The Economic Consequences of Immigrant Disenfranchisement: Evidence from the United States

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What are the effects of disenfranchisement on labor market outcomes? This paper studies a little-known episode in United States history in which twenty-three states and territories disenfranchised non-citizen immigrants from 1864-1926. This disenfranchisement represented a significant shock to the political equilibrium of the time: mayoral and gubernatorial voter turnout fell by 8.0 and 5.7 percentage points, respectively, in the years after disenfranchisement. Moreover, there is evidence that this political exclusion had real economic effects. Disenfranchised immigrants were 45-54% less likely to work in the public sector, and, as measured in Census samples from 1940-1960, children of immigrants who were exposed to disenfranchisement earned 6% less, as adults, than comparable children of natives. I document two contrasting mechanisms: states reduced spending on education following disenfranchisement, but disenfranchised immigrants were more likely to complete naturalization proceedings, thereby signaling their civic-mindedness or intent to assimilate.

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Introduction

In 2013, the Supreme Court struck down a key provision of the 1965 Voting Rights Act requiring certain jurisdictions to obtain federal permission – "pre-clearance" - in order to amend their voting laws.[1] This occurred in the midst of a broad movement towards tighter access to the ballot: since 2010, twenty states have implemented measures that have increased the costs of voting along several dimensions.[2] Given the vast body of research linking political institutions and economic development (La Porta et al 1997, Acemoglu et al 2001), it is possible that this movement will have adverse economic consequences for affected individuals.

Despite a large body of literature on the effects of *en*franchisement, there has been less work on the consequences of *dis*enfranchisement.[3] This is most likely a function of the progressive march of history: while the disenfranchisement of African-Americans in the Jim Crow South provides a notable exception, in recent centuries, developed nations are more characterized by extensions, rather than contractions, of the franchise. This study addresses this gap using a little-known series of state-level constitutional changes that, from 1864-1926, disenfranchised non-citizen immigrants in twenty-three states and territories around the United States (Keyssar 2000, Hayduk 2006).[4] These repeals directly disenfranchised recent immigrants who were not yet eligible for citizenship and, among other immigrants who were not yet citizens,

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[1] *Shelby County v Holder*, 570 U.S. (2013)

[2] Measures include requiring photo ID in order to vote, reduction of early voting periods, restrictions on voter registration drives, felon disenfranchisement, and mandating documentation of citizenship. States that passed one or more of these measures since 2010 are AL, AZ, FL, GA, IL, IN, IA, KS, MS, NE, NH, OH, RI, SC, SD, TN, TX, VA, WV, WI. Source: the Brennan Center (https://www.brennancenter.org/sites/default/files/analysis/New_Restrictions_2016.pdf).

[3] Papers that have studied the effects of enfranchisement include Miller 2008, Lott and Kenny 1999, Fujiwara 2015, Hoffman et al 2016, Carruthers and Wannamaker 2015, Funk and Gathman 2006, Aidt et al 2006, Aidt and Dallal 2008, Cascio and Washington 2014, Hinnerich and Petterson-Lidbom 2014, Hodler et al 2015, Kose et al 2015. Naudi (2012) addresses the impact of disenfranchisement in the Jim Crow south, and is the sole work I have been able to locate on the topic.

[4] One legal scholar declared the "history of alien suffrage" as being "largely unwritten" (Raskin 1993). These provisions, to my knowledge, have never before been used in the economics literature.

increased the cost of becoming eligible to vote. I exploit the timing of these repeals to estimate the effects of disenfranchisement on contemporary and intergenerational labor market outcomes for the disenfranchised and their children.

Economic theory provides several possible channels through which immigrant disenfranchisement might affect labor market outcomes for the disenfranchised, but does not yield unambiguous predictions. Disenfranchised individuals, lacking political voice, may face increased discrimination in the labor markets if those markets are not sufficiently competitive; conversely, disenfranchisement may spur outmigration of affected individuals, which could *improve* labor market outcomes for those that remain. Vote-maximizing politicians may shift public expenditures away from the disenfranchised – for example in education or transportation - thereby increasing frictions or increasing the cost of human capital investment, but this is possible only to the extent that spending is targetable, which depends both on the nature of the expenditure and on the spatial distribution of the disenfranchised population. Given that the disenfranchisement restricted the electorate to citizens, it is possible that this served to stimulate the rate of naturalizations among immigrants with a high valuation of voting, which may have signaled a degree of civic mindedness, or assimilation, that employers may demand. Children of the disenfranchised may be affected directly via altered public spending, or indirectly though the transmission of net parental effects.

I exploit the timing of these repeals to provide novel evidence of the effects of immigrant disenfranchisement on labor market outcomes for exposed cohorts. My identification strategy hinges on the fact that state-level disenfranchisements were constitutional, rather than statutory, and that states differed significantly in the ease with which they could amend their constitutions (Lutz 1994). To establish the relevance of these provisions, I estimate the effect of disenfranchisement on voter turnout, and find statistically significant negative effects: 8.0 percentage points in mayoral elections and 5.7 percent points in gubernatorial contests. There is no evidence of a pre-trend driving these results.

My short-run baseline specification leverages the structure of immigration policy at this time: immigrants could initiate naturalization proceedings at any point after arrival – and thus have the right to vote if residing in an alien voting state - but were required to reside in the United States for at least five years in order to become naturalized citizens. This motivates a triple-difference specification in which outcomes are compared between recent (i.e., residing in the country for fewer than five years) and non-recent immigrants, before and after the repeal of alien voting, in states that did and did not have these provisions. This stringent specification effectively controls for unobserved forces that would have affected all immigrants, and requires any confounding factors to vary discontinuously among immigrants at five years of residence in the United States. I find that there is no effect of disenfranchisement on labor force participation, employment status conditional on labor force participation, or occupational standing, and these null effects are relatively precisely estimated. I do find, however, that disenfranchised immigrants were, in my preferred specification, 45% less likely to work in public sector employment. This is robust to a placebo check using native men as a treatment population.

My inter-generational specification uses variation in paternal nativity, birthplace, and birth year relative to the year of disenfranchisement to estimate the effects of childhood exposure to parental disenfranchisement in 1940-1960 Census samples. I find no evidence of systematic effects on either labor force participation or employment status conditional on labor force participation. There is, however, a statistically significant income penalty for children of immigrants born around the time of disenfranchisement: the native-immigrant parental differential increases in magnitude to a statistically significant -6%, relative to the differential for individuals born prior to repeal. The pattern of coefficients suggests a causal role for parental disenfranchisement: effects are strongest for children who would have been exposed to disenfranchisement at an early age, and are not statistically different from zero for individuals who had reached adulthood by the time of disenfranchisement. By way of comparison, the effect of disenfranchisement on income is roughly equivalent to the effect of losing a year of education.

Finally, I explore mechanisms driving these results. I present evidence supporting two mechanisms through which disenfranchisement could have opposite effects on labor market outcomes for immigrants. First, there is evidence for a shift in public spending: states reduced spending on education in response to disenfranchisement, with the spending share devoted toward education falling by 5-7 percentage points, thereby possibly compromising the quality of education received by the children of the disenfranchised. As a second channel, I find that recent immigrants were more likely to complete citizenship proceedings following disenfranchisement, possibly signaling a greater degree of assimilation to employers and therefore mitigating any adverse effects.

This paper primarily contributes to three literatures. First, it augments what is known about the effects of changes in electorate on policies and outcomes. As noted, there is a voluminous body of literature on the effects of *en*franchisement of various groups on voter turnout, public spending, infant mortality, and long-run educational outcomes, but much less on the effects of *dis*enfranchisement (Naidu 2012, Kousser 1980). Second, it contributes to the literature on immigrant assimilation in the early 20th century. The United States experienced a massive wave of immigration from 1850-1930, and while scholars are beginning to understand the processes of economic and social assimilation of these immigrants, relatively little is known about the *political* assimilation of immigrants in this era. That immigrants were politically active during this era has implications for the role of immigrants in the passage of progressive-era legislation, as well the potential for transmission of institutions (Abramitzky and Boustan 2016; Shertzer 2016). Finally, it contributes to the large body of work linking democratization and country-level economic outcomes. This literature is decidedly mixed, with certain scholars arguing that democracy has a negligible effect on growth (Barro 1996) and others that it has a positive effect on growth (Acemoglu et al 2014). The contrasting mechanisms documented in this paper helps to rationalize these differing results, if different mechanisms are more or less prevalent in certain settings.

Section 2. Institutional background and causes of repeal.

a. Context of non-citizen voting

Starting with Wisconsin in 1848, twenty-two other states and territories from 1848-1890 adopted provisions in their state or territorial constitutions that allowed non-citizen immigrants the right to vote in local, state, and federal elections (Hayduk 2006, Keyssar 2000). The pattern of adoption was regional, and proceeded largely in three waves: mid-western states and territories enacted these provisions in the late 1840s and 1850s (WI, MI, IN, MN, NE, KS); Western territories during the Civil War (ID, MT, ND, SD), and Southern states following the Civil War (AL, AR, FL, GA, TX). While to my knowledge there has as yet been no economic work on the determinants of adoption of these law, legal scholars agree that these provisions were intended to lure recent immigrants to land-abundant, labor-scare states in order to expedite the economic development of those states (Neumann 1992; Raskin 1993). Moreover, this is echoed in contemporary sources. In the Michigan constitutional convention of 1850, for example, Delegate W.V. Morrison of Calhoun County contended:

"I consider that by extending the right of suffrage to foreigners, we are advancing the best interests of the State. We have expended thousands to induce emigrants to reside with us, and what have we effected? Wisconsin [which enacted non-citizen voting in 1848] has opened her doors - she has extended to them the right of suffrage, and thousands have poured in, developing the resources and adding to the riches of the State. Thousands have gone 'round by the Lakes; and if you question the travelers upon the Central Railroad, they will tell you they are going to Wisconsin. If any of them remain, their friends in Wisconsin tell them that rights are given there that are withheld in Michigan; and at the first opportunity they leave us and take up their abode there. And we may expend money by our emigration agents, but if we persist in our narrow policy, it will have but little effect."

This sentiment is corroborated in judicial decisions from the mid-19th century. For example, in *Spragins v Houghton* (1840), the Illinois Supreme court noted that the intent of granting the right to vote to immigrants was to "induce a flood of emigration to the state, and cause its early and compact settlement." (Harper-Ho 2000)[5]

An important feature of these provisions is that they were intended as a "pathway to citizenship": they enfranchised those non-citizen immigrants who had initiated citizenship proceedings by filing a "Declaration of Intent". During this era, naturalization was a two-part process in which first, the would-be citizen filed a Declaration of Intent – also known as "first papers" – that stated his or her intention to become a United States citizen. This declaration was not binding, and could be filed at any point following arrival at any "competent court", of which there were approximately 5,000 nation-wide (Rosberg 1977). No less than two years after filing first papers, and after a total period of residence of no less than five years, the applicant could file a petition for citizenship. Obtaining citizenship was not a requirement in this era, and many immigrants never even initiated the citizenship process: among adult male immigrants who had been in the United States for at least five years, the fraction who were *not* citizens was 30% in

[5] I explore the causes of adoption of these non-citizen immigrant voting provisions in the second chapter of my dissertation, Henderson (2016).

1900, 34% in 1910, 48% in 1920, and 35% in 1930 (Ruggles 2015).[6] Becoming a citizen conferred no direct material benefit during this era, although, with the entry of the United States into World War I, naturalized immigrants were liable to be drafted into the armed forces (Shertzer 2015). I leverage the structure of the naturalization process in my econometric specifications below.

States and territories began to repeal alien voting provisions starting with Nevada Territory in 1864, when it drafted a state constitution and was admitted to the Union. Georgia followed in 1877 with its new state constitution at the end of Reconstruction, and the mass of repeals began starting in 1889. Nine states and territories repealed their laws in either 1889 or the 1890s, either because they enacted alien voting as a territory but, upon entry to the Union, did not include alien voting in their new state constitutions (ID, MT, WA, WY) or because they amended their existing state constitutions to directly prohibit non-citizen voting (FL, LA, MN, MI, ND). The remaining twelve states that repealed these provisions after 1900 did so via constitutional amendment, except for Alabama in 1901 and Oklahoma in 1907. Repeals occurred at a relatively even pace after 1900, with four states each repealing their provisions over nine year intervals. Arkansas was the last state to have in place constitutional provisions allowing non-citizen immigrants the right to vote, repealing that provision in 1926.[7] See figure 1 for a map of the dates of repeal by state.

b. Causes of Repeal

[6] Although the fraction of *all* immigrants who were not citizens may be a more complete representation of immigrant behavior, the Census only collected citizenship status for adult males in 1900 and 1910.
[7] It is still the case that non-citizens are excluded from the electorate in all but a handful of local election, although there are current proposals to re-expand the franchise. See http://ronhayduk.com/immigrant-voting/ for a comprehensive overview.

