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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 means to promote public safety. Police make over 10 million arrests each year in the U.S., 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.

Criminal justice reform advocates argue that police should reduce its heavy reliance on enforcement of low-level offenses, an approach popularized since 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, 2021) as well as financially distressed communities (Department of Justice, 2015; Makowsky et al., 2019). Opposing scholars and critics argue that aggressive enforcement of low-level offenses has been instrumental in the crime decline of the last three decades (Bratton and Knobler, 2009; Zimring, 2011; Riley, 2020). Many of these same critics also contend 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] Clearly, a central issue in this debate is whether reductions in enforcement cause increases in crime, a question on which there is little empirical evidence.

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

[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 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 *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`)

arrests are generally nonrandom and often reflect or coincide with changes in underlying crime rates (Harcourt and Ludwig, 2006). 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 show 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, as a result, these events appear to meaningfully affect police willingness to engage with civilians and make arrests. However, these events are less likely to affect community social unrest or civilian criminal activity. Indeed, we show evidence from Google search trends that officer deaths attract limited attention in the community. This muted response stands in contrast to the amplified community attention paid towards high-profile deaths *by* police. A growing literature has documented that high-profile police use of force incidents are followed by large reductions in police arrests and increases in crime, both of which may be influenced by heightened social unrest in the community.[4] Our setting of limited community salience is uniquely suited for evaluating the impacts of changes in arrests on crime rates.

We examine data from over 1,500 municipalities between 2000-2018 and document that a line-of-duty death is followed by a significant 10% decline in police arrest activity over one to two months. 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, the 10% decline in arrests is substantial. 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

[4]We discuss this growing literature below, see for example Prendergast (2001); Shi (2009); Heaton (2010); Rivera and Ba (2019); Cheng and Long (2018); Devi and Fryer Jr (2020); Premkumar (2020); Ang et al. (2021); Prendergast (2021).

crime responses (Di Tella and Schargrodsky, 2004; Draca et al., 2011; Weisburd, 2021; Jabri, 2021; Lovett and Xue, 2022).

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 3.6% (2.6%) in felony "index" crimes, or the most serious violent and property crimes defined by the Federal Bureau of Investigation (FBI).[5] Our point estimates suggest an elasticity of crime-to-*total arrests* of 0.38 for violent crime and -0.10 for property crime. These estimates are novel in the literature, and they are notably less negative than the estimates of the crime-to-*police employment* (or *police presence*) elasticity found in previous studies (see Figure 8).

Given that the average decline in arrests has a magnitude of 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 magnitude and duration of treatment effects. We fail to find evidence of a threshold arrest decline magnitude or duration above which crime increases, even when examining arrest reductions that are larger than 30% or 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. In addition, we calculate that if all U.S. departments reduced their arrests for only two months per year by the average impact we observe after a line-of-duty death, this decline would translate to about 116,000 arrests foregone annually and a statistically insignificant impact on crime. Collectively, these estimates 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 interpret our estimates as reflecting the causal impact of a marginal reduction in arrest activity on crime. Doing so requires assuming that arrest activity is the only variable directly impacted by the line-of-duty death, and we provide evidence to rule out potential violations of this assumption. To address the concern that criminal offenders directly respond

[5] Index crimes include murder, rape, robbery, burglary, theft, and motor vehicle theft. We consider murder separately from other violent crime to account for changes in this outcome related to the officer death itself (see Section 4).

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.

One related challenge with studying the impact of police behavior on crime rates is that measured crime is partly a function of police reporting decisions. 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 56 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 911 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 relates to a growing literature on the impact of heightened public scrutiny of the police on police behavior and on criminal outcomes (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 community trust in police. We address a distinct but related question of whether reductions in arrests cause a change in crime, focusing on a context where civilian criminal behavior and distrust in the police are not elevated by a high-profile police scandal.

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. A related experimental literature has documented significant crime-reducing impacts from geographically-focused ("hot-spots") policing interventions (Braga and Bond, 2008; Braga et al., 2015; Weisburd et al., 2015; MacDonald et al., 2016), which entail multiple changes in enforcement, including greater police presence, and typically incorporate community participation. These various 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 55 cities and, to the best of our knowledge, represents the largest composite of 911 data used in an academic study to date.

The rest of our paper proceeds as follows. Section 1 provides background information on officer line-of-duty deaths. Section 2 describes our data, and Section 3 presents our empirical strategy. Section 4 describes our primary results, Section 5 provides robustness tests, Section 6 presents heterogeneity results, and Section 7 concludes.[6]

[6] A previous version of this study included a section with a case study of a single officer fatality in Dallas, TX. These analyses were based on public records requests made to the Dallas Police Department. We

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.[7] 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.[8]

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 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 commemorative fundraisers in honor of police officers who have died, often over National Police Week in May.[9] 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

requested the same data for the time period around the fatality multiple times, and upon further inspection, we found that our results varied significantly when using different versions of the records provided by the department. We have therefore decided to remove this section from the study.

[7]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/

[8]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

[9]See policeweek.org.

even memorial tattoos (Sierra-Arévalo, 2019).

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. 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. Similarly, researchers examining a publicized officer fatality event in New York City in 2014 have found that officers markedly reduced arrest and citation activity after the event (Sullivan and O'Keeffe, 2017; Chalfin et al., 2021). They further document that this reduction in arrests was not associated with any increase in serious crime, a finding that is consistent with our results at the national level. Ultimately, the aggregate effect of an officer death on police behavior can only be determined empirically. Our project provides the first national empirical estimate of this effect, and we find that police respond to peer deaths by reducing arrest activity in the short-term, and we do not find aggregate evidence that other dimensions of policing change, including use of force.[10]

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 by widespread protests and social unrest. As discussed above, deaths of civilians *by* police have been examined by researchers studying the impact of police reductions in effort, despite the fact that these civilian death events could also alter civilian behavior directly.[11]

Figure 2 plots the relative Google Trends search intensity of 137 high-profile deaths of civilians at the hands of police versus 71 officers killed in the field since 2010 using searches

[10]As discussed in Section 5.2 we do not find an effect of line-of-duty officer deaths on police use of force in our national sample.

[11]We discuss this growing literature above, see for example (Prendergast, 2001; Shi, 2009; Heaton, 2010; Rivera and Ba, 2019; Cheng and Long, 2018; Devi and Fryer Jr, 2020; Premkumar, 2020; Ang et al., 2021; Prendergast, 2021).

from the U.S. state where each event occurred.[12] Google Trends does not provide values for total number of searches; instead, 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. Given this type of output, the choice of an appropriate benchmark search term is critical, as a benchmark that is too popular would completely dwarf any evidence of search volume for officer death events.[13] We include as a benchmark topical searches for heart attacks, or myocardial infarction (as heart disease is the leading cause of death in the U.S.), which is searched relatively frequently and is not seasonal in search volume. This benchmark allows us to view a perceptible increase in searches at the time of the events and 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. 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 illustrative evidence supports our assumption that 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 5.3.

[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] For example, benchmarks that are sufficiently more popular, such as "Google" or "Youtube", would negate any perception of relative search volume for both civilian and officer deaths.

2 Data

2.1 Data Sources

This study combines national and local data sets from a large number of sources. Our sample includes 1,578 municipal police departments for agencies that report at least 9 years of continuous data through the present to the Federal Bureau of Investigation (FBI) Uniform Crime Report (UCR) program.

A total of 135 officer death events occur within 82 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.[14]

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 and quality, we first restrict to the agencies who report complete and continuous data on *both* arrests and crimes at the monthly level. Our sample period is 2000-2018. We include agencies whose records span at least nine consecutive years and include the latest year of data, 2018, meaning that each agency's panel starts between 2000 and 2010.[15] Our sample restriction differs from prior work that typically relies on *annual* data reporting or the population of municipalities.

Our crime and community activity outcomes also include records of 911 calls for

[14]We exclude 16 officer fatalities coded in the LEOKA data that could not be verified by either the Officer Down Memorial Page or an external source.

[15]We also clean the data to exclude a minority of observations where a police department lists crime or arrests as having a negative value. These negative values are very rare in practice. These missing values mean that the number of observations may differ slightly by crime or arrest outcome in our models. Negative numbers can be used to correct earlier reports of arrests or crimes that were misreported by an agency; however, they are not linked to a particular misreported month, so they cannot be used to update the crime or arrest data manually.

56 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. These data largely originate from departments' "computer-aided dispatch" systems for routing officers to calls, and in some cities the data include cases that are officer-initiated, such as a dispatch call to assist another officer. We remove all calls whose descriptions are indicative of an officer-initiated call, and we construct an agency-by-month count of number of civilian-initiated calls.

We also incorporate data on traffic stops collected by the Stanford Open Policing Project through open records requests. This data source covers 18 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 3).

2.2 Summary Statistics

Approximately 7 officer deaths occur in each year within our sample of 1,578 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 winter and 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.

[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.

Appendix Table A1 summarizes demographic characteristics of the sample at the yearly level. The average city in the sample has 41 thousand residents, is 68% white, has a poverty rate of 13%, and a median household income of $46 thousand dollars. In contrast, treated law enforcement agencies serve populations that are larger, more racially diverse, and more likely to live in poverty; on average, these cities have 240 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 (75 vs. 11 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 122 property crimes per month. The average police department makes 152 arrests per month, of which 83 are for "quality of life" or low-level offenses, 0.17 are for murder, 8 are for other violent crimes, and 20 are for property crimes.[17] For the sub-sample of agencies that have traffic stop and traffic fatality information, the average department makes over 6,200 traffic stops each month and the average city experiences 0.26 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 3. 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

[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.

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 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 B does not appear to show a temporary or systematic increase in serious felony or index crimes.

3 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 5. 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 loca-

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

tions 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 (12) and Appendix Figure A5). We also show that our baseline results are robust to a parsimonious model with no control variables or time trends, where treatment agencies are matched to control agencies using a nearest neighbor algorithm (Table A2, specification (13) and Appendix Figure A6).

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 5. 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. Lastly, as referenced above, we re-estimate a parsimonious version of the model with no demographic or time trend control variables, which compares matched treatment and control agencies selected using the nearest neighbor matching algorithm.

4 Results

Table 2 presents the central results. First, we examine murder crimes and arrests, as these outcomes capture the study treatment of a felonious death of an officer in the field. These analyses serve to validate the construction and linkage of our data, since our records of officer deaths and outcomes originate from different sources. For all analyses where violent crimes and arrests are the outcome, we exclude murder offenses. The top panel shows that the death of an officer while on duty coincides with a 39% increase in reported murder and a 11% increase in murder arrests. *We interpret this concurrent increase in murder as being a function of the officer death itself.* Indeed, if we adjust the murder outcome to subtract the number of officers killed in a fatality event, there is no significant change in murder in the focal month, as shown in Panel B of Figure 4 and the second line of Appendix Table A2, specification (1). Likewise, when this model is estimated in levels, the first month coefficient on reported murder is statistically indistinguishable from 1 (Appendix Table A2, specification

(8)), corresponding to the treatment of the officer death itself. We confirm the unexpected nature of treatment in Figure 4, which shows that there are no pre-trends in this outcome preceding an officer death.

Arrest activity is highly responsive to an officer death in the short-term. Total arrests decline by 9.5% in the month of an officer death, and these declines are similar in percentage magnitude across index (8.3%) and non-index (8.9%) arrests. The arrests for the lowest level offenses, "quality of life" arrests, decline at a higher rate of 9.4%. While the percentage declines are similar in magnitude across categories, the volume of arrests is greater for non-index and quality of life offenses, so these categories experience a greater decline in total arrest volume. Declines in traffic stops are large, but they are insignificant in the first two months following an officer death. The magnitude of these coefficients are roughly halved in the second month after the officer death. For nearly all arrest types, these coefficients are smaller and insignificant three to twelve months (the long-term effect) after the incident. An exception is the long-term coefficient for violent arrests; however, this long-term effect is not visible in the event-study version of the model, where there is no evidence of joint significance of post-period indicators (Figure 5). Overall, the event study versions of the arrest results in Figure 5 confirm the pattern of decreases in the first two months following an officer death and also provide evidence that there are no pre-trends in these outcomes.

Relative to the treatment group mean, the arrest decline in the two months following an officer death corresponds to an average decrease of 134 arrests, of which 20 arrests are for index violent and property crimes, 70 arrests are for "quality of life" offenses, and 44 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 3.6% (4.6%) in the month of an officer death (month after) with 95% confidence.

