Intragroup Redistribution in Diverse Societies: Evidence from Black Disenfranchisement

Jeffrey L. Jensen[*] Giuliana Pardelli[†] Tereza Petrovicova[‡]

Jeffrey F. Timmons[§]

September 21, 2024

Abstract

We examine intragroup redistribution in diverse societies using the most significant case of democratic backsliding in American history—the disenfranchisement of Southern Blacks during the Jim Crow era. This episode not only stripped Blacks of the right to vote, but it also changed the balance of power among Southern whites. Plantation elites could now control state governments by relying solely on support from whites in high Black-share counties, whereas they had previously needed statewide white support. Using fine-grained original data on state spending, we document how this shift intensified intra-white disparities in public goods access: allocations favoring elite interests rose and overall redistributive spending benefiting whites fell, with one key exception—redistributive spending on whites in high Black-population counties also rose. Not only does our paper demonstrate the conditional nature of intragroup solidarity and redistribution in divided societies, but it also reveals that there were three keys to the origin of the "Solid South"— political geography, notably the uneven spatial distribution of different social groups, legislative apportionment rules, which were based on population (rather than eligible voters), and targeted redistribution, which united elites with low-income whites in very specific geographies.

[*]New York University Abu Dhabi, Social Science Division, jeffrey.jensen@nyu.edu

[†]New York University Abu Dhabi, Social Science Division, giuliana.pardelli@nyu.edu

[‡]University of California San Diego, Department of Political Science, tpetrovicova@ucsd.edu

[§]New York University Abu Dhabi, Social Science Division, jft3@nyu.edu

1 Introduction

The large literature across the social sciences investigating the effects of ethnic and other forms of diversity on public spending has largely found that increased diversity tends to reduce public goods provision and overall redistribution (e.g., Alesina et al. 1999; Alesina and Glaeser 2004; Habyarimana et al. 2007; Miguel and Gugerty 2005; Dahlberg et al. 2012; Stichnoth and Van der Straeten 2013; Tabellini 2020). While most of this literature focuses on the overall level of redistribution in diverse societies, relatively little work has examined the determinants of within-group redistribution, particularly among the dominant group. There are some notable exceptions. For instance, Lieberman (2003) demonstrated that in Apartheid-era South Africa, a cross-class, white coalition sustained the implementation of high progressive taxes on white elites, facilitating increased redistribution. Conversely, Suryanarayan and White (2021) have shown that the enfranchisement of the Black population during the Reconstruction era in the US South led elites to form cross-class white coalitions that limited redistribution by weakening the capacity of Southern states to collect taxes. Given the paucity of work on this question, we still do not fully understand when outgroup marginalization leads to higher intragroup redistribution, nor do we know the determinants of elite incentives to forge cross-class coalitions sustained by redistribution.

We study these questions by examining the case of Black disenfranchisement in the American South during the Jim Crow era. This episode, arguably the most significant instance of democratic backsliding in American history, demonstrates how the share of lower-income, within-group voters required for elites to maintain power influences the extent and nature of intragroup redistribution. In this case, not only did the implementation of franchise restrictions between 1889 and 1906 by the eleven former Confederate states remove the right to vote from the near entirety of the South's Black population, but it also substantially reduced the number and geographic scope of lower-income white voters needed by rural elites to consolidate their uncontested power. Before Black disenfranchisement, the Democratic Party, the traditional party of Southern planters, required statewide support of white voters to win control of Southern state governments. After the removal of Black voters, however, the Democratic Party only needed support from whites in areas with a high Black population share to retain power. This shift occurred because redistricting rules mandated that Southern state legislatures apportion representation based on total population, including the disenfranchised Black population. Given the spatial concentration of Southern Blacks, these rules amplified the voting power of Southern whites residing in the 'Black Belt' and other former plantation areas across the South, relative to the majority of Southern whites living in counties with lower Black population shares. Prior to this shift, state redistributive expenditures, while modest, benefited lower-income whites across the state. Following it, overall redistributive spending fell, while targeted redistribution to lower-income whites in the high Black-share areas increased. Thus, our findings demonstrate that intragroup redistribution fundamentally depends on the level of support required from lower-income members of the dominant group for elites to maintain power. Equally important, they reveal three key factors that gave rise to more than a half-century of uncontested one-party rule in the American South: political geography, legislative apportionment rules, and targeted redistribution.

We begin by substantiating our political claims. Specifically, we present evidence that Black disenfranchisement drastically reduced the share and geographic scope of white support necessary in each state for the Democratic Party to control both the state house and governor's office. Using an original dataset specifying the apportionment of representation to each county in each chamber of the state legislatures, we determine the share of white voters needed from counties with the highest Black populations to secure a majority of legislative seats. We find that in disenfranchising states, legislative majorities could be achieved with support solely from white voters in counties constituting between 27% and 40% of the statewide white population. For the governorship, the removal of Black voters eliminated a key portion of the support base for non-Democratic candidates (e.g., Republicans, Populists), thus reducing the number of white votes Democrats needed to win elections at the state level. Our comparative analysis of statewide votes for non-Democratic candidates versus Democratic candidates from counties with high Black populations reveals that prior to disenfranchisement, even unanimous support from whites in high Black-share areas was insufficient to guarantee a Democratic victory. Following disenfranchisement, however, Democratic candidates were able to win statewide office with votes exclusively from whites in these areas. In short, Black disenfranchisement ensured that the Democratic Party could gain uncontested control over the state government simply by consolidating support from white voters in counties with high Black populations.

Next, we investigate how these changes in the distribution of political power influenced government spending patterns. Using a novel dataset documenting annual state government expenditures from 1880 to 1910 across the fourteen Southern states—defined as those states where slavery was legal and prominent in 1860—we analyze the fiscal implications of Black disenfranchisement for whites. Our data include not only total state spending, but also disaggregated data on key expenditure categories: judiciary, prisons, insane asylums, debt and interest payments, white colleges and universities, Confederate-veteran pensions, and common schools (the term for pre-high school public education at the time). These categories not only represent the majority of state expenditures in this period (averaging 72% of each state's budget), but they also encompass items that can be classified as either primarily redistributive or non-redistributive. Specifically, we categorize all spending on common schools and veteran pensions, which disproportionately benefited lower-income whites, as redistributive. Conversely, we classify state spending on the other five categories—asylums, courts, debt, prisons, and higher education—as non-redistributive. Furthermore, as detailed later, there are strong reasons to believe the last three likely disproportionately benefited elites.

Employing a difference-in-differences approach, we examine how spending patterns differed between Southern states that imposed voting restrictions and those that had not yet done so, or never would. While we do not observe a change in the overall level of state spending following Black disenfranchisement, our findings reveal a significant shift in the composition of state expenditures with meaningful distributional consequences for whites. We find that redistributive spending declined following the enactment of franchise restrictions. Yet, these restrictions did not result in a corresponding decrease in state funding for non-redistributive goods. Notably, spending on categories that likely benefited elites—such as colleges, prisons, and debt service—saw a significant increase. This is consistent with our argument that once Black disenfranchisement was formally adopted, Southern elites no longer needed the broad support of most whites within their states. Consequently, state-level spending shifted away from the categories that favored lower-income whites.

Critically, we also examine *which* whites in each state received more or less redistributive spending, using original data on the amount of state-level school funding apportioned to each county. Although we observe a decline in overall state-level redistributive spending—reflective of the reduced need for a broad-based white coalition—this overarching trend masks a sharp geographic divergence in state allocations for goods that benefit low-income whites. In line with our findings that white votes from high Black-share counties were both necessary and sufficient to secure total control of state governments, we show that state funding for common schools increased in counties with high Black population shares and decreased in other areas. Spending on Confederate pensions evinces a similar disparity. The increase in the geographic inequality of access to redistributive goods among whites highlights the reduced power of the majority of each state's whites following the disenfranchisement of Southern Blacks.

Our research makes three main contributions. First, it provides key insights for evaluating different political economy theories on distributive public spending in divided societies. Our case illustrates that the level of intragroup redistribution depends on the extent to which widespread within-group support is needed for elites to maintain power. Additionally, our analysis is relevant to understanding the distributive consequences of politically marginalizing minority groups in diverse societies. Our evidence indicates that these outcomes are contingent on how such marginalization alters the share of lower-income members of the dominant group needed for elites to maintain power. Under some circumstances, the marginalization of out-groups may compel elites to broaden their support base, potentially leading to increased within-group redistribution—e.g., the rise in progressive taxation observed during Apartheid in South Africa (Lieberman 2003). Conversely, in other circumstances, out-group marginalization may reduce the size of the within-group coalition necessary for dominant-group elites to maintain power, thereby diminishing their incentives to redistribute.

Second, our story identifies the mechanics underlying the formation of the "Solid South", a period in which the Democratic Party had uncontested control through the mid-1960s. It is well known that the adoption of Black disenfranchisement triggered the emergence of one-party rule (Kousser 1974), restoring political power back to the rural elites that had dominated Southern politics prior to the

Civil War (Olson 2024). To date, however, the specific mechanism connecting Black disenfranchisement to Southern elite hegemony has not been clearly identified. In particular, Black disenfranchisement could have led to multi-party contestation among whites based on class differences. Instead, we show that particular features of Southern political geography and apportionment rules created the conditions for rural-elite-led Democratic-Party hegemony. Although historians have mentioned the overrepresentation of high Black-share areas as a contributing factor (e.g., Grantham 1992; Perman 2003), the fundamental role that apportionment rules played in allowing rural elites to prevent statewide, multi-party contestation has not been fully recognized. Additionally, the pivotal role of lower-income whites in high Black-share areas during the post-disenfranchisement era has been largely overlooked. Our findings highlight that targeted redistribution to this particular subset of lower-income whites was crucial in sustaining the coalition that kept the Democratic Party in power.

This work also indirectly speaks to ongoing debates about this case and historical period. One longstanding debate questions the extent to which formal disenfranchisement meaningfully altered Southern politics. On one side, renowned scholars of the South, such as Key (1984), have argued that Southern Blacks were effectively disenfranchised through informal and often violent means before formal laws were enacted, suggesting that these legal restrictions had minimal additional impact on Southern politics. On the other side, scholars contend that while these informal tactics reduced Black political power, this group remained politically active and posed a significant electoral threat to Democratic Party dominance (Kousser 1974). Our findings support the latter perspective, demonstrating that Black disenfranchisement not only shifted the distribution power among whites but also redirected public spending away from the interests of lower-income whites. Additionally, our results align with previous analyses demonstrating that disenfranchisement bolstered Democratic dominance in state legislatures (Jones et al. 2012), as well as with recent research by Olson (2024), who utilized thousands of state legislative roll calls to illustrate that Black disenfranchisement shifted legislative voting patterns in favor of the Democratic Party and its more agrarian faction. Lastly, our work speaks to the literature examining the effects of Black disenfranchisement on Southern whites (Aldrich and Griffin 2018; Caughey 2019; Heersink and Jenkins 2020; Key 1984). Contrary to a strand in the historical literature that suggests that disenfranchisement facilitated a form of "progressivism for whites only" in the US South (e.g., Woodward 1981), our findings align with studies suggesting that Black disenfranchisement strengthened Southern elites vis-à-vis most lower-income whites (Kousser 1974, 1980).

Our final contribution is to provide the first broad picture of the fiscal impacts of Black disenfranchisement, using the most complete data of Southern state spending from this period that exists. While various aspects of this episode have been excavated and explored, there has not yet been a systematic examination of the isolated effects of Black disenfranchisement on overall state government spending and its distributional consequences—specifically comparing redistributive goods to other types of public spending. Our dataset not only complements other work showing that taxation and public spending in the South during this period were generally lower than in other states (Alston and Ferrie 2007; Galor et al. 2009; Jensen et al. 2024; Vollrath 2013), but also allows us to hone in on variation in redistributive spending within the South. Our findings align with Margo (2007), who demonstrated that, partly due to Black disenfranchisement, there was an increase in overall public spending (including both state and local expenditures) allocated to white students relative to Black students, and that this white-Black gap was most pronounced in the high Black-share areas. Furthermore, Kousser (1974) observed that disparities in public education among whites increased during this period, which he attributed to changes in local taxation. Overall, our results reveal that average state-level figures can also obscure significant geographic disparities in public spending among poor whites, highlighting the heterogeneous effects of Black disenfranchisement.

The remainder of the paper is organized as follows. We begin by substantiating our political claims, demonstrating how Black disenfranchisement in the American South diminished the need for statewide support from white voters. We then describe our data and archival sources. Next, we present a graphical analysis illustrating the expenditure trajectories of states with disenfranchisement laws and their temporal divergence from other states. The following sections outline our empirical approach and present our findings. We conclude with a discussion of the paper's limitations and its contributions to our understanding of intragroup redistribution in divided societies.

Rural Elites' Need for the Support of Lower-Income Whites

We first examine the claim that Black disenfranchisement significantly reduced the proportion of Southern whites needed for the Democratic Party to secure victories in both the state legislature and governorship across the eleven disenfranchising states. Validating this claim sets the stage for exploring the conditional nature of intragroup redistribution—that is, the idea that as the need for broad ingroup political support decreases, so do elite incentives for extensive redistribution. As we demonstrate, this shift in political power among Southern whites was enabled by the apportionment rules in state legislatures that included the disenfranchised population in their counts, ensuring that regions with high Black shares held the majority of legislative seats. This greatly magnified the value of white votes from high Black-population areas in state legislature elections. Yet, even in statewide gubernatorial elections, the exclusion of Black voters reduced the number of white votes needed to win. We show that non-Democratic candidates would have needed an implausibly large proportion of the white votes from low Black-share areas simply to compensate for the loss of Black non-Democratic voters, much less win gubernatorial elections. Overall, our evidence indicates that Black disenfranchisement allowed rural elites to control the state government by winning substantial support from low-income whites in areas with large Black population shares, thus eliminating the need for broad statewide support to maintain power.

The fact that the Democratic Party, the vehicle of Southern elites, initially required broad-based, statewide support of whites was itself a consequence of the enormous institutional changes imposed upon the South following its defeat in the US Civil War (1861-1865). Prior to the North's victory, the economies and political systems of Southern states were dominated by "planters," a small, rural elite whose wealth derived from the enslavement of millions of Black laborers (McCurry 2012; Wright 1978). Defeat not only resulted in the emancipation of more than 40% of the Confederacy's population but also guaranteed the right to vote to the formerly enslaved with the Congressional Reconstruction Acts of 1867 and 1868 (Foner 2014). While the redistributive threat posed by Republican "Reconstruction" governments had been violently eliminated by 1877, adult Black males formally retained the right to vote, continuing to pose a threat to the political dominance of the Democratic

Party (Kousser 1974; Perman 2003; Valelly 2009). In an environment of full adult male suffrage, suppressing this electoral challenge required extensive intimidation, violence, electoral manipulation, and fraud (Woodward 1981; Epperly et al. 2020; Jones et al. 2017; Weaver 2019). It also required a degree of progressive taxation and public spending on redistributive goods that, although modest by Northern standards, exceeded the levels previously seen in the South outside of the Reconstruction period (Jensen et al. 2024).[1]

Despite the widespread use of these illegal methods and the implementation of some redistributive fiscal policies, the region remained far from the one-party "Solid South" it would become in the 20th century. Instead, non-Democratic Party candidates for governor and the state legislature continued to receive substantial shares of the vote in many Southern states in the immediate post-Reconstruction years (approximately 1878-1900). Southern state legislatures in this period were characterized by two-party competition, and legislation remained relatively responsive to the preferences of both lower-income white and Black voters (Olson 2024). Furthermore, cross-racial class-based ("fusion") coalitions had successfully formed to win control of state governments in Virginia (early 1880s) and North Carolina (mid-1890s), and had nearly won statewide elections in most other states (Alabama, Arkansas, Florida, Georgia, Louisiana, Tennessee, and Texas). These anti-elite populists and often cross-racial working-class movements were driven in part by demands to reduce regressive taxation and increase public spending (Kousser 1974; Hahn 2006; Hyman 1989).

Democratic Party elites sought a more "permanent solution" to these rising electoral threats (Perman 2003, p. 15). Satisfying populist demands by increasing progressive taxation and spending on whites was an unpalatable option (Hyman 1989). Furthermore, the continued reliance on informal

[1] Although public education systems in the South were chronically underfunded compared to those in the North during the post-Reconstruction period, state funding for common schools significantly exceeded pre-war levels. Additionally, during this period, state-funded pensions for Confederate veterans were initiated. Consistent with the idea that elites required broad electoral support, areas experiencing the most intense electoral competition from populists witnessed the most substantial increases in pension support (Eli and Salisbury 2016).

repressive tactics was not entirely effective, as evidenced by the persistent voting of many Black citizens (Kousser 1999). Southern elites constantly feared a "powerful fusion coalition of Populists and Republicans who were either on the verge of taking control of the state government or in power already" (Perman 2003, p. 323). The historical record is clear that elites viewed the preservation of white supremacy through the disenfranchisement of Black voters as crucial for maintaining Democratic Party dominance and, by extension, their own hegemony. In a speech to the 1898 Louisiana Constitutional Convention which adopted poll taxes and a literacy test, Thomas J. Semmes, a delegate (and former Confederate senator) said: "What is the state? It is the Democratic Party [...] We meet here to establish the supremacy of the white race, and the white race constitutes the Democratic Party of this state."