Legal scholars generally agree that these non-citizen voting provisions were repealed largely due to rising anti-immigrant sentiment in the late 19th and early 20th century (Rosberg 1977, Raskin 1993, Tienda 2002). Nativism was growing strongly during this period due, most likely, to the changing composition of immigrants from Northwest Europe to Southeast Europe, the Panic of 1893 and ensuring depression, the assassination of President McKinley in 1901 by a second-generation immigrant, and the United States' involvement in World War One (Jaret 1999; Van Nuys 2002). This is mirrored by the several attempts of Congress, from 1897-1917, to restrict immigrant to those immigrants who were literate (Goldin 1994). An additional reflection is the convening of the "Dillingham Commission" in 1907, a congressional Commission ordered to "make full inquiry… into immigration" (Dillingham Commission Volume 1, Pg. 9). This rising sentiment led to the immigration restrictions in 1921 and 1924, which effectively ended the age of unrestricted mass migration to the United States, and the era of alien voting ended shortly thereafter.

Over the 19th century, non-citizen voting was a convenient target for nativists during periods of upheaval. Following the War of 1812, "which produced a militant nationalism and suspicion of foreigners" (Raskin 1993), several states that had previously extended the franchise to "inhabitants" altered their constitutions to restrict the franchise to "citizens". Moreover, states that entered the union in the period following the war adopted constitutions that did not allow non-citizens the right to vote. The mass immigration of Germans and Irish starting in the late 1840s provoked a similar sentiment: during a state constitutional convention in 1856, the Know-Nothing party – an openly anti-immigrant political organization that experienced brief popularity during the 1850s due to the perceived threat from rising Irish and German immigration – denounced states that had extended the franchise to non-citizen immigrants (Rosberg 1977).

Newspapers from the time corroborate the idea that anti-immigrant sentiment drove repeal: a 1902 editorial by the *Washington Post* decried that fact that "there are many States where an immigrant is not obliged to become a full-fledged citizen in order to be the peer of the native on election day" because these non-naturalized aliens who "take part in controlling our government… hold their allegiance to the sovereignty they left behind them." To the extent that the repeals of non-citizen voting provisions were driven by uniformly rising anti-immigrant sentiment across the country, identification of disenfranchisement effects is not threatened. Year fixed effects will control for broad trends in attitudes toward immigration, and state-specific time trends allows for differentially evolving unobserved attitudes.

Given that the legal basis for non-citizen voting was constitutional, rather than statutory, the timing of repeal of non-citizen immigrant voting was also determined in part by the ease with this states could amend their state constitutions. States vary significantly in the process of amending the state constitution, and "the variance in amendment rate is [partly explained]… by the difficulty of the amendment process." (Lutz 1994). In virtually all states (except Delaware), the process of constitutional amendment begins with passage in the legislature and ends with a popular referendum, with the amendment being adopted if it received a majority of votes in the referendum. Importantly, states differ on the method by which a majority is calculated in the referendum. Certain states require that a majority of "the electors voting at such election" constitutes passage of the amendment; however, to the extent that not all voters in the election vote for the referendum, this requires a passage rate of higher than 50% of those voting. Other states, however, allow for passage when the referendum "is approved by a majority of the votes cast thereon" (Aylsworth 1931). To the extent that voters vote only on a subset of elections on a particular ballot – a practice known as "roll-off voting", well-documented in economics and political science (Stephens and Charles 2013, Bullock and Dunn 1996) – then these latter states effectively impose a more stringent standard for constitutional amendments.

There is anecdotal evidence that the stringency of the amendment process explains the timing of repeal of noncitizen voting. In 1919, voters passed a proposed amendment in Arkansas that would have restricted the franchise to only citizens, 87,237-49,757. Given that 190,113 total votes were cast in the election, however, the vote count for the proposed amendment did not constitute a majority of *electors* voting at the election, and so the measure failed. Similarly, Nebraska in 1910 attempted to disenfranchise non-citizen immigrants and the referendum passed, 100,450-74,878, but failed to achieve a majority of the electors voting in the election (243,390). Only in 1918 did voters in Nebraska pass the constitutional amendment with a majority of all voters voting at the election (123,292 – 51,600, with 225,717 total votes cast).

Understanding the determinants of repeal is crucial: forces that may have been driving repeal could also drive any estimated effects, with those effects then erroneously attributed to the disenfranchisement. Therefore, I examine the determinants of the timing of repeal by estimating the relationship between state-level covariates as of 1890 and the year of repeal. Given that my sample size is so limited (n = 19), I present a series of scatterplots for demographic, political, and economic state characteristics (data sources are discussed in section 3.A, below). See figure 2 for details. There are two covariates which individually have a statistically significant relationship with the year of repeal: the fraction foreign born, and the fraction of adult foreign-born males that are not citizens. In order to test for the relative importance of these factors, I estimate the following simple specification:

$$Y_j = \beta D_j + \gamma L_j + \varepsilon_j \qquad (1)$$

D_j is a vector comprising the fraction foreign born and the fraction of adult male immigrants who are not citizens as of 1890. L_j is a vector of legalistic variables containing an indicator for whether the repeal took place via amending the state constitution, an indicator for the ease of repeal of the state constitution, and an interaction between these latter terms.[8] Given that states can only repeal their provisions conditional on adopting those provisions, the sample is limited to those states that had ever adopted alien voting provisions. Standard errors are bootstrapped to account for the small sample size.

Results are presented in table 1, and indicate that for states that had amended their constitutions in order to repeal the provisions, the stringency of amending was a significant determinant of the date of repeal. Amending states with a loose standard repealed their laws, on average, 11.5 years earlier the states with a strict standard, and this is statistically significant despite the very limited sample size. It also appears that the state which elected to repeal the provision via amendments to state constitutions, rather than by creating entirely new state constitutions, tended to repeal their provisions later. This is not surprising the costs involved in amending a state constitution: territories joining the union in the 1890s and early 1900s avoided the high cost of a separate constitutional amendment by incorporating the repeal into their new state constitutions. There is still some evidence that states with a high fraction of adult male non-citizen immigrants tended to repeal their provisions earlier, but controlling for legalistic variables reduces this estimate in size and statistical precision.

[8] I coded the indicator on the ease of constitutional amendment based on the text of the state constitution prevailing at the time of disenfranchisement. Those state constitutions that allowed for adoption of the proposed amendment based on a majority of the voters voting "thereon" was coded as having a loose standard of repeal; states that required a majority of all voters, were coded as a strict standard of repeal. I was not able to discern the ease of amendment for the state of Georgia.

In sum, a number of state-level demographic, political, and economic factors bear no statistical relation to the timing of repeal of alien voting provisions in both level and trend. The only meaningful predictors of the timing of repeal were whether states repealed their provisions via constitutional amendment or via an entirely new constitution and, more importantly, the ease with this the constitution can be amended. Why did some states have loose standards, and some strict standards? I defer this discussion to Henderson (2016), in which I discuss in greater detail the determinants of adoption and repeal of these provisions. However, to the extent that the determinants of the why states adopted certain constitutional amendment processes are orthogonal to unobserved trends jointly influencing the year of repeal and the outcome under study, this provides plausibly exogenous variation in the timing of repeal. I next discuss my data and specifications.

Section 3: Data and Methodology

a. Data sources

In order to quantify the effects of non-citizen disenfranchisement, I use data from a variety of sources. To assess the effects on vote totals and party vote shares, I collected municipal elections data from eleven large cities from 1880-1924 from *the Biographical Dictionary of American Mayors, 1820-1980.*[9] Where possible, I verified reported election vote totals with results from historical newspapers. Municipal election totals are surprisingly scarce from this era; therefore, I therefore, I supplement the municipal-level election results with state-level gubernatorial election data from 1870-1940 Burnham et al (1991).[10] State-level spending

[9] Milwaukee, Detroit, New Orleans, St. Louis, Baltimore, Buffalo, Cincinnati, Cleveland, Chicago, New York, and Pittsburgh

[10] The dearth of systematic municipal election results has been noted in the political science literature: "no national statistics on municipal elections are reported, and few states compile such information concerning their own municipalities." (Morlan 1984)

data was generously provided by Lawrence Kenny (Lott and Kenny (1999)), and covers 1870-1940 in four broad categories: aggregate, and then disaggregated to education, social services, and transportation. Data on the composition of state legislatures is from Burnham (1989).

I adjust spending levels for inflation using the historical CPI from the Minneapolis Fed. State-level demographic data from 1890 are from ICPSR 2896 (Haines 2010), and, for items not included in that dataset, are from the original 1890 census tables. I combine these with IPUMS census samples from 1900-1930 to construct state demographic covariates. I use IPUMS Census samples from 1900-1930 to examine the effects of repeal on the allocation of public employment, and the 1940-1960 IPUMS Census sample in order to explore the intergenerational effects of disenfranchisement.

Scholars disagree about the exact dates and durations of alien voting: “determining which states allowed aliens to vote is a difficult task” and “secondary sources on the subject of alien suffrage have disagreed about how many, which, and when states allowed noncitizens to vote.” (Rosberg 1977, Tienda 2002) Various authors have pegged the number from “at least 22” (Raskin 1993; Harper-Ho 2000), to as many as 35 (Tienda 2002). Much of this murkiness, however, stems from alien voting provisions in the early 19th century; focusing on repeals, occurring as they did from 1864-1926, allows for a much greater degree of certainty. I confirmed every date of repeal using the NBER/Maryland state constitution database (Wallis).

b. *Short-run Specification*

The timing of repeal bears significantly on my estimation strategy. Unobserved forces driving the repeal of these provisions may result in the misattribution of the consequences of those forces to disenfranchisement. Historical circumstances, however, argue for the existence of random component in the timing of repeal. Not only did states differ in the stringency of the standard required for constitutional amendment in the popular referendum, there was variation in the pre-referendum procedures required for constitutional amendment: some states require proposed amendments to be passed only once by state legislatures, while others required passage twice in succession.[11] To the extent that the method of constitutional amendment does not generally vary within states over time, the source of this variation is captured in the state fixed effects. Common shocks to all states – such as the passage of the 1921 and 1924 immigration acts – are captured by year fixed effects, and non-linear shocks to regions, such as changing composition of immigrant source countries – can be accounted for with region-year fixed effects. Finally, smoothly evolving trends can be accounted for by state-specific linear trends.

Defining the disenfranchised population is not completely straightforward. The alien voting provisions allowed "declarant aliens" – that is, those that had initiated citizenship proceedings by filing first papers – the right to vote. Upon the repeal of these provisions, only naturalized immigrants, who had obtained citizen status, could vote. Detailed citizenship status is measured in the 1900-1930 Censuses, which overlaps with much of the period in which states repealed their alien voting provisions, so it is possible to compare outcomes for individuals that are not citizens before and after repeal of the provisions, between states that did and did not enact these provisions. Attendant with this specification, though, is a selection issue: in this era, immigrants could, after specified periods, alter their citizenship status. This raises the possibility of selection into citizenship, which could bias the estimated effect of disenfranchisement if the pool of immigrants that remain non-citizens after repeal differs systematically from the pool of non-citizens prior to repeal. This selection could either bias true negative effects toward zero, if

[11] Alien voting states requiring passage once: WA, WY, ID, FL, MI, MT, MN, LA, AL, CO, OK, OR, KS, NE, SD, TX, MO, AR; alien voting states requiring passage twice in succession: NV, GA, ND, IN, WI

those immigrants that remained non-citizens after repeal would have derived little benefit from voting (and, conversely, suffered little penalty from disenfranchisement). This could generate spurious negative effects if, however, the pool of immigrants that remained non-citizens after repeal were systematically less motivated to assimilate, both politically and economically, and this was also manifested in worse labor market outcomes.

