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Women's Suffrage and Children's Education†

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While a growing literature shows that women, relative to men, prefer greater investment in children, it is unclear whether empowering women produces better economic outcomes. Exploiting plausibly exogenous variation in US suffrage laws, we show that exposure to suffrage during childhood led to large increases in educational attainment for children from disadvantaged backgrounds, especially Blacks and Southern Whites. We also find that suffrage led to higher earnings alongside education gains, although not for Southern Blacks. Using newly digitized data, we show that education increases are primarily explained by suffrage-induced growth in education spending, although early-life health improvements may have also contributed. (*JEL* H75, I21, I22, J13, J15, J16, N32)

Gender differences in social preferences have been documented across many settings. For instance, there is evidence that women favor higher levels of investment in children, are more prosocial, and are more egalitarian.[1] Thus, it has been argued that female empowerment could lead to greater investment in human capital and increase economic development (Duflo 2012). However, there is little empirical evidence that increases in women's influence lead to different policies or better economic outcomes. In this paper, we provide new evidence of the impact of enfranchising women in the United States on the human capital of the next generation.

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[1] See, e.g., Duflo (2003); Lundberg, Pollak, and Wales (1997); Andreoni and Vesterlund (2001); Croson and Gneezy (2009); Alesina and Giuliano (2011); Ashok, Kuziemko, and Washington (2015).

The passage of women's suffrage laws in the United States has been hailed as a "turning point in our Nation's history" (Obama 2013): newly empowered women exercised their vote in large numbers (Lott and Kenny 1999) and post-suffrage legislators voted for more progressive policies and more funds for public health, social services, and (at least in the South) education (Lott and Kenny 1999, Miller 2008, Carruthers and Wanamaker 2015). There is also evidence that changes in health spending led to a decline in child mortality (Miller 2008).

Building on these earlier findings, we estimate the *long-run* impact of exposure to suffrage during childhood on education and labor market outcomes. To examine mechanisms, we use newly digitized historical records to estimate the short-run impact of suffrage on local education expenditures, school enrollment, and infant mortality.

Theoretically, it is ambiguous whether women's suffrage would lead to long-term gains in education, and for whom. While studies show that public spending on health, social services, and, to some extent, education increased *on average*, these effects may not lead to education gains if spending was increased for populations with few gains from spending. For instance, some evidence suggests that the returns to education spending are largest for those with few resources (Jackson, Johnson, and Persico 2015; Lafortune, Rothstein, and Schanzenbach 2018; Carruthers and Wanamaker 2013). Thus, suffrage may have had little impact on education if newly empowered women advocated that funding only go to populations with greater political or economic influence (e.g., more-educated Whites).

Our analysis exploits variation in the timing of suffrage laws, as in Lott and Kenny (1999) and Miller (2008), and differential exposure to the laws across cohorts, in a difference-in-difference design. Thus, for each state, we compare the outcomes of individuals who were *older than* age 15 when suffrage was passed (comparison) to individuals who were *at most* age 15 when suffrage was passed (treated). The staggered adoption of suffrage laws allows us to include detailed birth-cohort-by-region fixed effects to control for other potential changes across cohorts. This design relies on the assumption that differential exposure to suffrage—among individuals born in the same region and birth cohort—only affects outcomes through suffrage-induced changes in human capital inputs.

We provide several pieces of evidence in favor of the identifying assumption. First, we use event studies to show that suffrage is not correlated with trends in our outcomes of interest. Second, we show that the timing of suffrage is not correlated with pre-suffrage state demographics (trends or levels), or the passage of other progressive laws or education policies. Thus, any remaining confounders would need to be correlated with the timing of suffrage and outside of these factors and the extensive set of state covariates and progressive laws that we control for in our analysis.

We bring together multiple historical data sources to examine the impacts of suffrage. We use the 1940, 1950, and 1960 decennial censuses to estimate the effects of suffrage on individuals' education and labor market outcomes. Moreover, we digitized city-level records from 1909 to 1927 to examine national impacts on school enrollment, school expenditures, and state and local revenue for schooling.

Finally, we digitized counts of deaths for each state, age, race, and gender from 1900 to 1932 to estimate impacts on mortality.

Our primary finding is that exposure to women's suffrage during childhood led to meaningful gains in educational attainment, particularly for children from economically disadvantaged backgrounds. Full exposure to suffrage between the ages of 0 and 15 increased educational attainment by 0.9 years for Black children (who averaged 5.2 years of education pre-suffrage) and by one year for White children from the South (who averaged 8.0 years of education pre-suffrage.) In contrast, full exposure to suffrage led to between 0.3 and 0.5 years of additional education for White children in the Northeast and in the West (who averaged 9.0 years of education pre-suffrage). Consistent with this, suffrage had significantly larger effects on the education of youth from states that had low average levels of education pre-suffrage.

We also find that suffrage increased earnings along with education, although not for Southern Blacks. These patterns could reflect the fact that low school quality together with labor market discrimination reduced the returns to schooling for this group (see, e.g., Card and Krueger 1992).

To explain the large and heterogeneous effects on education, we show that suffrage had disparate impacts on school spending across the country. On average, suffrage led to a 13.9 percent increase in schooling expenditures within five years of passage. However, expenditures post-suffrage increased more in percent terms in the South and more generally in states with lower average pre-suffrage education. For example, spending on education increased by nearly twice as much in the South (23 percent) as in the non-South (13 percent). Drawing on previous estimates of the impact of education spending (Jackson, Johnson, and Persico 2015), we calculate that growth in education spending could explain 110 percent, 95 percent, and 73 percent of the education impacts for Southern Whites, Southern Blacks, and Blacks nationwide. Altogether, this suggests that education spending is very likely to be the primary mechanism for our education effects.

We find less evidence that improvements in early-life health—proxied by reductions in infant mortality—can explain post-suffrage gains in education. In particular, contrary to our impacts on education, mortality improvements post-suffrage are small for Blacks outside the South and are significant for Whites in the non-South. Thus, while improvements in health may have contributed to increases in schooling, this does not appear to be a main channel for education gains.

We bolster the plausibility of these results by providing additional evidence on political mechanisms and on the mechanics of spending in the South. First, we show that suffrage increased voting for progressive bills in the Senate both by increasing "yay" votes by legislators elected post-suffrage and by reducing "nay" votes by incumbent legislators. Thus, suffrage appears to have swayed the votes of *existing* legislators in addition to changing the composition of legislators (Morgan-Collins 2021). Second, we show that in the South, 10 percent growth in local education spending translated to 10 percent growth in spending in White schools and 5.2–6.3 percent growth in spending in Black schools. This indicates that even in the presence of racism in the South, the growth in education

spending post-suffrage is likely to have been at least modestly passed on to Black schools.

This paper touches on several literatures. First, we add to the work on the effects of women's political representation on public spending and children's outcomes. The closest studies in this area show that electing women to public office in India leads to greater investment in female-preferred public goods and increases primary educational attainment (Chattopadhyay and Duflo 2004, Clots-Figueras 2012), but that electing women to local office in the United States has little impact on spending (Ferreira and Gyourko 2014). Our findings suggest that women's voting en masse can have wide-reaching effects on children's outcomes akin to the best-case effects of increasing women's representation.

Second, we add to the set of studies on the impact of suffrage on policy outcomes and children's well-being (see Lott and Kenny 1999, Miller 2008, Carruthers and Wanamaker 2015, summarized above, and Aidt and Dallal 2008). We make three additions to this literature. First, we show that childhood exposure to suffrage led to large and lasting improvements in human capital. We document that impacts on education were widespread (geographically and racially), larger in less-advantaged areas, and extended beyond the children of those with the greatest gains in political power post-suffrage.

Additionally, we provide the first national accounting of the impacts of suffrage on local education spending. This builds on Carruthers and Wanamaker (2015), who broke ground by showing that suffrage increased local schooling expenditures in three Southern states. We show that geographic variation in the impact of suffrage on spending is important for explaining heterogeneous effects on education.

As a third, more minor, contribution, we provide evidence that suffrage changed the voting behavior of incumbent politicians toward abstention. We show that this is a likely mechanism for the rise in the passage of progressive bills post-suffrage shown in earlier work (Miller 2008), together with the election of new and less conservative legislators (Morgan-Collins 2021). It also provides empirical evidence that women's lobbying changed legislators' voting behavior, which is frequently mentioned in historical accounts.

Finally, we contribute to the literature on the impact of public spending on educational attainment (e.g., Jackson, Johnson, and Persico 2015; Hyman 2017). The fact that we find similar gains in education for Blacks and Whites in the South, despite the fact that Whites likely experienced almost twice the increase in education expenditures, provides additional evidence of potential diminishing returns to education spending (Carruthers and Wanamaker 2013).

The remainder of the paper continues as follows. In Section I, we provide institutional background on the passage of suffrage laws and present evidence of the short-run impacts of suffrage on voting and the passage of progressive bills. We discuss the expected effects of suffrage on education in Section II. Section III describes our data sources, followed by an overview of our empirical strategy in Section IV. We present our main results on the long-run effects of suffrage in Section V, followed by evidence on mechanisms in Section VI, and robustness checks in Section VII. We conclude in Section VIII.

