

Political Foundations of Racial Violence in the Post-Reconstruction South*

Patrick A. Testa[†] Jhacova Williams[‡]

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Abstract

This paper shows how the geography of racial violence across the post-Reconstruction U.S. South was tied to the local political performance of the anti-Black Democratic Party. Analyzing close elections using a regression discontinuity design, we find that Southern counties where Democrats lost in presidential elections between 1880 and 1900 were nearly twice as likely to experience Black lynchings over the subsequent four years. Indicating elite influence, Democratic losses simultaneously precipitated a rise in Black crime allegations in local Democrat-affiliated newspapers. An early substitute for later de jure voter suppression, such violence played a critical role in the Democratic Party's consolidation of the South, together with the political and economic disenfranchisement of Southern Black people in the early 20th century. To help interpret these findings, we present a formal theory in which electoral losses signal local political weakness, rendering more salient the threat of minority opposition and inducing local elites to foment violent backlash.

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[†]Tulane University, Department of Economics and the Murphy Institute. Email: ptesta@tulane.edu.

[‡]American University, Department of Public Administration and Policy. Email: jhacovaw@american.edu.

“[T]he Negro’s vote became an important factor in all matters of state and national politics. But this did not last long. . . ‘No Negro domination’ became the new legend on the sanguinary banner of the sunny South, and under it rode the Ku Klux Klan, the Regulators, and the lawless mobs, which for any cause chose to murder one man or a dozen as suited their purpose best.”

—Ida B. Wells, *The Red Record* (1895)

1 Introduction

Racial violence was a pervasive feature of life in the U.S. South after the American Civil War (1861–65). Among the most common forms was lynching, which became widespread by the 1890s before gradually declining in the 20th century.¹ All told, more than 3,750 lynchings were carried out across the country between 1882 and 1932, with around 79% of those targeting Black people and 81% occurring in the states of the former Confederacy.²

Despite the prominence of lynching in American history, considerable debate exists over its underlying causes. Contemporary observers viewed lynching as an instrument for stifling Black empowerment after emancipation (Cutler, 1905; Johnson, 1924; Wells, 1892, 1895). Yet, to date, empirical evidence is limited that the rise of lynching stemmed from a perceived threat of free Black populations to white political hegemony (Jones et al., 2017). Prevailing accounts focus on the role of negative economic shocks (Raper, 1993; Tolnay and Beck, 1995) and the enforcement of traditional racial norms (Brundage, 1993) in describing lynchings’ deeper roots.

We provide systematic evidence that political factors directly shaped the dynamics of lynching activity across the South. From the end of Reconstruction in 1877 through the late 19th century, a resurgent all-white, conservative Democratic Party continued to face local political competition from those seeking to build multiracial coalitions throughout the region. We show that where Democrats suffered electoral defeat, lynchings of Black people surged.

Using a regression discontinuity (RD) design based on close elections, we identify the county-level effects of Democratic losses in presidential elections between 1880 and 1900 on the probability of lynching. Our estimates indicate that a Democratic electoral loss in a county resulted in a 9.3–11.0 percentage point (p.p.) increase in the probability of it experiencing at least one Black lynching event during the subsequent four-year period—a 66–79% increase on the sample mean. We estimate no such effect for white lynchings. These findings are robust to (i) alternative running polynomials, (ii) augmenting the MSE-optimal bandwidth to be smaller or larger, (iii) including an array of flexible controls for county demographic and spatial characteristics, (iv) controlling for historical factors related to slavery, (v) omitting individual states and election periods from the sample, and (vi) alternative measures of the outcome variable.

¹As is standard, this paper adopts a definition of lynchings as (i) extrajudicial killings, committed (ii) by mobs of three or more people and (iii) by reference to race, justice, or tradition (Seguin and Rigby, 2019).

²This is based on a combined sample of lynchings from the Historical American Lynching (HAL) Project (Hines and Steelwater, 2023) and Seguin and Rigby (2019), as shown for the former Confederate states in Figure 1.

Our RD estimates imply large impacts of electoral outcomes on the extensive margin, with even narrow Democratic losses in presidential elections triggering sizable increases in local racial violence. We perform several additional exercises to clarify the source of this discontinuity and assure a causal interpretation of our results. First, we find no evidence of endogenous sorting of electoral outcomes across a wide range of relevant observables. Overall, close election results appear unrelated to pre-treatment county characteristics—nor were they associated, given our focus on presidential elections, with post-election changes in county-level policy to which local racial violence might have been endogenous. Second, we perform heterogeneity analysis based on previous election outcomes. Effects are larger in counties where Democratic losses followed more comfortable Democratic wins, yet small and insignificant in places where previous elections had also seen Democratic defeat or been otherwise close. Such results are suggestive of the *signaling* potential of electoral outcomes, which we explore formally later in the paper. Under this interpretation, unexpected failure by Democrats to win in a given county, even narrowly, served to reveal the relative strength of the opposition, triggering a backlash.

Having established that Black lynchings occurred in direct response to local electoral outcomes, we then provide multifarious evidence on the strategic use of lynching as a political mechanism. Our findings suggest that lynching was not only used to intimidate Black voters but, in fact, systematically *promoted* among local Democratic elites to that end.

First, we show that Black lynchings were effective in boosting local Democratic performance in the South. Conditional on a county having experienced a Black lynching, a close Democratic defeat in a presidential election between 1880 and 1900 strongly predicts Democratic *victory* in elections after 1900. The same reversal of electoral fortune is not observed among counties with no history of lynching, with racial violence serving as a necessary condition for that effect. This outcome coincided with reductions in both political and economic participation among Black people. Among counties where Black lynchings occurred, close Democratic losses between 1880 and 1900 are associated with 14–19% lower voter turnout in the 1904–12 elections; 7–23% lower Black school enrollment in 1910; and 5–10% lower Black literacy as of 1910. The latter underscores a critical feedback mechanism, insofar as literacy tests served to further dampen Black political participation in the 20th century. Black political engagement remained depressed in these places through the time of the Voting Rights Act, as measured by a 15–41% reduction in Black voter registration in the 1960s. Together, these results highlight lynching’s various political and economic chilling effects.

Second, we show that increases in Black lynching followed Democratic electoral losses specifically in places where de jure forms of voter suppression had yet to be introduced. Using information on the timing of state election laws from [Jones et al. \(2012, 2017\)](#), we condition our RD estimation on a county’s exposure to poll taxes and other restrictions. Whereas Democratic losses led to increases in Black lynchings in places where Democratic elites were not yet entrenched through state-level Jim Crow laws, racial violence was *not* significantly more likely to follow Democratic electoral losses after the introduction of such laws. This is consistent

with lynching as a substitute for later de jure forms of voter suppression.

Lastly, we explore the role of local elites in promoting such racial backlash. We show that Southern newspapers, which often had ties to the Democratic Party, tended to spotlight stories about Black-committed crime in the aftermath of Democratic losses. Such accusations, which became pervasive in the post-Reconstruction period, were frequently invoked by lynch mobs (Wells, 1895). Using a variant of our RD strategy based on within-city variation in local newspaper content, we examine whether Democratic defeat in a given city’s county predicts a proliferation of published stories about Black-committed rape, murder, or robbery. Estimates suggest that a Democratic electoral loss in a city’s county between 1880 and 1900 resulted in a 43–98% increase in the frequency of Black crime accusations in that city’s newspapers—similar in size to our lynching effects. These results are (i) driven by newspapers with Democratic affiliations, (ii) contingent on accusations involving Black people, and (iii) robust to alternative accusation measures and empirical specifications. These findings suggest that Democratic elites made use of local newspapers for propagating anti-Black narratives, helping to operationalize racial hatred necessary for galvanizing post-election lynchings.

Our empirical findings are consistent with a political theory of lynching, in which revelations of local political weakness among Democrats prompted greater elite investment in violence against Black people. Such violence proved effective in boosting local Democratic strength, thus playing a role in consolidating the South behind the Democratic Party by the early 20th century. In the final part of the paper, we develop a formal model to interpret our findings, as well as to conceptualize the strategic use of violence by local elites against minority (e.g., racial, ethnic) individuals more generally. In the model, electoral outcomes provide a signal of the local elite’s strength, which in turn influences minority citizens’ level of acquiescence to the elite. Critically, an electoral loss *reveals* elite weakness and emboldens minority opposition. It follows that a weak-type local elite will invest more in fomenting violence after electoral losses, with larger investments made when the elite’s support among the majority is otherwise relatively high. Overall, this formal framework helps to clarify the interactions between the informational and strategic aspects of the political mechanism in our empirical analysis.

This paper offers new insight into the origins and consequences of racial violence in the U.S. South in the late 19th century. We provide the first quantitative evidence in support of a causal interpretation of political factors as a key driver of lynching, validating the early observations of contemporary journalists (Johnson, 1924; Wells, 1892, 1895) and sociologists (Blalock, 1967; Corzine et al., 1983; Cutler, 1905; Reed, 1972). These findings help explain lynching’s well-documented political *effects* (Jones et al., 2017; Williams, 2022),³ while corroborating existing descriptive evidence for its political foundations (Epperly et al., 2020; Hagen et al., 2013;

³Notably, Jones et al. (2017, 40) do not find evidence of lynching as being “strategic or politically motivated.” A key difference between our studies is of empirical strategy. Whereas they explore correlations between vote shares and lynching on the intensive margin (see their Table 2), our RD approach identifies causal effects based on electoral variation on the extensive margin, with otherwise non-monotonic variation on the intensive margin.

Olzak, 1990).⁴ This contrasts with the dominant, economic explanation for lynching, tying it to Black-white competition in the struggling postbellum cotton sector (Raper, 1993; Tolnay et al., 1989; Tolnay and Beck, 1995; Feigenbaum et al., 2020), as well as recent work emphasizing perceived Black violations of traditional racial norms and laws (Jones et al., 2017; Masera et al., 2022), which were often proximate to the incitement of lynch mobs. While we do not dispute an influence of these factors, our results affirm the importance of political ones. The latter were arguably first order: absent the political threat posed by Black people, there likely would not have been the same threat to white economic power, nor would elites have had the same incentive to fan racial outrage.⁵ Indeed, we find that both Black economic progress and narratives of Black deviancy and aggression were endogenous to local political conditions.

Our findings also relate to research in empirical political economy and development on the role of elites in shaping anti-minority sentiment through media (Adena et al., 2015; Blouin and Mukand, 2019; Voigtlander and Voth, 2015; Wang, 2021; Yanagizawa-Drott, 2014), a subset of which focuses on the intersection of politics, media, and race in the U.S. context (Ang, 2023; Bazzi et al., 2023b; Bernini et al., 2023; Esposito et al., 2023). Masera et al. (2022) study the proliferation of anti-Black narratives and violence in response to demand-side factors related to fears of racial mixing after the Civil War. This paper takes a step back to explore the supply-side foundations of these phenomena, with Black empowerment favoring elite investments in anti-Black hatred in order to suppress the political threat posed by Black people. Conceptually, this supports the framework of Glaeser (2005) on the supply of “hate-creating stories” by politicians. Separately, Ottinger and Posch (2022) study, as we do, the strategic use of newspapers by Southern elites in defense of white supremacy. Yet, our applications are distinct; while we focus on the use of anti-Black narratives in promoting racial violence and suppressing Black political participation, their emphasis is on the electoral mobilization of Southern whites against the Populist political threat. In the case of both Masera et al. (2022) and Ottinger and Posch (2022), we see our work as complementary in terms of further understanding the complex interplay between political power, social narratives, and group dynamics in diverse societies.

Finally, we contribute more generally to a large literature in public economics and political economy studying the effects of elections on political and economic outcomes (Lee et al., 2004; Pettersson-Lidbom, 2008). Differing from much of this prior work, we focus on *local* electoral outcomes in *national* elections, which as such entail neither direct effects in terms of policy outcomes nor incumbency effects. Shutting down such channels allows us to focus on social and informational effects of electoral outcomes. This complements a recent empirical

⁴Other recent work on postbellum racial violence among political economists includes Albright et al. (2021) on the 1921 Tulsa Race Massacre and Black wealth; Bazzi et al. (2022, 2023a) on Southern white migration and racial violence; Cook (2014) on lynching and patenting; Cook et al. (2018a,b) on segregation and racial violence; Henderson et al. (2021) on Confederate memorialization and lynching; Logan (2023) on local tax policy and racial violence; and Williams et al. (2021) on lynchings and regional inequality. For recent work on Black lynching by historians, see Berg (2011), Brundage (1993), Lancaster (2014), Pfeifer (2004) and Wood (2011).

⁵Looking beyond the U.S. context, our findings closely mirror Wilkinson (2006) on Hindu-Muslim riots in India, wherein close national elections induce elite incitement of local ethnic violence, for subsequent electoral gain.

literature focusing on women candidates, much of which likewise uses close election results to establish quasi-experimental conditions. Potential social effects of elections involving women have been shown to include role-model effects (Arvate et al., 2018; Baskaran and Hessami, 2018; Bochenkova et al., 2023; Delaporte and Pino, 2022; Ferreira and Gyourko, 2014), contagion effects among elites (Jankowski et al., 2019), and backlash effects (Bhalotra et al., 2017). The latter most closely resembles our setting, wherein electoral losses may in fact serve to embolden an electoral loser and their supporters. Meanwhile, our focus on elections' information effects builds on a body of work in theoretical political economy, mainly on nondemocracy, in which electoral outcomes serve as a signal of regime strength (Edmond, 2013; Egorov and Sonin, 2021; Little, 2017). In these models, electoral outcomes depend partly on the choices (e.g., manipulation) of an informed regime seeking to minimize its opposition. We focus by contrast on a (relatively) democratic setting where electoral outcomes are exogenous to the present elite's choices, which instead involve investments in violence *after* an election. At the same time, election results in our setting may nonetheless depend on the choices of the past members of the elite, insofar as investments in violence ensure a stronger elite going forward.

The remainder of the paper is organized as follows. Section 2 provides relevant historical background on lynching and the politics of the postbellum South. Section 3 establishes our RD strategy and main results. Section 4 explores empirical evidence for mechanisms and channels of influence, which we interpret using a formal theory in Section 5. Section 6 concludes.

2 Historical Background

This section provides important historical background on the postbellum U.S. South, both during and after Reconstruction. We first describe the threat to white political supremacy that followed the emancipation of enslaved Black people after the American Civil War (1861–65). We then discuss the evolution of racial violence in the decades after the war, set against the backdrop of Reconstruction's rise and fall. Lastly, we characterize the changes in racial politics that accompanied the racial violence of the post-Reconstruction period.

Emancipation and Black Political Participation. By mandating that Southern states include universal manhood suffrage in their new constitutions, the Reconstruction Act of 1867 permanently altered the Southern electorate. As a result, over 1 million newly-freed Black men, together with 300,000 poor, illiterate white men, were granted the right to vote (DuBois, 1935; Foner, 1988). With these rights, Southern Black men participated in the electoral process for the first time, holding political office in majority or near-majority percentages in some states.⁶

Postbellum Racial Violence and the Enforcement Acts. Occurring alongside these expansions in manhood suffrage were varying acts of racial violence and intimidation, including

⁶Black men constituted, for instance, about 60 percent of state delegates at the constitutional convention in South Carolina; 50 percent in Louisiana; and 40 percent in Florida (DuBois, 1935).

beatings, burnings, and lynchings. These violent acts sought, in part, to discourage Black political participation (DuBois, 1935; DeFina and Hannon, 2011). New organizations emerged, including the Ku Klux Klan (KKK), pledging violence to restore a government of white men.

By 1870, racial violence had become so pervasive in the South that President Ulysses S. Grant assembled two congressional investigations.⁷ The investigations documented vast acts of racial terror committed by members of the KKK and other groups that sought to deny equal rights to Black people. After much testimony, Congress drafted and passed the three Enforcement Acts of 1870 and 1871 (Levin Center, 2024). The first act prohibited groups from banding together in disguise “upon the public highways, or upon the premises of another” for the purposes of violating constitutional rights (U.S. Senate, 2023). The second act placed the administration of national elections in control of the federal government and extended power to federal judges and marshals to supervise voting locations (U.S. Senate, 2023). The third act gave the president military authorization to enforce against groups conspiring to deny equal protection under the law (U.S. Senate, 2023).

The three Enforcement Acts were intended to prevent racial violence against Black people and protect their rights as U.S. citizens. Insofar as local authorities had failed to address these violent acts, the Enforcement Acts meant that victims and survivors of racial terror could utilize federal courts to bring lawsuits against their perpetrators (Gardner, 2016; Frantz, 1964). By expanding the reach of federal power, the Acts also ensured more impartial adjudication of cases related to Klan-committed atrocities and weakened the group’s influence over state governments (Gardner, 2016). Yet, while the Enforcement Acts helped to restore law and order and protect the rights and lives of Black people in the South, such progress was short-lived.

The Decline of Reconstruction and the Rise of Lynching. Several Supreme Court rulings soon undermined the Enforcement Acts, chief among them the *United States v. Cruikshank* decision following the Colfax massacre. After the 1872 elections, a dispute ensued between Black and white men in Colfax, Louisiana over which political party had won. When the local sheriff instructed Black men to take over the courthouse, white men surrounded the building, setting it on fire and killing nearly 100 Black men (Frantz, 1964). Indictments under the Enforcement Act successfully charged the white men with conspiring to injure and oppress the victims because of their voting activity (Frantz, 1964). The Supreme Court reversed the convictions in 1883, however, citing that the Fourteenth Amendment, which superseded the Enforcement Acts, only permitted the federal government to intervene if states, not individuals, violated the

⁷For example, North Carolina politician and editor Joseph W. Holden testified: “There have been numerous outrages committed in that State by hands of men in disguise. In certain portions of the State, citizens of one class of political opinions have not felt safe either in their persons or property; murders have been committed, also maimings, mutilations, or scourgings. I have myself seen persons who have been whipped and I have seen the relatives of persons killed who came to the city of Raleigh to obtain protection from the governor...” Senate testimony from other witnesses included: “[T]hey always kept a man at the polls in every precinct, to report such [Black men] as voted the democratic ticket back to the League again, that they might be punished for it;” “I have heard of several cases...where [Black men] were so deterred, and ran away from the polls after coming there to vote;” “it would be dangerous for a [Black man] to vote contrary to the wishes of the league.” (U.S. Senate, 1871)

civil rights of freedmen (Frantz, 1964; Tolnay and Beck, 1995).

The *Cruikshank* ruling gutted the Enforcement Acts and marked the de facto end of Reconstruction in the South (Keith, 2009).⁸ Following this ruling, hundreds of cases in federal courts were dropped (Lane, 2008). Meanwhile, the Supreme Court continued overturning convictions and dismissing indictments under the Enforcement Acts based on the same reasoning—that state courts, rather than federal courts, should be used to enforce private matters.⁹

Yet, Southern states had shown that they would not punish violent crimes committed against Black people (Frantz, 1964). Instead, it soon became “unwritten law” across the South that lynching was a legitimize means of carrying out justice against Black people (Wells, 1900). Abstracting from any political motives, mobs often cited allegations of violent crime as grounds for lynching (Wells, 1895; Raper, 1993). One common pretext for lynching was alleged sexual misconduct of Black men involving white women, such as rape or sexual assault (Wells, 1892). Frequently without evidence or due process, such accusations galvanized racial violence while promoting new, harmful stereotypes of Black men as aggressive and overly-sexualized individuals (Woodward, 1955). Overall, lynchings became pervasive in the last two decades of the 19th century (see Figure 1), with the majority citing violent or property crimes as cause.¹⁰

As lynchings surged, many observers saw the criminal accusations proximate to the formation of lynch mobs as masking a deeper cause, one which was fundamentally political in nature. “Lynching,” argued activist and writer James Weldon Johnson (1924, 597), “was an instrument in driving the negro out of politics in the South, after the Reconstruction period.”

The (Racial) Politics of the Post-Reconstruction South. After Reconstruction’s demise, the Democratic Party sought to fully restore white dominance and reinforce racial divisions throughout the South (Chamberlain and Yanus, 2023). Yet, Democratic control of the Southern political landscape, and the racial hierarchy it upheld, faced a serious challenger in the form of pro-redistribution Southern populists (Chamberlain and Yanus, 2023). Critically, this movement was led by a biracial coalition of farmers and laborers, which had emerged out of the Farmers’ Alliance in the late 1870s (Abramowitz, 1953; Ali, 2011; Gerteis, 2007; Olzak, 1990). Its rise was further hastened by the severe depression of the 1880s, culminating in the populist People’s Party formal incorporation in 1892.

Historians and social scientists have long pointed to the success of Southern populism as a source of racial conflict and violence in the post-Reconstruction period (Hackney, 2011; Mickey, 2015). Indeed, where its opposition could lean on this biracial coalition politically, the

⁸Reconstruction would formally come to an end the following year, in 1877, with the withdrawal of all remaining federal troops from the former Confederate states, following the Compromise of 1876 (Foner, 1988).

⁹For example, in the *United States v. Harris*, a case in which a Tennessee sheriff and 19 others were indicted under the Enforcement Acts for beating four Black men, the Supreme Court dismissed the indictments on the basis that the Fourteenth Amendment limited Congress to taking corrective steps against state actions that violated the Fourteenth Amendment, not individual ones (U.S. Supreme Court, 1883).

¹⁰Among lynching records for our sample states and years, 89% have stated motives related to sex, violence, or property crime in the Project HAL data, while 59% have such motives in the Seguin and Rigby (2019) data.

Democratic Party’s dominance was credibly threatened (Gerteis, 2007; Key, 1949; Kousser, 1974; Valelly, 2009). To counter this political threat, the party’s Southern white elite sought to drum up anti-Black hatred that would divide Black and poor white voters (Glaeser, 2005; Ottinger and Posch, 2022; Woodward, 1955).¹¹ Resultant tensions meant that lynching rates tended to be higher during years in which Southern populists were on the ballot in national elections, and even more so if they were competitive (Inverarity, 1976; Olzak, 1990).

Eventually, the Southern populist challenge subsided, as the Black political threat waned and Jim Crow took hold. By 1904, all of the former Confederate states were almost wholly Democratic (see Figure 2), with support for Black voting rights being largely abandoned even amongst the Democrats’ residual opposition in the region (Valelly, 2009).

3 Empirical Evidence: Democratic Losses and Lynching

This section shows how the incidence of racial violence across the post-Reconstruction South was tied to the local performance of the Democratic Party in presidential elections. Among politically competitive Southern counties, we find that a Democratic electoral loss in a county between 1880 and 1900 nearly doubled the probability of a Black lynching occurring there over the subsequent four years, with no discernible effect on white lynchings. We establish a causal interpretation of these effects, before providing insight into mechanisms in Section 4.