To accommodate unobserved forces possibly common to all immigrants while also avoiding the issue of selection into citizenship, I exploit a discontinuity in the immigration policy of this time that identifies a treatment group that is not susceptible to endogenous naturalization: the pool of recent immigrants. As set forth in the Immigration Act of 1798, immigrants needed to maintain a residence of five continuous years in the United States in order to obtain citizenship. However, it is important to note that they could declare their intent to file for citizenship at any time after arrival. Thus, recent immigrants in alien voting states where the provisions were enacted were potential voters; after the provisions were repealed, they were disenfranchised until the completion of the five-year residency period and naturalization proceedings. This cutoff provides a natural treatment group: immigrants who had resided in the United States for fewer than five years. Specifically, using IPUMS Census samples from 1900-1930, I estimate the following model:

$$Y_{ist} = a_s + c_{t(r)} + d_s t + \beta R_{st} + \alpha Rcnt_{ist} + \gamma R_{st} * Rcnt_{ist} + \delta X_{ist} + G_{st} + \varepsilon_{ist} \qquad (2)$$

Y_{ist} is an outcome for individual *i* in state *s* and year *t*; X_{ist} is a vector of individual controls comprised of age, age squared, literacy status, an indicator for urban residence, region of birth indicators, year of immigration indicators; G_{st} is a vector of state-level controls comprising an indicator for the presence of laws barring aliens from public employment, and an indicator that takes the value 1 in the decade prior to the repeal of the voting provisions (Holmes 2003;

Fishback et al 2009).[12] This latter variable is intended to capture, in the repeated cross-section, unobserved forces leading to repeal (Hungerman et al 2015). The sample is restricted to foreign-born non-farm males aged 21-64 that had resided in the United States for fewer than fifteen years.[13] Eleven of twenty-three alien voting states and territories had repealed their provisions prior to 1900; I exclude these from the sample (results are robust to their inclusion). The variable R_{st} is an indicator that takes a value of 1 for the period following repeal for states that had the non-citizen voting provisions, and zero at all other times. The coefficient of interest is γ: it estimates the differential impact of repeal for recent immigrants, relative to less recent immigrants. I employ state and decade fixed effects in order to control for unobserved fixed factors that vary across states and time, respectively, and region-decade fixed effects and state-specific linear trends to account for time-varying shocks. I use IPUMS-provided person weights in estimation, and standard errors are clustered at the state level (Bertrand et al, 2004).

Given the nature of the disenfranchisement, it is preferable to use an econometric model that allows for the estimation of dynamic effects. It is likely that the effects of this episode of disenfranchisement changed over time, and possibly in different ways for different outcomes. The repeal of these non-citizen voting provisions in state *s* and year *t* affected two groups at different points: the *stock* of non-citizen immigrants in state *s* in repeal year *t*, and also the *flow* of future immigrants who, under the naturalization laws of the time, would have to reside in the country for at least five years in order to petition for citizenship. In the simplest scenario of constant immigrant flows, full citizenship, and no return migration, then the population of non-citizen immigrants is constant over time. However, it is also possible that there might be a

[12] 17 states had enacted this type of law as of 1924 (Holmes 2003; Fishback et al 2009). There is little overlap between this group and the states that had repealed, alien voting: only Indiana and Oregon are common to both.
[13] This cutoff was selected in order to maximize sample size while balancing the need to minimize the differences between recent and non-recent immigrants. Estimates do not change in magnitude with fewer years of residence in the United States, but lose precision.

“bulge” pattern: a large pool of non-citizens at the time of repeal is analogous to a large pool of immediately disenfranchised individuals (i.e., the stock).[14] If, for example, every citizenship-*eligible* non-citizen initiated citizenship proceedings at the time of repeal, then the stock effect would dissipate with a few years, leaving only the flow effect, the magnitude of which would depend on the annual level of migration to the United States. Because of data limitations, I am unable to differentiate between the two effects in my short-run specification; however, my intergenerational specification, below, allows for this.

c. Intergenerational Specification

There is a growing body of literature that supports the possibility of long-run effects of disenfranchisement. Notably, Kose et al (2015) studies the long-run effects of women’s suffrage using an event study specification similar to that employed here, and finds evidence for large positive effects of exposure to female suffrage on educational attainment of children: full exposure to women’s suffrage from ages 0-15 resulted in an additional year of education for black children. More generally, studies have shown that parental stress has adverse effects on child outcomes, with stressors ranging from financial (Leininger and Kalil, 2014) to emotional (Black et al 2014) to environmental (Currie and Rossin-Slater 2013).

Unfortunately, due to data limitations, I am not able to extend specification (2) which identifies the disenfranchisement effect using a discontinuity in duration of residence in the United States because available datasets do not collect the year of *parental* immigration. Therefore, as an approximation of parental disenfranchisement I focus on children of *all* immigrants. To the extent that this includes immigrants that were not disenfranchised by virtue

[14] IPUMS Census samples from 1900-1930 indicate that approximately 40% of immigrants did *not* pursue citizenship while in the United States.

of having been citizens, then estimated effects will tend to understate the true treatment effect. I estimate the long-run effects of parental disenfranchisement as the differential effect between children of natives and children of immigrant fathers of being born in a particular birth cohort and birth state, relative to the date of repeal of alien voting in that state. I use the children of immigrant fathers, rather than children of immigrant mothers and fathers, as my implicit treatment group because, in thirteen of the twenty-three alien voting states and territories, women's suffrage was enacted later than alien disenfranchisement; therefore, having an immigrant mother and native father should yield no direct effects of disenfranchisement on children. Furthermore, until 1922, married women were automatically assigned the citizenship status of their husbands, implying that a child of a foreign-born mother and native father would not be exposed to parental disenfranchisement, as the mother would not be disenfranchised (Smith 1998).

Using IPUMS samples from 1940-1960 together with the timing of repeal of alien voting provisions, I estimate:

$$y_{ibctr} = b_b + c_c + c * b_b + \sum_{f=-h}^{h} D_b \pi_f 1(c \in f) + \sum_{f=-h}^{h} P_{ibc} D_b \alpha_f 1(c \in f) + \beta X_{ibc} + \varepsilon_{ibc} \quad (3)$$

Y_{ibct} is an outcome for individual *i* born in birth state *b*, in birth cohort *c*, observed in census year *t* in census region *r* (for brevity, I omit the *r* and t subscripts on all other individual-level covariates in the specification). I control for fixed factors within birthplaces and factors common across birth years by including birthplace and birth cohort fixed effects. I further control for unobserved location-specific trends by including birth-state specific linear trends. Individual controls are race indicators, age, age squared, years of education, father birth region, and father nativity. The event-time indicators are grouped into bins of five years, with membership of individual born in cohort c in bin f denoted by $1(c \in f)$. These are a measure of exposure to the repeal of alien voting; for example, an individual born in 1906 in Wisconsin (which had a repeal year of 1908) will receive a value of 1 for the indicator denoting birth year 0-5 years prior to repeal, and a value of 0 for all other years. Individuals not born in alien voting states will receive a value of 0 for all exposure indicators. P_{ibc} is an indicator for having a foreign born father. The coefficients of interest are α_f: these trace the time path of the father-nativity differential for birth cohorts in relation to the year of repeal of the alien voting law. Census year fixed effects are included to control for the aging of the population, and the sample is restricted to males age 25-64. I include IPUMS person weights in estimation, and cluster standard errors at the birth state level.

It is important to note that exposure indicators imperfectly capture true exposure. Individuals born in a repeal state may move out of state prior to repeal, thus avoiding exposure to the post-repeal environment, and individuals not born in a repeal state may do the opposite. If such migration does not systematically vary with labor market potential, then it should only serve to attenuate my estimates. If, however, disenfranchisement spurred out-migration among positively selected individuals, then the composition of the remaining individuals, and their children, may generate a spurious negative intergenerational effect of disenfranchisement. Such a response would only be consistent with a very high implicit valuation of the right to vote; therefore, this possibility is unlikely. I return to this question in section 5, in which I discuss potential mechanisms for observed results.

Section 4: Results

a. Voter turnout

As a first step, I estimate the effects of the repeal of alien voting on voter turnout at the city and state levels in the late 1ate 19th and early 20th centuries. This is a necessary step for the analysis – akin to the first stage of a two-stage regression – but it is also of substantive interest. There is a large qualitative body of work on immigrant political participation in this era, but little by way of systematic analysis. There is evidence that immigrants in large cities were more likely to vote when doing so was necessary for the formation of a winning political coalition (Shertzer 2016), which suggests that immigrants considered the costs and benefits of voting; however, there is also anecdotal evidence of urban political machines using immigrant voters as a source of (extralegal) electoral support (Allen and Allen 1981). Such manipulation, to the extent that it was widespread, could serve to lessen the impact of alien disenfranchisement on voter turnout aggregates if those machines simply arranged for immigrants to be naturalized quickly in time for elections in spite of the residency requirements.

I first estimate the impact of the repeal of these laws on voter turnout at the municipal level using a panel of eleven large cities from 1880-1924. I construct voter turnout as the ratio of total recorded votes per election to the decennially interpolated population of males age 21 and over.[15] I estimate a parsimonious event study specification, including city fixed effects, year fixed effects, city-specific linear trends, and an indicator for whether women's suffrage is in effect. Because frequency of mayoral elections varies across cities – it ranges from two to four

[15] While this definition of voter turnout does not factor into the denominator the female population, when women's suffrage is in effect, female voters are accounted for both by the indicator for the presence of women's suffrage, and year fixed effects (once all states had adopted it, as they did by 1920). Finally, to the extent that this misses female voters, it will overstate true turnout, thus biasing the effects of disenfranchisement upward toward zero.

years in my sample – I use four-year bins in the event-study specification, and estimate three pre-treatment bins and post-treatment bins. The omitted bin corresponds to zero to four years prior to repeal.

Results are in figure 3, where coefficients from the event-time indicators are plotted. Regression results are in Appendix table 1a. There is a 9.3 percentage point drop in mayoral voter turnout following repeal of immigrant voting provisions, growing larger and gaining in statistical significance over time. Collapsing the event-time indicators into pre- and post-repeal periods, the average effect is a reduction of 8.3 percentage points (p = .057). Relative to the sample average turnout of 53.2%, this treatment effect scales into a 15% reduction. While meaningful, this is easily rationalized by the demographics of the sample cities: the male, voting-age population in these cities was, on average, 44.3% foreign-born. If, conservatively, 60% of the adult male immigrant population completed citizenship proceedings, then the disenfranchisement would affect the remaining 40% of the immigrant stock, which scales to (.4 * .443) 17.7% of the adult male population of the city. If only half of these individuals vote, and are thus barred from the polls by disenfranchisement, we would expect to see a reduction in voter turnout of 8.8 percentage point: approximately the effect size I estimate. Consistent with a causal effect of repeal of non-citizen voting provisions, I am unable to reject the null hypothesis that the coefficient on the pre-repeal bins are all equal to zero (f = .56, p = .64).

Given the small sample of cities in this analysis, this evidence must be interpreted with caution. I turn to the more complete gubernatorial election data as a check for these results. Gubernatorial elections also varied in their frequency during this period, just as the mayoral elections did, so I again use four-year bins in the estimation. However, because the data is more extensive – ranging from 1870-1940 – I am able to estimate the event-study for 16 years pre- and

16 years post-repeal. Again, I include year fixed effects, state fixed effects, and state-specific linear trends, and an indicator for the presence of women's suffrage. See figure 4 for a plot of the regression coefficients of the event-time indicators. There is a statistically significant reduction in voter turnout of 5.2 percentage points in the four-year period immediately following repeal, and this continues for at least sixteen years post repeal. The average effect size over this period is a statistically significant 6.6 percentage points ($p<.01$). Again, I am able to jointly rule out that the coefficients for the pre-repeal bins are jointly different from zero ($f = .68$, $p = .61$). Coefficients from the event-study specification are reported in Appendix table 1b.

b. Short run effects of disenfranchisement

I estimate specification (2) on IPUMS samples from 1900-1930. For outcomes, I use labor force participation, employment status conditional on labor force participation, occupational standing, and the likelihood of having a government job. Because the Census only started reporting government employment as a separate category starting in 1940, I manually assembled the indicator for having a government job based on employment codes in the IPUMS samples. I use three alternative measures, the construction of which is detailed in Appendix 2.

Results from this specification are in table 2. I find precisely estimated null effects for labor force participation, employment status conditional on labor force participation, and occupational standing. Perhaps unsurprisingly, recently immigrants generally have worse labor market outcomes than those with a longer period of residence in the United States, most likely reflecting greater assimilation. However, there is no evidence that this effect differs after alien voting is repealed. I do, however, find that disenfranchised immigrants were significantly less likely to obtain government employment. This is effect is statistically significant in two of the three definitions of what constitutions government stable across specification, despite the measures government employment being imperfectly correlated (pairwise correlations .55, .71, and .56). The effect sizes, while small in magnitude, are large relative to the overall likelihood of obtaining public employment in the sample. Public employment was not common in this era: of all adult males, 2-3% percent had a public sector job (depending on the definition), and among male immigrants, the frequency was .4% - .7%. Thus, the coefficient magnitudes correspond to large relative effects: 45% and 54% reductions for definitions one and three, respectively.

