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Public cooperation and the police: Do calls-for-service increase after homicides?

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ABSTRACT

Calls-for-service represent the most basic form of public cooperation with the police. How cooperation varies as a function of instances of police activity remains an open question. The great situational diversity of police activity in the field, matching the situational diversity of crime and disorder, makes it challenging to estimate causal effects. Here we use homicides as an indicator for the occurrence of a standardized set of highly visible, socially-intensive, acute police investigative activities and examine whether police calls-for-service change in response. We adopt a place-based difference-in-differences approach that controls for local fixed affects and common temporal trends. Estimates of the model using data from Los Angeles in 2019 shows that calls-for-service increase significantly in the week following a homicide. The effect pertains to both violent crime and quality of life calls for service. Partitioning the data by race-ethnicity shows that calls-for-service increase most when the homicide victim is Black. Partitioning the data by race-ethnicity and type of homicide shows that some types of calls are suppressed when the homicide is gang-related. The results point to opportunities for police to build trust in the immediate aftermath of homicides, when the public is reaching out for greater assistance.

1. Introduction

Public cooperation is central to the modern policing model (Jackson et al., 2012; Sampson & Bartusch, 1998). Public cooperation with the police, defined broadly, concerns any action undertaken to help police to secure public safety as a common benefit. Without individual and community cooperation, very few crimes would come to the attention of the police and very few crimes would ever be solved (Bottomley & Coleman, 1981; Braga, Turchan, & Barao, 2019; Eck, 1983; Leovy, 2015). While community trust may increase cooperation with the police (Tyler, 2005), it is apparently not essential for it. As discussed by Baumer (2002), individuals who mistrust the police may still turn to them for help if they lack other viable alternatives for responding to crime and disorder (see also Anderson, 1999; Black, 1976; Kääriäinen & Sirén, 2011; Rosenfeld, Jacobs, & Wright, 2003).

Emerging evidence seems to suggest that cooperation with police is strongly context dependent. It is often difficult to tease apart the effects of trust, defined as belief in the legitimacy and reliability of police, from situational conditions that drive cooperation (Murphy & Barkworth, 2014). Trust in the police varies across several key dimensions including the race-ethnicity of the individuals interacting with police, their past experiences with the criminal justice system and neighborhood disadvantage (Brunson & Miller, 2005). And trust in police is not itself a unidimensional quantity (Stoutland, 2001). Trust can be low because policing is perceived to be invasive and heavy-handed (i.e., over-policing) (Brunson, 2007; Goffman, 2014). It also can be low because policing is perceived to be absent when needed, or ambivalent and ineffectual when present (i.e., under-policing) (Anderson, 1999; Brunson & Wade, 2019; Leovy, 2015). However, low trust may be overridden by the seriousness of the immediate crime (Gottfredson & Hindelang, 1979), or counterbalanced by procedurally just actions of police officers during an encounter (Murphy, Mazerolle, & Bennett, 2014). As noted by Desmond et al. (2016, p. 859) “attitudes toward the criminal justice system might be poor, even misleading, indicators of their real-life dealings with the police.”

Desmond et al. (2016) see crime reporting as the most basic form of cooperation with police. Yet it remains challenging to estimate the effect of situational police actions on willingness of people to report crime. Desmond et al. (2016) used a high-profile instance of police violence against an unarmed Black man in Milwaukee, Wisconsin, in 2004 to examine the impact on police-calls-for service. Consistent with expectations, calls-for-service declined (particularly in Milwaukee’s Black

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neighborhoods) after the details of the case became widely known (but see Zoorob, 2020). Not surprisingly, such high-profile events can erode trust, increase legal cynicism (Kirk & Papachristos, 2011), and reduce cooperation with the police. Presumably, many smaller injustices can accumulate to much the same effect (Carr, Napolitano, & Keating, 2007).

The case explored by Desmond et al. (2016) represents the impact of just one high-profile event on subsequent police calls-for-service. It leaves open the question of the impact of more routine types of police action. Here we examine a type of repeated, but intense and highly visible police action where we might expect subsequent cooperation with the police to vary based on community and event context. Specifically, we treat homicide events as an indicator of instances of acute, socially intense police investigative activity localized to a given place and time. Using publically-available data from Los Angeles, CA, we evaluate whether police calls-for-service change significantly in response to the repeated occurrence homicides and the policing that those homicides entail. We hypothesize that calls-for-service should decrease in the aftermath of a homicide if the associated policing activity erodes trust or, more generally, creates the impression that calling the police is not a viable or worthwhile option. Conversely, calls-for-service should increase in the aftermath of a homicide, if policing activity builds trust, or improves the position of police in the community as a viable source of support.

The challenge with evaluating these hypotheses is that it is generally impossible to know if calls-for-service would have gone up or down naturally in the absence of a given homicide. In other words, we cannot observe both the outcome under the extant situational conditions and what *would have happened* if those situational conditions were different. To circumvent this challenge, we adapt the potential outcomes framework developed by Ridgway et al. (2019) in their study of gang injunctions in Los Angeles to the present case. Their approach compared the effect of the treatment condition (i.e., injunctions) in small geographic units (i.e., LAPD reporting districts), in pre- and post-treatment periods, with nearby geographic units as controls. The approach also assessed whether there were geographic spillover effects from treatment such as the displacement of crime or diffusion of crime control benefits (see also Hesseling, 1994; Ridgeway, Grogger, Moyer, & MacDonald, 2019; Telep, Weisburd, Gill, Vitter, & Teichman, 2014). Here we use homicide events as an indicator marking the treatment condition (i.e., acute, socially intensive and highly visible policing) and examine the subsequent effects on police calls-for-service, our measure of cooperation. We evaluate treatment effects for several different types of calls for service including violent crime calls (e.g., aggravated assault), property crime calls (e.g., burglary) and quality of life calls (e.g., "loud party"). We also evaluate treatment effects for homicides broken down by the race-ethnicity of the victim and type of homicide (i.e., domestic and gang-related), which may provide further insight into differences in trust in and cooperation with police across different communities and situational contexts. In particular, we see the ecological distinctions between domestic and gang-related homicides as useful for teasing apart the potential effects of policing and the fear of crime, which may also mediate calls-for-service (Klinger & Bridges, 1997).

The remainder of this paper proceeds as follows. The second section discusses the nature of police response to homicide events. We make the argument that the magnitude and regularity of the police response qualifies it as an intervention that can used as a quasi-experimental treatment condition. The third section introduces the study area and data used. We concentrate on homicides and calls-for-service occurring in Los Angeles in 2019. The fourth section outlines the potential outcomes approach used in analysis. Here we follow closely the identification strategy used by Ridgeway et al. (2019). The fifth section turns to the results of analysis. The final section discusses implications of the findings and highlights certain limitations.

2. Policing in response to a homicide

The question addressed below is how police activity impacts the public's willingness to cooperate with the police. We use calls-for-service as the most basic evidence of public cooperation (see Desmond et al., 2016). The central challenge is how to quantify police activity in such a way as to assess the impact on calls-for-service.

Police strategic and tactical behavior is highly variable in part because it is so situationally dependent (Groff et al., 2015; Kuang, Brantingham, & Bertozzi, 2017; Sherman, Gartin, & Buerger, 1989). Two calls-for-service that appear similar on the surface (e.g., simple batteries) may be radically different in the participants (e.g., male-male, male-female) and settings involved (e.g., street-based, domestic). The upshot is that different instances of policing cannot be easily treated as equivalent interventions, even for the same apparent crime type. The impact of police activity on future community cooperation (as well as crime) is therefore difficult to estimate beyond simple presence-absence effects.