3.1 Identification Strategy

We identify county-level effects of Democratic electoral losses on the probability of lynching activity targeted against Black people in the post-Reconstruction U.S. South using a regression discontinuity (RD) design, in which the key identifying assumption is that counties where a Democratic candidate *barely* lost are similar in all other ways to those where a Democrat barely won (see Lee et al., 2004; Ferreira and Gyourko, 2009; Eggers et al., 2015). Our primary estimating equation is the following:

$$\text{Any Lynching}_{c(s)\tau} = \beta \cdot \text{Democratic Loss}_{c\tau} + f(\text{Loss Margin}_{c\tau}) + \phi_{\tau} + \theta_s + \mathbf{X}'_{c\tau} \boldsymbol{\Gamma} + \varepsilon_{c\tau}, \quad (1)$$

where $\text{Any Lynching}_{c(s)\tau}$ in our analysis indicates whether any lynchings of Black (or white) people occurred in county c of state s during the four-year electoral period following presi-

¹¹Some elites explicitly supported using violence and intimidation to control the Black vote. Of Black people, Senator from Georgia Thomas E. Watson said “we have to lynch him occasionally, and flog him, now and then...” (Newton, 2016, 36); Senator from South Carolina Ben Tillman said “We of the South have never recognized the right of the negro to govern white men, and we never will. We have never believed him to be equal to the white man, and we will not submit to his gratifying his lust on our wives and daughters without lynching him” (Fordham, 2022, 109); and South Carolina Senator Martin W. Gary said “every Democrat must feel honor bound to control the vote of at least one negro, by intimidation” or other means (Epperly et al., 2020, 759).

dential election $\tau = \{1880, 1884, \dots, 1900\}$.¹² We intentionally focus on national elections, of which county-level outcomes were conceivably salient among the local masses and elite,¹³ while at the same time lacking any direct effect on actual local Democratic power. Importantly, this helps to shut down possible countervailing effects of policy on racial violence. We moreover focus on presidential rather than congressional elections, which saw relatively weak partisan competition on average during the period of study.¹⁴

Our primary regressor, *Democratic Loss* _{$c\tau$} , captures whether the Democratic candidate for president lost the popular vote in county c in a given election year τ . The period between 1880 and 1900 was crucial for the Democratic Party in regaining prominence as a national party. Among the eleven former Confederate states that make up our core sample,¹⁵ it was a period characterized by political struggle, as local Democratic elites worked with increasing success to disenfranchise Black voters and fend off Republican and Populist challengers. Meanwhile, lynching of Black people was also at its zenith in the South during this period (recall Figure 1). Figure 3 further shows the distribution of Black lynching events in our sample.

We exploit the fact that Democrats faced local political competition throughout the South between 1880 and 1900 to identify causal effects of Democratic losses on lynching over the subsequent electoral period. By interacting *Democratic Loss* _{$c\tau$} with a running variable for the Democratic loss margin, $f(\textit{Loss Margin}_{c\tau})$, we estimate treatment effects based on counties with very close election outcomes in a given election. Under the (testable) assumption that close election outcomes tend to occur in otherwise similar places, this strategy provides us with quasi-random treatment variation. We adopt a flexible, quadratic running polynomial for our main analysis, while reporting estimates of our main results based on linear and other polynomial choices as robustness. We furthermore adopt data-driven MSE-optimal bandwidth choices, which limit the set of observations to those relatively close to the Democratic loss threshold (Calonico et al., 2014). As illustration, Figure 4 shows the distribution of *highly* marginal cases, based on a 5 p.p. bandwidth.

Threats to Identification. Our empirical strategy in (1) faces two main challenges. The first concerns the standard RD assumption that relevant factors besides the outcome are continuous around the electoral zero threshold, $\textit{Loss Margin}_{c\tau} = 0$. If they are not, then estimates may reflect discontinuities in factors besides Democratic Party losses. To test this assump-

¹²For coding purposes, we make several default choices. First, a four-year electoral period is assumed to begin after November 8 of a given election year τ . Second, given limited variation in lynchings, we define our outcome variable by election period rather than have it vary by year within periods. We later relax this choice as a sensitivity exercise, which results in similar estimates. Third, we code the outcome as an indicator variable. We later show robustness to alternative measures. See Appendix A for more details.

¹³As an example, Appendix Figure A.2 shows excerpts from various articles, sourced from local and regional newspapers in our sample counties, reporting on county-level results in presidential elections.

¹⁴Across our Southern sample states and periods, the Democrats lost 103 of 893 total congressional elections, compared to 1,176 of 5,925 counties in presidential elections. Further complicating matters, many counties intersect with multiple congressional districts (Ferrara et al., 2021). In such cases, we cannot always discern whether a lynching within a given county occurred in response to results in one district versus another.

¹⁵See Appendix A for in-depth discussion and further analysis regarding our choice of sample states.

tion, we first examine the density of the running variable around the zero threshold. Insofar as electoral outcomes were at all manipulable in the post-Reconstruction South, such selection could generate differences between treatment and control counties in our sample. Using the formal test from McCrary (2008), we fail at conventional levels to reject the null hypothesis that $Loss\ Margin_{c\tau}$ is continuous at the zero threshold (see Appendix Figure B.1). Yet, such manipulation tests do not accommodate fixed effects, which may control for some state- or election-specific manipulation.¹⁶ We therefore further test for discontinuities in a wide set of relevant pre-treatment factors, in place of our outcome in equation (1). These include (i) various characteristics from the 1880 aggregate U.S. Census, such as log population density, Black population shares, and per capita manufacturing wages (Haines, 2010); (ii) 1880 characteristics based on linked Census records from the Census Linking Project (Abramitzky et al., 2020), including former slaveholder shares (from Bazzi et al., 2023a) and Confederate Army veteran shares (via Hall et al., 2019); (iii) Civil War battle locations (from Arnold, 2015); and (iv) geographic factors from the Global Agro-Ecological Zones [GAEZ] database. The latter includes cotton potential, which serves as a proxy for potential exposure to cotton-based shocks (see Tolnay and Beck (1995) and Feigenbaum et al. (2020) on cotton and lynchings). We fail to estimate statistically significant differences at the threshold across all outcomes, as shown in Table 1. Further reaffirming our identifying assumptions, our core results are also unchanged if we include all of these factors as flexible controls in our main RD analysis.

The second challenge concerns the spatial nature of our study. Numerous unobservables in space may be correlated with local election outcomes as well as lynchings. These factors are moreover likely to be correlated across time in nearby space: electoral outcomes could repeat themselves, while violent conflict may be “contagious.” We deal with these concerns in two main ways. First, we address the potential for location-based sorting bias through the inclusion of a set of spatial controls—state fixed effects (θ_s) and quadratic polynomials for county longitude and latitude ($\mathbf{X}_{c\tau}$)—in our preferred specification. Together, these account for relevant factors in space not fully captured by our unidimensional running variable.¹⁷ Results are robust to interacting these covariates with election year (ϕ_τ) as well as to additionally controlling for county-pair fixed effects based on nearest neighbors in longitude and latitude. Second, we allow for local serial correlation in unobservables by clustering our standard errors at the county level. For the purpose of defining clusters, counties are assumed to become different administrative units if their boundaries change across election periods, even if their formal identifiers remain unchanged in the data.¹⁸ We later demonstrate robustness of inference to alternative levels of clustering. Further details on our RD specification can be found in Appendix B.

¹⁶Appendix Figure B.1 shows how the density of the running variable is transformed by our baseline covariates.

¹⁷Longitude and latitude are often used as running variables in spatial RD designs (Cattaneo and Titiunik, 2022).

¹⁸Note that our RD strategy precludes the harmonization of county boundaries to a common year, as it is essential that vote margins correspond to their true values. Boundary changes likewise complicate the use of county fixed effects. Results are nonetheless robust to their inclusion, as well as unchanged if we restrict the sample to county identifiers with fixed land area over the sample period. See Appendix A for further discussion and analysis.

3.2 Political Foundations of Southern Lynchings

We now report our main findings on the political foundations of lynching activity in the post-Reconstruction South. We begin by establishing our baseline estimates for both Black and white lynchings, using the RD strategy outlined above.

Main Results. Table 2 reports estimates of β in equation (1), with our core findings displayed in panel (a). Our primary outcome of interest is a dummy for whether there were any lynchings of Black people in the four-year period following a given presidential election from 1880 through 1900. Besides the quadratic running polynomial, our baseline covariates include election period fixed effects (all columns), along with a set of spatial covariates that includes state fixed effects and quadratic polynomials for county longitude and latitude (even columns only). The estimates in columns 1–2 imply a 9.3–11.0 p.p. increase in the probability of a Black lynching over the four years following a Democratic electoral loss, equivalent to about a 66.4–78.6% increase over the (control) mean.

Meanwhile, estimates for white lynchings (columns 3–4), which were much rarer in the South and plausibly carried out for different reasons, are small and statistically indistinguishable from zero. These estimates contrast starkly to those for Black lynchings and suggest our findings to be distinct from a general violence effect. Overall, these results point to a substantial impact of Democratic Party electoral losses—even narrow ones—on lynchings of Black people in the post-Reconstruction South. This suggests that Black lynchings were in part politically motivated—although it remains to be seen whether they were intended to suppress Black political participation or at all promoted by local Democratic elites. We explore empirical evidence for the strategic use of lynching as a political mechanism in Section 4.

Note that across all specifications, estimates are based on the MSE-optimal bandwidths from Calonico et al. (2014), which limit the set of observations to those close to the electoral zero threshold, where local randomization is plausibly satisfied. Thus, while our full sample contains nearly 6,000 county-election observations, our main treatment effects are estimated from approximately a third of that, with the exact number of observations varying by outcome and other aspects of the specification.

At the same time, counties that experience competitive elections are likely to differ in relevant ways from less competitive ones. In general, RD strategies estimate a local average treatment effect (LATE) among counties with close elections. To address this, panel (b) of Table 2 reports estimates from a subsample of counties in election period τ that were *uncompetitive* in the previous presidential election, using as cutoff the sample median margin of Democratic electoral losses, $|Loss\ Margin_{c\tau}| = 15.90$. Excluding county-election observations whose vote margins fell within that bandwidth in $\tau - 1$, our baseline estimate for Black lynchings more than doubles, to 22.9 p.p., in column 2. Our estimates for white lynchings increase as well, although they remain statistically insignificant at conventional levels.

Finally, we complement our main tabular results with visual RD plots in Figure 5, which

adopt a common 10 p.p. bandwidth and fixed axes across outcomes. While illustrative, these show the same discontinuity for Black lynchings around the electoral zero threshold as in our tabular results. See Appendix Figure B.2 for alternative plots based on (i) the RD specifications and bandwidths from panel (a) of Table 2 and (ii) the restricted sample used for panel (b).

Interpreting Estimates. Our core results in panel (a) of Table 2 point to a large increase in racial violence following even *narrow* Democratic losses in a presidential election. When juxtaposed with a quasi-random interpretation of the RD framework, one puzzle is why narrow defeats were not seen simply as bad luck, relative to narrow victories, and thus treated similarly in terms of resultant violence. We now present evidence aimed at clarifying the large impact of local Democratic losses on the extensive margin, highlighting two key channels of effects. Namely, our findings suggest that the failure of Democrats to win in a given county had important *signaling* value as to the relative threat of its local opposition. Lynching, then, served as a *backlash* in the face of such revelations. We later advance a formal argument for this interpretation in Section 5.

Signaling Effects. We begin by probing further heterogeneity analysis in the spirit of panel (b) in Table 2. In Table 3, we estimate a large set of conditional RD specifications based on whether a county in election τ was (i) electorally uncompetitive in $\tau - 1$ (column 1–2), (ii) Democrat-won in presidential election $\tau - 1$ (column 3–4), or (iii) both (columns 5–6). Whereas the effect of Democratic losses on Black lynchings is large in counties that were uncompetitive or voted Democratic in the previous election, it becomes small and statistically insignificant in counties where results were close or where Democrats had previously lost. Considering these dimensions jointly, effect sizes among counties where narrow Democratic losses followed more comfortable Democratic victories further dwarf our baseline results, with estimates of .372 (.135), versus small and statistically insignificant increases in Black lynchings following narrow Democratic defeats otherwise, with much smaller estimates of .054 (.049).

Together, these results suggest that Democratic defeat was more salient in terms of its local impact in previous Democratic strongholds than in places where Democrats were already *believed* to be weak, given prior outcomes. This pattern points to a signaling explanation for our main RD effect. Under this interpretation, if a given county voted heavily Democratic in $\tau - 1$, then even a narrow Democratic loss in τ would constitute a strong, public signal as to Democrats' local weakness. Meanwhile, if the outcome in $\tau - 1$ had been close, then the outcome in τ would plausibly have been seen by locals as a toss-up, too, revealing little new information.¹⁹ It follows that, *on average*, the failure of Democrats to win in a given county, even narrowly, could have served to signal the local weakness of Democratic elites. Fearing such a signal might embolden minority opposition, members of the local white masses or Demo-

¹⁹Why are larger Democratic losses associated with less racial backlash in Figure 5? Such outcomes would likely have been less unexpected (i.e., a function of a more anti-Democratic voter base) as well as discouraging (i.e., large amounts of costly violence needed to be effective). The latter recalls Wilkinson (2006), in which violence is more likely when the preceding election was relatively close, such that shifting just a few votes matters.

cratic elite may have commenced a backlash against Black members of the opposition’s local constituency. Next, we provide evidence consistent with lynching as local backlash, before shedding light in Section 4 on the strategic use of lynching as a political instrument.

Backlash Effects. Relatively unified opposition among Southern Black people to the Democrats made them likely targets of local white ire. Appendix Figure A.2 illustrates how Black populations were frequently singled out in local newspapers as a source of blame for Democratic defeats. Two additional heterogeneity analyses further point to lynching as a form of *backlash* mounted against Black individuals in reaction to (unexpected) losses. First, whereas about 99% of sample observations had Black populations as of 1880,²⁰ effects are wholly driven by those with sizable, above-median Black population shares—where the Black voter would have had clear-cut implications for Democratic political fortunes (see Appendix Table B.2).

Second, such backlash would likely have been more substantial in the *immediate* aftermath of an electoral loss. We thus re-estimate treatment effects within electoral cycles based on temporal proximity to elections. Appendix Figure A.3 shows that effects closely follow electoral losses, with the largest estimates in the first two years afterward. This is consistent with racial violence occurring in direct reaction to such losses, spiking when passions were hottest.

Robustness Checks. To bolster a causal interpretation for our core results in Table 2, we conduct a large set of additional robustness checks. We report many of these in Table 4, with some more detailed sensitivity analyses featured in the Appendix.

Alternative Standard Errors. We show robustness of inference to more extreme serial and spatial autocorrelation in panel (a) of Table 4. In our baseline specification, we cluster standard errors at the county level, with counties assumed to become different administrative units if their boundaries changed across election periods. Alternative spatial or temporal choices for clusters result in similar standard errors. Row 1 in Table 4 shows five such alternatives, including clustering by (i) county Federal Information Processing Standard (FIPS) identifier, which tend to be fixed over time in spite of boundary changes; (ii) county-pair, which we match based on nearest neighbors in longitude and latitude within the optimal bandwidth; (iii) county-pair-by-decade; and (iv) state-by-election-period.

Varying Controls. Panel (b) of Table 4 considers alternate sets of covariates in equation (1). Estimates for Black lynchings remain large and significant at conventional levels in more conservative specifications that (i) omit all covariates other than the running variable (row 2) or (ii) omit the longitude and latitude polynomials from our preferred specification (row 3). Results are likewise robust to interacting (iii) state fixed effects and (iv) all spatial covariates with election period dummies (rows 4 and 5, respectively), which further account for the staggered introduction of place-specific policies designed to boost Democratic performance in the Jim Crow South. Row 6 incorporates, in addition to our baseline spatial controls, county-pair fixed

²⁰The average Southern county contained 33.3% (st. dev. = 23.9) Black population shares in 1880.

effects based on nearest neighbors in longitude and latitude, which we generate conditional upon counties being within the optimal bandwidth from row 1. Including over 450 county-pair FE in our Black lynching specification leaves estimates practically unchanged. As an alternative to state fixed effects, row 7 includes county fixed effects, based on the fixed-boundary identifiers at which our standard errors are clustered. Finally, row 8 further verifies the assumptions underpinning the RD by controlling for quadratic polynomials of all variables from Table 1, such as Black population shares, which results in little change in core estimates.

Alternative RD Specifications. We test sensitivity of our results to alternative bandwidths and running polynomials in panel (c) of Table 4. Rows 9 and 10 re-estimate the specification in row 1 but with the optimal bandwidths multiplied by factors of 0.5 and 1.5, respectively. Meanwhile, rows 11–13 of Table 4 vary our running polynomial, with estimates based on linear as well as hyper-flexible cubic and quartic running polynomials. Results remain substantively intact in all cases and significant at conventional levels for Black lynchings.

Sample Sensitivity. Our core analysis focuses on the eleven states of the former Confederacy. Importantly, all of those states were strongly Democratic in their elite composition and had the distinction of supporting the Democratic candidate in every presidential election between 1880 and 1916, illustrating the pervasive Democratic political identity that comprised the so-called “Solid South.” We moreover focus on presidential elections between 1880 and 1900, after which Democrats faced little local political competition within these states.

We explore sensitivity to these choices in Appendix Figure A.1. First, we show that our results are not particularly sensitive to omitting any of the sample states. Holding other aspects of the specification fixed, we drop in panel (a) each of the eleven former Confederate states one-by-one from the sample. No particular state appears to be driving our main effect. This includes the two states for which our lynching data are derived from Seguin and Rigby (2019), Texas and Virginia, the omission of which slightly decreases and increases our estimate, respectively. We further discuss questions of sample state selection in Appendix A.

Second, our results are robust to omitting any of the six sample election periods, as shown in panel (b) of Appendix Figure A.1. Of note is a slight increase in point estimates with the omission of the 1896 election, which saw a formal schism within the Democratic Party between the agrarian wing, led by nominee William Jennings Bryan, and the classically-liberal Bourbon Democrats, led by fiscal conservative and former-abolitionist John M. Palmer.

Alternative Outcome Measurement. Given the highly right-skewed nature of our lynching data, with few county-years recorded as having experienced multiple lynching events, our default outcome measure is an indicator variable for whether any lynching event occurred during a given four-year election period. We nevertheless consider several alternative outcome variables in Appendix Table A.1. These include measures based on (logged) counts and rates (per 10,000 individuals).²¹ We also consider a version of our outcome based on a more granular temporal

²¹Being highly right-skewed with numerous zero-valued observations, we specifically adopt an inverse hyperbolic

unit of analysis, of year period rather than election period. All alternatives produce estimates of similar relative magnitude to our baseline result, consistent with a 49.4–77.5% increase in the probability of a Black lynching in a given county following a Democratic electoral loss.

4 Solidifying the South: Lynching as a Political Mechanism

The late 19th century saw a wave of violence against Black people spread throughout the South in the form of lynchings. Our results so far show that these episodes tended to occur where Democrats had recently suffered electoral defeat in presidential contests. Insofar as Black voters generally supported the Democrats’ political opposition, such violence was plausibly intended to intimidate Black people and thereby suppress the Black vote.

In this section, we provide broad-based evidence for the strategic use of lynching as a local political instrument in the post-Reconstruction South. We begin by showing that lynching successfully facilitated Democratic entrenchment in once-competitive counties over the longer run. Conditional on a Black lynching having occurred in a county, a narrow Democratic defeat in the late 19th century predicts Democratic victory in presidential elections after 1900, together with decreases in Black political and economic engagement. In addition, we find that increases in Black lynching systematically followed Democratic electoral losses in the South *specifically* where de jure forms of voter suppression had yet to be introduced, consistent with lynching serving as an early substitute for Jim Crow. Lastly, we explore the channels through which local elites plausibly promoted racial violence. We show that Southern newspapers, which often had ties to the Democratic Party, tended to spotlight stories of Black-committed atrocities following Democratic losses. Such accusations comprised the vast majority of stated motives in lynching records. Altogether, these findings point to lynching as being a tool used by local Democratic elites to suppress Black political power and consolidate the “Solid South.”

4.1 Black Lynchings as Effective Voter Suppression

The previous section detailed the violent consequences of poor Democratic performance across the post-Reconstruction South. How effective was resultant lynching in suppressing the Black vote and boosting local Democratic performance? In the first of several complementary exercises, we show that counties where Democrats narrowly lost were subsequently more likely to be *won* by Democrats in the early 20th century. Crucially, this effect depends on a Black lynching having occurred in a county. Racial violence, we argue, proved key in bringing about a reversal of electoral fortunes for the Democratic Party in once-competitive areas of the South.

Table 5 shows estimates from this exercise. We begin in panel (a) by restricting to county-election observations that experienced any Black lynchings (i) through the four-year period fol-

sine function for our log transformations of these variables.

lowing election τ (odd columns) or (ii) strictly during the period following τ (even columns).²² Then, in columns 1–6, we examine whether a Democratic loss in a given county-election observation between 1880 and 1900 predicts a Democratic victory in the same county identifier for presidential elections in (i) 1904 (column 1–2), (ii) 1908 (column 3–4), and (iii) 1912 (column 5–6). The estimates reveal that, among Black lynching counties, narrow Democratic defeats between 1880 and 1900 predict sharply *increased* Democratic success in 1904 and 1908—though not in 1912, as Southern counties had ceased to be competitive at that point. Figure 6 illustrates these discontinuities and the electoral premium associated with Black lynchings for Democratic presidential candidates among these formerly-competitive counties.

Corresponding to this electoral reversal of fortune, columns 7–8 of panel (a) show that voter turnout rates in the early 20th century were about 14.5–18.3% lower, relative to the control mean, among counties that had experienced a Democratic electoral loss between 1880 and 1900. This pattern mirrors the broader decline in Black voting that occurred throughout the South during the early 1900s, much to the benefit of the Democratic Party (Jones et al., 2012).

Meanwhile, the same effects cannot be estimated among counties with no history of Black lynching as of period τ , as shown in panel (b). This heterogeneity points to racial violence as being an important, if not necessary, condition for both the Democratic Party’s reversal of electoral fortunes and the decline in voter participation that accompanied it, consistent with prior insights of contemporary observers (Wells, 1895) and modern political economists (Jones et al., 2017). In Section 4.2, we explore the role of local elites in fomenting such violence.

Finally, independent of the close electoral losses that may have caused them, we find Black lynchings themselves to be strongly correlated with these political outcomes. Conditional on our baseline controls, Appendix Table B.3 shows that counties that experienced at least one Black lynching over the 1880–1900 electoral period were 11–22% more likely see a Democratic win in 1904 or 1908, and 4% more likely in 1912, relative to the counterfactual. As above, these outcomes correspond to reduced voter turnout in lynching counties, by about 17%.

Interpreting Heterogeneous Effects. In general, counties that experience racial violence are also likely to have larger Black populations. While this is immaterial to our main RD analysis (i.e., in Table 2), it has the potential to affect the interpretation of our results in this section. We now briefly touch on two alternative explanations for the patterns in Table 5.

Lynching, or Just Larger Black Populations? To account for any mechanical influence of Black population size on the heterogeneous effects in Table 5, we match counties across panels (a) and (b) based on similarity in Black population shares as of 1880. Concretely, this involves defining, to the extent possible, pairs of counties with and without exposure to lynching among the combined estimating samples of each column. We then restrict the samples to these matched counties, holding other aspects of the specification fixed. In practice, this procedure drops

²²In principle, the latter is preferred for the purpose of evaluating heterogeneous effects. At the same time, having a county permanently “turn on” after its first recorded Black lynching allows for a larger split sample size.

observations with small Black population shares from the panel (b) regressions.²³ The results of this exercise, shown in Appendix Table B.4, differ little overall from the estimates in Table 5. Democratic losses over the 1880–1900 period continue to predict an increased probability of Democratic victory in the 1904 and 1908 presidential elections, together with reduced voter turnout, conditional on a Black lynching having occurred in a given county as of τ . Meanwhile, no such pattern is systematically present among non-lynching counties.

Black Outmigration. How much do the effects in Table 5 reflect voter suppression versus a relative decrease in the number of Black people? Indeed, if racial violence induced Black outmigration from a given county, there would be fewer Black people present to oppose Democrats. To evaluate the role of Black outmigration in driving our effects, we use the same heterogeneity analysis from Table 5 to look for discontinuities in the (i) percentage point change in Black population shares between 1880 and 1910 and (ii) number of new Black residents in a county between 1880 and 1910, as a share of county population in 1910. Appendix Table B.5 provides suggestive evidence that places experiencing Democratic losses between 1880 and 1900 did in fact see (relative) decreases in Black population sizes, conditional on a Black lynching having occurred. Overall, this suggests that some of Democrats’ electoral reversal of fortune in once-competitive counties stemmed from Black outmigration induced by racial violence. At the same time, this is unlikely to explain that entire effect: insofar as decreases in voter turnout were driven by declines in Black voter participation, Black outmigration would likely favor *increased* county-level voter turnout on average, at odds with our findings in Table 5.