As a placebo check, I re-estimate specification (2) on a sample that ought to have been directly unaffected by repeals: native adult males. Results are in table 3. While there is scope for potential general equilibrium effects – since the disenfranchisement of a fraction of the electorate renders the remaining voters more electorally valuable – this is likely to be small. This placebo check confirms this: there is no effect of alien disenfranchisement on labor force participation, occupational standing, or public sector employment for native males. There is a slight positive effect on employment status, but, relative to the sample mean of .916, this effect is negligible.

c. Intergenerational Effects of Disenfranchisement

I estimate specification (3) using IPUMS Census samples from 1940-1960. As outcomes, I use labor force participation, employment status conditional on labor force participation, and the natural log of wage and salary income. Results are in Figures 5 – 7, with the underlying regression output in appendix table 3.

There appears to be little intergenerational impact of parental disenfranchisement on labor force participation. The children of immigrant fathers were consistently less likely to be in the labor force, and the pattern of coefficients does not vary in a manner that suggests that disenfranchisement was a driving force. This is consistent with other research that finds that second-generation immigrants perform poorly relative to natives (Algan et al, 2010; Riphahn 2002). Similarly, I find little consistent evidence for an effect of disenfranchisement on employment status conditional on labor force participation.

I do, however, find a negative and statistically significant effect on income. For individuals born well prior to disenfranchisement, adult income of children of natives and children of immigrant fathers are statistically indistinguishable: the interaction terms on birth year bins are jointly statistically significant for individuals born five or more years prior to repeal (f = .18 p = .96). However, this differential widens sharply for those second-generation immigrants who were exposed to parental disenfranchisement at an early age: the father nativity gap for individuals who were born from five years prior to repeal, to twenty years after repeal, increases in magnitude to approximately 6%. These coefficients are jointly statistically significant at the 10% level (f = 2.25, p = .068), and, collectively, I can reject that the event-time coefficients are equal with strong significance (f = 2.47, p = .015). By way of context, the effect size of early-age disenfranchisement is almost identical in magnitude to an extra year of education in the sample (measured, in this sample, at 7.0%).

Given the joint statistical insignificance of pre-repeal periods, this pattern of coefficient estimates is consistent with a causal interpretation of disenfranchisement on the income of exposed children. This argues for a shock occurring to the income potential of exposed children with effects most salient for young children (or else there should be an effect for individuals born 5-10 years prior to disenfranchisement). It is important to note that because of attenuation bias due to both mis-measurement of the treated individuals (since not all immigrants were disenfranchised), as well as to possible migration. I now turn to the mechanisms driving these results.

Section 5: Mechanisms

The short-run results - that disenfranchisement had little effect on private labor market outcomes, but significantly reduced the probability of obtaining public sector employment – can be rationalized given the research on labor market institutions and political economy of the era. There is evidence that labor markets in the early 20th century were reasonably competitive (Fishback 1998, Rosenbloom 2002) insofar as more hazardous occupations paid higher wages in order to compensate for the risk, workers migrated from low-wage areas to high wage areas, and information costs were not insurmountably high. This would tend to predict a null effect of disenfranchisement on private labor market outcomes: the rents employers may earn by discriminating against disenfranchised workers falls as employers compete for those workers.

Furthermore, while labor markets were competitive, the markets for public employees were not. There is a large body of research demonstrating that this was an era of pervasive patronage employment in state and local governments, despite the abolition of federal patronage employment with the Pendleton Act of 1883 (Libecap 2007, Rauch 1995, Ujhelyi 2014, Folke et al 2011). If public employment was allocated according to electoral value, then it follows that disenfranchised immigrants, no longer directly valuable to politicians, would lose the patronage *largess*.

In this section, I test for mechanisms through which disenfranchisement may have affected intergenerational labor market outcomes. What explains the negative impact on income for the children of the disenfranchised? In particular, I test for migration responses; the effect of disenfranchisement on the incidence of naturalizations for immigrants; and shifts in political parties and public spending.

a. *Migration Response*

Scholars have shown that labor was relatively mobile in this era (Fishback 1998), which raises the possibility of a migration response to disenfranchisement: recent immigrants may have "voted with the feet" and re-located to states with immigrant voting laws still in place or, failing that, return to their home countries. While mass out-migration could effectively improve labor market outcomes for the immigrants that remain, selective out-migration could generate a spurious compositional effect: if better-educated immigrants exit the state in response to disenfranchisement, then the estimates based on remaining immigrants will, mechanically, appear to have worsened.

There are two factors, however, arguing against such a migration response: first, for immigrants who had resided in the United States for at least five years, applying for citizenship was a means of regaining the vote, mitigating the need for migration. While this did entail certain costs – waiting for the appropriate residency periods, filing paperwork, and paying small fees – they were almost certainly far less than costs of moving. Second, anecdotal evidence from this time suggests that a common method of directly purchasing votes was vis-à-vis the purchase of free alcoholic drinks (Allen and Allen, 1981); for this to have been an effective means of voter fraud, the average valuation placed on casting an independent vote must have been relative low. This, in turn, makes is relatively unlikely that immigrants would cross state lines in order to have the right to vote.

However, I empirically test for a migration response of disenfranchisement. Using IPUMS samples and published state aggregates, I create a state-decade panel from 1870-1940 and test for effects of disenfranchisement on overall immigrant population, the fraction of the population that is immigrant, and, in order to assess the extent of selection into migration, the fraction of immigrants in the population that are illiterate. Ideally, I would estimate the migration response among *recent* immigrants following on the spirit of specification (2), but data on year of immigration is only available starting in 1900.[16] I estimate an event-study specification including state fixed effects, decade fixed effects, and state by decade linear trends. The data – occurring only at decadal intervals – is not fine enough to estimate precise treatment effects, but allows for general assessment of the extent to which migration may be driving the estimated results (either through its effects on labor supply, or the composition of remaining workers).

Results are in table 4. I find little evidence of a systematic migration response. Both total population and foreign-born population were falling in alien voting states over this period – foreign-born slightly faster than total – but the effect is largely statistically insignificant. More importantly, it does not appear that the out-migration was driven by the foreign-born: the fraction of the overall population that is foreign born falls slightly in alien voting states, but the coefficients are not statistically distinguishable from zero (f = .76, p = .62). Finally, there appears to be a reduction in the incidence of illiteracy among the foreign-born following repeal – suggesting, if anything, negative selection out of the state – but, again, these coefficients are not jointly distinguishable from zero (f = .57, p = .77). If anything, this selection effect should serve to bias the negative effects of disenfranchisement toward zero.

b. Citizenship Response

It is possible that the repeal of alien voting provisions may have encouraged immigrants to complete naturalization proceedings. Those with a relatively high valuation of the act of voting would, upon repeal of the provisions, take the steps required to complete naturalization

[16] While an event-study is not feasible given the limited years available, difference in difference estimates show that the effect of repeal on the fraction of the foreign-born population that were recent immigrants is not statistically distinguishable from zero. Results available upon request.

and, therefore, be able to continue voting. This, in turn, could signal to employers a high degree of civic mindedness or assimilation, thereby stimulating labor demand. Existing research confirms this as a possible mechanism: scholars have found that naturalization conveys a significant wage premium (Steinhardt 2012, Bratsberg et al 2002, Fougere and Safi 2009).

I test for this by extending specification (2), but now estimating the effect of repeal on the incidence of naturalization for immigrants that had resided in the United States for up to ten years, relative to those with ten to twenty years of residence. Results, presented in table 5, indicate that recent immigrants responded to disenfranchisement with a strong drive to citizenship. Immigrants who had arrived fewer than ten years prior were, after enfranchisement, 5.1 percentage points more likely to complete citizenship than comparable immigrants who had resided in the United States for ten to twenty years. This is a substantial effect considering that, in the sample, only 13.7% of immigrants with ten or fewer years of residence, and 35.5% of all immigrants, had completed citizenship proceedings.

This result is generally consistent with Shertzer (2016), who argues that naturalizations are a valid proxy for immigrant voting behavior at the time. Moreover, it suggests that the disenfranchisement could have positively affected labor market outcomes because of the signal implicit in naturalizations.

c. Policy Responses

A large body of literature has established the importance of political regime and individual policies for labor market outcomes and determinants of labor market outcomes. Recent research shows that the election of Democratic governors reduced the racial wage gap in the late 20th century and increased the working hours of immigrants (Beland, forthcoming;

Beland and Unel 2015). A related literature has established that establishment of government policies from the mid-twentieth century such as Food Stamps and Medicaid improved health and human capital for exposed children (Almond et al 2011; Goodman-Bacon, 2015), and, while government transfer programs were rare in the early 20th century, there is evidence that receipt of Union Army pensions had significant and positive health and income effects for exposed children (Aizer et al, 2016). Furthermore, during this era, local governments invested heavily in public goods such as public health (Cutler and Miller 2005; Cain and Rotella 2001) and education (Goldin and Katz 1999, Kose et al 2015), with positive effects on health outcomes and education, respectively, for exposed cohorts.

Scholars have also demonstrated the link between enfranchisement and allocation of public resources. Miller (2008) studies women's enfranchisement in the Untied States and find that it led to significant state-level increases in social spending and large municipal-level increases in health-related spending. Miller also documents that legislators became more liberal in their voting patterns, a finding corroborated by Lott and Kenny (1999). Aidt and Dallal (2008) estimate the effects of women's enfranchisement in six European countries in the 20th century, and find that it led to an immediate increase of approximately 1% on social spending, with the effect growing over time. Research has also demonstrated that disenfranchisement of African-Americans in the south was associated with substantial reductions in education spending (Margo 1982, Kousser 1980, Pritchett 1989, Naidu 2012).

To test for policy channels, I estimate event-study models on the following outcomes: share voting democratic in gubernatorial elections, share voting democratic in mayoral elections fraction of seats held by democrats in upper and lower chambers of state legislatures (all presented in figure 8)a); and state-level total spending and budget shares devoted to education, transportation, and social services (all presented in figure 8)b). The event study specifications include state-fixed effects, year fixed effects, and state-specific linear trends.

The results almost uniformly indicate a lack of effect of immigrant disenfranchisement on these outcomes. There is some evidence that disenfranchisement shifted the composition of lower chambers of statehouses away from the Democratic Party, but without an accompanying shift in the composition of the upper statehouse, it is unlikely that this resulted in altered policy. More significant is the effect of disenfranchisement on the share of state spending devoted to education: it drops by 2.4 percentage points in the two years following repeal, increasing to 5.2-7.3 percentage points for at least ten years following repeal. Given that I control for years of education in the long-run specification above, this spending effect does not operate through reduced educational attainment. Instead, it is possible that it caused a reduction in the quality of education, or caused a shift in school attendance from public to parochial schools, which may have been of lower quality.

Section 6: Robustness Checks

I have addressed the timing of repeal of alien voting – it appears to have been driven in large part by the ease with which states can amend their constitutions, which varies from state to state for historical reasons – and my specifications control for smoothly evolving state-specific unobserved factors which may have influenced the timing of repeal. However, concern remains as to the exogeneity of the timing of repeal: if these provisions were repealed for reasons that are correlated with adverse labor market outcomes for immigrants or their children, then the observed intergenerational effects may be driven by omitted variables.

As a final check, I exploit the fact that the final step in the amending of a state constitution is public ratification in a general election. I have collected the vote totals for the ratifications for thirteen of the fourteen states that repealed their provisions via state constitution. See table 6 for the election totals. The margins of victory represent a proxy for the anti-immigrant sentiment prevailing at the time: those states that repealed their provisions by a wide margin may have been more likely to have had unobserved factors affecting outcomes for immigrants. I repeat specifications (2) and (3) using only those states that had a below-median margin of referendum victory as "treatment" states (AR, CO, IN, MO, SD, TX). In the sense that outcomes of close elections are relatively more susceptible to randomness, the timing of disenfranchisement in these states is more plausibly exogenous.

Results are presented in tables 7 and 8. While the estimates are somewhat less precise owing to the reduced sample size, the results largely mirror those from the baseline estimates: little evidence of short-run labor market effects on employment status or occupational standing and negative effects on public sector employment, and some evidence that exposure to parental disenfranchisement reduced long-run income. There are, however, two differences. First, in this sub-sample of states, the effect of disenfranchisement on labor force participation is a statistically significant .032, whereas, in the baseline specification, it is a statistically insignificant -.002. This may reflect the fact that five of the six states in this sample repealed their provisions after 1918; given the immigration restrictions introduced in 1921 and 1924, explicitly intended to alter the composition of immigrants in favor of Northwest Europeans, recent immigrants in these states may have been considered more employable. Second, the point estimates of event-time indicators in the intergenerational specification differ somewhat from those in the baseline specification. Again, this may be a result of the imposition of the restrictive immigration quotas: recent immigrants, after the quotas were imposed, would have been far more likely to hail from Northwest Europe, and thus, perhaps with greater English fluency or facing less racial animus, better able to manage the shock of disenfranchisement. Furthermore, these differing point estimates may reflect statistical noise: their 95% confidence intervals have considerable overlap.

