefficients are largely centered around zero in the pre-deployment period, suggesting the absence of pre-trends (Figure 7). Thefts and MVTs show evidence of a gradual decline post-deployment. Coefficients on robbery show no clear trend post-deployment, although the aggregate coefficient in Table 2 is negative and statistically significant.

5.2.1 Robustness Checks

The 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 A8); (ii) a conventional difference-in-differences model (Table A9);[13] (iii) monthly aggregation (Ta-

[8] Crime data for Arlington county which could serve as another potential control group is not available for the study period.

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

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

[11] The main concern is that this can lead to differential trends in outcomes between D.C. and control units, since the mid-2023 spike in violent crime was unique to D.C.

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

[13] 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.

ble A10); (iv) expressing outcomes as rates per 100,000 residents (Table A11); (v) monthly rates (Table A12); (vi) excluding police beats from the City of Baltimore (Table A13); and (vii) expanding the control pool to include all units within neighboring counties rather than only those adjacent to D.C. (Table A14). 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 A8), monthly aggregation (Table A10), and alternative control samples excluding Baltimore or expanding the control pool (Tables A13 and A14)—but become statistically insignificant when outcomes are expressed as rates (Tables A11 and A12).

As an additional robustness check, Table A15 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.

As a final 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 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 A16 presents the SDID estimates. The estimates closely resemble the main DDD results in Table 2. 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 A2 presents the event study graphs for this method.**

Taken together, the results using external controls corroborate the decline in robberies documented in section 5.1. Additionally, we find a reduction in motor vehicle thefts and thefts, alongside an increase in assaults, with no detectable effects on homicides or burglaries.[14] The

[14] 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

emergence of MVT and theft effects when using external controls may reflect policing or monitoring spillovers across D.C. PSAs, which could explain the null MVT and theft results in the within-D.C. analysis in section 5.1. By contrast, the coefficient for robbery remains very close to the coefficient from the within-D.C. analysis (Table 1), suggesting that spillover dynamics might only be present for MVT and thefts. The next sections discuss heterogeneity across crime categories and spillover effects.

5.2.2 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 (2) 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 A3 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 3). 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.

as burglaries—also exhibit no statistically significant response (Table 2, column 4). In addition, aggregating outcomes to the monthly level, which reduces sparsity in the data, yields similarly null estimates for homicides (Table A10). Results are also unchanged when we use PPML as the estimator, which is better suited for count outcomes with many zeros (Table A8).

5.2.3 Heterogeneous Effects across Crime Categories

We find significant and robust reductions in robberies, thefts, and motor vehicle thefts following deployment, alongside a significant increase in assaults with dangerous weapons, and no detectable effects on homicides or other property crimes such as burglaries. This pattern is consistent with a visibility-based deterrence mechanism: deployments of personnel increase the perceived risk of apprehension for offenses that are public and opportunistic such as robbery, theft, and vehicle thefts. By contrast, assaults and homicides are largely relational or impulsive, occurring in private settings or within established social networks.[15] Similarly, burglaries tend to occur in less visible contexts, making them less sensitive to deployment presence.

These heterogeneous effects are also consistent with prior research showing that visible policing disproportionately reduces crimes of opportunity, such as robbery, theft, and vehicle theft, while leaving interpersonal and low-visibility property crimes largely unaffected. Early studies similarly found that proactive or directed patrols can meaningfully reduce robberies and other public offenses. For example, Wilson and Boland (1978) and Sampson and Cohen (1988) reported significant negative associations between proactive enforcement and robbery rates across U.S. cities, a finding later confirmed using more rigorous empirical designs (e.g., Kubrin et al., 2010).

The increase in assaults with dangerous weapons (ADW) deserves special attention given that it is the only crime type that increases following deployment. While several mechanisms could potentially explain this pattern, we focus on two: a disruption channel and an enforcement interaction channel. First, deployment may disrupt organized criminal activity. For example, increased security presence may force gangs and criminal groups to reallocate across space, potentially bringing rival groups into conflict over territorial control. If this disruption mechanism is driving the results, we would expect to observe an increase in firearm-related assaults. Second, deployment may lead to increased interactions between civilians and deployed federal personnel. For example, reported assaults may rise due to physical confrontations during protests, arrests, or other enforcement interactions. This enforcement interaction mechanism would more likely manifest in an increase in non-firearm assaults.

To test these channels, we estimate our triple-difference specification separating ADW into firearm and non-firearm incidents. Table A17 presents these results.[16] We find that the estimated effect on ADW is driven entirely by an increase in non-firearm assaults in deployed

[15] Unfortunately, the D.C. incident-level data do not include information on whether the crime occurred in a public or private location (e.g., on the street versus indoors), which limits our ability to directly test whether deployment effects were concentrated in public spaces where visibility is highest.

[16] 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.

areas, while firearm assaults remain unchanged. This pattern appears more consistent with the enforcement interaction channel than with the disruption channel.

We explore the disruption channel using data on gunfire detection from ShotSpotter. ShotSpotter is an acoustic detection system deployed in several U.S. cities that uses a network of sensors to identify and geolocate gunshots in real time.[17] Because gun violence among criminal groups may go unreported to police, firearm assault data may understate changes in underlying gun activity. ShotSpotter data therefore provide a complementary measure of gunfire independent of crime reporting. Using geocoded ShotSpotter data, we estimate our triple-difference specification using the number of shots detected in a given PSA-week as the outcome variable. Table A18 presents these results. We find a statistically significant increase in detected gunfire following deployment. This result remains robust when using rates of shots per PSA population.

We complement this analysis with data on gun-related emergency department (ER) visits in DC. We obtain data on gun-related ER visits from the DC FASTER firearm injury surveillance system maintained by DC Health. Because these data are reported at the ward-month level, we estimate our triple-difference specification using this level of aggregation. Table A19 presents these results. In contrast to the ShotSpotter findings, we find no corresponding increase in gunshot-related ER visits.

Taken together, these results suggest a nuanced pattern. While we observe an increase in reported assaults with dangerous weapons, this increase is driven entirely by non-firearm incidents. At the same time, we detect an increase in overall gunfire activity. One possible interpretation is that deployment leads to an increase in *reported* ADW through the enforcement interaction channel while simultaneously disrupting criminal activity. The fact that the increase in detected gunshots does not translate into an increase in gunshot-related injuries suggests that these incidents may reflect demonstrative or expressive violence—for example, warning shots or signaling among criminal groups.

5.2.4 Deployment Spillovers within D.C.

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 D.C., (ii) non-deployed PSAs within D.C., 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 D.C. Panel C of Figure 5 illustrates the three

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

groups in the estimation sample, and Table 3 reports the corresponding estimates.

Results indicate some evidence of spillover effects across most crime categories. Coefficients for 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.

5.3 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:

$$\text{Deployed}_i = \alpha + \boldsymbol{\beta}' \, \text{Crime}_i + \gamma_1 \, \text{Population}_i + \gamma_2 \, \text{ShareNonWhite}_i + \gamma_3 \, \text{DistanceWH}_i + \varepsilon_i, \quad (4)$$

where Deployed_i is an indicator equal to one if PSA i received deployment on August 11, 2025.

[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.

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 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 4 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).

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%.

