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Collective Bargaining Rights, Policing, and Civilian Deaths

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ABSTRACT

Collective Bargaining Rights, Policing, and Civilian Deaths*

Do collective bargaining rights for law enforcement result in more civilian deaths at the hands of the police? Using an event-study design, we find that the introduction of duty to bargain requirements with police unions has led to a significant increase in non-white civilian deaths at the hands of police during the late twentieth century. We find no impact on various crime rate measures and suggestive evidence of a decline in police employment, consistent with increasing compensation. Our results indicate that the adoption of collective bargaining rights for law enforcement can explain approximately 10 percent of the total non-white civilian deaths at the hands of law enforcement between 1959 and 1988. This effect is robust to a contiguous county approach, accounting for heterogeneity in treatment timing, and numerous other specifications. While the relationship between police unions and violence against civilians is not clear ex-ante, our results show that the popular notion that police unions exacerbate police violence is empirically grounded.

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For the last decade, over 1,200 Americans have been killed by law enforcement each year,[1] a figure which is unprecedented among high-income nations.[2] Both today and historically, a disproportionate number of the civilians killed by law enforcement are non-white. Young, Black and Indigenous men are by far the most likely to be killed by law enforcement (Edwards et al., 2019). While the United States has had consistently high numbers of civilians killed by law enforcement, there was a marked rise between 1960 and 1975, when the number of civilians killed by law enforcement officers increased by over 53%. While this increase leveled off in the 1980s, killings and per capita killings remained substantially above levels from the 1950s and 1960s.[3] There is now a growing empirical literature on the causal factors that influence police killings of civilians,[4] however, the factors affecting police violence are still not empirically well-understood and the drastic rise in civilians killed by law enforcement during the mid-twentieth century remains largely unexplained. In this paper, we explore how the rise of law enforcement officer bargaining rights has impacted police killings of civilians by race. While we are principally interested in how the introduction of bargaining rights influences the number of civilians killed by race, we also consider a range of other outcomes, including total crime, measures of other types of crime, police employment, and officers killed in the line of duty.

Police unions and the collective bargaining process may affect police killings of civilians by shifting individual incentives for officers, including the likelihood that they will be prosecuted and convicted for potentially unjustified killings of civilians. This shift in incentives may occur through a number of channels, including legal and financial aid, professionalized communications support, direct influence on the investigative process itself (Rushin, 2016; Fisk and Richardson, 2017),[5] and broader protective legislative changes (Kadleck, 2003).

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[1]See https://fatalencounters.org. Last accessed October 12, 2020.

[2]See https://www.cnn.com/2020/06/08/us/us-police-floyd-protests-country-comparisons-intl/index.html. Last accessed March 3, 2021.

[3]These figures are generated using the Vital Statistics Multiple-Cause of Death File. See Appendix Figure A1 for the time series of police killings over time.

[4]For examples, see Alpert and MacDonald (2001); Pang and Pavlou (2016); Ariel et al. (2015) and Masera (2019).

[5]For example, in the event of a civilian death, a union representative may meet with their member before

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This collection of protections may shift the cost to officers to use lethal force in a manner that both effects the rate of killings by police and explains the increased rate of killings in the latter half of the twentieth century. Whether collective bargaining rights affect civilian deaths has become a heated source of public debate (Bushey, 2020; Kelly, 2020).

To understand the relationship between law enforcement bargaining rights and civilian deaths, we combine a number of historical data sources, including data on the roll-out of collective bargaining rights from 1959-1979 for different groups of public sector workers, and the particular elements of those rights, from the database created by Valletta and Freeman (1988), civilian deaths by law enforcement from 1959-1988 from the Vital Statistics Multiple-Cause of Death File, crime and police employment data from the FBI's Uniform Crime Reporting Data (US Department of Justice, FBI, 2004), and demographic data from the County and City Databooks 1944-1977 (ICPSR, 1981). Our empirical approach takes advantage of variation in the location and timing of when officers are granted bargaining rights to determine the impact of collective bargaining rights on police killings of civilians; we focus specifically on the adoption of *duty to bargain* provisions.[6] We control for cross-sectional unobserved heterogeneity by using county fixed effects and differences across time by including urban-by-year fixed effects and region-by-year fixed effects.[7] We test the robustness of our findings using a contiguous borders approach, following Dube et al. (2010),[8] and account for heterogeneity in treatment timing (Goodman-Bacon, 2020; Callaway and Sant'Anna, 2020; Sun and Abraham, 2020) along with a number of other specification tests.

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an official statement is made and coordinate with other officers involved regarding the narrative of the event. The collective agreements themselves frequently include differential procedural protections for the timing and course of interrogations and can restrict the release of relevant footage (Rushin, 2016; Fisk and Richardson, 2017).

[6]While the results of this work are robust to focusing on the legality of bargaining alone, the duty to bargain is the key measure necessary to allow broad-based collective bargaining and union formation in practice.

[7]Our identification strategy relies on trends in deaths by legal intervention and the timing of bargaining rights adoption being uncorrelated with trends in the other determinants of police killings.

[8]In this approach, we take advantage of collective bargaining rights discontinuities at state borders, using variation in bargaining rights within cross-state county pairs. Contiguous counties are an effective control group as neighboring counties are likely to share more similarities than those elsewhere in a state. Police departments on either side of the border may even be policing the same people, thus, to some extent holding the population affected by state policies constant.

Our results indicate that one to three years after collective bargaining rights are granted, there is little impact on police violence against civilians. This is consistent with the time it takes to form a union and negotiate a first contract.[9] However, over the medium and long-run, we find a marked increase in civilians killed by law enforcement. This effect is almost entirely concentrated in the non-white population, accounting for more than 10% of the non-white civilian deaths due to police between 1959 and 1988. This result is robust to various specifications and is *not* driven by the implementation of Law Enforcement Officers' Bills of Rights (LEOBRs). Similarly, the contiguous borders empirical approach identifies a substantial increase in non-white deaths, similar in magnitude to those in the event-study. Accounting for heterogeneity in treatment timing, as well as several other specification tests and robustness checks, confirm our results.

We find evidence that the introduction of bargaining rights decreases the number of police officers in the long-run, while other crime-related outcomes are not affected. Importantly, we find no meaningful impact on the number of officers killed in the line of duty[10], indicating that while policing may become more dangerous for the civilian population, it does not become safer for officers. Further, there is no change in total reported crime, violent crime, or property crime, potentially suggesting modest increases in police productivity along these dimensions. We explore whether the reduction in police employment can fully explain the rise in non-white civilian deaths. While the changes are correlated, our results suggest that reduced police employment does not explain this effect.

While the relationship between police unions and violence against civilians is not clear ex-ante, our results are consistent with the popular notion that law enforcement unions

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[9]The process to form a union in the United States, in the post-Taft Hartley era, is a remarkably slow process. As a first step, a potential bargaining agent needs to collect cards from a large share of potential members indicating their desire for union certification. For law enforcement, this exact threshold varies by state, but unions typically prefer to gather cards from a strong majority of members prior to filing for an election. This process can take months or years. After filing for an election there is regularly a several month delay prior to the vote itself. Negotiating a first contract after certification is another long process both because of the complexity of creating a first agreement and because employers frequently break new unions by refusing to negotiate a first contract.

[10]This is also true conditional on the number of officers.

exacerbate police violence against civilians. Furthermore, this effect can not be explained by bargaining rights uniformly shifting the marginal decision to shoot in a "risky" situation. In such a scenario, while we may see a greater increase in non-whites killed, we would still anticipate some increase in white deaths, which is not visible in the data. The scale of the effect on non-white deaths is substantial enough to explain a non-trivial portion of both the increase in civilian deaths and differential increase in non-white civilian deaths over the latter half of the twentieth century. The absence of pre-trends in the event study design, robustness of the size of the effect, as well as the demanding contiguous county approach, all support a causal interpretation of the impact of collective bargaining rights on non-white civilian deaths.

In the next section, we provide a brief overview of the history of the extension of bargaining rights to police and discuss the literature on police use of lethal force and police accountability in relation to law enforcement unions. In section 3, we discuss the sources of data we use to identify the effect of collective bargaining rights on civilian deaths by law enforcement officers, crime, and officer outcomes using the empirical strategy discussed in section 4. In section 5 and 6, we present our main results and a series of robustness checks. We conclude in section 7 with a brief discussion on the implications of our findings.

2   Background

2.1   Historical Background

Police unions have long occupied a novel political space and relationship to workers' movements in the United States. Law enforcement officers have historically worked with the state and employers to suppress labor organizing, particularly prior to the introduction of the National Labor Relations Act (Gourevitch, 2015; Slater, 2017). The earliest attempts for police to organize began in Boston and a handful of other cities during World War I led under the American Federation of Labor (AFL) (Levine, 1988). There were some successes

in these early organizing campaigns, during the period of labor peace in the war years, but they did not last after the war's conclusion. While formal AFL-affiliated police unions did not survive the war, benevolent associations and fraternal organizations did survive the death of this early movement with particular strength in the Midwest and Mid-Atlantic. These organizations continued to fill many of the social insurance and collegial roles that defined early trade unionism in the United States.

Further attempts were made to organize the sector during World War II and in the surge in organizing following the war. Both the American Federation of State, County and Municipal Employees (AFSCME) and the Teamsters played particularly prominent roles in these attempts, but both met limited success for both cultural and legal reasons.[11] With the introduction of public sector bargaining for some workers in the 1950s, law enforcement gradually gained the right to organize state by state starting in the late 1950s. Active, mandatory access to collective bargaining began in 1959 and rapidly expanded over the 1960s and 1970s (Levine, 1988) as part of the general push to public sector unionization.

Successful attempts at organizing began in the 1960s. Rather than being driven by organizing from unions in the AFL-CIO, they were in fact triggered by calls from the American Civil Liberties Union (ACLU) to create civilian reviews boards to handle complaints against the police.[12] The first major recognition occurred in 1964 in New York City with voluntary recognition from the city of the Police Benevolent Association (PBA) of New York. Organizing elsewhere rapidly followed as law enforcement bargaining rights expanded across the country.

Police unions, as with many public sector unions, have survived the broad decline in the labor movement over the last half century.[13] Today, police, along with teachers and core

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[11]These drives occurred when there was still no legal basis for public sector organizing in the United States.

[12]We acknowledge that this may suggest unionization is endogenous to the conditions that resulted in the ACLU wanting greater oversight. We provide evidence against this in our results section through the absence of pre-trends in civilian deaths before the right to collectively bargain was implemented in a given state.

[13]In fact, policing has been a rare sector that has experienced growth in the latter half of the 20th century while most private sector unions began to decline in the 1950s.

public sector workers, have the highest levels of union membership in the United States with roughly 75 percent of law enforcement officers being members of a union.[14] The Fraternal Order of Police represents over 300,000 members, although not all individuals are certified members of a bargaining agent. Relatively few officers are members of AFL-CIO affiliated unions with the largest being the International Union of Police Associations (IUPA) and the International Brotherhood of Police Officers (IBPO/IBCO).

2.2   Police Unions in Practice

Police unions engage in all of the activities of a traditional labor union. Their work is primarily grounded in bargaining and enforcing collective agreements that govern the employment relationship. In practice, this means bargaining collective agreements that typically cover wages, benefits, and disciplinary practices amongst other potential provisions. Under the Duty of Fair Representation (DFR), police unions, as with other unions, are required to offer the best possible protections to all members of the bargaining unit.

As in other workplaces, organizing a union for law enforcement is a slow process. Typically, the process begins with an expression of interest from some officers in a potential bargaining to a national or state union. At that point, union organizers are deployed to work with rank-and-file members to gather certification cards from a sufficient number of workers to trigger an election. This threshold varies from state to state, but unions typically prefer to achieve a strong majority to help ensure success in the election. Once the union files for an election, the employer will become aware of the drive if they have not already and often mount a campaign to discourage certification. The time between initial expression of interest and election can vary substantially and regularly takes months. After certification of an election, the union will then attempt to bargain a first contract with the employer. This is typically the largest hurdle for union survival as first union contracts that are negotiated are complex and employers often attempt to break the union by refusing to

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[14]By the end of our sample period, bargaining is illegal in 5 southern states while nearly 20 percent of states have the duty to collectively bargain, covering nearly 60 percent of the 1960 population.

or delaying bargaining.

In the event of a civilian killing by a law enforcement officer, the police union can act on the officer's behalf in a number of meaningful ways. While the particular circumstances will differ from bargaining unit to bargaining unit, there are several broad approaches that tend to be common. In the event of a death they will typically pay for and facilitate legal representation, meet with their member in advance of making a report if possible, potentially facilitate communication and coordination between other officers, and implement procedural protections during interrogations if permitted under the collective agreement (Rushin, 2016; Rad, 2018). Further, police unions will frequently come to the public defense of officers to prevent or delay charges.

Legal scholars have argued that police unions limit the discipline of officers (Walker, 2008) and that they hamper police accountability efforts (Rushin, 2016). For example, in examining 178 union contracts of the United States' largest police departments, Rushin (2016) finds that these agreements "limit officer interrogations after alleged misconduct, mandate the destruction of disciplinary records, ban civilian oversight, prevent anonymous civilian complaints, indemnify officers in the event of civil suits, and limit the length of internal investigations" (Rushin, 2016, p. 1192). Fisk and Richardson (2017) examine the evolution of police unions, providing qualitative evidence on how police unions act to protect police who are accused of misconduct.

2.3 Relevant Empirical Literature

There has been limited empirical evaluation of the impact of police unions or officers' right to collectively bargain on outcomes, particularly in the use of force against civilians. Rad (2018) constructs a broad index of policy protections for America's largest police departments using union contracts and LEOBRs covering 100 cities in the United States.[15] He documents categories of provisions that limit oversight, statutes of limitations on data

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[15]He does so using *Campaign Zero's* police contract database.

retention and use, and requirements for cities to pay for legal costs or provide compensation during a suspension. Rad (2018) also documents the statutes of limitations on filing a complaint, limitations on the types of complaints, and formal waiting periods that delay investigations. From these data, he constructs an index of provisions that limit recourse in the case of misconduct and finds that this index is strongly correlated with the number of unarmed civilian deaths by police, even after controlling for police force size, city population, region, violent crime rate, and measures of racial diversity. However, Rad (2018) does not distinguish between LEOBRs and collective agreements which may be correlated. Legal scholars have expressed independent concern about the impact of LEOBRs on police accountability aside from concerns about the effects of unionization (Keenan and Walker, 2004). One contribution of our work is to distinguish between the impact of collective bargaining rights and the impact of LEOBRs in our time frame.

There has also been recent work on the impact of police unions in setting compensation, the dynamics of the bargaining process, and police productivity. Mas (2006) presents evidence on the impact of deviations of awarded settlements relative to demands by police unions during final offer arbitration on police productivity. This work, focusing on New Jersey police bargaining units and municipalities between 1978 and 1996, finds that after New Jersey police officers lose in arbitration, arrest rates and average sentence length decline, and crime reports rise relative to when they win. Older evidence from Ichniowski et al. (1989) suggests that union police departments obtain higher compensation and better benefits relative to non-union police departments in states without collective bargaining rights.

The most closely connected papers to our work are Goncalves (2020) and Dharmapala et al. (2019). Dharmapala et al. (2019) provides quasi-experimental evidence on the effects of collective bargaining rights on violent incidents of misconduct. The authors take advantage of a 2003 Florida Supreme Court decision that resulted in collective bargaining rights being extended to sheriff's deputies in an arguably exogenous manner. They use a

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difference-in-difference approach, considering police officers in Florida who were unaffected by the decision as a control group. They find a statistically significant increase in reports of violent misconduct for officers who gain access to collective bargaining. We view our work as complementary to theirs in that we capture this phenomenon more broadly, both geographically and temporally. We consider the original implementation of bargaining rights, during a period which most states acquire these rights, while Dharmapala et al. (2019) focus on an era where police officers' unions and their bargaining rights were already largely established across the country.[16] We also build on their findings by examining a broader set of outcomes, including civilians killed by law enforcement by race, crime, police employment, and officer safety and by accounting for the simultaneous rise of other legal institutions that are believed to limit police accountability, specifically LEOBRs.

Goncalves (2020) focuses on the effect of police union certification on civilian deaths from 1985 to 2015 using state level data from Florida and from 1987 to 2013 using a national survey of police departments. He uses differential timing of unionization nationally and close elections in Florida to identify the effects of unionization. He finds small positive effects of unionization on deaths, most of which are statistically insignificant. Our work differs from Goncalves (2020) in at least three important ways. First, we focus on deaths by race while he focuses on aggregate deaths, which may mask heterogeneity in police use of lethal force. Second, our research examines a different treatment: the duty to bargain with law enforcement officers' unions. The duty to bargain may not only strengthen the local power of unions that ultimately establish themselves, but it may also have spillover effects on non-unionized departments within the same state. Finally, we focus on the time period before his begins - one with a rapid increase in police violence.

More broadly, our work contributes to the growing literature on the factors that affect police killings of civilians. To date, this literature has found evidence that third party involvement in administrative work around police involved death causes a decline in use

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[16]If the existence of police unions in some regions have cultural spillover effects, or practical spillover effects by offering legal support to departments outside of their own, later unionization may matter less.

of force (Alpert and MacDonald, 2001), while the impact on the use of technology, such as smartphones or body cameras, is mixed (Pang and Pavlou, 2016; Ariel et al., 2015).[17] Masera (2019) provides causal evidence on the impact of police militarization on the killings by police by leveraging institutional features of the 1033 program, showing militarization increases killings by police by 64 deaths per year. There is also evidence that on-duty-injuries within a police officers' network increase police misconduct shortly after the event (Holz et al., 2020). Our work also contributes to the rapidly growing literature that broadly confirms the presence of racial disparities in police treatment and use of force on minority populations (Fryer, 2019; Ross, 2015; Correll et al., 2014; Edwards et al., 2019).

Our expectation is that exposure to collective bargaining rights is most likely to lead to increased civilian deaths by legal intervention and reduced deaths of law enforcement officers if union certification reduces the likelihood of prosecution or other penalty in the event of an unjustified shooting. Union certification may also increase police shootings by decreasing the number of officers hired in a way that systematically increases the risk of fatal use of force against non-white civilians. However, it is certainly also feasible that collective bargaining improves officer quality, be it through selection or training that limits the use of lethal force against the civilian population. While either of these pathways are viable, the first explanation is consistent with much of the existing literature around police unions as discussed above.

3 Data Sources

We use four primary sources of data. Information on collective bargaining rights are from the NBER Public Sector Collective Bargaining Law Data Set (Valletta and Freeman, 1988). The data set consists of observations for 5 state and local government employee groups in 50 states from 1955-1985, one of which is law enforcement. For each group-state-year, the status of the state bargaining law is summarized by 14 numerical variables,

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[17]Pang and Pavlou (2016) finds that body cameras do not limit the use of force while Ariel et al. (2015) finds that it does.

which Valletta and Freeman (1988) judge to be most relevant for union activity. These variables are divided into five categories: contract negotiation, union recognition, union security, impasse procedures, and strike policy. We focus specifically on collective bargaining rights in the lower 48 contiguous states. The data distinguish between no provisions around collective bargaining rights, collective bargaining rights being prohibited, the employer being authorized but not required to bargain with a union, the right to present proposals, the right to meet and confer, and the implicit or explicit duty to bargain. Figure 1 shows the adoption of collective bargaining rights by state between 1959 and 1979. Following Ichniowski et al. (1989), our main specification focuses on the strongest provision, duty to bargain (implicit or explicit). As a test of internal validity, we also consider weaker provisions, including those where the employer is authorized but not required to bargain or both parties are authorized to present or confer over terms of employment.

We construct our main dependent variable, civilian deaths caused by law enforcement, from the 1959 to 1988 Vital Statistics Multiple Cause of Death Files (US DHHS and ICPSR 2007).[18] The Vital Statistics data report deaths by cause, age, race, and county of residence, which we use to create county-race specific mortality rates for deaths by law enforcement intervention, excluding deaths due to legal execution. Unlike the public use Multiple Cause of Death Files from 1999 - 2018 available from the Centers for Disease Control and Prevention (CDC), these data are not censored at small numbers of deaths by race, county, and cause, which is critical for our purposes. While the Vital Statistics data on deaths through legal intervention are imperfect due to the discretionary and voluntary nature of reporting (Sherman and Langworthy, 1979; Fyfe, 2002; Loftin et al., 2003), they are the most consistent and complete collection of deaths by law enforcement intervention over this period.[19]

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[18]We also construct a mortality rate, deaths per 100,000 civilians. We use the 1960 census and population profiles from the Surveillance, Epidemiology, and End Results (SEER) program to calculate population by demographic group.

[19]Government collected data accounts for roughly 50 percent of the police-involved fatalities in other non-government sources (Barber et al., 2016). Although an under-count, Appendix Figure A2 shows that the Vital Statistics closely follows the Fatal Encounters time series for reports of deaths due to police use of force. Unfortunately for our analysis, the Fatal Encounters data collection efforts began in 2000—well

Moreover, heterogeneity in the recording of civilian deaths due to law enforcement that is time invariant is captured by county fixed effects.[20]

Our other outcomes of interest, police employment (number of sworn officers), officers killed in the line of duty, total crime, and violent crime (murder, rape, assault, robbery) are taken from the FBI's Uniform Crime Reporting (UCR) datasets.[21] Agency level outcomes are aggregated to the county-level to construct crime outcomes. Due to inconsistency in reporting crime data in the UCR, we first only include agencies that fully report in the reporting year. Additionally, we construct a variable that captures the percentage of the population covered in the reporting year. This ideally helps to capture changes in the composition of agencies reporting in any given year. Demographics at the county level are taken from the County and City Data Book published from the United States Census Bureau (1944-1977). Our demographic characteristics include: population, population per square mile, white median income, black median income, public assistance recipients, percent of families with income less than $3,000, percent of the population using public transportation, percent of the civilian labor force, unemployment, percent of the population that is non-white, and percent of homes owner occupied that are non-white.

4   Empirical Approach and Identification

To analyze the impact of the adoption of collective bargaining rights on deaths due to legal intervention, we use two different samples to construct control groups for comparison. The primary sample consists of all the counties as of 1960 in the lower 48 contiguous states. The secondary sample includes only contiguous counties. Treated counties belong to states that adopted duty to bargain provisions between 1959 and 1988.[22] The control group consists

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after our sample period.

[20]This is a strong assumption as it is possible that the adoption of collective bargaining rights influences the interaction between police and county bureaucracies. It is unclear this would lead to more or less reporting of deaths due to policing, so predicting the direction of potential bias is difficult.

[21]Data on crime are from the UCR Offenses Known and Cleared by Arrests files. Data on police employment come from the UCR Law Enforcement Officers Killed and Assaulted files.

