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ABSTRACT

Do Police Make Too Many Arrests? The Effect of Enforcement Pullbacks on Crime*

Do reductions in arrests increase crime? We study line-of-duty deaths of police officers, events that likely impact police behavior through increased fear but are unlikely to directly impact civilian behavior. Officer deaths cause significant short-term reductions in all arrest types, with the largest reductions in arrests for lower-level offenses. In contrast, we find no evidence of an increase in crime or a change in victim reporting through 911 calls. There is also no apparent threshold of arrest decline beyond which crime increases. Our findings suggest that enforcement activity can be reduced at the margin without incurring public safety costs.

JEL Classification: J15, J18, K42

Keywords: policing, crime, deterrence, broken windows, Ferguson effect, community trust

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Civilians value living in safe communities, but little agreement exists over the most effective policy means to promote public safety. Law enforcement in the U.S. make over 10 million arrests each year, with severity ranging from serious violent and property arrests to officer-initiated arrests for minor offenses like loitering.[1] Nearly all enforcement actions involve some social cost, and these costs must be weighed against their benefits for crime reduction. While a large literature in economics and criminology has shown that increases in police *manpower* lead to reductions in crime (Chalfin and McCrary, 2017), relatively little is known about the efficacy of the various dimensions of police *enforcement*. As public pressure to reform policing in the U.S. has grown in recent years, a crucial question is whether there are forms of enforcement that can be scaled back without sacrificing public safety.

Police reform advocates argue that law enforcement should reduce its heavy reliance on sanctions for low-level offenses, an approach popularized in the 1980s as part of a "broken windows" policing philosophy (Kohler-Hausmann, 2018; Speri, 2020; Silva, 2020).[2] These calls for reform stem from growing concerns about the human and economic impact of low-level sanctions, which can impose long-term human capital, financial, and employment costs (Mello, 2018; Bacher-Hicks and de la Campa, 2020) and often target minority groups (Goncalves and Mello, 2020) as well as financially distressed communities (Department of Justice, 2015; Makowsky et al., 2019). Simultaneous to calls for reform, supporters of broken windows policing argue that aggressive enforcement of low-level offenses has been instrumental in the decline in crime of the last thirty years (Bratton and Knobler, 2009; Zimring, 2011; Riley, 2020). These defenders also claim that public scrutiny following recent high-profile police scandals has led to a decline in enforcement activity and, consequently, contributed to heightened levels of crime.[3]

In this paper, we evaluate whether reductions in police arrest activity lead to an increase in crime. Addressing this causal question is empirically difficult. Large changes in arrests are generally nonrandom and often reflect or coincide with changes in underlying

[1] Federal Bureau of Investigation (FBI) Uniform Crime Report. 2018. Table 29. `https://ucr.fbi.gov/crime-in-the-u.s/2018/crime-in-the-u.s.-2018/topic-pages/tables/table-29`.

[2] See for example, *Campaign Zero*, https://www.joincampaignzero.org/brokenwindows; Karma, Roge. 9/8/2020.

[3] A notable anecdotal example given of this hypothesis, often called the "Ferguson Effect," is Baltimore: in the three months after the death of Freddie Gray in April 2015, the city experienced 116 homicides, 53 more than for the same period in the previous year. In contrast, the total number of arrests made by the Baltimore Police Department actually *declined* over this period, from 12,153 in May-July of 2014 to 6,770 in May-July of 2015. (Calculation from Jacob Kaplan's Data Tool: `https://jacobdkaplan.com/crime.html`)

crime rates. We address this identification challenge by examining changes in police arrest behavior following a line-of-duty death of a fellow officer. We estimate responses to these line-of-duty officer deaths using difference-in-differences event study models that exploit the staggered occurrence of events across agencies.

We argue that an officer death shifts peer officer behavior, potentially through increasing fear of job risk or emotional distress. Line-of-duty deaths are acutely salient for police officer peers and could plausibly affect their willingness to engage with civilians and make arrests. However, these events are unlikely to affect community social unrest or underlying civilian criminal activity. Indeed, we show evidence from Google search trends that officer deaths attract limited attention in the community, in contrast with the significant attention paid towards high-profile deaths *by* police. This setting is therefore uniquely suited for evaluating the impact of changes in arrests on crime rates.

We examine data from over 2,000 municipalities between 2000-2018 and document that a line-of-duty death is followed by a significant short-term decline in police arrest activity. This effect is present for arrests of all offense types, including serious violent and property crime. While the percentage change across all categories is similar, the reduction in number of arrests is substantially greater for lower-level offenses. Using a series of event-study specifications, we confirm that these events are not preceded by significant changes in crime or arrest activity, suggesting that their timing is exogenous to the criminal environment. While the average impact we identify is short-lived (one to two months), the magnitude of the effect is substantial, on the order of a 5-10% reduction in arrests and a 20% reduction in traffic stops. Similarly-sized percent changes in police employment have been shown to cause significant reductions in crime (e.g. Evans and Owens, 2007; Chalfin and McCrary, 2018; Weisburst, 2019; Mello, 2019; Chalfin et al., 2020). In addition, the time horizon that we consider is comparable to notable studies of rapid changes in police presence that find crime responses (Di Tella and Schargrodsky, 2004; Draca et al., 2011; Weisburd, 2021).

In contrast to the observed decline in arrest activity after a line-of-duty death, we find small and statistically insignificant impacts on reported crimes. Our 95% confidence intervals rule out short-term (long-term) increases of greater than 3.4% (2.9%) in serious "index" crimes, or the most serious violent and property crimes defined by the Federal Bureau of Investigation (FBI).[4] Our point estimates suggest an elasticity of crime to total

[4]Index crimes include murder, rape, robbery, burglary, theft, and motor vehicle theft. We consider murder

arrests of 0.48 for violent crime and -0.12 for property crime, notably less negative than the estimates of the crime-to-*police employment* (or *police presence*) elasticity found in the literature (see Figure 7).

Given that the average decline in arrests has a magnitude of 5-10% and a duration of 1-2 months, an important question is whether our null crime impacts extend to larger or longer arrest declines. We study this question by examining heterogeneity across departments with varying treatment effects. We first consider heterogeneity by magnitude of arrest decline and fail to find evidence of a threshold reduction in arrests above which crime increases. We similarly investigate heterogeneity by duration of arrest reduction and fail to find evidence of crime increases, even for departments with arrest declines that persist for five or more months. While these findings are suggestive, as they do not rely on exogenous variation in size or duration of decline, our study provides the most direct evidence in the literature that a larger or longer-term reduction in arrests is feasible without crime increases. Collectively, these findings suggest that reforms which induce modest reductions in police arrest activity, and particularly enforcement against low-level offending, may not come at the cost of rising crime rates.

We argue that our estimates reflect the causal impact of a marginal reduction in arrest activity on crime. This interpretation requires assuming that arrest activity is the only variable directly impacted by the line-of-duty death, and we provide several pieces of evidence to rule out potential violations of this assumption. To address the concern that criminal offenders directly respond to the officer death separately from the arrest decline, we inspect the pattern of results in cities where the officer death did not lead to any arrest decline, and here we similarly find no impact on crime rates. To probe whether dimensions of enforcement other than arrests respond, we consider use-of-force deaths *by* police, and we find no change after an officer line-of-duty death.

To further test the assumptions of our design, we leverage detailed data on a case study in Dallas, TX, where an officer death in 2018 led to a greater-than-30% decline in arrests for more than two months. This magnitude and duration of decline is comparable to those found in studies of short-term surges in police presence after terrorist attacks which find immediate reductions in crime (Di Tella and Schargrodsky, 2004; Draca et al., 2011).

separately from other violent crime to account for changes in this outcome related to the officer death itself (see Section 5).

In Dallas, we continue to find null crime impacts as in our national sample, and we provide additional evidence that police presence and use of force do not change following the line-of-duty death of an officer. We also use this case study to corroborate that the decline in arrest activity is a behavioral response rather than a reduction in the number of working officers. These findings ameliorate concerns about an exclusion restriction violation, whereby the officer death changes the criminal environment beyond its impact on arrest activity.

One challenge with studying the impact of police behavior on crime rates is that measured crime is partly a function of police reporting. Some crime reports initiate with officer pro-activity, and even in cases where officers respond to a 911 call, they have discretion over whether a crime report is written and the incident is included in the crime rate. If officers respond to the death of a co-worker by reducing their propensity to record crimes, this effect will bias us away from finding an increase in crime. To address this concern, we hand-collected a large data set of 911 calls from over 70 police departments across the United States. These calls originate with civilians and therefore are unaffected by changes in officer reporting behavior. We estimate that the frequency of calls does not significantly change after an officer death. Further, we find that the propensity of officers to write a crime report conditional on a call does not decrease after a peer death. Concerns about the impact of officer reporting practices on official crime statistics are regularly raised in the policing literature (Levitt, 1998; Mosher et al., 2010), and our novel data are uniquely able to address this issue.

This study directly relates to mounting calls to curtail the practice of broken windows policing, whose central tenets include the aggressive enforcement of laws against low-level offending (Kelling and Wilson, 1982). An existing literature explores the impact of broken windows policing on crime, largely focusing on New York City, where the philosophy has been prominently adopted and where crime has declined dramatically since the 1990s. Researchers exploiting variation over time in misdemeanor arrests have shown that increases in arrest activity are associated with declines in overall crime (Kelling and Sousa, 2001; Corman and Mocan, 2005; Rosenfeld et al., 2007). Similarly, a number of studies have investigated the effects of geographically-focused policing interventions (within cities) that increase both police presence and enforcement activity for low-level crimes and found reductions in crime (e.g. Braga and Bond, 2008; MacDonald et al., 2016). However, reviews of this literature note that these program interventions are multi-faceted and that the largest crime reducing effects appear to be generated by programs that successfully involve the community, rather than those that are driven by order-maintenance strategies that increase enforcement activity (Braga et al., 2015; Weisburd et al., 2015). Further, Harcourt and Ludwig (2006) note that observational data studies in this literature potentially suffer from mean reversion bias: increases in enforcement are targeted towards areas with recent spikes in crime, and the increased enforcement tends to coincide with a reversion of the crime rate back to its historic average. We provide valuable new insights to the literature on broken windows policing by both expanding the scope of cities under study and exploiting variation in enforcement that is plausibly exogenous.

This study also relates to a growing literature on the impact of heightened scrutiny of the police on their behavior and on criminal outcomes. Several papers find that officers change their behavior after policing reforms and general social unrest that follow a policing scandal (Prendergast, 2001, 2021; Shi, 2009; Heaton, 2010; Rivera and Ba, 2019). Using recent data, Cheng and Long (2018) and Premkumar (2020) document that high-profile deaths of civilians at the hands of police lead to reductions in officer discretionary enforcement and concurrent increases in crime. In another recent study, Devi and Fryer Jr (2020), find that federal investigations of police departments are linked to both decreases in arrest activity and, when these investigations follow a viral video of a police use-of-force incident, increases in crime. Further, in a new working paper, Ang et al. (2021) find that in the aftermath of the killing of George Floyd by a Minneapolis police officer, both violent shootings increased and victim willingness to call the police decreased, suggesting that police scandals can both directly affect the criminal environment and trust in police in the community. We address a distinct but related question of whether reductions in arrests directly cause a change in crime, focusing on a context where crime and distrust in the police are not elevated from a high-profile police use-of-force incident.

A number of other papers study institutional changes in policing whose effects include a reduction in arrest activity. Chandrasekher (2016) and Mas (2006) document reductions in police enforcement during and after union contract negotiations and find varying degrees of crime increase as a result. In contrast, McCrary (2007) finds that court-ordered racial quotas for police hiring lead to a reduction in arrests but no significant increase in reported crimes, and Owens et al. (2018) similarly find that an intervention in Seattle aimed at slowing down police decision-making processes led to a reduction in arrests but did not lead to citywide crime increases. These studies examine policies and events that change several dimensions of police behavior and the criminal environment, and we contribute to this literature by focusing explicitly on the impact of arrest reductions in a setting where other features of law enforcement are not altered by policy.

Lastly, we contribute to the literature on policing and crime by collating numerous data sources to address multiple aspects of our setting. These data include monthly crime and arrest statistics and on-the-job officer deaths from the F.B.I. Uniform Crime Reports, data on traffic fatalities from the National Highway Traffic Safety Administration (NHTSA), records of traffic stops from the Stanford Open Policing Project, internet search popularity from Google Trends, and contextual information on officer deaths from the Officer Down Memorial Page website. We supplement these publicly accessible sources with data on 911 calls acquired through individual open records requests to police departments across the U.S. This data collection covers over 70 cities and, to the best of our knowledge, represents the largest composite of 911 data used in an academic study to date. We additionally compile micro-data on multiple aspects of police activity from the Dallas Police Department to further scrutinize mechanisms using a case study of an officer death in 2018.

1 Background: The Police Response to Officer Deaths

Approximately 60 police officers are feloniously killed each year in the United States. While this outcome is relatively rare, the job of a police officer is dangerous relative to other professions; in terms of total fatalities it ranks among the top 20 most dangerous occupations in the U.S.[5] Nearly all felonious killings of officers result from gunshot wounds, with a minority of these deaths resulting from vehicle collisions. Officers who are killed are demographically representative of typical police officers; the average officer killed is a 38-40 year old white male with over 10 years of service in his department.[6]

Though officer line-of-duty deaths are statistically rare, these incidents are acutely salient to other officers. Police scholars have long noted that a preoccupation with death

[5]Stebbins, Samuel, Evan Comen and Charles Stockdale. 1/9/2018. "Workplace fatlities: 25 most dangerous jobs in America." *USA Today.* https://www.usatoday.com/story/money/careers/2018/01/09/workplace-fatalities-25-most-dangerous-jobs-america/1002500001/

[6]FBI Uniform Crime Report. 2019. Summary Tables 14, 15 & 28. *Law Enforcement Officers Killed or Assaulted (LEOKA).* https://ucr.fbi.gov/leoka/2019/topic-pages/officers-feloniously-killed

and fatality risk is central to police culture, and officers often view their work in "life-or-death" terms (Marenin, 2016; Sierra-Arévalo, 2016). Officers are formally instructed about the potential perils of their work and how to protect their lives in the field, beginning with their training in the police academy. When an officer dies while on duty, their police department will typically commemorate the death with a formal police funeral, which often includes dress uniforms, dedicated music, a 21-gun salute, and a symbolic last radio call to the fallen officer or "end of watch call." After an officer has died, peers within their department will often place mourning bands on their shields in memory of the officer. Across the U.S., police departments hold yearly memorial ceremonies and hold commemorative fundraisers in honor of police officers who have died, often over National Police Week in mid-May.[7] Several national institutions focus on the commemoration of police officers who have died in the field; these include the National Law Enforcement Memorial Fund, Law Enforcement United and the Officer Down Memorial Page. Ethnographic research highlights the fact that officer deaths become a part of the "organizational memory" of a department, long after the deaths occur, through physical memorial plaques in headquarters, commemorative wrist bracelets, and even memorial tattoos (Sierra-Arévalo, 2019).

While officer deaths are not generally associated with increases in community unrest, police officers themselves have in some recent cases responded to deaths of peers by tying these incidents to rising distrust in the police. As an example, in 2014, two New York City Police Department officers were shot and killed while sitting in their patrol car by an individual who intentionally sought to target police officers.[8] In the aftermath, many rank and file officers expressed the sentiment that the event was due to an abandonment of the department by Mayor Bill de Blasio. As stated by Patrick Lynch, the head of the NYPD union, "[The officers' blood] starts on the steps of City Hall, in the office of the mayor." At the funeral for the officers, the majority of the attending NYPD officers turned their backs to de Blasio during his remarks.[9] In the month after the death, news outlets reported that arrest and citation activity by the department had declined significantly, seemingly as a

[7] See policeweek.org.

[8] Mueller, Benjamin and Al Baker. 12/20/2014. "2 N.Y.P.D. Officers Killed in Brooklyn Ambush; Suspect Commits Suicide." *The New York Times.* https://www.nytimes.com/2014/12/21/nyregion/two-police-officers-shot-in-their-patrol-car-in-brooklyn.html

[9] Flegenheimer, Matt. 12/21/2014. "For Mayor de Blasio and New York Police, a Rift is Ripped Open." *The New York Times.* https://www.nytimes.com/2014/12/22/nyregion/a-widening-rift-between-de-blasio-and-the-police-is-savagely-ripped-open.html

protest against the public and the mayor for their perceived ill will towards the department.[10] Researchers who have studied this highly publicized enforcement pullback in New York City have found that it was not associated with any increase in serious crime, a finding that is consistent with our results at the national level (Sullivan and O'Keeffe, 2017; Chalfin et al., 2021).

In general, police could change their arrest behavior in the wake of a peer death as a result of mourning or because a peer death can serve as a reminder of the dangers of the job. *A priori*, it is not altogether clear in which *direction* a line-of-duty death of an officer will impact fellow officers' behavior on the job. In recent work, Holz et al. (2019) analyze the impact of officer injuries in the Chicago Police Department and find that, after one of their peers has been injured in the field, officers do not change their arrest behavior but increase use of force and reduce their responsiveness to service requests, effects that the authors argue are linked to an increased perception of fear on-the-job. Indeed, the sociological literature on policing has frequently noted that officers' perception of pervasive on-the-job risks – the "danger imperative" (Sierra-Arévalo, 2016) – may contribute to excessive levels of enforcement and use of force (Legewie, 2016; Ouellet et al., 2019; Skolnick and Fyfe, 1993; Stoughton, 2014). In contrast, Sloan (2019) studies unprovoked ambushes of police officers in Indianapolis, Indiana and finds that these events cause officers to reduce the number of arrests they make, without increasing use of force. Likewise, it is possible that the line-of-duty death of a fellow officer and its acute reminder of the inherent risks of police work may lead to a reduction in officers' discretionary arrest enforcement. Ultimately, the aggregate effect can only be determined empirically. Our project provides the first national empirical estimate of the responsiveness of officer arrest behavior to an officer death, and we find that police respond to peer deaths by reducing arrest activity in the short-term and do not find aggregate evidence that other dimensions of policing, including use of force, change.[11]

While officer deaths are memorialized by other officers, awareness of these events is less pronounced among community members. Officer deaths do not tend to attract the public attention that is created by high-profile police killings of civilians, which are often followed

[10] Baker, Al and J. David Goodman. 12/31/2014. "Arrest Statistics Decline Sharply; Police Unions Deny an Organized Slowdown." *The New York Times.* https://www.nytimes.com/2015/01/01/nyregion/arrest-statistics-decline-sharply-police-unions-deny-an-organized-slowdown.html

[11] As discussed in Section 6.2 we do not find an effect of line-of-duty officer deaths on police use of force in our national sample.

by widespread protests and social unrest. Figure 2 plots the average Google search intensity of 137 high-profile deaths of civilians at the hands of police versus 82 officers killed in the field since 2010 using Google Trends data from the U.S. state where each event occurred.[12] While Google Trends does not provide values for total number of searches, it provides a measure of *relative search* volume. All quantities are reported relative to the time period with highest search volume, which is given a value of 100. Multiple search terms can be included at once, and we include as a benchmark a set of search terms related to heart disease (the leading cause of death in the U.S.), which is searched relatively frequently and is not seasonal in search volume.[13] We search each civilian and officer death separately within the state where the event occurred and plot the average within-state search intensities alongside the benchmark search term.

In relative terms, the public is far more aware of the civilian deaths at the hands of police in our sample versus the officer deaths, with the average civilian death having a search popularity value that is over three times the size of the average officer death. Search intensity for a civilian death persists to some degree in the weeks following a death, with subsequent spikes that may be associated with protests of the incident or an announcement of whether the involved officers will be charged. In contrast, the public awareness of an officer death quickly levels to zero after these events. Collectively, this evidence supports our assumption that while officer deaths are highly salient for other officers, the awareness of these deaths among community members is relatively minimal and short-lived. As a result, we argue that officer deaths are unlikely to spark a change in criminal activity or civilian behavior in the community, especially when compared to high-profile civilian deaths, which are highly salient and frequently followed by periods of social unrest. We include additional investigation of this assumption in Section 6.3.

In our paper, we view officer deaths as treatments that shift officer behavior but do not affect civilian behavior. This presumption, consistent with the evidence above, allows us

[12] Information on high-profile deaths of civilians is taken from "Black Lives Matter 805 Resource and Action Guide." Information on officer line-of-duty deaths is acquired from the *Officer Down Memorial Page* and is described in more detail in Appendix A3. The sample frame begins in 2010 to match the coverage of this list.

[13] The use of an appropriate reference benchmark is important in this analysis, as the choice of a benchmark that was sufficiently more popular, such as "Google" or "Youtube", would dwarf any perception of relative search volume for these individuals. The choice of a benchmark that shows a perceptible increase in searches at the time of the events allows us to compare the relative effect of events across time and space as well as between line-of-duty deaths and officer-use-of-force killings.

to interpret any change in crime that results from a reduction in arrest behavior following an officer death as a result of this change in arrest activity.

2 Conceptual Framework

Our objective is to identify the impact of a police officer fatality on their department's arrest activity and subsequently identify the impact of any changes in arrests on crime. The majority of our outcomes may be subject to reporting error and are potentially a simultaneous function of crime, reporting, and arrests. In this section, our purpose is to state clearly what assumptions we make about the measurement of each of our outcomes.

Our study treatment is an officer death, which we argue directly affects officer arrest behavior but does not directly affect victim reporting or civilian offending behavior. Our first objective is to quantify how an officer death changes the arrest activity of police officers, conditional on offenses that have occurred. Our second objective is to measure any changes in crime that result from this change in arrests.

Our data can be broadly grouped into two categories: police enforcement activity and crime activity. The production of the number of arrests of type k is a function of the underlying frequency of the crime, the probability of reporting by a victim, and the probability of a police arrest, conditional on victim reporting:

$$\text{TotalArrests}^k = \text{TotalCrime}^k \times \text{VictimReporting}^k \times \text{ArrestProbability}^k$$

One of our primary objects of interest will be the impact of an officer death on the number of arrests by a department.[14] The goal is to use information on TotalArrests^k to measure changes in the department's enforcement activity, or $\text{ArrestProbability}^k$. However, any change could also reflect responses by victims or offenders. As we note above, one of our key identifying assumptions is that officer deaths do not *directly* affect the frequency of crime. It may still be the case that VictimReporting^k or TotalCrime^k respond to a change in $\text{ArrestProbability}^k$ after an officer death. We directly address potential changes in TotalCrime^k through examining crime data (below).

[14] The relationships outlined in this section are simplified such that each crime is associated with a single victim and a single suspect. In practice, crimes can include multiple victims and suspects. The interpretation of outcomes in this study is comparable when there is more than one victim/suspect under the assumption that the number of victims/suspects does not change with the study treatment, a line-of-duty officer death.

To identify a response of $\text{ArrestProbability}^k$ to an officer death, we will pay particular attention to the impact on arrests for lower level offenses. We group these offenses into "quality of life" arrests, the most minor categories such as disorderly conduct, liquor violations, and drug possession, and non-index arrests, or any intermediate category between the most serious violent and property index crimes and "quality of life" arrests. These arrest types are more likely to result from interactions that are initiated by officers rather than civilian complaints (equivalent to $\text{VictimReporting}^k = 1$ in the relationship above). We will additionally appeal to data on traffic stops to identify changes in $\text{ArrestProbability}^k$. As is the case of low-level arrests, traffic stops are officer-initiated and do not depend on victim choices to report driving offenses (equivalent to $\text{VictimReporting}^k = 1$).

To evaluate the impact of a change in arrest activity ($\text{ArrestProbability}^k$) on criminal offending, we consider the production of a crime report for crime type k, which is a function of the frequency of offenses, victim reporting, and the police officer's choice to write an incident report:

$$\text{CrimeReports}^k = \text{TotalCrimes}^k \times \text{VictimReporting}^k \times \text{ReportProbability}^k$$

Our goal here is to evaluate the impact of a reduction in arrests (caused by an officer death) on the frequency of observable reported crime, CrimeReports^k, where the relationship is posited to be due to any changes in the number of true crimes, TotalCrimes^k. However, changes to the observed reported number of crimes, CrimeReports^k, may also be affected by victim reporting or reporting by the police conditional on the number of crimes committed. To address these concerns, we will use data on 911 calls for service. These records include all calls made to the police and are not filtered by the police, or in this case $\text{ReportProbability}^k = 1$. Therefore, any impact of an officer death on 911 calls will be a function of only underlying crime and victim reporting. Police reporting rates can also be directly estimated by looking at the share of calls that become an incident report, which corresponds to $\text{ReportProbability}^k$. While the frequency of 911 calls is still a function of victim reporting, as we have argued in Section 1, officer deaths are not as widely salient as civilian deaths by the police and are unlikely to directly affect victim reporting behavior.

Lastly, we will assess how traffic fatalities, which are a function of traffic offending, respond to changes in $\text{ArrestProbability}^k$ that follow an officer death. This outcome is not a function of victim or police reporting (VictimReporting^k = 1 and ReportProbability^k=1), as these incidents are very likely to be reported regardless of civilian trust or police department reporting practices and therefore are a true proxy for underlying traffic offending (Kalinowski et al., 2017).

3 Data

3.1 Data Sources

This study combines national and local data sets from a large number of sources. Our sample includes 2,048 municipal police departments and covers the period of 2000-2018. A total of 169 officer death events occur within 101 police departments during our sample period. A detailed accounting of the data sources, sample restrictions, and data cleaning used can be found in Appendix A3.

Information on officer deaths at the month by police department level is derived from the Law Enforcement Officers Killed or Assaulted (LEOKA) series of the Federal Bureau of Investigation (FBI) Uniform Crime Report (UCR). The analysis considers only officer deaths that result from felonious killings and excludes deaths resulting from accidents. This data is linked to information collected on officer deaths by the Officer Down Memorial Page website to determine cause of death.[15]

The arrest and crime data at the month by department level is also sourced from the FBI UCR data on crime reports and arrests. These national data are self-reported to the FBI by individual police departments with limited auditing and therefore have notable data quality issues. To address concerns about reporting accuracy, we first restrict to the agencies who report complete data on a regular basis. We include only law enforcement agencies who report crime and arrest outcomes at the monthly level for the full sample period of 2000-2018, but impose no additional restrictions related to city population size. This sample restriction differs from prior work that relies on annual data reporting. As part of our data cleaning we additionally detect and omit the most extreme outliers in this data, using a similar procedure as in the earlier literature (Evans and Owens, 2007; Weisburst, 2019; Mello, 2019; Chalfin et al., 2020) (see Appendix A3). Our results are robust to this cleaning process and are

[15]We exclude 19 officer fatalities coded in the LEOKA data that could not be verified by either the Officer Down Memorial Page or an external source.

similar using the raw data (see Table A2, specification (9)).

