Suffrage, Schooling, and Sorting in the Post-Bellum U.S. South.*

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May 25, 2012

Abstract

This paper estimates the political and economic effects of the 19th century disenfranchisement of black citizens in the U.S. South. Using adjacent county-pairs that straddle state boundaries, I examine the effect of voting restrictions on political competition, public goods, and factor markets. I find that poll taxes and literacy tests each lowered overall electoral turnout by 8-22% and increased the Democratic vote share in elections by 1-7%. Employing newly collected data on schooling inputs, I show that disenfranchisement reduced the teacher-child ratio in black schools by 10-23%, with no significant effects on white teacher-child ratios. I develop a model of suffrage restriction and redistribution in a 2-factor economy with migration and agricultural production to generate sufficient statistics for welfare analysis of the incidence of black disenfranchisement. Consistent with the model, disenfranchised counties experienced a 3.5% increase in farm values per acre, despite a 4% fall in the black population. The estimated factor market responses suggest that black labor bore a collective loss from disenfranchisement equivalent to at least 15% of annual income, with landowners experiencing a 12% gain.
JEL Codes: N30, O15, H70

*I thank Morgan Kousser, Jeremy Atack, Jim Snyder, and Michael Haines for data. Raj Arunachalam, Jesse Driscoll, Jeremy Dittmar, Arindrajit Dube, Oeindrila Dube, Ethan Kaplan, Claudia Goldin, Danny Hidalgo, Rick Hornbeck, Benjamin Lava, Trevon Logan, Bob Margo, Lindsay Mayka, Ted Miguel, Neal Richardson, James Robinson, and Noam Yuchtman all provided helpful comments. I also thank Edward Copenhagen and the staff at the Gutman Library Special Collections and Hi-Tech BPO. Monali Agarwal, Natalie Bau, Ferran Elias, and Raul Sanchez de la Sierra provided excellent research assistance. Financial assistance from the Weatherhead Center for International Affairs and the Canadian Institute for Advanced Research is gratefully acknowledged. All resulting mistakes are my own.

1 Introduction

A core question in political economy is the economic incidence of democracy. Who wins and who loses from changes in democratic institutions? Authors from Barrington Moore (1966) to Acemoglu and Robinson (2006) have argued that economic interests are pivotal in the social decision to extend the electoral franchise. In particular, scholars have argued that landowners have historically been hostile to democratic transitions, owing to the economic effects of democracy on agricultural land and labor (Rueschemeyer et al., 1992). In this paper, I test this idea by estimating local factor market responses to changes in 19th century Southern U.S. political institutions and redistribution. The reduced form estimates together with a simple model quantify how much disenfranchisement altered the distribution of income across factor owners.

Between 1870 and 1910, eleven Southern states passed legal restrictions on voting, such as poll taxes and literacy tests, which were aimed at lowering black electoral participation, but also affected poor whites. These suffrage restrictions in the U.S. South provide a unique opportunity to study the direct and indirect economic effects of changes in the electoral franchise. The effect of these laws on political competition, public good provision, and factor markets is estimated using a panel of counties matched into adjacent county-pairs that straddle state boundaries.[1] This spatial-discontinuity based identification strategy controls for a variety of institutional features that may confound cross-country analyses, as well as unobservable variables that could bias within-country, cross-state analyses. Historical data for counties that lie on state borders allow me to examine how changes in voting rights alter the mix of public goods available. In the empirical analysis, I also address general equilibrium and spillover effects that may arise from focusing on economically integrated treatment and control counties.

To motivate the estimation strategy and interpret the results, I provide a simple 2-factor political economy model, linking political institutions and redistribution with production and endogenous migration. The model predicts that following disenfranchisement, taxation and redistribution falls, inducing black outmigration in the spirit of interjurisdictional competition (Tiebout, 1956) and an increase in the value of land, despite higher wages. The model can be used to generate formulas that relate the land price and migration responses to the economic incidence of disenfranchisement on landowners and black labor.

Using the contiguous county-pair identification strategy, I find that poll taxes and literacy tests lower turnout, increase the Democratic party vote share, and lower the teacher-child ratio

[1] This identification strategy has been used most recently by Dube et al. (2009) to estimate local labor market effects of minimum wages. The research design addresses concerns about omitted variables, such as labor market conditions, that vary smoothly across political boundaries.

for blacks, with no effect on the ratio of white teachers to children. My results on turnout and partisan voting are consistent with historical evidence that these disenfranchisement laws independently lowered black political participation.[2] The consequent fall in black educational inputs is consistent with many theoretical political economy models, including the one in this paper, as well as existing research.

Besides public goods, the focus on economically similar counties also makes it possible to estimate the effects of disenfranchisement on land and labor markets. Despite the outmigration of black labor, the value of land increased in disenfranchised counties relative to adjacent counties where black Americans could vote more easily. While the empirical evidence highlights lowered redistribution as a main channel, landowners may have also benefited from increased segregation and discrimination in the labor market following black disenfranchisement. Since land values are an asset price that reflect actual and expected future profits, this result suggests that landowners were beneficiaries of restricted voting in the U.S. South.

With some assumptions and auxiliary estimates, the land price and migration results can be used to calculate average gains and losses for landowners and black workers. Unsurprisingly, black citizens, via reduced access to public goods as well as potentially many other discriminatory policies, bore the brunt of the welfare losses from disenfranchisement; my results suggest that poor whites benefited little, while landowners gained substantially. These combined results shed light on key debates in Southern political history about whether poor or rich whites benefited the most from changes in Southern political institutions (Woodward, 1951; Kousser, 1974; Perman, 2001).

The political economy of democratic and non-democratic institutions has been the subject of much recent economics literature.[3] Recent theoretical work has modeled the motivations for landlord opposition to democracy (Llavador and Oxoby, 2005; Acemoglu and Robinson, 2006; Galor et al., 2009). One explanation given is that the incidence of redistribution on an inelastic factor such as land motivates landowners to oppose extending the franchise. In examining

[2]That the laws even affected political outcomes such as voter turnout is controversial. V.O. Key (1949) held that the laws disenfranchising poor and black Southerners were largely rubber-stamping a *de facto* situation of low black and poor white turnout, suggesting that black political exclusion was a "fait-accompli" before 1890. Acemoglu and Robinson (2008) echo this perspective in their paper arguing that Southern elites used *de facto* power to maintain control despite the constraints of formal elections. Scholars since Kousser (1974) have documented voting laws' impact on turnout using larger datasets (Heckelman, 1995; Redding and James, 2001), although no paper has attempted to construct valid county-level control groups. The focus of the empirical literature on disenfranchisement has been using ecological regressions (Kousser, 2001) to infer the extent to which blacks were disenfranchised relative to whites.

[3]A large cross-country literature is devoted to estimating the effects of democratic institutions on economic growth, redistribution, and inequality (Barro, 1996; Rodrik, 1999; Przeworski, 2000; Mulligan et al., 2004; Gradstein and Milanovic, 2004).

the effects of political reforms on economic outcomes, this paper is perhaps closest to Baland and Robinson (2008), who look at the effect of introducing the secret ballot on employer vote buying, and find that land prices fell more in high-*inquilino* (roughly sharecropper) Chilean communas post-reform, as land no longer capitalized the ability to control *inquilino* votes. A few recent papers in economics have looked at Southern history for insights into the political economy of development, although none use the same data or identification strategy as this paper. Besley et al. (2010) look at state-year variation in the abolition of suffrage restrictions to estimate the effect of political competition on state-level economic growth. However, their sample period is well after the one considered here, and they do not consider the same margins of factor markets and public good provision. Similarly, Husted and Kenny (1997) examine the effect of voting restriction abolition on the size of government. Miller (2008) finds that laws extending the franchise to women induced improvements in public health. All of these papers study the 20th century removal of voting restrictions across US states, while this paper looks at their post-Civil War implementation within the South and is able to control for a substantial amount of unobserved variation by looking at counties just across state boundaries.

The paper proceeds, in Section 2, by reviewing the relevant literature and background on Southern history, describing the mechanisms tested in the paper. Section 3 provides a simple model that generates predictions about the effect of disenfranchisement on redistribution, migration, and land prices under different assumptions about the labor market. The model also generates simple formulas relating the reduced form estimates to welfare. Section 4 explains the data and empirical strategy, and discusses potential biases. Section 5 presents results and a variety of robustness checks. Section 6 calculates the incidence of disenfranchisement across different groups and Section 7 concludes.

2 Historical Context

The American Civil War (1861-1865) and the subsequent military occupation and Reconstruction[4] overturned Southern society, freeing and enfranchising a vast number of slaves with the 13th, 14th, and 15th amendments to the constitution.[5] Many of these institutional changes were implemented at bayonet-point, and only lasted in practice so long as the Union army remained willing to enforce the new constitutional amendments. With the withdrawal of Union troops in in 1870s, the rollback of black voting and civil rights began (also known as Southern "Redemption"). However, while the Democratic party, at the time controlled by elite Southern whites,

[4]Reconstruction generally refers to the period between 1863 and 1877, when the Union army occupied the Southern states that had attempted to secede.

[5]While the 13th amendment abolished slavery and the 14th amendment guaranteed ex-slaves constitutional rights, it is the 15th amendment that expanded the franchise to all male citizens over the age of majority.

successfully dominated state-level elections, it still had to contend with political competition from Republicans, Populists, and politically organized blacks. The federal government continued to prosecute white electoral fraud and violence. Locally, black officials and politicians, often Civil War veterans, still held a measure of political power. Legal disenfranchisement, running into the early 20th century, was designed to eliminate this remaining electoral competition, and it further reduced government expenditure on black services such as schools.

2.1 Black Political Participation After the Civil War

Black Americans enjoyed a brief window of participation in electoral politics immediately following the Civil War. The extension of the franchise under military occupation did not, unsurprisingly, convince all Southern whites that ex-slaves should be allowed to participate in politics. The presence of Northern troops allowed the Republican party to incorporate blacks as a key political constituency despite widespread white hostility towards black enfranchisement. Once states were admitted back into the union, elections were fiercely contested, with the Democratic party platform centering on returning the South to "home rule".

Black participation in elections during Reconstruction was extensive, and overwhelmingly partisan. The Republican Party was the vehicle for black claims on the state for civil and political rights, as well as whatever redistribution they could secure. As late as 1876, black male turnout rates in Louisiana and South Carolina (the two states which have voting data by race) were 75% and 78% of the eligible population (King, 2001, 2002). In fact, turnout rates during Reconstruction were much higher than in the next 50 years.

Political participation also brought representation. Foner (1988) lists 18 African Americans who served as state executives during Reconstruction. But the presence of black officials in local government was also pervasive, with blacks serving as justices of the peace, county commissioners, and sheriffs. Foner writes that "In virtually every county with a sizable black population, blacks served in at least some local office during Reconstruction ... assumed such powerful offices as county supervisor and tax collector, especially in states where these posts were elective." South Carolina, in 1868, had 74 black legislators out of 128, and it retained a majority black legislature until 1876.

With effective representation came redistribution, particularly in the form of public goods provision. Education was by far the most important of these, and the most sensitive to racial tensions. Schooling black Americans was a particularly sharp break from the pre-Civil War pe-

riod, and one opposed by many Southern whites, both rich and poor. However, the Freedman's Bureau[6] and Republican state education superintendents saw public education as necessary for ex-slaves to gain human capital, as well as a substitute for politically infeasible land reform. An educated black population was also seen as an electoral block that would politically favor the Republican Party. Republican educational superintendents levied property taxes to pay for an expanded education system, sometimes having to build school administrations completely from scratch. While constrained by hostility to taxes and racial integration, "Republicans had established, for the first time in Southern history, the principle of state responsibility for public education" (Foner, 1988).

2.2 Redemption and Disenfranchisement

Southern Redemption in the mid-1870s began reversing many of the gains made by blacks during Reconstruction. Northern troops were gradually withdrawn, allowing the Ku Klux Klan and White Leagues, together with less organized white violence (often called electoral "bulldozing") to coerce black voters. The resulting tilt in the balance of power restored the Democrats to power beginning in the early 1870s, a process completed with the 1876 Hayes-Tilden electoral compromise. Gerrymandering, local electoral changes and continued electoral fraud and force kept the Democrats in power. This came along with increased political representation of agrarian landowners and their favored policies. Politicians cut taxes, reduced expenditure, and passed a slew of labor and tenancy laws that clearly favored landowners at the expense of merchants and workers (Woodman, 1995). Redemption was a large blow to the political and economic rights of Southern blacks in particular. "In illiteracy, malnutrition, inadequate housing, and a host of other burdens, blacks paid the highest price for the end of Reconstruction and the stagnation of the Southern economy" (Foner, 1988).

Despite this reassertion of white political power, Redemption did not create the "Solid South": effective one-party rule had to wait until after the franchise restrictions studied in this paper. Kousser (1974) writes that "The methods that the Democrats had employed to end Reconstruction had not caused either turnout or opposition to cease by 1880." Black representation, even at the federal level, persisted after Redemption. George Henry White, a black congressman from North Carolina, served from 1897 to 1901, right before the state passed suffrage restrictions. Ten other Republican black congressmen, from North and South Carolina, Virginia, and Mississippi served in Congress between 1877 and 1900 (Middleton and Smith, 2002). In North Carolina's Second Congressional District "hundreds of other blacks held lesser

[6]The Freedman's Bureau was a Federal agency that was intended to solve many of the problems faced by newly freed slaves, such as securing jobs and housing, but was disbanded in 1868.

positions" (Anderson, 1980). While the Republican party was severely diminished and blacks were largely denied direct representation, political competition still existed. Even when black votes were controlled or manipulated, it often involved making transfers to black political brokers or having to engage in a panoply of electoral manipulation tactics. Goldman (2001) provides data showing that federal prosecutions of 15th amendment violations continued with equal vigor post-1877, and this was a perpetual concern to Democrats during the 1880s. To secure votes of blacks, patronage and public goods still had to be distributed, if perhaps in diminished quantities, and black legislators still "introduced resolutions that expressed black desires and demands for education" (Moneyhon, 1985) into the 1890s.

The 1890s, sometimes referred to as the "Restoration", marked the beginning of uncontested Democratic rule and racial segregation enshrined in law, either via legislative statutes or constitutional amendments that restricted the franchise. Historians and political scientists have discussed a number of motivations for legal disfranchisement. The first is a response to a wave of political challenges to the Democratic leadership. The Populist party in Georgia and North Carolina, fusion tickets between Republicans and Populists in Louisiana and South Carolina, and rival Democratic factions in other states all posed threats to the propertied whites that ran the Democratic party. The potential black votes to be tapped by "opportunistic" whites were a threat to the existing political elite. The hazard posed by black political participation became clear with the large gains made by rivals to the Democratic leadership during the 1880s, when Independent and Republican parties won between a third and half of the vote in gubernatorial elections. This was a far cry from the overwhelming dominance Democrats would exercise post-1910, when turnout and non-Democratic votes were abysmally low until after the Civil Rights movement.

One reason why Redemption did not completely eliminate black voting was that the 15th amendment came with a set of Enforcement Acts, designed to allow federal agents to police elections and repress Klan political violence. Recent scholarship has shown that effective and widespread federal enforcement of the 15th Amendment did not end after Redemption. Instead Cresswell (1987), Wang (1997), and Goldman (2001) have all shown that illegal electoral practices were prosecuted by the federal government into the 1890s. The Lodge "Force" Bill of 1890 would have increased the federal supervision of elections, and was the last Republican attempt to use the Federal government to secure black voting rights.

National Democrats successfully blocked passage of the Lodge bill, and proceeded to repeal the 1870 Enforcement acts in 1894. Republicans, preoccupied with Northern economic issues and foreign policy, acquiesced. With the abandonment of Republican support for black suffrage,

Southern states were free to legally restrict the franchise without worrying about federal intervention. The Republican retreat from federal enforcement of electoral law is often given as a reason for the particular timing of Southern disenfranchisement. Importantly for my identification strategy, this was national legislation, and therefore exogenous to the county and state-level variation considered in this paper.

A third postulated determinant of black disenfranchisement is that the laws were just one of a set of social institutions implemented in the late 19th century as the South experienced a nadir of race relations. In this story, a cultural tide of anti-black sentiment swept whites in the South, generating a wave of lynchings, even more formal segregation laws, and formal political exclusion. Conjectured causes of the wave of racial tension include demographics (Rabinowitz, 1978), lowered transportation and communication costs (Redding, 2003), and changing Northern ideas about black economic and political rights (Richardson, 2001). Again, much of this variation is at the national or regional level, and, to the extent that it varies within the South, it is unlikely to vary within the contiguous county pairs used in this paper. One strength of the identification strategy is that cultural trends that may be correlated with disenfranchisement are effectively controlled for.

2.2.1 Disenfranchisement Laws

The details of disenfranchisement varied from state to state. For example, in some states disenfranchisement was enacted via statute, while in others it was enacted by constitutional amendment. Figure 1 presents the pattern of disenfranchisement over time, and shows few reversals of the overall trend to disenfranchise. Generally, the poll tax required one to show a receipt for payment of the poll tax prior to voting registration.[7] While it was no more than 2 dollars, other features of the poll tax administration and context made it much more onerous than just the sum of money involved, although that alone was clearly costly for cash-strapped sharecroppers. Firstly, they often had to be paid between 9 and 6 months before the election. Secondly, in some states the tax was cumulative, so that all poll taxes for all preceding years of residence had to be paid before registering to vote.

