

Does Protest Against Police Violence Matter? Evidence from U.S. Cities, 1990 through 2019

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Abstract

An underlying premise of democratic politics is that protest can be an effective form of civic engagement that shapes policy changes desired by marginalized groups. But it is not certain that this premise holds up under scrutiny. This article presents a three-part argument that protest (1) signals the salience of a movement's focal issue and expands awareness that an issue is a social problem requiring a solution, (2) empowers residents in disadvantaged communities and raises a sense of community cohesion, which together (3) raise costs and exert pressure on elites to make concessions. The empirical analysis examines the likelihood that a city will establish a civilian review board (CRB). It then compares the effects of protest and CRB presence on counts of officer-involved fatalities by race and ethnicity. Two main hypotheses about the effect of protest are supported: cities with more protest against police brutality are significantly more likely to establish a CRB, and protest against police brutality reduces officer-involved fatalities for African American and Latino (but not for White) individuals. However, the establishment of CRBs does not reduce fatalities, as some have hoped. Nonetheless, mobilizing against police brutality matters, even in the absence of civilian review boards.

Keywords

police violence, racial inequality, protest, social movements

Racial tensions have escalated in the wake of deadly encounters with the police, which have spread a climate of fear and distrust in Black and Latino communities and among police officers serving those communities. This study examines whether the current protest movement opposing police brutality has had any tangible effects on two consequences desired by this social movement: implementing civilian review boards (CRBs) and reducing police-involved fatalities.¹ My key argument is that protest activity produces desired outcomes by signaling the salience of an issue and empowering communities. In turn, these two forces threaten the status quo by exerting pressure and threatening the power of elites and authorities, which increases the likelihood

they will make concessions to movement demands.

By adopting this approach we gain new leverage over a fundamental theoretical question about the consequence of civic engagement: do citizens' protests influence policy outcomes (Skocpol and Fiorina 1999)? Exploring this question has implications for a related body of research that seeks to discover

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if social movements matter, and, if so, under what conditions (e.g., see the review in Amenta, Andrews, and Caren 2019). Given the urgency of finding solutions to problems associated with racial inequality and disparities in policing, this is an opportune time to investigate the connections among these issues empirically.

An underlying assumption in the study of politics is that protest activity is a critical component of civil society that improves the functioning of a democratic polity (Baumgartner and Leech 1998; Gillion 2013). This assumption is credible in part because social movements—such as the civil rights movement, the LGBTQ movement, or the movement against police brutality—are commonly organized by underrepresented or marginalized groups who have relatively limited access to power (Amenta et al. 2010; Weldon 2011). As a result, protest frequently offers the only viable avenue for disadvantaged groups to express discontent (Andrews 2001).

DOES PROTEST MATTER?

Although many scholars assume protest has some effect on policy outcomes (Enos, Kaufman, and Sands 2019; Gamson 1975; Weldon 2011), reviews comparing research findings on this question from the United States and elsewhere cast doubt on this claim (Burstein 2021; Giugni, McAdam, and Tilly 1999). For example, Giugni (2007) finds that protest activity in the ecology, antinuclear, and peace movements had little effect on policy in the absence of support from allies and public opinion. Many scholars have found that protest alone is not sufficient to bring about policy change.

A variety of reasons might explain the null findings. Protest may have differential effects at different stages of the policymaking process. For instance, Soule and King (2008) find that protest has more potent effects at the early stages of the legislative process, but this effect is overpowered by public opinion in the final stages. Burstein and Linton's (2002) metanalysis also indicates that once public

opinion is taken into account, social movement organizations have little influence on policy. Using a political mediation argument, other scholars argue that protest will be successful when it can demonstrate that elites will benefit from their support (Biggs and Andrews 2015; King 2008). Political opportunity structure theorists maintain that success occurs when the balance of power shifts, or when allies are in power (Meyer 2004).

Others have found that protest matters to some types of outcomes more than others: it increases media attention, influences Congressional hearings and voting outcomes favoring protest issues, and improves government funding for desired programs (Amenta et al. 2019; McAdam and Su 2002; Olzak and Ryo 2007; Olzak and Soule 2009).

There is scattered evidence that protest affects desired outcomes when there is a direct correspondence between a movement's specific demands and outcomes. For example, consider Soule and Olzak's (2004) finding that Phyllis Schlafly's Eagle Forum, which mobilized anti-ERA protests aimed at state legislatures, was highly successful in defeating this legislation but more diffuse women's movement organizations had no effect. In an analysis of movement activity by ethnic and racial minorities during 1960 through 1995, Gillion (2013) discovered that protest significantly influenced the passage of minority rights legislation and shaped Supreme Court decisions favorable to the movement. Examining the early 1960s, Biggs and Andrews (2015) show that sit-in protests significantly increased desegregation in cities in the South. In a more recent period, Muñoz, Olzak, and Soule (2018) find that U.S. states with more environmental protest activity reduced carbon emissions significantly from 1990 through 2007, controlling for the enactment of relevant policies. These studies suggest protests are effective when they express specific goals and purposes.

The influence of protest on desired outcomes is also shaped by the resource environment. Movement success depends in part on resource capacity (Gamson 1975), which suggests that disadvantaged groups engaging

in protest will face obstacles to achieving movement goals, one of which may be policy change. But because protest is frequently a tactic of the powerless (or those with less access to power), it likely motivates more members from underrepresented groups to engage in the political process.

Studies of protest against police violence in disadvantaged minority neighborhoods illustrate this point. If over-zealous policing activities occur more frequently in minority neighborhoods (Epp, Maynard-Moody, and Haider-Markel 2014; Gelman, Fagan, and Kiss 2012), such actions likely raise resentment. Resentment, in turn, likely mobilizes an aggrieved community (Biggs and Andrews 2015). When racial disparities in economic well-being overlap with statistics showing police mistreatment of minorities, demands for police reform gain traction.² The BLM campaign against police violence has gained momentum in part by drawing attention to multiple aspects of racial injustice (Williamson, Trump, and Einstein 2018:401).³

CHANGES IN POLICING AND POLICE VIOLENCE

Since 1980, substantial changes in policing practices have encouraged more preemptive stops by officers for minor traffic infractions (Goel, Rao, and Shroff 2016). In addition, police departments expanded officers' discretion in handling suspects (as opposed to following strict guidelines) (Epp et al. 2014). As a result, the rising numbers of arrests fueled a rise in mass incarceration rates, disproportionately affecting poor communities and communities of color (see Pager 2007; Western and Muller 2013). During the crack-cocaine epidemic in the 1990s, police in urban America were given wide-ranging discretion that led to higher numbers of arrests and mistreatment of minorities in particular (Epp et al. 2014).

Some scholars trace the current deterioration in police-minority relations to policies associated with the "broken windows" argument (Fagan et al. 2009; Kelling and Wilson 1982). Applied to policing, these ideas

suggest (without evidence) that even mild signs of urban decay (broken windows) can encourage high crime rates (see Sampson and Raudenbush 1999). The policy implication of this perspective is that the use of "stop and frisk" and "zero tolerance" of drugs and crime are needed in areas with higher risks of violent crime. Furthermore, Epp and colleagues (2014) trace the source of racial disparities in police stops to widespread deployment of "investigative" stops in minority communities, whereby police disproportionately target individuals for minor infractions, but then proceed to use more intrusive and invasive actions that often include force.

The evidence on whether these changes in policing practices increased the use of deadly force by police is mixed. Zimring's (2017) analysis using data on police-involved killings from *The Guardian* finds no relationship between changes in patrol policies and police killings in Philadelphia from 2007 to 2015. In contrast, Eckhouse (2018) discovered that when New York moved away from patrol policies of "stop, question, and frisk" in 2013, it significantly reduced Black individuals' exposure to police stops, leading Eckhouse (2018:24–25) to conclude: "Reducing the number of occasions on which black citizens involuntarily interact with police is likely to substantively reduce black Americans' exposure to police violence."

Studies supporting this exposure argument find that the rates of "stop and frisk" events, arrests for misdemeanors, and deaths of civilians are significantly higher for Black and Latino individuals than for other groups.⁴ Some claim this is especially true for Black men (Bejan et al. 2018; Fryer 2018). However, Edwards, Lee, and Esposito (2019) use data from National Vital Statistics to show that police-involved fatality rates of Asian Americans are lower than for Whites, and that police-involved shootings of Native Americans are higher than the rate for African Americans.⁵ Gelman and colleagues (2012) analyzed 125,000 pedestrian stops by the New York Police Department and found that persons of African American and Latino

descent were more frequently stopped even when precinct and participation in crime by race were taken into account.

Scholars find that persistent racial inequalities in income, residential segregation, and implicit biases shape expectations that escalate violence during encounters between minorities and police officers (Gilbert and Ray 2016).⁶ Racial minorities are more likely to be stopped, questioned, or arrested by police, controlling for suspicious behaviors, presence of weapons, neighborhood poverty, and the racial composition of neighborhoods (Eberhardt et al. 2004; Goel et al. 2016; Gross and Mann 2017; Jones 2016; Ross 2015; but see Correll et al. 2007). Edwards and colleagues (2019) find that a Black man's chance of dying by police-involved shootings was 2.5 times higher than for a non-Latino White man (the rate for Latino men was 1.3 to 1.4 times higher).⁷ Recognition of such racial disparities in the treatment of minorities has generated the demand for more police accountability throughout the United States.

COMMUNITY RESPONSE: DEMANDING CIVILIAN OVERSIGHT

In response to demands to end police violence, civil rights campaigns during the 1960s frequently included pleas to establish CRBs, arguing that they provide for more accountability of police when compared to conventional methods of handling complaints about police misbehavior (i.e., internally within police departments) (Finn 2001). A CRB is a citizen-run committee or board and is defined as an "agency or procedure that involves participation by persons who are not sworn officers (citizens) in the review of citizen complaints against the police and/or other allegations of misconduct by police officers" (Walker and Archbold 2014:2). The authority of these boards ranges enormously across cities. Some CRBs can only make recommendations (usually to the police chief), whereas others have subpoena power and the ability to fire police officers (Ofer 2016).