Our crime and community activity outcomes also include records of the number of 911 calls for 72 cities in our sample. We have hand-collected these records through filing open records requests to police departments across the U.S., as this data is not available in any systematic or aggregated form at the national level. To our knowledge, this collection represents the largest sample of 911 calls that has been used in a quantitative research study to date. This data covers the period of 2005-2018, though the number of years varies by city.

We also incorporate data on traffic stops collected by the Stanford Open Policing Project through open records requests. This data source covers 24 cities in our sample. As a complement, we measure traffic fatalities in each city in our sample using data from the Fatality Analysis Reporting System (FARS) of the National Highway Traffic Safety Administration (NHTSA).

Lastly, we include data on yearly demographic characteristics of the cities in our sample from the U.S. Census and the American Community Survey. These variables allow us to control for changing demographic composition in the cities covered by our analysis sample (see Section 4).

3.2 Summary Statistics

Approximately 10 officer deaths occur in each year within our sample of 2,048 police departments, though there is variation in the number of deaths that occur each year.[16] The monthly pattern of officer deaths suggests that there may be some seasonality in this outcome throughout the year, with the highest number of deaths observed in the summer months (Figure 3). Over 90% of the officer deaths in our sample result from gunshot wounds (Table 1). Similar to the national statistics, officers who are killed in the sample are demographically representative; the average officer death is of a 37 year old white male with 11 years of experience.

Appendix Table A1 summarizes demographic characteristics of the sample at the yearly level. The average city in the sample has 39 thousand residents, is 69% white, has a poverty rate of 13%, and a median household income of $46 thousand dollars. In contrast,

[16] As noted above, the national total is approximately 60 deaths per year. Our sample is restricted to cities that regularly report monthly FBI crime data, and cover a sub-set of the country. See the Data Appendix for additional details on sample construction.

treated law enforcement agencies serve populations that are larger, more racially diverse, and more likely to live in poverty; on average, these cities have 255 thousand residents, are 54% white, and have a poverty rate of 16%. Treated cities are defined by having an officer death event; in turn, these departments also experience a greater number of officer assaults that result in injury each year (83 vs. 10 in the full sample).

Our estimation focuses on arrest and crime outcomes at the department by month level. Table 1 shows that the average department in our sample reports 0.2 murders, 18 other violent crimes and 121 property crimes per month. The average police department makes 157 arrests per month, of which 85 are for "quality of life" or low-level offenses, 0.17 are for murder, 9 are for other violent crimes, and 21 are for property crimes.[17] For the sub-sample of agencies that have traffic stop and traffic fatality information, the average department makes over 5,000 traffic stops each month and the average city experiences 0.2 fatal traffic accidents. In accordance with the fact that treated agencies serve much larger cities, treated agencies also have substantially higher levels of reported crime and make more arrests and traffic stops than the average department in the sample.

Given the clear differences between our treatment and control agencies, we employ a difference-in-differences model which includes detailed controls and department-specific fixed effects to control for baseline differences in outcome levels across agencies, as we discuss in Section 4. Our findings are robust to restricting the sample to include only treated agencies and solely exploiting variation in the timing of officer deaths, which provides reassurance that the baseline differences across the treatment and control agencies do not bias the results (see Table A2, specification (2)).

To provide a simple presentation of the time path of crime and arrests and our empirical strategy, Figure 1 plots the raw data around officer fatality events, comparing average outcomes in the treated year to the year prior for treated agencies. While these plots are not adjusted for any covariates or fixed effects, they accord with the overall pattern of findings in the study. Following the empirical strategy described below, these plots show logged outcomes, while the corresponding figures in levels are shown in Appendix Figure A1.[18]

Panel A confirms a large spike in murder offenses during the month of an officer

[17] In this paper, we exclude murder arrests and murder crimes from index violent crime or arrest sums and measure these outcomes separately. We do this to easily see the effects on murder (which is related to the officer death treatment) separately from other violent crimes.

[18] The log transformation used is $ln(y+1)$ to permit zeros in the outcome.

fatality, reflecting the officer fatality itself, and providing evidence that officer fatalities are measured accurately in the data. The size of the increase in the murder outcome corresponds to one additional death, as shown in Appendix Figure A1. Panel B of Figure 1 shows that total arrests decline in the month of an officer death and month after, with a drop of ≈ 0.1 log points or 10% in the first month. Despite this drop in total arrests, Panels C and D do not appear to show a temporary or systematic increase in violent or property crime. While there is a seeming change in violent crime between one and two months after the officer death in the raw data, we will show that this change does not persist in any of the regression specifications we present, nor does it appear in a levels version of the raw data (Appendix Figure A1).

4 Empirical Strategy

Our empirical strategy exploits the staggered occurrence of officer deaths over time in a difference-in-differences framework. A baseline regression will allow for effects to vary by the time horizon from the date of the incident:

$$\begin{aligned} Y_{it} =& \delta_0 D_{it}^0 + \delta_1 D_{it}^1 + \delta_{2-11} D_{it}^{2-11} + \delta_{12+} D_{it}^{12+} \\ &+ \beta X_{i,yr(t)} + \pi_{i,m(t)} + \theta_t + \gamma_i t + \epsilon_{it} \end{aligned} \tag{1}$$

In our primary specifications, we define our outcomes as $Y_{it} = log(y_{it} + 1)$ to approximate percentage changes and account for zero values for each outcome category, y_{it}; however, we show that our results are robust to other functional forms in Section 6. The dummy variables D_{it}^0, D_{it}^1, D_{it}^{2-11}, D_{it}^{12+} indicate that a department is 0, 1, 2 to 11, and 12 or more months after the occurrence of an officer death, respectively. The coefficients δ_{it}^k, which indicate the time-path of the effect, are the main object of interest.

We include a vector of covariates at the department-by-year level, $X_{i,yr(t)}$ to account for city-level demographic variation (summarized in Appendix Table A1). These controls include city-by-year resident age, sex, and race composition, as well as total population, median household income, poverty rate, and unemployment rate. City-by-month fixed effects, $\pi_{i,m(t)}$, remove all within-city seasonality in the outcome that is constant across years. We also include fixed-effects that vary at the year-by-month level, θ_t, which account for all sample-wide variation in the outcome over time.

Lastly, we include a city or department-specific linear time trend $\gamma_i t$. During our sample period, both crime and arrests are decreasing nationally, and this decline is occurring at different rates for different police agencies. Previous research has documented that locations with greater baseline levels of crime experienced more substantial declines during this time period (Friedson and Sharkey, 2015; Ellen and O'Regan, 2009), suggesting the need to account for cross-city differences in the time path of crime and arrests. We include this set of controls so as to isolate deviations from these downward trends due to line-of-duty officer deaths. Importantly, this set of controls leads to more *conservative* estimates of the size of arrest declines in the short and long-term, because without them, earlier periods of arrests prior to a officer death (contained in D^0_{it}) may be inflated upward. Indeed, we find qualitatively consistent results albeit with larger arrest declines when these controls are omitted (Table A2, specification (5) and Appendix Figure A7).

We consider an officer death event to be any instance where one or more officers in a department died in a particular month.[19] Some cities experience officer deaths at multiple points in time within our sample period. We allow these events enter our specification additively, denote each officer death event by d, and maintain one panel per city:

$$\begin{aligned} Y_{it} = &\sum_d \left(\delta_0 d^0_{idt} + \delta_1 d^1_{idt} + \delta_{2-11} d^{2-11}_{idt} + \delta_{12+} d^{12+}_{idt}\right) \\ &+ \beta X_{i,yr(t)} + \pi_{i,m(t)} + \theta_t + \gamma_i t + \epsilon_{it} \end{aligned} \tag{2}$$

The interpretation of our coefficients δ_k is that they represent the time-path of the effect of the average officer death event in a city (Sandler and Sandler, 2014; Neilson and Zimmerman, 2014). This formulation is equivalent to calculating time period lag variables for each event and then summing these lag variables across multiple events within a police department panel.

A key assumption of our empirical design is that the occurrence of an officer death is not correlated with time-varying shocks to the outcome. A partial test of this assumption is to check that an officer death does not appear to impact an outcome *prior* to the date of the incident. To evaluate this hypothesis, we will also run an event study version of the above

[19]In Appendix Table A2, we show that our results are robust to counting each officer death in a city-month as its own event.

regression, where we include indicators for each month around the date of the incident:

$$Y_{it} = \sum_{d} \sum_{\substack{k \in \{-\underline{T}, \overline{T}\} \\ k \neq -1}} \delta_k D_{idt}^k + \beta X_{i,yr(t)} + \pi_{i,m(t)} + \theta_t + \gamma_i t + \epsilon_{it} \tag{3}$$

To test that our treatment does not have significant pre-trends, we check that the values of δ_k for $k < -1$ are statistically insignificant.

We conduct a number of robustness checks to verify the validity of our results and assumptions of our specification which are detailed in Section 6. These include restricting the analysis to treated cities, estimating the model outcomes in levels and per capita terms, entering multiple officer deaths within a department-month additively, and creating a separate panel for each officer death treatment (vs. each treated city). Additionally, we pay careful attention to issues raised surrounding difference-in-differences event study models in the literature (Borusyak and Jaravel, 2017; Goodman-Bacon, 2018; Sun and Abraham, 2020) and include a number of robustness specifications to address these concerns.

5 Results

Table 2 presents the central results. First, we examine murder crime and murder arrest outcomes, as these outcomes capture the study treatment, or the felonious death of an officer in the field. We test murder outcomes separately from violent crime outcomes (excluding murder from these violent crime/arrests) so that we can easily measure their relationship to an officer death treatment. The top panel shows that the death of an officer while on duty coincides with a 32% increase in reported murder and a 12% increase in murder arrests. We interpret this concurrent increase in murder as being a function of the officer death itself; in fact, when this model is estimated in levels, the first month coefficient on reported murder is statistically indistinguishable from 1 (Appendix Table A2, specification (10)), corresponding to the treatment of the officer death itself.

Policing activity is highly responsive to an officer death in the short-term. Total arrests decline by 7.3% in the month of an officer death, and these declines are similar in percentage magnitude across index (7.1%), non-index (7.6%), and "quality of life" arrests (6.5%). The magnitude of these coefficients are roughly halved in the second month after the officer death and are smaller and insignificant three to twelve months (the long-term effect) after the incident. Traffic stop declines are large but insignificant in the first month and are 27% in the second month following an officer death, while the long-term effect is a smaller and statistically insignificant 8% decline. Relative to the treatment group mean, this two month decline in arrests corresponds to an average decrease of 3,174 traffic stops and 112 arrests, of which 13 arrests are for index violent and property crimes, 58 arrests are for "quality of life" offenses, and 38 arrests are for other non-index offenses in each treated city.[20] Collectively, this pattern of results shows that police reduce their enforcement activity following an officer death over the short-term and that this reduction is driven by a decline in enforcement of less serious offenses.

How does this sizable reduction in arrests affect crime outcomes? The third panel of Table 2 shows that crime and community activity *does not* increase as a result of this reduction in enforcement. Reported violent and property crime show no change within a year of an officer death. Our estimates imply that we can rule out increases in index crimes of more than 3.4% (3.6%) in the month of an officer death (month after) with 95% confidence. Over the longer-term, the estimates imply that we can rule out a 2.9% increase in index crime. The pattern of findings shows that a reduction in police enforcement of lower level offenses does not result in an increase in criminal activity.

Our finding of null crime effects from a marginal reduction in arrests is new to the economics literature on policing, and it is therefore useful to benchmark our estimates to prior work. To do so, we convert our estimates into elasticity form by dividing our violent and property crime coefficients by the total arrest coefficient for period 0.[21] Our property and violent crime elasticity estimates are not significantly negative, -0.12 for property crime and 0.48 for violent crime, and do not statistically differ from 0. Figure 7 shows that these elasticities are notably less negative than estimates given by the extensive literature on police manpower, which has generally found large and significant reductions in crime from increased police employment (e.g. Evans and Owens, 2007; Chalfin and McCrary, 2018; Weisburst, 2019; Mello, 2019; Chalfin et al., 2020). These elasticity comparisons serve to emphasize that our null results for crime are small relative to the expected increases from a comparable percent decline in manpower.

[20] The sub-category arrest counts are calculated from the coefficients on each arrest type and therefore do not sum directly to 112.

[21] The associated standard errors are constructed with the delta method: $var(Elasticity) = var(\beta_{crime})/\beta^2_{arrest} + var(\beta_{arrest}) * \beta^2_{crime}/\beta^4_{arrest}$.

Next, we investigate changes in 911 calls for service. As discussed in Section 2, this outcome is a function of crimes that occur and victim decisions to report these crimes but is not a function of police enforcement. This "less filtered" proxy for criminal activity also does not increase after an officer death. Instead, our point estimate for the short-term 911 call response is close to zero and slightly negative. Here, we can rule out a greater than 1.9% (3.7%) increase in 911 calls in month 0 (month 1) and a 2.8% increase over the remainder of the year after an officer fatality.

Lastly, we find that the number of fatal traffic accidents does not change following an officer death. While enforcement of traffic offenses has been shown to affect traffic offending (DeAngelo and Hansen, 2014; Goncalves and Mello, 2017), existing studies primarily focus on state highway patrols, which play a larger role in traffic enforcement than municipal police forces, which are the focus of this study. The traffic fatality outcome has the advantage that it is a function of traffic offenses and is a proxy for reckless driving but is not a function of either victim reporting or police reporting, as nearly all fatal traffic accidents are reported. Despite the large decrease in traffic stops following an officer death, the number of fatal traffic accidents does not change. Here, we can rule out increases in traffic fatalities of more than 5.0% within the first month, 3.3% in the second month, and 0.08% in the remainder of the year, with 95% confidence.

6 Robustness and Alternative Hypotheses

6.1 Robustness Tests

We conduct several robustness checks to scrutinize our results. We also directly consider alternative explanations for our pattern of findings.

First, we confirm that our results are not driven by time-varying shocks to crime which are correlated with the likelihood of an officer death. Figures 6 plots the event-study coefficients at the month level around the month of an officer death separately for violent and property crimes. The coefficients provide visual evidence that there is no change in crime that precedes an officer death. In Figure A2, we plot event study figures for each sub-category of index crimes and continue to find no evidence of pre-period changes in crime. Moreover, Figure 4 clearly displays a singular increase in reported murder that coincides with our treatment event of a felonious death of an officer and no pre-period changes in murders.

In Appendix Figure A4, we re-estimate the model dropping one treatment city at a time and plot the distribution of results. This exercise confirms that the estimates are not driven by outlier observations, as the total range of estimates are substantively close to the model estimate. Moreover, all of the alternative estimates are well within the confidence intervals implied by the baseline model.

Next, we randomize the timing of officer deaths among treated agencies (holding the number of deaths per agency fixed) and re-estimate the model 100 times using these randomized placebo treatments in Appendix Figure A5. Our model estimate for the first month decline in arrests lies well outside the distribution of estimates in the placebo distribution, confirming that the results we find are actually a function of the treatment and are unlikely to be driven by chance.

Appendix Table A2 includes a number of alternative specification tests, all of which find similar results to our preferred specification. We confirm that the estimates are similar when we restrict the sample to treated cities (2). Our estimates are robust to an alternative model that constructs a panel for each officer death treatment, rather than a panel for each city (3), and the results are also similar when we consider multiple officer deaths from the same event additively (4) rather than as a single event. Likewise, restricting the data to a common sample to agencies that have both crime and arrest data in (5) produces similar results.

Next, we include a specification that removes the city-specific linear time trend from the model (6). As discussed above, the size of the arrest declines are larger in this specification given the fact that crime and arrests are decreasing in this period at different rates across locations, and failure to adjust for these trends will increase the levels in the early pre-period reference group. We show in Appendix Figure A7 that the event study estimates without linear time trends look similar and continue to not have significant pre-period coefficients. The results are also similar when excluding the city-by-calendar month fixed effects from the model which adjust for seasonality in outcomes that may differ by department (7). In specification (8), we show that the results are robust to adding state-by-year fixed effects to the model, which flexibly control for state-level policy changes. To address the possibility that there could be unobserved trends in violence towards police at the city-by-month level, we control for monthly variation in assaults against officers that result in injuries in (9); these specifications compare the impact of an officer death holding fixed the number of assaults against officers. Further, excluding arrests for driving under the influence (DUI), the single offense which we show the strongest arrest decline (see Section 7.4 below) does not change the pattern or significance of the results in (10).

We find similar results when we use raw data that has not been cleaned for outliers or other errors (11) (See Appendix A3 for details on outlier cleaning). The results are similar when using counts of arrests and crimes as outcomes (12), though the standard errors are substantially larger and the estimates of the arrest decline are thus no longer significant. The results are robust to a per capita model (13) and an inverse hyperbolic sine model (14).

Recent research documents potential issues with the standard difference-in-differences design and suggest modified specifications, and we consider the robustness of our estimates to these approaches. To address the issue that time fixed effects are partly identified by treated agencies (Borusyak and Jaravel, 2017; Goodman-Bacon, 2018), we re-weight the data to increase the importance of untreated cities (untreated city weight=1000, treated city weight=1). Doing so effectively removes treated cities from the estimation of time fixed effects, and our results are unchanged (15). Sun and Abraham (2020) show that event study designs in the presence of treatment effect heterogeneity can produce estimands for each event-time coefficient that are contaminated by coefficients for other time periods. To address this concern, we present their estimator in (16), which explicitly constructs each event-time estimand as a positively-weighted average of cohort-specific treatment effects. We also present a graphical version of their approach with pre-period coefficients in Appendix Figure A6. This approach does not change our conclusions, though their specification does require treating each line-of-duty death as its own panel. To apply the logic of their approach to our baseline data structure with summed events within each city panel, we estimate a specification in (17) where the untreated agencies and all treated agency pre-periods are overweighted (untreated city weight=1000, treated city pre-period weight = 1000, treated city post-period weight=1).[22] Our results remain unchanged using this approach.

[22] A treated agency's pre-period is set as the months before any line-of-duty death within our sample.

6.2 Do Officer Deaths Only Impact Arrests?

We argue that the officer line-of-duty deaths we study have a direct impact on arrest activity but do not impact any other feature of the criminal environment, allowing us to infer the impact of arrests on crime. Akin to concerns about crime increasing following a high-profile civilian death at the hands of police, we might be concerned that an officer death itself directly causes civilian criminal activity or victim reporting to change. In particular, it might be the case that civilians fear that they will face a stronger punitive response after an officer death and are consequently deterred from offending. Any decline in offending resulting directly from the reaction to an officer death could mask an increase in crime resulting from the reduction of arrests, leading to a biased conclusion about the impact of arrests on crime. To address this concern, we ask whether cities with no arrest declines actually experience a *reduction* in crime, as the above story would suggest. In Section 7.1 below, we split the sample by the size of arrest declines in treated cities. We observe a flat relationship between the magnitude of arrest decline and level of crime change, and we do not see declines in crime for departments with no arrest declines, corroborating our claim that an officer death does not directly impact offending.

A related concern is that police may not only reduce arrests but also increase use of force following a line-of-duty death, consistent with research conducted in single jurisdictions (Holz et al., 2019; Legewie, 2016). We examine this question using national data on civilians killed by police from the UCR Supplemental Homicide Report and the crowd-sourced data resource, *Fatal Encounters*, in Table A5. We do not find evidence of any significant change in the number of civilians killed by police following a line-of-duty officer death using our national sample. In Section 8 below, we utilize a case study in Dallas, TX and confirm with richer data that use of force does not change after the officer death.

6.3 Changes in Crime Reporting

The majority of reported crimes initiate with civilian calls to the police. Victims could be more apprehensive about reporting crime incidents following an officer death, leading to a downwards bias in our estimates of the crime impact. Here, we appeal to evidence of community or crime activity discussed in Section 1 and 5. Most importantly, the Google Trends analysis suggests that civilians are relatively unaware of officer deaths when they occur and as a result will be less likely to respond to these events (Figure 2). Additionally, our measures of 911 calls and traffic fatalities show that complainant reports of offenses and driving offenses do not appear to change substantially after an officer death. In particular, traffic fatalities, which are not a function of victim reporting, do not change in the wake of an officer death.

Another possible explanation for why we find no increase in crime after an officer death is that police not only reduce the number of arrests that they make but also reduce the number of crime reports that they file. In several cases, police have some discretion over which victim complaints are officially filed as criminal incidents. If officers are less likely to file criminal reports after a peer officer death, the estimates of changes to reported crime could be biased downward. Indeed, a large literature in criminology has highlighted concerns about the potential for crime reports to be manipulated by changes in officer reporting standards (Bayley, 1983; Marvell and Moody, 1996; Levitt, 1997; Mosher et al., 2010). While we show in Section 5 that the total volume of 911 calls does not increase in the period after an officer line-of-duty death, we can measure changes in officer reporting directly among the large share of cities in our 911 data that record whether a call results in a criminal incident report being written. We are therefore uniquely able to address whether changes in police reporting are biasing our estimates of a crime effect, which we do in Table 2. We find that this conversion rate is unaltered by an officer death on average, suggesting that officers do not respond to these events by reporting fewer criminal incidents. Our estimates are quite precise and can rule out a greater than 0.9% decrease in the reporting rate, off a base of 20%. This test provides greater confidence in the null effects we identify for reported index crimes using the FBI UCR data.

In addition to providing direct information on police reporting practices, our 911 data cover a larger range of crimes than the UCR crime reports. The fact that we continue to find no impact of an officer line-of-duty death and resulting arrest reduction on this broader indicator of crime indicates that we are not missing impacts on lower level offending.

6.4 Alternative Mechanisms for Arrest Decline

An alternative explanation for the *channel* of the de-policing response is that the decline in arrests is attributable to the direct effect of the incapacitation of a single officer resulting from the death and the corresponding loss in manpower, rather than a change in the behavior of fellow officers. Similarly, there is the possibility that our arrest decline is due to fellow officers taking leave because of their colleague's death.

If either scenario were the case, our results would partly reflect a decline in officer manpower rather than a broad behavioral change in arrest enforcement intensity. However, these alternative stories are implausible given the size of the treatment effects we observe. If we make the conservative assumption that half of the officers employed in a police department are patrol officers that regularly make arrests, the average officer in our treated cities makes 3.2 arrests per month. In contrast, the first month coefficient in our models implies an average decline of 70 arrests, or roughly equivalent to 22 officers making zero arrests in this focal month. Even if the officer who died was exceptionally active in making arrests, it is very unlikely that their loss is driving the results that we find, nor is it likely that 22 officers would reduce their arrest activity to zero after a colleague's line-of-duty death.

A related hypothesis is that, in the wake of an officer's death, patrol officers are re-routed to work on apprehending the perpetrator of the murder, leading to a decline in their arrest activity. Figure 4 and Table 2 document an increase in murder arrests in the month of the officer death but no significant change in the months after. We take this finding to suggest that the typical investigation and arrest of the suspect occurs within the first month, reducing the plausibility of the second-month effect being driven by officer time reallocation.

In our Dallas case study (see Section 8 below), we can directly inspect additional related alternative hypotheses and mechanisms for the results. These include measuring changes in staffing at the time of an officer death and the potential for short term *increases* in patrol presence due to officers not spending time processing arrests. Our findings of negligible changes in officer presence in this case study serve as an additional validation that changes to officer presence are not generating the arrest decline or resulting lack of crime response. We therefore conclude that the more plausible story is a behavioral response by officers.

Lastly, we can learn more about the way that officers respond to a peer fatality by separately estimating responses to officer deaths that are caused by accidents rather than felony homicides. Appendix Table A7 estimates the full set of results for accidental officer deaths that occur on the job, which are nearly all a result of car accidents. Similar to the murder offense result in Table 2, this analysis confirms that fatal traffic accidents spike in month 0 by 15%, attributable to the officer fatality. Similar to felonious officer deaths, traffic stops sharply decline after accidental officer deaths, but in this setting there is no significant decline in arrests. Here as well, there is also no increase in crime. It appears that officers respond to accidental peer deaths by reducing their exposure to risky traffic settings by reducing traffic stops, but that there is no increased fear or distress related to making arrests. This weaker response implies that a peer fatality caused by a felony incident is more impactful in inspiring a behavioral response.

7 Heterogeneity

In this section, we consider how our arrest and crime impacts vary by different dimensions of the treatment and outcomes, and in particular, we ask whether the null finding of no increase in crime persists for subsamples of cities with particularly large or sustained declines in arrests.

One interpretation of our baseline findings is that the observed arrest declines are not sufficiently long in duration or large enough in size for potential offenders to notice a change in enforcement. As a result, one possible concern is that our estimates are not informative for a longer-term or larger change in enforcement that may be salient for offenders. We argue that low salience of enforcement changes could be a general feature of the environment and arrest enforcement in general. As documented by Lochner (2007), individuals are generally not aware of the probability of sanction from offending and are even less aware of changes in that probability. As a result, it could be the case that even a permanent change in enforcement would not be explicitly noticed by potential offenders.

Separate from the question of generalizing our estimates to larger or longer declines in arrests, we argue that our observed declines are already quantitatively meaningful. If all U.S. departments reduced their arrests for two months per year by the amount that we observe after line-of-duty deaths, this decline would translate to 92,448 fewer arrests per year.[23] These foregone arrests mean that affected individuals do not face criminal sanctions or their collateral consequences, which can include labor market penalties and the financial burdens of criminal justice fines or fees. The point estimates on crime likewise imply that there would be a national annual decrease of 2,107 violent crimes and an increase of 8,657

[23] This back-of-the-envelope calculation uses crime and arrest counts from the FBI UCR national statistics for 2019, see `https://ucr.fbi.gov/crime-in-the-u.s/2019/crime-in-the-u.s.-2019`.

property crimes, figures which are statistically indistinguishable from zero and comparatively small relative to the arrest decline.

Nevertheless, we will directly examine whether our effects vary by the magnitude or persistence of the arrest decline, two dimensions of treatment that are relevant for more permanent changes in enforcement.

7.1 Size of Arrest Decline and Crime Effect

To investigate variation in effect sizes by magnitude of arrest decline, we first estimate residuals of arrests and crimes conditional on the fixed effects in the model but excluding the treatment indicators, D_{it}. We then calculate the difference between residuals in the month of an officer death, $t = 0$, versus the residual for the month prior to the officer death, $t = -1$, for both the crime and arrest outcomes. These differences in residuals approximate the single month effect of an officer death on both arrests and crime rates in each city. We estimate a local linear regression between these two residuals, and we construct our 95% confidence intervals using a bootstrap procedure.[24]

Figure 8 plots the residual change in arrest against the residual change in crime, allowing us to trace an "arrest to crime curve." We plot binned values of the residuals overlaid with a local linear regression estimated using the full sample of residuals. The top figure presents the crime residuals for the first month and shows a flat relationship with the size of an arrest decline. In a range of a 20% decline to no change in arrests, the standard errors of the local linear regression reject crime increases of more than 3.5% with 95% confidence. In Panels B and C, we plot the crime residuals for the entire year after the officer death, and we similarly find a flat relationship with no evidence of crime increases for any magnitude of an arrest decline.

