Women's Enfranchisement and Children's Education: the Long-Run Impact of the U.S. Suffrage Movement

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Abstract

While a growing literature has shown that empowering women leads to increased short-term investments in children, little is known about its long-term effects. We investigate the effect of women's political empowerment on the human capital accumulation of children by exploiting plausibly exogenous variation in U.S. state and federal suffrage laws during the period 1910-1920. Following passage, voter turnout increased by 40%, directing lawmakers to place greater consideration for women's preferences - in particular for investments in child welfare - on the policy agenda. Using variation in the timing of the laws across states and in exposure to the laws within a state, we estimate that exposure to suffrage during childhood leads to large increases in educational attainment for some segments of the population. The increases in education are concentrated at primary levels of education and among children from economically disadvantaged backgrounds. For individuals born in the South, universal women's suffrage led to a one year increase in educational attainment, accounting for 24% of the 4.2 years gain between the 1880 and 1930 cohorts. The results suggest that the political empowerment of women raised the floor of achievement outcomes through the redistribution of resources towards public programs which benefited children with a high marginal return to investment.

JEL: I21, N32

We thank Doug Miller, Marianne Page, Hilary Hoynes, Scott Carrell, and Peter Lindert for many helpful conversations and support. We are also grateful for the input that we received from Celeste Carruthers, Bill Collins, Jae Wook Jung, Paco Martorell, Chris Meissner, Giovanni Peri, Shahar Sansani, Shu Shen, Marianne Wanamaker, and seminar participants at UC Davis. We benefited from data made publicly available by Daniel Aaronson and Bhashkar Mazumder; Daron Acemoglu, David Autor, and David Lyle; Claudia Goldin; Lawrence Kenny; and Adrianna Lleras-Muney. Our work was supported by a generous grant from the All-UC History Group. All errors are our own.

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1 Introduction

Women's economic and political empowerment is at the forefront of the policy agenda as a growing literature in economics suggests that empowering women could lead to economic development and growth. Research in both developed and developing countries has shown that greater economic and political power in the hands of women leads to increased household expenditures and funding for social programs directed towards children.[1] Together, these findings provide evidence of underlying systematic differences in preferences for investments in children between men and women. Less understood is whether these interests are shared closely enough amongst women to enact change on an aggregate level. And despite the increasing evidence of the positive effects of women's empowerment on short-term investments in children, little is known about its long-term benefits on human capital, development, or growth.

In this paper we provide the first of such evidence for a broad expansion of political power by analyzing whether women's enfranchisement in the Unites States led to increased educational attainment for exposed cohorts. The series of U.S. suffrage laws has been hailed as a "turning point in our Nation's history" (Obama, 2010); representing the single largest expansion, and arguably the most substantial transformation, of the American electorate. Newly empowered women exercised their vote in large numbers, as indicated by a 40% increase in voting among the adult population in the years following women's enfranchisement.[2] The lasting legacy of the suffrage movement is evident in the current political landscape, which features an increasing presence of women at all levels of government.

Importantly, the initial surge of women voters was not simply an expansion of the electorate; it ushered in a new era of policymaking. Responding to an expanded electorate which emphasized children's welfare as a top priority, lawmakers increasingly voted for liberal legislation and sharply expanded public spending programs. Prior work by Lott et al. (1999) and Miller (2008) establishes that expenditures on social and health spending programs rose by 36% and 24%, respectively, following the passage of the state laws.[3] Other investigations

[1] Duflo (2012) provides a nice overview of empirical findings in this vein. Also, see Thomas (1990); Duflo (2003); Thomas (1993); Chattopadhyay and Duflo (2004); Clots-Figueras (2012); Lundberg et al. (1997); Carruthers and Wanamaker (2014); Aidt and Dallal (2008). For a theoretical approach, see Doepke and Tertilt (2009), which develops a theoretical model for the incentives for power-sharing with women, linking the increase in women's power to men's desire to increase educational attainment of their children.

[2] For the rise in voting in gubernatorial races, see Lott et al. (1999); presidential elections, see authors' calculations in section 3.

[3] A related literature investigates the effects of black (dis)enfranchisement through the (enactment) removal of poll taxes and literacy tests, and finds that the ability to vote leads to greater expenditures directed towards those communities, higher teacher-pupil ratios and enrollment (Cascio and Washington, 2013; Naidu, 2012).

credit suffrage with an increase in education expenditures as well as the passage of public health initiatives, including the Maternal and Infancy Protection Act.[4] We hypothesize that public expenditures targeted at education, health, and social capital are a primary channel for improvements in education. Yet to date there is little evidence that the passage of suffrage and the accompanying infusion of public resources left any lasting impacts on the well-being of children, the intended beneficiaries.

The uniquely decentralized process of female enfranchisement in the United States provides an ideal context for studying the impact of women's political empowerment. The majority of suffrage laws were passed by U.S. state legislatures throughout the period from 1910 and 1920, and then mandated for the remaining states through a federal constitutional amendment. The quick succession of the laws in a short time period supports the comparison of outcomes across states while introducing substantial variation across cohorts within the state. In that sense, our study is well-positioned to provide evidence of a plausibly more broad-based and systemic empowerment to a literature that has previously been limited to expansions in female representation in local governments (Chattopadhyay and Duflo, 2004; Clots-Figueras, 2012).

Our empirical strategy exploits changes in voting laws across states and in exposure to the laws across cohorts, in a similar approach to the one successfully utilized in Lott et al. (1999) and Miller (2008). More specifically, we use a generalized difference-in-difference strategy that compares cohorts that were not of schooling age at the passage of the laws (control children) to cohorts that were of schooling age or not yet born when suffrage was enacted (treated children). The key identification assumption is that the timing of the laws is not correlated with differential trends in educational attainments across states. Previous studies have shown their timing to be uncorrelated with a host of state policies, economic and political factors, and demographics (Lott et al., 1999; Miller, 2008). We also rule out changes in the demographic composition of the state and other state policies as a source of confounding variation. Moreover, we explicitly test the identification assumption by estimating event study specifications that allow suffrage laws to have differential impact at each age of exposure to the laws. These specifications rule out the existence of differential pre-trends between our treatment and control groups.

To estimate the effects of women's enfranchisement on human capital, we utilize information on the state of birth and educational attainment of individuals from the 1880 to 1930 birth cohorts in the 1940, 1950, and 1960 decennial censuses. We supplement this data

[4] Passed in 1921, The Promotion of the Welfare and Hygiene of Maternity and Infancy Act, abbreviated as the Maternal and Infancy Protection Act or more commonly as the "Shepphard-Towner" Act, provided federal matching grants to states for the implementation of public health programs directed towards improving mother and infant health (Moehling and Thomasson, 2012).

with information on the literacy of individuals from the same cohorts in the 1920 and 1930 censuses.

We find that suffrage had a large positive impact on the education of children concentrated among those from economically disadvantaged backgrounds. In particular, we find that full exposure to suffrage between ages 0 and 15 leads to an additional year of education for black children, who have an average of 5.2 years of education in the pre-treatment period, as well as for white children from the South, who have 8.0 years of education in the pre-treatment period. Consistent with these findings, we show that the effects are concentrated at primary-level education, the mean schooling of the affected groups. The effects on years of education are mirrored in an event study of literacy attainment, although less precisely estimated. These results indicate that suffrage led to catch-up in large part by affecting the decision of children on the margin of leaving primary school.

The pattern of results suggests that the effects of suffrage were driven by the rise in expenditures following suffrage, although we explore other channels as well. We consider whether the results could be driven by a modeling effect by women, or an increased return to investment in girls, but find no evidence of larger effects for females. We also find little support for household bargaining behavior as a primary channel, as we find large effects among disenfranchised populations who would not have experienced an increase in bargaining power. Instead, we find that the results are consistent with a model of diminishing returns to investment, in which the largest impacts are seen among those with the fewest initial resources. We are given additional confidence in this interpretation by the fact that it is not sensitive to alternative measures of disadvantage or specifications.

We bolster the credibility of the estimates in a variety of ways. First, the effects of suffrage are similar among states that did not voluntarily adopt the laws, eliminating the possibility of endogenous law passage as a potential source of bias. The point estimates as well as the event study patterns are similar across both groups of states. Second, we stratify our estimation by migration status to investigate the role of endogenous migration. We show that our results are present only among individuals that did not migrate, and we find attenuation consistent with measurement error among the migrants. Finally, we show our results are insensitive to a variety of state-level controls, as well as to restricting the analysis to one census year at a time.[5]

Our results contribute to the growing literature in economics that has shown that women have different preferences than men regarding household and community expenditures and

[5] Goldin (1998) shows that the measure of educational attainment in the 1940 census is likely tainted by the rapid growth in high school attainment at the time of collection, implying that the data should be used with caution. Our results are invariant to the exclusion of the 1940 census.

investments in children. Research in developing countries has shown that income and assets in the hands of women lead to improved child health and to larger household expenditures on housing and health (Thomas, 1990; Duflo, 2003; Thomas, 1993; Lundberg et al., 1997). Moreover, increasing women's political power has been associated with greater investments in public goods preferred by women, improved infant health, and increased primary educational attainment for the cohorts affected by this political change (Aidt and Dallal, 2008; Chattopadhyay and Duflo, 2004; Clots-Figueras, 2012). These studies are limited to short-term effects and many study limited changes in political representation which may not be generalizable to other policies. Our findings contribute the first estimates of a broad-based expansion of women's political power.

Our findings also align well with an increasing number of papers that find that public health, social, and education programs – those expanded under suffrage – benefit populations with the most need. Studies of the distributional effects of large public spending programs such as Food Stamps, WIC, Medicaid, and Head Start (Almond et al., 2011; Hoynes et al., 2011; Currie and Gruber, 1996; Bitler et al., 2014) find larger effects among children with lower baseline outcomes. However, one limitation of this literature is that it focuses on short-term policy impacts, as each of these studies consider contemporaneous distributional effects. Instead, our findings suggest that expansions of government social and health programs produce *lasting* effects on human capital for individuals by raising the floor of educational attainment outcomes.

Finally, our estimates add an additional source of educational growth to explain the rapid rise in attainment during the early 20^{th} century. A large literature explores factors such as the institution of child labor and compulsory schooling laws, improved transportation options, philanthropic educational ventures, economic growth, and increasing economic self-sufficiency of blacks.[6] Nonetheless, a significant portion of the rise in attainment remains unexplained. We show that suffrage contributed to significant growth in education levels, accounting for 24% of the 4.2 years growth in educational attainment among Southern-born individuals in the sample.

The remainder of the paper continues as follows. We present theoretical expectations and prior literature in Section 2. In Section 3 we provide institutional background on the passage of suffrage laws. Section 4 describes our data sources, followed by an overview of our empirical strategy in Section 5. We present our results in Section 6, robustness checks in Section 7 and conclude in Section 8.

[6] See Goldin and Katz (2010) for an overview; Lleras-Muney (2002); Goldin and Katz (2003) for child labor and compulsory schooling laws; Aaronson and Mazumder (2011) for philanthropy in the South; and Collins and Margo (2006) for a detailed analysis of the evolution of racial gap in schooling.

2 Theoretical Expectations and Prior Literature

Although women had gained some economic rights prior to the passage of suffrage (Baker, 1984; Doepke and Tertilt, 2009), enfranchisement was an important landmark for their empowerment. The ability to vote gave women influence over the direction of policymaking in two ways. First, enfranchisement provided women with access to direct democracy. Prior to suffrage, women could only marginally affect the election of representatives by influencing a male proxy, such as their husband. The ability to cast their own vote allowed women to have a say in local policies and elect representation closer to their preferences. In aggregate, by nearly doubling the size of the electorate, suffrage shifted the interests of the median voter. Theory suggests that such a shift would be reflected in differential legislative representation following suffrage. Lott et al. (1999) show that this is indeed the case; liberal voting increased in both houses of Congress following suffrage.

A shared set of policy interests provides a second channel of influence for women. In the early years of suffrage in particular women's lobbies effectively created the perception of close political alignment among its members (Moehling and Thomasson, 2012; Lemons, 1973). In the case of Virginia gubernatorial election in 1920, a former anti-suffragist was handily defeated due to organized opposition from the League of Women Voters; who instead endorsed the opposing candidate for his support of progressive legislation, including improved roads to allow rural children to attend school (Walker et al., 2003). Examples such as these may have led politicians concerned about retribution at the polls, on the margin, to choose to push forward legislation favored by women. This is also consistent with models of distributive politics which suggest that politicians will respond to the enfranchisement of a distinct and recognizable group of constituents with "particularistic" interests through the distribution of resources (see, e.g. Cascio and Washington (2013); Dixit and Londregan (1996)).

Each of these political mechanisms supports a shift towards greater legislative efforts targeting children's welfare, a top policy priority among women of the suffrage movement. Although the movement was largely divided along racial lines, both white and black women saw suffrage as a vehicle for change (Wheeler, 1995; Green, 1997). For black women, sources suggest that suffrage was viewed as an opportunity to "help uplift the standard of their race through the franchise" (Wheeler, 1995), while white women hoped to use a newfound political power to address local concerns which could not be solved by club actions alone (Green, 1997). Moreover, women's organizations in the early 20th century lobbied for the passage of children's codes to regulate child work, guardianship, and mandatory school attendance (Lemons, 1973). The passage of the Maternal and Infancy Protection Act of 1921 and other public hygiene measures targeting child health have also largely been attributed to efforts of women's rights organization and the female-led Children's Bureau (Lemons, 1973).

