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The Effect of Redeploying Police Officers from Plain Clothes Special Assignments to Uniformed Foot-Beat Patrols on Street Crime

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The Effect of Redeploying
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Foot-Beat Patrols on Street
Crime

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Executive Summary

We evaluate the effect on reported daily criminal incidents of a sizable reallocation of police officers from plain clothes special-task force assignments to uniformed foot patrol. On September 1st, 2017, the San Francisco Police Department (SFPD) re-assigned 69 officers (roughly 3.5 percent of sworn officers in the department) to various foot patrol assignments across the city's ten police districts. We use microlevel data on criminal incidents to generate daily counts of crime by broad category for the ten most frequently reported offenses (accounting for over 90 percent of incidents reported to the police) for the 120-day period surrounding the September 1st policy change. We conduct an event study analysis to test for a discrete change in the daily level of criminal incidents coinciding in time with the reallocation of police officers. We document discrete and statistically significant declines in the daily number of larceny thefts and assaults reported to the police coinciding with the increase in the number of officers assigned to foot beats. We show that the observed declines are not evident for comparable time periods in earlier years. The decline in larceny theft is geographically broad-based across police districts within the city while the decline in assaults is concentrated in a few districts. We do not observe larger crime declines (either in absolute terms or proportional to pre-change crime levels) in districts that experienced greater increases in foot-beat assignments.

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Introduction

A key challenge faced by local police departments concerns how to optimally allocate officers across localities and assignments with an eye on minimizing street crime and the social costs of the public response to street crime (in terms of both budgetary outlays as well as the effect of policing on community relations). For example, police chiefs must decide how many officers should be assigned to patrol as opposed to specialized crime-fighting units, how policing intensity will vary across different neighborhoods in a city, the degree to which officers engage in proactive versus reactive policing, as well as the level of resources to be devoted to community engagement. Theoretically, the optimal deployment of various policing strategies would involve deploying officers and resources across alternative assignments in a manner that equalizes the benefit-cost ratio of each. For example, if the crime-mitigating benefits of devoting more officers to a particular community policing strategy relative to the budgetary and other social costs of the strategy exceed the comparable benefit-cost ratio for allocating another officer to patrol, reallocating officers from the latter assignment to the former would increase the social benefits from policing, holding expenditures constant.

To be sure, optimally allocating personnel across assignments requires information regarding the relative effectiveness of different interventions, assessment of budgetary and social costs associated with different interventions, and ideally an assessment of whether and when the returns to a particular policing strategy begin to diminish and perhaps even become negative. Existing research may or may not be informative regarding the relative efficacy of alternative strategies due to either a lack of consensus among researchers or heterogeneity in the effects of various interventions in different settings. For example, the consensus among criminologists for several decades was that random patrol has little impact on crime or citizen sense of safety. This conclusion followed from the disappointing findings of the Kansas patrol experimental intervention evaluated during the 1970s (Kelling et al. 1972), and was interpreted by many as indicating that officers should devote most effort to reactive responses to calls for service. However, a recent consensus panel report produced by the National Academies of Sciences concluded that there is robust empirical evidence supporting the effectiveness of specific targeted proactive policing strategies, including hot-spot policing, some community policing strategies, and procedural justice interventions (National Academies of Sciences 2018). As a further example, the relative effectiveness of a particular intervention may differ in a setting such as New York City where there is a high degree of pedestrian traffic and interpersonal daily interactions among strangers as opposed to Dallas where daily commuting by auto is proportionally more prevalent. In many instances, departments must experiment with new initiatives and make some attempt to evaluate the impacts of changes in resource deployment strategies.

This paper evaluates the effect of a sizable reallocation of police officers from plain clothes special-task force assignments to uniformed foot patrol on reported daily criminal incidents. On September 1, 2017, the San Francisco Police Department (SFPD) re-assigned 69 officers (roughly 3.5 percent of full-duty sworn officers in the department) to various foot patrol assignments across the city's ten police districts. We use microlevel data on criminal incidents to generate daily counts of crime by broad category for the ten most frequently reported offenses (accounting for over 90 percent of incidents reported to the police) for the 120-day period surrounding the September 1st policy increasing the presence of police officers in the street discontinuously. Following Dominguez

(2017), we test for a discrete change in the daily level of criminal incidents coinciding in time with the reallocation of police officers. We test for citywide and district-level effects associated with the policy change.

We document discrete and statistically significant declines in the daily number of larceny thefts and assaults reported to the police coinciding with the increase in the number of officers assigned to foot beats. Relative to crime levels during the 60-day period preceding the policy change, larceny thefts decline by approximately 16.9 percent while assault incidents decline by 19.1 percent. To assess whether the observed change reflects a seasonal crime pattern associated with changes in employment, tourism, or some other coincident factor, we estimate a series of placebo treatment effects for the fourteen years between 2003 and 2016, focusing on the same time period (two months before and two months after September 1st). As there were no comparable policy changes on September 1st of these years, we would not expect to see comparable effects on crime. In other words, to the extent that the observed effects in 2017 reflect a true effect of the intervention, the estimates for 2017 should be outliers relative to the distribution of placebo estimates for the years 2003 through 2016. Indeed, we find that that the declines in larceny theft and assault coinciding with the reassignment of officers in 2017 are clear outliers and more negative than any of the comparable estimates for the earlier years.

We test for geographic heterogeneity in treatment effects across police districts. The declines in larceny theft are fairly broad based, with independently significant effects in several police districts, and point estimates that are generally negative in each. We do not observe larger declines (either in absolute terms or proportional to pre-change crime levels) in districts that experienced greater increases in foot-beat assignments. The observed change in assaults appears to be concentrated in a few districts.

Literature Review

There are several avenues through which a reallocation of officers across assignments may impact crime rates. For the intervention we study, there is a visible increase in the presence of uniformed police officers. To the extent that potential offenses are deterred by a visible police presence, the increase in uniformed police officers may lower street crime in the areas with greater police visibility. Of course, it is theoretically plausible that criminal activity may simply be displaced to other areas of the city. To the extent that potential offenders are making rational choices to commit offenses for personal gain as proposed by the rational choice model of Gary Becker (1968), a spatially concentrated increase in uniformed officers may simply displace criminal activity to other parts of the city. On the other hand, if criminal offending in part reflects reflexive and not particularly premeditated responses to the availability and salience of criminal opportunities in the manner hypothesized by Ronald V. Clarke (1980), a visible police presence may deter criminal offending with little displacement to other areas.

If the increase in uniformed officers is offset by a decline in alternative types of enforcement (for example, fewer officers deployed to undercover special operations), the net effect on crime is theoretically ambiguous. Moreover, the gains associated with a reduction in one type of offense may be offset by increases in other crime rates. For example, uniformed officers on foot beats may be particularly effective in deterring theft from autos. However, special operations may be better at

disrupting motor vehicle theft, or organized shop-lifting rings. Reallocation of officers from one assignment to another thus may cause offsetting changes in different crime categories.

Over the past two decades, a growing body of empirical studies have analyzed the effects of police staffing levels on crime and in some instances, the effectiveness of specific deployment strategies. The new research addresses the methodological challenges associated with poor data quality and the uncertainty about the underlying causal process that generate non-experimental data on police and crime. The findings suggest that an expansion of police resources in a particular jurisdiction tends to reduce crime rates, and that such expansions often pass a cost-benefit test (Heaton 2010). Moreover, the research suggests that focused proactive efforts to address specific crime problems is often effective (National Academies of Sciences 2018).

Chalfin and McCrary (2018) estimate the effect of changes in city-level police staffing levels on crime rates using panel data for the period 1960 through 2010. The paper addresses the attenuation bias that occurs in panel data estimates due to measurement error in police staffing levels, making a strong case that bias helps explain why previous correlation-based research studies tend to find a null or even positive effect of police on crime. The authors make the case that the Uniform Crime Reports data on police are subject to considerable measurement error by documenting the large year-to-year changes in measured staffing levels in key cities throughout the United States. They then utilize a second source of annual staffing level counts (the Annual Survey of Governments) to estimate panel data crime models where the two independent measures of policing levels are used as instruments for one another. Accounting for attenuation bias leads to considerably larger (negative) effects of increases in police staffing levels on crime rates, with sizable and significant effects of the police in reducing homicide, robbery, burglary, and motor vehicle theft.

