The Effects of Restricting Police Chases on Public Safety

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Abstract

This paper examines the effects of police departments implementing restrictive vehicular pursuit policies that limit chases to suspects of violent felonies. Using stacked difference-in-differences models and synthetic control methods, I analyze the impact of these policy changes on fatal police chase accidents and motor vehicle theft. I compile an original dataset of restrictive pursuit policy enactments, linked to data from the Fatality Analysis Reporting System and Uniform Crime Reporting program. I estimate a 41.3% reduction in fatal police chase accidents following the policy adoption. Motor vehicle theft increases by an imprecise 3.5%, while its clearance rate declines by 10.5%.

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1 Introduction

Police officers play a pivotal role in maintaining public safety by preventing and responding to criminal activity. In Becker (1968), an increase in the probability of detection and apprehension, for which police presence is a primary input, should decrease the incentive to commit crime. This presence can be felt throughout society, from schools to stadiums, and especially on city streets. While on vehicle patrol, officers exercise a wide range of power and discretion to enforce the law, such as employing emergency driving to apprehend a fleeing suspect.

The decision by a police officer to engage in a vehicular pursuit is a balancing act between capturing a subject and mitigating risks to the public's safety (Pipes and Pape, 2001). Officers must gauge how dangerous the subject is, the conditions of the road, the surrounding area, the busyness of the streets, and other factors (Alpert, 1998). These decisions have fatal consequences, as there are around 434 fatalities from police pursuit crashes annually (National Highway Traffic Safety Administration, 2025). For comparison, there are 979 deaths annually from police shootings (Ward et al., 2024). With these risks in mind, police departments might enact policies that restrict when officers can engage in vehicular pursuits. These restrictive policies often limit officers to chase only for certain levels of crimes, such as crimes deemed "violent felonies." Setting a pursuit threshold creates a potential trade-off for public safety. Definitive pursuit rules can reduce fatal police chases, but they may also lower the probability of apprehension for offenses below the threshold, which subsequently may incentivize such crimes.

In this paper, I study the causal effect of implementing a threshold for pursuits to only violent felonies. Specifically, I use stacked difference-in-differences, following Cengiz et al. (2019); Deshpande and Li (2019); and Wing et al. (2024) to study the effect of switching to a restrictive policy on public safety measured by fatal pursuit accidents and motor vehicle theft. I extend my crime analysis by using synthetic control methods to examine differential effects by city. The analytical sample consists of over 50 large US cities from 1995 to 2019, of which 13 adopt a restrictive pursuit policy.

I find that the switch to a restrictive policy causes a reduction in fatal police chase accidents with varying effects on criminal behavior. The switch to a restrictive policy leads to a statistically significant 0.307 decrease in fatal police chase accidents per six-month period (a 41.3% reduction relative to the pre-period mean). In addition, I find a 0.345 (40.0%) reduction in the number of total fatalities from police chases per six-month period.

I do not find evidence of a change in motor vehicle thefts induced by the policy with an estimated 3.5% increase, which is insignificant at conventional levels. However, I do find a 10.5% decrease in the clearance rate for motor vehicle thefts. Together, these results suggest that there is not a detectable change in deterrence or detection despite a clear decrease in apprehension from restricting vehicle pursuits. Given the imprecise average effect on motor vehicle theft, I estimate city-level treatment effects using the synthetic control method for each of the 13 treated cities. Three cities show statistically significant increases in motor vehicle theft, while the remaining cities do not.

This paper is the first to study restrictive police chase policies in a causal framework on public safety, measured by traffic accidents/fatalities, crime, and clearance rates. Due to the nature of the policy, it directly speaks to the literature on the deterrence effect of police, studied through plausibly exogenous positive shocks to police size by COPS grants (Evans and Owens, 2007; Mello, 2019; Weisburst, 2019) or police deployment following terrorist acts (Di Tella and Schargrodsky, 2004; Draca et al., 2011). This literature finds that exogenous increases in police presence lower crime, particularly property offenses, and that the impacts are localized to areas of deployment. On the other hand, the impact of negative shocks to police size and thus the probability of apprehension has been less documented. Weisburd (2021) examines when cops are pulled away from their regular beat when responding to outside 911 calls unrelated to crime, such as incidents relating to mental health, fire, suicides, etc., and finds that a 10% decrease in police presence increases crime by 7.4%. Similarly, DeAngelo and Hansen (2014) find the mass layoff of Oregon State Police caused an increase in traffic fatalities and accidents, relating to my work on both 1) deterrence and 2) traffic safety. I also contribute to a small literature examining the drivers of motor vehicle theft (Ayres and Levitt, 1998; Gonzalez-Navarro, 2013; Van Ours and Vollaard, 2016).

Closest to my analysis, Gillooly et al. (2021) examine pursuit policy reforms by both the Roanoke County Police Department and Roanoke City Police Department, and provide evidence that more restrictive policies reduce pursuit activity without increasing crime. I build on their findings by analyzing pursuit police enactments across multiple cities and over time in a causal framework. Fatalities from police chases are less frequent than fatalities from police shootings but represent a similar high-risk form of police-civilian interaction that warrants policy attention. This study situates those risks within the larger literature on police use of force and accountability (Ba et al., 2021; Goncalves and Mello, 2021; Hoekstra and Sloan, 2022). As tighter pursuit policies reduce officer discretion in initiating pursuits, my work also aligns with studies on how officer discretion influences public safety (Gonçalves and Mello, 2023) and how internal monitoring affects enforcement activity (Ba and Rivera, 2019).

2 Background

A vehicular pursuit is defined as "an attempt by an officer in an authorized emergency vehicle to apprehend a suspect who is actively attempting to elude apprehension while operating a vehicle as defined by applicable law," (International Association of Chiefs of Police, 2019). Reaves (2017) finds that, across 115 agencies that provided data for 5,568 pursuits from 2009 to 2013, 15% of pursuits ended in a collision, with 2% ending in injuries and 0.5% ending in a fatality. Of these pursuits, 69% started due to a traffic violation, and 12% and 9% were for nonviolent and violent felonies, respectively. A similar report from the California Highway Patrol (2023) shows that, of the 11,985 chases reported across the state in 2022, 19% resulted in crashes, 6.7% ended in injuries, and 1.4% were fatal.[1]

Departments may set guidelines to ensure officers only initiate pursuits under specific criteria, given the risks at stake. In general, there are four types of pursuit policies: discretionary, permitted, restrictive, and prohibitive (International Association of Chiefs

[1]The respective figures in 2021 were 12,513, 20.1, 7.1, and 1.7.

of Police, 2019). Discretionary policies are typically the baseline and least restrictive category, allowing officers to initiate pursuits largely based on their discretion, while permitted policies require supervisor approval. Notably, experts suggest a restrictive policy, which limits officers to engage in pursuits only for crimes of a certain threshold, commonly set at violent felonies (Police Executive Research Forum, 2023). A department may enact a prohibitive policy that restricts officers from engaging at all, though only approximately 2% of local police departments had this policy in 2013 (Reaves, 2017).

There is descriptive evidence that restrictive policies are associated with fewer pursuits. Reaves (2017) finds that departments with restrictive pursuit policies employed 78% of all officers yet only accounted for 69% of all pursuits. Meanwhile, departments with discretionary policies disproportionately accounted for 19% of pursuits while employing only 11% of officers. Gillooly et al. (2021) report that switching to restrictive policies was associated with shorter pursuits by the Roanoke County Police Department and fewer pursuits by the Roanoke City Police Department, with no increase in criminal activity. Furthermore, after the Dallas Police Department adopted a restrictive policy in 2006, pursuits decreased from 354 in 2005 to 70 in 2007, and pursuit-related injuries and fatalities fell from 98 to 21 (Dallas Police Department, 2010).

Limiting vehicle pursuits to only violent felonies may reduce the probability of apprehension for property crimes like motor vehicle theft. Historically, motor vehicle theft has trended downward since the 1990s (Appendix Figure B2), likely due to advances in anti-theft technology.[2] Since 2019, however, motor vehicle theft has surged by 42% (Federal Bureau of Investigation, 2024), likely due to an information shock on how easily certain Hyundai and Kia vehicles can be stolen.[3] Around the same time, several governments adopted restrictive pursuit policies. The Washington State Legislature passed House Bill

[2] Ayres and Levitt (1998) find that the entrance of Lo-Jack, a company that sells hidden transmitters in vehicles, sharply reduces motor vehicle theft within a city. Van Ours and Vollaard (2016) show through a nature experiment in Europe that the engine immobilizer is a key contributor to the reduction in motor vehicle thefts.

[3] As described in Morrison (2023), some older Kia and Hyundai vehicles are not equipped with engine immobilizers. To steal such a car, one can "rip off the steering column cover, remove the ignition cylinder, and turn the rectangular nub behind it to start the engine." The trend gained traction on social media platforms such as TikTok and Instagram, where teenagers uploaded videos of themselves driving stolen vehicles.

1054 in 2021, which forced all law enforcement entities within the state to prohibit officers from initiating pursuits unless there was probable cause an individual had committed a violent or sex offense. Critics cited increased crime across the state, and in 2024, the law was amended to allow pursuits for a person who has "violated the law" and "poses a threat to the safety of others," (Ballotpedia, 2024; Schrager, 2024). New Jersey also reversed course on a statewide pursuit policy in March 2023, allowing officers to chase motor vehicle theft offenders just three months after a statewide restrictive policy was put in place (Johnson, 2022). In both cases, officials cited increases in motor vehicle theft as a driver for changing their pursuit policy, which motivates studying if restrictive pursuit policies cause increases in motor vehicle theft.[4]

3 Data

I combine publicly available datasets with my own collection of department pursuit policies. The Fatality Analysis Reporting System (FARS) from the National Highway Traffic Safety Administration (2024) provides population data for all fatal motor vehicle crashes across the United States. Each observation contains the date, time, city, county, and importantly, cause of the accident. I measure the number of fatal accidents caused by police pursuits within a city over a six-month period of time (i.e, a half-year). Given there is no national database on total police pursuits or police pursuit outcomes (to my knowledge), this is the best available public source to analyze pursuit outcomes across multiple cities and over several years. I use Uniform Crime Reporting (UCR) program data for my crime analysis.[5] The raw data provided from the FBI contains a fair share of measurement error, as noted in Evans and Owens (2007). I use a similar cleaning procedure used in Evans and Owens (2007) to detect outliers, with a more thorough explanation of the data cleaning process provided in Appendix A. Lastly, my sample starts

[4] I do not examine these two state-based policies due to the timing with the COVID-19 pandemic and the surge in stealing Kias and Hyundais, and I instead opt to use a city-level analysis in earlier years for identification.