[22]Treated counties are counties in states that adopt the duty to bargain. The timing of treatment is when a state adopts collective bargaining positions more generally.

of counties belonging to states where the duty to bargain is not required or prohibited.[23] The second sample consists of counties along state borders, taking advantage of discontinuities along borders. Once again, treated counties are border counties in states that adopt duty to bargain provisions, while the control group only consists of border counties in states with weaker or no collective bargaining provisions that are contiguous to treated counties.

Table 1 reports average demographic characteristics from the 1960 census, along with crime and vital statistic averages for 1960. Columns 1 through 4 refer to the full sample while columns 5 through 7 report average characteristics from the border county sample. According to Table 1, there are significant differences between counties whose states adopt bargaining rights and those that do not.[24] Counties with collective bargaining rights are typically larger, with lower median income for African Americans, and a lower percentage of the population earning less than $3,000. Counties exposed to collective bargaining rights also have a larger percentage of the population that is white and a lower percentage of the non-white population who are home-owners. Lastly, treated counties have lower violent crime rates, which is reflected in lower murder rates. Additionally, these counties have greater police presence per 1,000 residents. Although there are clear differences in demographic characteristics, deaths due to policing are not statistically different between the treated and control groups. When limiting the sample to border counties, key cross-sectional differences still exist, although are, as intended, similar. Nonetheless, there is no statistical difference as it relates to police killings of civilians.

Our empirical approach takes advantage of variation in the location and timing of when police officers are granted bargaining rights to determine the impact of exposure to collective bargaining rights on police killings of civilians. Table 2 reports the number of counties and the percent of 1960 population covered by the duty to bargain provision over the sample period. Figure 1 shows the geographic distribution (Panel 1a) and timing (Panel 1b) of

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[23]Lovenheim and Willén (2019) also focus on the duty to bargain provision when exploring the impact of collective bargaining on teachers.

[24]When referring to counties with bargaining rights or laws, we are specifically referencing those that have adopted duty to bargain provisions.

the roll-out of bargaining rights. Both show substantial temporal and geographic variation in bargaining rights adoption and there is no obvious pattern in the geographic timing of adopting collective bargaining rights. Law enforcement officers frequently secure bargaining rights when other public sector workers, such as teachers and firefighters, obtain bargaining rights or when other sectors already have them. It is plausible that there is nothing unique about the timing of the adoption of police bargaining rights outside of the general movements towards public sector bargaining rights.[25] We test for predictable variation in the timing and the location of the adoption of collective bargaining rights in Appendix Figure A3. Panel (a) tests if the timing of treatment is correlated with the number of non-white deaths due to legal intervention in 1960 while Panel (b) tests if pre-trends in non-white deaths due to policing are different between treated and untreated locations. Both show that pre-period deaths do not predict the timing and location of the adoption of collective bargaining rights. This provides evidence against the idea of the ACLU systematically driving union organizing efforts in areas already experiencing an increase in police caused civilian fatalities.

We control for key cross-sectional differences due to unobserved heterogeneity by using county fixed effects, and differences across time and region by using region-by-year fixed effects. In addition, we account for unobserved differences in policing and crime that vary by urban status and across time with urban-by-year fixed effects. For instance, urban-by-year fixed effects account for changes in policing practices that larger, more urban police departments may adopt due to economies of scale or budgetary bandwidth, but smaller, more rural departments may not benefit from. Urban-by-year fixed effects are constructed by interacting year indicator variables with five categories of urbanization in 1960.[26] Our identification strategy relies on pre-existing trends in deaths by legal intervention being unrelated to the date at which bargaining rights are granted. An additional test of this assumption is embedded in the event-study analysis, which captures the evolution of deaths

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[25]While typically the collinear adoption of policies is problematic for identification, channels through which these other forms of collective bargaining rights could impact police killings of civilians is unclear and we do not believe this to be a problem in our context.

[26]Urban status categories, $\mu : 0, 0 < \mu < 25, 25 \leq \mu < 50, 50 \leq \mu < 75, 75 \leq \mu \leq 100$.

due to legal intervention before and after the exposure to collective bargaining rights.

We estimate the following ordinary least squares equation:

$$K_{it} = \alpha_i + \gamma_{u(i)t} + \delta_{r(i)t} + \sum_{-6}^{-2} \pi_y D_i 1(t - T_i^* = y) + \sum_{0}^{9} \pi_y D_i 1(t - T_i^* = y) + \epsilon_{it} \qquad (1)$$

where, K is the number of civilian deaths caused by law enforcement in county i in year t, α is a set of county fixed effects, γ is a set of urban status-by-year fixed effects, and δ is a set of region-by-year fixed effects.[27] D_i is an indicator variable equal to one if a county ever gains bargaining rights and these rights eventually include the duty to bargain. The effect of bargaining rights on police killings of civilians is then captured in a set of event-year dummies $1(t - T_i^* = y)$, which are equal to 1 in the appropriate year. The reference year, $1(t - T_i^* = -1)$, is omitted. We focus our analysis on the five years before, $y \geq -5$, and eight years after, $y \leq 8$, bargaining rights are secured. Event years more than five years before, $y < -5$, and more than eight years after, $y > 8$, treatment are grouped together (event-year -6 and event-year 9) and omitted from the presentation of results. Given that the treatment is at the state level, all standard errors are accordingly clustered by state. The main results are obtained from ordinary least squares regressions, but we also present results using a Poisson model for robustness.[28]

We argue the appropriate outcome variable is the number of civilian deaths rather than deaths per capita for two reasons. First, given that police killings of civilians are relatively rare events in a county in a year, per-capita numbers are very small and offer limited variation. Second, high quality data on population by county by year over this time period are only available each decade. Given the relatively small number of deaths per county in each year, any systemic measurement errors in the population data will shroud the effects on non-civilian death. Despite these concerns, we conduct a weighted least squares analysis

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[27]We estimate a model including, X_{it} , an interaction of demographic characteristics from Table 1 with a linear time trend, as a robustness check.

[28]See Wooldridge on this topic here (As of July 24, 2019). We include the Poisson model with county and year fixed effects. The model provides qualitatively similar results to those present in the linear specification.

using deaths per capita as the dependent variable of interest.

We also augment our event-study design by grouping event-years in the pre and post-period, to estimate joint treatment effects. We estimate the following equation:

$$K_{it} = \alpha_i + \gamma_{u(i)t} + \delta_t + \sum_{\theta} \widetilde{\pi}_y D_i 1(t - T_i^* \in \theta) + \sum_{\eta} \widetilde{\pi}_y D_i 1(t - T_i^* \in \eta) + \epsilon_{it} \qquad (2)$$

where θ accounts for pre-period event-years $-5 \geq y \geq -2$ and η accounts for the short-run $(0 \geq y \geq 2)$, the medium-run $(3 \geq y \geq 5)$, and long-run $(6 \geq y \geq 8)$ event-years. This specification allows for testing the joint significance of pre-period trends.[29]

5 Results

Using the coefficients from equation 1, we plot pre-treatment and post-treatment effects, π, from an unbalanced panel with a solid line and circle markers.[30] We present 95 percent confidence intervals with dashed lines and circle markers and 90-percent confidence intervals are identified by the gray shaded area. Confidence intervals are constructed from heteroskedastic-robust standard errors, clustered at the state-level.[31] It is important to emphasize that we do not explicitly study the impact of forming a union itself, but rather the ability to form a union with bargaining power, so our estimates should be understood as an "intent-to-treat" estimate of unionization.

5.1 Civilian Deaths Due to Legal Intervention

We present event-study results for civilian deaths in Figure 2.[32] Panel (a) presents results for non-white civilian deaths while panel (b) focuses on white civilian deaths. According to panel (a), pre-treatment effects are relatively close to zero and statistically insignificant. The

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[29]Once again, the endpoints, $y < -5$ and $y > 8$, are grouped together and omitted from the presentation of results.

[30]The earliest state is treated in 1959, so we are unable to capture pre-trends in police killings of civilians for counties in states treated prior to 1964. We remove states that are treated prior to 1964 as a robustness check. See Section 6.1.

[31]We have also computed the standard errors using a wild cluster bootstrap and find no meaningful difference in statistical significance.

[32]The estimates are contained in columns 1 and 2 of Appendix Table A1.

estimated joint effect for the pre-period is .007 (.00885) and is statistically indistinguishable from zero.[33] The point-estimates for the pre-treatment effects provide additional evidence that our main specification account for pre-existing trends in police killing of non-white civilians, therefore, our post-treatment effects will identify a causal relationship between the exposure to collective bargaining rights and police killings of civilians if one exists.[34]

Post-treatment effects show no statistically significant impact on the killing of non-white civilians in the short-run (event-years $0-2$). The lack of any immediate, short-run effect is not surprising given that the duty to bargain will not immediately give rise to union formation – forming a union and negotiating the first contracts will take time.[35] We do, however, find a significant impact of the introduction of bargaining rights on police killings of non-white civilians in both the medium (event-years $3-5$) and the long-run (event-years $6-8$). Although we uncover a causal relationship between police killings on non-white civilians and the adoption to collective bargaining rights, we find no evidence of a relationship with white civilian deaths. Point estimates in panel (b) of Figure 2 are statistically indistinguishable from zero for both the pre-treatment and post-treatment effects.

We summarize our estimates in Appendix Table A2, which present joint fixed effects from equation 2. In the medium-run, an additional $0.029-0.031$ non-white civilians are killed each year in treated counties and in the longer run, an additional $0.032-0.036$ non-white civilians are killed each year. The average number of non-white deaths due to legal intervention in the year before treatment is 0.0403. As such, these results suggests that the introduction of bargaining rights increases non-white deaths due to policing by approximately 72% to 89% over the baseline rate in the medium and long-run.[36]

Figure 3 depicts the number of additional deaths attributed to the adoption of bargaining rights for law enforcement officers. The number of deaths are constructed from the pre-

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[33]See Appendix Table A2.

[34]This is essential for identification and is satisfied across a broad variety of specifications and outcome variables.

[35]Dharmapala et al. (2019) analysis showed that it took several years for Sheriff Departments in Florida to form unions in the early 2000s.

[36]See Appendix Table A2.

and post-treatment effects from the event-study design estimated in Figure 2. The number of deaths attributed to collective bargaining rights steadily increase throughout the 1970s. By 1980, collective bargaining is associated with 30 additional non-white deaths a year. According to Figure 3, the adoption of collective bargaining rights accounts for approximately 10 percent of all non-white deaths due to policing between 1960 and 1988.

Note that these results are only found in states that institute duty to bargain language.[37] The duty to bargain is an important state provision for union formation, drastically decreasing the risk to organizers and increasing the likelihood of actually reaching a first contract. In Figure 4, we show the event-study results by whether a state adopted "the duty to bargain" (panel (a) for non-white deaths and panel (c) for white deaths) rather than merely having employers being authorized or permitted to bargain.[38] The control group for this analysis consists of counties in states without collective bargaining laws and counties in states where collective bargaining is prohibited. The first row of figures presents the results for non-white deaths and the second row presents the results for white deaths. Comparing panels (a) with (b), it becomes clear that the duty to bargain provision completely explains the increase in non-white civilian deaths. States where employers do not have the duty to bargain, see no increases in non-white deaths. Given that we are estimating the "intent-to-treat" effect of police unions, these findings are intuitive: in states that merely grant authorization to bargain, union formation is substantially less likely.

To investigate whether these results are driven by geographies that have a large proportion of non-white citizens, we stratify the treatment group by quartiles based on the 1960 non-white population. Appendix Figure A4 plots pre- and post-treatment effects comparing estimates across non-white population shares to the original estimates in Panel (a) of Figure 2. While we find statistically significant increases in the long-run for treated counties in the first and second quartiles, the magnitude of our results is clearly driven by counties with a fraction of non-white residents in the highest quartile. This is not simply because non-white

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[37]We do not differentiate between states with or without compulsory arbitration.

[38]Refer to panel (b) for non-white deaths and panel (d) for white deaths.

areas have a higher level of police shootings: in a finding similar to Feigenberg and Miller (2018), Appendix Figure A5, shows the relationship between police killings of non-white civilians and the percentage of the population that is non-white has an inverted U-shape.[39] This result implies that the ability to collectively bargain has a larger impact on non-white civilian deaths in regions with a larger non-white population.

5.2   Crime, Expenditure, and Employment Related Outcomes

Previous research has shown that collective bargaining is associated with higher pay and better fringe benefits, as well as a shortened work week for law enforcement officers, all of which could influence performance and productivity (Bartel and Lewin, 1981; Feuille et al., 1985; Frandsen, 2016; Ichniowski et al., 1989). Moreover, Mas (2006), Chappell et al. (2006), and Chandrasekher (2016) find that collective bargaining influences police performance, measured by crime or arrests rates. Figure 5 presents event-study results for the impact of exposure to collective bargaining on the log of total reported crime per 100,000 people, the log of total violent crime, the log of number of sworn officers per 1,000 residents, and the number of officers killed in the line of duty.[40] From Panel (a) and Panel (b), we can see that there is no statistically significant increase in total crime or total violent crime.[41] Therefore, we can rule out higher crime rates driving the increase in police killings of non-white civilians. From panel (c), there is evidence that the number of sworn officers declines both in the medium and the long-run with the adoption of collective bargaining rights. This is generally consistent with the impact of unions on compensation (Bartel and Lewin, 1981; Frandsen, 2016), which can result in employers reducing employment levels.[42] Finally, in

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[39]A significant number of counties are rural and report having zero non-white residents in any given year. Appendix Figure A6 shows that our results are robust to restricting the sample to counties with non-white population above zero as well as restricting the sample to counties with the population above 50,000 residents in 1960.

[40]Table A3 provides a table of the estimates that underlie Figure 5

[41]We do not see any statistically significant impacts on arrest rates although there is a general rise, which is depicted in Figure A7 and Figure A8.

[42]The literature on what unions do is extensive (Doucouliagos et al., 2017) and the issues involved with public sector unionization is different than private sector unions Kuhn (1998). Moreover, research on public sector unions have provided mixed evidence employment levels. However, analysis that attempt to account

panel (d), we do not observe an increase in offers killed in the line of duty. This is also true when we condition on the number of sworn officers, strongly indicating that policing is not becoming safer for rank-and-file officers.[43]

In addition, we conduct a city-level analysis to check the validity of our crime regressions. Due to the voluntary nature of UCR reporting, we restrict the sample to cities with a population greater than 50,000 residents in every year and that fully report at least 25 times between 1960 and 1988. We report event-study results for the log of total crime per 100,000 residents, log of violent crime per 100,000 residents, log of murder per 100,000 residents, and log of sworn police officers per 1,000 residents in Appendix Figure A9.[44] The post-treatment effects at the city-level confirm our results that exposure to collective bargaining has no impact on crime. The post-treatment effects on police employment are negative but not statistically significant. Nonetheless, our results provide suggestive evidence that police employment decreased after treatment. Using the Annual Survey of Government, we find corroborating evidence for declines in police employment per 1,000 residents which are presented in Figure A10. In this figure we also show there are statistically significant increases in police expenditures per 1,000 residents. Taken in conjunction, this suggests that the duty to bargain increases in police compensation in the long-run.

6   Robustness and Further Explorations

So far, we have provided evidence that suggests our event-study design plausibly identifies the effect of the duty to bargain provision on killings by police. Nonetheless, there remain several threats to the causal interpretation of our results. In this section, we explore

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for endogeneity, typically find a negative relationship between unionization and employment. See Bartel and Lewin (1981),Trejo (1991), Anzia and Moe (2015), and Frandsen (2016) for a more comprehensive empirical evaluation of the impact of unionization on police employment.

[43]We are cognizant of the fact that the number of sworn officers is impacted by the treatment and will produce biased estimates. As such, these results are not presented, but are available upon request.

[44]To calculate the natural log of total murders we alter the number of murders so that $ln(murder) = \widehat{murder} = ln(2) - 1 + \frac{murder}{2}$, if the number of murders is less than 2. Otherwise we take the natural log of murders.

the robustness of our results for non-white civilians killed by law enforcement to various specifications, accounting for heterogeneity in treatment timing, a contiguous county strategy, the count nature of our outcome data, and perform a falsification test using information on suicides. We also explore plausible other explanations for the rise in non-white civilian deaths due to police intervention, such as civil rights protests, the introduction of Law Enforcement Officers' Bills of Rights (LEOBRs), and the reduction in the number of sworn officers. We also show how our results vary by actual union precise by the end of our sample period.

6.1   Using a Balanced Panel, Fixed Effects, & Covariates

The first row of Figure 6 shows our main estimates for comparison. The second row limits the treatment group to counties in states that are treated after 1963, so that we estimate pre- and post-treatment effects from a balanced panel. Restricting the sample in this manner reduces the post-treatment effects in the medium and long-run, but the estimates remain large and statistically significant.[45]

Our main specification includes county fixed effects, urban-by-year fixed effects, and region-by-year fixed effects. Row three of Figure 6 simplifies our model to include only county and year fixed effects. Post-treatment effects using only year fixed effects are similar to the joint effects in row one for the medium-run and slightly larger in the long-run. This provides evidence that county and year fixed effects capture most of the unobserved heterogeneity in our analysis. Therefore, it is not surprising that adding 1960 covariates, interacted with a linear time trend, has little influence on pre- and post-treatment effects (see row four). It is reasonable to be concerned that large counties or states are driving our results. Row five presents estimates from equation 1 dropping California and New York from the sample. Removing two of the largest states produces slightly smaller point estimates, but post-treatment effects in the medium and long-run are statistically significant.

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[45]The states removed from the sample are California, Massachusetts, and New Hampshire.

6.2 Heterogeneity in Timing

Row six of Figure 6 compares early adopters, states that adopted collective bargaining rights prior to 1970, to the control group. Row seven compares late adopters, states that adopted collective bargaining rights in 1970 and after, to the control group.[46] We find pre- and post-treatment effects for both early and late adopters are similar to the main specification, both 3-5 years after treatment and 6-8 years after treatment. However, estimates for the early adopters are estimated with considerably less precision. This is plausibly due to the fact that there are nearly half the number of early adopter counties relative to late adopters.[47] Early adopter counties are also heavily clustered in 1966 while the late adopters are more evenly distributed across time, thus providing more time variation as well.

The fact that the early and late adopter point estimates are similar suggests that it is reasonable to assume that the treatment effect estimated by a single difference-in-difference estimator would not be biased due to heterogeneity in the treatment effect across timing groups. While we focus on an event study design which some authors suggest should avoid the heterogeneity issue (Goodman-Bacon, 2020),[48] other authors have suggested that it still might be of concern. Specifically, Sun and Abraham (2020) argue that a similar bias exists in two-way fixed effects (TWFE) event-study analysis when there is heterogeneity in the dynamics of the average treatment effect on the treated across timing groups. Row six and seven provide evidence that this is not a concern in our analysis. Figure 6 shows that there is still heterogeneity in the treatment effects over time, which may produce biased estimates in the traditional difference-in-difference TWFE model.

To avoid the bias associated with TWFE models, we employ a Callaway and Sant'Anna (2020) estimator in an event-study framework. For each timing group, we use never-treated

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[46]Fourteen states adopted collective bargaining rights between 1959 and 1969 while 11 states adopted bargaining rights between 1970 and 1976.

[47]In each year, we have 432 more counties who are later adopters rather than early adopters (503).

[48]For completeness, in Appendix Figure A11 we plot the weights from a Goodman-Bacon difference-in-difference decomposition which suggests most of our variation is coming from a comparison of the treatment and control groups rather than a comparison across treatment-time groups.

locations in the calendar years before and after treatment. For instance, if the treatment timing group is 1964; the control group consists of never treated locations and the reference years will be from 1959 to 1972. If the treatment timing group is 1976, the control group consists of never treated locations and the reference years will be from 1971 to 1983. Appendix Figure A12 displays treatment effects from a balanced sample using the TWFE estimator as well as the Callaway and Saint'Anna estimator. The Callaway and Sant'Anna estimator suggests the pre-treatment effects are close to zero and post-treatment effects in both models are similar in direction, pattern, and magnitude.

6.3 Contiguous Border Counties

To further push the robustness of our results, we restrict the sample to contiguous border counties and exploit the variation in Figure A13. Contiguous border counties may serve as an effective control group, as neighboring counties are likely to share more similarities than those elsewhere. Figure 7 plots estimates from the event-study design, exploiting variation in timing and location along state borders. Once again the pre-treatment effects are statistically indistinguishable from zero. Post-treatment effects in the short-run and long-run are also statistically indistinguishable from zero, but post-treatment effects in the medium-run are positive and statistically significant.

As an alternative to the event-study approach above, we use an empirical strategy similar to Dube et al. (2010). In this approach, we exploit discontinuities at state borders, using the variation in bargaining rights within cross-state county pairs. In this case, the estimating equation is:

$$K_{ipt} = \alpha + \phi_i + \gamma_{t(p)} + \Psi X_{it} + \beta R_{it} + e_{ipt} \tag{3}$$

where K is the number of civilian deaths caused by law enforcement in county i, pair p, in year t, ϕ is a set of county fixed effects, γ is a set of year or pair-year fixed effects, and R_{it} is an indicator as to the presence of police bargaining rights. Similar to the event-study approach, the contiguous borders empirical approach exploits variation in timing and

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location to identify the impact of collective bargaining on civilian deaths and other crime-related outcomes.

Tables 3 and 4 report estimates from equation 3. In the first row of both tables, we present the results using year fixed effects. In the second row, we present the results using year and county-pair fixed effects. These results are remarkably consistent with our main results in the event-study specification. With only county and year fixed effects, there is no effect of access to collective bargaining rights on white deaths (with a treatment effect of -0.007 deaths), but a significant effect on non-white civilian death (with a treatment effect of 0.025 deaths).

In the most demanding contiguous county specification, we still find evidence that the duty to bargain increases killings of non-white civilians, but the effects are muted relative to other specifications. However, the effect size is still positive and 6.5 times larger in magnitude than the negative white death effect. The findings for crime, the number of sworn officers and the number of officers killed in the line of duty in the contiguous counties approach are similar in magnitude to those in Appendix Table A3, but again, the standard errors are relatively large in the full specification even if the coefficients are essentially unchanged.[49] The key reason the contiguous county specification is not our preferred one is because any mobility of police officers across state lines in response to unionization efforts will affect the estimates in these specifications more dramatically than in the simple event study design. Specifically, if unions draw-in higher quality candidates and these officers are more likely to avoid civilian killings, any estimates of the effects of unionization on civilian deaths will be biased downward. This effect will be more dramatic in contiguous county specifications if officers are more likely to move just across state lines.

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[49]The results are not statistically significant for violent crime and murder and similar in magnitude as to those in Appendix Table A3. The results are available from the authors upon request.

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6.4 Possion and Weighted Least Squares

Since our dependent variable consists of non-negative integers with a significant number of zeros, we check the robustness of our ordinary-least-squares results using a Poisson model. Due to concerns about the number of fixed effects, the Poisson model is estimated using only year and county fixed effects. Appendix Figures A14 plot estimates of the pre- and post-treatment effects. The estimates from the Poisson model provide similar results; no impact in the short-run and statistically or marginally statistically significant post-treatment effects in the medium and long-run. While the OLS estimates suggest the impact in the medium-run is 70 percent larger than the baseline, the estimates from the Poisson model indicate that the medium-run effect is 1.68 times larger than baseline or a 68 percent increase in non-white deaths. Both models result in similarly sized long-run point estimates of the impact of collective bargaining rights.