[20] The sub-category arrest counts are calculated from the coefficients on each arrest type and therefore do not sum directly to 134.

Over the longer-term, the estimates imply that we can rule out a 2.6% increase in index crime. While we observe a negative and significant long-term coefficient for violent crime, this effect is not evident or significant in the dynamic event study version of the estimation (Figure 6). Here, the lack of evidence of pre-trends is especially important; these plots confirm that officer deaths do not occur after an uptick in crime. Collectively, the pattern of findings for arrests and crime 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 the prior work on the impact of police manpower or presence on crime. To do so, we convert our estimates into an crime-to-*total arrest* elasticity 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, -0.10 for property crime and 0.38 for violent crime, and do not statistically differ from 0. Figure 8 shows that these crime-to-*arrest* elasticities are notably less negative when compared to the elasticity estimates of *police manpower* on crime, 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 given a change in *arrests* are small relative to the crime increases we would expect from a comparable percent decline in manpower.

Next, we investigate changes in 911 calls for service. 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. Here, we can rule out a greater than 3.8% (4.9%) increase in 911 calls in month 0 (month 1) and a 3.3% increase over the remainder of the year after an officer fatality.

Lastly, we find that the number of fatal traffic accidents does not increase following an officer death. The traffic fatality outcome has the advantage that it is a function of traffic offenses and is a proxy for reckless driving, but it is not a function of either victim reporting or police reporting, as nearly all fatal traffic accidents are reported. Despite the

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

large decrease in the point estimates on traffic stops following an officer death, the number of fatal traffic accidents does not change.[22] Here, we can rule out increases in traffic fatalities of more than 6.5% within the first month, 4.4% in the second month, and 0.04% in the remainder of the year, with 95% confidence. The estimate for the long-run impact on traffic fatalities is a marginally-significant *decline* of 2.5%, though we caution against interpreting this finding as a treatment effect given the time lag and lack of a short-term effect.

5 Robustness and Alternative Hypotheses

5.1 Robustness Tests

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

First, in Appendix Figure A2, 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 A3. 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. The first specification (1) replicates the baseline results and also includes an adjusted measure of the murder outcome that excludes officer fatalities. Using this adjusted outcome, we find no evidence that murders increase, confirming that the spike in murder is due to the treatment of the officer fatality itself.

Next, we show that the estimates are similar when we restrict the sample to treated

[22]While enforcement of traffic offenses has been shown to affect traffic offending (DeAngelo and Hansen, 2014; Goncalves and Mello, 2022), 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.

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.

Our estimates are similar when excluding the city-by-calendar month fixed effects from the model which adjust for seasonality in outcomes that may differ by department (5). In specification (6), 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.

Further, excluding arrests for driving under the influence (DUI), the single offense for which we observe the strongest arrest decline (see Section 6.4 below), does not change the pattern of the results in (7).

The results are also largely similar when using counts of arrests and crimes as outcomes (8). However, the standard errors are substantially larger, leading to less significant effects for our arrest declines. The results are also robust to a per capita model (9) and an inverse hyperbolic sine model (10).

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 (Borusyak and Jaravel, 2017; Goodman-Bacon, 2018). 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 (11), 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 A4. This methodology confirms our baseline findings, though their specification does require treating each line-of-duty death as its own panel.

The final issue we address relates to department-specific time trends in our outcomes. As we discuss above, crime is decreasing overall during our sample period, and this decline may be more pronounced in treated cities than non-treated cities. Our baseline specification includes city-specific linear time trends to address this issue. Nevertheless, our estimates show a significant decline in long-term violent crime and arrests after an officer death, which we are cautious to interpret as long-term treatment effects. Instead, this could be evidence that the difference in time trends across cities has not been sufficiently addressed in our preferred specification.

We consider two alternative specifications to probe the importance of department-specific time trends. In model (12), we show our baseline specification without controls for department-specific linear time trends. The size of the arrest declines are larger in this specification, and we continue to find no positive crime effects in any period and a long-term decline in violent crime. We show in Appendix Figure A5 that the event study estimates without linear time trends look similar to the baseline results. However, the long-term event study coefficients (period 6+) are more negative in this specification, highlighting the concern that treated departments could be on different time paths than untreated ones.

In model (13), we take an alternate approach to address this issue. We use a nearest neighbor matching approach to directly match pre-period trends of treated and untreated departments, similar to Cabral et al. (2021). Specifically, we use the nearest neighbor matching algorithm to match each treatment event to 10 control agency panels using information on demographic characteristics in the treatment year and lagged monthly crime and arrest levels in the year prior to treatment.[23] Importantly, these models benefit from the matching algorithm's ability to select control agencies with similar pre-treatment levels and trends, and after matching, the models exclude all demographic covariates and time trend variables. In this parsimonious specification, we find results that are consistent with our baseline model but do not show any evidence of divergence in long-term violent crime trends between treatment and control agencies. Appendix Figure A6 shows the raw means of arrests and crimes around the officer death, in addition to event study coefficients, confirming that treated and control agencies are well-matched on pre-period levels and trends.

5.2 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

[23] The matching variables are lagged values of log counts of violent and property crimes and arrests for periods -1, -2, and -3, and the slope of these outcomes between periods -3 to -12, as well as the treatment year city-level poverty rate, share white, share with a high school degree or less education, and total population. We chose to not use this specification as our preferred approach because several of our analyses require data that are only available for a subset of our cities. Using this approach for these additional analyses would require constructing a different set of matched control cities for each outcome.

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 officer fatalities that have no arrest declines actually experience a *reduction* in crime, as the above story would suggest. In Section 6.1 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.[24] For both outcomes, we find a small and statistically insignificant coefficient for the first-month effect of an officer death. This null finding provides illustrative evidence that there is not likely to be an increase in force in the immediate aftermath of the officer death. We find a marginally significant long-run *increase* in the Fatal Encounters measure. We view this evidence as suggestive that there is no use-of-force response to an officer fatality, as scholars have highlighted issues of under-reporting and data quality in these data series (Loftin et al., 2017; Renner, 2019; Goncalves, 2020).

5.3 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 from our 911 calls and traffic fatalities. 911 calls are a less filtered measure of victim crime reports

[24]This analysis excludes treatment events where the suspect of an officer fatality is shot and killed in the event to avoid a mechanical effect of the treatment on the outcome. The regressions include a panel for each treatment event in the data. Fatal Encounters was established in 2013 and includes back-filled data for earlier years; we restrict attention to records from 2010-2018 to address data quality issues in the data.

than crime rates, which are also a function of police officer decisions to record crimes. Traffic fatality events are nearly always reported and are not a function of victim reporting or police reporting behavior. Both of these measures show that complainant reports of offenses and driving offenses *do not appear to change* after 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 choose to 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). Within our 911 data, we are able to measure changes in officer reporting among cities that record whether a call results in a criminal incident report being written. This metric allows us to directly test whether the treatment of an officer death systematically changes the likelihood that police officers choose to report crimes, conditional on a 911 call response. 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 1.4% decrease in the reporting rate in the month of an officer fatality, off a base of 26%. 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.

5.4 Alternative Mechanisms for Arrest Decline

We have argued that the decline in arrests after an officer death is a behavioral response by fellow officers, caused by a heightened fear of on-the-job risk. A potential alternative explanation is that the decline is attributable to the direct effect of reduced manpower from the officer death. Similarly, it could be that our arrest decline is due to fellow officers taking leave because of their colleague's death or being re-routed to investigate their colleague's death and therefore not conducting regular patrols.

Our observed arrest declines are quantitatively too large to be solely due to a reduction in effective manpower. 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 4 arrests per month. In contrast, the first month coefficient in our models implies an average decline of 92 arrests, or roughly equivalent to 34 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 34 officers would reduce their arrest activity to zero after a colleague's line-of-duty death.

As a direct test of whether the arrest declines are due to officers investigating their colleague's death, we analyze our data separately by whether the suspect in the case is apprehended or killed within 48 hours. In these instances, any decline in arrests cannot be attributed to officers being re-routed to searching for a suspect. We present this analysis in Appendix Figure A8 (top two lines of figures). We find that our results are quantitatively similar regardless of whether the suspect is apprehended within 48 hours. This consistency implies that officer incapacitation is unlikely to be driving the arrest declines that we observe.

We can further validate a behavioral interpretation of the arrest decline by estimating responses to officer deaths that are caused by accidents rather than felony homicides. Appendix Table A5 estimates the arrest and crime results for accidental officer deaths that occur on the job, which are nearly all a result of car accidents. Here, officer fatalities are not counted as murders given their accidental nature. Officers do not respond to these events by reducing the number of arrests that they make and there is also no change in crime rates. This exercise shows that on-the-job fatalities caused by felony incidents are more impactful in inspiring a behavioral response from fellow officers.

How would the interpretation of our crime results change if the arrest decline were due to reduced effective manpower? If it is the case that an officer fatality has a meaningful impact on police presence, we should expect to see an increase in crime, given the large and robust literature on the impact of police manpower and employment (e.g. Evans and Owens, 2007; Chalfin and McCrary, 2018; Weisburst, 2019; Mello, 2019; Chalfin et al., 2020).

Therefore, this potential threat to our identifying assumptions would only bias our estimates towards finding crime increases, which we do not.

6 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 (holding fixed police presence) could be a general feature of the environment. 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 116,820 fewer arrests per year.[25] These foregone arrests would mean that affected individuals would 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 increase of 301 violent crimes and 12,697 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.

[25] 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`.

6.1 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.[26]

Figure 9 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 3.4% 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.

6.2 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 reduce low-level arrests over a longer time horizon? Though a two month reduction in low-level arrests is certainly not a permanent change, the 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

[26]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 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. Likewise, Jabri (2021) finds that predictive policing algorithms that increase local police presence within a *patrol shift* decrease serious felony crime. 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 6.1 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 10. 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.[27]

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 null finding is perhaps not surprising, since a sustained arrest decline is not likely to lead to a markedly different impact in the first month. However, it provides a placebo test that agencies with different durations of decline are not experiencing different crime responses 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 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. However, these

[27]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).

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.

6.3 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 A7 asks how our primary estimates vary with city-level characteristics. Overall, we find limited evidence of heterogeneity along these characteristics. Panel A shows that nearly all cuts of the data by department or city-level characteristics produce arrest declines that are similar in magnitude, of approximately 10%, with confidence intervals that overlap. Likewise, the average duration of arrest declines is quite similar across these sub-groups, between 2 to 4 months.[28] Consistent with this limited variation in average arrest decline response, the crime effects in month of treatment (Panel C) and in the first year after treatment (Panel D) are centered around 0 and have confidence intervals that overlap.

While none of the sub-groups statistically differ from one another, there is one suggestive pattern of interest. Cities with below median population have point estimates that show moderately larger reductions in arrests and increases in short term crime. It could be the case that these smaller departments experience more meaningful incapacitation of officers due to an officer death, and as a result might have a partial change in police presence that could be affecting crime. However, we are careful not to overstate this result, as these estimates are not statistically different from the other subgroups in our data, nor do we find positive crime impacts when stratifying directly on arrest reductions, as we showed in Section 6.1.

Appendix Figure A8 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 of similar size, with overlapping

[28] 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.

confidence intervals. There is no difference in the size of reduction for deaths occurring during traffic stops, or by officer age, experience, or gender. Likewise, as discussed above, the length of time it takes for the case to be cleared (suspect apprehended or in some cases killed), does not significantly change the arrest decline, suggesting that large numbers of officers are not incapacitated in the search for suspects when they remain at large for longer periods. 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. However, again, the event groups with larger arrest reductions do not exhibit a pattern of larger crime increases in response.

6.4 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 crime arrests and index crimes. For index crime arrests, we find significant decreases in robbery, aggravated assault, and motor vehicle theft arrests. For index crime, we observe no significant changes in any category in the first month of treatment or the month after. There is a long-term decline in aggravated assault arrests; here, we are cautious to interpret this as a treatment effect given the lack of long-term effects for any other sub-category of serious arrests.

The results for “quality of life” arrests and “non-index” arrests provide a more detailed picture of what types of arrests are reduced as a result of treatment. Table A4 shows that there are large and significant declines in arrests for weapons offenses, prostitution, driving under the influence of alcohol (DUI) (which is classified as a mid-level “non-index” offense), drug sale, drug possession, and arrests that are uncategorized in the UCR.[29] Several of these declines correspond to reductions that are greater than 10%. The results imply that over the two month period following an officer death, officers make 1.5 fewer arrests for weapons offenses, 3 fewer arrests for prostitution, 19 fewer DUI arrests, 9 fewer arrests for drug sales, 22 fewer arrests for drug possession, and 27 fewer uncategorized arrests in each treated city.[30] Given that we observe a large reduction in DUI arrests, we explicitly measure the

[29]The results also show marginally significant second month effects for other assault and vandalism.

