

Democratic Reversals and the Size of Government

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Abstract

While the fiscal and redistributive consequences of democracy is one of the central debates in political economy, most empirical studies analyze this question solely in the context of transitions to democracy. In this paper, we explore the consequences to taxation of democratic reversal using the systematic disenfranchisement of African Americans in the US South between 1880 and 1910. Following the federally-imposed extension of the franchise to the former slaves during Reconstruction (1865-1877), Southern states erected a series of legal restrictions, such as literacy tests and poll taxes, aimed primarily at preventing Southern African Americans from registering to vote. Using an original dataset of local and state taxes and a difference-in-differences estimation strategy, we demonstrate that the adoption of literacy tests for voting eligibility in each state was followed by a significant decline in tax revenues that is highly correlated to the share of each county's population who was African American. We also find that black disenfranchisement led to a shift of the tax burden onto urban counties and a greater reliance on indirect taxation. Our results survive a battery of robustness checks, alternative specifications and additional tests of the redistributionist thesis. The findings are not only consistent with standard models of redistribution following democratization, but also indicate that the elasticity of taxes with respect to enfranchisement is substantial and larger than the one suggested by the cross-national literature.

Highlights:

- Explore the fiscal consequences of democratic reversals using sub-national tax data following disenfranchisement of U.S. Southern African Americans.
- Context gives us precise measures of change in democracy, which corresponds to black population share of counties
- Higher black population share counties see significant declines in taxation following black disenfranchisement.
- Declines in taxation are larger than found in cross-national literature

Key words: Democracy, Political development, Redistribution, Inequality

Classification Codes: P16, O10

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1 Introduction

While many of the foundational theories in political economy predict that democratization should lead to a rise in redistributive taxation (e.g., Meltzer and Richard 1981, Acemoglu and Robinson 2000, Boix 2003), the redistributive and fiscal consequences of democracy remains unresolved.¹ The vast empirical literature investigating this relationship, however, is based almost exclusively in contexts of democratization. Yet what are the consequences of democratic reversals? Does a reversal lead to fiscal consolidation, lower taxation and less redistribution?

In this paper, we contribute to this broader debate in political economy by investigating the effects on taxation from the adoption of suffrage restrictions that largely disenfranchised millions of African Americans in the late 19th-Century U.S. South. The Confederacy’s defeat in the U.S. Civil War (1861-1865) resulted in not only the emancipation of millions of slaves, but also in the imposed extension of the franchise to this group, who comprised slightly less than half of the population of the former Confederate states.² Southern elites, through the Democratic Party, gradually regained political control of each state and over time erected suffrage restrictions, most notorious of which was the literacy test, aimed at stripping the franchise from African Americans. Between 1889 and 1908, every former Confederate state enacted these restrictions, largely removing the *de jure* political rights of African Americans.

We leverage a few key features of this episode to empirically examine the impact of black disenfranchisement on the size of Southern state and local governments. For one, the period of our study, 1880 to 1910, is an era of large-scale state and local fiscal expansion across US states (Wallis 2000). By focusing on democratic reversal in a period of fiscal expansion, we are much less likely to be capturing a spurious correlation between democratic reversal and declines in taxation. Second, due to slavery and the lack of meaningful redistribution of economic assets following emancipation, African Americans were significantly poorer on average than Southern whites (e.g., Ransom and Sutch 2001). Hence, this group’s systematic disenfranchisement should cause a dramatic shift of the median (eligible) voter up the income distribution. Yet the heterogeneous spatial distribution of the post-Civil War African-American population meant that the magnitude of this negative shock varied enormously. In some counties, the effective electorate was cut by more than half, while in heavily white counties it was largely unchanged.³ Therefore, the electoral consequences of this reversal should be largely proportionate to the share of a locality’s population who were

¹While some scholars have found that expansions in the franchise increased taxation and social spending (e.g., Acemoglu et al. 2014, Lindert 2004), others have found a non-linear relationship (e.g., Aidt and Jensen 2013, Aidt, Daunton and Dutta 2010), or no meaningful relationship (e.g., Profeta, Puglisi and Scabrosetti 2013). See, for instance, Bonica et al. (2013), Acemoglu et al. (2014) and Scheve and Stasavage (2017), for recent surveys of this voluminous theoretical and empirical literature.

²See Table 1 for more information on these states.

³See Appendix-Figure 10 for a map showing the magnitude of the spatial variation in black population share across counties of the South in this period.

African American. Unlike the cross-national literature which typically relies on categorical indices to measure regime type, the racialized nature of Southern disenfranchisement allows us to precisely measure the local effect of these restrictions on the magnitude of the “reversal”.

Our empirical strategy therefore exploits both variation across states in the timing of the restrictions’ enactment and the highly uneven spatial distribution of African Americans across counties to identify the effects of this large-scale shrinking of the electorate on local taxation. Specifically, we construct a county-level panel of real per capita taxes between 1880 and 1910. We then estimate a difference-in-differences model with heterogeneous treatments in which we leverage the differential timing across states in the adoption of these disenfranchising restrictions - especially literacy tests - and spatial variation in black population share to measure the fiscal consequences of disenfranchisement.

In our baseline models, which include county fixed effects and state-specific time trends, we find strong evidence that the presence of a state-imposed literacy test interacted with a county’s black-population share prior to disenfranchisement (i.e., in 1880) is negatively correlated with a county’s total per capita state and local taxation. Specifically, once a literacy test requirement was introduced, an increase in 1880 county black population share by one standard deviation was associated with a nearly 5.4% decrease in real per capita total tax receipts. While this finding is strongly consistent with the “*redistributionist*” hypothesis, we also find that in low black-population share urban counties, the onset of the literacy-test restriction is followed by an *increase* in overall per capita taxation. This increase, which we show is due in part to a post-restriction shift in the burden of state taxation onto the urban counties within these states, reveals additional consequences of large-scale changes in the size of the electorate.

Next, we test other implications of the redistributionist thesis of democratization. For one, declining tax revenues may not necessarily result in less redistribution if reductions in public spending primarily occurs on non-redistributive goods. We test for this possibility by examining public spending on education, especially public inputs into education spending on African Americans. We show that the adoption of the literacy test interacted with county black-population share is highly correlated with falling public spending on black educational inputs. We also test whether the adoption of the literacy test affected the composition of taxation. [Aidt and Jensen \(2009\)](#), for instance, argue that enfranchisement should not only lead to more taxation, but also a shift of the burden taxation away from indirect taxes to direct taxes borne primarily by the wealthy. We show that the adoption of literacy tests coincided with a shift in state taxes away from direct taxation, such as *ad valorem* property taxes, towards indirect taxation, such as excise taxes on alcohol and professional licensing fees.

We conduct a series of robustness tests to verify our claim that the decline in tax revenues is working through the channel of African-American disenfranchisement. First, to reduce the likelihood of bias induced

by measurement error, we test whether this finding is robust to using other types of suffrage restrictions, such as poll taxes, as well as other measures of the extent of local disenfranchisement. We find that the adoption of other voting restrictions is also strongly correlated with declining tax revenues.

A second set of robustness tests address concerns that our findings are biased by omitted factors. While the reversal of black political rights was facilitated by changes in the federal-level partisan environment (which we provide more detail about below), the timing of the adoption of suffrage restrictions by each state is endogenous. We therefore conduct numerous additional robustness checks to assuage these concerns. For one, we estimate a long-differences model taking changes in taxation instead of levels over the entire period of our panel.⁴ We again find that county black-population share is highly negatively correlated with the change in county-level taxation over 20- and 30-year periods. In order to test whether we are simply capturing anti-African-American (i.e., racist) preferences, we show that counties voting against the referenda calling for the adoption of disenfranchising literacy tests also experienced a reduction in taxation that is proportional to its black-population share. Finally, we perform a dynamic panel model that tests whether a county-specific trend (that also varies with black share) is correlated with taxation. We show that such a trend fails to capture the underlying relationship between state-imposed voting restrictions, county black-population share and per capita taxation.

A third set of robustness tests concerns the comparability of counties in literacy-test states with those who did not enact suffrage restrictions. To address this concern of uncommon covariate support between counties in treatment and non-treatment states, we match observations on covariates in 1910 using a genetic matching estimator (Diamond and Sekhon 2013). We also estimate the model on various samples, including only the counties in the 7 states that adopted the literacy test requirement. Across each test, we find a consistently negative relationship between black disenfranchisement and taxation.

Our findings have important implications for the core redistributionist theories of democracy (e.g., Boix 2003, Meltzer and Richard 1981, Acemoglu and Robinson 2000). If democratization leads to a shift of the median voter down the income distribution, then widespread *disenfranchisement* of the poorest members of society should lead to a new median voter who is much higher up the income distribution. As a result, the demand for redistributive taxation from the post-reversal median voter should decline and lead to lower taxation. Our findings strongly support this implication of the canonical redistributionist view of democracy, which has been increasingly challenged on both theoretical and empirical grounds (e.g., Ansell and Samuels 2014, Lizzeri and Persico 2004; Congleton 2007; Scheve and Stasavage 2010). Yet our data allows us to go farther and explore the mechanisms by which this happens. We find that not only does disenfranchisement lead to less redistributive taxation, but that also a shift occurs in the burden of non-local taxation onto the

⁴See Cascio and Washington (2013) for a similar estimation strategy.

South’s few urban areas. We demonstrate several channels by which this shift occurs.

Our paper joins a growing empirical literature using subnational data, especially from the US, to investigate this and other unresolved questions in political economy (Aggeborn 2016; Cascio and Washington 2013; Go and Lindert 2010; Ramcharan 2010; Nunn 2008; Nikolova and Nikolova 2017; Husted and Kenny 1997; Aidt, Daunton and Dutta 2010). Our ability to investigate the fiscal impact of large-scale disenfranchisement using sub-national units - in this case, more than a thousand counties across the South - is one of the few studies to use highly disaggregated local-level data to study this question.⁵ Furthermore, whether using cross-national (Acemoglu et al. 2014; Aidt and Eterovic 2011), cross-state (Husted and Kenny 1997) or across municipalities (e.g., Aidt, Daunton and Dutta 2010), most if not all study the fiscal consequences of large-scale suffrage *expansions*.⁶ Our evidence complements the findings of Naidu (2012), who shows that the adoption of suffrage restrictions in the South lead to declines in African-American voter turnout and public education inputs relative to whites, as well as rising values of economic assets owned by whites. Our results therefore provide additional evidence to the literature investigating the welfare consequences to African Americans from the Southern policies of systematic civil, economic and political inequality.

This paper proceeds as follows. We first provide some historical context in order to demonstrate the applicability of this case for exploring this key unresolved puzzle in political economy. We then provide a theoretical framework connecting this case with the political economy literature on democratization. We conclude by presenting our evidence that systematic disenfranchisement of African-Americans lead to substantial declines in the size of the southern fiscal state, thus providing empirical support for the redistributionist hypothesis.

2 Southern Democratization and Reversal

Following the U.S. Civil War, the victorious Northern states sought to fundamentally transform the structure of Southern politics. In addition to emancipating the slaves, Congress imposed the extension of the franchise to all adult males with the various Military Reconstruction Acts of 1867 and eventually the 15th Amendment in 1870.⁷ These acts, along with others such as the Enforcement Acts of 1870 and 1871, empowered the federal government to use the military and the judiciary to enforce the newly granted political and civil

⁵See Aidt, Daunton and Dutta (2010) and Chapman (2016) for studies investigating changes in municipal expenditures by British local governments in the 19th Century following large-scale extensions of the franchise.

⁶Many studies have tried to empirically leverage key federal interventions that expanded suffrage, such as the 19th Amendment’s extension of suffrage to women (e.g., Lott and Kenny 1999; Miller 2008) or the Voting Rights Act of 1965, which removed the legal restrictions African Americans faced in voting (e.g., Cascio and Washington 2013, Husted and Kenny 1997). Other studies have investigated the consequences of earlier expansions, although these were not a consequence of federal interventions (Go and Lindert 2010; Nikolova and Nikolova 2017).

⁷In particular, the Military Reconstruction Act placed 10 of the 11 Confederate states in military districts, and required political rights for blacks to be adopted as one of the conditions for restoration of representation in Congress. See Table 1 for more information about these states, which are hereafter called “Reconstruction states”.

rights of the former slaves. These federally imposed reforms radically altered Southern politics and society (Foner 2011). For instance, as reported in Table 1, African Americans in 1868 comprised more than half the registered voters in five states, and more than 40% in four more of the 10 Reconstruction states (Walton, Puckett and Deskins 2012, 247). As such, the previously non-existent Southern wing of the Republican Party, whose supporters consisted overwhelmingly of the former slaves, was highly competitive during the period of military Reconstruction (1863-1877).⁸ Furthermore, thousands of African Americans across the South were elected to local, state and federal office (Foner 2011).

Reconstruction faltered with the decline in the federal-level electoral fortunes of the Republican Party. This culminated in the 1874 wave election, which returned the Democrats to the majority of the House of Representatives for the first time since before the Civil War. In their strident opposition to Reconstruction, this capture of one chamber of Congress allowed them to block any additional federal military appropriations for the occupation of the South. Soon after, the recapture of each southern legislature and governorship by the Democratic Party, which Southerners called “Redemption”, was completed.

While the removal of federal troops severely limited the enforcement of black rights, African Americans retained the franchise and in practice remained politically active in the immediate post-Reconstruction period. Although its occurrence was substantially reduced, black politicians continued to be elected to local, state, and federal office. Opposition parties routinely won more than a third of the legislative seats, and occasionally challenged Democratic dominance by even winning a few state chambers and governorships.⁹ Moreover, black voters remained a threat to Southern Democrats since these could join populists and poor whites to form “fusion” tickets (Perman 2003).¹⁰ As a consequence, the Democratic Party’s maintenance of power was not assured and required violence, electoral fraud, and patronage to African Americans.¹¹ Yet, even if weakened, the Enforcement Acts remained in place, and the federal government occasionally pursued convictions for violations of black rights during the 1880s (Walton, Puckett and Deskins 2012). Between 1880 and 1900, Congress also overturned 26 congressional elections in which Republicans or Populists were defeated through electoral fraud (Kousser 1974: 263). Southern elites also feared that conspicuous uses of violence and fraud would invite further federal intervention, especially if the political environment swung towards the Republican Party.

⁸Between 1868 and 1877, a majority of both the state legislatures and governors in the 10 Reconstruction states were Republicans (Dubin 2007, 2010).

