The Fiscal Consequences of Black Disenfranchisement in the American South

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Abstract

We study the fiscal consequences of the large-scale disenfranchisement of Blacks in the American South during the Jim Crow era. Using an original annual dataset of state government spending in fourteen Southern states between 1880 and 1910, we find that disenfranchisement had no effect on overall state spending. Yet, the composition of state expenditures changed substantially. Spending on goods favored by elites, colleges and prisons, increased following disenfranchisement, while spending on redistributive goods, common schools and pensions, declined. Our findings have important implications for our understanding of the historical role that Black disenfranchisement played in American fiscal development.

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1 Introduction

An extensive literature has been dedicated to examining the effects of suffrage expansions on public spending in the U.S. (e.g., Cascio and Washington 2014; Husted and Kenny 1997; Lott and Kenny 1999; Miller 2008). However, our understanding of the fiscal consequences of widespread contractions in the franchise remains limited. In this paper, we conduct a comprehensive and fine-grained analysis of the changes in state government spending patterns during one of the most significant instances of democratic backsliding in U.S. history: the so-called *Jim Crow* era.

Between 1890 and 1907, each of the eleven former Confederate states implemented measures like poll taxes and literacy tests that strictly limited Black access to the ballot box. To investigate the effects of these restrictions, we draw upon an original dataset of annual state government expenditures from 1880 to 1910 across the fourteen states where slavery was legal and widespread in 1860. Our data encompass not only the total aggregate state spending but also detailed, disaggregated expenditures across several key categories: the judiciary, white colleges and universities, prisons, insane asylums, debt and interest payments, Confederate-veteran pensions, and common schools (pre-high school public education during this period). These seven items account for the majority of state expenditures, constituting an average of 72% of each state's budget. Moreover, they encompass a diverse range of expenditure types, including redistributive goods that are favored by low-income whites (common schools and Confederate-veteran pensions); collective goods preferred by elites (universities and prisons); and other expense categories that do not show clear bias towards any specific group (courts, insane asylums, and debt interest payments).

We use a staggered difference-in-differences approach to examine how spending patterns differed between states that implemented suffrage restrictions and those that either had not yet or never would disenfranchise. Our findings indicate that Black disenfranchisement did not affect overall state spending or allocations to non-targeted goods (courts, insane asylums, and debt interest payments). However, we observe a substantial shift in the composition of state spending with distributional consequences. Following the enactment of suffrage restrictions, spending on goods typically favored by elites increased (colleges and prisons), whereas spending on redistributive goods declined (common schools and pensions). In other words, the adoption of restrictive voting rights led to a significant reallocation of state spending, shifting funds towards elite goods and away from those preferred by lower-income whites.

These findings offer valuable insights that enable us to adjudicate among various political economy theories on the consequences of democratic backsliding. Notably, median-voter models and their offshoots suggest that large-scale disenfranchisement of lower-income groups should result in both reduced redistribution and a smaller fiscal state (e.g., Acemoglu et al. 2015; Boix 2003; Meltzer and Richard 1981). In contrast, our results show that changes in electoral rules primarily influenced the distribution of public funds, rather than altering the overall amount of spending. This aligns with the findings of prior research examining the impact of pro-democratic reforms, such as the extension of voting rights (Cascio and Washington 2014; Husted and Kenny 1997; Miller 2008) and the elimination of state-legislative malapportionment (Ansolabehere et al. 2002). However, our evidence is among the first to investigate the consequences of large-scale democratic reversal. Specifically, our results indicate that economic elites, leveraging their unchallenged authority, did not cut public spending but instead redirected it towards goods that disproportionately benefited their interests.

Our findings also contribute to a debate in economic history and American political development on whether formal Black disenfranchisement, achieved through mechanisms like poll taxes and literacy tests, had a meaningful effect on societal and economic outcomes. Some scholars suggest that political power had already shifted to white elites before the formal disenfranchisement measures were put in place (e.g., Key 1949; Acemoglu and Robinson 2008). Therefore, they argue that the adoption of these restrictions had minimal additional impact on diminishing Black political power. In contrast, others contend that formal disenfranchisement marked a critical turning point, shifting power from a contested arena to one where white elites held uncontested dominance (e.g., Kousser 1974; Naidu 2012). Our results demonstrate that formal disenfranchisement, representing a change in de jure rather than de facto power, played an important role in shaping the distribution of public resources.

The remainder of the paper proceeds as follows. We first provide relevant information on the

Jim Crow era South. Next, we describe our data and archival sources. We then present a graphical analysis that traces the spending trajectories of states that implemented disenfranchising measures and compare them with those of other states. The following section outlines our empirical approach and presents our findings. We conclude with a brief discussion of our contributions.

Background

Defeat in the American Civil War (1861-1865) resulted in the emancipation of more than 40% of the Confederacy's population and the extension of the suffrage to the formerly enslaved with the Congressional Reconstruction Acts of 1867 and 1868 (Foner 2014). While the redistributive threat that Republican "Reconstruction" governments posed had been eliminated by 1877, adult Black males formally retained the right to vote and they remained a threat to the political dominance of Southern Democratic elites (Kousser 1974; Perman 2003; Valelly 2009). Far from the 'One-party South' it would become in the 20th century, 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). 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 in several other states (Perman 2003). These anti-elite, populists and often cross-racial working-class movements were driven in part by the increase in regressive taxation and retrenchment in public spending that emerged in the post-Reconstruction period (Kousser 1974; Hahn 2006; Hyman 1989). The electoral threat they posed to Democratic Party rule ended with the adoption by eleven states of various suffrage restrictions, such as poll taxes and literacy tests, between 1889 and 1904 (Perman 2003; Valelly 2009). While not explicitly racial in nature, these restrictions removed the formal voting eligibility of the vast majority of the Black electorate (e.g., Keele et al. 2021; Kousser 1974). The historical record is clear that elites saw white supremacy as crucial for maintaining Democratic Party, and with it elite, 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." Despite scant empirical evidence on the extent to which poorer whites electorally supported Black disenfranchisement, many historians claim that an inter-class white coalition was sustained primarily through appeals to the "reinforcement of racial domination" rather than a cross-class fiscal bargain (Marx 1998, p. 143). The voting eligibility of most lower-income whites was initially preserved by the adoption of 'grandfather' clauses and other similar mechanisms.

The consequences of these institutional changes on Southern fiscal outcomes have been studied and documented incompletely. Previous studies have revealed that during this period, the American South exhibited a pattern of low expenditure on broad public goods, particularly public education, which is a characteristic often found in rural societies reliant on coerced labor (Alston and Ferrie 2007; Galor et al. 2009; Margo 2007; Suryanarayan and White 2021; Vollrath 2013). At the same time, Margo (2007) 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. Another noteworthy development during this period was the significant expansion of pensions for impoverished Confederate veterans after 1890. Rather than being solely driven by voting restrictions, however, Eli and Salisbury (2016) argued that increases in this highly redistributive benefit reflected a clientelistic, targeted response to anti-Democratic Party populist movements. Furthermore, Jensen et al. (2023) showed that progressive property taxes tended to increase after a state adopted suffrage restrictions. While various aspects of the historical record have been excavated and explored, there has been no systematic examination of the isolated effects of Black disenfranchisement on overall state government spending, nor has there been a comprehensive analysis of the distributional aspects of state expenditures, simultaneously considering goods favored by elites and those benefiting lower-income whites.

Data

Our analysis relies on an annual state-level panel of state government spending encompassing fourteen Southern states from 1880 to 1910. These states include all former slave states, and our timing captures the period just before and after the adoption of suffrage restrictions (i.e., 1889 to 1907). To investigate the trajectories of expenditures across states, we focus on four different outcomes. First, we consider total state-level expenditures. Next, we collected data on specific categories of public spending that can be broadly categorized into public goods preferred by elites, redistributive spending favored by lower-income voters, and public services not explicitly favored by either group.

Our data collection efforts yielded extensive coverage. Out of 434 possible state-year observations, we have data for total state spending and disaggregated expenditures across seven categories, for 403 state-years. Importantly, no state is missing more than 5 (out of a possible 31) years of data. To the best of our knowledge, no other study has such comprehensive, detailed, and nearly complete panel-style data for a similar historical period. As such, we are the able to explore the fiscal consequences of these particular historical developments with an unusually high-level of precision. The specific reports and years used to construct each of these variables are listed in Appendix B, which provides a comprehensive overview of all the data utilized in this paper.

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.

Spending on Goods Preferred by Elites

To capture state spending on goods favored by elites, we focus on two items. First, we measure how much each state spent on white universities and colleges. This is categorized as an elite good owing to the exceedingly low proportion of Southern whites, mostly males, attending college during this period.[1] Because Southern colleges were segregated, we were able to isolate public expenditure exclusively for white institutions of higher education. This category encompasses 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.

The second category of elite-targeted selective goods pertains to state expenditures on what we refer to as coercive capacity. Many scholars have documented the pervasive coercion of the Black population in the postbellum South, highlighting the use of incarceration as a means employed by elites to control the labor supply (e.g., Muller 2018; Rubio-Ramos 2022; Schwarz 2023). We quantify this phenomenon by examining state spending on penitentiaries, prisons, and various other expenditures associated with incarceration, such as expenses related to transporting convicts/prisoners and capture fees. When spending on these components of incarceration were not 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).

[1] Although we lack state-level data, we know that in 1910, the final year of our study, less than 3% of American adults aged 24 and older held a college degree (*Statistical Abstract of the United States* (U.S. Census Bureau, 1999, Tables 265 and 1426).

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 45% 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.[2]

For education, we created an annual panel of total state-level spending on common schools.[3] 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.[4]

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

[2]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.

[3]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.

[4]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).

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 revenue 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-Targeted State Spending

We also used the reports to compile expenditure data on essential items that did not clearly align with the preferences of either elites or the lower-income population. This approach allowed us to examine whether these items exhibited different trajectories compared to the categories favored by specific groups, effectively serving as a kind of placebo test. We selected three such items, collectively referred to as non-targeted spending, which include: the state judicial system, debt and interest payments, and insane asylums. These constituted core functions of the state in this period and accounted for a sizable share of overall state expenditures (more than 25% in many state-years). Additionally, these items are easily 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.[5]

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.

