Black Lives Matter's Effect on Police Lethal Use-of-Force*

Travis Campbell†

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Abstract

Has Black Lives Matter influenced police lethal use-of-force? A difference-in-differences design finds census places with Black Lives Matter protests experience a 15% to 20% decrease in police homicides over the ensuing five years, around 300 fewer deaths. The gap in lethal use-of-force between places with and without protests widens over these subsequent years and is most prominent when protests are large or frequent. This result holds for alternative specifications, estimators, police homicide datasets, and population screens; however, it does not hold if lethal use-of-force is normalized by violent crime or arrests. Protests also influence local police agencies, which may explain the reduction. Agencies with local protests become more likely to obtain body-cameras, expand community policing, receive a larger operating budget, and reduce the number of property crime-related arrests, but forego some black officer employment and college education requirements.

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†Department of Economics, University of Massachusetts Amherst, 105 Gordon Hall, 412 North Pleasant Street, Amherst, MA 01002. For correspondence, email: tbcampbell@umass.edu.

1. Introduction

Reacting to the acquittal of George Zimmerman for the killing of Trayvon Martin in 2013, Alicia Garza posted her reaction to Facebook:

> "black people. I love you. I love us. Our lives matter."

This post inspired activist Patrisse Cullors to create a viral Twitter tag #blacklivesmatters and, with the help of activist Opal Tometi, Black Lives Matter (BLM) was born. BLM did not transform into the protests movement it is known as today until the police killings of Eric Garner in New York City and Michael Brown in Ferguson, MO in 2014. With the world watching the unrest, BLM would quickly push the issue of police violence towards black people into the fore of American discourse. As BLM became a national movement, the movement garnished controversy from both ailes of American politics for using sometimes violent protest tactics. It was instrumental in publicizing a sequence of high-profile police killings including Tamir Rice, Walter Scott, Sandra Bland, Freddie Gray, Laquan McDonald, Philando Castile, and many others, which continued to bring exigence to lethal use-of-force by the police. A slew of reforms has followed, such as the United States Department of Justice distributing 21,000 police body cameras to law enforcement during 2014 and eight cities were issued consent decrees to improve policing.[1] While the correspondence between police reform, cultural shifts, and BLM is close, BLM's direct role is yet uncertain. Empirical literature set on disentangling BLM's part is sprouting(e.g. Hehman et al., 2018; Mazumder, 2019; Sawyer and Gampa, 2018; Trump et al., 2018; Skoy, 2020).

The current literature has a glaring lacuna: has BLM altered police lethal use-of-force? This study answers this question using nonprofit data on police killings from Fatal Encounters Dot Org, published data on BLM protests from Trump et al. (2018) during 2014q3-2015q3, and web scraped data from 2015q3-2019q4 from Ainsley's database of BLM protests. Difference-in-differences estimates suggest that places with BLM protests had 15% to 20% fewer incidents of lethal use-of-force than had BLM not taken place; approximately 300 fewer police homicides.

There are several potential challenges in estimating the effect of BLM protests on police homicides. First, there is considerable measurement error in data for police killings and BLM protests because current data relies heavily on media reporting that is susceptible to population-driven measurement error from under-reporting. There is currently no federal database with credible data on police killings, a long-standing problem (Fyfe, 2002). The Bureau of Justice Statistics deigned the Arrest-Related Deaths program to address this issue.

[1]The cities were Portland, OR; Los Angeles, CA; Los Angeles, CA; Albuquerque, NM; Cleveland, OH; Phoenix, AX; Ferguson, MW; Newark, NJ; Baltimore, MD.

The program conducts a census of all deaths occurring during the process of an arrest. The program initially failed, capturing, at best, 49% of law enforcement homicides and, at worst, 36% of homicides from 2003-2009 and in 2011 (Banks et al., 2015).

The Bureau of Justice Statistics redesigned the Arrest-Related Deaths program in 2015 to address this issue (Banks et al., 2016). Nonprofit and media organizations have filled the absence of reliable data on police homicides. Cataloging a combination of crowdsourced information, freedom of information act requests, and media coverage, groups like the Washington Post, the Guardian, Fatal Encounters, Mapping Police Violence, and Killed by Police have created publicly available datasets on police homicides. These initiatives lead 2015 FBI Director James Comey to proclaim, "It is unacceptable that The Washington Post and The Guardian newspaper from the U.K. are becoming the lead source of information about violent encounters between police and civilians." (Zuckerman et al., 2019).

Second, there may be characteristics that influence the likelihood of BLM protests that also affect police lethal-force, some of which may be unobservable. Research has found the following are significant correlates of BLM protests: poverty, educational attainment, population size, police killings, the democratic vote share, and the portion of the population that is black (Trump et al., 2018). Many vital characteristics may not be observable and, if not directly related to police homicides, may be correlated with determinants of lethal force.

Third, the prevalent rainfall instrumental variable design in the social movement literature is not valid because rainfall likely affects police use-of-force directly for the same reason moisture determines protest turnout; people go outside less when it's raining.

Fourth, since BLM protests are motivated by police killings, police homicides likely rise before BLM protests, but only in cities where the protests occur (Trump et al., 2018; Skoy, 2020). This pre-trend difference would break the identifying assumption of difference-in-difference estimators used in some related research (Mazumder, 2019; Cunningham and Gillezeau, 2018).

I address these problems with a stacked difference-in-difference design that leverages variation in BLM protests' location and timing to uncover the BLM's effect, contrasting four different estimators that build the above issues into the model. The benchmark specification is an unweighted two-way fixed effects estimator. The second estimator is per capita population-weighted least squares regressions, which accounts for any population-driven variance from the media neglecting protests or police homicides in less populated areas (Mazumder, 2019). I also gauge robustness to population screens and choice of dataset. The third estimator allows for semi-parametric selection on pre-protest correlates of use-of-force or BLM protests to address concerns with confounding variables. I also assess the influence of time-variant controls. To eliminate any pre-trend differences, the forth estimator balances police homicides between treated and controls places before protests and between control places before and after demonstrations initiate elsewhere, synthetic difference-in-differences (Arkhangelsky et al., 2019).

2. Literature

2.1. Black Lives Matter

The literature on BLM is small but proliferating. Research has focused on BLM's impact on racial attitudes and community trust of the police. A promising study by Mazumder (2019) is assessing BLM's role in shaping racial attitudes in the United States. This study is the most well-identified research design in the current literature, using an event study design that leverages variation in protests' location and timing. Their question is of particular interest because prior research has found neighborhood racial bias is associated with disproportionate lethal use-of-force Hehman et al. (2018). Research has also focused on how participating in BLM affects racial attributes. Sawyer and Gampa (2018) found that BLM participants' racial attitudes, especially white participants, became less pro-White.

Some research has paid particular attention to the protests in Ferguson, MO, in 2014. Contrary to the popular view that the events surrounding the shooting death of Mike Brown created a positive shock to crime, there is no evidence of the Ferguson insurrection increasing crime rates (Pyrooz et al., 2016). However, research has suggested the shooting of Mike Brown significantly reduced local black residents' trust of police (Kochel, 2019).

Trump et al. (2018) is the first empirical paper relating to BLM and police lethal use-of-force. Using a one-year cross-section, the researchers set out to find variables that are strong predictors of BLM protests. They find that city-level poverty, especially black poverty, black population share, population size, college-educated share, and the 2008 presidential democratic vote share are all positively associated with BLM protests.

Skoy (2020) is the first published study to estimate BLM's impact on police homicides. The author investigates BLM's short-term effects on police homicides using a monthly panel of states; they find that BLM protests reduce fatal police interactions in the proceeding month but do not increase crime or arrests. The study's scope is limited. The study does not investigate the longer-term impacts of the protests and does not identify mechanisms that explain the reduction in lethal use-of-force. The study's empirical methodology is problematic for aggregating above the level of treatment. While BLM protests may have state-level impacts through policy changes, the brunt of the movement will likely be felt at a city level, influencing local policy, the local police agency, and residents' attitudes. By aggregating to the state-level, the study cannot parse out how these local characteristics respond to BLM, which may drive the reduction in lethal force. Worse, omitting local differences ere BLM from their model makes their identifying assumption dubious.

2.2. Do Protests Work?

This study contributes to the literature on the causal effect of protesting. Since randomized control trials are difficult in this context, the choice of a quasi-experimental design is crucial. Currently, the literature's two most common identification strategies are event studies (Cunningham and Gillezeau, 2018; Mazumder, 2019; Van den Broek et al., 2017; Koku, 2011) and instrumental variable designs that commonly instrument for social conflict with rainfall, food scarcity, income shocks, and natural disasters.[2]

Event studies leverage variation in the timing and location of protests. Cunningham and Gillezeau (2018) is a recent application of this research design. The authors' findings suggest the African American uprisings during the 1960s, often a response to police violence, resulted in an increase in police homicides against nonwhite residents. While taking place half a century later, this dismal result highlights how the *a priori* sign of BLM's effect on police homicides is ambiguous.

Miguel et al. (2004) provided the seminal application of the rainfall instrument in social conflict literature. The authors use rainfall to identify plausibly exogenous variation in GDP growth and find GDP growth lowers civil conflict incidences in Sub-Saran Africa. Given the prevalence of agricultural production in the region, the authors argue that economic growth will be closely related to the annual change in rainfall (relevance). Because the authors' believe rain would unlikely affect civil wars through any other channel, the exclusion restriction is feasible. This application pathed the way for a large body of social conflict research that utilizes rainfall as an instrument with applications in developing countries (Gerling, 2017; Aidt and Leon, 2016; Hendrix and Salehyan, 2012; Brückner and Ciccone, 2011), and developed countries (Collins and Margo, 2007; Madestam et al., 2013; Huet-Vaughn, 2013; Wasow, 2017).

The subset of the social conflict literature that is most closely related to my application began when Madestam et al. (2013) used the rainfall instrument to answer a simple question plagued with endogeneity: do protests work? Using data on Tea Party protests that took place on Tax Day in the USA, April 15, 2009, the authors found good weather strengthened the protests, increasing republican votes in the 2010 midterm elections and support for tea party policies.

[2] See Nurmanova (2019), Martin-Shields and Stojetz (2019) and Burke et al. (2015) for comprehensive literature reviews of research using economic or climate shocks as instruments for social conflict.

Unlike the event study design, no BLM research has used an instrumental variable approach. One reason may be a concern with the identifying assumptions: relevance and exclusion. While the relevance of rainfall for protest participation is well established (Zhang, 2016), the exclusion restriction is unlikely *a priori* when the outcome involves police interactions. Rainfall likely determines lethal-use-of-force regardless of protest participation for the same reason rain alters protest turnout; people go outside less when it's raining. The weather may also affect lethal use-of-force indirectly through other channels. For example, Carleton and Hsiang (2016)'s thorough review of the social and economic impacts of climate found rising temperatures and low rainfall incites aggression and violent crime.

2.3. Police Use-of-force

This paper builds from a large body of work investigating lethal use-of-force determinants to specify control variables and identify potential mechanisms.