Empirical analyses of the effects of suffrage have uncovered large effects of women's suffrage laws on public spending, on social and health programs in particular. Lott et al. (1999) produced the first of such evidence, finding a 13.5% increase in state government expenditures and more liberal representation in Congress after the passage of suffrage. Miller (2008) extends these findings using a dataset of city-level expenditures and estimates a 36% increase in municipal expenditures towards charities and hospitals and a 24% increase in state spending on social programs. Both papers find that increases in spending were sharp and followed immediately after the passage of the laws (Lott et al., 1999; Miller, 2008). Miller's analysis also finds a reduction in child mortality as large as 15%, which he attributes to public sanitation projects funded after suffrage. Finally, Carruthers and Wanamaker (2014) link voting behavior post-suffrage to higher local spending on education for both white and black schools in three Southern states, with larger increases accruing to the white schools than blacks.[7]

Based on estimates from other interventions during this time period, the effect of these expansions in government health and education programs could have had a significant impact on affected children. Bleakley (2007) finds that a hookworm eradication scheme generated large increases in school attendance and literacy and long term effects on income, although no statistically significant impact on attainment.[8] Aaronson and Mazumder (2011) examine the impact of the Rosenwald Rural School Initiative, which took place in the South and aimed to narrow racial discrepancies in education by increasing the resources available to black rural communities.[9] They find significant effects on school attendance, literacy, years of schooling, cognitive test scores, and northern migration for blacks. Carruthers and Wanamaker (2013) clarify that the philanthropic funds actually benefited expenditures towards white and black schools, although white children did not show the same educational gains. Overall, these studies offer a large scope for improvements to human capital through targeted public programs, in particular from health and education spending.

Nonetheless, it remains ambiguous which populations, if any, would have benefited from such expansions. Importantly, although suffrage gave voting rights to women *de jure*, in

[7]The analysis in Carruthers and Wanamaker (2014) takes advantage of a unique dataset of *local* school spending by county and race in Alabama, Georgia, and South Carolina to overcome the limitation of prior studies of suffrage, which found null results of suffrage on *state* education spending. The measured increases in education spending are positive in the first years following suffrage, and increase in magnitude over time.

[8]Given the coincidental timing of the hookworm eradication scheme, one may be concerned about confounding variation. However, there is no correlation between the pre-treatment hookworm infection and the year of suffrage; all of the treated states adopted suffrage in the same year.

[9]From 1914-1931, approximately 5000 schools were constructed due to the enactment of Rosenwald Initiative in the rural south.

practice black men and women were disenfranchised until the 1960s through literacy tests and poll taxes (Cascio and Washington, 2013; Naidu, 2012). Therefore, the consequences of suffrage rely on the magnitude of the shifts in the preferences of the median voter resulting from the introduction of *white* women to the electorate. This leaves open three possibilities for children's education, depending on the level of altruism of white women. First, if white women are completely self-interested and vote for representatives that would only approve funding that directly impacts their children, then we would expect no impacts on resources outside that community. Thus, our average result will reflect the marginal effect of resources on white children (scaled by their proportion of the population). Second, suppose that white women are self-interested, but internalize the externality of negative outcomes, such as poor health, accruing to other children. In that case, we may expect an increase in resources targeted towards programs that benefit the population generally and which would indirectly improve the outcomes of white children (such as sanitation), and additional resources for goods that directly impact welfare of white children (such as local schooling). Finally, if white women are somewhat altruistic, then we expect that the median voter will influence representatives to increase resources for many public programs (public goods as well as school expenditures), though disproportionately for white children. This would also be consistent with a Tiebout-type model in which white women increase public provisions for the black community in order to attract black families and labor (Carruthers and Wanamaker, 2014). In the second and third cases, the measured average impact of the program will reflect the average marginal effect of increased resources across the benefiting populations.

We obtain some evidence of which of these models may be most realistic from prior analyses in the literature. The first piece of evidence comes from Miller (2008), who finds large increases in public health efforts and a decline in child mortality following suffrage. In results unreported, we repeat his analysis, disaggregating by race, and find similar effects in both populations, hinting that the public projects were not restricted to the white communities.[10] Second, Carruthers and Wanamaker (2014) find that both black and white schools experienced an increase in resources following suffrage, although larger increases were directed at the white schools. Each of these seems to suggest that the first model of preferences is inappropriate, although does not distinguish between the other two explanations. Therefore, we leave open the possibility of impacts on all populations.

[10] For this analysis, we digitized mortality records by state, age, race, and gender from 1900 to 1932 obtained from the Centers for Disease Control and Prevention at http://www.cdc.gov/nchs/products/vsus/vsus_1890_1938.htm. Results and data available from the authors on request.

3 Passage of Women's Suffrage at the State-Level

The passage of women's suffrage by states was an important first victory after a decades-long struggle for the women's rights movement.[11] According to one historian, "while some women had struggled to win the franchise since before the Civil War, not until the first decade of the twentieth century did it become a major issue to millions of women...New leaders, new tactics, new ideas, and new interest accounted for these leaps" (Lemons, 1973). In this section, we discuss the timeline of the passage of suffrage and explore the potential explanations for the timing of its spread.

We illustrate the sequence of the laws across states in Figure 1 using data from Lott et al. (1999) and Miller (2008). The first states to grant the vote to women were Wyoming, Utah, Colorado, and Idaho in 1869, 1870, 1893, and 1896, respectively. This group of "early adopter" states is noted for the distinctive environment in which they were passed. Located geographically in a cluster in the "Wild West", many political writings have discussed the favorable demographic and political conditions for suffrage (see Braun and Kvasnicka (2013) and references within). Among them are the notion of "frontier egalitarianism", greater equality of sexes due to the harsh conditions of the West and a less stringent amendment process, and a relatively low political "cost" of suffrage to men due to the low number of women in the West (Braun and Kvasnicka, 2013).

Almost fifteen years passed before the next state, Washington, adopted suffrage in 1910. Following, a number of states passed the law each year from 1911 until 1920, when suffrage was federally mandated by the Nineteenth Amendment. The Amendment obligated all states to enact suffrage, despite the fact that support of the Amendment was not unanimous. Three-fourths, or 36 of the 48 states, ratified the amendment prior to its passage, with additional states continuing to ratify over the next sixty years.[12] The remaining 12 states that did not voluntarily adopt the amendment provide us with a strong test of our empirical strategy,[13] and we check for differential effects across the two groups of states in Section 7.

Although historical election data do not record voter turnout by gender, the aggregate voting patterns following the passage of suffrage provide strong visual evidence of a "first stage" effect of the policy. In Figure 3 we present an event study of presidential elections[14]

[11] The Seneca Falls Convention, the first large-scale organizing meeting for women's rights, took place in 1848.

[12] Mississippi was the final state to ratify the amendment in 1984.

[13] See Figure 1 for a map which differentiates between these groups of states.

[14] We focus on presidential elections because turnout in presidential elections is higher than in other elections, and thus provides a more reliable measure of turnout (Cascio and Washington, 2013). Lott et al. (1999) perform a similar analysis using gubernatorial data and find a 48% increase in turnout following suffrage.

voter turnout relative to the population over 21, centered around the passage of suffrage in the state.[15] Voter turnout data are obtained from Electoral Data for Counties in the United States provided by ICPSR,[16] and population over 21 is estimated using decennial census data (Ruggles et al., 2010) and interpolation between censuses. Controlling for year-of-election fixed effects, state fixed effects, and state trends, we estimate that the turnout rate increased by 35 log points, or 41 percent, following suffrage. These estimates confirm that suffrage had a meaningful impact on the landscape of voting in the United States, and had the potential to generate a substantial shift in the preferences of the median voter.

Our empirical strategy relies upon the idiosyncratic nature of the timing of suffrage laws. Prior studies in this literature have explored this variation in detail and find few significant correlates of the laws. Dahlin et al. (2005) and Braun and Kvasnicka (2013) identify just two variables among many social, political, and cultural characteristic tested that are predictive of the passing of suffrage, the percent of women in non-agricultural occupations and the percent of women in the state. Miller (2008) performs a number of additional tests which verify that the laws were not correlated with any other progressive legislation during the period including regulations governing alimony and divorce, mother's pension, women's maximum hours, women's minimum wages, prohibition, worker's compensation, child labor, and compulsory schooling and were not correlated with the state literacy rate or manufacturing wages. These results serve to reassure us that the laws are not correlated with state-level observables.

Importantly, confounding variation in the context of our analysis must be sharply discontinuous in the year that suffrage was passed in the state, as smooth changes within states will be absorbed in the econometric specification. In Table 1 we explicitly test whether there were discrete changes in any of a host of demographic and economic variables with suffrage, including percent female. One variable only is individually statistically significant, and there is no systematic pattern that emerges from the results.

Nevertheless, we worry about the regional clustering of the passage of the laws in Figure 1, in particular if education outcomes are also spatially correlated. To gauge how problematic this variation may be for our design, we plot the mean educational attainment by birth cohort for each of four Census-defined regions in Figure 2 to see whether there are also strong regional differences in education levels. Across all cohorts, the West leads in educational attainment, followed by the Midwest, the Northeast, and the South, which has education levels far below the other regions. This figure validates our concern over the regional correlation

[15] In particular, we regress $\frac{\text{Total Turnout}_{st}}{\text{Population 21+}_{st}}$ for state s in year t on a set of event time dummies centered around the year of suffrage, state fixed effects, state time trends, region by year fixed effects, and state-level controls. All coefficients are measured relative to $t \leq -9$, the omitted category.

[16] http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/8611.

of the laws, and motivates a design which abstracts from these regional comparisons. In particular, we incorporate a region by birth cohort fixed effect in our empirical specification, imposing comparisons within regional cohorts. We do so as a cautionary step despite the previous evidence that the timing of the laws was not correlated with adult literacy (Miller, 2008), which suggests that this control may not be necessary, and therefore consider our estimates to be a conservative estimate of the effects of suffrage.

4 Data and Summary Statistics

In order to analyze the effect of women's suffrage laws on children's academic achievement we generate two pooled cross-sectional samples using data from the 1920 and 1930 censuses and the 1940, 1950 and 1960 U.S. decennial censuses. The bulk of our analysis focuses on impacts on educational attainment, which we observe in the latter sample; while the earlier samples allow us to also explore effects on literacy, which is not available after 1930. The data in each year are a 1% representative sample of the U.S. population and are publicly available through the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al., 2010). The Census collects detailed demographic and economic information at both the household and individual level. Relevant for our research design, the samples contain information on the year and state of birth, as well as the years of completed education for each individual (from 1940 on) and literacy (until 1930). The state of birth will serve as a proxy for childhood location for the analysis and jointly with year of birth can be used to determine the extent to which each individual was "treated" by suffrage. We discuss this in further detail in Section 5.

We obtain the dates of women's enfranchisement for each state from Lott et al. (1999). Following the prior literature, we use the date of earliest suffrage, although in some states women were not granted full voting rights.[17] The main motivation for doing so is the concern that the choice to extend partial or full suffrage rights may have been influenced by the uncertainty regarding the likelihood of federal enfranchisement, and that restricting to one group of states may therefore introduce selection into the analysis (Miller, 2008). In practice, the distinction has made little difference in prior work (Miller, 2008).

We conduct the following sample restrictions. First, we exclude individuals born in Alaska, the District of Columbia and Hawaii, which were not U.S. states by the time that the federal law was passed in 1920, and therefore not subject to the laws.[18] Second, we

[17] Presidential-only suffrage laws were passed in Illinois, Indiana, Iowa, Maine, Minnesota, Missouri, North Dakota, Ohio, Rhode Island, Tennessee, Vermont and Wisconsin. Arkansas and Texas, instead, passed primary-only laws (Miller, 2008).

[18] These states could serve as an interesting falsification test, but unfortunately compose too small of a

drop individuals born in Colorado, Idaho, Utah and Wyoming, the states that enacted early suffrage laws (between 1869 and 1896), due to data limitations. Cohorts treated by the laws in these states are between forty and seventy years old at first observation in 1940,[19] leading to issues of selective mortality and/or unbalanced panels.[20] After we exclude these early states, suffrage laws in the remaining states were enacted within a short time window, between 1910 and 1920.

Third, for the analysis of educational attainment we restrict the sample to individuals that are at least 20 years old to allow time for individuals to have completed schooling.[21] Further, the analysis sample for the literacy outcome is limited to individuals at least 15 years old in order to avoid bias from early schooling effects (Aaronson and Mazumder, 2011). Finally, we include only cohorts born between 1880 and 1930. With this restriction we obtain a panel that is balanced on cohort, although somewhat imbalanced on the age of treatment. Figure 4 visually depicts this variation across states by the year of suffrage. Highlighted in the figure is the substantial overlap across states in the "treatment age" of individuals in our sample, or the age of the individual when suffrage was passed in the state. This ensures that our estimates will not be biased due to variation in sample composition across age of treatment.