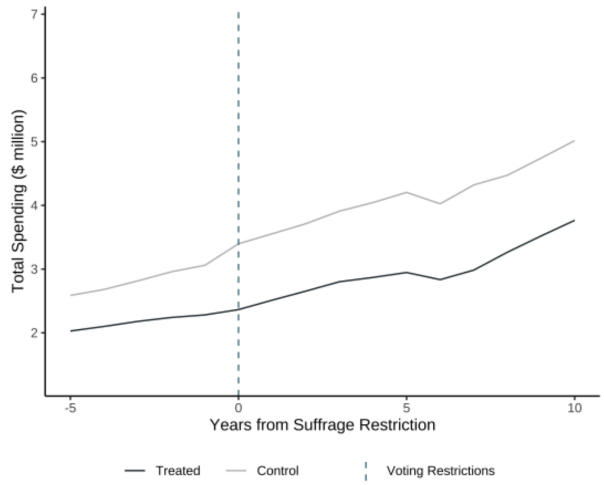
The Impact of Disenfranchisement

We start with graphical evidence. To determine the potential influence of Black disenfranchisement on public expenditures, we examine whether the spending trajectories of disenfranchising and non-disenfranchising states remain parallel before and after the implementation of voting restrictions. Given the staggered adoption of literacy tests and poll taxes across states, we construct these trends based on the number of years since the enforcement of suffrage restrictions. This approach provides a more accurate representation of the incremental divergence in spending between states with and without restrictions than figures illustrating year-by-year trends.

Figure 1 illustrates the potential effect of voting restrictions on total state government spending. The black line depicts total expenditures in disenfranchising states, denoted as "treated" states, over a 5-year period preceding and a 10-year period following the implementation of voting restrictions. The gray line shows expenditure patterns among the comparison group, reflecting the secular trend affecting all states, regardless of their voting regulations. The parallel trajectories of the two groups, both before and after the treatment, suggest that restrictive voting laws had a limited influence on

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

Figure 1: **Total State Spending in States with and without Voting Restrictions by Time since Adoption**



(a) Total State Spending

Notes: The black line shows average state spending across states that adopted disenfranchising measures. The gray line reflects the average trend in the comparison group which includes both never-treated and not-yet-treated units. For each state where voting restrictions were implemented in a given year, we calculate the average amount of spending among all states that had no restrictions in place either in that particular year or in the preceding years.

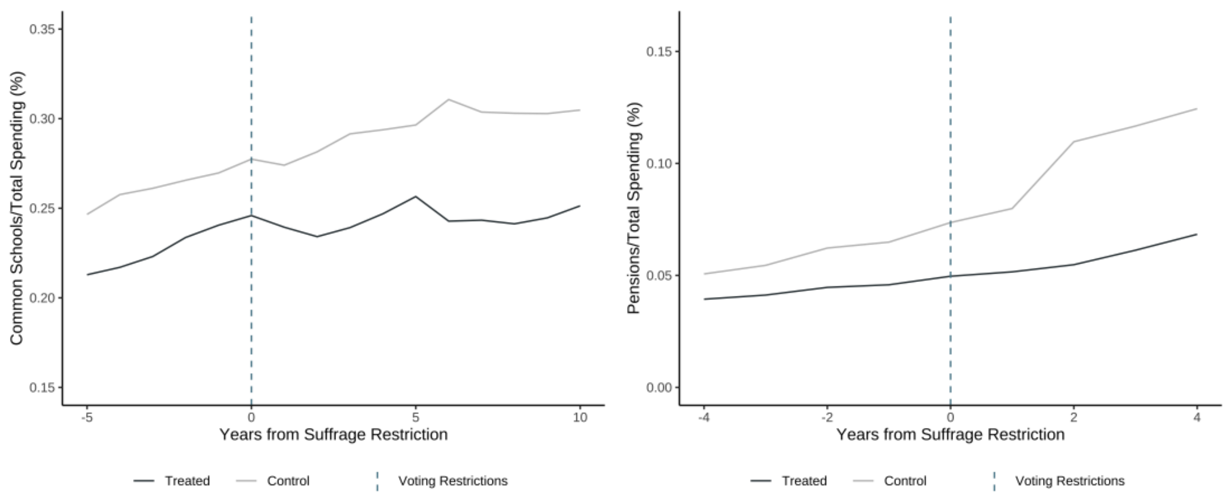
state expenditure patterns.

Figures 2-4 demonstrate the potential effect of voting restrictions on the different expenditure categories. Figure 2 shows spending on common schools and pensions, two public services that were favored by low-income whites. Figure 3 shows prisons and white higher education, goods skewed towards economic elites. Finally, Figure 4 shows the three non-targeted expenditures categories, namely courts, insane asylums, and debt payments.

The extent of divergence in redistributive spending following disenfranchisement varies across different items. The trends for common schools expenditures exhibit a slight divergence across the treated and comparison states, as depicted in Figure 2(a). Pension spending, in turn, seems to have been more strongly affected. As Figure 2(b) shows, although expenses in treated states are increasing, their rate of growth appears to be slower than that of the comparison group. This trajectory suggests that all states experienced an upward trend in pension expenditures during the period under observation. However, states that implemented franchise restrictions lagged behind the comparison group in escalating their pension spending following the enactment of these restrictions. This lag is particularly notable given that, prior to the imposition of voting restrictions, both sets of states had been tracking along parallel paths in terms of their pension expenditures. Overall, this graphical evidence suggests that suffrage restrictions led some states to decelerate the growth of pension spending and marginally decrease their funding for common schools.

The trends in state expenditures on elite goods, by contrast, exhibit a pronounced divergence between the treated and comparison states following the adoption of suffrage restrictions Notably, spending on colleges (Figure 3a) and prisons (Figure 3b) among the treated states grow at a substantially higher pace than the comparison group. Finally, non-targeted spending, specifically expenditures allocated to courts (Figure 4a), insane asylums (Figure 4b), and debt payments (Figure 4c), reveals no discernible differences in trends.

Figure 2: **Spending on Redistributive Public Goods by Time since Adoption of Voting Restrictions**

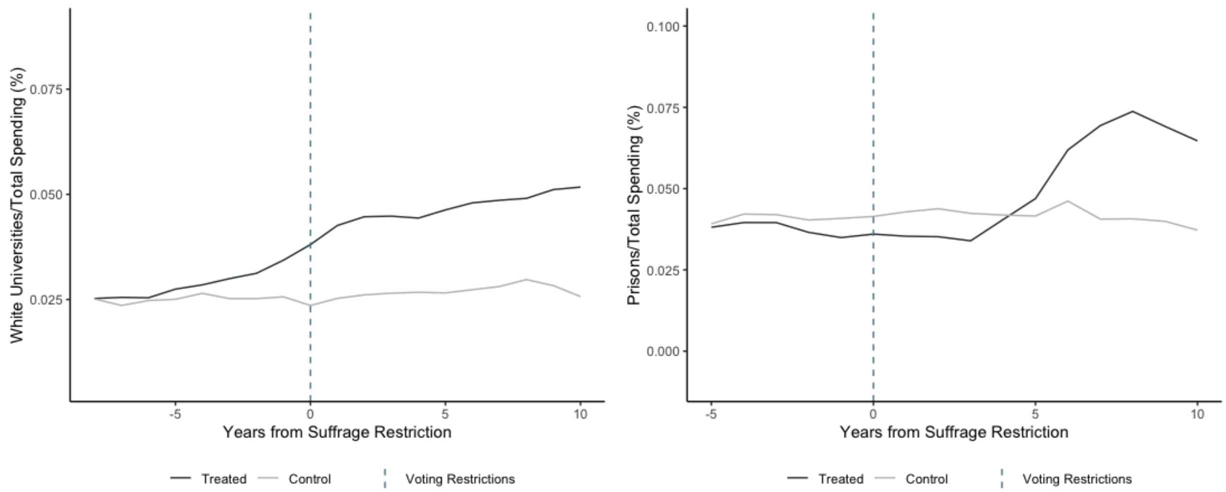


(a) Spending on Common Schools (%)

(b) Spending on Pensions (%)

Notes: The black lines show average share of spending on (a) commons schools and (b) pensions among states that adopted disenfranchising measures. In Figure (a) the comparison group includes both never-treated and not-yet-treated units. For each state where voting restrictions were implemented in a given year, we calculate the average amount of spending per category among all states that had no restrictions in place in that particular year. In (b), the gray line reflects the average trend of not-yet-treated states only, as never-treated states did not have a pension spending category during the period under analysis. The post-treatment period in this figure is therefore shorter, reflecting the number of years that elapsed between the implementation of suffrage restrictions in the last two states to adopt such measures.

Figure 3: **Spending on Elite Public Goods by Time since Adoption of Voting Restrictions**

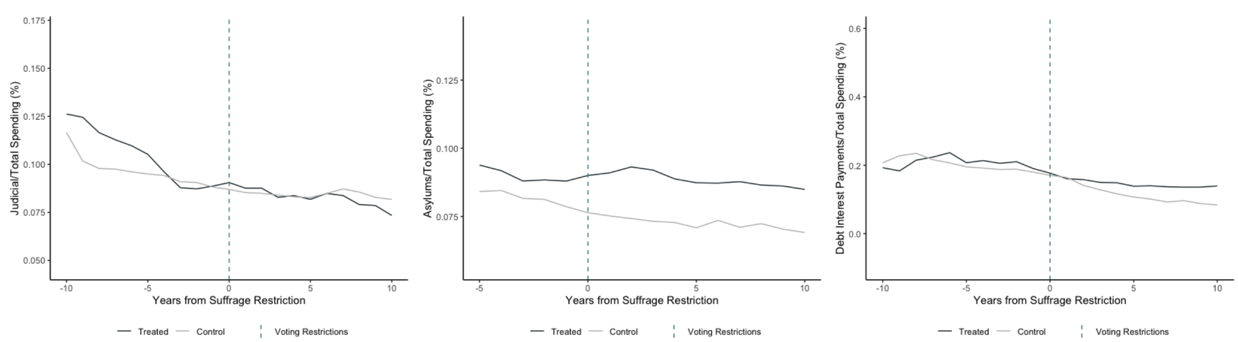


(a) Spending on Colleges (%)

(b) Spending on Prisons (%)

Notes: The black lines show average share of spending on (a) colleges and (b) prisons across states that adopted disenfranchising measures. The gray lines reflect the average trend of the comparison group, which includes both never-treated and not-yet-treated units. For each state where voting restrictions were implemented in a given year, we calculate the average amount of spending per category among all states that had no restrictions in place in that particular year.

Figure 4: **Non-Targeted Spending by Time since Adoption of Voting Restrictions**



(a) Spending on Courts (%)

(b) Spending on Asylums (%)

(c) Spending on Debt Interest (%)

Notes: The black lines show average share of spending on (a) courts, (b) insane asylums, and (c) debt interest payments among states that adopted disenfranchising measures. The comparison group includes both never-treated and not-yet-treated units. For each state where voting restrictions were implemented in a given year, we calculate the average amount of spending per category among all states that had no restrictions in place in that particular year.

Difference-in-Differences Approach

To estimate the effect of suffrage restrictions on the distributional content of state-level spending, we employ difference-in-differences models. We use the earliest year of implementation of poll taxes and/or literacy tests as a measure of the timing at which each state has received treatment. Until recently, the canonical approach has been to apply generalizations of the two-way fixed effects model to the multi-period, staggered timing cases. This involves regressing the outcome on unit and period fixed effects and a treatment indicator:

$$Y_{it} = \beta D_{it} + \alpha_i + \mu_t + \varepsilon_{it} \tag{1}$$

Where Y_{it} are the expenditures allocated to a specific category in state i at time t, α_i are state fixed effects and μ_t are period fixed effects.