One exception may be policing surrounding homicide events. We posit that policing in these instances is relatively standardized, socially-intensive and acute (Braga et al., 2019; Carter & Carter, 2016; Hough, McCorkle, & Harper, 2019; Leovy, 2015). Homicides trigger a standardized sequence of actions on the part of police. Homicide victims are usually discovered by someone other than the police (e.g., a family member). A call to the police by this third party leads to uniform patrol involvement. One or more patrol units verifies a death has occurred (or appears possible for a critically injured victim), which triggers an additional sequence of events. A uniformed supervisor, or command officer assumes command of the crime scene and homicide detectives are assigned. At this point, the number of involved police personnel grows considerably. For example, as part of a parallel research study, we examined the crime scene logs for 16 LAPD homicide files for the period 2000–2010 from South Los Angeles Divisions. Over this period, homicide scenes saw a mean of 44.3 (sd = 21.3) officials involved from different city agencies. This included a mean 31.1 (sd = 16.4) uniformed officers, 7.1 (sd = 4.5) plain-clothes detectives, and between 15 and 25 LAPD vehicles. A mean of 6.1 (sd = 4.9) additional personnel from paramedic services or the fire department and the coroner's office were also typically present. We expect these estimates to remain representative in 2019 as there has not been a substantive change in policy governing homicide investigations.

The actions taken at a homicide are strictly regulated, with clear recognition that every action may eventually prove critical in later legal proceedings. Uniformed police personnel are directed to immediately establish a perimeter around the crime scene, protect visible evidence, institute crowd-control measures for curious onlookers, start a crime scene log to record all personnel present at the scene, complete field interviews with all potential witnesses, start "door-knocking" for additional witnesses, immediately search for and secure known suspects, diagram the surrounding area and note all parked vehicles, interview ambulance personnel, and retrieve incident and call-for-service histories for the location. All of these actions occur within the first several hours after the homicide is reported.

Investigation begins with the arrival of detectives, who assume command, oversee crime scene processing, and direct uniformed officers in investigative tasks (e.g., field interviews with potential witnesses). Following the initial activity at the crime scene, detectives become the primary source of engagement with the public, which includes gathering additional evidence and interviewing witnesses and suspects. Typically, homicide investigations start with a high rate of active engagement with the public, peaking in the first few days following the event. The rate of public activity declines after several days, as the investigation switches to analysis of evidence and in-depth interviews with specific witnesses and suspects, often away from the crime scene (e.g., in a police station). Ultimately, homicide cases may be cleared by arrest or some other exceptional means (Wellford & Cronin, 2000), or remain open if unsolved. Many cases are solved quickly if the suspect remained at the scene (e.g., domestic homicides). Others are so-called "whodunits" with no immediate suspect and few initial leads (e.g., gang-related drive-by shooting). In Los Angeles, the average homicide clearance rate stands at approximately 70%, while the national average is around 60%.

We consider the socially intense and acute nature of policing following a homicide as akin to a treatment intervention. We can therefore speak of pre- and post-treatment periods centered around any homicide event. Over the course of a year, there may be many such interventions, each with pre- and post-treatment periods. Pre-treatment conditions may correspond to a baseline set of expectations. Police presence may be perceived as desirable, unwanted, or neutral by the community. Calls-for-service in the community may occur at a baseline rate consistent with general community attitudes (but see Baumer, 2002). The socially intensive policing process following a homicide may have a neutral effect, build or erode trust, with a corresponding neutral, positive or negative effect on calls-for-service. Studies have shown that public opinion of police is generally higher when there is more (informal) interaction with police (Maxson, Hennigan, & Sloane, 2003).

3. Identification and estimation

The policing procedures surrounding any one homicide are relatively standardized in Los Angeles, which means that treatment conditions are very similar across a collection of homicides occurring in different locations at different times. This fact allows the use of a difference-in-differences model to estimate the impact of policing on future cooperation of the public. Here we follow the identification strategy used by Ridgeway et al. (2019) to estimate the average treatment effect on the treated (ATT) LAPD reporting districts as well as on adjacent reporting districts, where we might expect to see some spillover effects. Reporting districts are described in more detail in the next section.

Following Ridgeway et al. (2019), let $i = 1, 2, \ldots, N$ be an index corresponding to each unique homicide occurring in the time period of observation. Below i will index the 254 homicides occurring in Los Angeles in 2019. Let $d = 1, 2, \ldots, M$ be an index for each unique reporting district (RD) in Los Angeles. In 2019, there were a total of 1135 RDs spanning the whole city. Let t be a measure of time relative to the date of occurrence of homicide i in RD d. In the present case, we will focus on police calls-for-service aggregated by week of the year, maximally from 1 to 52. Thus, $t = 0$ corresponds to the week in which the homicide occurs in RD d. The pre-treatment period is marked by $t < 0$ and the post-treatment period by $t \geq 0$. Now we define three different types of observational states. Let $D_{id} = 1$ if homicide i occurred in RD d. Let $S_{id} = 1$ if RD d is adjacent to the RD in which homicide i occurred. For completeness, let $C_{id} = 1$ if RD d is a second-order neighbor of the RD in which homicide i occurred.

RDs marked by $D_{id} = 1$ are treatment units that experienced the socially intensive policing process triggered by a homicide. It is possible that this socially intensive process may impact more than the RD in which the homicide occurs, building or eroding trust in adjacent RDs. Thus, $S_{id} = 1$ marks RDs that might experience a spillover effects. Finally, $C_{id} = 1$ marks RDs that are close enough to homicide i to be ecologically similar, but not so close that we expect spillover effects from the policing intervention associated with homicide i.

We now define the potential outcomes of interest. Again following Ridgeway et al. (2019), let $Y_{idt}(D,S)$ be the calls-for-service volume associated with homicide i in RD d at time t, measured in relative time. In this notation, $Y_{idt}(1,0)$ is the potential outcome associated with being a treatment RD, $Y_{idt}(0,1)$ is the potential outcome associated with being a spillover RD, and $Y_{idt}(0,0)$ is the potential outcome associated with the absence of treatment (i.e., control). The ATT for treatment and spillover RDs are given by (Ridgeway et al., 2019, p. 525):

$$ATT_D = E[Y_{idt}(1,0)\,|D = 1] - E[Y_{idt}(0,0)\,|D = 1] \tag{1}$$

$$ATT_S = E[Y_{idt}(0,1)\,|S = 1] - E[Y_{idt}(0,0)\,|S = 1] \tag{2}$$

Eq. 1 states that the average treatment effect on the treated is the difference between the expected volume of calls in treatment RDs given that they received treatment and the expected volume of calls in the treatment RDs given that they did not received treatment. Eq. 2 makes the same statement about spillover RDs. Eqs. 1 and 2 thus underscore the fundamental causal inference problem (Imbens & Rubin, 2015). The first term in each equation is the expected volume of calls in an RD given that a homicide occurred and is represented by the observed data. By contrast, the second term in each equation is not observable. It is the expected volume of calls that *would have happened* if the homicide had not occurred. This is the counterfactual condition.

However, as noted by Ridgeway et al. (2019, p. 525), if we assume that the RDs in question would follow parallel trends in the absence of treatment, then we can use a difference-in-differences model to estimate the ATT for treatment and spillover RDs. Specifically, Ridgeway et al. (2019, p. 525) write:

$$\begin{aligned}\Delta_D = {} & E[Y_{idt}(1,0)\,|D_{id} = 1, t \geq 0] - E[Y_{idt}(0,0)\,|D_{id} = 1, t < 0] \\ & -(E[Y_{idt}(0,0)\,|D_{id} = 0, S_{id} = 0, t \geq 0] - E[Y_{idt}(0,0)|D_{id} = 0, S_{id} = 0, t < 0])\end{aligned} \tag{3}$$

$$\begin{aligned}\Delta_S = {} & E[Y_{idt}(0,1)\,|S_{id} = 1, t \geq 0] - E[Y_{idt}(0,0)\,|S_{id} = 1, t < 0] \\ & -(E[Y_{idt}(0,0)\,|D_{id} = 0, S_{id} = 0, t \geq 0] - E[Y_{idt}(0,0)|D_{id} = 0, S_{id} = 0, t < 0])\end{aligned} \tag{4}$$