The benefits of establishing a CRB can be evaluated from different vantage points. From the perspective of civil-rights activists and reform-minded citizens, the establishment of a CRB might be considered a successful movement outcome. Yet studies show that numbers of reported complaints of police brutality rise following the establishment of complaint procedures, such as CRBs (see also Edelman, Uggen, and Erlanger 1999; McVeigh, Welch, and Bjarnason 2003). Such increases could lend support to those opposed to these policy changes. But Pryor and colleagues (2019) found more complaints of police brutality were filed in cities with CRBs because they establish a legitimate channel for making complaints.⁸

Critics often accuse CRBs of being ineffective if they lack independent authority to investigate, issue subpoenas, or prosecute (Ray 2020; Walker and Archbold 2014). For instance, Green and Aldebron's (2019) analysis concludes that few CRBs have the capability to sustain complaints of police brutality. Farber and Kalbfield (2019) find that of over 10,000 citizen complaints filed in Chicago, only 5.7 percent were sustained. Such statistics undermine support for CRBs.

Despite these drawbacks, demands for more independent CRBs have been endorsed by policy analysts, activists, and ordinary citizens engaging in protest across the United States (e.g., see Campaign Zero 2021b). These movement activists express the hope that CRBs will bring transparency and accountability to police departments, which could increase levels of trust between citizens and the police. The protest movement against police violence rests on demands for civilian review boards with more effective authority and for police departments to impose more restrictions on the use of force (Ray 2020).

THEORETICAL ARGUMENT

My core argument is that protest produces both policy change and changes in police behaviors concerning lethal violence through three key mechanisms: signaling, community empowerment, and threats to elites in power.

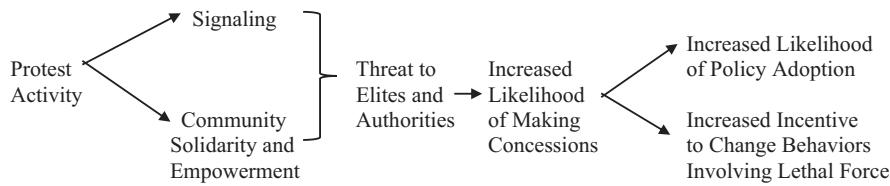


Figure 1. Mechanisms Connecting Protest Activity to Social Movement Outcomes

Figure 1 displays the connections among these mechanisms that link protest activity to these two outcomes.

The argument in Figure 1 implies that protest activates a signaling mechanism and increases community solidarity that empowers disadvantaged groups. Both mechanisms threaten the power and control of elites and authorities by challenging the status quo and by shifting the balance of power in a community. These threats, in turn, raise costs to inaction by elites and provide incentives to make concessions to protesters' demands. Such concessions increase the likelihood that policy change will occur, and they provide incentives to change behaviors related to the use of force during encounters between police and citizens.

Protest as a Signaling Mechanism

Protest sends a powerful signal to elites and authorities that a community demands a change in the status quo (Rafail, Walker, and McCarthy 2015; Williamson et al. 2018; Wouters and Walgrave 2017; Vélez et al. 2015). Protest activity expands the scope of recognition and concern about an issue (Walgrave and Vliegenthart 2012). Peak periods of protests surrounding an issue (such as occurred in Ferguson, Missouri, or in Minneapolis, Minnesota) increase its salience, which raises public concern and debate (Vandegrift and Connor 2020; Weitzer 2017). Such awareness increases the likelihood the issue will be framed as a social problem deserving of attention (Cress and Snow 2000; Hilgartner and Bosk 1988; Wouters and Walgrave 2017).

Increases in protest activity also signal that a movement's capacity to mobilize resources is rising (Edwards and Gillham

2013; Olzak and Ryo 2007). Rising levels of resources facilitate an expanded support base (McCarthy and Zald 1977), which increases a movement's chances of attaining concessions (Amenta et al. 2019; Gamson 1975).

Protest attracts media attention, which further amplifies the scope of protest movements' demands. Dramatic events—such as large-scale protests—capture media attention that expands the audience beyond local media markets (Amenta et al. 2010; Giugni 2007; Rafail et al. 2015). For example, the year 2014 witnessed widespread protest over the police shooting of Michael Brown on August 10, 2014. Figure 2 provides some supporting evidence on this point. It shows that Google recorded a spike in the number of online searches for the topic “police violence” in August 2014, just after the deaths of Eric Garner and Michael Brown. But searches for topics related to police violence did not reach their peak until December, which witnessed huge protests over police violence in many cities. Note that these trends in searches peak well after an event has occurred, seemingly tracking the gradual mobilization efforts by BLM and other local community organizations well into the following weeks and months.

Community Empowerment and Solidarity

Mobilization in the form of protest creates new movement affiliations and sustains existing ties. Protest can expand the scope of civic engagement, which potentially increases the involvement of a more diverse population in the decision-making process. Such increases in civic engagement produce a heightened sense of community solidarity (Gross and

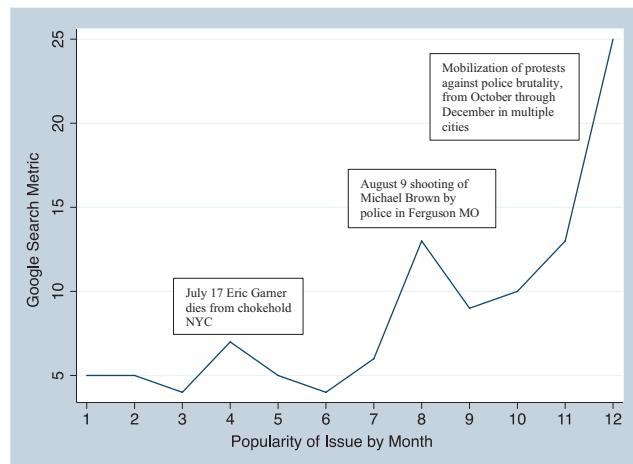


Figure 2. Trends in Google Searches for Police Violence in 2014

Source: Google Trends (<https://trends.google.com/trends/?geo=US>).

Note: Google Metric uses a popularity metric that is equal to 100 when an issue has reached its peak. This graph shows that searches for police violence increased five-fold from January to December 2014.

Mann 2017; Sampson, Raudenbush, and Earls 1997).

Protest and other forms of civic engagement raise perceptions of community efficacy (Jones 2016). Increases in collective efficacy can change the power balance in communities. For instance, Sharkey (2018) finds a dramatic decrease in rates of crime in poor, urban communities where and when there has been recent mobilization activity. He argues this is partly due to collective actions that established community-policing programs, neighborhood watches, and local self-help organizations. Sampson and colleagues (2005) offer another relevant example. Based on research by Skogan and Hartnett (1997) and Fung (2004), Sampson and colleagues (2005:710) reveal how engagement in community politics in Chicago created a sense of empowerment in a disadvantaged community:

Residents did not spontaneously begin attending beat meetings as a result of local social ties; rather the meetings had their origin in a structural initiative—by organizing, hosting, and supporting beat meetings, the police created an equal opportunity

for involvement across the city. . . . In an interesting way, Chicago's policing program has evened the playing field with regard to opportunities to participate in community governance, with the greatest increase in collective participation seen by African-Americans.

I make a parallel argument here: protest that expands the scope of civic engagement will increase the involvement of a more diverse population in the decision-making process.

Perceptions of Threat to Elites

Protest that signals movement support, spreads awareness of an issue, and empowers a group that is newly engaged in political action raises perceptions that the existing balance of power is under threat (Biggs and Andrews 2015). This is because a shift in the balance of power puts pressure on elites and authorities whose positions and policies depend on maintaining the status quo. These threats imply that elites and authorities risk losing political power, support, and elections if they ignore protester demands.

Theoretical Conclusion

Because protest threatens to raise political and material costs to elites, protest increases the chance that elites will make concessions to protesters' demands. Furthermore, a high volume of protest raises the visibility and recognition of protester demands, amplifying these costs. This implies that concessions by elites to protester demands will be more likely during peak cycles of protest.

Hypotheses

The arguments above imply that a higher volume of protest against police brutality in a city makes it more likely that police violence will be framed as an urgent problem requiring a change in policing policies. To test these ideas, Hypothesis 1 suggests that *cities with more protests against police violence will have a higher likelihood of establishing a CRB, controlling for population composition, levels of violent crime, and racial disparities in income.*

To the extent that policies related to CRBs increase police accountability, they ought to energize and empower residents. This implies that collective efficacy would be most powerful in disadvantaged communities that have experienced functioning CRBs over time. Such an argument implies that the longevity of CRBs should gradually reduce the prevalence of fatal encounters between police and minority residents. Hypothesis 2 is that *officer-involved fatalities will be reduced as the length of time since a CRB has been established increases.*

If over-zealous policing and racial bias are associated with higher police-involved fatalities of African Americans and Latinos, then social protest will raise costs to authorities. These costs, in turn, create incentives to implement policies that reduce the use of deadly force. This implies that cities with more protest activity ought to see a reduction in fatality rates, especially for minority groups. The third hypothesis holds that *cities with more protest against police violence will have lower rates of fatal encounters between minorities and police, net of the presence*

of a CRB (but protest will leave the number of fatal encounters with White individuals unaffected).

RESEARCH DESIGN AND MEASURES

I designed the CRB project to follow 170 U.S. cities (with populations greater than 100,000) beginning in 1980 through the end of 2018. The aim was to create an event-history dataset to examine the effect of prior protests on the rate of founding a CRB. Because the sources of data on protest events differ for the two subperiods (1980–1989 and 1990–2018), I concentrate on reporting the results on CRB foundings from 1991 through 2018.⁹ In addition, prior to 1989, the number of CRB foundings is very small, which hampers estimation of the effects on the founding rate of CRBs over the early 1980 through 1989 period (see Appendix Table A1). Thus, I analyze the effect of protest from 1990 through 2017 on CRB foundings from 1991 through 2018.

For the 1990 through 2018 period, I rely on sources of data on protest from Newsbank, which includes news reports from thousands of local newspapers. Most important, the Newsbank data allow us to characterize protest events by geographic focus. Specifically, these data provide information about whether a protest focuses on (1) local events/issues, (2) national/general issues, or (3) an event taking place in another city.¹⁰

A focus on protest events from 1990 through 2018 confers several advantages. Over this period, public concern with police violence in the United States has increased.¹¹ At the same time, this interval includes events from the early 1990s, which witnessed a major uptick in a crack-cocaine epidemic that coincided with an upsurge in aggressive campaigns of urban policing (Epp et al. 2014).

Measuring Concessions

Analysis of CRB foundings and police-involved fatalities represents two ways to examine the theoretical argument that protest

increases the chance authorities will make concessions. Many types of concessions to movements are possible, but in the first analysis, concessions take the form of establishing a CRB. This analysis relies in part on mission statements by BLM and Campaign Zero (2021a), which maintain that CRBs can potentially improve police accountability.¹²

In the second analysis, concessions take the form of restraining police use of force. In this research decision, I follow Zimring (2017) and Eberhardt (2016); this work suggests police departments that restrain the use of force have significantly lower police-involved fatalities. There is no consensus on the definition of the use of force (National Institute of Justice 2020), but strategies for reducing police violence involve written regulations (see Campaign Zero 2021b), raising sanctions due to citizen complaints, implementing active training sessions (Eberhardt 2016), and changing messages conveyed to officers by heads of police departments. Finally, police themselves may change behaviors, perhaps in reaction to community sentiment. All of these factors have in common the goal of rethinking the norms and behaviors related to the use of force by police.