Police department policies matter. Terrill and Paoline (2017) find officers use force less readily when agencies have restrictive policy framework. There is strong evidence that body-worn cameras reduce use-of-force. One careful study by Ariel et al. (2015) randomly assigned body-worn cameras to shifts of 988 officers in Rialto over 12 months. Body-worn cameras halved use-of-force incidents. How the department files use-of-force forms are also relevant. For example, rates of force are lower when supervisors or other personnel are required to file use-of-force forms than when officers file their own forms (Mcelvain and Kposowa, 2008). However, this may reflect changes in reporting, not changes in force. Police unions, the size of the police force, feelings of loyalty to other officers, and codes of silence also play a role (Skolnick, 2008).

Both demographic and psychological attributes influence the likelihood of use-of-force. Officers who are female, college-educated, experience, or nonwhite are less likely than their demographic counterparts to use force. Black people are more likely to be subjected to non-lethal force than white people (Alpert and Macdonald, 2001; Fryer, 2019; Tregle et al., 2019; Paoline III and Terrill, 2007). There is still debate over a racial disparity in lethal force because the existence depends on normalization. On the one hand, if the fatal force incidents are normalized by population, police-citizen interactions, or total arrests, then a racial-disparity in lethal force is generally found (Menifield et al., 2019; Tregle et al., 2019; Buehler, 2017). Normalizing by population also suggests that white officers are no more likely than officers of color to use deadly force Menifield et al. (2019). On the other hand, if the officer homicides are normalized by violent crime arrests or weapons offense arrests, then a racial disparity does not exist (Cesario et al., 2019; Tregle et al., 2019; Fryer, 2019). Ross (2015) is a notable exception. The authors find strong evidence of racial bias in police shootings that cannot be resolved by controlling for the violent crime rate. Hoekstra and Sloan (2020) gives compelling evidence for the importance of race in use-of-force. Using administrative officer dispatch data, the authors isolate arguably random assignment of officer race after conditioning on place and time. White officers use force 60% more than black officers, and when white officers are dispatched into black neighborhoods, white officers are five times as likely to use gun force.

The neighborhood also has a part. Racially biased communities tend to have higher rates of police homicides (Hehman et al., 2018), as with settings with mid-level violent crime rate (Klinger et al., 2016). Areas with a high proportion of black-on-white homicides experience a higher rate of police homicides, especially by white police officers; however, the effect diminishes when the police force is demographically proportional to the neighborhood policed (Legewie and Fagan, 2016).

Lethal use-of-force most commonly results from a gunshot, which requires an officer to both choose to fire their weapon and for the officer to hit their target. The latter requisite is often neglected, but some research shows the importance of police training (Joshua et al., 2007; Donner and Popovich, 2018).

3. Methodology and Data

3.1. Empirical model

The primary aim of this study to evaluate the impact of BLM protests on incidents of lethal use-of-force. The benchmark model is a stacked difference-in-difference design with two-way fixed effects:

$$\frac{Y_{c,i,t}}{N_{c,i,t}} = \mu + \sum_{k=-4}^{4} \beta_k D_{k,c,i,t} + X'_{c,i,t}\kappa + \alpha_{c,i} + \delta_{c,t} + \epsilon_{c,i,t} \tag{1}$$

where Y is the count of lethal use-of-force and N is the normalization variable (none, population, officers, violent crime, or total arrests) in census place i during quarter t within cohort c and X is a vector of time-variant controls. The benchmark specification controls for population flexibly by fitting a linear control for the population for each cohort-population decile. Stacking by cohort aligns the first protest quarter. For each cohort, all treated places (at least one BLM protests) and control places (no BLM protests) during the sample are kept, and each cohort is then 'stacked' on top of each other. Stacking aligns the timing of treatment - the first protest - between all treated places, $t = 0$ corresponds to the first protest for all treated places. Stacking by cohort also allows for cohort-place fixed effects $\alpha_{c,i}$ and cohort-quarter fixed effects $\delta_{c,t}$. The standard errors are clustered by place since this is the level protests are assigned. The standard errors account for possible correlation within a place in the changes in lethal use-of-force.

This difference-in-differences model identifies the effect of the BLM protest on lethal use-of-force if police homicides would move in parallel between places with and without protests had the protests never occurred. This assumption holds if all determinants of BLM protests are either time-invariant or common across all places: $E(\epsilon_{c,i,t}|\{D_k\}_{k=-3}^{3}, \alpha_{c,i}, \delta_{c,t}) = 0$. While not directly testable, I use a common practice assessing the parallel trends assumption with leading terms. Specifically, this specification allows for trends to deviate four years before a protest occurring (β_{-4}, β_{-3}, β_{-2}, β_{-1}); detecting a difference during these years would indicate a violation of the parallel trends assumption.

We estimate the percentage change in police homicides per normalizing variable by dividing the estimated β_k from Equation 1 by the average lethal use-of-force per normalizing variable among places exposed to BLM protests one year prior to the first protest $((\bar{b}_{-1}))$. The annual percentage change in police homicides in year k is $\frac{\beta_k - \sum_{k=-4}^{-1} \beta_k/4}{\bar{b}_{-1}}$ and the average, annual percentage change is:

$$\%\Delta\text{Lethal Force} = \frac{\sum_{k=0}^{4} \beta_k/5 - \sum_{k=-4}^{-1} \beta_k/4}{\bar{b}_{-1}}.$$

Another interesting statistic is the total change in lethal use-of-force attributable to BLM protests, which is the product of the average quarterly change in lethal use-of-force after protests, the total number of quarter-places exposed to at least one protest (e), and the average normalizing variable among places exposed to BLM protests one year before the first protest $((\bar{n}_{-1}))$:

$$\Delta\text{Total Lethal Force} = \%\Delta\text{Lethal Force} * \overline{\mathrm{Y/N}}_{-1} * e * \overline{\mathrm{N}}_{-1}.$$

To gauge the robustness of the results, Equation 1 is approximated with four different estimators, detailed below, with the following form:

$$(\hat{\mu}, \hat{\beta}, \hat{\alpha}, \hat{\delta}) = \arg\min_{\mu,\beta,\alpha,\delta} \sum_i \sum_t \left(\frac{Y_{c,i,t}}{N_{c,i,t}} - \mu - \sum_{k=-3}^{3} \beta_k D_{k,c,i,t} - \alpha_{c,i} - \delta_{c,t} \right)^2 w_{c,i,t} \qquad (2)$$

If Equation 1 is correctly specified, then all four estimators are all consistent. If Equation 1 is incorrectly specified, then some of the alternative estimators are still consistent given the weights are appropriately penalized. Thus, similarity between the estimators is consistent with Equation 1 being the correct specification, which would bolster confidence in the results.

3.1.a. Ordinary Least Squares

The benchmark estimator is ordinary least squares (OLS) without normalization of police homicides.

$$w_{c,i,t} = 1 \qquad \text{and} \qquad N_{c,i,t} = 1 \tag{3}$$

The estimator is identified if all BLM protest determinants are either time-invariant or common across all places.

3.1.b. Per Capita Population Weighted Least Squares

The second estimator is per capita population weighted least squares (WLS), which accounts for population-driven heteroscedasticity.

$$w_{c,i,t} = \sqrt{\text{Population}_{c,i,t}} \qquad \text{and} \qquad N_{c,i,t} = \text{Population}_{c,i,t} \tag{4}$$

Like OLS, the estimator is consistent if BLM protests determinants are time-invariant or common across all places, thus its contrast to Equation 3 gives a diagnostic test for model specification (Solon et al., 2015). The key difference is that weighting by population places more weight on observations with greater precision if the media neglects events in less populated areas (Madestam et al., 2013). However, Dickens (1990) showed that weighting by the population could actually decrease precision if group sizes are similar and is not necessary when all geographic group sizes are large, in the "hundreds of thousands or larger." To address this concern, we also test if the OLS estimates hold when using populations screens.

3.1.c. Doubly Robust Inverse Probability Weighting

The previous specifications assume that all characteristics associated with BLM protests either do not change over time or the change is standard across places. While not directly testable, I evaluate its feasibility by assessing the benchmark specification's sensitivity to the inclusion of pre-protest controls via ridge-net regularized inverse-probability weighting. The controls are established correlates of either BLM protests or police homicides (see Sections 2.3 and 2.2); they include local police agency characteristics, crime rates, city demographics, population density, democratic vote share in the 2008 presidential election, historic protests, and consent decrees.

The procedure has five steps. First, the control variables are collapsed into 2013 means by census place.[3] Second, ten datasets are imputed with a multivariate normal distribution

[3]Since 2013 is the last year before the first cohort of BLM protests; this ensures the control variables do not contaminate the impact of the protests.

is. Third, ridge logistic regression is used to estimate the propensity score using the stacked imputed datasets:

$$D = X_{2013}\theta + Y_{Pre}\gamma + v$$

where D is the treatment indicator, X is a matrix of the control variables listed above, and Y is a matrix of pretreatment annual pretreatment outcome means. The penalty is chosen with 10-fold cross validation. Fourth, the propensity scores are averaged over the imputed datasets weighted by the fraction of missing data.[4] Fifth, inverse probability weights are constructed using the propensity scores $\hat{P}$:

$$w_{c,i,t} = \hat{\omega}_i = 1\{D_i = 1\}\left(\frac{\hat{\mathbb{E}}(D_i)}{1 - \hat{\mathbb{E}}(D_i)}\right) + 1\{D_i = 0\}\left(\frac{\hat{P}_i}{1 - \hat{P}_i}\right) \qquad \text{and} \qquad N_{c,i,t} = 1. \tag{5}$$

This estimator is doubly-robust. The estimator is consistent if the logit model for BLM protests is correctly specified and Equation 1 is misspecified. Alternatively, the estimator is consistent but inefficient, if Equation 1 is correctly specified, and the probit model is misspecified (See Imbens and Wooldridge, 2009).

3.1.d. Synthetic Difference in Differences

The final estimator is an adaption of Arkhangelsky et al. (2019)'s doubly robust alternative to the synthetic control method, which they name synthetic difference-in-differences (SDID). Unlike the synthetic control method, which matches the pretreatment outcomes over units with time fixed effects, the SDID approach also balances control outcomes over time and adds unit fixed effects. Like the IPW estimator, SDID is doubly robust. If Equation 1 is correctly specified, then the estimator is consistent for most weighting schemes. If the weights are correct, then the estimator is consistent even when the basic fixed effects model given in Equation 1 is miss-specified. The main advantage is double-bias reduction; if the unit-weights do not fully balance the underlying signal in the pretreatment period, time-weights may balance the remainder.