I. Background on Women's Suffrage

At the turn of the twentieth century, women had few, if any, political rights (Baker 1984, Keyssar 2000).[2] Thus, the passage of suffrage laws in the early part of the century provided the first opportunity for many women to enter into the political sphere and influence political outcomes and local policy.

De jure, suffrage laws applied to all women. But de facto, Black women's participation was severely limited by the presence of literacy tests, poll taxes, and fear of retribution in the South (Cascio and Washington 2014, Naidu 2012). Hence, access to voting following suffrage was largely limited to White women. In spite of this, suffrage led to a significant shift in the electorate and in policymakers' priorities, as we discuss below.

Figure 1 illustrates the timing of the passage of the first suffrage law in each state using data from Lott and Kenny (1999) and Miller (2008).[3] In our analysis, we exploit the variation in suffrage laws passed after 1900, beginning with Washington in 1910.[4] Between 1910 and 1919, an additional 24 states passed suffrage laws. In 1920, the United States ratified the Nineteenth Amendment, which guaranteed that sex could not be used as a basis of exclusion from voting. Three-fourths of the 48 states voted in support of ratification, and the remaining 12 states, labeled as "Mandated" in Figure 1, adopted it by mandate in 1920.

Historical accounts suggest that suffrage led to increased attention and support for policies targeting children's welfare. For example, one year after the passage of suffrage in North Carolina, "politicians passed unusually liberal appropriations for the states educational and child-caring institutions" (Schuyler 2006, 171). These accounts and subsequent research suggest that this was due both to the election of new, progressive legislators (Morgan-Collins 2021) and to women's lobbying of existing legislators. In particular, women's lobbies created the perception of a close political alignment among its members, which appears to have made politicians more willing to support progressive legislation (Lemons 1973, Moehling and Thomasson 2012).[5] We provide empirical evidence supporting the narrative that suffrage had an impact on legislators' voting patterns (see Section IA).

Because suffrage increased voting primarily for White women, one might expect gains primarily for White children. However, there are reasons to suspect that there

[2] The most common form of political voice for women was the right to vote for school boards, although anecdotally school elections had low female participation (Youmans 1921). School board voting rights were extended during the mid- to late nineteenth 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.

[3] Following 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 anticipation of the 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). See Teele (2018) and Keyssar (2000) for greater detail on the passage of suffrage laws.

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

[5] As an example of the successful organization of women, in the Virginia gubernatorial election in 1921, former antisuffragist George Tucker was handily defeated due to opposition from the League of Women Voters. The League instead endorsed the opposing candidate, Elbert Trinkle, for his support of progressive legislation, including improved roads to allow rural children to attend school. Further, Westmoreland Davis, the previous Virginia governor who had endorsed Tucker, lost his bid for the Senate (Walker, Dunn, and Dunn 2003).

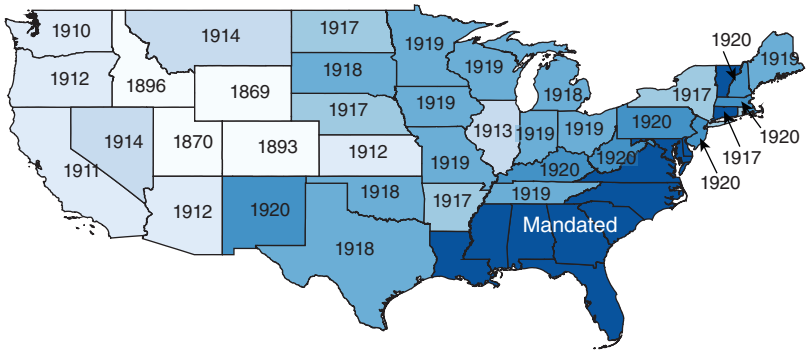

FIGURE 1. TIMING OF SUFFRAGE LAWS

Notes: Suffrage laws are obtained from Lott and Kenny (1999) and Miller (2008), and the year in each state indicates the first suffrage law passed in the state. "Mandatory states" implemented suffrage as a result of the Nineteenth Amendment in 1920. See text for further detail.

could be some gains for Black children as well. While Black women in the South could not directly lobby legislators, qualitative evidence shows that Black women's organizations had some (infrequent) success in convincing White women's organizations to request funds for Black youth. For instance, this form of cooperation led to the founding of the Fairworld School for Negro Girls in South Carolina, and of a training school for "delinquent African American boys" in North Carolina (Schuyler 2006, 156–58). Similar examples also led to funding for homes for delinquent Black children (Schuyler 2006, 171). In that sense, suffrage may have reduced the costs of gaining access to policymakers not only for White women, but also for Black women (at least for issues related to child welfare.)

A second channel for gains for Black children is that a portion of suffrage-induced education spending may have simply been "passed through" to Black schools through fixed allocation mechanisms (or norms). We quantify the likely magnitude of such pass-through in Section VI.

A. *Voting, Progressive Legislation, and Public Spending Post-Suffrage*

The passage of suffrage laws has been linked to an immediate and significant change in voter participation in gubernatorial elections nationwide and other elections in specific states (Lott and Kenny 1999, Corder and Wolbrecht 2016); and in the passage of progressive legislation in Congress, particularly in the Senate (Lott and Kenny 1999, Miller 2008). Because these mechanisms are relevant for impacts on public spending and education, we briefly reexamine these outcomes to verify the immediacy of the effects on voter turnout and the source of rapid effects in the Senate. We then provide background on the current evidence on the impacts of suffrage on education spending.

First, we examine the relationship between the passage of suffrage laws and voting in presidential elections, which typically have higher turnout than other races (Cascio and Washington 2014). We regress the log of the number of votes relative

to the population over 21 on event-time indicators for the years around the passage of suffrage (using the timing of suffrage laws in Figure 1) together with state and election-year fixed effects. Online Appendix Figure A.1 shows that voter participation increased by 45 log points, or 56 percent, in the year after suffrage. Consistent with prior work, this suggests that suffrage significantly expanded the voting population, although these estimates indicate that women voted at roughly half the rate of men.

Second, we consider whether suffrage had impacts on the progressive voting of *existing* legislators, which could have hastened the post-suffrage change in policies. In particular, we measure the impact of suffrage on progressive voting (i) by all senators (as in Miller 2008), which includes changes in the composition of elected senators, and (ii) by senators present before and after suffrage ("incumbents.")[6]

Panel A of online Appendix Table A.1 shows that overall, conditional on state and year fixed effects, suffrage leads to a significant rise in the propensity to vote *in favor* of a progressive bill, consistent with Miller (2008). Panel B shows that, conditional on individual and year fixed effects, incumbents are more likely to *abstain* from votes on progressive bills post-suffrage. This comes from a decline in voting *against* progressive bills.[7] Thus, suffrage appears to have both increased voting *for* progressive bills by newly elected senators, which drives the overall effect, and reduced voting *against* progressive bills by incumbents. The presence of both of these channels helps to explain the rapid change in legislation following suffrage.

Multiple studies show that suffrage led to an increase in public spending, particularly on health (by up to 36 percent) and social programs (by up to 24 percent) (Lott and Kenny 1999, Miller 2008). However, there is inconsistent evidence of impacts on education spending. While Lott and Kenny (1999) and Miller (2008) find no effect of suffrage on state-level education spending across the country, Carruthers and Wanamaker (2015) find positive effects of suffrage on county-level education spending for three Southern states, with larger impacts for White schools relative to Black schools.

While these earlier studies of education spending provide important insights into the impacts of suffrage, each of them has critical limitations. First, Lott and Kenny (1999) and Miller (2008) focus on state spending, which accounted for less than 20 percent of local education expenditures during this period (Benson and O'Halloran 1987). Thus, their null estimates may not capture the full effects of suffrage. Second, the estimates in Carruthers and Wanamaker (2015) may be specific to the South (or a subset of the South). Third, the estimates in Carruthers and Wanamaker (2015) rely on variation in the White female share across counties, which could be measured with error and may not necessarily correspond to a larger "dose" of suffrage.[8] This could lead to attenuation or reduced precision

[6] In particular, we first regress the share of progressive bills for which a Senator votes in a particular way (yay, nay, or abstains) on an indicator for the years after suffrage along with year and state fixed effects. We then add individual fixed effects when we examine incumbents. For details on the coding of progressive bills and voting data, see online Appendix B.

[7] Event studies in online Appendix Figure A.2 illustrate that these changes to incumbent voting took place immediately after suffrage. They also show that the results are robust to including region-by-year fixed effects and state trends, although the inclusion of trends sometimes introduces pre-trends where there are none.

[8] For instance, counties with a smaller share of White women may experience a bigger impact of suffrage if the discrepancy between men and women's preferences for spending is larger in those counties.

in the estimates. To fill these gaps, in Section VI, we revisit the effects of suffrage on education expenditures using new historical data on *city-level* spending that has *national* coverage.

II. Expected Effects of Suffrage

We hypothesize that increases in public spending on health and education are two main channels by which suffrage would have impacted education. In this section, we briefly discuss the predicted education effects of these channels, other potential mechanisms, and testable hypotheses.

