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ABSTRACT

Gains in 20th century real wages and reductions in the black-white wage gap have been linked to the mid-century ascent of school quality. With a new dataset uniquely appropriate to identifying the impact of female voter enfranchisement on education spending, we attribute up to one-third of the 1920-1940 rise in public school expenditures to the Nineteenth Amendment. Yet the continued disenfranchisement of black southerners meant white school gains far outpaced those for blacks. As a result, women's suffrage exacerbated racial inequality in education expenditures and substantially delayed relative gains in black human capital observed later in the century.

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1 Introduction

> *"The men have been carelessly indifferent to much of this civic housekeeping, as they have always been indifferent to details of the household... The very multifariousness and complexity of a city government demand the help of minds accustomed to detail and variety of work, to a sense of obligation for the health and welfare of young children and to a responsibility for the cleanliness and comfort of others."*
>
> - Jane Addams[1]

At the dawn of the twentieth century, the United States held a position of distinction in the provision of public education. Among its Western Hemisphere peers, the U.S. exhibited the highest "common school" (grade 1-8) enrollment rates and was showing early leadership in the race towards mass secondary education. Indeed, the country had enjoyed a substantial and persistent mass education advantage since the middle of the previous century, aided by the country's commitment to a set of "egalitarian principles" (Goldin, 2001). These principles included "public funding, openness, gender neutrality, local (and also state) control, separation of church and state, and an academic curriculum," and they drove the United States to world leadership in education provision by 1900. As the twentieth century progressed, the United States strengthened its leadership position in the provision of public education and brought secondary education to the masses. By the dawn of the second world war, the median 19-year-old was a secondary school graduate (Goldin, 1998).

The human capital consequences of gains in school resources and quality are somewhat controversial for the latter part of the 20^{th} century (Hanushek, 1996) but less-so for earlier cohorts of Americans. Card & Krueger (1992a) and Card & Krueger (1992b) document an associated upward trend in the rate of return to schooling for white and black Americans. And because black school quality was rising more quickly than white after 1930, they attribute a substantial portion of the narrowing black-white wage gap to corresponding improvements in relative black human capital.

The available literature cites the continued application of American egalitarian principles and strong labor market demand for an educated workforce to explain the steady advance of public

[1] Quoted in Harper (1922), p. 178.

education provision (Goldin & Katz, 2009). Yet, as we show in Figure 1, the growth of education provision was not constant over the course of the century. The commitment of states and local school districts to the funding of public education exhibits a marked uptick around 1920, the same year that the Nineteenth Amendment to the U.S. Constitution guaranteed full voting rights to all adult females. Given the timing of these changes, is there a role for universal suffrage in explaining the renewed commitment of school districts to public education finance? In addition to marking the beginning of accelerated growth in overall school spending, relative black school quality measures also dipped to unprecedented lows in this same period before rising again in the 1930s and 40s.[2] Is there an explanation for this shift in relative resources that is related to the expanded electorate under the Nineteenth Amendment?

An extensive empirical literature documents a greater propensity of women to support the provision of public goods, to hold other-regarding preferences, to foster the expansion of government to benefit child welfare and, in some ways, to hold Goldin's "egalitarian principles" closer to heart.[3] Standard models of electoral competition indicate that policy makers will respond to shifting preferences of their electoral base by altering public service allocations and their own voting behavior. The testable implication is that the enfranchisement of women would have resulted in greater education expenditures following the ratification of the Nineteenth Amendment.[4] Other literature has identified a measurable impact of expanded suffrage on government provision of other services.[5]

Interestingly, this literature has found no increase in education spending in the wake of suffrage despite the fact that education was one of the fastest-growing elements of state and local expenditures in this period and despite evidence from a variety of literatures that women have stronger preferences than men for child welfare (Lott & Kenny (1999); Miller (2008)). We hypothesize that

[2]See Margo (1990) Table 2.5, pp 20-21.

[3]See, *inter alia*, Eckel & Grossman (1996); Croson & Gneezy (2009); Li et al. (2011); Doepke & Tertilt (2014).

[4]An important caveat to this expectation, however, is the possibility that the impact of suffrage on public spending was fleeting as policymakers became less wary of the female vote (Moehling & Thomasson, 2012).

[5]Husted & Kenny (1997); Lott & Kenny (1999); Miller (2008).

a muted or negligible response to suffrage at the state level belied a strong local response. Control of schools was highly decentralized in the early 20^{th} century, and the majority of education funds were local outlays resulting from taxes administered by counties and school districts.[6]

A second implication, at least for the segregated South, is that white spending stood to gain more from women's preferences than did black. Severe disenfranchisement through poll taxes, literacy tests, and voter intimidation meant that although the Southern electorate became more female after 1920, it did not become any less white until later decades. Because schools were segregated and education expenditures were race-specific, a testable implication is that the expansion of voting rights to women affected expenditures on white schools differently from black schools. To our knowledge, we are the first to examine this question in the literature. For a later period, Cascio & Washington (2013) show that the Voting Rights Act, which extended the *de jure* franchise to black southerners, resulted in higher education expenditures in counties with higher black population concentrations.

We use a new county-level panel dataset of annual education expenditures for three Southern U.S. states - Alabama, Georgia, and South Carolina - to test the notions that suffrage elevated public resources for education and that benefits accrued more heavily to white schools. These data are unique in that they are disaggregated by county and race, and we know of no other long-running source of education spending at this level of detail.[7] Alabama, Georgia, and South Carolina are not among the twenty-nine states that granted full voting rights to adult females prior to 1920. Rather, the counties in this panel were compelled to extend the voting franchise by the passage of the Nineteenth Amendment, which none of the three states ratified until after 1950.

This single point of intertemporal variation in suffrage reduces the threat of policy endogeneity

[6]Nationwide, local sources contributed 83 percent of school revenues for the 1919-1920 school year, versus 44 percent for 2004-2005 (Snyder et al., 2008).

[7]We focus on Southern states in order to test all model predictions in Section 2, including differential implications by race. Other Southern states are excluded from the analysis either due to the lack of consistent expenditure reporting over the time period in question or because spending data are not disaggregated by race. See Appendix B for evidence that these three states did not diverge significantly from the rest of the nation in their response to women's suffrage.

(from, for instance, unobserved progressivism that led states to extend suffrage rights and increase education spending at the same time) but presents the challenge of identifying variation in women's right to vote. We therefore exploit spatial variation in the "dosage" of suffrage, namely cross-county variation in white female population shares and estimated female voter shares as measures of the relative power, perceived or actual, of women in the democratic process.

Consistent with expectations, a higher dose of suffrage is associated with higher education spending after 1920. Each percentage point increase in the white female population share increased per-pupil spending by 0.7 percent, indicating that up to one-third of 1920-1940 expenditure gains are attributable to women's suffrage. The results further indicate that expanded suffrage had a significant, positive impact on both black and white school expenditures, but that white school spending gains far outpaced those for black schools. After having stabilized in the latter part of the 1910-1920 decade, the ratio of black to white per-capita spending fell by 19 percent in the years following suffrage. Our estimates indicate that *all* of this relative decline can be attributed to the Nineteenth Amendment, and we cautiously propose that the ratio of black to white education expenditures would have been substantially higher in its absence.

The question of whether mass enfranchisement affects the provision of education has bearing for modern-day developed and developing countries, where the decisive voter is less proximate to decision-makers and where the returns to expansion of public education are steep (Duflo, 2001). Improving public schools in the United States had long-term impacts on wages and inequality, and our findings imply that women's suffrage was partly responsible.

2 Historical and Theoretical Foundation

This study is motivated by the observation that per capita education expenditures in the United States demonstrate a marked uptick around 1920. The accelerated growth of education funding may be the result of numerous causes: the impact of World War I and the ensuing recession, rising living standards and incomes of the "roaring twenties," a modernizing work force and a rising demand by employers for formal human capital, changes in compulsory schooling requirements, or expanded "free tuition" legislation.[8] Our study examines the role of an expanded electorate in

[8]See Goldin & Katz (2009) for a more thorough discussion of the social, economic, and political landscape that contributed to the growth of public education in the early 20^{th} century.

explaining the expenditure increase.

A causal role for female suffrage is supported by two distinct lines of economic research. First, models of electoral competition indicate that policymakers act in accordance with the views of decisive or "swing" voters in the electorate.[9] The Nineteenth Amendment doubled the size of the electorate in some states; if the new voters also exhibited a greater preference for spending on public education, we should observe an acceleration in expenditures as a result.[10]

Second, a series of empirical results document a greater preference of women for goods that enhance child welfare and for the provision of public goods in general. In the intra-household con-

[9]The early, seminal literature on competitive political economy indicates that politicians adopt policy platforms matching the preferences of voters at the median of an issue spectrum (Bowen, 1943; Black, 1948; Baumgardner, 1993). If women are more public-goods loving than men, and if there is variability within the male electorate on public goods preferences, the decisive voter along the spectrum of preferences regarding education provision shifts decidedly towards more education funding after women's suffrage, even if the decisive voter is still male. A similar outcome obtains in models of probabilistic voting (Lindbeck & Weibull, 1987, 1993; Persson & Tabellini, 2000). In this case, voters choose candidates based on their policy platforms and on individual or group-specific preferences over candidates which are independent of policy and imperfectly observed by candidates. The equilibrium outcome of the model is that candidates adopt platforms which represent a weighted social welfare function where the weights reflect both the size of a particular group and the expected responsiveness of groups/individuals to policy changes in terms of votes. In either case, and under relatively weak assumptions, expanding the electorate at the scale realized by the Nineteenth Amendment would have shifted platforms of vote-maximizing candidates towards the preferences of newly enfranchised voters.

[10]A critical view of electoral competition models might emphasize the role of political activism outside of voting *per se* as a driver of political behavior and public expenditures. Women were certainly active in the political process and pressed their agendas prior to being granted suffrage (Schuyler, 2006). Nevertheless, to the extent that newly-acquired voting rights reflected a more potent voice in the political process, we expect a change in the allocation of public monies to more closely reflect female preferences.

text, a number of studies have shown an increased propensity of women to invest in the health and welfare of their own children, relative to their male counterparts.[11] As a result, welfare outcomes for children in the household also tend to rise with the mother's financial resources.[12]

In addition to an increased propensity to invest financial resources in their own children, women also appear to prefer higher quantities of public goods in general and goods benefitting children (not necessarily their own) in particular.[13] Doepke & Tertilt (2009) propose that the expansion of women's legal rights in the nineteenth century, prior to women's suffrage, resulted in increased investments in children's education. In terms of the Nineteenth amendment, Moehling & Thomasson (2012) attribute state participation in the Sheppard-Towner maternal education program to women's suffrage, although the effect seems to wane over time. Lott & Kenny (1999) credit the enfranchisement of women with an increase in overall government expenditures and revenue after 1920. At the state level, however, they find no significant impact on specific components of government expenditure including social services and education. Miller (2008) demonstrates that suffrage and the increased voting power of women resulted in a sizable increase in local public health spending and a decrease in child mortality rates, but no change in state educational spending.[14] Neither Miller (2008) nor Lott & Kenny (1999) examined the impact of suffrage on *local* educational spending, which we contend would have been more sensitive to women's enfranchisement given the dominant role of local districts in determining the allocation of resources to public schools.

