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The Strange Career of Jim Crow: Labor Scarcity and Discrimination in the American South

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THE STRANGE CAREER OF JIM CROW: LABOR SCARCITY AND DISCRIMINATION IN THE AMERICAN SOUTH*

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Abstract:

By economic theory, competitive markets should eliminate taste-based discrimination in the labor market. In this paper, I test whether market forces can similarly affect non-market discrimination. Specifically, I estimate the effects of relative labor scarcity on racial violence and political participation in the American South from 1865 to 1900. Random variation in labor scarcity across Southern counties was created by differential troop losses in the American Civil War. I find counties with 10 percentage-point higher death rates in the Civil War had 24-33% fewer lynchings of African Americans from 1866 to 1900. They also had 3.6-5.6% higher voter turnout. These effects persisted for at least two decades after the counties' relative labor scarcity disappeared. In the very long run (100 years), however, these counties saw much worse discrimination than average, possibly due to their much larger black populations. This suggests relative levels of discrimination were not culturally determined and can change fairly quickly.

JEL Codes: J15, N31, N32

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1. Introduction

By economic theory, competition among producers should eliminate taste-based discrimination in the labor market.[1] Booker T. Washington (1895) echoed the sentiment in his Atlanta Compromise: "No race that has anything to contribute to the markets of the world is long in any degree ostracized." While this is an appealing benefit of competition, some of the most damaging acts of discrimination occur outside of markets, including violence, exclusion from political processes, and limited access to public goods. In the American Civil Rights Movement, for example, wage inequality was a secondary issue to voting, access to public spaces and education, and equal protection by the justice system. Similar non-market discrimination persists against minority groups throughout the world, with prejudices especially pronounced in parts of the Middle East, South and Southeast Asia, and Africa (World Values Survey, 2014).[2] While competition may reduce taste-based discrimination in wages and employment over time, it is unclear whether market forces hold any sway on these more serious acts of non-market discrimination.

In this paper I test whether a specific market condition, a labor supply shock, can affect non-market discrimination. To test this, I use data from the Postbellum American South (1865-1900), a setting with substantial discrimination against African Americans. Previous to this period, the South had an enormous labor supply shock, losing one-in-four white males from its labor force in the

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[1] Becker (1957/1971) first argued that, given a sufficiently large number of minority workers, employers can only satisfy a taste for discrimination at a positive cost to themselves. Arrow (1972, 1973) and others extended the model to show that discriminating firms will exit in a competitive market. Taste-based discrimination can persist in models with increasing returns to scale, under imperfect competition, imperfect information (Black, 1995; Lang & Lehman, 2012), or posted wage offers (Lang, Manove, & Dickens, 2005). Still, "The notion that employer prejudice is 'driven out of the market' in the long run remains a staple of most textbook treatments of the employer prejudice model," (Charles & Guryan, 2008). Residual discrimination in many current markets is thought to be *statistical* (Arrow, 1998; Aigner & Cain, 1977; Altoniji & Black, 1999; List, 2004).

[2] Based on survey respondents that answered "people of another race" or "people of another religion" in response to a question on who they would not want as neighbors.

American Civil War (1861-1865). I estimate the effect of Southern counties' labor scarcity, which was randomly determined by military deaths in the Civil War, on 1) racial violence (lynchings) and 2) black political participation (voting) from 1865-1900.

Labor scarcity did in fact lead to decreased discrimination on both margins. In terms of violence, counties with greater labor scarcity (higher Civil War deaths) saw far fewer lynchings: estimations suggest counties with 10 percentage-point higher death rates in the Civil War had 24 to 33% fewer lynchings from 1866-1900. In terms of political participation, counties with 10 percentage-point higher death rates had 3.6-5.6% higher voter turnout from 1868-1900. This is particularly surprising since counties with higher death rates had more eligible voters that were black—all else equal, this should have resulted in lower voter turnout due to intimidation and informal restrictions against black voters (Jones, Troesken, & Walsh, 2012).

To measure labor scarcity, I use new data on county-level death rates for the Confederate Army. The results have a causal interpretation due to the random nature of the death rates. Unlike modern armies, Confederate companies (units of roughly 100 men) were raised at the city or county level, making them geographically homogenous. Neighboring counties with similar initial characteristics thus experienced very different death rates as their young men were sent to different battles and campaigns. As Shelby Foote noted, "You do have a big problem when you have units that are from states, and counties, and even towns, and one of those regiments can get in a very tight spot in a particular battle, like in the cornfield at Sharpsburg, and the news may be that there are no more young men in that town...they're all dead," (Burns, 1990).

Generals made troop assignments for strategic reasons which were uncorrelated with counties' pre-war characteristics. Confederate armies were also very large, generally comprised of hundreds of companies from several states—one county's characteristics would not have any effect

on generals' decisions for armies this large. For robustness, I also generate predicted death rates using data on the actual battles in which each company fought and use them in 2SLS estimations. These predicted death rates purge the estimates of any unobserved factors not related to the generals' decision-making, such as a county's pre-war health conditions, which might have affected deaths from disease.

Instead of market forces, many believe governments to be the only relevant agents in changing discrimination. There is strong evidence that government actions have reduced discrimination (Collins, 2001; Donohue & Heckman, 1991; Wright, 2013). Darity & Mason (1998) argue that in the U.S., most of the black-white income convergence of the last century occurred in from 1965-1975, the decade immediately following the Civil Rights Act of 1964. They further argue that competitive forces did little to improve the standing of black workers before or since. This paper, then, gives strong empirical evidence that market forces can in fact reduce discrimination in addition to government efforts. Jha (2013) finds a similar result in South Asia, where economic necessity led to better Hindu-Muslim relations in port cities. Voightlander and Voth (2012) show German cities more heavily involved in international trade saw greater decreases in anti-Semitic beliefs over time, though they ascribe the improvements to an increased exposure to different beliefs, not market forces.[3]

In addition to the results in the decades immediately following the Civil War, I also evaluate changes in non-market discrimination in the longer run (at least 15-35 years after the war). Labor forces across counties were of similar sizes by 1880 (Larsen, 2015), yet decreased violence and

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[3] Clingingsmith, Khwaja, and Kremer (2009) similarly show that participation in the Hajj, an Islamic pilgrimage, leads to more tolerant beliefs about other religious groups, though again the authors argue this results from a change of beliefs, in this case due to religious teachings of peace, not due to commercial interactions.

increased voting continued at least through 1900. Thus changes in discrimination were not immediately removed once the excess labor demand was satisfied. This may suggest a shift in these counties' racial norms—transitory changes in racial treatment may outlast the market forces that led to the improved treatment. Jha (2013) finds a similar result, with increased religious tolerance lasting for centuries after the economic necessity of the groups' interactions had eroded.

However, in the very long run (100 years after the Civil War), the improved racial outcomes were reversed. Voting from 1920-1960 and black voter registration in 1960 were much lower in previously labor-scarce counties. Racially motivated murders in from 1950-1970, including murders of civil rights workers of both races, were 55% more likely in counties with 10 percentage-point higher death rates in the Civil War. These counties were also more likely to refuse to integrate their schools, precipitating court actions.

These results are surprising given the earlier findings. While I cannot give direct evidence on a channel through which the reversals occurred, increased discrimination may have arisen due to the larger black populations in the previously labor-scarce counties. Black populations grew dramatically in these counties, with larger black populations lasting through the entire 20th century (Larsen, 2015). Becker (1957/1971) predicts that tasted-based discrimination by employers increases in the size of the minority population as the marginal employer of minority workers will be more prejudiced. In the political science literature, the "Racial Threat" hypothesis (Key, 1949; Blalock, 1967) posits that racism increases with the size of the underprivileged group as the majority group feels a greater threat to their power.

While these reversals were unfortunate, they strongly reject the idea of racial treatment being constant and culturally determined in the U.S. South—in just 2-3 generations, the same areas that had prevented lynchings were killing Civil Rights workers. This contrasts strongly with the

findings of Voigtlander and Voth (2012) and Jha (2013) who find an extreme persistence of cultural traits, lasting several centuries.

The results further contribute to an important historical argument, put forth by C. Vann Woodward in *The Strange Career of Jim Crow* (1955/1974). Woodward gives a preponderance of evidence against the then-prevailing notion that the Southern racial system on the eve of the Civil Rights Movement was as it had always been:

> "…things have not always been the same in the South. In a time when the Negroes formed a much larger proportion of the population than they did later, when slavery was a live memory in the minds of both races…the race policies accepted and pursed in the South were sometimes milder than they became later. The policies of proscription, segregation, and disfranchisement that are often described as the immutable 'folkways' of the South, impervious alike to the legislative reform and armed intervention, are of a more recent origin…the belief that they are immutable and unchangeable is not supported by history (1955/1974, p. 65)

While Woodward is widely read, and his work had significant bearing on the repeal of the Southern system—Martin Luther King called the book "the historical bible of the Civil Rights Movement" (Woodward, 1974)—his theory remains controversial. Many historians argue Woodward's anecdotes of better racial treatment prior to 1900 run counter to the broader trends (see discussion in Rabinowitz, 1988). Furthermore, they argue that the system codified by 1900 was merely a formal manifestation of the *de facto* discrimination that was already ubiquitous in the South. While I cannot take a stand on changes in aggregate discrimination in the region, the dramatic changes in the relative levels of discrimination across counties point to some flexibility in the racial system of the Postbellum South. This could suggest, as Woodword argues, that the Jim Crow system did not flow directly from the slave order and it was not a direct embodiment of the *de facto* discrimination already existing in the region.

2. Historical Background

2.1 Discrimination in the Postbellum South

The American Civil War (1861-1865) and the 13th Amendment (1865) ended slavery in the United States, but it took much longer for the South to remedy its system of racial castes. By the turn of the century, blacks' voting rights had been curtailed and nearly every facet of public life became segregated, both through informal norms and formal "Jim Crow" laws. That the system was economically damaging for African Americans has been shown in terms of limited educational opportunities (Margo, 1990), voting rights (Naidu, 2012), occupational mobility (Wright, 1986), and restricted migration for employment (Naidu, 2010). The Southern black population further suffered unequal treatment by the justice system, lack of access to public goods, a racial etiquette of deference towards whites (Alston & Ferrie, 1993), and considerable violence.

During Reconstruction (1865-1877), the federal government intervened directly in Southern politics. Among other things, this ensured African American participation, with many men even holding offices, especially in South Carolina and Mississippi. Troops also enforced restrictions on voting and office-holding for former Confederate supporters, severely handicapping the power of landowners (Foner, 1990).The Freedmen's Bureau also provided African Americans with legal assistance, arbitrated wage disputes, subsidized school construction, and provided some measure of protection from violence (Foner, 1990). By the 1870s, however, federal interest in the South waned and "Redeemers" were elected throughout the South, restoring much of the pre-war order. The removal of federal troops from the region was finally secured in 1877 in exchange for Louisiana, Florida, and South Carolina's electoral votes, pushing Rutherford B. Hayes into the White House (Woodward, 1971).