⁹Between the late 1870s and the late 1890s, opposition parties obtained on average 10 to 20% of the state legislative seats and received between 20 and 35% the gubernatorial votes. Five states had a legislative chamber which had at least one-third of its members were not from the Democratic Party (AL, FL, LA, NC, VA). Only SC did not have a gubernatorial race in which a non-Democratic candidate received at least 40% of the vote. See Dubin 2007; 2010.

¹⁰Fusion tickets between black and white voters won gubernatorial races in Virginia (1882) and North Carolina (1896) and nearly won races in others, such as Alabama (1892, 1894), Arkansas (1890) and Georgia (1894).

¹¹For instance, extra-legal lynchings peaked in the decades following Reconstruction, and declined substantially after their systematic disenfranchisement. (Kousser 1974; Tolnay and Beck 1995).

In what is often called the Era of Disenfranchisement (1889-1908), Southern elites sought to “ensure the subordination of African Americans and the dominance of the political and economic elite of the Democratic Party” by erecting *de jure* voting restrictions (Perman 2003). Early efforts to disenfranchise African Americans primarily entailed enacting poll taxes by legislative statute.¹² It has been estimated that these early statutory restrictions reduced black turnout by half (Kousser 1974, 67-68). The restrictions enacted under Mississippi’s Constitution of 1890, which included the literacy test, set the template for the complete disenfranchisement that would eventually occur in much of the South.¹³

These ambitions were initially checked, however, as this violation of the 15th Amendment could invite further federal-level interventions into Southern politics. Indeed, upon gaining control of Congress and the Presidency for the first time since 1874, House Republicans, with the support of President Benjamin Harrison, passed the Lodge Bill of 1890, which would have authorized federal supervision and regulation of Congressional elections (i.e., enforcement of African-Americans’ right to vote in federal elections). Yet significant changes in the federal-level political context during the 1890s caused these concerns to abate. This Lodge Bill’s defeat in the Senate and the following wave election in 1890 that swept Democrats into control of the House of Representatives signaled the end of Federal engagement in Southern electoral politics. Following the 1892 federal elections, which delivered Democratic control of Congress and the Presidency for the first time since before the Civil War, most of the primary provisions of the Enforcement Acts were repealed. Finally, the US Supreme Court ruled in *Williams vs. Mississippi* (170 U.S. 213, 1898) that the state’s various disenfranchisement clauses in its constitution, despite being applied overwhelmingly towards only African Americans, did not violate the Equal Protection Clause of the 14th Amendment.

With the removal of the remaining federal-level obstacles, each Reconstruction state, as well as a few other former slave states, erected constitutional or statutory suffrage restrictions between 1895 and 1908. While existing records indicating registered voters by race are limited, the available data demonstrates the devastating and immediate effect that literacy tests had on African-American *de jure* political rights. In 1896, two years prior to the adoption of Louisiana’s 1898 Constitution which enacted the literacy test, slightly less than half of the state’s registered voters were African American (which was approximately equivalent to their population share). By 1900, this share had fallen to below 5 percent - a more than 90% decrease (Walton, Puckett, and Deskins 2012: 342). By 1910, only 0.5% of the state’s African-American adult males remained registered, which comprised less than 1% of the state’s registered voters.

Table 1 demonstrates the arc of this large-scale suffrage expansion and its subsequent reversal in each of

¹²Georgia implemented a cumulative poll tax in 1877 (followed by both Florida and Tennessee in 1889).

¹³Because the 15th Amendment (1870) prohibited suffrage discrimination based on race, Southern states devised other restrictions, such as literacy requirements to vote. Literacy tests were administered in order to remove an otherwise eligible voter who would not qualify based on the state’s literacy requirement. In practice, these were used to disenfranchise African Americans (Kousser 1974).

the Southern states.¹⁴ Column 2 reports the share of the state’s registered voters who were African American in the 10 Reconstruction states.¹⁵ In addition to reporting the share of each state’s population who were black, Table 1 also includes the black population share in the 25th- and 75th-percentile county in each state in 1870. This highlights the within-state spatial variation in the consequences of these political shocks, which is a key to our empirical strategy. Lastly, the year and type of suffrage restriction adopted, if any, across the Southern states is also shown. While 7 of 10 Reconstruction states adopted the stringent literacy test, all ten states adopted some restriction targeting African-American voters.

Table 1
Democratization and Reversal in the South, 1868-1910

	Slave Sh. of Population 1860 (%)	Black Sh. of Registered Voters, 1868 (%)	Black Sh. of Population, 1870(%)	1870 Black Share of lowest quart- tile county (%)	highest quart- tile county (%)	Type of Suffrage Restriction	Enactment
Panel A: Reconstruction States							
Alabama	45	63	48	10	70	LT, PT, etc.	Constitution (1901)
Arkansas	26	35	25	2	53	PT	Statute (1892)
Florida	44	58	49	10	63	PT	Statute (1889)
Georgia	44	50	46	12	67	LT, PT, etc.	Constitution (1877)*
Louisiana	47	65	50	32	77	LT, PT, etc.	Constitution (1898)
Mississippi	55	56	54	22	75	LT, PT, etc.	Constitution (1890)
N. Carolina	33	41	37	10	55	LT, PT, etc.	Constitution (1900)
S. Carolina	57	63	59	32	72	LT, PT, etc.	Constitution (1895)
Texas	30	46	31	3	49	PT	Statute (1902)
Virginia	31	47	42	12	64	LT, PT, etc.	Constitution (1901)
Panel B: Other pre-war slave states							
Delaware	2		18	16	24	PT	Constitution (1831)
Kansas			5	0	8		
Kentucky	19		17	2	32		
Maryland	13		22	11	51		
Missouri	10		7	0	13		
Tennessee	25	40	26	5	38	PT	Statute (1889)
W. Virginia			4	0	9		

Note: LT = Literacy Test. PT = Poll Tax. Constitution = Constitutional Convention or Amendment. Sources: Kousser (1974); Valelly (2009), Walton, Puckett and Deskins (2012, Table 13.9); US Census (1860 and 1870). * Georgia enacted the literacy test in 1908.

These restrictions have been shown to not only have had significant consequences for eligibility and turnout of African Americans, but also on the electoral fortunes of the Republican Party (Kousser 1974; Naidu 2012).¹⁶ Only after the adoption of *de jure* disenfranchisement was the Democratic Party able to establish unchallenged supremacy (i.e., the “One-Party South”). Between 1900 and the passage of the

¹⁴We define a Southern state as one in which slavery was legal in 1860. Note that Kansas was a territory in 1860 and entered the Union as a free state in 1861. While slavery was legal in the area that became West Virginia in 1860, it seceded from Virginia during the Civil War, and was admitted as a state in 1863.

¹⁵The Military Reconstruction Acts of 1867 required the military to register voters in these 10 states. As such, we know the racial breakdown of registered voters in each of the states. This data is unavailable for the other former slave states.

¹⁶Contra Kousser (1974) and Naidu (2012), Bertocchi and Dimico (2017) claim that African-American turnout and registration declined in Mississippi prior to the adoption of the “disenfranchising” constitution in 1890. This evidence is line with a prior literature that argued in the words of Key (1984) that “formal disenfranchisement measures did not (cause) the decimation of the Southern electorate. They, rather, recorded a fait accompli brought about by more fundamental political processes.”

Voting Rights Act in 1965, the Democratic Party won every chamber of the legislatures of the ten former Reconstruction states, and very frequently with complete unanimity (Dubin 2007). The share of African-Americans who were registered to vote remained at less than 10% for more than 50 years (Keyssar 2001).

3 Theoretical Implications of Southern Democratization - and its Reversal - on Taxation

Following Meltzer and Richard (1981) and Acemoglu et al. (2014), we conceive of a non-democratic regime as one with a narrow franchise. Democratization, therefore, results in a significant increase in the share of adults with the right to vote. If the incidence of redistributive taxation is determined by the median voter and access to *de jure* political power is widespread, then the median voter should prefer higher taxes as her incomes diverges from mean income (Meltzer and Richard 1981). Moreover, if democratic reforms result in enfranchising primarily below-average income residents, then democratization should lead to a substantial widening of the gap between mean income and that of the median voter. By the same logic, a reversal of democracy - particularly in cases in which the share of lower-income residents who are enfranchised declines - should lead to a narrowing of the difference between the mean and the median voter's income. As a result, demand for redistributive taxation from the post-reversal median voter should fall.

A number of theories have either directly challenged this redistributionist hypothesis or provided important qualifications. Criticisms of this median-voter framework emphasize that the masses should struggle to overcome collective action problems to pose a redistributive threat to elites; instead, a society's incidence of redistribution reflects intra-elite conflicts and preferences (Ansell and Samuels 2014, Lizzeri and Persico 2004). On the other hand, Aidt, Daunton and Dutta (2010) argue that when taxation falls heavily on the middle class that the extent of enfranchisement has a U-shaped relationship with redistributive spending. Chapman (2016), by comparison, argues that redistribution will takes an inverted U-shaped relationship when the poor pay taxes and transfers only take the form of public goods. While concurring with the redistributionist thesis that the poor will demand more redistribution, Acemoglu and Robinson (2008) argue that democratization may not result in greater redistribution if elites' *de facto* power allows them to use their resources (e.g., greater capacity for collective action, lobbying, intimidation and violence) to distort the political process.¹⁷

There are various features of the post-Civil War South that allows us to directly test these arguments.

¹⁷Furthermore, in practice, there are many sources of electoral distortion that may cause policy deviations from the preferences of the median voter (e.g., the poor vote with lower probability than the rich, elites possess more resources to influence elections, etc.). Nonetheless, for any degree of electoral distortion, the post-reversal divergence between the mean and median voter's income should result in greater political support for redistribution. While largely finding that levels of redistribution reflect the preferences of the median voters, Corneo and Neher (2015) also provide evidence that policy bundling can lead to sub-median-voter levels of redistribution.

First, given that the expansion of the franchise to the former slaves was externally imposed by Congress in 1867, and subsequently enforced by federal troops, the concern of reverse causation is minimized.¹⁸ The second key to our estimation strategy regards the factors that influenced state and local taxation in the South. Unlike black political rights which were strongly influenced by the federal government and the federal-level partisan environment, taxation in this period was completely determined by political processes internal to the South. This was also an era in which state and local government was much larger than the federal government (Wallis 2000) and was responsible for most of the key public services (e.g., schools, infrastructure).¹⁹

Perhaps the most critical feature is that ad valorem general property taxes comprised the vast majority of tax revenues, at both the state and especially the local level (Seligman 1969; Hollander 1899). Any increase (reduction) in taxes therefore was borne by (benefited) the owners of property. Due to the lack of meaningful land reform following the Civil War, ad valorem property taxes in the South fell mostly on whites. Furthermore, the extension of the franchise to the largely illiterate and impoverished former slaves effected an unambiguous downward shift in the wealth of the median voter. Given the reliance on general property taxes, the vast majority of Southern blacks would have benefitted directly from (and presumably would therefore have supported) a large-scale increase in ad valorem property taxes to fund redistributive public policies.²⁰ In the 19th Century, redistribution largely took the form of greater public services, especially public education (Lindert 2004). Put simply, a clear implication of the redistributionist theory is that the revolutionary political reforms imposed upon the South during Reconstruction should have resulted in a large increase in demand for and provision of public services. According to Foner (2011, 364), this is precisely what happened: “Serving an expanded citizenry.....Republican government affected virtually every facet of Southern life. Not only the scope of its activity, but the interests it aspired to serve distinguished the Reconstruction state from its predecessors....Public schools, hospitals, penitentiaries and asylums for orphans and the insane were established for the first time or received increased funding.”

This expansion of public services would necessitate an expansion in the fiscal capacity of Southern state and local governments. According to a special report from Congress in 1872, total state and local per capita taxes across the 10 Reconstruction states increased nearly 40% between 1860 and 1870.²¹ Because Southern

¹⁸Many theories of regime change emphasize the importance of elite preferences, which could be correlated with views on redistribution and public goods (Lizzeri and Persico 2004), or on the incidence and timing of transitions to democracy (see e.g., Geddes 1999). This potential unobserved source of bias is not a concern here.

¹⁹Federal tax revenues in this period were not only low, but consisted almost exclusively of indirect taxes, such as excise taxes and tariffs. Furthermore, the types of public goods provided by the federal government, such as national defense, would have been constant across counties (i.e., the units of comparison in our study). The period of this study ends prior to the great expansion of the federal government in the 1930s, both in size and in areas of involvement (e.g., transfers, infrastructure).

²⁰The suggests that the argument by Chapman (2016) that the poor may prefer lower taxation if they also pay taxes is therefore less relevant given the Southern tax structure.

²¹This rapid increase in tax revenues occurred despite a nearly 20% decline in the assessed value of non-slave wealth over the same period. See the Report of the Joint Select Committee on the Condition of Affairs in the Late Insurrectionary States,

whites owned almost all of the land and other productive resources (Ransom and Sutch 2001), and most taxation derived from general property taxes, this expansion in public services and the fiscal state more generally was funded primarily by taxing the landed elite (i.e., the former slaveholding planters).²²

Similarly, the subsequent disenfranchisement of African Americans largely removed voters almost exclusively from the lower end of the Southern wealth distribution. As these suffrage restrictions, such as literacy tests, precisely targeted African Americans, the redistributionist thesis suggests that the extent to which their adoption should negatively affect county-level tax revenues should be proportionate to the share of a county's total population who were African American.²³ Other theories in this literature would predict different consequences to large-scale but spatially heterogeneous disenfranchisement. According to Chapman (2016), in counties where African Americans comprise a minority of the population, their disenfranchisement would lead to more redistribution; as county black-population share rises, however, and a therefore the elite comprises a larger proportion of the electorate, taxation should fall. On the other hand, the "retrenchment theory" by Aidt, Daunton and Dutta (2010) suggests that taxation would fall the most in counties in which African-Americans are approximately half the population. Tax revenues may actually increase in high *and* low black-population share counties following the introduction of the LT restrictions. Other important theories suggest their should be little relationship between a county's black population and tax revenues, as redistribution would be determined by other factors, such as intra-elite conflicts (e.g., Ansell and Samuels 2014, Lizzeri and Persico 2004).

A final concern is that black political power largely ended with the demise of federal Reconstruction. If white elites used their de facto power to distort the political process, then "de jure" disenfranchisement should have a diminished effect on observed redistribution (Acemoglu and Robinson 2008). While tax rates fell following the end of Reconstruction, tax *revenues* certainly did not collapse.²⁴ Additionally, sustained levels of spending on public education, for instance, remained. Most tellingly, the gaps in black-white education inputs, such as spending per pupil and teachers per pupil, were either small or non-existent in the 1880s (Margo 1990; Naidu 2012). Furthermore, the change in Southern taxes as a share of economic output between 1880 and 1890, the first decade following Reconstruction and prior to *de jure* disenfranchisement, actually

²¹41st Congress, 1872.