6 Cost-Benefit Analysis

We conduct a benefit–cost analysis over a 30-day window that aligns with how deployments are typically authorized (e.g., 30/60 days). To calculate the benefits of crime reductions, we rely on

the DDD coefficients for treated and spillover PSAs (Table 3). We monetize these benefits using contingent-valuation (CV) values from Cohen et al. (2004) as compiled in Chalfin (2016).[19] 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.[20] 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 $c \in \{\text{Robbery}, \text{Theft}, \text{MVT}, \text{Assault}\}$ estimated in treated and spillover PSAs, respectively (estimates obtained from Table 3). We focus on robberies, motor vehicle thefts, thefts, and assaults since these are the coefficients that are consistently significant across specifications. Let V_c denote the CV dollar value per incident (in 2025 USD). The total 30-day benefit for crime c is:

$$\text{Benefit}_c = \left(\hat{\beta}_{T,c}\, N_T + \hat{\beta}_{S,c}\, N_S\right) \times \frac{30}{7} \times V_c. \tag{5}$$

Column 4 of Table 5 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, $V_{\text{mvt}} = \$8{,}121$ per avoided motor-vehicle theft (MVT), and $V_{\text{assault}} = \$233{,}039$ per avoided assault, the implied 30-day benefits are \$4.39 million for robbery, \$0.35 million for theft, \$1.71 million for MVT, and -\$4.72 for assault, for a total net benefit of \$1.73 million (2025 USD).

Deployment costs We compute deployment costs by separately estimating expenditures for the D.C.–based National Guard and for out-of-state Guard units. For the D.C. Guard, we rely on the projected total cost of \$201 million reported in Mayes-Osterman (2025) for the

[19] 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.

[20] 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.

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 D.C. 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 D.C. Guard) (Babb, 2025; Mayes-Osterman, 2025). Multiplying personnel, per-day cost, and effective days produces an estimated cost of roughly $36 million. Combining the D.C. and out-of-state components yields a total 30-day deployment cost of approximately $90 million, reported in Column 5 of Table 5.

Cost–benefit assessment Over a 30-day window, the crime-reduction benefits in Table 5 sum to $1.73 million, while deployment costs amount to $89.86 million. The implied benefit–cost ratio is around 0.02, meaning that each dollar of spending generated about two cents in social benefits. Put differently, total costs exceeded benefits by a factor of more than fifty. 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 increase the net welfare loss associated with the intervention.

We also note that assault accounts for a substantial share of the estimated benefits due to their relatively high CV estimates. Even when excluding assaults, however, the difference between benefits and costs remains stark. Over a 30-day window, the crime-reduction benefits sum to $6.45 million (net of assault effects). The implied benefit–cost ratio is therefore 0.07 (89.86/6.45), meaning that each dollar of spending generated seven cents in social benefits. Total costs still exceeded benefits by a factor of fourteen.

6.1 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 dis-

missal 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 D.C. deployment the same overarching objective of reducing street-level crime through heightened visibility but differs in implementation and scale: while the D.C. 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 D.C. 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, MVTs, and assaults as a result of SPP. We assume the SPP operates over 176 school days (i.e., $176/30 = 5.87$ effective months).[21] Gonzalez and Komisarow (2020) estimate SPP averted 207 robberies, 465 motor vehicle thefts (MVTs), and 1,476 assaults, while it increased thefts by 396 due to theft spillovers.[22] Using CV values per averted crime on column 3 of Table 5, total school-year benefits are $27.62M (robbery) −$0.49M (theft), $3.78M (MVT), and $343.97M (assault). Normalizing by duration yields monthly benefits of $4.71M (robbery), -$0.08M (theft), $0.64M (MVT), and $58.60M (assault) for a combined

[21] 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.

[22] The robbery, theft, and assault estimates are taken directly from Table 7 of Gonzalez and Komisarow (2020), which reports the total number of averted crime 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.

$63.87M per effective 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.[23] 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 $63.87 million exceed its monthly operating costs of $3.30 million for a benefit–cost ratio of roughly 19.35. In other words, every dollar spent on the community-based program generated about $19.35 in social benefits under the same valuation framework.

Taken together, the two interventions reveal stark differences in cost effectiveness. The D.C. federal deployment generated an estimated $1.73 million in monthly social benefits at a cost of $89.86 million, yielding a benefit–cost ratio of 0.02. In contrast, Chicago's community-based Safe Passage Program produced significantly higher monthly benefits of $63.87 million at a cost of $3.3 million, corresponding to a benefit–cost ratio of 19.35. Measured by benefits per dollar spent, the Chicago program was therefore nearly three orders of magnitude more cost effective than the D.C. deployment.

When excluding assaults—which account for a substantial share of estimated benefits in both settings—the contrast remains pronounced. Without assault-related impacts, Safe Passage generates approximately $5.27 million in benefits per 30 operating days against $3.30 million in costs, yielding a benefit–cost ratio of 1.60. By comparison, the D.C. deployment generates about $6.45 million in benefits against $89.86 million in costs, corresponding to a benefit–cost ratio of 0.07. Thus, even under this conservative comparison, the Chicago intervention remains over 20 times more cost effective than the D.C. deployment. While both initiatives share a deterrence goal, the Chicago program achieves higher crime-reduction benefits at a fraction of the cost, highlighting the efficiency gains from localized, data-driven community monitoring relative to large-scale federal deployments.

7 Discussion and Limitations

Limitations We note several limitations with this study. First, the analysis focuses on a restricted set of so-called *index offenses*, which represent the only categories publicly reported by the D.C. Metropolitan Police Department. As a result, we cannot assess whether deployments

[23] 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 $\text{CPI}_{2025}/\text{CPI}_{2015} = 323.976/237.017 = 1.3669$.

affected other types of crimes such as drug offenses, disorderly conduct, 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 on the effect of deployment towards zero. Although this is a plausible argument, we believe this explanation is unlikely to account for 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.

A final limitation 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, D.C., 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.

External Validity Although this paper focuses on Washington, D.C., 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 federal deployment–our D.C. case offers insight into their likely mechanisms and effects elsewhere.

8 Conclusion

This paper provides the first causal evidence on the effects of the 2025 federal deployment of National Guard troops and federal agents in Washington, D.C. Using high-frequency administrative

crime data and a triple-differences design, we find that the intervention produced only limited reductions in crime. Declines are concentrated in robberies and motor-vehicle thefts—offenses that are public, opportunistic, and highly visible—while other violent and property crimes show no meaningful response. 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.

Our findings also suggest that the intervention's effectiveness was constrained by its symbolic rather than data-driven deployment strategy. The cost–benefit analysis underscores this inefficiency: estimated social benefits amount to roughly one-tenth of total deployment costs, implying limited returns to large-scale federal policing surges. To benchmark these figures, we compare the D.C. 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 and vehicle thefts at roughly one-twentieth of the cost. While both interventions rely on visibility to discourage public and opportunistic crimes, the comparison illustrates that sustained, community-embedded models can deliver similar public-safety benefits far more efficiently, with substantially lower fiscal and institutional costs than short-term, federally led deployments.

More broadly, the 2025 D.C. case highlights the tradeoffs inherent in federally driven public-safety interventions. While highly visible deployments may signal government resolve, their fiscal and institutional costs can outweigh short-term deterrence gains. Future research should compare such federal interventions with localized, civilian-based strategies—such as community monitoring or hotspot policing—that may achieve comparable crime reductions at far lower cost.