[5] In particular, I use Jacob Kaplan's cleaned Property Stolen and Recovered (Supplement to Return A); Offenses Known and Clearances by Arrest (Return A); and Law Enforcement Officers Killed and Assaulted (LEOKA) datasets (Kaplan, 2024, 2025a,b). Kaplan performs some general cleaning but leaves more intensive data cleaning to the user.

in 1995 and ends in 2019, due to data limitations in 2020 and onward from the FBI's transition away from the UCR to the National Incident-Based Reporting System.[6]

Many police departments do not formally report or publicly record internal policy changes, such as vehicular pursuit policies. Instead, I manually compiled these policies across cities where possible for the 200 most populous cities in the United States. This yields a sample of 56 cities, of which I analyze 13 adopters. I focused primarily on policy changes towards a violent felony threshold. This policy is recommended by the Police Executive Research Forum (2023) and creates a discrete standard for which offenses permit a pursuit. Additionally, the Dallas Police Department reported roughly an 80% decline in pursuits following the adoption of such a policy (Dallas Police Department, 2010), illustrating that limiting chases to violent felonies can produce a meaningful reduction in pursuits. Appendix A provides more explanation on the data gathering process.

Appendix Table B1 documents how the effective dates of restrictive pursuit policies were determined for each of the 13 adopting cities. The effective dates range from 2000 to 2014, with a cluster around 2005. The sample does not encompass all large, metropolitan cities with restrictive pursuit policies due to difficulty pinpointing the effective dates for some cities and unreliable crime reporting for others. There are 43 cities used strictly as controls in the sample, although the set of control units varies by treated cohort, and some late-adopters are excluded from the formal analysis window. These control cities are primarily governed by baseline "discretionary" pursuit policies, as discussed further in Appendix A. Cities are dispersed across populous regions of the United States, although a relatively large share reside in California. Figure 1 displays the geographic distribution of cities in the sample. A complete list of cities in the sample appears in Appendix Table B2.

The main outcomes are fatal police chase accidents, police chase fatalities (the total number of fatalities from police chases), logged motor vehicle theft, and motor vehicle theft clearance rate, measured at a city, six-month window. FARS provides two measures:

[6] As discussed in Addington (2019), the FBI urged departments to report incident level crime data to National Incident-Based Reporting System instead of the UCR by January 2021. I encountered data oddities beginning in 2020, perhaps due to the transition and other confounding factors from the COVID-19 pandemic.

the number of fatal traffic accidents involving police pursuits (fatal police chase accidents) and the total fatalities resulting from those accidents (police chase fatalities). Motor vehicle theft data are sourced from the UCR.[7] The motor vehicle theft clearance rate is calculated as the proportion of monthly motor vehicle theft arrests to incidents.

Table 1 displays the sample means for the outcomes of interest and other characteristics of adopting cities compared to non-adopting cities. In general, cities that adopt restrictive pursuit policies are more populated and have more fatal police chase accidents, police chase fatalities, and motor vehicle theft offenses. Appendix Figure B1 displays histograms of the four measures of interest. Fatal police chase accidents are a rather rare outcome, with less than one on average in a given city-half-year. Previewing my findings in the raw data, the average fatal police chase accidents in a city-half-year decreases from 0.744 to 0.453, or a 39.1% reduction, following the enactment of a restrictive policy, while control cities slightly increase from 0.348 to 0.355. Meanwhile, motor vehicle theft in treated cities decreases 12.9% after enactment, compared to a 9.7% decrease in control cities. This pattern suggests the policy does not lead to an increase in motor vehicle theft in treated cities compared to control cities, and that motor vehicle theft may move in the *opposite* expected direction. However, this is only evidence represented in sample means, and I leave my causal analysis to Section 4.

4 Methodology

4.1 Stacked DiD

I analyze the treatment effect of restrictive vehicular pursuit policies using the staggered rollout across cities in a difference-in-differences (DiD) framework. I classify restrictive policies as a binary indicator based on whether the department implemented a violent felony clause within their pursuit policy. There may be other smaller policy changes occurring in police departments that do not meet the same level of restrictiveness in the remaining control cities, but this would only underestimate the treatment effect.

[7] Across figures and tables, I present the average monthly motor vehicle theft within a city and unit of time instead of the log transformation. However, all regression output uses the log transformation.

Similarly, police departments may have varying levels of restriction prior to enactment, such as allowing chases based on supervisor approval or for any crimes above traffic infractions, which again may provide an underestimate.

Given the staggered adoption of treatment, I use a stacked difference-in-differences design (Cengiz et al., 2019; Deshpande and Li, 2019). The stacked estimator uses a panel of "sub-experiments" to ensure that treated units are never used as counterfactuals. In particular, this approach avoids comparisons where early-treated units serve as controls for later-treated units (Goodman-Bacon, 2021) and mitigates bias arising from heterogeneous treatment effects (Baker et al., 2022). Following Wing et al. (2024), I construct each sub-experiment with an equal number of pre-periods and post-periods. I also apply the Wing et al. (2024) weighting adjustment to account for differential implicit weighting created by the stacked estimator. The weights address potential bias that arises when treated and control unit shares differ across sub-experiments by scaling observations according to the relative sizes of treated and control groups.

I first create a dataset for each sub-experiment ("stack") and then append all the "stacks" into one large panel. From the original dataset, let $\tau = T_{\min}, \ldots, T_{\max}$ index calendar time. Additionally, let the adoption time for a treated city i in sub-experiment s be denoted by α_s. Each sub-experiment uses an event-time window centered on α_s, with k_{pre} pre-periods and k_{post} post-periods. Following this, there are two inclusion criteria for how a stack is built. Firstly, the calendar-time window $(\alpha_s - k_{\text{pre}}, \alpha_s + k_{\text{post}})$ for sub-experiment s must fall entirely within the set $(T_{\min}, T_{\max})$. Secondly, there is a clean control imposition: every control unit within this window must not have an adoption time α_i occur at or before $\alpha_s + k_{\text{post}}$. To summarize, I only analyze a sub-experiment if its entire event-time window is observable, and only non-treated units are used as comparisons within each stack.

Calendar time τ is measured in six-month intervals for each city i. In the stacked dataset, t indexes a common event-time relative to adoption in sub-experiment s, with $t = 0$ denoting the first treated period. I consider three years prior to treatment (i.e., $k_{\text{pre}} = 6$) and 4 years following the policy (i.e., k_{post}=8) to balance the need to track outcomes before and after the policy change while maintaining as many sub-experiments as possible within the time frame of available data.[8] Thus, each stack includes event times $t \in \{-6, \ldots, 8\}$.

In the dataset combining all stacks, I estimate the following regression:

$$Y_{ist} = \beta_1 Post_{ist} \ + \theta X_{ist} + \gamma_{is} + \delta_{st} + \epsilon_{ist} \tag{1}$$

where i indexes cities, s indexes sub-experiments, and t indexes the event-time. Y_{ist} is the reported outcome of interest. $Post_{ist}$ is a binary variable equal to 1 for treated city i in sub-experiment s in post-adoption event times $t = 0, \ldots, 8$. γ_{is} denotes $sub-experiment \times city$ fixed effects, which capture time-invariant characteristics of a city within a sub-experiment. The fixed effects can vary across sub-experiments. δ_{st} represents $sub-experiment \times event-time$ fixed effects. These fixed effects capture time-specific shocks common to all cities within a sub-experiment, which effectively provides calendar-time fixed effects. In this specification, β_1 is the average treatment effect on the treated (ATT).[9] I also include a vector of time-varying city characteristics, denoted by X_{ist}, that include the unemployment rate and population. I follow the weighting methods proposed in Wing et al. (2024), to account for different compositions of treated and control units by sub-experiment, and I also provide results without the weighting procedure. Standard errors are clustered by city to allow for dependence across time and sub-experiments within cities.

A primary assumption underlying the stacked DiD models is that treated cities would have followed similar trends to control cities in the absence of the policy intervention. While this "parallel trends" assumption is not directly testable, I provide evidence suggesting whether it holds by presenting dynamic DiD analyses for each outcome. The following equation is used to visualize treatment effects before the policy effective date:

[8] For example, San Antonio is treated in 2013 and only has pre-treatment data for 3 years. Thus, San Antonio could not be considered in the analysis sample for any value of $k_{\text{pre}} > 6$. Similarly, I can only observe post data through 2019. Thus, treatment dates toward the end of the sample period could not be included if treated after 2015. See Table B1 for a complete list of treatment cities and timing of adoption.

[9] For brevity, I refer to this ATT as the average treatment effect (ATE) throughout the paper.

$$Y_{ist} = \sum_{\substack{k=-6 \\ k \neq -1}}^{8} \left[\beta_k T_{is} \times 1[t = k] \right] + \theta X_{ist} + \gamma_{is} + \delta_{st} + \epsilon_{ist} \quad (2)$$

with indexing similar to Equation 1. T_{is} indicates whether city i is treated during sub-experiment s. The β_k coefficients represent treatment effects relative to the baseline time t = -1. This is unlike Equation 1, which produces the average treatment effect from times $t = 0$ to $t = 8$ relative to the entire pre-period. To provide evidence that treated and control groups trended similarly before the policy adoption, the treatment effects before time t = -1 should remain flat and near zero. The model also assumes the policy is not confounded by other possible shocks that could affect the outcomes of interest.

4.2 Synthetic Control Method

To supplement the stacked DiD estimations, I use synthetic control methods to examine the effect of restrictive pursuit policies on motor vehicle theft for each treated city. The nature and frequency of the UCR data allow me to apply the synthetic control method to motor vehicle theft, as opposed to other infrequent outcomes such as fatal police chase accidents. Similar to the stacked DiD estimation, each treated city has its own sample that consists only of the treated city i and clean control cities in the donor pool, denoted by j. Each sample is fully balanced and has the same amount of pre-periods and post-periods as the stacked sub-experiments. These samples are identical to the sub-experiments mentioned in the stacked approach.

The synthetic control method generates a counterfactual for how the treated city would have behaved absent a policy change. This counterfactual is created by minimizing the following function:

$$(X_i - X_j W)' V (X_i - X_j W) \quad (3)$$

where X_i is a vector containing pre-period values of predictors for the treated city i, X_j is a matrix of the same predictors for the donor (control) cities, W is a vector of non-negative weights assigned to donor cities summing to one, and V is a diagonal matrix indicating the importance weights assigned to each predictor variable. I choose pre-treatment logged motor vehicle theft (MVT), population, and the unemployment rate as covariates to build the unobserved counterfactual. To avoid overfitting, I match the outcome of interest, logged motor vehicle theft, on periods t = -6, -4, and -2. I match the remaining covariates on all pre-periods. An average treatment effect is then estimated by averaging the differences between the observed outcome, MVT_i, and its counterfactual in each half-year in the post-period.

Following Abadie et al. (2010), I run placebo tests for each donor city j within a sub-experiment sample, replicating the synthetic control procedure used for city i. Each donor city j is classified as treated and has its own counterfactual created. A placebo treatment effect is then generated. After this, I calculate the ratio of the post-treatment root mean squared prediction error (RMSPE) to the pre-treatment RMSPE for each city within a sample. This is used to uncover a p-value based on the rank of the treated city's RMSPE ratio compared to the placebos'. For example, if a treated city has the 2nd largest RMSPE ratio out of 50 cities, its p-value would be 0.04 = (2/50).