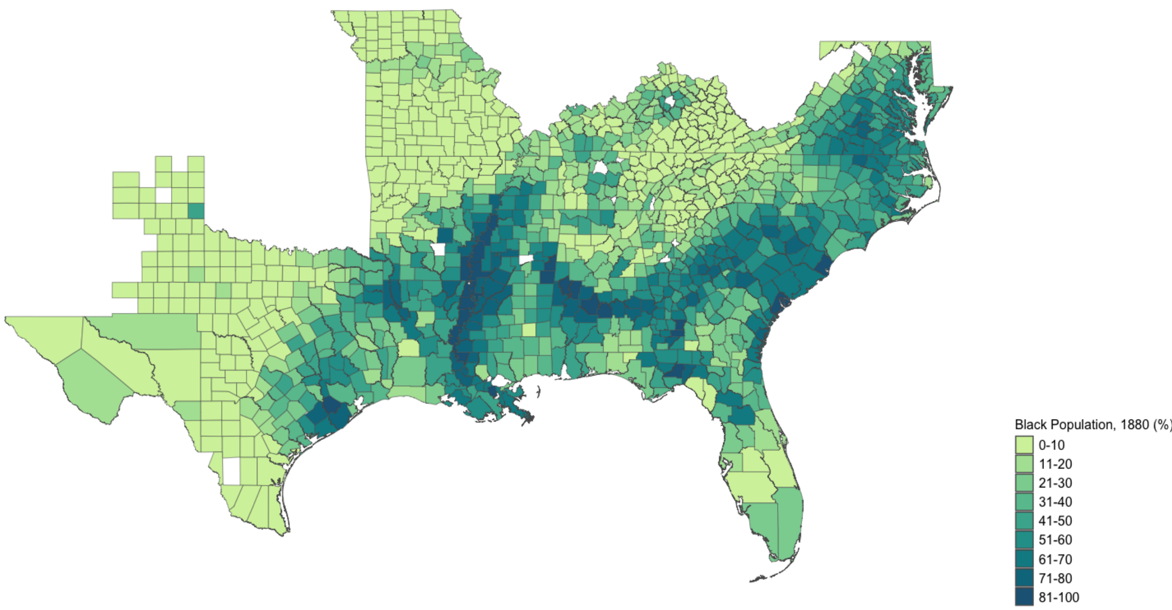
Thus, between 1889 and 1906, eleven Southern states adopted a range of disenfranchising measures through statutes, constitutional amendments, or entirely new constitutions, often using informal voter suppression tactics in order to secure the necessary majorities (Kousser 1974). Most states implemented several measures, including poll taxes, literacy tests, multibox laws, and secret ballot laws, that enabled local Democratic election officials to systematically disenfranchise African Americans (Kousser 1974; Perman 2003; Valelly 2009).[2] While not explicitly racial in nature, these restrictions removed the formal voting eligibility of the vast majority of the Black electorate (e.g., Greenberger 2022; Keele et al. 2021; Heersink and Jenkins 2020; Kousser 1974). Meanwhile, the voting rights of most lower-income whites were initially preserved through the adoption of 'grandfather' clauses and other similar mechanisms (Valelly 2009).

The disenfranchisement of Black voters significantly reduced electoral threats to the dominance of rural elites in two key ways, one well-understood and another less obvious but equally critical for our analysis. First, scholars of the South have long recognized that since most Southern Blacks supported the Republican Party (or "fusion" parties), their removal substantially reduced the total statewide support for parties contesting Democratic Party rule. Also, due to the legacy of slavery in

[2]We present these states, organized by the type of disenfranchisement measure and the date of adoption, in Appendix Table A.1.

the still largely agrarian-based Southern economy, Blacks, who comprised between 25% and 61% of the total population in each of the eleven disenfranchising states in 1880, were highly concentrated spatially. This is evident in Figure 1. Thus, areas where Blacks constituted a substantial share of the population, which were precisely where the preponderance of rural elites lived, no longer posed an electoral threat to those elites—both in local elections and in the election of representatives to the state legislature (Jones et al. 2012).

Figure 1: **Black Population Share across Counties, 1880**



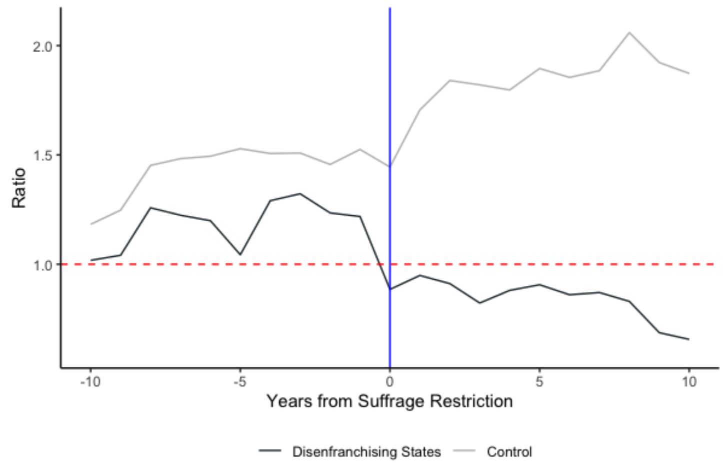
Yet, what scholars of this period have failed to fully document is that the disenfranchisement of Blacks also eliminated the need for elites to build a broad, statewide coalition of whites for the Democratic Party to maintain unrivaled political control. This effect occurred in both gubernatorial and, most dramatically, state legislative elections.

We first demonstrate that Black disenfranchisement reduced the need for statewide white support for Democrats to maintain control of the governorship. Using county-level returns from all gubernatorial elections between 1880 and 1910, we show that removing Black voters meant that Democratic governors could win solely with votes from high Black-share counties, defined as those with above-median Black-population shares. To do this, we calculate the total statewide votes cast for non-Democratic candidates and then divide this number by the total votes for the Democratic gubernatorial candidate only from the counties that exceed the Black-population share in the state's median county. A ratio below 1 indicates that the Democratic candidate could win with votes solely from high Black-share counties. Conversely, a ratio above 1 suggests that broader support was necessary for a Democratic victory.

Figure 2 illustrates the temporal progression of this ratio across the fourteen Southern states in our sample. The black line represents the average ratio of total statewide votes for non-Democratic candidates to votes for the Democratic Party's candidate solely from counties with an above-median Black population share in the eleven states that adopted disenfranchising measures. The gray line tracks the average ratio in the comparison group, which includes all states that did not have such measures in place in a given year. Prior to disenfranchisement, the average ratio is always above 1 indicating that votes from areas with higher Black shares were insufficient to overcome the total votes for non-Democratic candidates, usually a Republican or Populist. Thus, the Democratic candidate would typically require votes from their state's below-median Black share counties to win. After the introduction of suffrage restrictions, however, the average ratio is always below 1, indicating that the Democratic Party could win gubernatorial elections by relying solely on votes from counties with above-median Black shares. This pattern contrasts with the trend among states that did not have disenfranchising measures in place at the same time, where the average ratio of non-Democratic to Democratic votes consistently exceeds 1 throughout the entire period, indicating a continuing need for broader electoral support. Further supporting our claim, the evolution of this electoral ratio in individual states reveals that nine out of eleven states regularly posted values above 1 prior to implementing disenfranchisement measures. Following disenfranchisement, seven states only displayed ratios below 1. The remaining four states saw their ratios drop significantly post-disenfranchisement, often hovering close to 1, a marked change from their pre-disenfranchisement values. In other words, the need for broad-based statewide white support to win gubernatorial elections, which was essential before disenfranchisement, was eliminated thereafter.

Looking more closely at how the geographic distribution of votes shifted following disenfran-

Figure 2: **Ratio of Non-Democratic Votes in the State to Democratic Votes in High Black-Share Counties, 1880-1910**



Notes: The black line represents the average ratio of total Non-Democratic votes for governor across the state to Democratic votes in counties with an above median Black-population share. The gray line shows the average ratio in states that had not implemented voting restrictions yet, or never would. Our overall sample comprises 14 Southern states, including those that did not adopt disenfranchising measures. For each state that enacted voting restrictions in a given year, the counterfactual is calculated as the average ratio from states where these restrictions were not yet in place during the same year. When either line exceeds the threshold of 1, it indicates that the Democratic Party candidate did not receive enough votes from higher Black-share areas alone to win the gubernatorial election.

chisement provides additional evidence for our claim that the elimination of Black voters substantially reduced the need for Democratic candidates to rely on white support in low Black-share areas. As reported in Appendix Table A.2, non-Democratic (e.g., Republican, Populist, Fusion) candidates garnered a large proportion of their votes from the high Black-population share counties prior to disenfranchisement (on average, they received between 50 and 60% of their total votes from these counties).[3] After Black disenfranchisement, the average number of total votes for non-Democratic candidates in these same high-Black share counties declined by 66%.[4] The decline of predominantly

[3]Specifically, this is the average total number of votes for non-Democratic gubernatorial candidates in the above-state-median Black-population share counties for every election from 1880 until each state adopted its disenfranchising measure. We provide further details on how this is calculated in Appendix Table A.2.

[4]While in a few states the decline in total votes for non-Democratic candidates from high Black-share

Black non-Democratic voters was so large that it would have required an average of 67% of Democratic voters in counties with Black-population shares below the state's median to switch to the non-Democratic candidate merely to compensate for the votes lost, let alone secure a victory.[5] The unlikelihood of converting this many Democratic voters suggests why, as demonstrated in Figure 2, rural elites were able to win relying almost solely on white voters from high Black-share areas.

The far more important effect of Black disenfranchisement, however, was on the state legislature, the branch ultimately responsible for public spending. We demonstrate that Black disenfranchisement fundamentally changed the way the votes translated into seats, greatly overweighing white votes from high Black-population share counties. Disenfranchisement had this effect, in part, because representation in the legislature for each county was based on total population rather than eligible voters. Given the spatial concentration of the Black population (as shown in Figure 1), this bias to representation was also highly spatially concentrated, magnifying the value of these county's white voters inversely to their proportion of the county's total population. In fact, as we show, because this apportionment scheme so over-represented the high Black-population share areas in each state, it was not only true that Black disenfranchisement made it possible for a small minority of each state's white population to control the state legislatures but that winning these seats was essential for establishing control.

To demonstrate this effect, we focus on the apportionment of representation in the two legislative chambers of each state. Specifically, we calculate the proportion of white votes required to secure a majority of seats, relying solely on counties with the highest Black populations.[6] The results, presented in Table 1, are differentiated by legislative chamber (lower and upper house). The first column displays the proportion of each state's white population residing in the highest Black-share counties

---

counties was closer to 50%, in most states the decline was closer to 80%. We provide further details and the values for each state in Appendix Table A.2.

[5] Appendix Table A.2 details the specific values for each state.

[6] This calculation is based on the apportionment reflected in either state constitutions or statutes stipulating county representation as of 1910.

that together held a majority of lower house seats. For instance, for Alabama's House of Representatives, Democrats could win a majority of seats with counties that contained only 29% of the state's white population. These winning-coalition counties had a Black population share ranging from 40% to 88%, as detailed in the second column. In seven of the eleven states, Democrats would not need to win any county with less than 40% Black population share to secure a majority of seats. The third column shows the average Black-population share across the highest Black-share counties that held a majority of the seats in the House of Representatives. For Alabama in 1910, for example, this average was 64%. Columns 4 to 6 mirror this analysis for each state's upper house (i.e., the state senate). Across the 22 chambers in the 11 disenfranchising states, the proportion of the state's white population residing in these pivotal counties never exceeded 40%. That is, a minority of white voters residing in the highest Black-share counties was sufficient to secure a majority in each state's legislature. The average Black-population share in these counties in 1910 was 55%.

Combined, the evidence in Figure 2 and the apportionment data in Table 1 demonstrates not only how Black disenfranchisement significantly reduced the share of state whites required for Democrats to achieve total control of state governments, but also that significant support from poor whites in high Black-share counties was crucial. It is widely known the disenfranchisement of Blacks coincides with the onset of uncontested Democratic Party rule under the control of rural elites, an era known as the "Solid South" (Key 1984). Our results show that the unique political geography and apportionment rules of Southern states (in conjunction with the targeted redistribution we show shortly) were key mechanisms connecting Black disenfranchisement to the rise of the Solid South, and the avoidance of multi-party competition among whites.

Table 1: **State Legislature Majority in High Black-Population Share Counties, 1910**

	Lower House			Upper House		
	Share of State's White Pop. in Winning Coalition Counties (%)	Range of Black-pop. Share in Winning Coalition Counties (%)	Average Black-pop. Share in Winning Coalition Counties (%)	Share of State's White Pop. in Winning Coalition Counties (%)	Range of Black-pop. Share in Winning Coalition Counties (%)	Average Black-pop. Share in Winning Coalition Counties (%)
Disenfranchising						
Alabama	29	(40, 88)	64	27	(42, 88)	65
Arkansas	39	(17, 85)	47	36	(19, 85)	49
Florida	39	(40, 76)	52	37	(41, 76)	54
Georgia	36	(48, 86)	63	33	(50, 86)	64
Louisiana	34	(43, 92)	61	36	(42, 92)	61
Mississippi	30	(55, 94)	74	28	(56, 94)	75
N. Carolina	40	(31, 65)	46	38	(33, 65)	47
S. Carolina	37	(59, 87)	68	34	(61, 87)	69
Tennessee	38	(21, 75)	35	38	(21, 75)	35
Texas	38	(18, 64)	34	38	(18, 64)	34
Virginia	40	(35, 74)	51	39	(36, 74)	51
Non-Disenfranchising						
Kentucky	46	(10, 49)	19	45	(10, 49)	19
Maryland	31	(15, 52)	32	20	(24, 52)	35
Missouri	62	(3, 20)	7	55	(4, 20)	8

Notes: Columns 1 and 4 display the share of each state's total white population residing in the set of highest Black-population share counties comprising a majority of seats in the state's lower (1) and upper (4) houses, respectively, in 1910. Columns 2 and 5 report the lowest and highest Black-population share among this set of winning coalition counties in the lower (2) and upper (5) chambers. Columns 3 and 6 report the average black population share of these set of winning coalition counties. Disenfranchising are the 11 Southern states that adopted literacy tests and/or poll taxes. Sources: Kousser (1974); Valelly (2009); US Census, 1910. Legislative representation was manually coded from the relevant statutes or constitutional provisions specifying the apportionment for each chamber of each legislature circa 1910.

Black Disenfranchisement and White Ingroup Redistribution

Having illustrated how Black disenfranchisement reduced the number and shifted the geographic distribution of electorally significant low-income whites, we now turn our focus to its fiscal consequences, specifically examining state government expenditures. Our analysis utilizes an original annual state-level panel dataset covering fourteen Southern states for the same 1880 to 1910 period. We focus on the trajectories of eight distinct spending outcomes. Initially, we examine total state-level expenditures and subsequently analyze specific categories that we classify as either redistributive or non-redistributive, based on which social groups they disproportionately favored.

To represent redistributive spending benefiting lower-income whites, we analyze state expenditures on primary education (common schools) and Confederate veteran pensions. Primary school education is widely considered the key redistributive good of this era (Lindert 2004); this was particularly true in the American South, where state common school spending was financed with progressive property taxes and comprised approximately 25% of state expenditures (Jensen et al. 2024).[7] The 1880s and 1890s also saw a significant expansion by Southern state governments of pensions for Confederate veterans, funded through the same progressive property taxes.[8] Furthermore, they were exclusively directed towards lower-income white recipients, specifically impoverished Confederate veterans and their widows.

We also collected data on five expenditure categories that we classify as non-redistributive spending: debt (and associated interest) payments, courts, prisons, white higher education, and insane asylums. Beyond not benefiting lower-income whites disproportionately, it is also likely that some of these spending categories favored elites. For instance, due to the segregated nature of Southern educational institutions, we can accurately identify public funding directed toward white universities and colleges. Given the extremely low college enrollment rates during this period, these expenditures almost certainly primarily benefited white elites.[9] Additionally, the allocations for incarceration—which include state spending on penitentiaries and prisons, along with associated costs such as transporting convicts and capture fees—were, in all likelihood, a priority for rural elites, as Southern prisons were not only used for standard penal purposes but also as a tool to coerce and control the labor supply of Southern Blacks (e.g., Muller 2018; Rubio-Ramos 2022; Schwarz 2023). A similar argument likely also applies to debt (Queralt 2022). Collectively, these five categories accounted for an average of approximately 43% of total state expenditures between 1880 and 1910.

[7] Although our total includes funding for Black common schools, the allocation disproportionately benefited white students, especially towards the end of our study period (Margo 2007). We discuss this issue further below.

[8] Unlike Union veterans, whose pensions were funded and provided by the federal government, pensions for Confederate Army veterans were funded entirely by Southern state governments.

[9] Less than 3% of American adults aged 24 and older held a college degree in 1910 (*Statistical Abstract of the United States*, U.S. Census Bureau, 1999, Tables 265 and 1426).

The data was manually coded from the various annual or biennial reports of each state's comptroller, auditor, or treasurer. This extensive effort yielded data for 403 out of a potential 434 state-year observations, covering total state spending and disaggregated expenditures across seven categories. Notably, no state lacks data for more than five out of the possible 31 years. This data set is, to our knowledge, the most comprehensive and detailed panel available for this period. Such thorough coverage allows us to analyze the fiscal impacts of historical developments from this period with a higher level of precision than previous studies. Details on the specific reports and years used to construct each variable are thoroughly documented in Appendix B, which provides a comprehensive overview of the data collected for this study.

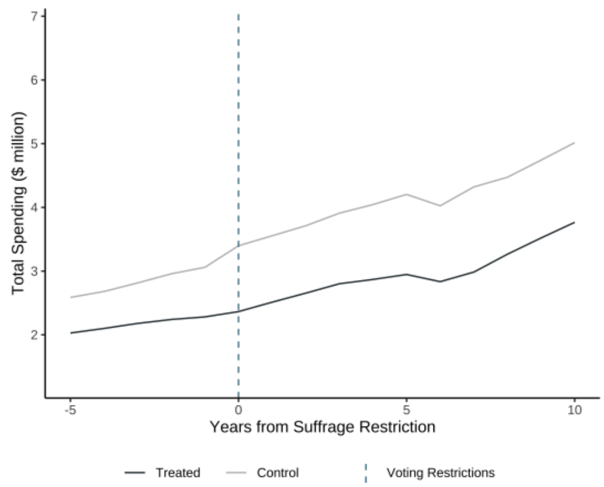
Trends in State Expenditures

We begin by graphically displaying the trends in each of our fiscal outcomes before and after the implementation of franchise restrictions.[10] Figure 3 shows total state government spending before and after disenfranchisement. The black line represents total expenditures in states that implemented disenfranchising measures, referred to as "treated" states, while the gray line illustrates expenditure trends in the comparison group. This group reflects general trends across all states, regardless of changes in their voting laws. States are aligned so that year zero corresponds to the year when they implemented either a poll tax, a literacy test, or both. The parallel trajectories of the two groups before and after the enactment of these laws indicate that the voting restrictions had minimal impact on the overall trends in state expenditures.

