Intensified Scrutiny and Bureaucratic Effort: Evidence from Policing and Crime After High-Profile, Officer-Involved Fatalities*

Deepak Premkumar†

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Abstract

This paper provides the first estimates of how high-profile, officer-involved fatalities (OFs) affect the arresting patterns for the involved police department and crime in that jurisdiction. To address the simultaneous effects that could occur after an OF—(1) greater scrutiny of police, (2) reduced community cooperation with identifying and locating suspects, (3) reduced civilian crime reporting, and (4) changes in offending behavior—I develop a theoretical model of policing behavior to provide empirical predictions of the changes in arrests due to each of the four possible channels. Following a high-profile OF, theft arrests drop by 3–11%, while arrests for the least serious offenses (e.g., marijuana possession and disorderly conduct) see sharp declines of up to 33%. Notably, arrests do not change for violent crime or more serious property crimes. These findings are consistent with the model's prediction for scrutiny as the causal channel for the reduction in arrests. While the decline in arrests for theft is temporary, it persists for the least serious offenses, representing a sustained transition to a lower equilibrium effort. I also find substantial increases in *offending*: There is a significant rise of 10–17% in both murders and robberies. There are also smaller increases of 3–7% in theft and motor vehicle theft.

JEL Codes: K42, H76, J15, D73, H41

Keywords: Ferguson Effect, officer-involved fatalities, scrutiny, police officer effort, crime

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†Public Policy Institute of California. premkumar@ppic.org.

1 Introduction

How can law enforcement agencies provide safety without compromising trust or imposing undue burden on communities? Recent calls to defund the police have popularized one answer to this question—that they cannot. Theoretically, though, police departments allocate services to reduce crime in their jurisdiction, subject to a constraint imposed by the police budget and operational costs. In the United States, the aggregate annual expenditure on policing services is about $115 billion, while the cost of crime victimization is estimated to be over $310 billion each year (Chalfin, 2015; Auxier, 2020). Several studies (Evans and Owens, 2007; Chalfin and McCrary, 2018; Mello, 2019) find that US cities are underpoliced, in the sense that the additional hiring of police officers would result in welfare gains from reduced crime costs. However, many of these studies do not account for the social costs of policing, broader costs imposed upon on a community from an increase in police presence or the confrontational nature of a policing strategy (McCrary and Premkumar, 2019).

Police departments may not internalize these social costs either: First, these social costs may not be broadly understood across the community if they are concentrated in a subset of the population (Ang, 2020), particularly in the presence of a common set of laws that inhibit public access to complaint, disciplinary, and use of force records for police officers (Bies, 2017). Even if social costs were understood, municipalities do not have market-based mechanisms to elicit their optimal allocation, since policing is a public good. Furthermore, the bureaucratic structure of police departments limits electoral tools to signal and enact local preferences (Friedman and Ponomarenko, 2015). Thus, communities may have to rely on alternative means—such as increased scrutiny through protests, media coverage, or additional monitoring (e.g., cop-watch groups)—to raise awareness and advocate for their preferred policing allocation (Battaglini, Morton, and Patacchini, 2020; Ouss and Rappaport, 2020). In the wake of highly-publicized, officer-involved fatalities, some commentators have suggested that police effort has been reduced due to intensified scrutiny from the community and media, widely known as the *Ferguson Effect* (Comey, 2015; Mac Donald, 2015).

This paper provides the first empirical examination of a key question of policy interest: how does a high-profile, officer-involved fatality in a jurisdiction affect local policing behavior and crime? Using a national analysis that exploits the plausibly exogenous timing of these fatalities to credibly identify causal effects, I explore how police officer effort is affected by public scrutiny, as measured by community awareness, media coverage, and protests. To determine what constitutes "high profile," I create a novel dataset by merging crowdsourced information on officer-involved fatalities (hereafter, OFs) with web-scraped data on the amount of media coverage and local search engine patterns of each incident. I combine this with the FBI Uniform Crime Report, which collects arrest and crime data from most police departments. I use monthly arrest counts, disaggregated by race and crime type, to analyze policing effort in 2,740 police departments, 52 of which experience at least one high-profile, officer-involved fatality between 2005–2016. I then assess whether there are changes in arresting patterns in these treated departments after a fatality relative to control departments—who experience no OFs or less publicized ones—adjusting for changes in population, temporal variation in arrests nationally, time invariant department- and jurisdiction-level characteristics, and county-specific linear trends. Finally, leveraging a similar empirical design, I test to see if these OFs induce any changes in offending behavior.

To include the variation arising from departments that have multiple high-profile OFs, I utilize an event study regression design that allows for more than one event per observational unit. I subset the events to a sample of officer-involved fatalities that generate over 1,000 articles of news coverage. I restrict fatalities to this *high-profile* subset, because they must be large enough shocks to plausibly affect department-level metrics of officer behavior—which is not plausible for the vast majority of OFs, given how common they are (Krishnan, McCrary, and Premkumar, 2017). I also adjust the timing of these incidents to reflect community awareness by assessing when the community first starts searching for the incident using Google Trends. Further, I demonstrate that the municipalities with high-profile OFs experience larger and more frequent Black Lives Matter protests than control jurisdictions.

I measure changes in policing effort through the standard metric of arrests, also known as clearances (Mas, 2006; Shi, 2009; Heaton, 2010).[1] After an OF, there are numerous potential channels that could affect arrests: First, public scrutiny could increase psychic costs for officers (i.e., loss of morale when interacting with a more aggrieved community) and/or their perceived costs of mistakes, making it more costly for officers to exert effort. However, OFs may also affect arrests through other channels besides policing effort, including (2) reduced community cooperation in identifying and locating suspects, (3) reduced civilian reporting of crimes, and (4) changes in criminal behavior, in response to the OF itself and to subsequent signals of policing effort. I develop a novel theoretical model of a police officer's objective function, which provides a set of empirical predictions by offense type to identify the relevant mechanisms given any changes in arrests. Discerning which mechanisms are occurring is integral to test the existence of the Ferguson Effect—that is, whether high-profile OFs cause reductions in policing effort due to increased community scrutiny of the police. As increased scrutiny drives up the marginal cost of officer effort, the model predicts that declines in effort should occur most prominently in arrest types that have low marginal benefit (MB) to the officer, primarily corresponding to offenses that have relatively low social cost, such as drug possession.

Following a high-profile OF, officers do indeed curtail arrests for less serious offenses that generate lower marginal benefits. I find that arrests are reduced by about 1–4% for all Part I offenses (murder, aggravated assault, robbery, motor vehicle theft, burglary, theft). This result is driven by theft arrests, which fall by 7%. Reductions in arrests are more marked

[1] In this paper, clearing an offense, or clearances, is synonymous with an arrest, and they are used interchangeably for the remainder of the paper.

among crime types that have lower social costs. Using Part II offenses, a broader set that contain so-called quality of life crimes, I create an index of low MB arrests (e.g., disorderly conduct, liquor law violation, and marijuana possession). These low MB arrests exhibit a sharper decrease of up to 23%, exemplified most prominently by arrests for marijuana possession, which decline by up to 33%. These arrests are the easiest to reduce, as they usually are officer-initiated and officers have the most discretion in determining whether to intervene. Notably, there are no reductions in higher-return arrests, such as those for violent crime or more serious property crime. These findings are consistent with the model's prediction for scrutiny being the causal channel for the reduction in arrests. An empirical analysis of Laquan McDonald's death, a high-profile OF that has separation in the timing of the incident and community awareness, bolsters that story. While the decline in effort for theft is temporary, it persists for the least serious offenses, representing a sustained transition to a lower equilibrium effort. Reductions in arrests occur for both Black and white suspects, but reductions for Black suspects are suggestively larger in theft and marijuana possession, although racial differences are not statistically significant in the latter case.

Despite arrest reductions exclusively occurring in less serious offenses, I also find substantial increases in serious offending. Most notably, there is a significant rise of 10–17% in murders and robberies. There are also smaller increases of 3–7% in theft and motor vehicle theft. Because community awareness of the incident and reductions in officer effort are usually simultaneous, it is difficult to identify whether the cause of the offending response is from a signal of lower apprehension risk or a reaction to the high-profile fatality itself. Regardless, the additional crimes, especially the rise in murder, cause a tremendous loss of welfare.

Building on past studies, this paper most closely relates to a literature that analyzes the determinants of police officer behavior.[2,3] Mas (2006) illustrates how arrest rates and average sentence length declines, and crime reports rise after police unions in New Jersey lose in arbitration for wage negotiations. Chandrasekher (2016) uses evidence from a union work slowdown in New York to find that officers drastically reduced the number of ticket citations but maintained arrests levels for all serious crimes. Heaton (2010) examines the impact of a policy change following a racial profiling incident in New Jersey, which led to sizable reductions in motor vehicle theft arrests for minorities, with partially corresponding increases in auto thefts afterward. Finally, Shi (2009) follows the effects of civil unrest in Cincinnati after a white officer killed a young, unarmed Black man, and finds that there is a

[2]Officer behavior changing as a result of a policy change or high-profile event: Prendergast (2001); Levitt (2002); Evans and Owens (2007); Klick and Tabarrok (2005); McCrary (2007); DeAngelo and Hansen (2014); Morgan and Pally (2016); Krishnan, McCrary, and Premkumar (2017); Shjarback et al. (2017); Cheng and Long (2018); Long (2019); Mello (2019); Rivera and Ba (2019); Rosenfeld and Wallman (2019); Devi and Fryer (2020)

[3]Discrimination/disparities in policing: Donahue and Levitt (2001); Knowles, Persico, and Todd (2001); Persico (2002); Anwar and Fang (2006); Legewie (2016); Legewie and Fagan (2016); Goncalves and Mello (2017); Manski and Nagin (2017); Fryer (2019); Rozema and Schanzenbach (2019); Ang (2020); Fryer (2020); Heckman and Durlauf (2020)

sharp decline in arrests, particularly for less serious crimes and violations. Previous studies have provided evidence from case studies or singular events, but this project provides the first national analysis of police effort and offending, estimating the effects from 72 high-profile, officer-involved fatalities and 2,740 police departments. To the extent that public scrutiny after high-profile OFs can be separated from the incident itself, this paper also highlights a channel of how communities initiate change in bureaucratic behavior when they do not have a direct mechanism to signal or translate their preferences. The particular context of policing is especially timely and has sizable welfare consequences given the significant cost of crime (Chalfin and McCrary, 2018).

The remainder of the paper is organized as follows: Section 2 provides the background on the Ferguson Effect. Section 3 presents a stylized model of the police officer's objective function and predictions for empirical analysis. Section 4 discusses the numerous datasets in the analysis, how they are cleaned, and why each one is chosen. Section 5 details the empirical specification used, and also highlights the results, showing how policing and offending behavior changes after a high-profile OF. Section 6 discusses the results in the broader context of the model, and works to explore the mechanisms that may be driving the findings. Finally, Section 7 concludes.

2 Background on the Ferguson Effect

Over the past six years, there has been a renewed focus on policing issues following intense collective action from numerous organizations, highlighting the discrepancy between community preferences and the allocation of policing services. From thousands of Black Lives Matter protests across the nation (Elephrame, 2018) to a recently passed California bill that limits officer use of force (Gardiner, 2019), much of that attention is often traced back to a single event: an infamous officer-involved fatality in Ferguson, Missouri.

In August 2014, a white Ferguson police officer fatally shot an unarmed Black teen, Michael Brown. Tension from elements of the event compounded upon racial anxieties and community mistrust, instigating protests in Ferguson, as well as national coverage of the officer-involved fatality (Swaine, 2014). The subsequent coverage brought attention to racial inequalities in policing across the country. In response to the rise in scrutiny of certain law enforcement practices, then FBI Director James Comey commented that the increase in violent crime in 2015 was a result of decreased officer activity, largely due to heightened attention and criticism from their local communities: *"I don't know whether that explains it entirely, but I do have a strong sense that some part of the explanation is a chill wind that has blown through American law enforcement over the last year"* (Comey, 2015).

What he was referring to as the *chill wind* became known as "the Ferguson Effect" to law enforcement officers and crime-focused social scientists. These speculations on the Ferguson

Effect have found evidence in anecdotes from law enforcement officers, such as a Chicago PD officer who was being assaulted on the job, but did not draw her firearm because of fear of media backlash (Hawkins, 2016). A more general consequence of the Ferguson Effect could be a reduction in the amount of discretionary police activity, where previously, officers may have exited their patrol cars to illustrate their presence in communities, question people they deem suspicious, and arrest those whom they deem are causing crime or disorder.

Though the effect is named for an incident in 2014, Ferguson was far from the first time where an officer-involved fatality generated community consternation toward the local police department. Before Michael Brown died, Eric Garner died on video from a police officer performing an illegal chokehold maneuver in New York in 2014. In an Oakland metro station, Oscar Grant also died on video in 2009 after an officer shot Grant in the back as he laid face down. These events carry a common thread of intense protests and continuous news coverage. The impetus for the unrest and attention is likely related to the perceived unjust nature of the fatality. It also relates to the discrepancy between community preferences for policing services and current practices. However, whether the change in scrutiny has shaped policing behavior, and to what extent, is still an empirical question. The phenomenon has been studied before with various empirical strategies (Morgan and Pally, 2016; Shjarback et al., 2017; Rosenfeld and Wallman, 2019), but the broader implications are harder to take away, because of limitations from study design. Cheng and Long (2018), and Rivera and Ba (2019) most closely resemble the analysis in this study, providing both a national and a city-specific analysis of the Ferguson Effect (St. Louis and Chicago, respectively).

Cheng and Long (2018) estimate the spillover effects of the death of Michael Brown on arrests and crime in large US cities. Cheng and Long's national difference-in-differences analysis defines treated jurisdictions as eight predominantly Black cities (out of 47 large cities in the sample). Similar to the results I present in this paper, Cheng and Long find larger decreases for misdemeanors than felonies, as well as an increase in murders. This paper complements work from Cheng and Long (2018) by directly estimating how local policing behavior and crime change after a high-profile, officer-involved fatality occurs in that jurisdiction, using a broader set of police departments and OFs in the empirical analysis.

Rivera and Ba (2019) test the effects of internal self-monitoring versus public monitoring of policing arising from the death of Laquan McDonald. After internal reforms without simultaneity of public scrutiny, they find that officers receive less civilian complaints without changes in crime and arrests. Conversely, after McDonald's death, crime and civilian complaints increase without commensurate increases in arrests. Like Rivera and Ba (2019), this study recognizes the potential limitations of previous uses of large scandals as exogenous shocks without grappling with the simultaneous effects on the community and police officers. This paper allays that concern by providing a novel theoretical model of officer behavior, which provides empirical predictions of the effects by offense type for each mechanism. The resulting arrest pattern after an OF align with scrutiny as the channel. Finally, an empirical analysis of the death of Laquan McDonald (discussed in Section 6)—where there is a year gap between the incident and broad community awareness—makes clear that police behavior is largely unchanged until public scrutiny occurs.

If the Ferguson Effect exists, it should locally manifest in jurisdictions that experience high-profile OFs, necessitating the inclusion of more comprehensive treatment data from across the nation and an empirical specification that internalizes the time-varying aspect of the scrutiny. This paper combines data from 2,740 police departments, 52 of which are treated, between 2005–2016 to study the impact of high-profile OFs on police behavior and crime.

3 Theory and Stylized Model

After a high-profile OF, it is plausible that there are (1) increases in scrutiny of police, (2) reductions in community cooperation in identifying and locating suspects (Sunshine and Tyler, 2003), (3) reductions in civilian reporting of crimes (Desmond, Papachristos, and Kirk, 2016)[4], and (4) changes in criminal behavior—both in response to the OF and to signals of policing effort (Becker, 1968; Cloninger, 1991; Persico, 2002; Kirk and Papachristos, 2011; Rosenfeld and Wallman, 2019). These channels may affect the number of arrests for the involved police department. To assess whether increased scrutiny from high-profile, officer-involved fatalities lead to reductions in policing effort, I develop a theoretical model of the officer's objective function to provide a set of empirical predictions to identify the mechanisms occurring given a set of observed changes in arresting patterns.

The officer's objective is to maximize utility by determining the amount of effort (e) to exert. The level of effort differs by type of crime. For simplification, let there be two types of crime, $c = \{L, H\}$, which represent offenses with low and high social cost of crime victimization respectively. Officers earn a baseline salary (w) and have a reward function (f_c) that monotonically increases in the number of arrests (A_c). There are three general parameters that enter into the utility function parameters: a cost of effort multiplier (ϕ), community cooperation (F), and reported crime (RC_c).[5] The objective of the officer is to

[4]A recent replication of Desmond, Papachristos, and Kirk (2016) illustrated that the main findings were not statistically significant after excluding an outlier (Zoorob, 2020). The original authors re-ran the analysis without the outlier—now controlling for temperature—and arguably find that there is still a decline in 911 calls (Desmond, Papachristos, and Kirk, 2020). Regardless, reductions in civilian reporting after an officer-involved fatality are still possible.

[5]I consider individual officers as small, homogeneous agents. This reference point is consistent with a principal-agent framework, where the community (principal) does not have full information on the amount of effort the officers (agents) use. It also aligns with my informal interviews with and direct observations of police officers, which highlighted the large amount of discretion officers employ when enforcing the law, and the occasional disconnect between the stated policies of the police department and what is being executed on the ground. If the focus was on a change in policy, rather than officer behavior in response to scrutiny, I could reconstitute the model at the department level with the main comparative static results in tact, assuming that

maximize utility:

$$\max_{e_L,e_H} U(e_L, e_H; \phi, F, RC_L, RC_H) = w + f_L(A_L) - \frac{1}{2}\phi e_L^2 + f_H(A_H) - \frac{1}{2}\phi e_H^2$$
$$= w + f_L(A_L(e_L; F; RC_L)) - \frac{1}{2}\phi e_L^2 +$$
$$f_H(A_H(e_H; F, RC_H)) - \frac{1}{2}\phi e_H^2$$

To reward effort in policing, many police departments determine promotions based on arrests, either "a high volume of them or a string of high-quality ones" (Samaha, 2017). More arrests (A) increase the likelihood of promotion and ancillary payments, since officers are compensated by the number of hours worked and overtime pay for court appearances related to their arrests. The officer's marginal benefit of arrest is generally proportional to the social cost of crime ($\frac{df_H}{dA_H} > \frac{df_L}{dA_L}$). Naturally, when mapping to the real world, there is more heterogeneity in the social cost of crime, with Table 1 detailing the specific costs by offense.[6] Violent crimes, such as murder and aggravated assault, are tremendously socially costly and constitute high marginal benefit arrests (H). Property crimes, however, vary by offense type, where motor vehicle theft is quite costly and may be a high marginal benefit arrest, but theft—which has an order of a magnitude less cost—may be closer to a low marginal benefit arrest (L). The least serious offenses—such as marijuana possession or disorderly conduct—have no estimate because they do not have clear and distinct victims, making the cost relative to other crimes negligible. Thus, these offenses are low marginal benefit arrests.

Arrests are a concave function of effort ($\frac{\partial^2 A_c}{\partial e_c^2} < 0$), while the number of reported crimes is a combination of on-view reporting—crimes spotted by officers when patrolling—and crimes reported by the community. Officers also face convex costs related to effort, which is parameterized by the function $\frac{1}{2}\phi e_c^2$.

Taking the derivative of the utility function with respect to e_c (i.e., effort for either low or high social cost crimes), the first-order condition is:

$$\frac{\partial U}{\partial e_c} = \frac{df_c(A_c(e_c; F, RC_c))}{dA_c} \cdot \frac{\partial A_c}{\partial e_c} - \phi e = 0$$

departments take (past) crime as given and the objection function is crime minimization.

In the model, city-level crime is not directly responsive to the individual effort of a single officer, but rather the aggregate effort at the department level. Although individual officers are not jointly determining their effort with the resulting amount of crime in the model, they are incentivized to be proactive in their policing through the reward function (f_c), where officers receive employment, monetary, and/or utility returns based on the arrest type they clear, consistent with descriptions of police overtime and promotions (Samaha, 2017; Hughes, 2020; Mastrorocco and Ornaghi, 2020). Officers take reported crime, not the true level of crime, as given because they are unable to police beyond the offenses known.

[6] Table 1 uses the cost of crime estimates from Chalfin and McCrary (2018). Reflecting the broader literature, these estimates only account for the direct victimization costs—rather than the broader costs imposed on a community—and consequently, they are an undercount.

Thus, the equilibrium effort ($e^\star$) for an officer satisfies

$$\implies e^\star = \frac{1}{\phi}\left[\frac{df_c}{dA_c}\cdot\frac{\partial A_c}{\partial e_c}\right] \tag{1}$$

where equilibrium effort for a specific offense is determined by the marginal benefit of arrest to the officer, marginal arrest generated for an additional work hour of effort, and the cost of effort.[7]

The Ferguson Effect—or reduced officer effort as a result of increased scrutiny from a high-profile, officer-involved fatality—manifests itself through exogenous increases in the cost of effort multiplier, ϕ. Intensified scrutiny from media coverage, community attention, and protests may result in additional psychic costs, as officers experience disutility from protests and interactions with a community that may be apprehensive and critical of them. Additionally, officers may perceive higher costs of mistakes and misconduct, as well as police departments more broadly, potentially shaping their behavior (or policing strategies) to serve a more vigilant community where civilians are recording officers as they make stops and more journalists are attentive to policing issues.[8] This channel is evinced by anecdotes such as the Chicago PD officer who did not draw her firearm while being assaulted because of fear of media coverage (Hawkins, 2016).

Thus, the comparative static of interest is $\frac{\partial e^\star}{\partial \phi}$: how does the rise in the marginal cost of effort—from intensified scrutiny—affect equilibrium effort? Let $e^\star$ be the choice of effort

[7]Aside from the intensive margin of effort within a shift, higher effort exertion may be represented by taking on longer shifts or doing overnight shifts.

[8]In other police officer objective function models, such as Shi (2009), the ϕ parameter most closely resembles increases in expected oversight costs from the likelihood of a complaint being filed, the officer being found guilty, and the penalty imposed as a result. However, the expected monetary or employment penalty is likely to be low, which is why there is no explicit civil, criminal, or administrative punishment mechanism built into this model (Schwartz, 2014; Rushin, 2017; Rushin, 2019; Grunwald and Rappaport, 2020). In the collective bargaining agreements negotiated between police unions and many US cities, there often are clauses in the contract requiring the city to indemnify police officers (Schwartz, 2014; Schanzenbach, 2015). Schwartz (2014) found that "governments paid approximately 99.98% of the dollars that plaintiffs recovered in lawsuits alleging civil rights violations by law enforcement. Law enforcement officers in my study never satisfied a punitive damages award entered against them[...]." These lawsuits include wrongful death claims, which usually range into the millions of dollars. These cases are common in these high-profile OFs, usually resulting in the family settling with the city. Even in cases where the police department admits wrongdoing, criminal charges for officers are rare, and very few result in convictions. This New York Times article provides a breakdown of wrongful death settlement amounts and police officer accountability by incident.

