

5-1-2023

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### Recommended Citation

Makowsky, Michael D. and Warren, Patrick L. (2023) "Firearms and Lynching," *Journal of Law and Economics*: Vol. 66: No. 2, Article 2.

Available at: <https://chicagounbound.uchicago.edu/jle/vol66/iss2/2>

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# Firearms and Lynching

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## Abstract

We assess firearms as a means of Black residents' self-defense in the Jim Crow South. We infer access to firearms by race and place by measuring the fraction of suicides committed with a firearm. Corroborating anecdotal accounts and historical claims, state bans on pistols and increases in White law enforcement personnel served as mechanisms to disarm the Black community, while having no comparable effect on White residents' firearm access. The interaction of these mechanisms with changing national market prices for firearms provides a credible identification strategy for Black residents' firearm access. Rates of Black residents' lynching decreased with their greater access to firearms.

I had already determined to sell my life as dearly as possible if attacked. I felt if I could take one lyncher with me, this would even up the score a little bit. (Wells 1970, p. 62)

## 1. Introduction

In this paper, we investigate the relationship between access to firearms and racialized violence in the Jim Crow South, where Black residents were subject to state and local governments that were rarely better than indifferent to their safety and, at their worst, actively supportive of terrorist violence targeting them (Adler 2019; Johnson 2014; Wright 1996). Whether firearms served to aid Black residents in defending themselves in the Jim Crow South is an open question, both in its narrow application to African American history and its broader relevance to firearms policy. At the same time, we document the impact of facially race-neutral but practically discriminatory policies on Black residents' versus White residents' firearm access in this period.

The impact of firearm access on violence has proved challenging to adjudicate. There are difficulties in both measuring access and inferring the direction

We thank Taylor Schenker for her work in helping us input data. We also thank Jeffrey Adler, Howard Bodenhorn, Karen Clay, William Dougan, Richard Hornbeck, Shawn Kantor, Carl Kitchens, Martha Olney, Omer Ozak, Jared Rubin, Alex Tabarrok, Robert Tamura, Evan Taylor, Jhacova Williams, and participants in the Purdue Economics Department and Virtual History Seminars for useful comments and advice.

[*Journal of Law and Economics*, vol. 66 (May 2023)]

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of any causal relationship with violence (Duggan 2001; Kleck 2004, 2015; Manski and Pepper 2018). The costs and benefits of private firearm ownership have been assessed in myriad research contexts, including their relationship to deterrence of crime (Duggan 2001; Kleck 2015; Lang 2016; Manski and Pepper 2018), self-defense (Cheng and Hoekstra 2013; Kovandzic, Schaffer, and Kleck 2013; McClellan and Tekin 2017), homicide and suicide rates (Edwards et al. 2018; Riddell et al. 2018; Siegel, Ross, and King 2013), and broadly defined social costs (Cook and Ludwig 2006).

These quantities have been estimated in the context of contemporary justice systems, but the impact of firearms on individual and public safety depends on the institutional and historical context, particularly when considering arguments that individual rights to firearm ownership can serve as a bulwark against a tyrannical government. At the same time, the historic and continuing consequences of unchecked racial violence are difficult to overstate (Beck and Tolnay 1992; Cook 2014; Cook, Logan, and Parman 2018a; Jones, Troesken, and Walsh 2017; King, Messner, and Baller 2009; Messner, Baller, and Zevenbergen 2005; Williams 2022), and the relationship between violence and firearm ownership might be very different in a regime where the formal institutions of law are not equally available to a substantial share of the population. Physical safety was in far greater question in the recent past, however, particularly for Black southerners (Adler 2008). Self-defense effects may be difficult to ascertain because of omitted-variable bias, and the relatively safe conditions of the modern developed world might render self-defense effects too small to precisely identify. Self-defense effects may have been far larger for Black residents of the Jim Crow South than in the present day.

Historical measures of firearm ownership or access have proved difficult to obtain (Brennan, Lizotte, and McDowall 1993), especially in periods when groups might have had their ownership restricted by law, practice, or broader social norms. Surveys, for instance, are unreliable if firearm ownership is restricted, particularly where restrictions are strongly or unevenly applied. A proxy measure of ownership for which disclosure of ownership is not endogenous to political power or social standing is needed. The fraction of suicides committed with a firearm can serve as such a proxy. The percentage of suicides committed with a firearm, compared with a variety of other broadly available proxies, has been repeatedly found to be the best cross-sectional measure of firearm ownership rates (Cerqueira et al. 2019; Cook 1991; Cook and Ludwig 2006, 2019). Recent research has continued to apply and validate this measure as applied to a variety of data and contexts since the 1970s (Azrael, Cook, and Miller 2004; Cook 1983; Briggs and Tabarrok 2014; Hemenway and Miller 2000; Miller, Azrael, and Hemenway 2002; Nagin 2020), including in panel settings (Cerqueira et al. 2019).<sup>1</sup>

<sup>1</sup> Kleck (2004, p. 17) claims that the percentage of suicides committed with a firearm (PSF) is “virtually perfect” as a cross-sectional proxy but fails as a cross-temporal proxy. This claim, however, is based on the Pearson correlation coefficient between PSFs across years in the General Social Survey, without controlling for the cross-sectional variation across states. Once cross-sectional variation is included in the estimation (in our case, as a within-state estimation), PSF performs far better as a cross-temporal proxy (Cerqueira et al. 2019; Cook and Ludwig 2006, 2002).