[30]We assume that uncategorized arrests are likely to be for offenses that are not listed as options for

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 (7)).

6.5 Demographics of Arrestees

Another treatment dimension of interest is who is affected by the reduction in arrests that we observe. We investigate whether the declines are concentrated among particular demographic groups by regressing demographic-specific measures of log arrests on our treatment, using our preferred specification. Table A6 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 9.5% decline. The share of Black arrestees, 36%, and male arrestees, 76%, exceeds their respective population shares of 15% and 49% in the treatment sample. As a result, the equivalent percent declines across groups leads to a reduction in the disparity in levels of arrests across races and genders.

7 Conclusion

This study examines the causal impact of reducing police arrest activity on public safety. Using data on over 1,500 police departments between 2000-2018, we find that police respond to an officer fatality by reducing the number of arrests they make, particularly for low-level offenses. Our research collates data from numerous sources, including information on arrests, crimes, 911 calls for service, traffic stops, and traffic fatalities, in order 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 arrest reductions do not come at the cost of increases in serious crime.

By tracing an "arrest to crime curve" using variation across treated cities, we do not find a threshold level beyond which an arrest decline results in a crime increase. Moreover, examining treatment effects that last for differing amounts of time, we do not find evidence

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.

that arrest declines which persist for longer periods result in crime increases. Because the observed arrest decline is largest 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 findings stand in sharp contrast to the literature on police manpower, which documents significant reductions in crime from marginal increases in police presence. Consequently, our study provides new insights into this prior work by suggesting that the channel of the crime-reducing effect of police employment is likely general deterrence related to police presence rather than increased arrest activity.

Our findings raise important questions for future research. At a high level, if officers have some scope to reduce marginal arrests without increasing crime, one might ask whether officers are effectively optimizing their behavior to minimize crime, and if this is not their explicit objective, what objectives and incentives are motivating officer choices. At the same time, 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 how a permanent change in low-level enforcement or decriminalization policies would 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	0.221	(1.617)	354504	2.350	(6.357)	18510
Murder Arrests	0.165	(1.266)	354507	1.574	(4.890)	18510
Policing Activity						
Arrests	151.9	(479.4)	354507	964.5	(1716.5)	18510
Index Arrests	28.4	(94.2)	354507	177.0	(339.0)	18510
Violent Arrests	8.4	(41.1)	354507	62.0	(157.7)	18510
Property Arrests	20.0	(58.2)	354507	115.1	(200.4)	18510
Non-Index Arrests	40.9	(136.9)	354507	268.2	(505.4)	18510
Quality of Life Arrests	82.6	(263.9)	354507	519.2	(931.9)	18510
Traffic Stops	6200.8	(9489.0)	1491	9130.5	(11114.0)	423
Crime and Community Activity						
Index Crimes	140.0	(549.6)	354507	1023.5	(2032.5)	18510
Violent Crimes	18.3	(105.0)	354507	165.8	(412.0)	18510
Property Crimes	121.6	(452.9)	354507	857.7	(1654.9)	18510
911 Calls for Service	12235.3	(14869.1)	5724	25793.5	(19687.8)	1487
Crime Report Rate (911 Calls)	0.22	(0.08)	4458	0.26	(0.08)	1305
Fatal Traffic Accidents	0.26	(1.09)	283906	1.60	(3.61)	17040
Number of Agencies	1578					
Number of Treated Agencies	82					
Total Officer Death Events	135					
Treatments Per City (Treated)	1.65					
Officer Characteristics						
Cause of Death	*Gunfire*: 136		*Vehicular Assault*: 11		*Other*: 4	
Race	*White*: 115		*Black*: 20		*Other*: 16	
Gender	*Male*: 141		*Female*: 10			
Age	36.86	(9.16)				
Experience	11.14	(8.41)				

Notes: The number of agencies, number of treated agencies and total officer death events are from the data with crime and arrest activity outcomes. For the traffic stop outcomes, they are 18, 5, and 17. For the traffic accident outcome, they are 1252, 75, and 124. For 911 call outcomes, they are 56, 13, and 29. 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.

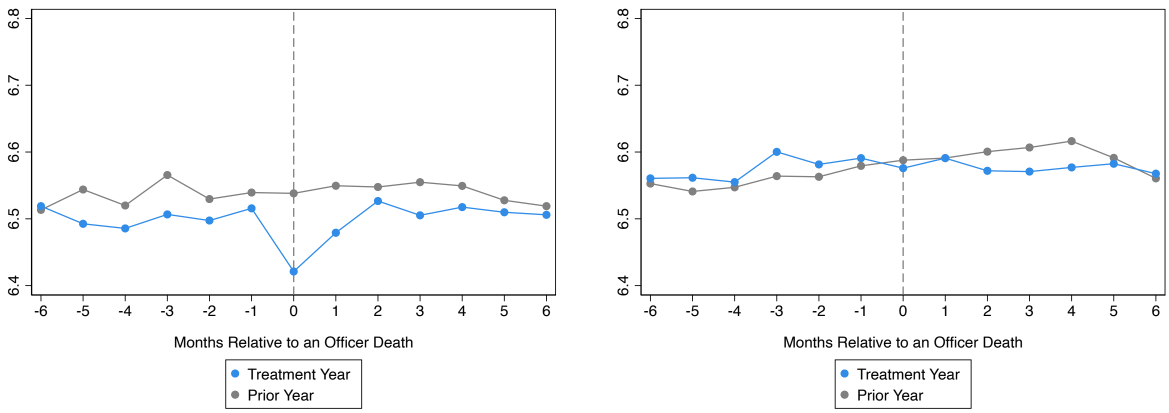
Table 2: Impact of an Officer Death on Policing and Crime

	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	0.391***	(0.058)	0.033	(0.039)	0.015	(0.013)	0.22	2.35	354504
Murder Arrests	0.111**	(0.044)	0.071	(0.043)	-0.000	(0.023)	0.17	1.57	354507
Policing Activity									
Arrests	-0.095***	(0.026)	-0.044*	(0.023)	-0.001	(0.023)	151.9	964.5	354507
Index Arrests	-0.083**	(0.033)	-0.024	(0.031)	-0.012	(0.027)	28.4	177.0	354507
Violent Arrests	-0.105***	(0.035)	-0.054**	(0.027)	-0.050**	(0.023)	8.4	62.0	354507
Property Arrests	-0.075**	(0.036)	-0.026	(0.037)	-0.009	(0.031)	20.0	115.1	354507
Non-Index Arrests	-0.089***	(0.024)	-0.076***	(0.026)	-0.013	(0.022)	40.9	268.2	354507
Quality of Life Arrests	-0.094***	(0.037)	-0.042	(0.032)	0.007	(0.030)	82.6	519.2	354507
Traffic Stops	-0.068	(0.107)	-0.146	(0.122)	-0.021	(0.094)	6201.7	9130.5	1477
Crime and Community Activity									
Index Crimes	0.003	(0.017)	0.015	(0.016)	0.000	(0.013)	140.0	1023.5	354507
Violent Crimes	-0.036	(0.027)	0.039	(0.029)	-0.034*	(0.018)	18.3	165.8	354507
Property Crimes	0.010	(0.018)	0.012	(0.016)	0.002	(0.014)	121.6	857.7	354507
911 Calls for Service	0.004	(0.018)	0.017	(0.016)	0.009	(0.012)	12239.0	25770.4	5682
Crime Report Rate (911 Calls)	-0.005	(0.005)	-0.003	(0.006)	0.001	(0.006)	0.22	0.26	4420
Fatal Traffic Accidents	-0.023	(0.045)	-0.016	(0.031)	-0.025*	(0.013)	0.26	1.60	283906

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 and arrest outcomes are 1578, 82, and 135, respectively. For the traffic stop outcomes, they are 18, 5, and 17. For the traffic accident outcome, they are 1252, 75, and 124. For 911 call outcomes, they are 56, 13, and 29. 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

A. Total Arrests B. Index Crimes

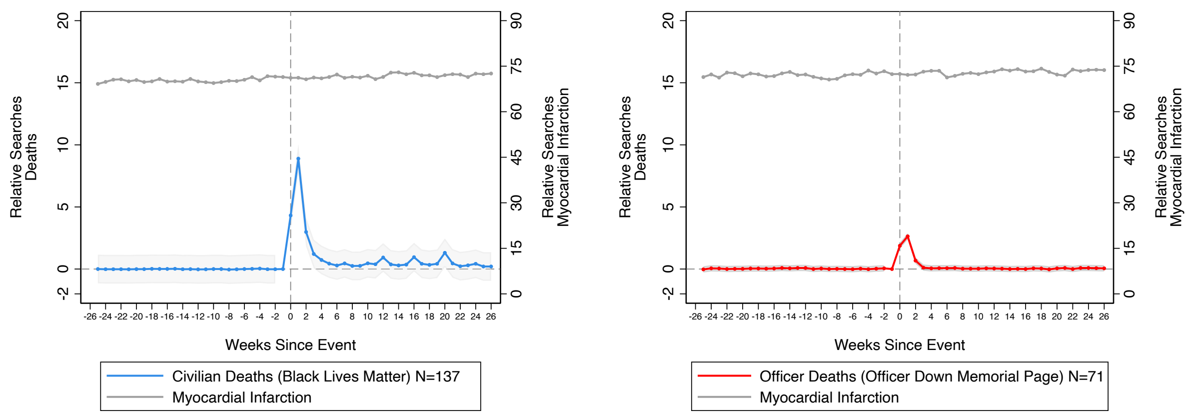


Notes: This figure plots the unadjusted data around the officer death events. Outcomes are defined as $Y_{it} = log(y_{it} + 1)$. There are 125 officer death events in 76 agencies after excluding events that do not have enough periods before and after the event. Index crimes include rape, robbery, aggravated assault, burglary, theft, and motor vehicle theft.

Figure 2: Google Trends Analysis

A. Civilians Killed by Police

B. Officers Killed in the Line-of-Duty

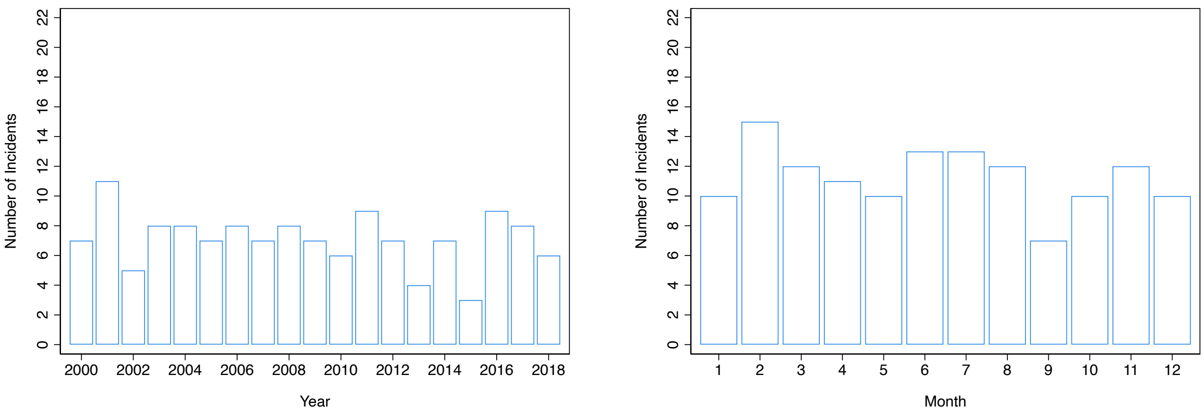


Notes: Each search term is an exact first and last name for the individual. We identify high-profile civilian deaths using a list compiled by *Black Lives Matter*, and identify officer deaths by linking the FBI LEOKA data we use in this project to records from the *Officer Down Memorial Page* to obtain 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