This procedure has three steps. First, IPW weights are estimated that match the pretreatment outcomes between cohort-places with and without protests, ω_i. The cohort-place propensity scores are the ridge-penalized predicted values of the equation:

$$D = Y_0\eta + v$$

where Y_0 is a matrix of pre-protest lethal use-of-force demeaned by cohort-place and cohort-

[4]For a discussion of stacked multiple imputation for regularized regression, which is theimputation procedure used here, see Wan et al. (2015) and Zhao and Long (2017).

quarter. The ridge penalty is selected with 10-fold cross-validation. Using the cohort-place propensity scores $\hat{P}_i$, the cohort-place IPW weights are:

$$\hat{\omega}_{c,i} = 1\{D_{c,i} = 1\}\Big(\frac{\hat{\mathbb{E}}(D_{c,i})}{1 - \hat{\mathbb{E}}(D_{c,i})}\Big) + 1\{D_{c,i} = 0\}\Big(\frac{\hat{P}_{c,i}}{1 - \hat{P}_{c,i}}\Big).$$

Second, IPW weights are estimated that match the pre- and post-protest lethal use-of-force of the control cohort-places (demeaned by cohort-place and cohort-quarter). The time propensity scores are the lasso-penalized predicted values of the equation:

$$D^* = Y_{\text{Control}}\eta + \nu$$

and are denoted as $\hat{P}^*$. D^* is an indicator for $t \geq 0$. The so-called plugin penalty is used to achieve a sparse model.[5] The cohort-time IPW weights are:

$$\hat{\lambda}_{c,t} = 1\{D^*_{c,t} = 1\}\Big(\frac{\hat{\mathbb{E}}(D^*_{c,t})}{1 - \hat{\mathbb{E}}(D^*_{c,t})}\Big) + 1\{D^*_{c,t} = 0\}\Big(\frac{\hat{P}^*_{c,t}}{1 - \hat{P}^*_{c,t}}\Big)$$

Third, the final weights are the product of the cohort-place and cohort-time weights:

$$w_{c,i,t} = \hat{\omega}_{c,i}\hat{\lambda}_{c,t} \qquad \text{and} \qquad N_{c,i,t} = 1. \tag{6}$$

Identification now assumes either selection is on fixed effects or or the weights are correctly penalized.

3.2. Data and Sample Construction

3.2.a. Lethal Force Data

There is currently no federal database with reliable police killings data (Fyfe, 2002; Banks et al., 2015; Klinger et al., 2016; White, 2016). Nonprofit and media organizations have filled the absence of data. Through a combination of crowdsourcing, freedom of information act requests, and media coverage, public datasets are now available on police homicides including KilledByPolice.net, The Homicide Record by the Los Angeles Times, Mapping Police Violence (MPV), the Washington Post, the Counted by the Guardian, and Fatal Encounters Dot Org.

Legewie and Fagan (2016) analyse the quality of the latter three sources, which are widely used (e.g. Trump et al., 2018; Skoy, 2020; Cesario et al., 2019; Nix et al., 2017).[6] Of the

[5] Since the number of places is substantially greater than the number of quarters, a sparse model is required for the logistic regression to converge.

[6] See Bor et al. (2015) and the ensuing correspondence for a discussion of the quality of MPV.

1147 total police killings in 2015, the authors find Fatal encounters were missing 33 incidents, the Guardian was missing 49 incidents, and the Washington Post was missing 184 incidents. While Fatal Encounters was the most complete, the information on race was subpar.

Police homicides are measured as fatal encounters with police resulting from asphyxiation, bludgeoning, a gunshot, pepper spray, or a taser that are not suicides. The benchmark estimates use D. Brian Burghart's nonprofit Fatal Encounters Dot Org. The organization operates three main methods for collecting data: 1) Paid researchers (85% of data), 2) Public records requests, and 3) Crowdsourcing. Paid researchers aggregate data from other sources listed above. All data are then verified by a principal investigator, cited, and checked against published sources. The dataset is updated regularly and begins in 2000. The Fatal Encounters data are detailed. For each police-involved fatality, they describe the incident, the address of the death, but the information on race, weapons, and the disposition of death are worse than MPV.

MPV is the highest quality data on lethal force from 2013 to 2019. The organization gathers data from the other previously mentioned databases, improving their quality and completeness by "'searching social media, obituaries, criminal records databases, police reports, and other sources to identify the race of 91 percent of all victims in the database." MPV also has detailed information on the alleged arming of the victim. However, MPV does not have data before 2013, implying pre-trend differences in police homicides before BLM protests cannot be tested, a significant drawback. Hence, MPV is only used to see if the estimates hold using alternative data or vary by race or alleged arming of the victim.

The definition of police homicides used by Fatal Encounters is too broad: all lethal interactions with police, whether on- or off-duty, including suicides. MPV, however, only includes cases where "a person dies as a result of being shot, beaten, restrained, intentionally hit by a police vehicle, pepper-sprayed, tasered, or otherwise harmed by police officers, whether on-duty or off-duty." Figure 1 displays the proportion of total fatal encounters by cause of death during 2013-2019 and contrasts Fatal Encounters with MPV. Gun force is particularly lethal; however, the injury may have been self-inflicted, thus not lethal use-of-force. Figure 1a shows that the Fatal Encounters data suggests gunshots account for 69.13% of the 10725 deaths. However, according to MPV, there were only 7642 police homicides, 95.75% of which resulted from a gunshot, shown by 1b. Vehicle-related deaths make up most of the discrepancy. To reduce error, police homicides are restricted to fatal encounters from asphyxiation, bludgeoning, a gunshot, pepper spray, or a taser that are not suicides. Figure 1c and Figure 1d compare this restricted definition of police homicides between the two databases by injury. The restricted definition misses 39 cases in the MPV data but makes the case-of-death distribution and total cases similar, bolstering confidence in the measure's quality.

3.2.b. Black Lives Matter Protest Data

The BLM protests data builds from published data by Trump et al. (2018) who use a rolling web-search to count the number of protests by census place from August 9th, 2014 through August 9th, 2015. I then web-scrape data from August 10th, 2015 through 2018 from a website maintained by Alisa Robinson. She is a graduate of the political science department at the University of Chicago. Her data is publicly available through a Creative Commons license.

This paper is concerned with the effect of BLM protests against police violence, particularly the public gathering of individuals. For this reason, I exclude online demonstrations, protests by professional athletes, protests against presidential candidates, or protests against conservative talks at universities.

Figure 2 illustrates the evolution of the difference in the cumulative number of protests between places with and without demonstrations. If a rally occurs, then, on average, seven more occur over the subsequent five years. Three demonstrations during the first year of protest are usual, with one or two following events annually. The movement does not taper.

3.2.c. Data for Control Variables

The decennial census is used for the census place geographic size and the number of houses while the annual intercensal census is used for population (U.S. Census Bureau, 2019a,b). I use the 2013 five-year American Community Survey for data on poverty rate, labor force participation rate, unemployment rate, full-time employment rate, the black population share, the black poverty rate, and educational attainment measures, including the portion of people with less than a high school, some college, or college education (U.S. Census Bureau, 2013). The year 2013 is chosen to ensure control variables do not contaminate the effect of the BLM protests.

I next use Jacob Kaplan's concatenated files of the Uniform Crime Reporting data to obtain data on property crime rates, violent crime rates, assaults on police, and felonious police deaths (Kaplan, 2018).

To measure a city's protest history before BLM, we use the Dynamics of Collective Action dataset. The publically available dataset counts the number of protests and hate crimes in each city based on media reports. The dataset codes each event according to the participants and demand of the action. The data counts the number of pro-black civil rights protests, pro-anti-police brutality protests, black initiated protests, and racist events, which include hate crimes and protests against the civil rights of racial minorities.

The Annual Survey of Public Employment and Payroll is used for measures of the annual number of police officers and the average wage for a police officer for each place.

I use the 2013, 2016, and 2016 Police Body-Worn Camera Supplement Law Enforcement Management and Administrative Statistics (LEMAS) data to complement the data on police wages and the number of police; the average value of the two sources is taken for each place. The LEMAS also provides information on agency characteristics, including officer demographics, unionization, use-of-force reporting, authorized equipment, police training, use of cameras, and community policing initiatives (United States Department of Justice, 2013).

Last, the city level democratic vote share in the 2008 presidential election is taken from Einstein and Kogan (2016).

3.2.d. Sample and Covariate Balance

The final dataset includes any census place with a population of at least 20,000. I collapse the data into quarterly counts of BLM protests and police homicides for each census place from 2000q1 until 2019q4. Figure 3 maps each police homicide and protest in the sample.

Table 1 reports the covariate balance between the places with at least one BLM protest (treated group) and places without a BLM protest (control group) under each weighting scheme. Columns 1 and 2 show the unweighted means for each control variable by treated status. The results are consistent with the findings of Trump et al. (2018); places that have at least one BLM protests tend to have a higher poverty rate, a larger black population share, a higher black poverty rate, more college education, and a larger population. These differences persist when weighting by population; however, the IPW weights balance the covariates closely with two notable exceptions: population and the crime index. The balance for these exceptions is, none-the-less, improved. These exceptions are balanced when using weights that balance either pre-BLM lethal force (Columns 7 and 8) or both pre-BLM and control deadly force (Columns 9 and 10).

4. Results

Table 2 reports results for the impact of BLM on lethal use-of-force by police. Column 1 is the benchmark ordinary least squares estimator that includes cohort-census place and cohort-quarter fixed effects, along with a linear population-population decile interaction. The estimate is striking. Following BLM protests, lethal use-of-force fell by 16.8% (s.e.=0.045), on average. If the model is correct, then BLM protests are responsible for approximately 300 fewer people being killed by the police from 2014 through 2019. The payoff for protesting is substantial; every 5 of the 1,654 protests in the sample correspond with approximately one less person killed by the police over the following years. The police killed one less person for every four thousand participants.

The remaining columns gauge the robustness of alternative estimators. Column 2 reports the population-weighted per capita regression. Normalizing police homicides by population and weighting by population accounts for population-driven variance implicit to media-based data from under-reporting of protests or police homicides in low-population areas. The estimates are slightly larger; BLM protests correspond with a 19.8% (s.e.=0.053) reduction in lethal use-of-force. Column 3 reports the WLS regression estimate where weights balance the inverse probability of having at least one protest between cities based on their average 2013 characteristics. The estimates are again larger; BLM protests associate with an 18.6% reduction in police homicides. Column 4 gives the WLS estimates where the weights balance the number of police homicides over the four years before the cohorts first BLM protests, between places with and without an eventual protest. These weights take into account BLM protests being more likely in places with recent police killings (Trump et al., 2018). The estimates are almost identical to column 3. Column 5 balances the data according to the signals in the control outcomes. The estimates grow, BLM protests are associated with an 18.8% reduction in lethal use-of-force. Last, column 6 reports the synthetic difference-in-differences estimates that weight by the product the event-place and event-quarter inverse probability weights, which has the best double-robustness properties. The estimate suggests that balancing homicides by cohort-place and cohort-time increases BLM's impact to a 21.1% (s.e.=0.079) reduction in police homicides.

As with all difference-in-differences applications, these estimates' validity rest on assuming parallel trends between places with and without exposure, had protests never occurred. Figure 5 gauges the validity of the assumption for the four main specifications by allowing trends to deviate for four years preceding BLM. No pre-trend difference is detected for any specification; however, the test could be underpowered, making the magnitude of the trend difference two years before BLM concerning. In the case that there were meaningful, albeit undetected, pre-trend differences, the close correspondence between the OLS estimate, Figure 5a and the SDID estimate, Figure 5d bolsters confidence in the results. The figure also suggests BLM's impact heightens with time regardless of the estimator.

