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THE IMPACT OF FEAR ON POLICE BEHAVIOR AND PUBLIC SAFETY

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ABSTRACT

We examine how changes in the salience of workplace risk affect police behavior and public safety. Specifically, we investigate cases of police officer deaths while on duty. Officers respond to a peer death by decreasing arrest activity for one to two months, consistent with heightened fear. Reductions are largest for low-level arrests and are more pronounced in smaller cities. Crime does not increase on average during this period, nor do we observe crime spikes in cities with larger or longer arrest declines. While shocks in fatality risk generate substantial enforcement responses, officer fear is unlikely to harm public safety.

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The job of a police officer is dangerous, with a fatality rate that ranks among the top twenty across professions in the United States.[1] It is also high stakes – law enforcement actions have the capacity to affect crime outcomes but may also impose large economic, social, and human consequences for sanctioned individuals.[2] An open question is whether any emotional response to perceived on-the-job risk could reduce the social optimality of officer decisions and whether such responses could compromise public safety. This issue is particularly important in the U.S. context, where officers arrest over 10 million individuals each year, most of which are for lower-level misdemeanor offenses, and rates of incarceration and crime are high relative to other countries.[3]

Research at the intersection of economics and neuroscience suggests that fear can affect both the interpretation of risks and response to risks (See Camerer et al. 2005 for a review), and economists have found that heightened emotional states can have real-world impacts, often within short time horizons.[4] Moreover, a broad literature has identified non-pecuniary drivers of workplace behavior, including social connectedness (Bandiera et al., 2010), interpersonal comparisons (Ager et al., 2022), and grief from personal loss (Graddy and Lieberman, 2018). Because of officers' significant enforcement discretion, changes in their emotional mindset have the potential to affect decisions such as when to make an arrest, which could have material consequences for public safety.

This paper asks two questions: First, how do changes in the salience of fatality risk impact officer behavior? Second, do these changes have downstream consequences for public safety? We examine cases of police officer deaths in the line-of-duty. We show that, after the death of an officer, fellow officers markedly reduce their arrest activity. By making fewer arrests, officers remove themselves from potentially risky interactions with suspected offenders, consistent with risk mitigation due to heightened fear. Despite this decline in arrests, crime

[1] 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/

[2] See Mello (2018); Bacher-Hicks and de la Campa (2020a,b); Weisburst (2022); Goncalves and Mello (2022) for examples.

[3] In 2019, annual arrests in the U.S. exceeded 10 million, with less than 20% of arrests corresponding to serious felony offenses (FBI UCR, `https://ucr.fbi.gov/crime-in-the-u.s/2019/crime-in-the-u.s.-2019/tables/table-29`). In 2021, the U.S. ranked 6th among all countries in the share of population incarcerated (World Prison Population Brief, `https://www.prisonstudies.org/highest-to-lowest/prison_population_rate`). Cross-country comparisons of crime show that the U.S. homicide rates rank 40th among 229 countries (World Bank, `https://data.worldbank.org/indicator/VC.IHR.PSRC.P5`).

[4] For applications in finance, see Lo et al. (2005); Cohn et al. (2015); Duxbury et al. (2020). For emotional responses to football game losses, see Card and Dahl (2011) and Eren and Mocan (2018). Further, exposure to violent crime and war have been found to alter risk preferences (Brown et al., 2019; Voors et al., 2012).

does not increase on average in the ensuing period, nor do we find crime increases in cities with larger or longer arrest declines. This lack of a crime effect may be attributable to the types of arrest reductions we observe, which are most concentrated among low-level offenses and are potentially less instrumental to improving public safety. These impacts suggest that, while shocks to fatality risk can lead to substantial enforcement responses, officer fear does not ultimately contribute to higher rates of crime.

A preoccupation with fatality risk is central to police culture, and officers often view their work in "life-or-death" terms. They are formally instructed on the potential perils of the work and on self-protection in the field, beginning with their police academy training. When an officer dies while on duty, their department typically honors them with a formal police funeral, including dress uniforms, dedicated music, a 21-gun salute, and a symbolic "end of watch call" to the fallen officer.[5] A majority of officers (84%) cite that they worry about their safety on the job, and officers feel that the public does not understand the risks and dangers inherent in their occupation, or the challenges of policing more broadly.[6]

Theoretically, it is unclear in what direction on-the-job fear will impact police behavior and public safety, and previous studies focused on individual cities have yielded conflicting findings. Heightened fear may contribute to more aggressive policing (Holz et al., 2020), or it may lead officers to disengage from situations that risk causing them harm (Sullivan and O'Keeffe, 2017; Sloan, 2019; Chalfin et al., 2021). The criminal environment could change if officers adjust their arrest activity and their altered enforcement is important for incapacitating or deterring crime. Conversely, crime may not change if any marginal changes in enforcement are not central to maintaining public safety.

We examine a sample of 1500 municipalities between 2000 and 2018, and our empirical strategy uses a difference-in-differences design that exploits the staggered occurrence of line-of-duty deaths across agencies. We first 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

[5]Ethnographic research also highlights that officer deaths become a part of a department's "organizational memory" long after the deaths occur (Marenin, 2016; Sierra-Arévalo, 2016, 2019).

[6]Pew Research (2017) "Behind the Badge": `https://assets.pewresearch.org/wp-content/uploads/sites/3/2017/01/06171402/Police-Report_FINAL_web.pdf`

is exogenous to the criminal environment.

To further characterize the behavioral arrest response, we ask how our estimated effects vary by city characteristics. We use a synthetic control approach to estimate event-specific treatment effects, where each city's arrest and crime rates are compared to a weighted average of outcomes for cities without an officer death over the same period. We find the biggest arrest declines in smaller cities and those with fewer crimes per capita, consistent with a peer death being a greater shock to officers operating in a less-active criminal environment.

We provide several pieces of evidence that the observed arrest reductions are due to behavioral changes in enforcement decisions rather than changes to police manpower. First, using conservative assumptions, the magnitude of the effect we observe is too large to be driven by officers taking time off following the death of their peer. Second, arrest declines occur even in officer fatality cases where the offender is apprehended quickly, suggesting that the effect is not due to peer officers being diverted to investigate the incident. Third, no arrest decline occurs after accidental deaths of peers (e.g. car accidents), which should have a similar incapacitation effect on peers. This result also rules out productivity spillovers from the deceased officer as an important channel for the arrest decline. Finally, using police employment data from Florida, we find that a peer death does not induce changes in officer quits or hires. The lack of employment-level responses from officers is consistent with the literature on how workplace risk is priced into wages, which yields the smallest wage-risk gradients among occupations with higher baseline levels of fatality risk (Viscusi and Aldy, 2003).

We next turn to how officer deaths and the resulting enforcement response impact public safety. In contrast to the observed arrest decline, 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, the most serious violent and property crimes defined by the Federal Bureau of Investigation (FBI).[7] Our results are robust to a variety of specification choices, and they collectively suggest that public safety is not significantly worsened by heightened officer fear.

Despite the significant variation across cities in magnitude of arrest decline, we find no evidence of crime increases in cities whose characteristics predict a bigger arrest effect, again leveraging our synthetic control estimates. To further probe the lack of crime impact, we

[7]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 3).

directly stratify cities by their estimated magnitude and length of arrest decline. 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.

A key challenge with studying how changes in policing impact crime rates is that measured crime is partly a function of police reporting decisions. 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 (Levitt, 1998; Mosher et al., 2010). To address this concern, we hand-collected a large data set of 911 calls from 56 police departments in our baseline analysis sample. 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.

Our study relates to the labor economics literature on workplace safety, to which we make two important contributions. First, while many studies have examined variation across occupations and firms in measures of fatality risk to identify a wage-risk gradient (see Viscusi and Aldy 2003 for a review), we innovate on this approach by using variation within a given workplace in the *salience* of workplace risk. Second, the vast majority of the literature takes injury or fatality risk as an exogenous feature of a job rather than a characteristic that is partly determined by worker behavior. Two notable exceptions are Guardado and Ziebarth (2019), who use employee weight (and how it varies with compensation) as an indirect measure of investment in safety, and Kohlhepp (2022), who studies the overtime decisions of traffic officers and its impact on injury risk. We contribute to this understudied area by documenting a direct behavioral response to workplace risk and how it affects the relevant total productivity measure of public safety.

We also contribute to a growing body of work that studies the impact of unexpected deaths of individuals, which have been used to identify productivity spillovers (Azoulay et al., 2010; Jaravel et al., 2018), labor market frictions (Jäger and Heining, 2022), and the impact of leaders (Jones and Olken, 2005; Bennedsen et al., 2020). In contrast to the previous literature, the death events we study occur at work, leading to a shift in fellow employees' perception of their workplace safety. In addition, we provide evidence that our effects are driven by an emotional response rather than productivity spillovers or direct incapacitation from the deceased individual. In that spirit, our study is perhaps closest to work by Graddy and Lieberman (2018) on artist productivity after the death of a family member.

In the law enforcement setting, a number of other papers study institutional changes that affect officer enforcement behavior, and this work finds mixed effects of these changes on crime (Mas, 2006; McCrary, 2007; Owens et al., 2018). We likewise complement a growing literature on the impact of heightened public scrutiny on police behavior (Prendergast, 2001, 2021; Shi, 2009; Heaton, 2010; Rivera and Ba, 2019; Ba, 2020), which finds that following a scandal, officers often reduce discretionary enforcement and crime increases (Cheng and Long, 2018; Premkumar, 2020; Devi and Fryer Jr, 2020), but that victim and community trust in police may also decline (Ang et al., 2021).

Our primary focus is on identifying the impacts of an officer death. However, if police officers are the only individuals who directly respond to these events and their response is solely manifested by a reduction in arrests, then our results can be interpreted to indicate the impact of a marginal change in arrests on crime. Indeed, we show evidence from Google search trends that officer deaths attract limited attention in the community. Viewed through this lens, our work contributes to an important open question in the economics of crime of how changes in police *enforcement* impact crime (Chandrasekher, 2016; Bacher-Hicks and de la Campa, 2020a; Chalfin et al., 2021). 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 notably less negative than the estimates of the crime-to-employment (or police presence) elasticity found in previous studies (e.g. Evans and Owens, 2007; Chalfin and McCrary, 2018; Weisburst, 2019; Mello, 2019; Chalfin et al., 2020), which study similarly-sized percent changes in police employment. While suggestive, these estimates point to the potential for reforms which reduce the scope of arrest activity without the cost of elevated crime rates.