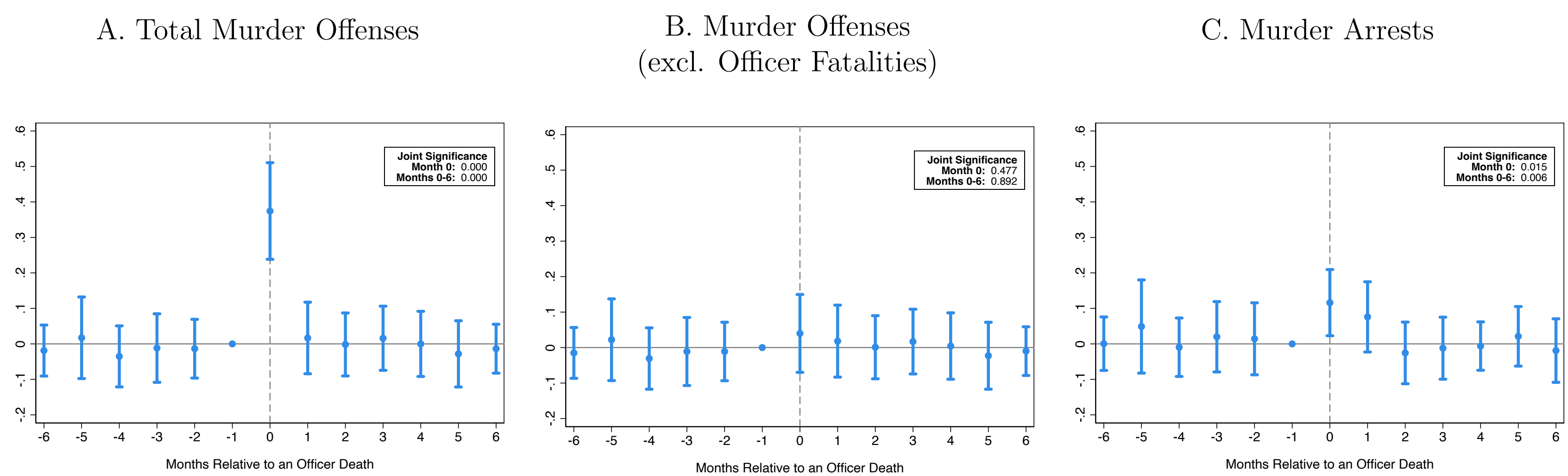
A. Officer Deaths by Year

B. Officer Deaths by Month



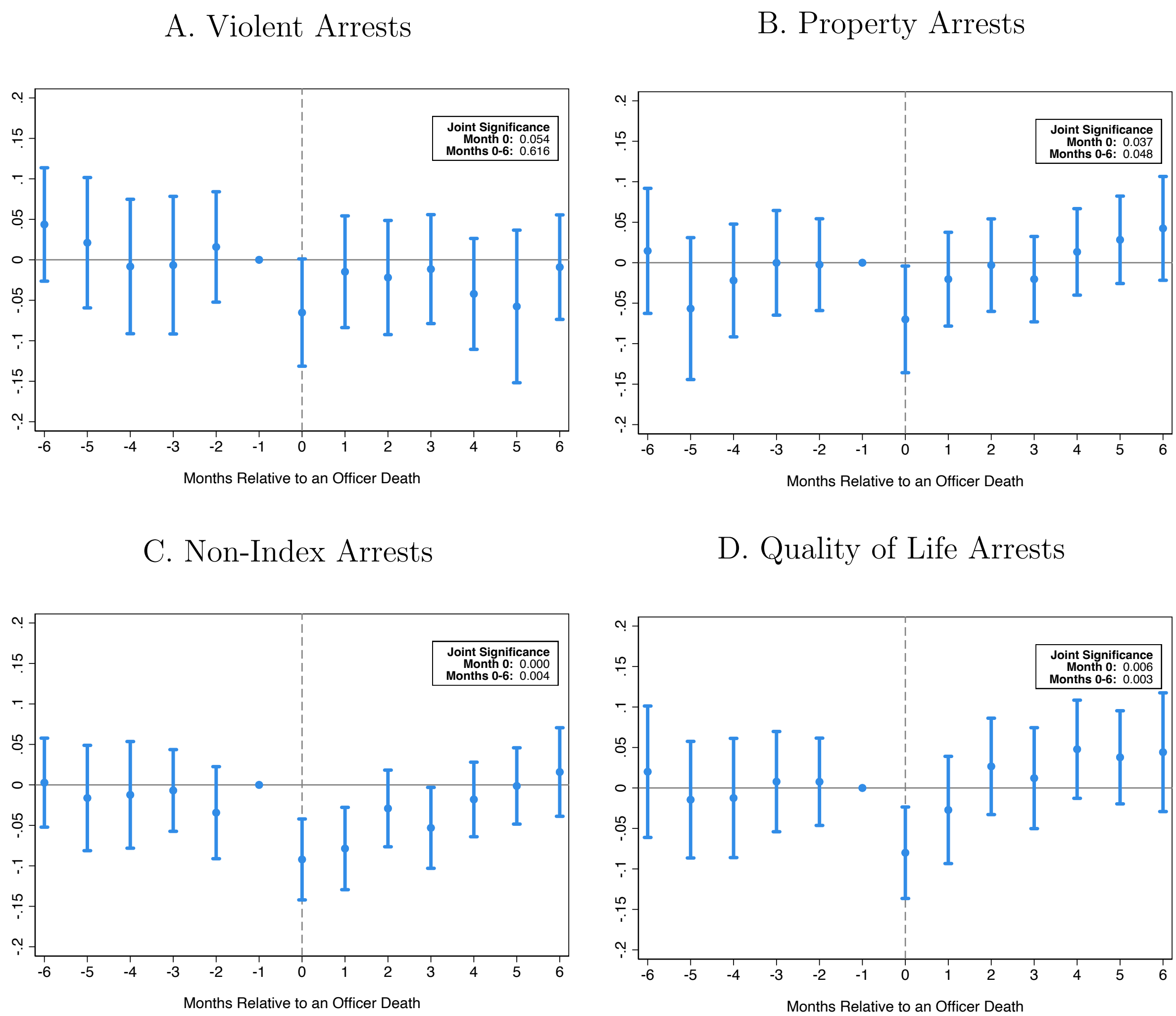
Notes: In 1,578 departments in our sample, there are a total of 135 officer death events in which 151 officers were killed.

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.

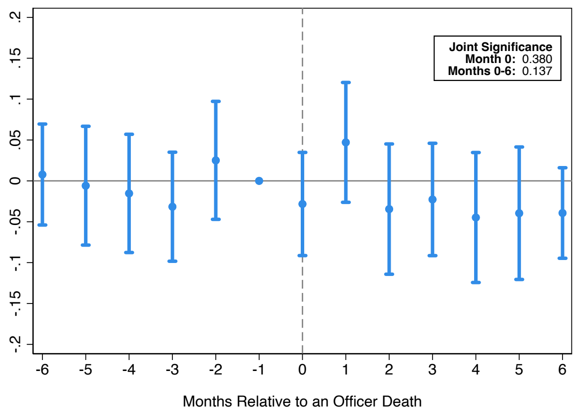
Figure 5: Event-Study: Arrests



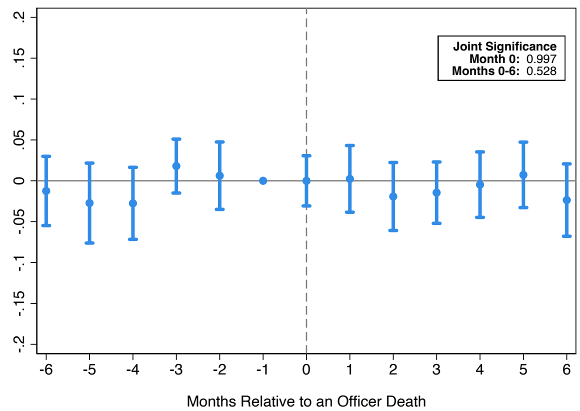
Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Months -6 and 6 include all months before month -6 and all months after month 6, respectively. Standard errors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

A. Violent Crimes



B. Property Crimes

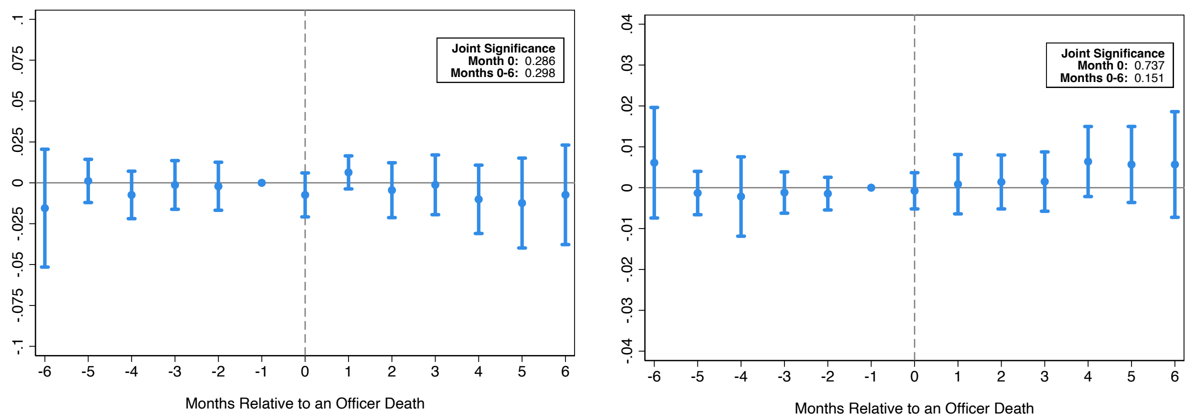


Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Months -6 and 6 include all months before month -6 and all months after month 6, respectively. Standard errors 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: Event-Study: 911 Calls

A. 911 Calls

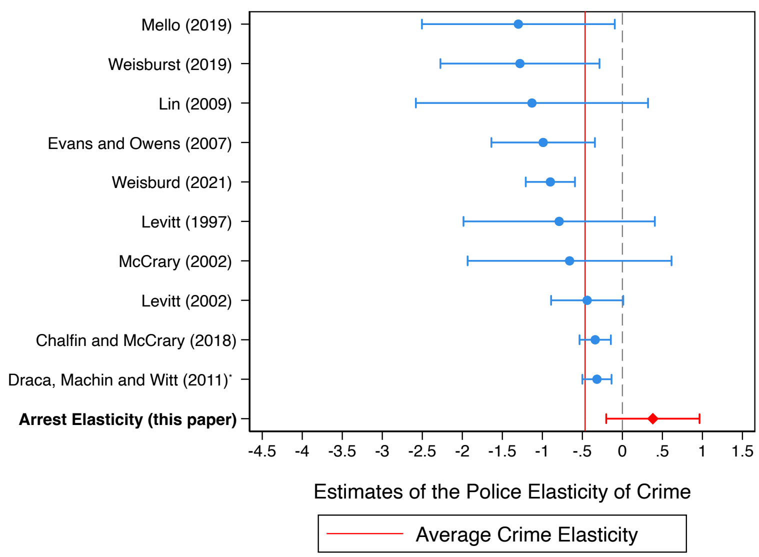
B. Crime Report Rate (911 Calls)



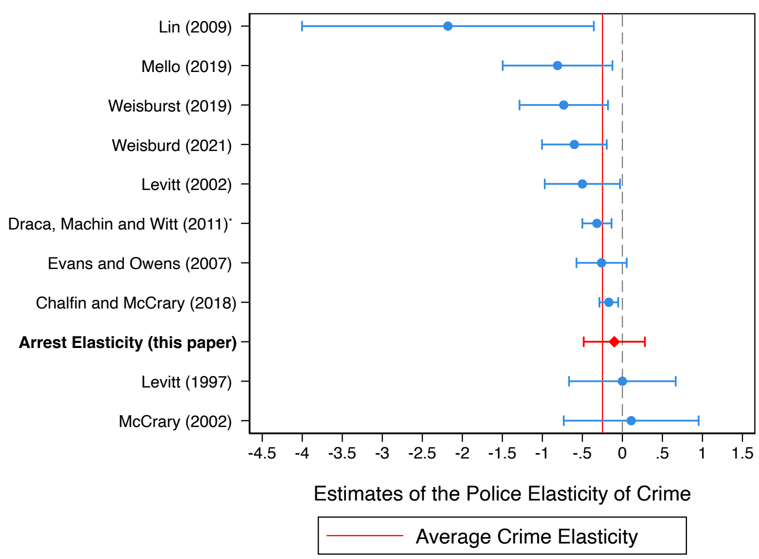
Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Months -6 and 6 include all months before month -6 and all months after month 6, respectively. Standard errors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