²²As example of the dramatic rise in taxation during Reconstruction, in Mississippi, the state tax rate on personal property rose from approximately 1 mill on each dollar of the assessed value of land (i.e., \$1 for every \$1000 of assessed land) in 1868 to 14 mills in 1874 (Hollander 1899).

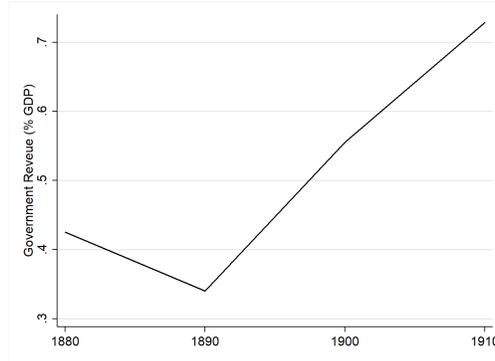
²³Although these voting restrictions should have also disenfranchised many poor whites, states adopted "*grandfather clauses*" that largely maintained white suffrage. Furthermore, the adoption of these restrictions was clearly targeted at African Americans (Valelly 2009). For instance, the convention president of the 1901 Alabama Constitutional Convention, John B. Knox said in his first public address: "And what is it that we want to do? Why it is, within the limits imposed by the Federal Constitution, to establish white supremacy in this State" (Address of Hon. John B. Knox, 1901).

²⁴By our calculations, the average Southern county levied approximately \$2.1 (in 1890 \$) per capita in taxes in both 1870 and 1880. There was, of course, substantial variation both within and across states in how much tax revenue per capita changed following Reconstruction. In Arkansas, Mississippi and South Carolina, the average county experienced a more than 20% decline in real taxes per capita. In the remaining 14 states, the average county in four states saw a small decline and ten experienced an increase.

increased relative to the North. Figure 1 shows the difference in tax revenue as a share of output between the North and South between 1880 and 1910, the years of our study. The beginning of disenfranchisement in 1890 coincides with a sharp divergence between these regions in the size of the fiscal state.

Figure 1

Difference in Tax Revenue (% of Output), North less South



Note: The tax data in the numerator aggregates all state and local (county and municipal) level taxes to the state level. The denominator, state real income, measures the value of total output in each state. The average value for the 18 Northern states that existed and in which slavery was illegal in 1860 was subtracted from the average for the 15 Southern states in which slavery was legal in 1860.

This is important for at least two reasons. For one, this undermines Key’s (1984) famous claim that *de jure* disenfranchisement had relatively minor welfare implications, as blacks were *de facto* already disenfranchised. Second, this alleviates a potential source of bias that likely plagues the empirical literature using cross-national data from large-scale suffrage expansions, such as the extension of the franchise to women; namely, these democratic reforms often occurred in a period of fiscal expansion (Scheve and Stasavage 2010). Investigating the consequences of democratic reversal in a period of fiscal expansion ameliorates concern that our case selection is simply capturing a period of fiscal retrenchment. This claim is buttressed by our finding that low black population share counties in the South experience increases in taxation comparable to the average county in the North.

4 Estimation Strategy

4.1 Data

Our baseline sample consists of all counties from the 17 Southern states in which slavery was legal in 1860.²⁵ The panel spans from 1880 to 1910 in which the variables are measured at 10-(Census-)year intervals.

4.1.1 Variables of Interest

Our empirical strategy leverages two particular features of the Southern post-Civil War setting. First, we exploit the great variation across Southern counties in the share of the total population who were black.²⁶ While the shocks to democracy were determined by federal- and state-level policy, the spatial distribution of their effects varied enormously. We use this spatial variation in the black population to precisely measure the potential consequences of the adoption of suffrage restrictions on the size of the electorate. The share of each county’s population who were African American was taken from each US Census between 1880 (the first Census following Reconstruction’s ending) and 1910 (the first Census following the adoption of all of the various suffrage restrictions). This approach for measuring changes in “democracy” significantly reduces the measurement error pervasive in this literature’s cross-national datasets.²⁷

Second, we exploit the differential timing by which states adopted suffrage restrictions. We use [Valelly \(2009\)](#) to determine the timing when each state adopted a suffrage restriction, which we measure with two separate binary indicators. Our primary measure is the timing of a state’s adoption of the literacy test, which is considered the more restrictive of the suffrage restrictions ([Kousser 1974](#); [Valelly 2009](#); [Walton, Puckett and Deskins 2012](#)). We code this variable as 0 until a literacy test restriction is adopted, at which point the value is coded as 1 for the next 10-year interval of our panel. For example, South Carolina’s adoption of the literacy test in 1895 is coded as 0 for each prior panel-observation (1880 and 1890) and 1 for each subsequent observation (1900 and 1910). As reported in Table 1, seven Southern states adopted the literacy test restriction. Our second measure is the timing of a state’s adoption of any suffrage restrictions (e.g., poll tax, literacy test, etc.). We use this as a robustness check on the literacy test indicator.²⁸ The estimates for

²⁵See Table 1 for the list of states we code as meeting this criteria.

²⁶Appendix-Figure 10 displays the extensive spatial variation in black share in 1880. In the ten Reconstruction states, African Americans were a majority in nearly a third of the counties; likewise, they comprised less than 15% of a county’s total population in nearly a third of counties, as well. See Table 1 for a similar measure of this variation by state in 1870.

²⁷The importance of error in measuring variation across countries in the quality of democratic institutions, and the consequences of using these variables to estimate the impact of democracy has been studied extensively. See, for instance, [Acemoglu et al. \(2014\)](#) for a discussion of this literature.

²⁸States also implemented proof of poll tax payment requirements for voter registration that differed slightly by the tax rate and extent of accumulated tax necessary to vote, as well as other voting restrictions in this period ([Valelly 2009](#)). As such, the literacy test requirement is not only more stringent and targeted, but is the also the most comparable restriction between states.

the *any restriction* indicator, which produces similar results, are presented in the appendix.

A comparison of counties in states that adopted a literacy test restriction in this period and counties in states that did not illustrates important differences and similarities. Most notably, the states that did not adopt this restriction (henceforth, *comparison* states) had an average county black-population that was 13% lower. A direct comparison of means across states, however, is misleading because of significant variation within each state. The 12 states that would adopt any voting restriction were also slightly richer, less urbanized, and collected more taxes than the other 5 states. While comparing similar counties in suffrage restriction states to those in ones without restrictions is one of our estimation strategies, our results remain significant when we restrict the sample to only the counties in the 10 Reconstruction states, as well as only counties in the 7 states that adopted the literacy test.

4.1.2 Dependent Variable - Taxation per capita

Our main dependent variable is the total amount of state and local taxes per capita levied in each county at 10-year intervals from 1880 through 1910. The source for this data is the 1880 Census and the Census' *Wealth, Debt and Taxation* reports for 1890, 1902 and 1912, respectively.²⁹ With the exception of 1900, each source provides the amount of tax revenues collected within each county by each level of government (i.e., state, county and municipal). Due to the availability of only aggregated state & local tax data for 1900, we combine municipal, county and state taxes levied in each county to create a measure of total taxation for each county that is consistent across each panel year. We then divide total tax revenues by the number of inhabitants to create a measure of taxes per capita for each county. For each measure of county taxation, we make an inflation adjustment so that the value of tax revenue for each county-decade observation is in 1880 dollars.³⁰ Appendix-Figure 11 displays the distribution of this variable for each county-decade observation in the sample. There is little graphical evidence of a systematic difference in the level of taxation between states. The growth rate of taxes per capita by county from 1880-1910 is plotted in Appendix-Figure 12.

4.1.3 Control Variables

We include a number of covariates to control for additional factors that may affect the level of taxation across counties. Each census in our period of study (1880-1910) includes a measure of the value of agriculture and manufacturing output by county, which we use as a proxy of economic output per county. This controls for a county's ability to raise greater tax revenues. We include separate measures of the inflation-adjusted

²⁹These reports mark the first time that the federal government began systematically collecting detailed information on revenues and expenditures of the thousands of sub-federal political and administrative units (i.e., state and local governments).

³⁰All dollar-value variables are adjusted to reflect 1880 price levels. Consumer price index figures are available from the Federal Reserve Bank of Minneapolis: <https://www.minneapolisfed.org/community/teaching-aids/cpi-calculator-information/consumer-price-index-1800>.

value of agricultural and manufacturing output per capita as reported by each Census between 1880 and 1910. Each census also collects the amount of real personal property. We include the real per capita value of this for each panel-year. We also include the log of total county population and population density as proxies for economic development and possible economies of scale and capacity to raise more tax revenues. We proxy for social fractionalization, especially due to high rates of immigration, which has been argued to lower support for public goods and redistribution (Bisin and Verdier 2017), by including the share of each county’s population who were foreign born.

Scholars have also argued that an inclusive suffrage does not necessarily lead to high levels of redistributive taxation, as the amount of redistribution that occurs in equilibrium is determined by the distribution of *de facto* power in a society (Acemoglu and Robinson 2008, Acemoglu et al. 2014). We include several measures to control for this across counties. First, we include a measure of the Gini coefficient of land inequality, which is often used as a proxy for variation in the *de facto* power of elites.³¹ Given the highly rural nature of the late 19th-Century South (less than 20% lived in urban areas of at least 2500 people in 1900), land inequality may also proxy for variation across counties in the demand for redistributive taxation. As mentioned above, many scholars of American politics have argued that the disenfranchising restrictions did not indicate the ending of political power of African Americans, but was rather reflective of the *de facto* disenfranchisement of the immediate post-Reconstruction period. We proxy for this possibility by including a measure of the number of known lynchings that occurred in a county in each panel-decade (available from Bailey et al. 2008). Because black voters were overwhelmingly Republican, we also use county-level vote share for Republican Party gubernatorial candidates as an alternative measure of local *de facto* disenfranchisement (data available from Hirano and Snyder (2007)).

Table 2 contains descriptive statistics for the main variables, reported separately by whether the county-year observation was in a state that adopted a voting restriction versus those that had not yet done so or did not do so at all.

4.2 Estimation Strategy

We use the state-specific timing of each voting restriction’s enactment to estimate the impact of black disenfranchisement on fiscal outcomes. We identify the effect of disenfranchisement by exploiting the enormous within-state variation in county black-population share. However, note that in the presence of significant out-migration, employing contemporaneous values of county black-population share may conflate disenfranchisement and out-migration. Most concerningly, disenfranchisement may cause selective migration effects,

³¹Acemoglu, et al. (2014) uses a country’s Gini coefficient of land inequality as a measure of the relative resources elites can use to distort the political process, as well as a proxy for the demand for redistribution.

Table 2
Descriptive Statistics

	No Restriction		Any Restriction	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
	Fiscal Size Variable			
Log Total Tax Revenue*	1.420	0.948	1.555	0.748
	Demographic Variables			
Black Share	0.209	0.229	0.339	0.245
Log Total Population	9.222	1.366	9.569	0.809
Foreign Share	0.036	0.069	0.014	0.041
Log Population Density	3.055	1.103	3.345	0.830
	Political and Economic Variables			
Republican Share	0.354	0.200	0.264	0.196
Lynching	0.501	3.076	2.140	6.897
Log Agricultural GDP*	4.135	0.842	4.222	0.851
Log Manufacturing GDP*	2.946	1.313	3.525	1.248
Land Gini	0.433	0.127	0.434	0.071
Observations	3352		2010	

Note: Voting Restrictions: Restrictions from [Kousser 1974](#); [Valelly 2009](#). Economic variables: computed from Wealth, Debt and Taxation reports 1890, 1902 and 1912; US Census 1880. Demographic variables: US Census, 1880-1910. For additional sources, see main text. The number of observations applies to the dependent variable, taxation per capita.

*per capita real 1880 USD

wherein more productive African Americans expecting to receive fewer public goods migrate away. To minimize these potential concerns, our primary measure of the magnitude of disenfranchisement is 1880 county black-population share and we restrict attention to just the pre-treatment (i.e., 1880) values of all time-varying covariates. Nonetheless, we show below that our findings are robust to using contemporaneous values for county black-population share and the other covariates.

Our primary estimation strategy is a difference-in-differences model with heterogeneous treatment effects. Specifically, it interacts 1880 county black-population share with a dummy indicating the timing of the adoption of the literacy test as follows:

$$\ln(y_{cst}) = \gamma LT_{st} + \theta(bs_{cs1} \times LT_{st}) + \mathbf{x}_{cs1}\mathbf{\Gamma} + \alpha_c + \nu_{st} + \varepsilon_{cst}, \quad (1)$$

where $\ln(y_{cst})$ is the logged per-capita real tax revenue in county c , in state s in year t . bs_{cs1} is the county black-population share in 1880 (black population over total population) and LT_{st} is a state-specific dummy indicating the presence of a literacy restriction at time t . α_c , and ν_{st} are a set of county-fixed effects and

state-specific linear time trends, respectively.³² \mathbf{x}_{cs1} contains a set of 1880 controls entered directly and interacted with LT_{st} . Finally, ε_{cst} is the error term. All errors are robust to arbitrarily heteroskedasticity and clustered at the county level.

While county fixed effects account for level differences between areas with respect to unobservable political or economic demand for taxation, state-specific time trends further condition on between-state temporal changes in taxation that is perhaps coincidental with adopting a literacy test. The coefficient on the literacy test, γ , therefore captures the following average treatment effect: the mean shock (i.e., relative to a linear state-specific trend) to taxation in counties in literacy-test states relative to similar counties in the comparison states.

The familiar identifying assumption is difference-in-difference invariance i.e., taxation shocks (relative to linear state-specific trends) in LT-state counties would have occurred at the same level as taxation shocks in comparable counties where restrictions were not adopted. In our setting, the main effect, which is assumed to be homogeneous across time and counties, is inappropriate precisely because the literacy test was applied predominately to African Americans. Therefore, our coefficient of interest is θ , which characterizes the difference in the slope on black-population share for states adopting the literacy-test voting restriction.

