

# Political Participation in a Violent Society: The Impact of Lynching on Turnout in the Post-Reconstruction South

Daniel B. Jones<sup>a</sup>  
Werner Troesken<sup>b</sup>  
Randall Walsh<sup>c</sup>

**Abstract.** How does violence against a group impact 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 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, they reduced local black voter turnout by 2.5 percentage points. A series of tests suggest this relationship can be interpreted as causal.

---

<sup>a</sup> University of South Carolina, Email: [daniel.jones@moore.sc.edu](mailto:daniel.jones@moore.sc.edu)

<sup>b</sup> University of Pittsburgh and National Bureau of Economics Research, Email: [troesken@pitt.edu](mailto:troesken@pitt.edu)

<sup>c</sup> University of Pittsburgh and National Bureau of Economics Research, Email: [walshr@pitt.edu](mailto:walshr@pitt.edu)

## 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>

---

<sup>1</sup> In a field experiment, for example, Collier and Vicente (2013) 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 Person (2011) and Powell (2013).

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 2,000 blacks in the American South.

Exploiting data on lynching and voter turnout, we compile a panel of Southern counties that extends from 1882 to 1912. We then use a difference-in-differences strategy to answer two questions related to the broader frameworks explained above. We first ask if lynching was politically motivated, and run several tests in search of evidence that whites were using 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 to 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, Troesken, and Walsh, 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 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 were simply frightening, 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) 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 disenchanting, disempowered, or disengaged, crime victims should be reconceptualized as political actors—indeed, as potential activists.”

---

<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.

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 turnout rates. While looking at actual turnout rates 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 couple 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 system of governance and justice, while in the settings considered by other recent papers violence do not necessarily signal a broader failure in the justice system. 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

Figure 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, Figure 2 provides a sense of the cross sectional variation in lynching across 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.

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 might have been an element in the system of paternalism, in 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 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.

---

<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.

In their authoritative account of lynching, Tolnay and Beck (1996, 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.) Beck and Tolnay (1990), however, do 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 increase the lynching rate. We adopt multiple strategies to control for cotton price shocks, and other events affecting regional economies, that might confound our empirical analysis, though multiple strategies, including the addition state-year fixed effects to our regressions.

### **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.

The 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 1882-1930. The data report:

---

<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.

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 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 variation in “percent black” across counties and across time to econometrically assess the impact of lynching on black turnout in particular. 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

---

<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 Flanigan, 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.

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 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, does lynching have an impact on 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 impact the interpretation of our results on the impacts 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 in this setting. 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 2,462 lynchings in our data, only two report offenses involving politics or voting. Far more commonly, the alleged offense is related to murder (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

---

<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.

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 expect lynching to spike during election years, and to fall off sharply in the months following an election. Figures 1 and 3 provide no evidence of such patterns. Figure 1 plots total lynchings (across all states in our sample) by year, with Congressional election years shaded in red. Figure 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, Figure 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, 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.

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, Troesken, and Walsh, 2012). We draw on evidence presented in that paper on one state (North

Carolina) to provide one illustrative example. Figure 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, Figure 2 provides a sense of the impact that the disenfranchising laws had.<sup>9</sup>

Given this, 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: because the laws drove black voter turnout to 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:

$$\text{Any Lynching}_{ct} = a + \beta_1 [\text{Any Law}]_{ct} + \beta_2 [\% \text{ black}]_{ct} + \theta_c + \tau_t + \varepsilon_{ct} \quad (1)$$

where “Any Lynching” is an indicator variable equal to one if a lynching occurs in county  $c$  in year  $t$ .<sup>10</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

---

<sup>9</sup> More details on the construction of estimates reported in Figure 2 and similar estimates for other states can be found in Jones, Troesken, and Walsh (2016).

