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

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Abstract

Election results act as powerful signals, shaping social behavior in ways that can be dramatic and even violent. This paper shows how racial violence in the post-Reconstruction U.S. South was tied to the local performance of the anti-Black Democratic Party in presidential elections. Using a regression discontinuity design based on close presidential vote shares, we find that Southern counties where Democrats lost the popular vote between 1880 and 1900 were nearly twice as likely to experience Black lynchings in the following four years. This backlash was enkindled by local elites, who amplified narratives of Black criminality through newspapers after such defeats. These findings point to the strategic use of racial violence by Democratic elites, prefiguring the formal vote suppression of Jim Crow.

Keywords: racial violence, Black political and economic outcomes, U.S. South, close elections, regression discontinuity, backlash, information, media, elites.

JEL Codes: N31, P16, D72, J15, I31, O10, D83.

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"[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 4,100 lynchings were carried out across the country between 1882 and 1932, with around 75% of those targeting Black people and 76% 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 present evidence that political factors systematically shaped the dynamics of lynching activity across the South. After Reconstruction ended in 1877, a resurgent anti-Black Democratic Party initially faced local competition from groups seeking to build multiracial coalitions throughout the region. We show that in counties where Democrats were outperformed in presidential elections, lynchings of Black people surged. This pattern suggests that the results of competitive elections had a powerful effect on social behavior in the pre-Jim Crow period, extending beyond their direct impacts on officeholding and policy.

Using a regression discontinuity (RD) design based on close presidential vote shares in counties from 1880 to 1900, we estimate the effect of a local Democratic "defeat" on lynching. Our results indicate that a (narrow) Democratic loss increased the probability of a Black lynching in a county by about 10 percentage points over the next four years—an 80% rise relative to the sample mean—while having no comparable effect on white lynchings. These findings are robust to (i) alternative running polynomials, (ii) varying the MSE-optimal bandwidth, (iii) incorporating flexible controls for county demographic and spatial characteristics, (iv) accounting for contemporaneous economic shocks and historical factors related to slavery,

[1] As 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).

[2] This 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.

and (v) omitting individual states and periods. Meanwhile, we find no similar discontinuities under alternative RD thresholds, underscoring the significance of the "win-lose" threshold.

Our RD estimates imply that presidential election results in a county can have dramatic effects, with narrow Democratic losses leading to large increases in racial violence. This is puzzling on its face, as a party's performance in a particular county does not determine who wins the presidency or sets policy. In fact, between *close* losses and wins, there is virtually no difference: we find no evidence of endogenous sorting among counties around the threshold. To explain this, we develop a conceptual framework, proposing two key factors driving the salience of presidential election results at the county level. First, recent vote shares signal the relative strengths of different political groups. When local actors lack complete information about the political environment, even a narrow (Democratic) loss can serve as a focal point, potentially facilitating mobilization among (pro-Black) opposition (Anagol and Fujiwara, 2016; Granzier et al., 2023). Second, local (Democratic) elites have an incentive to foment a backlash in anticipation of such mobilization (Glaeser, 2005).

We document a wide and varied set of evidence in support of this explanation. First, we show that effects are larger in counties where Democratic losses in presidential elections followed more comfortable Democratic wins, compared to small and insignificant effects in places with previous elections characterized by Democratic defeat or that were otherwise close. Such heterogeneous effects suggest that our results are driven, not by close Democratic losses in perennially-competitive places, but rather by relatively unexpected losses, which served to *reveal* novel Democratic weakness in the face of relatively pro-Black political opposition. Meanwhile, effects are attenuated among Democratic losses in congressional elections, which actually put a member of the opposition in power, thus potentially combatting lynching.

Second, we explore the role of local elites in galvanizing racial violence. We show that Southern newspapers, which often had ties to the Democratic Party, tended to spotlight stories about Black-committed crime (e.g., rape) in the aftermath of Democratic losses. Such accusations, which became pervasive in the post-Reconstruction period, were frequently invoked by lynch mobs and are positively associated with Black lynchings in our data. We also consider the conditions for elite influence. The effects of Democratic losses on anti-Black accusations, together with our core lynching results, are driven entirely by counties with an all-white, Democratic elite, nonetheless facing a relatively large Black electorate. Strikingly, these effects go away with the (staggered) introduction of state laws formally disenfranchising Black voters—consistent with the strategic use of racial violence by Democratic elites, as an early substitute for the de jure means of vote suppression associated with the Jim Crow era.

Lastly, we show that Black lynchings were effective in boosting local Democratic performance in the South. Among counties that subsequently experienced a Black lynching, a close Democratic defeat in a presidential election between 1880 and 1900 strongly predicts Democratic *victory* in elections after 1900. Importantly, the same reversal of electoral fortune is not observed among counties with no such history of lynching. This legacy coincided with

reductions in political participation among Black people. In counties where Black lynchings did occur, close Democratic losses between 1880 and 1900 are associated with lower levels of voter turnout in the 1904–12 elections, with lower rates of voter registration observed after the passage of the Voting Rights Act of 1965, which served to dismantle Jim Crow. Meanwhile, the same places saw fewer schooling inputs and higher Black out-migration—particularly by high-human capital types—resulting in lower levels of Black school enrollment and literacy by 1910. The latter underscores a critical feedback mechanism, insofar as literacy tests served to further dampen Black political participation in the 20th century. Together, these results highlight lynching's various political and economic chilling effects.

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; 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 outcomes and narratives of Black deviancy and aggression were endogenous to local political conditions.

Our findings also connect 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), particularly in the U.S. context (Ang, 2023; Bazzi et al., 2023; Esposito et al., 2023). Masera et al. (2022) examine the spread of anti-Black narratives and violence in response to fears of racial

[3]Notably, 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.

[4]Other recent work on postbellum racial violence among political economists includes Albright et al. (2021) on Black wealth; Bazzi et al. (2022, 2025) on Southern white migration; Chyn et al. (2024) on Freedmen's Bureaus; Cook (2014) on Black patenting; Cook et al. (2018a,b) on segregation; Henderson et al. (2021) on Confederate memorialization; Logan (2023) on tax policy; and Williams et al. (2021) on regional inequality. For recent work on lynching by historians, see Berg (2011), Brundage (1993), Lancaster (2014), Pfeifer (2004) and Wood (2011).

[5]Looking 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.

mixing after the Civil War. We take a step back to explore the supply-side foundations of these dynamics, showing how Black empowerment prompted elite investments in anti-Black hatred to suppress the political threat posed by Black people, as previously argued in Glaeser (2005). Separately, Ottinger and Posch (2024) study the strategic use of newspapers by Southern elites in defense of white supremacy, with emphasis on the electoral mobilization of Southern white voters against the populist political threat—distinct from our focus on Black political suppression. Together with Masera et al. (2022) and Ottinger and Posch (2024), our work deepens understanding of the complex interplay between political power, social narratives, and group dynamics in diverse societies. Turning to contemporary relevance, meanwhile, our findings resonate with work on anti-immigrant rhetoric in Europe and the U.S. today. Since the 2010s, anti-immigration advocates have mobilized new support through inflammatory rhetoric, leading to a shift in public debate and, in extreme cases, hate crimes (Freitas-Montiero and Prömel, 2024; Riaz et al., 2024). This mirrors the use of anti-minority politics by both major political parties through the Jim Crow era—during which pro-Black voices remained absent in Southern politics for decades—while offering insight into the enduring relevance of such strategies.

Finally, we contribute to a nascent literature on the social and behavioral effects of elections (Baskaran and Hessami, 2018; Bochenkova et al., 2023; Ferreira and Gyourko, 2014). Differing from previous work on close elections as a form of quasi-experiment for studying the policy effects of officeholders (Lee et al., 2004; Pettersson-Lidbom, 2008), we study an electoral unit—counties in presidential elections—with no direct impact on political outcomes at all. This closely follows Anagol and Fujiwara (2016) and Granzier et al. (2023), who document positive effects of candidate rank among election *losers* on success in subsequent contests. While we share in their focus on the salience of electoral rankings, this paper looks beyond the voter to consider the responses of elite players, seeking to contain coordination among popular opposition groups. We highlight racial violence as a key channel through which local Democratic elites impeded the mobilization of a racially-progressive opposition in the pre-Jim Crow U.S. South. Such "backlash" effects recall Bhalotra et al. (2017), wherein women's electoral success in India served to embolden an anti-woman electorate.

The remainder of the paper is organized as follows. Section 2 provides relevant historical and conceptual background on politics and racial violence in the post-Reconstruction South. Section 3 establishes our RD strategy and main results. Section 4 explores empirically the mechanisms by which close Democratic losses gave rise to lynching. Section 5 highlights some politico-economic implications of our results over the long term. Section 6 concludes.

2 Historical and Conceptual Background

This section presents relevant background for our empirical analysis. We begin with historical background on the coevolution of Black political power and racial violence in the U.S. South in the decades following the American Civil War (1861–65). We then outline a conceptual

framework to explicate general mechanisms through which increases in power among minority groups, such as Southern Black people, may enkindle local violence.

2.1 Historical Background

Following the military defeat of the Confederacy and the passing of the Reconstruction Act of 1867, the Southern states were mandated to include universal manhood suffrage in their new constitutions. 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. These 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" with the intent of violating anyone's 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 granted the president military authorization to enforce against groups conspiring to deny equal protection under the law (U.S. Senate, 2023).

These Acts were intended, in part, to prevent racial violence against Black people and protect their rights as U.S. citizens. Insofar as local authorities had failed to address racial violence, the Enforcement Acts meant that victims and survivors of racial terror could now utilize federal courts to bring lawsuits against their perpetrators (Gardner, 2016; Frantz, 1964).

[6]Black 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).

[7]For 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...." 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)

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 ablaze and killing nearly 100 Black men (Frantz, 1964). Indictments under the Enforcement Acts successfully charged the white men involved with conspiring to injure and oppress the victims because of their voting activity (Frantz, 1964). The Supreme Court reversed those convictions, however, citing that the Fourteenth Amendment, which superseded the Enforcement Acts, only permitted the federal government to intervene if states, not individuals, violated the civil rights of freedmen (Frantz, 1964; Tolnay and Beck, 1995).

The *Cruikshank* ruling thus 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 per 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), wherein mobs would cite allegations of violent crime as grounds for lynching (Wells, 1895; Raper, 1993). One common pretext for lynching was alleged sexual misconduct by Black men involving white women, including rape (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 sexual, 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.

[8] Reconstruction 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).

[9] For 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).

[10] Among 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.

"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. 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 hastened by the severe depression of the 1880s, culminating in the incorporation of the People's Party in 1892.

Historians and social scientists have since 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 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, 2024; 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).

2.2 Conceptual Background

We now outline a conceptual framework to clarify, in more general terms, the mechanisms that underpin the rich history described above. This will serve to guide our empirical analysis throughout the remainder of the paper. The foundations of our conceptual framework follow Blalock (1967), whose "power threat hypothesis" posits that competition for political power may result in increased use of violence by a majority group. Concretely, it contends that as the political threat posed by a minority group increases, so should the majority's use of various social control measures, including racial violence, to maintain its political power.

[11] Some 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, "[w]e 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 Gary said, "every Democrat must feel honor bound to control the vote of at least one negro, by intimidation" or otherwise (Epperly et al., 2020, 759).

An understanding of Blalock (1967) in our historical context means that lynchings of Black people were plausibly an instrument for maintaining political power among white Democrats, who comprised the majority of the Southern electorate. Indeed, the mass enfranchisement of Black men after the Civil War, coupled with the legitimate electoral threat of the multiracial populist coalition, resulted in a shift in political power and resource allocation, with Black individuals and communities seeing gains in civil rights, human capital, school funding, and labor legislation (Cascio and Washington, 2014; Logan, 2020; Valelly, 2009). Had these individuals continued to vote, elections would have become more competitive, threatening the white, Democrat-controlled governments that comprised most Southern states (Epperly et al., 2020).

Meanwhile, there were initially few de jure means of disenfranchising Black voters available to local Democratic elite. While Southern states initially attempted to maintain and strengthen their political power by enacting laws that would restrict Black men from voting, Republicans, who controlled the federal government, overturned those laws and continued to enforce voting rights for Black men (Epperly et al., 2020; Valelly, 2009). By contrast, violence and intimidation by individuals could *not* be punished by the federal government under the Supreme Court's reinterpretation of the Enforcement Acts, as described in Section 2.1.[12]

Left indeterminate in the application of Blalock (1967) to our historical context are the concrete mechanisms through which (i) the Black "power threat" was revealed or made credible and (ii) the white Democratic majority galvanized the carrying out of mob violence against Black people. We now describe these factors—broadly, informational and strategic—which we later explore empirically in Section 4.

Election Results, Information, and Mobilization. Local political actors (e.g., voters, elites) assess the relative strengths of different political groups, in part, using recent vote shares. When actors lack complete information about the local political environment, vote share rankings serve as a useful focal point, facilitating coordination and mobilization among members of the "winning" group—even in cases where those rankings are closely determined, or where they do not directly select the officeholder (Anagol and Fujiwara, 2016; Granzier et al., 2023).[13] This may be further amplified by various behavioral effects stemming from public perceptions of candidate rankings—such as if minority voters become more emboldened by being on the "winning side" (Baskaran and Hessami, 2018; Granzier et al., 2023).

Given the potential for such informational effects, local Democratic elites in our setting would have had reason to pay close attention to the placement of their party's presidential

[12] Southern Democratic elite indeed saw violence as a means to disenfranchise Black voters, absent legal means. Future Senator Frank S. White said at the 1900 Alabama Democratic Convention, "[w]e have disfranchised the African in the past by doubtful methods, but in the future we'll disfranchise them by law" (Perman, 2003).

[13] Concretely, if political mobilization by a given group is the risk-dominant outcome whenever that group is stronger, and if political actors can infer the relative strength of each group in a given place in $t+1$ from its candidate's vote share in t, then the unique equilibrium under incomplete information about group strength involves mobilization by the group with the larger voter share, regardless of the actual vote share differential. This result arises from a "global game" framework, originated in Carlsson and van Damme (1993) and analogous to the formal argument underlying "runner-up effects" in Anagol and Fujiwara (2016).

candidate in Southern counties. Indeed, the party's local defeat might, for instance, mobilize (successful) challenges by opposition parties in other contests (Anagol and Fujiwara, 2016). We provide evidence along such lines in Appendix Table A.1, wherein even a narrow Democratic loss in a county across the 1880–1900 presidential elections predicts a relative decline in the number of Democratic *local* officeholders in the very short run, all else fixed.

Accordingly, media reporting suggests that significant attention was paid to county election results during this period, not only of vote margins but also overall rankings.[14] Appendix Figure A.2 shows that local newspapers systematically discussed such outcomes in proximity to presidential elections.[15] For specific examples of reporting, see Appendix Figure A.3. Meanwhile, the framing of reporting (i.e., of losses or wins) in a county's newspapers varied with whether the Democrats had lost or won it. Appendix Table A.2 shows that, even among closely-determined counties, a Democratic electoral loss in a presidential election is associated with a higher probability of local news reporting on *losses*, compared to a lower probability of reporting on *wins*. Notably, Democratic newspapers had more extensive circulation and influence in the South over this period than did Republican newspapers (Byman, 2021).

Elite Strategy, Racial Hatred, and Backlash. Just as news media inform on electoral outcomes in the aftermath of elections, they may also allow for local elite to communicate strategically with the public for its own benefit (Glaeser, 2005). Crucially, the media may be used by political elites to spread anti-minority sentiment—and even to incite violence against minority individuals (Adena et al., 2015; Ottinger and Posch, 2024; Yanagizawa-Drott, 2014).

In the context of the post-Reconstruction South, newspapers increasingly published stories accusing Black people, especially men, of rape and other crimes (Woodward, 1955). In practice, this plausibly served as a strategy through which local elite could supply or operationalize racial hatred and, in turn, galvanize collective action needed to carry out a lynching. Crucially, insofar as a lynching was carried out by private actors and not public officials, it often went unpunished (Myrdal, 2017; Walker et al., 2018). Lacking legal recourse, many Black Americans "urged the[ir] race to sacrifice its political rights for the sake of peace" (Wells, 1892, 13). In other words, prior to de jure means for disenfranchising minority voters, newspapers offered an alternative channel through which local Democratic elite could frustrate Black political mobilization in places where it had deemed the Black power threat credible.

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

[14] As a modern example, "bellwether counties" serve as indicators of the national power distribution in an election.

[15] Discussion in newspapers of the phrases, "loses county," "lost county," "wins county," or "won county," together with the term "president∗," is high during and in the few months leading up to November in presidential election years across the 1880–1900 elections, before declining to low levels in the months thereafter.

politically-competitive Southern counties, a close Democratic "loss" between 1880 and 1900 nearly doubled the probability of a Black lynching over the subsequent four years, with no discernible effect on white lynchings. We establish a causal interpretation of these effects, before exploring evidence on mechanisms in Section 4.

3.1 Data

Prior to outlining our estimation strategy and results, we introduce and provide a short description of our primary data and their sources. For more details, including summary statistics for our sample variables, see Appendix C.

Outcome Variables. Our primary outcome is an indicator of lynching activity. Lynching data for the former Confederacy, available for after 1881, are coded at the county level and 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).[16] For secondary analyses, we use a measure of anti-Black crime accusations in city newspapers, based on `newspapers.com`, which we link to data on newspapers' partisan affiliation from Gentzkow et al. (2014a,b).

Political Variables. Our main explanatory variation is based on county-level vote tabulations for presidential elections over the 1880–1900 period from Clubb et al. (2006). For the 1896 election, in which William Jennings Bryan was nominated by multiple parties, we supplement these data with information from Robinson (1934). We derive additional information on the racial and partisan composition of local officeholders (e.g., mayors) from Logan (2020) and Kestenbaum (2023), respectively. For secondary analyses, we derive data on state-level Jim Crow laws and voter registration rates for 1962–67 from Jones et al. (2012) and the United States Commission of Civil Rights (1968), respectively.

Other Variables. Various county-level observables come from the aggregate U.S. Censuses, including population density, Black population shares, Black literacy, and manufacturing wages (Haines, 2010). Other variables are based on (i) linked records from the Census Tree Project (Buckles et al., 2023), including former slaveholder shares (via Bazzi et al., 2025) and Confederate Army veteran shares (based on Hall et al., 2019); (ii) Civil War battle locations (from Arnold, 2015); and (iii) geographic factors from the Food and Agriculture Organization of the United Nations' (FAO) Global Agro-Ecological Zones (GAEZ) database. To proxy for potential exposure to agricultural shocks (Feigenbaum et al., 2020; Tolnay and Beck, 1995), we interact the latter with contemporaneous per pound prices from the United States Department of Agriculture's (USDA) Crop Production Historical Track Records.

[16] For more information on these data, including on potential selective reporting concerns, see Appendix C.2.

3.2 Identification Strategy

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

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

where $Any\ Lynching_{c(s)\tau}$ in our analysis indicates whether at least one lynching of a Black (or white) individual occurred in county c of state s during the four-year electoral period following the presidential election held in November of $\tau = \{1880, 1884, ..., 1900\}$.[17] We intentionally focus on national elections, of which county-level results provide information as to the relative strengths of local political groups (see Section 2.2), while lacking direct impacts on actual local (Democratic) power. This minimizes countervailing effects of policy on racial violence. For the same reason, we focus on presidential rather than congressional elections for our main analysis.

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 τ. The period from 1880 to 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, 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 in the South during this period 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, $f(Loss\ Margin_{c\tau})$, we estimate treatment effects based on counties with very close vote shares in a given election. Under the (testable) assumption that close elections tend to occur in otherwise similar places, this strategy provides us with quasi-random treatment variation. We adopt a flexible, linear running polynomial for our main analysis, while reporting estimates based on 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 percentage points (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

[17] See Appendix C for further detail and robustness regarding data and variable coding choices.

around the Democratic loss threshold, $Loss\ Margin_{c\tau} = 0$. If they are not, then estimates may reflect discontinuities in factors besides Democratic Party losses. To test this assumption, we first examine the density of the running variable around the loss 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 ($p = 0.4$) to reject the null hypothesis that $Loss\ Margin_{c\tau}$ is continuous at the loss threshold (see Appendix Figure B.1). This is consistent with previous work on election results in large elections across an array of settings (Eggers et al., 2015).[18] We also test for discontinuities in a wide set of relevant pre-treatment factors, as described in Section 3.1, in place of our outcome in equation (1). We fail to estimate statistically significant differences at the loss threshold across all outcomes, as shown in Table 1. Further reaffirming our identifying assumptions, our core results are 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 results 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 ($\mathbf{X}_{c\tau}$) Together, these account for relevant factors in space not fully captured by our unidimensional running variable.[19] We also show robustness to more demanding specifications. 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.[20] We later demonstrate robustness of inference to alternative levels of clustering. Further details on our RD specification can be found in Appendix B.