Figure 8: Arrest-to-Crime Elasticity (this paper)
vs. Police Manpower-to-Crime Elasticities

A. Violent Crimes



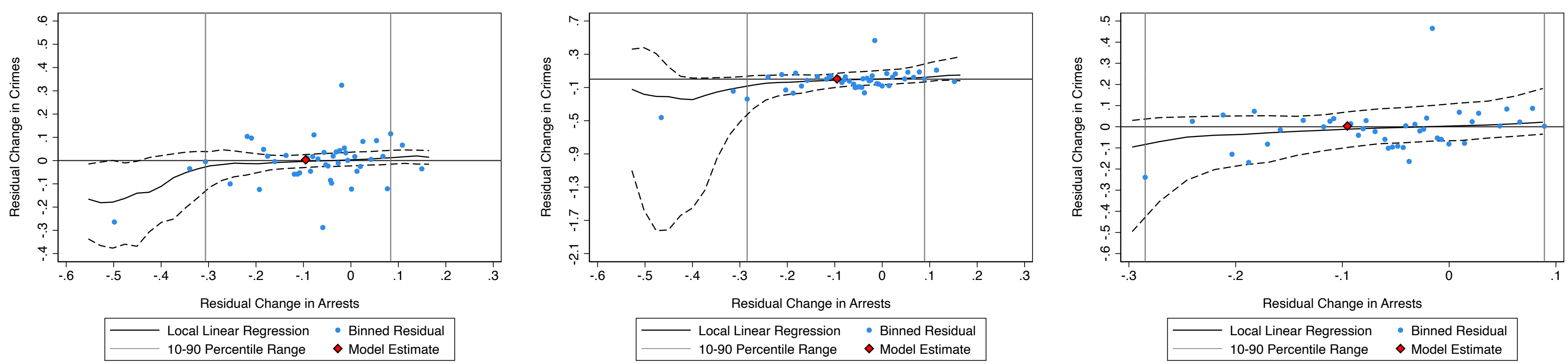
B. Property Crimes



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 9: Arrest to Crime Curve

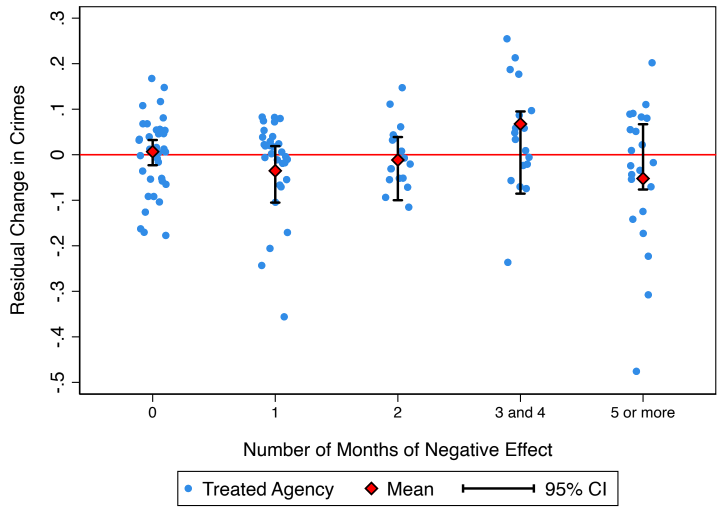
A. Month Effect ($t = 0$) B. Year Effect ($t = 0, ..., 11$) C. Year Effect Zoomed-In ($t = 0, ..., 11$)



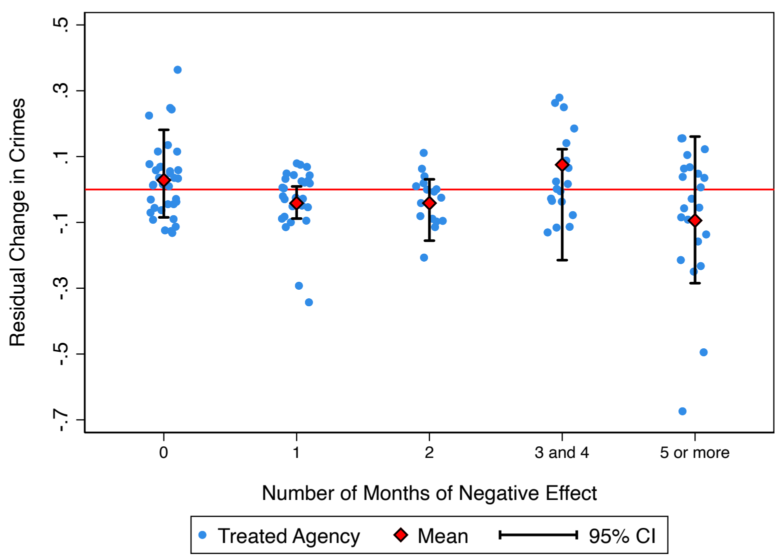
Notes: The residual changes in arrest and crime are estimated conditional on covariates, a department-specific linear time trend, department-by-calendar month and year-by-month fixed effects and differenced relative to the month prior to a line-of-duty death. The x-axis on all plots shows the residual 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 10: 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.

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	75.2	(349.7)	29564	582.8	(1397.1)	1544
Number of Officers Killed by Felony	0.005	(0.085)	29564	0.096	(0.332)	1544
Number of Officers Assaulted	10.8	(48.1)	29564	74.9	(176.6)	1544
% Black	7.7	(12.0)	29564	15.0	(17.8)	1544
% Hispanic	16.8	(20.8)	29564	22.6	(21.2)	1544
% White	68.0	(24.7)	29564	54.2	(24.6)	1544
% Male	48.8	(3.4)	29564	48.9	(1.8)	1544
% Female-Headed Household	31.3	(8.2)	29564	33.8	(7.1)	1544
% Age <14	20.2	(4.7)	29564	20.8	(4.4)	1544
% Age 15-24	14.3	(6.8)	29564	16.6	(6.9)	1544
% Age 25-44	27.2	(5.2)	29564	28.4	(3.9)	1544
% Age >45	38.3	(8.6)	29564	34.2	(7.8)	1544
% < High School	15.9	(11.0)	29564	17.7	(9.4)	1544
% High School Graduate	28.3	(9.5)	29564	25.7	(7.1)	1544
% Some College	28.3	(7.3)	29564	29.4	(5.7)	1544
% College Graduate or More	27.6	(16.1)	29564	27.2	(13.3)	1544
Unemployment Rate	4.8	(3.1)	29564	5.6	(2.3)	1544
Poverty Rate	12.7	(8.7)	29564	15.7	(7.5)	1544
Median Household Income	45658.5	(20918.3)	29564	40249.9	(15112.0)	1544
Population	41205.4	(133018.3)	29564	243160.3	(504777.6)	1544
Number of Agencies	1578					
Number of Treated Agencies	82					

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	0.391***	(0.058)	0.033	(0.039)	0.015	(0.013)	0.22	2.35	354504
adj. for Officer Death	0.052	(0.047)	0.031	(0.039)	0.015	(0.012)	0.22	2.34	354495
Arrests	-0.095***	(0.026)	-0.044*	(0.023)	-0.001	(0.023)	151.9	964.5	354507
Violent Crimes	-0.036	(0.027)	0.039	(0.029)	-0.034*	(0.018)	18.3	165.8	354507
Property Crimes	0.010	(0.018)	0.012	(0.016)	0.002	(0.014)	121.6	857.7	354507
(2) Restrict to Treated Cities									
Murder Offenses	0.393***	(0.058)	0.031	(0.039)	0.013	(0.013)	2.35	2.35	18510
Arrests	-0.097***	(0.026)	-0.044**	(0.022)	-0.005	(0.021)	964.5	964.5	18510
Violent Crimes	-0.037	(0.028)	0.035	(0.030)	-0.036*	(0.018)	165.8	165.8	18510
Property Crimes	0.010	(0.020)	0.013	(0.016)	0.005	(0.014)	857.7	857.7	18510
(3) Separate Panel for Each Event									
Murder Offenses	0.379***	(0.057)	0.034	(0.038)	0.014	(0.011)	0.64	6.51	366498
Arrests	-0.100***	(0.024)	-0.050**	(0.020)	-0.008	(0.018)	255.4	1888.9	366501
Violent Crimes	-0.024	(0.025)	0.048*	(0.028)	-0.022	(0.016)	43.9	415.4	366501
Property Crimes	0.012	(0.016)	0.015	(0.013)	0.005	(0.010)	235.5	1935.9	366501
(4) Counting Multiple Officer Deaths Additively									
Murder Offenses	0.359***	(0.056)	0.035	(0.032)	0.019*	(0.011)	0.22	2.35	354504
Arrests	-0.085***	(0.023)	-0.043**	(0.021)	-0.004	(0.021)	151.9	964.5	354507
Violent Crimes	-0.025	(0.022)	0.038	(0.025)	-0.026	(0.016)	18.3	165.8	354507
Property Crimes	0.009	(0.017)	0.011	(0.014)	0.001	(0.012)	121.6	857.7	354507

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
(5) Drop Agency × Month									
Murder Offenses	0.393***	(0.058)	0.033	(0.037)	0.016	(0.013)	0.22	2.35	354504
Arrests	-0.092***	(0.026)	-0.040*	(0.024)	-0.002	(0.023)	151.9	964.5	354507
Violent Crimes	-0.036	(0.025)	0.037	(0.028)	-0.033*	(0.018)	18.3	165.8	354507
Property Crimes	0.011	(0.019)	0.013	(0.018)	0.002	(0.014)	121.6	857.7	354507
(6) Add State-by-Year FE									
Murder Offenses	0.389***	(0.058)	0.032	(0.039)	0.013	(0.013)	0.22	2.35	354504
Arrests	-0.102***	(0.026)	-0.049**	(0.023)	-0.005	(0.022)	151.9	964.5	354507
Violent Crimes	-0.036	(0.027)	0.039	(0.030)	-0.028	(0.018)	18.3	165.8	354507
Property Crimes	0.004	(0.018)	0.007	(0.015)	-0.003	(0.013)	121.6	857.7	354507
(7) Remove DUI Arrests									
Murder Offenses	0.391***	(0.058)	0.033	(0.039)	0.015	(0.013)	0.22	2.35	354504
Arrests	-0.090***	(0.026)	-0.037	(0.024)	0.002	(0.023)	139.2	895.4	354507
Violent Crimes	-0.036	(0.027)	0.039	(0.029)	-0.034*	(0.018)	18.3	165.8	354507
Property Crimes	0.010	(0.018)	0.012	(0.016)	0.002	(0.014)	121.6	857.7	354507
(8) Levels Model									
Murder Offenses	1.337***	(0.502)	0.053	(0.271)	-0.153	(0.130)	0.22	2.35	354504
Arrests	-69.192*	(36.695)	-21.615	(51.944)	-3.457	(47.503)	151.9	964.5	354507
Violent Crimes	-4.655	(8.450)	2.090	(9.000)	-5.475	(9.548)	18.3	165.8	354507
Property Crimes	-8.650	(21.749)	12.234	(20.065)	-24.597	(26.627)	121.6	857.7	354507

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
(9) Per Capita Model (Per 100K Residents)									
Murder Offenses	1.944***	(0.407)	0.133	(0.113)	0.013	(0.042)	0.29	0.65	354504
Arrests	-41.918***	(10.609)	-22.632**	(9.960)	-6.843	(9.320)	456.1	457.1	354507
Violent Crimes	-1.752	(1.446)	0.863	(1.484)	-1.676	(1.090)	32.2	51.9	354507
Property Crimes	-1.383	(6.385)	3.669	(5.623)	-0.121	(5.216)	293.2	344.9	354507
(10) Inverse Hyperbolic Sine Model									
Murder Offenses	0.498***	(0.074)	0.039	(0.049)	0.020	(0.016)	0.11	0.72	354504
Arrests	-0.097***	(0.026)	-0.045*	(0.024)	-0.002	(0.023)	4.8	6.4	354507
Violent Crimes	-0.042	(0.031)	0.045	(0.033)	-0.041**	(0.019)	2.0	4.1	354507
Property Crimes	0.010	(0.019)	0.011	(0.017)	0.002	(0.014)	4.4	6.2	354507
(11) Sun & Abraham (2020) IW Estimator									
Murder Offenses	0.380***	(0.044)	0.032	(0.034)	0.011	(0.007)	0.64	6.51	366498
Arrests	-0.090***	(0.024)	-0.040*	(0.021)	0.003	(0.009)	255.4	1888.9	366501
Violent Crimes	-0.029	(0.024)	0.043	(0.027)	-0.028***	(0.007)	43.9	415.4	366501
Property Crimes	0.012	(0.017)	0.014	(0.015)	0.005	(0.006)	235.5	1935.9	366501
(12) Drop Time Trend									
Murder Offenses	0.376***	(0.059)	0.017	(0.039)	-0.002	(0.011)	0.22	2.35	354504
Arrests	-0.138***	(0.028)	-0.089***	(0.024)	-0.049**	(0.023)	151.9	964.5	354507
Violent Crimes	-0.044	(0.029)	0.030	(0.031)	-0.041**	(0.020)	18.3	165.8	354507
Property Crimes	-0.007	(0.022)	-0.007	(0.019)	-0.017	(0.017)	121.6	857.7	354507
(13) Nearest Neighbor Matching									
Murder Offenses	0.379***	(0.059)	0.003	(0.040)	-0.011	(0.015)	1.0	6.3	59435
Arrests	-0.118***	(0.024)	-0.055***	(0.020)	-0.018	(0.017)	469.8	1881.0	59436
Violent Crimes	-0.052	(0.032)	0.047	(0.034)	-0.028	(0.017)	74.3	396.4	59436
Property Crimes	-0.001	(0.020)	-0.004	(0.018)	-0.011	(0.014)	414.6	1831.2	59436

