

# Firearms and Lynching\*

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## Abstract

We assess firearms as a means of Black self-defense in the Jim Crow South. We infer firearm access 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 firearms. The interaction of these mechanisms with changing national market prices for firearms provides us with a credible identification strategy for Black firearm access. Rates of Black lynching decreased with greater Black firearm access.

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“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.” -  
Ida B. Wells-Barnett, Black anti-lynching activist, 1918 <sup>1</sup>

# 1 Introduction

In this paper, we investigate the relationship between firearm access and racialized violence in the Jim Crow South, where Black citizens 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 versus White firearm access in this same period.

The impact of firearm access on violence has proven challenging to adjudicate. There are difficulties in both measuring access and inferring the direction 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 within myriad research contexts, including their relationship to criminal deterrence (Duggan, 2001; Kleck, 2015; Lang, 2016; Manski and Pepper, 2018), self-defense (Cheng and Hoekstra, 2012; Kovandzic et al., 2013; McClellan and Tekin, 2017), homicide and suicide rates (Edwards et al., 2018; Riddell et al., 2018; Siegel et al., 2013), and broadly defined social costs (Cook and Ludwig, 2006).

All of these quantities were 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 et al., 2018a; Jones et al., 2017; King et al., 2009; Messner et al., 2005; Williams, 2019), 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, in particular for Black southerners (Adler, 2008). Self-defense effects may be difficult to ascertain because of omitted variable bias concerns, and the relatively safe conditions of the modern developed world might render self-defense effects

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<sup>1</sup>“Crusade for Justice: The Autobiography of Ida B. Wells”, 1970

too small to precisely identify. Self-defense effects may be far larger for Black residents of the Jim Crow South than in the present day.

Historical measures of firearm ownership or access have proven difficult to obtain (Brennan et al. 1993), especially in periods when groups might have their ownership restricted by either law, practice, or broader social norms. Surveys, for instance, are unreliable if firearm ownership is restricted, particularly for members of groups where restrictions are being strongly or unevenly applied. We need a proxy measure of ownership where disclosure of ownership is not endogenous to political power or social standing. The fraction of suicides that were committed with a firearm can serve as such a proxy. The percentage of suicides committed with a firearm, compared to 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 et al., 2004; Cook, 1983; Briggs and Tabarrok, 2014; Hemenway and Miller, 2000; Miller et al., 2002; Nagin, 2020), including in panel settings (Cerqueira et al., 2019).<sup>2</sup>

The fraction of suicides employing a firearm is a particularly attractive proxy for firearm access in our setting. It is available, by race and geography, from the early twentieth century.<sup>3</sup> Our data allow us to measure White and Black firearm access and violent deaths between 1913 and 1999. No other proxy of firearm access is available for nearly that long of a panel. 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, removing 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 to us unlikely that this would bias the recorded method by which the suicide was accomplished i.e. whether a firearm was employed. Thus, compared to other proxies for firearm access, differential “reporting” as a function of firearms restrictions presents a relatively modest concern.

We use hand-coded data from 1913 to 1950 to separately calculate the percent of suicides

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<sup>2</sup>Kleck (2004) claims that percentage of suicides committed with a firearm (PSF) is “virtually perfect” as a cross-sectional proxy, but fails as an cross-temporal proxy. This claim, however, is based on the Pearson correlation coefficient between PSFs across years within the GSS, 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).

<sup>3</sup>The early to mid 20th century vital statistics records we use in our analysis, up through the Civil Rights Movement, identifies individuals as White or “colored”, the latter referring to all non-White individuals. Given our emphasis on the former Confederate states up through the end of the Civil Rights Movement, the overwhelming majority of non-White individuals are African-American former slaves or their descendants, who we will uniformly refer to as “Black” throughout this paper.

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 number of historiographically identified Black lynching deaths (Beck and Tolnay, 2019), and show that in states and years where 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>4</sup> Reverse causality is an important concern when analyzing the relationship between firearms and violence—violent victimization may lead individuals to seek out firearms, biasing the observed effect of firearms upward. In this historical setting there is also the potential for an additional omitted-variable bias in the other direction—states in which firearm access by Black residents is particularly restricted due to 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 within 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 hand-coded set of national firearm prices. These IV results allow us to both correct for the bias 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 firearms access. Second, states and years in which Black citizens have lower rates of firearm access due to these shifts have significantly higher lynching rates. A one standard-deviation reduction in our proxy for Black firearm access reduces Black lynchings by about 1.4 per year, which is about 65% 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, however, and eventual consequence of these laws was the disarmament of Black communities, increasing their vulnerability to racial violence while leaving White access to firearms entirely unchanged. When considering the merits of the right to bear arms, and limitations to those

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<sup>4</sup>In their survey of 41 papers examining the effect of firearm ownership on crime, Kleck (2015) found three papers employed a causal inference strategy to address endogeneity.

rights, equity of application and enforcement should be a first-order concern.

## 1.1 Black Disarmament, Lynching, and Self-Defense

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, 1994). Black 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*, 1875, the Court held the federal government had no authority to punish members of the Ku Klux Klan for confiscating the firearms of two Black men, setting a precedent that effectively delegated the defense of constitutional rights to state government, including the rights to peaceably assemble and bear arms. 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 descriptions of Black citizen forming armed *ad hoc* militias to deter lynchings. Dr. 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 within Black newspapers across the country (Boyle, 2007; Johnson, 2014). In his recounting of his experience as an attorney serving the Civil Rights Movement, Donald B. Kates made note of the broad endorsement of firearm possession within the movement and “attributed the relative quiescence of the Klan to the fact that the Black community was so heavily armed” (Kates, 1979). Cobb (2014), from his point of view as a former field secretary for the SNCC Student Nonviolent Coordinating Committee (SNCC) 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 role 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 sur-

vival mechanism.” Intra-racial violence, however, was not the only threat. Within 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 come from tables compiled in the *Mortality Statistics Annual Report* ([Bureau of the Census, 1959](#)). 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 U.S resided in a registration state and the first former Confederate state (NC) began reporting. By 1920, over 80% of the population was in a registration state, including 7 former Confederate states that reported statistics broken down by race. In 1928, all but one confederate state (Texas) reported, which joined in 1933. States without substantial Black populations did not report by race in early years, but beginning in 1937 all states (and DC) 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.

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>5</sup> Other than a gap from 1946-1948 for all states and one 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: 1913-1950

From these vital statistics, we use 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 et al., 2002](#)), countries ([Hemenway and Miller, 2000](#); [Killias, 1993](#)), and within states over time ([Azrael et al., 2004](#); [Cerqueira et al., 2019](#)).

While the percent of suicides by firearm has proven the most reliable proxy for firearm access in contemporary contexts, it remains untested further into the past. As a validation exercise, [Figure 1](#) presents binned scatter plots and linear estimates of the relationship between our proxy,  $PSF_{st}$ , and the per-capita rate of all non-suicide deaths involving a firearm. These statistics are calculated for the entire population and are highly correlated.

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<sup>5</sup>In some early years, firearms deaths are pooled with deaths from explosives. In later years, when explosives deaths are broken out, they are quite rare.

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 where the institutions are more captured have more miscoding of Black firearm homicides as suicides, we will overstate the rate of Black firearm access in those places and times. Of course, if they miscode many Black non-firearm homicides as suicides, we will understate Black 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 instrumental-variable strategy for our impact estimates.

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

The time series presented in Figure 2a and the history presented in Section 1.1 suggest that former Confederate states had some success in disarming Black citizens in the Jim Crow era.<sup>6</sup> Prior to 1920, Black firearm access outpaced White access. That ordering was reestablished after World War II, but from 1920 to 1940, White firearm access increased substantially alongside a decline in Black firearm access. The increase in White 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 firearm possession suggests that Jim Crow era efforts to disarm Black people were effective, but this gap is 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 within a state in a given year is as much a barometer for the ambient level of violence leveled a Black citizens every day as it is a historical measure of terrorist events (Tolnay and Beck, 1995).<sup>7</sup>

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<sup>6</sup>For maps of  $PSF$  by state and era, see Appendix Figure A.4.