[18] That is not to say such manipulation is absent in U.S. history. Pulaski County, Arkansas, saw businesses burned and ballot boxes stolen in 1888 (Summers, 2001). Outside the South, 26% of the electorate in Adams County, Ohio, was punished in 1910 for a vote-buying scheme in which votes were traded for as little as a whiskey (Lehoucq, 2003). Yet, while incidents like these may stand out, electoral fraud occurred more universally through the use of systemic violence and legal voter disenfranchisement—reducing the need to manipulate individual voters in pivotal cases via the wholesale exclusion of particular voting blocs (Aidt and Jensen, 2017; Kuo and Teorell, 2017). Importantly, such measures would be unlikely to result in sorting around the threshold.

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

[20] Note 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 C for further discussion and analysis.

3.3 Main Results: 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 an indicator for whether there were any lynchings of Black individuals in the four-year period following a given presidential election from 1880 through 1900. Besides a linear 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). Our preferred estimate in column 3 implies a 10.4 p.p. increase in the probability of a Black lynching over the four years following a local Democratic Party "loss" in a given county, equivalent to about an 80% increase over the (control) mean.

We also estimate effects for white lynchings. Although white people were less frequent targets of mob violence than Black people, white lynchings were not unusual. On the surface, the accusations underlying white lynchings did not differ significantly from those cited in lynchings of Black people. Theft, in particular, was one of the more common reasons given for a white lynching (Campney, 2021). At the same time, white-on-white lynchings were often conducted *privately*—distinct from the public spectacles that typically characterized white-on-Black lynchings, even for the same accusation—suggesting that "such a mob was driven by different concerns than mobs lynching Black men similarly accused" (Smångs, 2016, 1357).

Estimates for white lynchings, shown in columns 5–8, are small and statistically indistinguishable from zero. These contrast starkly to the estimates for Black lynchings and suggest our findings to be distinct from a general violence effect.

All of these tabular estimates are based on the MSE-optimal bandwidths from Calonico et al. (2014), which limit the set of observations to those close to the Democratic loss 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 perhaps a quarter of that, with the exact number of observations varying by outcome and other factors.

At the same time, counties that experience competitive elections may differ in relevant ways from less competitive ones. In general, an RD strategy estimates the 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, limited to those within the sample median margin of Democratic electoral losses, $|Loss\ Margin_c| = 16.2$. Excluding county-election observations whose vote margins fell within that bandwidth in $\tau - 1$, our baseline estimate for Black lynchings nearly doubles, to 18.6 p.p., in column 3. Our estimates for white lynchings increase as well, although they remain statistically insignificant at conventional levels.

Finally, we complement our tabular results with visual RD plots in Figure 5, which adopt a fixed 10 p.p. bandwidth and axes across outcomes. While merely illustrative, these show the same discontinuity for Black lynchings around the loss threshold as in our tabular results.[21]

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 3, 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 3. For our baseline specification, we clustered 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 3 shows two such alternatives, which cluster by county-decade and state-by-election-period.

Varying Controls. Panel (b) of Table 3 considers alternate sets of covariates in equation (1). Estimates for Black lynchings remain large and significant at conventional levels in more conservative specifications that omit all covariates besides the running variable (row 2), all spatial covariates (row 3), or the longitude and latitude polynomials (row 4).

Results are likewise robust to more demanding specifications. As an alternative to state fixed effects, row 5 includes county fixed effects, based on the fixed-boundary identifiers at which our standard errors at clustered. Row 6 incorporates, in addition to our baseline spatial controls, county-pair fixed effects based on nearest neighbors in longitude and latitude, which we generate conditional upon counties being within the optimal bandwidth from row 1. Each of these has the effect of making our estimates more precise. Row 7 and 8 further verify the assumptions underpinning the RD by controlling for quadratic polynomials of (i) 1880 Black population shares and (ii) all variables from Table 1, respectively, neither resulting in much change to our estimates. Finally, row 9 and 10 further control for potential state-level electoral manipulation, with our results unchanged when we interact various spatial covariates with an indicator for whether a state had yet enacted any Jim Crow voting laws (e.g., ballot requirements, poll taxes), based on information taken from Jones et al. (2012).

Alternative RD Specifications. We test sensitivity of our results to alternative bandwidths and running polynomials in panel (c) of Table 3. Rows 11 and 12 re-estimate the specification in row 1 but with the optimal bandwidths multiplied by factors of 0.5 and 1.5, respectively. Meanwhile, rows 13–15 of Table 3 vary our running polynomial, with estimates based on quadratic 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 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

[21] See Appendix Figure E.1 for alternative plots based on (i) the RD bandwidths from panel (a) of Table 2 and (ii) the restricted sample from panel (b).

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 C.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. Second, our results are robust to omitting any of the six sample election periods, as shown in panel (b).

We also consider the possibility that contemporaneous local and state elections, through their more material local impacts, may be confounding our results. Rows 16 and 17 in panel (d) of Table 3 show that point estimates do not meaningfully change when excluding such cases. We further discuss questions of sample selection in Appendix C.

Alternative Outcome Measurement. Given the infrequency of 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 C.2. These include measures based on (logged) counts and rates (per 10,000 individuals).[22] We also consider a version of our outcome based on a more granular temporal unit of analysis, of year period rather than election period. These produce estimates of roughly similar magnitudes to our baseline, consistent with a 30.0–97.8% increase in the probability of a Black lynching in a given county following a Democratic loss.

Placebo Analysis. Lastly, we conduct a set of placebo analyses based on alternative (i) RD thresholds and (ii) effect windows. First, Figure 6 estimates equation (1) using a variety of "placebo" Democratic loss margins. Specifically, given an actual threshold of Loss Margin$_{c\tau} = 0$, the x-axis shows estimates from alternative thresholds Loss Margin$_{c\tau} + s$, where s ranges from -50 to 50 p.p. The results confirm that it is only the true win-lose threshold that is systematically salient, not any other. Second, Appendix Table C.3 examines the sensitivity of estimates to alternative effect windows. Whereas our core estimation is based on a 48-month effect window following the conclusion of a given presidential election τ, estimates become smaller and less precise the more we shift this window to begin prior to the election. Estimates fully converge to zero once all lynchings in the effect window in fact precede an election.

4 Mechanisms: Elections, Information, and Elite Strategy

The RD estimates in Table 2 point to dramatic effects of presidential election results in counties, with even *narrow* Democratic losses leading to large increases in lynching. This section

[22] Being highly right-skewed with numerous zero-valued observations, we specifically adopt an inverse hyperbolic sine function for our log transformations of these variables.

presents several additional findings meant to clarify the mechanisms underlying this effect. Following our conceptual framework in Section 2.2, we provide evidence for two key types of factors—broadly, informational and strategic—through which such election results were salient and galvanized violent backlash, respectively.

4.1 Electoral Information and Racial Violence

Given a quasi-random interpretation of the RD framework, a puzzle emerges as to why a narrow electoral defeat is not merely perceived as bad luck, relative to a narrow victory, and thus treated the same way in terms of resultant violence. Yet, even closely-determined elections may offer useful signals as to the relative strengths of different political groups going forward (Anagol and Fujiwara, 2016). When political actors lack complete information about the true distribution of political preferences in the local population, a Democratic loss, however narrow, has the potential to mobilize pro-Black opposition. The same loss, in turn, stands to inspire an anti-Black backlash in anticipation. Of course, such effects require that election results indeed constitute relatively informative signals. We consider this dimension now.

Election Results as Signals of Political Strength. We begin by probing further heterogeneity analysis in the spirit of panel (b) of Table 2. In Table 4, we estimate a large set of conditional RD specifications based on whether a county in election τ was (i) Democrat-won in presidential election $\tau - 1$ (column 1–4), (ii) electorally uncompetitive in $\tau - 1$ (column 5–8), or (iii) both (columns 9–12). The logic of this exercise is as follows: in counties where Democrats had lost in $\tau - 1$, particularly if by large margins, another (narrow) Democratic loss in τ would not constitute much in terms of new information—it may even imply a strengthening Democratic hand. Likewise, in a perennially-competitive county, a narrow loss would indeed be akin to a narrow win, with both outcomes being equally uninformative. The complementary cases, on the other hand, would constitute relatively informative signals, potentially hastening changes in local political conditions (Bursztyn et al., 2020).

Our findings are consistent with these notions. Whereas the effect of Democratic losses on Black lynchings is large in counties that voted Democratic or were uncompetitive 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 .403 (.130), versus small and statistically insignificant increases in Black lynchings following narrow Democratic defeats otherwise, with much smaller estimates of .045 (.042).

Together, these patterns are consistent with informational factors underlying our main RD effect.[23] Under this interpretation, the Democratic Party's failure to win in a given county,

[23] Why 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.,

even narrowly, would on average have served to signal their relative weakness locally. Fearing such a signal might embolden minority opposition, Southern Democrats would in turn have had incentive to foment (violent) backlash—a prospect for which we find evidence in Section 4.2.

Conditioning on the (Populist) Opposition. The extent of information revealed by a Democratic loss would also depend on to whom the party's candidate lost. For example, a Democrat losing to a member of the populist (i.e., more pro-Black) opposition would signal something rather different than a Democrat losing to a "lily-white Republican" (i.e., more anti-Black), all else fixed. Following our conceptual framework in Section 2.2, we would expect the former outcome to be of relatively greater salience to anti-Black political actors than the latter.

We examine the potential heterogeneity of effects along this dimension in Appendix Table C.4, exploiting the fact that the Democrats' primary opposition candidate (i.e., of first or second place) varied across counties within states and elections. For example, the populist nominee of the People's Party in 1892, James B. Weaver, won 3 counties in Virginia that year, while securing second in another 14 counties. First, columns 1–2 re-estimate our main effect using a version of the sample that excludes the 1896 election, in which the Democrats and the populist coalition were aligned under a shared nominee, William Jennings Bryan. Second, columns 3–4 further restrict the sample to those observations in which the Democrats' primary opposition candidate was affiliated with the third-party "populist coalition," as defined in the table notes.

The latter estimates, associated with the effect of Democrats losing a county to a member of the unambiguously pro-Black populist coalition, imply a 50.4–54.2 p.p increase in the probability of a Black lynching in the subsequent four years. This effect size amounts to a 319–360% increase over the (control) mean and is about four times larger than the ones estimated in columns 1–2. This notably dovetails with Ottinger and Posch (2024), who emphasize the populist cause as a key driver underlying anti-Black propaganda in the postbellum South.

Beyond Counties and Informational Effects. Our core results above are based on county-level vote shares from presidential elections, of which outcomes convey information as to the strengths of local political (e.g., opposition) groups. Importantly, these outcomes lack the direct impacts on local Democratic power, including local policy, that would be associated with other types of elections. We now expand our analysis to examine congressional district elections, thus shedding some light on the relative importance of the latter channel. Indeed, if the Democratic candidate were to win in a *congressional* election, they might choose to use the power of the office to reduce protections against racial violence, thus attenuating the overall treatment effect. Meanwhile, the relative effects of an election through informational versus direct power channels may vary with the electoral opposition. A populist political opposition, for instance, may amplify the informational effect, as in Appendix Table C.4, but also the countervailing "power" effect associated with the office itself.

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.

We begin by estimating a variant of equation (1) at the unit of the congressional district (CD), using a version of our lynching data mapped to the CD level (Ferrara et al., 2021). Our full sample consists of eleven two-year CD election periods, with secondary samples omitting (i) midterm election years and (ii) the 1880 elections, the latter mostly predating our first sample lynching. RD estimates based on these, shown in columns 1–3 of Appendix Table C.5, are imprecise and, while positive, relatively small—equivalent to a 4.3% increase over the (control) mean in column 2, versus a 79.2% increase in column 1 of Appendix Table C.4.

Restricting the sample to those CD elections with a populist primary opposition, meanwhile, results in larger estimates, albeit still smaller than the county-level analog. The estimate in column 5, for instance, is equivalent to a 154.5% increase over the (control) mean, versus a 360% increase in column 3 of Appendix Table C.4. Of course, one must exercise some caution when interpreting these estimates, given the small sample size available for CD-level analysis.

Overall, these findings imply that election results retain some informational salience across a variety of electoral contexts. At the same time, there exists a meaningful countervailing force when elections also shape the local distribution of political power, in the form of the officeholder's direct (policy) impacts, which stand to mitigate such informational effects. Next, we turn our focus to a different kind of elite impact—the one associated with elites on the *losing* end of an election, seeking to contain oppositional mobilization.

4.2 Elite Influence and Racial Backlash

If poor Democratic performance in post-Reconstruction Southern locales confirmed the credibility of the Black "power threat," then it stands to reason that it would also have galvanized a Democratic backlash in turn. Absent de jure means for Democratic elite to disenfranchise the Black electorate, such backlash might commence, nonetheless, in the form of racial violence and intimidation. Indeed, our results thus far suggest as much. Yet, mob violence depended on the decentralized effort of many local actors, which would have been costly to direct and coordinate. In this section, we show how local Democratic elite operationalized and fomented racial hatred through the strategic use of newspaper media. These effects, together with our core lynching effects from Section 3, are driven by places with an all-white, wholly-Democratic elite facing large Black populations therein, peaking in the pre-Jim Crow period.

Partisan Media and the Supply of Racial Hate. Collective action needed to carry 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 immediate aftermath of Democratic losses (see Appendix Figure C.2). Along similar lines, the rise of Black lynching in the post-Reconstruction South often followed newspaper stories documenting atrocities accused of Black people, often against white women (Woodward, 1955; Glaeser, 2005). Such accusations 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 (Wells, 1892, 1895).

This next analysis explores variation in the content of local newspapers in the aftermath of presidential elections. Insofar as newspapers throughout the South were strongly affiliated with the Democratic Party at the time (Gentzkow et al., 2015), they plausibly served as important political instruments in the aftermath of Democratic county losses. For instance, by operationalizing racial hatred through the dissemination of anti-Black atrocity stories, newspapers may have aided local elites in galvanizing the kinds of post-electoral lynching activity documented in this paper. Such a pattern would point to the strategic use of media by local elites for supressing Black political participation.

Estimation. To estimate the effect of local Democratic electoral losses in presidential elections 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 anti-Black crime accusations in its newspapers' content, by estimating the regression discontinuity (RD) design,

$$\%\ \text{Accusations}_{n(c)t(\tau)} = \beta \cdot \text{Democratic Loss}_{c\tau} + f(\text{Loss Margin}_{c\tau}) + \phi_{\tau} + \Upsilon_{t(\tau)} + \alpha_{\sigma(c)} + \varepsilon_{nt},$$

where $\%\ Accusations_{nt}$ measures the rate of anti-Black crime accusations in newspaper n in city σ of Southern county c for a given year t within a four-year period following $\tau \in \{1880, ..., 1900\}$. All regressions include fixed effects for election period (ϕ_{τ}) and newspaper city or town (α_{σ}). As in the within-election version of our main analysis (in Appendix Table C.2), we also include dummies for news year minus most recent election year (Υ_t) to account for cyclic shocks in newspaper content within electoral periods. For robustness, we separately estimate effects using our baseline spatial covariates from equation (1), of state fixed effects and quadratic polynomials in county longitude and latitude, as well as newspaper fixed effects.

We begin by building a comprehensive, time-varying sample of newspapers pages from `newspapers.com`,[24] which we link to information on cities' historical counties (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. As newspaper units often enter or exit our sample within election periods (e.g., due to newspaper splits and mergers), we adopt calendar years as the temporal unit for this analysis.[25]

We then define anti-Black crime accusation rates for each year in our 1880–1900 election period sample. To construct this variable, we count the total number of pages per newspaper-year across our sample states and periods that plausibly feature an anti-Black crime accusation.

[24] See Beach and Hanlon (2022) and Ferrara et al. (2022) on use of `newspapers.com` to build historical data.

[25] As an example, the *Memphis Daily Appeal* runs in our sample from 1881–89. Meanwhile, its competition, the *Memphis Avalanche*, runs 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).

Following Glaeser (2005), we search for all mentions of "negro rape" and "negro murder," plus "negro robbery." In practice, this also identifies similar phrases, such as "negro intended robbery" (see Appendix Figure D.1 for examples). Our script additionally allows for the plural of the word "negro" and the 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).

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.

Newspaper Results. Columns 1–6 of Table 5 reveal that a close Democratic loss in a city's county between 1880 and 1900 is associated with a 28.9–88.4% increase in the frequency of anti-Black crime accusations in that city's newspapers, relative to the control mean. This is similar to the effect size for Black lynchings in Table 2 and suggests the use of newspapers to propagate racial hatred where Democrats performed relatively poorly in presidential elections.

To test whether this effect is related to Democratic affiliations common among Southern newspapers at the time, we split our sample by the political leanings of newspapers in columns 7–10 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 perhaps more striking.

Estimates indeed remain positive among Democratic newspapers in columns 7–8. Interestingly, they become *negative* among the smaller sample of non-Democratic newspapers in columns 9–10. Though marginally insignificant, these latter point estimates are somewhat large. This points to narrow Democratic *victories* spurring anti-Black antagonism among local *non-Democratic* elites. Whether this was intended as punishment against Black people for "insufficient" support or evidence of non-Democrats' gradual embrace of anti-Black politics by the late 19th 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—and that even relatively pro-minority parties may be willing to resort to anti-minority politics when it suits them, strategically.

We further elaborate upon and show robustness of these results along a number of dimensions in the Appendix. Appendix Table D.1 augments our analysis using an array of alternative (a) inference strategies, including higher-level clustering, (b) covariates, including quadratic controls for 1880 Black population shares, and (c) RD specifications, including varying bandwidths. In panel (d), we explore in a series of sensitivity analyses the relevant sources of variation among the search terms used to build our outcome variable (e.g., "negro rape" versus

"negroes raped"), together with (null) estimation based a placebo outcome measure that omits the term "negro(es)" entirely. Finally, panel (e) confirms that our results are not driven by a small handful of observations with the highest rates of anti-Black crimes accusations.

Overall, these findings are consistent with local newspapers offering a core channel through which local elites fanned anti-Black animus in the post-Reconstruction South, particularly in times of poor Democratic performance. Notably, this complements Ottinger and Posch (2024) on the use of newspapers for catalyzing white political mobilization in the face of pro-Black political movements. Our analysis, in contrast, focuses on the use of anti-Black atrocity narratives for the suppression of Black political participation, by galvanizing lynching activity that concurrently occurred after Democratic electoral losses. 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.

Newspaper Accusations and Lynchings. Our measure of anti-Black crime accusations is inspired by prior historical work on 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" 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 commonly legitimized by accusations of violent atrocities, such as the rape of a white woman.[26] Though these narratives, like lynching, declined in prominence in the 20th century, accompanying stereotypes have persisted in the national consciousness through film (e.g., *The Birth of a Nation*) and literature (e.g., *Native Son*).

Further supporting the use of accusatory news articles in inciting lynchings of Black people, Figure 7 shows how the frequency of accusations in a county increased over the period leading up to a Black lynching in our data, before decreasing thereafter. Importantly, the dynamics of this relationship vary with the contemporary electoral conditions. Such accusations, *overall*, tended to co-occur with or follow a lynching, more suggestive of retrospective reporting. Among counties coming off of close Democratic losses,[27] however, accusations tended to co-occur with or, indeed, *precipitate* a Black lynching, in line with our proposed mechanism.

Elite Composition, Institutions, and the (Black) Power Threat. The same local conditions that gave rise to higher lynching rates after Democratic losses also served to fuel accusatory newspaper stories. Table 6 shows that both of these outcomes are specifically driven by the places that were most plausibly characterized by Blalock's (1967) Black "power threat," with an all-white, Democratic local elite facing a relatively large Black population.

We begin by conditioning our sample on whether a given county had a white-only (columns 1–2) or Democrat-only (columns 3–4) elite as of presidential election τ, based on the set of

[26] Accusations of sexual violence are most common amongst both outcomes, as shown in Appendix Table E.1.
[27] We follow Table 4 and define "competitive" based on the median vote margin among sample Democratic losses.

local- and state-level public officeholders serving a given county at the time. Data on the racial composition of elite come from Logan (2020), who documents a relatively small but impactful group of Southern Black public officials beginning in the Reconstruction period, while partisan information come primarily from the Political Graveyard (Kestenbaum, 2023).

Estimates across panels (a) and (b) of Table 6 reveal both our lynching results from Table 2 and our newspaper results from Table 5 to be driven by counties with a white- or Democrat-only elite composition. At the same time, these results also depend on the presence of a large Black local constituency, on which to potentially pin the blame for poor Democratic fortune. Although 99% of our observations had Black populations as of 1880,[28] columns 5–6 show that both sets of results are wholly driven by those with above-median Black population shares.