The predicted effect of greater health spending on education is ambiguous. On the one hand, reductions in the duration or severity of sickness spells would be expected to lead to increases in school attendance post-suffrage. On the other hand, reductions in mortality (as shown in Miller 2008) would lead to a greater presence of weak survivors, which would tend to reduce impacts on completed education. We can not observe reductions in sickness to examine the first channel; however, in Section VI we use bounding methods to estimate the role of survivors in our education effects.

The predicted effect of education spending on completed years of education is generally positive. However, there is evidence that education gains may be *larger* for groups with lower pre-suffrage spending. As one example, studies of the philanthropic "Rosenwald Initiative"—which was closely timed with suffrage—show that Black children benefited more from increases in school spending than White children (Aaronson and Mazumder 2011, Carruthers and Wanamaker 2013). Similarly, school spending increases have been shown to have strong impacts on education outcomes in districts with low levels of spending (Jackson, Johnson, and Persico 2015; Hyman 2017; Lafortune, Rothstein, and Schanzenbach 2018). To test for this form of heterogeneity in our setting, we estimate differential impacts of suffrage by race as well as by pre-suffrage average education in the state.[9]

Finally, changes in women's bargaining power in households or in girls' aspirations could also lead to improvements in education. We expect changes in bargaining power to primarily benefit White children, since Black women were largely disenfranchised. Suffrage could also make girls more motivated to remain in school (e.g., from a desire to become an educated voter, changes in gender norms, or greater investment from parents). Thus, we test for larger education gains for girls relative to boys post-suffrage.

III. 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

[9] Although we discuss the impacts of health and education spending separately, there could be interactions between these effects; positive if healthy students are more attentive, or negative if health improvements lead to classroom overcrowding.

capital. For brevity, we provide an overview of the data sources here and include detailed descriptions in online Appendix B.

Long-Run Outcomes.—We analyze the effect of women's suffrage laws on children's educational and labor market outcomes using two samples constructed from the 1920–1930 and 1940–1960 US decennial censuses, respectively. The data for each census year are a 1 percent representative sample of the US population and are publicly available through the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al. 2020). Relevant for our research design, the samples contain information on individuals' year and state of birth, as well as years of completed education and earnings for each individual (for the 1940–1960 censuses) and literacy (for the 1920–1930 censuses).[10]

Our main analysis sample for educational attainment and labor market outcomes includes individuals who are (i) at least 20 years old at the time of the census and (ii) were born between 1880 and 1930 in states that adopted suffrage between 1910 and 1920.[11] This ensures that for each state that passed suffrage between 1910 and 1920, we observe individuals born between 30 years before suffrage and up to 10 years after suffrage.[12] For our analysis of impacts on literacy, we expand the sample to include individuals ages 15 and above (Aaronson and Mazumder 2011).

State-Level Controls.—For controls, we merge on state-by-cohort measures of the demographic and economic composition of the state and measures of exposure to other education policies. These 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 control for the state-by-cohort compulsory attendance requirement, the child labor educational requirement (following Stephens and Yang 2014), and exposure to the Rosenwald Initiative during childhood (following Aaronson and Mazumder 2011).

Mortality Counts.—To perform detailed analyses of the impact of suffrage on mortality, we digitized annual counts of deaths by state, age, race, and gender from 1900 to 1932 from the *Mortality Statistics*. The data include all deaths from participating states, which grew from 10 states in 1900 to 48 states by 1932.

Education Spending and School Enrollment.—To examine education spending and enrollment patterns, we digitized city-level enrollment, education expenditures, and revenue sources from the Report of the Commissioner of Education and Biennial Survey of Education for cities with populations of 10,000 and over. Each

[10] We drop observations where years of education or income were imputed by the census. The 1950 census only collected years of education for one individual per household, so we have fewer observations for that year.

[11] Hence, 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 US states by 1920, and for which we do not have either a date of suffrage or state-level controls.

[12] Our results remain the same if we keep states that passed suffrage prior to 1910 or keep individuals over age 25—see Section VII.

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). We digitized the annual reports from 1909 to 1911 and 1913 to 1915 and the biennial reports from 1917 and 1927 (12 academic years in total).

For our main analyses, we keep cities for which we have information on enrollment, spending, and revenues, and which have available information for at least 7 of the 12 years. This helps achieve balance across years and across outcomes.[13] We drop cities that we identify as outliers, defined as having enrollment and spending above the ninety-ninth percentile. Our final dataset contains city-year observations with enrollment, spending, and revenue from 1909 through 1927 for 42 states and 523 cities. This is the most comprehensive data on education spending used to analyze the impacts of suffrage to date.[14]

IV. Empirical Strategy

We estimate the effect of suffrage using a difference-in-differences framework that compares the outcomes of cohorts who in the year that suffrage was passed in their state of birth were beyond schooling-age ("comparison"), at schooling age or not yet in school ("partially exposed"), and not yet born ("fully exposed"). We define exposure using state of birth because it is less likely to be an outcome of suffrage than state of residence and provides a reasonable proxy for childhood location.

We first estimate the effects of voting laws for each age of exposure to suffrage in an event-study model. This allows us to visually inspect whether cohorts exposed at older ages (who we argue should be less affected by suffrage) have small treatment effects, and it allows us to show the pattern of treatment effects among children exposed at younger ages.

We estimate

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

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. The terms 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, and a vector of

[13] We have also run the results requiring cities to appear in 8, 9, or 10 years or including cities that appear in fewer years. The results remain the same.

[14] Other sources of education spending begin many decades after suffrage, such as the Census of Governments (which begins in 1972) or the Historical Database on Individual Government Finances (which begins in 1967) (Jackson, Johnson, and Persico 2015).

state-cohort controls, Z_{cs}, which includes state demographics, employment, wages, and education policies in the year of birth, c. Region by cohort fixed effects, ϕ_{rc}, control for unobservable differences across regions over time that may be related to the clustered passage of suffrage laws.[15] The variation used for identification of the coefficients of interest, β_a, is thus generated by differential exposure to suffrage within cohorts and across states (within regions), as well as within states and across cohorts.

To increase power, we group together treated ages greater than or equal to 30 and treated ages less than or equal to -10. We also group together pairs of consecutive ages of treatment, such that individuals treated at ages -10 and -9 are both assigned $AgeTreat_{cs} = -9$, individuals treated at ages -8 and -7 are both assigned $AgeTreat_{cs} = -7$, and so forth.[16] All coefficients are measured relative to the omitted category, which is treatment 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 generalized difference-in-differences approach, as follows:[17]

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

where $SuffExp015_{cs}$ 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.[18] We define the relevant age of exposure ending at the typical school-leaving age, 15 years, which we calculate 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.

[15] Importantly, this helps control for important differences in education outcomes across regions (see online Appendix Figure A.3.) When we exclude these fixed effects, the average effect for Whites increases by four-fold, and the estimate becomes statistically significant—see online Appendix Table A.2. Interestingly, the standard errors change very little when we include these fixed effects, though, which suggests that we are not losing excessive identifying variation.

[16] Grouping in this manner also allows us to estimate state trends and region by birth cohort fixed effects without dropping additional event-time dummies.

[17] Although we refer to this as the average effect, β_1 may not precisely correspond to the average treatment effect in the population due to uneven weighting across states (Goodman-Bacon 2018, de Chaisemartin and D'Haultfoeuille 2019, Borusyak and Jaravel 2016). However, reassuringly, our results are similar across the difference-in-difference and our event studies, which are less susceptible to weighting issues (Goodman-Bacon 2018).

[18] Formally,

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

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

A. *Identifying Assumptions and Testable Implications*

The identifying assumptions for this model are that (i) suffrage laws are not correlated with an unobserved trend in education outcomes across states, and that (ii) there are no confounding events with suffrage. We address assumption (i) in part by including linear state-specific trends to minimize the influence of unobserved trends. Nonetheless, there may remain some (potentially small) correlations with unobserved time-varying factors that remain threats to our identification.

We provide evidence of the plausibility of assumption (i) by testing whether suffrage was preceded by a systematic change in any of a number of state policies, demographics, or economic activity. To diagnose the importance of any preexisting trend, we estimate a modified event-study model, in which we replace the pre-suffrage indicators with a linear trend, as follows:

$$(4) \qquad Y_{st} = \alpha_0 + \alpha_1 \mathit{YearRelSuffrage}_{st} + \sum_{y=1}^{10} \beta_y \left(\mathit{YearRelSuffrage}_{st} = y\right) + \gamma' Z_{st} + \delta_s + \phi_{rt} + \epsilon_{st}.$$

Here, Y_{st} is a state- (or city-) characteristic in state (or city) s and $\mathit{YearRelSuffrage}_{st}$ is a linear trend in years since suffrage in state s, and $\sum_{y=1}^{10}\left(\mathit{YearRelSuffrage}_{st} = y\right)$ are indicators for each year after suffrage. The coefficient of interest is α_1. Because we include indicators for each year after suffrage, α_1 is mechanically only identified only from the data prior to suffrage and therefore gives the slope of Y_{st} over time *prior to suffrage*. We include state (or city) fixed effects, region by year fixed effects, and the same state time-varying controls as in equation (2).[19]

To reduce noise in the estimation of the pre-trend (α_1), we estimate this using the sample of states (or cities) for which we have at least three years of data prior to suffrage. Thus, we analyze outcomes for 31 states for the majority of the state-level regressions, and for 2,357 cities across 41 states for the city-level regressions.