The results thus far indicate BLM protests reduce lethal use-of-force locally but do not explain why. One way BLM may impact lethal use-of-force is by pressuring local police agencies to change. Table 8 reports difference-in-difference estimates of BLM protests' impact on police agency-level variables. All regressions include agency and time fixed effects. Column 1 gives BLM's impact on the adoption of body-worn cameras; BLM protests double the likelihood of the local agency obtaining body-worn cameras, 103.7% (s.e.=0.271). Given that a randomized control trial found body-worn cameras halved the likelihood of force (Ariel et al., 2015), this increase in body-worn cameras following BLM protests is likely related to the fall in police homicides. Some police agencies assign officers to regular geographic patrols as a community policing initiative. Column 2 reports the effect of BLM protests on the number of said officers. BLM protests increase the number of officers with regular geographic patrols by 43.7% (s.e.=0.159), around one hundred officers. Police agencies may also respond to the protests by encouraging SARA-type (scanning, analysis, response, assessment) problem solving, which demands community engagement. Column 3 reports the effect of BLM protests on the number of officers encouraged to engage in SARA-type problem-solving projects; there is a 118.5% (0.363) increase in the number of officers. The stark increases in both SARA-type officers and set geographic patrol officers are consistent with police agencies expanding community policing due to pressure from BLM protests; however, the impact of these policies on lethal force is understudied. Column 5 reports that BLM protests decrease the number of black police officers by 6% (s.e.=0.060) and Column 6 shows a negligible impact on white officers. Because a reduction in black officers could correspond to a rise in use-of-force, especially in predominantly black cities (e.g. Hoekstra and Sloan, 2020), this is not consistent with the fall in lethal force. Column 6 reports BLM protests lead to an insignificant fall in experienced officers (measured as the total number of offices less recruits) and a 37.8% (s.e.=19.7) fall in agencies requiring at least some college for new officers, corresponding with more expected force (Paoline III and Terrill, 2007). Cities with BLM protests increase their operating budgets for the police (Column 9) and experience a decrease in property crime-related arrests (Column 10). BLM does not meaningfully alter violent crime-related arrests (Column 10) or assaults on officers (Column 12).

5. Robustness

5.1. Alternative Data, Race, and Alleged Arming

Race is an important factor in use-of-force. Not only are black people are more likely to be subjected to non-lethal force than white people (Alpert and Macdonald, 2001; Fryer, 2019; Tregle et al., 2019), but white officers use gun force at a rate well above black officers when dispatched into black neighborhoods (Hoekstra and Sloan, 2020). Table 7 reports estimates using data from Mapping Police Violence and subsets incidents of lethal force by race and alleged arming to investigate if BLM has influenced lethal force differentially and to see if the main results hold with alternative data. Because this dataset begins in 2013, there is not enough time before BLM to allow for differences in trends before protests. Column 1 reports a baseline difference-in-difference estimate; BLM protests reduce lethal force by 8.4% (0.043). Column 2 reports a population-weighted per capita regression; lethal force per capita falls by 14.5% (s.e.=0.062) following BLM protests. Both of these estimates are smaller, albeit similar, to the benchmark estimates using the Fatal Encounters Dot Org dataset reported in Table 2. The estimates by race are inconclusive. There is an imprecise fall in both white and black police homicides of a similar magnitude to the overall reduction. The results are also mixed for incidents of lethal use-of-force against unarmed individuals. Column 7 reports a 12.1% (0.084) fall in unarmed lethal force and Column 8 reports a 23.5% (0.121) fall in unarmed lethal force per capita. While the size of both suggests the bulk of the reduction in lethal force is against unarmed individuals, only the latter is distinguishable from zero.

5.2. Intensity of Protest

The specifications so far use an indicator for having at least one BLM protest to capture the impact of protesting, which may fail to capture the protests' intensity. Table 3 reports the percentage change in lethal force from BLM protests by maximum protests size quartile to capture large events and the total number of protest quartile to differentiate persistent protests from others. The fall in lethal use-of-force is large and precise in the fourth quartile of maximum protest size; census places with at least one protest with over three hundred participants experience a 16.5% (s.e.=0.069) to 26.3% (s.e.=0.066) reduction in lethal use-of-force depending on specification. Places that do not experience a protest of this size do not experience a statistically significant decrease in police homicides for most specifications. The estimates by the total number of protests are less intuitive. As we expected, places with frequent protests have large and precise falls in police homicides. However, the largest fall is not found in the fourth quartile, but the second, questionable.

To further disentangle BLM protests' intensity, Figure 7 display case studies for census places with the most protests in descending order. All regressions include time and place fixed effects, along with linear population-population decile interaction. Many of these cities were home to high profile police killings that sparked large and sustained protests, notably the police killings of Freddie Gray in Baltimore, Michael Brown in Ferguson, Eric Garner in New York City, Tamir Rice in Cleveland, Jamar Clark in Minneapolis, and Sam Dubose in Cincinnati. In almost all of these cases, there is a substantial decrease in lethal use-of-force following the protests. There are exceptions: St. Louis, Minneapolis, San Francisco, and Portland have an increase in police homicides. Given this result, it is not surprising that Minneapolis and Portland became bedrocks of BLM protests in 2020.

5.3. Normalization

The choice of the variable used to normalize use-of-force is controversial in the racial disparity in use-of-force literature; racial disparities are found if lethal force is normalized by population or total arrests but are not found if normalized by violent crime arrests (Cesario et al., 2019; Tregle et al., 2019; Fryer, 2019). Table 4 reports estimates that assess how normalization impacts the result. Column 1 gives the benchmark estimate. Without normalization, BLM protests reduce lethal force by 16.8% (0.045). Column 2 reports estimates normalize lethal use-of-force by population, which slightly reduces the estimate. BLM reduces police homicides per capita by 13.7% (s.e.=0.070). Column 3 gives results when normalizing police homicide by the number of officers, resulting in a large and imprecise decrease in lethal use-of-force of 40.6% (0.282). This change is likely due to conditioning on non-missing data on the number of police officers. Column 4 normalizes lethal force by violent crime arrests and Column 5 normalizes by total arrests. The result does not hold in either column. When normalizing the violent crime arrests or total arrests, BLM's impact on lethal force is not distinguishable from zero and is unbelievably large. The estimates are not robust to normalizing by violent crime or total arrests.

5.4. Population Screen

The benchmark results omit all census places with a population below 20,000. This screen may be too low. Media-based data likely has population-driven variance from neglecting protests or fatal police encounters in less populated areas (Madestam et al., 2013). To address this concern, we contrasted the OLS estimator with per capita population WLS. However, Dickens (1990) showed that weighting by the population could actually decrease precision if group sizes are similar and is not necessary when all geographic group sizes are large. A higher population screen is hence an alternative solution to the population-driven variance issue.

Table 5 reports estimates with sequentially higher population screens. Column 1 reports the benchmark estimate; BLM reduces lethal force by 16.8% (0.045). This finding is remarkably stable. Columns 2-5 show that raising the population screens to 40,000 to 100,000 does not meaningfully change the result's magnitude but slightly reduces the precision due to the smaller sample size. However, raising the population screen even higher increases the magnitude of the estimates (Columns 6 and 7). This increase is likely because of a positive correlation between population size and BLM protests (Trump et al., 2018).

5.5. Specification

The fall in lethal use-of-force following BLM protests is robust to specification, as shown in Table 6. Police homicides are not normalized in any specification, and the regressions are not weighted unless specified otherwise. Column 1 reports the baseline estimate with cohort-place and cohort-time fixed effects; BLM reduces lethal force by 12.1% (s.e. 0.048). This estimate falls to a 10.1% (0.040) reduction from including in cohort-time-population decile fixed effect, Column 2. Still, it almost doubles to a 23.8% (s.e. 0.066) reduction when adding cohort-place linear time trends instead, Column 3. Columns 4-7 add time-variant controls to the baseline specification. Controlling for population and consent decrees increases the magnitude to around 16%, which is undone from the additional crime and city-demographic controls.[7] With all time-variant controls, the regression reported in Column 8 is weighted by the inverse probability of BLM, balancing the 2013 average police agency characteristics, crime controls, local demographics, and labor market controls, population and housing density, democratic vote share, historical protests and hate crimes, and consent decrees. Balancing by pretreatment controls increases the magnitude of the estimate to 17.0% (s.e. 0.057). Column 9 weights the same regression specification by the inverse probability of having a BLM protest balancing the annual, average police homicide over the four years before BLM. Balancing the pretreatment outcomes enlarges the estimate to -18.2% (0.078). Column 10 shows that balancing by pretreatment outcomes and control outcomes reduces the precision but does not influence the last specification's magnitude.

Figure 6 depicts how the evolution of lethal force changes by the specification. Figure 6a is the baseline estimate previously shown in 5a. After BLM protests, lethal force falls by 16.8% (s.e.=0.045) on average, and the decrease grows with time. Since this estimate assumes parallel trends, the magnitude of the trend difference two years before BLM is concerning; while not statistically significant, the test may be underpowered. This concern is mitigated when adding cohort-time-population decile interactions, Figure 5b, but the impact on lethal force falls to 11.9% (s.e.=0.039), but continues to grow with time. The smaller BLM impact is likely because BLM activity is related to population size, implying this specification may diminish BLM's impact. Figure 6c shows that linear time trends do not impact the pre-BLM placebo estimates but increase the magnitude to 21.6% (s.e.=0.058). Figure 6c shows that adding both linear time trends and a cohort-time-population decile interaction leads to estimates similar to the baseline model while eliminating the pre-BLM trend differences.

[7] All controls are linear interactions with cohort except for the cohort-population decile-linear population interaction, which was used in the benchmark specification.

6. Conclusion

Difference-in-differences estimates suggest that census places with Black Lives Matter protests have experienced a 15% to 20% decrease in police homicides from 2014 to 2019, approximately 300 fewer police homicides. This fall in lethal use-of-force is growing over time and is prominent when protests are large or frequent. While this reduction is robust to specification, estimator choice, choice of data, and population screens, it did not hold if lethal use-of-force is normalized by violent crime or arrests. BLM protests also increase the probability of a police agency having body-cameras, expand community policing, and reduce the number of future property crime-related arrests, which may partially explain the lethal force reduction.