Recent literature on difference-in-differences (DID) designs has raised important concerns regarding the estimand of two-way fixed effects specifications in a staggered adoption setting. Specifically, when the treatment is implemented gradually and the effects vary over time, the estimand may not fully capture the true treatment effect (e.g., Sun and Abraham 2021; Callaway and Sant'Anna 2021; de Chaisemartin and D'Haultfoeuille 2022). To address these concerns, we adopt the estimation method proposed by Callaway and Sant'Anna (2021) (henceforth CSA). The CSA approach assumes irreversibility of treatment, which is applicable to our study since literacy tests and/or poll taxes, once implemented by a state, remained in place throughout the period under analysis. Our data set comprises 14 states, of which 11 implemented restrictive measures at different points in time. As previously mentioned, literacy tests and poll taxes were introduced in various states during different years, namely 1890, 1891, 1893, 1896, 1900, 1902, and 1907.

Much like conventional difference-in-differences designs, unbiased estimation in our study hinges upon a parallel trends assumption. In our specific case, this implies that if there had been no voting restrictions, public expenditures in both disenfranchising and non-disenfranchising states would have, on average, followed parallel trajectories. CSA also propose estimators that use units treated at later points in time (not-yet-treated) instead of the never-treated units as controls. In this case, the parallel trends assumption is applied only to the groups that eventually receive treatment, and not to the never-treated group. These estimators become particularly valuable when a never-treated group is absent, as is the case in our pension spending models. However, even in situations where never-treated groups exist, the use of not-yet-treated units offers a larger control group, resulting in more precise estimations. As a result, our analyses consistently incorporate not-yet-treated states into the comparison group.

Figure 5 presents CSA results for our first variable of interest, total state spending. The dynamic event-study estimates, presented in Figure 5(a), show the average treatment effect based on the duration of exposure to the treatment. Effects are shown for five years before and ten years after disenfranchisement. Consistent with the simple graphical evidence presented earlier, the dynamic event study coefficients show no clear effect of voting restrictions on overall state spending levels throughout the period under analysis. Importantly, all of the event time estimates prior to the implementation of suffrage restrictions are close to zero. There is no evidence of the existence of any pre-trends or systematic pre-treatment effects. Figure 5(b) shows the average treatment effect on the treated (ATT) for each of the seven implementation years, as well as their average. The aggregate ATT across all cohorts for total state spending is -0.11. However, the effect is not statistically different from zero, with the 95% confidence interval ranging from -0.37 to 0.13. Although there is some heterogeneity in the estimated effects across different cohorts, the average treatment effect indicates there is no systematic decline in total spending after the implementation of suffrage restrictions.

Figure 6 shows the results from models using our two measures of redistributive spending as dependent variables. These models test whether voting restrictions affected the level of state spending on (a) common schools and (b) pensions. Once again, in line with the patterns depicted in Figure 2(a), there is no evidence of any effect on common school expenditures. The event study estimates, as well as the point estimates of the average and the cohort-specific ATTs are always close to zero.[6] Figure 6(b) shows the dynamic event-study estimates and 95% confidence intervals with pension spending

[6] Appendix Figure A.1 shows the point estimates of the average and cohort-specific ATTs.

as the dependent variable. Here, the adoption of disenfranchising measures display progressively stronger negative effects on pension spending over time. The aggregate ATT across cohorts is -0.14, equivalent to three-fifths of a standard deviation. The effect is statistically different from zero, with the 95% confidence interval ranging from -0.19 to -0.07. All the point estimates of the group-specific ATTs are negative and all but one of these effects are statistically significant (see Figure A.2).

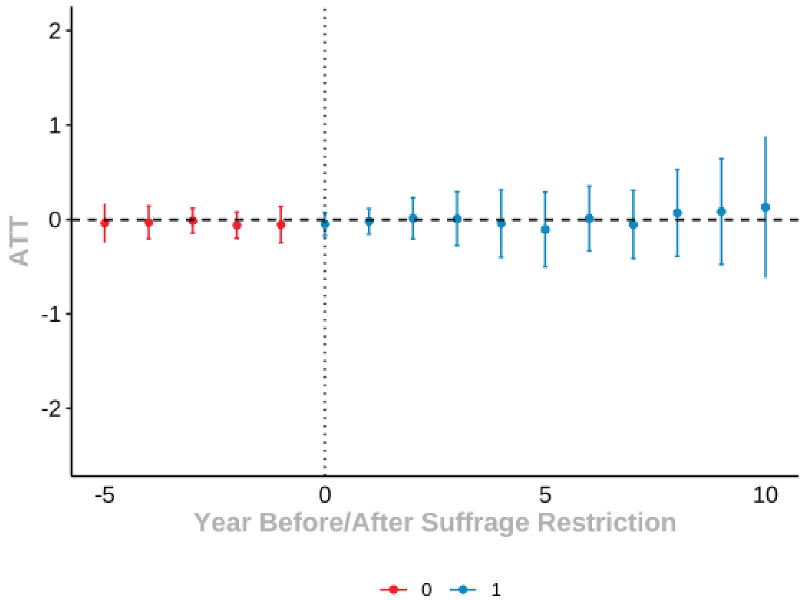
We then turn to the analysis of the impact of suffrage restrictions on spending allocated to public goods that are more closely aligned with elite interests. The estimated coefficients for the pre-treatment period are close to zero, lending credibility to the identifying assumptions of the model. The dynamic event-study coefficients show a positive effect of suffrage restrictions on college expenditures, as illustrated in Figure 7(a). The estimates exhibit a gradual increase over time, although they do not appear to reach statistical significance. However, when we aggregate the average treatment effect on the treated (ATT) across cohorts, we observe a positive and statistically significant effect of 0.04, with the 95 % confidence interval ranging from 0.01 and 0.08.[7] This effect is equivalent to approximately two-fifths of a standard deviation. A similar pattern is observed for spending on prisons. Although the dynamic event-study estimates are consistently positive, they are not always statistically significant. Nonetheless, the ATT based on cohort aggregation is 0.05, equivalent to two-fifths of a standard deviation, and is marginally statistically significant. The 95% confidence interval ranges from -0.001 to 0.11.[8]

Next, we turn our attention to the broader non-targeted categories of public spending. Similar to the patterns observed in total spending, the dynamic event study coefficients displayed in Figure 8 fail to reveal any discernible effects of voting restrictions on any of the outcome variables, specifically spending on courts, insane asylums, and debt interest payments. Figure A.5 illustrates the average treatment effect on the treated (ATT) for each cohort, along with their collective average. Despite some variation in the estimated treatment effects across different years, the average effects remain close to zero.

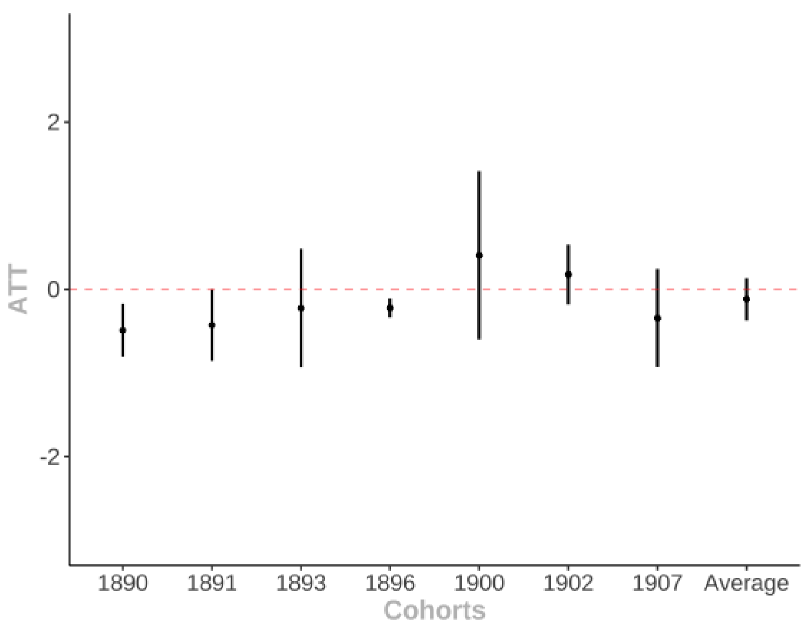
[7] Figure A.3 shows the point estimates of cohort-specific ATTs.

[8] See Figure A.4 for the cohort-specific ATTs.

Figure 5: **Overall Effect of Suffrage Restrictions Total State Spending (million $)**



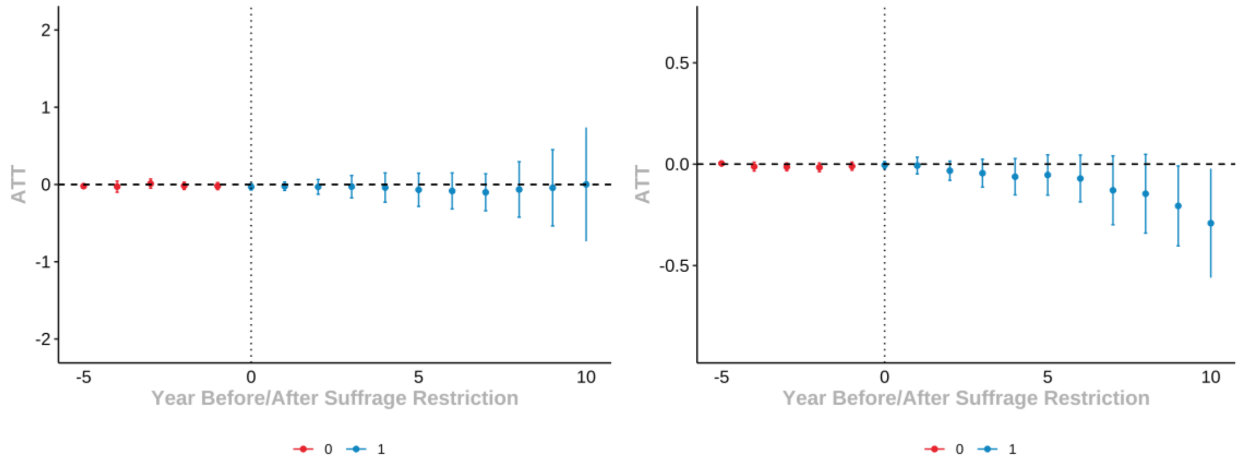
(a) **Event-Study Estimates**



(b) **Average and Cohort-Specific ATT Estimates**

Notes: Figure (a) shows event time estimates and 95% confidence intervals for total state spending. Dynamic ATTs are calculated based on 5 pre-treatment and 10 post-treatment years. Figure (b) shows 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 6: **State Redistributive Spending: Average Effect by Length of Exposure to Suffrage Restrictions**