Table 1 shows our estimates of α_1. Of the 19 outcomes we analyze, just four are significant at the 5 percent level: log manufacturing wages per earner ($\alpha_1 = 0.02$), log doctors per capita ($\alpha_1 = 0.04$), log White mortality under age 5 ($\alpha_1 = -0.056$), and log school enrollment ($\alpha_1 = -0.01$). Moreover, the direction of bias from these is not obvious. For instance, the effect of a reduction in White mortality or more doctors per capita on education could be negative due to the presence of weak survivors, or positive due to health improvements. Similarly, higher manufacturing wages could reduce school attendance through the substitution effect, or increase attendance through improvements in family income. Last, the slight negative trend in school enrollment is 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. Included in these

[19] We exclude any controls that are directly related to the outcome in order to increase our ability to detect a trend. To improve balance, we set $\mathit{YearRelSuffrage} = 10$ for all years at least 10 years after suffrage, and $\mathit{YearRelSuffrage} = -10$ for all years at least 10 years before suffrage. For the city-level outcomes, we also group together -10 and -9, -8, and -7, etc., since we only observe cities biennially.

Table 1—Trend in State and City Characteristics Prior to Suffrage

	Trend coefficient (1)	Standard error (2)	p-value (3)	Observations (4)	N states (5)
Percent White	0.253	0.282	0.377	357	31
Percent urban	−0.292	0.515	0.575	357	31
Percent foreign	0.229	0.147	0.131	357	31
ln population	−0.006	0.015	0.713	357	31
Percent emp. manuf.	−0.001	0.002	0.598	357	31
ln manuf. wage per earner	0.022	0.010	0.039	357	31
ln avg. farm value	0.006	0.032	0.854	357	31
ln tax-reported income per capita	0.095	0.065	0.153	357	31
ln number hospitals	−0.053	0.043	0.229	357	31
ln doctors per capita	0.040	0.011	0.001	357	31
ln mortality—Whites ages 0–5	−0.056	0.024	0.027	294	30
ln mortality—Blacks ages 0–5	−0.064	0.074	0.395	283	29
ln number of schools per capita	−0.052	0.052	0.327	357	31
Compulsory attendance	−0.097	0.294	0.744	357	31
Schooling for child labor	−0.566	0.366	0.132	357	31
Pred years ed—Whites (summary index)	−0.051	0.061	0.410	357	31
Pred years ed—Blacks (summary index)	−0.074	0.057	0.203	320	31
ln school enrollment (city data)	−0.009	0.003	0.009	2,357	41
ln school spending (city data)	0.000	0.010	0.972	2,357	41

Notes: This table presents results from 19 regressions, where the outcome is shown in the first column and the key coefficient of interest is on a trend in the number of years since suffrage. The regressions also include indicators for each year after suffrage, region-year fixed effects, state (or city) fixed effects, and state-year controls. Importantly, because we include indicators for each year after suffrage, the coefficient on the trend (shown in column 1) is identified only from presuffrage years, and therefore the p-value in column 3 can be interpreted as a test for whether there is a significant pre-trend for each outcome. "Pred years ed" is an education index generated by regressing the mean education for state-cohort cells presuffrage on state covariates (shown in the table) in the year of birth, and then obtaining fitted values for all state-cohort observations (separately for whites and blacks). For state-year cells where we do not observe mortality, we use a prediction that omits mortality as a covariate. The sample for each regression 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.

15 outcomes is a predicted education index for Whites and Blacks, which we create by regressing the mean education for state-cohort cells pre-suffrage on state covariates (shown in the table) in the year of birth, and then obtaining fitted values for all state-cohort observations.[20] Regressions are run separately for Whites and Blacks. The trend in this index is highly insignificant ($p = 0.41$ for Whites; $p = 0.20$ for Blacks). This is consistent with previous investigations that have shown few correlates of suffrage (King, Cornwall, and Dahlin 2005; Braun and Kvasnicka 2013; Miller 2008). Reinforcing this, in the next section we also find no trend in *observed* education across cohorts.

To complement this analysis, we also directly examine the correlations between the year of suffrage or whether a state passed suffrage in 1920 ("late") with pre-suffrage levels, pre-suffrage changes, and contemporaneous changes in state covariates. We do this for the set of states that passed suffrage after 1917 (as above) and the full set of states, and present the results in online Appendix Tables A.3 and

[20] For state-year cells where we do not observe mortality, we use a prediction that omits mortality as a covariate.

A.4, respectively. In 10 out of 11 specifications, we cannot reject the hypothesis that the covariates do not significantly predict the timing of suffrage.

One might also be worried that suffrage was bundled with other progressive era laws that could have improved education. Online Appendix Table A.5 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. 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 do not find evidence for this (see online Appendix Tables A.6, A.7, and A.8.)

V. 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 percent confidence intervals by age of treatment.

For Blacks, shown in panel A, we find large, positive, and statistically significant effects for children who were exposed to suffrage *prior to* age 15. Further, younger exposure is generally associated with larger increases in education. Exposure to suffrage between the ages of 12 and 15 leads to roughly one-quarter of a year of additional education, while exposure to suffrage between the ages of 0 and 5 leads to around three-quarters of a year of education. We do not find any differential impact of exposure within the ages of 0 to 5, suggesting that our effects are not driven by very-early-life exposure to suffrage.[21] Importantly, we find no effect of suffrage on the education of those that were exposed to suffrage *after* age 15. This is consistent with our hypothesis that the education of individuals who had already left school would be unaffected by the passage of suffrage.

In contrast, for Whites, shown in panel B, the effects hover at zero and are flat at all ages of treatment. One possible explanation for these null effects is that the marginal public dollar is less productive for relatively more advantaged populations (e.g., Carruthers and Wanamaker 2013). In Section VA, we test whether there are varying impacts *within* White and Black populations to probe whether less-advantaged White populations benefited more from suffrage.

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.[22] Additionally, the

[21] In online Appendix Table A.9 we test the marginal effect of exposure to suffrage between ages 11–15, 6–10, and 0–5 using a spline in exposure to suffrage. We find that the impact of an additional year of suffrage exposure is roughly 0.1 between the ages 6–15, and that, conditional on exposure at later ages, the effects of exposure between ages 0–5 has little additional impact on education outcomes.

[22] We formally test for an effect of suffrage beyond age 15 in online Appendix Table A.10 by including the effect of exposure between ages 16 and 22 and between 23 and 30 as additional covariates in the regression. We find an insignificant effect of suffrage exposure after age 15, while the coefficient on exposure between age 0 and 15 is similar to our base specification.

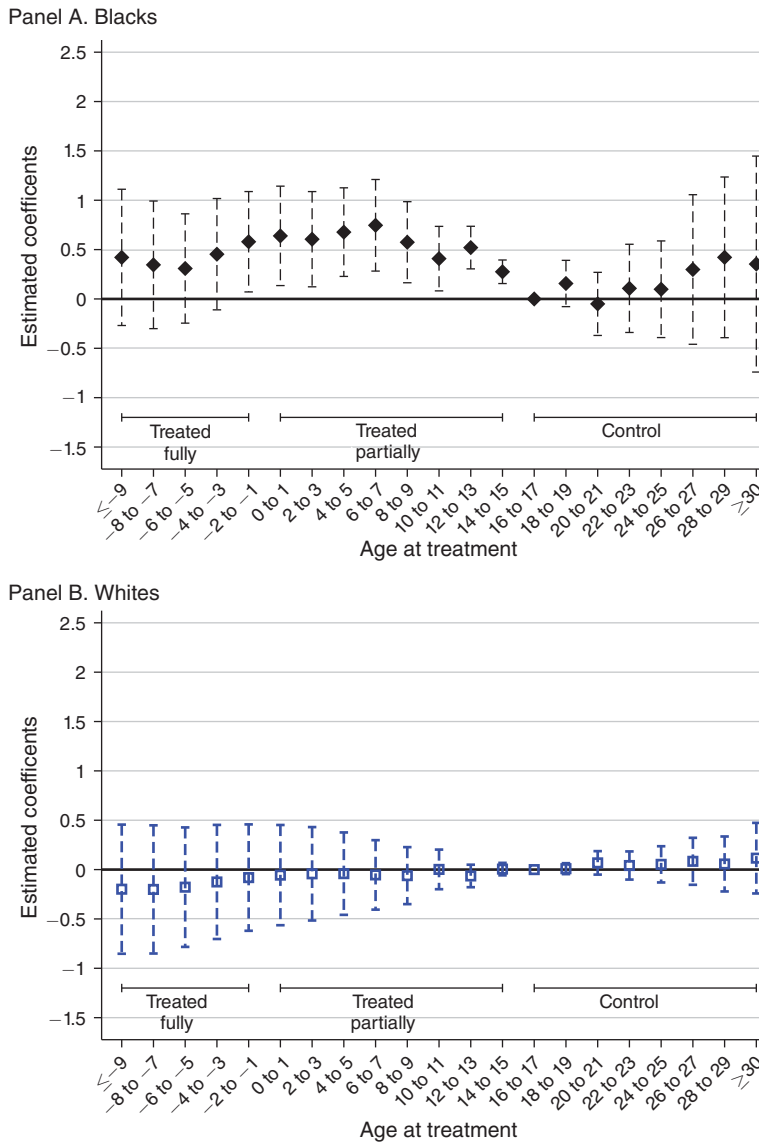

FIGURE 2. LONG-RUN EFFECT OF SUFFRAGE ON YEARS OF EDUCATION BY AGE OF EXPOSURE

Notes: This figure plots the estimated coefficients (and 95 percent confidence intervals) obtained from event study specifications where the outcome is 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 who are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900.