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Tables and Figures

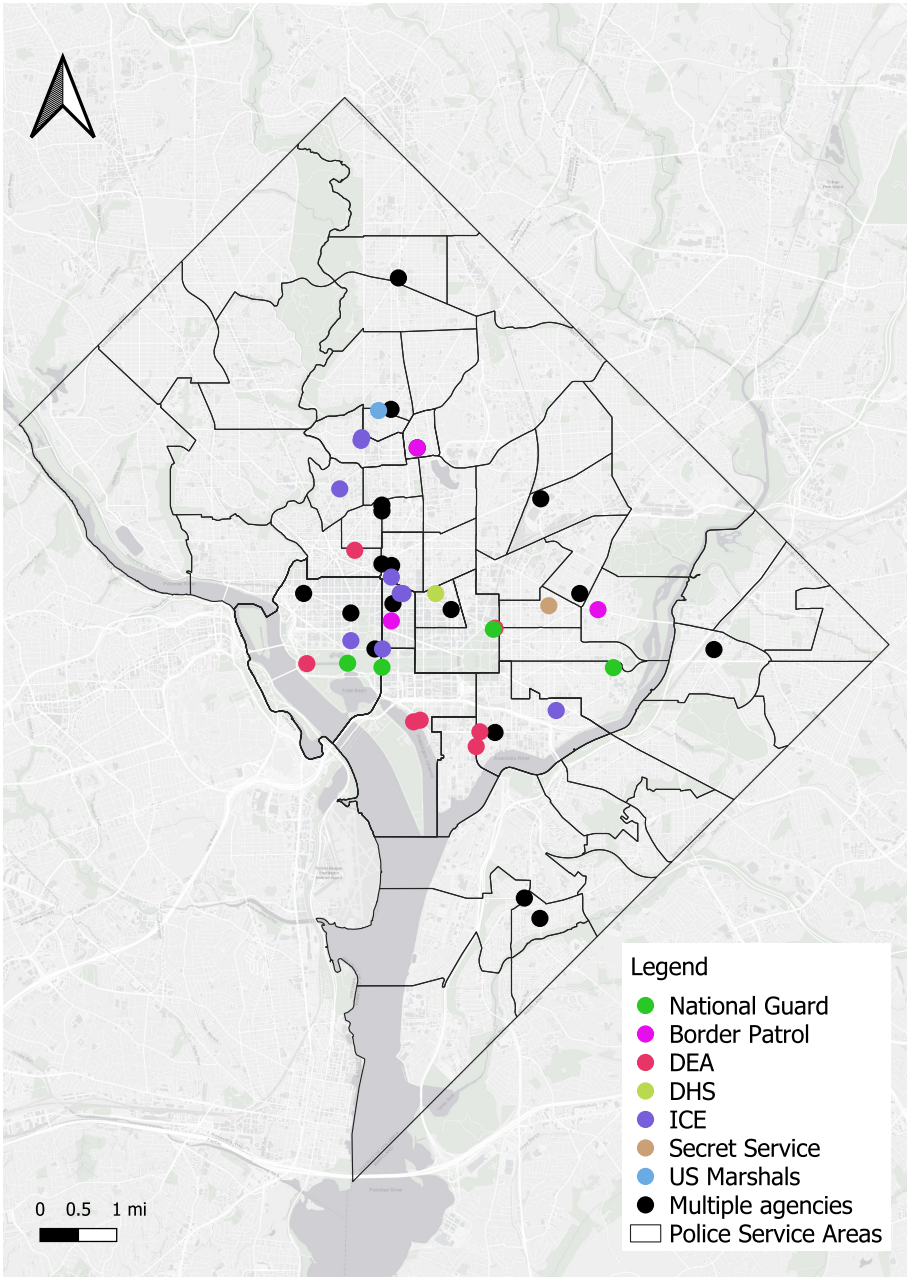
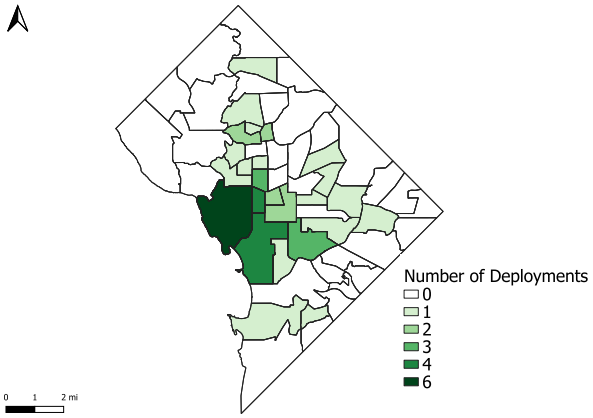
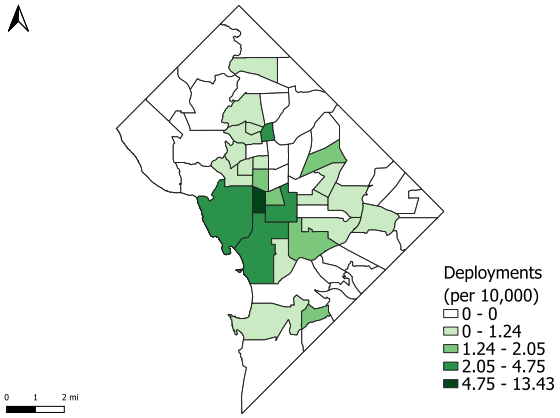


Figure 1: Troop and Agency Geolocation

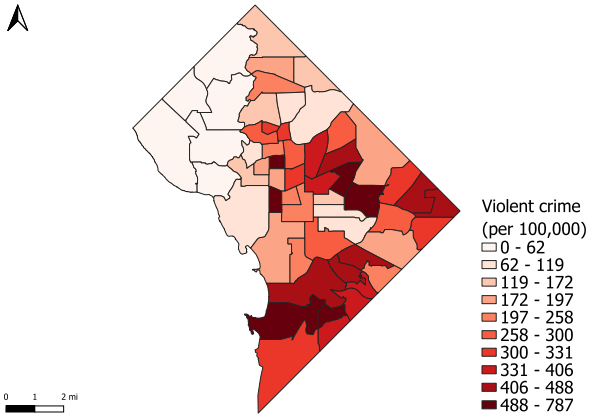
Notes: Reported Location of Troops and other deployed Agencies. Source: (Washington Post Staff, 2025).