5 Results

5.1 Police Chase Incidents

I first examine the effect of restrictive pursuit policies on fatal police chase accidents and police chase fatalities, often the intended goal of such policies. Figure 2 displays dynamic treatment effects relative to the six-month interval before the effective date, using results from the stacked DiD model with covariates and the weighting procedure. To support the "parallel trends" assumption, pre-treatment coefficients should be approximately zero, signaling no differing effect prior to the policy implementation. A concern may be that departments adopt their policy in response to an increase in police chase fatalities, similar to an Ashenfelter Dip (Ashenfelter, 1978). Figure 2a, which reports fatal police chase accidents, shows these estimates are small and statistically insignificant, though the standard errors are large. After adoption, the estimates are negative, with the largest dip about a year after implementation.

Figure 2b displays dynamic DiD estimates for total police chase fatalities and shows pre-treatment coefficients close to zero, also with relatively large standard errors. The effect of the policy is not seen until about a year after the policy has been put into place, similar to the results using fatal police chase accidents as an outcome. Since the effective date falls within period t=0, officers may pursue based on the prior non-restrictive policy during the initial period of the event-time, which would attenuate the estimate.[10] Regardless, the estimated negative effect of the policy on police chase fatalities continues in the years following this lag.

The estimated average treatment effects for pursuit outcomes are provided in Table 2. Columns (1) and (2) present the results from Equation 1, with Column (2) including covariates. Columns (3) and (4) provide the same set of results using the Wing et al. (2024) weighting procedure, with the latter column including covariates. Across model specifications, estimates remain stable. Statistical significance only varies when examining the effect on fatal police chase accidents, but the difference in p-value is marginal.[11] Using Column (4) as a preferred specification, the policy leads to a statistically significant 0.307 decrease in the number of fatal police chase accidents in a half-year. Relative to a pre-treatment mean of 0.744, this is an economically meaningful 41.3% reduction. Similarly, the policy adoption results in a 0.345 decrease in the number of police chase fatalities, which is a 40.0% reduction compared to the pre-treatment mean and statistically significant at the 10% level.

In general, the results provide economically meaningful evidence that restrictive pursuit policies cause decreases in the number of fatal police chase accidents and police chase fatalities. Statistical significance varies across specification, which may partly reflect the rarity of such outcomes. The outcomes are measured only within the city boundaries of the local police department and do not include instances where the department's pursuit spills into a neighboring jurisdiction. Also, spillover pursuits from other jurisdictions or pursuits initiated by agencies, such as a state police department, may contribute to

[10] In my analysis, the only city that had its effective date event-time pushed back was Philadelphia, PA. The policy went into place 12/31/2008, so I treat the actual enactment date as 1/1/2009.

[11] For example, adding covariates to the model with sub-experiment fixed effects decreases the p-value from 0.053 to 0.041.

measurement noise in the outcomes.

5.2 Crime

I now examine the effect of restrictive pursuit policies on crime deterrence by examining logged motor vehicle theft and motor vehicle theft clearance rates. I first present the dynamic DiD estimates in Figure 3. The dynamic treatment effects in Figure 3a suggest that there is a slight uptick in motor vehicle theft about 1 year after the policy is put into place. Furthermore, the treatment effects at latter periods of time t exhibit large standard errors, indicating some type of unobserved heterogeneity. Using Column (4) from Table 2 as a preferred model, the policy is associated with a 3.5% increase in motor vehicle theft, though this estimate is statistically insignificant, and the 95% confidence interval only rules out effect sizes larger than 14.0%.

Following this null result, I present synthetic control estimates for each city, shown in Figure 4. These estimates are calculated as the difference between each treated city's observed outcomes and its synthetic control counterfactual, as displayed in Appendix Figure B3. Of the thirteen treated cities, seven exhibit an increase in logged motor vehicle theft. Three of the four cities with the largest increases are statistically significant at conventional levels, and the estimated p-value for Louisville is just above 0.10. Three cities experience little effect. Last, three cities show declines in motor vehicle theft, though all estimates are statistically insignificant. Differences in the salience of the policy may explain the heterogeneous effects across cities. Cities with large increases in motor vehicle theft (Louisville, San Antonio, and Denver) publicly announced their policy changes through local news coverage.[12] This publicity may have signaled a lower probability of apprehension and encouraged opportunistic crimes like motor vehicle theft. Two of the three largest increases occurred in cities adopting such a policy in the early 2000s, when motor vehicle theft was more prevalent and opportunistic. Improvements in anti-theft technology and tracking may explain why later adopting cities do not experience similar large increases in motor vehicle theft.

[12] See local coverage in the following embedded hyperlinks: Louisville, San Antonio, and Denver.

I next examine the effect on the clearance rate for motor vehicle theft. Even if the policy does not change the volume of motor vehicle theft offenses, it may reduce the clearance rate if officers initiate fewer pursuits and subsequently make fewer arrests for motor vehicle theft crimes. Figure 3b shows that after the policy is implemented, the clearance rate for motor vehicle theft begins to decrease in treated cities relative to control cities, but the estimated treatment effects suddenly increase around time $t = 7$. At this time, the motor vehicle theft clearance rate in San Jose suddenly spikes from below 20% to above 40%, placing the city in the 99th percentile of motor vehicle theft clearance rates in the sample. Despite this increase, however, the estimated effect of the policy is a statistically significant 1.4 percentage point decrease in the clearance rate, which corresponds to a 10.5% drop relative to the pre-period mean.

The spike in San Jose's clearance rate appears to be an anomaly, as the number of arrests returns to levels before the spike shortly after, while offenses remain high (Appendix Figure B4a). To assess this sensitivity, I create a new dataset without the San Jose sub-experiment and plot the treatment effects alongside the results from the original stacked dataset (Appendix Figure B4b). The treatment effects excluding the San Jose sub-experiment behave similarly but remain negative throughout the post-period, and the standard errors are noticeably smaller. With these results, the policy is shown to reduce clearance rates for motor vehicle theft by 1.8 percentage points, a statistically significant 13.9% decrease relative to the pre-treatment mean.[13] I retain the full-sample estimate as the primary result, but note the effect is larger in magnitude and more precise when excluding San Jose.

Taken together, the combination of little change in motor vehicle theft and a decline in its clearance rate presents a unique deterrence finding. Because the clearance rate is defined as arrests relative to offenses, the results imply that officers make fewer arrests of motor vehicle theft offenders following the policy change. However, motor vehicle theft does not increase materially on average, which contrasts with a simple deterrence prediction. This paired finding may suggest that arrests stemming from pursuits have

[13] In the stacked dataset without San Jose, the motor vehicle clearance rate mean before treatment is 12.9%. The p-value of the estimate is 0.004.

limited marginal deterrent effect for motor vehicle theft.

5.3 Additional Analysis

Restricting pursuits to only violent felonies is shown to have some economic trade-offs. Fatal police chase accidents and police chase fatalities are shown to decrease, but the clearance rate for motor vehicle theft also decreases. However, these outcomes do not necessarily encapsulate the entire impact of the policy. The model also may be sensitive to the particular way outcomes are measured, and the specific definition of the main outcomes is a subjective decision.

I begin by testing sample sensitivity in the stacked DiD using a leave-one-out re-estimation that omits one sub-experiment at a time. Figure 5 presents this analysis. The estimates for fatal police chase accidents remain negative, and the median p-value across the leave-one-out estimates is 0.051, a slight increase from the full sample p-value of 0.041. The police chase fatalities leave-one-out estimates show a similar pattern, and the estimates have a median p-value of 0.070, compared to the full sample p-value of 0.057. As indicated previously, the motor vehicle theft clearance rate estimate is sensitive to whether the San Jose sub-experiment is included, and statistical significance varies modestly across leave-one-out specifications. In a panel that excludes the San Jose sub-experiment throughout, the clearance rate estimates remain consistent and statistically significant.

The estimated benefits of the policy may be sensitive to the choice of outcome measure. Given the rarity of fatal police chases, it may be simpler to analyze whether or not any fatal accident occurred within a city, six-month interval. Figure 6 presents the dynamic effects of the policy using an indicator variable for the occurrence of any fatal police chase accident as the outcome. Table 2 reports an 13.9 percentage point decrease on average for treated cities relative to control cities, which is statistically significant at the 10% level. A potential concern is that total traffic fatalities could increase if drivers anticipate fewer pursuits for traffic infractions, reducing the policy's traffic-safety benefits. Figure 6 shows no detectable increase, and Table 3 estimates a statistically insignificant reduction of 0.417 fatalities, a 1.1% decrease. This result may be expected if the policy primarily affects non-violent property offenders, since some treated departments already prohibited pursuits for traffic infractions before adoption.

Motor vehicle theft may not be the only crime incentivized by the policy change. Therefore, I also use indexes for property crime, violent crime, and total crime as dependent variables. The property crime index should capture other lower-level crimes, such as theft, that no longer constitute as reasons for a pursuit, and also includes motor vehicle theft. Violent crime should act as a natural placebo, as officers are still able to pursue suspects of violent felonies. To check for endogenous policy adoption, I estimate the effect on the total crime index (property plus violent crime). Last, I examine the clearance rate for each index.

The dynamic treatment effects for each index and its clearance rate are presented in Figure 6. Across indexes, there is no detectable increase in crime. The treatment effects before adoption are near zero, which supports the assumption that crime in treated cities trended similarly to respective control cities. After adoption, the clearance rates remain mostly stable. Table 3 shows the point estimates are all statistically insignificant. There is a 4 percentage point decrease in the violent crime index, though this the estimate is statistically insignificant. The estimates are also shown to be stable when adding covariates.

To check for sensitivity to broader economic or demographic changes in treated cities, I examine dynamic trends in logged population, unemployment rate, and logged number of employed officers. Appendix Figure B5 displays these dynamic treatment effects. Treated cities have an increasing trend in population before the policy adoption, though this growth starts to taper off after the policy adoption. The unemployment rate in treated cities is decreasing before the time of adoption but reverses course. Regardless, the results with and without these covariates are robust. Following the policy adoption, there is a 4.8% increase in employed officers. While this increase may be due to an increase in population, it could signal either improved officer retention or increased hiring in response to altered job conditions. Given the number of officers employed may be endogenous to the policy, I exclude this variable from any vector of covariates.[14]

5.4 Cost-Benefit Calculation

I use the estimates from the effects on police chase fatalities and crime to provide back-of-the-envelope welfare calculations. I first calculate the benefit by the estimated decrease in police chase fatalities (0.345) multiplied by the value of a statistical life ($9,000,000, as estimated by Viscusi (2010)), which is equal to $3,105,000 in a city, six-month interval. Next, I use my estimate from the expected increase in motor vehicle theft to calculate costs of the policy. Because I present the MVT estimates in logs, I recover a level change by first constructing the post-period counterfactual for treated cities from the control-city trend. From Table 1, I calculate the expected change absent the policy to be the difference in MVT in untreated cities at the time of treatment ($\mathbb{E}[Y \mid T{=}0, \text{Post}{=}1] - \mathbb{E}[Y \mid T{=}0, \text{Post}{=}0]$), a 9.7% decline relative to $\mathbb{E}[Y \mid T{=}0, \text{Post}{=}0]$. I then compute the expected post-period MVT in a treated city, absent the policy, as $700.11 \times 6 \times (100 - 9.7)\% \approx 3{,}835$.[15] Applying the estimated 3.5% increase to this counterfactual implies about 134 additional thefts in a city, six-month interval. At a social cost of $10,722 per theft (McCollister et al., 2010), this produces an expected cost of $\approx \$1,440{,}000$.