The authors also provide an innovative cost-benefit calculation of policing that is based on the average effect of additional police on crime rates, concluding that the benefit cost ratio of hiring additional police exceeds one so long as the willingness to pay to avoid a homicide among the general public exceeds four million dollars. Employing a value-of-statistical life estimate of seven million dollars, the authors estimate that each dollar spend on policing reduces the costs of criminal victimization by \$1.63. Based on these findings, the authors conclude that many cities in the United States are under-policed.

There are a number of other studies that address a key methodological strategy to studying the police-crime relationship. To be specific, increases in crime may lead to an increase in police staffing levels to the extent that local authorities respond to public demands for public safety. The practical implications of this bidirectional causality are that simple correlations or partial correlations measured with basic regression analysis are insufficient to uncover the true effects of higher policing levels on crime rates.

For example, suppose that crime increases due to a new drug introduced in a local community. In response to this change, the community may hire additional police to address the crime uptick. If the additional police only partially address the problem, what we would observe empirically is a coincident increase in crime and police staffing levels, creating the false impression that the police are increasing crime rates. If the additional police reduce crime to what it was before, what we would observe empirically is an increase in police staffing corresponding to no change in crime (creating the false impression that police have no effect).

Much of the research on this question identifies clear sources of variation in policing that arguably have little to do with crime trends. These “quasi-experiments” provide an empirical basis for estimating the effect of exogenous changes in policing on crime rates. Among the most prominent is Evans and Owens (2007), who utilize city-level panel data for the period 1990 to 2001 to estimate the effect of changes in police staffing levels on crime rates that are induced by the receipt of federal grants subsidizing the hiring of additional police officers. The COPS program under the 1994 Violent Crime Control and Law Enforcement Act enacted a federal inter-governmental grant program aimed at hiring an additional 10,000 police officers across the United States. Evans and Owens document an effect of receiving the grant on new hires and demonstrate that receiving an award was independent of local crime rates. The study found that the hiring of new police officers generated statistically significant reductions in robbery, aggravated assault, auto theft and burglary. The authors also found a marginally significant (through imprecisely measured) effect on murder rates. They concluded that the monetized value of estimated crime reduction exceeded budgetary outlays for the new officers.

Machin and Marie (2011) find similar evidence for the UK. The Street Crime Initiative awarded resources to 10 of 43 police districts across England and Wales amounting to 24 million pounds per year in fiscal years 2003 and 2004. The authors find a significant decline in robbery, the monetary benefits of which outweighed the additional police expenditures.

Several studies have exploited staffing changes that occur either in response to a terrorist attack or to an increase in the threat of a terrorist attack. Di Tella and Schargrotsky (2004) utilize an exogenous increase in police presence outside Jewish institutions in Buenos Aires, Argentina to estimate whether an increase in police presence deters auto theft. Following a 1994 terrorist attack on an Argentine Jewish center that killed 85 people and wounded 300, Argentina increased police presence to 24 hours per day outside all Jewish and Muslim institutions throughout the country. The authors analyze data on auto thefts for city blocks within three predominantly Jewish neighborhoods. They test for a differential effect of this change in policy on the immediate areas, adjacent areas, and nearby but more distant areas. Comparing monthly auto thefts before and after the terrorist attack, the authors document a sharp decline (on the order of 75 percent) in auto thefts for blocks containing a Jewish institution. There are no measurable effects (negative or positive) in neighboring blocks, or those that are two or more away.

Klick and Tabarrok (2005) exploit changes in the terror-alerts levels under the Homeland Security Advisor System established by the Department of Homeland Security in the wake of the September 11 terrorist attack. Analyzing a 500-day period in 2002 and 2003, the authors exploit the fact that during high-alert time periods the Washington D.C. police increase policing resources by roughly 50 percent. According to the authors, the temporary increase was achieved during these periods through extending the length of shifts from 8 to 12 hours. The authors compare average daily crime on days during high alert periods to other days, and find significant reductions in daily crime when the high alert system was activated. The effect survives controlling for proxy measures of tourist visits and occurs predominantly through reduction in property crime. The authors also show that the declines are largest in the district encompassing the White House, Congress, the Supreme Court, and the Washington Mall. The authors argue that there is reason to believe patrolling resources increase disproportionately in this particular district.

DeAngelo and Hansen (2014) address a somewhat different yet related question. Specifically, the authors assess the impact of changes in the staffing levels of state police on traffic fatalities and non-fatal traffic injuries. The authors exploit an exogenous shock to state funding for all agencies in Oregon associated with a lagged budgetary response to an initiative that greatly reduced revenue from property taxes in the state. State legislation enacting broad budgetary cuts across multiple agencies caused an abrupt change to the number of state troopers patrolling state highway and freeways. The authors demonstrate a notable increase in traffic fatalities, an increase in the spread between clocked speeds and speed limits for those cited, and an increase in non-fatal traffic related injuries. To isolate the effect of the staffing change on fatal and non-fatal injuries, the authors compare the changes observed in Oregon to three alternative non-experimental comparison groups: all other U.S. states, the states of Washington and Idaho, and a weighted average of states identified using the synthetic comparison estimator of Abadie, Diamond, and Hainmueller (2010). Relative to all comparison groups, the authors estimate statistically significant increases in traffic fatalities, non-fatal traffic injuries, and statistically significant reductions in staffing levels.

The paper goes on to perform a cost-benefit analysis of state police staffing levels, providing estimates of the number of additional fatalities associated with the staffing change and estimates of the hypothetical cost-per-life saved associated with increasing staffing levels to historical levels. The authors estimate that reducing fatalities via increased state police FTE would cost \$309,000 per life saved, an estimate far below conventional estimates of the value of a statistical life.

In addition to quasi-experimental research on the police-crime relationship, there is recent evidence that urban expenditures on private security have large crime-prevention effects. Cook and MacDonald (2011) study the effect of private security officers hired by business improvement districts (BID) in Los Angeles during the 1990s. The authors find a benefit-cost ratio of 20 to 1 in terms of the value of crime prevented and no evidence of displacement to other areas. The creation of a BID had no measureable effect on crime in the absence of security expenditures.

The recent National Academies of Sciences consensus panel study (2018) provides an extensive review of the effectiveness of specific policing strategies that often concentrate policing resources either in specific places or towards specific tasks. For example, the panel concludes that there is strong evidence that concentrating policing in very small hot spots (often a few city block faces) can have appreciable effects on local crime rates, as can strategies that focus on individuals at high risk of committing a crime or becoming a violent crime victim. The panel concluded that the research evidence pertaining to the broad application of stop, question and frisk strategies, or "broken-windows" (or zero-tolerance) style policing of less serious misdemeanor offenses is mixed at best, though there is evidence supporting the benefits from cleaning up trash-strewn lots and eliminating other indicators of disorder in high crime areas.

We are unaware of studies that directly test for an impact of redeploying plain clothes police to uniform patrols. Nonetheless, the existing research certainly suggests that under certain circumstances, visible increases in policing deter criminal activity. In the following section, we lay out our strategy for evaluating such a change occurring in San Francisco.

Estimation Strategy, Data, and Crime Characteristics in San Francisco

The City and County of San Francisco faces crime control challenges that are unique relative to other jurisdictions in the United States. First, the city's daytime population swells to roughly 125 percent of the resident population due to a large net inflow of commuters, a pattern unique among U.S. cities. Second, literally millions of tourists visit the city each year, many from surrounding counties, as well as tourists from other states, and other countries. Finally, daily life in San Francisco involves a high degree of pedestrian foot traffic in very densely populated areas of the city and a high proportion of individuals commuting to work via public transit. In recent years, the city has experienced pronounced increases in the volume of larceny theft, with the increases in thefts from autos and shoplifting being of particular concern. The shift in officer assignments that we study here was motivated in part by a desire to address the city's larceny theft problem.

In this section, we first describe the policy change and the magnitude of the officer reassignments. We then discuss our empirical strategy for evaluating whether the change impacted crime rates within the city. Finally, we present a descriptive analysis of crime trends in the city and characterize how measures of human activity within the city (growth in the resident population, employment, commuting, tourist visits) co-vary with recent crime trends.

3.1 Reassigning Officers to Uniformed Foot-Beat Patrols

In late August 2017, the captains of each of San Francisco's ten police stations received a memo instructing them to reassign a fixed number of officers to uniformed foot patrol. In total, the number of officers reassigned to uniformed foot beats increased by 69, a reallocation of roughly 3.5 percent of full-duty sworn employees. There was heterogeneity across districts in both the number of officers reassigned as well as the proportional increase of officers on foot patrol.