Literacy tests generally consisted of a requirement to read a section of the national or state constitution prior to voting registration. In the low-education environment of the rural U.S. South, the literacy test was likely to bind for a large segment of the population, particularly for poor blacks and whites. Georgia's literacy test, for example, entailed being able to correctly

[7] Ogden (1958) summarizes the poll taxes as they existed in the early 1950s.

read and write any paragraph of the state or U.S. Constitution. Virginia's test involved a citizen applying to the registrar "in his own handwriting" (Key, 1949). While there were a variety of other pieces of disenfranchisement legislation passed simultaneously, the poll tax and literacy tests were the most important for restricting voting, and were the object of the 1965 Voting Rights Act.

2.3 Economic Effects of Disenfranchisement

> *There is no surer way to drive the best of them [Black Americans] from the state than by keeping up this continual agitation about withdrawing from them the meager educational opportunities that they now have. Their emigration in large numbers would result in a complication of the labor problem.* -J.W. Joyner North Carolina School Superintendent 1910

Attributing the dismal state of Southern black education to the restricted franchise has a long history. Horace Mann Bond (1934), Charles Dabney (1936), and Louis Harlan (1968) all linked discrimination in public education provision to political exclusion of African Americans. Statistically, the fall in the quality and quantity of black education following disenfranchisement has been shown in a number of papers. Margo (1982) uses Louisiana data and finds that disenfranchisement increased racial gaps in school expenditure. Kousser (1980) and Pritchett (1989) find similar results in North Carolina, where racial gaps in education expenditures jump sharply after disenfranchisement. As a counterfactual, Kentucky, the only state without voting restrictions, in fact passed an 1881 referendum proposal to equalize black-white schooling expenditures, although it is unclear how binding it was. The unequal provision of schooling in the South, and its consequences, has a comprehensive treatment in Margo (1990) and Collins and Margo (2006), complementing a large literature studying segregated schooling and wages (Welch, 1973; Orazem, 1987; Fishback and Baskin, 1991; Card and Krueger, 1992).

The economic history of the post-bellum U.S. South contains a vibrant debate on the extent of black labor market mobility (Higgs, 1977; Wright, 1986; Alston and Kauffman, 2001; Ransom and Sutch, 2001). While many authors argue that black labor was mobile, others suggest that non-economic mechanisms such as laws or paternalist norms kept black labor at least partially immobile. Nonetheless, blacks moved regularly during Reconstruction and likely even later (Shlomowitz 1984).[8] The role of mobility and interjurisdictional competition in providing the efficient level of local public goods has been an influential idea in economics since Tiebout and

[8] See the results in Appendix Table A4-A5.

recent research has integrated the local political economy of taxation and public good provision into models of interjurisdictional sorting (Epple and Romer, 1991). These themes were brought together by Margo (1991), who found that labor market mobility induced local jurisdictions in the South to provide some level of education to disenfranchised blacks.

Margo documents that black citizens migrated in response to school quality, and writes that "Although black parents could not vote at the ballot box, they could, and did, vote with their feet in search of schools for their children." In Lowndes county, Alabama, Foner and Lewis (1980) find "there [were] perhaps a hundred Negro farmers ... Not one of these men has been attracted away ... they remain on account of the good schools for their children." (Foner and Lewis, 1980, p. 241)

Political forces and redistribution were motivations for migration more generally. Faced with the loss of political representation and civil rights after Redemption, the first of the notable migration waves out of the South began. While small in actual numbers, the famous Kansas exodusters were the first large scale migration of blacks out of the South, in 1879-1880. Contemporaneous newspapers were stunned at the large movement of blacks out of the historically slave states. "Kansas Fever" was used to describe the understandable migration response to the *de facto* loss of civil and political rights that accompanied Redemption (Painter, 1992). While the magnitudes involved in the Kansas migration wave turned out to be fairly small (Cohen, 1991), it had an effect on national politics, even instigating the formation of a Senate Committee on the Colored Exodus. The later Indiana Exodus was portrayed in the Northern press as being explicitly for the purposes of winning political representation and redistribution (Richardson, 2001). Hahn (2003) writes that black migration between Southern states leapt during the 1880s and 1890s, consistent with my argument in this paper, and that they were hoping for "better schools, better economic prospects, and better social circumstances."

Landlords benefited from the restricted franchise in a variety of ways. Property owners reaped any tax savings allowed by reducing government expenditures on black public goods, and the elimination of political competition allowed for a slew of additional policies that favored rural landowners. Insecurity of black person and property, allowed employers to offer protection rather than increased wages to retain black labor (Alston and Ferrie, 1999). Legal labor mobility restrictions (Naidu, 2010; Cohen, 1991; Bernstein, 2001) as well as an extensive convict leasing system (Lichtenstein, 1996) kept agricultural labor costs low. In addition, the weak political institutions and one-party system of the post-disenfranchisement South allowed the easy translation of landowner wealth into political influence, ensuring extensive representation of landowners at all levels of government. At the federal level, disenfranchisement guaranteed

an elite white lock on Senate and House representation, and landowners were effectively able to stymie federal intervention into the Southern political economy. As an example, Alston and Ferrie (1999) suggest that these Southern Democratic representatives were able to dilute the impact of many New Deal welfare programs, which they did to protect the private labor-market paternalism that guaranteed a steady workforce to agricultural employers.

The economic effects of disenfranchisement on poor whites is a controversial area in Southern history. While it is clear that some poor whites were disenfranchised by the laws, some states and counties were selective in enforcing them or had "grandfather clauses" that enabled whites of all classes to vote. However, while existing evidence suggests that whites as a whole benefited from a superior public-goods and taxation package following black political exclusion, scholars differ over the distribution of these benefits between rich and poor whites.

3 A Simple Model of Suffrage Restriction

In order to generate predictions and interpret the empirical results, I model disenfranchisement in a jurisdiction with taxes and race-specific public goods chosen under probabilistic voting, with competitive factor markets and endogenous migration. Poll taxes and literacy tests increase the costs of voting for black citizens, which lowers their probability of being pivotal, decreasing the weight politicians put on their welfare and lowering taxes on land and black public goods. Factor markets respond, with black labor leaving in response to the lowered redistribution and land prices rising, reflecting the lower taxes increase the returns to purchasing land and engaging in production. Wages increase to offset the lower public goods, but does not fully compensate for the lowered redistribution in political equilibrium. Besides generating predictions, the model can be used to translate the estimated effects of disenfranchisement on factor markets into formulas for the economic welfare of factor owners.[9]

The model shares some features with Austen-Smith (2000), although I have a two-factor economy and use a variant of probabilistic voting instead of legislative bargaining. I construct a 1-period model of in a jurisdiction, such as a county, where land is fixed and labor is mobile. Assume a mass 1 of agents, with races B and W, indexed by κ, which is an input into agricultural production and also raises the return to migration. κ is an index of wealth, although it could also measure human capital or ability. Blacks have κ distributed with a convex cumulative distribution function $F_B(\kappa)$, and the distribution of κ for whites is given by $F_W(\kappa)$. Assume that θ_B is the fraction of the population that is black, so that the population distribution of κ is given by the mixture $F(\kappa) = (1-\theta_B)F_W(\kappa) + \theta_B F_B(\kappa)$. F_B is first order stochastically

[9]This is in the spirit of "sufficient statistics" from public finance (Chetty, 2009).

dominated by F_W, so $F_B(\kappa) \geq F_W(\kappa)$. I restrict κ to be positive, so that $F_W(0) = F_B(0) = 0$, and assume that F is strictly increasing. Thus blacks have lower κ than whites, reflecting either lower capital, education, or lower security of person and property.

Agents have utility given by $c + G_i$, including both private consumption c and local public goods G_i that depend on race i. Agents can work locally for an endogenous race-specific wage w_i, where $i \in (B, W)$, migrate, or engage in local production. Agents have 1 unit of labor, and engaging in own production requires 1 unit of land, at price v, and gives access to a Cobb-Douglas production function that takes κ and an aggregate of black and white labor (n_b and n_w), which are imperfect substitutes. The supply of land is fixed at $L < 1$, and can be thought of as being initially owned by absentee landowners. Agricultural output y is given by:

$$y = \frac{\kappa^{\alpha}({n_w}^{1-\alpha} + {n_b}^{1-\alpha})}{1-\alpha} \tag{1}$$

The $1-\alpha$ in the denominator is a normalization that simplifies the algebra. Labor demands will be given by $n_i = \kappa {w_i}^{\frac{-1}{\alpha}}$, so before-tax profits from production are equal to:

$$\Pi(w_B, w_W)\kappa = \frac{\alpha\kappa}{1-\alpha}\left({w_B}^{1-\frac{1}{\alpha}} + {w_W}^{1-\frac{1}{\alpha}}\right) \tag{2}$$

To capture the incidence of the land tax, I assume a tax rate on profits, τ and separate public goods for black and whites, G_W and G_B, all of which are constrained to be non-negative. So the post-tax welfare of a (white) landowner with given κ is given by:

$$W^L(\kappa) \equiv (1-\tau)\frac{\alpha\kappa}{1-\alpha}\left({w_B}^{1-\frac{1}{\alpha}} + {w_W}^{1-\frac{1}{\alpha}}\right) + G_W - v \tag{3}$$

Workers of either race i can choose to either migrate (M), earning $m\kappa$, or stay locally as a worker, earning $w_i + G_i$, from the local wage and the local public goods. I further assume no black employers, so that $\theta_W > L$ and black wealth is below the minimum that would make purchasing land and producing more profitable than migration, which is both tractable and empirically justifiable.[10] The assumption that the returns to migration are increasing in κ captures the empirical fact that migrants tended to be more educated in this period[11]. So the indirect utility of an agent of race i and wealth κ is given by the maximum utility over the three occupations landowner, migrant, or worker:

[10] Blacks owned minimal land in the South in 1870.

[11] All the results would go through if instead the returns to wage-labor were increasing in κ, returns to outmigration were fixed at a sufficiently low level so that outmigrants were at the bottom of the κ distribution.

$$W_i(\tau, G_i, \kappa) \equiv max(W^L(\kappa), m\kappa, w_i + G_i) \tag{4}$$

The government budget constraint implies that revenues R raised from taxing profits fund black and white public goods, which each cost $C(G_i)$, which is an increasing and convex function of the amount of the public good provided.[12]

$$R \equiv C(G_B) + C(G_W) = \tau\Pi(w_B, w_W)\overline{\kappa}L \tag{5}$$

Where $\overline{\kappa}$ is the mean level of wealth across landowners. This then implies that the taxes are given by $\tau = \frac{R}{\Pi(w_B, w_W)\overline{\kappa}L}$.

Assumption 1: $(1-\tau)\Pi(w_B, w_W) > m > \max_i \frac{w_i+G_i}{F^{-1}(1-L)}$.

While this is an assumption on endogenous variables, it is sufficient to ensure that agents sort into occupations by wealth, with the poorest agents working locally and receiving public goods and wages according to their race. Those with wealth $\kappa > \frac{w_i+G_i}{m}$ but not greater than $F^{-1}(1-L)$ migrate, while those who have enough wealth, with $\kappa > F^{-1}(1-L))$, to make buying land and producing profitable receive $W^L(\kappa)$. Given policies τ, G_W, G_B, an economic equilibrium exists with market clearing in land and labor markets of both races. Wages are set by labor markets for each race clearing:

$$\frac{\overline{\kappa}L}{{w_i}^{\frac{1}{\alpha}}} = F_i(\frac{w_i + G_i}{m}) \tag{6}$$

Land prices will be set by the land market clearing, with all agents with $\kappa \geq \kappa^* = F^{-1}(1-L)$ become landowners with $(1-\tau)\Pi(w_B, w_W)\kappa^* + G_W - v = m\kappa^*$, or:[13]

$$v = ((1-\tau)\Pi(w_B, w_W) - m)F^{-1}(1-L) + G_W \tag{7}$$

To capture disenfranchisement, I use a variant of probabilistic voting(Coughlin and Nitzan, 1981; Lindbeck and Weibull, 1987), where the idiosyncratic shocks are to the cost of voting rather than the preference for one party. I assume two parties, Democrat D and Republican R competing for votes in the election. Each party can commit to a vector of taxes and public goods (τ, G_B, G_W). Voters receive a uniformly distributed cost of voting $\eta - \overline{\eta}$, with η distributed on $[0,1]$ and $\overline{\eta} \in (0,1)$, except that black citizens have their turnout cost increased by

[12] If instead costs of public good provision were specified as $C(G_B + G_W)$ then increases in black public goods would increase the cost of white public goods, which would result in positive effects of disenfranchisement on white public goods and local white population.

[13] The assumption of no blacks landowners is $((1-\tau)\Pi(w_B, w_W) - m){F_B}^{-1}(\frac{1-L}{\theta_B}) < 0$.

a factor $\frac{1}{\Phi}$ with $\Phi \leq 1$. Φ will be the weight that politicians put on black welfare in equilibrium, and disenfranchisement can be modelled as a decrease in Φ, which increases turnout costs. So an agent votes Democrat if: $W_i(\tau^D, G_i^D, \kappa) - W_i(\tau^R, G_i^R, \kappa) + \overline{\eta} > \eta$ and votes Republican if: $W_i(\tau^R, G_i^R, \kappa) - W_i(\tau^D, G_i^D, \kappa) + \overline{\eta} > \eta$. That is, only voters whose partisan preferences are greater than their cost of voting cast a ballot. Total votes are then given by $V = (\theta_W + \theta_B \Phi)\overline{\eta}$.

Assumption 2: C'' is sufficiently high to ensure that $\int_k W(\kappa)dF(\kappa) - C(G_B) - C(G_W)$ is concave in G_B and G_W at $\tau = 0$.

Under these assumptions, it is relatively straighforward to obtain the following two propositions (all proofs in Appendix A).

Proposition 1: An interior political equilibrium exists, is symmetric and unique, with τ, G_B, G_W that solve:

$$\max_{\tau, G_W, G_B} \theta_W \int_k W_W(\tau, G_W, \kappa)dF_W(\kappa) + \Phi\theta_B \int W_B(\tau, G_B, \kappa)dF_B(\kappa) \tag{8}$$

subject to (5) and market clearing conditions (6) and (7).

Proposition 2: With competitive labor markets for black and white labor and probabilistic voting, an increase in turnout costs (decrease in Φ) for blacks lowers turnout ($\frac{dV}{d\Phi} = \overline{\eta}\theta_B > 0$), decreases black public goods ($\frac{dG_B}{d\Phi} > 0$), and increases black out-migration ($\theta_B \frac{dF_B(\frac{w_B(G_B)+G_B}{m})}{dG_B} > 0$) while the price of land decreases ($\frac{dv}{d\Phi} < 0$), despite lower wages ($\frac{dw_B}{d\Phi} < 0$).

While the intuition behind most of these comparative statics is obvious, the land price results follows from the fact that an interior choice of G_B equates the marginal benefit for landowners plus the marginal benefit for black workers with the marginal cost, which is borne by landowners. Since black workers always benefit from more G_B, any increases in G_B at the optimum must make landowners worse off, lowering the demand for land and lowering the land price.

The assumption of locally competitive labor markets, while natural, may not reflect the institutional reality of the U.S. South. Two other possibilities include wages set exogenously by a competitive fringe labor market, holding black wages fixed at $\overline{w_B}$, or monopsonistic labor markets, with efficient rationing, where black wages are set to maximize profits. Nonetheless all the comparative statics in Proposition 2 remain true under these alternative assumptions about the labor market.[14]

[14]When wages are exogenous, this follows easily. When wages are set monopsonistically, then at the political

The model also generates formulas for the implied changes in economic welfare across groups. Political changes, such as disenfranchisement, result in a variety of policy changes, not all of which are observed. For example, in this model both wages and public goods change after disenfranchisement. Nonetheless, by looking at the appropriate economic responses, the distributional consequences of a policy change can still be inferred. Define $W^L \equiv \int_{\kappa^*}^{\infty} W(\kappa) dF(\kappa)$ as the aggregate welfare of landowners, and define $W^i \equiv \int_0^{\kappa^*} W_i(\kappa) dF_i(\kappa)$ as the aggregate welfare of white and black workers/migrants. The assumption that land is in fixed supply implies that the price of land reflects the welfare of the marginal producer. An assumption on the distribution of the κ for landowners together with the linearity of landowner welfare in κ will allow me to translate the impact on the marginal landowner to average landowner welfare. Similarly, the assumption that the marginal migrant is the same under disenfranchisement as would be in response to a wage change allows me to convert the migration response to disenfranchisement into a measure of the change in worker welfare.

Proposition 3: If $F(\kappa|\kappa > \kappa^*)$ is Pareto with shape parameter b, then at equilibrium:

- $\frac{dW^L}{d\Phi} = \frac{1}{b-1}\frac{dv}{dp}$
- $\frac{dW^i}{d\Phi} = \frac{\frac{dF_i}{d\Phi}}{\frac{dF_i}{dw_i}}$

The Pareto distribution relates the marginal κ to the average by a simple multiplicative factor, and will also receive empirical justification below. While the specific formulas depend on details of the model, they reflect very general economic intuition: the price response of an inelastic factor and the quantity response of an elastic factor reveal the incidence on factor owners.

4 Identification Strategy: Contiguous County Pairs

My identification strategy relies on matched adjacent county-pairs that lie on state boundaries. This empirical strategy extends the spatial discontinuity methodology of Holmes (1998), Dube et al. (2009), and Duranton et al. (2011) to estimate the effect of disenfranchisement on political and economic outcomes. The sample consists of counties that lie on state borders in 1870, as shown in Figure 3. Counties are then matched into adjacent pairs p. Note that the same county can be in multiple pairs. Therefore the same county is included multiple times, which induces correlations in the unobservables across county-pairs.[15] Multidimensional clustering (Cameron

equilibrium it remains true that the effect of public goods on the wage does not offset the losses from additional taxes. See Appendix A for more details.