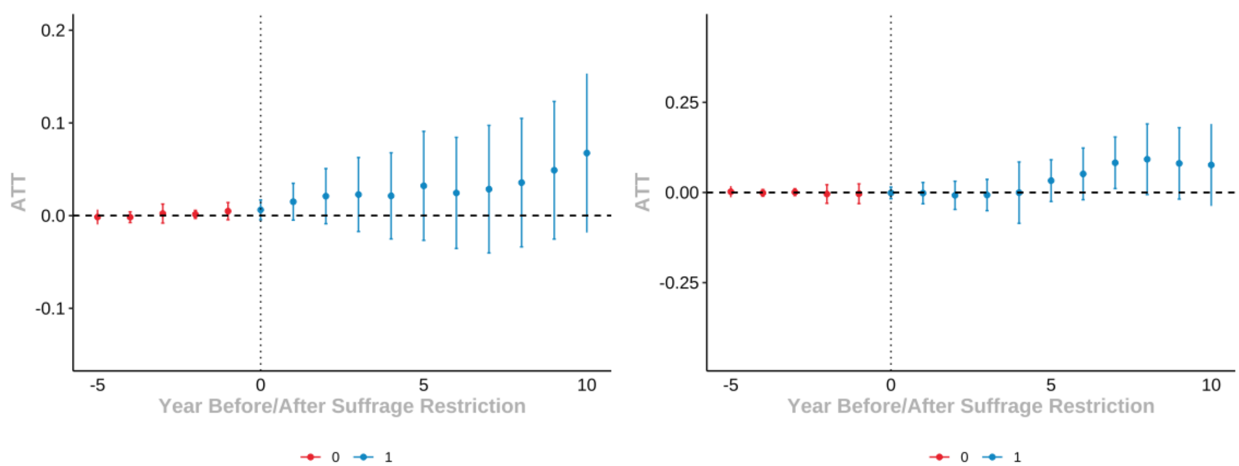


(a) Spending on Common Schools

(b) Spending on Pensions

Notes: Event time estimates and 95% confidence intervals for spending on (a) common schools and (b) pensions. Dynamic ATTs are calculated based on 5 pre- and 10 post-treatment years. In Figure (a), the sample includes 14 Southern states, of which three are never-treated. The sample for Figure (b) encompasses 11 eventually-treated states. Standard errors clustered at the state level.

Figure 7: **State Spending on Public Goods Favored by Elites: Average Effect by Length of Exposure to Suffrage Restrictions**

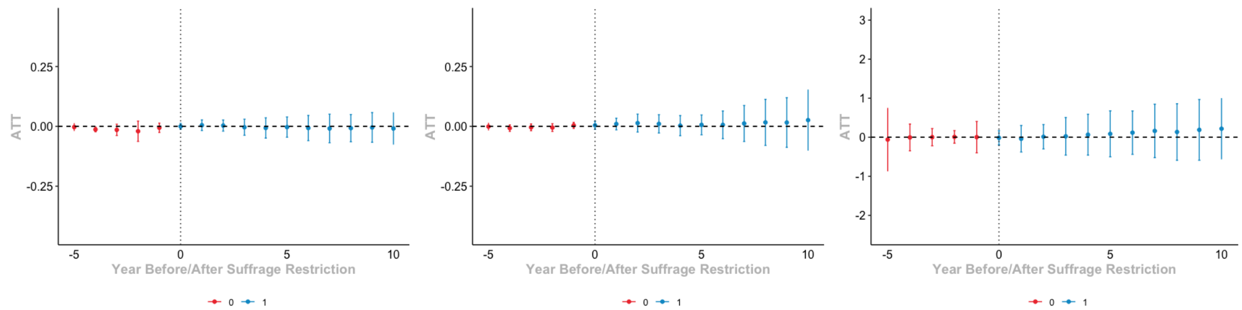


(a) Spending on Colleges

(b) Spending on Prisons

Notes: Event time estimates and 95% confidence intervals for spending on (a) colleges and (b) prisons. Dynamic ATTs are calculated based on 5 pre- and 10 post-treatment years. Sample includes 14 Southern states, of which three are never-treated. Standard errors clustered at the state level.

Figure 8: **Non-Targeted State Spending: Average Effect by Length of Exposure to Suffrage Restrictions**



(a) Spending on Courts (b) Spending on Asylums (c) Spending on Debt Interest Payments

Notes: Event time estimates and 95% confidence intervals for spending on (a) courts, (b) insance asylums, and (c) debt interest payments. 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.

Table 1 shows the average and cohort-specific ATT and 95% confidence intervals for each of our main dependent variables, namely total expenditures, and spending on common schools, pensions, colleges, and prisons. These results are summarized in Figure 9, which presents the average cohort ATTs. Figure A.6 in the Appendix shows the same overall average of cohort-specific ATTs but for the dependent variables measured as shares of total state spending.[9] The results are quite similar. Overall, the CSA estimates indicate that the introduction of voting restrictions did not significantly affect total state spending. There was, however, a noticeable shift in the allocation of fiscal resources, with public goods favored by elites receiving increased funding (colleges and prisons), while spending on redistributive public goods either decreased (pensions) or remained stagnant (common schools).

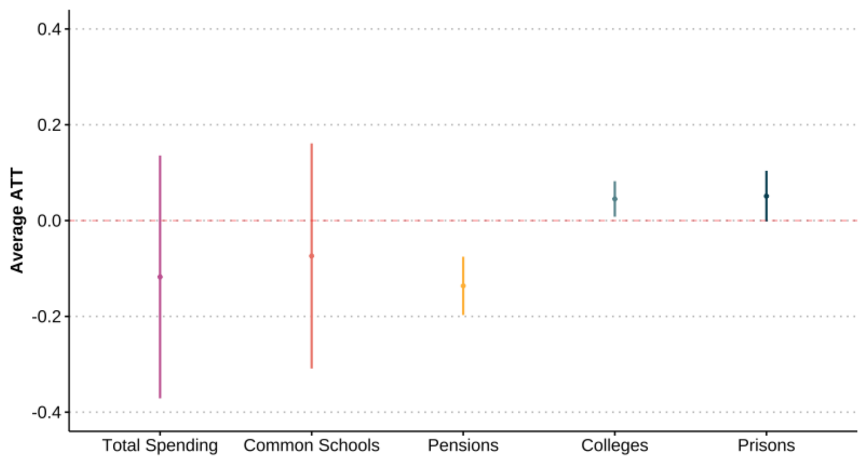
[9]The full results for each dependent variable are presented in Figures A.7 through A.13.

Table 1: **Average and Cohort-Specific ATT Estimates: Overall Impact of Suffrage Restrictions on State Expenditures**

	Total Spending	Common Schools	Pensions	Colleges	Prisons
	(1)	(2)	(3)	(4)	(5)
Average	−0.117	−0.074	−0.136	0.0451	0.051
	[−0.371, 0.136]	[−0.312, 0.164]	[−0.194, −0.078]	[0.007, 0.082]	[−0.003, 0.106]
1890	−0.490	−0.28	−0.236	0.005	0.091
	[−0.806, −0.174]	[−0.616, 0.048]	[−0.373, −0.099]	[-0.061, 0.071]	[0.001, 0.182]
1891	−0.427	−0.195	−0.233	−0.022	0.016
	[−0.856, 0.001]	[−0.403, 0.012]	[−0.369, −0.096]	[−0.055, 0.010]	[−0.035, 0.068]
1893	−0.224	−0.366	−0.162	0.040	−0.061
	[−0.933, 0.484]	[−0.519, −0.214]	[−0.247, −0.077]	[0.003, 0.077]	[−0.139, 0.016]
1896	−0.222	−0.041	−0.172	0.043	0.159
	[−0.336, −0.107]	[−0.080, −0.002]	[−0.229, −0.114]	[0.026, 0.060]	[0.093, 0.225]
1900	0.406	0.249	−0.028	0.093	0.046
	[−0.600, 1.411]	[−0.383, 0.881]	[−0.119, 0.063]	[0.015, 0.170]	[−0.062, 0.153]
1902	0.177	−0.174	−0.004	0.073	0.070
	[−0.181, 0.536]	[−0.390, 0.041]		[0.017, 0.130]	[0.045, 0.095]
1907	−0.344	0.069		0.069	−0.034
	[−0.930, 0.242]	[−0.284, 0.422]		[0.062, 0.076]	[−0.079, 0.009]
Observations	451	448	363	447	450

Notes: Models estimated using the Callaway and Sant'Anna (2021) did R package. 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 and never treated states for all dependent variables, except model (3) which uses not-yet-treated units only.

Figure 9: **Average ATT Estimates: Total Spending and Categories**



Notes: Estimates and 95% confidence intervals of the average treatment effect on the treated (ATT) for suffrage restrictions, aggregating all cohort-specific effects. The outcomes of interest are total state expenditures, spending on common schools, pensions, prisons, and colleges. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method.

Identifying Assumptions and Robustness Checks

The key identifying assumption of our design is the absence of differential trends between states experiencing treatment at different points in time. Given the protracted and sometime uncertain process necessary to approve disenfranchisement measures, the year in which such policies were effectively implemented within each state should not be correlated with other variables such as population size or income level. To support this assumption, we first look at whether any observable characteristics of states consistently predict the timing of adoption of voting restrictions. We conduct a regression of indicators for each of the seven cohorts of treatment (1890, 1891, 1893, 1896, 1900, 1902, and 1907) on a set of characteristics measured in 1880. The results in Table A.1 show that no state characteristic systematically predicts treatment timing across cohorts. Using TWFE regressions, we also investigate whether time-varying factors might predict the year of adoption of suffrage restrictions. As the results in Table A.2 indicate, no state characteristic is consistently linked to the timing of treatment, both when considering each cohort separately (columns 1-7) and when looking at all cohorts together (column 8).

Readers might, however, still worry about the possibility of other factors influencing our outcomes of interest in ways that are correlated with the staggered introduction of voting restrictions across states. As a robustness test, we therefore use an alternative estimator proposed by de Chaisemartin and D'Haultfoeuille (2022), which is robust to treatment effect heterogeneity across groups and time periods, and allows for the inclusion of covariates. In these analyses, we control for population size, urban population, and state output level. The results remain consistent with the findings obtained from our primary specifications and, in some cases, display even stronger effects (see Figures A.14 through A.18). The event-study estimates confirm the absence of any significant effect of restrictions on total state spending, while pointing to a negative effect on state spending on both common schools and pensions. As before, estimates of the treatment effect for spending on prisons and colleges, are fairly strong and positive throughout the post-treatment period.

Next, we examine whether states in different cohorts display any significant differences in outcome levels prior to treatment. It is worth noting that testing for the absence of significant disparities in pre-treatment outcome levels across cohorts is a strong test, since the standard difference-in-differences assumption only requires that there are no counterfactual differences in the outcome trends. Table A.3 displays the average level of each spending variable by treatment timing (columns 1-7), and presents the results of unconditional F-tests for the difference in means across the seven cohorts (column 8). None of the outcomes display any significant differences. This suggests that the timing of treatment is not associated with the level of outcomes in the pre-treatment period.