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) drops the agency-by-month fixed effect. Model (6) adds state by year fixed effects. Model (7) removes the DUI arrests counts from the total arrests. Models (8), (9) and (10) consider alternate functional forms, using a levels, a per capita and an inverse hyperbolic sine, respectively. Model (11) uses Sun and Abraham (2020)'s proposed estimator. Model (12) drops the department-specific linear time trends and Model (13) uses a nearest neighbor matching approach. 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	0.391***	(0.058)	0.033	(0.039)	0.015	(0.013)	0.22	2.35	354504
Murder Arrests	0.111**	(0.044)	0.071	(0.043)	-0.000	(0.023)	0.17	1.57	354507
B. Index Arrests									
Rape	-0.014	(0.029)	-0.042	(0.033)	-0.001	(0.018)	0.28	2.08	354507
Robbery	-0.094***	(0.035)	-0.059	(0.047)	0.003	(0.023)	1.7	15.6	354507
Aggravated Assault	-0.088**	(0.035)	-0.036	(0.028)	-0.056**	(0.025)	6.4	44.3	354506
Burglary	0.004	(0.040)	0.022	(0.045)	0.014	(0.028)	3.7	20.7	354507
Theft	-0.072*	(0.042)	-0.034	(0.042)	-0.022	(0.034)	14.9	82.6	354507
Motor Vehicle Theft	-0.098*	(0.055)	-0.118*	(0.062)	-0.044	(0.062)	1.4	11.8	354507
C. Index Crime									
Rape	-0.040	(0.035)	0.042	(0.038)	-0.006	(0.021)	1.3	10.1	353656
Robbery	-0.004	(0.030)	0.009	(0.032)	-0.017	(0.017)	5.9	61.0	354382
Aggravated Assault	-0.044	(0.034)	0.036	(0.030)	-0.034	(0.021)	11.1	94.8	354355
Burglary	0.041	(0.029)	0.023	(0.031)	0.010	(0.020)	24.0	175.3	354478
Theft	-0.026	(0.029)	-0.013	(0.026)	-0.022	(0.022)	81.9	541.9	354506
Motor Vehicle Theft	0.026	(0.033)	-0.009	(0.031)	0.011	(0.023)	15.7	140.5	354389

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	0.013	(0.024)	0.014	(0.024)	-0.005	(0.010)	0.01	0.10	354507
Arson	0.023	(0.041)	-0.058	(0.041)	-0.012	(0.022)	0.15	0.85	354507
Other Assault	-0.028	(0.034)	-0.058*	(0.035)	-0.002	(0.030)	13.6	89.2	354507
Weapons	-0.083**	(0.042)	-0.007	(0.038)	-0.018	(0.023)	2.3	17.1	354507
Prostitution	-0.079*	(0.042)	-0.104*	(0.057)	-0.038	(0.041)	1.2	15.5	354507
Other Sex Offense	-0.052	(0.034)	-0.042	(0.040)	-0.010	(0.028)	0.92	6.68	354507
Family Offense	-0.022	(0.050)	0.057	(0.043)	0.032	(0.040)	0.58	4.14	354506
DUI	-0.164***	(0.048)	-0.108***	(0.042)	-0.031	(0.034)	12.7	69.1	354507
Drug Sale	-0.154*	(0.088)	-0.101	(0.091)	-0.108	(0.110)	3.8	35.4	354506
Forgery	-0.006	(0.039)	-0.037	(0.043)	-0.002	(0.028)	1.04	5.38	354507
Fraud	-0.011	(0.046)	-0.007	(0.046)	0.053	(0.033)	1.71	8.29	354507
Embezzlement	-0.028	(0.046)	-0.017	(0.033)	0.019	(0.025)	0.23	1.07	354507
Stolen Property	0.008	(0.048)	0.056	(0.047)	0.056	(0.042)	1.49	7.49	354505
Runaway	0.034	(0.041)	0.015	(0.043)	0.011	(0.045)	1.16	7.87	354507
B. Quality of Life Arrests									
Disorderly Conduct	-0.013	(0.049)	-0.023	(0.050)	0.011	(0.043)	5.3	29.4	354506
Curfew/Loitering	-0.069	(0.067)	0.018	(0.059)	-0.019	(0.065)	2.3	30.7	354507
Vandalism	-0.069	(0.042)	-0.073*	(0.043)	-0.040	(0.035)	2.9	17.1	354507
Gambling	-0.049	(0.031)	-0.004	(0.032)	-0.016	(0.021)	0.06	0.65	354506
Vagrancy	0.007	(0.077)	-0.006	(0.075)	0.042	(0.075)	0.55	6.02	354507
Drunkenness	-0.056	(0.068)	0.015	(0.064)	-0.010	(0.060)	8.9	44.3	354507
Liquor	-0.058	(0.071)	-0.053	(0.068)	-0.001	(0.059)	5.0	27.8	354507
Drug Possession	-0.107**	(0.054)	-0.109*	(0.060)	-0.044	(0.063)	17.5	102.8	354507
Uncategorized Arrests	-0.100*	(0.059)	-0.003	(0.043)	0.056	(0.044)	40.1	260.5	354507

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	(0.045)	-0.016	(0.031)	-0.025*	(0.013)	0.26	1.60	283906
Accidents involving Alcohol	0.012	(0.043)	-0.004	(0.032)	-0.018	(0.022)	0.09	0.57	256978
Fatal Use-of-Force									
Supplementary Homicide Report	0.024	(0.025)	-0.024	(0.018)	0.003	(0.006)	0.02	0.16	359733
Fatal Encounters	0.044	(0.037)	-0.025	(0.039)	0.030**	(0.014)	0.03	0.26	172760
Accidental Officer Death									
Murder Offenses	0.006	(0.040)	0.061	(0.044)	0.005	(0.015)	0.23	2.45	329669
Arrests	-0.019	(0.026)	0.008	(0.031)	0.011	(0.030)	155.2	967.9	329672
Violent Crimes	0.031	(0.045)	0.004	(0.044)	0.019	(0.023)	19.0	183.3	329672
Property Crimes	0.009	(0.027)	-0.049	(0.037)	-0.005	(0.024)	125.1	986.9	329672

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. Fatal Use-of-Force panel includes two measures of civilians killed by police. First 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, which we restrict to the sample period of 2010-2018 for data quality reasons. 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. “Assaults on Officers” measures officer line-of-duty assaults from the FBI UCR LEOKA data. “Accidental Officer Death” panel shows the four main outcomes using the accidental officer death as a treatment instead of felonious death. * p<0.1,** p<0.05, *** p<0.01.

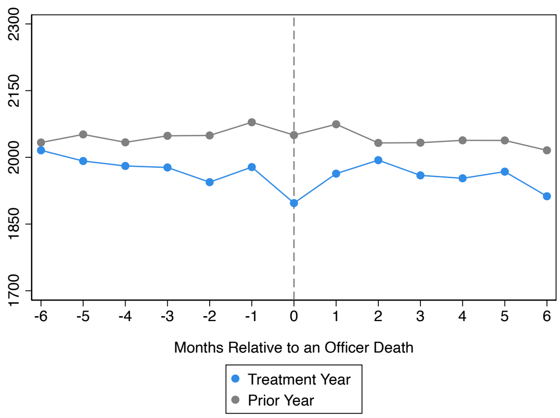
Table A6: Heterogeneity, Arrestee Demographics

	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
Policing Activity										
Total Arrests	-0.095***	(0.026)	-0.044*	(0.023)	-0.001	(0.023)	151.9	964.5	354507	
Black	-0.069**	(0.029)	-0.006	(0.030)	0.015	(0.022)	40.0	353.1	354507	0.499
White	-0.107***	(0.029)	-0.062**	(0.025)	-0.005	(0.024)	108.2	590.7	354507	0.760
Male	-0.093***	(0.026)	-0.042*	(0.023)	-0.003	(0.022)	114.1	736.6	354507	0.951
Female	-0.097***	(0.029)	-0.049*	(0.028)	0.004	(0.025)	37.8	227.9	354507	0.959
Adult	-0.096***	(0.028)	-0.043*	(0.025)	0.000	(0.024)	130.5	832.7	354507	0.980
Juvenile	-0.097**	(0.042)	-0.077*	(0.045)	-0.019	(0.036)	21.3	131.9	354507	0.980

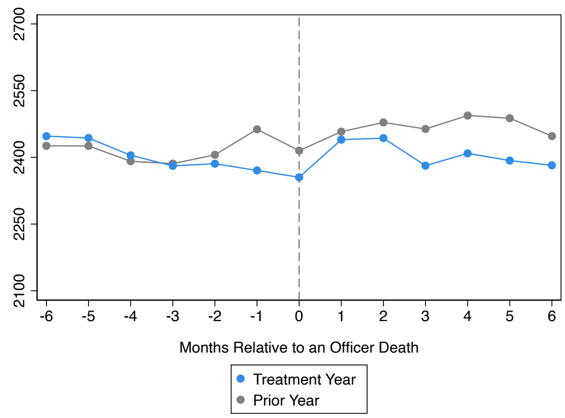
Notes: Regressions in include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Regressions also include a dummy variable for 12 or more months after the occurrence of an officer death. Outcomes 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. * p<0.1,** p<0.05, *** p<0.01.