Eq. 3 subtracts the difference in expected call volume during pre- and post-homicide periods in control RDs (i.e., $Y_{idt}(0,0) \mid D_{id} = 0,\ S_{id} = 0$) from the difference in expected call volume during pre- and post-homicide periods in homicide treatment RDs (i.e, $Y_{idt}(1,0) \mid D_{id} = 1$). Eq. 4 does the same for spillover RDs. Importantly, the difference-in-differences estimator includes terms that can be estimated given only observable data. It is possible therefore to use OLS regression to estimate causal effects (Ridgeway et al., 2019, p. 526; see also Wing, Simon, & Bello-Gomez, 2018):

$$Y_{idt} = \beta_1 D_{id} 1(t \geq 0) + \beta_2 S_{id} 1(t \geq 0) + \gamma X_{it} + \mu_{id} + \epsilon_{idt} \tag{5}$$

where Y_{idt} is the observed call volume associated with homicide i in RD d at relative time t. The terms D_{id} and S_{id} are, respectively, indicators (dummy coded variables) that take values of 1 if RD d is the site of homicide i, or adjacent to it, and 0 otherwise. The terms $1(t \geq 0)$ are indicator variables that take values of 1 if it is the post-homicide period, and 0 otherwise. The term μ_{id} captures fixed effects for each RD that are stationary in time. For example, land-use variation and static demographic differences between RDs are modeled by μ_{id}. Time varying effects, common to all RDs, are captured by γ, where $X_{it}$is a vector of indicators that map relative time t to calendar time (e.g., $t = 0$ in RD d may be week 37 in calendar time). Thus, γ may capture the gradual evolution of legal cynicism. The uncorrelated error is estimated by ϵ_{idt}.

The main parameters of interest are β_1 and β_2, which correspond to ATT_D and ATT_S, respectively. The estimates from the regression control for stable differences between RDs and for global temporal trends common to all RDs. Fixed differences between RDs may include different baseline levels of legal cynicism, collective efficacy and crime, all of which can affect the baseline likelihood that people call the police. Global temporal trends might include seasonal and secular variation in crime that impacts the likelihood that people call the police.

4. Study area and data

Los Angeles, CA, is a diverse city of approximately 3.89 million people with dozens of distinct communities. Census figures from 2019 indicate that 48.6% of the population is Latino, 28.5% White, 8.9% Black, and 11.6% Asian. Approximately 38% of households have a median household income of $50,000 or less, while 11% have a median household income of $200,000 or more. The city covers 469 mile^2 (1215

Table 1
Volume of police-calls-for service in 2019 by categorical group and type

Group	N	% Subgroup	% Grand Total
Violent Crime			
Robbery	12,990	9.5	2.3
Battery	64,856	47.2	11.5
ADW	51,706	37.6	9.1
Shots Fired	7877	5.7	1.4
Total	137,429	100.0	24.3
Property Crime			
Burglary	46,153	61.3	8.2
Theft	28,099	37.3	5.0
GTA	1004	1.3	0.2
Total	75,256	100.0	13.3
Quality of Life			
Intoxication	13,357	3.8	2.4
Disturbance	163,149	46.3	28.9
Minor Disturbance	64,924	18.4	11.5
Vandalism	18,691	5.3	3.3
Dispute	84,797	24.1	15.0
Screaming	7522	2.1	1.3
Total	**352,440**	**100.0**	**62.4**
Grand Total	**565,125**	**–**	**100.0**

sq. km) divided into several major subregions. These subregions are reflected in the geographic organization of the Los Angeles Police Department (LAPD). The Department is divided into four Bureaus covering the San Fernando Valley (Valley Bureau), West Los Angeles (West Bureau), Downtown and East Los Angeles (Central Bureau) and South Los Angeles (South Bureau). The Bureaus are further subdivided into twenty-one patrol Divisions. Each patrol division includes multiple neighborhoods. Patrol divisions are divided into Basic Car Areas that are patrolled by a single patrol unit staffed by two uniformed officers. Larger divisions may have as many as six or seven basic car areas. Finally, at the smallest geographic scale is a Reporting District (RD). RDs are similar in size to census block-groups and are organized to have roughly the same number of people. Thus, RDs tend to be larger in areas of the city with lower population density (e.g., Hollywood Hills) and smaller in high density areas (median = 0.26 mile^2 [0.67 km^2]; inter-quartile range = 0.30 mile^2 [0.78 km^2]). RDs are larger than the micro-geographic footprint of a homicide crime scene, which may be limited to a single street address or street segment. However, knowledge about a homicide may extend farther afield than the crime scene itself over local social networks. As a unit of analysis, RDs may provide a conservative estimate of effect size that balances the acute, local nature of policing surrounding homicides and the impact on broader community cooperation with police.

A city of the size and diversity of Los Angeles generates a massive number of police calls-for-service. It also generates a large number of homicides. Here we analyze open-source data made available by the City of Los Angeles (see https://data.lacity.org and https://geohub.lacity.org). In 2019, the LAPD recorded 1,095,430 calls, or 3001 calls per day on average. These include both public- and police-initiated calls. We drop all calls that were initiated by the police, as these are indicative of police discretion, rather than active cooperation by members of the public. We only focus on those calls related to violent and property crime as well as "quality of life" disorder. The specific call types and volume of calls in each of these groupings are listed in Table 1. We include these three broad call groups because they potentially reflect different reporting constraints. Observed violence crime may induce a greater sense of obligation to call the police than observed social disorder. Thus, changes in quality of life calls for service may be more indicative of changes in voluntary cooperation compared with violent crime calls. It is not immediately clear how property crime calls should respond. We do not exclude possible duplicate calls related to the same event. Our goal is the analysis of cooperation with the police following homicide events, rather than the crime and disorder being reported. Thus, we seek to capture the difference between a single "shots fired" event that generates five calls to the police and the counterfactual that this single event generated (for example) only one call to the police. The former nominally reflects a greater level of cooperation with the police than the latter.

Table 2
Los Angeles homicides in 2019 by race-ethnicity of victim

Race-Ethnicity of Victim	N	%	Population %	Rate per 100 k
Latino	119	46.9	48.6	6.2
White	24	9.4	28.5	2.1
Black	102	40.2	8.9	28.8
Asian	2	0.8	11.6	0.4
Other	7	2.8	2.4	7.3
Total	254	100.0	100.0	6.4

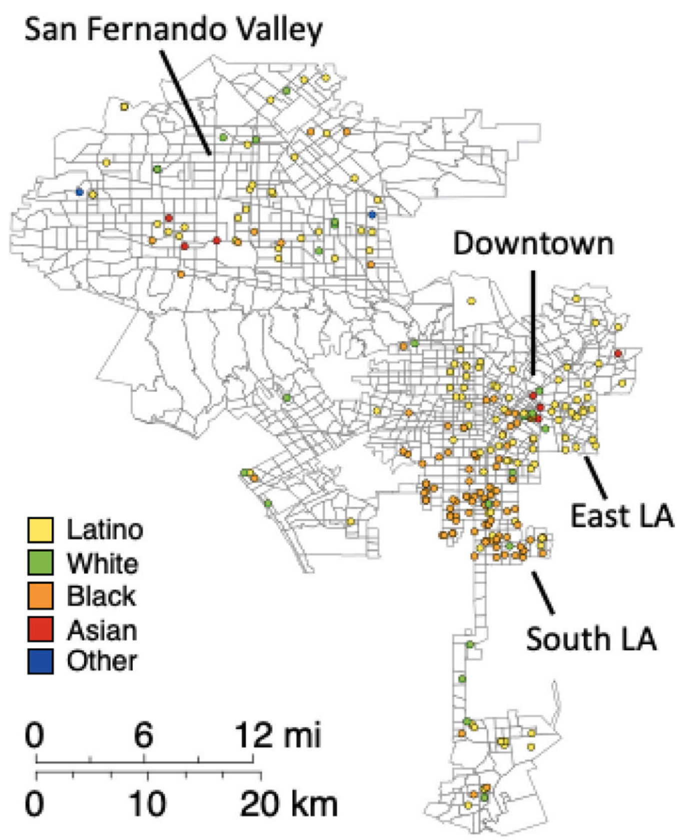

Fig. 1. Spatial distribution of homicides by race of victim in 2019. Latinx (green), White (blue), Black (red). (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

These three categories make up just over half of all calls-for-service (51.6%). Across the three categories, quality of life calls make up the majority of calls fielded by the LAPD (62.4% of the total). Violent crime calls are next most abundant (24.3%), followed by property crime calls (13.3% of calls). Specific types of crime and disorder dominate within each of the categories. Nearly two-thirds of quality of life calls are recorded as minor (e.g., "loud party" or "fireworks"), or major disturbances (e.g., "group with gun" or "woman with knife").

