



# Political participation in a violent society: The impact of lynching on voter turnout in the post-Reconstruction South



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## ABSTRACT

How does violence against a group affect political participation? In theory, the targeted group may either become politically mobilized or may become discouraged and withdraw from political participation. To address this question, we assess the impact of lynchings on differential rates of black turnout in the post-Reconstruction American South. We first provide evidence that lynchings are not politically motivated. We then show that, even though lynchings were not politically motivated, exposure to lynching reduced local black voter turnout by roughly 2.5 percentage points. A series of specification tests suggest this relationship can be interpreted as causal.

## 1. Introduction

Three broad frameworks shape how economists, and social scientists in general, think about the relationship between violence and political engagement. First, much recent work in economics and political science emphasizes how exposure to violence may lead members of the minority group to mobilize politically (e.g., turning out to vote at higher rates) in an attempt to install representatives in government who will work to provide better protection (e.g., Bellows and Miguel, 2009; Blattman, 2009). Second, another body of research presents evidence that violence (to oneself or one's group members) may cause or exacerbate mistrust in the government's ability or willingness to provide protection, which may in turn lead affected individuals to turn away from the political process altogether (e.g., Blanco, 2013). Along the same lines, exposure to lawlessness and violence may generate fear that participating in the political process will also be met by violence, which may also discourage turnout.<sup>1</sup> Third, there is a large literature, going back decades, that explores how politicians and dominant social groups in many different social and historical settings used violence strategically to deter voter participation and undermine the democratic process (e.g., Tolnay and Beck, 1992, 1995). In this setting, violence is endogenous and, to the extent it is costly, most common in places where electoral outcomes are in doubt (Collier and Vicente, 2012). This logic suggests a more general point: it is possible that the relationship between violence and political

engagement is not general, but varies across time and place, depending on the broader context.<sup>2</sup>

In this paper, we revisit the American South during the late 1800s and early 1900s to explore the relationship between violence and voter participation. Violence was a pervasive feature of life in the postbellum South, with whites frequently engaging in anti-black violence to punish and terrorize blacks who violated established norms regarding race. There is also a large historical literature suggesting that Southern Democrats (whose voting base was exclusively white) used violence to discourage blacks from voting and undermine the competitiveness of Republican candidates (whose voting base included both whites and blacks). The South is a useful natural setting in which to explore the relationship between violence and political activity in part because the rules governing voting in the South, particularly in relation to race, underwent sharp changes over the course of the nineteenth and early twentieth century. As we explain below, these changes foster clean identification and allow us to explore the political implications of violence across different institutional settings. In addition, while it is often difficult to define and measure violence outside of well-defined contemporary settings there is at least one dimension of the Southern proclivity to engage in anti-black violence that is well defined, documented, and measured. As explained below, there is a widely used database that records the lynching of more than 2000 blacks in the American South.

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<sup>1</sup> In a field experiment, for example, Collier and Vicente (2014) show how an anti-violence campaign in Nigeria decreased the perceived threat of violence and promoted voter turn-out. See also, Aidt and Franck (2015) who show how the so-called Swing Riots altered electoral outcomes in early nineteenth-century Britain.

<sup>2</sup> For recent and general models of political violence, see Besley and Persson (2011) and Powell (2013).

Exploiting data on lynching and voter turnout, we compile a panel of Southern counties that extends from 1882 to 1912. In light of the literature mentioned above, we then use a difference-in-differences strategy to answer two questions. We first ask if lynching was politically motivated, and run several tests in search of evidence that whites used lynching strategically to deter black turnout. More precisely, if lynching were both costly and politically motivated, one would expect to observe the following patterns: lynching would spike during election years; lynching would increase in the weeks preceding an election, and drop off sharply in the weeks following; lynching would be more frequent in counties where black voters had substantial clout at the ballot box and where electoral outcomes were in doubt; and lynching would decline after laws were passed effectively disenfranchising African American voters. The data are inconsistent with each of these predictions, however.

Having rejected the hypothesis that lynching was being used strategically to alter electoral outcomes, we then ask if lynching nevertheless had an impact on black voter turnout. The results indicate that, while lynching was not politically motivated, black voter turnout dropped by 2.4–4 percentage points in counties in which African Americans were lynched in the months leading up to an election. For comparison, the magnitude of this local effect is similar in size to the estimated effect of poll taxes and literacy tests (Jones et al., 2012), two well-known tools used in the post-Reconstruction South to disenfranchise black voters. Our results in this paper are robust to a variety of concerns and threats to identification, including issues related to the ecological fallacy, reverse causality, time trends and unobserved time-varying shocks, changes in cotton prices, and lynching induced migration among African Americans.

Given these results, one might reasonably ask: how does one reconcile the finding that lynching was not politically motivated with the finding that it nevertheless deterred voter turnout among African Americans? Two mechanisms suggest themselves, and we explore both in the analysis that follows. The first is that lynching was a general indicator of a county's (or a region's) ability to inflict violence and punishment on blacks in a variety of settings, including but not limited to, the political. In this way, while whites lynched blacks for reasons other than politics, the capacity to lynch would have been highly correlated with the ability and willingness to inflict punishments for political acts. The second mechanism is that lynching was a form terrorism that, regardless of motivation, scared blacks from engaging in any activity that necessitated interacting with whites. This mechanism is directly related to the development literature which suggests people might withdraw from politics in response to violence. If it were the first mechanism, and lynching served as a general indicator of the capacity to inflict violence, one would expect lynching to have a persistent and lasting effect on voter turnout in the years before and after a lynching. Alternatively, if it were the second mechanism, and lynching simply represented a general threat of violence, one would expect a fleeting effect, whereby a lynching in year  $t$  would be uncorrelated with voter turnout among blacks in the years preceding and following year  $t$ . Consistent with the second mechanism, the data suggest that the effects of lynching were fleeting.

The analysis here contributes to a growing literature assessing the effects of violence and crime on political (and, more generally, community) engagement. Several recent papers find evidence that violence leads to increased political and community participation. Bellows and Miguel (2009) document that individuals in areas in Sierra Leone that received more exposure to civil war in the 1990s are more likely to report voting, joining local political groups, and attending community meetings after the end of the war. Blattman (2009) provides similar evidence from Uganda.<sup>3</sup> While not explicitly related to politics, Voors et al. (2012)

<sup>3</sup> He compares Ugandan youths who were abducted to serve as new recruits for rebel forces to non-abducted youths. Abducted youths who returned are significantly more likely to report voting in a survey, but are no more likely to report increased non-political community participation. The survey evidence suggests that the channel through which this occurs is exposure to violence; amongst abductees, those that report witnessing the most violence are the most likely to report voting.

provide evidence from a field experiment in Burundi which suggests that individuals exposed to violence are significantly more altruistic towards their neighbors. Bateson (2012) draws on survey evidence on crime victimization and voting from five continents. She finds that recent victims of crimes (both property and violence crimes) are significantly more likely to vote; she concludes that “rather than being seen as disenfranchised, disempowered, or disengaged, crime victims should be reconceptualized as political actors—indeed, as potential activists.”

Our paper is distinct in two ways. One distinction is data-orientated: recent work in economics draws primarily from survey data to measure political participation. By contrast, we assess the effect of lynching on actual voting behavior. While looking at actual turnout is not without its own set of problems, it does provide another, complementary window through which to view the effects of violence on political participation and behavior. The other distinction is contextual. While the most recent economic literature looks to the developing world, particularly modern Africa, we look at very different historical setting (the American South).

The differences with regard to context are significant on a number of levels. First, we focus on violence aimed at a specific and oppressed minority group; other recent papers in economics and political science look at populations more generally. To the extent that oppressed minority groups have a higher baseline level of fear, one might imagine violence impacting those groups differently than majority groups. It may be that oppressed minority groups have the most mistrust to start with, so we might expect the outcome to shift to the “violence reduces turnout” prediction. Second, in the American South the prevalence of lynching was a manifestation of larger failures in the justice system, while in the settings considered by other recent papers violence does not necessarily signal broader systemic failure. Consider, for example, Bateson (2012). Her survey measures whether respondents have been victims of a crime, but those respondents may expect the criminal to be prosecuted. In our setting, however, vigilantism ruled, and the victims of lynching (both actual and potential) could not have expected the perpetrators to have been charged, tried, and convicted, in a court of law. This too is presumably more likely to cause citizens to doubt the government's ability/willingness to protect them and cause withdrawal from the political system, rather than “activism.”

## 2. Lynching in the American South: preliminary observations

Fig. 1 plots the total number of African Americans lynched in the South over time from 1882–1912.<sup>4</sup> The data follow an inverted U-shaped pattern. The number of lynchings rose during the 1880s and early 1890s, and peaked at 101 in 1892. After 1893, however, that trend is reversed, and the number of lynchings returns to pre-1890 levels by the mid-1900s. Mapping the location of all lynchings between 1882 and 1912, Fig. 2a provides a sense of the cross sectional variation in lynching across states in the South and border states. It shows that lynching was geographically dispersed, but that most lynchings occurred the deep South, and fewer lynchings occurring in the border states. Fig. 2b maps lynchings by county, our unit of analysis later in the paper, across the same time period.

The causes of lynching in the American South have been the object of extensive scholarly discussion and debate. Writers during the early twentieth century argued that lynching was the result of Southern backwardness, and that it would gradually die out as the South industrialized and urbanized. A related line of thought suggests lynching was an element in the system of paternalism, under which African Americans sacrificed mobility in exchange for higher wages and protection from violence (Alston and Ferrie, 1993, 1999). Brundage (1993) and others suggest that lynching was a form of ritualized violence whereby white vigilante groups punished blacks for crimes (both real and imagined) and

<sup>4</sup> These data include 9 Southern states: Alabama; Arkansas; Florida; Georgia; Louisiana; Mississippi; North Carolina; South Carolina; and Tennessee. The data are drawn from the HAL Lynching Database, which is described in more detail in a later section.

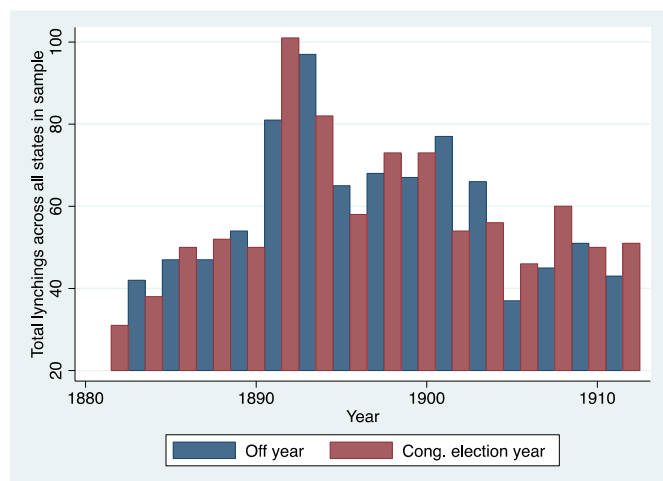


Fig. 1. Counts of lynchings of African Americans by year for all states in sample (1882–1912).

violating established color lines. As we discuss in a later section, at least at a surface level, the data are consistent with this suggestion. In the lynching database we draw on, we observe the stated reason for the lynching. In a vast majority of cases, the alleged offense of the lynching victim was rape, murder, or attempts at either of those crimes.

In their authoritative account of lynching, Tolnay and Beck (1995, pp. 19, 92–93) acknowledge the motivational evidence documented by Brundage but hypothesize that lynching had deeper causes, and might have stemmed from the perceived economic and political threats posed by African Americans. That said, they find no evidence that lynching was higher in more competitive electoral districts or that lynching rates plummeted in the wake of laws disenfranchising blacks. (We revisit their empirical analyses on this front in a later section.)