The fraction of suicides with a firearm is a particularly attractive proxy for firearm access in our setting. It is available, by race and geography, from the early 20th century.<sup>2</sup> Our data allow us to measure White residents' and Black residents' firearm access and violent deaths between 1913 and 1999. No other proxy of firearm access is available for a panel nearly that long. It is a grim fact that those who kill themselves have, in large part, placed themselves beyond the threat of further consequences meted out by the government, which removes a potential source of bias. The preferences of the families, friends, or communities of those who commit suicide, and the sensitivity of authorities to their preferences, may vary by race. This could bias the fraction of events recorded as suicides, but it seems unlikely that this would bias the recorded method by which a suicide was accomplished, that is, whether a firearm was employed. Thus, compared with other proxies for firearm access, differential reporting as a function of firearm restrictions presents a relatively modest concern.

We use hand-coded data from 1913 to 1950 to separately calculate the percentage of suicides committed with a firearm for White and Black individuals in each state that reported the necessary vital statistics. We first analyze the correlation between firearm access and the number of historiographically identified Black residents' lynching deaths (Beck and Tolnay 2019) and show that in states and years in which Black residents had better access to firearms, they suffered fewer lynchings. This correlation opens the door to the possibility of firearms serving an important role in self-defense in a poorly institutionalized state.

But even with new reliable measures of firearm access, caution is still warranted in interpreting conditional correlations as unbiased causal relationships.<sup>3</sup> Reverse causality is an important concern when analyzing the relationship between firearms and violence—violent victimization may lead individuals to seek out firearms, which biases the observed effect of firearms upward. In this historical setting, there is also the potential for additional omitted-variable bias in the other direction—states in which firearm access by Black residents is particularly restricted because of racially motivated policy may also be states with otherwise higher levels of violence against their Black citizens. Of course, attenuation is also a concern, as our measure of firearm access is a noisy proxy for true access rates.

To address these potential biases, we use instrumental variables (IV) in a control-function approach, with two very different shifts in the availability of firearms—one based on White law enforcement manpower as a shift of the costs of maintaining access to a firearm for Black residents and one based on state laws that affected the availability of firearms, both interacted with a hand-coded set of national firearm prices. These IV results allow us to both correct for the bias

<sup>2</sup> Vital statistics from the early 20th century through the period of the civil rights movement identify individuals as White or "colored," the latter referring to all non-White individuals. Given our emphasis on former Confederate states, the overwhelming majority of non-White individuals in our sample are African American former slaves or their descendants, to whom we uniformly refer as "Black" throughout this paper.

<sup>3</sup> In a survey of 41 papers examining the effect of firearm ownership on crime, Kleck (2015) finds three papers that employed a causal inference strategy to address endogeneity.

in the basic results and sign that bias. Both approaches deliver the same results. First, they shift firearm access in the expected way. More White law enforcement officers and the enactment of (anecdotally racially motivated and enforced) handgun-access restrictions in low-price environments reduce Black residents' firearm access. Second, states and years in which Black citizens have lower rates of firearm access because of these shifts have significantly higher lynching rates. A 1-standard-deviation reduction in the proxy for Black residents' firearm access reduces Black residents' lynchings by about 1.4 per year, which is about 65 percent of a standard deviation.

These results also reveal something about the mechanisms through which states in the Jim Crow South could target Black residents. Law enforcement budgets and pistol-possession restrictions could at least be plausibly neutral on their face with respect to race. The intent and eventual consequence of these laws, however, was the disarmament of Black communities, which increased their vulnerability to racial violence while leaving White residents' access to firearms entirely unchanged. When considering the merits of the right to bear arms, and limitations on those rights, equity of application and enforcement should be a first-order concern.

In the decades preceding the Civil War, southern states passed a variety of limitations on the rights of both free Black individuals and slaves to own or use firearms (Cottrol and Diamond 1991; Tahmassebi 1991). This approach continued after the Civil War, albeit with greater dependence on uneven de facto enforcement to produce the intended outcome (Cottrol and Diamond 1995). Black residents' disarmament was of primary importance to White southerners during Reconstruction and was heavily featured in the Black codes (Cottrol and Diamond 1991; Burkett 2008; Cramer 1994).

If the first question is whether Black households were disarmed by Jim Crow, then the natural, and perhaps more important, next question is whether access to firearms mattered. In *United States v. Cruikshank* (92 U.S. 542 [1875]), the Supreme Court held that the federal government had no authority to punish members of the Ku Klux Klan for confiscating the firearms of two Black men, which set a precedent that effectively delegated the defense of constitutional rights, including the rights to peaceably assemble and bear arms, to state governments. For Black residents of the Jim Crow South, the message was clear: they were on their own.

In their discussion of the role of firearms in African American history, Cottrol and Diamond (1991) recount numerous incidents of Black citizens forming armed ad hoc militias to deter lynchings. Ossian Sweet's 1925 armed stand in his Detroit home against a violent White mob and subsequent legal plea of self-defense sparked an outpouring of pride and proved to be a seminal moment in the call to armed self-defense in Black newspapers across the country (Boyle 2007; Johnson 2014). In recounting his experience as an attorney serving the civil rights movement, Donald B. Kates notes the broad endorsement of firearm possession in the movement and "attributed the relative quiescence of the Klan to

the fact that the Black community was so heavily armed” (Kates 1979, p. 186). Cobb (2014), from his point of view as a former field secretary for the Student Nonviolent Coordinating Committee in Mississippi, makes a compelling case that the acceptance of armed self-defense was an existential necessity for activists throughout the broader movement.

Beyond their function as terrorism, lynchings also illustrate a fundamental lack of access to protection from the state. Describing the situation facing the Black residents of a post-Reconstruction New Orleans, Adler (2019, p. 25) asserts that “violence was so endemic during the early 1920s, and legal institutions were so indifferent toward such crime, especially African-American intraracial homicide, that self-help, even violent self-help, became a survival mechanism.” Intraracial violence, however, was not the only threat. In early 20th-century New Orleans, Adler (2008) finds that about a quarter of White killers targeted Black residents, while a tenth of Black killers targeted White residents.

## 2. Data and Sample Statistics

Our data on causes of death are from tables compiled in the *Mortality Statistics Annual Report* by the Bureau of the Census. These statistics were gathered from reports submitted by physicians and coroners from an increasing number of registration states. By 1910, over half the population of the United States resided in a registration state, and the first former Confederate state (North Carolina) began reporting. By 1920, over 80 percent of the population was in a registration state, including seven former Confederate states that reported statistics broken down by race. In 1928, all Confederate states reported but Texas, which joined in 1933. States without substantial Black populations did not report by race in early years, but beginning in 1937 all states (and the District of Columbia) began fully reporting by race. We coded data by hand from the reports on cause of death by race and state from 1910 to 1950 (see Table 1).

The categorization schema for cause of death changes numerous times, but our key variables track consistently: deaths by homicide, accident, and suicide, both overall and from firearms.<sup>4</sup> Other than a gap from 1946 to 1948 for all states and a year (1945) for Arkansas during which firearm-related suicides were not separately reported, these cause-of-death outcomes are consistently reported for all former Confederate states once they enter the panel.

### 2.1. Estimates of Firearm Ownership, 1913–50

From these vital statistics, we calculate the key independent variable—our metric of firearm possession, by race, which is the fraction of suicides that are the result of self-inflicted firearm injuries. This statistic has been shown to be a good proxy for household firearm possession across cities (Cook 1983), states (Miller, Azrael, and Hemenway 2002), and countries (Hemenway and Miller 2000; Killias

<sup>4</sup> In some early years, firearm deaths are pooled with deaths from explosives. In later years, when explosives deaths are broken out, they are quite rare.

Table 1  
Summary Statistics for Former Confederate States, 1913–50

	Mean	SD	Min	Max	N
Lynchings of Black residents	1.22	2.20	.00	13.00	317
Black residents' suicides	17.03	7.69	3.00	56.00	317
Percentage of Black residents' suicides with firearm	.56	.16	.00	317	
White residents' suicides	167.08	82.04	24.00	482.00	317
Percentage of White residents' suicides with firearm	.59	.09	.18	.80	317
Instrumental variables:					
Pistol ban	.19	.39	.00	1.00	277
Firearm licensing	.21	.41	.00	1.00	277
Shotgun price (real dollars)	125.18	35.74	74.50	207.26	277
White police per 1,000 Black residents	2.42	1.52	.25	7.75	317

**Note.** Observation counts are smaller so that variables can be interacted with shotgun prices, which are not reliably available because of metal shortages and associated rationing in 1942–45.

1993) and within states over time (Azrael, Cook, and Miller 2004; Cerqueira et al. 2019).

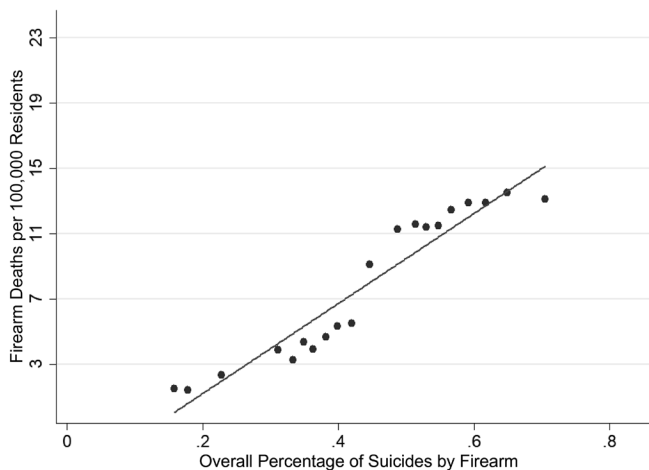
While the percentage of suicides by firearm has proven to be the most reliable proxy for firearm access in contemporary contexts, it remains untested for the past. As a validation exercise, Figure 1 presents binned scatterplots and linear estimates of the relationship between our proxy,  $PSF_{st}$ , and the per capita rate of all nonsuicide deaths involving a firearm. These statistics are calculated for the entire population and are highly correlated.<sup>5</sup> If places with more firearms have more firearm-related deaths, as seems natural, this provides some validation for our proxy for firearm access.

The capture of the institutions of justice by one race might lead to differential miscoding of homicides as suicides or accidents. If this miscoding differentially occurs for firearm deaths, it could affect this proxy for firearm access. In particular, if places and times with institutions that are more captured have more miscodings of Black residents' firearm homicides as suicides, we will overstate the rate of Black residents' firearm access in those places and times. Of course, if many Black residents' nonfirearm homicides are miscoded as suicides, we will understate Black residents' firearm access. Even if there is unbiased reporting, this proxy will always suffer from significant measurement error, as idiosyncratic shocks and random chance for a small number of observed suicides introduce noise. Either form of measurement error provides a good reason to pursue an IV strategy for impact estimates.

Figure 2 displays a time series of  $PSF_{st}$  for White residents' and Black residents' suicides, with locally linear population-weighted mean percentages for all former Confederate states bounded by 95 percent confidence intervals. States are included in the mean as they enter the sample.

The time series in Figure 2 and the history presented in Section 1 suggest that former Confederate states had some success in disarming Black residents in the

<sup>5</sup> The underlying bivariate regression is  $\beta(PSF^{All}) = 27.5 (1.04); p < .01, R^2 = .47$ .



**Figure 1.** Relationship between suicides by firearm and nonsuicide firearm deaths per capita, 1910–50.

Jim Crow era (for maps, see Figure OA4 in the Online Appendix). Prior to 1920, Black residents' firearm access outpaced White residents' access. That ordering was reestablished after World War II, but from 1920 to 1940 White residents' firearm access increased substantially alongside a decline in Black residents' firearm access. The increase in White residents' firearm access is, perhaps, a product of soldiers returning home from World War I with weapons or increased experience with and interest in firearms. The coinciding decrease in Black residents' firearm possession suggests that Jim Crow-era efforts to disarm Black people were effective, but this gap closed by 1940.

## 2.2. Lynching

Records of lynching are the product of decades of arduous research (Bailey and Tolnay 2015; Beck and Tolnay 2019, 1990). Using quantitative and qualitative rubrics for designating a murder as a lynching, the count of lynchings in a state in a given year is as much a barometer of the ambient level of violence leveled at Black citizens every day as it is a historical measure of terrorist events (Tolnay and Beck 1995).<sup>6</sup>

Our analysis of the lynching records from the Beck and Tolnay (2019) lynching data uses a sample of 335 state-years from former states of the Confederacy for 1913–50.<sup>7</sup> Figure 3 displays a time series of Black residents' lynchings in for-

<sup>6</sup> Lynchings are correlated with segregation (Cook et al. 2018a, 2018b), Black residents' migration (Tolnay and Beck 1992a), the size of the Black population (Tolnay and Beck 1992b; Christian 2017), cotton prices (Beck and Tolnay 1990), and local politics (Beck, Tolnay, and Bailey 2016).

<sup>7</sup> The Beck and Tolnay (2019) database does not include Black residents' lynchings from Texas, so we exclude the state from our analysis.



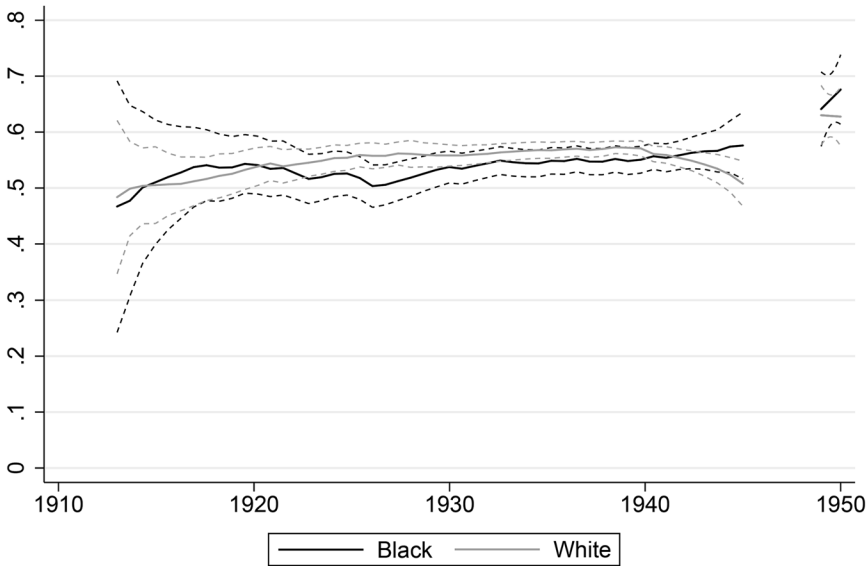


Figure 2. Trends in suicides by firearm

mer Confederate states. The sample includes a mean of 2.16 lynching deaths per year, with 41 percent of state-years experiencing at least one lynching death and a maximum of 13 in Georgia in 1922.<sup>8</sup> Lynching deaths per state capita steadily decrease through the window, with upticks in 1919 and 1933. For more on lynching rates in the South, see Beck and Tolnay (1990).

### 2.3. Covariate Controls and Excluded Instruments

Our covariate controls include the logged estimated population and the percentage of the population that is Black recorded in the census, linearly interpolated within decades. All regressions include log mean wages for Black and White residents as reported to or imputed from the US census (Ruggles et al. 2021) and interpolated between census years. For 1910–40, we impute them using the share of marginal output received by workers and reported decennially by state and industry in Turner et al. (2007) and Tamura, Simon, and Murphy (2016).<sup>9</sup> To control for the availability of services to the Black community that are relevant to violent and accidental death outcomes, we include the number of Black firefighters and Black physicians per capita of Black residents reported in each decennial cen-

<sup>8</sup> There are 21 recorded lynchings in Georgia in 1919, but we do not have sufficient vital statistics data for Georgia for inclusion in the analysis until 1922.

<sup>9</sup> We calculate the real ratio of mean earnings reported in the 1940 census by state, race, and industry to nominal output per worker by state and industry. We then apply this ratio to the nominal output per worker in each decennial census between 1910 and 1940, backing out imputed average wages of Black and White residents, linearly interpolating mean wages in the intervening years. A similar procedure was employed in Choquette (2020).