1 Data

1.1 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.[8]

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.[9] 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 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.[10]

[8]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.

[9]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.

[10]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 requested

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 2).

1.2 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.[11] 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 2). 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.

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

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.

[11] 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.

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.[12] 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 2. 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 logged outcomes in the treated year to the year prior for treated agencies. While these plots are not adjusted for any covariates or fixed effects, they accord with the overall pattern of findings in the study.[13] 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.

2 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

[12] 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.

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

from the date of the incident:

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

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 3.1. The dummy variables D^0_{it}, D^1_{it}, D^{2-11}_{it}, D^{12+}_{it} 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^k_{it}, 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 Table A1). These controls include city-by-year resident age, sex, and race composition, as well as total population, median household income, poverty rate, and unemployment rate. City-by-month fixed effects, $\pi_{i,m(t)}$, remove all within-city seasonality in the outcome that is constant across years. We also include fixed-effects that vary at the year-by-month level, θ_t, which account for all sample-wide variation in the outcome over time.

Lastly, we include a city or department-specific linear time trend $\gamma_i t$. During our sample period, both crime and arrests are decreasing nationally, and this decline is occurring at different rates for different police agencies. Previous research has documented that locations with greater baseline levels of crime experienced more substantial declines during this time period (Friedson and Sharkey, 2015; Ellen and O'Regan, 2009), suggesting the need to account for cross-city differences in the time path of crime and arrests. We include this set of controls so as to isolate deviations from these downward trends due to line-of-duty officer deaths. Importantly, this set of controls leads to more *conservative* estimates of the size of arrest declines in the short and long-term, because without them, earlier periods of arrests prior to a officer death (contained in D^0_{it}) may be inflated upward. Indeed, we find qualitatively consistent results albeit with larger arrest declines when these controls are omitted (Table A2, specification (12) and Figure A4). 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 Figure A5).

We consider an officer death event to be any instance where one or more officers in a department died in a particular month.[14] 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 regression, where we include indicators for each month around the date of the incident:

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

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 3.1. 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

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

neighbor matching algorithm. We likewise display analogous estimates using synthetic control methods, which construct a weighted control group for each treated unit.

3 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.[15] 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 3 and the second line of Table A2, specification (1). Likewise, when this model is estimated in levels, the first month coefficient on reported murder is statistically indistinguishable from 1 (Table A2, specification (8)), corresponding to the treatment of the officer death itself. We confirm the unexpected nature of treatment in Figure 3, 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 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, the effects are smaller and insignificant three to twelve months (the long-term effect) after the incident.[16] Overall, the event study versions of the arrest results in Figure 4 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.

[15] For all analyses where violent crimes and arrests are the outcome, we exclude murder offenses.

[16] 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 4).

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 19 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.[17] Collectively, the estimates show 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.

Why might officers reduce the number of arrests that they make following the death of a peer? When an officer is killed, peers in their department are sharply reminded of the potential risks of working in law enforcement. This salient shock to the perception of risk could increase fear among officers and cause them to take new protective actions to reduce this risk. Officers have a high degree of discretion over the ways in which they engage in their jobs in the field. In particular, interactions with civilians which are "officer-initiated," which can include arrests, do not occur unless an officer is motivated to participate in the activity. Following the death of a colleague, officers may feel that engaging in an adversarial interaction with a suspect is not worth the potential risk of injury or death that could occur during that interaction. Our finding that officers decrease low-level arrests suggests that they may adjust their threshold for what types of offenses are serious enough to be worth their enforcement effort.

How do crime outcomes change after an officer death? Crime rates may be viewed as a marker of police effectiveness, and here, we are interested in how changes in the emotional state of officers could have consequences for public safety. The third panel of Table 2 shows that crime and community activity *do not* increase in the ensuing period. We find small and statistically insignificant estimates for both violent and property crimes. 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. 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 5). 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.

We next 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 decisions

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

to officially record crimes or police enforcement decisions. This less "filtered" proxy for criminal activity also does not increase after an officer death. Our point estimate for the short-term 911 call response is close to zero, and we can rule out a greater than 3.9% (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. The traffic fatality outcome has the advantage that it is a function of traffic offenses and is a proxy for reckless driving but is not a function of either victim reporting or police reporting, since nearly all fatal traffic accidents are reported. Despite the large decrease in the point estimates on traffic stops following an officer death, the number of fatal traffic accidents does not change.[18] 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.

3.1 Robustness Specification Tests

In this section, we conduct several robustness checks to scrutinize our results. First, in Figure A1, we re-estimate the model dropping one treatment event 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 Figure A2. 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.

Table A2 includes a number of alternative specification tests, all of which find similar results to our preferred specification. The first specification replicates the baseline results and also includes an adjusted measure of the murder outcome that excludes officer fatalities (1).

[18] While enforcement of traffic offenses has been shown to affect traffic offending (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.

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 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 rather than as a single event (4). Our estimates are also similar when excluding the city-by-calendar month fixed effects from the model which adjust for seasonality in outcomes that may differ by department (5). Additionally, 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 (6). Further, excluding arrests for driving under the influence (DUI), the single offense for which we observe the strongest arrest decline (see Section 4.3 below), does not change the pattern of the results in (7).

The results are also largely similar when we alter the measurement of the key outcomes. For example, the estimates are consistent when we use 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 (e.g. 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 Figure A3. 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 (or less conservative), and we continue to find no positive crime effects in any period and a long-term decline in violent crime. We show in Figure A4 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.[19] 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.

4 Heterogeneity

In this section, we consider how our arrest and crime impacts vary by characteristics of the city and line-of-duty death. 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 following an officer death.

[19] 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.

4.1 Size of Arrest Decline and Crime Effect

To identify heterogeneity in treatment effects, we estimate an individual arrest treatment effect for each death event in our sample. We do this by adopting a synthetic control approach, which constructs a control unit that is a weighted average of multiple control units, which minimizes the difference in a set of pre-period characteristics between the treated agency and the weighted "synthetic" control unit.[20] We then take the difference between treatment and synthetic control in the post-period to identify the death's effect on arrest and crime rates for each officer death, which we denote generically by $\hat{\tau}$. We plot the average of arrest and crime outcomes across our treatment agencies versus the synthetic control groups in Figure A5. The plot confirms that, as in our nearest neighbor approach, treated and synthetic control agencies are well-matched on pre-period trends and our post-period effects are consistent with our baseline results, showing a one to two month arrest decline but no change in crime.

We then ask how these event-specific treatment effects vary with city and incident characteristics. We focus first on the first-month arrest effect estimates and regress these estimates on city and incident characteristics, where observations are weighted by the inverse of the standard error squared of each $\hat{\tau}_i$. Our results are shown in Table 3. In column 1, we regress on agency-level covariates. Arrest declines are more negative in cities with smaller population, lower crime rate, and a higher black share of population, and there is no relation with officers per capita. These patterns are consistent with the officers in these cities facing a bigger shock from a peer death since they work in a relatively less-active criminal environment. A joint F-test of all covariates rejects the null that there is no relation to the arrest effects, though this test is only marginally significant (p-value = 0.065).

In column 2, we regress the arrest effects on characteristics of the officer death event. We find no relation between the arrest impact and whether the deceased officer was white versus non-white, male versus female, whether the incident was during a traffic stop, or whether the main suspect was apprehended within 48 hours. As we discuss in Section 5, this final result

[20]For each event, we restrict attention to a "donor pool" that consists of the one hundred cities that are closest in terms of the nearest neighbor matching algorithm and who do not have an officer death of their own in the year before and after the treated agency's event. The same matching variables used in the Nearest Neighbor models cited above are used to create the donor pool. Then, we implement a synthetic difference-in-differences estimation method for each treatment event. We match the pre-period arrest and crime outcomes as well as characteristics of log population, city-level poverty rate, share white and share with a high school degree or less education in the procedure. From the procedure, we obtain the treatment and control series for each time period. We conduct placebo method by replacing the treatment unit with a control unit and we repeat this procedure 100 times to obtain variance estimates.

is notable, since it indicates that the decline in enforcement is not driven by fellow officers shifting from their regular activity to apprehending the suspect. Column 3 combines the city and incident characteristics in one regression. The coefficients on population, share black, and crime rate have a similar magnitude as in Column 1, but only population is still statistically significant.

Do cities that we predict to have bigger arrest declines also experience increases in crime rates? We next use the agency and event characteristics to construct a *predicted* version of our treatment-specific estimates, $E(\hat{\tau}|X)$. We then split treatments into three groups based on the values of $E(\hat{\tau}|X)$: the top quartile of predicted arrest declines, the interquartile range, and the bottom quartile.

Figure 7 plots the arrest and crime changes over time for these three separate groups, where arrest and crime changes are individual treatment-specific synthetic control estimates ($\hat{\tau}_i$) and groups are defined by quartiles of the predicted arrest effect.[21] In each figure, we present the interquartile range of estimated effects in the dashed gray lines around the median treatment effect. The benefit of this approach is that it splits the sample into groups with different sizes of arrest decline while only leveraging variation in pre-treatment characteristics. The left panel plots the pattern for officer deaths with the largest predicted arrest declines, while the right panel plots the pattern for the smallest predicted arrest declines. Despite the substantial variation in arrest declines, there is no systematic increase in crime across any group. In particular, we do not identify an increase in crime for treatments in the top quartile of arrest declines, where the median arrest decline is greater than 15%.

While the patterns above suggest a null crime effect even for agencies with larger arrest declines, this analysis is limited to the predictable variation in arrest effects based on city and incident characteristics. We next investigate variation in effects across cities based directly on the estimated magnitude of arrest decline. To do so, we return to our baseline estimation strategy from Section 2. 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,

[21] Table A3 shows the average agency and death characteristic covariates for each binned group.

and we construct our 95% confidence intervals using a bootstrap procedure.[22]

Figure A6 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. The top figure presents the crime residuals for the first month and shows a flat relationship with the size of an arrest decline. Within a range of a 20% arrest 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.