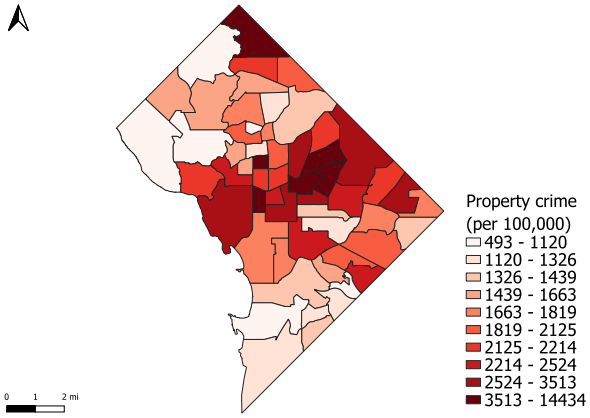
(a) PSA deployment



(b) PSA deployment rate



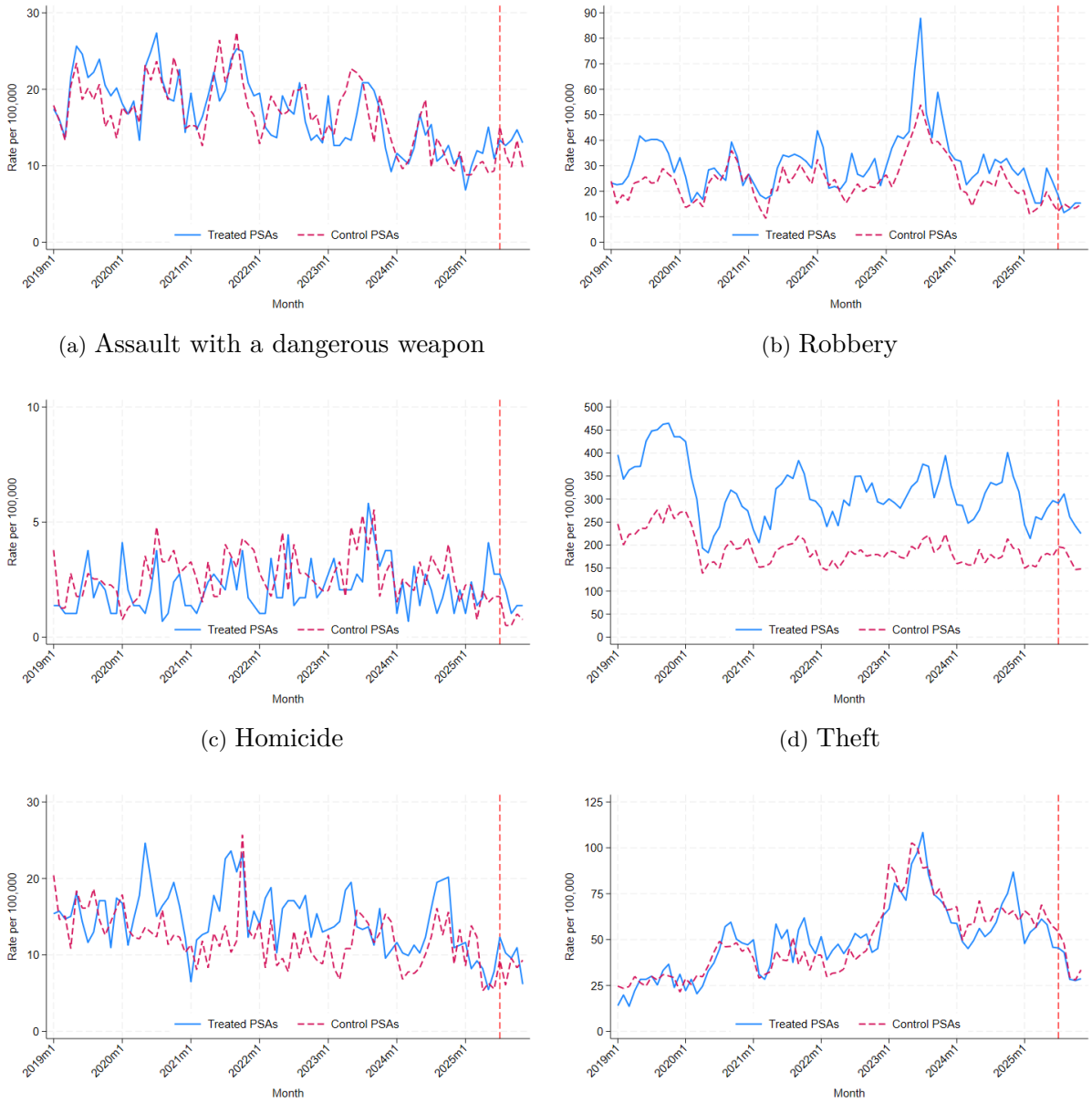
(c) Violent crimes



(d) Property crimes

Figure 2: 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. Crimes rates are calculated per 100,000 population in the PSA. Analysis uses population data from 2020 census.