Source: 1940–1960 decennial censuses

Table 2—Long-Run Effect of Suffrage Exposure on Years of Education

				Whites		Blacks	
	All	Whites	Blacks	Males	Females	Males	Females
Suff share 0–15	0.091	0.062	0.884	0.027	0.092	1.259	0.551
	(0.203)	(0.197)	(0.295)	(0.189)	(0.221)	(0.693)	(0.270)
Mean education	9.647	9.967	6.810	9.850	10.078	6.400	7.171
Observations	1,555,475	1,393,855	157,028	688,363	705,492	74,351	82,677

Notes: This table presents results from regressions of completed years of education on suffrage exposure between ages 0–15 (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 White and Black coefficients are the same ($p < 0.045$). 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 who 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

increasing and then flat coefficients between the ages of 0 to 15 resemble 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, Johnson, and Persico 2015; Chetty and Hendren 2018), which bolsters our confidence in these results.

We present the difference-in-differences estimates for all, Whites, and Blacks in columns 1–3 of Table 2. On average, full exposure to suffrage between the ages of 0 to 15 leads to a statistically insignificant 0.09 increase in years of schooling. Consistent with our event studies, for Whites, full exposure to suffrage led to a statistically insignificant 0.06 year increase in education. For Blacks, full exposure to suffrage led to 0.88 years of additional education ($p < 0.01$). This effect is statistically significantly larger than the effect on Whites ($p < 0.045$), and represents a 13 percent gain relative to the average years of completed education for 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 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, we do not find larger impacts on the education of women. 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 cannot rule out that they are the same. For Blacks, we actually find a larger effect of suffrage exposure on men than on women (1.26 years compared with 0.55 years). This is potentially a reflection of the fact that men had lower human capital investments at baseline—reflected in lower average levels of education—which could result in a higher return to investment.

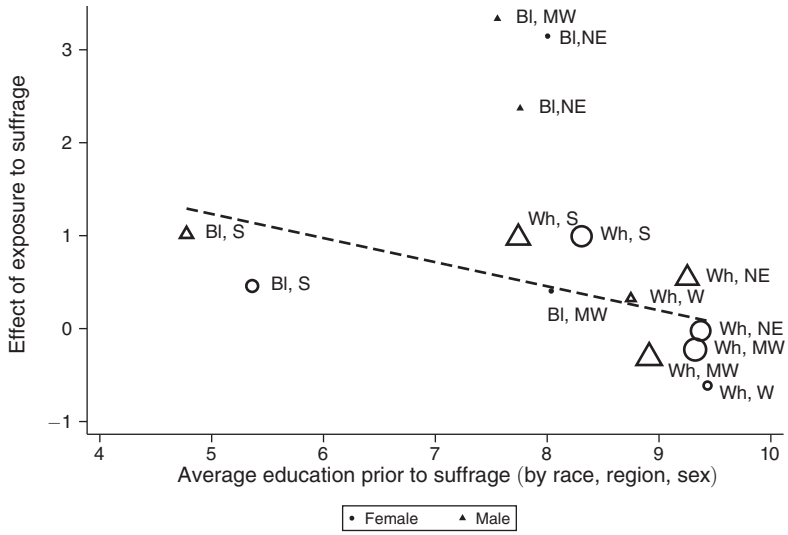

Figure 3. Heterogeneous Effects of Exposure to Suffrage (Ages 0–15) on Education By Presuffrage Average Education

Notes: This figure shows coefficients (on the *y*-axis) from regressions of educational attainment on exposure to suffrage between ages 0–15, estimated separately for groups defined according to region of birth, race, and gender. The *x*-axis shows the average presuffrage educational attainment (average attainment among individuals who were age 16 or older by the passage of suffrage in the state). Marker size for each group is proportional to 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 who are at least 20 years old at the time of observation. We exclude states that passed suffrage prior to 1900.

Source: 1940–1960 decennial censuses

A. *Sources of Treatment Effect Heterogeneity*

The fact that we find larger impacts of suffrage on the education of Blacks is consistent either with (i) suffrage leading to particularly large changes in Black communities, counter to historical narratives; or (ii) suffrage having a larger impact for communities with fewer resources. To distinguish between these explanations, we now examine whether suffrage had heterogeneous impacts by pre-suffrage education levels and within racial groups.[23]

Descriptively, Figure 3 plots the impact of suffrage for groups defined by region, race, and gender against the average level of education for each group prior to suffrage, which we measure using individuals who were at least age 16 at suffrage. It shows a clear negative relationship between the impact of suffrage and pre-suffrage

[23] We find similar patterns using other measures of socioeconomic status (see online Appendix Figure A.4).

education levels: groups that averaged 4 to 8 years of education pre-suffrage gained roughly one year of additional education post-suffrage, while groups that averaged 9 or more years education pre-suffrage experienced little or no gain.

Figure 3 also shows that the impacts of suffrage are present among low-educated Whites, in addition to Blacks. In particular, White children in the South, who averaged 8 years of education prior to suffrage, gained an additional 0.96 years in education (SE: 0.47) following suffrage, and Whites in the Northeast and West, who averaged 9.3 and 9.1 years of education prior to suffrage, gained an additional 0.54 (SE: 0.21) and 0.27 (SE: 0.17) years, respectively.[24,25]

Next, we formally estimate this relationship by adding to our baseline specification an interaction between suffrage exposure and the pre-suffrage average education in the state. We calculate pre-suffrage education separately for all, Whites, and Blacks.

Table 3 reports the the main effect of suffrage, i.e., the impact of suffrage for a group with zero pre-suffrage education, and the interaction with pre-suffrage education. For the whole sample, the coefficient on the main effect is 2.53 and the coefficient on the interaction is -0.28 ($p < 0.01$). This implies that the effect of full exposure to suffrage goes down by 0.28 years with every additional year of pre-suffrage education for a group.[26] Columns 2 and 3 of Table 3 shows a similar pattern *within* Whites and Blacks: the interaction with pre-suffrage education is -0.31 for Whites ($p < 0.01$) and -0.17 for Blacks ($p > 0.10$). These results are consistent with 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 higher level of education in that sample.

B. *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 estimate the effect of exposure to suffrage on one minus the cumulative distribution function of educational attainment (1-CDF) (Duflo 2001). This gives the impact of suffrage on the probability of having a level of education greater than a particular threshold. In practice, we estimate a series of regressions where the outcome is an indicator for whether the completed education of individual i is greater than p, where p takes on the discrete values from 0 to 17 (Almond, Hoynes, and Schanzenbach 2011; Duflo 2001).

[24] See online Appendix Table A.11 for the coefficients estimated for each region and race. We are able to reject that the effects for Whites across regions are the same ($p = 0.07$). 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.

[25] Online Appendix Figure A.5 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 to that of Blacks, with larger gains for those exposed at younger ages, and leveling off for those exposed by age 5. But White children in the South exposed between the ages of 15 and 30 also experience some small increases in education.

[26] 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.

Table 3—Long-Run Effect of Suffrage Exposure on Years of Education: Interaction with Presuffrage Education by State

	All	Whites	Blacks
Suffrage share 0–15	2.532	2.794	1.948
	(0.600)	(0.665)	(1.123)
Suffrage share 0–15 × pre-period education	−0.278	−0.305	−0.166
	(0.065)	(0.071)	(0.166)
Mean education	9.647	9.967	6.810
Observations	1,555,475	1,393,855	157,024

Notes: This table presents results from regressions of completed years of education on suffrage exposure between the ages of 0–15 and the interactions between suffrage exposure and average pre-suffrage education in each state (and race for columns 2 and 3). Presuffrage average education is calculated using individuals who were are least age 16 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 who 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

We plot the coefficients from this series of regressions in panel A (for Blacks) and panel B (for Whites) of Figure 4, together with distribution of educational outcomes pre-suffrage. For Blacks, we find that the impact of suffrage on education attainment is concentrated between 4 and 9 years of education. For Whites, we find small effects between 7 and 9 years of education. Relative to the baseline distributions, the impacts for both Blacks and Whites are clustered around the median for each group, which is 5 years for Blacks and 8 years for Whites. Thus, it appears that one of the main benefits of suffrage may have been to help raise the schooling of children who otherwise would have been at the bottom-to-middle of the education distribution.

