POLITICAL FOUNDATIONS OF RACIAL VIOLENCE IN THE POST-RECONSTRUCTION SOUTH*

PATRICK A. TESTA AND JHACOVA WILLIAMS

Election results act as powerful signals, shaping social behavior in ways that can be dramatic and even violent. This article 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. Despite no corresponding changes in local officeholding, these defeats were salient among local elites. We show that Southern newspapers, closely aligned with the Democratic Party, amplified narratives of Black criminality in the aftermath of Democratic losses. Such accusations were, in turn, frequently invoked by lynch mobs. These findings point to the strategic use of racial violence by Democratic elites, foreshadowing the institutionalized vote suppression of Jim Crow. *JEL codes:* N31, D72, J15, I31, O10, D83.

The 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)

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I. 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 twentieth 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 U.S. history, considerable debate exists over its underlying causes. Contemporary observers viewed it as an instrument for stifling Black empowerment after emancipation (Wells 1892, 1895; Cutler 1905; Johnson 1924). 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, Troesken, and Walsh 2017). Prevailing accounts focus on the role of negative economic shocks (Raper 1993; Tolnay and Beck 1995) and enforcement of traditional racial norms (Brundage 1993) in describing lynchings' deeper roots.

We present evidence that political factors systematically shaped patterns of racial violence across the South. After Reconstruction ended in 1877, a resurgent anti-Black Democratic Party faced local challenges from groups seeking to build multiracial coalitions (Kousser 1974). We show that in counties where Democrats lost the presidential popular vote, lynchings of Black people surged, even though such outcomes did not alter local officeholding. This pattern indicates that competitive elections in the pre–Jim Crow South influenced social behavior in ways that extended well beyond their direct effects 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 prob-

1. As is standard, this study 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 I.

ability 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—with 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, 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 the results of presidential elections in counties had 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—nor do we find evidence of endogenous sorting among counties around the threshold. To explain these findings, we develop a conceptual framework, proposing two key factors underlying 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, helping facilitate mobilization among (pro-Black) opposition (Anagol and Fujiwara 2016; Granzier, Pons, and Tricaud 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 with 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 by relatively unexpected losses, which reveal novel Democratic weakness in the face of relatively pro-Black political opposition. Meanwhile, effects are attenuated for Democratic losses in congressional elections, which actually put a member of the opposition in office, with the power to potentially combat lynching.

Second, we explore the role of local elites in galvanizing racial violence. We show that Southern newspapers—which actively reported on the results of presidential elections in counties and usually had strong 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 after Reconstruction and are positively associated with Black lynchings in our data, were frequently invoked by lynch mobs. We also consider various 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-Democratic, all-white 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.

Last, we show that Democrats had a larger electoral imprint during the early twentieth century in counties that experienced postelectoral racial violence. Whereas a worse electoral performance by Democrats in presidential elections between 1880 and 1900 predicts a lower probability of Democratic victory between 1904 and 1912 among counties where racial violence did not materialize, places where Black lynchings did occur after presidential elections were more likely to be won by the Democrats in the early twentieth century. Using a causal mediation analysis rooted in our RD design, we confirm that Black lynchings had a positive and significant mediating effect on the probability of Democratic victory between 1904 and 1912, helping bring about an electoral reversal of fortune for Southern Democrats. This effect worked through reductions in local voter turnout, echoing Jones, Troesken, and Walsh (2017) on lynching and Black political participation. Importantly, our findings suggest that such effects had strategic underpinnings, with small yet pivotal decreases in Democratic vote shares around the win-lose threshold fueling racial antagonism after an election. By contrast, Jones, Troesken, and Walsh (2017), whose focus on far larger differences in vote shares assumes a similar effect on lynchings for close wins and losses, do not detect such political motivations.

Indeed, we offer the first quantitative evidence for the importance of political factors in explaining the racial violence of the post-Reconstruction U.S. South, validating the early obser-

vations of contemporary journalists (Wells 1892, 1895; Johnson 1924) and sociologists (Cutler 1905; Blalock 1967; Reed 1972; Corzine, Creech, and Corzine 1983). These findings help explain lynching's well-documented political effects (Jones, Troesken, and Walsh 2017; Williams 2022), while corroborating existing descriptive evidence for political foundations (Olzak 1990; Hagen, Makovi, and Bearman 2013; Epperly et al. 2020).[3] This contrasts with the dominant, economic explanation for lynching, tying it to Black-white competition in the struggling postbellum cotton sector (Tolnay, Beck, and Massey 1989; Raper 1993; Tolnay and Beck 1995; Feigenbaum, Mazumder, and Smith 2020), as well as recent work emphasizing perceived Black violations of traditional racial norms and laws (Jones, Troesken, and Walsh 2017; Masera, Rosenberg, and Walker 2022), which were often proximate to the incitement of lynch mobs. Although we do not dispute an influence of these factors, our results affirm the importance of political ones. The latter were arguably first order: without 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. 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 antiminority sentiment through media (Yanagizawa-Drott 2014; Adena et al. 2015; Voigtlander and Voth 2015; Blouin and Mukand 2019; Wang 2021), particularly in the U.S. context (Ang 2023; Bazzi et al. 2023; Esposito et al. 2023). Masera, Rosenberg, and Walker (2022) examine the spread of anti-Black narratives and violence in response to fears of racial 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

3. Other empirical work on postbellum racial violence includes Albright et al. (2021) on Black wealth; Bazzi et al. (2022) on Southern white migration; Chyn, Haggag, and Stuart (2024) on Freedmen's Bureaus; Cook, Logan, and Parman (2018) on segregation; Logan (2023) on tax policy; and Williams, Logan, and Hardy (2021) on regional inequality. For work among historians, see Brundage (1993), Pfeiffer (2004), Berg (2011), Wood (2011), and Lancaster (2014).

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. Along with Masera, Rosenberg, and Walker (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.[4] Turning to contemporary relevance, our findings resonate with work on anti-immigrant rhetoric in Europe and the United States 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, Bischof, and Wagner 2024). This mirrors the use of antiminority politics by both major political parties through the Jim Crow era—during which pro-Black voices remained silent 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 (Ferreira and Gyourko 2014; Baskaran and Hessami 2018; Bochenkova, Buonanno, and Galletta 2023). Differing from previous work on close elections as a form of quasi-experiment for studying the policy effects of officeholders (Lee, Moretti, and Butler 2004; Pettersson-Lidbom 2008), we study an electoral unit—counties in presidential elections—with no direct effect on political outcomes at all. This closely follows Anagol and Fujiwara (2016) and Granzier, Pons, and Tricaud (2023), who document positive effects of candidate rank among election losers on success in subsequent contests. Although we share in their focus on the salience of electoral rankings, this article 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 echo Bhalotra, Clots-Figueras, and Iyer (2017), wherein women's electoral success likewise serves to embolden a more antiwoman electorate.

4. More broadly, a body of work finds episodes of white backlash have tended to follow Black political empowerment after the Civil War (Kuziemko and Washington 2018; Ang 2019; Bernini et al. forthcoming; Ramos-Toro forthcoming).

II. 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 after the American Civil War (1861–65). We outline a conceptual framework to explicate general mechanisms through which increases in power among minority groups, such as Southern Black people, may kindle local violence.

II.A. 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, along 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.[5]

1. *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 (Du Bois 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.[6] The investigations documented vast acts

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

6. 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: "They 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,

of racial terror committed by KKK members 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. 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. 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 citizens. Insofar as local authorities had failed to address racial violence, the Enforcement Acts meant that victims and survivors of racial terror could now use federal courts to bring lawsuits against their perpetrators (Frantz 1964; Gardner 2016). By expanding the reach of federal power, the acts also ensured more impartial adjudication of cases related to KKK-committed atrocities and weakened the group's influence over state governments (Gardner 2016). While the Enforcement Acts helped restore law and order and protect the rights and lives of Black people in the South, such progress was short-lived.

2. *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 after 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. 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 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).

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).[7] After 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 by the same reasoning—that state courts, rather than federal courts, should be used to enforce private matters.[8]

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 legitimate 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 (Woodward 1955). Overall, lynchings became pervasive in the last two decades of the nineteenth century (see Figure I), with the majority citing sexual, violent, or property crimes as cause.[9]

As lynchings surged, many observers saw the criminal accusations proximate to the formation of lynch mobs as masking a deeper cause, one that was fundamentally political in nature. "Lynching," argued activist and writer James Weldon Johnson

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

8. 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).

9. 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, whereas 59% have such motives in the Seguin and Rigby (2019) data.

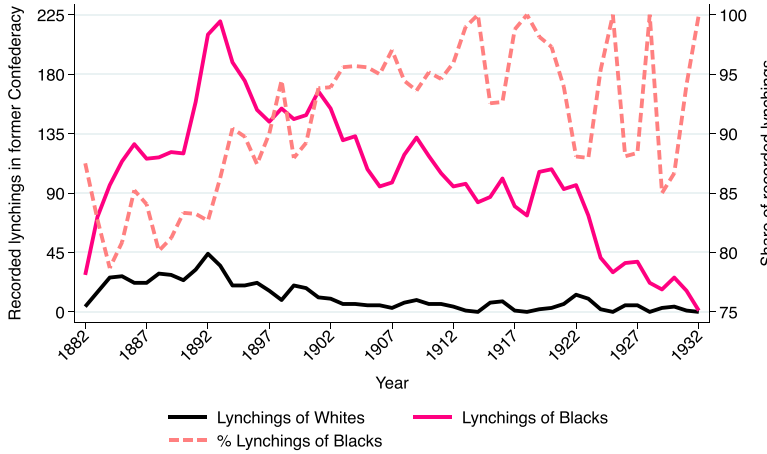


FIGURE I

Lynchings in the Former Confederacy, 1882–1932

Two-year moving averages in the frequency of recorded lynchings across the 11 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).