5 Main Results

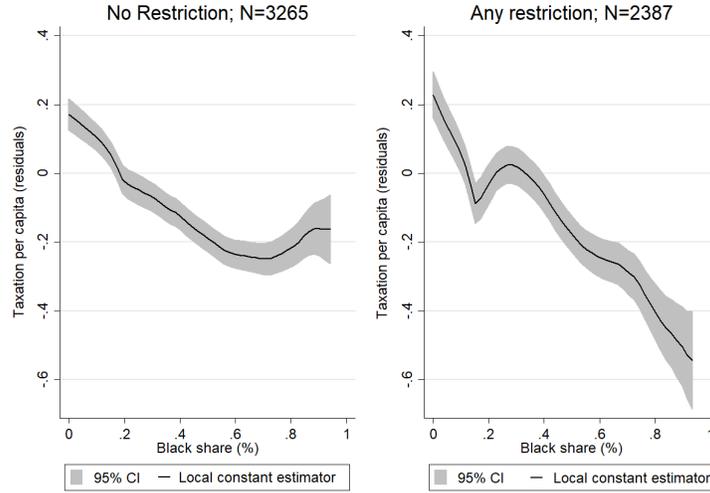
We begin by visually displaying in Figure 2 the underlying relationship between black-population share (x-axis) and total taxation per capita (y-axis), by presence of the any restriction to suffrage, after controlling for county- and time-fixed effects. The figure presents Nadaraya-Watson non-parametric regression residuals of total taxation per capita. Graphically, there is strong evidence of common covariate overlap in both panels. Also, both panels show that per capita taxation is initially decreasing in black share. As black share increases above 50%, however, there is a significant divergence in taxation between counties in states with voting restrictions and those without any restriction.

5.1 Difference-in-Differences Estimates

We now report the estimates from Equation 1. Table 3 reports these estimates when the enactment of a literacy test requirement (LT) is used as the indicator of *de jure* disenfranchisement, and county black-population share and the other covariates are held at their pre-treatment values (i.e., 1880). The first column of Table 3 shows that the effect of being in a literacy test state on (logged) total tax revenue (per capita in real

³²Since we include a full set of county fixed effects we cannot estimate the unconditional effect of $\theta(bs_{cs1})$. This is the case for all time-invariant variables that may impact the level of taxation.

Figure 2
Taxation Residuals, by Presence of Any Voting Restriction



Note: Residuals estimated from linear regression of logged taxation per capita against year and county dummy variables from 1880-1910; smoothed using Nadaraya-Watson nonparametric regression with Epanechnikov kernel (bandwidth chosen by rule-of-thumb estimator).

1880 \$USD) is decreasing in county black-population share. Columns 2-4 show that the relationship between disenfranchisement and redistribution is robust to the further inclusion of state-specific trends, demographic and political characteristics, such as lynchings, as well as economic characteristics, such as land inequality, agricultural output, manufacturing output and population density.

In addition to being statistically significant, the estimate is quantitatively large - an increase in 1880 black population share by one standard deviation has a negative effect on county per capita tax revenues of approximately 5.4% in states that enacted a literacy test restriction. In multiplicative interaction models, marginal effects should be evaluated at each level of the continuous interacted variable (Hainmueller, Mumolo and Xu n.d.). We plot the predicted change in per capita total taxation for LT states by 1880 black share in Figure 3A (left). The (negative) marginal effect of disenfranchisement is linearly increasing in black share and reaches significantly negative levels for areas with more than 62% black share.

Table 10 of the Appendix reports similar findings with the implementation of *any restriction* as an indicator for de jure disenfranchisement. In Column 1, we find that with the introduction of any voting restriction, a one standard deviation increase in 1880 county black-population share reduced per capita county tax revenue by 4.7%. Although the coefficient is still highly significant, it is approximately one-eighth smaller than with the literacy test. Given the less restrictive effect of poll taxes on enfranchisement, this smaller, but still statistically significant, relationship is expected. In Column 2, we find that the negative effect of disenfranchisement is robust to conditioning on economic and political covariates (entered

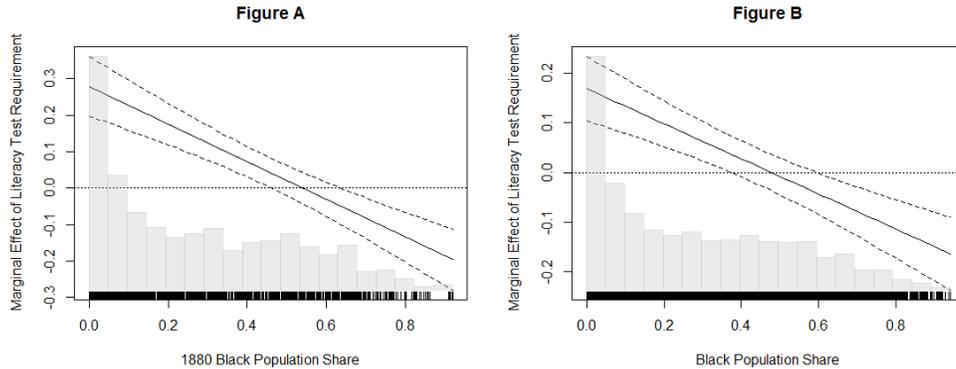
Table 3
Racial Disenfranchisement and Taxation

	(1)	(2)	(3)	(4)
Literacy Test Req. ×	-0.518***	-0.496***	-0.426***	-0.304**
Black Share ₁₈₈₀	(0.079)	(0.092)	(0.095)	(0.097)
Literacy Test Req.	0.279***	0.500***	0.548*	1.273***
	(0.044)	(0.058)	(0.223)	(0.327)
State-specific trends	No	Yes	Yes	Yes
Controls ₁₈₈₀	No	No	Yes	Yes
Literacy Test Req. ×				
Controls ₁₈₈₀	No	No	Yes	Yes
Observations	5029	5029	4634	4634
R ²	0.61	0.69	0.73	0.71
Counties	1332	1332	1200	1200

The dependent variable is (log) total tax revenue per capita, in real 1880 \$ USD. All regressions include county-fixed effects. Column 1 further includes year-fixed effects, while Columns 2-4 include state-specific linear trends. The sample includes 17 states from 1880-1910. Robust standard errors clustered at the county-level in parentheses. * significant at the $p < .05$; ** $p < .01$; *** $p < .001$

Figure 3

The Marginal Effect of Racial Disenfranchisement on Taxation



Note: Solid line indicates the marginal effect of literacy test requirement, with 95% confidence intervals in dotted lines. Grey and black lines capture the density of black share. Figure A uses estimates from Tables 3; Figure B reproduces equivalent estimates from Table 4

individually and interacted with any restriction).

Note that for both models in Tables 3 and 10, the coefficient on the dummy variable indicating a literacy test state or any restriction state is positive and highly significant. This indicates that states that adopted suffrage restrictions experienced considerable fiscal expansion relative to other states. Given that the relationship of county black-population share on taxation is strongly negative, we return to this counter-intuitive finding in Section 5.3.

Table 4
Racial Disenfranchisement and Taxation, 1880-1910: Sensitivity to Time-varying Covariates

	(1)	(2)	(3)	(4)
Literacy Test Req. \times	-0.486***	-0.497***	-0.500***	-0.409***
Black Share	(0.075)	(0.083)	(0.084)	(0.083)
Literacy Test Req.	0.290***	0.441***	0.360***	0.323***
	(0.041)	(0.044)	(0.042)	(0.041)
Black Share	1.129***	0.152	0.482*	0.231
	(0.216)	(0.247)	(0.218)	(0.219)
State-specific trends	No	Yes	Yes	Yes
Time-varying Controls	No	No	No	Yes
N	5254	5254	4806	4788
R^2	0.55	0.64	0.74	0.74
Counties	1436	1436	1341	1325

The dependent variable is (log) total tax revenue per capita, in real 1880 \$ USD. All regressions include county- and time-fixed effects. The sample includes 17 states from 1880-1910. Robust standard errors clustered at the county-level in parentheses.

* significant at the $p < .05$; ** $p < .01$; *** $p < .001$

By using only pre-treatment values, our baseline model (Equation 1) utilizes a conservative measure of the incidence of disenfranchisement to mitigate migration concerns. Nonetheless, the mostly qualitative historical literature suggests that large-scale black migration did not begin until World War I. As a result, we now test whether our results hold when estimating Equation 1 using contemporaneous values for all time-varying variables. Table 4 indicates that the sharp reduction in fiscal size for high black population share counties in LT states remains strongly significant when using time-varying values. Although the magnitude of the effect is slightly smaller, the essential relationship between disenfranchisement and redistributive taxation is robust to state specific linear trends and interactions between time-variant control variables and the literacy test requirement. Consistent with the point estimates from Table 3, Column 1 of Table 4 indicates an increase in county black share by one standard deviation was associated with a 8.9% reduction in per capita total taxation for counties in states that enacted a literacy test requirement.

In the appendix, Columns 3-4 of Table 10 report similar estimates when using contemporaneous values

and the *any restriction* indicator for de jure disenfranchisement. As above, we find that the elasticity of taxation with respect to any voting restriction is negative and robust to economic covariates and covariate-*any restriction* interactions. We note that the estimate is again slightly smaller in magnitude than the literacy test restriction indicator in Table 4. As a final comparison, we reproduce the marginal effect of any disenfranchisement on taxation in Figure 3B (right). In this specification, literacy restriction counties with contemporaneous black-population share of greater than 44% experience a significant reduction in per capita taxation relative to comparable counties in states without restrictions. Given the indistinguishable results when using the pre-treatment or contemporaneous values, we focus on contemporaneous black share as a more precise measure of the scope of disenfranchisement in the robustness tests in Section 6.

5.2 Long-Difference Estimates

As noted in Section 2, states differed in not only whether they implemented voting restriction, but also in the type of restriction and timing of its implementation. Even amongst states that were able to pass constitutional amendments to restrict the vote, legislative and budget constraints may have limited the speed with which legislators and governors could adjust state budgets. The resulting differences in the temporal lag between ratifying literacy tests, implementing tests, the timing of elections, setting new budgets and so forth, could introduce significant measurement in our primary model. For instance, one state may implement a restriction and nearly immediately be able to act based on a significantly diminished electorate; on the other hand, another state may take many years for this to take effect based on their election schedules (e.g., senators and governors serve 4 years in some states, whereas in another they only serve 2 years). Furthermore, any potential endogeneity of the timing in which states adopted suffrage restrictions, LT_{st} , would upward bias the point estimates presented thus far.

One additional approach of many that we use to estimate the essential relationship between the scope of suffrage restrictions and per capita tax revenues is a long-difference model. Specifically, we estimate the following model:

$$\Delta_{1,2} \ln(y_{cs}) = \gamma bs_{cs1} + \theta(bs_{cs1} \times LT_s) + \mathbf{x}_{cs1} \mathbf{\Gamma} + \nu_s + \epsilon_{cs}, \quad (2)$$

where $\Delta_{1,2} \ln(y_{cs})$ is the real growth rate of revenues between $t = 1$ and $t = 2$. As above, bs_{cs1} is county black population share in 1880, while LT_s is an indicator for states that would ever pass a literacy test requirement. \mathbf{x}_{cs1} is a set of pre-treatment variables in 1880 and ν_s captures state-fixed effects. Therefore, γ characterizes the slope on black share in counties in the comparison states and $\gamma + \theta$ is the slope in the “treatment” states (i.e., states introducing a voting restriction). We expect the main coefficient of interest is θ - which is the difference between the two groups - to be negative.

Long-difference models are used to directly account for measurement error and endogeneity in the cross-sectional time-series models above. They are equivalent to differencing the end and beginning of the event-panel model (Equation 1). State-fixed effects are included to remove level differences between states that adopted a literacy restriction and those that did not. Therefore, we exploit the same identifying assumption (difference-in-difference invariance): in the absence of a literacy test, real per capita taxation would have grown at the same rate in treatment and comparison states with the same 1880 black population share (adjusting for the covariates). The coefficient of interest in Equation 2 captures the effect of the literacy test on the differential growth rate of real taxation per capita in restriction versus no-restriction counties with similar 1880 black population share and covariates.

Reassuringly, in Table 5 we find that our results are robust to a long-difference specification. The estimate in Column 4 is highly significant and indicates that a one standard deviation increase in county black share is associated with a 17% decrease in the growth rate of per capita tax receipts in restriction counties relative to similar counties in states without a literacy-test restriction. Interestingly, in Appendix-Table 12 we find that the *any voting restriction* indicator leads to even larger 25.7% reduction in the differential growth rate of per capita taxation.

Table 5
Racial Disenfranchisement and Taxation: Long-Difference Estimates

	(1)	(2)	(3)	(4)
Literacy Test Req. \times	-0.378*	-0.888***	-0.715***	-0.765***
Black Share ₁₈₈₀	(0.177)	(0.186)	(0.187)	(0.181)
Literacy Test Req.	-0.266***	-0.184	0.008	-0.045
	(0.069)	(0.183)	(0.161)	(0.160)
Black Share	-0.071	0.301*	0.351**	0.462***
	(0.121)	(0.127)	(0.135)	(0.136)
State fixed effects	No	Yes	Yes	Yes
Controls				
Baseline ₁₈₈₀	No	No	Yes	Yes
Additional ₁₈₈₀	No	No	No	Yes
N	1106	1106	1057	1057
R^2	0.12	0.38	0.42	0.42

The dependent variable is the growth rate in total tax revenue (per capita in real \$USD) between 1880 and 1910. The sample includes 17 states from 1880-1910. Robust standard errors clustered at the county-level in parentheses.

* significant at the $p < .05$; ** $p < .01$; *** $p < .001$

5.3 Positive Effects of Disenfranchisement on Taxation

The positive coefficient on the literacy-test indicator in Tables 3 and 4 indicates that low black-population share counties experienced an increase in taxation following disenfranchisement. We now investigate this finding that runs counter to our other evidence that is consistent with the redistributionist thesis.