[11] See Doepke & Tertilt (2014) for a summary of empirical findings.

[12] Anthropometric status, nutrition, and child survival rights have all been shown to increase with the mother's income share. See Atkin (2009) and Duflo (2003) for the anthropometric results, Rubalcava et al. (2009) for nutritional status, and Thomas (1990) for child survival results. See Wolpin (1993) for a more complete summary of the literature on health outcomes of children.

[13] See, *inter alia*, Eckel & Grossman (1996); Croson & Gneezy (2009); Li et al. (2011); Doepke & Tertilt (2014).

[14] See also Husted & Kenny (1997) for evidence that enfranchisement of the poor via the elimination of poll taxes and literacy tests increased public welfare spending.

3 Data

3.1 Education Statistics

We utilize a newly transcribed dataset of county-level black and white public school statistics between 1910 and 1940 for three Southern states: Alabama, Georgia, and South Carolina. Focusing on Southern states allows us to test for impacts of suffrage on spending inequality between races, and these three states were chosen for their consistency in reporting expenditure values for black and white schools separately over the critical years 1910-1940. Each state's department of education or equivalent office published an annual or biennial report containing statistics on revenues and expenditures. The data and data collection process are described in detail in Carruthers & Wanamaker (2013).

Local school districts were the locus of control over schooling expenditures in this era both because the majority of revenues were locally-sourced and because state contributions to schools were subject to the spending discretion of local districts. Snyder et al. (2008) report the nationwide contribution of revenue receipts from federal, state and local sources, and we present these data in Table 1. Nationwide, federal spending is minimal throughout the period of interest. Local control waned over time; the 1919-1920 school year saw 83.2% of revenues emanating from local sources, a number which had fallen to 68% by 1939-1940.[15] State appropriations filled the gap left by the relative decline of local school revenues between 1920 and 1940. Critically, however, education expenditures reported in state department of education reports include all spending from state and federal transfers, since local school districts were the clearinghouse for all public education support.

For the states in our sample, school districts are sub-county constructs. But education expenditures are not consistently published at the sub-county level, and we have no ability to measure female voter power at the school district level. As a result, we perform our analysis at the county level and maintain that the southern county is the best available unit of analysis given data constraints.[16] Alabama and Georgia report statistics by district, which we then aggregate to the county

[15]Importantly, we do not observe a discernible break in the local funding share of total receipts between 1919/20 and 1929/30.

[16]The number of distinct school districts per county varied over time; in 1920 the ratio was

level. The South Carolina data were aggregated to the county level before being published. Echoing the previous literature, Appendix B shows that the state-level education spending response was small or negligible for these three states, implying that any change in education provision after the Nineteenth Amendment would have been driven by local decisions.

Transcribed education data are assembled into a county-by-race panel for 1910-1940 describing the school finances of each county.[17] This panel is matched to additional county-level variables from industrial and agricultural censuses taken in 1910, 1920, 1930, and 1940 (Center, 2011).[18] Relevant statistics from these reports include crop value per capita, the percent of land devoted to agriculture, and manufacturing employment and earnings. Annual measures are interpolated between census years to fully populate the panel.[19] Philanthropic activity directed toward black schools was an important factor in both black and white school spending (Carruthers & Wanamaker, 2013), and we match each county and year with the number of new Rosenwald classrooms built therein.[20] We additionally control for the presence of secret ballots, which pertain to Georgia

1.73 for Alabama, 1.56 for Georgia and 41.2 for South Carolina where individual townships were granted the power to levy school taxes in addition to county taxes. The sample average in 1920 is 8.3.

[17]Georgia schools data are reported biannually between 1930 and 1940, and we interpolate linearly between reporting years.

[18]County boundaries changed over time, and Georgia continued to form new counties throughout the period in question. We rely on county boundary change data from Horan & Hargis (1995) and aggregate up to the "supercounty" level to ensure consistent boundaries over time. This brings the number of counties from 272 to 235.

[19]Because the interpolation process itself may generate discrete changes in 1920, we test the robustness of our results to a set of controls interacted with year fixed effects in Appendix C.

[20]Benefactor Julius Rosenwald challenged localities with matching grants for black school construction. Over 5,000 rural schools for black students were built by the Rosenwald Foundation between 1916 and 1933, and each is documented in the online catalog at Fisk University: http://rosenwald.fisk.edu/.

in 1922 and later years.[21]

3.2 Measuring Voter Power

The conceptual framework outlined above generates testable implications regarding the dosage of suffrage treatment across local areas. We use three dosage measures: the percentage of the voting population white and female in 1920 when the amendment was enacted; the percentage of the voting population white and female in each year (interpolated between census years); and the estimated density of female voters as a percentage of the voting-age population in 1920 (an early turnout proxy).

To accurately size the male and female voting-age population, we require more granular population statistics than those available in published census volumes. We populate decennial 1910-1940 age-by-race-by-gender cells for each county in the three-state sample using data from the genealogy website Ancestry.com.[22] The 1920 ratio of white females to the voting-age population is the first proxy above. The second is calculated by interpolating the number of white females and the size of the voting-age population between census years. For the third (female voter percentage), we match female population data to total voter counts for the 1920 general election (Clubb et al., 2006), the first where all U.S. women could participate. We use Bayesian methods with informative priors to infer 1920 voter turnout rates by gender for each county. See Appendix A for details on this procedure.[23] We then use the estimated female turnout to calculate female turnout as a percent of the voting-age population.

[21]The expected impact of secret ballots on education expenditures is ambiguous, but as the reaction of political systems to voters or potential voters is the effect of interest, we control for this variation in voting structures which may have impacted female voter uptake.

[22]Ancestry.com has transcribed the full manuscripts of U.S. Census returns through 1940 from the originals housed at the National Archives. The data is indexed and searchable, facilitating tabulations by county and demographic characteristics. Outside of limitations to enumeration in the original census year and the readability of census manuscripts, there are no known limitations to the Ancestry.com data.

[23]Estimates of race and gender-specific turnout proved too noisy for this exercise.

Each of these proxies has its merits. Fixed measures of voter power in 1920 are less likely to be endogenous to education expenditure trends than a population share that changes over time, but relying on a dosage proxy from a point in time increases measurement error in other years. Population percentages represent potential female voter power and are less likely to be endogenous to education expenditures than voter percentage would be. But if policymakers responded to female voter turnout at the polls more so than potential voters, the voter turnout measure is a better proxy for suffrage dose. The share of the population consisting of voting females is a distilled form of the population share proxy, and one that allows the effect of suffrage to operate through the political process *per se*. The choice of suffrage dosage matters little for our overall conclusions, and results for all three proxies are reported in Section 4.

Given the central role female population percentages take in our analysis, it is appropriate to question *which* counties had higher shares of white voting-age females in and around 1920. Table 2 describes the correlation between 1920 white female population shares and other observable county features as of 1920.[24] White females are conditionally more prevalent in counties with lower black-white ratios, higher shares of land devoted to agriculture, fewer adults employed in manufacturing, and lower crop values per capita. Overall, these observable county-level covariates explain 75% of the intra-state variation in 1920 white female population shares. Principal results to follow control for these covariates (which are time-varying in our specifications, rather than being fixed in 1920) and essentially test whether the remaining (25%) variation in female population shares is associated with differentially higher or lower education spending after the 19^{th} Amendment, relative to before.[25]

[24]The regression is $PctF_{c,1920} = \beta X_{c,1920} + \epsilon_{ct}$ where the dependent variable is expressed in 0-100 percentages and $X_{c,1920}$ is a vector of covariates from the bottom of Table 3, excluding variables with no cross-sectional variation in 1920 (secret ballots and Rosenwald classrooms).

[25]Given the economic and statistical significance of black-white population ratios, robustness checks test whether results are sensitive to a more flexible quadratic control for this variable. Point estimates are nearly equivalent in sign and significance. See Appendix C.

4 Empirical Strategy and Results

Table 3 lists descriptive statistics for school spending and other school quality outcomes, suffrage treatment measures, and Census controls. The gap between white and black spending is striking. Between 1910 and 1940 in these three states, black students were allocated 24 cents for every dollar directed toward white students. This gap narrowed after 1940, particularly in the wake of civil rights legislation more than two decades after the close of our panel.

We consider 1921 (that is, the 1920-1921 school year) to be the first post-suffrage reporting year for these states. Financial reports that form the basis of these data referred to academic years (e.g., August 1919 - July 1920). The Nineteenth Amendment was officially in place as of August 1920, well after funds were spent for the 1919-1920 school year. Though there may have been anticipatory effects on spending immediately prior to the Nineteenth Amendment, we are most interested in the change in school spending trends *after* policymakers faced a new electorate.[26]

The stylized facts relevant to this application are presented in Figures 1 and 2. The top panel of Figure 1 plots per-capita school spending by year for all of the United States, where a substantial increase in expenditures is evident in the years immediately following 1920. The same metric for the transcribed three-state sample of local school data is located in the bottom panel of Figure 1. The trajectory of spending in these states mimics the nationwide change with a sharp upward shift in spending trends after 1920.

In both panels, a noticeable dip in expenditures is apparent during the war years (1914-1918). These reductions are likely a result of reductions in municipal tax revenues, but there is no reliable data on local tax receipts at the county level for this period. If post-1920 gains are just a recovery from this reduction and unassociated with suffrage, the results should show no differential response in counties with more voting power among women. On the other hand, if reductions in spending around World War I are somehow correlated with the proxies for suffrage used in the empirical

[26]If there are anticipatory effects, they will serve to bias our results toward zero. Note that any long-term expenditure changes would have been funded out of changes in state and local taxes which themselves would have taken some time to come into effect. In the near term, local leaders who were cognizant of the political economy implications of suffrage could have redirected funds from other public service areas to benefit education.

results below, the interpretation of those results is muddled given the possibility for "catch-up" spending in the hardest-hit municipalities.[27] We return to this issue in Section 4.1 and Appendix Section D.

The spending trends highlighted in Figure 1 differed by race. Figure 2 shows that the spending trajectory shifts in 1921 were steeper in white schools. In panel A of Figure 2, it is apparent that post-1920 growth in black expenditures per pupil exhibited a much slower increase. The same is true for specific school resources. Teachers per pupil (panel B) and average teacher salary (panel C) show sharp shifts for whites after 1920, but the increases are more muted for blacks, especially in the case of teacher salaries.[28]

Importantly, panel D of Figure 2 indicates that these expenditure and resource shifts did not coincide with changes in enrollment *per capita*. Thus the post-1920 changes in spending per pupil, where we will focus our analysis, are not likely driven by any sudden, demand-side changes in the size of the student population. We do not rule out that subsequent enrollment may have responded to higher school quality, but simply highlight no discrete shift in these metrics at the 1921 juncture.