Additional Evidence. Three additional exercises provide further evidence for racial violence as a tool for the disenfranchisement of Black people in the post-Reconstruction South.

Educational Effects. Just as lynching facilitated a Black retreat from civil society in once-competitive Southern counties, it also stifled Black *educational* engagement. Insofar as literacy tests played an important role in denying Black people their right to vote in the early 20th century, this would have served as a critical feedback mechanism, in amplification of lynching’s political effects. Pursuing a similar heterogeneity analysis to Table 5, we find that Democratic losses in presidential elections between 1880 and 1900 predict lower literacy rates among Blacks—but not whites—in 1910, by about 5.7–9.1%, relative to the control mean (columns 1–4 of panel a in Table 6). This literacy deficit corresponds to an analogous drop in school enrollment among Black children (columns 5–6), of about 7.8–23.0%. Once again, these effects are contingent on a Black lynching having occurred in a county as of election τ , further suggesting racial violence to be a core channel through which Black empowerment—both political and economic—was diminished in once-competitive counties.

Longer-run Political Effects. How persistent was the depression of Black political partici-

²³For example, this increases the mean Black population share among those counties with no histories of Black lynchings as of τ , as featured in column 1 of panel (b) in Table 5, from 33.14 (.53) to 40.38 (1.20), relative to a mean of 40.23 (.67) among counties within the same bandwidth that did experience a Black lynching.

patation among those once-competitive counties that underwent episodes of racial violence? We now move forward in time to explore whether such areas still had lower levels of Black political participation in the time of the Voting Rights Act (VRA), which dismantled racial discrimination in national elections throughout the country. To do this, we use voter registration data for 1962–64 (pre-VRA) and 1966–67 (post-VRA) from the [United States Commission of Civil Rights \(1968\)](#), which contains such data for non-white people in all sample states except for Tennessee and Texas.²⁴ We then examine whether narrow Democratic losses between 1880 and 1900 predict lower voter registration in counties among (i) non-whites and (ii) whites.²⁵ Table 7 shows a pattern similar to the previous two tables, with demonstrable effects only for non-white voters (columns 1–4 of panel a) and only conditional upon a Black lynching having occurred in a county as of election period τ . Concretely, this history is associated with about 15.5–40.8% lower Black voter registration in the 1960s—the baseline of which was already less than half the rate that of whites at that point in time. The magnitude of effects changes little with the passage of the VRA, which caused increases in voter registration overall ([Ang, 2019](#)). This result recalls [Williams \(2022\)](#), wherein lynchings served as a negative shock to Black political participation that persisted thereafter through local culture and institutions.

Interactions with De Jure Voter Suppression. Additional evidence that Black lynchings served as voter suppression comes from examining how our core RD results in Table 2 vary with the presence of other, de jure forms of voter suppression. Historically, lynching proliferated across the U.S. South particularly after several Supreme Court decisions and the withdrawal of federal troops removed key protections for Black people ([Woodward, 1955](#)). Thereafter, it was not until the early 1900s that such anti-Black antagonism would begin to subside, coinciding with the rise of Jim Crow and the decline of Democratic political competition in the South ([Epperly et al., 2020](#); [Glaeser, 2005](#)). Such timing suggests that lynching may have served as an early substitute for Jim Crow laws in the process of suppressing Black votes.

Table 8 provides quantitative evidence in support of this interpretation. We estimate effects separately for states that had yet to enact any Jim Crow voting laws as of election period τ and those that had, based on the years of states’ first-implemented (i) ballot requirements (e.g., literacy tests, multi-box laws) and (ii) poll taxes from [Jones et al. \(2012\)](#). All sample states had done so by 1903, with about half of close-election (< 50 p.p.) observations falling within each subsample (see Appendix Table A.2). Columns 1–2 of Table 8 consider whether a state had yet enacted any of those laws, while columns 3–6 break them out by subtype. In all cases, estimates are large and statistically different from zero only prior to the advent of such laws.²⁶

²⁴In addition, a large number of counties in Mississippi and North Carolina are absent from the report. For post-VRA, Arkansas and Virginia are wholly excluded as well.

²⁵For unknown reasons, scaling non-white (white) voter registration counts by the number of eligible non-white (white) voters in 1960 results in a few rates exceeding 100. For this reason, we opt to instead scale by total non-white (white) population. Results are generally similar when using the voter-scaled measure, with column 1 of Table 7 becoming -8.220 (4.664) if we do not truncate rates at 100 versus -5.063 (4.143) if we do.

²⁶See Appendix Figure B.3 for a visual comparison of effect sizes under Jim Crow relative to the pre-Jim Crow benchmark in column 1, holding fixed all other aspects of the latter’s specification.

Put differently, lynchings of Black people systematically followed Democratic electoral losses before the introduction of Jim Crow laws but not after.²⁷ Indeed, a loss would arguably have no longer signaled Democrats' political vulnerability to Black people after the advent of Jim Crow, as Black voting was effectively deterred (Jones et al., 2012). Moreover, Jim Crow would likely have superseded lynching as a more-efficient punishment strategy for any (illicit) Black political participation that *did* occur, reduced demand for more decentralized, violent backlash (Epperly et al., 2020). Overall, this is consistent with Black lynching serving as an early substitute for later de jure forms of Southern voter suppression.

Reconciling Results with RD Assumptions. Before proceeding, we briefly comment on how the results in this section thus far relate to the RD assumptions laid out above. Importantly, these results suggest that Black lynchings served as effective means for voter suppression in the post-Reconstruction South. Insofar as this undermined electoral competition in the short run, the identifying assumptions underpinning our RD strategy could potentially be threatened. We highlight two reasons for why this is not a salient concern. First, we found no evidence of endogenous sorting or systematic manipulation of electoral outcomes over the 1880–1900 period in Section 3.1 (as further probed in Appendix B). The evidence in this section, meanwhile, is based on electoral outcomes after 1900. Second, if lynchings and related unobservables did in fact systematically undermine electoral competition in favor of Democrats during the sample period, we would expect that to only attenuate our estimates. This is based on the fact that lynchings are triggered by Democratic losses, whereas any systematic voter suppression resulting from lynchings would on the margin be expected to result in Democratic *victory*. If this were in fact the case, one might choose to interpret our core estimate as a lower bound.

4.2 Elite-Induced Violence? Partisan Media and Anti-Black Narratives

Our results so far are consistent with Black lynching serving as a tool for voter suppression, to the benefit of local Democratic elites in the post-Reconstruction South. Yet, they do not necessarily imply such efforts to be elite-led, nor do they describe *how* local elites may have helped to induce racial violence. While many lynchings actively involved members of the elite, such pervasive racial violence would have required the decentralized efforts of the broader pro-Democratic masses (Raper, 1993; Epperly et al., 2020). Unlike later de jure sources of voter suppression, this would have been costly for local elites to direct and coordinate.

Collective action needed for carrying out racial violence would arguably have been most successful when passions were hottest. This is evidenced, for instance, by the concentration of our lynching effects in the close aftermath of Democratic electoral losses (see Appendix Figure A.3). Along similar lines, newspaper stories about atrocities accused of Black people, often against white women, became increasingly pervasive after Reconstruction and coincided with the rise of Black lynching in the South (Woodward, 1955; Glaeser, 2005). Such accusations

²⁷Of course, lynching continued to occur for reasons unrelated to our treatment (Wells, 1895; Jones et al., 2017).

importantly provided motive for racist individuals to lynch Blacks, even as the desired ends of anti-Black hatred and violence among many elite had more to do with the stifling of Black empowerment, as noted as far back as Wells (1892, 1895).

In this section, we explore variation in the content of local newspapers after Democratic electoral losses. Insofar as newspapers throughout the South were strongly affiliated with the Democratic Party at the time (Gentzkow et al., 2015), they may also have been served as important political instruments in the aftermath of Democratic electoral losses. In particular, by operationalizing racial hatred through the dissemination of anti-Black atrocity stories, newspapers may have aided local elites in galvanizing the post-electoral lynching activity that we have documented in this paper. Such a pattern would be consistent with the strategic use of media by local elites for inducing Black voter intimidation and suppression, complementing other recent work focused on white political counter-mobilization in the non-Jim Crow South (Ottinger and Posch, 2022; Bernini et al., 2023).

Estimation. To estimate the effect of Democratic electoral losses between 1880 and 1900 on the prevalence of anti-Black atrocity narratives, we exploit within-city variation in local newspaper content over time, using a modified version of equation (1). Concretely, we examine whether close Democratic losses in a given city’s county predict increases in Black crime accusations in its newspapers’ content, using city fixed effects to capture baseline levels of anti-Black sentiment in a locale. To test this, we build a comprehensive, time-varying sample of newspapers from [newspapers.com](https://www.newspapers.com),²⁸ which we link with information on cities’ historical county (as of 1900) from the Census Place Project (Berkes et al., 2023). The latter ensures that newspapers are responding to electoral outcomes within their city’s contemporaneous county.

For our outcome, we define rates of Black crime accusations for all 24 years of our 1880–1900 election period sample. As newspaper units often enter or exit our sample on an annual basis—for instance, due to splits and mergers across newspapers—we adopt calendar years as the temporal unit for this analysis.²⁹ As in the within-election version of our main analysis (featured in Appendix Table A.1), we include dummies for news year minus election year in our estimating regression, in addition to our usual election period fixed effects, to account for cyclic shocks to newspaper content within electoral periods.

To construct our outcome variable, we count the total number of pages per newspaper-year across our sample states and period that plausibly feature a Black crime accusation. Following Glaeser (2005), we search for mentions of “negro rape” and “negro murder” as well as “negro robbery.” In practice, this also identifies similar phrases, such as “negro intended robbery” (see Appendix Figure A.4 for other examples). Our script additionally allows for the plural of the

²⁸See Beach and Hanlon (2022) and Ferrara et al. (2022) on use of [newspapers.com](https://www.newspapers.com) to build historical data.

²⁹As an example, the *Memphis Daily Appeal* runs in our sample from 1881–89. Meanwhile, its competition, the *Memphis Avalanche*, ran from 1885–90. The two merge in our sample in 1890 to become the *Memphis Appeal-Avalanche*. Separately, another paper, the *Memphis Commercial*, runs in our sample until 1894 and would later merge in as well to become the modern-day *Commercial Appeal*. For more, see Gentzkow et al. (2011, 2015).

word “negro” and a past tense of the crime mentioned (“raped,” “murdered,” “robbed”). Our baseline measure sums all of these and then divides by the total number of pages per newspaper-year that feature the generic word category “th*” to produce a rate (out of 100). Results are robust to excluding the plural or past tense modifications (as in Glaeser, 2005), as well as to allowing single words to co-occur anywhere on the same page (as in Ottinger and Posch, 2022). We also perform placebo exercises excluding the term “negro” entirely.

This measure is inspired by prior historical work on anti-Black atrocity narratives and lynchings (Wells, 1892; Woodward, 1955). After Reconstruction, new forms of racial antagonism spread throughout the South, including stereotypes of Black violence and aggression. Caricatures of the Black “brute” or “buck” were distinct from Black inferiority narratives used to rationalize slavery before and immediately after the Civil War and, indeed, served a different purpose. As Black lynchings began to surge in the 1880s, they were typically legitimized by such accusations of violent rape, murder, and property crime. Corroborating this, Figure 7 shows how the frequency of accusations in a county increased over the period leading up to a Black lynching in our data. Though these narratives declined in prominence in the 20th century, accompanying stereotypes have persisted in the national consciousness through the help of popular film (e.g., *The Birth of a Nation*) and literature (e.g., *Native Son*).

As much as possible, we furthermore augment our newspaper data with information on newspapers’ partisan affiliations during the sample period from Gentzkow et al. (2014a,b). Occasionally and as needed, we assign affiliation information to a daily (weekly) newspaper based on the contemporaneous affiliation of its weekly (daily) counterpart. We moreover assume that any newspaper with “Democrat” in its title is affiliated as such. Overall, known newspaper affiliations in our sample are Democratic nearly 90% of the time, while about a third of newspapers in our sample have no known affiliation.

Results. Columns 1–2 of Table 9 reveal that a Democratic electoral loss in a city’s county between 1880 and 1900 is associated with a 43.1–97.4% increase in the frequency of Black crime accusations in that city’s newspapers, relative to the control (victory) mean. This is similar to the effect size for Black lynchings in Table 2 and suggests the use of newspapers to propagate racial hatred after local Democratic electoral losses.

To test whether this effect is related to the strong Democratic affiliations of most Southern newspapers at the time, we split our sample by the political leanings of newspapers in columns 3–6 and re-estimate effects. If increases in racial antagonism after Democratic losses are elite-led, we would expect estimates to be positive only among newspapers with Democratic affiliations. What we find is even more striking. While estimates indeed remain positive among Democratic newspapers in columns 3–4, they become *negative* among the smaller sample of non-Democratic newspapers in columns 5–6. This is consistent with narrow Democratic *victories* spurring anti-Black antagonism among local *non-Democratic* elites. Whether this was intended as punishment against Black people for “insufficient” support for the Democrats’ op-

position versus a sign of the Republicans’ and Populists’ gradual political embrace of white supremacy by the late 20th century remains an open question. Regardless, it suggests that Democrats did not have a monopoly on the strategic use of anti-Black hatred—they merely had more channels through which to disseminate it, to their political advantage.

As robustness, we explore several alternative measures of our outcome in Appendix Table A.3. The first (column 2) excludes plural and past tense modifiers of our search terms, searching only for “negro rape,” “negro murder,” and “negro robbery.” The second (column 3) allows the words in these phrases to co-occur anywhere on the same page. The third (column 4) does both of these. In all cases, estimates remain large and statistically significant, with slightly smaller effect sizes in the “weak measure” cases of columns 3–4. Finally, we re-estimate effects in column 5 using a placebo measure that omits “negro(es)” from the search terms in our script and searches more generally for “rape,” “murder,” and “robbery.” Effects disappear, consistent with increased crime accusations after Democratic losses only being against Black people.

We furthermore break out Black crime accusations by type of crime in Appendix Table A.4. Effects are somewhat larger for accusations of robbery, although all estimates are large and significant at conventional levels. Finally, results are robust to (i) including the 16 variables from Table 1 as flexible controls in the analysis, (ii) controlling for newspaper fixed effects, (iii) using a linear RD running polynomial, and (iv) adjusting for mass points in the county-level explanatory variation, as shown together in Appendix Table B.6.

Overall, these findings are consistent with local newspapers being a core channel through which local elites sought to operationalize racial hatred for political gain. This finding complements [Ottinger and Posch \(2022\)](#) on the use of newspapers for fanning racial grievance and catalyzing white political mobilization in the face of Black political empowerment in the pre-Jim Crow South. Our analysis, in contrast, focuses on the use of anti-Black atrocity narratives for suppressing Black political participation, by galvanizing lynching activity that concurrently occurred after Democratic electoral losses in the post-Reconstruction South. Together, we provide a more complete picture of how Southern elites strategically used racial hatred and violence to maintain white supremacy, long after the Civil War dismantled formal Black slavery.

5 Conceptual Model: A Political Theory of Racial Violence

In this section, we develop a formal model to help interpret our empirical findings as well as to conceptualize the strategic use of violence by local elites against minority (e.g., racial, ethnic) individuals more generally.³⁰ Present a relatively large, unified, and politically-enfranchised minority group, we propose that the prevailing local elite may invest in violence against minority individuals if electoral outcomes reveal that it is relatively weak. We model such an environment herein, deriving formal conditions for the use of violence by local elites as well as

³⁰We mean “minority” only in the national sense; a minority might in fact make up a *local* population majority.

implications of anti-minority violence for the political strength of local elites over the longer run. These results further clarify the causes and consequences of lynching laid out in this paper.

Environment. The model consists of two sets of players, each of whom are of fixed size and live for two periods: (i) a local elite, which makes decisions as a unified bloc, and (ii) a unit mass of minority citizens, indexed by $i \in [0, 1]$. We abstract from other players, namely “majority” citizens who in our context are likely to be aligned with the elite and whose inclusion would not affect the model’s overall results. There is no time discounting.

The local elite privately has a *strength* of $\sigma_t \in \{L, H\}$, where $\sigma_t = H$ denotes a strong elite (e.g., powerful, politically entrenched), while $\sigma_t = L$ denotes a weak elite (e.g., vulnerable to political opposition). The latter type is particularly relevant to our empirical analysis.³¹ The local elite is strong in period t with probability $\pi(S_t, \underline{S}) \in (0, 1)$, where $(S_t, \underline{S}) \in (0, 1)$ represents the *state of elite support* at the very beginning of period t . This depends on both $S_t \in [0, 1]$, which captures the *level of minority acquiescence* to the elite, and \underline{S} , which captures some (fixed) *baseline level of support* (i.e., the support of the “majority”). We assume that $\pi' > 0$ as well as that $\pi'' \leq 0$ and $\pi''_{S_t \underline{S}} \geq 0$ such that strength acquisition is favored by having support from multiple sources.

Local elite strength σ_t varies over time with the level of minority acquiescence. The elite enter period t conferred with some level of S_t , which, through $\pi(S_t, \underline{S})$, maps to its initial strength. In contrast to σ_t , the state of elite support (S_t, \underline{S}) is commonly known, made public together with a discrete *election outcome*, $\omega_t = \{0, 1\}$, at the very beginning of period t prior to any decision-making. S_{t+1} , meanwhile, is endogenous to decisions made during period t . In particular, minority citizens have two possible strategies in period t : oppose ($s_{it} = 0$) or acquiesce to the elite ($s_{it} = 1$), where $S_{t+1} = \int_0^1 s_{it} di$. Minority opposition here ought not be political; it could, for example, involve economic competition or violations of elite-preferred norms. Regardless, if enough minority citizens are emboldened to oppose the elite in period t , a strong local elite may become weak by period $t + 1$. On the other hand, if minority citizens are sufficiently *deterred* from opposing the elite, a weak elite in t may become strong by $t + 1$.

Differentiating Local Elite Types. A weak local elite differs from a strong one on two salient dimensions. First, its *electoral outcomes*: whereas a strong local elite is always associated with electoral victory ($\omega_t = 1$), a weak local elite encounters an electoral loss ($\omega_t = 0$) with $\Pr(\omega_t = 0 | \sigma_t = L) = h \in (0, 1)$.³² One possible interpretation of h is as the degree of elite weakness.³³ While electoral outcomes have no direct bearing on local elite power (i.e., they are local tallies for national elections), they nonetheless provide a useful *signal* of local elite

³¹In historical context, strong-type elites might be Southern Democrats under a Jim Crow regime, with weak types being those lacking, as of yet, the same means for achieving de facto one-party rule.

³²Note that while local elite type is a function of citizens’ support, electoral outcomes are random conditional upon elite type. This theoretical set-up mirrors our quasi-experimental empirical design in Section 3.

³³The model would be substantively the same if the local elite type space was continuous, from weak to strong, with relatively strong types having $h \rightarrow 1$. Concretely, the probability of the elite being a relatively weak type, conditional on a loss being observed, would still be larger than the probability of it being a relatively strong type.

strength. Indeed, minority citizens observe ω_t , together with (S_t, \underline{S}) , at the start of period t .

Second, a weak local elite differs from its strong counterpart in terms of its *violence technology*. In the model, the local elite can influence minority citizens' choices of s_{it} through the strategic use of violence. Concretely, if the elite observes a minority opposing it, it may execute some violence against them. However, whereas a strong elite has a monopoly on violence, allowing it to operationalize local state instruments, a weak elite must rely on more decentralized forms (e.g., lynching). These are distinguished by cost structure, with:

$$c(\sigma_t) = \begin{cases} c \cdot \theta_t \{\text{violence} > 0\}, & \text{if } \sigma_t = H \\ \phi(p_t), & \text{if } \sigma_t = L, \end{cases} \quad (2)$$

where $c > 0$ denotes a small and singular fixed cost of violence for strong types, while $\phi(p_t)$ denotes the cost of *elite investment in violence* for weak types $p_t \in [0, 1]$, with $\phi'(0) = 0$, $\phi' > 0$ for $p_t > 0$, and $\phi'' > 0$. The latter entails positive and increasing marginal costs associated, for example, with the dissemination of propaganda across multiple media sources for the purposes of fomenting violence against minorities among party rank and file. In this example, p_t would be not the intensity of violence but, rather, the probability that the rank and file receive pro-violence messaging from the elite or, in turn, the probability that a member of the public commits an act of violence against oppositional members of the minority.

Conditional on a weak local elite, the expected share of consumption surviving to period $t + 1$ for minority citizens engaged in opposition against the elite is assumed to be $1 - p_t$. Meanwhile, under a strong local elite, minority consumption under opposition survives deterministically with $1 - \theta_t$, where $\theta_t \in \{0, 1\}$. Overall, to avoid violence and ensure survival, minorities may choose to acquiesce to the elite, such as by withdrawing from civic life altogether (Jones et al., 2017; Williams, 2022).

Payoffs and Timing. This environment above implies the following payoff for the local elite:

$$U_{\sigma t}(p_t, S_{t+1} | \omega_t) = -c(\sigma_t) + \pi(S_{t+1}, \underline{S})V_{\sigma}^H + (1 - \pi(S_{t+1}, \underline{S}))V_{\sigma}^L,$$

where V_{σ}^H and V_{σ}^L denote exogenous type-transition payoffs associated with period $t + 1$ strength and weakness, respectively. For brevity, we let $\Delta V \equiv V_{\sigma}^H - V_{\sigma}^L > 0$.

Meanwhile, a minority citizen's expected payoff for period 1 is either

$$u_{it}(s_{it} = 0, S_{t+1} | \omega_t) = (1 - \mathbb{E}(\text{violence}_t | \omega_t))y_{it}(s_{it} = 0, S_{t+1})$$

under opposition, where

$$\mathbb{E}(\text{violence}_t | \omega_t) = \Pr(\sigma_t = H | \omega_t)\theta_t + \Pr(\sigma_t = L | \omega_t)p_t,$$

based on conditional probabilities following Bayes' theorem:

$$\Pr(\sigma_t = H|\omega_t = 1) = \frac{\pi(S_t, \underline{S})}{h + (1-h)\pi(S_t, \underline{S})} \text{ and } \Pr(\sigma_t = H|\omega_t = 0) = 0,$$

and where $y_{it}(s_{it} = 0, S_{t+1})$ denotes minority economic payoffs under opposition, with $y'_i(S_{t+1}) < 0$; or otherwise a minority citizen's payoff for period 1 is

$$w_{it}(s_{it} = 1, S_{t+1}|\omega_t) = (1 - \delta_i)y_{it}(s_{it} = 1, S_{t+1})$$

under acquiescence, where $\delta_i \in [0, 1]$ serves as an idiosyncratic psychological “tax” associated with acquiescing to the elite. Let $\delta_i \sim_{i.i.d.} F(\cdot)$, where F could be any well-behaved continuous cumulative distribution function but is assumed to be uniform $U(0, 1)$ for analytic ease. Finally, note that $y_{it}(s_{it} = 0, S_{t+1}) = y_{it}(s_{it} = 1, S_{t+1})$ given non-atomic citizens. In other words, no one citizen can expect to impact aggregate local economic conditions.

The timing of the game for a given generation of local elite and minority citizens is:

1. *Period t.* The local elite observes its strength, $\sigma_t \in \{L, H\}$, while citizens observe only the state of elite support, (S_t, \underline{S}) , and a discrete election outcome, $\omega_t \in \{0, 1\}$.
2. The local elite chooses its level of investment in violence against oppositional minority citizens. If $\sigma_t = H$, it chooses $\theta_t \in \{0, 1\}$. If $\sigma_t = L$, it chooses $p_t \in [0, 1]$.
3. Minority citizens choose whether to acquiesce to the elite, $s_{it} \in \{0, 1\}$. Any violence against oppositional minority citizens occurs.
4. *Period t + 1.* Given the new state of elite support, (S_{t+1}, \underline{S}) , the local elite observes its new strength, $\sigma_{t+1} \in \{L, H\}$, and payoffs are accrued.³⁴

See Figure 8 for a visual summary of the sequence of information and actions occurring within each period t .