We further check the validity of our results by estimating the impact of collective bargaining on deaths per 100,000 civilians. Appendix Figure A15 plot pre- and post-treatment effects for a mortality rate estimated from a weighted least squares regressions, where 1960 population is used as weights. Mortality rates produce estimates that are qualitatively similar and are marginally statistically significant in the medium and long run.

6.5 Suicides as a Falsification Test

As a falsification test, we estimate the impact of the adoption of collective bargaining rights on suicides as reported in the Vital Statistics data. The intuition for this is if there is systematic measurement error in death classifications by race that is correlated with the timing of state adoptions of the duty to bargain, it should be reflected in other measures of death. We focus on suicides specifically since recording in the Vital Statistics depends on the cooperation of multiple agencies similar to police killings. We have no reason to expect that the reporting of suicides should be systematically affected by the adoption of collective bargaining rights, but it is plausible that it may reflect any systematic measurement error.

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In Appendix Figure A16, we report the treatment effects for suicides per 100,000 residents. We find no evidence of changes in suicides due to the adoption of collective bargaining rights. This provides additional evidence that we are capturing changes in police behavior and not changes in reporting. However, we still have to be cautious in interpreting our estimates as police killings in the Vital Statistics are an under-count of all police killings that actually occur. Nonetheless, we have no evidence of systematic differences in measurement error that may impact the reporting behavior of police killings in response to collective bargaining.

6.6 Protests

Cunningham and Gillezeau (2019) find a positive relationship between uprisings in the 1960s and civilian deaths due to legal intervention. Violent protest in black communities were typically triggered by confrontations between Black Americans and the police. Citizens and community leaders frequently called for civilian review boards and police unions made of mostly white police officers formed in opposition to external oversight (McCormick, 2015). It is possible that police unions formed in response to protests, or are simply coterminous with protests. To explore this possibility, we estimate a model that includes event-year indicators for periods before and after the first protest occurs in a county. According to Appendix Figure A17, post-treatment effects are slightly smaller, but are statistically significant in the medium-run and marginally statistically significant in the long-run, suggesting that simultaneous occurrence of protests are not driving our results.

6.7 Law Enforcement Officers' Bill of Rights

Through state legislative acts, police have acquired additional protections under Law Enforcement Officers' Bills of Rights (LEOBRs). These are a set of rights related to disciplinary action that supersede police union contracts and are generally applied statewide. Many of the provisions are similar to protections negotiated in collective bargaining agreements (Rushin, 2016). LEOBRs comprise a list of details with regard to who can lead an investigation, the length of an investigation, access to evidence, the scope of disciplinary

action, and restrictions on external oversight. As noted above, it is possible that collective bargaining is triggering the treatment effects found through the introduction of LEOBRs or merely occurring at approximately the same time.

In order to control for simultaneous changes in the legal environment, we control for timing of the introduction of LEOBRs in different states.[50] In panel (a) of Figure 8, we plot event-study estimates for the impact of collective bargaining on non-white deaths, controlling for LEOBRs with event-year indicators for periods before and after the adoption of LEOBRs. Controlling for LEOBRs has little impact on the estimated treatment effects for access to collective bargaining. In panel (b), we estimate the impact of LEOBRs, controlling for collective bargaining rights with event-year indicator variables, and find no effects. This is counter to the common assertions about LEOBRs and suggests that there is a limited effect of these pieces of legislation on police killings of non-white civilians.

In Appendix Figure A18, we explore whether the timing of the introduction of LEOBRs is correlated with the adoption of law enforcement collective bargaining rights. In this figure, we show in the solid red line (with triangle markers) whether a state adopts collective bargaining rights; in the same panel, we plot the probability of a state adopting a LEOBR relative to the adoption of collective bargaining rights (with a black line and circle markers). As expected, the probability of treatment is equal to 1 in the year of treatment and remains at 1 afterwards. As for LEOBRs, the relatively flat black line suggests that the timing of the adoption of LEOBRs is not related to the timing of collective bargaining rights. This is consistent with our finding that the adoption of a LEOBR does not explain the medium or long-run increase in non-white civilian deaths associated with the adoption of collective bargaining rights.

This result is somewhat surprising given some of the commonalities in provisions across

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[50]We obtain the dates for the adoption of LEOBRs from Keenan and Walker (2004). There are 15 states that have adopted formal LEOBRs as of writing, 8 within the time frame of our data: Florida: 1974, Maryland 1974, California: 1976, Rhode Island: 1976, Virginia: 1978, Wisconsin: 1979, Nevada: 1983, Illinois:1983, Louisiana: 1985, Delaware: 1986, Tennessee: 1989, West Virginia: 1990, New Mexico: 1991, Minnesota: 1991, and Kentucky: 1994.

collective bargaining agreements and LEOBRs. This suggests two possibilities. First, LEO-BRs may be largely shifting existing measures that limit accountability out of collective agreements and into state legislation. Alternatively, this might indicate that our core result may not be driven exclusively or primarily through procedural protections, but rather through officer culture in a manner that cannot be replicated by LEOBRs.[51]

6.8 Duty to Bargain and Actual Union Certification

A key question is whether the duty to bargain matters for non-white civilian deaths because it increases the ability for law enforcement officers to actually form unions that offer explicit protections and supports or whether the duty to bargain creates a general culture that increases non-white civilian deaths independent of actual union certification. To disentangle these possibilities, we leverage data from the Law Enforcement Management and Administrative Statistics (LEMAS) database in 1987. These data contain 861 counties in our sample with 429 of them treated.

In Figure 9, we again show the effects of states adopting the duty to bargain, but we split the treatment group into counties that have at least one union in Panel (a) and those that do not in Panel (b). It is clear that the largest effects on non-white civilian deaths occur in counties that have at least one police union as of 1987. However, we also find some evidence of spillover effects: even in counties that do not have a union as of 1987, there is a smaller increase in civilian deaths in the last years of the event study, which is statistically significant in event year 7 and marginally statistically significant in event year 8. We take the results of this exercise as suggesting our main findings are largely driven by police departments that actually certify a union, but there is evidence of spillover effects on non-unionized officers within states with the duty to bargain.

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[51]It is important to note that our analysis does not rule out the possibility that LEOBRs influence police violence. Our LEOBR analysis is from an unbalanced panel and does not include the full universe of LEOBR adoptions, since many states implemented these protections after our sample period.

6.9 Reduction in Sworn Officers and Non-White Civilian Deaths

Given that we find that there is a non-trivial decline in the number of sworn officers in states that adopt the duty to bargain, it is reasonable to wonder whether the reduction in the number of officers is affecting the marginal decision to shoot in a discriminatory fashion. We investigate this possibility here by conditioning on the number of sworn officers in our non-white civilian death event study design. The exercise requires two qualifications. The first qualification is that the number of sworn officers is an endogenous variable to the adoption of the duty to bargain, so causal interpretation of any mediation of the effect should be tentative. However, this exercise is informative about whether changes in the number of officers and non-white civilian deaths are occurring in the same places and times. The second qualification is that there is measurement error in police employment data so its use as a independent variable is questionable (Chalfin and McCrary, 2018).

With those qualification in mind, Figure 10 presents the results with panel (a) showing the results for non-white civilian deaths and panel (b) white civilian deaths. Conditioning on the number of sworn police officers reduces the medium- and long-run effects by less than half. However, the point estimates are still positive and significant. Thus, even if we give a fully causal interpretation to these results – which again is dubious – the reduction in police employment does not uniformly explain the full effect.

7 Conclusion

We find that the introduction of collective bargaining rights for law enforcement drives a substantial increase in non-white civilians killed by law enforcement over both the medium and the long-run. These are large effects: our findings suggest that access to collective bargaining rights accounts for 10 percent of the total non-white civilian deaths from the 1959 to 1988. We find no associated increase in white civilian deaths. Thus, our results suggest that the popular notion that police unions are related to increased violence against

civilian is empirically grounded, at least historically. These results are robust to numerous specifications and a contiguous county design, and cannot be explained by the simultaneous adoption of LEOBRs.

We also find suggestive evidence that collective bargaining rights decrease police employment, which would be consistent with increasing compensation. We do not find a significant impact on crime nor the number of officers killed in duty. This suggests that collective bargaining rights do not affect police safety. This points to a particularly discouraging scenario where policing itself is becoming no safer even while more civilians are killed in the law enforcement process.

A reasonable ex-ante prior would be that collective bargaining rights and police unions shift the marginal decision to shoot in a difficult situation. However, in the absence of racial bias, a uniform shift in the incentives to shoot should lead to more civilians being killed regardless of race and less officers being killed in the line of duty. Our results are not consistent with this simple model, as we do not find any meaningful positive impact on killings of white civilians. While our results do not fully rule out the possibility that there is actually a small positive impact on killings of white civilians that is indistinguishable from zero, our results are consistent with bargaining rights inadvertently triggering a discriminatory use of force. Specifically, if, on average, police are far more likely to perceive themselves as in a difficult or risky situations when they are in the presence of Black or Indigenous men, the change in the marginal situation to shoot will predominately impact non-white men.

We do not take our results as evidence that law enforcement officers should be stripped of their collective bargaining rights, particularly since we have not precisely identified the channel through which these effects are operating. It is possible that channel driving the results is not grounded in collective agreements themselves, but rather the particular culture associated with police unions. In this case, efforts to limit collective bargaining rights or even permissive subjects of bargaining would likely have little impact. We believe a conservative implication of our findings is that employers and reformers within the police unions should

prioritize public safety, particularly of the non-white population, in bargaining and internal communications. These internal efforts may be particularly powerful tools for reducing police violence against civilians given findings from Ba and Rivera (2019) who find that internal police union communications can have strong effects on police behavior and reduce civilian complaints. In this scenario, reform movements within police unions may become a powerful tool in protecting civilians.

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8   Figures

Figure 1: Police Union Bargaining Rights Adoption, 1959-1979

- ☐ No Provision/Prohibited (11)
- ☐ Authorized to Bargain (10)
- ☐ Right to Present or Confer (2)
- ☐ Duty to Bargain (25)

(a) Collective Bargaining Rights

- ☐ 1972-1976 (6)
- ☐ 1968-1971 (10)
- ☐ 1964-1967 (6)
- ☐ 1959-1963 (3)
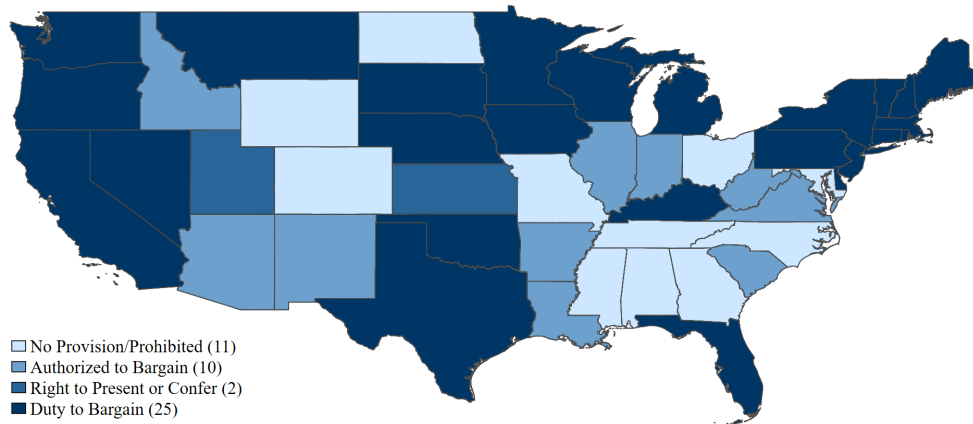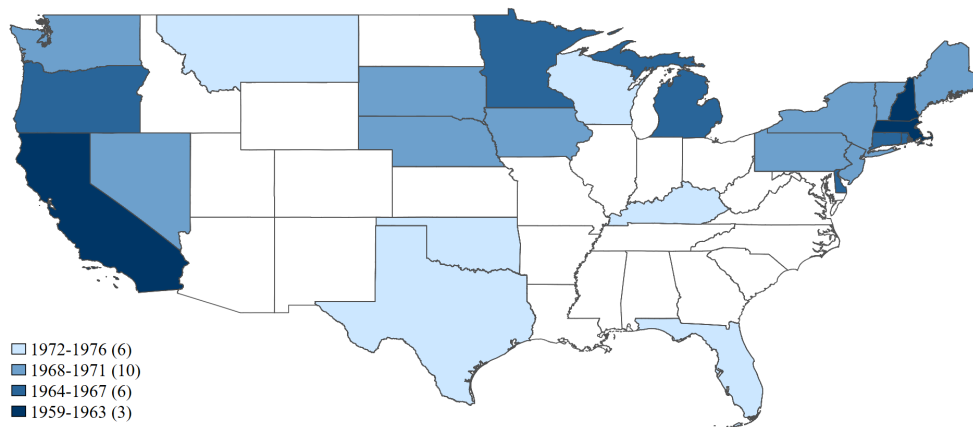
(b) Timing - Duty to Bargain

Figure 2: Duty to Bargain Event Study: Results for Civilian Deaths
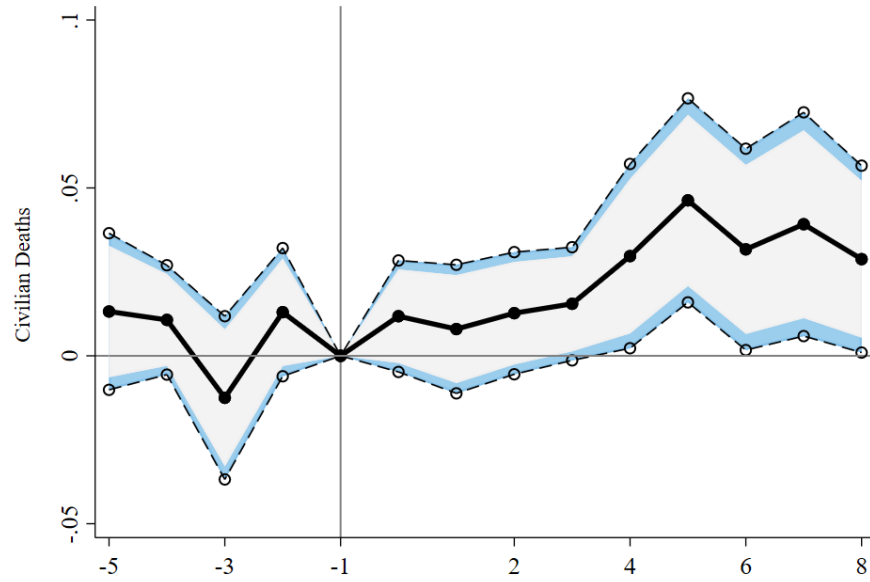
(a) Non-White Civilian Deaths
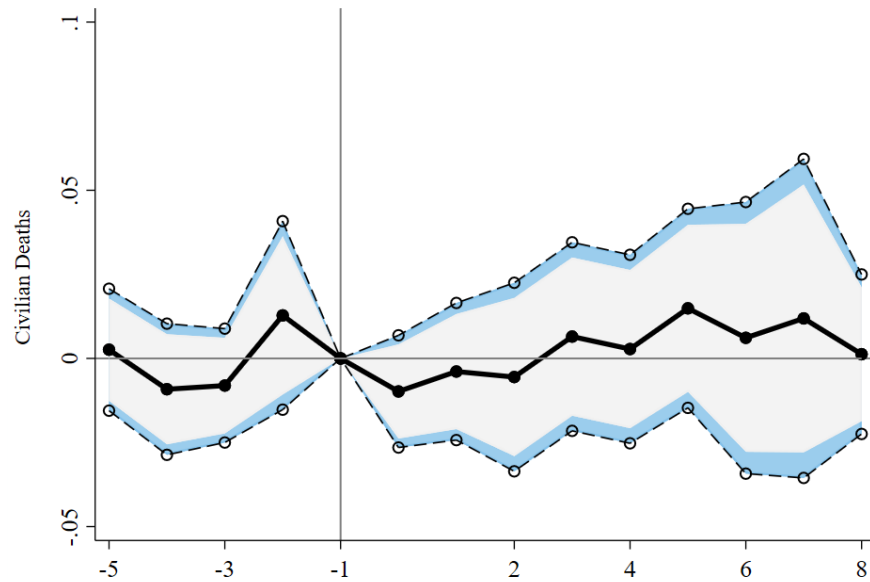
(b) White Civilian Deaths

Notes: This Figure displays ordinary least squares coefficients from equation 1. The horizontal axis represents event-years (years before and after the adoption of the duty to bargain). Regressions includes county, C, region-by-year R-Y, effects, and urban-group-by-year U-Y, effects. The 95% and 90% confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by state, and are presented in blue and gray.

Figure 3: Additional Non-White Deaths per Year Due to the Adoption of the Duty to Bargain Requirement
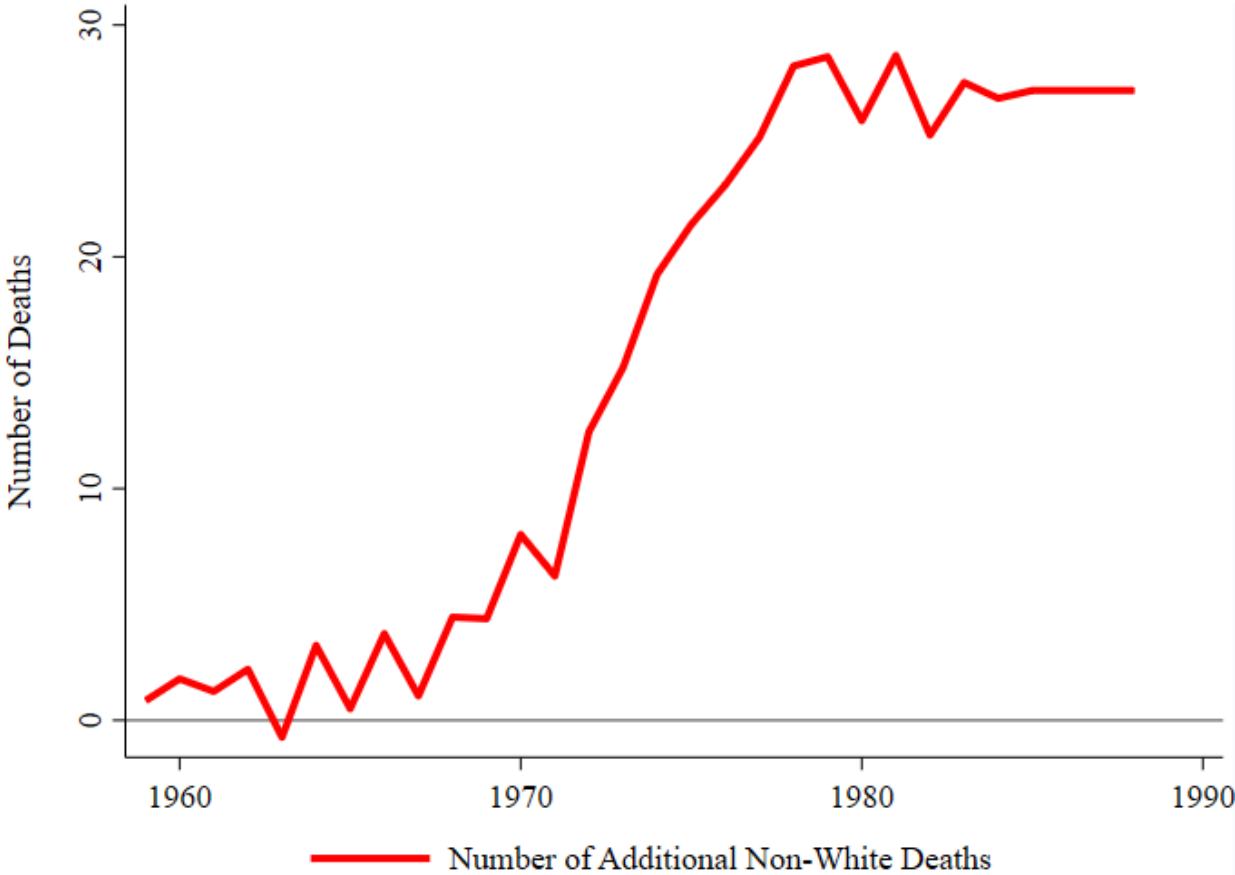
Notes: The number of additional non-white deaths per year are constructed from the event-study estimates in panel (a) of Figure 2.