[15] I also estimate a specification where the unit of observation is single counties, but the variables are all differenced from the mean of the adjacent counties. Results are very similar and available upon request.

et al., 2008) is used to adjust the standard errors for both within-state over time correlations of county residuals, as well as within border-segment (the set of counties on both sides of a given border) within-year correlations. This not only accounts for the multiple instances of a given county in the data, but also the cross-pair correlations in the error term that can propagate, via the multiple county pairs a county can be part of, over the entire border segment. There are only 12 states in the sample, so there may be concerns about too few clusters. Standard fixes for this problem, such as the wild bootstrap, do not yet have multidimensional analogues, and in any case the standard errors are uniformly larger with clustering than without.

I balance the sample of counties over time on 5 core variables presidential turnout, presidential fraction Democratic, white teacher-child ratios, land value per acre, and log fraction black population. The other core dependent variable, black teacher-child ratio, is missing a large number of observations in 1870, and for that variable I instead balance the sample over pair-years. I estimate the following model for a variety of outcome variables y:

$$y_{p(c)cst} = \beta(D^P_{st} + D^L_{st}) + \sum_{t=1870}^{1920} \alpha_t X_{c,1860} + \gamma log(pop_{ct}) + \delta_c + \delta_{p(c)t} + \epsilon_{p(c)cst} \tag{9}$$

Where c indexes county, $p(c)$ denotes a county adjacent to c from another state, s denotes state, and t denotes year. D^P_{st} and D^L_{st} are dummies denoting the presence of a poll-tax and literacy test, respectively. $X_{c,1860}$ denotes a vector of county characteristics in 1860, described below. δ_c denotes a county fixed effect, and $\delta_{p(c)t}$ denotes a county-pair cross year fixed effect.

For all of these regressions I report the estimate of β, which is the effect of an additional poll tax or literacy test. As the laws are highly collinear, particularly when looking at census year variation, there is little independent information in the individual dummies, and the coefficients on each of the separate laws are very unstable. Results are very similar when the coefficients are estimated separately and summed.

4.1 Threats to Identification

While the county fixed effects control for any time-invariant county characteristics, it is the pair-year fixed effects that provide the novel identification in this empirical model. Spontaneous racial violence, time-varying geographic or climatic conditions, land productivity, labor market shocks or cultural values are all unlikely to respect state borders, and so the within-pair identification effectively deals with these sources of potential bias. My identification strategy is particularly important for the economic outcomes, as agricultural land, climate and usage patterns are likely highly variable across states. In addition, labor market conditions are also highly heterogeneous

across space, something the pair-year fixed effects control for. In the context of the American South, a key confounding variable is racist cultural values or discriminatory beliefs, which are also unlikely to vary discontinuously at the state boundaries.

However, there are potential confounds to the identification strategy. Other state-year legislation that is contemporaneous with disenfranchisement is the most obvious source of potential bias. While legislation passed after disenfranchisement was likely the results of a shrunken electorate, and therefore part of the changed political equilibrium induced by disenfranchisement, it is not possible to rule out all unobserved state-year variation that may have caused changes at the state border. As discussed in the historical background section, however, the main source of unobserved state-year variation likely correlated with disenfranchisement is the political threat to the Democratic party. The dynamic evidence below will suggest that this is not a confound in the border sample. The political threats leading to disenfranchisement were happening at the federal and state levels, and were independent of changes between cross-state border counties. Another concern is laws that are contemporaneous with disenfranchisement. This includes franchise restrictions that were not just the poll tax or the literacy test, which I deal with below, as well as potentially a wide variety of segregation laws. Besides the fact that transportation and public services were largely *de facto* segregated before the legal changes, it is difficult to see how segregation alone can account for the patterns on political variables and public goods found in this paper. Finally, disenfranchisement implied a whole suite of additional policies that vary at the state-year level but are consequences of the new political equilibrium created by suffrage restriction, and are part of the estimated coefficients.

Another potential confound is unobserved county-year variables that vary at state boundaries that could contaminate the effects estimated here. Relevant factors that vary include particularly racist or lenient sheriffs or county registrars, differential landlord control of local politics, or local black political power. Some of this can be examined by looking at heterogeneity on observable variables, but inability to rule out all other sources of county-year variation is a key limitation of this research design.

A further problem with interpreting the estimates arises from potential general equilibrium effects, as treated counties are being compared to their spatially contiguous neighbors, which are most vulnerable to spillover or substitution effects from treatment. Particularly with respect to migration, my results may be overstating the true effect. I address this by looking at heterogeneity in migration and transportation costs across both county-pairs and individual counties.

4.2 Data

Data on poll-tax and literacy test laws is obtained from Kousser (1974), Davidson and Grofman (1994), and Ogden (1958), and are shown in Figure 1. I obtain county-decade-level data for Southern states (census regions 31, 32, and 33) from 1870 to 1920. The sample period is chosen to be post-Civil War and pre-Depression, since these two events radically altered Southern society.

County pairs are formed using the 1870 census boundary file map, downloaded from the National Historical Geographic Information System at `www.nhgis.org`. ArcGIS is used to first find the set of southern counties that are touching state boundaries. Then for each county in the resulting sample, the counties that are tangent to that county are selected. Each border county is therefore matched into $n \geq 1$ adjacent counties. The sample is restricted to the county-pairs where each county belongs to a different state. Homogeneous 1870 counties are constructed by intersecting all the census maps from 1870 to 1920, matching the resulting polygons to the other data for the relevant year, and then averaging the polygons over the 1870 county boundaries, weighting by the polygon's share of the 1870 county area. The map is shown in Figure 2.

Historical election returns for presidential and congressional elections are from ICPSR. Gubernatorial returns are provided by Jim Snyder, and used previously in Hirano and Snyder (2008). County-level agricultural and population census data is obtained from ICPSR and Michael Haines. I use cotton suitability measured by potential cotton yields calculated by the Food and Agricultural Organization (FAO), and ruggedness measured by the county-level standard deviation of altitude, from the National Oceanic and Atmospheric Administration (NOAA). Education data was constructed using census data combined with state education reports, with details and additional robustness checks provided in Appendix B (Table A1).

I balance the main sample on 5 key observable variables: presidential turnout, democratic vote share, white teacher-child ratios, land value per acre, and fraction black. The other key variable is black teacher-child ratios, but many observations are missing in 1870, owing to the poor quality of both the census and the state education reports. Nonetheless, in the robustness checks I confirm that the main results are robust to balancing the sample on this variable as well.

5 Empirical Results

Some basic patterns can already be seen in the summary statistics, presented in Table 1. Turnout in all elections decreases as the number of disenfranchisement laws increases, as does the vote share of Democrats, and the per-acre value of land. However, it is difficult to see much at this level of aggregation in black teacher-child ratio, while the white teacher-child ratio increases with the number of disenfranchisement laws. Finally, disenfranchised counties have a higher fraction black population in this crude comparison, likely reflecting the characteristics of the states that disenfranchised first.

Table 2 examines pre-disenfranchisement differences in the sample. I construct a dummy variable for "early disenfranchisement", indicating if the state disenfranchised prior to 1896, which is the mid-year of the sample. I check to see on what 1860 variables counties on state borders differ based on states being "early" vs "late" disenfranchisers, to see if timing of disenfranchisement is systematically related to county-level outcomes in 1860. There are few statistically significant effects, and reassuringly, geographic variables that are not expected to change across state boundaries show no differences based on timing of disenfranchisement. The significant differences, on farm acreage, farm equipment, and urbanization, suggest that early disenfranchising border counties are less agricultural and rural than late disenfranchisers. I will control for year-specific effects of the significant covariates (as $X_{c,1860}$) in all specifications, and add all the other 1860 outcomes as covariates as specification checks.

Figures 3 and 4 show the results of a dynamic specification on presidential turnout and the fraction voting Democratic in presidential elections. While gubernatorial elections are highly heterogeneous in timing and congressional elections are plagued by pervasive gerrymandering as well as missing data, presidential elections are both high-frequency and regularly timed. The high frequency allows inclusion of leads and lags, and the fixed timing eliminates concerns of endogenous election timing. Formally, the points on the graph at time T relative to the passage of a disenfranchisement law is the cumulated sum of the coefficients $\sum_{k=-3}^{k=T} \beta_k$, where the β_k are the coefficients from a regression of the form:

$$\log(y_{p(c)cst}) = \sum_{k=-3}^{3} \beta_k (D^p_{st+k} + D^L_{st+k}) + \sum \beta_t X_{c,1860} + \delta_c + \delta_{p(c)t} + \epsilon_{cp(c)st} \qquad (10)$$

The cumulative effect of disenfranchisement laws on presidential turnout shows no pre-existing trend, but does register a large 25% drop in turnout immediately following the passage of the laws. The effects on partisan vote share are similar, showing a 2% increase the fraction voting Democrat with a significant increase following disenfranchisement and no pre-existing

differences within the county-pair.

Table 3 shows the effect of disenfranchisement on turnout and fraction Democratic vote share using the main specification. The election data is averaged over the decade following the census year for consistency across specifications. For turnout, poll taxes or literacy tests reduce presidential turnout by 8-12%, congressional turnout by 10-15% and gubernatorial turnout by 20-22%. This is consistent with other numbers in the literature (as discussed by Kousser (1974)). The next 3 columns of Table 2 show the effect of disenfranchisement on fraction Democratic vote share. There is a 0.6% increase in Democratic presidential vote share significant at the 5% level, a 1.4% increase in congressional vote share, and a 5-7% effect on gubernatorial vote shares. In sum, this Table shows that legal disenfranchisement lowered turnout to the advantage of the Democratic party.

The differences across elections are instructive, although they could be due to changing samples, as the available election data changes across the ICPSR and Hirano-Snyder samples. The largest effect on turnout is in gubernatorial elections, which falls by around 20% in both specifications after a disenfranchisement law. This is consistent with state level redistribution as the mechanism that is the focus of this paper. Because federal elections were generally between 2 parties, the laws would naturally favor the Democrats when partisan Republicans were disenfranchised, which could explain the significant coefficient on the Democratic vote share in presidential and congressional elections. However, the difference in magnitude between the effects on turnout and on vote-share suggest that likely not all the disenfranchised were voting Republican, or the changing composition of the electorate changed voting behavior.

In sum, the results on turnout and voting support the view of more recent Southern historians (e.g. Kousser) rather those of older political historians (exemplified by Key): the formal legal institutions of the South were important for reducing turnout, benefiting the Democrats, and potentially altering the political equilibrium of the South.

5.1 Education

Tables 4A and 4B show results on education, measured as black and white teachers per child and per pupil. As schools were effectively segregated, the number of black teachers is a proxy for black schooling inputs. Column 1 of Table 4A reports the effect of disenfranchisement on the log of the black teacher to eligible student ratio, with a constant $k > 0$ added.[16] The

[16]The constant is chosen to make the distribution skew-free, but none of the results depend on the choice of k.

estimated coefficient implies a semi-elasticity of -0.23. In order to rule out that this is driven by the $log(x+k)$ transformation, Column 2 uses the level of teachers/child and obtains a significant negative effect, implying a roughly 10% negative effect over the pre-disenfranchisement mean. Column 3 uses Teacher-pupil ratios instead of teacher-child, and implies a roughly 15% fall over the pre-disenfranchisement mean. Finally Column 4 uses pupil-child ratio, and while the coefficient is negative it is small and insignificant in both of the main specifications. The slightly larger effects on teacher-pupil ratios and the negative coefficients on pupil-child ratios may reflect an additional effect of parents withdrawing their children from lower-quality schools.

Table 4B shows the same results for white school inputs. In contrast to the effects on black schooling inputs, there is no effect of disenfranchisement on white teacher-child ratios. There is a negative effect on teacher-pupil ratios in Column 3, but inspection of Column 4 reveals that this is driven by an increase in enrollment among whites rather than an increase in teachers. The difference between the teacher-child and teacher-pupil ratios likely reflects the poorer data quality in the latter, but it may also reflect the endogeneity of pupils. The increase in white enrollment may be due to improvements in other dimensions of school quality or the white returns to schooling, but it is difficult to interpret in the absent of any effect on teacher-child ratios. While data availability does not allow me to pursue analysis of other measures of school quality, the evidence in Table 4A and 4B shows that disenfranchisement was associated with lower black educational inputs, with little discernible effect on white educational inputs. While it is difficult to rule out, for example, differential state trends in black schooling, owing to the few states and years in the sample, the evidence is consistent with the model's predictions: the altered political equilibrium induced by black disenfranchisement lowered public spending on black schools.

5.2 Factor Markets

Table 5 presents the effects of disenfranchisement on a variety of variables from the agricultural census. Columns 1 and 2 look at the effect on farm values and farm values per acre, and report that an additional instance of a poll tax or literacy test is associated with a 7% increase in farm values and a 3% increase in farm values per acre. Column 3 looks at the effect of disenfranchisement on farm output, and finds smaller and not robustly significant impacts of disenfranchisement on output. This is indirect evidence that the increase in farm values per acre is not coming from an increase in productivity, but rather a reduction in costs.

The next three columns of Table 5 look at the effects of disenfranchisement on population changes, and finds that black population falls relative to the white population. Column 5 shows

a fall in the fraction black of 0.1-0.4%, and Columns 6 and 7 show that this is driven by a 3-4% fall in the black population, with at most a 1% increase in the white population, depending on covariates. Thus, disenfranchisement caused a change in the racial composition of counties by increasing black outmigration, consistent with the predictions of the model.

In Appendix C Table A2, I show other margins of landowner and population adjustment. I find some evidence of increased investment in land and capital by landowners, although the latter could be an appreciation of capital values analogous to the rise in land values. I also find no significant effect on urbanization.

5.3 Robustness

Together, the results from Tables 3-5 show that disenfranchisement was effective in reducing turnout and increased vote shares for the Democratic party. Consistent with the model, the results also show that black public goods fell following disenfranchisement, with little effect on white public goods, measured as teacher-child ratios. The results also document that farm values went up, and that this is not solely due to increased acreage or productivity. Finally, disenfranchisement resulted in a reduction in the fraction black of a county, a result driven largely by black emigration rather than white in-migration.

Tables 6-8 show a number of robustness tests. The outcomes are the 6 core variables that are the focus of the paper: Presidential turnout, Democratic vote share, black and white teachers/child, farm value per acre, and fraction black. Table 6 shows a variety of alternative groups of control variables, ranging from no controls at all, to only controlling for population, to subsets of controls based on the partition in Table 2. The decreases in turnout, increases in Democrat vote shares, decreased black teacher-child ratios, no effects on white teacher-child ratios, and increases in farm values together with decreases in the fraction of the population that is black, all remain robust to these specifications.

Table 7 holds the specification fixed, but varies the sample. Panel A uses the full unbalanced sample. Panel B restricts attention to the counties that reported black teachers throughout the 1870 -1920 period. Panel C replicates the analysis using the 1920 census county map instead of the 1870, to account for any possible changes in county borders (and potential changes in pairs). Again, the pattern of results remains unchanged. Panel D estimates the difference-in-differences specification (standard errors clustered by county) on the entire county-year panel, including both border and non-border counties:

$$y_{cst} = \beta(D^p_{st} + D^L_{st}) + \sum \beta_t X_{c,1860} + \gamma log(pop_{ct}) + \delta_c + \delta_t + \epsilon_{ct}$$

While the effects of disenfranchisement on elections and schooling are very similar to the border-pair sample and specification, the effect on land values is about 50% smaller and insignificant, while the effect on fraction black is positive in sign, although insignificant. This suggests differences between the border identification strategy and the full sample, particularly with respect to factor markets. One interpretation could be that the border sample and specification controls much more effectively for unobserved differences in economic variables that may confound the straight county-year panel, although another interpretation could be that general equilibrium effects cause the effects on the border sample to be overstated. I return to this point in the next section.

Finally, Panel E reports a placebo specification, where I match each county that lies on a state border with its neighboring counties that lie *in the same state*, which I counterfactually assume are not affected by the disenfranchisement laws. I then re-estimate the main specification. If my identifying assumption is correct, then the effect of poll taxes and literacy tests in this regression should be insignificant. As Panel E shows, there is no significant effect of disenfranchisement on any of the outcome variables (although the coefficients on the election variables are large), suggesting that there is not a spurious effect occurring at the state borders.

Table 8 examines robustness to different variants of the definition of disenfranchisement. Panels A and B disaggregate the main disenfranchisement variable, using the full set of 1860 controls, while Panels C and D include other covariates or interactions that may alter the interpretation of the disenfranchisement variable, so I include only the baseline 1860 covariates. Panel A examines the effect of the poll tax and literacy test separately, as well as reporting the sum of the coefficients. Results on the sum of the coefficients are very similar to the main specification, although the effect on fraction black loses some precision. To the extent that the effects of the individual laws are interpretable, they suggest that while the poll tax reduced turnout, both laws increased Democrat vote shares. The literacy test reduced education (for both blacks and whites, it seems), while the poll tax induced the land value appreciation as well as the black outmigration. Panel B disaggregates the independent variable differently, instead counting the number of disenfranchisement laws passed rather than the particular types. Panel B suggests that it was both laws that together that had an effect on all the dependent variables of interest, rather than one law alone.

Panel C controls for alternative disenfranchisement laws, in particular the secret ballot and

the property requirement. The effects of the poll tax and the literacy test on voting remain unchanged in sign and significance, although they fall somewhat in magnitude. Validating our focus on the two disenfranchising laws is the fact that the secret ballot and the property requirement have no effect on voting outcomes. Controlling for the other two laws does reduce the size and significance of the effect on black teacher-child ratios, with the secret ballot being significant and substantial. Nevertheless the effect of the poll tax and the literacy test on black schooling is substantial and almost significant at 10%. None of the laws have any effect on white schooling, and the effects of poll taxes and literacy tests on farm values per acre and fraction black remains unchanged.