(a) Assault with a dangerous weapon

(b) Robbery

(c) Homicide

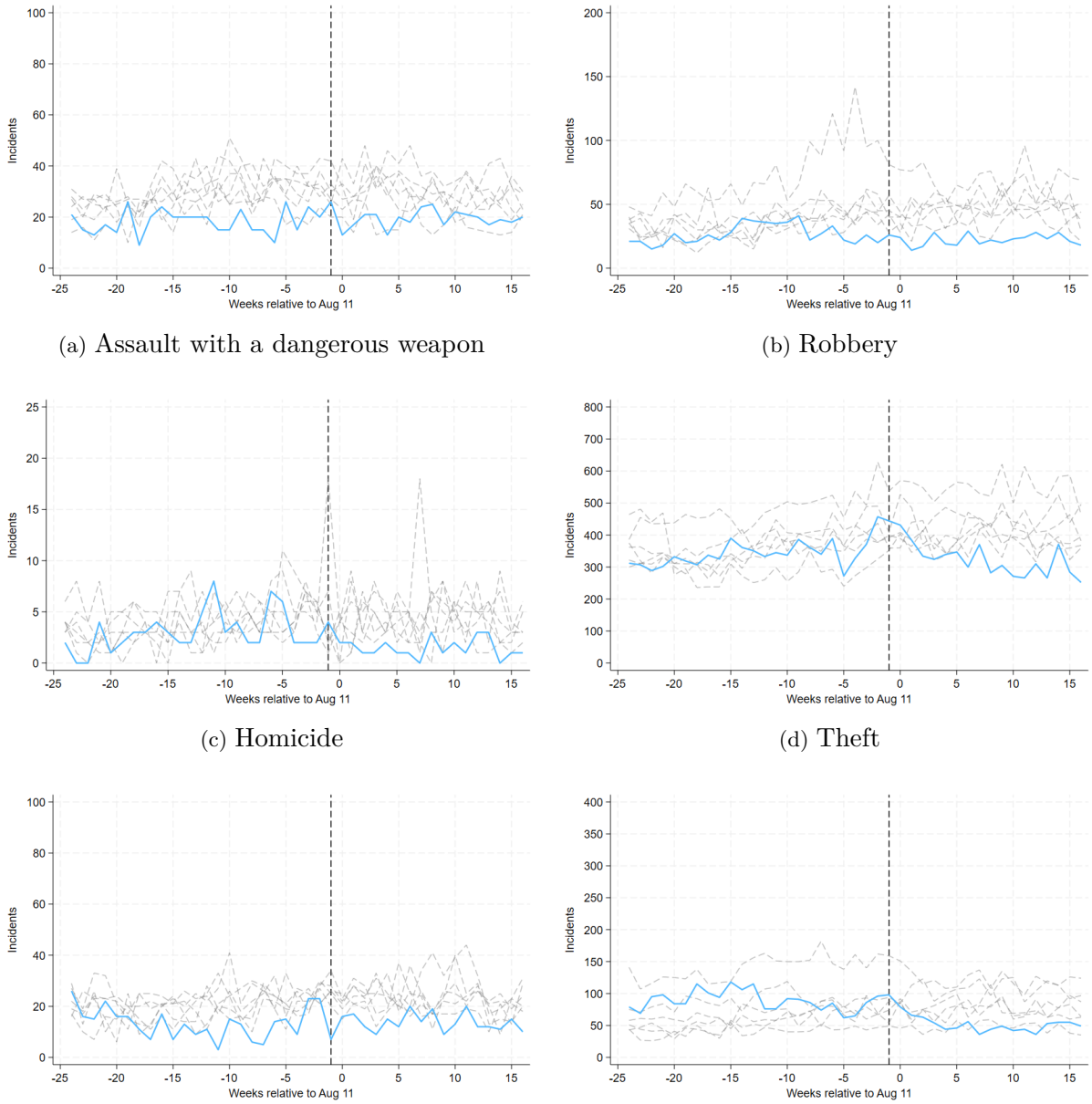
(d) Theft

(e) Burglary

(f) Motor vehicle theft

Figure 3: Monthly Crime Trends by Deployment Status (2019-2025)

Notes: Monthly citywide crime rate by offense category and by deployment status. Crimes rates in each group (Deployment and No deployment) are 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 week 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

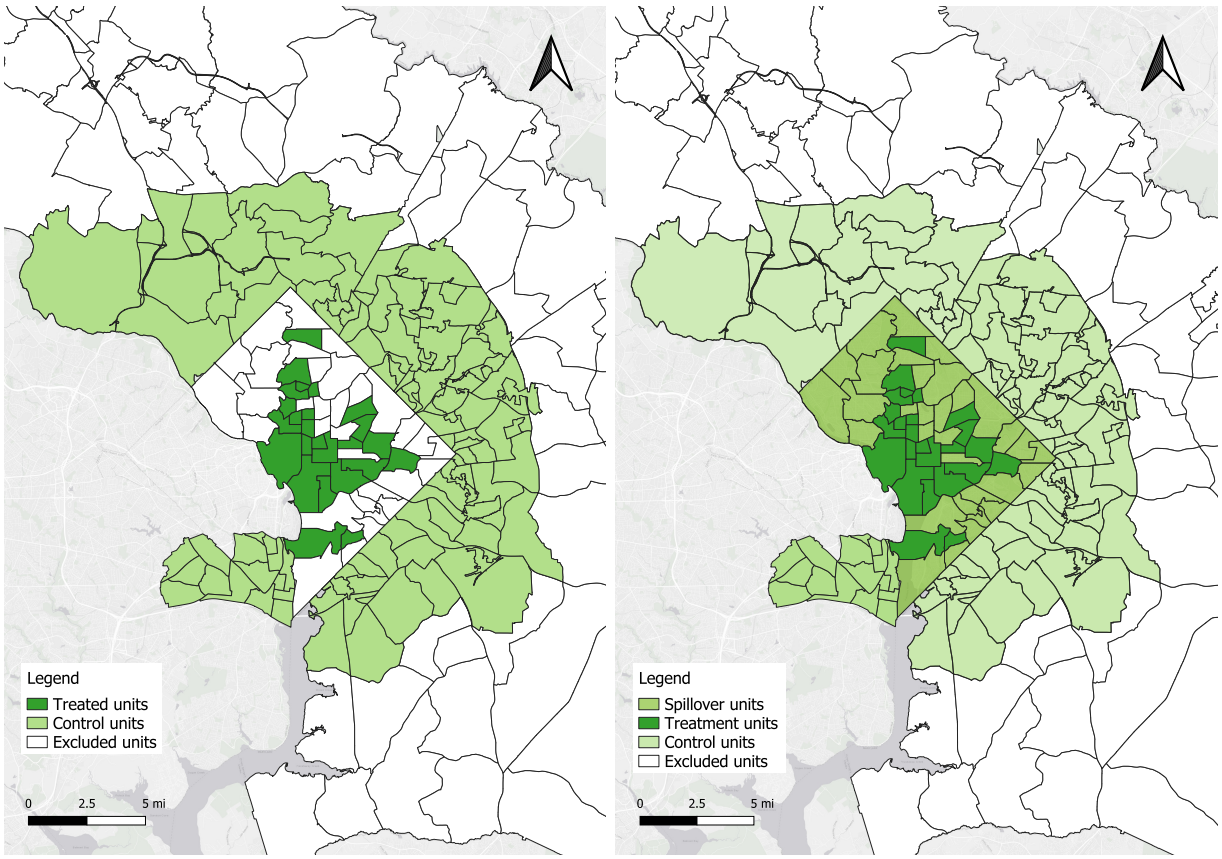
(d) Theft

(e) Burglary

(f) Motor vehicle theft

Figure 4: Weekly Crime Trends, All years (24-week window around Deployment cutoff)

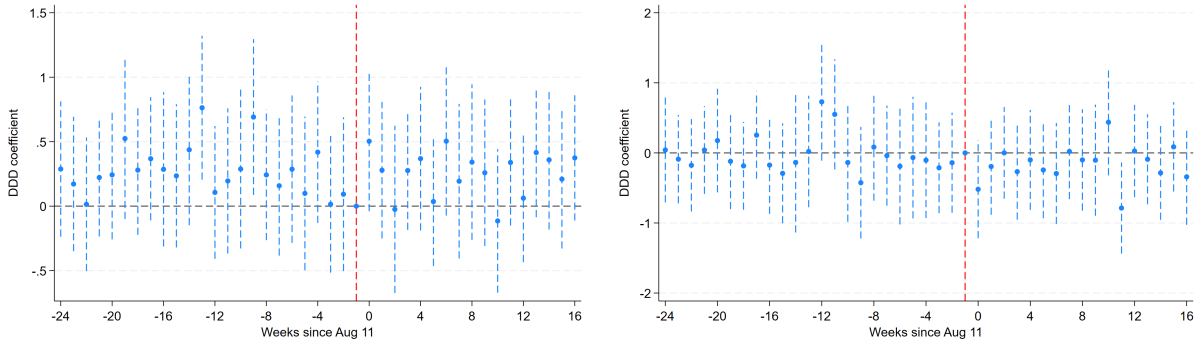
Notes: Weekly citywide crime incidents by offense category. Blue line depicts year 2025. Dashed lines depict years 2019-2024. Sample restricted to a 24-week window around deployment cutoff. The dashed vertical line marks the week prior to August 11, 2025 (National Guard deployment). Violent crime includes 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 is excluded due to low frequency. Data from the Metropolitan Police Department (District of Columbia Metropolitan Police Department, 2025).



(a) Reported deployment (b) Spillovers

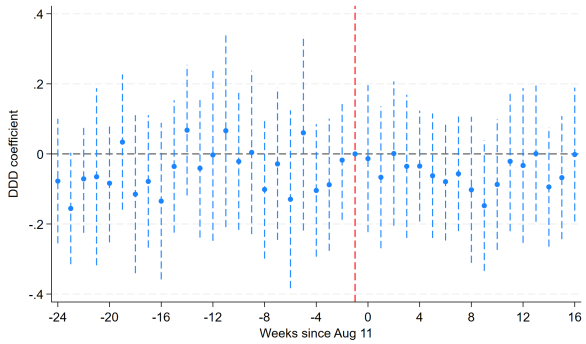
Figure 5: Treatment and Control unit definitions

Notes: 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, Prince George 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.