The increases in officers assigned to foot beats occurred in police districts where there is generally a high volume of larceny theft. **Table 1** displays the number of larceny thefts reported for the four-month period centered around September 1, 2017. In reading this table, consider the largest absolute increases in foot patrols occurs in the Central, Tenderloin, Mission, Southern, and Northern stations. Similarly, 72 percent of larceny thefts for the period that we study occur within these five police districts. While the increases in the remaining districts are smaller, the proportional increases are quite large, with some districts going from zero foot patrol officers to some positive number. It is our understanding that

Table 1: Larceny Theft Incidents by District for the Four-Month Period Surrounding September 1, 2017

District	Count	Percentage
Bayview	969	5.96
Central	2,924	18.00
Ingleside	651	4.01
Mission	1,872	11.52
Northern	2,797	17.21
Park	844	5.19
Richmond	1,111	6.84
Southern	3,578	22.02
Taraval	930	5.72
Tenderloin	572	3.52

most of the officers reassigned came from special task force assignments, many of which were plain clothes.

3.2 Estimation Strategy and Data Description

Our empirical strategy exploits the abrupt increase in officers assigned to foot beats to test for an impact of the change in assignments on daily criminal incidents. Specifically, we use incident-level data to generate daily criminal incident totals by crime category and test for a discrete break in crime trends coinciding with the change in assignment policy. We fit flexible trends to the pre- and post-policy change periods to permit flexibility in the underlying time paths of factors that may impact crime trends during the period of the year that we study (for example, changes in tourist visits, people returning to work from summer vacations, etc). We also propose a simple non-parametric test based on comparing the estimated break in trend in 2017 to a distribution of placebo estimates using similar timing but in years where there was no change in police assignments. We also assess whether the effects we see vary by police district.

Our estimation strategy is based on estimation of the following simple model. Define the variable $Crime_t$ as the count of daily crime incidents occurring on day t , where t measures day relative to September 1, 2017 (equal to zero on September 1, 2017). Define the variable $After_t$ as a dummy variable equal to one for observations where $t > -1$. Our principal results revolve around estimation of various specifications of the following equation:

$$Crime_t = \beta_0 + \beta_1 t + \beta_2 t^2 + \gamma_0 After_t + \gamma_1 After_t t + \gamma_2 After_t t^2 + \varepsilon_t \quad (1)$$

where β_0 , β_1 , β_2 , γ_0 , γ_1 , and γ_2 , are parameters to be estimated via ordinary least squares and ε_t is a mean zero error term. The specification of equation (1) basically fits a quadratic time trend to daily crime totals for the days preceding the policy change, a separate quadratic time trend to the time period following the policy change (i.e., days where $t > -1$), and an intercept shifter (given by the coefficient γ_0 pre-multiplying the variable $After_t$) that measures the discrete change in crime associated with the shift in officer assignments. The coefficient γ_0 , provides our key estimate of the effect of the reassignments on crime and is interpreted as the difference in crime in the days immediately following the policy change relative to what would have occurred absent the re-assignment of 69 additional officers to foot beats.

We estimate equation (1) separately for each of the ten crime categories that are most frequently reported to the SFPD for the city overall and for each of the ten police districts. The specific incident categories that we study include assault, burglary, drug/narcotic offenses, fraud, larceny, other offenses, robbery, suspicious occurrences, vandalism, and vehicle theft. Collectively, these ten offense categories account for approximately 92 percent of the criminal incidents reported to the SFPD. We focus on a relatively narrow four-month time window (the period from July 1, 2017 through October 31, 2017) to ensure that the specification of time trends is sufficiently flexible to model typical crime time trends during the late summer/early fall. We also explore the sensitivity of our estimates to varying the time window of analysis.

One issue with our estimation strategy concerns the possibility that daily crime levels may change systematically around September 1st in all calendar years for reasons that have nothing to do with

police deployment strategies. For example, tourist visits to the city may decline discretely as children return to school. Alternatively, workers on vacation leave may return en masse at the end of August as local children return to school. To rule out the possibility that any observed effect reflects time patterns that are commonly observed in other years, we propose the following non-parametric test. For each crime category, we estimate equation (1) for each year from 2003 through 2016, effectively testing for discrete changes in crime at September 1st in each year. Next, we pool these 14 placebo estimates with the estimate for 2017 and perform the following one and two-tailed test. First, define $\hat{\gamma}_0^i$ as the estimated change in daily crime occurring at September 1st in year i , where $i = (2003, \dots, 2017)$. Using the distribution of $\hat{\gamma}_0^i$ we next calculate the proportion of estimates with values equal to or less than the value of the estimate for 2017, or $P(\hat{\gamma}_0^i \leq \hat{\gamma}_0^{2017})$. We interpret this empirical probability as the p-value from a one-tailed test of the hypothesis that the 2017 estimate is an outlier relative to the distribution of placebo estimates.

The one-tailed test is basically an assessment of the degree to which the estimate for 2017 is in the bottom percentiles of the estimates distribution. An alternative more stringent test would assess the degree to which the 2017 estimate is an outlier based on its percentile in the distribution of the absolute value of the estimates distribution. Specifically, to the extent that $P(|\hat{\gamma}_0^i| \geq |\hat{\gamma}_0^{2017}|)$ is small, one would conclude that the estimate for 2017 represents an unusually large deviation from what is typical among both positive and negative breaks in trends. We interpret this estimated probability as the p-value from a two-tailed test of the hypothesis that the 2017 estimate is an outlier relative to the distribution of placebo estimates.

The data for this project are incident level crime data collected by SFPD and publicly posted at the Data SF website. The data includes incidents occurring from calendar years 2003 through mid-year 2018. Our analysis makes use of the incident data for the year 2003 through 2017. The data we use as well as the programs generating our empirical results are available upon request.

3.3 Crime in San Francisco: Trends and Likely Key Determinants

Before presenting our estimation results, we present a brief overview of crime trends in San Francisco and key sources of variation in human activity over time and space in the city that likely contributes to the volume of criminal activity. We begin by documenting monthly trends in reported criminal incidents by crime category. **Figure 1** shows monthly incident totals for each of the ten categories we study for the period from January 2003 through December 2017. The data reveal cyclical patterns for several of the offenses, suggesting little evidence of recent surges in criminal activity. For drug/narcotics offenses, recorded incidents actually decline from roughly 1,000 offenses per month in 2010 to roughly 300 or so incidents per month by the end of 2017. Larceny theft exhibits a pronounced increase in incident totals beginning in roughly 2010. From 2003 through 2010, monthly larceny thefts hover around 2,100 incidents per month. From 2010 through 2017 however, monthly incidents nearly double to over 4,000 incidents per month, with the increase occurring steadily over this time period.

Figure 2 presents several gauges of the volume of human interaction within the city that can be thought of as displaying variation over time in factors that likely contribute to the overall volume of crime. Between 2010 and 2017, we see the resident population of the city increase from

Figure 1: Monthly Criminal Incident Trends for the Ten Most Frequent Crime Categories