While the significant variation in repeal dates implies that is it unlikely that coincidental policies or changes drive the observed results, two significant institutional changes occurred during this era that, if overlapping with the timing of immigrant disenfranchisement, could bias my results: women's suffrage, starting in 1869, and compulsory education laws, starting in 1852. As a final check, I assess the degree to which the timing of repeal of alien voting is correlated with both. I find that the degree of correlation is very low: .246 for women's suffrage, and .248 for compulsory education laws.

Section 7. Conclusion.

This paper presents novel evidence on the effects of immigrant disenfranchisement on contemporaneous and intergenerational labor market outcomes using a little-known set of state-level constitutional changes that disenfranchised non-citizen immigrants in twenty-three states and territories from 1864-1926. Using a triple-difference specification comparing outcomes for recent and less recent immigrants, I find that disenfranchisement did not significantly affect labor force participation, employment status conditional on labor force participation, or occupational standing for disenfranchised immigrants relative to non-disenfranchised immigrants. Disenfranchised immigrants were, however, 45-54% less likely to be employed in public sector employment following disenfranchisement.

I also document evidence of adverse intergenerational consequences of disenfranchisement: the income differential for children of natives and children of immigrant fathers, not statistically distinguishable from zero for cohorts of children born more than five years before repeal of alien voting, grew in magnitude to a statistically significant -6%, with the effect lasting for up to twenty years after repeal. I explore several mechanisms for this and document two contrasting channels: states significantly reduced the shares of their budgets devoted toward educational spending in response to disenfranchisement, which may have affected the quality of education received, and therefore the human capital development of children of the disenfranchised. However, disenfranchisement also spurred immigrants with a high valuation of the right to vote to complete citizenship, which may have served as a signal of civic mindedness to employers. This, in turn, could have improved the contemporaneous labor market outcomes of disenfranchised immigrants and, to the extent that occupation mobility was not perfect in this era, and that of the children.

These contrasting mechanisms may serve to rationalize the contrasting results from the literature on democratization and economic growth. The findings of this literature are mixed, with certain authors documenting no effect of democracy on growth (Gerring et al 2005), and others finding a positive effect (Acemoglu et al 2014). The results presented in this paper imply that the connection between political participation and economic outcomes is not straightforward: different channels may predominate in different settings. This, in turn, suggests that analyses based on cross-country data may, depending on the sample, period, and specification, capture differing mechanisms.

More broadly, these results suggest that increasing the costs of voting can have significant and persistent long-term economic effects for affected populations. Policy-makers considering measures that restrict access to the ballot box would be wise to consider the lessons of history.

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Figure 1: Dates of Repeal of Alien Voting