Model Predictions. Solving backward from stage 3, clearly if a minority citizen observes an electoral defeat, $\omega_t = 0$, they will acquiesce to the elite if and only if $p_t > \delta_i$, since they will *know* that the local elite is weak. This in turn implies $S_{t+1}^*(p_t|\omega_t = 0) = p_t$. If a minority citizen observes an electoral victory, however, then their beliefs about the local elite's type will be updated using Bayes' theorem, such that

$$S_{t+1}^*(p_t|\omega_t = 1) = \frac{\pi(S_t, \underline{S})\theta_t + h(1 - \pi(S_t, \underline{S}))p_t}{h + (1-h)\pi(S_t, \underline{S})}.$$

In both cases, whether or not a minority citizen acquiesces depends on the local elite's level of investment in violence. Crucially, electoral victories allow for weak-type local elites to pool with the strong types, generating potential differences in the level of acquiescence to the

³⁴At this point, a transition to a new “generation” of local elite would occur, with stage 1 beginning again.

elite across electoral outcomes. We describe these using the following result, letting $\Delta S_{t+1} = S_{t+1}^*(p_t|\omega_t = 1) - S_{t+1}^*(p_t|\omega_t = 0)$ be the *support premium* associated with the subsequent minority acquiescence to a weak-type local elite after overseeing an electoral victory:

Proposition 1. *For a weak-type elite, the support premium associated with occurrence of electoral victory, ΔS_{t+1} , is such that:*

- (i) $\Delta S_{t+1} > 0$ in the absence of any violence following either electoral outcome, $p_t(\omega_t = 1) = p_t(\omega_t = 0) = 0$.
- (ii) ΔS_{t+1} is decreasing in $p_t(\omega_t = 0)$ and increasing in $p_t(\omega_t = 1)$.
- (iii) $p_t(\omega_t = 0) > p_t(\omega_t = 1)$ is a necessary condition for $\Delta S_{t+1} < 0$.

Parts (ii) and (iii) are particularly important going forward, as they will inform how large investments in violence by a weak-type elite that has just overseen an electoral loss can result in it having higher levels of future minority acquiescence than had it experienced a victory. All proofs are in Appendix C.

Deriving Optimal Violence Strategies. We turn now to deriving the equilibrium violence strategies, beginning with the case of the strong-type elite. In stage 2, a strong-type elite, who by definition is always coming off of an electoral win in stage 1 ($\omega_t = 1$), maximizes:

$$\max_{\theta_t \in \{0,1\}} -c\theta_t + V_H^L + \pi \left(\frac{\pi(S_t, \underline{S})\theta_t + h(1 - \pi(S_t, \underline{S}))p_t}{h + (1 - h)\pi(S_t, \underline{S})}, \underline{S} \right) \Delta V.$$

Following the type-specific cost structure laid out above, we make an additional assumption to ensure that the cost of violence for a strong type is always sufficiently small, relative to that of the weak type, such that the threat of violence under a strong type is always (more) credible:

Assumption 1. $\frac{c}{\Delta\pi(1)} < \Delta V < \frac{\phi'(1)}{\pi'(1)}$, where $\pi'(1) \equiv \frac{\partial\pi(S_{t+1}^*)}{\partial p_t}|_{p_t=1}$ and $\Delta\pi(1) \equiv \pi(S_{t+1}^*(\theta_t = 1)) - \pi(S_{t+1}^*(\theta_t = 0))$ denote the marginal return on strength from violence for a weak- and strong-type elite, respectively, such that the relative cost of investing in violence at very high levels is sufficiently high for a weak type and sufficiently low for a strong type.

For instance, threats of violence against oppositional minorities might always be backed by the state for a strong-type elite (e.g., under Jim Crow) versus partly reliant on decentralized actors for a weak-type (e.g., to carry out lynchings).

Henceforth taking $\theta_t^* = 1$ as given, we now focus on the elite player of interest—the weak kind—and its violence strategy, p_t^* , under different electoral outcomes in stage 1. Beginning with the case in which a weak type nonetheless experiences electoral victory, it maximizes:

$$\max_{p_t \in [0,1]} -\phi(p_t) + V_L^L + \pi \left(\frac{\pi(S_t, \underline{S}) + h(1 - \pi(S_t, \underline{S}))p_t}{h + (1 - h)\pi(S_t, \underline{S})}, \underline{S} \right) \Delta V, \quad (3)$$

which is uniquely maximized by $p_t^*(\omega_t = 1) \in (0, 1)$ as a function of the relevant exogenous parameters in the model (S_t, \underline{S}, h) , with relevant comparative statics detailed below. In contrast, when the local elite is *revealed* to be weak by a loss—even a narrow one—it maximizes:

$$\max_{p_t \in [0,1]} -\phi(p_t) + V_L^L + \pi(p_t, \underline{S})\Delta V, \quad (4)$$

whose solution $p_t^*(\omega_t = 0) > p_t^*(\omega_t = 1)$ always, in order to compensate for its inability to pool with strong types electorally and the effects thereof on minority citizen beliefs. Together, these strategies and beliefs comprise a unique perfect Bayesian equilibrium. Formally:

Proposition 2. *There exists a unique perfect Bayesian equilibrium $(p_t^*, \theta_t^*, (s_{it}^*)_{i \in [0,1]}, S_{t+1}^*)$. In equilibrium, a weak-type elite always uses violence to suppress minority opposition, $p_t^*(\omega_t) \in (0, 1)$, where elite investment in violence is greater, and violence against oppositional minorities in turn more probable, following an electoral defeat, $p_t^*(0) > p_t^*(1)$, all else fixed.*

In other words, revelations of local elite weakness with an electoral loss favor *greater* elite investment in violence against oppositional minorities. Together with Proposition 1(ii)-(iii), this shows how violence may be used strategically by a more vulnerable local elite as a means of shoring up support and strength in the face of minority opposition—a kind of compensatory *backlash* to electoral defeat—thereby facilitating elite entrenchment over the longer run.

Besides these differences in violence strategies and minority acquiescence across wins and losses for weak-type local elite, equilibrium values vary with the exogenous parameters of the model in important ways. Of relative relevance is the following:

Proposition 3. *The level of investment in violence by a weak-type elite after an electoral loss, $p^*(\omega_t = 0)$, is weakly increasing in the baseline level of support, \underline{S} .*

In other words, having a high base of support in the form of \underline{S} further favors investment in violence following an electoral loss. That is, an elite with a higher baseline level of support (e.g., *narrow losers*) may be more likely to invest in violence after an electoral loss.

Interpreting Our Empirical Findings. Our theoretical and empirical designs begin from similar foundations: like Democrats in the post-Reconstruction South, this section has focused on politically-competitive local elites facing non-local elections, whose outcomes are determined (quasi-)randomly and have no direct local policy impact yet are nonetheless revelatory. From these similar foundations, they yield parallel conclusions, which we briefly review here.

First, Proposition 1 finds that $p_t(\omega_t = 0) > p_t(\omega_t = 1)$ is necessary for $S_{t+1}^*(p_t|\omega_t = 1) - S_{t+1}^*(p_t|\omega_t = 0) = \Delta S_{t+1} < 0$. That is, the future state of elite support may in fact be greater among those overseeing electoral losses, but only insofar as they invest more in anti-minority violence than winners. Such political benefits from using violence are shown to indeed be realized throughout Tables 5–7 in Section 4: former Black lynching counties in

particular exhibit a mapping from close electoral losses among Democrats in the late 1800s to sharply increased probabilities of electoral success in the early 1900s.

Second, Proposition 2 finds that elite investments in violence are indeed greater, and anti-minority violence in turn more probable, following an electoral defeat, wherein $p_t(\omega_t = 0) > p_t(\omega_t = 1)$ all else fixed as part of a perfect Bayesian equilibrium. This predicts our main result in columns 1–2 of Table 2, which estimates a large, positive discontinuity in the probability of Black lynchings following electoral defeat, as well as columns 3–4 of Table 9 on the concurrent use of newspapers by local Democratic elites to disseminate anti-Black narratives. This prediction continues to hold if elite investments in violence p_t are assumed to be rival and distributed amongst the opposition: insofar as Tables 5 and 7 establish that $\Delta S_{t+1} < 0$ as a result of anti-minority violence, the size of the opposition will have simultaneously shrunk in response to the threat of violence, meaning greater quantities distributed among a smaller opposition pie.

Finally, Proposition 3 asserts that, conditional upon an electoral loss, elite investments in violent will increase further with the baseline level of support, \underline{S} . This suggests that a relatively close electoral loss will induce greater investment in violence than a landslide loss in a place where the local elite has enjoyed lower favor historically. This further mirrors our empirical findings above (e.g., in Table 3; away from the zero threshold in Figure 5).

Altogether, these theoretical findings are consistent with revelations of political weakness among Democrats in the post-Reconstruction South (as revealed through even narrow electoral defeats, i.e., $\omega_t = 0$) serving to galvanize lynching activity (i.e., high p_t) against newly-enfranchised Black people and thereby consolidate support for the Democratic Party (i.e., higher state of support $(S_{t+1}, \underline{S}) \Rightarrow$ more probable $\sigma_{t+1} = H$) in the decades thereafter.

6 Conclusion

Less than five decades after the American Civil War freed four million enslaved Black Americans, the Democratic Party had fully established one-party rule across the South, thus ensuring that Black people continued to lack political and economic power for at least another half-century. While the civil rights movement subsequently ended de jure racial discrimination in the 1960s, the legacy of this prolonged disenfranchisement persists today. Black people residing in the South remain at a disadvantage relative to their white counterparts—and that says nothing of the millions more who fled the region during the 20th century, often enduring continued discrimination at destination (Althoff and Reichardt, 2022; Andrews et al., 2017; Boustan, 2010; Collins and Wanamaker, 2022; Craemer et al., 2020; Derenoncourt, 2022).

Ultimately, to advance progress on these dimensions means first understanding root causes. As we show, racial violence was central to denying Black people their power after emancipation. Even after the Enforcement Acts shuttered the paramilitary terrorism of the immediate postbellum period, lynch mobs arose in evasion of federal law to replace it. Lynching surged in the 1880s and 1890s, killing thousands of Black people and helping to bring about

a broad-based Black retreat from political and economic society (Cook, 2014; Jones et al., 2017; Williams, 2022). This outcome was not accidental. Rather, our research suggests that Democratic political elites strategically used racial hatred and violence as a means of maintaining white political hegemony in the South, in spite of emancipation and the 15th Amendment. These findings have important implications for modern day, as a wave of democratic backsliding threatens to spread throughout the Western world. Indeed, absent sufficient enforcement, ethnoracial violence will always serve as a tool for promoting the disenfranchisement of minority individuals and the survival of exclusionary institutions.

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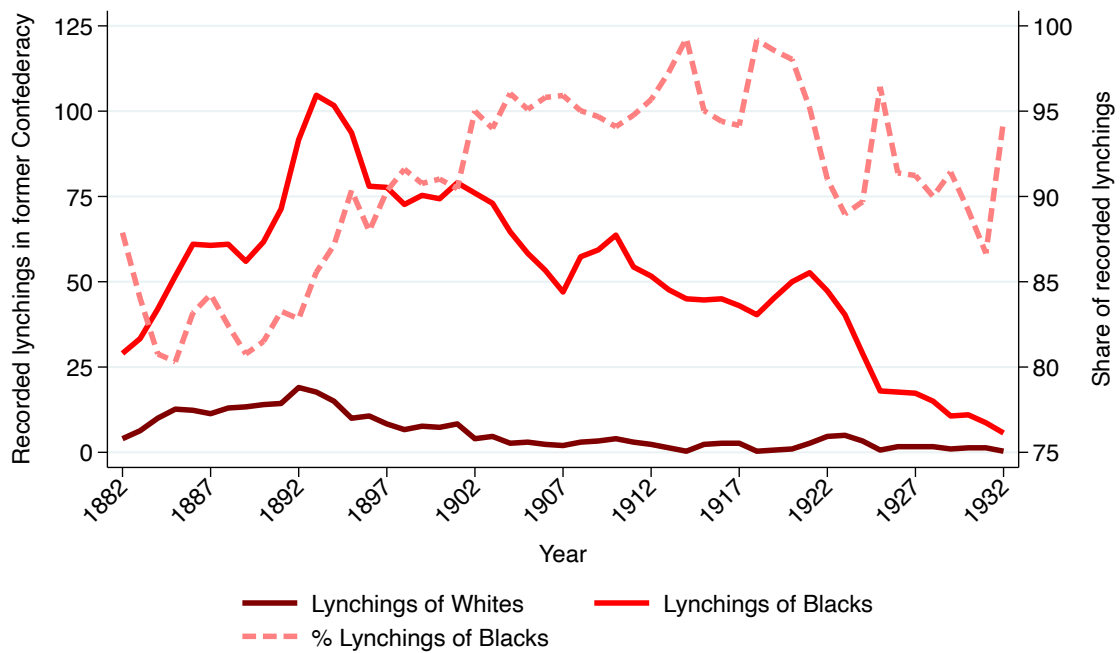
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Figures and Tables

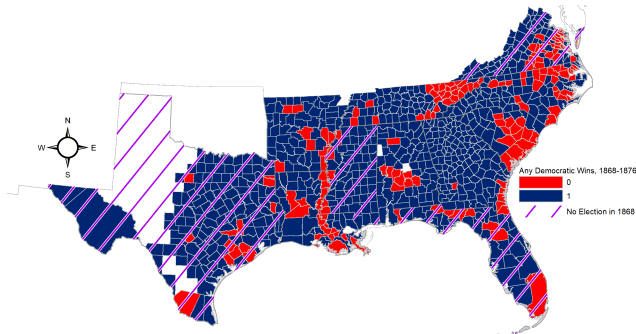
Figure 1: Lynchings in the Former Confederacy, 1882–1932



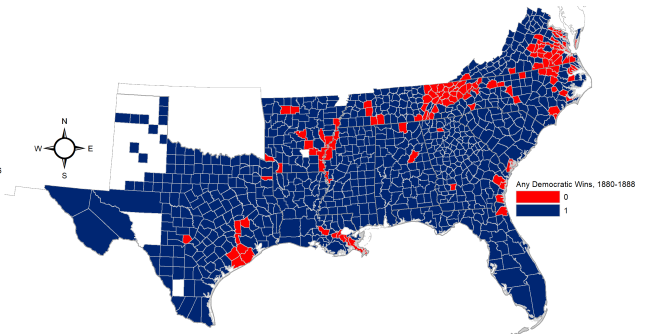
Notes: Three-year moving averages in the frequency of recorded lynchings across the eleven former Confederate states from 1882 to 1932 of whites (dark solid) and Blacks (light solid), as well as the share that were of Blacks (dashed). Lynching data based on the Historic American Lynching (HAL) Project from [Hines and Steelwater \(2023\)](#) except for Texas and Virginia, which are from [Seguin and Rigby \(2019\)](#). HAL data available at <http://people.uncw.edu/hinese/HAL/HAL%20Web%20Page.htm> (last accessed on April 24, 2023). Seguin and Rigby (2019) data available at <https://davidrigbysociology.com> (last accessed on July 30, 2023).

Figure 2: Consolidation of the “Solid South,” 1868–1912

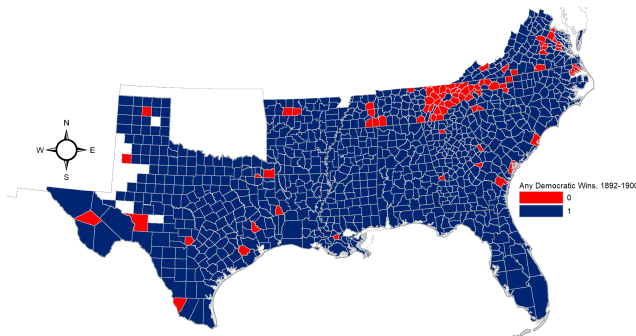
(a) Any Democratic Victories, 1868–1876



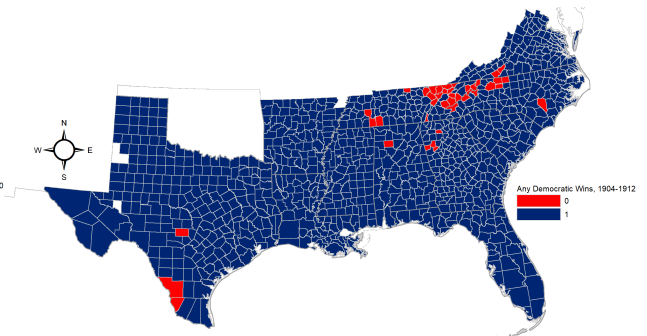
(b) Any Democratic Victories, 1880–1888



(c) Any Democratic Victories, 1892–1900

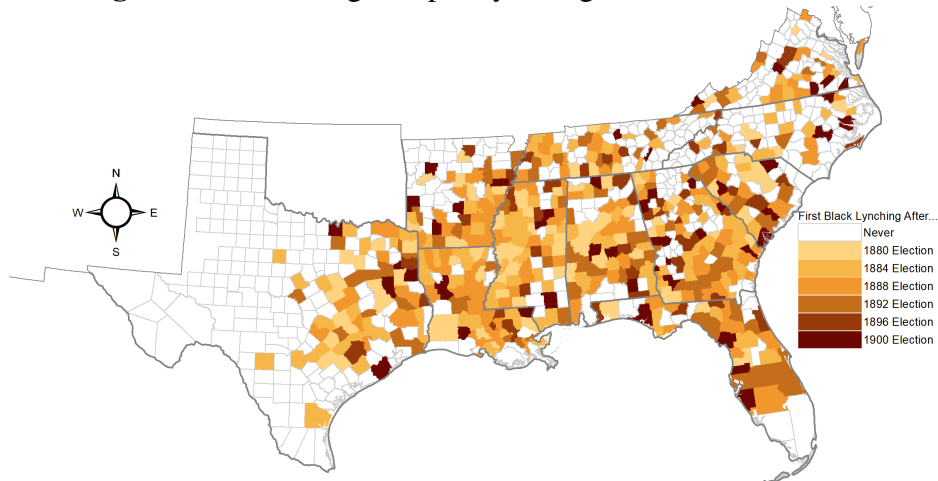


(d) Any Democratic Victories, 1904–1912



Notes: Map shows whether there were any Democratic presidential wins for a given sample county (in blue) over the four labeled election periods. For the purpose of the figure, counties boundaries are based on the (a) 1870, (b) 1880, (c) 1900, and (d) 1910 U.S. Censuses.

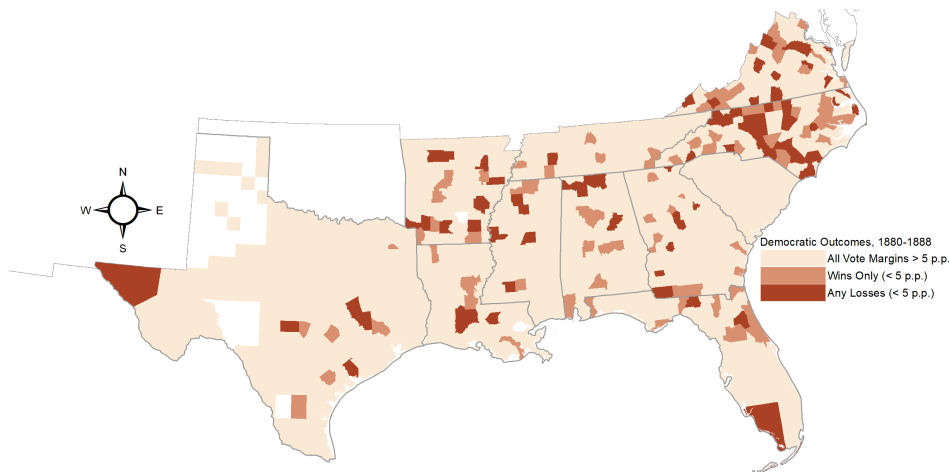
Figure 3: Visualizing Sample Lynching Variation, 1882–1904



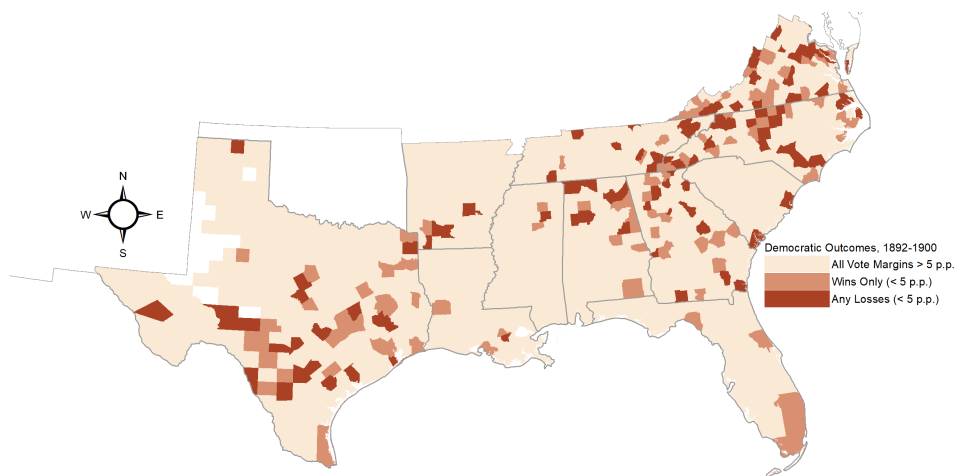
Notes: Map shows the spatial and temporal distribution of Black lynchings in our main sample, broken down by a county’s election period of first Black lynching in the sample. “Never” includes counties that experienced a lynching outside of the sample period. See Appendix A for details on data construction and coding. For the purpose of the figure, counties boundaries are based on the 1900 U.S. Census.