Another potential concern regards the possibility of anticipatory behavior, which could occur if office holders had prior knowledge about the timing of implementation of voting restrictions. It is important to highlight that if such anticipatory behavior existed, it would likely lead to a downward bias in our results. This is due to the fact that we would be comparing treated states to states yet to be treated, which may have prematurely adjusted their outcomes in anticipation of future changes in voting laws. Hence, in a scenario where anticipatory effects are present, our estimates should be perceived as a lower-bound, potentially representing a conservative approximation of the true effects of suffrage restrictions.

One important question revolves around how school expenditures were distributed among different racial groups. Despite the absence of a clear divergence in the funding trends for common schools across states (Figure 2), it is possible that white students may have indirectly benefited from Black disenfranchisement due to a reallocation of funds from black schools to white schools (Margo 2007). Unfortunately, state-level spending specifically designated for white schools is unavailable in the state superintendent reports. Furthermore, no state provides comprehensive data on overall public spending for both white and Black schools before and after the adoption of suffrage restrictions. However, out of the fourteen states in our study, nine do offer annual data on the number of white teachers throughout this period. We use this data to examine whether there were divergent trends in the number of teachers in white schools among states that enforced disenfranchisement measures. This is a pertinent metric because teacher salaries constituted the majority of overall common school spending. Our analysis reveals that disenfranchisement did not result in an increase in the number of white teachers (see Appendix Figures A.19 and A.20), suggesting that what was being taken from

Blacks was not being given to low-income whites.

Lastly, readers might worry that suffrage restrictions were accompanied by broader constitutional changes, which could have affected state governments' finances in ways that go beyond the modification of voting regulations. Indeed, most states in our sample embedded these voting restrictions within newly established state constitutions. These constitutions often contained provisions regarding taxation and state resource allocation. To address any potential concerns that our findings might be influenced by additional fiscal measures implemented concurrently with voting restrictions, we thoroughly examine the specific provisions outlined in each state's constitution in the Appendix and perform additional robustness checks (see Appendix C).

Conclusion

In this paper, we evaluate the fiscal effects of the widespread disenfranchisement of African Americans across eleven Southern states between 1890 and 1907, using a unique annual data set of total state spending and seven distinct spending categories that include goods preferred by elites (universities and prisons), by lower-income whites (common schools and pensions), and by neither (courts, debt, and insane asylums). We do not find that disenfranchisement affected the trajectory of overall state expenditures; nor do we find that it had a meaningful effect on spending for basic essential services provided by state governments, namely courts, debt payments, and insane asylums. Instead, we find that franchise restrictions meaningfully shifted state expenditures away from redistributive goods favored by lower-income whites (common schools and pensions) towards those favored by economic elites (white universities and prisons).

Our findings contribute to a long literature exploring the fiscal consequences of political institutions both in the US and across countries. Most relevantly, our results are consistent with a series of papers showing that large expansions of the franchise in the US primarily affected the composition of public spending (e.g., Cascio and Washington 2014; Miller 2008). However, our study stands out as one of the first to demonstrate this phenomenon in the context of a large-scale democratic reversal.

The fiscal consequences of Black disenfranchisement for Southern Blacks are not examined in this paper. While some aspects of this subject have been previously explored (e.g., Margo 2007; Naidu 2012), a comprehensive account of the broader implications of franchise restrictions remains a critical area for future research. Yet, our findings do speak to ongoing debates regarding whether low-income whites gained economically from Black disenfranchisement. Previous research has demonstrated that in apartheid-era South Africa, a cross-class, white coalition was sustained through the implementation of high progressive taxes on elites and targeted redistributive spending for lower-income whites (Lieberman 2003). However, our findings for the U.S. reveal a different pattern. Rather than showing an increase in targeted redistribution, our results align with the prevailing view that the fiscal benefits of disenfranchisement were disproportionately directed toward Southern elites (Kousser 1974; Perman 2003; Woodward 1981).

Lastly, our results provide support for the fiscal-contract theory of the state (e.g., Bates 1983; Levi 1988; North and Weingast 1989; Timmons 2005), especially 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. 2023). Uncontested power did not lead Southern elites to shrink the state (and with it their tax burden), but instead to shift public spending in favor of their interests.

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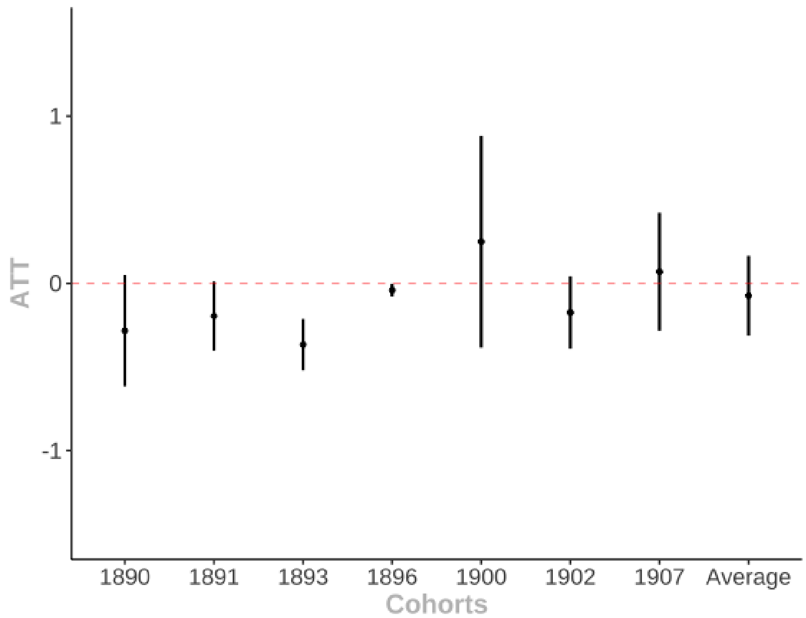
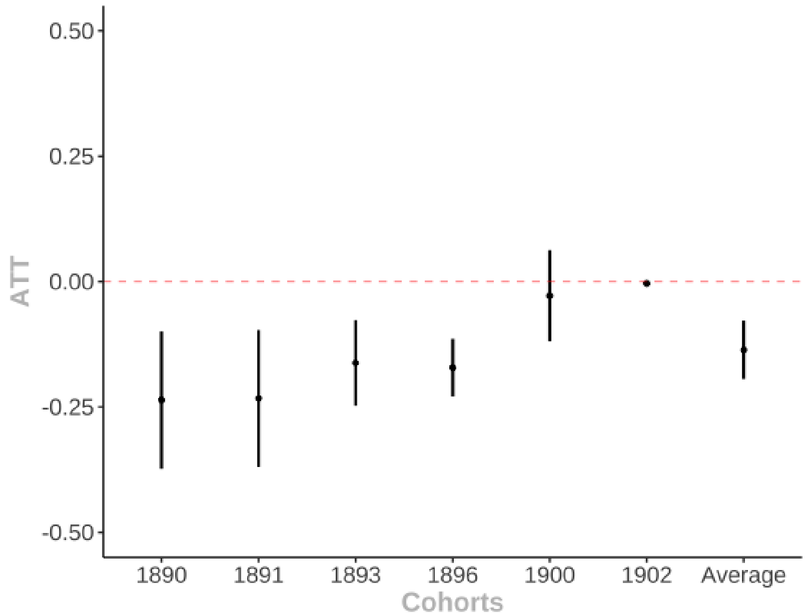
Appendix

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



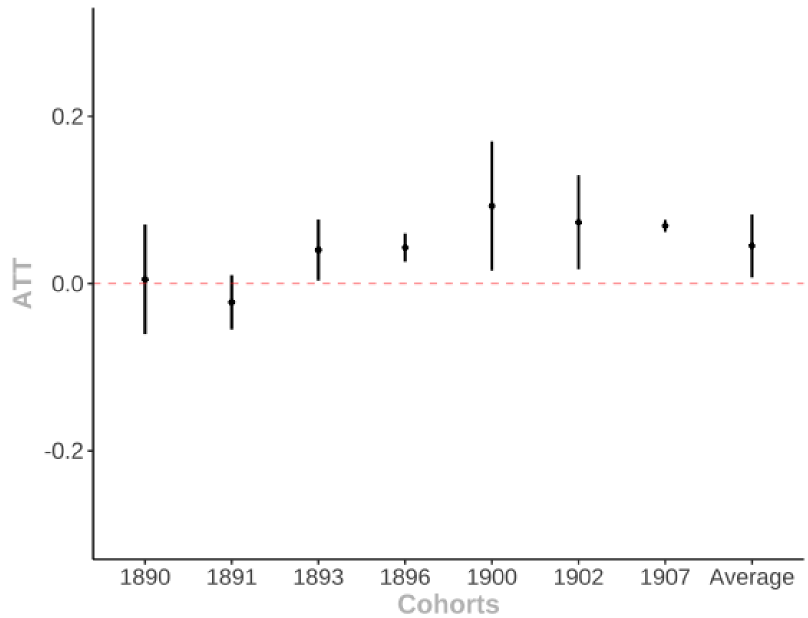
Notes: This figure shows the cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907), as well as the average of all cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample 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**



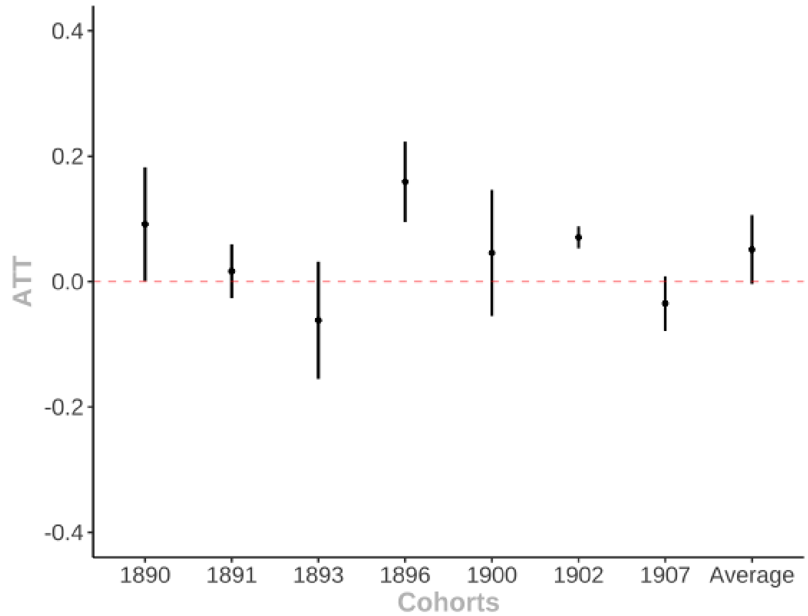
Notes: This figure shows the cohort-specific ATT estimates for each of the relevant adoption years (1890, 1891, 1893, 1896, 1900, and 1902), as well as the average of all cohort-specific effects. The sample includes 11 eventually-treated Southern states. The estimates for the last cohort (1907) are not 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**