More recent evidence, from scholars who have compiled data on both completed lynchings and *averted* lynchings, provides mixed evidence on the relationship between the local political climate and the likelihood of lynchings. In a pooled cross-section of counties, Hagen et al. (2013) and Makovi et al. (2016) observe a positive correlation between the share of Democratic voters in a county and the likelihood of mob formation. However, conditional on mob formation, they find a negative correlation between the share of Democrat voters and intervention (or, a positive correlation between Democrats and the likelihood that a lynching is completed). Beck et al. (2016) find the opposite: counties with a higher share of Democrats are associated with a higher rate of lynching interventions. As we will discuss later, in our data, we observe no relationship between the political composition of counties and the likelihood of lynching. Why do our results differ on this margin? Unlike these other papers, we analyze our data as a county-by-election panel with county fixed effects. Moreover, without additional analysis to facilitate causal inference, the presence of correlations between political composition of a county and the likelihood of a lynching of course does not imply that lynchings are politically motivated. Instead, such correlations highlight that political climate (and the other county-level characteristics correlated with political composition) may be an important factor, even if not causal, in providing an environment where lynchings can occur.

Additionally, Beck and Tolnay (1990) and Makovi et al. (2016) find some evidence that lynching rates were higher in times and places of low cotton prices and economic distress. Christian (2014) offers further evidence, showing that in high-cotton producing counties, reductions in cotton prices increased lynching rates. We adopt multiple strategies to control for cotton price shocks, and other events affecting regional economies, that might confound our empirical analysis.

In a careful study of Georgia and Virginia, Brundage (1993) categorizes the nature of lynch mobs into five basic types. He finds that the vast majority (upwards of 70%) of lynchings are conducted by private mobs

or mass mobs; less common are lynchings conducted by vigilante mobs (e.g., the Ku Klux Klan), posses, and unclassifiable mobs. Of the two most common types: Private mobs were relatively small groups “usually comprised of friends and family members of the [alleged] victim, and their participation was motivated by their desire to exact revenge.” Mass mobs, on the other hand, were very large gatherings (numbering in the hundreds and – in some cases – thousands). These mobs were initiated by alleged “heinous crimes”. Brundage writes “Mass mobs, despite their size, acted swiftly. In Georgia, they captured and executed more than 53 percent of their victims within a day of the alleged crime and more than 85 percent within a week.”

### 3. Data

To explore the relationship between lynching and voter participation rates among African Americans in the South, we employ four types of data: (1) data on lynching, (2) voter turnout data, (3) demographic data, and (4) data on laws restricting black voting rights. In this section, we briefly describe the sources we employ for each of these forms of data.

Lynching data are drawn from the “Historical American Lynching” (HAL) data collection.<sup>5</sup> HAL reports all known lynchings occurring in Southern states (with the exceptions of Virginia and Texas) from 1882 to 1930. The data report: the date of the lynching, the county where the lynching occurred, the race and sex of the victim, and the reported offense that initiated the lynching. Ultimately, our analysis is focused on the impact of violence against African Americans in the post-Reconstruction period, so we restrict the HAL sample to lynchings with black victims that occurred between 1882 and 1912. Moreover, because we are interested in the potential impact of lynchings on political outcomes, for our main analysis we further restrict the sample to lynchings that occurred during the same calendar year as a Congressional election (but before the first week of November, when elections occurred).

We pair the lynching data with county-level voter turnout data for all Congressional elections occurring between 1882 and 1912.<sup>6</sup> These data report total votes cast (in general and split by party) within each county. The data do not report turnout rates, nor do they report race-specific turnout rates. We draw on county-level demographic data from decennial Census to account for both of these issues. We construct turnout rates by simply dividing total votes cast by total population within the county.<sup>7</sup> The demographic data also allow us to construct the share of the population within a county that is black. As discussed further in section 4, we take advantage of variation in “percent black” across counties and across time to econometrically assess the impact of lynching on changes in black turnout in particular (relative to white turnout in the same county and year). Where possible, we linearly interpolate between Censuses to obtain intercensal estimates. One difficulty in compiling county-level data for this period is that, in some places, county boundaries changed over time. To address this, in cases where county boundaries changed over time, we created county-groups with time consistent borders. As a result, our database consists of county versions. Where borders did not change, these county versions contain a single county with time consistent borders. Where borders changed, these county versions contain a

<sup>5</sup> The HAL database is constructed and made available by Elizabeth Hines and Eliza Steelwater (<http://people.uncw.edu/hinese>). Although there is a newer database on lynching, that new database differs from HAL only because it includes Western states. Because we focus on the South, these newer data are not relevant to the analysis.

<sup>6</sup> *Electoral Data for Counties in the United States: Presidential and Congressional Races, 1840–1972*. This dataset is publicly available online at <http://www.icpsr.umich.edu/> (study number 8611). The data were originally compiled by Jerome Clubb, William Flanagan, and Nancy Zingale.

<sup>7</sup> One might be interested in the percent of the *voting-eligible* population that votes. However, there is not sufficient information in all of the Censuses to consistently construct such a measure. In particular, the 1870 and 1880 Censuses do not report county-level counts of population that are broken down by race, gender, and age. Thus, it is worth keeping in mind that, throughout the paper, any reference to turnout rates (and changes in turnout rates) are somewhat deflated as a result of taking total population as the base.

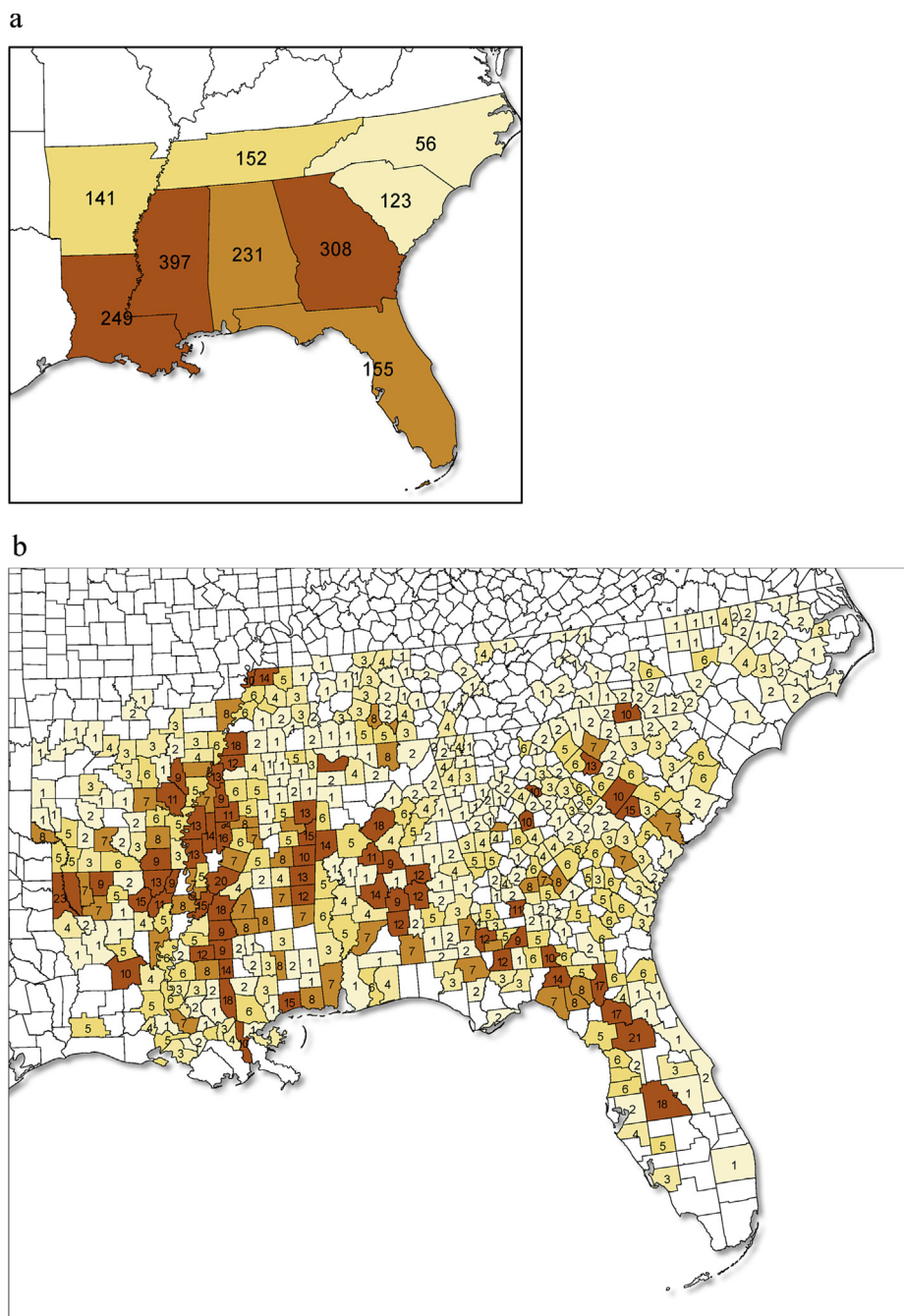


Fig. 2. a: Counts of lynchings of African Americans by state for all years in sample (1882–1912). b: Counts of lynchings of African Americans by county for all years in sample (1882–1912).

group of two or more counties with time consistent borders. Of course, we base our interpolations on the county versions with time consistent borders.

As noted above, our analysis will account for state-level variation in laws that disenfranchised African-American voters. More precisely, if lynching had been politically motivated, lynching rates would have dropped following their passage. These laws came in three varieties: poll taxes; literacy requirements; and ballot box measures. Poll tax laws required individuals to provide evidence that they have paid their “poll tax” for the current year (and in some cases required evidence that individuals were current over a long-number of years) in order to vote. Literacy-test laws typically required individuals to read and explain a portion of the state constitution, to the arbitrary satisfaction of an election official, prior to voting. Ballot box laws were more complicated and came in several varieties. For example, some of these laws eliminated

party identifiers, while others introduced “multi-box” polling stations. These multi-box stations required multiple ballots (typically 8) each to be placed in a separate ballot box. A mistake in filing a single ballot would disqualify all ballots. Table 1 describes when and where these franchise restrictions were enacted. In all of the states in our sample (with the exception of Georgia), states introduced ballot box laws before, or concurrent with, the adoption of poll taxes or literacy tests.

#### 4. Results

In this section, we report results addressing two distinct empirical questions. First, in subsection 4.1, we ask: Is there evidence that lynching was politically motivated? Ultimately, we find no such evidence. Then, in subsection 4.2, we turn to the main focus of the paper and ask: Although lynching was not politically motivated, did lynching have an impact on

**Table 1**  
Timing of franchise-restricting laws.

State	Ballot box	Poll tax	Literacy test
Alabama		1902	1903
Arkansas	1892 <sup>a</sup>	1893-1904 <sup>b</sup> , 1910	
Florida	1889 <sup>c</sup>	1889	
Georgia		1877	1908
Louisiana	1896 <sup>d</sup>	1900	1899
Mississippi	1891 <sup>e</sup>	1891	1892
North Carolina	1899 <sup>c</sup>	1902	1902
South Carolina	1882 <sup>c</sup>	1896	1896
Tennessee	1890 <sup>f</sup>	1890	

a. The Election Law of 1891 mandated a secret ballot and standardized ballots. In concert, these requirements were a type of de-facto literacy test. Illiterate voters could no longer rely on party symbols and/or similar devices to vote straight party and the secret ballot requirement created barriers to voting assistance.

b. Invalidated by U.S. Circuit Court January 7, 1905, reenacted prior to the Election of 1910.

c. Several States adopted complicated “multi-box” election laws that required multiple ballots (typically 8) each to be placed in a separate ballot box. A mistake in filing a single ballot would disqualify all ballots – thus, these laws operated much like a de facto literacy test.

d. Combination of Secret Ballot requirement and an onerous change in registration requirements.

e. Secret Ballot.

f. Secret Ballot w/out Party Identifiers & New Registration Law.