4.2 Length of Arrest Decline and Crime Effect

To examine heterogeneity in effect sizes by duration of arrest decline, we take our residuals calculated in Section 4.1 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 A7. 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.[23]

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.

In this exercise, we stratify our sample by an outcome of the treatment rather than using pre-treatment experimental variation in the duration of arrest decline. As a result, we do

[22]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.

[23]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).

not claim to have identified the causal impact of arrest declines at various durations. Similar caution is needed in interpreting our second analysis in Section 4.1, which stratifies effects by magnitude of arrest decline using estimated residuals from our model. We note that this methodological limitation is not a concern for the above analysis that stratifies estimates by predictions of treatment-specific synthetic control estimates. Across these three tests however, the results are remarkably consistent; they imply that there is not a threshold magnitude or duration of arrest decline within our sample that does generate a crime increase.

4.3 Crime and Arrest Sub-Types

Next, we estimate the baseline 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 A4 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. 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. For index crime, we observe no significant changes in any category in the first month of treatment or the month after.

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 A5 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.[24] 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.[25] Given that we observe a large reduction in DUI arrests, we explicitly measure the subset of fatal traffic accidents that involve a drunk driver (Table A7). These alcohol-related accidents do not respond to the reduction in DUI arrests associated with an officer death. Likewise, as

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

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

discussed above, the decline in total arrests persists after excluding DUI arrests (see Table A2, specification (7)).

4.4 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.

5 Mechanisms

5.1 Is the Decline in Arrests a Behavioral Response?

We argue that the arrest decline after an officer death is a behavioral response by fellow officers caused by heightened fear of on-the-job risk. We consider here the alternative explanation that the decline is due to a reduction in effective manpower, from either the deceased officer themselves or from their peers.

Quantitatively, our observed arrest declines are 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 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. If we make the additional conservative assumption that officers are given ten days (half a month) of bereavement leave,[26] this decline would correspond to 68 officers taking leave, or a quarter of the average treated department's patrol force. Even if the officer who died was exceptionally active in making arrests, it is very unlikely that the loss of

[26] Ten days leave is higher than what we observe anecdotally, and we pick this number to be conservative. Our online searching indicates that three days leave is a common amount offered, e.g.: `https://www.tdcj.texas.gov/divisions/hr/benefits/leave-paid.html`.

the deceased officer is driving the results that we find, nor is it likely that one in four officers would reduce their arrest activity to zero after a colleague's line-of-duty death.

Alternatively, effective manpower could be impacted if officers are diverted from normal activities to investigate the death of their peer. As a direct test of whether the arrest declines are due to officers investigating their colleague's death, we revisit our analysis of event-specific impacts in Section 4. Table 3 indicates that arrest declines are similar in magnitude in cases where the suspect in the case is apprehended or killed within 48 hours, providing validation 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, events that likewise incapacitate a deceased officer. Table A7 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.

5.1.1 Employment-level Outcomes

Beyond temporary leave, it could be the case that officers choose to exit the police profession following the death of a peer. To investigate this question, we link records of officer deaths to data on employment spells for police in Florida from the Florida Department of Law Enforcement. Results of this analysis are presented in Panel A of Table A7. We are able to confirm the officer death effect in this data but fail to find robust evidence of any behavioral responses on officer employment. On net, the number of full-time equivalent officers is unchanged after an officer death and there is no systematic change in quits, firings or hirings. If anything, officer quitting appears to slightly decline in the long-term period after a death (effect size is equivalent to 1 additional officer employed off of a base of 513 during this period). Collectively, this evidence shows that while officers do seem to respond to peer deaths by reducing the number of arrests that they make, peer deaths do not motivate officers to quit policing.

5.2 Do Police Change other aspects of their Behavior?

5.2.1 Police Discretion in Recording Crimes

One alternative 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 that they 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 and Figure 6, 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.

5.2.2 Police Use of Force

It could be the case that officers 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., 2020; 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 A7.[27] For both outcomes, we find a small and statistically insignificant coefficient for the first-month effect of an officer death, implying no change in use of force. In the long-run, we find a marginally significant increase in only the Fatal Encounters measure.

Broadly, we view this evidence as suggestive that there is no use-of-force response to an

[27] 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.

officer fatality, as scholars have highlighted issues of under-reporting and data quality in these data series (Loftin et al., 2017; Renner, 2019; Goncalves, 2021). Both data sources are known to suffer from significant under-reporting and to have varying quality over time (Loftin et al., 2017; Renner, 2019; Goncalves, 2021), so we consider these results to be suggestive evidence.

5.3 Do Officer Deaths Impact Civilian Behavior?

We are also interested in whether an officer death itself directly causes civilian criminal activity or victim reporting behavior 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. To address this question, 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 4.1 above, 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 any declines in crime for departments with no arrest declines. This pattern supports a story where officer deaths generate fear and behavioral responses among peer officers but do not directly impact civilian offending behavior.

A second concern relates to whether we might be missing changes in crime that occur for categories outside of the most serious UCR Index I offenses. Here, we can examine effects from our 911 data collection. These 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.

One way to further probe the question of whether an officer death affects civilian behavior is to ask whether officer deaths are actually salient to civilians. Figure A8 plots the Google Trends search intensity of 71 officers killed in the field, which we compare to 137 high-profile deaths of *civilians* at the hands of police since 2010 using searches from the U.S. state where each event occurred.[28] Google trends provides a metric of *relative search* volume that is normalized

[28]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. 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.

between 0 and 100 and is a function of terms entered in a search (selected by the user). We include topical searches for heart attacks as a benchmark (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.[29]

In relative terms, the public is far less aware of the officer deaths than civilian deaths at the hands of police, with the average civilian death having a search popularity metric 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 is quite small and quickly levels to zero after these events. Collectively, this evidence implies that the awareness of these deaths among community members is relatively minimal and short-lived. As a result, we hypothesize that officer deaths are unlikely to spark a change in criminal activity or civilian behavior in the community.

If it is the case that an officer death has limited direct effects on civilian or offender behavior, it is also possible to connect our findings to the open and unresolved question of whether and how changes in marginal arrest enforcement may impact crime. Viewed through this lens, our work provides new insights about the importance of changes in arrest enforcement to public safety, and it is 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.[30] 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 A9 shows that these crime-to-*arrest* elasticities are notably less negative

[29] All quantities are reported relative to the time period and search term with highest search volume, which is given a value of 100. We include topical searches for heart attacks as a benchmark (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. 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. 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. We purposefully select our benchmark to show that there is evidence of some salience of officer deaths in our data.

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

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. To put our magnitudes in context, 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 effect of 13,000 more crimes.

6 Conclusion

How does fear affect officer behavior and police efficacy? Policing is a dangerous and high-stakes profession where the undercurrent of fear has the capacity to influence officer actions. Likewise, if fear elicits a behavioral response among officers, the quality of police performance could suffer, with potential adverse consequences for public safety.

We examine these questions by examining officer deaths in the line-of-duty as shocks to the saliency of fatality risk for peer officers. Using data on over 1,500 police departments between 2000-2018, we find that police respond to an officer fatality by substantially reducing the number of arrests they make, with the largest effects for low-level arrests. When an officer chooses not to engage with a suspect and make an arrest, the officer is minimizing their likelihood of interacting with an individual who could cause that officer physical harm; thus, the reduction in arrest activity we observe is consistent with risk mitigation due to heightened fear. While we observe a sharp 10% decline in arrests in the one to two months following an officer death, we fail to find any evidence that this shock to risk salience reduces public safety. Further, we probe heterogeneous crime responses and do not find evidence that crime increases in settings where officers reduce arrests by larger amounts or longer durations. Collectively, the results imply that officer fear reduces enforcement, but that this fear is unlikely to contribute to higher rates of crime.

In our analysis, we find limited evidence that officers change their behavior in dimensions other than arrest enforcement, or that civilian or offender behavior is directly impacted by an officer death. Given this pattern of findings, our work may offer suggestive insights for understanding the impact of arrest reductions on crime, which remains an open question in the economics of crime literature.

This interpretation of our findings raises 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 at baseline, and if this is not their explicit objective, what objectives and incentives are motivating officer choices. Such questions are critical to the broader debate about the utility of law enforcement's heavy reliance on policing low-level offenses, an approach popularized since the 1980s as part of a "broken windows" policing philosophy (Bratton and Knobler, 2009; Kohler-Hausmann, 2018; Speri, 2020; Silva, 2020; Zimring, 2011; Riley, 2020). Related work on policies that affect arrest enforcement, such as changes in the classification of felony offenses (Dominguez et al., 2019) or the decriminalization of marijuana (e.g. Adda et al., 2014; Mark Anderson et al., 2013; Chu and Townsend, 2019; Dragone et al., 2019) have shown limited or mixed evidence of subsequent crime increases. Alternatively, some researchers have found crime-reducing benefits of particular types of enforcement, such as "hot spots" policing (e.g. 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.

Our work aims to understand how fear impacts policing; and does not evaluate a specific policy. Nonetheless, our findings are informative for potential policy reforms. In particular, we note that the behavioral responses we observe are present for numerous similar events in national data, suggesting that they are driven by a large group of officers, rather than individual officers that deviate from policing norms. This broad response implies that if a goal of police reform is to change the nature of enforcement, interventions that impact department-level policy could be more effective than targeted officer-level interventions.

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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 A4 and Table A5 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 A4 and Table A5 for the list of crime and arrest sub-types. “Crime Report Rate (911 Calls)” is the share of calls that result in an officer writing a crime incident report. * p<0.1,** p<0.05, *** p<0.01.