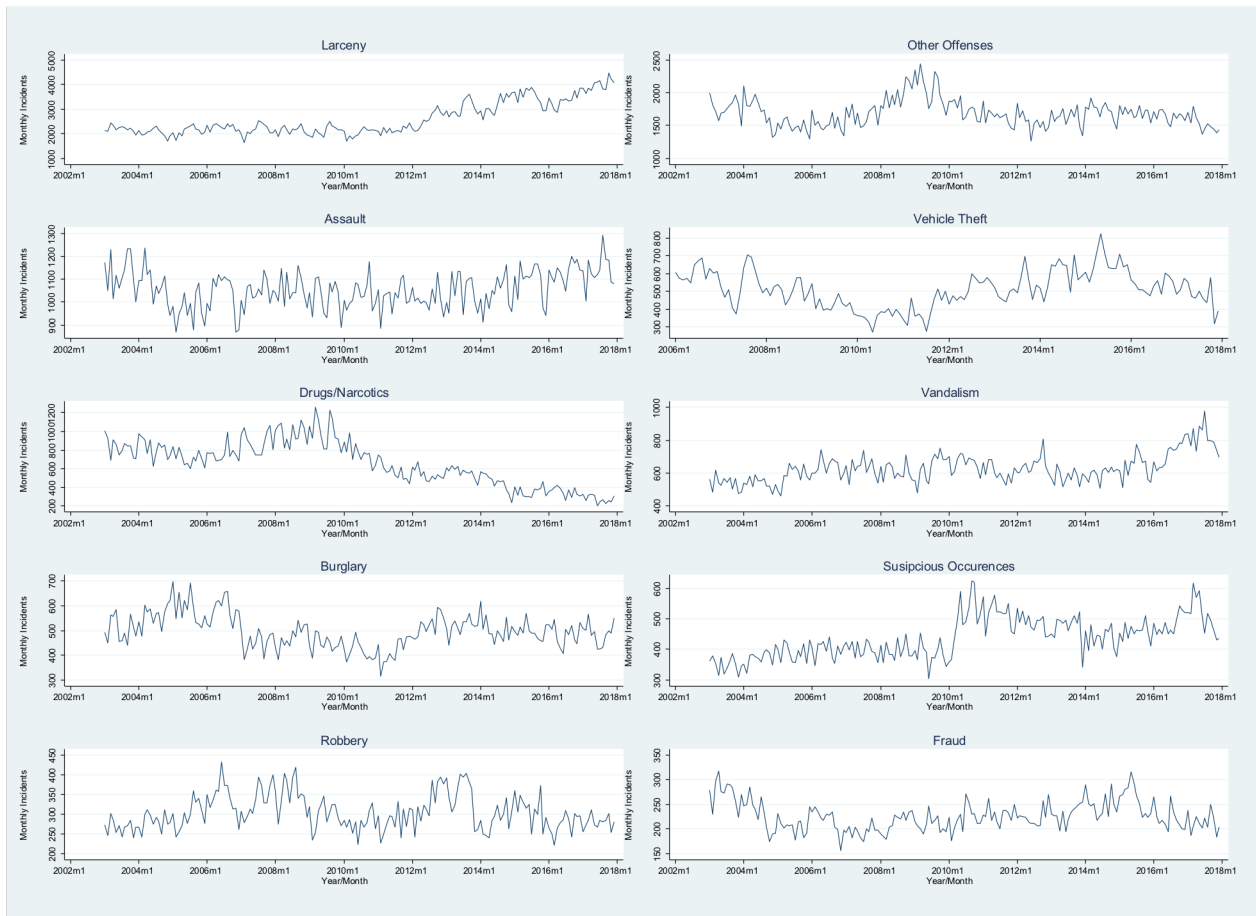
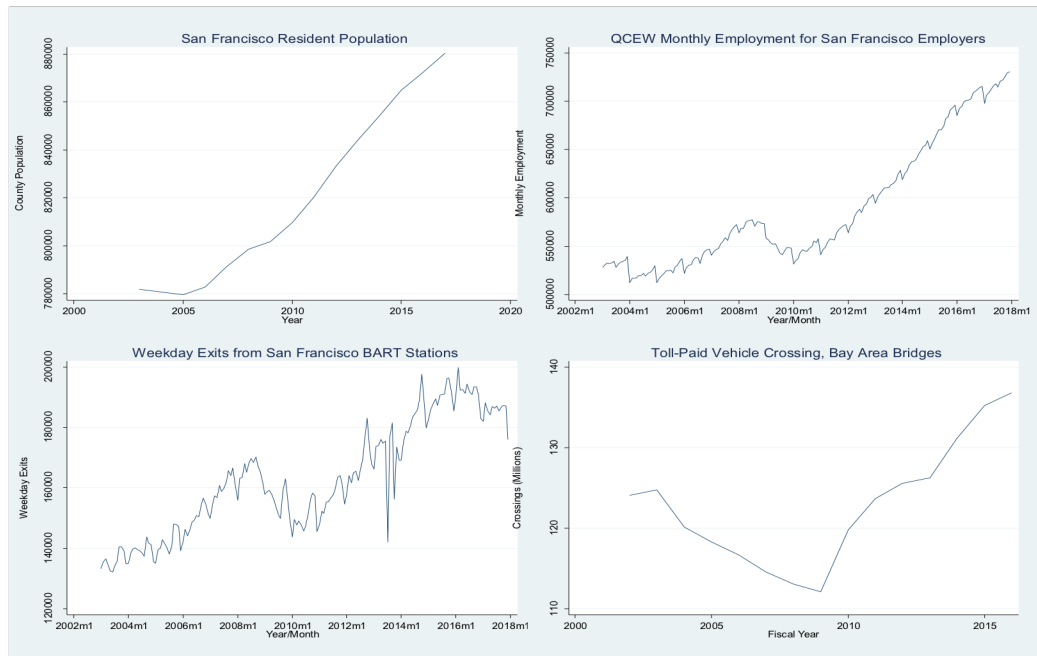


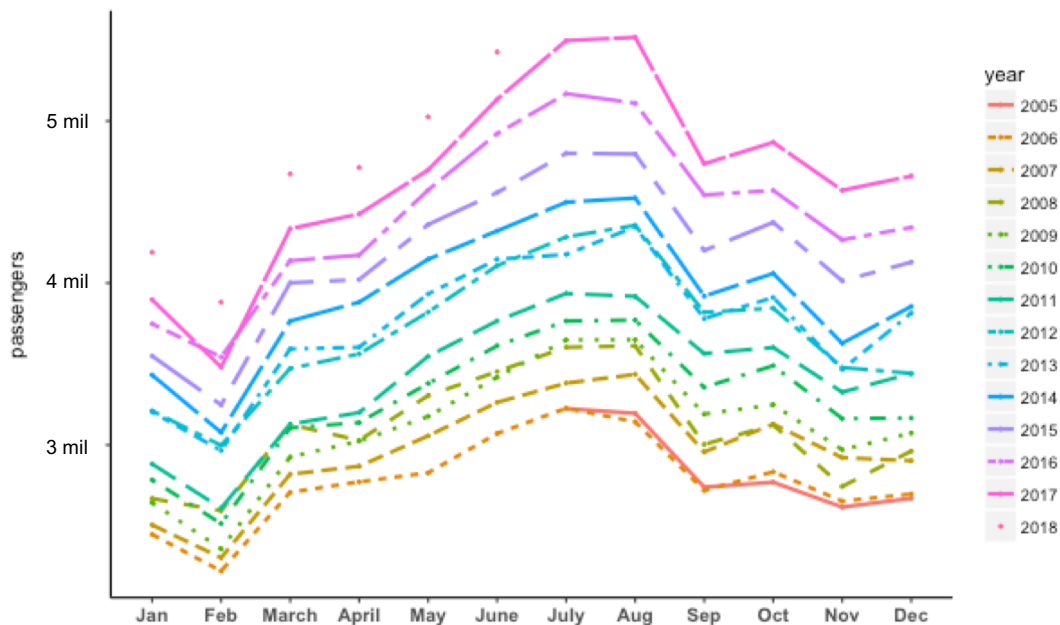
Figure 2: Monthly Criminal Incident Trends for the Ten Most Frequent Crime Categories



approximately 809 to 880 thousand (a 9 percent increase). Employment within the city, measured by the employment totals reported to the Census Bureau by San Francisco employers participating in the Quarterly Survey of Employment and Wages (QCEW), increases from roughly 530,000 to 730,000 (a 38 percent increase). Data on typical weekday exits from BART stations located within San Francisco increases from roughly 150,000 to over 180,000 (a 20 percent increase) between 2010 and 2017. In addition, we see a pronounced increase in total bridge crossings throughout the Bay Area of approximately 22 percent, reflecting an overall increase in economic activity from 2010 onward. We have already noted that the city's daytime population swells above the resident population due to a daily net inflow of commuters.

San Francisco has also experience a pronounced increase in tourist visits during the time period when larceny theft levels are increasing. **Figure 3** displays monthly passenger landings at San Francisco International Airport for each year between 2005 and 2018. The figure reveals two patterns that are relevant to our study. First, there are pronounced increases in tourist visits over the entire period depicted, but especially for the period from 2009 through 2018. Second, in each year we observe pronounced declines in tourist visits between August and September, suggesting that a key source of potential crime victims declines during the time of year that we study. This is an important factor that suggests we may systematically observe drops in crime towards the end of August with the decline in tourism. Our non-parametric hypothesis tests that compare the estimate for 2017 to earlier years is thus particular important for ruling out this possible alternative explanation.