C. *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.

Literacy.—We analyze effects on literacy as a proxy for whether suffrage led to increases in measurable skills. 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

[27] Among the 1900 cohort, Whites and Blacks had literacy rates above 98 percent and 82 percent, respectively (Collins and Margo 2006).

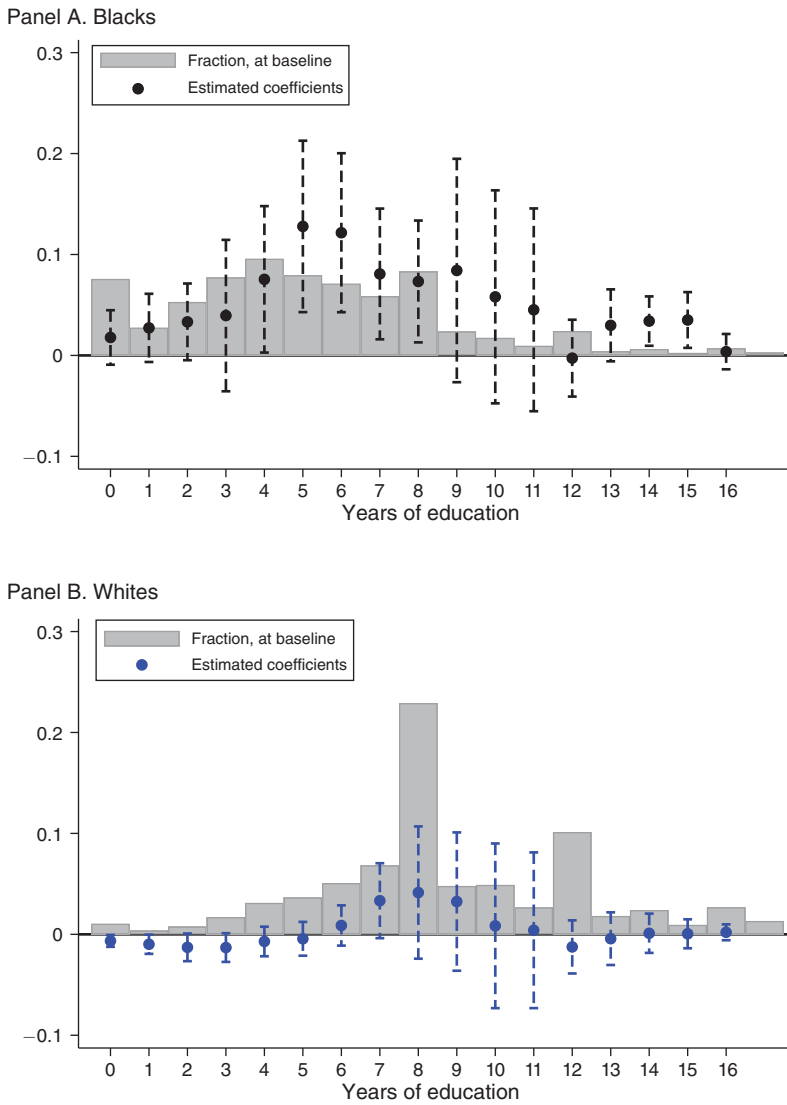

FIGURE 4. DISTRIBUTIONAL EFFECTS OF EXPOSURE TO SUFFRAGE (AGES 0–15) ON EDUCATION

Notes: These figures plot the estimated coefficients (and 95 percent confidence intervals) from a series of regressions of indicators for whether an individual obtained x or greater years of education (1-CDF), where x is represented on the x-axis, on suffrage exposure between ages 0–15. Regressions are estimated separately for Whites 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. For reference, we also show a histogram of the completed education of individuals who were exposed to suffrage after age 15, who serve as the comparison group in these regressions. The sample consists of individuals born between 1880 and 1930, and who 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

little variation, online Appendix Figure A.6 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.[28] While these results are measured with error, this provides suggestive evidence that suffrage led to improvements in literacy together with extended schooling.

Labor Market Outcomes.—Next, we analyze whether suffrage impacted labor market outcomes, including the likelihood that an individual is employed (which we define as having nonzero wage earnings), real wage earnings (in 1960 dollars and including zeros), and the log of real wage earnings.[29,30] Here we limit our sample to men and women between the ages of 30 and 65. Motivated by the heterogeneity in our education analysis, we allow the effect of exposure to suffrage on labor outcomes to vary for individuals in the South and by pre-suffrage education.

Panel A of Table 4 shows that full exposure to suffrage for Southern Whites led to a large and significant effect on log earnings (22 percent), and a positive effect on earnings ($170, $p > 0.10$), with no impact on employment.[31] We also find a significant effect on earnings for Blacks outside the South ($464), along with large increases in employment (12.7 p.p.). However, for Southern Blacks, we find an insignificant impact on employment and a marginally significant decline in earnings. The absence of labor market gains for this group is perhaps not entirely surprising—it is consistent with prior evidence of labor market discrimination and the low quality of education for this group (Card and Krueger 1992, Karbownik and Wray 2019, Bhalotra and Venkataramani 2015).[32] The point estimates are noisy, though, and we cannot rule out some small earnings gains.

In panel B, we find that impacts on earnings levels are larger for Whites from states with low average education pre-suffrage ($p < 0.10$). We also find that impacts on employment are suggestively larger for Blacks from states with higher education, consistent with our larger effects for Blacks from outside the South. These results reinforce our conclusions above: that suffrage-induced education gains led to improvements in labor market outcomes for many groups, but—importantly—not for Southern Blacks.

[28] However, we note that there is a pre-trend in this outcome, which suggests that suffrage may have had spillover impacts to older groups, or may have been preceded by policies that improved the quality but not the quantity of schooling for older cohorts.

[29] For reference, $1 in 1960 is the equivalent of $8.75 in 2020, according to the Bureau of Labor Statistics.

[30] We 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 2014), and find similar results.

[31] We find no impact of suffrage exposure on labor market outcomes for Whites outside the South, consistent with the absence of education gains for that group. For the event study figures for log income for Whites, see online Appendix Figure A.7.

[32] In online Appendix Figure A.8 we show distributional effects on earnings by race and by South/non-South, using the same methodology as the distributional effects on education in Figure 4. It confirms the lack of effects for Southern Blacks at any level of earnings, and indicates that impacts on earnings for Southern Whites and Blacks outside the South were concentrated around the median to seventy-fifth percentile of earnings.

Table 4—Long-Run Effect of Suffrage Exposure on Labor Market Outcomes: Interaction with South and Presuffrage Education

	Whites			Blacks		
	Earnings > 0	Earnings	ln(earnings)	Earnings > 0	Earnings	ln(earnings)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Interaction with South						
Suffrage share 0–15	−0.001	−0.049	0.044	0.127	464.290	0.178
× non-South	(0.008)	(62.244)	(0.028)	(0.051)	(105.829)	(0.112)
Suffrage share 0–15	−0.029	171.504	0.220	−0.041	−211.774	−0.118
× South	(0.041)	(183.668)	(0.066)	(0.082)	(125.735)	(0.138)
Mean *Y*	0.539	2,040.533	7.866	0.618	1,131.662	7.032
Observations	1,053,059	1,053,059	574,210	117,665	117,665	72,415
Panel B. Interaction with pre-period education						
Suffrage share 0–15	0.129	875.571	0.057	−0.231	−248.694	0.064
	(0.084)	(409.932)	(0.262)	(0.188)	(300.492)	(0.249)
Suffrage share 0–15	−0.015	−96.118	0.000	0.041	53.515	−0.007
× pre-period education	(0.009)	(48.222)	(0.029)	(0.024)	(42.978)	(0.038)
Mean *Y*	0.539	2,040.533	7.866	0.618	1,131.665	7.032
Observations	1,053,059	1,053,059	574,210	117,663	117,663	72,413

Notes: This table presents results from regressions of an indicator for having positive wage earnings (columns 1 and 4), real wage earnings (including zeros), $1960 (columns 2 and 5), and log real earnings on either (i) suffrage exposure between ages 0–15 interacted with an indicator for non-South and an indicator for South (panel A), or (ii) suffrage exposure between ages 0–15 and the interaction between suffrage exposure and presuffrage average education in each state (panel B). Presuffrage average education is calculated using individuals who were are least age 16 in the year that suffrage was passed. For reference, $1 in 1960 is the equivalent of $8.75 in 2020. 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 who 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

VI. Mechanisms

We interpret our education results as the reduced-form effect of women's increased 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.

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 do not observe much of intrahousehold 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 is unlikely to be the *only* channel.

Mechanism 2: Health Improvements.—Second, to examine heterogeneous improvements in health, we regress the log of the number of infant mortalities in

a state on an indicator for the years after suffrage together with our controls for state demographics and policies, state and year fixed effects, and state linear time trends. We do this separately for Whites and Blacks, and then test for interactions with South or pre-suffrage levels of education (determined by state and race, as previously).