The estimated benefit and cost calculations are presented in Figure 7, with 95% confidence intervals represented in shaded bars. At the upper bound, the estimated benefits ($6.24M) just outweigh the estimated costs ($5.67M). The large confidence intervals on MVT also cannot rule out *decreases* in the social cost of MVT up to ≈ $2,400,000, and the lower bound on the estimated benefits is slightly negative (≈ -$35,000). The model does not encompass all costs and benefits but is a useful benchmark encompassing the metrics observed in this study (i.e., MVT and lives lost from police chases). For example, the model does not account for benefits such as avoided injuries, general traffic wreckage, and hospital costs, but it also does not capture the decrease in the clearance rate

[14] When including this variable in the vector of covariates, the estimate on logged motor vehicle theft increases to 0.045 but remains imprecise ($p = 0.362$).

[15] This multiplies the presented monthly mean by six months and then imposes the control-group post change, assuming the treated cities would have trended similarly to control cities absent the policy. As shown in Figure 3, MVT in treated trended similarly to control cities before the policy adoption.

for MVT. Ultimately, this benefit can be perceived as a lower bound, which appears to outweigh the calculated costs through MVT offenses.

6 Conclusion

This paper examines the effects of police departments adopting restrictive vehicular pursuit policies that limit chases to suspects of violent felonies. Exploiting these policy changes using stacked difference-in-differences models, I estimate the adoption reduces fatal police chase accidents and fatalities by 41.3% and 40.0%, respectively. The policy adoption is associated with a statistically insignificant 3.5% increase in motor vehicle theft, while clearance rates decrease by a statistically significant 10.5%. The latter results present a contrasting pattern: while officers make fewer arrests, there is no distinct increase in motor vehicle theft. Results from synthetic control methods further highlight that in a few select cities, motor vehicle theft significantly increases following the policy change.

To put these results in perspective, there were an estimated 434 deaths from police pursuits annually in the United States between 2015 and 2020 (National Highway Traffic Safety Administration, 2025). Compared to approximately 979 civilian deaths from police shootings each year over the same period (Ward et al., 2024), fatalities from police chases are less frequent but still represent a significant challenge to public safety and health. This paper aims to contextualize the effects of limiting such chases through restrictive pursuit policies. I find the policies are welfare-enhancing through the value of lives saved compared to the cost of motor vehicle theft offenses. The cost-benefit calculation potentially produces a lower bound estimate for the benefit by only capturing lives saved, though in certain contexts the cost of motor vehicle theft offenses may also be high. Altogether, the findings suggest that public safety is likely to improve when departments adopt restrictive vehicular pursuit policies, providing policy-relevant evidence for agencies considering adoption.

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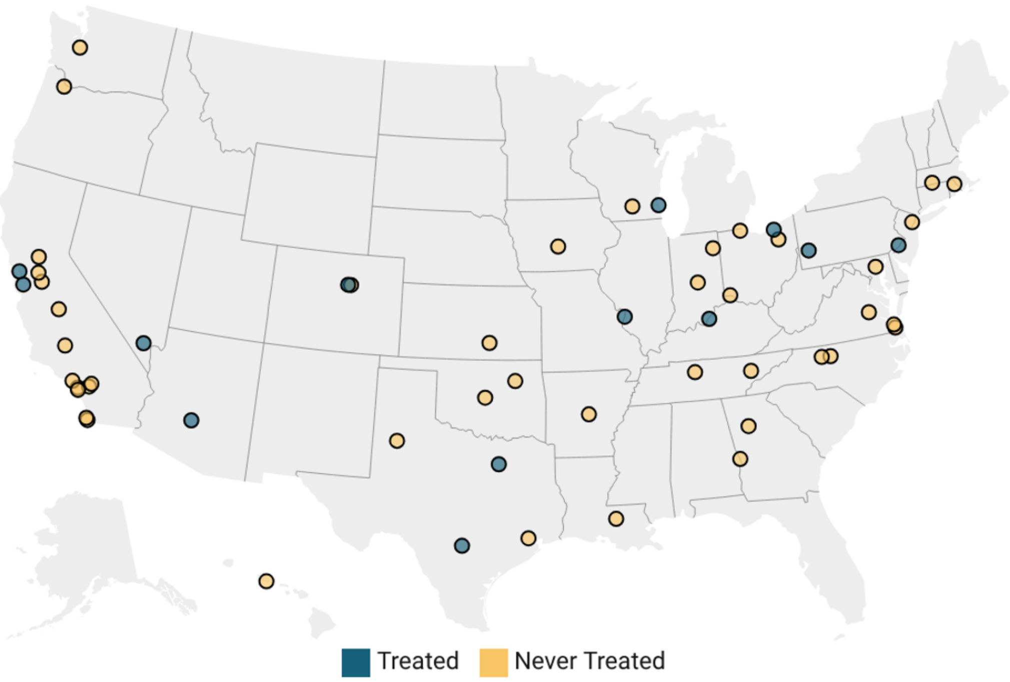
10, 13, 31, 32, 33, 41]

7 Figures & Tables

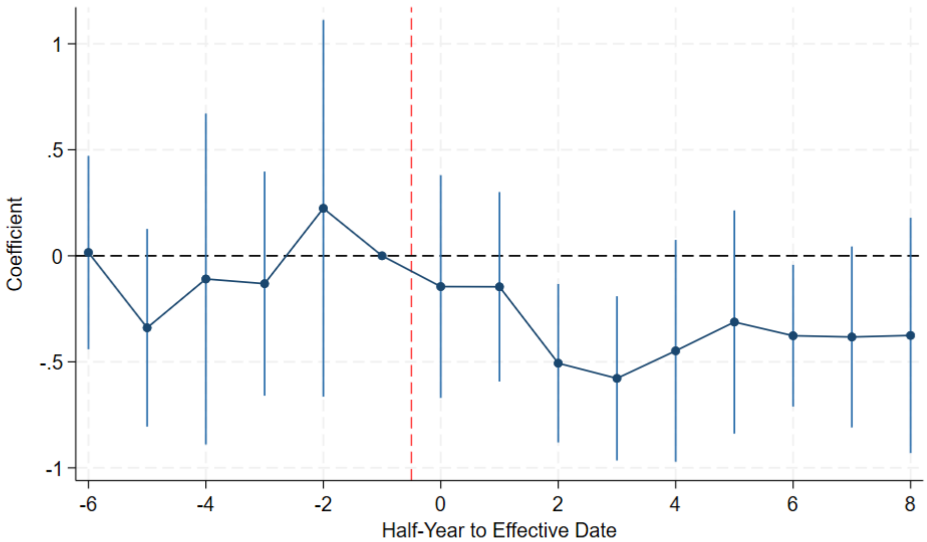
Figure 1: Placement of cities in analytical sample



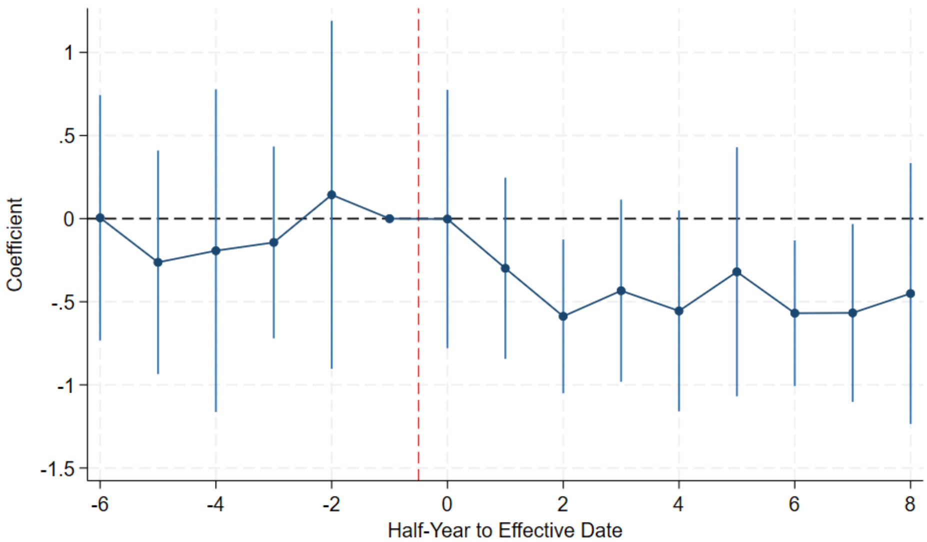
Notes: The map provides the locations for all cities used in the analysis. A complete list of cities can be found in Appendix Table B2.