Figure 3: Passenger Landings in San Francisco



Finally, **Figures 4 through 7** present heat maps of criminal incidents for four of the most frequently reported crime categories that paint a portrait of the broad geography of criminal offending in the

city. To interpret these maps, note that the volume of crime is lowest in bluer areas and the highest in more yellow areas. Larceny theft (depicted in **Figure 4**) is clearly concentrated in the downtown area and along the Embarcadero (the area inclusive of Fisherman's Wharf and other tourist spots), both key employment centers and areas frequently visited by tourists. Assaults (**Figure 5**) exhibit concentrations in the Tenderloin region, the Mission District, and the Bayview section of the city. Auto theft (**Figure 6**) is more evenly distributed across the city, though still concentrated in the city center and areas with dense resident population and employment. Finally, we see a heavy concentration of reported drug offenses in a very small area of downtown roughly coinciding with the Tenderloin district (depicted in **Figure 7**).

Figure 4: Location of Larceny Theft Incidents in San Francisco, 2017

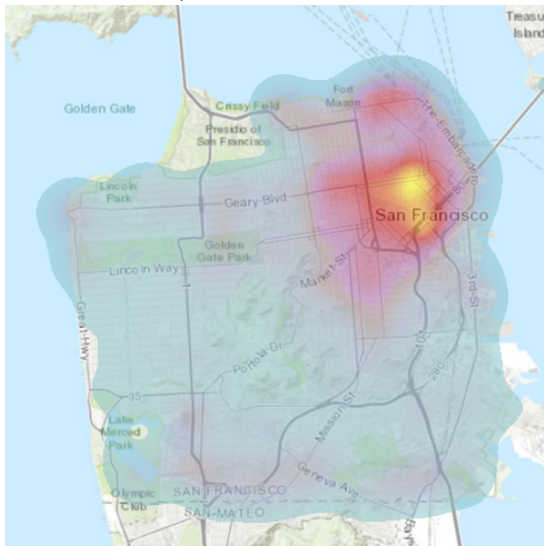


Figure 5: Location of Assault Incidents in San Francisco, 2017

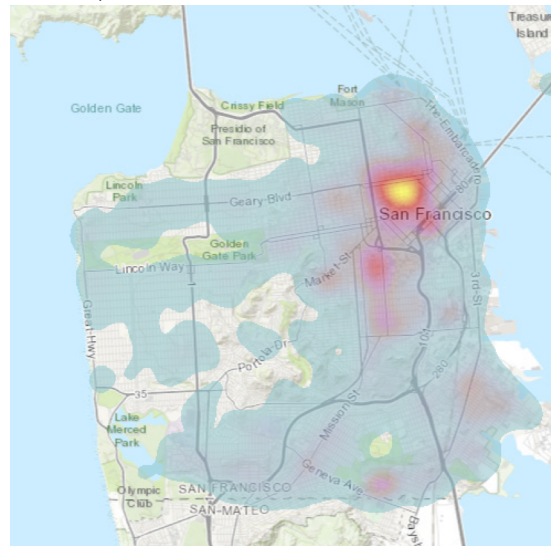


Figure 6: Location of Vehicle Theft Incidents in San Francisco, 2017

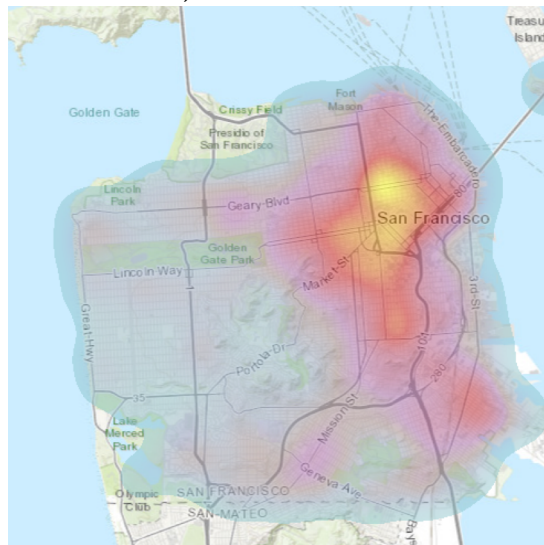
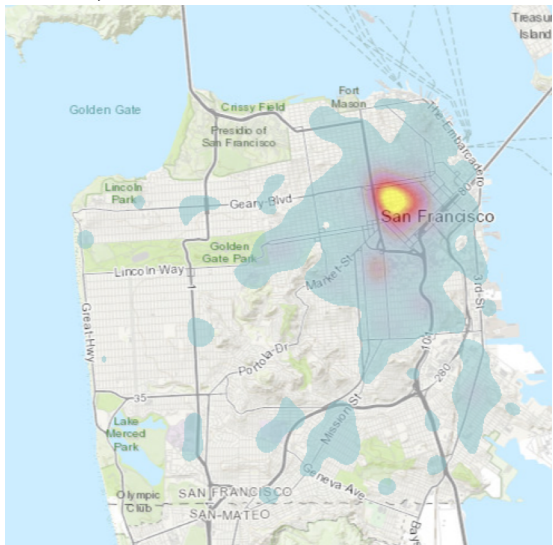


Figure 7: Location of Drug Incidents in San Francisco, 2017



There are several inferences that one might draw from this descriptive analysis that are relevant to the analysis to follow. First, crime patterns are quite distinct in the city and concentrated in certain high volume areas characterized by concentration of employment and residents, heavy foot traffic, and popular tourist destinations. One might hypothesize a priori that foot beats might be particularly effective in such settings.

Second, the nature of crime differs in different regions of the city, with larceny theft being particularly severe in the city's center and along the Embarcadero, and assault and drug offenses concentrated in areas such as the Tenderloin, the Mission District, and Bayview. Given the varying geography of crime, one would not expect to see similar effects of a change in the number of officers assigned to foot beats in each region, as the composition of offenses occurring within different police districts varies.

Finally, the data reveal a clear steady increase in larceny theft co-occurring with growth in population, accelerated growth in employment and public transit commuting into the city, and sustained and sizable increases in tourist visits. Given this complex set of contributors to likely criminal opportunities in the city, it's difficult to fashion an appropriate denominator for constructing a crime rate as one would for a region where there was less day-to-day and within-day variation in the population at risk relative to the resident population. For this reason, our analysis focuses on daily crime levels.

Empirical Results

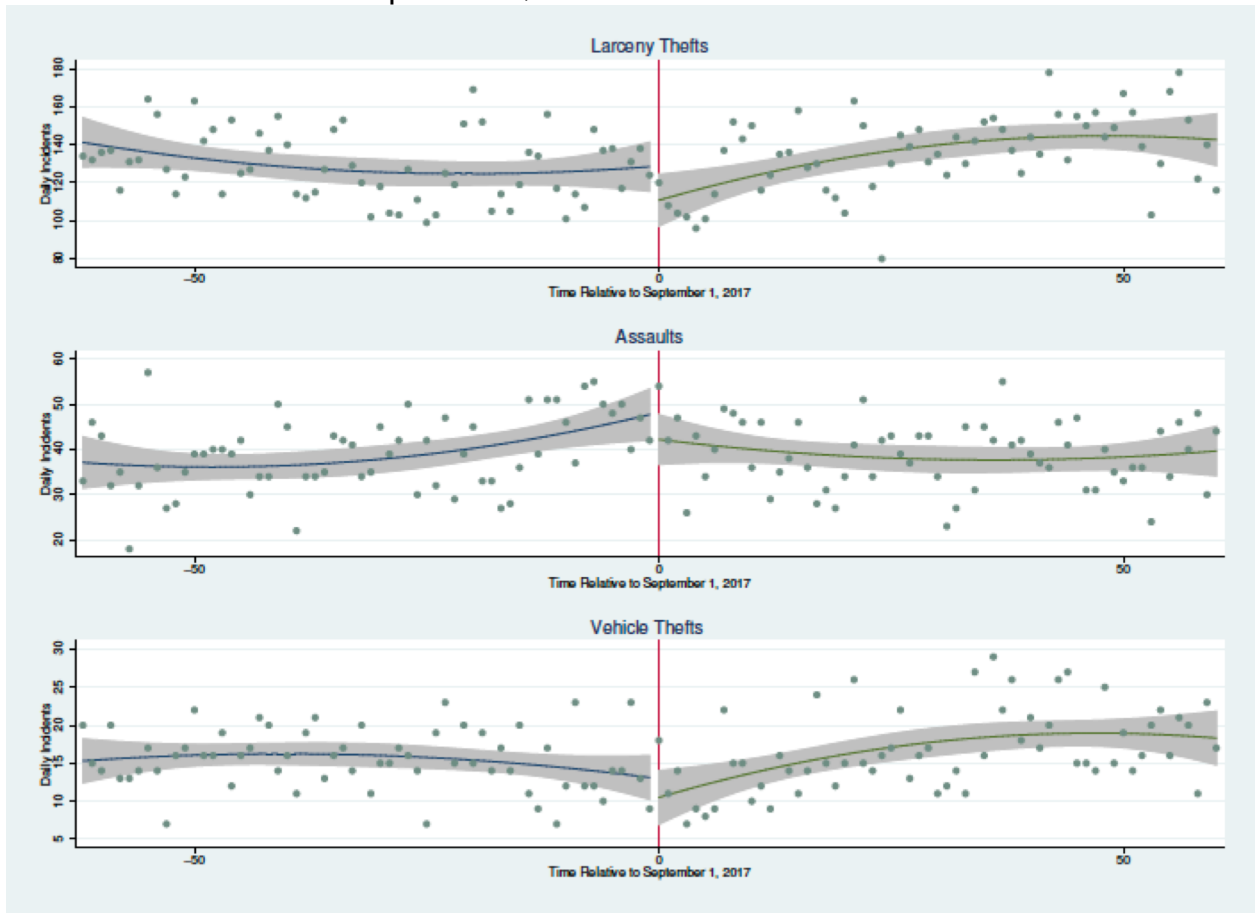
Here we present our principal empirical findings. We first present estimated effects by offense category for the city overall for the year 2017. Next, we assess whether the estimates for 2017 are outliers relative to placebo estimates from non-intervention years. Finally, we explore heterogeneity in effect sizes across police districts for the crimes where there is some evidence of a citywide impact.