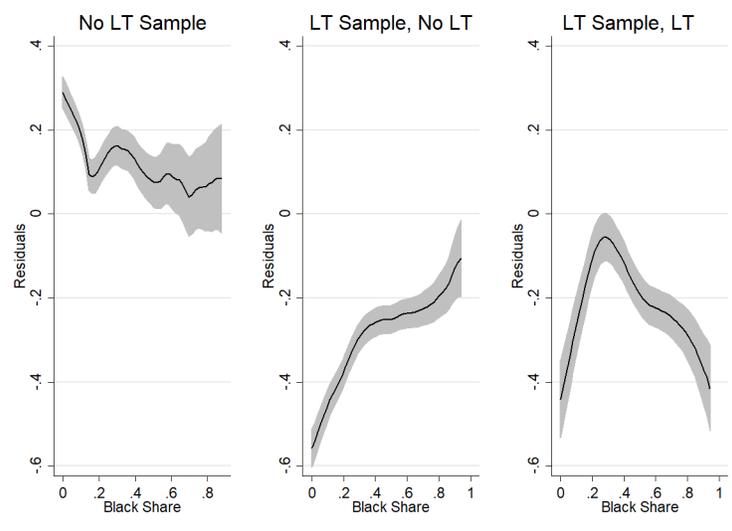
We first try to determine whether the literacy test caused this increase, or whether it is due to some unobserved county-level factor. We explore this visually by plotting the residuals from a regression of per capita total taxation against year and county dummies separately for counties in non-literacy-test states, literacy-test states prior to the adoption of this restriction, and literacy test states after its adoption. Figure 4 shows that there is a surprisingly similar relationship between black share and total taxation in low black share counties for literacy-test states with existing literacy-test restrictions and literacy-test states that had not yet passed this restriction. That is, the expansion of per capita taxation in black share is mirrored for literacy-test states - whether or not they had yet implemented a literacy-test restriction - while there is no discernible relationship in non-literacy-test states. This suggests that the positive effect of disenfranchisement we identify is not directly driven by the literacy-test restriction *per se*, but instead by unobserved county characteristics. We show below that these low black-population share counties in which per capita tax revenues increased are highly concentrated among the relatively few urban counties in literacy-test states.³³

Yet this does not explain why these urban counties in the literacy-test states increased their per capita tax revenues relative to similar counties in non-restriction states. To unpack these findings, we disaggregate total per capita taxation into the total state and local taxes per capita levied in each county. A disaggregation of taxation receipts reveals two patterns through which the positive effect might have operated. First, we find substantial evidence that the within-state distribution of state taxes shifted towards low black-population share counties following the adoption of literacy-test restriction. To demonstrate this, we compute each county's share of total state taxation revenues for each period between 1880 and 1910 solely on the counties in the seven literacy-test states. Figure 5 plots regression residuals from regressing each county's share of total state tax revenues on county- and year-fixed effects against county black-population share, by whether the state had adopted literacy test yet (i.e., left is pre-LT adoption; right is post-LT adoption). We find that low black-population share counties prior to this restriction's implementation also had to bear an increasing share of the state tax burden (left). The key, however, is that we see a sharp shift in the share borne by high black-population counties following the adoption of the literacy test (right).

We now provide evidence for two channels through which this occurs. First, the literacy test precipitated a shift in the composition of state taxes away from direct taxation towards indirect taxation, such as licensing

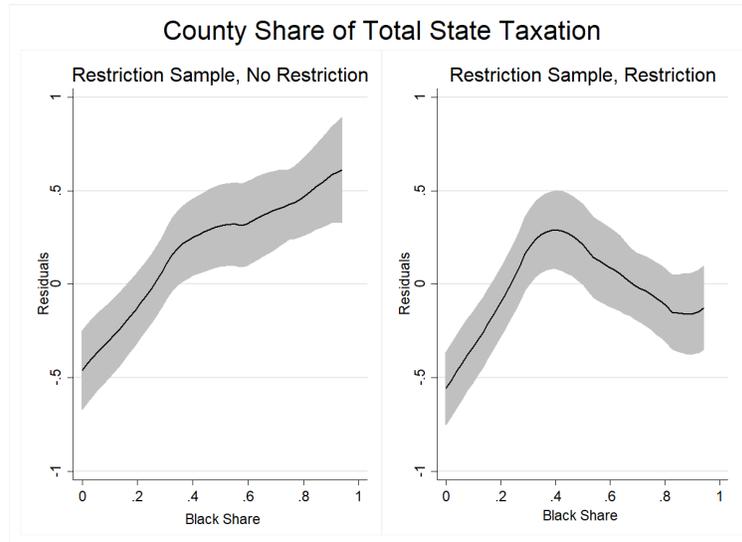
³³We use the urban data provided by the 1910 Census, which indicates the total county population living in cities of more than 2500 residents.

Figure 4
Taxation Residuals, by Presence of LT



Note: Residuals estimated from linear regression of logged taxation per capita against year and county dummy variables from 1880-1910; smoothed using Nadaraya-Watson nonparametric regression with Epanechnikov kernel (bandwidth chosen by rule-of-thumb estimator). No LT Sample = county-year observations for states in which no literacy restriction (LT) was ever implemented (N=3,600). LT Sample, No LT = LT states before LT has been implemented (N=2,055); LT Sample, LT = LT states after LT has been implemented (N =778).

Figure 5
Share of Total State Taxation Residuals, By Presence of Literacy Test



Note: Black lines indicate residuals from regression of county share of total state taxation on county- and year-fixed effects, with 95% confidence intervals in grey. The solid black line includes only county-year observations with the LT; the dashed line includes the converse.

fees and excise taxes. This incidence of indirect taxation fell more heavily on urban counties.³⁴ The second (and more important) channel by which the burden of state taxes was shifted onto urban counties is related to the effects of the literacy test on ad valorem property tax revenues collected. These tax revenues can be affected by changes in both statutory rates and assessments of property values. While the appraised value of property was supposed to reflect current market values, tax revenues could be undermined if they were strategically undervalued. While each level of government (state, county, municipal) could levy their own property taxes (i.e., each level could set their own rates), the assessed value of taxable property used by each level was determined by local officials. Thus, the adoption of the literacy test would allow large landowners in high black-population share counties (which were overwhelmingly rural) to comprise a greater share of post-restriction electorate selecting the local property assessors. If property values in high black share counties were systematically undervalued, this would not only lower tax revenues from each level of government collected in high black share counties (assuming rates were unchanged), but it would also *necessarily* increase the relative proportion of total statewide assessed property in the low black-population share counties. This is one reason we see such a large shift in the state tax burden towards lower black-population share counties.

We demonstrate this using total assessed property values in each county from 1880 to 1910 to create a measure of per capita assessed property values in each county.³⁵ First, we plot the marginal effect of racial disenfranchisement (again, the coefficient on the interaction of the literacy-test dummy and county black population share) on per capita assessed property values in Figure 6. As with taxation, we find a negative relationship between disenfranchisement and assessed valuation that is precisely estimated and significantly negative for areas above 40% black share.

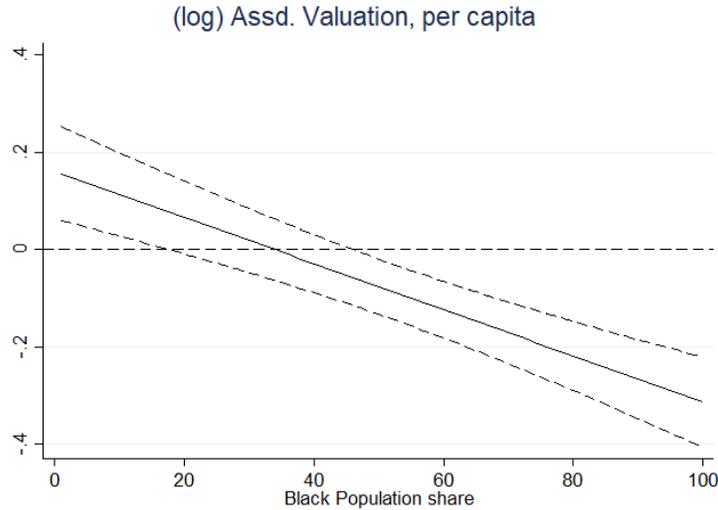
While this evidence explains the within-state shift in the state taxation burden, it does not account for why assessments in low black-population areas increased relative to similar counties in non-literacy-test states. As we indicated above, we find that this is largely driven by urban counties, almost all of which have black-population shares of less than 40%. To test this, we employ both the long-difference and difference-in-differences models leveraging the state-specific timing of the literacy-test restriction. This indicator is interacted with a dummy for counties housing any urban population in 1910 (i.e., the county contained a town with a population greater than 2,500 persons). We find that per capita assessed wealth grew by as much as 12.2% (16.1% of the sample mean change) between 1880-1910 in urban counties in literacy-test states relative to similar counties in non-restriction states. By comparison, non-urban counties in literacy-test states experience a 16.9% decline relative to similar counties in non-LT states. In the difference-in-differences

³⁴We discuss this evidence in more detail in Section 6.1.

³⁵This data was collected from the same sources used for the tax revenue data (i.e., the 1880 Census and from various *Wealth, Debt and Taxation* reports for the later panel years).

Figure 6

Marginal Effect of Disenfranchisement on Total Assessed Property Valuation



Note: Solid line indicates the marginal effect of literacy test requirement, with 95% confidence intervals in dotted lines. Grey and black lines capture the density of black share.

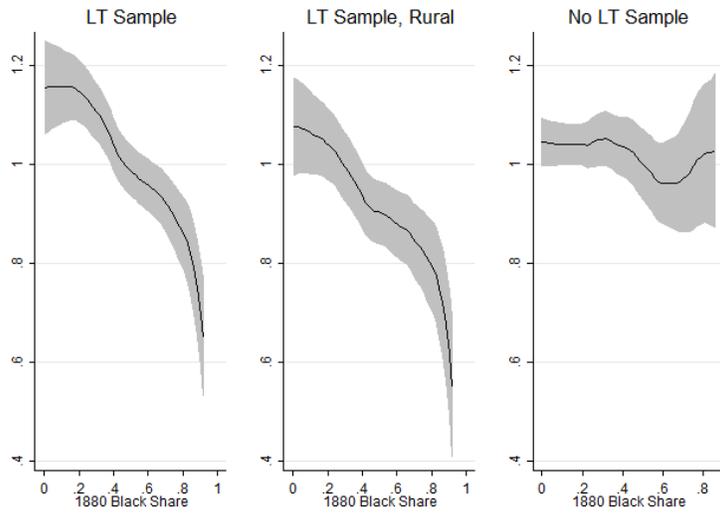
model, the timing of the literacy test is correlated with a 10.1% (1.8% of the sample mean) increase in per capita property assessments in urban areas in literacy-test states. Similarly, rural counties in literacy-test states experienced a decrease of 7.7%. The estimates are reported in Table 11.

Do urban counties drive the observed statistical increase in taxation following the adoption of this restriction? We test this using the long-difference specification. Figure 7 presents the relationship between the change in per capita taxation between 1880 and 1910 against county black-population share for three different samples: i) all counties in the literacy-test states (left - LT Sample), ii) all rural counties in the literacy-test states (middle - LT Sample, Rural), and counties in the non-literacy-test states (right - No LT Sample). The negative relationship between taxation and disenfranchisement is reflected for all values of black share in the literacy-test states. While we do not observe a non-linear relationship, we still see that full LT sample (left) shows that counties under 40% black share experienced an expansion of fiscal revenue by up to 6% relative to similar counties in non-LT states (right). However, when the sample is restricted just to rural counties in LT states (middle), the relative shift upwards in low-black share is no longer statistically different than similar counties in non-LT states. Furthermore, at all levels of county black-population share, change in taxation is decreasing in county black share.

In sum, shifts in the within-state distribution of the burden of taxation explains much of the increase in taxation observed in low black-population share counties following the adoption of the literacy test.

Figure 7

Change in Taxation (1880-1910) Residuals, by Presence of LT



Note: Residuals estimated from linear regression of change of logged taxation per capita against state dummy variables from 1880-1910; smoothed using Nadaraya-Watson nonparametric regression with Epanechnikov kernel (bandwidth chosen by rule-of-thumb estimator). No LT Sample = states in which no literacy restriction (LT) was ever implemented (N=3,600) ; LT Sample = states in which an LT was implemented (N = 2,832).

Furthermore, we show how this was caused primarily by differential changes in assessed property values between high and low black-population share counties that did not occur in non-literacy-test states. This highlights an important mechanism by which redistribution declines following political transitions.

6 Robustness

The county fixed-effects regressions in the previous section provide consistent evidence of a negative relationship between the adoption of a literacy test (and other voting restrictions) on per capita state and local tax revenues and the proportion of a county's population who were African American. By controlling for various county demographic and economic factors, we believe we have mitigated some of the principal sources of selection problems. In addition, the inclusion of county fixed effects and state-specific trends greatly reduces the likelihood that some latent factor accounts for the relationship between state-level voting restrictions and local-level taxation. In other words, such an unobserved factor would have to vary in time to account for county fixed effects, vary across time to account for the inclusion of time trends and vary within states across time to account for the state-specific trends. Nonetheless, consistent estimation of our results depends on an untestable conditional independence assumption: $E[\epsilon_{cst} | \mathbf{x}_{cst}, \delta_c, \mu_t, \epsilon_{st}] = 0$.

In the following section we perform a number of further sensitivity tests to address this and related

concerns. First, we perform a number of additional tests of the implications to the “redistributionist thesis” of democratic reversal. One concern is that per capita tax revenues alone is an insufficient test of this hypothesis. Therefore, we also investigate whether we observe changes in both public spending and tax composition to provide additional evidence that the adoption of the literacy tests restrictions did indeed precipitate a decline in redistribution. Second, we conduct a number of additional robustness tests to assuage concerns that our estimates are biased by omitted factors. Third, we check whether our estimates are affected by comparability issues between counties in the treatment (i.e., literacy test and other suffrage restrictions) and the comparison (i.e., no suffrage restrictions) states. Similarly, we conduct additional tests on whether measurement error is affecting our estimates. Lastly, we check whether migration out of high black-population share counties in treatment states is biasing our results.

6.1 Additional Tests of the Redistributionist Thesis

We first address concerns that declines in tax revenues following the adoption of suffrage restrictions are not a sufficient test of changes in the incidence of redistribution. If, for instance, non-redistributionary spending that mostly benefited elites (e.g., elite universities) fell more rapidly than declines in overall revenues, the level of redistribution may be unaffected. Similarly, taxation, while falling, may become more progressive. We provide evidence against both of these concerns. First, we demonstrate that public spending on education was decreasing in black-population share in literacy test states following this restriction’s adoption. Similarly, we show that the overall composition of state taxes shifted away from direct taxation towards more indirect taxation.

6.1.1 Changes in Redistributive Spending

We now test whether redistributionary public spending also declined following the adoption of the literacy test. It is possible that overall redistribution may not have fallen if the decline in state and local public spending was concentrated primarily on non-redistributionary goods.³⁶ We explore this proposition by analyzing whether the adoption of the literacy test predicts declines in public education spending, which Lindert (2004) identifies as among the most redistributive of public policies.³⁷ Public education spending is also an appropriate test because it represented a substantial share of overall public spending in this period (Hollander 1899). Furthermore, the need for public education spending existed in every county; and, every

³⁶Another possibility is that despite declining tax revenues redistribution levels are maintained through borrowing. This concern is not valid here, as Southern state constitutions prohibited state and local governments from persistently spending more than their revenues.