Figure 2: Dynamic effects of a restrictive policy on pursuit outcomes

(a) Fatal police chase accidents



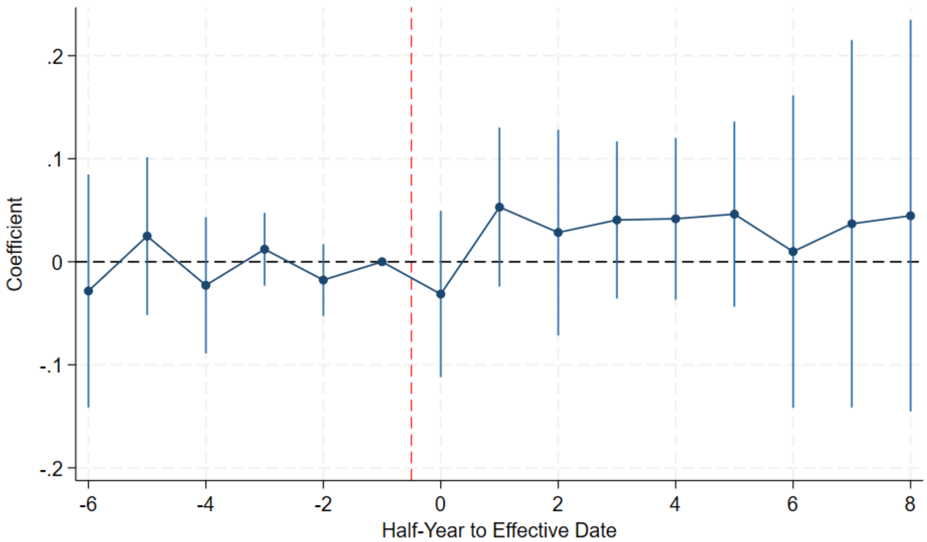
(b) Police chase fatalities



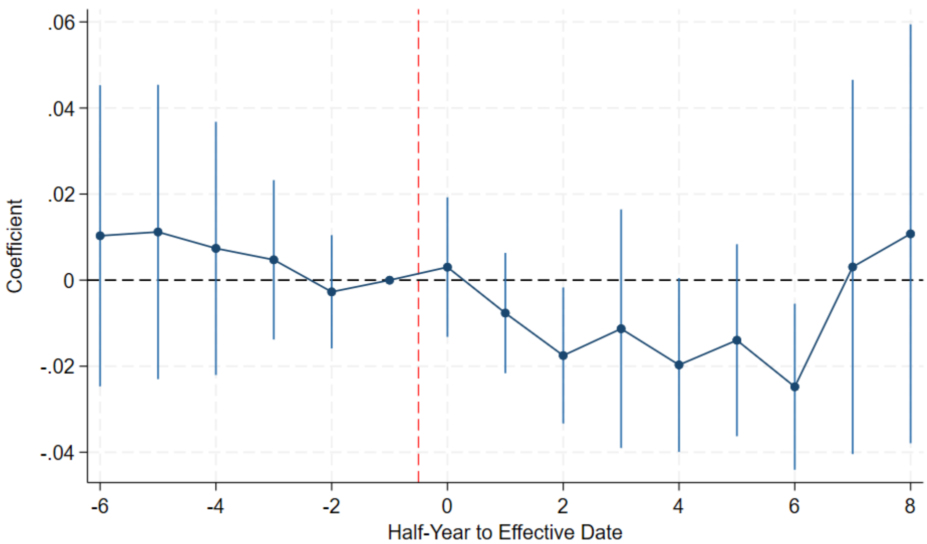
Notes: The unit of observation is a city-half-year. The plotted stacked difference-in-differences estimates include $sub - experiment \times city$ and $sub - experiment \times event - time$ fixed effects and time-varying covariates (as described in Equation 1). Standard errors are clustered by city. 95 percent confidence intervals are shown. Outcome data are collected from Fatality Analysis Reporting System (FARS). Controls include logged population and the unemployment rate.

Figure 3: Dynamic effects of a restrictive policy on crime deterrence

(a) Logged motor vehicle theft

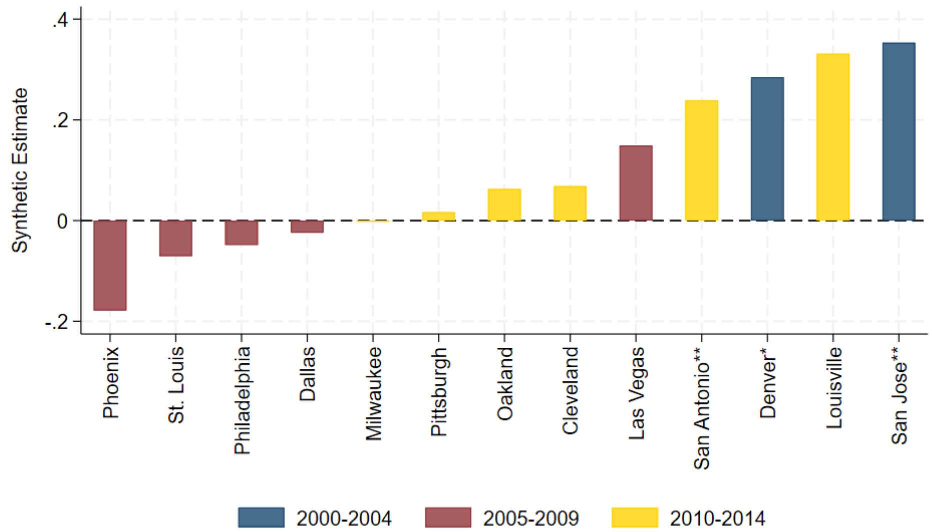


(b) Motor vehicle theft clearance rate



Notes: The unit of observation is a city-half-year. The plotted stacked difference-in-differences estimates include $sub - experiment \times city$ and $sub - experiment \times event - time$ fixed effects and time-varying covariates (as described in Equation 1). Standard errors are clustered by city. 95 percent confidence intervals are shown. Outcome data are collected from Uniform Crime Reporting (UCR) Program data. Controls include logged population and the unemployment rate.

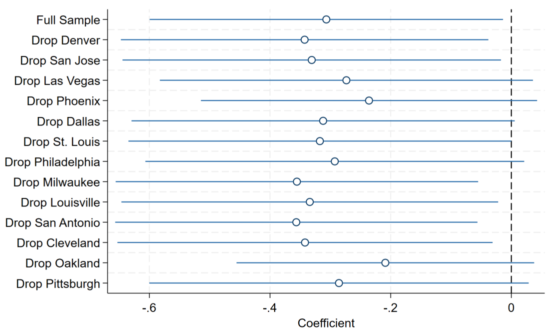
Figure 4: Synthetic control estimates of logged motor vehicle theft by city



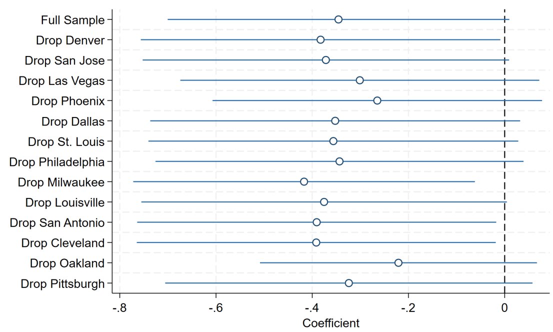
Notes: Each bar reflects the estimated average treatment effect on logged motor vehicle theft by city, calculated by the difference of the realized outcome and its counterfactual created by synthetic control methods. Blue bars reflect a city enacted its policy between 2000 and 2004, red bars reflect a policy enactment between 2005 and 2009, and yellow bars reflect a policy enactment between 2010 and 2014. Outcome data are collected from the UCR. Statistical significance is determined by the city's RMSPE ratio. * RMSPE ratio < 0.1, ** RMSPE ratio < 0.05, *** RMSPE ratio < 0.01.

Figure 5: Leave-one-out analysis by primary outcome

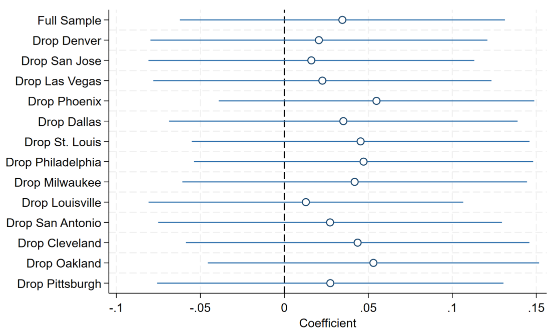
(a) Fatal police chase accidents



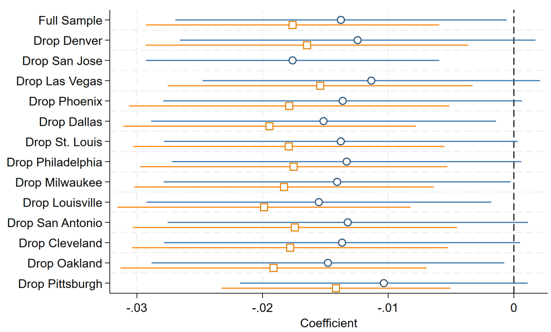
(b) Police chase fatalities



(c) Logged MVT



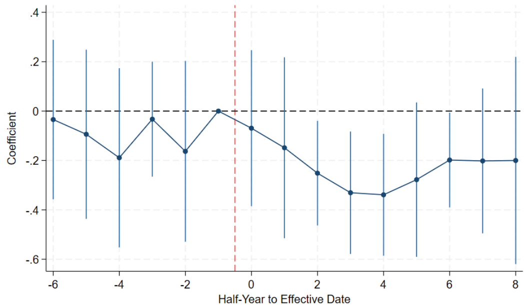
(d) MVT clearance rate



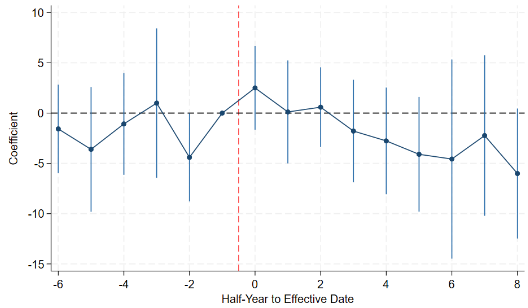
Notes: The unit of observation is a city-half-year. Fatal police chase accidents and police chase fatalities are sourced from FARS. Motor vehicle theft data are collected from the UCR. The orange squares and confidence intervals in s (d) reflect the leave-one-out exercise within a sample already excluding San Jose.