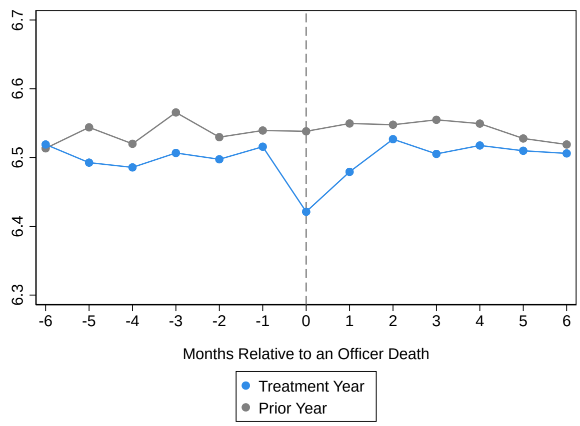
Table 3: Predicting Treatment-Specific Synthetic Control Arrest Effects

Predicting Agency-Level Arrest Effect	Agency	S.E.	Incident	S.E.	All	S.E.
Log Population	0.021*	(0.011)			0.022*	(0.011)
% Black Population	-0.002*	(0.001)			-0.002	(0.001)
Crime Rate	0.015*	(0.009)			0.014	(0.009)
Officers Per Population	-0.005	(0.019)			-0.003	(0.020)
Officer Non-White			-0.002	(0.036)	-0.010	(0.036)
Officer Female			-0.016	(0.090)	-0.026	(0.089)
During Traffic Stop			-0.010	(0.047)	-0.021	(0.047)
Cleared within 48 hrs			0.047	(0.041)	0.035	(0.041)
Weighted Mean	-0.070		-0.070		-0.070	
Variance	0.027		0.027		0.027	
F-Statistic	2.281		0.332		1.230	
p-value	0.065		0.856		0.289	
Observations	120		120		120	

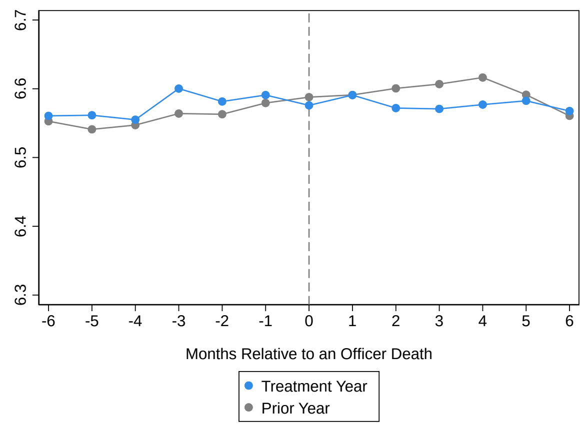
Notes: A set of 100 nearest-neighbor agencies that do not experience officer death within a year of treatment agency's officer death event is generated by matching on demographic characteristics in the treatment year and lagged monthly crime and arrest levels in the year prior to treatment. Then, from this set, a synthetic control agency is created by matching on demographic characteristics in the treatment year. There are 120 matched pairs. The synthetic difference-in-differences is estimated and post-period treatment effects are obtained. The table shows the results of regressing agency-level treatment effect for each respective post-period on covariates. The covariates are the first reported measure for each department. "Weighted Mean" shows the average treatment effect weighted by inverse of the standard error squared and "Variance" is the variance of the treatment effects. * $p<0.1$,** $p<0.05$, *** $p<0.01$.

Figure 1: Unadjusted Data Around Events, Log Outcomes

A. Total Arrests



B. Index Crimes

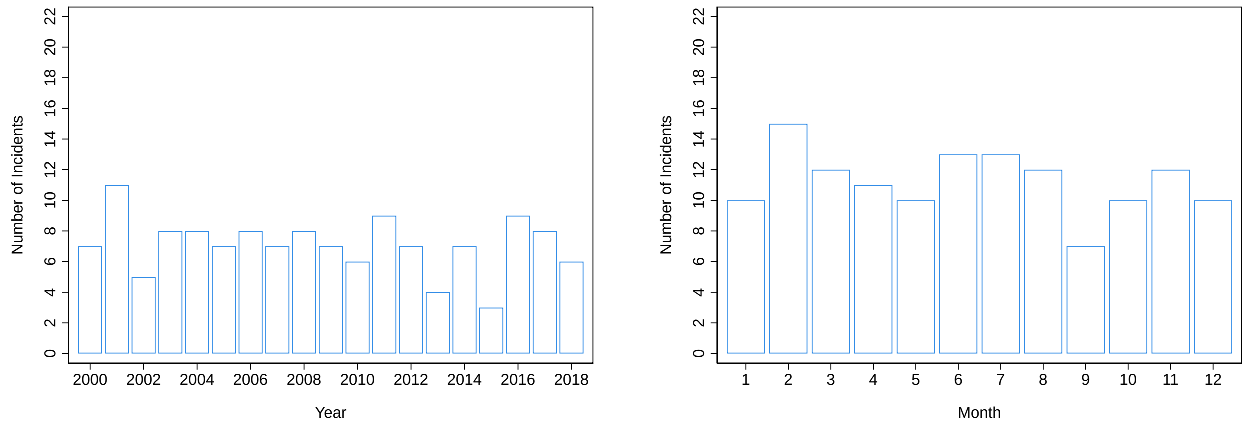


Notes: This figure plots the unadjusted data around the officer death events. Outcomes are defined as $Y_{it} = log(y_{it} + 1)$. There are 125 officer death events in 76 agencies after excluding events that do not have enough periods before and after the event. Index crimes include rape, robbery, aggravated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

Figure 2: Variation in Officer Deaths

A. Officer Deaths by Year

B. Officer Deaths by Month



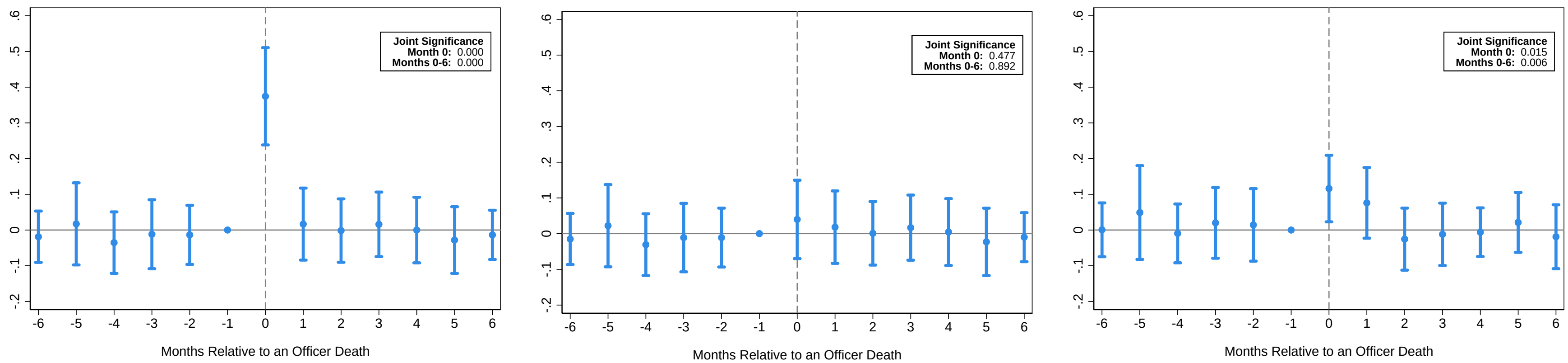
Notes: In 1,578 departments in our sample, there are a total of 135 officer death events in which 151 officers were killed.

Figure 3: Event-Study: Murder Outcomes

A. Total Murder Offenses

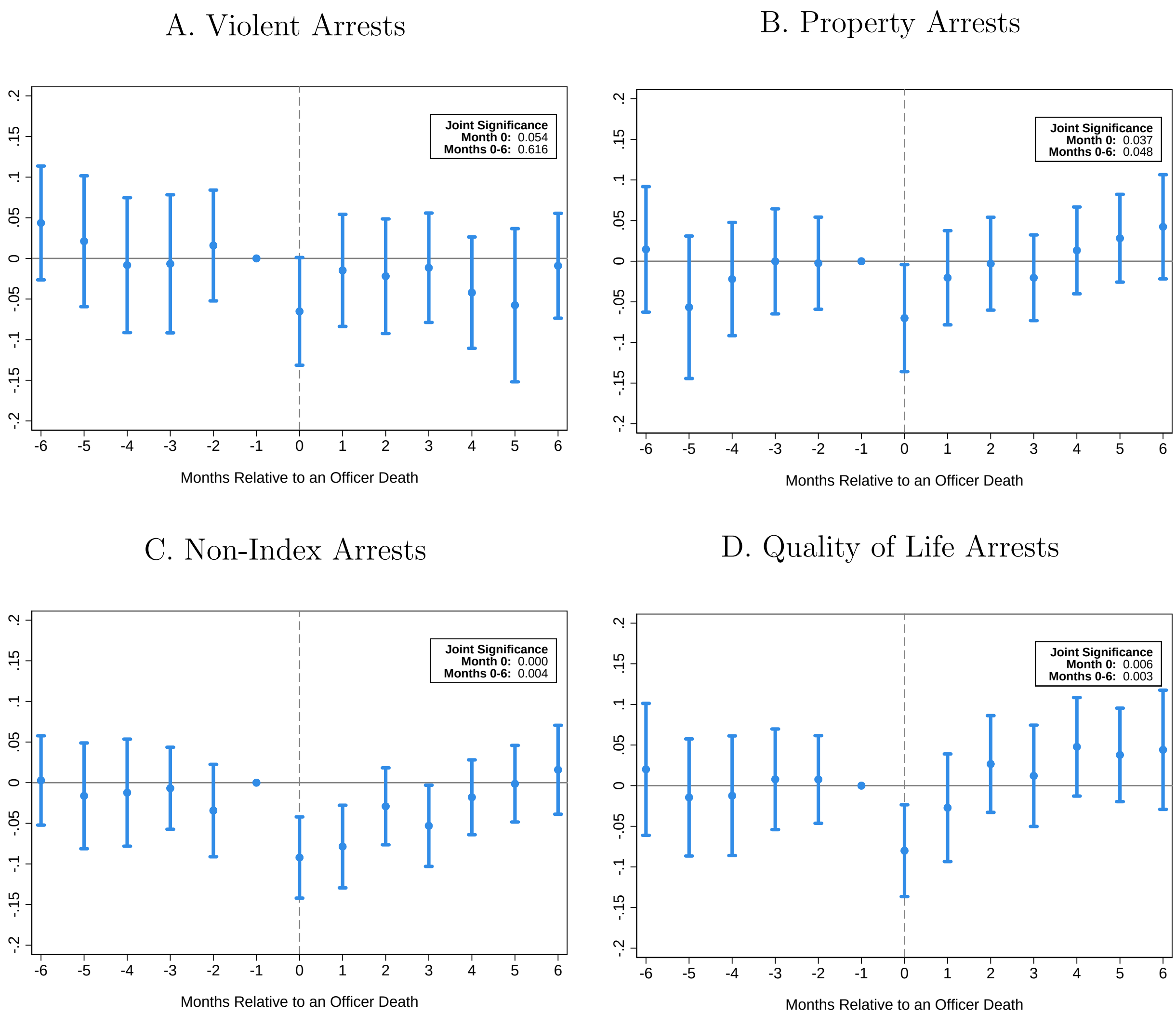
B. Murder Offenses (excl. Officer Fatalities)