4.1 Difference-in-Difference Identification

The apparent shift in post-1920 resources cannot be attributed to suffrage *per se* if there are other events impacting spending in the years immediately following 1920. To identify the contribution of women's suffrage specifically to this expenditure growth, we utilize a difference-in-difference estimator where treatment is the suffrage dosage proxy interacted with an indicator, $POST_t$, equal to 1 in all years after 1920. If women's suffrage led to higher education expenditures, we should

[27]Male casualties may have been a source of cross-county variation in the gender ratio, but war-induced variation in our suffrage dose proxies were probably very small. Only 0.19 percent of the voting-age population were killed in the war, and cross-county variation in war casualties likely account for a small share of the 9.9-point standard deviation of female population shares.

[28]The 1919 marker for white teachers per 1,000 pupils is affected by one outlier in Georgia. That year, the state reported atypically low white enrollment figures for Gordon County. This probable reporting error attenuates results for white teachers and spending per pupil downward.

observe a larger post-1920 effect in counties where proxies for female voter power are higher.[29]

A difference-in-difference estimator of the treatment effect of women's suffrage is as follows:

$$Y_{ct} = \delta_0 + \delta_1 POST_t * D^F_{ct} + \delta_2 D^F_{ct} + \theta_c + \theta_t + \beta X_{ct} + \varepsilon_{ct}, \quad (1)$$

where Y_{ct} is a measure of public educational spending for county c at time t (in 1925 dollars), D^F_{ct} is a measure of female voter power, θ_c is a county fixed effect, θ_t a year fixed effect, and ε_{ct} is an error term.[30] $\mathbf{X}_{ct}$ is a matrix of time-varying county-level observable characteristics summarized at the bottom of Table 3. Economic controls (value of crops per capita, percent of land devoted to agriculture, average annual income in manufacturing, share of adults working in manufacturing), population controls (black-white population ratio, cubic function of total county population, adult female population share) and other variables that may have affected school spending (presence of a secret ballot, number of newly constructed Rosenwald classrooms) are included in all estimates.[31]

[29] A number of other econometric tools are available here. The most flexible of these is a modified event study estimator where year fixed effects are interacted with dosage proxies to generate year-by-year estimates of the impact of suffrage dosage on spending. This strategy, however, makes hefty demands on our sample of 235 counties. We present the results of this analysis in Appendix D and note that the results are consistent with those obtained in other specifications below but are imprecisely estimated as would be expected given our limited sample size. In addition, we have estimated a more flexible functional form for the post-1920 impact which identifies the impact of the suffrage dosage on the level of resources and on both the linear and quadratic growth trends. Those results, available upon request, imply similar effects to the ones identified here.

[30] The presence of year fixed effects precludes identification of a coefficient on $POST_t$ as a stand-alone regressor.

[31] Agricultural economic activity was a close substitute to schooling in rural areas and also drove incomes, but Southern economies were shifting to a more industrial emphasis over this time period. We thus include measures of both agricultural and manufacturing variables. Outside private funds, proxied by Rosenwald classrooms, may have crowded out local investment in school spending or may have been crowded in by it and, if so, are endogenous. Our results are not sensitive to including this control. The Appendix discusses robustness checks with additional controls for county-wide literacy. Although literacy is an important proxy for human capital within a given area, we exclude

We estimate δ_1 over a 20-year post-Amendment horizon, and robust standard errors are clustered within counties in all estimates.

Of course, voter density and population shares are not randomly assigned across counties and may be correlated with potential confounders, including the size of the school-age population, constituent preferences for education, and migration in response to or anticipation of better school resources. In this context, any such unobserved, county-specific and time-invariant variable that is correlated with both a county's (proxied) female voter power and the school spending measure will be absorbed in the county fixed effect. Similarly, any post-1921 impact that is common across counties will be absorbed by year fixed effects. Identification comes from the differential effect of female voter power on education spending in the years following 1920 relative to the pre-suffrage years in the panel.

The remaining concern and primary threat to our identification strategy is the possibility that unobserved and heterogeneous trends in omitted variables are more prevalent in high-dosage areas, and that these omitted variables affect the educational spending in ways we falsely attribute to suffrage. In robustness checks discussed in Appendix C, inferences are qualitatively unchanged when we undertake additional steps to recognize heterogeneous trends in the analysis. Additionally, balancing tests show that the correlation between pre-suffrage spending trends and our suffrage dosage metrics are largely insignificant.

Finally, the interpretation of δ_1 hinges on the assertion that the identified impact of the dosage proxies in the post-1920 years is attributable to suffrage *per se*. Alternative explanations which are consistent with a differential impact after 1920 for counties with higher female voter power are difficult to come by. To our minds, the leading contender is that δ_1 is measuring a differential rebound from World War I spending reductions in these counties which is unassociated with suffrage. If higher female populations somehow reduced spending during World War I *more* than other counties, δ_1 may simply reflect post-1920 rebounds from these spending reductions. Contrary to this hypothesis, however, we observe no difference in wartime spending based on suffrage dosage. These results and further discussion are located in Appendix D.

this control from our main analysis. Available literacy data pertain to the population age ten and older, which may be endogenous to improving school quality.

4.1.1 Baseline Results

Results corresponding to each of three proxies, which are measured in percentage points (0-100), are reported in Tables 4 - 6, respectively. The top row of each table gives the δ_1 coefficient under a variety of functional forms. Column 1 in each table includes year fixed effects and X_{ct} controls interpolated between census years. Column 2 replaces year fixed effects with state-year fixed effects, and Column 3 in each table drops the X_{ct} variables which were interpolated between census years (all variables except secret ballot legislation and Rosenwald school controls) out of concern that post-1920 estimated impacts are spuriously driven by changes in the slope of the interpolated covariates as of the 1920 census. The impact on overall spending is largely impervious to these changes in functional form.

We turn first to results for the impact of suffrage via a higher share of white females in 1920, which varies across counties but not time (Table 4). This dosage proxy, measured at a point in time on the eve of women's suffrage, is least subject to endogenous time-varying omitted variables (e.g., mobility) but most subject to error in measuring the time-varying power of new voters. Each percentage point increase in the white female population in 1920 (per Columns 1 - 3 of Table 4) is associated with an increase in education expenditures per pupil of between 0.7 and 0.9 log points between 1920 and 1940, indicating that the overall treatment effect of increasing female voting power from null to the typical dosage was 19 to 26 log points.[32]

Table 5 lists coefficient estimates for $POST * D^F_{ct}$ with the annual population share of white females standing in for D^F_{ct}, a value that varies both over time and across counties. This proxy is a more accurate measure of women's potential voting bloc in a given year, but also more likely to be endogenous. Nevertheless, the first row of Column 1 - 3 point estimates are in broad agreement with those from Table 4, and the implied suffrage treatment effect is equivalent to that in Table 4, 19 to 26 log points.[33]

Our third proxy for women's voter power is the share of the 1920 population who we estimate to have been female voters (Table 6). Standard errors are bootstrapped to reflect estimation of the

[32]Obtained by multiplying each coefficient by the average dosage value, 29.85, from Table 3.

[33]Obtained by multiplying each coefficient by the average dosage value, 30.21, from Table 3.

proxy variable.[34] Incrementally higher female turnout is associated with expenditure impacts of between 1 and 2 log points, and the implied suffrage treatment effect is somewhat smaller at 7 to 13 log points.[35] We note that treatment effects from this third dosage are highly susceptible to attenuation bias from measurement error since female turnout is estimated (see Appendix A). Still, we view the results in Table 6 as confirmation that women's proximity to the political process itself was responsible for the observed impact of female population shares observed in Tables 4 and 5.

Our results are comparable to estimates of suffrage treatment effects on public spending overall in this era. Using a similar methodology, Lott & Kenny (1999) estimate that typical turnout gains from suffrage increased state-level spending by 14 percent immediately and 21 percent after 25 years (p. 1176).

The size of the suffrage treatment effect also indicates that the Nineteenth Amendment played a dominant role in the growth of public school expenditures after 1920. Average expenditure (per pupil) in the three Southern states in our sample increased by approximately 78 log points between the pre-suffrage years of 1910-1920 and the post-suffrage years through 1940. Our estimates indicate that between 24 and 33 percent of that increase emanated from an expanded electorate.

4.2 Testing the Interracial Prediction

In this section, we test whether white schools benefitted differentially from suffrage relative to their black counterparts. Segregated schools in this era resulted in fully separable school budgets, allowing us to test the interracial prediction directly by replacing Y_{ct} in Equation 1 with black expenditures per (black) pupil, white expenditures per (white) pupil, and the ratio of black to white per pupil school expenditures. The ratio allows us to gauge the impact of suffrage on this common measure of relative school quality.[36]

[34]Bootstrapped standard errors are computed by estimating the model 100 times for 125 randomly sampled counties (with replacement).

[35]Suffrage "treatment" for this proxy implies going from 0 to an average of 7 percent of the electorate as female voters. See Table 3.

[36]Because enrollment changes were minimal following 1920 (see Figure 2), changes in the black-white funding ratio are driven by changes in expenditures.

Results across Tables 4 - 5 (Columns 1-3, Rows 3-4) indicate that female suffrage exacerbated gaps in black and white school quality by raising the level of white school spending more than black. Point estimates for black spending gains, while sometimes statistically significant, range from 0.27 to 0.50 log points per white female share while the same metrics for white spending range from 0.54 to 0.70 log points. Although the coefficients differ in Table 6, the implied suffrage effect for both black and white education spending is the same.

The possibility that black spending increased *at all* following the enfranchisement of predominantly white women evokes Myrdal's (1944) paradox: given widespread disenfranchisement of blacks in the South, why were they provided any public services? And then why were blacks provided *more* public education resources following the Nineteenth Amendment? The answer lies beyond the scope of this paper and the data at hand, but three possibilities are worthy of note. First, Margo (1991) proposed that Tiebout sorting by black families led localities to compete for black labor by supporting black schools; perhaps new women voters were more attuned to the importance of black labor. Second, education leaders may have held the perception that new women voters were more altruistic toward the welfare of black families and schools. Last, the observed gains in black spending may have been driven by omitted factors affecting both white and black school systems. Still, even if *no* post-1921 changes in black spending are attributable to suffrage, and if the trajectory of black spending is an adequate counterfactual to the trajectory of white spending, the implication is that women's suffrage was nevertheless responsible for a large share of the overall rise in white education spending after and including 1921.

The finding that white schools benefitted far more from suffrage than did black schools suggests that the ratio of black to white school expenditures, the so-called education "gap," suffered at the hands of the Nineteenth Amendment. Indeed, we estimate in the second rows of Tables 4 - 5 that the relative quality of black schools fell substantially and significantly following women's suffrage. The mean value of black to white spending per pupil, multiplied by 100, was 23.8 over this period (see Table 3), and we estimate that each percentage point increase in the white female population percentage decreased this ratio by 0.52 to 0.57 (Tables 4 - 5).[37] Taking the more conservative point

[37]We note that this ratio is very noisy, however, as it is derived from four series of transcribed data: white and black spending, and white and black enrollment. R^2 statistics indicate that Equation 1 estimates of the black-white ratio have much less overall explanatory power than estimates

estimates from Table 5, the treatment effect of suffrage on the black-white expenditure ratio was between 15 and 16 cents per dollar of white school spending. To scale this effect, we note that the average pre-suffrage ratio of black to white per pupil expenditures was 27.2 and the post-suffrage ratio in the same fell to 22.0. Results based on voter turnout (Table 6) echo these conclusions.