(1924), "was an instrument in driving the negro out of politics in the South, after the Reconstruction period."

3. *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 proredistribution 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; Olzak 1990; Gerteis 2007; Ali 2011). 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 after Reconstruction (Hackney 2011; Mickey 2015). Indeed, where its opposition could lean on this biracial coalition politically, the Democratic Party's dominance was credibly threatened (Key 1949; Kousser 1974; Gerteis 2007; 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 (Woodward 1955; Glaeser 2005; Ottinger and Posch 2024).[10] Resultant tensions meant that lynching rates tended to be higher during years in which Southern populists were on the ballot in national elections, 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 II), with support for Black voting rights being largely abandoned even among the Democrats' residual opposition in the region (Valelly 2009).

II.B. Conceptual Background

We outline a brief conceptual framework to clarify, in more general terms, the mechanisms that underpin the rich history. This will guide our empirical analysis throughout the remainder of the article. The foundations of our 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

10. 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, "we of the South have never recognized the right of the negro to govern White men, and we never will. We have never believed him to be equal to the White man, and we will not submit to his gratifying his lust on our wives and daughters without lynching him" (Fordham 2022, 109); and South Carolina Senator Martin 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).

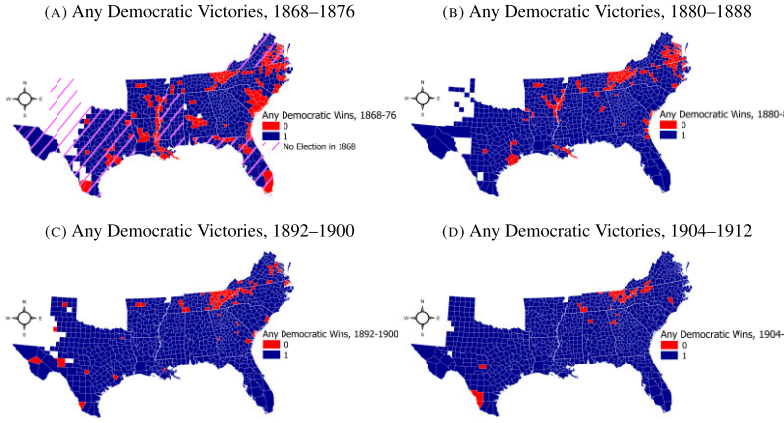


FIGURE II

Consolidation of the "Solid South," 1868–1912

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 (Panel A) 1870, (Panel B) 1880, (Panel C) 1900, and (Panel D) 1910 U.S. Censuses.

of various social control measures—including racial violence—to maintain political power.[11]

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 constituted the majority of the Southern electorate. Indeed, before Jim Crow, there were few de jure means of disenfranchising Black voters available to local Democratic elite.[12]

Left indeterminate in our application of Blalock (1967) to this historical context, however, are the concrete mechanisms through which (i) the local 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

11. Besides violence, stigma-based norms, institutionalized discrimination, and nonviolent hate crimes may serve as social control measures against the minority by the majority (Price, Darity, and Headen 2008).

12. Southern Democratic elites 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, "we have disfranchised the African in the past by doubtful methods, but in the future we'll disfranchise them by law" (Perman 2003).

these factors—broadly, informational and strategic—that we explore empirically in Section IV.

1. *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 may serve as a natural 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, Pons, and Tricaud 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, Pons, and Tricaud 2023).

In our setting, this means that where Democrats did not place first in presidential contests, they risked facing successful challenges by (pro-Black) opposition parties in other contests. Indeed, we provide evidence along such lines in Online Appendix Table A.1, wherein even a close Democratic loss in a county across the 1880–1900 presidential elections predicts relative fewer Democratic local officeholders in the very short run, all else fixed.

2. *Elite Strategy, Racial Hatred, and Backlash.* Given the potential for such informational effects, a local elite has strong incentives to pay close attention to the placement of its party's candidates in terms of local vote shares. Where (minority) opposition parties are rendered credible by a relatively strong performance, local elites may seek to respond in turn.

Commonly, the media are used by political elites to spread antiminority sentiment—and even incite violence against mi-

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).

nority individuals (Yanagizawa-Drott 2014; Adena et al. 2015; Ottinger and Posch 2024). In the post-Reconstruction South, newspapers increasingly published stories accusing Black people, especially men, of rape and other crimes (Woodward 1955). In practice, this served as a strategy through which elites could supply or operationalize local 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, Spohn, and DeLone 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, before de jure means for disenfranchising minority voters, newspapers offered an alternative channel through which Democratic elites could frustrate Black political mobilization in places where it had deemed the Black power threat credible.

III. Empirical Evidence: Democratic Losses and Lynching

This section shows how the incidence of racial violence across the post-Reconstruction South was tied to the local performance of the Democratic Party in presidential elections. Among politically competitive Southern counties, 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 IV.

III.A. Data

Before 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 Online Appendix C.

1. *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).[14] For secondary analyses, we use data from newspapers.com to derive measures of anti-Black crime accusations as well as election reporting in city newspapers. Data on newspapers' partisan affiliation come from Gentzkow et al. (2014a, 2014b).

2. *Political Variables.* Our main explanatory variation is based on county-level vote tabulations for presidential elections over 1880–1900 from Clubb, Flanigan, and Zingale (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 primarily derive information on the partisan composition of public officeholders matched to counties (e.g., mayors) from Kestenbaum (2023) and on racial composition from Logan (2020).[15] For secondary analyses, we take information on the timing of state-level Jim Crow laws from Jones, Troesken, and Walsh (2012).

3. *Other Variables.* Various county-level observables come from the aggregate U.S. Censuses, including population density, Black population shares, 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. 2023) and Confederate Army veteran shares (based on Hall, Huff and Kuriwaki 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 (Tolnay and Beck 1995; Feigenbaum, Mazumder, and Smith 2020), we interact the latter with contemporaneous per pound prices from the U.S. Department of Agriculture's (USDA) Crop Production Historical Track Records.

14. For more information on these data, including on potential selective reporting concerns, see Online Appendix C.2.

15. We augment for former to account for gubernatorial landscape in the cases of William E. Cameron of Virginia, elected in 1881; Daniel J. Russell of North Carolina, elected in 1896; and Alvin Hawkins of Tennessee, elected in 1880. We supplement the latter with data from the U.S. Postal Service (2025) and South Carolina Legislature (2025).

III.B. 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 an 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, Moretti, and Butler 2004; Ferreira and Gyourko 2009). Our primary estimating equation is:

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

where *Any Lynching*$_{c(s)\tau}$ in our analysis indicates whether at least one lynching of a Black (or white) person occurred in county c of state s in the four-year period after a given presidential election, held in November $\tau = \{1880, 1884, ..., 1900\}$.[16] We focus on national elections, of which county-level results provide information as to the relative strengths of local political groups (see Section II.B), while lacking direct effects on actual (Democratic) power. This minimizes countervailing officeholder effects, for example, of local policy. 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 1880 to 1900 was crucial for the Democratic Party in regaining prominence as a national party. Among the 11 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 I). Figure III 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, $f(\textit{Loss Margin}_{c\tau})$, we estimate

16. See Online Appendix C for further detail and robustness regarding data and variable coding choices.

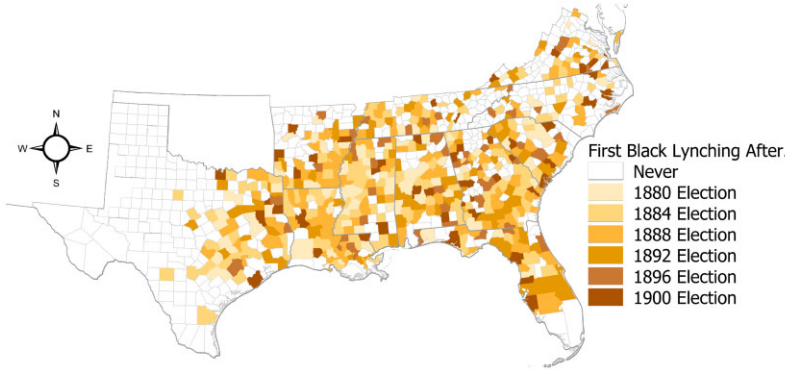


FIGURE III

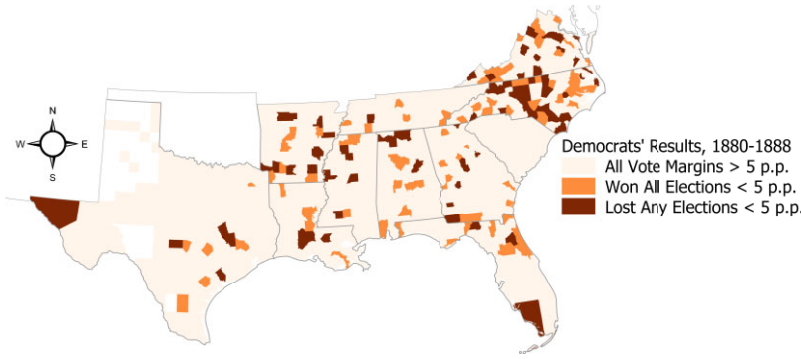
Visualizing Sample Lynching Variation, 1882–1904

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 Online Appendix C for futher details on data construction and coding. For the purpose of the figure, counties boundaries are based on the 1900 U.S. Census.

treatment effects based on counties with very close vote shares in a specific 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 adopt data-driven MSE-optimal bandwidth choices, which limit the set of observations to those relatively close to the Democratic loss threshold (Calonico, Cattaneo, and Titiunik 2014). Figure IV shows the distribution of highly marginal cases, based on a 5 percentage point bandwidth.