Further evidence that anti-Black antagonism was a strategic response by local elite to the threat of Black empowerment can be found from examining the relevance of other, de jure forms of voter suppression for our results. Indeed, lynching proliferated across the U.S. South after several Supreme Court decisions removed key protections for Black people (Woodward, 1955), only subsiding with the rise of Jim Crow and the decline of Democratic political competition in the South (Epperly et al., 2020; Glaeser, 2005).

Table 7 provides quantitative evidence in support of this interpretation. Concretely, 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 year that a given state implemented (i) ballot requirements (e.g., literacy tests, multi-box laws) and (ii) poll taxes from Jones et al. (2012). First, columns 1–2 of Table 7 confirm that lynchings of Black people systematically followed Democratic electoral losses before the introduction of Jim Crow laws but not after.[29] Columns 3–6, meanwhile, split our newspaper sample along the same lines, first using the full set of newspapers in columns 3–4 and then only Democrat-affiliated ones in columns 5–6. Among the latter, we find that close Democratic losses in presidential elections led to relatively higher rates of anti-Black crime accusations in the absence of Jim Crow voting laws but not after their implementation. Perhaps more strikingly, we find in columns 3–4 a reversal of estimate sign among our entire newspaper sample for the post-Jim Crow period, when Democrats became firmly entrenched but other parties did not. This again suggests that non-Democratic elites were more likely to resort to anti-minority politics in the Jim Crow era, particularly when Democrats were achieving local electoral success. Of course, by the early 20th century, those groups comprised only a small electoral minority in the face of Democratic political hegemony.

5 Solidifying the South: Lynchings and Electoral Reversal

We have shown that poor local Democratic performance in the post-Reconstruction South precipitated an elite-led backlash. Absent de jure means to disenfranchise Black voters, Demo-

[28] The average sample county contained 35.2% (st. dev. = 23.9) Black population shares in 1880.
[29] Of course, lynching continued to occur for reasons unrelated to our treatment (Wells, 1895; Jones et al., 2017).

cratic elites circulated anti-Black crime accusations throughout local newspapers, fomenting racial terror. How effective was resultant lynching at boosting local Democratic performance and suppressing Black empowerment? In this section, we focus on the legacies of these events for subsequent Democratic political success and local Black politico-economic outcomes, beginning with the former.

Democratic Losses, Lynching, and Reversals of (Electoral) Fortune. In the first of several complementary exercises, we present suggestive evidence that counties where Democrats narrowly lost were subsequently more likely to be *won* by Democrats in the early 20th century. Crucially, this depends on a Black lynching having subsequently occurred in a county. Racial violence, we argue, proved key in bringing about a reversal of electoral fortunes for the Democratic Party across once-competitive areas of the South.

Table 8 shows estimates from this exercise. We begin in panel (a) by restricting to county observations that experienced any Black lynchings during the four-year period following election τ. Then, in columns 1–6, we examine whether a Democratic loss in a given county between 1880 and 1900 predicts a Democratic victory in the same county 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* probability of Democratic success in 1904 and 1908—converging to 1—though not in 1912, as practically all Southern counties had ceased to be competitive at that point. Appendix Figure E.2 further illustrates the Democratic electoral premium associated with Black lynchings among these formerly-competitive counties.

In addition, columns 7–8 show that Democratic county losses in post-Reconstruction presidential elections, conditional on a Black lynching having subsequently occurred, correspond to a larger number of Democratic *local* officeholders over the 1904–12 election periods. In counties where Democrats had previously (narrowly) lost and that subsequently saw racial violence (panel a), the average number of Democratic local officeholders during this period is roughly 0.35—compared to about 0.13 in counties where no lynching followed (panel b). These estimates, which are conditional on the total number of local officeholders (e.g., mayoral, postmaster) matched to a given county in the Political Graveyard (Kestenbaum, 2023), represent a reversal upon the short-run relationship between Democratic performance in presidential contests and local Democratic officeholding, as shown in Appendix Table A.1.

Crucially, the same effects cannot be estimated among counties with no such history of Black lynching, as shown in panel (b). This heterogeneity points to racial violence as being an important, if not necessary, condition for the Democratic Party's reversal of electoral fortunes in the lead up to Jim Crow, consistent with prior insights of contemporary observers (Wells, 1895) and modern political economists (Jones et al., 2017).

Close Losses, Accusations, Lynchings, and Electoral Reversal. Overall, our findings so far suggest a clear chain of events, wherein close Democratic losses led to an elite-led backlash and

resultant violence against Black individuals. Together, these helped set the stage for increased Democratic success by the early 20th century. Whereas these various causal links have thus far been demonstrated in piecemeal, we try now to succinctly tie together these various threads.

Building on our previous analysis in Figure 7, which demonstrated a positive association in time between local anti-Black news accusations and lynchings of Black people, Figure 8 shows that, within a given election period, there exist positive correlations for both Black lynchings *and* accusatory newspaper articles with local Democratic victory in early 20th century presidential elections. As in Figure 7, these relationships are significantly larger if the former followed close Democratic losses. Concretely, a county that experienced either a Black lynching or a 1 p.p. increase in the rate of anti-Black newspaper accusations following a close Democratic loss was about 30% more likely to witness a Democratic win during the 1904 or 1908 presidential elections than one that did not. The same patterns can likewise be found when looking at the number of Democratic local officeholders, as shown in Appendix Figure E.3. Collectively, these findings serve to drive home the behavioral framework that we laid out in Section 2.2.

Democratic Losses, Lynching, and Black Outcomes. We now turn our focus to documenting various contemporaneous effects on Black politico-economic outcomes associated with this overall chain of events. We begin with a discussion of Black political participation in the early 20th century, before examining questions of education and migration.

Black Political Participation. To what extent was Democrats' electoral reversal of fortune influenced by declines in Black political participation? Appendix Table E.2 explores various such measures for the early 20th century. First, columns 1–4 demonstrate relatively lower levels of voter turnout as having occurred alongside the Democratic resurgence shown in Table 8. In counties where Democrats had previously (narrowly) lost and that subsequently saw racial violence (panel a), rates of voter turnout in the 1904–12 presidential elections averaged about 25%—compared to around 35% in counties without such lynchings (panel b).

As before, estimates become small and statistically insignificant with the election of 1912, when Jim Crow granted the Democratics one-party rule throughout the "Solid South." Columns 5–8, meanwhile, fast forward in time to the 1960s, when the Voting Rights Act (VRA) dismantled racial discrimination in national elections throughout the country. Using voter registration data from the United States Commission of Civil Rights (1968),[30] we find that relatively lower levels of non-white political participation re-emerged in these places by the 1960s, especially after the VRA's passage, which caused increases in voter registration overall (Ang, 2019). Among Southern counties where Democrats had lost in presidential elections and that subsequently witnessed racial violence during the post-Reconstruction period (panel a), the number of Black registered voters (per total Black persons) averaged about 18% by 1967, compared to about 27% among counties with no such lynchings (panel b)—both much smaller than the

[30]These contain data for white and non-white people in all sample states except for Tennessee and Texas, as well as a large number of counties in Mississippi and North Carolina. For the post-VRA period, Arkansas and Virginia are wholly excluded as well.

overall mean among white people of 57%. This result recalls Williams (2022), in which lynchings served as a negative shock to Black political participation that persisted over time through local culture and institutions.

Black Education and Selective Out-Migration. Black political disenfranchisement in the South also reduced the quantity and quality of Black education. To the extent that lynching played a role in stifling Black political participation and securing Democratic entrenchment throughout the South, it is plausible that it may also have facilitated a decline in Black-held human capital. Indeed, the education of ex-slaves had been a key priority among non-Democratic leaders pursuing a strategy of Black political empowerment (Naidu, 2012). Such efforts were abandoned once the Democrats achieved one-party rule in the South, as literacy tests played an important role in denying Black people the right to vote.

Pursuing a similar heterogeneity analysis to Table 8, 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 12%, relative to the control mean (columns 1–2 of panel a in Table 9). This literacy deficit corresponds to an analogous drop in school enrollment among Black children (columns 3–4), of about 25%. Crucially, such effects are contingent on a Black lynching having subsequently occurred during the four-year period following that election, further suggesting racial violence to be a core channel through which Black empowerment—both political and economic—was diminished in once-competitive Southern counties.

What precisely is behind such large deficits in Black educational attainment? The most obvious cause would be fewer educational inputs provided to the local Black population, following its electoral disenfranchisement. Using data from Carruthers and Wanamaker (2019), columns 5 and 6 of panel (a) show that these effects indeed coincide with fewer teachers per pupil among Black, but not white, schools in the 1910s. A second, related explanation for these patterns involves selective out-migration by high human-capital types. Columns 7–10 of panel (a) provide some suggestive evidence in favor of this interpretation, with a large, relative decline in the Black literate population between 1870 and 1910 among treated, former-lynching counties. This outcome, which is not wholly exclusive to literate Black people, may likewise be behind some of the Democrats' reversal of electoral fortune in these places more generally.

Interpreting Heterogeneous Effects. We conclude our analysis by briefly touching on two questions related to the heterogeneity analyses presented in this section. First, we want to address the concern that counties in panels (a) of Tables 8 and 9, in addition to having experienced racial violence, may have simply had larger Black populations than those counties in panels (b). To account for any mechanical influence of Black population size on these heterogeneous effects, we match counties across panels (a) and (b) based on similarity in Black population shares as of 1880. This involves defining within election years pairs of counties with and without exposure to lynching, with otherwise similar Black population shares, among the combined estimating samples of each column. We then restrict the samples to those respec-

tive subsets of all matched counties. In practice, this procedure primarily drops observations with small Black population shares from the panel (b) regressions. The results of this exercise, shown in Appendix Table E.3, differ little overall from the estimates in Tables 8 and 9.

Second, we offer a 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 in presidential elections over the 1880–1900 period in Section 3.2 (and as probed further 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 increased with 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.

6 Conclusion

Less than five decades after the U.S. 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 still persists. Black people residing in the South remain worse off in terms of incomes and educational attainment relative to white people—and that says nothing of the millions who fled the region in the 20th century, often enduring continued discrimination elsewhere (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 backslid-

ing 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 norms and institutions.

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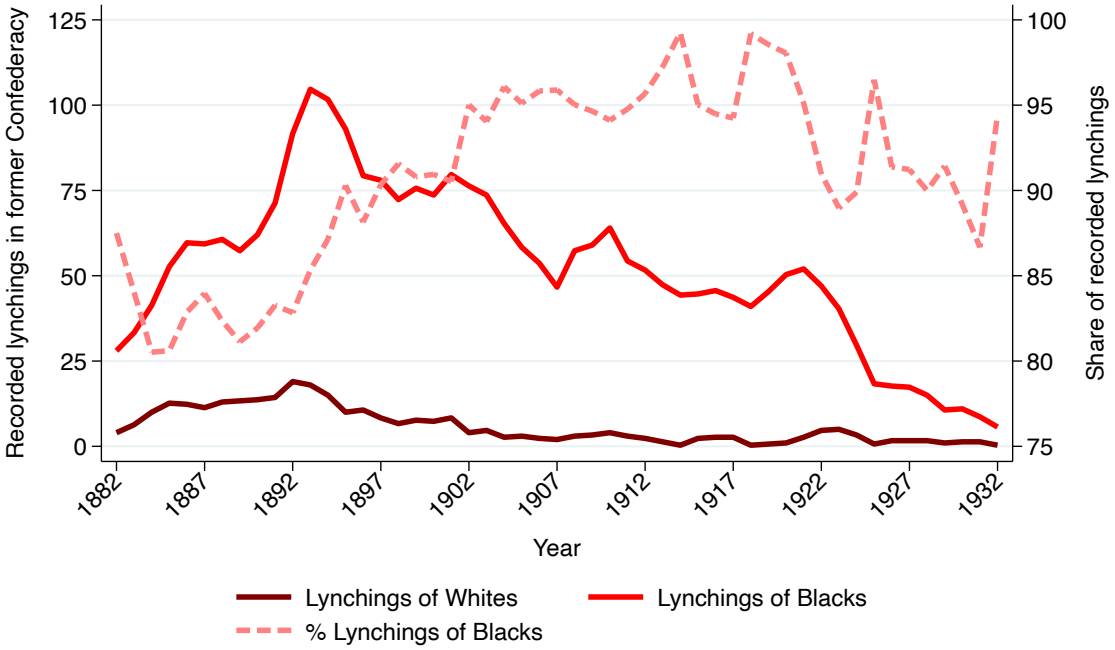
Figures and Tables

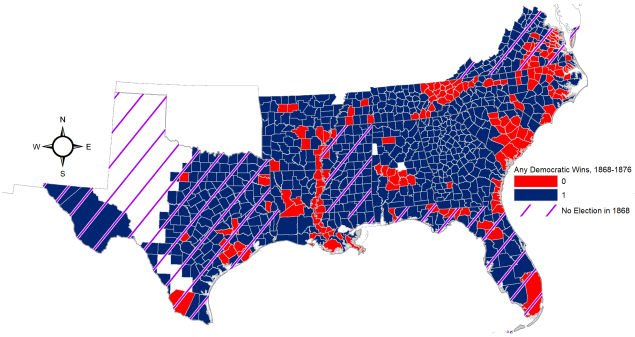
Figure 1: Lynchings in the Former Confederacy, 1882–1932



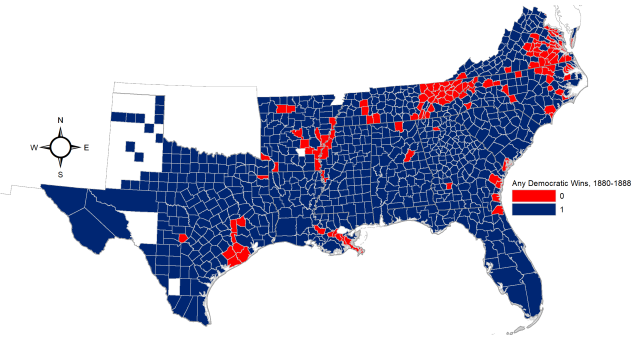
Notes: Three-year moving averages in the frequency of recorded lynchings across the eleven former Confederate states from 1882 to 1932 of whites (dark solid) and Blacks (light solid), as well as the share that were of Blacks (dashed). In total, there are 4,121 lynchings recorded over this period, with 3,140 occurring in former Confederate states. 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

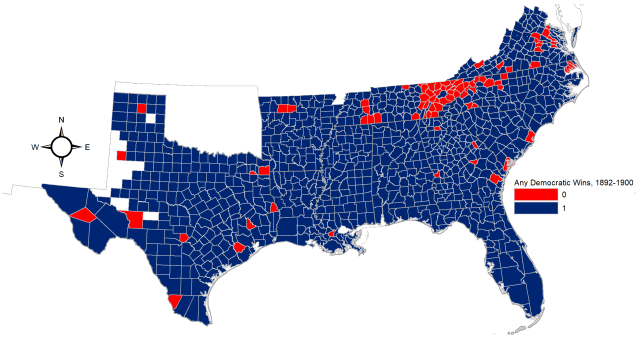
(a) Any Democratic Victories, 1868–1876



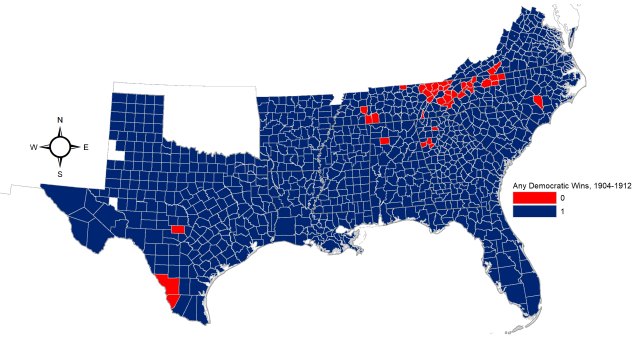
(b) Any Democratic Victories, 1880–1888



(c) Any Democratic Victories, 1892–1900

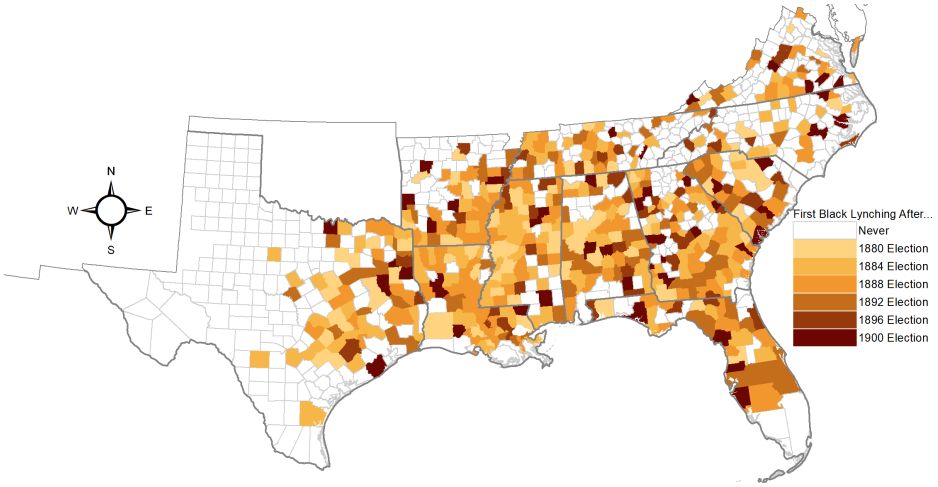


(d) Any Democratic Victories, 1904–1912



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.

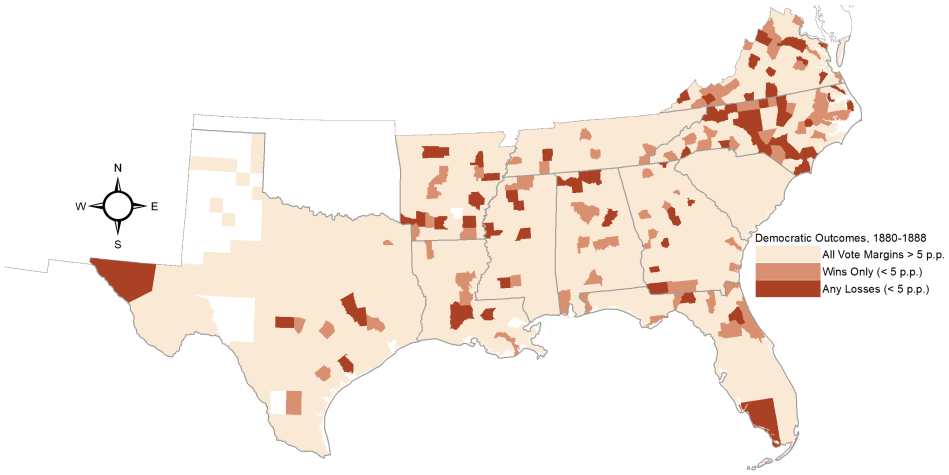
Figure 3: Visualizing Sample Lynching Variation, 1882–1904



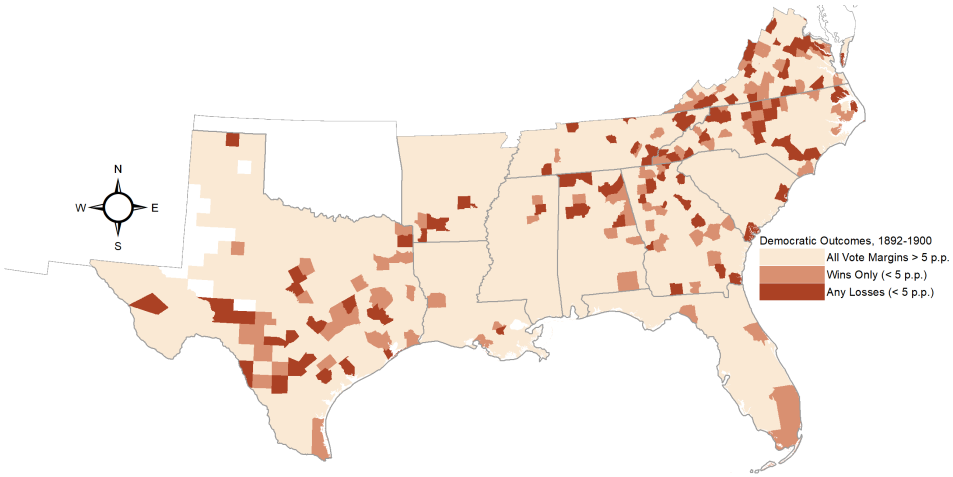
Notes: Map shows the spatial and temporal distribution of Black lynchings in our main sample, broken down by a county's election period of first Black lynching in the sample. "Never" includes counties that experienced a lynching outside of the sample period. See Appendix C for details on data construction 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