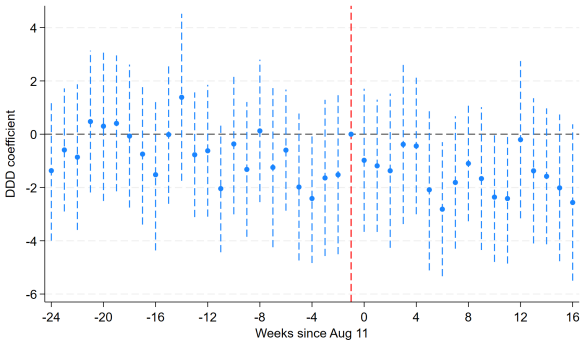


(a) Assault with a dangerous weapon

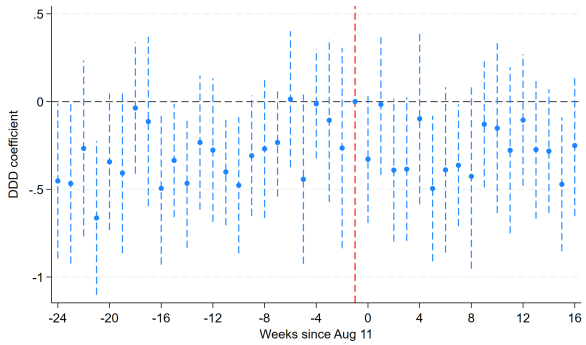
(b) Robbery



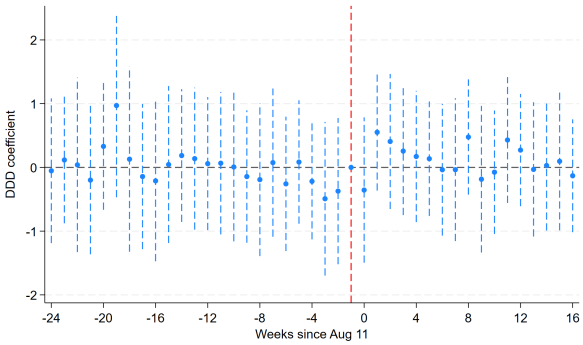
(c) Homicide



(d) Theft



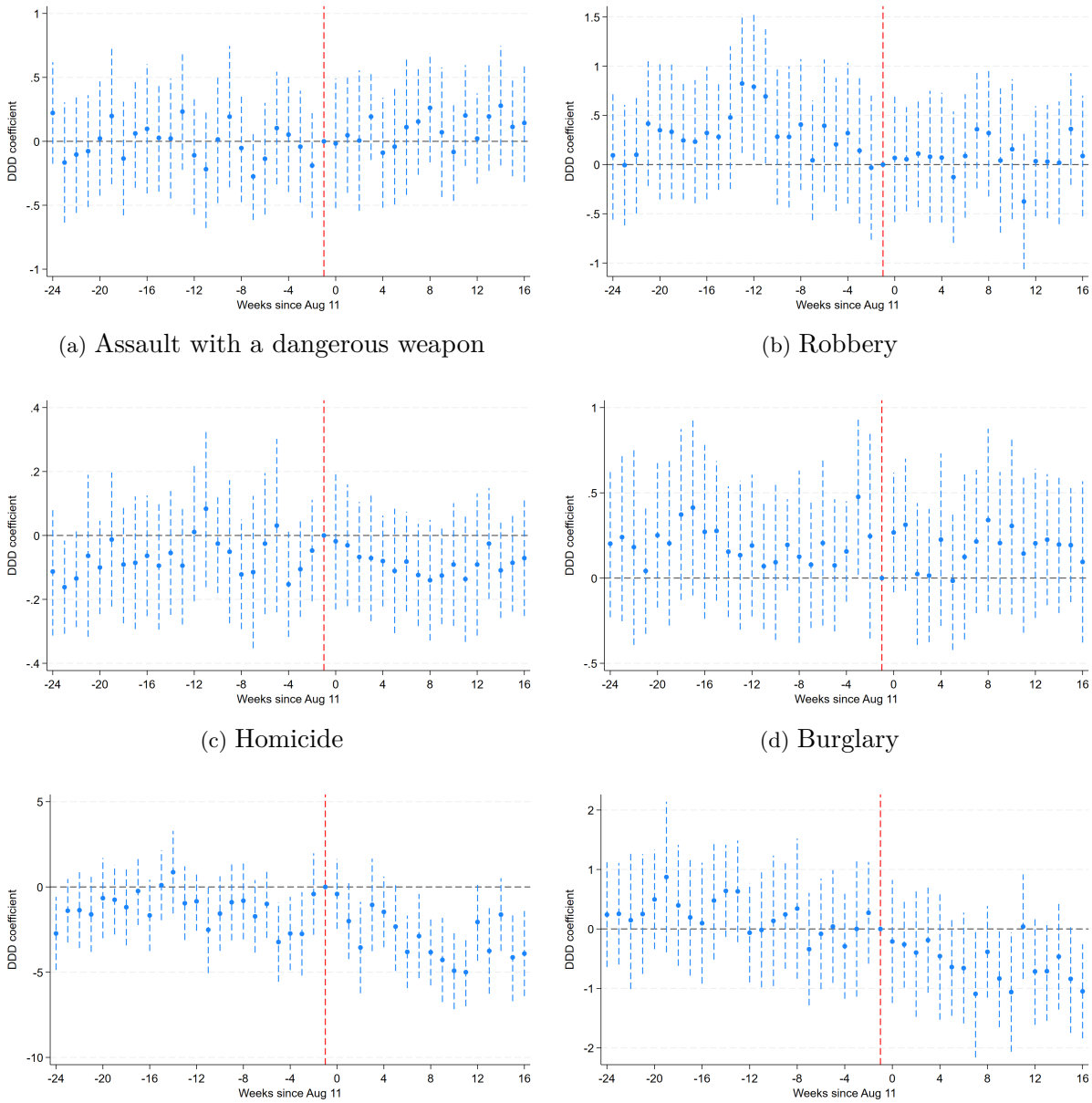
(e) Burglary



(f) Motor vehicle theft

Figure 6: Triple Differences (DDD) Event Study plots, Weekly

Notes: Each dot gives the coefficient estimates β_k from Equation (2). 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.



(a) Assault with a dangerous weapon

(b) Robbery

(c) Homicide

(d) Burglary

(e) Theft

(f) Motor vehicle theft

Figure 7: Triple Differences (DDD) Event Study plots, Weekly, non DC controls

Notes: Each dot gives the coefficient estimates β_k from Equation (2). 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 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. 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 means refers to mean in control units, before Aug. 11. Analysis from year 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.