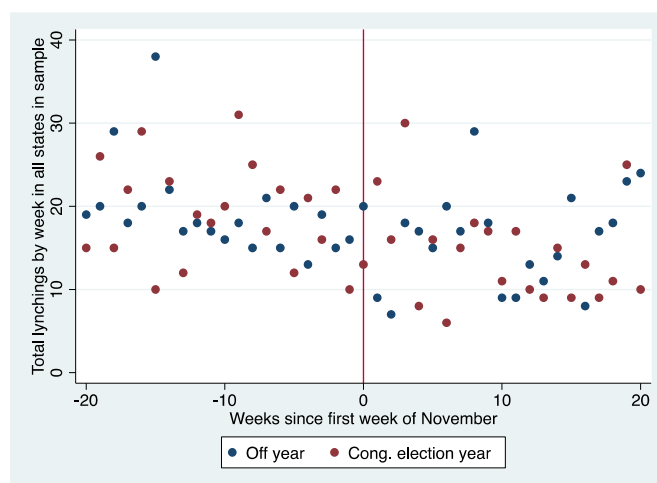
Sources: **Alabama**, *Literacy Requirement & Poll Tax*– Revised Alabama State Constitution Adopted 1901; **Arkansas**, *Poll Tax* – state constitutional amendments ratified in 1892, Invalidated by U.S. Circuit Court due to electoral challenge January 7, 1905 (Knight v. Shelton), reinstated as an amendment to Arkansas Constitution in 1909. *Secret Ballot* – BRANAM C. “Another Look at Disfranchisement in Arkansas”, 1888–1894. *Arkansas Historical Quarterly*, September 2010; 69(3):245–256; **Florida**, *Multi-Box & Poll Tax* – Constitutional amendment of 1885 empowers state to adopt Poll-Tax & Multi-Box adopted by legislature in 1889. See Perman, M. “Struggle for Mastery: Disenfranchisement in the South 1888–1908” 2001, UNC Press. & Lee, R.W. “The Florida Election Canvassing System” *Nova L. Rev.* 851, spring, 2002.; **Georgia**, *Poll Tax* – Cumulative Poll Tax requirement included in 1877 Amendments to State Constitution. *Literacy Test* – Amendment to the state constitution in Fall of 1908.; **Louisiana**, *Poll Tax & Literacy Test* – Amendments to the State Constitution adopted in 1898. *Ballot Law* – Perman (2001).; **Mississippi**, *Poll-Tax, Literacy Test & Multi-Box* – Amendments to the state constitutions adopted in 1890.; **North Carolina**, *Poll Tax & Literacy Test* – Amendments to the State Constitution adopted August 1900, *Multi-Box* – Enacted by State Legislature on 6 March 1899.; **South Carolina**, *Poll Tax & Literacy Test* – Amendments to State Constitution ratified December 1895. *Multi-Box* – Perman (2001).; **Tennessee**, *Ballot & Registration Laws* – Legislative Acts of 1889 Ch.s 188, 207, 218 & Legislative Acts of 1890 Ch. 23 – Ch. 25. *Poll Tax* – Legislative Acts of 1890, Ch. 26 Effective March 1890.

black voter turnout?

We feel that it is important to first address the motives driving lynching for two reasons: first, by drawing on modern econometric methodologies we provide new quantitative evidence on the determinants of lynching; second (and more importantly), the forces that drive lynching shape the interpretation of our results on the effects of lynching on turnout. In particular, because our outcome measure in the difference-in-differences analysis of subsection 4.2 is political participation, a relationship between political participation and the counties that are selected for “treatment” (i.e., lynching in an election year) would raise substantial concerns around the validity of our empirical approach. Because lynching ultimately does not appear to be politically motivated, we feel confident in drawing causal inference on the impacts of lynching on black voter turnout, although we of course conduct a variety of robustness tests in subsection 4.2 to address additional concerns.

#### 4.1. Was lynching politically motivated?

Our analysis starts with the question: can we find evidence that lynching was politically motivated? We begin by looking at the raw data on lynching. As noted above, the HAL database includes information about the victims’ alleged offenses. This information was originally based on newspaper accounts and other journalistic reports. Of the 2462 lynchings in our data, only two report offenses involving politics or voting. Far more commonly, the alleged offense is related to murder



**Fig. 3.** Average number of lynchings of African Americans across all years by weeks before/after the first week of November (all states, 1882–1912).

(42.8% of observations) or rape (30.5% of observations).<sup>8</sup> Of course, as Tolnay and Beck argue, the stated motivation for any given lynching might mask deeper underlying causes. Hence, building on Tolnay and Beck (who were writing 25 years ago as sociologists, not as economists concerned with identification), we perform a number of simple tests in search of evidence that a political threat from African Americans drove whites to lynch blacks.

First, if lynching were motivated by a political threat, we would expect lynching to spike during election years, and to fall off sharply in the months following an election. **Figs. 1 and 3** provide no evidence of such patterns. **Fig. 1** plots total lynchings (across all states in our sample) by year, with Congressional election years shaded in red. **Fig. 3** plots total lynchings by week for the weeks before and after the first week of November, when an election would occur if it is an election year. Lynchings in election years are plotted in red, while lynchings in other years are plotted in blue. In both figures, there is no clear evidence that there is a dramatically larger number of lynchings in election years. Moreover, **Fig. 3** reveals no evidence of a sudden drop-off in lynching after an election beyond the general downward trend that appears to occur in all years, regardless of whether an election was taking place or not. In **Appendix A (Section A.1)**, we further probe this point using regression analysis. There we show: (1) the likelihood that a county experiences a lynching is not affected by whether or not it is an election year; and (2) although lynching is generally less likely to occur in the months following the first week of November in election years, it is not *differentially* less likely to occur after the first week of November in election years. Finally, we also show that these patterns hold both in counties with small and large black populations. This is important as we take advantage of cross-county variation in the size of the black population in our main analysis.<sup>9</sup> In the same section of the Appendix (**Table A.2**), we report a second test taking advantage of the fact that Mississippi and Louisiana hold within-state elections (e.g., Gubernatorial elections, state legislative elections) in odd years. If lynching were entirely politically motivated, we would expect higher lynching rates in Mississippi and Louisiana in odd years,

<sup>8</sup> In both cases, these figures include murder and rape, but also include assistance in such crimes or “intent to murder or rape.

<sup>9</sup> Of course, it remains possible – despite the evidence presented in **Figs. 1 and 3** – that lynching is politically motivated, but powerful individuals who encouraged lynching for political purposes did not have precise control over the timing of mob formation. We think that this is unlikely in light of historical evidence discussed in a previous section. In particular, as Brundage notes: most mobs were either (1) “private mobs” (family and friends of alleged victims of a crime) who were not spurred on by local officials or other power individuals, or (2) “mass mobs,” which, while potentially organized by locally powerful individuals did occur quite quickly after the alleged crime, and therefore would be picked up in our figures.

but the same rate of lynching as other states in even years. Instead, Mississippi and Louisiana have higher rates of lynching than other states regardless of whether there is a federal Congressional election in a given year.

Next, we ask if lynching declines after the introduction of disenfranchising laws (ballot box laws, poll taxes, literacy tests). In a companion paper, we show that these laws have a negative impact on black turnout (Jones et al., 2012). We draw on evidence presented in that paper on one state (North Carolina) to provide an illustrative example. Fig. 4 plots our estimates of white turnout, black turnout, and the ratio of black-to-white turnout in North Carolina. Vertical dotted lines indicate the adoption of ballot box laws, poll taxes, and literacy tests. Notably, black turnout declines upon the adoption of these laws, falling to roughly zero. While North Carolina represents a particularly stark case, Fig. 4 provides a sense of the large impact of disenfranchising laws.<sup>10</sup>

Given this result, if whites were using lynching strategically to deter black turnout, and lynching was costly, we would expect lynching to fall off sharply after these laws were passed because the political motivation for lynching would have been negligible. Put another way, because the laws drove black voter turnout to essentially zero, no further reduction in black turnout could be had by lynching.

Given the variation across states in the timing of the introduction of disenfranchising laws, we employ a difference-in-differences strategy to assess the impact of disenfranchisement on the likelihood of lynching. Specifically, variations on the following equation are estimated:

$$\text{AnyLynching}_{ct} = a + \beta_1 [\text{Any Law}]_{ct} + \beta_2 [\% \text{black}]_{ct} + \beta_3 [\% \text{Repub.}]_{c(t-2)} + \theta_c + \tau_t + \varepsilon_{ct} \quad (1)$$

where “Any Lynching” is an indicator variable equal to one if a lynching occurs in county  $c$  in year  $t$ .<sup>11</sup> “Any Law” is our difference-in-differences “treated” indicator and is equal to one if any law listed in Table 1 has been enacted in the state containing county  $c$  in year  $t$ . We also control for the percent of county  $c$ 's population that is black, and – in some specifications – interact percent black with the “Any Law” dummy. Similarly, we control for the vote share that went to the Republican candidate in the previous Congressional election ( $[\% \text{Repub.}]_{c(t-2)}$ ), and – in some specifications – interact that with “Any Law”. We also include county fixed effects ( $\theta_c$ ) and year fixed effects ( $\tau_t$ ).

Results are reported in Table 2. Column 1 reports the results of estimating Equation (1). As Tolnay and Beck (1999) argue in their “political threat model,” if lynching is politically motivated then we would expect lynching to decline after more formal means of disenfranchising black voters have been enacted. Table 2 provides no evidence that this is the case. The coefficient on “Any Law” (the “treatment effect” of disenfranchising laws on lynching) cannot be statistically distinguished from zero. Of course, these results may conceal heterogeneity across counties: it may be that any changes in lynching are local to counties with a large black population, as black voters (or Republican voters) do not represent a “political threat” in areas where they are a sufficiently small part of the population. We explore this possibility in Columns 2 and 3, which interact the “Any Law” indicator with percent black and lagged Republican vote share (Column 2) or indicator variables indicating whether a county has a low (<40%), medium (>40%, but <60%), or high (>60%) percent black or lagged Republican vote share (Column 3). Both specifications test for the possibility that the laws might have a larger impact in areas with a higher share of African Americans or Republican voters. Column 3 allows for the possibility that this is especially true in areas where African Americans or Republicans represent a pivotal group within the electorate (>40%, but <60%). Ultimately, in both columns, we see that the effect of

<sup>10</sup> More details on the construction of estimates reported in Fig. 4 and similar estimates for other states can be found in Jones et al. (2016).

<sup>11</sup> Results are similar here and in the previous table if we use “rate of lynching per 1000 blacks” as our outcome measure.

disenfranchising laws on lynching does not vary with the size of the black population or Republican presence within the county.

Juxtaposed with more narrative treatments of the postbellum South, our finding that lynching was not regularly and systematically motivated by the political threat African Americans posed to whites might seem surprising. There is, for example, much anecdotal evidence to support the view that whites used fraud, violence, and intimidation to discourage blacks from voting and secure Democrat control of the electoral system. Indeed, Southern politicians often publically announced and celebrated their use of violence and fraud to suppress black political influence (e.g., Tindall, 1952). These observations, however, do not necessarily imply that lynching was the instrument of choice. On the contrary, because lynching was very costly to administer, one expects that it would have been used rarely and that Southern politicians may have preferred other modes of voter suppression. More precisely, lynching is an informally administered death penalty, and as such, it is an extreme measure that exposed its perpetrators to at least some risk of criminal and social sanctions.