Racial relations between Reconstruction and 1900 may still have been better in some respects than the period afterwards, however (Woodward, 1955/1974). Public accommodations, railroad travel, and other services that became hallmarks of the Jim Crow system were not uniformly segregated before 1900, with considerable variation across states and localities (Woodward, 1955/1974). Many African Americans were also able to buy property during this period, and despite very low levels of income in 1865, made significant economic progress by 1900 (Higgs, 1977a). In sum, African Americans faced significant prejudice in the Postbellum South, but there was considerable variation in discrimination prior to 1900.

2.2   Southern Labor Markets

The Civil War also altered Southern labor markets. The war resulted in over 750,000 deaths (Hacker, 2011), more than all other American wars combined. The South bore a disproportionate number of the fatalities, losing at least one-in-four white men of military age (Vinovskis, 1989). Overall the former Confederate states lost 4% of their population, a loss greater than any nation experienced in WWI (McPherson, 1996), and the largest mortality event in American history.[4]

With few migrants from outside the region, communities with particularly heavy losses were left to hire workers from neighboring areas. The majority of these migrants were African Americans: by 1880, black populations were significantly larger in counties with more Civil War deaths, and the margin remains in place today (Larsen, 2014). Figures 1 and 2 show the growth of counties' black and white populations in the decades following the war.

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[4] The Spanish Flu epidemic of 1918 cost the United States roughly .6% of its population (U.S. Department of Health & Human Services, 2014). WWII and the Vietnam War cost the United States .3% and .03% of its population respectively (Chambers, 1999). Singer & Small (1972) also find that in 93 wars between 1816 and 1965, there were few cases where battle deaths exceeded even 2% of the pre-war population.

3. Data

All findings in this paper exploit variations in labor scarcity in Southern counties following the American Civil War. Labor scarcity is measured using new data I have constructed on county-level deaths in the Confederate Army. From a variety of sources, I have counted deaths for seven of the eleven former Confederate States: Alabama, Arkansas, Florida, Georgia, Louisiana, South Carolina, and Virginia. Death rates are reported as the total military deaths attributed to a county divided by the number of military-eligible men in the county's population (white males aged 15-39 in the 1860 Census). Variations in death rates for Georgia, Louisiana, and Virginia can be seen in Figures 4-6, ranging from less than 5% to over 50% of the military-eligible population in some counties.

Producing county-level death rates involves counting deaths by Confederate company (a unit of roughly 100 soldiers) from existing military records and then mapping them back to counties of origin. Unlike modern armies, Civil War units were raised locally, so nearly all Confederate companies can be connected to a county of origin. A majority of companies even carried geographic designations in their nickname, such as the Richmond Greys (from Richmond, Virginia) and the Bartow Yankee Killers (from Bartow County, Georgia), evidencing the common place of origin for the soldiers in the unit. More detail on the dataset and its construction can be found in the appendix.

The distribution of death rates across counties has a wide variance due to the recruitment pattern, as seen in Figure 3. For example, Company F of the 26th Regiment North Carolina Infantry

experienced 100% casualties during three days at Gettysburg (McGee, 2014). [5] [6] Nearly all of those

men are documented as living in Caldwell County, which was sparsely populated, before the war.

Variations in such losses were randomly allocated across Southern counties. The death rates

were driven by troop movements, made by generals for strategic reasons unrelated to the counties'

pre-war conditions. Aside from strategic considerations, many battles and the level of casualties that

occurred were random, not determined by the generals' planning. To cite a prominent example, in

1863, Confederate president Jefferson Davis wanted Robert E. Lee to send part of his army to relieve

Vicksburg, Mississippi, then under siege by U. S. Grant. Lee instead persuaded Davis to let him

invade Pennsylvania. Once his army traveled north, however, the cavalry distanced itself to the

point that Lee had no idea where the Federal troops were. The two armies met by chance as a

column of Southerners fell on Gettysburg in search of a rumored cache of shoes, leading to three

days of fighting and the highest casualties of any battle in American history.

Most movements were also made at the "army" level, units comprised of hundreds of

companies from many different states. At Gettysburg, Robert E. Lee commanded a few thousand

companies from every Confederate state plus Missouri, Kentucky, and Maryland. One county's

characteristics could not have affected generals' decisions in an army this large. Table 1 shows

further that death rates were uncorrelated with pre-war county characteristics as measured in the

1860 census, including wealth, manufacturing output, population, and the percent of the county's

population that were slaves. The death rates, then, are randomly distributed and allow for causal

inference in the analysis.

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[5] Regiments were units of 10-12 companies, designed to have at least 1,000 men. Companies within regiments almost always moved together, thus the regiment was the smallest unit of troops for which generals made assignments.

[6] Casualties include those killed, wounded, or captured (many wounded or captured died later as well).

One concern is that some units were exposed to more battles because of greater "skill." A few units did gain reputations as good fighters during the war, but it is unclear whether these were actually superior to other units in any way. For example, the famed "Stonewall Brigade," originally commanded by Stonewall Jackson, was perhaps the most celebrated unit in the Confederate Army. Despite its initial reputation, however, it performed poorly in many later battles, both with and without its famous commander (Foote, 1958). Even if such a unit were chosen for assignments based on its perceived skill, this could not have been driven by an unobserved characteristic of a single company out of hundreds.

Another concern is that the death rates mis-measure the true treatment in counties with larger black populations. In such counties, the death rates (measured as a percentage of white men) would not have created as significant scarcities of labor. This measurement error should simply attenuate the estimates since the measured treatment was greater than the actual treatment for such counties. Alternately, I could construct death rates as the percent of a county's total population, white and black, that died in the war, which would measure the treatment more accurately. However, since nearly every Confederate soldier was white, such a measure would then be strongly correlated with the relative sizes of the white and black populations. This in turn could be correlated with a host of unobservable factors, especially those present in plantation counties. For this reason, I use the percent of the county's white, military-eligible population that died in the estimations, a measure that is uncorrelated with the relative sizes of the black and white populations.

For robustness, death rates are also predicted using data on the actual battles each company fought, listed in the Stewart Sifakis' *Compendium of Confederate Armies* for each Southern state. This purges the death rates of any unobserved factors not related to the generals' decision-making, such as a county's pre-war health conditions. In practice, the predicted death rates are very close to the

actual death rates for most counties, suggesting that even deaths from disease in the war were driven by factors such as battle wounds, the length of soldiers' marches, and the intensity of fighting they took part in.

A further advantage of the Civil War data is that, while both capital and labor were destroyed in huge amounts, the destruction of individual counties' capital and labor were independent of each other. Soldiers who died on battlefields often came from far away while cities destroyed for strategic reasons were largely spared civilian casualties. This allows their effects to be estimated separately, whereas similar research generally lumps them together as wartime destruction.

4. Relative Labor Scarcity and Discrimination 1865-1900

I test the effects of labor scarcity in the Postbellum American South on non-market discrimination in two categories: protection from violence (lynchings) and political participation (voter turnout). These measures are intended to proxy for the *de facto* racial treatment in Southern counties. Official voting restrictions, such as poll taxes and literacy tests, were not common until the 1890s, so preventing African Americans from voting usually involved completely extralegal measures, such as intimidation and violence. Lynchings were similarly outside of the law.

While more direct measures of discrimination would be preferable, I use these proxies since they are the only relevant series with county-level variation in the time period of which I am aware. Lynchings tended to be well-reported, often showing up even in Northern newspapers. They do, however, represent a very extreme form of racial violence, producing noisy estimates of the true

levels of racial violence in a county. For political participation, voting totals survive for large

elections, though voter registration and characteristics of the voters, such as race, do not. Still, the

lynching and voting data are remarkably consistent data series for the time period and each proxy

for different aspects of the non-market discrimination that occurred in Southern counties.

4.1   Results—Violence towards African Americans, 1866-1900

4.1.1   Baseline Specification

Lynchings clearly had racial motivations. While whites were often lynched in the South,

more than 90% of the documented lynchings were of black men (HAL, 2014). Victims were usually

accused of some crime and the lynchings were intended as a means of vigilante justice, but

convictions were often dubious and sometimes trials were never held. Regardless, lynchings served

to remind the black community of their "place" in the Southern system. Jones, Troesken, & Walsh

(2012) show that lynchings were an effective deterrent to black voting, for example. They were also

intended to enforce racial norms on interracial relationships, specifically between black men and

white women (Dray, 2002).

Data on lynchings were first collected by Monroe Work at Tuskegee Institute (NAACP, 1919)

and have been added to by the Historic American Lynchings (HAL) Project by Hines & Steelwater

(2014). Records indicate the victim's name and several details for each lynching, including the year

and county where it occurred. This data is widely used, but does not cover Virginia and only begins

in 1881. I supplement the HAL data with lynchings in Virginia beginning in 1880 from Brundage

(1993). I also include lynchings reported in Northern newspapers from 1866 to 1880 using a Proquest

Historical Newspaper search. Since most counties have no lynchings in a given year, I aggregate the

data over the period from 1866 to 1900, by which time the Jim Crow system became ubiquitous (Woodward, 1955/1974). Total lynchings in this period are divided by the county's average black population. They are reported as lynchings per 1,000 in the black population.

The estimations include region fixed effects (for the Appalachian, Piedmont, Coastal Plain, and Coastal regions) by state as well as a variety of other county-level covariates. The estimating equations are of the form:

$$\left(\frac{\boldsymbol{Lynchings}}{\boldsymbol{Black\ Population}/\boldsymbol{1000}}\right)_{ir} = \boldsymbol{\beta_0} + \boldsymbol{\beta_1}(\boldsymbol{Death\ Rate_{ir}}) + \boldsymbol{X_{ir,1860}} + \boldsymbol{\beta_3\alpha_r} + \boldsymbol{\varepsilon_{irt}}$$

for counties i in state-regions r. Data comes from counties in Virginia, South Carolina, Georgia, Alabama, Florida, Louisiana, and Arkansas.

Table 2 shows the effects of counties' death rates in the American Civil War on lynchings from 1866-1900. Due to the distribution and number of zero observations in the lynching data, I use Poisson regressions. In the baseline specification (Column 1), the estimated coefficient for the death rate's effect is -3.341, significant at the 10% level. Accounting for the logarithm used in a Poisson regression, a 10 percentage-point increase in the death rate (roughly one standard deviation) translates to a 28.4% reduction in lynchings at the mean. Across all specifications, a 10 percentage-point increase in the death rate is estimated to decrease lynchings from 24 to 33%. Many counties in the data had death rates that differed from neighboring counties by much more than 10 percentage points, so the estimated effects are very large.