Table 1: The Effect of Deployment on Crime, DDD Design, Weekly

	(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 2: The Effect of Deployment on Crime, DDD Design, Weekly, 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 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 means refers to mean in control units, before Aug. 11. Analysis from year 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 3: The Spillover Effects of Deployment on Crime, DDD Design, Weekly, 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 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. Post× 2025×Non-deployed refers to DC PSAs without reported deployment. Control units created using police beats from Montgomery county and 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 means refers to mean in control units, before Aug. 11. Analysis from year 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 4: 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

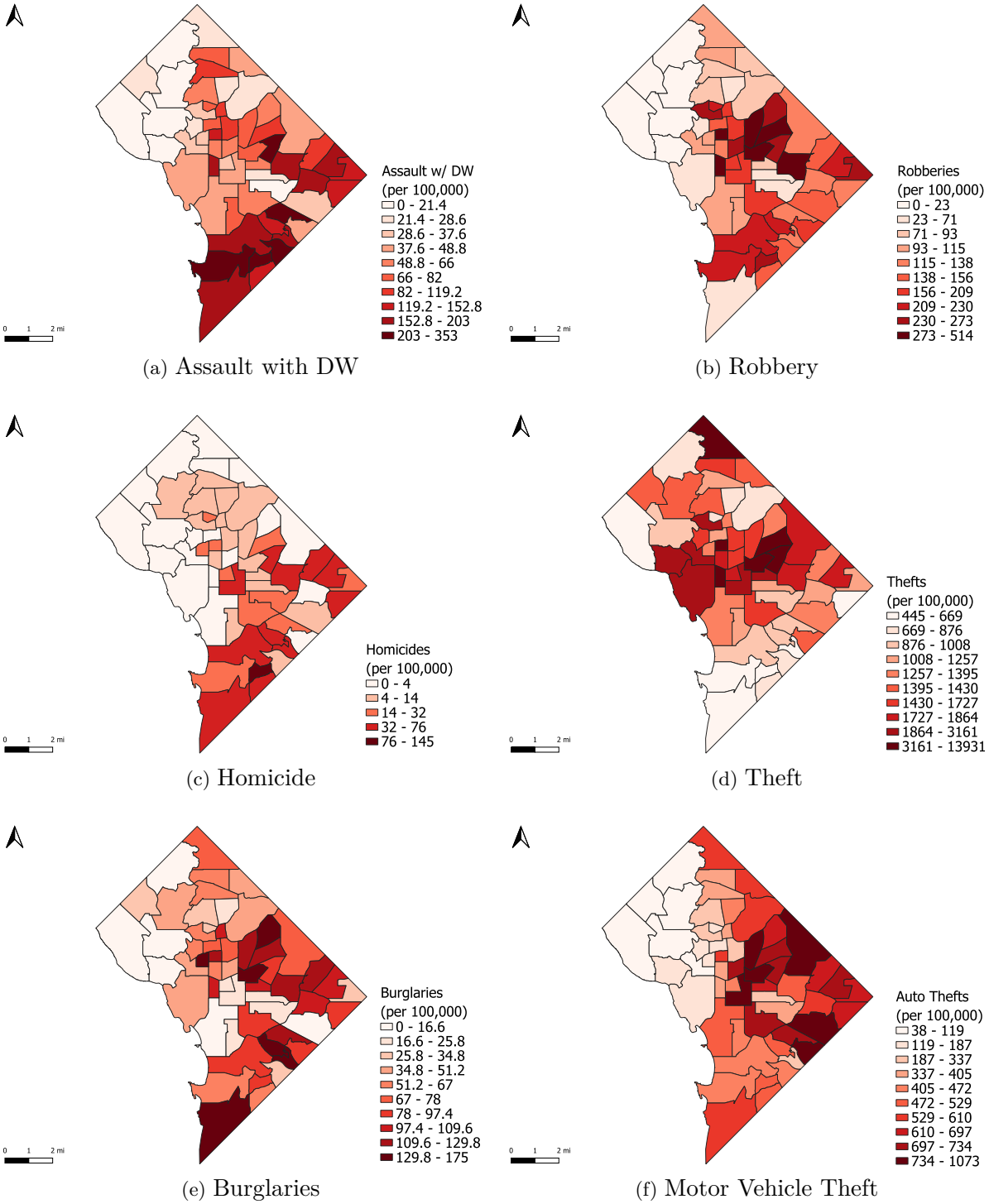
Notes: Each observation is a PSA. Each column reports estimates from Equation (4). Each estimate gives the association between the probability that there was a deployment in the PSA on Aug. 11, 2025, and pre-deployment crime levels and demographic controls. Columns (1)–(2) use crime totals from the 1-month 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 5: 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	
Assault w/ DW	0.106	0.065	233,039	−4.72	
Total	—	—	—	1.73	89.86

Notes: DDD coefficients in columns 1 and 2 are per PSA–week and come from Table 3, 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). Adjusted to 2025 dollars using 2010 annual CPI index of = 218.056 and August 2025 index = 323.976 (US Inflation Calculator, 2025). Robbery \$89,804→\$133,426; MVT \$5,466→\$8,121; Theft \$827→\$1,229; Assault \$156,823→\$233,039 . Total benefits in column 4 are computed as $(\hat{\beta}_{\mathrm{T}} \times N_T + \hat{\beta}_{\mathrm{T}} \times N_S) \times (30/7) \times$ (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.–based 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) - 958 (DC 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 (50 million from DC Guard + 36 million from out-of-state Guard).