7.2 Length of Arrest Decline and Crime Effect

How informative is our baseline null finding for answering how crime would respond in an environment where police permanently reduce their enforcement against low-level offending? Though a two month reduction in low-level arrests is certainly not a permanent change, the

[24]Standard errors (dashed lines) are produced by reproducing the results through block bootstrapping (re-sampling police department panels) 200 times and plotting the 5th and 95th percentile of the local linear regression lines from these iterations.

literature on the impacts of police presence has documented responses to changes in policing at much shorter time horizons. Di Tella and Schargrodsky (2004), Klick and Tabarrok (2005), and Draca et al. (2011) analyze the impact of rapid increases in police presence in small geographic regions after a terrorist attack or heightened threat of an attack, and these studies all estimate reductions in criminal activity that are detectable within a week of the increased police presence. More strikingly, Weisburd (2021) finds in Dallas, TX, that reductions in the presence of police officers in a police beat lead to increases in car theft, and the crime response is within an hour of the police reduction. This previous literature highlights that, while our baseline estimates do not speak directly to a permanent change in arrest activity, they can rule out short-term responses that are commonly observed for changes in *police presence* and thus are informative about differences in the crime elasticity with respect to manpower versus arrest activity. Nevertheless, we will investigate this issue directly in our data.

To examine heterogeneity in effect sizes by duration of arrest decline, we take our residuals calculated in Section 7.1 and calculate for each city the number of consecutive months after an officer death where the residual is lower than the residual for the month prior to the death. We bin arrest decline durations into groups from 0 months to 5 or more months. We then plot the post-fatality crime residual for each city, separately by length of the arrest reduction, as shown in Figure 9. For each duration of arrest effect, we calculate the 95% confidence interval of the average crime residual for a particular group using a bootstrap procedure.[25]

The top panel presents the crime impact for the first month. We see that the average residual crime effect is close to zero for all time horizons. This finding is perhaps not surprising, since a sustained arrest decline is not likely to lead to a markedly different impact in the first month. In the bottom panel, we plot the crime residuals averaged over the entire year after the officer death. Over this longer time horizon, we continue to find average effects that are small and statistically insignificant for all durations of arrest decline.

Because we are stratifying our sample by an outcome of the treatment rather than

[25] Similar to our arrest-to-crime curve estimation, we utilize a block bootstrap, re-sampling police department panels in 200 iterations. In each iteration, we re-calculate the number of months with residuals lower than the pre-period month and re-group departments into duration bins. We then calculate the average crime residual for each group, $\hat{\mu}^b$. We use quantiles of $\hat{\mu}^b$ to determine the 95% confidence interval (Efron, 1982).

using experimental variation in the duration of arrest decline, we do not claim to have identified the causal impact of arrest declines at various durations. Similar caution is needed in interpreting our previous analysis stratifying by magnitude of decline. Consider, for example, that departments who reduce their arrest activity more dramatically or for longer than average may be aware that their city is less likely to respond to this decline with increased crime. However, these results do provide suggestive evidence that there is not a certain magnitude or duration of arrest decline within our sample that does generate a crime increase. We will analyze the issue further in the following section by examining heterogeneity across types of departments and officer fatality characteristics.

7.3 Police Department and Officer Fatality Characteristics

In this section, we explore variation in the arrest and crime impacts of an officer line-of-duty death by the characteristics of the agency and incident. The top left panel of Appendix Figure A8 asks how our primary estimates vary with city-level characteristics, and we find evidence of heterogeneous arrest effects. Significant arrest declines are present for all splits of the data, except for cities with below median arrests per index offense. The arrest decline is more negative in cities with a below-median population, roughly 15%, though the difference between below-median and above-median cities is statistically insignificant. This pattern is also evident when dividing cities by assaults on officers per officer capita, the crime rate, and officers per population. The average arrest decline duration for all groups is between 2 to 4 months, with similarly sized declines across groups.[26]

The top right and bottom panel present how our crime impacts vary by city characteristics, separately for the month of the incident and the year following the incident, and we largely continue to find insignificant impacts. The only significant positive crime effect is for cities with a below median population. One interpretation of this coefficient is that the more substantial decline in arrests among small cities is leading to an increase in crime. However, we do not find significant impacts for cities with few assaults on officers per officer capita, a low crime rate, or few officers per population, nor do we find positive crime impacts when

[26]The average number of months of arrest decline within the first year after an officer death is calculated by first estimating residual arrests, conditional on all covariates excluding treatment. We then count the number of months with lower residuals than the month preceding treatment and average this month duration for each sub-group. Confidence intervals are calculated using the 5th and 95th percentile of each average across 200 bootstrap iterations.

stratifying directly on arrest reductions, as we showed in Section 7.1. Because we are by design considering the significance of several coefficients at once, we interpret this positive significant effect as an artifact of multiple hypothesis testing.

Appendix Figure A9 conducts a similar exercise splitting the treatment events according to observable characteristics of the officer fatality. Again, nearly all characteristics of officer fatalities are linked to significant arrest reductions. There is no difference in the size of reduction for deaths occurring during traffic stops, or by officer age, experience, or gender. The point estimate of arrest decline is larger for white officers relative to black officers and for vehicular assault relative to gunfire, but the differences in these estimates is not significant. Likewise, the arrest decline durations for each group is roughly similar, ranging from 2 to 4 months.[27] However, again, the event groups with larger arrest reductions do not exhibit a pattern of larger crime increases in response.

7.4 Crime and Arrest Sub-Types

Next, we estimate the model separately for each crime and arrest sub-type in the analysis to explore which categories are driving changes in the aggregate outcome sums. Table A3 displays the sub-type results for index crimes and index crime arrests. For index crime arrests, we find significant decreases in robbery arrests and motor vehicle theft arrests. For index crime, we observe a decline in aggravated assaults and an increase in burglaries post treatment; but these changes are not robust to the more flexible event study formulation of the model shown in Appendix Figure A2. Appendix Figure A2 and Table A3 both show that none of the sub-categories of index crime show a significant post-treatment increase.

The sub-category results for "quality of life" arrests and non-index arrests show several categories with large point estimates but few individual categories that are statistically significant for the first or second month. The results suggest that the arrest declines in these categories are driven by large decreases in arrests for drug possession, prostitution, and driving under the influence of alcohol (DUI) (which is classified as a mid-level "non-index" offense) (Table A4). The results imply that over the two month period following an officer death, officers make 23 fewer arrests for drug possession, 2.3 fewer arrests for prostitution, and 13.3 fewer DUI arrests in each treated city.[28] Given that we observe a large reduction in

[27]The decline duration and confidence intervals are calculated using the same approach as the prior figure.
[28]We assume that uncategorized arrests are likely to be for offenses that are not listed as options for

DUI arrests, we explicitly measure the subset of fatal traffic accidents that involve a drunk driver (Table A5). These alcohol-related accidents do not respond to the reduction in DUI arrests associated with an officer death. Likewise, as discussed above, the decline in total arrests persists after excluding DUI arrests (see Table A2, specification (9)).

7.5 Demographics of Arrestees

Another treatment dimension of interest is who is affected by the reduction in arrests that we observe. We investigate whether the arrest declines we observe are concentrated among particular demographic groups of arrestees, by regressing log arrests of particular demographics groups on our treatment, using our preferred specification.

Table A6 (Panel A) shows that we observe arrest declines across all race, gender, and age groups following an officer death in the line-of-duty. While the point estimates vary somewhat across groups, we cannot reject that any of the demographic sub-group declines differ in magnitude from the total arrest effect of a 7% decline. The share of Black arrestees, 36%, and male arrestees, 77%, exceeds their relative population shares of 16% and 49% prior to treatment. This also means that an equivalent percent decline in arrests for these groups corresponds to a more meaningful reduction in arrest disparities for these groups. Overall, the findings suggest a uniform percent decline in arrests across demographic groups, meaning that the treatment cuts across groups rather than being concentrated among arrestees of a particular type.

8 Case Study: Dallas Police Department

We interpret the decline in arrests after an officer line-of-duty death as due to heightened fear of workplace risk or emotional distress. We also argue that other dimensions of policing and the criminal environment are unchanged, allowing us to infer the impact of marginal changes in arrests on crime.

To further test these claims, we study a particular line-of-duty death of an officer working for the Dallas Police Department, where we have collected detailed information on 911 calls, crimes, arrests, and use of force, as well as information on the officers involved in

reporting in UCR. Given the broad number of offense categories available for reporting in UCR, we argue that these arrests are for other low-level offenses.

different forms of police activity. These rich data allow us to assess various dimensions of officer behavior and test whether arrest activity is the primary dimension of behavior that responds to a line-of-duty death.

Dallas is a useful context to study these outcomes as it is a large urban center with a diverse population. Relative to other treated agencies in the sample, Dallas has higher levels of crime and arrests, more police officers and more officers assaulted or killed in the line-of-duty. These differences are likely attributable to demographics; Dallas is substantially larger than the average treated city, 1.2 million versus 255 thousand residents, and has higher rates of poverty and is more racially diverse (see Appendix Tables A8 and A9).

The event we study is an officer death that occurred in 2018.[29] On Tuesday, April 24, officer Rogelio Santander was shot by a shoplifting suspect at a Home Depot store during an attempt to arrest the individual. Officer Santander died from the gunshot injury the following morning; two other officers were critically wounded in the incident. The suspect fled the scene but was detained later the same day.[30]

We examine the impact of this event on policing and crime in Dallas by plotting the time path of weekly activity around the initial incident date of April 24. We plot the same outcome around April 24 of the previous year as a counterfactual absent the officer death. We highlight the unadjusted weekly plots because they are both easy to visualize and display a long time path of effects. In the appendix, we also replicate the analysis for each outcome using daily activity around the event (Appendix Figure A10), as well as a regression discontinuity in time using daily data (Appendix Figure A12). These alternative approaches are useful for zooming in on the local changes around the cut-off and for estimating the significance in any local break in trends around the event.

First, we document a drop in arrests made following the officer fatality, consistent with the broader pattern of results in this study (Panel A of Figure 10). From a pre-period base of around 600 weekly arrests, arrest activity declines to 400 weekly arrests or approximately 33%, a far larger percentage decline than our nationwide finding. This

[29] Our data coverage for Dallas includes the time period 2014-2019. This time period includes the nationally covered shooting of 5 police officers in Dallas in July of 2016. While this event is included in our main national data set, we focus on the April 2018 event as it is more typical of the officer deaths in our sample, and was not highly publicized in the media. Further, we do not include 2016 as an additional pre-period year because the July 2016 event lies within the comparable sample window of calendar months we use in our analysis of Officer Santander's death in April 2018.

[30] https://www.odmp.org/officer/23665-police-officer-rogelio-santander-jr

reduction in activity persists for 9 weeks but appears to return to the pre-period average afterwards. Interestingly, the reduction in arrests is city-wide rather than concentrated in the Northeast division where Officer Santander worked (see Appendix Figure A11). Next, we confirm the patterns observed in the nationwide analysis; we see no systematic change in the frequency of calls to the police, total crime reports, or the probability of reporting a crime conditional on receiving a 911 call (Panels B-D of Figure 10).

Compiling the data at the daily level produces strikingly similar patterns in arrest, call, and crime outcomes at the daily level (Panels A-D of Appendix Figure A10). The decline in arrests clearly begins on the focal date of the officer death. Likewise, the regression discontinuity in time (RD) shows a sharp 44% decrease in daily arrests (Panels A-B of Appendix Figure A12). While 911 calls are unchanged around the date of the incident (Panels C-D of Appendix Figure A12), the RD estimates do indicate a small but significant 5-9% decline in daily crime around the initial date of the incident. This decrease appears to be entirely driven by a local decrease in the share of 911 calls that result in a crime report being written, likely due to an officer behavioral response (Panel E-H of Appendix Figure A12). While these regressions show a modest break in crime outcomes, this change returns to pre-period levels in less than ten days while the much larger arrest decline persists for two months.

Next, we plot the probability of an arrest *conditional* on a response to a call for service, and we observe a reduction in this arrest rate after the officer death using weekly data (Panel E of Figure 10). This finding suggests that some of the decline in enforcement occurs even among incidents reported by civilians rather than solely through reductions in officer-initiated activity. It also provides evidence that the reduction in arrests is a behavioral response by officers, likely from heightened fear, rather than a reduction in manpower from the officer death and from other officers taking time off or being re-assigned. As further evidence on this question, we plot the total number of officers we observe responding to calls for service, a measure of workplace attendance, and find little change around the focal event (Panel F of Figure 10). Likewise, these findings are consistent in both the daily plots and regression discontinuity in time (Appendix Figures A10, Panel E-F, and A12, Panels I-L).

Another possible police response could be to change the allocation of officers within the city, which we investigate by plotting the number of officers responding to calls for service in high crime police beats (Panel H of Figure 10).[31] We find a short-term increase in officer presence in these areas; however, the daily plots of results show that this effect is driven by the concerted response to the actual shooting event of Officer Santander, which occurred in a high crime beat (Appendix Figure A10, Panel F and H). While this adjusted set of allocations could act as its own deterrent against crime (Draca et al., 2011; Weisburd, 2021), this single day and quantitatively small increase in presence is likely not great enough to have an impact on criminal offending. Reassuringly, despite this single day spike in officer presence, the regression discontinuity in time estimates show no significant change in officer presence in these locations around the event (Appendix Figure A12, Panel O-P).

An additional hypothesis we consider is whether officer patrol presence could actually *increase* because officers are spending less time making and processing arrests while on shift. If this is the case, the increase in officer presence could be counteracting the decline in arrests, resulting in no change in crime. We can test this by calculating the average number of officers observed responding to 911 calls per hour, a measure that will capture short term changes in officer presence on patrol. Panel I of Figure 10 shows that there is no increase in this measure around the officer death event, suggesting that this channel is unlikely to be driving the results. Again, the daily plots and regression discontinuity in time estimates reaffirm this flat relationship (Appendix Figures A10, Panel I, and A12, Panel Q-R).

Next, we examine the number of instances of use of force by Dallas officers as a complement to the national analysis using fatal use-of-force incidents (Panel G of Figure 10). We see a similarly flat time path to the year prior, suggesting that officers are not responding to the incident by changing this dimension of enforcement, a pattern that is also confirmed in the daily plots and regression discontinuity in time figures (Appendix Figures A10, Panel G, and A12, Panel M-N).

As a complement to the investigation of effects by arrestee demographics in Section 7.5, we can also explore how arrest declines differ across groups using the Dallas Case Study. Here, we are additionally able to look at effects for Hispanic arrestees and first-time (repeat) arrestees, groups that are not available in the national UCR data. Table A6 shows that all arrestee sub-types experience declines in arrests following the officer death, with percent changes that are in similar ranges as the total decline in arrests. Tests of difference show that the decline in arrests are larger for white arrestees and youth arrestees (where youth

[31] High crime beats are defined as police beats in the top 25th percentile of total crime reports.

is defined as less than 30), and are somewhat smaller for repeat offenders and Hispanic arrestees.

Collectively, the case study we investigate in Dallas provides further confirmation that officers are responding to the death of a peer by markedly reducing arrest activity, that the decline is not driven by any effective declines or changes in manpower, and that 911 calls and crime do not increase. Further, we continue to find that dimensions of enforcement other than arrests are largely unchanged.

9 Conclusion

This study examines the causal impact of reducing police arrest activity on public safety. Using data on over 2,000 police departments between 2000-2018, we find that police respond to an officer line-of-duty death by reducing the number of arrests they make, particularly for low-level offenses. Our research collates data from numerous sources, including information on arrests, reported crimes, 911 calls for service, traffic stops, and traffic fatalities, to provide evidence that an officer death directly reduces police arrest behavior but does not have an independent or direct impact on other dimensions of police or civilian behavior. Critically, we find that these reductions in arrests do not come at the cost of increases in serious crime.

By tracing an “arrest to crime curve” using variation across our treated cities, we do not find a threshold level beyond which an arrest reduction results in a crime increase. Moreover, examining treatment effects that last for differing amounts of time, we do not find evidence that arrest declines which are sustained for longer periods result in crime increases. Because the observed decline in enforcement is concentrated among arrests for low-level offenses, we argue that there could be scope to reduce low-level arrests from current levels without causing meaningful increases in crime. Our results stand in marked contrast to the literature on police manpower, which documents significant reductions in crime from marginal increases in employment. Because of this divergence in findings, our results also provide new insight into this prior work by suggesting that the channel of effect for police employment is likely general deterrence related to police presence rather than increased arrest activity.

Our findings raise important questions for future research. In contrast to our results, some research has found crime-reducing benefits of particular types of enforcement, such as hot spots policing (Blattman et al., 2017) and forms of “focused” deterrence that target small groups of frequent offenders (Braga et al., 2018; Chalfin et al., 2021). More research is needed to provide precise information on which forms of arrests and sanctions provide crime-reducing benefits.

While our analysis benefits from utilizing quasi-experimental variation in police enforcement, we observe relatively short-term fluctuations in arrests, and an open question is how crime responds to longer-term reductions in arrests. Related work on the reclassification of offenses from felonies to misdemeanors in California finds that these changes reduced arrests and had no impact on violent crime, while modestly increasing property crime (Dominguez et al., 2019). Separately, examinations of the decriminalization of marijuana show limited evidence of subsequent crime increases (Adda et al., 2014; Mark Anderson et al., 2013; Chu and Townsend, 2019; Dragone et al., 2019). While these studies offer valuable insights into the crime impacts of their respective changes in enforcement practices, they do not speak directly to the impact of changes in overall arrest activity, and we argue that our study provides the first evidence on this question. As police departments and municipalities may begin to alter their approach to enforcement in the coming years, more research will be needed to understand whether a permanent change in low-level enforcement or decriminalization policies would likewise affect public safety and community trust in police.

A full appraisal of any dimension of law enforcement requires weighing crime reducing benefits alongside the collateral costs on the individuals who are sanctioned, including potential reductions in earnings and employment. The growing chorus of protests against police use of force and misconduct have made clear the dissatisfaction of many with the state of American policing, and recent research has documented the numerous harms of law enforcement overreach. Our study argues that, at least in the context of marginal enforcement of low-level offenses, these harms are unlikely to be justified by crime-reducing benefits.

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Tables & Figures

Table 1: Summary Statistics

	Full Sample			Treated Agencies		
	Mean	S.D.	N	Mean	S.D	N
Murder Outcomes						
Murder Offenses	0.213	(1.519)	465778	2.447	(6.069)	22870
Murder Arrests	0.170	(1.298)	329477	1.589	(4.980)	17638
Policing Activity						
Arrests	156.9	(492.2)	329571	968.8	(1739.0)	17649
Index Arrests	29.6	(97.3)	329564	178.0	(343.0)	17665
Violent Arrests	8.9	(43.0)	329542	63.4	(161.1)	17655
Property Arrests	20.7	(59.4)	329543	114.6	(200.6)	17666
Non-Index Arrests	42.4	(141.1)	329535	271.2	(513.7)	17669
Quality of Life Arrests	85.1	(270.6)	329541	520.2	(942.0)	17659
Traffic Stops	5283.5	(8484.6)	1945	7270.8	(9820.0)	595
Crime and Community Activity						
Index Crimes	139.0	(543.5)	465782	1124.5	(2033.8)	22960
Violent Crimes	17.8	(99.4)	465797	175.3	(393.2)	23000
Property Crimes	121.2	(452.9)	465786	950.3	(1678.8)	22959
911 Calls for Service	15094.6	(17301.5)	5822	27458.4	(21368.7)	2135
Crime Report Rate (911 Calls)	0.19	(0.10)	4483	0.20	(0.11)	1831
Fatal Traffic Accidents	0.24	(1.07)	397404	1.79	(3.69)	21660
Number of Agencies	2048					
Number of Treated Agencies	101					
Total Officer Death Events	169					
Treatments Per City (Treated)	1.67					
Officer Characteristics						
Cause of Death	*Gunfire*: 166		*Vehicular Assault*: 7		*Other*: 5	
Race	*White*: 132		*Black*: 24		*Other*: 22	
Gender	*Male*: 168		*Female*: 10			
Age	37.17	(9.34)				
Experience	11.10	(8.24)				

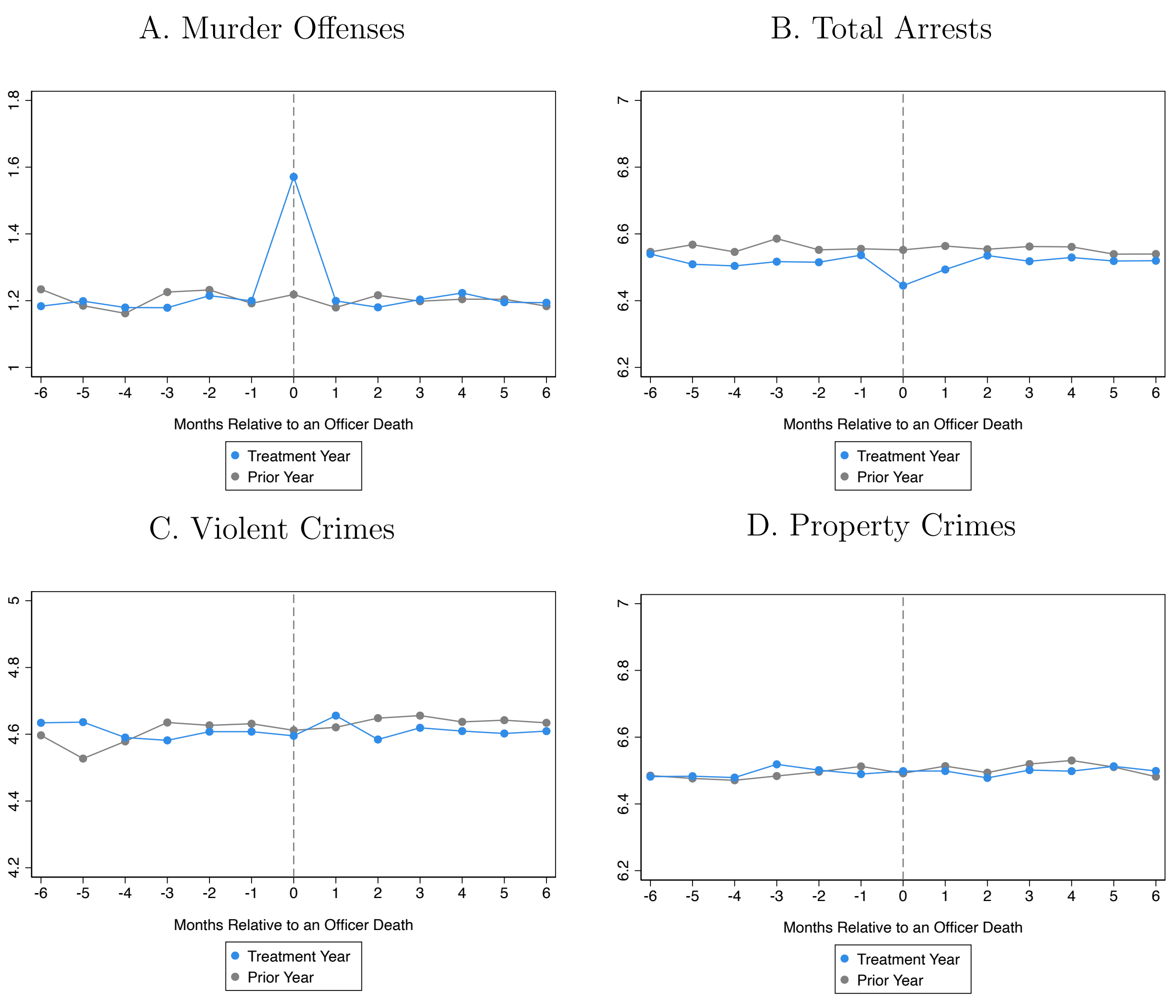
Notes: The number of agencies, number of treated agencies and total officer death events are from the data with crime activity outcomes. For arrest activity outcomes, they are 1450, 78, and 131, respectively. For the traffic stop outcomes, they are 24, 7, and 21. For the traffic accident outcome, they are 1743, 95, and 161. For 911 call outcomes, they are 72, 23, and 59. All arrest and crime subcategories exclude murder outcomes. Violent crimes and arrests include rape, robbery and aggravated assault. Property crimes and arrests include burglary, theft and motor vehicle theft. See Table A3 and Table A4 for the list of crime and arrest sub-types. "Crime Report Rate (911 Calls)" is the share of calls that result in an officer writing a crime incident report. The officer characteristics are from the *Officer Down Memorial Page*. Other causes of death include assault and stabbed.

