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WHO BENEFITED FROM WOMEN'S SUFFRAGE?

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ABSTRACT

While a growing literature has shown that women prefer investments in child welfare and increased redistribution, little is known about the long-term effect of empowering women. Exploiting plausibly exogenous variation in U.S. suffrage laws, we show that children from economically disadvantaged backgrounds who were exposed to women's political empowerment during childhood experienced large increases in educational attainment, especially blacks and Southern whites. We also find improvements in earnings among whites and blacks that experienced educational gains. We employ newly digitized data to map these long-term effects to contemporaneous increases in local education spending and childhood health, showing that educational gains were linked to improvements in the policy environment.

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1 Introduction

Gender gaps in preferences are prevalent across many domains. Two robustly documented differences are in the priority placed on child welfare and in the tendency toward redistribution. Women favor higher levels of investment in children, are more pro-social, and are more egalitarian (Duflo, 2003; Lundberg et al., 1997; Andreoni and Vesterlund, 2001; Croson and Gneezy, 2009; Alesina and Giuliano, 2011; Ashok et al., 2015). Thus, it has been argued that female empowerment could be a vehicle for economic development by making human capital investments a priority (Duflo, 2012). Yet, there is surprisingly little evidence connecting changes in women’s influence to policy or to economic outcomes, and whether this reduced disparities. In this paper, we quantify the impact of a substantial increase in women’s political power in the U.S., given by the passage of suffrage laws, on the education and labor market productivity of the next generation.

The U.S. suffrage laws that we study, the majority of which were enacted between 1910 and 1920, have been hailed as a “turning point in our Nation’s history” (Obama, 2010). Previous work on this topic has established that newly empowered women exercised their vote in large numbers, as demonstrated by a 40% increase in voting among the adult population in the years following women’s enfranchisement (Lott and Kenny, 1999). Lawmakers increasingly voted for liberal legislation and sharply expanded health and social spending by 36% and 24%, respectively (Lott and Kenny, 1999; Miller, 2008). Schooling expenditures per pupil in the South also rose by 29% (Carruthers and Wanamaker, 2014). However, despite the rhetorical emphasis on women’s preferences for education, to date, there has not been a national accounting of the effects of suffrage on total schooling expenditures and there is no evidence of the effect of suffrage on long-term human capital gains.

Theoretically, it is ambiguous whether women’s suffrage would lead to long term gains in education, and for whom. Educational gains depend on the effects of suffrage on public spending on human capital inputs, as well as the elasticity of schooling to spending. With respect to spending, we could expect *uniform* growth in spending if women viewed spending as too low everywhere, *higher* growth in less-educated, more-racially-diverse areas if women were averse to inequality, or *lower* growth in these areas if women with greater political or economic influence (e.g. more educated whites) used suffrage to capture resources for their own children. Educational gains from these changes in spending are also quite uncertain, as some evidence suggests that the returns to spending are largest for those with few resources (Jackson et al., 2016; Lafortune et al., 2018; Carruthers and Wanamaker, 2013).

Our analysis exploits variation in the timing of laws across states, as in Lott and Kenny (1999) and Miller (2008) and differential exposure to the laws across cohorts. Thus, for each

state, we compare individuals who were not of schooling age at the time of the passage of the law in one's state of birth (comparison children) to individuals that were of schooling age or not yet born when suffrage was enacted (treated children). The analysis relies on the assumption that childhood exposure to suffrage laws only affects education and labor outcomes through *suffrage-induced* changes in human capital inputs. In support of this, we show that passage of suffrage is not correlated with trends prior to suffrage in a large number of state demographics, a summary index of these covariates, or with education spending. Moreover, our event studies provide direct evidence that suffrage is not correlated with trends in our outcomes of interest. We also show that that passage of suffrage is not significantly correlated with other progressive laws and that these laws cannot predict our estimated effects on education.

A significant advantage of our work is the depth of historical data sources we bring to bear on this topic. We estimate effects on long-run education and labor market participation using the 1940, 1950 and 1960 IPUMS decennial censuses. To examine mechanisms for these results, we leverage two newly-digitized data sets. We digitized records of city-level school enrollment and expenditures to gain insight into the impact of suffrage on local schooling investments and schooling responses contemporaneous with suffrage. To our knowledge, this data has not been used previously for any study of this period. We also digitized state-by-race mortality records to allow us to examine heterogeneity in the effects of suffrage on infant mortality, extending the findings of Miller (2008).

We find that women's political empowerment was influential for educational attainment. We show that suffrage led to large gains for children from economically disadvantaged backgrounds, which we proxy for with historical levels of education by state and race. Full exposure to suffrage between the ages of 0 and 15 leads to an additional year of education for black children, who had an average of 5.2 years of education prior to suffrage, as well as for white children from the South, who had 8.0 years of education prior to suffrage. The effect of suffrage is smaller for more advantaged children, and is concentrated at primary-level education.

We follow up on these results by examining the effects of suffrage on labor market outcomes. We find that suffrage increased income alongside education gains. These effects are particularly stark for those that experienced large increases in education, such as Southern whites. We view this as suggestive evidence that suffrage led to improved labor market outcomes through human capital improvements.

We conclude by mapping these long-term effects to the contemporaneous impacts of these laws on education spending and childhood health. We find that suffrage increased local education expenditures by 9% on average, and show suggestive evidence that this

growth was higher in cities with a higher share black, in the South, and with lower pre-suffrage per-capita spending and education, where we find the largest gains in educational attainment. Further, we find a simultaneous rise in school enrollment in these cities. We also show similar heterogeneity in the impacts of suffrage on infant mortality. The totality of the results indicate that woman’s voting was particularly transformative for children from more racially-diverse, less-educated cities.

Returning to our hypotheses, these patterns indicate that suffrage generated widespread growth in education spending, and helped reduce disparities in completed education. We find no evidence that women exercised their newfound political power to divert funds to those with the greatest political or economic influence. We show that schooling gains are inversely related to pre-suffrage human capital, which could reflect diminishing returns to spending or the higher growth in spending in more disadvantaged areas.

Our paper joins together several literatures. First, we extend the existing knowledge of the effects of women’s political representation on children’s well-being. The best evidence for this comes from studies of the election of women to public office, and finds greater investment in public goods preferred by women and increased primary educational attainment (Chattopadhyay and Duflo, 2004; Clots-Figueras, 2012).¹ However, the policies enacted by women elected to political office may not be representative of women’s preferences more generally, since the type of women that run for office may have a distinct set of policy objectives, and while in office political considerations may also influence policy positions. Studies of the broad enfranchisement of women, on the other hand, have not linked women’s voting to children’s outcomes beyond childhood mortality (see earlier cited papers on U.S. suffrage and Aidt and Dallal (2008)). Further, while these papers show increases in *aggregate* spending, they do not address questions of how spending is distributed. Our findings contribute the first long-term estimates of a large-scale expansion of women’s political power as well as the first evidence that women’s empowerment had larger impacts for less-advantaged areas.

Second, we contribute to the growing literature in economics that examines preferences for redistribution across men and women (Andreoni and Vesterlund, 2001; Croson and Gneezy, 2009; Alesina and La Ferrara, 2005; Alesina and Giuliano, 2011). A major limitation of this literature is that the studies either rely on cross-sectional survey responses or on experimental dictator-type games. Extrapolating from these studies to policy predictions relies on strong assumptions of internal and external validity. One example of a phenomenon that may reduce their external validity comes from a disparate set of studies which show that preferences for redistribution may be attenuated when beneficiaries are from a different race (Luttmer,

¹While this pattern holds in many cases, female representatives do not always alter spending patterns; see e.g. Ferreira and Gyourko (2014).

2001; Fong and Luttmer, 2011; Alesina et al., 1999). Our results provide causal evidence of the impact of women’s empowerment on public educational investments and show that the growth in education spending was not attenuated in more racially diverse cities.

Additionally, we contribute to contemporary and historical strands of the education literature. To the former, our findings align well with an increasing number of papers that find that public health, social, and education programs – those expanded under suffrage – can lead to significant gains among populations with the most need (Almond et al., 2011; Hoynes et al., 2011; Currie and Gruber, 1996; Bitler et al., 2014). Our study also informs the body of work on the rise in educational attainment during the early 20th century, which has thus far has not accounted for the total gains in education over this period.²

The remainder of the paper continues as follows. In Section 2 we provide institutional background on the passage of suffrage laws and discuss findings from prior literature. We present the expected effects of suffrage in Section 3. 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.

2 Background on the Passage of Women’s Suffrage

Although women had gained some economic rights prior to the passage of suffrage (Doepke and Tertilt, 2009), enfranchisement was an important landmark for their empowerment. Prior to suffrage, women had very few, if any political rights (Baker, 1984; Keyssar, 2000),³ and 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 voice in local policies and elect representation closer to their preferences, radically expanding their access to the political system.

We illustrate the sequence of the passage of suffrage laws across states in Figure 1 using data from Lott and Kenny (1999) and Miller (2008).⁴ In our analysis, we will exploit the

²A 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. 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 the racial gap in schooling.

³The most common form of political voice for women was the right to vote for school boards, although anecdotally school elections resulted in low female participation (Youmans, 1921). School board voting rights were extended during the mid- to late- 19th century in 21 states (Keyssar, 2000). Since these laws preceded the passage of state and presidential suffrage by over 30 years, our results should be interpreted as the effect of full voting rights above any existing school voting rights.

⁴Following the prior literature, our focus is the timing of the earliest state or presidential suffrage law passed in the state, since subsequent laws may have been passed strategically in the anticipation of the

variation in suffrage laws passed after 1900, beginning with Washington in 1910.⁵ Between 1910 and 1919 an additional 24 states passed suffrage laws, culminating in the 1920 passage of the Nineteenth Amendment, a federal mandate for women’s voting rights, which obligated all states to enact suffrage. Three-fourths of the 48 states ratified the Nineteenth Amendment prior to its passage, and the remaining 12 states, labeled as “Mandated” in Figure 1, adopted it by mandate in 1920. Importantly, if education only increased in voluntary states due to endogenous law adoption, we would not expect to find effects on education in the mandated states, who did not influence – and in fact opposed – the timing of suffrage. We check this in Section 7.

Voting patterns after the passage of suffrage laws indicate that women participated robustly in elections after suffrage, though women’s turnout was typically still not as high as the turnout for men (Corder and Wolbrecht, 2016). In Appendix Figure A.1 we construct an event study of the log of voter turnout relative to the population over 21 for presidential elections.⁶ Controlling for state and year fixed effects, we estimate that the turnout rate increased by 45 log points, or 56 percent, following suffrage. These estimates confirm that suffrage had a meaningful impact on voting in the United States (Lott and Kenny, 1999).

By nearly doubling the size of the electorate, suffrage may have shifted the interests of the median voter towards greater legislative efforts targeting children’s welfare, which at the time was known as a top policy priority among women (Lemons, 1973; Moehling and Thomasson, 2012). Empirical analyses of the effects of suffrage have indeed uncovered a nationwide transformation of the government. Lott and Kenny (1999) find that suffrage led a 13.5% increase in state government expenditures and more liberal representation in Congress. Extending these results, Miller (2008) 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 the increases in spending were sharp and followed immediately after the passage of the laws, although the duration of the health spending increases has been debated (Moehling and Thomasson, 2012). Miller’s analysis also finds that suffrage reduced child mortality by as much as 15%, which he attributes to public sanitation projects funded after suffrage.

The effects of suffrage on education expenditures are less clear. Miller (2008) and Lott and Kenny (1999) both report no significant effect of suffrage on state education spending.

Nineteenth Amendment. 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).

⁵This helps balance exposure to suffrage across states in our sample, as we discuss in Section 4.

⁶We focus on presidential elections because turnout tends to be higher, and therefore more reliable, than in other elections (Cascio and Washington, 2013). Voter data is described in Appendix Section B.

However, Carruthers and Wanamaker (2014) link suffrage to higher local (total) education spending in three Southern states, with larger spending increases for white schools than black schools. One possible explanation for the discrepancy is that the earlier studies analyzed *state* spending, which accounted for less than 20% of local education expenditures during this period (Benson and O’Halloran, 1987). Another possibility is that suffrage led to more education spending in the South, but had no impact on average. We will aim to reconcile these findings using our data on city-level expenditures.

3 Expected Effects of Suffrage

To guide our analysis, we develop several testable hypotheses about how the expansion of women’s voting rights could affect educational spending, children’s educational attainment, and whether we should expect differential gains across subpopulations.

While prior work has investigated the mean effect of suffrage on social, health, and, to a limited extent, educational spending, there is little evidence on how this additional spending was distributed across or within states. Robust evidence from lab experiments and cross-sectional surveys shows that women have a stronger preference for redistribution, are more pro-social, and are more egalitarian (Andreoni and Vesterlund, 2001; Croson and Gneezy, 2009; Alesina and La Ferrara, 2005; Alesina and Giuliano, 2011; Duflo, 2012). Yet, it is not clear whether these preferences are salient in women’s voting decisions. One way women could achieve equality could be to elect representatives that vote to bring the per-child spending allocation closer to the national (or state) median spending. We would then expect suffrage-induced spending increases in our data to be *larger* in cities with below-median spending, with smaller positive, zero, or negative changes in spending in higher-spending cities.

On the other hand, if equalizing children’s outcomes conflicts with other preferences, in practice support for egalitarian policies may be weak. For example, women may prefer, above all, to maximize resources for one’s own child, which could imply that cities with a higher share of more influential women, such as the highly educated or whites, might use suffrage to capture more resources. There is also evidence that individuals tend to support less redistribution in ethnically diverse cities (Luttmer, 2001; Fong and Luttmer, 2011; Alesina et al., 1999), which could attenuate spending increases there. These forces could cause suffrage to have a *smaller* effect on spending in cities that have a higher share of blacks or in cities that have lower average education.