4.1 Citywide Estimates for 2017

We begin with a graphical illustration of the nature of our estimation strategy for identifying the effects of the increase in foot-beat assignments on the three crimes where we find preliminary evidence of an effect. **Figure 8** presents scatter plots of daily crime totals for the entire city against time measured relative to September 1, 2017 for larceny theft, assaults, and vehicle theft for the time period from July 1, 2017 through October 31, 2017. In addition to the data points, the figure fits quadratic time trends to each side of the intervention date along with 95 percent confidence intervals for the trend predictions (the grey shaded areas). We show the raw scatter plot for these three offenses as these are the categories for which there is some preliminary evidence of an impact of the reassignments on crime.

There are several common patterns observed in the scatter plots for these offenses. First, for each we observe a break in the trend line associated in time with the officer reassignments, with a dip in daily crime totals associated with the policy change. Second, the data reveal a clear cyclical pattern every seven observations suggesting that there are day-of-week mean differences in crime.

Figure 8: Scatter Plot of Daily Incidents of Larceny Thefts, Assaults, and Vehicle Thefts against Date Measured Relative to September 1, 2017



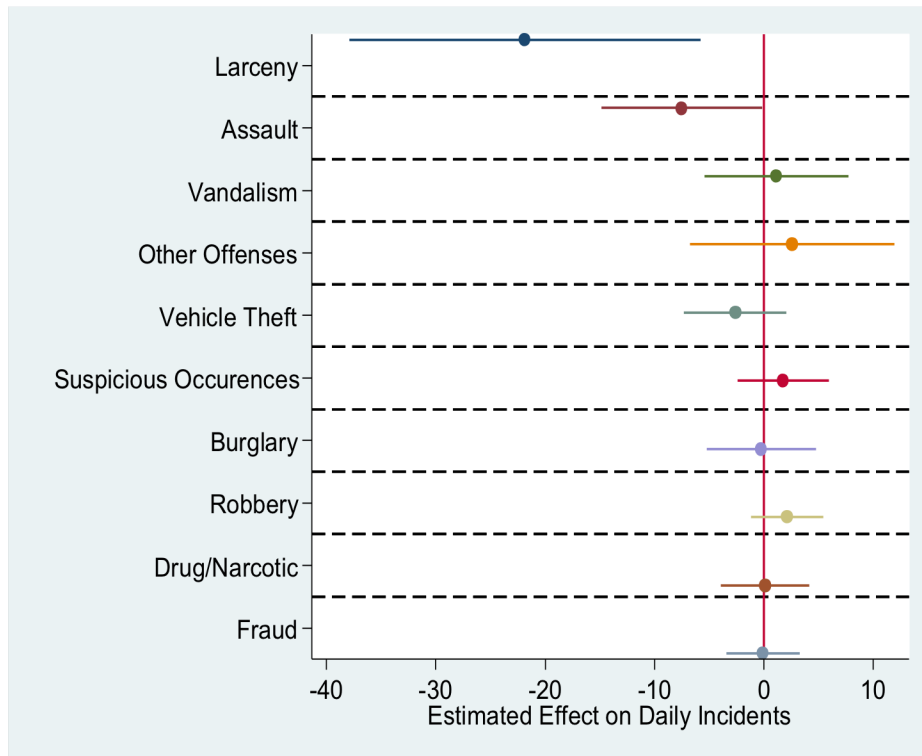
The specification of the model in equation (1) above basically fits separate quadratic trends to the daily crime totals for the two months prior to the officer reassignments and the two months following the reassignments with an allowance for a discontinuous break corresponding to the policy change (where the empirical estimate of the parameter γ_0 provides the estimated discontinuous change in crime). Hence, we estimate equation (1) above augmented to include a complete set of day-of-week fixed effects for each crime category. **Figure 9** summarizes the key parameter estimates from each of these ten models. For each crime, the figure displays the point estimate of the discontinuous change in crime (the colored dot), the 95 percent confidence interval estimate (the horizontal line drawn through the dot), and a vertical line at the value of zero for the purpose of visibly displaying whether a zero-effect lies within the 95 percent confidence interval. For larceny theft, we observe a decline in daily criminal incidents of 21.85, with the estimate statistically significant at the one percent level of confidence. We also observe a significant decline in assault incidents equal to 7.51 (with a p-value of 0.046), and a statistically insignificant decline of 2.61 for vehicle theft (with a p-value of 0.271). There are no measurable impacts on any of the other ten categories.

An alternative manner of summarizing the results would be to calculate the effect sizes relative to base incident levels in the pre-change period. For larceny theft, the decline of 21.85 incidents

amounts to 16.9 percent decline in larceny theft relative to average daily incidents for the two preceding months. The comparable proportional declines for assault is 19.1 percent.

Of course, it may be the case that crime totals for these offenses change in a similar manner every year around September 1st. We now turn to assessing whether this is the case.

Figure 9: Estimated Discontinuous Breaks in Daily Crime Trends around September 1, 2017



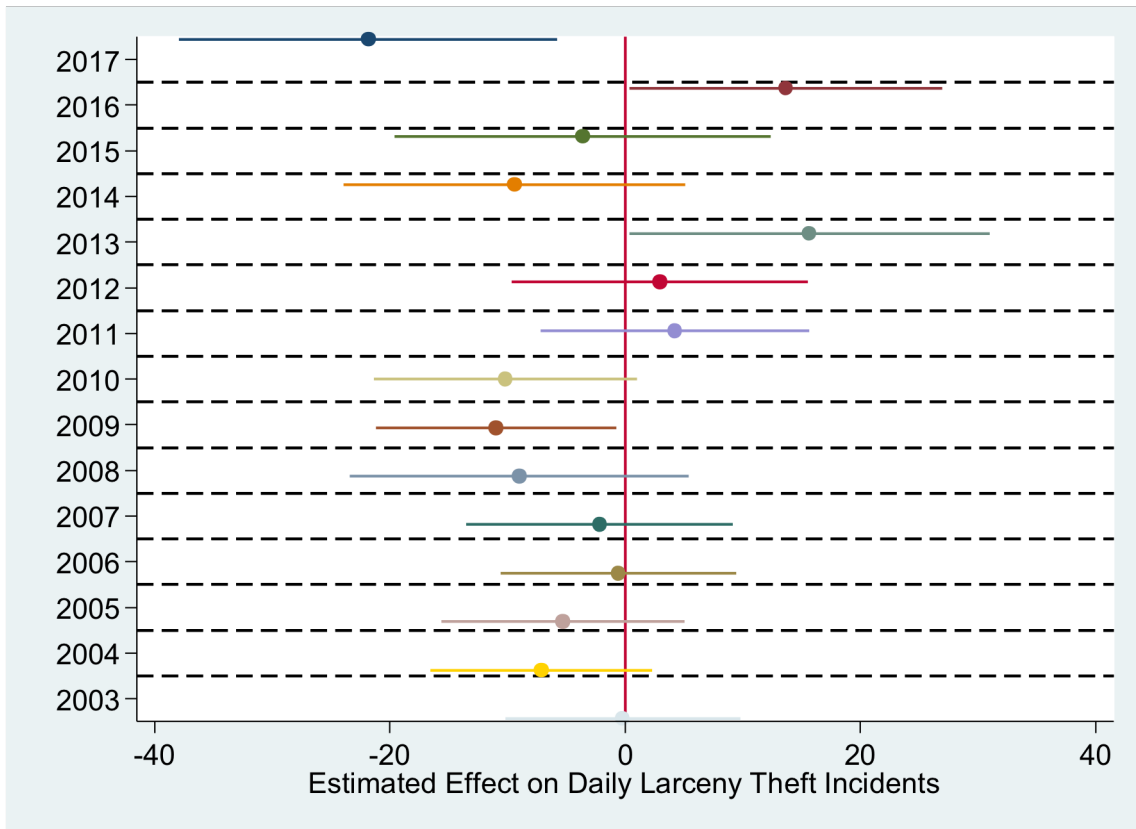
4.2 Comparing the Breaks to Those from Previous Years