Outside their initial probationary period when they first join the force, firing or demoting patrol officers is quite difficult, since they usually are protected through union contracts (Schanzenbach, 2015; Rushin, 2017; Rushin, 2019; Hughes, 2020). Officers who are fired often end up re-hired in nearby jurisdictions, as complaint and disciplinary records of officers are often kept hidden, including from prosecutors, public defenders, and even other police departments (Friedersdorf, 2015; Bies, 2017; Kelly, Lowery, and Rich, 2017; Grunwald and Rappaport, 2020). Schanzenbach (2015) highlights that "the expense and low success rate deter cities from pursuing misconduct." Moreover, legal protections and officer contracts unequivocally encourage proactive policing and high arrest rates, whether through qualified immunity, indemnification, overtime compensation, or future career prospects. The theoretical underpinning for these protections is to shift the risk- and cost-burden from the officer to the city. Consequently, the protection from legal ramifications and costs allow a rational officer to police proactively, thereby reducing crime.

that maximizes utility for the given community cooperation, reported crime, and scrutiny parameters. By the envelope and implicit function theorems,

$$\frac{\partial e_c^\star}{\partial \phi} = -\frac{\dfrac{\partial^2 U}{\partial e_c \partial \phi}}{\dfrac{\partial^2 U}{\partial e_c^2}}$$

Because the scrutiny parameter only affects the cost function, the mixed partial derivative with respect to ϕ is

$$\frac{\partial^2 U}{\partial e_c \partial \phi} = -e_c \implies -\frac{\partial^2 U}{\partial e_c \partial \phi} = e_c > 0$$

and the second-order derivative with respect to e_c is

$$\frac{\partial^2 U}{\partial e_c^2} = \underbrace{\frac{d^2 f_c}{dA_c^2}}_{=0} \cdot \underbrace{\left[\frac{\partial A_c}{\partial e_c}\right]^2}_{>0} + \underbrace{\frac{df_c}{dA_c}}_{>0} \cdot \underbrace{\frac{\partial^2 A_c}{\partial e_c^2}}_{<0} - \underbrace{\phi}_{>0} < 0$$

confirming that the utility function is concave. $\dfrac{d^2 f_c}{dA_c^2} = 0$ because the marginal reward for each arrest is set proportional to the social cost and does not diminish.[9]

Consequently,

$$\frac{\partial e_c^\star}{\partial \phi} = \frac{e_c}{\dfrac{d^2 f_c}{dA_c^2} \cdot \left[\dfrac{\partial A_c}{\partial e_c}\right]^2 + \dfrac{df_c}{dA_c} \cdot \dfrac{\partial^2 A_c}{\partial e_c^2} - \phi} = \frac{e_c}{\dfrac{df_c}{dA_c} \cdot \dfrac{\partial^2 A_c}{\partial e_c^2} - \phi} < 0$$

Despite there not being a direct measure of officer effort, because arrests are (weakly) monotonically increasing in effort, decreased effort can be exhibited by reductions in arrests holding all else constant. Explicitly,

$$\frac{\partial A_c^\star}{\partial \phi} = \frac{\partial A_c^\star}{\partial e} \cdot \frac{\partial e_c^\star}{\partial \phi} = \frac{\partial A_c^\star}{\partial e} \cdot \frac{e_c}{\dfrac{df_c}{dA_c} \cdot \dfrac{\partial^2 A_c}{\partial e_c^2} - \phi} < 0 \tag{2}$$

Additionally, because $\dfrac{\partial A_L^\star}{\partial e_L} > \dfrac{\partial A_H^\star}{\partial e_H}$ and $\dfrac{df_H}{dA_H} \gg \dfrac{df_L}{dA_L}$, where differences between the highest and lowest social cost crimes are several orders of magnitude, then

$$\left|\frac{\partial A_L^\star}{\partial \phi}\right| > \left|\frac{\partial A_H^\star}{\partial \phi}\right| \tag{3}$$

Therefore, an exogenous increase in scrutiny, which increases the MC of effort, reduces officer

[9]If I relax this assumption, allowing $\dfrac{d^2 f_c}{dA_c^2} < 0$, then $\dfrac{\partial^2 U}{\partial e_c^2}$ would still be less than zero.

effort and arrests in equilibrium. Further, model predicts that this decrease in arrests will primarily occur for offenses that produce lower marginal benefit to the officer (i.e., generally low social cost crimes). I explore this mapping empirically in Section 5 when testing for reductions in arrests after controlling for population, and a number of other department- and municipality-level controls.

However, in addition to policing behavior, a high-profile OF may affect community cooperation (Sunshine and Tyler, 2003), which has its own impact on arrests. First, I need to determine the sign of the second-order mixed partial derivative:

$$\frac{\partial^2 U}{\partial e_c \partial F} = \underbrace{\frac{d^2 f_c}{dA_c^2}}_{=0} \cdot \underbrace{\frac{\partial A_c}{\partial F}}_{>0} \cdot \underbrace{\frac{\partial A_c}{\partial e_c}}_{>0} + \underbrace{\frac{df_c}{dA_c}}_{>0} \cdot \underbrace{\frac{\partial^2 A_c}{\partial e_c \partial F}}_{>0} > 0$$

Thus, the sign of the comparative static is

$$\frac{\partial A_c^\star}{\partial F} = \frac{\partial A_c^\star}{\partial e} \cdot \frac{-\dfrac{\partial^2 U}{\partial e_c \partial F}}{\dfrac{\partial^2 U}{\partial e_c^2}} = \frac{\partial A_c}{\partial e_c} \cdot \frac{-\dfrac{df_c}{dA_c} \cdot \dfrac{\partial^2 A_c}{\partial e_c \partial F}}{\dfrac{df_c}{dA_c} \cdot \dfrac{\partial^2 A_c}{\partial e_c^2} - \phi} = \frac{-\dfrac{\partial A_c}{\partial e_c} \cdot \dfrac{\partial^2 A_c}{\partial e_c \partial F}}{\dfrac{\partial^2 A_c}{\partial e_c^2} - \phi \Big/ \dfrac{df_c}{dA_c}} > 0 \tag{4}$$

suggesting that decreases in community cooperation from a high-profile OF result in reductions in equilibrium arrests. Increased scrutiny and reduced community cooperation are separate mechanisms that both result in declines in arrests, and therefore the latter could confound the Ferguson Effect estimate. However, for the least serious offenses (L), such as marijuana possession or disorderly conduct, the arrests are largely on-view, meaning that those clearances require the little to no community cooperation (i.e., the officer does not have a previous incident report or a warrant, but rather detains the suspect based on something they witnessed), whereas the opposite is true for more serious offenses (e.g., murder)[10]. Because $\frac{\partial^2 A_H}{\partial e_H \partial F} \gg \frac{\partial^2 A_L}{\partial e_L \partial F} \approx 0$, then

$$\frac{\partial A_H^\star}{\partial F} > \frac{\partial A_L^\star}{\partial F} \approx 0 \tag{5}$$

Therefore, if empirical findings show reductions in arrests and they are concentrated in more serious offenses (H), then the reductions are a result of a decline in community cooperation after a high-profile OF. Conversely, if the reductions are primarily in low marginal benefit arrests (L), then the resulting mechanism is a decline in officer effort from additional scrutiny.

Finally, since arrests are based on police officers' knowledge of reported crime, it is im-

[10]In fact, in California, police officers are currently not allowed to arrest a suspect for a misdemeanor crime, unless they have probably cause to believe they witnessed the crime in their presence ("on-view") or they have a warrant. Here is the relevant legal section.

portant to understand how RC_c is affected following a high-profile OF. Offending, one part of RC_c, could decrease based on a perceived rise in the cost of engaging in crime (Cloninger, 1991), or it could increase due to diminishing apprehension risk (Becker, 1968) or reactions to the high-profile fatality itself, where diminished perceptions of police legitimacy and procedural justice lead to declines in legal compliance (Persico, 2002; Kirk and Papachristos, 2011; Rosenfeld and Wallman, 2019). Similarly, civilian reporting of crime, the other component of RC_c, may change after a fatality (Desmond, Papachristos, and Kirk, 2016). In order to properly assess the existence of the Ferguson Effect, I am interested in changes in ϕ holding all else constant. Thus, in the Discussion section, I explore the relevant mechanisms by directly controlling for changes in reported crime, closing an intermediary channel where arrests may fluctuate from changes in offending or reporting. Thus, I focus on the two remaining channels: scrutiny (Equation 3) and community cooperation (Equation 5).

Nevertheless, to understand how changes in reported crime after a high-profile OF may affect police effort and arrests, I provide the comparative static analysis below.

$$\frac{\partial^2 U}{\partial e_c \partial RC_c} = \underbrace{\frac{d^2 f_c}{dA_c^2}}_{=0} \cdot \underbrace{\frac{\partial A_c}{\partial RC_c}}_{>0} \cdot \underbrace{\frac{\partial A_c}{\partial e_c}}_{>0} + \underbrace{\frac{df_c}{dA_c}}_{>0} \cdot \underbrace{\frac{\partial^2 A_c}{\partial e_c \partial RC_c}}_{>0} > 0$$

The sign of the comparative static is

$$\frac{\partial A_c^\star}{\partial RC_c} = \frac{\partial A_c^\star}{\partial e} \cdot \frac{-\dfrac{\partial^2 U}{\partial e_c \partial RC_c}}{\dfrac{\partial^2 U}{\partial e_c^2}} = \frac{\partial A_c}{\partial e_c} \cdot \frac{-\dfrac{df_c}{dA_c} \cdot \dfrac{\partial^2 A_c}{\partial e_c \partial RC_c}}{\dfrac{df_c}{dA_c} \cdot \dfrac{\partial^2 A_c}{\partial e_c^2} - \phi} = \frac{-\dfrac{\partial A_c}{\partial e_c} \cdot \dfrac{\partial^2 A_c}{\partial e_c \partial RC_c}}{\dfrac{\partial^2 A_c}{\partial e_c^2} - \phi \Big/ \dfrac{df_c}{dA_c}} > 0 \tag{6}$$

Expectantly, as officers are aware of more crimes, the equilibrium level of arrests increases—or vice versa. But, that maxim does not strictly hold for the least serious offenses (L), where $\frac{\partial^2 A_H}{\partial e_H \partial RC_H} \gg \frac{\partial^2 A_L}{\partial e_L \partial RC_L} \approx 0$. Given the significant number of L offenses that go without arrest (e.g., marijuana possession), an increase in them is unlikely to change the marginal arrest of effort, especially compared to higher social cost crimes, where police services may be reallocated to address the additional cost burden.[11] As a result,

$$\frac{\partial A_H^\star}{\partial RC_H} > \frac{\partial A_L^\star}{\partial RC_L} \approx 0 \tag{7}$$

[11] In fact, Section 4, I discuss how many L offenses are not even recorded by the FBI, unless they result in an arrest, since reports would be substantially undercounted.

4 Data Description

4.1 Arrest and Crime Data

The study combines government data on arrests, reported crime, and demographic information with crowd-sourced data on officer-involved fatalities. The backbone of this study uses large administrative datasets from FBI's Uniform Crime Report, which contain data from nearly every police department in the US. The data contains monthly arrests for Part I offenses (murder, aggravated assault, robbery, motor vehicle theft, burglary, theft) and known offenses (i.e., crimes reported by civilians and/or observed by the police).[12,13]

Separately, it details Part II arrests, a broader offense classification set than Part I—notably containing the least costly crimes, such as disorderly conduct or marijuana possession.[14] I create an index of low marginal benefit (MB) arrests—Part II clearances that produce little return to the officer in terms of promotion or compensation, and generally have the lowest social costs of crime victimization: curfew/loitering, disorderly conduct, public drunkenness, Driving Under the Influence (DUI), liquor violations, marijuana possession, marijuana sale, prostitution, suspicion, vagrancy, and vandalism.[15] DUI offenses have the potential for substantially higher social costs (DeAngelo and Hansen, 2014), but the arrests are included in the low MB index because officers have anecdotally indicated that there is less priority placed on their enforcement relative to other offenses (Murgado, 2012).[16] By compiling certain Part II offenses into an index, I reduce the false discovery rate—the likelihood of finding falsely significant, spurious effects (Anderson, 2008). There also may be larger effects on these arrests because officers have more discretion in determining whether to make an arrest.

The arrest data is reported separately by race of suspect. I focus on the arrests of Black and white suspects, and the results are run at the agency-month-race level. In order to ensure that the data maps accurately to the jurisdictions, I clean it using rules described in Section A.2, following Evans and Owens (2007) and Mello (2019).

For this study, I use monthly arrest and crime counts to analyze policing effort and offending behavior in the aftermath of 72 high-profile OFs in 52 of 2,740 police departments

[12] The exact rules for how they record crime and arrest data are further explained in Section A.1. It is important to highlight that theft and motor vehicle theft are mutually exclusive categories. Murder encompasses both murder and non-negligent manslaughter.

[13] For this study, I exclude rape, since the FBI's definition of rape was modified in 2013. Subsequently, changes in rape offense or arrest around that time could not be separated from the change in definition. I also do not include arson because it's uncommon and provided on a different supplemental dataset.

[14] Part II offenses usually have no distinct victims, and subsequently, they are far less likely to be reported by the community. Thus, the FBI only reports them if they result in arrest.

[15] A weapon arrest is a violation of a local law or ordinance, "prohibiting the manufacture, sale, purchase, transportation, possession, concealment, or use of firearms" (FBI, 2004). Suspicion arrests are detentions that are not tied to any specific offense and the offender is released without formal charges being placed.

[16] The results discussed later in the paper are qualitatively similar if DUI is excluded from the low marginal benefit index.

in the analysis sample between 2005–2016. Table 2 reports that treated jurisdictions have on average lower arrest rates for whites than control jurisdictions, as well as higher rates for Blacks across all types of crime: violent, property, and the low marginal benefit arrests. Across race, the arrest rates for Blacks are substantially higher than whites for all crime types. Typically, treated departments are policing larger and more populous cities with an average population of nearly 500,000 compared to nearly 150,000. These treated cities have about 11 percentage points more Blacks and fewer whites, while having similar educational attainment and slightly higher poverty rates, driven by Black poverty.[17]

4.2 Officer-Involved Fatality Data

The official government estimates for officer-involved fatalities, in the FBI Supplementary Homicide Report within the category of "Justifiable Homicides," are extremely undercounted, resulting in non-governmental estimates having up to three times the number of fatalities (Krishnan, McCrary, and Premkumar, 2017). The most comprehensive dataset is from Fatal Encounters (FE). FE has managed to create a sophisticated collection system: collating data from Freedom of Information Act requests, web-scraping of news sources, and using paid researchers to run additional searches and data checks from public sources.[18] The dataset contains individual information on victim age, race, gender, location of death, cause of death, and a brief description of the incident. With over 1,100 incidents per year, officer-involved fatalities are quite common, comprising 7–9% of annual homicides.

For this study, I focus on the subset of fatal encounters that generate comparatively high amounts of media coverage, ones that could plausibly engender enough local scrutiny toward officers that they change behavior.[19] I create this high-profile subset by scraping the number of news articles written about each incident. In Section A.3, I describe the scraping procedure in more detail. Figure 1 provides a histogram of the distribution of news articles in bins of 500 articles.[20] The two obvious break points in the distribution are when fatalities generate more than 1,000 and 2,500 news articles. After manual inspection of each OF above 1,000 articles, many of them report on a community action after the death, often a protest, in line with the mechanism of increased scrutiny. While choosing the 2,500 threshold may lead to

[17] These summary statistics required an imperfect merge of the 2014 American Community Survey, and as a result, it uses only a subset of the observations used in the analysis.

[18] To find out more information, see here on the Fatal Encounters website.

[19] Most fatalities do not result in significant media coverage and subsequently produce limited community awareness and protests. Additionally, any empirical design that includes large US cities and leverages the timing of all officer-involved fatalities would be hampered by the frequency in which they occur (e.g., Los Angeles experiences more than 12 OFs every year in the sample frame). The effects shown in this paper will be identified off of departments and time periods that experience no OFs or low-profile ones.

[20] The bin from 0-500 articles is excluded since it contained over 99.9% of officer-involved fatalities, rendering the rest of the histogram indecipherable. Similarly, given the heavy right skew of the distribution, the histogram does not depict some of the most high-profile OFs. There are nine OFs that are covered by more than 20,000 news articles each, which are inputted into the farthest right bin.

larger treatment effects, there is reduced statistical power due to the small number of OFs.[21] Therefore, I define "high-profile" as having at least 1,000 news articles written about the incident.[22]

To see how the high-profile fatalities vary across space, Figure 2 provides a map of the US where city dot size corresponds to the number of events that occurred during the study frame. The geographical dispersion of the treated jurisdictions is ideal for conducting a national analysis. As expected, there are a few clusters in population centers like Chicago, New York, and Los Angeles. However, the salient takeaway is the numerous incidents in relatively non-populous localities, particularly in the southern region of the US. This finding suggests that incident-specific characteristics may explain how high profile an OF is, such as whether it is captured on video or seen by witnesses, rather than the number of news outlets—as demonstrated by the death of Walter Scott in North Charleston, one of the highest-profile OFs in the dataset.

The proliferation of cellphone cameras and the advent of social media has played a factor in how much attention fatalities receive (Lacoe and Stein, 2018; Battaglini, Morton, and Patacchini, 2020), with many of the fatalities toward the end of sample frame (Figure B.1). One concern is the advent of social media may have changed the locality of these incidents, dispersing public scrutiny to many jurisdictions. To assess this issue, I integrate new data on Black Lives Matter (BLM) protests from August 2014 to August 2015.[23] I find that there is distinctly more protest activity in treated police department municipalities than control municipalities, with an average of 4.74 protests versus 0.78 respectively (Table 2). There are sizable differences in attendance as well, with control jurisdictions having an average turnout of just over 200 people versus over 1,200 people in treated jurisdictions. I discuss this issue further in Section 5.4, illustrating through empirical tests that certain types of spillovers do not seem to be present. However, if there is a spillover effect, it would likely attenuate the results, resulting in them being a lower bound of the true effect.

Finally, I use data from Google Trends to modify the timing of each fatality to align with when community members started searching for the incident, when public scrutiny can plausibly begin. I describe the protocols used to determine whether to change the event time in Section A.4. This modification is necessary for OFs such as Laquan McDonald, who died in October 2014 but the Chicago community did not become fully aware until November 2015, when a video of the shooting was released. I discuss this specific case in more detail in

[21]In Section B.4, I show that the results are qualitatively robust to increasing the threshold levels of 2,500 and 5,000 articles.

[22]Because the focus is on high-profile fatal encounters, I am not concerned with missing data in FE. Conducting manual checks for victim names mentioned in prominent articles on fatal encounters with police, I confirm full reporting of these OFs.

[23]I use replication data from Trump, Williamson, and Einstein (2018), who source their information from the online platform Elephrame (2018). Elephrame has collated data on over 2,700 BLM protests across the country, from protests of over 50,000 to gatherings of fewer than ten, with related news article links, subjects of the protest, attendance estimates, and location.

Section 6. Overall, there are 10 events in the analysis that have timing changed.

The treatment sample is 72 high-profile OFs spread across 52 jurisdictions. The maximum number of OFs in a single jurisdiction is six (in Los Angeles). Table 3 illustrates that high-profile OFs typically involve young to middle-aged Black men who die from gunshot wounds. The victims are predominantly unarmed and not suffering from mental impairment, and thus plausibly consistent with a public perception of injustice, which could be reflected in the intensified scrutiny from additional media coverage of the incident.

5 Empirical Strategy and Results

In this section, I describe the empirical specifications and the corresponding results related to whether high-profile, officer-involved fatalities affect arresting and offending behavior. The identification strategy relies on the quasi-random timing of the OFs to induce a plausibly exogenous shock of increased scrutiny to the local police department. The empirical strategy leverages aspects of scrutiny directly by only including incidents that reach over 1,000 articles of media coverage and adjusting treatment time to when the local community first searches for the incident.[24] Thus, control jurisdictions include departments who experience no OFs or less publicized ones. Section 5.1 discusses difference-in-differences (DD) and triple difference (DDD) estimation using only the highest-profile, officer-involved fatality per jurisdiction. Then, Section 5.2 outlines why the "integrated" event study design is the preferred specification, providing the empirical model and the respective results. Both sections discuss heterogeneity in effects by offender race. Finally, Section 5.3 explores the impact of a high-profile, officer-involved fatality on crime outcomes, using the DD and event study strategies.

5.1 Difference-in-Differences and Triple Difference

5.1.1 Difference-in-Differences and Triple Difference Econometric Model

Using only the highest profile OF per police department, I estimate the effect on arrests, where the identifying variation is from the plausibly exogenous timing of fatalities. The DD estimating equation is

$$Y_{it}^{c} = \mu_i + \lambda_t + \rho(t)_i + \beta Pop_{iy} + \theta(Treat * Post_{it}) + \nu NegBin_{it} + \alpha PosBin_{it} + e_{it}^{c} \quad (8)$$

[24] A concern from creating a news article cutoff for treatment is that media coverage may be correlated to other time-varying characteristics that affect the timing of treatment, such as the population of a city or the time period of analysis, such as post-Ferguson (Figure B.1). The empirical analysis addresses the concern by including department and month-of-sample fixed effects, while controlling for population, and county-specific linear trends.

where Y_{it}^c represents log arrests of crime type c in department i in month t.[25] Crime type c consists of a variety of violent, property, and low marginal benefit offenses, as well as those respective crime categories. The coefficient of interest is θ on $Treat*Post_{it}$, which represents the approximate percent change (divided by 100) in the involved department's arrests in the 8 months before a fatality to 16 months after relative to control departments. To have θ measure a window of time just before and after the event, I include binned endpoints outside the event window, where $NegBin_{it}$ and $PosBin_{it}$ control for 8 months before and 16 months after an OF respectively (McCrary, 2007).[26]

I use department and month-of-sample fixed effects (μ_i, λ_t), and county-specific linear trends ($\rho(t)_i$).[27] Pop_{iy} controls for yearly log population. Standard errors are clustered at the police department level. I discuss the potential theoretical mechanisms that may induce changes in arrests after an OF, as well as their predicted empirical effect by offense type, in Section 3.