Figure 6: Dynamic effects on additional outcomes

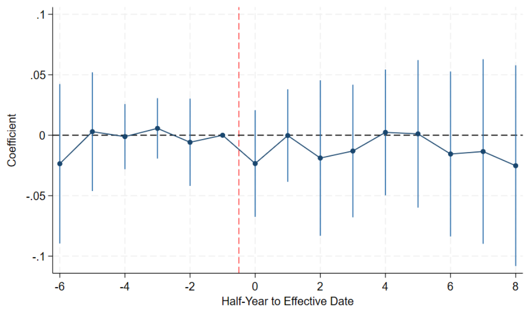
(a) Binary fatal accident



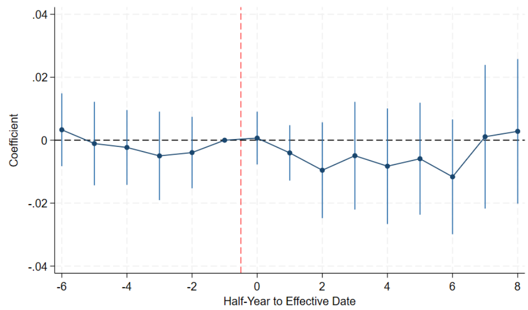
(b) Total traffic fatalities



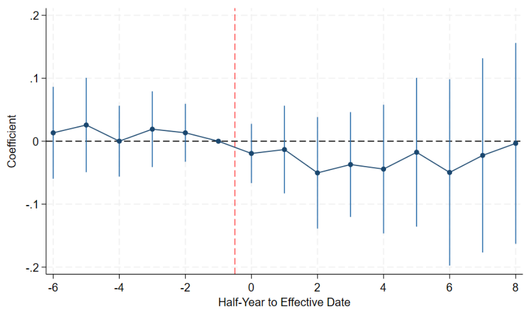
(c) Property crime index



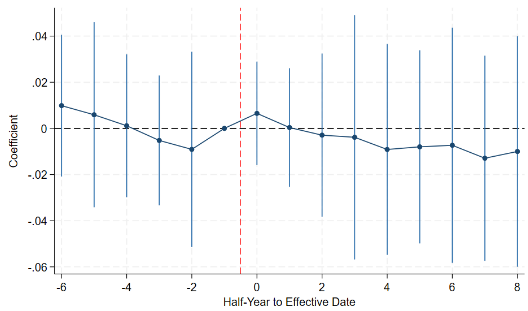
(d) Property crime clearance rate



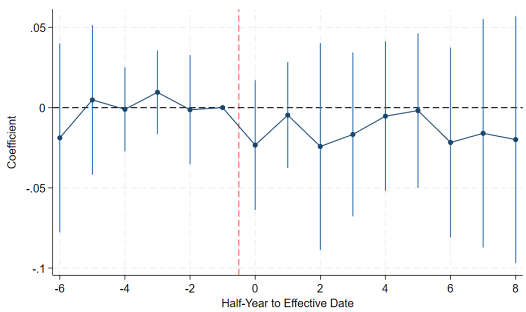
(e) Violent crime index



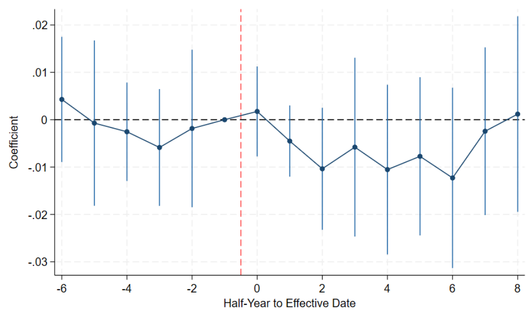
(f) Violent crime clearance rate



(g) Total crime index

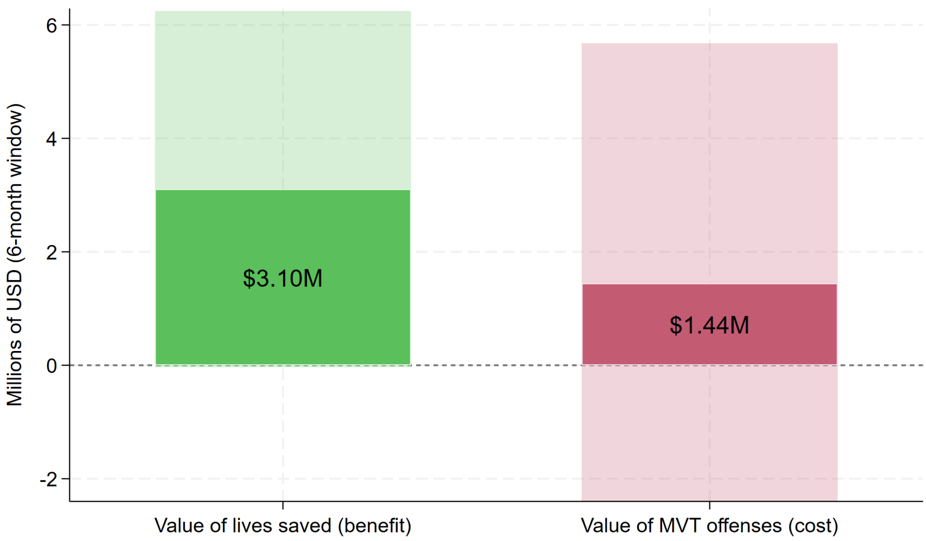


(h) Total crime clearance rate



Notes: The unit of observation is a city-half-year. See notes from Figure 3 for model specification. Crime data are collected from the UCR. Controls include population and unemployment rate. All indexes are logged.

Figure 7: Back-of-the-envelope welfare effects



Notes: The green bar reflects the estimated benefit of the policy through the value of lives saved, with the lighter shade reflecting the 95% confidence intervals. The red bar provides the estimated cost of the policy through increase in motor vehicle theft, with the 95% confidence interval shown in the lighter red shade. The benefit is calculated as the estimated reduction in police chase fatalities multiplied by the value of a statistical life ($0.345 \times \$9{,}000{,}000$) in a city, six-month interval. The cost is calculated as the estimated increase in motor vehicle theft in a city, six-month interval times the social cost of motor vehicle theft ($700 \times 6 \times (100\text{-}9.7)\% \times 3.5\% \times \$10{,}722$)

Table 1: Summary statistics of treated and control groups

	Treated		Control	
	Pre-Period	Post-Period	Pre-Period	Post-Period
Fatal police chase accidents	0.744	0.453	0.348	0.355
	(0.973)	(0.701)	(0.711)	(0.719)
Police chase fatalities	0.885	0.564	0.393	0.405
	(1.227)	(0.913)	(0.840)	(0.848)
MVT	700.11	609.45	321.02	289.83
	(555.34)	(451.41)	(416.64)	(381.64)
MVT clearance rate	0.133	0.117	0.109	0.107
	(0.106)	(0.104)	(0.070)	(0.070)
Population	826,934	872,683	509,137	526,452
	(429,934)	(470,310)	(663,470)	(680,775)
Officers	2,087	2,204	1,150	1,157
	(1,499)	(1,505)	(1,718)	(1,730)
Unemployment rate	7.008	6.799	7.397	7.002
	(2.245)	(2.476)	(3.071)	(2.959)
N	78	117	2916	4374

Notes: The unit of observation is a city-half-year. The dataset consists of the appended sub-experiments by enactment time. There are a total of 13 treated cities across 13 sub-experiments, and there is an average of 37.38 control cities per sub-experiment. A city-half-year unit may appear several times depending on its sub-experiment and event-time. Following Wing et al. (2024), each observation in a sub-experiment is assigned an event-time, which allows for sample means to be calculated for control cities before a policy is enacted. Police chase accident data are from the Fatality Analysis Reporting System. Motor vehicle theft and population are collected from Uniform Crime Reporting Program: Offenses Known and Clearances by Arrest, and Uniform Crime Reporting Program: Property Stolen and Recovered. Employed officer and population counts are collected from Uniform Crime Reporting Program: Law Enforcement Officers Killed and Assaulted. Unemployment rate data are collected from the Bureau of Labor Statistics.

Table 2: Estimates of the effect of restrictive pursuit policies on primary outcomes

	(1)	(2)	(3)	(4)
Fatal police chase accidents	-0.298*	-0.306**	-0.298*	-0.307**
	(0.151)	(0.146)	(0.151)	(0.146)
Police chase fatalities	-0.333*	-0.344*	-0.334*	-0.345*
	(0.181)	(0.177)	(0.181)	(0.177)
Logged MVT	0.041	0.036	0.041	0.035
	(0.049)	(0.049)	(0.049)	(0.049)
MVT clearance rate	-0.014**	-0.014**	-0.014**	-0.014**
	(0.007)	(0.007)	(0.007)	(0.007)
Controls	N	Y	N	Y
Weighting Procedure	N	N	Y	Y
Observations	7485	7485	7485	7485

Notes: The unit of observation is a city-half-year. Standard errors are in parentheses, clustered by city. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dataset comprises of the appended sub-experiments mentioned in Table 1. Controls include population and the unemployment rate. Columns (3) and (4) use the weighting procedure described in Wing et al. (2024).

Table 3: Estimates of the effect of restrictive pursuit policies on alternative outcomes

	(1)	(2)
A: Alternative outcomes		
Any fatal police chase accident	-0.134*	-0.139*
	(0.076)	(0.074)
Fatal traffic accidents	-0.494	-0.417
	(1.873)	(1.975)
B: Crime indexes		
Property crime	-0.006	-0.008
	(0.026)	(0.023)
Property crime clearance rate	-0.002	-0.003
	(0.007)	(0.007)
Violent crime	-0.040	-0.041
	(0.054)	(0.053)
Violent crime clearance rate	-0.006	-0.006
	(0.021)	(0.021)
Total crime	-0.012	-0.014
	(0.024)	(0.021)
Total crime clearance rate	-0.004	-0.005
	(0.007)	(0.007)
Controls	N	Y
Observations	7485	7485

Notes: The unit of observation is a city-half-year. Standard errors are in parentheses, clustered by city. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dataset comprises of the appended sub-experiments mentioned in Table 1. Controls include population and the unemployment rate. All output uses the weighting procedure proposed in Wing et al. (2024).

Appendix A Data Appendix

A.1 Uniform Crime Reporting Program

Data from the Uniform Crime Reporting program require thorough data cleansing to account for reporting errors or quirks by agencies. To spot outliers in my dataset, I regress the monthly motor vehicle theft for a city on a quadratic time trend. An observation was flagged if it experienced either a greater than 100% increase compared to its predicted value or a greater than 75% decrease relative to its predicted value. This helped identify both outliers compared to previous months and departments in a stage between reporting and not-reporting (or vice versa). I visually inspected all flagged observations, compared them to the rest of the city time series, and replaced any flagged values as missing if deemed to be a keypunch error. Other flags arise from cities that infrequently reported and had random months with observations. For the departments that routinely switched between reporting and not reporting, I replaced all observations to missing until the city consistently reported. Given I analyze large metropolitan areas, I changed any observations of zero monthly offenses equal to missing.

Instead of imputing missing values, I condensed my analysis to the average monthly values for motor vehicle theft within a half-year. For example, if two months were missing within a six-month window, the average was calculated using the four observed months without imputing the missing values. Additionally, in my difference-in-difference analysis, I forced a balanced sample for each sub-experiment. Therefore, if a city had unreliable data for a half-year, it would be excluded from any analysis within the sub-experiment 16 half-years window.

Problems also may arise from the UCR's hierarchy rule of crime classification under cases of multiple offenses. From the UCR Handbook, a reporting agency "must locate the offense that is highest on the hierarchy list and score that offense involved and not the other offense(s) in the multiple-offense situation."[16] Property crime such as motor vehicle theft may be underreported as a result.

[16] More of the UCR reporting quirks and Hierarchy Rule can be read here: `https://ucr.fbi.gov/additional-ucr-publications/ucr_handbook.pdf`

A.2 Updated Policies

To my knowledge, no longitudinal database of police department vehicular pursuit policies currently exists. Therefore, I collected policy data across cities independently. The starting sample consisted of the 200 most populous cities that reported to the UCR in 2019, screened further for cities with consistent crime reporting and available policy data, as described below. I developed a policy database that codes whether the pursuit policy of the city's police department includes a violent felony threshold for initiating a pursuit. This margin is emphasized in professional guidance (Police Executive Research Forum, 2023) and provides a discrete threshold that is comparable across cities for distinguishing which offenses permit a pursuit.