In this section we test for discontinuous breaks in crime rates for each year from 2003 through 2016 using the same four-month period that we analyze for 2017. Given that similar reassignments of officers to uniform foot beat did not occur on September 1st of these previous years, we do not expect a priori to observe changes in crime. To the extent that we do see declines that are comparable in magnitude to what we observe for 2017, we would conclude that our 2017 estimates reflect a seasonal pattern rather than an effect of the officer reassignments. On the other hand, to the extent that the 2017 estimates stand apart from these placebo estimates, the evidence suggestive of an effect of policing on crime would be strengthened.

We illustrate the basic logic of this exercise in **Figure 10**. The figure graphically presents placebo estimates of the parameter γ_0 for each year between 2003 and 2016 along with the accompanying 95 percent confidence interval as well as the treatment effect estimate for 2017 for comparison. For the years 2003 through 2016, some of the estimates are positive, some are negative, and many are very close to zero. Relative to these estimate, the 2017 estimate of a

decline in daily larceny theft of roughly 22 incidents per day is a clear outlier. The estimate is both the most negative point estimate of those displayed as well as the largest estimate in absolute value.

Figure 10: Estimated Discontinuous Breaks in Larceny Theft Around September 1, for each year 2003 through 2017



The results of this exercise are summarized for all ten offenses studied in **Table 2**. The first column presents the rank (from smallest to largest) of the 2017 estimate in the distribution of the 15 estimates for each year between 2003 and 2017. The second column presents the proportion of estimates (inclusive of the 2017 treatment effect) that are equal to or less than the value for 2017. Again, we interpret this as the p-value from a one-tailed test of the hypothesis that the reassignment of police to foot beats in 2017 caused a decline in the number of daily incidents for the given crime category, where the 15 estimates provide the non-parametric sampling distribution for the treatment effect. In the third column of figures, we first take the absolute value of each parameter estimate and then calculate the rank of the 2017 estimate (with estimates ranked for smallest to largest). The final column presents the proportion of estimates that are (in absolute value) at least as large as the estimate for 2017. Again, we interpret this probability as the p-value from a two-tailed test of the hypothesis that the reassignment of police to foot beats in 2017 caused a decline in the number of daily incidents for the given crime category.

The results in **Table 2** suggest that the decline in larceny theft in 2017 was unusual, both in that the estimate is the most negative and largest in absolute value. The p-value from the one and two-tailed tests are both 0.067 (the smallest possible value given that our sampling distribution has only 15 values). For assault, the estimate for 2017 is the most negative of the 15 values but is not the largest in absolute value (with one prior year yielding a larger coefficient). Hence, while the one-tailed test yields the lowest p-value (of 0.067), the two-tailed tests gives a p-value of 0.133.

Table 2: Rank of 2017 Criminal-Incident Discontinuity Estimate (Actual Value and Absolute Value of the Estimate) Relative to the Distribution of Discontinuity Estimates for the Years 2003 through 2017 by Incident Category

Category	Rank, $\hat{\gamma}_0^{2017}$	$P(\hat{\gamma}_0^i \geq \hat{\gamma}_0^{2017})$	Rank $ \hat{\gamma}_0^{2017} $	$P(\hat{\gamma}_0^i \geq \hat{\gamma}_0^{2017})$
Assault	1/15	0.067	14/15	0.133
Burglary	9/15	0.600	3/15	0.867
Drugs	8/15	0.533	3/15	0.867
Fraud	5/15	0.333	1/15	1.000
Larceny	1/15	0.067	15/15	0.067
Other Offenses	13/15	0.867	4/15	0.800
Robbery	14/15	0.933	10/15	0.400
Suspicious Occ.	10/15	0.667	8/15	0.533
Vandalism	7/15	0.467	2/15	0.933
Vehicle theft	7/15	0.467	6/15	0.667

The first column of figures presents the rank (from smallest to largest) of the discontinuous change in daily crime incidents on September 1, 2017 relative to distribution of comparable changes estimates for each year between 2003 and 2017. All estimates are based on a regression of daily incidents on a quadratic function of the running variable, a dummy indicating days following September 1st, interaction terms between the quadratic function and the after dummy, and a complete set of day-of-week fixed effects. Each regression uses data on each day's incidents for the period from July 1 through October 31st of each year. The second column presents the proportion of estimates that are less than or equal to the estimate for 2017, with the value corresponding to the p-value from a one-tailed test that the estimate for 2017 is an outlier relative to the placebo distribution. The third column presents the rank (with rank from smallest to largest) of the absolute value of the 2017 estimate relative of the distribution of estimates for the period 2003 through 2017. The final column presents the proportion of estimates that are at least as large as the estimate for 2017, providing a two-tailed test of the hypothesis that the 2017 estimate is an outlier relative to the placebo distribution.

The results from this exercise do not support an effect of the reassignments on vehicle theft. Estimates from six previous years are more negative than the estimate we observe for 2017. Moreover, among the distribution of estimated absolute values, there are five estimates from previous years that are larger than the 2017 figure. Given the size of the corresponding p-values (0.467 from the one-tailed test and 0.607 from the two-tailed test), the results in **Table 2** suggest that the changes in vehicle theft observed at September 1st for 2017 is not unusual relative to comparable changes from previous years.

Regarding the remaining seven offenses, there is no evidence from this exercise indicative of an impact of the reassignment on the volume of crime incidents for these remaining categories.

4.3 Estimates by Police District

Our final set of results assesses whether the effects we observe vary across police district. All ten police districts were ordered to increase foot patrols. However, the increases differed across district. Of course, one should keep in mind the patterns displayed in the heat maps (displayed in **Figures 4 through 7**). While crimes certainly occur throughout the city, the figures reveal clear areas of high concentration and crime problems that appear to vary qualitatively from district to district. Hence, the effect of an increase in uniformed foot patrols may vary with the overall level of crime in an area and the nature of crime in the different police districts.

Figure 11 presents estimates by police district of the discontinuous break in larceny theft coinciding with the reassignment of officers to foot beats. The figure shows significant negative effects in the Ingleside, Mission, Northern, and Richmond districts. The estimates for the remaining districts are all close to zero and statistically insignificant.

Figure 12 presents comparable estimates for daily assaults. Here we see negative sizable estimates for three districts (Bayview, Central, and the Mission) with statistically significant effects in two districts (the Mission and Bayview). Interestingly, the areas where we observe negative points estimates for the assault effects correspond to the police districts where assaults appear to be concentrated (as revealed by the heat map displayed in **Figure 5**).

Finally, **Figure 13** presents estimated changes in vehicle theft by police district. Here we see sizable but statistically insignificant declines in the Bayview, Mission, and Ingleside districts, and insignificant and near-zero estimates for the remaining seven regions.