<sup>7</sup>Lynchings are correlated with segregation (Cook et al., 2018a,b), Black migration (Tolnay and Beck, 1992b), the size of the Black population (Tolnay and Beck, 1992a; Christian, 2017), cotton prices (Beck and Tolnay, 1990), and local politics (Beck et al., 2016).



Our analysis of the lynching records from the Beck-Tolnay lynching data (Beck and Tolnay, 2019) uses a sample of 335 state-years from former states of the Confederacy, 1913-1950.<sup>8</sup> Figure 2b displays a time series of overall Black lynchings in former Confederate states. Our sample includes a mean of 2.16 lynching deaths per year, with 41% of state-years experiencing at least one Black lynching death and a maximum of 13 in Georgia in 1922.<sup>9</sup> Lynching deaths per state capita steadily decrease through our 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 percent of the population that is Black recorded in the Census, linearly interpolated within decades. All regressions include log mean Black and White wages as reported to or imputed from the U.S. Census (Ruggles et al., 2021), and interpolated between census years. For the years 1910 to 1940, we impute these using the share of marginal output received by workers and reported decennially by state and industry in Turner et al. (2007) and Tamura et al. (2016).<sup>10</sup> To control for availability of services to the Black community that are relevant to violent and accidental death outcomes, we include the number of Black firemen and Black physicians per Black capita reported in each decennial Census, linearly interpolated within decades. We also include the percentage of the Black population in a state that were farmers given that within the era a shotgun was considered a necessity of the occupation. Given their reliance on imputed wage estimates based on worker output, all regressions using data prior to 1940 also include estimates of physical capital stocks within the farming, manufacturing, and service industries (Tamura et al., 2016; Turner et al., 2007). All control variables are summarized in Appendix Table A.1.

To implement our control-function approach (see Section 3.2) we use three instrumental variable identification strategies. Our first identification strategy uses the interaction of state bans of pistol sales with the price of firearms as a measure of the impact of *de facto* disarmament policy mechanisms. Within our analytic sample, 4 of our 10 Confederate states have bans on pistol sales and carrying in effect for a portion of the years observed (Figure 3).

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<sup>8</sup>As of July 15, 2020, the lynching database does not include Black lynchings from Texas, so we exclude it from our analysis of lynchings.

<sup>9</sup>There are 21 recorded lynchings in Georgia in 1919, but we do not have sufficient vital-statistics data for Georgia until 1922.

<sup>10</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 Black and White wages, linearly interpolating mean Black and White wages in the intervening years. A similar procedure was employed in Choquette (2020).



These bans always served to reduce Black firearms access within our 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 the quantity of firearms demands. Similarly, price effects can be expected to be weaker when lower cost options are prohibited, limiting purchases to goods targeting consumers with less elastic demand. Our data on the implementation of these policies comes from a reviews of state firearms laws by Warner (1938), Spitzer (2017), and Frassetto (2014).<sup>11</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 1913 and 1968, validating these prices, when possible, using records from the US Department of Agriculture Yearbook. Shotgun prices varied considerably year to year, as presented in Figure 3. We include both an indicator for pistol bans and its interaction with firearm prices in the first stage estimation, but only restrict the interaction term in the second stage out of concern that the introduction of pistol bans are correlated with the number of lynchings.<sup>12</sup> Omitted factors that might be correlated to 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 the national prices and local policies should be an exogenous shift in firearm access.

Our second identification strategy uses the number of White law enforcement officers per 1000 Black residents,  $law_{st}^{White}$ , measured as the number of police, sheriffs, sheriff’s 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, the act of confiscation nonetheless requires sufficient manpower to accomplish the task—ambitions of disarming of Black individuals were conditional on local state capacity. Further, 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 against Black men and women should not correlate with their labor force size as doing nothing is rarely labor intensive. Figure A.3 displays the average value of this metric over time for former Confederate states.<sup>13</sup>

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<sup>11</sup>The relevant laws banning the sales of pistols include SC Code (1932) §1255; Ark. Dig. Stat. §3509; and Tenn. Code Ann. §11009, 1931 Fla. Laws 2069, § 7 Warner (1938). Arkansas, Tennessee, and South Carolina each banned the carrying and sale of Pistols. Florida allowed cities to ban pistols in 1927, but the salience of that capacity grew in importance when it was extended to unincorporated villages, broadening in frequency across the state in 1931.

<sup>12</sup>Estimates that include the pistol ban indicator as a restricted variable as well are qualitatively unchanged ( $p < 0.01$ ) from our reported results

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

Our third identification strategy interacts the number of White law enforcement officers per 1000 Black residents with the price of firearms. As in our first strategy with pistol bans, this approach allows us to identify the effects of firearm access by isolating the effects of state capacity for disarmament away from the potential omitted variable bias in law enforcement prioritization. The result of this alternative strategy also serve as a robustness check for law enforcement as a stand-alone instrument and test of the exclusion restriction—if both 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 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 a set of previously discussed proxies, our estimation specification and identification strategy must also address non-linear outcome variables and the potential for omitted variable bias and reverse causality.

#### 3.1 State Panel Regressions

Black lynching deaths are count data characterized by significant over-dispersion, with zero recorded Black lynching victims accounting for more than half of the state-year observations in the sample (51.6%). 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 will build our core estimation of lynching outcomes using the Poisson estimation with two-way fixed effects.

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

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

$PSF_{st}$  is race-specific percent of suicides with a firearm as a proxy for firearm access,  $\mathbf{X}_{st}$  is a vector of control variables,  $\gamma_s^0$  are state-specific intercepts,  $\gamma_s$  are state-specific time trends, and  $\delta_t$  are year-specific intercepts.

To accommodate the unavailability of data in the three years following World War II,  $PSF_{st}$  is carried back from 1950 to account for 1946 to 1948.<sup>14</sup>

Both Poisson and negative-binomial regressions are scaled by the size of the Black population within the state and year.  $\mathbf{X}$  includes measures of within-state and within-year variation in economic conditions and demographics. To account for both 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 our estimates we implement an instrumental variable strategy using a control-function approach. In the first stage, we estimate a two-way fixed-effect OLS model of Black firearm access on year ( $\delta_{t1}$ ) fixed effects, state-specific intercepts and year trends ( $\gamma_{s1}^0 + \gamma_{s1}t$ ) and, covariate controls ( $\mathbf{X}_{st}$ ), and one or more restricted variables ( $\mathbf{R}_{st}$ ). 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 ( $\hat{\epsilon}_{st1}$ ) as a control function (Lin and Wooldridge, 2019). Formally, our joint model is given by variants of

$$\text{(1st Stage)} \quad PSF_{st}^{Black} = \lambda_0 + \lambda_1 \mathbf{R}_{st} + \lambda_2 \mathbf{X}_{st} + \gamma_{s1}^0 + \gamma_{s1}t + \delta_{t1} + \epsilon_{st1} \quad (2)$$

$$\text{(2nd Stage)} \quad \ln(\mu_{st}) = \beta_0 + \beta_1 PSF_{st}^{Black} + \beta_2 \epsilon_{st1} + \beta_3 \mathbf{X}_{st} + \gamma_{s2}^0 + \gamma_{s2}t + \delta_{t2} + \epsilon_{st2}, \quad (3)$$

where the exact contents of  $\mathbf{R}$  and  $\mathbf{X}$  vary by specification.

## 4 Results and Discussion

### 4.1 Gun Access and Lynching in State Panel

Table 2 includes four count models of Black lynching deaths, within all of which we observe negative relationships between our proxy for Black firearm access ( $PSF_{st}^{Black}$ ) and Black

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

lynching deaths. The coefficients on White firearms are also negative, but slightly smaller not statistically different from zero. Historical anecdotes of Black resistance offer at least one explanation: when it comes to the mob, there is little question whether members were armed or who would eventually win if the conflict turned violent. The only question was whether or not the cost of a lynching would include White lives, and the answer depended on *Black* firearms.