Panel D of Table 8 interacts the disenfranchisement variable with the lagged log of the black population, to see if the effect of disenfranchisement was concentrated in counties with large black populations. The interaction is demeaned to keep the main coefficient comparable with previous specifications. The only significant heterogeneity is on the turnout and fraction black variables, where a larger black population is associated with a larger decrease in voting and a larger change in the fraction black. At least with this linear interaction, the results do not appear to be driven by just the counties with large black populations, as the main effects of the poll tax and literacy test remain unchanged.

In Appendix C, I estimate the effect of disenfranchisement on racial violence. Results using lynchings[17], measured both as a count and a binary variable, give an imprecise (i.e. largely not significant) but negative coefficient. This 0 effect is consistent with the idea that violence was not an effective substitute for legal disenfranchisement; otherwise lynchings may have fallen substantially. There are difficulties of interpretation; a fall in lynchings post-disenfranchisement could suggest that, since lynchings could be politically motivated, the *de jure* disenfranchisement of blacks made the *de facto* use of violence unnecessary. However there could also be an increase in racist violence following disenfranchisement, as local law enforcement would no longer be under as much political pressure to enforce the rule of law vis-a-bis black citizens. Owing to these contradictory interpretations, this paper does not pursue the analysis of lynchings. Nonetheless, identifying the effect of disenfranchisement from contiguous counties does provide more confidence that voter intimidation and coercion, which are not likely to respect state boundaries, are not confounding the estimates of the impact of the legal changes.

[17]The lynchings data is from the Historical American Lynchings project, collapsed to the county-decade level.

5.4 Addressing Spillovers

Given the focus on contiguous county-pairs, there may be concerns that the effects are driven by general equilibrium effects. For example, the effects could be overstated due to black migrants leaving the treated county for the within-pair control county. Table 9 uses two approaches to empirically examine the possibility of spillovers. In Panel A of Table 9, I attempt to deal with this by constructing an alternative set of control counties. Instead of matching counties to their immediate neighbors, counties are matched to the interior "neighbors of their neighbors" in the adjacent state. If the effects were driven by spillovers, we would expect the effects in this sample to be smaller than the immediately adjacent comparison. However, as Panel A shows, the results are of the same order of magnitude as the border county sample and robustly significant across specifications.

Panel B of Table 9 takes another approach to assessing spillovers. I test for heterogeneous effects by the distance between the centroids of the two counties in a pair. This is an imperfect measure, potentially correlated with many other county characteristics such as area, and does not account for the distribution of population within a county. Nonetheless, if spillovers are a substantial concern, then effects should be attenuated in county-pairs whose centroids are very far apart on average, at least under the assumption of a uniform distribution of population within a county. Agents are less likely to move over longer distances, and less likely to move to adjacent counties that have more of their area farther away.

Very few of the interactions are significant across specifications. Only fraction Democrat shows a robustly larger increase in response to disenfranchisement in farther away counties. One reason for the mixed results can be seen in Figures 5 and 6, which shows the coefficients from subsamples of counties with centroids within 35 kilometers of each other through 105 kilometers, in increments of 10 km. In Figure 5, the effect of disenfranchisement on fraction black is larger, albeit more imprecise, in counties whose centroids that are very close together. I interpret this as a larger outmigration of blacks (and indeed the interaction is driven by the effect on the black population rather than the white population). This should imply a potentially greater increase in wages, and the positive effect on the land value should be attenuated. This is what we see in Figure 6. Despite this heterogeneity, the coefficients very quickly reach the sample average, suggesting that the spillovers are only present in a small part of the sample. While it is difficult to completely rule out the presence of general equilibrium effects, the two exercises conducted in this section suggest that the estimates in this paper are not merely an artifact of the adjacent border county empirical design.

6 The Incidence of Disenfranchisement on Land and Labor

In order to apply Proposition 3 to calculate incidence, I need two auxillary parameters. First I need to benchmark the migration response to income differences between counties, allowing an estimate of the income-equivalence of disenfranchisement. I obtain an estimate of $\frac{dF_B}{dw_B}$ from regressions using the linked census schedules from 1870-1880. As Proposition 3 shows, under the assumptions of the model, where the marginal migrant is the same for both disenfranchisement as well as labor income changes, this translates an aggregate migration response into a "money metric" that can then be used to calculate welfare of the potentially migrating populations. To obtain the effect on the average landowner I need an estimate of the scale parameter for the tail of the α distribution, which I obtain from the individual farm schedules extracted from the 1880 agricultural census by Ransom and Sutch (2001).

In Appendix D, I use linked 1870-1880 IPUMS census schedules to estimate the individual migration response to agricultural income growth, controlling for land value growth. For my calibration, I need to calculate an elasticity of migration with respect to *black* wages, which are not available in the 1880 data, so I assume that black wages w_B are equal to a constant share of agricultural labor income Y, so that growth rates of the two variables are identical. Expressed as a percentage of local black income[18], Proposition 3 implies that:

$$\frac{\frac{\Delta W_B}{\Delta \Phi}}{F_B \times w_B} = \frac{\frac{\frac{\Delta F_B}{F_B}}{\Delta \Phi}}{\frac{\frac{\Delta F_B}{\Delta w_B}}{w_B}} \tag{11}$$

The numerator is the semi-elasticity of black population with respect to disenfranchisement from Table 5. The denominator, under the assumption that black wages grow at the same rate as agricultural labor income, the coefficient β_y from the black IPUMS border sample estimated in the Appendix. This yields a 19% implied fall in per-worker black income from disenfranchisement. Using different estimates from Table 5 and Table A5 yield effects between 15-41% of rural black labor income per worker, implying elasticities of black labor income with respect to voting turnout between 1 and 2. Using Ng and Virts (1993) estimate of black per-worker labor income in 1880 of $w_B = \$66.21$, and border county black population in 1880 (1.9 million), to get that total black income falls between 19 and 52 million 1880 dollars. The historical price

[18]Note that this calculation assumes all blacks in the South were eventually disenfranchised, so that the migrants left the region entirely. This is so that we can ignore potentially complicated effects of agents moving to states that have not yet disenfranchised, but will. If agents are sufficiently forward looking and know all southern states are going to disenfranchise, then the response to disenfranchisement could be to either not move or leave the region entirely.

conversion calculator at `eh.net` suggests a conversion rate of 20 in purchasing power terms from 1880 to 2000, resulting in an implied loss of black income from disenfranchisement is between 382 million and 1 billion current-day dollars.

6.1 Tail Parameter for κ

Second, to obtain an estimate for the welfare gains of landowners, I need to obtain an estimate of the shape parameter of the distribution of κ to use Proposition 3. I use the Ransom and Sutch farm sample from the 1880 agricultural census to estimate the shape parameter of the distribution of κ, under the assumption that the distribution of κ will be close to the distribution of capital stocks across farms.

Figure 5 plots the log of the rank of a farm's equipment stock against the log of the equipment stock for all white-owned farms with more than 250 acres. The linearity of the resulting graph corroborates the Proposition 3 assumption that the distribution has a Pareto tail. Using the Gabaix and Ibragimov (2011) estimator, which regresses the log of the capital stock rank of a farm minus $\frac{1}{2}$ on the log of the capital value, gives a Pareto shape coefficient of $b = 1.29$. Using the formula from Proposition 3 yields an increase in landowner welfare of 12%, implying an elasticity with respect to turnout of just below 1. Aggregate 1880 farm value in the border counties is 400 million which implies an aggregate increase in landowner wealth of $.12 \times 400 = 48.4$ million. To convert this stock into a flow requires an assumption about the discount rate, which at 6% per year (Davis 1965) becomes 79% per decade, which implies a flow increase of $0.79 \times 48.4 = 38.2$ million dollars in landlord income. Transforming this into current day dollars would yield an equivalent transfer of roughly 764 million dollars.

While the calculations in this section should be taken with many caveats, given the reliability and availability of historical data, they suggest large losses borne by black labor and large gains for landowners. Given the mobility levels of black workers in response to agricultural income, the effect of disenfranchisement on black migration looks substantial, and given the inequality in capital across farms, the increase in land price implies sizeable increases in inframarginal producer profits.

7 Conclusion

This paper has estimated the impacts of Southern disenfranchisement on political competition, public good provision, land values, and migration using contiguous cross-state county pairs. I find that poll taxes and literacy tests lowered turnout, increased Democratic vote shares, and lowered black school quality. I also find that land and labor markets respond to the fall in

redistribution, resulting in higher land prices and increased black out-migration. By looking at land prices and migration decisions, I am able to infer the welfare implications of franchise restriction across groups, and find evidence of substantial black losses and landowner gains.

While this paper has not directly estimated long-run impacts of Southern disenfranchisement, it is likely that the effects of black political exclusion and educational under-provision persist through the intergenerational transmission of human capital and wealth (Sacerdote, 2005). Besides the effects on national racial inequality and skewed public-goods provision in the region, the "Solid South" engendered by formal disenfranchisement shaped the political and economic landscape of the United States for much of the 20th century, impeding welfare state expansion during the New Deal and later serving as a regional haven for low-wage manufacturing (Alston and Ferrie, 1999; Cobb, 1982; Holmes, 1998).

The results in this paper suggest that, consistent with a large body of formal theory and historical evidence, restricting the franchise lowers redistribution and public good provision. When decentralized in an open economy, as in the U.S. South, these effects are capitalized into land values and migration decisions. This paper suggests that the landowners of the U.S. South benefited from franchise restriction, so much so that it outweighed the ensuing loss of black labor. Historically, large landowners have often been the social group most opposed to democratization around the world, and the U.S. was no exception. Understanding how markets adapt to and constrain nondemocratic politics in settings like the U.S. South is a promising area for future research.

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Appendix A: Proofs

Proof of Proposition 1 and 2: Integrating over η, the expected net votes for Democrats are given by:

$$\begin{aligned}\Delta V^D &= \theta_W 2 \int W_W(\tau^D, G_W^D, \kappa) - W_W(\tau^R, G_W^R, \kappa) dF_W(\kappa) \\ &+ \theta_B 2\Phi \int W_B(G_B^D, \kappa) - W_B(G_B^R, \kappa) dF_B(\kappa)\end{aligned}$$

The Democrats will choose τ, G_B, G_W to maximize ΔV^D and the Republicans will try to minimize ΔV^D, each taking the other's policy vector as given. With Assumption 1, the equilibrium will be characterized by 3 cutoffs, $\kappa^* = F^{-1}(1-L)$, $\kappa_{W*} = \frac{w_W + G_W}{m}$ and $\kappa_{B*} = \frac{w_B + G_B}{m}$, that designate thresholds for when agents enter into production or migration or wage labor (with the latter being race-specific). Thus I can the use the market-clearing expressions for the wage and land price to write the political equilibrium as the following optimization problem, using the budget constraint to eliminate τ, the land market clearing constraint to eliminate v, and writing the labor market clearing constraints as $w_B(G_B)$ and $w_W(G_W)$:

$$\max_{G_W, G_B} W^L(G_B, G_W) + W^{PW}(w_W(G_W) + G_W) + \Phi W_B(w_B(G_B) + G_B)$$

Where

$$\begin{aligned}W^L(G_B, G_W) &= \\ \theta_W \int_{\kappa^*}^{\infty} &\left\{ \frac{\alpha}{1-\alpha} (\overline{\kappa} L)^{\alpha - 1} ((\theta_B F_B(\kappa_{B*}))^{1-\alpha} + (\theta_W F_W(\kappa_{W*}))^{1-\alpha}) \right. \\ &- \left. \frac{C(G_B) + C(G_W)}{\overline{\kappa} L} \right\} (\kappa - \kappa^*) + m\kappa^* dF_W(\kappa)\end{aligned}$$

and

$$W^{PW}(w_W(G_W) + G_W) = \theta_W \int_0^{\kappa^*} max(m\kappa, w_W(G_W) + G_W) dF_W(\kappa) \tag{12}$$

$$W^B(w_B(G_B) + G_B) = \theta_B \int_0^{\kappa^*} max(m\kappa, w_B(G_B) + G_B) dF_B(\kappa) \tag{13}$$

To show that the net effect of increased public goods on black welfare is positive, the fall in the wage cannot be too large, that is: $\frac{dw_B}{dG_B} \in (-1, 0)$. Implicitly differentiating the black labor market clearing condition. $\frac{\overline{\kappa} L}{w_B^{\frac{1}{\alpha}}} = \theta_B F_B(\kappa_{B*})$ yields:

$$\frac{dw_B}{dG_B} = \frac{-f_B(\frac{w_B + G_B}{m})}{f_B(\frac{w_B + G_B}{m}) + \frac{m\overline{\kappa} L}{\theta_B \alpha w_B^{\frac{\alpha+1}{\alpha}}}} \in (-1, 0) \tag{14}$$

Note also that if ${f_B}' > 0$, then $w_B(G_B)$ is concave in G_B.[19].

Thus the first-order condition for black public goods is:

$$-\Phi\frac{dW_B}{dG_B} = \frac{dW^L}{dG_B} \tag{15}$$

Since $\frac{dW_B}{dG_B} > 0$, at the optimum $\frac{dW^L}{dG_B} < 0$. Expanding this expression yields:

$$\Phi(\frac{dw_B(G_B)}{dG_B} + 1)F_B(\frac{w_B(G_B) + G_B}{m}) = \frac{C'(G_B)}{\theta_B}(1 - \frac{F^{-1}(1-L)}{\overline{\kappa}} \tag{16}$$

and the analogous condition for G_W is:

$$L + \theta_W F_W(\frac{w_W + G_W}{m})(\frac{dw_W}{dG_W} + 1) = C'(G_W)(1 - \frac{F^{-1}(1-L)}{\overline{\kappa}} \tag{17}$$

Clearly, if C is sufficiently convex, then (12) has negative second-derivatives in G_B, G_W. Also clearly from the fact that G_W does not appear in the first-order condition for G_B, the cross-partials between G_W and G_B are 0. Thus the Hessian is positive definite and the function is concave.

From (16) and the assumption on C it is immediate that $\frac{dG_B}{d\Phi} > 0$.

From (17) it also follows that $\frac{dG_W}{d\Phi} = 0$.

Thus $\frac{dF_B}{d\Phi} = (\frac{dF_B}{dw_B}\frac{dw_B}{dG_B} + \frac{dF_B}{dG_B})\frac{dG_B}{d\Phi} = \frac{dF_B}{dG_B}(1 + \frac{dw_B}{dG_B})\frac{dG_B}{d\Phi} > 0$.

Write the land price as:

$$\begin{aligned} v &= ((1-\tau)\Pi(w_B, w_W) - m)F^{-1}(1-L) + G_W \\ &= (W^L(G_B, G_W) - mF^{-1}(1-L)L)\frac{F^{-1}(1-L)}{L(\overline{\kappa} - F^{-1}(1-L))} + G_W - mF^{-1}(1-L) \end{aligned}$$

And since none of the other terms beside W^L in the expression depend on G_B, and the optimum from (15) implies that $\frac{dW^L}{dG_B} < 0$, it must be that $\frac{dv}{d\Phi} < 0$.

Proof of Proposition 3:

First consider the equation for the land price (with $\kappa^* = F^{-1}(1-L)$ as above):

$$(1-\tau)\Pi(w_B, w_W, G_B)\kappa^* + G_W - v = m\kappa^* \tag{18}$$

[19]If black labor markets are monopsonistic with efficient rationing, each employer gets $\frac{\kappa\theta_B F_B(\frac{w_B+G_B}{m})}{\overline{\kappa}L}$ black workers, and if an interior optimum exists, black wages are set according to $\frac{\partial\Pi(w_B,w_W)}{\partial w_B} = 0$. The assumption of efficient rationing implies that it does not matter if I consider firms as individually setting wages or whether the government is regulating them in order to maximize aggregate profits: both yield the same first-order condition for black wages. It can be seen that $\frac{w_B(G_B)}{dG_B}$ is still in $(-1, 0)$ and decreasing in G_B, so the comparative statics will remain the same under this assumption about wage setting. It is also easy to see that all the comparative statics will remain true under exogenously fixed wages $\overline{w_B}$, as any offsetting effects of public goods on black wages can be ignored.

Differentiating (18) it can be seen that a small change in Φ yields:

$$\frac{d(1-\tau)\Pi(w_B, w_W)}{d\Phi}\kappa^* L = \frac{dv}{d\Phi} \tag{19}$$

By decreasing the tax rate (and possibly the wage), disenfranchisement increases the price of land, just enough to make the marginal producer indifferent. Thus the land price change captures the welfare change for the marginal producer. To convert this into the effect on the average welfare of all producers, I integrate over the conditional distribution of κ

$$\frac{dW^L}{d\Phi} = \int_{\kappa^*}^{\infty} \frac{d(1-\tau)\Pi(w_B, w_W)}{d\Phi}\kappa - \frac{dv}{d\Phi}dF(\kappa) = L(\frac{d(1-\tau)\Pi(w_B, w_W)}{d\Phi})E[\kappa|\kappa > \kappa^*] - \frac{dv}{d\Phi})$$

Suppose F is a Pareto distribution with shape parameter $b > 1$ and scale parameter $a < \kappa^*$, then the conditional mean $E[\kappa|\kappa > \kappa^*] = \frac{b\kappa^*}{b-1}$ [20]

Therefore, using (18) the average change in producer welfare is given by:

$$\frac{d(1-\tau)\Pi(w_B, w_W)}{d\Phi}E[\kappa|\kappa > \kappa^*] - \frac{dv}{d\Phi} = \frac{dv}{d\Phi}\frac{E[\kappa|\kappa > \kappa^*]}{\kappa^*} - \frac{dv}{d\Phi} = (\frac{dv}{d\Phi})\frac{1}{b-1}$$

Thus the change in average producer welfare is given by:

$$\frac{1}{b-1}\frac{dv}{d\Phi}$$

which establishes the result.