Along these lines, the data above make it clear that a black man had to be accused of a fairly serious transgression before whites felt free to organize a lynch mob. Hence, if white politicians wanted to discourage blacks from voting, there were probably more parsimonious and lower-cost methods that could be aimed and timed more precisely and exposed the perpetrators to fewer risks. The logic here parallels Fryer (2016) who shows that racially biased police officers today are not more likely to shoot a black suspect than a white, but they are far more likely to engage in less costly modes of discrimination. Of course, none of this analysis precludes the possibility that lynching had an effect on voter turnout among blacks even though it was not designed to do so, which we explore in the next subsection.

## 4.2. The impact of lynching on black voter turnout

### 4.2.1. Empirical approach

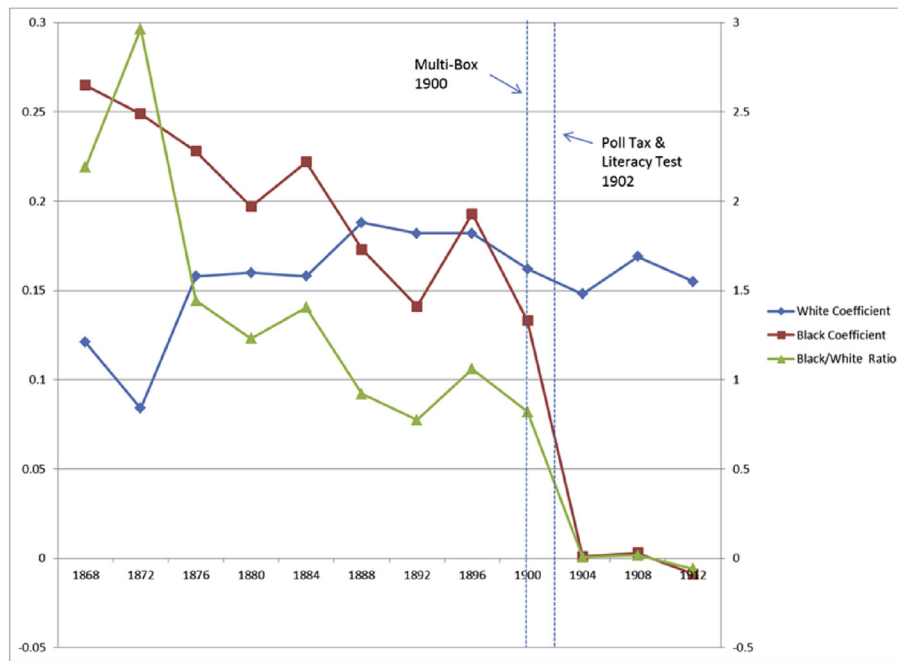
We now turn to the main focus of our paper: Does lynching (even if not politically motivated) have an impact on black voters' turnout rates? To answer this question, we use a modified difference-in-differences strategy, estimating the impact of a lynching within a county on local turnout. As explained above, because we cannot directly observe race-specific turnout in our data, we take advantage of the variation in the size of the black population across counties (and across time) to draw inference on the differential impact of lynchings on black turnout. The approach taken here most closely mirrors the ecological inference methodology of Goodman (1953, 1959).<sup>12</sup>

Specifically, we estimate variations of the following equation:

$$\text{Turnout}_{ct} = a + \beta_1 [\text{Lynch0}]_{ct} + \beta_2 [\% \text{black} * \text{Lynch0}]_{ct} + \beta_3 [\text{Any Law}]_{ct} + \beta_4 [\% \text{black} * \text{Any Law}]_{ct} + \beta_2 [\% \text{black}]_{ct} + \theta_c + \tau_t + \varepsilon_{ct} \quad (2)$$

Turnout<sub>ct</sub> measures the turnout rate in Congressional elections in county  $c$  in election year  $t$ . As is standard for a difference-in-differences approach, we include county fixed effects and year fixed effects.

<sup>12</sup> A number of methodological advances have been made in the area of Ecological Inference since Goodman's early work. In general, the newer methodologies attempt to improve inference on group-specific participation rates by placing additional cross-electorate structure on the process that generates group-specific turnout rates. In general this work has not addressed the type of “treatment-control” analysis that we undertake here (see for instance Hudson et al., 2010 for overviews). We have not adopted these approaches in our analysis for two reasons. First, even if these methods could be adopted for the analysis here, we are concerned that the regularity that they impose on cross-electorate group-level turnout rates would work to smooth out just the type of discrete, policy-driven (lynching) differences that we seek to identify. Second, these approaches are by their nature fairly complicated. Thus, while Hudson et al. (2010) demonstrate that these methods can markedly improve the quality of inference on group-level turnout, they don't “solve” the Ecological Inference problem and could make it more difficult to interpret the potential sources of bias in the type of analysis we undertake.



Note: This figure is drawn from a companion paper (Jones, Troesken, and Walsh, 2012) on the effects of disenfranchising policies on black voter turnout. Here, we reproduce estimate black (and white) voter turnout rates for one illustrative state (North Carolina).

Fig. 4. Turnout in North Carolina.

“Lynch0” indicates that a lynching has occurred within county *c* in election year *t* (prior to the election).<sup>13</sup> In the difference-in-differences framework, the “Lynch” indicator variable is our treatment. However, to identify the effect of a lynching on turnout amongst African-Americans in particular, we include the interaction of percent black (within the county in year *t*, “% black”) and the lynching indicator. The coefficient on the interacted term provides an estimate of the differential impact of lynching on black turnout.

To see why, consider first a simple model of turnout where white turnout is unaffected by black turnout and vice versa. This model is given by:

$$\text{Total votes cast} = a(\text{Total white population}) + b(\text{Total black population})$$

In this model, “a” measures the likelihood of a white citizen voting and “b” measures the likelihood of a black citizen voting. Dividing through by total population yields:

$$\text{Turnout rate} = a(\text{white frac. of pop.}) + b(\text{black frac. of pop.})$$

In our sample, “white frac. of pop.” is roughly equal to 1-“black frac. of pop.” We can rewrite the simple model as:

$$\text{Turnout rate} = a + (b-a)(\text{black frac. of pop.})$$

Hence, in a regression of Turnout on “% black”, the coefficient on “% black” reveals black voters' differential rate of turning out (relative to white voters). In our regression equation above, interacting “% black” with the lynching indicator identifies how the black voters' differential rate of turning out *changes* when a lynching occurs and is therefore of primary interest.<sup>14</sup>

<sup>13</sup> The “0” in “Lynch0<sub>ct</sub>” is meant to indicate that the lynching occurred within county *c*. Shortly, we will introduce similar variables indicating that a lynching occurred within, for instance, 100 miles of county *c*, which we will call “Lynch100”.

<sup>14</sup> We use a similar approach in Jones et al. (2016) to test the effects of poll taxes and literacy tests on black turnout. In that paper, we conduct a variety of tests to probe the validity of the approach.

The model in Equation (2) identifies the impact of a lynching in county *c* in year *t* on African American turnout in that county. However,

Table 2  
Relationship between disenfranchising laws and lynching.

Variables	(1) Any lynching in county	(2) Any lynching in county	(3) Any lynching in county
Any law	0.017 (0.016)	0.025 (0.025)	0.020 (0.022)
Pct. black	0.119 (0.202)	0.135 (0.201)	
Any law X Pct. black		-0.021 (0.041)	
Lagged Repub. Vote Pct.	-0.004 (0.027)	-0.009 (0.045)	
Any Law X Lagged Repub. Vote Pct. Pct. black: (0.4, 0.6)		0.002 (0.051)	-0.007 (0.022)
Pct. black: [0.6, 1]			0.012 (0.037)
Law X % black (0.4, 0.6)			0.025 (0.019)
Law X % black [0.6, 1]			-0.008 (0.023)
Lag Pct. Repub.: (0.4, 0.6)			0.050 (0.103)
Lag Pct. Repub.: [0.6, 1]			-0.013 (0.016)
Law X Lag % Repub.: (0.4, 0.6)			-0.076 (0.110)
Law X Lag % Repub.: [0.6, 1]			-0.006 (0.023)
County & year FE's	X	X	X
Observations	10,919	10,919	10,919
R-squared	0.115	0.115	0.116

there is little reason to expect that the effect of violence is contained within county borders. To address this concern, in some specifications we broaden our definition of the lynching “treatment,” considering an observation to be treated if a lynching occurred either within the given county or in a neighboring county. A county is considered “neighboring” if it is less than 100 miles from the reference county (measured from county centroid to county centroid). In specifications using this broader definition of lynching, we replace our “Lynch0” indicator with a “Lynch100” indicator which is equal to one if there is a lynching within county  $c$  or within any county within 100 miles of county  $c$ . Admittedly, theory provides no guidance on how far or near a lynching must be in order to generate a local impact on turnout, so we adopt the 100 mile radius merely to provide some sense of the impact of nearby lynchings. We do, however, conduct analysis later in the paper testing the effect of lynchings within concentric circles around a county with radii both smaller and larger than 100 miles.

The main threat to our approach is ecological bias or ecological fallacy. A typical ecological fallacy concern would be the possibility that white and black turnout rates are related to the size of the black population within a county. For instance, if white voters turnout at higher rates in counties with a large share of black voters, a simple ecological inference approach would erroneously attribute the higher white turnout to African American voters.<sup>15</sup> The inclusion of county-level fixed effects in all of our specifications largely addresses this concern. (While there is some degree of within-county variation in percent black, county fixed effects largely capture whether a county has a “high” or “low” share of black voters and therefore minimize such concerns.) In our analysis, the issue is more nuanced. Specifically, the main threat to our approach is the possibility that a lynching causes a disproportionate reduction in white turnout (relative to black turnout) in counties with a large share of black voters but not in counties with a low share of black voters. In this case, it would appear as though a lynching has a negative impact on black turnout when in fact white voters drive the result.

We take two specific steps to see if lynching is working to deter mainly black turnout, and having little, if any, effect on white turnout. First, blacks voted Republican, while whites tended to vote Democrat.<sup>16</sup> Given the paucity of white Republicans, if lynching were mainly affecting black turnout, we should only observe lynching having an effect on Republican turnout. With this in mind, we test for the impact of lynching on Democratic and Republican turnout separately.

Our second approach for addressing concerns about ecological bias involves the disenfranchisement laws described in Table 1. As explained above, black turnout quickly approach zero once disenfranchising laws have been enacted. Thus, after a law has been enacted, lynching cannot further impact black turnout. To account for this, we fully interact “Lynch0”, “% black”, and “Lynch0 \* % black” with the dummy indicating that “Any Law” has been passed. The coefficient on that non-interacted “Lynch0 \* % black” remains of primary interest. That coefficient identifies the differential impact of lynching on black turnout *prior* to the introduction of disenfranchising laws. Given the interaction with “Any

Law”, the model can be thought of as a triple-difference model. Essentially, our claim is that if lynching has an impact, it should be on voters within counties that have recently experienced a lynching (or near those counties, as we discuss below) but that have not yet experienced “formal” means of disenfranchisement. Post-“Any law” observations experiencing a lynching are essentially a placebo test; there should be limited impact there. This triple-difference approach, in addition to allowing us to focus our main results on observations where lynching may have an impact on black turnout, allows us to probe concerns around ecological inference. In other words, if lynching is correlated with changes in turnout amongst white voters in high black counties (which in turn is mistakenly being identified as an impact on black turnout), there would be no reason to expect that correlation to disappear once formal means of disenfranchisement (which primarily impact black voters) have been enacted. (One concern with this approach is that the laws could have been endogenous, and a response to declining lynching rates; we address this possibility later in the paper.)