After these restrictions we are left with a total of 1,555,475 observations for the analysis of educational attainment, of which 572,126, 227,541, and 755,808 are from the 1940, 1950, and 1960 Censuses respectively.[22]

Table 2 presents descriptive statistics for our sample, first for whites and blacks in the entire sample, and then for each of the four census regions separately. The first two columns illustrate the significant variation in educational environments across races. Relative to blacks, whites had substantially higher educational attainment, with a mean of 9.96 years compared with the black mean of 6.76 years. Moreover, blacks have lower average exposure to suffrage laws, mainly because they are concentrated in the east coast and in the south, where suffrage laws were passed later. Whites in the sample tend to come from states with a higher rate of literacy, and higher wages.

National means obscure important regional differences, however, such as those previewed

sample to draw any meaningful conclusions from the results.

[19]Unfortunately, educational attainment was first collected in 1940, so we can not observe these cohorts at a younger stage.

[20]We check the sensitivity of our results to these sample restrictions in analyses not shown. None of the sample restrictions are consequential for the results.

[21]75% of the sample completed at most 12 years of education, which corresponds to a school leaving age of 18. Additionally, our results are not sensitive to alternative age cutoffs, such as age 25.

[22]We have fewer observations from the 1950 Census because in that year the Census only collected information on years of education for one individual per household.

in Figure 2. For example, individuals from the South have much lower educational attainment than the rest of the country, 6.45 and 9.12 years for blacks and whites respectively, reflected in the lower literacy rates of the region. On the other hand, individuals from the West have higher levels of education, 10.38 and 11.02 years for blacks and whites respectively.[23] Again, these regional differences highlight the need of within-region comparisons in the presence of regional variation in the timing of suffrage laws.

We augment the Census data with supplementary state-level variables to control for variation in demographic and economic composition and policies in the state, which may be confounded with suffrage. First, we source state-level variables over the period 1880-1939 from a combination of Lleras-Muney (2002)[24] and the ICPSR data series "Historical, Demographic, Economic, and Social Data: The United States".[25] The variables include: percentage female; population in thousands; percentage black; percentage illiterate; employment in manufacturing; total wages paid in manufacturing in thousands; total value of farm property; percentage urban population; percentage foreign born; and number of farms.[26] The data from Lleras-Muney (2002) span the years 1915-1939 and have been utilized in many previous studies of this time period, such as Goldin and Katz (2010). The ICPSR data series, which harmonizes information from Census of Manufacturing and Census of Agriculture, allows us to extend this set of controls for the period from 1880-1915.[27]

Second, we include state-by-cohort varying measures of the intensity of education policies. One such set of interventions is the introduction of state compulsory education and child labor laws, which were adopted in some form by all states by the early twentieth century (Goldin and Katz, 2003; Lleras-Muney, 2002). The laws used minimum schooling levels, maximum school entry age, and minimum school leaving ages as levers in a policy that induced large gains in schooling (see Lleras-Muney (2002), for example). Since these laws varied substantially over time, we include a control for the relevant compulsory education and child labor laws for each cohort in the state by aggregating annual data from Goldin and Katz (2003).[28] [29] The Rosenwald Rural Schools Initiative, discussed previously, is another

[23] Note that the number of observation in the West is small, thus these averages might be imprecise.

[24] These data are compiled from a number of sources; see http://www.econ.ucla.edu/alleras/research/data.html for details.

[25] For the ICPSR data source, see: http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/2896.

[26] Lleras-Muney (2002) has additional information on educational expenditures from Biennial Survey of Education; number of tax returns and total net income reported by state from Statistical Abstract of the United States. We do not include these because these information are only available starting 1916.

[27] This data was reported every 10 years from 1860 forward; we linearly interpolate the intermediate years. Following Lleras-Muney (2002), all monetary values are adjusted for inflation using the Consumer Price Index, 1982-1984 as the base period.

[28] In particular, we include a control for the relevant school leaving age and working age for the cohort in each state.

[29] Data for the schooling laws spans the years 1900-1939. For access, visit http://scholar.harvard.edu/

of such policies. We aggregate the county-level Rosenwald student exposure measure from to generate a measure of the average reach of Rosenwald over the childhood of each individual (Aaronson and Mazumder, 2011).[30]

5 Empirical Strategy

Our empirical strategy utilizes a generalized difference-in-difference approach, which compares the outcomes of cohorts born prior to the enfranchisement of women in the state, and hence less treated or untreated by the laws, to those born after the law's passage in the state, who were completely treated. We begin by estimating an average effect of the laws, as follows:

$$YrsEd_{icsrt} = \beta_0 + \beta_1 SuffExp_{cs} + \gamma_1 X_{icst} + \gamma_2 Z_{cs} + \theta_c + \delta_s + \chi_s * c + \tau_r * \phi_c + \epsilon_{icsrt} \quad (1)$$

where i, c, s, r, and t represent individual, cohort, state of birth, region of birth, and survey year, respectively, and $SuffExp$ is a measure of exposure to the suffrage laws. δ_s and θ_c flexibly control for differential political, education, and education climates across states and cohorts, respectively. A state-level trend, $\chi_s * c$, controls for linear changes in education at the state level across different years of birth. We also include individual controls, X_{icst}, such as race, age, and gender, to absorb differences across demographic groups in educational attainment. Moreover, we add a variety of state-cohort controls, Z_{cs}, to account for time-varying non-linear changes in state demographics, employment, and changes in education policy and availability. These controls include percentage white, percentage female, percentage foreign, percentage urban, percentage literate, population, real manufacturing wages, employment in manufacturing, real farm value, measure of intensity of Rosenwald schools, and compulsory schooling age and are measured at the year of birth.[31]

Finally, we include region by cohort fixed effects, $\tau_r * \phi_c$, to control for unobservable differences across regions over time which may be responsible for the regional spread of suffrage and correlated with education outcomes.[32] Stephens and Yang (2014) highlight the salience of this issue in the compulsory schooling literature, arguing that region by cohort fixed effects act as an important control for otherwise omitted variables. Given the similar timing and substance of the suffrage movement and child labor laws, we impose the same specification to preempt similar concerns. The remaining variation across cohorts within a state comprises the key identifying variation for the analysis.

goldin/pages/data.

[30] For further details about this data, visit http://www.jstor.org/stable/10.1086/662962

[31] We experiment with the sensitivity of our results to varying functional forms for these controls in Section 7 and find little differences across the specifications.

[32] We group states into four regions, West, Midwest, Northeast, and South, using the Census classification.

In our preferred specification, we define suffrage exposure as the share of time between birth and age 15 that women are able to vote in an individual's state of birth,[33] $PercentTreatBy15_{acs}$. Formally,

$$PercentTreatBy15_{acs} = \sum_{a=0}^{15} \frac{1(c + a > YearSuffrage_s)}{16} \tag{2}$$

where $YearSuffrage_s$ is the year in which suffrage was passed in the state. We define the relevant age of exposure ending at the typical school-leaving age, 15 years,[34] at which point children are on the margin of leaving school and are susceptible to policy changes.

The identifying variation for the coefficient of interest, β_1, is generated by differential exposure to suffrage within cohorts and across states (within regions), as well as within states and across cohorts (within regions). Figure 5 illustrates this variation. For parsimony, we group together states who passed suffrage in a short span from another. Moving along the line shows the variation across cohorts controlling for the year of suffrage (within-state variation), while the horizontal spread depicts the variation across states (within-cohort). Since the law never "turns off", the percent of exposure is collinear with the birth year and the year of suffrage. In terms of our identification, this implies that we cannot disentangle the effects of being exposed at an early age and being exposed for a longer period between 0 and 15.

There are two identifying assumptions needed to estimate an unbiased estimate of the effects of suffrage within this model. First, we require that there not be any confounding events with suffrage, which we discuss in Section 3. Second, non-suffrage granting states must represent a plausible counterfactual for the outcomes in suffrage-granting states. Threats to identification, then, are any differential trends among states that are correlated with the passage of suffrage laws, which may also influence educational outcomes.

To move beyond an average effect, we estimate a event-study specification, which allows us to estimate differential effects of the policy by age of exposure. In particular, we estimate:

$$YrsEd_{icsrt} = \beta_0 + \sum_{a=-5}^{25} \beta_a \mathbb{1}(AgeTreat_{cs} = a) + \gamma_1 X_{icst} + \gamma_2 Z_{cs} + \theta_c + \delta_s + \chi_s * c + \tau_r * \phi_c + \epsilon_{icsrt}, \tag{3}$$

where $AgeTreat_{cs} = YearSuffrage_s - c$. We set $AgeTreat_{cs}$ equal to 30 for all $AgeTreat_{cs} \geq$

[33]We define treatment using state of birth because that is our best proxy for the location where an individual spent their childhood.

[34]Calculated as the sum of the median age that the typical child began school prior to suffrage , and the average years of schooling prior to suffrage, 7 and 8 years respectively (Collins and Margo (2006) and author's calculations.)

30 and to -10 for all $AgeTreat_{cs} \leq -10$.[35] Grouping in this manner increases the precision of our estimates and allows us to estimate state trends and region by birth cohort fixed effects without dropping additional event-time dummies. All coefficients are measured relative to the omitted category, $AgeTreat_{cs} = 30$.

This specification also provides a natural test of the identifying assumption of the model. By estimating separate treatment coefficients for each age of exposure, we can examine whether the measured effects of exposure are due to a discrete increase at the time of the law's passage, or reflect a trend in education outcomes between cohorts. In the case of the former, we expect that the indicators for age of treatment will be close to 0 for all ages after 15, and positive and increasing for age of treatment less than 15. This specification also tests the linearity of the treatment effects. If the effects are linear, then the marginal impact of an additional year of exposure should be equivalent across all ages. A growing literature on in-utero exposure to public programs suggests that health improvements are most impactful prior to birth up to age 5. If our impacts are driven primarily by that mechanism, we expect to see small effects at all ages except 0 to 5 (Hoynes et al., 2012); in which case the linear estimates are simply smoothing the non-linear effect.

6 Results

In Table 3 we present the results for the entire sample, by race, and by race and gender. Column (1) of Table 3 shows the estimated results for all individuals using equation (1). Overall, we find that full exposure to suffrage, from 0% to 100% of the period from age 0 to 15, leads to a 0.10 increase in average years of schooling across the sample, although not estimated with statistical precision. Based on the theoretical predictions, this is not entirely surprising. Significant gains to some pockets of the population may be masked in the mean effect.

Moving forward, we begin to disaggregate these results to move beyond a mean treatment effect, first by race and then further by gender. Here we use race as a proxy for educational and political advantage to test our theoretical predictions. Marked gaps in educational attainment, reflective of differences in the baseline investment in the human capital across black and white children, further separate the race groups.

The estimates for suffrage exposure for the white and black subgroups are displayed in Table 3, Columns (2) and (3).[36] For whites, we find that suffrage exposure increased educa-

[35]Moreover, we group the age at treatment indicators into groups of two. For example, $AgeTreat_{cs} = -10$ and $AgeTreat_{cs} = -9$ both become $AgeTreat_{cs} = -9$, $AgeTreat_{cs} = -8$ and $AgeTreat_{cs} = -7$ both become $AgeTreat_{cs} = -7$, and so forth.

[36]We exclude individuals that did not qualify as neither white nor black from this subgroup analysis. The

tion by a statistically insignificant 0.05 years. The small point estimate here indicates that either the newly empowered white women did not, on average, use their enfranchisement to divert resources towards their community, or that the resources had little effect on the relatively more educated white children. Evidence presented in Section 2 negates the plausibility of the former explanation. Rather, it seems more likely that the marginal impact of funding for this group, on average, was low, consistent with Jackson et al. (2015).

Column (3) shows the equivalent impact for black children. Here we find that full exposure to suffrage produced gains of 1.12 years, significant at the 1% level. At the mean level of black educational attainment, this increase represents a 16% gain in completed education. Taken together with the results for white children, the estimates lend themselves towards an interpretation in which the less-advantaged benefited most from the great expansion in public expenditures. We will explore this theory further shortly.

In the remaining four columns of Table 3 we analyze whether suffrage differentially affects male and female children. Some of the prior studies in the bargaining literature have found that transfers of power to women tend to be passed on to female children and likewise for men to male children (Qian, 2008; Duflo, 2003). For suffrage, there are two main channels by which the gender differences may take form. The first is that suffrage increases the value of daughters, directly through political power and indirectly by providing a mechanism through which women can improve their economic and social standing. As a result, the marginal returns to investment for parents also rise. Second, there may be a modeling effect for younger girls inspired by women's expanded political rights. We analyze these differential effects by splitting the sample both by race and by gender. Therefore columns (4) and (5) contain results for the sample of white males and females, while the last two columns present results for the sample of black males and females, respectively. Again, we find no statistically significant result for either of the white subsamples. The point estimate for black males is slightly higher than for females, 1.35 versus .885, although the standard error is larger as well.

The validity of our estimates relies on the assumption that the timing of suffrage is not correlated with trends in educational achievement. As discussed earlier, the event-study specification in equation 3 provides a visual test of this claim. Drawing on our earlier results, we estimate the event study for each race separately.