1. *Threats to Identification.* Our empirical strategy in equation (1) faces two main challenges. The first concerns the standard RD assumption that relevant factors besides the outcome are continuous around the Democratic loss threshold, $Loss\ Margin_{c\tau} = 0$. If they are not, 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 gen-

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



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

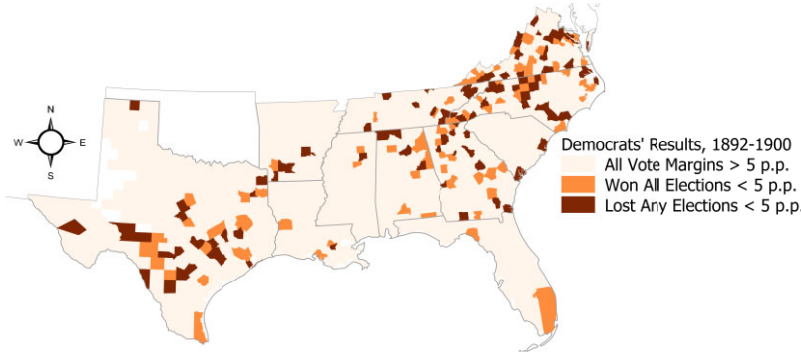


FIGURE IV

Visualizing Sample Treatment Variation, 1880–1900

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

erate differences between treatment and control counties in our sample. Using the formal test from McCrary (2008), we fail at conventional levels ($p = .4$) to reject the null hypothesis that $Loss\ Margin_{c\tau}$ is continuous at the loss threshold (see Online 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).[17] We also test for discontinuities in a wide set of relevant pretreatment factors, described in Section III.A, in place of our outcome in equation (1). We fail to estimate statistically significant differences at the loss threshold across all factors, shown in Table I. 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 and lynching. 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 ($\mathbf{X}_{\mathbf{c}\tau}$). Together, these account for relevant factors in space not fully captured by our unidimensional running variable.[18] 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.[19] We later demonstrate robustness of inference to alternative levels of

17. 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). Although 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 (Kuo and Teorell 2017). Importantly, such measures would be unlikely to result in sorting around the threshold.

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

19. 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 Online Appendix C for further discussion and analysis.

TABLE I

REGRESSION DISCONTINUITY BALANCE TESTS: PRETREATMENT 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	0.090	3.397	0.345	0.064	0.071	83.766	−0.001	0.000
	(0.084)	(2.21)	(0.420)	(0.470)	(0.066)	(79.0)	(0.002)	(0.000)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial polynomial	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
Running 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

TABLE I
CONTINUED

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)
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	−0.153	0.725	1.82	0.251	2.745	−2.088	0.023	0.110
	(0.280)	(0.590)	(2.90)	(1.94)	(6.74)	(2.76)	(0.047)	(0.150)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial polynomial	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
Running polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
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 pretreatment county-level characteristics. All characteristics are measured as of 1880 except for columns (7) and (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 U.S. Department of Agriculture's (USDA) Crop Production Historical Track Records. See Section III.B for more details on variables. Estimates based on linear running polynomials and the MSE-optimal bandwidth from Calonico, Cattaneo, and Titiunik (2014). See Online Appendix Table B.1 for estimates based on quadratic running polynomials and Online Appendix Table B.2 for estimates based on a single, fixed bandwidth. 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. $*p < .10$, $**p < .05$, $***p < .01$.

clustering. Further details on our RD specification are found in Online Appendix B.

III.C. Main Results: Political Foundations of Southern Lynchings

We 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.

1. *Main Results.* Table II reports estimates of β in equation (1), with our core results shown in Panel A. Our primary outcome of interest is an indicator for whether there were any lynchings of Black people in the four-year period after a presidential election from 1880 through 1900. Besides a linear running polynomial, our baseline covariates include election period fixed effects (all columns), as well as a set of spatial covariates that includes state fixed effects and quadratic polynomials for county longitude and latitude (even columns only). Featuring all of these, our preferred estimate in column (3) implies a 10.4 percentage point increase in the probability of a Black lynching over the four years after a local Democratic Party loss in a county, equivalent to about an 80% increase over the (control) mean.

We also estimate effects for white lynchings as a placebo outcome. Although white people were less frequent targets of mob violence than Black people, white lynchings did occur. That being said, white-on-white lynchings were more often conducted privately—distinct from the public spectacles that typically characterized white-on-Black lynchings—suggesting that "such a mob was driven by different concerns than mobs lynching Black men similarly accused" (Smångs 2016, 1357). Ultimately, estimates for white lynchings (columns (5)–(8)) are small and statistically indistinguishable from zero. This contrasts starkly to the estimates for Black lynchings and suggests our findings to be distinct from a general violence effect.

All of these tabular estimates are based on the MSE-optimal bandwidths from Calonico, Cattaneo, and Titiunik (2014), which limit the set of observations to those close to the Democratic loss threshold, where local randomization is plausibly satisfied. Thus, although our full sample contains nearly 6,000 county-election observations, our main treatment effects are estimated from per-

TABLE II

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)
	Panel A: Full sample ($N = 5{,}914$)							
Democrat lost in election τ	0.105**	0.125**	0.104**	0.138***	–0.006	–0.003	–0.009	–0.006
	(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
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial polynomial	No	No	Yes	Yes	No	No	Yes	Yes
Optimal bandwidth	14.77	21.32	15.65	20.17	24.44	29.01	24.81	29.43
Running 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	1,866	2,255	2,569	2,276	2,595

TABLE II
CONTINUED

Dependent variable	Any lynchings of . . . people after election τ							
	Black				White			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel B: Uncompetitive counties in election $\tau - 1$ only ($N = 4{,}306$)							
Democrat lost in election τ	0.197**	0.261**	0.188***	0.255**	0.026	0.033	0.016	0.025
	(0.077)	(0.110)	(0.072)	(0.100)	(0.024)	(0.026)	(0.023)	(0.025)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial polynomial	No	No	Yes	Yes	No	No	Yes	Yes
Optimal bandwidth	17.02	21.48	18.34	21.61	19.07	24.66	20.59	25.33
Running 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	651	908	703	913	744	1,081	843	1,116

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 based on linear (odd columns) and quadratic (even) running polynomials and the MSE-optimal bandwidth from Calonico, Cattaneo, and Titiunik (2014). See Online Appendix Table B.3 for estimates based on a single, fixed bandwidth. All regressions include election period fixed effects, while columns (3) and (4) and (7) and (8) also include state fixed effects and quadratic polynomials for county longitude and latitude. Standard errors are clustered at the county level. $*p < .10$, $**p < .05$, $***p < .01$.

haps 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, Table II, Panel B 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 in which vote margins fell within that bandwidth in $\tau - 1$, our baseline estimate for Black lynchings nearly doubles, to 18.8 percentage points, in column (3). Our estimates for white lynchings increase as well, while remaining statistically insignificant at conventional levels.

We complement these tabular results with visual RD plots in Figure V, which show the same discontinuity for Black lynchings around the loss threshold as in our tabular results.[20]

2. *Robustness Checks.* To bolster a causal interpretation for our core results in Table II, we present a suite of additional robustness checks.

3. *Inference and Standard Errors.* Given the (incomplete) historical record of lynching events on which our sample is based, the LATE associated with a Democratic presidential loss in a county corresponds to roughly six additional Black lynchings.[21] To address concerns about inference given the relatively small effective sample size associated with this LATE, we cross-validate our empirical model using a leave-one-out exercise, which tests the sensitivity of our empirical model and its estimates to dropping individual observations one at a time. The distribution of RD point estimates, as well as associated p-values, are plotted in

20. See Online Appendix Figure E.1 for alternative RD plots based on narrower bandwidths and restricted samples.

21. Inspired by Figure V, this is based on observations within 2.5 percentage points of the RD threshold. This bandwidth implies a set of 220 observations, of which 96 have loss margins between 0 and 2.5. Together with the control outcome mean and effect size in Table II, this translates to about 16 recorded lynchings among the "control" observations, versus about 22 among the "treated" observations, for about 6 additional lynchings associated with the LATE.