A third possibility is that women may have perceived all areas to be deficient in resources devoted to children. In that case, suffrage may have increased spending by an equal

proportion or by a set level in all cities.

It is important to note that these predictions leave open any effect of suffrage on *average* education spending. If suffrage mainly led to redistribution of state funds from more-affluent to less-affluent populations, there could have been no effect on expenditures on average, in line with the findings on state education spending in Lott and Kenny (1999) and Miller (2008). Another possibility is that suffrage could have expanded average per-child spending, but potentially in unequal ways, consistent with the larger resource gains among white relative to black schools in the Deep South (Carruthers and Wanamaker, 2014).

The effects of suffrage on education will depend on the sum total of the changes in social programs following the passage of these laws, as well as the elasticities of education with respect to these changes, as the returns to spending may be different across subpopulations. Responses to similarly sizable social interventions during this time period indicate that increases in human capital inputs have inconsistent effects on educational attainment. As one illustration, Bleakley (2007) finds that a hookworm eradication scheme in the South generated large increases in school attendance and literacy and long term effects on income, although no statistically significant impact on attainment.⁷ Aaronson and Mazumder (2011) find that a similarly-timed school-building program in the South (the “Rosenwald Initiative”) had significant effects on school attendance, literacy, and years of schooling for blacks. In a follow-up study, Carruthers and Wanamaker (2013) find that Rosenwald increased expenditures for white and black schools, although white children did not show the same educational gains. These last two studies provide the clearest evidence that the returns to suffrage-induced spending may have been heterogeneous, with larger increases in education among communities that had fewer initial resources.

Allowing women to vote may affect education through changes in household investments as well. For example, suffrage could increase women’s bargaining power,⁸ which could increase household spending on children. Since black women (and men) were disenfranchised until the 1960s through literacy tests and poll taxes (Cascio and Washington, 2013; Naidu, 2012), we would then expect white children to benefit most from this channel.

There may also be additional channels specific to girls, who now can look forward to having the ability to vote when they reach adulthood. Within households, this could increase the value of, and thus investment in, daughters. Additionally, if suffrage opens up more prominent roles to women, directly or indirectly by changing gender norms, young girls may be inspired to invest more in education. Each of these would suggest larger education gains

⁷We control for the possible overlap between this health intervention with region by birth year fixed effects.

⁸Women could gain bargaining power either directly through their ability to vote or through policies that may benefit women enacted by their chosen representatives.

among females relative to males (Jensen and Oster, 2009; Jayachandran and Kuziemko, 2011; Beaman et al., 2012; Jayachandran, 2015).⁹

We use these hypotheses to structure our analysis of the heterogeneous effects of suffrage, and explore specific mechanisms in Section 6.4.

4 Data

One of the strengths of our analysis is the large number of data sources we access to provide the most comprehensive description of the effects of suffrage on human capital. For brevity, we describe the pertinent details of each data source here and relegate the details of the construction of these variables to Appendix Section B.

4.1 Long-Term Outcomes

We analyze the effect of women’s suffrage laws on children’s educational and labor market outcomes using two pooled cross-sectional samples using data from the 1920 and 1930 U.S. decennial censuses and the 1940, 1950 and 1960 ones. 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). Relevant for our research design, the samples contain information on the year and state of birth, as well as the years of completed education and earnings for each individual (from 1940 on) and literacy (until 1930).¹⁰

Unless otherwise noted, our analysis sample consists of individuals at least 20 years old, who were born between 1880 and 1930 in states that adopted suffrage between 1910 and 1920.¹¹ This ensures that we observe cohorts that were at least 30 years old at the time of the passage of suffrage and that were born 10 years after suffrage in every state in the sample. Our results are not sensitive to analyzing slightly older individuals, who are more certain to have completed schooling, or to keeping early suffrage states (see Appendix Tables C.1 and C.2).

⁹We acknowledge that our identification strategy may be underpowered to detect effects from bargaining and increased value of daughters, as women’s suffrage may lead to national changes in these channels and/or spillovers across states.

¹⁰The 1950 Census only collected information on years of education for one individual per household, resulting in fewer observations in that year.

¹¹Hence, we exclude individuals born in the early adopter states Colorado, Idaho, Utah and Wyoming. We also exclude those born in Alaska, the District of Columbia and Hawaii, which were not U.S. states by 1920, and for which we do not have either a date of suffrage or state-level controls.

4.2 State Level Controls

We augment the Census data with state-by-cohort measures of the demographic and economic composition of the state and time-varying policies that could affect education choices. They include the percentage female; population; percentage white; percentage black; percentage illiterate; employment in manufacturing; total wages paid in manufacturing; total value of farm property; percentage urban population; and percentage foreign born. We also construct two measures of compulsory schooling for each cohort in the state, the compulsory attendance requirement and the child labor educational requirement, as well as exposure to the Rosenwald Initiative during childhood.

4.3 Education Spending and Mortality

To gain insight into the effects of suffrage on health, we digitized the *Mortality Statistics* files, which provide us with annual counts of deaths by state, age, race, and gender from 1900 to 1932. Data are only available for the participating states, which consists of 10 states in 1900 and grows to 48 states by 1933.

We also digitized city-level enrollment, education expenditures, and revenues from the Report of the Commissioner of Education and Biennial Survey of Education for cities with populations of 10,000 and over. Each report contains data for a single academic year (e.g. 1909 to 1910), which we will hereafter refer to by the calendar year of the start of the term (e.g. 1909). The dataset includes annual data from 1909 to 1911 and 1913 to 1915 and biennial data between 1917 and 1927 (12 academic years in total). To our knowledge, this is the most comprehensive panel of city-level schooling resources and enrollment spanning this period. For our main analyses, we keep cities that appear in all three (enrollment, spending, and revenue) datasets, and that have available information for at least 7 of the 12 years, which helps achieve balance across years.¹² We drop cities that we identify as outliers, that have enrollment and spending above the 99th percentile. Our final dataset contains city-year cells detailing enrollment, spending, and revenue variables from 1909 through 1927 for 42 states and 523 cities.

5 Empirical Strategy

We estimate the effect of suffrage using a difference-in-differences framework that compares the outcomes of cohorts born prior to the enfranchisement of women in their state of

¹²We have also run the results requiring cities to appear in 8, 9, or 10 years, which reduces the number of cities in the analysis, or including cities that appear in fewer years.

birth, and hence less exposed or unexposed to the laws, to those born after the law’s passage in their state of birth, who were fully exposed to the laws. We define exposure using state of birth both because it is exogenous to the treated child and provides a reasonable proxy for childhood location.

We start by estimating the effects of voting laws by age of exposure in an event-study-type model. This allows us to visually examine the trend in outcomes among cohorts exposed at older ages, who we argue should be less affected by exposure to suffrage. It also provides information regarding the linearity of the treatment effects, which may reveal information regarding mechanisms at work. For example, if our impacts are primarily driven by health improvements at an early age, we might expect to see small effects at all ages except 0 to 5 (Hoynes et al., 2016).

We estimate:

$$Y_{icsrt} = \alpha_0 + \sum_{a=-10}^{30} \beta_a \mathbb{1}(AgeTreat_{cs} = a) + \gamma_1' X_{icst} + \gamma_2' Z_{cs} + \theta_c + \delta_s + \chi_s \times c + \tau_{ct} + \phi_{rc} + \epsilon_{icsrt}, \quad (1)$$

where i , c , s , r , and t represent individual, cohort, state of birth, region of birth, and survey year, respectively, and $AgeTreat_{cs}$ is the age of individual i in the year that women’s suffrage was passed in s . δ_s and θ_c flexibly control for differential political, education, and education climates across states and cohorts, respectively. A state-level trend, $\chi_s \times c$, controls for linear changes in education at the state level across different years of birth, and cohort by survey year fixed effects, τ_{ct} , further control for the aging of cohorts over time. We also include a vector of individual controls, X_{icst} , including race, age, and gender, to absorb differences across demographic groups in educational attainment, and a vector of controls for the demographics and policies available in ones year of birth, Z_{cs} , to account for time-varying non-linear changes in state demographics, employment, income, and changes in education policy and availability.¹³ Region by cohort fixed effects, ϕ_{rc} control for unobservable differences across regions over time that may cause the clustered passage of suffrage laws.^{14,15} The variation used for identification of the coefficients of interest, β_a , is thus generated by

¹³We experiment with the sensitivity of our results to varying functional forms for these controls in Section 7 and find few differences across the specifications.

¹⁴Without these fixed effects, the clustered passage of laws could be a problem for our identification if education outcomes are also spatially correlated (Stephens and Yang, 2014), which we find is the case in Appendix Figure C.1. Hence, we impose comparisons within regional cohorts. Appendix Table C.3 shows that without these fixed effects, we find larger and more precise average effect for whites.

¹⁵One concern might be that by including region by cohort fixed effects, we might be throwing away a lot of identifying variation. If this were the case, we would expect this to show up in larger standard errors. In fact, our analytical standard errors are unchanged when we add these fixed effects (see Appendix Table C.3). We also validate that this design does not lead to a large likelihood of picking up treatment effects by chance in our randomization test in Section 7.

differential exposure to suffrage within cohorts and across states (within regions), as well as within states and across cohorts.

We plot the event studies for the ages of suffrage exposure from -10 and 30, setting the treated age equal to “30” for all $AgeTreat_{cs} \geq 30$ and to “-10” for all $AgeTreat_{cs} \leq -10$.¹⁶ 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, exposure to suffrage at ages 16 or 17. We perform regressions separately by race to take account of the marked gaps in educational attainment and in human capital investments across black and white children during this period.

We summarize the average effect of additional exposure to suffrage using a more generalized form of difference-in-differences, as follows:

$$Y_{icsrt} = \alpha_0 + \beta_1 SuffExp015_{cs} + \gamma_1' X_{icst} + \gamma_2' Z_{cs} + \theta_c + \delta_s + \chi_s \times c + \tau_{ct} + \phi_{rc} + \epsilon_{icsrt} \quad (2)$$

where $SuffExp015$ is a continuous measure of exposure to the suffrage laws, defined as the share of time between birth and age 15 that women are able to vote in an individual’s state of birth.¹⁷ We define the relevant age of exposure ending at the typical school-leaving age, 15 years, at which point children are on the margin of leaving school and are susceptible to policy changes. We arrive at 15 years as the sum of the median age of school entry (7) and average completed schooling (8) (Collins and Margo (2006)). However, since there is a wide distribution of school entry and leaving ages, this is only a rough approximation, and we will use our event study specification as a data-driven way to validate the relevance of this margin.

5.1 Identifying Assumption

The identifying assumptions for this model are that state-level suffrage laws are uncorrelated with time-varying unobserved characteristics of states that are predictive of human capital outcomes (no pre-trends), and that there are no confounding events with suffrage. We control for time-invariant characteristics of states and for linear state-specific trends across cohorts to minimize the influence of these unobserved characteristics. Nonetheless, there

¹⁶To gain additional precision, we also pool together two consecutive years of treatment ages, e.g. $AgeTreat_{cs} = -10$ and $AgeTreat_{cs} = -9$ both become $AgeTreat_{cs} = -9$, $AgeTreat_{cs} = -8$ and $AgeTreat_{cs} = -7$ both become $AgeTreat_{cs} = -7$, and so forth.

¹⁷Formally,

$$SuffExp015_{cs} = \sum_{a=0}^{15} \frac{1(c+a > YearSuffrage_s)}{16} \quad (3)$$

where $YearSuffrage_s$ is the year in which suffrage was passed in the state.

may remain some (potentially small) correlations with unobserved time-varying factors that remain threats to our identification.

Since we cannot directly test for the influence of unobserved characteristics, we provide indirect evidence of the plausibility of the first assumption by testing whether suffrage was preceded by a systematic change in state policy or composition that we could misattribute to suffrage. To diagnose the importance of any pre-existing trend, we estimate a modified event-study model, in which we replace the pre-suffrage indicators with a linear trend, as follows:

$$Y_{st} = \alpha_0 + \alpha_1 \textit{YearRelSuffrage}_{st} + \sum_{y=1}^{20} \beta_y \mathbb{1}(\textit{YearRelSuffrage}_{st} = y) + \gamma' Z_{st} + \delta_s + \phi_{rt} + \epsilon_{st} \quad (4)$$

Y_{st} is a state- (or city-) characteristic in state (or city) s and $\textit{YearRelSuffrage}_{st}$ is a linear trend in years since suffrage in state s , and $\sum_{y=1}^{20} \mathbb{1}(\textit{YearRelSuffrage}_{st} = y)$ are indicators for each year after suffrage. Note that since we include indicators for each year after suffrage, α_1 , the coefficient on $\textit{YearRelSuffrage}_{st}$, is mechanically only identified only from the data prior to suffrage, and gives the slope in Y_{st} prior to suffrage. We include state (or city) fixed effects, region by year fixed effects, and the same state time-varying controls¹⁸ as in equation 2. To reduce noise in the estimation of the pre-trend, we estimate this using the sample of states (or cities) for which we have at least three years of data prior to suffrage. We analyze 31 states for the majority of the state-level regressions, and 2,129 cities across 41 states for the city-level regressions.

We estimate Equation 4 for a variety of state and city-level economic indicators, health outcomes, public investments, and compulsory schooling requirements taken from the state-year panel of state characteristics compiled by Lleras-Muney (2002) and our city-level schooling data. We also create a predicted education index by regressing the mean education in each state and cohort on the state covariates at birth, using only observations prior to suffrage, and then obtaining fitted values from the model for all observations. This aggregation of our covariates, weighted by their importance for education, increases our ability to detect a trend in factors relevant for human capital.