The second part of the proposition follows from the following equations:
$\frac{dW_B}{d\Phi} = F_B(\frac{w_B+G_B}{m})\frac{d(w_B+G_B)}{d\Phi}$
$\frac{dM_B}{d\Phi} = -f_B(\frac{w_B+G_B}{m})\frac{d(w_B+G_B)}{d\Phi}$
$\frac{dM_B}{dw_B} = -f_B(\frac{w_B+G_B}{m})$
Putting these together yields:

$$\frac{dW_B}{d\Phi} = \frac{\frac{dF_B}{d\Phi}}{\frac{dF_B}{dw_B}} \tag{20}$$

[20]The cdf of the Pareto distribution is $G(\kappa) = 1 - (\frac{a}{\kappa})_b$, so the pdf of the truncated Pareto distribution is $g(\kappa|\kappa > \kappa^*) = \frac{b\frac{a_b}{\kappa^{b+1}}}{(\frac{a}{\kappa^*})^b} = \frac{b(\kappa^*)_b}{\kappa^{b+1}}$. Integrating $\int_{\kappa^*}^{\infty} \frac{b(\kappa^*)_b}{\kappa_b}d\kappa$ gives $\frac{ba^b{\kappa^*}^{1-b}}{b-1}$. Dividing this by $1 - G(\kappa^*) = (\frac{a}{\kappa^*})_b$ yields the conditional expectation $E[\kappa|\kappa > \kappa^*] = \frac{b\kappa^*}{b-1}$.

Appendix B: Education Data Details

To construct the education variables, I draw the relevant variables (teachers, pupils, and eligible children (aged 5-20) by race and gender) from the 1890 census, which was the first census to collect education data at the county level. Unfortunately, the 1890 census microdata was destroyed, but the individual 10% IPUMS sample is available for the other census years, and they are used to construct analogs to the 1890 variables. Note that these are not directly comparable, as the 1890 numbers are not constructed by aggregating the individual schedules, but instead are compiled by the census bureau. The other years are likely to be unrepresentative due to undersampling, as only the 10% census sample is available, and self-reported occupation is used rather than administrative reports. The census also measured school attendance by asking if the child had attended school at least once. Needless to say, this is a poor metric of true school attendance. While this should not effect the point estimates, the mismeasurement in the census is likely to increase the standard errors.

State education reports were collected for available years closest to census years and data from additional education reports collected by Morgan Kousser was added. The reports are idiosyncratic and often report very different information for each state, particularly in the early years. While there exists a great deal of data, for example teachers' wages or value of school property in some states for some years, very few variables exist for all Southern states over all census years in my sample. The variables selected are those that can be compared with the data constructed from the census. The data from the state education reports is combined with the census variables. From the reports, white and black teachers, pupils, and eligible students are extracted. Often the eligible students are from the most recent census, so the data is occasionally redundant. I then average all the observations from all the sources over each decade, and use that as my measure of educational inputs.

In Table A1, I show the effect of disenfranchisement on schooling inputs measured separately in the census data and in the state report data. As can be seen, the sample size for the state-report data is much smaller than from the census. In the census data, the effect of disenfranchisement on black teacher-child ratios is negative and significant in specifications with both the restricted and full-set of 1860 control variables, while it is insignificant in the state-report data. For black teacher-pupil ratios, the effect of disenfranchisement in negative in both samples and both specifications, and, save for the fuller specification on the census data, significant. For the white schooling input variables, the effects of disenfranchisement are almost always insignificant.

Appendix C: Other Outcome Variables and Heterogeneity

In Table A2, I estimate the effect of disenfranchisement on a variety of other variables of interest. In Panel A, I example the effect of disenfranchisement on alternative political variables, including a measure of lynchings. Columns 1-4 use the Hirano-Snyder data on congressional voting, which are corrected for errors in the ICPSR data, but are not as comprehensive. Effects are consistent with Table 3, with large significant falls in turnout of roughly the same magnitude as Table 3 and much larger increases in Democrat vote share, either perhaps non-classical measurement error or a consequence of the different sample available. The next four columns, 5-8, examine lynchings, both as a continuous measure and as a binary variable. The data are from the Historical American Lynchings project, and only go back to 1880. The effect of disenfranchisement is mixed, with most specifications showing no effect, but the specification with controls showing a significant positive effect of disenfranchisement on lynchings.

In Panel B I look at other variables from the agricultural and population censuses. Columns 1-2 look at equipment per acre, and find a positive effect of disenfranchisement on the value of equipment. This is consistent with increased value of already purchased equipment (owing to lower taxation or labor costs), but it is also consistent with increased investment. Columns 3-4 use log farm size as a dependent variable, and find a negative effect, consistent with increased sharecropping (the census counted operators, not owners, as separate farms), but it could be consistent with entry of more smaller farmers or the fragmentation of larger farms. Columns 5-8 show that disenfranchisement resulted in a increase in improved acreage as well as overall farm acreage. While the model assumed fixed supply of land, this result suggests that farmers could indeed bring new land into cultivation. Without information on the costs of improving land or bringing new land into cultivation, it is difficult to adjust the implied incidence for this, but we can interpret the true effect as lying between the effect on land value per acre and total land value. Columns 9 and 10 examine the effect on the log of urban population (controlling for log population), to check if disenfranchisement resulted in additional migration to or away from cities, but the effects are insignificant and of mixed signs, suggesting no large impact on urbanization.

In Table A3 I interact the disenfranchisement independent variable with a variety of fixed county characteristics that proxy for institutional characteristics of the South that may have exacerbated the effects of disenfranchisement. I am interested in proxies for the parts of the South that were particularly dependent on low-cost black agricultural labor. As proxies, I use the log of the fraction slave in 1860, a dummy for bordering the Mississippi River, the measure of cotton suitability from FAO, and finally a "plantation" dummy, using the list of designated

plantation counties in Brannen (1924) as the source.[21] I estimate the same specification as in section 5.4.

Table A3 shows the results on the 6 main variables of interest. Reassuringly, the main effects remains unchanged from Tables 3-5. The political effects of disenfranchisement are uniformly and substantially larger in the black labor intensive parts of the South. The effects on land values are also larger, which is unsurprising as plantation districts are both highly productive as well as highly dependent on black labor. The falls in fraction black population are somewhat larger in magnitude, but this effect is not robust across proxies or specifications with the full set of controls (not shown).

There also seems to be substantial heterogeneous results on public good provision, although the coefficients switch sign depending on the proxy used. Both black and white education seems to increase following disenfranchisement in the Mississippi border areas, perhaps reflecting economic and political transfers towards these areas following disenfranchisement. White education decreases following disenfranchisement in the plantation counties and in the highly cotton suitable counties, but black education increases in the former and decreases in the latter. This likely reflects heterogeneity in school system funding across these various locations, where labor-intensive jurisdictions responded to disenfranchisement with a variety of mixes of black and white public goods in order to attract different types of labor. Some counties could have reacted to state-level disenfranchisement by increasing school provision to retain black labor, even at the expense of white labor, while other labor-intensive counties could have cut spending on both types of public goods following disenfranchisement.

[21] While likely endogenous, the plantation measure in Brannen has been widely used by economic historians. See Alston and Kauffman (2001).

Appendix D: Estimating Individual Income-Migration Elasticities

I use the linked 1870-1880 IPUMS sample for two reasons. First, it is the only 10-year linked census, as the linkage is to the 1880 individual schedules and the 1890 individual schedules have been destroyed (which is why I cannot use these for reliable estimates of the impact of disenfranchisement on migration), and therefore lets me look at the shortest migration window. 1870-1880 is also the beginning of my sample period, and therefore unlikely to be contaminated by the effects of the disenfranchisement laws. I restrict my sample to men aged 16-60, and match them to the agricultural census data for their county of residence in 1870. Sample summary statistics are in Table A4. Notably, the mean levels of migration are very high, with approximately 40% of men changing counties between 1870 and 1880. Agricultural wages, unfortunately, are not available at the county level in 1880. Therefore, I look at black migration as a function of county growth in agricultural income. I estimate the following model at the individual level:

$$Migrate_i = \beta_0 + \beta_y g_{ya} + \beta_v g_{va} + X_a \beta_a + X_i \beta_i + \epsilon_{ia} \tag{21}$$

Where i denotes individual and a denotes county. $Migrate$ is a dummy indicating that the individual changed counties between 1870 and 1880. g_{ya} is the growth rate in agricultural output in county a, and g_{va} is the growth rate in the land value. By controlling for g_{va} I am adjusting for the share of the agricultural income growth that is going into land values, and thus making g_{ya} a better estimate of the returns to labor. X_a is a vector of county-level covariates (growth of black teachers, and total population), and X_i is a vector of individual covariates (age, age-squared, urban, and literacy). For comparison, I estimate the same regressions for whites and blacks separately. Standard errors are clustered at the county level.

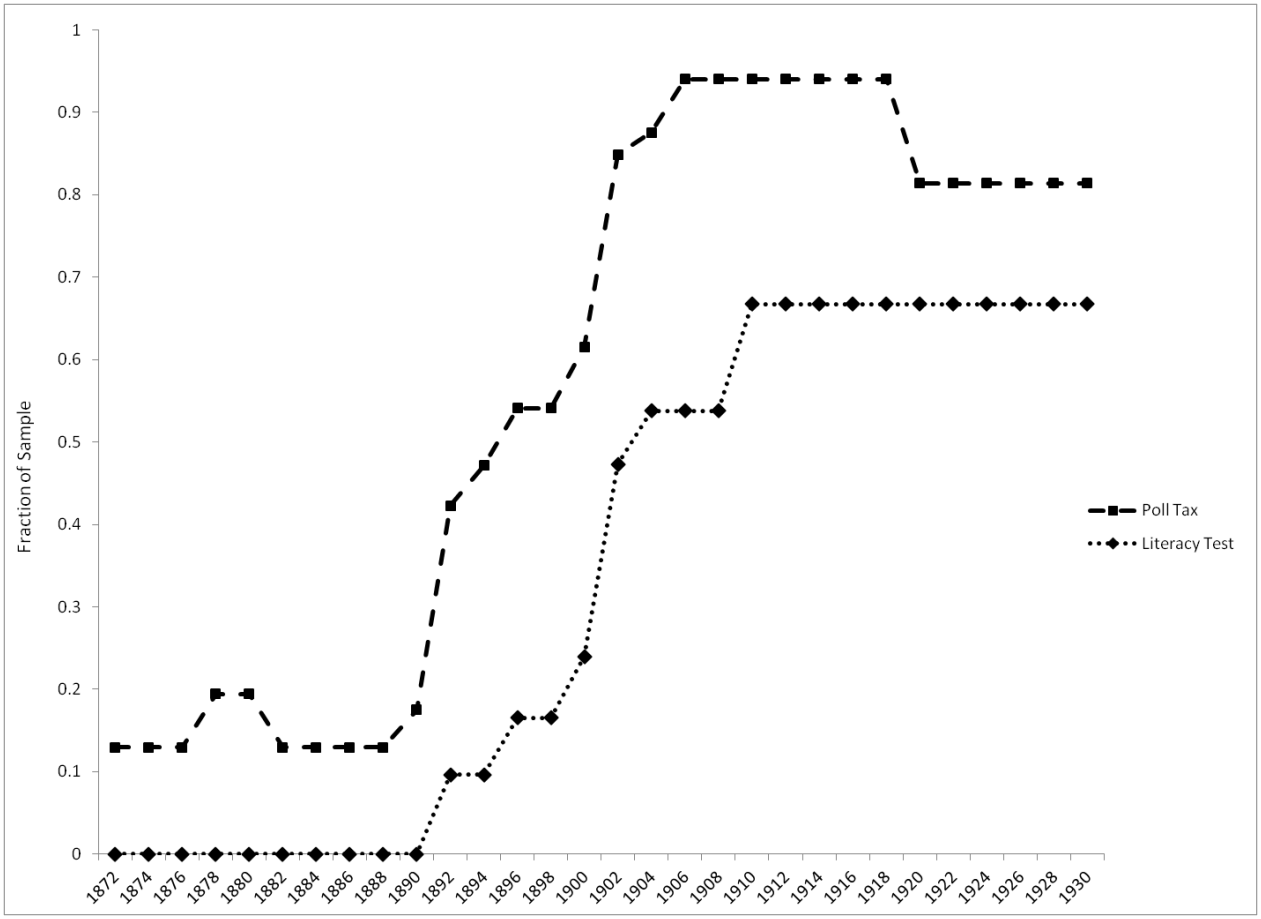
Results are in Table A5. On the full set of Southern blacks, I estimate a β_y of -0.12 without controls, decreasing in magnitude to -0.11 when individual controls are added. A 1% increase in the growth rate of agricultural income is associated with an 11 percentage point increase in the probability of staying in the county. The coefficient on the growth rate of black teachers is also positive and significant at 10% confidence without the individual controls, falling slightly (and becoming marginally insignificant) when controls are added.

When estimated on the sample of border county residents, the migration response to income growth increases by almost a factor of 2. Thus a 1% increase in the growth rate of agricultural income in a border county is associated with a 18 to 22 percentage point fall in the probabil-

ity of outmigration. This may reflects greater state-level opportunities for residents of border counties. The coefficient on black teacher growth falls substantially in magnitude and becomes insignificant.

The results for whites are included to illustrate that a) whites did not respond nearly as much as blacks to county-level income growth, and b) the summary statistics indicate that while the migration rates for both whites and blacks are high, blacks are on average more likely to migrate than whites. While this could be due to the poor quality of the linkage, owing to black illiteracy and general difficulties with the 1870 census, it could also be evidence that black mobility during the decade following Reconstruction was high in both levels (as seen in the summary statistics) and in its responsiveness to agricultural income.

Figure 1: Timing of Southern Disenfranchisement



Notes: Y axis fraction of counties affected by law in sample.

Figure 2: Sample Counties (after balancing)