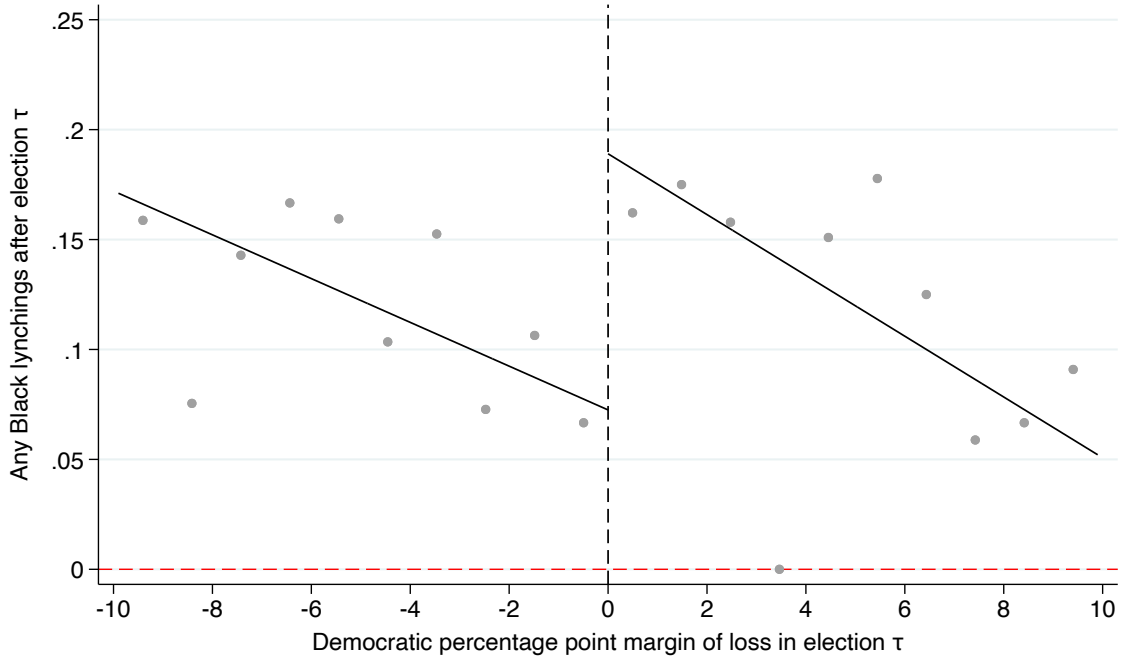
(b) Close Democratic Wins and Losses, 1892–1900



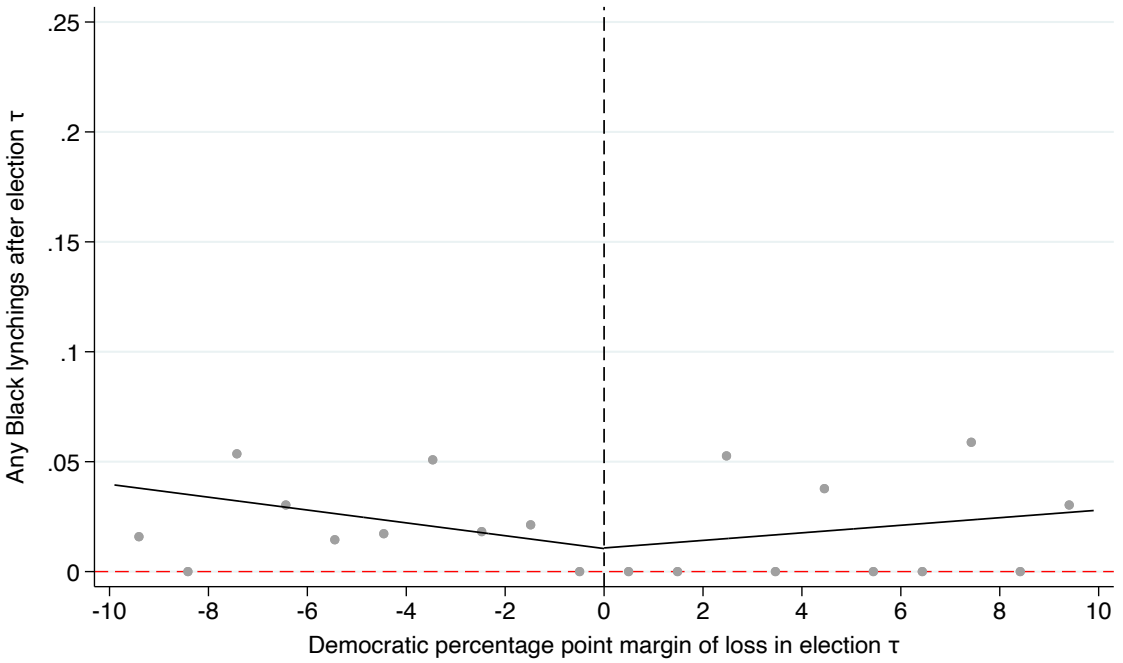
Notes: Map shows the distribution of close Democratic wins and losses, based on a very narrow 5 percentage point bandwidth, for sample counties over two election periods, 1880–1888 and 1892–1900. Counties that experienced any narrow Democratic losses during a given period in dark red. Counties that experienced 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 C 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

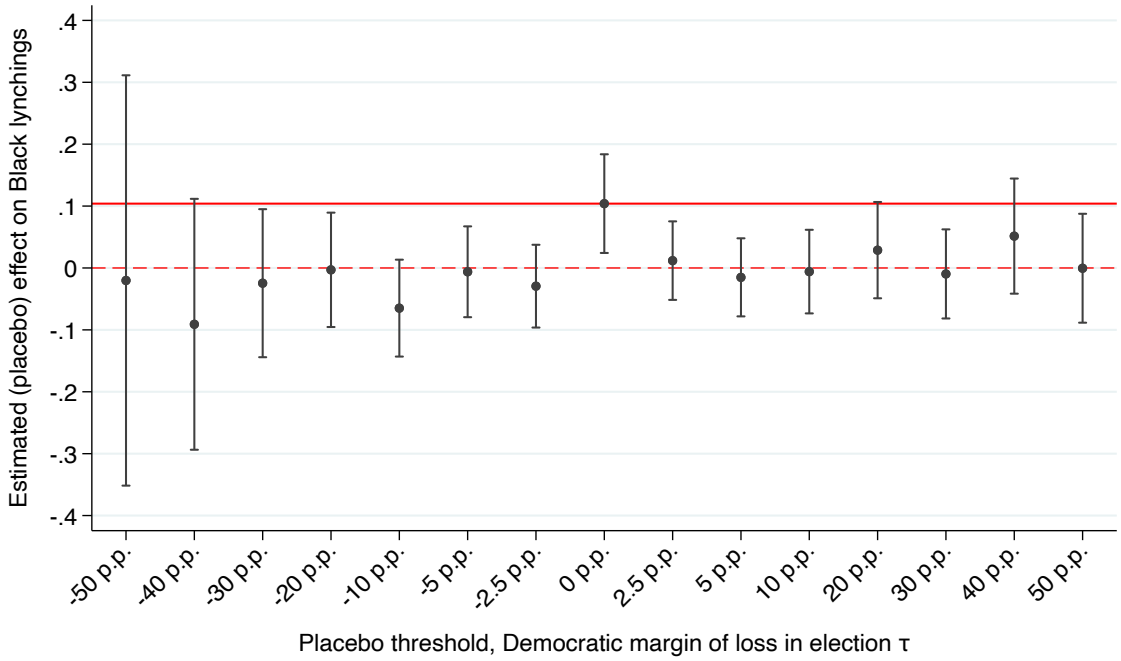


(b) Any White Lynchings After Election τ



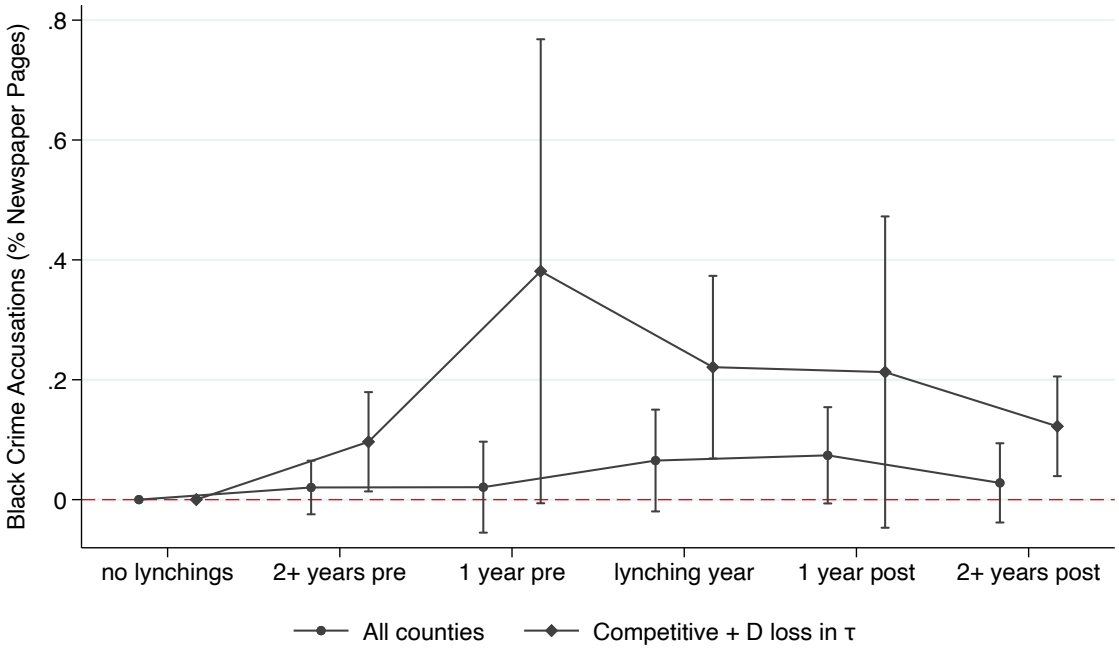
Notes: Binned estimates of the probability of (a) Black and (b) white lynchings during the four-year election period following a presidential election $\tau \in \{1880, ..., 1900\}$ by the Democratic margin of loss in τ. Negative values on the x-axis indicate the Democratic candidate won a given county, 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: Placebo Effect Estimates Based on Alternative Vote Share Thresholds



Notes: RD estimates of the probability of Black lynchings during the four-year election period following a presidential election $\tau \in \{1880, ..., 1900\}$, using a set of "placebo" Democratic margins of victory in τ, where the solid red line denotes the baseline estimate in Table 3. Given an actual Democratic loss threshold of Loss Margin$_{c\tau} = 0$, x-axis shows estimates from alternative thresholds Loss Margin$_{c\tau} + s$, where s ranges from -50 to 50 p.p. 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 95% confidence intervals.

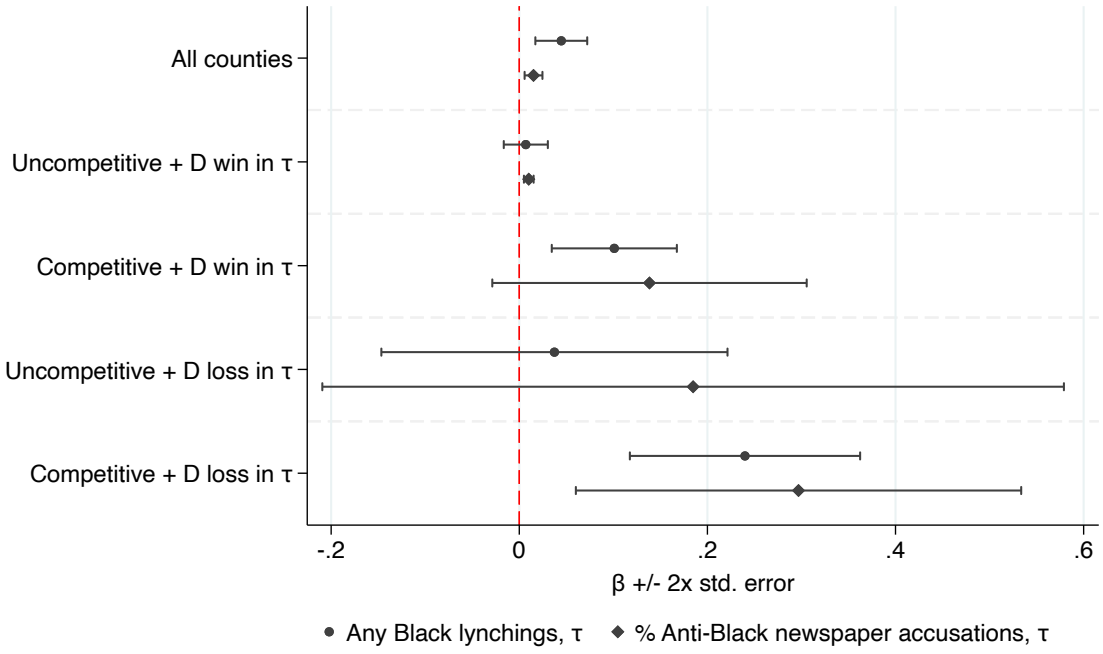
Figure 7: Dynamics of Black Lynchings Around Anti-Black Crime Accusations



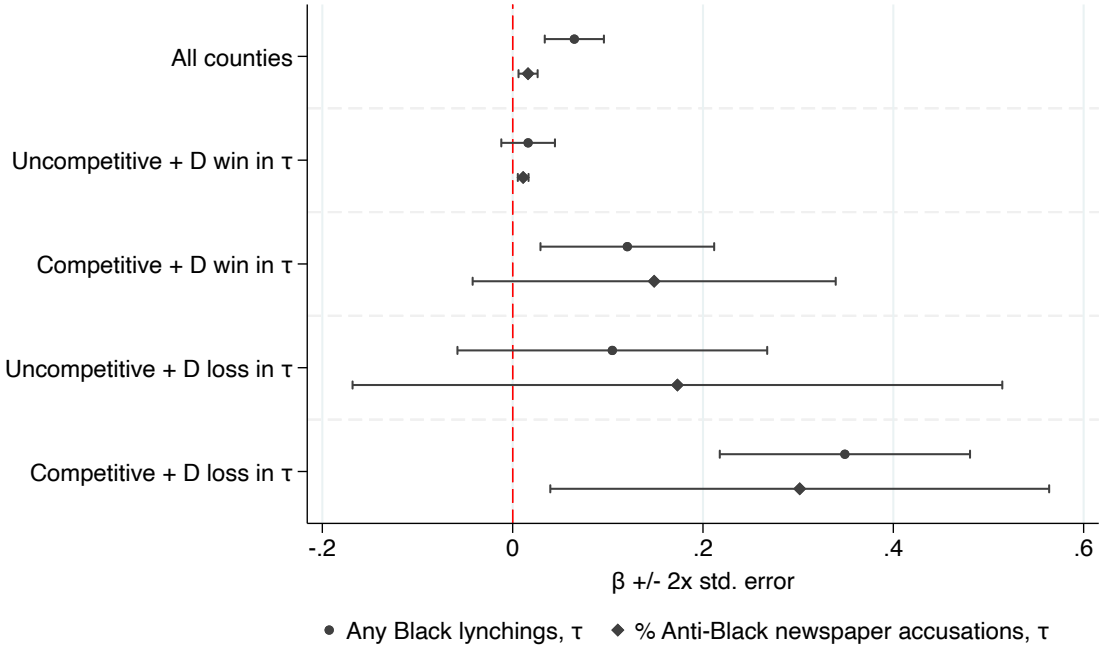
Notes: The average frequency of anti-Black crime accusations in newspapers in the years leading up to, during, and immediately after a Black lynching event in a given county (relative to the average frequency among never-lynching counties), as a share of total newspaper pages. The "competitive + D loss" subsample conditions on the set of electorally-competitive counties that Democrats lost in the most recent presidential election τ, using the median vote margin among sample Democratic electoral losses ($|Loss\ Margin_c| = 16.2$) as the cutoff for the former. The averages of estimates are 0.034 (0.024) for the full-county sample and 0.145 (0.038) for the "competitive + D loss" subsample. Regressions include 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: Lynchings, Anti-Black Accusations, and Electoral Reversal

(a) Democrats Won County in 1904



(b) Democrats Won County in 1908



Notes: Estimates of the probability of a Democratic popular vote win in a county in the 1904 (panel a) and 1908 (panel b) presidential elections by whether at least one Black lynching occurred in that county and the average frequency of anti-Black crime accusations in newspapers during the four-year election period following a presidential election $\tau \in \{1880, ..., 1900\}$. Row labels correspond to different conditional effects, where "competitive" conditions on the set of electorally-competitive counties in the most recent presidential election τ, using the median vote margin among sample Democratic electoral losses ($|Loss\ Margin_c| = 16.2$) as the cutoff for the former. All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude. For context, the probability that the Democratic candidate won in a given county among former Confederate states was 0.87 in 1904, 0.84 in 1908, and 0.95 in 1912. Standard errors are clustered at the county level. Error bars represent 95% confidence intervals.

Table 1: Regression Discontinuity Balance Tests: Pre-Treatment County Characteristics

Dependent Variable:	Log Population Density (1)	% Black Population (2)	% Former Slaveholders (3)	% Confederate Veterans (4)	Any Civil War Battles (5)	Average Farm Size (6)	Return on Cotton Potential (7)	Return on Tobacco Potential (8)
Democrat Lost County in Election τ	.090	3.397	.345	.064	.071	83.766	-.001	0.000
	(0.084)	(2.21)	(0.42)	(0.47)	(0.066)	(79.0)	(0.002)	(0.000)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	22.57	18.97	25.82	18.07	12.62	27.60	22.40	21.13
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Control outcome mean	-11.73	35.49	7.90	30.41	0.20	212.84	0.05	0.06
Observations	2,064	1,719	2,303	1,653	1,199	2,411	2,042	1,937

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	-.153	.725	1.82	.251	2.745	-2.088	.023	.110
	(0.28)	(0.59)	(2.90)	(1.94)	(6.74)	(2.76)	(0.047)	(0.15)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	18.66	17.01	24.11	18.75	17.57	25.70	17.80	15.29
Control outcome mean	32.95	2.38	15.43	38.27	94.07	44.46	0.83	0.29
Observations	1,703	1,574	2,195	1,698	1,598	2,272	1,618	1,406

Notes: This table reports bias-corrected local-polynomial RD estimates corresponding to equation (1) for various pre-treatment county-level characteristics. All characteristics are measured as of 1880 except for columns 7–8, which interact indexes of theoretical cotton and tobacco potential per unit of land from the Food and Agriculture Organization of the United Nations' (FAO) Global Agro-Ecological Zones (GAEZ) database with per pound prices as of presidential election $\tau \in \{1880, ..., 1900\}$ from the United States Department of Agriculture's (USDA) Crop Production Historical Track Records. See Section 3.2 for more details on variables. Estimates are based on linear running polynomials and the MSE-optimal bandwidth from Calonico et al. (2014). See Appendix Table B.1 for estimates based on quadratic running polynomials. 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)	(5)	(6)	(7)	(8)
	(a) Full Sample ($N = 5,914$)							
Democrat Lost in Election τ	.105**	.125**	.104**	.138***	-.006	-.003	-.008	-.006
	(0.044)	(0.054)	(0.041)	(0.053)	(0.013)	(0.015)	(0.013)	(0.015)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	No	No	Yes	Yes	No	No	Yes	Yes
Optimal bandwidth	14.76	21.37	15.62	20.22	24.55	28.87	24.92	29.30
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Control outcome mean	0.13	0.14	0.13	0.14	0.02	0.02	0.02	0.02
Observations	1,396	1,992	1,481	1,879	2,263	2,563	2,281	2,587
	(b) Uncompetitive Counties in Election $\tau - 1$ Only ($N = 4,306$)							
Democrat Lost in Election τ	.198**	.261**	.186***	.257**	.025	.032	.016	.023
	(0.077)	(0.11)	(0.071)	(0.10)	(0.023)	(0.026)	(0.023)	(0.025)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	No	No	Yes	Yes	No	No	Yes	Yes
Optimal bandwidth	16.72	21.38	18.21	21.39	18.90	24.72	20.36	25.76
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Control outcome mean	0.12	0.14	0.13	0.14	0.02	0.02	0.02	0.02
Observations	648	912	712	912	746	1,098	841	1,150

Notes: This table reports bias-corrected local-polynomial RD estimates corresponding to equation (1) for whether there were any Black (columns 1–4) and white (columns 5–8) lynchings in a given county during the four-year election period following a presidential election $\tau \in \{1880, ..., 1900\}$. Counties in panel (a) include those in the former Confederate states. Panel (b) restricts the sample to counties that were electorally uncompetitive in $\tau - 1$, within the median vote margin among sample Democratic electoral losses ($|Loss\ Margin_c| = 16.2$). Estimates are based on linear (odd columns) and quadratic (even) running polynomials and the MSE-optimal bandwidth from Calonico et al. (2014). All regressions include election period fixed effects, while columns 3–4 and 7–8 also include the set of spatial covariates, which 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: Identification and Robustness Checks on RD Estimates in Table 2