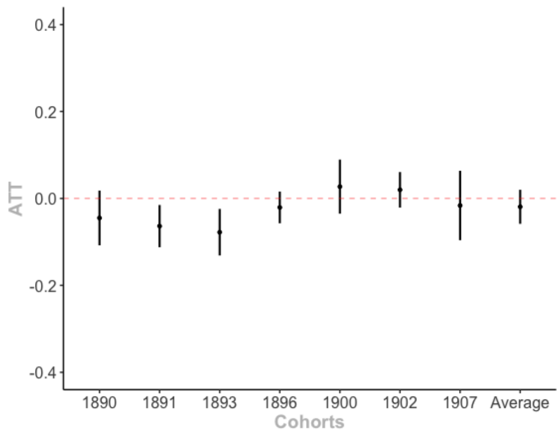
Notes: This figure shows the cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907), as well as the average of all cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample 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**

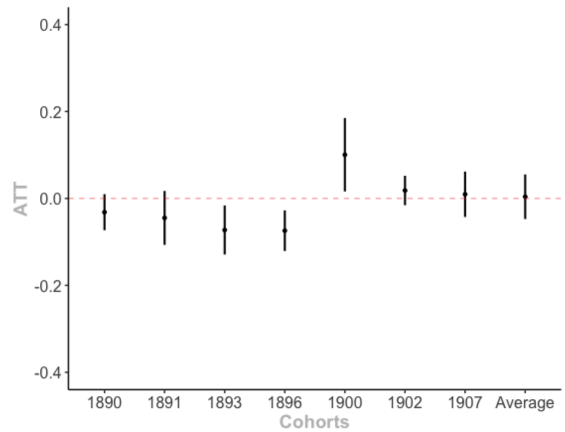


Notes: This figure shows the cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907), as well as the average of all cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample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

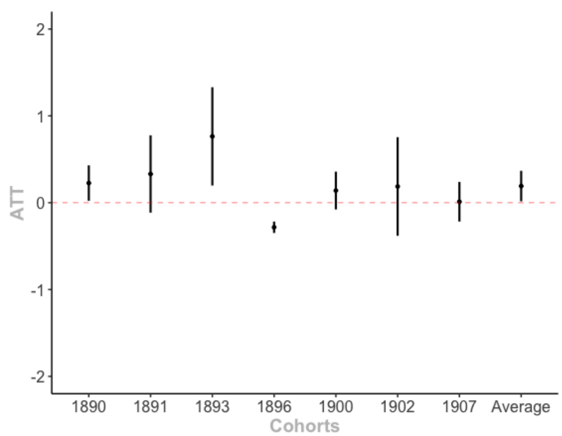
Figure A.5: **Overall Effect of Suffrage Restrictions on Non-Targeted Expenditures: Average and Cohort-Specific ATT Estimates**



(a) Spending on Courts



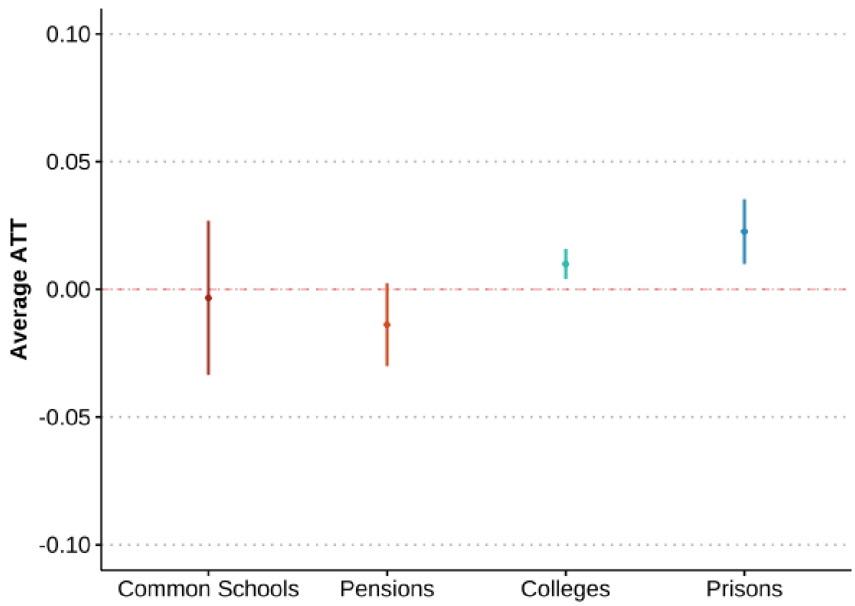
(b) Spending on Asylums



(c) Spending on Debt Interest Payments

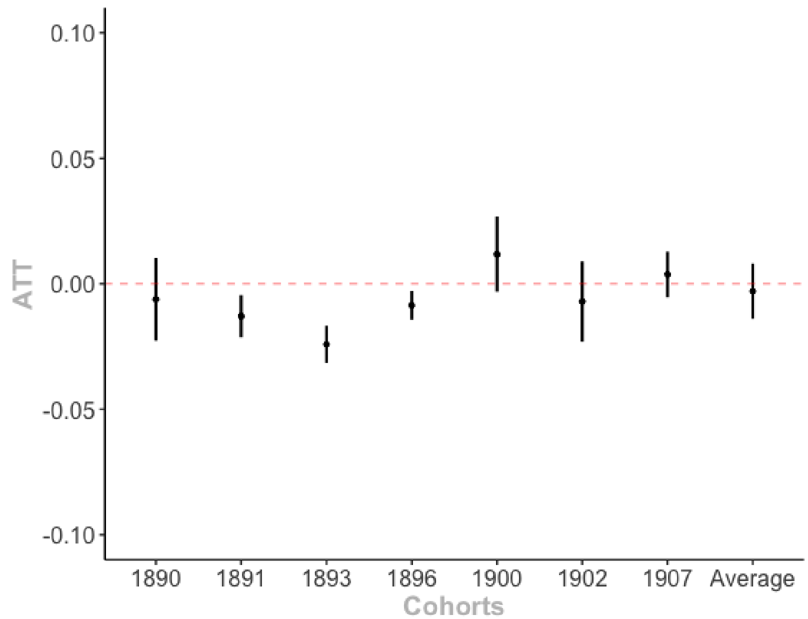
Notes: This figure shows the cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907), as well as the average of all cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample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

Figure A.6: **Average ATT Estimates: Share of Spending Allocated to Various Categories**



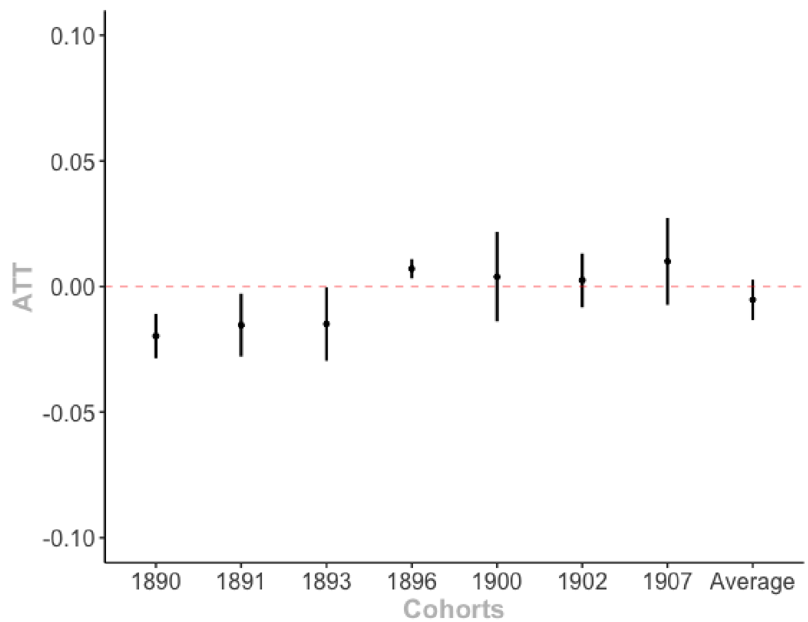
Notes: Estimates and 95% confidence intervals of the average treatment effect on the treated (ATT) for suffrage restrictions, aggregating all cohort-specific effects. Outcomes include the share of spending allocated to common schools, pensions, colleges, and prisons. All estimates are calculated using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

Figure A.7: **Overall Effect of Suffrage Restrictions on the Share of Spending Allocated to Insane Asylums**



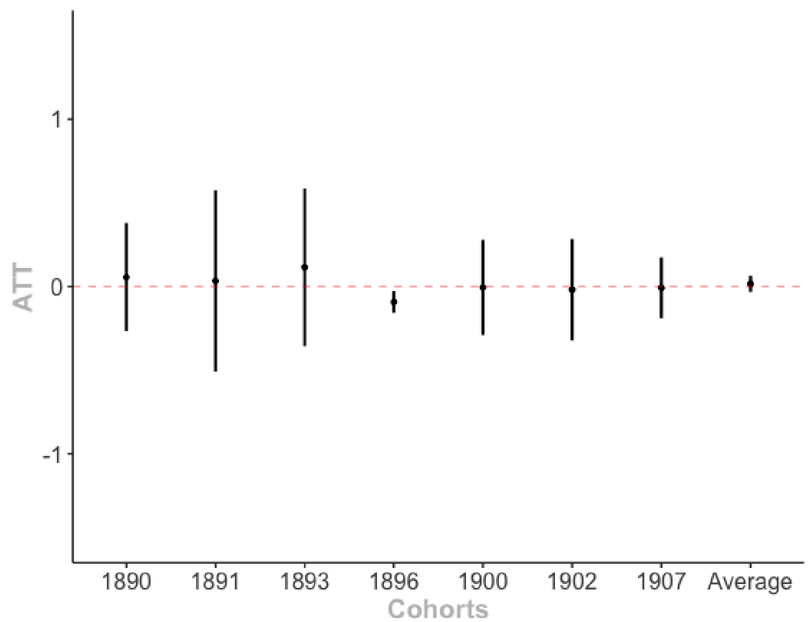
Notes: Cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907) and average of cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 14 Southern states, three of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

Figure A.8: **Overall Effect of Suffrage Restrictions on the Share of Spending Allocated to Courts**