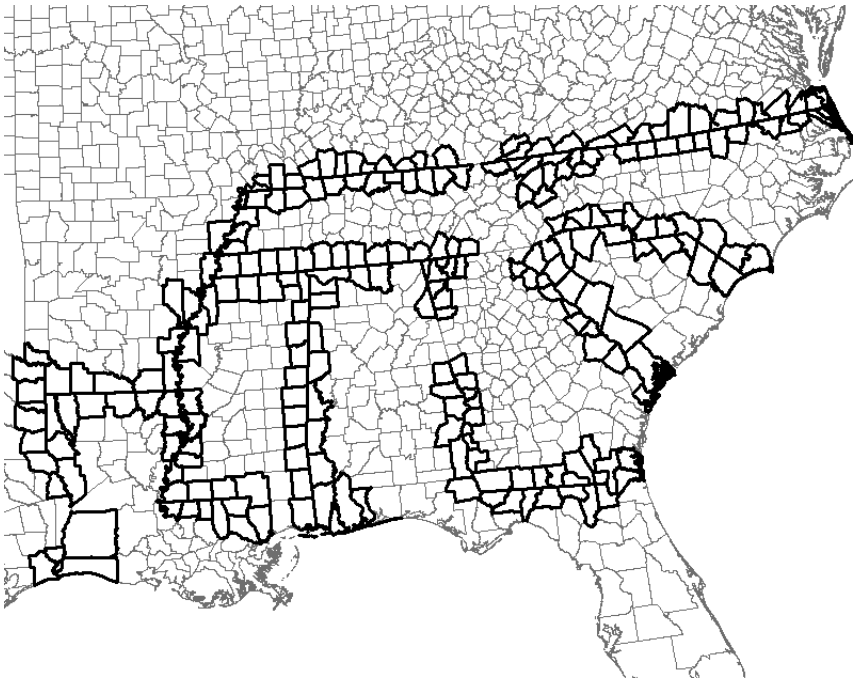
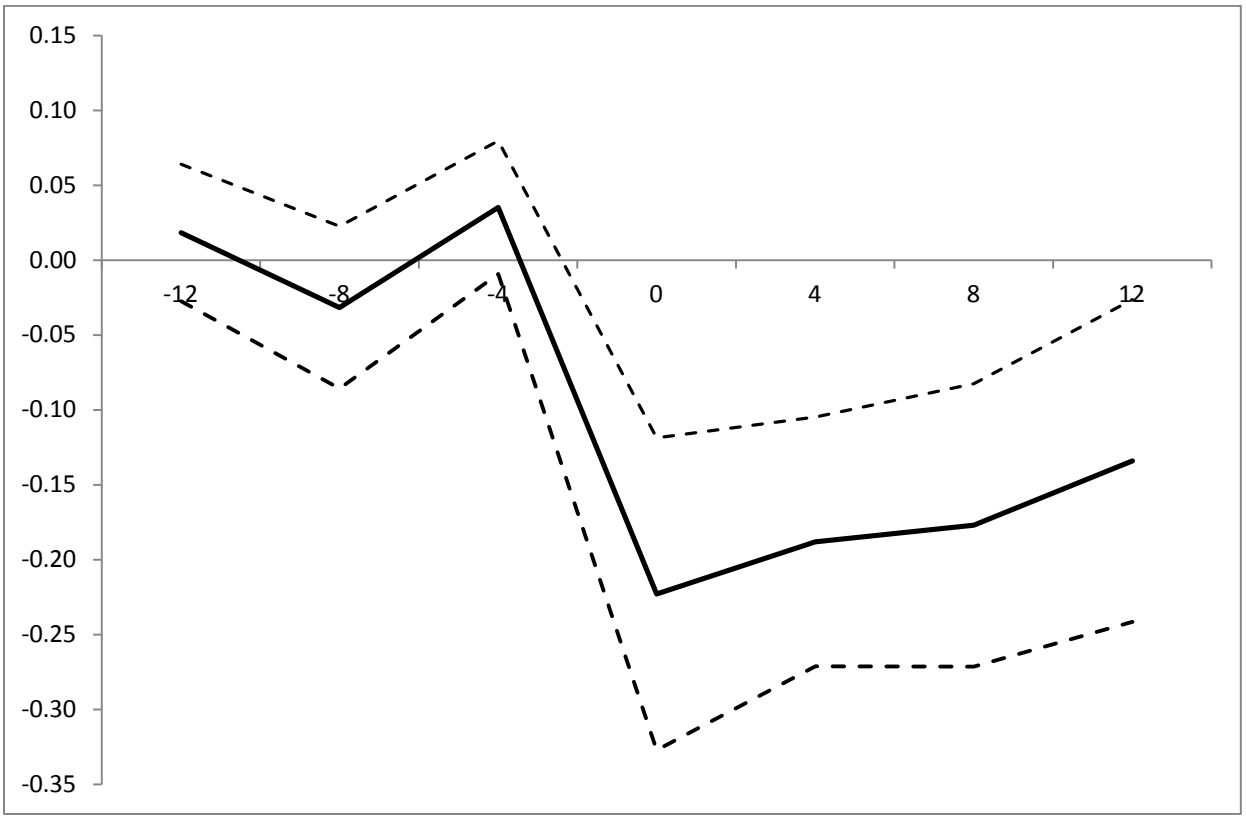
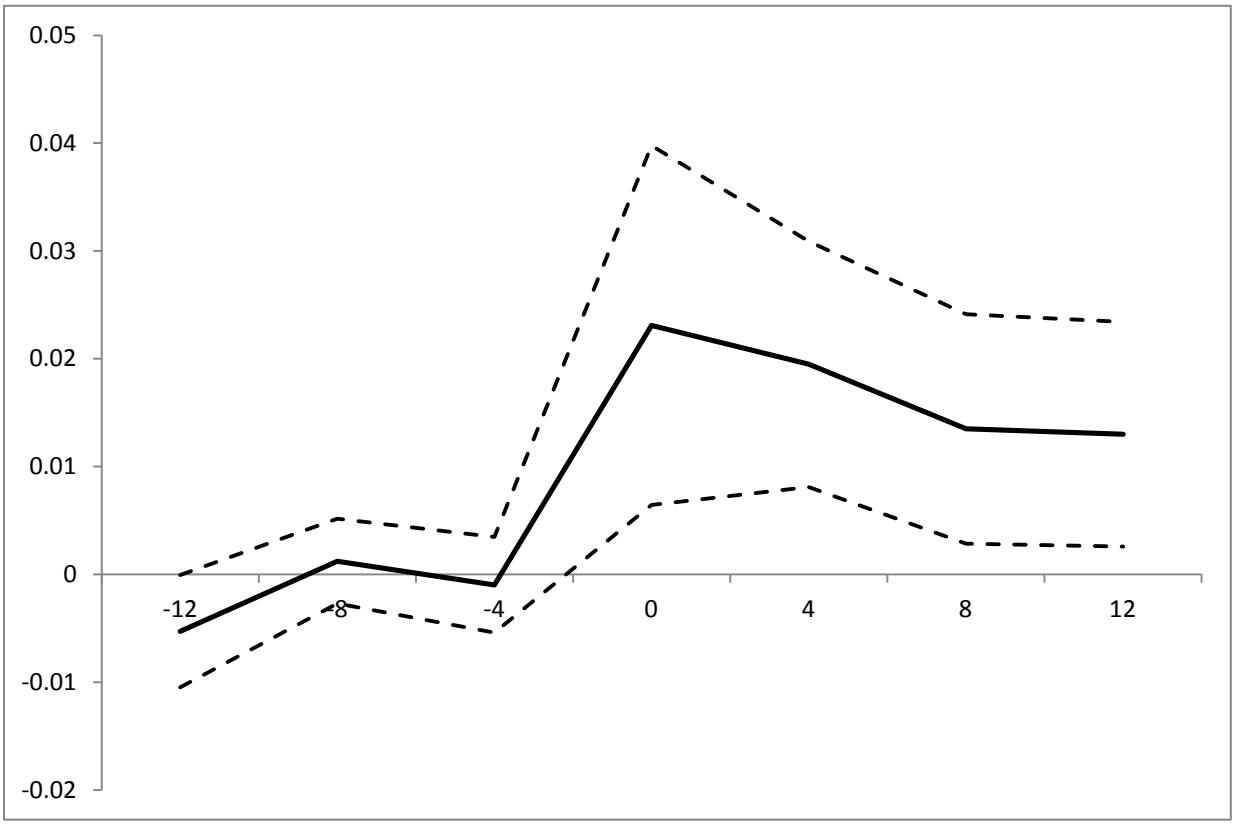


Figure 3: Effect of Disenfranchisement on Log(Total Votes) in Presidential Elections



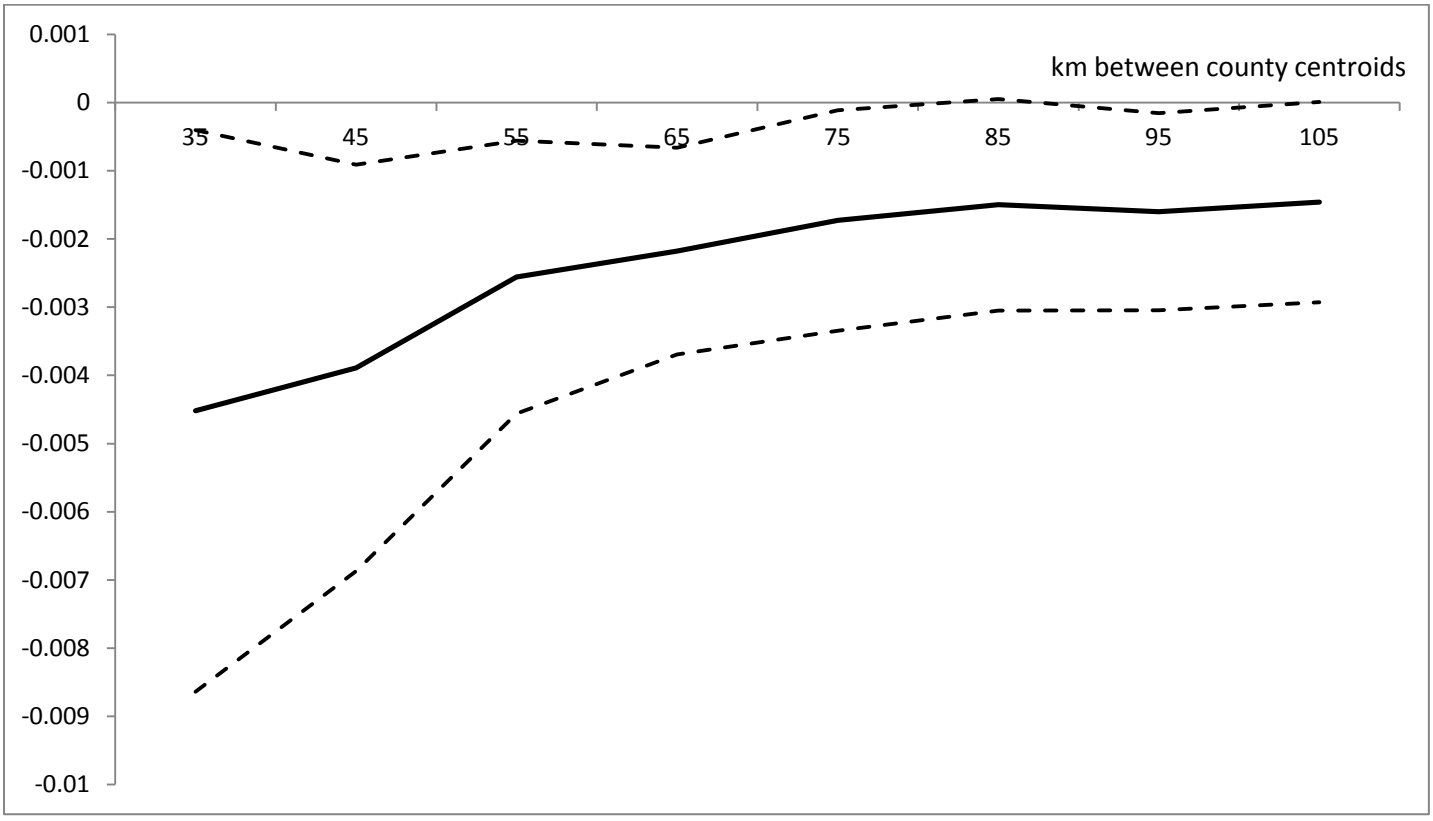
Notes: Coefficients from dynamic specification discussed in text, with 95% confidence intervals.

Figure 4: Effect of Disenfranchisement on Log(Fraction Democrat) in Presidential Elections



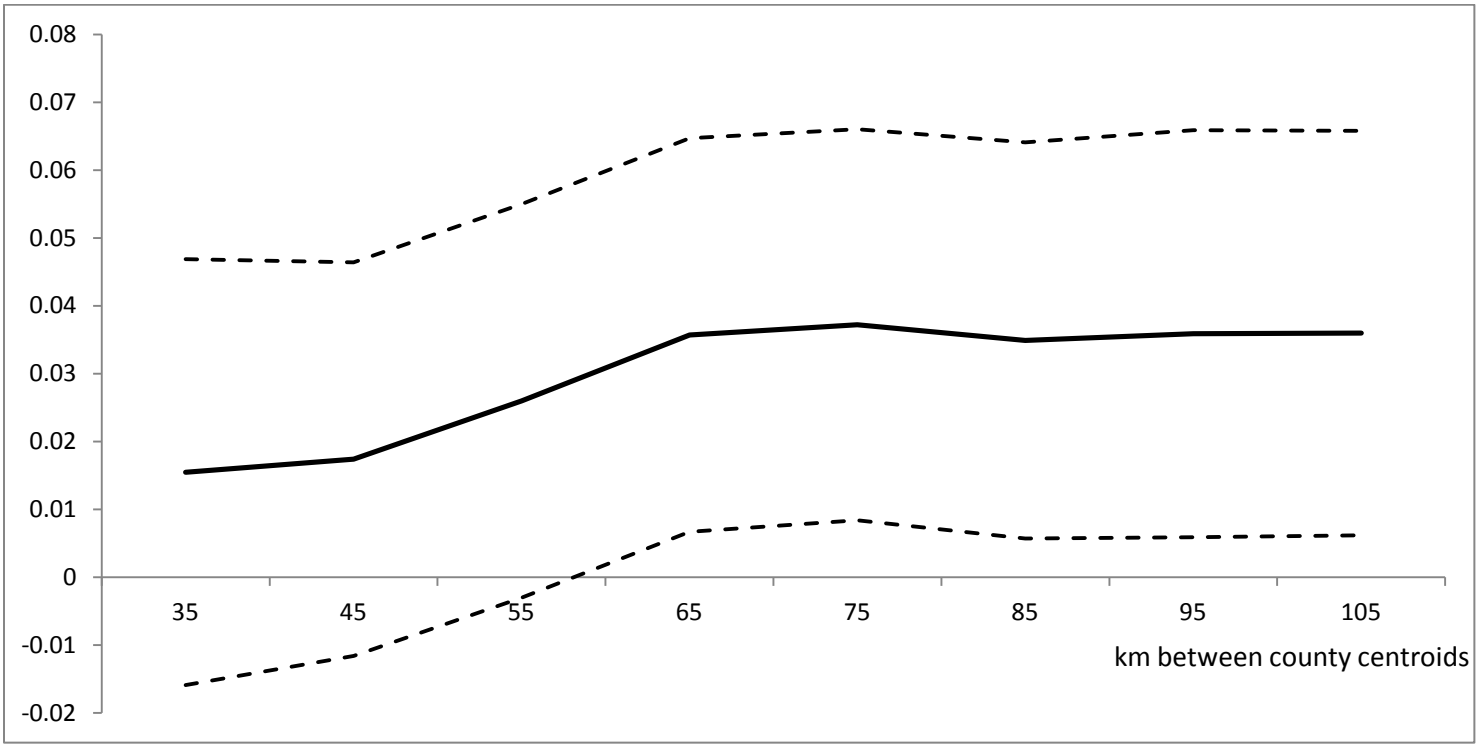
Notes: Coefficients from dynamic specification discussed in text , with 95% confidence intervals.

Figure 5: Effect of Disenfranchisement on Log(Fraction Black) by Distance Between County Centroids



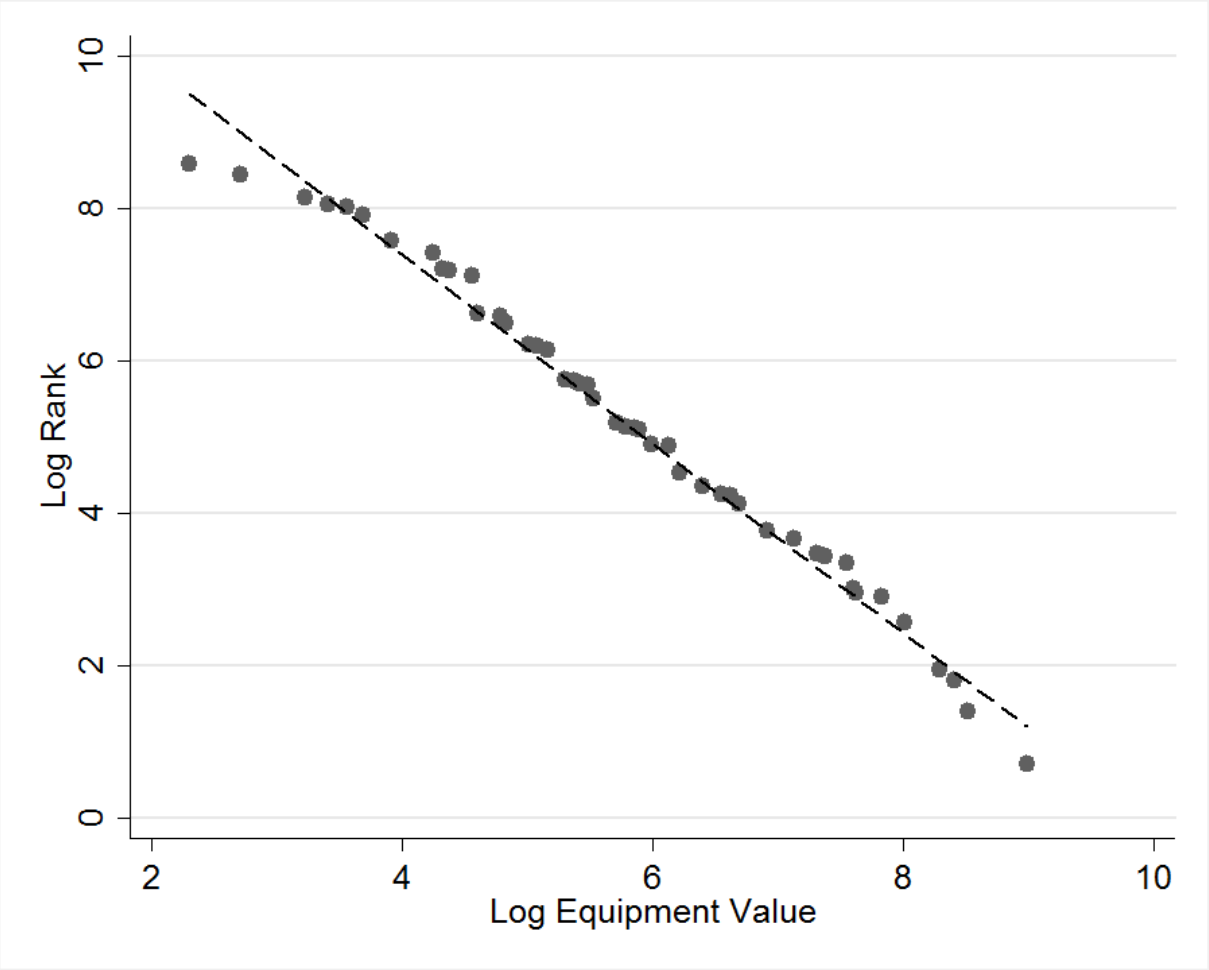
Notes: Coefficients from subsamples with county centroids < magnitude of x-axis , with 95% confidence intervals.

Figure 6: Effect of Disenfranchisement on Log(Land Value/Acre) by Distance Between County Centroids



Notes: Coefficients from subsamples with county centroids < magnitude of x-axis , with 95% confidence intervals.

Figure 7: Log Rank- Log Capital Value Plot from 1880 Agricultural Census



Notes: Sample from Ransom and Sutch extract of 1880 census, restricted to white owned farms with more than 250 acres.