Figure 4: Weaker Bargaining Rights Relative to Duty to Bargain.
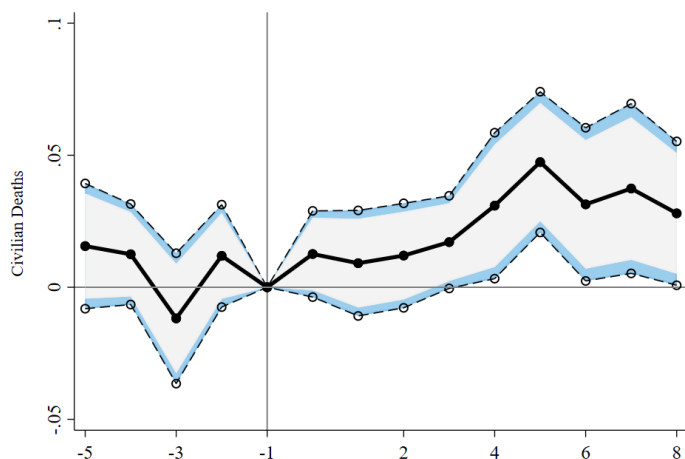
(a) Duty to Bargain: Non-White
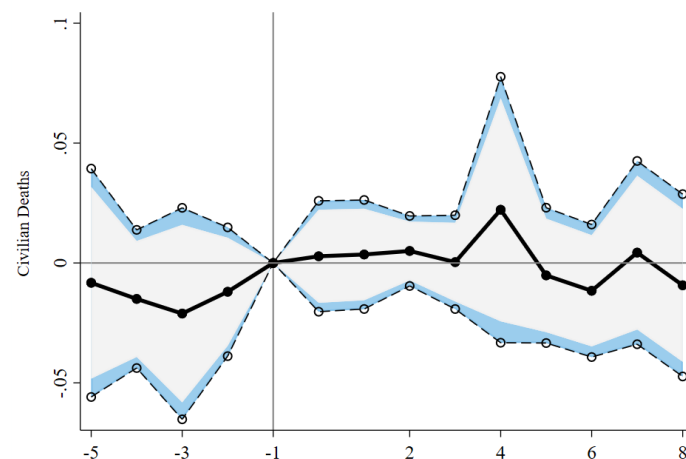
(b) Authorized to Bargain: Non-White
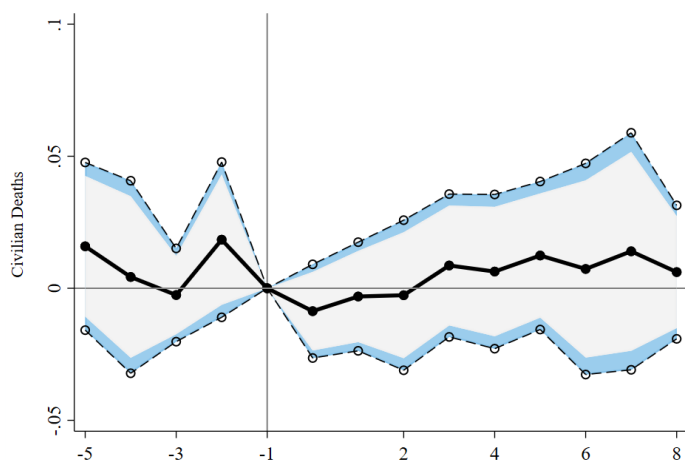
(c) Duty to Bargain: White
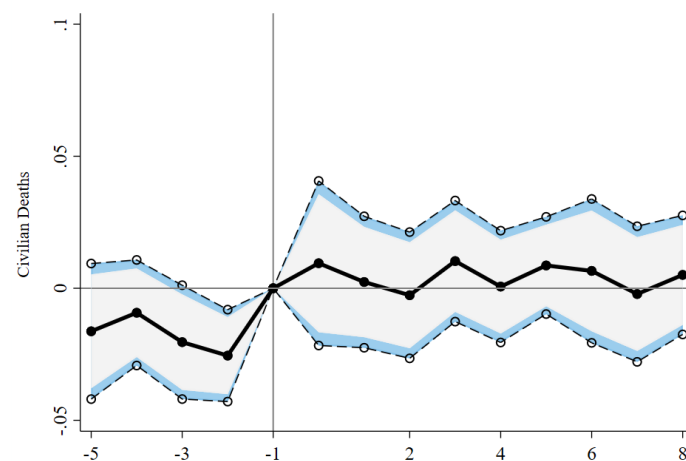
(d) Authorized to Bargain: White

Notes: Panel (a) displays least-squares coefficients for the introduction of collective bargaining rights. The treated groups in Panel (a) and Panel (c) are states that introduce a duty to bargain while the treated group in Panel (b) and Panel (d) are states that merely authorize bargaining. The control group includes states with no provision to bargain or where collective bargaining is prohibited. Twenty-five states have the duty to bargain and 12 states merely provide authorization to collectively bargain. The 95% and 90% confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by state, and are presented in blue and gray.

Figure 5: Duty to Bargain Event Study: Results for Crime and Sworn Officers
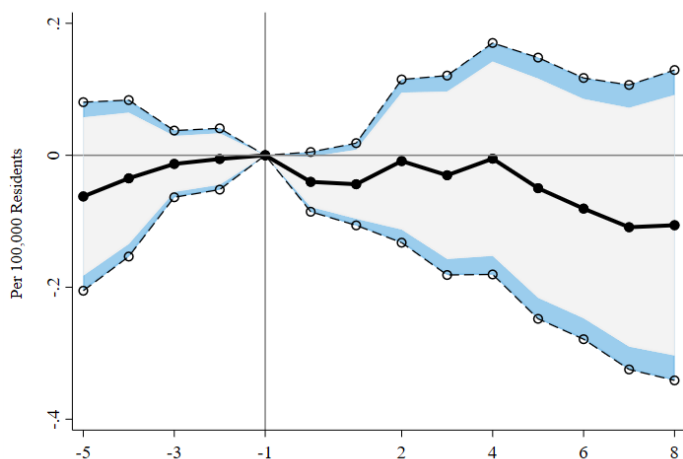
(a) Total Crime
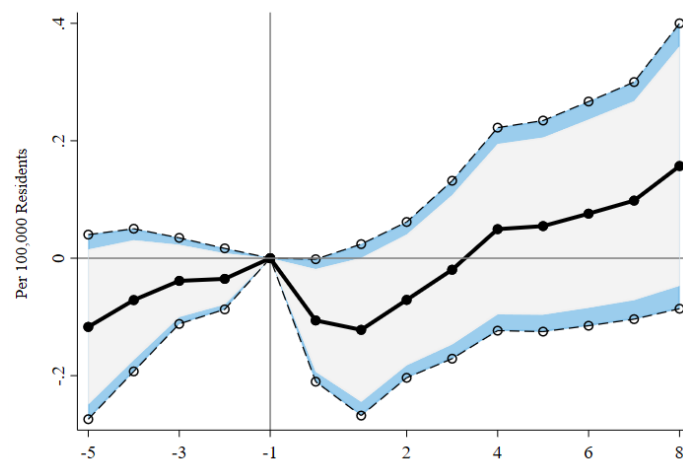
(b) Violent Crime
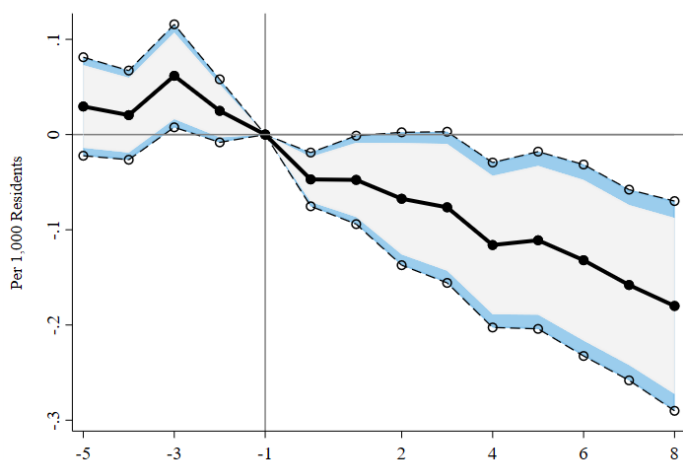
(c) Sworn Officers
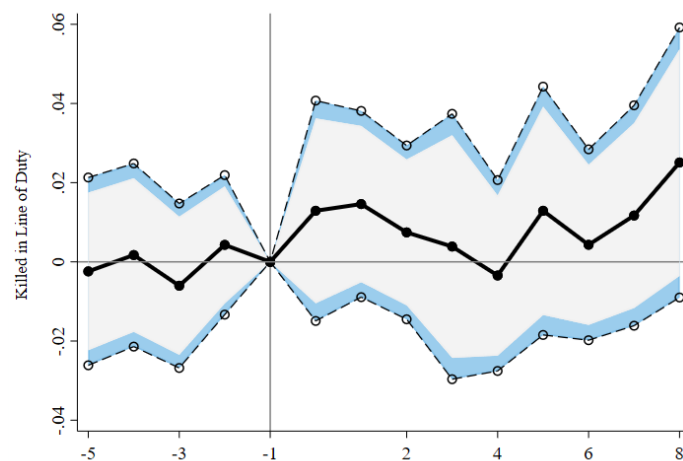
(d) Officers Killed

Notes: The figures display ordinary least squares coefficients from equation 1. The horizontal axis represents event-years (years before and after the adoption of the duty to bargain). Regressions includes county, C, region-by-year R-Y, effects, and urban-group-by-year U-Y, effects. In panel (a), the dependent variable is natural log of total crime per 100,000 residents. In panel (b), the dependent variable is the natural log of violent crime per 100,000 residents. In panel (c), the dependent variable is the natural log of sworn police officers per 1,000 residents. In panel (d), the dependent variable is the number of officers killed in the line of duty. The 95% and 90% confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by state, and are presented in blue and gray.

Figure 6: Duty to Bargain: Robustness Checks - Non-White Deaths
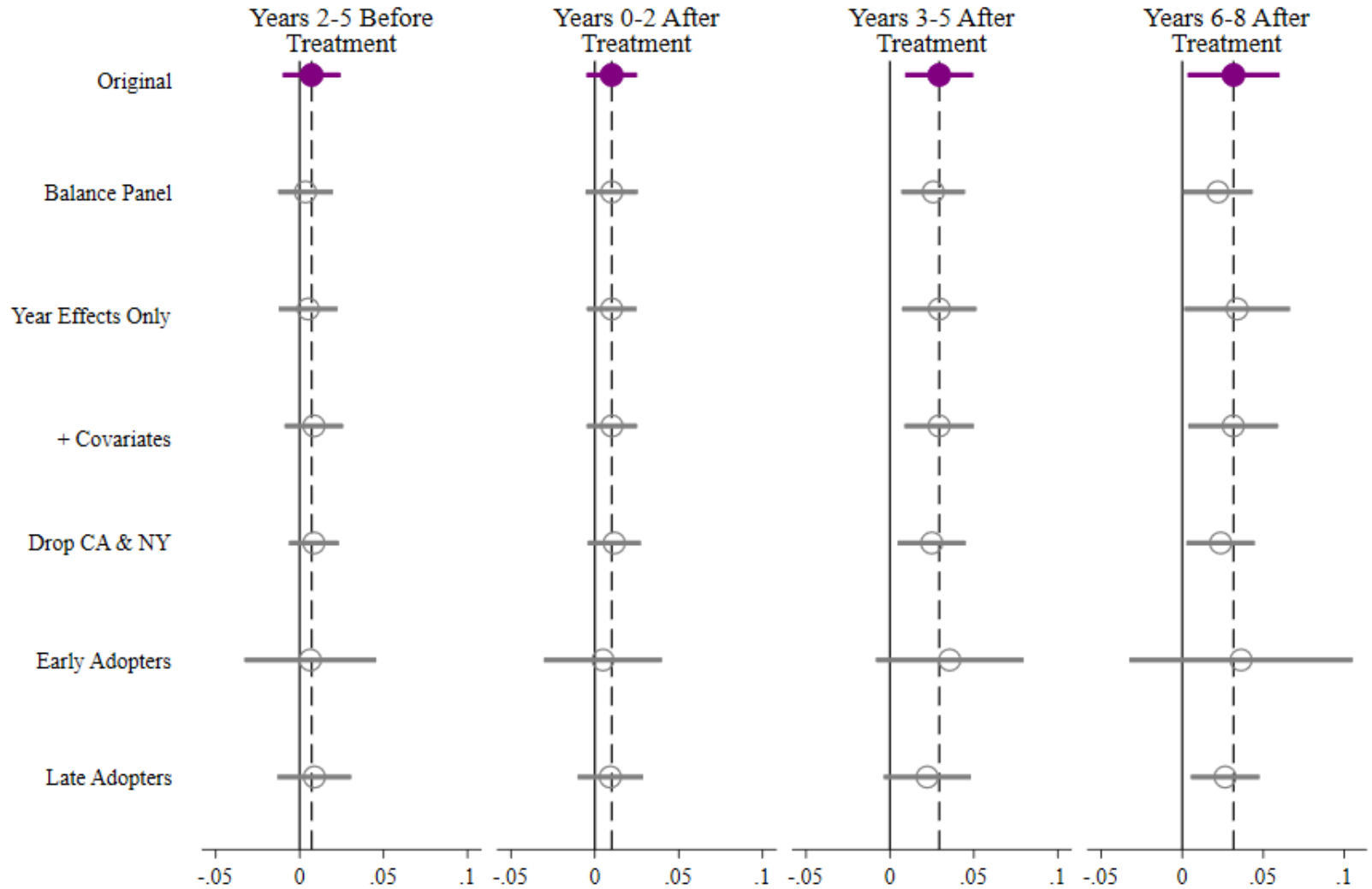
Notes: All rows include county, C, region-by-year, R-Y, and urban-group-by-year, U-Y, effects, except for row 4 which only includes year, Y, fixed effects. The 95% confidence intervals estimated using the heteroskedasticity-robust standard errors, clustered by state, are presented by bold line. Joint least-squares coefficients are presented by the circle marker. "+ Covariates" includes population, population per square mile, white median income, black median income, public assistance recipients, the % of families with income <$3,000, the % of the population using public transportation, the % of the civilian labor force, unemployed, the % of the population that is non-white and the % of homes owner occupied that are non-white.

Figure 7: Duty to Bargain Event Study: Results Using Only Contiguous Counties
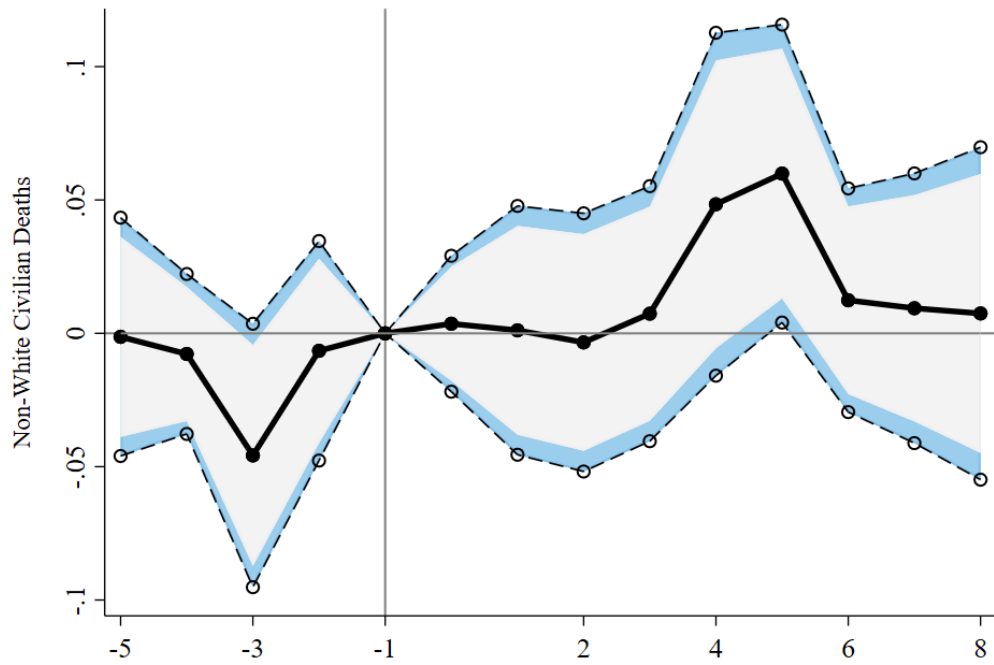
Notes: Dependent Variable - non-white deaths. Sample restricted to contiguous border counties of treated locations (see Figure A13). The 95% and 90% confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by state, and are presented in blue and gray.

Figure 8: Duty to Bargain and Law Enforcement Officers' Bill of Rights Event Study Results
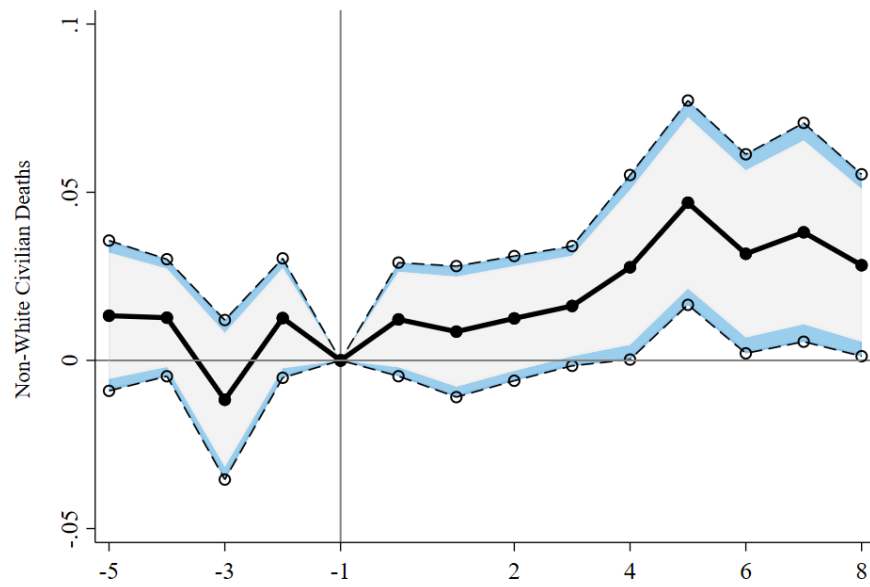
(a) Duty to Bargain Controlling for LEOBR Adoption
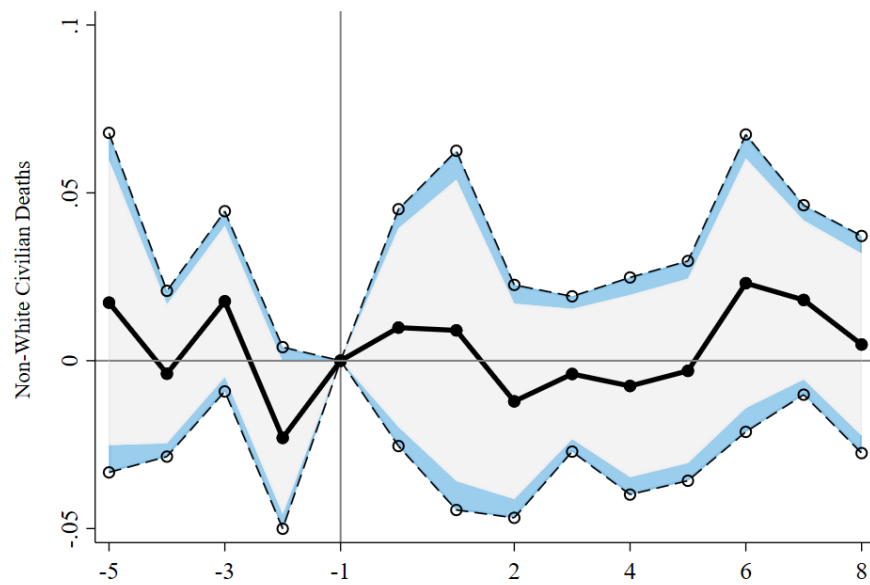
(b) LEOBRs Controlling for Duty to Bargain Adoption

Notes: Dependent Variable - non-white deaths. Panel (a) displays least-squares coefficients for the duty to bargain, accounting for the time before and after the adoption of LEOBRs. Panel (b) displays coefficients for the adoption of LEOBRs, accounting for the time before and after the adoption of the duty to bargain. The 95% and 90% confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by state, and are presented in blue and gray.

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Figure 9: Duty to Bargain Event Study in Counties With and Without Unions as of 1987
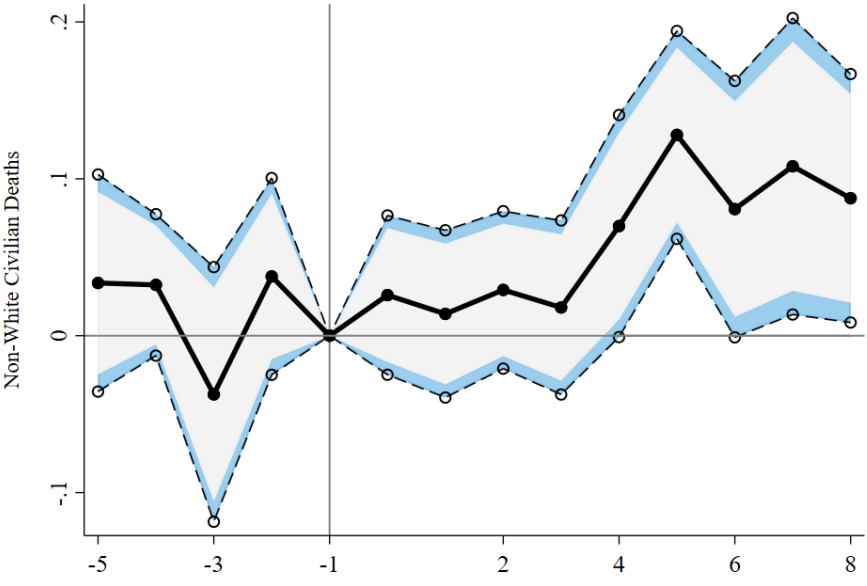
(a) Duty to bargain and at least one union in 1987
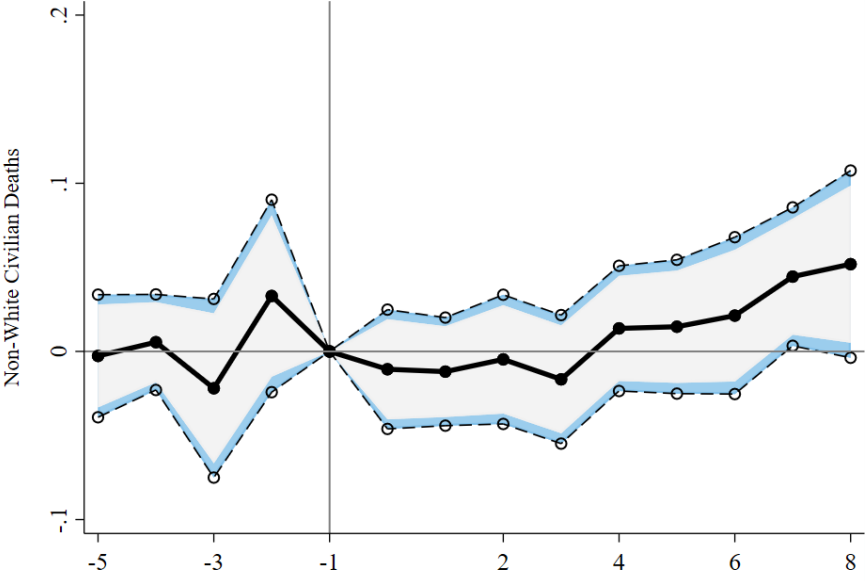
(b) Duty to bargain and no union membership in 1987

Notes: Panel (a) reports the estimated effects of the duty to bargain where the treatment group consists only of counties with at least one law enforcement officers' union in 1987 and is in a state with bargaining rights. The comparison group is all counties in states with limited bargaining rights regardless of local union status. Panel (b) reports the estimated effects of the duty to bargain where the treatment group consists only of counties with no law enforcement officer union membership in 1987 and is in a state with bargaining rights. The comparison group is the same as in Panel (a). Data on union status is obtained from the Law Enforcement Management and Administrative Statistics (LEMAS) data in 1987 and contains 861 counties (429 treated). The 95% and 90% confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by state, and are presented in blue and gray.