Table 1 shows estimates of α_1 . Of the 19 outcomes we analyze, just four are significant at the 5 percent level: percent foreign ($\alpha_1 = -0.346$), income reported per capita ($\alpha_1 = -0.05$), number of schools per capita ($\alpha_1 = -0.05$), and white mortality under age 5 ($\alpha_1 = -0.047$). Since we only analyze individuals born in the U.S., the decline in the share of foreigners does not directly influence our estimation, and the direction of any indirect effects are not obvious. The effect of a reduction in white mortality has a similarly ambiguous bias. The

¹⁸We exclude any controls that are directly related to the outcome, though, in order to increase our ability to detect a trend.

direct effect of increasing the number of surviving children is likely to reduce education, since survivors are negatively selected. However, coinciding improvements in health could improve education. The negative trends in income per capita and number of schools per capita, which are inputs to human capital, are most likely to bias us against finding an effect.

The remaining 15 coefficients are not significant, typically small in magnitude, and are not systematic in the predicted effects on human capital. There is no significant trend leading up to suffrage in the size, racial composition, urbanicity, manufacturing wages, farm value, white or black child mortality, number of hospitals, compulsory attendance, schooling enrollment or schooling expenditures. Importantly, when we examine the predicted education indices, which pool together all of these covariates, we do not find statistically significant pre-trends in predicted white ($p=0.12$) and black ($p=0.58$) education. This is consistent with other similar investigations that have shown few correlates of suffrage (Dahlin et al., 2005; Braun and Kvasnicka, 2013; Miller, 2008). Reinforcing this, in the next section we also find no trend in *observed* education across cohorts.

One might also be worried that suffrage was bundled with other progressive era laws that could have improved education. Appendix Table C.4 finds no correlation between the year that suffrage was passed and the year of several other laws, including prohibition and women’s minimum wage. Moreover, the direction of the coefficients indicate that, if anything, suffrage was typically passed after these laws, which means that any effect of these other laws would have been expected to show up in the pre-trends analysis. Nonetheless, to test for the possible influence of other progressive laws, we add indicators for the presence of each law at birth to our predicted education index. Then we estimate whether cohorts exposed to suffrage are predicted to have a higher level of education, due to their exposure to different state characteristics and laws alone. We find a statistically insignificant effect, implying that correlated exposure to other progressive laws can not explain our findings (See Appendix Table A.1).¹⁹ Similarly, the timing of suffrage could be associated with other infusions of spending, like during the New Deal, or contemporaneous changes in compulsory schooling laws. Again, we don’t find evidence for this (see Appendix Tables C.5, C.6 and C.7).

6 Results

We present the results for the event study specification separately by race in Figure 2, where we plot the estimated coefficients as well as their 95% confidence intervals by age of

¹⁹We exclude mortality from this prediction in order to be able to use the full sample of states and the number of schools per capita, which we attribute to suffrage. The results are less precise when we include these variables.

treatment.

For blacks, shown in Panel A, the event study indicates that suffrage had a statistically insignificant effect on the education of those that were exposed to suffrage after age 15, who we expected would have made school-leaving decisions prior to suffrage. We also find large, positive, statistically significant effects for children that were exposed to suffrage before age 15. Among those exposed to suffrage before age 15, younger exposure is generally associated with larger increases in education until age 5, when the effects level off. Exposure to suffrage by age 5 increases educational attainment by roughly 1 year of additional education.

In contrast, for the white sample in Panel B, the effects hover at zero and are flat at all ages of treatment. The null effect for this sample 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. In the following section, we test whether there are varying impacts *within* whites and blacks, which could help us to rule out one of these explanations.

Across both samples, the pattern of the coefficients provides strong evidence in favor of our empirical strategy. The absence of an impact of suffrage among individuals exposed to suffrage after age 15 suggests that our effects are not capturing differential trends in educational attainment across cohorts.²⁰ Additionally, the shape of the coefficients across ages 0 to 15 resembles the age-pattern of effects resulting from exposure to other important childhood interventions, such as increases in school spending and exposure to high-quality neighborhoods (Jackson et al., 2016; Chetty and Hendren, 2016), which bolsters our confidence in these results.

We present the coefficients from the difference-in-differences model in Table 2. On average, full exposure to suffrage from age 0 to 15 is associated with a statistically insignificant 0.13 increase in years of schooling. Analyzing whites and blacks separately,²¹ we find that for whites, full exposure to suffrage increased education by a statistically insignificant 0.10 years. For blacks, full exposure to suffrage led to 0.99 years of additional education ($p < 0.01$), which is statistically significantly larger than the effect on whites ($p < 0.03$). This increase represents a 15% gain relative to the mean years of completed education among blacks.

In the remaining four columns of Table 2 we analyze whether suffrage differentially improves outcomes for girls, a pattern shown in previous studies of female empowerment (Qian,

²⁰We formally test for an effect of suffrage beyond age fifteen in Appendix Table A.2 by testing the effect of exposure between age 16 and 22 and between 23 and 30 as additional covariates. We find an insignificant effect of suffrage exposure after age 15, while the coefficient on exposure between age 0 and 15 is similar to the base specification.

²¹We exclude individuals that did not qualify as neither white nor black from this subgroup analysis. The excluded sample is small, with only 4,592 observations.

2008; Duflo, 2003; Beaman et al., 2012). This could occur if, for example, parents perceived daughters to be more valuable after suffrage, and therefore perceived the returns to investing in the human capital of daughters to be higher. Additionally, there may be changes in gender attitudes and modeling effects for younger girls inspired by women’s expanded political rights.

Contrary to these predictions, the results do not show a larger increase in the education of girls relative to boys. We find a statistically insignificant impact of suffrage for white women, and while the point estimate is larger than the impact for white men, we could not rule out that they are the same. For blacks, we actually find a larger effect of suffrage exposure on men than on women (1.39 years compared with 0.60 years).

6.1 Sources of Treatment Effect Heterogeneity

The differential impact of suffrage across races is consistent with our earlier hypothesis that suffrage may have had larger impacts for those with fewer initial resources. To investigate this possibility further we use a more rigorous test: whether suffrage also had larger effects for more disadvantaged individuals *within* racial groups. Our main measure of disadvantage throughout is group-specific pre-suffrage education levels, which we calculate using individuals age 16 and above at suffrage. In the Appendix we find similar patterns using other measures of socioeconomic status.²²

As a first step, we provide descriptive evidence of the differential effects of suffrage by separately estimating the effect for each region, race and gender, and then plotting these coefficients against the mean education level prior to suffrage. Figure 3 shows a negative relationship between the size of the coefficient and pre-suffrage education. Subgroups with lower levels of pre-suffrage education gain approximately one year of additional education post-suffrage, while subgroups with higher levels of pre-suffrage education have little or no gain. We also notice that the impacts are no longer solely concentrated among black individuals. We formally show this in Appendix Table A.3, where we allow our effects to vary by region. White children in the South, who averaged 8 years of education prior to suffrage, gained an additional 0.96 years in education (se: 0.45) following suffrage, and whites in the Northeast and West, who averaged 9 years of education prior to suffrage, gained an additional 0.42 (se: 0.19) and 0.49 (se: 0.20) years, respectively.^{23,24}

²²See Appendix Figure C.3.

²³We are able to reject that the effects for whites across regions are the same ($p = 0.08$). For blacks, we can not reject that the effects are the same in all regions outside the West, which we exclude from the test due to concerns about small sample size and overfitting.

²⁴Appendix Figure C.4 shows this in an event study by allowing for differential effects for white and black children from the South and non-South. The age pattern of effects for whites from the South is very similar

With this in mind, we formally test for a relationship between the effect of suffrage and state-level disadvantage within races. To do so, we add an interaction between suffrage exposure and the pre-suffrage average education in the state, calculated separately for each sample, to our base specification. We report the main effect and the interaction in Table 3. The coefficient on the main effect, which represents the average effect for a group with zero pre-suffrage education, is 2.91 for the whole sample. Further, consistent with Figure 3, the coefficient on the interaction is negative and significant. With each additional year of pre-suffrage education, the effect of full exposure to suffrage goes down by 0.31 years. As a basic check on the fit of this model, we plug in the pre-suffrage mean education levels of whites and blacks, and obtain estimates close to our baseline difference-in-difference effects.

In columns (2)-(3) of Table 3 we show the results for whites and blacks separately. For whites, we find a strong negative gradient in the effects of suffrage with respect to pre-suffrage education; for blacks, the interaction is also negative, although not statistically significant. These results substantiate our hypothesis that the impact of suffrage was near-universal at low levels of education for both whites and blacks, but does not appear in the average effect for whites because of the composition of the sample.

6.2 Impacts on the Distribution of Education

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.

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).²⁵

to that of blacks, with larger gains for those exposed at younger ages, and leveling off for those exposed by age 5. But we can see that white children in the South exposed between the ages of 15 and 20 also experienced some small increases in education, perhaps because whites had higher average education prior to suffrage.

²⁵Specifically, we estimate:

$$G_{icsrtp} = \beta_0 + \theta_p \text{SuffExp015}_{cs} + \gamma_1 X_{icst} + \gamma_2 Z_{cs} + \rho_s + \chi_s \times c + \delta_c * \psi_t + \tau_{ct} + \phi_{rc} + \epsilon_{icsrt}, \quad (5)$$

Panels A and B of Figure 4 plot the coefficients obtained from this estimation procedure for the black and white samples, respectively. For blacks, we find that the impact of suffrage on education attainment is concentrated between 4 and 7 years of education, while for whites we find small effects between 7 and 9 years of education.²⁶ To check the alignment of these effects with the distribution of educational attainment, 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.

6.3 Literacy and Labor Market Outcomes

The previous discussions focused on the impact of suffrage on the quantity of education attained. In this section, we examine whether the extended time in school led to the acquisition of literacy, and whether the impacts on education translated into gains in the labor market.

6.3.1 Literacy

We analyze effects on literacy on individuals ages 15 and above as a proxy for whether suffrage led to increases in measurable skills (Aaronson and Mazumder, 2011). Note, though, that since literacy was near-universal by the 1900 cohort, especially among whites, this measure will only pick up improvements in very basic abilities (Collins and Margo, 2006).²⁷ Even with this little variation, Appendix Figure A.2 indicates that there were some positive impacts on literacy, with up to a 5 percentage point increase for black children exposed at the youngest ages. While the results are measured with error, this is suggestive evidence that suffrage led to improvements in literacy together with extended schooling.

6.3.2 Labor Market Outcomes

Next, we analyze whether suffrage impacted labor market outcomes, including the log of wage income and the likelihood that an individual has non-zero income.²⁸ Here we limit

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

²⁶Among blacks we also find increased likelihood of post-secondary education, although these effects are small and unlikely to drive our overall effects among this group.

²⁷Among the 1900 cohort, whites and blacks had literacy rates above 98% and 82%, respectively (Collins and Margo, 2006).

²⁸We test the sensitivity of these results to dropping data from the 1940 census, which, unlike the other censuses, does not report the earnings of self-employed workers (Collins and Wanamaker, 2014b), and find similar results.

our sample to working-age men and women ages 30 to 65 years old. Given the differential effects on educational attainment across regions and pre-suffrage education, we also allow for differential labor market effects along these lines.

Panel A of Table 4 shows that full exposure to suffrage led to a statistically significant 34 percent increase in income for whites in the South, and to insignificant effects for whites outside the South, commensurate with the small average effects on education there.²⁹ We also find an insignificant effect on income for blacks. This could be because blacks experienced lower returns to skill in the labor market or were exposed to lower quality of education, particularly in the segregated South (Bleakley, 2007; Karbownik and Wray, forthcoming). The point estimates are noisy, though, and we can not rule out large income gains for blacks. In Panel B, we find patterns of effects on income that mimic the effects on education: suffrage led to significant increases in income for individuals from states with low average education prior to suffrage. These heterogeneous effects, although only precisely estimated for blacks, indicate that income gains followed from the improvements in human capital after suffrage.

Appendix Table A.4 shows that suffrage increased the employment of blacks outside the South, but had small and imprecise effects for other groups. Note that this selection into working may attenuate the effect of suffrage on income for blacks shown above.

6.4 Mechanisms and Implications

We interpret our results as the reduced form effect of increased women’s bargaining power and public spending, which could affect human capital through improvements in health and educational quality. In this section we explore which of these mechanisms, if any, could account for the larger impact of suffrage on the education of less-advantaged groups.

6.4.1 Mechanism 1: Bargaining

First, political empowerment may increase the bargaining power of women in the household by reducing a woman’s reliance on her husband. Our evidence is weakest for this channel since we don’t observe much of intra-household behavior, including spending. Nevertheless, while this channel may have contributed to the effects on white children, it is less plausible for disenfranchised black communities. Thus, while bargaining may be a contributing factor to our estimates, it cannot be the *only* channel.

²⁹We show the corresponding event study figures in Appendix Figure A.3.

6.4.2 Mechanism 2: Health Improvements

The second mechanism is through health improvements, which could have been facilitated through increased public spending and health projects. Miller (2008) provides evidence of this channel in the aggregate, however in order to reconcile health improvements with the heterogeneous impact on education, we require more detailed estimates of suffrage-induced health improvements. We start by re-estimating whether the passage of suffrage ($YearRelSuffrage_{st} > t$) affected state-level mortality for blacks and whites separately, after controlling for state demographics and state and year fixed effects, as well as state linear time trends to account for the significant negative trend in mortality among whites prior to suffrage shown in Table 1. We then extend prior work by testing whether suffrage had differential impacts on infant mortality across race, region of birth, and group-level disadvantage, measured by mean education level prior to suffrage.

Appendix Table A.5 presents these results. Consistent with Miller (2008), we find that suffrage led to declines in mortality on average, with similar effects among blacks and whites. Moreover, in Columns (2) and (3) we show that mortality improvements are larger in the South and among more disadvantaged groups. However, the pattern of these results does not fully mirror those of educational improvements, as the effects are similar by race and equally large for whites in the South and non-South. This suggests that differential mortality alone did not generate the distributional education effects that we observe.