DEPENDENT VARIABLES

The first dependent variable is the hazard of establishing a CRB from 1991 through 2018. The second dependent variable is a count of police-involved fatalities of Black, Latino, and White individuals, 2001 through the end of 2019.

Civilian Review Boards

Coding information on CRBs uses three vital pieces of information from each city's police department website: establishment date, scope of authority, and meeting schedule. I consulted city websites, online news reports, and published city histories of CRBs to corroborate this information (e.g., Fairley 2020; Finn 2001; Green and Aldebron 2019; Hickman and Poore 2016; Ofer 2016; Schneider,

Agee, and Chronopoulos 2020; Walker and Archbold 2014). Online news reports were especially useful for updating information on CRB programs that were recently established, defunct, or never met.

For 11 cities, this information was missing or contradictory. Phone calls to each of these police departments were extremely helpful. All spokespersons were able to identify a CRB establishment date and tell us whether or not the CRB had independent authority, if one existed. Using this full set of information, my research team and I then coded each CRB's history: its establishment, its functioning, and in some cases, its dissolution. I calculate the age of a CRB in each city (cities lacking CRBs had an age of zero). This information allows an examination of Hypotheses 1 and 2.

Because event-history methods incorporate information on the timing of events, these methods are useful for analyzing the rate at which a specific outcome occurs (see Olzak 1989). This method follows a set of relevant units, such as cities or countries over time, until they experience one or more specified events (or do not experience such events). These methods take the timing of events into account explicitly, which allows researchers to compare rates across units. To test Hypothesis 1, I use event-history analysis to estimate the length of time it takes for a city to establish a CRB. I chose to use a flexible piecewise-exponential model that allows the hazard to vary in an unconstrained manner at preselected time points (determined by inspection of the cumulative hazard curves and other diagnostics). Use of the piecewise-exponential parameterization allows for each time segment to have its own baseline rate of CRB establishment. This means the hazard can vary during earlier, middle, or late periods of time when the city (or some other unit) is at risk of establishing a CRB.

The dataset of observations on CRB foundings includes cities that have not yet established a CRB by a particular date. In 1980, less than 6 percent of the full set of 170 largest population cities in the United

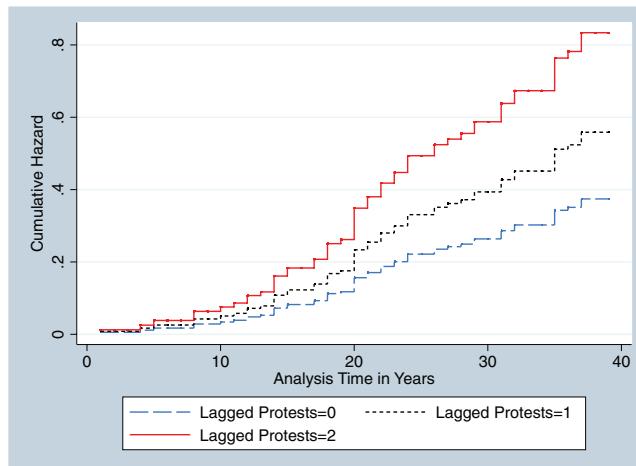


Figure 3. Cox Proportional Hazards Regression; Effect of Different Levels of Protest on the Rate of Establishing a CRB

States had a CRB. But by the end of 2018, 76 (45 percent) of the original 170 cities had one. Ten early adopters include Los Angeles (1920s), Minneapolis (1965), Kansas City, MO (1970), Dayton (1971), Berkeley and Orlando (1973), Flint and Detroit (1974), Lincoln (1975), and Memphis (1977). Observations on cities begin on January 1, 1991 (and include cities that have not yet established a CRB by that date). The unit of analysis is a center city in a year that was served by a corresponding single police department.¹³

Figure 3 compares the cumulative hazards of establishing a CRB from 1991 through the end of 2018, for cities with zero, one, or two protests in a year. The graph suggests the protest count increases the hazard of founding a CRB, and the effect of protest rises over time. The latter point provides evidence that a piecewise-exponential model would be appropriate because it allows the hazard to vary across time periods.

Some CRBs exist in name only, a fact we will return to in the Discussion section. By the end of the observation period, only 53 cities had a CRB that met at least once a year. By the end of 2018, 20 percent of cities had a CRB with independent authority (80 percent had either no CRB or had one that lacked authority). Appendix Table A1 lists the founding dates by city.

Officer-Related Fatalities

Empirical analysis of incidents involving police violence has been hampered by several obstacles. Departments are not required to collect such data, and, even if they do, they are not required to release this information (Farber and Kalbfleld 2019). Many scholars have analyzed the Law Enforcement Management and Administrative Statistics (LEMAS) dataset on citizen complaints filed (on force, language, disrespect, and coercion). But this source is plagued by the fact that different procedures are used in different departments, and there are discrepancies in whether reports concern alleged or verified complaints (Beck and Uchida 2019; Garner et al. 2018; Hickman and Poore 2016; Wiersema, Loftin, and McDowell 2000; Williams, Bowman, and Jung 2019). In response to such criticism, several organizations began archiving media and public records sources and making them available online.¹⁴

Data on officer-involved fatalities used in this article combine all unique events archived in two of these sources: *Fatal Encounters* and *Mapping Police Violence*. The *Fatal Encounters* dataset contains information on police-involved fatalities from 2000 through 2019, taken from news reports, television videos, Google listings, and crowdsourcing.¹⁵ The second source is *Mapping Police Violence*,¹⁶

an online archive that, in 2013, began tracking persons who died as the result of “being chased, beaten, arrested, restrained, shot, pepper sprayed, tasered, or otherwise harmed by police officers.” I combined these two sources and dropped duplicate records. These data distinguish fatalities of Black, Latino, and White individuals.

INDEPENDENT VARIABLES

Protest against Police Brutality

For the period beginning in 1990, my research group and I use information from Newsbank to gather data on protests against police brutality. This source provides a comprehensive archive of reports drawn from 3,960 local U.S. newspapers or news outlets (such as the Associated Press). To develop a reliable coding scheme using Newsbank sources, we compared the performance of 25 different word string combinations (e.g., “demonstrations AND police,” “demonstrations AND brutality,” “protest AND violence,” “demonstration AND police-involved shootings,” “protesters AND police violence,” “protest AND police brutality,” “protest AND police shootings, “march* against police”) for all 170 cities across a randomly drawn set of years. We kept track of each search string and its specific listings. (See the online supplement for the coding manual.)

This iterative procedure yielded the maximum number of relevant events using “Protest* AND Police AND Brutality.” For each city, we added another set of search terms with the name of the city, then replaced the city name with the state to see if additional events in that city were reported. We used the date option to further filter our results. Coders were instructed to code more than one event if they were contained within a single report.

No method of protest event analysis is perfect, but we are convinced this method captured all if not most of the relevant protest events that were reported. The broad scope of our method means we also uncovered many irrelevant reports (e.g., duplicate reporting

of the same event, follow-up commentary, letters to the editor, comments by celebrities). For instance, a search of the terms “protest and police and brutality” for Las Vegas, Nevada, 2000 through 2019, yielded 77 reports, but only seven events fit the criteria of being a public, collective protest against police violence. Editorials and letters to the editor were dropped unless they contributed information on a protest event not reported elsewhere. In addition, a single news report could contain more than one relevant event. Contrary to our expectations, substituting the words “demonstration,” “march,” or “rally” for “protest” yielded fewer relevant events.

Our search of the online Newsbank archives yielded 1,228 protests against police brutality in 170 cities from 1990 through the end of 2018. The majority of protest events (71.4 percent) expressed solely *local* concerns. These were protests over specific events that occurred, actions taken or not taken by a city’s police department, or misbehavior by officers; this also included complaints about a CRB’s decision to acquit one or more officers and protests over other decisions made about complaints against police brutality filed in that city. Proximity to an event likely heightens awareness. Thus, I anticipate that protest with a local focus will have the strongest effect on the founding rate of CRBs and on fatalities.

Another 10.1 percent of events (124) expressed more general, *nationwide* concerns about police brutality and unequal treatment by the police by race or expressed support for a national movement to reduce police violence. An example of this category occurred in Bakersfield, California, in March 2017, when the Walk for Justice group organized an event to “bring awareness” of police brutality.

The final category of 227 events (18.5 percent) protested the occurrence of police brutality in *another city*. These types of events happened in many cities where groups protested the death of George Floyd (in Minneapolis, Minnesota).

Research on protest movements often compares the consequences of violent and

nonviolent events. With a few memorable exceptions (e.g., the protests following the Rodney King verdict and those that followed the death of Oscar Grant in Oakland), there were surprisingly few protests against police brutality where protesters used violence (against persons, property, or buildings).¹⁷

Organizational Dimensions of Protest

Approximately half of all protest events listed one or more organizations represented at the event. The vast majority of these organizations were locally based (as opposed to national organizations such as Black Lives Matter or the NAACP). These organizations were frequently named after the person who died during an encounter with police. Although a few rallies were large, information on protest size shows the majority of demonstrations from 1990 through 2018 were small, an average of 25 to 50 persons. Comparison of reports including and excluding one or more named organizations showed no significant differences, and therefore I did not include this information in the analysis.¹⁸

CONTROL VARIABLES

Persistent racial disparities in income, segregation, and violent crime increase minorities' exposure to the police. Higher rates of exposure to police increase the chances these encounters will turn violent (Edwards et al. 2019; Johnson et al. 2019). In response, poor and minority communities have mobilized campaigns pushing for more civilian oversight and police accountability. However, rising crime and disorder can be used as reasons for objecting to civilian review boards, if they are seen as a threat to police control (Schneider et al. 2020).

Poverty, crime, and racial residential segregation are empirically related to higher rates of crime, especially violent offenses (Sampson, Wilson, and Katz 2018). If efforts to combat high crime rates entail the use of more aggressive policing techniques in cities, then these factors of disadvantage will

be associated with more police-involved fatalities.