We present the results of this estimation in Figure 6, where we plot the estimated coefficients as well as their 95% confidence intervals against the age of treatment. We begin by discussing the estimated effects for blacks which are plotted in dark markers on the figure. For this group, the figure shows that suffrage had small, insignificant effects for cohorts

excluded sample is small, with only 4,592 observations.

that were exposed to suffrage after age 15, and larger, positive, and statistically significant effects for black children that were exposed to such laws at younger ages. The point estimate steadily increases as the age of exposure decreases, and becomes steady at an effect of magnitude around 1 year for children exposed at age 5 and earlier. For the white sample, plotted in the unfilled markers, the effects hover at zero and are flat at all ages of treatment.

Importantly, the pattern of the coefficients is compelling evidence in favor of our empirical strategy. The flat coefficients for the sample treated after age 15 suggests that our effects are not capturing differential trends in educational attainment across cohorts.[37] We consider this as definitive and compelling evidence that our estimates are not tainted by endogenous adoption of the laws.

Reassured by this validation, we begin to probe the potential channels which may have generated this patterns of increasing coefficients with decreasing age of exposure. We posit two explanations. The first is a model of child investments which includes complementarities between early investments and later investments (see, e.g. Heckman (2007), Cunha and Heckman (2007)). This theory is re-enforced by recent empirical work on childhood investments, which shows that interventions may be more effective when introduced at early ages because they occur at a critical stage of development during the programming of the body (Hoynes et al., 2012). Under this explanation, children exposed at younger ages experience larger effects because the marginal return to investment is higher. For example, they might experience health improvements at a young age, which lead to improved learning during school. The second explanation is a simple accumulation effect. Children that are treated at younger ages have more time to experience higher quality schooling and sanitation, and therefore remain in school longer.

One way to distinguish between these effects is by investigating whether the effects of suffrage are in fact higher during the earliest ages, as the former theory would suggest. In Table 4 we quantify the slope of the event study for first exposure at three age ranges; 0-5, 6-10, and 11-15. Consistent with the figure, Table 4 reveals that the effect of suffrage is positive and larger during primary school, from age 6 to 10, and that there is little additional effect to exposure prior to age 5. The flattening of the impact at primary school age, just after a critical stage of development, is suggestive that the second explanation may be more appropriate, although not definitive. If that is the case, then the crucial mechanism is more likely expenditures on schooling and social support during schooling ages, rather than improved health at early ages.

[37]We formally test for an effect of suffrage beyond age fifteen in Appendix Table A.1 by including the average exposure between age 16 and 22 and between 23 and 30 as additional covariates. The measures of exposure at later stages are much smaller in magnitude and not significant, while the coefficient on exposure between age 0 and 15 remains stable.

6.1 Understanding Impacts Across Subgroups

We now turn towards further understanding the pattern of results captured in Table 3 and Figure 6.

There are several mechanisms that would be consistent with the larger effects estimated for blacks. The first is that the aggregate amount of resources which reached the black community following suffrage surpassed the gain in resources for the white population. In other words, the programs which expanded most as a result of suffrage were those which were most likely to be utilized by blacks, such as social and health spending. Evidence presented in section 2 suggests that this was not the case for education expenditures, but since we do not have program utilization rates by race for this period, we cannot rule out this channel. The second explanation posits that the increases in spending were more or less equivalent, but that blacks had a higher marginal return to investments, due to the low baseline level of resources in that community. If this is the case, then we would expect that other disadvantaged groups would have benefited similarly from the laws, perhaps proportional to their initial level of education.

To explore this more deeply, we look into whether we see larger effects for more disadvantaged groups *within* racial groups as well. To do so, we first re-estimate equation (1) on a series of subgroups defined by region, race and gender. We also generate a measure of "propensity to be impacted", which we simplify as π, by calculating the average education level of the subgroup cell for non-treated individuals, i.e individuals who were age 16 or older at the passage of suffrage. Figure 7 plots the coefficient estimates for *PercentTreatBy15* from each of the regressions against the pre-treatment education. A clear negative relationship between the size of the coefficient and π emerges, implying larger treatment effects among groups that have lower levels of education at baseline. We also notice that the impacts are no longer solely concentrated among black individuals. White boys and girls in the South, who have average educational attainment of 8 years at baseline, also experienced a positive effect of the policy of a reasonable magnitude, comparable to the effect size for blacks with similar initial education levels.[38] We perform the same exercise using other measures of advantage - share of individuals that own a home, share of individuals in urban locations, and average log income - and include the results in Figure A.1. The patterns are quite similar regardless of the metric used.

Then, we formally test for a relationship between the effect size and π by adding an interaction between *PercentTreatBy15* and π to equation (1). To gain additional variation, we define π by state, race, and gender. The main effect and the interaction are reported in

[38] In results not shown, we estimate that full exposure to suffrage leads to a 1.08 year gain in education for white children in the South, significant at the 5% level.

Table 5. The effects for the sample as a whole are included in Column (1). The coefficient on *PercentTreatBy15*, 1.23, represents the average effect for a group with zero education at baseline. The coefficient on the interaction is negative and significant, consistent with Figure 7. In Columns (2)-(5) we show the specification repeated within gender-race subgroups and find similar effects. This suggests that the impact of suffrage was near-universal at low levels of education across all races, but does not appear in the average effect for whites because of the composition of the sample.

To gain a richer understanding of the effects on attainment, we employ distributional methods to identify the margin of educational attainment most impacted by suffrage. Specifically, we look to estimate the effects of exposure to suffrage on the cumulative distribution function (CDF) of educational attainment (Duflo, 2001), and whether the treatment causes there to be an increase in the probability of having *higher* levels of education (1-CDF). In the case of a binary treatment, this simplifies to comparing the CDF of educational attainment for the untreated and treated groups; the difference represents the shift resulting from the policy. The same intuition can be extended to a continuous measure of treatment, such as in our context, although the implementation is more complex.

In practice, we estimate a progression of models in which we substitute the continuous education variable with a dummy that indicates whether the completed education of individual i is greater than p (1- CDF), where p takes on the discrete values from 0 to 17 (Almond et al., 2011; Duflo, 2001).[39] [40]

Panels (a) and (b) of Figure 8 plot the coefficients obtained from this estimation procedure for the black and white samples, respectively. For blacks, we find that the effect is concentrated between 4 and 6 years of education, while for whites we find small effects between 7 and 9 years of education. Recalling the mean education levels from Figure 7 these effects line up well with the mean level of education for blacks overall (5.2 years) and for whites from the South (8 years), the impacted subgroups. To check the alignment of these effects with the *distribution* of estimates, we also show the fraction of the population at each level of education at baseline. Now it becomes clear that largest impact appears close to the median for each group, 5 and 8 years for blacks and whites respectively. Thus, it appears that one of the main benefits of suffrage may have been to help raise the bottom and middle of the distribution of historically less educated communities.

[39]Specifically, we estimate:

$$G_{icsrtp} = \beta_0 + \theta_p SuffExp_{cs} + \gamma_1 X_{icst} + \gamma_2 Z_{cs} + \rho_s + \chi_s * c + \delta_c * \psi_t + \tau_r * \phi_c + \epsilon_{icsrt}, \tag{4}$$

where G_{icsrtp} is a dummy that indicates whether the completed education of individual i is greater than p.

[40]The Census does not allow reporting of attainment beyond 17 years. We do not believe this influences our estimation.

6.2 Literacy

The previous discussions focused on the impact of suffrage on the quantity of education attained. An additional important question, in particular for labor outcomes, is whether the additional time in school led to the acquisition of additional skills which may be rewarded in the labor market. Unfortunately, the census data is not ideal for looking at this question broadly because it has few measures of human capital beyond years of education. However, a crude measure is the attainment of literacy, which was collected by the Census until 1930. Since literacy is acquired with approximately three years of schooling (Collins and Margo, 2006), a margin surpassed by most children in our sample, we expect our estimates to be quite imprecise. Nonetheless, the event study in Figure 9 suggests that there were positive impacts on literacy, with up to a 5 percentage point increase for black children exposed at the youngest ages. The shape of the plot mimics the pattern of the coefficients for education, with small or zero effects for individuals exposed after age 16, increasing effects for children exposed during schooling age, and a relative flattening of the cumulative impact for children born when suffrage was already enacted. While the results are measured with error, this is suggestive evidence that suffrage led to improvements in skills together with extended schooling.

6.3 Discussion of Mechanisms and Magnitude of Estimates

We interpret our results as a reduced form effect of the combination of the following three channels. The first is through health improvements, facilitated through increased public spending and health projects. The link between health and cognition is well-established in the literature, and we believe may be quite relevant here given the magnitude of improvements documented in Miller (2008). The role of this channel will depend on the role of selection relative to other health gains, as the two processes have opposing implications for education outcomes. Declines in mortality as a result of suffrage are likely to negatively select weak individuals, biasing our results against finding an effect. On the other hand, greater vitality among children triggers a channel for positive effects on achievement. Therefore, a positive effect on achievement could be generated through health improvements only if the gains among the non-selected children are greater than the effects of selection.

A second channel is through increases in educational expenditures following suffrage. While studies find ambiguous effects of spending on educational attainment in contemporary United States (see Hanushek (1986) for a review),[41] the impact of educational programs initiated during this period have been found to be more uniformly positive. The closely

[41] Jackson et al. (2015) is a recent exception.

timed Rosenwald initiative, for example, was found to improve education of black children by a similar magnitude to suffrage (Aaronson and Mazumder, 2011).

Third, suffrage may increase the bargaining power of women in the household, which may result in increased education if mothers prefer a higher level of human capital investment than fathers. By reducing a woman's reliance on her husband, voting power may have shifted her decisionmaking power within the household. This channel would only be relevant for women who were able to execute their voting power, and would be less plausible for disenfranchised black communities. Thus, while it may be a contributing factor to our estimates, it cannot be the *only* channel.

Although we cannot disentangle the three channels, we can gain additional information about the role of the education channel by using the measured "per dollar" effect of the Rosenwald program and scaling it by the increased educational expenditures following suffrage. Using data from Aaronson and Mazumder (2011), we perform a back-of-the-envelope calculation[42] and estimate the gains in education per dollar spent on building the Rosenwald schools to be around 0.08 years of education per child impacted. Carruthers and Wanamaker (2014) estimate a 26 log point increase in schooling expenditures per child-year following suffrage, the equivalent of an additional \$1.29 in per-pupil expenditures. Since our exposure to suffrage is measured over sixteen years, nine of which are during schooling ages, this amounts to a minimum of \$11.61 additional investment in each child. Using the returns from Rosenwald yields an additional 0.93 years in schooling.

Of course, a strong caveat should be placed on these estimates, as the programs from suffrage are not entirely comparable to the effects of the Rosenwald initiative. Namely, Rosenwald was primarily a capital-building initiative and there is no historical evidence suggesting that additional schools were built as a result of suffrage. However, this calculation does make clear the sizable increase in schooling expenditures alone following suffrage, and places our estimates within reasonable bounds of the expected outcomes following such an investment.

[42]The total cost of building 5,000 Rosenwald schools amounted to close to \$26M, and each school operated for around 20 years. The program increased the annual supply of teachers by 13,764, the equivalent of 619,300 children annually (at 45 children to a teacher). We calculate the amount spent on each child per year as $\frac{\text{Total Cost}}{\text{Number of Children Per Year} \times \text{Years of Operation}} = \frac{26{,}000{,}000}{619{,}300 \times 20} = 2.1$ dollars. Given that each child was exposed for 7 years, the total per child spending is equal to 14.7. We can then calculate the measured "per dollar" effect of the Rosenwald program as $\frac{\text{Educational Gains}}{\text{Total Cost Per Child}} = \frac{1.2}{14.7} = 0.08$ years per dollar.

7 Robustness

In this section, we conduct a variety of robustness exercises to address potential concerns and alternative explanations for our estimates.

7.1 Endogenous Passage of Laws

If the timing of passage of suffrage laws is correlated with pre-existing trends in education or with other state-level policies, then a naive specification could mis-attribute the trend or the effects of simultaneously evolving programs as measured effects of suffrage. We address this potential endogeneity in four ways. First, we have discussed evidence from prior work (Miller, 2008) that suffrage was not correlated with a host of progressive legislation which could have contributed to educational attainment during the period.[43] Second, we showed compelling visual illustration in our event study that the impacts on educational attainment are sharply increasing at age 15 and flat for individuals beyond the margin of schooling. Third, we find little evidence of coinciding variation in demographic or economic measures coincident with suffrage that may be an indicative of global changes occurring with the passing of suffrage (see Table 1).

Now, we estimate an additional specification where we add an interaction between the measure of suffrage exposure and whether the state adopted suffrage involuntarily. Finding positive effects for voluntary states only would be worrisome, as it would suggest that the results are biased by endogenous adoptions of suffrage laws. Instead, our event study results, shown in Figure 10, show the effects of suffrage are similar for the two groups of states. Moreover, Table 6 shows that suffrage had a statistically significant *larger* effect in involuntary states compared to voluntary states, which is the opposite of what we would expect if the timing of suffrage laws was endogenous. We do not place much emphasis on the magnitude of the difference, however, as we believe it is likely driven by the differing composition of the samples across the two sets of states.