Figure 4: Visualizing Sample Treatment Variation, 1880–1900

(a) Close Democratic Wins and Losses, 1880–1888



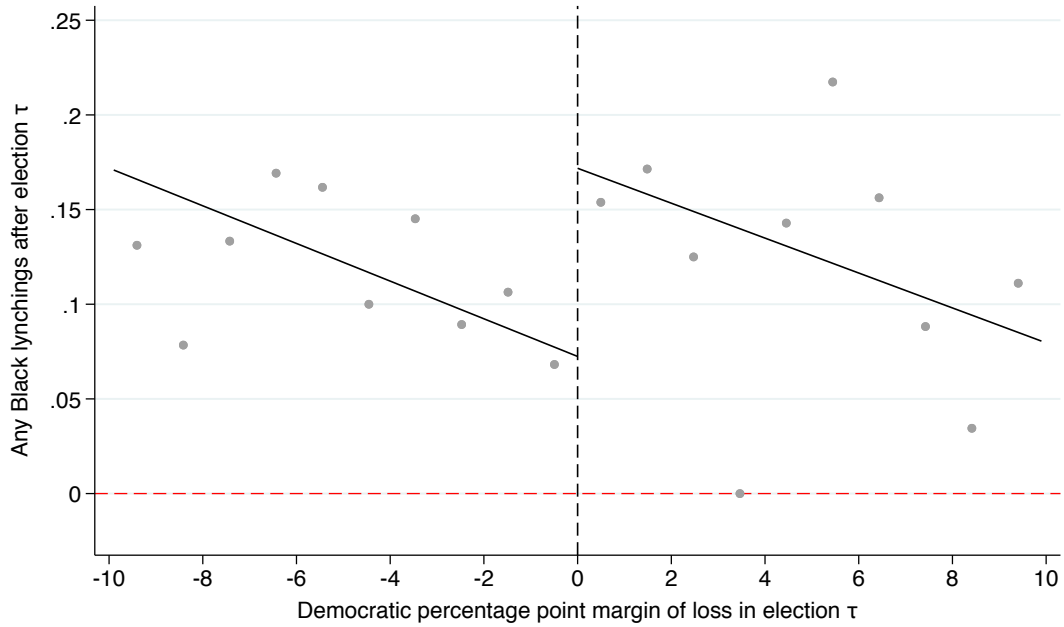
(b) Close Democratic Wins and Losses, 1892–1900



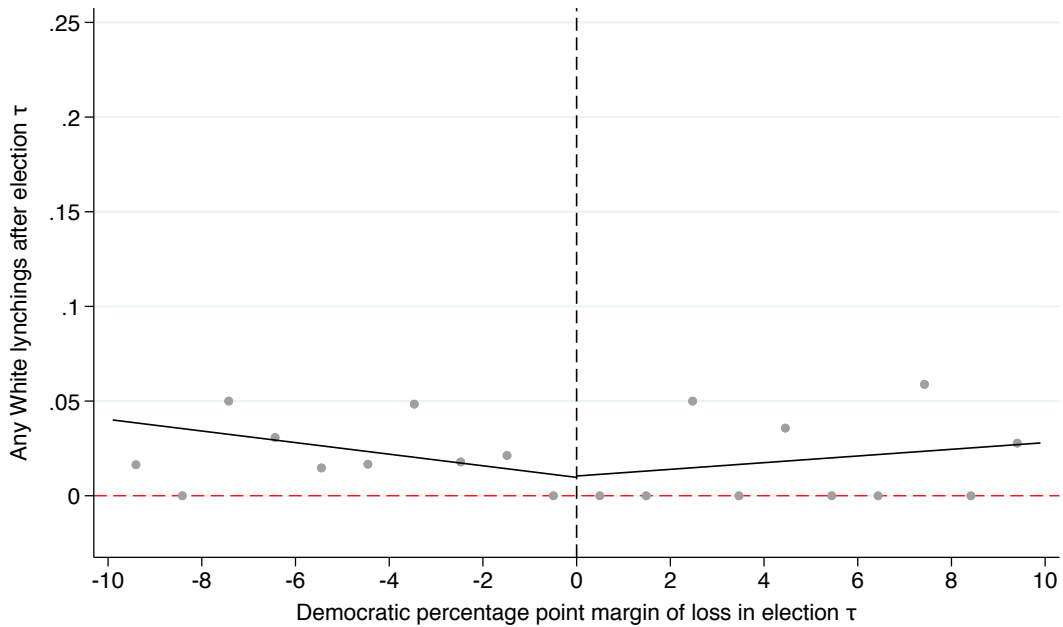
Notes: Map shows the distribution of close Democratic wins and losses, based on a very narrow 5 percentage point bandwidth, for sample counties over two election periods, 1880–1888 and 1892–1900. Counties that experienced any narrow Democratic losses during a given period in dark red. Counties that experienced only narrow Democratic wins (i.e., not narrow losses) during a given period in light red. Counties that experienced neither in light tan. See Appendix A for details on data construction and coding. For the purpose of the figure, counties boundaries are based on the 1880 (top) and 1900 (bottom) U.S. Censuses.

Figure 5: Lynchings by Democratic Loss Margin in Presidential Elections, 1880–1900

(a) Any Black Lynchings After Election τ

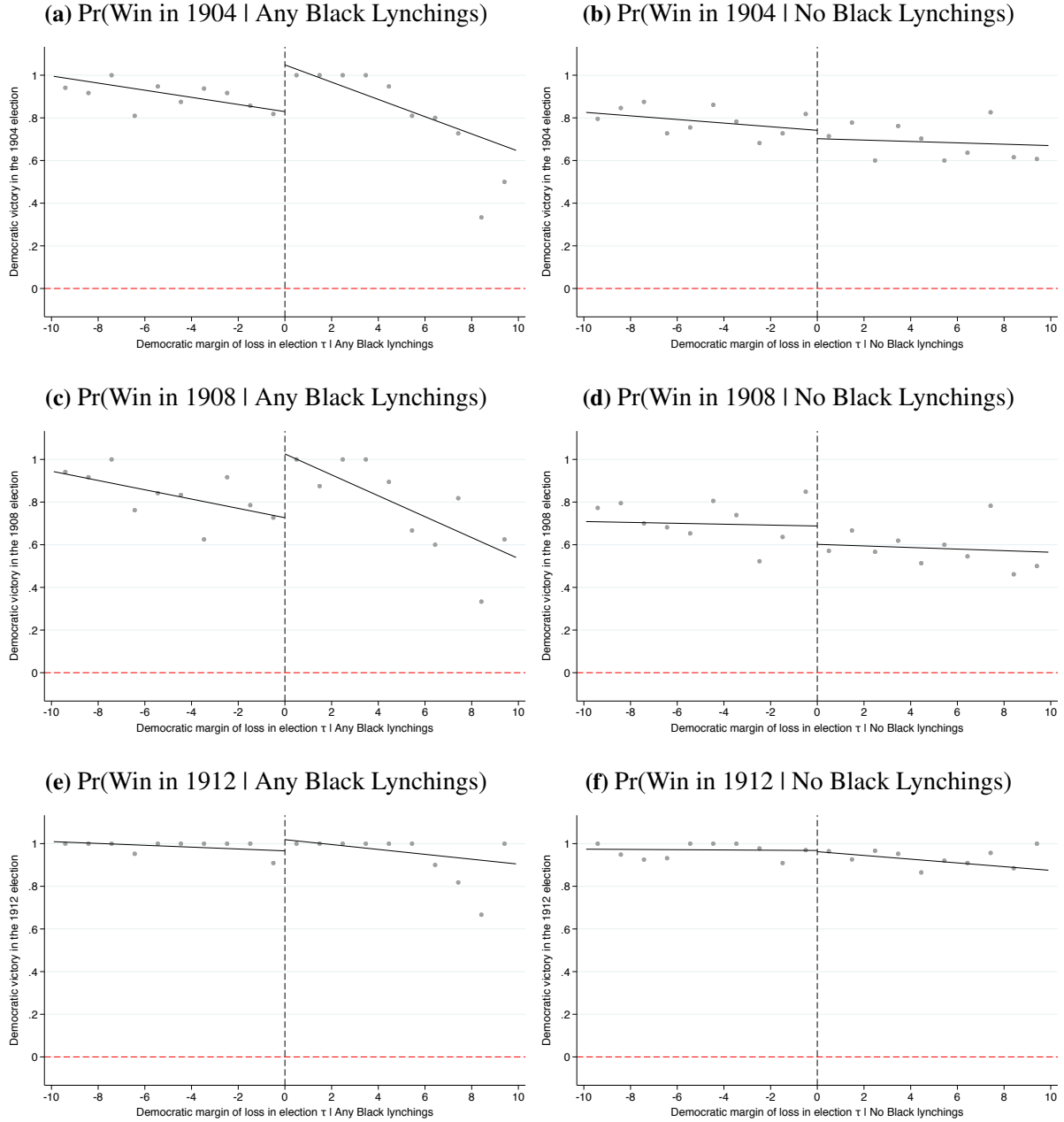


(b) Any White Lynchings After Election τ



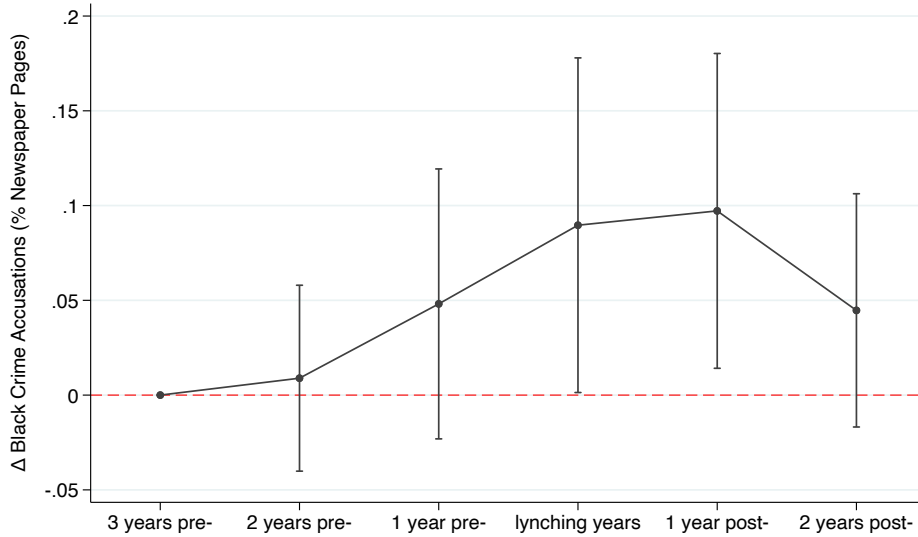
Notes: Binned estimates of the probability of (a) Black and (b) white lynchings during the four-year election period following a presidential election $\tau \in \{1880, \dots, 1900\}$ by the Democratic margin of victory in τ . Negative values on the x -axis indicate the Democratic candidate won a given county, while positive values indicate that they lost. All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. For RD estimates and associated p -value ranges, see Table 2.

Figure 6: 20th Century Democratic Wins By Loss Margins, 1880–1900



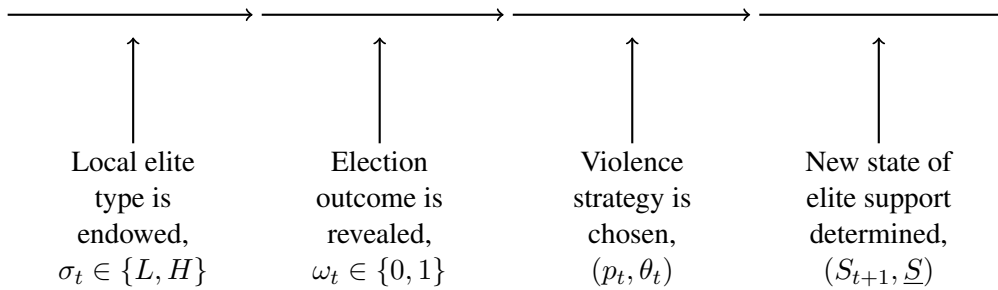
Notes: Binned estimates of the probability of a Democratic electoral victory in a county during the 1904 (panels a–b), 1908 (panels c–d), and 1912 (panels e–f) presidential elections by the Democratic margin of victory in presidential election $\tau \in \{1880, \dots, 1900\}$, conditional on a Black lynching having occurred in a given county through election period τ (panels a, c, and e) versus none having occurred (panels b, d, and f). Negative values on the x -axis indicate the Democratic candidate won a given county, while positive values indicate that they lost. All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. For RD estimates and associated p -value ranges, see Table 5.

Figure 7: Frequency of Black Crime Accusations Around Black Lynchings



Notes: This figure reports the average frequency of Black crime accusations in newspapers in the years leading up to, during, and immediately after a Black lynching event in a given county, relative to the average frequency 3 years prior. Regression includes year fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. Standard errors are clustered at the county level. Error bars represent 95% confidence intervals.

Figure 8: Sequence of Information and Actions in Period t



Notes: Figure shows the overall sequence of information and actions in the game during each period t . See text for more details.

Table 1: Regression Discontinuity Balance Tests: County Characteristics in 1880

Dependent Variable:	Log Population Density (1)	% Black Population (2)	% Former Slaveholders (3)	% Confederate Veterans (4)	Any Civil War Battles (5)	Average Farm Size (6)	Cotton Potential (7)	Tobacco Potential (8)
Democrat Lost County in Election τ	0.091 (0.090)	3.035 (2.249)	0.445 (0.578)	-0.647 (0.709)	0.071 (0.062)	83.310 (83.171)	0.005 (0.021)	-0.004 (0.006)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	27.60	28.10	28.49	35.89	21.49	32.59	26.01	27.62
Control outcome mean	-11.80	34.68	11.00	45.11	0.17	216.10	0.63	0.83
Observations	2,504	2,544	2,554	3,051	2,034	2,815	2,402	2,517

Dependent Variable:	Percent Aged 5–17 (9)	Manufacturing Wages per Capita (10)	Manufacturing Output per Capita (11)	Agricultural Output per Capita (12)	Real Estate per Capita (13)	Personal Property per Capita (14)	State Taxes per Capita (15)	Local Taxes per Capita (16)
Democrat Lost County in Election τ	0.016 (0.241)	0.455 (0.603)	3.563 (3.573)	0.627 (1.960)	-1.984 (6.267)	-3.271 (3.027)	-0.008 (0.042)	0.036 (0.125)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	27.58	25.81	24.31	24.58	26.51	32.97	30.33	24.55
Control outcome mean	32.86	2.26	15.18	38.75	94.04	45.47	0.84	0.30
Observations	2,504	2,386	2,279	2,282	2,412	2,838	2,646	2,268

Notes: This table reports the RD estimates corresponding to equation (1) for various pre-treatment county-level characteristics. See Section 3.1 for more details on variables. Estimates are based on quadratic running polynomials and the MSE-optimal bandwidth from Calonico et al. (2014). All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Lynchings After Democratic Electoral Losses, 1880–1900

Dependent Variable:	Any Lynchings of [...] People After Election τ			
	Black		White	
	(1)	(2)	(3)	(4)
	(a) Full Sample ($N = 5,849$)			
Democrat Lost County in Election τ	0.093** (0.045)	0.110** (0.046)	-0.002 (0.014)	-0.004 (0.014)
Election period fixed effects	Yes	Yes	Yes	Yes
Spatial covariates	No	Yes	No	Yes
Optimal bandwidth	21.56	19.91	25.85	27.15
Control outcome mean	0.14	0.14	0.02	0.02
Observations	2,035	1,879	2,390	2,475
	(b) Uncompetitive Counties in Election $\tau - 1$ Only ($N = 4,261$)			
Democrat Lost County in Election τ	0.225** (0.090)	0.229** (0.089)	0.031 (0.023)	0.026 (0.023)
Election period fixed effects	Yes	Yes	Yes	Yes
Spatial covariates	No	Yes	No	Yes
Optimal bandwidth	22.91	22.12	23.16	22.37
Control outcome mean	0.16	0.14	0.02	0.02
Observations	1,036	983	1,048	996

Notes: This table reports the RD estimates corresponding to equation (1) for whether there were any Black (columns 1–2) and white (columns 3–4) lynchings in a given county during the four-year election period following a presidential election $\tau \in \{1880, \dots, 1900\}$. Counties in panel (a) include those in the former Confederate states. Panel (b) restricts the sample to counties that were electorally uncompetitive in $\tau - 1$, using the median vote margin among sample Democratic electoral losses ($|Loss\ Margin_{c\tau}| = 15.90$) as the cutoff. Counties are matched with previous elections based on like county identifiers. Estimates are based on quadratic running polynomials and the MSE-optimal bandwidth from Calonico et al. (2014). All regressions include election period fixed effects, while columns 2 and 4 enter set of spatial covariates that includes state fixed effects and quadratic polynomials for county longitude and latitude. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Heterogeneous Effects: RD Estimates Conditional on Previous Election

Dependent Variable:	Any Black Lynchings After Election τ					
	Uncompetitive County in Election $\tau - 1$		Democrat-Won in Election $\tau - 1$		Uncompetitive + Democrat-Won in $\tau - 1$	
	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Lost County in Election τ	0.229** (0.089)	0.051 (0.057)	0.140** (0.059)	0.036 (0.068)	0.372*** (0.135)	0.054 (0.049)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	22.12	16.71	26.40	20.46	19.33	19.15
Control outcome mean	0.14	0.13	0.14	0.17	0.13	0.14
Observations	983	929	1,651	644	603	1,199

Notes: This table reports the RD estimates corresponding to equation (1) for whether there were any Black lynchings in a given county during the four-year election period following a presidential election $\tau \in \{1880, \dots, 1900\}$, conditional on whether a county was electorally uncompetitive in $\tau - 1$, using the median vote margin among sample Democratic electoral losses ($|Loss\ Margin_{c\tau}| = 15.90$) as the cutoff (columns 1–2), whether Democrats won a given county in the previous election $\tau - 1$ (columns 3–4), and both (columns 5–6). Estimates are based on quadratic running polynomials and the MSE-optimal bandwidth from Calonico et al. (2014). Previous election information matched based on like county identifiers. All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Identification and Robustness Checks on RD Estimates in Table 2

Dependent Variable:	Any Black Lynchings	Any White Lynchings
	(1)	(2)
	(a) Alternative Standard Errors	
1. Baseline (Columns 2 and 4 of Table 2)	0.110**	-0.004
Clustering by County	(0.046)	(0.014)
Clustering by County FIPS Identifier	(0.046)	(0.014)
Clustering by County-Decade	(0.047)	(0.014)
Clustering by Matched County-Pair	(0.046)	(0.014)
Clustering by County-Pair-Decade	(0.047)	(0.014)
Clustering by State-Election-Period	(0.046)	(0.014)
	(b) Alternative Control Sets	
2. No Controls or Fixed Effects	0.085*	-0.005
	(0.046)	(0.014)
3. No Longitude and Latitude Controls	0.083*	-0.006
	(0.045)	(0.014)
4. Baseline w/ State \times Period Fixed Effects	0.099**	-0.002
	(0.045)	(0.013)
5. Baseline w/ All Spatial Covariates \times Period FE	0.090**	-0.002
	(0.043)	(0.013)
6. Baseline w/ County-Pair Fixed Effects, Matched on Proximity in Longitude and Latitude	0.094***	-0.009
	(0.032)	(0.012)
7. Baseline w/ County Fixed Effects, Based on Unique County Boundaries	0.138***	-0.021*
	(0.028)	(0.013)
8. Controlling for All Variables From Table 1	0.094**	-0.005
	(0.047)	(0.015)
	(c) Alternative RD Specifications	
9. Optimal Bandwidth \times 0.5	0.154**	0.007
	(0.062)	(0.013)
10. Optimal Bandwidth \times 1.5	0.110**	-0.004
	(0.046)	(0.014)
11. Linear Running Polynomial	0.054*	-0.007
	(0.031)	(0.011)
12. Cubic Running Polynomial	0.106**	0.002
	(0.049)	(0.014)
13. Quartic Running Polynomial	0.113**	0.007
	(0.050)	(0.014)

Notes: This table reports the RD estimates corresponding to equation (1) for whether there were any Black (column 1) and white (column 2) lynchings in a given county during the four-year election period following a presidential election $\tau \in \{1880, \dots, 1900\}$. Estimates are based on quadratic running polynomials and the MSE-optimal bandwidth from Calonico et al. (2014), unless otherwise specified in panel (c). All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude, unless otherwise specified in panel (b). Standard errors are clustered at the county level, unless otherwise specified in panel (a). See Section 3.2 for a detailed overview of the items in each row. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: 20th Century Political Outcomes by Narrow Democratic Losses, 1880–1900

Dependent Variable:	1904		Democrat Won in . . .		1912		Voter Turnout in	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(a) Only Counties with Black Lynchings . . .								
<i>Any Lynchings Through the Election Period After τ:</i>								
Democrat Lost County in Election τ	0.248***		0.299***		0.033		-4.490**	
	(0.067)		(0.084)		(0.036)		(2.222)	
<i>Any Lynchings During the Election Period After τ:</i>								
Democrat Lost County in Election τ		0.223**		0.407***		0.023		-5.423*
		(0.100)		(0.110)		(0.017)		(3.156)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	16.95	17.15	19.56	17.26	19.64	14.54	21.36	25.42
Control outcome mean	0.93	0.93	0.88	0.90	0.99	1.00	30.87	29.61
Observations	457	200	543	200	547	167	568	303
(b) Only Counties with No Black Lynchings . . .								
<i>No Lynchings Through the Election Period After τ:</i>								
Democrat Lost County in Election τ	-0.035		-0.086		-0.021		-1.267	
	(0.056)		(0.058)		(0.029)		(1.878)	
<i>No Lynchings During the Election Period After τ:</i>								
Democrat Lost County in Election τ		0.003		-0.048		-0.010		-0.337
		(0.048)		(0.054)		(0.026)		(1.615)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	29.02	35.18	31.58	32.76	28.94	30.03	23.19	28.63
Control outcome mean	0.84	0.88	0.79	0.82	0.97	0.98	36.63	35.49
Observations	1,780	2,613	1,896	2,482	1,778	2,315	1,465	2,145

Notes: This table reports the RD estimates corresponding to equation (1) for whether there was a Democratic electoral victory in a given county in the 1904 (columns 1–2), 1908 (columns 3–4), and 1912 (column 5–6) presidential elections, as well as the average voter turnout rate in a county across the 1904–12 elections (column 7–8). Regressions in panel (a) restrict to counties in which a Black lynching occurred at some point either through the four-year period following election τ (odd columns) or strictly during the period following τ (even columns), while regressions in panel (b) restrict to the complementary cases without Black Lynchings. Counties matched with future elections based on like county identifiers. Estimates are based on quadratic running polynomials and the MSE-optimal bandwidth from [Calonico et al. \(2014\)](#). All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: 20th Century Educational Outcomes by Narrow Democratic Losses, 1880–1900

Dependent Variable:	Literacy Rate Among [...] People in 1910				School Enrollment Among [...] Children in 1910			
	Black		White		Black		White	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(a) Only Counties with Black Lynchings...								
<i>Any Lynchings Through the Election Period After τ:</i>								
Democrat Lost County in Election τ	-3.891*** (1.425)		0.523 (1.093)		-4.499* (2.688)		-0.888 (1.644)	
<i>Any Lynchings During the Election Period After τ:</i>								
Democrat Lost County in Election τ		-6.127*** (2.047)		0.299 (1.531)		-12.934*** (3.883)		-3.123 (2.944)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	23.68	20.03	24.07	19.19	20.35	17.60	23.92	18.71
Control outcome mean	68.01	67.38	91.92	92.15	57.74	56.15	73.68	72.77
Observations	689	246	703	226	570	203	697	222
(b) Only Counties with No Black Lynchings...								
<i>No Lynchings Through the Election Period After τ:</i>								
Democrat Lost County in Election τ	-2.122 (1.720)		-0.346 (0.816)		-1.173 (2.461)		-0.261 (1.036)	
<i>No Lynchings During the Election Period After τ:</i>								
Democrat Lost County in Election τ		-1.802 (1.448)		-0.568 (0.746)		-0.793 (2.251)		-0.114 (0.962)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	25.47	27.10	28.74	28.58	26.11	23.69	23.14	23.13
Control outcome mean	68.44	68.48	90.24	90.62	57.16	57.38	73.49	73.61
Observations	1,598	2,120	1,769	2,224	1,616	1,889	1,513	1,885

Notes: This table reports the RD estimates corresponding to equation (1) for the county-level literacy rate in 1910 among Blacks (column 1–2) and whites (column 3–4) and the county-level school enrollment rate in 1910 among Blacks (column 5–6) and whites (column 7–8) aged 6–14. Regressions in panel (a) restrict to counties in which a Black lynching occurred at some point either through the four-year period following election τ (odd columns) or strictly during the period following τ (even columns), while regressions in panel (b) restrict to the complementary cases without Black Lynchings. Estimates are based on quadratic running polynomials and the MSE-optimal bandwidth from [Calonicó et al. \(2014\)](#). All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Political Participation in the Time of the Voting Rights Act