6.4.3 Mechanism 3: Education Spending

The third channel is through increases in educational expenditures following suffrage, which had the capability to reinforce and support increased demand for education. We use our data on city-level spending, revenues and enrollment to examine this mechanism. In order to trace out the timing of effects on spending, we estimate:

$$Y_{ca} = \alpha_0 + \sum_{t=-3}^7 \beta_t \mathbb{1}(YearRelSuffrage_{ca} = t) + \gamma' Z_{sa} + \delta_c + \phi_a + \epsilon_{ca} \quad (6)$$

where c and a index city and academic year, respectively. Given that the data captures academic year spending and enrollment, we match suffrage year to the academic year, so that $YearRelSuffrage_{ca} = t$ is an indicator for t academic years since suffrage. Similar to our attainment event studies, we group together two consecutive indicators, and our omitted category includes the academic year that suffrage was passed and the year prior. Z_{sa} , δ_c and ϕ_{at} indicate state demographic controls, city and academic year fixed effects, respectively.

We start by testing the effects of suffrage on *average* spending, which we hypothesized

in Section 3 could be either positive or zero. The first column of Table 5 shows that log expenditures were not significantly different in the immediate years before or after suffrage, but increased by 9.4% three years after suffrage was enacted, an effect that persisted in the following four years.³⁰ The remaining columns show that the increases in expenditures are mirrored in higher school revenues, which is driven by increases in local – not state – funding. Similar to the Census analysis, we find positive but smaller and insignificant average effects on enrollment.

Next, we want to use this data to understand whether suffrage might have improved the human capital of disadvantaged groups because of *larger* increases in the educational resources directed towards these groups. To answer this question, we allow the estimated effect of suffrage on spending and enrollment to differ across three measures of “status” or “advantage”: higher average level of education in the state prior to suffrage (the same measure used in our earlier analysis), living in the non-South, and a lower black share of the city population in 1910.^{31,32}

These coefficients are available in Appendix Table A.6, but for ease of interpretation we present them graphically in Figure 5. The figure shows the implied effects of suffrage for the 75th and 25th percentile of each of our continuous measures of status (average education and share black) and for the South and non-South, as well as their difference. The results show that both more- and less-advantaged cities experienced increases in log expenditures after suffrage, as indicated by the rise in the black and white markers in the left panels. Suggestively, areas with lower education, higher share black, and in the South appear to have experienced larger increases in spending, as shown by the rise in the blue markers, which trace the difference between the disadvantaged and advantaged cities.³³ We find that educational expenditures in the South increased by 20%, consistent with Carruthers and Wanamaker (2014), roughly twice the percent increase outside the South. The differences across areas are not typically statistically significantly different, though our limited sample of cities may reduce our power here.

Post-suffrage school enrollment follows a similar path as expenditures, with larger gains in cities with lower education, higher share black, and in the South. We are able to reject that the difference in enrollment gains is zero for each of the measures of disadvantage. Overall,

³⁰We find comparable effects on expenditures per pupil as well.

³¹We thank Claudia Goldin for generously providing us with the data on black population used in Goldin and Katz (2010). We match these data to 233 cities in our sample.

³²We have also tried estimating effects across a variety of other measures of status, including per-capita school expenditures prior to suffrage, and the results hold.

³³Similarly, when we use pre-suffrage per capita spending as a measure of status, we find that cities with spending at the 25th percentile experienced higher growth in spending than cities at the 75th percentile beginning 3 years after suffrage (statistically significantly higher beginning 5 years after suffrage.)

these results show suffrage led to higher growth in educational expenditures, particularly in lower-educated, more racially-diverse cities. Enrollment gains were also larger in these cities, which matches the pattern of gains in educational attainment we find in the Census. The results are less consistent with the hypothesis that women restricted funds to less racially diverse cities.³⁴

6.4.4 Magnitude of Effects

The multiple-pronged treatment resulting from suffrage generated educational gains similar to other notable educational interventions. The closely timed Rosenwald initiative, for example, was found to improve education of black children by a similar magnitude to suffrage (Aaronson and Mazumder, 2011). These sizable educational gains are not limited to interventions at the turn of the 20th century. The effects of suffrage are akin to the one year increase in the attainment of black students from court-ordered desegregation (Johnson, 2015), somewhat larger than the 0.6 additional years of attainment from a decrease in the pupil-teacher ratio by 10 students (Card and Krueger, 1992), and similar to the 0.9 year increase in attainment of children from poor families resulting from a 20% increase in per-pupil spending (Jackson et al., 2016).

7 Robustness

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

7.1 Mandatory States

As we alluded to earlier, the mandated states provide a useful test of our identification because we can be sure that their adoption of suffrage was not endogenous. We therefore estimate whether our effects are present among these states by adding an interaction between the measure of suffrage exposure and whether the state adopted suffrage involuntarily. The results in Appendix Table A.7 show that suffrage had a statistically significant *larger* effect in involuntary, mandated states compared to voluntary states. We do not place much emphasis on the magnitude of the difference, however, as there are many reasons, that could account for this, such as the differing composition of the the two sets of states.

³⁴However, we are not able to track how the funds were distributed within the city, so it is possible that whites could have captured funds in cities with a larger black population (Carruthers and Wanamaker, 2013).

7.2 Randomization Test with Placebo Suffrage Laws

To provide further evidence that our main findings are specific to suffrage, we perform a randomization test that allows us to determine whether our effects could have arisen by chance (Athey and Imbens, 2017). We randomly draw a placebo suffrage year between 1910 and 1920 for each state, without replacement, and use this to assign individual suffrage exposure. We then use our difference-in-difference model in Equation 2 to estimate the effect of this placebo suffrage exposure on educational attainment. We repeat this 1000 times. Appendix Figure A.4 presents the distribution of placebo effects for blacks and whites together with the “true” difference-in-difference estimate in the red line. The conclusions from the figure conform to our main estimates. For blacks, our estimate of suffrage is much larger than what we would estimate from any other combination of placebo suffrage laws, and is very unlikely to have arisen by chance ($p < 0.01$), while for whites many of the placebo laws generate the same or larger effects than our estimates ($p > 0.21$), reinforcing the large standard errors around that estimate.

7.3 Migration

An additional concern is whether internal migration might influence our estimates. If future parents that value investments in education are more likely to migrate to areas with earlier passage of suffrage laws, our effects could be biased by changes in the composition of parents in one’s state of birth. If this were the case, we should be able to predict our increases in education using the changes in demographics of the state across cohorts. As we discuss in Section 5.1, we do not find evidence for this (see Appendix Table A.1). In our favor, prior studies of the Great Migration - a likely source of movement during this period - suggest that the degree of selection into migration was small (Collins and Wanamaker, 2014a).

Migration from one’s state of birth can also introduce measurement error in our measure of exposure to suffrage laws. 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 Appendix Table A.8. For blacks, we find that the point estimate for non-movers is similar to our main estimate, and we find an insignificant effect on the education of movers. For whites, we find a statistically insignificant effect for movers and non-movers, as in our main estimate. This attenuation from movers indicates that our estimates are likely to be a lower-bound on the effects of suffrage.

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.³⁵ We absorb the national effects of the War with our cohort fixed effects, however, its effects could still bias our results if mobilization rates across states are correlated with suffrage. Controlling for region fixed effects, we find no correlation between the year of suffrage and the proportion of the state serving in WW2, which we obtain from Acemoglu et al. (2004). As additional reassurance, we note that the G.I. bill funded college enrollment, and has been found to have benefited primarily whites, both of which are inconsistent with our finding (Turner and Bound, 2003).

7.5 Additional checks

We run a variety of additional specifications to verify the robustness of the results. In Appendix Table A.9 we check the sensitivity of our results to utilizing a binary measure for exposure between the ages of 0 and 15. The effect of any exposure to suffrage is 0.3, which roughly aligns with the average effects in the event study. Appendix Table A.10 shows the results by census year. The results are generally the same across Census samples, although there is attenuation in the 1940 census consistent with the measurement anomalies reported in previous studies (Goldin, 1998). Appendix Table C.3 show the effects of adding the controls in our main specification one at a time.

In Appendix Table A.11 we document the insensitivity of our results to the choice of state level controls. In Panel (A), we replicate our main results, where we control for state covariates at birth. However, one may be worried these covariates could potentially be endogenous to suffrage, hence we consider alternative ways to specify our these controls. In Panel (B) we include the same control variables as in our main results 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. In Panel (C), instead, we interact the level of the control variables in 1900 with a linear trend (Hoynes et al., 2016). Here we run the risk of under-controlling for confounding variation. Again, the coefficients are steady. Moreover, Panels (D) to (F) show that our results are not sensitive to dropping compulsory law controls, adding controls

³⁵Early cohorts in our sample born from 1880 to 1900 were also eligible to serve in WWI. 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.

for progressive laws, or controlling for trends interacted with the pre-suffrage education level of the state. Overall, we are reassured that the estimates are not sensitive to the choice of controls.

8 Conclusion

This paper presents new evidence on the effects of women’s political empowerment on 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. Moreover, we show that suffrage led to gains in the labor market among children that experienced improvements in education.

We trace these long-term effects to the contemporaneous impacts of these laws on education spending and childhood health. Using newly digitized data, we find that while all cities experienced increases in log expenditures after suffrage, those with a higher share black, in the South, and with lower pre-suffrage average education, experienced larger gains, mirroring our educational attainment results. We also show similar patterns in the impacts of suffrage on infant mortality. This suggests that the policies resulting from suffrage were effective at raising human capital investments and the attainment of students for children from more racially-diverse, less-educated communities.

On the whole, this article provides compelling evidence for the role of female voter preferences in influencing policy, both towards greater investments in children and less advantaged groups. 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 question is of great relevance today given the push for gender equality in the workplace. We leave it for future research to provide evidence in this area.

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9 Tables

Table 1: Estimated Trend in State and City Characteristics Prior to Suffrage

	Trend Coef.	SE	P-value	N	States
Pct. White	0.037	0.091	0.691	326	31
Pct. Urban	0.160	0.191	0.409	326	31
Pct. Foreign	-0.346	0.072	0.000	326	31
Ln Pop	0.013	0.007	0.086	326	31
Pct. Emp. Manuf.	-0.008	0.008	0.292	326	31
Ln Manuf. Wage per Earner	0.000	0.004	0.936	326	31
Ln Avg. Farm Value	-0.007	0.017	0.699	326	31
Ln Tax-Reported Income per Capita	-0.051	0.010	0.000	326	31
Ln Number Hospitals	0.012	0.008	0.137	326	31
Ln Doctors per Capita	0.030	0.037	0.430	326	31
Ln White Mortality Under Age 5	-0.047	0.011	0.000	271	30
Ln Black Mortality Under Age 5	0.009	0.026	0.718	261	29
Ln Number of Schools per Capita	-0.051	0.010	0.000	326	31
Compulsory Attendance	-0.057	0.087	0.520	326	31
Schooling for Child Labor	0.087	0.066	0.195	326	31
Predicted Yrs. Ed. for Whites (Summary Index)	-0.014	0.009	0.121	261	29
Predicted Yrs. Ed. for Blacks (Summary Index)	0.038	0.067	0.576	261	29
Ln School Enrollment (City Data)	0.000	0.025	0.999	2179	41
Ln School Spending (City Data)	-0.016	0.016	0.328	2179	41

Notes: The trend coefficient and p-value shown in each row come from a regression of the outcome shown in the first column on a trend in the number of years elapsed since suffrage, indicators for each year since suffrage, region-year fixed effects, state (or city) fixed effects, and state-year controls. Sample includes all states (or cities) for which we have at least three years of data prior to the passage of suffrage. Estimates are weighted using state (or city) population weights and standard errors are clustered at the state level. Sources: State characteristics from 1915 to 1930 are taken from Lleras-Muney (2002); infant mortality records from 1900 to 1930 are digitized from the Centers for Disease Control and Prevention; and records on city-level education spending are digitized from the 1909 to 1911 and 1913 to 1915 Report of the Commissioner of Education and the 1917 to 1927 Biennial Survey of Education for cities with populations of 10,000 and over.