Table 2: Impact of an Officer Death on Policing and Crime

	1st Month (t=0)	S.E.	2nd Month (t=1)	S.E.	Long-Term (t=2,...,11)	S.E.	Outcome Mean Full	Outcome Mean Treated	N
Murder Outcomes									
Murder Offenses	0.321***	(0.047)	-0.005	(0.037)	0.005	(0.013)	0.21	2.45	465778
Murder Arrests	0.118***	(0.045)	0.054	(0.034)	0.022	(0.025)	0.17	1.59	329477
Policing Activity									
Arrests	-0.073***	(0.024)	-0.043*	(0.022)	-0.003	(0.021)	156.9	968.8	329570
Index Arrests	-0.071**	(0.031)	-0.002	(0.026)	0.007	(0.022)	29.6	178.0	329564
Violent Arrests	-0.078**	(0.038)	-0.042	(0.027)	-0.025	(0.023)	8.9	63.4	329542
Property Arrests	-0.070**	(0.033)	-0.009	(0.034)	-0.002	(0.027)	20.7	114.6	329543
Non-Index Arrests	-0.076***	(0.025)	-0.064**	(0.025)	-0.001	(0.021)	42.4	271.2	329535
Quality of Life Arrests	-0.065**	(0.031)	-0.047	(0.032)	-0.004	(0.030)	85.1	520.2	329541
Traffic Stops	-0.163	(0.111)	-0.269**	(0.123)	-0.086	(0.107)	5288.0	7348.0	1918
Crime and Community Activity									
Index Crimes	0.004	(0.015)	0.008	(0.014)	0.007	(0.011)	139.0	1124.5	465782
Violent Crimes	-0.035	(0.024)	0.014	(0.024)	-0.022	(0.014)	17.8	175.3	465797
Property Crimes	0.009	(0.016)	0.006	(0.015)	0.009	(0.012)	121.2	950.3	465786
911 Calls for Service	-0.018	(0.019)	-0.002	(0.020)	0.002	(0.013)	13692.8	26992.7	6456
Crime Report Rate (911 Calls)	0.001	(0.004)	-0.000	(0.006)	0.004	(0.003)	0.20	0.20	5043
Fatal Traffic Accidents	-0.023	(0.037)	-0.020	(0.027)	-0.016	(0.012)	0.24	1.79	397404

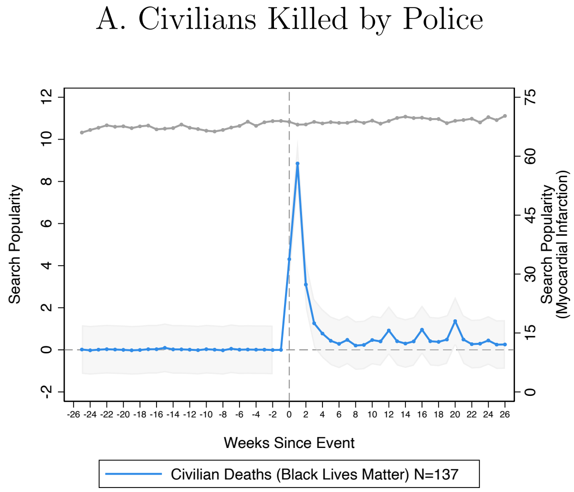
Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Regressions also include a dummy variable for 12 or more months after the occurrence of an officer death. Outcomes are defined as $Y_{it} = log(y_{it} + 1)$ and outcome means are given in levels. Standard errors are clustered at the department level. The number of agencies, number of treated agencies, and total officer death events for crime outcomes are 2048, 101, and 169, respectively. For arrest activity outcomes, they are 1450, 78, and 131 For the traffic stop outcomes, they are 24, 7, and 21. For the traffic accident outcome, they are 1743, 95, and 161. For 911 call outcomes, they are 72, 23, and 59. All arrest and crime subcategories exclude murder outcomes. Violent crimes and arrests include rape, robbery and aggravated assault. Property crimes and arrests include burglary, theft and motor vehicle theft. See Table A3 and Table A4 for the list of crime and arrest sub-types. "Crime Report Rate (911 Calls)" is the share of calls that result in an officer writing a crime incident report. * p<0.1,** p<0.05, *** p<0.01.

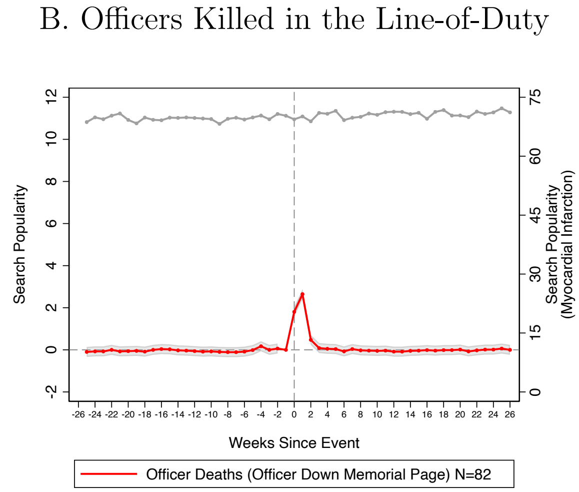
Figure 1: Unadjusted Data Around Events, Log Outcomes



Notes: This figure plots the unadjusted data around the officer death events. Outcomes are defined as $Y_{it} = log(y_{it} + 1)$. There are 160 officer death events in 96 agencies after excluding events that do not have enough periods before and after the event. Violent crimes include rape, robbery, and aggravated assault, but exclude murder, which is shown in Panel A. Property crimes include burglary, theft, and motor vehicle theft.

Figure 2: Google Trends Analysis

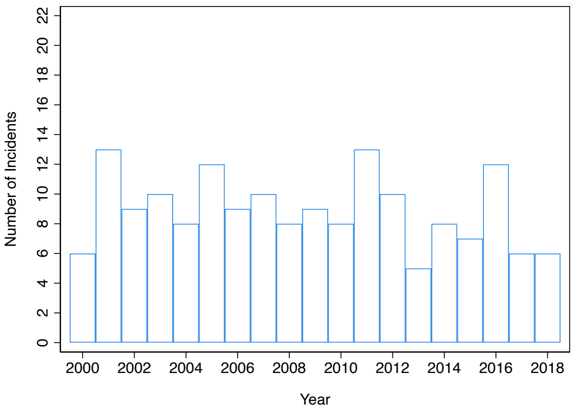




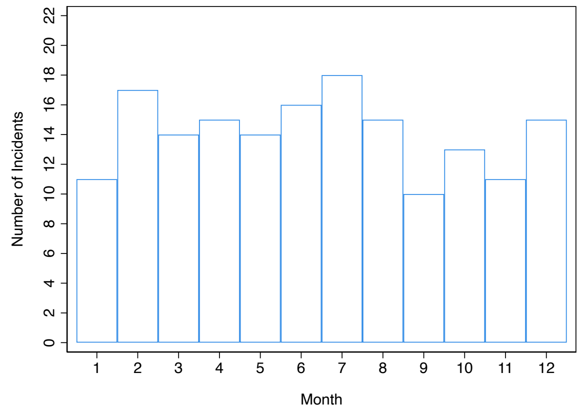
Notes: Each search term is an exact first and last name for the individual. We identify high-profile civilian deaths using a list compiled by *Black Lives Matter*, and identify officer deaths by linking the FBI LEOKA data we use in this project to records from the *Officer Down Memorial Page* to obtain officer names. Each search is centered around the time period of -1. Each search is benchmarked by topical searches for the most common cause of death, heart disease, which is relatively stable in popularity across time and locations within the U.S. Google Trends plots relative search intensity with a maximum search popularity in each search of 100. Relative search intensity is calculated in the year around the event in the state of the event. The gray line plots the search popularity for myocardial infraction. The gray shaded area represents the 95% confidence interval from regressing search popularity on weeks with the individual fixed effect.

Figure 3: Variation in Officer Deaths

A. Officer Deaths by Year

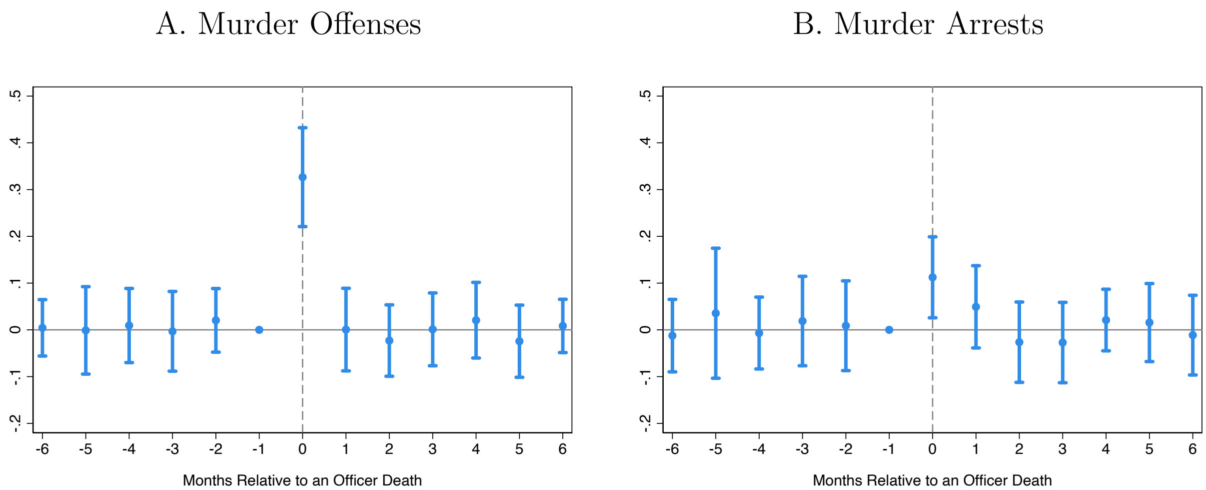


B. Officer Deaths by Month



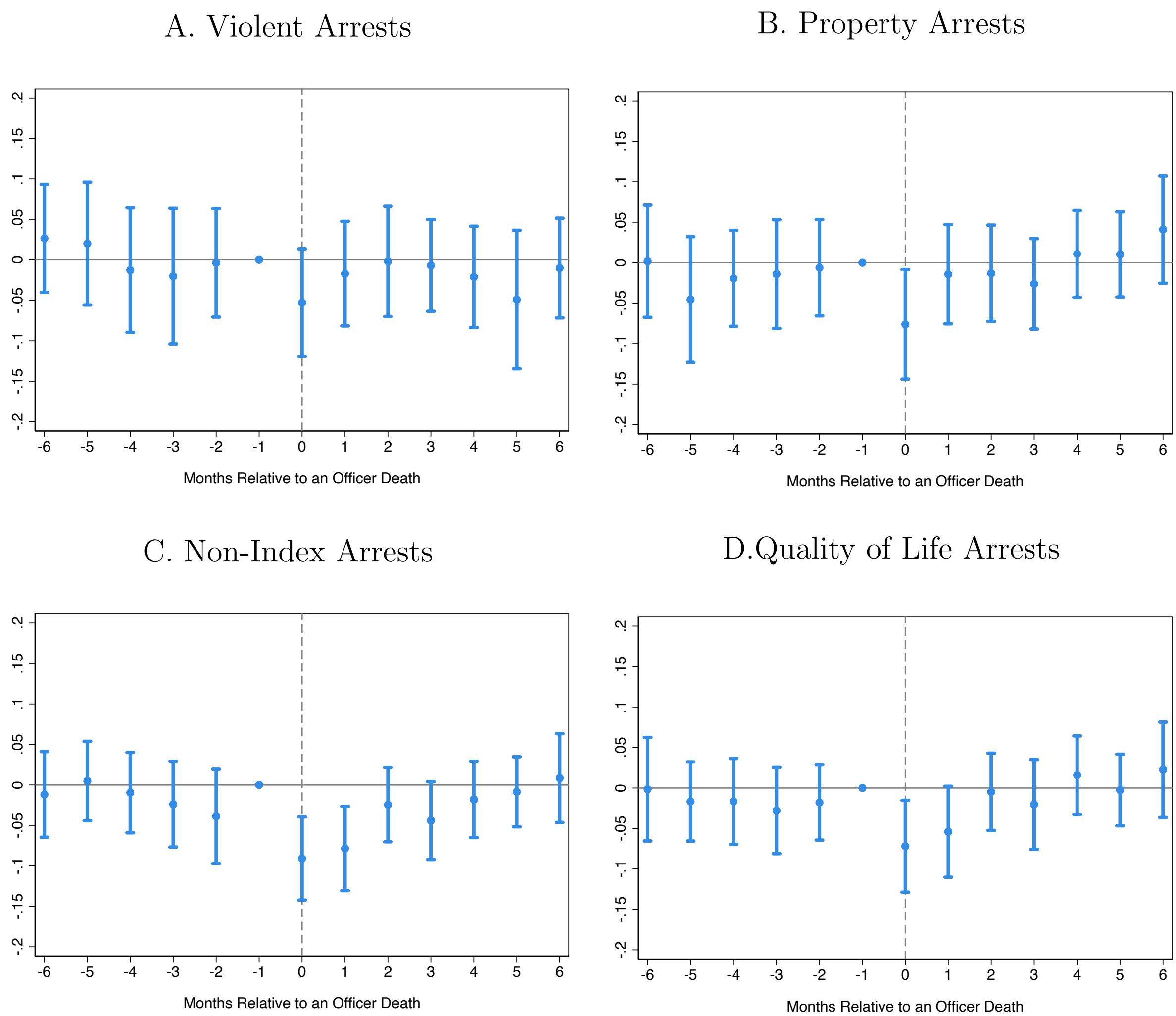
Notes: In 2,048 departments in our sample, there are a total of 169 officer death events in which 178 officers were killed.

Figure 4: Event-Study: Murder Outcomes



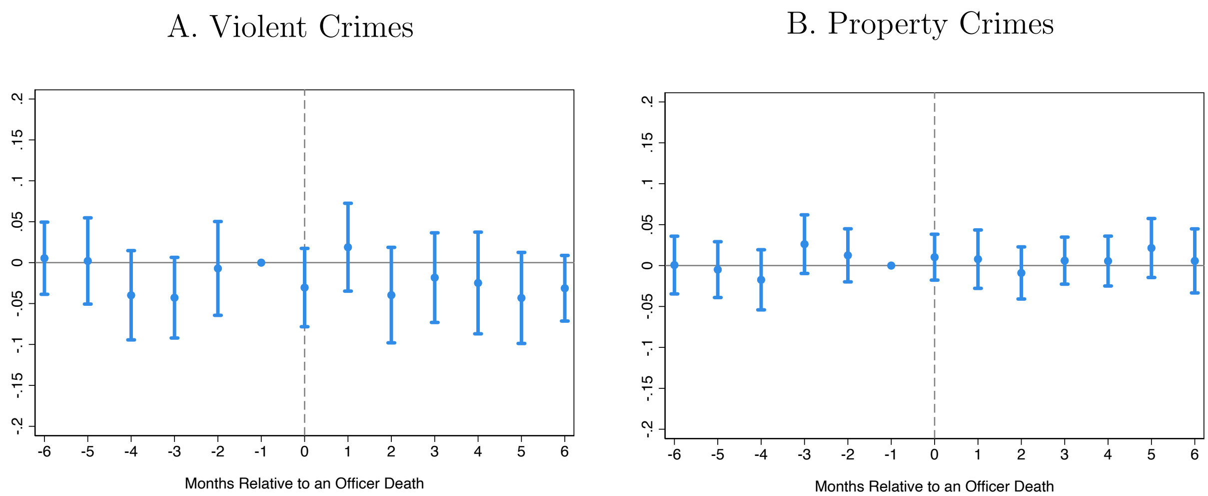
Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Months -6 and 6 include all months before month -6 and all months after month 6, respectively. Standard errors are clustered at the department level.

Figure 5: Event-Study: Arrests



Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Months -6 and 6 include all months before month -6 and all months after month 6, respectively. Standard errors are clustered at the department level. See Table A4 for the list of arrest sub-types. Violent arrests include rape, robbery and aggravated assault. Property arrests include burglary, theft and motor vehicle theft.

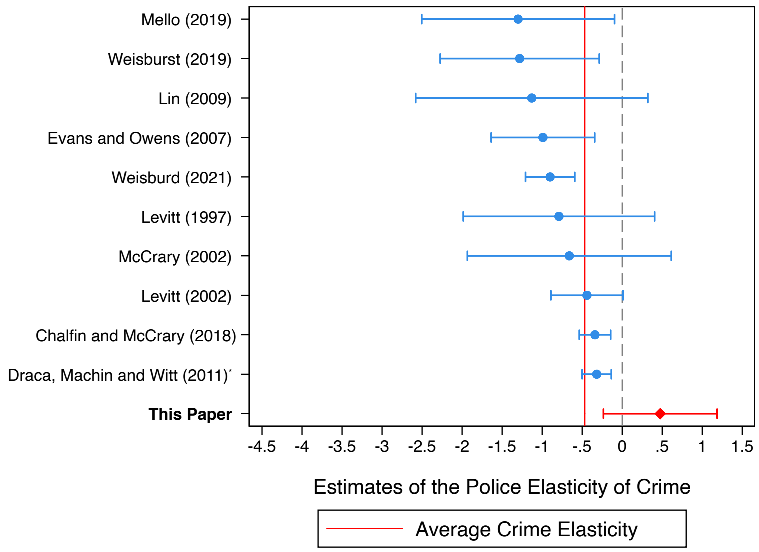
Figure 6: Event-Study: Crimes



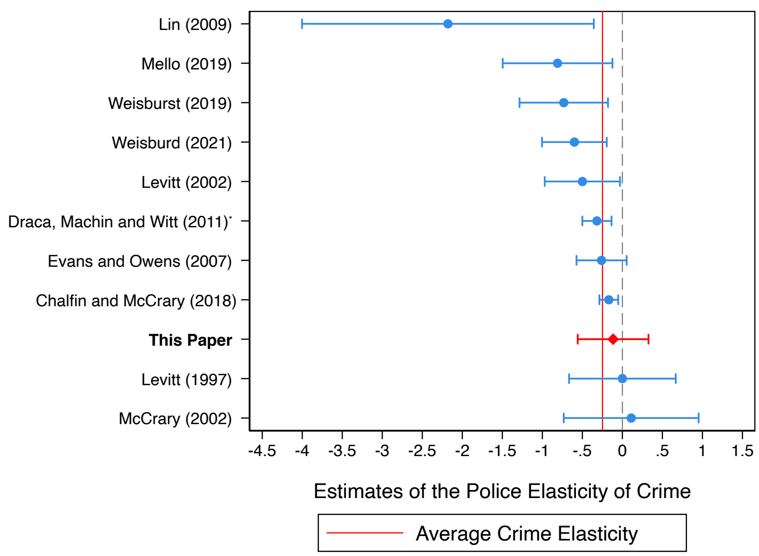
Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Months -6 and 6 include all months before month -6 and all months after month 6, respectively. Standard errors are clustered at the department level. Violent crimes include rape, robbery, and aggravated assault. Property crimes include burglary, theft, and motor vehicle theft.

Figure 7: Estimates of the Police Manpower/Employment Elasticity of Crime

A. Violent Crimes

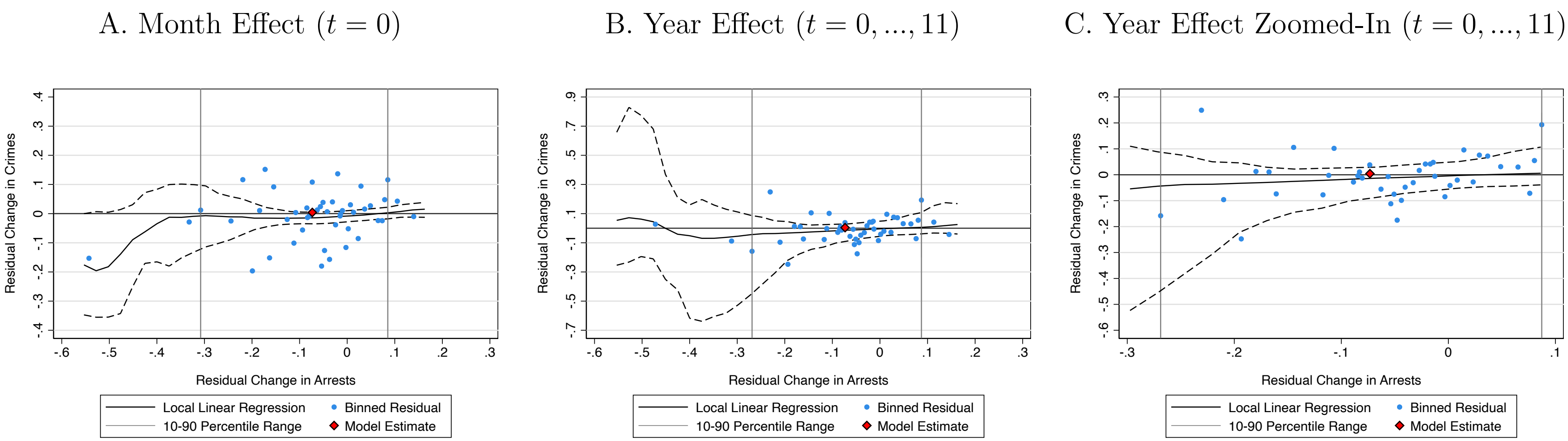


B. Property Crimes



Notes: The estimates of the police elasticities of violent and property crimes are from recent articles. Draca et al. (2011) estimates an elasticity of total crime with respect to police employment. For the Levitt (1997) estimates, we take the elasticity estimates from McCrary (2002) correcting for a coding error in the original paper. The estimates from this paper use the crime elasticity with respect to changes in total arrest enforcement. The red bars represent the average elasticities of all articles excluding our estimates, weighted by the inverse of their variance.

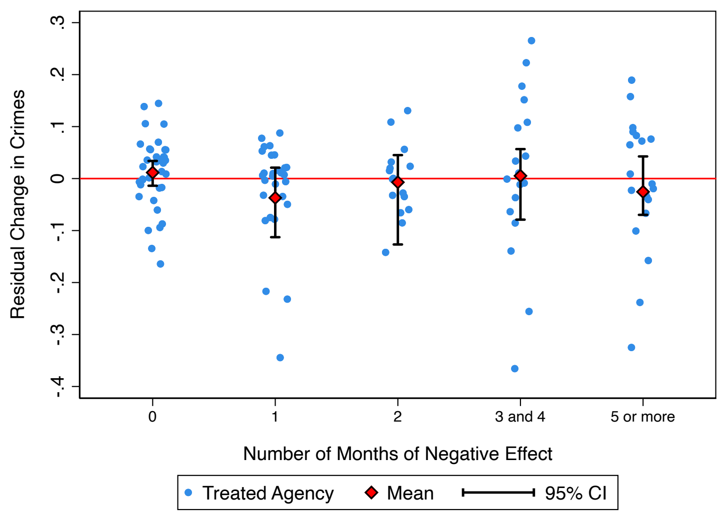
Figure 8: Arrest to Crime Curve



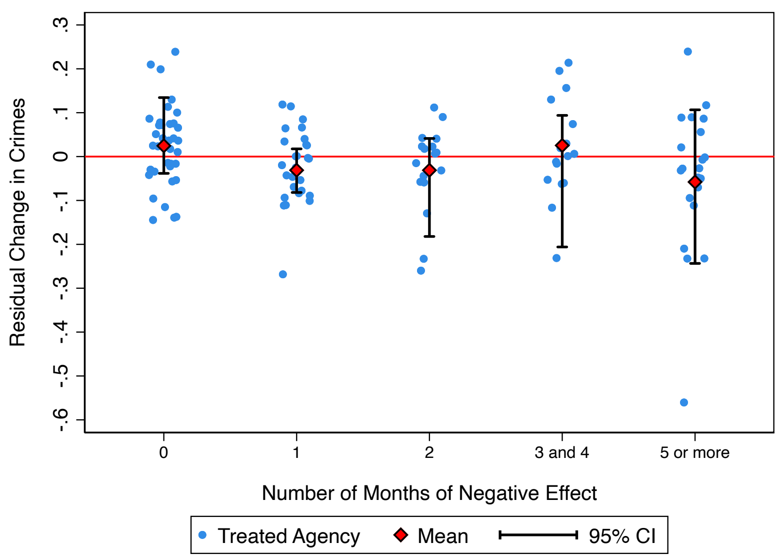
Notes: The residual changes in arrest and crime are estimated conditional on covariates, a department-specific linear time trend, department-by-calendar month and year-by-month fixed effects and differenced relative to the month prior to a line-of-duty death. The x-axis on all plots shows the residual change in arrests in the month of an officer death. Figure A shows the residual change in crime in the month of an officer death. The Year Effect plots the average monthly residual change in crimes in the year following the officer death event. Each plot has 50 binned values of the residuals. Residuals that are below 5th percentile or above 95th percentile are dropped from the plots. Standard errors (dashed lines) are produced by reproducing the results through block bootstrapping (re-sampling police department panels) 200 times and plotting the 5th and 95th percentile of the local linear regression lines from these iterations. The gray bars represent the 90-10 percentile range.