To control for these potentially confounding factors, I include measures of population size, racial composition, wealth, violent crime rates, and racial disparities in income. To ensure proper time ordering (and to avoid simultaneity) all measures are lagged at least one year (but see the fatalities analyses for a comparison of different lag structures). These measures are from the U.S. Census and are updated every 10 years. For the years 1980 through 2000, these indicators were provided by Stephanie Kent (see Carmichael and Kent 2017; Jacobs and Carmichael 2002; Jacobs and Wood 1999; Kent 2010). Interpolation procedures allow for the estimation of annual measures of these city characteristics between census years. All three population numbers (population, percent Black, and percent Latino) are logged due to skewness. In addition, Black and Latino populations are unevenly distributed across these cities. Logging these measures brings them closer to a normal distribution, which facilitates modeling.

Black residential segregation is measured with the isolation index. This index is defined as the probability a Black resident shares a census district area with another Black person (the exposure index indicates the probability of sharing residence with a non-Black person). Many scholars argue that the isolation index is preferable to other alternatives because it explicitly measures a group's relatively low level of exposure to other racial groups (Massey and Denton 1988:288).

Another control variable is the level of violent crime, taken from the FBI's Uniform Crime Reporting Statistics (UCRS). This is the aggregated count of murder and non-negligent manslaughter, rape, robbery, and aggravated assault in a city per million residents.

I also included a measure for the presence of Black mayors. Saltzstein (1989:539) argues that Black mayors raise minority representation in police departments and increase the tendency of city officials to respond to minority interests. She reports that "the presence of black mayors is the single best predictor

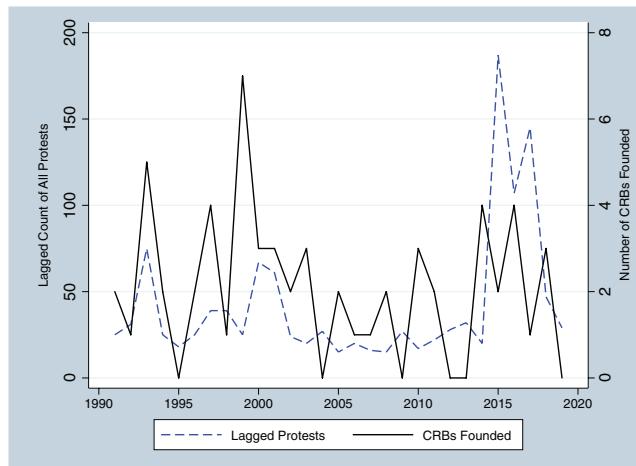


Figure 4. Annual CRB Foundings and Lagged Protest against Police Violence

of the adoption of civilian review boards with authority to investigate citizen complaints.” Accordingly, the analysis includes a dummy variable indicating whether the city has a Black mayor in the focal year. Stephanie Kent provided me with relevant information through 2000 (Kent 2010), which I extended through 2018 by searching city websites and newspaper records of election results.

Structural features of a city’s governing bodies likely influence whether protest movement demands receive attention. Comparing the two prevalent types of city governance in the United States suggests cities with a mayor–council form would be more open to citizen demands than would the more bureaucratic city–manager form. Because mayors and city council members are elected directly, whereas city managers are hired by city councils, and thus citizens have more direct access to officials, I expect cities with a mayor–council form will be more likely to respond to protester demands (Svara and Watson 2010).

Appendix Table A2 reports the descriptive statistics and correlations among city-level covariates, each lagged one year from the dependent variable. There are few surprises here: a city’s police force is highly correlated (.62) with population size. As others have found, the natural log of percent Black is positively correlated with the violent crime

rate (.49) and racial isolation (.93). Because the correlation between percent Black and Black isolation is high, in the CRB analysis I removed the isolation measure from the model, but this did not affect the results, and the coefficient for percent Black remained statistically insignificant.

CRB FOUNDING ANALYSIS

Modeling Decision and Results

The first analysis focuses on the effects of city characteristics on the hazard of establishing a CRB. To analyze founding rates, I use a piecewise-exponential specification described earlier. To ensure proper time ordering of the effects and to follow the logic of the theoretical argument, protest and all other independent variables are lagged one year or more. After exploring different lag structures for protest effects, I found a one-year lag had the greatest effect on CRBs (a lag of two years was positive, but not statistically significant).

Figure 4 graphs the number of one-year lagged protests and number of CRBs established, 1991 through 2018. Peak protest periods were followed by a rise in the number of CRBs, and the rate increases steeply after 2014. This peak corresponds to the emergence of the Black Lives Matter movement nationwide.

Table 1. Effect of Protests with a Local, National, or Distant Focus on the Founding Rate of Civilian Review Boards, 1991 through 2018, in 156 U.S. Cities

	(1)	(2)	(3)	(4)	(5)
<i>Time Pieces</i>					
1 < 10 years	-17.374*** (2.503)	-17.165*** (2.412)	-17.363*** (2.103)	-17.549*** (2.094)	-17.315*** (2.404)
10 > u < 29 years	-16.560*** (2.193)	-16.187*** (2.113)	-15.908*** (1.830)	-16.298*** (1.841)	-16.543*** (2.140)
<i>Protests</i>					
Count of All Protests	.604*** (.110)				
Protests with Local Focus		.561*** (.130)			.576**** (.131)
Protests with National Focus			.006 (1.019)		-.189 (1.89)
Protests with Focus on Events in Another City				1.402*** (.287)	1.374*** (.303)
<i>Demographic Controls</i>					
Ln Percent Black	.941 (.699)	.977 (.703)	1.076 (.642)	1.049 (.640)	.977 (.701)
Ln Percent Hispanic	.689*** (.260)	.685** (.255)	.622* (.250)	.621* (.249)	.695** (.255)
Violent Crime Rate in Millions	-.008* (.004)	-.008* (.004)	-.004 (.003)	-.004 (.003)	-.008* (.004)
Median Family Income in 1,000s	.031* (.015)	.029* (.014)	.029* (.013)	.030* (.014)	.030* (.015)
Ratio of Black to White Median Income	-8.525*** (1.740)	-8.626*** (1.692)	-8.329*** (1.554)	-8.180*** (1.528)	-8.514*** (1.651)
Ln Black Isolation Index	-.461 (.728)	-.510 (.720)	-.636 (.618)	-.619 (.628)	-.508 (.724)
<i>City Characteristics</i>					
Black Mayor (0,1)	.781 (.610)	.731 (.610)	.077 (.601)	-.013 (.606)	.688 (.637)
Number of Sworn Officers (in 100s)	-.018 (.010)	-.017 (.010)	-.009* (.004)	-.009* (.004)	-.017 (.011)
Mayor–Council Form of Government	.367 (.368)	.360 (.478)	.287 (.450)	.316 (.433)	.375 (.462)
Offset Parameter Ln of Population Size	1.00	1.00	1.00	1.00	1.00
Number of Spells	3,465	3,465	3,465	3,465	3,465
Wald Chi-Square	13497***	13140***	10857***	12039***	13579***

Note: Estimates using *stpiece* command in Stata (14.2). All observations clustered by state location. All covariates lagged one year. Robust standard errors are in parentheses.

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

Table 1 gives the estimated effects for the theoretically-relevant variables on the hazard of establishing a CRB from 1991 through 2018. The effect of all protests (undifferentiated by focus) is positive and statistically

significant in column 1. The results in column 1 of Table 1 report a coefficient of .604 for the effect of all protests combined. This means one protest in a city would raise the likelihood of establishing a CRB by 82 percent,

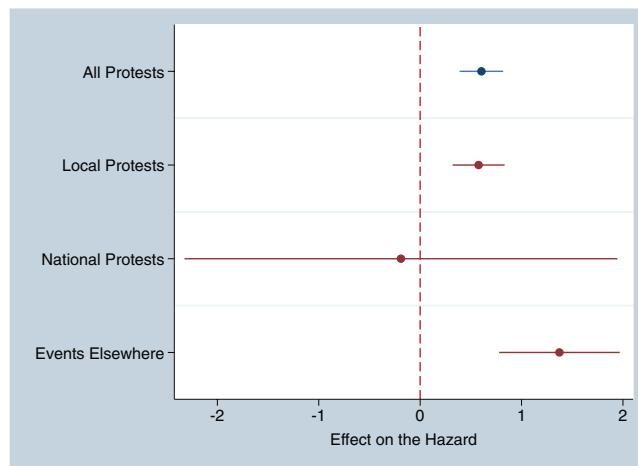


Figure 5. Effect of Protest on the Founding Rate of CRBs: 1991 through 2018

compared to a city-year without a single protest.

Over the next four columns, we see that it is useful to distinguish protests by geographic focus: Table 1 shows that a protest's focus on a specific event, either in the local setting or on a specific violent event taking place elsewhere, matters most. In contrast, protests expressing broad concerns about police brutality nationwide seem to have little effect.

Figure 5 uses the coefficients from the full model in column 5 of Table 1 to plot the point estimates for the effects of protests with different geographic foci. Figure 5 shows that the effect of "national events" is close to zero, with a large confidence interval. Local protests and protests against police violence that occurred in some other city had strong positive effects on this rate. Stepping back from the results, the finding regarding the influence of events taking place elsewhere makes sense. Protests over events with a high degree of salience that have diffused more easily may carry more weight (Gross and Mann 2017).

Few of the control variables have significant effects on the rate. Larger and wealthier cities are more likely to have the resources to establish a CRB. Minority population size increases the rate, but not always significantly so.

Nevertheless, measures of racial disparity matter. CRBs are more likely where the ratio

of Black to White income is low, which is consistent with the fact that racial inequalities generate grievances that can influence policymakers.¹⁹ However, the effect of having a higher rate of violent crime decreases the chances of implementing a CRB, and this effect is statistically significant in three out of five models.

Few other characteristics influence the establishment of a CRB. The effect of having a mayor–council form of government is positive, but it is not significant. Nor does having a Black mayor significantly affect the chances of having a CRB. Cities with larger police forces are generally less likely to establish CRBs compared to cities with smaller forces (net of the effect of protest counts). These findings are consistent with the suspicion that larger police forces have more political clout to resist CRBs.