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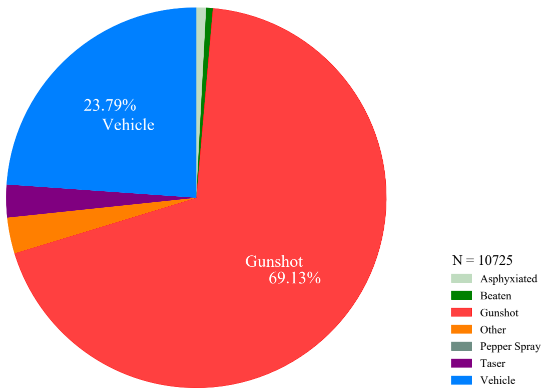
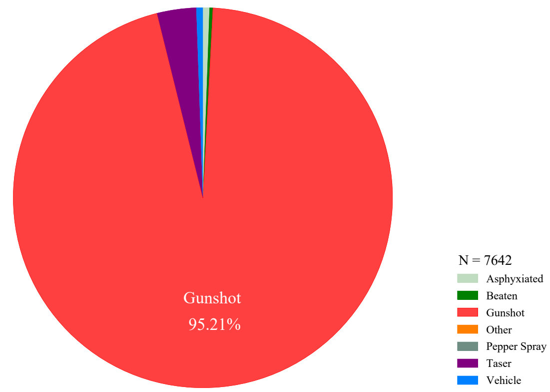
List of Figures

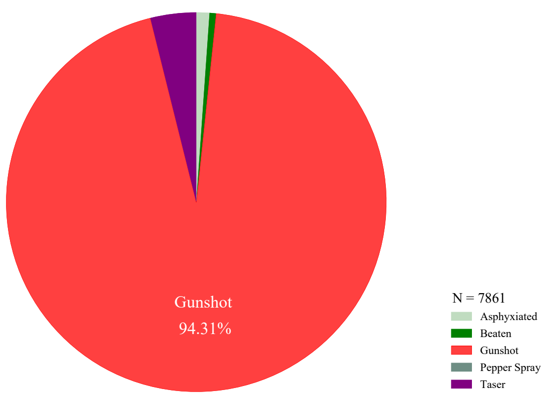
Figure 1: Proportion of Total Fatal Encounters with the Police by Cause of Death from 2013-2019



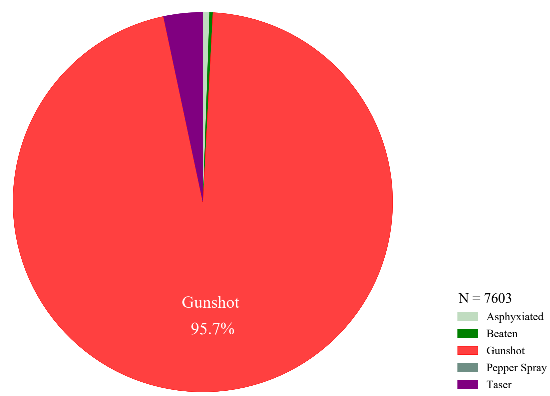
(a) Fatal Encounters



(b) Mapping Police Violence



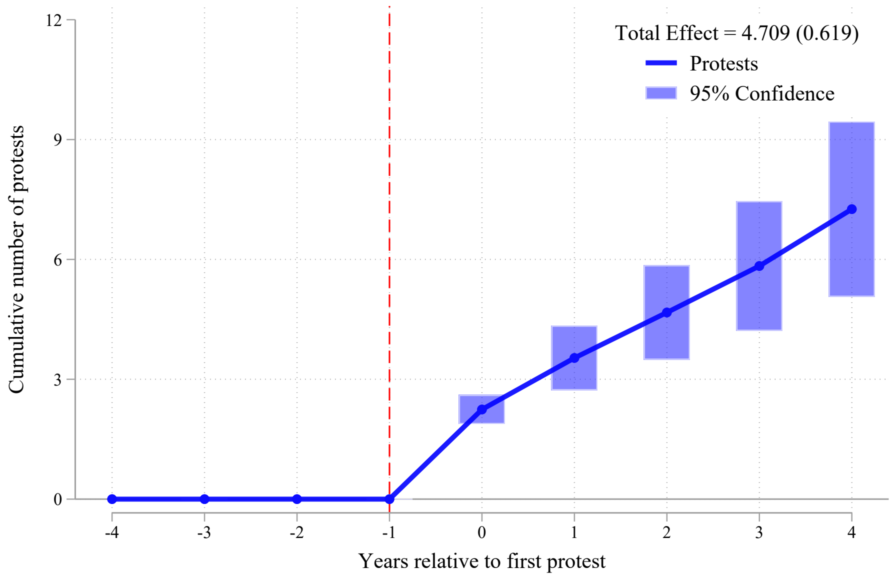
(c) Fatal Encounters Restricted



(d) Mapping Police Violence Restricted

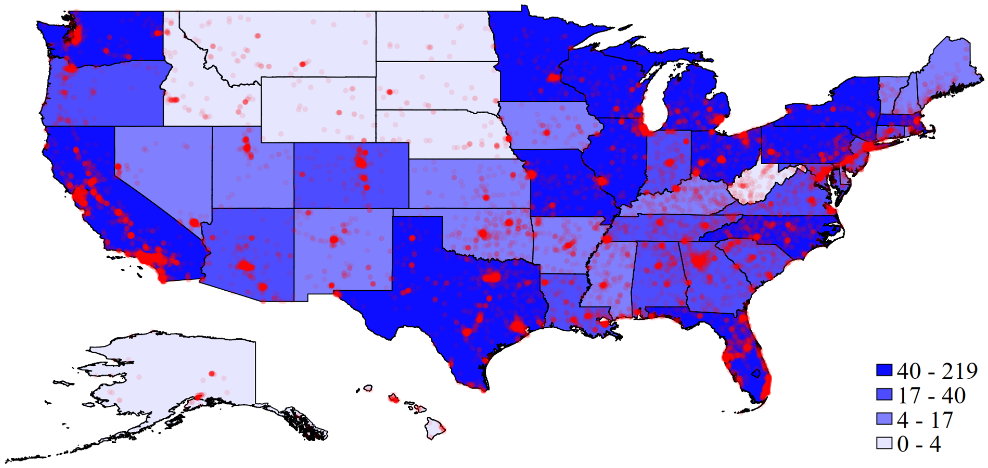
Notes: The figure shows the proportion of fatal encounters attributed to different causes of death from 2013-2019. The figure compares two different sources of data, Fatal Encounters and Mapping Police Violence.

Figure 2: Evolution of the Cumulative Number of Black Lives Matter Protests

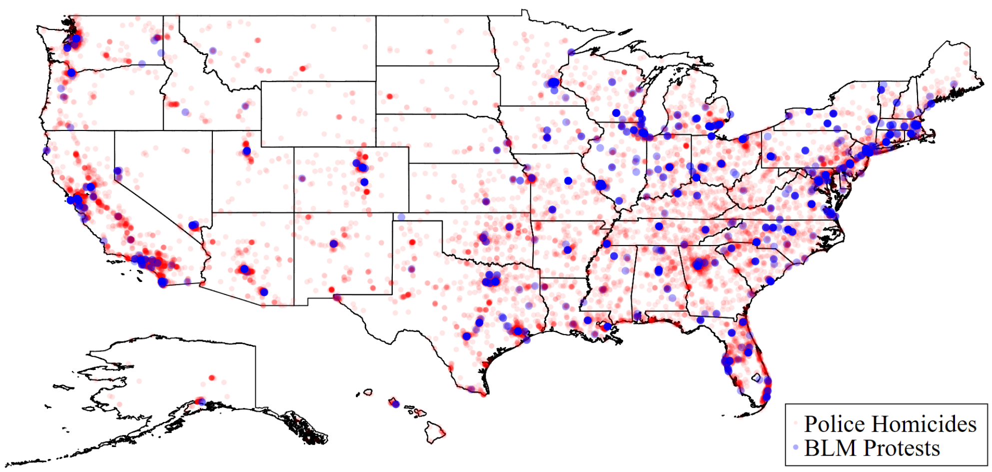


Notes: The figure shows the evolution of the average difference in the cumulative number of Black Lives Matter (BLM) protests between the treated and control census places. The green line gives the $\hat{\beta}_k$ from a regression of the total number of BLM protests in census place i during quarter t on the right-hand side of Equation 1. The regression is a stacked difference-in-difference estimate that includes cohort-quarter and cohort-place fixed effects. The green shaded area is the 95% confidence interval based on robust standard errors that are clustered by place.

Figure 3: Maps of Police Homicides and Black Lives Matter Protests



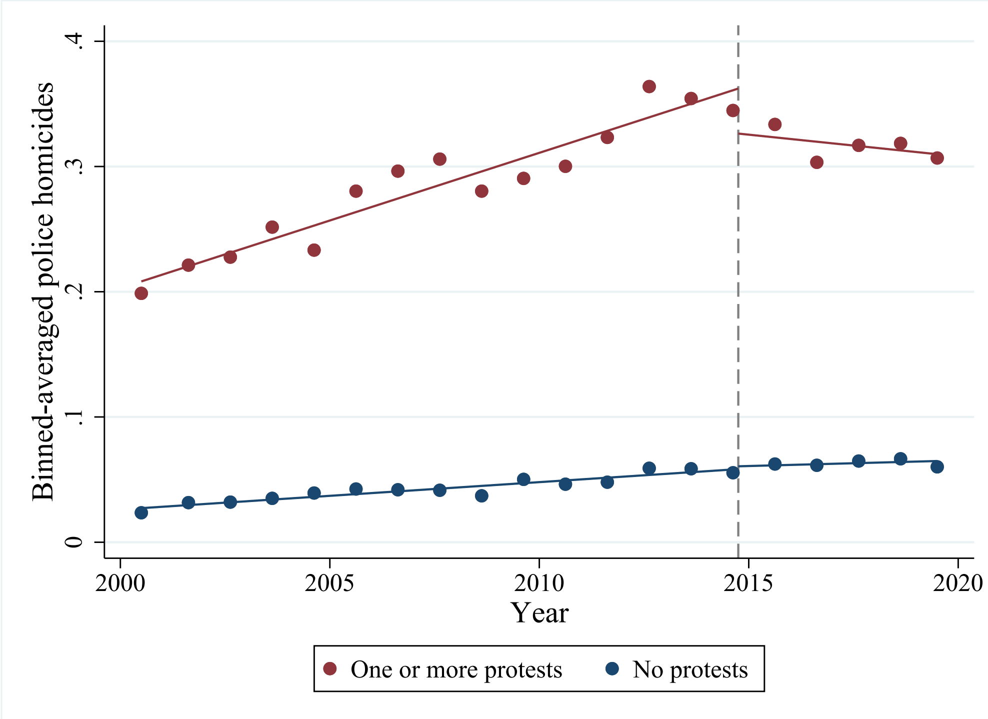
(a) Police Homicides over Protest Totals by State



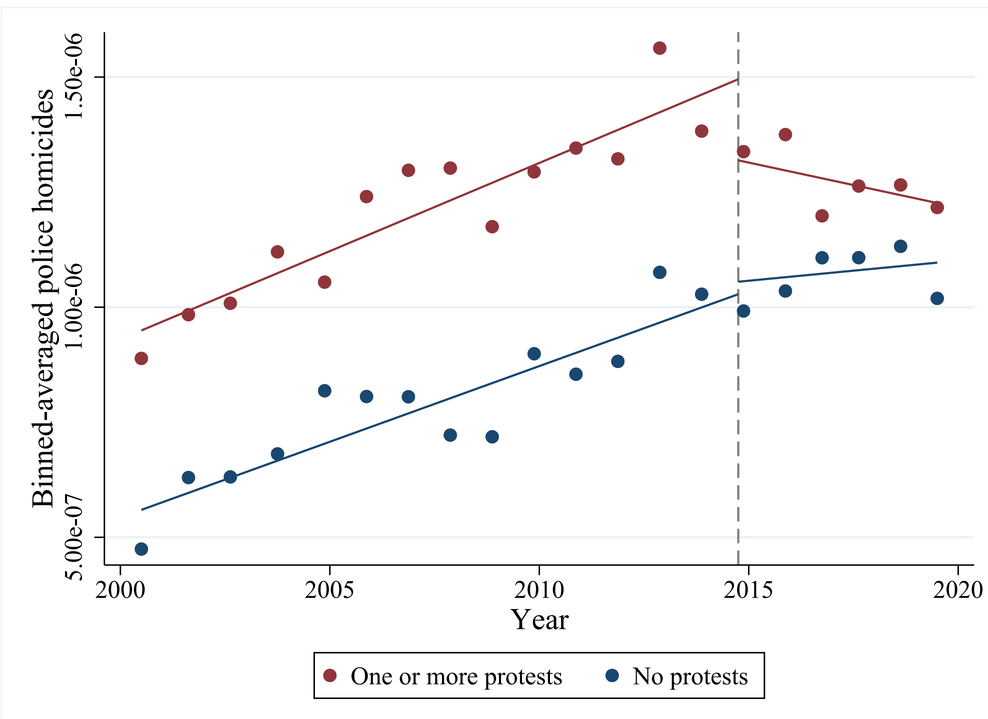
(b) Police Homicides and Daily Protests

Notes: The figures show the location of police homicides and Black Lives Matter protests from 2000 to 2019. Blue denotes Black Lives Matter Protests. Red indicates a police homicide.