A Appendix Tables and Figures



(a) Assault with DW

(b) Robbery

(c) Homicide

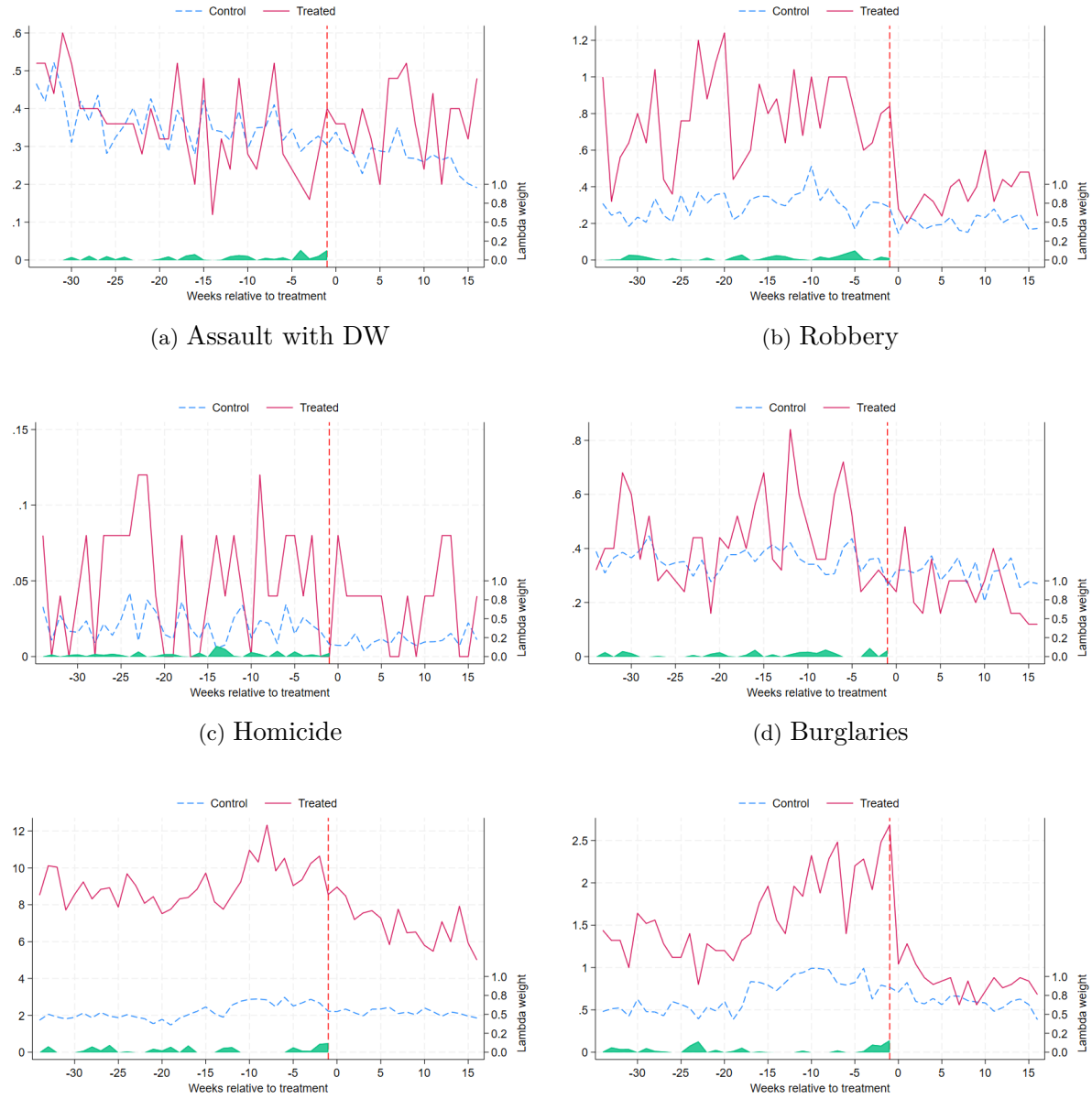
(d) Theft

(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.



(a) Assault with DW

(b) Robbery

(c) Homicide

(d) Burglaries

(e) Theft

(f) Motor Vehicle Theft

Figure A2: Crime Rates across PSAs: Crime Subcategories

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* × *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).

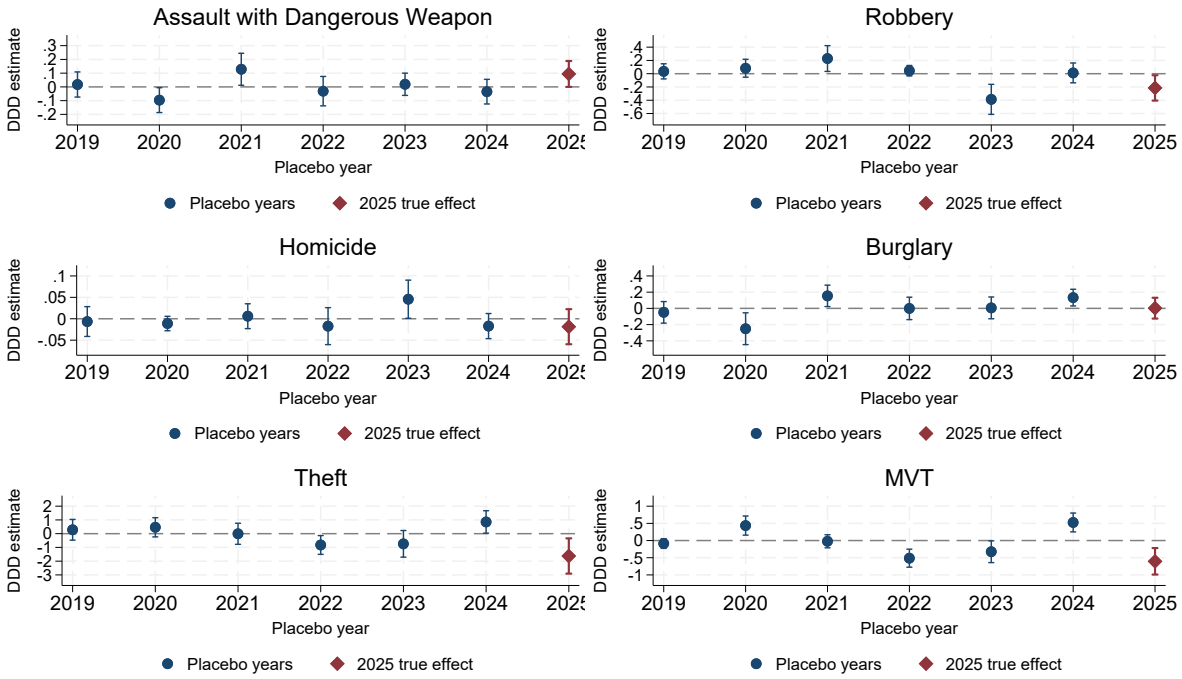


Figure A3: Placebo Cutoffs (Aug. 11 of Non-treatment years), DDD estimates, non-DC controls

Notes: Each blue dot gives the coefficient estimates β_k from Equation (2) 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 A13. 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.

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 2021–August 2025, based on 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 2025 divided by 365.25. "Theft" combines theft from auto and theft/other.

Table A2: The Effect of Deployment on Crime, DDD Design, PPML, Weekly

	(1)	(2)	(3)	(4)	(5)	(6)
	Assault w/ DW	Robbery	Homicide	Burglary	Theft	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 means 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 A3: The Effect of Deployment on Crime, DiD Design, Weekly

	(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 (3). 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 A4: The Effect of Deployment on Crime, DDD Design, Weekly, Crime rate

	(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 means 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 A5: The Effect of Deployment on Crime, DDD Design, Weekly, District level

	(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 means 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 A6: The Effect of Deployment on Crime, DDD Design, Monthly

	(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 means 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 A7: The Effect of Deployment on Crime, DDD Design, Aggregate Crime Categories

	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 × 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 × 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 means 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 A8: The Effect of Deployment on Crime, DDD Design, Weekly, 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 means refers to mean in control units, before Aug. 11. Analysis from year 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, non-DC controls, DID, Weekly

	(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 (3). 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 5. 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 A10: 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 means refers to mean in control units, before Aug. 11. Analysis from year 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 A11: The Effect of Deployment on Crime, DDD Design, Weekly, 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 means refers to mean in control units, before Aug. 11. Analysis from year 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 A12: 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 means refers to mean in control units, before Aug. 11. Analysis from year 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 A13: The Effect of Deployment on Crime, DDD Design, Weekly, 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 means refers to mean in control units, before Aug. 11. Analysis from year 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 A14: The Effect of Deployment on Crime, DDD Design, Weekly, non-DC controls, incluidng 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 means refers to mean in control units, before Aug. 11. Analysis from year 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 A15: 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 means 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 A16: 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 A17: Decomposing the Effect on Assaults with Dangerous Weapons, DDD Design, Weekly, 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 means refers to mean in control units, before Aug. 11. Analysis from year 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 A18: Shots detected via ShotSpotter Alerts, DDD Design, Weekly, 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 means 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 A19: 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 means 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 A20: The Spillover Effects of Deployment on Crime, DDD Design, Weekly, 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 means refers to mean in control units, before Aug. 11. Analysis from year 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 (4). 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 (4). 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, 2025, 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 (4). 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 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 (4). 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 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.