C. Murder 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.

Figure 4: Event-Study: Arrests

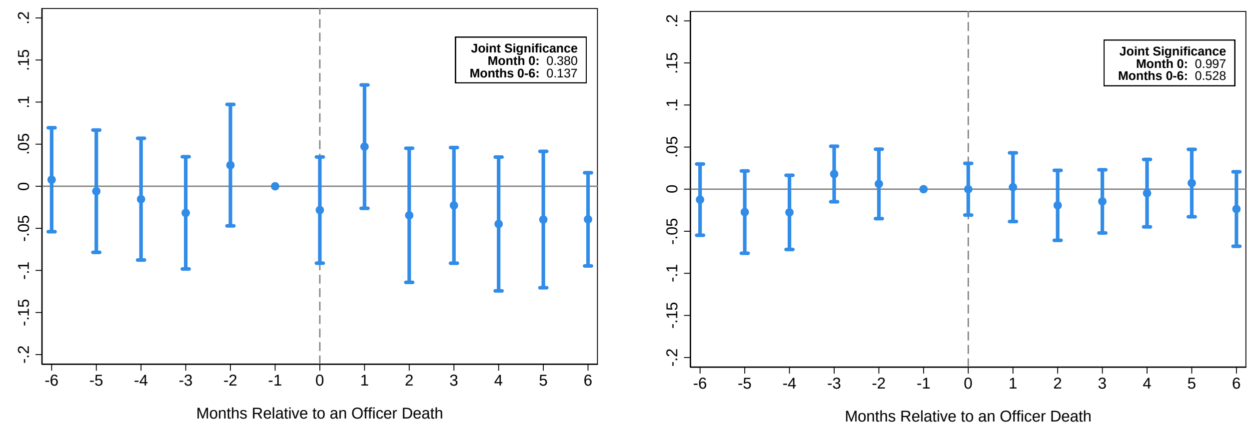


Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Months -6 and 6 include all months before month -6 and all months after month 6, respectively. Standard errors are clustered at the department level. See Table A5 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 5: Event-Study: Crimes

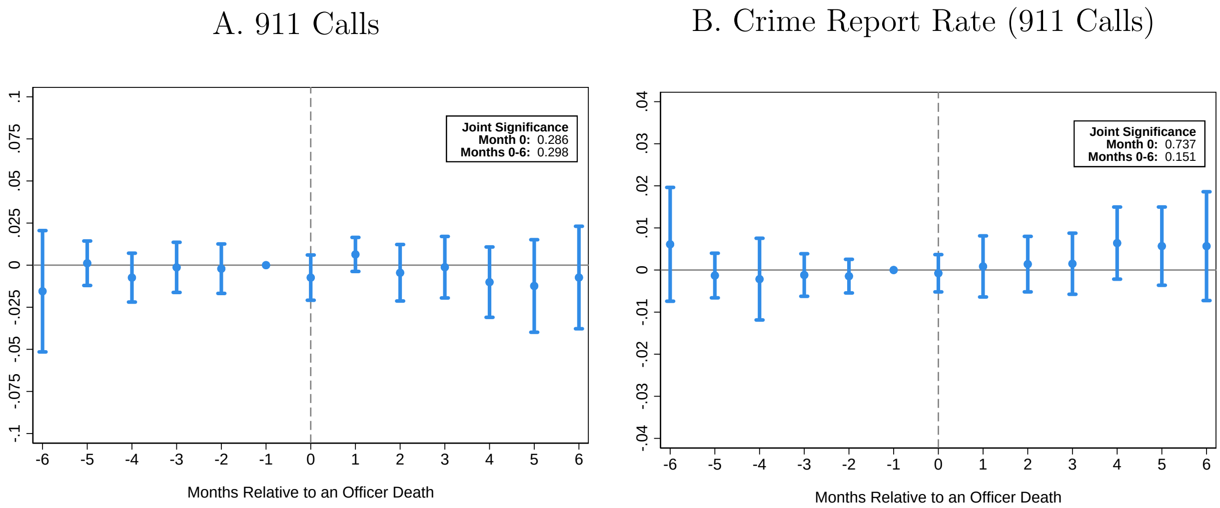
A. Violent Crimes

B. Property Crimes



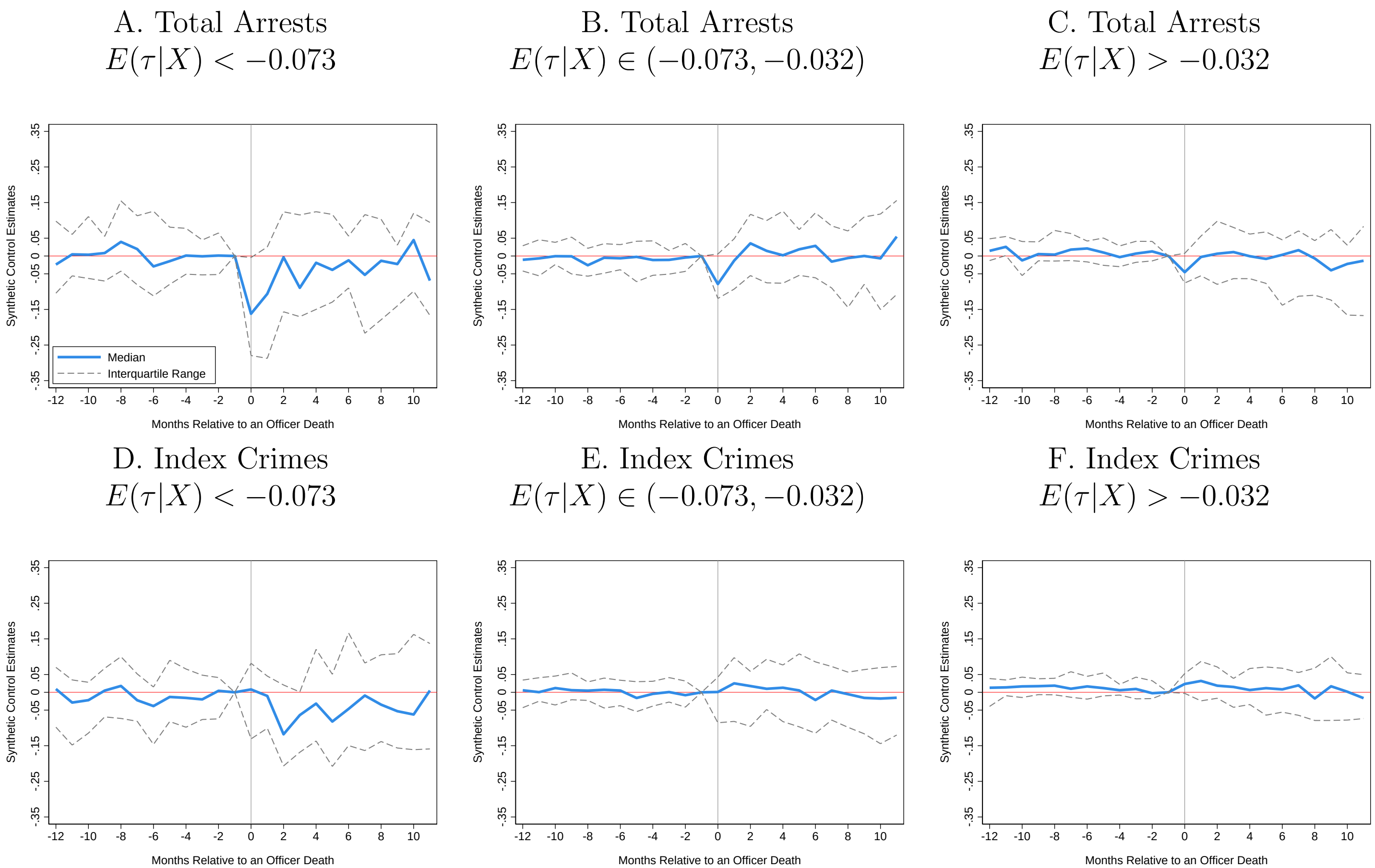
Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Months -6 and 6 include all months before month -6 and all months after month 6, respectively. Standard errors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

Figure 6: Event-Study: 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 7: Heterogeneity by Predicted Arrest Effect Size,
Plotting τ by $E(\tau|X)$ Quartiles



Notes: A set of 100 nearest-neighbor agencies that do not experience officer death within a year of treatment agency's officer death event is generated by matching on demographic characteristics in the treatment year and lagged monthly crime and arrest levels in the year prior to treatment. Then, from this set, a synthetic control agency is created by matching on demographic characteristics in the treatment year. There are 120 matched pairs. The synthetic difference-in-differences is estimated and post-period treatment effects are obtained. Panels A, B and C show the treatment effect for total arrests, separately by predicted arrest effect quartiles. Panels D, E and F show the treatment effect for index crimes, separately by predicted arrest effect quartiles.