<sup>10</sup> Results are similar here and in the previous table if we use “rate of lynching per 1,000 blacks” as our outcome measure.

the “Any Law” dummy. 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 3 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 do not represent a “political threat” in areas where they are a sufficiently small part of the population. We explore this in Columns 2 and 3, which interact the “Any Law” indicator with percent black (Column 2) or indicator variables indicating whether a county has a low (<40%), medium (>40%, but <60%), or high (>60%) percent black (Column 3). Both test for the possibility that the laws might have a larger impact in areas with a higher share of African Americans. Column 3 allows for the possibility that this is especially true in areas where African Americans represent a pivotal group within the electorate (>40%, but <60%). Ultimately, in both columns, we see that the effect of disenfranchising laws on lynching does not vary with the size of the black population within the county (this can be seen from the coefficients interacting the presence of a law with the continuous control or binary indicators for size of the black population). If anything, these results suggest a very small and only marginally significant *positive* impact of disenfranchising laws on lynching in counties with a small black population.

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 would 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 the 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 simpler 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 precludes the possibility that lynching had an effect 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 impact of lynchings on black turnout. Specifically, we estimate variations of the following equation:

$$\text{Turnout}_{ct} = a + \beta_1 [\text{Lynch0}]_{ct} + \beta_2 [\% \text{ black} * \text{Lynch0}]_{ct} + \beta_3 [\text{Any Law}]_{ct} \quad (2) \\ + \beta_4 [\% \text{ black} * \text{Any Law}]_{ct} + \beta_5 [\% \text{ black}]_{ct} + \theta_c + \tau_t + \varepsilon_{ct}.$$

$\text{Turnout}_{ct}$  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. “Lynch0” indicates that a lynching has occurred within county  $c$  in election year  $t$  (prior to the election).<sup>11</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 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: (Total votes cast)= $a$ (Total white population)+ $b$ (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: Turnout (rate)= $a$ (white frac. of pop.)+ $b$ (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: Turnout= $a$ +( $b$ - $a$ )(black frac. of pop.). Hence, in a regression of Turnout on “%

---

<sup>11</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”.

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>12</sup>

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, there is little reason to expect that the effect of violence is contained within county borders. To address this, 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 the ecological fallacy. Of particular concern is 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 (and if white voter turnout is unaffected by the lynching of an African American), we may

---

<sup>12</sup> We use a similar approach in Jones, Troesken, and Walsh (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.

underestimate the effect of lynching on turnout. Alternatively, if white voters turn out at higher rates in counties with a large share of black voters, but stop turning out after any disenfranchising event occurs (lynching, poll taxes, etc.), we would overestimate the impact of a lynching on black turnout (our estimates would imply that black turnout is falling (in part) because white turnout is in fact declining.)<sup>13</sup>

We, therefore, take two steps to see if lynching is working to deter mainly black turnout, and having a little, if any, effect on white turnout. First, blacks voted Republican, while whites tended to vote Democrat. If lynching was mainly affecting black turnout, we should only observe lynching having an effect on Republican turnout.

The 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 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

---

<sup>13</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 that is true.

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.

#### *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 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; 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 effect of a lynching on black turnout rates prior to the adopting of disenfranchising laws) is between .02 and .04 during the period in which 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 Appendix Table A.2, 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>14</sup>

A second table in the appendix (Appendix Table A.3) 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.3 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 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>15</sup>

Finally, 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.4. 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.

---

<sup>14</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.

<sup>15</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.

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 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, 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; it 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>16</sup>

---

<sup>16</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 Republican turnout. However, we observe only a drop in Republican turnout.

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 “lynch0” 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 (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. 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. Figure 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 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 in two ways. First, results presented in Figure 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 Figure 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>17</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>18</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 measureable 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) 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

---

<sup>17</sup> For the factors shaping black migration during the Great Migration, see the following: Black et al. (2015), Collins (1997), Collins and Wanamaker (2014, 2015), Fryer and Levitt (2012), Hornbeck and Naidu (2014), and Tolnay and Beck (2012).

<sup>18</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.

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 1/6<sup>th</sup> of the effect on turnout.<sup>19</sup> If we instead take the average of effect of lynching on out-migration, we would estimate that 1/12<sup>th</sup> 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

---

<sup>19</sup> Likewise, it is worth noting that the the upper tail of the 99<sup>th</sup> 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.

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 in the period we study. In our most conservative approach, it appears that out-migration has the ability to explain just 1/6<sup>th</sup> of our estimated effect of lynching on turnout.

## **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 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 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.