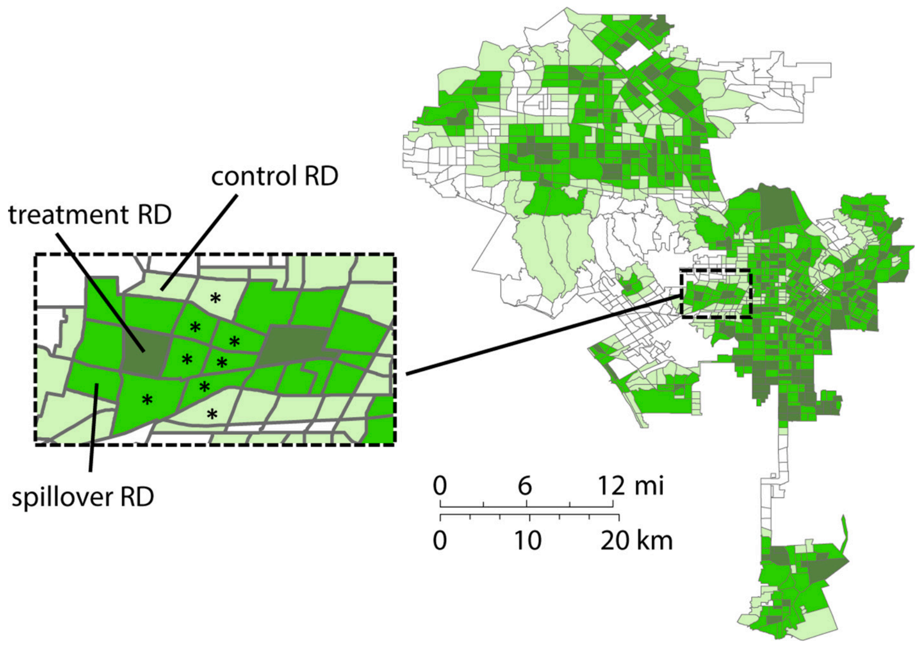

Fig. 2. Map of LAPD with treatment (dark green), spillover (medium green) and control (light green) Reporting Districts (RDs) indicated. Treatment status is determined by a homicide occurring within the RD. Spillover and control RDs are first- and second-order neighbors of treatment RDs. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Table 2 reports summary statistics for homicides in 2019 by race-ethnicity of the victim based on open-source data.[1] There were 254 homicides in 2019, nearly 77% below the peak in 1992 ($N = 1092$) and the lowest since 2013 ($N = 251$). The victimization rate across racial-ethnic groups varies substantially. Latino victims represent 46.9% of homicides and 48.6% of the population, based on 2019 US Census figures. White victims represent 9.4% of homicides, but make up 28.5% of the population. Black victims represent 40.2% of homicides, but make up 8.9% of the population. The unequal distribution of homicide by race-ethnicity extends to the geography of events. While no community is immune from homicide, there are areas where homicides are much more concentrated (Fig. 1). For example, LAPD's South Bureau accounted for 101 (39%) of the homicides in 2019 though it represents 12.3% of the land area and approximately 16.1% of the population.

5. Short-term effects model

The model specified by Eq. (5) requires that we define both observational time windows t around each homicide i, to encompass pre- and post-homicide periods, and the RDs d that include each homicide, and adjacent spillover and control units. Ideally, the observational units should be defined such that the effects of each homicide i on calls-for-service are independent of all other homicides. That is, an observational time window and collection of RDs associated with homicide i should not overlap with those associated with homicide j.

Here we aggregate calls-for-service data to week number. Inspection of the data for all 254 homicides that occurred in 2019 reveals potential sources of interference for time windows of different lengths. Restricting the analyses to a three-week interval, which includes one week prior to and one week after the focal week in which a homicide occurs (i.e., $t = -1, 0, 1$), yields the largest possible sample size, with limited spatial overlap in units. Homicides occurring in the same treatment RD within a two-week buffer before and after the focal week are dropped. A total of 49 homicides were dropped under these constraints, including 27 homicides falling within the same week and 22 homicides within the proscribed buffer period in the same RD. We evaluate the impact of excluding homicides in the Appendix. Homicides occurring within the buffer period, but in RDs that are first- or second-order neighbors of one another are retained using the following rules. Treatment RDs (D) are retained over spillover RDs (S), and spillover RDs are retained over control (C) RDs. For example, if RD d was the site of homicide i, but was also a second-order neighbor homicide j, then d's role as a treatment RD is retained and its role as a control RD is dropped. If RDs are spillover (S) or control (C) units for more than one homicide in the same calendar week, then one role is retained at random. For example, if RD d is a post-treatment second-order neighbor for homicide i in calendar week 27 (i.e., $1(t \geq 0)$ and $C_{id} = 1$) and is also a pre-treatment second-order neighbor for homicide j in calendar week 27 (i.e., $1(t < 0)$ and $C_{jd} = 1$), then one of these roles is retained and the other dropped at random. This produces a weakly balanced panel with a stable number of observations, but an unequal number of time points per RD in some instances. Overall, none of the homicide treatment units ($D = 1$) are missing time points; that is, all homicide treatment RDs are guaranteed to have observations for weeks $t = -1, 0, 1$, measured in relative time. Approximately 6.6% ($n = 253$ of 3810 possible) of spillover RDs ($S = 1$) had one instance of a pair randomly dropped to avoid overlapping roles. Approximately 15.1% ($n = 1062$ of 7056 possible) of control RDs ($C = 1$) has one instance of a pair randomly dropped to avoid overlapping roles.

Fig. 2 maps the RD locations of all 205 homicides retained in the dataset and their associated spillover and control RDs. The inset in Fig. 2 shows the spatial arrangement of RDs around two separate homicides that occurred on 11/18/19 and 12/14/19. The overlapping neighbors are marked with stars. In this example, the overlapping RDs are all retained because the two homicides occurred more than two weeks apart. The role played by the same RD is allowed to switch. The overlapping RDs shown Fig. 2 function as spillover units, when included in the analysis of the first homicide, and as control units when included as

[1] There is a discrepancy in the total number homicides in 2019 reported in the City of Los Angeles public data ($n = 254$), the City's annual crime report ($n = 253$), released in February of 2020 (http://lapdonline.org/statistical_data/pdf_view/66185), and in the FBI's Uniform Crime Reports ($n = 258$). The discrepancy between the public data and annual crime report can be explained by one late reporting homicide. The discrepancy with the UCR is difficult to rectify, but may relate to differences in nonnegligent homicide.

Table 3
Calls-for-service summary statistics by RD type and time period

	$t = -1$		$t = 0$		$t = 1$	
All Calls	mean	sd	mean	sd	mean	sd
D	15.5	11.0	17.2	11.3	15.8	11.2
S	12.6	9.7	13.0	9.7	12.7	9.8
C	11.4	9.0	11.4	9.0	11.5	9.1
Violent Calls						
D	4.6	4.3	5.8	4.5	4.5	4.0
S	3.5	3.4	3.7	3.7	3.4	3.4
C	3.1	3.3	3.1	3.4	3.2	3.4
Property Calls						
D	1.7	1.8	1.7	1.9	1.6	1.8
S	1.5	1.7	1.4	1.6	1.5	1.8
C	1.3	1.6	1.3	1.6	1.3	1.5
Quality Calls						
D	9.2	6.7	9.7	7.1	9.7	7.3
S	7.6	6.2	7.9	6.1	7.8	6.2
C	6.9	5.6	6.9	5.6	7.0	5.7

part of the second homicide. By way of example, if these homicides had occurred within two weeks of one another, then each overlapping first- and second-order neighbor would have been retained at random and included in the analysis with only one or the other homicide. Thus, RD d might appear as $S = 1$ and $t = -1, 0, 1$ for the first homicide, but $C = 1$ and $t = 0, 1$ for the other; the pre-treatment instance of RD d serving as control was randomly dropped in favor of a post-treatment role as a spillover RD. Importantly, if improved cooperation with the police, or legal cynicism becomes entrenched following any one homicide, then the switching of roles of RDs across homicides would work against observable treatment effects.

Table 3 reports the mean and standard deviation in calls-for-service in homicide (D), spillover (S) and control (C) RDs by relative time measured in weeks (i.e., $t = -1, 0, 1$). The main conclusion for the sample under study is that calls for service are consistently higher in RDs that experience a homicide (D) compared with spillover (S) and control (C) RDs and consistently higher in spillover RDs compared with control. However, there is no obvious trend between pre- and post-homicide periods.

6. Results

Table 4 presents the results of the short-term potential outcomes model broken down by call type. There is an increase in calls-for-service (ATT_D) for all call types combined of 1.38 calls per week in RDs that experience a homicide. This estimate is significant when using conventional standard errors, robust standard errors (HC3), or standard errors clustered by LAPD Basic Car areas and Divisions (see Ridgeway

Table 4
Estimated short-term effect of a homicide on weekly calls-for-service

		Conventional		Robust (HC3)		Cluster Basic Car		Cluster Division		Cluster Bureau	
	Estimate	SE	*p*-value	SE	p-value	SE	p-value	SE	p-value	SE	p-value
All Calls											
ATT_D†	1.38	0.25	<0.001	0.33	<0.001	0.33	<0.001	0.31	<0.001	0.60	0.10
ATT_S	0.08	0.11	0.46	0.12	0.5	0.13	0.54	0.11	0.46	0.07	0.32
Violent Calls											
ATT_D	0.85	0.12	<0.001	0.16	<0.001	0.17	<0.001	0.18	<0.001	0.37	0.10
ATT_S	−0.06	0.06	0.26	0.06	0.25	0.07	0.31	0.04	0.12	0.03	0.09
Property Calls											
ATT_D	0.07	0.07	0.35	0.08	0.40	0.07	0.35	0.07	0.35	0.07	0.40
ATT_S	0.01	0.03	0.67	0.03	0.73	0.03	0.78	0.04	0.79	0.03	0.76
Quality of Life Calls											
ATT_D	0.46	0.18	0.01	0.23	0.05	0.23	0.05	0.19	0.03	0.20	0.10
ATT_S	0.14	0.09	0.11	0.09	0.14	0.09	0.12	0.07	0.05	0.03	0.02

† ATT_D and ATT_S are the average treatment effect on the treated RDs (D) and displacement RDs (S), respectively.

Table 5
Estimated short-term effect of a homicide on weekly calls-for-service including homicide type

		Conventional		Robust (HC3)		Cluster Basic Car		Cluster Division		Cluster Bureau	
All Calls	Estimate	SE	p-value	SE	p-value	SE	p-value	SE	p-value	SE	p-value
ATT_D†	1.38	0.24	<0.001	0.33	<0.001	0.33	<0.001	0.31	<0.001	0.60	0.11
ATT_S	0.08	0.11	0.48	0.12	0.50	0.13	0.54	0.11	0.46	0.07	0.32
Domestic	−0.10	0.25	0.69	0.34	0.77	0.30	0.73	0.33	0.76	0.29	0.75
Gang-related	−0.16	0.11	0.16	0.13	0.23	0.16	0.31	0.22	0.47	0.19	0.45
Violent Calls											
ATT_D	0.85	0.12	<0.001	0.16	<0.001	0.17	<0.001	0.18	<0.001	0.37	0.10
ATT_S	−0.07	0.06	0.23	0.06	0.25	0.07	0.31	0.04	0.12	0.03	0.08
Domestic	−0.06	0.13	0.65	0.13	0.66	0.13	0.67	0.14	0.70	0.08	0.52
Gang-related	0.07	0.06	0.23	0.06	0.23	0.06	0.26	0.05	0.18	0.02	0.05
Property Calls											
ATT_D	0.07	0.07	0.33	0.08	0.40	0.07	0.35	0.07	0.35	0.07	0.40
ATT_S	0.01	0.03	0.73	0.03	0.74	0.03	0.79	0.04	0.80	0.03	0.78
Domestic	0.11	0.07	0.13	0.08	0.17	0.08	0.15	0.06	0.08	0.08	0.25
Gang-related	−0.04	0.03	0.24	0.03	0.24	0.03	0.25	0.04	0.30	0.02	0.16
Quality of Life Calls											
ATT_D	0.46	0.18	0.01	0.23	0.05	0.23	0.05	0.19	0.03	0.20	0.10
ATT_S	0.14	0.08	0.11	0.09	0.14	0.09	0.12	0.07	0.05	0.03	0.02
Domestic	−0.15	0.19	0.42	0.29	0.59	0.25	0.54	0.34	0.65	0.38	0.71
Gang-related	−0.19	0.09	0.03	0.10	0.05	0.11	0.09	0.17	0.28	0.19	0.39

† ATT_D and ATT_S are the average treatment effect on the treated RDs (D) and displacement RDs (S), respectively.

Table 6
Estimated short-term effect of a homicide on weekly calls-for-service by race-ethnicity of the victim

Latino		Conventional		Robust (HC3)		Cluster Basic Car		Cluster Division		Cluster Bureau	
All Calls	Estimate	SE	p-value	SE	p-value	SE	p-value	SE	p-value	SE	p-value
ATT_D†	1.28	0.34	<0.001	0.47	0.01	0.46	0.01	0.34	0.001	0.60	0.12
ATT_S	0.17	0.15	0.26	0.18	0.34	0.19	0.38	0.13	0.22	0.11	0.22
Violent Calls											
ATT_D	0.69	0.16	<0.001	0.19	<0.001	0.21	<0.001	0.25	0.01	0.45	0.22
ATT_S	−0.03	0.07	0.68	0.07	0.69	0.07	0.69	0.06	0.61	0.05	0.59
Property Calls											
ATT_D	0.02	0.10	0.87	0.10	0.87	0.09	0.85	0.11	0.88	0.06	0.80
ATT_S	−0.01	0.04	0.81	0.05	0.82	0.04	0.81	0.04	0.80	0.04	0.79
Quality of Life Calls											
ATT_D	0.57	0.26	0.03	0.39	0.14	0.38	0.13	0.29	0.06	0.15	0.03
ATT_S	0.21	0.12	0.07	0.13	0.11	0.14	0.14	0.10	0.04	0.03	0.01
White											
All Calls											
ATT_D	0.24	1.09	0.83	0.98	0.81	0.99	0.81	1.11	0.84	1.66	0.90
ATT_S	−1.23	0.55	0.03	0.55	0.03	0.58	0.03	0.66	0.07	0.35	0.03
Violent Calls											
ATT_D	0.24	0.54	0.66	0.59	0.68	0.61	0.69	0.55	0.67	0.99	0.83
ATT_S	−0.55	0.27	0.04	0.29	0.06	0.34	0.11	0.49	0.28	0.38	0.24
Property Calls											
ATT_D	0.08	0.33	0.80	0.39	0.83	0.36	0.82	0.36	0.82	0.29	0.79
ATT_S	−0.11	0.16	0.51	0.16	0.51	0.15	0.37	0.15	0.38	0.23	0.60
Quality of Life Calls											
ATT_D	−0.08	0.82	0.92	0.72	0.91	0.73	0.91	0.89	0.92	1.29	0.95
ATT_S	−0.57	0.41	0.16	0.43	0.18	0.37	0.11	0.28	0.05	0.16	0.03
Black											
All Calls											
ATT_D	2.39	0.47	<0.001	0.61	<0.001	0.64	<0.001	0.38	<0.001	0.42	0.01
ATT_S	0.17	0.23	0.47	0.26	0.52	0.27	0.53	0.21	0.43	0.18	0.41
Violent Calls											
ATT_D	1.28	0.25	<0.001	0.30	<0.001	0.31	<0.001	0.16	<0.001	0.26	0.02
ATT_S	−0.04	0.13	0.74	0.14	0.76	0.16	0.79	0.15	0.77	0.02	0.11
Property Calls											
ATT_D	0.16	0.13	0.22	0.17	0.34	0.14	0.24	0.05	0.01	0.03	0.01
ATT_S	0.06	0.07	0.35	0.07	0.34	0.07	0.34	0.06	0.33	0.07	0.44
Quality of Life Calls											
ATT_D	0.95	0.34	0.01	0.37	0.01	0.38	0.02	0.28	0.003	0.17	0.01
ATT_S	0.15	0.17	0.39	0.19	0.44	0.15	0.33	0.16	0.35	0.12	0.29

† ATT_D and ATT_S are the average treatment effect on the treated RDs (D) and displacement RDs (S), respectively.

et al., 2019). The result is not significant when clustered at the Bureau scale. Similar patterns are seen for violent crime and quality of life calls, but not for property crime calls. Violent crime calls increase by 0.85 calls per week per RD with a homicide, while quality of life calls increase by 0.46 calls per week. Property crime calls do not increase significantly in RDs experiencing a homicide.

There do not appear any spillover effects for any of the call types (ATT_S) (Table 4). RDs adjacent to those experiencing a homicide do not see a statistically significant increase or decrease in calls-for-service, a result consistent across different approaches for computing standard

Table 7
Estimated short-term effect of a homicide on weekly calls-for-service for Latino victims by homicide type

Latino		Conventional		Robust (HC3)		Cluster Basic Car		Cluster Division		Cluster Bureau	
All Calls	Estimate	SE	p-value	SE	p-value	SE	p-value	SE	p-value	SE	p-value
ATT_D†	1.28	0.34	<0.001	0.47	0.01	0.46	0.01	0.34	0.001	0.60	0.12
ATT_S	0.17	0.15	0.27	0.17	0.34	0.19	0.38	0.13	0.22	0.11	0.22
Domestic	−0.01	0.39	0.98	0.42	0.98	0.48	0.98	0.48	0.98	0.07	0.90
Gang-related	−0.03	0.16	0.87	0.17	0.88	0.19	0.89	0.26	0.92	0.16	0.87
Violent Calls											
ATT_D	0.69	0.16	<0.001	0.19	<0.001	0.21	0.001	0.25	0.01	0.45	0.22
ATT_S	−0.03	0.07	0.68	0.07	0.69	0.07	0.69	0.06	0.61	0.05	0.58
Domestic	0.13	0.18	0.47	0.19	0.48	0.24	0.58	0.34	0.70	0.05	0.08
Gang-related	0.13	0.07	0.07	0.07	0.06	0.07	0.05	0.07	0.06	0.02	0.01
Property Calls											
ATT_D	0.02	0.10	0.88	0.10	0.87	0.09	0.86	0.11	0.89	0.06	0.80
ATT_S	−0.01	0.04	0.79	0.05	0.80	0.04	0.79	0.04	0.78	0.04	0.78
Domestic	0.06	0.11	0.62	0.11	0.63	0.12	0.64	0.09	0.53	0.04	0.29
Gang-related	−0.14	0.05	0.003	0.04	0.001	0.04	0.002	0.05	0.02	0.02	0.01
Quality of Life Calls											
ATT_D	0.57	0.26	0.03	0.39	0.14	0.38	0.13	0.29	0.06	0.15	0.03
ATT_S	0.21	0.12	0.07	0.13	0.11	0.14	0.14	0.10	0.05	0.03	0.01
Domestic	−0.20	0.30	0.51	0.31	0.53	0.31	0.53	0.22	0.37	0.07	0.06
Gang-related	−0.02	0.12	0.87	0.14	0.88	0.15	0.89	0.17	0.90	0.16	0.90

† ATT_D and ATT_S are the average treatment effect on the treated RDs (D) and displacement RDs (S), respectively.

Table 8
Estimated short-term effect of a homicide on weekly calls-for-service for Black victims by homicide type

Black		Conventional		Robust (HC3)		Cluster Basic Car		Cluster Division		Cluster Bureau	
All Calls	Estimate	SE	p-value	SE	p-value	SE	p-value	SE	p-value	SE	p-value
ATT_D†	2.36	0.47	<0.001	0.61	<0.001	0.63	<0.001	0.39	<0.001	0.42	0.01
ATT_S	0.14	0.23	0.54	0.26	0.58	0.27	0.58	0.22	0.51	0.19	0.49
Domestic	−0.07	0.74	0.92	0.74	0.92	0.51	0.88	0.50	0.88	0.23	0.77
Gang-related	−0.81	0.29	0.01	0.31	0.01	0.33	0.02	0.14	<0.001	0.15	0.01
Violent Calls											
ATT_D	1.28	0.25	<0.001	0.30	<0.001	0.32	<0.001	0.17	<0.001	0.28	0.02
ATT_S	−0.04	0.13	0.77	0.14	0.79	0.16	0.82	0.14	0.80	0.01	0.07
Domestic	−0.40	0.40	0.32	0.40	0.32	0.33	0.23	0.37	0.29	0.11	0.04
Gang-related	0.01	0.16	0.94	0.17	0.95	0.19	0.95	0.18	0.95	0.23	0.96
Property Calls											
ATT_D	0.16	0.13	0.24	0.17	0.35	0.14	0.26	0.05	0.01	0.03	0.01
ATT_S	0.06	0.07	0.39	0.07	0.38	0.07	0.38	0.06	0.36	0.07	0.48
Domestic	0.28	0.21	0.19	0.23	0.23	0.18	0.13	0.10	0.01	0.07	0.03
Gang-related	−0.06	0.08	0.49	0.07	0.43	0.06	0.37	0.07	0.40	0.07	0.49
Quality of Life Calls											
ATT_D	0.92	0.34	0.01	0.37	0.01	0.38	0.02	0.27	0.004	0.16	0.01
ATT_S	0.12	0.17	0.47	0.19	0.51	0.16	0.42	0.16	0.45	0.13	0.40
Domestic	0.05	0.53	0.92	0.47	0.91	0.33	0.88	0.28	0.86	0.19	0.81
Gang-related	−0.77	0.21	<0.001	0.20	<0.001	0.22	0.001	0.21	0.002	0.19	0.03

† ATT_D and ATT_S are the average treatment effect on the treated RDs (D) and displacement RDs (S), respectively.

errors.

Homicide type does not have a strong effect on calls-for-service (Table 5). Eq. (5) was modified to include dummy coded variables for domestic and gang-related homicides (non-domestic, non-gang homicides serve as the reference group). Domestic homicides do not impact significantly any of the call types. Gang-related homicides may exert a small downward pressure equivalent to 0.19 fewer quality of life calls per RD per week.

Table 6 presents the results of separate analyses by the race-ethnicity of the victim. The race-ethnicity of the victim may or may not reflect local community composition. Victims may be killed outside of their neighborhood of residence (Tita & Griffiths, 2005), or may be disproportionately sampled from the local population (see Table 2 for the city-wide picture). We consider the race-ethnicity of the victim to be relevant to the spread of information over the local social network, potentially impacting calls for service via the social context of the homicide. The local racial-ethnic composition of treatment and neighboring RDs is absorbed by the unit fixed-effects in Eq. (5). In general, we see significant effects of a homicide on calls-for-service when the victim is Latino or Black, but not when the victim is white. This may be due to the sample size differences in number of homicides by race-ethnicity (see Table 2). The volume of calls related to all crime and disorder types increases by 1.28 calls per RD per week when the homicide victim is Latino, and by 2.39 calls per RD per week when the victim is Black. Quality of life calls-for-service also increased significantly by 0.95 calls per RD per week when the victim is Black. There is no indication of any spillover effects based on the race-ethnicity of the victim.

Tables 7 and 8 repeat the analysis by race-ethnicity of the victim while also including homicide types. Here we see different patterns from those reported above. For Latino victims, the type of homicide makes little difference in calls-for-service in the period immediately following a homicide. There may be a small increase in violent crime calls of 0.13 calls per RD per week and a small decrease in property crime calls of −0.14 calls per RD per week associated with gang-related homicides. These effects appear to cancel one another out when all call types are combined. By contrast, for Black victims, gang-related homicides have a significant negative impact on calls-for-service. All calls to the police are lower by 0.81 per RD per week in the aftermath of gang-related homicides. Quality of life calls are lower by 0.77 calls per RD per week following gang-related homicides. Violent and property calls are not impacted by homicide type when the victim is Black. After controlling for homicide type, there is still a significant increase for three of the four call types; $ATT_D = 2.36$ for all calls; $ATT_D = 1.28$ for violent calls; $ATT_D = 0.92$ for quality of life calls. As above, there appears to be no spillover effect (i.e., ATT_S is non-significant in all cases).

7. Discussion

The analyses presented here suggest that the socially intensive policing surrounding homicide events tends to increase police calls-for-service over the short term. This observation generally holds for all call types combined, violent crime calls and quality of life calls, but not for property crime calls. The effects are significant, but the effect sizes are small. For all call types combined, the increase of 1.38 calls per week per RD represents an 8.9% increase over the pre-treatment mean of 15.5 calls per week (see Tables 3 and 5). The increase of 0.85 violent crime calls per week per RD represents a 18.5% increase on the pretreatment mean of 4.6 calls per week. The increase of 0.46 quality of life calls per week per RD represents an 5.0% increase on the pretreatment mean of 9.2 calls per week. These increases are not likely to be noticed by police on a day-to-day basis, given the daily volatility in call volumes. Overall, the 205 homicides examined here may have added around 566 calls, or about 0.1% of the city-wide violent crime, property crime and quality of life disorder calls for the year (see Table 1). There is no evidence that the policing surrounding homicides have effects that spread beyond the immediate area into adjoining RDs.

Including information on the type of homicide alone does not substantially alter these general results. Gang-related homicides may entail a slight reduction in quality of life calls. Repeating these analyses broken down by the race-ethnicity of the victim does reveal some important variation. When homicide victims were Latino, overall calls and violent crime calls saw significant increases, but quality of life calls did not. The increases were both below the estimates for the general model with all homicides included (compare Tables 5 and 6). When victims were White, there was no significant change in any call type. When victims were Black, there were significant increases in all calls, violent crime calls and quality of life calls, with all increases above the estimates for the general model.

When homicide type is combined with race-ethnicity of the victim we see that domestic homicides have no significant impact on calls-for-service. By contrast, gang-related homicides have a significant impact on calls-for-service when the homicide victim is Latino or Black. Gang-related homicides that involve a Latino victim push up violent crime calls-for-service, but push down property crime calls by about the same amount. Gang-related crimes that involve a Black victim push down all calls and quality of life calls significantly, but do not have a significant impact on violent crime and property crime calls.

There are three broad interpretations of these results, none of which are mutually exclusive. First, we must recognize that the occurrence of a homicide has the potential to increase the fear of crime, independent of any effects related to the police response to that homicide. Thus, a homicide might impact calls-for-service via changes in the fear of crime alone. We suspect that this is an incomplete answer based on the analyses broken down by race-ethnicity and homicide type (see below). A related explanation might be that awareness of a homicide triggers greater awareness of crime in general, which prompts an increase in calls-for-service. Here the issue may be really about attention limitation (e.g., de Fockert, Rees, Frith, & Lavie, 2001), rather than fear of crime. In general, people have only so-much bandwidth to pay attention to what is going on in their local environment. Knowledge that a homicide occurred may prompt some of that bandwidth to switch (for a time) to tracking crime and disorder. The nature and magnitude of policing might play a limited role in this attention switching. Media coverage might also play a role, especially if it is the primary means by which people become aware of a homicide in their local area.

Alternatively, a homicide might create an awareness of policing, which prompts increased calls-for-service. The idea here is that people are generally aware of the crime and disorder in their local environment, but unaware of the of the viability of different courses of action. A homicide produces a high visibility policing event that "reminds" members of the public of police services, which prompts more calls to the police. This would replace their tendency to ignore or deal with crime in some other way (Langton, Berzofsky, Krebs, & Smiley-McDonald, 2012), at least over the short term. Related is the idea that a display of competence and compassion in the police response to a homicide might enhance the sense that policing is a viable option for dealing with observed crime and disorder. Indeed, in spite of the tragedy surrounding a homicide, competent management and fast resolution of the case might strongly influence people's subjective evaluation of police legitimacy (Leovy, 2015; Tyler, 2005; Vaughn, 2020). In Los Angeles, 77.6% of the 2019 homicides have been cleared (i.e., solved) by the police, which exceeds by a substantial margin the national average clearance rate of around 59%.[2] Calls for service may be expected to increase under such conditions.

The observed patterns in calls-for-service conditioned by the race-ethnicity of the homicide victim aligns with research from crime victimization surveys. Numerous studies have shown that African Americans tend to report crime at higher rates than Latino victims, and Latino victims at rates higher than White victims (Bachman, 1998; Baumer, 2002; Felson, Messner, & Hoskin, 1999; Langton et al., 2012). Here we see a similar rank order structure in the effects of a homicide on calls-for-service. Xie and Lauritsen (2012) found that the social context of a crime further mediates reporting behavior, with assaults involving a Black victim and Black offender being reported at higher rates than other victim-offender pairs. Our case is not directly comparable. We know the race of the victim in focal homicides, but not the race-ethnicity of any of the parties involved in subsequent calls-for-service. In principle, our observed calls-for-service could involve any of the victim-offender pairs (or victim-offender-witness triplets) discussed by Xie and Lauritsen (2012). We might also expect our results to differ based on the specific call context in the manner described by Xie and Lauritsen (2012). In the absence of additional data, the null hypothesis is that the impact for all possible victim-offender pairs in subsequent events is the same.

The impact of homicide type on calls-for-service has important implications. Fear of crime is connected to an individual's perception (rational or irrational) of their own risk of being the victim of a crime. Knowledge that a homicide has occurred nearby may increase such perceptions of risk. We posit, however, that these perceptions are likely to be different based on the type of homicide. In general, we expect that domestic homicides are less likely to increase subjective fear than gang-related homicides. This is because domestic homicides are often "socially contained" events involving the suspect and victim and perhaps an extended family (Goldstein, 1994). They also tend to be much less visible, occurring behind closed doors rather than in public settings. A gang-related homicide may have a much broader social impact. This impact is amplified by the fact that gang-related violent crimes more often to occur in visible public settings, involve strangers and indiscriminate targeting of victims (Maxson, Gordon, & Klein, 1985; Pizarro & McGloin, 2006). Gang-related homicides are also more likely to trigger additional violent crimes compared with non-gang homicides (Brantingham, Yuan, & Herz, 2020). Individuals may be more likely to fear follow-on consequences of a gang-homicide compared to a domestic-homicide. How increased fear translates into calls-for-service might vary. Calls might go up if the perceived protection offered by policing, in relation to an observed crime and disorder problem, outweighs perceived consequences of calling the police (e.g., retribution for "snitching"). The reverse might also hold. The results presented here suggest that all calls, violent calls and quality of life calls are suppressed when the victim of a gang-related homicide is Black. This might indicate that fear of retribution is higher and/or more stringent taboos against cooperation with the police operate in these social contexts following a homicide. The converse may hold, though weakly, for gang-related homicides where the victim is Latino.