Although it would add substantially to this study, measuring police union strength in a city turns out to be more difficult than originally thought. As a proxy for political leverage of uniformed officers, I searched for data on the unionization of departments, but found little variation in unionization rates.²⁰ The LEMAS data include a measure of the collective bargaining rights of sworn personnel, but these data are available only for three time points (2000, 2007, and 2013). Note,

Table 2. Comparison of Different Estimation Procedures: Effects of Three Types of Protest on the Rate of CRB Foundings, 1991 through 2018

	Piecewise-Exponential (<i>stpiece</i>)	Exponential (<i>streg</i>)	Cox PH (<i>stcox</i>)	Weibull (<i>streg</i>)
Count of Protests with Focus on Local Events	.576**** (.130)	.621*** (.130)	.437*** (.098)	.361*** (.093)
Count of Protests with National/General Focus	-.189 (1.089)	-.105 (1.088)	-.691 (.977)	-.575 (.940)
Count of Protests Concerning Events in Another City	1.374*** (.303)	1.422*** (.291)	.577* (.187)	.852*** (.176)

Note: All models include control variables: ln percent Black, ln percent Latino, violent crime rate per million, median household income, ratio of Black to White income, Black isolation index, Black mayor, number of sworn officers, mayor–council government, and population size. All covariates are lagged one year, and observations are clustered by state.

however, that the bivariate correlation of collective bargaining rights and the presence of a CRB is weakly positive (.025) for nearly 2,000 police departments surveyed in 2007.

Because most CRBs lack the authority to subpoena officers and witnesses, publish public reports, and render a final judgment on a citizen’s complaint, it seemed reasonable to replicate the analysis using the establishment of CRBs with independent authority as the dependent variable. In this analysis, a city’s lagged protest count has a stronger effect on the rate of establishing a CRB with an independent authority (when compared to the effect of all protests on the establishment of all CRBs). For local protests, having one protest in the previous year doubles the rate of establishing a CRB with independent authority from 1991 through 2018. For protests about events that happened elsewhere, the rate increases by 5-fold. These results add more evidence in support of my main argument that mobilization matters.

Robustness Checks: CRB Analysis

Table 2 presents robustness checks that examine whether the effects found for protest focus hold up under different model specifications. I compare piecewise-exponential estimates to two parametric models (exponential and Weibull), in addition to the Cox proportional

hazard model.²¹ Even though these are quite different specifications, the patterns are the same: across all specifications, events that are locally focused or focused on an event that happened elsewhere significantly and positively increase the hazard of founding a CRB in a city.

Some readers may wonder if effects of these “sympathy protests” (protest over an event taking place in another city) were due to the surge in BLM protests that took place from 2014 to 2016. Fortunately, the piecewise-exponential model can be used to directly test this hypothesis by reestimating a model that uses a new time piece for the 2014 through 2016 interval. As might be expected, the effect of this time interval significantly raises the hazard of adopting a CRB. However, even when using this specification, the effect of lagged number of sympathy events remains positive and significant. Taken together, this evidence reinforces the findings regarding the power of protest to change policy.

OFFICER-RELATED FATALITIES

Modeling Decision and Results

Does protest affect fatalities in the same way it affects CRBs? The dependent variable used to answer this question is the annual

Table 3. Cities with Highest Number of Police-Involved Fatalities by Group, 2000 through 2019

City	Total Fatalities	Annual Mean	Standard Deviation
<i>Black</i>			
Chicago, IL	237	11.9	7.2
Houston, TX	164	8.2	5.0
Los Angeles, CA	108	5.4	2.6
Philadelphia, PA	106	5.3	3.7
Dallas, TX	82	4.1	2.8
170 Cities, 2000 to 2019	3,076	.90	1.9
<i>Latino</i>			
Los Angeles, CA	187	9.4	3.2
San Antonio, TX	118	5.9	4.6
Phoenix, AZ	68	3.4	3.7
Tucson, AZ	48	2.4	1.3
Albuquerque, NM	39	2.4	2.7
170 Cities, 2000 to 2019	1,682	.50	1.43
<i>White</i>			
Phoenix, AZ	83	4.2	1.4
Houston, TX	77	3.9	2.8
Las Vegas, NV	74	3.7	2.3
Los Angeles, CA	54	2.7	2.1
San Antonio, TX	47	2.4	1.8
170 Cities, 2000 to 2019	2,198	.65	1.24

count of officer-related fatalities in a city for three race/ethnic groups: Blacks, Latinos, and Whites. The mean number of annual fatalities in a city in the sample is .90, .49, and .65 for each of these groups, respectively. Table 3 reports the cities that contributed the highest number of deaths for each group from 2000 through 2019. The annual mean number of Black fatalities is highest in Chicago (nearly 12 persons per year), but Latino deaths in Los Angeles are not far behind (over 9 per year). The last row in each column reports the total number of police-related fatalities and overall mean number of fatalities for each group in all 170 cities for 2000 through 2019.

Given the structure of the data, count models are appropriate. I explored several estimation procedures appropriate for analyzing count data. Based on these diagnostics and the knowledge that fatality counts are overdispersed and there are many city-years with zero fatalities in the data, I settled on the negative binomial panel model (*xtnbreg* in

Stata).²² I explored several other alternatives, including poisson regression (*xtpoisson*), mixed-level negative binomial including level two for state-level effects (*menbreg*), maximum likelihood estimators in poisson regression (*ppml*), and pooled OLS with cross-sectional dependence (*xtscc* in Stata). Although the parameterizations are different, the pattern of effects remains the same across all these methods for estimating effects on count data.

Effects of Protest Focus on Fatalities

I first compared the effects of protest by distinguishing its geographic focus: (1) local events, (2) national or general concerns with police brutality, or (3) an event of police violence that occurred elsewhere. To ensure proper time ordering of protest with outcomes, I explored different lag structures of one-, two-, and three-year lags (as I did in the CRB analysis). I found that only

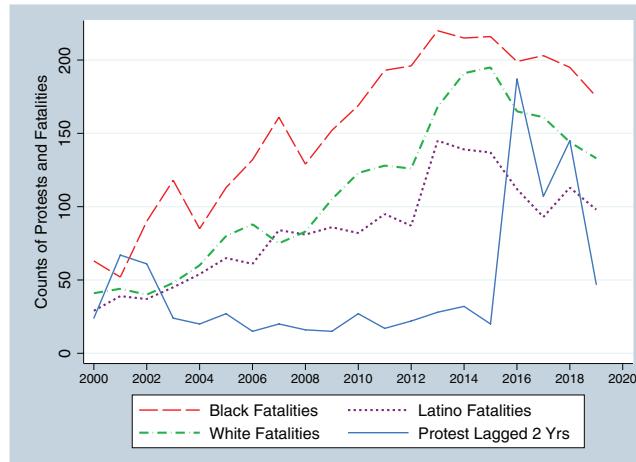


Figure 6. Annual Count of Lagged Protests and Fatalities, 2000 through 2019

locally-focused events had any significant effect on fatalities (protests with a focus on another city's events or a national-level focus had no significant effect on fatalities). Using a lag of two years improved the fit of the model, suggesting the effect of protest might play out gradually over time. For simplicity, I focus on only locally-oriented protests here.

Figure 6 graphs the annual number of locally-focused protests (lagged two years) and the number of officer-related fatalities for Black, Latino, and White individuals, 2000 through 2019. Black fatalities far outnumber those of other groups for most years; White fatalities follow a similar time trend and outnumber fatalities of Latinos for most years (which is not surprising, given the numerical differences in population size in many cities).

The results suggest local protest closely tracked the peak years of Black fatalities. Is this just an effect of the surge in BLM protests? Similar to the CRB analysis, I specified a dummy variable that equals one for the years 2014 through 2016, zero otherwise. This measure was positive and significant, but this effect disappears when a time trend is included. Most importantly, the effect of local protests (lagged two years) remains negative and significant on Black fatalities with this new specification (and no other effects on any other variables were found).

Table 4 displays the results for testing Hypothesis 2, that the length of time a CRB has operated in a city will lower police fatalities. There are important differences across all three groups for other measures, but the relevant finding for CRBs is that the length of time a CRB has been functioning has no significant effect on officer-related fatalities for any race/ethnic group. This result runs counter to Hypothesis 2.

Table 4 shows that locally-focused protest has divergent effects on subsequent police violence across the three race/ethnic groups. The key result is the significant and negative effect of local protests against police brutality on fatalities of Black and Latino individuals, but not for White individuals. This pattern supports Hypothesis 3. The estimated effect of -101 for African Americans in column 1 in Table 4 implies that just one protest in a city would reduce fatalities by 11 percent (the mean number of African American fatalities in a city year is .90, so one protest would reduce fatalities to .80, or by 11 percent). This result is also consistent with movement claims that minorities face systemic racial and ethnic disparities in their treatment by police.²³ The results are consistent with a similar analysis conducted at the state level from 2010 to 2017, which found BLM protest significantly decreased Black individuals' fatal encounters with police (Skoy 2021).

Table 4. Negative Binomial Estimates of the Effect of Local Protest on the Number of Officer-Involved Fatalities of Black, Latino, and White Individuals in a City-Year, 2001 through 2018

	Black Fatalities	Latino Fatalities	White Fatalities
<i>Prior Protest Count</i>			
Number of Local Protests (lagged two years) ^a	-.101*** (.029)	-.067* (.032)	-.026 (.033)
<i>Lagged Dependent Variable</i>			
Lagged Fatalities of Group	.023** (.008)	.036** (.012)	.047** (.016)
<i>CRB Longevity</i>			
CRB Age	.002 (.003)	.001 (.003)	-.003 (.003)
<i>Demographic Controls</i>			
Ln Percent Black	.473*** (.132)	-.439*** (.124)	-.513*** (.118)
Ln Percent Latino	.083 (.061)	1.049*** (.087)	-.075 (.064)
Violent Crime Rate/Millions	.001 (.001)	.001 (.001)	.001 (.001)
Median Family Income in 1,000s	-.010* (.004)	-.009* (.005)	-.018*** (.004)
Ratio of Black to White Median Income	-.627* (.297)	-1.121*** (.290)	-.795** (.262)
Ln Black Isolation Index	.278* (.135)	.151 (.112)	.258* (.114)
<i>City Characteristics</i>			
Black Mayor	-.214 (.114)	-.163 (.206)	.027 (.147)
Number of Sworn Officers in 100s	-.008*** (.002)	-.003** (.001)	-.009*** (.002)
Mayor–Council Form Of Government	-.223 (.127)	-.099 (.118)	-.007 (.130)
Time Trend	.061*** (.007)	.047*** (.008)	.089*** (.007)
Offset Parameter for Ln Population Size	1.00	1.00	1.00
Constant	-13.941*** (.516)	-14.511*** (.588)	-10.658*** (.490)
Number of Observations	3,060	3,060	3,060
Log Likelihood	-3068	-1881	-2859
Wald Chi-Square (13 df)	436***	437***	307***

Note: Estimates using *xtnbreg* command in Stata (14.2). Standard errors are in parentheses.

^aAll other covariates lagged one year.