Figure 10: Duty to Bargain Event Study: Results Conditioning on Police Employment
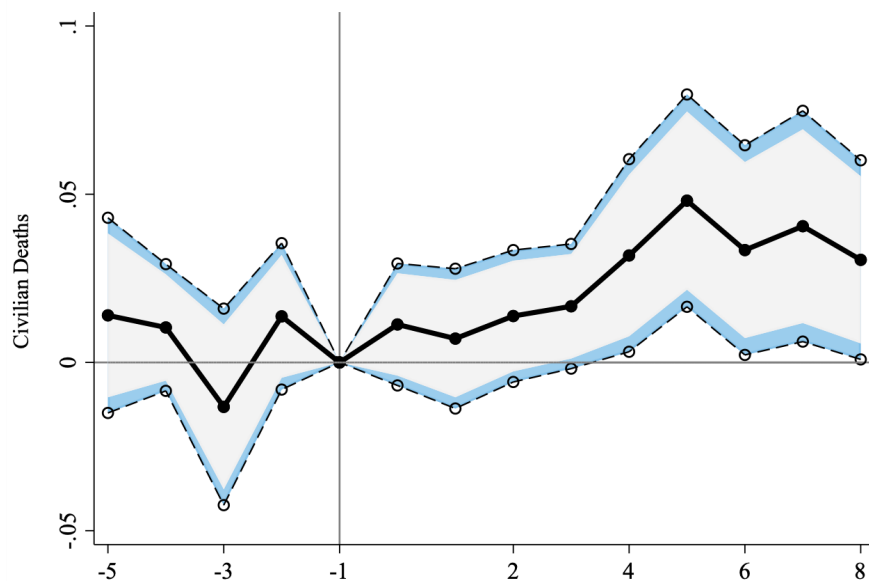
(a) Non-White Civilian Deaths
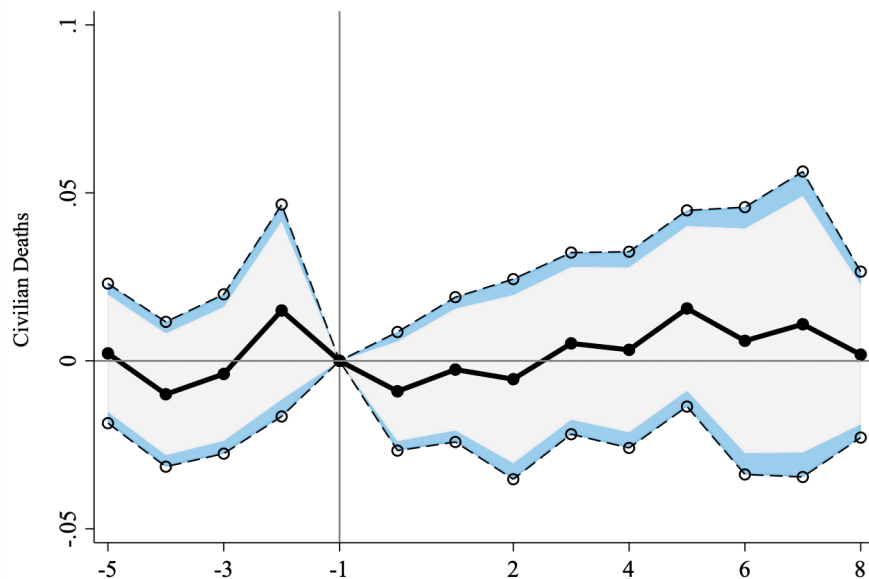
(b) White Civilian Deaths

Notes: Figures display ordinary least squares coefficients from equation 1 with the dependent variable being the number of non-white deaths, conditional on the number of sworn police officers. The horizontal axis represents event-years (years before and after the adoption of the duty to bargain). Regressions includes county, C, region-by-year R-Y, effects, and urban-group-by-year U-Y, effects. The 95% and 90% confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by state, and are presented in blue and gray.

9 Tables

Table 1: Summary Statistics by Duty to Bargain Requirement

	Full Sample				Border Counties Sample		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Treatment	Control	T-Test of	Treatment	Control	T-Test of
	Counties	Group	Group	Differences	Group	Group	Differences
Population	57,996	73,963	43,875	<0.01	83,484	49,055	0.06
Population Per Square Mile	218.6	286.22	158.79	0.34	621.61	173.2	0.21
Median Income, White	7,908.85	7,948.04	7,874.18	0.42	8,224.34	7,901.95	0.09
Median Income, Black	2,498.29	2,081.64	2,866.77	<0.01	2,179.76	2,743.67	<0.01
Public Assistance Recipients	2,334.09	2,873.95	1,856.66	<0.05	3,257.88	2,130.34	0.22
% of Families with Income < $3,000	35.62	31.65	39.13	<0.01	29.56	38.62	<0.01
% of the Population using Public Transportation	1.74	1.78	1.71	0.62	2.27	1.78	0.06
% of the Civilian Labor Force, Unemployed	5.18	5.19	5.16	0.71	5.09	5.39	<0.05
% of the Population, Non-White	9.78	4.08	14.82	<0.01	3.36	13.09	<0.01
% of Homes Owner Occupied, Non-White	26.21	19.55	32.1	<0.01	17.72	30.6	<0.01
Total Crime, Per 100,000 Residents	799.74	845.85	757.22	0.19	841.41	817.06	0.88
Violent Crime, Per 100,000 Residents	40.59	33.71	46.93	<0.01	28.67	43.68	<0.01
Murder, Per 100,000 Residents	3.79	3.1	4.43	<0.01	2.68	4.69	<0.01
Police, Per 1,000 Residents	55.56	58.54	52.57	<0.01	61.98	53.48	<0.01
Police Officers Killed, On Duty	0.03	0.03	0.02	0.16	0.03	0.02	0.5
Civilian Deaths Due to Legal Intervention	0.08	0.08	0.08	0.82	0.12	0.08	0.42
Civilian Deaths Due to Legal Intervention, Non-White	0.04	0.03	0.04	0.47	0.04	0.04	0.89
Civilian Deaths Due to Legal Intervention, White	0.04	0.05	0.03	0.24	0.08	0.03	0.13
Number of Counties	3064	1438	1626	.	480	649	.

Note: This table reports average characteristics from the 1960 Census as well as the 1960 UCR Offenses Known and Cleared by Arrests, UCR Law Enforcement Officers Killed and Assaulted, and Vital Statistics.

Table 2: State Adoptions of Collective Bargaining Rights for Law Enforcement Officers by Year

Treatment Status	Number of Counties	Percent of Counties	Percent of 1960 Population
Treated	1438	46.9	59.9
Year Treated			
1959	24	0.8	3.2
1962	58	2.7	12.1
1964	41	4.0	13.6
1966	181	9.9	21.6
1968	111	13.5	32.8
1969	88	16.4	42.6
1970	192	22.7	44.5
1971	99	25.9	46.1
1972	147	30.7	49.6
1973	120	34.6	51.3
1974	310	44.7	57.1
1976	67	46.9	59.9
Untreated	1626	53.1	40.1

Notes: Data are from the 1960 Census of Population and the NBER Public Sector Collective Bargaining Law Data Set (Valletta and Freeman, 1988).

Table 3: Contiguous Borders Only and the Duty to Bargain on Civilian Deaths

Model	All Deaths	Non-White Deaths	White Deaths
Year FEs	0.024**	0.030***	-0.005
	[0.012]	[0.010]	[0.008]
Pair-Year FEs	0.003	0.01	-0.008
	[0.015]	[0.010]	[0.008]
Obs	70,320	70,320	70,320

Notes: Rows indicate different model specifications. Each set of cell estimates are from separate OLS regressions. Standard errors are clustered by border segment - state. *** p<.01, ** p<.05, * p<0.1

Table 4: Contiguous Borders Only and the Duty to Bargain on Crime and Officer Outcomes

Model	Total Crime	Violent Crime	Murders	Sworn Officers	Officers Killed
Year FEs	-0.077	0.185	0.018	-0.149***	0.001
	[0.088]	[0.123]	[0.042]	[0.036]	[0.006]
Pair-Year FEs	0.01	0.024	0.032	-0.143***	-0.007
	[0.107]	[0.156]	[0.036]	[0.055]	[0.011]
Obs	61,982	57,180	61,982	58,216	58,221

Notes: Rows indicate different model specifications. Each set of cell estimates are from separate OLS regressions. Standard errors are clustered by border segment - state. *** p<.01, ** p<.05, * p<0.1

A   Appendix

Figures

Figure A1: Police Killings and Duty to Bargain Provisions Over Time
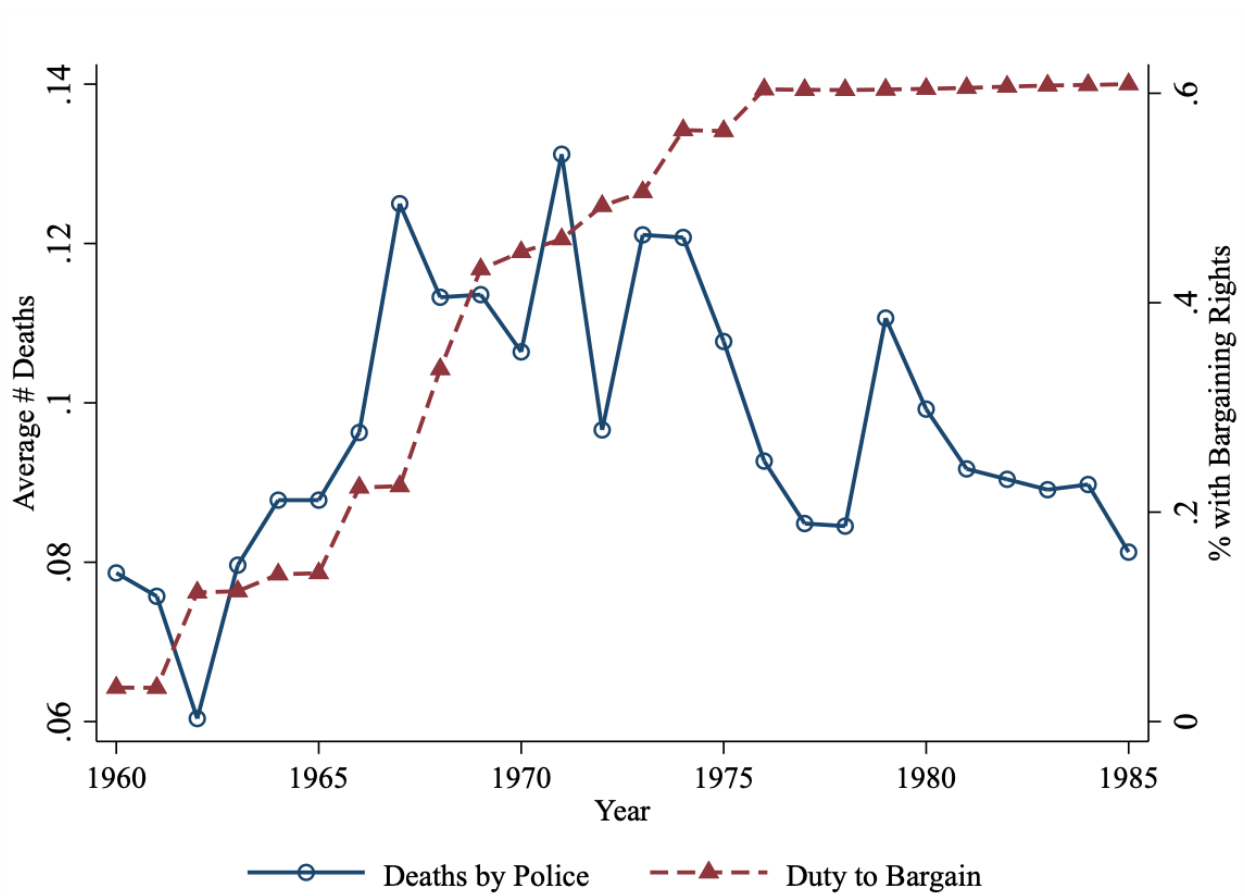
Figure A2: Police Killings, 2000-2016
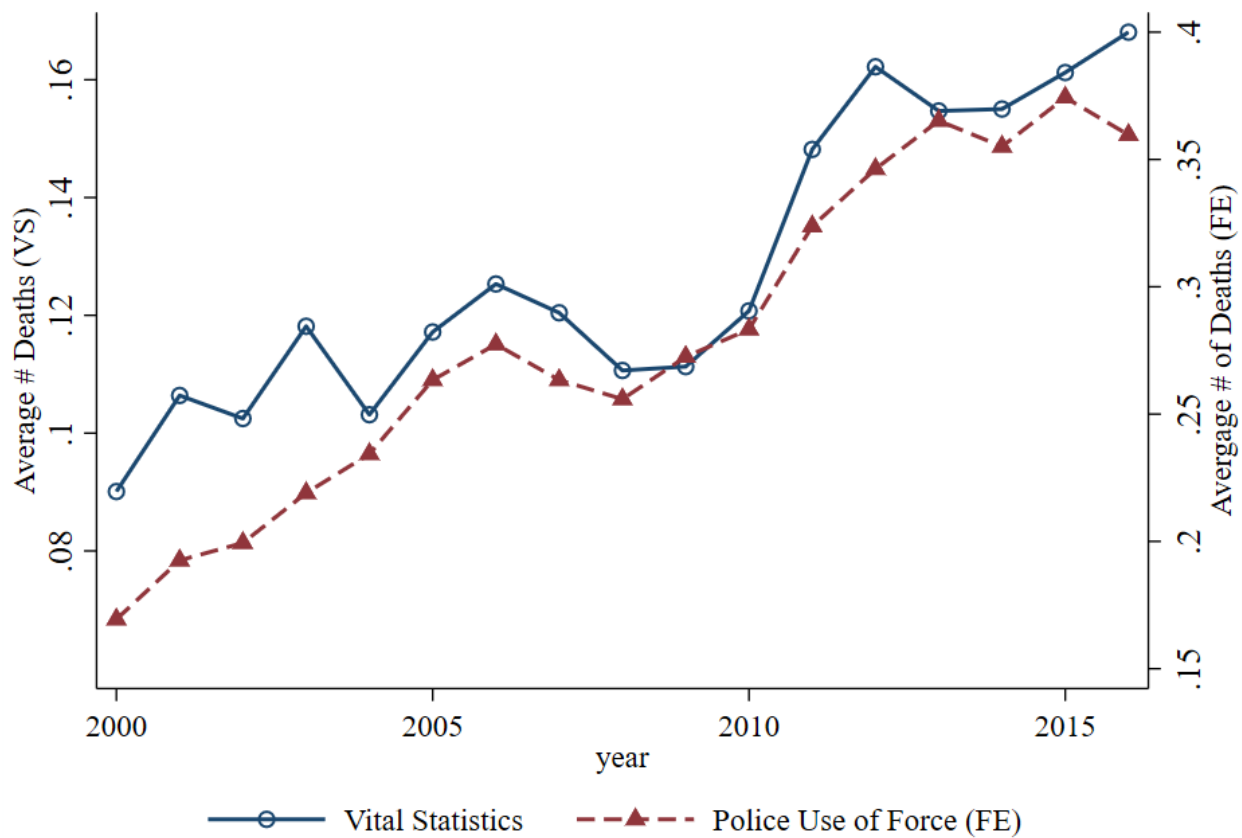
Notes: Data are from the 1998 to 2016 Vital Statistics Multiple Cause of Death Files and the Fatal Encounters Data (see Fatal Encounters Website).

Figure A3: Variation in Timing and Location of Duty to Bargain Provisions and non-White Deaths
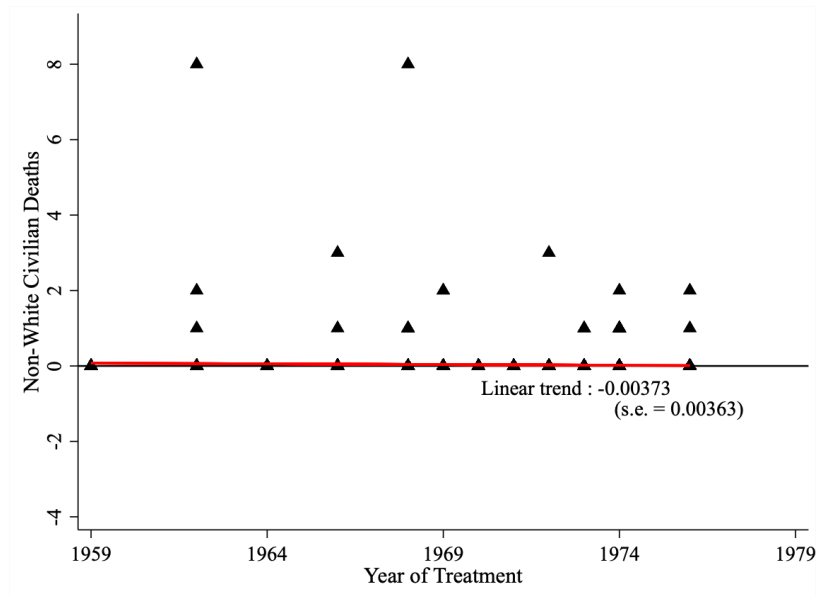
(a) Variation in Timing

(b) Variation in Location

Notes: Panel (a) displays regression coefficients and predicted values are from an univariate regression of non-white deaths due to legal intervention in 1960 on the year collective bargaining rights are adopted. Panel (b) plots the average difference in pre-period non-white deaths due to policing in treated counties relative to untreated counties prior to 1964.

Figure A4: Police Killings by Shares of Non-White Population
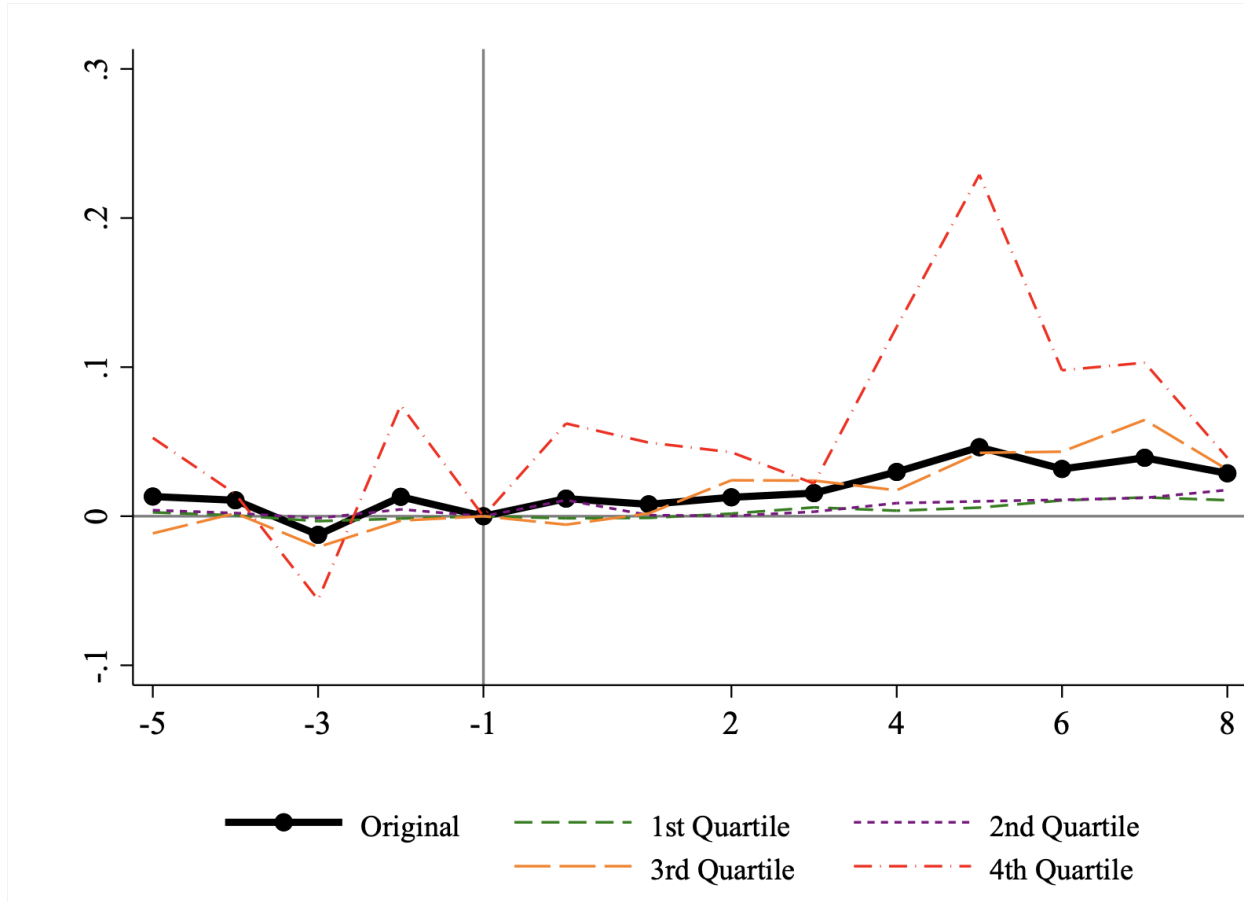
Notes: Figure displays least squares estimates comparing treated counties in a given quartile of shares of non-white population to the entire control group. Confidence intervals are not displayed, but the post-treatment effects are statistically significant in the long-run for counties in the first and second quartiles. Estimates in the third and fourth quartile are measured less precisely, although the estimates are large.

Figure A5: Police Killings and Racial Composition
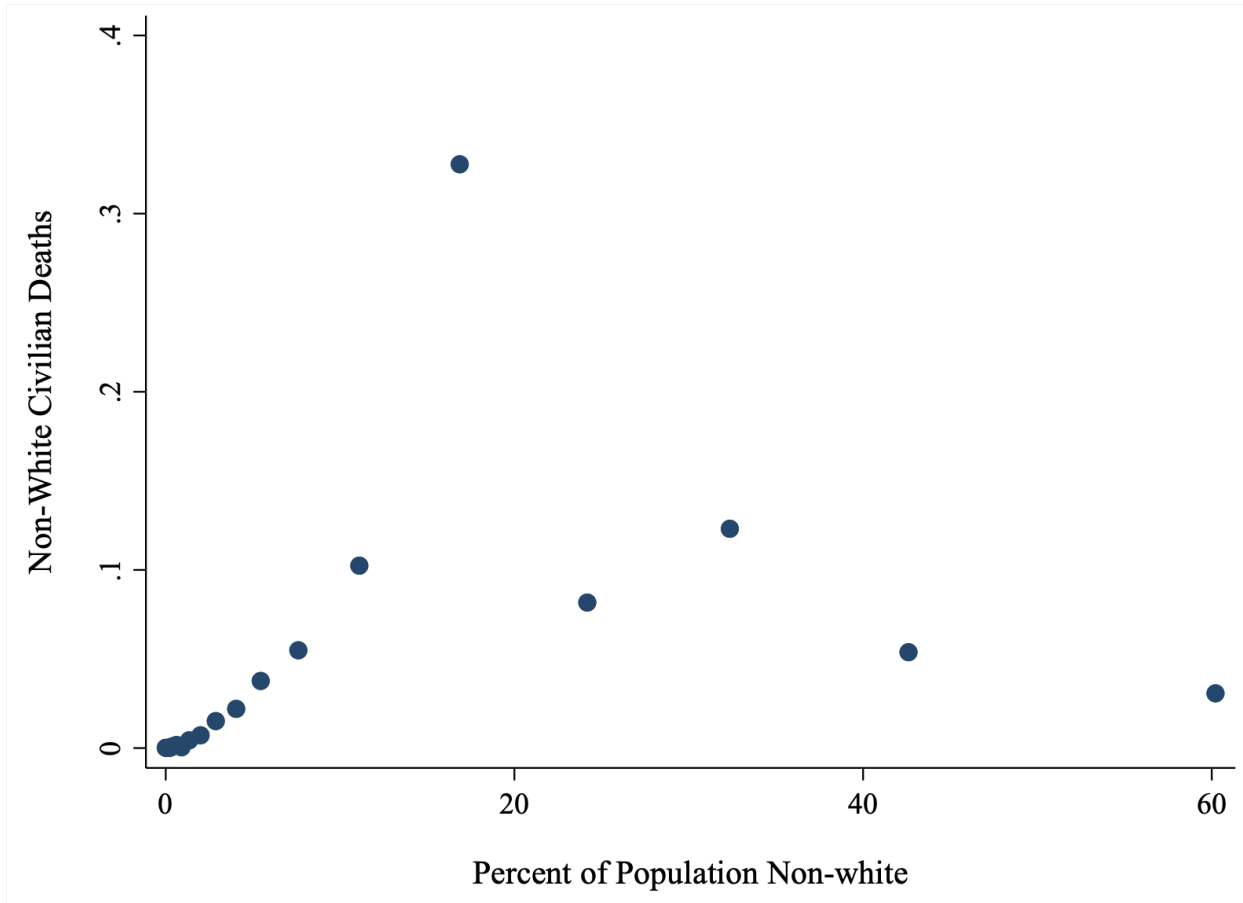
Notes: Data are from the 1960 Census of population and the 1959 to 1988 Vital Statistics Multiple Cause of Death Files (US DHHS and ICPSR 2007).