Figure A1: Unadjusted Data Around Events, Level Outcomes

A. Total Arrests

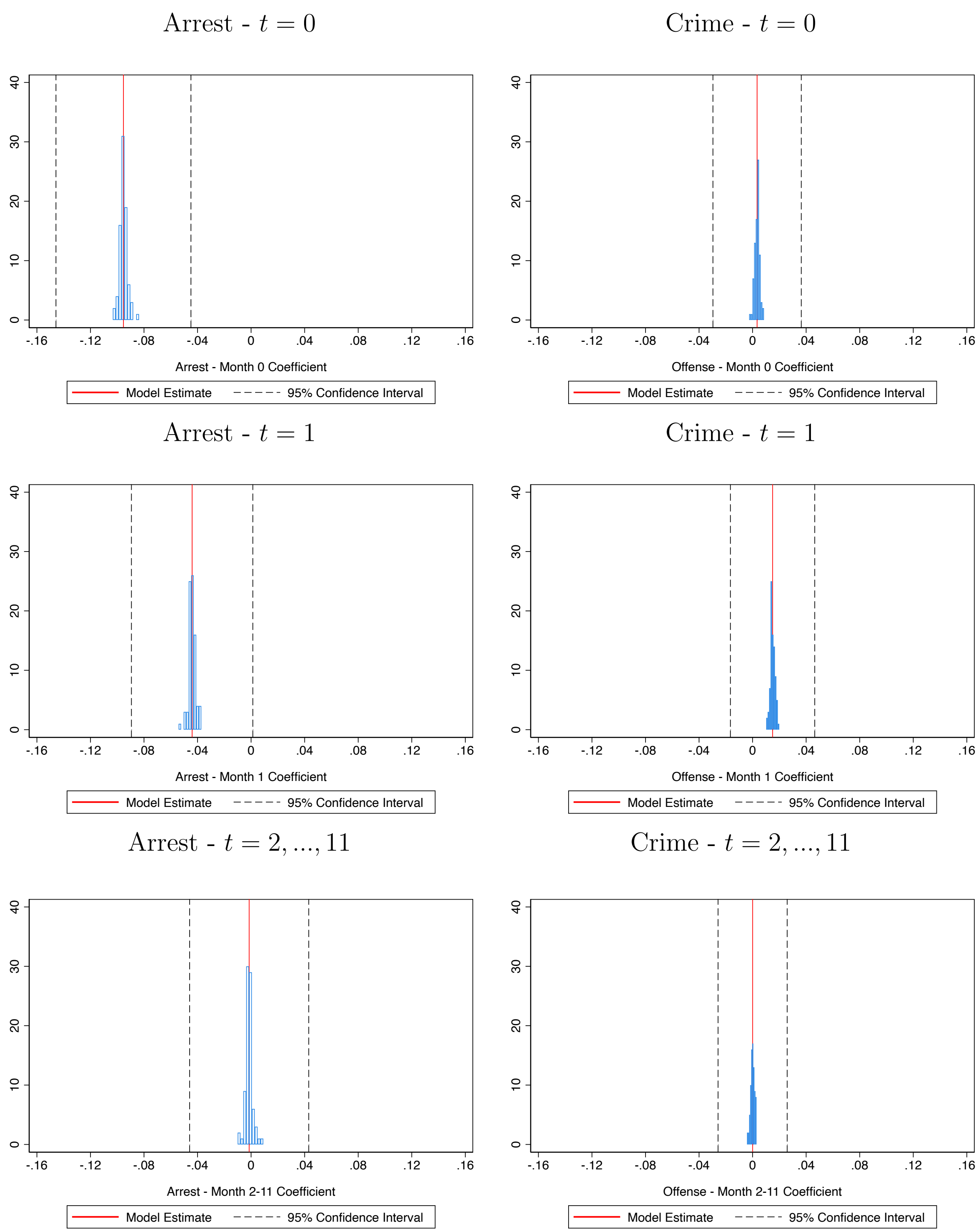


B. Index Crimes



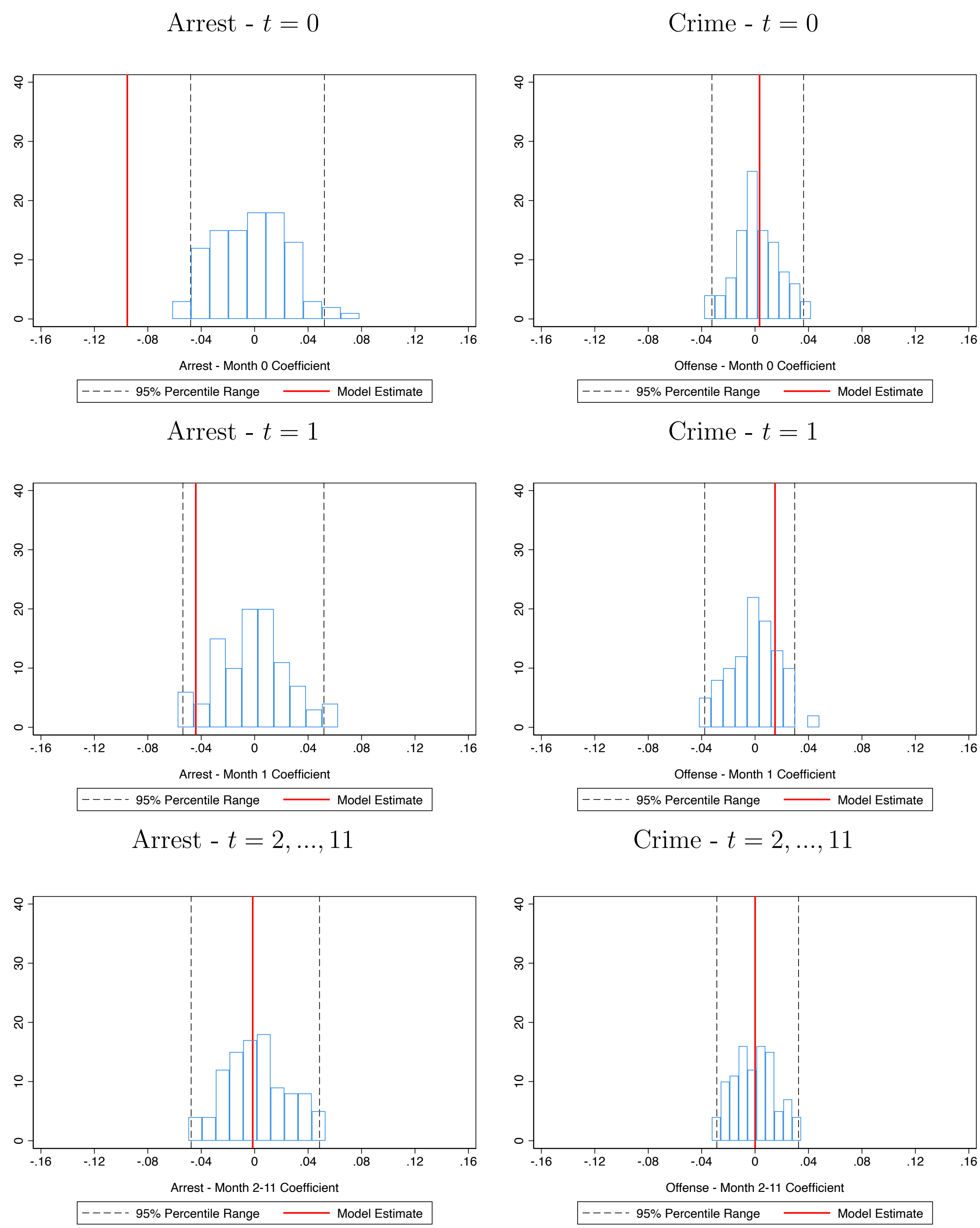
Notes: This figure plots the unadjusted data around the officer death events. There are 125 officer death events in 76 agencies after excluding events that do not have enough periods before and after the event. Index crimes include rape, robbery, aggravated assault, burglary, theft, and motor vehicle theft.

Figure A2: Distribution of Coefficients Dropping Single Treated Agency



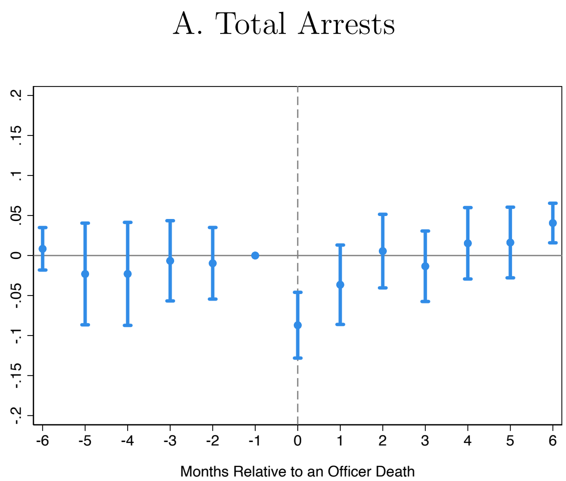
Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Regressions also include a dummy variable for 12 or more months after the occurrence of an officer death. Standard errors are clustered at the department level. We re-estimate the model dropping one treatment city at a time. There are 82 treated cities.

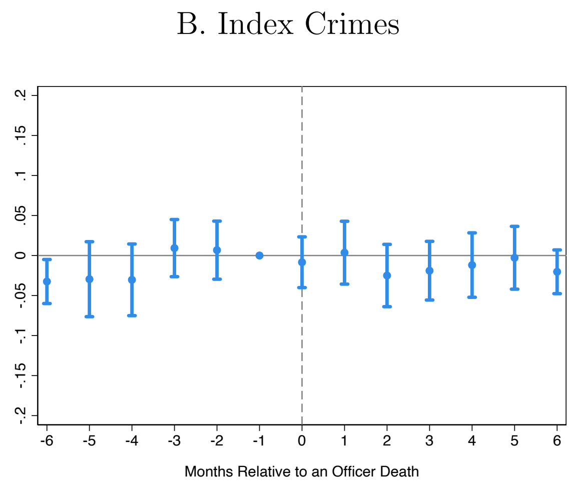
Figure A3: Placebo Treatment Timing



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.

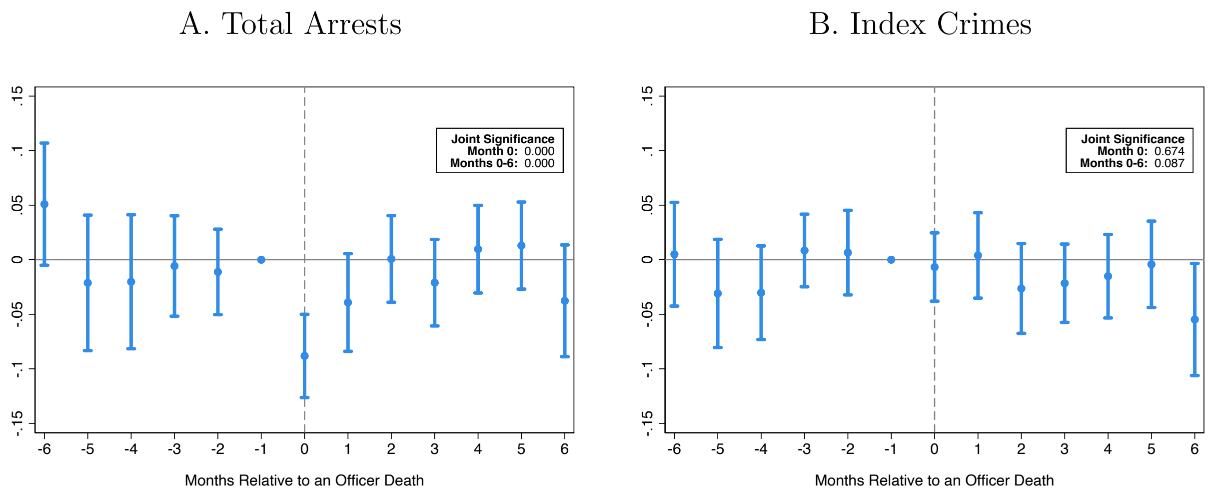
Figure A4: Event-Study: Sun and Abraham (2020)





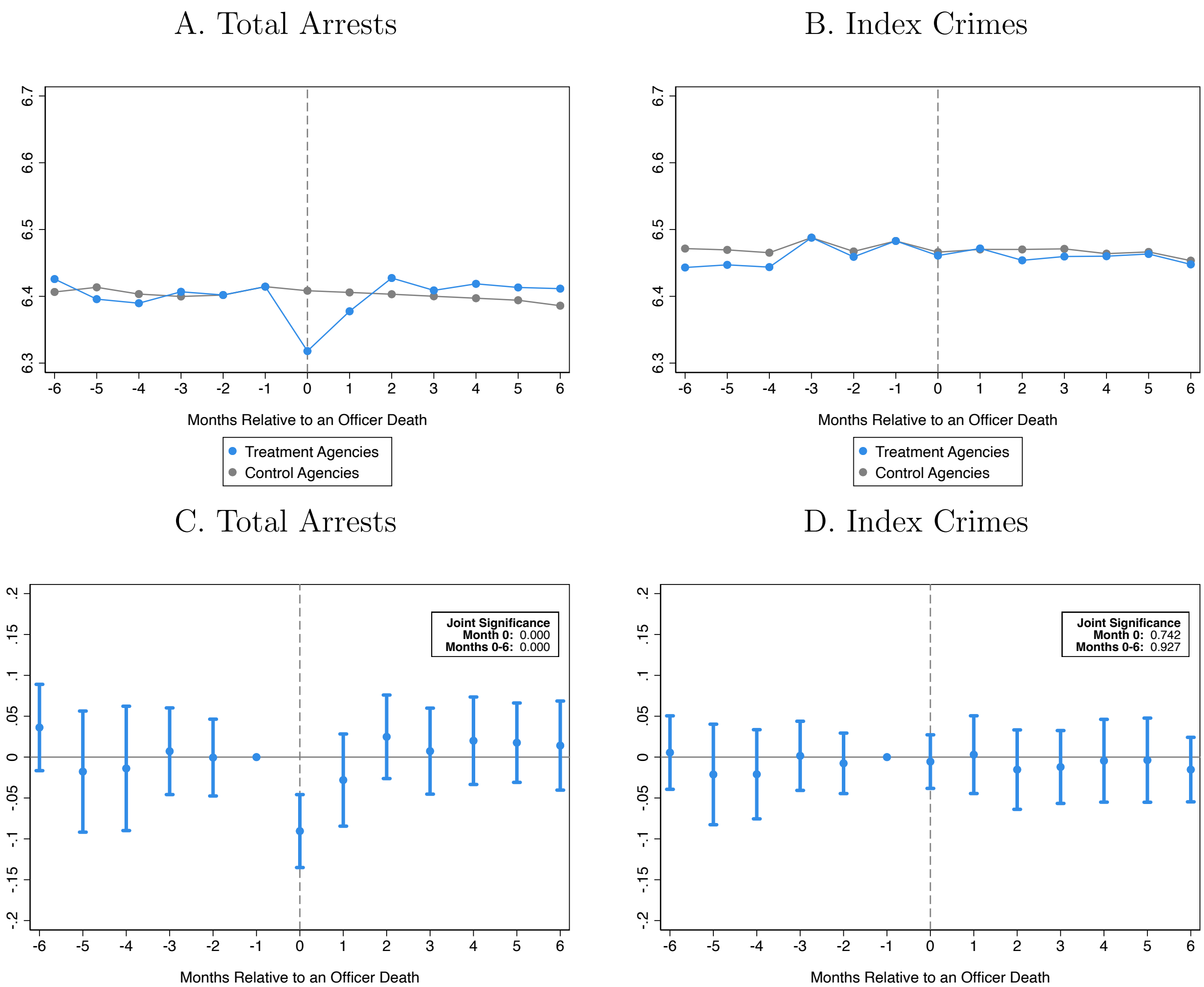
Notes: This figure plots Sun and Abraham (2020)'s proposed "interaction-weighted" coefficient estimator. This estimator combines cohort-specific treatment effects, based on treatment timing, using strictly positive weights. To estimate this model, we include a separate panel for each treatment event, 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 A5: Event-Study: Omitting Agency-Specific Linear Time Trends