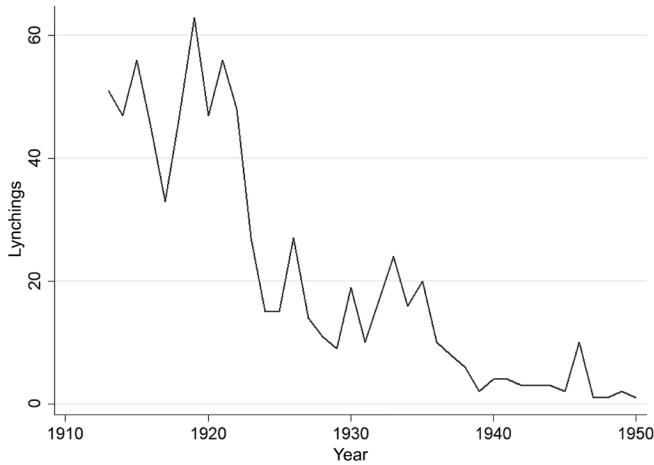


Figure 3. Trends in lynchings of Black residents

sus, linearly interpolated within decades. We also include the percentage of the Black residents in a state that were farmers given that during that era a shotgun was considered an occupational necessity. Because of their reliance on imputed wage estimates based on worker output, all regressions using data prior to 1940 also include estimates of physical capital stocks in the farming, manufacturing, and service industries (Tamura, Simon, and Murphy 2016; Turner et al. 2007). All control variables are summarized in Table OA2.

To implement our control-function approach (see Section 3.2), we use three IV identification strategies. The first uses the interaction of state bans of pistol sales and the price of firearms as a measure of the impact of de facto disarmament policy mechanisms. In the analytic sample, four of 10 Confederate states had bans on pistol sales and carrying in effect (Spitzer 2015; Frassetto 2013) for a portion of the years observed (Figure 4). The bans always reduced Black residents' firearm access in the sample, but the observed magnitudes are conditional on the price of firearms. Sales bans have a smaller net effect when higher prices have already reduced firearm demand. Similarly, price effects can be expected to be weaker when lower-cost options are prohibited, which limits purchases to goods targeting consumers with less elastic demand. The data on policy implementation are from reviews of state firearm laws in Warner (1938), Spitzer (2017), and Frassetto (2013).<sup>10</sup>

Using data collected by hand from archived Sears, Roebuck catalogs, we recorded the lowest price of a 12-gauge double-barrel shotgun each year between

<sup>10</sup> The relevant laws banning the sales of pistols include S.C. Code, sec. 1255 (1932); Ark. Dig. Stat., sec. 3509; Tenn. Code Ann., sec. 11009; and 1931 Fla. Laws 2069, sec. 7; (see Warner 1938). Arkansas, Tennessee, and South Carolina banned the carrying and sale of pistols. Florida allowed cities to ban pistols in 1927, and the salience of that capacity grew in importance when the law was extended to unincorporated villages in 1931.

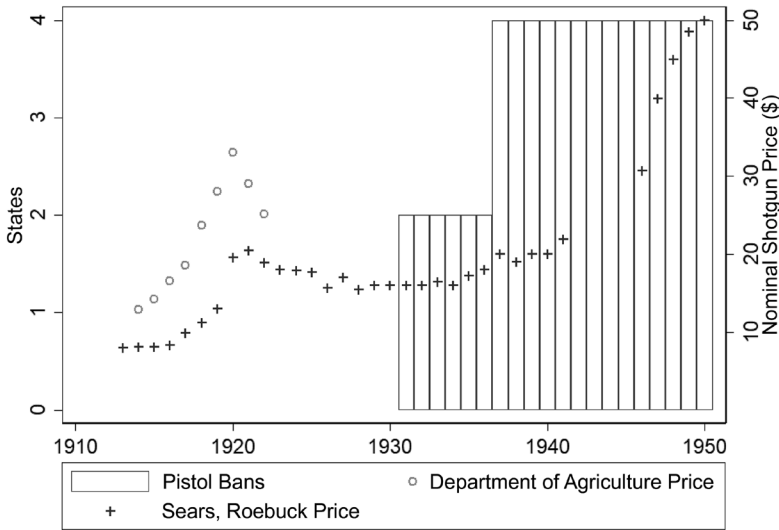


Figure 4. Shotgun prices and states with pistol bans

1913 and 1968, validating the prices, when possible, using records from the US Department of Agriculture’s yearbooks. Shotgun prices varied considerably year to year, as presented in Figure 4.<sup>11</sup> We include an indicator for pistol bans and its interaction with firearm prices in the first-stage estimation but restrict the interaction term in the second stage out of concern that the introduction of pistol bans is correlated with the number of lynchings.<sup>12</sup> Omitted factors that might be correlated with firearm prices, the adoption of gun restrictions, and lynching should be jointly captured by the direct effects. With these direct effects in place, the interaction of national prices and local policies should be an exogenous shift in firearm access.

The second identification strategy uses the number of White law enforcement officers per 1,000 Black residents,  $Law_{st}^{white}$ , measured as the number of police officers, sheriffs, sheriffs’ deputies, and constables reported in the decennial census between 1910 and 1950 (intervening years are linearly interpolated). While numerous laws granted local law enforcement the discretion to confiscate firearms disproportionately from Black residents, actual confiscation requires sufficient manpower to accomplish the task—ambitions of disarming Black individuals were conditional on local state capacity. Furthermore, while White law enforcement officers did participate in lynchings, their most common complicity was to not intervene in any way. The ability of law enforcement to ignore violence

<sup>11</sup> Nominal shotgun prices are from the US Department of Agriculture and the Sears, Roebuck catalog for 1913–50. Prices are not reliably available during the peak years of US involvement in World War II (1942–45).