In columns 1 and 2, we estimate a Poisson panel regression with robust standard errors, state and year fixed effects, our full set of control covariates, and state-specific linear 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 our negative (but not statistically significant) results may be biased towards zero because of overdispersion. In columns 3 and 4 we estimate a conditional negative binomial model over the same specification. These results are in the same direction, slightly larger, and more precise ( $p < 0.05$ ) results. In these estimates the attenuation from overdispersion is mitigated, but the results depend on a very strong assumption of 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 and a relationship that is worth exploring in more detail.

## 4.2 Race-Based Disarmament

In Table 3, we present three different fixed-effect regression specifications to observe the relationship between state policies, local law enforcement capacity, and Black firearm access. The first column includes an indicator for a pistol ban and its interaction with firearm prices, the second includes White law-enforcement officers per Black resident, and the third includes both White law enforcement and its interaction with price. In all three cases, there is a strong and significant relationship between the variables of interest and Black firearm access, and always in the expected direction. As long as gun prices are low, places with pistol bans have lower rates of Black firearm access than those without. When gun prices are high, these effects are diminished. Similarly, states with more White officers per Black resident have lower rates of Black gun access, but these effects are similarly diminished when gun prices are high. We then replicate these specifications for White firearms access in Columns 4 through 6. None of these relationships are economically or statistically significant.

The strong relationships in the first three columns mean that factors we have identified

as potential instruments might qualify as such. They do, in fact, shift Black firearm access. The null results in the last three columns further suggest that the factors are not correlated anything related to general shifts in firearm access, as White access is not affected. These instruments are quite narrowly tailored to affect Black firearm access, alone.

In addition to supporting the role these policy variables play in our instrumental-variable strategy, these results also provide some fairly strong statistical evidence for the sort of race-specific disarmament that has been prominently reported in the historical literature, as discussed in Section 1.1, above. 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 having a similar effect on Whites. This differential disarmament could go a long way toward explaining the Black-White firearm access gap that arose in the Jim Crow South.

### 4.3 Disarmament and Lynching

In Table 4, we present the second-stage results of our IV specifications, including the coefficients on the control function ( $\epsilon_{st1}$ ) in the second stages. These specifications all have the Poisson functional form and use the indicated excluded instrument. In all three estimation strategy variants, the estimated negative effect of Black firearm access on lynchings is quite large and statistically significant ( $p < 0.01$ ). A one s.d. (0.16) increase in our Black firearm access proxy decreases expected number of Black lynchings by between 0.8 and 1.4 lynchings per year, about half standard deviation. The coefficient on  $\epsilon_{st1}$  in the 2nd stage are large and positive in all specifications, indicating the simple panel regressions suffer from positive bias in the estimated relationship between Black firearm access and lynchings. The consistency of estimated coefficient on the endogenous variable across the variants demonstrates the robustness of the restricted variables to concerns of event-driven reverse causality.

Taken together, these results tell a consistent story about how Black firearm access can shift the lynching risk that Black residents of the Jim Crow South faced. Simple panel correlations show a small negative relationship between Black 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 where lynchings are more likely otherwise have greater measured Black gun access. A very plausible story, consistent with the historical record, is one in which Blacks residents in fear of lynchings seek out firearms to protect themselves. But in places and times where policy choices and economic circumstances made it difficult, Black residents had less access to firearms, perhaps due to the increased enforcement of disarmament laws targeting Black residents. 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

Concerns over sample selection in our lynching results are similarly allayed by results reported in Table A.2. Restricting the analysis to smaller, earlier, samples when rates of lynching were higher yield similar, and slightly stronger estimated coefficients on the effect of Black firearm access. When we apply the same sample restrictions to both control function strategies, law enforcement capacity remains a strong instrument and the estimates remain consistent (Table A.2). Our identification strategy using pistol ban and firearm prices is weakened, while still yielding qualitatively consistent results, when restricting the sample reduces the number of observation years in the 1940s. This is unsurprising, because a significant portion of the variation in state laws occurs during the late 1930's through the 40s.

## 5 Conclusion

Drawing on historical vital statistics, we show that efforts to disarm Black residents under Jim Crow were successful, as the intra-war period was characterized by a significant relative decline in Black residents' 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 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 instrumental variables strategies to mitigate any positive bias, and produce results that reinforce our observation of the value of firearms in the Black community during 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 effective 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”.<sup>15</sup> Sims and the Deacons were both correct in their assessment and successful in their mission.

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<sup>15</sup>Quoted from pg. 7 of [Cobb \(2014\)](#).



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Table 1: Summary Statistics: Former Confederate States, 1913-1950

Lynchings and Suicide Estimation Variables								
	Black				White			
	mean	sd	min	max	mean	sd	min	max
Black Lynchings	1.22	2.20	0.00	13.00				
Suicides	17.03	7.69	3.00	56.00	167.08	82.04	24.00	482.00
Percent Suicide w/ Firearm (PSF)	0.56	0.16	0.00	0.89	0.59	0.09	0.18	0.80
Observations	317				317			
Instrumental Variables								
	All							
	mean	sd	min	max				
Pistol Ban	0.19	0.39	0.00	1.00				
Firearms Licensing	0.21	0.41	0.00	1.00				
Shotgun price (real dollars)	125.18	35.74	74.50	207.26				
Observations	277							
	All							
	mean	sd	min	max				
White Police per 1000 Black capita	2.42	1.52	0.25	7.75				
Observations	317							

**Note:** Summary stats are for the key variables in the relevant estimations. Observation counts are smaller for samples that include shotgun prices, which are not reliably available due to metal shortages and associated rationing during the peak years of US involvement in World War II (1942-1945).

Table 2: Black Lynching Deaths in Former Confederate States, 1913-1950

Black Lynching Count				
$PSF^{Black}$	-0.950 (0.918)	-0.939 (0.914)	-1.185** (0.591)	-1.194** (0.582)
$PSF^{White}$		-0.699 (0.916)		-1.098 (1.161)
Model	Poisson	Poisson	Negative Binomial	Negative Binomial
State + Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Time Trend	Yes	Yes	Yes	Yes
N	317	317	317	317

**Note:** All specifications estimate a model for the count of Black lynching deaths within a state and year, using the specified functional-form. All models are conditional on state and year fixed effects, state-specific linear time trends, and include the following covariates: log mean Black and White wages; log total population; Non-White percentage of population; Black Doctors, Firemen, and Farmers per Black capita; and real capital per worker, both as totals and separately estimated within farming, manufacturing, and service sectors. See Table A.1 for covariate summary statistics. Includes all member states from the Confederacy except Texas (insufficient data).