Online Appendix Table A.12 shows that suffrage led to declines in mortality, particularly for Whites. Mortality improvements are larger in the South and for groups with lower pre-suffrage education. However, unlike our impacts on education, the mortality effects are small for Blacks outside the South and significant for Whites in the non-South ($p < 0.10$). In that sense, our impacts on education to not appear to be explained by improvements in mortality.[33]

In online Appendix Table A.13, we perform a bounding exercise following Lee (2009) to examine the potential role of selection from infant mortality in our education estimates. In particular, we trim the top (bottom) of education outcomes to obtain the lower (upper) bound of the impact of suffrage. We obtain a lower bound that is similar to our main results, and an upper bound that is at least four times as large as our main estimates for both Whites and Blacks. This suggests that our baseline estimates, if anything, may be an underestimate of the true effects of suffrage accounting for selection.

Mechanism 3: Education Spending.—Third, we use our data on city-level spending, revenues, and enrollment to examine dynamic impacts on education spending, revenue, and enrollment post-suffrage. We estimate

$$(5) \qquad Y_{ca} = \alpha_0 + \sum_{t=-5}^{7} \beta_t \big(YearRelSuffrage_{ca} = t \big) + \gamma' Z_{sa} + \delta_c + \phi_a + \epsilon_{ca},$$

where c and a index city and academic year, respectively. Thus, $YearRelSuffrage_{ca} = t$ is an indicator for t academic years since suffrage. We pair together consecutive academic years to increase power and omit the pair of years consisting of the year that suffrage was passed and the preceding year. The variables Z_{sa}, δ_c, and ϕ_{at} indicate state demographic controls, city and academic year fixed effects, respectively.

Table 5 examines *average* impacts on log expenditures (column 1), log total, local, and state revenue (columns 2–4), and school enrollment (column 5). Schooling expenditures post-suffrage increased by 3.4 percent within one year ($p > 0.10$), by 10.8 percent ($p < 0.05$) within three years, and by 13.9 percent ($p < 0.05$) within five years. Further, these effects persist for at least seven years post-suffrage.[34,35] Suffrage also led to an 11 to 15 percent increase in revenue within the first seven years post-suffrage. This increase in revenue was driven primarily by increases in *local* revenue (city + county), with insignificant (and often negative) impacts on state revenue. Finally, we find positive but insignificant average effects on enrollment.

[33] However, there may be other unobserved improvements in health (e.g., reductions in the severity and duration of sickness spells) that could have contributed to increases in schooling attendance.

[34] We find comparable effects on expenditures per pupil.

[35] We note that funding increases of this magnitude were relatively common during this period, which had rapid growth in school funding. School revenues grew by at least 10 percent in over 50 percent of the consecutive years in our sample, indicating that funding levels were relatively malleable. See online Appendix Table A.14.

Table 5—Short-Run Effect of Suffrage on log Education Expenditures, log Enrollment, and log Tax Revenues

Years relative to suffrage	Expenditures (1)	Tax revenues Total (2)	State (3)	Local (4)	Enrollment (5)
5+ years prior	−0.083	−0.090	−0.132	−0.116	0.006
	(0.047)	(0.058)	(0.222)	(0.072)	(0.036)
3–4 years prior	−0.025	−0.027	−0.207	−0.019	0.001
	(0.017)	(0.031)	(0.157)	(0.038)	(0.014)
0–1 years after	0.034	0.032	−0.168	0.046	0.022
	(0.022)	(0.025)	(0.122)	(0.032)	(0.012)
2–3 years after	0.108	0.107	−0.051	0.112	0.025
	(0.032)	(0.038)	(0.203)	(0.045)	(0.020)
4–5 years after	0.139	0.154	−0.262	0.168	0.031
	(0.038)	(0.049)	(0.316)	(0.065)	(0.024)
6+ years after	0.135	0.148	−0.086	0.128	0.044
	(0.045)	(0.057)	(0.371)	(0.099)	(0.033)
Observations	5,183	5,183	4,565	5,172	5,183
Pre *Y* mean	13.52	13.62	11.37	13.46	9.40
Number of states	42	42	41	42	42
Number of cities	523	523	521	523	523
Number of cities in NE	232	232	232	232	232
Number of cities in MW	177	177	177	177	177
Number of cities in S	87	87	86	87	87
Number of cities in W	27	27	26	27	27

Notes: This table presents results from regressions where the outcome is either log city schooling expenditures (column 1), log total revenue (total, from the state, or from local sources (city + county); columns 2–4), or log enrollment, and the key variables of interest are indicators for the number of 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 seven 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

To examine heterogeneous effects of suffrage, we next run a model that allows the estimated effect of suffrage on spending and enrollment to differ across three measures of economic "advantage": (i) the average level of education in the state prior to suffrage (the same measure used in our earlier analysis); (ii) the Black share of the city population in 1910; and (iii) the non-South.[36]

For easy interpretation, the left panels of Figure 5 present the implied effects of suffrage on log expenditures for cities in the seventy-fifth and twenty-fifth percentiles of pre-suffrage level of education (panel A) and share Black (panel B), and for the South and non-South (panel C). Alongside these level effects, we also show the difference between the seventy-fifth and twenty-fifth percentiles (panels A and B) or between the South and non-South (panel C).[37] We find that both more- and less-advantaged cities experienced increases in log expenditures after suffrage. However, suggestively, areas with lower education, higher share Black, and in the

[36] We thank Claudia Goldin for generously providing us with the data on Black population used in Goldin and Katz (2008). We match these data to 233 cities in our sample.

[37] See online Appendix Table A.15 for the individual coefficients.

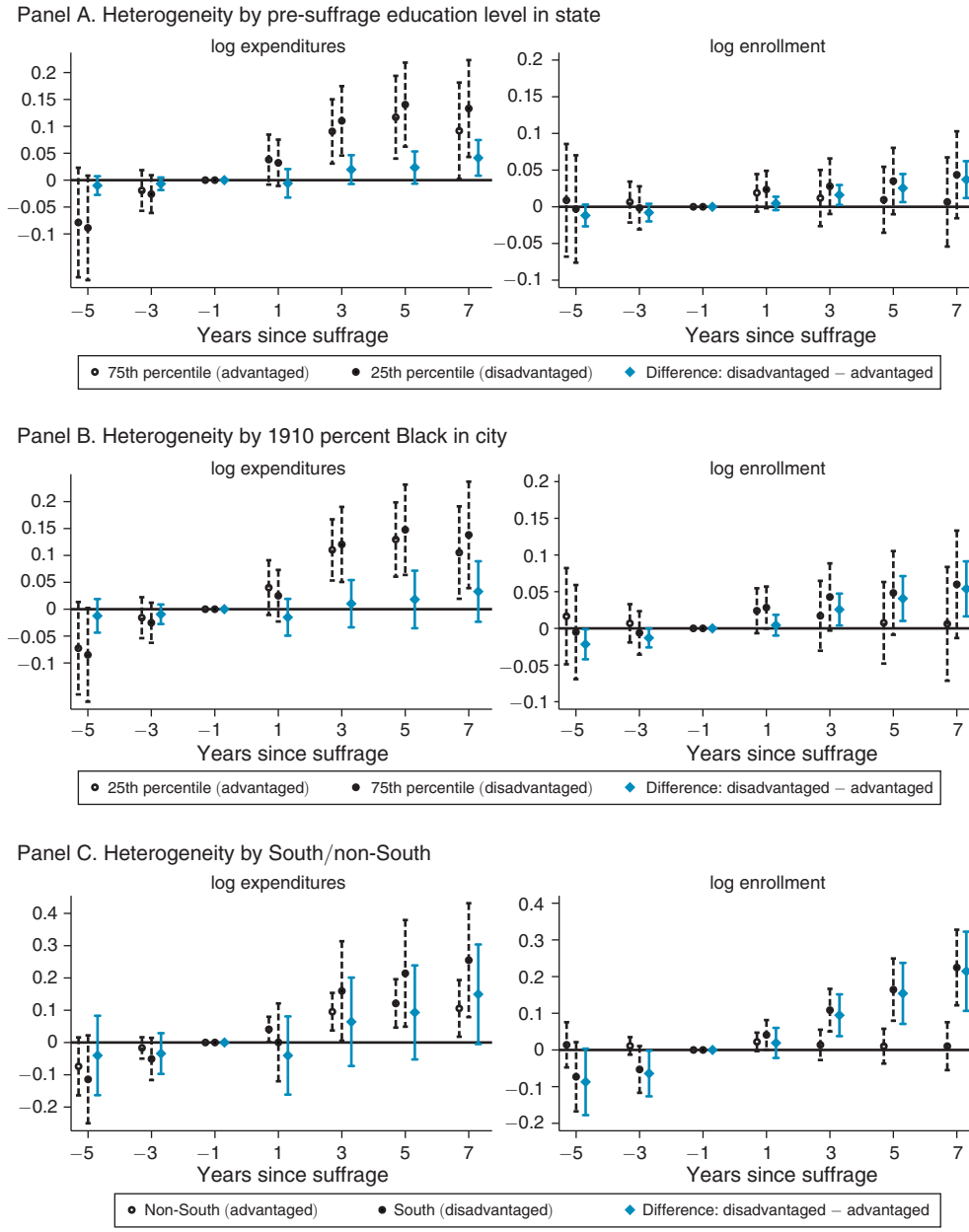

Figure 5. Short-Run Effect of Suffrage on City-Level log School Expenditures and log Enrollment

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 seventy-fifth and twenty-fifth 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 seven 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

South appear to have experienced larger increases in spending. For example, our results imply that educational expenditures increased by 21 log points in the South within five years (23 percent), or roughly twice the 13 percent increase outside the South.[38] The differences across areas are not typically statistically significantly different, but we may have less power due to the limited number of cities in our sample (and in each of these subgroups.)