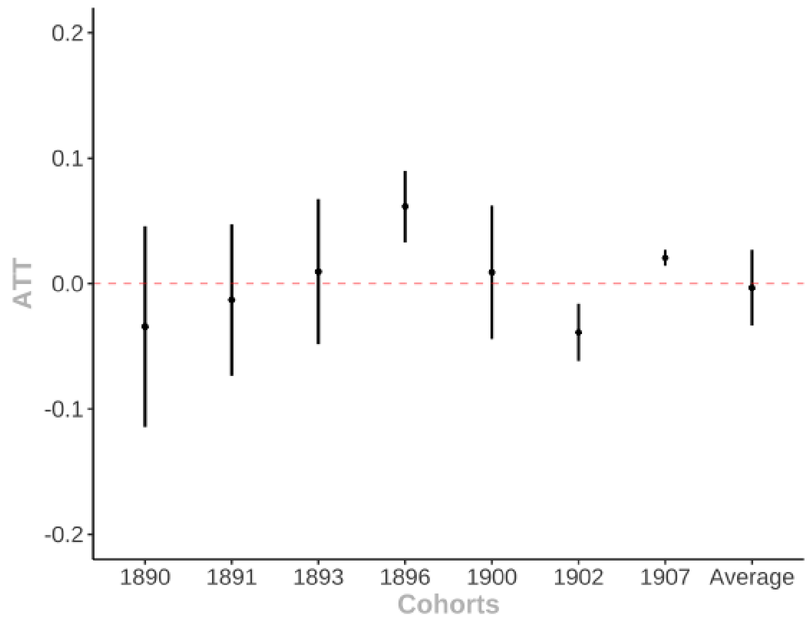
Notes: Cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907) and average of cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 14 Southern states, three of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

Figure A.9: **Overall Effect of Suffrage Restrictions on the Share of Spending Allocated to Debt Interest Payments**



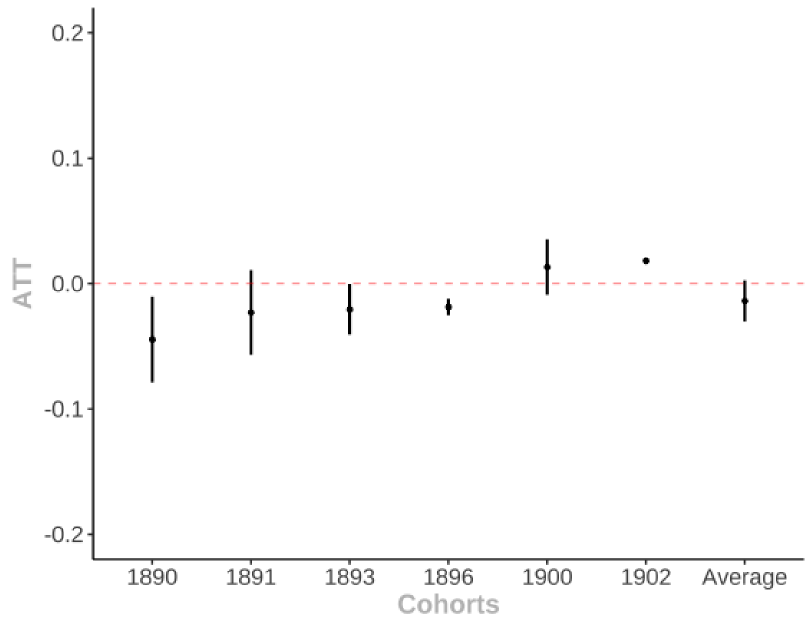
Notes: Cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907) and average of cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 14 Southern states, three of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

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



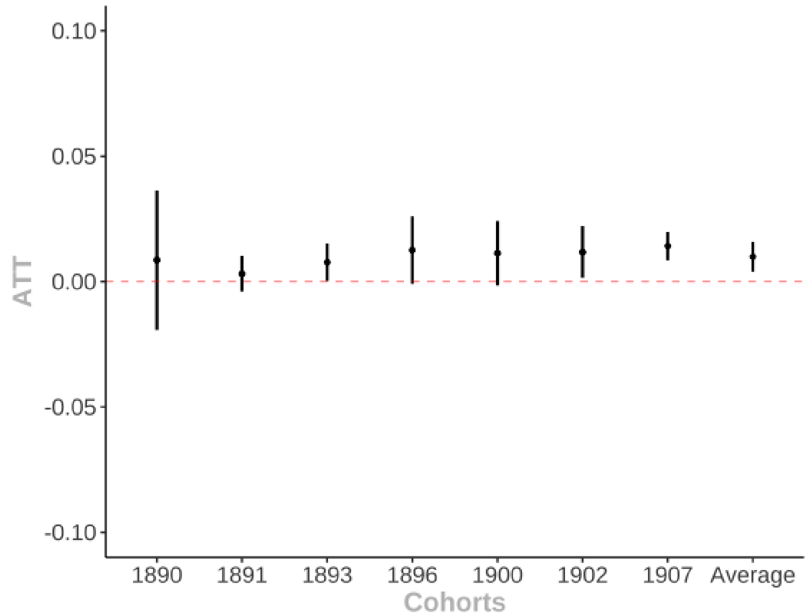
Notes: Cohort-specific ATT estimates for each of the adoption years (1890, 1891, 1893, 1896, 1900, 1902 and 1907) and average of all cohort-specific effects. All estimates are calculated using Callaway and Sant'Anna (2021) doubly robust estimation method. The sample includes 14 Southern states, three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

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



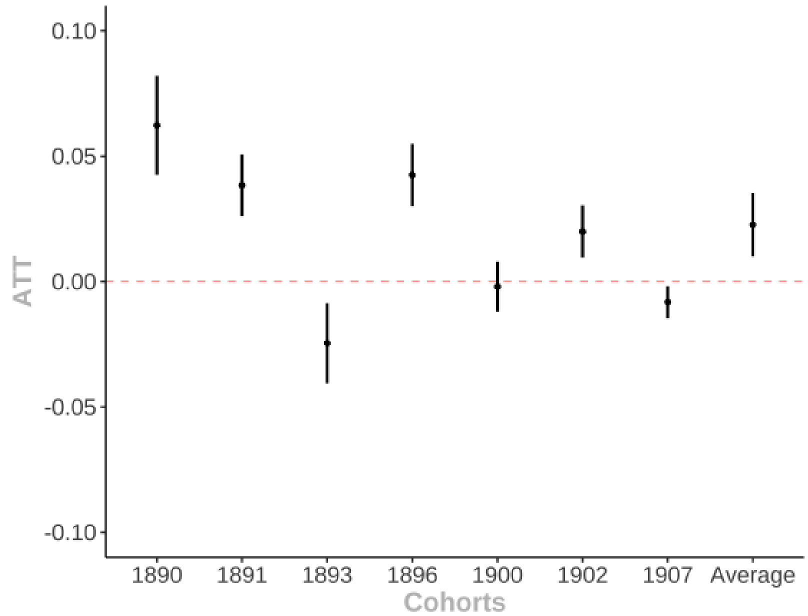
Notes: Cohort-specific ATT estimates for each of the relevant adoption years (1890, 1891, 1893, 1896, 1900, and 1902) and average of all cohort-specific effects. The sample includes 11 eventually-treated Southern states. The estimates for the last cohort (1907) are not available because there are no 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.12: **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.13: **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.

Table A.1: **Potential Time-Invariant Predictors of Treatment Timing**

	1890 Cohort	1891 Cohort	1893 Cohort	1896 Cohort	1900 Cohort	1902 Cohort	1907 Cohort
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Population size (log)	−2.749	−0.398	1.225	1.623	5.463*	−1.749	−2.272
	(3.036)	(1.839)	(1.997)	(1.680)	(2.495)	(2.065)	(2.048)
Output (log)	1.456	0.384	−0.413	−1.157	−3.373**	0.732	1.293
	(1.675)	(1.014)	(1.101)	(0.927)	(1.376)	(1.139)	(1.130)
White Population (%)	−0.816	0.897	−0.156	−0.226	2.440	−1.347	−1.645
	(1.697)	(1.028)	(1.116)	(0.939)	(1.394)	(1.154)	(1.144)
Urban Population (%)	−6.569	−1.303	2.970	4.802	10.263	−4.329	−5.509
	(7.064)	(4.279)	(4.646)	(3.909)	(5.805)	(4.805)	(4.765)
Observations	14	14	14	14	14	14	14
R^2	0.338	0.384	0.273	0.486	0.553	0.223	0.236

Notes: This table shows the results from seven separate OLS regressions where the dependent variables are indicators for the implementation of suffrage restrictions starting in 1890, 1891, 1893, 1896, 1900, 1902, and 1907. The explanatory variables are measured in 1880.

Table A.2: **Potential Time-Varying Predictors of Treatment Timing**

	1890 Cohort	1891 Cohort	1893 Cohort	1896 Cohort	1900 Cohort	1902 Cohort	1907 Cohort	All Cohorts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pop. size (log)	−0.542*	−0.048	0.115	0.151	−0.068	−0.204	0.299	−0.297
	(0.301)	(0.190)	(0.190)	(0.190)	(0.302)	(0.189)	(0.189)	(0.610)
Output (log)	−0.082	−0.034	−0.015	0.059	0.026	0.010	0.084*	0.049
	(0.074)	(0.046)	(0.047)	(0.046)	(0.074)	(0.046)	(0.046)	(0.150)
White Pop. (%)	0.559*	0.125	−0.088	−0.193	0.056	0.160	−0.363*	0.257
	(0.316)	(0.199)	(0.199)	(0.199)	(0.317)	(0.199)	(0.198)	(0.640)
Urban Pop. (%)	0.042	−0.008	−0.005	−0.029	0.009	−0.001	−0.032	−0.024
	(0.044)	(0.028)	(0.028)	(0.028)	(0.044)	(0.028)	(0.028)	(0.089)
Observations	434	434	434	434	434	434	434	434
R^2	0.009	0.004	0.002	0.005	0.002	0.006	0.010	0.002

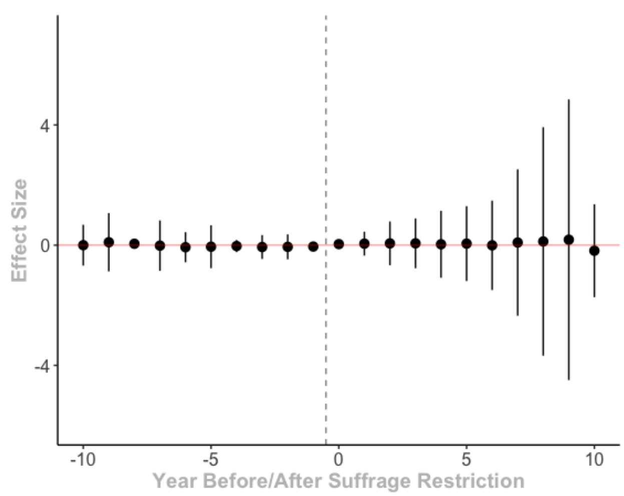
Notes: This table shows the results from eight separate TWFE regressions where the dependent variables are indicators for the adoption of suffrage restrictions in 1890, 1891, 1893, 1896, 1900, 1902, 1907, and all years (column 8). The explanatory variables are obtained from the census every 10 years and are interpolated for intercensal years.