Table 3: Disarmament by Law and State Capacity: Former Confederate States, 1913-1950

	Black PSF			White PSF		
$pistol_{st}$	-0.277***			0.051		
	(0.081)			(0.053)		
$pistol_{st} \times price_t^{SG}$	0.003***			-0.001		
	(0.001)			(0.001)		
$Law_{st}^{White} / Pop_{st}^{Black}$	-0.085***	-0.159***		-0.004	-0.005	
	(0.019)	(0.034)		(0.016)	(0.018)	
$law^{White} \times P_y^{SG}$		0.001**			0.000	
		(0.000)			(0.000)	
<i>Trend</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
<i>StateFE</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
<i>YearFE</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
<i>Controls</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
<i>N</i>	277	317	277	277	317	277

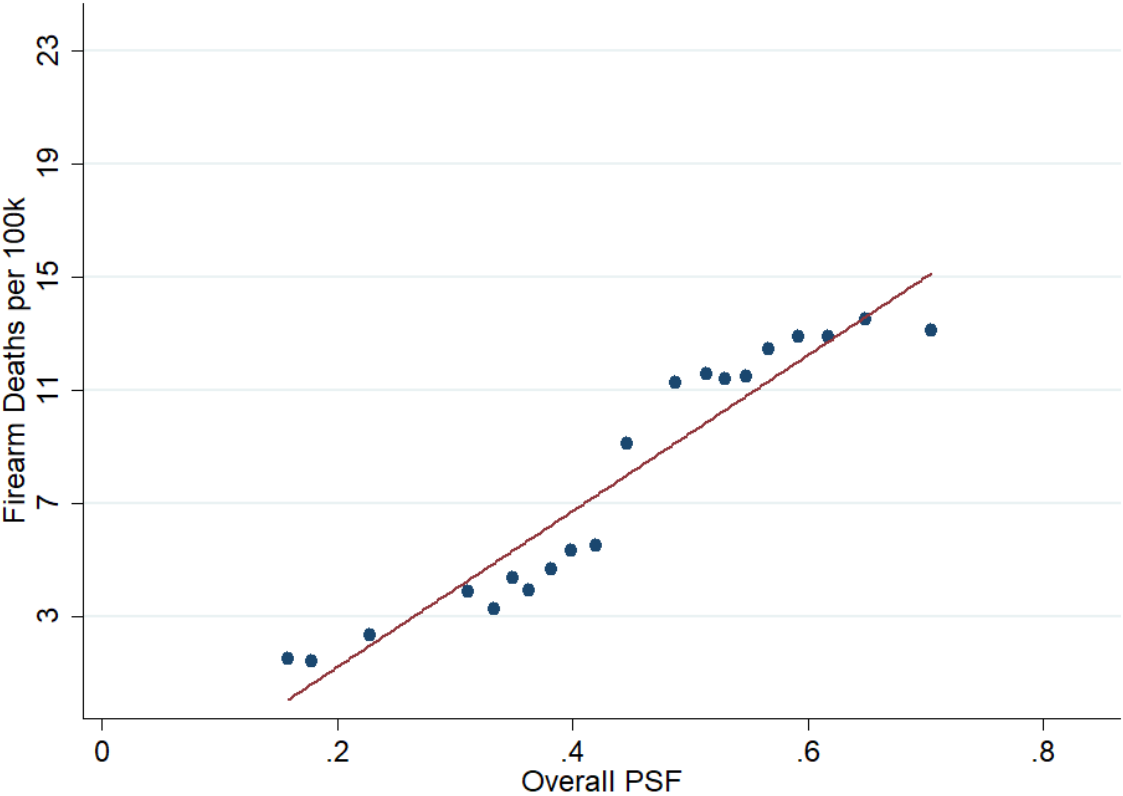
**Note:** Estimated effects of state bans on pistols ( $pistol_{st}$ ) and White police officers per Black capita  $Law_{st}^{White} / Pop_{st}^{Black}$  on Percents of Black and White Suicides from Firearms ( $PSF^{Black}, PSF^{White}$ ). Columns 1,3, 4, and 6 include interactions of key variables with real shotgun prices. Observation counts are smaller for analytic samples that include shotgun prices, which are not reliably available during the peak years of US involvement in World War II (1942-1945). All estimations include state and year fixed effects, state-specific linear time trends, and include the following covariates: log mean Black and White wages; log total population; Non-White percentage of population; Black Doctors, Firemen, and Farmers per Black capita; and real capital per worker, both as totals and separately estimated within farming, manufacturing, and service sectors. See Table A.1 for covariate summary statistics. Includes all member states from the Confederacy except Texas (insufficient data).

Table 4: Control Function Poisson w/ Instrumental Variables: Black Lynching Deaths in Former Confederate States, 1913-1950

	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage
$PSF^{Black}$		-9.581*** (3.567)		-8.818*** (2.672)		-11.662*** (2.577)
$pistol_{st}$	-0.277*** (0.081)	0.411* (0.248)				
$pistol_{st} X price_t^{SG}$	0.003*** (0.001)					
$Law_{st}^{White} / Pop_{st}^{Black}$			-0.085*** (0.019)		-0.159*** (0.034)	-0.258 (0.304)
$law^{White} \times P_y^{SG}$					0.001** (0.000)	
$\epsilon_{st1}(control\ func.)$		8.652** (3.620)		7.942*** (2.231)		10.847*** (2.749)
$PSF^{White}$	0.080 (0.088)	0.295 (1.030)	-0.012 (0.102)	-0.500 (0.845)	0.054 (0.082)	0.410 (1.005)
<i>IV</i>	$Pistol_{st} \times P_y^{SG}$		$Law^{White}$		$law^{White} \times P_y^{SG}$	
<i>F</i>		21.22		20.22		9.333
<i>Trend</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
<i>StateFE</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
<i>YearFE</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
<i>Controls</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
<i>N</i>	277	277	317	317	277	277

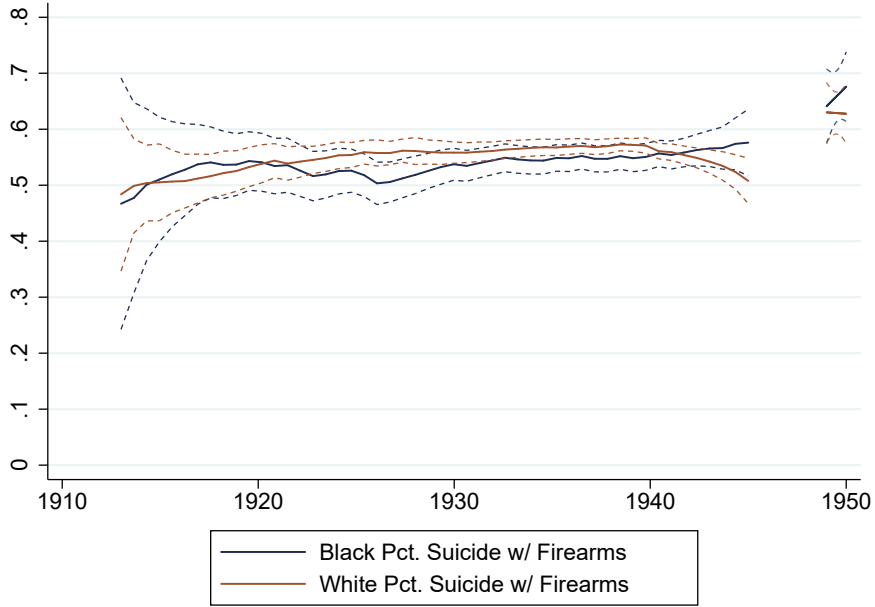
**Note:** First- and second-stage results from instrumental variables estimation of the effect of the Percent of Black Suicides from Firearms ( $PSF^{Black}$ ) on Black lynching deaths. The second stage is an Poisson maximum-likelihood estimated regression which includes the first state error estimate ( $\epsilon_{st1}$ ) as a control function. Observation counts are smaller for analytic samples that include shotgun prices, which are not reliably available during the peak years of US involvement in World War II (1942-1945). Each model reports *F* statistics for the restricted variable reported in the first stage. All models are conditional on state and year fixed effects, state-specific linear time trends, and include the following covariates: log mean Black and White wages; log total population; Non-White percentage of population; Black Doctors, Firemen, and Farmers per Black capita; and real capital per worker, both as totals and separately estimated within farming, manufacturing, and service sectors. See Table A.1 for covariate summary statistics. Includes all member states from the Confederacy except Texas (insufficient data).

Figure 1: Population-Weighted Binned Scatterplots of Relationship between Percent Suicide by Firearms and Non-Suicide Firearm Deaths per Capita 1910-1950.