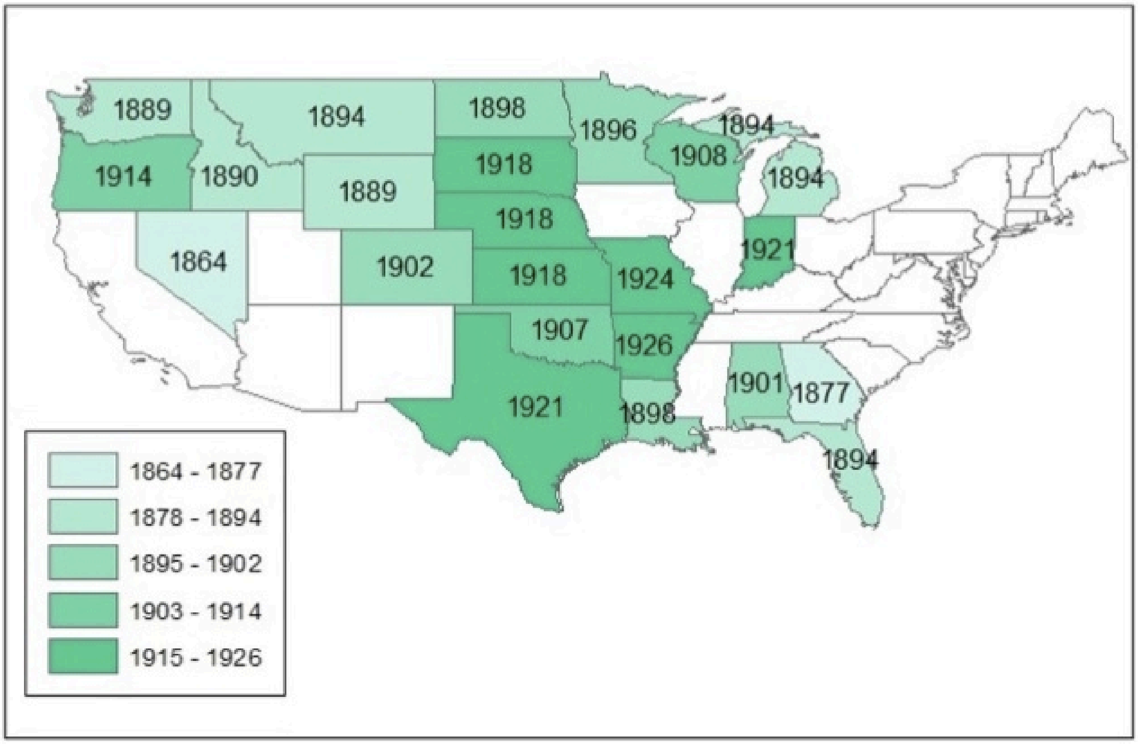

Figure 2 – Scatterplot of Possible Correlates with Timing of Repeal

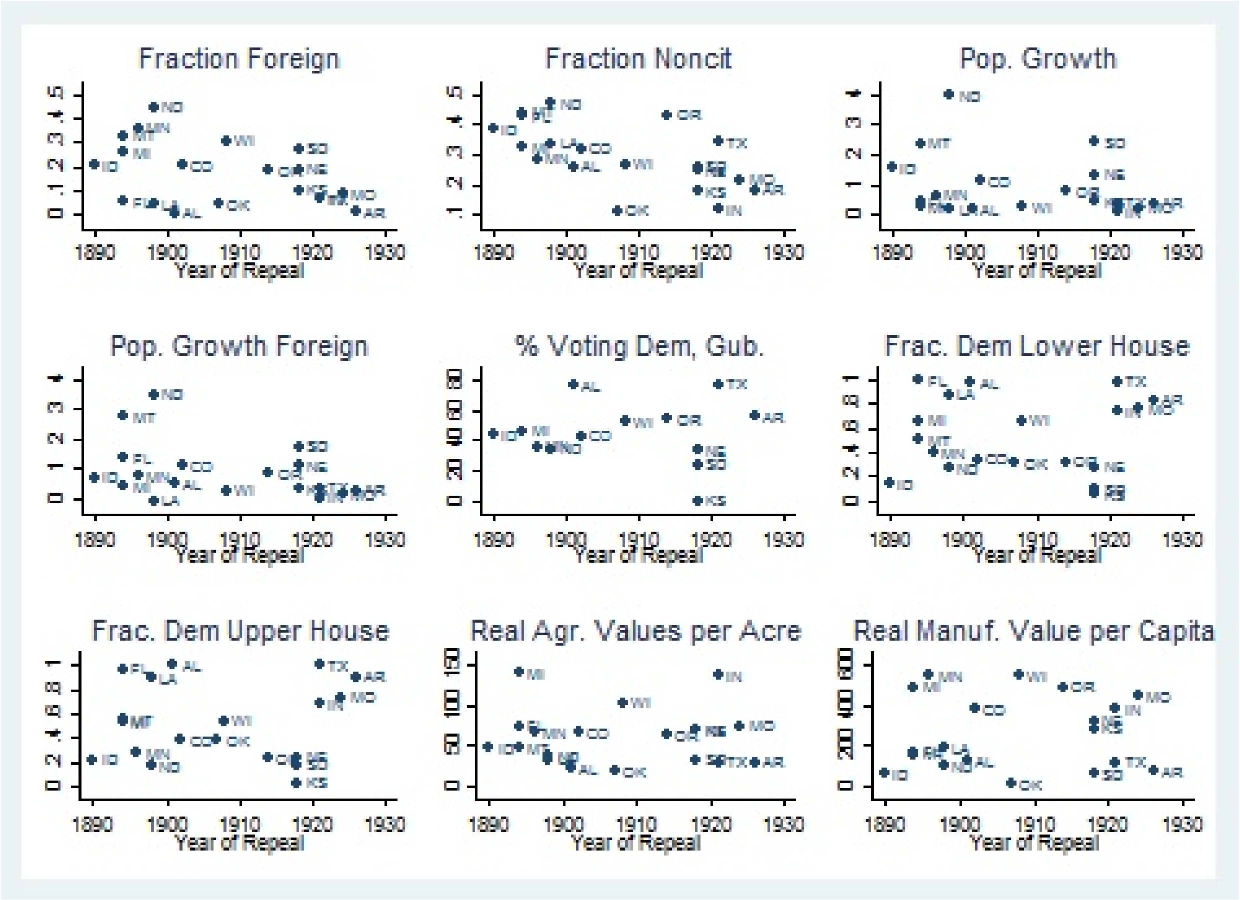

Figure 3: Effect of Disenfranchisement on Mayoral Voter Turnout

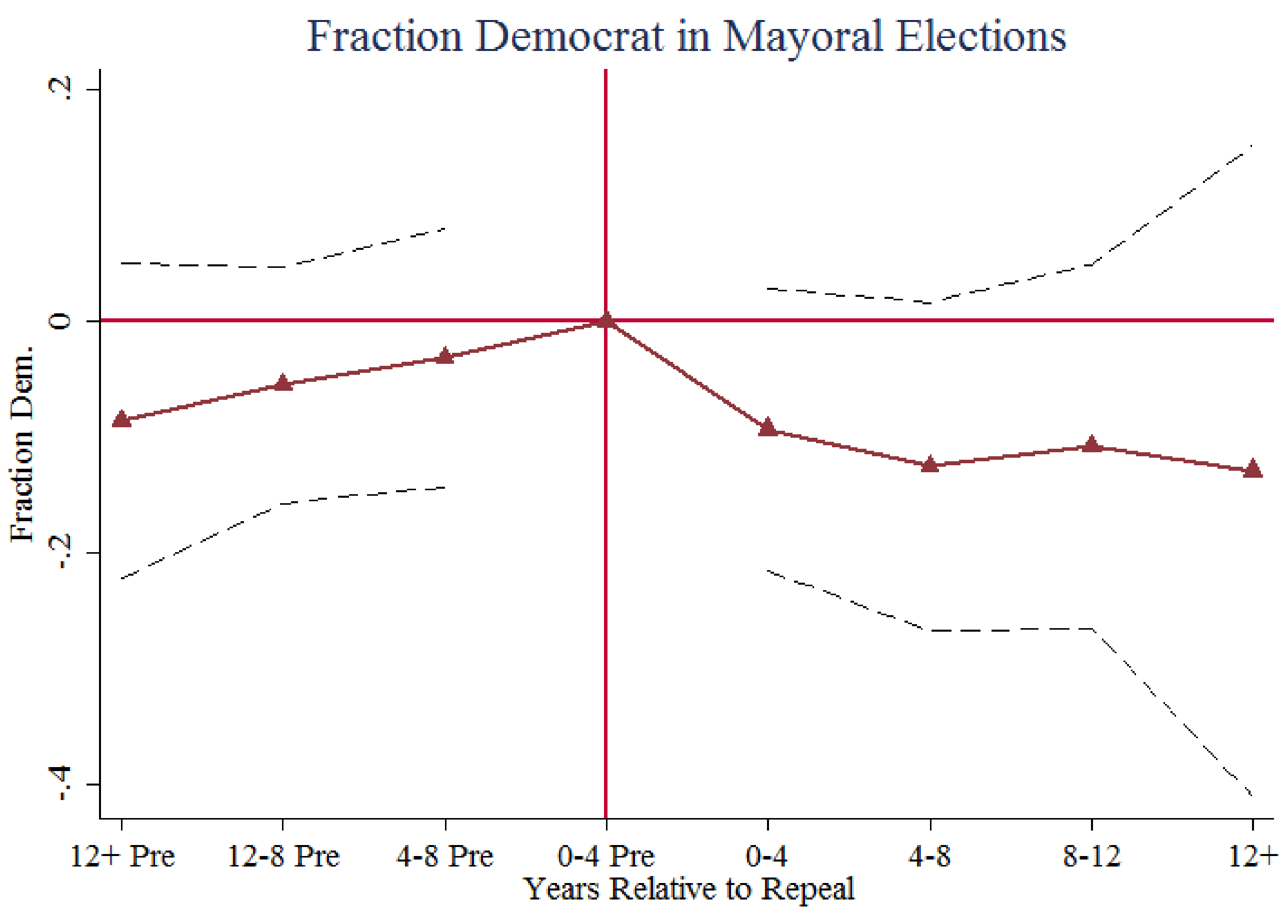

Figure 4: Effect of Immigrant Disenfranchisement on Gubernatorial Voter Turnout

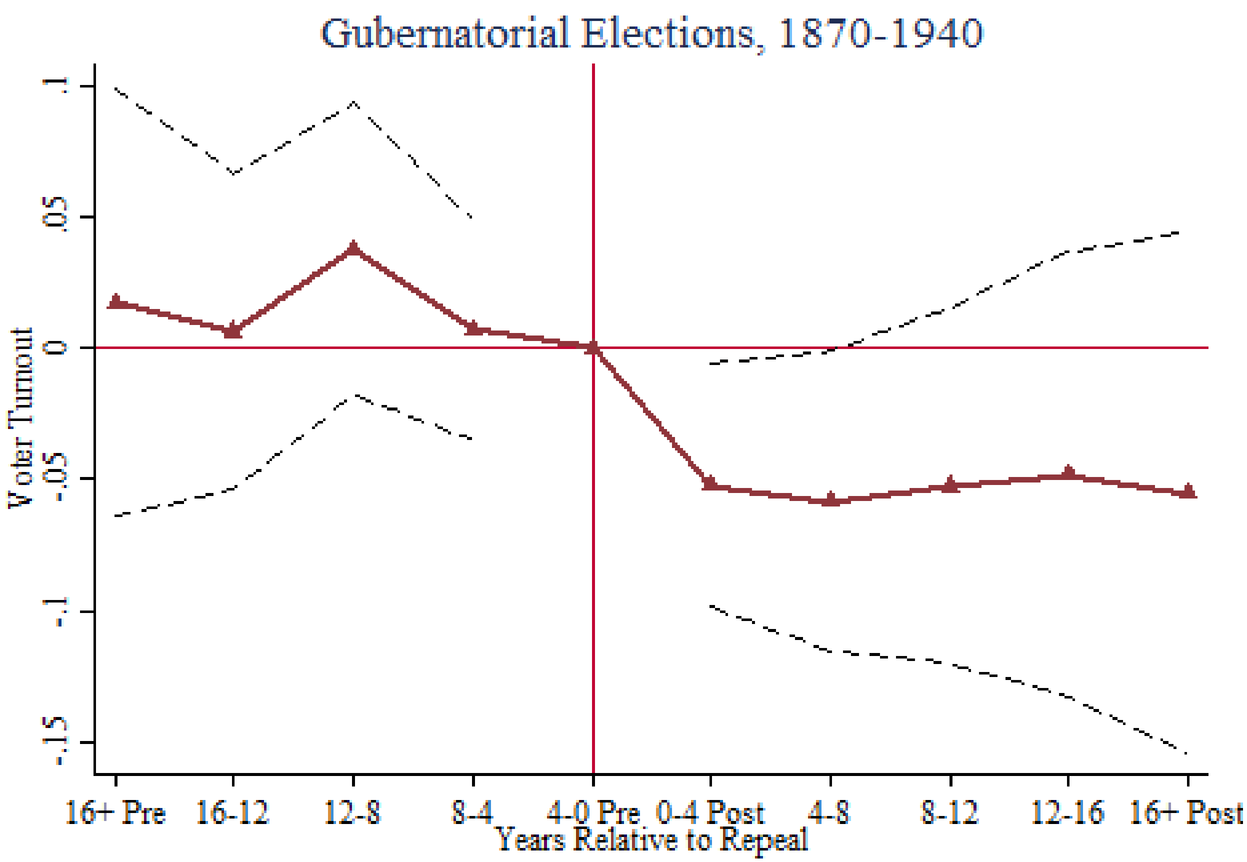

Figure 5: Intergenerational Effect of Parental Disenfranchisement on Labor Force Participation

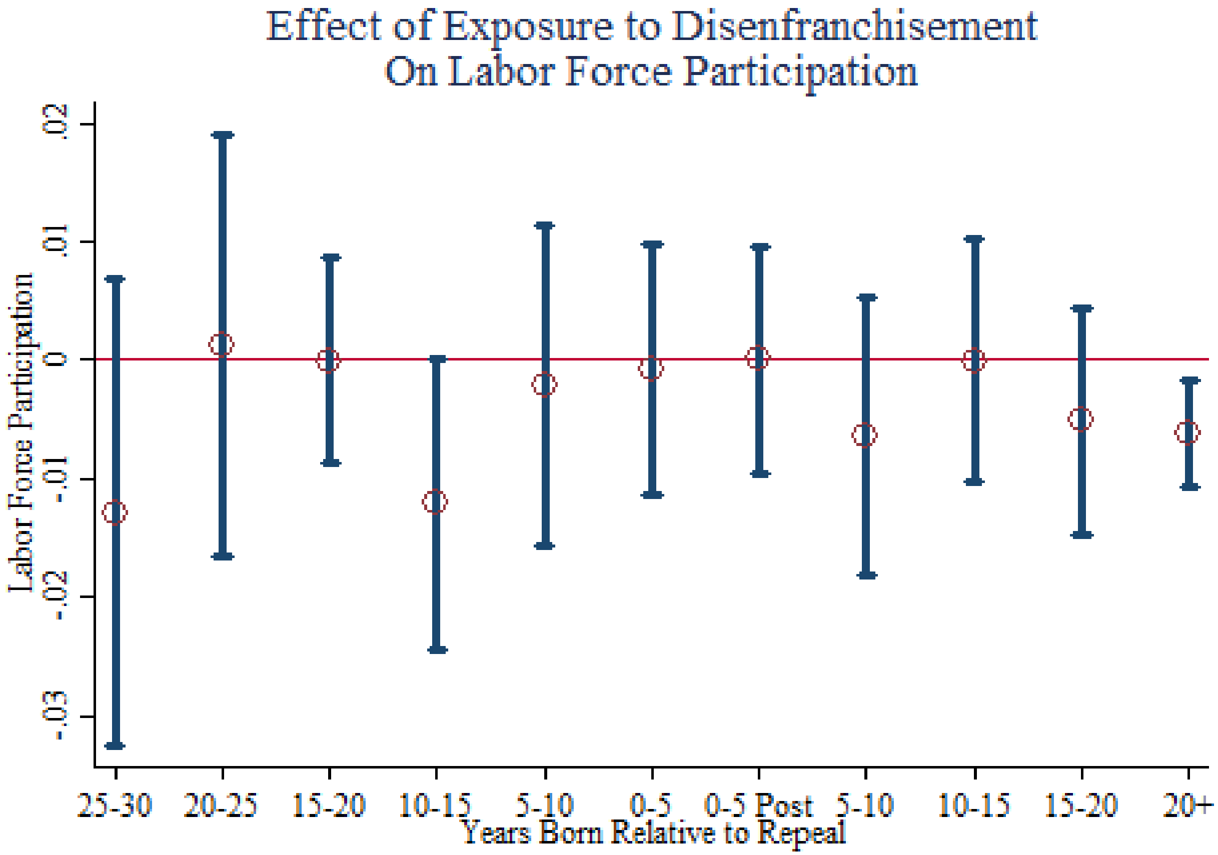

Figure 6: Intergenerational Effect of Parental Disenfranchisement on Employment Status