On their face, then, our estimates indicate that the entirety of the post-1920 reduction in relative black spending can be attributed to women's suffrage. The size of the coefficients also suggest that, in the absence of women's suffrage, the black/white ratio would have risen substantially after 1920 rather than exhibit such a drastic fall. For blacks in the South, female enfranchisement was a mixed blessing, likely bringing additional resources to their schools but also lowering the quality of their schools relative to their white counterparts.

4.3 Duration of Impact

Next we turn to the question of whether the estimated effects were fleeting or persistent over time. The expected timing of any spending response to voter enfranchisement is ambiguous in this context. The Nineteenth Amendment was never reversed, yet Moehling & Thomasson (2012) find evidence that the impact of suffrage on public health expenditures waned quickly as policymakers became accustomed to the female vote.

At the same time, the influx of voters following 1920 was unprecedented and it took years, if not decades, for the full impact of female enfranchisement to form (see, for example, short-term versus long-term estimates by Lott & Kenny (1999)). This was especially true in the South where female voter uptake lagged the rest of the nation but grew over time. Our own estimates of female voter turnout indicate an increase from 20.2% in 1920 to 29.0% in 1940. (See Appendix A, Figure 3). This gradual increase in female voter participation may have resulted in changes in behavior relative to the counterfactual well into the 1920s and 1930s as elected officials continued to gauge the preferences of the new electorate. In addition, because education expenditures were subject to a public budgeting process with significant lags, female voter preferences may have taken several years to find their way to expenditure outcomes. Consistent with this last conjecture, in Appendix Section E we show that spending *growth* was steeper after suffrage in counties with more local

for other outcomes.

control of school budgets.

To map out the spending changes over time, we modify our difference-in-difference estimator in Equation 1 to include 5-year time dummies:

$$Y_{ct} = \delta_0 + \delta_a T_{1921,1925} * D^F_{ct} + \delta_b T_{1926,1930} * D^F_{ct} + \delta_c T_{1931,1935} * D^F_{ct} + \delta_d T_{1936,1940} * D^F_{ct}$$

$$+\delta_2 D^F_{ct} + \theta_c + \theta_t + \beta X_{ct} + \varepsilon_{ct}, \tag{2}$$

where each T_{t_0,t_1} is an indicator for $t \in [t_0, t_1]$. This functional form allows us to measure the differential impact of suffrage over four time horizons: 1921-1925, 1926-1930, 1931-1935, and 1936-1940, each relative to the omitted window of 1910-1920.

The coefficients $\delta_a - \delta_d$ are reported in Columns 4 - 7 of Tables 4 - 6. Each column corresponds to the coefficient on the year group dummy interacted with a dosage proxy. For each outcome of interest, findings unambiguously indicate that the impact of suffrage began slowly in the early 1920s and accelerated over time with no evidence of tapering by 1940. Looking first to Table 4, the estimated impact on spending per pupil accelerated from an insignificant 0.27 log points per population share between 1921 and 1925 to 1.4 log points per population share between 1936 and 1940 - a result that is echoed by Table 5. Results using voter share (Table 6) as the proxy are smaller, but reflect the same trajectory. In this formulation, black spending gains are rarely statistically significant, but white spending follows the same increasing pattern. As a result, the impact on the ratio of black to white expenditures deepens over time, although it appears to level off after 1935 and is estimated to reverse in Table 6. These estimates are consistent with those from an event study estimator discussed in Appendix D.

Thus we conclude that women's suffrage had a substantial impact on school spending that grew over the course of the 1920s and 1930s. As a byproduct of this finding, we conclude that women's suffrage contributed to the growing black-white school quality gap at least through 1940 and perhaps longer.

4.4 The Impact on School Resources

Our final question is whether spending gains manifested as discernible changes in segregated school resources that voters would have been aware of and that might have directly affected student success. We focus on black and white teachers per pupil and average black and white teacher

salaries. Trends in these resources are depicted in Figure 2, where we see a noticeable upward shift in school resources after the 19^{th} Amendment, much more so for white school resources than for black. We estimate Equation 1 for these four outcomes. The percent of the voting-age population who were white and female in a given year (corresponding to Table 5) serves as the dosage proxy. Though this time-varying dosage is more subject to endogenous mobility, it is less subject to measurement error at points well removed from 1920, which is important for estimating the impact of suffrage on resources that are measured with less consistency than spending.

Table 7 lists results for teachers per pupil and teacher salaries. In the first two rows, the suffrage dosage proxy is correlated with positive and significant increases in white teacher salaries, but substantial reductions in the same for black teachers. By these estimates, suffrage brought an increase in white teacher salaries of between 9 and 15 percent.[38] At the same time, reductions in black salaries were on the order of 45% under a conservative point estimate of -1.5 log points. Although the estimates are imprecisely measured, we find increases in both black and white teaching forces of between 2 and 3 teachers per 1000 enrolled students.[39] This is a 5 to 10 percentage point increase above the values listed in Table 3.

Although the fiscal impact of suffrage is the main emphasis of our analysis, these results indicate that local administrators may have increased white school quality by increasing the number of teachers and paying them more and partially offset those costs by reducing the pay to black school teachers. The fact that the number of black school teachers increased commensurate with white is somewhat surprising, but given the large reduction in black teacher salaries, it is consistent with an overall negative effect on black school spending.

[38]Computed as the product of point estimates and mean values for this dosage proxy, e.g., 0.003×30.21.

[39]Note that the exclusion of one outlier 1919 entry for white teachers per pupil (one county in Georgia reported very little white enrollment in that year, likely in error) increases the sign and significance of that outcome and suggests an impact of 4 to 5 additional white teachers per 1,000 white pupils.

5 Conclusion

A steady rise in school resources over the course of the 20th century is a much-celebrated feature of the United States' education system, and an associated rise in the relative quality of black schools after 1940 has been linked to reductions in the black-white wage gap for workers later in the century. In the three Southeastern states we examine, over the years 1921 - 1940, real county-level per pupil school spending increased more than twofold over average spending from 1910 to 1920. The estimates in this paper attribute up to one-third of this increase to female voter enfranchisement via the Nineteenth Amendment. Spending gains were higher in counties with higher female representation in the electorate around 1921 and later, and higher for white schools than for black schools.

Our findings are consistent with economic models of intra-household bargaining and conceptual expectations about the localized political economy response to asymmetric voter enfranchisement. The reaction of public finance allocations to the extension of women's voting rights provides strong support for the idea that suffrage shifted and increased the pivotal voter's preferences for public education. The adoption of women's suffrage in the United States and the subsequent impact of suffrage on public education represents a historic episode that should shape expectations for the relationship between women's rights and human capital accumulation in modern developing countries. As women gain electoral power, public resources for education improve.

At the same time, these findings are an important caveat to the equalizing impacts of voter enfranchisement noted elsewhere in the literature. Cascio & Washington (2013) show that the Voting Rights Act of 1965 led to a sizable, significant increase in the ratio of black to white education expenditures. We find that, forty-five years earlier, women's suffrage resulted in the reverse. With evidence from the 19th century, Acemoglu & Robinson (2000) propose that franchise expansion in Europe led to a reduction in income inequality after 1870. In contrast, and although the black-white wage gap cannot be directly measured prior to 1940, we conjecture that selective franchise expansion in the United States exacerbated racial income inequality by limiting the relative ascent of black human capital acquisition.

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A Voter Turnout Data

This study makes use of voter turnout estimates by gender to estimate the impact of females' electoral participation on school spending and resource trends. Precise data on the gender of 1920 voters are not available at the county level. Instead, we have county-level counts of *total* votes for every general election during this period (Clubb et al., 2006). We use Bayesian methods with informative priors to infer gender vote shares for each county, relying on total turnout statistics as well as demographic data from the U.S. Census.

For each county i, we observe the number of voters V_i, the adult population N_i, and the gender composition x_{ji}, where $j = 1, 2$ indicates female and male groups, respectively.

We start by modeling the number of voters V_i as a draw from a binomial distribution:

$$V_i \sim \text{bin}\left(z_i, N_i\right), \tag{3}$$

where z_i is the probability that an individual votes. That probability in turn is the weighted sum of the group-specific probabilities. It becomes:

$$z_i = x_{1i}p_{1i} + x_{2i}p_{2i}, \tag{4}$$

where p_{1i} and p_{2i} represent the probability that a female and male votes, respectively. Our goal is to find the posterior distributions that characterize p_{ji} for $j = 1, 2$ for the 1920 election.

We follow Corder & Wolbrecht (2004) and approximate the logistic transformations θ_{ji} of the group specific probabilities p_{ji} with normal distributions. That is, $p_{ji} = \frac{\exp(\theta_{ji})}{1+\exp(\theta_{ji})}$ and $\theta_{ji} \sim \text{norm}\left(\mu_j, \tau_j\right)$, where μ_j is the mean of the distribution and τ_j is its precision. We use uninformative normal distributions as priors on μ_1 (female groups). For μ_2 (male groups), we choose normal prior distributions that reflect the turnouts observed for the 1912 and 1916 presidential election, for which women did not vote. We assume gamma distributions for the priors on the precisions τ_j for all groups.

Equations 3 and 4, along with the specified prior distributions complete the model. The model is solved through numerical simulations. Specifically, we use Metropolis-Hasting algorithms and Markov Chain Monte Carlo (MCMC) techniques. The posterior distributions of p_{1i} and p_{2i} are obtained based on the data (V_i, N_i and x_{ji}) and MCMC draws from the prior distributions of μ_j and τ_j. The point estimates of the group specific turnouts are obtained by sampling from their respective

posterior distributions. For each MCMC draw, we also compute the mean of p_{ji} across counties. The results yield the aggregate posterior distributions of p_j. The aggregate point estimates of p_j are obtained by sampling from the aggregate posterior distributions.

Our estimates indicate that female voter turnout rose over time. Figure 3 gives the estimated value of p_1 between 1920 and 1940. Female turnout rates increased steadily through 1936 before falling somewhat in the 1940 election.

B Extension: State Spending After Suffrage

The analysis in this paper is limited to the provision of state and local education resources in three Southern states. The sample is thus limited in its ability to address the responses of counties other than those located in the three states included in our panel.

Whether the three states in the panel are representative of the nation cannot be tested directly with county-level data from other states. We can, however, estimate the impact of female suffrage on *state* level education spending for these three states compared to the nation at large. We employ the same state-level finance data and socioeconomic control variables used by Lott & Kenny (1999) and Miller (2008) to replicate previous work estimating the effect of suffrage on state-level educational spending.[40] We then go on to test whether the three sampled states reacted differently than the rest of the nation.