Figure A6: Duty to Bargain Event Study: Population Robustness Checks
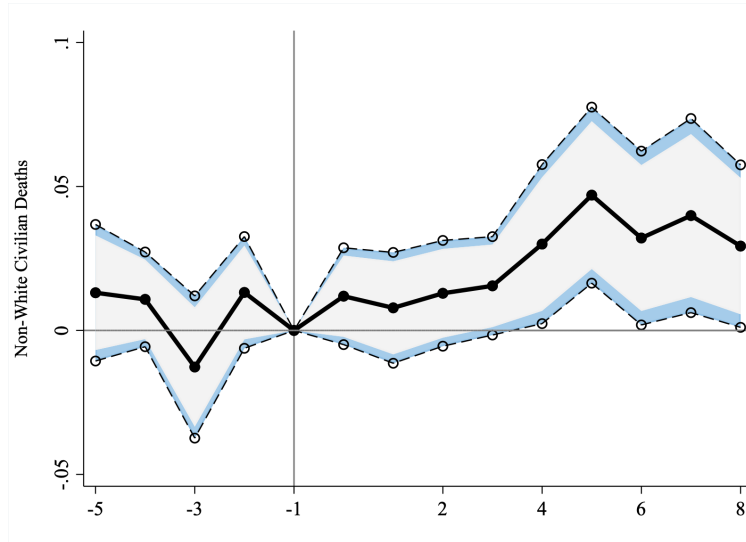
(a) Non-White - Drop if Zero Non-White Population
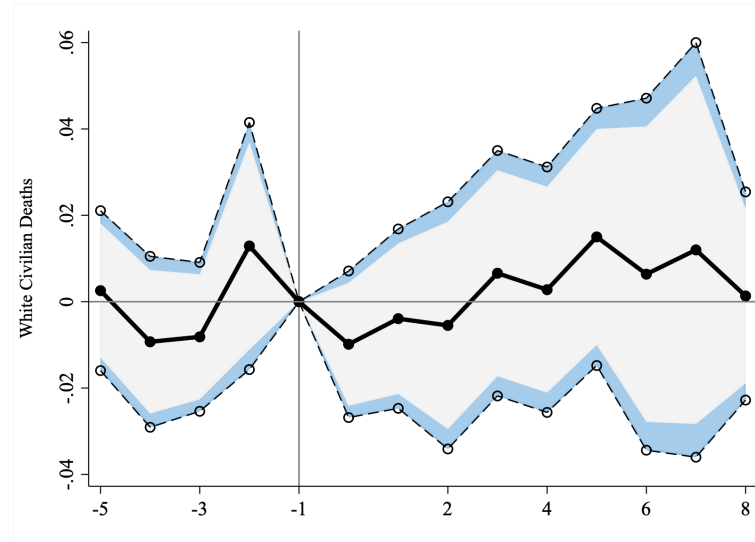
(b) White - Drop if Zero Non-White Population
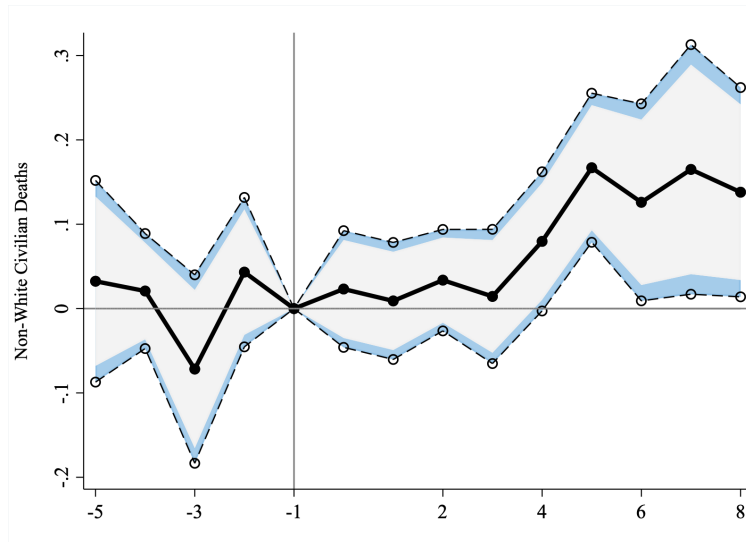
(c) Non-White - Drop if 1960 Population 50,000
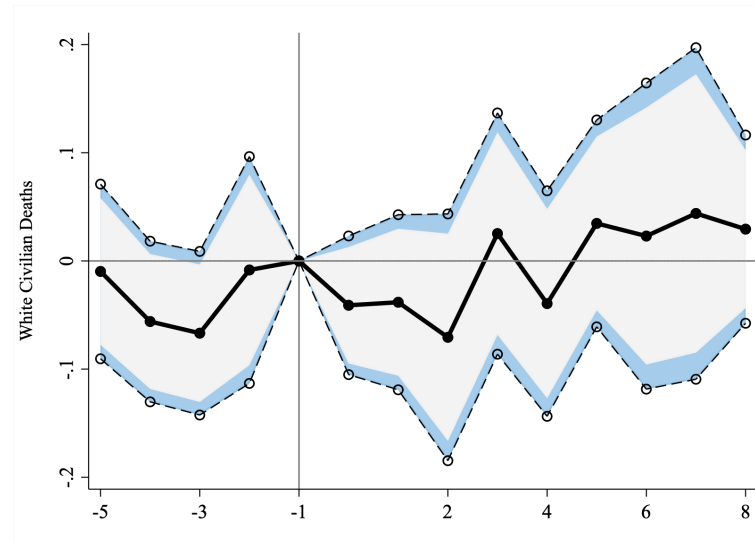
(d) White - Drop if 1960 Population 50,000

Notes: Panel (a) & (b) drop any county that ever recorded zero non-white residents in any year between 1960 and 1988. Panel (c) & (d) restrict the sample to counties with the population above 50,000 residents in 1960. The 95% and 90% confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by state, and are presented in blue and gray.

Figure A7: Duty to Bargain Event Study: UCR Arrests Sample
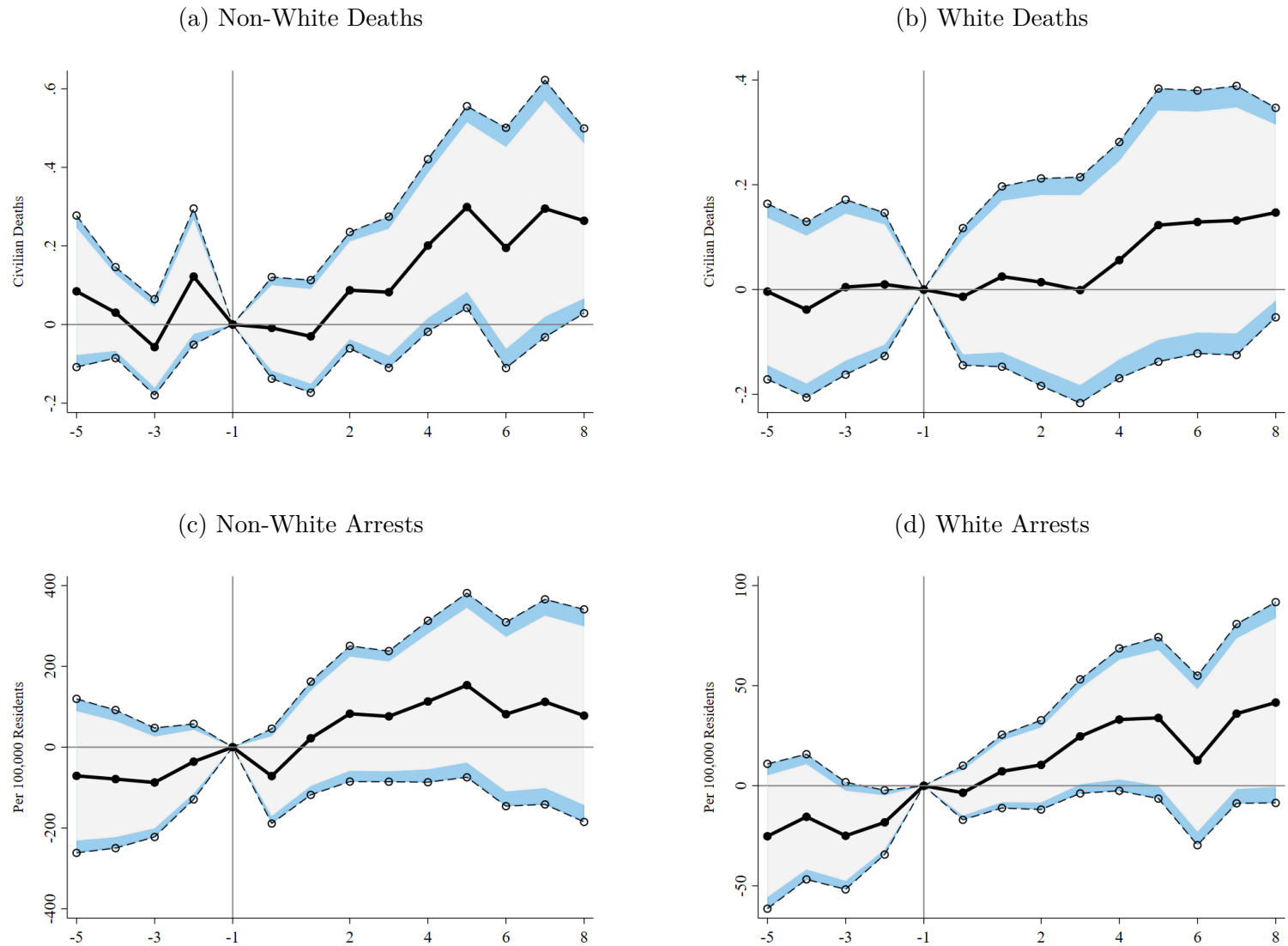
(a) Non-White Deaths

(b) White Deaths

(c) Non-White Arrests

(d) White Arrests

Notes: The regression specification includes county, C, effects, urban-by-year, U, effects and region-by-year, R, effects. Panels (a) and (b) correspond to civilian deaths by race while panels (c) and (d) report total arrests by race. The sample is limited to the 231 counties that report at least 28 years between 1960 and 1988 (149 are treated). The 95% and 90% confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by state, and are presented in blue and gray.

Figure A8: Duty to Bargain Event Study: UCR Arrests Sample – by Arrest Type

(a) Non-White Arrests – Violent Crimes
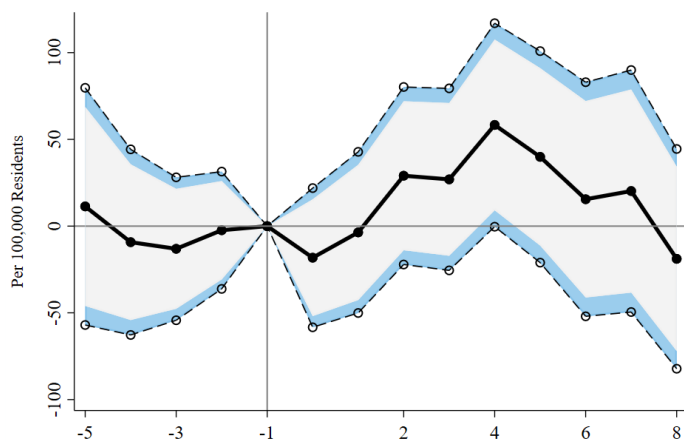
(b) White Arrests – Violent Crimes
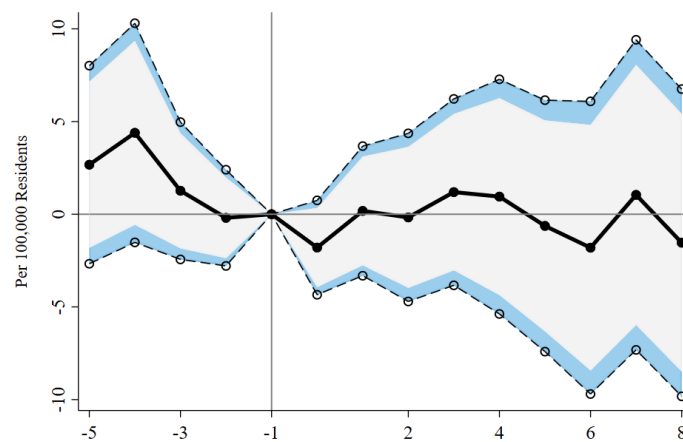
(c) Non-White Arrests – Property Crimes
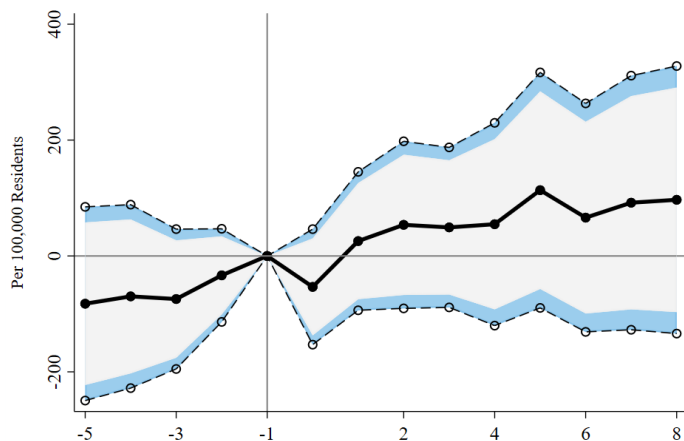
(d) White Arrests – Property Crimes
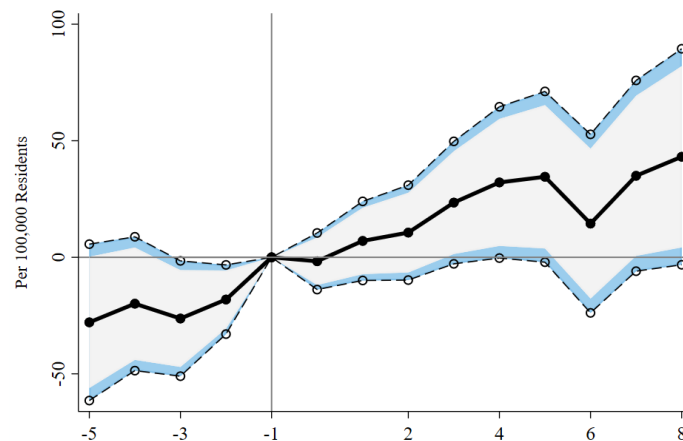
Notes: The regression specification includes county, C, effects, urban-by-year, U, effects and region-by-year, R, effects. Panels (a) and (b) correspond to violent crime arrests by race while panels (c) and (d) report property crime arrests by race. The sample is limited to the 231 counties that report at least 28 years between 1960 and 1988 (149 are treated). Treatment group corresponds to counties with cities treated prior to 1987. The 95% and 90% confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by state, and are presented in blue and gray.

Figure A9: Duty to Bargain Event Study: Crime at the City Level

(a) Total Crime

(b) Violent Crime

(c) Sworn Officers

(d) Murder
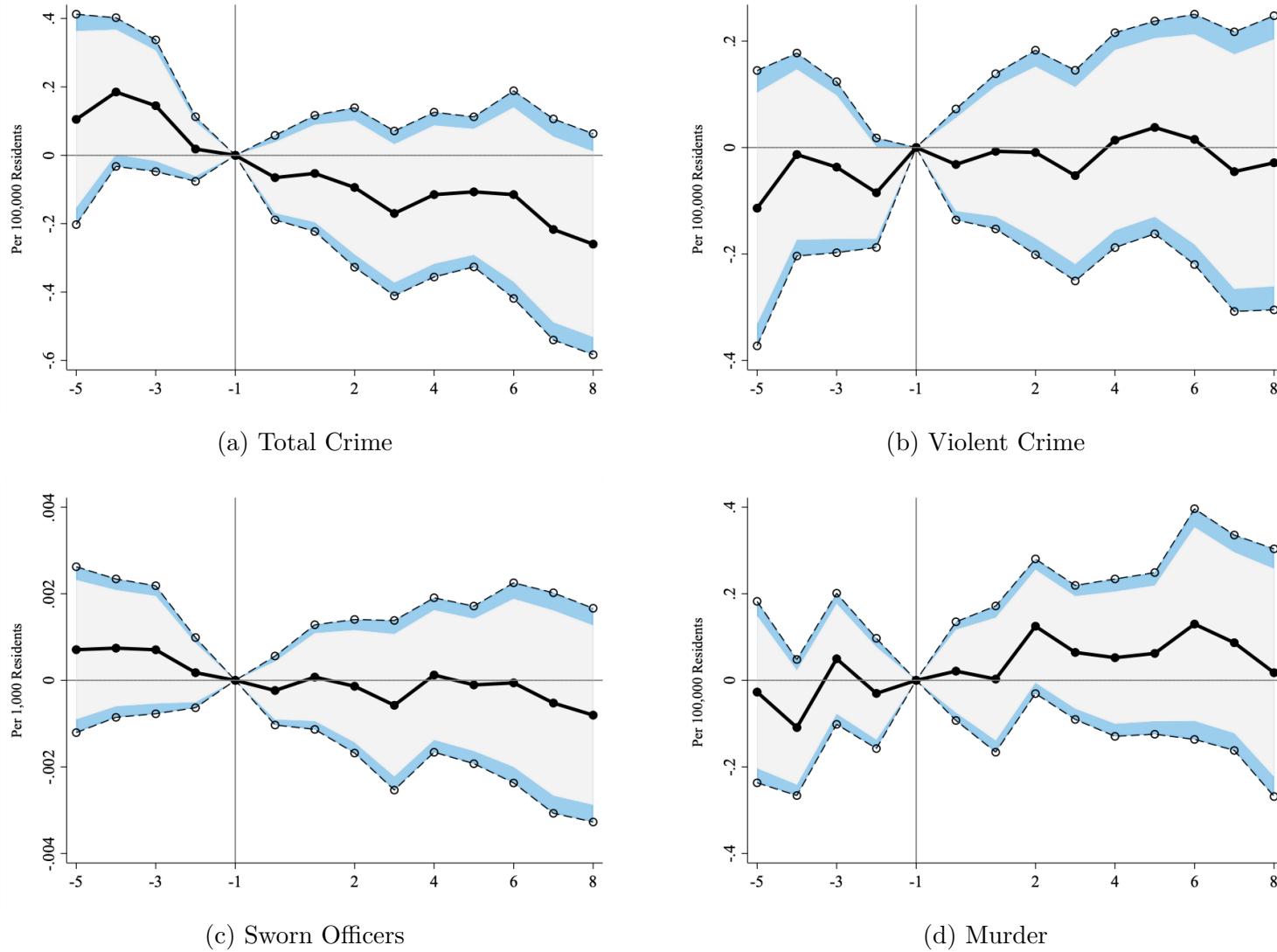
Notes: Figure displays ordinary least squares coefficients from equation 1 for cities with the population greater than 50,000 and report at least 25 years between 1960 and 1988. In panel (a), the dependent variable is the natural log of total crime per 100,000 residents. In panel (b), the dependent variable is the natural log of violent crime per 100,000 residents. In panel (c), the dependent variable is the natural log of murder per 100,000 residents. In panel (d), the dependent variable is the log of sworn police officers per 1,000 residents. The 95% and 90% confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by state, and are presented in blue and gray.