## References

- Aidt, T. S., & Franck, R. (2015). Democratization under the threat of revolution: Evidence from the Great Reform Act of 1832. *Econometrica*, 83(2), 505-547.
- Alston, L.J., & Ferrie, J.P. (1993). Paternalism in agricultural labor contracts in the U.S. South: Implications for the growth of the welfare state. *American Economic Review*, 83(6):852-76.
- Alston, L.J., & Ferrie, J.P. (1999). *Paternalism and the American welfare state: Economics, politics, and institutions in the U.S. South, 1865-1965*. Cambridge: Cambridge University Press.

- Bateson, R. (2012). Crime victimization and political participation. *American Political Science Review*, 106(03), 570-587.
- Beck, E.M., & Tolny, S.E. (1990). The killing fields of the deep South: The market for cotton and the lynching of blacks, 1882-1930." *American Sociological Review*, 55(4):526-39.
- Bellows, J., & Miguel, E. (2009). War and local collective action in Sierra Leone. *Journal of Public Economics*, 93(11), 1144-1157.
- Black, D. A., Sanders, S. G., Taylor, E. J., & Taylor, L. J. (2015). The impact of the Great Migration on mortality of African Americans: Evidence from the Deep South. *The American economic review*, 105(2), 477-503.
- Besley, T., & Persson, T. (2011). The logic of political violence, *Quarterly Journal of Economics*, 126(4), 1411-45.
- Blattman, C. (2009). From violence to voting: War and political participation in Uganda. *American Political Science Review*, 103(02), 231-247.
- Blanco, L.R. (2013). The impact of crime on trust in institutions in Mexico. *European Journal of Political Economy*, 32(1):38-55.
- Cascio, E. & Washington, E. (2014). Valuing the vote: The redistribution of voting rights and state funds following the Voting Rights Act of 1965. *Quarterly Journal of Economics*, 129(1):379-433.
- Christian, C. (2014). Lynching, labour, and cotton in the U.S. South. Job market paper, Oxford University.
- Collier, P., & Vicente, P. C. (2012). Violence, bribery, and fraud: the political economy of elections in Sub-Saharan Africa. *Public Choice*, 153(1-2), 117-147.
- Collier, P., & Vicente, P. C. (2014). Votes and violence: evidence from a field experiment in Nigeria. *The Economic Journal*, 124(574), F327-F355.
- Collins, W. J. (1997). When the tide turned: Immigration and the delay of the great black migration. *The Journal of Economic History*, 57(03), 607-632.
- Collins, W.J. & Wanamaker, M.H. (2014). Selection and economics gains in the great migration of African Americans: New evidence from linked census data. *American Economic Journal: Applied Economics*, 6(1):220-52.
- Collins, W.J. & Wanamaker, M.H. (2015). The great migration in black and white: New evidence on the selection and sorting of Southern migrants. *Journal of Economic History*, 75(4):947-92.
- Fryer Jr, R. G. (2016). An empirical analysis of racial differences in police use of force (No. w22399). National Bureau of Economic Research.

- Fryer, R. G., & Levitt, S. D. (2012). Hatred and Profits: Under the Hood of the Ku Klux Klan. *The Quarterly Journal of Economics*, qjs028.
- Hornbeck, R., & Naidu, S. (2014). When the levee breaks: Black migration and economic development in the American South. *American Economic Review*, 104(03):963-90.
- Jones, D. B., Troesken, W., & Walsh, R. (2012). A Poll Tax by any Other Name: The Political Economy of Disenfranchisement (No. w18612). National Bureau of Economic Research.
- Powell, R. (2013). Monopolizing violence and consolidating power. *The Quarterly Journal of Economics*, 128(2), 807-859.
- Tindall, G.B. (1952). The question of race in the South Carolina constitutional convention of 1895," *The Journal of Negro History*, 37(3),277-303.
- Tolnay, S. E., & Beck, E. M. (1992). Racial violence and black migration in the American South, 1910 to 1930. *American Sociological Review*, 103-116.
- Tolnay, S. E., & Beck, E. M. (1995). *A festival of violence: An analysis of southern lynchings, 1882-1930*. University of Illinois Press.
- Voors, M. J., Nillesen, E. E., Verwimp, P., Bulte, E. H., Lensink, R., & Van Soest, D. P. (2012). Violent conflict and behavior: a field experiment in Burundi. *American Economic Review*, 102(2), 941-964.