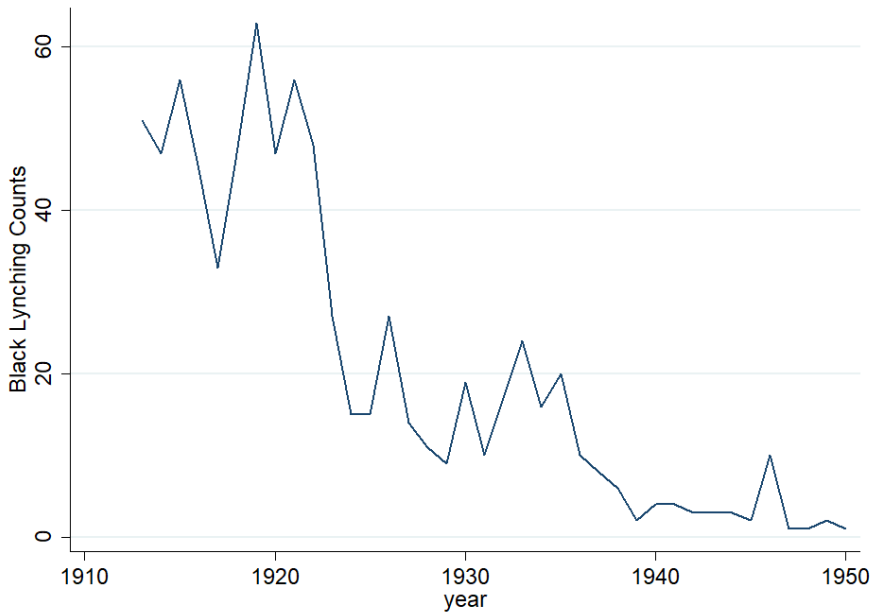


**Note:** Underlying bivariate regression:  $\beta(PSF^{All}) = 27.5 (1.04)$ ,  $p < 0.01$ ,  $R^2 = 0.47$

Figure 2: Trends in Firearm Access and Black Lynchings 1910-1950

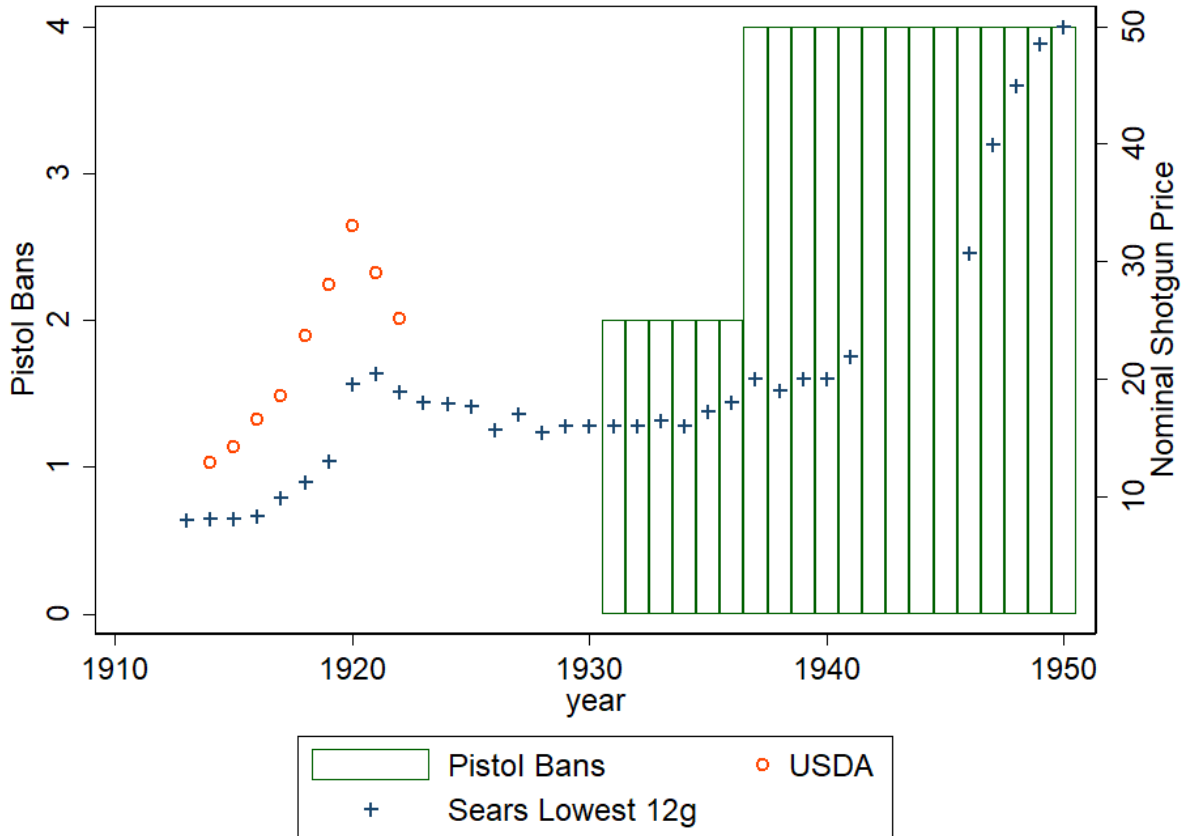


(a) Percent Suicide by Firearm by Race



(b) Black Lynchings by Year

Figure 3: Pistols Bans and Shotgun Prices



**Note:** Nominal shotgun prices as reported by Department of Agriculture and the Sears-Roebuck Catalogue, 1913-1950. Prices are not reliably available during the peak years of US involvement in World War II (1942-1945). State laws banning the sale and/or possession pistols from [Spitzer \(2015\)](#) and [Frassetto \(2014\)](#).

# A Appendix

Figure A.1: Example of Mortality Data from 1938 Vital Statistics of the U.S.

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## VITAL STATISTICS OF THE UNITED STATES

TABLE 18.—DEATHS FROM EACH CAUSE, BY RACE AND SEX:

(Exclusive of

List No.	CAUSE OF DEATH	MAINE				MARYLAND					
		Total deaths	White		All other		Total deaths	White		All other	
			M	F	M	F		M	F	M	F
	XVII.—Violent and accidental deaths-----	783	551	228	4	1,584	869	375	261	79	
163-171	Suicide-----	152	125	27		276	204	60	10	2	
163	Suicide by solid or liquid poisons, etc.*-----	8	3	5		22	8	14			
164	Suicide by poisonous gas-----	14	11	3		68	56	12			
165	Suicide by hanging or strangulation-----	31	25	6		48	35	13			
166	Suicide by drowning-----	16	9	7		18	11	3	3	1	
167	Suicide by firearms-----	74	70	4		92	76	10	6		
168	Suicide by cutting or piercing instruments-----	9	7	2		10	8	2			
169	Suicide by jumping from high places-----					14	10	2	2	1	
170	Suicide by crushing-----					1		1			
171	Suicide by other means-----					3		3			
172-175	Homicide-----	13	9	4		114	30	9	61	14	
173	Homicide by firearms-----	7	4	3		59	13	3	27	6	
174	Homicide by cutting or piercing instruments-----					27	4	1	19	3	
175	Homicide by other means-----	6	5	1		28	13	5	5	5	
176-198	Accidental, other, or undefined-----	618	417	197	4	1,194	635	306	190	63	
176	Attack by venomous animals-----										
177	Poisoning by food-----	1	1			11	5	4	5	1	
178	Accidental absorption of poisonous gas-----	5	4	1		18	14	4			
	Not associated with symbols 201-214-----	5	4	1		18	14	4			
	Associated with symbols 201-214-----										
179	Other acute accidental poisonings (except gas)-----	6	4	2		17	8	4	4	1	
180	Conflagration-----	22	8	14		25	13	3	5	4	
181	Accidental burns (except conflagration)-----	33	19	14		78	15	36	12	15	
	Not associated with symbols 201-214-----	32	19	13		71	15	34	11	11	
	Associated with symbols 201-214-----	1	1	1		7		2	1	4	
182	Accidental mechanical suffocation-----	7	4	3		28	13	5	4	6	
	Not associated with symbols 201-214-----	7	4	3		26	11	5	4	6	
	Associated with symbols 201-214-----					2					
183	Accidental drowning-----	100	78	20	2	144	99	8	34	3	
	Not associated with symbols 201-214-----	65	48	15	2	201	70	3	27	1	
	Associated with symbols 201-214-----	35	30	5		43	29	5	7	2	
184	Traumatism by firearms-----	19	18	1		16	9	2	3	2	
185	Traumatism by cutting or piercing instruments-----	7	5	2		9	5	3	1		
	Not associated with symbols 201-214-----	6	4	2		4	2	1	1		
	Associated with symbols 201-214-----	1	1			5	3	2			
186a	Traumatism by fall-----	175	86	89		349	148	163	28	10	