Dependent Variable:	Registered [...] Voters (% [...] in 1960)							
	Non-White				White			
	Pre-VRA		Post-VRA		Pre-VRA		Post-VRA	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(a) Only Counties with Black Lynchings...								
<i>Any Lynchings Through the Election Period After τ:</i>								
Democrat Lost County in Election τ	-5.044**		-4.352**		0.299		-2.744	
	(2.320)		(1.997)		(2.917)		(5.484)	
<i>Any Lynchings During the Election Period After τ:</i>								
Democrat Lost County in Election τ		-6.073*		-11.089***		4.352		-6.985
		(3.498)		(3.016)		(5.769)		(7.792)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	26.92	24.20	21.06	19.63	21.84	23.29	22.98	23.94
Control outcome mean	18.90	17.61	28.10	27.17	46.10	45.80	56.40	55.73
Observations	433	180	279	118	338	175	311	155
(b) Only Counties with No Black Lynchings...								
<i>No Lynchings Through the Election Period After τ:</i>								
Democrat Lost County in Election τ	-0.407		-0.206		-1.552		-3.901	
	(2.129)		(3.105)		(3.472)		(4.510)	
<i>No Lynchings During the Election Period After τ:</i>								
Democrat Lost County in Election τ		-1.657		-0.340		-1.234		-1.633
		(1.963)		(2.498)		(2.805)		(3.742)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	31.94	25.13	29.52	35.40	27.31	27.99	23.71	28.87
Control outcome mean	19.41	19.63	26.87	27.01	46.02	46.00	58.21	58.03
Observations	941	1,014	802	1,167	834	1,098	684	998

Notes: This table reports the RD estimates corresponding to equation (1) for county-level rates of voter registration in 1962–64 (columns 1–2 and 5–6) and 1966–67 (columns 3–4 and 7–8) among non-whites (columns 1–4) and whites (columns 5–8). Regressions in panel (a) restrict to counties in which a Black lynching occurred at some point either through the four-year period following election τ (odd columns) or strictly during the period following τ (even columns), while regressions in panel (b) restrict to the complementary cases without Black lynchings. Estimates are based on quadratic running polynomials and the MSE-optimal bandwidth from Calonico et al. (2014). All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: RD Estimates Conditional on Jim Crow Voting Laws

Dependent Variable:	Any Black Lynchings After Election τ					
	Any Jim Crow Voting Laws		Any Ballot Requirements		Any Poll Taxes	
	Before	After	Before	After	Before	After
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Lost County in Election τ	0.148** (0.067)	0.045 (0.052)	0.117** (0.055)	0.064 (0.062)	0.125** (0.060)	0.058 (0.056)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	21.84	22.74	19.84	26.37	22.96	21.87
Control outcome mean	0.16	0.12	0.15	0.14	0.17	0.11
Observations	1,148	972	1,499	504	1,305	820

Notes: This table reports the RD estimates corresponding to equation (1) for whether there were any Black lynchings in a given county during the four-year election period following a presidential election $\tau \in \{1880, \dots, 1900\}$, conditional on a county's state yet having enacted any Jim Crow laws (columns 1–2), any ballot requirement laws (e.g., literacy test, multi-box, or secret ballot laws) (columns 3–4), and any poll taxes (columns 5–6) as of τ . Estimates are based on quadratic running polynomials and the MSE-optimal bandwidth from Calónico et al. (2014). State-level Jim Crow data come from Jones et al. (2012). All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. See Appendix Figure B.3 for a comparison of effect sizes under Jim Crow relative to the pre-Jim Crow benchmark in column 1, holding fixed all other aspects of the latter's specification. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Partisan Media and Black Crime Accusations in Newspapers

Dependent Variable:	Frequency of Black Crime Accusations (% Pages in Newspaper)					
	Any		Democratic		Non-Democratic	
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Lost County in τ	0.185*** (0.041)	0.069*** (0.022)	0.285*** (0.054)	0.428*** (0.015)	-0.455*** (0.094)	-0.579*** (0.039)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
News year – Election year FE	Yes	Yes	Yes	Yes	Yes	Yes
City fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	No	Yes	No	Yes	No	Yes
Optimal bandwidth	11.74	4.45	10.58	3.77	11.97	7.37
Control outcome mean	0.19	0.16	0.19	0.18	0.17	0.21
Observations	2,109	747	1,326	406	182	131

Notes: This table reports the RD estimates for the frequency of Black crime accusations in a given newspaper-year during the four-year election period following a presidential election $\tau \in \{1880, \dots, 1900\}$, including conditional on a newspaper's partisan affiliation between Democratic (columns 3–4) or non-Democratic (columns 5–6). Estimates are based on quadratic running polynomials and the MSE-optimal bandwidth from Calónico et al. (2014). Black crime accusations based on accusations of rape, murder, or robbery in articles archived at newspapers.com. Data on newspaper affiliations come from Gentzkow et al. (2014a,b). All regressions include fixed effects presidential for election period, newspaper publication year minus most recent election year, and newspaper city or town. Additional spatial covariates in even columns include quadratic polynomials in county longitude and latitude. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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A Data Appendix

This Appendix describes the dataset and sample construction used for our empirical analysis. Specifically, we (i) describe our choices of states and election years, (ii) provide further discussion of how our main treatment and outcome variables are coded, (iii) show summary statistics for our main variables, and (iv) provide more details on our newspaper data construction.

A.1 Sample

In this first section, we describe the choices of states and years used for our analysis, while providing additional details and exercises to address potential concerns about these choices.

Choice of Sample States. For our analysis, we focus on the eleven former Confederate states of Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia. These arguably best capture what would become consolidated as the “Solid South” by the early 20th century—a regional Democratic Party stronghold in which all states by 1903 were characterized by some form of explicit voter suppression of Black people. All of these states were strongly Democratic in their elite composition and voted for the Democratic presidential candidate in every election from 1880, the first election following the end of Reconstruction in the South, through 1916, although nontrivial political competition from Republicans and Populists continued to exist *within* many of these states throughout the late 19th century (see Figure 2). Other states sometimes labeled as Southern—including the five “border states” of Missouri, Kentucky, West Virginia, and Maryland, and Delaware not under Confederate control during the American Civil War—occasionally opposed Democrats during this time and are less consistently grouped with the Solid South (Paxson, 1915).

Besides this shared political context, lynchings of Black was also common across all eleven of the former Confederate states in the decades following the end of Reconstruction (see Figure 3). As we seek to study the impact of Democratic political performance on the probability of local lynchings, and implications thereof for subsequent Democratic Party entrenchment, it is therefore natural to focus on these eleven states that have these elements in common.

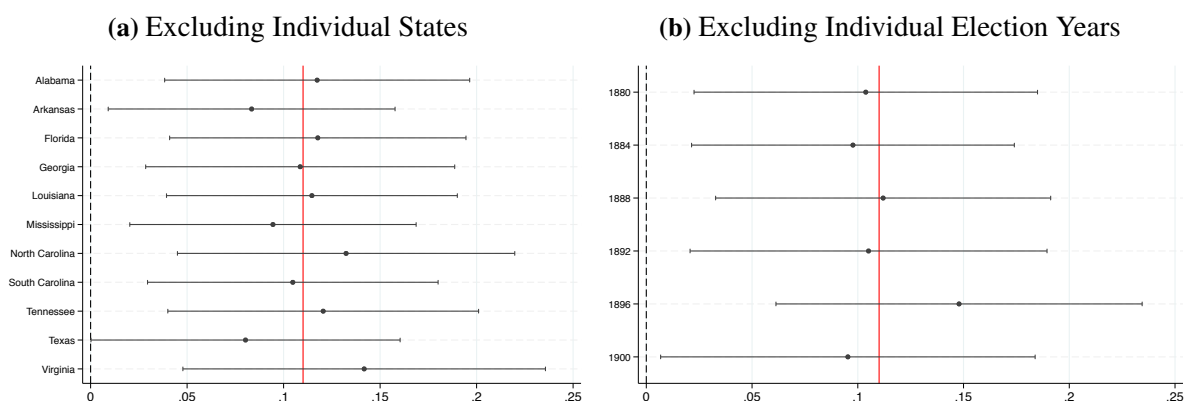
Meanwhile, it is not clear *a priori* what relationship might exist, if any, between local Democratic political performance and racial violence outside of the former Confederate states. Although lynchings of Black people occurred wherever sizable Black populations were present during the period of study (as well as lynchings involving non-Black people of color in some parts of the country), local elite political interests with respect to the political empowerment of minority individuals varied considerably, including among Democrats. For instance, former border states such as Kentucky and Maryland shared many industrial interests with the Northeast, which splintered the Democratic Party along fiscal and monetary issues and bolstered Republicans. As such, Bourbon Democrats like Grover Cleveland and Alton B. Parker

tended to perform well, while agrarian Democrats like William Jennings Bryan did not. This schism came to a head in the 1896 presidential election, when the Bourbon faction nominated the Kentuckian fiscal conservative and former-abolitionist John M. Palmer under the National Democratic Party label (Brown, 1980; Schlup, 1978). Likewise, the Democratic Party had lost much of its influence statewide in Delaware by the 1890s. In such cases, it is not clear how lynchings might be related to local political elite interests, including those of Democrats.

Even within our sample states, local elites were in some places strongly non-Democratic, such as in some areas of Virginia, where the “Readjusters” split off from the Democrats over fiscal issues to form a biracial coalition with Republicans (Dailey, 2009); in North Carolina, where Republicans and the agrarian Populists formed an electoral union, which brought several hundred Black people to public office (Edmonds, 1951); and in Tennessee, where an Eastern “Unionist” region consistently voted Republican.

Given this historical nuance, we explore sample sensitivity along several dimensions. First, we drop each of the eleven former Confederate states one-by-one from the sample in panel (a) of Appendix Figure A.1, holding all other aspects of the specification fixed. On one hand, no particular state appears to be driving our main effect. On the other hand, point estimates do increase if either Virginia, North Carolina, or Tennessee are omitted from the sample, highlighting the salience of non-Democratic local elite interests in parts of those states. Beyond these former Confederate sample states, effects vary by state. In Delaware and Kentucky, where Democrats had reduced prominence, estimates in equation 1 are in fact negative, of -.621 and -.153, respectively. In West Virginia, Maryland, and Missouri, estimates are positive but less precise, of .009, .062, and .069, respectively. With these states included in our baseline specification, our overall estimate is positive but noisy, of .028 (.030). Overall, this points to a less-salient Democratic elite identity in many border state areas compared to in the former Confederate states, serving to reaffirm our choice of sample states.

Figure A.1: Sensitivity Tests: Excluding Individual States and Years



Notes: This figure reports the RD estimates corresponding to equation (1) for whether there were any Black lynchings in a given county during the four-year election period following a presidential election $\tau \in \{1880, \dots, 1900\}$. Panel (a) excludes sample states one-by-one, where the excluded state is reported on the vertical axis. Panel (b) excludes sample election periods one-by-one, where the excluded election is reported on the vertical axis. All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. Compare estimates to column 4 in Table 2 (shown in solid red). Standard errors are clustered at the county level. Error bars represent 90% confidence intervals.

Choice of Sample Period. Our sample period begins with the 1880 election, which was the first following the end of Reconstruction in the South and the resultant exodus of all federal troops. Our main sample period concludes with the 1900 election, for two related reasons: (i) Jim Crow laws impeded Black voting in all former Confederate states by 1903; (ii) the Solid South was in turn arguably consolidated by the 1904 election, with little political competition within the South and Democrats thus losing few counties in the South in the elections after the 1900 election (see Figures 2 and 6). As with states, our results are not sensitive to omitting any election period during this window, as shown in panel (b) of Appendix Figure A.1.

A.2 Variables

In this second section, we describe in detail how we code the explanatory and dependent variables used in our main analysis, while also providing additional details and sensitivity exercises to further address potential concerns about these choices.

Coding the Treatment Variable. Our explanatory variable $Democratic\ Loss_{c\tau}$ in equation (1) captures whether the Democratic candidate for president lost the popular vote in county c in a given election year τ , where $\tau = \{1880, 1884, \dots, 1900\}$. Concretely, this is coded as a 1 if the Democratic Party's candidate for president received second place at best in a given election year and as a 0 otherwise. By interacting $Democratic\ Loss_{c\tau}$ with our running variable, defined as the Democratic loss margin $f(Loss\ Margin_{c\tau})$, we estimate treatment effects based on counties with very close election outcomes in a given election year. Vote share data for candidates used to construct these variables are from Clubb et al. (2006). Treatment and running variable values are then matched to lynching events for all counties with the same identifier across both election and lynching records during a given election period. Finally, election periods τ are stacked to generate time-varying treatment and running variables. In our setting, close elections in one election period are not generally a permanent feature, so counties may enter and leave the dataset conditional upon the bandwidth of the analysis. Specifically, about 22% of county-election-years experiencing a close election $|Loss\ Margin_{c\tau}| < 5$ in τ in our sample did so again in $\tau + 1$. To illustrate these dynamics, Figure 4 shows the distribution of counties with $|Loss\ Margin_{c\tau}| < 5$ for the two periods of 1880–88 and 1892–1900.

Measuring Electoral Periods. We are interested in the effect of electoral outcomes on lynching activity over the subsequent four-year election period. We assume for coding purposes that electoral periods begin after November 8 of a given election year τ and last through November 7 of $\tau + 1$. This choice is based on the fact that presidential elections may be held any day between November 2 and 8, with the 1892 election being held on the latter. Results are not sensitive to omitting election month lynchings from the sample entirely. Relative to our baseline estimate of .110 (.046) in row 1 of Table 4, this yields estimates of .111 (.046). This robustness stems from the fact that the probability of a lynching occurring in a given county during any

given month is extremely low. Specifically, among all sample counties that experienced a Black lynching in a given four year election period, restricting to a bandwidth of 50 p.p., less than 1% experienced all of their Black lynching activity during an election month.

Use of Presidential Elections. For our treatment variation, we rely on election results from national rather than local elections. We do so principally because the former lack a direct effect on actual local Democratic power or county-level policy outcomes, to which racial violence might be endogenous. For instance, if a Democrat loses a local election (e.g., mayor, sheriff, judge), the resultant (Republican or Populist) winner might be more likely to punish perpetrators of racial violence, thus attenuating treatment effects. We moreover focus on presidential elections rather than congressional elections. In addition to congressional elections generally being less competitive for Democrats, the lynching records are primarily coded at the county level, and congressional-district and county boundaries are often not congruent. For further detail on this latter concern, see our discussion of boundary harmonization below

At the same time, our choice to rely on results from presidential elections rests on the assumption that the local outcomes of presidential elections are likely to have nonetheless been psychologically salient among the local elite and masses—representing, for instance, a useful signal of the relative strength of the local opposition, particularly in contrast to past electoral outcomes (see, for instance, Table 3). We provide anecdotal evidence that county-level presidential election results were indeed locally salient in Appendix Figure A.2, which shows a collection of post-election newspaper excerpts from newspapers.com. Importantly, these excerpts discuss results with emphasis on the extensive margin (i.e., who won versus lost) but often election closeness, too. Moreover, a number of the excerpts reference the racial attributes of the opposition’s local constituency.

County Identifiers and Boundary Harmonization. Because we are interested in the county-level outcomes of Democrats and lynching activity in response to those outcomes within a given county, our default data coding requires that county identifiers (i.e., FIPS code and/or name) in the [Clubb et al. \(2006\)](#) election records match exactly with those in the raw lynching records from [Seguin and Rigby \(2019\)](#) or the Historic American Lynching (HAL) Project from [Hines and Steelwater \(2023\)](#). This choice is largely irrelevant, with a few exceptions. First, a few counties in Texas, Arkansas, and South Carolina have missing or non-existent voting data during the sample period and therefore cannot be matched to lynching records at all. These county-year observations are by default omitted from the analysis. Second, some data cannot be merged across the election records (for a given election year τ) and lynching records (for a given year t during the four-year period following τ) based on these county identifiers—for example, if there was a county split or merger that resulted in a new set of county identifiers. In such cases, one alternative option to our coding choice would be to match a lynching location to its proximate election county by manually crosswalking the latter’s boundaries with the former’s. However, it is not always clear in such cases that a given lynching would have been

Figure A.2: Salience of County-level Presidential Election Results: Newspapers

(a) Lincoln County, NC (1896)

(b) Jefferson County, AR (1900)

THE ELECTION IN LINCOLN.
A Close Fight in the County.—
Bryan Wins on a Small Margin
 The vote of Lincoln county was especially large on last Tuesday. The vote polled for the Legislature being 2,416, about 300 more than was polled two years ago. We give the vote for county legislative ticket as follows:

PRESIDENT.	
Bryan.....	1,349
McKinley.....	1,010

sas in Tuesday's election. General apathy, notwithstanding persistent warnings, made inroads, and in consequence the **Democrats lost** several counties. In Jefferson they failed to turn out, and for the first time in ten years the county goes Republican. Bryan received 919 votes and McKinley 1,069 so far as reported, and McKinley's majority will be about 250 in

(c) Coffee County, GA (1900)

(d) Fulton County, GA (1896)

COFFEE FOR MCKINLEY.
 Pearson, Ga., November 6.—(Special.)—Negroes and populists voted solidly for republicans. The outlook in Coffee county gives a small republican majority. This precinct gives McKinley's electors a majority of 4; Brantley for congress, 1.

Atlanta, Ga., Nov. 3.—Fulton county gives Bryan and Sewall a majority of 1,356, out of a total vote of 7,734. McKinley's vote was 3,189 in the county. Out of the seven city wards, McKinley carried one, the Fourth, which has a large negro population. This is the home ward of Hen-

(e) Limestone County, AL (1884)

(f) Elizabeth City County, VA (1892)

ATHENS, ALA.
Wheeler's Majority in the County—
Death of Mrs. Sallis Block.
 Special to the American.
 ATHENS, ALA., Nov. 6.—Gen. Wheeler, Democrat, carried this county over Day, Republican, by 61 majority. The county gives the Blaine Electors a small majority.

Fired on the Negroes.
 FORTRESS MONROE, November 8.—Elizabeth City and County gives Harrison, 1,293; Cleveland, 871. The Huntington Rifles were called out and fired on the crowd of colored voters. A man named Briggs was wounded in the thigh. All is quiet now.

(g) Buncombe County, NC (1896)

(h) Clinch County, GA (1896)

ASHEVILLE, Nov. 3.—The **Republicans won** to-day in the city and apparently in the county. The majority in Asheville for the Radical ticket is about 150 and it is figured that the same ticket will carry the county by 150 to 250. In the negro precinct, the second, the Republican majority was 319. The election

Homerville, Ga., Nov. 3.—A conservative estimate gives this, Clinch county, to **Brantley for congress by 300 majority. Bryan wins** by 100 majority. The official count won't change these figures materially. All the populists and dissatisfied democrats on the money question voted for McKinley.

Notes: Examples of Southern newspapers describing the county-level results of presidential elections from between 1880 and 1900. Panel (a) shows an excerpt from the *Lincoln Times-News*, printed November 5, 1896. Panel (b) shows an excerpt from the *Arkansas Democrat*, printed November 8, 1900. Panel (c) shows an excerpt from the *Atlanta Constitution*, printed November 6, 1900. Panel (d) shows an excerpt from the *Morning News*, printed November 4, 1896. Panel (e) shows an excerpt from the *Tennessean*, printed November 6, 1884. Panel (f) shows an excerpt from the *Daily Arkansas Gazette*, printed November 8, 1892. Panel (g) shows an excerpt from the *Newton Enterprise*, printed November 3, 1896. Panel (h) shows an excerpt from the *Morning News*, printed November 4, 1896. Clippings screencapped from newspapers.com.

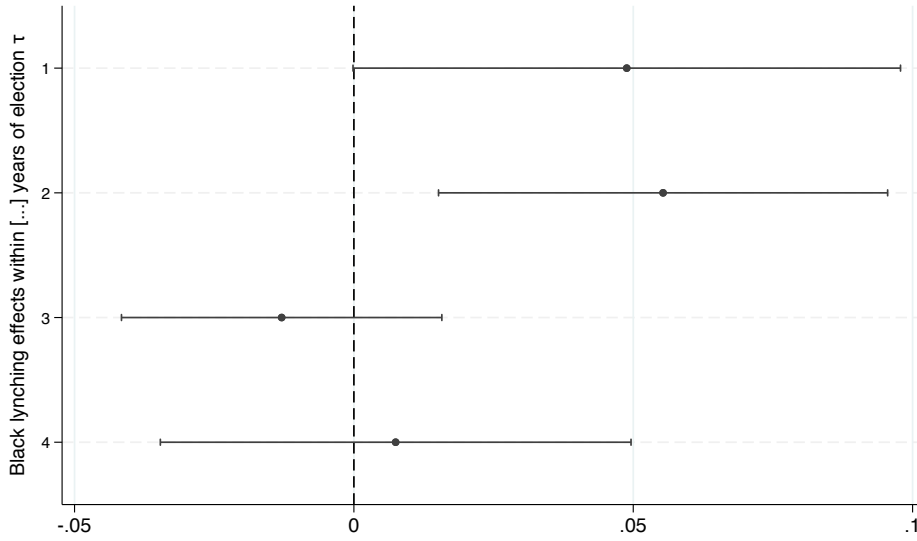
carried out in response to the election result of the *crosswalked* county, given the discrepancy in county identifier and associated boundaries, nor is it always possible to clearly crosswalk lynching locations to a single proximate election county (e.g., in the case of county mergers). A second option is to drop affected county-year observations from the analysis entirely. Neither option ultimately affects our estimates relative to the baseline of .110 (.046) in row 1 of Table 4, which becomes .092 (.046) and .107 (.046), respectively.

In general, our identification strategy precludes harmonizing county boundaries to a single, common year, as it is essential for our RD that electoral margins (and the local elite outcomes they represent) reflect their true values for a given county-election period. As a result, for the purpose of defining clusters, a county is assumed to become a different administrative unit if its boundaries change across election periods, even if its formal identifiers remain unchanged in the data (as is also the case with our assignment of county fixed effects in Table 4). That being said, we do harmonize boundaries for longer-run data, for which boundaries are likely to differ more greatly (e.g., in Tables 6 and 7), as well as the pre-treatment characteristics in Table 1, to decadal county boundaries for 1880–1900, using the county-to-county area-based crosswalks from Ferrara et al. (2021). Our main results in row 1 of Table 4 are unaffected if we restrict the sample to the set of county identifiers with land areas that are unchanged over the sample period, with an estimate of .109 (.046). If we make the same sample restriction and then also include county fixed effects, estimates and precision increase somewhat, to .169 (.030).

Coding the Outcome Variable. Our main outcome variable $Any\ Lynching_{c(s)\tau}$ in equation (1) indicates whether any lynchings of Black (or white) people occurred in county c of state s during the four-year electoral period following the conclusion of election τ , based on presidential elections years $\tau = \{1880, 1884, \dots, 1900\}$. Given that few counties experience many lynching events, our primary outcome is defined as an indicator variable for whether a lynching event occurred during a given election period. We nevertheless define alternative outcome variables based on logged counts and rates (per 10,000 persons), which yield similar estimates to our baseline (see columns 1–2 and 4–5 of Appendix Table A.1). Estimates based on unlogged counts and rates remain large but are noisy, at 0.073 (0.059) and 2.091 (1.882), respectively, relative to means of .229 (.705) and .596 (2.939), using our preferred specification otherwise.

For similar reasons, we define the outcome variable by election period rather than have it vary by year within election periods. We relax this choice in columns 3 and 6 of Appendix Table A.1, which results in highly similar relative effect sizes. Note the difference in outcome mean here relative to in Table 2: whereas about 14% of county-election-year periods had Black lynching events during the sample period, only about 4% of county-years had them. Appendix Figure A.3 furthermore separately estimates effects by the number of years since a presidential election. Hence, row 1 considers only the 12 months after an election period, and so on. This figure shows lynchings as clearly being a response to Democratic electoral losses, with larger effects occurring in the year or two immediately after an election.

Figure A.3: Dynamic Effects Within Election Periods



Notes: This figure reports the RD estimates corresponding to equation (1) for whether there were any Black lynchings in a given county during the four-year election period following a presidential election $\tau \in \{1880, \dots, 1900\}$, conditional upon year period since the election (e.g., row 1 uses lynchings only within a year of the election). Estimates are based on a quadratic running polynomial and the MSE-optimal bandwidth from [Calonico et al. \(2014\)](#). All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. Standard errors are clustered at the county level. Error bars represent 90% confidence intervals. The outcome (control) means are 0.416 in row 1, 0.304 in row 2, 0.486 in row 3, and 0.486 in row 4.