In Column 2 I estimate the effect of labor scarcity on lynchings using a negative binomial regression, which relaxes the assumption that the distribution has a mean equal to its variance. The results change very little. In Column 3 I drop all counties with an urban population in the 1860 (pre-treatment) Census since lynchings were mostly a rural phenomenon. This raises the magnitude of

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the coefficient, which is significant at the 5% level. In all the remaining columns, I use a Poisson regression while omitting the urban counties. Most of the South was rural, however, so this does not greatly reduce the sample.

Column 4 estimates the effect of Civil War death rates on lynchings for only the Piedmont and Coastal Plain regions. While there was significant variation in death rates within the Appalachian and Coastal regions, their average death rates were much lower than the other two regions. In the Appalachians there was somewhat lower military participation, though participation was still very high. In coastal counties there was greater participation in blockade running, then Confederate Navy, and home guard units (which "guarded" their home counties, in this case the Confederate coastline, but rarely participated in battles). Reducing the sample thus removes potential biases from the Appalachian and Coastal regions' different experiences in the war. The estimated coefficient in Column 4, however, is very similar to earlier estimates.

Column 5 includes several pre-treatment county characteristics measured in the 1860 Censuses of Population, Agriculture and Manufacturing (Haines, 2010). These include the percent of the population that were slaves, per capita wealth, per capita manufacturing output, rail access, and cotton, corn, and tobacco output. These measures proxy for a variety of different pre-war county types. The results are again unchanged.

Military participation rates present a more serious concern. If counties' participation rates reflected their pre-war characteristics, such as pro-Southern or pro-slavery sentiments, estimates would be biased. Such a bias would likely be positive, however, and simply attenuate the estimated effect of the death rates. In practice, military participation rates were very high throughout the South

and varied little within regions.[7] Still, the regression reported in Column 6 controls for military participation rates as well as desertion rates and two proxies for capital destruction in the war (the number and size of battles that occurred in the county). The results are very similar to the other estimations.

Alston & Ferrie (1993) have shown that lynchings were much less common in plantation counties. Though these counties had worse racial treatment on most margins, paternalist landowners offered protection from violence as a benefit to prevent their low-wage workers from migrating. To ensure that the results are not simply showing the effects of paternalism, in Column 7 I add a variable for plantation counties. For simplicity, I use an indicator variable for whether or not slaves made up 40% or more of the county's 1860 population. Using the actual percentage of a county's population that were slaves (a continuous variable) does not affect the estimates. I also include an interaction term to test for a differential effect of labor scarcity on lynchings in plantation counties. Not surprisingly, plantation counties had a strong negative effect on lynchings, as Alston & Ferrie predict. The interaction term suggests the death rate may have had little effect on lynchings in these counties as well. The main effect of death rates on lynchings, however, changes very little — the effect of a 10 percentage-point increase in the death rate on lynchings, absent paternalism, is estimated at −33.2%. This suggests the effects estimated in the previous columns are not driven by plantation counties.

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[7] The counties in Appalachia participated somewhat less due to ideological differences, with many men even fighting for the Union. The coastal areas had lower participation since many served in "Home Guard" units (which guarded the coast but were not incorporated into the traditional army), served in the Navy, or worked as blockade runners.

4.1.2   IV Results

A potential bias in the estimates remains due to deaths from disease, which might represent an unobserved county characteristic driving both the death rate and postwar outcomes. Over half of the deaths in the American Civil War occurred due to disease (Hacker, 2011).[8] These non-combat deaths were still heavily influenced by where troops served, the intensity of their fighting, marching, and other assignments, and injuries sustained in battle. All of these factors were still determined by commanding officers.

However, pre-war county characteristics could also influence the prevalence of disease. For example, Chulhee Lee (1997) shows that Union soldiers from rural areas died at higher rates than those from urban areas due to less prior exposure to several diseases. Lee's results should be less applicable in the South, however, as the South was almost completely rural, and the urban areas that did exist were smaller than Northern cities by several orders of magnitude.[9] Rural Southerners would have had greater pre-war disease exposure than their Northern counterparts as well, as nearly all the fighting took place in the South, in Southern environments and Southern climates. Regardless, Table 2 Column 3 shows the results while omitting counties with any urban populations in the 1860 census, and the results become stronger.

Other correlations between death rates and a county's unobserved health conditions before the war could still exist, however. To deal with this, I generate predicted death rates for each company based solely on the battles they fought. These predicted death rates are driven only by the

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[8] Hacker (2001) suggests disease was even more of a problem for Southern troops, who he estimates died from disease at rates 10-20% higher than those of the North. This was in large part due to their poorer rations and equipment. Many Confederate soldiers had no shoes, for example, and food was often in short supply.

[9] There were fewer urban areas as well—the Confederate States had only one city (New Orleans) among the nation's 20 largest in 1860, and only 11 of the top 100. Massachusetts alone had 18.

random nature of troop assignments. They will only be biased if generals' decisions were influenced by counties' pre-war characteristics, which is unlikely for the several reasons mentioned earlier. In addition to accounting for counties' pre-war health and disease exposure, the predicted death rates will also control for any other unobserved differences between counties that could have affected death rates, provided these factors were not correlated with the generals' decision-making.

Death rates are predicted using Sikfakis' (1995) *Compendium of Confederate Armies* for each state, which lists battles fought for each Confederate regiment. Due to the number of battles—Sifakis records hundreds—most are grouped into count-variables for small, medium, and large battles. Dummy variables for a handful of major battles are included as well. Formally, the estimating equation for companies j in branch k from a county i in state-region r is:

$$Deaths_{ijkr} = \beta_0 + \beta_1 Small_{ijkr} + \beta_2 Medium_{ijkr} + \beta_3 Large_{ijkr}$$
$$+ \sum_{l=1}^{N} \beta_l Major\ Battles_{ijkr} + \ln(Pop_{\cdot ir,1860}) + X_{ir,1860} + \alpha_k + \gamma_r + \varepsilon_{ijkr}$$

Military branches k include the infantry, cavalry, and artillery. Small, medium, and large battles are the total number of battles in those categories, where the battle sizes are determined by the number of troops present, not the degree of casualties. To construct predicted death rates by county, the deaths are summed over all the companies in the county and divided by the military-age population of white men:

$$\widehat{Death\ Rate}_{ir} = \frac{\sum_{j=1}^{n} \widehat{Death\ Rate}_{ijkr}}{Military\ Age\ Pop_{\cdot ir,1860}}$$

Figure 7 shows the actual and predicted death rates for counties in southwest Georgia.

The predicted death rates are included in a 2SLS regression, with the first stage being the prediction equation with all other covariates included. As the battle information is a very strong predictor of counties' death rates, the partial F stat is extremely high and not reported in the table.

Table 3 Column 1 shows the results of the 2SLS regression. In this case, the estimated effect of a 10 percentage-point increase in a county's Civil War death rate is a 32% decrease in lynchings, in line with the previous specifications.

4.1.3   Robustness

For robustness, Columns 2-5 of Table 3 explore some potential channels of causation that would not be consistent with market forces affecting discrimination. First, it could be that the size of the black population had increasing returns in preventing violence. This would suggest that larger groups of African Americans could protect themselves without any change in white behavior. In Column 2 I control for the size of the black population, and the results are unchanged. Alternately, it could be that the high Civil War death rates left these counties with fewer white men to carry out acts of racial violence. Column 3 controls for the size of the white population, and this does not appear to be the case. Labor scarcity might potentially have caused structural changes in counties' economies, and it could be that it was these structural changes and not forces in the labor markets that lead to decreased racial violence. In Column 4 I control for the growth of counties' manufacturing output from 1860-1900. In Column 5 I control for the change in counties' cotton output (the main export crop) and corn output (the main subsistence crop). The results in Columns 4 and 5 and very similar to earlier results, suggesting structural changes of these kinds are not driving the results.

4.2 Results—Political Participation, 1865-1900

4.2.1 Baseline Specification

A second proxy for racial treatment in the South from 1865-1900 is the county's participation levels in major elections. Total votes cast in presidential, congressional, and gubernatorial elections are available through the ICPSR's *United States Historical Election Returns, 1824-1968.* The log of total votes is regressed while controlling for the log of the voting-age population (males over 21), so the dependent variable can equivalently be thought of as voter turnout (as a percent of eligible voters). The estimations are equivalent to Frisch-Waugh regressions where the left-hand side variable is the residual from regressing the log of the voting age population on the log of total votes. The remaining right-hand side variables thus explain variations in voting behavior that cannot be explained by the size of the potential voting pool. Similar measures have been used previously by Naidu (2012) and Jones, Troesken, and Walsh (2012) as proxies for variations in black voter participation. Total black votes have been estimated for a handful of elections by using ecological inference (Redding & James, 2010). However, this method assumes a constant relationship between total votes and a county's black population. I, on the other hand, am testing for differences in voting behavior for a given black population.

I assume an increase in voter turnout represents at least some increase in black voting, though I cannot say how many of the additional votes are cast by black voters. It is likely that if these groups were in direct political competition, both would vote more if blacks were more able to vote. Thus some part of the increased voting would clearly come from increased white voter participation. However, if black voting were restricted, there would be no need for increased white voting, so any increase should be at least partially composed of black voters.

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It is possible, however, that labor scarcity affected voting through some other means, and thus an increase in voter turnout does not necessarily represent any increase in votes cast by African Americans. Such alternate channels would be structural changes that occurred in counties due to Civil War deaths, but not necessarily associated with any change in discriminating behavior. I attempt to control for as many of these channels as possible, such as the sizes of black and white populations, urban status, region and structural changes in the counties' economies. All of the voting results are also in line with those reported for lynchings, a proxy that measures discrimination more directly, albeit with considerable noise due to their less-frequent occurrence.

In the estimations, I use a pooled sample of each major election from 1868 to 1900 in Alabama, Arkansas, Florida, Georgia, Louisiana, South Carolina, and Virginia. Region fixed effects are again included by state. Election specific fixed effects (year fixed effects for each type of election) are also included by state. The baseline model is of the form:

$$log(Total\ Votes)_{irt} = \beta_0 + \beta_1(Death\ Rate_{irt}) + \beta_2 log(Voting\ Age\ Pop_{irt})$$

$$+X_{ir,1860} + \beta_3\alpha_r + \beta_4\gamma_t + \varepsilon_{irt}$$

for state-regions r and elections (state x office x year) t. The estimated effects of death rates on voting thus come from comparisons within state-regions and within individual elections. To allow for correlation in the error terms within regions and across time, I use multi-dimensionally clustered standard errors in the inference, clustered by state x region and election (office x year) (Cameron, Gelbach, & Miller, 2011). These standard errors perform well even with less than 40 clusters (Cameron, Gelbach, & Miller, 2011).

Table 4 shows the effects of counties' death rates in the American Civil War on electoral participation from 1868-1900. The baseline specification estimates a 5.2% increase in voting in counties with 10 percentage-point higher death rates in the Civil War, significant at the 1% level.

Overall, the results suggest a 3.6 to 5.6% increase in voting in counties with 10 percentage-point higher death rates.