To situate results with those of Lott & Kenny (1999) and Miller (2008), we estimate a dynamic fixed effects specification informed by both studies:

$$ln(Y_{st}) = \alpha + SUFFRAGE_{st}\beta_1 + SUFFRAGE_{st} * THREESTATE_s\beta_2$$

$$+\mathbf{X}_{st}\psi + \theta_s + \alpha(t) + \theta_s\alpha(t) + \theta_t + \varepsilon_{st}, \tag{5}$$

where $ln(Y_{st})$ is the natural log of states' per-capita education expenditures, $SUFFRAGE_{st}$ is a measure of suffrage treatment, $THREESTATE_s$ is an indicator equal to one for Alabama, Geor-

[40]We thank Larry Kenny for graciously providing these data and associated documentation. State expenditure and revenue data from 1870-1915 were originally provided to Lott and Kenny by John Wallis.

gia, and South Carolina, and $\mathbf{X}_{st}$ is a matrix of socioeconomic controls.[41] The parameter $\alpha(t)$ controls for linear trends common to all states, θ_s is a state fixed effect, and $\theta_s\alpha(t)$ controls for state-specific trends. Specifications with and without time trend controls (i.e., $(\alpha(t) + \theta_s\alpha(t))$) are estimated. Equation 5 additionally controls for year fixed effects (θ_t) since cross-state variation in the timing of women's suffrage is not collinear with any one year fixed effect. There are two measures of suffrage treatment. The first follows Lott & Kenny (1999) and defines $SUFFRAGE_{st}$, that is, the "dosage" of suffrage, to be the product of the number of years since suffrage was implemented and the share of adults who are female in a given year. Second, following Miller (2008), $SUFFRAGE_{st}$ is defined to be a binary indicator equal to one in states and years with women's suffrage.[42] Table 8 summarizes suffrage and summary statistics for the nation and the three-state sample. Clearly, the three Southern states exhibited lower spending than the rest of the country, and they had less exposure to suffrage rights prior to the 1920 Nineteenth Amendment. Although the South may not be representative in terms of the levels of spending outcomes, Equation 5 tests whether they were fundamentally different in terms of the impact of suffrage on the growth of spending after suffrage.

Equation 5 is estimated with and without an interaction between the $SUFFRAGE_{st}$ variable and the $THREESTATE_S$ indicator. Results are reported in Table 9. Columns 1 and 3 report the estimated coefficients for two different proxies of $SUFFRAGE_{st}$ without the interaction for our sample states. The Lott & Kenny (1999) replication in Column 1 measures no statistically significant increase in education expenditures while the Column 3 specification attributes to female suffrage a marginally significant 14.3 percent gain in educational spending, relative to linear

[41] Controls include the presence of a literacy test, secret ballot indicator, number of motor vehicle registrations, log population density, fraction of the population rural, fraction of the population black, fraction of the population older than 65, fraction of women working, fraction of the population illiterate, fraction of the labor force in manufacturing, fraction of the population foreign-born, and the average real manufacturing wage.

[42] These suffrage dosage measures are inappropriate to the exercise in the main text as all states in our three-state sample were forced to grant women the right to vote by federal legislation in 1920.

state-specific time trends. The second coefficients of Columns 2 and 4 measure the difference in post-suffrage spending in the three-state subsample, relative to the baseline impact in other states. Neither interaction is statistically significant, indicating that state educational spending in these three states responded no differently to suffrage than the rest of the nation.

C Robustness and Falsification Checks

The primary difference-in-difference identification strategy estimates the change in log per-pupil education spending following 1921, with fixed effects to control for unobserved heterogeneity within counties and years. In this section we present results from five robustness checks of this empirical approach before turning to pre-treatment "effects" of the suffrage dosage.

C.1 Robustness Checks

Results for the five variations are listed in Table 10. Column 1 repeats baseline results from Table 5. Coefficients represent estimates of the impact of 19^{th} Amendment dosage ($POST * D^F_{ct}$) on outcomes listed in the leftmost column. Given the strong relationship between female population shares and the overall black-white population ratio (see Table 2 and related discussion), in Column 2 we modify the X_{ct} vector of controls to control for a quadratic rather than linear function of the black-white population ratio. Point estimates, standard errors, and statistical significance are nearly unchanged relative to the baseline model.

Next, we modify dependent variables to measure per-adult (voting age) spending outcomes rather than per-pupil outcomes. Column 3 lists results for $POST * D^F_{ct}$ coefficients when Equation 1 is estimated for spending per adult of voting age. The dependent variable denominator is necessarily interpolated between census years, increasing measurement error. Results from our preferred model, reported in the main text, utilize pupil counts that are available each year of the panel. Column 3 coefficient estimates depart quantitatively from baseline results, but our interpretation of the impact of the 19^{th} Amendment is broadly consistent with inference drawn from baseline results. Higher dosages of women's suffrage from higher female population shares lead to significantly higher education spending, benefitting white students more so than black students.

Column 4 lists results when we estimate a regression adjustment model for per-pupil outcomes,

which allows the impact of $\mathbf{X}_{ct}$ covariates to change in the new regime (e.g., by letting the impact of crop value to vary across pre-suffrage and post-suffrage years). Specifically, regression adjustment ensures that the γ coefficients are not also incorporating the effects of control variables whose impact may have changed in 1920. The regression adjustment estimating equation is as follows:

$$ln(Y_{ct}) = \gamma_0 + \gamma_1 POST_t + \gamma_2 POST_t D_{ct}^F + \mathbf{X}_{ct}\beta + POST_t(\mathbf{X}_{ct} - \bar{\mathbf{X}})\Psi + \theta_c + \varepsilon_{ct}, \quad (6)$$

where $\bar{\mathbf{X}}$ is a vector of means and other variables defined as before. In expectation, the average treatment effect evaluated at the sample mean is $(\gamma_1 - \gamma_0) + \gamma_2 D_{ct}^F$. Table 10 lists $\hat{\gamma}_2$ estimates in Column 4. The impact of dosage on per-pupil spending overall is very similar with regression adjustment, although the impact of dosage on white per-pupil spending is estimated to be much larger than in the baseline model, leading to a much more negative impact on the ratio of black to white per-pupil spending. Our baseline results, in that sense, are conservative.

The principal threat to our basic difference-in-difference identification strategy is the possibility that areas with higher dosages of women's suffrage – that is, areas with higher female population shares – possess an inherent trend toward higher education spending regardless of women having the right to vote. If so, a difference-in-difference estimator could detect significant but biased shifts after 1920 in these locations.

One strategy to address this threat is to control for interactions between a linear time trend and pre-"treatment" (pre-suffrage) observable variables, like so:

$$ln(Y_{ct}) = \delta_0 + \delta_1 POST * D_{ct}^F + \delta_2 D_{ct}^F + \theta_c + \theta_t + \beta_1 X_{ct} + \beta_2 t * X_c^{1920} \varepsilon_{ct}, \quad (7)$$

where X_c^{1920} is the vector of observed economic and population covariates as of 1920 and t is a time trend.[43] In addition to observables included in the main analysis (summarized at the bottom of Table 3) we control for county-wide literacy rates among the population over ten years of age.[44] If there are different time trends in high-dosage locations, and these trends are correlated with an

[43] See Hoynes & Schanzenbach (2009) and Carruthers & Wanamaker (2013) for other non-experimental applications of this method. It is not feasible to replace X_{ct}^{1920} with D_{ct}^F in Equation 7 as the differential trend by dosage is precisely the object of interest.

[44] Haines, Michael R. and Inter-university Consortium for Political and Social Research (2010).

observable in X_c^{1920}, they will be accounted for in the $t * X_c^{1920}$ term in Equation 7. The parameter δ_1 is then estimated net of these trends.

Column 5 of Table 10 lists coefficient estimates for δ_1 after controlling for these trends. The overall impact of suffrage dosage on per-pupil spending is mitigated but still positive and statistically significant, and the ratio of nominal per-pupil black to white spending falls by roughly the same percent as in baseline results.

Lastly, Column 6 lists results from the main Equation 1 specification with the addition of controls for county-wide literacy. Literacy is a sensible proxy for countywide human capital, but we omit this from the list of controls in our main analysis for two reasons. First, available literacy data cover all persons over ten years of age, which may be endogenous to improving school quality. Second, we only observe literacy as of 1910, 1920, and 1930, so data for the last ten years of the panel must be extrapolated. Nevertheless, controlling for this imperfect measure of literacy has almost almost no bearing on point estimates. Column 6 results are nearly identical to baseline results in Column 1.

C.2 Pre-treatment and Falsification Tests

It remains possible that heterogeneous, unobserved, and endogenous trends are present and induced increases in education spending in high-dose counties, even in the absence of women's suffrage. Though we cannot test this possibility directly, we can use pre-suffrage spending and population data to (1) assess the importance of our key dosage proxy in determining trends in educational spending *prior* to suffrage and (2) project spending outcomes as if the 19^{th} Amendment never happened. Specifically, we estimate the following for school years 1910 up to and including 1920, the last fiscal year before district leaders faced an expanded electorate:

$$ln(Y_{ct}) = \delta_0 + \delta_1 trend + \delta_2 D_{ct}^F + \delta_3 trend * D_{ct}^F + \theta_c + \beta X_{ct} + \varepsilon_{ct}, \tag{8}$$

where D_{ct}^F is the share of the voting-age population who are white females in a given year t (analogous to Table 5), $trend$ is a linear time trend, θ_c is a county fixed effect, and X_{ct} includes control variables defined above (with the exception of secret ballot laws and Rosenwald school controls, which were not present until after 1920, and with the addition of countywide literacy rates interpolated between 1910 and 1920). Table 11 lists point estimates for δ_1, δ_2, and δ_3 coefficients for our

four dependent variables of interest.

The first row of Table 11 results shows that pre-suffrage conditional trends in school spending were downward for black schools and insignificantly sloped for white schools. The second row of estimates indicates that counties with higher shares of white voting-age females tended to realize lower black and white school spending, although the log-sum of black and white spending exhibited no significant association with female population shares. Most important are results for the interaction $trend * D^F_{ct}$. If high-dose counties were on a differentially upward spending trajectory prior to suffrage, a coincidental acceleration of that trend around 1921 would manifest as a spurious "effect" of suffrage dosage. However, prior to 1921, we observe no significant log spending trends in high-dose counties. Point estimates for log spending outcomes are very small and insignificant. Interestingly, the nominal ratio of black to white spending was on a downward path in incrementally more female counties, although the 0.09 point estimate is a modest fraction of our main result for this outcome (-0.57 per Table 5, Column 1).

To underscore the point that pre-suffrage conditions do not foretell post-suffrage outcomes, Figure 4 plots actual spending outcomes (dots) against projections (lines) from predicted values of Equation 8. Point estimates from the 1910-1920 model are fit to observed right-hand-side variables from 1921 and later. Solid lines trace average predictions (unweighted) across counties. The pre-suffrage model does a very poor job of fitting 1921 and later outcomes, greatly understating the path of each outcome's realized time series.[45] Which is to say that – based on observable county characteristics, prevailing trends, and county fixed effects – post-suffrage school spending significantly exceeded expectations.