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

The pattern of results for control variables on fatalities is unsurprising: a group's larger proportion in the population raises their fatality count. Controlling for population size, it is interesting that violent crime has no effect on fatalities, suggesting a high rate of violent

crime in a city is not necessarily a factor in officer-related fatalities (at least in the aggregate, not at the event level). Cities with higher household incomes have significantly fewer officer-related fatalities for all groups,²⁴ as do cities with higher Black/White income

ratios.²⁵ The effect of Black individuals' residential isolation has a positive and (weakly) significant effect on Black fatalities, which may be affected by the high correlation of this measure with the log of percent Black (.93). When the measure for Black isolation is included but percent Black is dropped, the effect of Black isolation has a positive and larger effect on Black fatalities, which suggests this measure is capturing some of the variance related to the size of the Black population.²⁶

Body Camera Policies

The implementation of a CRB is just one option open to city politicians and police departments for improving police accountability. Since 2015, the establishment of policies mandating the use of body cameras has received considerable policy and scholarly attention (for a review, see Maskaly et al. 2017). I located two comprehensive sources of information that list the establishment date of body camera programs, their scope, and public access rules: Access to Police Body-Worn Camera Video (Reporters Committee for Freedom of the Press 2020) and Police Born-Worn Camera Legislation Tracker (2018). These archives detail all U.S. body-camera legislation passed at the state level. To capture differences within states, I also consider variation in local laws concerning public access to video, restrictions on police access, and other factors.

I used this information to code the establishment date and level of mandate for each state listed by these sources. The variable "body-camera-mandate" was coded 1 if legislation had been enacted that required police to wear body cameras at all times (this was the most stringent mandate), 0 otherwise. The category "otherwise" includes cases in which states mandated body cameras but allowed local authorities to override, amend, or ignore this mandate. As of June 2018, 16 of the 170 cities required police to use body cameras at all times.

Table 5 highlights the effect of body cameras on fatalities, including the same set of

covariates used to estimate the effect of protest and CRBs on fatalities. It shows that body camera mandates significantly reduce Black and Latino fatalities. However, it is important to note that most of these mandates were implemented in recent years, and these years also witnessed a slight decline in fatalities (see Figure 6). The effect of lagged protests remains strong for Black fatalities. Thus, these results are offered as supporting evidence, but the results are by no means conclusive.²⁷ Because the implementation of body camera policies is very recent, it would be premature to conduct a founding rate analysis on them (as was done with CRB foundings).

Robustness Checks: Coarsened Exact Matching

The assessment of causality in observational studies presents several challenges, including different chances of assignment to a treatment, unobserved heterogeneity, and sample selection bias. For example, in this study, it is likely that cities experiencing protests are systematically different from others, which might bias the results. To gain some leverage over this problem, I turned to coarsened exact matching (CEM). CEM reduces the imbalance in covariates that exists between treated and control groups, defined here as cities that had protests versus cities with no protests.

Given existing criticism concerning bias that hampers causal inferences when using observational studies (Morgan and Winship 2015), the results shown here may be due to an unbalanced distribution of factors among cities (Iacus, King, and Porro 2011). I use coarsened exact matching techniques to address the potential imbalance among covariates. Winter and Sampson (2017) offer an excellent example of the utility of this method.²⁸

The coarsened exact matching method is designed to "control for potentially confounding influence of pretreatment control variables in observational data" (Iacus, King, and Porro 2012:1). The CEM uses a "binning" algorithm to coarsen values of

Table 5. Negative Binomial Estimates of the Effect of Body Cameras on the Number of Officer-Involved Fatalities of Black, Latino, and White Individuals in a City-Year, 2001 through 2019

	Black Fatalities	Latino Fatalities	White Fatalities
<i>Body Cameras</i>			
Body Cameras Mandate	-.323* (.154)	-.562* (.283)	-.409 (.227)
<i>Prior Protest Count</i>			
Locally-Focused Protests (two-year lag)	-.099*** (.029)	-.066* (.032)	-.023 (.033)
<i>Lagged Dependent Variable</i>			
Lagged Fatalities of Group	.024** (.008)	.034** (.012)	.046** (.016)
<i>CRB Longevity</i>			
CRB Age	.002 (.003)	.001 (.003)	-.003 (.003)
<i>Demographic Controls</i>			
Ln Percent Black	.469*** (.133)	-.444*** (.125)	-.515*** (.119)
Ln Percent Latino	.087 (.062)	1.056*** (.088)	-.075 (.064)
Violent Crime Rate/Millions	.001 (.001)	.001 (.002)	.001 (.001)
Median Family Income 1,000s	-.009* (.004)	-.009* (.005)	-.018*** (.004)
Ratio of Black to White Median Income	-.678* (.299)	-1.200*** (.295)	-.837** (.264)
Ln Black Isolation Index	.289* (.136)	.153 (.114)	.257* (.115)
<i>City Characteristics</i>			
Black Mayor	-.212 (.114)	-.170 (.206)	.027 (.147)
Number of Sworn Officers in 100s	-.008*** (.002)	-.003** (.001)	-.009*** (.002)
Mayor–Council Form of Government	-.218 (.128)	-.093 (.120)	-.017 (.131)
Time Trend	.063*** (.007)	.050*** (.008)	.091*** (.007)
Offset Parameter for Ln Population Size	1.00	1.00	1.00
Constant	-13.951*** (.520)	-14.456*** (.594)	-10.633*** (.492)
Number of Observations	3,060	3,060	3,060
Log Likelihood	-3066	-1879	-2857
Wald Chi-Square (13 df)	438***	435***	312***

Note: Estimates using *xtnbreg* command in Stata (14.2). All covariates lagged one year. Standard errors are in parentheses.

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

theoretically-important covariates to match controls with treatment observations. The procedure then drops observations that lack

a match, producing a smaller dataset that can be used for further analysis. The goal is to reduce imbalance between treatment (cities

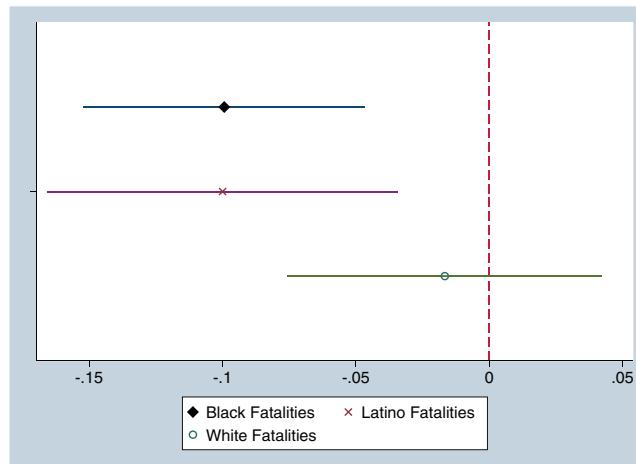


Figure 7. Effect of Local Protests on Fatalities in Each Group

Note: Using CEM matching with weights, NBREG estimates in Appendix Table A3.

with protest) and controls (Iacus, King, and Porro 2009).

Several changes to the data were required to use the CEM method. Because CEM requires a bivariate treatment effect, I collapsed information on the count of protests into a dichotomous variable that equals one if there was at least one protest in a year, zero otherwise.

The \mathcal{L} statistic is a measure of imbalance, which ranges from a low of 0 to a high of 1, with higher scores indicating maximum imbalance between cities with and without protest. CEM produced a multivariate distance score (\mathcal{L}) of .57. The CEM procedure estimates an original imbalance (\mathcal{L}) using uncoarsened measures of .988, which suggests a considerable drop in imbalance using CEM. My choice of variables was informed by the substantive importance of each measure to the outcome. Variables include lagged count of Black fatalities, log of population size, log of Black population size, log of Black isolation, and number of sworn officers. The CEM procedure uses variable cutoff points to produce various strata that are used to create the matched sample. I specified the cutoff points at the quartile ranges for the variables. For technical details using Stata, see Blackwell and colleagues (2009).

I then analyze the effect of CRBs on fatalities using the matched sample (see Appendix Table A3) and *nbreg*. The procedure *xtnbreg* cannot be used here because CEM requires constant weights (for a similar strategy using OLS and weighted CEM, see Winter and Sampson 2017).

The coefficients from the CEM analysis in Appendix Table A3 are remarkably similar to those seen in Table 4 despite the differences among models and reduced sample size. Comparing the results from the CEM weighted results in Table A3 to Table 4, we see that the effects of lagged local protests are nearly identical for Black individuals (the coefficients are -0.101 and -0.099 , respectively). For White individuals, the effect of protest is negligible in both analyses. The exploration of treatment effects turned out to be useful in correcting for imbalance among cities: for Latino individuals, the effect of protest is stronger using the CEM technique. The coefficient for the effect of protest on Latino fatalities is -0.067 in Table 4 and -0.100 for the CEM-weighted results in Table A3.

Figure 7 plots the effect of local protests on fatalities for each group using the weighted CEM procedure that produced the coefficients in Appendix Table A3. Because the CEM procedure aims to reduce imbalance,

this difference makes sense if cities with a higher percentage of Latinos are systematically different on one or more dimensions. Use of this matching method lends more support to the argument that protest reduces police-involved fatalities for minorities.

Although causality cannot be completely verified, the results using the coarsened exact matching method lend credibility to the original conjecture that protest against police brutality reduces Black and Latino fatalities significantly. It also bears repeating that despite the shortcomings of observational data, analyses using it can still inform significant questions about relationships and associations.

DISCUSSION AND CONCLUSIONS

The findings regarding the effect of protest on CRBs and fatalities may be surprising to some, because they run counter to studies that find protest has little direct effect on policy outcomes desired by social movements. Some scholars explain these null findings by arguing that the effect of protest depends on having political allies or favorable public opinion on an issue (Amenta et al. 2010; Giugni 2007; Olzak and Soule 2009; Weaver 2014). Unfortunately, no city-level surveys of public opinion exist for all cities in this study that cover the period studied here. But it is revealing that a majority of Americans now support Black Lives Matter (although these percentages have recently declined, see Thomas and Horowitz 2020).

The theoretical argument contributes several new insights for existing theoretical explanations of the effect of protest on policy outcomes desired by social movements. I offer the argument that protest itself changes perceptions of police behavior and mobilizes ordinary citizens through a series of interconnected mechanisms. Protest achieves desired outcomes when it signals the importance of an issue and empowers an aggrieved community to take action. These processes, in turn, pose a threat to existing elites and authorities,

raising the cost of maintaining the status quo, which provides incentives for authorities to make concessions.