Table 2: Effect of Suffrage on Years of Education

				Whites		Blacks	
	All	Whites	Blacks	Males	Females	Males	Females
Suff Share 0-15	0.128 (0.211)	0.099 (0.202)	0.993*** (0.262)	0.068 (0.194)	0.127 (0.224)	1.385** (0.601)	0.602** (0.226)
Mean Education	9.647	9.967	6.810	9.850	10.078	6.400	7.171
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. We are able to reject that the coefficients for the white and black coefficients are the same ($p < 0.03$). 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 3: Effect of Suffrage on Years of Education - Interaction with Pre-Suffrage Education Levels

	All	Whites	Blacks
Suff Share 0-15	2.913*** (0.542)	2.888*** (0.634)	2.603** (1.108)
Suff Share 0-15 x Pre-Period Education	-0.313*** (0.059)	-0.308*** (0.069)	-0.245 (0.159)
Mean Education	9.647	9.967	6.810
Observations	1555475	1393855	157024

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-suffrage education levels, which is calculated for each state (and race for columns (2) and (3)) as the average education in that sample among individuals age 16 and above in the year that suffrage was passed. 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 Log Income -
Interactions with South and with Pre-Suffrage Education Levels

	All	Whites	Blacks
<i>A: Interaction with South</i>			
Suff Share 0-15	0.018 (0.024)	0.018 (0.026)	0.092 (0.196)
Suff Share 0-15 * South	0.160 (0.107)	0.341*** (0.103)	-0.254 (0.297)
Mean Y	8.394	8.465	7.813
Observations	1108107	978614	126319
<i>B: Interaction with Pre-Period Education</i>			
Suff Share 0-15	0.140** (0.060)	0.152 (0.228)	0.875** (0.387)
Suff Share 0-15 x Pre-Period Average Education	-0.012* (0.006)	-0.011 (0.025)	-0.143** (0.054)
Mean Y	8.394	8.465	7.813
Observations	1108103	978614	126317

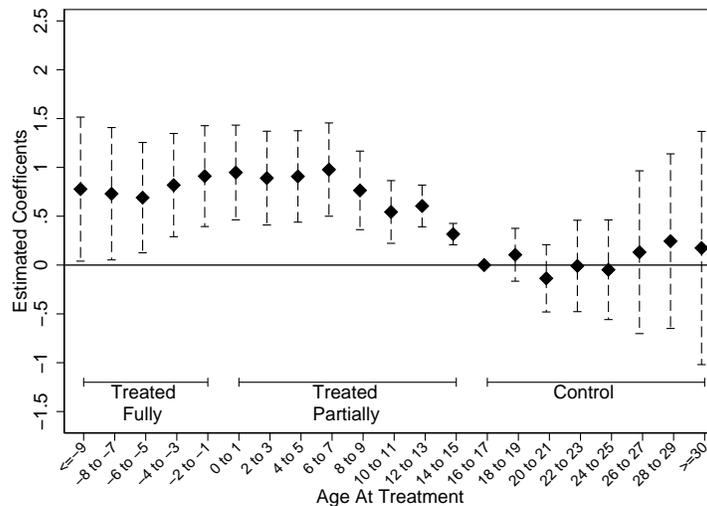
Notes: This table contains results obtained when the dependent variable is log income and the independent variable is suffrage exposure, which is defined as the share of time between birth and age 15 that an individual was exposed to a suffrage law in his state of birth. Moreover, we include interactions between suffrage exposure and either South (Panel A) or pre-suffrage education levels (Panel B). Pre-suffrage education is calculated for each state (and race for columns (2) and (3)) as the average education in that sample among individuals age 16 and above in the year that suffrage was passed. 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 between 30 and 60 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 Log Expenditures, Log Enrollment, and Log Revenues

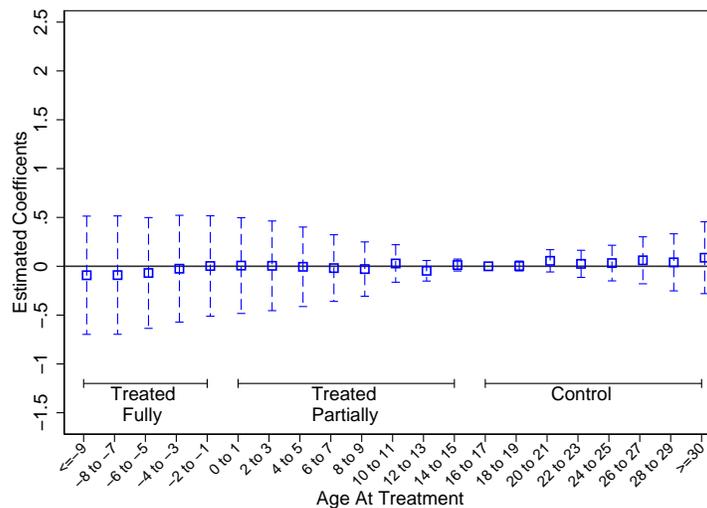
<i>Years Relative to Suffrage</i>	Revenues				
	Expenditures	Total	State	Local	Enrollment
-3 Years	-0.025 (0.021)	-0.044 (0.041)	-0.157 (0.207)	-0.049 (0.051)	0.018 (0.017)
1 Years	0.036* (0.021)	0.038 (0.025)	-0.127 (0.130)	0.047 (0.030)	0.019* (0.010)
3 Years	0.099*** (0.033)	0.098** (0.040)	-0.054 (0.190)	0.104** (0.040)	0.029 (0.021)
5 Years	0.113*** (0.034)	0.118** (0.048)	-0.305 (0.303)	0.132** (0.059)	0.040 (0.026)
7 Years	0.098** (0.042)	0.081 (0.057)	-0.173 (0.400)	0.058 (0.097)	0.065 (0.040)
Obs	5183	5183	4565	5172	5183
Pre-Suffrage Y Mean	13.52	13.62	11.37	13.46	9.40
N Cities	523	523	521	523	523
N States	42	42	41	42	42

Notes: This table contains results obtained when the dependent variables are the ones listed in the column headers, and the independent variables of interest are academic years since suffrage. All regressions include controls for state-level characteristics, and city and academic year fixed effects. Estimates are weighted using city population in 1910, and standard errors are clustered on state. The sample consists of all cities with available expenditure, revenue and enrollment data, which we observe for at least 7 years, and which are not outliers. Source: 1909 to 1911 and 1913 to 1915 Report of the Commissioner of Education, and 1917 to 1927 Biennial Survey of Education for cities with populations of 10,000 and over. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 2: Effect of Suffrage at Each Age of First Exposure on Years of Education, By Race



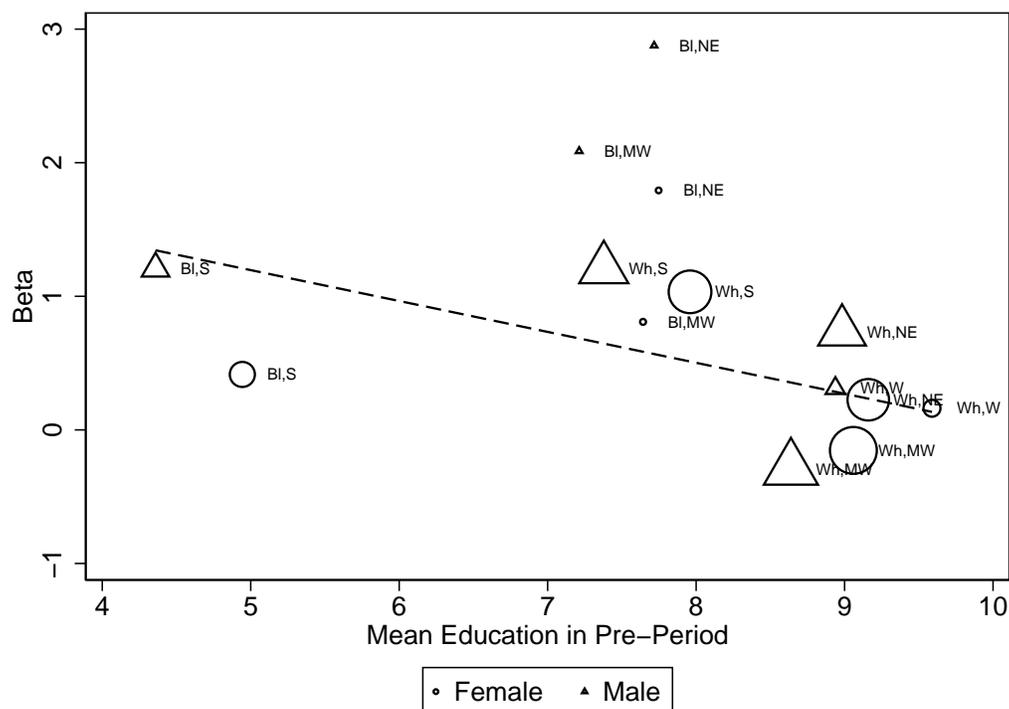
(a) Blacks



(b) Whites

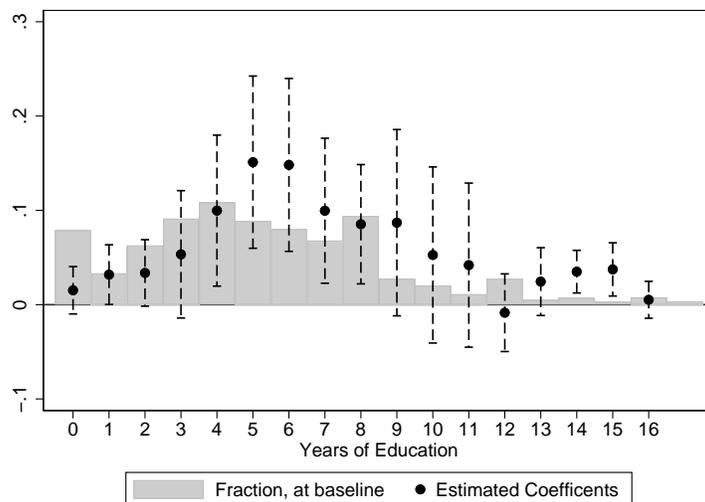
Notes: This figure plots the estimated coefficients (and 95% confidence intervals) obtained from event study specifications that analyze the effect of suffrage at each age of first exposure on educational attainment, estimated separately for whites and blacks. All specifications include controls for 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 16 to 17 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 3: Subgroup Averages of Pre-Suffrage Education and the Estimated Effects of Suffrage on Years of Education

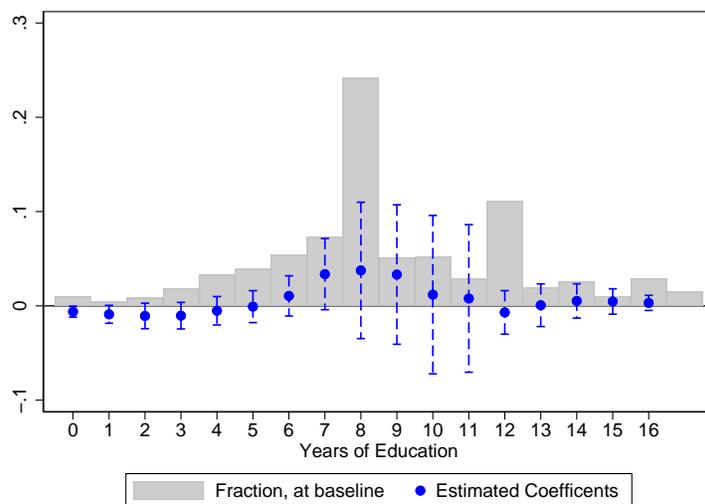


Notes: To create this figure, we first estimate specifications that analyze the effect of suffrage exposure on educational attainment separately for demographic groups defined according to region of birth, race and gender. We then plot the estimated coefficients along with the average pre-suffrage educational attainment (average attainment among individuals that were age 16 or older by the passage of suffrage in the state) for each demographic group, with the circle/triangle size representing the number of observations in each 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 4: Effect of Suffrage on the Distribution of Years of Education, By Race



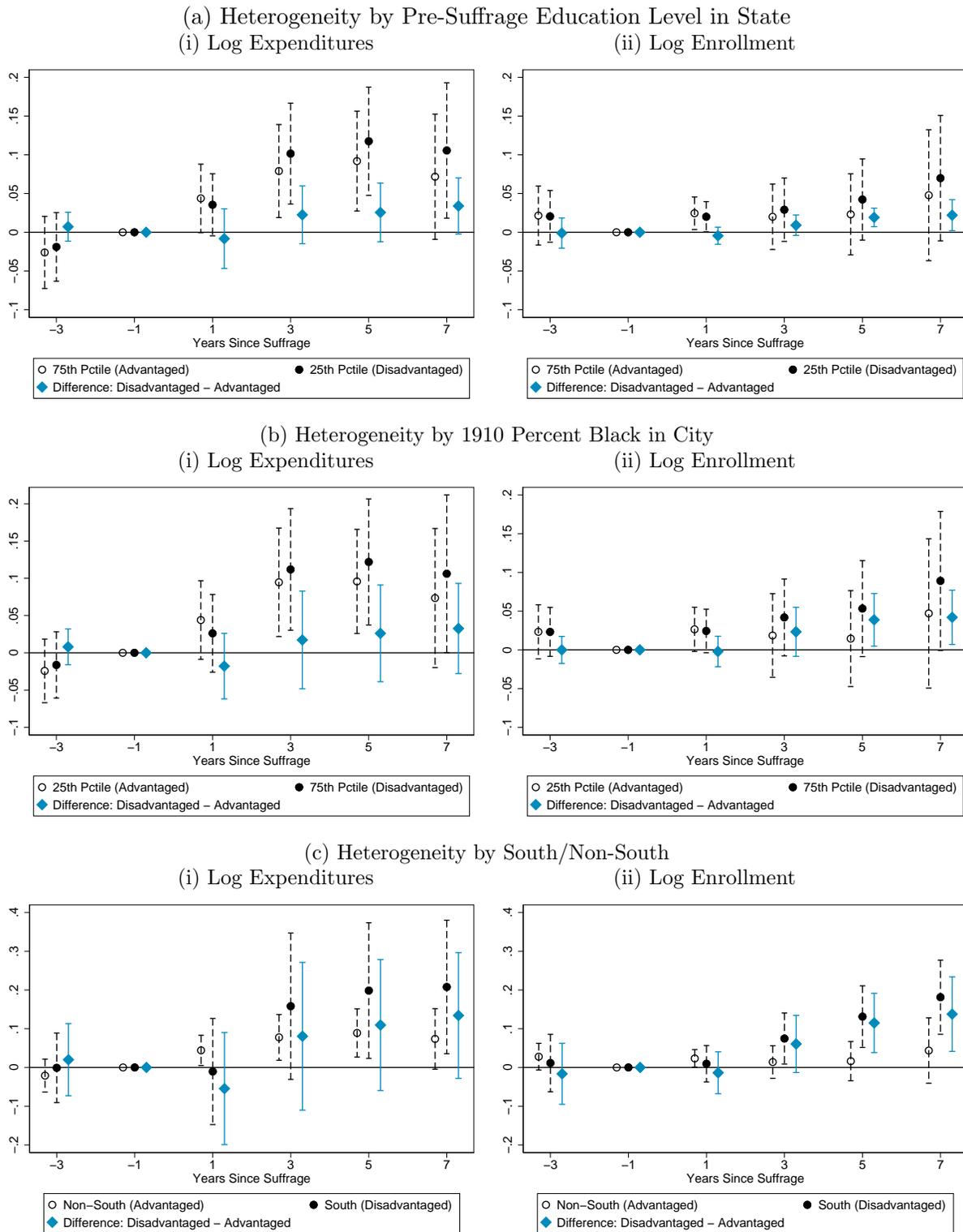
(a) Blacks



(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 5: Effect of Suffrage on City Level Expenditures and Enrollment



Notes: These figures are obtained from event study specifications that analyze the effect of suffrage on log expenditures and log enrollment, and that include an interaction between academic years since suffrage and one of our three measures of advantage. The figures show the implied effects of suffrage for the 75th and 25th percentile of each of our continuous measures of status - education and share black - and for the South and non-South, as well as their difference. All regressions include controls for state-level characteristics, and city and academic year fixed effects. Estimates are weighted using city population in 1910, and standard errors are clustered on state. The sample consists of all cities with available expenditure, revenue and enrollment data, which we observe for at least 7 years, and which are not outliers. Source: 1909 to 1911 and 1913 to 1915 Report of the Commissioner of Education, and 1917 to 1927 Biennial Survey of Education for cities with populations of 10,000 and over. * p<0.10, ** p<0.05, *** p<0.01.