A1 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		N
							Full	Treated	
(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: Agency-Level Characteristics by Predicted Arrest Effect Size

Agency-Level Characteristics by Predicted Arrest Effect Size (Months 0 & 1)	Full Sample	Top Quartile $E(\tau\|X) <$ -0.073	IQR $E(\tau\|X) \in$ $(-0.073, -0.032)$	Bottom Quartile $E(\tau\|X) >$ -0.032
Log Population	12.041	10.537	12.166	13.402
% Black Population	20.301	22.858	20.868	16.403
Crime Rate	4.753	3.953	4.448	6.286
Officers Per Population	2.432	2.388	2.379	2.574
Officer Non-White	0.117	0.034	0.113	0.214
Officer Female	0.050	0.138	0.032	0.000
During Traffic Stop	0.133	0.172	0.145	0.071
Cleared within 48 hrs	0.840	0.621	0.871	1.000

Notes: A set of 100 nearest-neighbor agencies that do not experience officer death within a year of treatment agency's officer death event is generated by matching on demographic characteristics in the treatment year and lagged monthly crime and arrest levels in the year prior to treatment. Then, from this set, a synthetic control agency is created by matching on demographic characteristics in the treatment year. There are 120 matched pairs. The synthetic difference-in-differences is estimated and post-period treatment effects are obtained. This table shows the department characteristics splitting the sample by the predicted treatment effect size from Table 3. * $p<0.1$,** $p<0.05$, *** $p<0.01$.

Table A4: 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 A5: 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 A6: Heterogeneity, Arrestee Demographics

	1st Month		2nd Month		Long-Term		Outcome Mean			p-value
	(t=0)	S.E.	(t=1)	S.E.	(t=2,...,11)	S.E.	Full	Treated	N	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

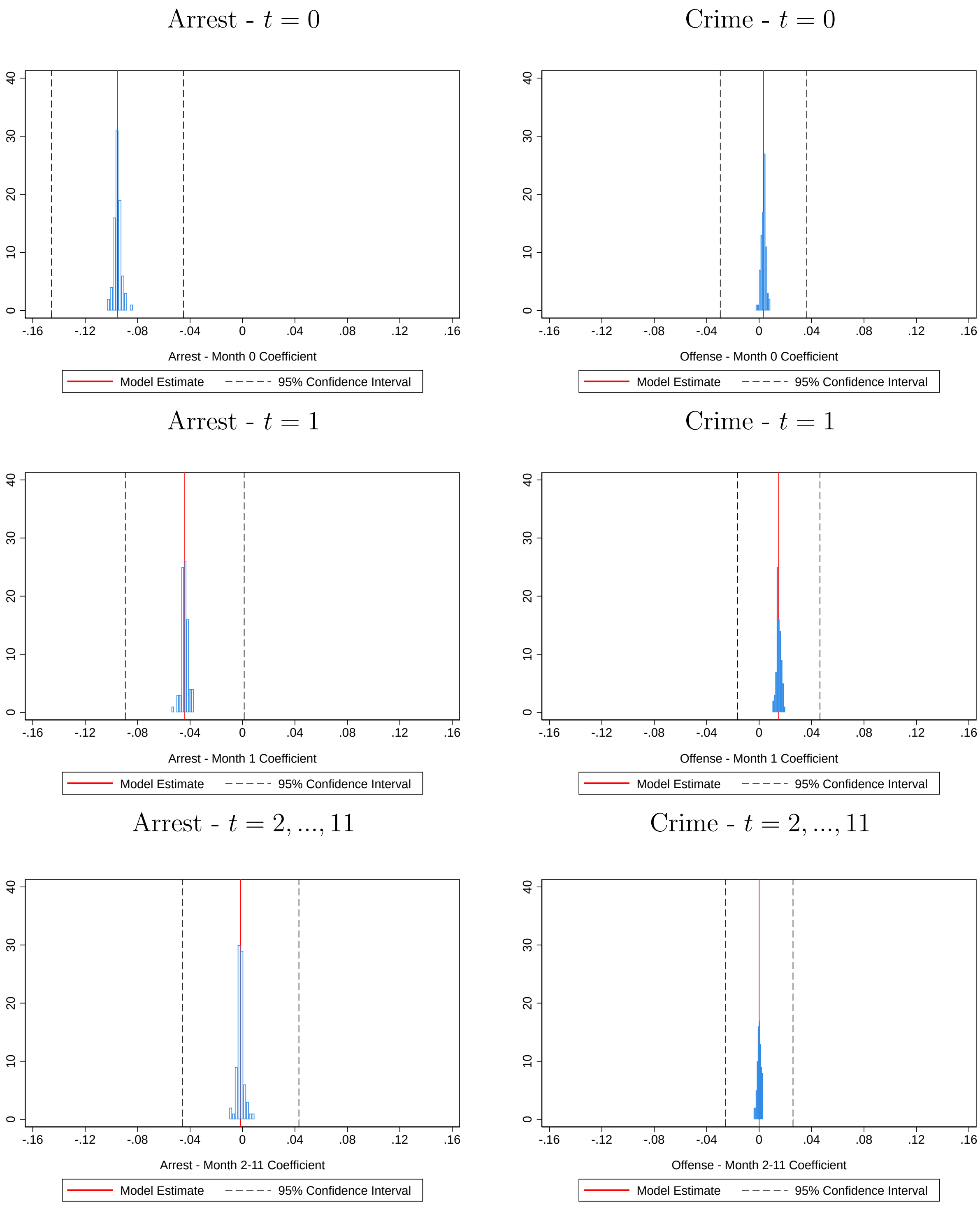
Notes: Regressions in include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Regressions also include a dummy variable for 12 or more months after the occurrence of an officer death. Outcomes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

Table A7: 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
A. Employment Outcomes, Florida									
Full-Time Equivalent Officers	0.005	(0.014)	0.003	(0.014)	0.004	(0.014)	108.0	512.7	71736
Number of Hired Officers	0.229	(0.204)	-0.231	(0.157)	-0.080	(0.065)	0.8	2.9	71736
Number of Fired Officers	0.022	(0.075)	0.109*	(0.058)	0.003	(0.022)	0.1	0.4	71736
Number of Officer Deaths	0.630***	(0.049)	0.024	(0.031)	0.002	(0.006)	0.0	0.1	71736
Number of Officer Quits	-0.047	(0.062)	0.039	(0.072)	-0.042**	(0.021)	0.6	2.4	71736
B. 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
C. 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
D. 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

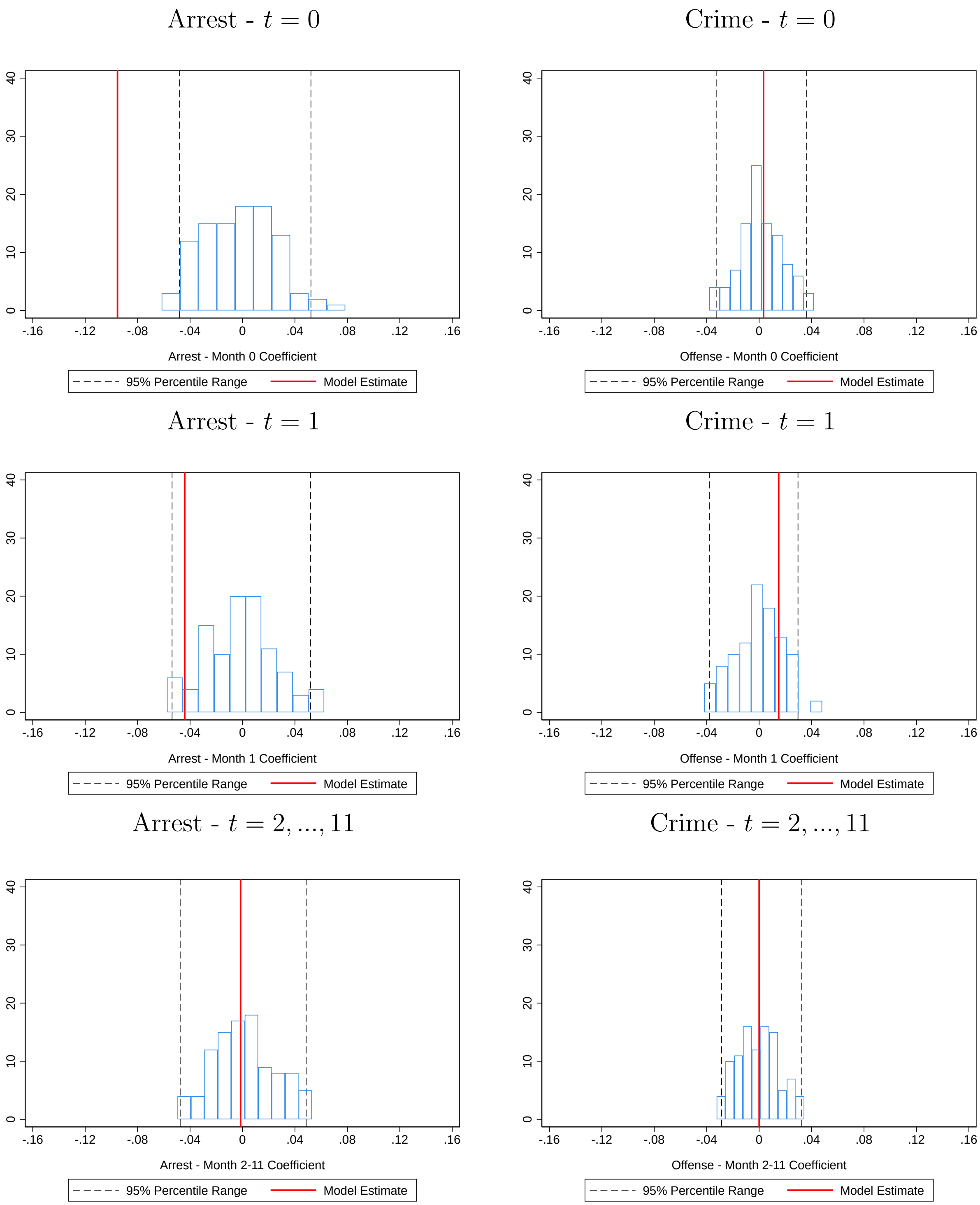
Notes: All regressions include department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Additionally, regressions in Panels B, C and D also include a vector of covariates at the department-by-year level. Regressions also include a dummy variable for 12 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. "Employment Outcomes, Florida" panel uses the officer-level data from the Florida Department of Law Enforcement and covers all law enforcement agency officer employment spells from 2000 to 2016. Reasons for termination include violations of policies or standards, failure to qualify, misconduct, etc. Officer quits include all voluntary separations. "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. "Accidental Officer Death" panel shows the four main outcomes using the accidental officer death as a treatment instead of felonious death. There are 73 officer accidental death events. * $p<0.1$,** $p<0.05$, *** $p<0.01$.