Table 1: Summary Statistics

Variable	Neither Poll Tax nor Literacy Test			Either Poll Tax or Literacy Test			Both Poll Tax and Literacy Test		
	Obs	Mean	Std. Dev	Obs	Mean	Std. Dev	Obs	Mean	Std. Dev
Mean Presidential Votes	1677	2446.68	1641.87	878	2374.21	2285.44	815	1579.35	1176.97
Mean Congressional Votes	1645	2046.15	1342.15	782	1994.38	1780.41	748	1207.69	1033.35
Mean Gubernatorial Votes	1677	836.32	512.86	878	730.71	579.16	748	405.95	315.90
Fraction Presidential Democrat	1677	0.04	0.05	878	0.05	0.05	815	0.09	0.09
Fraction Congressional Democrat	1645	0.05	0.05	782	0.07	0.06	748	0.14	0.13
Fraction Gubernatorial Democrat	1677	0.58	0.21	878	0.71	0.61	748	0.90	0.15
Log(Black Teachers/ Black Eligible Children+k)	1677	-5.55	1.22	878	-5.54	1.35	815	-5.56	1.33
Black Teachers/ Black Eligible Children	1677	0.006	0.007	878	0.008	0.014	815	0.008	0.023
Black Teachers/ Black Pupils	1441	0.019	0.031	830	0.018	0.024	815	0.016	0.018
Black Pupils/ Black Eligible Children	1672	0.32	0.23	877	0.44	0.22	815	0.53	0.16
Log(White Teachers/ White Eligible Children+k)	1677	-4.23	0.60	878	-4.05	0.59	815	-4.02	0.76
White Teachers/ White Eligible Children	1677	0.01	0.02	878	0.01	0.01	815	0.02	0.02
White Teachers/ White Pupils	1603	0.03	0.06	867	0.03	0.03	811	0.03	0.02
White Pupils/ White Eligible Children	1674	0.43	0.20	878	0.59	0.18	815	0.69	0.13
Value of Farm Land and Buildings	1677	1990416	1889019	878	3984064	5563643	815	6501714	8050184
Value of Farm Land and Buildings/ Farm Acreage	1677	8.15	7.85	878	16.61	19.49	815	22.03	25.32
Farm Output / Farm Acreage	1677	3.99	3.54	878	8.36	9.39	815	12.31	11.79
Farm Acreage	1677	256696	127294	878	221999	97197	815	296269	143390
Fraction Black	1677	0.35	0.25	878	0.34	0.24	815	0.43	0.25
White Population	1677	11069	7129	878	13696	13033	815	17061	13685
Black Population	1677	7487	8406	878	8197	12096	815	14099	12480

Note: k chosen to make the distribution of log(x+k) have 0 skew.

Table 2: 1860 Differences Among Early and Late Disenfranchising States

1860 Agricultural Variables

	(1) Log(Farm Value/Acre)	(2) Log(Imp Acreage)	(3) Log(Farm Acres)	(4) Log(Equip. Value)
Disenfranchised Before 1896	-0.00576	-0.411	-0.414*	-0.522**
	(0.0836)	(0.274)	(0.248)	(0.250)
Pair FE	Y	Y	Y	Y
N	478	478	478	478

1860 Population Variables

	(1) Log(Pop)	(2) Frac. Slave	(3) Frac. Urban	(4) Frac. Free Colored
Disenfranchised Before 1896	-0.299	0.00527	0.0843***	0.00820
	(0.395)	(0.0179)	(0.0153)	(0.00773)
Pair FE	Y	Y	Y	Y
N	478	478	478	478

Geographic Variables

	(1) Ruggedness	(2) River	(3) Cotton Suitability	(4) Longitude	(5) Latitude	(6) Log Area
Disenfranchised Before 1896	6.035	-0.0135	-0.0107	2687.0	7443.3	-0.344
	(5.457)	(0.0430)	(0.0132)	(5860.7)	(10699.0)	(0.239)
Pair FE	Y	Y	Y	Y	Y	Y
N	478	478	478	478	478	478

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors multidimensionally clustered on border segment and state. Independent variable is a binary variable indicating if a county was disenfranchised prior to 1896.

Table 3:Effect of Disenfranchisement on Turnout and Political Competition

	Log(Total Votes Cast)			Log(Fraction Democrat)		
Panel A: Balanced Sample	(1)	(2)	(3)	(4)	(5)	(6)
	Presidential	Congress	Governor	Presidential	Congress	Governor
Poll Tax + Literacy Test	-0.122**	-0.153**	-0.216**	0.00624**	0.0141**	0.0585*
	(0.0296)	(0.0376)	(0.0559)	(0.00189)	(0.00405)	(0.0280)
Log(Population)	0.494**	0.405**	0.272**	-0.0271*	-0.0375*	0.0232
	(0.0788)	(0.108)	(0.103)	(0.0109)	(0.0165)	(0.0169)
Sample Counties	Border	Border	Border	Border	Border	Border
Years	Census	Census	Census	Census	Census	Census
1860 Controls	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Pair-Year FE	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
N	3370	3175	3303	3370	3175	3303
Panel B: Balanced Sample	(1)	(2)	(3)	(4)	(5)	(6)
	Presidential	Congress	Governor	Presidential	Congress	Governor
Poll Tax + Literacy Test	-0.0810**	-0.108**	-0.197**	0.00586**	0.0130**	0.0751*
	(0.0160)	(0.0206)	(0.0556)	(0.00181)	(0.00288)	(0.0291)
Log(Population)	0.479**	0.409**	0.256**	-0.0317**	-0.0450**	0.0472**
	(0.0791)	(0.0967)	(0.0916)	(0.00829)	(0.0120)	(0.0140)
Sample Counties	Border	Border	Border	Border	Border	Border
Years	Census	Census	Census	Census	Census	Census
1860 Controls	All	All	All	All	All	All
Pair-Year FE	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
N	3370	3175	3303	3370	3175	3303

Notes: + p<0.1, * p<0.05, ** p<0.01. Standard errors multi-dimensionally clustered on border-segment X year and state. Baseline 1860 controls are year specific effects of log 1860 fraction urban, log 1860 improved acreage, and log 1860 farm acreage. All 1860 controls include all the dependent variables in Table 2.

Table 4a: Effect of Disenfranchisement on Black Teachers/Pupils

		Black		
Panel A: Balanced Sample	(1)	(2)	(3)	(4)
	Log(Teacher/ Child)	Teacher/Child	Teacher/Pupil	Pupil/Child
Poll Tax + Literacy Test	-0.232***	-0.000948*	-0.00381***	-0.0194
	(0.0744)	(0.000484)	(0.00136)	(0.0140)
Log(Population)	-0.168	-0.00168	0.000523	-0.0350
	(0.222)	(0.00199)	(0.00360)	(0.0271)
Sample Counties	Border	Border	Border	Border
Years	Census	Census	Census	Census
1860 Controls	Baseline	Baseline	Baseline	Baseline
Pair-Year FE	Y	Y	Y	Y
County FE	Y	Y	Y	Y
N	3370	3370	3086	3086
		Black		
Panel B: Balanced Sample	(1)	(2)	(3)	(4)
	Log(Teacher/ Child)	Teacher/Child	Teacher/Pupil	Pupil/Child
Poll Tax + Literacy Test	-0.222*	-0.000864	-0.00331**	-0.00295
	(0.123)	(0.000538)	(0.00137)	(0.0169)
Log(Population)	-0.371*	-0.00353	-0.00512*	-0.0240
	(0.215)	(0.00248)	(0.00279)	(0.0258)
Sample Counties	Border	Border	Border	Border
Years	Census	Census	Census	Census
1860 Controls	All	All	All	All
Pair-Year FE	Y	Y	Y	Y
County FE	Y	Y	Y	Y
N	3370	3370	3086	3086

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors multi-dimensionally clustered on border-segment X year and state. Log(Teacher/Child) is calculated as Log(Teacher/Child+k), where k is chosen so as to minimize the skewness in the resulting variable. Baseline 1860 controls are year specific effects of log 1860 fraction urban, log 1860 improved acreage, and log 1860 farm acreage. All 1860 controls include all the dependent variables in Table 2.

Table 4b: Effect of Disenfranchisement on White Teachers/Pupils

Panel A: Balanced Sample	White			
	(1)	(2)	(3)	(4)
	Log(Teacher/ Child)	Teacher/Child	Teacher/Pupil	Pupil/Child
Poll Tax + Literacy Test	-0.0232	-0.000173	-0.00331**	0.0301*
	(0.0342)	(0.000822)	(0.00143)	(0.0171)
Log(Population)	0.0697	0.000632	0.00466	0.0214
	(0.0498)	(0.00142)	(0.00558)	(0.0198)
Sample Counties	Border	Border	Border	Border
Years	Census	Census	Census	Census
1860 Controls	Baseline	Baseline	Baseline	Baseline
Pair-Year FE	Y	Y	Y	Y
County FE	Y	Y	Y	Y
N	3370	3370	3281	3367

Panel B: Balanced Sample	White			
	(1)	(2)	(3)	(4)
	Log(Teacher/ Child)	Teacher/Child	Teacher/Pupil	Pupil/Child
Poll Tax + Literacy Test	-0.0430	-0.000466	-0.00438**	0.0373*
	(0.0333)	(0.000877)	(0.00187)	(0.0194)
Log(Population)	-0.0840*	-0.00348**	0.00740	0.0274*
	(0.0506)	(0.00141)	(0.00629)	(0.0159)
Sample Counties	Border	Border	Border	Border
Years	Census	Census	Census	Census
1860 Controls	All	All	All	All
Pair-Year FE	Y	Y	Y	Y
County FE	Y	Y	Y	Y
N	3370	3370	3281	3367

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors multi-dimensionally clustered on border-segment X year and state. Log(Teacher/Child) is calculated as Log(Teacher/Child+k), where k is chosen so as to minimize the skewness in the resulting variable. Baseline 1860 controls are year specific effects of log 1860 fraction urban, log 1860 improved acreage, and log 1860 farm acreage. All 1860 controls include all the dependent variables in Table 2.

Table 5: Effect of Disenfranchisement on Land and Labor

Panel A: Balanced Sample	(1) Log(Farm Value)	(2) Log(Value/Acre)	(3) Log(Output/Acre)	(5) Log(Fraction Black)	(6) Log(White Pop)	(7) Log(Black Pop)
Poll Tax + Literacy Test	0.0793***	0.0325**	0.0149	-0.00430***	0.0111***	-0.0418***
	(0.0171)	(0.0151)	(0.0124)	(0.000995)	(0.00409)	(0.0154)
Log(Population)	1.022***	0.607***	0.562***	0.00879	0.963***	1.120***
	(0.0599)	(0.0423)	(0.0475)	(0.00755)	(0.0296)	(0.0509)
Sample Counties	Border	Border	Border	Border	Border	Border
Years	Census	Census	Census	Census	Census	Census
1860 Controls	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Pair-Year FE	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
N	3370	3370	3370	3370	3370	3370

Panel B: Balanced Sample	(1) Log(Farm Value)	(2) Log(Value/Acre)	(3) Log(Output/Acre)	(5) Log(Fraction Black)	(6) Log(White Pop)	(7) Log(Black Pop)
Poll Tax + Literacy Test	0.0802***	0.0358**	0.0189**	-0.00143*	0.00223	-0.0328***
	(0.0207)	(0.0151)	(0.00954)	(0.000750)	(0.00337)	(0.0125)
Log(Population)	0.925***	0.564***	0.556***	-0.000503	0.982***	1.092***
	(0.0686)	(0.0340)	(0.0636)	(0.00745)	(0.0259)	(0.0636)
Sample Counties	Border	Border	Border	Border	Border	Border
Years	Census	Census	Census	Census	Census	Census
1860 Controls	All	All	All	All	All	All
Pair-Year FE	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
N	3370	3370	3370	3370	3370	3370

Notes: *p<0.1, **p<0.05, *** p<0.01. Standard errors multi-dimensionally clustered on border-segment X year and state.Baseline 1860 controls are year specific effects of log 1860 fraction urban, log 1860 improved acreage, and log 1860 farm acreage. All 1860 controls include all the dependent variables in Table 2.

Table 6: Robustness To Alternative Specifications

	Political Variables		Log(Teacher Child Ratios)		Factor Markets	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: No Controls	Log(Pres. Turnout)	Log(Frac. Dem)	Black	White	Log(Value/Acre)	Log(Frac. Black)
Poll Tax + Literacy Test	-0.178***	0.00985***	-0.305***	-0.0263	0.0424***	-0.00415***
	(0.0349)	(0.00345)	(0.0506)	(0.0333)	(0.0143)	(0.00154)
1860 Controls	None	None	None	None	None	None
N	3370	3370	3370	3370	3370	3370
Panel B: Population Only	Log(Pres. Turnout)	Log(Frac. Dem)	Black	White	Log(Value/Acre)	Log(Frac. Black)
Poll Tax + Literacy Test	-0.183***	0.0102***	-0.303***	-0.0273	0.0375***	-0.00424***
	(0.0309)	(0.00321)	(0.0504)	(0.0329)	(0.0140)	(0.00146)
Log (Population)	0.639***	-0.0406***	-0.233	0.122**	0.593***	0.0114
	(0.0614)	(0.0109)	(0.186)	(0.0532)	(0.0412)	(0.00926)
1860 Controls	None	None	None	None	None	None
N	3370	3370	3370	3370	3370	3370
Panel C: Agricultural Controls	Log(Pres. Turnout)	Log(Frac. Dem)	Black	White	Log(Value/Acre)	Log(Frac. Black)
Poll Tax + Literacy Test	-0.103***	0.00546***	-0.245***	-0.0212	0.0333**	-0.00348***
	(0.0262)	(0.00150)	(0.0897)	(0.0362)	(0.0167)	(0.000913)
Log (Population)	0.492***	-0.0288***	-0.154	0.0912	0.561***	-0.00183
	(0.0651)	(0.00895)	(0.210)	(0.0584)	(0.0474)	(0.00817)
1860 controls	Agriculture	Agriculture	Agriculture	Agriculture	Agriculture	Agriculture
N	3370	3370	3370	3370	3370	3370
Panel D: Demographic Controls	Log(Pres. Turnout)	Log(Frac. Dem)	Black	White	Log(Value/Acre)	Log(Frac. Black)
Poll Tax + Literacy Test	-0.106***	0.00775***	-0.259***	-0.0260	0.0400**	-0.00305***
	(0.0214)	(0.00199)	(0.0639)	(0.0221)	(0.0177)	(0.000596)
Log (Population)	0.460***	-0.0314***	-0.402*	0.0577	0.606***	0.00885
	(0.0874)	(0.0104)	(0.228)	(0.0669)	(0.0380)	(0.00919)
1860 controls	Demographic	Demographic	Demographic	Demographic	Demographic	Demographic
N	3370	3370	3370	3370	3370	3370
Panel E: Geographic Controls	Log(Pres. Turnout)	Log(Frac. Dem)	Black	White	Log(Value/Acre)	Log(Frac. Black)
Poll Tax + Literacy Test	-0.128***	0.00889***	-0.328***	-0.0685**	0.0309**	-0.00246*
	(0.0182)	(0.00209)	(0.0876)	(0.0271)	(0.0137)	(0.00135)
Log (Population)	0.711***	-0.0419***	-0.218	0.0792*	0.581***	0.00932
	(0.0664)	(0.0110)	(0.160)	(0.0459)	(0.0389)	(0.00774)
1860 Controls	Geographic	Geographic	Geographic	Geographic	Geographic	Geographic
N	3370	3370	3370	3370	3370	3370

Notes: * p<0.1, ** p<0.05, *** p<0.01. All specifications control for log population. Standard errors multi-dimensionally clustered on border-segment X year and state. All 1860 controls are all the dependent variables in Table 2. Panel A runs the main specification with no controls. Panel B include only log of population as a control. Panel C includes the log of population and the year-specific effects of the 1860 agricultural variables from Table 2. Panel D includes the log of population and the year-specific effects of the 1860 population variables from Table 2. Panel E includes the log of population and the year-specific effects of the 1860 geographic variables from Table 2.

Table 7: Robustness To Alternative Samples

	Political Variables		Log Teacher-Child Ratios		Factor Markets	
	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Pres. Turnout)	Log(Frac. Dem)	Black	White	Log(Land Val/Acre)	Log(Frac. Black)
Panel A: Unbalanced						
Poll Tax + Literacy Test	-0.0989***	0.00449**	-0.215*	-0.0350	0.0304**	-0.00178**
	(0.0186)	(0.00176)	(0.119)	(0.0308)	(0.0147)	(0.000746)
1860 Controls	All	All	All	All	All	All
N	3780	3780	3672	3787	3791	3791
Panel B: Balanced on Black Teacher-Child						
Poll Tax + Literacy Test	-0.0888***	0.00671***	-0.214**	-0.0339	0.0367**	-0.00227**
	(0.0193)	(0.00173)	(0.105)	(0.0405)	(0.0158)	(0.000951)
1860 Controls	All	All	All	All	All	All
N	2724	2724	2724	2724	2724	2724
Panel C: 1920 borders						
Poll Tax + Literacy Test	-0.0543**	0.00460**	-0.229*	0.0100	0.0378***	-0.00155*
	(0.0266)	(0.00211)	(0.137)	(0.0205)	(0.0145)	(0.000835)
1860 Controls	All	All	All	All	All	All
N	3192	3192	3192	3192	3192	3192
Panel D: Full County-Year Panel Sample						
Poll Tax + Literacy Test	-0.152***	0.00812**	-0.347**	-0.0472	0.0155	0.00348
	(0.0485)	(0.00320)	(0.114)	(0.0467)	(0.0313)	(0.00452)
1860 Controls	All	All	All	All	All	All
N	5526	5526	5526	5526	5526	5526
Panel E: Within State Placebo						
Poll Tax + Literacy Test	-0.0310	0.00453	-0.0410	-0.00956	0.0308	0.00187
	(0.0173)	(0.00268)	(0.0668)	(0.0467)	(0.0267)	(0.00333)
1860 Controls	All	All	All	All	All	All
N	2764	2764	2764	2764	2764	2764

Notes: *p<0.1, ** p<0.05, *** p<0.01. All specifications control for log population. Standard errors multi-dimensionally clustered on border-segment X year and state. All 1860 controls are all the dependent variables in Table 2. Panel A uses all counties in the sample with data. Panel B looks only at those counties that have non-missing variables in every year. Panel C uses the 1920 county borders to match county-pairs instead of 1870. Panel D uses the full set of Southern counties, controlling for county and year fixed effects, as well as year specific effects of all the 1860 variables from Table 2. Panel E is an interior county placebo, which matches border counties to the adjacent counties in the same state and estimates the main specification.