To summarize robustness and falsification exercises, we find that results from the principal identification strategy are robust – with important caveats regarding economic significance – to denominator changes in the dependent variable and two alternative treatments of time-varying trends in the effect of other control variables. Moreover, pre-suffrage estimates of spending outcomes suggest that counties with more females were not on differential spending trajectories prior to the 19^{th} Amendment. Overall, the evidence indicates that the 19^{th} Amendment is responsible in part

[45] Allowing for higher-ordered time trends exacerbates the poor performance of the pre-suffrage model.

for post-1920 gains in educational spending, and that white students benefitted more than black students.

D Event Study Estimates

Other econometric tools are available to identify the effect in question in this paper. In particular, the post-1921 differential impact of women's suffrage dosage can be identified using an event study estimator. The event study estimator is conceptually similar to a difference-in-difference estimator, but the treatment effect is estimated in each year after the event in question. Pre-treatment effects are estimated as a falsification test.

In this context, the event study estimator traces the impact of suffrage dose over time by replacing the $POST_t * D^F_{ct}$ in Equation 1 with a set of interactions between year fixed effects and D^F_{ct}. For dosage, we utilize time-varying white female population shares (i.e., the dosage proxy from Table 5 results) and estimate the following:

$$Y_{ct} = \delta_0 + \sum_{\kappa=1910}^{1940} \delta_\kappa 1(t = \kappa) * D^F_{c\kappa} + \delta_2 D^F_{ct} + \theta_c + \theta_t + \beta X_{ct} + \varepsilon_{ct}, \tag{9}$$

where variables are defined as for Equation 1 above. Each δ_κ measures the suffrage treatment effect in that year. County fixed effects and year fixed effects account for variation in outcomes that are constant across counties or years.

We present the point estimates for $\delta_{1910} - \delta_{1940}$ when Y_{ct} represents per pupil expenditures in Panel A of Figure 5. Estimates are noisy, but the treatment effect of suffrage hovers around zero for 1910-1920 before beginning a steady upward climb. The treatment effect is consistently positive and frequently statistically significant after 1925, and reflects the increasingly important role of suffrage over time (also documented in Tables 4 - 6).

Estimates of the suffrage treatment effect on white expenditures (Panel B of 5) follow a pattern similar to that for overall expenditures with statistically significant treatment effects in the 1930s. The annual treatment effect on black expenditures increases more slowly after 1921 and tapers off more quickly in the 1930s (Panel C). Point estimates in each case match those in the main results, which reflect and average effect across 1921-1940.

Pre-1920 estimates serve as an important falsification test. Namely, as Figure 4 makes clear, school expenditures suffered substantially during the World War I era. If these reductions are some-

how related to the suffrage proxies incorporated in our analysis, the resulting estimates, although unbiased, are measuring not the impact of suffrage but, rather, the impact of having a particular population structure in the World War I years as captured by the proxies. Clearly this is not the effect of interest.

Contrary to what we might expect if the World War I impact was correlated with suffrage dosage proxies, we observe no significant "effect" of suffrage in the pre-1920 years. The point estimates hew close to zero and show no particular deviation in the war years. This is consistent with the interpretation that the relationship between spending and the suffrage dosage proxies is attributable to suffrage *per se*.

In short, event study estimates, although noisier than our main results, reflect a similar pattern of suffrage treatment effects on school expenditures in these three southern states.

E Duration of Impact - Extension

In Section 4.3 of the main text, we discuss results showing that the impacts of suffrage were larger in later years of the panel. This begs the question of why female enfranchisement should have taken so much time to impact local public education, since the potential electorate shifted sharply beginning with the 1920-1921 school year. Even if the new electorate proved to be a strong and credible threat, policymakers may have been constrained in their ability to quickly shift large amounts of public expenditures toward school budgets.

We posit that counties where local revenues (from property, poll, and miscellaneous local taxes) account for a larger share of school expenditures just prior to suffrage would have been able to more flexibly respond to enfranchisement in the years following. With this idea in mind, we estimate a differenced version of Equation 1 and replace the dosage proxy with the ratio of locally-sourced school revenues to total school expenditures (as in other parts of the study, by race) to better understand the relationship between local control and *growth* in spending items of interest.

The local tax data on hand are not ideal for testing the overall revenue response to suffrage,[46]

[46]We do not observe the total value of all local tax collections, but only the portion appearing in school budgets. Furthermore, local tax revenue data are sporadically and inconsistently reported in the state reports we have transcribed.

but nevertheless, serve as valuable (albeit noisy) measures of the tax base readily available to public schools.

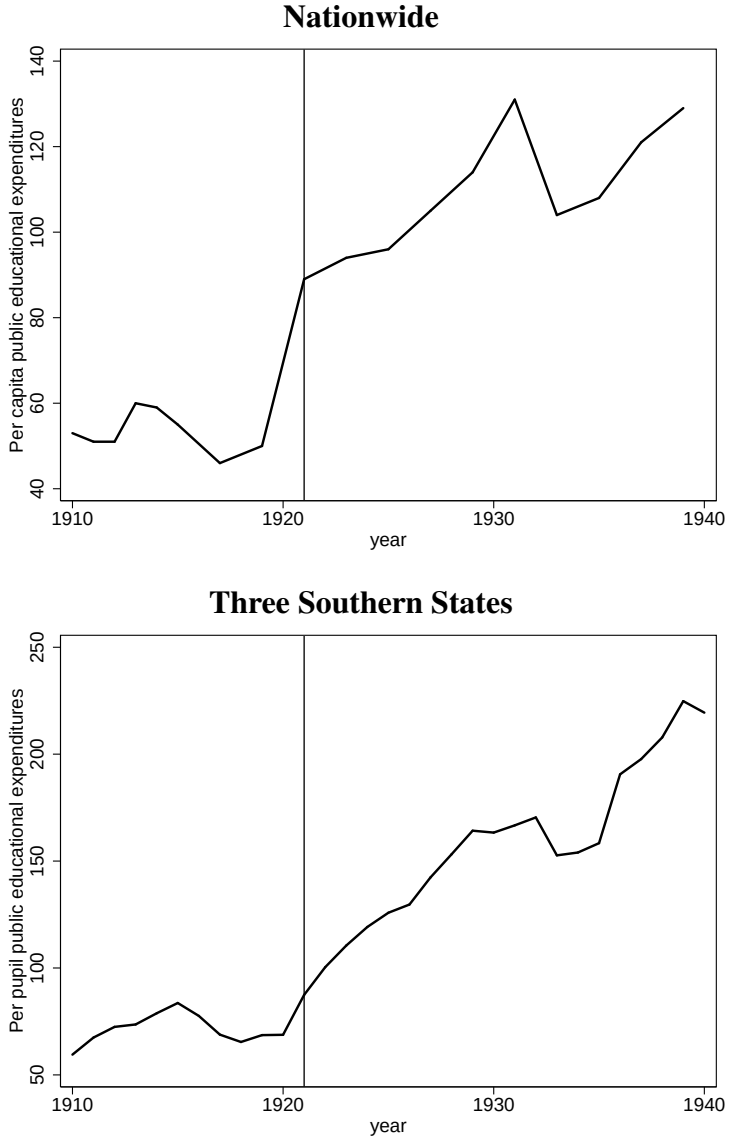
Specifically, we estimate the following:

$$\Delta Y_{ct} = \tilde{\delta}_0 + \tilde{\delta}_1 \Delta D_c^F + \tilde{\delta}_2 \Delta POST * D_c^F + \beta \Delta X_{ct} + \varepsilon_{ct}, \tag{10}$$

where D_c^F is the (0-100) percent of school expenditures accounted for by local revenues and other variables are defined as before. Point estimates for $\tilde{\delta}_1$ and $\tilde{\delta}_2$ are in Table 12. The first coefficient of interest, $\tilde{\delta}_1$, estimates the pre-suffrage relationship between spending growth and incremental increases in the local control of school spending. Results for $\tilde{\delta}_1$ in Column 1 indicate that counties with more local control of school spending were generally on a declining spending path prior to suffrage. Column 2 lists point estimates for $\tilde{\delta}_2$, a gage of whether incrementally greater shares of local funds are associated with deviations from underlying Column 1 trends. Indeed, growth in per-pupil spending (total, black, and white) appears to be steeper after suffrage in counties with more local control of school budgets. Thus, the new electorate may have taken some time to be fully reflected in school resources because local leaders were constrained by limited discretionary funds.

Tables and Figures

FIGURE 1: Trends in per capita public educational expenditures (1982-1984 dollars)



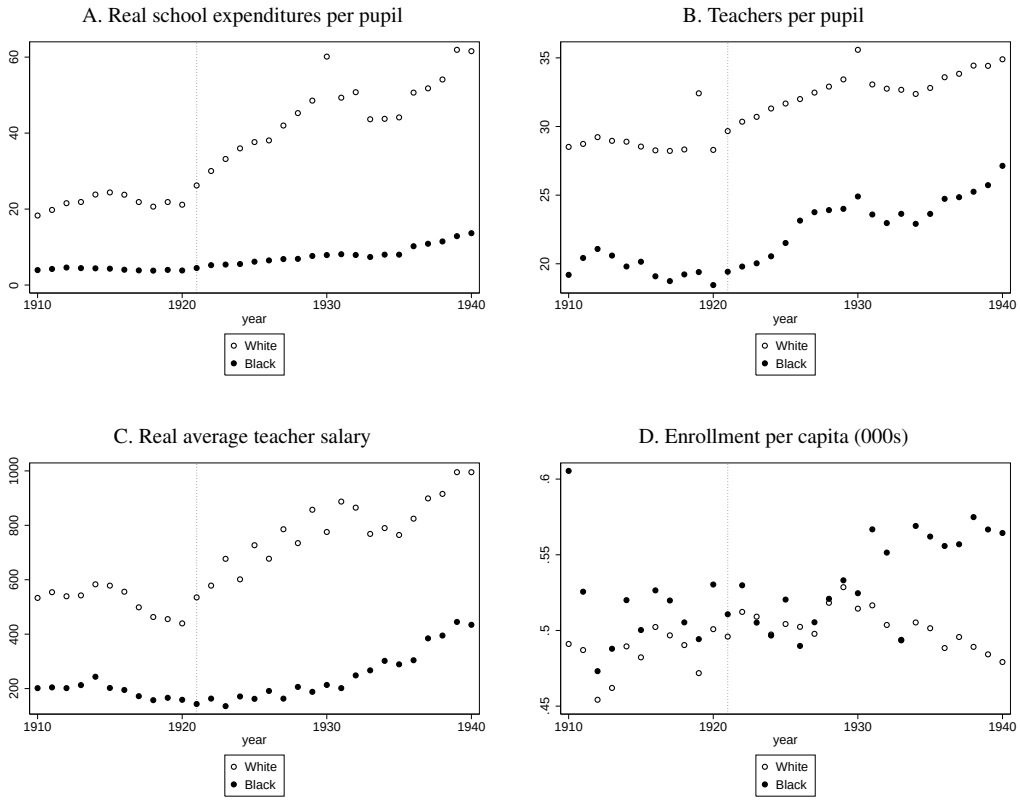
Source: Nationwide: Authors' calculations, Carter et al. (2006). Three Southern States: Authors' calculations and annual reports of states' Department of Education or equivalent office in AL, GA, and SC.