7.2 Selective Migration

An additional concern is whether selective migration might be influencing our estimates. If later cohorts in a state who have higher levels of suffrage treatment are also more likely to migrate to areas with higher investments in education, there may be a correlation between migration decisions and suffrage which would introduce bias to our estimates. This is of

[43] In Table 11 we show that the laws are also not correlated with WWII mobilization rates, which are a source of educational gains during the period. Source: Acemoglu et al. (2004).

particular concern for our study due to the overlap between our period of observation with the Great Migration movement, during which over a million black individuals moved from the South to northern cities (Chay and Munshi, 2012). In our favor, prior studies suggest that there were only small, positive selection effects of the Great Migration (Collins and Wanamaker, 2014), limiting the scope for bias, but that does not preclude the possibility of other selective migration.

We can check for this type of bias by simply estimating our preferred specification separately by migration status of individuals. If migration is a source of positive bias, we would expect our results to be largest among individuals that migrated compared to those that did not. We check for this by stratifying our sample by "Movers", individuals observed in a different state from their state of birth, and "Non Movers", and display our results in Table 7. For blacks, we find that the point estimate is substantially the same as our previous estimate among non movers, and it is not statistically different from zero for movers. These estimates do not suggest a role for selective migration, but are reconcilable by measurement error. For some proportion of movers, we have likely mis-assigned treatment time by using suffrage laws enacted in the state of birth, which would result in the observed attenuation.

7.3 Alternative controls

Properly controlling for confounding variation across cohorts, such as secular changes in education, technology, and economic development, is of utmost importance to the analysis. At the same time, we do not want to control for endogenous changes, which would bias our estimates. In our main analysis, we strike a compromise by including proxies for these factors measured at birth. In Table 8, we test the sensitivity of our results to this decision. In Column (1) we replicate our main results, showing the results for the whole sample in Panel (A), the black sample in Panel (B), and the white sample in Panel (C). In Column (2), we include the same control variables, but now averaged between ages 0 to 15. This specification better reflects the environment that children experience during schooling, but potentially introduces endogenous controls if some of the environment was shaped by the passage of suffrage. The coefficients are similar across Columns (1) and (2), with a slight decline in the coefficient for the black subsample. In Column (3), instead, we interact the level of the control variables in 1900 with a linear trend (Hoynes et al., 2012). Here we run the risk of under-controlling for confounding variation. Again, the coefficients are steady. Overall, we are reassured that the estimates are not sensitive to the functional form of our controls.

7.4 World War II and the G.I. bill

It is difficult to discuss growth in educational outcomes in the early twentieth century without mention of World War II and the G.I. bill, each of which had a strong influence on the educational decisions of the cohorts coming of age during that era.[44] The G.I. Bill provided federal financial support for veterans returning from war and has been credited with increasing the college completion rate by up to 50% (Bound and Turner, 2002). Studies of the effects of the bill often take advantage of the variation in the proportion serving across cohorts, comparing the cohorts with high participation rates, born between 1921 and 1926, to nearby cohorts. Due to the overlap in the cohorts in our sample and the veterans impacted by the G.I. Bill, the effects of this policy pose a potential risk as a confounder, especially if there is additional variation in mobilization rates across states. However, given that our effects are entirely concentrated in primary and secondary schooling, subsidies to college would be an unlikely explanation. In addition, Turner and Bound (2003) find that for blacks living in the South, one of the groups that most benefited from suffrage, the GI bill has no effect, which adds to the inconsistency. Nonetheless, we check for any correlation between mobilization rates and the timing of suffrage in case there was an externality of participation in the war on primary education. We present the results in Table 11. Controlling for regional dummies, we do not find any significant relationship between the year of suffrage and the proportion serving in WW2 in the state. Based on these two discrepancies, we find no role for the G.I. bill in our estimates.

7.5 Additional checks

In Tables 9 and 10 we check the sensitivity of our results to utilizing a binary measure for exposure between ages 0 and 15 and to running our preferred specification separately by census year. As might be expected, using a dummy for exposure simply produces a weighted average of the effects in the event study. This turns out to be about 0.3, or one third the size of our previously estimated effect, due to the nonlinearity of the point estimates across ages. Finally, we find the results are generally unchanged across census samples, although there is attenuation in the 1940 census consistent with the measurement anomalies reported in previous studies (Goldin, 1998).

[44] Early cohorts in our sample born from 1880 to 1900 were also eligible to serve during the First World War. Since these cohorts are concentrated among our "control group", we can look for evidence of bias from the War in the form of pre-trend for the children too old to experience the benefits of suffrage. Our event studies show no evidence of this, however, indicating that any effect of the War is absorbed by our control variables.

8 Conclusion

This paper presents new evidence on the effects of women's political empowerment on investments in children's human capital. We find that exposure to the post-suffrage regime during childhood leads to substantial gains in educational attainment, concentrated amongst populations with low levels of education at baseline. In particular, full exposure to suffrage between age 0 and 15 leads to approximately one year of additional education for blacks, and for whites from the South, the least advantaged groups in the sample. Our effects are concentrated in primary schooling, which is the mean educational attainment of the affected groups. This suggests that the policies resulting from suffrage were effective at raising the attainment of students at the lower half of the education distribution. Using literacy as a proxy for skills attained, we also provide suggestive evidence that these gains in quantity of education translated into improved human capital. We do not find any indication that our results are driven by endogenous adoption of the laws, selective migration, or omitted policy variables.

We examine the channels by which suffrage induced improvements in education, attributing a large share of the impact to the sharp rise in public expenditures following enfranchisement. This finding is in line with the the impacts of a number of other transfers of economic and political resources to women, and highlights an important commonality between the two types of transfers. While suffrage did not represent an increase in the economic holdings of women, the greater ability to influence public spending indirectly had the equivalent effect. This result is also consistent with other interventions timed closely with suffrage, which find a strong role for public and philanthropic investments in improving educational outcomes. One limitation of this finding is that we are not able to disentangle the effects of the increase in health, social, and education expenditures, as well as accompanying legislation, that accumulated as a result of suffrage. We are able to rule out in-utero exposure and early life health experiences as a primary channel, but still leave open the possibility of early childhood health improvements as a contributor to lengthened stay in school.

This article quantifies the effects of political empowerment of women in the United States. However, parallels between these results and modern interventions suggest that the channels we highlight would translate to other settings. In particular, there is growing evidence that public expenditures in contemporary settings also produce the largest gains for children with the lowest baseline achievement. A recent paper, Jackson et al. (2015), suggests that education expenditures produce substantial effects on educational attainment for children from poor families and no effect on children from non-poor families. Similar patterns are emerging from studies of Food Stamps, WIC, Medicaid, and Head Start (Almond et al., 2011;

Hoynes et al., 2011; Currie and Gruber, 1996; Bitler et al., 2014). A less understood aspect of our findings, and in this literature in general, is the role of spillovers between children that do not exhibit gains in education themselves, but may have unobserved effects that lead to greater gains at the bottom of the distribution. The relative contribution of this channel will be important for the design of future policy.

On the whole, this article provides compelling evidence for the role of female voter preferences in influencing policy. As political power increasingly equates to economic holdings, a future promising avenue for research is to understand whether women's economic power can lead to similar gains. This research is already advancing in the developing world, and the results for children are promising (Qian, 2008). However, a large gap remains in the developed context, where questions remain whether advances in the relatively smaller gap between men and women would have any impact on educational outcomes. Nonetheless, this question is of great relevance today given the push for gender equality in the workplace, highlighted as a policy priority in the recent presidential State of the Union address (Obama, 2015). We leave it for future research to provide evidence in this area.

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9 Tables

Table 1: Estimated Changes in State Observables After Suffrage

	% White	% Female	% Urban	% Literate	Pop.	Farm Value	Man. Empl.	Man. Wages
A: Post								
Post Suffrage	0.06	-0.12	-0.14	-0.05	48.08	29.69**	0.14	-0.75
	(0.11)	(0.07)	(1.25)	(0.07)	(239.70)	(13.17)	(0.09)	(1.24)
Mean Y	89.0	48.9	31.4	92.4	4209.5	96.7	3.5	24.0
B: Post and Change in Trend								
Post Suffrage	0.07	-0.10	0.72	-0.06	-173.31	32.60**	0.15	-0.77
	(0.10)	(0.07)	(1.28)	(0.08)	(208.48)	(16.04)	(0.09)	(1.23)
Post Suffrage Trend	0.01	0.04*	1.46	-0.01	-376.13	5.43	0.02	-0.03
	(0.05)	(0.02)	(0.87)	(0.03)	(247.42)	(6.05)	(0.05)	(0.29)
Mean Y	89.0	48.9	31.4	92.4	4209.5	96.7	3.5	24.0
Observations	1364	1364	1364	1364	1364	1364	1364	1364

Notes: This table contains results obtained when the dependent variable is the state-level observable list in the column header, and the main independent variable is either an indicator for post-suffrage (Panel A), or and indicator for post-suffrage and its interaction with a linear time trend (Panel B). Farm value and wages are calculated in 100 thousands. All monetary values are adjusted to 1982-84 dollars. All regressions include state and year fixed effects, state linear time trends, as well as region-by-year fixed effects. Estimates are weighted using population weights, and standard errors are clustered at the state level. The sample excludes states that passed suffrage prior to 1900. Sources: Lleras-Muney (2002) and the ICPSR series "Historical, Demographic, Economic, and Social Data: The United States" (2896). * p<0.10, ** p<0.05, *** p<0.01.

Table 2: Sample Demographic and State Characteristics

	All		Northeast		Midwest		South		West	
	B	W	B	W	B	W	B	W	B	W
Individual Demographics										
Years of Education	6.76	9.96	9.41	10.30	9.41	10.24	6.45	9.12	10.38	11.02
	(3.80)	(3.31)	(3.23)	(3.08)	(3.33)	(3.06)	(3.73)	(3.66)	(3.55)	(3.21)
Age	42.14	43.03	40.16	42.53	41.18	43.99	42.32	42.76	38.32	40.32
	(13.41)	(13.71)	(12.54)	(13.47)	(13.16)	(13.98)	(13.46)	(13.64)	(11.38)	(12.91)
Female	0.53	0.51	0.52	0.51	0.51	0.51	0.53	0.51	0.53	0.50
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Percent 0-15 Treated	0.45	0.47	0.59	0.47	0.57	0.46	0.43	0.45	0.79	0.72
	(0.42)	(0.43)	(0.42)	(0.42)	(0.44)	(0.43)	(0.41)	(0.42)	(0.35)	(0.39)
State-level Controls At Birth										
Percent Urban	0.12	0.19	0.39	0.28	0.27	0.17	0.09	0.10	0.35	0.26
	(0.18)	(0.28)	(0.38)	(0.37)	(0.30)	(0.26)	(0.14)	(0.15)	(0.32)	(0.30)
Percent Literate	0.78	0.91	0.95	0.94	0.96	0.96	0.76	0.81	0.95	0.94
	(0.12)	(0.09)	(0.01)	(0.01)	(0.02)	(0.02)	(0.11)	(0.11)	(0.05)	(0.07)
Value of farm per acre	44.38	83.26	67.77	63.57	140.36	128.98	37.17	47.38	109.97	73.25
	(54.54)	(82.52)	(42.62)	(45.23)	(92.74)	(100.03)	(45.14)	(57.58)	(87.64)	(76.37)
Annual Manufacturing Wages	6.30	19.71	51.58	44.69	21.64	15.54	2.97	3.13	10.54	7.27
	(13.24)	(23.72)	(26.15)	(26.07)	(18.61)	(15.80)	(2.06)	(2.16)	(7.88)	(7.10)
Observations	157028	1393855	7381	397080	8128	509551	140982	421211	537	66013

Notes: "B" is an indication for the black subsample; "W" is an indication for the white subsample. Standard deviations are shown in parentheses. Percent 0-15 treated is defined is defined as the share of time between birth and age 15 that an individual was exposed to a suffrage law in his state of birth. Farm value and wages are calculated in 100 thousands. All monetary values are adjusted to 1982-84 dollars. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses.

Table 3: Baseline Estimates of the Effect of Suffrage on Years of Education

				Whites		Blacks	
	All	Whites	Blacks	Males	Females	Males	Females
Suff Share 0-15	0.100	0.052	1.123***	0.034	0.066	1.346**	0.881***
	(0.201)	(0.193)	(0.274)	(0.177)	(0.220)	(0.603)	(0.215)
Mean Education	9.634	9.958	6.759	9.840	10.072	6.351	7.126
R-Squared	0.197	0.126	0.219	0.136	0.117	0.212	0.215
Observations	1555475	1393855	157028	688363	705492	74351	82677

Notes: This table contains results obtained when the dependent variable is years of education and the main independent variable is suffrage exposure, which is defined as the share of time between birth and age 15 that an individual was exposed to a suffrage law in his state of birth. All regressions include controls for demographics and state-level characteristics, birth state and birth year fixed effects, birth state linear time trends, as well as region-by-birth year and census year-by-birth year fixed effects. Estimates are weighted using Census sample weights, and standard errors are clustered on the state of birth. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses. * p<0.10, ** p<0.05, *** p<0.01.