Figure A.2: Example of Firearms Pricing from 1926 Sears-Roebuck Catalog

# Double Barrel Shotguns

KIND OF GAME	12 GAUGE DRAMS	16 GAUGE SHOT	20 GAUGE DRAMS	20 GAUGE SHOT	SHOT SIZES GAUGES
SMALL DUCKS	3 1/4	1 1/2	2 1/2	1 2 1/4	7 1/2
PIGEONS					
QUAIL	3	1 OR 1 1/2	2 1/2	1 2 1/4	8
SNIPES					
WOODCOCK					
SHORE BIRDS	3	1	2 1/2	1 2 1/4	7 1/2
REED BIRDS					10

## Double Barrel Hammerless Shotgun.

This American Shotgun is manufactured for us by a well known Eastern firm of fire arm manufacturers.

**BARRELS**—Blued steel, matted top rib, left barrel full choke, right barrel slightly modified; positive extractor. Locking lug is solid extension from barrel.

**ACTION**—Hammerless, snap top lever; automatic thumb safety and casehardened frame.

**STOCK**—Pistol grip, finely checkered; rubber butt plate; length, 14 inches; drop, 3 to 3 1/4 inches; snap fore-end checkered. Packed for shipment, 14 pounds.

**GN 10 1/4**—12-gauge, 30 or 32-inch barrels. State length of barrels wanted. Weight, 7 1/4 to 8 1/2 pounds. \$19.45

**GN 11 1/4**—16-gauge, 30-inch barrels only. Weight, 7 1/4 to 7 3/4 pounds. \$19.50

**GN 12 1/4**—20-gauge, 28-inch barrels only. Weight, 7 to 7 1/4 pounds. \$19.55

## 410-Caliber Double Barrel Hammerless Gun.

**BARRELS**—Blued finish, 26 inches long. Chambered for both 44 XL shot cartridges and the 410 smokeless powder loaded shells. Positive extractor.

**STOCK**—Pistol, checkered grip. Snap checkered fore-end. A good grade, light weight gun, very effective for squirrels, rabbits and small game. Weight, about 6 lbs. Shipping wt., 10 lbs. \$23.78

**GN 19 1/4**

## Ithaca Hammerless Field Grade Shotgun.

**BARRELS**—Blued steel, choke bored. Raised matted rib. Positive extractor.

**FRAME**—Steel box frame, mottled finish. Cross-bite extension rib; top lever automatic safety; full reinforced breech.

**STOCK**—Selected walnut, pistol grip, highly polished and checkered. Fore-end of walnut finely checkered. State length of barrels wanted.

Weight, packed for shipment, 20-gauge, 11 pounds; 10, 12 and 16-gauge, 14 pounds.

**GN 7 1/4**—12-gauge, 30 or 32-inch barrels. Weight, 7 1/4 to 8 pounds. \$36.18

**GN 7 1/2**—20-gauge, 26 or 28-inch barrels. Weight, 5 1/2 to 6 pounds. \$36.18

**GN 7 3/4**—16-gauge, 28 or 30-inch barrels. Weight, 6 to 7 pounds. \$36.18

**GN 7 7/8**—12-gauge, 32-inch barrels. Weight, 9 to 9 3/4 pounds. \$36.18

## Lefever Nitro Special Double Barrel Hammerless Shotgun.

The well-known Lefever gun now at a price within reach of all. Barrels of tested nitro steel, left full choke and right slightly modified. Checkered pistol grip, walnut stock, securely fastened to frame with bolt running through entire length of stock. Non-breakable coil mainsprings. Weight, 7 1/4 pounds. Shipping weight, 13 pounds.

**GN 7 3/4**—12-gauge, 30-inch barrels only. \$27.85

## L. C. Smith Hammerless Guns.

**BARRELS**—Full choke bored. Tapered matted rib. Positive extractor.

**FRAME**—Solid steel, casehardened. Compensating extension rib; patent safety slide and top lever.

**STOCK**—Selected walnut, full pistol grip, finely checkered and highly polished. Checkered walnut fore-end.

State length of barrels. Weight, packed for shipment, 14 pounds.

**GN 2 1/4**—12-gauge, Field grade, 30 or 32-in. barrels. Weight, 7 1/4 to 8 pounds. \$45.60

**GN 2 1/2**—16-gauge, Field grade, 28 or 30-in. barrels. Weight, 6 1/4 to 7 pounds. \$45.60

**GN 3 1/2**—12-gauge, Ideal grade, 30 or 32-in. barrels. Weight, 7 1/4 to 8 pounds. \$49.90

## L. C. Smith Hammer Gun.

**BARRELS**—Full choke bored, for black or smokeless powder. 12-gauge, 30 or 32 inches. Raised matted rib. Positive extractor.

**ACTION**—Patent Crossbolt lock, top snap, compensating fore-end, rebounding bar lock, circular hammers.

**FRAME**—Solid steel, casehardened, mottled finish. The breech is made strong and substantial.

**STOCK**—American walnut, one-half pistol grip, finely checkered and finished. Checkered walnut fore-end.

**GN 8 1/4**—12-gauge, 30 or 32-inch barrels. State which is wanted. Weight, 7 1/4 to 8 1/2 pounds. Shipping weight, 14 pounds. \$35.80

## American Double Barrel Lock Gun.

**BARRELS**—Blued steel, polished. Choke bore in both barrels. Hollow matted rib. Positive extractor.

**ACTION**—Bar lock; circular hammers; top lever action. Frame of solid steel with full, substantial breech.

**STOCK**—One-half pistol grip, checkered and highly polished. Checkered fore-end. Shpg. wt. 14 lbs.

**GN 7 1/4**—12-gauge, 30-inch barrels. Weight, 7 1/4 to 8 pounds. \$17.95

**GN 7 1/2**—16-gauge, 28-inch barrels. Weight, 6 1/4 to 7 pounds. \$18.05

**GN 7 3/4**—20-gauge, 26 or 28-inch barrels. State length. Weight, 6 1/4 to 7 pounds. \$18.15

## Complete Double Barrel Hammerless Gun Outfit.

American made Hammerless Gun. Fitted with 12-gauge, 30 or 32-inch blued steel barrels; 16-gauge, 30-inch barrels; or 20-gauge, 26 or 28-inch barrels. Positive extractor, taper choke bored, full pistol grip checkered stock and fore-end. Outfit consists of gun, 25 Pointer shells, 1 bottle gun oil, 1 Tomlinson cleaner, 1 cleaning rod with swab, 1 scratch brush and wiper and 1 improved duck call. Weight, packed for shipment, 22 pounds. Cannot be sent by parcel post.

**GN 15 1/4**—12-gauge outfit, 30 or 32-inch barrels. State length. \$21.15

**GN 16 1/4**—Same as GN 15 1/4, but in 16-gauge, 28 or 30-inch barrels. State length. \$21.25

**GN 17 1/4**—Same as above, but in 20-gauge, 26 or 28-inch barrels. State length. \$21.35

## 22-Caliber Rifle Outfit.

Ideal for Small Game and Target.