Figure A10: Duty to Bargain Event Study: City Expenditures
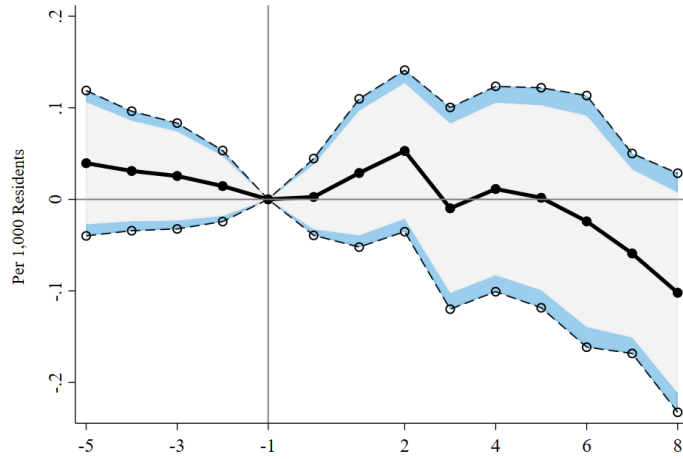
(a) Police Employment
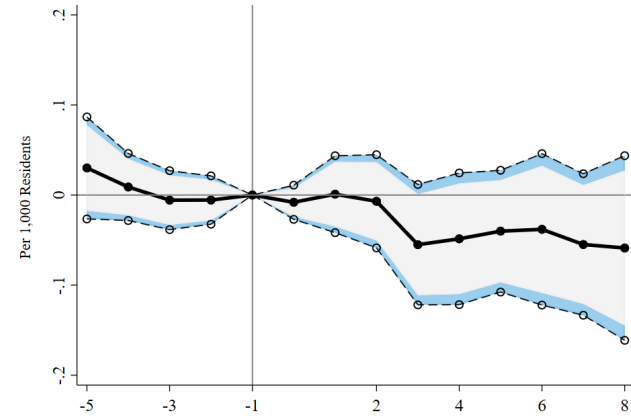
(b) Firefighters Employment
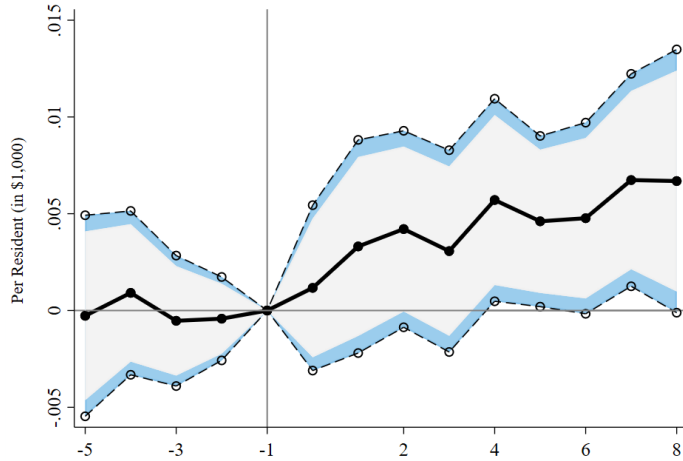
(c) Police Expenditures
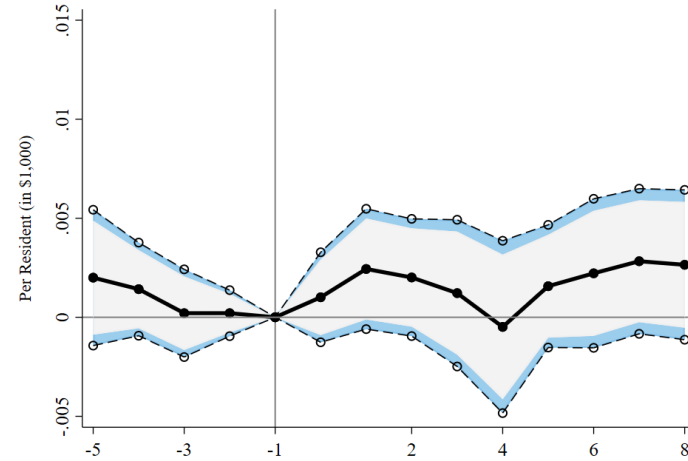
(d) Firefighters Expenditures

Notes: Figure displays ordinary least squares coefficients from equation 1. Employment and expenditure data are taken from the Annual Survey of Governments. Sample limited to municipalities that report 43 out of 47 years, 1960 to 2006. Panels (a) and (b) display employment levels for police and fire departments while panels (c) and (d) display expenditures per capita. The 95% and 90% confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by state, and are presented in blue and gray.

Figure A11: Difference-in-Difference Decomposition: Non-White Deaths
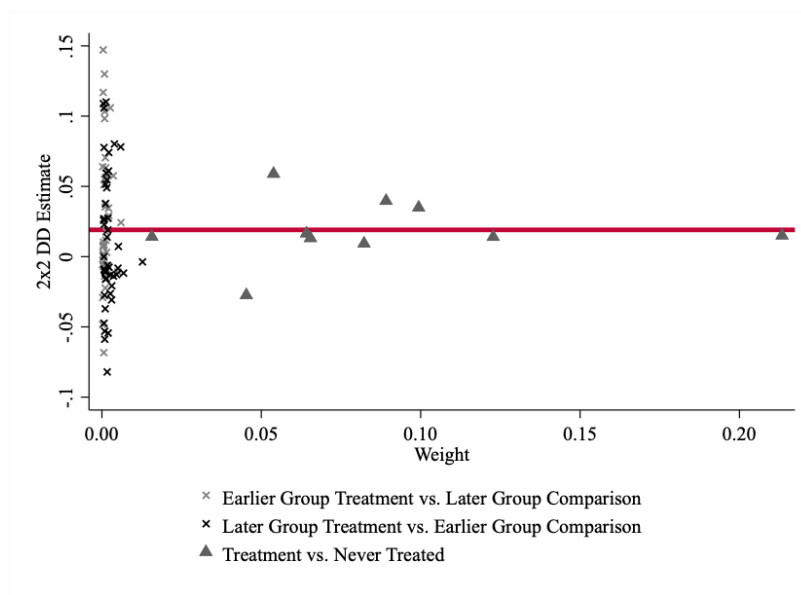
Notes: The DiD estimate is 0.019. Weights(DiD) from decomposition: Early vs Late (Control) 0.053(0.028), Later vs Early (Control) 0.096(0.009), and Treated versus Never Treated 0.851(0.020).

Figure A12: Callaway & Sant'Anna Estimator: Non-White Deaths
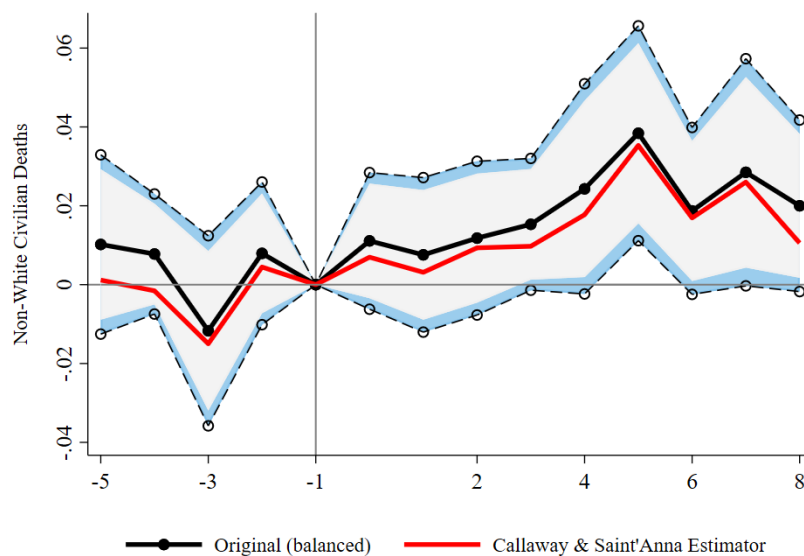
Notes: The figure plots estimates from a balanced panel using two-away fixed effects estimator (black) and Callaway & Sant'Anna estimator (red). 95% and 90% confidence intervals using heteroskedasticity-robust standard errors, clustered by state, are reported for the TWFE estimator.

Figure A13: Duty to Bargain Adoption, Contiguous Counties, 1959-1979

1972-1976 (175)
1968-1971 (199)
1964-1967 (71)
1959-1963 (34)
Control Group (231)
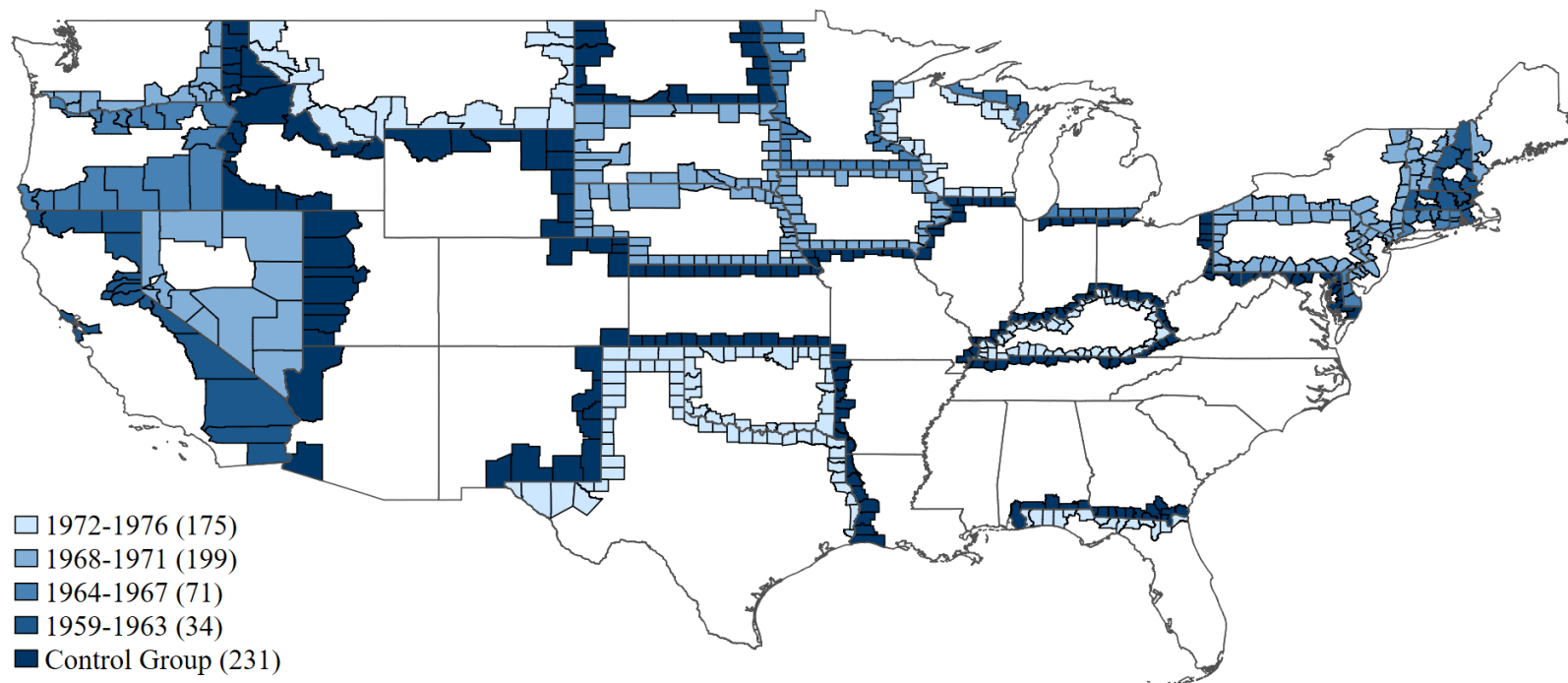
Figure A14: Duty to Bargain Event Study: Results for Non-White Deaths Using Poisson Regression
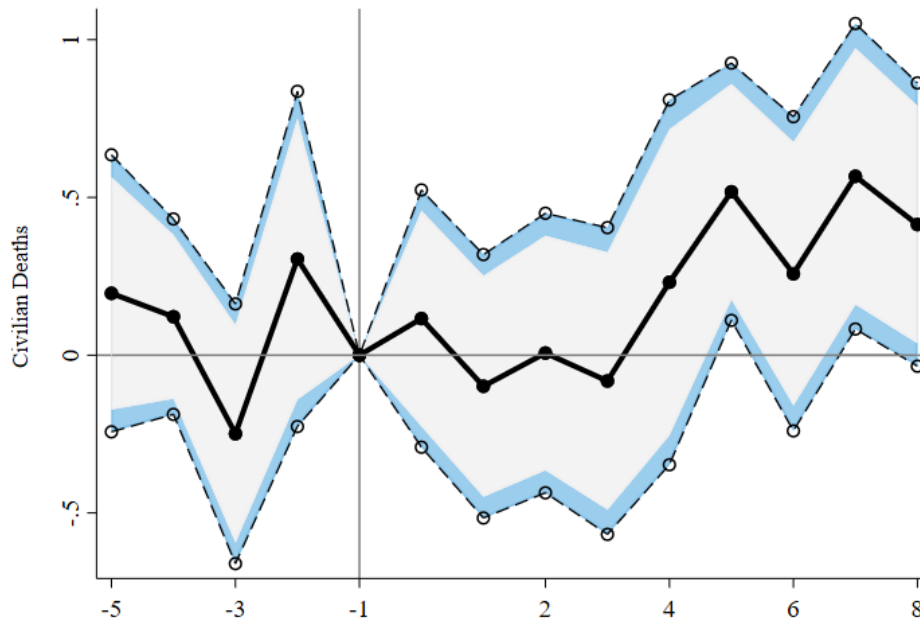
(a) Non-White
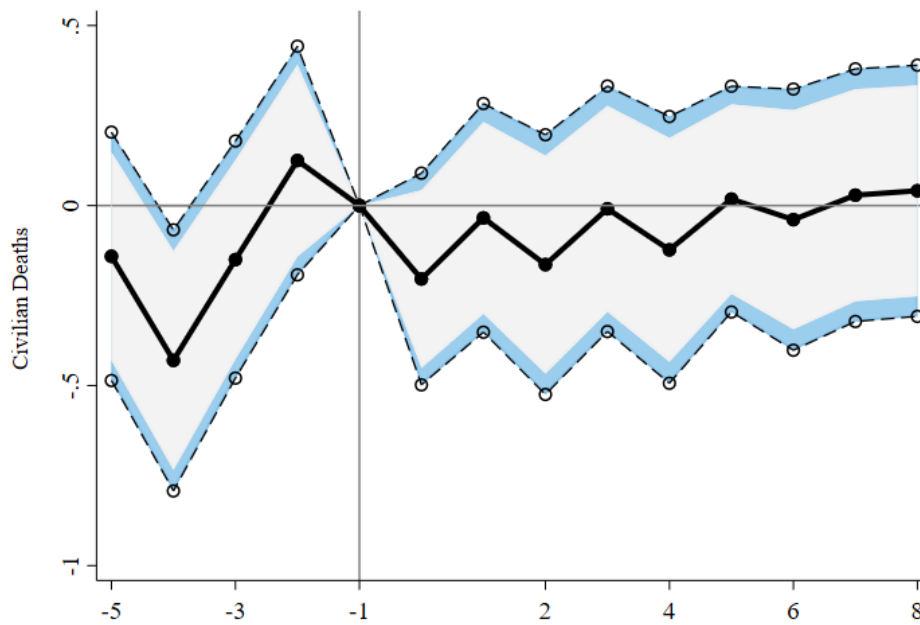
(b) White

Notes: Includes county, C, region-by-year R-Y, effects, and urban-group-by-year U-Y, effects. The horizontal axis represents event-years (years before and after the adoption of collective bargaining rights). The 95% and 90% confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by state, and are presented in blue and gray.

Figure A15: Duty to Bargain Event Study: Results for Civilian Deaths Per Capita
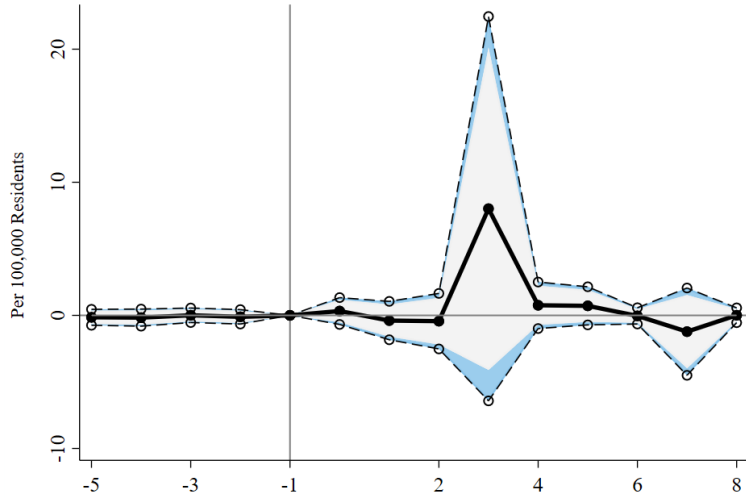
(a) Non-White Civilian Deaths
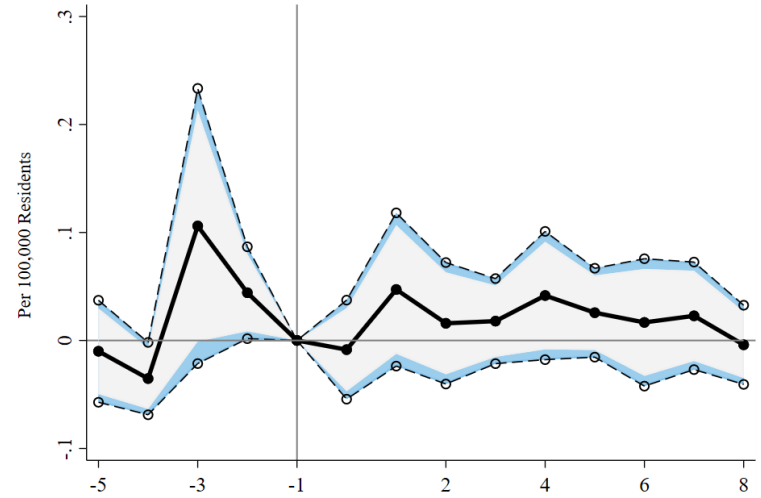
(b) White Civilian Deaths
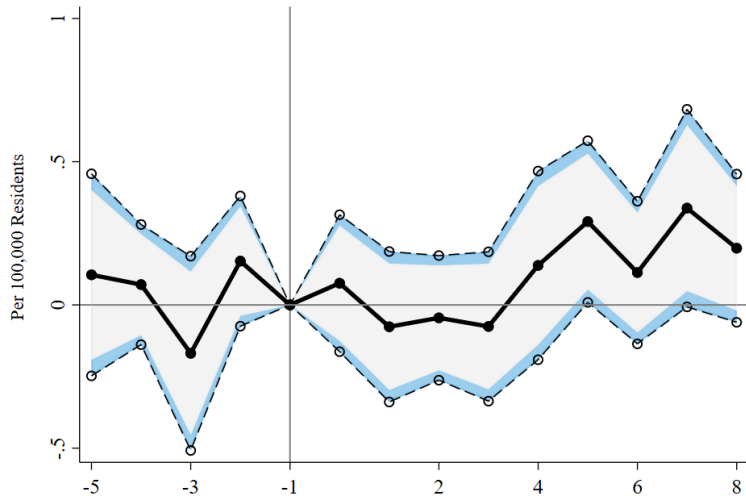
(c) Non-White Civilian Deaths - Population Weights
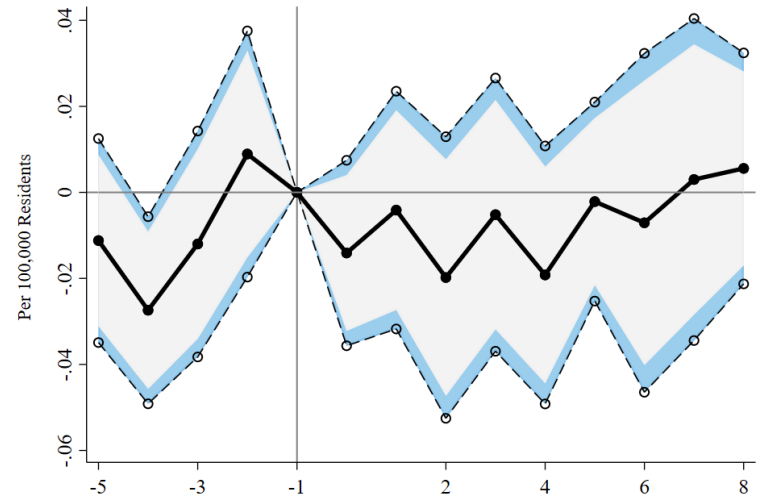
(d) White Civilian Deaths - Population Weights

Notes: Panels (a) & (b) display least squares coefficients for the relationship between the duty to bargain and police killings of civilians. Panels (c) & (d) display weighted least square coefficients - 1960 population for non-whites and whites are used as weights. The 95% and 90% confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by state, and are presented in blue and gray.

Figure A16: Duty to Bargain Event Study: Suicides

(a) Non-White

(b) White

(c) Non-White - Population Weights

(d) White - Population Weights
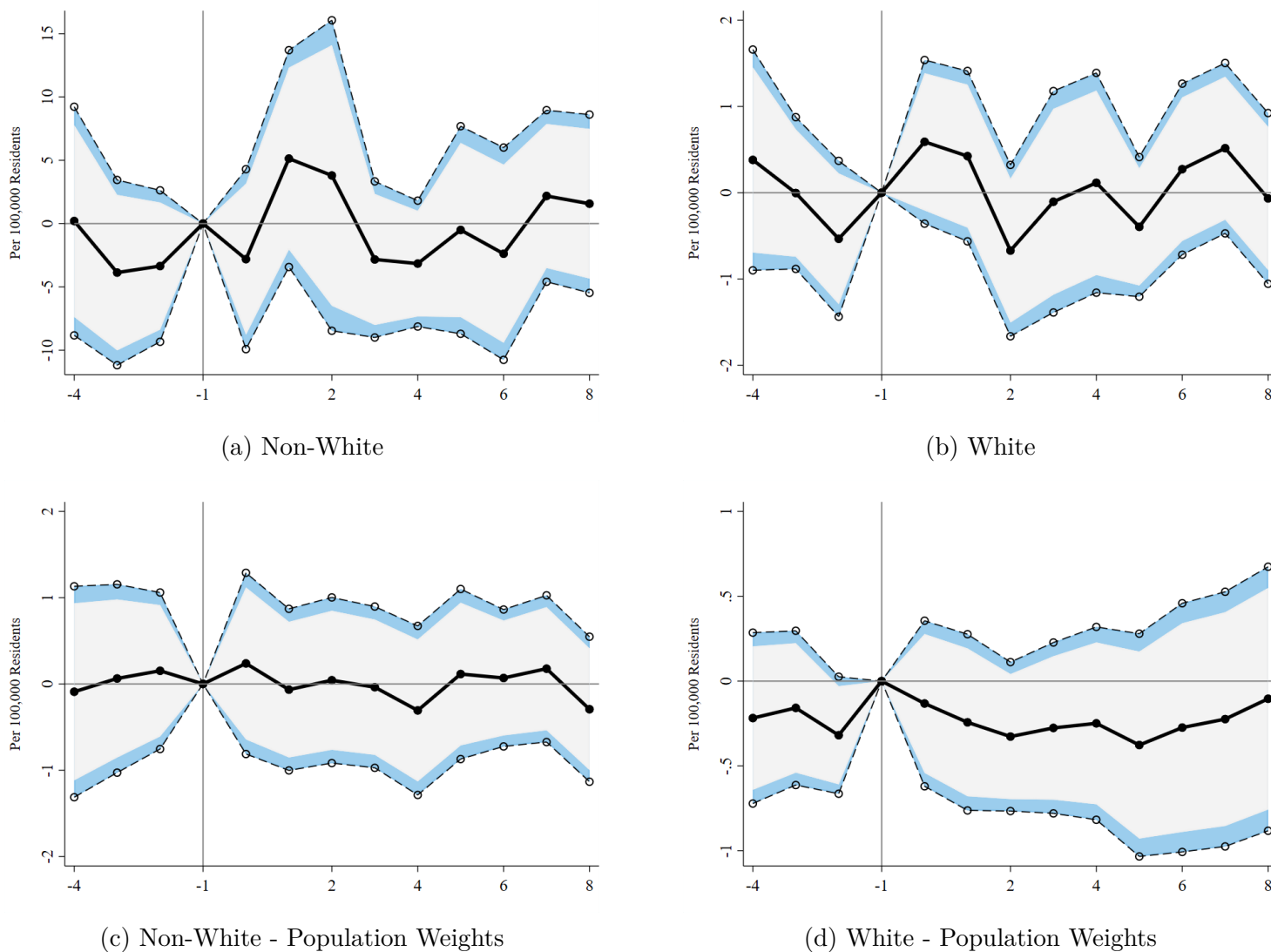
Notes: Figure displays ordinary least squares coefficients from equation 1. To create a balanced panel, we remove states that adopted the duty to bargain prior to 1964. Since we do not have population by race in 1959 we begin with event years 4 years prior to treatment. In panels (a) and (b), the dependent variable is number of suicides per 100,000 residents for non-white and white civilians, respectively. In panels (c) and (d), non-white and white population in 1960 are used as weights. The 95% and 90% confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by state, and are presented in blue and gray.