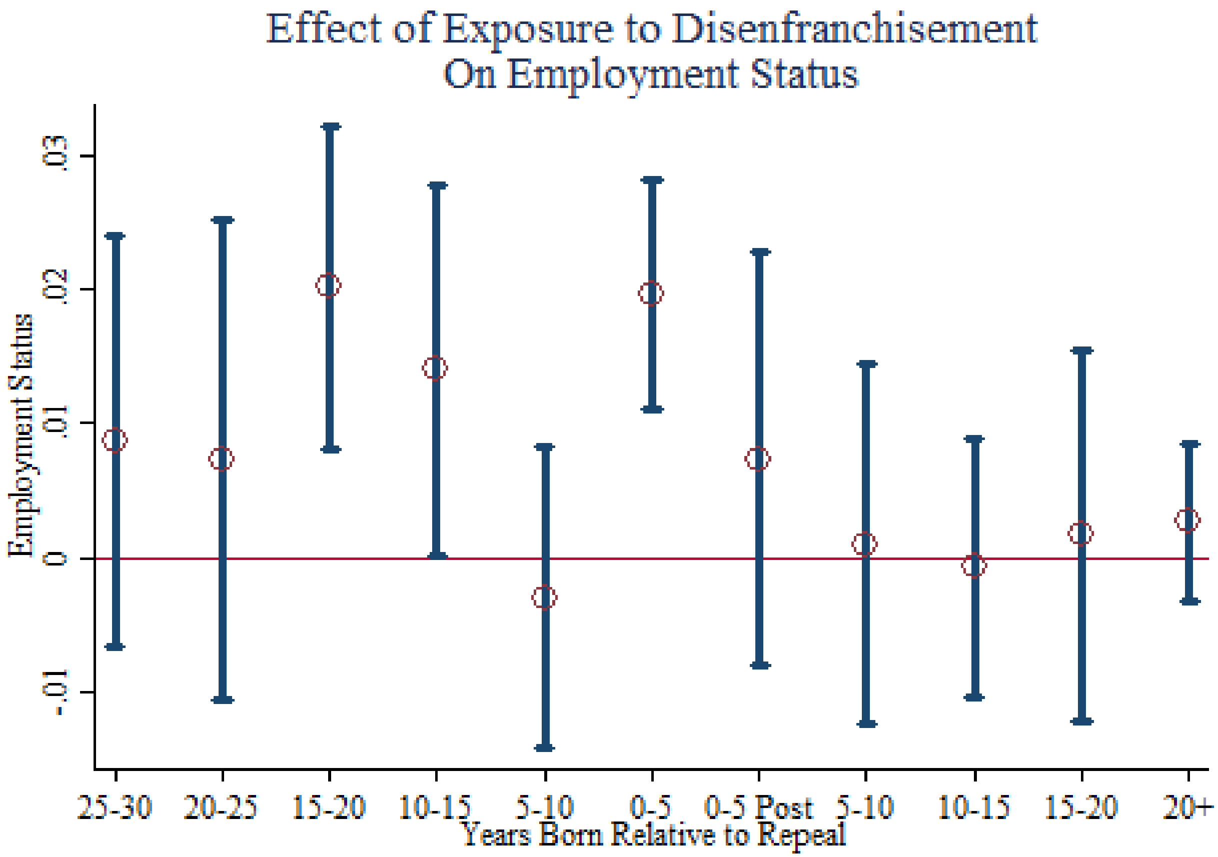

Figure 7: Intergenerational Effect of Parental Disenfranchisement on Income

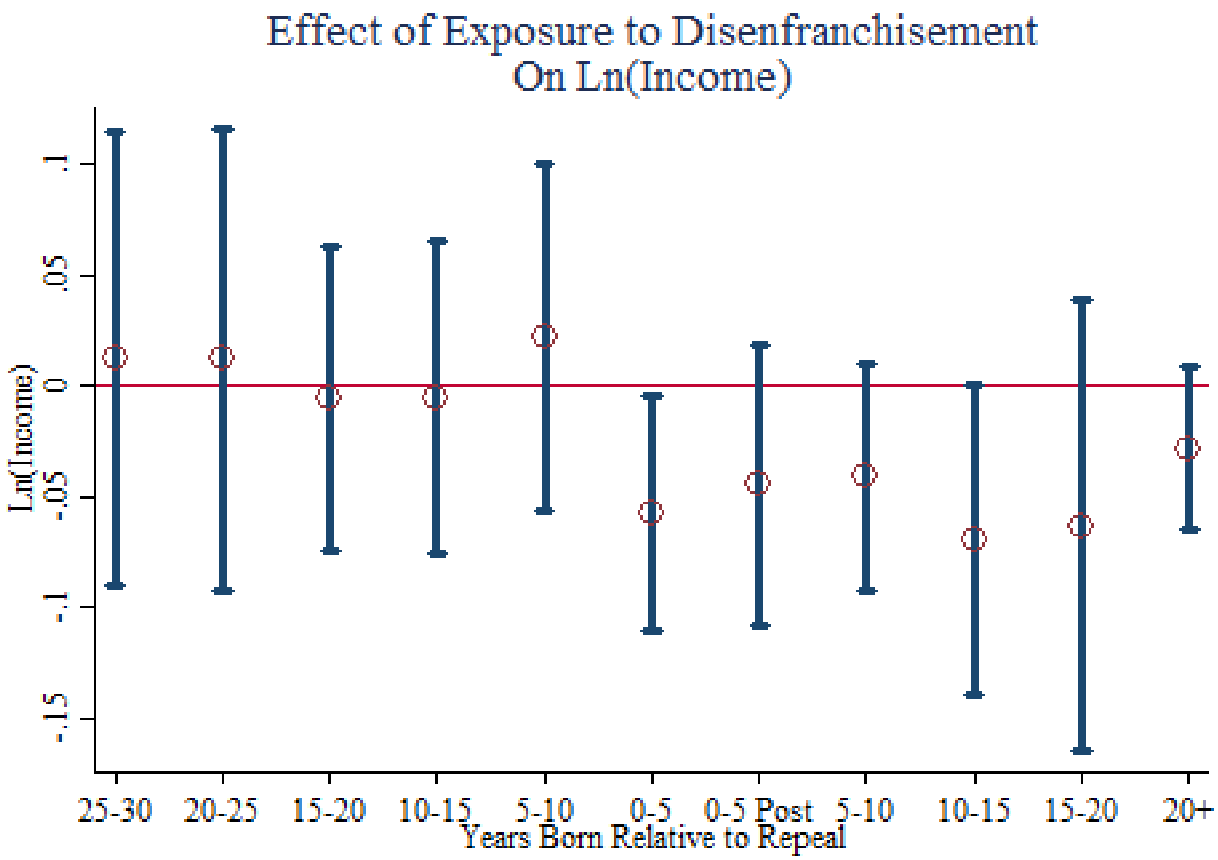

Figure 8)a): Political Channels

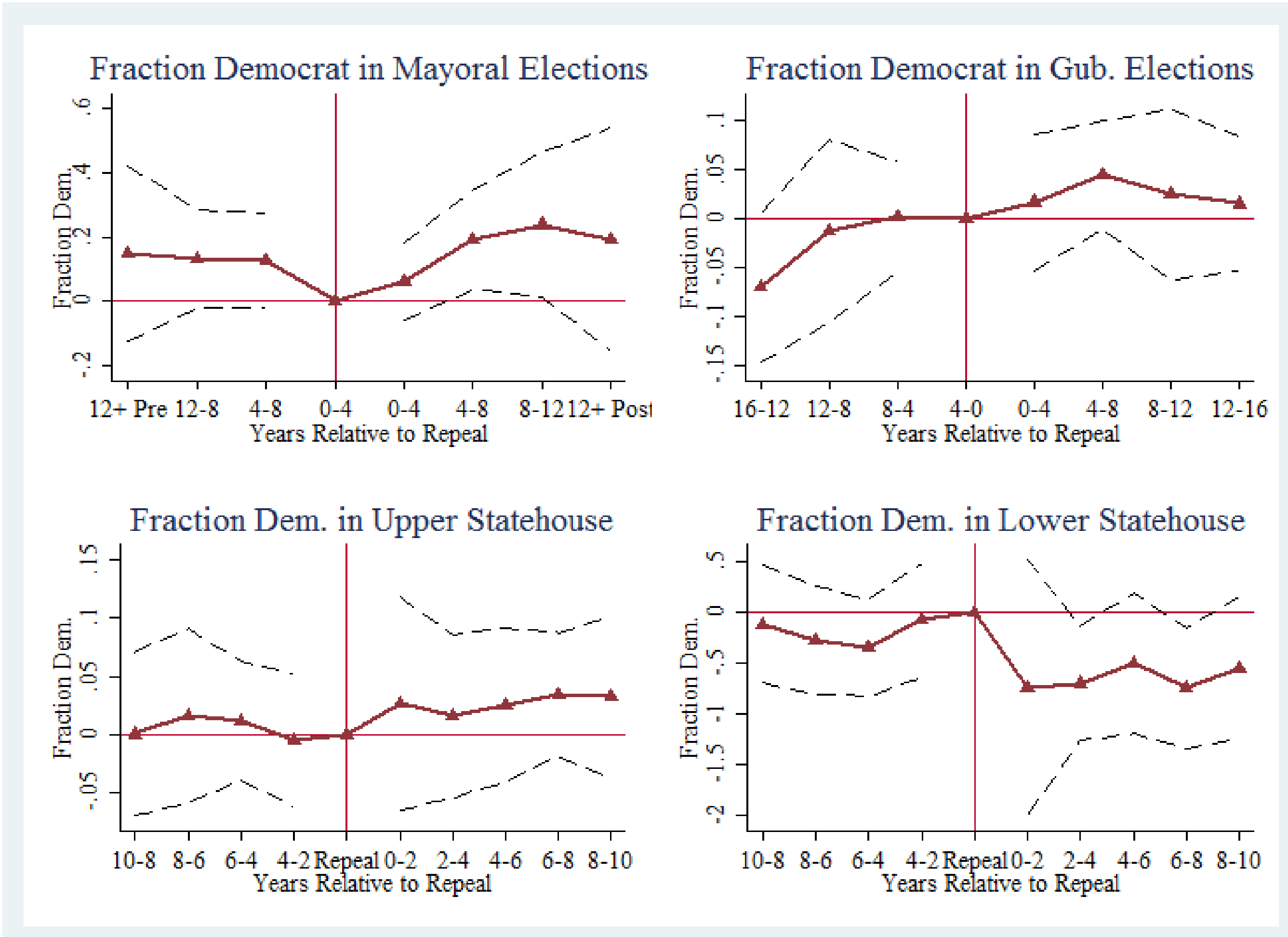

Figure 8)b): State Expenditure Channels

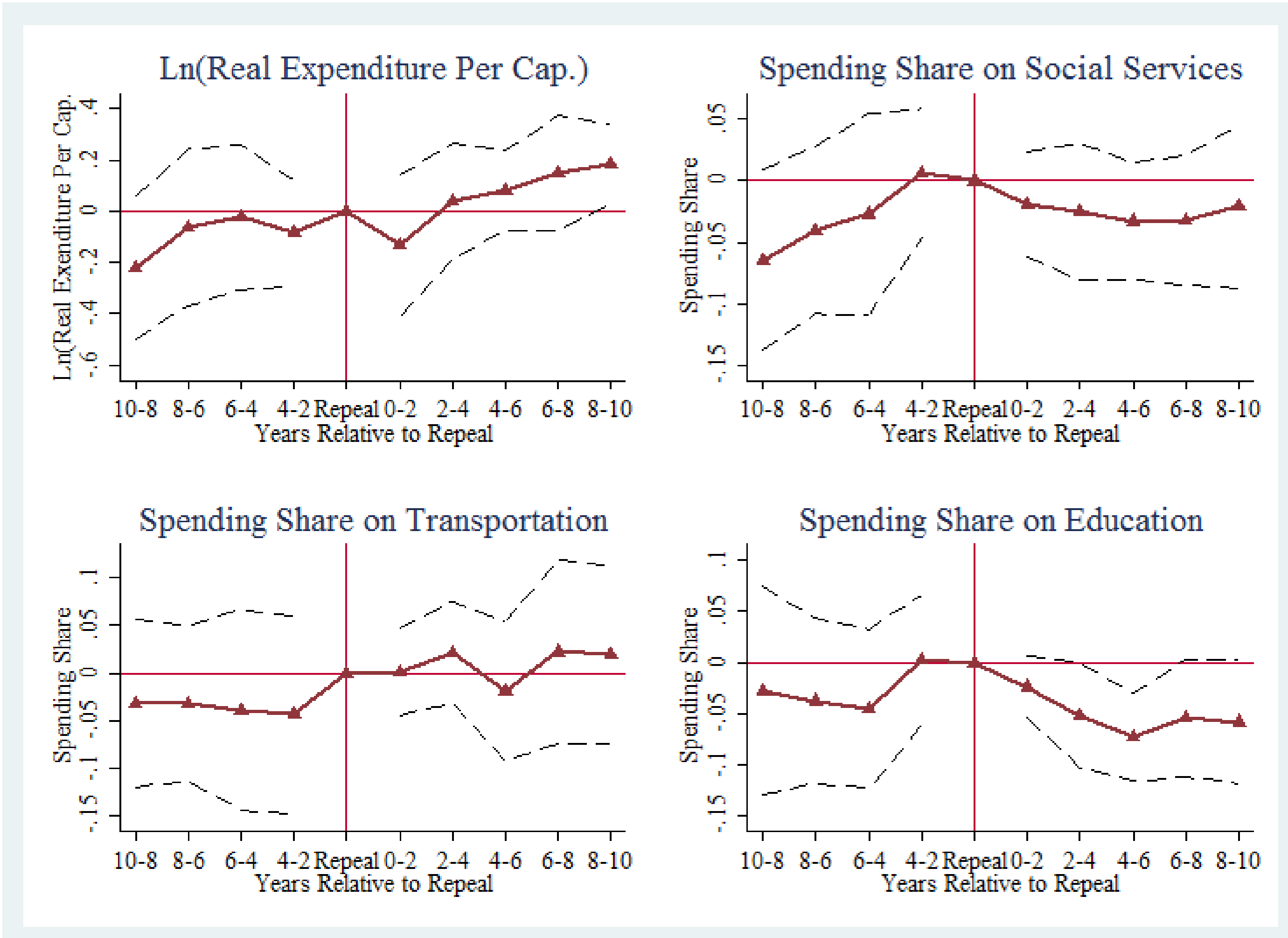

Table 1: Determinants of Year of Disenfranchisement

	Year Repeal	Year Repeal	Year Repeal
Fraction Foreign-Born	-12.983		-24.309
	(21.175)		(22.492)
Fraction Adult Foreign-Born Males Noncitizen	-60.726		-39.059
	(27.620)**		(28.029)
Repealed via Amendment		22.917	20.566
		(4.994)***	(3.315)***
Loose Repeal Standard		-3.333	4.353
		(4.729)	(4.931)
Loose Repeal Standard * Repealed via Amendment		-12.617	-11.518
		(5.900)**	(5.662)**
R^2	0.38	0.52	0.68
N	19	19	19

State level aggregates are from published Census aggregates from 1890, and Haines (2010). Standard errors have been bootstrapped (n = 500). * p<0.1; ** p<0.05; *** p<0.01

Table 2: Short-run Effects of Disenfranchisement on Labor Market Outcomes

	LFP	Employed?	Occupational Standing	Public Sector 1	Public Sector 2	Public Sector 3
Repeal	0.002	0.018	1.112	-0.003	-0.003	-0.002
	(0.009)	(0.007)**	(0.197)***	(0.003)	(0.003)	(0.002)
Resident < 5 years	-0.001	-0.107	-0.095	0.001	-0.001	0.001
	(0.003)	(0.010)***	(0.185)	(0.001)	(0.002)	(0.001)
Repeal * Resident < 5 years	-0.002	-0.003	-0.147	-0.003	-0.001	-0.002
	(0.009)	(0.013)	(0.258)	(0.001)**	(0.002)	(0.001)*
R^2	0.02	0.03	0.06	0.01	0.01	0.01
N	172,913	75,350	172,913	172,913	172,913	172,913

Notes: Data are from IPUMS Census samples, 1900-1930. Sample is restricted to non-farm male immigrants age 21-64 (inclusive) who have resided in the United States for 15 years or fewer. Individual level controls include age, age squared, literacy status, urban status, and indicators for year of immigration and birth region. State-level controls are an indicator for the pre-repeal period in alien voting states, and an indicator for the presence of state regulation barring aliens from public employment (Fishback et al 2009). "Public Sector 1", "Public Sector 2", and "Public Sector 3" refer to different methods of coding public sector employment. See appendix 2 for details. The sample does not include those states that had repealed alien voting prior to 1900: FL, GA, LA, MN, MI, WA, MT, ID, WY, ND, and NV. All specifications include state fixed effects, year fixed effects, state-specific linear trends, and region by decade fixed effects. IPUMS person weights are used in estimation. * p<0.1; ** p<0.05; *** p<0.01

Table 3: Placebo Check for Spillover of Disenfranchisement Effects

	LFP	Employed?	Occupational Standing	Public Sector 1	Public Sector 2	Public Sector 3
Repeal	0.002	0.003	0.317	0.002	-0.001	0.001
	(0.006)	(0.001)***	(0.218)	(0.002)	(0.002)	(0.001)
R^2	0.02	0.01	0.06	0.01	0.01	0.00
N	1,529,342	938,681	1,529,342	1,529,342	1,529,342	1,529,342

Notes: Data are from IPUMS Census samples, 1900-1930. Sample is restricted to non-farm male natives age 21-64 (inclusive). Individual level controls include age, age squared, literacy status, and urban status. State-level controls are an indicator for the pre-repeal period in alien voting states, and an indicator for the presence of state regulation barring aliens from public employment (Fishback et al 2009). "Public Sector 1", "Public Sector 2", and "Public Sector 3" refer to different methods of coding public sector employment. See appendix 2 for details. The sample does not include those states that had repealed alien voting prior to 1900: FL, GA, LA, MN, MI, WA, MT, ID, WY, ND, and NV. All specifications include state fixed effects, year fixed effects, state-specific linear trends, and region by decade fixed effects. IPUMS person weights are used in estimation. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table 4: Migration Response to Disenfranchisement, 1870-1940