Figure 9: Crime Impact by Length of Arrest Decline

A. Month Effect ($t = 0$)

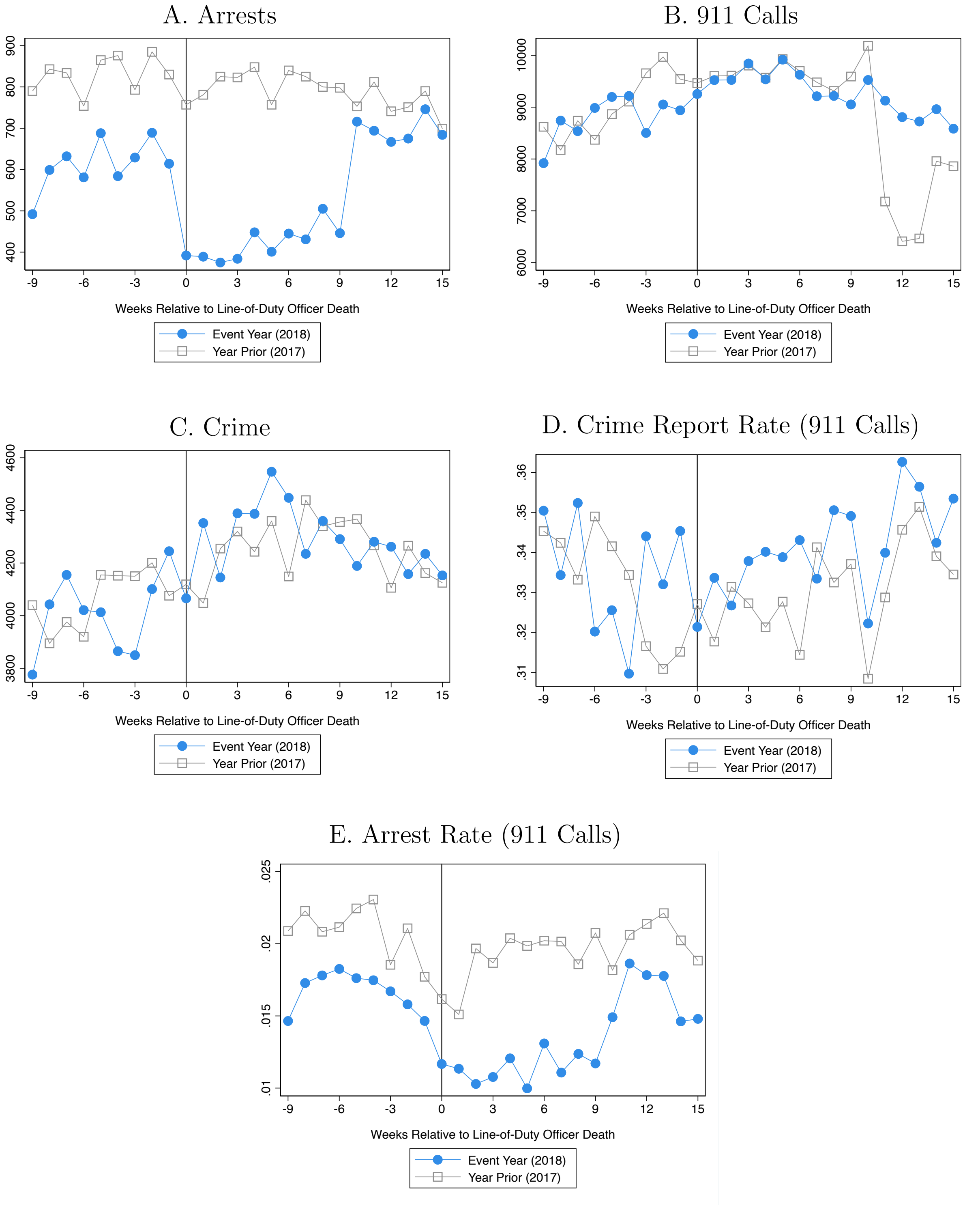


B. Year Effect ($t = 0, ..., 11$)



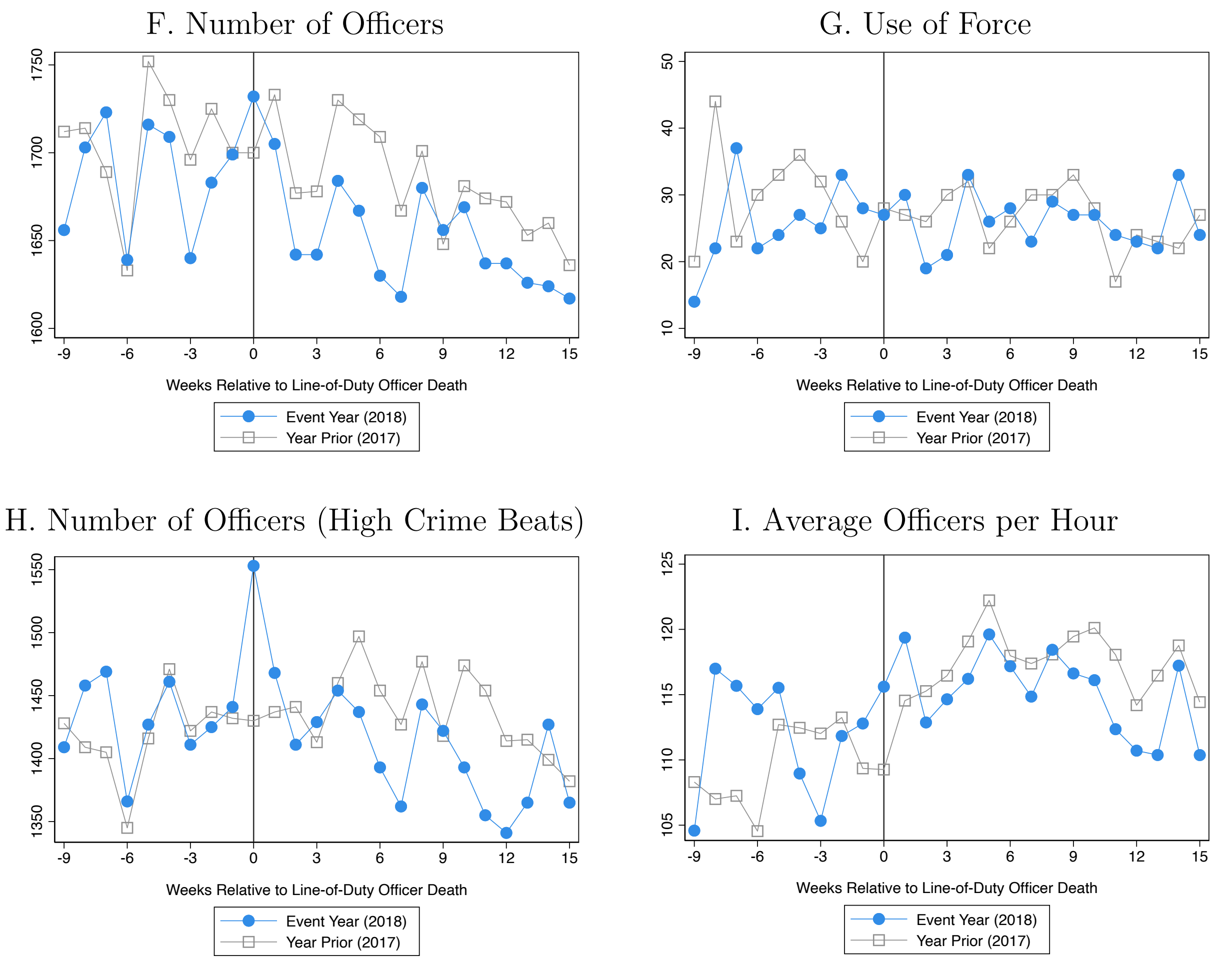
Notes: The residual changes in arrest and crime are estimated conditional on covariates, a department-specific linear time trend, department-by-calendar month and year-by-month fixed effects and differenced relative to the month prior to a line-of-duty officer death. The length of arrest effect (x-axis) is determined by the number of consecutive months where the department's estimated arrest residuals are more negative than the residual for the month prior to the line-of-duty officer death. Each plot shows the treated department's values of the residuals, during the month of the officer death, or the average effect for the year following an officer death. The gray bars represent the 95% confidence interval for each duration of arrest decline calculated using a bootstrapping approach with 200 replications. The bootstrap re-samples police departments and recalculates the arrest decline duration as well as the corresponding residual change in crime for each bin in each iteration.

Figure 10: Case Study in Dallas, Texas



Notes: This figure plots outcomes using data from the Dallas Police Department around the date of the shooting of Officer Rogelio Santander on April 24, 2018. Data is plotted using calendar weeks, with week 0 containing the shooting event. Data from the Dallas Police Department were obtained via open records requests to the department. Crime is measured when any official crime report is logged, and is not restricted to serious offenses (Panel B). The crime report rate is the share of 911 calls that result in a crime report being written by a responding officer (Panel D). The arrest rate is the share of 911 calls that result in an arrest (Panel E).

Figure 10: Case Study in Dallas, Texas (Continued)



Notes: This figure plots outcomes using data from the Dallas Police Department around the date of the shooting of Officer Rogelio Santander on April 24, 2018. Data is plotted using calendar weeks, with week 0 containing the shooting event. Data from the Dallas Police Department were obtained via open records requests to the department. Use of force is calculated using "Response to Resistance" data and officer shooting data published by the Dallas Police Department (Panel G). Panel F and H plot the number of officers observed responding to any 911 call in the whole city and high crime beats, respectively. High crime beats are defined as beats in the top 25th percentile of total crime reports.

A1 Appendix Tables & Figures

Table A1: Summary Demographic Characteristics

	Full Sample			Treated Agencies		
	Mean	S.D.	N	Mean	S.D	N
Characteristics of Cities						
Number of Police Officers	74.2	(327.3)	38912	604.7	(1310.5)	1919
Number of Officers Killed by Felony	0.005	(0.081)	38912	0.093	(0.323)	1919
Number of Officers Assaulted	10.3	(47.7)	38912	83.0	(180.6)	1919
% Black	8.4	(13.2)	38912	16.0	(18.8)	1919
% Hispanic	15.6	(20.0)	38912	21.0	(20.0)	1919
% White	68.7	(24.3)	38912	54.2	(23.6)	1919
% Male	48.6	(3.3)	38912	48.8	(1.9)	1919
% Female-Headed Household	31.5	(8.3)	38912	34.1	(7.2)	1919
% Age <14	20.2	(4.7)	38912	20.6	(4.6)	1919
% Age 15-24	14.2	(6.5)	38912	16.5	(6.7)	1919
% Age 25-44	27.0	(5.1)	38912	28.4	(3.9)	1919
% Age >45	38.6	(8.5)	38912	34.5	(7.8)	1919
% < High School	15.7	(10.8)	38912	16.9	(9.2)	1919
% High School Graduate	28.3	(9.4)	38912	25.4	(7.4)	1919
% Some College	28.4	(7.2)	38912	29.0	(5.5)	1919
% College Graduate or More	27.6	(16.3)	38912	28.8	(13.7)	1919
Unemployment Rate	4.8	(3.2)	38912	5.5	(2.3)	1919
Poverty Rate	12.7	(8.7)	38912	15.6	(7.5)	1919
Median Household Income	45623.0	(21669.9)	38912	40741.4	(14714.1)	1919
Population	39493.7	(126490.2)	38912	255086.2	(483591.1)	1919
Number of Agencies	2048					
Number of Treated Agencies	101					

Notes: The characteristics information are from the data with crime activity outcomes. Officer related information are from the FBI's Law Enforcement Officer Killed or Assaulted (LEOKA) that covers the period 2000-2018. Demographics data come from the 2000 U.S. Census and the American Community Survey 5-year estimates from 2010 to 2018. For years 2001 to 2009, the demographics information are linearly interpolated.

Table A2: Robustness Specifications

	1st Month (t=0)	S.E.	2nd Month (t=1)	S.E.	Long-Term (t=2,...,11)	S.E.	Outcome Mean Full	Outcome Mean Treated	N
(1) Baseline Specification									
Murder Offenses	0.321***	(0.047)	-0.005	(0.037)	0.005	(0.013)	0.21	2.45	465778
Arrests	-0.073***	(0.024)	-0.043*	(0.022)	-0.003	(0.021)	156.9	968.8	329570
Violent Crimes	-0.035	(0.024)	0.014	(0.024)	-0.022	(0.014)	17.8	175.3	465797
Property Crimes	0.009	(0.016)	0.006	(0.015)	0.009	(0.012)	121.2	950.3	465786
(2) Restrict to Treated Cities									
Murder Offenses	0.322***	(0.048)	-0.006	(0.038)	0.003	(0.013)	2.45	2.45	22870
Arrests	-0.072***	(0.024)	-0.041*	(0.021)	-0.003	(0.019)	968.8	968.8	17649
Violent Crimes	-0.038	(0.024)	0.008	(0.023)	-0.026*	(0.014)	175.3	175.3	23000
Property Crimes	0.005	(0.017)	0.004	(0.014)	0.008	(0.011)	950.3	950.3	22959
(3) Separate Panel for Each Event									
Murder Offenses	0.311***	(0.046)	-0.002	(0.035)	0.004	(0.011)	0.61	6.57	481267
Arrests	-0.078***	(0.023)	-0.049**	(0.020)	-0.009	(0.017)	270.0	1930.2	341819
Violent Crimes	-0.026	(0.022)	0.021	(0.022)	-0.014	(0.012)	42.5	419.9	481291
Property Crimes	0.008	(0.014)	0.005	(0.012)	0.008	(0.009)	235.9	2051.9	481279
(4) Counting Multiple Officer Deaths Additively									
Murder Offenses	0.300***	(0.045)	0.003	(0.032)	0.010	(0.011)	0.21	2.45	465778
Arrests	-0.067***	(0.021)	-0.042**	(0.021)	-0.004	(0.020)	156.9	968.8	329570
Violent Crimes	-0.026	(0.020)	0.018	(0.021)	-0.017	(0.013)	17.8	175.3	465797
Property Crimes	0.008	(0.015)	0.006	(0.013)	0.005	(0.010)	121.2	950.3	465786
(5) Common Sample: Arrests & Crimes									
Murder Offenses	0.321***	(0.055)	0.021	(0.038)	0.014	(0.014)	0.23	2.36	329818
Arrests	-0.073***	(0.024)	-0.043*	(0.022)	-0.003	(0.021)	156.9	968.8	329570
Violent Crimes	-0.045*	(0.027)	0.026	(0.027)	-0.028*	(0.017)	19.4	167.6	329772
Property Crimes	0.008	(0.019)	0.020	(0.015)	0.007	(0.014)	126.8	862.1	329815

Table A2: Robustness Specifications (Continued)

	1st Month (t=0)	S.E.	2nd Month (t=1)	S.E.	Long-Term (t=2,...,11)	S.E.	Outcome Mean Full	Outcome Mean Treated	N
(6) Drop Time Trend									
Murder Offenses	0.309***	(0.047)	-0.017	(0.035)	-0.008	(0.011)	0.21	2.45	465778
Arrests	-0.122***	(0.024)	-0.092***	(0.022)	-0.054***	(0.021)	156.9	968.8	329570
Violent Crimes	-0.029	(0.026)	0.023	(0.024)	-0.016	(0.018)	17.8	175.3	465797
Property Crimes	-0.019	(0.019)	-0.021	(0.018)	-0.019	(0.015)	121.2	950.3	465786
(7) Drop Agency × Month									
Murder Offenses	0.324***	(0.046)	-0.001	(0.035)	0.006	(0.013)	0.21	2.45	465778
Arrests	-0.072***	(0.024)	-0.039*	(0.023)	-0.003	(0.021)	156.9	968.8	329571
Violent Crimes	-0.037	(0.023)	0.013	(0.024)	-0.021	(0.014)	17.8	175.3	465797
Property Crimes	0.003	(0.017)	-0.000	(0.016)	0.009	(0.012)	121.2	950.3	465786
(8) Add State-by-Year FE									
Murder Offenses	0.322***	(0.047)	-0.004	(0.036)	0.006	(0.013)	0.21	2.45	465778
Arrests	-0.076***	(0.024)	-0.044**	(0.023)	0.000	(0.021)	156.9	968.8	329570
Violent Crimes	-0.033	(0.024)	0.017	(0.024)	-0.017	(0.015)	17.8	175.3	465797
Property Crimes	0.009	(0.016)	0.007	(0.014)	0.008	(0.011)	121.2	950.3	465786
(9) Control for Officer Assaults									
Murder Offenses	0.321***	(0.047)	-0.005	(0.037)	0.005	(0.013)	0.21	2.45	465778
Arrests	-0.079***	(0.024)	-0.047**	(0.022)	-0.006	(0.020)	156.9	968.8	329570
Violent Crimes	-0.043*	(0.024)	0.011	(0.025)	-0.027*	(0.015)	17.8	175.3	465797
Property Crimes	0.008	(0.016)	0.005	(0.015)	0.008	(0.011)	121.2	950.3	465786
(10) Remove DUI Arrests									
Murder Offenses	0.321***	(0.047)	-0.005	(0.037)	0.005	(0.013)	0.21	2.45	465778
Arrests	-0.064***	(0.025)	-0.036	(0.023)	0.001	(0.021)	156.9	968.8	329570
Violent Crimes	-0.035	(0.024)	0.014	(0.024)	-0.022	(0.014)	17.8	175.3	465797
Property Crimes	0.009	(0.016)	0.006	(0.015)	0.009	(0.012)	121.2	950.3	465786

Table A2: Robustness Specifications (Continued)

	1st Month (t=0)	S.E.	2nd Month (t=1)	S.E.	Long-Term (t=2,...,11)	S.E.	Outcome Mean Full	Outcome Mean Treated	N
(11) Raw Data									
Murder Offenses	0.369***	(0.053)	-0.004	(0.036)	0.003	(0.013)	0.22	2.44	466939
Arrests	-0.083***	(0.027)	-0.039*	(0.022)	0.001	(0.021)	156.9	967.8	330360
Violent Crimes	-0.034	(0.024)	0.025	(0.026)	-0.022	(0.014)	17.8	175.1	466746
Property Crimes	0.011	(0.016)	0.010	(0.015)	0.009	(0.012)	121.2	951.1	466944
(12) Levels Model									
Murder Offenses	0.713***	(0.251)	0.003	(0.247)	-0.162	(0.146)	0.21	2.45	465778
Arrests	-27.167	(36.419)	9.763	(52.836)	14.678	(41.661)	156.9	968.8	329570
Violent Crimes	-5.544	(7.913)	-4.433	(8.615)	-6.398	(8.115)	17.8	175.3	465797
Property Crimes	-28.541	(23.714)	-6.279	(17.645)	-28.206	(21.424)	121.2	950.3	465786
(13) Per Capita Model (Per 100K Residents)									
Murder Offenses	1.499***	(0.293)	0.025	(0.099)	0.003	(0.036)	0.21	2.45	465778
Arrests	-33.016***	(10.534)	-20.933**	(10.072)	-5.505	(8.573)	156.9	968.8	329570
Violent Crimes	-1.602	(1.199)	-0.296	(1.285)	-0.927	(0.949)	17.8	175.3	465797
Property Crimes	-1.159	(6.052)	2.613	(5.586)	4.322	(5.219)	121.2	950.3	465786
(14) Inverse Hyperbolic Sine Model									
Murder Offenses	0.411***	(0.060)	-0.010	(0.046)	0.007	(0.016)	0.21	2.45	465778
Arrests	-0.075***	(0.024)	-0.044*	(0.023)	-0.003	(0.021)	156.9	968.8	329570
Violent Crimes	-0.041	(0.028)	0.017	(0.027)	-0.026*	(0.015)	17.8	175.3	465797
Property Crimes	0.009	(0.017)	0.006	(0.015)	0.009	(0.012)	121.2	950.3	465786

Table A2: Robustness Specifications (Continued)

	1st Month (t=0)	S.E.	2nd Month (t=1)	S.E.	Long-Term (t=2,...,11)	S.E.	Outcome Mean Full	Treated	N
(15) Re-weight (Overweight Untreated Obs.)									
Murder Offenses	0.320***	(0.046)	-0.006	(0.037)	0.004	(0.013)	0.21	2.45	465778
Arrests	-0.073***	(0.024)	-0.043*	(0.022)	-0.003	(0.021)	156.9	968.8	329570
Violent Crimes	-0.035	(0.024)	0.014	(0.024)	-0.022	(0.014)	17.8	175.3	465797
Property Crimes	0.011	(0.019)	0.007	(0.019)	0.010	(0.019)	121.2	950.3	465786
(16) Sun & Abraham (2020) IW Estimator									
Murder Offenses	0.313***	(0.034)	-0.002	(0.030)	0.004	(0.007)	0.61	6.57	481267
Arrests	-0.070***	(0.023)	-0.040*	(0.021)	0.001	(0.008)	270.0	1930.2	341819
Violent Crimes	-0.028	(0.023)	0.021	(0.021)	-0.015**	(0.007)	42.5	419.9	481291
Property Crimes	0.009	(0.015)	0.007	(0.013)	0.010**	(0.005)	235.9	2051.9	481279
(17) Re-weight (Overweight Untreated and Pre-period Obs.)									
Murder Offenses	0.285***	(0.053)	-0.044	(0.045)	-0.025	(0.020)	0.21	2.45	465778
Arrests	-0.090***	(0.023)	-0.062**	(0.024)	-0.018	(0.020)	156.9	968.8	329570
Violent Crimes	-0.045	(0.029)	0.001	(0.027)	-0.036*	(0.020)	17.8	175.3	465797
Property Crimes	0.009	(0.021)	0.001	(0.020)	-0.001	(0.020)	121.2	950.3	465786

Notes: The baseline specification is a replicate of output in Table 2 and each subsequent model is a variant of this baseline. Model (2) restricts the sample to treated cities. Model (3) uses a separate panel for each officer death treatment rather than each department. Model (4) counts multiple death events additively rather than as a single event. Model (5) restricts the sample to cities with both the arrest and crime outcomes. Model (6) and (7) drop the department specific linear time trend and agency-by-month fixed effect, respectively. Model (8) adds state by year fixed effects. Model (9) controls for monthly variation in assaults against officers that result in injuries. Model (10) removes the DUI arrests counts from the total arrests. Model (11) uses the uncleaned raw data. Models (12), (13) and (14) consider alternate functional forms, using a levels, a per capita and an inverse hyperbolic sine, respectively. Model (15) re-weights the data with control city weight=1000 and treated city weight=1. Model (16) uses Sun and Abraham (2020)'s proposed estimator and Model (17) re-weights the data with control city weight=1000, treated city pre-period weight = 1000 and treated city weight=1. Standard errors are clustered at the department level. * p<0.1,** p<0.05, *** p<0.01.

Table A3: Index Crimes and Arrests by Type

	1st Month (t=0)	S.E.	2nd Month (t=1)	S.E.	Long-Term (t=2,...,11)	S.E.	Outcome Mean Full	Outcome Mean Treated	N
A. Murder Outcomes									
Murder Offenses	0.321***	(0.047)	-0.005	(0.037)	0.005	(0.013)	0.21	2.45	465778
Murder Arrests	0.118***	(0.045)	0.054	(0.034)	0.022	(0.025)	0.17	1.59	329477
B. Index Arrests									
Rape	0.005	(0.037)	-0.025	(0.033)	0.013	(0.019)	0.29	2.15	327359
Robbery	-0.057*	(0.033)	-0.061	(0.048)	0.010	(0.022)	1.9	15.9	327437
Aggravated Assault	-0.055	(0.037)	-0.021	(0.028)	-0.039	(0.026)	6.8	45.5	327891
Burglary	0.028	(0.041)	0.016	(0.046)	0.021	(0.027)	3.9	20.8	327951
Theft	-0.059	(0.042)	-0.007	(0.038)	-0.010	(0.031)	15.3	81.8	328382
Motor Vehicle Theft	-0.070*	(0.043)	-0.113	(0.073)	-0.058	(0.070)	1.5	12.2	327848
C. Index Crime									
Rape	-0.020	(0.031)	0.004	(0.035)	0.002	(0.019)	1.2	10.5	462607
Robbery	-0.016	(0.028)	0.005	(0.029)	-0.015	(0.015)	5.9	65.0	463483
Aggravated Assault	-0.049*	(0.029)	0.004	(0.027)	-0.022	(0.017)	10.7	100.1	463988
Burglary	0.056**	(0.025)	0.018	(0.025)	0.021	(0.017)	24.0	198.3	464536
Theft	-0.032	(0.026)	-0.017	(0.021)	-0.012	(0.017)	81.7	597.3	465157
Motor Vehicle Theft	-0.009	(0.027)	-0.002	(0.029)	-0.003	(0.020)	15.5	155.5	464309

Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Regressions also include a dummy variable for 12 or more months after the occurrence of an officer death. Outcomes are defined as $Y_{it} = log(y_{it} + 1)$ and outcome means are given in levels. Standard errors are clustered at the department level. * p<0.1,** p<0.05, *** p<0.01.

Table A4: Non-Index Arrest Outcomes by Type

	1st Month (t=0)	S.E.	2nd Month (t=1)	S.E.	Long-Term (t=2,...,11)	S.E.	Outcome Mean Full	Outcome Mean Treated	N
A. Non-Index Arrests									
Manslaughter	0.032	(0.020)	0.022	(0.025)	0.001	(0.013)	0.01	0.10	328146
Arson	0.031	(0.044)	-0.051	(0.038)	0.009	(0.017)	0.15	0.86	328209
Other Assault	-0.017	(0.034)	-0.044	(0.036)	0.001	(0.029)	14.1	90.1	328315
Weapons	-0.041	(0.041)	-0.007	(0.036)	-0.012	(0.023)	2.5	17.4	328189
Prostitution	-0.056	(0.043)	-0.085*	(0.051)	-0.037	(0.042)	1.3	16.0	328139
Other Sex Offense	-0.045	(0.034)	-0.016	(0.047)	-0.016	(0.027)	0.97	6.98	328165
Family Offense	0.021	(0.068)	0.069	(0.052)	0.046	(0.051)	0.55	3.85	328179
DUI	-0.103**	(0.042)	-0.086**	(0.038)	-0.009	(0.030)	13.0	70.4	328313
Drug Sale	-0.094	(0.068)	-0.057	(0.068)	-0.055	(0.066)	4.0	36.2	328240
Forgery	0.008	(0.041)	-0.019	(0.042)	0.008	(0.028)	1.05	5.19	328177
Fraud	0.006	(0.047)	-0.010	(0.047)	0.065*	(0.033)	1.76	8.33	328270
Embezzlement	-0.014	(0.055)	-0.007	(0.038)	0.026	(0.029)	0.24	1.11	328181
Stolen Property	0.040	(0.061)	0.075	(0.059)	0.079	(0.062)	1.53	7.11	328209
Runaway	0.034	(0.042)	0.017	(0.044)	0.014	(0.043)	1.20	8.23	328173
B. Quality of Life Arrests									
Disorderly Conduct	-0.006	(0.046)	-0.030	(0.044)	0.018	(0.039)	5.5	29.8	328364
Curfew/Loitering	-0.057	(0.064)	0.022	(0.048)	-0.015	(0.063)	2.5	32.3	328308
Vandalism	-0.050	(0.042)	-0.064	(0.045)	-0.035	(0.031)	3.0	17.5	328366
Gambling	-0.016	(0.033)	0.001	(0.032)	-0.002	(0.020)	0.06	0.67	328263
Vagrancy	0.016	(0.071)	-0.017	(0.068)	0.036	(0.072)	0.57	6.03	328301
Drunkenness	-0.025	(0.062)	0.010	(0.064)	0.021	(0.057)	9.0	43.9	328407
Liquor	-0.015	(0.070)	-0.062	(0.072)	0.001	(0.056)	5.1	28.5	328399
Drug Possession	-0.101	(0.064)	-0.125*	(0.067)	-0.053	(0.074)	18.1	101.6	328385
Uncategorized Arrests	-0.037	(0.044)	-0.016	(0.041)	0.027	(0.044)	41.4	260.9	328555

Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Regressions also include a dummy variable for 12 or more months after the occurrence of an officer death. Outcomes are defined as $Y_{it} = log(y_{it} + 1)$ and outcome means are given in levels. Standard errors are clustered at the department level. * p<0.1,** p<0.05, *** p<0.01.

Table A5: Additional Outcomes

	1st Month (t=0)	S.E.	2nd Month (t=1)	S.E.	Long-Term (t=2,...,11)	S.E.	Outcome Mean Full	Outcome Mean Treated	N
Traffic Accidents									
Fatal Traffic Accidents	-0.023	(0.037)	-0.020	(0.027)	-0.016	(0.012)	0.24	1.79	397404
Accidents involving Alcohol	0.039	(0.042)	0.012	(0.034)	0.017	(0.019)	0.09	0.63	352716
Fatal Use-of-Force									
Supplementary Homicide Report	0.017	(0.015)	-0.012	(0.013)	0.001	(0.007)	0.01	0.08	465784
Fatal Encounters	0.040	(0.026)	0.002	(0.025)	-0.003	(0.008)	0.01	0.12	466944

Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Regressions also include a dummy variable for 12 or more months after the occurrence of an officer death. Outcomes are defined as $Y_{it} = log(y_{it} + 1)$ and outcome means are given in levels. Standard errors are clustered at the department level. "Accidents involving alcohol" is the number of fatal traffic accidents with at least one driver with the blood alcohol concentration 0.01 g/dL or higher involved in a crash. Officer Involved Deaths panel includes two measures of civilians killed by police. First, the "Felon Killed by Police" measure is a count of deaths at the hands of officers from the Supplementary Homicide Report of the FBI UCR series. Second, *Fatal Encounters* is a count of civilians killed by police from a crowd-sourced data series. Both measures exclude records of deaths of suspects involved in the line-of-duty officer death event during month 0, as well as records of civilian deaths that occur before the officer death in month 0. * p<0.1,** p<0.05, *** p<0.01.

Table A6: Heterogeneity, Arrestee Demographics

A. UCR	1st Month (t=0)	S.E.	2nd Month (t=1)	S.E.	Long-Term (t=2,...,11)	S.E.	Outcome Mean: Full	Outcome Mean: Treated	N	p-value Diff. total
Total Arrests	-0.073***	(0.024)	-0.043*	(0.022)	-0.003	(0.021)	156.9	968.8	329570	
Black	-0.035	(0.028)	-0.005	(0.028)	0.013	(0.021)	42.0	351.1	329557	0.299
White	-0.079***	(0.028)	-0.053**	(0.024)	-0.004	(0.022)	111.0	595.6	329567	0.875
Male	-0.071***	(0.024)	-0.039*	(0.022)	-0.004	(0.021)	117.1	745.0	327753	0.956
Female	-0.064**	(0.030)	-0.041	(0.029)	0.009	(0.024)	38.5	228.7	327753	0.816
Adult	-0.072***	(0.025)	-0.040*	(0.024)	-0.000	(0.022)	133.7	838.9	327732	0.980
Juvenile	-0.082*	(0.046)	-0.072	(0.050)	-0.025	(0.043)	21.9	134.6	327737	0.873

B. Dallas	RD Estimate	S.E.	% Change	DOW residualized	S.E.	% Change	Outcome Mean: Pre	Outcome Mean: Post	N	p-value Diff. total
Total Arrests	-39.92***	(5.08)	-44.22%	-35.73***	(5.08)	-35.38%	104.2	86.7	365	
Black	-18.70***	(4.58)	-42.08%	-18.88***	(4.58)	-39.39%	55.2	46.2	365	0.592
White	-27.97***	(2.80)	-73.99%	-27.10***	(2.80)	-67.08%	31.9	17.9	365	0.000
Hispanic	-2.60	(2.31)	-15.07%	-0.45	(2.31)	-1.99%	23.9	23.0	365	0.005
Male	-29.51***	(5.01)	-40.76%	-27.71***	(5.01)	-33.86%	82.9	69.2	365	0.651
Female	-10.36***	(1.43)	-57.96%	-9.74***	(1.43)	-48.07%	21.2	17.4	365	0.114
Old	-15.58***	(4.22)	-35.57%	-13.27***	(4.22)	-26.01%	56.8	47.9	365	0.143
Youth	-23.91***	(3.22)	-51.93%	-23.12***	(3.22)	-45.79%	47.8	39.1	365	0.082
Repeat Offender	-16.55***	(2.19)	-36.34%	-16.33***	(2.19)	-34.40%	55.3	45.6	365	0.034
New Offender	-22.15***	(4.35)	-50.68%	-19.35***	(4.35)	-36.15%	48.9	41.1	365	0.431

Notes: Regressions in Panel A include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Regressions also include a dummy variable for 12 or more months after the occurrence of an officer death. Outcomes are defined as $Y_{it} = log(y_{it} + 1)$ and outcome means are given in levels. Standard errors are clustered at the department level. The last column reports the p-value from testing whether the first month effects of the sub-group are equal to the total arrests effect. Juvenile is defined to be people arrested under 18 years of age. Panel B shows the results of a regression discontinuity regression using date as the running variable. DOW-residualized column includes the results with day-of-the-week fixed effects. Outcome means are given for the 50 days around the cutoff and percent changes are relative to the local polynomial left-estimate. The "Youth" category in this panel refers to arrestees under 30 years old. Repeat offenders are arrested individuals who we observe with a prior arrest by the Dallas Police Department, using arrest data going back to 2014. The last column reports the p-value from testing whether the percent changes of the sub-group are equal to the total arrests effect. * p<0.1,** p<0.05, *** p<0.01.

Table A7: Impact of an Accidental Officer Death on Policing and Crime

	1st Month (t=0)	S.E.	2nd Month (t=1)	S.E.	Long-Term (t=2,...,11)	S.E.	Outcome Mean Full	Outcome Mean Treated	N
Murder Outcomes									
Murder Offenses	0.003	(0.033)	0.044	(0.032)	0.019	(0.016)	0.21	2.42	465778
Murder Arrests	-0.033	(0.052)	-0.058	(0.041)	0.001	(0.019)	0.17	1.55	329477
Policing Activity									
Arrests	-0.020	(0.026)	0.001	(0.032)	0.008	(0.029)	156.9	969.6	329570
Index Arrests	0.040	(0.054)	0.011	(0.045)	0.029	(0.037)	29.6	190.7	329564
Violent Arrests	-0.000	(0.053)	-0.052	(0.049)	-0.020	(0.026)	8.9	72.9	329542
Property Arrests	0.023	(0.056)	0.031	(0.047)	0.034	(0.045)	20.7	117.9	329543
Non-Index Arrests	0.007	(0.037)	0.002	(0.039)	0.004	(0.036)	42.4	275.9	329535
Quality of Life Arrests	-0.053	(0.034)	0.017	(0.039)	0.004	(0.035)	85.1	502.9	329541
Traffic Stops	-0.233***	(0.071)	-0.061	(0.074)	-0.208	(0.244)	5288.0	5487.6	1918
Crime and Community Activity									
Index Crimes	0.006	(0.023)	-0.036	(0.028)	0.001	(0.019)	139.0	1201.1	465782
Violent Crimes	0.017	(0.036)	0.008	(0.036)	0.024	(0.022)	17.8	179.3	465797
Property Crimes	0.000	(0.023)	-0.046	(0.031)	-0.003	(0.019)	121.2	1022.9	465786
911 Calls for Service	0.027	(0.016)	0.036*	(0.018)	0.021	(0.019)	15096.5	28843.3	5737
Crime Report Rate (911 Calls)	-0.007	(0.005)	-0.009*	(0.005)	-0.008	(0.005)	0.19	0.19	4418
Fatal Traffic Accidents	0.149***	(0.051)	-0.011	(0.043)	0.006	(0.016)	0.24	2.17	397404

Notes: This table uses an accidental officer death as a treatment instead of officers killed by felony. All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Regressions also include a dummy variable for 12 or more months after the occurrence of an officer death. Outcomes are defined as $Y_{it} = log(y_{it} + 1)$ and outcome means are given in levels. Standard errors are clustered at the department level. The number of agencies, number of treated agencies, and total accidental officer death events for crime outcomes are 2048, 66, and 95, respectively. For arrest activity outcomes, they are 1450, 51, and 73. For the traffic stop outcome, they are 24, 11, and 19. For traffic accident outcome, they are 1743, 58 and 87. For 911 call outcomes, they are 72, 19, and 36. All arrest and crime subcategories exclude murder outcomes. Violent crimes and arrests include rape, robbery and aggravated assault. Property crimes and arrests include burglary, theft and motor vehicle theft. See Table A3 and Table A4 for the list of crime and arrest sub-types. "Crime Report Rate (911 Calls)" is the share of calls that result in an officer writing a crime incident report. * p<0.1,** p<0.05, *** p<0.01.

Table A8: Summary Statistics - Dallas

	Dallas			Treated Agencies		
	Mean	S.D.	N	Mean	S.D	N
Murder Outcomes						
Murder Offenses	14.864	(5.205)	228	2.447	(6.069)	22870
Murder Arrests	2.548	(2.435)	228	1.589	(4.980)	17638
Policing Activity						
Arrests	4233.8	(1142.6)	228	968.8	(1739.0)	17649
Index Arrests	621.3	(173.6)	228	178.0	(343.0)	17665
Violent Arrests	160.0	(31.7)	228	63.4	(161.1)	17655
Property Arrests	461.3	(152.5)	228	114.6	(200.6)	17666
Non-Index Arrests	999.7	(173.7)	228	271.2	(513.7)	17669
Quality of Life Arrests	2612.7	(865.1)	228	520.2	(942.0)	17659
Crime and Community Activity						
Index Crimes	6835.0	(2002.8)	228	1124.5	(2033.8)	22960
Violent Crimes	1002.0	(302.4)	228	175.3	(393.2)	23000
Property Crimes	5833.0	(1732.9)	228	950.3	(1678.8)	22959
911 Calls for Service	50957.7	(4012.8)	136	27458.4	(21368.7)	2135
Crime Report Rate (911 Calls)	0.30	(0.02)	136	0.20	(0.11)	1831
Fatal Traffic Accidents	11.93	(3.91)	228	1.79	(3.69)	21660

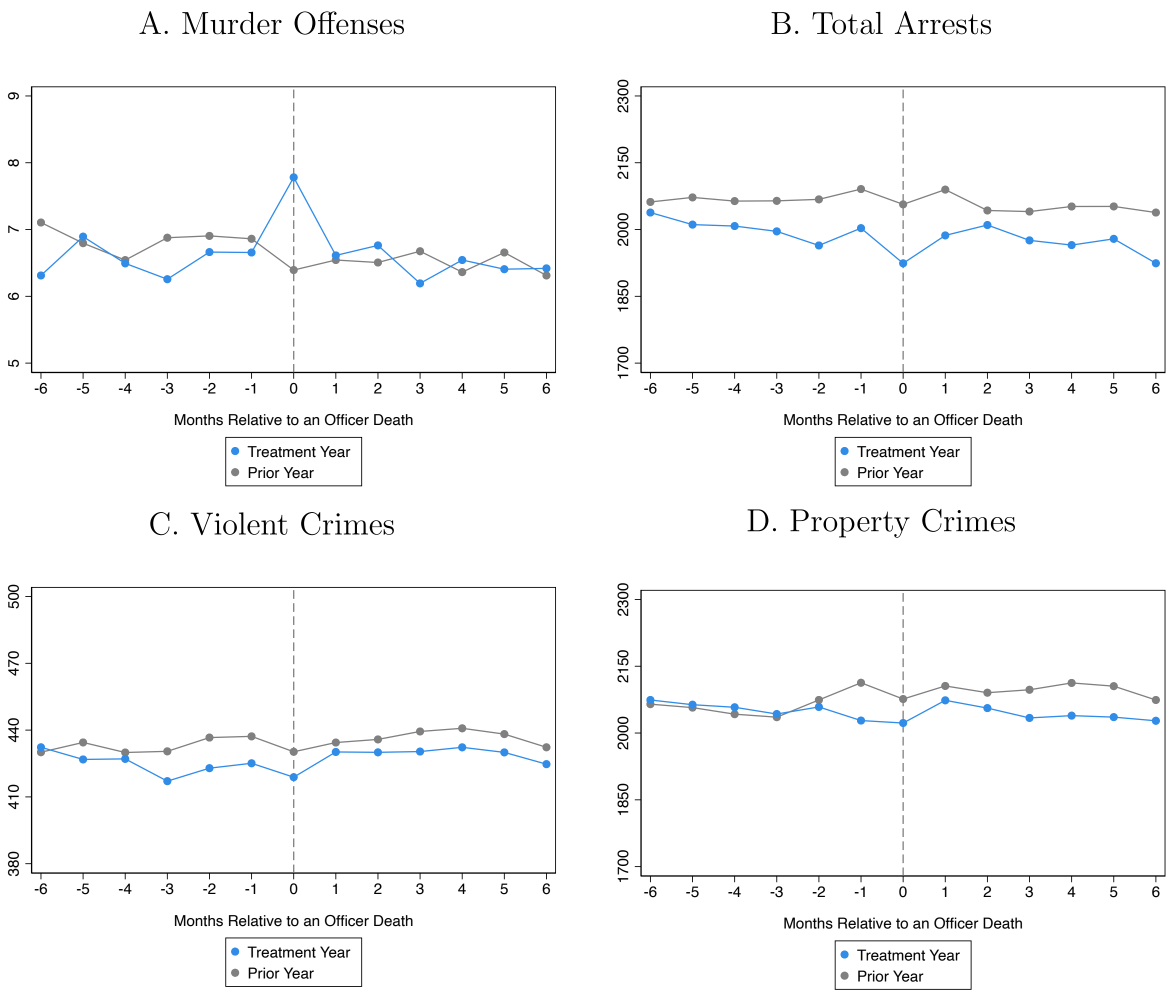
Notes: This table shows outcomes in Dallas and the treated cities. All arrest and crime subcategories exclude murder outcomes. Violent crimes and arrests include rape, robbery and aggravated assault. Property crimes and arrests include burglary, theft and motor vehicle theft. See Table A3 and Table A4 for the list of crime and arrest sub-types. "Crime Report Rate (911 Calls)" is the share of calls that result in an officer writing a crime incident report. The officer characteristics are from the *Officer Down Memorial Page*. Other causes of death include assault and stabbed.

Table A9: Summary Demographic Characteristics - Dallas

	Dallas			Treated Agencies		
	Mean	S.D.	N	Mean	S.D	N
Characteristics of Cities						
Number of Police Officers	3187.9	(287.2)	19	604.7	(1310.5)	1919
Number of Officers Killed by Felony	0.474	(0.964)	19	0.093	(0.323)	1919
Number of Officers Assaulted	215.8	(41.0)	19	83.0	(180.6)	1919
% Black	24.7	(0.5)	19	16.0	(18.8)	1919
% Hispanic	39.9	(2.2)	19	21.0	(20.0)	1919
% White	30.9	(1.9)	19	54.2	(23.6)	1919
% Male	50.0	(0.3)	19	48.8	(1.9)	1919
% Female-Headed Household	36.4	(0.8)	19	34.1	(7.2)	1919
% Age <14	22.4	(0.3)	19	20.6	(4.6)	1919
% Age 15-24	14.7	(0.7)	19	16.5	(6.7)	1919
% Age 25-44	33.3	(1.1)	19	28.4	(3.9)	1919
% Age >45	29.5	(1.9)	19	34.5	(7.8)	1919
% < High School	27.0	(1.8)	19	16.9	(9.2)	1919
% High School Graduate	21.2	(0.8)	19	25.4	(7.4)	1919
% Some College	22.7	(0.2)	19	29.0	(5.5)	1919
% College Graduate or More	29.0	(1.4)	19	28.8	(13.7)	1919
Unemployment Rate	5.1	(0.7)	19	5.5	(2.3)	1919
Poverty Rate	17.8	(1.5)	19	15.6	(7.5)	1919
Median Household Income	34403.7	(2410.4)	19	40741.4	(14714.1)	1919
Population	1215364.1	(43030.8)	19	255086.2	(483591.1)	1919

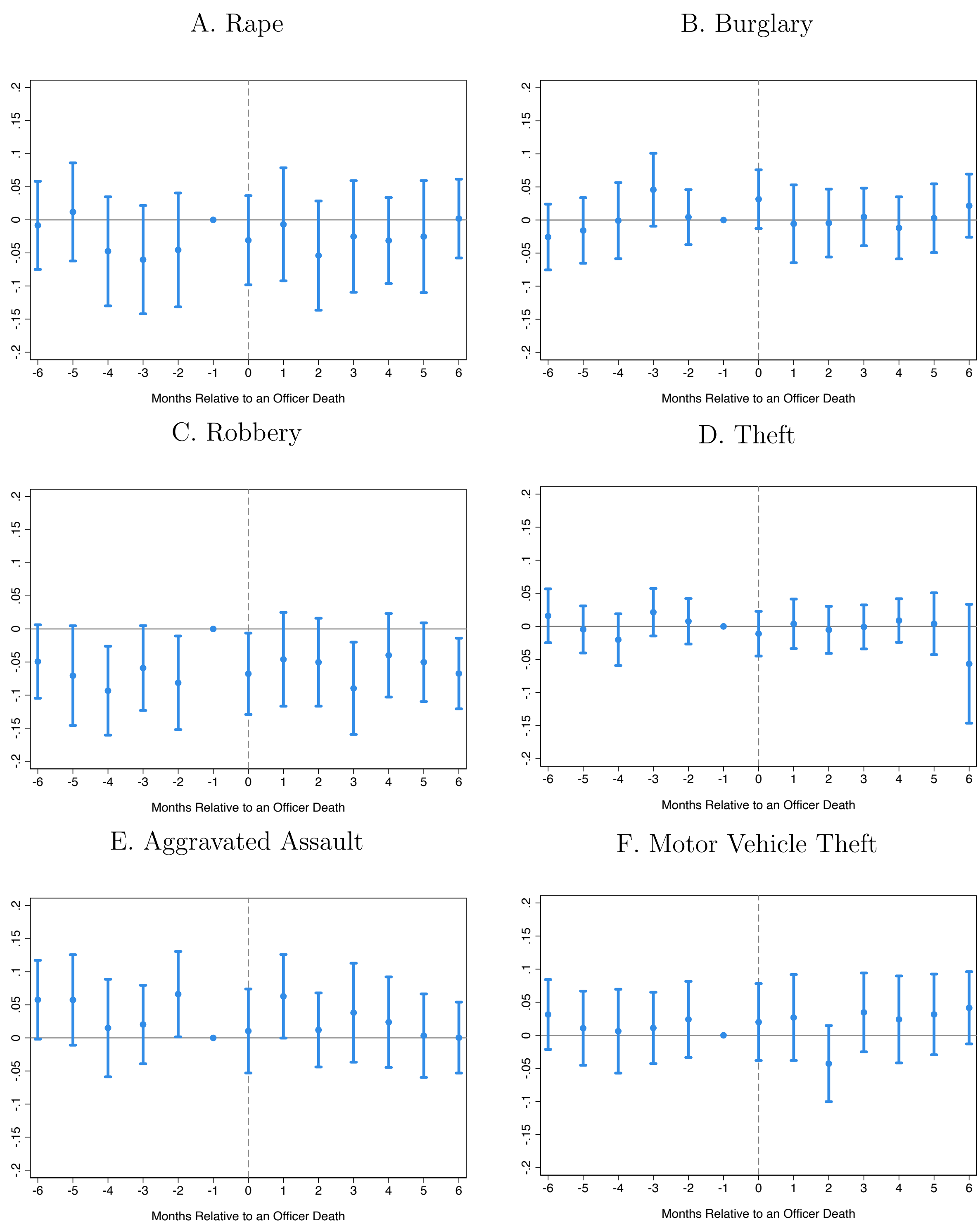
Notes: This table shows the demographic characteristics of Dallas and the treated cities. The characteristics information are from the data with crime activity outcomes. Officer related information are from the FBI's Law Enforcement Officer Killed or Assaulted (LEOKA) that covers the period 2000-2018. Demographics data come from the 2000 U.S. Census and the American Community Survey 5-year estimates from 2010 to 2018. For years 2001 to 2009, the demographics information are linearly interpolated.

Figure A1: Unadjusted Data Around Events, Level Outcomes



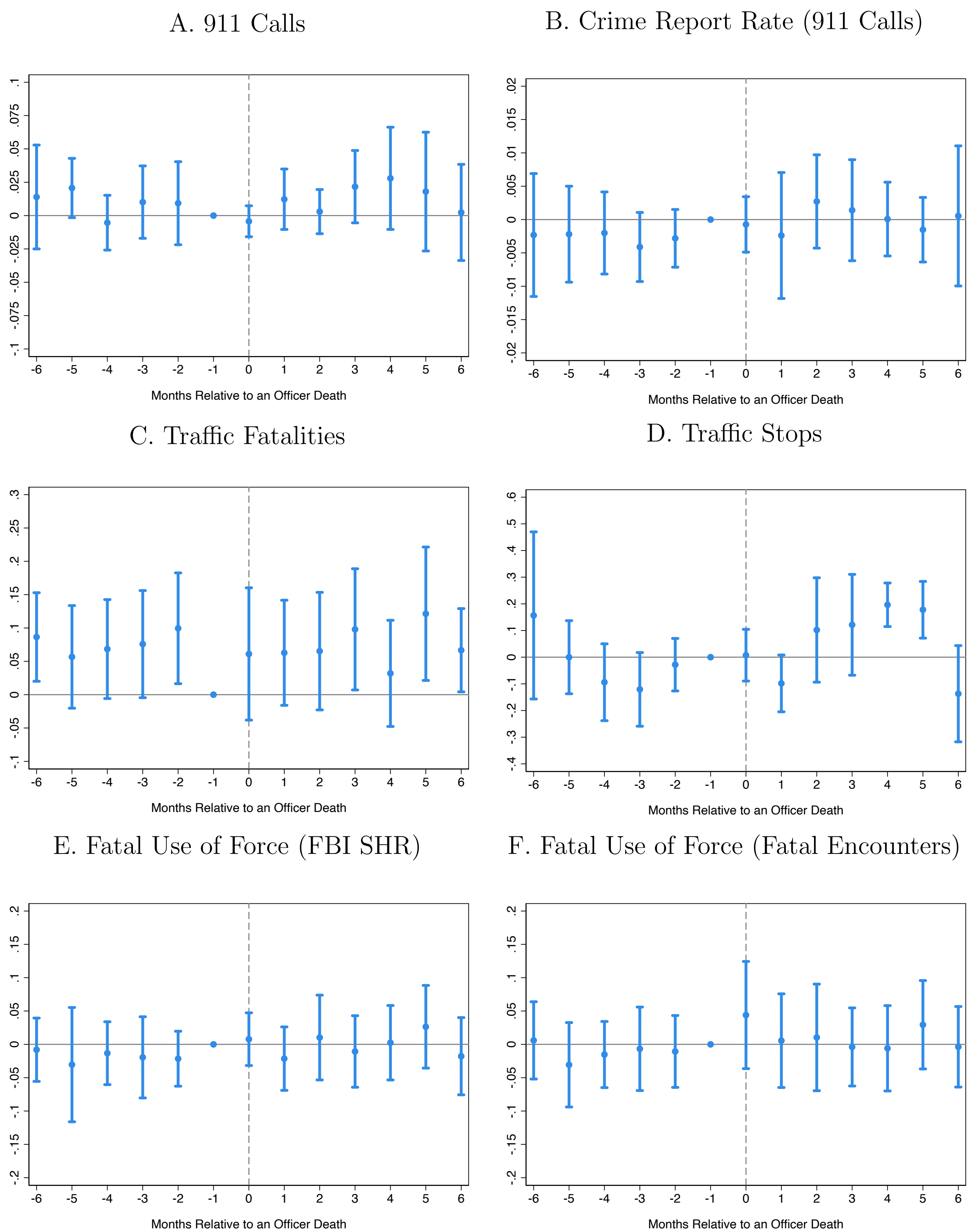
Notes: This figure plots the unadjusted data around the officer death events. There are 160 officer death events in 96 agencies after excluding events that do not have enough periods before and after the event. Violent crimes include rape, robbery, and aggravated assault. Property crimes include burglary, theft, and motor vehicle theft.

Figure A2: Event-Study: Index Crimes



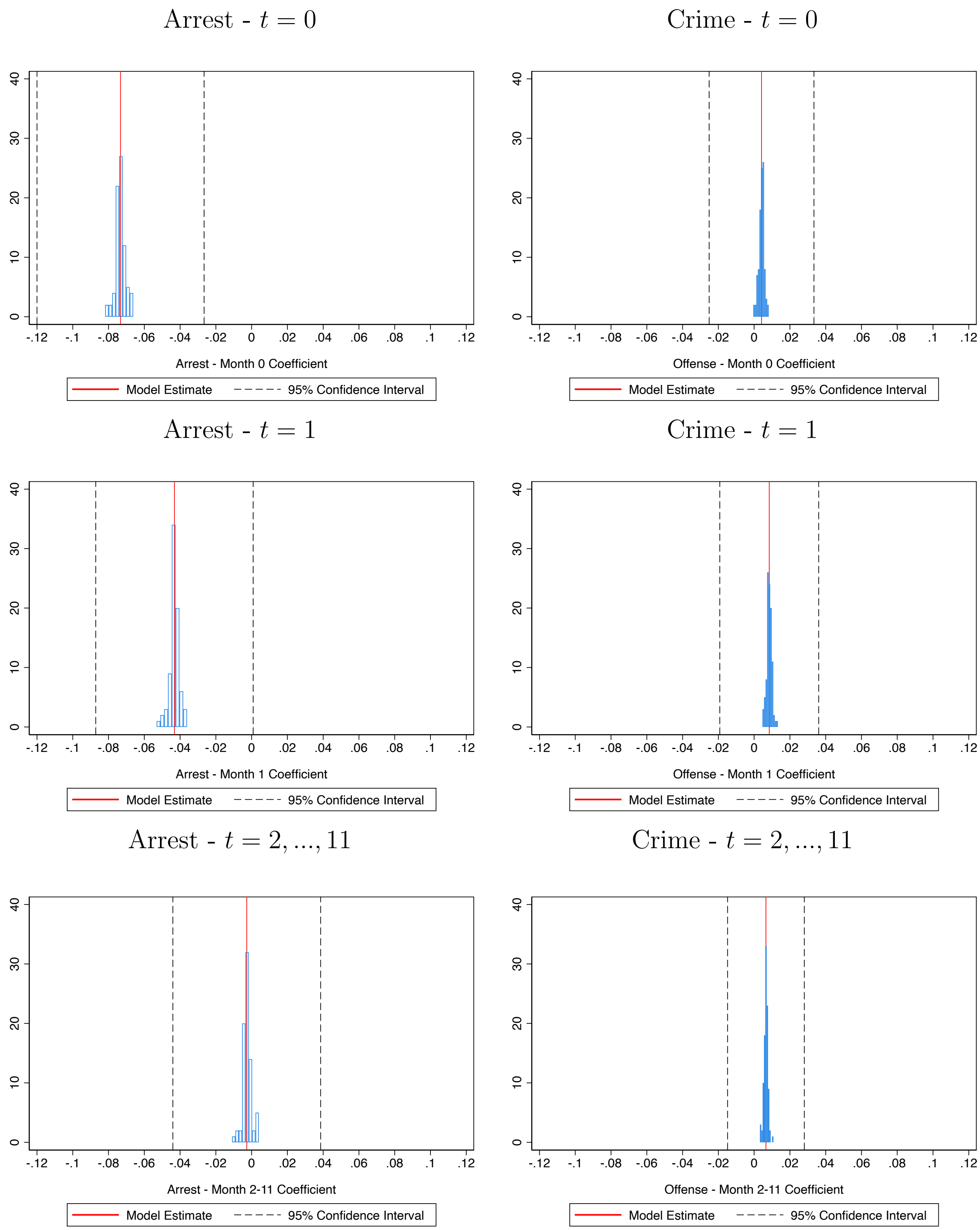
Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Months -6 and 6 include all months before month -6 and all months after month 6, respectively. Standard errors are clustered at the department level.