Table 8: Robustness To Alternative Disenfranchisement Definitions

	Political Variables		Log (Teacher-Child Ratios)		Factor Markets	
	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Pres. Turnout)	Log(Frac. Dem)	Black	White	Log(Land Val/Acre)	Log(Frac. Black)
Panel A: Separate Laws						
Poll Tax	-0.156***	0.00572**	-0.159	0.0239	0.0452**	-0.00288**
	(0.0379)	(0.00253)	(0.217)	(0.0716)	(0.0222)	(0.00145)
Literacy Test	0.00892	0.00611***	-0.299***	-0.125**	0.0250	0.000262
	(0.0508)	(0.00236)	(0.114)	(0.0525)	(0.0355)	(0.00265)
Poll Tax + Literacy Test	-0.147***	0.0118***	-0.458**	-0.101	0.0702**	-0.00262
	0.0343	0.00358	0.240	0.0616	0.0326	0.00168
Controls	All	All	All	All	All	All
N	3370	3370	3370	3370	3370	3370
Panel B: Number of Laws						
Either Poll Tax or Literacy Test	-0.00616	0.00156	-0.323	0.0259	-0.00973	-0.00306*
	(0.0673)	(0.00317)	(0.213)	(0.0843)	(0.0320)	(0.00176)
Both Poll Tax and Literacy Test	-0.161***	0.0117***	-0.446*	-0.0860	0.0715**	-0.00294**
	(0.0386)	(0.00357)	(0.240)	(0.0699)	(0.0308)	(0.00143)
Controls	All	All	All	All	All	All
N	3370	3370	3370	3370	3370	3370
Panel C: Alternative Laws						
Poll Tax + Literacy Test	-0.143***	0.00579*	-0.151	-0.0493	0.0402**	-0.00479***
	(0.0486)	(0.00325)	(0.0976)	(0.0596)	(0.0196)	(0.000908)
Secret Ballot	-0.0949	-0.00890	-0.838***	0.00308	-0.0228	-0.000803
	(0.0674)	(0.00748)	(0.101)	(0.0666)	(0.0309)	(0.00207)
Property Requirement	0.132	0.00615	0.0634	0.107	-0.0213	0.00240
	(0.132)	(0.0102)	(0.223)	(0.139)	(0.0481)	(0.00364)
Controls	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
N	3370	3370	3370	3370	3370	3370
Panel D: Lagged Black Population Interaction						
Poll Tax + Literacy Test	-0.112***	0.00591***	-0.237***	-0.0174	0.0301*	-0.00372***
	(0.0329)	(0.00172)	(0.0747)	(0.0342)	(0.0157)	(0.00104)
Log Black Population(t-10)	0.0419	0.00343	0.118	0.0624	-0.0398	0.0168***
	(0.0364)	(0.00335)	(0.0954)	(0.0396)	(0.0272)	(0.00450)
(Poll Tax + Literacy Test) X Log Black Population(t-10)	-0.0556***	0.000619	-0.0646	-0.0435	0.00474	-0.00280***
	(0.0168)	(0.00136)	(0.0478)	(0.0345)	(0.00753)	(0.00104)
Controls	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
N	3248	3248	3248	3248	3248	3248

Notes: *p<0.1, ** p<0.05, *** p<0.01. All specifications control for log population. Standard errors multi-dimensionally clustered on border-segment X year and state.Baseline 1860 controls are year specific effects of log 1860 fraction urban, log 1860 improved acreage, and log 1860 farm acreage. All 1860 controls include all the dependent variables in Table 2.

Table 9: Spillover/General Equilibrium Effects

	Political Variables				Log (Teacher-Child Ratios)				Factor Markets			
	(1)		(2)		(3)		(4)		(5)		(6)	
	Log(Pres. Turnout)		Log(Frac. Dem)		Black		White		Log(Land Val/Acre)		Log(Frac. Black)	
Panel A: Interior County Control												
Poll Tax + Literacy Test	-0.128***	-0.0965***	0.00678***	0.00621***	-0.237***	-0.257***	-0.0118	-0.0253	0.0300***	0.0367**	-0.00418***	-0.00262***
	(0.0305)	(0.0202)	(0.00207)	(0.00169)	(0.0605)	(0.0908)	(0.0387)	(0.0326)	(0.0116)	(0.0150)	(0.00104)	(0.000779)
Log (Population)	0.573***	-0.0371***	0.00423	0.0761*	0.542***	0.0232***	0.526***	-0.0336***	-0.186	-0.142***	0.517***	0.0138**
	(0.0589)	(0.00844)	(0.143)	(0.0460)	(0.0349)	(0.00704)	(0.0487)	(0.00757)	(0.130)	(0.0474)	(0.0336)	(0.00576)
Controls	Baseline	All	Baseline	All	Baseline	All	Baseline	All	Baseline	All	Baseline	All
N	13816	13816	13816	13816	13816	13816	13816	13816	13816	13816	13816	13816
N	3370	3370	3370	3370	3370	3370	3370	3370	3370	3370	3370	3370
Panel B: Distance Interaction												
Poll Tax + Literacy Test	-0.0812***	-0.121***	0.00585***	0.00606***	-0.223*	-0.234***	-0.0434	-0.0225	0.0359**	0.0319**	-0.00147**	-0.00430***
	(0.0161)	(0.0300)	(0.00168)	(0.00161)	(0.119)	(0.0732)	(0.0327)	(0.0344)	(0.0154)	(0.0154)	(0.000721)	(0.00100)
Poll Tax + Literacy Test X Centroid Distance (km)	0.0147	-0.960	0.249***	0.355***	5.09	3.96	-0.141	-1.31	.767	1.16*	0.0962*	-0.000857
	(0.00106)	(1.18)	(0.0622)	(0.0972)	(3.24)	(3.53)	(0.885)	(1.13)	(0.593)	(0.700)	(0.0540)	(0.0563)
Log (Population)												
Controls	Baseline	All	Baseline	All	Baseline	All	Baseline	All	Baseline	All	Baseline	All
N	3370	3370	3370	3370	3370	3370	3370	3370	3370	3370	3370	3370

Notes: *p<0.1, **p<0.05, *** p<0.01. All specifications control for log population. Standard errors multi-dimensionally clustered on border-segment X year and state.Baseline 1860 controls are year specific effects of log 1860 fraction urban, log 1860 improved acreage, and log 1860 farm acreage. All 1860 controls include all the dependent variables in Table 2. Interactions with Centroid Distance are multiplied by 1000 to save space.

Table A1: Education from Different Samples

	Census Data							
	Black				White			
	Teacher/Child		Teacher/Pupil		Teacher/Child		Teacher/Pupil	
Poll Tax + Literacy Test	-0.00160**	-0.00129**	-0.00340**	-0.00213	0.000975	0.000571	-0.00330	-0.00900
	(0.000678)	(0.000533)	(0.00163)	(0.00152)	(0.000644)	(0.000811)	(0.00427)	(0.00675)
Log(Population)	0.00178	-0.000999	0.0124**	0.00258	-0.000546	-0.00152	0.00698	-0.00368
	(0.00227)	(0.00276)	(0.00578)	(0.00444)	(0.00211)	(0.00187)	(0.00539)	(0.0101)
Sample Counties	Border	Border	Border	Border	Border	Border	Border	Border
Years	Census	Census	Census	Census	Census	Census	Census	Census
1860 Controls	Baseline	All	Baseline	All	Baseline	All	Baseline	All
Pair-Year FE	Y	Y	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y	Y	Y
N	3348	3348	2995	2995	3370	3370	3274	3274
	State Report Data							
	Black				White			
	Teacher/Child		Teacher/Pupil		Teacher/Child		Teacher/Pupil	
Poll Tax + Literacy Test	0.00118	-0.000126	-0.00389***	-0.00425***	0.000615	0.00129	-0.00230**	-0.00154
	(0.00158)	(0.00156)	(0.000842)	(0.00114)	(0.00150)	(0.000958)	(0.000930)	(0.00169)
Log(Population)	-0.00267	0.00200	-0.00994***	-0.0128***	-0.00791	-0.00146	0.00108	0.0238*
	(0.00383)	(0.00435)	(0.00192)	(0.00317)	(0.00546)	(0.00458)	(0.00585)	(0.0122)
Sample Counties	Border	Border	Border	Border	Border	Border	Border	Border
Years	Census	Census	Census	Census	Census	Census	Census	Census
1860 Controls	Baseline	All	Baseline	All	Baseline	All	Baseline	All
Pair-Year FE	Y	Y	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y	Y	Y
N	1410	1410	1662	1662	1412	1412	1641	1641

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors multi-dimensionally clustered on border-segment X year and state.Baseline 1860 controls are year specific effects of log 1860 fraction urban, log 1860 improved acreage, and log 1860 farm acreage. All 1860 controls include all the dependent variables in Table 2.

Table A2: Other Outcome Variables

	Political Variables							
	Hirano-Snyder Congress Data				Lynchings (1880-on only)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:	Log(Turnout)		Log(Frac. Democrat)		Black Lynchings		Black Lynchings >0	
Poll Tax + Literacy Test	-0.205***	-0.128***	0.0657***	0.0582**	0.0751	0.220***	-0.00502	0.00290
	(0.0370)	(0.0409)	(0.00675)	(0.00995)	(0.0784)	(0.0734)	(0.0146)	(0.0197)
Log(Population)	0.374	0.524***	-0.0379	-0.0839***	0.0975*	0.0992	0.0539**	0.130***
	(0.268)	(0.181)	(0.0269)	(0.0247)	(0.0580)	(0.0836)	(0.0253)	(0.0429)
1860 Controls	Baseline	All	Baseline	All	All	Baseline	All	Baseline
Pair-Year FE	Y	Y	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y	Y	Y
N	2436	2436	2436	2436	1688	1688	1688	1688

	Agricultural and Population Variables									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel B:	Log(Equip. Value/ Acre)		Log(Farm Size)		Frac. Imp. Acreage		Log(Farmland)		Log(Urban Pop.)	
Poll Tax + Literacy Test	0.0152**	0.0144**	-0.0221	-0.0331***	0.00388	0.00414*	0.0444***	0.0382***	0.0175	-0.0909
	(0.00725)	(0.00657)	(0.0156)	(0.0114)	(0.00333)	(0.00240)	(0.0113)	(0.0112)	(0.132)	(0.0808)
Log(Population)	0.206***	0.210***	-0.306***	-0.280***	0.108***	0.108***	0.345***	0.304***	4.348***	4.666***
	(0.0275)	(0.0273)	(0.0637)	(0.0567)	(0.0174)	(0.0160)	(0.0508)	(0.0647)	(0.512)	(0.411)
1860 Controls	Baseline	All	Baseline	All	Baseline	All	Baseline	All	Baseline	All
Pair-Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	3370	3370	3370	3370	3370	3370	3370	3370	3370	3370

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors multi-dimensionally clustered on border-segment X year and state.Baseline 1860 controls are year specific effects of log 1860 fraction urban, log 1860 improved acreage, and log 1860 farm acreage. All 1860 controls include all the dependent variables in Table 2.

Table A3: Heterogeneity by Institutional Characteristics

Panel A: Elections	Log(Total Votes)				Log(Fraction Democrat)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Poll Tax + Literacy Test	-0.125***	-0.136***	-0.128***	-0.120***	0.00648***	0.00856***	0.00670***	0.00592***
	(0.0358)	(0.0291)	(0.0288)	(0.0312)	(0.00179)	(0.00201)	(0.00151)	(0.00191)
X Log(Fraction 1860 Slave)	-0.107***				0.00823***			
	(0.0235)				(0.00129)			
X Mississippi		-0.162***				0.0274***		
		(0.0196)				(0.00391)		
X Cotton Suitability			-0.175***				0.0142***	
			(0.0561)				(0.00463)	
X Plantation				-0.0266				0.00364
				(0.0322)				(0.00300)
N	3370	3370	3370	3370	3370	3370	3370	3370

Panel B:Public Goods	Log(Black Teacher/Child)				Log(White Teacher/Child)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Poll Tax + Literacy Test	-0.232***	-0.195**	-0.243***	-0.238***	-0.0238	0.0145	-0.0332	-0.0136
	(0.0719)	(0.0763)	(0.0765)	(0.0758)	(0.0350)	(0.0323)	(0.0371)	(0.0307)
X Log(Fraction 1860 Slave)	0.0228				-0.0195			
	(0.0932)				(0.0446)			
X Mississippi		0.436***				0.445***		
		(0.0742)				(0.0279)		
X Cotton Suitability			-0.327***				-0.307***	
			(0.120)				(0.0540)	
X Plantation				0.0623*				-0.109***
				(0.0366)				(0.0375)
N	3370	3370	3370	3370	3370	3370	3370	3370

Panel C: Factor Markets	Log(Land Value/Acre)				Log(Fraction Black)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Poll Tax + Literacy Test	0.0329**	0.0410***	0.0342**	0.0289*	-0.00448***	-0.00405***	-0.00463***	-0.00368***
	(0.0157)	(0.0147)	(0.0147)	(0.0164)	(0.00147)	(0.00103)	(0.00112)	(0.000841)
X Log(Fraction 1860 Slave)	0.0119				-0.00591***			
	(0.0113)				(0.00179)			
X Log(Mississippi)		0.0998***				0.00299		
		(0.0191)				(0.00194)		
X Cotton Suitability			0.0527**				-0.00988	
			(0.0224)				(0.00661)	
X Plantation				0.0411**				-0.00714*
				(0.0174)				(0.00385)
N	3370	3370	3370	3370	3370	3370	3370	3370

Notes: * p<0.1, ** p<0.05, *** p<0.01. All specifications control for log population. Standard errors multi-dimensionally clustered on border-segment X year and state. Baseline 1860 controls included: year specific effects of log 1860 fraction urban, log 1860 improved acreage, and log 1860 farm acreage.

Table A4:Linked 1870-1880 Individual Summary Statistics

Panel A: Full Sample

	White			Black		
Variable	Obs	Mean	Std.	Obs	Mean	Std.
Migrate Between 1870-1800 Dummy	21396	0.37	0.48	18095	0.43	0.49
% Change County Agricultural Output	21396	-0.17	0.46	18095	-0.21	0.41
% Change County Black Teachers	21396	0.74	1.47	18095	1.00	1.64
% Change County Land Values	21396	0.28	0.58	18095	0.16	0.52
% Change County Population	21396	0.27	0.22	18095	0.22	0.18
Age	21396	16.53	14.17	18095	15.80	14.18
Age-squared	21396	474.12	631.50	18095	450.64	626.37
Urban	21396	0.10	0.30	18095	0.07	0.26
Literate	21396	0.53	0.50	18095	0.12	0.33

Panel B: Border County Sample

	White			Black		
Variable	Obs	Mean	Std.	Obs	Mean	Std.
Migrate Between 1870-1800 Dummy	2415	0.38	0.49	1895	0.44	0.50
% Change County Agricultural Output	2415	-0.15	0.41	1895	-0.16	0.42
% Change County Black Teachers	2415	0.89	1.40	1895	1.12	1.50
% Change County Land Values	2415	0.37	0.54	1895	0.24	0.56
% Change County Population	2415	0.28	0.16	1895	0.26	0.16
Age	2415	16.38	14.00	1895	15.76	14.17
Age-squared	2415	464.00	620.00	1895	449.07	622.70
Urban	2415	0.05	0.22	1895	0.06	0.24
Literate	2415	0.50	0.50	1895	0.11	0.31

Table A5: 1870-1880 Migration-Income Results

	Dependent Variable is County 1870-1880 Out-Migration Dummy							
	Blacks				Whites			
	All South		Border Counties		All South		Border Counties	
% Change County Agricultural Output	-0.124**	-0.111**	-0.184***	-0.224**	0.0369	0.0337	0.0857	0.0732
	(0.0693)	(0.0571)	(0.0817)	(0.0889)	(0.0460)	(0.0406)	(0.0916)	(0.0854)
% Change County Black Teachers	-0.0142**	-0.0129	0.00577	0.00864	0.00406	0.00418	-0.0127	-0.00694
	(0.00858)	(0.00802)	(0.0200)	(0.0202)	(0.0104)	(0.00948)	(0.0283)	(0.0251)
% Change County Land Values	-0.0119	-0.00987	-0.114*	-0.0904	-0.0323	-0.0294	-0.0381	-0.0289
	(0.0315)	(0.0307)	(0.0659)	(0.0724)	(0.0419)	(0.0362)	(0.0695)	(0.0703)
% Change County Population	0.0355	0.0159	0.162	0.226	0.0292	-0.00543	-0.439***	-0.436***
	(0.113)	(0.106)	(0.243)	(0.248)	(0.181)	(0.151)	(0.210)	(0.207)
Age		-0.0133***		-0.0137***		-0.0147***		-0.0160**
		(0.00329)		(0.00512)		(0.00373)		(0.00719)
Age-squared		0.000238***		0.000269***		0.000248***		0.000290**
		(0.0000574)		(0.0000956)		(0.0000646)		(0.000122)
Urban		-0.0313		0.222**		-0.0398		-0.0143
		(0.0483)		(0.100)		(0.0589)		(0.109)
Literate		0.0226		-0.0422		0.00372		-0.0524
		(0.0228)		(0.0547)		(0.0180)		(0.0463)
Sample								
N	18095	18095	1895	1895	21396	21396	2415	2415

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors are clustered at the county level. Data is individual level data from the linked IPUMS census schedules from 1870-1880.