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.

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	0.015 (0.013)	0.014 (0.014)	0.008 (0.011)
Pct. black	0.129 (0.160)	0.129 (0.163)	
Any law X Pct. black		0.001 (0.035)	
Pct. black: (0.4, 0.6)			-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)
State & year FE's	X	X	X
State-specific trends	X	X	X
Observations	10,919	10,919	10,919
R-squared	0.115	0.115	0.116

Robust standard errors (clustered at county-version level) in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

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

VARIABLES	(1) Turnout	(2) Turnout	(3) Turnout	(4) Turnout	(5) Turnout	(6) 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	8,595	8,595	8,595	8,595	8,595	8,595
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 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) Turnout	(2) Turnout	(3) Turnout	(4) Turnout	(5) Turnout	(6) 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	8,595	8,595	8,595	8,595	8,595	8,595
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

Table 5: Effect of lynching on party-specific turnout

VARIABLES	(1) Turnout for Republican	(2) Turnout for Democrat	(3) Turnout for Republican	(4) 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	8,595	8,595	8,595	8,595
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

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

VARIABLES	(1) Turnout	(2) Turnout	(3) 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	8,595	8,595	8,595
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

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

VARIABLES	(1) Percent black	(2) Percent black	(3) 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	2,269	2,269	2,269
R-squared	0.987	0.987	0.987
<i>99% conf. interval around est. impact of 2 S.D. increase in lynching</i>	<i>[-0.00407, 0.00734]</i>	<i>[-0.00342, 0.00941]</i>	<i>[-0.00644, 0.00368]</i>

Robust standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Figures**

Figure 1: Counts of lynchings of African Americans by year for all states in sample (1882-1912)

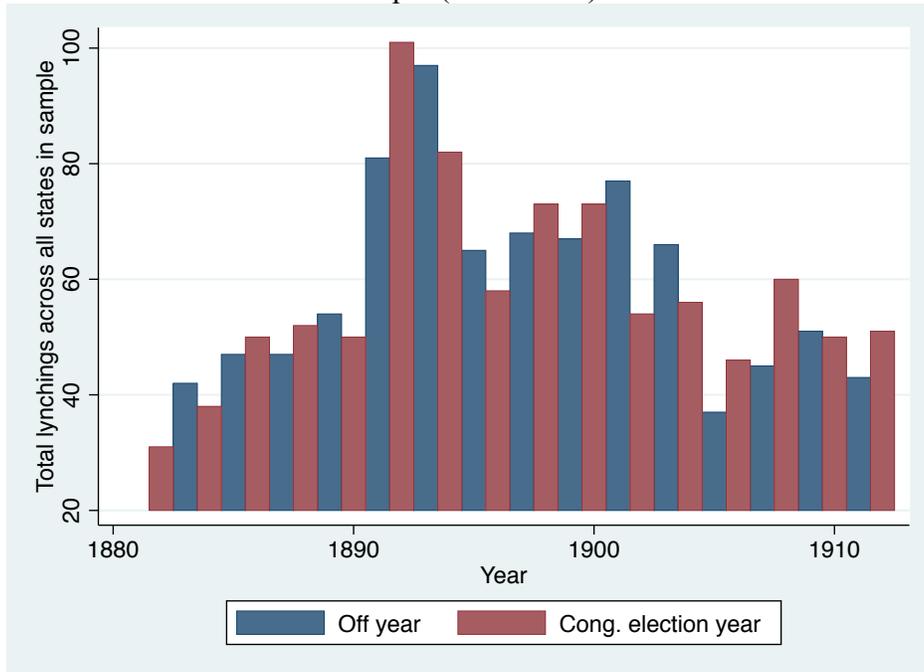


Figure 2: Counts of lynchings of African Americans by state for all years in sample (1882-1912)