Dependent Variable:	Any Black Lynchings (1)	Any White Lynchings (2)
	(a) Alternative Standard Errors	
1. Baseline (Columns 3 and 7 of Table 2)	.104**	-.008
Clustering by County	(0.041)	(0.013)
Clustering by County-Decade	(0.041)	(0.014)
Clustering by State-Election-Period	(0.041)	(0.014)
	(b) Alternative Control Sets	
2. No Controls or Fixed Effects	.097**	-.009
	(0.045)	(0.014)
3. No Spatial Covariates	.105**	-.006
	(0.044)	(0.013)
4. No Longitude and Latitude Controls	.074*	-.009
	(0.040)	(0.013)
5. Baseline w/ County Fixed Effects, Based on Unique County Boundaries	.056***	-.011
	(0.022)	(0.0090)
6. Baseline w/ County-Pair Fixed Effects, Matched on Proximity in Longitude and Latitude	.091***	-.008
	(0.035)	(0.012)
7. Controlling for Quadratic Polynomial in 1880 Black Population Shares	.096**	-.007
	(0.041)	(0.013)
8. Controlling for All Variables From Table 1	.091**	-.01
	(0.043)	(0.014)
9. Baseline w/ State $\times$ Pre-Jim Crow FE	.104**	-.008
	(0.041)	(0.013)
10. Baseline w/ Spatial Covariates $\times$ Pre-Jim Crow	.100**	-.007
	(0.039)	(0.013)
	(c) Alternative RD Specifications	
11. Optimal Bandwidth$\times 0.5$	.160**	.007
	(0.068)	(0.013)
12. Optimal Bandwidth$\times 1.5$	.108**	-.008
	(0.044)	(0.013)
13. Quadratic Running Polynomial	.138***	-.006
	(0.053)	(0.015)
14. Cubic Running Polynomial	.126**	.003
	(0.057)	(0.015)
15. Quartic Running Polynomial	.138**	.007
	(0.056)	(0.015)
	(d) Alternative Samples	
16. Excluding States w/ Election Years Contemporaneous with τ	.097*	.018
	(0.058)	(0.023)
17. Excluding States w/ Election Months Contemporaneous with τ	.090*	.007
	(0.052)	(0.020)

Notes: This table reports bias-corrected local-polynomial 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 $\tau \in \{1880, ..., 1900\}$. Estimates are based on linear 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). Panel (d) excludes observations with gubernatorial elections held during the same year (row 16) or month (row 17) as presidential election τ. See Section 3.3 for a more detailed overview of the items in each row. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Conditioning on Previous Election Outcomes

Dependent Variable:	Any Black Lynchings After Election τ											
Sample:	Democrat-Won in Previous Election $\tau-1$				Uncompetitive in Previous Election $\tau-1$				Uncompetitive + Democrat-Won in $\tau-1$			
	Yes		No		Yes		No		Yes		No	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Democrat Lost in Election τ	.160**	.164**	.050	.041	.186***	.257**	.068	.089	.403***	.388***	.045	.092
	(0.063)	(0.068)	(0.066)	(0.075)	(0.071)	(0.10)	(0.049)	(0.067)	(0.13)	(0.15)	(0.042)	(0.058)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	14.69	27.98	12.75	20.60	18.21	21.39	14.52	16.66	10.44	19.78	17.88	18.94
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Control outcome mean	0.12	0.14	0.16	0.17	0.13	0.14	0.15	0.14	0.09	0.14	0.14	0.14
Observations	918	1,710	414	627	712	912	827	923	268	604	1,131	1,171

Notes: This table reports bias-corrected local-polynomial 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 $\tau \in \{1880, ..., 1900\}$, conditional on whether Democrats won a given county in the previous election $\tau - 1$ (columns 1–4), whether a county was electorally uncompetitive in $\tau - 1$, within the median vote margin among sample Democratic electoral losses ($|Loss\ Margin_c| = 16.2$) as the cutoff (columns 5–8), and both (columns 9–12). Estimates are based on linear (odd columns) or quadratic (even) 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 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 5: Partisan Media and Anti-Black Crime Accusations in Newspapers

Dependent Variable:	Frequency of Anti-Black Crime Accusations (% Pages in Newspaper)									
Newspaper Affiliation:	Any						Democratic		Non-Democratic	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Democrat Lost County in τ	.126*	.139**	.168***	.136**	.055**	.118***	.104**	.168***	-.368	-.312
	(0.073)	(0.069)	(0.042)	(0.056)	(0.024)	(0.037)	(0.042)	(0.060)	(0.23)	(0.21)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
News year − Election year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County spatial covariates	Yes	Yes								
City FE			Yes	Yes			Yes	Yes	Yes	Yes
Newspaper FE					Yes	Yes				
Optimal bandwidth	19.71	21.41	16.04	14.40	14.83	14.54	19.20	16.05	11.64	11.70
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Control outcome mean	0.20	0.20	0.19	0.19	0.19	0.19	0.20	0.19	0.20	0.20
Observations	3,234	3,524	2,745	2,535	2,614	2,563	2,212	1,917	206	206

Notes: This table reports bias-corrected local-polynomial RD estimates for the frequency of anti-Black crime accusations in a given newspaper-year during the four-year election period following a presidential election $\tau \in \{1880, ..., 1900\}$, including conditional on a newspaper's partisan affiliation being Democratic (columns 7–8) or non-Democratic (columns 9–10). Estimates are based on linear (odd columns) and quadratic (even) running polynomials and the MSE-optimal bandwidth from Calonico et al. (2014). Anti-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 for election period, newspaper publication year minus most recent election year, and state (columns 1–2), city (columns 3–4, 7–10), or newspaper (columns 5–6). County spatial covariates in columns 1–2 include quadratic polynomials in county longitude and latitude. Standard errors are clustered at the county level, except in columns 9 and 10, which are heteroskedasticity-robust. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Heterogeneous Effects: Elite Composition and the Black Power Threat

	Elite Composed of [...] at τ				Large Black Constituency in 1880	
	Whites Only		Democrats Only			
	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
	(a) Dep. Var: Any Black Lynchings After Election					
Democrat Lost in Election τ	.108***	-.055	.126***	-.005	.177***	-.031
	(0.042)	(0.13)	(0.044)	(0.098)	(0.065)	(0.038)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	15.14	21.18	14.55	21.85	14.04	26.85
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear
Control outcome mean	0.13	0.20	0.13	0.16	0.15	0.11
Observations	1,378	87	1,101	413	735	1,028
	(b) Dep. Var: Frequency of Anti-Black Crime Accusations (% Pages)					
Democrat Lost in Election τ	.155***	-.007	.245***	.038**	.104**	-.048
	(0.045)	(0.037)	(0.056)	(0.016)	(0.049)	(0.046)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes
News year − Election year FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	13.72	18.47	16.26	11.29	21.47	8.29
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear
Control outcome mean	0.19	0.20	0.17	0.23	0.20	0.16
Observations	2,159	330	1,770	734	2,037	642

Notes: This table re-estimates Tables 2 and 5 conditional on whether a given county had a white-only (columns 1–2) or Democrat-only (columns 3–4) elite composition as of election τ, as well as whether it had an above-median Black population in 1880 (columns 5–6). Elite composition based on local- and state-level public officeholders serving a given county (e.g., mayors, state legislators) at τ from Logan (2020) (columns 1–2) and Kestenbaum (2023) (columns 3–4), as well as its governor at τ. 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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Heterogeneous Effects: Before and After State Jim Crow Laws

Dependent Variable:	Any Black Lynchings After Election τ		Frequency of Anti-Black Crime Accusations (% Pages in Newspaper)			
			Newspaper Affiliation			
			Any		Democratic	
Pre- or Post-Jim Crow?	Pre-	Post-	Pre-	Post-	Pre-	Post-
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Lost in Election τ	.135**	.028	.265***	-.283***	.291***	-.014
	(0.056)	(0.047)	(0.044)	(0.060)	(0.048)	(0.074)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes
Years since election FE			Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes				
City FE			Yes	Yes	Yes	Yes
Optimal bandwidth	14.66	25.61	8.37	11.13	9.28	11.32
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear
Control outcome mean	0.15	0.13	0.16	0.22	0.17	0.23
Observations	905	839	1,133	591	886	386

Notes: This table re-estimates Tables 2 and 5, conditional on a county's state having enacted any Jim Crow laws (i.e., poll taxes, literacy test, multi-box, or secret ballot laws) as of election τ. State-level Jim Crow data come from Jones et al. (2012). 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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Electoral Reversals of Fortune in Lynching Counties

Dependent Variable:	Democrat Won County in… 1904		1908		1912		Democratic Local Officeholders in 1904–12 Election Periods	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(a) Counties with Any Black Lynchings							
Democrat Lost in Election τ	.118*	.250**	.162*	.377***	-.004	-.011	.207**	.222**
	(0.064)	(0.11)	(0.083)	(0.11)	(0.006)	(0.012)	(0.087)	(0.11)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	18.92	18.66	21.24	19.48	11.56	17.14	19.13	24.50
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Control outcome mean	0.93	0.93	0.90	0.91	1.00	1.00	0.14	0.13
Observations	218	217	259	232	136	194	221	307
	(b) Counties with No Black Lynchings							
Democrat Lost in Election τ	-.010	-.014	-.045	-.078	-.015	-.018	.017	.022
	(0.049)	(0.055)	(0.050)	(0.062)	(0.026)	(0.029)	(0.034)	(0.039)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	21.91	31.40	28.26	28.16	19.07	28.82	21.14	33.11
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Control outcome mean	0.83	0.86	0.79	0.79	0.97	0.97	0.11	0.10
Observations	1,776	2,356	2,174	2,170	1,548	2,220	1,716	2,473

Notes: This table reports bias-corrected local-polynomial 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 number of Democratic local officeholders in a given county across the 1904–12 election periods (column 7–8). Regressions in panel (a) restrict to counties in which a lynching occurred at some point during the four-year period following τ, while those in panel (b) restrict to the complementary cases without lynchings. For context, the probability that the Democratic candidate won in a given county among former Confederate states was 0.87 in 1904, 0.84 in 1908, and 0.95 in 1912. Local officeholder composition based on individuals linked to a given county and holding public office at from Kestenbaum (2023). Set of offices restricted to local ones (e.g., mayoral, postmaster) in which the officeholder took office after 1904, through 1916. Counties matched with future elections based on like county identifiers. Estimates are based on linear (odd columns) and quadratic (even) 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, as well as the total number of unique local officeholders in the county over the outcome period in columns 7–8. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Education and Out-Migration in Lynching Counties

Dependent Variable:	Literacy Rate Among [...] People, 1910		School Enrollment Among [...] Children, 1910		Teachers per 100 [...] Pupils, 1910s		Rate of Literate [...] Population Change, 1870–1910		Rate of Illiterate [...] Population Change, 1870–1910	
	Black	White	Black	White	Black	White	Black	White	Black	White
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	(a) Counties with Any Black Lynchings									
Democrat Lost in τ	-7.914***	1.043	-13.747***	-.355	-.275**	-.037	-369.357*	-14.978	-152.571**	32.026
	(2.00)	(1.47)	(3.70)	(2.33)	(0.13)	(0.21)	(220.5)	(78.4)	(67.8)	(62.7)
Period τ lynchings only	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	12.14	15.57	11.00	18.46	13.85	15.24	16.36	16.18	16.41	15.65
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Control outcome mean	66.65	91.67	55.65	72.78	1.91	2.72	326.60	128.20	27.99	79.11
Observations	141	173	124	211	96	102	182	179	185	176
	(b) Counties with No Black Lynchings									
Democrat Lost in τ	-1.047	-.454	.465	.538	.259	.062	-206.351	-267.14	-164.04	-34.547
	(1.27)	(0.75)	(1.92)	(0.85)	(0.35)	(0.094)	(270.9)	(185.8)	(123.4)	(63.8)
Period τ lynchings only	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	25.39	18.88	23.05	20.36	23.53	18.99	31.18	19.51	21.88	20.73
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Control outcome mean	68.27	90.07	57.48	73.60	2.20	2.65	655.51	325.47	97.01	113.90
Observations	1,944	1,503	1,794	1,611	1,059	890	2,269	1,536	1,715	1,624

Notes: This table reports bias-corrected local-polynomial RD estimates corresponding to equation (1) for literacy rates in 1910 among Blacks (column 1) and whites (column 2); school enrollment rates in 1910 among Blacks (column 3) and whites (column 4) aged 6–14; the average number of teachers per 100 Black (column 5) and white (column 6) pupils in schools in the 1910s; the rate of population change in the number of literate Black (column 7) and white (column 8) people in 1910 relative to 1870; and the rate of population change in the number of illiterate Black (column 9) and white (column 10) people in 1910 relative to 1870. Regressions in panel (a) restrict to counties in which a Black lynching occurred at some point during the four-year period following τ, while regressions in panel (b) restrict to the complementary cases without Black lynchings. 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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix

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A Expanding on Conceptual Framework

This Appendix expands on our conceptual framework in Section 2.2, with several pieces of evidence in its support.

Candidate Rankings as Information. The first part of our conceptual framework argues that political actors in the post-Reconstruction South had means to evaluate the local state of the Black "power threat" through election outcomes—namely, the placement of the Democratic candidate for president in county-level returns. This argument builds on previous evidence on candidate rankings as having informational effects independent of direct policy, incumbency, or other officeholder effects (Anagol and Fujiwara, 2016; Granzier et al., 2023). Such evidence has focused on ranking effects among non-winners, wherein, e.g., even narrow second-place "winners" fare better relative to third-place "losers" in subsequent contests.

To further motivate this line of argument in our setting—and in turn explain why Democratic elites might have invested in anti-Black violence in such places—we explore whether narrow Democratic "losers" at the county level in presidential contests between 1880 and 1900 fared worse in subsequent *local* election results, relative to non-Democratic "winners." Insofar as they did, this would provide Democratic elites with at least one incentive to be attune to even the narrowest of Democratic losses, despite those county-level results having no direct bearing on policy or other officeholder effects in themselves.

To identify such effects, we estimate a regression discontinuity (RD) design akin to our primary empirical strategy from Section 3:

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

where $Local\ Officeholders_{c(s),\tau+1}$ captures the number of Democratic local officeholders in county c of state s as of presidential election $\tau + 1$. In our preferred specification, this count subtracts the number matched to the same county identifier as of election τ. This measure is based on the set of individuals matched to a given county and holding public office in a given year from the Political Graveyard (Kestenbaum, 2023), with the set of offices restricted to local ones (e.g., mayoral, postmaster). $Democratic\ Loss_{c\tau}$, meanwhile, captures whether the Democratic candidate for president lost the popular vote in county c in a given election year τ. By interacting $Democratic\ Loss_{c\tau}$ with a running variable for 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. Further controls include election period fixed effects, state fixed effects, quadratic polynomials in county longitude and latitude, and the total number of local officeholders in a given county as of a given election period (i) τ and (ii) $\tau + 1$. Please refer to Appendix B for further detail on our overall RD strategy.

Table A.1: Local Democratic Officeholders After Democratic Presidential Losses, 1880–1900

Dependent Variable:	Democratic Local Officeholders in County as of [...]							
	Election $\tau+1$ Minus as of τ				$\tau+1$		τ	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democrat Lost County in Election τ	-.064***	-.070**	-.059**	-.068**	-.043**	-.044*	.018	.019
	(0.025)	(0.030)	(0.023)	(0.029)	(0.018)	(0.025)	(0.019)	(0.024)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Total officeholders controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	21.16	31.04	23.62	32.40	22.42	28.15	18.76	27.60
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Control outcome mean	0.00	0.00	0.00	0.00	0.04	0.03	0.04	0.03
Observations	1,972	2,695	2,196	2,790	2,096	2,506	1,741	2,475

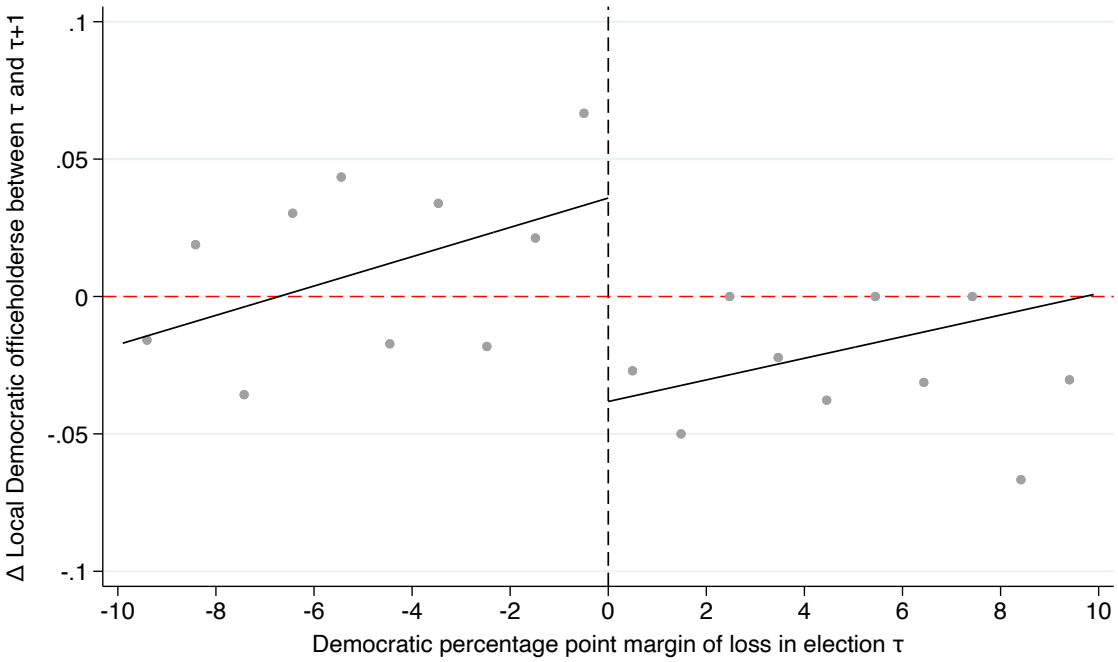
Notes: This table reports bias-corrected local-polynomial RD estimates for the number of Democratic local officeholders in a given county as of election $\tau+1$ following a given presidential election $\tau \in \{1880, 1884, ..., 1900\}$ relative to as of election τ. Local officeholder composition based on individuals linked to a given county and holding public office at from Kestenbaum (2023). Set of offices restricted to local ones (e.g., mayoral, postmaster). Columns 3–8 control for the total number of local officeholders in a given county as of a given election period (i) τ and (ii) $\tau+1$. Estimates are based on linear (odd columns) and quadratic (even) 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 for county longitude and latitude. Standard errors are clustered at the county level. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A.1 shows that even a narrow Democratic loss in a county across the 1880–1900 president elections predicts a relative decline in the number of Democratic local officeholders by the following electoral period, all else fixed. Notably, there is no significant discontinuity associated with the number of Democratic officeholders as of the contemporaneous presidential election τ. Rather, the discontinuity emerges only as of the following presidential election, driven by a relative decline in the number of Democratic local officeholders serving counties where the party had narrowly lost four years prior (see Appendix Figure A.1). Of course, as we show in Table 8, where Black lynchings occur downstream of a Democratic loss, we observe a *larger* number of Democratic local officeholders by the early 20th century—a reversal from the short-run relationship shown in Appendix Table A.1.

Elite Interest in County-level Election Results. Next, we provide a few pieces of evidence that significant (Democratic) attention was, indeed, paid to county-level election results during this period, based on patterns in media reporting. Appendix Figure A.2 presents the probability of a given newspaper reporting on county-level outcomes in presidential elections (including predictions thereof) in a given month, as compared to presidential election months (of November, or month 0) across the elections during the pivotal post-Reconstruction period of 1880 to 1900. These outcomes are based on whether an archived newspaper, as published on `newspapers.com`, has at least one page that printed the terms "president$*$" and either "loses county," "lost county," "wins county," or "won county" (i.e., reporting on electoral losses or wins). To illustrate how these probabilities change over time, we build a fully-balanced

newspaper-month-year panel of newspapers across these six four-year electoral periods across all newspapers with at least one such positive search result. We then calculate a positive search rate across all such observations within each newspaper-election-month bin.

Figure A.1: Change in Local Democratic Officeholders by Democratic Loss Margin in Presidential Elections, 1880–1900



Notes: Binned estimates of the change in the number of Democratic local officeholders in a given county as of election $\tau + 1$ following a given presidential election $\tau \in \{1880, 1884, ..., 1900\}$, relative to as of election τ, by the Democratic margin of loss 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 A.1.