Figure A3: Event-Study: 911 Calls and Traffic Outcomes



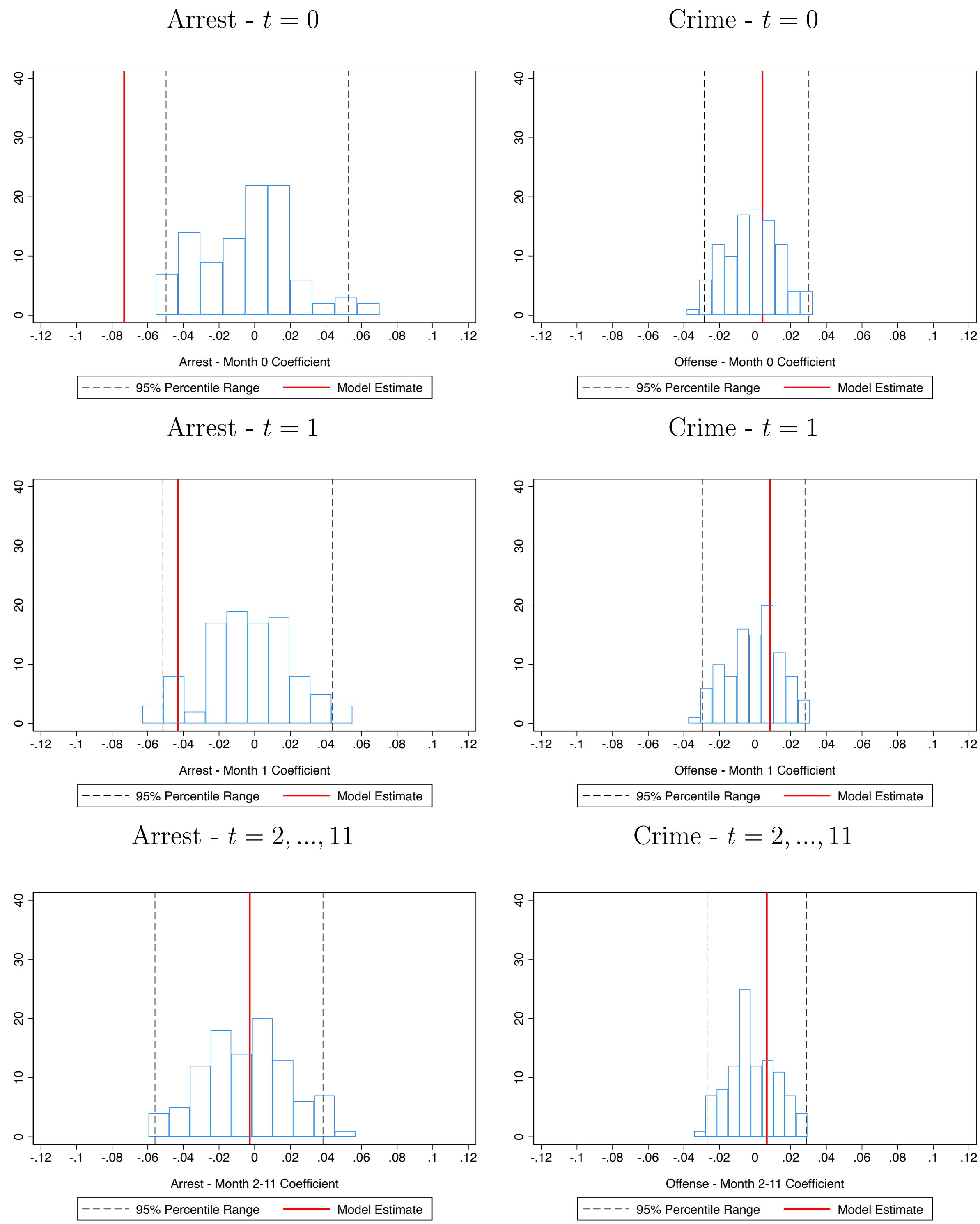
Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Months -6 and 6 include all months before month -6 and all months after month 6, respectively. Standard errors are clustered at the department level. Panels E and F measure fatal use of force by police officers; both measures exclude records of deaths of suspects involved in the line-of-duty officer death event during month 0, as well as records of civilian deaths that occur before the line-of-duty officer death in month 0.

Figure A4: Distribution of Coefficients Dropping Single Treated Agency



Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Regressions also include a dummy variable for 12 or more months after the occurrence of an officer death. Standard errors are clustered at the department level. We re-estimate the model dropping one treatment city at a time. There are 78 treated cities for the arrest outcome and 101 treated cities for the crime outcome.

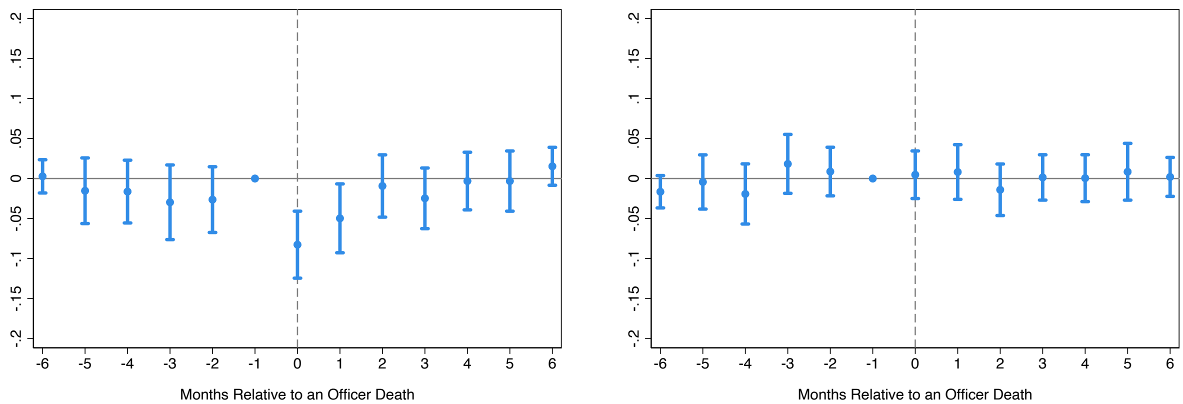
Figure A5: Placebo Treatment Timing



Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Regressions also include a dummy variable for 12 or more months after the occurrence of an officer death. Standard errors are clustered at the department level. The timing of officer deaths among treated agencies is randomized holding the number of officer deaths per agency constant. The model is re-estimated 100 times to construct the placebo distribution.

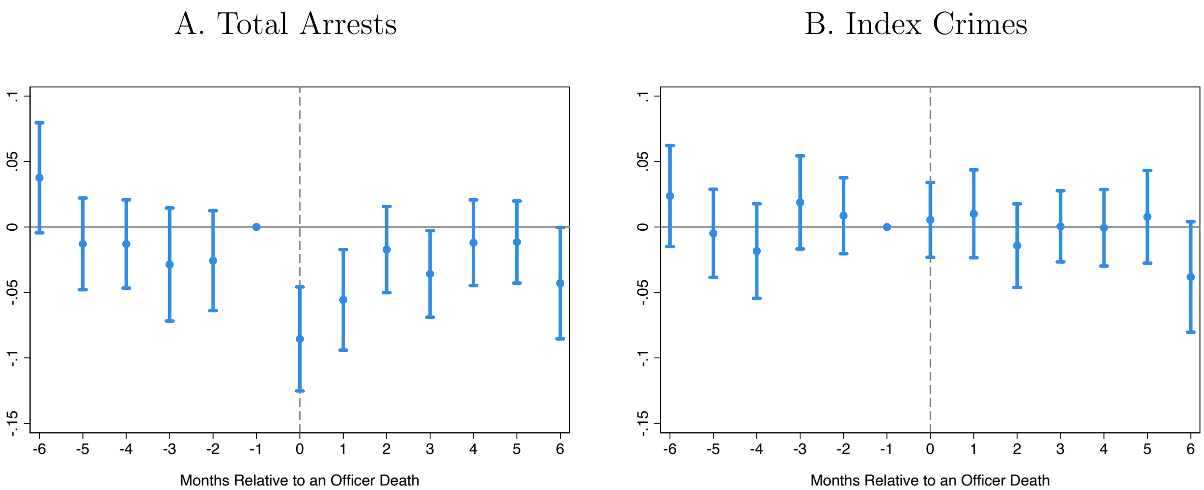
Figure A6: Event-Study: Sun and Abraham (2020)

A. Total Arrests B. Index Crimes



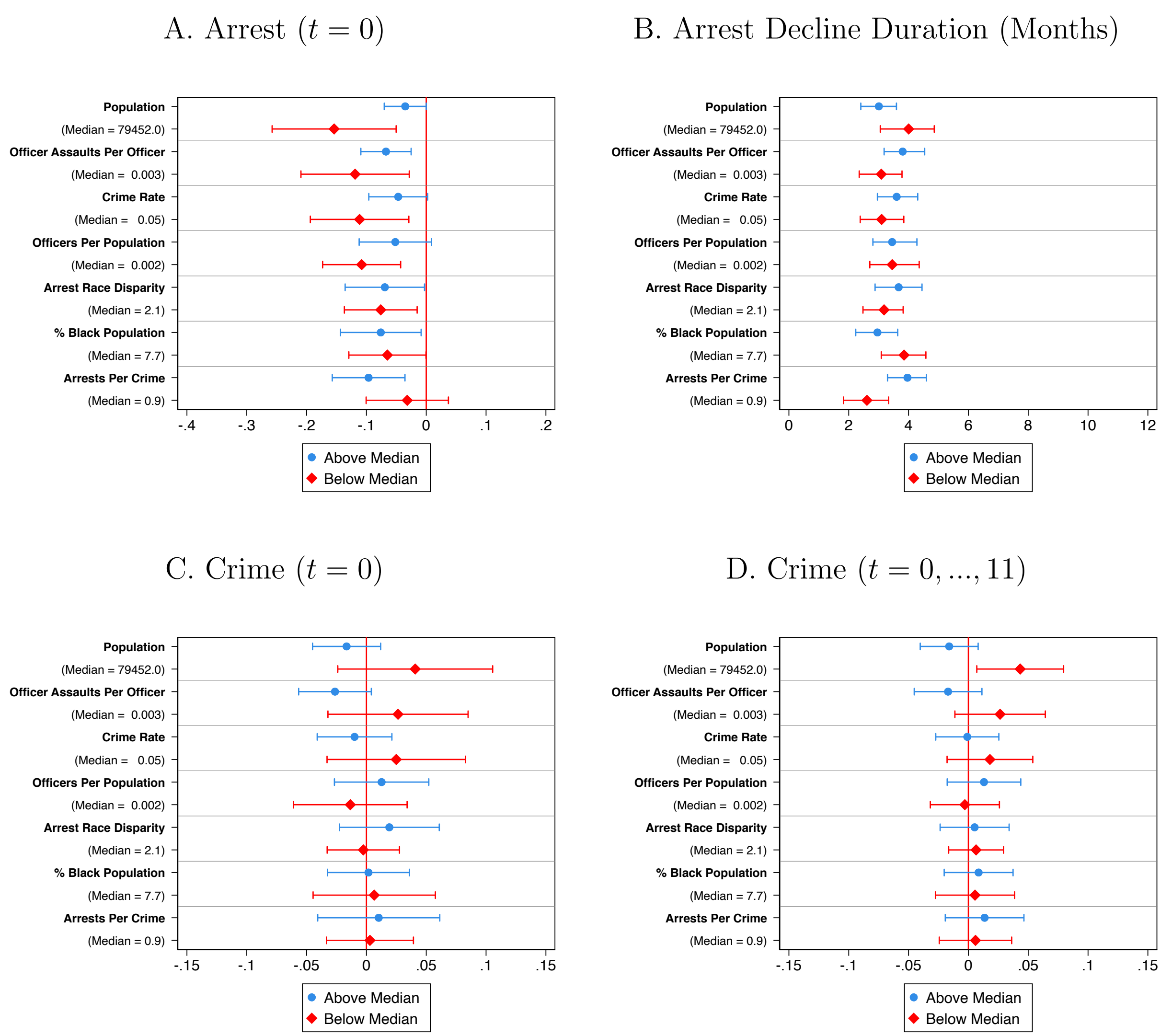
Notes: This figure plots Sun and Abraham (2020)'s proposed "interaction-weighted" coefficient estimator. This estimator combines cohort-specific treatment effects, based on treatment timing, using strictly positive weights. To estimate this model, we include a separate panel for each treatment event, rather than each city. All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Months -6 and 6 include all months before month -6 and all months after month 6, respectively. Standard errors are clustered at the department level.

Figure A7: Event-Study: Omitting Agency-Specific Linear Time Trends



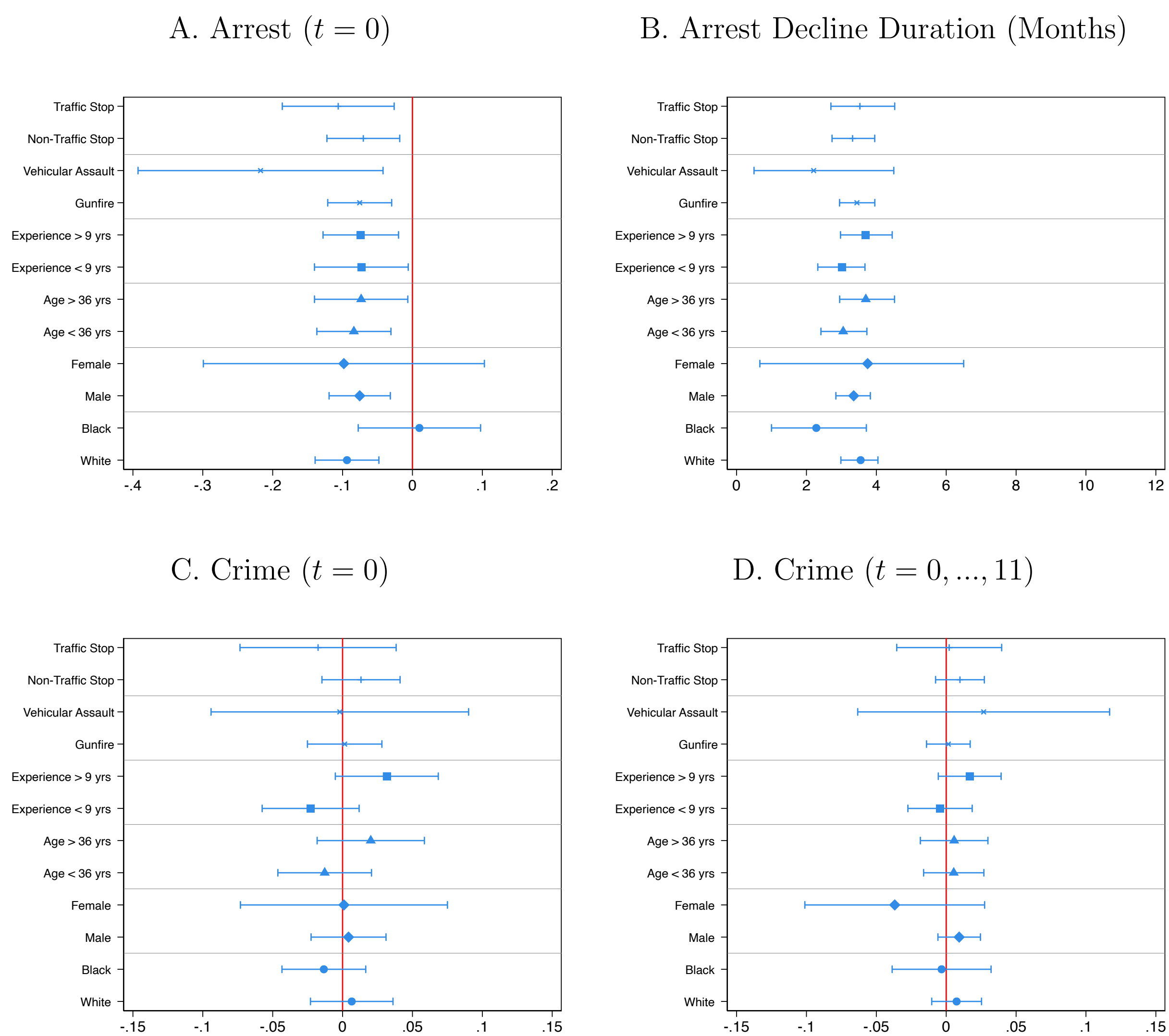
Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects. Months -6 and 6 include all months before month -6 and all months after month 6, respectively. Standard errors are clustered at the department level.

Figure A8: Crimes and Arrests by Department Characteristics



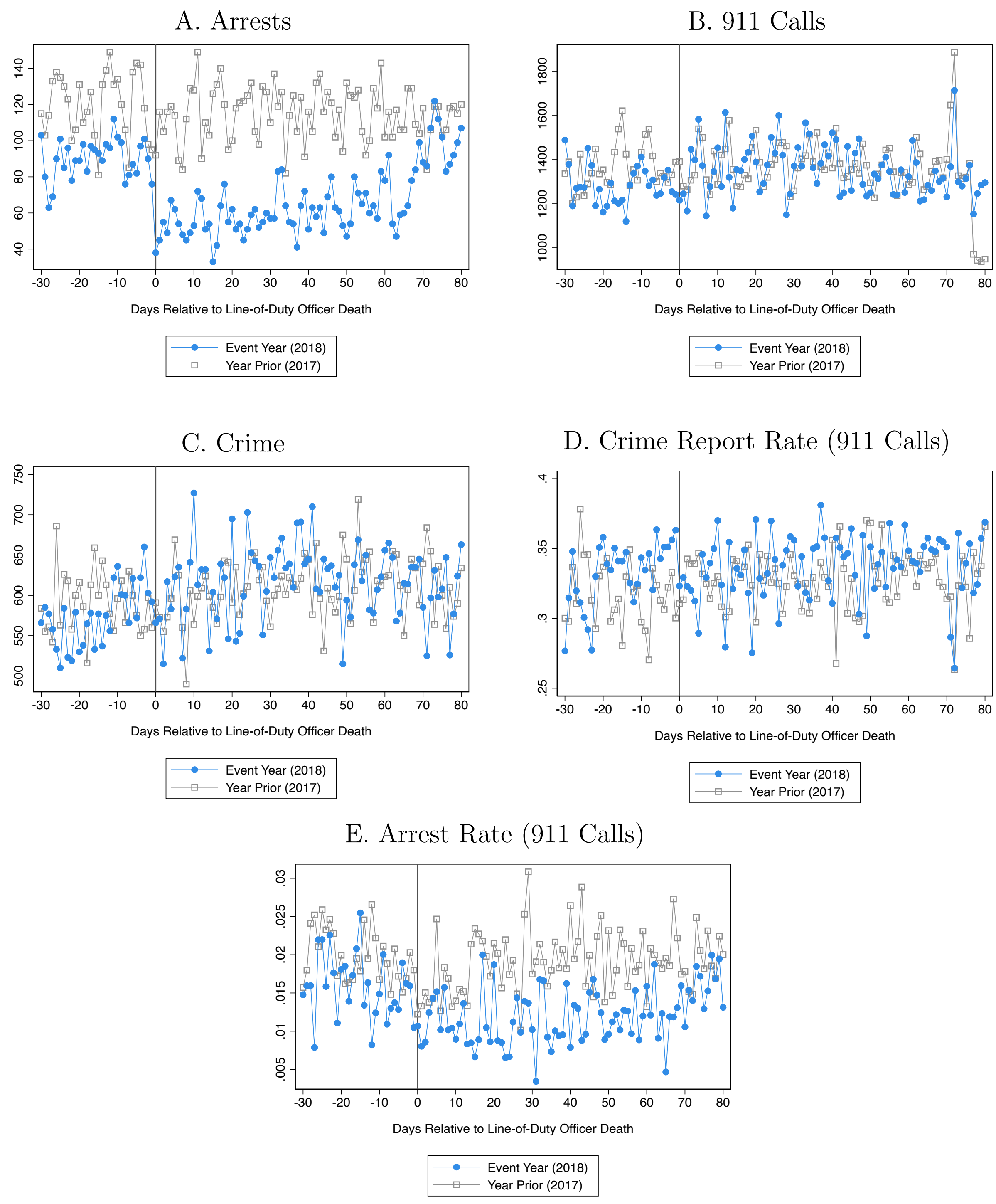
Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Regressions also include a dummy variable for 12 or more months after the occurrence of an officer death. Standard errors are clustered at the department level. This figure uses the demographics data from the 2000 U.S. Census, the American Community Survey 5-year estimates from 2010 to 2018 and the FBI's Law Enforcement Officer Killed or Assaulted (LEOKA). Each category uses the first reported measure to split by median. The Arrest Race Disparity is the ratio of arrests for Black civilians per Black population to arrests for white civilians per white population. Panel B shows the average arrest decline duration in the year following the death, and is determined by the number of consecutive months where the department's estimated arrest residuals are more negative than the residual for the month prior to the line-of-duty officer death. The gray bars in this figure represent the 95% confidence interval for each characteristic calculated using a bootstrapping approach with 200 iterations.

Figure A9: Crimes and Arrests by Officer Death Characteristics



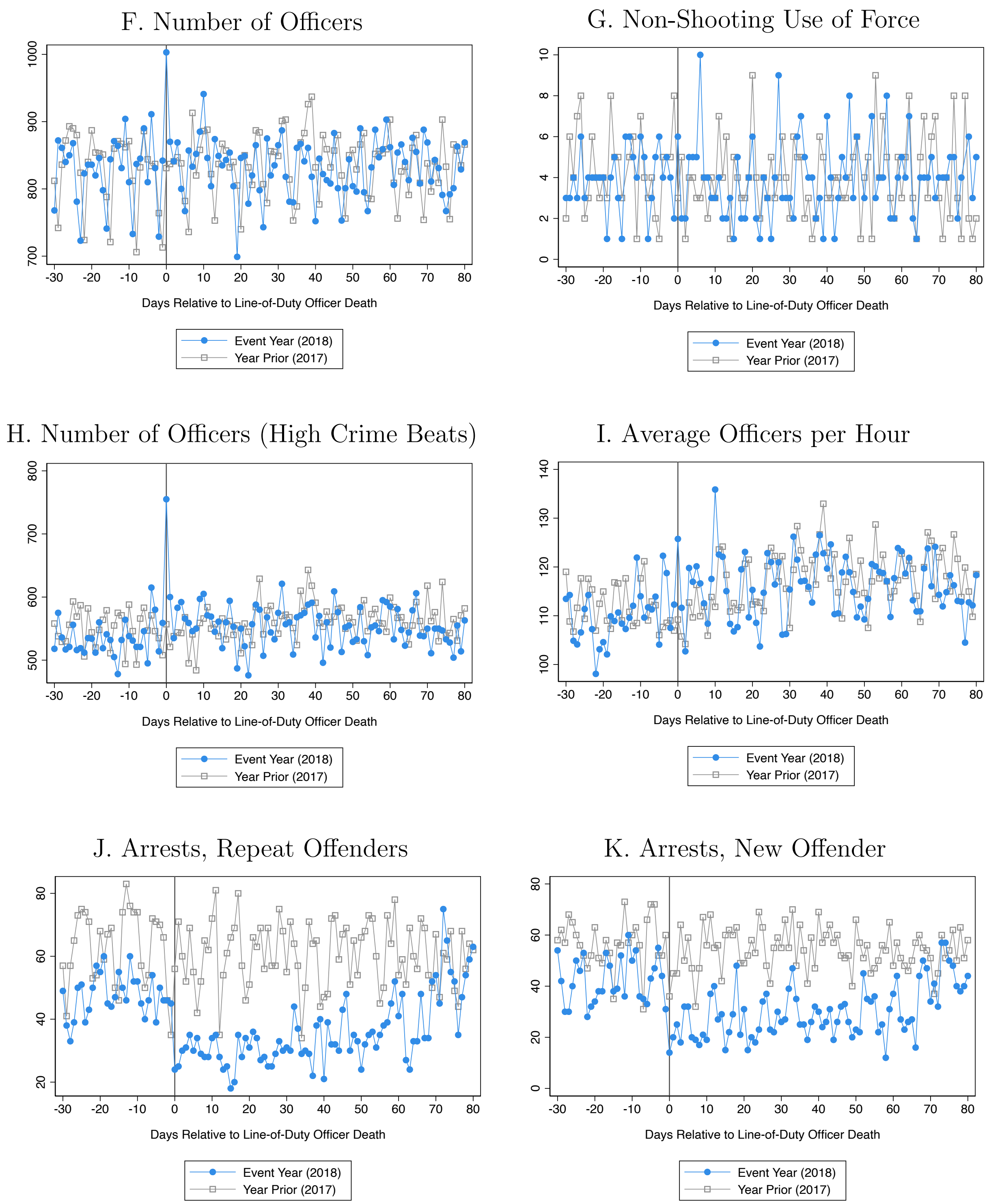
Notes: This figure uses a separate panel for each officer death treatment. For officer death events including multiple officer deaths, whether black or female officer was involved and average officer age and experience are used. All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Regressions also include a dummy variable for 12 or more months after the occurrence of an officer death. Standard errors are clustered at the department level. This figure uses records of officer death characteristics from the *Officer Down Memorial Page.* Panel B shows the average arrest decline duration in the year following the death, and is determined by the number of consecutive months where the department's estimated arrest residuals are more negative than the residual for the month prior to the line-of-duty officer death. The gray bars in this figure represent the 95% confidence interval for each characteristic calculated using a bootstrapping approach with 200 iterations.