Department pursuit policies and adoption dates were identified through a multi-step process. Initial searches used search engines and Google News to locate news articles, policy announcements, and other online documentation. News coverage was prioritized when available as it often corroborated implementation timing and sometimes documented both prior and updated policy language. When news coverage was insufficient, policy language was gathered from police department manuals and, when available, historic versions of those manuals. I used FOIA requests to uncover previous manuals and to identify when certain language, such as a violent felony clause, was added to the policy. Finally, I used department reports to corroborate policy timing and, in one case, to infer implementation when no dated policy document could be located. When only the current manual was available and it described a discretionary policy, I treated the city as never having adopted a restrictive policy unless I found evidence to the contrary. For a handful of cities, I observe adoption of a violent felony threshold after the sample period but cannot locate earlier policy language. In these cases, I classify the prior policy as less restrictive.

For most adopting cities, there is limited documentation of the prior policy. I therefore provide three illustrative examples of pre-adoption pursuit guidelines for treated cities used in the analysis. Two years prior to their policy enactment, the Las Vegas Metropolitan Police Department's policy was described as "broad and discretionary" but called for supervisor guidance in ongoing pursuits (Las Vegas Sun, 2003). News coverage from Milwaukee highlights that officers could chase using their discretion prior to their restrictive policy adoption. Specifically, officers were permitted to pursue based on the "danger to the community represented by continuing to attempt to stop the vehicle versus the danger represented by the uncaptured fleeing person," as stated by the Milwaukee Police Chief (Milwaukee Journal Sentinel, 2010). Prior to their policy adoption, the Louisville Police Department allowed chases for any felonies (Louisville Metro Police Department, 2012).

The pursuit policies of police departments generally fall under four categories: discretionary (any crime allowed), permitted (any crime allowed but supervisor approval needed), restrictive (certain crime threshold needed), and prohibitive (no pursuits allowed) (International Association of Chiefs of Police, 2019). Therefore, control cities may still have varying degrees of restrictiveness. I found most cities without a violent felony clause in their vehicular pursuit policy, who I thus classified as untreated, predominately have discretionary pursuit policies. For example, among control cities used in the last sub-experiment of the sample, 21 had a discretionary policy, 8 allowed pursuits for any crimes above traffic infractions, 4 were "later-adopting" cities in which I am unsure of the prior policy, and 1 had a permitted policy. Additionally, 3 cities with discretionary policies reference there must be "imminent danger" for a pursuit to be initiated.

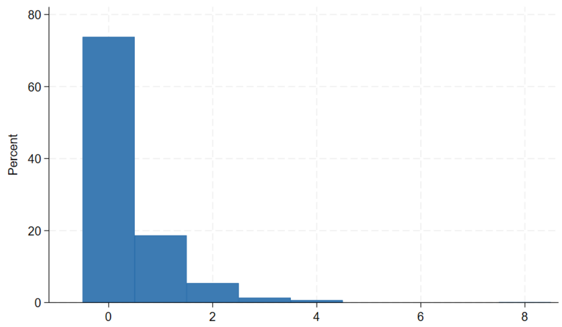
The sample is not encompassing of all cities with restrictive pursuit policies within the time frame of the analysis. Data limitations from inconsistent UCR reporting did not allow examination of policies adopted by New Orleans, LA; Portland, OR; and Tampa, FL. The following cities had unclear effective dates: Boston, MA; Buffalo, NY; Detroit, MI; Jackson, MS; Kansas City, MO; Lexington, KY; Memphis, TN. In cities such as Lexington and Memphis, only state residents can initiate public records request for previous versions of the policing manual. There are some cities that adopted prior to the data sample, including Columbus, OH; Durham, NC; San Francisco, CA; Salt Lake City, UT; and Tucson, AZ. A few adopting cities are trimmed for having too few post-periods in the stacked analysis (Little Rock, AR, 2017; Madison, WA, 2017). Baltimore (adopted June 2015) is excluded from the baseline analysis because the adoption timing coincides with citywide civil unrest in April–May 2015 following the death of Freddie Gray, which may raise concerns about confounding shocks with treatment.[17]

[17] Including Baltimore yields the following estimates (standard errors) using the main specification: fatal chase accidents: −0.265 (0.142); police chase fatalities: −0.304 (0.170); logged motor vehicle theft: 0.041 (0.046); motor vehicle theft clearance rate: -0.015 (0.060).

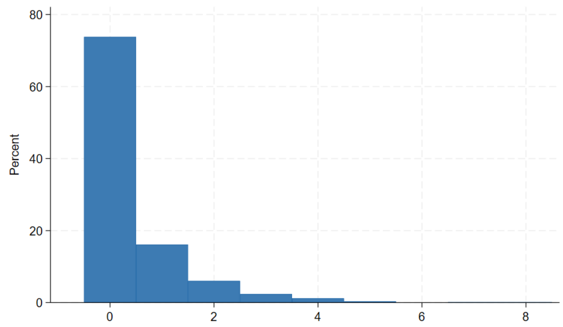
Appendix B Appendix Figures and Tables

Figure B1: The distribution of primary outcomes

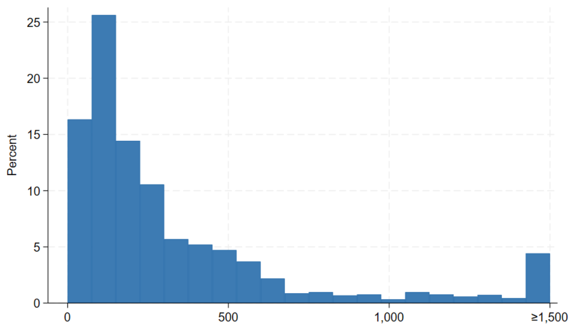
(a) Fatal police chase accidents



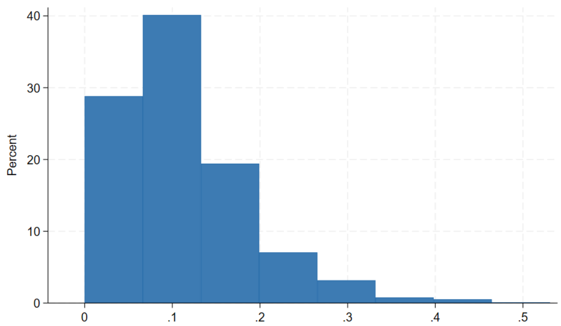
(b) Police chase fatalities



(c) Motor vehicle theft



(d) Motor vehicle theft clearance rate



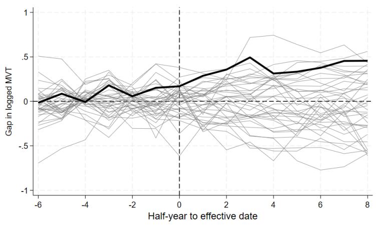
Notes: Unit of observation is a city-half-year. Fatal police chase accidents and police chase fatalities are sourced from the Fatality Analysis Reporting System (FARS). Motor vehicle theft data are collected from Uniform Crime Reporting (UCR) Program data. The histograms are created from the "stacked" dataset and exclude observations outside of the stacked window. Units that show up multiple times throughout the dataset are only counted once. Motor vehicle theft and motor vehicle theft clearance rate contain no zero values.

Figure B2: Time series of motor vehicle theft

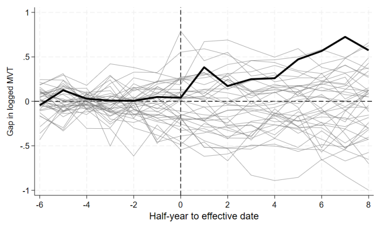
Notes: The plot of data is the average motor vehicle theft for never-treated cities in a six-month period from 1995 to 2019. Data are collected and cleaned from the Uniform Crime Reporting (UCR) Program data.