Finally, as an additional test of our ability to accurately identify black and white rates of voter participation, in Appendix B we draw on data from Louisiana which reports parish-level, race-specific registration counts for a subset of the elections in our sample period. Using these data, we are able to show that black and white voter registration rates are not impacted by the racial composition of the parish. Second, implementing a basic version of our ecological inference approach, we calculate predicted black voter registration rates and compare these to the actual registration rates, ultimately finding that predicted rates are quite similar to actual rates.<sup>17</sup>

#### 4.2.2. Main results

As explained above, after the disenfranchising laws, there were no African Americans left for lynching to disenfranchise because poll taxes and literacy requirements had already relegated all (or nearly all) blacks to the status of non-voter. Given this observation, if our ecological decomposition approach is working correctly, and all lynching-induced reductions in turnout are driven by changes in the behavior of African-Americans, we would expect the effects of lynching on turnout to be concentrated in the years preceding passage of poll taxes and literacy requirements, which left blacks completely disenfranchised.

We begin by reporting results from simple specifications wherein we do *not* allow the effect of lynching to depend on whether a disenfranchising law has been passed, and therefore expect to find that lynching has a relatively small impact on voting. These results are in Table 3. Columns 1–3 and Columns 4–6 differ only in that Columns 1–3 examine the impact of a lynching that occurs within the same county as the one being observed (the “Lynch0” indicator), while Columns 4–6 allow lynching to impact turnout in neighboring counties within 100 miles (the “Lynch100” indicator). In both cases, the first column (Columns 1 and 4) report results from the baseline model. Columns 2 and 5 add state-specific linear trends. Columns 3 and 6 include state-year fixed effects. All specifications reveal a relatively small impact of lynching on black voter turnout, with black turnout falling by roughly 1 percentage point (relative to white turnout in the same county-year pairings); this estimate is imprecise in Columns 1–3.

Table 4 reports results from our main specifications where the effects of lynching are allowed to vary across the pre- and post-disenfranchisement periods (that is, the triple-difference approach). The table is formatted similarly to Table 3: Columns 1–3 estimate the impacts of a within-county lynching, Columns 4–6 estimate the impacts of a “nearby” lynching. For brevity, we report only the primary coefficients of interest. In all six models, the coefficient on the interaction between lynching and percent black (which identifies the differential effect of a lynching on black turnout rates prior to the adopting of disenfranchising laws) is between 0.02 and 0.04 during the period in which

<sup>15</sup> It is worth noting that there are many ecological fallacy-related concerns that are minimized based on the patterns observed in subsection 4.1. In particular, the concern that turnout rates (for white or black voters) are higher in counties with a large black population would be a very serious concern if it was also true that lynching was substantially more likely in counties with a large black population. Table 2 shows that there is no evidence that is true.

<sup>16</sup> As Kantowitz (2000, p. 100–01) explains: “The legislators intended the law [ballot box laws] to be applied racially, for by the early 1880s, most white voters supported Democrats and most black voters backed Republicans. Black Democrats hung on in a few low country areas, but their importance was more symbolic than substantive. Similarly, a few white men continued to support the Republican Party. For example, in July 1880, “as Republicans crowded the Spartanburg County courthouse to discuss the fall campaign, an upcountry Democratic newspaper acknowledged the presence of ‘fifty or sixty whites’ among the participants. But white Republicans were heavily burdened by the bitter legacy of the federal occupation and the ever-growing legend of Republican corruption, and they remained outliers in a party that was almost entirely black.”

<sup>17</sup> See Appendix B for more detail.



**Table 3**  
Effect of lynching on black voter turnout (difference-in-difference-style models).

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout
Lynch0 X % Black	−0.007 (0.008)	−0.009 (0.007)	−0.010 (0.007)			
Lynch0	0.003 (0.003)	0.003 (0.003)	0.004 (0.003)			
Lynch100 X % Black				−0.009*** (0.004)	−0.011*** (0.003)	−0.008** (0.003)
Lynch100				0.003** (0.001)	0.003** (0.001)	0.002* (0.001)
Any law X % Black	−0.071*** (0.006)	−0.074*** (0.007)	−0.080*** (0.007)	−0.070*** (0.006)	−0.073*** (0.007)	−0.080*** (0.007)
Any law	0.001 (0.003)	−0.004 (0.003)	0.078*** (0.010)	0.001 (0.003)	−0.004 (0.003)	0.078*** (0.010)
Pct. black	0.041 (0.034)	0.043 (0.028)	0.067** (0.028)	0.046 (0.034)	0.048* (0.029)	0.071** (0.028)
County & Year FE's	X	X	X	X	X	X
State-specific trends		X			X	
State-year FE's			X			X
Observations	8595	8595	8595	8595	8595	8595
R-squared	0.790	0.802	0.860	0.790	0.802	0.860

Robust standard errors (clustered at county-version level) in parentheses \*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.1.

blacks still have voting rights, and in all models the interaction is significant at the 10 percent level or higher. Moreover, we see that the coefficient on the triple interaction (Lynch X Law X Pct. Black) is positive and almost perfectly offsets the coefficient on the simple interaction for the pre-law period (Lynch X Pct. Black). Thus, in the years where black voters still had access to the ballot box, lynching has a clear negative impact on local turnout rates. After disenfranchisement, as one would expect (unless our results were driven by something other than a change in black turnout is driving the result), lynching does not affect black turnout – because it is already essentially zero.

In the Appendix, we report a series of additional tests. In Appendix Table A.3, we adopt a slightly more flexible model wherein we allow lynchings that occur in the second half of the year (that is, closer to an election) to have a differential impact. In models allowing for an impact of lynchings within 100 miles of a county, we find some evidence that this matters: lynchings later in the year have a larger impact. The same is not true when our “treatment” definition is restricted to lynchings that occur within the county being observed; there, our estimates of the differential effect of a late-year lynching are imprecise.<sup>18</sup>

A second table in the appendix (Appendix Table A.4) tests whether our estimated impact of lynching on turnout is *genuinely* picking up the effect of lynching on turnout, or if the lynching variable merely serves as a proxy for general racial tension and/or failure to provide African Americans protection under the law (either of which could explain lower average black turnout). However, note that if lynching merely serves as a proxy for unobserved local factors, then it should not matter that a lynching occurs prior to an election. Instead, a lynching immediately after an election should serve as an equally good proxy. This motivates the test we conduct: If lynching directly impacts turnout, then *only* lynchings in the months leading up to an election should matter. If lynching merely serves as a proxy for other phenomena, then lynchings in the months following an election should also “predict” reduced black turnout. To test this, Appendix Table A.4 reports results from specifications where we include a dummy variable indicating that a lynching has occurred in a county (or near a county) in the year *following* an election. As with the main lynching indicator, this is interacted with percent black

<sup>18</sup> This could be driven by the fact that within-county lynchings are relatively rare (mechanically, they occur less often than lynchings that are *either* within-county or within-100 miles); splitting the sample based on timing of lynching may excessively reduce identifying power.

and the presence of a disenfranchising law. As the table reports, the relationship between a post-election lynching and turnout is close to zero or imprecisely estimated.<sup>19</sup>

We can also show that the lynching of a *white* person has no effect on black turnout. These results are reported in Appendix Table A.5. This essentially serves as a placebo test: if any lynching impacted black turnout, the link between violence against an oppressed minority and turnout amongst that group is less clear. These results also help to address concerns about the ecological fallacy. If lynchings also discouraged white Republicans from voting (not just blacks), then we would expect to observe a white lynching reducing turnout. There is, however, no evidence of this effect.

An additional placebo test is aimed at further testing the validity of our approach against ecological fallacy concerns. In particular, during this time period women could not vote. Thus, if we replace “Pct. Black” with “Pct. Female” (within a county-year) in our specifications, the coefficients on “Pct. Female” (and all coefficients interacted with “Pct. Female”) should not be significantly different than zero. We show that this is the case in Appendix Table A.6.<sup>20</sup>

Finally, given that prior research has found that lynching is more likely in areas that face a negative economic shock driven by cotton prices, we show in Appendix Table A.7 that our results are robust to including controls for local agriculturally-driven economic shocks.<sup>21</sup> Our main results are unchanged when we include these controls.<sup>22</sup>

Next, we further test whether our results are genuinely driven by reductions in *black* voter turnout by assessing the effects of lynching on party-specific voting. If lynching is working to deter mainly black voter turnout, it should be the case that the results are driven by effects on

<sup>19</sup> One specification yields an estimated effect on turnout significant at the 10% level. That coefficient is roughly half the size of the corresponding estimate from Table 4. Thus, insofar as “lynching as a proxy” has some explanatory power, it cannot explain the entire (or even more than half of the) estimated effect of a pre-election lynching on turnout.

<sup>20</sup> We thank an anonymous referee for suggesting this test.

<sup>21</sup> Specifically, we draw on the agricultural census to identify acreage of three major crops in the South by county (cotton, corn, tobacco) and interact the county-level acreage data with time-series data on prices of each crop. The resulting variable captures fluctuations in local economic conditions based on the interaction of local dependence on these crops and the year-specific global price of the crops.

<sup>22</sup> Though not reported, we also tested whether results vary by the presence of incumbents in the Congressional elections we study, which might be the case if local elites have substantial power over lynchings and use them strategically. We found no heterogeneity by incumbency status.

**Table 4**  
Effect of lynching on black voter turnout (triple-difference-style models, allowing the effect of lynching to vary depending on existing disenfranchising policies).

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout
Lynch0 X % Black	-0.028** (0.013)	-0.027** (0.013)	-0.024* (0.013)			
Lynch0 X % Blk X Law	0.034** (0.016)	0.030* (0.016)	0.023 (0.015)			
Lynch100 X % Black				-0.040*** (0.007)	-0.037*** (0.007)	-0.027*** (0.007)
Lynch100 X % Blk X Law				0.042*** (0.008)	0.036*** (0.007)	0.026*** (0.007)
Any law X % Black	-0.074*** (0.006)	-0.076*** (0.007)	-0.082*** (0.007)	-0.098*** (0.008)	-0.095*** (0.008)	-0.095*** (0.008)
County & Year FE's	X	X	X	X	X	X
State-specific trends		X			X	
State-year FE's			X			X
Observations	8595	8595	8595	8595	8595	8595
R-squared	0.790	0.802	0.860	0.793	0.804	0.861

Robust standard errors (clustered at county-version level) in parentheses \*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.1.

Republican turnout because blacks regularly voted Republican, not Democrat. However, if lynching impacted both white and black voter turnout, and for some reason the effects on white turnout were higher in counties with large black population shares (if one is concerned about ecological fallacy biasing our results, it has to work through this channel), we would expect effects on turnout for both Democrats and Republicans. To assess this possibility, in Table 5, we report results wherein we re-estimate our most robust model (including state-year fixed effects) but take “Republican votes per capita” and “Democratic votes per capita” as outcomes in Columns 1 and 3 and Columns 2 and 4 respectively. Columns 1 and 2 estimate the impact of within-county lynchings, Columns 3 and 4 broaden the treatment to lynchings within 100 miles. The estimate coefficients from Columns 1 and 3 (where we focus on “Republican votes per capita”) are nearly identical to the corresponding estimates from Table 4. That is, the observed decline in turnout appears to be entirely driven by Republican turnout. A lynching has no effect on Democratic votes cast (per capita).