Figure 4: Binscatter of Lethal Use-of-force by Treatment Status



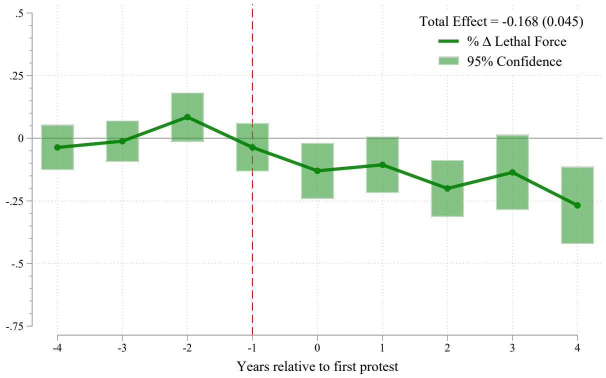
(a) Evolution of lethal use-of-force



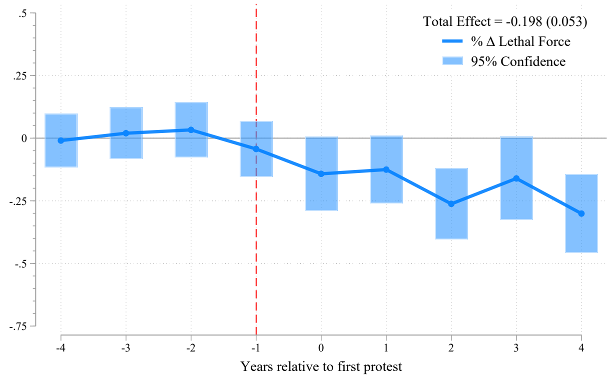
(b) Evolution of lethal use-of-force per capita weighted by inverse probability of treatment matched on pretreamtent controls

Notes: Figure 4a reports an unweighted bin scatter of homicides by treatment. Figure 4b reports a population weighted bin scatter of homicides per capita by treatment. Both figures also show the linear regression lines with a disconuinty at the start of the first Black Lives Matter protest. 'Treated= 0' refers to the group of places that enver had a protest. 'Treated= 1' refers to the group of places that had at least one protest.

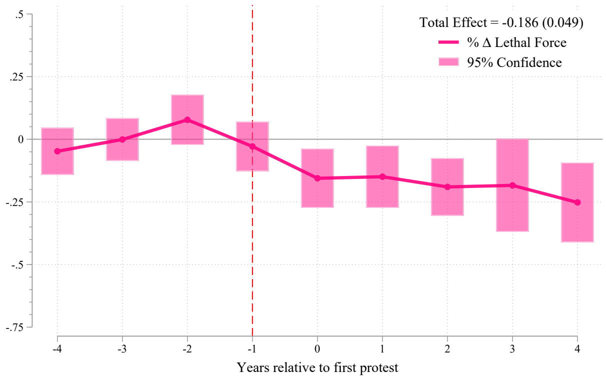
Figure 5: Evolution of Impact of Black Lives Matter Protests on Police Homicides



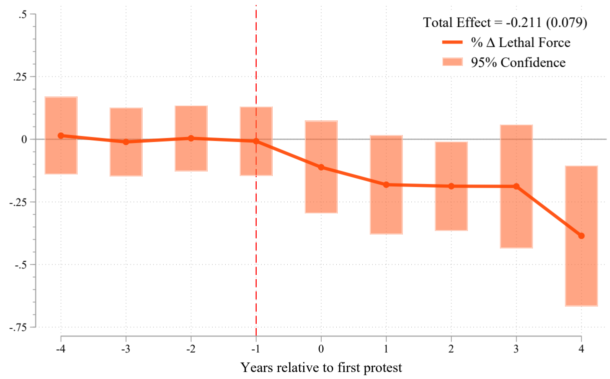
(a) Evolution of lethal use-of-force



(b) Evolution of lethal use-of-force per capita weighted by inverse probability of treatment matched on pre-treamtent controls



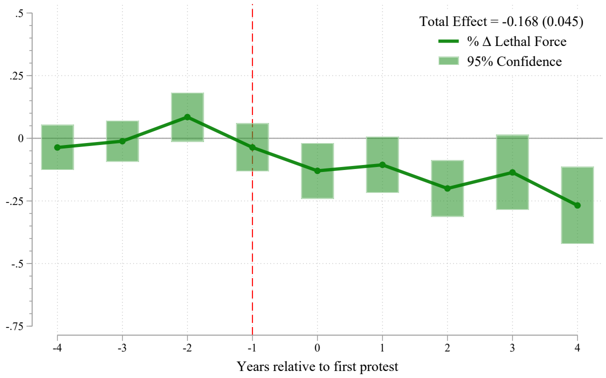
(c) Evolution of lethal use-of-force per capita weighted by inverse probability of treatment matched on pre-treamtent controls



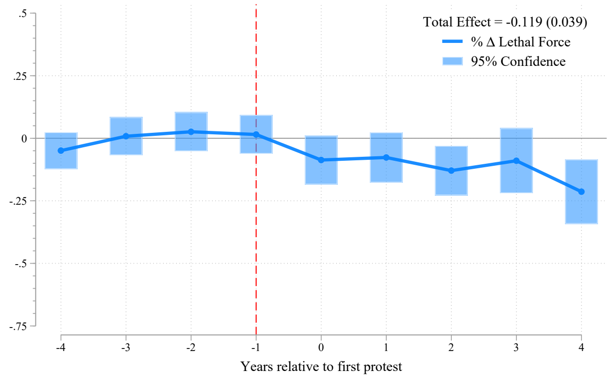
(d) Evolution of lethal use-of-force weighted by inverse probability of treatment matched on pretreatment outcomes and control outcomes

Notes: The figure shows the estimates of the stacked difference-in-difference model given by Equation 1. Each sub-figure reports a different weighting scheme; all specifications include cohort-place and cohort-time fixed effects. The shaded area in each figure is the 95% confidence interval based on robust standard errors that are clustered by place. Figure 5a depicts the ordinary least squares estimates. Figure 5b displays the per capita weighted least squares estimate that uses population weights to adjust for population-driven heteroscedasticity. Figure 5c displays the weighted least squares estimate that uses elastic-net regularized inverse probability weights to balance pretreatment control variables between the treated and control groups. Figure 5d displays the weighted least squares estimate that uses elastic-net regularized inverse probability weights to balance pretreatment control variables and pretreatment lethal use-of-force per capita between the treated group and control group.

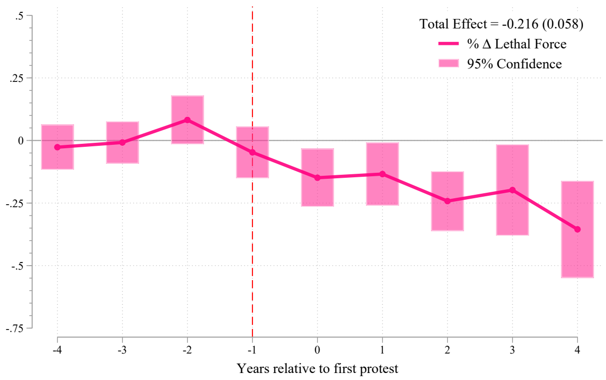
Figure 6: Robustness of Estimated Impact of Black Lives Matter Protests on Police Homicides to Specification



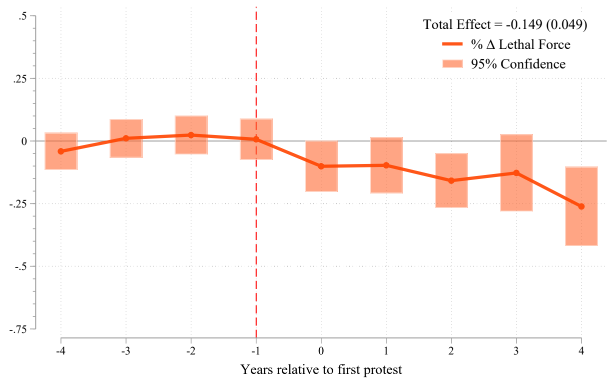
(a) Evolution of lethal use-of-force with cohort-time, cohort-unit fixed effects and population control.