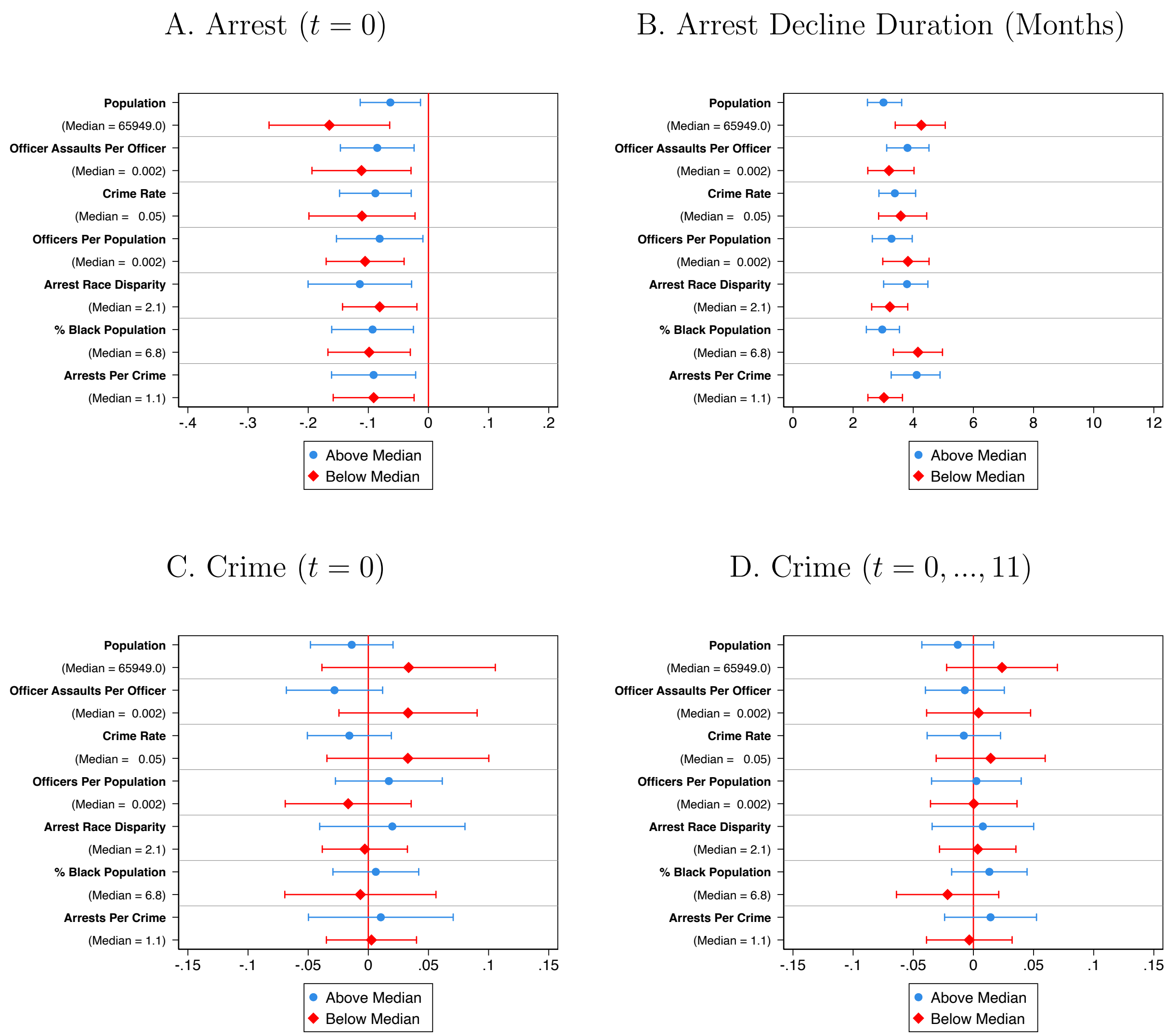
Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects. Months -6 and 6 include all months before month -6 and all months after month 6, respectively. Standard errors are clustered at the department level.

Figure A6: Event-Study: Nearest-Neighbor Matching



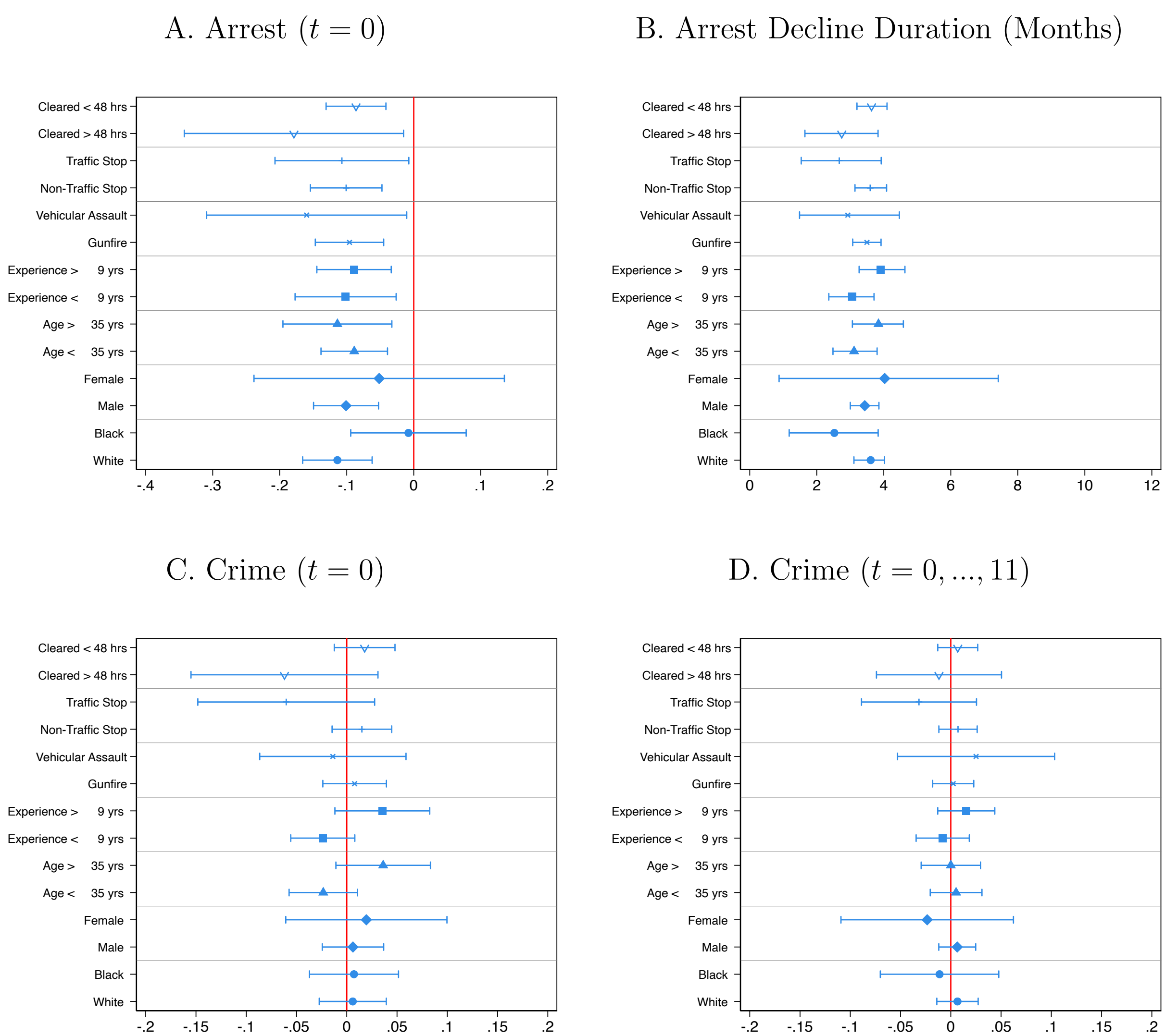
Notes: This figure uses the nearest-neighbor matching approach to match treatment event to 10 control agencies using information on demographic characteristics in the treatment year and lagged monthly crime and arrest levels in the year prior to treatment. There are 114 matched pairs of 75 treatment agencies and 625 control agencies. Panels A and B plot the unadjusted data around the officer death events. Panels C and D plot the event study estimates. 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: 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 bars in this figure represent the 95% confidence interval for each characteristic calculated using a bootstrapping approach with 200 iterations.

Figure A8: 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 bars in this figure represent the 95% confidence interval for each characteristic calculated using a bootstrapping approach with 200 iterations.

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. A benchmark would not be necessary if Google Trends data contained absolute search volume, but unfortunately this data series only includes relative measures of search volume that are a function of the topics and terms used to pull the data. The use of a benchmark is therefore critical to 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 verify each officer fatality event in the sample using the web resource *Officer Down Memorial Page* (ODMP) and exclude death events from LEOKA that are not able to be verified in ODMP. This website is also used to gather characteristics of the fatality event and officer who was killed, which is used in the heterogeneity analysis. 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) and Arrest Data (UCR Arrest) The Uniform Crime Report Offenses Known and Clearances By Arrest (UCR Crime) 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. The Uniform Crime Report Arrests by Age, Sex, and Race (UCR Arrest) data set contains the number of arrests for each crime type by age, sex and race at the month level. 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, 2020b). This dataset covers the period 2000-2018. We include all departments that consistently and continuously report monthly data on *both* crime and arrests for at least 9 years in this period, up until and including the last year of the data, 2018.

Use-of-Force Data (UCR Supplementary Homicide Reports) The Uniform Crime Report Supplementary Homicide Reports (UCR Supplementary Homicide Reports) data set contains the number of homicides. We utilize the version cleaned and formatted by Jacob Kaplan available from ICPSR (Kaplan, 2020c) 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. We exclude treatment events in which a suspect was killed during the officer fatality event in order to measure the police behavioral response to an officer fatality, rather than features of the event itself.

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. As in the UCR Supplementary Homicide Report, we exclude treatment events in which a suspect was killed during the officer fatality event. Fatal Encounters was established in 2013 and backfills earlier record years which causes quality to decrease in earlier record years. To address this issue, we restrict attention to the period 2010-2018.

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. We then use the intersection between this data set and our analysis sample.

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 2000 to 2018 for any accidents and 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. 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. 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 these covariates for the years 2001 to 2009.

A3.2 Sample Restrictions

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 the 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 both crimes and arrests monthly each year in the period 2000-2018 (for example, this procedure drops agencies that report annually or biannually). To increase sample size, we include any agency that reports at least 9 years of consecutive data through 2018, or agencies that begin reporting between 2000-2010.

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.