That lynching only affects Republican turnout not only helps to address the concerns about the ecological fallacy; as we explained above (see footnote 15), there were very few white Republicans left in the South after 1880. This result also suggests any potential confounding events associated with the regional economy, such as shocks to cotton prices, are not driving the results in our main specification.<sup>23</sup>

In Table 6, we more flexibly explore the relationship between turnout and distance from a nearby lynching. We have already found that both lynchings within a county and within 100 miles of a county have an impact on black voter turnout. Here, we assess the relative impacts of lynchings at various distances within a single specification. Specifically, we modify our main estimating equation to allow for separate effects of lynchings that occur within a county, within 50 miles of a county (excluding that county), within 100 miles of a county (excluding everything within 50 miles of the county), and within 200 miles of a county (excluding everything within 100 miles of the county). In other words, we allow for distinct effects of lynchings that take place in increasingly distant concentric circles around a county. There are two primary reasons

for doing so. First, this approach allows us to assess the rate at which the effects of lynching decay over space. Second, one might be concerned that general trends in turnout generate a spurious correlation between lynching and voter turnout. If so, one would not expect to observe any decay across space in the effects of lynching: a lynching in a county 200 miles away from county *i* would have the same effect as a lynching in county *i*. Thus, testing the effects of lynchings in concentric circles around a county incorporates a placebo test: there should be a much larger impact of a lynching in county *i* than from a lynching that occurred 100–200 miles away from that county.

Before discussing the results in Table 6, note that “lynch” means that a lynching happened within county *c*; lynch(50 mile radius) means a lynching happened within 50 miles excluding within-county lynchings; lynch(100 mile radius) means lynching happened within 100 miles but not within 50 miles; and so on.

The results of Table 6 indicate that lynchings that occur within some county *c* have the largest impact on turnout in county *c*. Increasing the flexibility of the model also changes the “control” group: here effects are estimated relative to county-year observations that have not experienced a lynching within 200 miles. Thus, our estimated effects of a within-county lynching are larger than they were in Columns 1–3 of Table 4

**Table 5**  
Effect of lynching on party-specific votes.

Variables	(1)	(2)	(3)	(4)
	Turnout for Republican	Turnout for Democrat	Turnout for Republican	Turnout for Democrat
Lynch0 X % Black	-0.026*** (0.007)	0.005 (0.011)		
Lynch0 X % Blk X Law	0.035*** (0.009)	-0.002 (0.013)		
Lynch100 X % Black			-0.022*** (0.005)	0.004 (0.005)
Lynch100 X % Blk X Law			0.027*** (0.006)	-0.009* (0.005)
Any law X % Black	-0.057*** (0.006)	-0.018*** (0.005)	-0.070*** (0.007)	-0.013** (0.006)
County & Year FE's	X	X	X	X
State-year FE's	X	X	X	X
Observations	8595	8595	8595	8595
R-squared	0.731	0.718	0.733	0.719

Robust standard errors (clustered at county-version level) in parentheses \*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.1.

<sup>23</sup> For example, building on Tolnay and Beck’s findings discussed above, one might develop the following hypothesis. Perhaps reductions in cotton prices, and other agricultural commodities, have two effects: they increase lynching; and leave voters dejected and too discouraged to vote. In this case, it would not be lynching that is driving the reduction in turnout, but the change in agricultural prices. To the extent that one expects such mechanisms to affect voters in both parties, one would observe a drop in Democratic as well as Republican turnout. However, we observe only a drop in Republican turnout.

**Table 6**  
Impact of lynching by distance in concentric circles around county.

Variables	(1)	(2)	(3)
	Turnout	Turnout	Turnout
Lynch X Pct. black (Within-county)	-0.056*** (0.016)	-0.053*** (0.016)	-0.046*** (0.015)
Lynch (50 mile radius) X Pct. blk. (Excluding within-county)	-0.038*** (0.011)	-0.036*** (0.010)	-0.027*** (0.010)
Lynch (100 mile radius) X Pct. blk. (Excluding within-50 miles)	-0.044*** (0.010)	-0.040*** (0.010)	-0.033*** (0.009)
Lynch (200 mile radius) X Pct. blk. (Excluding within-100 miles)	-0.006 (0.010)	-0.006 (0.010)	-0.008 (0.009)
County & Year FEs	X	X	X
State-specific trends		X	
State-year FE's			X
<i>Testing difference between impact of within-county lynching and lynching in a concentric circle with radius of X miles from county center</i>			
<i>P-Val. (Diff.: Within- vs. 50-mile)</i>	0.219	0.321	0.171
<i>P-Val. (Diff.: Within- vs. 100-mile)</i>	0.379	0.001	0.333
<i>P-Val. (Diff.: Within- vs. 250-mile)</i>	0.001	0.230	0.010
Observations	8595	8595	8595
R-squared	0.794	0.805	0.862

Robust standard errors (clustered at county-version level) in parentheses\*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.1.

(where the control group consisted of any county-year observation that did not experience a within-county lynching), with estimates suggesting a five percentage point reduction in black turnout relative to white turnout in the same county-year pairings. Consistent with our prior results, lynchings within 50 or 100 miles also have a clear impact on turnout, but the estimated impacts are smaller than for within-county lynchings (although, we note that – in most specifications – we cannot reject the hypothesis that a within-county lynching has a different impact than a lynching within 50 or 100 miles). Lynchings that are between 100 and 200 miles away, on the other hand, have no detectable effect on turnout. Moreover, we can reject that the impact of a local lynching is the same as the impact of a lynching that is between 100 and 200 miles away. In short, Table 6 documents that local lynchings matter much more than distant lynchings. As with the results on Democratic turnout, the results showing that lynching does not have broader regional effects on voting seems inconsistent with the idea that regional economic shocks (e.g., reductions in cotton prices) are generating a spurious correlation between lynching and voter turnout among Republicans.

Finally, we turn to searching for evidence of a dynamic relationship between lynching and turnout. The tests we run here allow us to address two issues. The first is a standard concern for any difference-in-differences model: it should not be the case that “treated” counties follow a measurably different trend in the outcome variable in the years preceding treatment (this is the standard parallel trends assumption). Second, we can test the possibility that lynching might have a lasting effect on voter turnout. This is done by including (and interacting with relevant variables) dummy variables in the regression indicating the number of periods since or until a lynching occurs within a given county, rather than simply including a dummy indicating that a lynching has occurred in the election year of a given observation. We report the results from allowing for dynamic effects of lynching (before and after it occurs) graphically. Fig. 5 plots the estimated effect of lynching on black turnout (that is, coefficients involving the “Lynch X Pct. Black” interaction) for: 2 elections prior to the election year that the lynching took place, 1 election year prior, the election year during which the lynching occurred, 1 election after the lynching occurred, and 2 elections afterwards.

There are two main takeaways: First, there is no evidence of a pre-trend in turnout in counties that will soon experience a lynching. Estimated black voter turnout one to two elections prior to a lynching is not

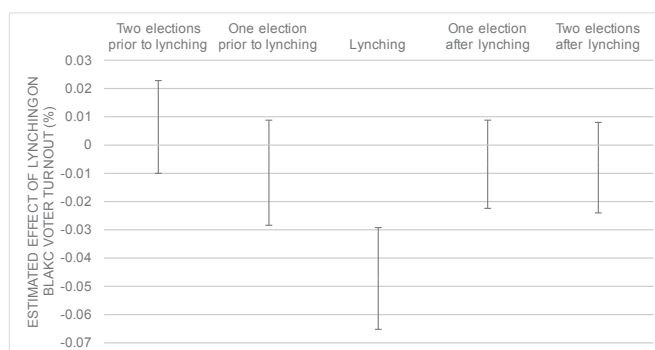


Fig. 5. Dynamics of the effect of lynching on black voter turnout.

statistically distinguishable from turnout in counties that will not soon experience a nearby lynching. Second, there is no evidence of a persistent effect of a lynching on black voter turnout: estimates return to pre-lynching levels starting with the first election after a “treated” county-year observation.

To summarize the findings thus far, we have found that lynching has a clear impact on voter turnout, but these effects are local in time and space. Lynching has the largest impact on turnout in the county where the lynching occurred, and it impacts that county only for the year during which the lynching occurs. We believe that both of these facts point towards a genuine impact of lynching on turnout, rather than an alternative explanation wherein lynching simply reflects some underlying trend which impacts turnout.

There is, however, one final alternative explanation for our results which requires some attention: sorting. In particular, one might worry that lynching induced black out-migration from counties, and this out-migration – rather than a decline in black voting rates – explains our results. We address this concern in two ways. First, results presented in Fig. 5 at least partially address this concern. If out-migration from a county explained the decline in turnout, then (except in the case of very temporary relocation) we would expect a persistent effect of lynching, which was not evident in analyses reported in Fig. 5.

Second, and more directly, we assess whether there is a relationship between lynching and the racial composition of counties. While there is a vast literature exploring how the factors that affected black out-migration after 1910 (during the Great Migration),<sup>24</sup> there is comparatively little evidence for the period under consideration here (1882–1912); and at least some of the literature that does exist for the earlier period (e.g., Alston and Ferrie, 1999) suggests that violence and lynching might have instilled fear and hindered black migration out of the South.<sup>25</sup> To explore the connection between lynching and out-migration for the 1882–1912 period, we construct a county-by-decade panel and estimate a difference-in-differences model assessing whether there is a measurable relationship between county racial composition and recent lynchings. In these models, we take “share of the population that is black” as an outcome variable, and control for whether any lynchings occurred in the county in the preceding decade, county fixed effects, decade fixed effects, and total population. We measure the presence of lynchings in three ways: (1) a simple indicator variable if any lynching occurred within the county in the preceding decade (equal to 1 for roughly 50% of county-decade observations), (2)

<sup>24</sup> For the factors shaping black migration during the Great Migration, see the following: Black et al. (2015), Collins (1997), Collins and Wanmaker (2014, 2015), Fryer and Levitt (2012), and Hornbeck and Naidu (2014).

<sup>25</sup> Probably the paper with the evidence most relevant to our analysis here is Christian (2014), who explores the effects on lynching on black migration for two periods: 1910 to 1920; and 1920 to 1930. Christian finds no effect for the 1910 to 1920 period, and small effects for the 1920 to 1930 period. Like Christian's results for the 1910 to 1920 period, we find no evidence that lynching generated black outmigration during the period before 1912. See Christian (2014), Table A.2.

**Table 7**  
Lynching in the most recent decade and shifts in the size of the black population across Censuses.

Variables	(1)	(2)	(3)
	Percent black	Percent black	Percent black
Any lynch (1/0)	0.002 (0.002)		
Total lynchings		0.001 (0.001)	
Lynchings/1000 Black pop.			-0.001 (0.001)
County FE's	X	X	X
Decade FE's	X	X	X
Observations	2269	2269	2269
R-squared	0.987	0.987	0.987
99% conf. interval around est. impact of 2 S.D. increase in lynching	[-0.00407, 0.00734]	[-0.00342, 0.00941]	[-0.00644, 0.00368]

Robust standard errors in parentheses \*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.1.

the number of lynchings within the county in the preceding decade, and (3) the rate of lynchings per 1000 blacks in the county.

Results are reported in Table 7. Notably, we see no clear relationship between lynching and the composition of counties. For instance, the results reported in Column 2 suggest that the presence of a lynching within a county in the preceding decade leads to a 0.2 percentage point change in the black population share. Other measures of lynching (Columns 2 and 3) reveal a similarly small average effect. To provide a sense of the precision of the estimated effects around zero and a uniform way to compare the magnitudes of the effects of the three distinct lynching measures, we calculate the estimated effect of a two standard deviation increase in each lynching measure and report the 99% confidence interval around that effect in the bottom panel of the table. It is worth noting that a two standard deviation increase in the primary measure (“any lynching”) is roughly equivalent to switching from a county without a lynching to a county with at least one lynching. Because it is out-migration that we are concerned about, the lower ends of the confidence intervals are of primary interest. Our results suggest that, even at the extreme left end of the confidence interval, the magnitude of the effect of lynching on the size of the black population ranges from 0.3 percentage points (Column 2) to 0.6 percentage points (Column 3).