Figure A1: Distribution of Coefficients Dropping Single Treated Agency



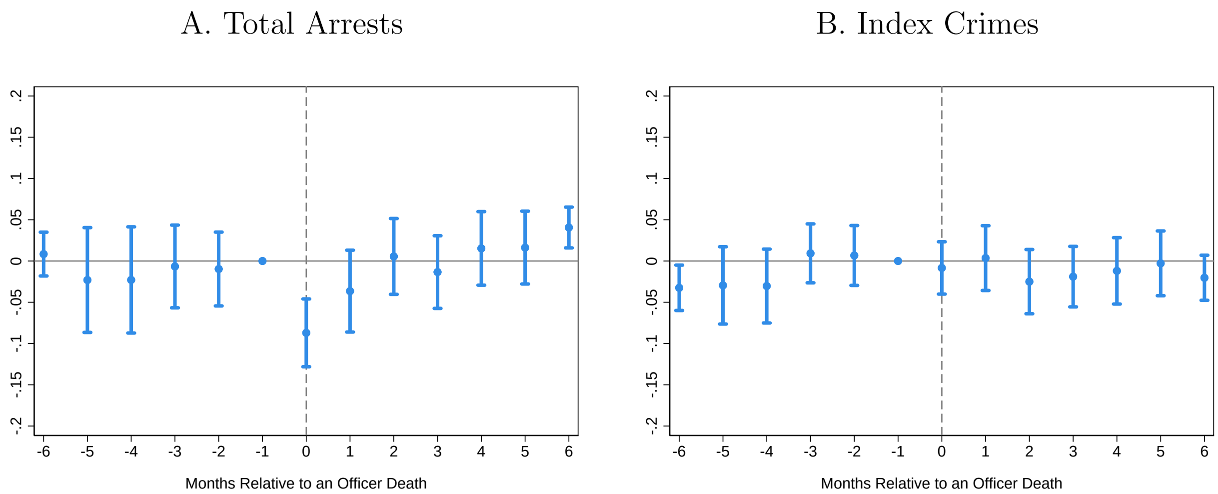
Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects and department-specific linear time trends. Regressions also include a dummy variable for 12 or more months after the occurrence of an officer death. Standard 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 A2: 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.

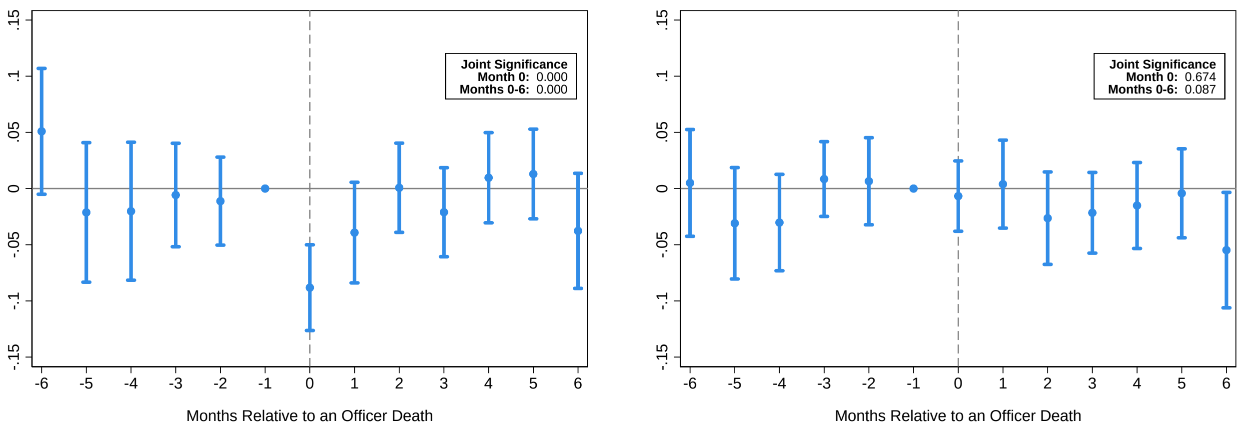
Figure A3: Event-Study: Sun and Abraham (2020)



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 A4: Event-Study: Omitting Agency-Specific Linear Time Trends

A. Total Arrests B. Index Crimes

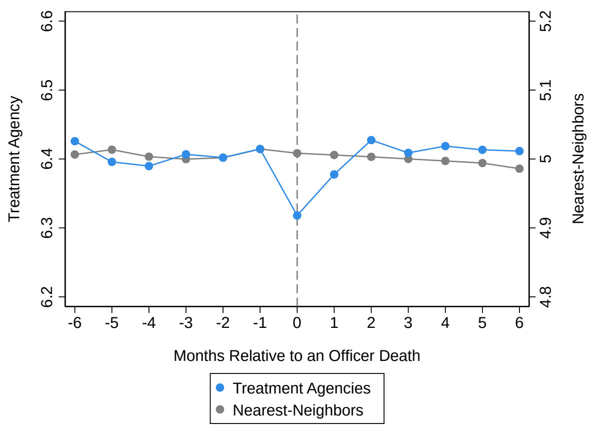


Notes: All regressions include a vector of covariates at the department-by-year level, department-by-calendar month and year-by-month fixed effects. Months -6 and 6 include all months before month -6 and all months after month 6, respectively. Standard errors are clustered at the department level.

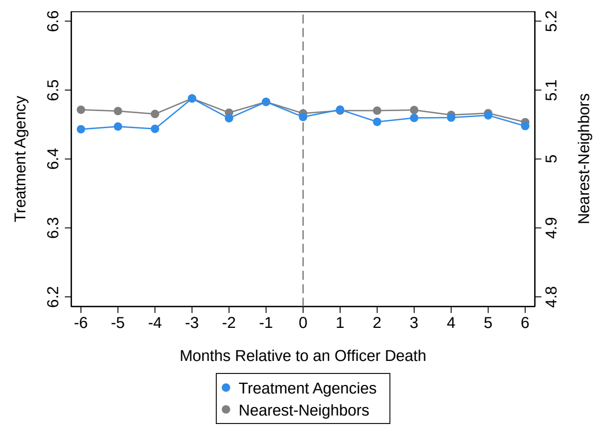
Figure A5: Raw Data: Nearest-Neighbor Matching and Synthetic Control

Nearest-Neighbor Matching

A. Total Arrests

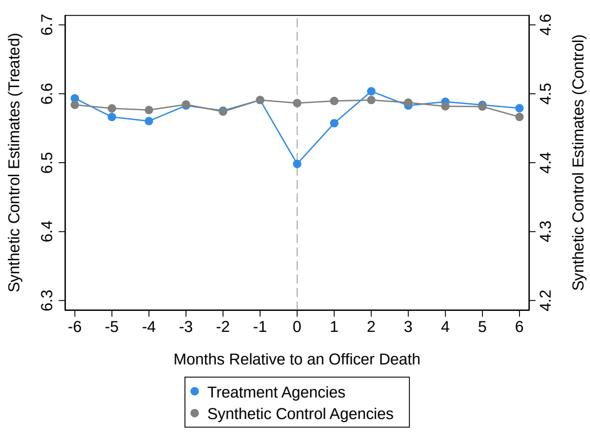


B. Index Crimes

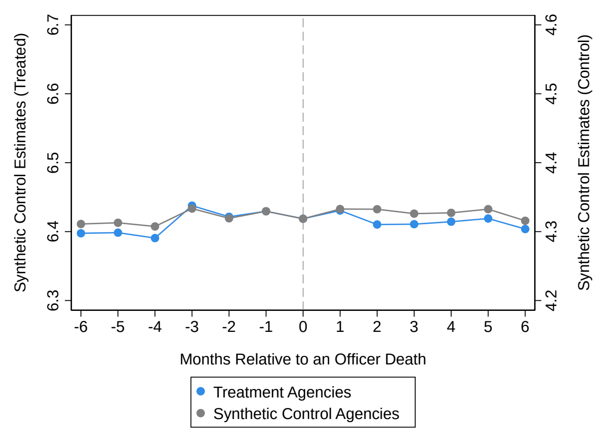


Synthetic Control

C. Total Arrests



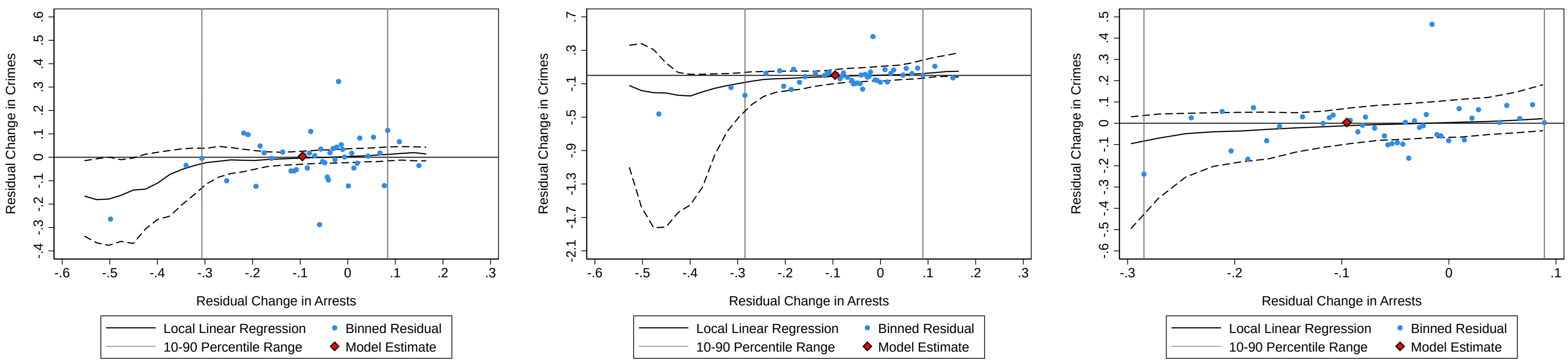
D. Index Crimes



Notes: This figure plots the data around the officer death events. Panels A and B 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 C and D use the synthetic difference-in-differences estimation method. A set of 100 nearest-neighbor agencies that do not experience officer death within a year of treatment agency's officer death event is generated by matching on demographic characteristics in the treatment year and lagged monthly crime and arrest levels in the year prior to treatment. Then, from this set, a synthetic control agency is created by matching on demographic characteristics in the treatment year. The synthetic difference-in-differences is estimated and control and treatment series of all periods are obtained. There are 120 matched pairs.