Measuring these estimates against a null hypothesis of zero effect may not be the correct test, however. Counties with higher death rates experienced significant immigration of newly freed slaves, resulting in a larger share of their population that was black (Larsen, 2015). With this, the null hypothesis of the effect of the death rate being zero is almost certainly too high. Using data on electoral participation by race inferred for 1876 and estimated for 1880, 1892, and 1900 (Redding & James, 2010), black voter participation averaged 45% over this period while white voter turnout was very high at 72%. Combining this information with the estimated effects of death rates on the change in the black population (Larsen, 2015), I estimate a reduced form impact of a 1.3% decrease in a county's total votes with a 10 percentage-point increase in the death rate, assuming that the death rate affected only the size of the black population and nothing else. This is shown in Table 4 as the null hypothesis $\hat{\beta} > -.13$ and represents the expected outcome for voting if racial treatment were unaffected by the labor scarcity. 95% confidence intervals are also shown to illustrate the range of null hypotheses that could be similarly rejected.

Table 4 Column 2 estimates the effects in only the Piedmont and Coastal Plain regions, removing potential biases from counties in the Appalachian and Coastal regions. The results are slightly reduced, but still significantly above zero and well above the estimated null hypothesis of $-.13$. Columns 3 and 4 include the census and military controls explained previously, and the results change very little. Column 5 again drops the counties with urban populations in the pre-treatment period, with the magnitude of the coefficient increasing to .556, significantly different from zero at the 1% level.

Column 6 focuses the estimation on a time period that preceded the labor market's return to equilibrium. Elections included are from 1868 (the first year Southern states voted again in federal elections) to 1880, by which time the population difference across high and low-death counties is indistinguishable. Over this time period, the estimated effect of labor scarcity is even larger, suggesting the strongest effect when excess labor demand was greatest.

4.2.2   Robustness

Table 5 Column 1 estimates the effect of labor scarcity on voter turnout using the predicted death rates (2SLS). These again remove any potential biases in the death rates that were not correlated with generals' decision-making, such as a county's pre-war health conditions. The estimated effect of the predicted death rates is very similar to those of the actual death rates used in Table 4. Column 2 presents a falsification test, with the dependent variable being the log of total votes in the twelve years before the American Civil War (1848-1860). The death rates have no significant effect on these pre-treatment outcomes, indicating there was no different voting pattern in the high death-rate counties before the war.

The remaining columns in Table 5 repeat the further robustness checks and tests for alternate channels of causation that were performed for the lynching results in Table 3. Having a larger black population (Column 3), which could potentially lead to greater political power for African Americans or greater protection for black voters going to the polls, does not affect the results. Having a smaller white population (Column 4) due to higher death rates, which might leave fewer men to intimidate black voters, obstruct polls, or create other barriers to black voting, similarly leaves the results unaffected. Accounting for structural changes in counties economies (Columns 5-6) again leaves the results little changed.

Since death rates were distributed randomly, the results can be interpreted causally, but it is impossible to show that any of the additional voters in high-death counties were actually black. However, in Table 5 Column 7 I pursue an alternate strategy by instrumenting the size of a county's black population with the county's death rate. The effect of death rates on the change in black population is estimated in the first stage, and the effect of the change in black population on voting is estimated in the second stage. Because an IV regression produces a local-average treatment effect, the black population in the second stage is interpreted as the change in black population that was sensitive to variations in labor scarcity, which is exactly what I would like to estimate. This would include migrants, for example, but not the larger black populations in plantation counties. The coefficient, then, does not indicate that having a larger black population led to more voting in the South on average. Clearly this was not the case. Instead, it indicates the larger the black population that was influenced by the labor scarcity, the higher the voter turnout. I also estimate a new null hypothesis since one step in the previous calculation is accounted for in the first stage. From the data on actual voter participation by race, a 10 percentage-point increase in the fraction of potential voters that was black should reduce voting by 2.7%. While not significantly different from zero, the IV estimate is well above the estimated null hypothesis.

Unfortunately, it is difficult to determine whether the exclusion restriction has been satisfied for this estimation. The Civil War death rates are uncorrelated with unobservable county characteristics, but it is difficult to determine whether the full effect of labor scarcity on voting comes through these marginal black voters (and possibly the consequent increase in white voters to oppose them). As stated earlier, it is still possible that labor scarcity created other structural changes in the counties that in turn influenced voting. If such is the case, the exclusion restriction would not be satisfied.

4.3   Channels of Causation

I interpret the preceding results as causal impacts of labor scarcity on non-market discrimination in the Postbellum South due to the random nature of counties' Civil War death rates. However, the channel through which these effects come is still unclear. Military deaths and labor scarcity could have created several changes in these counties, some of which could have in turn led to decreases in lynchings and increases in voter turnout. I explore several potential channels below.

4.3.1   Non-Market Channels

As I have shown, several channels that are not consistent with market conditions affecting discrimination can be rejected. These include a larger black population, which might have offered some measure of protection or power to that community, and a smaller white population, which conceivably could have been less able to carry out acts of discrimination.

Some channels that might be considered economic, but not necessarily market forces, include structural changes and paternalism. Structural changes in the counties' economies, including growth of manufacturing and changes in cotton and corn output, are similarly unable to explain the results. Paternalism can explain decreased racial violence in plantation counties, but it seems to have been independent of the effect of Civil War death rates on discrimination.

Government action is another potential channel for the effects. In this case the Freedmen's Bureau may have been especially capable of preventing violence and aiding in voter turnout. If the Freedmen's Bureau was more active in counties with higher death rates, then this would explain the preceding results without market forces playing any role. However, in Section 5, I show that the previous results hold for the period 1880-1900 and perhaps beyond, long after the Freedmen's Bureau (1872) and Federal Troops (1877) had left the South.

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4.3.2   Market Channels

While I cannot take a strong stand on the exact channel through which the effects come, I interpret the results as improved treatment of African Americans by white Southerners due to labor market conditions. I consider three possible channels arising from labor markets:

First, employers could have actively reduced discrimination in their communities.[10] Minority workers prefer to live in areas with better racial treatment, negatively affecting firms located in more prejudiced areas (Dray, 2002). If employers were sufficiently powerful, they could have offered decreased discrimination as a fringe benefit to entice scarce minority workers. Alston & Ferrie (1993), for example, have shown Southern landowners were able to protect black workers and their families from racial violence. It seems plausible they could have allowed greater access to voting as well, if it benefited them economically. Given the extreme inequality in political and economic power in the Postbellum South, it is likely that employers (landowners) were involved in the observed decreases in discrimination at some level, whether they played an active or passive role.

Second, labor scarcity leads to decreased competition in labor markets. In the Postbellum South, such competition may have led whites to use violence to intimidate competing black workers and induce them to migrate (Christian, 2015). The demand for such racial violence would decrease with labor scarcity. It seems unlikely that this would occur without at least tacit agreement by the landowners, though.

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[10] There is evidence of employers removing taste-based discrimination from labor markets during this time period. Higgs (1977b) shows that for many professions in Virginia from 1900-1909, black and white workers earned similar wages, especially at integrated firms. Brown & Phillips (1986) show 19th century manufacturers in California were willing to substitute their preferred labor force of white women, who were scarce, for Chinese workers, who faced open prejudice in the community.

Lastly, labor scarcity may have simply led to higher wages. If acts of non-market discrimination are inferior goods, then this discrimination would necessarily decrease in areas with greater labor scarcity. High incomes within and across countries tend to be correlated with lower discrimination today, though there is no evidence of a causal link. This might be thought of as an extension of modernization theory, whereby economic progress creates social progress, thus creating a correlation between income gains and more enlightened views on race. Some women and minority workers tend to have poorer outcomes during recessions as well (Hogarth et al., 2009), though these results do not hold for all minority groups. This might indicate, among other things, that employers or co-workers have latent racial preferences that only become pronounced when incomes fall.

5. Persistence of Racial Treatment, 1881-1920

The previous section analyzes 1865-1900, the full period in which Woodward (1955/1974) suggests racial treatment still had considerable variance across the South. I now analyze the period from 1880-1900 separately to test for the persistence of racial treatment as a cultural norm. The size of the labor force across counties with different death rates converged by 1880, which suggests that economic considerations in such counties were likely no different beyond that point. I attribute effects that remained in 1880-1900 and beyond to an actual change in the racial norms, distinct from a short-term improvement in racial relations exploited for economic gain.

Table 6 repeats the main specification for the effect labor scarcity on racial violence for three time periods: 1881-1900, 1901-1920, and 1881-1920. Civil War death rates greatly reduced lynchings in the period after labor-market convergence (Column 1). The results are robust to the tests presented in the previous sections (not shown). While the negative effect from 1901-1920 (Column 2)

is not statistically significant, the effect over the period as a whole (1881-1920, Column 3) was very strong. From these estimates, the negative effect of Civil War death rates on racial violence clearly lasted beyond the initial period of labor scarcity (ending around 1880) and indicates some degree of persistence in racial treatment.

Columns 4-6 repeat the results for electoral participation. The estimate for 1881-1900 again indicates some persistence in racial treatment from the earlier period. The results are also robust to the tests presented in the previous sections (not shown). The effect does not remain after 1900, however. By this time, nearly every Southern state had installed significant restrictions against black voters, and it appears the constraints were binding—there was no significant effect of previous labor scarcity on voting behavior in these decades

6. Racial Treatment in the Very Long Run

The previous sections show that: first, counties with greater labor scarcity saw decreases in non-market discrimination from the end of the Civil War to 1880. Second, the effects lasted beyond 1880, with electoral participation higher in counties with higher death rates until 1900 and racial violence reduced in these counties through 1920. I now test the evolution of non-market discrimination through the Civil Rights Era (1950-1970).

Unfortunately, the beneficial effects of labor scarcity on discrimination not only disappeared in the very long run, they were actually reversed. Following the enactment of a host of state-level institutions to enforce White supremacy from 1890-1910, all the gains of the previous periods fell away.

Figure 8 shows the effects of death rates on electoral participation over 20-year intervals

starting in the antebellum period. The line plots the regression coefficients from the baseline

specification with 95% confidence intervals shown in dotted lines. As reported previously, there was

no effect of death rates on voting in the pre-treatment period (1848-1860, plotted at 1850 on the

graph). The strongest effect is found from 1868-1880 (plotted at 1870 on the graph), but there was

still a significant positive effect from 1881-1900. After that time the effect decreased and became

strongly negative and statistically significant from 1921-1940. The negative effect from 1941 to 1960

is also significant at the 10% level.

Other measures suggest the condition in the formerly labor-scarce counties was even worse

than Figure 8 indicates. Table 7 shows the effect of Civil War death rates on other indicators of

discrimination from 1950-1970. These proxies are not measured in earlier periods, so they are not

directly comparable to the earlier results in the same way that the voting data are. Still, they paint a

picture of significantly poorer racial treatment in the counties more affected by the Civil War. Panel

A shows the effect of Civil War death rates on the percent of non-white adults registered to vote in

1960. This data was collected by the U.S. Civil Rights Commission. Most Southern counties are

included, though only a fraction of the counties in Georgia were surveyed. Across all estimations, a

10 percentage-point increase in the death rate translates to a 22 to 31% reduction in black voter

registration, a very large effect.