A Empirical Appendix: Further Results

Table A.1: Correlation between Exposure to Suffrage and Predicted Education Using State Covariates and Progressive Laws

	State Covariates		Include Prog. Laws	
	(1) White	(2) Black	(3) White	(4) Black
Suff Share 0-15	0.068 (0.288)	0.134 (0.378)	0.015 (0.425)	0.369 (0.477)
Observations	704	704	704	704

This table contains results obtained from regressions when the dependent variable is predicted years of education in each state and year of birth and the main independent variable is exposure to suffrage between ages 0 and 15. We obtain predicted years of education as the fitted values from a regression of mean education in each state and cohort on the state covariates at birth shown in Table 1, excluding mortality and number of schools, using only observations prior to suffrage. For columns 3 and 4, we add indicators for the presence of a number of progressive policies at birth (worker’s compensation, prohibition, women’s minimum wage, mother’s pension, women’s club chapter, maximum hour law) to the prediction step. The coefficients in the table come from regressions that include controls for demographics and state-level characteristics, as in the main analysis, as well as state fixed effects, birth year fixed effects, state linear trends, and region-by-birth year fixed effects. Regressions are weighted by population, and standard errors are clustered at the state level. We exclude states that passed suffrage prior to 1900. Source: Mean education estimated in 1940 to 1960 censuses; for state covariates and progressive laws, see notes of Tables 1 and C.4. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2: Effect of Suffrage on Years of Education -
Effects Beyond Age 15

	All	Whites	Blacks
Suff Share 0-15	0.131 (0.241)	0.098 (0.234)	0.868*** (0.278)
Suff Share 16-22	-0.006 (0.079)	-0.016 (0.076)	0.027 (0.356)
Suff Share 23-30	-0.046 (0.116)	-0.034 (0.116)	-0.492 (0.512)
Mean Education	9.647	9.967	6.810
R-Squared	0.194	0.124	0.215
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. 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.

Table A.3: Effect of Suffrage on Years of Education -
Differential Effects by Region

	All	White	Black
Suff Share 0-15 x Northeast	0.394** (0.181)	0.424** (0.185)	1.737** (0.674)
Suff Share 0-15 x Midwest	-0.145 (0.244)	-0.148 (0.240)	0.983*** (0.332)
Suff Share 0-15 x South	1.260** (0.511)	0.959** (0.453)	0.816** (0.376)
Suff Share 0-15 x West	0.456** (0.191)	0.486** (0.203)	12.890*** (4.192)
Mean Education	9.647	9.967	6.810
P-Value NE=MW=S=W	0.069	0.082	0.033
P-Value NE=MW=S	0.032	0.044	0.500
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. 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 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. The bottom rows of the table test the hypothesis that the effects are equal for all four regions (NE, MW, S, W) or for all regions except the West, since there we have some concerns about overfitting for blacks in the West. For reference, the number of observations for whites (blacks) in the NE, MW, S, and W, respectively, is: 397,080 (7,381); 509,551 (7,946); 421,211 (140,982); 66,013 (537). The average years of education for whites (blacks) pre-suffrage in the NE, MW, S, and W, respectively, is: 9.3 (7.9); 9.1 (7.8); 8.0 (5.1); 9.1 (7.9). Source: 1940-1960 decennial censuses. * p<0.10, ** p<0.05, *** p<0.01.

Table A.4: Effect of Suffrage on Likelihood of Positive Income - Interactions with South and with Pre-Suffrage Education Levels

	All	Whites	Blacks
<i>A: Interaction with South</i>			
Suff Share 0-15	0.001 (0.005)	0.003 (0.005)	0.116*** (0.038)
Suff Share 0-15 * South	0.007 (0.041)	0.012 (0.040)	-0.171** (0.071)
Mean Y	0.634	0.628	0.692
Observations	1633910	1457463	171569
<i>B: Interaction with Pre-Period Education</i>			
Suff Share 0-15	-0.021 (0.020)	0.134* (0.074)	-0.160 (0.145)
Suff Share 0-15 x Pre-Period Average Education	0.003 (0.002)	-0.014* (0.008)	0.028 (0.019)
Mean Y	0.634	0.628	0.692
Observations	1633903	1457463	171567

Notes: This table contains results obtained when the dependent variable is likelihood of positive income, and the 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 between 30 and 60 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 A.5: Effect of Suffrage on Log Infant Mortality -
Interactions with South and with Pre-Suffrage Education Levels

	(1)	(2)	(3)
<i>A: All</i>			
Post Suffrage	-0.058 (0.036)	-0.046 (0.038)	-0.350** (0.145)
Post Suffrage x South		-0.086** (0.037)	
Post Suffrage x Pre-Period Average Education			0.032* (0.016)
Mean Y	8.052	8.052	8.052
Observations	846	846	846
N States	43	43	43
<i>B: Whites</i>			
Post Suffrage	-0.092** (0.043)	-0.077* (0.044)	-0.607** (0.240)
Post Suffrage x South		-0.099** (0.042)	
Post Suffrage x Pre-Period Average Education			0.056** (0.027)
Mean Y	7.869	7.869	7.869
Observations	810	810	810
N States	43	43	43
<i>C: Blacks</i>			
Post Suffrage	-0.116 (0.081)	-0.055 (0.096)	-0.861*** (0.206)
Post Suffrage x South		-0.363*** (0.132)	
Post Suffrage x Pre-Period Average Education			0.101*** (0.027)
Mean Y	4.774	4.774	4.786
Observations	754	754	752
N States	43	43	43

Notes: The dependent variable is log infant mortality. Post suffrage is a dummy variable that takes the value of one if the state passed suffrage by the current year. We include interactions between post suffrage and either South (column 2) or pre-suffrage education levels (column 3). Pre-suffrage education is calculated for each state (and race for Panels B and C) as the average education in that sample among individuals age 16 and above in the year that suffrage was passed. All regressions include controls for state-level characteristics, state and year fixed effects, and state linear time trends. Estimates are weighted using population weights, and standard errors are clustered on the state. We exclude states that passed suffrage prior to 1900. Source: 1900 to 1932 mortality records by state, age, race, and gender from the Centers for Disease Control and Prevention. * p<0.10, ** p<0.05, *** p<0.01.

Table A.6: Effect of Suffrage on Log Expenditures and Enrollment –
Heterogeneity Across Advantaged and Disadvantaged Areas

	Expenditures			Enrollment		
	Pre-Ed	1910 Black	South	Pre-Ed	1910 Black	South
<i>Years Relative to Suffrage</i>						
-3 Years	0.104 (0.167)	-0.025 (0.022)	-0.021 (0.022)	0.003 (0.172)	0.023 (0.018)	0.028 (0.018)
1 Years	-0.107 (0.345)	0.046 (0.028)	0.044** (0.020)	-0.057 (0.099)	0.027* (0.015)	0.023* (0.012)
3 Years	0.488 (0.342)	0.093** (0.038)	0.078** (0.030)	0.186 (0.119)	0.016 (0.028)	0.014 (0.021)
5 Years	0.557 (0.347)	0.093** (0.036)	0.089*** (0.032)	0.371*** (0.111)	0.011 (0.032)	0.016 (0.026)
7 Years	0.687** (0.337)	0.070 (0.048)	0.074* (0.040)	0.448** (0.177)	0.043 (0.050)	0.044 (0.043)
-3 Years * Pre-Characteristic	-0.014 (0.019)	0.094 (0.143)	0.020 (0.047)	0.002 (0.019)	-0.001 (0.104)	-0.016 (0.040)
1 Years * Pre-Characteristic	0.016 (0.038)	-0.209 (0.263)	-0.055 (0.074)	0.009 (0.011)	-0.025 (0.117)	-0.014 (0.028)
3 Years * Pre-Characteristic	-0.044 (0.037)	0.202 (0.392)	0.080 (0.097)	-0.018 (0.013)	0.272 (0.189)	0.061 (0.038)
5 Years * Pre-Characteristic	-0.050 (0.038)	0.306 (0.388)	0.109 (0.086)	-0.037*** (0.012)	0.454** (0.203)	0.115*** (0.039)
7 Years * Pre-Characteristic	-0.066* (0.036)	0.382 (0.361)	0.134 (0.083)	-0.043** (0.020)	0.491** (0.209)	0.138*** (0.049)
Obs	5183	2453	5183	5183	2453	5183
Pre-X Mean	8.93	0.08	0.19	8.93	0.08	0.19
Pre-X 25th Pct	8.83	0.01		8.83	0.01	
Pre-X 75th Pct	9.35	0.09		9.35	0.09	
N Cities	523	233	523	523	233	523
N States	42	37	42	42	37	42

Notes: This table contains results obtained when the dependent variables are log expenditures and log enrollment, and the independent variables of interest are academic years since suffrage interacted with one of our three measures of advantage, as listed in each column. All regressions include controls for state-level characteristics, and city and academic year fixed effects. Estimates are weighted using city population in 1910, and standard errors are clustered on state. The sample consists of all cities with available expenditure, revenue and enrollment data, which we observe for at least 7 years, and which are not outliers. Source: 1909 to 1911 and 1913 to 1915 Report of the Commissioner of Education, and 1917 to 1927 Biennial Survey of Education for cities with populations of 10,000 and over. * p<0.10, ** p<0.05, *** p<0.01.

Table A.7: Effect of Suffrage on Years of Education –
Mandatory vs Not Mandatory States

	All	Whites	Blacks
Suff Share 0-15	0.095 (0.208)	0.083 (0.202)	0.760** (0.327)
Suff Share 0-15 x Mandatory States	0.335*** (0.108)	0.192 (0.122)	0.637*** (0.231)
Mean Education	9.647	9.967	6.810
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. 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 A.8: Effect of Suffrage on Years of Education –
By Whether Individual Migrated From State of Birth

	Whites			Blacks		
	All	Non Movers	Movers	All	Non Movers	Movers
Suff Share 0-15	0.099 (0.202)	0.029 (0.223)	0.244 (0.151)	0.993*** (0.262)	1.378*** (0.413)	0.637 (0.483)
Mean Education	9.967	9.743	10.447	6.810	6.319	7.505
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 A.9: Effect of Suffrage on Years of Education -
Sensitivity to Measure of Exposure

	All	Whites	Blacks
Suffrage by 15	0.013 (0.019)	0.004 (0.020)	0.314*** (0.057)
Mean Education	9.647	9.967	6.810
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 A.10: Effect of Suffrage on Years of Education -
Sensitivity to Census

	1940	1950	1960	1950, 1940 Pop	1960, 1940 Pop
<i>A: Blacks</i>					
Suff Share 0-15	0.126 (0.281)	1.237** (0.540)	1.483*** (0.450)	2.816** (1.265)	1.149** (0.458)
Mean Education	6.009	6.984	7.272	6.426	6.502
Observations	61004	22447	73577	15839	50924
<i>B: Whites</i>					
Suff Share 0-15	0.098 (0.162)	0.355 (0.233)	-0.090 (0.221)	0.329* (0.189)	-0.056 (0.199)
Mean Education	9.567	10.056	10.173	9.704	9.735
Observations	509583	204510	679762	148663	483804

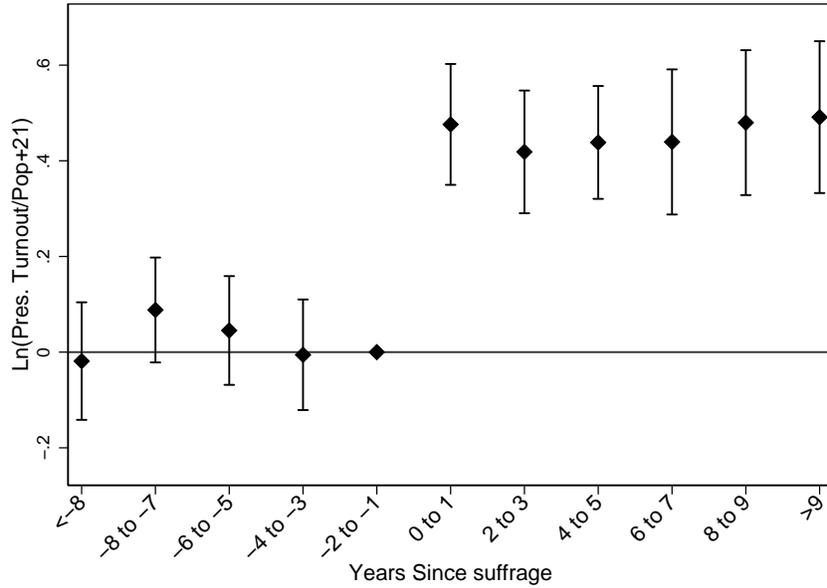
Notes: This table contains results obtained when the dependent variable is years of education and the main independent variable is suffrage exposure, which is defined as the share of time between birth and age 15 that an individual was exposed to a suffrage law in his state of birth. All regressions 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 A.11: Effect of Suffrage on Years of Education - Sensitivity to State Controls