Figure B3: Synthetic control results by city

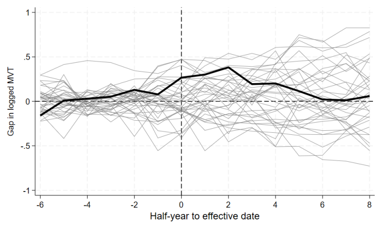
(a) Denver



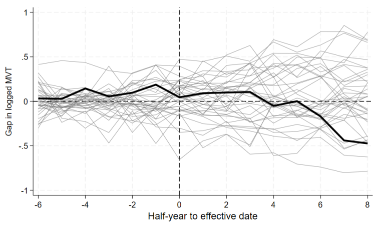
(b) San Jose



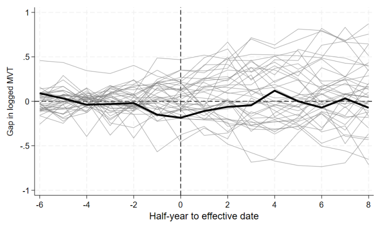
(c) Las Vegas



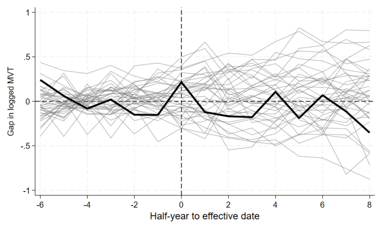
(d) Phoenix



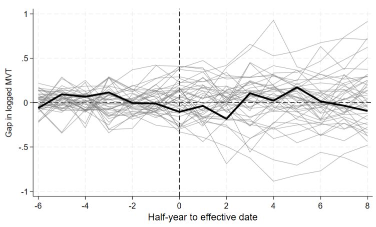
(e) Dallas



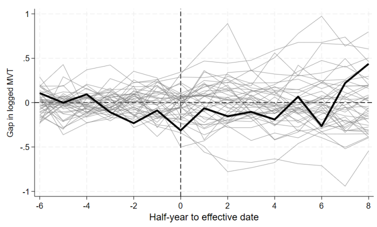
(f) St. Louis



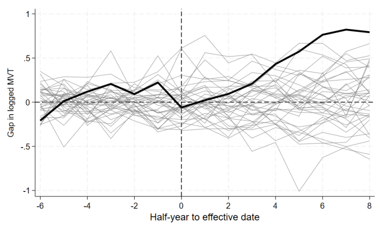
(g) Philadelphia



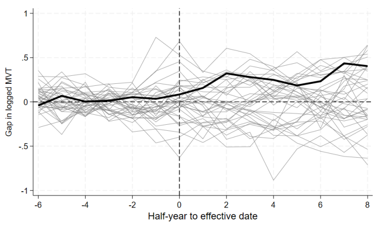
(h) Milwaukee



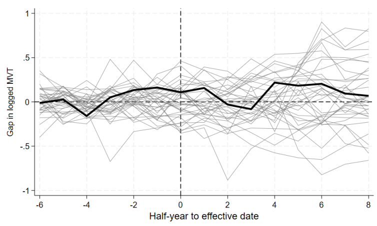
(i) Louisville



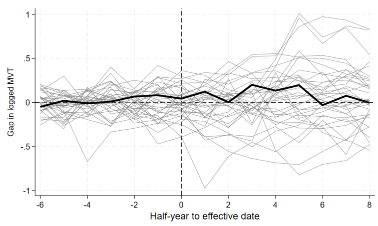
(j) San Antonio



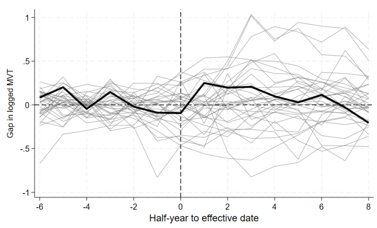
(k) Cleveland



(l) Oakland



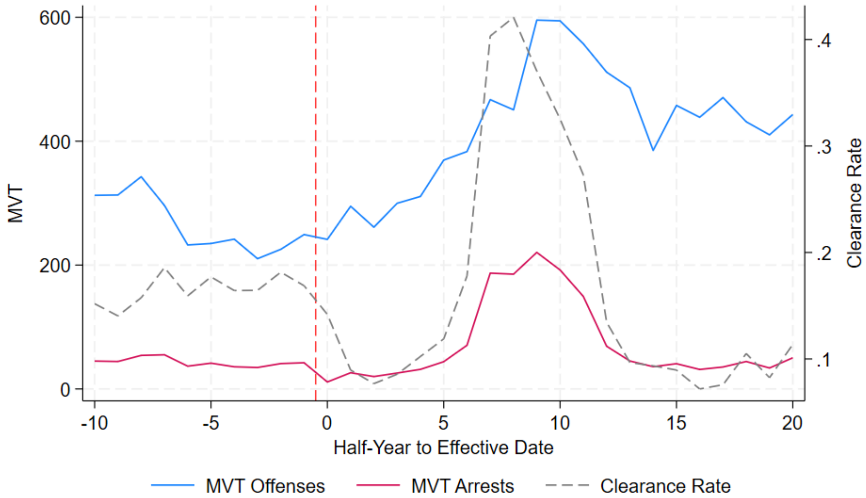
(m) Pittsburgh



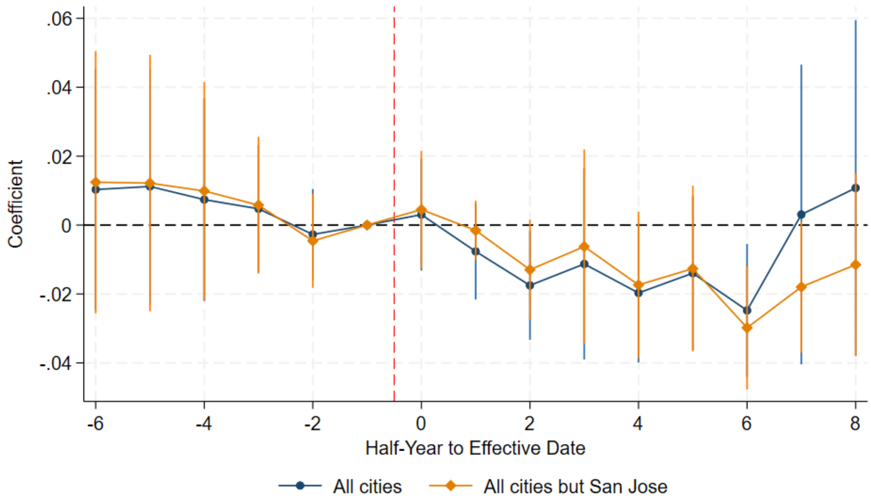
Notes: The unit of observation is a city-half-year. Motor vehicle theft data are collected from Uniform Crime Reporting program data. Each graph presents the difference between the realized outcome and the counterfactual outcome for the treated city, represented by the thick black line, and for donor pool cities, represented by the light gray lines.

Figure B4: San Jose case study

(a) MVT offenses and arrests trends in San Jose



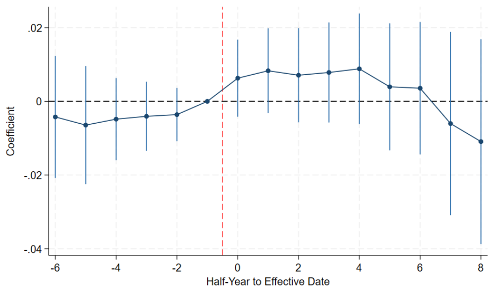
(b) Dynamic effects on motor vehicle theft clearance rate



Notes: Data are collected from the UCR. In panel (a), the blue line denotes the average monthly motor vehicle theft *offenses* within event-time for San Jose, and the red line denotes the average monthly motor vehicle theft *arrests*. The gray dashed line is the clearance rate for motor vehicle theft, defined as arrests divided by offenses. In panel (b), the blue circle coefficient corresponds to the whole sample, and the orange diamond coefficients correspond to the entire sample excluding San Jose. Both specifications include covariates and the Wing et al. (2024) weighting procedure.

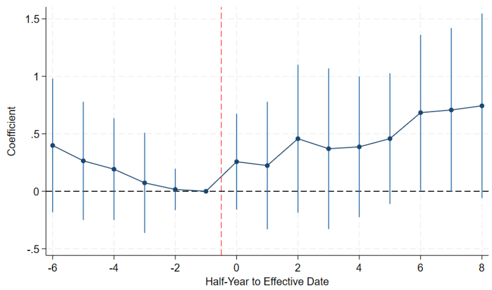
Figure B5: Dynamic effects on potential covariates

(a) Logged population



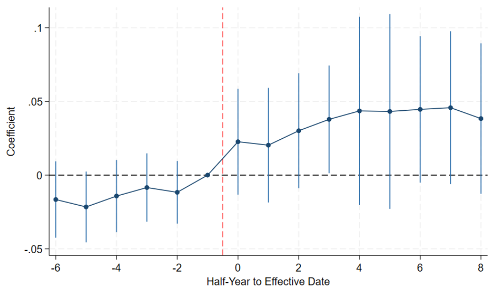
$\hat{\beta}\,(SE) = 0.007\,(0.010)$

(b) Unemployment rate



$\hat{\beta}\,(SE) = 0.320\,(0.381)$

(c) Logged officers



$\hat{\beta}\,(SE) = 0.048^{**}\,(0.020)$

Notes: Unit of observations are city-half-years. Population and officer data are from LEOKA, and unemployment data are from the BLS. Since an increase in officers may be endogenous to the policy adoption and crime outcomes, it is not used as a covariate throughout the main regression output presented in in Tables 2 and 3.

Table B1: Enactment dates and sources

City	Enactment	Sources (hyperlinks)
Denver	10/2000	News article; Police document
San Jose	09/2001	Police document
Las Vegas	01/2005*	Police documents (1, 2)
Phoenix	11/2005	News article; Police document
Dallas	06/2006	Police document
St. Louis	08/2006	FOIA request
Philadelphia	12/2008	Law office; Police document
Milwaukee	03/2010	News article; Police document
Louisville	12/2012	News article
San Antonio	05/2013	News article
Cleveland	03/2014	News article
Oakland	08/2014	News article; Police document
Pittsburgh	11/2015	News article

Notes: Enactment dates were pinpointed through news coverage surrounding the policy switch or available police documents. All sources in the third column are clickable hyperlinks.
*In the case of Las Vegas, no original date could be found but was backtracked through documents. Document 1 shows how the vehicular pursuit policy underwent three revisions in 2005, and how the number of pursuits decreased from 254 in 2005 to 50 in 2006. Document 2 provides the department's pursuit policy and historical data on which crimes officers pursued. Additionally, the police chief stated in December 2002: "We don't want to nail down our officers to anything specific because that would take away their discretionary power and impede their ability to do their jobs." (Las Vegas Sun). In a forum post in 2005, a Las Vegas Metro PD officer stated they had "currently implemented a restricted pursuit policy... whereas we will pursue violent felonies only." (Officer.com forum post). With this in mind, January 2005 was used as an imputed enactment date.

Table B2: Treated and control cities with enactment years

Treated Cities	**Enactment**	**Untreated Cities**	
Cleveland, OH	2014	Akron, OH	Madison, WI
Dallas, TX	2006	Anaheim, CA	Modesto, CA
Denver, CO	2000	Atlanta, GA	Nashville, TN
Las Vegas, NV	2005	Aurora, CO	Newark, NJ
Louisville, KY	2012	Bakersfield, CA	Norfolk, VA
Milwaukee, WI	2010	Baltimore, MD	Oklahoma City, OK
Oakland, CA	2014	Baton Rouge, LA	Portland, OR
Philadelphia, PA	2008	Chesapeake, VA	Providence, RI
Phoenix, AZ	2005	Chula Vista, CA	Richmond, VA
Pittsburgh, PA	2015	Cincinnati, OH	Riverside, CA
San Antonio, TX	2013	Columbus, GA	Sacramento, CA
San Jose, CA	2001	Des Moines, IA	San Bernardino, CA
St. Louis, MO	2006	Fort Wayne, IN	San Diego, CA
		Fresno, CA	Santa Ana, CA
		Greensboro, NC	Springfield, MA
		Honolulu, HI	Stockton, CA
		Houston, TX	Tacoma, WA
		Indianapolis, IN	Toledo, OH
		Knoxville, TN	Tulsa, OK
		Little Rock, AR	Wichita, KS
		Los Angeles, CA	Winston Salem, NC
		Lubbock, TX	

Notes: This table comprises the treated and untreated cities in the analytical sample. Some "untreated" cities may have adopted a restrictive policy near the end of the sample and are trimmed from a sub-experiment if partially treated within it. Due to too short of a post-period, their policy adoption is not analyzed.

Table B3: Summary statistics for additional outcomes

	Treated		Control	
	Pre-Period	Post-Period	Pre-Period	Post-Period
Any fatal police chase	0.474	0.342	0.251	0.253
	(0.503)	(0.476)	(0.434)	(0.435)
Total traffic fatalities	37.72	38.15	21.73	22.54
	(28.65)	(27.02)	(26.91)	(27.85)
Property crime	3,856	3,493	2,017	1,893
	(2,385)	(2,057)	(2,075)	(1,992)
Property crime clearance rate	0.144	0.137	0.146	0.142
	(0.068)	(0.067)	(0.056)	(0.060)
Violent crime	686.42	655.19	380.14	371.71
	(436.45)	(375.56)	(551.25)	(505.41)
Violent crime clearance rate	0.390	0.372	0.416	0.405
	(0.147)	(0.134)	(0.123)	(0.119)
Total crime	4,542	4,148	2,397	2,265
	(2,688)	(2,343)	(2,591)	(2,470)
Total crime clearance rate	0.183	0.176	0.184	0.181
	(0.075)	(0.074)	(0.058)	(0.060)
N	72	132	2610	4785

Notes: The unit of observation is a city-half-year. The dataset consists of the appended sub-experiments by enactment time. There are a total of 13 treated cities across 13 sub-experiments, and there is an average of 37.38 control cities per sub-experiment.