Table 3 provides descriptive evidence that Black OFs are higher profile with an average of over 20,500 news articles written about each incident compared to the next highest of white OFs at 3,000. It is possible that community scrutiny can operate along racial dimensions, increasing the marginal cost of effort for arresting Black suspects without significant changes for white suspects. The new outcome (Y_{it}^{cr}) is race-specific (r) log arrests, where $r \in \{Black, white\}$. To test for that, the DDD specification is quite similar to the DD model

$$\begin{aligned} Y_{it}^{cr} &= Black*\mu_i + Black*\lambda_t + Black*\rho(t)_i + \beta Pop_{iy} + \omega(Black*Treat*Post_{it}) \\ &\quad + \delta(Treat*Post_{it}) + \nu(Black*NegBin_{it}) + \alpha(Black*PosBin_{it}) + e_{it}^c \end{aligned} \tag{9}$$

with the interaction of Black arrests ($Black$) on the Treat*Post variable and the binned ends of the event window. Further, I interact the Black dummy with the police department and month-of-sample fixed effects, as well as the county-specific linear trends, allowing for the ability to control for race-specific unobservables and trends. For the DDD, the coefficient of interest is ω on the Black*Treat*Post variable, identifying racial differences in the Treat*Post coefficient.

[25] To not lose data where arrests and crime counts are zero, I transform the distribution to log(variable+1). Running the specification using the inverse hyperbolic sine transformation is nearly indistinguishable in terms of coefficients and standard errors. For simplicity, I focus only on the log+1 transformation.

[26] With heterogeneous event dates, balanced panel data becomes unbalanced in event time. Subsequently, each municipality would have a varying amount of leads and lags. Since I prefer θ to compare periods just before and after the event, introducing binned endpoints controls periods before and after the broader event window, while also limiting changes in estimates from sample composition.

[27] I employ county trends to control for the steady decline in arresting patterns during my sample frame. For example, certain municipalities gradually reduced how strictly they enforce certain drug laws. County-specific trends reflect the upstream effects prosecutors have by choosing what crimes to charge. If I use department-specific trends, the results are similar, but computationally more intensive.

5.1.2 DD and DDD Results

The impact on officer effort is measured by statistically significant drops in log arrests in the 16 months after the highest profile OF per jurisdiction. After accounting for other potential channels, reductions in low marginal benefit arrests (e.g., generally low social cost offenses) represent evidence of the effect of scrutiny on policing effort. Figure 3 presents the DD coefficients from separate regressions of individual arrest types in descending order of their respective social cost of crime. With the exception of motor vehicle theft arrests, none of these regressions have significant differences between the average pre-treatment trends of control and treated departments, providing a partial test of the identifying assumption: treated police departments would trend similarly to control police departments in the absence of the high-profile OF. I first focus on the changes in arrests of more serious crimes that cause significant social costs. After a high profile, officer-involved fatality, the involved police department experiences no change in violent crime arrests (murder, aggravated assault, robbery).[28] However, property crime arrests (motor vehicle theft, burglary, and theft) decrease by 5.1% (Table B.2), driven by a significant decline in theft arrests (7.4%)—the least socially costly property offense (Table 1).[29]

Shifting to the least serious offenses, I see much sharper reductions in arrests for marijuana sale (13.3%), disorderly conduct (14.0%), and marijuana possession (14.1%). These decreases help drive the overall decline in low marginal benefit arrests of 9.5%, relative to the eight months before an OF (Table B.3).[30] This effect pattern is broadly reflective of the reductions in policing effort occurring along less socially costly crimes, evinced by Figure 3.

In Table B.1, I present the DDD race results for violent crime arrests in Column (3), while Column (4) runs the test of racial parallel pretrends. The table highlights the lack of racial differences in the (lack of) change in violent crime arrests. On the other hand, Table B.2 highlights suggestive evidence that Black theft arrests fall by 6.0% relative to white theft arrests. The DDD empirical design does not uncover any significant racial differences for low MB clearances (Table B.3). Overall, the changes in arrests following an OF do not appear to vary significantly by race.

[28] The actual DD tables, such as Table B.1, are in the appendix. Column (1) provides the DD estimate, while Column (2) runs the test of parallel pretrends. The "Pretrend Test" variable is generated by splitting the pre-treatment period, the eight months before an OF, in half and testing for statistically significant differences in arrests between 5-8 months versus 1-4 month before an OF. If the pretrend test is significant, then the identifying assumption of the DD is not met.

[29] An important caveat is that property crime arrests experience statistically significant differences in pre-treatment trends between treated and control departments, driven from pre-treatment differential trends in motor vehicle theft arrests. I present the property crime arrest coefficient only for comparison with the respective event study coefficients, which are similar in size and do not suffer from differential pretrends.

[30] For more explanation on the low MB index, refer to Section 4.

5.2 Event Study Design

5.2.1 Event Study Econometric Model

Since the effects on arrests could vary over the event window, the DD estimates are not as informative as results from an event study. The canonical models of event studies, such as Jacobson, LaLonde, and Sullivan (1993) and McCrary (2007), replace the Treat*Post coefficient with event time dummies, where there is a dummy for each unit of time away from the event within a specified event window. For this study, the preferred event window is -3 to 5 quarters (-9 to 17 months) around the event with a time dummy for each quarter. The dummy coefficients are normalized to the period before the event.

However, unlike in McCrary (2007), the study involves observational units potentially having multiple events (e.g., Los Angeles Police Department, with six high-profile police fatalities), which complicates the design. There are a few proposed solutions to accommodate a multiple treatment design. One suggestion is narrowing the event sample to the first or largest treatment per observational unit, where the argument is that all other treatments afterward could be potentially correlated to that one. However, by using only the first treatment, the statistical power from additional events is lost—from 72 to 52 events—taking away the variation in timing of later treatments. Moreover, drops in arrests from future events would be incorrectly attributed to later event dummies.

The second solution comes from the finance literature and it is used more regularly in recent economics papers (Lafortune, Rothstein, and Schanzenbach, 2018). Each observational unit gets expanded by the number of events for that unit. For example, since I have data on Los Angeles from 2005–2016 and it has six events, I would expand the dataset to include six Los Angeles, each with their own unique event date. I would then cluster at the observational unit level, allowing for correlated shocks across expansion sets. Similar to the first solution, there are unaccounted treatments in the pre- or post-trend of each expanded observational unit. Both designs could result in biased estimates of pre- or post-trends (Sandler and Sandler, 2014).

To address limitations in previous designs, I implement an "integrated" approach, which adapts aspects of designs in McCrary (2007), and Sandler and Sandler (2014). Unlike in the "first event" or "expansion" designs, multiple time dummies can be turned on (e.g., if an observation is 1 quarter past an event in 2014 and 5 quarters past an event in 2013, then the +1 and +5 dummies should equal one). The preferred event window is -3 to 5 quarters (-9 to 17 months) around the event with a time dummy for each quarter. Because certain departments experience multiple OFs, the binned endpoints of the event window need to be the sum of pooled time dummies outside of the event window, taking on values up to six in the dataset for Los Angeles. Consequently, I define a flexible "dummy" setup of D_{it}^{j}[31]:

[31]For simplicity, the shown stepwise function is a representation of the month event time dummies, which I pool to create the quarter time dummies. Technically, the function is not a dummy function since it outputs

$$D_{it}^{j} = \begin{cases} \sum_{j=-J}^{a-1} \sum_{m=1}^{n_i} \mathbb{1}(t = \tau_{im} + j) \text{ for } j < a \\ \sum_{m=1}^{n_i} \mathbb{1}(t = \tau_{im} + j) \text{ for } a \leq j \leq b \\ \sum_{j=b+1}^{J} \sum_{m=1}^{n_i} \mathbb{1}(t = \tau_{im} + j) \text{ for } j > b \end{cases} \tag{10}$$

For the function above, τ_{im} is the date of the event m in jurisdiction i, a and b define the event window of choice, t is the month-of-sample, and $j \in \{-143, ..., 143\}$ represents the lead/lag counter, where J is the maximum value the lead/lag counter takes for the sample of 2005–2016. $\mathbb{1}$ is an event indicator function. If there is no event date (i.e., police department i did not experience a high-profile OF), then τ_{im} is missing and then the function always returns zero. Department i experiences n_i events.

Figure 4 shows a visual example of the dummy structure in months, using simulated data of a department with two officer-involved fatalities: one in January 2006 and the other in May 2006—the highlighted months. Thus, the event time dummy zero is turned on for those respective months (the highlighted cells). Since May 2006 is also four months after an OF occurred in the municipality, it needs have the fourth lag dummy turned on. However, since the event window is between -3 to 3 months around the event, all events outside three months get pooled together in a negative and positive bin (-4+ and 4+), so the positive bin is turned on for May 2006 (and each month afterward). Only the negative and positive bins can take on values greater than one. Since the bins only serve as controls for time periods outside the preferred event window, the coefficients of interest in the event window bear the same interpretation as standard designs. As shown in Figure 4, there are missing values for a few event time dummies, at the beginning of the simulated data frame, because of data limitations. For example, if I only have data from January 2005 to October 2006, I have no information on whether an OF happened in 2004, so the corresponding lags (event time dummies 1 to 3) need to be set to missing. Thus, the remaining data that the regression uses for its estimates is denoted by the green rectangle. In practice, I use a wider event window of -9 to 17 months around an event, and I pool those month dummies into quarter dummies (-3 to 5 quarters) to increase precision. The identifying assumption for the empirical strategy is the same as the DD: the timing of treatment is plausibly random, and absent experiencing a high-profile OF, control and treated departments would trend similarly.

To understand how police officers' arrests shift following an OF, the event study design can non-parametrically estimate changes in arrests after a high-profile OF by month, crime type, and race. Given parallel pre-treatment trends between treated and control departments, any change in arrests after an OF is reflective of the impact of the event on officer behavior.

numbers aside from zero and one.

Following the DD model, the preferred estimating equation is

$$Y_{it}^{c} = \mu_i + \lambda_t + \rho(t)_i + \beta Pop_{iy} + \sum_{j=a-1, j\neq -1}^{b+1} \theta_j D_{iq}^{j} + e_{it}^{c} \tag{11}$$

where the dummy structure (Equation 10) replaces the Treat*Post variable. The θ_j's are the coefficients of interest on event time dummies, D_{iq}^{j}, where I pool the month dummies into quarter dummies (q) to increase precision. The quarter before the event is dropped, and the coefficient estimates are normalized such that $\theta_{-1} = 0$. The other θ_j's represent the percent change in the outcome in the quarter before the event to j quarters after, relative to changes in the control jurisdictions, after controlling for yearly population, time-invariant city characteristics, county-specific linear trends, and temporal variation in arrests nationally. With all of the specifications, I cluster the standard errors at the department level.

To estimate changes in arrests by race, I create a separate dummy set for both Black and white arrests by interacting a Black and a white dummy with the original event time dummy set, using the same integrated approach detailed above.

$$\begin{aligned} Y_{it}^{cr} &= Black * \mu_i + Black * \lambda_t + Black * \rho(t)_i + \beta Pop_{iy} \\ &+ \sum_{j=a-1, j\neq -1}^{b+1} \chi_j * Black * D_{iq}^{j} + \sum_{j=a-1, j\neq -1}^{b+1} \gamma_j * white * D_{iq}^{j} + e_{it}^{c} \end{aligned} \tag{12}$$

Both sets of coefficients of interest, χ_j and γ_j, are identifying the percent change in race-specific arrests from their race-specific quarter before the high-profile OF to j quarters after.

5.2.2 Event Study Results

Unlike the DD estimates, the event study graphs integrate all of the high-profile OFs, including cases in which there are multiple OFs per police department, providing more events (72) and richer variation. Event study graphs have the coefficient on log arrests on the vertical axis and event time dummies as the horizontal axis, where period 0 is when a police department is involved in a high-profile OF. Figure 5 shows a slight suggestive reduction in all Part I arrests (violent and property crime plus simple assault and negligent manslaughter) that expands over the first few quarters in a downward arc shape, reaching the low point in Q3 and then returning to the initial equilibrium in Q5. The effects range from about 0% to -4.3%, which are being driven by reductions of 2–8% in property crime arrests. On the contrary, violent crime arrests exhibit minimal change, with the exception of a significant increase of 6.7% in Q3. There is a persistent and continual decline in low MB arrests, where the sharpest drop of 23% is about three times that of property crime arrests.

Consistent with the DD results, murder, aggravated assault, and burglary exhibit minimal change in arrests, aside from a suggestive uptick in assault arrests in Q3 and a suggestive decline in burglary arrests in Q1 (Figure 6). Theft arrests, again, show a significant decline of 3–11%, instigating the effects observed in property crime arrests. These changes are dwarfed by those of the low MB arrest categories (Figure 7).[32] Marijuana possession arrests experiences the most prominent change, falling by 15–33% in Q1–Q5.[33] Disorderly conduct arrests follow a similar pattern with reductions between 12–20% for Q1–Q5. These patterns are emblematic of significant reductions in other low MB arrests after Q0, such as liquor violations (20–33%) or marijuana sale arrests (9–20%). None of the aforementioned arrest categories demonstrate any statistically significant differences in the pre-treatment trends between control and treated departments.

Since race-specific arrests tend to move jointly, as evidenced by the DDD, an event study can be more informative in displaying the effect heterogeneity. For most offense types, there is not racial heterogeneity in arrests. However, Figure 8 shows that theft arrests suggestively fall more for Black offenders (4-16%) compared to white ones (1-7%). Similarly, Black arrests for marijuana possession suggestively drop more than white arrests (8-41% versus 3-25% respectively).[34] Although none of the individual coefficients were significantly different in either offense type, Black theft arrests are jointly different than white arrests, but that is not the case for marijuana possession.

5.3 Crime Analysis: Difference-in-Differences (DD) and Event Study

5.3.1 DD Results

I employ the same DD regression as Equation 8 with Y_{it}^{c} now representing log offenses of crime type c. Figure 9 presents the DD coefficients from separate regressions of individual arrest types in descending order of their respective social cost of crime. None of these regressions have significant differences between the average pre-treatment trends of control and treated departments. After a high profile, officer-involved fatality, the involved police department experiences a substantial increase in violent crime of 8.6% (Table B.4), driven by sharp increases in murders (14.4%) and robberies (12.5%). There are more moderate increases in property crime of 5.3% (Table B.5), largely from a significant rise in motor vehicle thefts (7.9%) and thefts (5.1%).

[32] In a few graphs, the vertical axis of the log point scale may be shifted to accommodate larger confidence intervals from larger effects.

[33] Concern about reductions in marijuana possession arrests being driven by a consistent downward trajectory during 2005–2016 should be assuaged by (1) the inclusion of county-specific linear trends, (2) the effect pattern being robust to changes in state laws, and (3) the trend not continuing if the event window is expanded (i.e., for that regression, Q-3 is higher than Q-4).

[34] However, white marijuana possession arrests are significantly different in control and treated departments three quarters before an OF, violating the identification assumption.

5.3.2 Event Study Results

Using the previous event study design, I employ Equation 11 with Y_{it}^{c} as log offenses, increasing the number of fatalities in the treatment sample from 52 to 72. Consistent with the DD findings, Figure 10 documents the significant and sizable increases of 10–17% in murders and robberies for Q0–Q4. The increases in murder taper off in Q5 after an OF, whereas robberies remain at elevated levels. There are also smaller increases of 3–7% in motor vehicle theft and theft (Figure 11). Unlike in the DD, the increases for motor vehicle theft and theft are too noisy to be statistically significant, but for theft and property crime in general, there seems to be consistent pattern of steady increases after an OF.

5.4 Spillover Analysis

One remaining concern is that the treatment exposure may not be specific to the involved department, potentially leading to spillover effects in other municipalities (Morgan and Pally, 2016; Cheng and Long, 2019). If these spillovers occur nationally, they are already being controlled for by the month-of-sample fixed effects. Additionally, the results are robust to spillovers that occur across jurisdictions with similar populations or within a state, since the size and significance of effects are largely unchanged after interacting the population group or state with the month-of-sample fixed effects.

The most likely scenario is that these high-profile OFs affect policing and crime in geographically-adjacent agencies. I directly test this in Section B.2, where I use the DD empirical model (Equation 8) but expand the geographic definition of treatment to the county, rather than just the involved police department. Overall, the average effects on arrests and crime in the treated county are heavily attenuated (Treat*Post in Column 1). Separating the effects between the involved department and the spillover departments that reside in the same county in Column 3, I find that the spillover departments (Spillover*Treat*Post + Treat*Post) experience reductions in arrests that are much more minor than the involved agency (Treat*Post). The involved department sees reductions in theft (8.7%), disorderly conduct (12.9%), and marijuana possession arrests (16.6%) that are similar to the main results, while the spillover agencies have reductions of 2–3%—none of which are significantly different than zero. This pattern is especially true for crime, where the spillover jurisdictions experience no change in offending behavior.

If spillovers exhibit some other idiosyncratic pattern that is not being controlled for or has not been examined, then the coefficients of interest would be attenuated, assuming the spillover effect has the same sign as the effect on the treated city, consistent with the geographic spillover analysis. In that case, the magnitude of the results would be a lower bound of the true effect. Overall, the findings largely substantiate the hypothesis that police departments that experience a high-profile OF instigate much greater scrutiny, with nearly two-thirds of them facing protests (Table 2). Consequently, there are sharp drops in policing effort for less serious offenses, evinced by reductions in low marginal benefit arrests of up to 23%.

6 Discussion

These results broadly show reductions in arrests for less serious offenses without changes in arrests for more serious ones. Conversely, there are large increases in violent crime and more moderate increases for property crime. To reconcile the arrest results with increases in reported crime, Section B.3 provides the arrest event study figures with controls for log reported crime, closing the intermediary channels of offending behavior or crime reporting.[35] The results with the crime controls demonstrate a qualitatively similar story as the previous event study findings. For example, because of the increase in murders after an OF, the murder arrest estimates are expectantly lower when controlling for crime than not; however, even after adjusting for changes in reported crime, none of the (now) reductions in murder arrests approach statistical significance. This case is broadly representative of a lack of significant reductions in more serious arrests, suggesting that changes in community cooperation are not driving the estimates. Given that reductions exclusively occur in less serious arrests, predominantly the low MB ones, the findings are consistent with the model's prediction for scrutiny being the causal mechanism (Equation 3). The model illustrates that the intensified scrutiny after a high-profile OF increases the marginal cost of effort for officers, potentially manifesting through increases in psychic costs or perceived costs of mistakes. As a result, they reduce effort, and consequently, arrests for offenses that have less monetary, personal, and/or professional return are diminished.

Given recent legalization of the possession and (regulated) sale of marijuana in a handful of states, these accentuated estimates are internally consistent with less reward to the officer because of less cost to society. Importantly, none of the results are not being driven by changes in state law, as the findings are unchanged after interacting the month-of-sample fixed effects with the state. Further, there are other low MB arrests—such liquor violations and disorderly conduct—that did not have major changes in law, yet still exhibited sizable drops after high-profile fatalities. These clearances are low social cost crimes, where officers

[35] An important caveat when interpreting these results is that crime is an endogenous control and may cause selection bias. Because the purpose of this empirical exercise is a mechanism analysis, I am less concerned about bias that limits causal interpretation. Specifically, I run Equation 11 with the added controls of log violent crime, property crime, and for Part I arrests, the specific offense. I use these covariates because I am interested in the effects of scrutiny on policing effort, which I measure through changes in arrests after controlling for other intermediary channels such as civilian crime reporting and offending behavior. Assuming arrests are (weakly) monotonically increasing with the amount of reported crime, it would still be a decline in policing effort if arrests are unchanged after rise in crime. I do not set Y_{it}^{c} to be the arrest rate, or the ratio of arrests to crime, because that presumes that the proportional increase in arrests from a rise in crime is equivalent across offenses. Judging from the differing estimates on the crime control in the regressions, that is not the case (e.g., an increase in murders does not result in the same increase in arrests as an increase in thefts).

face the lowest marginal benefit from an arrest. Naturally, these are also the cases where officers have the most discretion in determining whether to make an arrest and they usually are on-view (i.e., an officer observes the offense and hence does not require much community cooperation). The lack of community cooperation necessary for low MB arrests—where the effects are most concentrated—is instrumental in disentangling the mechanism as scrutiny.

Moderate reductions in theft arrests—the least costly of Part I crimes (Table 1)—are still consistent with the theoretical prediction that the declines occur for lower marginal benefit arrests. I find additional assurance in that there are no significant drops in burglary, robbery, or motor vehicle theft arrests (Figure B.2)—all of which have substantially higher social cost per crime. Moreover, there is no evidence of officers reducing arrests for the sale of heroine/cocaine and only suggestive declines in weapon violation arrests—two Part II offenses that have a higher return and more social cost. The results in Part II arrests, in concert with the findings on Part I arrests, are empirically consistent with the model's prediction of scrutiny being the relevant mechanism.

This notion is further bolstered after examining at the effect heterogeneity by media coverage by modifying the threshold of what fatalities are considered 'high profile.' Figure B.6 illustrates that the general pattern of effects is robust across thresholds: no reduction in violent crime arrests, moderate decreases in property crime arrests (driven by declines in theft arrests), and sharp drops in low MB arrests. As the threshold for high profile incidents increases from 1,000 articles of news coverage to 2,500 or 5,000, higher scrutiny incidents drive larger declines in property and low MB arrests, where reductions from the 5,000 article threshold are twice that of the 1,000 article threshold.

The declines in effort are present across white and Black arrests for the most part, suggesting that the scrutiny increases the marginal cost of effort for policing generally. But, among the categories where the steepest declines in effort are observed—theft for Part I, marijuana possession for Part II—the effects on arrest for Black offenders are suggestively larger than white offenders. The motivating anecdote of the Ferguson Effect may explain the race results: Officers may not want to make public, street-level arrests of Blacks, potentially substituting their time to patrolling in their vehicle. Racial divergence in arrests may be found by exploring this story more carefully with the additional incident-level data.