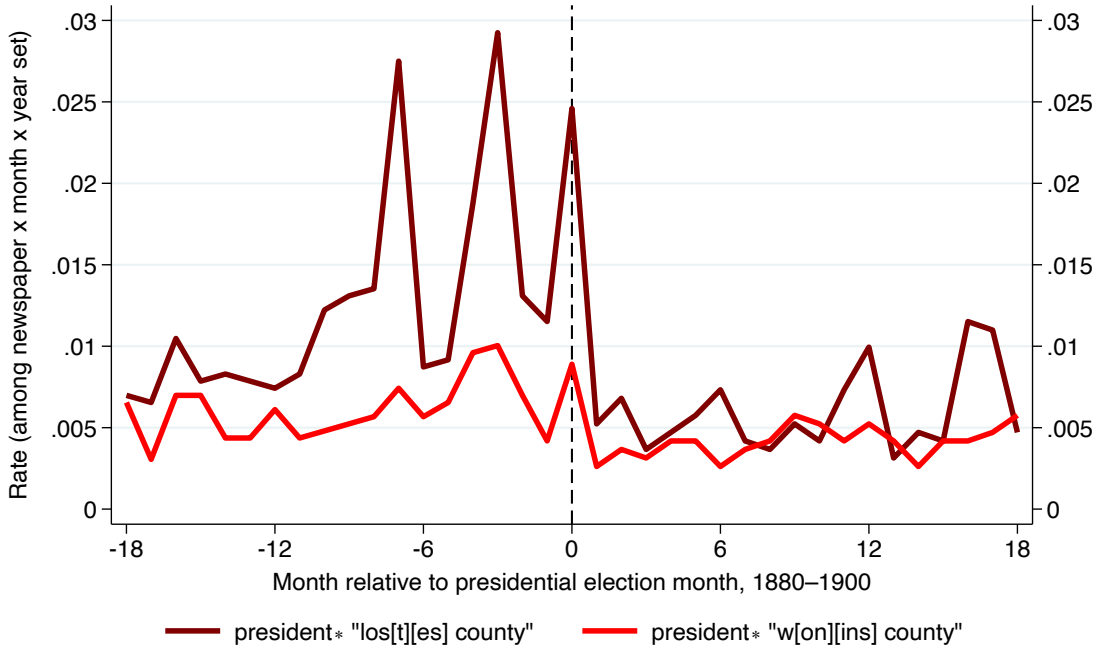
Appendix Figure A.2 shows that newspaper reporting on county-level outcomes in presidential elections peaks during the November of a given election year, remaining relatively low over the months thereafter. Such reporting also remains relatively rare prior to elections, with one exception; in the six or so months leading up to an election, such reporting begins to occur in earnest, suggestive of speculative reporting in the lead up period.

Meanwhile, whether a newspaper reports on an electoral loss versus a win in its county following a presidential election plausibly depends on whether the Democratic Party experienced a loss or win in that county in that election. To explore this, we regress the probability that a given newspaper reports on an electoral loss or win (in a given newspaper-year in the election period τ) on whether the Democratic Party lost in a given county in a given presidential election, among all newspaper-years with any such positive search results (as featured in Appendix Figure A.2). Concretely, we estimate the following regression discontinuity (RD) design,

$$\text{Pr(Report Outcome)}_{n(c)t(\tau)} = \beta \cdot \text{Democratic Loss}_{c\tau} + f(\text{Loss Margin}_{c\tau}) + \phi_\tau + \Upsilon_{t(\tau)} + \alpha_{\sigma(c)} + \varepsilon_{nt},$$

for newspaper n in city σ of Southern county c for a given year t during the four-year pe-

Figure A.2: Dynamics of Election Outcome Reporting in Newspapers, 1880–1900



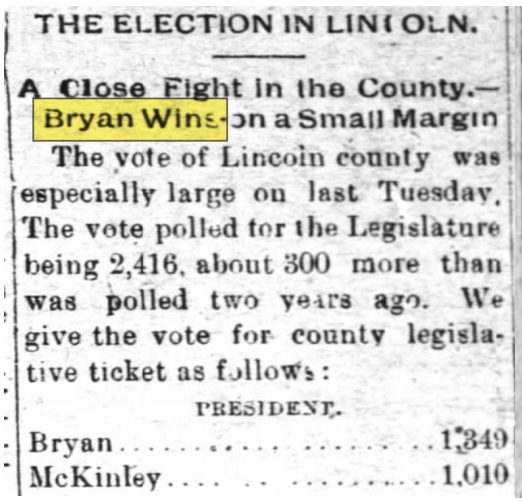
Notes: Monthly averages in the probability of newspapers reporting on an electoral loss (dark solid) or win (light solid) in the 36 month period surrounding a given presidential election between 1880 and 1900, where month 0 corresponds to the November of a given election year. Newspaper reporting collected from articles archived at newspapers.com (last accessed on July 16, 2024). Full balanced newspaper-month panel consists of all newspapers with at least one positive search result for "president∗" and either "loses county," "lost county," "wins county," or "won county" across the four-year 1880–1900 electoral periods.

riod following $\tau \in \{1880, ..., 1900\}$. In other words, we examine whether Democratic losses in a given city's county predict increases (decreases) in its newspapers' reporting on electoral losses (wins). Importantly, the vast majority of newspapers in the post-Reconstruction South were affiliated with the Democratic Party. These estimates correspond to local average treatment effects, where $Democratic\ Loss_{c\tau}$ is interacted with a linear running polynomial in $Loss\ Margin_{c\tau}$, such that estimates are based on relatively close Democratic losses. All regressions include fixed effects for presidential election period (ϕ_τ), newspaper year minus the most recent election year (Υ_t), and the newspaper city or town (α_σ).

Appendix Table A.2 reports these estimates. As expected, a Democratic electoral loss in a given county during a given presidential election is associated with a larger probability of a newspaper in that county reporting "president∗" alongside either "loses county" or "lost county" (column 1–5), versus a smaller probability of reporting "president∗" alongside either "wins county" or "won county" (column 6–10). These effect signs are maintained using the full sample of newspapers on record as having reported on election results (columns 1 and 6); among the subset of newspapers-years that followed relatively close elections based on the data-driven MSE-optimal bandwidth from Calonico et al. (2014) (columns 2–3 and 7–8); and among the even smaller subset of Democrat-affiliated newspapers that comprise most of the sample (columns 4–5 and 9–10), based on the affiliations in Gentzkow et al. (2014a,b) (see Section 4.2 for more details on partisan linking).

Figure A.3: Salience of County-level Presidential Election Results: Newspapers

(a) Lincoln County, NC (1896)



THE ELECTION IN LINCOLN.

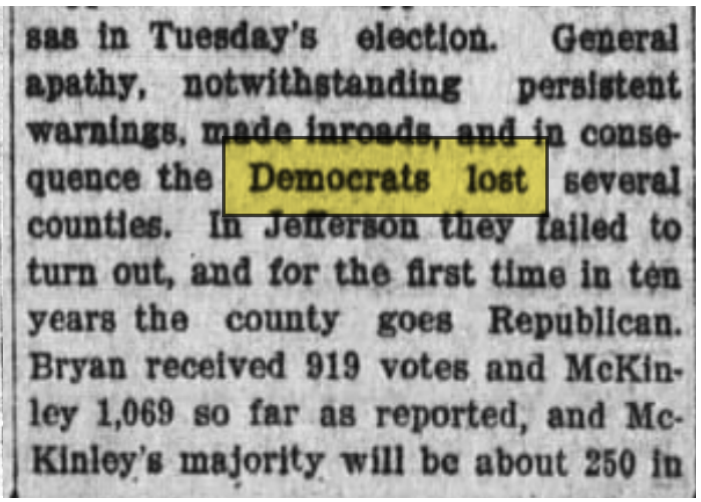
A Close Fight in the County.—Bryan Wins on a Small Margin

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.

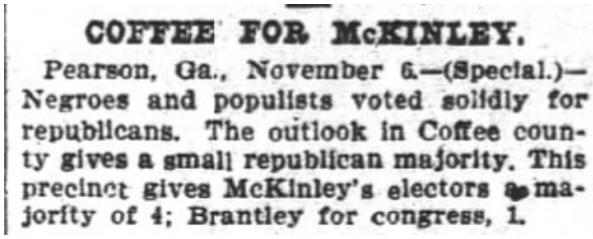
Bryan.....1,349
McKinley.....1,010

(b) Jefferson County, AR (1900)



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

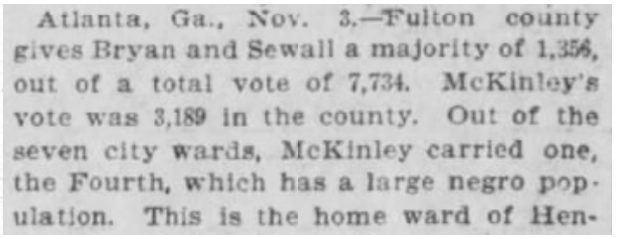
(c) Coffee County, GA (1900)



COFFEE FOR McKINLEY.

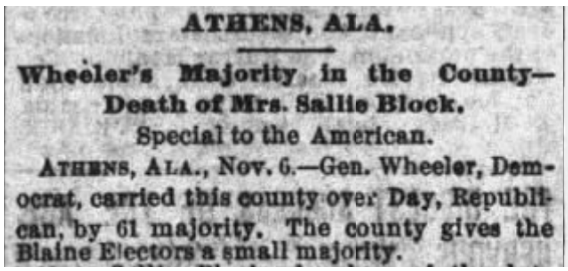
Pearson, Ga., November 6.—(Special.)—Negroes and populists voted solidly for republicans. The outlook in Coffee county gives a small republican majority. This precinct gives McKinley's electors a majority of 4; Brantley for congress, 1.

(d) Fulton County, GA (1896)



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)



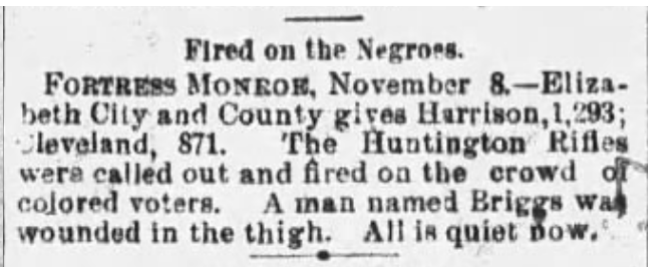
ATHENS, ALA.

Wheeler's Majority in the County—Death of Mrs. Sallie 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.

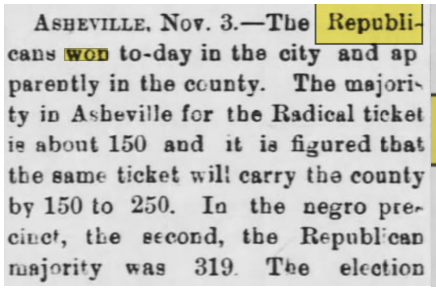
(f) Elizabeth City County, VA (1892)



Fired on the Negroes.

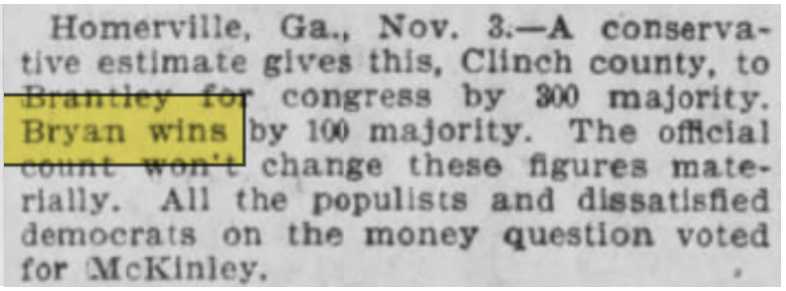
FORTRESS MONROE, November 8.—Elizabeth City and County gives Harrison, 1,293; Cleveland, 871. The Huntington Rifles were called out and fired on the crowd of colored voters. A man named Briggs was wounded in the thigh. All is quiet now.

(g) Buncombe County, NC (1896)



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

(h) Clinch County, GA (1896)



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.

Table A.2: Democratic Losses in Counties and Election Outcome Reporting in Newspapers, 1880–1900

Dependent Variable:	Relative Probability of News Reporting on...									
	Electoral Loss in County					Electoral Win in County				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Democrat Lost County in τ	.103*	.302***	.438***	1.063***	.965***	-.108**	-.249***	-.190	-.294***	-.539***
	(0.056)	(0.056)	(0.14)	(0.060)	(0.16)	(0.051)	(0.056)	(0.14)	(0.038)	(0.17)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Years since election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample newspapers	All reporting	All reporting	All reporting	Democratic	Democratic	All reporting	All reporting	All reporting	Democratic	Democratic
MSE-limited bandwidth	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Vote margin bandwidth	100	7.28	13.32	7.93	10.73	100	9.14	14.99	7.02	10.78
Clustered SE	Yes	Yes	No	Yes	No	Yes	Yes	No	Yes	No
Control outcome mean	0.75	0.69	0.73	0.71	0.74	0.44	0.51	0.49	0.54	0.47
Observations	1,161	167	329	141	211	1,161	210	356	118	211

Notes: This table reports RD estimates for the relative probability of newspapers reporting on an electoral loss (columns 1–5) or win (6–10) in a given newspaper-year during the election period following a Democratic Party loss in a given county in a given presidential election $\tau \in \{1880, ..., 1900\}$. Columns 1 and 6 use all newspaper-years reporting on election outcomes during the sample period (see the notes for Appendix Figure A.2). Columns 2–3 and 7–8 further restrict the sample based on MSE-optimal bandwidth from Calonico et al. (2014). Column 4–5 and 9–10 further condition the sample on a newspaper's partisan affiliation being Democratic. Estimates are based on linear running polynomials. Newspaper reporting collected from 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 year minus most recent election year, and newspaper city or town. Standard errors are clustered at the county level, except in columns 3, 5, 8, and 10, which are heteroskedasticity-robust. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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),

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

recall that $Democratic\ Loss_{c\tau}$ captures whether the Democratic candidate for president lost the popular vote in county c in a given election year τ. By interacting $Democratic\ Loss_{c\tau}$ with a running variable for 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.

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.

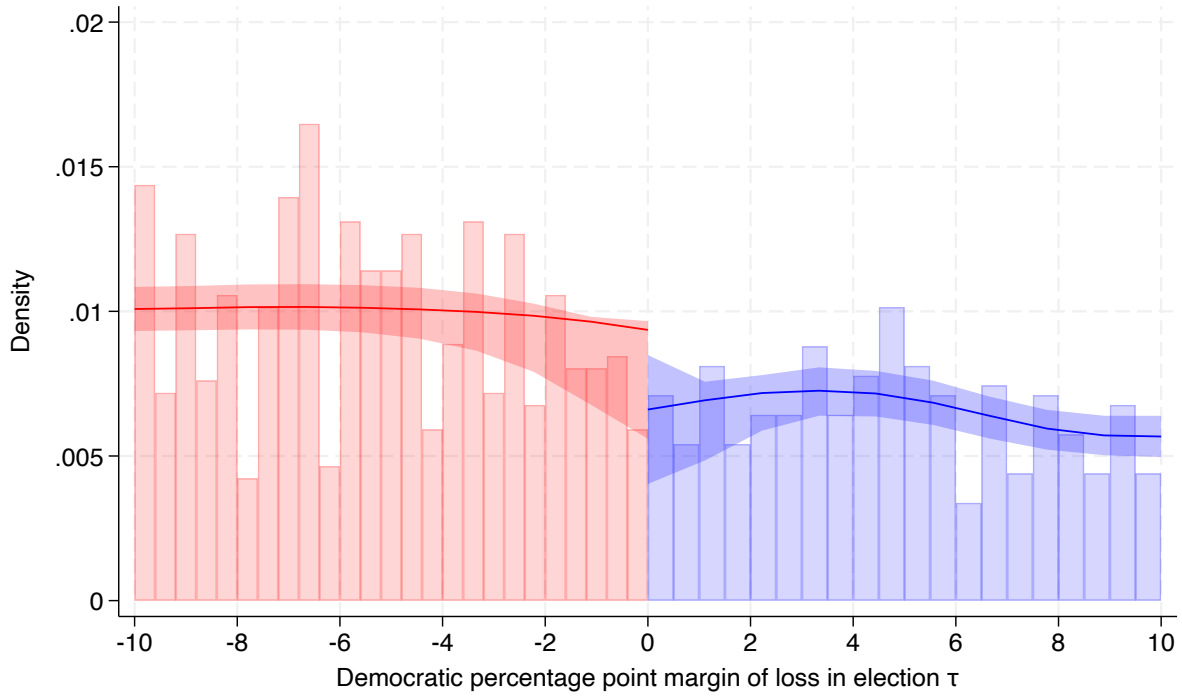
We adopt a linear running polynomial for our main analysis, while also reporting estimates of our main results using a quadratic polynomial where possible through. We furthermore adopt a triangular kernel, which places greater weight on observations close to the Democratic loss threshold, $Loss\ Margin_{c\tau} = 0$.

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 Democratic loss 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 loss 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) and assuming a linear estimating polynomial, we fail to reject the null hypothesis ($p = 0.4$) that the density function of $Loss\ Margin_{c\tau}$ is continuous at the loss threshold.

Of course, 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. Reassuringly, the balance test results in Table 1 also 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 loss threshold in our context. Appendix Table B.1 shows that this remains the case if we instead use a quadratic running polynomial. 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 3.

Figure B.1: McCrary Density Test in Democratic Vote Share Margins



Notes: Figure illustrates the density test from Cattaneo et al. (2018) following McCrary (2008), based on a linear estimating polynomial, using the Democratic margin of loss in presidential elections $\tau \in \{1880, ..., 1900\}$ among our full sample of counties ($p = 0.4$). Error bars represent 95% confidence intervals.

Table B.1: Quadratic Running Polynomials in Table 1

Dependent Variable:	Log Population Density	% Black Population	% Former Slaveholders	% Confederate Veterans	Any Civil War Battles	Average Farm Size	Return on Cotton Potential	Return on Tobacco Potential
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democrat Lost County in Election τ	.109	3.777	.513	.13	.093	96.639	.001	0.00
	(0.10)	(2.51)	(0.46)	(0.53)	(0.071)	(95.1)	(0.002)	(0.001)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	30.75	28.04	35.87	27.01	21.76	31.27	25.98	29.11
Polynomial	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic
Control outcome mean	-11.77	35.34	7.80	30.41	0.18	215.92	0.05	0.06
Observations	2,627	2,457	2,964	2,380	1,985	2,644	2,314	2,528

Dependent Variable:	Percent Aged 5–17	Manufacturing Wages per Capita	Manufacturing Output per Capita	Agricultural Output per Capita	Real Estate per Capita	Personal Property per Capita	State Taxes per Capita	Local Taxes per Capita
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Democrat Lost County in Election τ	-.222	1.044	6.609	0.000	3.185	-1.60	.020	.111
	(0.29)	(0.74)	(4.21)	(2.21)	(8.41)	(3.71)	(0.055)	(0.19)
Election period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	32.09	24.20	23.76	23.92	25.69	30.43	28.23	24.20
Polynomial	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic	Quadratic
Control outcome mean	32.92	2.27	15.32	38.87	93.73	44.71	0.83	0.31
Observations	2,724	2,200	2,161	2,166	2,268	2,581	2,441	2,181

Notes: This table reports bias-corrected local-polynomial RD estimates corresponding to equation (1) for various pre-treatment county-level characteristics. See Section 3.2 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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Data and Sample Robustness

This Appendix describes the dataset and the construction of the sample used for our empirical analysis. Specifically, we (i) show summary statistics for our main variables, (ii) describe our choices of states and election years, (iii) provide further discussion of how our main treatment and outcome variables are coded, and (iv) show some additional robustness exercises affirming these choices.

C.1 Data and Sample

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

Summary Statistics Appendix Table C.1 reports summary statistics for our core sample, restricting the set of observations to those within 50 percentage points around the Democratic loss threshold, such that summary statistics focus on relatively competitive places.

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 be known 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, local elite political interests with respect to minority empowerment varied considerably, even among some 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.

Moreover, local Southern elites were in some areas 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 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 C.1, holding all other aspects of the specification fixed. No particular state appears to be driving our main effect. Point estimates do increase if some states, such as Virginia, North Carolina, or Tennesse,e are omitted from the sample, highlighting the salience of non-Democratic local elite interests in parts of those states. Beyond former Confederate sample states, effects vary by state. In Delaware, Kentucky, and West Virginia, where Democrats had reduced prominence, point estimates in equation (1) are in fact negative, of -.021, -.154, and -.082, respectively. In Maryland and Missouri, estimates are positive but less precise, of .096 and .043, respectively. With these states included in our baseline specification, our overall estimate is positive but small and noisy, of .011 (.025). 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.