Figure A10: Daily Plots: Case Study in Dallas, Texas



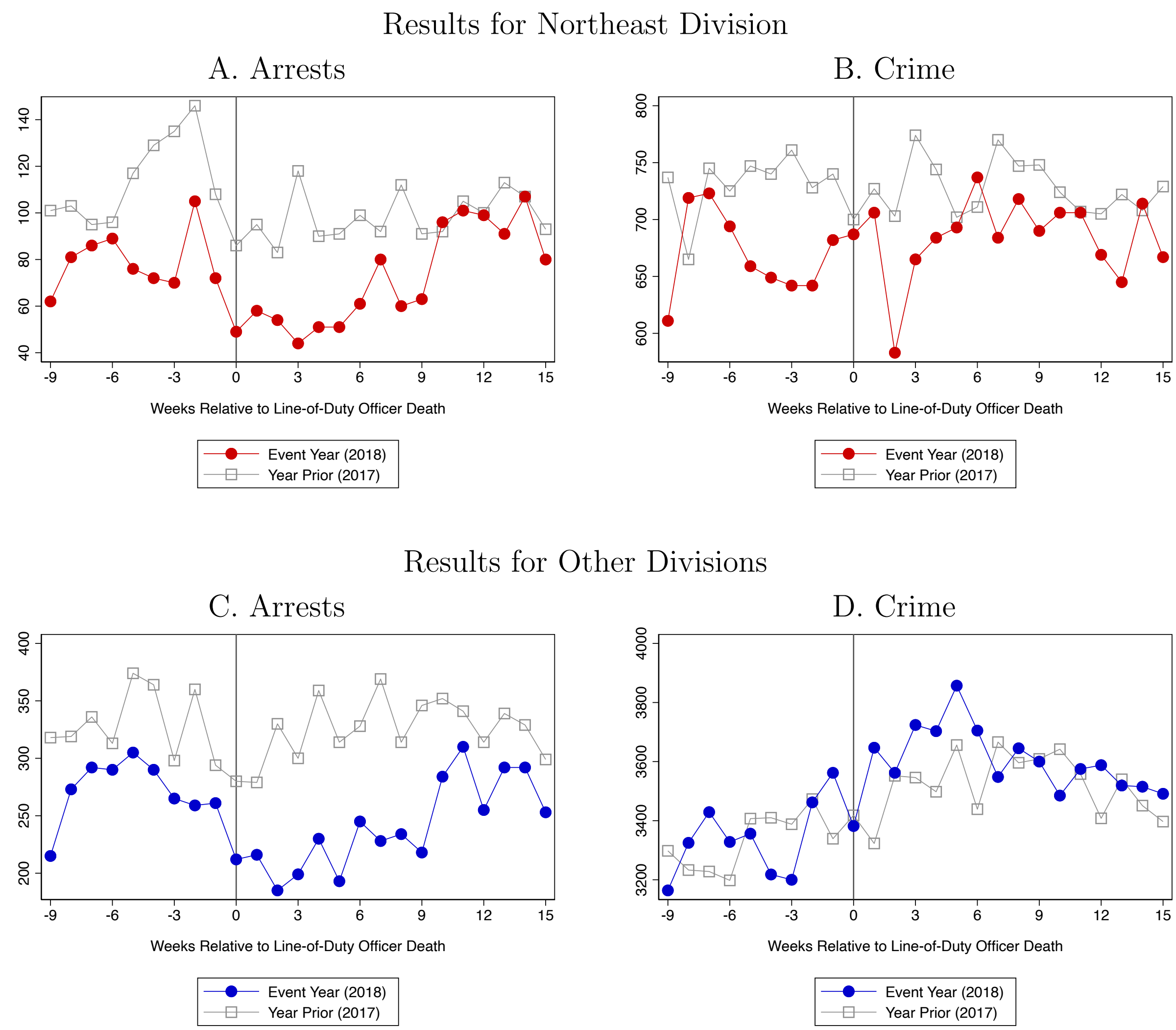
Notes: This figure plots outcomes using data from the Dallas Police Department around the date of the shooting of Officer Rogelio Santander on April 24, 2018 (similar to Figure 10). Crime is measured when any official crime report is logged, and is not restricted to serious offenses (Panel B). The crime report rate is the share of 911 calls that result in a crime report being written by a responding officer (Panel D). The arrest rate is the share of 911 calls that result in an arrest (Panel E).

Figure A10: Daily Plots: Case Study in Dallas, Texas (Continued)



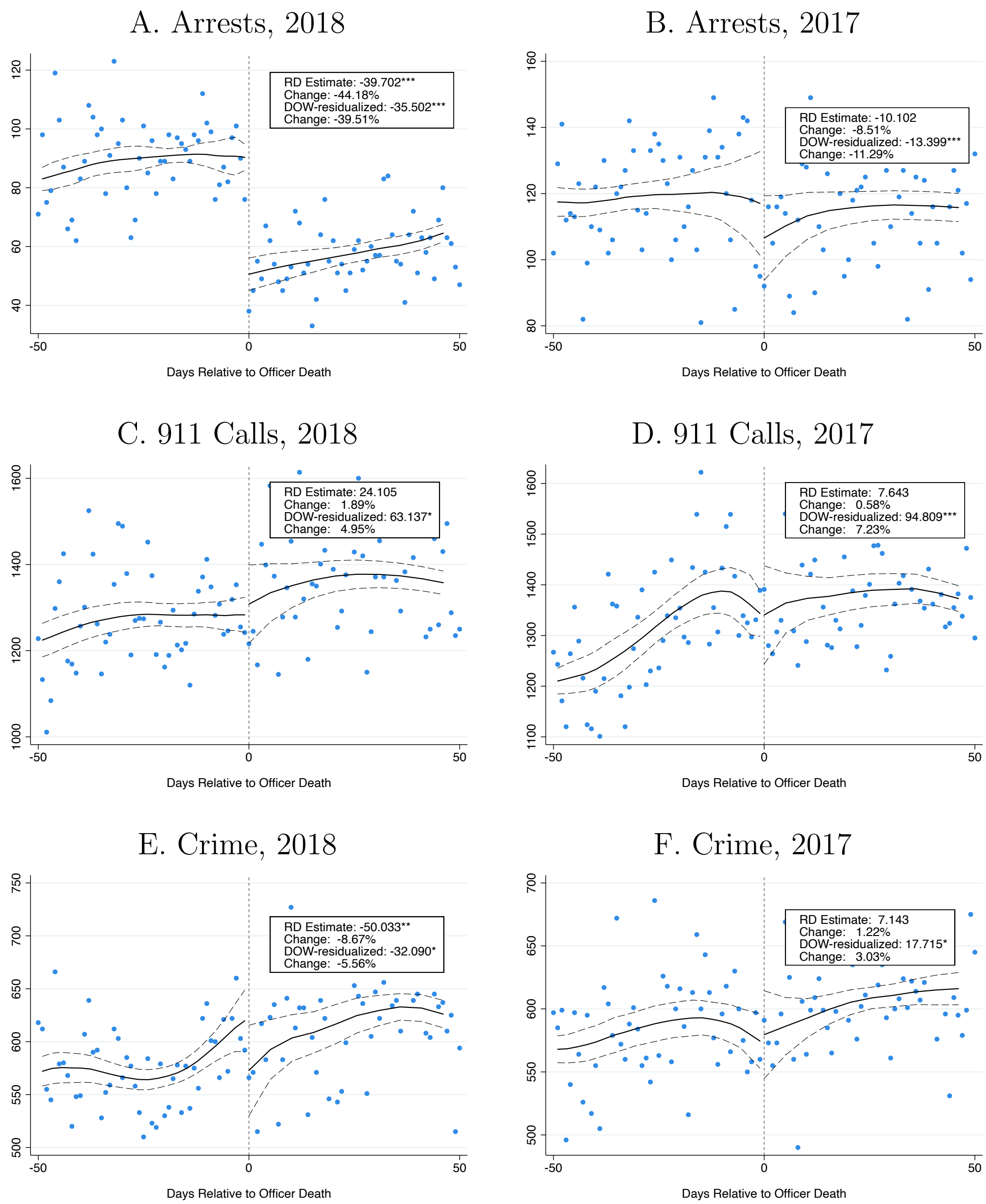
Notes: This figure plots outcomes using data from the Dallas Police Department around the date of the shooting of Officer Rogelio Santander on April 24, 2018 (similar to Figure 10). Non-shooting use of force is calculated using "Response to Resistance" data published by the Dallas Police Department (Panel G). Panel F and H plot the number of officers observed responding to any 911 call in the whole city and high crime beats, respectively. High crime beats are defined as beats in the top 25th percentile of total crime reports. Repeat Offenders are individuals who have been arrested for a previous offense since 2014 (Panel J).

Figure A11: By Geography Area: Case Study in Dallas, Texas



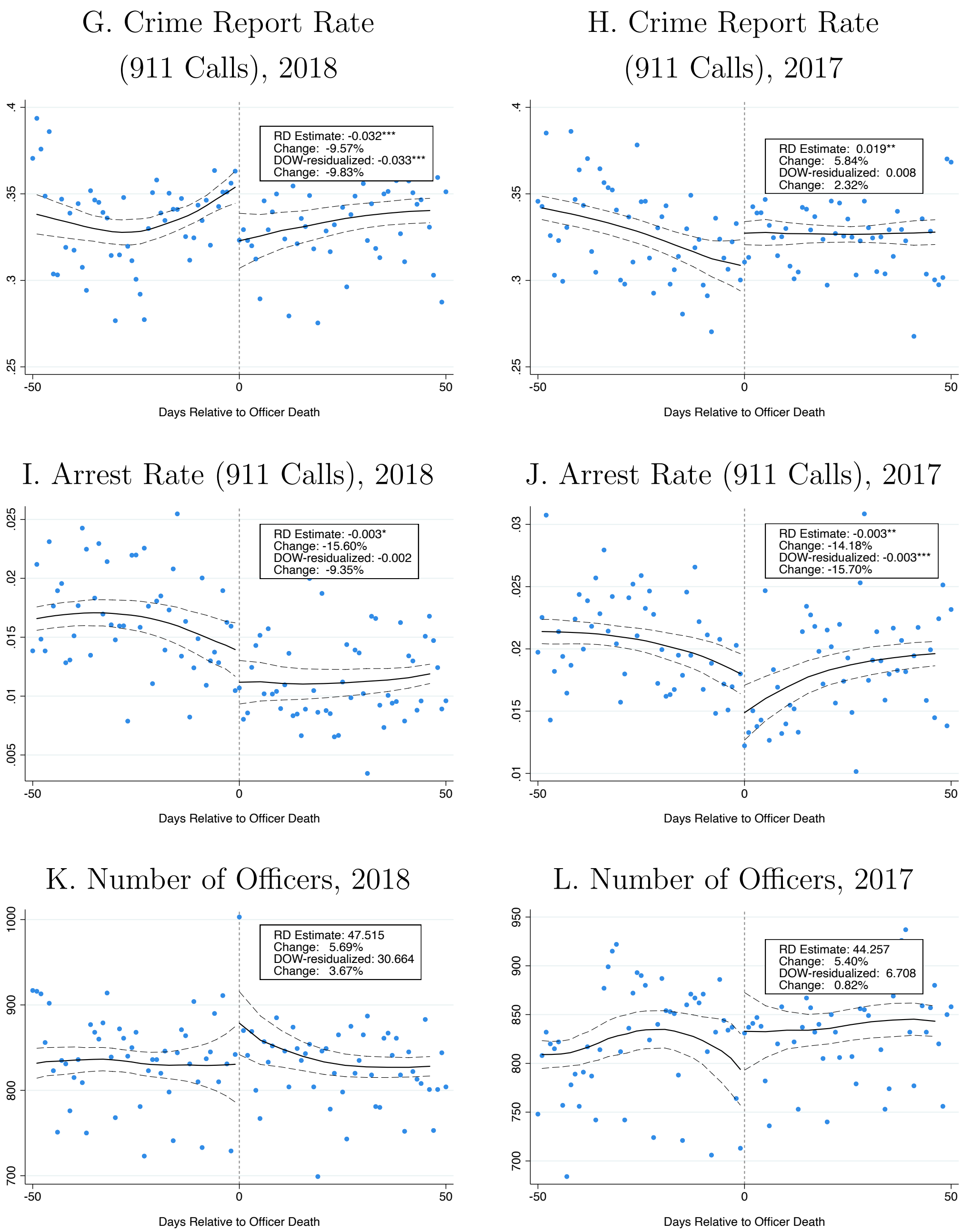
Notes: This figure plots outcomes using data from the Dallas Police Department around the date of the shooting of Officer Rogelio Santander on April 24, 2018 (similar to Figure 10). Panels A and B show the weekly results for the police division where Officer Santander worked. Panels C and D show the results for other police divisions in Dallas.

Figure A12: Regression Discontinuity: Case Study in Dallas, Texas



Notes: This figure plots outcomes using data from the Dallas Police Department around the date of the shooting of Officer Rogelio Santander on April 24, 2018 (similar to Figure 10). The graphs show the results of a regression discontinuity regression using date as the running variable and a symmetrical optimal bandwidth to reduce mean-squared error. Percent change in the outcome relative to the level before the cut-off is also shown. * p<0.1,** p<0.05, *** p<0.01.

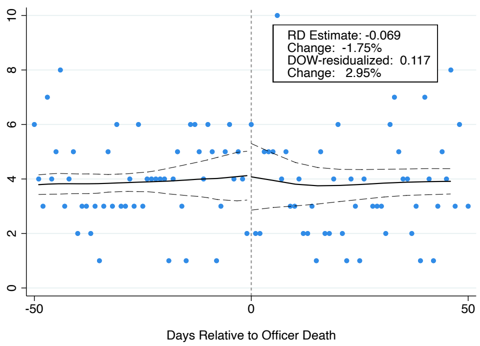
Figure A12: Regression Discontinuity: Case Study in Dallas, Texas (Cont.)



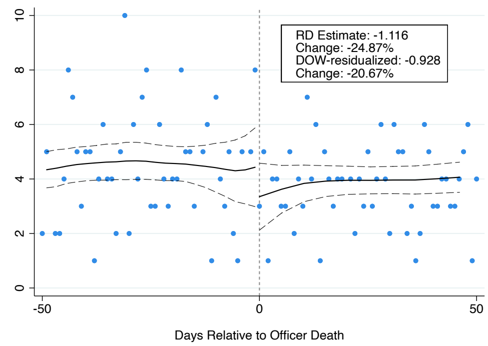
Notes: This figure plots outcomes using data from the Dallas Police Department around the date of the shooting of Officer Rogelio Santander on April 24, 2018 (similar to Figure 10). The graphs show the results of a regression discontinuity regression using date as the running variable and a symmetrical optimal bandwidth to reduce mean-squared error. Coefficient effects with significance are bolded on the plot. Percent change in the outcome relative to the level before the cut-off is also shown. * p<0.1,** p<0.05, *** p<0.01.

Figure A12: Regression Discontinuity: Case Study in Dallas, Texas (Cont.)

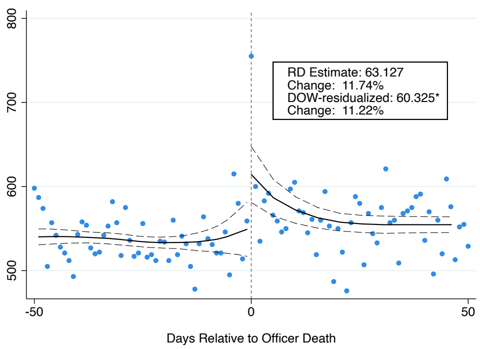
M. Use of Force, 2018



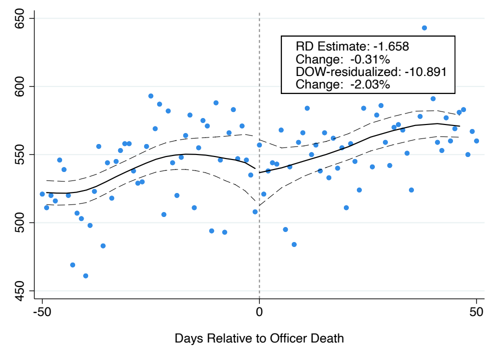
N. Use of Force, 2017



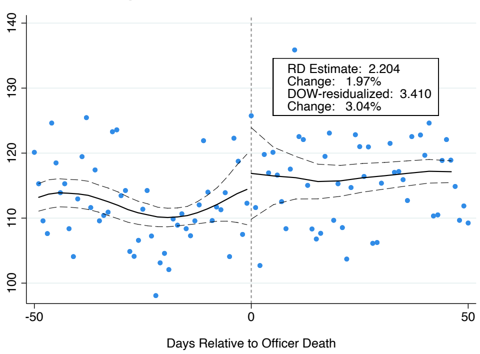
O. Number of Officers (High Crime Beats), 2018



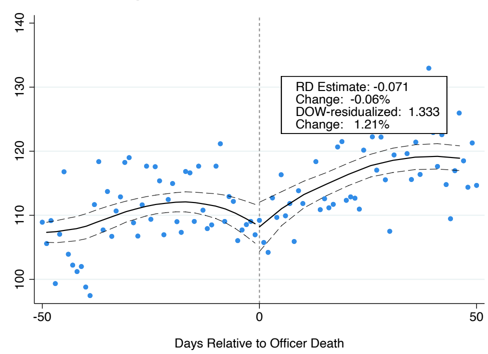
P. Number of Officers (High Crime Beats), 2017



Q. Average Officers per Hour, 2018

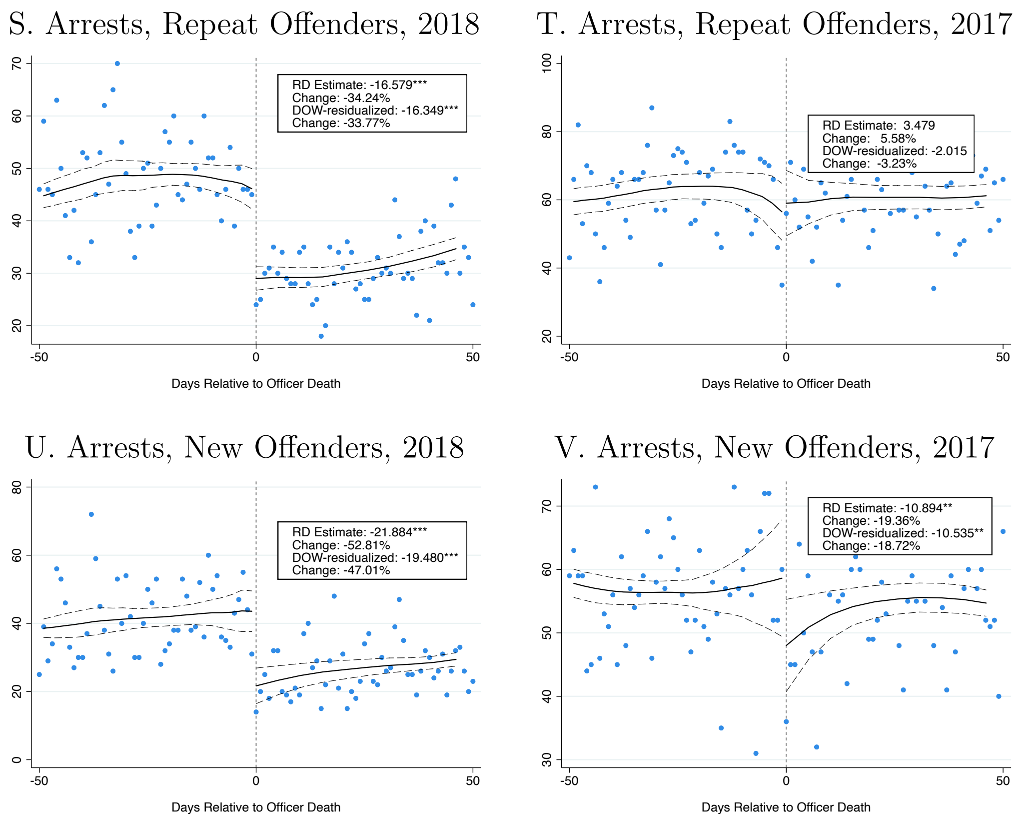


R. Average Officers per Hour, 2017



Notes: This figure plots outcomes using data from the Dallas Police Department around the date of the shooting of Officer Rogelio Santander on April 24, 2018 (similar to Figure 10). The graphs show the results of a regression discontinuity regression using date as the running variable and a symmetrical optimal bandwidth to reduce mean-squared error. Coefficient effects with significance are bolded on the plot. Percent change in the outcome relative to the level before the cut-off is also shown. * p<0.1,** p<0.05, *** p<0.01.

Figure A12: Regression Discontinuity: Case Study in Dallas, Texas (Cont.)



Notes: This figure plots outcomes using data from the Dallas Police Department around the date of the shooting of Officer Rogelio Santander on April 24, 2018 (similar to Figure 10). The graphs show the results of a regression discontinuity regression using date as the running variable and a symmetrical optimal bandwidth to reduce mean-squared error. Coefficient effects with significance are bolded on the plot. Percent change in the outcome relative to the level before the cut-off is also shown. * p<0.1,** p<0.05, *** p<0.01.

A2 Google Search Trends Description

Each search term is an exact first and last name for the individual in the U.S. state where the death occurred. We identify high-profile civilian deaths using a list compiled by *Black Lives Matter*, and identify officer deaths by linking the FBI LEOKA data we use in this project to records from the *Officer Down Memorial Page* to obtain officer names. Each search is centered around the time period of -1. Further, each search is benchmarked by topical searches for the most common cause of death, heart disease, which is relatively stable in popularity across time and locations within the U.S. Google Trends plots relative search intensity with a maximum search popularity in each search of 100. The use of a benchmark is important in this analysis, as it helps to rescale other outcomes in terms of their importance over time and across geographic areas.

A3 Data Appendix

A3.1 Data Sources

Law Enforcement Officers Killed or Assaulted (UCR LEOKA) The FBI's Law Enforcement Officers Killed or Assaulted (LEOKA) data set contains detailed information on total officer employment and officers that are killed or assaulted in the field for each month. We use officers feloniously killed in the line-of-duty as a measure of officer deaths and all assaults on sworn officers whether or not the officers suffered injuries. We utilize the version cleaned and formatted by Jacob Kaplan available from ICPSR (Kaplan, 2020a). This dataset covers the period 2000-2018.

Crime Offense Data (UCR Crime) The Uniform Crime Reporting Program Data: Offenses Known and Clearances By Arrest data set contains offenses reported to law enforcement agencies. The crimes reported are homicide, forcible rape, robbery, aggravated assault, burglary, larceny-theft, and motor vehicle theft for each month. We utilize the version cleaned and formatted by Jacob Kaplan available from ICPSR (Kaplan, 2020b). This dataset covers the period 2000-2018.

Arrest Data (UCR Arrest) The Uniform Crime Reporting Program Data: The Arrests by Age, Sex, and Race data set contains the number of arrests for each crime type by age, sex and race. We use the total arrests and arrest sub-types in our analysis. We utilize the version cleaned and formatted by Jacob Kaplan available from ICPSR (Kaplan, 2020c). This dataset covers the period 2000-2018.

Use-of-Force Data (UCR Supplementary Homicide Reports) The Uniform Crime Reporting Program Data: Supplementary Homicide Reports data set contains the number of homicides. We utilize the version cleaned and formatted by Jacob Kaplan available from ICPSR (Kaplan, 2020d) covering the period 2000-2018. We use the "felons killed by police" circumstance in our analysis after restricting the sample to the agencies with other UCR outcomes. There are 582 agencies with at least one such event. Agency-by-month-level outcome is replaced as zeros when missing if the agency has reported any murder and follow the outlier cleaning method described below.

Use-of-Force Data (Fatal Encounters) Fatal Encounters is a national crowd-sourced database of all deaths through police interaction. We remove suicidal deaths from our analysis and restrict the sample to the agencies with other UCR outcomes. The data set covers the period 2000-2018 and we use 906 agencies with at least one such event.

Traffic Stop Data We use the standardized traffic stop data from the Stanford Open Policing Project. Each row of the data represents a traffic stop that include information on date, location, subject and officer characteristics and stop characteristics. We collapse the data at city-month level and drop the first and last month for each city to account for incomplete months. Each city has different period coverage and Pittsburgh, Pennsylvania and Gilbert, Arizona have the longest period (February 2008 to April 2014). We use 25 cities.

Traffic Accident Data: Fatality Analysis Reporting System (FARS) We use the Fatality Analysis Reporting System (FARS) of the National Highway Traffic Safety Administration (NHTSA) to create measure of traffic fatalities and those involving alcohol. The data include information on fatal injuries in a vehicle crashes. We collapse the accident-level data at city-month level to generate counts. For the accidents involving alcohol, we use the number of drunk drivers involved in a crash. This data element is most reliable from 2008 to 2014 when drivers with the blood alcohol concentration (BAC) 0.01 g/dL or greater are counted. Prior to 2008, all individuals involved in accidents are counted. After 2014, the BAC level measure is changed to 0.001 g/dL or greater for counting. The data covers 1,766 cities from 2000 to 2018 for any accidents and 1,561 cities from 2008 to 2014 for accidents involving alcohol.

911 Call Dispatch Data We have hand-collected administrative 911 dispatch call records through submitting open-records requests to cities across the U.S. This study includes 72 cities with dispatch data. The data sets for each city vary in the way that they record calls and must be cleaned in order to harmonize the data across cities. Each data set collected is first cleaned to exclude records of interactions that were initiated by officers rather than a civilian complainant call, which are sometimes included in dispatch data when an officer reports his location in such an interaction to a dispatcher. These may include records of officers assisting other officers in distress, assisting the fire department, or responding to traffic violations. We code domestic violence calls using key words included in the 911 call description field. High-, medium- and low-severity calls are classified by utilizing the priority code ranking for calls. Lastly, we calculate the share of calls that result in an officer writing a crime incident report or "Crime Report Rate (911 Calls)" through examining the outcome or disposition of each call which is coded as a field in our data.

Demographic Data (U.S. Census and American Community Survey) We use the 2000 United States Census and the American Community Survey (ACS) 5-year estimates from 2010 to 2018 to provide information on city characteristics. Specifically, we report each city's population, share Black, Hispanic and white, share male, the share of female-headed household, the share in each age category, the share in each education category, the unemployment rate, the poverty rate and median household income. We linearly interpolate for years 2001 to 2009.

A3.2 Sample Restrictions and Identifying Outliers

The UCR data suffer from reporting and measurement issues. To alleviate concerns about data quality, we take following procedures to extensively clean the outcomes of interest. First, we restrict our analysis to municipal police departments serving cities with population larger 2,000 residents and to period 2000-2018. Then, we keep departments that consistently report these outcomes after replacing any negative arrest or crime values as missing. Specifically, we only retain agencies that report crimes monthly each year in the period 2000-2018 (for example, this procedure drops agencies that report annually or biannually).

To clean the potential outliers in the UCR data, we separately regress each arrest and crime outcomes on a cubic polynomial of time for each department. These outcomes are the raw values plus one to account for the large number of zeros in the data. Then, we calculate the percent deviation of the predicted value from the actual value and replace the value as missing when it is greater than the 99.875th percentile or below the 0.125th percentile of these percent deviations within population groups. These population groups are departments serving cities with population size of 2,000-4,999, 5,000-9,999, 10,000-24,999, 25,000-49,999, 50,000-99,999 and 100,000 or higher and are defined using the first reported population in the data. Finally, we take same procedure to clean larger group categories of arrest and crime by type. Then, we replace any subgroups of outcomes as missing if the larger group category is identified as an outlier.

We merge the UCR data together using the originating agency identifiers, the Traffic Stop, FARS and 911 Calls data using the city name and Census data using the Federal Information Processing Standards (FIPS) Place code.