The focus on street-level clearances may also provide clarity in understanding the persistence of the decline in certain low MB arrests, such as marijuana possession or liquor violations (Figure 7). These arrests show no return to the original equilibrium effort for at least 1.5 years afterward, contrasting declines in arrests such as theft (Figure 6). The sustained transition to a lower equilibrium effort may be a result of officers enduring persistent increases in psychic costs or perceived-error costs in street-level interactions for these low MB clearances. When examining arrest types that involve less public interactions (e.g., vehicle stops, or interactions initiated by incident reports or warrants)—such as DUI (not shown), marijuana sale, or theft—the low point in officer effort is Q3 or Q4 before the effect abates toward the original equilibrium.[36] This abatement may partially represent a shift from pedestrian encounters to traffic stops, as was the case in Chicago after a high-profile OF, where Hausman and Kronick (2019) show that pedestrian stops declined by over 80% in late 2015 and early 2016 while traffic stops increased by 200%. That theory of substitution is also consistent with the lack of reduction in motor vehicle theft arrests and potentially even the suggestive declines in weapon violation arrests (Figure B.2).

To further substantiate the impact of scrutiny from media coverage, community attention, and protests, consider the case study of Laquan McDonald's death: In October 2014, he was fatally shot in Chicago, where a police incident report incorrectly described McDonald lunging at an officer with a knife. Given the misinformation, there was a subsequent lack of awareness and scrutiny, resulting in a marginal change in misdemeanor arrests (Figure 12). In February 2015, a journalist uncovered the official autopsy report, which raised questions since McDonald was shot 16 times—with some of bullets entering from his back (Kalven, 2015). He also revealed that there was footage of the incident that the Chicago Police Department was refusing to release. Misdemeanor arrests decline by 12% in the month afterward, but subsequently return to the original arrest level. In November 2015, the Chicago Police Department were forced by a court order to release the video, which directly contradicted the initial incident report. Consequently, low-level arrests sharply and persistently declined by 7–25%, coinciding with the spike in scrutiny—measured by local Google searches of the incident, protests, and over 71,000 news articles. The separability between the incident and community awareness, as well as the delayed reductions in low MB arrests, help negate other potential explanations such as a police response to the incident itself.

Recent papers have suggested that reductions in policing effort in municipalities such as Chicago are the result of oversight of ACLU monitoring or Department of Justice pattern-or-practice (PoP) investigations, rather than public scrutiny itself (Cassell and Fowles, 2018; Devi and Fryer, 2020). This is difficult to test because public scrutiny of policing—occasionally in response to high-profile OFs—often instigates PoP investigations. Of the 31 PoP investigations between 2005–2016, six were at least partially induced by high-profile, officer-involved fatalities, including by the two highest-profile incidents in the analysis—the deaths of Freddie Gray in Baltimore and Michael Brown in Ferguson.[37] After removing the departments that experienced PoP investigations as a result of high-profile OFs, the main results are remarkably similar. Moreover, as a consequence of the public outcry following the video release of

[36]The low point in these arrests at Q3 or Q4 may align with latent scrutiny coming from the court proceedings of the involved officer(s) or released video evidence. In a handful of high profile cases, it took months to have an announcement from the prosecutor or the grand jury decision whether to charge the officer. Although it is rare that grand juries are convened to consider charges against an officer (and even rarer for them to be charged), the sample is unique in that I subset to only high-profile events.

[37]PBS Frontline provides data on PoP investigations here, including the rationale for initiating it, the start date, and the outcome of the investigation. Because of a lack of consistent reporting of departmental data, only two of the six high-profile OFs that likely induced PoP investigations are in the analysis sample.

Laquan McDonald's death (November 2015), there was a PoP investigation into the Chicago Police Department that began in January 2016, which happened to coincide with the start of an unrelated monitoring agreement with the ACLU. The sharp drop in arrests in November and December (Figure 12), before the investigation or the monitoring took place, suggest that the public scrutiny plays a pivotal role in the reduction of policing effort.

Despite arrest reductions exclusively occurring in less serious crimes, I find substantial increases in serious *offenses* in the aftermath of these high-profile, officer-involved fatalities (Figure 9): For violent crimes, Figure 10 documents a generally statistically significant rise of 10–17% in robberies and murders for Q0–Q4. There is also evidence of smaller increases in property crime, driven by theft (Figure 11). Table B.14 and Table B.15 provide evidence that these results are robust to increasing the threshold for the 'high-profile' treatment inclusion. The highest profile deaths—ones that generate at least 5,000 articles of coverage—draw even greater increases of 27% in murder, 11% in aggravated assault, and 12% in burglary.

The additional crimes, especially the rise in murder, cause a tremendous loss of welfare (Table 1). These results are consistent with Evans and Owens (2007), Chalfin and McCrary (2018), and Mello (2019), which find that violent crime is more sensitive to fluctuations in aggregate policing effort than property crime. This theory becomes more plausible if low MB arrests signify police presence to marginal offenders. However, because community awareness of the incident and reductions of officer effort are usually simultaneous, it is difficult to identify the cause of the offending response.

One important direction for future research is investigating whether these crime increases are the result of a reduction in policing effort or a reaction to the high-profile OF itself. For the former theory, decreases in arrests for primarily lower-level offenses—many of which involve public, street-level interactions—would be internalized by marginal offenders as a signal of lower apprehension risk, decreasing the expected costs from engaging in crime (Becker, 1968). Alternatively, marginal offenders may be reacting to the incident itself and subsequent (lack of) legal proceedings, where diminished perceptions of police legitimacy and procedural justice lead to declines in legal compliance and result in people "taking the law into their own hands" (Persico, 2002; Kirk and Papachristos, 2011; Leovy, 2015; Rosenfeld and Wallman, 2019).

7 Conclusion

Amidst an unprecedented rise in widespread civil unrest touched off by episodes of police brutality, the national discourse has turned a spotlight on issues in policing, and in particular, police use of force. There are over 1,100 officer-involved fatalities per year, meaning that 7–9% of homicides in the US involve the police killing a civilian. Some of the largest demonstrations of civil unrest in recent history were sparked by incidents of police use of force.

This paper provides the first estimates of how high-profile, officer-involved fatalities affect the arresting patterns for the involved police department and crime in that jurisdiction. To credibly identify the causal effect of public scrutiny on police officer effort, I use a national analysis that exploits the plausibly exogenous timing of these fatalities. I measure scrutiny through community awareness, media coverage, and local protests of OFs. This phenomenon has been dubbed the Ferguson Effect, named after a high-profile fatality in Ferguson, Missouri. The research question is complicated by the possibility that, after an OF, there may be several different mechanisms affecting arrests, such as (1) greater scrutiny of police, (2) reduced community cooperation in the clearance of crime, (3) reduced civilian reporting of crime, and (4) changes in offending behavior. I develop a novel theoretical model of an officer's objective function that uses insights into the institutional details of policing to provide model predictions that are empirically testable. I use these predictions to guide the analysis, tracing broader patterns in the changes of arrests by offense type to determine whether effort declines and what the causal mechanism is.

To carry out this analysis, I utilize an event study design to estimate effects when there are multiple "events" per observational unit, combining strategies from McCrary (2007) and Sandler and Sandler (2014). I run this integrated design on a large administrative dataset from the FBI that is merged with novel data on OFs. The empirical strategy directly leverages aspects of scrutiny by only including incidents that reach over 1,000 articles of media coverage and adjusting treatment timing to when the local community first searches for the incident, as measured by Google Trends. Further, I show that treated municipalities experience larger and more frequent protests.

This paper explores a largely unanswered question, and these findings suggest that officers *do* reduce their effort following a highly publicized OF, but not evenly across crime types. Consistent with the model prediction, in the presence of intensified scrutiny, officers curtail arrests for offense types that generate lower marginal benefit for themselves (whether monetary, personal, or professional). Theft arrests experience reductions of 3–11%, while there are declines of up to 23% in low MB arrests. The most marked change is in marijuana possession arrests, which drop 15–33% after Q0. Similar trends can be seen across a handful of low MB arrests, including disorderly conduct, liquor violation, and marijuana sale arrests. These low-level arrests are the easiest to reduce as they usually are officer-initiated stops, where they have the most discretion in determining whether to intervene. Notably, these reductions are not present with higher return arrests, demonstrated by the insignificant changes in violent crime or more serious property crime clearances. Further, the results are robust to numerous changes in empirical specification, even changes in state law and modifying the treatment sample by increasing the high-profile threshold (Section B.4).

These results suggest that intensified scrutiny leads to reductions in police effort for arrests of both Black and white offenders, suggesting that the ϕ parameter in Section 3 is not race-specific overall. However, among the margins where the steepest declines in effort are observed—theft for Part I, marijuana possession for Part II—there is suggestive evidence of larger effects for Black offenders.

Despite arrest reductions exclusively occurring in less serious crimes, I also find substantial increases in serious offending. Most notably, there is a significant rise of 10–17% in murders and robberies. There are also smaller increases of 3–7% in theft and motor vehicle theft. The violent crime effects, particularly for murder, are more prominent for the most publicized deaths—ones that are reported on over 5,000 times (Table B.14). The additional crimes, especially the rise in murder, cause a tremendous loss of welfare. Future research should investigate whether these crime increases are the result of a reduction in policing effort or a reaction to the high-profile fatality itself.

I have shown that in the aftermath of high-profile, officer-involved fatalities the ensuing scrutiny from the community (the principal) imposes additional costs on police officers (agents), who in turn reduce effort and their enforcement of lower-level offenses (Prat, 2005). The empirical results are consistent with other policies that increase the marginal cost of effort for officers, such as findings on the effect of ACLU monitoring imposed from a consent decree (Cassell and Fowles, 2018) and Department of Justice "pattern-or-practice" investigations (Devi and Fryer, 2020). Given that the sharpest and most persistent drops in arrests are from low social cost offenses such as marijuana possession, liquor violations, and disorderly conduct, these reductions may reflect an allocation of services that more closely aligns with the preferences of the community, who signal and translate them through awareness campaigns, media coverage, and protests (Battaglini, Morton, and Patacchini, 2020). These drops may even be considered socially beneficial, after accounting for the social costs of policing and incarceration. However, if the drop in the least serious arrests is what leads to a rise in the most serious offenses—consistent with the notion that these low MB arrests are a signal of police presence for marginal offenders—then the increases in crime likely offset any welfare gains. Regardless of the mechanism, these high-profile, officer-involved fatalities impose tremendous crime costs on the involved jurisdictions, warranting further prioritization of evidence-based policies to reduce them.

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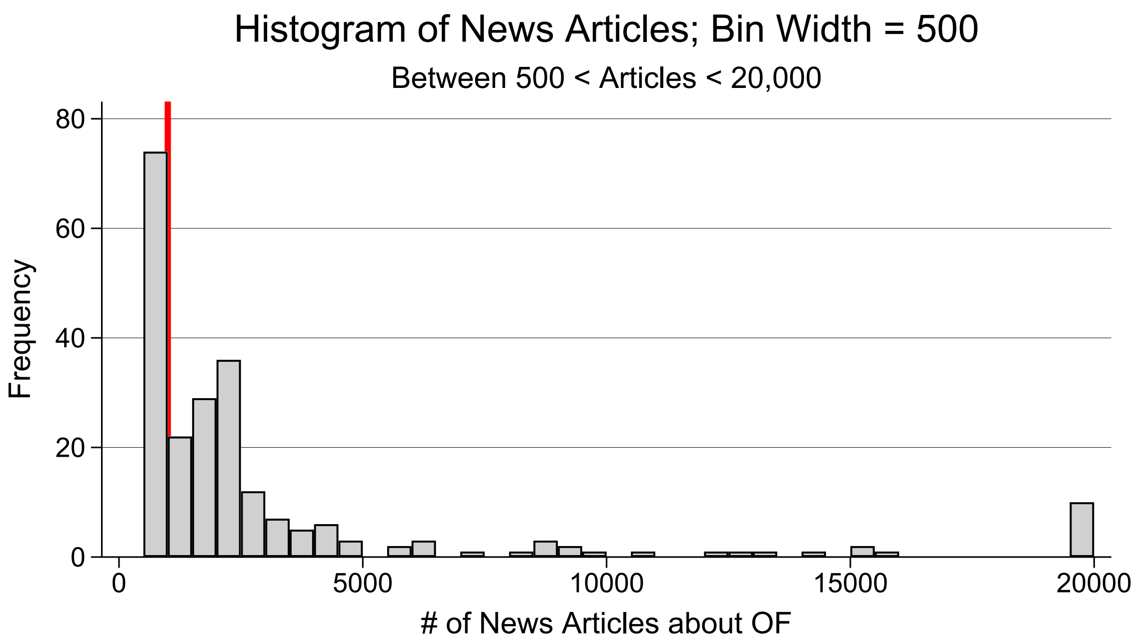
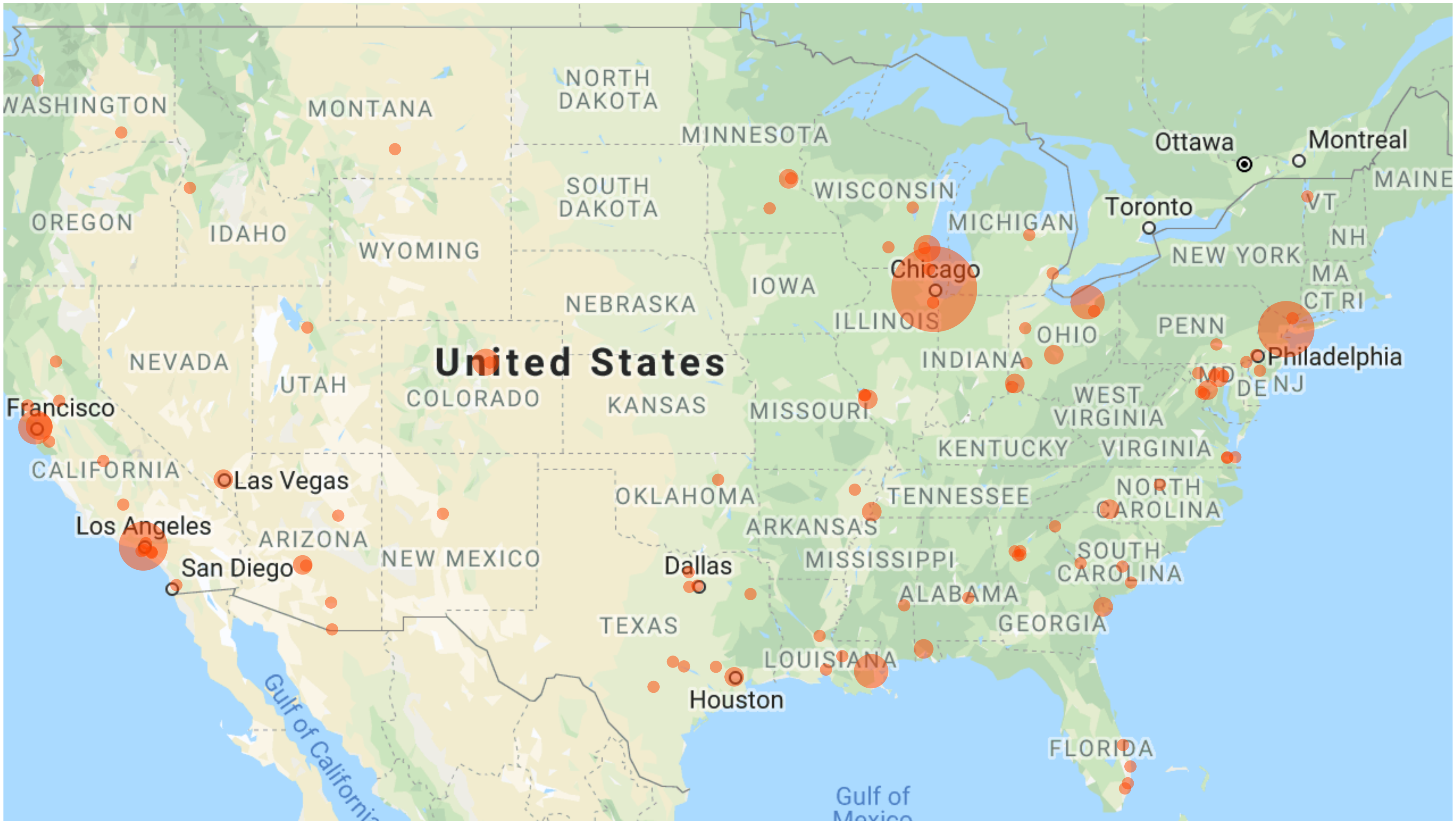
8 Figures

Figure 1: Histogram of News Articles on Officer-Involved Fatalities (2005-2016)



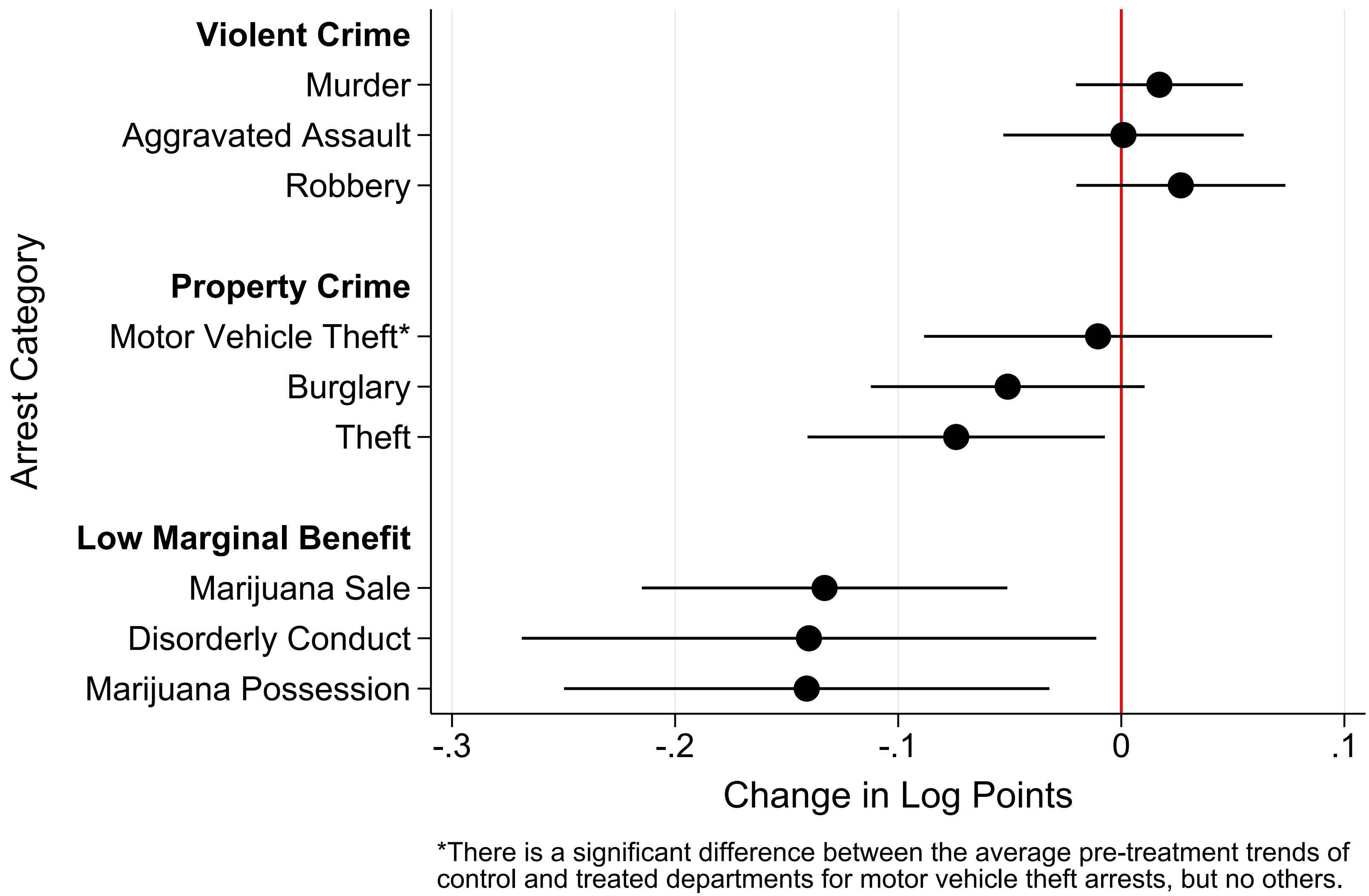
99% of the mass would be in the first bin (0-500 articles) and there's heavy right skew (max = 400k). The histogram censors the first bin to visually accomodate the distribution. For incidents with > 20,000 articles (9 observations), they are binned in the last histogram bar. [Overall: 11,155 observations]

Figure 2: Map of High-Profile, Officer-Involved Fatalities (2005–2016)



The map illustrates the geographic variation in high-profile, officer-involved fatalities. The size of the dot corresponds to the number of fatalities in that city during 2005–2016. The map includes fatalities that are not used in the analysis sample, likely because the involved department poorly reported data to the FBI. Figure B.1 shows the distribution of timing of high-profile OFs.