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). As with states, our results are not sensitive to omitting any election period during this window, as shown in panel (b) of Appendix Figure C.1.

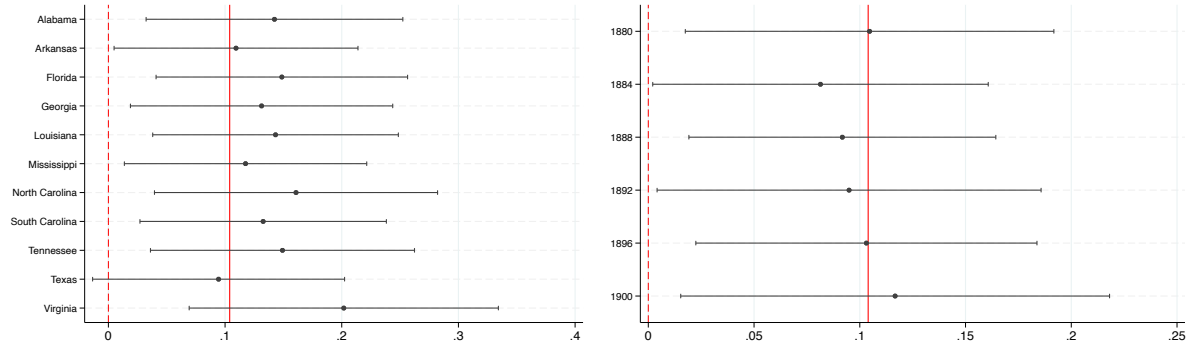
Table C.1: Summary Statistics

	Obs.	Mean	St. dev.	Min.	Max.
Outcome variables					
Any Black lynchings after election τ	3,924	0.14	0.35	0	1
Any White lynchings after election τ	3,924	0.02	0.15	0	1
Democratic victory in the 1904 election	3,924	0.83	0.37	0	1
Democratic victory in the 1908 election	3,924	0.79	0.41	0	1
Democratic victory in the 1912 election	3,924	0.95	0.23	0	1
Voter turnout rate in presidential elections, 1904–12	3,754	34.14	16.93	3.99	82.93
Rate change of Black literate, 1870–1910	3,795	526.22	3029.72	-100	51677.78
Rate change of Black illiterate, 1870–1910	3,797	68	798.68	-100	19166.67
Black school enrollment rate in 1910 (ages 6–14)	3,804	57.74	14.44	0	100
Black literacy rate in 1910	3,833	68.42	10.20	0	100
Black teachers per 100 pupils in the 1910s	2,332	2.24	1.68	0.81	33.33
Non-white voter registration (1962–4)	2,037	19.25	12.27	0	95.12
Non-white voter registration (1966–7)	1,781	27.37	12.54	0	78.07
Controls					
County longitude	3,924	-86.08	6.82	-105.13	-75.65
County latitude	3,924	34.11	2.65	25.42	39.20
Logged population density (per sqr. meter) in 1880	3,863	-11.81	1.13	-20.57	-7.77
% Black population in 1880	3,863	34.48	22.43	0	91.90
% Former slaveholders as of 1880	3,854	7.70	3.93	0	23.38
% Confederate veterans as of 1880	3,854	30.36	4.98	0	100
Any Civil War battles fought in county	3,868	0.17	0.37	0	1
Average farm size (acres per number of farms)	3,815	236.97	583.96	6	9417.50
Farm output per capita	3,825	39.33	18.24	0	143.34
Cotton potential index $\times$ price per pound	3,868	0.06	0.02	0	0.10
Tobacco potential index $\times$ price per pound	3,868	0.06	0.01	0	0.09
% Population ages 5-17	3,863	32.82	2.84	0	50
Manufacturing wages per capita	3,858	2.19	4.41	0	47.34
Manufacturing output per capita	3,858	15.18	28.47	0	456.74
Real estate property per capita	3,799	93.59	57.51	3.20	493.16
Personal property per capita	3,799	43.41	35.85	0	403.36
State taxes per capita	3,798	0.83	0.51	0.18	4.68
Local taxes per capita	3,798	0.28	0.87	0	9.27
Explanatory variables					
Democratic loss in election τ	3,924	0.27	0.44	0	1
Democratic margin of loss in election τ	3,924	-13.26	23.25	-50	49.90
White-only elite at election τ	3,924	0.96	0.20	0	1
Democrat-only elite at election τ	3,924	0.82	0.38	0	1
Any state Jim Crow voting laws election τ	3,924	0.39	0.49	0	1
Newspaper variables					
Frequency of Black crime accusations (% pages)	5,976	0.24	1.60	0	100
Frequency of Black rape accusations (% pages)	5,976	0.02	0.29	0	12.50
Frequency of Black murder accusations (% pages)	5,976	0.16	1.28	0	87.50
Frequency of Black robbert accusations (% pages)	5,976	0.06	0.85	0	50
Democratic newspaper	4,813	0.89	0.31	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.

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

(a) Excluding Individual States **(b)** Excluding Individual Election 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 $\tau \in \{1880, ..., 1900\}$. 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 3 in Table 2 (shown in solid red). Standard errors are clustered at the county level. Error bars represent 95% confidence intervals.

C.2 Variables

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

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 $\tau = \{1880, 1884, ..., 1900\}$. 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 Appendix Table C.2).

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 C.2, 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 C.2 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.

Selective Reporting of Lynchings. Historical lynching records, which comprise the raw sample upon which our analysis is based, have commonly been derived from or corroborated using historical newspaper data. Yet, despite the work of researchers, countless lynchings may still be

forgotten by history. Another, related concern is of selective reporting—with some lynchings having potentially been buried *intentionally* by biased actors. Although there exists no way to directly test for this possibility, we do conduct a version of the test in Appendix Figure B.1, in which we examine the density of counties' average Democratic vote share margin across the 1880–1900 elections for the subsample of lynching-positive counties. Based on this, we fail to reject the null hypothesis ($p = .47$) that this density is continuous at the loss threshold. This is also the case ($p = .79$) if we use only county-years with a Democratic-only local elite (from Table 6). In other words, counties that were electorally competitive during the period of study, on average, exhibit similar probabilities of having recorded at least one lynching, regardless of whether Democrats tended to barely lose or barely win. It stands to reason that such places were not systematically more or less likely to have (selectively) reported on Black lynchings, at least in a way that would confound our estimates.

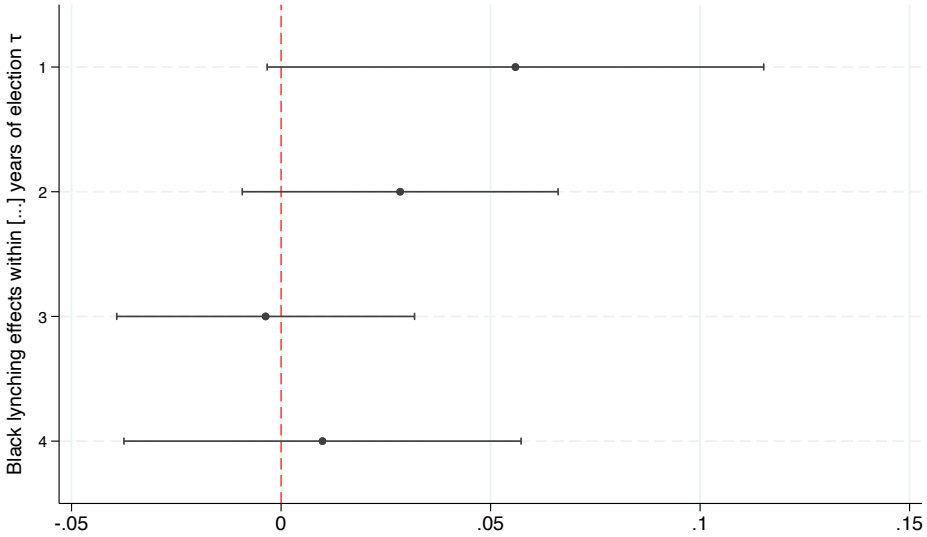
Table C.2: Alternative Outcome Measures in Table 2

Dependent Variable:	Log Black Lynchings		Log Black Lynchings (per 10,000)		Any Black Lynchings	
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Lost in τ	.083*	.116**	.072	.225**	.024**	.034**
	(0.043)	(0.055)	(0.077)	(0.11)	(0.011)	(0.014)
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	17.80	21.77	19.67	19.30	16.45	20.44
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Control outcome mean	0.15	0.16	0.24	0.23	0.04	0.04
Observations	1,665	2,022	1,782	1,742	5,981	7,292

Notes: This table reports bias-corrected local-polynomial RD estimates corresponding to equation (1) using alternative measures of the outcome. Columns 1–2 use logged counts of lynchings in a given county during the four-year election period following a presidential election $\tau \in \{1880, ..., 1900\}$. Columns 3–4 use logged rates of lynchings (per 10,000 Black people) during the four-year election period following τ. Columns 5–6 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 linear (odd columns) and quadratic (even) 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 5–6 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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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 $\tau = \{1880, 1884, ..., 1900\}$. 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). For the 1896 election, in which William Jennings Bryan was nominated by multiple parties, we supplement these data with information from Robinson (1934). Treatment and running variable values are

Figure C.2: Dynamic Effects Within Election Periods



Notes: This figure reports bias-corrected local-polynomial 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 $\tau \in \{1880, ..., 1900\}$, 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 linear 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 95% confidence intervals.

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 analysis sample conditional upon the bandwidth of the analysis. For instance, only about one fifth 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 across the subsequent four-year election period. For our primary lynching outcome variable, electoral periods consist of a four-year effect window that begins after the conclusion of the November of a given election year from 1880 through 1900. The exact choice of timing is ultimately of little consequence (see, for instance, rows 1–3 of Appendix Table C.3), which reflects the fact that few sample lynchings (i.e., less than 1%) occur during an election month (see row 7).

Table C.3: Sensitivity and Placebos of Effect Windows in Main RD Estimates

Dependent Variable:	Any Black Lynchings (1)	Any White Lynchings (2)
	(a) Outcome Includes Lynchings During Election Period After...	
1. December of Election Year τ	.102**	-.009
	(0.041)	(0.013)
2. November of Election Year τ	.104**	-.008
	(0.041)	(0.013)
3. October of Election Year τ	.084**	-.009
	(0.039)	(0.013)
4. September of Election Year τ	.082**	-.009
	(0.037)	(0.013)
5. August of Election Year τ	.078**	-.007
	(0.035)	(0.013)
6. July of Election Year τ	.066*	-.007
	(0.034)	(0.013)
	(b) Outcome Excludes Lynchings During...	
7. All Election Months	.080**	-.009
	(0.038)	(0.013)
	(c) Placebo: (Partial) Pre-Treatment Effect Windows	
8. 1 Year Before November of Year τ	.071**	-.010
	(0.035)	(0.011)
9. 2 Years Before November of Year τ	.065**	-.022**
	(0.033)	(0.011)
10. 3 Years Before November of Year τ	.051*	-.015
	(0.030)	(0.014)
11. 4 Years Before November of Year τ, w/ Lynchings Fully Pre-Treatment	.003	-.013
	(0.027)	(0.014)
	(d) Placebo: Delaying Effect Window by an Election Period	
12. 4 Years After November of Year τ, w/ Lynchings 4–8 Years Post-Treatment	-.024	.003
	(0.038)	(0.021)

Notes: This table reports bias-corrected local-polynomial 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 in November of year $\tau \in \{1880, ..., 1900\}$. Rows vary when these electoral period windows begin, as specified in the row headers. 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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Lack of sensitivity to exact cutoff date is also natural considering the presence of some measurement error in reporting dates. Recorded lynching dates often vary somewhat across sources (e.g., different newspapers), with incorrect dates used or delays in reporting by days or weeks. For this reason, we limit to months as the most granular unit of temporal analysis.

In other cases, the act of lynching was carried out weeks after the underlying grievance was aired. In one example of the latter, Duncan McPherson, a Black North Carolina man alleged to be affiliated with a third-party group, was originally wanted by police in 1892 in connection

with an Election Day "disturbance"; this led to a series of events that culminated in McPherson being lynched over a week later, on November 17.[1] In this instance, a lynching may in fact be the fulfillment of pre-election threats made with the intension of *causing* Democratic victory in that election. Consistent with the possibility of some anticipatory effects, Appendix Table C.3 shows a gradual attenuation of estimates as we vary the start of election periods to earlier and earlier months, including to pre-election months (rows 4–6) and years (rows 8–10). Estimates fully converge to zero when *all* lynchings coded to the outcome's electoral period *precede* the electoral treatment date (row 11), as well as when they are delayed by a full election period (row 12). Additional exercises in the paper, particularly those in Table 4 on unexpected electoral losses, further control for such anticipatory effects.

County Identifiers and Boundary Harmonization. Because we are interested in county-level electoral results, as well as 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* those in the raw lynching records from Seguin and Rigby (2019) or the Historic American Lynching (HAL) Project from Hines and Steelwater (2023). That said, 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 between, on one hand, election records (for a given election year τ) and, on the other, 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 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). Thus, a very small number of Southern lynchings during the sample period ultimately remain out of our analysis sample.

More generally, our identification strategy precludes harmonizing county boundaries to a single, common year. This is because it is essential for our RD strategy that electoral margins (and the local information they represent) reflect their true values for a given county-election period. As a result, for the purpose of defining clusters for inference, 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 3). That being said, we do harmonize boundaries for longer-run

[1] As reported in "Horse and Halter," *Oxford Public Ledger*, November 25, 1892, p. 2.

data, for which boundaries are likely to differ more greatly (e.g., in Tables 9 and E.2), 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 3 are unaffected if we restrict the sample to the set of county identifiers with land areas that are unchanged over the sample period.

Unit of Analysis. As described in Section 3.2, our core estimation is based on county-level vote shares from presidential elections. This precludes the possibility for election results to have direct impacts on local Democratic power, including local policy, which would attenuate our effects. Such is the case, for instance, for our analysis in Appendix Table C.5 below, which uses congressional elections. Indeed, if the Democratic candidate were to win in a congressional election, they might choose to use the power of the office to reduce protections against racial violence. Counties also offer a far larger sample size, characterized by more significant local competition between Democrats and other major political parties, than congressional districts. Finally, the lynching records on which we rely are primarily coded at the county level, and congressional district and county boundaries are often not congruent. Such inconsistencies further result in a loss of sample size for the congressional analysis below. For further detail on this latter concern, see our discussion of boundary harmonization above.

Table C.4: Conditioning on the (Populist) Opposition

Dependent Variable:	Any Black Lynchings After Election τ			
Primary Opposition:	Any		Populist	
	(1)	(2)	(3)	(4)
Democrat Lost in Election τ	.103**	.160***	.504**	.542**
	(0.041)	(0.060)	(0.24)	(0.25)
Election period FE	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes
Optimal bandwidth	17.47	18.66	11.59	23.53
Polynomial	Linear	Quadratic	Linear	Quadratic
Control outcome mean	0.13	0.14	0.14	0.17
Observations	1,375	1,461	75	147

Notes: This table reports bias-corrected local-polynomial RD estimates corresponding to equation (1) for whether there were any Black lynchings in a given county following a given election, conditional on its highest vote-receiving non-Democrat being of any non-Democratic party affiliation (columns 1–2) or part of the third-party "populist coalition" (columns 3–4). The latter is coded based on candidate affiliation with the People's Party, Greenback Party, Prohibition Party, (Union) Labor Party, Readjuster Party, or other non-Democratic and non-Republican independents. Sample is based on four-year election periods following a presidential election $\tau \in \{1880, ..., 1900\}$, excluding the 1896 election in which Democrats and the populist coalition were aligned. Estimates are based on linear (odd columns) and quadratic (even) 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 C.5: Beyond Informational Effects: Congressional Districts

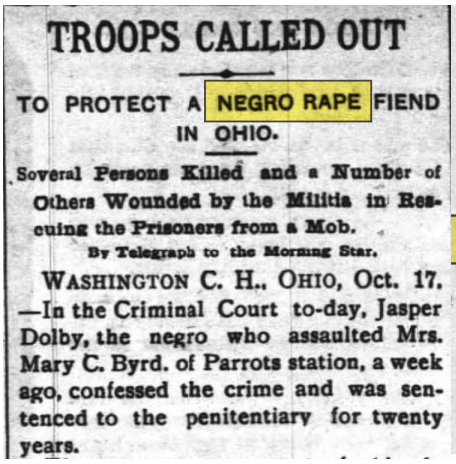
Dependent Variable:	Any Black Lynchings After Election τ					
Primary Opposition:	Any			Populist		
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Lost in Election τ	.017	.017	.020	.401*	.510***	.802***
	(0.15)	(0.14)	(0.15)	(0.22)	(0.13)	(0.29)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes
Exclude midterm elections	No	Yes	No	No	Yes	No
Exclude 47th Congress	No	No	Yes	No	No	Yes
Optimal bandwidth	13.36	6.78	14.41	11.16	21.19	15.93
Control outcome mean	0.42	0.40	0.46	0.53	0.33	0.49
Observations	227	70	219	42	34	54

Notes: This table reports bias-corrected local-polynomial RD estimates corresponding to equation (1) for whether there were any Black lynchings in a given congressional district following a given election, conditional on its highest vote-receiving non-Democrat being of any non-Democratic party affiliation (columns 1–3) or part of the third-party "populist coalition" (columns 4–6). The latter is coded based on candidate affiliation with the People's Party, Greenback Party, Prohibition Party, (Union) Labor Party, Readjuster Party, or other non-Democratic and non-Republican independents. Sample is based on two-year election periods following a national election $\tau \in \{1880, ..., 1900\}$, except for columns 3 and 6, which exclude 1880. Lynchings are mapped to congressional district boundaries using the area-based crosswalks from Ferrara et al. (2021). 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 district longitude and latitude. Standard errors are heteroskedasticity-robust. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D Expanding on Newspaper Analysis

Figure D.1: Examples of Newspaper Data

(a) "Negro rape"



TROOPS CALLED OUT

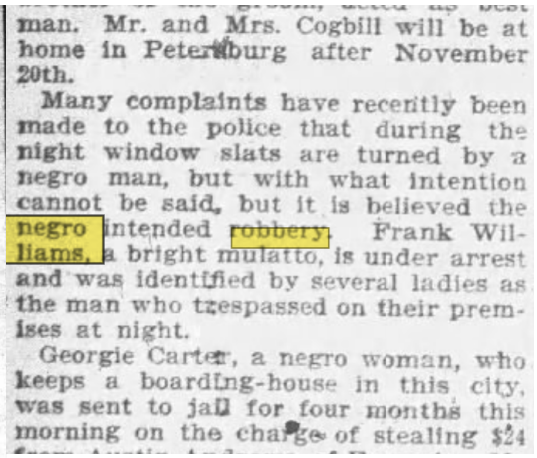
TO PROTECT A NEGRO RAPE FIEND IN OHIO.

Several Persons Killed and a Number of Others Wounded by the Militia in Rescuing the Prisoners from a Mob.

By Telegraph to the Morning Star.

WASHINGTON C. H., OHIO, Oct. 17.—In the Criminal Court to-day, Jasper Dolby, the negro who assaulted Mrs. Mary C. Byrd. of Parrots station, a week ago, confessed the crime and was sentenced to the penitentiary for twenty years.

(b) "Negro robbery"

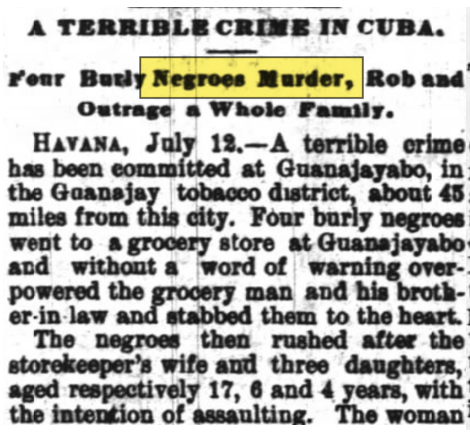


man. Mr. and Mrs. Cogbill will be at home in Petersburg after November 20th.

Many complaints have recently been made to the police that during the night window slats are turned by a negro man, but with what intention cannot be said, but it is believed the negro intended robbery. Frank Williams, a bright mulatto, is under arrest and was identified by several ladies as the man who trespassed on their premises at night.

Georgie Carter, a negro woman, who keeps a boarding-house in this city, was sent to jail for four months this morning on the charge of stealing $24

(c) "Negroes murder"

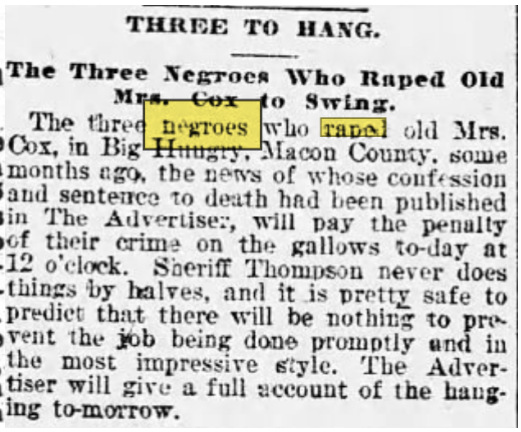


A TERRIBLE CRIME IN CUBA.