TABLE 1: Distribution of Revenue Receipts for U.S. Schools, 1919/1920-1969/70

	Federal	State	Local
1919-20	0.3	16.5	83.2
1929-30	0.4	16.9	82.7
1939-40	1.8	30.3	68.0
1949-50	2.9	39.8	57.3
1959-60	4.4	39.1	56.5
1969-70	8.0	39.9	52.1

Notes: Percent of total revenue receipts for United States public schools emanating from each fiscal source.
Source: Snyder et al. (2008)

FIGURE 2: Trends in per pupil expenditures, teachers per pupil, average teacher salaries, and enrollment per capita, 1910-1940



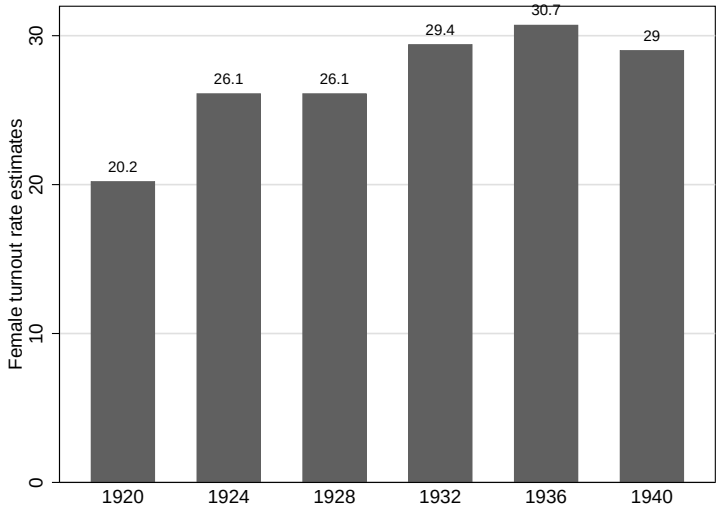
Source: Authors' calculations and numerous annual reports of states' Department of Education or equivalent office. County-level data are averaged across years without weights. Salary and expenditure statistics are in inflation-adjusted 1925 dollars.

TABLE 2: Correlates of 1920 white female population shares

Observable county characteristic	Coefficient (st. err.)
Total Population (000s)	-0.950 (0.79)
Crop Value Per Capita	-0.0106* (0.0053)
Percent of Land Devoted to Agriculture (0-100)	0.0459** (0.022)
Black-white Ratio (x100)	-7.97*** (0.68)
Average Annual Manufacturing Earnings	0.000114 (0.0027)
Percent of Adults in Manufacturing (0-100)	-0.160** (0.063)
Observations (counties)	236
R^2	0.75

Source: Authors' calculations and numerous annual reports of three Southern states' Department of Education or equivalent office. The table lists results of a simple regression of the percent of counties' 1920 population who are white females (0-100 percent) against other observable features of counties in 1920, restricted to counties with observable expenditures per pupil in 1920. Robust standard errors are in parentheses below each coefficient. *** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

FIGURE 3: Estimates of female voter turnout rates, 1920-1940



Source: Nationwide: Authors' calculations from Clubb et al. (2006) for AL, GA, LA, SC, NC.

TABLE 3: County schooling resources and population summary statistics, 1910-1940

	(1)	(2)
	Mean	Standard deviation
Dependent variables		
ln(per-pupil educational expenditures)	2.94	(0.57)
ln(per-pupil white educational expenditures)	3.40	(0.65)
ln(per-pupil black educational expenditures)	1.67	(0.66)
Ratio of black to white per-pupil expenditures (x100)	23.83	(26.13)
ln(ave. white teacher salary)	6.43	(0.44)
ln(ave. black teacher salary)	5.24	(0.75)
White teachers per 1000 pupils	31.39	(13.36)
Black teachers per 1000 pupils	21.98	(9.48)
Suffrage dosage measures		
Percent of electorate white female	30.21	(9.85)
Percent of 1920 electorate white female	29.85	(9.27)
Percent of 1920 electorate female voters	7.00	(4.07)
Socioeconomic control variables		
Population (in thousands)	30.51	(35.84)
Crop value per capita	115.31	(55.20)
Percent of land devoted to agriculture	65.0	(15.8)
Black-white population ratio	1.08	(1.03)
Secret ballot (Georgia 1922 and later)	0.33	(0.47)
Average annual manufacturing earnings	635.82	(189.01)
Percent of adults in manufacturing	6.71	(6.62)
New Rosenwald classrooms (non-zero from 1921-1933)	0.36	(1.60)
n (county-years)	7,148	

Source: Authors' calculations and numerous annual reports of three Southern states' Department of Education or equivalent office.

TABLE 4: Estimated changes in local educational spending *per pupil*, after the Nineteenth Amendment
Dosage Proxy: Percent White Female Population in 1920

	20-Year Window (through 1940)			1921-1925	1926-1930	1931-1935	1936-1940
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Coefficient on $POST_t$*D^F_{ct}							
ln(All Spending per Pupil)	0.0069***	0.0086***	0.0065***	0.0027	0.0069***	0.010***	0.014***
	(0.0017)	(0.0017)	(0.0015)	(0.0019)	(0.0020)	(0.0020)	(0.0022)
Ratio of Black to White Spending Per Pupil	-0.57***	-0.56***	-0.61***	-0.46**	-0.53**	-0.71***	-0.73***
	(0.16)	(0.16)	(0.13)	(0.18)	(0.21)	(0.16)	(0.16)
ln(White Spending per Pupil)	0.0054***	0.0070***	0.0073***	0.0010	0.0051**	0.0081***	0.014***
	(0.0018)	(0.0018)	(0.0016)	(0.0019)	(0.0022)	(0.0022)	(0.0024)
ln(Black Spending per Pupil)	0.0027	0.0043**	0.00043	0.0022	0.0063**	0.0018	-0.0015
	(0.0021)	(0.0019)	(0.0020)	(0.0023)	(0.0027)	(0.0030)	(0.0030)
Year Fixed Effects	Y	N	Y	Y	Y	Y	Y
State-Year Fixed Effects	N	Y	N	N	N	N	N
Interpolated Controls	Y	Y	N	Y	Y	Y	Y
Observations	7,147	7,147	7,147	7,147	7,147	7,147	7,147
Number of counties	235	235	235	235	235	235	235

Notes: Coefficient estimates of Equations 1 (Columns 1-3) and 2 (Columns 4-7) with *per pupil* expenditures (in natural logs) or the ratio of black to white per-pupil expenditures as the dependent variable. Cluster-robust (by county) standard errors are in parentheses below each coefficient. *** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

TABLE 5: Estimated changes in local educational spending *per pupil*, after the Nineteenth Amendment
Dosage Proxy: Percent White Female Population in Each Year

	20-Year Window (through 1940)			1921-1925	1926-1930	1931-1935	1936-1940
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Coefficient on $POST_t$*D^F_{ct}							
ln(All Spending per Pupil)	0.0070***	0.0085***	0.0063***	0.0026	0.0072***	0.010***	0.014***
	(0.0016)	(0.0016)	(0.0014)	(0.0018)	(0.0020)	(0.0019)	(0.0020)
Ratio of Black to White Spending Per Pupil	-0.53***	-0.52***	-0.54***	-0.42***	-0.50***	-0.67***	-0.68***
	(0.16)	(0.16)	(0.12)	(0.17)	(0.20)	(0.16)	(0.16)
ln(White Spending per Pupil)	0.0055***	0.0070***	0.0067***	0.00084	0.0056***	0.0083***	0.013***
	(0.0018)	(0.0018)	(0.0015)	(0.0019)	(0.0022)	(0.0022)	(0.0023)
ln(Black Spending per Pupil)	0.0032	0.0050***	0.0010	0.0027	0.0067***	0.0024	-0.0010
	(0.0020)	(0.0019)	(0.0018)	(0.0022)	(0.0026)	(0.0028)	(0.0027)
Year Fixed Effects	Y	N	Y	Y	Y	Y	Y
State-Year Fixed Effects	N	Y	N	N	N	N	N
Interpolated Controls	Y	Y	N	Y	Y	Y	Y
Observations	7,147	7,147	7,147	7,147	7,147	7,147	7,147
Number of counties	235	235	235	235	235	235	235

Notes: Coefficient estimates of Equations 1 (Columns 1-3) and 2 (Columns 4-7) with *per pupil* expenditures (in natural logs) or the ratio of black to white per-pupil expenditures as the dependent variable. Cluster-robust (by county) standard errors are in parentheses below each coefficient. *** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

TABLE 6: Estimated changes in local educational spending *per pupil*, after the Nineteenth Amendment
Dosage Proxy: Female Voter Percentage in 1920

	20-Year Window (through 1940)			1921-1925	1926-1930	1931-1935	1936-1940
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Coefficient on $POST_t$*D^F_{ct}							
ln(All Spending per Pupil)	0.0099**	0.0182***	0.0115**	-0.0015	0.0115**	0.0161***	0.0221***
	(0.0047)	(0.0051)	(0.0045)	(0.0062)	(0.0058)	(0.0051)	(0.0056)
Ratio of Black to White Spending Per Pupil	-1.29**	-1.34**	-1.51***	-1.07*	-1.17	-1.68***	-1.46***
	(0.55)	(0.57)	(0.49)	(0.59)	(0.75)	(0.53)	(0.54)
ln(White Spending per Pupil)	0.0084	0.0147**	0.0141***	-0.0040	0.0083	0.0143**	0.0252***
	(0.0052)	(0.0057)	(0.0051)	(0.0052)	(0.0063)	(0.0069)	(0.0073)
ln(Black Spending per Pupil)	-0.0070	0.0045	-0.0102*	-0.0094	0.0027	-0.0110	-0.0117
	(0.0070)	(0.0065)	(0.0060)	(0.0075)	(0.0115)	(0.0099)	(0.0099)
Year Fixed Effects	Y	N	Y	Y	Y	Y	Y
State-Year Fixed Effects	N	Y	N	N	N	N	N
Interpolated Controls	Y	Y	N	Y	Y	Y	Y
Observations	7,147	7,147	7,147	7,147	7,147	7,147	7,147
Number of counties	235	235	235	235	235	235	235

Notes: Coefficient estimates of Equations 1 with *per pupil* expenditures (in natural logs) or the ratio of black to white per-pupil expenditures as the dependent variable. Bootstrapped standard errors are in parentheses. *** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

TABLE 7: Estimated changes in teacher salaries and teachers *per pupil* after the Nineteenth Amendment
Dosage Proxy: Percent White Female Population in Each Year

	Observations	Counties	20-Year Window (through 1940)		
			(1)	(2)	(3)
Coefficient on $POST_t$*D^F_{ct}					
ln(ave. white teacher salary)	7,115	235	0.003*	0.004***	0.005***
			(0.002)	(0.002)	(0.001)
ln(ave. black teacher salary)	7,115	235	-0.025***	-0.015***	-0.018***
			(0.006)	(0.005)	(0.004)
white teachers per 1000 pupils	7,145	235	0.086	0.087	0.010
			(0.079)	(0.082)	(0.063)
black teachers per 1000 pupils	7,145	235	0.076	0.087	0.002
			(0.071)	(0.072)	(0.054)
Year Fixed Effects			Y	N	Y
State-Year Fixed Effects			N	Y	N
Interpolated Controls			Y	Y	N

Notes: Coefficient estimates of Equations 1 with teachers per pupil, or the log of inflation-adjusted average teacher salary as the dependent variable. Cluster-robust (by county) standard errors are in parentheses below each coefficient. *** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

TABLE 8: Spending and suffrage summary statistics, three-state sample versus all states (1870 - 1940)

	(1)	(2)
Variable	All states	Three-state subsample
ln(total expenditures)	2.816	2.052
	(0.963)	(0.962)
ln(education expenditures)	1.312	0.790
	(1.212)	(1.313)
$YEARSINCE_{st}$*Percent Female	0.200	0.166
	(0.238)	(0.238)
Female suffrage (0,1)	0.415	0.329
	(0.493)	(0.471)
n (state-years)	1,882	122

Source: Authors' calculations and Lott & Kenny (1999). Cluster-robust (by state) standard deviations are in parentheses. $YEARSINCE_{st}$ is equal to the number of since elapsed since full suffrage. $pctfem$ is the share of the adult population that is female. $SUFF_{st}$ is an indicator for full suffrage in state s in year t, which is achieved for all states by 1920.