Panel B shows the results for racially-motivated murders and murders of civil rights workers

(including whites) from 1950-1970 as reported by the Southern Poverty Law Center. Again, these are

not directly comparable to the lynchings data, but both are representative of the efforts made,

through violent intimidation, to suppress African Americans in these counties. Very few counties

had more than one murder, and those that did tended to have multiple murders in the same event,

such as the murder of three Civil Rights workers outside Philadelphia, Mississippi in the summer of 1964. For this reason, the dependent variable in the regressions is simply a 1 for those counties with one or more murders in this period and a 0 for those with none. Panel B reports the results from a linear probability model, though the results are robust to other specifications that bound the fitted values between 0 and 1. A 10 percentage-point increase in the Civil War death rate resulted in a 5.3 to 8.3 percentage-point increase in the probability of having a racially-motivated murder in the Civil Rights Era, a 43 to 64% increase at the mean.

I cannot directly account for the mechanism driving such large changes in these counties' relative levels of discrimination. The market forces that decreased discrimination following the Civil War had long-since evaporated, so other, possibly non-economic, factors caused the reversal. A plausible explanation is that the size of the black populations in the previously labor-scarce counties drove the changes. Black populations grew dramatically in these counties, with larger black populations lasting through the entire 20th century (Larsen, 2015). Two prominent theories predict greater discrimination in areas with larger minority populations. Becker (1957/1971) predicts that tasted-based discrimination by employers increases in the size of the minority population as the marginal employer of minority workers will be more prejudiced. This prediction which holds true empirically for the United States in recent decades (Charles & Guryan, 2008).

The results also follow an established pattern of discrimination from the political science literature, the "Racial Threat" hypothesis (Key, 1949; Blalock, 1967), which posits that racism increases with the size of the underprivileged group as the majority group feels a greater threat to their power. Significant evidence supports the racial threat hypothesis: "whites' negative racial attitudes increase with higher percentages of blacks in the county, metropolitan area, and state, and not just in the South," (Oliver & Mendelberg, 2000, p.574). These relationships may be more nuanced

and may not be causal (Oliver & Mendelberg, 2000), but they are a statistical regularity in the 20th century.

The significant changes in relative levels of discrimination over time lead to three important conclusions. First, they show that racial discrimination can change, and change quickly, even without direct government intervention. The results presented here contradict those of Voigtlander & Voth (2012) who show an extreme persistence of cultural traits, specifically anti-Semitic beliefs, over several centuries in Germany. Their own work shows, however, that areas with greater market interaction did in fact lose their racist beliefs over the period. In general, however, the contrast between Voigtlander & Voth's (2012) results and those presented here may come in the nature of the minority group. In the German case, Jews were a very small minority that was used as a scapegoat. Moreover, Jews did not even live in the prejudiced areas for long periods of time. In the Southern case, African Americans lived in all parts of the South in large numbers and were an economic necessity in the labor force. Rather than a scapegoat, they were both a needed source of labor and a threat to white power, and in these circumstances it appears racial treatment, rather than being static, can in fact change and may do so very quickly.

Second, these results further strengthen Woodward's argument that things were not as they always were, but rather they changed significantly. First, they reject the notion that discrimination in the South was culturally determined and inflexible. Most contemporary commentators argued along these lines—the de jure racial system merely reflected the region's unchanging cultural norms. For them, the racial order established by 1900 "was regarded as 'the final settlement,' the 'return to sanity,' the 'permanent system,'"(Woodward, 1955/1974, p. 7). A critic of the racial system agreed that these beliefs were long-held and inflexible: "The mind of the South is almost impervious to change…That mind is, in every essential respect, merely the ancient mind of the South," (Cash,

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1929). Instead it seems the "mind of the South" was amenable to considerable change, even over a few short decades.

While Woodward argues the period before 1900 was in some ways less racist than the Jim Crow era that followed, these results paint an even darker picture—areas that were the least racist after the Civil War had become the most racist by the Civil Rights Era. The grandchildren of people who prevented lynchings, for example, were considerably more likely to commit racially-motivated murders by the 1950s.

Third, the results in the very long run contradict the idea of steady social progress over time. Martin Luther King famously stated "the arc of the moral universe is long, but it bends towards justice," (1965). This contrasts considerably with Figure 8, in which the arc bends in exactly the opposite direction. King borrowed his words from an assertion on the inevitability of the abolitionist cause in the 19th Century:

> "Look at the facts of the world. You see a continual and progressive triumph of the right. I do not pretend to understand the moral universe; the arc is a long one, my eye reaches but little ways; I cannot calculate the curve and complete the figure by the experience of sight; I can divine it by conscience. And from what I see I am sure it bends towards justice," (Parker, 1858).

The results in this section, however, strongly contradict the inevitability of social progress, specifically with regard to racial discrimination. Counties with greater labor scarcity following the Civil War in fact retrogressed from having relatively low levels of discrimination to becoming among the worst offenders by the Civil Rights Era.

7. Conclusion

This paper presents strong evidence that market forces can affect non-market discrimination, similar to the predicted effect of market forces on labor-market discrimination in economic theory.

Labor market scarcity led to improved racial treatment in the most affected counties following the Civil War. That discrimination changed after the war (1865-1880) in areas with higher death rates, and that it remained changed after the labor-market convergence (1880 to 1900 or 1920) suggests a possible shift in the racial norms in these areas, not simply a short-term economic calculation.

In the very long-run, however, the beneficial effects of the initial post-war period were not only erased but also reversed. In three short generations, counties with better-than-average racial treatment had become the worst offenders. The results presented here thus contradict those of Voigtlander & Voth (2012) and Jha (2013) who show an extreme persistence of cultural traits over several centuries in Germany and South Asia. While I cannot assess changes in aggregate discrimination over the period, the sharp changes in relative discrimination strengthen Woodward's (1955/1974) argument that racial discrimination in the South was not as it "always had been." Neither was the Jim Crow system inevitable, since many areas experienced relatively lower levels of discrimination before 1900. In his words, "before [the Jim Crow system] appeared in this form there occurred an era of experiment and variety in race relations of the South in which segregation was not the invariable rule." That these counties later became more discriminatory than average shows evidence of a steady process of calculations rather than a constant, culturally-determined system of discrimination. What contemporary Southerners thought of as linear progress in racial relations and what Martin Luther King hoped would be a "long arc" bending towards justice had in fact been an arc bending away, this in a period of modernization and economic growth in one of the most developed countries in the world.

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Appendix

1. Data

1.1 Deaths by Confederate Company

The original source for companies' deaths is the U.S. Archives' *Compiled Service Records for Confederate Soldiers* for each state. The Archives assembled these records starting in 1903, combining all existing documents into a single file for each soldier.[11] Deaths are generally listed on multiple documents in the soldier's file such as muster rolls (lists of each soldier in a company and their condition in a given month), battle reports, hospital records, Union prison records, letters notifying family members of the soldier's death, the family's claims for back pay, burial information, etc.

Several states have produced secondary sources that summarize the Archives' records—these are the source for most of the counts reported here. Counts for Georgia's infantry and all of Louisiana's soldiers come from direct summaries of the *Compiled Service Records* (GA Div.of Confed. Pensions and Records & Henderson, 1964; Booth, 1920). In Virginia and South Carolina, state-funded projects have supplemented the records with information from other sources such as pension applications, census records, cemetery indexes, and newspapers (Library of Virginia, 2014; Chambers, 2014). Alabama maintains a database with summarized records for most of the state's Confederate soldiers—the state archives has made these records for every soldier its employees have

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[11] Each former Confederate state's archives relinquished their Confederate military records, which were added to those seized by the Union Army at the end of the war. They are further supplemented with documents from the Union Army, such as records of prisoners of war (including hospitalizations and deaths), lists of soldiers surrendering to various Union armies in 1865, and soldiers released from Union custody upon swearing allegiance to the United States.

found in various sources over the last several decades (AL Dept. of Archives & History, 2014).[12]

Table A.1 in the Appendix gives more details on the sources for each state.

Roughly one quarter of soldiers in the *Compiled Service Records* cannot be classified as either

dead or living at war's end. Several companies' muster rolls end in December 1864 or January 1865,

for example.[13] The degree of losses from January 1865 to Lee's surrender in April was, however,

relatively small compared to the other years of the war.[14] In Virginia and South Carolina these

soldiers are not a problem since the state projects have used all available sources to supplement the

records. All soldiers with missing records are implicitly assumed to have survived the war—

subsamples of Georgia companies suggest most of the soldiers without final documentation did in

fact live through the war's end. I have not found evidence to this point that missing records are in

any way correlated with initial county characteristics.

Several groups are excluded from the counts because they cannot be connected to a county

of origin. These include soldiers who served at the regimental level or higher (officers with a rank

above captain, surgeons, musicians, and officers' staffs), soldiers in specialized units (which drew

from many areas throughout the state), or soldiers that served under the authority of Confederate

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[12] The database does not contain every soldier, but it is a random sample and does include a large majority of the state's soldiers that served in the Confederate Army.

[13] As the siege lines at Petersburg neared the Confederate capital of Richmond, the Confederate Archives were shipped south. Some records after this date were never centrally collected, and some earlier records were lost in transit to Charlotte, where they were eventually captured. Much of what happened to individual soldiers after January of 1865 is thus difficult to ascertain in the *Compiled Service Records* unless supplemented by a document from Union records (such as a surrender, which was common in 1865) or a death record other than a muster roll.

[14] Greer (2005) estimates that less than 10% of Confederate casualties (soldiers dead, wounded, or captured) came in 1865.

States of America (rather than their home state)[15]. This includes the Navy, though there were very few Confederate sailors in the war. In all cases, soldiers can still be assigned to counties of origin if they began the war serving in a local company since their records are grouped with the original unit. Together, these excluded groups made up a small minority of the Confederate military.

1.2 Deaths by County

Unlike modern armies, military units in the American Civil war were raised locally, so nearly all Confederate companies can be mapped back to a county of origin. This is accomplished using records from Civil War historians as well as some summary documents included with the Archives' records. The roots of county-based military units preceded the war—the South had long used a militia system for local defense, based at the county level (Fleming, 1905). Some of these companies had been mustered into U.S. service in the Mexican war, and they similarly formed Confederate units in 1861-62. Nearly all additional Confederate units were raised at the county or city level as well. As mentioned, most companies even carried geographic designations in their nicknames, such as the Chunky Heroes (from Chunky, Mississippi), the Catahoula Guerillas (from Catahoula County, Louisiana) and the Hot Spring Hornets (from Hot Spring County, Arkansas). In my preliminary sample of Virginia soldiers, I can assign counties for over 95% of the documented deaths.