Figure 3: Difference-in-differences (DD) Coefficient Estimates by Arrest Category

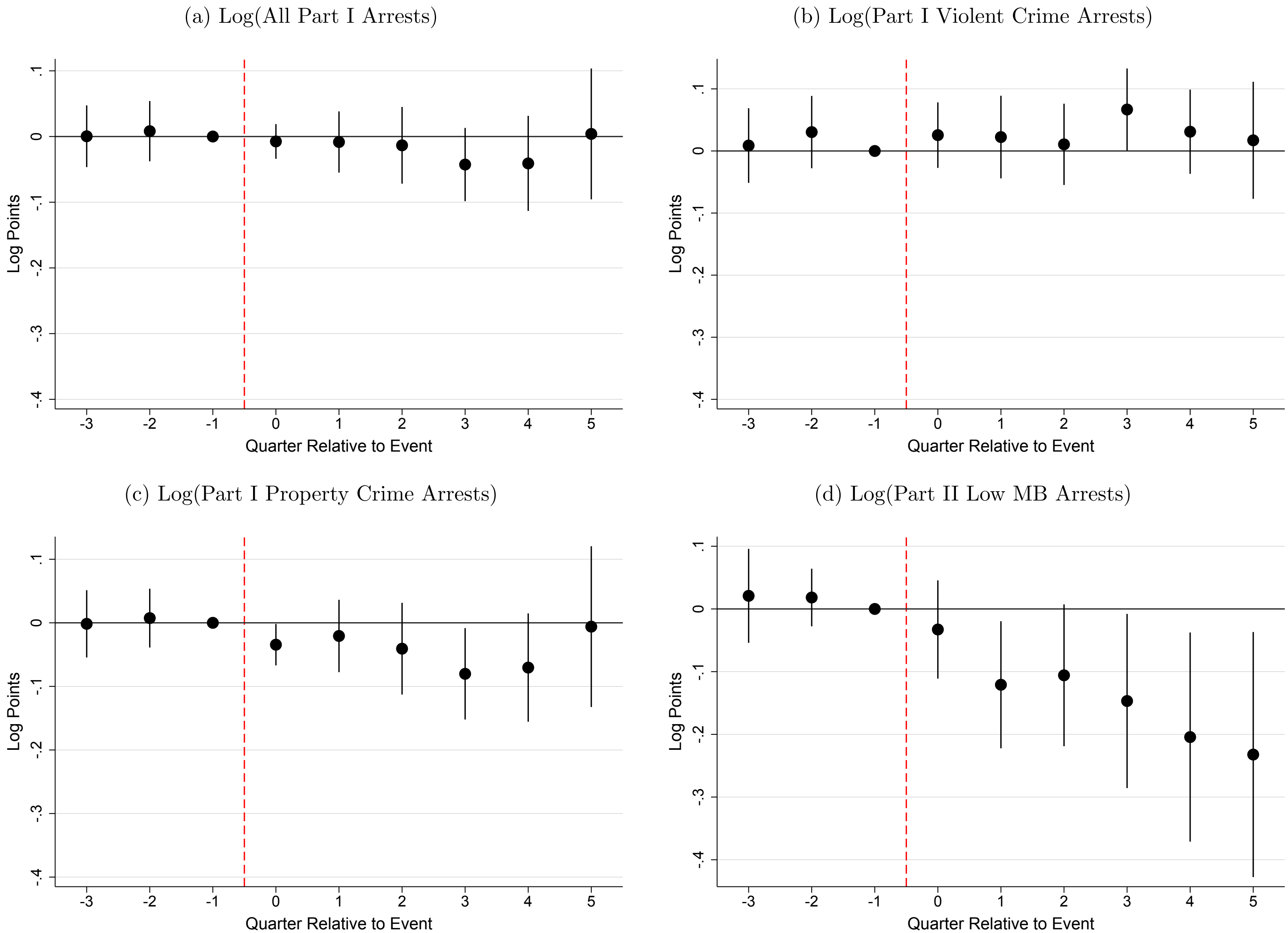


Circles display DD coefficients from separate regressions—in descending order of the social cost of crime—using a sample of city police departments with fewer than 9 outliers and a population greater 10,000. Lines represent the 95% confidence interval using standard errors clustered at the department level. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. The arrest DD tables begin with Table B.1. [740,838 observations; 52 treated; 2,687 control agencies]

Figure 4: Example Integrated Dummy Setup; Event window: -3 to 3 months around event with binned endpoints

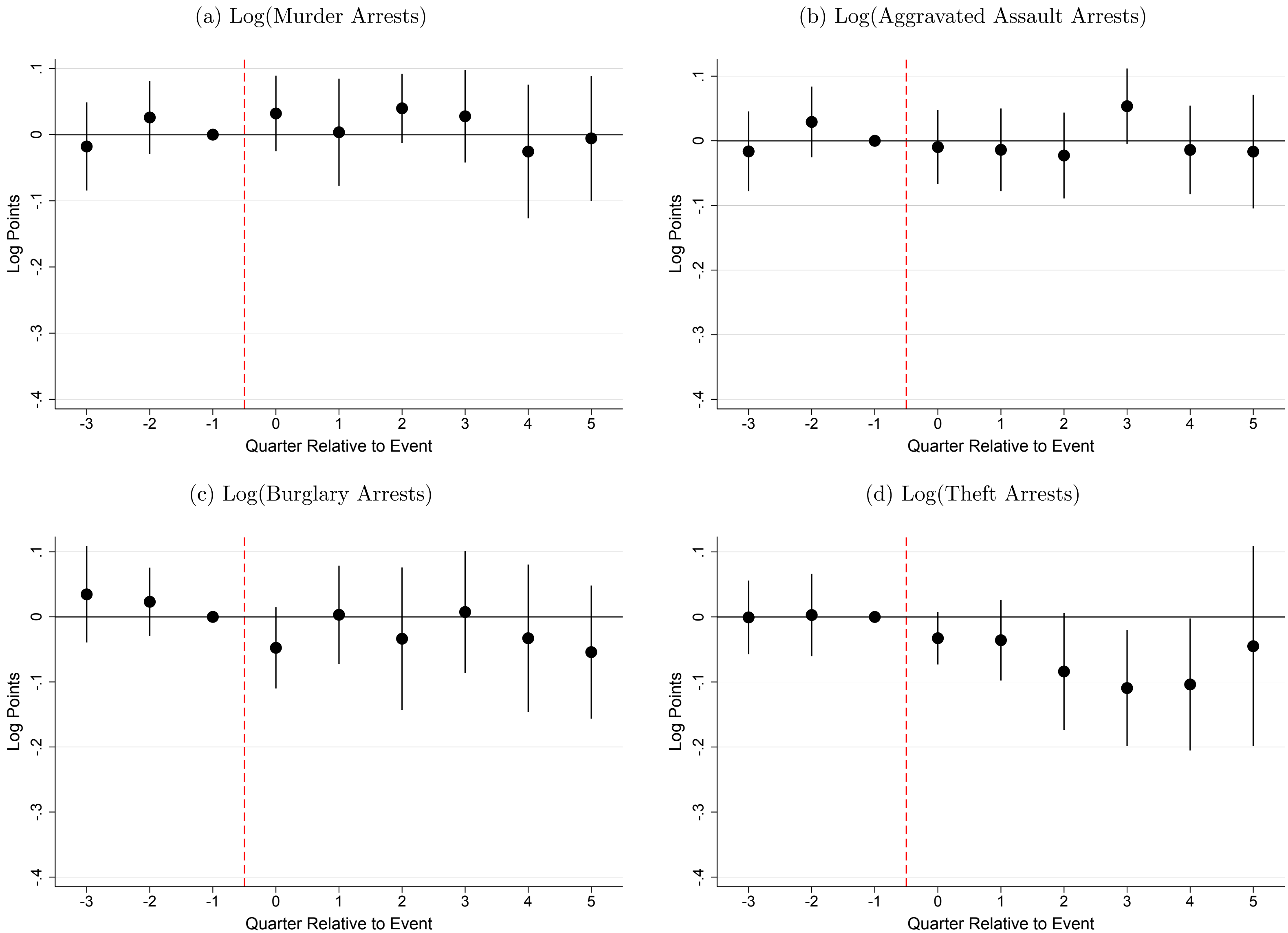
Month Relative to Event	**-4+**	**-3**	**-2**	**-1**	**0**	**1**	**2**	**3**	**4+**	**Total**
Ferguson: 2005M1	2	0	0	0	.	.	.	.	.	2
Ferguson: 2005M2	2	0	0	0	0	.	.	.	.	2
Ferguson: 2005M3	2	0	0	0	0	0	.	.	.	2
Ferguson: 2005M4	2	0	0	0	0	0	0	.	.	2
Ferguson: 2005M5	2	0	0	0	0	0	0	0	.	2
Ferguson: 2005M6	2	0	0	0	0	0	0	0	0	2
Ferguson: 2005M7	2	0	0	0	0	0	0	0	0	2
Ferguson: 2005M8	2	0	0	0	0	0	0	0	0	2
Ferguson: 2005M9	2	0	0	0	0	0	0	0	0	2
Ferguson: 2005M10	1	1	0	0	0	0	0	0	0	2
Ferguson: 2005M11	1	0	1	0	0	0	0	0	0	2
Ferguson: 2005M12	1	0	0	1	0	0	0	0	0	2
Ferguson: 2006M1	1	0	0	0	1	0	0	0	0	2
Ferguson: 2006M2	0	1	0	0	0	1	0	0	0	2
Ferguson: 2006M3	0	0	1	0	0	0	1	0	0	2
Ferguson: 2006M4	0	0	0	1	0	0	0	1	0	2
Ferguson: 2006M5	0	0	0	0	1	0	0	0	1	2
Ferguson: 2006M6	.	0	0	0	0	1	0	0	1	2
Ferguson: 2006M7	.	.	0	0	0	0	1	0	1	2
Ferguson: 2006M8	.	.	.	0	0	0	0	1	1	2
Ferguson: 2006M9	.	.	.	.	0	0	0	0	2	2
Ferguson: 2006M10	.	.	.	.	.	0	0	0	2	2

Figure 5: Effect of High-Profile, Officer-Involved Fatality on Arrests



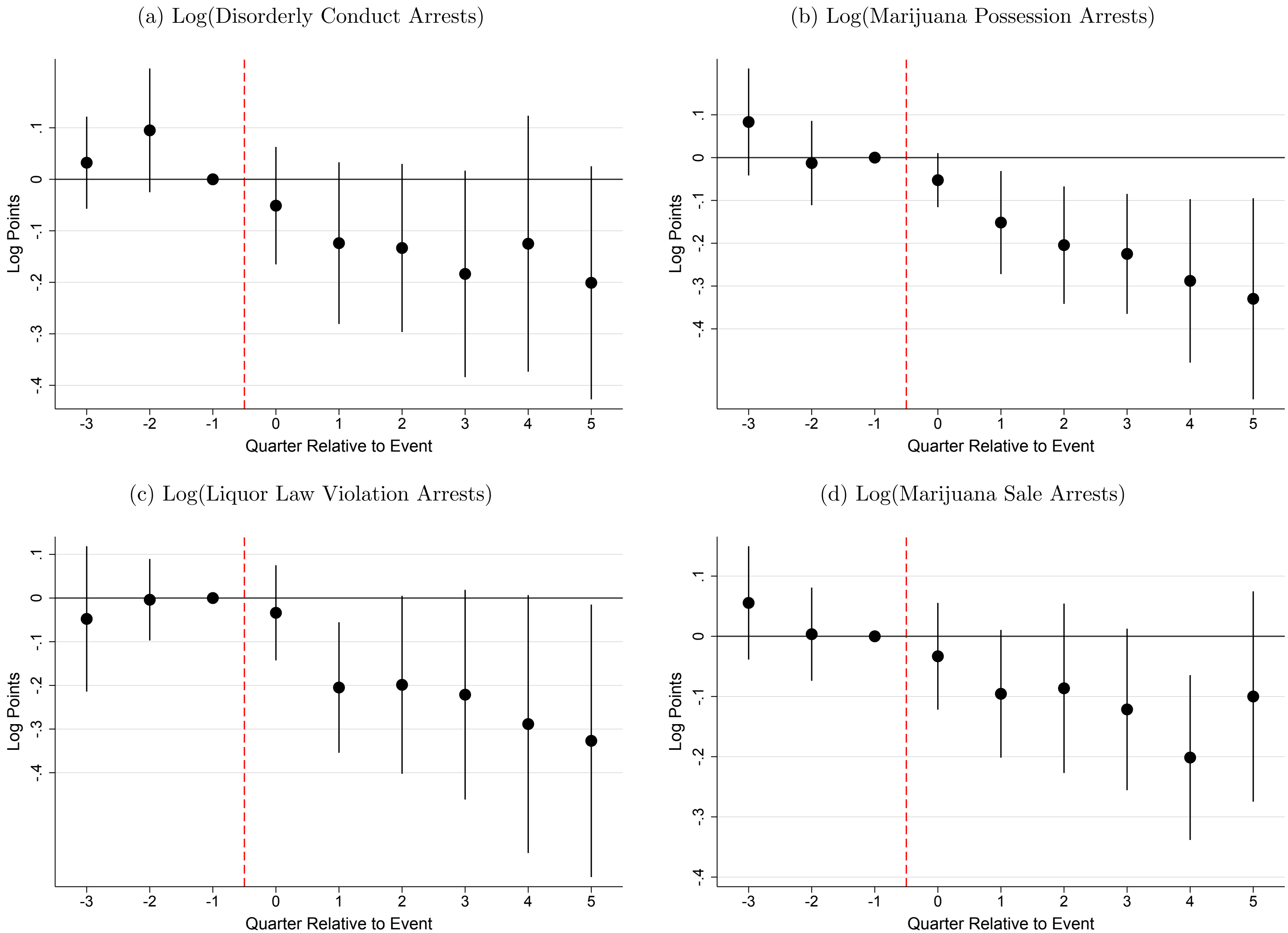
Using a sample of city police departments with over 10,000 people and fewer than 9 outliers from 2005–2016, the event time coefficients (circles) right of the red dotted line are the change in arrests after a high-profile, officer-involved fatality. Lines represent the 95% confidence interval using standard errors clustered at the department level. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. [598,788 observations; 52 treated; 2,687 control agencies]

Figure 6: Effect of High-Profile, Officer-Involved Fatality on Violent and Property Crime Arrests



Using a sample of city police departments with over 10,000 people and fewer than 9 outliers from 2005–2016, the event time coefficients (circles) right of the red dotted line are the change in arrests after a high-profile, officer-involved fatality. Lines represent the 95% confidence interval using standard errors clustered at the department level. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. [598,788 observations; 52 treated; 2,687 control agencies]

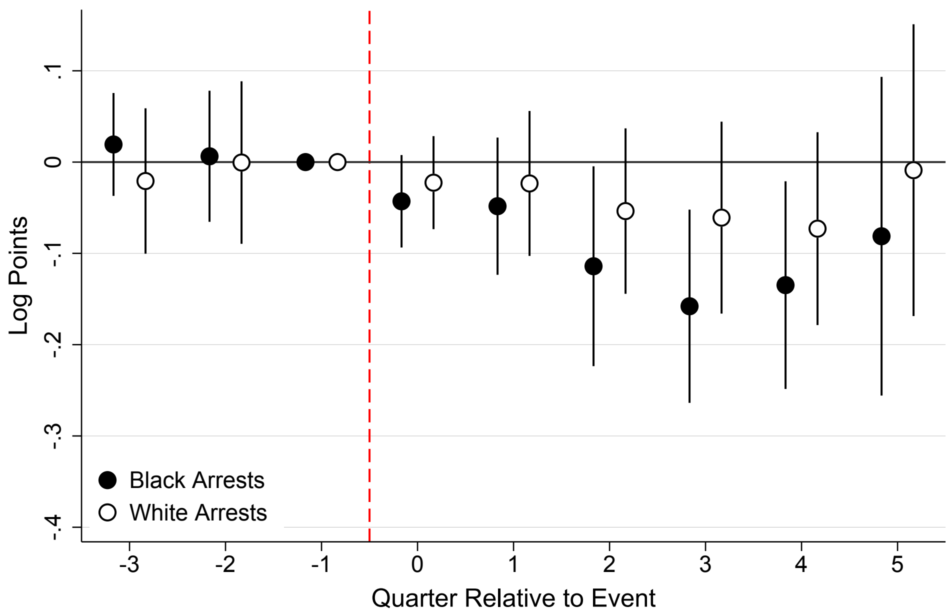
Figure 7: Effect of High-Profile, Officer-Involved Fatality on Low MB Arrests



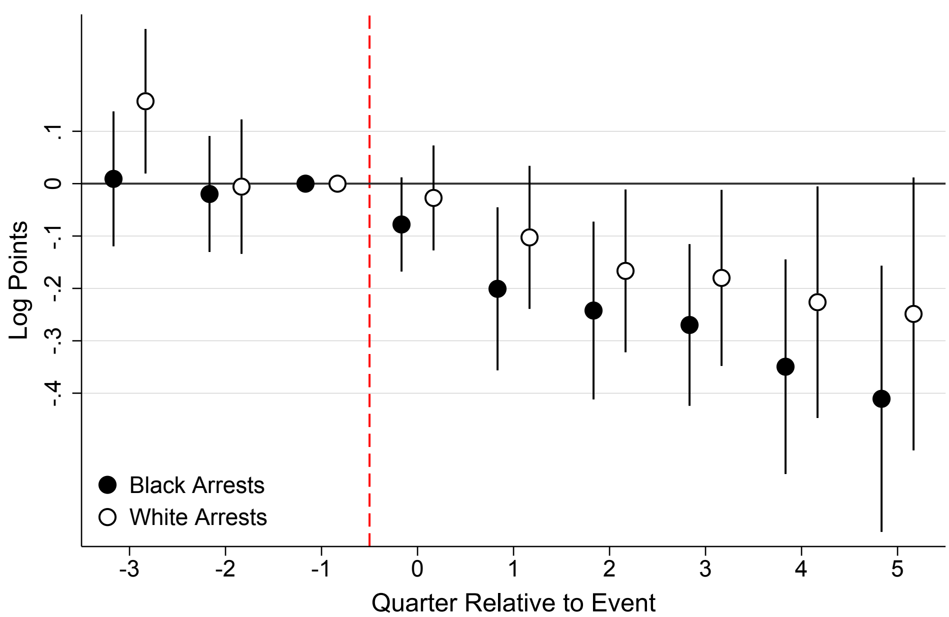
Using a sample of city police departments with over 10,000 people and fewer than 9 outliers from 2005–2016, the event time coefficients (circles) right of the red dotted line are the change in arrests after a high-profile, officer-involved fatality. Lines represent the 95% confidence interval using standard errors clustered at the department level. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. [598,964 observations; 52 treated; 2,687 control agencies]

Figure 8: Effect of High-Profile, Officer-Involved Fatality on Race-Specific Arrests

(a) Log(Theft Arrests)

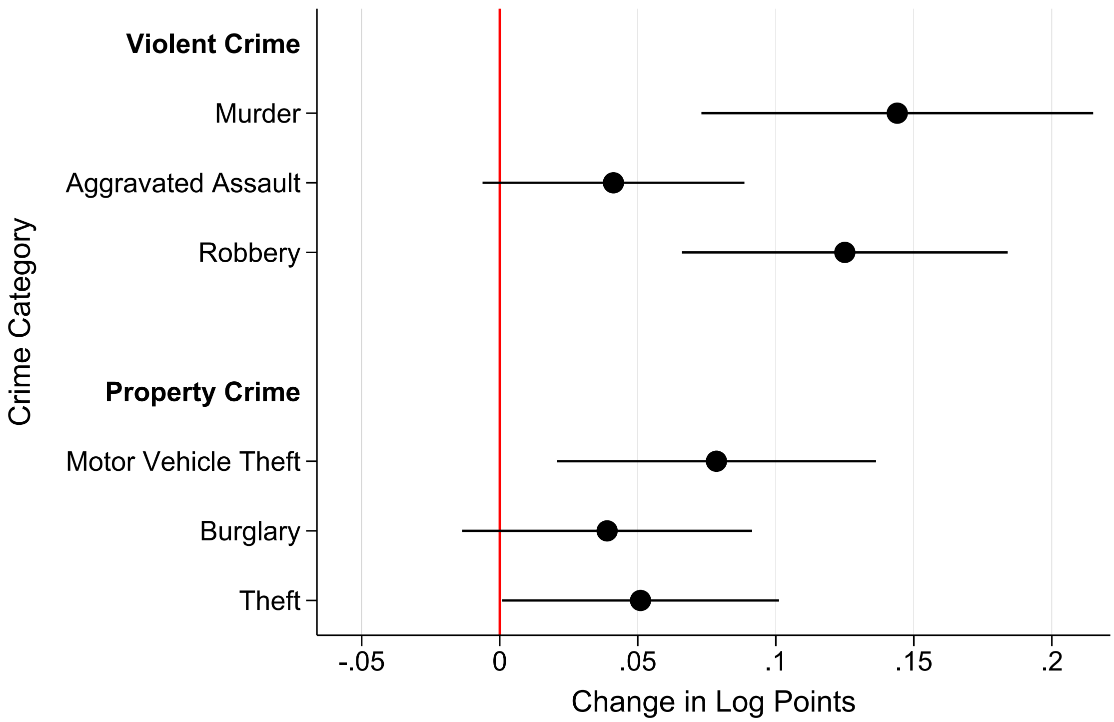


(b) Log(Marijuana Possession Arrests)



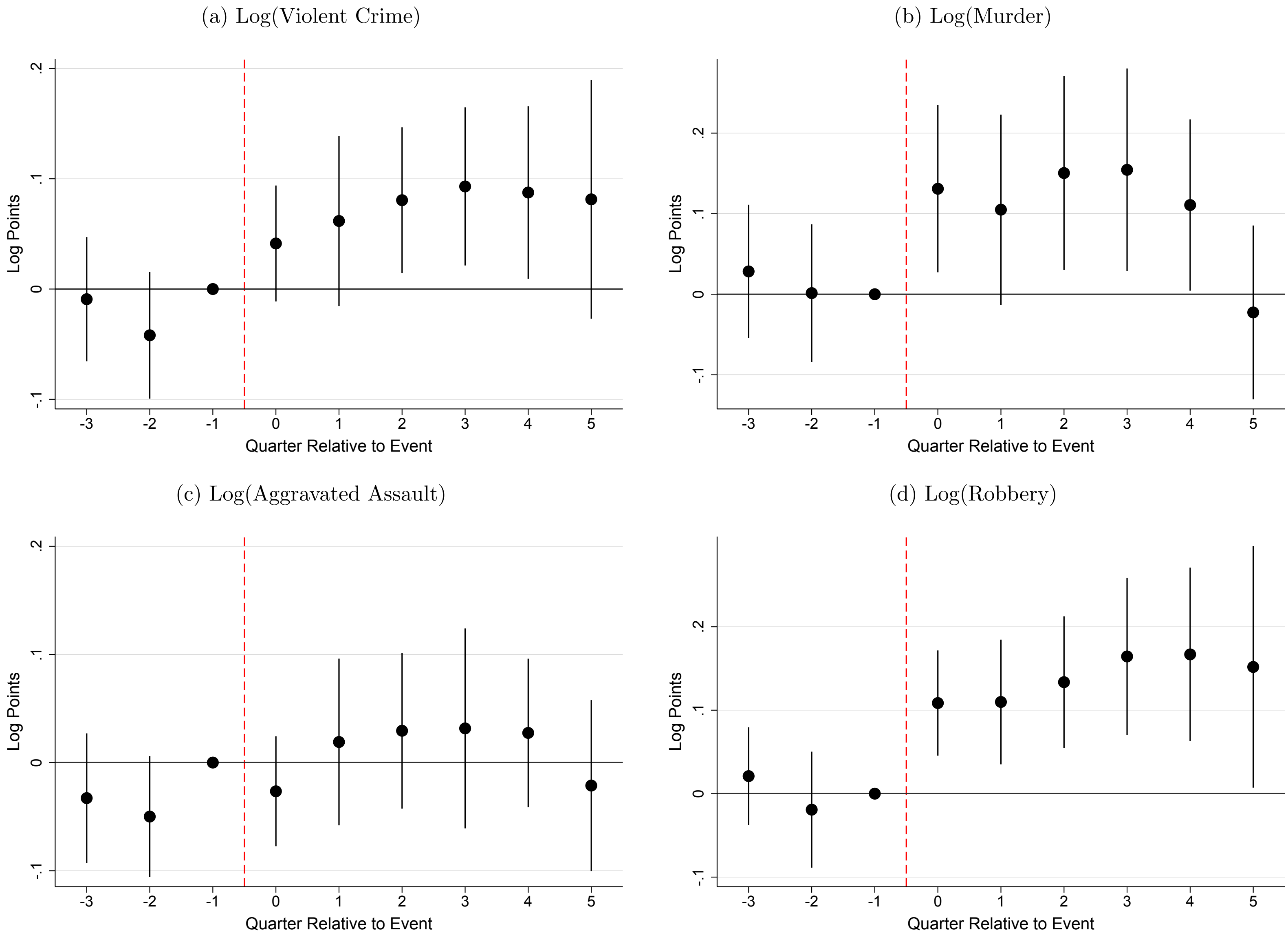
Using a sample of city police departments with over 10,000 people and fewer than 9 outliers from 2005–2016, the event time coefficients (circles) right of the red dotted line are the change in arrests after a high-profile, officer-involved fatality. Lines represent the 95% confidence interval using standard errors clustered at the department level. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. [598,788 observations; 52 treated; 2,687 control agencies]