Next, we examine the changes in trends across various expenditure categories as a share of total spending. Figure 4 illustrates expenditures on the two items that we classify as redistributive goods: common schools (4a) and pensions (4b). Figure 5 presents trends in the share of state spending on the five non-redistributive goods: white higher education (5a), prisons (5b), debt and interest (5c), courts (5d), and asylums (5e).

[10] To construct these figures, we follow the method outlined by Paglayan (2022).

Figure 3: **Total State Spending ($) in States with and without Voting Restrictions**



(a) Total State Spending

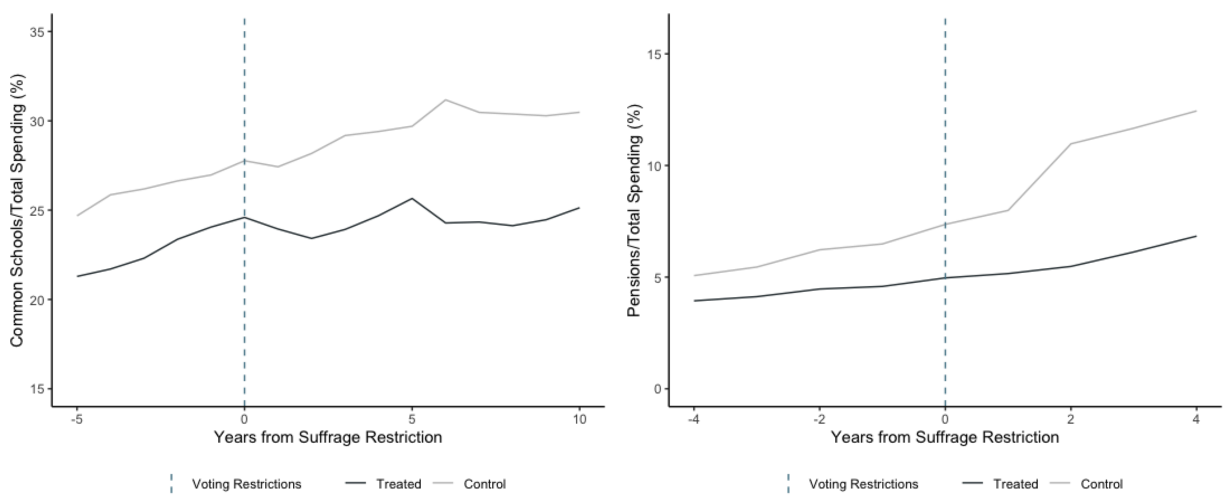
Notes: The black line represents the average state expenditure across disenfranchising states. The gray line illustrates the trend among comparison group states, which encompasses both never-treated and not-yet-treated states. For each state that implemented voting restrictions in a specific year, we calculate the average expenditure from all states without such restrictions during the same year. We then aggregate these values based on the number of years before and after the disenfranchisement to construct the counterfactual trend represented by the gray line.

Redistributive spending exhibits distinct patterns across categories. As shown in Figure 4a, trends for the share of state spending on common schools display a modest divergence between treated and comparison states. We will explore this further in subsequent sections by analyzing how state governments distributed common school funds within states. In contrast, Figure 4b shows a more pronounced divergence in pension spending between states that implemented disenfranchisement measures and those that did not, or had yet to do so. These graphical patterns provide preliminary evidence suggesting that the adoption of suffrage restrictions may have changed the trajectory of redistributive expenditures across states.

In comparison, the trends in state expenditures for the five non-redistributive goods exhibited different patterns. Following Black disenfranchisement, we observe either an increase in the proportion of spending allocated to certain categories—such as white higher education (Figure 5a), prisons (Figure 5b), and debt payments (Figure 5c)—or the spending trajectory between the treated and comparison states remained unchanged, as seen in allocations to asylums (Figure 5d) and courts (Figure

5e). Particularly noteworthy is the positive shift in the share of spending allocated to the three categories most likely favored by elites. Coupled with the decline in spending on redistributive goods, these trends suggest that Black disenfranchisement led to a reallocation of state funds away from goods benefiting lower-income whites across the state toward those aligning with elite preferences.

Figure 4: **Redistributive Spending (% of State Total) in States with and without Voting Restrictions**

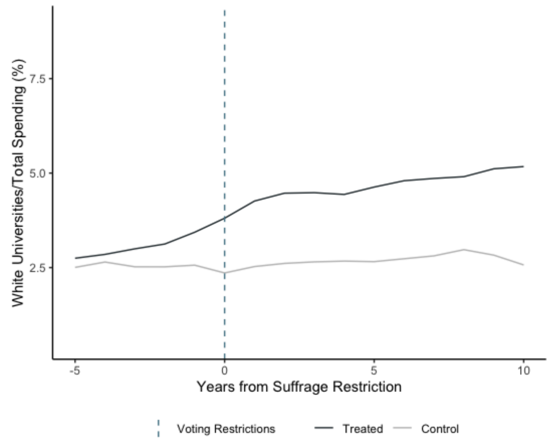


(a) Spending on Common Schools (%)

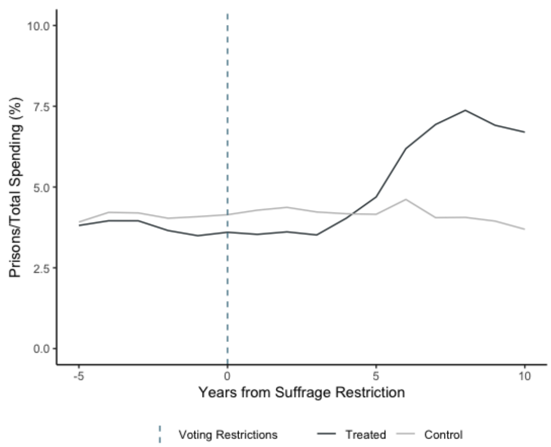
(b) Spending on Pensions (%)

Notes: The black lines represent the average share of state expenditures on (a) common schools and (b) pensions in disenfranchising states. In Figure (a), the comparison group encompasses both states that never adopted these restrictions and those that had not yet done so. The counterfactual trend, represented by the gray line, is calculated by averaging the expenditures of all states without voting restrictions for each year a particular state implemented such measures. These averages are then aggregated according to the number of years before and after disenfranchisement. In Figure (b), the comparison group is limited to not-yet-treated states only, as the never-treated states did not record pension spending during the period under study. As a result, the post-treatment period in this figure is shorter, reflecting the number of years that elapsed between the implementation of suffrage restrictions in the last two states to adopt such measures.

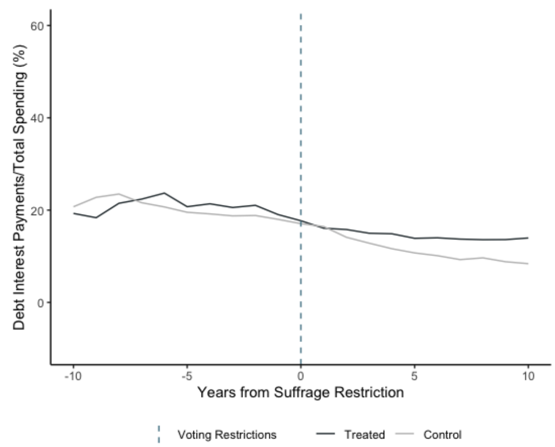
Figure 5: **Non-Redistributive Spending (% of State Total) in States with and without Voting Restrictions**



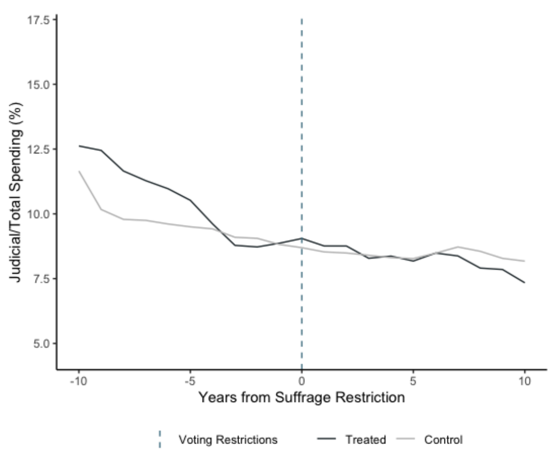
(a) Spending on White Higher Education (%)



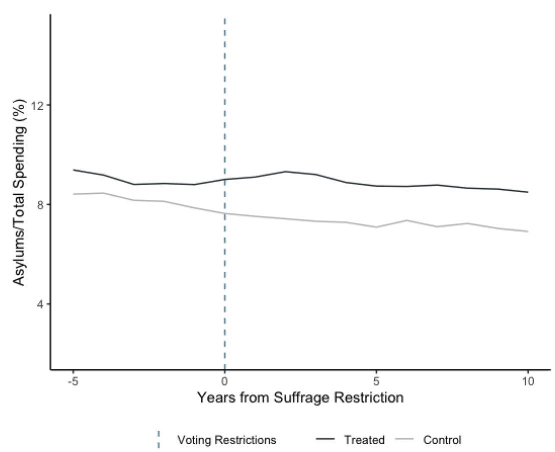
(b) Spending on Prisons (%)



(c) Spending on Debt & Interest Payments (%)



(d) Spending on Courts (%)



(e) Spending on Asylums (%)

Notes: The black lines show average share of spending on (a) white higher education, (b) prisons, (c) debt and interest payments, (d) courts, and (e) insane asylums across disenfranchising states. The gray lines reflect the average trend of the comparison group, which includes both never-treated and not-yet-treated states.

Empirical Strategy

To estimate the effects of suffrage restrictions on state spending, we employ the estimation method proposed by Callaway and Sant'Anna (2021), which compares the before-and-after differences in outcomes between states that introduced suffrage restrictions and those that did not change their voting laws during the same period. This estimator avoids the shortcomings of traditional two-by-two difference-in-differences comparisons by providing reliable estimates even when treatment effects vary over time or among treated units. The key assumptions—no anticipation of policy changes and the existence of parallel trends in the absence of policy intervention—are further discussed in subsequent sections.[11]

Our independent variable measures the presence of voting rights restrictions in each Southern state during a given year. We define the initiation of these restrictions as the earliest year when poll taxes and/or literacy tests were implemented. Once introduced, these measures were not reversed during our period of study.

Main Results

Table 2, Panel A presents the results from the simple aggregation of the group-time average treatment effects using the Callaway and Sant'Anna (2021) estimator. This aggregation provides a weighted average of all group-time average treatment effects, with weights proportional to group size, and avoids the issue of negative weights that can arise in two-way fixed effects regressions.

The coefficient on the *Suffrage Restriction* indicator in Model 1 reveals that the implementation of voting laws led to an average reduction in total public spending, although this effect is not precisely

[11] We favor the Callaway and Sant'Anna approach over two-way fixed effects (TWFE) regression model because the latter relies on strong assumptions about the uniformity of treatment effects, which are necessary for delivering consistent estimates in staggered adoption research designs (see, e.g., Sun and Abraham 2021; Callaway and Sant'Anna 2021; de Chaisemartin and D'Haultfoeuille 2022; Goodman-Bacon 2021).

estimated. A similar trend is observed in spending on redistributive goods. Specifically, expenditures on both common schools (Model 2) and pensions (Model 3) show negative coefficients. While the coefficient for common schools is not statistically different from zero, the coefficient for pension spending is both large and statistically significant, indicating that the implementation of voting restrictions resulted in a decrease in this category equivalent to approximately three quarters of a standard deviation. In contrast, expenditures on colleges (Model 4), prisons (Model 5), and debt financing (Model 6) all showed positive coefficients, with the latter two being statistically significant. The implementation of voting restrictions led to a 50% increase in prison expenditures over the average and a 54% rise in debt service payments. However, the impact of suffrage restrictions on spending for the judiciary (Model 7) and asylums (Model 8) was negligible.

Table 2: **Effect of Suffrage Restrictions on State Expenditures: Average ATT Estimates**

Panel A *Simple Aggregation*	Total Spending	Schools	Pensions	Colleges	Prisons	Debt	Courts	Asylums
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Suffrage Restrictions	−0.194	−0.131	-0.174^{***}	0.034	0.063^{**}	0.222^{**}	−0.028	−0.009
	(0.157)	(0.129)	(0.050)	(0.021)	(0.031)	(0.106)	(0.023)	(0.025)
Observations	451	448	363	447	449	448	451	451
Panel B *Group Aggregation*	Total Spending	Schools	Pensions	Colleges	Prisons	Debt	Courts	Asylums
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Suffrage Restrictions	−0.117	−0.079	-0.136^{***}	0.045^{**}	0.060^{**}	0.191^{**}	−0.019	0.004
	(0.132)	(0.115)	(0.030)	(0.018)	(0.025)	(0.081)	(0.021)	(0.028)
Observations	451	448	363	447	449	448	451	451

Notes: *p<0.1; **p<0.05; ***p<0.01. Models estimated using the Callaway and Sant'Anna (2021) estimator. The unit of analysis is the state-year. States are coded as treated after the first suffrage restriction is implemented. Standard errors are clustered at the state level. The control group includes not-yet-treated states.

Panel B reports the results of an alternative method of aggregation, which examines the average effects specific to each cohort. This approach addresses a limitation of the simple aggregation method, which can overweight the effects of early-treated groups due to their more frequent observation during post-treatment periods. According to Callaway and Sant'Anna, this parameter is a preferred measure for summarizing the overall impact of a treatment, as it averages group-specific treatment effects across cohorts. The results show slight changes in coefficient magnitudes, yet the substantive conclusions remain consistent. Spending on redistributive goods either declined, as seen with pensions, or stagnated, as in the case of common schools. In contrast, spending on non-redistributive goods showed either minimal change in response to restrictions (courts and asylums) or increased, as in the case of items plausibly aligned with elite priorities, such as debt, colleges, and prisons. Notably, in Panel B, college spending increases by 45% above the mean following the implementation of suffrage restrictions. In Appendix Figure A.8, we analyze spending shares for each item, rather than absolute values, as the dependent variables. The average ATT estimates for each spending category as a percentage of total spending align with our main findings.

Event Study

To test for parallel trends and assess dynamic treatment effects during the post-treatment period, we present event study results generated using the Callaway and Sant'Anna (2021) estimator, which is robust to treatment effect heterogeneity. These figures confirm the parallel trends assumption: the coefficients for the years leading up to the introduction of franchise restrictions are uniformly close to zero, indicating the absence of discernible pre-trends across all dependent variables.

The dynamic event-study estimates, illustrated in Figure 6, reflect the average treatment effect based on the duration of exposure to voting restrictions, covering five years before and ten years after disenfranchisement. Echoing the simple graphical evidence provided in the previous section, the dynamic event study coefficients reveal no significant impact of voting restrictions on overall state spending (a). Additionally, there is no evidence that disenfranchisement had any meaningful impact on the allocation of state resources to common schools (b). Both the event study estimates and the point estimates of the average and cohort-specific ATTs, shown in Appendix Figure A.1, consistently approach zero. The impact on pension spending (c), however, is strongly negative. All point estimates of the group-specific average treatment effects are negative, and every estimate, save for one, is statistically significant (Figure A.2).

Spending on non-redistributive goods exhibits varying patterns. The three categories that we think most closely aligned with elite interests—white colleges (d), prisons (e), and debt and interest payments (f)—exhibit increasing post-treatment dynamic effects. Specifically, for college expenditures, six out of seven group-specific average treatment effects are positive, with five statistically significant (Figure A.3). Similarly, prison expenditures show a consistent increase after the imposition of suffrage restrictions, with nearly all group-specific average treatment effects being positive, of which three are statistically significant (Figure A.4). Debt and interest payments also rise post-treatment, with nearly all group-specific effects being positive and three reaching statistical significance (Figure A.5). In contrast, expenditures on courts (g) and insane asylums (h) exhibit no notable change post-disenfranchisement, with dynamic event study coefficients remaining close to zero (Figures A.6 and A.7).

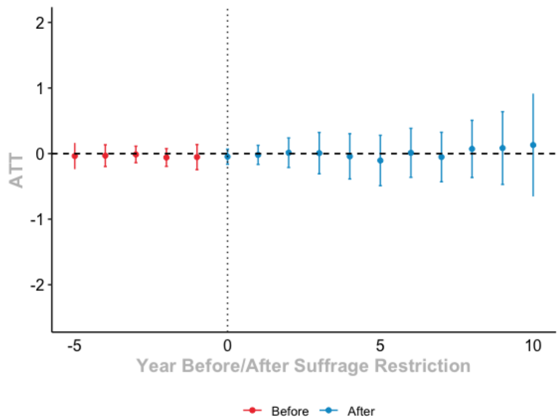
In Appendix Figures A.9 to A.15 we examine various spending categories expressed as a share of total state expenditures. The substantive findings align with those obtained using absolute values, except for debt and interest payments which, in this case, exhibit no significant changes following suffrage restrictions.

Threats to Inference

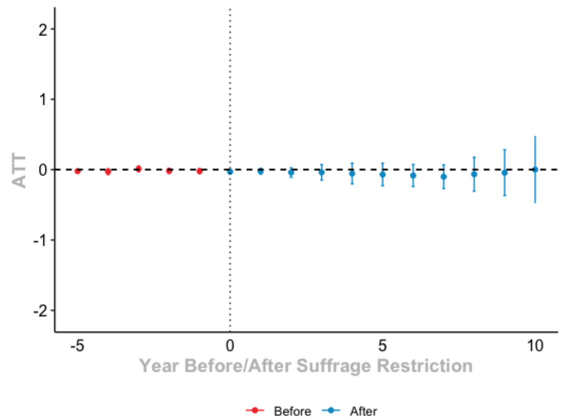
Before turning to the question of targeted redistribution, we first consider potential threats to inference. A key identifying assumption in our design is that states experiencing treatment at different times do not exhibit differential trends. Given the lengthy and often uncertain process required to pass these disenfranchising measures, it is plausible to assume that the timing of these restrictions is not systematically correlated with state characteristics, such as income levels or population size. To validate this assumption, we examine whether any observable state characteristic consistently predicts the adoption or timing of voting restrictions. We begin with a regression analysis using a binary dependent variable indicating whether a state adopted voting restrictions, with pre-treatment observ-

Figure 6: **Event-Study Estimates: Effects of Suffrage Restrictions on Total State Spending and Categories Based on Length of Exposure to Treatment**

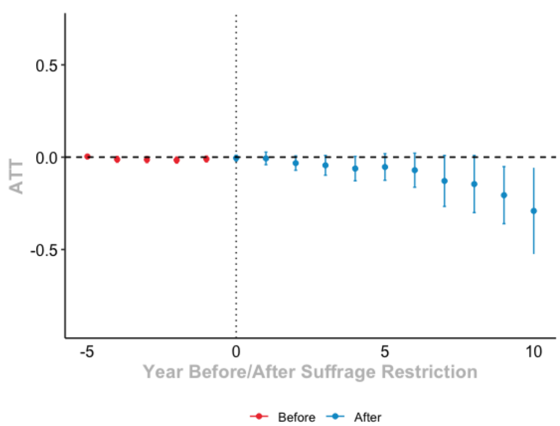
(a) Total State Spending



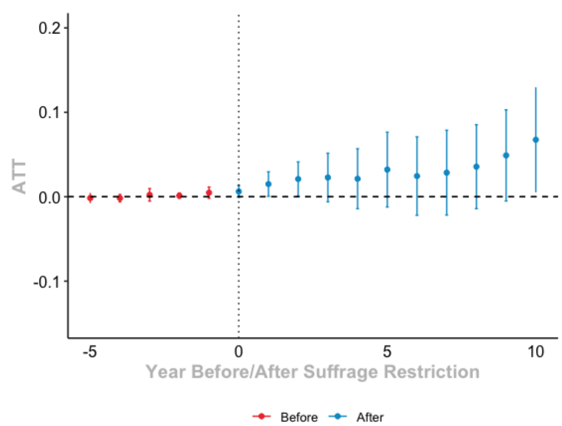
(b) Spending on Common Schools



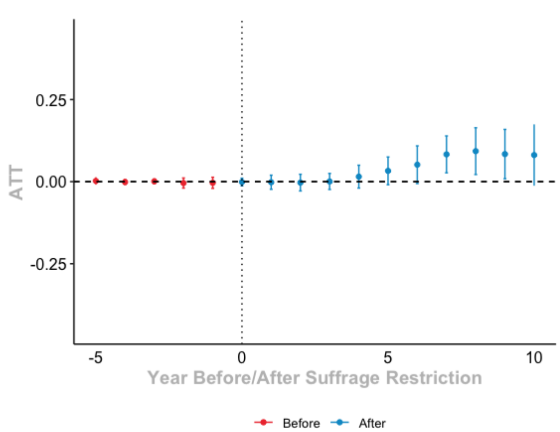
(c) Spending on Veteran Pensions



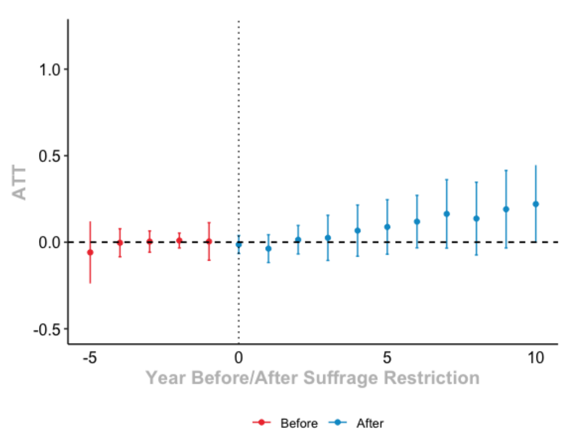
(d) Spending on White Colleges



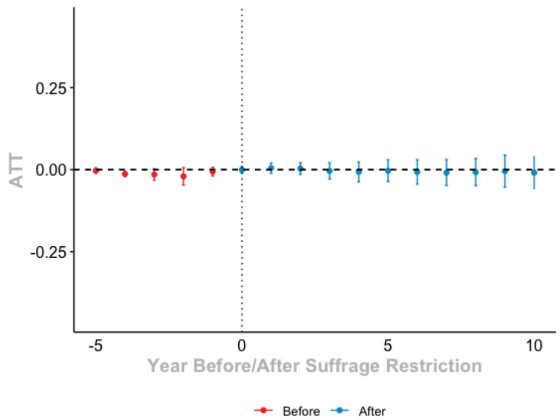
(e) Spending on Prisons



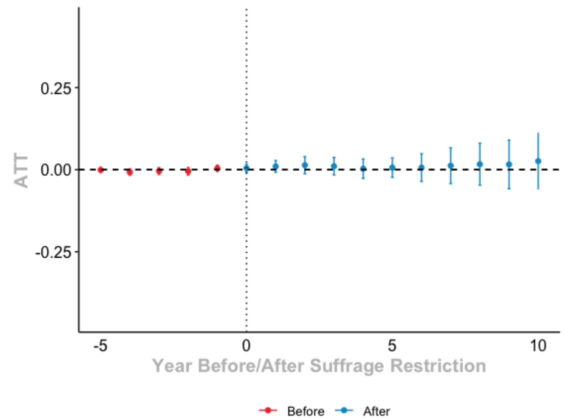
(f) Spending on Debt & Interest



(g) Spending on Courts



(h) Spending on Asylums



Notes: Event time estimates and 95% confidence intervals for spending on (a) total state spending, (b) common schools, (c) pensions, (d) colleges, (e) prisons, (f) debt interest payments, (g) courts, and (h) insane asylums (million \$). Dynamic ATTs are calculated based on 5 pre- and 10 post-treatment years. The sample includes 14 Southern states, of which three are never-treated. The sample for Figure (c) encompasses 11 eventually-treated states. Standard errors clustered at the state level. All estimates calculated using Callaway and Sant'Anna (2021) doubly robust estimation method.

ables as predictors. The results, detailed in Table A.3, reveal that no state characteristic—including population size, total output, white population share, urban population, or bureaucratic capacity—consistently predicts either the presence of restrictions or the cohort classification of each state. We then use two-way fixed effects models to evaluate whether time-varying factors affect the adoption year of suffrage restrictions. The results, presented in Table A.4, indicate that none of the variables, including the prevalence of violence (measured by the number of lynchings recorded in each state), consistently correlate with the timing of restrictions. This holds true both in analyses of individual cohorts (columns 1-7) and across all cohorts (column 8).

Next, we assess whether cohorts exhibit significant differences in pre-treatment outcome levels. Notably, testing for the absence of such disparities in outcome *levels* is a stringent test, since the standard difference-in-differences assumption only requires that there are no counterfactual differences in the outcome *trends*. Table A.5 displays the average level of each spending variable by treatment timing (columns 1-7), and the results of unconditional F-tests for the difference in means across the seven cohorts (column 8). The results suggest that treatment timing is not associated with pre-treatment outcome levels.

Another potential concern involves anticipatory behavior, which could arise if officeholders had prior knowledge of the timing of voting restrictions or acted as if Black voters had already been disenfranchised before the measures were officially implemented, as argued by Key (1984), but controverted by others (Kousser 1974; Olson 2024). Anticipatory behavior, if it existed, should downward bias our estimates, but not substantially challenge our findings.

Lastly, there may be concerns that the constitutional reforms accompanying the suffrage restrictions influenced state finances through channels other than changes to voting laws. In our sample, most states implemented these voting restrictions as part of broader constitutional revisions, which often included new provisions on taxation and resource allocation. To address concerns that our findings might be confounded by these additional fiscal measures, we have meticulously analyzed the relevant constitutional provisions for each state. These details are provided in Appendix C, along with additional robustness checks. Our further analysis confirms that our findings remain consistent, even when accounting for other constitutional changes implemented alongside the suffrage restrictions.

When Elites Need Lower-Income Ingroup Members

Thus far, we have demonstrated how disenfranchisement and the resulting shifts in the composition of the white coalition necessary to maintain Democratic Party rule influenced state spending patterns, increasingly skewing state resources away from goods primarily benefiting lower-income whites and toward those favored by elites. We now turn to question of what happened to the level of redistribution for low-income whites in high Black-share counties, the group whose support was both necessary and sufficient for uncontested Democratic dominance. To investigate this, we analyze the county-level distribution of state spending on common schools and pensions from 1880 to 1910, using data manually compiled from each state's annual or biennial reports of the auditor, treasurer, or superintendent. This allows us to measure targeted redistribution for these politically crucial whites.

We employ TWFE models, interacting a voting restrictions indicator with the pre-treatment white population share across counties, measured in 1880. This modeling approach is supported by two key considerations: first, the Callaway and Sant'Anna (2021) estimator does not allow for continuous interaction terms, and second, as evidenced by a recent reanalysis of 37 published studies, both standard TWFE and advanced DiD estimators generally lead to similar substantive conclusions (Chiu et al. 2024). Results are detailed in Table 3. Columns (1) and (2) present the models with the suffrage restrictions indicator alone and with additional pre-treatment controls—namely, total population, urban population share, local economic output, and the assessed value of property per white capita in 1880—each interacted with year indicators. To address serial correlation and heteroskedasticity, we cluster standard errors at the county level. These first two models show a negative impact of suffrage restrictions on per capita school funding allocations. To examine whether this decline occurred uniformly across counties, we introduce an interaction term between suffrage restrictions and the county white population share in 1880. As reported in columns (3) and (4), Black disenfranchisement led to a rise in state common school spending. However, this positive effect diminishes with an increasing county white population share, as reflected by the negative interaction coefficient.[12]

The marginal effects, depicted in Figure 8, further illustrate that once the white population share exceeds 43%, the effect of suffrage restrictions on school funding becomes negative. This pattern suggests that while counties with larger Black populations saw increased educational allocations from the state, predominantly white counties faced reductions in school funding after disenfranchisement. Specifically, in counties with a near-zero white population share in 1880, the implementation of suffrage restrictions is associated with an increase in per capita school funding by about seven percent of a standard deviation. In contrast, counties that were entirely white in 1880 observe a reduction in funding by nearly one tenth of a standard deviation.

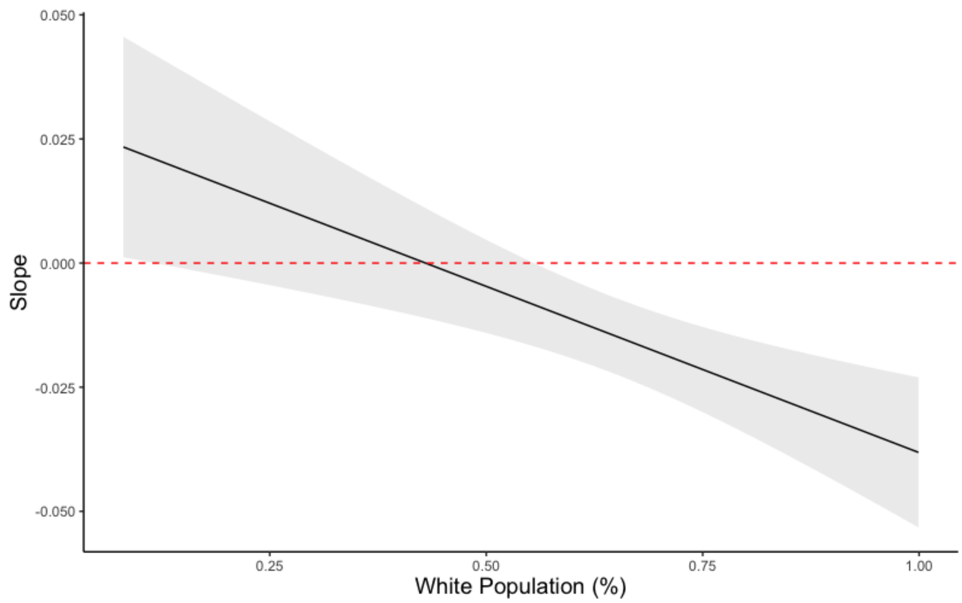
[12] In light of recent critiques underscoring interpretability issues with two-way fixed effects models (Kropko and Kubinec 2020), we also employ a simpler specification that includes only county fixed effects. The results, presented in Appendix Table A.6, are consistent with those from the TWFE analysis in Table 3.

Table 3: **Heterogeneous Effects of Disenfranchisement on State Apportionment for Education Per Capita across Counties**

	State Apportionment for Education per Capita			
	(1)	(2)	(3)	(4)
Suffrage Restriction	-0.022^{***}	-0.014^{***}	0.027^{**}	0.029^{**}
	(0.004)	(0.004)	(0.013)	(0.013)
Suffrage Restriction × White Population 1880 (%)			-0.074^{***}	-0.067^{***}
			(0.019)	(0.019)
Observations	25,638	22,286	24,530	22,286
R^2	0.857	0.959	0.937	0.959
County FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Time-invariant covariates		✓		✓

Notes: The dependent variable measures per capita state educational funding across counties. All models account for year and county fixed effects, along with state-specific trends. To ensure comparability, we restrict our sample to counties within states that implemented disenfranchisement at some point in time. The base term on white population share in 1880 is absorbed by the county fixed effects. The specifications in columns (2) and (4) include the following pre-treatment controls interacted with year indicators: county output, population size, the share of urban population, and the assessed value of property per white capita in 1880. Additionally, columns (3) and (4) features an interaction term between suffrage restrictions and the local White population share in 1880.

Figure 8: **Marginal Effects: The Moderating Role of White Population Share on the Relationship Between Disenfranchisement and State Apportionment for Education per Capita**



It is important to note that our estimates almost certainly understate the benefits accruing to poor whites in high-Black share areas. This underestimation stems from our use of a dependent variable that measures total state common school revenues apportioned to each county normalized by the *total* county population—i.e., average expenditures per capita. This approach not only overlooks the significant disparities in educational funding between Black and white schools during this period, but also fails to recognize that this gap in per-pupil funding grew over time. The absence of comparable and relatively complete county-level data on state spending for white versus Black schools across states throughout the entire period limits our ability to conduct a systematic analysis. Nevertheless, existing research underscores the increasing magnitude of this bias, particularly following Black disenfranchisement. Notably, using the five Southern states for which data is available, Margo (2007) showed that the disparity in average statewide spending on teacher salaries per pupil between white and Black schools increased 82% - 372% between 1890 and 1910.[13] Additionally, Margo demonstrated that this white-Black expenditure gap was not uniform within states but was considerably

[13]For instance, in Alabama, the public spending on white teachers per white pupil and Black teachers

wider in counties with higher Black population shares.[14] Hence, our per capita variable reflects state spending on white students accurately in counties with a high white population share. However, it fails to capture the full extent of spending on whites in high Black share areas, and this downward bias becomes more pronounced as the Black population share increases and as the period progresses. In other words, these figures represent the lower bound of estimated spending allocated to whites in high Black-share counties, as they assume equal spending on white and Black students—an assumption we know to be false.[15]

Appendix Table A.7 and Figure A.16 show the regression results and marginal effects with county-level pension spending per white capita as the dependent variable.[16] These results are highly consistent with our argument, with the caveat that we could only locate this data for seven states.[17] In other

---

per Black pupil was \$3.14 and \$3.10, respectively, in 1890—with a white-Black gap ratio of 1.01. By 1910, this had risen to \$10.07 for white pupils and decreased to \$2.69 for Black pupils, resulting in a gap ratio of 3.74—a 270% increase over 20 years (Margo 2007).

[14]In Alabama, the average county with a Black-population share between 0 and 25% had a white-Black spending gap ratio of 2.23 in 1910. For counties with a Black-population share over 75%, this ratio exceeded 10, about five times higher. In other states with comparable data, the ratio in counties with high Black-population shares was up to eight times greater than in those with low Black-population shares.

[15]This broad claim is further supported by Kousser's (1980) findings that whites in high Black-share areas in North Carolina experienced a disproportionate increase in public education spending post-disenfranchisement.

[16]Unlike common-school spending, Confederate-veteran pensions only went to white veterans and their widows. Thus, normalizing these expenditures by each county's white population is appropriate.

[17]While state-level totals for spending on pensions were easily located, the allocations to each county were not systematically reported within or across states. Some states reported them (and only for some years), and some states did not. This prevented us from compiling a dataset as comprehensive as the one for school spending.

words, we find clear geographic divergence in the two primary redistributive goods from this period: low-income whites in high-white share counties receive significantly less redistributive spending after disenfranchisement than their counterparts in high Black-share counties.

Conclusion

In this paper, we examine intragroup redistribution and public spending in diverse societies by focusing on the systematic disenfranchisement of the Southern Black population in eleven states between 1889 and 1906. As Bateman (2018, p. 6) shows, the conjunction of democratization and disenfranchisement is neither rare nor unique to America; rather, it is the "result of deliberate choices made by elite political coalitions looking to gain and hold on to power." We find that this episode of democratic backsliding provides a key insight: the extent of within-group redistribution is determined by whether elites require broad or narrow support from lower-income members of their group to sustain their hold on power. Our analysis demonstrates that, following Black disenfranchisement, Southern rural elites no longer required broad statewide white support to control state governments and instead only needed support from whites in high Black-share areas. This was due to the political geography of these states, where legislative representation was based on total population rather than the eligible electorate. As a result, stripping the franchise from Black voters changed the incentives of elites to engage in intragroup redistributive spending, with varying impacts on white populations across different regions of each state.

We investigate how this shift in the number and geographic distribution of electorally significant low-income whites affected within-group redistribution using unique annual data from fourteen Southern states between 1880 and 1910. This dataset includes total state spending and seven distinct expenditure items, categorized into redistributive goods benefiting lower-income whites (common schools and pensions), and non-redistributive goods (courts, debt, white universities, prisons, and insane asylums), some of which are likely elite-preferred goods. Our findings indicate that disenfranchisement did not significantly alter the trajectory of overall state expenditures or affect spending on basic state services such as courts, and insane asylums. However, we observe a significant reallocation of state funds away from redistributive goods toward elite-preferred expenditures following the removal of the electoral threat posed by Southern Blacks. This is consistent with the claim that rural elites, no longer needing broad-based white support after disenfranchisement, reduced spending on goods benefiting lower-income whites. Additionally, our data allows us to explore whether this shift affected which whites received more or less redistribution. As expected, we observe that state spending on common schools increased in counties with high Black-population shares, while declining in predominantly white counties. This targeted allocation minimized the scale of the redistributive efforts needed from elites, while ensuring they maintained the necessary support for political control.

Our findings have a number of implications for the comparative literature on redistribution in non-inclusive, diverse societies. In particular, they underscore the conditional nature of intragroup redistribution, especially within dominant groups. In contexts where maintaining control requires broad support from low-income members of the dominant group, higher levels of within-group redistribution are likely. Yet, in settings where only a narrow base of low-income members is needed, lower levels of redistribution tend to prevail. It is important to note that certain aspects of our case likely facilitated the reduction of redistribution by elites and their ability to overlook the demands of most Southern whites. For one, during this period, the American South was relatively undeveloped (e.g., less than 20% of the population lived in an urban area), and characterized by extensive economic inequality, not only between whites and Blacks (Derenoncourt et al. 2024), but also within the white population itself—conditions typically inconducive to high redistributive spending (Galor et al. 2009; Beramendi et al. 2019; Hollenbach 2021). Second, the region's political institutions—malapportioned legislatures with a first-past-the-post electoral system—may have been particularly suited to minority rule (Ansolabehere and Snyder 2008; Beramendi and Jensen 2019). A fruitful avenue for future research involves examining how political institutions might weaken or exacerbate the effects of intragroup power dynamics on redistribution.

Our research also speaks to the political economy literature on the fiscal consequences of democratic reversals. While not the primary focus of our study, our observation that overall public spending remains stable despite the mass disenfranchisement of 20% to 60% of the poorest voters in each state contributes to ongoing debates about the effects of democratization on fiscal outcomes (Acemoglu et al. 2015; Meltzer and Richard 1981; Scheve and Stasavage 2017), and the implications of enfranchisement and disenfranchisement for the formation of intra-group cross-class coalitions that either facilitate or block redistribution (Lieberman 2003; Suryanarayan and White 2021). Our findings challenge conventional median-voter theories, which would predict reductions in fiscal spending, and instead align more closely with fiscal-contract theories of the state (e.g., Bates 1983; Levi 1988; North and Weingast 1989; Timmons 2005). This is particularly the case if considered in conjunction with work showing that the incidence of Southern state taxes in this period fell primarily on the landed elite (Jensen et al. 2024). Uncontested political power did not lead to a reduction in the size of the state or the tax burden of the elites but to a shift in public expenditures to better serve elite interests. This aligns with prior research on large-scale suffrage expansions in the U.S., which similarly found that these reforms mostly affected the distribution rather than the overall level of public spending (Cascio and Washington 2014; Husted and Kenny 1997; Miller 2008).

Perhaps most importantly, our paper also provides an institutional narrative that helps explain the emergence of the one-party, Solid South. Although we primarily explore the fiscal consequences of these political shifts, particularly in terms of intragroup redistribution, future research could investigate how subsequent shifts in these institutions affected the intragroup bargaining dynamics among whites. For instance, while the inclusion of the disenfranchised Blacks in the counts for state legislative apportionment was critical to the overrepresentation of whites residing in these areas, the Great Migration, during which millions of Southern Blacks left the South, dramatically reduced the population in these areas in the years after our study. This demographic change raises questions about how rural elites maintained power. We know that many Southern state legislatures simply refused to reapportion as required by their state constitutions, or did so infrequently, until compelled to by the Supreme Court's decision in Reynolds v. Sims, 377 U.S. 533 (1964), which mandated equal population in state legislative districts. This, combined with the Voting Rights Act of 1965, which empowered the federal government to enforce the voting rights of Southern Blacks, led to the dismantling of the "Solid South" (Schuit and Rogowski 2017; Cascio and Washington 2014).

Beyond the northward migration of Blacks, the region also experienced a significant exodus of Southern whites (Bazzi et al. 2023). Additionally, the South, predominantly agrarian during our study period and lagging behind other U.S. regions in urban development, underwent rapid urbanization after Black disenfranchisement—from less than 20% urban in 1900 to nearly 60% in 1960. Examining these long-run structural shifts is essential to understand how the distribution of power between rural elites and low-income whites evolved, and to elucidate how these elites retained political control. Additionally, this analysis is crucial to comprehending changes in state spending patterns, particularly in light of industrialization's effects on intra-elite dynamics and their fiscal preferences (Mares and Queralt 2015, 2020; Beramendi et al. 2019; Hollenbach 2021; Emmenegger et al. 2021). Finally, analyzing these structural changes may help reconcile our findings, which emphasize elite control, with Caughley's (2019) observations, which suggest an increase in power among low-income whites following the onset of the Great Depression. In short, our paper helps explain momentous political economic developments in the American South and provides new avenues for research on a variety of ongoing debates in both American and comparative political economy.

References

Acemoglu, Daron, Suresh Naidu, Pascual Restrepo, and James Robinson. 2015. "Democracy, redistribution, and inequality." In *Handbook of income distribution*. Vol. 2 Elsevier pp. 1885–1966.

Aldrich, John H., and John D. Griffin. 2018. *Why Parties Matter: Political Competition and Democracy in the American South*. Chicago, IL: University of Chicago Press.

Alesina, Alberto, and Edward Ludwig Glaeser. 2004. *Fighting poverty in the US and Europe: A world of difference*. Oxford University Press, USA.

Alesina, Alberto, Reza Baqir, and William Easterly. 1999. "Public goods and ethnic divisions." *The Quarterly journal of economics* 114(4): 1243–1284.

Alston, Lee, and Joseph Ferrie. 2007. *Southern Paternalism and the American Welfare State: Economics, Politics, and Institutions in the South, 1865-1965.* Cambridge University Press.

Ansolabehere, Stephen, and James Snyder. 2008. *The End of Inequality: One Person, One Vote and the Transformation of American Politics*. WW Norton.

Bateman, David A. 2018. *Disenfranchising democracy: Constructing the electorate in the United States, the United Kingdom, and France*. Cambridge University Press.

Bates, Robert. 1983. *Essays on the Political Economy of Africa*. University of California Press.

Bazzi, Samuel, Andreas Ferrara, Martin Fiszbein, Thomas Pearson, and Patrick A Testa. 2023. "The other great migration: Southern whites and the new right." *The Quarterly Journal of Economics* 138(3): 1577–1647.

Beramendi, Pablo, and Jeffrey Jensen. 2019. "Economic Geography, Political Inequality, and Public Goods in the Original 13 U.S. States." *Comparative Political Studies* 52(13-14): 2235–2282.

Beramendi, Pablo, Mark Dincecco, and Melissa Rogers. 2019. "Intra-Elite Competition and Long-Run Fiscal Development." *The Journal of Politics* 81(1): 49–65.

Callaway, Brantly, and Pedro H.C. Sant'Anna. 2021. "Difference-in-differences with multiple time periods." *Journal of Econometrics* 225(2): 200–230.

Cascio, Elizabeth U, and Ebonya Washington. 2014. "Valuing the vote: The redistribution of voting rights and state funds following the voting rights act of 1965." *The Quarterly Journal of Economics* 129(1): 379–433.

Caughey, Devin. 2019. *The unsolid south: Mass politics and national representation in a one-party enclave*. Princeton University Press.

Chiu, Albert, Xingchen Lan, Ziyi Liu, and Yiqing Xu. 2024. "What To Do (and Not to Do) with Causal Panel Analysis under Parallel Trends: Lessons from A Large Reanalysis Study." *Working Paper.* Available at `https://yiqingxu.org/papers/english/2023_panel/CLLX.pdf`.

Dahlberg, Matz, Karin Edmark, and Heléne Lundqvist. 2012. "Ethnic diversity and preferences for redistribution." *Journal of Political Economy* 120(1): 41–76.

de Chaisemartin, Clément, and Xavier D'Haultfoeuille. 2022. "Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: a survey." *The Econometrics Journal* p. utac017.

Derenoncourt, Ellora, Chi Hyun Kim, Moritz Kuhn, and Moritz Schularick. 2024. "Wealth of two nations: The US racial wealth gap, 1860–2020." *The Quarterly Journal of Economics* 139(2): 693–750.

Eli, Shari, and Laura Salisbury. 2016. "Patronage politics and the development of the welfare state: Confederate pensions in the american south." *The Journal of Economic History* 76(4): 1078–1112.

Emmenegger, Patrick, Lucas Leemann, and André Walter. 2021. "No direct taxation without new elite representation: Industrialization and the domestic politics of taxation." *European Journal of Political Research* 60(3): 648–669.

Epperly, Brad, Christopher Witko, Ryan Strickler, and Paul White. 2020. "Rule by Violence, Rule by Law: Lynching, Jim Crow, and the Continuing Evolution of Voter Suppression in the U.S." *Perspectives on Politics* 18(3): 756–69.

Foner, Eric. 2014. *Reconstruction: America's Unfinished Revolution, 1863-1877.* Harper Collins.

Galor, Oded, Omer Moav, and Dietrich Vollrath. 2009. "Inequality in landownership, the emergence of human-capital promoting institutions, and the great divergence." *The Review of Economic Studies* 76(1): 143–179.

Goldin, Claudia, and Lawrence F Katz. 2010. *The race between education and technology*. Harvard University Press.

Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics* 225(2): 254–277.

Grantham, Dewey. 1992. *The life and death of the Solid South*. University Press of Kentucky.

Greenberger, Michael. 2022. "Undoing Reconstruction: Racial Threat and the Process of Redemption, 1870-1920." *Social Science Quarterly* 103(3): 649–669.

Habyarimana, James, Macartan Humphreys, Daniel N Posner, and Jeremy M Weinstein. 2007. "Why does ethnic diversity undermine public goods provision?" *American political science review* 101(4): 709–725.

Hahn, Steven. 2006. *The Roots of Southern Populism: Yeoman Farmers and the Transformation of the Georgia Upcountry, 1850-1890*. Oxford University Press.

Heersink, Boris, and Jeffery A. Jenkins. 2020. *Republican Party Politics and the American South, 1865-1968*. Cambridge University Press.

Hollenbach, Florian. 2021. "Elite interests and public spending: Evidence from Prussian cities." *The Review of International Organizations* 16(1): 189–211.

Husted, Thomas A, and Lawrence W Kenny. 1997. "The Effect of the Expansion of the Voting Franchise on the Size of Government." *Journal of Political Economy* 105(1): 54–82.

Hyman, Michael. 1989. "Taxation, Public Policy, and Political Dissent: Yeoman Disaffection in the Post-Reconstruction Lower South." *The Journal of Southern History* 55(1): 49–76.

Jensen, Jeffrey, Giuliana Pardelli, and Jeffrey Timmons. 2024. *Representation and Taxation in the American South, 1820-1910*. Cambridge University Press.

Jones, Daniel B., Werner Troesken, and Randall Walsh. 2012. *A Poll Tax by Any Other Name: The Political Economy of Disenfranchisement*. NBER Working Paper No. 18612, Available at: https://www.nber.org/system/files/working_papers/w18612/w18612.pdf.

Jones, Daniel B., Werner Troesken, and Randall Walsh. 2017. "Political Participation in a Violent Society: The Impact of Lynching on Turnout in the Post-Reconstruction South." *Journal of Development Economics* 129: 29–46.

Keele, Luke, William Cubbison, and Ismail White. 2021. "Suppressing Black Votes: A Historical Case Study of Voting Restrictions in Louisiana." *American Political Science Review* 115(2): 694–700.

Key, V.O. 1984. *Southern Politics in State and Nation*. Univ. of Tennessee Press.

Kousser, J Morgan. 1974. *The shaping of southern politics: Suffrage restriction and the establishment of the one-party south, 1880-1910*. Vol. 102 Yale University Press.

Kousser, J Morgan. 1980. "Progressivism-for middle-class whites only: North carolina education, 1880-1910." *The Journal of Southern History* 46(2): 169–194.

Kousser, J. Morgan. 1999. *Colorblind Injustice: Minority Voting Rights and the Undoing of the Second Reconstruction*. Chapel Hill, NC: University of North Carolina Press.

Kropko, Jonathan, and Robert Kubinec. 2020. "Interpretation and identification of within-unit and cross-sectional variation in panel data models." *PloS one* 15(4): e0231349.

Levi, Margaret. 1988. *Of rule and revenue*. University of California Press.

Lieberman, Evan S. 2003. *Race and regionalism in the politics of taxation in Brazil and South Africa*. Cambridge University Press.

Lindert, Peter. 2004. *Growing public: Volume 1, the story: Social spending and economic growth since the eighteenth century*. Vol. 1 Cambridge University Press.

Mares, Isabela, and Didac Queralt. 2015. "The Non-Democratic Origins of Income Taxation." *Comparative Political Studies* 48(14): 1974–2009.

Mares, Isabela, and Didac Queralt. 2020. "Fiscal innovation in nondemocratic regimes: Elites and the adoption of the Prussian income taxes of the 1890s." *Explorations in Economic History* 77(1).

Margo, Robert. 2007. *Race and Schooling in the South, 1880-1950*. University of Chicago Press.

McCurry, Stephanie. 2012. *Confederate reckoning: Power and politics in the civil war south*. Harvard University Press.

Meltzer, Allan, and Scott Richard. 1981. "A rational theory of the size of government." *Journal of Political Economy* 89(5): 914–927.

Miguel, Edward, and Mary Kay Gugerty. 2005. "Ethnic diversity, social sanctions, and public goods in Kenya." *Journal of public Economics* 89(11-12): 2325–2368.

Miller, Grant. 2008. "Women's suffrage, political responsiveness, and child survival in American history." *The Quarterly Journal of Economics* 123(3): 1287–1327.

Muller, Christopher. 2018. "Freedom and convict leasing in the postbellum south." *American Journal of Sociology* 124(2): 367–405.

North, Douglass, and Barry Weingast. 1989. "Constitutions and commitment: the evolution of institutions governing public choice in seventeenth-century England." *The Journal of Economic History* 49(04): 803–832.

Olson, Michael P. 2024. ""Restoration" and representation: Legislative consequences of Black disfranchisement in the American South, 1879–1916." *American Journal of Political Science* .

Paglayan, Augustina. 2022. "Education or Indoctrination? The Violent Origins of Public School Systems in an Era of State-Building." *American Political Science Review* 116(4): 1242–57.

Perman, Michael. 2003. *Struggle for mastery: Disfranchisement in the South, 1888-1908.* UNC Press.

Queralt, Didac. 2022. *Pawned States: State building in the era of international finance*. Princeton University Press.

Rubio-Ramos, Melissa. 2022. From Plantations to Prisons: The Race Gap in Incarceration After the Abolition of Slavery in the U.S. ECONtribute Discussion Papers Series 195.

Scheve, Kenneth, and David Stasavage. 2017. "Wealth inequality and democracy." *Annual Review of Political Science* 20: 451–468.

Schuit, Sophie, and Jon C Rogowski. 2017. "Race, representation, and the voting rights act." *American Journal of Political Science* 61(3): 513–526.

Schwarz, Susanne. 2023. "'The Spawn of Slavery'? Race, State Capacity, and the Development of Carceral Institutions in the South." *Studies in American Political Development* 37(2): 181–198.

Stichnoth, Holger, and Karine Van der Straeten. 2013. "Ethnic Diversity, Public Spending, and Individual Support for the Welfare State: A Review of the Empirical Literature." *Journal of Economic Surveys* 27(2): 364–389.

Sun, Liyang, and Sarah Abraham. 2021. "Estimating dynamic treatment effects in event studies with heterogeneous treatment effect." *Journal of Econometrics* 225(2): 175–199.

Suryanarayan, Pavithra, and Steven White. 2021. "Slavery, Reconstruction, and Bureaucratic Capacity in the American South." *American Political Science Review* 115(2): 568–84.

Tabellini, Marco. 2020. "Gifts of the immigrants, woes of the natives: Lessons from the age of mass migration." *The Review of Economic Studies* 87(1): 454–486.

Timmons, Jeffrey F. 2005. "The fiscal contract: States, taxes, and public services." *World Politics* 57(4): 530–567.

Valelly, Richard. 2009. *The Two Reconstructions: the Struggle for Black Enfranchisement*. University of Chicago Press.

Vollrath, Dietrich. 2013. "Inequality and school funding in the rural United States, 1890." *Explorations in Economic History* 50(2): 267–284.

Walton, Hanes, Sherman Puckett, and Donald Deskins. 2012. *The African American electorate: A statistical history*. CQ Press.

Weaver, Michael. 2019. "'Judge Lynch' in the Court of Public Opinion: Publicity and the Delegitimation of Lynching." *American Political Science Review* 113(2): 293–310.

Woodward, C. Vann. 1981. *Origins of the New South, 1877–1913: A history of the South*. LSU Press.

Wright, Gavin. 1978. *The political economy of the cotton South: households, markets, and wealth in the nineteenth century*. WW Norton.

Online Appendices

Intragroup Redistribution in Diverse Societies: Evidence from Black Disenfranchisement

Appendix A: Supplementary Data and Analysis

Table A.1: **Southern States: Year and Type of Black Disenfranchisement**

	Black Population Share 1880 (%) (1)	Type of Suffrage Restriction (2)	Year Enacted (3)
Alabama	47.5	LT, PT	1900
Arkansas	26.3	PT	1891
Florida	47.0	PT	1890
Georgia	47.0	LT, PT	1906*
Kentucky	16.5	None	
Louisiana	51.5	LT, PT	1896
Maryland	22.5	None	
Mississippi	57.5	LT, PT	1890
Missouri	6.7	None	
N. Carolina	38.0	LT, PT	1900
S. Carolina	60.7	LT, PT	1893
Tennessee	26.1	PT	1890
Texas	24.7	PT	1900
Virginia	41.8	LT, PT	1902

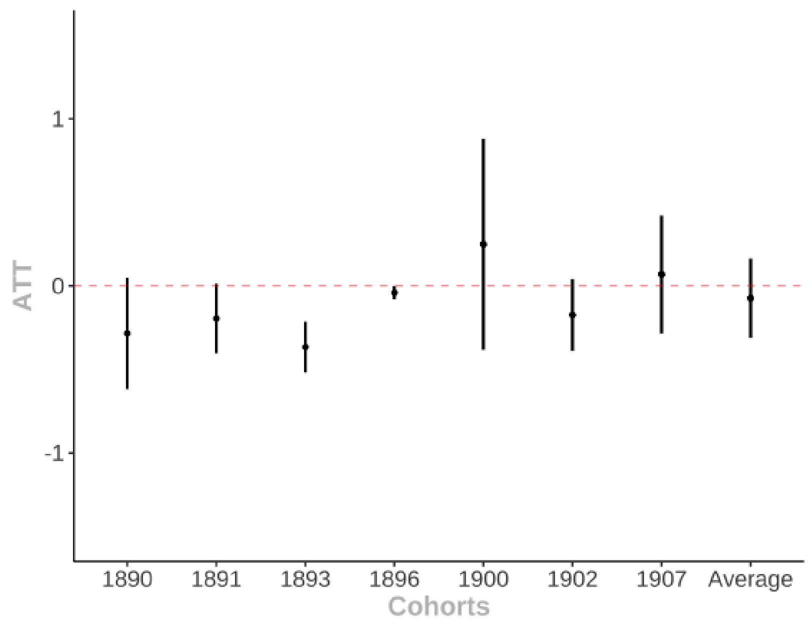
Notes: LT = Literacy Test. PT = Poll Tax. *Georgia enacted the literacy test in 1906 and poll tax in 1877. Sources: Kousser (1974); Valelly (2009), Walton et al. (2012, Table 13.9); US Census (1880).

Table A.2: **Gubernatorial Votes for Non-Democratic Candidates in High Black-Share Counties**

State	Average Number of Non-Democratic Votes		Average Share of Non-Democratic Votes		% of Dem Votes from LBS Needed to Match
	Before Disenfranchisement (1)	After Disenfranchisement (2)	Before Disenfranchisement (3)	After Disenfranchisement (4)	(5)
Alabama	28,695	5,340	50%	16%	53%
Arkansas	38,861	20,673	61%	42%	40%
Florida	20,112	4,315	80%	54%	149%
Georgia	13,874	0	39%	0%	-
Louisiana	35,566	8,746	71%	26%	61%
Mississippi	11,421	2,079	67%	36%	46%
North Carolina	81,582	37,742	59%	36%	64%
South Carolina	3,115	1,473	53%	43%	8%
Tennessee	79,782	58,164	63%	52%	59%
Texas	112,025	54,741	81%	69%	88%
Virginia	44,641	8,351	48%	22%	103%

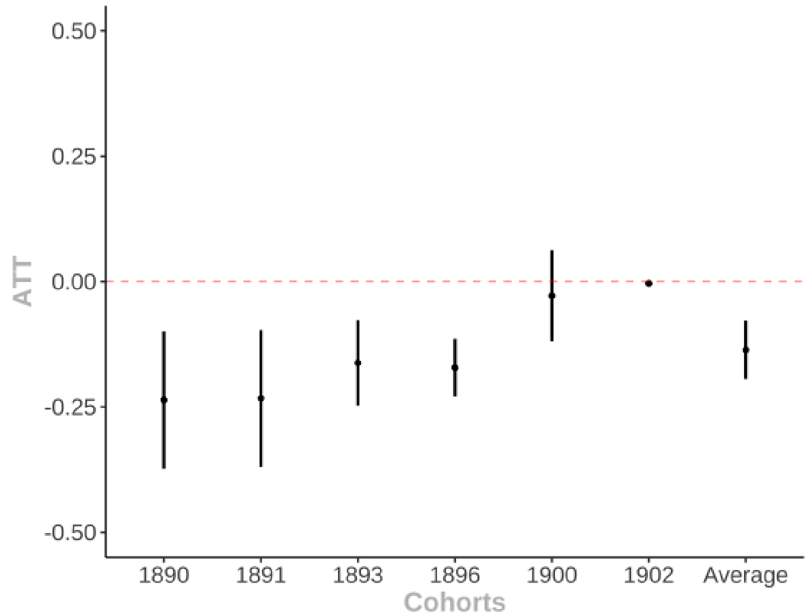
Notes: Columns (1) and (2) display the total number of votes for non-Democratic Party candidates for Governor from counties with high Black population shares within each state, before (1) and after (2) Black disenfranchisement, respectively. Columns (3) and (4) present the average share of total statewide non-Democratic votes originating from these high Black share counties in each state. "High Black share counties" are defined as those where the Black population percentage in 1880 exceeded that of the median county in each state. The averages for the period *before disenfranchisement* refer to gubernatorial elections held between 1880 and the year before franchise restrictions were implemented in each state. The averages for the period *after disenfranchisement* cover gubernatorial elections from the year restrictions were put in place through to 1910. Finally, column (5) shows the percentage of Democratic votes in low Black share areas (LBS) that non-Democratic candidates would need to convert to make up for disenfranchisement losses.

Figure A.1: **Overall Effect of Suffrage Restrictions on Common School Expenditures: Average and Cohort-Specific ATT Estimates**



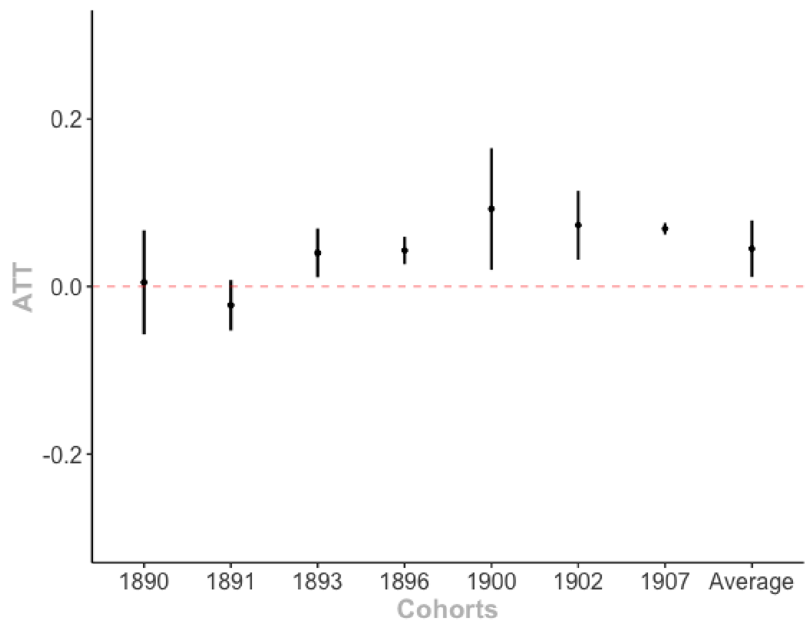
Notes: This figure shows the cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907), as well as the average of all cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 14 Southern states, three of which are never-treated. Standard errors clustered at the state level.

Figure A.2: **Overall Effect of Suffrage Restrictions on Pension Expenditures: Average and Cohort-Specific ATT Estimates**



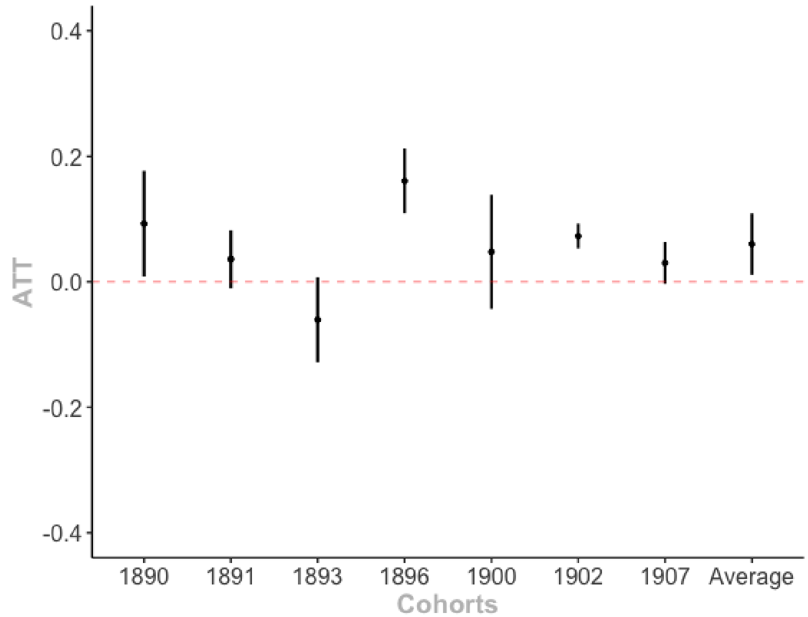
Notes: This figure shows the cohort-specific ATT estimates for each of the relevant adoption years (1890, 1891, 1893, 1896, 1900, and 1902), as well as the average of all cohort-specific effects. The sample includes 11 eventually-treated Southern states. The estimates for the last cohort (1907) are not available because there are no never-treated states. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. Standard errors clustered at the state level.

Figure A.3: **Overall Effect of Suffrage Restrictions on College Expenditures: Average and Cohort-Specific ATT Estimates**



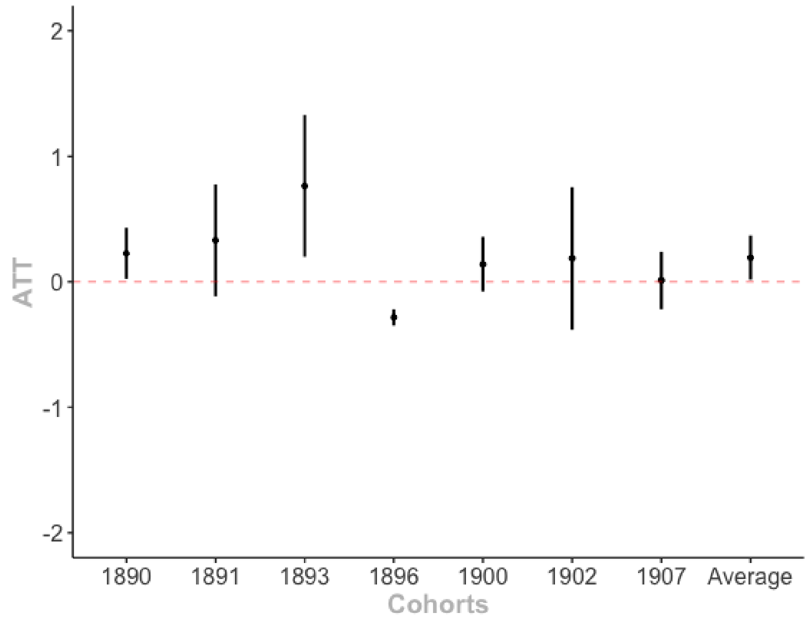
Notes: This figure shows the cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907), as well as the average of all cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 14 Southern states, three of which are never-treated. Standard errors clustered at the state level.

Figure A.4: **Overall Effect of Suffrage Restrictions on Prison Expenditures: Average and Cohort-Specific ATT Estimates**



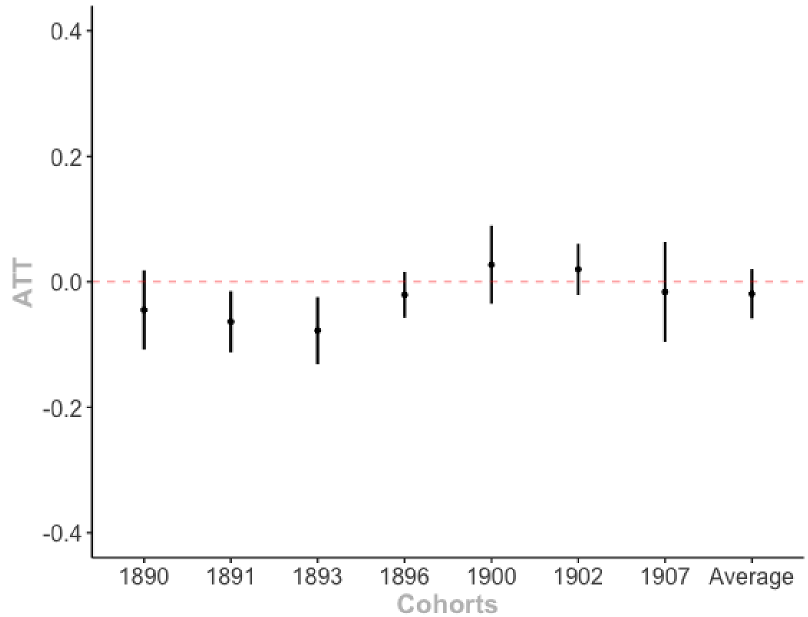
Notes: This figure shows the cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907), as well as the average of all cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 14 Southern states, three of which are never-treated. Standard errors clustered at the state level.

Figure A.5: **Overall Effect of Suffrage Restrictions on Debt and Service Payments: Average and Cohort-Specific ATT Estimates**



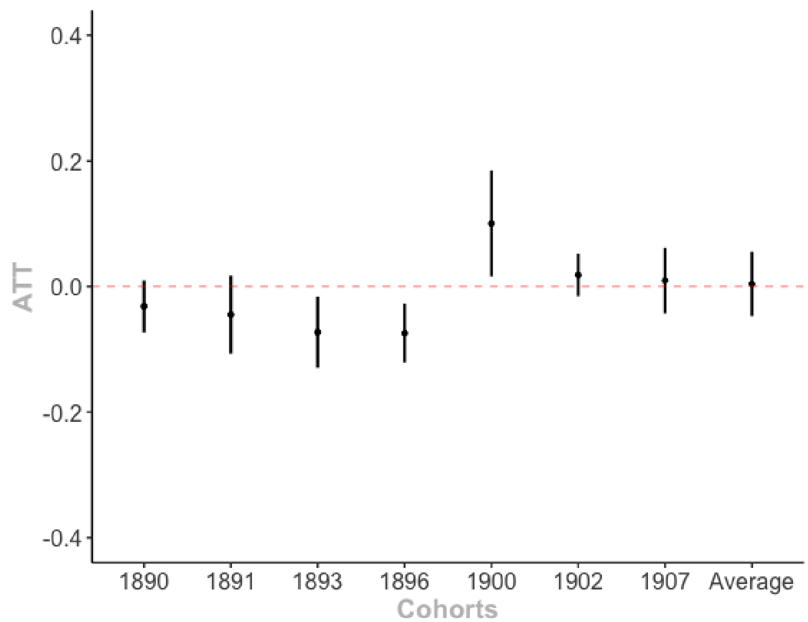
Notes: This figure shows the cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907), as well as the average of all cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 14 Southern states, three of which are never-treated. Standard errors clustered at the state level.

Figure A.6: **Overall Effect of Suffrage Restrictions on Judicial Expenditures: Average and Cohort-Specific ATT Estimates**



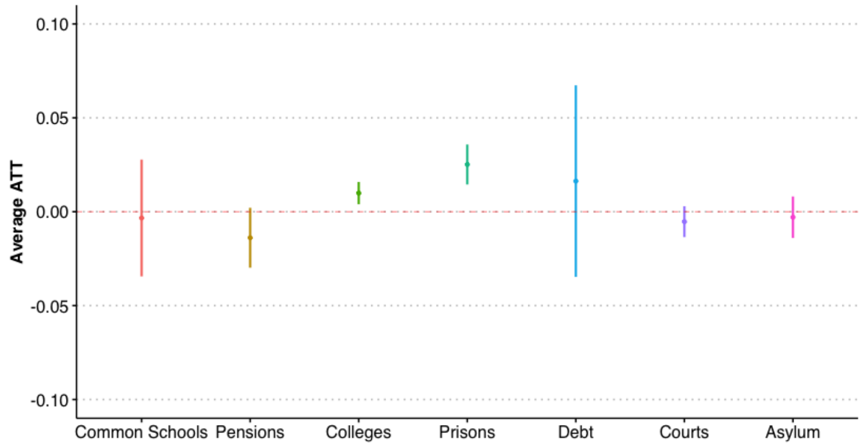
Notes: This figure shows the cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907), as well as the average of all cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 14 Southern states, three of which are never-treated. Standard errors clustered at the state level.

Figure A.7: **Overall Effect of Suffrage Restrictions on Asylum Expenditures: Average and Cohort-Specific ATT Estimates**



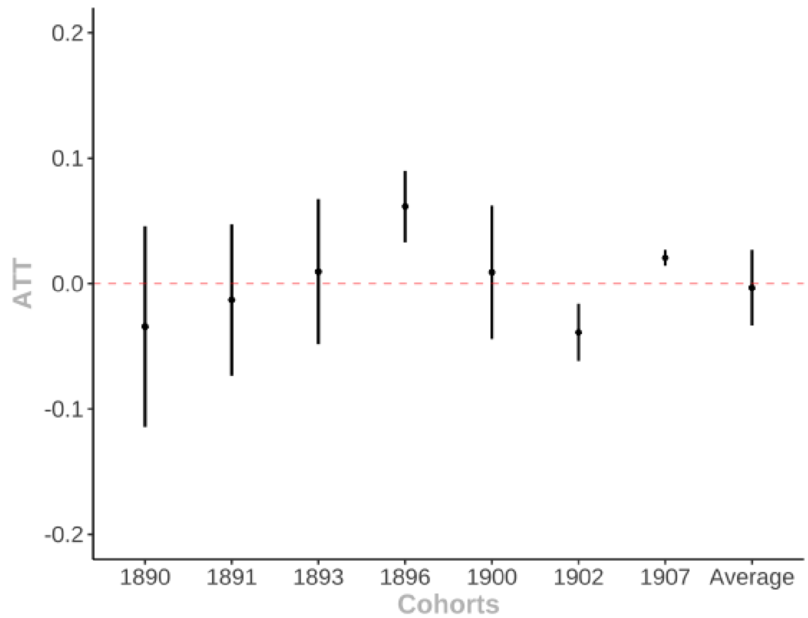
Notes: This figure shows the cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907), as well as the average of all cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 14 Southern states, three of which are never-treated. Standard errors clustered at the state level.

Figure A.8: **Average ATT Estimates: Share of Spending Allocated to Various Categories**



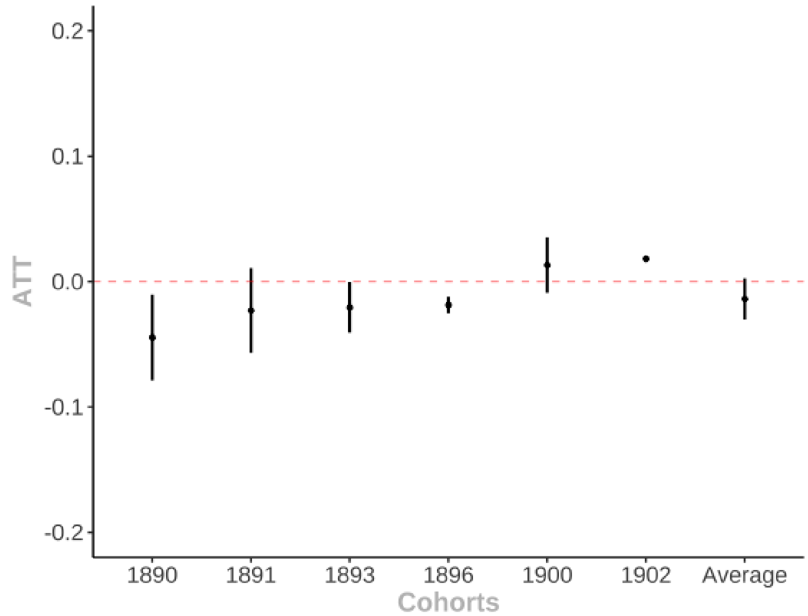
Notes: Estimates and 95% confidence intervals of the average treatment effect on the treated (ATT) for suffrage restrictions, aggregating all cohort-specific effects. Outcomes include the share of spending allocated to common schools, pensions, colleges, prisons, debt service, court systems, and asylums. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method.

Figure A.9: **Overall Effect of Suffrage Restrictions on the Share of Spending Allocated to Common Schools**



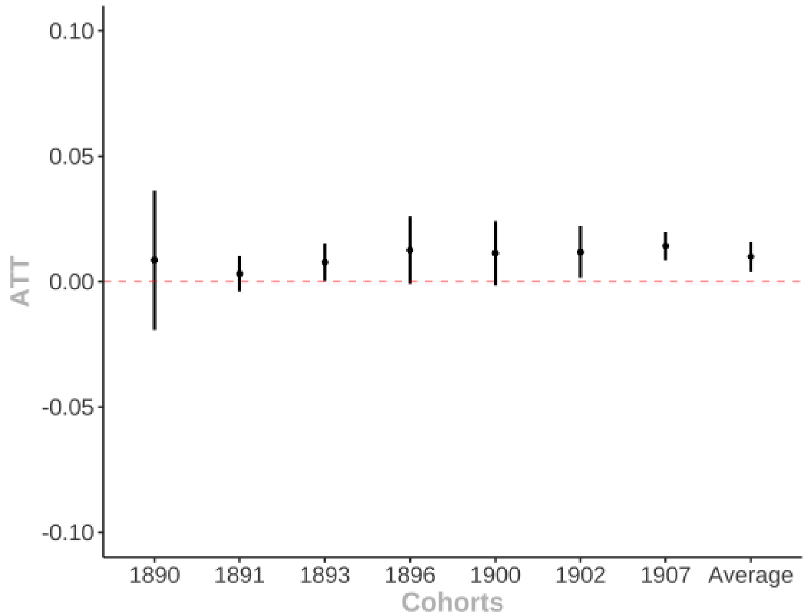
Notes: Cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907) and average of all cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 14 Southern states, three of which are never-treated. Standard errors clustered at the state level.

Figure A.10: **Overall Effect of Suffrage Restrictions on the Share of Spending Allocated to Pensions**



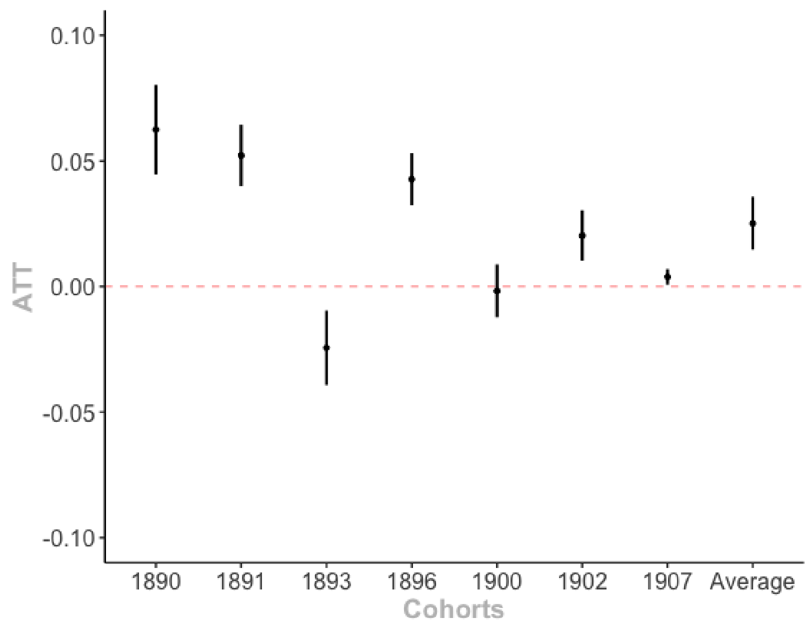
Notes: Cohort-specific ATT estimates for each of the relevant adoption years (1890, 1891, 1893, 1896, 1900, and 1902) and average of all cohort-specific effects. The sample includes 11 eventually-treated Southern states. The estimates for the last cohort (1907) are not available because there are no never-treated states. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. Standard errors clustered at the state level.

Figure A.11: **Overall Effect of Suffrage Restrictions on the Share of Spending Allocated to Colleges**



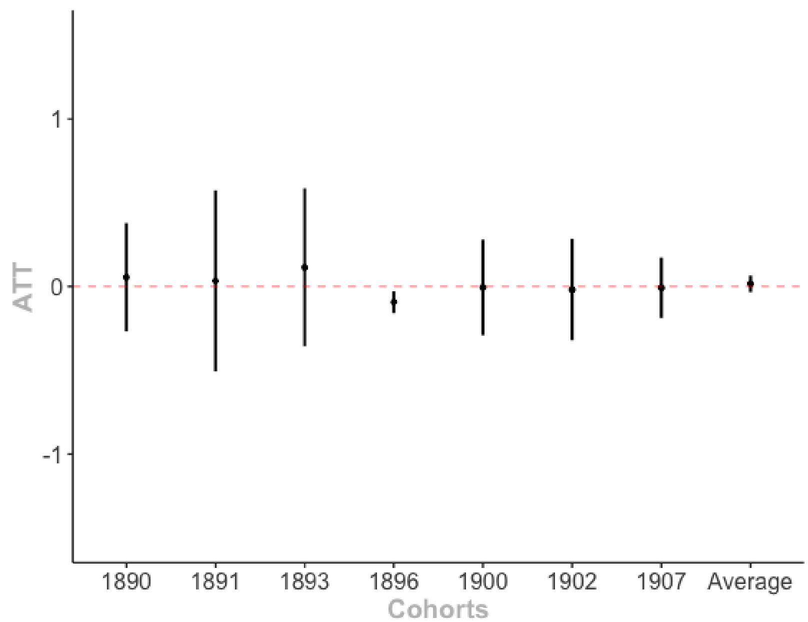
Notes: Cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907) and average of all cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 14 Southern states, three of which are never-treated. Standard errors clustered at the state level.

Figure A.12: **Overall Effect of Suffrage Restrictions on the Share of Spending Allocated to Prisons**



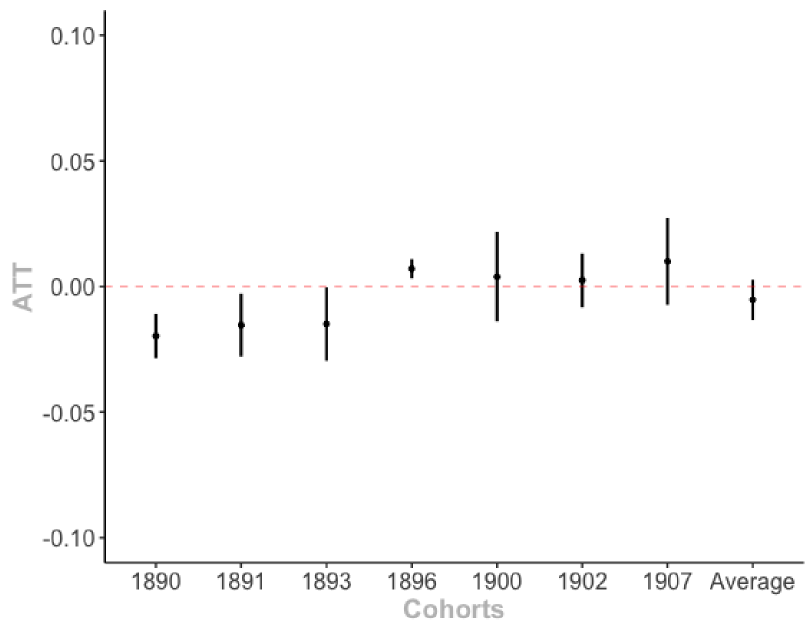
Notes: Cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907) and average of all cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 14 Southern states, three of which are never-treated. Standard errors clustered at the state level.

Figure A.13: **Overall Effect of Suffrage Restrictions on the Share of Spending Allocated to Debt Interest Payments**



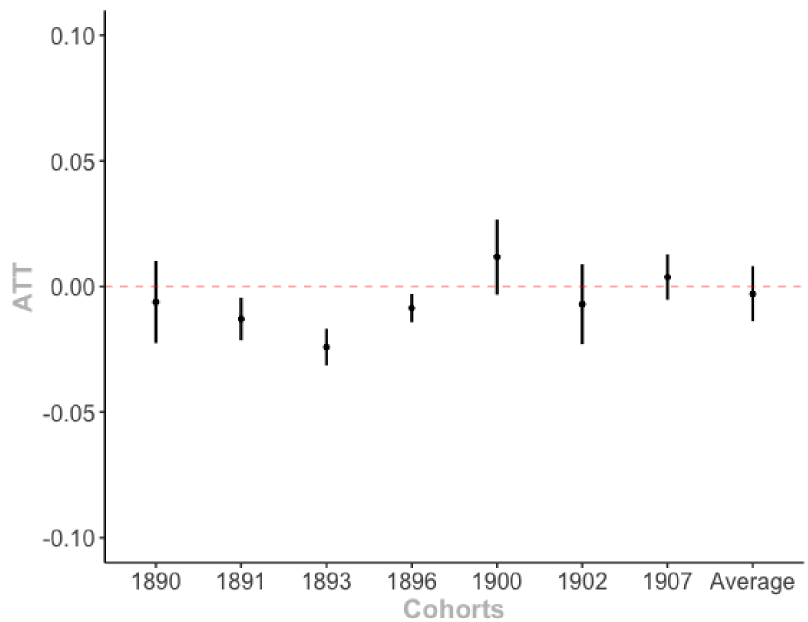
Notes: Cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907) and average of cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 14 Southern states, three of which are never-treated. Standard errors clustered at the state level.

Figure A.14: **Overall Effect of Suffrage Restrictions on the Share of Spending Allocated to Courts**



Notes: Cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907) and average of cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 14 Southern states, three of which are never-treated. Standard errors clustered at the state level.

Figure A.15: **Overall Effect of Suffrage Restrictions on the Share of Spending Allocated to Insane Asylums**



Notes: Cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907) and average of cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 14 Southern states, three of which are never-treated. Standard errors clustered at the state level.

Table A.3: **Potential Time-Invariant Predictors of Treatment Timing**

	1890 Cohort	1891 Cohort	1893 Cohort	1896 Cohort	1900 Cohort	1902 Cohort	1907 Cohort	All Treated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population (log)	−2.749	−0.398	1.225	1.623	5.463*	−1.749	−2.272	1.143
	(3.036)	(1.839)	(1.997)	(1.680)	(2.495)	(2.065)	(2.048)	(1.519)
Output (log)	1.456	0.384	−0.413	−1.157	−3.373**	0.732	1.293	−1.078
	(1.675)	(1.014)	(1.101)	(0.927)	(1.376)	(1.139)	(1.130)	(0.838)
White Pop. (%)	−0.816	0.897	−0.156	−0.226	2.440	−1.347	−1.645	−0.852
	(1.697)	(1.028)	(1.116)	(0.939)	(1.394)	(1.154)	(1.144)	(0.849)
Urban Pop. (%)	−6.569	−1.303	2.970	4.802	10.263	−4.329	−5.509	0.324
	(7.064)	(4.279)	(4.646)	(3.909)	(5.805)	(4.805)	(4.765)	(3.534)
Public Officials (log)	0.867	−0.306	−0.845	−0.335	−1.785	1.264	1.162	0.023
	(1.549)	(0.938)	(1.019)	(0.857)	(1.273)	(1.054)	(1.045)	(0.775)
Observations	14	14	14	14	14	14	14	14
R^2	0.338	0.384	0.273	0.486	0.553	0.223	0.236	0.834

Notes: This table displays the outcomes of eight distinct ordinary least squares (OLS) regressions. Each regression uses a different dependent variable, which represents whether suffrage restrictions were implemented in specific years: 1890, 1891, 1893, 1896, 1900, 1902, and 1907, or at any point in time (column 8). All explanatory variables are measured in 1880, the pre-treatent period.

Table A.4: **Potential Time-Varying Predictors of Treatment Timing Among Disenfranchising States**

	1890 Cohort	1891 Cohort	1893 Cohort	1896 Cohort	1900 Cohort	1902 Cohort	1907 Cohort	All Cohorts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population (log)	-0.027	0.081	0.027	-0.051	0.002	-0.042	-0.076	-0.085
	(0.110)	(0.090)	(0.041)	(0.059)	(0.061)	(0.047)	(0.093)	(0.188)
Output (log)	-0.004	-0.023	-0.028	0.054	0.044	-0.008	0.054	0.090
	(0.060)	(0.039)	(0.034)	(0.051)	(0.059)	(0.015)	(0.069)	(0.116)
White Pop. (%)	0.596	0.241	-0.012	-0.093	0.093	0.420	-0.490	0.754
	(0.494)	(0.266)	(0.096)	(0.121)	(0.246)	(0.322)	(0.517)	(0.742)
Urban Pop. (%)	0.291	-0.241	0.056	-0.255	-0.204	0.082	0.037	-0.234
	(0.404)	(0.307)	(0.064)	(0.152)	(0.455)	(0.186)	(0.135)	(0.797)
Number of Lynchings	0.000	-0.002	0.004	0.009	-0.004	0.003	-0.001	0.009
	(0.002)	(0.002)	(0.005)	(0.009)	(0.007)	(0.003)	(0.002)	(0.011)
Observations	338	338	338	338	338	338	338	338
R^2	0.294	0.122	0.122	0.137	0.293	0.127	0.124	0.168

Notes: This table shows the results from eight separate two-way fixed effects regressions where the dependent variables are indicators that take on a value of one in the year of adoption of suffrage restrictions in 1890, 1891, 1893, 1896, 1900, 1902, 1907, and all treatment cohorts (column 8). Only treated states are included in the sample. The explanatory variables are obtained from the census every 10 years and are interpolated for intercensal years.

Table A.5: **Pre-Treatment Oucome Levels, by Cohort (thousand $)**

	1890 Cohort	1891 Cohort	1893 Cohort	1896 Cohort	1900 Cohort	1902 Cohort	1907 Cohort	F-Test P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total Spending	1026.73	1374.29	1080.34	1504.91	1841.06	2931.91	1441.34	0.879
Spending on Schools	157.84	213.72	0.00	170.96	776.04	714.39	309.79	0.843
Spending on Pensions	0.96	0.00	0.00	0.00	24.57	0.00	0.00	0.943
Spending on Prisons	5.23	48.86	67.63	0.00	90.70	31.58	5.04	0.714
Spending on Colleges	30.63	14.77	17.50	13.75	54.24	56.50	8.00	0.864

Notes: The table presents mean outcome levels for states that implemented suffrage restrictions in 1890, 1891, 1893, 1896, 1900, 1902, and 1907. All variables are measured in 1885. Column 8 displays the p-value of unconditional F-Tests, which assess the equality of means across all seven groups of states.

Table A.6: **Heterogeneous Effects of Disenfranchisement on State Apportionment for Education Per Capita across Counties**

	State Apportionment for Education per Capita			
	(1)	(2)	(3)	(4)
Suffrage Restriction	-0.028^{***}	-0.013^{***}	0.035^{***}	0.044^{***}
	(0.005)	(0.004)	(0.012)	(0.012)
Suffrage Restriction × White Population 1880 (%)			-0.090^{***}	-0.089^{***}
			(0.019)	(0.018)
Observations	25,638	22,286	24,530	22,286
R^2	0.84490	0.94750	0.92525	0.94785
County FE	✓	✓	✓	✓
Time-invariant covariates		✓		✓

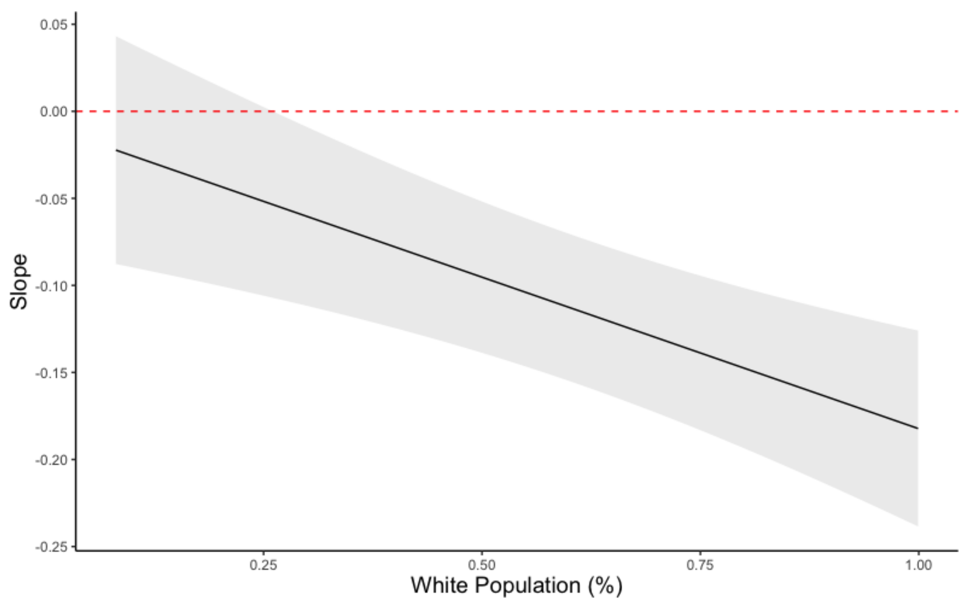
Notes: All models account for county fixed effects and include state-specific trends. The specifications in Columns (2) and (4) include the following time-invariant control variables interacted with time trends: county output, population size, the urban population share, and the assessed value of property per white capita in 1880. Columns (3) and (4) include an interaction term between suffrage restrictions and the local White population share in 1880.

Table A.7: **Heterogeneous Effects of Disenfranchisement on State Pension Spending per White Capita across Counties**

	State Pension Spending per White Capita			
	(1)	(2)	(3)	(4)
Suffrage Restriction	-0.097^{***}	-0.107^{***}	-0.003	-0.008
	(0.017)	(0.021)	(0.023)	(0.036)
Suffrage Restriction × White Population 1880 (%)			-0.165^{***}	-0.174^{***}
			(0.039)	(0.048)
Observations	10,847	9,946	10,274	9,946
R^2	0.767	0.795	0.769	0.796
County FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Time-invariant covariates		✓		✓

Notes: The dependent variable measures state spending on pensions per white capita across counties. All models account for year and county fixed effects, along with state-specific trends. The sample includes only states that implemented disenfranchisement at some point in time. The base term on white population share in 1880 is absorbed by the county fixed effects. The specifications in columns (2) and (4) include the following pre-treatment controls interacted with year indicators: county output, population size, the share of urban population, and the assessed value of property per white capita in 1880. Additionally, columns (3) and (4) features an interaction term between suffrage restrictions and the local White population share in 1880.

Figure A.16: **Marginal Effects: The Moderating Role of White Population Share on the Relationship Between Disenfranchisement and State Spending on Pensions per White Capita**



Appendix B: Data Construction Details and Sources

In this appendix, we detail the construction and sources of each variable used in this study. Information on data construction is presented in Table B1, and the sources for each observation are provided in Table B2.

Total State Public Spending

To examine how Black disenfranchisement influenced the overall fiscal trajectory of states, we utilize the total state government expenditures for each year. This measure usually represents the total amount disbursed by the state treasury, as documented in the annual or biennial reports of the state auditor, comptroller, or treasurer. In some cases, state government expenditures on public schools (referred to then as common schools) and prisons were not executed through the state treasury. When and where this occurred, we located the separate reports for these items and integrated their amounts into the total state spending.

Redistributive Spending

To capture redistributive spending preferred by lower-income Whites, we collected data on two key measures: state government expenditures on primary education (common schools) and pensions for Confederate veterans and their widows. We focus on these two goods and classify them as redistributive for several reasons. First, these two items alone accounted for approximately 30% of total expenditures across states during this period. Although there were certainly other categories that could potentially be considered as redistributive spending (*e.g.*, state hospitals), common schools and Confederate pensions were the primary redistributive items of this era. Additionally, they were financed through property taxes (often explicitly), which fell primarily on the state's wealthier residents.[1]

[1] For instance, each year between 1903 and 1910, the Alabama state government levied a 6.5 mill property tax (0.65% annual tax on the total assessed value of taxable property). Of this, 3 mills were dedicated solely for common schools and 1 mill for Confederate pensions.

For education, we created an annual panel of total state-level spending on common schools.[2] Importantly, funding for common schools did not include expenditures on high schools and universities, the benefits of which mostly accrued to upper-income residents. Simply put, common school spending represented the largest and most important redistributive allocation for both Southern states and other similarly developed states in this era (Goldin and Katz 2010; Lindert 2004). This spending was funded by the wealthy, and the benefits accrued mostly to lower-income residents, notably Whites.[3]

To measure total state spending on common schools annually between 1880 and 1910, we again used the state auditor, comptroller, or treasurer reports. In this period, some states changed the allocation process, with state-mandated property taxes (including poll taxes) for common schools no longer flowing through the state treasury. In cases where a state ceased to include the amount of state expenditures on common schools in their primary reports, we resorted to the state superintendent of public education reports.

The second primary redistributive good in our analysis relates to pensions for Confederate army veterans. Since Confederate veterans were ineligible for federal Civil War veteran pensions, Southern states began providing modest pensions for Confederate veterans in the 1880s, initially targeting soldiers disabled in the war. Although these pension programs started with limited scope and targeting, they expanded substantially over time to encompass widows and impoverished veterans. In 1889, the year of the introduction of the first suffrage restriction, the average Southern state allocated approximately 1% of its total expenditures towards pensions. By the end of our study period, pensions for Confederate veterans constituted over 20% of total state spending in four states and averaged around 10% across all fourteen states. Similar to common schools, Confederate pensions were predominantly financed through progressive property taxes, as most states had a dedicated rev-

[2]While this data includes revenue from regressive poll taxes and some other sources (such as liquor taxes), the vast majority of these revenues originated from property taxes. As previously mentioned, thirteen of the fourteen states had a dedicated stream of property taxes allocated to common schools.

[3]Due to segregated schools, we know that by 1910, per pupil spending for white students was on average more than three times that for black students (Margo 2007, Table 2.5).

enue stream specifically earmarked for pensions. Notably, this spending exclusively benefited white recipients, as the Confederate army primarily comprised white men. Moreover, it was intended for impoverished veterans, although the extent to which non-impoverished whites received these benefits remains unclear. We have a nearly complete panel of spending on pensions for each state between 1880 and 1910 for each state.

Non-Redistributive State Spending

We also used the reports to compile expenditure data on essential items that did not primarily benefit the lower-income population. We selected five such items, which include: the state judicial system, insane asylums, debt and interest payments, higher education, and coercion. These constituted core functions of the state in this period and accounted for a sizable share of overall state expenditures (more than 43% on average). Additionally, these items are easily and uniformly identifiable in the state reports, enabling us to gather nearly comprehensive data that is comparable across states. Insane (lunatic) asylums, in particular, served an outsized role in this period as the primary way to treat mental illness, and one that fell almost entirely on state governments.[4]

Higher education expenditures encompass spending on colleges, universities, medical schools, as well as technical, agricultural, and engineering schools. Additionally, we included state public funding for private colleges (such as the Maryland General Assembly's annual allocation for Johns Hopkins University). We excluded from this category spending on schools designated for teacher preparation, historically known as normal schools. We also excluded all federal disbursements to universities (usually under the provisions of the Morrill-Land Grant Acts) that were expended through each state's treasury.

We quantify state expenses on coercion by examining allocations to penitentiaries, prisons, and various other items associated with incarceration, such as expenses related to transporting convicts/prisoners and capture fees. When spending on these components of incarceration were not

[4]Between 1880 and 1910, the institutionalization rate (per 100,000) in state asylum facilities rose threefold (US Bureau of the Census, 1926, p. 112).

executed through the state treasury, and therefore occasionally not reported in the state's primary fiscal transactions report, we located this information in the distinct report specific to the penitentiary (often sourced from the penitentiary's warden or its Board of Trustees).

Other Data

Our principal covariates include the total state population, the percentage of the population residing in urban areas with a minimum population of at least 2500 individuals, and the combined value of agricultural and manufacturing output across states. We obtained these data from the 1880, 1890, 1900, and 1910 Censuses and employed linear interpolation to estimate values for years not covered by the Census.

Table B1: Variables and Sources: Annual State-level Data

	Description	Source
Spending Outcomes		
Total State Expenditures, 1880-1910	total annual state government spending	see auditor, comptroller & treasurer reports in Table B2
State Spending Higher Education, 1880-1910	annual state spending on white colleges and universities	see auditor, comptroller & treasurer reports in Table B2
State Spending Prisons, 1880-1910	annual state spending on prisons, penitentiary and expenditures on incarceration	see auditor, comptroller & treasurer reports and penitentiary reports in Table B2
State Spending Common Schools, 1880-1910	annual state spending on public primary (common) schools	see auditor, comptroller & treasurer reports and superintendent reports in Table B2
State Spending Confederate Pensions, 1880-1910	annual state spending on pensions for Confederate Army veterans and widows	see auditor, comptroller & treasurer reports in Table B2
State Spending Courts, 1880-1910	annual state spending on the state judicial system	see auditor, comptroller & treasurer reports in Table B2
State Spending Insane Asylums, 1880-1910	annual state spending on state insane and lunatic asylums	see auditor, comptroller & treasurer reports in Table B2
State Spending Debt, 1880-1910	annual state spending on state government interest and debt	see auditor, comptroller & treasurer reports in Table B2
Main Controls		
State output, 1880-1910	value of total agricultural and manufacturing output	US Census (1880, 1890, 1900, 1910)
State Population, 1880-1910	total state population	US Census (1880, 1890, 1900, 1910)
Urbanization, 1880-1910	percentage of state population living in urban areas of at least 2500 residents	US Census (1880, 1890, 1900, 1910)

Table B2: Sources: State Expenditure Data

Alabama

Annual Report of the Auditor of the State of Alabama: 1880, 1881, 1883-1910

Arkansas

Biennial Report of the Auditor of State, To the Governor of the State of Arkansas: 1880-1910

Florida

Annual Report of the Comptroller of the State of Florida: 1881, 1882, 1883, 1884, 1885, 1886, 1887, 1888, 1889, 1890, 1891, 1892, 1893, 1894, 1895, 1896, 1897, 1898, 1899, 1901, 1902, 1903, 1904, 1905, 1906, 1907, 1908, 1909, 1910

Georgia

Annual Report of the Comptroller General: 1882, 1883, 1884, 1885, 1886, 1887, 1888, 1889, 1890, 1891, 1892, 1893, 1894, 1895, 1896, 1897, 1898, 1899, 1900, 1901, 1902, 1903, 1904, 1905, 1906, 1907, 1908, 1909, 1910

Annual Report of the Treasurer of the State of Georgia: 1881

Kentucky

Biennial Report of the Auditor of Public Accounts: 1881, 1883, 1885, 1887, 1889, 1891, 1893, 1895, 1897, 1899, 1901, 1903, 1905, 1907, 1909, 1911

Louisiana

Biennial Report of the Auditor of Public Accounts for the State of Louisiana: 1882, 1884, 1886, 1888, 1890, 1892, 1894, 1896, 1898, 1900, 1902, 1904, 1906, 1908, 1910

Maryland

Annual Report of the Comptroller of the Treasury Department of the State of Maryland to the Governor of Maryland: 1881, 1882, 1883, 1885, 1886, 1888, 1889, 1890, 1891, 1893, 1894, 1895, 1896, 1897, 1899, 1900, 1902, 1903, 1904, 1906, 1907, 1908, 1909, 1910

Mississippi

Report of the Auditor of Public Accounts: 1881, 1883, 1885, 1887, 1889, 1891, 1893, 1895, 1897, 1899, 1901, 1903, 1905, 1907, 1909 (located in the *Annual/Biennial Report of the Departments and Benevolent Institutions of the State of Mississippi*)

Missouri

Biennial Report of the State Auditor to the General Assembly of the State of Missouri: 1882-1910

Report of the State Treasurer of the State of Missouri: 1880

Table B2 (*cont.*)

North Carolina

Annual Report of the Auditor of the State of North Carolina: 1880, 1882-1910 (located in the *Executive and Legislative Documents Laid Before the General Assembly*)

Report of the Superintendent of Public Instruction of North Carolina to the Governor: 1880, 1882, 1884, 1886, 1888, 1890, 1892, 1894, 1896, 1898, 1900, 1902, 1904, 1906, 1908, 1910

South Carolina

Report of the Comptroller General to the General Assembly of the State of South Carolina: 1881, 1883, 1884, 1885, 1886, 1887, 1888, 1889, 1890, 1892, 1893, 1894, 1895, 1896, 1897, 1898, 1899, 1900, 1902, 1903, 1904, 1906, 1907, 1909, 1911 (located in the *Reports and Resolutions of the General Assembly of the State of South Carolina*)

Annual Report of the State Superintendent of Education of the State of South Carolina: 1881, 1882, 1883, 1884, 1886, 1888, 1890, 1892, 1893, 1896, 1897, 1898, 1900, 1902, 1903, 1904, 1905, 1906, 1907, 1909

Tennessee

Biennial Report of the Comptroller of the State of Tennessee: 1880-1910

Texas

Annual Report of the Comptroller of Public Accounts: 1881, 1883, 1884, 1886, 1887, 1888, 1889, 1890, 1891, 1892, 1893, 1894, 1896, 1897, 1898, 1900, 1901, 1902, 1903, 1904, 1905, 1906, 1907, 1908, 1909, 1910

Virginia

Report of the Auditor of Public Accounts: 1880, 1881, 1882, 1883, 1884, 1885, 1886, 1887, 1888, 1889, 1890, 1892, 1893, 1894, 1895, 1896, 1897, 1898, 1899, 1900, 1901, 1903, 1904, 1905, 1906, 1907, 1908, 1909, 1910 (located in the *Reports of the Public Officers of the State, Boards and Institutions of the Commonwealth of Virginia*)

Appendix C

Alternative Explanations

In the following section, we detail the concurrent changes implemented across states alongside voting restrictions, concluding that only in a singular instance might these provide an alternative explanation for the shifts in state expenditures we observe. It is crucial to underscore that even though states occasionally stipulated explicit thresholds for school taxes, they rarely, if ever, defined any limits or minimum thresholds for expenditures on prisons, colleges, and pensions. Therefore, most of our discussion pertains to provisions imposed by states on common school expenditures.

Within the fourteen states of our sample, four altered suffrage rules via a statute of the state legislature, namely Arkansas, Florida, Tennessee, and Texas. Even though a bargain might have been established when these were passed, mandatory spending on schools remained unchanged. Other states, such as North Carolina and Georgia, modified suffrage rules through constitutional amendments, which were not passed simultaneously with amendments that affected public finances. Lastly, Alabama, Louisiana, Mississippi, South Carolina, and Virginia altered suffrage through constitutional conventions that instituted entirely new state constitutions.

Louisiana established a maximum property tax rate at 6 mills and defined a minimum property tax rate for schools at 1.25 (of 6) mills. This alteration goes against our argument that suffrage restrictions led to a reduction in school expenditures. If anything, it indicates that in the absence of minimum thresholds, school spending could have declined even further post the implementation of suffrage restrictions. Furthermore, the constitution imposed a limit of $15,000 on funding allocated to the state university. However, this limit was repealed in 1904 (six years after the adoption of the new state constitution)[5], supporting our argument that political elites were likely to modify spending allocations after increasing their political power. The 1898 constitution also placed constraints on

[5]The cap on spending was eliminated, and the subsequent amendment introduced a revised stipulation that stated: "and the General Assembly shall make such additional appropriations as may be necessary for its maintenance and support and improvement, and for the establishment, in connection with said

the expenditure towards Confederate pensions, a stipulation that was repealed and replaced with a higher amount every couple of years.

In Louisiana, therefore, the fiscal regulations introduced alongside suffrage restrictions, as per constitutional provisions, did not predict or align with the ensuing shifts in expenditures we observe. In fact, these regulations seem designed to direct expense allocations in the opposite direction to our theoretical predictions. This suggests that certain political factions or specific political groups possibly foresaw the potential risks attached to suffrage restrictions and took measures to preemptively mitigate them. In other words, absent these constraints, the downward downward trends we witness in redistributive spending could have been even more pronounced.

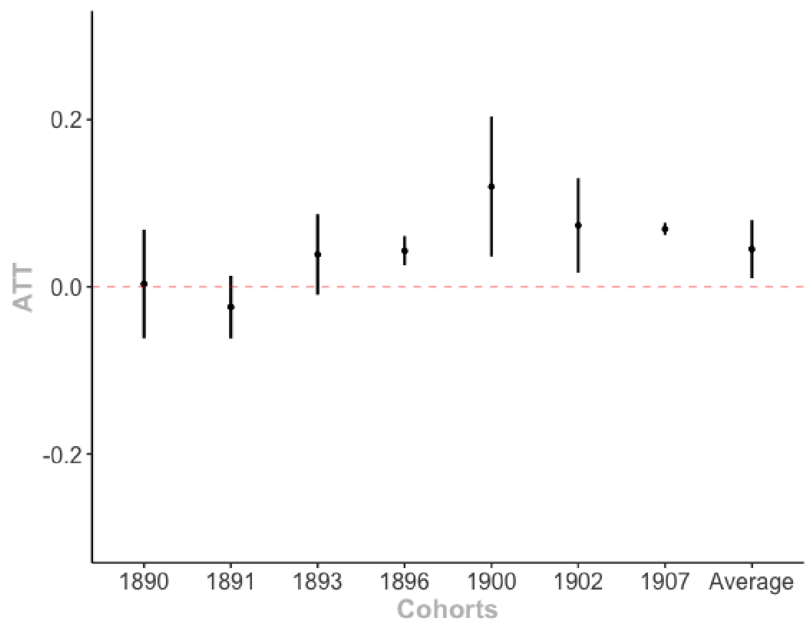
In Mississippi, the constitution mandated a minimum of 4 months of free education, yet did not specify any funds to be allocated to common schools. In South Carolina, the constitution decreed a 3-mill levy for common schools, a requirement that did not reflect any change, as this was the same rate already mandated prior to their 1895 disenfranchising constitution. This constitution also directed that all net profits from alcohol sales be assigned to common schools, though the details of the prior arrangement remain unclear. In Virginia, the constitutional mandate stated that the "state shall levy no less than 1 mill and no more than 5 mills for public schools," a stipulation that did not mark any deviation from the earlier provisions in place prior to the constitution.

Alabama is the only case that might pose a threat to inference, suggesting that our findings might not accurately reflect the effect of the treatment of interest. This is because changes in state finances could stem from other constitutional provisions adopted in tandem with suffrage restrictions in the state. Specifically, Alabama established a maximum property tax rate of 6.5 mills and designated a mandatory 3-mill tax for schools, marking an increase from previous measures. These changes run counter to our argument, thereby reducing the risk that they may be the primary drivers of our results. In fact, these shifts suggest our findings might provide a conservative estimate of the potential impact of suffrage restrictions on school spending. Moreover, Alabama set a minimum level of funding for

institution, of such additional scientific or literary departments as the public necessities and the well-being of the people of Louisiana may require."

the University of Alabama. Although the state ultimately appropriated significantly more than this minimum ($35,000), one might be concerned that this provision could underlie the increase in college expenditures we observe following disenfranchisement. To alleviate this concern, we replicate our college spending analyses excluding Alabama from the sample. The results are displayed in Figure C1, which shows that our findings remain unchanged.

Figure C1: **Overall Effect of Suffrage Restrictions on College Expenditures, excluding Alabama: Average and Cohort-Specific ATT Estimates**



Notes: Cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907) and average of all cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 13 Southern states (all except Alabama), three of which are never-treated. Standard errors clustered at the state level.