Not every soldier came from the company's listed county of origin, but a wide majority did. I can document 80 to 90% of soldiers as living in the company's county of origin for the subsamples I have tested. Most men preferred to serve in units from their home towns and counties, and the army

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[15] Nearly all soldiers served in units organized by their states, not the Confederate government, though the Confederate government appointed the generals to command armies, made up of regiments from several states.

saw far less desertion and more camaraderie from organizing units in this fashion (Costa & Kahn, 2003). Genealogical sources follow this pattern of finding soldiers' military records using their known county of origin. The largest source of measurement error on this account is from soldiers living near county borders that joined a company from the adjacent county. To the extent this occurred equally in both directions it would simply attenuate the estimates.

Deaths by Confederate company, then, are connected to the company's county of origin to calculate the total deaths a county experienced in the war. These figures are divided by the county's military-eligible population (white men age 15-39 in the 1860 U.S. Census) to construct death rates for each county in 1860.[16] Figures 4 through 6 show the distribution of county death rates for Georgia, Louisiana, and Virginia.

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[16] As new counties were created over time, the deaths are aggregated between all the counties from which the new county was formed. Thus there are fewer county-level observations over time in the sample as more aggregation takes place in the data.

Tables and Figures

FIGURE 1:

AVERAGE GROWTH OF COUNTIES' BLACK POPULATIONS BY CIVIL WAR DEATH RATES, 1860-1920 (ALABAMA, GEORGIA, LOUISIANA, SOUTH CAROLINA, & VIRGINIA)
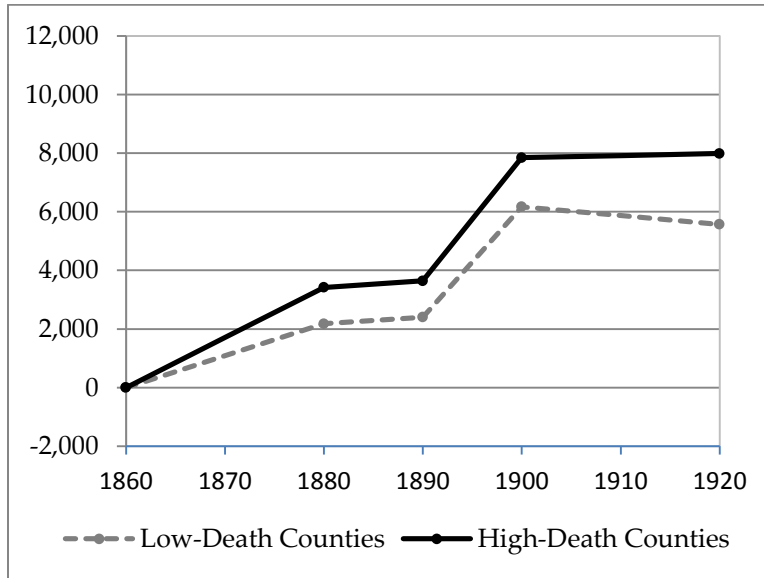
FIGURE 2:

AVERAGE GROWTH OF COUNTIES' WHITE POPULATIONS BY CIVIL WAR DEATH RATES, 1860-1920 (ALABAMA, GEORGIA, LOUISIANA, SOUTH CAROLINA, & VIRGINIA)

41
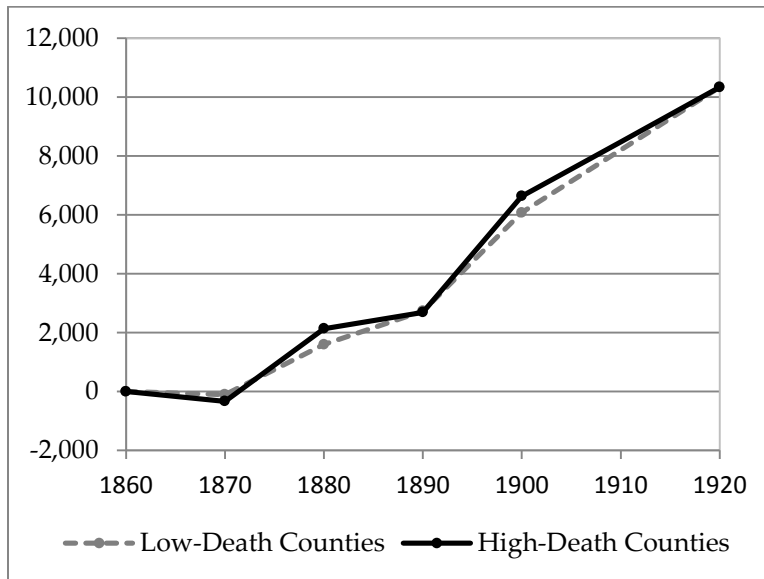
FIGURE 3:

DEATH RATES FOR NEIGHBORING COUNTIES IN SOUTHWEST GEORGIA IN THE AMERICAN CIVIL WAR
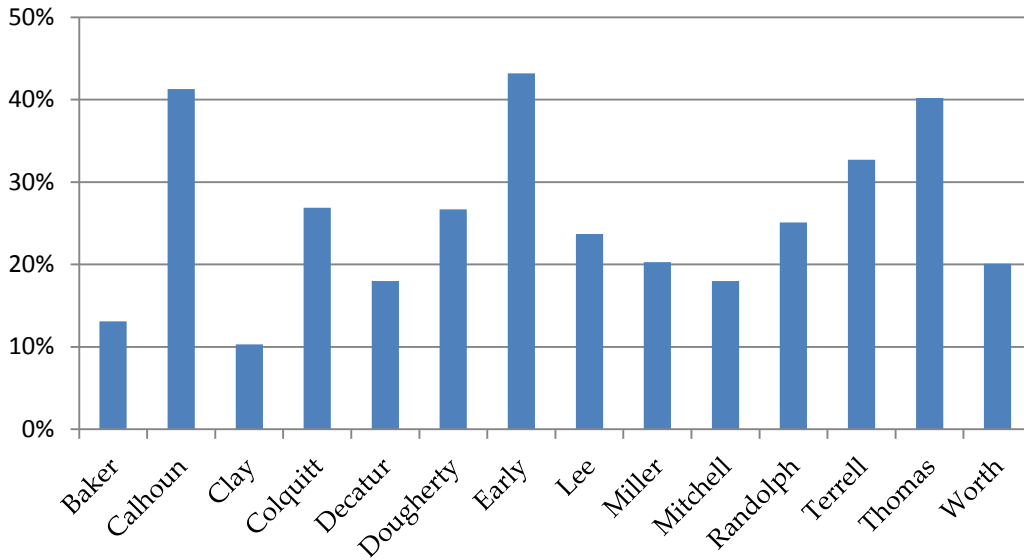
Note: Death rates calculated as the number of soldiers reported dead from each county divided by the white male population age 15-39 in the 1860 U.S. Census

FIGURE 4:

GEORGIA DEATH RATES IN THE AMERICAN CIVIL WAR BY COUNTY
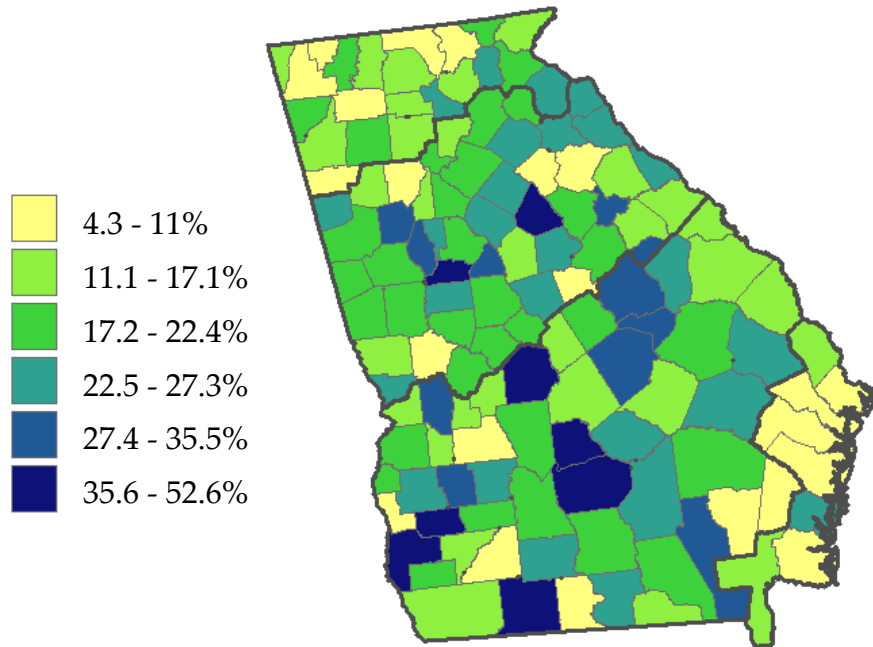
4.3 - 11%
11.1 - 17.1%
17.2 - 22.4%
22.5 - 27.3%
27.4 - 35.5%
35.6 - 52.6%

Notes: Death rates calculated as the number of deaths in each county divided by the number of white men age 15-39 in the 1860 U.S. Census.

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FIGURE 5:
LOUISIANA DEATH RATES IN THE AMERICAN CIVIL WAR BY PARISH
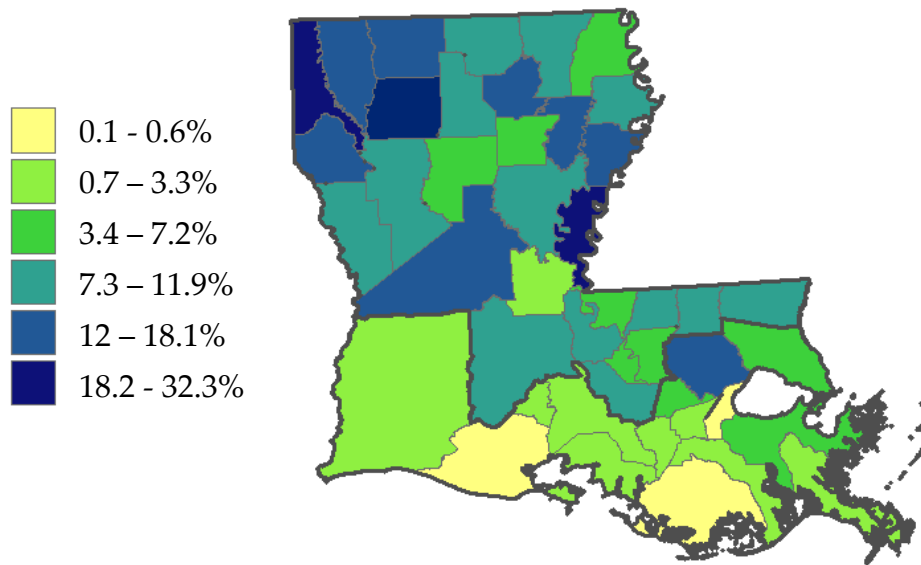
Notes: Death rates calculated as the number of deaths in each parish divided by the number of white men age 15-39 in the 1860 U.S. Census.