A second theoretical contribution concerns the effect of a local versus national focus of protest. What do these results imply for theories of protest movements? There is little prior theory to guide us, but several differences among these types of events provide some clues. Local events and protest over events taking place in another city (e.g., Rodney King protests outside Los Angeles in 1992) tend to be about a specific victim. They often involve an unarmed victim, use of chokeholds, or severe beatings. It seems plausible that, given these characteristics, dramatic cases of extreme force by police are more likely to evoke stronger emotions in residents living in the same city as the event. Further research examining the emotions stirred during encounters between local citizens and the police could be useful in addressing these ideas (for a review of emotions and social movements, see Jasper 2011).

The adage that “all politics are local” rings true here. In contrast to the majority of studies that examine national social movements (e.g., the civil rights, women’s, and environmental movements), my results suggest activism targeted to local concerns has a greater chance of success. If this finding holds more generally, then previous studies reporting null effects of protest may have missed the effect of local protests, if the effects of protest with different foci cancel each other. To my knowledge, few other studies (if any) have compared the effect of protests with different geographic foci.

Another theoretical insight offered by my argument is that protest transforms a community, activating and empowering ordinary citizens to press for changes in social policy. This implies that protest can invigorate city politics in multiple ways: it expands the scope of civic engagement to include more members of marginalized groups. For example, CRBs could recruit underrepresented minorities to serve on citizen advisory boards. Diversification of citizens’ involvement in

city politics is an admirable goal that could go far in addressing concerns about civic engagement of marginalized groups. As the diversity of cities increases, providing more access to decision-making may become even more important in the future.

These insights also contribute to the broader debate about the connection of protest to democratic politics. Protest has been conceptualized as a critical component of civic engagement that sustains the vitality of democratic politics (Gillion 2013; Skocpol and Fiorina 1999). Some influential scholars argue that the overall decline in civic engagement has had negative consequences for civil society (Putnam 2000). My analysis offers several reasons for skepticism about this claim. First, we see that two types of protest actions encourage the establishment of a CRB in a city: protests concerning local police behavior and protests concerning police violence that occurred in another city. Second, from 2001 through 2019, locally-focused protests against police brutality systematically lowered the subsequent number of police-involved fatalities of race and ethnic minorities. Third, several measures of racial disadvantage and the proportion of minorities in a city increase the likelihood a civilian oversight program will be established. All of these conclusions support efforts by movement activists to continue pressing for more police reforms.

At the same time, CRBs and other police reforms are no panacea. Despite the decades-long existence of civilian oversight programs, less than half of the 170 largest population cities analyzed here had a CRB by the end of 2018. Most CRBs do not have subpoena power, nor are most of them independent of police leadership. Still others exist only on paper, never meeting. Many departments have body cameras, but their use is rarely mandated, even in large police departments. Recommendations by Campaign Zero and others to implement restraints on the use of force hold promise but have not yet been fully implemented. Put differently, these programs are under institutionalized, lacking support

from police departments and leadership by elites in many cities.

We can speculate that if more of these recommended reforms were established, they might gain more legitimacy. Such legitimacy could potentially boost their efficacy and reduce resistance to them. It seems a natural next step for future research to explore whether the implementation of these reforms has had a discernible effect on other measures of minority-police relations.

As with all studies, this one has limitations and shortcomings. Characteristics of cities served by a single police force are relatively limited by census availability. Data on race/ethnicity is even more problematic when taken at face value. Reliance on official measures of race and ethnic composition of cities cannot capture the fluidity of race and other features related to mixed ethnic and racial identity (Saperstein and Penner 2012).

Local political dynamics, the racial makeup of city councils, histories of the content and outcomes of city council debates over CRBs, differences in the specific personalities of mayors, and other idiosyncratic features of city politics are also regrettably missing from our account (see McGregor 2015). The archives I used that listed fatalities did not contain information over the whole period on whether victims were armed. Most damaging is the problem that we do not have a full picture of all encounters that are “at risk” of becoming fatalities—what specific factors unfold over time that deter or encourage encounters that end in death? Data on all encounters and their resolutions would be fascinating to analyze. These are significant shortcomings, but they open opportunities for new research lines of inquiry.

The analysis of the effect of body camera mandates offers more support for the argument that protest matters for policy change. Although admittedly preliminary, my investigation of the effect of body cameras suggests recent policy changes in police procedures can affect fatalities during police encounters. Are there other policies designed to minimize mistreatment of minorities by police? A variety of

racial bias training programs have been introduced, but few cross-city studies exist, and thus the efficacy of such programs remains an open question (Headley and Wright 2019). Programs teaching about implicit bias among police officers have been tested with randomized experiments, but evidence that these programs reduce prejudice or acts of violence by authorities remains mixed (Epp et al. 2014; Paluck and Green 2009). Recent research by Jennifer Eberhardt (2016) with the Oakland Police Department provides evidence that city leadership can attenuate some of the racial disparities in language and behaviors used by police officers (see also Eckhouse 2018). Wood, Roithmayr, and Papachristos (2019) suggest network structures and partner pairings play a role in the persistence of police misconduct. These results suggest some additional avenues for intervention.

Since 2020, there has been mobilization to defund or substantially change the role of policing in cities.²⁹ It is too early to predict how these nascent movements will fare, but if Eberhardt's (2016) work holds more generally, the goal of defunding or disbanding police departments seems remote. However, efforts by some of these same movements that advocate adding mental health experts and other counselors to policing efforts seem to have gained momentum.³⁰

Campaign Zero (2021a), a nonprofit devoted to ending police violence, initiated the UseofForceProject in 2020 (Campaign Zero 2021b). The UseofForceProject surveyed the 100 largest U.S. cities that have adopted, rejected, or have under review a set

of eight more-restrictive use-of-force policies. Such policies include requiring comprehensive reporting, requiring officers to exhaust all other means before shooting, bans on chokeholds and strangleholds, a use-of-force continuum policy, and restricting shooting at moving vehicles. As part of this project, Sinyangwe (2016) found that adoption of at least one of these restrictive-force policies reduced police-involved fatalities significantly from January to July 2016. Based on these findings, Campaign Zero has developed a set of recommended policies restricting the use of force. However, Sinyangwe (2016) also reports that only one-third of the cities studied had one or more of these policies in place. As more data on the effectiveness of these policies becomes available, answers to questions about which policies are more and less effective will become clearer.

My results show that protests influence cities to establish more powerful citizen oversight boards and they lower fatalities in minority communities. Changes in the form of police oversight, implementation of programs that increase accountability, and changes in policing regarding the use of force may take more time to show an effect. Using this reasoning, if the adoption of more effective policies restricting the use of force by the police were more widespread, it might be the case that the public (and perhaps also police) would endorse these programs in their own communities. Such acceptance could potentially diminish the high levels of conflict and mistrust that currently exist between minority populations and the police.

APPENDIX: DESCRIPTIVE STATISTICS AND ANALYSIS TABLES

Table A1. Annual Listing of CRB Founding Rates in Cities

1980	Oakland, CA						
1983	San Francisco, CA						
1984	Cleveland, OH						
1987	Dallas, TX						
1989	Indianapolis, IN						
1990	Long Beach, CA						
1991	St. Petersburg, FL	Virginia Beach, VA					
1992	Rochester, NY						
1993	Bridgeport, CT	New York, NY	St. Paul, MN	Syracuse, NY	Winston-Salem, NC		
1994	Fort Lauderdale, FL	Philadelphia, PA					
1996	Grand Rapids, MI	San Jose, CA					
1997	Charlotte, NC	Las Vegas, NV	Pittsburgh, PA	Tucson, AZ			
1998	Durham, NC						
1999	Baltimore, MD	Boise, ID	Knoxville, TN	Seattle, WA	Springfield, MO	Tempe, AZ	Washington, DC
2000	Albany, NY	Riverside, CA	Spokane, WA				
2001	Austin, TX	New Haven, CT	Portland, OR				
2002	Miami, FL	Providence, RI					
2003	Cincinnati, OH	Louisville, KY	Salt Lake City, UT				
2005	Denver, CO	Tacoma, WA					
2006	Hartford, CT						
2007	Chicago, IL						
2008	Boston, MA	Buffalo, NY					
2010	Atlanta, GA	Oklahoma City, OK	Springfield, MA				
2011	Houston, TX	Huntsville, AL					
2014	Albuquerque, NM	Jersey City, NJ	Omaha, NE				
2015	St. Louis, MO	Tampa, FL					
2016	Greensboro, NC	Newark, NJ	Sacramento, CA	Toledo, OH			
2017	Wichita, KS						
2018	Anaheim, CA	Aurora, CO	Nashville, TN				

Table A2. Descriptive Statistics and Correlation Matrix for Covariates in Figure 3, Analysis of CRB Foundations, 1991 through 2018

		Mean	Std.	1	2	3	4	5	6	7	8	9	10	11	12	13
1	Ln Population	12.4	.73	1												
2	Ln Percent Black	2.6	1.1	.06	1											
3	Ln Percent Latino	2.4	1.1	.22	-.34	1										
4	Violent Crime Rate per Million	82.8	51.5	.11	.49	-.13	1									
5	Median Family Income in 1,000s	48.6	14.6	.06	-.36	.15	-.53	1								
6	Black-White Income Ratio	.71	.20	-.12	-.42	.23	-.07	-.03	1							
7	Ln Black Isolation	3.18	1.10	.16	.93	-.35	.48	-.37	-.47	1						
8	Black Mayor	.03	.18	.09	.15	-.08	.12	-.04	-.10	.15	1					
9	N of Sworn Officers in 100s	9.99	30.2	.62	.12	.09	.11	-.02	-.07	.16	.07	1				
10	Mayor-Council Government	.79	.41	-.29	.14	-.36	.11	-.06	-.04	.12	.04	.03	1			
11	Protest: Local Focus	.17	.64	.25	.07	.04	.09	-.01	-.05	.10	.01	.32	.03	1		
12	Protest: National Focus	.02	.17	.14	.02	.03	.01	.03	-.06	.04	.02	.18	.02	.21	1	
13	Protest: Event Elsewhere	.04	.30	.08	.05	.02	.05	.03	-.07	.06	.02	.06	.03	.12	.08	1

Table A3. Weighted NBRG Following CEM Matching for the Association of Lagged Protests with Black, Latino, and White Fatalities, 2001 through 2019