Table A.1: Alternative Outcome Measures in Table 2

Dependent Variable:	Log Black Lynchings (1)	Log Black Lynchings (per 10,000) (2)	Any Black Lynchings (3)	Log White Lynchings (4)	Log White Lynchings (per 10,000) (5)	Any White Lynchings (6)
Democrat Lost in τ	0.079* (0.045)	0.341** (0.163)	0.027** (0.013)	-0.006 (0.015)	0.002 (0.019)	-0.001 (0.004)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes
Year – Election year FE	–	–	Yes	–	–	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	22.79	18.81	20.38	30.90	24.92	25.46
Control outcome mean	0.16	0.44	0.04	0.02	0.02	0.01
Observations	2,160	1,765	7,381	2,738	2,319	9,068

Notes: This table reports the RD estimates corresponding to equation (1) using alternative measures of the outcome. Columns 1 and 4 use logged counts of lynchings in a given county during the four-year election period following a presidential election $\tau \in \{1880, \dots, 1900\}$. Columns 2 and 5 use logged rates of lynchings (per 10,000 Blacks and whites, respectively) during the four-year election period following τ . Columns 3 and 6 allow for variation in the outcome by year, using indicators for whether there were any lynchings in a given county-year during the four-year election period following τ . Estimates are based on quadratic running polynomials and the MSE-optimal bandwidth from [Calonico et al. \(2014\)](#). All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. Columns 3 and 6 also include fixed effects for observation year period year minus most recent election year. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.3 Summary Statistics

In this section, we report summary statistics for our core sample in Appendix Table A.2. We restrict the set of observations to those within a bandwidth of 50 percentage points around the electoral zero threshold, such that summary statistics focus on relatively competitive places.

Table A.2: Summary Statistics

	Obs.	Mean	St. dev.	Min.	Max.
Outcome variables					
Any Black lynchings after election τ	4,048	0.14	0.35	0	1
Logged number of Black lynchings after election τ	4,048	0.16	0.43	0	3
Logged rate of Black lynchings after election τ (per 10,000)	3,950	0.49	1.27	0	7.50
Any White lynchings after election τ	4,048	0.02	0.15	0	1
Logged number of White lynchings after election τ	4,048	0.03	0.17	0	3.09
Logged rate of White lynchings after election τ (per 10,000)	3,973	0.03	0.22	0	3.76
Democratic victory in the 1904 election	4,048	0.84	0.37	0	1
Democratic victory in the 1908 election	4,048	0.79	0.40	0	1
Democratic victory in the 1912 election	4,048	0.95	0.22	0	1
Voter turnout rate in presidential elections, 1904–12	3,853	34.15	16.87	3.99	82.95
Black literacy rate in 1910	3,935	68.53	10.29	0	100
White literacy rate in 1910	3,974	90.74	6.49	55.63	99.71
Black school enrollment rate in 1910 (ages 6–14)	3,902	57.68	14.55	0	100
White school enrollment rate in 1910 (ages 6–14)	3,974	73.80	7.20	40.19	92.65
Non-white voter registration (1962–4)	2,056	19.23	12.26	0	95.12
White voter registration (1962–4)	2,054	46.50	13.17	3.19	90.94
Non-white voter registration (1966–7)	1,812	27.30	12.49	0	78.07
White voter registration (1966–7)	1,807	57.31	13.93	1.94	95.84
Explanatory variables					
Democratic loss in election τ	4,048	0.27	0.44	0	1
Democratic margin of loss in election τ	4,048	-13.30	23.08	-50	49.90
Any state Jim Crow voting laws	4,048	0.47	0.50	0	1
Any state multi-box or secret ballot laws	4,048	0.22	0.42	0	1
Any state literacy tests	4,048	0.02	0.13	0	1
Any state poll taxes	4,048	0.42	0.49	0	1
Controls					
County longitude	3,980	-86.42	6.91	-105.13	-75.65
County latitude	3,980	34.02	2.63	25.42	39.20
Logged population density (per sqr. meter) in 1880	3,973	-11.85	1.20	-20.57	-7.77
% Black population in 1880	3,973	33.93	22.51	0	91.90
% Former slaveholders as of 1880	3,947	10.63	5.01	0	28.11
% Confederate veterans as of 1880	3,947	44.54	9.32	0	83.33
Any Civil War battles fought in county	4,048	0.16	0.37	0	1
Average farm size (acres per number of farms)	3,909	252.56	1,118.18	6	60,000
Farm output per capita	3,920	39.31	18.39	0	171.43
Cotton potential index	3,980	0.64	0.20	0	1
Tobacco potential index	3,980	0.83	0.13	0	1
% Population ages 5-17	3,973	32.75	3.18	0	50
Manufacturing wages per capita	3,967	2.15	4.36	0	47.34
Manufacturing output per capita	3,967	14.92	28.13	0	456.74
Real estate property per capita	3,892	93.68	58.11	3.20	668.15
Personal property per capita	3,892	43.82	36.64	0	403.36
State taxes per capita	3,890	0.83	0.52	0.18	5.68
Local taxes per capita	3,890	0.28	0.86	0	9.27
Newspaper variables					
Frequency of Black crime accusations (% pages)	6,026	0.24	1.60	0	100
Frequency of Black rape accusations (% pages)	6,026	0.02	0.28	0	12.5
Frequency of Black murder accusations (% pages)	6,026	0.16	1.27	0	87.5
Frequency of Black robbert accusations (% pages)	6,026	0.06	0.85	0	50
Democratic newspaper	4,838	0.90	0.30	0	1

Notes: Table provides summary statistics for variables based on counties in the eleven former Confederate states and election years between 1880 and 1900, restricting to a bandwidth of 50 p.p. so to focus on relatively competitive county-election observations.

A.4 Newspaper Data

This final section of Appendix A considers variations of our newspaper measure, as described in Section 4, and provides examples of newspaper clippings identified by our script.

Table A.3: Alternative Word Measures in Table 9

Dependent Variable: Measure:	Frequency of Black Crime Accusations (% Pages in Newspaper)				
	Strict Measure, Main Version	Strict Measure, Version 2	Weak Measure, Version 1	Weak Measure, Version 2	Omitting “Negro(es)”
	(1)	(2)	(3)	(4)	(5)
Democrat Lost County in τ	0.069*** (0.022)	0.035*** (0.005)	8.730*** (1.477)	4.996*** (0.765)	-1.537 (1.059)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes
News year – Election year FE	Yes	Yes	Yes	Yes	Yes
City fixed effects	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	4.44	4.86	4.56	4.88	5.22
Control outcome mean	0.16	0.02	20.13	12.82	22.29
Observations	747	861	756	861	936

Notes: This table reports the RD estimates for the frequency of crime accusations in a given newspaper-year during the four-year election period following a presidential election $\tau \in \{1880, \dots, 1900\}$. Estimates are based on quadratic running polynomials and the MSE-optimal bandwidth from Calónico et al. (2014). Crime accusations based on accusations of rape, murder, or robbery in articles archived at newspapers.com. Column 2 excludes plural and past tense modifiers of our search terms, searching only for “negro rape,” “negro murder,” and “negro robbery.” Column 3 allows for words to co-occur anywhere on the same page. Column 4 does both of the above. Column 5 use a placebo measure that omits “negro(es)” from the search terms in our script and search more generally for “rape,” “murder,” and “robbery.” All regressions include fixed effects for election period, newspaper publication year minus most recent election year, and newspaper city or town. Additional spatial controls include quadratic polynomials in county longitude and latitude. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Varying Crime Accusations in Table 9

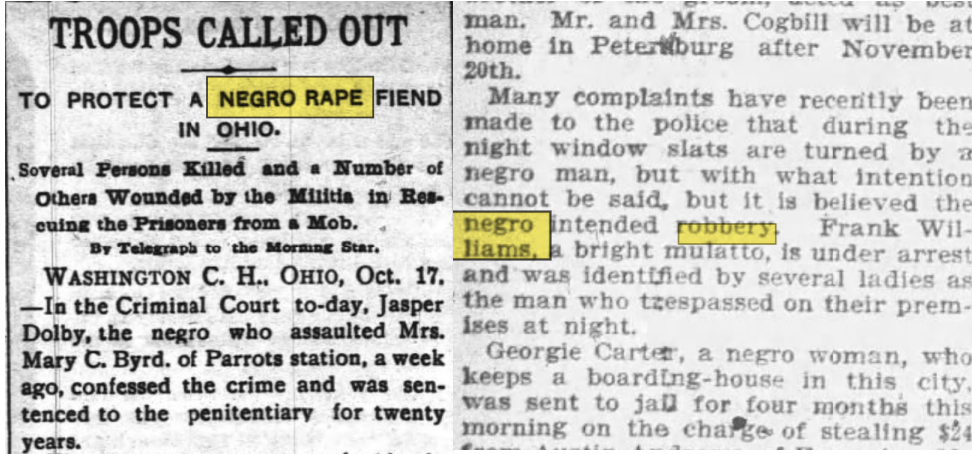
Dependent Variable:	Frequency of Black. . .					
	Rape Accusations		Murder Accusations		Robbery Accusations	
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Lost County in τ	0.045** (0.018)	0.035** (0.016)	0.109*** (0.028)	0.048*** (0.012)	0.070*** (0.016)	0.099*** (0.013)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
News year – Election year FE	Yes	Yes	Yes	Yes	Yes	Yes
City fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	No	Yes	No	Yes	No	Yes
Optimal bandwidth	11.53	6.57	9.62	5.02	11.22	8.28
Control outcome mean	0.02	0.02	0.11	0.10	0.06	0.04
Observations	2,080	1,258	1,761	915	2,017	1,542

Notes: This table reports the RD estimates for the frequency of Black crime accusations in a given newspaper-year during the four-year election period following a presidential election $\tau \in \{1880, \dots, 1900\}$. Estimates are based on quadratic running polynomials and the MSE-optimal bandwidth from Calónico et al. (2014). Black crime accusations based on accusations of either rape, murder, or robbery in articles archived at newspapers.com. All regressions include fixed effects for election period, newspaper publication year minus most recent election year, and newspaper city or town. Additional spatial controls in even columns include quadratic polynomials in county longitude and latitude. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.4: Examples of Newspaper Data

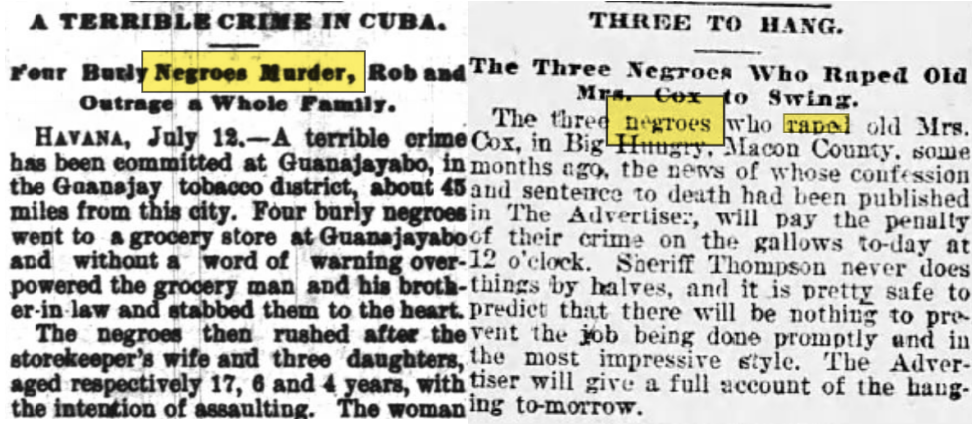
(a) “Negro rape”

(b) “Negro robbery”



(c) “Negroes murder”

(d) “Negroes raped”



Notes: Examples of newspaper data generated by our keywords. Panel (a) shows select output for the search “negro rape,” as featured in the fourth page of the *Wilmington Morning Star* on October 18, 1894. Panel (b) shows select output for the search “negro robbery,” as featured in the sixth page of the *Virginia-Pilot* on November 13, 1901. Panel (c) shows select output for the search “negroes murder,” as featured in the first page of the *News and Observer* on July 13, 1895. Panel (d) shows select output for the search “negroes raped,” as featured in the second page of the *Montgomery Advertiser* on May 26, 1893. Clippings screencapped from newspapers.com.

B Identification and RD Appendix

To identify the causal effects of Democratic electoral losses on local lynching activity, we adopt a regression discontinuity (RD) design, based on the identifying assumption that counties in which Democrats barely lost are similar in all relevant ways to those in which Democrats barely won. Reiterating equation (1),

$$\text{Any Lynching}_{c(s)\tau} = \beta \cdot \text{Democratic Loss}_{c\tau} + f(\text{Loss Margin}_{c\tau}) + \phi_t + \theta_s + \mathbf{X}'_{c\tau} \Gamma + \varepsilon_{c\tau},$$

recall that $\text{Democratic Loss}_{c\tau}$ captures whether the Democratic candidate for president lost the popular vote in county c in a given election year τ . By interacting $\text{Democratic Loss}_{c\tau}$ with a running variable for the Democratic loss margin, $f(\text{Loss Margin}_{c\tau})$, we estimate treatment effects based on counties with very close election outcomes in a given election year.

In this Appendix, we provide additional details on this estimating strategy. First, we describe the process of estimating this RD specification, including assumptions made therein. Second, we highlight additional evidence in support of our identifying assumptions. Finally, we report estimates from several additional heterogeneous effects exercises cited in the text.

RD Specification. Our baseline RD specification adopts the data-driven approach from [Calonico et al. \(2014\)](#), whose `rdrobust` package in Stata computes and automatically selects the MSE-optimal bandwidth under which local randomization is likely to be satisfied given the data. As such, this bandwidth may vary by outcome variable and other aspects of the specification, such as the running variable polynomial.

We adopt a quadratic running polynomial for our main analysis, while also reporting estimates of our main results using linear and other polynomial choices in [Table 4](#). We furthermore adopt a triangular kernel, which places greater weight on observations close to the electoral zero threshold, $Loss\ Margin_{c\tau} = 0$. Relative to our baseline estimate of .110 (.046) in row 1 of [Table 4](#), instead adopting a uniform kernel results in a slightly smaller estimate of .081 (.041). We do not adjust for mass points in the running variable in our county-level analysis. Relative to our baseline estimate of .110 (.046) in row 1 of [Table 4](#), adjusting for mass points produces an estimate of .107 (.045). We do adjust for mass points as robustness in [Appendix Table B.6](#) below, which uses county-level explanatory variation to predict changes in newspaper content within cities over time, rendering mass points a more salient concern.

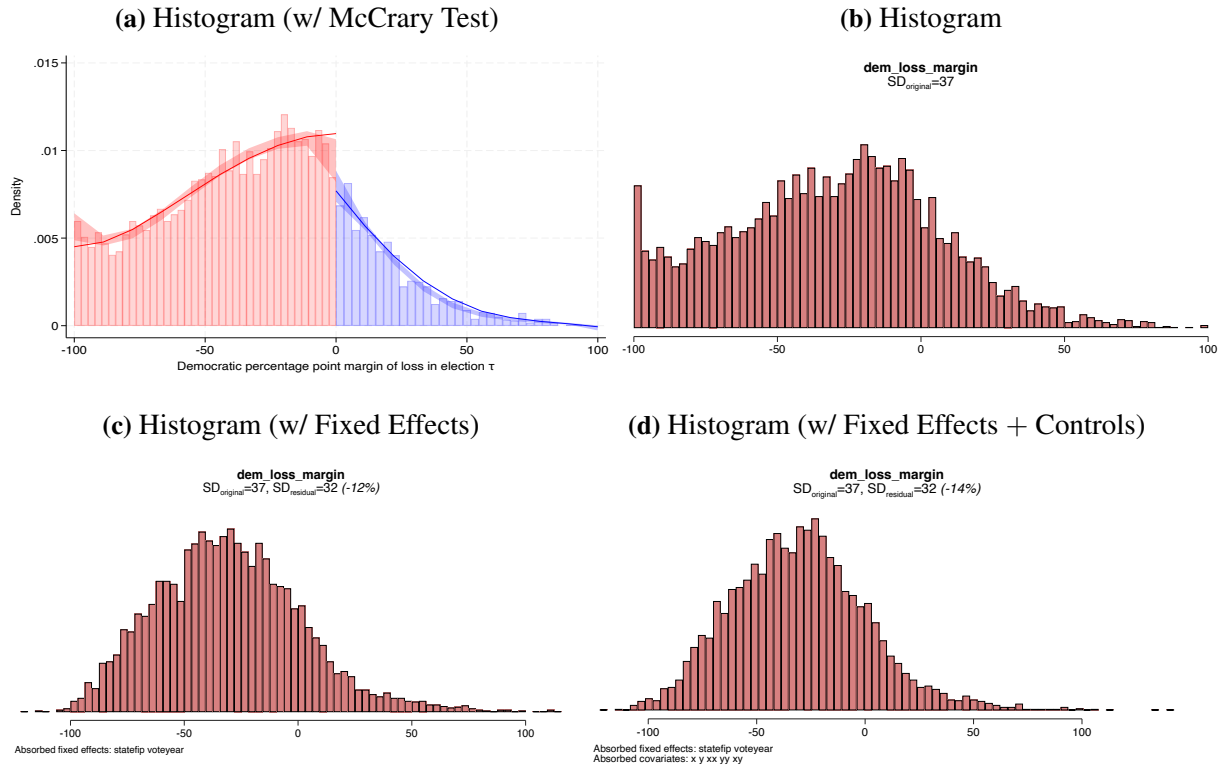
Verifying RD Assumptions. Our empirical strategy faces a number of challenges. Of central importance is the assumption that relevant factors besides the outcome are continuous around the electoral zero threshold, $Loss\ Margin_{c\tau} = 0$. If they are not, then estimates may reflect discontinuities in other factors besides Democratic Party losses. To test the assumption that close elections are in fact occurring in otherwise similar places, we first examine the distribution of the running variable around the zero threshold. Insofar as electoral outcomes were potentially manipulable in the post-Reconstruction South, such selection could generate differences between treatment and control counties. For instance, if the set of local Democratic elites successfully manipulated local election returns in their favor, resulting in a narrow *win* in a given county, a lynching may not have occurred where it otherwise would have, likely attenuating the treatment effect.

Using the formal test based on [McCrary \(2008\)](#), we fail to reject the null hypothesis that $Loss\ Margin_{c\tau}$ is continuous at the zero threshold. For instance, when the optimal bandwidth is computed relatively liberally, based on the mean squared error of the sum of densities ($|Loss\ Margin_{c\tau}| < 44.45$), the p -value is .123 (see [Appendix Figure B.1](#), panel a). Meanwhile, $p = .150$ when the optimal bandwidth is more conservatively selected, using the optimal bandwidth ($|Loss\ Margin_{c\tau}| < 19.91$) from our main analysis in row 1 of [Table 4](#). Strikingly, when we use only the subsample of counties that had been uncompetitive in election $\tau - 1$ from

panel (b) of Table 2, these p -values become much larger: .781 and .808, respectively.

At the same time, it is worth noting that such standard manipulation tests for RD designs do not accommodate the fixed effects or controls, nor the clustering of standard errors, featured in our empirical model. As illustration, note in Appendix Figure B.1 (panels b–d) how the distribution of the running variation appears even smoother once variance from fixed effects or controls are absorbed, based on the `fedistr` package in Stata. Meanwhile, the balance test results in Table 1 fail, using equation (1), to estimate statistically significant differences at the threshold across a large set of pre-treatment outcomes, conditional on the baseline set of fixed effects and controls. This constitutes additional, strong evidence against endogenous sorting of electoral outcomes around the zero threshold in our context. Appendix Table B.1 shows that this remains the case if we instead estimate a local linear specification. Finally, further reaffirming our identifying assumptions, our main results are minimally changed when we include all of these factors as flexible controls in our RD analysis in Table 4.

Figure B.1: Conditional Distributions of Democratic Loss Margins



Notes: Figure (panel a) illustrates the density test from [Cattaneo et al. \(2018\)](#) following [McCrary \(2008\)](#), using the Democratic margin of loss across presidential elections $\tau \in \{1880, \dots, 1900\}$ in our full sample ($p = .123$). Error bars represent 90% confidence intervals. Figure further shows histograms (panels b–d) of observations over the Democratic margin of loss across presidential elections $\tau \in \{1880, \dots, 1900\}$. Panels (a) and (b) rely on all unconditioned sample variation; panel (c) replicates panel (b) after absorbing variance from the election period and state fixed effects in our empirical model; and panel (d) replicates panel (b) the same figures after absorbing variance from both fixed effects and quadratic polynomials in county longitude and latitude.

Table B.1: Local Linear Comparisons in Table 1

Dependent Variable:	Log Population Density (1)	% Black Population (2)	% Former Slaveholders (3)	% Confederate Veterans (4)	Any Civil War Battles (5)	Average Farm Size (6)	Cotton Potential (7)	Tobacco Potential (8)
Democrat Lost County in Election τ	0.056 (0.069)	2.412 (1.981)	0.212 (0.484)	-0.583 (0.606)	0.038 (0.057)	67.807 (70.654)	-0.007 (0.018)	-0.006 (0.005)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	24.83	17.70	20.86	23.71	12.15	20.94	21.29	24.75
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Control outcome mean	-11.79	34.82	10.98	45.00	0.19	217.82	0.63	0.83
Observations	2,313	1,686	1,967	2,221	1,184	1,960	2,020	2,309

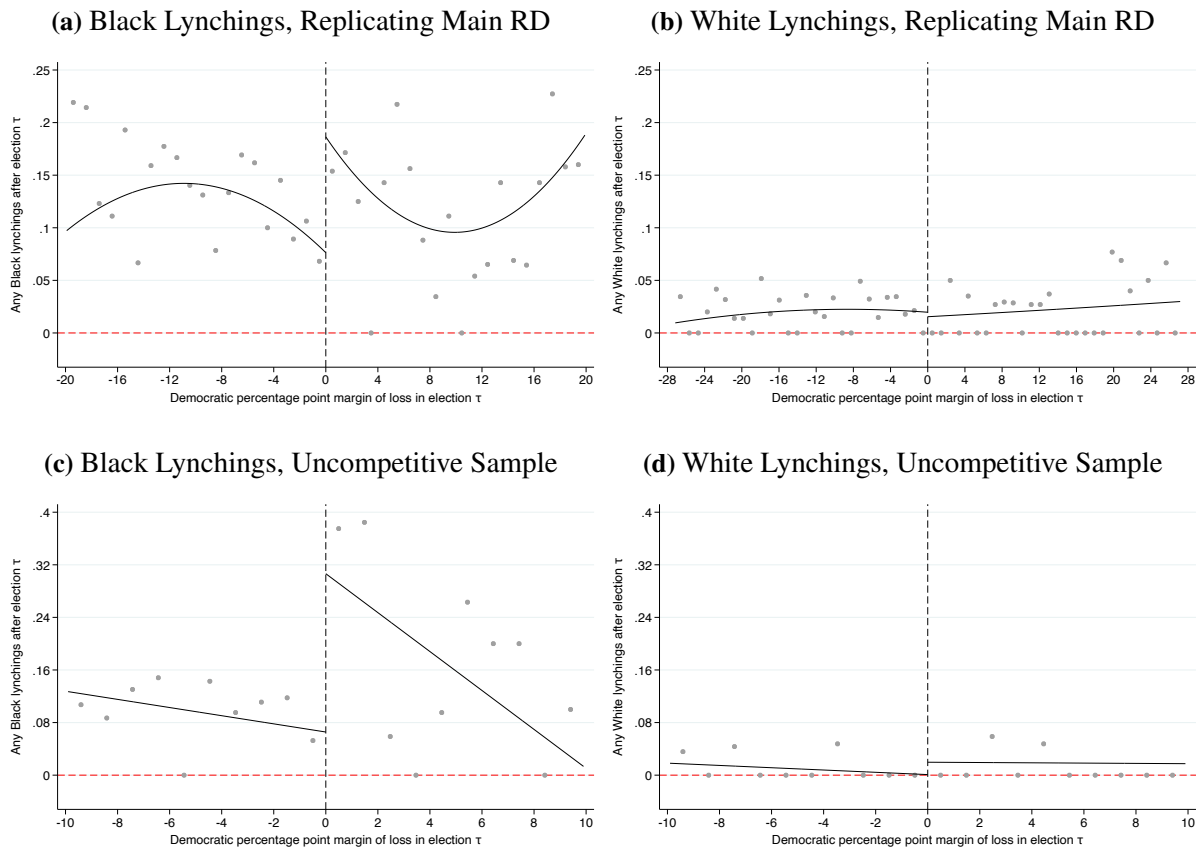
Dependent Variable:	Percent Aged 5–17 (9)	Manufacturing Wages per Capita (10)	Manufacturing Output per Capita (11)	Agricultural Output per Capita (12)	Real Estate per Capita (13)	Personal Property per Capita (14)	State Taxes per Capita (15)	Local Taxes per Capita (16)
Democrat Lost County in Election τ	-0.021 (0.220)	0.183 (0.452)	0.303 (2.437)	0.751 (1.660)	-2.486 (5.222)	-2.883 (2.505)	-0.008 (0.036)	0.027 (0.103)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	18.18	20.13	25.98	18.66	17.82	22.95	18.20	16.40
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Control outcome mean	32.88	2.28	15.44	38.06	93.90	44.66	0.84	0.29
Observations	1,720	1,895	2,393	1,749	1,678	2,147	1,697	1,564

Notes: This table reports the RD estimates corresponding to equation (1) for various pre-treatment county-level characteristics. See Section 3.1 for more details on variables. Estimates are based on linear running polynomials and the MSE-optimal bandwidth from Calonico et al. (2014). All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.1 Robustness and Heterogeneity in Section 3

This section of Appendix B reports alternative versions and extensions of our main results, including Figure 5 and Table 2. For the former, Appendix Figure B.2 presents alternative versions of our main RD plots, using (i) the RD specifications and bandwidths from panel (a) of Table 2 and (ii) the restricted sample from panel (b) of Table 2.