	All	Whites	Blacks
<i>A: Baseline: State Controls At Birth</i>			
Percent of 0-15 Treated	0.128 (0.211)	0.099 (0.202)	0.993*** (0.262)
<i>B: Substitute Cumulative State Controls 0-15</i>			
Percent of 0-15 Treated	0.128 (0.211)	0.197 (0.139)	1.027*** (0.343)
<i>C: Substitute Pre-State Controls*Birthyear</i>			
Percent of 0-15 Treated	0.128 (0.211)	0.004 (0.239)	1.033*** (0.246)
<i>D: Drop Controls for Compulsory Schooling</i>			
Percent of 0-15 Treated	0.128 (0.211)	0.099 (0.202)	0.993*** (0.262)
<i>E: Add Controls for Progressive Laws</i>			
Percent of 0-15 Treated	0.128 (0.211)	0.099 (0.202)	0.993*** (0.262)
<i>F: Add Trend in Pre-Education</i>			
Percent of 0-15 Treated	0.128 (0.211)	0.099 (0.203)	0.971*** (0.256)

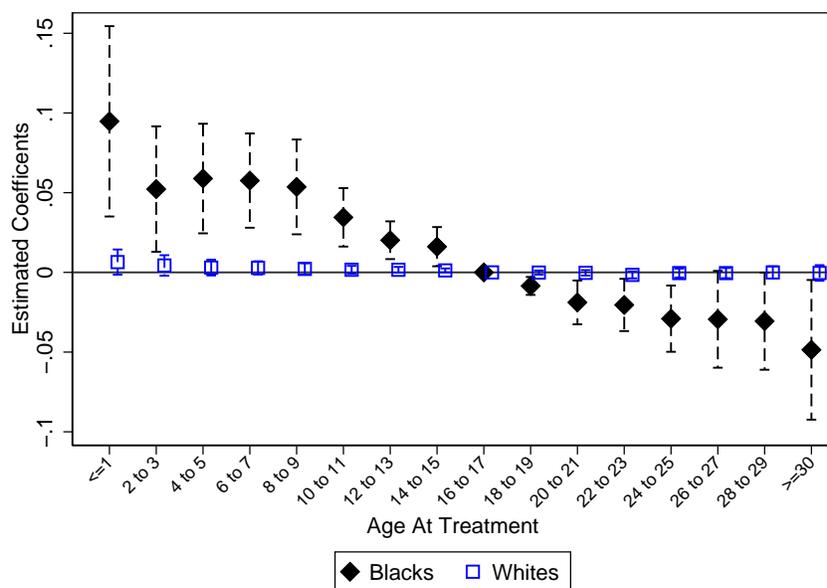
Notes: This table contains results obtained when the dependent variable is years of education and the main independent variable is suffrage exposure, which is defined as the share of time between birth and age 15 that an individual was exposed to a suffrage law in his state of birth. Each panel and column presents estimates from separate regressions, see text for details. All regressions include controls for demographics, 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.

Figure A.1: Effect of Suffrage on Presidential Turnout



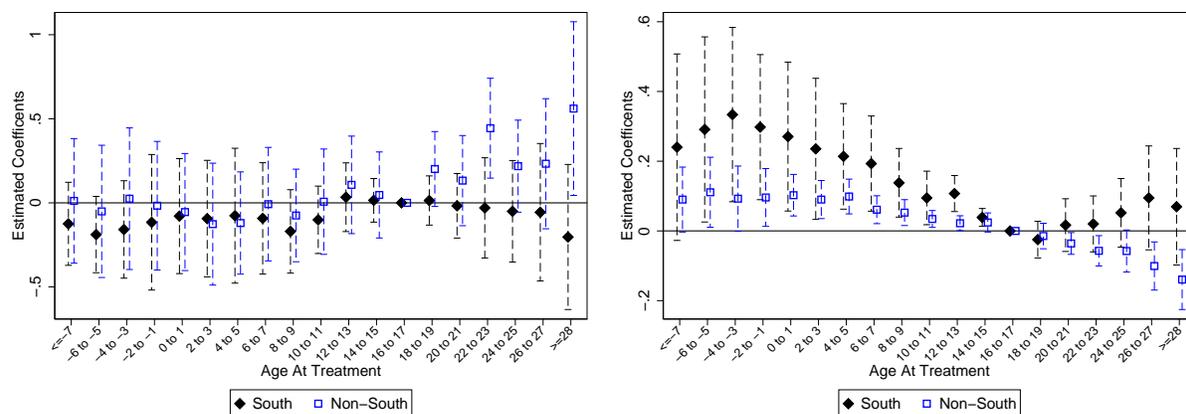
Notes: This figure plots the estimated coefficients obtained from an event study specification that analyzes the effect of suffrage on state-level presidential turnout, defined as the natural logarithm of total number of votes at the presidential elections divided by the voting eligible age, 21+. We control for state and year fixed effects, weight the estimates using population weights, and cluster the standard errors at the state level. The two years prior to the passage of suffrage are the omitted category, so estimates are relative to that point. 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 A.2: 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 16 to 17 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 A.3: Effect of Suffrage at Each Age of First Exposure on Log Income, by South/Non-South

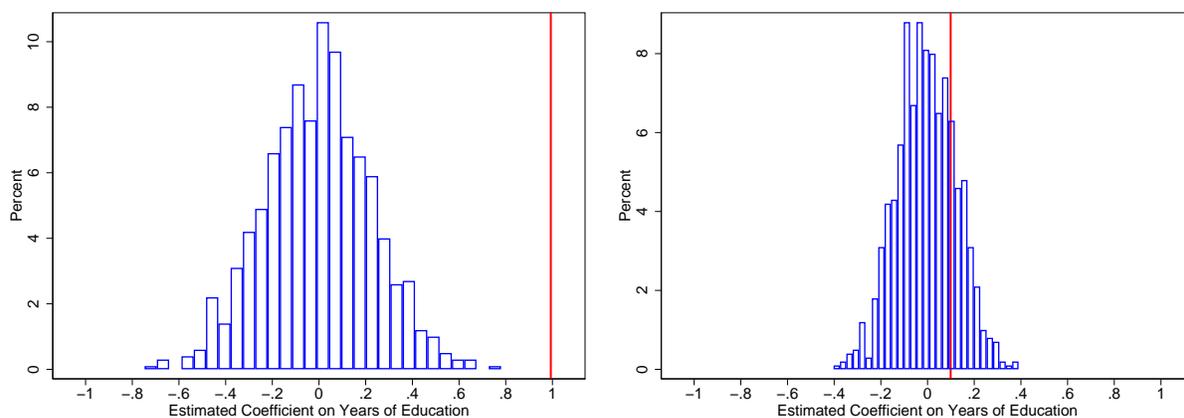


(a) Blacks

(b) Whites

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 log income, and includes an interaction between the age at treatment dummies and whether the state of birth is in the South or Non-South, 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 16 to 17 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 between 30 and 65 years old at the time of observation. We exclude states that passed suffrage prior to 1900. Source: 1940-1960 decennial censuses.

Figure A.4: Effect of Placebo Suffrage Laws on Years of Education, By Race



(a) Blacks

(b) Whites

Notes: These figures plot the distribution of the estimated difference-in-differences coefficients on suffrage exposure obtained from 1000 repetitions where we randomly assign a year of suffrage between 1910 and 1920 to each state. The red line indicates the estimated effect when we use the real suffrage laws. 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. 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.

B Data Appendix

B.1 Voter Turnout

Voter turnout data are obtained from the data series: “Electoral Data for Counties in the United States”, provided by ICPSR, see <http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/8611>. Population over age 21 is estimated using decennial census data (Ruggles et al., 2010). We use linear interpolation to obtain population estimates between censuses.

B.2 State Controls

We source these measures from a combination of Lleras-Muney (2002)³⁶ and the ICPSR data series “Historical, Demographic, Economic, and Social Data: The United States”.³⁷ 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 1880-1914.³⁸

B.3 Compulsory Schooling

We obtain data on compulsory schooling requirements from 1990 to 1939 from Goldin and Katz (2003) and from 1940 to 1944 from Acemoglu and Angrist (2001), and assign the relevant laws following Stephens and Yang (2014). Since we only have these laws beginning in 1910, we assume that cohorts that turned 14 before 1910 (born between 1880-1896) were exposed to the 1910 laws. The measure of compulsory attendance, CA is defined for each cohort c born in state s as follows: $CA_{cs} = \min\{DropoutAge_{cs} - EnrollmentAge_{cs}, Years\ of\ School\ Needed\ to\ Dropout_{cs}\}$, where each of the components of CA are determined by the prevailing laws in state s in the year that c turns 14. Child labor, CL_{cs} is defined as: $CL_{cs} = \max\{WorkPermitAge_{cs} - EnrollmentAge_{cs}, Education\ for\ Work\ Permit_{cs}\}$. See Stephens and Yang (2014) for more detail.

³⁶These data are compiled from a number of sources; see <http://www.econ.ucla.edu/alleras/research/data.html> for more detail.

³⁷See: <http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/2896>.

³⁸This 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.

B.4 Rosenwald Initiative

We aggregate the county-level Rosenwald student exposure measure from Aaronson and Mazumder (2011) to generate a measure of the average reach of Rosenwald over the childhood of each individual. For further detail about this data, visit <http://www.jstor.org/stable/10.1086/662962>.

B.5 Mortality Statistics

The *Mortality Statistics* were originally published by the U.S. Bureau of the Census, but we obtained pdf files from the Centers for Disease Control and Prevention. Original pdf's can be downloaded from http://www.cdc.gov/nchs/products/vsus/vsus_1890_1938.htm. We used optical character recognition (OCR) to convert the pdfs to Excel files and a research assistant manually checked the values.

B.6 City-level Education Data

During our period of interest, city-level education statistics were published either in the Report of the Commissioner of Education (RCE) (annually, academic years 1909/10 until 1915/16) or in the Biennial Survey of Education (BSE) (biennially, from 1917/18 on). We downloaded pdfs for all of the years we digitized, 1906 to 1911 and 1913 to 1928, from HathiTrust Digital Library (<https://www.hathitrust.org/>), except 1923/24, which we scanned ourselves for better image quality. We selected three tables to digitize in each year: the school census, which has enrollment and attendance; the “receipts of school systems”, which contains sources of revenue; and the expenses and outlays table, which has total current expenditures. We digitized this information for all cities with populations over 10,000 using an external digitization service.

We then took several steps to obtain our final city panel data. First, we harmonized the naming conventions across years by manually looking for cases where the name changed very slightly across years (e.g. “Windham (P. O. Willimantic)” became “Windham (P. O., Willimantic)”). Second, we manually identified cities that merged or split, and generated consistent names for these cities. Third, since the reporting categories for local revenue varied across years, we aggregated these to create a comparable measure over time. We define revenue from local sources as total revenue minus revenue from the state.

C Online Appendix

Table C.1: Effect of Suffrage on Years of Education -
Keep Early States

				Whites		Blacks	
	All	Whites	Blacks	Males	Females	Males	Females
Percent of 0-15 Treated	0.165 (0.196)	0.130 (0.185)	1.036*** (0.263)	0.124 (0.184)	0.132 (0.202)	1.490** (0.606)	0.612*** (0.223)
Mean Education	9.671	9.987	6.813	9.873	10.097	6.403	7.175
R-Squared	0.195	0.125	0.215	0.135	0.116	0.208	0.213
Observations	1581878	1419943	157155	701079	718864	74410	82745

Notes: The sample includes all states, including those that passed suffrage prior to 1900. Suff Share 0-15 is defined as the share of time between birth and age 15 that suffrage law passed in an individual's 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. Source: 1940-1960 decennial censuses. * p<0.10, ** p<0.05, *** p<0.01.

Table C.2: Effect of Suffrage on Years of Education -
Individuals 25 or Older Only

				Whites		Blacks	
	All	Whites	Blacks	Males	Females	Males	Females
Percent of 0-15 Treated	0.145 (0.205)	0.108 (0.197)	1.225*** (0.254)	0.081 (0.194)	0.131 (0.219)	1.770*** (0.625)	0.687*** (0.254)
Mean Education	9.568	9.888	6.706	9.777	9.995	6.320	7.048
R-Squared	0.192	0.122	0.213	0.133	0.112	0.207	0.210
Observations	1424162	1276966	143098	629908	647058	67855	75243

Notes: The sample excludes states that passed suffrage prior to 1900, and is composed of individuals age ≥ 25 . Suff Share 0-15 is defined as the share of time between birth and age 15 that suffrage law passed in an individual's 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. Source: 1940-1960 decennial censuses. * p<0.10, ** p<0.05, *** p<0.01.