Four Burly Negroes Murder, Rob and Outrage a Whole Family.

HAVANA, July 12.—A terrible crime has been committed at Guanajayabo, in the Guanajay tobacco district, about 45 miles from this city. Four burly negroes went to a grocery store at Guanajayabo and without a word of warning overpowered the grocery man and his brother-in-law and stabbed them to the heart. The negroes then rushed after the storekeeper's wife and three daughters, aged respectively 17, 6 and 4 years, with the intention of assaulting. The woman

(d) "Negroes raped"



THREE TO HANG.

The Three Negroes Who Raped Old Mrs. Cox to Swing.

The three negroes who raped old Mrs. Cox, in Big Hungry, Macon County, some months ago, the news of whose confession and sentence to death had been published in The Advertiser, will pay the penalty of their crime on the gallows to-day at 12 o'clock. Sheriff Thompson never does things by halves, and it is pretty safe to predict that there will be nothing to prevent the job being done promptly and in the most impressive style. The Advertiser will give a full account of the hanging to-morrow.

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.

Table D.1: Identification and Robustness Checks on RD Estimates in Table 5

Dependent Variable:	Frequency of Anti-Black Crime Accusations (% Pages)	
Running Polynomial:	Linear	Quadratic
	(1)	(2)
	(a) Alternative Standard Errors	
1. Baseline (Columns 3 and 4 of Table 5)	.168***	.136**
Clustering by County	(0.042)	(0.056)
Clustering by County-Decade	(0.046)	(0.051)
Clustering by State-Election-Period	(0.049)	(0.055)
	(b) Alternative Control Sets	
2. No Controls or Fixed Effects	.081	.064
	(0.076)	(0.078)
3. No City FE w/ State FE	.126*	.139**
Quadratic Polynomial in County Longitude and Latitude	(0.073)	(0.069)
4. No City FE w/ State FE	.127*	.138**
Quadratic Polynomial in City Longitude and Latitude	(0.072)	(0.069)
5. Baseline w/ County-Pair Fixed Effects,	.152***	.131**
Matched on Proximity in Longitude and Latitude	(0.044)	(0.056)
6. Controlling for Quadratic Polynomial in	.129***	.111**
1880 Black Population Shares	(0.041)	(0.056)
7. Controlling for All Variables From Table 1	.125***	.115**
	(0.041)	(0.056)
8. Baseline w/ State $\times$ Pre-Jim Crow FE	.160***	.120**
	(0.047)	(0.055)
9. Baseline w/ County Spatial Covariates $\times$ Pre-Jim Crow	.144***	.139***
	(0.046)	(0.048)
	(c) Alternative RD Specifications	
10. Optimal Bandwidth$\times 0.5$	.200***	.244***
	(0.035)	(0.039)
11. Optimal Bandwidth$\times 1.5$	.150***	.129***
	(0.050)	(0.048)
	(d) Alternative Outcomes	
12. Strict Measure:	.001	.021*
"Negro" + "Rape," "Murder", and/or "Robbery"	(0.0070)	(0.011)
13. Phrase Measure:	.138***	.069
"Negro" + "Raped," "Murdered", and/or "Robbed"	(0.038)	(0.045)
14. Plural Phrase Measure:	.049***	.049***
"Negroes" + "Raped," "Murdered", and/or "Robbed"	(0.011)	(0.015)
15. Placebo Measure:	-.458	-.322
"Rape," "Murder", and/or "Robbery"	(1.17)	(1.50)
	(e) Alternative Samples	
16. Excluding Observations w/ % > $\mu + \sigma$	.033**	.066***
	(0.017)	(0.020)
17. Excluding Observations w/ % > $\mu + 2\sigma$	.046**	.081**
	(0.023)	(0.032)

Notes: This table reports bias-corrected local-polynomial RD estimates for the frequency of anti-Black crime accusations in a given newspaper-year during the four-year election period following a presidential election $\tau \in \{1880, ..., 1900\}$. Estimates are based on linear (column 1) and quadratic (column 2) running polynomials and the MSE-optimal bandwidth from Calonico et al. (2014). All regressions include fixed effects for election period, newspaper publication year minus most recent election year, and newspaper city or town, unless otherwise specified in panel (b). Standard errors are clustered at the county level, unless otherwise specified in panel (a). See Section 4.2 for a detailed overview of the items in each row. Significance levels are denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E Additional Results

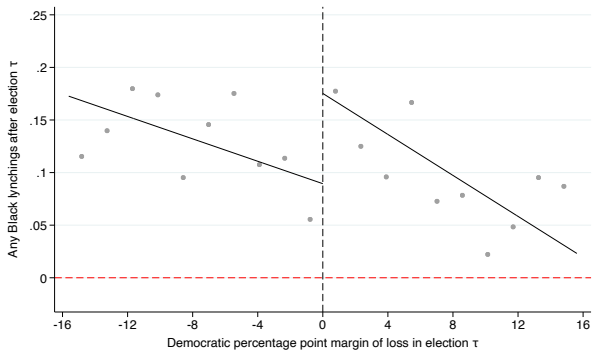
E.1 Robustness and Heterogeneity in Section 3

This Appendix reports alternative versions and extensions of our many results.

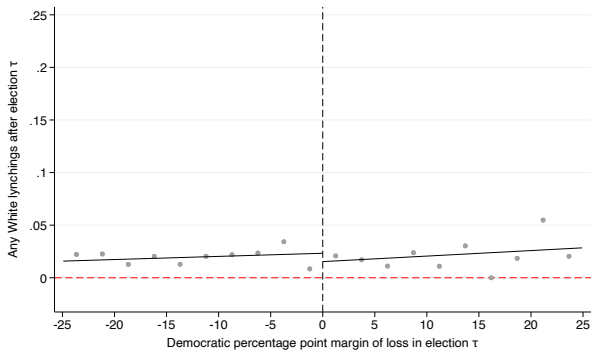
To begin, Appendix Figure E.1 presents alternative versions of our main RD plots, using (i) the RD specifications and bandwidths from panel (a) of Table 2 and (ii) the restricted sample from panel (b) of Table 2.

Figure E.1: Replicating Figure 5 with Alternative Specifications and Samples

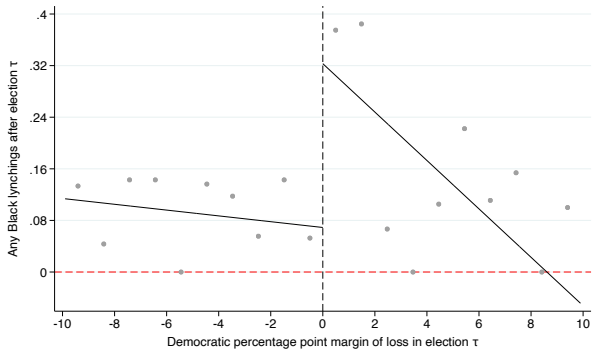
(a) Black Lynchings, Replicating Main RD



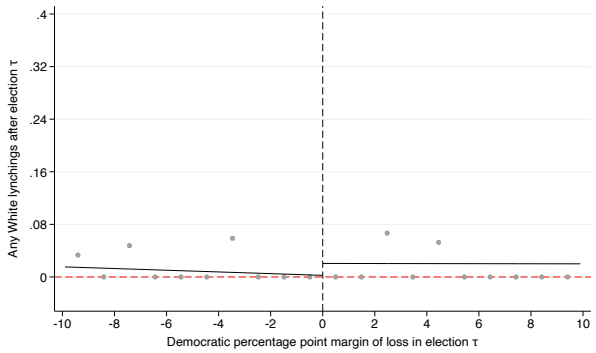
(b) White Lynchings, Replicating Main RD



(c) Black Lynchings, Uncompetitive Sample



(d) White Lynchings, Uncompetitive Sample



Notes: Binned estimates of the probability of Black and white lynchings during the four-year election period following a presidential election $\tau \in \{1880, ..., 1900\}$ by the Democratic margin of loss 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 3 and 7, 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.

E.2 Robustness and Heterogeneity in Section 4

Table E.1: Lynchings and Anti-Black Newspaper Stories by Accusation Type

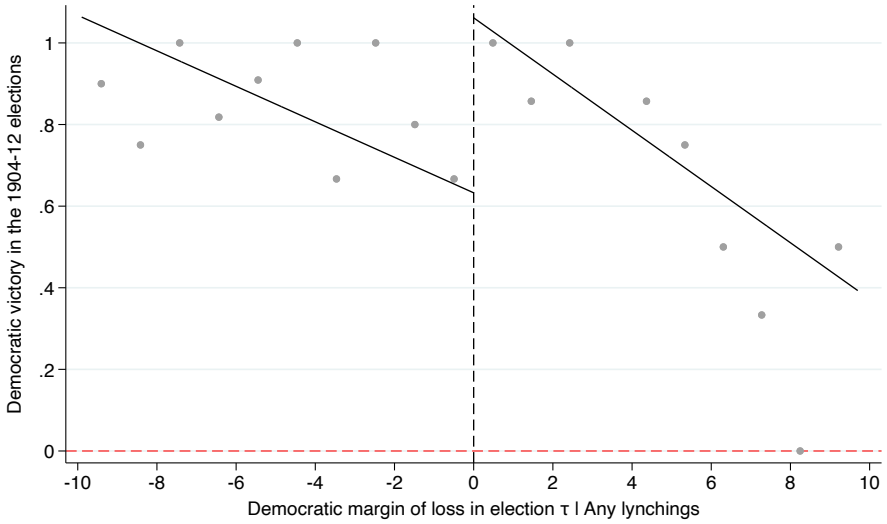
Accusation Type:	Sex		Murder		Property	
	(1)	(2)	(3)	(4)	(5)	(6)
	(a) Dep. Var: Any Black Lynchings After Election					
Democrat Lost in Election τ	.051*	.083**	.048	.065*	.002	.003
	(0.027)	(0.037)	(0.032)	(0.039)	(0.011)	(0.011)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	No	No	Yes	Yes	Yes	Yes
Optimal bandwidth	24.65	24.72	15.47	20.72	17.14	29.51
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Control outcome mean	0.07	0.07	0.05	0.06	0.01	0.01
Observations	2,267	2,270	1,464	1,925	1,610	2,600
	(b) Dep. Var: Frequency of Anti-Black Crime Accusations (% Pages)					
Democrat Lost in Election τ	.028**	.052***	.035	.029	.042***	.011
	(0.014)	(0.018)	(0.032)	(0.046)	(0.012)	(0.013)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes
News year − Election year FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	11.47	12.46	12.46	14.44	14.58	23.66
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Control outcome mean	0.02	0.02	0.12	0.12	0.05	0.07
Observations	2,109	2,235	2,235	2,552	2,563	3,859

Notes: This table re-estimates Tables 2 and 5 conditional on the type of criminal accusation made underlying a given lynching event (in panel a) and associated with a given newspaper story (panel b). The vast majority of accusations made are of sex- (columns 1–2), homicide- (columns 3–4), and property-related (columns 5–6) crimes. Estimates are based on linear (odd columns) and quadratic (even) 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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

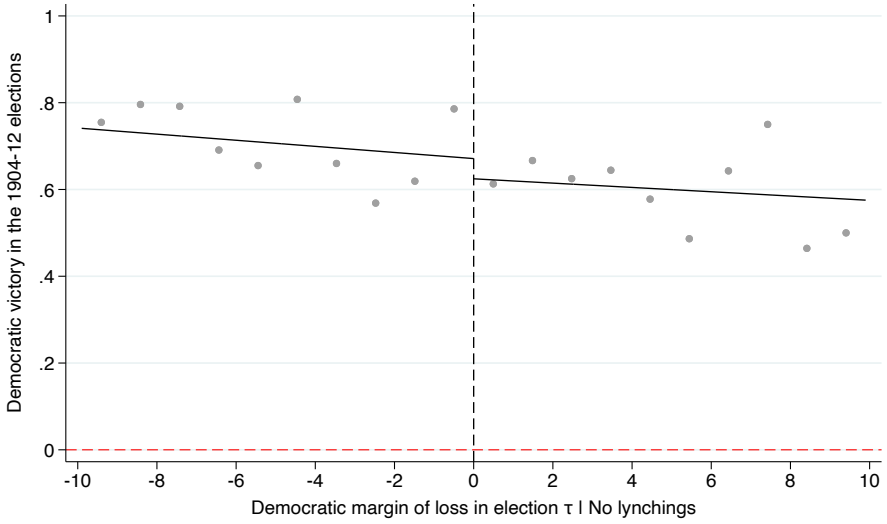
E.3 Robustness and Heterogeneity in Section 5

Figure E.2: 20th Century Democratic Wins By Loss Margins, 1880–1900

(a) Pr(Wins in 1904–12 | Any Black Lynchings)

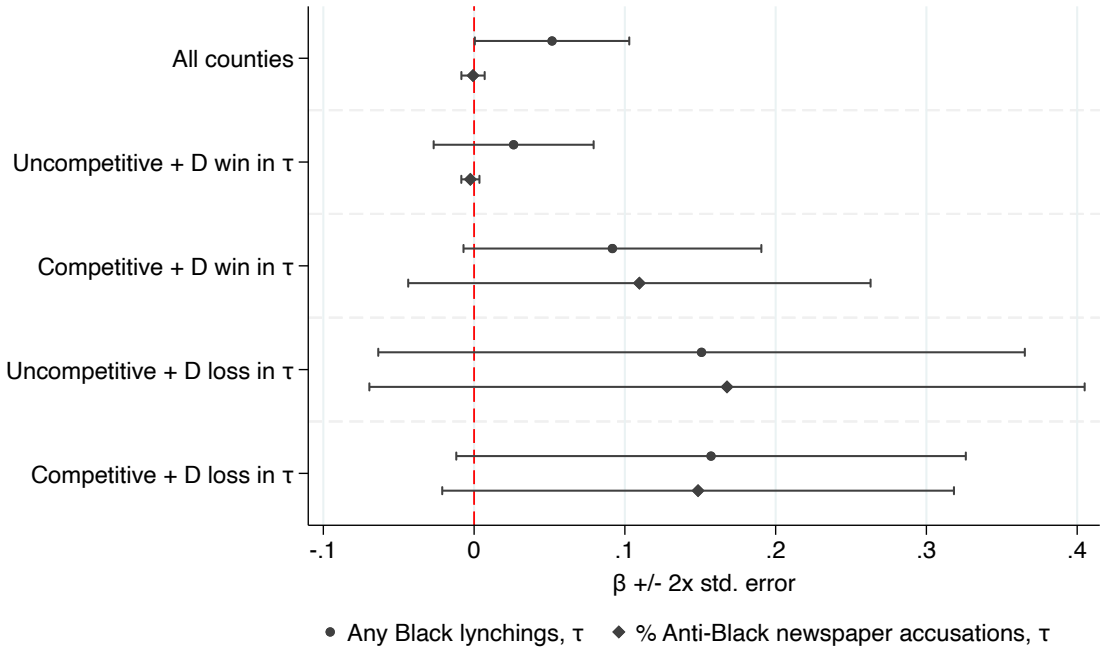


(b) Pr(Wins in 1904–12 | No Black Lynchings)



Notes: Binned estimates of the probability of a Democratic electoral victory in a county during the 1904–1912 presidential elections by the Democratic margin of loss in presidential election $\tau \in \{1880, \ldots, 1900\}$, conditional on a lynching having occurred in a given county in the four-year period following τ (panel a) versus none having occurred (panel b). 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 8. For context, the probability that the Democratic candidate won in a given county among former Confederate states was 0.87 in 1904, 0.84 in 1908, and 0.95 in 1912.

Figure E.3: Estimating Figure 8 Using Local Democratic Officeholders



Notes: Estimates of the number of Democratic local officeholders in a given county across the 1904–12 election periods by whether at least one Black lynching occurred in that county and the average frequency of anti-Black crime accusations in newspapers during the four-year election period following a presidential election $\tau \in \{1880, ..., 1900\}$. Row labels correspond to different conditional effects, where "competitive" conditions on the set of electorally-competitive counties in the most recent presidential election τ, within the median vote margin among sample Democratic electoral losses ($|Loss\ Margin_c| = 16.2$) as the cutoff for the former. All regressions include election period fixed effects, state fixed effects, and quadratic polynomials in county longitude and latitude, as well as the total number of unique local officeholders in the county over the outcome period. Standard errors are clustered at the county level. Error bars represent 95% confidence intervals.

Table E.2: Lynching Counties and Political Participation Amid Jim Crow's Rise and Fall

Dependent Variable:	Voter Turnout (% Eligible Voters) in [...]				Registered [...] Voters (% [...] in 1960)			
					White	Non-White	White	Non-White
	1904–12	1904	1908	1912	1962–64		1966–67	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(a) Counties with Any Black Lynchings							
Democrat Lost in Election τ	-5.984**	-5.895*	-5.543*	-2.324	.936	-4.383	-6.662	-10.768***
	(2.70)	(3.25)	(2.84)	(2.27)	(4.00)	(3.08)	(6.91)	(2.90)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	17.90	23.12	19.36	24.22	22.74	12.55	17.62	13.92
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Control outcome mean	30.55	27.80	31.46	28.54	45.62	19.43	55.83	28.66
Observations	188	298	227	305	162	81	96	79
	(b) Counties with No Black Lynchings							
Democrat Lost in Election τ	-.611	.319	1.42	-.152	-1.437	-2.23	-3.197	-.588
	(1.54)	(1.79)	(1.49)	(1.77)	(2.63)	(1.83)	(3.81)	(2.81)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	16.86	20.67	24.99	14.91	21.48	20.53	18.00	18.88
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Control outcome mean	35.59	35.19	37.14	33.78	45.56	19.75	57.54	27.21
Observations	1,359	1,663	1,971	1,250	869	839	671	694

Notes: This table reports bias-corrected local-polynomial RD estimates corresponding to equation (1) for county-level rates of voter turnout in presidential elections in 1904–12 (column 1), 1904 (column 2), 1908 (column 3), and 1912 (column 4) and county-level rates of voter registration in 1962–64 (columns 5–6) and 1966–67 (7–8) among whites (columns 5 and 7) and non-whites (columns 6 and 8). Regressions in panel (a) restrict to counties in which a lynching occurred at some point during the four-year period following τ, while those in panel (b) restrict to the complementary cases without lynchings. 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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E.3: Re-estimating Table 8 and 9 Using Counties with Similar Black Shares in 1880

Dependent Variable:	Democrat Won in...		Black Literacy Rate,	Black School Enrollment,	Teachers per 100 Black Pupils,	Rate of [...] Population Change, 1870–1910	
	1904	1908	1910	1910	1910s	Literate Black	Illiterate Black
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	(a) Counties with Any Black Lynchings Following τ						
Democrat Lost in τ	.177**	.197**	-4.923***	-10.733***	-.253*	-414.763*	-121.729*
	(0.087)	(0.094)	(1.87)	(3.50)	(0.14)	(229.7)	(72.8)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched sample?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	21.91	28.26	25.39	23.05	23.53	31.18	21.88
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Control outcome mean	0.93	0.91	67.22	56.10	1.90	358.72	34.47
Observations	261	338	311	294	187	358	260
	(b) Counties with No Black Lynchings Following τ						
Democrat Lost in τ	.002	-.161	-2.429	-1.810	1.151	-382.827	-105.164
	(0.11)	(0.14)	(3.56)	(4.09)	(0.76)	(493.7)	(314.2)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched sample?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	21.91	28.26	25.39	23.05	23.53	31.18	21.88
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Control outcome mean	0.87	0.80	65.84	59.39	1.94	459.39	151.97
Observations	223	294	275	261	149	312	228

Notes: This table reports bias-corrected local-polynomial RD estimates corresponding to equation (1) for whether there was a Democratic electoral victory in a given county in the 1904 (columns 1) and 1908 (columns 2); literacy rates in 1910 among Blacks (column 3); school enrollment rates in 1910 among Blacks (column 4) aged 6–14; the number of Black teachers per 100 Black pupils in schools in the 1910s (column 5); the rate of population change in the number of literate Black people in 1910 since 1870 (column 6); and the rate of population change in the number of non-literate Black people in 1910 since 1870 (column 7). Regressions in panel (a) restrict to counties in which a Black lynching occurred at some point during the four-year period following τ, while regressions in panel (b) restrict to the complementary cases without Black lynchings. Both panels restrict to a sample of counties matched within election periods based on similarity in on Black population shares in 1880, as described in Section 5. 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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.