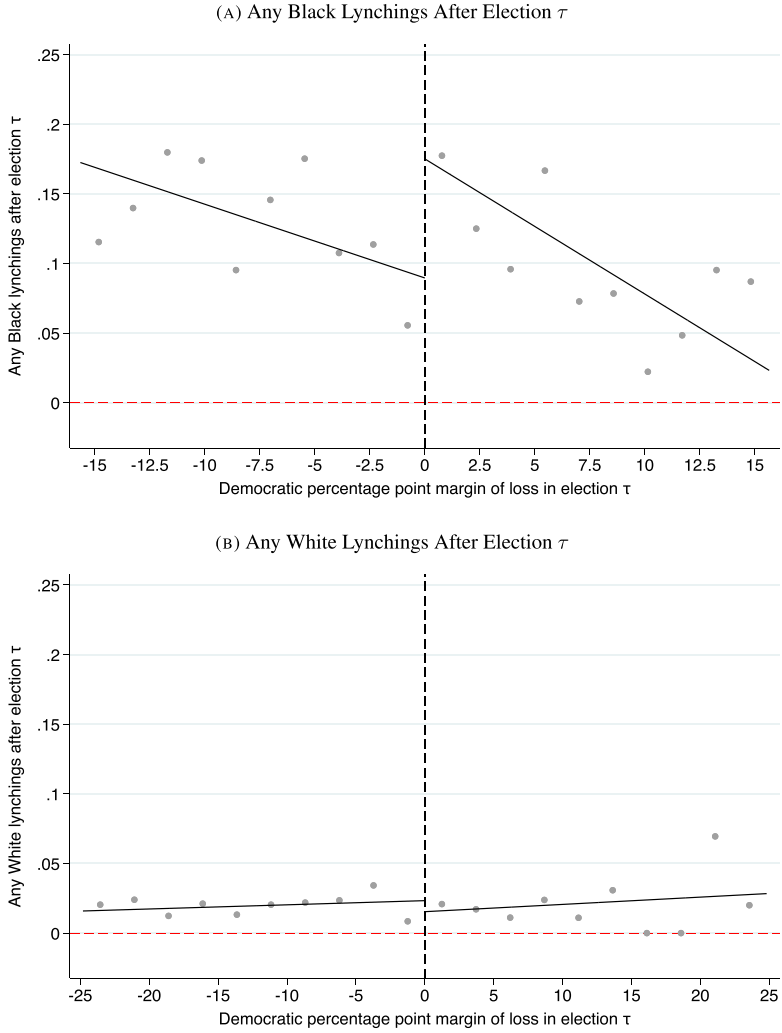


FIGURE V

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

Binned estimates of the probability of (Panel A) Black and (Panel 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. Bandwidth values on the x-axis based on the optimal bandwidths for each regression in Table II. For RD estimates and associated p-value ranges, see Table II.

Online Appendix Figure B.2. These point estimates range from 0.08 and 0.12, all with $p < .05$.

We further show robustness of inference to more extreme serial and spatial autocorrelation in Table III, Panel A. 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 III shows two such alternatives, which cluster by county-decade and state-by-election-period.

4. *Varying Controls.* Table III, Panel B 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 on counties being within the optimal bandwidth from row 1. Each of these has the effect of making our estimates more precise. Rows 7 and 8 further verify the assumptions underpinning the RD by controlling for quadratic polynomials of 1880 Black population shares and all variables from Table I, respectively, neither resulting in much change to our estimates. Finally, rows 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 from Jones, Troesken, and Walsh (2012).

5. *Alternative RD Specifications.* We test sensitivity of our results to alternative bandwidths and running polynomials in Table III, Panel C. Rows 11 and 12 reestimate the specification in row 1 but with the optimal bandwidths multiplied by factors of 0.5 and 1.5, respectively. Meanwhile, rows 13–15 vary our running polynomial, with estimates based on quadratic and hyper-

TABLE III

IDENTIFICATION AND ROBUSTNESS CHECKS ON RD ESTIMATES IN TABLE II

Dependent variable	Any Black lynchings (1)	Any white lynchings (2)
	Panel A: Alternative standard errors	
1. Baseline (Table II, columns (3) and (7))	0.104**	−0.009
Clustering by county	(0.041)	(0.013)
Clustering by county-decade	(0.041)	(0.013)
Clustering by state-election-period	(0.041)	(0.012)
	Panel B: Alternative control sets	
2. No controls or fixed effects	0.097**	−0.009
	(0.045)	(0.014)
3. No spatial covariates	0.105**	−0.006
	(0.044)	(0.013)
4. No longitude and latitude controls	0.074*	−0.009
	(0.039)	(0.013)
5. Baseline w/ county fixed effects, based on unique county boundaries	0.055***	−0.010
	(0.022)	(0.009)
6. Baseline w/ county-pair fixed effects, matched on proximity in longitude and latitude	0.091***	−0.007
	(0.035)	(0.012)
7. Controlling for quadratic polynomial in 1880 Black population shares	0.096**	−0.007
	(0.041)	(0.013)
8. Controlling for all variables from Table I	0.091**	−0.010
	(0.043)	(0.014)
9. Baseline w/ state × pre–Jim Crow FE	0.104**	−0.009
	(0.041)	(0.013)
10. Baseline w/ spatial covariates × pre–Jim Crow	0.103**	−0.007
	(0.039)	(0.013)
	Panel C: Alternative RD specifications	
11. Optimal bandwidth × 0.5	0.160**	0.007
	(0.068)	(0.013)
12. Optimal bandwidth × 1.5	0.108**	−0.008
	(0.044)	(0.013)
13. Quadratic running polynomial	0.138***	−0.006
	(0.053)	(0.015)
14. Cubic running polynomial	0.127**	0.003
	(0.057)	(0.015)
15. Quartic running polynomial	0.138**	0.007
	(0.056)	(0.015)
	Panel D: Alternative samples	
16. Excluding states w/ election years contemporaneous with τ	0.096*	0.016
	(0.058)	(0.024)

TABLE III
CONTINUED

Dependent variable	Any Black lynchings (1)	Any white lynchings (2)
17. Excluding states w/ election months contemporaneous with τ	0.089∗ (0.052)	0.007 (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 based on linear running polynomials and the MSE-optimal bandwidth from Calonico, Cattaneo, and Titiunik (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 III.C for a more detailed overview of the items in each row. $*p < .10$, $**p < .05$, $***p < .01$.

flexible cubic and quartic running polynomials. Results remain substantively intact in all cases and significant at conventional levels for Black lynchings.

6. *Sample Sensitivity.* Our analysis focuses on the 11 states of the former Confederacy. Importantly, all of those states were strongly Democratic in their elite composition and had the distinction of supporting the Democratic candidate in every presidential election between 1880 and 1916, illustrating the pervasive Democratic political identity that comprised the so-called solid South. We moreover focus on presidential elections between 1880 and 1900, after which Democrats faced little local political competition in these states.

We explore sensitivity to these choices in Online 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 1 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 effects, may be confounding our results. Table III, Panel D, rows 16 and 17 show that point estimates do not meaningfully change when excluding such cases. We further discuss questions of sample selection in Online Appendix C.

Finally, Online Appendix Tables B.2 and B.3 fully replicate the findings from Table I and Table II, respectively, using a fixed bandwidth of 15 percentage points across all outcomes and specifications.

7. *Alternative Outcome Measurement.* Given the infrequency of events, our default outcome measure is an indicator variable for whether any lynching occurred during a specific four-year election period. We nevertheless consider several alternative outcome variables in Online Appendix Table C.2. These include measures based on (logged) counts and rates (per 10,000 people).[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 county after a Democratic loss. Finally, Online Appendix Figure C.2, Panel A separately estimates effects for each year period since a presidential election (e.g., column (1) considers the 12 months after an election). Effects are large and significant only in the two years afterward, suggesting lynching was reactive rather than proactive.

8. *Placebo Analysis.* Last, we conduct a set of placebo analyses based on alternative RD thresholds and effect windows. First, Figure VI, Panel A estimates equation (1) using a variety of "placebo" Democratic loss margins. Specifically, given an actual threshold of Loss $\text{Margin}_{c\tau} = 0$, the x-axis shows estimates from alternative thresholds Loss $\text{Margin}_{c\tau} + s$, where s ranges from -50 to 50 percentage points. The results confirm that it is only the true win-lose RD threshold that is systematically salient, not any other.

Second, Panel B uses a set of placebo effect windows, with the 48-month election period associated with our lynching outcome shifted forward or backward in time by four-year increments, relative to our explanatory variation. Expanding on this, Online Appendix Table C.3 further examines the sensitivity of our estimates to <4-year shifts in the effect window. In all cases, es-

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.