A very excellent American made Rifle. Chambered for 22 short, long and long rifle cartridges. All working parts made of good quality hardened steel, carefully assembled. Takedown model. We include a three-piece cleaning rod with a brass wire bristle brush, and 100 22-caliber long cartridges. Shipping weight, 7 pounds. Cannot be shipped by parcel post.

**GN 15 1/4**—Complete outfit, as described. \$5.88

**SEARS, ROEBUCK AND CO. 897**



Figure A.3: White Law Enforcers per 1000 Black Residents in Former-Confederate States, 1910-1950

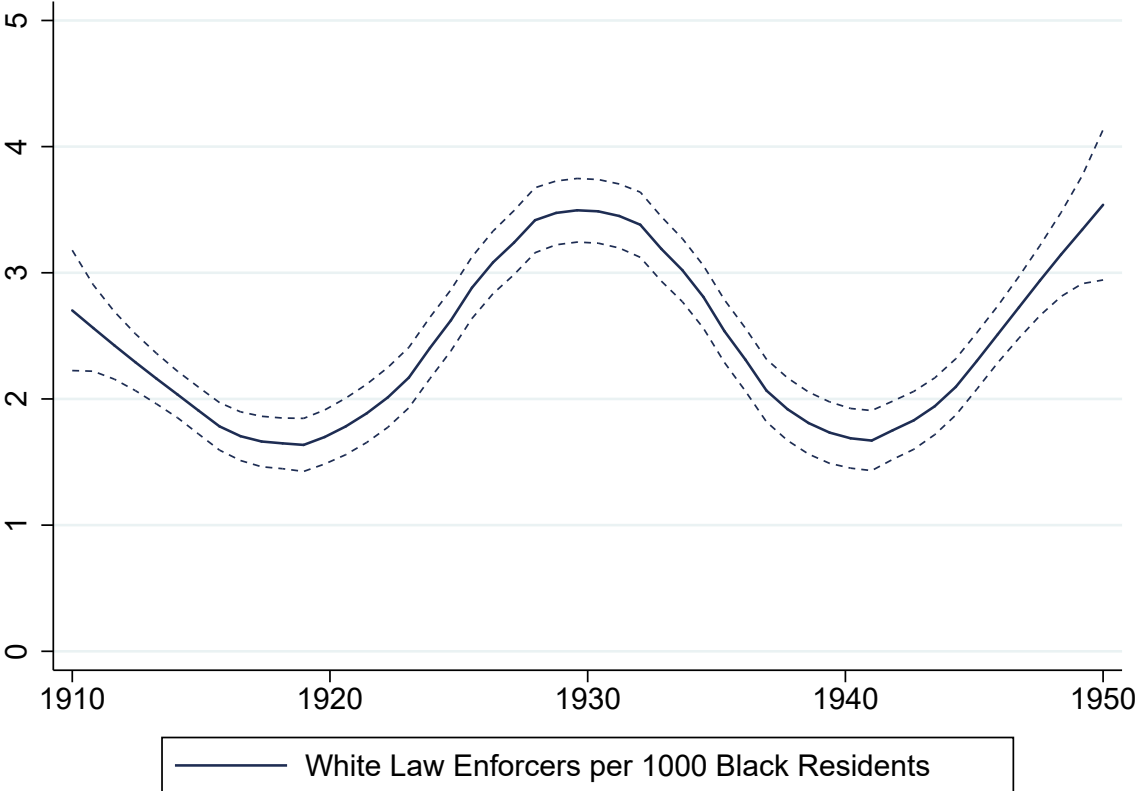


Figure A.4: Percent Suicide by Firearm, by State and Era

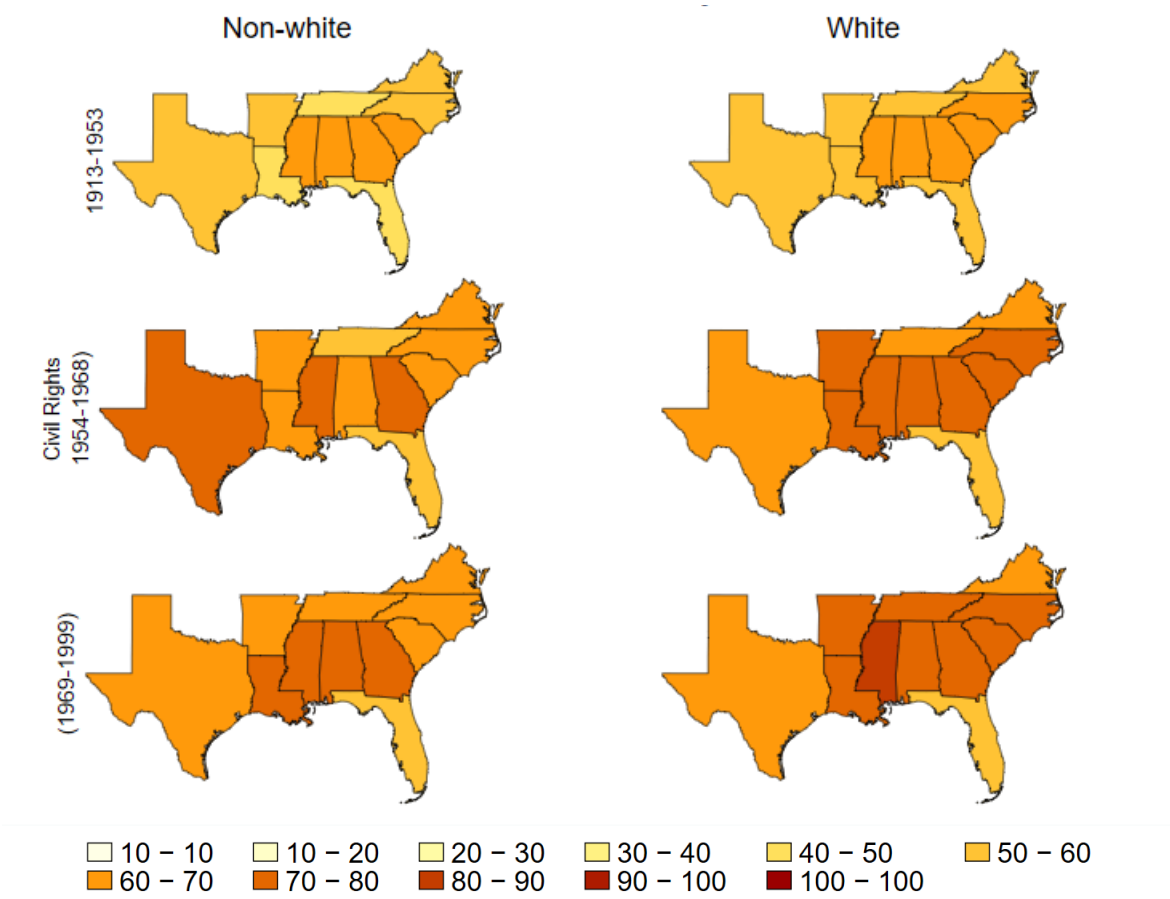
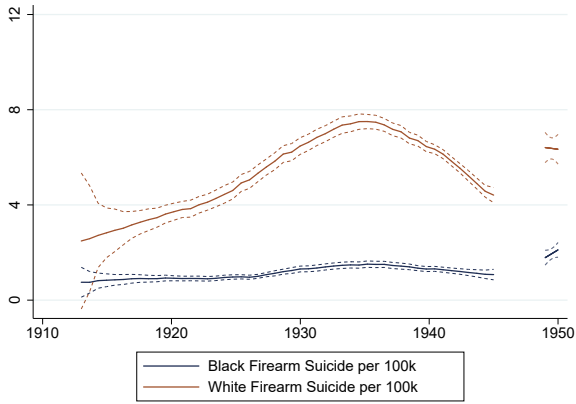
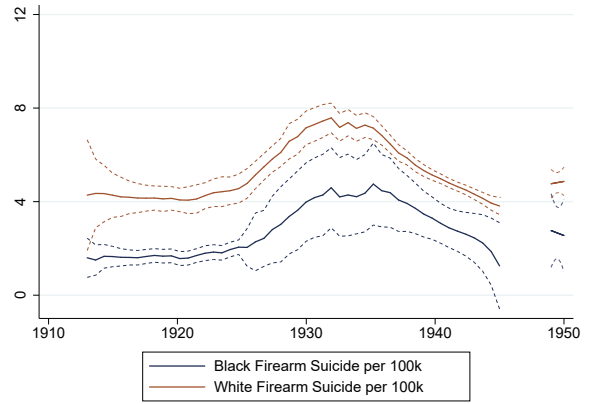