Figure 11: Estimated Discontinuous Breaks in Daily Larceny Theft by Police Districts

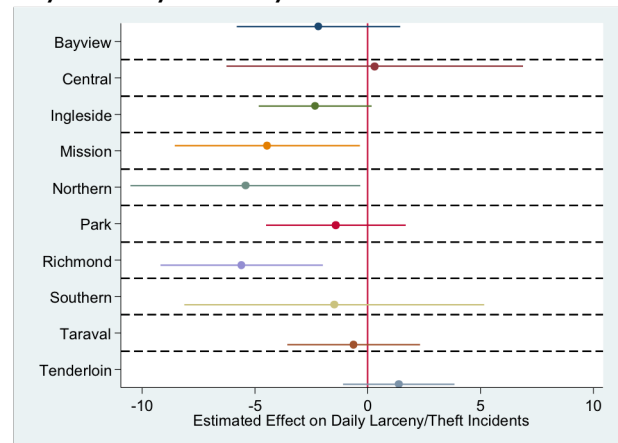


Figure 12: Estimated Discontinuous Breaks in Daily Assaults by Police Districts

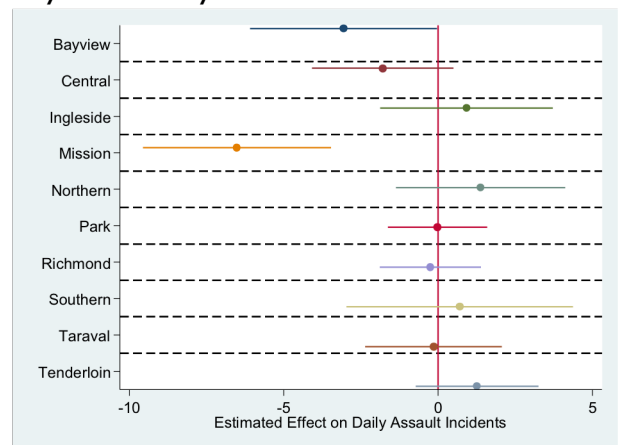
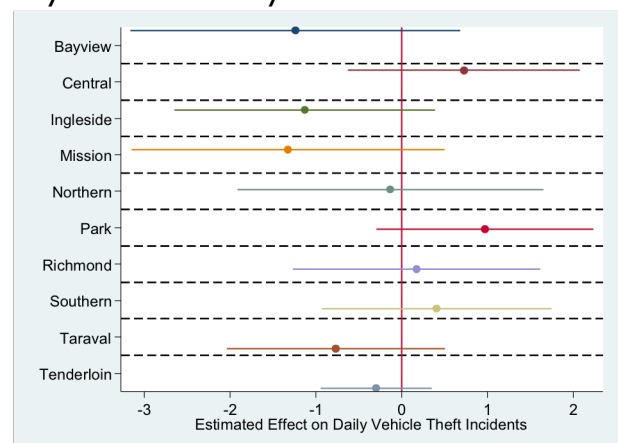


Figure 13: Estimated Discontinuous Breaks in Daily Vehicle Theft by Police Districts



We also estimated separate models by police district for each of the other crime categories where we do not see if a citywide impact. These results are for the most part imprecisely measured and yield few significant findings.

We should note that we do not see bigger declines in larceny theft in areas experiencing bigger absolutes increases in foot patrols. However, pre-intervention crime levels and offense composition differ across district in a manner that suggests one would not expect uniform effects of increased street patrols in different regions of the city.

Robustness Checks

Before concluding, we subject our key findings to a few additional robustness checks. First, we assess whether our conclusions regarding the effect of reassigning officers to foot beats is sensitive to the time window that we use to estimate the discontinuous break in crime. Recall we analyze data for the four-month period centered around September 1, 2017 (roughly a 120-day window). We begin by re-estimating the models using a 30-day window, a 40-day window, a 50-day window, and so on through 120 days. Of course, the estimates using shorter time windows will be higher variance given the smaller sample of data points used to estimate the quadratic trends on either side of the police change. However, generating estimates using these shorter time windows allows for more flexibility and ensures that our estimates are not artifacts imposing quadratic functional forms on more complex time trends in the pre and post periods.

Figure 14 presents the estimates for larceny theft. The estimates effect using the shorter time windows are larger than our estimate using the 120-day window. All are statistically significant at conventional levels. Regardless, the results suggest that the finding pertaining to larceny theft is robust to altering the time window of analysis.

Figure 15 presents comparable analysis for assault. Here, we observe a statistically insignificant effect for the shortest time window, and marginally significant effects for the remaining estimates. The point

Figure 14: Larceny Theft Estimates Using Various Time Windows around September 1, 2017

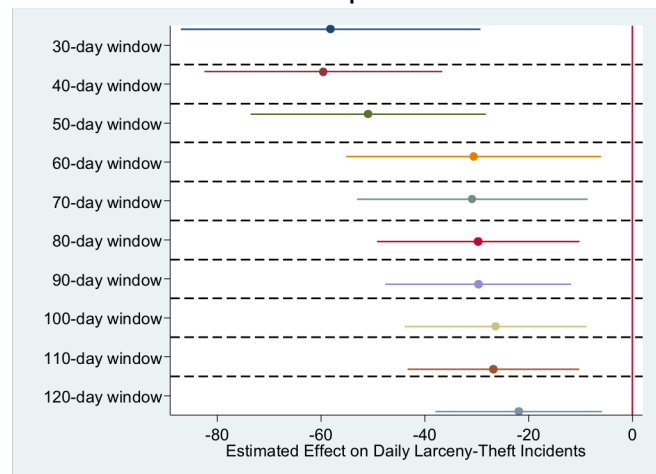
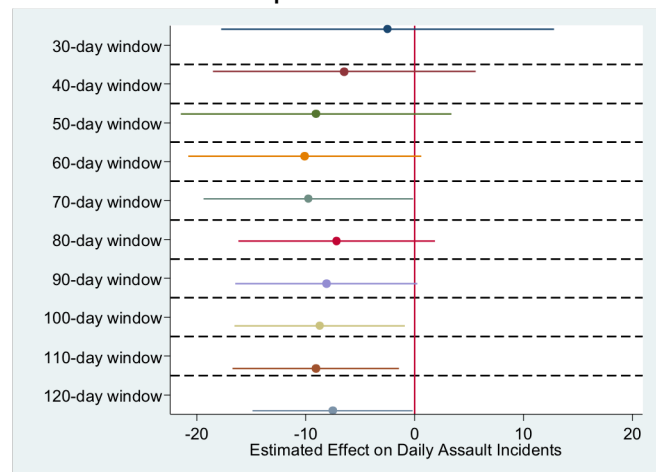


Figure 15: Assault Estimates Using Various Time Windows around September 1, 2017



estimates are generally comparable across the models.

In addition to varying the analysis window for the most frequently reported crime models, we also estimated separate models for all of the other less frequently-reported incident categories. We found modest statistically significant positive effects of the re-assignment of officers to uniformed foot beats on daily incidents classified as “extortion”, “secondary code”, and incidents categorized as “non-criminal”. These positive effects may be reflective of more incidents recorded simply as a result of more police on the street observing these additional offenses.

Conclusion

The findings of this study are several. We find a discrete decline in larceny theft equal to roughly 16.1 percent of typical daily levels associated with a reassignment of 69 officers to uniform foot-beat patrols. We also find a significant decline in assaults. For larceny theft, the decline we observe around September 1, 2017 is quite large relative to changes observed around the similar time period in earlier years, as is the 2017 decline in assaults. While we observe a decline in motor vehicle theft, the decline is not statistically significant and we see similar declines in early September in previous years, suggesting the auto theft effect for 2017 may simply be an artifact of the underlying variability of this estimator.

We should note that the event study methodology employed here basically measures the change in crime within the days immediately following the policy change. We cannot with any degree of certainty assess whether the impacts we measure here are short-lived. Moreover, while our results indicate that average daily larceny thefts and assaults are reduced by the shift in assignments citywide, suggesting that on net any possible displacement effects must be smaller than the geographically concentrated declines, it still may be the case that a more detailed geo-spatial analysis may reveal some crime displacement from areas receiving increases foot-beat patrols to surrounding neighborhoods. In future work, we will employ more geographically disaggregated data to test for effect on specific blocks and displacement effects in areas adjacent to areas receiving foot beats.

Finally, we do not have access to information pertaining to the cost of the policy change and thus cannot do a benefit-cost analysis of the crime declines. Moreover, such an analysis must make some effort to assess whether there are long term effects (perhaps in the opposite direction) of moving officers out of special operations assignments.

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