Figure B.2: Replicating Figure 5 with Alternative Specifications and Samples



Notes: Binned estimates of the probability of Black and white lynchings during the four-year election period following a presidential election $\tau \in \{1880, \dots, 1900\}$ by the Democratic margin of victory in τ . Negative values on the x -axis indicate the Democratic candidate won a given county, while positive values indicate that they lost. All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. Panels (a) and (b) adopt the RD specification (including optimal bandwidths) from the preferred specifications in panel (a) of Table 2 (columns b and d, respectively). Panels (c) and (d) adopt the restricted sample of counties that were relatively uncompetitive in the previous election $\tau - 1$ from panel (b) of Table 2.

Table B.2: Heterogeneous Effects: RD Estimates Conditional on Black Population Size

Dependent Variable:	Any Lynchings of Black People After Election τ			
	Above-Median Black		Below-Median Black	
	(1)	(2)	(3)	(4)
	(a) Full Sample			
Democrat Lost County in Election τ	0.134** (0.061)	0.149** (0.063)	-0.044 (0.043)	-0.011 (0.047)
Election period fixed effects	Yes	Yes	Yes	Yes
Spatial covariates	No	Yes	No	Yes
Optimal bandwidth	22.06	19.54	26.16	22.77
Control outcome mean	0.17	0.17	0.07	0.02
Observations	1,414	1,256	781	692
	(b) Uncompetitive Counties in Election $\tau - 1$ Only			
Democrat Lost County in Election τ	0.263* (0.136)	0.316** (0.133)	0.053 (0.085)	0.065 (0.090)
Election period fixed effects	Yes	Yes	Yes	Yes
Spatial covariates	No	Yes	No	Yes
Optimal bandwidth	20.35	19.42	32.03	30.57
Control outcome mean	0.19	0.18	0.07	0.03
Observations	573	538	555	520

Notes: This table reports RD estimates for whether there were any Black lynchings in a given county during the four-year election period following a election $\tau \in \{1880, \dots, 1900\}$, conditional on whether it had above-sample median 1880 Black population shares. Counties in panel (a) include those in the former Confederate states. Panel (b) restricts the sample to counties that were electorally uncompetitive in $\tau - 1$, using the median vote margin among sample Democratic electoral losses ($|Loss\ Margin_{c\tau}| = 15.90$) as the cutoff. Estimates are based on quadratic running polynomials and the MSE-optimal bandwidth from [Calonico et al. \(2014\)](#). All regressions include election period fixed effects, while columns 2 and 4 enter set of spatial covariates that includes state fixed effects and quadratic polynomials for county longitude and latitude. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.2 Robustness and Heterogeneity in Section 4

Table B.3: Correlating Lynchings with Outcomes in Table 5

Dependent Variable:	Democrat Won in . . .						Voter Turnout in	
	1904		1908		1912		1904–12	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any Black Lynchings, 1880–1903	0.104*** (0.019)	0.169*** (0.028)	0.118*** (0.022)	0.173*** (0.031)	0.036*** (0.013)	0.037** (0.017)	-5.403*** (0.625)	-5.637*** (0.817)
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Within optimal bandwidth only?	No	Yes	No	Yes	No	Yes	No	Yes
Control outcome mean	0.88	0.83	0.84	0.78	0.95	0.95	31.98	33.17
Observations	1,278	745	1,278	745	1,278	745	1,218	708
Adj. R ²	0.18	0.24	0.20	0.28	0.11	0.06	0.68	0.67

Notes: This table reports the OLS estimates for regressions of whether there was a Democratic electoral victory in a given county in the 1904 (columns 1–2), 1908 (columns 3–4), and 1912 (column 5–6) presidential elections, as well as the average voter turnout rate in a county across the 1904–12 elections (column 7–8), on whether a county experienced at least one Black lynching across the 1880–1900 electoral periods. Even columns restrict to sample counties within the optimal bandwidth ($|Loss\ Margin_{c\tau}| < 19.91$) from our main analysis in row 1 of Table 4. All regressions include election state fixed effects, and quadratic polynomials in county longitude and latitude. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: Re-estimating Table 5 Using Counties with Similar Black Shares in 1880

Dependent Variable:	Democrat Won in...				Voter Turnout in			
	1904		1908		1912		1904–12	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(a) Only Counties with Black Lynchings...								
<i>Any Lynchings Through the Election Period After τ:</i>								
Democrat Lost County in Election τ	0.248***		0.299***		0.033		-4.490**	
	(0.067)		(0.084)		(0.036)		(2.225)	
<i>Any Lynchings During the Election Period After τ:</i>								
Democrat Lost County in Election τ		0.223**		0.407***		0.023		-5.423*
		(0.101)		(0.110)		(0.018)		(3.164)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	16.95	17.15	19.56	17.26	19.64	14.54	21.36	25.42
Control outcome mean	0.93	0.93	0.88	0.90	0.99	1.00	30.89	29.59
Observations	457	200	543	200	547	167	568	303
(b) Only Counties with No Black Lynchings...								
<i>No Lynchings Through the Election Period After τ:</i>								
Democrat Lost County in Election τ	-0.010		-0.222*		0.031		-0.596	
	(0.099)		(0.134)		(0.024)		(3.107)	
<i>No Lynchings During the Election Period After τ:</i>								
Democrat Lost County in Election τ		0.034		-0.011		0.052*		-1.944
		(0.081)		(0.093)		(0.030)		(3.420)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	29.02	35.18	31.58	32.76	28.94	30.03	23.22	28.65
Control outcome mean	0.89	0.94	0.85	0.92	0.99	0.99	33.14	29.85
Observations	287	306	297	288	286	274	237	250

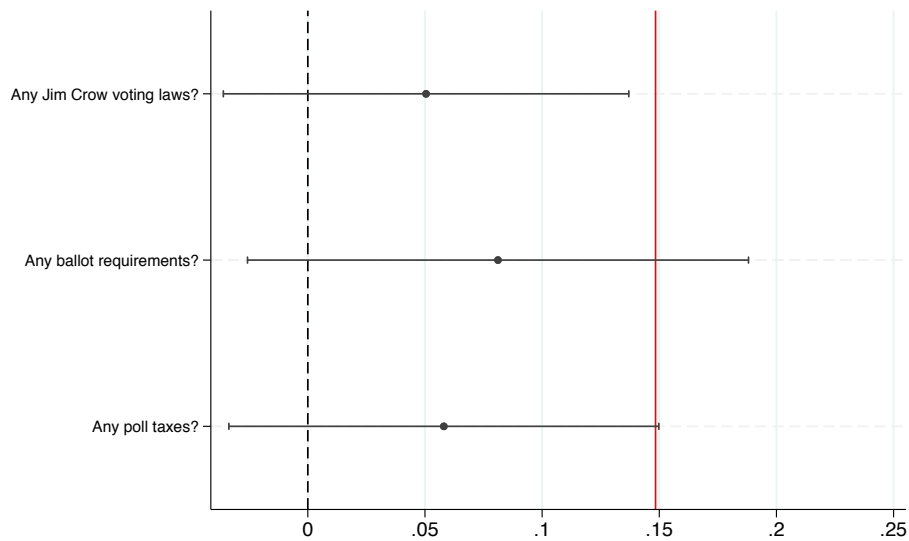
Notes: This table reports the RD estimates corresponding to equation (1) for whether there was a Democratic electoral victory in a given county in the 1904 (columns 1–2), 1908 (columns 3–4), and 1912 (column 5–6) presidential elections, as well as the average voter turnout rate in a county across the 1904–12 elections (column 7–8). Regressions in panel (a) restrict to counties in which a Black lynching occurred at some point either through the four-year period following election τ (odd columns) or strictly during the period following τ (even columns), while regressions in panel (b) restrict to the complementary cases without Black lynchings. Both panels restrict to a sample of counties matched on Black population shares in 1880, as described in Section 4. Estimates are based on quadratic running polynomials and the MSE-optimal bandwidth from Calonico et al. (2014). All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5: Black Outmigration by Narrow Democratic Losses, 1880–1900

Dependent Variable:	$\Delta\%$ Black Population, 1880–1910				Δ Blacks, 1880–1910 (% Population)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(a) Only Counties with Black Lynchings...								
<i>Any Lynchings Through the Election Period After τ:</i>								
Democrat Lost County in Election τ	-4.082***		-4.082***		-5.615**		-5.615**	
	(1.533)		(1.535)		(2.540)		(2.542)	
<i>Any Lynchings During the Election Period After τ:</i>								
Democrat Lost County in Election τ		-3.285		-3.285		-8.243*		-8.243*
		(2.719)		(2.727)		(4.236)		(4.259)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched sample?	No	No	Yes	Yes	No	No	Yes	Yes
Optimal bandwidth	28.67	20.81	28.67	20.81	26.38	18.61	26.38	18.61
Control outcome mean	-1.83	-2.40	-1.83	-2.40	11.27	11.68	11.27	11.68
Observations	822	256	822	256	767	222	767	222
(b) Only Counties with No Black Lynchings...								
<i>No Lynchings Through the Election Period After τ:</i>								
Democrat Lost County in Election τ	0.955		-0.508		-0.778		2.006	
	(0.861)		(2.537)		(1.336)		(4.671)	
<i>No Lynchings During the Election Period After τ:</i>								
Democrat Lost County in Election τ		-0.058		-2.675		-1.761		4.278
		(0.785)		(2.118)		(1.225)		(3.641)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched sample?	No	No	Yes	Yes	No	No	Yes	Yes
Optimal bandwidth	23.43	25.81	23.43	25.81	29.39	33.20	29.39	33.20
Control outcome mean	-3.00	-2.79	-2.42	-1.49	6.84	7.83	10.37	12.55
Observations	1,532	2,062	239	248	1,788	2,510	288	290

Notes: This table reports the RD estimates corresponding to equation (1) for the percentage point change in Black population shares from 1880 to 1910 (columns 1–4) and the number of new Black residents between 1880 and 1910 as a share of 1910 population (columns 5–8). Regressions in panel (a) restrict to counties in which a Black lynching occurred at some point either through the four-year period following election τ (odd columns) or strictly during the period following τ (even columns), while regressions in panel (b) restrict to the complementary cases without Black lynchings. Columns 3–4 and 7–8 restrict to a sample of counties matched on Black population shares in 1880, as described in Section 4. Estimates are based on quadratic running polynomials and the MSE-optimal bandwidth from Calonico et al. (2014). All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure B.3: Comparing Main RD Estimates Based on Jim Crow Voting Laws



Notes: This figure reports the RD estimates corresponding to equation (1) for whether there were any Black lynchings in a given county during the four-year election period following a presidential election $\tau \in \{1880, \dots, 1900\}$. Regressions are conditional upon whether a given county's state had yet introduced any Jim Crow voting laws as of τ . Compare estimates to the pre-Jim Crow benchmark from column 1 of Table 8 (shown in solid red). See that table's notes for more details. Standard errors are clustered at the county level. Error bars represent 90% confidence intervals.

B.3 RD Robustness for Newspaper Analysis

Table B.6: Robustness Exercises for Table 9

Dependent Variable:	Frequency of Black Crime Accusations (% Pages in Newspaper)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democrat Lost County in τ	0.069*** (0.022)	0.125*** (0.043)	0.209*** (0.040)	0.047** (0.020)	0.088*** (0.008)	0.124*** (0.028)	0.146*** (0.029)	0.088*** (0.018)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
News year – Election year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Newspaper fixed effects	No	No	No	Yes	No	No	No	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Table 1 controls	No	No	Yes	No	No	No	Yes	No
Adjust for mass points?	No	Yes	No	No	No	Yes	No	No
Optimal bandwidth	4.44	15.58	10.40	5.70	3.74	11.11	7.95	4.84
Polynomial	Quadratic	Quadratic	Quadratic	Quadratc	Linear	Linear	Linear	Linear
Control outcome mean	0.16	0.19	0.20	0.16	0.15	0.20	0.16	0.16
Observations	747	2,631	1,857	1,049	614	1,979	1,512	861

Notes: This table reports the RD estimates for the frequency of Black crime accusations in a given newspaper-year during the four-year election period following a presidential election $\tau \in \{1880, \dots, 1900\}$. Estimates are based on quadratic (columns 1–4) or linear (columns 5–8) running polynomials and the MSE-optimal bandwidth from Calonic et al. (2014). Black crime accusations based on accusations of rape, murder, or robbery in articles archived at newspapers.com. All regressions include fixed effects presidential for election period, newspaper publication year minus most recent election year, and newspaper city or town. Columns 4 and 8 additionally include newspaper fixed effects. Additional spatial controls include quadratic polynomials in county longitude and latitude. Columns 3 and 7 also control for quadratic polynomials of all variables from Table 1. Columns 2 and 6 re-estimate effects after adjusting for mass points in the county-level explanatory variation. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Theory Appendix

Proposition 1. *For a weak-type elite, the support premium associated with occurrence of electoral victory, ΔS_{t+1} , is such that:*

- (i) $\Delta S_{t+1} > 0$ in the absence of any violence following either electoral outcome, $p_t(\omega_t = 1) = p_t(\omega_t = 0) = 0$.
- (ii) ΔS_{t+1} is decreasing in $p_t(\omega_t = 0)$ and increasing in $p_t(\omega_t = 1)$.
- (iii) $p_t(\omega_t = 0) > p_t(\omega_t = 1)$ is a necessary condition for $\Delta S_{t+1} < 0$.

Proof. Under a uniform distribution, the aggregate level of acquiescence to the elite by minority citizens is $S_{t+1}^*(p_t|\omega_t = 0) = p_t$ and $S_{t+1}^*(p_t|\omega_t = 1) = \frac{\pi(S_t, \underline{S})\theta_t + h(1 - \pi(S_t, \underline{S}))p_t}{h + (1-h)\pi(S_t, \underline{S})}$ following an electoral loss and win, respectively.

We can define:

$$\begin{aligned} \Delta S_{t+1} &= \frac{\pi(S_t, \underline{S})\theta_t + h(1 - \pi(S_t, \underline{S}))p_t(\omega_t = 1)}{\pi(S_t, \underline{S})\theta_t + h(1 - \pi(S_t, \underline{S}))} - p_t(\omega_t = 0) \\ &= \frac{\pi(S_t, \underline{S})\theta_t(1 - p_t(\omega_t = 0)) + h(1 - \pi(S_t, \underline{S}))[p_t(\omega_t = 1) - p_t(\omega_t = 0)]}{\pi(S_t, \underline{S})\theta_t + h(1 - \pi(S_t, \underline{S}))}, \end{aligned}$$

which is clearly positive if $p_t(\omega_t = 1) = p_t(\omega_t = 0) = 0$, strictly decreasing in $p_t(\omega_t = 0)$, and strictly increasing in $p_t(\omega_t = 1)$.

Finally, $\Delta S_{t+1} < 0$ if and only if

$$\pi(S_t, \underline{S})\theta_t(1 - p_t(\omega_t = 0)) < h(1 - \pi(S_t, \underline{S}))[p_t(\omega_t = 0) - p_t(\omega_t = 1)],$$

for which $p_t(\omega_t = 0) > p_t(\omega_t = 1)$ is a necessary condition. □

Proposition 2. *There exists a unique perfect Bayesian equilibrium $(p_t^*, \theta_t^*, (s_{it}^*)_{i \in [0,1]}, S_{t+1}^*)$. In equilibrium, a weak-type elite always uses violence to suppress minority opposition, $p_t^*(\omega_t) \in (0, 1)$, where elite investment in violence is greater, and violence against oppositional minorities in turn more probable, following an electoral defeat, $p_t^*(0) > p_t^*(1)$, all else fixed.*

Proof. The first-order necessary condition of equation (3) for the existence of $p_t^*(\omega = 1)$ is

$$-\frac{\partial \phi(p_t^*(\omega = 1))}{\partial p_t} + \frac{\partial \pi(S_{t+1}^*(p_t), \underline{S})}{\partial S_{t+1}} \Bigg|_{p_t=p_t^*(\omega_t=1)} \frac{h(1 - \pi(S_t, \underline{S}))}{h + (1-h)\pi(S_t, \underline{S})} \Delta V = 0, \quad (\text{C.1})$$

whereas the first-order condition of equation (4) for the existence of $p_t^*(\omega = 0)$ is

$$-\frac{\partial \phi(p_t^*(\omega = 0))}{\partial p_t} + \frac{\partial \pi(S_{t+1}^*(p_t), \underline{S})}{\partial S_{t+1}} \Bigg|_{p_t=p_t^*(\omega_t=0)} \Delta V = 0. \quad (\text{C.2})$$

In both cases, we can rule out $p_t^*(\omega) = 0$ given that $c'(0) = 0$, $\pi' > 0$, $h \in (0, 1)$, $\pi(S_t, \underline{S}) \in (0, 1)$, and $\Delta V > 0$. We can furthermore rule out $p_t^*(\omega) = 1$ given Assumption 1.

The rest of the proof relates to the claim that $p_t^*(\omega_t = 0) > p_t^*(\omega_t = 1)$. Suppose instead that $p_t^*(\omega_t = 0) < p_t^*(\omega_t = 1)$. Then it must be the case that $\frac{\partial \phi(p_t^*(\omega_t=1))}{\partial p_t} > \frac{\partial \phi(p_t^*(\omega_t=0))}{\partial p_t}$ following from $\phi'(p_t)|_{p_t>0} > 0$. This would imply together with equations (C.1) and (C.2) that

$$\frac{\partial \pi(S_{t+1}^*(p_t), \underline{S})}{\partial S_{t+1}} \Big|_{p_t=p_t^*(\omega_t=0)} < \frac{\partial \pi(S_{t+1}^*(p_t), \underline{S})}{\partial S_{t+1}} \Big|_{p_t=p_t^*(\omega_t=1)} \frac{h(1 - \pi(S_t, \underline{S}))}{h + (1 - h)\pi(S_t, \underline{S})}. \quad (\text{C.3})$$

However, recall that $S_{t+1}^*(p_t^*(\omega_t = 0)) = p_t^*(\omega_t = 0)$ and $S_{t+1}^*(p_t^*(\omega_t = 1)) = \frac{\pi(S_t, \underline{S}) + h(1 - \pi(S_t, \underline{S}))p_t^*(\omega_t=1)}{\pi(S_t, \underline{S}) + h(1 - \pi(S_t, \underline{S}))}$. Under $p_t^*(\omega_t = 0) < p_t^*(\omega_t = 1)$, $S_{t+1}^*(p_t^*(\omega_t = 0)) < S_{t+1}^*(p_t^*(\omega_t = 1))$ if $p_t^*(\omega_t = 1) < S_{t+1}^*(p_t^*(\omega_t = 1))$, which is indeed true for all $p_t^*(\omega_t = 1) \in (0, 1)$. Hence, if $p_t^*(\omega_t = 0) < p_t^*(\omega_t = 1)$, then $S_{t+1}^*(p_t^*(\omega_t = 0)) < S_{t+1}^*(p_t^*(\omega_t = 1))$.

Hence, if $p_t^*(\omega_t = 0) < p_t^*(\omega_t = 1)$, then

$$\frac{\partial \pi(S_{t+1}^*(p_t), \underline{S})}{\partial S_{t+1}} \Big|_{p_t=p_t^*(\omega_t=0)} \geq \frac{\partial \pi(S_{t+1}^*(p_t), \underline{S})}{\partial S_{t+1}} \Big|_{p_t=p_t^*(\omega_t=1)}$$

following from $\pi'' \leq 0$. Moreover, the remainder of the right-hand side of condition (C.3), derived using Bayes' theorem, is a probability strictly less than 1. Hence, it cannot be the case that condition (C.3) holds. Hence, $p_t^*(\omega_t = 0) > p_t^*(\omega_t = 1)$ by contradiction. \square

Proposition 3. *The level of investment in violence by a weak-type elite after an electoral loss, $p^*(\omega_t = 0)$, is weakly increasing in the baseline level of support, \underline{S} .*

Proof. Implicit differentiation involving equation (C.2) with respect to \underline{S} yields:

$$-\frac{\partial^2 \phi}{\partial p_t^2} \frac{\partial p_t^*(0)}{\partial \underline{S}} + \left[\frac{\partial^2 \pi(S_{t+1}^*(p_t^*(0)), \underline{S})}{\partial S_{t+1}^2} \frac{\partial S_{t+1}^*(p_t^*(0))}{\partial p_t} \frac{\partial p_t^*(0)}{\partial \underline{S}} + \frac{\partial^2 \pi(S_{t+1}^*(p_t^*(0)), \underline{S})}{\partial S_{t+1} \partial \underline{S}} \right] \Delta V = 0.$$

Rearranging and solving for $\frac{\partial p_t^*(0)}{\partial \underline{S}}$ yields:

$$\frac{\partial p_t^*(0)}{\partial \underline{S}} = \frac{\frac{\partial^2 \pi(S_{t+1}^*(p_t^*(0)), \underline{S})}{\partial S_{t+1} \partial \underline{S}} \Delta V}{\frac{\partial^2 \phi}{\partial p_t^2} - \frac{\partial^2 \pi(S_{t+1}^*(p_t^*(0)), \underline{S})}{\partial S_{t+1}^2} \frac{\partial S_{t+1}^*(p_t^*(0))}{\partial p_t}} \Delta V \geq 0$$

following from $\pi'' \leq 0$ and $\pi''_{S_t \underline{S}} \geq 0$. \square