(b) + Cohort-time-population decile interaction



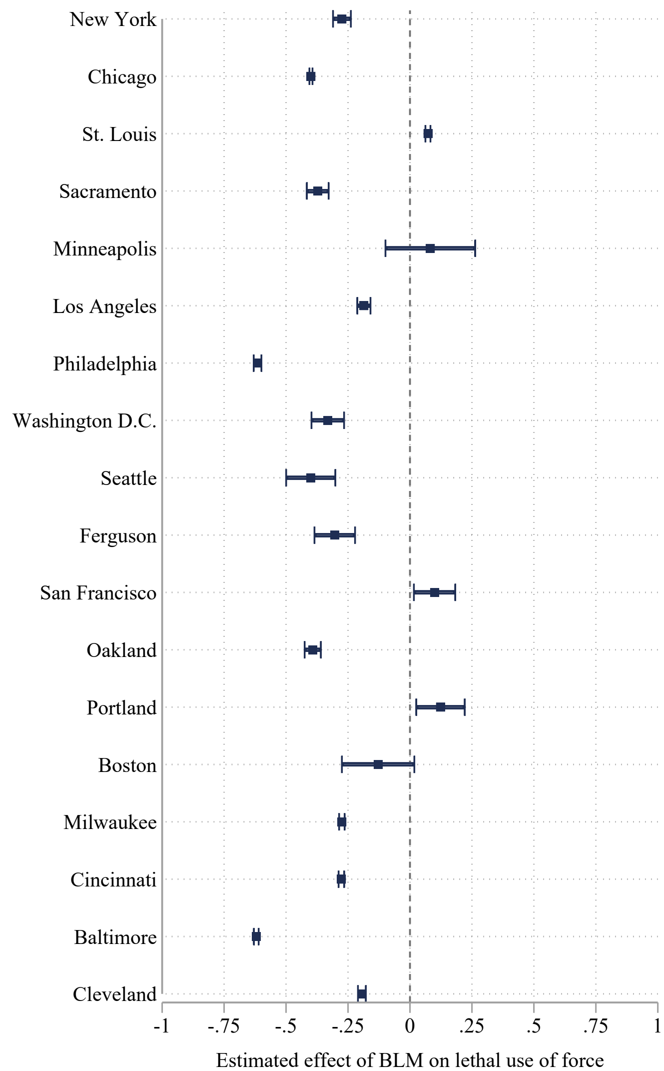
(c) + Cohort-unit specific linear time trends



(d) + Cohort-time-population decile interaction and cohort-unit specific linear time trends

Notes: The figure assess the robustness the stacked difference-in-difference model given by Equation 1 to alternative specifcations. All specifications include cohort-place, cohort-time, cohort-population decile fixed effects and are estimated with ordinary least squares. The shaded area in each figure is the 95% confidence interval based on robust standard errors that are clustered by place. Figure 6a depicts the benchmark estimates. Figure 6b shows estimates that also include an a cohort-time-population decile interaction. Figure 6c displays estimates that also include cohort-unit specific linear time trends. Figure 6d displays estimates that also include both a cohort-time-population decile interaction and unit specific linear time trends.

Figure 7: Case Studies of Cities with Large Number of Protests



Notes: No weights. List is descending in number of protests and alphabetical if tie. The figure reports the ordinary least squares estimates of Equation 1 for individual case studies. The regressions include state and time fixed effects. The bars depict the 95% confidence interval based on robust standard errors that are clustered by place.

List of Tables

Table 1: Covariate Balance of Select Control Variables

	Unweighted		Population		IPW Controls		IPW Unit		IPW Unit-Time	
	(1) Treated	(2) Control	(3) Treated	(4) Control	(5) Treated	(6) Control	(7) Treated	(8) Control	(9) Treated	(10) Control
Poverty	16.59	16.04	16.38	16.08	16.59	16.10	16.59	16.35	15.54	13.20
	(5.82)	(7.43)	(4.43)	(7.35)	(5.82)	(6.53)	(5.82)	(7.25)	(6.04)	(6.95)
Labor force participation rate	64.37	65.31	65.69	65.71	64.37	64.00	64.37	65.43	64.18	65.04
	(5.51)	(6.38)	(4.10)	(5.86)	(5.51)	(6.27)	(5.51)	(5.74)	(5.50)	(5.73)
Unemployment rate	8.91	8.34	9.26	8.39	8.91	8.92	8.91	8.56	8.44	8.01
	(3.31)	(3.50)	(3.07)	(3.37)	(3.31)	(3.82)	(3.31)	(3.30)	(3.21)	(3.20)
Full time employment rate	1.40	1.60	1.43	1.57	1.40	1.45	1.40	1.59	0.90	0.98
	(1.20)	(1.44)	(1.19)	(1.41)	(1.20)	(1.26)	(1.20)	(1.42)	(0.97)	(1.14)
Black populiation	0.63	0.60	0.63	0.58	0.63	0.62	0.63	0.59	0.42	0.36
	(0.42)	(0.45)	(0.41)	(0.46)	(0.42)	(0.44)	(0.42)	(0.46)	(0.39)	(0.42)
Black poverty rate	0.30	0.23	0.29	0.22	0.30	0.27	0.30	0.22	0.30	0.22
	(0.13)	(0.16)	(0.08)	(0.14)	(0.13)	(0.16)	(0.13)	(0.13)	(0.12)	(0.13)
< High school	0.06	0.06	0.07	0.06	0.06	0.06	0.06	0.06	0.09	0.10
	(0.07)	(0.09)	(0.09)	(0.09)	(0.07)	(0.08)	(0.07)	(0.09)	(0.08)	(0.10)
High school	0.10	0.11	0.11	0.11	0.10	0.12	0.10	0.11	0.17	0.18
	(0.13)	(0.14)	(0.12)	(0.13)	(0.13)	(0.14)	(0.13)	(0.13)	(0.12)	(0.13)
Some college	0.09	0.10	0.09	0.10	0.09	0.10	0.09	0.10	0.15	0.17
	(0.11)	(0.12)	(0.10)	(0.12)	(0.11)	(0.11)	(0.11)	(0.12)	(0.10)	(0.11)
College	0.16	0.14	0.16	0.14	0.16	0.15	0.16	0.13	0.26	0.22
	(0.21)	(0.19)	(0.19)	(0.18)	(0.21)	(0.20)	(0.21)	(0.18)	(0.21)	(0.18)
Population (100,000s)	2.50	0.55	17.03	0.87	2.50	0.79	2.50	1.11	2.53	1.14
	(6.03)	(0.42)	(25.23)	(0.70)	(6.03)	(0.76)	(6.03)	(0.86)	(6.04)	(0.89)
Officer safety	0.24	0.46	0.16	0.41	0.24	0.33	0.24	0.39	0.25	0.39
	(0.38)	(0.47)	(0.32)	(0.45)	(0.38)	(0.43)	(0.38)	(0.44)	(0.39)	(0.44)
Violent crime index (100s)	3.11	0.84	14.69	1.30	3.11	1.33	3.11	1.63	2.73	1.50
	(9.46)	(0.99)	(36.75)	(1.47)	(9.46)	(1.64)	(9.46)	(1.67)	(7.42)	(1.52)
Property crime index (100s)	6.22	2.80	18.46	4.09	6.22	4.33	6.22	5.07	5.21	4.44
	(11.06)	(2.88)	(35.39)	(4.33)	(11.06)	(5.31)	(11.06)	(4.94)	(8.75)	(4.47)
Officer wage	31.51	32.02	33.81	34.11	31.51	31.41	31.51	34.56	35.29	38.46
	(10.54)	(11.12)	(10.56)	(11.80)	(10.54)	(11.40)	(10.54)	(11.86)	(11.66)	(12.93)
Share of black officers	0.12	0.07	0.15	0.06	0.12	0.09	0.12	0.06	0.12	0.06
	(0.12)	(0.11)	(0.12)	(0.09)	(0.12)	(0.12)	(0.12)	(0.08)	(0.11)	(0.09)
Population density (10,000s per mile)	0.39	0.36	0.75	0.39	0.39	0.33	0.39	0.38	0.39	0.38
	(0.32)	(0.37)	(0.78)	(0.37)	(0.32)	(0.28)	(0.32)	(0.33)	(0.32)	(0.34)
2008 pres. democratic vote share	0.65	0.56	0.69	0.56	0.65	0.60	0.65	0.55	0.65	0.55
	(0.15)	(0.15)	(0.14)	(0.15)	(0.15)	(0.15)	(0.15)	(0.14)	(0.15)	(0.14)
N	22327	1296129	22327	1296129	22327	1296129	22327	1296129	22327	1296129

Notes: This table displays the 2013 average values for places that eventually have a Black Lives Matters protest (treated) and for places that do not have a Black Lives Matter protests before 2020 (Control). The column titles refer to different weights described in Section 3. Population refers to weighting by 2013 annual population, IPW Controls refers to weighting by the inverse probability of eventually having a protest using 2013 control variables, IPW Unit refers to weighting by the inverse probability of eventually having a protest using annual incidents of lethal force as covariates, and IPW Unit-Time refers to weighting by the product of IPW unit and IPW time weights.

Table 2: Impact of Black Lives Matter Protests on Police Homicides

	(1)	(2)	(3)	(4)	(5)	(6)
%ΔLethal Force	-0.168	-0.198	-0.186	-0.186	-0.188	-0.211
	(0.045)	(0.053)	(0.049)	(0.063)	(0.059)	(0.079)
ΔTotal Lethal Force	316	373	350	349	265	297
	(84.5)	(99.6)	(92.1)	(118)	(83.0)	(111)
Average outcome pre-protest ($\overline{\mathrm{Y}/\mathrm{N}_{-1}}$)	0.368	0.000	0.368	0.368	0.276	0.276
Average normalization pre-protest ($\overline{\mathrm{N}}_{-1}$)	1	261,320	1	1	1	1
Total place-quarters after protest (e)	5100	5100	5100	5100	5100	5100
Total lethal force post-protest	1,815	1,815	1,815	1,815	1,815	1,815
Places with protests	283	283	283	283	283	283
Places without protests	1,265	1,265	1,265	1,265	1,265	1,265
Total number of protests	1,654	1,654	1,654	1,654	1,654	1,654
Total number of protesters	343,230	343,230	343,230	343,230	343,230	343,230
Number of cohorts	13	13	13	13	13	13
Sample size	1,318,456	1,318,456	1,318,456	1,318,456	1,318,456	1,318,456
Normalization	None	Popula-tion	None	None	None	None
Population weights		✓				
Pre-treatment control inverse probability weights			✓			
Event-place inverse probability weights				✓		✓
Event-quarter inverse probability weights					✓	✓

Notes: This table reports the benchmark estimates detailed in Section 3. All regressions control for population decile, a linear population-population decile interaction, cohort-census place, and cohort-time (quarterly) fixed effects. Standard errors are clustered by census place and reported in parenthesis.

Table 3: Protest Effect Heterogeneity by Size and Frequency

	(1)	(2)	(3)	(4)	(5)
Maximum protest size					
Quartile 1 (≤ 40)	-0.106	-0.121	-0.122	-0.227	-0.200
	(0.105)	(0.107)	(0.107)	(0.116)	(0.120)
Quartile 2 (≤ 100)	-0.049	-0.077	-0.095	-0.136	-0.118
	(0.090)	(0.089)	(0.089)	(0.104)	(0.112)
Quartile 3 (≤ 300)	-0.041	-0.090	-0.062	-0.158	-0.135
	(0.079)	(0.085)	(0.082)	(0.134)	(0.135)
Quartile 4 (> 300)	-0.165	-0.220	-0.212	-0.263	-0.217
	(0.069)	(0.065)	(0.066)	(0.093)	(0.081)
Total number of protests					
Quartile 1 (≤ 1)	-0.056	-0.066	-0.070	-0.099	-0.079
	(0.103)	(0.104)	(0.105)	(0.123)	(0.124)
Quartile 2 (≤ 2)	-0.204	-0.239	-0.240	-0.374	-0.349
	(0.139)	(0.141)	(0.140)	(0.161)	(0.167)
Quartile 3 (≤ 5)	0.034	0.011	-0.025	-0.117	-0.085
	(0.091)	(0.091)	(0.091)	(0.108)	(0.121)
Quartile 4 (> 5)	-0.153	-0.205	-0.189	-0.248	-0.221
	(0.060)	(0.056)	(0.057)	(0.086)	(0.079)
Cohort-place fixed effects	✓	✓	✓	✓	✓
Cohort-time fixed effects	✓	✓	✓	✓	✓
Population controls		✓	✓	✓	✓
Consent decress controls			✓	✓	✓
Cohort-place linear time trend				✓	✓
Cohort-time-population fixed effects					✓

Notes: This table assess heterogeneity in the impact of protests by the size and frequency of protests using a modification of the ordinary least squares regression given by Equation 3. The protest indicators are interacted with either the marximum protest size quartile or the total number of protest quartile. All regressions control for population decile, a linear population-population decile interaction, cohort-census place, and cohort-time (quarterly) fixed effects. Standard errors are clustered by census place and reported in parenthesis.

Table 4: Robustness of Estimates to Normalization

	(1)	(2)	(3)	(4)	(5)
%ΔLethal Force	-0.168	-0.137	-0.406	1.401	2.380
	(0.045)	(0.070)	(0.282)	(0.988)	(1.746)
ΔTotal Lethal Force	316	272	1,141	-3,824	-6,625
	(84.5)	(139)	(792)	(2,698)	(4,861)
Average outcome pre-protest ($\overline{Y/N_{-1}}$)	0.368	0.000	0.001	0.002	0.001
Average normalization pre-protest ($\overline{N}_{-1}$)	1	261,320	739	301	915
Total place-quarters after protest (e)	5100	5100	5100	5100	5100
Total lethal force post-protest	1,815	1,815	1,815	1,815	1,815
Places with protests	283	283	283	283	283
Places without protests	1,265	1,265	1,265	1,265	1,265
Total number of protests	1,654	1,654	1,654	1,654	1,654
Total number of protesters	343,230	343,230	343,230	343,230	343,230
Benchmark	None	Population	Officers	Violent Arrests	Total Arrests
Sample size	1,318,456	1,318,456	800,504	1,146,908	1,157,089

Notes: This table reports the robustness of the estimates to using various benchmark variables (dividing policing homicides by a variable prior to the regression). All regressions control for population decile, a linear population-population decile interaction, cohort-census place, and cohort-time (quarterly) fixed effects. Standard errors are clustered by census place and reported in parenthesis.

Table 5: Roboustness of Estimates to Different Population Screens

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
%ΔLethal Force	-0.168	-0.169	-0.168	-0.163	-0.160	-0.201	-0.191
	(0.045)	(0.046)	(0.047)	(0.047)	(0.050)	(0.061)	(0.096)
ΔTotal Lethal Force	316	312	304	294	285	332	257
	(84.5)	(84.9)	(84.9)	(84.7)	(89.3)	(101)	(129)
Average outcome pre-protest ($\overline{\mathrm{Y}/\mathrm{N}_{-1}}$)	0.368	0.448	0.555	0.619	0.710	0.961	1.152
Average normalization pre-protest ($\overline{\mathrm{N}}_{-1}$)	1	1	1	1	1	1	1
Total place-quarters after protest (e)	5100	4117	3256	2912	2516	1722	1168
Total lethal force post-protest	1,815	1,772	1,710	1,672	1,641	1,504	1,304
Places with protests	283	223	174	154	132	90	61
Places without protests	1,265	552	290	169	99	26	6
Total number of protests	1,654	1,525	1,443	1,406	1,353	1,207	1,080
Total number of protesters	343,230	326,669	318,463	315,766	309,218	290,730	274,522
Population screen	20,000	40,000	60,000	80,000	100,000	175,000	250,000
Number of cohorts	13	13	12	9	8	7	6
Sample size	1,318,456	581,186	285,042	130,420	72,045	21,029	7,209

Notes: This table reports the robustness of the estimates to using various population screens (omitting observations with a population below the screen at any time during the sample). All regressions control for population decile, a linear population-population decile interaction, cohort-census place, and cohort-time (quarterly) fixed effects. Standard errors are clustered by census place and reported in parenthesis.

Table 6: Roboustness of Estimates to Regression Specification

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
%ΔLethal Force	-0.121	-0.101	-0.238	-0.168	-0.169	-0.126	-0.113	-0.170	-0.182	-0.181
	(0.048)	(0.040)	(0.066)	(0.045)	(0.046)	(0.049)	(0.049)	(0.057)	(0.078)	(0.104)
ΔTotal Lethal Force	228	191	447	316	318	237	213	319	343	254
	(90.2)	(75.1)	(124)	(84.5)	(86.4)	(92.1)	(92.1)	(107)	(147)	(146)
Average outcome pre-protest ($\overline{Y/N}_{-1}$)	0.368	0.368	0.368	0.368	0.368	0.368	0.368	0.368	0.368	0.276
Average normalization pre-protest ($\overline{N}_{-1}$)	1	1	1	1	1	1	1	1	1	1
Total place-quarters after protest (e)	5100	5100	5100	5100	5100	5100	5100	5100	5100	5100
Total lethal force post-protest	1,815	1,815	1,815	1,815	1,815	1,815	1,815	1,815	1,815	1,815
Places with protests	283	283	283	283	283	283	283	283	283	283
Places without protests	1,265	1,265	1,265	1,265	1,265	1,265	1,265	1,265	1,265	1,265
Total number of protests	1,654	1,654	1,654	1,654	1,654	1,654	1,654	1,654	1,654	1,654
Total number of protesters	343,230	343,230	343,230	343,230	343,230	343,230	343,230	343,230	343,230	343,230
Number of cohorts	13	13	13	13	13	13	13	13	13	13
Sample size	1,318,456	1,318,456	1,318,456	1,318,456	1,318,456	463,564	448,112	448,112	448,112	448,112
Cohort-place fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort-time fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort-time-population quintile fixed effects		✓								
Cohort-place linear time trend			✓							
Population controls				✓	✓	✓	✓	✓	✓	✓
Consent decree controls					✓	✓	✓	✓	✓	
Demographic and labor market controls						✓	✓	✓	✓	
Crime controls							✓	✓	✓	✓
Pre-treatment control inverse probability weights								✓		
Event-place inverse probability weights									✓	
Event-place and event-quarter inverse probability weights										✓

Notes: This table reports the robustness of the estimates to using various benchmark specifications of time variant control variables, inverse probability weights, and fixed effects. Standard errors are clustered by census place and reported in parenthesis.

Table 7: Effect of Protests on Police Homicides by Race or Alleged Arming of Victim

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
%ΔLethal Force	-0.084	-0.145	-0.132	-0.095	-0.113	-0.110	-0.121	-0.235
	(0.043)	(0.062)	(0.085)	(0.086)	(0.083)	(0.118)	(0.084)	(0.121)
ΔTotal Lethal Force	158	274	81	71	83	82	65	126
	(81.1)	(117)	(52.0)	(64.1)	(60.6)	(87.9)	(45.1)	(64.9)
Average outcome pre-protest ($\overline{Y/N_{-1}}$)	0.342	0.000	0.111	0.000	0.132	0.000	0.097	0.000
Average normalization pre-protest ($\overline{N}_{-1}$)	1	245080	1	141521	1	53746	1	245080
Total place-quarters after protest (e)	5525	5525	5525	5525	5525	5525	5525	5525
Total lethal force post-protest	2,765	2,765	836	836	1,095	1,095	778	778
Places with protests	314	314	314	314	314	314	314	314
Places without protests	1,257	1,257	1,257	1,257	1,257	1,257	1,257	1,257
Total number of protests	1,753	1,753	1,753	1,753	1,753	1,753	1,753	1,753
Total number of protesters	350,150	350,150	350,150	350,150	350,150	350,150	350,150	350,150
Sample size	43,988	43,988	37,604	37,604	37,604	37,604	43,988	43,988
Police homicide subset	Total	Total	White	White	Black	Black	Unarmed	Unarmed
Benchmark	None	Population	None	White	None	Black	None	Population
Weight	None	Population	None	White	None	Black	None	Population

Notes: This table reports the robustness of the estimates to using different data from Mapping Police Violence and decomposes incidents of lethal force by race or alleged arming of the victim. All regressions control for population decile, a linear population-population decile interaction, census place, and time (quarterly) fixed effects. Standard errors are clustered by census place and reported in parenthesis.

Table 8: Impact of Black Lives Matter Protests on Police Agency Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Impact of protest (%Δ)	1.037	0.437	1.185	-0.060	-0.000	-0.019	-0.378	0.026	0.103	-0.051	0.113	-0.134
	(0.271)	(0.159)	(0.363)	(0.022)	(0.009)	(0.011)	(0.197)	(0.020)	(0.044)	(0.073)	(0.145)	(0.068)
Average outcome pre-protest ($\overline{Y/N_{-1}}$)	0.115	256	118	213	685	1,105	0.144	0.980	168	16.9	208	467
Places with protests	200	119	119	119	119	119	119	119	119	363	363	363
Places without protests	3,608	851	851	851	851	851	851	851	851	16,465	16,465	16,465
Total number of protests	1,235	357	357	357	357	357	357	357	357	6,794	6,794	6,794
Total number of protesters	309,423	50,596	50,596	50,596	50,596	50,596	50,596	50,596	50,596	1,271,143	1,271,143	1,271,143
Sample size	107,912	1,892	1,915	1,953	1,953	1,929	1,964	1,990	1,936	174,886	174,886	174,875
Outcome	Body cameras	Patrol officers	Sara officers	Black officers	White officers	Exp. officers	College required	Force doc.	Budget (millions)	Officer assault	Violent arrests	Property arrests
Years	2010-2016	2013, 2016	2013, 2016	2013, 2016	2013, 2016	2013, 2016	2013, 2016	2013, 2016	2013, 2016	2000-2019	2010-2019	2010-2019
Time unit	Quarter	Annual	Annual	Annual	Annual	Annual	Annual	Annual	Annual	Annual	Annual	Annual

Notes: This table reports the impact of Black Lives Matter protests on various police agency characteristics. The data for police agency characteristics for column one comes from the 2016 Law Enforcement Administrative Statistics (LEMAS) Body-Worn Camera supplement, for columns 2-9 from the 2013 and 2016 LEMAS, and for columns 10-12 from Jacob Kaplan's concatenated files of the Uniform Crime Reporting. All regressions control for census place and time fixed effects. Standard errors are clustered by census place and reported in parenthesis. Estimates using the LEMAS data, columns 1-9, are weighted by the sampling weight. Body cameras are an indicator for the agency having body cameras, Patrol officers are the number of officers with designated geographic patrol areas, SARA officers refer to the number of officers encouraged to engage in SARA-type problem-solving projects, Black officers is the number of Black officers, White officers is the number of White officers, Exp. officers are the number of officers less the number of newly recruited officers during the survey year, College required indicates the agency requires newly hired officers to have at least a two-year college degree, Force doc. indicates the agency requires documentation when any one of the following types of force is used: chemical, gun discharge, gun display, or neck restraint, Budget is the agency's annual operating budget in millions of dollars, Violent arrests is the annual count of arrests made by the agency for violent crimes, Property arrests is the annual count of arrests made by the agency for property crimes, and Officer assault is the annual count of police officers assaulted at the agency.