Figure 9: Crime Analysis: Difference-in-differences (DD) Coefficients by Crime Category



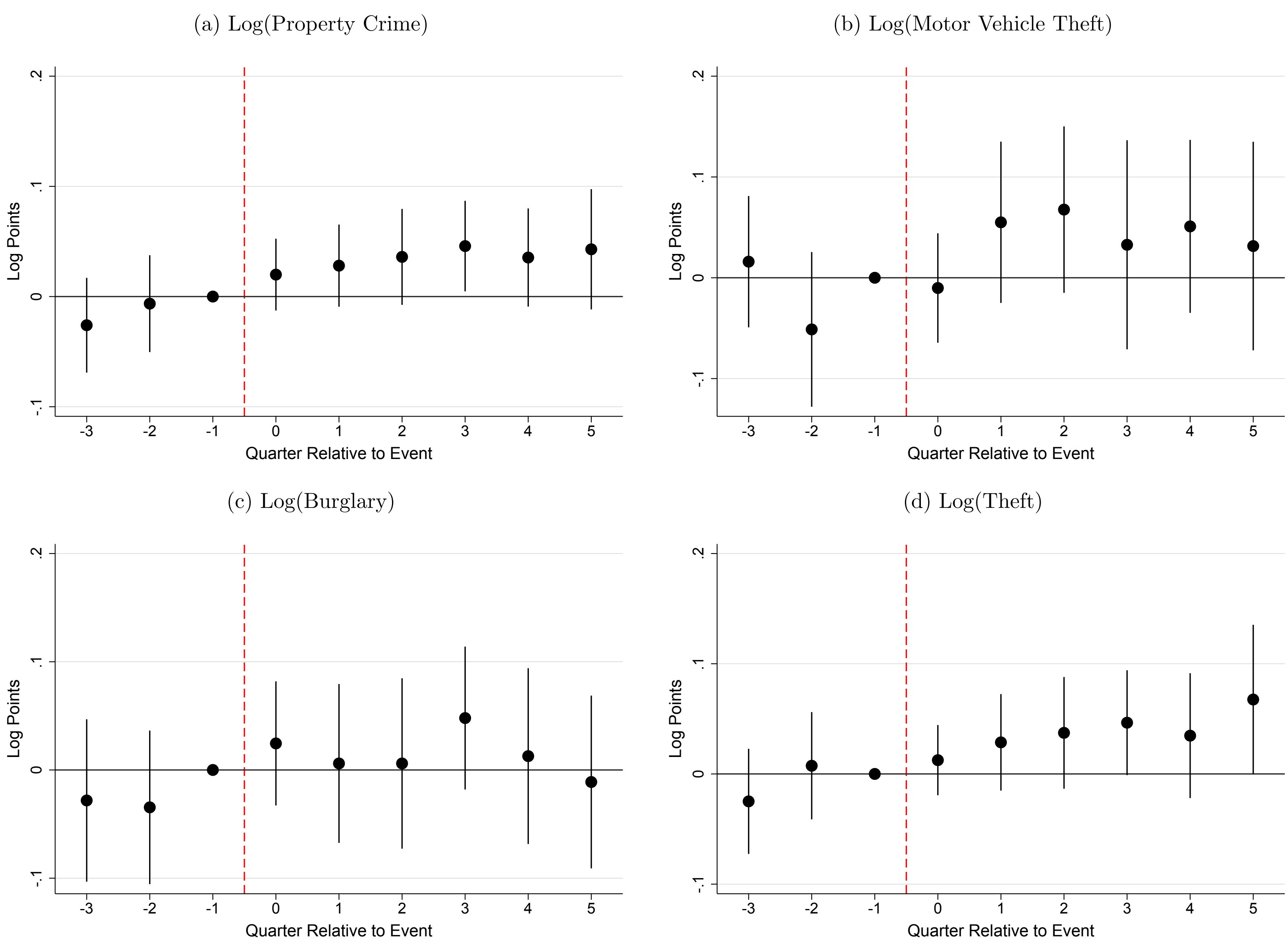
Circles display DD coefficients from separate regressions—in descending order of the social cost of crime—using a sample of city police departments with fewer than 9 outliers and a population greater 10,000. There are no significant differences between the average pre-treatment trends of control and treated departments in any these regressions. Lines represent the 95% confidence interval using standard errors clustered at the department level. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. The crime DD tables begin with Table B.4. [740,838 observations; 52 treated; 2,687 control agencies]

Figure 10: Crime Analysis: Effect of High-Profile, Officer-Involved Fatality on Violent Crime



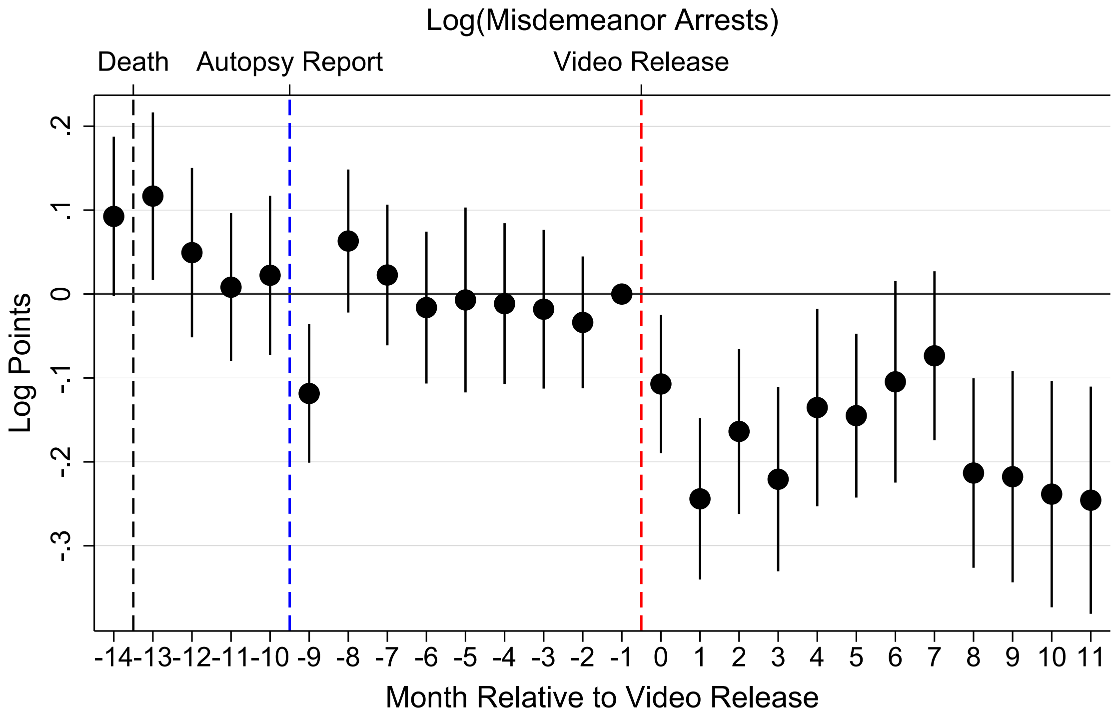
Using a sample of city police departments with over 10,000 people and fewer than 9 outliers from 2005–2016, the event time coefficients (circles) right of the red dotted line are the change in crime after a high-profile, officer-involved fatality. Lines represent the 95% confidence interval using standard errors clustered at the department level. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. [598,788 observations; 52 treated; 2,687 control agencies]

Figure 11: Crime Analysis: Effect of High-Profile, Officer-Involved Fatality on Property Crime



Using a sample of city police departments with over 10,000 people and fewer than 9 outliers from 2005–2016, the event time coefficients (circles) right of the red dotted line are the change in crime after a high-profile, officer-involved fatality. Lines represent the 95% confidence interval using standard errors clustered at the department level. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. [598,788 observations; 52 treated; 2,687 control agencies]

Figure 12: Case Study of Laquan McDonald and the Chicago Police Department (2014–2017)



The dotted black line (left) depicts the month of death for Laquan McDonald (October 2014). The dotted blue line (middle) denotes when an article with the associated autopsy report was released (February 2015). The dotted red line (right) highlights when the video of his death was released to the public and the community became aware of the incident (November 2015). Circles display monthly event time coefficients for a regression of log misdemeanor arrests at the beat level in Chicago from 2014–2017. Lines represent the 95% confidence interval using standard errors clustered at the district level. I control for district-level fixed events and linear trends. [19,080 observations; 22 districts; 274 beats]

9 Tables

Table 1: Chalfin and McCrary (2018): Cost of Crime and Police

	Cost per Officer	Officers per 100K Population	Annual Cost per Capita
Sworn police	$130,000	262.7	$341
	Cost per Crime	Crimes per 100K Population	Annual Expected Cost per Capita
Murder	$7,000,000	9.9	$693
Aggravated Assault	$38,924	418.9	$163
Robbery	$12,624	286.4	$36
Motor vehicle theft	$5,786	454.3	$26
Burglary	$2,104	976.2	$21
Theft	$473	2,623.30	$12
		Grand Total:	$995
		Income per Capita:	$26,267

Table 2: Mean (S.D.) of Municipality Arrest, Characteristics, and Protest Data

	Pure Controls	Treated PDs	Overall
Arrest Rate (per 100,000 pop)			
White Violent Arrests	37.1	27.8	36.7
	(200.3)	(34.0)	(196.1)
Black Violent Arrests	50.9	72.7	51.9
	(111.2)	(62.5)	(109.6)
White Property Arrests	84.7	71.0	84.1
	(275.4)	(54.9)	(269.7)
Black Property Arrests	166.1	181.8	166.8
	(345.6)	(294.8)	(343.5)
White Low MB Arrests	188.2	184.7	188.1
	(489.6)	(168.8)	(480.2)
Black Low MB Arrests	246.8	269.6	247.8
	(651.7)	(252.5)	(639.6)
Local Characteristics (ACS 2014)			
Population	143,208.4	487,497.1	158,057.7
	(353,407.5)	(629,482.6)	(376,151.6)
% White	56.9	44.4	56.4
	(23.5)	(17.5)	(23.4)
% Black	11.9	23.3	12.4
	(14.9)	(19.9)	(15.3)
Poverty Rate	17.1	20.7	17.3
	(8.4)	(6.5)	(8.3)
White Poverty Rate	12.4	12.5	12.4
	(7.2)	(5.8)	(7.1)
Black Poverty Rate	26.1	28.8	26.3
	(13.9)	(8.4)	(13.7)
% Bachelor's	32.2	32.4	32.2
	(14.8)	(11.9)	(14.7)
Square Miles	55.0	124.0	58.0
	(134.3)	(128.9)	(134.8)
Population Density	3,715.7	4,340.5	3,742.8
	(3,813.3)	(2,948.8)	(3,782.1)
BLM Protests (Aug. 2014–Aug. 2015)			
Number of BLM Protests	0.78	4.74	0.95
	(2.83)	(6.98)	(3.23)
Attendance	210.62	1,237.47	254.91
	(2,093.86)	(2,496.65)	(2,123.08)
% Protesting	21.24	66.01	23.17
	(40.90)	(47.37)	(42.19)
Observations	162,132	7,308	169,440

Pure control departments have had no high-profile OFs. Treated PDs have had at least one OF. There are 105 high-profile OFs with a maximum of 6 in one jurisdiction, Los Angeles.

Table 3: Fatalities by Victim & Incident Characteristics with Mean News Articles

	Share	**Mean News Articles**	**Number**
Gender			
Female	14%	2,234	10
Male	86%	18,044	62
Race/Ethnicity			
Asian	1%	1,610	1
Black	74%	20,575	53
Hispanic	11%	2,740	8
Native American	3%	1,385	2
White	11%	3,034	8
Cause of Death			
Asphyxiation	3%	2,160	2
Beaten	7%	52,442	5
Gunshot	90%	13,454	65
Armed?			
Allegedly Armed	40%	4,230	21
Unarmed	60%	27,309	32
Behavioral Health Issue?			
No	76%	22,485	44
Yes, Alcohol/Drug Use	2%	6,340	1
Yes, Mental Illness	22%	8,012	13

Source: Fatal Encounters (2005–2016); 72 observations; ASR Sample

Appendices

A Appendix: Data

A.1 Inputting Rules for the Uniform Crime Report

The counts in the FBI data are based on the Hierarchy Rule, which states that for multiple-offense incidents the police department should only record the most serious offense/arrest, providing a ranking of Part I offenses and their severity (FBI, 2004). Though this reporting framework suggests that the data could be an undercount, the National Incident-Based Reporting System (NIBRS)—an FBI dataset that contains every reported criminal incident—suggests that for most offenses, around 90%, only one crime occurs. Thus, this undercount is not a grave concern, at least for the sample of jurisdictions that report to NIBRS. Additionally, offenses are distinct through the 'Separate of Time and Place' rule. Even if a criminal commits numerous offenses in a short period of time, but they are perpetrated in different locations, then they must be recorded as separate incidents (FBI, 2004), providing assurance that the data is a close reflection of reality.

A.2 Cleaning the Uniform Crime Report

For the primary outcomes of interest, I use the FBI's "Offenses Known and Clearances by Arrest" and the "Arrests by Age, Sex, Race" dataset, cleaned and formatted by Jacob Kaplan (Kaplan, 2018; Kaplan, 2019). Then, I extensively clean the data myself. I further allay concerns about the reporting quality by narrowing the analysis sample to departments that report at least six years of data between 2005–2016 to both datasets. Additionally, I only keep departments report each month consistently across years, excluding certain departments who, for example, file only twice a year (i.e., input all of their crime and arrest statistics under July and December). Following Evans and Owens (2007) and Mello (2019), I fit a local polynomial function for both crime and arrests for each department and set all of values outside the 99.9% confidence interval as an outliers. The threshold was determined by visually inspecting a random sample of departments. These outliers are then set to missing. Moreover, observations are flagged as outliers if all the values in both violent and property crime (or arrests) are zero for the entire quarter and the population of the jurisdiction is above 5,000.

As pointed out in Chalfin and McCrary (2018), the population variable jumps discretely in census years for many jurisdictions. In order to alleviate that concern, I fit a local polynomial to smooth out the population variable at the census threshold and use the smoothed population variable as a control in the analysis. Finally, I use a "clean" subset of the data, whose sample inclusion is city police departments, who have a population greater than 10,000, and fewer than 9 outliers across the 12-year sample frame. In Evans and Owens (2007), they similarly use the city departments with a population threshold of 10,000 as determinants for sample inclusion, but they drop agencies if they have four or more outliers. I chose a higher threshold because I had additional methods detecting of outliers, and I have monthly data as to opposed yearly data.

A.3 Scraping News Articles for Officer-Involved Fatalities

To measure how high-profile an officer-involved is, I scrape the number of news articles written about the incident, as determined by a search engine's news classification. Each search about the OF requires each article to contain the victim's full name, the city where the incident took place, and the word "police" or the word "killing." I read each case report on the incidents that have more than 1,000 articles to remove "falsely positive" high-profile occurrences. There are over 100 cases where I demote a fatality's news article number to zero if they are erroneously high-profile. Some examples of erroneous cases with inflated news hits are (1) articles about a different, more famous person with same name in the same city, (2) people participating in multiple crimes, often multiple murders, which gave them notoriety, making the coverage not about the OF itself (e.g, one of the Boston Marathon bombers, Tamerlan Tsarnaev), or (3) victims engaging in shootouts with police. This is not an exhaustive list of reasons, but it provides the general overview for the type of OF that is be excluded from the event sample to study exogenous shocks of scrutiny on policing effort. Lowering the threshold, to 500 articles for example, results in many false positives and not as many protests in the affected jurisdictions, bolstering the argument for a higher threshold.

A.4 Modifying Event Time Using Google Trends

A key component in measuring the causal impact of high-profile, officer-involved fatalities on policing and offending behavior is ensuring that the timing aligns with community awareness of the incident, when public scrutiny (as well as other possible mechanisms) can plausibly begin. I input the name of each victim in Google Trends, constraining the results to search trends in the locality where they were killed a year before they died to the end of the sample frame (December 2016).[38] Since some of the victim names are common, often there is 'noise' in the Google Trends figures, evidenced by searches for the victim name in the locality prior to their death. I conduct a visual test of the trends, and determine the first month after the date of death in which searches spike beyond the 'noise' for that search term. For the fatalities whose selected months are different than the month of death, I work to cross-validate the timing by exploring what prompted the rise in community awareness,

[38]For the more high-profile incidents, a completed search phrase automatically comes up when searching the name. For example, when typing in "Laquan McDonald," Google Trends will allow you examine search trends of his name, but also will suggest the "Murder of Laquan McDonald." In those instances, I compare both trends of strictly the name being searched and the search phrase in the locality to determine the proper community awareness time.

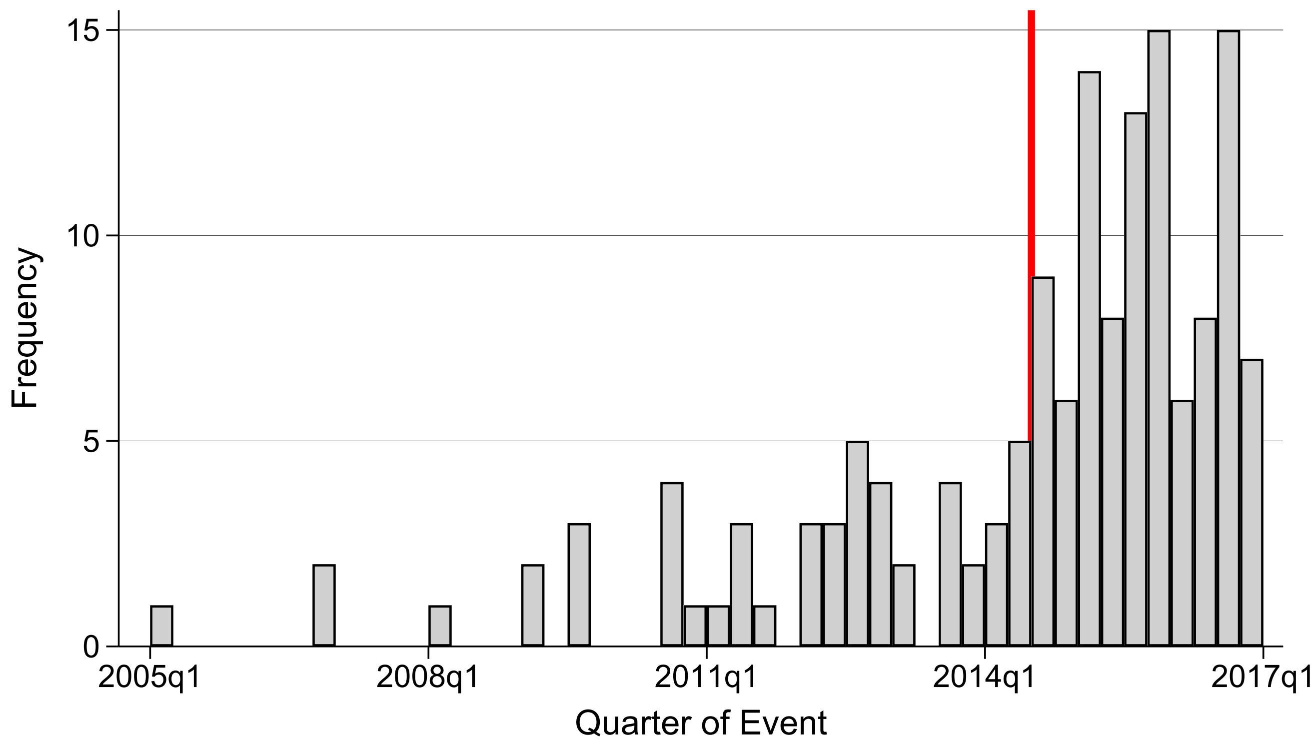
typically by finding a news article that describes revelations about the case (e.g., video of the incident that is uncovered or obtained).

For example, Derek Williams was killed in Milwaukee in July 2011, and searches for that name can be accessed here. There appears to be noise in the search trends, where that name is searched in Milwaukee intermittently throughout the sample frame, including before his death. In fact, the local maximum before September 2012 is November 2010, eight months before his death. The first spike in searches occurs on September 2012, over a year after his death, when the Milwaukee Journal Sentinel (MLS) obtained and posted the squad car surveillance video of his final moments on their website (link to MLS article here, and link to New York Times article detailing timeline of events). Thus, the time of the event is modified to be September 2012, the first date of widespread community awareness.