Figure A6: Arrest to Crime Curve

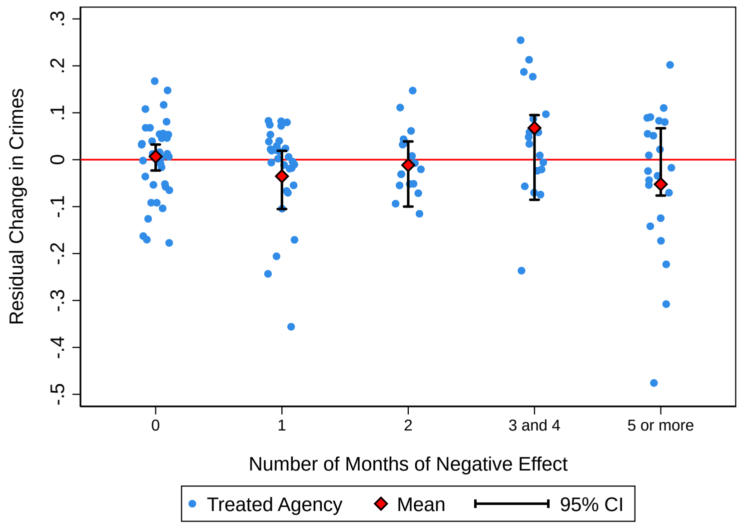
A. Month Effect ($t = 0$) B. Year Effect ($t = 0, ..., 11$) C. Year Effect Zoomed-In ($t = 0, ..., 11$)



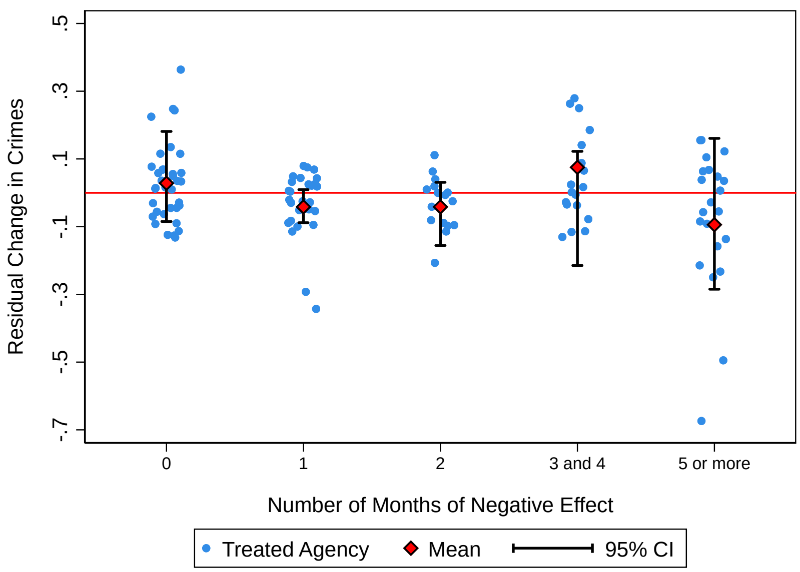
Notes: The residual changes in arrest and crime are estimated conditional on covariates, a department-specific linear time trend, department-by-calendar month and year-by-month fixed effects and differenced relative to the month prior to a line-of-duty death. The x-axis on all plots shows the residual 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 A7: Crime Impact by Length of Arrest Decline

A. Month Effect ($t = 0$)



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

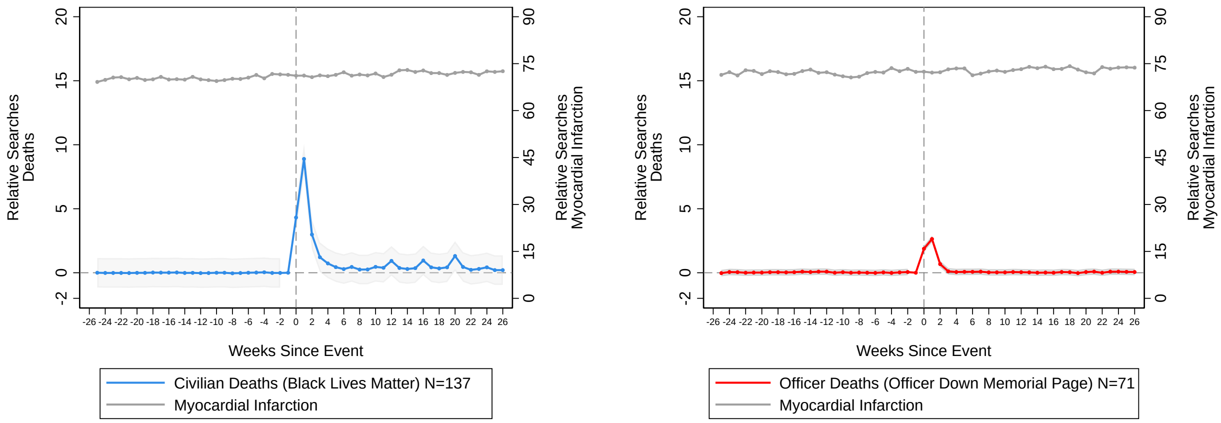


Notes: The residual changes in arrest and crime are estimated conditional on covariates, a department-specific linear time trend, department-by-calendar month and year-by-month fixed effects and differenced relative to the month prior to a line-of-duty officer death. The length of arrest effect (x-axis)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

Figure A8: Google Trends Analysis, Search Volume *Relative* to Benchmarks

A. Civilians Killed by Police

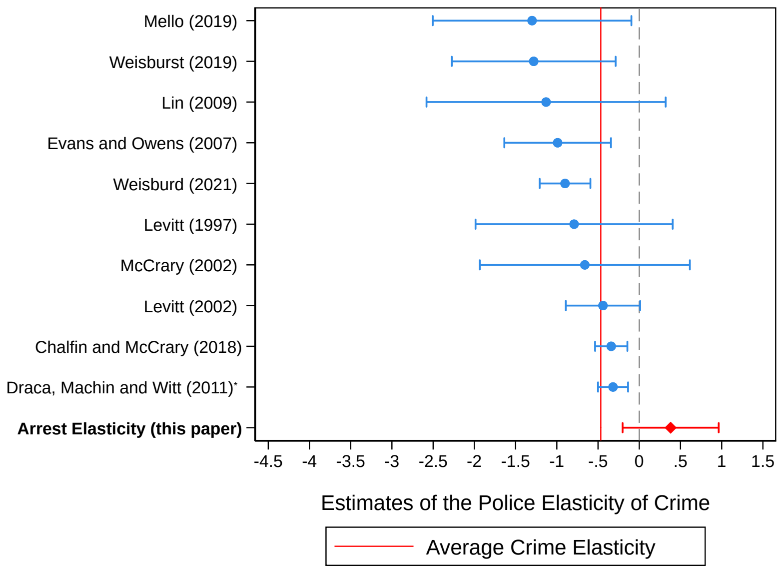
B. Officers Killed in the Line-of-Duty



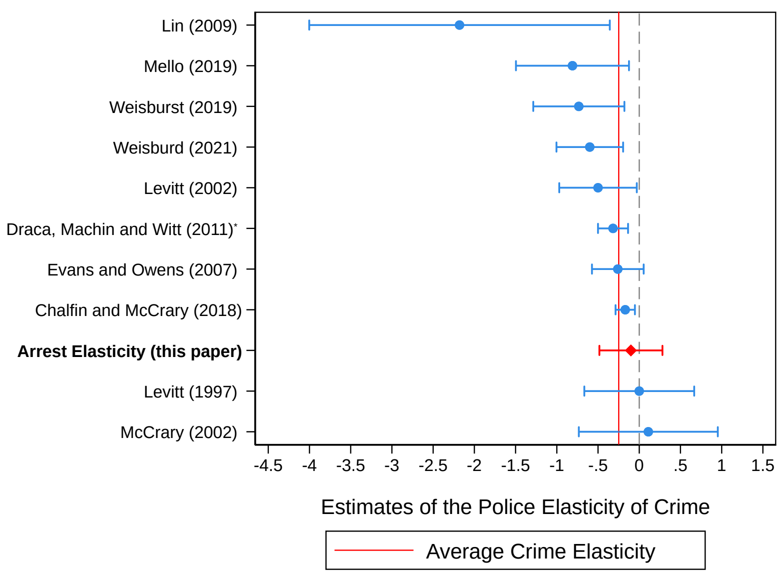
Notes: Each search term is an exact first and last name for the individual. We identify high-profile civilian deaths using a list compiled by *Black Lives Matter*, and identify officer deaths by linking the FBI LEOKA data we use in this project to records from the *Officer Down Memorial Page* to obtain 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 A9: 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.

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.

Employment Data: Florida Department of Law Enforcement (FDLE) Florida Department of Law Enforcement (FDLE) has information on all officer employment spells employed including the employing agency, start and end dates of the spell and the reason for separation. We restrict attention to all law enforcement agency officer employment spells that cover the period 2000 to 2016.

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.