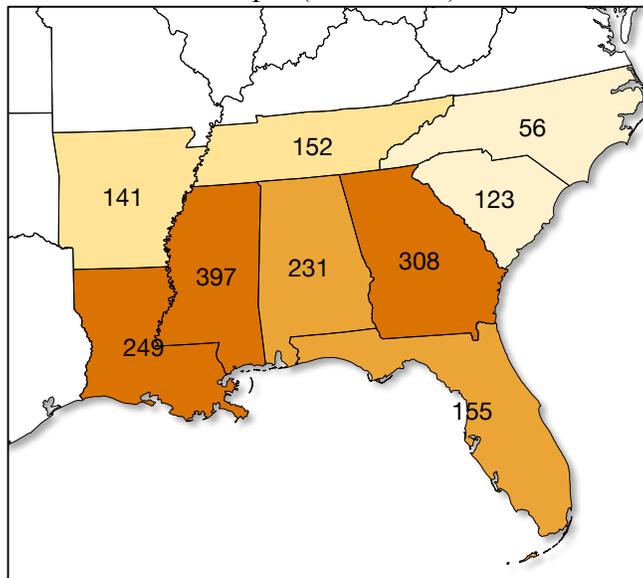


Figure 3: Average number of lynchings of African Americans across all years by weeks before/after the first week of November (all states, 1882-1912)

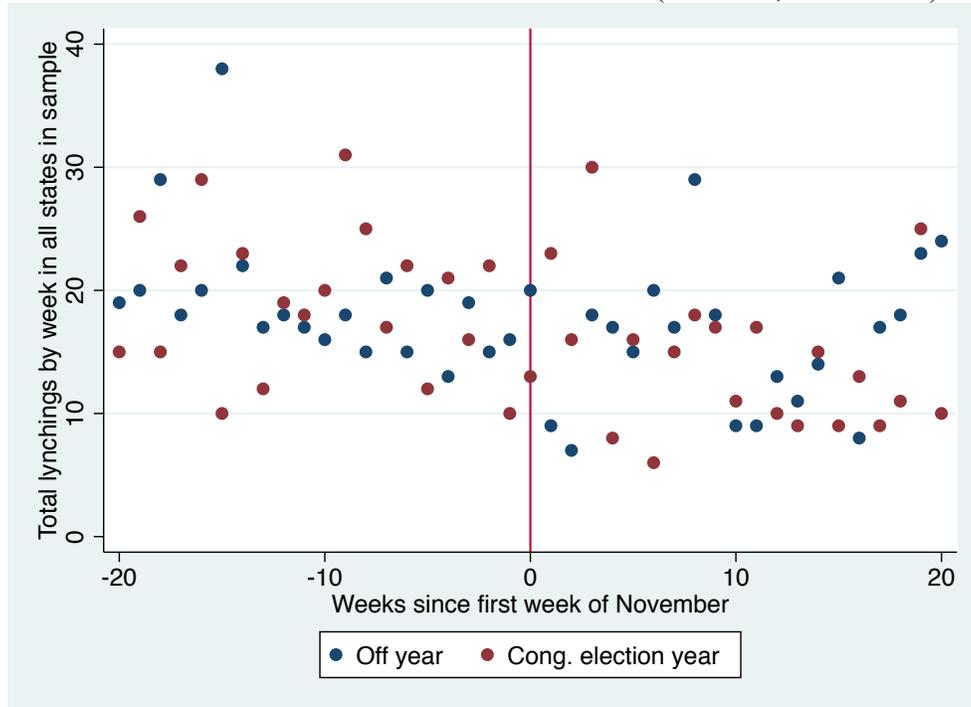
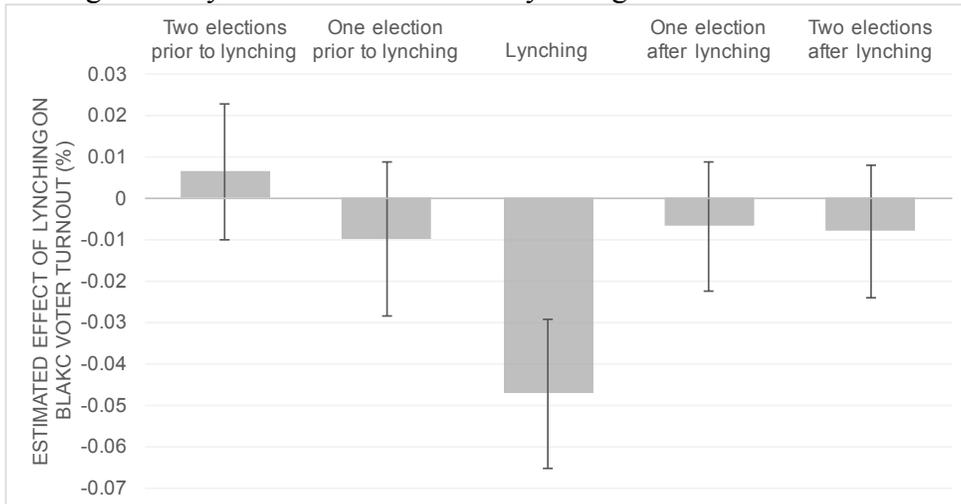