B Appendix: Figures and Tables

B.1 Supplemental Findings

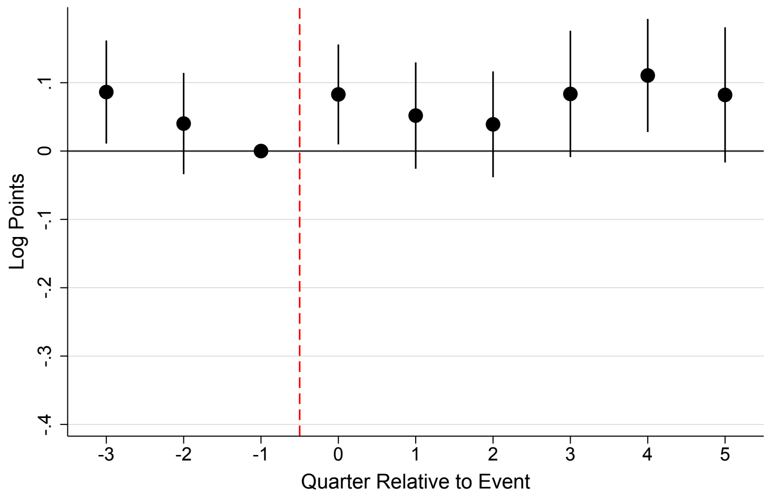
Figure B.1: Timing of High-Profile, Officer-Involved Fatalities (2005–2016)



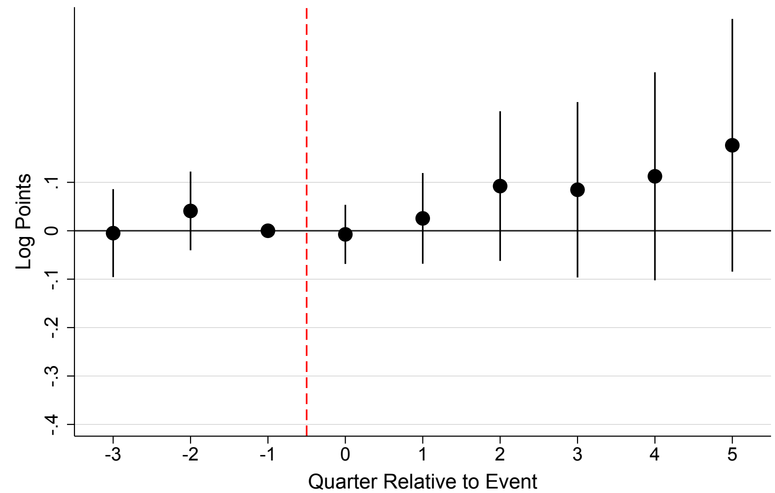
151 fatalities in total from 2005-2016. High profile is defined as ≥ 1,000 news articles. Red line depicts the death of Michael Brown in Ferguson, MO (August 2014). The map includes fatalities that will not be used in the main analysis sample, likely because the involved department poorly reported data to the FBI.

Figure B.2: Effect of Officer-Involved Fatality on Non-Low Marginal Benefit Arrests

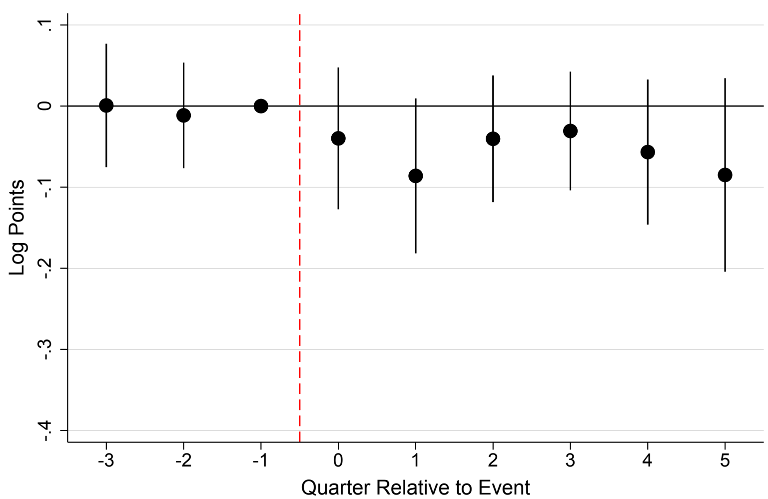
(a) Log(Robbery Arrests)



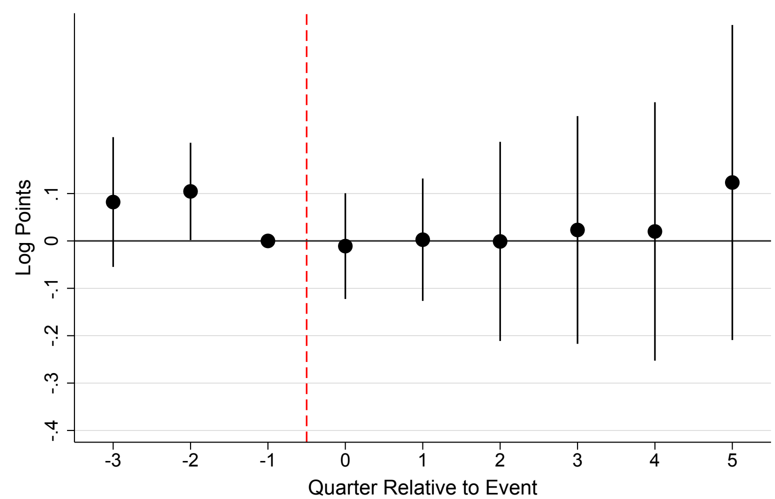
(b) Log(Motor Vehicle Theft Arrests)



(c) Log(Weapon Arrests)



(d) Log(Heroin/Cocaine Sale Arrests)



Using a sample of city police departments with over 10,000 people and fewer than 9 outliers from 2005–2016, the event time coefficients (circles) right of the red dotted line are the change in arrests after a high-profile, officer-involved fatality. Lines represent the 95% confidence interval using standard errors clustered at the department level. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. [598,788 observations; 52 treated; 2,687 control agencies]

Table B.1: Effect of the Highest Profile, Officer-Involved Fatality on Log(Violent Crime Arrests)

	DD	Pre-trend	DDD	Pre-trend
	(1)	(2)	(3)	(4)
Panel A: Violent Crime Arrests				
Treat*Post	0.029	0.037	0.021	0.028
	(0.026)	(0.030)	(0.031)	(0.038)
Pretrend Test		0.016		0.015
		(0.029)		(0.041)
Black*Treat*Post			0.017	0.018
			(0.033)	(0.037)
Black Pretrend Test				0.003
				(0.039)
Panel B: Murder Arrests				
Treat*Post	0.017	0.010	0.018	0.013
	(0.019)	(0.024)	(0.024)	(0.031)
Pretrend Test		-0.015		-0.009
		(0.029)		(0.036)
Black*Treat*Post			-0.002	-0.008
			(0.038)	(0.049)
Black Pretrend Test				-0.011
				(0.054)
Panel C: Aggravated Assault Arrests				
Treat*Post	0.001	0.019	-0.007	0.018
	(0.028)	(0.033)	(0.035)	(0.042)
Pretrend Test		0.035		0.051
		(0.029)		(0.039)
Black*Treat*Post			0.017	0.001
			(0.034)	(0.038)
Black Pretrend Test				-0.031
				(0.039)
Observations	740,838	740,838	740,838	740,838
Number of Agencies	2,739	2,739	2,739	2,739

Coefficients are from double (DD) and triple difference (DDD) regressions, plotted in Figure 3. Standard errors, clustered at the agency level, in parentheses. Uses only city police departments with less than 9 outliers and a population greater 10,000. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends, which is interacted with the black dummy for the DDD. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.2: Effect of the Highest Profile, Officer-Involved Fatality on Log(Property Crime Arrests)

	DD	Pre-trend	DDD	Pre-trend
	(1)	(2)	(3)	(4)
Panel A: Property Crime Arrests				
Treat*Post	-0.051**	-0.031	-0.032	-0.001
	(0.025)	(0.028)	(0.030)	(0.036)
Pretrend Test		0.039**		0.060**
		(0.020)		(0.030)
Black*Treat*Post			-0.039	-0.060*
			(0.025)	(0.033)
Black Pretrend Test				-0.042
				(0.035)
Panel B: Burglary Arrests				
Treat*Post	-0.051	-0.066*	-0.076**	-0.078*
	(0.031)	(0.036)	(0.035)	(0.043)
Pretrend Test		-0.029		-0.004
		(0.030)		(0.046)
Black*Treat*Post			0.050	0.024
			(0.038)	(0.052)
Black Pretrend Test				-0.050
				(0.075)
Panel C: Theft Arrests				
Treat*Post	-0.074**	-0.056	-0.044	-0.021
	(0.034)	(0.038)	(0.038)	(0.045)
Pretrend Test		0.035		0.045
		(0.022)		(0.029)
Black*Treat*Post			-0.060*	-0.071**
			(0.032)	(0.036)
Black Pretrend Test				-0.021
				(0.035)
Observations	740,838	740,838	740,838	740,838
Number of Agencies	2,739	2,739	2,739	2,739

Coefficients are from double (DD) and triple difference (DDD) regressions, plotted in Figure 3. Standard errors, clustered at the agency level, in parentheses. Uses only city police departments with less than 9 outliers and a population greater 10,000. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends, which is interacted with the black dummy for the DDD. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: Effect of the Highest Profile, Officer-Involved Fatality on Log(Low MB Arrests)

	DD	Pre-trend	DDD	Pre-trend
	(1)	(2)	(3)	(4)
Panel A: Low Marginal Benefit Arrests				
Treat*Post	-0.095**	-0.116**	-0.082	-0.103*
	(0.048)	(0.051)	(0.054)	(0.058)
Pretrend Test		-0.042		-0.040
		(0.027)		(0.032)
Black*Treat*Post			-0.025	-0.027
			(0.032)	(0.035)
Black Pretrend Test				-0.003
				(0.028)
Panel B: Disorderly Conduct Arrests				
Treat*Post	-0.140**	-0.159**	-0.148**	-0.163**
	(0.066)	(0.070)	(0.067)	(0.074)
Pretrend Test		-0.038		-0.029
		(0.037)		(0.045)
Black*Treat*Post			0.017	0.008
			(0.040)	(0.045)
Black Pretrend Test				-0.018
				(0.055)
Panel C: Marijuana Possession Arrests				
Treat*Post	-0.141**	-0.165**	-0.148**	-0.180**
	(0.055)	(0.066)	(0.058)	(0.071)
Pretrend Test		-0.047		-0.063
		(0.043)		(0.052)
Black*Treat*Post			0.014	0.031
			(0.053)	(0.057)
Black Pretrend Test				0.033
				(0.048)
Observations	741,014	741,014	741,014	741,014
Number of Agencies	2,739	2,739	2,739	2,739

Coefficients are from double (DD) and triple difference (DDD) regressions, plotted in Figure 3. Standard errors, clustered at the agency level, in parentheses. Uses only city police departments with less than 9 outliers and a population greater 10,000. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends, which is interacted with the black dummy for the DDD. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: Effect of the Highest Profile, Officer-Involved Fatality on Log(Violent Crime)

	DD	Pre-trend
	(1)	(2)
Panel A: Violent Crime		
Treat*Post	0.087***	0.096***
	(0.027)	(0.030)
Pretrend Test		0.018
		(0.025)
Panel B: Murder		
Treat*Post	0.144***	0.142***
	(0.036)	(0.040)
Pretrend Test		-0.004
		(0.047)
Panel C: Aggravated Assault		
Treat*Post	0.041*	0.059**
	(0.024)	(0.030)
Pretrend Test		0.034
		(0.032)
Panel D: Robbery		
Treat*Post	0.125***	0.119***
	(0.030)	(0.034)
Pretrend Test		-0.012
		(0.025)
Observations	740,838	740,838
Number of Agencies	2,739	2,739

Coefficients are from double difference (DD) regressions, plotted in Figure 9. Standard errors, clustered at the agency level, in parentheses. Uses only city police departments with less than 9 outliers and a population greater 10,000. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5: Effect of the Highest Profile, Officer-Involved Fatality on Log(Property Crime)

	DD	Pre-trend
	(1)	(2)
Panel A: Property Crime		
Treat*Post	0.053**	0.061**
	(0.022)	(0.030)
Pretrend Test		0.016
		(0.021)
Panel B: Motor Vehicle Theft		
Treat*Post	0.079***	0.099***
	(0.029)	(0.038)
Pretrend Test		0.040
		(0.032)
Panel C: Burglary		
Treat*Post	0.039	0.046
	(0.027)	(0.034)
Pretrend Test		0.014
		(0.029)
Panel D: Theft		
Treat*Post	0.051**	0.056
	(0.026)	(0.035)
Pretrend Test		0.010
		(0.023)
Observations	740,838	740,838
Number of Agencies	2,739	2,739

Coefficients are from double difference (DD) regressions, plotted in Figure 9. Standard errors, clustered at the agency level, in parentheses. Uses only city police departments with less than 9 outliers and a population greater 10,000. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.2 Spillover Analysis of Police Departments in the Same County as the Involved Agency

Table B.6: Spillover Analysis of Agencies in the Same County on Log(Violent Crime Arrests)

	DD	Pre-trend	DDD	Pre-trend
	(1)	(2)	(3)	(4)
Panel A: Violent Crime Arrests				
Treat*Post	0.014	0.013	0.040	0.051*
	(0.010)	(0.013)	(0.028)	(0.031)
Pretrend Test		-0.002		0.023
		(0.011)		(0.028)
Spillover*Treat*Post			-0.035	-0.055*
			(0.028)	(0.032)
Spillover Pretrend Test				-0.040
				(0.029)
Panel B: Murder Arrests				
Treat*Post	0.002	0.002	0.025	0.016
	(0.004)	(0.004)	(0.022)	(0.024)
Pretrend Test		-0.002		-0.017
		(0.004)		(0.028)
Spillover*Treat*Post			-0.026	-0.017
			(0.022)	(0.024)
Spillover Pretrend Test				0.018
				(0.028)
Panel C: Aggravated Assault Arrests				
Treat*Post	0.012	0.017	0.018	0.030
	(0.008)	(0.012)	(0.027)	(0.033)
Pretrend Test		0.011		0.023
		(0.014)		(0.031)
Spillover*Treat*Post			-0.010	-0.022
			(0.027)	(0.034)
Spillover Pretrend Test				-0.023
				(0.032)
Observations	741,532	741,532	741,532	741,532
Number of Counties	967	967	967	967

Coefficients are from double (DD) and triple difference (DDD) regressions, where Treat is now defined at the county level and Spillover references departments that reside in the same county as the involved department. Standard errors, clustered at the agency level, in parentheses. Uses only city police departments with less than 9 outliers and a population greater 10,000. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends, which is interacted with the spillover dummy for the DDD. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.7: Spillover Analysis of Agencies in the Same County on Log(Property Crime Arrests)

	DD	Pre-trend	DDD	Pre-trend
	(1)	(2)	(3)	(4)
Panel A: Property Crime Arrests				
Treat*Post	-0.010	-0.011	-0.047*	-0.032
	(0.014)	(0.015)	(0.027)	(0.030)
Pretrend Test		-0.002		0.029
		(0.011)		(0.020)
Spillover*Treat*Post			0.028	0.008
			(0.029)	(0.032)
Spillover Pretrend Test				-0.038
				(0.023)
Panel B: Burglary Arrests				
Treat*Post	-0.021*	-0.024**	-0.031	-0.045
	(0.011)	(0.011)	(0.034)	(0.039)
Pretrend Test		-0.006		-0.029
		(0.011)		(0.030)
Spillover*Treat*Post			0.020	0.038
			(0.038)	(0.041)
Spillover Pretrend Test				0.036
				(0.031)
Panel C: Theft Arrests				
Treat*Post	-0.015	-0.019	-0.087**	-0.077*
	(0.013)	(0.016)	(0.036)	(0.041)
Pretrend Test		-0.009		0.021
		(0.013)		(0.021)
Spillover*Treat*Post			0.066*	0.046
			(0.038)	(0.042)
Spillover Pretrend Test				-0.038
				(0.024)
Observations	741,532	741,532	741,532	741,532
Number of Counties	967	967	967	967

Coefficients are from double (DD) and triple difference (DDD) regressions, where Treat is now defined at the county level and Spillover references departments that reside in the same county as the involved department. Standard errors, clustered at the agency level, in parentheses. Uses only city police departments with less than 9 outliers and a population greater 10,000. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends, which is interacted with the spillover dummy for the DDD. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.8: Spillover Analysis of Agencies in the Same County on Log(Low MB Arrests)

	DD	Pre-trend	DDD	Pre-trend
	(1)	(2)	(3)	(4)
Panel A: Low Marginal Benefit Arrests				
Treat*Post	-0.045**	-0.064***	-0.087*	-0.114**
	(0.018)	(0.021)	(0.053)	(0.055)
Pretrend Test		-0.039***		-0.054**
		(0.015)		(0.027)
Spillover*Treat*Post			0.064	0.081
			(0.054)	(0.054)
Spillover Pretrend Test				0.034
				(0.029)
Panel B: Disorderly Conduct Arrests				
Treat*Post	-0.029*	-0.045***	-0.129*	-0.145**
	(0.016)	(0.017)	(0.068)	(0.069)
Pretrend Test		-0.033**		-0.032
		(0.014)		(0.038)
Spillover*Treat*Post			0.106*	0.113*
			(0.062)	(0.065)
Spillover Pretrend Test				0.014
				(0.039)
Panel C: Marijuana Possession Arrests				
Treat*Post	-0.038	-0.069*	-0.166***	-0.199***
	(0.030)	(0.042)	(0.058)	(0.073)
Pretrend Test		-0.061**		-0.067
		(0.029)		(0.047)
Spillover*Treat*Post			0.150**	0.158**
			(0.063)	(0.073)
Spillover Pretrend Test				0.017
				(0.044)
Observations	741,708	741,708	741,708	741,708
Number of Counties	967	967	967	967

Coefficients are from double (DD) and triple difference (DDD) regressions, where Treat is now defined at the county level and Spillover references departments that reside in the same county as the involved department. Standard errors, clustered at the agency level, in parentheses. Uses only city police departments with less than 9 outliers and a population greater 10,000. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends, which is interacted with the spillover dummy for the DDD. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.9: Spillover Analysis of Agencies in the Same County on Log(Violent Crime)

	DD	Pre-trend	DDD	Pre-trend
	(1)	(2)	(3)	(4)
Panel A: Murder				
Treat*Post	0.002	0.002	0.149***	0.149***
	(0.004)	(0.004)	(0.039)	(0.042)
Pretrend Test		-0.002		0.001
		(0.004)		(0.049)
Spillover*Treat*Post			-0.151***	-0.160***
			(0.039)	(0.041)
Spillover Pretrend Test				-0.018
				(0.049)
Panel B: Aggravated Assault				
Treat*Post	0.012	0.017	0.069**	0.088***
	(0.008)	(0.012)	(0.027)	(0.032)
Pretrend Test		0.011		0.037
		(0.014)		(0.030)
Spillover*Treat*Post			-0.051*	-0.077**
			(0.031)	(0.036)
Spillover Pretrend Test				-0.051
				(0.034)
Panel C: Robbery				
Treat*Post	-0.000	-0.006	0.134***	0.124***
	(0.011)	(0.011)	(0.031)	(0.035)
Pretrend Test		-0.012		-0.019
		(0.011)		(0.029)
Spillover*Treat*Post			-0.136***	-0.151***
			(0.029)	(0.034)
Spillover Pretrend Test				-0.030
				(0.030)
Observations	741,532	741,532	741,532	741,532
Number of Counties	967	967	967	967

Coefficients are from double (DD) and triple difference (DDD) regressions, where Treat is now defined at the county level and Spillover references departments that reside in the same county as the involved department. Standard errors, clustered at the agency level, in parentheses. Uses only city police departments with less than 9 outliers and a population greater 10,000. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends, which is interacted with the spillover dummy for the DDD. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

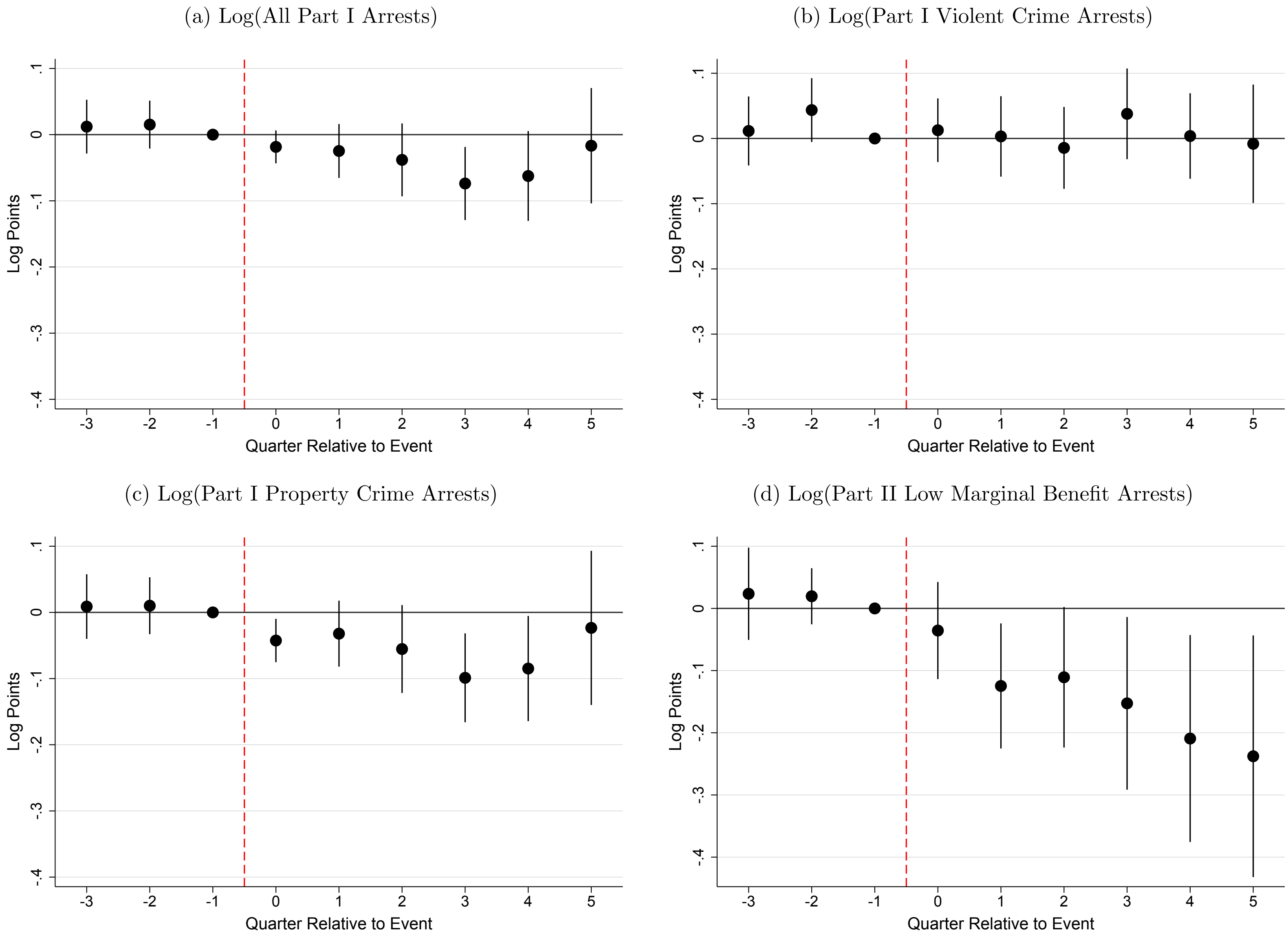
Table B.10: Spillover Analysis of Agencies in the Same County on Log(Property Crime)