Table C.3: Effect of Suffrage on Years of Education -
Insensitivity of Results to the Addition of Controls

	(1)	(2)	(3)	(4)	(5)	(6)
<i>A: All</i>						
Suff Share 0-15	0.462** (0.217)	0.442* (0.231)	0.484** (0.181)	0.482** (0.191)	0.480** (0.191)	0.128 (0.211)
Mean Education	9.647	9.647	9.647	9.647	9.647	9.647
Observations	1555475	1555475	1555475	1555475	1555475	1555475
<i>B: Whites</i>						
Suff Share 0-15	0.417* (0.224)	0.369 (0.236)	0.423** (0.172)	0.426** (0.179)	0.425** (0.179)	0.099 (0.202)
Mean Education	9.967	9.967	9.967	9.967	9.967	9.967
Observations	1393855	1393855	1393855	1393855	1393855	1393855
<i>C: Blacks</i>						
Suff Share 0-15	1.502*** (0.312)	1.470*** (0.279)	1.312*** (0.235)	1.275*** (0.226)	1.262*** (0.239)	0.993*** (0.262)
Mean Education	6.810	6.810	6.810	6.810	6.810	6.810
Observations	157028	157028	157028	157028	157028	157028
BSt,BY FE	Yes	Yes	Yes	Yes	Yes	Yes
BSt Trends		Yes	Yes	Yes	Yes	Yes
State Controls			Yes	Yes	Yes	Yes
Compulsory and Rosenwald				Yes	Yes	Yes
CYxBY FE					Yes	Yes
RegionxBY FE						Yes

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 C.4: Correlation between Timing of Suffrage and Progressive Era Laws

Year of Workers' Compensation Law	-0.145 (0.102)					
Year of Prohibition		0.040 (0.082)				
Year of Women's Minimum Wage Law			0.382 (0.488)			
Year of State Mother's Pension Law				0.389 (0.282)		
Year of State General Federation of Women's Clubs Chapter					0.696 (0.417)	
Year of Women's Maximum Hour Law						-0.270 (0.391)
Observations	47	29	15	46	48	40

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 the year of the listed Progressive era law. All regressions include region fixed effects. Sources: Suffrage laws are from Lott and Kenny (1999) and Miller (2008). Data on mother's pension laws, state General Federation of Women's Clubs chapter establishment, women's maximum hour laws, women's minimum wage laws from Skocpol (1992); workers' compensation law dates from Kantor and Fishback (1996); and state prohibition laws from Depew et al. (2013).

Table C.5: Correlation between Timing of Suffrage and New Deal Spending

	Outcome = Year Suffrage		
	(1)	(2)	(3)
Total Relief per Capita (1967 dol.)	0.018 (0.027)		
Direct Relief per Capita (1967 dol.)		0.015 (0.039)	
Work Relief per Capita (1967 dol.)			0.031 (0.070)
Observations	36	36	36
Region FE	Yes	Yes	Yes
X mean	133	74	32

Notes: This table contains results obtained when the dependent variable is the year that suffrage was approved in each state and the main independent variable is the generosity of New Deal relief spending in the state, the total (1967 \$) spent between 1929 and 1940 normalized by the 1930 population (Fishback et al., 2007). All regressions include region fixed effects. Total relief is the sum of direct and work relief, and is sourced from data made available from Fishback et al. (2007). The sample excludes states that passed suffrage prior to 1900. Suffrage laws are from Lott and Kenny (1999) and Miller (2008). * p<0.10, ** p<0.05, *** p<0.01.

Table C.6: Correlation between Suffrage and Compulsory Schooling Laws

	Comp. Attendance	Child Labor
Post-Suffrage Law	-0.532 (0.476)	0.408 (0.426)
Observations	1440	1440

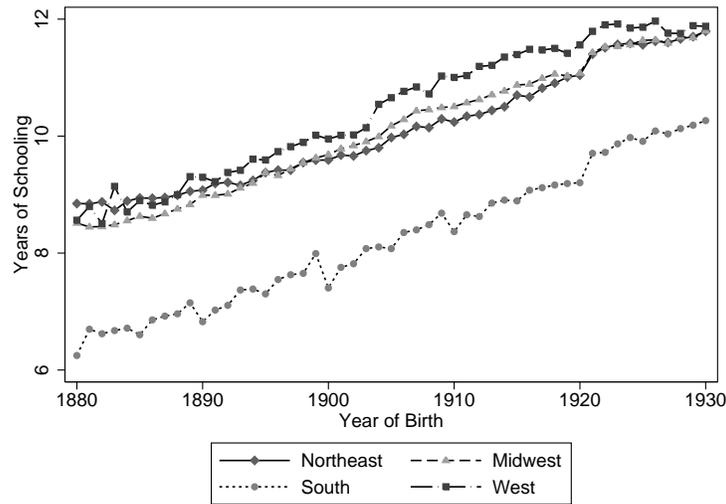
Notes: This table contains results obtained when the dependent variable is the parameter of a compulsory schooling or child labor law and the main independent variable is an indicator for whether suffrage was passed in the state. All regressions include state fixed effects, state trends, and region by year fixed effects. Standard errors are clustered at the state level. Sources: Data used in Goldin and Katz (2003) obtained from the website of Claudia Goldin. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.7: Correlation between Suffrage and the Elements of Compulsory Schooling Laws

	Age Leave Sch.	Age Work	Min Sch. to Work	Min Sch. to Drop
Post-Suffrage Law	-0.191 (0.397)	0.438 (0.807)	-0.334 (0.533)	7.133 (4.772)
Observations	1440	1440	1424	1434

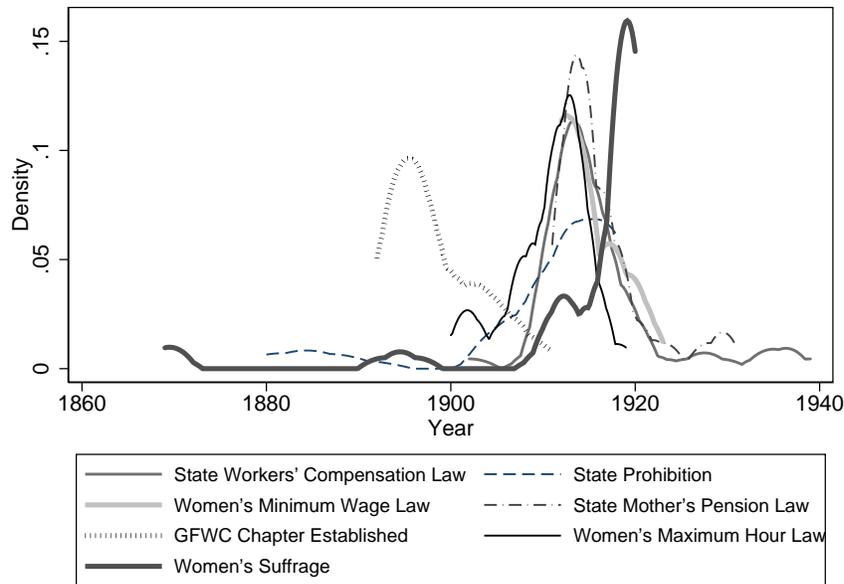
Notes: This table contains results obtained when the dependent variable is the parameter of a compulsory schooling or child labor law and the main independent variable is an indicator for whether suffrage was passed in the state. All regressions include state fixed effects, state trends, and region by year fixed effects. Standard errors are clustered at the state level. Sources: Data used in Goldin and Katz (2003) obtained from the website of Claudia Goldin. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.1: Average Educational Attainment Across Cohorts and Regions



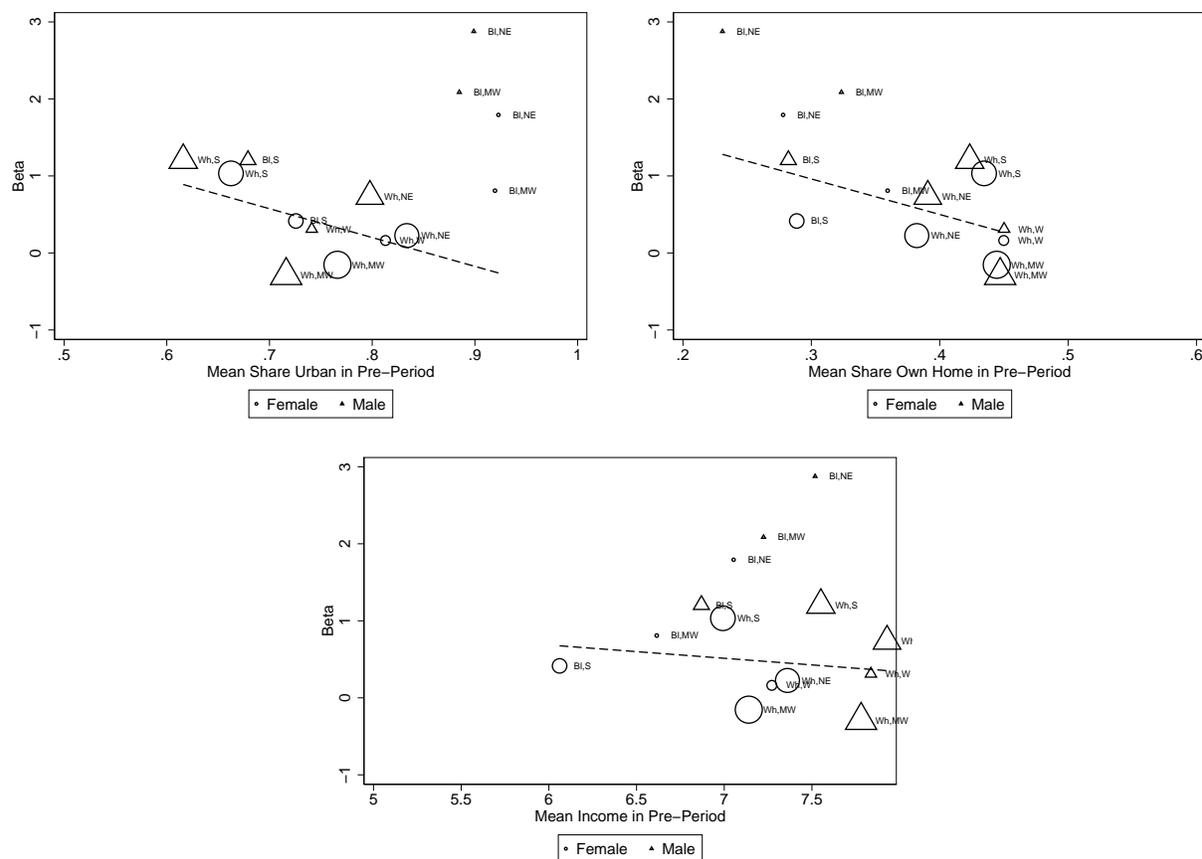
Notes: This figure plots the (weighted) average number of years of completed schooling for U.S. born residents by birth cohort and region. The sample consists of individuals born between 1880 and 1930, and that are at least 20 years old at the time of observation. We exclude states that passed suffrage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

Figure C.2: Progressive Era Events over Time



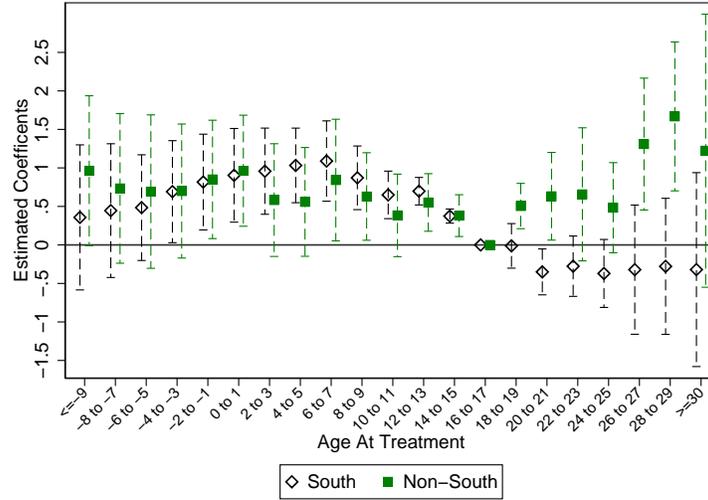
Sources: Suffrage laws are from Lott and Kenny (1999) and Miller (2008). Data on mother's pension laws, state General Federation of Women's Clubs chapter establishment, women's maximum hour laws, women's minimum wage laws from Skocpol (1992); workers' compensation law dates from Kantor and Fishback (1996); and state prohibition laws from Depew et al. (2013).

Figure C.3: Subgroup Averages of Pre-Suffrage Disadvantage and the Estimated Effects of Suffrage on Years of Education

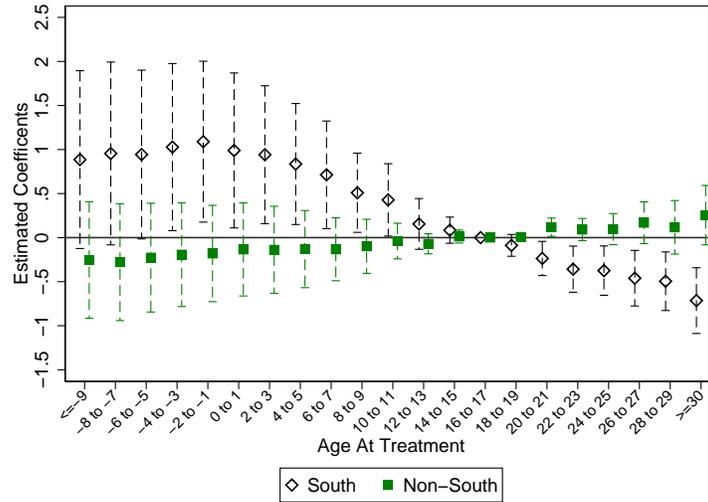


Notes: To create these figures, we first estimate specifications that analyze the effect of suffrage exposure on educational attainment separately for demographic groups defined according to region of birth, race and gender. We then plot the estimated coefficients along with the three different average pre-suffrage measure of disadvantage for each demographic group, with the circle/triangle size representing the number of observations in each 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 C.4: Effect of Suffrage at Each Age of First Exposure for Whites on Years of Education, By South/Non-South



(a) Blacks



(b) Whites

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 and includes an interaction between the age at treatment dummies and whether the state of birth is in the South or Non-South, 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 16 to 17 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.