TABLE 9: Estimated changes in state education expenditures after women's suffrage, three-state sample versus all states

	(1)	(2)	(3)	(4)
Suffrage proxy	$YEARSINCE_{st}$*Percent female		$SUFF_{st}$	
Time trend controls	No	No	Yes	Yes
$SUFFRAGE_{st}$	-0.132	-0.121	0.143*	0.150*
	(0.178)	(0.179)	(0.086)	(0.087)
$SUFFRAGE_{st} * THREESTATE_s$		-0.220		-0.160
		(0.383)		(0.230)
Observations	1,827	1,827	1,827	1,827
Adjusted R-squared	0.71	0.71	0.78	0.78

Notes: Selected coefficient estimates from Equation 5 for state-level public expenditures. $YEARSINCE_{st}$ is equal to the number of since elapsed since full suffrage. $SUFF_{st}$ is an indicator for full suffrage in state s in year t, which is achieved for all states by 1920. Time trend controls ($\alpha(t) + \theta_s * \alpha(t)$ in Equation 5) are excluded in Columns 1 and 2. Cluster-robust (by state) standard errors are in parentheses below each coefficient. Additional controls include socioeconomic variables listed in the text, state fixed effects, and year fixed effects.
*** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

TABLE 10: Estimated changes in local educational spending after the Nineteenth Amendment: Robustness Checks
Dosage Proxy: Female Voter Percentage in Each Year

	(1)	(2)	(3)	(4)	(5)	(6)
Model	Baseline	Quadratic black-white ratio control	Spending per adult	Regression adjustment	$t * X_{ct}^{1920}$ interactions	Literacy controls
ln(All Spending)	0.0070***	0.0069***	0.0040**	0.0078***	0.0049***	0.0070***
	(0.0016)	(0.0016)	(0.0017)	(0.0029)	(0.0019)	(0.0016)
Ratio of Black to White Spending	-0.53***	-0.54***	-1.83*	-0.82***	-0.49***	-0.53***
	(0.16)	(0.15)	(1.12)	(0.25)	(0.19)	(0.16)
ln(White Spending)	0.0055***	0.0057***	0.0036*	0.0103***	0.0052**	0.0054***
	(0.0018)	(0.0018)	(0.0019)	0.0035)	(0.0021)	(0.0018)
ln(Black Spending)	0.0032	0.0029	-0.0052*	0.0044	0.0069**	0.0031
	(0.0020)	(0.0020)	(0.0029)	(0.0043)	(0.0029)	(0.0020)
Year Fixed Effects	Y	Y	Y	Y	Y	Y
State-Year Fixed Effects	N	N	N	N	N	N
Interpolated Controls	Y	Y	Y	Y	Y	Y
Observations	7,134	7,134	7,134	7,134	7,134	7,134
Number of Counties	232	232	232	232	232	232

Notes: Baseline results for the impact of suffrage dosage on per-pupil spending and the black-white per-pupil ratio ($POST_t D_{ct}^F$ from Equation 1) are in Column 1. Column 2 adds the square of each county's interpolated black-white ratio to the list of controls. Column 3 substitutes per-pupil dependent variables for per-adult of voting age dependent variables. Column 4 lists $POST_t D_{ct}^F$ coefficient estimates from regression adjustment (Equation 6). Column 5 lists $POST_t D_{ct}^F$ coefficient estimates from Equation 7, which modifies the basic difference-in-difference model by including controls for interactions between a linear time trend and 1920 covariates. Column 6 adds over-10 literacy rates to the lost of controls.
*** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

TABLE 11: Spending Trends and Female Population Shares Prior to Suffrage

	(1)	(2)	(3)	(4)
Outcome	All Spending	Ratio of Black to White Spending	White Spending	Black Spending
$trend$	-0.0087*	-1.0189**	0.0033	-0.0214***
	(0.0051)	(0.5093)	(0.0060)	(0.0066)
D_{ct}^F	-0.0096	-0.3471	-0.0153**	-0.0264***
	(0.0066)	(0.5325)	(0.0070)	(0.0067)
$trend * D_{ct}^F$	-0.0003	-0.0843**	0.0003	-0.0003
	(0.0003)	(0.0401)	(0.0003)	(0.0005)
Observations	2,540	2,540	2,540	2,540
Number of Counties	234	234	234	234

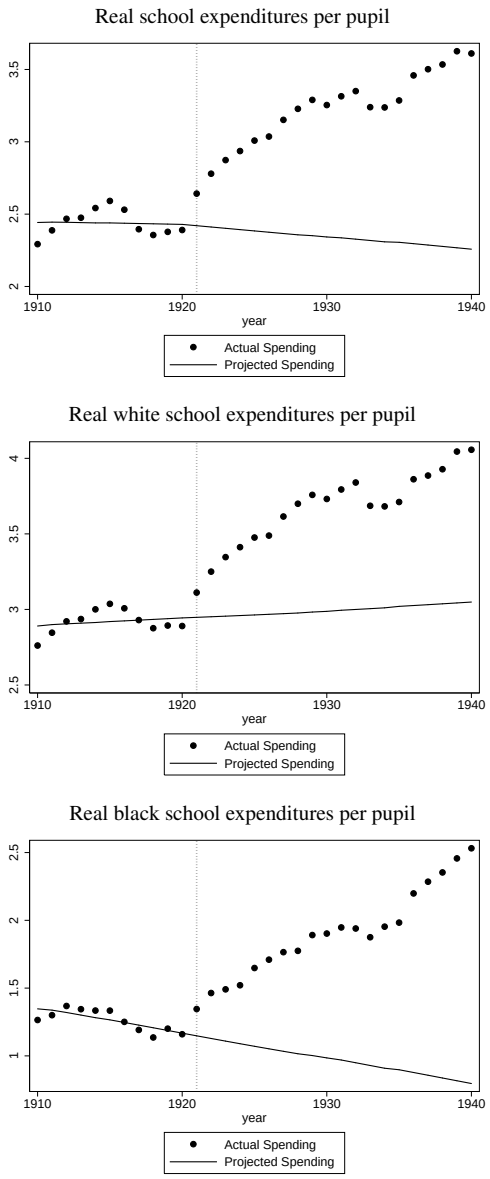
Notes: Selected results from Equation 8, a pre-suffrage model of school expenditures and population characteristics.
*** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

TABLE 12: Equation 10 Results: Local Control and Growth in School Spending After Suffrage

	(1)	(2)
Coefficient on	D_{ct}^F	$POST_t * D_{ct}^F$
ln(All Spending per Pupil)	-4.6E-04***	7.5E-04***
	(07.0E-05)	(7.0E-05)
Ratio of Black to White Spending per Pupil	2.4E-03	1.4E-03
	(0.009)	(0.013)
ln(White Spending per Pupil)	-4.1E-04***	7.0E-04***
	(1.1E-04)	(1.0E-04)
ln(Black Spending per Pupil)	-5.9E-04***	1.1E-03***
	(9.0E-05)	(1.1E-04)
Observations	6,775	
Number of Counties	227	

Notes: The table lists results for the impact of local control (ratio of locally-sourced school revenues to total school expenditures, by race) on *growth* in per-pupil spending and the black-white per-pupil ratio. Point estimates for D_c^F in Equation 10), the pre-suffrage trend in counties with incrementally more local control are in Column 1. Point estimates for $POST_t D_c^F$, representing the post-suffrage deviation from underlying trends, are in Column 2. *** indicates statistical significance at 99% confidence (with respect to zero), ** at 95%, and * at 90%.

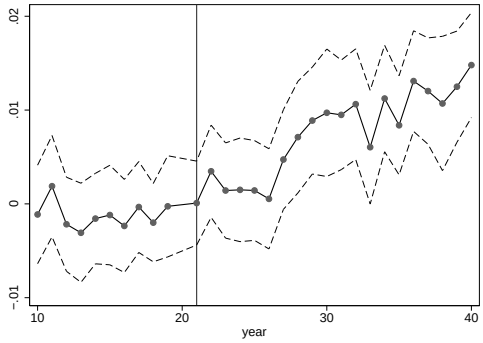
FIGURE 4: Actual Spending Outcomes Versus Projected Outcomes Based on Pre-Suffrage Estimates



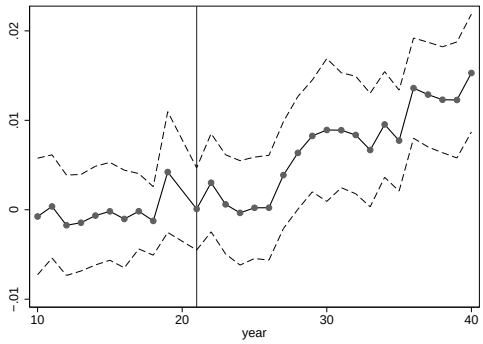
Source: Authors' calculations and numerous annual reports of states' Department of Education or equivalent office. County-level data are averaged across years without weights. Figures plot actual spending outcomes (dots) against predicted values (lines) from the pre-suffrage spending model (Equation 8).

FIGURE 5: Event Study Coefficients and 95% Confidence Intervals

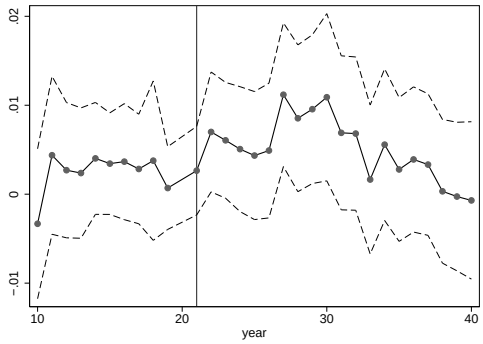
A. Real school expenditures per pupil



B. Real white school expenditures per pupil



C. Real black school expenditures per pupil



Source: Authors' calculations and numerous annual reports of states' Department of Education or equivalent office. Estimates from Equation 9 with 95 percent confidence intervals.