Table 4: Effect of Suffrage on Years of Education - Differential Effects Across Different Ages of Exposure

	All	Whites	Blacks
Treated between 11-15	0.009	0.007	0.086***
	(0.012)	(0.012)	(0.030)
Treated between 6-10	0.007	0.001	0.112***
	(0.017)	(0.017)	(0.037)
Treated by age 5	0.004	0.003	0.024
	(0.011)	(0.010)	(0.019)
Mean Education	9.634	9.958	6.759
R-Squared	0.197	0.126	0.219
Observations	1555475	1393855	157028

Notes: This table contains results obtained when the dependent variable is years of education and the main independent variables are "Treated between x-y", which are defined as the share of time between ages x and y that an individual was exposed to a suffrage law in his state of birth. All regressions include controls for demographics and state-level characteristics, birth state and birth year fixed effects, birth state linear time trends, as well as region-by-birth year and census year-by-birth year fixed effects. Estimates are weighted using Census sample weights, and standard errors are clustered on the state of birth. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses. * p<0.10, ** p<0.05, *** p<0.01.

Table 5: Effect of Suffrage on Years of Education - Interaction with Pre-treatment Education Levels

		Whites		Blacks	
	All	Males	Females	Males	Females
Suff Share 0-15	1.234**	2.400***	3.032***	3.079**	3.506***
	(0.568)	(0.742)	(0.595)	(1.474)	(1.160)
Suff Share 0-15 x Pre-Period Education	-0.128**	-0.269***	-0.323***	-0.281	-0.394**
	(0.058)	(0.084)	(0.059)	(0.189)	(0.172)
Mean Education	9.634	9.840	10.072	6.351	7.125
R-Squared	0.197	0.136	0.117	0.212	0.215
Observations	1555424	688363	705492	74346	82655

Notes: This table contains results obtained when the dependent variable is years of education and the main independent variable is suffrage exposure, which is defined as the share of time between birth and age 15 that an individual was exposed to a suffrage law in his state of birth. Moreover, we include interactions between suffrage exposure and pre-treatment education levels, calculated as the average education in demographic cells defined by gender, race, and state. All regressions include controls for demographics and state-level characteristics, birth state and birth year fixed effects, birth state linear time trends, as well as region-by-birth year and census year-by-birth year fixed effects. Estimates are weighted using Census sample weights, and standard errors are clustered on the state of birth. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses. * p<0.10, ** p<0.05, *** p<0.01.

Table 6: Effect of Suffrage on Years of Education - Mandatory vs Not Mandatory States

	All	Whites	Blacks
Suff Share 0-15	0.064	0.030	0.953***
	(0.192)	(0.188)	(0.305)
Suff Share 0-15 x Mandatory States	0.288***	0.198**	0.491**
	(0.091)	(0.093)	(0.234)
Mean Education	9.634	9.958	6.759
R-Squared	0.197	0.126	0.219
Observations	1555475	1393855	157028

Notes: This table contains results obtained when the dependent variable is years of education and the main independent variable is suffrage exposure, which is defined as the share of time between birth and age 15 that an individual was exposed to a suffrage law in his state of birth. Suffrage exposure is interacted with indicators for "mandatory" and voluntary states, where "mandatory states" are the state that did not pass suffrage prior to the Nineteenth Amendment nor voluntarily ratified it. All regressions include controls for demographics and state-level characteristics, birth state and birth year fixed effects, birth state linear time trends, as well as region-by-birth year and census year-by-birth year fixed effects. Age at treatment 30 and older is the omitted category so estimates are relative to that point. Estimates are weighted using Census sample weights, and standard errors are clustered on the state of birth. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses. * p<0.10, ** p<0.05, *** p<0.01.

Table 7: Effect of Suffrage on Years of Education - Checks for Migration

	Whites			Blacks		
	All	Non Movers	Movers	All	Non Movers	Movers
Suff Share 0-15	0.052	0.003	0.137	1.123***	1.663***	0.520
	(0.193)	(0.228)	(0.129)	(0.274)	(0.382)	(0.457)
Mean Education	9.958	9.724	10.445	6.759	6.257	7.454
R-Squared	0.126	0.140	0.120	0.219	0.259	0.198
Observations	1393855	949891	443964	157028	92760	64268

Notes: This table contains results obtained when the dependent variable is years of education and the main independent variable is suffrage exposure, which is defined as the share of time between birth and age 15 that an individual was exposed to a suffrage law in his state of birth. All regressions include controls for demographics and state-level characteristics, birth state and birth year fixed effects, birth state linear time trends, as well as region-by-birth year and census year-by-birth year fixed effects. Estimates are weighted using Census sample weights, and standard errors are clustered on the state of birth. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses. * p<0.10, ** p<0.05, *** p<0.01.

Table 8: Effect of Suffrage on Years of Education - Sensitivity to Alternative Controls

	At Birth	Cumulative 0-15	Pre*Birthyear
A: All			
Suff Share 0-15	0.100	0.207	0.035
	(0.201)	(0.130)	(0.250)
Mean Education	9.634	9.634	9.634
R-Squared	0.197	0.197	0.197
Observations	1555475	1555475	1555475
B: Blacks			
Suff Share 0-15	1.123***	0.882***	1.191***
	(0.274)	(0.228)	(0.323)
Mean Education	6.759	6.759	6.759
R-Squared	0.219	0.219	0.219
Observations	157028	157028	157028
C: Whites			
Suff Share 0-15	0.052	0.156	-0.023
	(0.193)	(0.136)	(0.233)
Mean Education	9.958	9.958	9.958
R-Squared	0.126	0.126	0.126
Observations	1393855	1393855	1393855

Notes: This table contains results obtained when the dependent variable is years of education and the main independent variable is suffrage exposure, which is defined as the share of time between birth and age 15 that an individual was exposed to a suffrage law in his state of birth. All regressions include controls for demographics and state-level characteristics, birth state and birth year fixed effects, birth state linear time trends, as well as region-by-birth year and census year-by-birth year fixed effects. Estimates are weighted using Census sample weights, and standard errors are clustered on the state of birth. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses. * p<0.10, ** p<0.05, *** p<0.01.

Table 9: Effect of Suffrage on Years of Education - Sensitivity to Measure of Exposure

	All	Whites	Blacks
Suffrage by 15	0.025	0.015	0.326***
	(0.021)	(0.022)	(0.061)
Mean Education	9.634	9.958	6.759
R-Squared	0.197	0.126	0.219
Observations	1555475	1393855	157028

Notes: This table contains results obtained when the dependent variable is years of education and the main independent variable is suffrage exposure, which is equal to one if an individual is exposed to suffrage in his state of birth at age 15 or younger. All regressions include controls for demographics and state-level characteristics, birth state and birth year fixed effects, birth state linear time trends, as well as region-by-birth year and census year-by-birth year fixed effects. Estimates are weighted using Census sample weights, and standard errors are clustered on the state of birth The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses. * p<0.10, ** p<0.05, *** p<0.01.

Table 10: Effect of Suffrage on Years of Education - Sensitivity to Census

	1940	1950	1960	1950, 1940 Pop	1960, 1940 Pop
<u>*A: Blacks*</u>					
Suff Share 0-15	0.094	1.805***	1.528***	3.235**	1.135**
	(0.270)	(0.521)	(0.473)	(1.460)	(0.436)
Mean Education	6.009	7.000	7.272	6.417	6.502
R-Squared	0.155	0.224	0.227	0.192	0.176
Observations	61004	22447	73577	15839	50924
<u>*B: Whites*</u>					
Suff Share 0-15	0.106	0.336	-0.069	0.372	-0.037
	(0.172)	(0.220)	(0.210)	(0.224)	(0.186)
Mean Education	9.567	10.164	10.173	9.755	9.735
R-Squared	0.096	0.126	0.134	0.097	0.110
Observations	509583	204510	679762	148663	483804

Notes: This table contains results obtained when the dependent variable is years of education and the main independent variable is suffrage exposure, which is defined as the share of time between birth and age 15 that an individual was exposed to a suffrage law in his state of birth. All regressions include controls for demographics and state-level characteristics, birth state and birth year fixed effects, birth state linear time trends, as well as region-by-birth year and census year-by-birth year fixed effects. Estimates are weighted using Census sample weights, and standard errors are clustered on the state of birth. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses. * p<0.10, ** p<0.05, *** p<0.01.

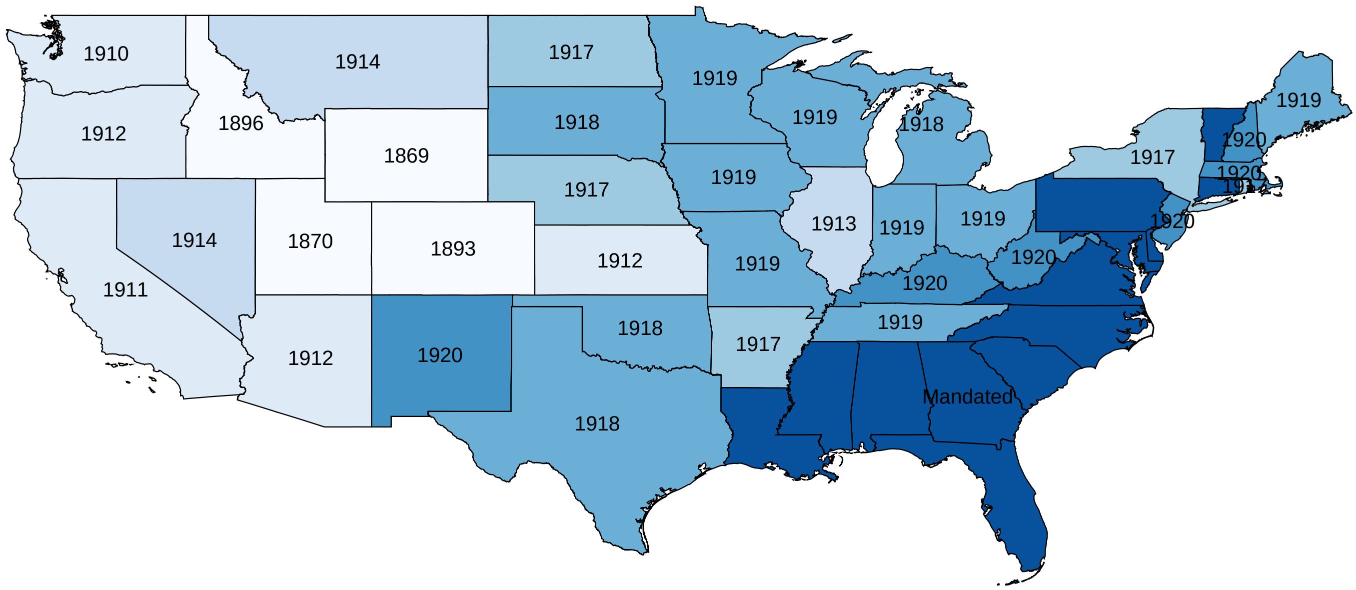
Table 11: Correlation between Timing of Suffrage and WWII Mobilization Rates

	(1)	(2)
WW2 Mobilization Rate	-105.914**	-46.278
	(44.630)	(58.686)
R-Squared	0.109	0.435
Observations	48	48
RegionxBY FE	No	Yes

Notes: This table contains results obtained when the dependent variable is the year of suffrage approved in each state and the main independent variable is WWII mobilization rates. All regressions include state and year fixed effects, state linear time trends, as well as region-by-year fixed effects. All regressions include population weights, and standard errors are clustered at the state level. The sample excludes states that passed suffrage prior to 1900. Suffrage laws are from Lott et al. (1999) and Miller (2008). WWII mobilization rates are from Acemoglu et al. (2004). * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