³⁷While Lindert (2004) ranks public spending on unemployment compensation, housing subsidies and public health as more redistributive in nature, data on these types of spending - if any expenditures on these were made in this era - is unavailable at the county level.

county had existing public support for education that preceded the first time period of our panel.

To capture changes in redistributive public spending, we require measures of public education that are consistent across the various states and are available for all or most of the time frame of our panel. Unlike tax revenues, the federal government did not produce county-level reports of spending on public education. We therefore located and digitized separate reports issued by the each state’s superintendent of education. We focus on measures of public education expenditures that were comparable across state reports (both within states over time and across states). The most consistent measures were total education spending by race and annual teacher salaries by race. In addition to being a measure of inputs rather than outcomes, such as literacy, which could be biased by unobserved factors, these variables should quickly reflect changes in government resources and priorities. As school funding decisions were made either annually or biannually, we might expect expenditures on school inputs to vary immediately following laws that determined their scope. For ease of comparison between areas with different public goods demands, we divide total (state and county) education spending on African Americans by total state and education spending on African Americans and whites combined.

In Column 1 of Table 6 we recover surprisingly similar point estimates that are similar in magnitude and direction as in Table 3, indicating a tight correspondence between public goods spending on African Americans and taxation in this period. Following the introduction of the literacy test requirement, an increase in black-population share by 1% was associated with a *decrease* in total education expenses for African Americans (as a share of total expenses) by 0.4%. This effect is precisely estimated even after we include state-specific time trends and a full set of time-varying covariates. A similar results holds when we focus on the average annual wage of African-American teachers (as a fraction of the average annual wage of any teacher). The introduction of the literacy test requirement in high black-population share counties was associated with a sharp and immediate decrease in African-American teacher salaries. This pattern is similarly robust to controls and state-time trends.

6.1.2 Composition of Taxes

We now investigate whether black disenfranchisement affected the structure of taxation in the American South. In addition to leading to an increase in the overall size of the government, theory suggests that transitions to democracy should effect a shift in the burden of taxation, as well. To wit, [Aidt and Jensen \(2009\)](#) argue that enfranchisement should lead not only to greater redistributive public spending, but also a greater reliance on direct taxes.³⁸ This shift in tax composition is redistributive because direct taxation,

³⁸Specifically, the authors argue that this shift will occur following enfranchisement when the costs of collecting direct taxes is relatively low. This test is not relevant to us, as we would not expect the costs of collecting direct taxes to *increase* between the 1870s, when most taxation consisted overwhelmingly of direct taxes, and the early 20th Century. Put differently,

Table 6
Racial Disenfranchisement and Education Spending

	Total Expenses on Blacks		Annual Wage, Black Teachers	
	Total Expenses		Annual Wage	
	(1)	(2)	(3)	(4)
Literacy Test Req. × Black Share	−0.408*** (0.038)	−0.425*** (0.036)	−0.324*** (0.025)	−0.323*** (0.025)
Literacy Test Req	0.112*** (0.021)	0.099*** (0.021)	0.037*** (0.013)	0.116*** (0.019)
Black Share	−0.287* (0.154)	0.301** (0.145)	0.490*** (0.091)	0.599*** (0.098)
State-specific trends	No	Yes	No	Yes
Controls	No	Yes	No	Yes
Observations	1,633	1,573	908	884
R ²	0.520	0.692	0.937	0.945

Robust standard errors in parentheses.

+ significant at the $p < .1$; * $p < .05$; ** $p < .01$; *** $p < .001$

such as property and income taxes, falls more heavily on the wealthy than indirect taxes.³⁹ We test for this implication of the redistributionist thesis, by analyzing whether the adoption of the literacy test coincides with a shift towards greater reliance on indirect taxation.

Due to data limitations with local-level taxes, we focus on the composition of state taxes levied over time in six literacy test states.⁴⁰ We located state reports issued by state auditors, comptrollers or treasurers, which delineated not only the amount of tax revenues collected in each state, but also their source.⁴¹ We used this to create a variable that measures the share of total state tax revenues that consisted of direct taxes. We coded all ad valorem property taxes, corporate taxes, income taxes and capitation taxes as direct taxes. Our data collection strategy was to find one state report for at least every five years between 1880 and 1910 for each literacy test state; and, we tried to find at least three reports before the adoption of the literacy test and three following its adoption.

Figure 8 shows the trend between 1880 and 1910 in the proportion of each state’s total state tax revenues due to direct taxation. The vertical bar indicates the year when each state adopted the literacy test. While

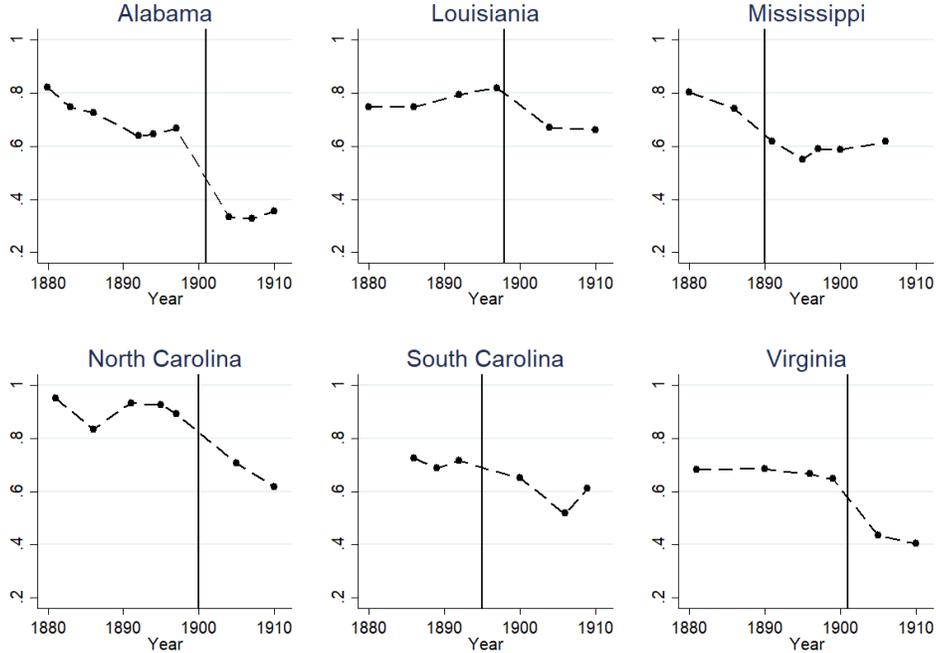
we should not expect a negative technology shock in the collection of direct taxes to coincide with the adoption of suffrage restrictions.

³⁹Aidt and Jensen (2009) define direct taxation as “all forms of taxes that are levied on earnings, incomes, and profits of persons when they are earned. This category includes the (personal) income tax, property taxes, inheritance taxes, assessed taxes, land taxes, and corporation taxes. Indirect taxes are, in contrast, levied on those incomes when they are spent.” Because only the federal government could levy customs taxes, states and local governments could only levy market taxes. In the American South, these primarily included excise taxes on goods, such as alcohol, and professional licensing fees.

⁴⁰We had to construct this dataset because we were unable to locate a source for county-level data delineating the composition of state- or local-level taxes consistently across states or over time. Georgia was excluded because its late date of adopting the literacy test (1906) does not allow use to use the same time frame.

⁴¹These reports, which the state constitutions typically required either annually or biennially, provided detailed information about state finances. Only a few, however, provided any state- or county-level detail of local finances.

Figure 8
Share of State Taxes from Direct Taxation



there is great variation across the states in the pre-literacy test period, the share of state tax revenues derived from direct taxation was quite high and varied between 65% (in Alabama) and 95% (in North Carolina). In the period following the adoption of the literacy test, the direct taxation share fell in each state and never exceeded 70%. While this evidence is not dispositive that shift in the composition of taxation occurred following black disenfranchisement, it is supportive of this prediction. This is also consistent with the evidence presented above of a shift in the burden of state taxation to the low black-population share counties. This shift likely occurred through a greater reliance on professional license fees and excise taxes.

6.2 Robustness - Endogeneity concerns

A principal concern is that our estimates are biased by omitted factors or reverse causality. We therefore conduct a number of tests to provide confidence that we are capturing the fiscal consequences of disenfranchisement. First, using the county-level electoral returns in the referendums asking voters whether to hold a constitutional convention for the purposes of installing suffrage restrictions or the post-convention vote on whether to ratify the constitution that included the disenfranchising restrictions, we run the same model on only those counties in which a majority voted in opposition to these proposals.⁴² As these counties were more

⁴²We use the pre-convention referendum only cases in which the post-convention referendum data is unavailable (usually because a ratifying referendum was not held).

urban and had lower black population share, they are more directly comparable with the counties in states that did not adopt voting restrictions. As a result, these two groups constitute a fixed-reference population to which treatment was assigned in a manner plausibly exogenous to local politics (i.e., due to political processes at the state, but not the local level). As we explain in detail below, this test confirms a differential effect of voting restrictions nearly identical to that in our baseline specification. Next, we perform a placebo test in which we recode voting restrictions as if they had occurred ten or twenty years earlier (Stasavage 2014; Dincecco and Katz 2016). When doing so, we are unable to find a relationship between the lagged ten-year or twenty-year interaction term and contemporaneous tax revenue.

6.2.1 Attitudes Towards Racial Disenfranchisement and Placebo Test

Another concern is that our findings are capturing county-level variation in racial attitudes towards African Americans instead of the effects of *de jure* disenfranchisement. We have argued that due to the inclusion of county fixed effects, this is highly unlikely; changes in county racial attitudes would have to occur simultaneous to the state-level adoption of voting restrictions but be uncorrelated to the inclusion of state trends. Regardless, we test for this possibility by using the county-level support for referenda on whether to enact the suffrage restriction. County-level vote counts for referenda on adopting new constitutions or statutes are available for Alabama, Arkansas, Florida, Georgia, Louisiana, North Carolina, South Carolina and Virginia (Walton, Puckett and Deskins 2012). Our sample, and the spatial distribution of counties in which a majority of voters opposed the disenfranchising restrictions, is shown in Appendix-Figure 14.

Although the estimate is slightly smaller and less precise, Table 7 shows that the onset of state-level voting restrictions was associated with a nearly identical reduction in taxation in counties voting against disenfranchisement as in our baseline specification (Table 3). An increase in black share by one standard deviation was associated with a statistically significant reduction in per capita taxation by 7% for counties in which a majority voted against, yet nonetheless experienced, disenfranchisement. These results provide strong evidence of the negative relationship between disenfranchisement and redistribution is robust to racial attitudes of the county’s voters. In addition, given the likely differences in the structure of taxation and property holdings in these areas (e.g., more urban, more manufacturing) as compared to counties voting in favor of the disenfranchising referenda, these findings highlight the primacy of voting restrictions as a determinant of local levels in per capita taxation.

Next, we perform a simple placebo test to directly address omitted variable bias (Stasavage 2014; Dincecco and Katz 2016). If the observed reduction in taxation in high black population share counties was due to some unobservable trend that preceded our measure of racial disenfranchisement, we might expect the level of lagged racial disenfranchisement ($bs_{cst} \times D_{st-1}$) to be correlated with taxation. As such, we recode each

Table 7*Racial Disenfranchisement and Taxation: Sensitivity to Racial Attitudes*

	(1)	(2)	(3)	(4)
Literacy Test Req. × Black Share	-0.318* (0.131)	-0.400** (0.137)	-0.382** (0.143)	-0.303* (0.142)
Literacy Test Req.	0.145* (0.058)	0.263*** (0.060)	0.251*** (0.062)	0.239*** (0.063)
Black Share	2.449*** (0.444)	1.705*** (0.432)	1.279** (0.451)	1.096* (0.470)
State-specific trends	No	Yes	Yes	Yes
Controls				
Baseline	No	No	Yes	Yes
Additional	No	No	No	Yes
Observations	1789	1789	1740	1740
R^2	0.68	0.73	0.74	0.75
Counties	511	511	499	499

The dependent variable is (log) total tax revenue per capita, in real 1880 \$ USD. All regressions include county- and time-fixed effects. The sample is restricted to county-year pairs in which either the state had no restriction (N=1,497) or a majority in the county voted against the referendum calling for a voting restriction (N=596). Robust standard errors clustered at the county-level in parentheses. The dependent variable is total taxation per capita, in 1910 USD.

* significant at the $p < .05$; ** $p < .01$; *** $p < .001$

restriction as occurring ten or twenty years prior and re-estimate Equation 1 with a full set of fixed effects and controls. Appendix-Figure 15 shows the coefficient of the ten and twenty-year placebo measures. In addition to neither estimate being statistically significant, the sign on the estimate for the 10-year lag is actually positive.

6.2.2 Trends in Taxation including Reconstruction Era Taxation

Thus far, we have restricted our attention to the post-Reconstruction period. Extending the panel to 1870 would introduce a number of limitations. For one, the immediate post-Civil War period initiated a wave administrative changes, including the creation of many new counties; thus, beginning the panel in 1870 reduces the sample of counties by nearly 20%. Second, and relatedly, the Reconstruction Era witnessed an unprecedented expansion in direct federal involvement in the South, including in law enforcement, legal protection and service provision, with indeterminate effects on per capita tax receipts. Finally, comparable data on some of the covariates, especially lynching, is unavailable before 1880.

Despite its limitations, we can use pre-1880 data to provide addition tests of whether the parallel trends assumption for our difference-in-differences models is violated. Specifically, we use data on county-level tax revenues provided by the 1870 Census to extend the panel back one panel decade. First, we provide graphical

evidence of parallel trends in per capita total taxation in Appendix-Figure 13. As the primary comparison of interest is between high versus low black-population share counties *within* each state, we plot literacy test restriction states and no literacy restriction states separately. The figure illustrates a differential upward shift in taxation localized to literacy restriction state counties with 1870 black share below the median (21%), beginning in 1890. Recall that the first state to adopt a LT would do so in 1890. Further, the figure does not indicate any evidence of a coincident fiscal expansion in low-black share counties in the comparison states.

Next, we included 1870 taxation data as a robustness test in Appendix-Table 13. Specifically, Columns 1-3 replicate Table 3, leveraging the interaction between 1870 African-American share and the LT restriction indicator. The results mirror earlier findings - there is evidence of a significant and large negative relationship between black-population share and per capita taxation.

6.3 Robustness - Measurement Concerns

A related concern with the benchmark model is measurement error induced by miscoding the timing of literacy restrictions or the extent of local African-American political participation. To test whether the observed decline in taxation is working through the channel of black disenfranchisement, we test an additional implication: county Republican vote share. Because African Americans in the South overwhelmingly supported the Republican Party, we should observe a differential effect on the decline in Republican vote share in high black population share counties in literacy test states. Reassuringly, Table 8 shows comparable effects of Republican vote share on taxation receipts. Similarly, Appendix-Table 10 indicates a comparable effect when we broaden our focus to any form of racial disenfranchisement. In conjunction, these findings suggest that there is limited bias from either miscoding the extent or timing of disenfranchisement.

We also conduct further tests in this section to assuage concerns of uncommon covariate overlap. First, we re-estimate the main model for a subset of counties matched on a full set of covariates in 1910 using a genetic matching estimator. Table 9 shows that the relationship between voting restrictions and taxation remain robust to nearly all model specifications. Lastly, Appendix-Table 14 indicates the same relationship exists even when the sample is restricted to the 7 states that passed literacy restrictions.

6.3.1 Republican Vote Share

As a secondary measure of African-American political participation (i.e., instead of our proxy, county black population share), we use county-level Republican vote share in gubernatorial elections. As discussed in Section 2, most African Americans supported Republican candidates (or for one of various Fusion or Populist parties), and the vast majority of elected African-American officials were Republican (Foner 1993). If Re-

publican vote share captures African-American political participation, standard theory would suggest that a restriction of the franchise to African Americans would reduce taxation levels in high Republican share areas. We find that this is precisely the case - a 1% increase Republican share is correlated with 1.6% decrease in total taxes per capita in states that adopted literacy tests. Furthermore, the effect of our preferred measure of disenfranchisement, black population share, is still to lower taxation levels in those states.

Table 8
Racial Disenfranchisement and Taxation: Sensitivity to Alternative Measure of Political Participation

	(1)	(2)	(3)	(4)
Literacy Test Req. × Black Share	-0.494*** (0.137)	-0.371** (0.137)	-0.512** (0.170)	-0.456** (0.172)
Literacy Test Req. × Rep. Share	0.018 (0.162)	-0.152 (0.156)	-1.964*** (0.549)	-2.126*** (0.608)
Literacy Test Req. Black Share	0.280** (0.097)	0.501*** (0.095)	0.558*** (0.113)	0.538*** (0.117)
Black Share	1.194*** (0.238)	0.225 (0.266)	-0.217 (0.198)	-0.239 (0.200)
Republican Share	-0.229*** (0.062)	0.125 (0.064)	0.039 (0.049)	0.037 (0.048)
State-specific trends Controls	No	Yes	Yes	Yes
Baseline	No	No	Yes	Yes
Additional	No	No	No	Yes
N	4177	4177	2825	2809
R ²	0.61	0.66	0.76	0.76
Counties	1265	1265	1166	1151

The dependent variable is the growth rate in total tax revenue (per capita in real \$USD) between 1880 and 1910. All regressions include county- and time-fixed effects included. The sample includes 17 states from 1880-1910. Robust standard errors clustered at the county-level in parentheses.

* significant at the $p < .05$; ** $p < .01$; *** $p < .001$

6.3.2 Genetic Matching and Literacy-Test Sample

One concern is that counties in Literacy Test states are fundamentally different than counties in non-restriction states. To ensure common covariate support we construct a dataset with observations matched on black share, baseline and additional controls via the genetic matching estimator (Diamond and Sekhon 2013). Covariate overlap (or positivity in the data) would be violated if certain values of black-population share (or other covariates) existed in counties that passed restrictions but not in comparison counties. Figure 2 provides some graphical evidence that this concern is limited in a regression without covariates. With the inclusion of further covariates, a causal comparison between restriction and non-restriction counties is only

possible if there is identifying variation in taxation levels across values of black share and each covariate. The failure to address covariate overlap can lead to results that are highly model dependent (i.e. not robust to modeling choices (King and Zeng 2005; Samii 2016)).

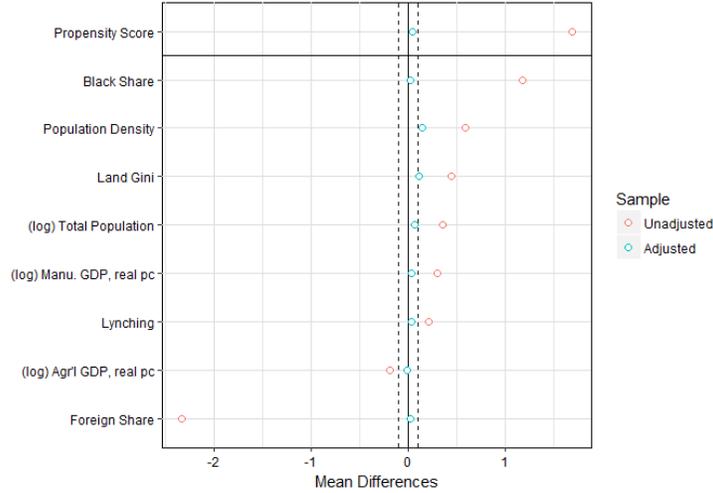
We restrict our attention to 1910 in order to maximize the number of county observations with literacy test requirements. The genetic matching estimator, a generalization of propensity score and Mahalanobis Distance matching, uses a genetic search algorithm to determine weights and then generates as many weighted samples as there are observations in the dataset. Next, the algorithm identifies the sample that minimizes the difference between covariate probability distributions in treatment and control groups (as measured by p-values of Kolmogorov-Smirnov tests and paired t-tests for all variables). The weights from this sample are used to generate new weights and the algorithm iterates until the proposed weights converge (Diamond and Sekhon 2013).

By construction, matching reduces covariate imbalance, which will reduce the degree of model dependence, inefficiency and bias. Figure 9 shows the improvement in covariate balance by comparing the standardized mean difference in covariates before and after matching. The hollow dots capture the standardized mean differences between restriction and no-restriction counties for each covariate. With the exception of population density (for which the standardized mean differences are less than 0.3), none of the mean differences in the matched samples exceed the rule-of-thumb threshold of 0.1 (denoted by the dotted line, see Stuart, Lee and Leacy 2013). In Table 9, we report estimates from Equations 1. The results are consistent with previous findings. For every one standard deviation increase in black share, counties that are in states with a literacy-test restriction witnessed a 12.3% reduction in per capita total tax revenue as compared to counties in states without this suffrage restriction.

As a related approach to ensuring common overlap, we estimate Equation 1 only for the 7 states that would eventually pass literacy test requirements. The coefficient of interest therefore captures the mean difference in tax revenue between counties in states with literacy-test requirements and comparable counties in states that had not yet implemented this voting restriction. In Appendix-Table 14, we find that the mean difference is robust to a full set of fixed effects, state-specific time trends and time-varying covariates. Between 1880-1910, an increase in black share of 1% is associated with a 0.14% reduction in (log) total tax revenue per capita for counties in states with voting restrictions. We also run the same model on the set of counties in the 10 Reconstruction states, and find nearly identical results.

Figure 9

Covariate balance improvement with matching



Note: Genetic matching estimator. Standardized mean differences in covariates between counties with and without literacy restrictions in 1910.

Table 9

Racial Disenfranchisement and Taxation: Sensitivity to Matching

	(1)	(2)	(3)	(4)
Literacy Test Req. ×	-0.787***	-0.524*	-0.421*	-0.512**
Black Share	(0.200)	(0.239)	(0.176)	(0.157)
Literacy Test Req.	-0.040	-1.189***	-1.171***	-0.770***
Black Share	(0.086)	(0.106)	(0.077)	(0.109)
Black Share	0.739***	0.328	0.412*	0.463**
	(0.176)	(0.218)	(0.165)	(0.143)
State-fixed effects	No	Yes	Yes	Yes
Controls				
Baseline	No	No	Yes	Yes
Additional	No	No	No	Yes
Counties	591	591	591	591
R ²	0.09	0.18	0.51	0.55

The dependent variable is 1910 (log) Total Tax Revenue per capita, in real 1880 \$USD. Robust standard errors in parentheses.

+ significant at the $p < .1$; * $p < .05$; ** $p < .01$; *** $p < .001$

7 Conclusion

In this paper, we use the systematic disenfranchisement of African Americans in the US South to empirically explore the relationship between democracy and redistributive taxation. Using county-level data from the US South allows us to contribute to this debate for a number of reasons. By focusing on democratic reversals, we test a clear implication of the canonical redistributionist view of democracy: if changes in suffrage cause the median voter to move up the income distribution, then demand for redistributive taxation should decline. Our case also allows us to study this implication in a period of fiscal expansion, alleviating concerns that we also suffer from a potential source of bias that likely affects the literature’s tendency to focus on suffrage expansions. Moreover, we minimize endogeneity and measurement issues as suffrage restrictions were targeted at a well-defined racial group, allowing us to precisely measure the share of each county’s population that was affected by this shock to democracy. Enormous spatial variation across counties in the share of the population who were African American and the differential timing in each state’s adoption of suffrage restrictions allow us to estimate a difference-in-differences model of the effects of this reversal to democracy on state and local taxes collected in each county. As reported in Table 3, we find strong support that higher black population share counties in states that adopted literacy tests collected significantly lower total per capita tax revenues. An important contribution of this paper is that we not only demonstrate that disenfranchisement leads to lower redistribution, we provide a number of different mechanisms by which this occurs.

These findings are supported by a plethora of robustness tests. First, we estimate the difference in the growth rates in taxation using a long-differences estimator (as reported in Table 5 and Appendix-Table 10). Second, we use the fact that voting restrictions were implemented at the state-level, often against the preferences of certain counties. In Table 7, we show that voting restrictions were associated with lower taxation in high black share counties even when those counties voted against the initial referenda calling for racial disenfranchisement. We also perform a confounding exercise that asks whether a preceding county-specific trend (that also varies with black share) is correlated with taxation. In Appendix-Figure 15, we show that such a trend fails to capture the underlying relationship between voting restrictions and taxation. Finally, we show that our findings are robust to multiple measures of local disenfranchisement (Table 8 and Appendix-Table 12), matching (Table 9) and sub-setting the sample only to states that would experience a literacy test voting restriction (Appendix-Table 14). In sum, our findings provide strong empirical support the the canonical redistributionist view of democracy.

This paper also contributes to a long-running debate on the welfare implications and significance of

the adoption of suffrage restrictions in the US South. Key's (1984) "fait accompli" thesis argues that *de jure* disenfranchisement had little effect, and rather *de facto* disenfranchisement, which was accomplished by violence, electoral fraud and intimidation, was already in effect. Other scholars, however, have argued that while black political power was diminished in the post-Reconstruction era, African Americans remained both enfranchised and politically active. Our evidence supports the latter view. In this respect, our findings complement previous work on the devastating effects of voting restrictions on African-American welfare, and, in particular, on human-capital accumulation (Naidu 2012; Margo 1990). This speaks to the importance of formalized political inequality, even in a context of large disparities in *de facto* power and resources. *De jure* disenfranchisement removed one of the few remaining tools African Americans in the South possessed to promote their interests. Instead, they were largely reduced to revolutionary violence or migration. As we saw in the period immediately following the Era of Disenfranchisement (and after our period of study), millions of African-Americans participated in what became known as the Great Migration to the North. This enormous exodus saw the share of the nation's African Americans living outside the South to rise from 10% in 1910 to approximately 50% by 1970.

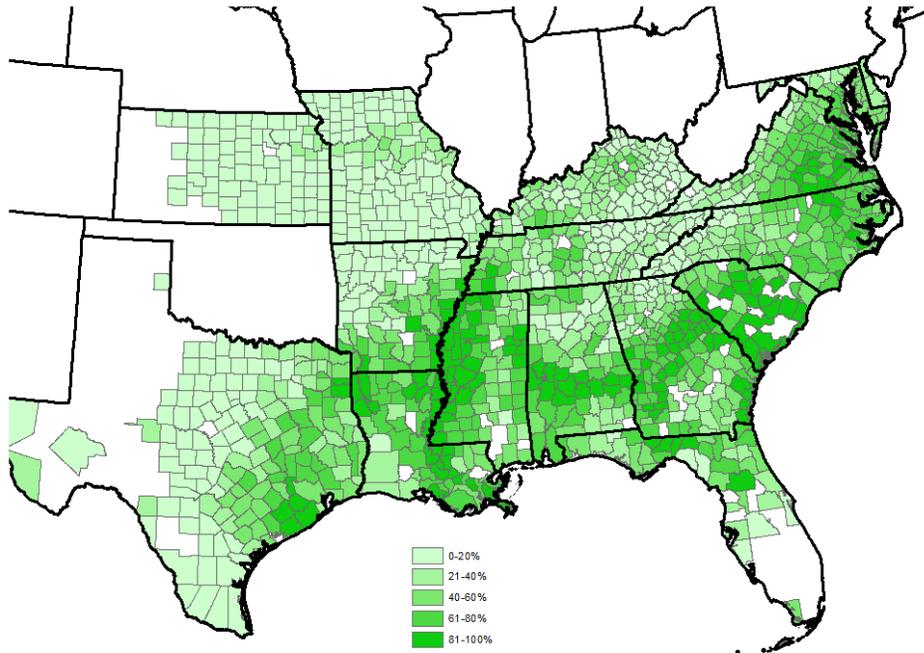
Acknowledgments

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8 Appendix

Figure 10

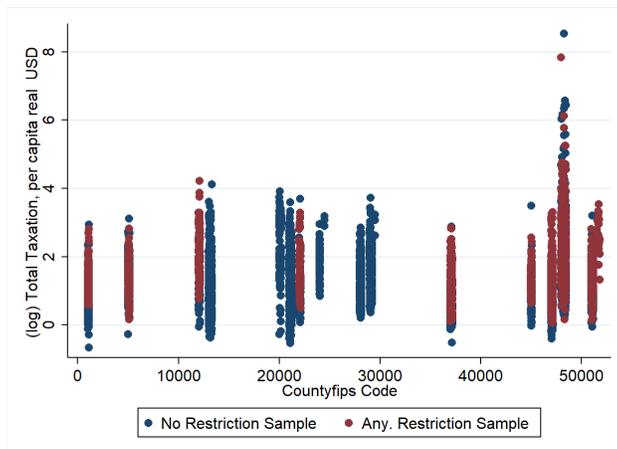
African-American Population Share, 1880



Note: Source: 1880 US Census

Figure 11

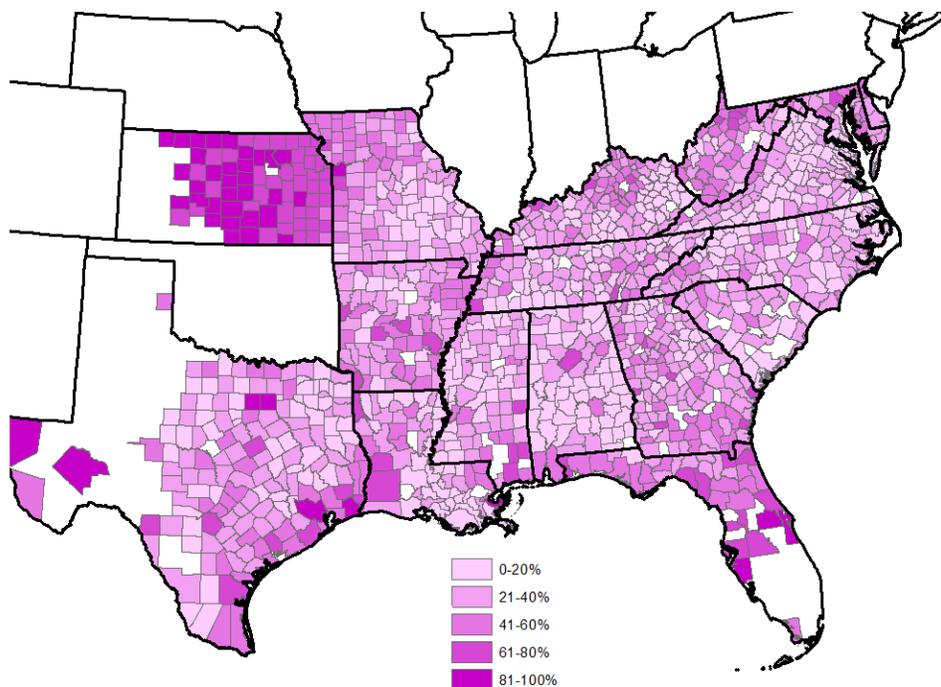
(log) Total Tax Revenue (1880-1910), in real 1880 \$ USD per capita



Note: Each dot represents a county-year pair and vertical lines represent states in the sample, arrayed by thousands of state's FIPS code. For example, all values above 1000 are counties in Alabama, whose FIPS codes range between 01001 and 01133. Source: 1880 Census and the Census' *Wealth, Debt and Taxation* reports for 1890, 1902 and 1912.

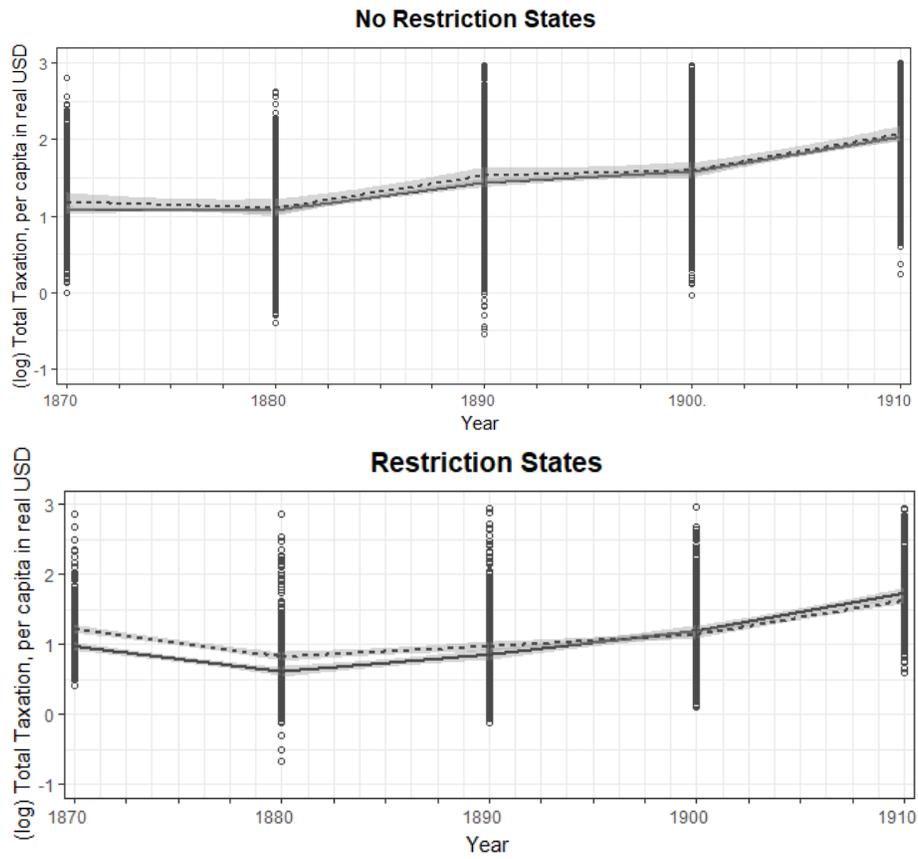
Figure 12

% Δ (log) Total Tax Revenue per capita, in real 1880 \$ USD



Note: This figure captures the growth rate in tax revenues from 1880-1910. Source: 1880 Census and the Census' *Wealth, Debt and Taxation* reports for 1890, 1902 and 1912.

Figure 13
Trends in (log) Total Taxation, per capita real 1880 \$USD



Note: Trends in all state and local (county and municipal) taxes, by year and literacy test restriction status. See Table 1 for list of states by restriction status. Thick dark lines represent areas with 1870 black share below the median (21%); scattered lines above median 1870 black share counties.

Table 10
Any Voting Restriction and Taxation

	Pre-treatment (1880) Covariates		Time-varying Covariates	
	(1)	(2)	(3)	(4)
Any Restriction	0.240*** (0.031)	0.268*** (0.036)	0.206*** (0.028)	0.191*** (0.036)
Any Restriction × Black Share ₁₈₈₀	-0.207** (0.064)	-0.217*** (0.065)		
Any Restriction × Black Share			-0.172** (0.059)	-0.163** (0.059)
Black Share			0.153 (0.218)	0.121 (0.217)
State-specific trends	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Any Restriction × Controls	No	Yes	No	Yes
Observations	4634	4634	4788	4788
R^2	0.71	0.70	0.72	0.72
Counties	0.71	0.70	0.72	0.72

The dependent variable is (log) total tax revenue per capita, in real 1880 \$ USD. All regressions include county-fixed effects as well as state-specific linear trends. Even numbered columns further condition on baseline and additional covariates - each individually and interacted with the any restriction indicator. Covariates include (log) per capita, real 1880 \$ USD manufacturing output, (log) per capita, real 1880 \$ USD agricultural output, land Gini, (log) population density, foreign share and total lynching deaths. The sample includes 17 states from 1880-1910. Robust standard errors clustered at the county-level in parentheses. * significant at the $p < .05$; ** $p < .01$; *** $p < .001$

Table 11
Urban Counties and Assessed Valuation

	(log) Assd. Valuation, Real USD per capita			
	Long-Diff Model		Diff-in-Diff Model	
LT Sample	0.564***	0.232***		
× Urban Sample	(0.118)	(0.063)		
LT Sample	-1.796***	1.878***		
	(0.200)	(0.403)		
Urban Sample	-0.401***	0.002		
	(0.107)	(0.041)		
1880 Black Share		0.001		
		(0.079)		
LT × Urban Sample			0.098*	0.101**
			(0.038)	(0.034)
LT			-0.019	-0.077***
			(0.030)	(0.019)
Black Share			0.351	0.564*
			(0.308)	(0.245)
State FE	No	Yes		
State specific trends			No	Yes
Controls	No	Yes	No	Yes
Observations	1223	1118	5179	4688
R^2	0.20	0.24	0.17	0.42
Counties	1223	1118	1407	1306

Robust standard errors in parentheses.

+ significant at the $p < .1$; * $p < .05$; ** $p < .01$; *** $p < .001$

Table 12
Any Voting Restriction and Taxation: Long Difference Estimates

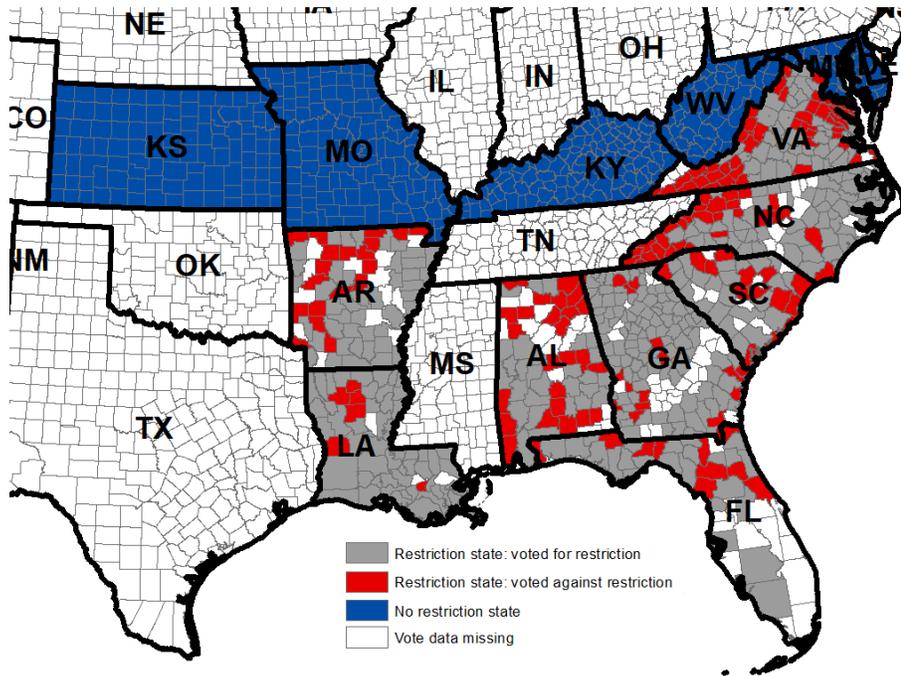
	(1)	(2)	(3)	(4)
Any Restriction ×	0.127	-1.528***	-1.188***	-1.170***
Black Share ₁₈₈₀	(0.310)	(0.341)	(0.335)	(0.330)
Any Restriction	-0.308***	-1.122***	0.332*	0.323*
	(0.065)	(0.082)	(0.141)	(0.139)
Black Share	-0.543	1.249***	1.072***	1.135***
	(0.295)	(0.326)	(0.324)	(0.323)
State-fixed effects	No	Yes	Yes	Yes
Controls				
Baseline ₁₈₈₀	No	No	Yes	Yes
Additional ₁₈₈₀	No	No	No	Yes
N	1106	1106	1057	1057
R^2	0.09	0.38	0.42	0.42
Clusters	1106	1106	1057	1057

The dependent variable is the growth rate in total tax revenue (per capita in real \$USD) between 1880 and 1910. The sample includes 17 states from 1880-1910. Robust standard errors clustered at the county-level in parentheses.

* significant at the $p < .05$; ** $p < .01$; *** $p < .001$

Figure 14

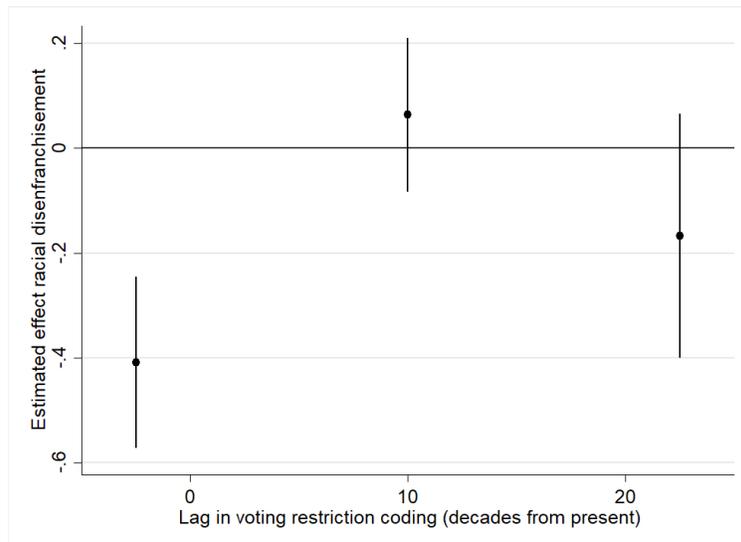
Sample restricted to areas opposed to voting restrictions



Note: Source: See main text

Figure 15

Placebo Test: Effect of Lagged Disenfranchisement



Note: Each line represents a regression similar to Table 1, with the timing of the literacy test requirement lagged. Solid dots represent parameter estimates of the main coefficient of interest: contemporaneous black share interacted by presence of literacy test requirement. Solid lines represent 95% confidence intervals.

Table 13*Racial Disenfranchisement and Taxation (1870-1910)*

	(1)	(2)	(3)
Literacy Test Req. \times Black Share ₁₈₇₀	-0.597*** (0.086)	-0.580*** (0.091)	-0.226* (0.095)
Literacy Test Req.	0.144** (0.044)	0.516*** (0.048)	1.403*** (0.171)
State-specific trends	No	Yes	Yes
Controls ₁₈₇₀	No	No	Yes
Literacy Test Req. \times Controls ₁₈₇₀	No	No	Yes
Observations	5810	5810	5309
Adjusted R ²	0.58	0.64	0.65
Counties	1220	1220	1103

The dependent variable is (log) total tax revenue per capita, in real 1880 \$ USD. All regressions include county-fixed effects. Column 1 further includes year-fixed effects, while Columns 2-4 include state-specific linear trends and controls (excluding lynching). The sample includes 17 states from 1870-1910. Robust standard errors clustered at the county-level in parentheses.

* significant at the $p < .05$; ** $p < .01$; *** $p < .001$

Table 14*Racial Disenfranchisement and Taxation: Restricted to Literacy Restrictions Sample*

	(1)	(2)	(3)	(4)
Literacy Test Req. \times Black Share	-0.509*** (0.077)	-0.513*** (0.086)	-0.529*** (0.087)	-0.375*** (0.087)
Literacy Test Req.	0.886*** (0.040)	0.303*** (0.046)	0.303*** (0.045)	0.240*** (0.045)
Black Share	1.042* (0.439)	0.832* (0.340)	0.980** (0.330)	0.373 (0.325)
State-specific trends	No	Yes	Yes	Yes
Controls				
Baseline	No	No	Yes	Yes
Additional	No	No	No	Yes
N	2304	2304	2197	2183
R ²	0.34	0.70	0.71	0.73
Counties	608	608	584	570

The dependent variable is the growth rate in total tax revenue (per capita in real \$USD) between 1880 and 1910. The sample includes 7 Southern states that passed literacy test requirements from 1880-1910. Robust standard errors clustered at the county-level in parentheses.

* significant at the $p < .05$; ** $p < .01$; *** $p < .001$

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