FIGURE 6:
VIRGINIA DEATH RATES IN THE AMERICAN CIVIL WAR BY COUNTY

Notes: Death rates calculated as the number of deaths in each county divided by the number of white men age 15-39 in the 1860 U.S. Census.

FIGURE 7:
ACTUAL AND PREDICTED DEATH RATES FOR COUNTIES IN SOUTHWEST GEORGIA

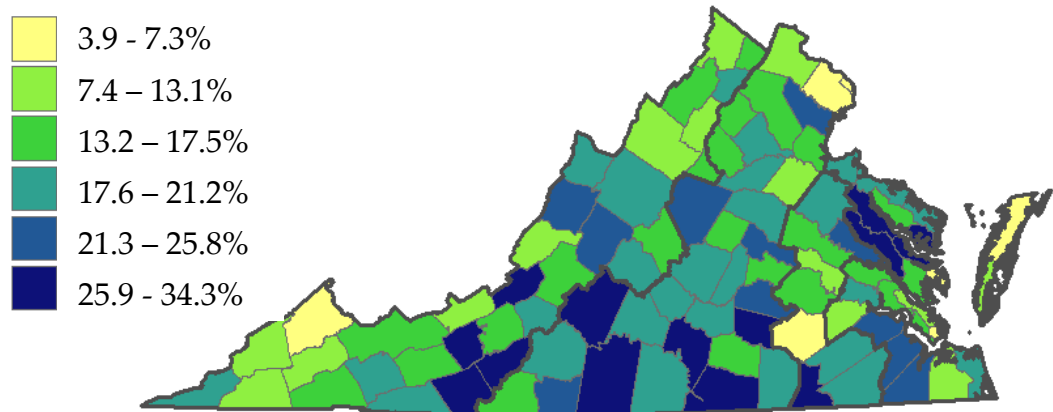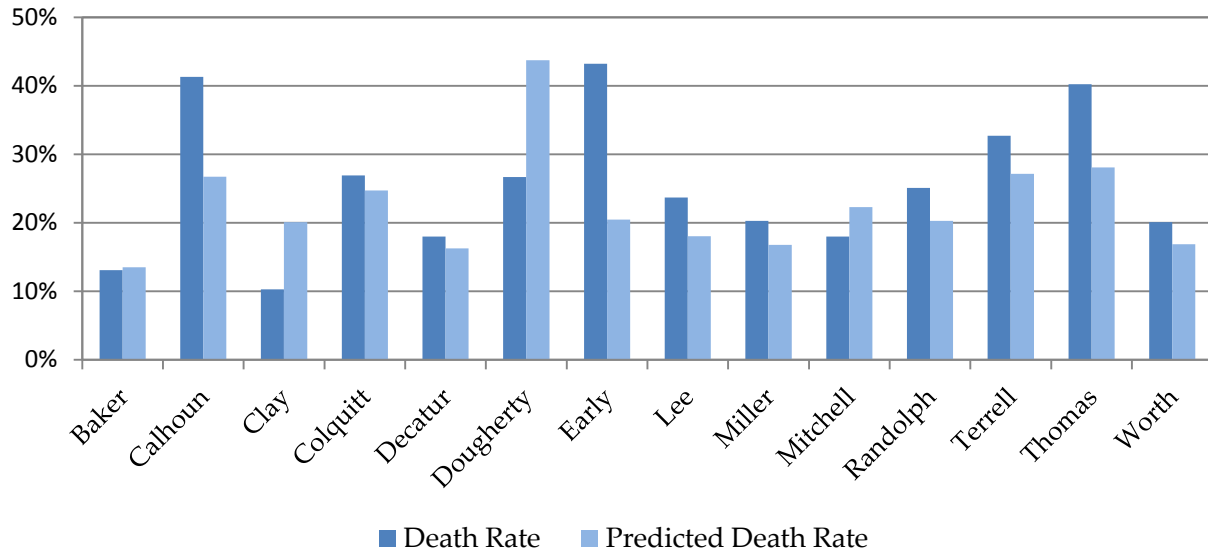**Notes:** Death rates calculated as the number of deaths in each county divided by the number of white men age 15-39 in the 1860 U.S. Census. Predicted death rates are estimated using battle data for each company in the given counties from Sifakis (1995).

FIGURE 8:

EFFECT OF CIVIL WAR DEATH RATES ON LOG TOTAL VOTES BY 20-YEAR PERIOD, 1848-1960
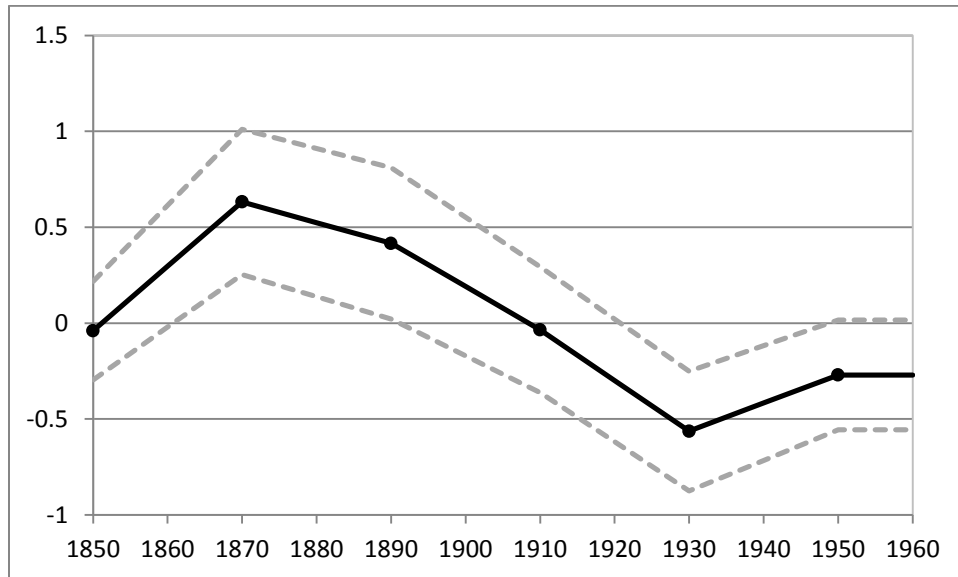
Notes: Log total votes are for congressional, presidential, and gubernatorial elections. Points represent coefficient in regression of log total votes on the Civil War death rate, an indicator for urban status in 1860, and state x region and election (office x year) fixed effects. Dotted lines represent 95% confidence intervals, constructed using multi-dimensionally clustered standard errors, clustered by state x region and election (office x year). Points are plotted in the middle of each 20-year period. "1850" result represents voting from 1848-1860, "1870" result represents voting from 1868-1880. The South did not vote in federal elections from 1861-1867.

TABLE 1:

BALANCE TEST: PARTIAL CORRELATIONS OF 1860 COUNTY CHARACTERISTICS WITH DEATH RATES IN THE AMERICAN CIVIL WAR

VARIABLES	(1) Death Rate	Mean of Variable
1860 Population (1,000s)	.00009 (.00026)	12.45
% of County Pop. Slaves (1860)	.0398 (.0352)	.358
Per Capita Wealth (1860)	-.0216 (.0227)	.641
Per Capita Mfg. Output (1860)	-.135 (.297)	.014
Arkansas	-.014* (.008)	
Florida	.109*** (.021)	
Georgia	.186*** (.012)	-
Louisiana	-.019* (.012)	-
South Carolina	.248*** (.016)	-
Virginia	.089*** (.011)	-
Coast	-.029* (.017)	-
Coastal Plain	.028** (.012)	-
Piedmont	.038*** (.011)	-
Mean of Variable	.159	
Observations	337	
R-squared	.577	

Notes: The excluded state in the regressions is Alabama. The excluded region is Appalachian. Death rates are the total number of deaths in companies from the county divided by the county's white male population aged 15-39 in the 1860 Census. State x Region effects not included to show the effects of individual states and regions more clearly.

TABLE 2:

DEATH RATES IN THE AMERICAN CIVIL WAR AND LYNCHINGS PER 1,000 IN BLACK POPULATION, COUNTIES, 1866-1900

VARIABLES	(1) Lynchings/1,000 in Black Pop.	(2) Lynchings/1,000 in Black Pop.	(3) Lynchings/1,000 in Black Pop.	(4) Lynchings/1,000 in Black Pop.	(5) Lynchings/1,000 in Black Pop.	(6) Lynchings/1,000 in Black Pop.	(7) Lynchings/1,000 in Black Pop.
Death Rate	-3.341*	-2.869*	-3.665**	-3.282*	-2.762**	-3.702**	-4.033**
	(1.801)	(1.628)	(1.810)	(1.729)	(1.355)	(1.813)	(2.030)
Urban	-.732**	-.712**					
	(.313)	(.317)					
Plantation County							-1.459***
							(.416)
Death Rate x Plantation							3.285
							(2.263)
Urban Counties	YES	YES	NO	NO	NO	NO	NO
Regions	ALL	ALL	ALL	Piedmont, Coastal Plain	ALL	ALL	ALL
State x Region F.E.	YES	YES	YES	YES	YES	YES	YES
Additional Controls	-	-	-	-	Census	Military	-
Effect—10 Pct. Pt. Increase in Death Rate	-28.4%	-24.9%	-30.7%	-28.0%	-24.1%	-30.9%	-33.2%
Observations	223	223	207	179	207	207	207
Pseudo R-squared	.322	-	.329	.060	.379	.329	.354

Notes: Robust standard errors in parentheses for regression coefficients. Columns 1 and 3-7 show estimations from Poisson regressions. Column 2 uses a negative binomial regression. Lynchings were mostly a rural phenomenon, so counties with urban populations in 1860 are dropped in columns 3-7.
*** p<0.01, ** p<0.05, * p<0.1.

ROBUSTNESS CHECKS AND CHANNELS OF CAUSATION: DEATH RATES AND LYNCHINGS PER 1,000 IN BLACK POPULATION, 1866-1900

VARIABLES	(1) Lynchings/1,000 in Black Pop.	(2) Lynchings/1,000 in Black Pop.	(3) Lynchings/1,000 in Black Pop.	(4) Lynchings/1,000 in Black Pop.	(5) Lynchings/1,000 in Black Pop.
Death Rate	-3.854*	-2.448*	-2.686*	-5.141**	-3.977**
	(2.227)	(1.433)	(1.512)	(2.223)	(1.913)
Log 1860 Black Pop.		-.445**			
		(.201)			
Log 1860 White Pop.			.820**		
			(.361)		
% Change Manufacturing				-.00027	
				(.00041)	
% Change Cotton Production					.0032**
					(.0013)
% Change Corn Production					.0963
					(.0796)
Urban Counties	NO	NO	NO	NO	NO
Regions	ALL	ALL	ALL	ALL	ALL
State x Region F.E.	YES	YES	YES	YES	YES
Effect—10 Pct. Pt. Increase in Death Rate	-32.0%	-21.7%	-23.6%	-40.2%	-32.8%
Observations	207	207	207	207	207
Pseudo R-quared	.157	.361	.358	.367	.354

Notes: Robust standard errors in parentheses for regression coefficients. Estimates in Column 1 come from a 2SLS regression. All other estimations come from Poisson regressions. Lynchings were mostly a rural phenomenon, so counties with urban populations are dropped in all estimations.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

DEATH RATES IN THE AMERICAN CIVIL WAR AND LOG TOTAL VOTES IN CONGRESSIONAL, PRESIDENTIAL, AND GUBERNATORIAL ELECTIONS IN SOUTHERN COUNTIES, 1868-1900

VARIABLES	(1) Log Total Votes 1868-1900	(2) Log Total Votes 1868-1900	(3) Log Total Votes 1868-1900	(4) Log Total Votes 1868-1900	(5) Log Total Votes 1868-1900	(6) Log Total Votes 1868-1880
Death Rate	.517***	.356*	.449**	.436***	.556***	.629***
	(.185)	(.213)	(.178)	(.149)	(.180)	(.196)
Log Black Eligible Voters	-	-	-	-	-	-
					-	
Regions	ALL	Piedmont, Coastal Plain	ALL	ALL	ALL	ALL
State x Region F.E.	YES	YES	YES	YES	YES	YES
Election F.E.	YES	YES	YES	YES	YES	YES
Addl. Controls	-	-	Military	Census	-	-
Urban Counties	YES	YES	YES	YES	NO	YES
					-	
95% Conf. Interval (Death Rate)	[.154, .879]	[-.060, .773]	[.101, .797]	[.143, .729]	[.203, .909]	[.245, 1.014]
95% Conf. Interval (Ln Black Pop.)	-	-	-	-		
Null Hypothesis p-value	$\hat{\beta} > -.13$ .0005***	$\hat{\beta} > -.13$ .0223**	$\hat{\beta} > -.13$ .0011***	$\hat{\beta} > -.13$ .0001***	$\hat{\beta} > -.13$ .0001***	$\hat{\beta} > -.13$ .0001***
Observations	8,330	5,921	8,330	8,330	7,727	3,940
R-squared	.715	.711	.717	.715	0.695	.743

Notes: Columns 1-4 report multi-dimensionally clustered standard errors in parentheses, clustered by state x region and election (office x year). Column 5 reports standard errors clustered by county. Regressions control for the log of the number of eligible voters (men aged 21 and over) in the election year, state x region and election (state x office x year) fixed effects, and urban/rural status in 1860. Eligible voters in years between censuses are estimated by linear interpolation. The null hypothesis reported is the expected reduced form effect of Civil War death rates on the log of total votes, assuming 1) the death rates only affect the size of the black population and 2) that black and white voters' propensity to vote remains constant with an increase in the black population. Rejection indicates that their propensities to vote did in fact change with the death rates. *** p<.01, ** p<.05, * p<.1.

TABLE 5:

ROBUSTNESS CHECKS AND CHANNELS OF CAUSATION: DEATH RATES AND LOG TOTAL VOTES, COUNTIES, 1868-1900

VARIABLES	(1) Log Total Votes 1868-1900	(2) Log Total Votes 1848-1860	(3) Log Total Votes 1868-1900	(4) Log Total Votes 1868-1900	(5) Log Total Votes 1868-1900	(6) Log Total Votes 1868-1900	(7) Log Total Votes 1868-1900
Death Rate	.358*	-.119	.495***	.542***	.366*	.327*	-
	(.202)	(.265)	(.153)	(.165)	(.221)	(.177)	
Log Black Population	-	-	.0134	-	-	-	.357
			(.084)				(.280)
Log White Population	-	-	-	-.0996	-	-	-
				(.101)			
% Change Manufacturing	-	-	-	-	-.00001	-	-
					(.00006)		
% Change Cotton Production	-	-	-	-	-	.0005	-
						(.0005)	
% Change Corn Production	-	-	-	-	-	-.124**	-
						(.055)	
95% Conf. Interval (Death Rate)	[-.038, .755]	-	[.194, .796]	[.218, .866]	[-.068, .800]	[-.0190, .673]	[-.192, .905]
Null Hypothesis	$\hat{\beta} > -.13$	-	$\hat{\beta} > -.13$	$\hat{\beta} > -.13$	$\hat{\beta} > -.13$	$\hat{\beta} > -.13$	$\hat{\beta} > -.12$
p-value	.0160**	-	.0000***	.0000***	.0249**	.0096***	.0252**
Partial F-stat	-	-	-	-	-	-	11.33
Observations	8,330	2,896	8,330	8,330	8,330	7,727	8,330
R-squared	.729	0.734	0.715	0.716	0.737	0.703	.616

Notes: Multi-dimensionally clustered standard errors in parentheses, clustered by state x region and election (office x year). Regressions control for the log of the number of eligible voters (men aged 21 and over) in the election year, state x region and election (state x office x year) fixed effects, and urban/rural status in 1860. Counties from all regions are included. Eligible voters in years between censuses are estimated by linear interpolation. The null hypothesis reported is the expected reduced form effect of Civil War death rates on the log of total votes, assuming 1) the death rates only affect the size of the black population and 2) that black and white voters' propensity to vote remains constant with an increase in the black population. Rejection indicates that their propensities to vote did in fact change with the death rates. *** p<0.01, ** p<0.05, * p<0.1

PERSISTENCE OF EFFECTS OF DEATH RATES ON LOG TOTAL VOTES AND LYNCHINGS PER 1,000 IN BLACK POPULATION, 1880-1920

VARIABLES	(1) Lynch.'s/1,000 Blk. Pop. 1880-1900	(2) Lynch.'s/1,000 in Blk. Pop. 1901-1920	(3) Lynch.'s/1,000 in Blk. Pop. 1880-1920	(4) Log Total Votes 1881-1900	(5) Log Total Votes 1901-1920	(6) Log Total Votes 1881-1920
Death Rate	-3.656**	-2.086	-3.423**	.416**	-.0347	.175
	(1.807)	(1.539)	(1.521)	(.201)	(.445)	(.247)
Urban Counties	NO	NO	NO	YES	YES	YES
Regions	ALL	ALL	ALL	ALL	ALL	ALL
State x Region F.E.	YES	YES	YES	YES	YES	YES
Additional Controls	NO	NO	NO	NO	NO	NO
Effect—10 Pct. Pt. Increase in Death Rate	-30.6%	-18.8%	-29.0%	-	-	-
95% Conf. Interval (Death Rate)	-	-	-	[.022, .811]	[-.906, .837]	[-.310, .660]
Null Hypothesis	-	-	-	$\hat{\beta} > -.13$	$\hat{\beta} > -.13$	$\hat{\beta} > -.13$
p-value	-	-	-	.0066	.8302	.2174
Observations	209	210	210	4,473	4,276	8,749
R-squared	-	-	-	.729	.777	.756
Pseudo R-squared	.329	.130	.345	-	-	-

Notes Columns 1-3: Robust standard errors in parentheses. All estimations from Poisson regressions. Lynchings were mostly a rural phenomenon, so counties with urban populations are dropped in all estimations.

Notes Columns 4-6: Multi-dimensionally clustered standard errors in parentheses, clustered by state x region and election (office x year). Regressions control for the log of the number of eligible voters (men aged 21 and over) in the election year, state x region and election (state x office x year) fixed effects, and urban/rural status in 1860. Eligible voters in years between censuses are estimated by linear interpolation. The null hypothesis reported is the expected reduced form effect of Civil War death rates on the log of total votes, assuming 1) the death rates only affect the size of the black population and 2) that black and white voters' propensity to vote remains constant with an increase in the black population

TABLE 7:
CIVIL WAR DEATH RATES AND INDICATORS FOR RACIAL TREATMENT IN THE CIVIL RIGHTS ERA, 1950-1970

PANEL A: PERCENT OF NON-WHITE ADULTS REGISTERED TO VOTE

VARIABLES	(1) All Regions	(2) Piedmont & Coastal Plain	(3) Census Controls	(4) Military Controls
Death Rate	-.638***	-.752***	-.532***	-.581**
	(.183)	(.190)	(.196)	(.244)
State x Region Fixed Effects	YES	YES	YES	YES
Mean of Dependent Variable	.242	.240	.242	.242
Effect—10 Pct. Pt. Increase in Death Rate at Mean	-26.3%	-31.4%	-22.0%	-24.0%
Observations	217	173	217	217
R-squared	.224	.278	.294	.231

PANEL B: RACIALLY-MOTIVATED MURDERS AND MURDERS OF CIVIL RIGHTS WORKERS

VARIABLES	(1) All Regions	(2) Piedmont & Coastal Plain	(3) Census Controls	(4) Military Controls
Death Rate	.667**	.827**	.583**	.529*
	(.323)	(.323)	(.293)	(.298)
State x Region Fixed Effects	YES	YES	YES	YES
Mean of Dependent Variable	.122	.129	.123	.123
Effect—10 Pct. Pt. Increase in Death Rate at Mean	54.7%	64.1%	47.4%	43.0%
Observations	238	202	220	220
R-squared	.201	.215	.273	.217

TABLE A.1: DATA SOURCES AND DESCRIPTIONS BY STATE

	Source	Type	Soldiers	Original Sources	Notes
ALABAMA	*ADAH Civil War Service Database*	Database	Majority Sample	Multiple, including *Compiled Service Records*	Majority sample, but not exhaustive listing
ARKANSAS	Edward G. Gerdes Civil War Home Page	Website	All	*Compiled Service Records*	Pages for each Arkansas company
FLORIDA	*Biographical Rosters of Florida's Confederate and Union Soldiers, 1861-1865*, Vol. 1-5	Book	All	*Compiled Service Records*, supplemented with other records	*Forthcoming*, pending research grant
GEORGIA	**Infantry:** *Roster of the Confederate Soldiers of Georgia*, Vol. 1-6	**Infantry:** Book	All	**Infantry:** *Compiled Service Records*	
	Cavalry, Artillery: *Compiled Service Record*, regimental histories	**Cavalry, Artillery**: Microfilm, books		**Cavalry, Artillery:** *Compiled Service Records*, supplemented with other records (regimental histories only)	**Cavalry, Artillery:** *Forthcoming*
LOUISIANA	Andrew Booth, *Records of Louisiana Confederate Soldiers*	Book	All? (see Notes)	Probably *Compiled Service Records*	Source published in 1920, probably missing many soldiers
SOUTH CAROLINA	Bing Chambers, *And Were the Glory of Their Times*	Book (unpublished)	Deaths only	*Compiled Service Records*, supplemented with other records	
VIRGINIA	*Virginia Military Dead*	Database	Deaths only	*Compiled Service Records*, supplemented with other records	