It is also possible that the police response to gang-related homicides with Black victims in some way suppresses future calls. This might be the case if the community perceived the police response to homicide devalues the victim (Leovy, 2015). However, current evidence suggests that other situational and event characteristics matter more in solving homicides than the race of the victim (Braga et al., 2019; Regoeczi, 2018; Vaughn, 2020).

8. Limitations

The analyses presented here are limited in several important ways. First, we chose to focus on short-term effects captured in the week following a homicide. We therefore do not know if the observed effects persist. It is challenging to expand the window of observation beyond a single week pre- and post-treatment since doing so will tend to restrict the sample to locations in the city where homicides are rare. The generalizability of the findings might therefore be compromised; that is, how cooperation with the police is impacted in communities where homicides are rare may provide limited guidance on what to expect in communities where homicide is more common. Analyses presented in the Appendix suggest that settings with multiple homicides in the same week do not respond any differently than settings with single-victim homicides. Future work should seek to include cases where homicides occur in consecutive weeks. We might expect consecutive homicides to amplify treatment effects associated with acute, socially intensive policing–analogous to the contagion effects seen with crime (Brantingham et al., 2020; Loeffler & Flaxman, 2017). However, it is not clear whether to expect linear or non-linear compound effects. Publicly available data for additional years should allow exploration of alternative long-term treatment effect models presented by Ridgeway et al. (2019).

The analyses also face certain spatial limitations. Our results concern only the narrow spatial window around homicide events (i.e., census block-group-sized RD). While we do not find evidence for spillover of effects to immediately adjacent areas, this does not address whether there are changes in cooperation (or legal cynicism) at larger spatial scales. Our models do account for secular temporal trends common to all RDs, but this does not exclude possibility that there is also change at neighborhood scales of organization (e.g., Kim & Hipp, 2020). In addition, we note that uncontrolled edge effects may bias our results. Numerous RDs in our analysis border other jurisdictions in Los Angeles

[2] Official clearance rate statistics can be found at https://openjustice.doj.ca.gov/exploration/crime-statistics/crimes-clearances

Table 9
Estimated short-term effect of a multiple homicides in a focal week (M_{id}) on weekly calls-for-service

All Calls	Estimate	Conventional SE	Conventional p-value	Robust (HC3) SE	Robust (HC3) p-value	Cluster Basic Car SE	Cluster Basic Car p-value	Cluster Division SE	Cluster Division p-value	Cluster Bureau SE	Cluster Bureau p-value
ATT_D†	1.47	0.24	<0.001	0.31	<0.001	0.33	<0.001	0.32	<0.001	0.64	0.11
ATT_S	0.11	0.11	0.33	0.11	0.35	0.13	0.41	0.09	0.24	0.06	0.19
Multiple Homicides	−0.07	0.23	0.77	0.24	0.77	0.25	0.78	0.21	0.74	0.22	0.77
Violent Calls											
ATT_D	0.89	0.12	<0.001	0.15	<0.001	0.16	<0.001	0.18	<0.001	0.37	0.10
ATT_S	−0.01	0.05	0.83	0.06	0.84	0.07	0.87	0.04	0.78	0.03	0.76
Multiple Homicides	0.19	0.11	0.08	0.12	0.12	0.12	0.10	0.12	0.13	0.06	0.04
Propety Calls											
ATT_D	0.09	0.07	0.20	0.08	0.26	0.07	0.24	0.07	0.21	0.080	0.355
ATT_S	0.01	0.03	0.67	0.03	0.67	0.03	0.72	0.03	0.71	0.028	0.706
Multiple Homicides	0.01	0.07	0.90	0.08	0.91	0.08	0.91	0.09	0.92	0.031	0.785
Quality of Life Calls											
ATT_D	0.50	0.18	<0.01	0.23	0.03	0.23	0.03	0.20	0.02	0.24	0.14
ATT_S	0.10	0.08	0.21	0.09	0.23	0.08	0.21	0.06	0.10	0.06	0.17
Multiple Homicides	−0.27	0.17	0.11	0.18	0.12	0.20	0.17	0.19	0.16	0.19	0.25

† ATT_D and ATT_S are the average treatment effect on the treated RDs (D) and displacement RDs (S), respectively.

and Ventura Counties. While we find no evidence for displacement effects between RDs *within* our study area, we cannot exclude the possibility that homicides and the attendant policing in nearby settings have not had an effect.

Second, it is difficult for us to evaluate the parallel trends assumption, which is critical to difference-in-differences estimation. This assumption requires that the difference in calls-for-service between treatment and control units would be constant over time in the absence of treatment. The strategy used by Ridgeway et al. (2019), which was to compare pre-treatment trends over multiple periods between treated and untreated units, is not feasible in the present case because of the limited pre-treatment observation window before each homicide. The spatial proximity of control units to treated units may help ensure that calls-for-service follow parallel trends, due to spatial autocorrelation in ecological circumstances. Future work may seek to conduct more systematic balancing of treatment and control units using propensity matching (Ryan, Kontopantelis, Linden, & Burgess, 2019) or synthetic control methods (Abadie, Diamond, & Hainmueller, 2010; Ben-Michael, Feller, & Rothstein, 2018).

Third, we need to be aware of potential violations of the so-called stable unit treatment value assumption (SUTVA), which requires that treatment administered to one unit does not interfere with treatment the treatment in other units. While the absence of spillover effects from homicide RDs to the first-order neighboring RDs provides some confidence that SUTVA holds for spatial interactions, we have not evaluated interactions over time. Indeed, the reason why we selected short observational windows of one week before and one week after a homicide was the recognition that sometimes homicides follow one another closely in space and time (Brantingham et al., 2020). Thus, the effect on calls-for-service of a homicide occurring in one RD might reasonably impact the calls-for-service in the same or nearby RD a few weeks in the future. As above, developing a model to handle compound treatments would be worthwhile.

Fourth, we have assumed that police response to a homicide is relatively standardized. This is plausible based on LAPD policy guidelines which mandate how to secure the crime scene and initiate investigation. No two homicides are identical, however. Thus, we should expect some level of variation in police response based on victim, suspect, location and other situational characteristics (Braga et al., 2019). More detailed information will be needed to quantify such variation and to establish if it impacts patterns of subsequent cooperation with the police.

Finally, we should be clear that we cannot distinguish formally between the two most likely explanations for the general increases in calls-for-service following homicides. It is plausible that police actions in the aftermath of homicide impact community trust. Increases in trust might therefore stimulate increased calls-for-service. It is also plausible that homicides increase fear of crime. Calls-for-service subsequently increase because of an absence of credible alternatives for dealing with crime and disorder (Baumer, 2002; Kirk & Papachristos, 2011). In either case, the empirical increase in calls-for-service following homicide represents *opportunities to build trust*. These are instances where the public in a local area has reached out beyond some baseline level. Engaging with those calls and solving problems can potentially set up a virtuous feedback loop where people remained more inclined to call the police for help.

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Appendix A

The main analyses focused on 205 of the 254 homicides that occurred in Los Angeles in 2019. These were single-victim homicide events with two-week buffer periods before and after the focal week (i.e., $t = 0$). He we assess the impact of including focal weeks with two or more homicides. Weeks with multiple homicides, and single events with multiple homicide victims, may produce larger treatment effects. Of the 49 homicides excluded from the main analyses, 22 occurred within the same week. Eighteen of the 22 homicides were multi-victim events. We modified Eq. (5) to include an indicator variable $M_{id} = 1$ if RDs d was the site of two or more homicides during an observational window $t = -1$, 0, 1. Thus, $M_{id} = 0$ corresponds to the observational windows for each of the 205 single-victim homicide events. Table 9 shows that weeks with multiple homicides do not significantly impact the estimated treatment effects (compare with Table 4). A different model is required to assess whether weeks with consecutive homicides have compounding effect on calls-for-service.

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