	Ln(Total Pop)	Ln(Total Foreign Born Pop)	Fraction Foreign Born	Fraction Foreign Born Illiterate
More than 30 years pre repeal	0.019	-0.077	-0.047	0.013
	(0.272)	(0.426)	(0.049)	(0.029)
20-30 years pre-repeal	0.187	0.163	-0.011	0.028
	(0.253)	(0.319)	(0.018)	(0.026)
10-20 years pre-repeal	0.034	0.048	-0.007	0.011
	(0.126)	(0.169)	(0.014)	(0.026)
0-10 years post-repeal	-0.042	-0.064	0.009	-0.010
	(0.137)	(0.177)	(0.010)	(0.010)
10-20 years post-repeal	-0.275	-0.317	0.019	-0.008
	(0.218)	(0.282)	(0.016)	(0.020)
20-30 years post-repeal	-0.604	-0.693	0.027	-0.017
	(0.326)*	(0.410)*	(0.024)	(0.021)
30 +	-1.133	-1.185	0.042	-0.008
	(0.435)**	(0.545)**	(0.028)	(0.031)
R^2	0.97	0.95	0.91	0.76
N	479	479	479	479

Data are from IPUMS samples and published Census aggregates from 1890. All specifications include state fixed effects, decade fixed effects, and state-specific linear trends. Standard errors are clustered at the state level.* p<0.1; ** p<0.05; *** p<0.01

Table 5: The Effect of Disenfranchisement on Naturalization of Disenfranchised Immigrants

	Naturalized?	Naturalized?	Naturalized?	Naturalized?
Repeal	0.057	0.048	0.020	0.007
	(0.043)	(0.059)	(0.058)	(0.059)
Resident < 10 years	-0.326	-0.078	-0.079	-0.079
	(0.011)***	(0.006)***	(0.006)***	(0.006)***
Repeal * Resident < 10 years	0.015	0.045	0.052	0.051
	(0.016)	(0.017)**	(0.019)***	(0.019)***
State-level controls	Y	Y	Y	Y
State and Year Fixed Effects	N	Y	Y	Y
State-specific linear trends	N	N	Y	Y
Region-decade Fixed Effects	N	N	N	Y
R^2	0.27	0.29	0.29	0.29
N	269,600	269,600	269,600	269,600

Notes: Data are from IPUMS Census samples, 1900-1930. Sample is restricted to non-farm male immigrants age 21-64 (inclusive) who have resided in the United States for 15 years or fewer. Individual level controls include age, age squared, literacy status, urban status, and indicators for year of immigration and birth region. State-level controls are an indictor for the pre-repeal period in alien voting states, and an indictor for the presence of state regulation barring aliens from public employment (Fishback et al 2009). The sample does not include those states that had repealed alien voting prior to 1900: FL, GA, LA, MN, MI, WA, MT, ID, WY, ND, and NV. All specifications include state fixed effects, year fixed effects, state-specific linear trends, and region by decade fixed effects. IPUMS person weights are used in estimation.
* p<0.1; ** p<0.05; *** p<0.01

Table 6 – Constitutional Amendment Elections Totals

State	Year	Result	Winning Margin (%)
AR	1926	87237-49757	27.4
CO	1902	44769-27077	24.6
FL	1894	11691-5664	34.7
IN	1921	130242–80574	23.6
KS	1918	238453-91617	44.5
MI	1894	117088-31537	57.6
MN	1896	97980-52454	30.3
MO	1924	175580-152713	7.0
NE	1918	123292–51600	41.0
OR	1914	164879-39847	61.1
SD	1918	49318-28394	26.9
TX	1921	57622-53910	3.3
WI	1908	86576-36733	40.4

Notes: these figures come from state blue books, legislative manuals, reports from the Secretary of State, and Aylsworth (1931). Details sources available upon request.

Table 7: Robustness Check on Short-run effects of Disenfranchisement on Labor Market Outcomes

	LFP	Employed?	Occupational Standing	Public Sector 1	Public Sector 2	Public Sector 3
Repeal	0.013	-0.092	1.312	-0.001	-0.002	-0.002
	(0.015)	(0.006)***	(0.366)***	(0.005)	(0.005)	(0.003)
Resident < 5 years	-0.002	-0.102	-0.158	0.001	-0.001	0.001
	(0.002)	(0.007)***	(0.185)	(0.001)	(0.002)	(0.001)
Repeal * Resident < 5 years	0.032	0.007	-0.364	-0.002	-0.008	-0.004
	(0.005)***	(0.006)	(0.494)	(0.004)	(0.004)**	(0.001)***
R^2	0.02	0.03	0.05	0.01	0.01	0.01
N	165,014	72,499	165,014	165,014	165,014	165,014

Notes: Data are from IPUMS Census samples, 1900-1930. Sample is restricted to non-farm male immigrants age 21-64 (inclusive). Individual level controls include age, age squared, literacy status, urban status, and indicators for year of immigration and birth region. State-level controls are an indictor for the pre-repeal period in alien voting states, and an indictor for the presence of state regulation barring aliens from public employment (Fishback et al 2009). The sample does not include those states which had repealed alien voting prior to 1900: FL, GA, LA, MN, MI, WA, MT, ID, WY, ND, and NV. All specifications include state fixed effects, year fixed effects, state-specific linear trends, and region by decade fixed effects. IPUMS person weights are used in estimation. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table 8: Robustness Check on Intergenerational Effects of Disenfranchisement

	Labor Force Participation	Employment Status	Ln(Real Income)
Age at Repeal 25-30	-0.027	0.002	0.074
	(0.021)	(0.013)	(0.091)
Age at Repeal 20-25	0.013	-0.015	-0.061
	(0.006)**	(0.010)	(0.071)
Age at Repeal 15-20	0.005	0.012	0.052
	(0.008)	(0.009)	(0.087)
Age at Repeal 10-15	-0.008	0.012	0.091
	(0.010)	(0.016)	(0.051)*
Age at Repeal 5-10	0.004	0.012	0.060
	(0.013)	(0.004)**	(0.049)
Age at Repeal 0-5	0.002	0.022	0.021
	(0.010)	(0.006)***	(0.049)
Age at Repeal -5-0	0.013	0.013	0.054
	(0.011)	(0.008)	(0.055)
Age at Repeal -10--5	0.010	-0.003	-0.008
	(0.010)	(0.009)	(0.037)
Age at Repeal -15--10	0.008	-0.005	-0.044
	(0.007)	(0.010)	(0.041)

Age at Repeal -20--15	0.016	0.014	-0.090
	(0.007)**	(0.005)***	(0.045)*
Age at Repeal less than -20	-0.035	0.014	-0.133
	(0.023)	(0.005)**	(0.060)**
R^2	1,411,112	1,309,642	1,130,960
N	0.05	0.02	0.20

Data are from IPUMS samples from 1940-1960. Sample is restricted to native males aged 25-64 (inclusive). Individual level controls include age, age squared, educational attainment, race indicators, father nativity, and father birth region indicators. All specifications include birthplace fixed effects, birth cohort fixed effects, birth state-specific linear birth cohort trends, and survey year fixed effects. IPUMS sample line weights are used in estimation. Standard errors are clustered at the birth state level.* $p<0.1$; ** $p<0.05$; *** $p<0.01$

Appendix Table 1a: Effect of Disenfranchisement on Mayoral Voter Turnout, 1880-1924

	Voter Turnout
12+ Years Pre	-0.086
	(0.070)
8-12 Years Pre	-0.055
	(0.052)
4-8 Years Pre	-0.031
	(0.057)
0-4 Years Post	-0.094
	(0.062)
4-8 Years Post	-0.126
	(0.073)*
8-12 Years Post	-0.108
	(0.080)
12+ Years Post	-0.129
	(0.144)
R^2	0.84
N	175

The sample includes mayoral election data from eleven cities from 1880-1924: Milwaukee, Detroit, New Orleans, St. Louis, Baltimore, Buffalo, Cincinnati, Cleveland, Chicago, New York, and Pittsburgh (Holli and Jones 1981). . Population estimates of males age 21 and over were linearly interpolated from decadal IPUMS totals. The specification includes city fixed effects, year, fixed effects, city-specific linear trends, and an indicator for whether woman's suffrage is in effect. * p<0.1; ** p<0.05; *** p<0.01

Appendix Table 1b: Effect of Disenfranchisement on Gubernatorial Voter Turnout, 1870-1940

	Voter Turnout
16+ Years Pre	0.017
	(0.042)
12-16 Years Pre	0.006
	(0.031)
8-12 Years Pre	0.038
	(0.028)
4-8 Years Pre	0.007
	(0.021)
0-4 Years Post	-0.052
	(0.024)**
4-8 Years Post	-0.058
	(0.029)*
8-12 Years Post	-0.052
	(0.035)
12-16 Years Post	-0.048
	(0.043)
16+ Years Post	-0.055
	(0.051)

R^2	0.93
N	1,235

Gubernatorial election data is from Burham et al, 1991. Population estimates of males age 21 and over were linearly interpolated from decadal IPUMS totals. The specification includes state fixed effects, yea, fixed effects, state-specific linear trends, and an indicator for whether woman's suffrage is in effect. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Appendix Table 2: Government Employment Codes

Coding 1. Government job indicator equals 1 if occupation equals:

1900	1910 and 1920	1930
Clerks in government offices	Firemen – fire department	Firemen – fire department
Employees of government (not clerks)	Guards, watchmen, and doorkeepers	Guards, watchmen, and doorkeepers
Officials of Government	Garbagemen and scavengers	Garbagemen and scavengers
	Other laborers (public service)	Detectives
	Detectives	Marshals and constables
	Marshals and Constables	Probation and truant officers
	Probation and truant officers	Sheriffs
	Sheriffs	Officials and inspectors – city
	Officials and inspectors – city	Officials and inspectors – county
	Officials and inspectors – county	Officials and inspectors – state
	Officials and inspectors – state	Officials and inspectors – United States
	Postmasters	Policemen
	Other United States officials	Other public service pursuits
	Policemen	
	Life-Savers	

Lighthouse keepers

Other occupations (public service)

Government employment is classified from IPUMS occupation codes.
1900: 24, 31, 48.
1910 and 1920: 800-820 and 824-826.
1930: 178-189, 191.

Coding 2: Government Job indicator equals 1 if IND1950 ("Industry, 1950 basis") equals:

"Postal Service", "Federal Public Administration", "State public administration", "local public administration", and "Public Administration, level not specified"

All instances when the occupation string indicated a military position (for example, "Soldier", "Sailor", "U S Navy") were excluded from this definition.

Coding 3: Government Job indicator equals 1 if IND1950 ("Industry, 1950 basis") equals:

"Local Public Administration"

Appendix Table 3: Event study models of intergenerational labor market effects of parental disenfranchisement

	Labor Force Participation	Employment Status	Ln(Real Income)
Age at Repeal 25-30	-0.013	0.009	0.012
	(0.010)	(0.008)	(0.052)
Age at Repeal 20-25	0.001	0.007	0.012
	(0.009)	(0.009)	(0.053)
Age at Repeal 15-20	-0.000	0.020	-0.006
	(0.004)	(0.006)***	(0.035)
Age at Repeal 10-15	-0.012	0.014	-0.005
	(0.006)*	(0.007)*	(0.036)
Age at Repeal 5-10	-0.002	-0.003	0.022
	(0.007)	(0.006)	(0.040)
Age at Repeal 0-5	-0.001	0.020	-0.058
	(0.005)	(0.004)***	(0.027)**
Age at Repeal -5-0	-0.000	0.007	-0.045
	(0.005)	(0.008)	(0.032)
Age at Repeal -10--5	-0.006	0.001	-0.041
	(0.006)	(0.007)	(0.026)
Age at Repeal -15--10	-0.000	-0.001	-0.070
	(0.005)	(0.005)	(0.036)*

Age at Repeal -20--15	-0.005	0.002	-0.063
	(0.005)	(0.007)	(0.052)
Age at Repeal less than -20	-0.006	0.003	-0.028
	(0.002)***	(0.003)	(0.019)
R^2	0.04	0.02	0.20
N	1,972,901	1,830,910	1,571,902

Data are from IPUMS samples from 1940-1960. Sample is restricted to native males aged 25-64 (inclusive). Individual level controls include age, age squared, educational attainment, race indicators, father nativity, and father birth region indicators. All specifications include birthplace fixed effects, birth cohort fixed effects, birth state-specific linear birth cohort trends, and survey year fixed effects. IPUMS sample line weights are used in estimation. Standard errors are clustered at the birth state level.* p<0.1; ** p<0.05; *** p<0.01