	Black Fatalities	Latino Fatalities	White Fatalities
<i>Prior Protest Count</i>			
Number of Local Protests (lagged two years)	-.099*** (.027)	-.100** (.034)	-.017 (.030)
<i>Lagged Dependent Variable</i>			
Lagged Fatalities of Group	.068** (.008)	.056*** (.010)	.142*** (.014)
<i>CRB Longevity</i>			
CRB Age	.001 (.001)	-.001 (.001)	-.004** (.001)
<i>Demographic Controls</i>			
Ln Percent Black	.477*** (.070)	-.331*** (.073)	-.353*** (.067)
Ln Percent Latino	.150*** (.035)	1.094*** (.063)	-.078* (.039)
Violent Crime Rate/Millions	.001* (.0007)	.003** (.001)	.001 (.001)
Median Family Household Income 1,000s	-.010*** (.002)	-.008* (.003)	-.020*** (.003)
Ratio of Black to White Median Income	-1.136*** (.175)	-1.177*** (.224)	-1.312*** (.195)
Ln Black Isolation Index	.221** (.077)	.075 (.064)	.010 (.065)
<i>City Characteristics</i>			
Black Mayor	-.117 (.097)	-.193 (.179)	-.050 (.128)
Number of Sworn Officers in 100s	-.007*** (.001)	-.004*** (.001)	-.010*** (.001)
Mayor–Council Form of Government	-.158* (.057)	-.173** (.066)	-.100 (.063)
Time Trend	.046*** (.005)	.038*** (.007)	.073*** (.006)
Offset Parameter for Ln Population Size	1.00	1.00	1.00
Constant	-14.815*** (.322)	-15.792*** (.418)	-10.993*** (.314)
Number of Observations	2,880	2,880	2,880
Log Likelihood	-4071	-2543	-3513
Pseudo R-Square	.11	.18	.09

Note: Estimates using *nbreg* command in Stata (14.2) following CEM procedure using matched and weighted samples. Protests are lagged two years. All other covariates lagged one year. Standard errors are in parentheses.

p* < .05; *p* < .01; ****p* < .001 (two-tailed tests).

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Notes

1. Despite recent media attention to protests against police brutality by Black Lives Matter (BLM), it is important to note that this movement has deep historical roots. For example, in 1951, the "We Charge Genocide" campaign (that included Paul Robeson and W.E.B. Du Bois) petitioned the United Nations to examine police violence used against Black individuals (Civil Rights Congress 1970). During the late 1960s and early 1970s, the Black Panthers monitored police activities directed against Black people and regularly mobilized protests opposing police violence (Davenport 2010). Following the urban riots of the mid-1960s, the Kerner Commission on Civil Disorders recommended the adoption of civilian review boards (CRBs) (National Advisory Commission on Civil Disorders 1968). During the 1960s and 1970s, civil rights leaders, including Martin Luther King Jr., framed police violence against minorities as a key civil rights issue.
2. An example of a protest that combines these claims took place in Miami in February 1989: "a newly formed coalition of black groups called on Metro commissioners Tuesday to end police brutality, treat blacks with respect and open the local economy to minorities" (Gehrke and Gemoules 1989).
3. See the June 2017 special issue on Black Lives Matter in *Ethnic and Racial Studies*.
4. For examples, see Edwards, Esposito, and Lee (2018); Epp and colleagues (2014); Gelman and colleagues (2012); and Mayson and Stevenson (2020).
5. An anonymous reviewer asked why few studies of police fatalities include analyses of Asian Americans and Native Americans. This is an important question that deserves more attention. For analyses at city or county levels, the size of these populations is often too small to analyze adequately. Unfortunately, separate analyses of Asian and Pacific Islander fatalities were not feasible in my analysis. Only four cities out of the 170 in my data had a discernible population of Asian and Pacific Islanders. Moreover, police-involved fatalities of Asians and Pacific Islanders are rare. The distribution of Native Americans is equally small and is concentrated in just a handful of cities (and analysis of cities would miss information on tribal nations).
6. An anonymous reviewer suggested I examine these arguments using data on misdemeanor arrests. Unfortunately, outside of New York City, recent public reports of misdemeanors are scarce (Mayson and Stevenson 2020:976). In analyzing court records on misdemeanors from eight jurisdictions with data available, Mayson and Stevenson (2020) find that the per-capita misdemeanor case filing rate is two to four times higher for African Americans than for Whites, and that these racial disparities were greatest in Chicago. For analysis of racial disparities in incarceration rates by race for misdemeanors in U.S. cities from 1890 through 1910, see Olzak and Shanahan (2014).
7. For more evidence on the implications of racial disparities in exposure to police, see Krivo, Petersen, and Kuhl (2009), Ross (2015), Vélez, Lyons, and Santoro (2015), and Weisbord and colleagues (2017). For evidence on race differences in police-involved fatalities, see DeGue, Fowler, and Calkins (2016), Holmes, Painter, and Smith (2018), and Nix and colleagues (2017).
8. Using the 2007 LEMAS (Law Enforcement Management and Administrative Statistics) dataset, I found that cities with CRBs had significantly more complaints about the use of excessive force by police, controlling for sample selection bias, city population size, minority police chiefs, and percentage Black officers in a city's police department.
9. I initially analyzed protest against police violence from 1980 through 1989, using data from the Dynamics of Collective Action (DCA) Project (McAdam et al. 2009). The DCA data contain information on timing and location of protests against police brutality but do not provide any detail on the geographic focus of an event. In my first analysis, protest (undifferentiated by geographic focus) had no significant effect on the founding rate when using the DCA data. This null finding may indicate that effects of local, national, and other city protests differ and might be canceling each other out. The set of cities analyzed are still the ones that were in the project at the start of the study period.
10. I thank an anonymous reviewer for suggesting this idea.
11. Google researchers who have tracked online frequencies of issues support this claim (<https://blog.gdeltproject.org/were-talking-more-about-police-brutality-since-august-2014/>).
12. For example, one of Campaign Zero's central demands includes CRBs (<https://www.joincampaignzero.org/#vision>).
13. This means city blocks, neighborhoods, or census tracts would not be an appropriate unit of analysis.
14. Around 2014, *The Guardian* and *The Washington Post* began archiving officer-involved

- fatalities in the United States (see <https://www.theguardian.com/us-news/series/counted-us-police-killings>; https://www.washingtonpost.com/graphics/2018/national/police-shootings-2018/?utm_term=.229d4d205ab2). For an analysis of police killings using the *Guardian* dataset, see Zimring (2017).
15. See <https://fatalencounters.org/> for details. Data collection comes from paid researchers, public records, and crowdsourcing (the website states 85 percent of events were collected by paid researchers). For a study using these data, see Edwards and colleagues (2019). The major advantage of this dataset is that it links events to one or more media sources. The data used here on fatalities do not contain information on whether the victim was armed for the whole period (but the *Washington Post* archives do contain this information for the years 2015 through 2019).
 16. I used the *Mapping Police Violence* (MPV) dataset because it identifies records also contained in the *Fatal Encounters* dataset (for details, see <https://mappingpoliceviolence.org/aboutthedata>).
 17. I analyzed data from 1991 through 2018 and compared the effects of violent and nonviolent protest. This analysis showed that nonviolent protest has the same effect as the combined count of protests on CRBs, but the lagged count of violent protests has no significant effect on the founding rate of CRBs during this period.
 18. In an attempt to gather information on local NAACP chapters, I consulted the *Mapping American Social Movements Project* at the University of Washington, which documents the establishment of local NAACP chapters from 1912 to 1964. These data have been extended for a smaller set of cities through 1977, which is before the beginning of the present study (see Williamson et al. 2018) (https://depts.washington.edu/moves/NAACP_database.shtml).
 19. The effect of protest might depend on factors that raise the level of grievances. To explore this idea, I created an interaction term with Black-White income ratio and protest. Unfortunately, this interaction term was correlated .95 with the count of lagged protests, creating problems of multicollinearity.
 20. Investigative reporting of 82 large police departments by Reuters (Levinson 2017) found that police union contracts frequently include written provisions for erasing disciplinary records, giving officers access to videotapes and other investigative materials, disqualifying complaints using time limits, and allowing substitution of vacation days for an adjudicated suspension. This report found that the majority of cities in their sample used one or more of these procedures to circumvent citizen complaints. The legal protection of “qualified immunity” has been described as another obstacle to holding police accountable for the use of undue force (<https://www.reuters.com/investigates/special-report/usa-police-immunity-methodology/>).
 21. I explored the Cox PH model because of its wide applicability in analyzing survival data. The Cox PH model has appeal, in part, because it does not require specification of the baseline hazard. However, this model assumes the effects of covariates do not vary over time. Inspection of the plots of the residuals over time suggests this model is not as appropriate as others in this table. In addition, there are many tied founding dates (see Appendix Table A1), which is also problematic for the Cox model (Woolridge 2010:714).
 22. The Hausman test comparing *xtpoisson* with *xtnbreg* specifications indicated there were no systematic differences between them (for Black fatalities, the chi-square was 18.72, which is not statistically significant). However, comparison of the Akaike’s Information Criteria (AIC) and Bayesian Information Criteria (BIC) estimates indicated that *xtnbreg* improves over *xtpoisson*. For details on the pseudo-maximum likelihood estimates with poisson regression (*ppml* in Stata), see Santos Silva and Tenreyro (2010). For details on the panel model with cross-sectional dependence (*xtscc* in Stata), see Hoechle (2007).
 23. I replicated these results from Table 4 for men in each race/ethnic category. The effects are almost identical as those seen in Table 4 for all fatalities. This is unsurprising because the ratio of male to female fatalities during police encounters is about ten to one in all cities.
 24. Note that overall median income is more highly associated with White fatalities. For similar findings, see Feldman and colleagues (2019).
 25. Some cities had observed ratios of over 1.0 in 2010. Cities where this ratio is over 1.0 have a very tiny Black population (less than 1 or 2 percent Black, which may explain this high ratio). Dropping all outlier cities (with a ratio over 1) did not affect any other measures (and the effect of this income ratio remains negative and significant).
 26. Replacing the Black isolation index with the Black-White dissimilarity index does not help matters much, as the correlation with this measure and log of percent Black is .70. The dissimilarity index is weakly and positively related to Black fatalities when substituted for the Black isolation measure.
 27. Caution should also be taken with these results because information from these sources on body camera mandates is available and most reliable at the state level. Unfortunately, the number of states with these mandates is too small to conduct multi-level analysis, which would have been ideal.
 28. For studies using alternative but similar matching techniques, see Minkoff (2016) and Wang and Soule (2012).
 29. On the movement to defund the police, see <https://www.afsc.org/defundpolice?gclid=CjwKCAjwr56>

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30. Using Google trends, I found that since August 2020, the topic “defunding the police” has sharply declined, whereas the topic “mental health policing” has shown several peaks over the past year.

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