Figure 4: Turnout in North Carolina



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).

Figure 5: Dynamics of the effect of lynching on black voter turnout



## Appendix A

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

We further probe this last point through regression analysis. As in Figure 3, we restrict analysis to the 20 weeks immediately before and after the first week of November for all years from 1882-1912, 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. 8<sup>th</sup>”), 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. 8<sup>th</sup>) in election years; there is no reason to expect a sharp drop after Nov. 8<sup>th</sup> 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. 8<sup>th</sup> 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. 8<sup>th</sup>”). 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. 8<sup>th</sup> 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.

Appendix Table A.1: Timing of lynching

VARIABLES	(1) Any lynching	(2) Any lynching	(3) Any lynching	(4) Any lynching
Post-Nov. 8 <sup>th</sup>	-0.009*** (0.002)	-0.009*** (0.002)	-0.007*** (0.002)	-0.012*** (0.004)
Election year	-0.002 (0.002)			
Post-Nov. 8 <sup>th</sup> 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<0.01, \*\* p<0.05, \* p<0.1

A.2 Additional results

Appendix Table A.2: Allow for differential effect of lynching that occurs in the second half of an election year

VARIABLES	(1) Turnout	(2) Turnout	(3) Turnout	(4) Turnout	(5) Turnout	(6) 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* (0.008)	-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)
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	8,595	8,595	8,595	8,595	8,595	8,595
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

Appendix Table A.3: Allow for impact of lynching in the next year on election outcomes in current year

VARIABLES	(1) Turnout	(2) Turnout	(3) Turnout	(4) Turnout	(5) Turnout	(6) Turnout
Lynch0 (next year)	-0.005	-0.000	-0.019			
X % Black	(0.016)	(0.015)	(0.013)			
Lynch0 (next year)	0.015	0.008	0.027*			
X % Blk X Law	(0.017)	(0.016)	(0.014)			
Lynch100 (next year)				-0.009	-0.005	-0.011*
X % Black				(0.007)	(0.007)	(0.006)
Lynch100 (next year)				0.012	0.007	0.017**
X % Blk X Law				(0.008)	(0.008)	(0.007)
Any law X % Black	-0.075***	-0.077***	-0.084***	-0.106***	-0.101***	-0.105***
	(0.006)	(0.007)	(0.007)	(0.009)	(0.009)	(0.009)
County & Year FE's	X	X	X	X	X	X
State-specific trends		X			X	
State-year FE's			X			X
Observations	8,595	8,595	8,595	8,595	8,595	8,595
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

Appendix Table A.4: Impact of a lynching with a white victim on black turnout

VARIABLES	(1) Turnout	(2) Turnout	(3) Turnout	(4) Turnout	(5) Turnout	(6) Turnout
Lynch0 (white victim)	-0.014	-0.008	-0.000			
X % Black	(0.020)	(0.020)	(0.015)			
Lynch0 (white victim)	0.013	0.005	-0.004			
X % Blk X Law	(0.030)	(0.029)	(0.023)			
Lynch100 (white victim)				0.002	0.005	0.000
X % Black				(0.008)	(0.008)	(0.007)
Lynch100 (white victim)				-0.006	-0.008	-0.005
X % Blk X Law				(0.010)	(0.009)	(0.009)
Any law X % Black	-0.071***	-0.074***	-0.080***	-0.070***	-0.072***	-0.079***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
County & Year FE's	X	X	X	X	X	X
State-specific trends		X			X	
State-year FE's			X			X
Observations	8,595	8,595	8,595	8,595	8,595	8,595
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