Figure A17: Duty to Bargain Event Study: Controlling for 1960s Riots
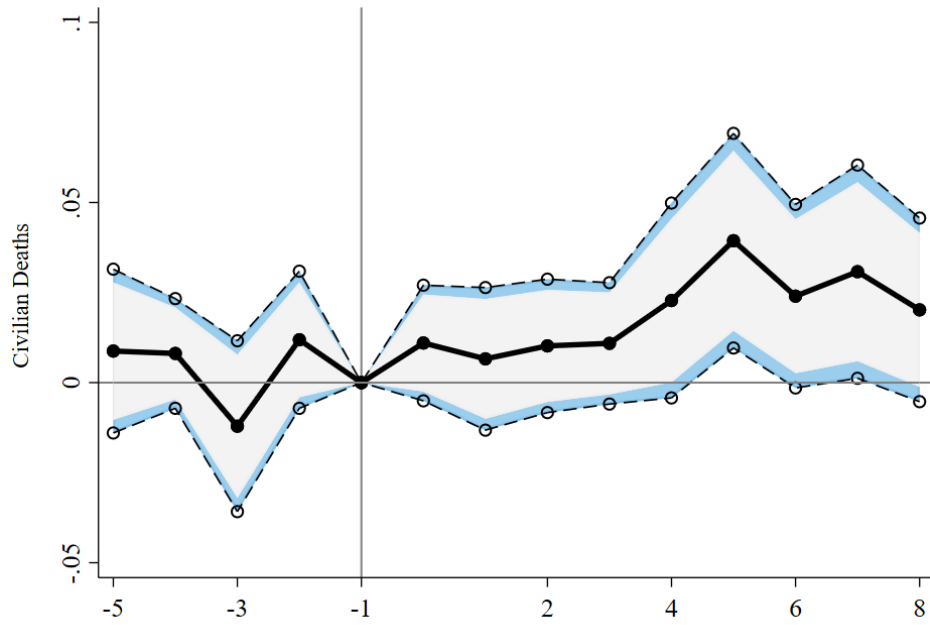
(a) Non-White
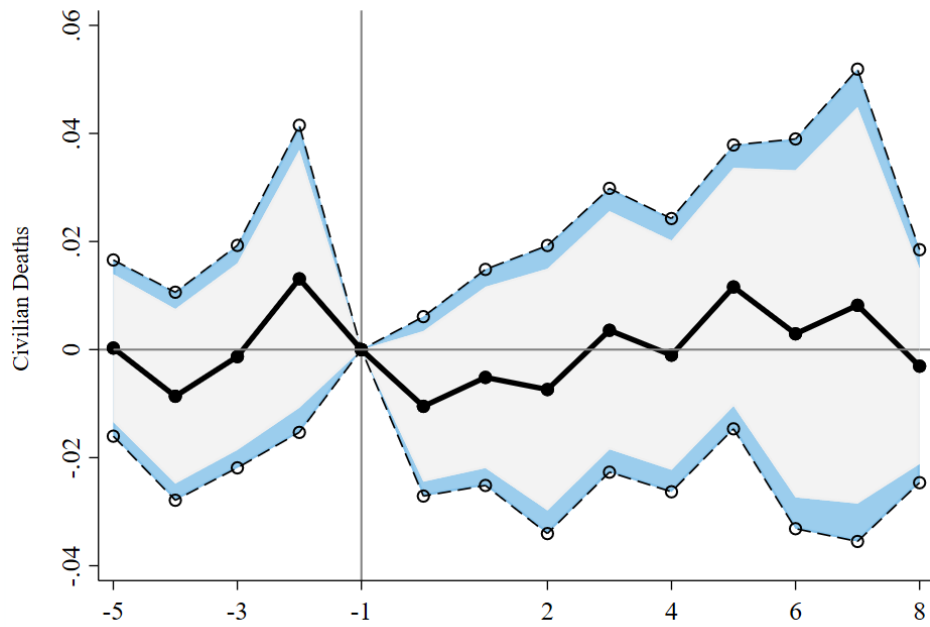
(b) White

Notes: Regression models includes event-year indicators for time before and after the first uprising occurs. The 95% and 90% confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by state, and are presented in blue and gray.

Figure A18: Relative Timing of Law Enforcement Officers' Bill of Rights Adoption with Duty to Bargain Adoption
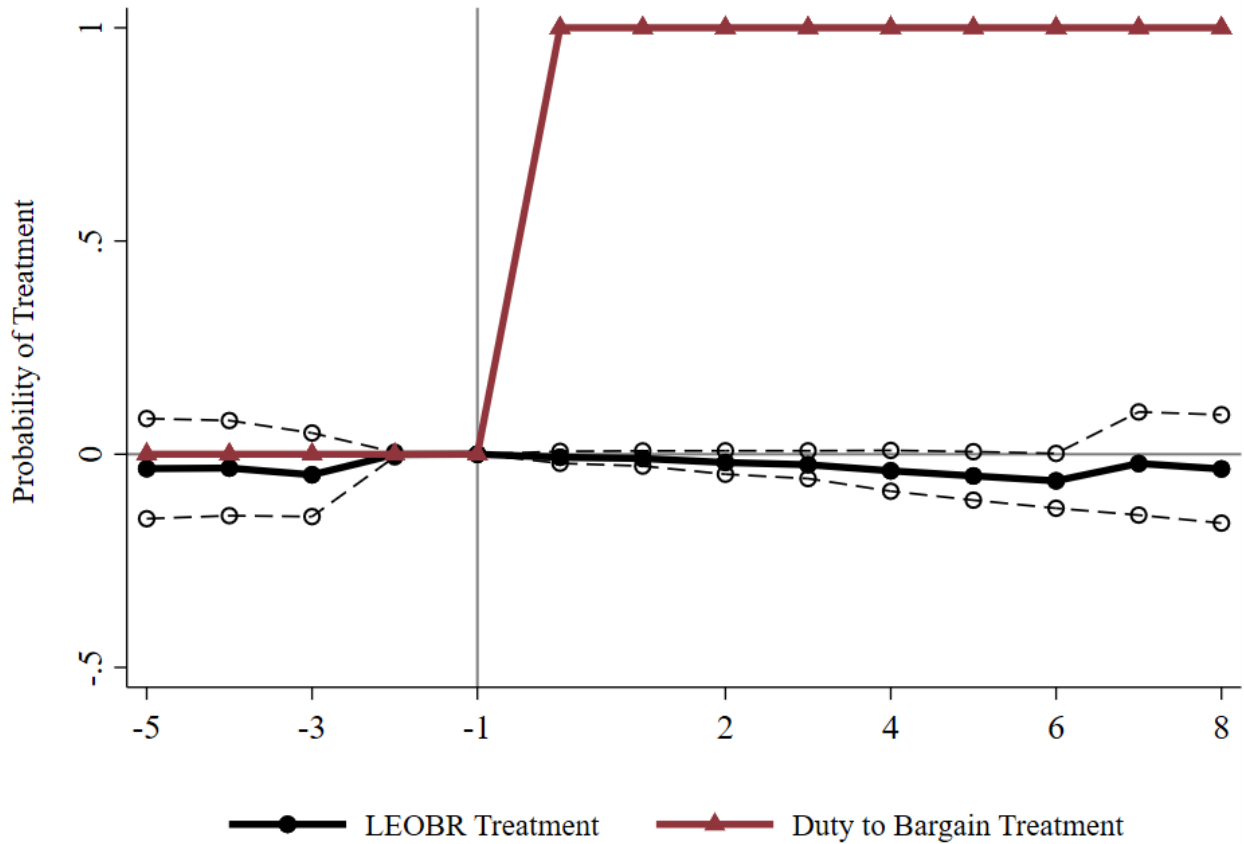
Notes: The dependent variable is an indicator variable for the adoption of collective bargaining rights with the duty to bargain (triangle marker) or the adoption of a LEOBR (circle marker).

Tables

Table A1: The Duty to Bargain vs Authorization to Bargain

	(1)	(2)	(3)	(4)
	Collective Bargaining Rights			
	Duty to Bargain		**Authorized to Bargain**	
	Non-White Deaths	White Deaths	Non-White Deaths	White Deaths
Years Before Treatment				
-5	0.0156	0.0159	-0.00826	-0.0163
	[0.0121]	[0.0162]	[0.0243]	[0.0131]
-4	0.0125	0.00427	-0.0150	-0.00926
	[0.00971]	[0.0186]	[0.0147]	[0.0102]
-3	-0.0118	-0.00256	-0.0211	-0.0204*
	[0.0126]	[0.00901]	[0.0225]	[0.0110]
-2	0.0119	0.0184	-0.0120	-0.0255***
	[0.00990]	[0.0150]	[0.0137]	[0.00888]
Years After Treatment				
0	0.0126	-0.00867	0.00280	0.00949
	[0.00833]	[0.00905]	[0.0118]	[0.0159]
1	0.00912	-0.00311	0.00354	0.00237
	[0.0102]	[0.0105]	[0.0116]	[0.0127]
2	0.0120	-0.00263	0.00501	-0.00263
	[0.0101]	[0.0145]	[0.00745]	[0.0122]
3	0.0171*	0.00863	0.000355	0.0103
	[0.00895]	[0.0138]	[0.00998]	[0.0117]
4	0.0309**	0.00633	0.0222	0.000641
	[0.0141]	[0.0149]	[0.0283]	[0.0108]
5	0.0474***	0.0124	-0.00517	0.00864
	[0.0136]	[0.0143]	[0.0144]	[0.00939]
6	0.0314**	0.00731	-0.0116	0.00658
	[0.0148]	[0.0204]	[0.0141]	[0.0139]
7	0.0374**	0.0140	0.00433	-0.00219
	[0.0164]	[0.0229]	[0.0195]	[0.0131]
8	0.0280*	0.00613	-0.00931	0.00506
	[0.0139]	[0.0129]	[0.0194]	[0.0115]
Observations	133,748	133,748	94,308	94,308
R-squared	0.014	0.022	0.026	0.023
Number of Counties	2,306	2,306	1,626	1,626

Notes: Table reports least-squares coefficients for collective bargaining rights, based on the provision granted by the state. The control group consists of states with no provision to bargain or where collective bargaining is prohibited. *** p<.01, ** p<.05, * p<0.1

Table A2: Duty to Bargain Event Study: Police Killings of Civilians by Race

	(1)	(2)	(3)	(4)	(5)
Panel A: Non-White Deaths					
Pre-Period (Event Years -5 to -2)	0.00508	0.00741	0.00824	0.00711	0.00851
	[0.00896]	[0.00835]	[0.00813]	[0.00885]	[0.00897]
Shorter-Run Effect (Event Years 0 to 2)	0.0100	0.0122*	0.0120	0.0101	0.0102
	[0.00763]	[0.00708]	[0.00721]	[0.00774]	[0.00774]
Medium-Run Effect (Event Years 3 to 5)	0.0294**	0.0313***	0.0311***	0.0294***	0.0293***
	[0.0114]	[0.0102]	[0.0107]	[0.0104]	[0.0106]
Longer-Run Effect (Event Years 6 to 8)	0.0339**	0.0357**	0.0358**	0.0317**	0.0315**
	[0.0167]	[0.0148]	[0.0148]	[0.0145]	[0.0142]
Observations	177,712	177,712	177,712	177,712	177,712
R-squared	0.002	0.013	0.079	0.014	0.081
Number of fips	3,064	3,064	3,064	3,064	3,064
Panel B: White Deaths					
Pre-Period (Event Years -5 to -2)	0.00444	0.00604	0.00299	0.00527	0.00301
	[0.0133]	[0.0135]	[0.0102]	[0.0120]	[0.00961]
Shorter-Run Effect (Event Years 0 to 2)	-0.00739	-0.00520	-0.00487	-0.00601	-0.00527
	[0.0102]	[0.0102]	[0.0100]	[0.00929]	[0.00912]
Medium-Run Effect (Event Years 3 to 5)	0.00856	0.00966	0.0104	0.00799	0.00919
	[0.0139]	[0.0134]	[0.0133]	[0.0123]	[0.0126]
Longer-Run Effect (Event Years 6 to 8)	0.00829	0.00758	0.00837	0.00638	0.00796
	[0.0191]	[0.0182]	[0.0174]	[0.0177]	[0.0172]
Observations	177,712	177,712	177,712	177,712	177,712
R-squared	0.005	0.012	0.068	0.021	0.075
Number of Counties	3,064	3,064	3,064	3,064	3,064
Year Fixed Effects	X				
Urban-by-Year Fixed Effects		X	X	X	
+Covariates			X		X
Region-by-Year Fixed Effects				X	X

Notes: Each column reports estimates from separate OLS regressions. All columns include state and year fixed effects. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<0.1

Table A3: Duty to Bargain Event Study: Crime and Employment Outcomes

	(1) Total Crime	(2) Violent Crime	(3) Murder	(4) Sworn Officers	(5) Officers Killed
Pre-Period (Event Years -5 to -2)	-0.0420	-0.0595	-0.0184	0.0339*	-0.00203
	[0.0384]	[0.0399]	[0.0208]	[0.0186]	[0.00775]
Shorter-Run Effect (Event Years 0 to 2)	-0.0204	-0.105*	-0.0281	-0.0506**	0.0135
	[0.0355]	[0.0571]	[0.0219]	[0.0228]	[0.0135]
Medium-Run Effect (Event Years 3 to 5)	0.00100	0.0140	-0.00368	-0.0927**	-0.0376
	[0.0839]	[0.0750]	[0.0314]	[0.0414]	[0.0525]
Longer-Run Effect (Event Years 6 to 8)	-0.0482	0.0807	0.00234	-0.140***	0.0172
	[0.101]	[0.0951]	[0.0417]	[0.0478]	[0.0178]
Observations	80,496	73,742	80,496	75,542	75,542
R-squared	0.516	0.619	0.078	0.573	0.005
Number of Counties	3,059	3,058	3,059	3,059	3,059

Notes: Each column reports estimates from separate OLS regressions. All columns include county, year, and urban-by-year fixed effects. Standard errors are clustered at the state level. *** $p<.01$, ** $p<.05$, * $p<0.1$

Table A4: Duty to Bargain Event Study: Joint Effects Robustness Checks

	(1) Original	(2) Balance Panel	(3) Year FE Only	(4) Covariates	(5) Drop CA & NY	(6) Early Adopters	(7) Late Adopters
Pre-Period (Event Years -5 to -2)	0.00711	0.00711	0.00508	0.00851	0.00848	-0.0133	0.0101
	[0.00885]	[0.00885]	[0.00896]	[0.00897]	[0.00767]	[0.0220]	[0.00997]
Shorter-Run Effect (Event Years 0 to 2)	0.0101	0.0101	0.01	0.0102	0.0116	0.0114	0.00741
	[0.00774]	[0.00774]	[0.00763]	[0.00774]	[0.00815]	[0.0260]	[0.00805]
Medium-Run Effect (Event Years 3 to 5)	0.0294***	0.0294***	0.0294**	0.0293***	0.0249**	0.0349	0.0234*
	[0.0104]	[0.0104]	[0.0114]	[0.0106]	[0.0104]	[0.0251]	[0.0118]
Longer-Run Effect (Event Years 6 to 8)	0.0317**	0.0317**	0.0339**	0.0315**	0.0237**	0.00774	0.0363**
	[0.0145]	[0.0145]	[0.0167]	[0.0142]	[0.0108]	[0.0363]	[0.0149]
Observations	177,712	177,712	177,712	177,712	170,984	112,404	159,616
R-squared	0.014	0.014	0.002	0.081	0.014	0.025	0.012
Number of Counties	3,064	3,064	3,064	3,064	2,948	1,938	2,752
Mean DV	0.0403	0.0403	0.0403	0.0403	0.0231	0.117	0.0197

Notes: Each column reports estimates from separate OLS regressions. All columns include county, region-by-year, and urban-by-year fixed effects. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<0.1

Table A5: Duty to Bargain Event Study: Robustness Checks

	(1) Original	(2) Balance Panel	(3) Year FE Only	(4) Covariates	(5) Drop CA & NY	(6) Early Adopters	(7) Late Adopters
Years Before Treatment							
-5	0.0156	0.0156	0.0122	0.0185	0.014	-0.015	0.0221
	[0.0126]	[0.0126]	[0.0139]	[0.0125]	[0.0120]	[0.0245]	[0.0154]
-4	0.0132	0.0132	0.00947	0.0156	0.0112	0.0138	0.00965
	[0.00915]	[0.00915]	[0.00895]	[0.00944]	[0.00725]	[0.0241]	[0.00888]
-3	-0.0123	-0.0123	-0.0117	-0.0119	-0.00278	-0.0347	-0.00849
	[0.0124]	[0.0124]	[0.0121]	[0.0125]	[0.0104]	[0.0400]	[0.0135]
-2	0.0131	0.0131	0.0115	0.0133	0.012	-0.0166	0.0185*
	[0.00979]	[0.00979]	[0.00805]	[0.00988]	[0.00832]	[0.0201]	[0.00980]
Years After Treatment							
0	0.0113	0.0113	0.00922	0.0116	0.0111	0.0196	0.00541
	[0.00849]	[0.00849]	[0.00857]	[0.00861]	[0.00922]	[0.0328]	[0.00810]
1	0.00744	0.00744	0.00725	0.00751	0.0106	0.00408	0.00605
	[0.00981]	[0.00981]	[0.00844]	[0.00975]	[0.0100]	[0.0336]	[0.00952]
2	0.0121	0.0121	0.0142	0.0122	0.0135	0.00877	0.0118
	[0.00934]	[0.00934]	[0.00874]	[0.00922]	[0.00948]	[0.0223]	[0.0103]
3	0.0148*	0.0148*	0.0135	0.0147*	0.0194**	0.00451	0.0151
	[0.00866]	[0.00866]	[0.00905]	[0.00836]	[0.00776]	[0.0344]	[0.00937]
4	0.0290**	0.0290**	0.0275*	0.0289**	0.0225	0.0242	0.0240**
	[0.0140]	[0.0140]	[0.0143]	[0.0140]	[0.0138]	[0.0456]	[0.0115]
5	0.0456***	0.0456***	0.0481***	0.0454***	0.0334**	0.0751***	0.0330*
	[0.0154]	[0.0154]	[0.0169]	[0.0158]	[0.0147]	[0.0262]	[0.0171]
6	0.0309**	0.0309**	0.0353**	0.0307**	0.0179	-0.00698	0.0372**
	[0.0153]	[0.0153]	[0.0164]	[0.0152]	[0.0108]	[0.0285]	[0.0171]
7	0.0383**	0.0383**	0.0416**	0.0381**	0.0329**	0.0307	0.0399**
	[0.0169]	[0.0169]	[0.0190]	[0.0165]	[0.0141]	[0.0534]	[0.0150]
8	0.0279*	0.0279*	0.0261	0.0277*	0.0215**	0.00161	0.0343**
	[0.0143]	[0.0143]	[0.0164]	[0.0140]	[0.0106]	[0.0354]	[0.0154]
Observations	177,712	177,712	177,712	177,712	170,984	112,404	159,616
R-squared	0.014	0.014	0.002	0.081	0.014	0.025	0.012
Number of Counties	3,064	3,064	3,064	3,064	2,948	1,938	2,752
Mean DV	0.0403	0.0403	0.0403	0.0403	0.0231	0.117	0.0197

Notes: Each column reports estimates from separate OLS regressions. All columns include county, region-by-year, and urban-by-year fixed effects. Standard errors are clustered at the state level. *** p<.01, ** p<.05, * p<0.1

Table A6: Law Enforcement Officers' Bill of Rights Event Study

	(1) LEOBR Timing	(2) CBR Timing	(3) CBR +LEOBR	(4) LEOBR Only	(5) LEOBR +CBR
Years Before Treatment					
-5	-0.0338	0***	0.0133	0.0199	0.0173
	[0.0599]	[0]	[0.0114]	[0.0244]	[0.0258]
-4	-0.0325	0***	0.0127	-0.00213	-0.00387
	[0.0569]	[0]	[0.00888]	[0.0112]	[0.0126]
-3	-0.0482	0***	-0.0117	0.0213	0.0177
	[0.0500]	[0]	[0.0121]	[0.0136]	[0.0137]
-2	-0.000776	0***	0.0126	-0.0188	-0.0230
	[0.00270]	[0]	[0.00906]	[0.0122]	[0.0138]
Years After Treatment					
0	-0.00723	1***	0.0122	0.0132	0.00986
	[0.00714]	[0]	[0.00862]	[0.0177]	[0.0180]
1	-0.0100	1***	0.00857	0.00876	0.00904
	[0.00905]	[0]	[0.00994]	[0.0276]	[0.0273]
2	-0.0194	1***	0.0125	-0.0104	-0.0121
	[0.0140]	[0]	[0.00946]	[0.0168]	[0.0177]
3	-0.0246	1***	0.0162*	-0.00390	-0.00395
	[0.0166]	[0]	[0.00908]	[0.0115]	[0.0118]
4	-0.0389	1***	0.0277*	-0.00638	-0.00753
	[0.0244]	[0]	[0.0140]	[0.0171]	[0.0165]
5	-0.0509*	1***	0.0469***	-0.00229	-0.00302
	[0.0290]	[0]	[0.0155]	[0.0160]	[0.0167]
6	-0.0624*	1***	0.0317**	0.0278	0.0231
	[0.0328]	[0]	[0.0151]	[0.0195]	[0.0226]
7	-0.0218	1***	0.0381**	0.0266*	0.0181
	[0.0617]	[0]	[0.0166]	[0.0133]	[0.0144]
8	-0.0345	1***	0.0283**	0.0106	0.00482
	[0.0648]	[0]	[0.0138]	[0.0152]	[0.0165]
Observations	91,920	91,920	91,920	91,920	91,920
R-squared	0.126	1.000	0.016	0.016	0.016
Number of Counties	3,064	3,064	3,064	3,064	3,064

Notes: Column 1 - dependent variable is 0/1 adoption of Officers Bill of Rights. Column 2 - dependent variable is 0/1 adoption of collective bargaining rights. Columns 3 & 5 plot coefficients from a regression that includes event years before and after the adoption of collective bargaining rights and LEOBRs. Column 3 plots treatment effects for collective bargaining rights while column 5 display estimates for LEOBRs. Column 4 reports estimates of LEOBRs on non-white civilian deaths not accounting for collective bargaining rights. *** p<.01, ** p<.05, * p<0.1