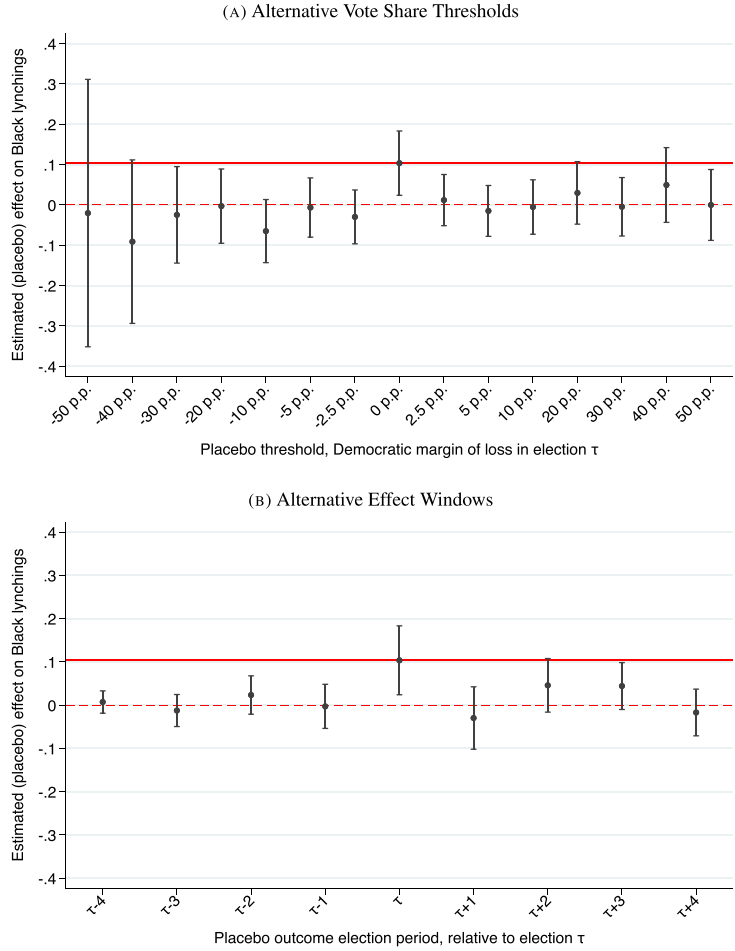


FIGURE VI

Placebo Effects

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" (Panel A) Democratic margins of victory in τ and (Panel B) lynching effect windows, where the solid red line denotes the baseline RD estimate in Table III, row 1. Given an actual Democratic loss threshold of Loss $\text{Margin}_{c\tau} = 0$, the x-axis in Panel A shows estimates from alternative thresholds Loss $\text{Margin}_{c\tau} + s$, where s ranges from -50 to 50 percentage points. Given an actual treatment election of τ, the x-axis in Panel B shows estimates using alternative election periods $\tau + e$, where s varies the four-year effect window following τ from $e = -4$, meaning four election periods (i.e., 16 years) before τ, to $e = 4$, meaning four election periods after τ. 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.

timates cannot be distinguished from zero when lynchings in the effect window lie outside of the election period of interest. Finally, Online Appendix Figure C.2, Panel B replicates Panel A of the same figure using the years leading up to a presidential election. Such pretreatment effects are each estimated as null. Overall, these results show that only the true effect window is of systematic importance in terms of lynching effects.

IV. Mechanisms: Elections, Information, and Elite Strategy

The RD estimates in Table II point to dramatic effects of presidential election results in counties, with even narrow Democratic losses leading to significant increases in lynching. This section presents several further findings meant to clarify the mechanisms underlying this effect. Following our conceptual framework in Section II.B, 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.

IV.A. Electoral Information and Racial Violence

Given a quasi-random interpretation of the RD framework, a puzzle emerges as to why a narrow electoral defeat was 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.

1. *Election Results as Signals of Political Strength.* We begin by probing further heterogeneity analysis in the spirit of Table II, Panel B. In Table IV, we estimate a large set of conditional RD specifications based on whether a county in election τ was (i) Democrat-won in presidential election $\tau - 1$ (columns

TABLE IV

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	0.160**	0.164**	0.050	0.041	0.188***	0.255**	0.067	0.091	0.392***	0.363**	0.046	0.094
	(0.063)	(0.068)	(0.066)	(0.075)	(0.072)	(0.10)	(0.049)	(0.067)	(0.130)	(0.150)	(0.042)	(0.058)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial polynomial	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	14.72	28.02	12.75	20.60	18.34	21.61	14.48	16.49	10.82	20.42	17.67	18.71
Running polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Split-sample p-value	.11	.13	.11	.13	.11	.09	.11	.09	.00	.02	.00	.02
Control outcome mean	0.12	0.14	0.16	0.17	0.13	0.14	0.15	0.14	0.10	0.14	0.14	0.14
Observations	921	1,716	414	627	703	913	834	923	277	630	1,134	1,175

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 based on linear (odd columns) or quadratic (even) running polynomials and the MSE-optimal bandwidth from Calonico, Cattaneo, and Titiunik (2014). See Online Appendix Table B.4 for estimates based on a single, fixed bandwidth. The split-sample p-value corresponds to the null hypothesis that the difference between coefficients on $Democratic\ Loss_{c\tau}$ across subsamples is zero (e.g., between columns (1) and (3)). 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. $*p < .10$, $**p < .05$, $***p < .01$.

(1)–(4)), (ii) electorally uncompetitive in $\tau - 1$ (columns (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, and it may even imply a strengthening Democratic hand. Likewise, in a perennially competitive county, a narrow loss would 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, Egorov, and Fiorin 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 where results were close or where Democrats had previously lost. Considering these dimensions jointly, effect sizes among counties where close Democratic losses followed more comfortable Democratic victories further dwarf our baseline results, with estimates of 0.39 (0.13) in column (9), compared with small and statistically insignificant increases in Black lynchings otherwise of 0.05 (0.04) in column (11).[23] These differences are significant, associated with a value of $p < .01$.

Together, these patterns are consistent with informational factors underpinning our main RD effect.[24] Under this interpretation, the Democratic Party's failure to win in a county, 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 IV.B.

23. As with our core results, these findings are fully robust to fixing our bandwidth at 15 percentage points across all outcomes and specifications, as shown in Online Appendix Table B.4.

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

2. *Conditioning on the (Populist) Opposition.* The type and extent of information revealed by a Democratic loss also depends 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 II.B, 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 Online Appendix Table C.4, exploiting the fact that the Democrats' primary opposition candidate (i.e., of first or second place) varied across counties in 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) and (2) reestimate 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) and (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 percentage point increase in the probability of a Black lynching in the subsequent four years. This amounts to a 319%–360% increase over the (control) mean, four to five times greater than estimates based on the residual nonpopulist opposition (columns (5) and (6)). This result dovetails with Ottinger and Posch (2024), who emphasize the threat of the populists in driving anti-Black propaganda in the postbellum South.

3. *Beyond Counties and Informational Effects.* Our core results above are based on county-level vote shares from presidential elections, which convey certain information as to the strengths of local political (e.g., opposition) groups. We expand our analysis to consider congressional district (CD) elections. These differ from presidential elections in two key ways. First, the salience of informational effects is likely to be weaker of CD elections, insofar as presidential election results are more widely

observed and known.[25] Second, county vote shares in presidential elections lack direct effects on local Democratic power, including local policy. By contrast, if a non-Democrat were to win a CD election, he might use the power of the office to draw attention to or otherwise combat racial violence, attenuating the overall effect.

To test this, we estimate a variant of equation (1) for CDs, shown in Online Appendix Table C.5, using a version of our lynching data mapped to the CD level (Ferrara, Testa, and Zhou 2021). Our full sample consists of 11 two-year CD election periods, with secondary samples omitting midterm election years (columns (2) and (5)) and the 1880 elections (columns (3) and (6)), the latter mostly predating our first sample lynching. Columns (4)–(6) alternatively adopt our outcome measure from Table II, based on four-year presidential election periods. RD estimates based on any of these are imprecise and relatively small—equivalent to a 4% increase over the (control) mean in column (2), compared with an 80% increase in Table II, column (3).

Ultimately, these findings suggest that the effects of CD election results are indeed lesser than those of presidential elections. Even if they do retain some informational effect, there is also a meaningful countervailing force, wherein election results also stand to shape the local distribution of elite types and, in turn, policy. Next we turn our focus to a different kind of elite impact—the one associated with elites on the losing end of an election.

IV.B. Elite Influence and Racial Backlash

If poor Democratic performance in post-Reconstruction Southern locales rendered the Black power threat credible, 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 in the form of racial violence and intimidation. Indeed, our results thus far suggest as much. Yet mob violence depended on decentralized efforts 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

25. Indeed, we estimate that the rate of newspaper reporting on presidential election results exceeds reporting on CD election results by about 339% during the sample period. See Online Appendix D for concrete details on the differential rate of newspaper reporting by election type.

of newspaper media. These effects, along with our core lynching effects from Section III, are driven by places with an all-white, Democratic elite facing large Black populations therein, peaking in the pre—Jim Crow period.

1. *Partisan Media and the Supply of Racial Hate.* Collective action 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 Online 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 with white, female victims (Woodward 1955; Glaeser 2005). Such accusations importantly provided motive for racist individuals to lynch Black people, 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 after 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 postelectoral lynching activity documented in this article. Such a pattern would point to the strategic use of media by local elites for suppressing Black political participation.

2. *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 city's county predict increases in anti-Black crime accusations in its newspapers' con-

tent, by estimating the RD design,

$$\begin{aligned} \%\ \text{Accusations}_{n(c)t(\tau)} = \beta \cdot \text{Democratic Loss}_{c\tau} \\ + f(\text{Loss Margin}_{c\tau}) + \phi_\tau + \Upsilon_{t(\tau)} \\ + \alpha_{\sigma(c)} + \varepsilon_{nt}, \end{aligned} \tag{2}$$

where $\%\ Accusations_{nt}$ measures the rate of anti-Black crime accusations in newspaper n in city σ of Southern county c for 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 (Online Appendix Table C.2), we also include dummies for election cycle year (Υ_t) to account for cyclic shocks in newspaper content in electoral periods. For robustness, we also estimate effects using our baseline spatial covariates from equation (1), of state fixed effects and quadratic polynomials in county longitude and latitude, and more granularly using newspaper fixed effects.

We begin by building a comprehensive, time-varying sample of newspapers pages from newspapers.com (as of June 10, 2023).[26] We link cities to their historical counties (as of 1900) from the Census Place Project (Berkes, Karger, and Nencka 2023). This ensures that newspapers are responding to electoral outcomes in their city's contemporaneous county. As newspaper units often enter or exit our sample within election periods (e.g., due to splits and mergers), we adopt calendar years as the temporal unit for this analysis.[27]

We define anti-Black crime accusation rates for each year in the 1880–1900 election period sample. To construct this variable, we count the total number of pages per newspaper-year across our sample of analysis that plausibly feature an anti-Black crime accusation. 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 Online Appendix Figure D.1 for examples). Our

26. See Beach and Hanlon (2022) and Ferrara, Ha, and Walsh (2022) on use of newspapers.com to build historical data.

27. As an example, the *Memphis Daily Appeal* runs in our sample from 1881 to 1889. Meanwhile, its competition, the *Memphis Avalanche*, runs from 1885 to 1890. 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 later merged to become the modern-day *Commercial Appeal*. For more, see Gentzkow, Shapiro, and Sinkinson (2011); Gentzkow et al. (2015).

script also allows for the plural of the word "negro" and the past tense of the crime mentioned ("raped," "murdered," "robbed").[28] 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 augment the newspaper data with information on newspapers' partisan affiliations during the sample period from Gentzkow et al. (2014a, 2014b). 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 in nearly 90% of cases, while about a third of newspapers in our sample have no known affiliation.

3. *Newspaper Results.* Table V, columns (1)–(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 II 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 the Democratic affiliations especially among Southern newspapers during this period, we split our sample by the political leanings of newspapers and reestimate 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 when using only Democratic newspapers in columns (7) and (8). Meanwhile, they become negative among the smaller sample of non-Democratic newspapers in columns (9) and (10). Though marginally insignificant, the point estimates are somewhat large. This suggests close

28. In principle, this might pick up cases in which a "negro was raped," rather than the reverse. In practice, such cases are rare. Whereas "negro raped" would be more likely than "negroes rape" to result in this, our results are not sensitive to dropping any one phrase from our set of search inputs, shown in Online Appendix Figure D.2.

TABLE V

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	0.126*	0.139**	0.168***	0.136**	0.055**	0.118***	0.104**	0.168***	−0.368	−0.312
	(0.073)	(0.069)	(0.042)	(0.056)	(0.024)	(0.037)	(0.042)	(0.060)	(0.230)	(0.210)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election cycle year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	No	No	No	No	No	No	No	No
City FE	No	No	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Newspaper FE	No	No	No	No	Yes	Yes	No	No	No	No
County spatial polynomial	Yes	Yes	No	No	No	No	No	No	No	No
Optimal bandwidth	19.71	21.41	16.04	14.40	14.83	14.54	19.20	16.05	11.64	11.70
Running polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Split-sample p-value							.09	.05	.09	.05
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 $\tau \in \{1880, \ldots, 1900\}$, including conditional on a newspaper's partisan affiliation being Democratic (columns (7) and (8)) or non-Democratic (columns (9) and (10)). 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, 2014b). Estimates based on linear (odd columns) and quadratic (even) running polynomials and the MSE-optimal bandwidth from Calonico, Cattaneo, and Titiunik (2014). See Online Appendix Table D.1 for estimates based on a single, fixed bandwidth. The split-sample p-value corresponds to the null hypothesis that the difference between coefficients on *Democratic* $Loss_{c\tau}$ across subsamples is zero (e.g., between columns (7) and (9)). All regressions include fixed effects for election period, election cycle year, and state (columns (1) and (2)), city (columns (3) and (4), (7)–(10)), or newspaper (columns (5) and (6)). Columns (1) and (2) control for 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. $*p < .10$, $**p < .05$, $***p < .01$.

non-Democratic losses may have spurred anti-Black antagonism among local non-Democratic elites. We discuss such instances of reverse backlash in greater detail below.

We elaborate on and show robustness of these results along a number of dimensions in the Online Appendix. As with our main results, Online Appendix Table D.1 reestimates Table V using a fixed bandwidth of 15 percentage points. Online Appendix Table D.2 further augments our analysis using an array of alternative (Panel A) inference strategies, including higher-level clustering; (Panel B) covariates, including quadratic controls for 1880 Black population shares; and (Panel C) RD specifications, including varying bandwidths. In Panel D, in a series of sensitivity analyses we explore 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 suppressing 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.

4. *Salience of (Close) Losses among Local Elites.* Next we provide evidence that the same Democratic elite propagating anti-Black hatred after county-level presidential losses viewed close losses as differentially salient—relative to close wins—in the first place. Although Section II.B previously outlined our reasoning for why this may be the case, we augment the RD analysis to explore evidence that it indeed was.

Importantly, the same newspapers that reported on Black-committed crime in the late nineteenth century also provided

close coverage of presidential elections, including which party lost or won in a county. To show this, we document the average sample intensity of newspaper reporting on county losses and wins in presidential elections over the two-year period before and after all presidential elections between 1880 and 1900. These measures, constructed from articles archived at newspapers.com (as of May 22, 2025), are based on all mentions of "loses county" or "lost county" (for losses), or "wins county" or "won county" (for wins), which co-appear with the words "presiden∗" and "vote∗," except for Louisiana, for which we search "parish" instead of "county."

Summed counts of these keyword elements are averaged across cities that had any such reporting over the sample period and plotted in Figure VII, where two patterns immediately stand out. First, the intensity of coverage of county presidential election results peaks around those elections (i.e., during November, or month 0, specifically after the presidential election), and rising somewhat less during the months near other, nonpresidential elections, when past or future presidential elections were likely being referenced. Second, coverage focusing on electoral losses (i.e., framed in terms of who lost a county) was far more common (dark solid, mean = .315 words) than coverage focusing on wins (light solid, mean = .085), independent of election outcomes. This is despite the fact that Democrats, who tended to own the newspapers, tended to win elections in the South, not lose. Together, these two patterns confirm that Southern newspapers actively reported on presidential election results, particularly losses, at the county level. For more concrete examples of reporting, see Online Appendix Figure D.3.

With this descriptive evidence as proof of concept, we augment equation (2) using a binary outcome of whether a newspaper-year had any news coverage of county presidential election results after election τ. This measure, which comprises the same keywords shown in Figure VII, allows us to test whether such reporting was overall more common in the aftermath of (close) Democratic losses at the county level in presidential elections, relative to close wins—a plausible indicator that the Democratic elite who dominated Southern news media saw such outcomes as differentially salient.

Table VI, column (1) shows this to be the case, with a roughly 39% increase in the probability of any news coverage of county election outcomes after a close Democratic loss compared with a close win. Columns (3)–(6) further confirm this to be driven by

TABLE VI

DEMOCRATIC LOSSES IN COUNTIES AND ELECTION REPORTING IN NEWSPAPERS, 1880–1900

Dependent variable	Probability of reporting on county election outcome in newspapers					
Newspaper affiliation	Any		Democratic		Non-Democratic	
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat lost in election τ	0.043***	0.117***	0.119***	0.161***	−0.051	0.007
	(0.016)	(0.026)	(0.020)	(0.029)	(0.095)	(0.140)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes
Election cycle year FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	8.39	14.19	10.58	14.46	15.50	15.91
Running polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Split-sample p-value			.10	.11	.10	.11
Control outcome mean	0.11	0.12	0.12	0.11	0.12	0.12
Observations	1,558	2,499	1,341	1,777	240	245

Notes. This table reports bias-corrected local-polynomial RD estimates for the relative probability of news reporting on electoral outcomes in a given newspaper-year during the election period following a presidential election $\tau \in \{1880, \ldots, 1900\}$, including conditional on a newspaper's partisan affiliation being Democratic (columns (4)–(6)). A newspaper-year is considered to have had election reporting if newspapers.com (accessed May 22, 2025) shows at least one positive search result for "loses county" or "lost county" (for losses), or "wins county" or "won county" (for wins), coappearing with the words "presiden∗" and "vote∗," except for Louisiana, for which we use "parish" instead of "county." See Figure VII for dynamics of those keyword counts over time. Data on newspaper affiliations come from Gentzkow et al. (2014a, 2014b). Estimates based on linear (odd columns) or quadratic (even) running polynomials and the MSE-optimal bandwidth from Calonico, Cattaneo, and Titiunik (2014). The split-sample p-value corresponds to the null hypothesis that the difference between coefficients on *Democratic Loss*$_{c\tau}$ across subsamples is zero (e.g., between columns (3) and (5)). All regressions include fixed effects for election period, election cycle year, and newspaper city. Standard errors are clustered at the county level, except in columns (5) and (6), which are heteroskedasticity-robust. $*p < .10$, $**p < .05$, $***p < .01$.

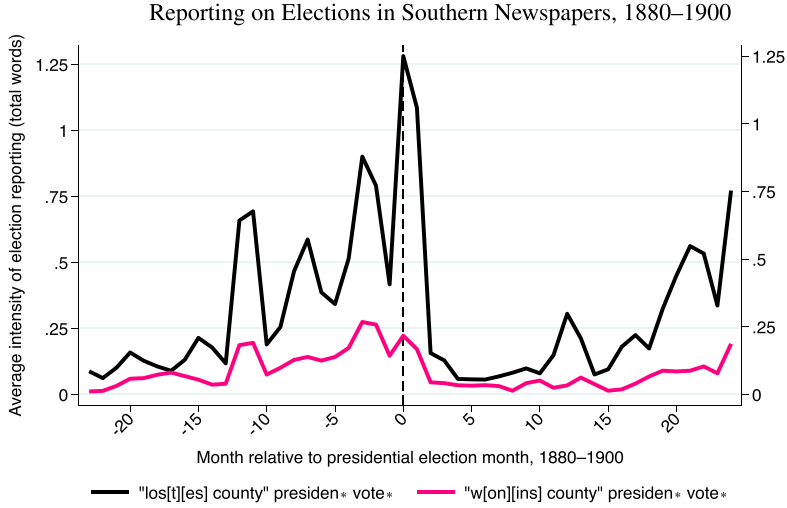


FIGURE VII

Reporting on Elections in Southern Newspapers, 1880–1900

Two-month moving averages of the intensity of a city's news reporting on county losses (dark solid, mean = .315) and wins (light solid, mean = .085) of the presidential vote over the two-year period before and after a given presidential election between 1880 and 1900, where month 0 corresponds to the November of a given election year. News reporting based from articles archived at newspapers.com (accessed May 22, 2025). Balanced city-month panel consists of all newspapers, across all six four-year 1880–1900 electoral periods, with at least one positive search result for "loses county" or"lost county" (for losses), or "wins county" or "won county" (for wins), coappearing with the words "presiden*" and "vote*," except for Louisiana, for which we use "parish" instead of "county." Counts of those words are summed within each city to derive a measure of monthly reporting intensity, with averages derived in turn.

the Democrat-affiliated newspapers in our sample, with a 99% increase in probability among those papers alone. Together, these results provide further evidence that a close presidential loss was indeed differentially salient to local Democratic Party elites, relative to a close win, as revealed by starkly heightened news coverage by those same elites after such outcomes.

5. *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

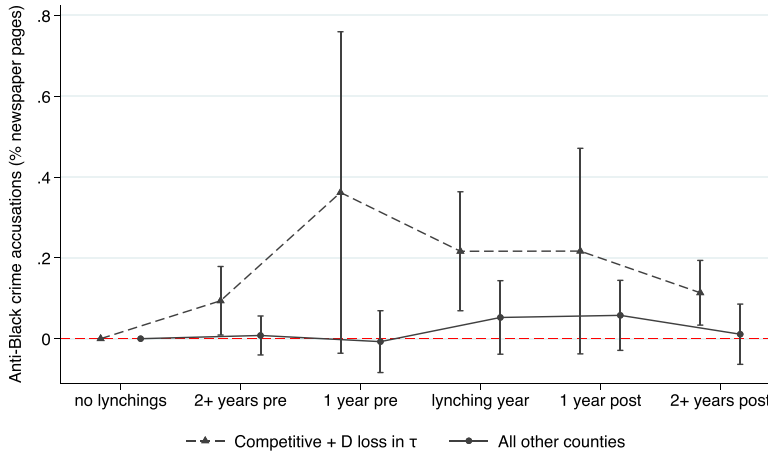


FIGURE VIII

Dynamics of Anti-Black Crime Accusations around Black Lynchings

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 0.140 (0.034) for the "competitive + D loss" subsample and 0.019 (0.027) otherwise, with a p-value for these differences of .004. p-values for effect-period differences are .075 (2+ years pre), .063 (1 year pre), .050 (lynching year), .231 (1 year post), and .061 (2+ years post). Regressions include elecion period fixed effects, election cycle 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.

Black violence and aggression. Caricatures of the Black "brute" or "buck" were distinct from Black inferiority narratives used to rationalize slavery before and immediately after the Civil War and served a different purpose. As Black lynchings began to surge in the 1880s, they were commonly legitimated by accusations of violent atrocities, such as the rape of a white woman.[29]

Supporting the use of crime accusations in inciting lynchings of Black people, Figure VIII shows how the frequency of accusations in newspapers increased in counties over the period lead-

29. Accusations of sexual violence are most common among both outcomes, shown in Online Appendix Table E.1.

ing up to a Black lynching in our data, before decreasing thereafter. The dynamics of this relationship, meanwhile, vary with the contemporary electoral conditions. Among counties coming off of close Democratic losses,[30] accusations tended to co-occur with or precipitate a Black lynching, in line with our proposed mechanism. Otherwise, such accusations tended to co-occur with or even follow a lynching, more suggestive of retrospective reporting. These differences, within and across years, are statistically significant at conventional levels (e.g., $p = .05$ for lynching years), with lynching consistently positively correlated with anti-Black accusations only where Democrats had recently lost the presidential vote.

Corroborating this, a mediating role of anti-Black accusations on Black lynching can also be found by way of formal causal mediation analysis. In Online Appendix Table E.2, column (2), we estimate that roughly a quarter of our lynching effects is explained by the indirect effect of close Democratic losses, through our newspaper variation.

6. *Elite Composition, Institutions, and the (Black) Power Threat.* Further supporting a relationship between elite-driven crime accusations and racial violence, the same local conditions that gave rise to higher lynching rates after Democratic losses also served to fuel anti-Black newspaper stories. Table VII shows that both outcomes are specifically driven by the places that were most plausibly characterized by Blalock's (1967) Black power threat, with an all-Democrat, white-only local elite facing a relatively large Black population.

We begin by conditioning our sample on whether a given county had a Democrat-only (columns (1) and (2)) or white-only (columns (3) and (4)) elite as of presidential election τ, based on the set of local- and state-level public officeholders matched to a county at the time. See Section III.A or the table notes for information on these data, including sources. Estimates across both panels of Table VII reveal our lynching results from Table II and our newspaper results from Table V to be driven chiefly by counties with a Democrat- or white-only elite composition. At the same time, effects depend on the presence of a large Black local constituency (columns (5) and (6)), which was likely to be blamed

30. We follow Table IV and define "competitive" based on the median vote margin among sample Democratic losses.

TABLE VII

HETEROGENEOUS EFFECTS: ELITE COMPOSITION AND THE BLACK POWER THREAT

	Elite composed of . . . at τ				Large Black	
	Democrats only		whites only		constituency in 1880	
	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Dep. var: Any Black lynchings after election					
Democrat lost in election τ	0.127***	−0.006	0.110***	−0.092	0.177***	−0.031
	(0.044)	(0.098)	(0.042)	(0.120)	(0.065)	(0.038)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Spatial polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	14.52	21.84	14.61	22.75	14.06	26.85
Running polynomial	Linear	Linear	Linear	Linear	Linear	Linear
Split-sample p-value	.11	.11	.08	.08	.00	.00
Control outcome mean	0.13	0.15	0.13	0.16	0.15	0.11
Observations	1,100	414	1,298	138	735	1,028

TABLE VII
CONTINUED

	Elite composed of . . . at τ				Large Black	
	Democrats only		whites only		constituency in 1880	
	Yes (1)	No (2)	Yes (3)	No (4)	Yes (5)	No (6)
	Panel B: Dep. var: Frequency of anti-Black crime accusations (% pages)					
Democrat lost in election τ	0.245***	0.038**	0.127**	−0.028	0.229***	−0.019
	(0.056)	(0.016)	(0.052)	(0.050)	(0.049)	(0.055)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes
Election cycle year FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	16.26	11.29	12.86	12.05	12.87	10.00
Running polynomial	Linear	Linear	Linear	Linear	Linear	Linear
Split-sample p-value	.08	.08	.31	.31	.06	.06
Control outcome mean	0.17	0.23	0.19	0.23	0.19	0.16
Observations	1,770	734	1,861	441	1,250	801

Notes. This table reestimates Tables II and V conditional on whether a given county had a Democrat-only (columns (1) and (2)) or white-only (columns (3) and (4)) elite composition as of election τ, as well as whether it had an above-median Black population in 1880 (columns (5) and (6)). Measures of elite composition are based on local- and state-level public officeholders matched to a given county (e.g., mayors, representatives) as of τ. Information on partisan composition (columns (1) and (2)) based primarily on Kestenbaum (2023), augmented to account for the gubernatorial landscape in the cases of William E. Cameron of Virginia, elected in 1881, Daniel J. Russell of North Carolina, elected in 1896, and Alvin Hawkins of Tennessee, elected in 1880. Information on racial composition (columns (3) and (4)) based primarily on Logan (2020), augmented with additional Black officeholders from the U.S. Postal Service (2025) and South Carolina Legislature (2025). Estimates based on linear running polynomials and the MSE-optimal bandwidth from Calonico, Cattaneo, and Titiunik (2014). The split-sample p-value corresponds to the null hypothesis that the difference between coefficients on *Democratic Loss*$_{c\tau}$ across subsamples is zero (e.g., between columns (1) and (2)). 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. $*p < .10$, $**p < .05$, $***p < .01$.

for a poor Democratic performance. Although 99% of our observations had Black populations as of 1880,[31] effects are driven wholly by those with above-median Black population shares. Estimate differences for these sample splits are mostly significant at conventional levels.

Additional 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 (Glaeser 2005; Epperly et al. 2020).

Table VIII provides quantitative evidence in support of this interpretation. Concretely, we estimate effects separately for county-years with any Jim Crow voting laws enacted and those without, based on whether a county's state had implemented any ballot requirements (e.g., literacy tests, multibox laws) or poll taxes from Jones, Troesken, and Walsh (2012). First, Table VIII, columns (1) and (2) confirm that lynchings of Black people systematically followed Democratic electoral losses before the introduction of Jim Crow laws but not after, with differences significant at $p < .05$.[32] Columns (3)–(6), meanwhile, split our newspaper sample along the same lines, first using the full set of newspapers in columns (3) and (4) and only Democrat-affiliated ones in columns (5) and (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 column (4) a reversal of estimate sign among the entire newspaper sample for the Jim Crow period, at which point Democrats had been firmly entrenched. We discuss possible reasons for this result now.

7. *Reverse Backlash.* Several minor results throughout the article suggest the existence of a limited reverse backlash effect

31. The average sample county contained 35.2% (st. dev. = 23.9) Black population shares in 1880.

32. Of course, lynching continued to occur for reasons unrelated to our treatment (Wells 1895; Jones, Troesken, and Walsh 2017).

TABLE VIII

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	0.160***	0.010	0.265***	−0.283***	0.291***	−0.014
	(0.056)	(0.050)	(0.044)	(0.060)	(0.048)	(0.074)
Election period FE	Yes	Yes	Yes	Yes	Yes	Yes
Election cycle year FE	—	—	Yes	Yes	Yes	Yes
State FE	Yes	Yes	No	No	No	No
City FE	—	—	Yes	Yes	Yes	Yes
County spatial polynomial	Yes	Yes	No	No	No	No
Optimal bandwidth	14.68	22.48	8.37	11.13	9.28	11.32
Running polynomial	Linear	Linear	Linear	Linear	Linear	Linear
Split-sample p-value	.03	.03	.06	.06	.45	.45
Control outcome mean	0.15	0.12	0.16	0.22	0.17	0.23
Observations	844	818	1,133	591	886	386

Notes. This table reestimates Tables II and V, conditional on a county's state having any Jim Crow laws (i.e., poll taxes, literacy test, multibox, or secret ballot laws) as of a given county-year. State-level Jim Crow data come from Jones, Troesken, and Walsh (2012). Estimates based on linear running polynomials and the MSE-optimal bandwidth from Calonico, Cattaneo, and Titiunik (2014). The split-sample p-value corresponds to the null hypothesis that the difference between coefficients on *Democratic Loss*$_{c\tau}$ across subsamples is zero (e.g., between columns (1) and (2)). 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. $*p < .10$, $**p < .05$, $***p < .01$.

in counties where white Democrats did not monopolize the local elite, such as in parts of Virginia, North Carolina, and Tennessee. These include Table V, columns (9) and (10), Table VII, column (4), and Table VIII, column (4). In such places, close Democratic wins were more likely to spur anti-Black antagonism, in the form of anti-Black newspaper articles and racial violence. Such estimates tend to be imprecise, but that may be an artifact of the rarity of such places.

Taking these point estimates seriously, there are several plausible explanations for this reversal of sign. One is that non-Democratic elites decreased their reporting on anti-Black crime accusations after local Democratic losses, perhaps to forestall violence, spurring a relative decline in racial antagonism. Another explanation is that non-Democratic elites also sometimes resorted to antiminority politics, particularly during the Jim Crow era. That is, Democrats did not have a monopoly on the use of anti-Black hatred—they merely had more channels through which to disseminate it in the South—and even relatively prominority parties may be willing to resort to antiminority politics when it strategically suits them. The latter would remain consistent with our conceptual framework in Section II.B, insofar as Black voters were increasingly excluded from non-Democratic majority coalitions in such places.

V. Solidifying the South: Lynching and Electoral Reversal

Throughout this article, we have shown that poor Democratic performance in presidential elections precipitated an elite-led backlash in places across the post-Reconstruction South. Absent de jure means to disenfranchise Black voters, 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 final section, we focus on the political legacies of these events. Using a variety of approaches, we provide suggestive evidence that they indeed helped facilitate an electoral reversal of fortune for Southern Democrats by the early twentieth century.

To begin, we estimate raw correlations between Black lynchings in τ and the probability of later Democratic electoral victory in a county, using presidential elections between 1904 and

1912. Table IX, columns (1) and (2) show that overall and among those counties that fall within the MSE-optimal bandwidth of our preferred specification in Table II, places that experienced Black lynching events in presidential postelection periods between 1880 and 1900 were 2–4 percentage points more likely to see Democrats win in presidential elections between 1904 and 1912. This correlation is particularly strong in the bandwidth-restricted column (2), suggesting a greater marginal benefit of violence under relatively close electoral conditions.

Columns (3) and (4) further rely on the local variation in Black lynching around the RD win-lose threshold for τ, based on estimation that holds the Democratic loss margin in τ fixed at zero. These results show that, whereas a worse electoral performance by Democrats between 1880 and 1900 predicts a lower probability of Democratic victory between 1904 and 1912 among counties where racial violence did not materialize, places where Black lynchings did occur after presidential elections were in fact more likely to be won by the Democratic Party in the early twentieth century. In addition to its standalone, positive correlation with downstream Democratic electoral success, racial violence attenuates the correlation between past vote shares and later victory, as measured by the interaction effect between loss margins and lynching.

Thus far, the results in Table IX, columns (1)–(4) have been largely descriptive. Yet the description they offer is nonetheless real—showing how many once-similar places around the win-lose threshold went on to be rather different from each other in the early twentieth century, in terms of electoral composition, depending on whether local Democrats barely lost, with violent backlash ensuing in turn, during the late nineteenth century. Overall, this suggests that racial antagonism helped facilitate the consolidation of the solid South by 1912.

Further supporting these results, we also estimate a causal mediation analysis, which better retains the causal interpretation of our baseline RD framework. Table IX, column (5) explores the effect of close Democratic losses in presidential elections at the county level on the probability of Democratic victory in the early twentieth century through the Black lynching channel. In addition to the direct correlation between Democratic electoral performance in the nineteenth century and local Democratic victory in the twentieth century, this shows how Black lynchings had a pos-

TABLE IX

ELECTORAL REVERSALS OF FORTUNE

Dependent variable	Democrat won county in 1904, 1908, or 1912				
	(1)	(2)	(3)	(4)	(5)
Any black lynching in period τ	0.021***	0.038***	0.051***	0.047***	0.020**
	(0.006)	(0.009)	(0.013)	(0.011)	(0.008)
Democrat loss margin in election τ			−0.001***	−0.003***	
			(0.000)	(0.001)	
Democrat loss margin			0.001***	0.003***	
× any black lynching			(0.000)	(0.001)	
Causal mediation analysis					
Democrat lost county in election τ					−0.013
Direct effect					(0.018)
Democrat lost county in election τ,					0.004*
Indirect effect through lynching					(0.002)
Election period FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Spatial polynomial	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	100.00	15.65	100.00	15.65	15.65
Running polynomial	—	—	Linear	Linear	Linear
Outcome control mean	0.95	0.96	0.95	0.96	0.96
Observations	5,914	1,481	5,914	1,481	1,481

Notes. This table reports estimates for whether there was a Democratic electoral victory in a given county in the 1904, 1908, or 1912 presidential elections on whether that county experienced a Black lynching event in the four-year election period following a presidential election $\tau \in \{1880, \ldots, 1900\}$. Counties matched with future elections based on county identifiers. Columns (2) and (4) limit the sample based on the MSE-optimal bandwidth derived from Calonico, Cattaneo, and Titiunik (2014) in Table II, column (3). Columns (3) and (4) further interact the indicator for Black lynchings in τ with the Democratic Party's vote share margin of loss in the preceding presidential election. Column (5) presents estimates of indirect effects of close Democratic losses in presidential elections through the Black lynching channel, as well as direct effects, based on output from the mediate package in Stata. 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. $*p < .10$, $**p < .05$, $***p < .01$.

itive mediating effect, helping bring about an electoral reversal of fortune for Southern Democrats.

These results remain qualitatively similar when using anti-Black newspaper accusations rather than Black lynchings, consonant with their mediating role in enkindling racial violence in the aftermath of local Democratic presidential losses (see Online Appendix Table E.3). Results are similar when looking at Democratic success at the local level, as measured by the number of Democratic local officeholders over the 1904–1912 election periods (see Online Appendix Table E.4). These estimates, which are conditional on the total number of local officeholders (e.g., mayoral, postmaster) matched to a county in the Political Graveyard (Kestenbaum 2023), represent a reversal on the short-run relationship between Democratic performance in presidential contests and local Democratic officeholding, as shown in Online Appendix Table A.1.

Last, we consider an important channel through which such racial antagonism helped bring about an electoral reversal of fortune for the Southern Democratic Party: local political participation. After 1900, Black voter turnout collapsed in the South, to the benefit of the Democratic Party (Jones, Troesken, and Walsh 2017). The results in Online Appendix Table E.5 show how Black lynchings plausibly contributed to these trends.

Overall, these findings are consistent with lynching of Black people serving to bring about a boost in Democratic Party's electoral fortunes in post-Reconstruction South. Although somewhat suggestive, these results further clarify the motives underlying elite efforts to foment racial violence, building on the prior insights of contemporary observers (Wells 1895) and modern political economists (Jones, Troesken, and Walsh 2017; Williams 2022).

VI. 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 ended de jure racial discrimination in the 1960s, the legacy of this prolonged disenfranchisement 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 twentieth century, often enduring continued discrimination elsewhere (Boustan 2010; Craemer et al. 2020; Althoff and Reichardt 2022; Collins and Wanamaker 2022; Derenoncourt 2022).

Bringing about progress on these dimensions means first understanding root causes. As we show, racial violence was central to denying Black people 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 bringing about a broad-based Black retreat from political and economic society (Cook 2014; Jones, Troesken, and Walsh 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 across the South, in spite of emancipation and the Fifteenth Amendment. These findings have important implications for the modern day, as a wave of democratic backsliding spreads throughout the Western world. Indeed, absent sufficient enforcement, ethnoracial violence remains a tool for promoting the disenfranchisement of minority people and the survival of exclusionary norms and institutions.

Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

Data Availability

The data underlying this article are available in the Harvard Dataverse, https://doi.org/10.7910/DVN/08YUBP (Testa and Williams 2025).

Tulane University and National Bureau of Economic Research, United States
American University, United States

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