Figure A.5: Firearm Suicide Death Rates per 100k Residents by Race



(a) Former Confederate



(b) Non-Confederate

Table A.1: Summary Statistics: Control Variables

	mean	sd	min	max
Black % Farmers	143.66	75.19	33.28	370.76
Black Doctors per 1000 Black capita	0.25	0.23	0.00	1.22
Black Firemen per 1000 Black capita	0.05	0.07	0.00	0.31
Real Total Capital per Worker	39763.40	16997.01	15804.65	99627.51
Real Services Capital Stock	76891.52	17311.88	48104.86	143193.97
Real Farm Capital Stock	8074.65	3013.95	4027.82	19275.36
Real Manufacturing Capital Stock	16724.36	5619.64	6297.89	35746.30
Mean Real Black Wages	2295.85	644.97	1219.94	4315.94
Mean Real White Wages	5778.39	1107.19	3385.28	8493.58
Non-White %	0.33	0.10	0.16	0.53
Observations	317			

Table A.2: Sample Sensitivity: Black Lynching Deaths in Former Confederate States: 1913-1950

	1913-1950		-1949		-1948		-1947		-1946	
PSF <sup>Black</sup>	-0.993 (0.924)	-1.246** (0.571)	-0.995 (0.923)	-1.275** (0.569)	-0.985 (0.921)	-1.299** (0.562)	-0.994 (0.924)	-1.344** (0.559)	-0.992 (0.920)	-1.337** (0.562)
PSF <sup>White</sup>	-0.659 (0.885)	-1.048 (1.146)	-0.673 (0.872)	-1.075 (1.144)	-0.759 (0.871)	-1.169 (1.133)	-0.778 (0.860)	-1.210 (1.131)	-0.778 (0.860)	-1.228 (1.133)
<i>Model</i>	<i>Poisson</i>	Negative Binomial	<i>Poisson</i>	Negative Binomial	<i>Poisson</i>	Negative Binomial	<i>Poisson</i>	Negative Binomial	<i>Poisson</i>	Negative Binomial
<i>Trend</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
<i>StateFE</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
<i>YearFE</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
<i>Controls</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
<i>N</i>	317	317	307	307	297	297	287	287	277	277

**Note:** All specifications estimate an model for the count of Black lynching deaths within a state and year: columns 1-3 report a Poisson regression model with state and year fixed effects, columns 4-6 a negative binomial regression model conditional on state with year fixed effects. All estimations include state and year fixed effects, state-specific linear time trends, and include the following covariates: log mean Black and White wages; log total population; Non-White percentage of population; Black Doctors, Firemen, and Farmers per Black capita; and real capital per worker, both as totals and separately estimated within farming, manufacturing, and service sectors. Includes all member states from the Confederacy except Texas (insufficient data).

Table A.3: Control Function Poisson w/ Instrumental Variables: Black Lynching Deaths in Former Confederate States, 1913-1950

	1913-1950		-1949		-1948		-1947		-1946	
	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage
$PSF^{Black}$		-8.818*** (2.672)		-7.470*** (2.434)		-7.327*** (2.615)		-6.878*** (2.648)		-7.575*** (2.869)
$Law_{st}^{White}/Pop_{st}^{Black}$	-0.085*** (0.019)		-0.096*** (0.023)		-0.097*** (0.024)		-0.097*** (0.026)		-0.091*** (0.027)	
$\epsilon_{st1}(control\ func.)$		7.942*** (2.231)		6.585*** (1.994)		6.454*** (2.171)		5.986*** (2.214)		6.688*** (2.479)
$PSF^{White}$	-0.012 (0.102)	-0.500 (0.845)	-0.035 (0.117)	-0.655 (0.821)	-0.035 (0.118)	-0.727 (0.826)	-0.027 (0.117)	-0.699 (0.826)	-0.003 (0.111)	-0.534 (0.848)
$F$	20.22		18.11		16.40		14.28		11.75	
$Trend$	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
$StateFE$	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
$YearFE$	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
$Controls$	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
$N$	317	317	307	307	297	297	287	287	277	277

**Note:** First- and second-stage results from instrumental variables estimation of the effect of Black Firearms on lynching. First stage estimates a state-year panel regression and the inclusion of  $Law_{st}^{White}/Pop_{st}^{Black}$  as a restricted variable. The second stage is an Poisson maximum-likelihood estimated regression which include  $\epsilon_{st1}$  as a control function for variation in  $PSF^{Black}$  otherwise correlated with the error term. All estimations include state and year fixed effects, state-specific linear time trends, and include the following covariates: log mean Black and White wages; log total population; Non-White percentage of population; Black Doctors, Firemen, and Farmers per Black capita; and real capital per worker, both as totals and separately estimated within farming, manufacturing, and service sectors. Includes all member states from the Confederacy except Texas (insufficient data).

Table A.4: Control Function Poisson w/ Instrumental Variables: Black Lynching Deaths in Former Confederate States, 1913-1950

	pistol <sub>st</sub> × P <sub>t</sub> <sup>SG</sup>						law <sup>White</sup> × P <sub>y</sub> <sup>SG</sup>					
	-1950		-1949		-1948		-1950		-1949		-1948	
PSF <sup>Black</sup>	-9.581***	(3.567)	-10.886**	(4.620)	0.669	(5.667)	-11.662***	(2.577)	-18.051***	(3.675)	-15.566***	(3.108)
pistol <sub>st</sub>	-0.277***	(0.081)	0.411*	(0.248)	-0.178	(0.102)	0.557**	(0.265)	-0.297**	(0.110)	0.211	(0.229)
pistol <sub>st</sub> × P <sub>t</sub> <sup>SG</sup>	0.003***	(0.001)	0.002**	(0.001)	0.003**	(0.001)						
PSF <sup>White</sup>	0.080	(0.088)	0.295	(1.030)	0.070	(0.094)	0.315	(1.038)	0.072	(0.090)	-0.489	(1.130)
law <sup>White</sup>									0.054	(0.082)	0.410	(1.005)
law <sup>White</sup> × P <sub>y</sub> <sup>SG</sup>									-0.159***	(0.034)	-0.258	(0.304)
ε <sub>st1</sub> (CF)	8.652**	(3.620)	9.937**	(4.753)	-1.656	(5.874)			10.847***	(2.749)	17.233***	(3.935)
F	21.22		5.81		10.53		9.33		4.28		3.22	
Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
StateFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
YearFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	277	277	267	267	257	257	277	277	267	267	257	257

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**Note:** First- and second-stage results from instrumental variables estimation of the effect of Black Firearms on lynching. First stage estimates of  $PSF^{Black}$  include state bans on pistols ( $pistol_{st}$ ), White police officers per Black capita  $Law_{st}^{White}/Pop_{st}^{Black}$ , and each's interaction with real shotgun prices. The second stage is an Poisson maximum-likelihood estimated regression which include  $\epsilon_{st1}$  as a control function for variation in  $PSF^{Black}$  otherwise correlated with the error term. All estimations include state and year fixed effects, state-specific linear time trends, and include the following covariates: log mean Black and White wages; log total population; Non-White percentage of population; Black Doctors, Firemen, and Farmers per Black capita; and real capital per worker, both as totals and separately estimated within farming, manufacturing, and service sectors. Includes all member states from the Confederacy except Texas (insufficient data).