The right panels of Figure 5 show that 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. This aligns well with the pattern of gains in educational attainment we find in the census.

In sum, we find a close link between increases in education spending, education enrollment, and completed education. We interpret this as evidence that changes in education spending post-suffrage were in all likelihood the primary channel for educational gains. We reinforce this intuition below by verifying that our effects could quantitatively be explained by the post-suffrage increases in education spending.

Pass-Through to White and Black Schools.—Our results show that suffrage increased school spending at the *city level*. To connect these effects to increases in education in the South, which was highly segregated, we next consider how much of this growth in city-level spending would have been "passed through" to White and Black schools (i.e., the elasticity of spending at Black or White schools to city-level spending).

Since there are no existing estimates of pass-through (to our knowledge), we estimate this using county-level data from South Carolina and Georgia on education expenditures for White schools, Black schools, and in total (Carruthers and Wanamaker 2019; see online Appendix B for details.) We regress log spending (or per pupil spending) for White or Black schools on log county spending (or per pupil spending), together with county and year fixed effects. To focus on pass-through after suffrage, we estimate a separate coefficient on log expenditures after 1920.[39]

The results shown in online Appendix Tables A.16 and A.17 show that 10 percent growth in county (per pupil) funding leads to 10 percent (10 percent) growth in county (per pupil) spending for White schools, and 6.3 percent (5.1 percent) growth in county (per pupil) spending for Black schools.[40] Using the smaller estimates of pass-through, this implies that suffrage led to a 11.2 and 23 percent rise in spending in Black and White schools in the South, respectively. Relative to the rest of the country, this places the percent rise in education spending for Blacks in the South at the average percent growth outside of the South, and the percent rise in spending for White schools in the South substantially above the mean of the percent growth

[38] These estimated impacts on spending in the South are consistent with Carruthers and Wanamaker (2015), despite our different identification strategies, states in the analysis, and data sources.

[39] In general, we find that pass-through to Black schools *increases* after 1920, in contrast to Carruthers and Wanamaker (2015).

[40] The impacts on White and Black schools do not average to one because in the data total spending is frequently larger than the sum of spending on White and Black schools. We interpret this as reflecting administrative costs, although it may also be an error in transcription.

outside the South. This matches up with the patterns of education growth (which was higher for Whites in the South relative to the non-South, and similar for Blacks in the South and non-South).

Magnitude and Timing of Effects.—The up-to-one-year increase in educational attainment from suffrage that we document is large, but aligns with other sizable education interventions. This effect is similar in magnitude to the impact of the Rosenwald school-building initiative (Aaronson and Mazumder 2011) and court-ordered desegregation (Johnson 2015). It is also not statistically distinguishable from the estimated impact of a 20 percent increase in per pupil spending throughout schooling for children from poor families in Jackson, Johnson, and Persico (2015) (0.92 years) and is within the bounds implied by the estimates in Hyman (2017) (0.043–1.04 years).[41,42]

Moreover, our impacts *across groups* fit with the previously estimated effects of education spending. In particular, when we scale the average effect from Jackson, Johnson, and Persico (2015) by our changes in education spending, we find that education spending could explain 110 percent, 95 percent, and 73 percent of our effects for Southern Whites, Southern Blacks, and Blacks nationwide, respectively.[43] This suggests that education spending is very likely to be the primary mechanism for our education effects.

The immediacy of the effects of suffrage on education funding and educational attainment is also not particular to suffrage. For instance, Cascio, Gordon, and Reber (2013) and Gordon (2004) find that cities are able to adjust local education funding in 1 to 3 years in response to federal education grants. Further, Jackson, Johnson, and Persico (2015) find that cohorts that benefited from any increase in spending show at least some evidence of improvements in education. This suggests that the education responses to spending following suffrage appear to be generalizable beyond this setting.

VII. Robustness

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

[41] See table 3 of Jackson, Johnson, and Persico (2015), which shows a 0.46 year increase in schooling for a 10 percent increase in spending. We obtain a comparable estimate from Hyman (2017), by applying the conversion from post-secondary enrollment to completed schooling described in that paper to the 4.3 p.p. increase in post-secondary enrollment in low-income districts (Table 6). We thus calculate that a 10 percent increase in spending leads to at most a 0.52 year increase in completed schooling ($0.043 \times 3 \times 4$) and at least a 0.043 increase in completed schooling ($0.043 \times 1 \times 1$).

[42] As another comparison, our effects are somewhat larger than the estimated impacts of compulsory schooling laws, which increased schooling between 0.04 and 0.4 years for White men (Stephens and Yang, 2014), but it is somewhat difficult to compare this regulation-style policy with the infusion of resources from suffrage.

[43] We calculate these by scaling the 0.92 effect from Jackson, Johnson, and Persico (2015) by the change in spending for the group five years post-suffrage as a fraction of 20 percent, and dividing that by our estimated effect for the group. For Southern Whites this is $(23\%/20\%) \times (0.92/0.96) = 1.1$; for Southern Blacks this is $((23\% \times 0.63)/20\%) \times (0.92/0.7) = 0.95$, taking account 63 percent pass-through; and for Blacks on average this is $(13.9\%/20\%) \times (0.92/0.88) = 0.73$.

Mandatory States.—First, we check whether suffrage had an impact on education in states that were *mandated* to accept suffrage by the Nineteenth Amendment (i.e., states that did not vote to ratify the Amendment.) This allows us to rule out the possibility that our effects are driven by endogenous adoption of suffrage laws. Panel A of online Appendix Table A.18 shows that in fact suffrage had a *larger* effect on education in mandated states relative to voluntary states. Nevertheless, panel B shows that our effects are also robust to dropping the mandatory states. Thus, the impact of suffrage does not appear to depend on how suffrage was adopted.

Randomization Test with Placebo Suffrage Laws.—Second, we perform a randomization test that allows us to determine whether our effects could have arisen by chance (Athey and Imbens 2017). In particular, we randomly draw a placebo suffrage year between 1910 and 1920 for each state, and then assign placebo individual suffrage exposure based on that placebo year. We then use equation (2) to estimate the effect of placebo suffrage exposure on educational attainment, separately for Blacks and Whites. We repeat this 1,000 times. Consistent with our main results, this test produces a p-value below 0.01 for the impacts of suffrage on Blacks' education and a p-value of 0.31 for the impacts of suffrage on Whites' education (see online Appendix Figure A.9).

Migration by Parents or Children.—Third, we consider the potential role of migration by parents (pre-birth) or by children (post-birth) in our results. The fact that we found little change in state demographics following suffrage in Section IVA provides evidence against selective migration by parents. To examine the role of child migration, we stratify our results by individuals that remain in the same state of birth ("non-movers") or not ("movers"). We find that our effects are concentrated among non-movers (see online Appendix Table A.19). Thus, it does not appear that education gains following suffrage were a result of migration.

World War II and the G.I. Bill.—Fourth, we check whether exposure to suffrage might be correlated with the likelihood of serving in World War II and, hence, with eligibility for the G.I. Bill. Controlling for region fixed effects, we find no correlation between the year of suffrage and the proportion of the state serving in World War II (which we obtain from Acemoglu, Autor, and Lyle 2004).[44]

Additional Checks.—Finally, we run a variety of additional specifications to verify the robustness of the results. These include trying alternative sample restrictions, e.g, examining results by census year (online Appendix Table A.20); keeping states that passed suffrage prior to 1900 (online Appendix Table A.21); only keeping individuals over 25 (online Appendix Table A.22); or restricting the sample to a smaller range of treatment ages (online Appendix Figure A.10). They also include

[44] Early 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.

testing alternative measures of treatment and controls, e.g., running a model with a binary measure of exposure to suffrage (online Appendix Table A.23); substituting our baseline state controls (measured at birth) with average state conditions between ages 0 and 15 or an interaction between state conditions in 1900 with a linear trend (online Appendix Table A.24, panels A–C); dropping compulsory law controls; allowing the effect of compulsory laws to vary by age; adding controls for progressive laws; controlling for trends interacted with the pre-suffrage education level of the state; and dropping states that had Rosenwald school (online Appendix Table A.24, panels D–H). Our conclusions do not change across any of these specifications.

VIII. Conclusion

This paper presents new evidence on the effects of women's political empowerment on children's human capital. We find that exposure to suffrage during childhood led to substantial gains in educational attainment, particularly for children from economically disadvantaged backgrounds. Full exposure to suffrage between the ages of 0 and 15 increased educational attainment by slightly less than one year for Black children and White children from the South, who had the lowest levels of education pre-suffrage. We also provide new evidence that suffrage led to disparate increases in education spending across the country. These increases in spending appear to explain our heterogeneous impacts on education.

On the whole, this article provides compelling evidence for the role of female voter preferences in influencing policy, both toward 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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