Table A.3: **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

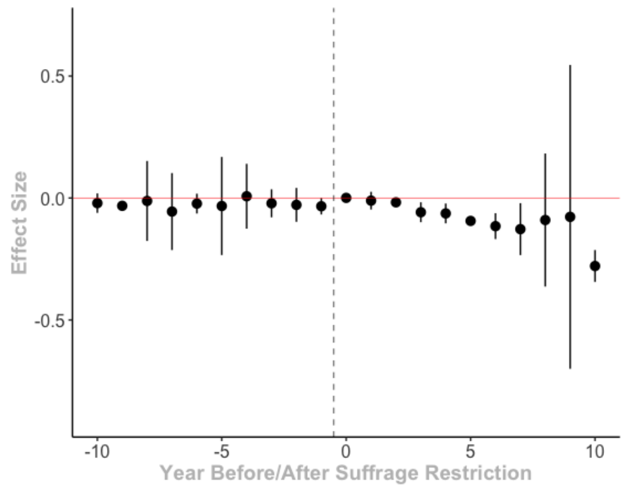
Notes: The table presents mean outcome levels for states that implemented suffrage restrictions in 1890, 1891, 1893, 1896, 1900, 1902, and 1907. All variables are measured in 1885. Column 8 displays the p-value of unconditional F-Tests, which assess the equality of means across all seven groups of states.

Figure A.14: **Total State Spending: Event Study with Controls**



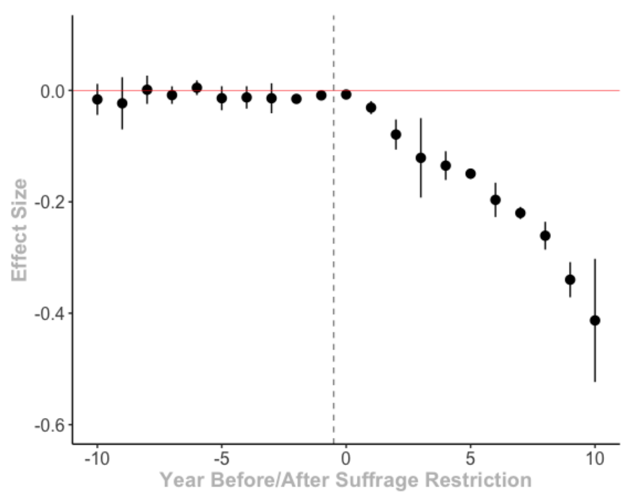
Notes: The figure shows the coefficients and confidence intervals of the DID$_m$ estimators (de Chaisemartin and D'Haultfoeuille 2022). Standard errors are clustered at the state level. All regressions include the following controls: population size, urban population, and state output.

Figure A.15: **Spending on Common Schools: Event Study with Controls**



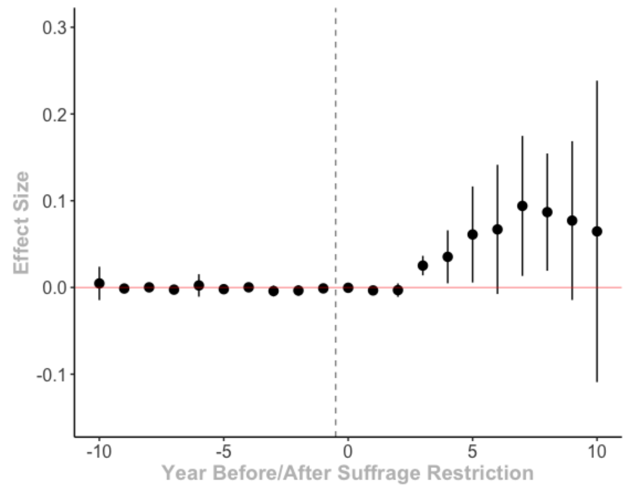
Notes: The figure shows the coefficients and confidence intervals of the DID_m estimators (de Chaisemartin and D'Haultfoeuille 2022). Standard errors are clustered at the state level. All regressions include the following controls: population size, urban population, and state output.

Figure A.16: **Spending on Pensions: Event Study with Controls**



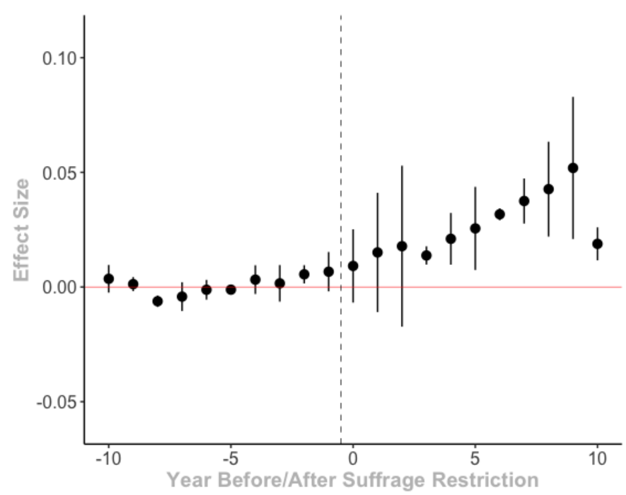
Notes: The figure shows the coefficients and confidence intervals of the DID_m estimators (de Chaisemartin and D'Haultfoeuille 2022). Standard errors are clustered at the state level. All regressions include the following controls: population size, urban population, and state output.

Figure A.17: **Spending on Prisons: Event Study with Controls**



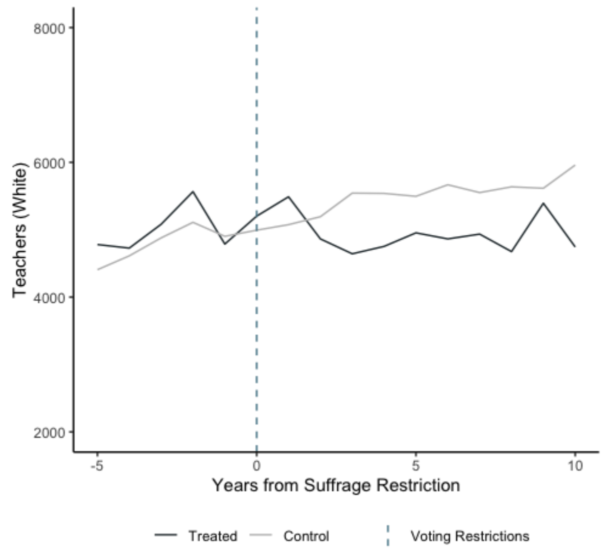
Notes: The figure shows the coefficients and confidence intervals of the DID_m estimators (de Chaisemartin and D'Haultfoeuille 2022). Standard errors are clustered at the state level. All regressions include the following controls: population size, urban population, and state output.

Figure A.18: **Spending on Colleges: Event Study with Controls**



Notes: The figure shows the coefficients and confidence intervals of the DID_m estimators (de Chaisemartin and D'Haultfoeuille 2022). Standard errors are clustered at the state level. All regressions include the following controls: population size, urban population, and state output.

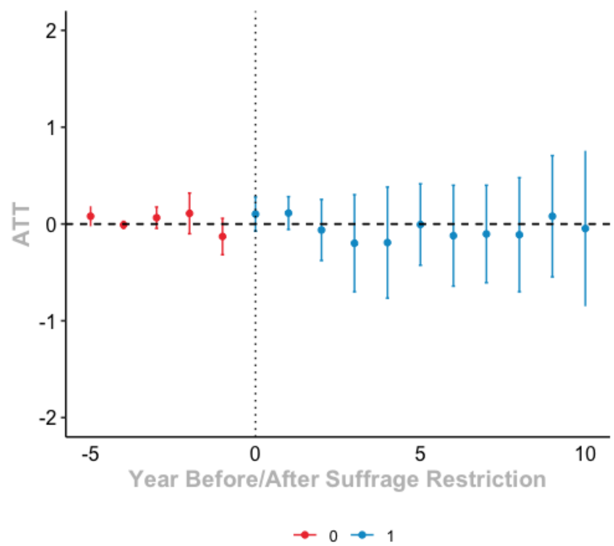
Figure A.19: **Number of White Teachers in States with and without Voting Restrictions by Time since Adoption**



(a) Number of White Teachers

Notes: The black line show average number of white teachers across states that adopted franchise restrictions. The gray line reflects the average trend in the comparison group which includes both never-treated and not-yet-treated units. For each state where voting restrictions were implemented in a given year, we calculate the average number of white teachers among all states that had no restrictions in place either in that particular year or in the preceding years.

Figure A.20: **Number of White Teachers: Average Effect by Length of Exposure to Suffrage Restrictions**



(a) Number of White Teachers (log)

Notes: Event time estimates and 95% confidence intervals for the number of teachers in white schools. 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.

Appendix B: Data Construction Details

In this appendix, we provide information on the construction and sources for each variable in this book. This information on data construction can be found in Tables B1. The sources used to construct each observation are listed below in Table B2.

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)[1], supporting our argument that political elites were likely to modify spending

[1] 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 institution, of such additional scientific or literary departments as the public necessities and the well-

allocations after increasing their political power. The 1898 constitution also placed constraints on 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

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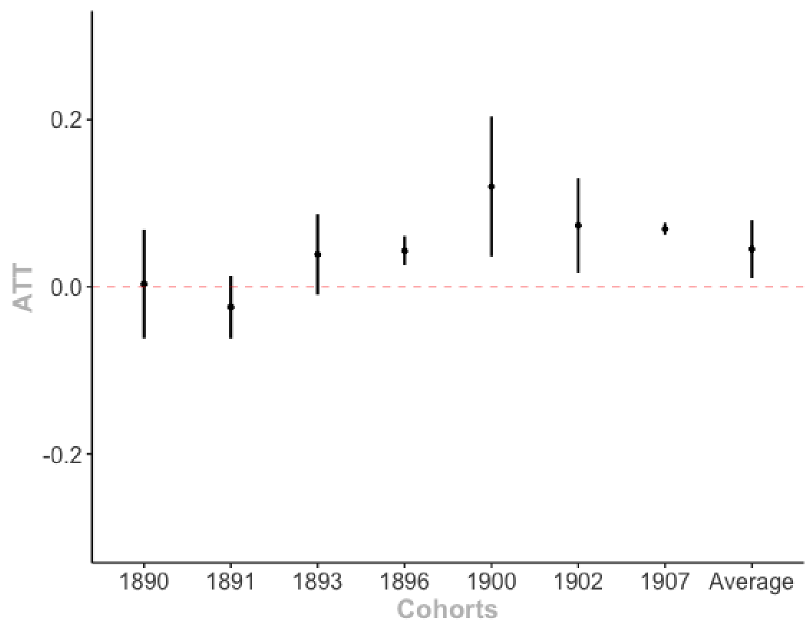
being of the people of Louisiana may require."

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.