	DD	Pre-trend	DDD	Pre-trend
	(1)	(2)	(3)	(4)
Panel A: Motor Vehicle Theft				
Treat*Post	0.022	0.026	0.097***	0.126***
	(0.017)	(0.017)	(0.033)	(0.041)
Pretrend Test		0.008		0.058*
		(0.007)		(0.033)
Spillover*Treat*Post			-0.106***	-0.136***
			(0.036)	(0.047)
Spillover Pretrend Test				-0.058
				(0.037)
Panel B: Burglary				
Treat*Post	-0.021*	-0.024**	0.043	0.048
	(0.011)	(0.011)	(0.030)	(0.036)
Pretrend Test		-0.006		0.009
		(0.011)		(0.032)
Spillover*Treat*Post			-0.046	-0.066
			(0.033)	(0.041)
Spillover Pretrend Test				-0.039
				(0.038)
Panel C: Theft				
Treat*Post	-0.015	-0.019	0.028	0.028
	(0.013)	(0.016)	(0.024)	(0.034)
Pretrend Test		-0.009		-0.000
		(0.013)		(0.024)
Spillover*Treat*Post			-0.029	-0.049
			(0.029)	(0.041)
Spillover Pretrend Test				-0.039
				(0.037)
Observations	741,532	741,532	741,532	741,532
Number of Counties	967	967	967	967

Coefficients are from double (DD) and triple difference (DDD) regressions, where Treat is now defined at the county level and Spillover references departments that reside in the same county as the involved department. Standard errors, clustered at the agency level, in parentheses. Uses only city police departments with less than 9 outliers and a population greater 10,000. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends, which is interacted with the spillover dummy for the DDD. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

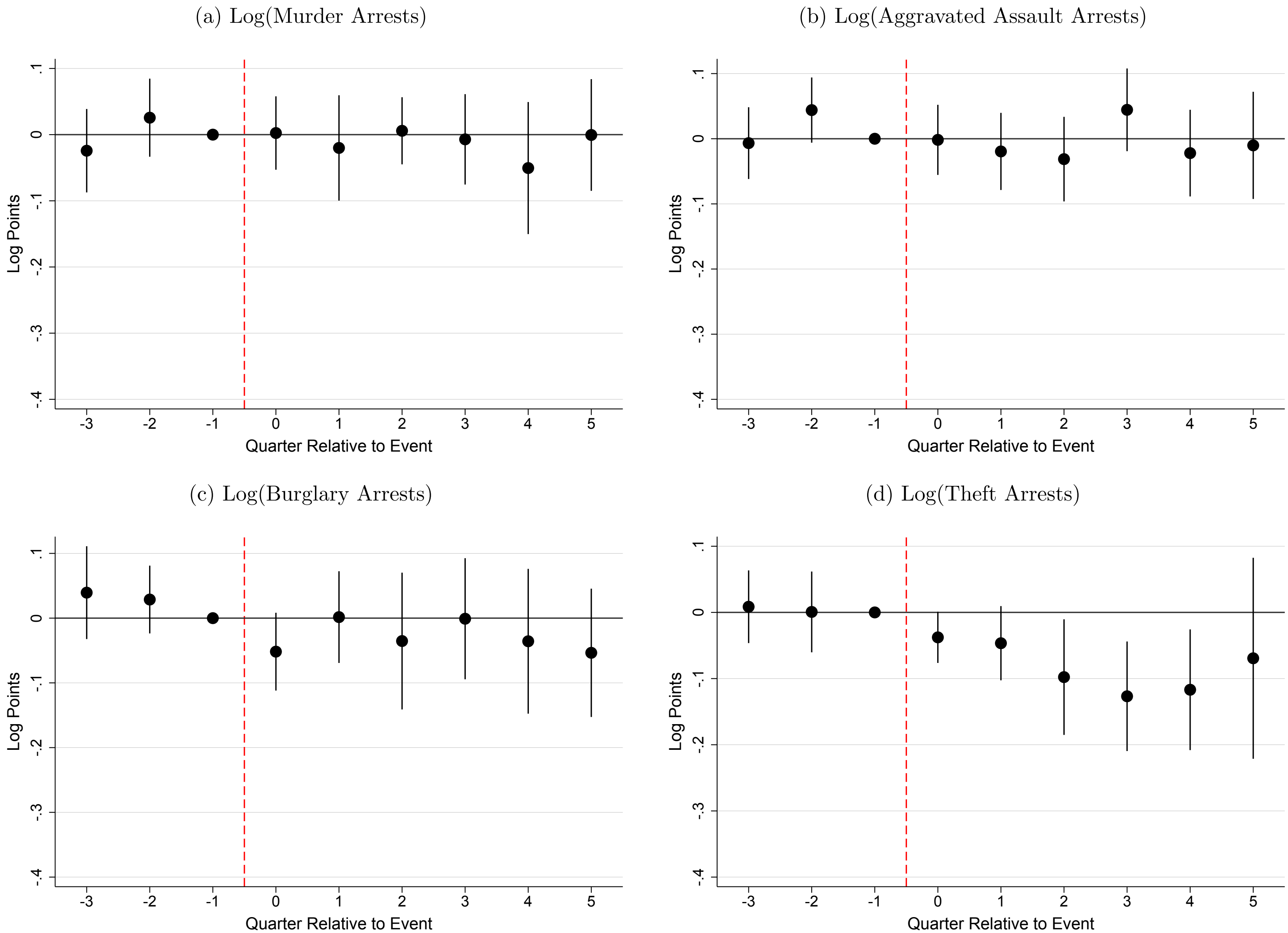
B.3 Arrest Analysis with Crime Controls: Effect of High-Profile, Officer-Involved Fatality on Arrests

Figure B.3: Effect of High-Profile, Officer-Involved Fatality on Arrests with Crime Controls



Using a sample of city police departments with over 10,000 people and fewer than 9 outliers from 2005–2016, the event time coefficients (circles) right of the red dotted line are the change in arrests after a high-profile, officer-involved fatality. Lines are the 95% confidence interval using standard errors clustered at the department level. Each regression controls for population, violent and property crime, and where possible, the specific offense. All regressions have month-of-sample and department fixed effects, as well as linear county trends. [598,910 observations; 52 treated; 2,687 control agencies]

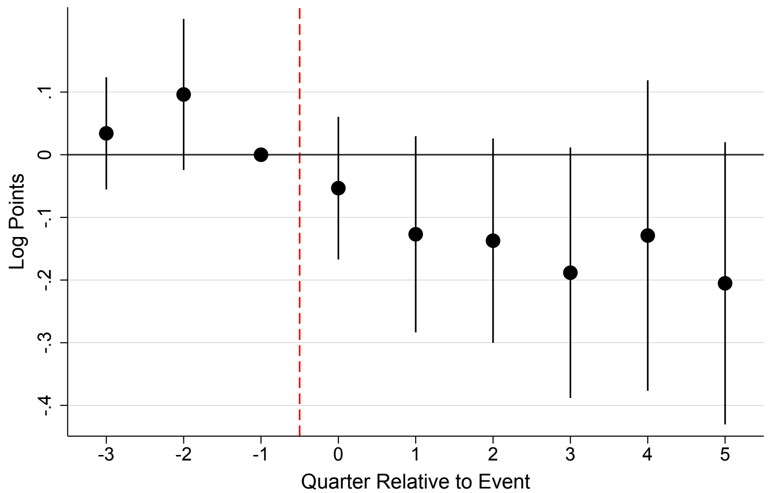
Figure B.4: Effect of High-Profile, Officer-Involved Fatality on Violent and Property Crime Arrests with Crime Controls



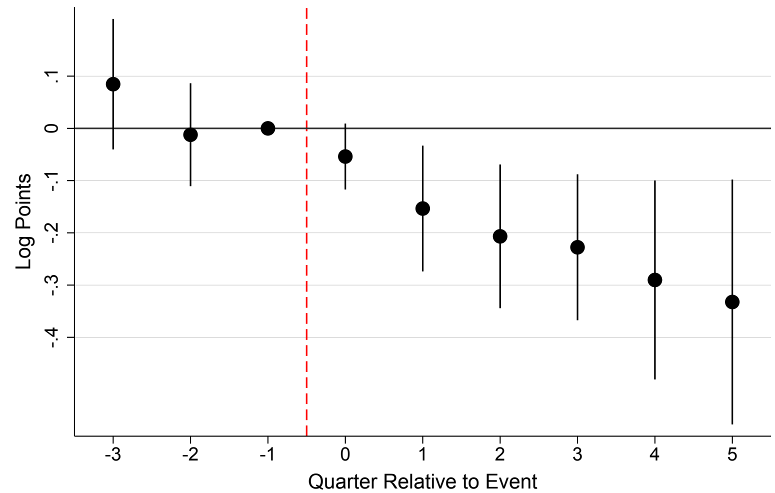
Using a sample of city police departments with over 10,000 people and fewer than 9 outliers from 2005–2016, the event time coefficients (circles) right of the red dotted line are the change in arrests after a high-profile, officer-involved fatality. Lines represent the 95% confidence interval using standard errors clustered at the department level. Each regression controls for population, violent and property crime, and their specific offense type. All regressions have month-of-sample and department fixed effects, as well as linear county trends. [598,910 observations; 52 treated; 2,687 control agencies]

Figure B.5: Effect of High-Profile, Officer-Involved Fatality on Low Marginal Benefit Arrests with Crime Controls

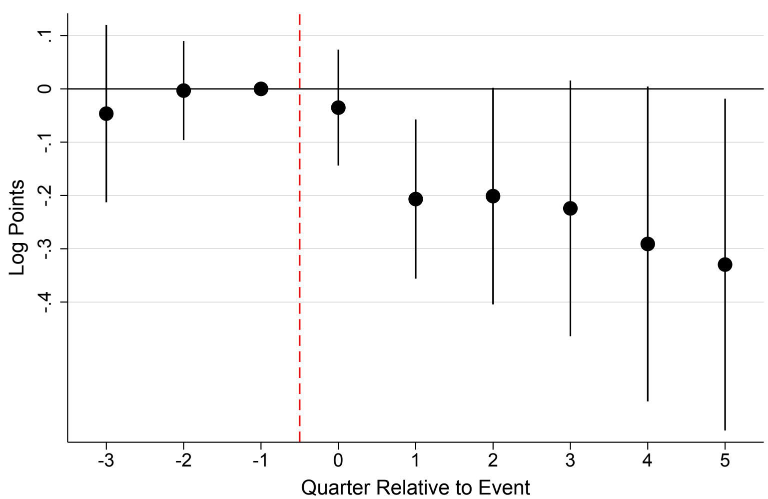
(a) Log(Disorderly Conduct Arrests)



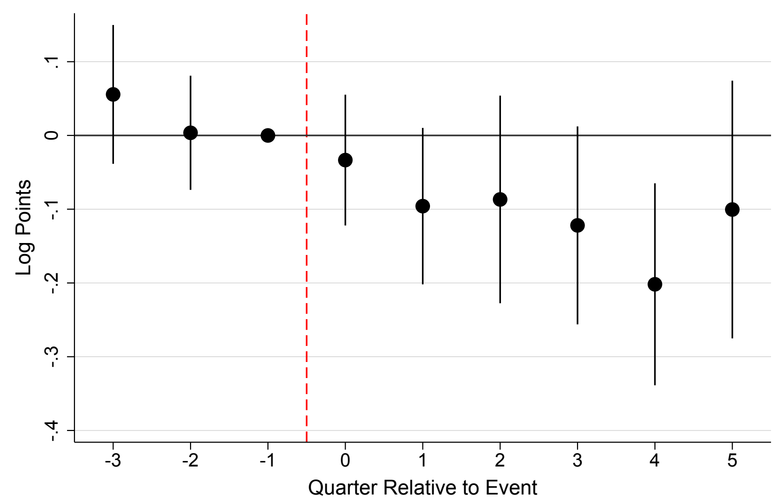
(b) Log(Marijuana Possession Arrests)



(c) Log(Liquor Law Violation Arrests)



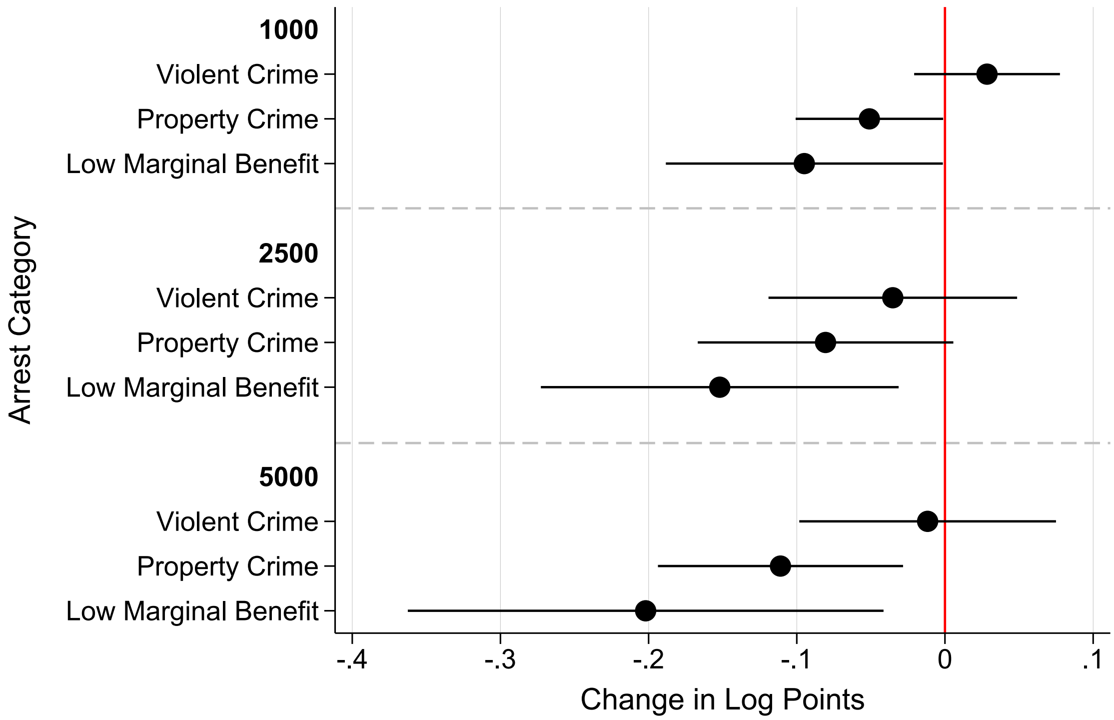
(d) Log(Marijuana Sale Arrests)



Using a sample of city police departments with over 10,000 people and fewer than 9 outliers from 2005–2016, the event time coefficients (circles) right of the red dotted line are the change in arrests after a high-profile, officer-involved fatality. Lines represent the 95% confidence interval using standard errors clustered at the department level. Each regression controls for population, and violent and property crime. All regressions have month-of-sample and department fixed effects, as well as linear county trends. [599,088 observations; 52 treated; 2,687 control agencies]

B.4 Effect Robustness and Heterogeneity by Media Article Threshold

Figure B.6: Difference-in-differences (DD) Coefficient Estimates by Arrest Category and Media Article Threshold



Using different thresholds for what fatalities are considered high profile and included in the analysis, the circles display DD coefficients from separate regressions—in descending order of the social cost of crime. There is a significant difference between the average pre-treatment trends of control and treated departments for property crime arrests using the 1000 article threshold, but no others. I use a sample of city police departments with fewer than 9 outliers and a population greater 10,000. Lines represent the 95% confidence interval using standard errors clustered at the department level. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. The arrest DD tables by threshold begin on the next page. The number of officer-involved fatalities used in analysis by threshold: [1,000: 72; 2,500: 32; 5,000: 15]

Table B.11: Effect of the Highest-Profile, Officer-Involved Fatality on Log(Violent Crime Arrests) by Media Coverage Threshold

	1000	2500	5000
	(1)	(2)	(3)
Panel A: Violent Crime Arrests			
Treat*Post	0.029	-0.035	-0.012
	(0.026)	(0.043)	(0.044)
Panel B: Murder Arrests			
Treat*Post	0.017	0.031	0.035
	(0.019)	(0.032)	(0.055)
Panel C: Aggravated Assault Arrests			
Treat*Post	0.001	-0.066	-0.003
	(0.028)	(0.046)	(0.044)
Panel D: Robbery Arrests			
Treat*Post	0.027	0.009	-0.031
	(0.024)	(0.032)	(0.043)
Observations	741,532	741,532	741,532
Number of Agencies	2,739	2,739	2,739

Coefficients are from double difference (DD) regressions, using different thresholds for what fatalities are considered high profile [1,000: 72 fatalities; 2,500: 32; 5,000: 15]. There are no significant differences between the average pre-treatment trends of control and treated departments in any these regressions. Standard errors, clustered at the agency level, in parentheses. Uses only city police departments with less than 9 outliers and a population greater 10,000. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.12: Effect of the Highest Profile, Officer-Involved Fatality on Log(Property Crime Arrests) by Media Coverage Threshold

	1000	2500	5000
	(1)	(2)	(3)
Panel A: Property Crime Arrests‡			
Treat*Post	-0.051**	-0.081*	-0.111***
	(0.025)	(0.044)	(0.042)
Panel B: Motor Vehicle Theft Arrests‡			
Treat*Post	-0.010	-0.057	-0.004
	(0.040)	(0.077)	(0.067)
Panel C: Burglary Arrests			
Treat*Post	-0.051	-0.003	0.081
	(0.031)	(0.059)	(0.075)
Panel D: Theft Arrests			
Treat*Post	-0.074**	-0.154**	-0.251***
	(0.034)	(0.063)	(0.084)
Observations	741,532	741,532	741,532
Number of Agencies	2,739	2,739	2,739

‡There is a significant difference between the average pre-treatment trends of control and treated departments for property crime and motor vehicle theft arrests using the 1000 article threshold, but no others.

Coefficients are from double difference (DD) regressions, using different thresholds for what fatalities are considered high profile [1,000: 72 fatalities; 2,500: 32; 5,000: 15]. Standard errors, clustered at the agency level, in parentheses. Uses only city police departments with less than 9 outliers and a population greater 10,000. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.13: Effect of the Highest Profile, Officer-Involved Fatality on Log(Low MB Arrests) by Media Coverage Threshold

	1000	2500	5000
	(1)	(2)	(3)
Panel A: Low Marginal Benefit Arrests			
Treat*Post	-0.095**	-0.152**	-0.202**
	(0.048)	(0.062)	(0.082)
Panel B: Marijuana Sale Arrests			
Treat*Post	-0.133***	-0.084	-0.065
	(0.042)	(0.069)	(0.053)
Panel C: Disorderly Conduct Arrests			
Treat*Post	-0.140**	-0.239*	-0.193*
	(0.066)	(0.123)	(0.101)
Panel D: Marijuana Possession Arrests			
Treat*Post	-0.141**	-0.168*	-0.296**
	(0.055)	(0.099)	(0.137)
Observations	741,708	741,708	741,708
Number of Agencies	2,739	2,739	2,739

Coefficients are from double difference (DD) regressions, using different thresholds for what fatalities are considered high profile [1,000: 72 fatalities; 2,500: 32; 5,000: 15]. There are no significant differences between the average pre-treatment trends of control and treated departments in any these regressions. Standard errors, clustered at the agency level, in parentheses. Uses only city police departments with less than 9 outliers and a population greater 10,000. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.14: Effect of the Highest-Profile, Officer-Involved Fatality on Log(Violent Crime) by Media Coverage Threshold

	1000	2500	5000
	(1)	(2)	(3)
Panel A: Violent Crime			
Treat*Post	0.087***	0.110**	0.132**
	(0.027)	(0.052)	(0.062)
Panel B: Murder			
Treat*Post	0.144***	0.192***	0.267***
	(0.036)	(0.038)	(0.052)
Panel C: Aggravated Assault			
Treat*Post	0.041*	0.053	0.114**
	(0.024)	(0.045)	(0.051)
Panel D: Robbery			
Treat*Post	0.125***	0.147***	0.132*
	(0.030)	(0.050)	(0.068)
Observations	741,532	741,532	741,532
Number of Agencies	2,739	2,739	2,739

Coefficients are from double difference (DD) regressions, using different thresholds for what fatalities are considered high profile [1,000: 72 fatalities; 2,500: 32; 5,000: 15]. There are no significant differences between the average pre-treatment trends of control and treated departments in any these regressions. Standard errors, clustered at the agency level, in parentheses. Uses only city police departments with less than 9 outliers and a population greater 10,000. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.15: Effect of the Highest Profile, Officer-Involved Fatality on Log(Property Crime) by Media Coverage Threshold

	1000	2500	5000
	(1)	(2)	(3)
Panel A: Property Crime			
Treat*Post	0.053**	0.036*	0.028
	(0.022)	(0.021)	(0.024)
Panel B: Motor Vehicle Theft			
Treat*Post	0.079***	0.063*	0.026
	(0.029)	(0.034)	(0.044)
Panel C: Burglary			
Treat*Post	0.039	0.086**	0.118**
	(0.027)	(0.036)	(0.047)
Panel D: Theft‡			
Treat*Post	0.051**	0.002	-0.014
	(0.026)	(0.026)	(0.038)
Observations	741,532	741,532	741,532
Number of Agencies	2,739	2,739	2,739

‡There is a significant difference between the average pre-treatment trends of control and treated departments for theft using the 5000 article threshold, but no others.

Coefficients are from double difference (DD) regressions, using different thresholds for what fatalities are considered high profile [1,000: 72 fatalities; 2,500: 32; 5,000: 15]. Standard errors, clustered at the agency level, in parentheses. Uses only city police departments with less than 9 outliers and a population greater 10,000. Each regression controls for population. All regressions have month-of-sample and department fixed effects, as well as linear county trends. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.