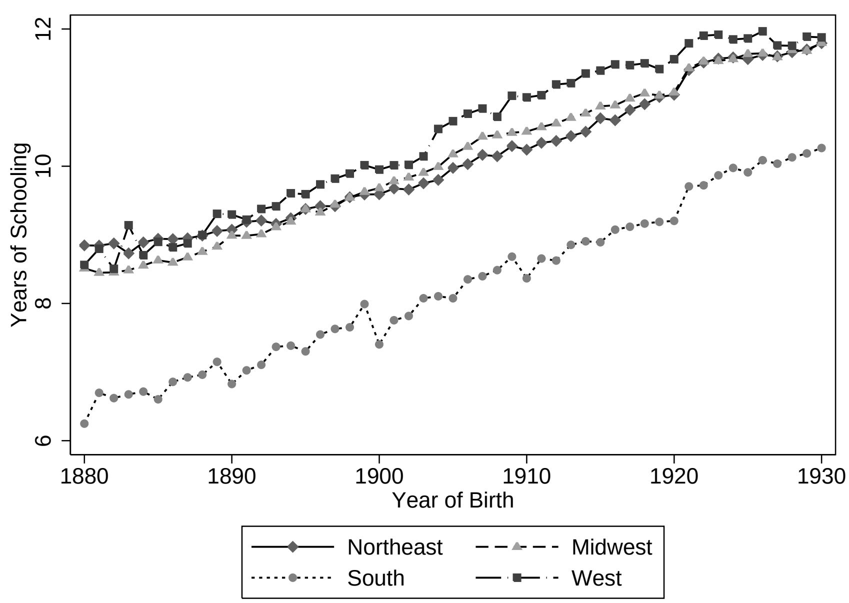
10 Figures

Figure 1: Timing of Suffrage Laws



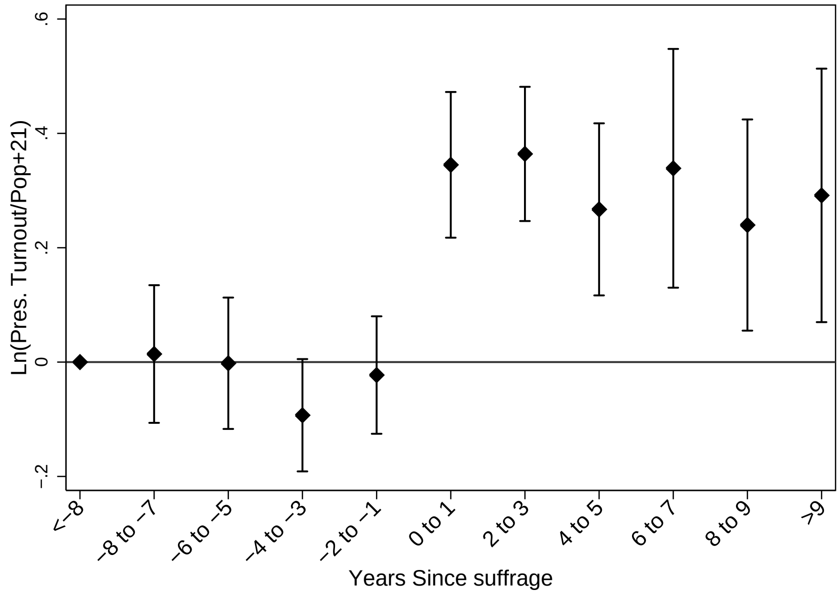
Notes: Suffrage laws are from Lott et al. (1999) and Miller (2008). Years are for the first suffrage law in the state. "Mandatory states" implemented suffrage as a result of the Nineteenth Amendment, in 1920. See text for details.

Figure 2: Average Educational Attainment Across Cohorts and Regions



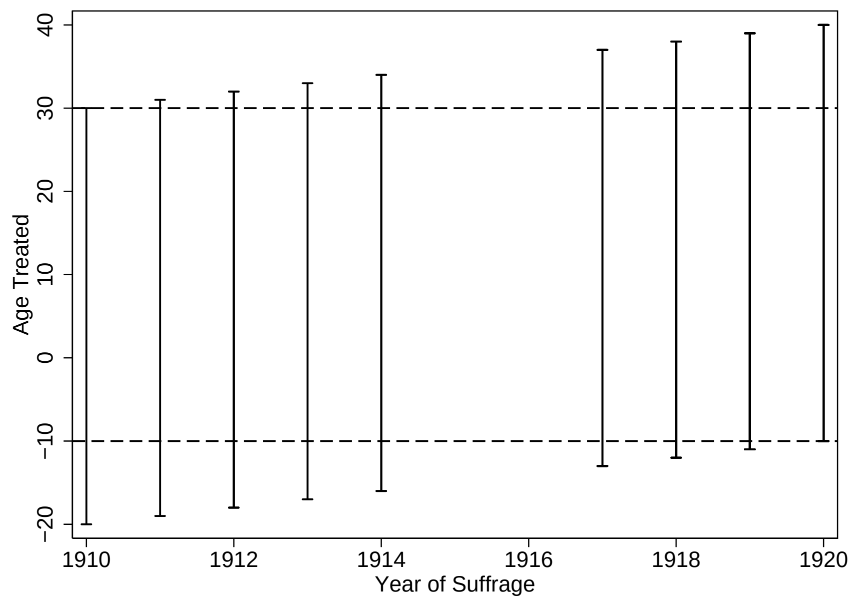
Notes: This figure plots the (weighted) average number of years of completed schooling for U.S. born residents by birth cohort and region. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses.

Figure 3: Effect of Suffrage on Presidential Turnout



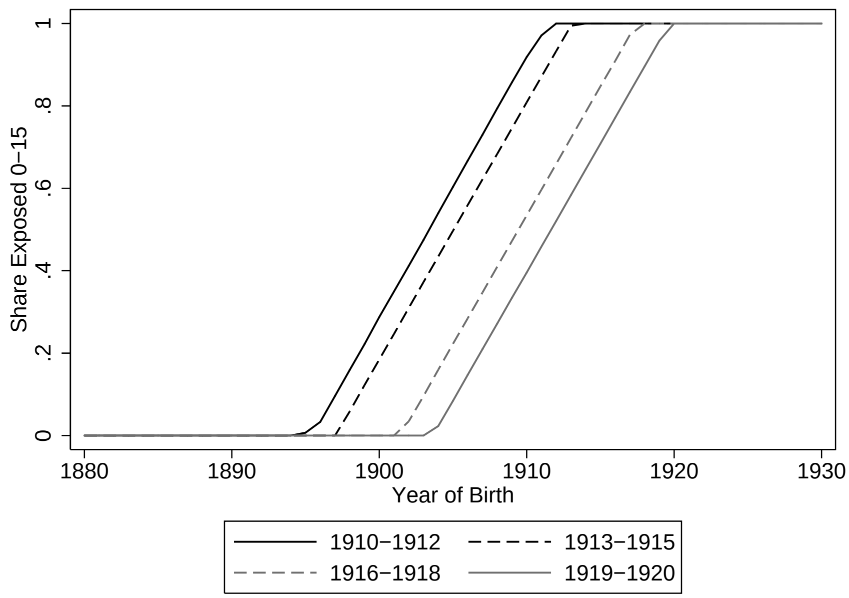
Notes: This figure plots the estimated coefficients obtained from an event study specification that analyzes the effect of suffrage on state-level presidential turnout, defined as the natural logarithm of total number of votes at the presidential elections divided by the voting eligible age, 21+. The specification includes state and year fixed effects, state linear time trends, region-by-year fixed effects, and state controls such as percentage white, percentage female, percentage urban, percentage literate, population, farm value, employment, and wages. Years since suffrage -8 and earlier is the omitted category so estimates are relative to that point. Estimates are weighted using population weights, and standard errors are clustered at the state level. The sample excludes states that passed suffrage prior to 1900. Sources: Turnout: "Electoral Data for Counties in the United States: Presidential and Congressional Races, 1840-1972" (ICPSR 8611); Population: 1900-1930 censuses.

Figure 4: Variation in Age of First Exposure to Suffrage Among the 1880-1930 Cohorts, by Suffrage Year



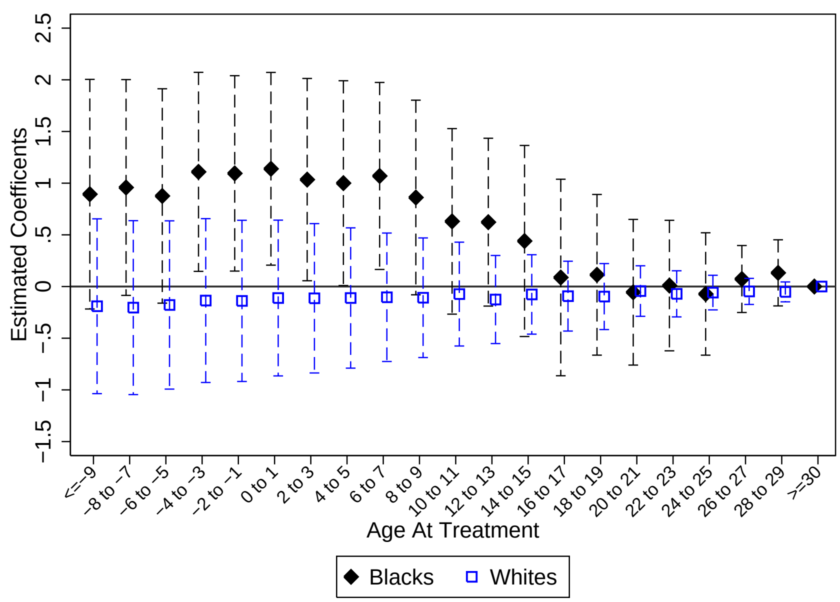
Notes: This figure presents the amount of variation in age of first treatment in the sample by the year in which suffrage was passed in the state. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses.

Figure 5: Variation in Exposure to Suffrage, by Cohort



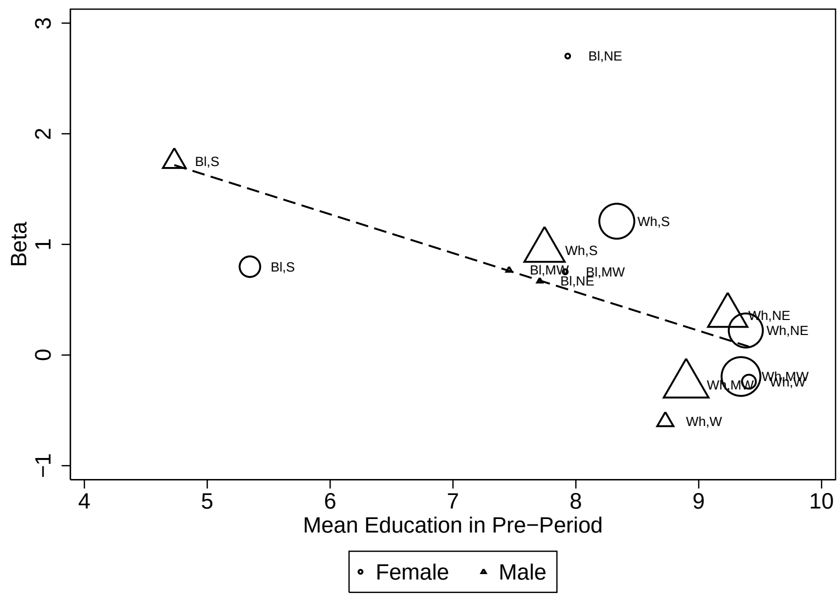
Notes: This figure presents the amount of variation in suffrage exposure in the sample, by cohort. We plot this variation separately for four groups, which enacted suffrage laws between the period 1910-1912, 1913-1915, 1916-1918, and 1919-1920 respectively. "Percent Exposed 0-15" is defined as the share of time between birth and age 15 that an individual was exposed to a suffrage law in his state of birth. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses.

Figure 6: Effect of Suffrage at Each Age of First Exposure on Years of Education, By Race



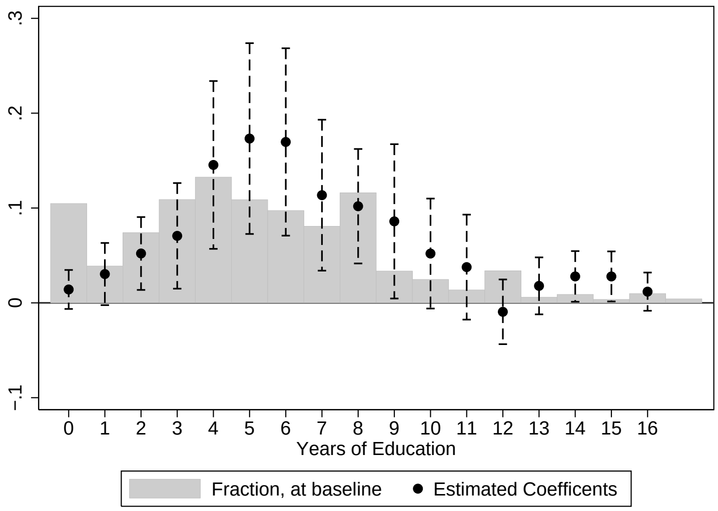
Notes: This figure plots the estimated coefficients (and 95% confidence intervals) obtained from event study specifications that analyze the effect of suffrage at each age of first exposure on educational attainment, estimated separately for whites and blacks. All specifications include controls for demographics and state-level characteristics, birth state and birth year fixed effects, birth state linear time trends, as well as region-by-birth year and census year-by-birth year fixed effects. Age at treatment $\geq$ 30 is the omitted category so estimates are relative to that point. Estimates are weighted using Census sample weights, and standard errors are clustered on the state of birth. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses.