<sup>12</sup> Estimates that include the pistol ban indicator as a restricted variable are qualitatively unchanged ( $p < .01$ ) from the reported results.

against Black men and women should not correlate with their labor force size, as doing nothing is rarely labor intensive. Figure OA3 displays the average value of this metric over time for former Confederate states.<sup>13</sup>

The third identification strategy interacts the number of White law enforcement officers per 1,000 Black residents with the price of firearms. As in the strategy with pistol bans, this approach allows us to identify the effects of firearm access by isolating the effects of state capacity for disarmament from the potential omitted-variable bias in law enforcement prioritization. The result of this alternative strategy also serves as a robustness check for law enforcement as a stand-alone instrument and test of the exclusion restriction—if the stand-alone and interaction terms produce similar estimates in the second stage, that will allow for greater confidence in both estimation strategies.

### 3. Empirical Models of Black Citizens' Firearm Access and Lynchings

Insight into the value of firearms to Black men and women living in the Jim Crow South presents several challenges to producing high-quality estimates of the impact of firearm access on violent deaths. In addition to challenges of measuring firearm access, which we address with proxies, our estimation specification and identification strategy must also address nonlinear outcome variables and the potential for omitted-variable bias and reverse causality.

#### 3.1. State Panel Regressions

Black residents' lynching deaths are count data characterized by significant overdispersion, with no recorded Black lynching victims accounting for more than half of the state-year observations in the sample (51.6 percent). Following Wooldridge (1999), in this panel setting we estimate fixed-effect Poisson regression models of state-year panel lynching counts. Given the significant overdispersion of the data, we also include conditional negative binomial regressions for reference and comparison. As Wooldridge points out, these estimates depend on a number of fairly strong assumptions. While overdispersion may be inflating the standard errors of our Poisson estimation, we build our core estimation of lynching outcomes using the Poisson estimation with two-way fixed effects.

We first estimate models in which lynching counts in state  $s$  in year  $t$ ,  $y_{st}$ , are distributed according to a Poisson function  $F(\mu_{st})$ :

$$\ln(\mu_{st}) = \alpha + \beta_1 \text{PSF}_{st} + \beta_3 \mathbf{X}_{st} + \gamma_s^0 + \gamma_s t + \delta_t, \quad (1)$$

where  $\text{PSF}_{st}$  is the race-specific percentage of suicides with a firearm as a proxy for firearm access,  $\mathbf{X}_{st}$  is a vector of control variables,  $\gamma_s^0$  is a group of state-specific intercepts,  $\gamma_s$  is a group of state-specific time trends, and  $\delta_t$  is a group of year-

<sup>13</sup> As a caution against possible reverse causality in communities that experienced the violent theater of lynchings, we estimated otherwise identical specifications using 1- and 2-year lagged measures of  $\text{Law}_{st}^{\text{White}}$  (not reported). These estimates produced comparable, statistically significant results.

specific intercepts. To accommodate the unavailability of data in the 3 years following World War II,  $PSF_{st}$  is carried back from 1950 to account for 1946–48.<sup>14</sup>

Both Poisson and negative binomial regressions are scaled by the size of the Black population in the state and year. The vector  $\mathbf{X}$  includes measures of within-state and within-year variation in economic conditions and demographics. To account for time-invariant state characteristics, particularly differing state cultures of racial acrimony, and broad national variation across time, all specifications include state and year fixed effects and state-specific time trends.

### 3.2. Instrumental Variables: Poisson Control-Function Estimates

To address the potential for bias in the estimates, we implement an IV strategy using a control-function approach. In the first stage, we estimate a two-way fixed-effects ordinary least squares model of Black residents' firearm access on year ( $\delta_{1t}$ ) fixed effects, state-specific interceptions and year trends ( $\gamma_{s1}^0 + \gamma_{s1}t$ ), covariate controls ( $\mathbf{X}_{st}$ ), and at least one restricted variable ( $R_{st}$ ). In the second stage, for lynchings, we estimate a Poisson two-way fixed-effects model of the count of lynchings over the same set of control variables, the endogenous variable of interest ( $PSF_{st}^{Black}$ ), and the estimated first-stage residual ( $\hat{\varepsilon}_{st1}$ ) as a control function (Lin and Wooldridge 2019). Formally, our joint model is given by variants of

$$PSF_{st}^{Black} = \lambda_0 + \lambda_1 R_{st} + \lambda_2 \mathbf{X}_{st} + \gamma_{s1}^0 + \gamma_{s1}t + \delta_{1t} + \varepsilon_{st1} \quad (2)$$

(the first stage), and

$$\ln(\mu_{st}) = \beta_0 + \beta_1 PSF_{st}^{Black} + \beta_2 \varepsilon_{st1} + \beta_3 \mathbf{X}_{st} + \gamma_{s2}^0 + \gamma_{s2}t + \delta_{1t} + \varepsilon_{st2} \quad (3)$$

(the second stage), where the contents of  $R$  and  $\mathbf{X}$  vary by specification.

## 4. Results and Discussion

### 4.1. Access to Guns and Lynching in the State Panel

Table 2 includes four count models of Black citizens' lynching deaths that show negative relationships between the proxy for Black residents' firearm access ( $PSF_{st}^{Black}$ ) and Blacks' lynching deaths. The coefficients on White residents' firearm access are also negative but are slightly smaller and not statistically different from 0. Historical anecdotes of Black citizens' resistance offer at least one explanation: there were no questions about whether members of a mob were armed or who would eventually win if the conflict turned violent. The only question was whether the cost of a lynching would include White residents' lives, and the answer depended on Black residents' access to firearms.

We first estimate a Poisson panel regression with robust standard errors, state and year fixed effects, the full set of control covariates, and state-specific linear

<sup>14</sup> Using linearly interpolated values between 1945 and 1950 produces similar results that have greater estimated precision, but the estimates of  $PSF_{st}$  in 1950 are likely to be a more accurate proxy for firearm ownership between 1946 and 1948 than estimates during or at the close of World War II.

Table 2  
Lynching Deaths of Black Residents

	Poisson		Negative Binomial	
	(1)	(2)	(3)	(4)
PSF <sup>Black</sup>	-.950 (.918)	-.939 (.914)	-1.185* (.591)	-1.194* (.582)
PSF <sup>White</sup>		-.699 (.916)		-1.098 (1.161)

**Note.** The specifications estimate a model for the count of lynching deaths of Black residents in a state and year. Models are conditional on state and year fixed effects and state-specific linear time trends and include log mean Black and White residents' wages; log total population; non-White percentage of the population; Black doctors, firefighters, and farmers per capita of Black residents; and real capital per worker as totals and separately for farming, manufacturing, and service sectors. The sample includes the Confederate states except Texas (because of insufficient data). Standard errors are in parentheses.  $N = 317$ .

\*  $p < .05$ .

time trends. The Poisson estimator has the benefit of making weak assumptions about the relationship between the variance and the mean of the outcome variable, but the negative (but not statistically significant) results may be biased toward 0 because of overdispersion. We also estimate a conditional negative binomial model over the same specification. These results are in the same direction, slightly larger, and more precise ( $p < .05$ ). In these estimates the attenuation from overdispersion is mitigated, but the results depend on a very strong assumption about the relationship between overdispersion and the conditional mean, and concerns over endogeneity remain.

The correlations are clear across all four models. In states and years in which Black residents had more access to firearms, there were fewer lynchings. Given all the threats to identification, these results are, at best, suggestive of a relationship that is worth exploring in more detail.

#### 4.2. Race-Based Disarmament

Table 3 presents three fixed-effects regression specifications to demonstrate the relationship between state policies, local law enforcement capacity, and Black residents' firearm access. In the specifications for Black residents' firearm access, there is a strong and significant relationship between the variables of interest and access, always in the expected direction. As long as gun prices are low, places with pistol bans have lower rates of firearm access by Black residents than those without bans. When gun prices are high, the effects are diminished. Similarly, states with more White officers per Black residents have lower rates of gun access for Black residents, but these effects are similarly diminished when gun prices are high. We then replicate the specifications for White residents' firearm access. These relationships are not economically or statistically significant.

Table 3  
Disarmament by Law and State Capacity

	PSF <sup>Black</sup>			PSF <sup>White</sup>		
	(1)	(2)	(3)	(4)	(5)	(6)
Pistol <sub>st</sub>	-.277**			.051		
	(.081)			(.053)		
Pistol <sub>st</sub> × Price <sub>y</sub> <sup>SG</sup>	.003**			-.001		
	(.001)			(.001)		
Law <sub>st</sub> <sup>White</sup> / Population <sub>st</sub> <sup>Black</sup>		-.085**	-.159**		-.004	-.005
		(.019)	(.034)		(.016)	(.018)
Law <sub>st</sub> <sup>White</sup> × Price <sub>y</sub> <sup>SG</sup>			.001*			.000
			(.000)			(.000)
N	277	317	277	277	317	277

Note. Observation counts are smaller for samples that include shotgun prices, which are not reliably available in 1942–45. The estimations include state and year fixed effects, state-specific linear time trends, and log mean Black and White residents’ wages; log total population; non-White percentage of the population; Black doctors, firefighters, and farmers per capita of Black residents; and real capital per worker as totals and separately for farming, manufacturing, and service sectors. The sample includes all Confederate states except Texas (because of insufficient data). Standard errors are in parentheses.

\*  $p < .05$ .

\*\*  $p < .01$ .

The strong relationships in the specifications for Black residents’ firearm access mean that factors we identify as potential instruments might qualify as such. They do shift Black residents’ firearm access. The null results for White residents’ firearm access further suggest that the factors are not correlated with anything related to general shifts in firearm access. White residents’ access is not affected. These instruments are quite narrowly tailored to affect Black residents’ firearm access alone.

In addition to supporting the role these policy variables play in our IV strategy, the results also provide some fairly strong statistical evidence for the sort of race-specific disarmament that is prominently reported in the historical literature. Although policies like increased police employment or the enactment of pistol bans were, on their face, race neutral, these results show that in practice they had the consequence of disarming Black residents without similarly affecting White residents. This differential disarmament could help explain the gap in firearms access for Black and White residents that arose in the Jim Crow South.

#### 4.3. Disarmament and Lynching

Table 4 presents the second-stage results of the IV specifications, including the coefficients on the control function ( $\varepsilon_{stl}$ ) in the second stages. The specifications all have the Poisson functional form and use the indicated excluded instrument. In all three variants, the estimated negative effect of Black residents’ firearm access on lynchings is quite large and statistically significant ( $p < .01$ ). A

Table 4  
Control-Function Analysis

	Pistol <sub>st</sub> × Price <sub>y</sub> <sup>SG</sup>		Law <sup>White</sup>		Law <sup>White</sup> × Price <sub>y</sub> <sup>SG</sup>	
	First Stage	Second Stage	First Stage	Second Stage	First Stage	Second Stage
PSF <sup>Black</sup>		-9.581** (3.567)		-8.818** (2.672)		-11.662** (2.577)
Pistol <sub>st</sub>	-.277** (.081)	.411+ (.248)				
Pistol <sub>st</sub> × Price <sub>t</sub> <sup>SG</sup>	.003** (.001)					
Law <sub>st</sub> <sup>White</sup> / Population <sub>st</sub> <sup>Black</sup>			-.085** (.019)		-.159** (.034)	-.258 (.304)
Law <sup>White</sup> × Price <sub>y</sub> <sup>SG</sup>					.001* (.000)	
ε <sub>st1</sub>		8.652* (3.620)		7.942** (2.231)		10.847** (2.749)
PSF <sup>White</sup>	.080 (.088)	.295 (1.030)	-.012 (.102)	-.500 (.845)	.054 (.082)	.410 (1.005)
F-statistic		21.22		20.22		9.333
N	277	277	317	317	277	277

**Note.** Results are from instrumental variables estimation of the effect of the percentage of Black residents' suicides from firearms on Black residents' lynching deaths. The second stage is a Poisson maximum-likelihood estimated regression that includes the first-stage error estimate ( $\varepsilon_{st1}$ ) as a control function. Observation counts are smaller for samples that include shotgun prices, which are not reliably available in 1942–45. The *F*-statistics are for the restricted variable from the first stage. All models are conditional on state and year fixed effects and state-specific linear time trends and include log mean Black and White residents' wages; log total population; non-White percentage of the population; Black doctors, firefighters, and farmers per capita of Black residents; and real capital per worker as totals and separately for farming, manufacturing, and service sectors. The sample includes Confederate states except Texas (because of insufficient data). Standard errors are in parentheses.

+  $p < .10$ .

\*  $p < .05$ .

\*\*  $p < .01$ .

1-standard-deviation (.16) increase in the proxy for Black residents' firearm access decreases the expected number of lynchings by between .8 and 1.4 lynchings per year, about half a standard deviation. The coefficients on  $\varepsilon_{st1}$  in the second stage are large and positive in all specifications, which indicates that the simple panel regressions suffer from positive bias in the estimated relationship between Black residents' firearm access and lynchings. The consistency in the estimated coefficients on the endogenous variable across the variants demonstrates the robustness of the restricted variables to concerns about event-driven reverse causality.

Taken together, these results tell a consistent story about how Black citizens' firearm access could shift the lynching risk that Black residents of the Jim Crow South faced. Simple panel correlations show a small negative relationship be-



tween Black residents' firearm access and lynchings, but those estimates are confounded by some mixture of reverse causality, omitted-variable bias, and bias in the measure of firearm access, whereby places and times that otherwise are more likely to have lynchings have greater measured access to guns by Black residents. A very plausible story, consistent with the historical record, is one in which Black residents in fear of lynchings seek out firearms to protect themselves. But where policy choices and economic circumstances made it difficult, Black residents had less access to firearms, perhaps because of the increased enforcement of disarmament laws targeting them. That reduction in access led to more lynching victims, as Black residents were not able to protect themselves or rely on the institutions of law enforcement to protect them.

#### 4.4. Sensitivity Analysis

The results are not particularly sensitive to the years include in the analysis. In the Online Appendix, we consider a variety of time windows. Restricting the analysis to smaller, earlier samples when rates of lynching were higher yields similar estimated coefficients on the effect of Black residents' firearm access (Table OA2). When we apply the same sample restrictions to both control-function strategies, law enforcement capacity remains a strong instrument, and the estimates are consistent (Table OA3). The identification strategy using pistol bans and firearm prices is weakened, while still yielding qualitatively consistent results, when observations from years in the 1940s are reduced (Table OA4). This is unsurprising, because a significant portion of variations in state laws occurred during the late 1930s through the 1940s.

### 5. Conclusion

Drawing on historical vital statistics, we show that efforts to disarm Black residents under Jim Crow were successful, as the intrawar period was characterized by a significant relative decline in their access to firearms. This decline may have had substantial consequences in a world in which the formal institutions of the law would not protect Black citizens' lives and property. Using suicide records as a proxy for firearm access, we find a negative relationship between Black residents' firearm access and the number of recorded lynchings.

Our analysis is limited by both its reliance on a proxy for firearm access and the coarseness of the data. Interpretation of the results is also limited by the reasonable expectation of reverse causality: that individuals acquired firearms in response to violence in their communities. While we separately employ several IV strategies to mitigate positive bias and produce results that reinforce our observation of the value of firearms in the Black community during the early and mid-20th century, extrapolation and comparison across eras remain difficult.

The history of the Jim Crow South abounds with anecdotal accounts of the Black community making use of firearms to defend themselves. Effective policing and public safety were not made available to the Black community, and firearms

made both self-defense and community defense possible. Charles Sims, president of the Deacons for Defense, stated it plainly: “We decided since we didn’t have protection from the law, by the law, we should organize a group to protect our peoples in the neighborhood” (Cobb 2014, p. 7). Sims and the Deacons were both correct in their assessment and successful in their mission.

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