How much of our estimated effect of lynching on turnout can be explained by any possible impact of lynching on out-migration? Column 1 is most directly comparable to the lynching indicator employed in main analyses, so we compare the possible impact of a lynching on racial composition from that column (−0.4 percentage points at the far left end of the 99% confidence interval) to our estimate of the impact of lynching on turnout from Table 4. There we estimated that a lynching led to a 2.4 percentage point decline in black voting rates. Thus, even allowing for an extreme realization of the impact of lynching on out-migration, that relationship can only explain at most 1/6th of the effect on turnout.<sup>26</sup> If we instead take the average of effect of lynching on out-migration, we would estimate that 1/12th of the impact on turnout is driven by out-migration.

Thus, while out-migration in response to lynching is an important alternative explanation for our findings, the evidence suggests it does not drive our results. We do not see a persistent effect of lynching on turnout (as you might expect if out-migration was the explanation). Moreover, there is limited evidence that lynching has any impact on out-migration

<sup>26</sup> Likewise, it is worth noting that the upper tail of the 99th confidence interval is positive, and suggests an increase in the black share of the population of roughly 0.4 percentage points in response to two standard deviation increase in lynching frequency. Under this realization, we would in fact be underestimating the effect of lynching on turnout.

in the period we study. In our most conservative approach, it appears that out-migration has the ability to explain just 1/6th of our estimated effect of lynching on turnout. In contrast, Tolnay and Beck (1992) estimate a simultaneous equations model to explore how lynching and migration interact, and they find evidence of larger effects. Their analysis, however, focuses on periods after 1910 and the onset of the Great Migration. Most of our identification is coming from the years before 1910, when African Americans still had access to the ballot box.

The lynchings we have studied in this paper thus far are “completed” lynchings in the sense that a mob formed and the target of the lynching was killed. More recently, researchers have compiled data on “averted” lynchings (Hagen et al., 2013; Makovi et al., 2016; Tolnay et al., 2016); situations where a mob formed, but there was some form of intervention before the target of the lynching could be killed. Hagen et al. (2013) report that 1/3rd of all mob formation events were averted. Most commonly, local law enforcement intervened, though in some cases private citizens intervened (Makovi et al., 2016).

We draw on Makovi, Hagen, and Bearman's (2016) data on averted lynchings in three Southern states (Georgia, Mississippi, North Carolina) to examine whether only completed lynchings reduced turnout or whether it was the mere threat of violence that reduced turnout (as in averted lynchings).<sup>27</sup> To do so, we simply repeat our main specification, but replace the “Lynch0” (or “Lynch100”) indicator with an “Averted Lynch0” (or “Averted Lynch100”) indicator. “Averted Lynch0” is equal to one if a lynching was attempted in a county-year pairing, but was averted (or, in the case of “Averted Lynch100”, an averted lynching happened within 100 miles of the county). If the mere threat of violence drives our results, then results in these specifications should be similar to our main results. If, on the other hand, the completion of a lynching drives our results then we would expect no impact of an averted lynching.

First note that in Appendix Table 8, we show that our main results (the impact of completed lynchings) hold in the limited set of states available in the Makovi et al. data. Table 8 reports the impacts of averted lynchings on turnout. In short, we find no impact on turnout from averted lynchings. Thus, our main results were not driven by the mere threat of violence, but the completion thereof. While we hesitate to draw too strong of an inference from these results, this pattern is consistent with a narrative wherein lynchings have a negative effect on turnout when they serve as an example of an absence of justice—this would be true of a completed lynching (where local law enforcement did *not* intervene), but not an averted lynching.

## 5. Conclusion

How does criminal violence impact political participation? Blanco (2013) provides evidence that victimization or fear of victimization is associated with reduced trust in government and electoral systems. Thus, violence could have either a positive or negative impact on voter turnout. Either voters turn out to replace the existing government with one that will provide protection and justice, or voters become discouraged and turn away from the political process altogether. Some existing work in the development literature points towards the former possibility (e.g. Bellows and Miguel, 2009; Blattman, 2009). We revisit this question in an historical setting (i.e., the post-Reconstruction American South) relevant to elections in many developing countries.

Our central results are twofold. First, lynching in the American South does not appear to have been strategic or politically motivated in the sense of being designed to discourage blacks from voting. There is, for example, no systematic evidence that lynching fell off during election years, in the months following an election, or in the wake of laws disenfranchising African Americans. Nor is there any evidence of heightened lynching rates in competitive electoral areas. These patterns comport well with the qualitative historical literature suggesting that

<sup>27</sup> We thank these authors for generously sharing their data on averted lynchings.

**Table 8**

Effect of averted lynchings in Georgia, Mississippi, and North Carolina (triple-difference-style models, allowing the effect of lynching to vary depending on existing disenfranchising policies).

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout
Averted Lynch0	0.005	0.012	0.014			
X % Black	(0.039)	(0.041)	(0.030)			
Averted Lynch0	−0.010	−0.012	−0.018			
X % Blk X Law	(0.041)	(0.042)	(0.032)			
Averted Lynch100				0.002	0.004	−0.001
X % Black				(0.013)	(0.014)	(0.012)
Averted Lynch100				0.006	0.007	0.012
X % Blk X Law				(0.015)	(0.015)	(0.013)
Any law X % Black	−0.094***	−0.115***	−0.113***	−0.096***	−0.117***	−0.116***
	(0.011)	(0.010)	(0.010)	(0.011)	(0.010)	(0.010)
County & Year FE's	X	X	X	X	X	X
State-specific trends		X			X	
State-year FE's			X			X
Observations	2801	2801	2801	2801	2801	2801
R-squared	0.891	0.896	0.909	0.891	0.896	0.909

Robust standard errors (clustered at county-version level) in parentheses \*\*\*p &lt; 0.01, \*\*p &lt; 0.05, \*p &lt; 0.1.

lynching was a form of ritualized violence, typically aimed at punishing blacks for (perceived) extreme violations of established norms, color lines, and laws. These patterns also create an historical setting that complements more recent work in development economics that explores the impact of politically-motivated violence. By contrast, our setting lends itself to an exploration of the political implications of ostensibly non-political violence.

Second, although lynching was not politically motivated, it does appear to have discouraged voter participation rates among African Americans. The evidence for this proposition is robust to concerns about the ecological fallacy. If, for example, lynchings were also impacting voter participation among whites, we would expect to observe an impact in voter turnout among Democrats, but we do not. The result is also

robust to concerns about lynching causing a drop in voter turnout by inducing black out-migration. We show, for example, that out-migration must have been unrealistically large to explain our results, and find little evidence consistent with the proposition that lynching altered black migration patterns during our study period. Aside from the development literature, these findings inform our understanding of American economic history. From the early 1900s through the passage of the Voting Rights Act in 1965, African Americans in the U.S. South were fully disenfranchised and denied access to the voting booth. As [Cascio and Washington \(2014\)](#) show, denying blacks the right to vote had significant effects on the distribution of public funds. This paper presents the first systematic evidence on the role that lynching played in undermining black access to the voting booth.

## Appendix A

### A.1 Assessing timing of lynching relative to elections

Here we use formal regressions to test if the timing lynchings is related to elections. As in [Fig. 3](#), we restrict the analysis to the 20 weeks immediately before and after the first week of November for all years from 1882 to 1912, and treat the 20 weeks before and after as distinct time periods. We regress a dummy variable indicating that a lynching has occurred in a 20-week period in a given county on indicators for whether the 20-week period is after the first week of November (“post-Nov. 8th”), whether the year in question is an election year, and the interaction of the two. We also include county and year fixed effects, as well as a control for the share of the population that is black. Results are reported in Table A.1. Following Tolnay and Beck, if lynching is politically motivated we would expect more lynching in election years, and a sharp drop in lynching just after an election (post-Nov. 8th) in election years; there is no reason to expect a sharp drop after Nov. 8th in off years. The specification reported in Column 1 of Table A.1 omits year fixed effects so that we can identify the average effect of election years on lynchings. We instead include a linear time trend in that specification. Notably, the “Election year” indicator reveals no clear difference between elections years and off years in the likelihood that a county will experience a lynching. The interaction term “Post-Nov. 8th X Election year” is also very close to zero. This suggests that there is no dropoff in lynching just after an election beyond the general decline in lynchings that happens every year (as indicated by the negative coefficient on “Post-Nov. 8th”). Column 2 reports the results of the specification including year fixed effects. There, we cannot assess the general difference between election years and non-election years, as the “Election year” indicator is absorbed by year fixed effects. However, we can still assess whether there is a decline in lynchings just after elections above and beyond the normal trend (“Post-Nov. 8th X Election year”). As in Column 1, there is no evidence of a differential decline after an election.

Finally, it is worth considering whether we detect a different pattern in counties with especially large black populations. This is important for two reasons: first, we might expect that politically motivated lynching occurs exclusively in counties with a large – and therefore politically important – black population; second, our identification strategy for our main analysis takes advantage in variation in the size of the black population to identify black turnout rates. With this in mind, Columns 3 and 4 report results separately for observations below and above the median of black population share (0.37). The pattern of results is generally similar across the two columns.

**Table A.1**  
Timing of lynching.

Variables	(1)	(2)	(3)	(4)
	Any lynching	Any lynching	Any lynching	Any lynching
Post-Nov. 8th	−0.009*** (0.002)	−0.009*** (0.002)	−0.007*** (0.002)	−0.012*** (0.004)
Election year	−0.002 (0.002)			
Post-Nov. 8th X Election year	0.000 (0.003)	0.000 (0.003)	0.002 (0.003)	−0.001 (0.005)
Linear time trend	X			
Year FEs		X	X	X
County FEs	X	X	X	X
Sample restriction			Below median share black	Above median share black
Observations	42,356	42,356	21,284	21,072
R-squared	0.042	0.044	0.043	0.042

Robust standard errors (clustered at county-version level) in parentheses\*\*\*p &lt; 0.01,

\*\*p &lt; 0.05, \*p &lt; 0.1.

A related test takes advantage of the fact that Mississippi and Louisiana hold within-state elections in odd years (gubernatorial elections, state legislative elections), but – of course – hold Congressional elections (our main focus in this paper) at the same time as other states. If lynching were purely politically motivated we might expect to see higher rates of lynching in Mississippi and Louisiana compared to other states in odd years (as we do, Column 1 of Table A.2), but these rates would equalize in even years. Note that in the table below, “Election year” still refers to “Congressional election year”, or even year. We do not find that lynching in MS and LA equalizes with other states in even years (Election year X MS or LA dummy in all columns of Table A.2); instead, it simply appears that MS and LA have a higher rate of lynching than other states, and this is independent of whether it is an election year in other states or not.

**Table A.2**  
Lynchings by election year in MS or LA vs. other states.

Variables	(1)	(2)	(3)
	Any lynching	Any lynching	Any lynching
Election year	−0.001 (0.001)	−0.001 (0.001)	
MS or LA	0.021*** (0.005)		
Election year X MS or LA	−0.006 (0.005)	−0.006 (0.005)	−0.006 (0.005)
Year trend	X	X	
Year dummies			X
County fixed effects		X	X
Observations	42,356	42,356	42,356
R-squared	0.008	0.041	0.043

Robust standard errors (clustered at county-version level) in parentheses \*\*\*p &lt; 0.01,

\*\*p &lt; 0.05, \*p &lt; 0.1.

## A.2 Additional results

**Table A.3**  
Allow for differential effect of lynching that occurs in the second half of an election year.

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout
Lynch0 X % Black	−0.044** (0.017)	−0.045*** (0.017)	−0.037** (0.016)			
LateLynch0 X % Blk.	0.028 (0.025)	0.031 (0.025)	0.028 (0.021)			
Lynch0 X % Blk X Law	0.045** (0.020)	0.045** (0.021)	0.038** (0.019)			
LateLynch0 X % Blk X Law	−0.020 (0.027)	−0.026 (0.028)	−0.031 (0.024)			
Lynch100 X % Black				−0.036*** (0.007)	−0.033*** (0.007)	−0.024*** (0.007)
LateLynch100 X % Blk				−0.015* (0.008)	−0.014* (0.007)	−0.014** (0.007)
Lynch100 X % Blk X Law				0.038*** (0.008)	0.032*** (0.008)	0.022*** (0.007)
LateLynch100 X % Blk X Law				0.018** (0.008)	0.017** (0.008)	0.018** (0.007)

Table A.3 (continued)

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout
Any law X % Black	-0.074*** (0.006)	-0.076*** (0.007)	-0.082*** (0.007)	-0.105*** (0.008)	-0.102*** (0.008)	-0.102*** (0.008)
County & Year FE's	X	X	X	X	X	X
State-specific trends		X			X	
State-year FE's			X			X
Observations	8595	8595	8595	8595	8595	8595
R-squared	0.791	0.802	0.860	0.794	0.805	0.862

Robust standard errors (clustered at county-version level) in parentheses \*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.1.

Table A.4

Allow for impact of lynching in the next year on election outcomes in current year.

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout
Lynch0 (next year)	-0.005 (0.016)	-0.000 (0.015)	-0.019 (0.013)			
X % Black						
Lynch100 (next year)	0.015 (0.017)	0.008 (0.016)	0.027* (0.014)			
X % Blk X Law						
Lynch100 (next year)				-0.009 (0.007)	-0.005 (0.007)	-0.011* (0.006)
X % Black						
Lynch100 (next year)				0.012 (0.008)	0.007 (0.008)	0.017** (0.007)
X % Blk X Law						
Any law X % Black	-0.075*** (0.006)	-0.077*** (0.007)	-0.084*** (0.007)	-0.106*** (0.009)	-0.101*** (0.009)	-0.105*** (0.009)
County & Year FE's	X	X	X	X	X	X
State-specific trends		X			X	
State-year FE's			X			X
Observations	8595	8595	8595	8595	8595	8595
R-squared	0.791	0.803	0.861	0.794	0.805	0.862

Robust standard errors (clustered at county-version level) in parentheses \*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.1.

Table A.5

Impact of a lynching with a white victim on black turnout.

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout
Lynch0 (white victim)	-0.014 (0.020)	-0.008 (0.020)	-0.000 (0.015)			
X % Black						
Lynch0 (white victim)	0.013 (0.030)	0.005 (0.029)	-0.004 (0.023)			
X % Blk X Law						
Lynch100 (white victim)				0.002 (0.008)	0.005 (0.008)	0.000 (0.007)
X % Black						
Lynch100 (white victim)				-0.006 (0.010)	-0.008 (0.009)	-0.005 (0.009)
X % Blk X Law						
Any law X % Black	-0.071*** (0.006)	-0.074*** (0.007)	-0.080*** (0.007)	-0.070*** (0.007)	-0.072*** (0.007)	-0.079*** (0.007)
County & Year FE's	X	X	X	X	X	X
State-specific trends		X			X	
State-year FE's			X			X
Observations	8595	8595	8595	8595	8595	8595
R-squared	0.790	0.802	0.860	0.790	0.802	0.860

Robust standard errors (clustered at county-version level) in parentheses \*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.1.

**Table A.6**

Placebo test: Effect of lynching on female voter turnout (triple-difference-style models, allowing the effect of lynching to vary depending on existing disenfranchising policies).

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout
Lynch0 X % Female	0.011 (0.200)	0.054 (0.192)	0.046 (0.174)			
Lynch0 X % Fem X Law	0.064 (0.243)	-0.061 (0.231)	-0.160 (0.217)			
Lynch100 X % Female				0.076 (0.153)	0.148 (0.152)	0.006 (0.140)
Lynch100 X % Fem X Law				0.070 (0.177)	-0.041 (0.177)	-0.013 (0.162)
Any law X % Female	0.067 (0.132)	0.130 (0.159)	0.062 (0.166)	0.023 (0.166)	0.146 (0.174)	0.050 (0.177)
County & Year FE's	X	X	X	X	X	X
State-specific trends		X			X	
State-year FE's			X			X
Observations	8594	8594	8594	8594	8594	8594
R-squared	0.774	0.788	0.844	0.774	0.788	0.845

Robust standard errors (clustered at county-version level) in parentheses \*\*\*p &lt; 0.01, \*\*p &lt; 0.05, \*p &lt; 0.1.

**Table A.7**

Effect of lynching on black voter turnout, with controls for agricultural economic shocks (triple-difference-style models, allowing the effect of lynching to vary depending on existing disenfranchising policies).

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout
Lynch0 X % Black	-0.025* (0.013)	-0.025* (0.013)	-0.022* (0.013)			
Lynch0 X % Blk X Law	0.032** (0.015)	0.029* (0.015)	0.021 (0.014)			
Lynch100 X % Black				-0.037*** (0.007)	-0.035*** (0.007)	-0.026*** (0.007)
Lynch100 X % Blk X Law				0.040*** (0.008)	0.035*** (0.007)	0.025*** (0.007)
Any law X % Black	-0.076*** (0.006)	-0.076*** (0.007)	-0.084*** (0.007)	-0.098*** (0.008)	-0.094*** (0.008)	-0.097*** (0.008)
Corn Acres <sub>c</sub> * Price <sub>t</sub> (z-score)	-0.001 (0.003)	-0.001 (0.002)	-0.002 (0.002)	-0.001 (0.003)	-0.001 (0.002)	-0.002 (0.002)
Tobacco Acres <sub>c</sub> * Price <sub>t</sub> (z-score)	-0.014*** (0.003)	-0.014*** (0.003)	-0.006*** (0.002)	-0.013*** (0.003)	-0.013*** (0.003)	-0.006*** (0.002)
Cotton Acres <sub>c</sub> * Price <sub>t</sub> (z-score)	0.005*** (0.001)	0.003** (0.001)	0.004*** (0.001)	0.004*** (0.001)	0.003** (0.001)	0.004*** (0.001)
County & Year FE's	X	X	X	X	X	X
State-specific trends		X			X	
State-year FE's			X			X
Observations	8587	8587	8587	8587	8587	8587
R-squared	0.794	0.805	0.861	0.796	0.806	0.862

Robust standard errors (clustered at county-version level) in parentheses \*\*\*p &lt; 0.01, \*\*p &lt; 0.05, \*p &lt; 0.1.

**Table A.8**

Effect of lynchings in Georgia, Mississippi, and North Carolina (triple-difference-style models, allowing the effect of lynching to vary depending on existing disenfranchising policies).

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout
Lynch0 X % Black	-0.039** (0.015)	-0.037** (0.017)	-0.031** (0.016)			
Lynch0 X % Blk X Law	0.052*** (0.018)	0.050*** (0.019)	0.047*** (0.017)			
Lynch100 X % Black				-0.038*** (0.010)	-0.030*** (0.010)	-0.029*** (0.010)
Lynch100 X % Blk X Law				0.048*** (0.012)	0.042*** (0.012)	0.042*** (0.011)
Any law X % Black	-0.097*** (0.011)	-0.117*** (0.010)	-0.115*** (0.010)	-0.117*** (0.012)	-0.132*** (0.010)	-0.129*** (0.010)
County & Year FE's	X	X	X	X	X	X
State-specific trends		X			X	
State-year FE's			X			X
Observations	2801	2801	2801	2801	2801	2801
R-squared	0.891	0.896	0.909	0.893	0.897	0.910

Robust standard errors (clustered at county-version level) in parentheses \*\*\*p &lt; 0.01, \*\*p &lt; 0.05, \*p &lt; 0.1.



**Appendix B. Assessing concerns regarding the ecological fallacy: voter registration in Louisiana**

One concern with our estimates is that we cannot observe vote shares by race. Instead, we use an interaction between lynching and the proportion black to infer the effect lynching on black turn-out rates. This raises the possibility of the well-known ecological fallacy. To assess the severity of biases resulting from our ecological decomposition approach, we turn to one setting where race-specific voting data are available. In particular, race-specific voter registration data are available for Louisiana for the years, 1878, 1880, 1886, 1888, 1896, and 1898. The data are at the county (parish) level. With these data, we can assess whether there is a correlation between percent black and white voters' likelihood of participating in elections. A strong correlation would suggest that our ecological decompositions are biased, as high white turnout in high black counties would lead us to overestimate the likelihood of a black citizen turning out to vote.

In Table B.1, we regress white and black registration rates against the black population share in each parish. The regressions also include year and parish fixed effects. The results indicate that there is no systematic relationship between the black population share and the voter registration rate for either race.

We can also use the Louisiana data perform a benchmarking exercise. To do this, we employ the estimating strategy we described in the text to predict trends in voter registration using the aggregated (black + white) voter turnout data. This is the same indirect measure of turnout we use in the text of the paper. We can then compare our indirect measure with actual black registration in Louisiana. If the behavior of our estimates diverge sharply from the behavior of the true registration rates, we will know that our measure is unreliable because of the ecological bias (or perhaps some other unexplained factor). Alternatively, if the true measure and our estimates behave similarly overtime, we can be more confident in the econometric results reported in the following sections.

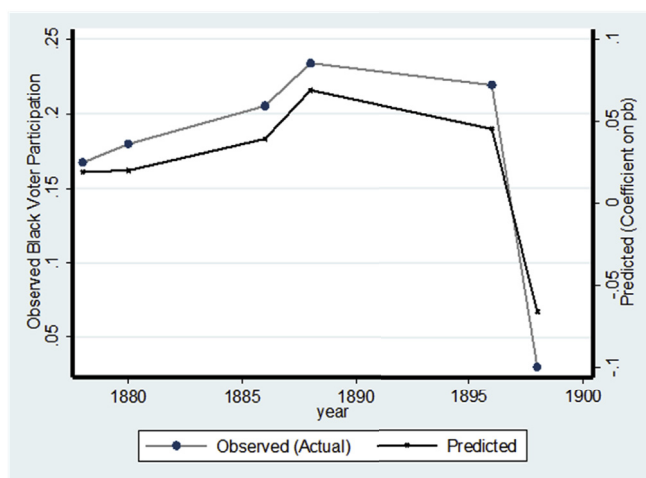
The results of this benchmarking exercise are reported in Figure B.1. Notice that our indirect measure (the coefficient of a regression of overall county level registration against proportion black) maps, almost perfectly, the true or direct measure of black voter registration reported by Louisiana authorities in the nineteenth century. Although there are many missing years in these data, both measures follow a mild upward trend before 1896, and drop by above 15 percentage points in 1898. The patterns in Figure B.1, and the preceding regression results suggest our ecological decomposition does not appear to yield a misleading estimate of black turnout.

**Table B.1**  
Relationship between Percent black and registration rate in Louisiana Parishes.

Variables	(1)	(2)
	White reg. rate	Black reg. rate
Pct. black	0.0238 (0.155)	-0.110 (0.0967)
Constant	0.163* (0.0854)	0.224*** (0.0553)
Parish version and year FE's	X	X
Observations	335	335
Adjusted R-squared	0.542	0.739

Standard errors (bootstrapped and clustered at county version level) in parentheses \*\*\*p < 0.01, \*\*p < 0.05, \*p < 0.1.

Notes to table: Columns 1 and 2 report the relationship between pct. black and white or black registration rates (measured as  $[race] \text{ registration count} / [race] \text{ population count}$ ) for the full sample.



**Fig. B.1.** Observed black voter participation versus estimated black voter participation in Louisiana.

## Appendix C. Supplementary data

Supplementary data related to this article can be found at <http://dx.doi.org/10.1016/j.jdeveco.2017.08.001>.

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