Figure 7: Subgroup Averages of Pre-Treatment Education and the Estimated Effects of Suffrage on Years of Education

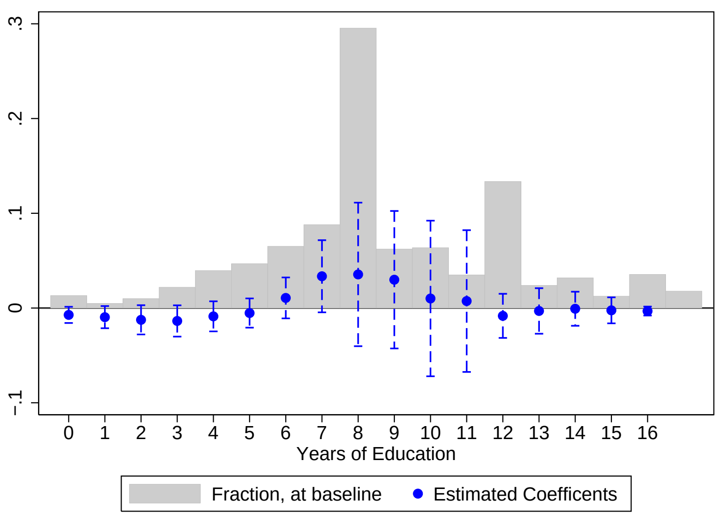


Notes: To create this figure, we first estimate specifications that analyze the effect of suffrage exposure on educational attainment separately for demographic groups defined according to region of birth, race and gender. We then plot the estimated coefficients along with the average pre-treatment educational attainment (average attainment among individuals that were age 16 or older by the passage of suffrage in the state) for each demographic group. Regions are abbreviated as follows: "S" for South, "W" for West, "MW" for Midwest, and "NE" for Northeast, and race is abbreviated as: "Bl" for black and "Wh" for white. We do not show blacks in the West due to their small sample size, but an equivalent figure that includes all groups is available on request. All regressions include controls for demographics and state-level characteristics, birth state and birth year fixed effects, birth state linear time trends, as well as region-by-birth year and census year-by-birth year fixed effects. Estimates are weighted using Census sample weights, and standard errors are clustered on the state of birth. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses.

Figure 8: Effect of Suffrage on the Distribution of Years of Education, By Race



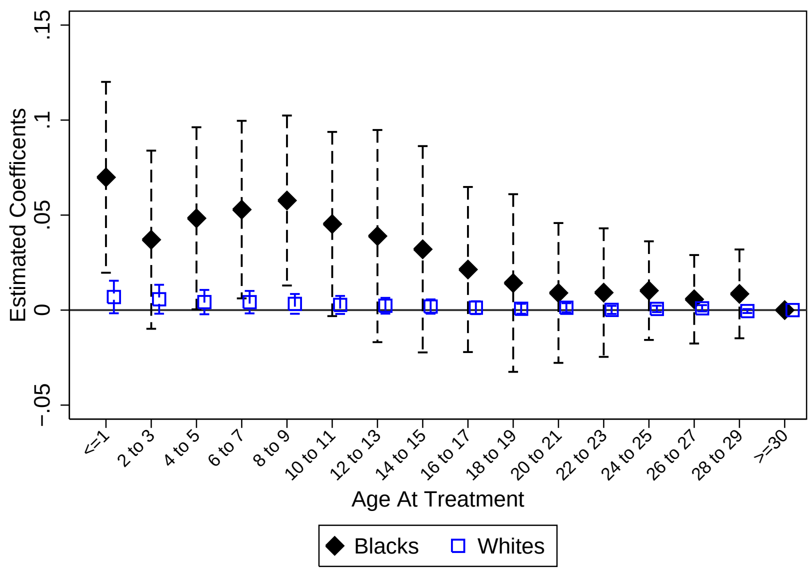
(a) Blacks



(b) Whites

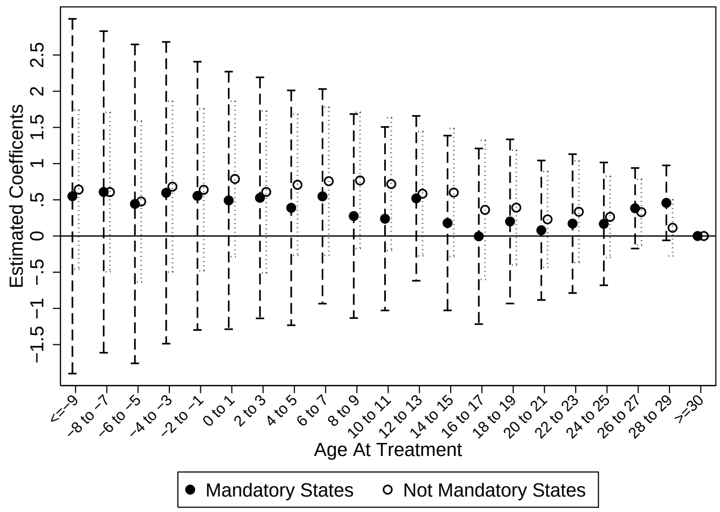
Notes: These figures plot the estimated coefficients (and 95% confidence intervals) obtained from specifications that analyze the effect of suffrage exposure on the likelihood that an individual completes x or greater years of education (1-CDF), where x is represented on the x-axis. All specifications are estimated separately for white and blacks, and they include controls for demographics and state-level characteristics, birth state and birth year fixed effects, birth state linear time trends, as well as region-by-birth year and census year-by-birth year fixed effects. Estimates are weighted using Census sample weights, and standard errors are clustered on the state of birth. The graph also contains a histogram for the share of the "untreated" population - for whom the share of time between birth and age 15 that an individual was exposed to a suffrage law in his state of birth is zero - that has each discrete level of education. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses.

Figure 9: Effect of Suffrage at Each Age of First Exposure on Literacy, By Race

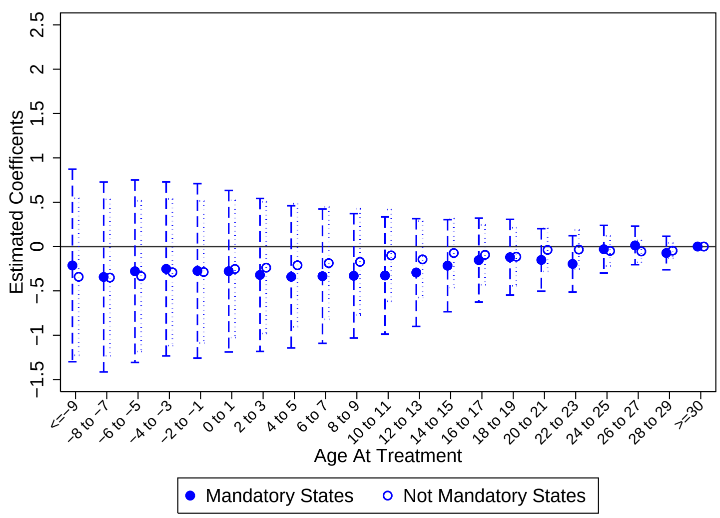


Notes: This figure plots the estimated coefficients (and 95% confidence intervals) obtained from event study specifications that analyze the effect of suffrage at each age of first exposure on literacy attainment, separately for whites and blacks. All specifications include controls for demographics and state-level characteristics, birth state and birth year fixed effects, birth state linear time trends, as well as region-by-birth year and census year-by-birth year fixed effects. Age at treatment ≥ 30 is the omitted category so estimates are relative to that point. Estimates are weighted using Census sample weights, and standard errors are clustered on the state of birth. The sample consists of individuals born between 1880 and 1915, and that are at least 15 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1920-1930 decennial censuses.

Figure 10: Effect of Suffrage at Each Age of First Exposure on Years of Education, Mandatory vs Not Mandatory States



(a) Blacks



(b) Whites

Note: This figure plots the estimated coefficients (and 95% confidence intervals) obtained from event study specifications that analyze differential effects of suffrage across mandatory and voluntary states at each age of first exposure on educational attainment, separately for whites and blacks. "Mandatory states" are the state that did not pass suffrage prior to the Nineteenth Amendment nor voluntarily ratified it; all others adopted the laws voluntarily. All specifications include controls for demographics and state-level characteristics, birth state and birth year fixed effects, birth state linear time trends, as well as region-by-birth year and census year-by-birth year fixed effects. Estimates are weighted using Census sample weights, and standard errors are clustered on the state of birth. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses.

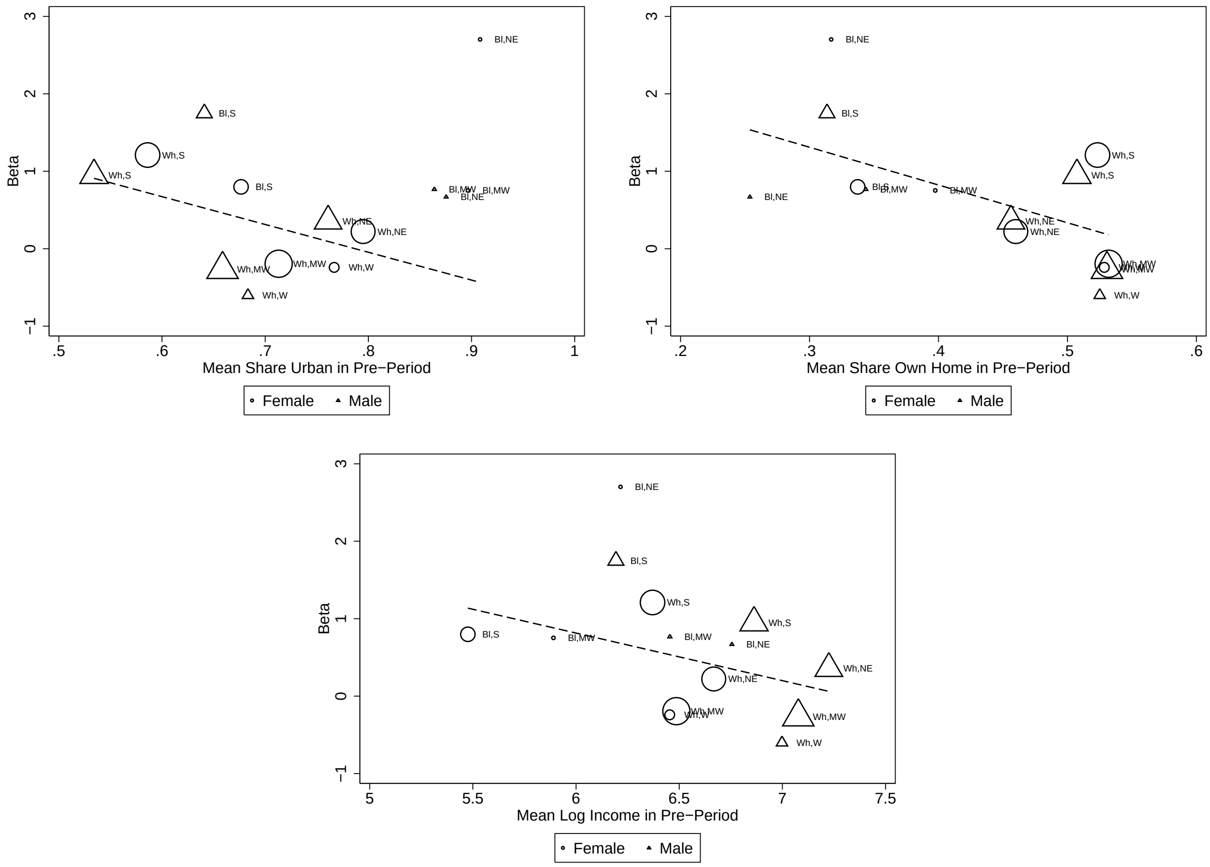
A Appendix: Further Results

Table A.1: Effect of Suffrage on Years of Education - Effects Beyond Age 15

	All	Whites	Blacks
Suff Share 0-15	0.091	0.035	1.183***
	(0.251)	(0.240)	(0.286)
Suff Share 16-22	0.018	0.001	0.278
	(0.086)	(0.082)	(0.314)
Suff Share 23-30	-0.040	-0.044	-0.220
	(0.109)	(0.107)	(0.446)
Mean Education	9.634	9.958	6.759
R-Squared	0.197	0.126	0.219
Observations	1555475	1393855	157028

Notes: This table contains results obtained when the dependent variable is years of education and the main independent variables are "Suff Share x-y", which are defined as the share of time between ages x and y that an individual was exposed to a suffrage law in his state of birth. All regressions include controls for demographics and state-level characteristics, birth state and birth year fixed effects, birth state linear time trends, as well as region-by-birth year and census year-by-birth year fixed effects. The state controls include percentage white, percentage female, percentage urban, percentage literate, population, farm value, employment, and wages, as well as controls for compulsory schooling laws and the Rosenwald school initiative. All regressions include sample weights, and standard errors are clustered at the state level. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses. * p<0.10, ** p<0.05, *** p<0.01.

Figure A.1: Subgroup Averages of Pre-Treatment Disadvantage and the Estimated Effects of Suffrage on Years of Education



Notes: To create these figures, we first estimate specifications that analyze the effect of suffrage exposure on educational attainment separately for demographic groups defined according to region of birth, race and gender. We then plot the estimated coefficients along with the three different average pre-treatment measure of disadvantage for each demographic group. Regions are abbreviated as follows: "S" for South, "W" for West, "MW" for Midwest, and "NE" for Northeast, and race is abbreviated as: "Bl" for black and "Wh" for white.. We do not show blacks in the West due to their small sample size, but an equivalent figure that includes all groups is available on request. All regressions include controls for demographics and state-level characteristics, birth state and birth year fixed effects, birth state linear time trends, as well as region-by-birth year and census year-by-birth year fixed effects. Estimates are weighted using Census sample weights, and standard errors are clustered on the state of birth. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses.