Low Fees, Large Barriers to Education: Evidence from Rate Bill Abolition in the United States[*]

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Abstract

Until the late 19th century, families in some municipalities paid small user fees, called rate bills, for their children to attend public schools. Urban school districts gradually repealed these fees and funded public education through local taxes. States eventually abolished rate bills, forcing rural areas to provide public education without tuition requirements. Using United States Census data and a staggered adoption difference-in-differences approach, I show that state-level rate bill abolition increased rural primary school attendance by 7.2 percentage points. These results suggest that small costs can be an obstacle to school attendance and inhibit the diffusion of education.

Keywords: free public education, tuition, rate bill abolition, primary school attendance

JEL Codes: N31, I22, H75

1   Introduction

During the late 19th century, the United States became one of the most educated societies in the world. 97.1% of all children between the ages of 5 and 14 were enrolled in primary school in 1890 (Lindert, 2004). A large fraction of these children attended public school: 89.6% of children aged 5-14 were enrolled in public schools by 1910, compared to only 54.6% enrolled in public schools in 1830 (Lindert, 2004). Over this time period, public schools became free to attend through the abolition of tuition payments and other fees, shifting the burden of education funding from students and their families to society writ large. This shift predates the first wave of compulsory schooling laws (CSL's), which eventually made primary school mandatory to attend.[1] This paper analyzes the impact of state-level rate bill abolition laws, which prevented public schools from charging tuition, on attendance in rural areas. Although these 19th century rate bills were small relative to income, I find that their abolition led to increased attendance in rural areas, implying that even low levels of tuition can inhibit educational attainment.

There is limited evidence on the effect of rate bills on attendance in the United States. The 1826 imposition of a rate bill between 25 cents and 2 dollars per academic quarter in New York City led to a 13% drop in attendance (Cubberley, 1919). For those more advanced courses that were associated with the $2 fee, the drop in attendance was above 90%. Go (2015) studies the effect of rate bill abolition in Connecticut and finds that larger initial rate bills, and therefore larger decreases at abolition, led to larger increases in attendance when the state banned tuition requirements in 1868. However, this is the first paper to investigate the abolition of rate bills across multiple states using the exogenous implementation of free public schools in rural areas.

Using data from the United States Decennial Censuses of 1850 through 1880, I investigate the effect of state-level rate bill abolition laws on attendance in rural areas using a staggered adoption difference-in-differences framework. These policies were effectively imposed upon rural areas, via state legislatures or referenda, by urban areas, which had already repealed rate bills prior to these state-level bans. I find that the abolition of rate bills increased rural attendance by 7.2 percentage points for individuals below the age of seven when the laws were passed. These results imply an increased average

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[1]Compulsory schooling laws are discussed in greater detail in Section 5.3.

educational attainment of 0.72 years for children educated in rural areas,[2] approximately 53% larger than the effect of various compulsory schooling laws on educational attainment estimated in Black, Devereux and Salvanes (2008),[3] three times as large as the effects of CSL's requiring nine or more years of school on educational attainment estimated in Stephens Jr and Yang (2014) and Clay, Lingwall and Stephens Jr (2021),[4] and approximately 27% of the increased educational attainment associated with the expansion and diffusion of the American high school in the early 1900's (Goldin, 1998). The positive effects on attendance persist for individuals observed above the age of nine, indicating that the abolition of rate bills induced children to continue attending through late primary school and thus provided the foundation for the "high-school revolution" documented by Goldin (1998). These results suggest that the presence of tuition for post-secondary schools in the United States and primary and secondary schools in some developing countries, even at low levels, may depress attendance and educational attainment.

Increased attendance in response to rate bill abolition is somewhat surprising for a few reasons. Rate bills were not expensive, even for the time period. In New York state, average tuition for the full 18-week academic term in 1841-1842 was only 0.3% the annual wages of a non-farm worker (Go and Lindert, 2010). Based on the author's calculations, rate bills in the relatively expensive American South for an entire academic year were only 2.5% of annual wages for farmers.[5] While the marginal student's family presumably earned less than the averages used here, these statistics demonstrate the small magnitude of rate bills at this time. Additionally, tuition and user fees were not the only costs associated with primary schools at this time. Non-attendance may have been driven by the opportunity cost of children working rather than the pecuniary cost of rate bills. Child labor laws were extremely rare in the 19th century, and some children worked in both rural and urban areas.[6] The elimination of rate bills only altered one aspect of the decision for marginal school attendees, leaving opportunity

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[2]This number is calculated based on a 7.2 percentage point increase in attendance over ten years of childhood and relies on various assumptions discussed in Section 6.

[3]However, these results for CSL's are statistically insignificant when clustering at the state level, driven by large standard errors. In contrast, my results are robust to clustering at the state level and using the wild bootstrap to account for a low number of (effective) clusters.

[4]The results referenced here are for all whites ages 25-54 (Table 1 Column 6 of Stephens Jr and Yang (2014)) and all men (Table 2 Column 1 of Clay, Lingwall and Stephens Jr (2021)).

[5]Data on rate bill magnitudes are from Go and Lindert (2010). Data on wages are from Census Bureau (1975).

[6]Limited data in the 1860-1880 Censuses suggest that between 4% and 6% of primary-school-age children worked overall, with slightly more working in rural areas than urban areas.

costs unaffected, but still led to increased attendance. Lastly, public schools charging rate bills were not the only options for children seeking primary education. Some states and municipalities also provided pauper schools free of charge to those individuals on lower rungs of the socioeconomic ladder, for whom rate bills at public schools represented a higher fraction of income.[7] For the wealthy, parents may have chosen to send children to private schools instead of tuition-charging public schools. In fact, "[rate bill] abolition was accompanied generally by a decreased attendance at private schools" (Cubberley, 1919), implying some level of substitution between these two alternatives.[8] Other primary school options meant that rate bills would have only inhibited a subset of individuals considering public schools from attending. However, the removal of these charges still induced attendance among marginal students that could not afford private schools or were unable to attend pauper schools for eligibility or availability reasons. Thus, the striking result here is that even small fees were large barriers to school attendance.

These results are economically and historically significant in the broader development of the United States throughout the 19th and 20th centuries. Estimates for returns to education primarily range from 4.3% (Goldin and Katz, 2000) to 14.8% (Oreopoulos, 2006) per year. Therefore, the returns generated by an additional 0.72 years of educational attainment suggest increased income of between 3.1% and 10.7% for the rural populations induced to attend school by the abolition of rate bills. Of particular importance is the impact of education on agricultural productivity, which has been well-documented and is especially relevant to those rural individuals affected by state-level rate bill abolition (Schultz, 1978; Huffman, 2001; Goldin and Katz, 2000), highlighting the economic significance of this shift in school funding to the development of the United States.

These education-promoting policies could also induce structural change in the United States through three channels. First and most directly, highly-educated workers are more suited for non-agricultural occupations (Goldin and Katz, 2000, 2008).[9] Second, increased productivity of the agricultural sector

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[7] While the quality of pauper schools is debated, those families that utilized them would have been explicitly choosing to do so in lieu of costly public schools. The existence of pauper schools as a free option in some regions would if anything attenuate the estimates in this paper towards zero.

[8] Specifically, this suggests that wealthier families explicitly chose to send children to private schools because they would be required to pay tuition for either option.

[9] The results of Goldin and Katz (2000) specifically suggest that education inducing switching from farm- to non-farm occupations. The estimated overall returns to education are larger than the returns within farm occupations or the returns within non-farm occupations. This implies that the returns are partially driven by changes in composition from the farm

allows for "surplus" workers to reallocate into other sectors of the economy (Cao and Birchenall, 2013; Emerick, 2018). Third, increased education is associated with higher innovation and research (Goldin and Katz, 2000; Toivanen and Väänänen, 2016), thus encouraging shifts towards more technology-intensive areas like industry.

Rate bill abolition was a major step in the shift towards accessibility of education for the entire population. Free public schools in the 19th century were sometimes referred to as "common schools" because they were available to all children that wished to attend (Cubberley, 1919; Goldin and Katz, 2008). Additionally, as is mentioned above, rate bill abolition set the stage for the gains experienced in the first half of the 20th century as a result of the high school revolution (Goldin, 1998). This contrasts the earlier iterations of secondary schooling in the United States and abroad; secondary education was primarily used as preparation for college and university, as opposed to granting terminal degrees in their own right. By contributing to the shift towards universal primary schooling in rural areas, rate bill abolition provided the foundation upon which the United States became the most educated country in the world in the late 19th and early 20th centuries.[10]

This paper also contributes to the literature on discontinuities and non-linearities for prices around zero. This is relevant to the discussion of post-secondary education today, where community colleges charge very low levels of tuition. Denning (2017) shows that post-secondary attendance is more sensitive to decreased tuition when the price is already close to zero. Similarly, significant research in health economics finds that very small co-pays significantly reduce the probability a prescription is filled, even when the benefits of medication greatly outweigh the cost to the customer (Harris, Stergachis and Ried, 1990; Choudhry et al., 2011; Baicker, Mullainathan and Schwartzstein, 2015; Gross, Layton and Prinz, 2020). In both cases, even low prices seem to matter to potential consumers of education or healthcare. This sentiment is echoed in my results, where the elimination of small rate bills leads to large attendance increases.

The rest of the paper proceeds as follows: Section 2 provides background on the education system in the United States during the the 19th century. Section 3 introduces the data utilized in this paper. Section 4 describes the staggered adoption difference-in-differences empirical strategy and the wild

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group to the non-farm group.

[10] "Mass secondary schooling in the early twentieth century was made possible because universal elementary education had already spread throughout most sections of the nation" (Goldin and Katz, 2008).

bootstrap. Section 5 highlights the key findings and provides various robustness checks. Section 6 discusses the economic and historical significance of rate bill abolition and the resulting increase in attendance. Section 7 concludes.

2   Background

Rate bills were first implemented by school districts as education systems expanded in the 18th and 19th centuries as an alternative to increased government funding, thus sharing the cost of public school provision between the the local population writ large and the specific families of students utilizing the service (Lindert, 2004; Go, 2009).[11] The magnitude of rate bills varied across states and school districts but was generally small relative to incomes. Go and Lindert (2010) estimate that tuition for 18 weeks of schooling in 1841-1842 was approximately 0.3% of annual wages for the average non-farm worker in New York state.[12] In the case of New York City, for which Cubberley (1919) provides the most detail, rate bills were charged based on classes; more advanced courses like astronomy and bookkeeping required much higher rate bills than elementary classes on alphabet and spelling. The largest of these fees was $2, equivalent to approximately $52 in 2019. Although rate bills were higher and wages lower in the American South, I calculate that tuition for a full academic year was only 2.5% of average annual income for agricultural workers. The exact structure of rate bills also varied across school districts: "In most communities these tuition payments were charged for days in attendance exceeding some number of days that were provided by the community free of direct charge. In others they were levied for the full term" (Goldin and Katz, 2008).[13]

Over time, the general population thought of public provision of education as more important to society. As these attitudes shifted, school districts reduced rate bill requirements to encourage

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[11] "Starting in the 17th century, more and more localities [in the United States] developed their own school districts. Their funds came mainly from local property taxation, but also from tuition, donations, and occasional help from state land-sale revenues" (Lindert, 2004).

[12] Go and Lindert (2010) find that it took 0.16 weeks of non-farm wage work to pay the rate bills for 18 weeks of school, which is 0.3% of a 52-week year. While a large fraction of the population, especially in rural areas, was involved in agriculture, non-farm wage data is more available for this time period.

[13] This is discussed in greater detail in Section 6 with regards to how the estimated effects on attendance translate to educational attainment.

attendance. By 1850, rate bills provided only around 35% of public school funding in New York state, a significant decrease from 75% in 1825. Tuition and fees were primarily replaced by local taxes and state subsidies for public schools, shifting the cost from those families utilizing public education to society writ large. Go (2009) discusses the various factors that affected this shift in popular opinion and finds that increasing property values and inequality induced voters to favor property taxes and the primary funding source for public schools.

This change towards fully-funded schools rather than partial tuition began primarily in cities; urban areas typically repealed rate bills of their own volition in an attempt to increase school attendance and educational attainment, just as New York City had done in the 1830's (Cubberley, 1919; Goldin and Katz, 2008).[14] In addition to providing free schools at the local level, urban areas attempted to impose rate bill abolition on entire states.[15] Rural areas, on the other hand, continued to charge tuition and vote against state-level rate bill abolition until state legislatures and public referenda forced their hand. Rural voters in New York overwhelming chose the status quo in multiple referenda before the state legislature finally abolished rate bills in 1867. These rural-urban differences were typical in other states as well, as one might expect given that many urban areas in the rest of the country had repealed rate bills prior to state-level abolition; rural areas clearly preferred to charge tuition and fees well after major cities provided primary schooling free of charge.

3   Data

Information on rate bill abolition laws is collected from Go (2009), which draws on many different sources, including Cubberley (1919), Goldin and Katz (2008), Swift (1911), and Mead (1918). Table 1 shows the dates of passage for rate bill abolition, as well as first instances of state-level compulsory schooling laws (Goldin and Katz, 2008; Clay, Lingwall and Stephens Jr, 2012; Snyder and Tan, 2005).

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[14]At least eleven cities in New York provided free schools before the state abolished rate bills in 1867: New York City, Buffalo, Hudson, Rochester, Brooklyn, Williamsburg, Syracuse, Troy, Auburn, Oswego, and Utica. Other major cities such as Baltimore, Charleston, Mobile, New Orleans, Louisville, Cincinnati, Chicago, and Detroit also provided free schools at least 25 years prior to state-level rate bill abolition (Cubberley, 1919).

[15]"[The cities] would not tolerate the rate bill [anywhere in the state], and, despite their larger property interests, they favored tax-supported schools" (Cubberley, 1919).

Figure 1 graphically represents which states are included and the variation in timing across states with regards to their abolition of rate bills.[16]

I utilize the 1850-1880 Full Count Censuses from the Integrated Public Use Microdata Series (Ruggles et al., 2019). Key variables in the data include information on an individuals attendance status, age, race, gender, various measures of socio-economic status, urbanicity,[17] current state, and state of birth. I restrict my dataset to native-born whites between the ages of 5 and 14 in rural areas.[18] Table 2 shows relevant summary statistics for each of the four groups of states in my dataset: those that abolish rate bills prior to 1850, between 1850 and 1860, between 1860 and 1870, and between 1870 and 1880.

In each Census year utilized here, the Census asked some variation of "Was the person at school within the last year?" for every person in the household.[19] Figure 2 plots the attendance rates by age for individuals between the ages of 5 and 18, broken down across dimensions of gender and urbanicity. Men and women attended school at similar rates, regardless of where they lived. Attendance in urban areas was higher than rural areas. This may have been due to rural children working rather than attending school (Cubberley, 1919).[20] In addition, many urban areas had eliminated rate bills prior to this time, thus removing a potential obstacle to attendance. Either of these channels, or rural-urban differences in returns to education, may have contributed to the disparities in attendance among primary-age children.

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[16]Restrictions for specific robustness checks are discussed in the appropriate sections.

[17]Individuals are considered to be rural if there are fewer than 2,500 people in the municipality. Approximately 75% of the sample is considered rural by this metric, despite the low threshold for classification as urban. This highlights the rural nature of the United States at this time.

[18]Children of other races or nationalities may have been subject to discrimination that prevented attendance regardless of any user fees charged. I choose ages 5 through 14 to focus on primary-school-age children. These choices mirror the literature on compulsory schooling laws. Urban areas are used as a placebo test since rate bill abolition was not binding to areas that already provided free public schools.

[19]The question included above was the question in 1850. The following questions were used in 1860, 1870, and 1880: "Did the person attend school within the last year?" "Did the person attend school within the last year?" and "Had the person attended school within the past year?" I treat these four as equivalent questions about one's school attendance. According to Goldin and Katz (2008), these attendance figures can be reasonably interpreted as enrollment rates; I will use the term "attendance" to mirror the format of the question, but it is worth noting the slight difference between the two.

[20]Limited data in the 1860-1880 Censuses suggest that between 4% and 6% of primary-age children worked overall, with slightly more working in rural areas than urban areas.

4 Empirical Strategy

4.1 Identification

In order for the effect of rate bill abolition to be identified, I assume that the legislation and referenda that brought about these changes were not accompanied by other education reform policies. It is necessary that the abolition of rate bills is uncorrelated with other shifts that might affect attendance, such as the availability of education or the returns to education.[21] If this assumption were violated, then one would expect such educational changes to affect attendance in urban areas as well as rural areas. I therefore use urban areas, which had already repealed rate bills prior to the state-level policies, as a placebo test to confirm that other educational reforms were not correlated with the timing of rate bill abolition laws. While I find strong, positive, statistically significant effects of rate bill abolition on attendance in rural areas, I estimate a null effect in urban areas. This evidence reinforces the assumption that other attendance-promoting policies were uncorrelated with treatment status, as such policies would presumably affect urban areas as well as rural ones.

I also assume that individuals above the age of ten when these policies are implemented are unaffected by rate bill abolition. The educational trajectory of an older individual is unlikely to be affected by minor changes in pecuniary costs, so the abolition of rate bills for the remaining years does not affect their attendance status going forward. For example, one would not expect any change in attendance for an individual that had dropped out of school at age 11 in response to rate bill abolition at age 13. This is consistent with evidence that attendance did not increase for children at older age groups unless these policies were implemented when they themselves were younger, shown in Table A.1. If this assumption were violated, then the estimated coefficients would therefore be interpreted not as the true effect of rate bill abolition but instead as the effect of rate bill abolition on young children less the effect of rate bill abolition on older children, thus attenuating the results towards zero. The results are robust to various upper bounds on ages that would be affected by these policies, shown in the sensitivity analysis in Table A.6.

Similarly, I assume that rural areas continued to charge rate bills until state-level policies abolished

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[21]The availability of and returns to education almost surely changed over this time, but I only assume that these changes were orthogonal to treatment status.

their use. Given the well-documented repeal of rate bills in urban areas and no corresponding evidence in rural areas, as well as the voting patterns in rural areas against rate bill abolition (Cubberley, 1919), I believe this to approximate the truth. If this assumption were violated, then the misclassification of control units in the treatment group would attenuate the estimated coefficients towards zero, and the results here would understate the true importance of rate bills as a barrier to attendance.

4.2 Treatment

Treatment is assigned based on a child's age when rate bills were abolished in their state. For the primary specification, $Treatment = 1$ for individuals that were younger than seven years old when the rate bills were abolished, $PartialTreatment = 1$ for individuals that were between the ages of seven and ten years old inclusive when the rate bills were abolished, and individuals older than ten years old when these laws are passed are given both $Treatment = 0$ and $PartialTreatment = 0$. All individuals are considered untreated prior to rate bill abolition.

Consider Connecticut, which passed a rate bill abolition law in 1868. All individuals observed in the 1850 and 1860 Censuses are considered untreated. Children born in 1856 or 1857 are untreated in the 1870 Census, since they were likely unaffected by these policies that were implemented at ages 12 and 11, respectively. Individuals born between 1858 and 1861 are considered partially-treated, since they presumably started school prior to rate bill abolition but the law took effect during their prime school-going years. Children born after 1861 are considered fully-treated, whether they are observed in 1870 or 1880, because the law was passed before they turned seven.

Over the four Decennial Censuses used here, most individuals fall into either the "fully-treated" or "fully-untreated" categories. Only 4.8% of individuals are considered partially-treated; 69.2% of children in my sample are fully-treated, and 26.0% of individuals are fully-untreated. Therefore, this analysis is primarily a comparison between individuals on either extreme that either started school after these rate bill abolition policies took effect or attended school prior to the abolition of tuition payments. It is therefore unsurprising that results are not sensitive to alternative age cutoffs for full and partial treatment, shown in Table A.6, as the fully-treated and untreated groups comprise the vast majority of the data.

4.3 Estimating Equation

I employ the following staggered adoption difference-in-differences specification with two-way fixed effects:

$$Y_{aist} = \alpha + \beta_1 Treatment_{ast} + \beta_2 PartialTreatment_{ast} + \gamma_{as} + \delta_{at} + \varepsilon_{aist} \tag{1}$$

where the outcome variable Y_{aist} is the attendance status of an individual i observed at age a in state s and Census year t, γ_{as} is a state-by-age fixed effect, δ_{at} is a Census year-by-age fixed effect, and ε_{aist} is the error term. Year-by-age fixed effects improve upon the combination of age and year fixed effects by allowing attendance differences between ages to vary across the four Census years considered here.[22] Similarly, state-by-age fixed effects improve upon state fixed effects because they account for differences in attendance rates by age for each state, rather than simply accounting for the overall attendance level in a given state with no regard for how attendance differs by age across states.[23]

Errors are clustered at the state level (thus allowing for correlation in the error terms between individuals in the same state across all Census years) due to serial correlation in the treatment variable, as is prescribed by Bertrand, Duflo and Mullainathan (2004). Therefore, I obtain my critical values from the t-distribution with $G - 1 = 26$ degrees of freedom rather than from the normal distribution to account for the low number of clusters. Additionally, I utilize the wild bootstrap with Rademacher weights as prescribed in Cameron, Gelbach and Miller (2008) to construct a 95% confidence interval for the effect of state-level rate bill abolition on attendance. The wild bootstrap better controls the size of the statistical test relative to conventional confidence intervals, which may be prone to over-rejection of the null hypothesis when clusters are heterogeneous or the number of treated clusters is small.[24]

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[22]My findings are robust to including year and age fixed effects instead of year-by-age fixed effects.

[23]My findings are robust to including state fixed effects instead of state-by-age fixed effects.

[24]The potential pitfalls of a low number of effective clusters have been well documented by Carter, Schnepel and Steigerwald (2017), MacKinnon and Webb (2017a), and Lee and Steigerwald (2018). MacKinnon and Webb (2017b) describes the how the wild bootstrap improves upon other methods to control size; if anything, the wild bootstrap can lead to severe under-rejection of the null hypothesis when the number of treated or untreated clusters is very low. This sentiment is mirrored in Athey and Imbens (2022). Therefore, the methodology employed here is a conservative test for statistical significance.

5 Findings

5.1 Main Results

Column 1 of Table 3 shows the results of my primary specification. I find that rural children at younger ages when rate bill abolition laws come into effect are 7.2 percentage points more likely to attend school than the control group.[25] The estimated coefficient is statistically significant at the 1% level when critical values are taken from the t-distribution with $G-1 = 26$ degrees of freedom to account for the low number of clusters. Confidence intervals from the wild bootstrap are shown in brackets below the standard errors in Table 3. The estimated coefficient remains significant at the 1% level using this more conservative methodology. The results presented here suggest that small rate bills were a large obstacle to primary school attendance and that their removal led to increased educational attainment in rural areas of the United States in the late 19th century.

Column 2 of Table 3 shows the results of Equation 1 estimated on urban areas, which provide a useful placebo test. Note that rate bills were typically repealed in urban areas by the time these state-level abolition policies were passed, so state-level rate bill abolition should not have any impact on urban attendance. Estimated coefficients for urban areas are close to zero and statistically insignificant, in stark contrast to that in rural areas.[26] The null results of the placebo test suggest that the results in Column 1 of Table 3 are driven by the rate bill abolition rather than state-level trends or other policies.

Column 3 of Table 3 shows the results of my primary specification including only children at least ten years old when they are observed in the Census. The coefficient is qualitatively similar to that estimated on the full primary-school-age population and statistically significant using both critical values from the t-distribution with $G-1 = 26$ degrees of freedom and the wild bootstrap. This indicates that positive effects on attendance persist for children throughout primary school, as opposed to being concentrated in younger children.[27] Primary education is effectively a prerequisite to secondary school

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[25]Table A.2 shows the results of Equation 1 estimated separately for men and women. The estimated coefficients for men and women are qualitatively similar; the estimated coefficient on rural men is slightly larger than that for women, but the two are not statistically different.

[26]This supports the identifying assumption that rate bill abolition did not coincide with additional attendance-promoting policies, as is discussed in Section 4.1.

[27]Note that many younger children were already attending schools prior to these policies, as is shown in Figure 2. If

attendance. Therefore, by increasing school attendance for children between the ages of 10 and 14 that were closest to secondary school, this policy provided the foundation for the "high-school revolution" in the early 20th century, which has been documented by Goldin (1998).

Similarly, Column 2 of Table 4 shows the results of my primary specification including only rural children of lower socio-economic status (SES).[28] These results are qualitatively similar to those presented in Table 3. Column 3 presents the results on high-SES children in rural areas. While this coefficient is positive and statistically significant at the 5% level, it is also statistical different from the coefficient estimated on low-SES individuals.

5.2   The American South

While no Decennial Censuses were collected while the American Civil War was ongoing (1861-1865), it is possible that school attendance in southern states was affected in the aftermath of the conflict, either negatively due to opportunities for farm labor and the destruction of infrastructure or positively due to changes in norms and returns to education. Many southern states passed rate bill abolition laws during the same decade as the conflict, further complicating the issue. However, the American Civil War should not affect observed attendance in northern states because it was not ongoing during any Census year. While attendance everywhere may have changed during the war, only some states experienced severe destruction and societal change that would have had long-term effects on school attendance observed in 1870 or 1880.

In order to avoid the potential confounds of the American Civil War, I replicate the estimation of Equation 1 for the 18 states that did not join the Confederacy and had abolished slavery prior to 1860.[29] Results are presented in Table 5 and are qualitatively similar to those results presented for the

one is to think of the marginal student that drops out of school to work, this would be more likely for students at least ten years old than those younger students, especially if the older individuals had already attained some level of education prior to entering the labor force.

[28]Low socio-economic status is defined as the maximum occupation score in the household under 20. Occupation score is calculated as the median income, in hundreds of 1950 US dollars, for a given occupation in 1950. For reference, a farmer has an occupation score of 14, below the threshold for low socio-economic status as I have defined it. Approximately 60% of rural children are classified as low-SES by this metric.

[29]Excluded states are Arkansas, Florida, Georgia, Louisiana, Maryland, Missouri, South Carolina, Virginia, and West

full sample of 27 states.[30] I still find large, statistically significant increases in rural attendance, both for children observed between the ages of 5 and 14 and between the ages of 10 and 14, as a result of the rate bill abolition policies and no corresponding increases in urban areas.

5.3 Compulsory Schooling Laws

Some states passed early forms of compulsory schooling laws in the late 19th century, requiring students to attend school up to a certain age or for a certain number of years. While some articles finds positive (although often statistically insignificant) effects of 20th century compulsory schooling laws or use them as instruments to investigate the causal impact of education on wages or various other life outcomes (Clay, Lingwall and Stephens Jr, 2012, 2021; Margo and Finegan, 1996; Angrist and Keueger, 1991; Acemoglu and Angrist, 2000; Staiger and Stock, 1997; Lochner and Moretti, 2004; Lleras-Muney, 2005; Black, Devereux and Salvanes, 2008; Oreopoulos and Salvanes, 2011; Stephens Jr and Yang, 2014),[31] research from Landes and Solmon (1972) shows that the pre-1880 laws were not effective at increasing attendance, both because these laws were not strictly enforced and were not binding to large swaths of the population that already attended school beyond what was required.[32]

I account for the passage of CSL's using two separate methods. First, I estimate the following equation:

$$Y_{aist} = \alpha + \beta_1 Treatment_{ast} + \beta_2 PartialTreatment_{ast} + \beta_3 CSL_{st} + \gamma_{as} + \delta at + \varepsilon_{aist} \qquad (2)$$

Equation 2 is equivalent to Equation 1 with an additional control for whether or not the state had passed a compulsory schooling law prior to the Census year in which the individuals are observed. Information on compulsory schooling laws is drawn from Go (2009), Goldin and Katz (2008), Clay, Lingwall and

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Virginia.

[30]Estimated coefficients are slightly lower when southern states are excluded from the analysis. I believe that this is driven by either the larger magnitudes of rate bills in the South, the higher prevalence of rate bills in the South, or a combination of those two factors.

[31]Clark and Royer (2013) and Oreopoulos (2006) examine similar policies and outcomes in the United Kingdom.

[32]The first compulsory schooling law in Massachusetts was passed in 1852 but was not considered effective until 1890 (Deffenbaugh and Keesecker, 1935).

Stephens Jr (2012), and Snyder and Tan (2005). Results for this adjusted analysis are displayed in Column 2 of Table 6.

Second, I restrict my sample to the 25 states with compulsory schooling legislation dates after 1870, [33] truncate my data to only include the 1850 through 1870 Decennial Censuses, and estimate Equation 1 on the restricted sample. Results for this adjusted analysis are displayed in Column 3 of Table 6. No matter how compulsory schooling laws are accounted for, the results are qualitatively similar to the estimation of Equation 1 on all four Census years.

5.4 Two-Way Fixed Effects and Heterogeneous Treatment Effects

Recent literature has emphasized potential issues with staggered adoption difference-in-differences specifications (Callaway and Sant'Anna, 2021; De Chaisemartin and D'Haultfoeuille, 2017; De Chaisemartin and d'Haultfoeuille, 2020; De Chaisemartin and D'Haultfoeuille, 2022; Goodman-Bacon, 2021; Rios-Avila, Sant'Anna and Callaway, 2021). The two-way fixed effects (TWFE) estimator identifies the weighted sum of all possible two-group two-period DID estimators but does not necessarily constrain all weights to be positive. In staggered adoption contexts with heterogeneous treatment effects, the use of previously-treated units as controls may cause some weights to be negative, thus complicating the interpretation of the estimates.

I conduct two separate robustness checks in order to account for these potential problems. First, I conduct a Bacon Decomposition, the results of which are shown in Figure A.1 (Goodman-Bacon, 2021). The figure suggests that the weights are negative for no two-group two-period comparison. Therefore, I conclude that negative weights do not bias the results presented earlier in this section. Second, I estimate coefficients based on the estimator proposed by Callaway and Sant'Anna (2021). The estimated Average Treatment Effects on the Treated (ATT's) using this estimator is shown in Column 4 of Figure A.2 alongside those from various other specifications. Estimated coefficients according to this procedure are slightly larger and more statistically significant than those from the primary specification, suggesting that negative weights if anything attenuate my results towards zero.

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[33]Only Massachusetts and Vermont had passed compulsory schooling laws prior to 1870.

5.5 Sensitivity Analysis

The results from the primary specification are not sensitive to the exact definition of treatment. Table A.6 shows the results of Equation 1 using various alternative definitions of full and partial treatment. Column 1 shows results when the oldest partial treatment cohort is aged 9 when rate bill abolition laws are passed in their state, Column 2 shows results when the oldest partial treatment cohort is aged 10, and Column 3 shows the results when the oldest partial treatment cohort is aged 11. Individuals are considered untreated if they are older than those partial treatment thresholds; individuals aged 10 when laws are passed are considered untreated in Column 1 but partially-treated in Column 2, individuals aged 11 are considered untreated in Column 2 but partially-treated in Column 3, and individuals older than 11 years old are untreated in Column 3.

Similarly, the rows of Table A.6 indicate the youngest age at which individuals are considered partially-treated. Individuals younger than these threshold when rate bill abolition laws are passed are instead considered fully-treated. Row 1 sets this threshold at 6 years old for the youngest cohort of partial treatment, Row 2 sets it at 7 years old, and Row 3 uses 8 years old.

For example, the middle entry of Table A.6 shows the results with treatment as it is normally defined for the primary specification; individuals younger than 7 years old are considered fully-treated, individuals between the ages of 7 and 10 are partially-treated, and individuals older than 10 years old when these laws are passed are considered untreated. For any combination of upper and lower thresholds, results are qualitatively similar to the primary specification with respect to estimated coefficients and inference using either the t-distribution or the wild bootstrap. This suggests that the exact definition of treatment does not affect the results presented throughout this paper.

5.6 Randomization Inference Test

I conduct a randomization inference test as a robustness check for two main reasons. First, randomization tests are a useful tool when the sample size is low because the size of the test is exact and does not depend on the distribution of errors (Kennedy, 1995; Young, 2019). Although there are over 17 million individual observations in the primary specification, they are divided among only 27

clusters. Therefore, randomization may provide a useful check for inference, in addition to the various methodologies employed here. Second, there was a general trend towards increased primary attendance and enrollment in the United States during the 19th century. Although the use of Census year-by-age fixed effects should account for various changes over time, including changes in both attendance age profiles and overall attendance levels, estimated coefficients could still be spuriously positive and statistically significant by capturing these broad changes over time. If this were the case, then estimated coefficients using randomized treatment timing may provide similar results, even though this randomized timing is not based in reality.

For the purposes of the randomization test, I assign treatment using the date of rate bill abolition for a random state, rather than the actual year in which that state implemented the policy. Dates of rate bill abolition are drawn with replacement, and treatment is still assigned at the state level. I then estimate Equation 1 with the randomized indicator variables for full treatment and partial treatment one thousand times and compare those estimated coefficients using randomized treatment timing to that in Table 3. The estimated coefficients under randomized treatment timing are shown in Figure A.3, where the vertical red line illustrates the estimated coefficient from the primary specification with realized treatment timing.

The results of the primary specification are considered statistically significant at the $p\,\%$ level by the randomization inference test if the estimated coefficient using realized treatment is greater than $100-p\,\%$ of those using randomized treatment timing. In fact, the estimated coefficient using realized treatment was greater than every single estimated coefficient in the one thousand replications of randomized treatment timing. This suggests that the results are statistically significant by the randomization test, and neither identification nor inference are threatened by the low number of clusters nor the general trends in increased attendance or enrollment over this time period.

5.7   Other Robustness Checks

I consider an alternative empirical specifications in which partially-treated individuals are considered by their age when rate bills are abolished rather than as a single category for all children between the ages of 7 and 10. Formally, I estimate the following equation:

$$Y_{aist} = \alpha + \beta_1 \mathbb{1}\{\text{year law passed}_s - t + a \leq 6\}$$
$$+\beta_2 \mathbb{1}\{\text{year law passed}_s - t + a = 7\}$$
$$+\beta_3 \mathbb{1}\{\text{year law passed}_s - t + a = 8\}$$
$$+\beta_4 \mathbb{1}\{\text{year law passed}_s - t + a = 9\} \tag{3}$$
$$+\beta_5 \mathbb{1}\{\text{year law passed}_s - t + a = 10\}$$
$$+\gamma_a s + \delta_a t + \varepsilon_{aist}$$

Results are presented in Table A.3. This specification is slightly more flexible than Equation 1 in that it allows for heterogeneous treatment effects for those individuals that were already school-aged when these rate bill abolition policies came into effect and are therefore considered partially-treated. However, the disadvantage of this specification is that there are very few individuals in each partially-treated group, since only 4.8% of the sample is partially-treated in the primary specification. Despite that potential issue, the estimated coefficients on each partially-treated cohort behave as one might expect; the coefficient for the $age = 7$ cohort is large, since these are individuals that are on the edge of full treatment, and the coefficient for the $age = 10$ cohort is small, since these are the individuals that are more similar to the untreated group.

It is possible that households may migrate from states that allow tuition charges to those that have banned rate bills. To account for this possible form of selection into treatment, I drop all individuals that do not reside in their state of birth (approximately 15% of all observations) and find qualitatively similar results, which are shown in Table A.4. The pattern of positive, statistically significant effects in rural areas and null effects in urban areas continues for those individuals that reside in their state of birth, indicating that interstate mobility does not affect the results from the primary specification.[34]

Lastly, I estimate Equation 1 without the seven states in which all individuals are fully-treated in the data. Results are presented in Table A.5 and are qualitatively similar to those in Table 3. This suggests that the inclusion of always-treated states does not inhibit estimation and inference of the staggered adoption difference-in-differences results.

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[34]With regards to intrastate migration: it is unlikely that rate bill abolition would induce within-state rural-urban migration because these policies make rural areas relatively more attractive for families of potential students. On the other hand, urban-rural migration is not concerning due to the general trend of urbanization at this time.

6 Economic and Historical Significance

The increased primary school attendance here corresponds to an extra 0.72 years of average education for the rural population.[35] This estimate may understate the true effect on educational attainment for three reasons. First, estimated coefficients may be attenuated towards zero for the reasons discussed in Section 4.1. Specifically, the inclusion of older individuals in the control group and the assumption that rural areas charged rate bills until states abolished their use may lead to the inclusion of control observations in the treatment group. Second, the Decennial Census asked whether or not an individual had attended school at any time in the previous year, suggesting that individuals attending part-time would answer yes to this question. Given the structure of the data, I can only observe the changes at the extensive margin; the 7.2 percentage point increase could be shared between individuals switching from no attendance to part-time attendance or from no attendance to full-time attendance. However, these policies could also induce shifts at the intensive margin from part-time attendance to full-time attendance, which would not be picked up in the estimation step because those individuals would have already answered "yes" regardless. As long as the shift from no attendance to part-time attendance is less than the shift from part-time attendance to full-time attendance, the 0.72 additional years is an underestimate.[36] Third, only primary school age children are considered in this analysis. It is possible that finishing primary school would induce some individuals to also receive either a secondary or post-secondary education. Unfortunately, only attendance is observed, and there is no data on education attainment for this time period. Therefore, estimates are based entirely on the flow of education, rather than the observed stock when an individual enters the labor force, which would account for any cumulative gains of education beyond primary school. This continuation value is an indirect but potentially important aspect of the impact of these policies on educational attainment.

The literature has typically found that increased education has large, direct effects on wages. Duflo (2001) exploits rapid school construction in Indonesia in the 1970's to estimate returns to education and finds that an additional year of education increased income by 10.6 percent. This setting corresponds well

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[35]Individuals are 7.2 percentage points more likely to attend for any year in a 10-year period. $0.072 \times 10 = 0.72$

[36]It is reasonable to expect the shift from part-time to full-time attendance to be greater than that for no-attendance to part-time given the structure of rate bills in some areas: "In most communities these tuition payments were charged for days in attendance exceeding some number of days that were provided by the community free of direct charge" (Goldin and Katz, 2008).

to the 19th century United States because the population of Indonesia at the time was primarily rural, largely involved in agriculture, and these policies considered in both cases revolve around increasing the fraction of the population with a primary school education. Based on these results from Duflo (2001), an additional 0.72 years of educational attainment for rural individuals would translate to a 7.6 percent increase in rural income as a result of rate bill abolition. Table 7 shows various estimates from other papers, many exploiting compulsory schooling laws in the United States, In general, these estimated returns to an additional year of education are positive and statistically significant, ranging from 4.3% to 14.8%.[37]

Education also has indirect effects on the economy and society beyond increased wages. Increased agricultural productivity is important in the history of the development of the broader economy of the United States. Cao and Birchenall (2013) and Emerick (2018) show that positive shocks to agricultural productivity lead to labor shifting away from agriculture and into other sectors of the economy. Therefore, increased educational attainment that led to higher agricultural productivity would also induce structural change, effectively "freeing up" workers to leave the agricultural sector and instead work in the services sector or the burgeoning industrial sector at the time.

In addition, high-education workers are well-suited to non-agricultural jobs. Although one might think of typical industrial jobs in the 19th century as utilizing "unskilled" labor, workers still had to be educated to properly operate the new machinery and techniques that were being incorporated into the industrial sector. Goldin and Katz (2000) shows that workers with higher education were more likely to hold non-agricultural jobs, indicating that additional education prepared future workers for other sectors as well as agriculture.

Increased education can also lead to higher rates of innovation in an economy. In a stylized setting, Toivanen and Väänänen (2016) find that additional technical universities in Finland led to more patents being filed in the United States by Finnish inventors. Although this is not directly comparable to the 19th century United States, it is reasonable to extrapolate that increased education would lead to higher rates of invention and greater research and development, in addition to simply allowing workers in the agricultural and other sectors to make better use of contemporary modern techniques and technologies. Technological innovation would in turn demand greater skills from workers, again highlighting the

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[37]This range excludes the estimate from Stephens Jr and Yang (2014), which is an outlier in the literature at -0.3%.

importance of education and literacy.

Lastly, increased primary school attendance was crucial as the United States became one of the most-educated countries. By 1890, the primary school enrollment rate was approximately 97.1%, higher than every country in the world (Lindert, 2004).[38] These policies also provided the foundation for the "high-school revolution" documented in Goldin (1998). The author herself acknowledges this: "mass secondary schooling in the early twentieth century was made possible because universal elementary education had already spread throughout most sections of the nation" (Goldin and Katz, 2008). Thus, the increased primary school attendance induced by rate bill abolition was a crucial step in the development of the education system in the United States.

7   Conclusion

This paper uses United States Census data from the 19th century to investigate the effect of state-level rate bill abolition laws on primary school attendance in rural areas. I find that preventing local school districts from charging tuition and user fees led to increased attendance for children that started school after abolition laws took effect. Cubberley (1919) was indeed correct in asserting that "the [rate bill] charge was small, but it was sufficient to keep many poor children away from the schools." Rate bill abolition was an important policy that increased primary school attendance in the 19th century and helped the United States eventually become the most educated nation in the world during the 20th century.

The analysis is subject to some limitations, primarily driven by the (lack of) available data on this topic. I rely on state-level policy changes rather than more granular rate bill information at the school district level. This paper also relies on attendance as it is marked in the 1850 through 1880 Censuses, which is an indicator variable rather than a measure of how much an individual has attended school both in the past year and throughout their childhood. Lastly, I am unable to comment on overall educational attainment other than back-of-the-envelope calculations because the United States Census did not ask

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[38]This figure considers children between the ages of 5 and 14 enrolled in either public or private schools. 85.7% of children between the ages of 5 and 14 were enrolled specifically in public schools, which was also the highest rate in the world.

about completed educational attainment until 1940, well after rate bills were abolished throughout the country.

Despite these limitations, this paper contributes to the literature on the history of education in the United States. Denison (1985) and Goldin (1998) find that increased emphasis on education in the early 20th century contributed to the economic success of the United States, and I find that rate bill abolition in the 19th century increased attendance even before such policies as compulsory schooling laws and the standardization of the American high school, initiating the expansion and diffusion of access to education.

These results have various implications for the modern education systems. Tuition requirements for primary and secondary schools persist in many developing countries. Places like India have only recently abolished tuition payments for public schools, and some countries in Africa continue to charge students to attend public school (Fujimoto, Lagakos and Vanvuren, 2022). These countries have much lower levels of primary school attendance than the rest of the world, in some cases lagging behind the United States by over a century (Roser and Ortiz-Ospina, 2013). The continued presence of tuition requirements for public schools likely inhibits educational attainment and, in turn, economic development.

Post-secondary education in the United States also provides an interesting parallel for primary schools in the 19th century. Post-secondary education is not compulsory, community colleges typically charge low levels of tuition, and the marginal returns to this additional education are very large (Marcotte et al., 2005; Marcotte, 2019). The elimination of small pecuniary costs to attend community college or other post-secondary institutions may induce large gains in modern educational attainment and economic productivity, just as the elimination of rate bills for primary schools led to increased attendance in the 19th century.

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8 Tables

State	Rate Bill Abolition	Compulsory Schooling
New Hampshire	1789	1871
Maine	1820	1875
Massachusetts	1827	1852
Delaware	1829	1907
Pennsylvania	1834	1895
Louisiana	1847	1910
Wisconsin	1848	1879
Indiana	1852	1897
Ohio	1853	1877
Illinois	1855	1883
Iowa	1858	1902
West Virginia	1863	1897
Vermont	1864	1867
Maryland	1865	1902
Missouri	1866	1905
New York	1867	1874
California	1867	1874
Connecticut	1868	1872
Rhode Island	1868	1883
South Carolina	1868	1915
Arkansas	1868	1909
Michigan	1869	1871
Florida	1869	1915
Georgia	1870	1916
Virginia	1870	1908
New Jersey	1871	1875
Utah	1890	1890

Table 1: Rate Bill Abolition Dates.

Sources: Go (2009), Clay, Lingwall and Stephens Jr (2012), and Snyder and Tan (2005)

Table 2: Summary Statistics in 1850

	Before 1850	1850-1859	1860-1869	1870 or later
% attending school	75.2	63.9	67.0	43.6
% attending school (rural only)	74.9	63.7	66.2	42.2
% female	49.3	48.9	49.2	49.1
% rural	82.3	94.8	85.1	93.5
age	9.3	9.2	9.3	9.3
occupation score	21.5	18.5	20.5	20.1
occupation score (rural only)	20.1	18.0	19.1	19.6
N	1,059,576	1,066,815	1,367,436	431,562

Source: 1850 Census, Ruggles et al. (2019)

The above table gives relevant summary statistics for school-age children in the 27 states considered here in 1850. Columns are organized by the decade in which the state abolished rate bills for students. I compare these groups across various observable dimensions related to school attendance, including age, urbanicity, gender, socio-economic status, and attendance itself. Literacy was not recorded in the 1850 Census.

Table 3: Staggered Adoption Differences in Differences

	Rural prob(att)	Urban prob(att)	Rural 10+ prob(att)	Urban 10+ prob(att)
Full Treatment	0.0720**	0.0003	0.0790**	0.0147
($age < 7$ when law passed)	(0.0202)	(0.0224)	(0.0232)	(0.0226)
	[0.029,0.127]***	[-0.049,0.068]	[0.031,0.094]***	[-0.042,0.086]
Partial Treatment	0.0367	0.0015	0.0425	0.0140
($11 > age > 6$ when law passed)	(0.0209)	(0.0146)	(0.0219)	(0.0136)
	[-0.008,0.084]	[-0.023,0.047]	[-0.006,0.094]	[-0.010,0.060]
N	17,610,766	5,704,146	8,398,881	2,636,717

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Standard errors clustered at the state level in parentheses. Critical values are obtained from the t-distribution with 26 degrees of freedom. Confidence intervals obtained from the Wild Bootstrap are shown in brackets. Column 1 shows the results from Equation 1 estimated on rural individuals between the ages of 5 and 14. Column 2 shows the results from Equation 1 estimated on urban individuals between the ages of 5 and 14. Column 3 shows the results from Equation 1 estimated on rural individuals between the ages of 10 and 14. Column 4 shows the results from Equation 1 estimated on urban individuals between the ages of 10 and 14. All regressions include state-by-age fixed effects and Census year-by-age fixed effects, as well as a constant term. No other controls are included.

Table 4: Staggered Adoption Differences in Differences - Heterogeneity by Socioeconomic Status

	All prob(att)	Low SES prob(att)	High SES prob(att)	Difference prob(att)
Full Treatment	0.0720**	0.0783***	0.0571*	0.0212*
($age < 7$ when law passed)	(0.0202)	(0.0200)	(0.0209)	$\chi^2(1) = 4.85$
	[0.029,0.128]***	[0.034,0.129]***	[0.013,0.120]**	p=0.028
Partial Treatment	0.0367	0.0386	0.0340	0.046
($11 > age > 6$ when law passed)	(0.0209)	(0.0216)	(0.0208)	$\chi^2(1) = 0.14$
	[-0.006,0.087]	[-0.006,0.088]	[-0.007,0.092]	p=0.712
N	17,610,766	10,992,025	6,618,741	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Standard errors clustered at the state level in parentheses. Critical values are obtained from the t-distribution with 26 degrees of freedom. Confidence intervals obtained from the Wild Bootstrap are shown in brackets. Column 1 shows the results from Equation 1 estimated on all rural individuals between the ages of 5 and 14. Column 2 shows the results from Equation 1 estimated on low-SES individuals only. Column 3 shows the results from Equation 1 estimated on high-SES individuals only. Column 4 shows the differences between the estimated coefficients in Column 2 and Column. All regressions include state-by-age fixed effects and Census year-by-age fixed effects, as well as a constant term. No other controls are included.

Table 5: Staggered Adoption Differences in Differences - Excluding American South

	Rural prob(att)	Urban prob(att)	Rural 10+ prob(att)	Urban 10+ prob(att)
Full Treatment	0.0401*	-0.0181	0.0400*	0.0006
($age < 7$ when law passed)	(0.0190)	(0.0177)	(0.0179)	(0.0190)
	[0.003,0.106]*	[-0.053,0.041]	[0.005,0.099]*	[-0.037,0.068]
Partial Treatment	0.0096	-0.0180	0.0092	-0.0026
($11 > age > 6$ when law passed)	(0.0187)	(0.0088)	(0.0183)	(0.0063)
	[-0.027,0.074]	[-0.037,0.007]	[-0.026,0.072]	[-0.016,0.014]
N	13,307,070	5,015,349	6,344,541	2,315,178

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Standard errors clustered at the state level in parentheses. Critical values are obtained from the t-distribution with 17 degrees of freedom. Confidence intervals obtained from the Wild Bootstrap are shown in brackets. Column 1 shows the results from Equation 1 estimated on rural individuals between the ages of 5 and 14, excluding those children in the American South. Column 2 shows the results from Equation 1 estimated on urban individuals between the ages of 5 and 14. Column 3 shows the results from Equation 1 estimated on rural individuals between the ages of 10 and 14. Column 4 shows the results from Equation 1 estimated on urban individuals between the ages of 10 and 14. All regressions include state-by-age fixed effects and Census year-by-age fixed effects, as well as a constant term. No other controls are included.

Table 6: Staggered Adoption Differences in Differences - Compulsory Schooling Laws

| | Rural | Rural: CSL on RHS | Rural: 1850-1870 |
	prob(att)	prob(att)	prob(att)
Full Treatment	0.0720**	0.0754***	0.0668**
($age < 7$ when law passed)	(0.0202)	(0.0178)	(0.0217)
	[0.029,0.128]***	[0.032,0.120]***	[0.017,0.122]**
Partial Treatment	0.0367	0.0357	0.0278
($11 > age > 6$ when law passed)	(0.0209)	(0.0198)	(0.0207)
	[-0.006,0.087]	[-0.006,0.084]	[-0.015,0.078]
N	17,610,766	17,610,766	11,762,302

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Standard errors clustered at the state level in parentheses. Critical values are obtained from the t-distribution with 26 degrees of freedom (24 degrees of freedom for Column 3). Confidence intervals obtained from the Wild Bootstrap are shown in brackets. Column 1 shows the results from Equation 1 estimated on rural individuals between the ages of 5 and 14, replicating Column 1 of Table 3. Column 2 shows the results from Equation 2 estimated on rural individuals between the ages of 5 and 14. Column 3 shows the results from Equation 1 estimated on rural individuals between the ages of 5 and 14 using only data from 1850 through 1870, excluding Massachusetts and Vermont. All regressions include state-by-age fixed effects and Census year-by-age fixed effects, as well as a constant term. No other controls are included except the indicator for a state having passed a compulsory schooling law in Equation 2.

Table 7: Annual Returns to Education

Paper	Context	Annual Returns
Angrist and Keueger (1991)	US Compulsory Schooling	6.0-10.1%
Card and Krueger (1992)	1980 US Cross-Section	5.1-7.4%
Staiger and Stock (1997)	US Compulsory Schooling	9.8%
Goldin and Katz (2000)	1915 Iowa Cross-Section	4.3%
Duflo (2001)	Indonesia 1970's School Construction	10.6%
Oreopoulos (2006)	UK Compulsory Schooling	14.8%
Oreopoulos and Salvanes (2011)	US Compulsory Schooling	13.1% + non-pecuniary
Clay, Lingwall and Stephens Jr (2012)	US Compulsory Schooling	11.4%
Stephens Jr and Yang (2014)	US Compulsory Schooling	-0.3%
Clay, Lingwall and Stephens Jr (2021)	US Compulsory Schooling	7.4%

Table 7 shows various estimates in the literature for the returns to an additional year of education. Some articles utilize cross-sectional variation in education to investigate how attainment affects wages. Many other papers exploit compulsory schooling laws in either the United States or the United Kingdom in order to avoid endogeneity concerns. In the most relevant paper to the context discussed here, Duflo (2001) investigates the rapid construction of primary schools in 1970's Indonesia, which was surprisingly similar to the 19th century United States with respect to urbanicity, reliance on agriculture, and primary school attendance.
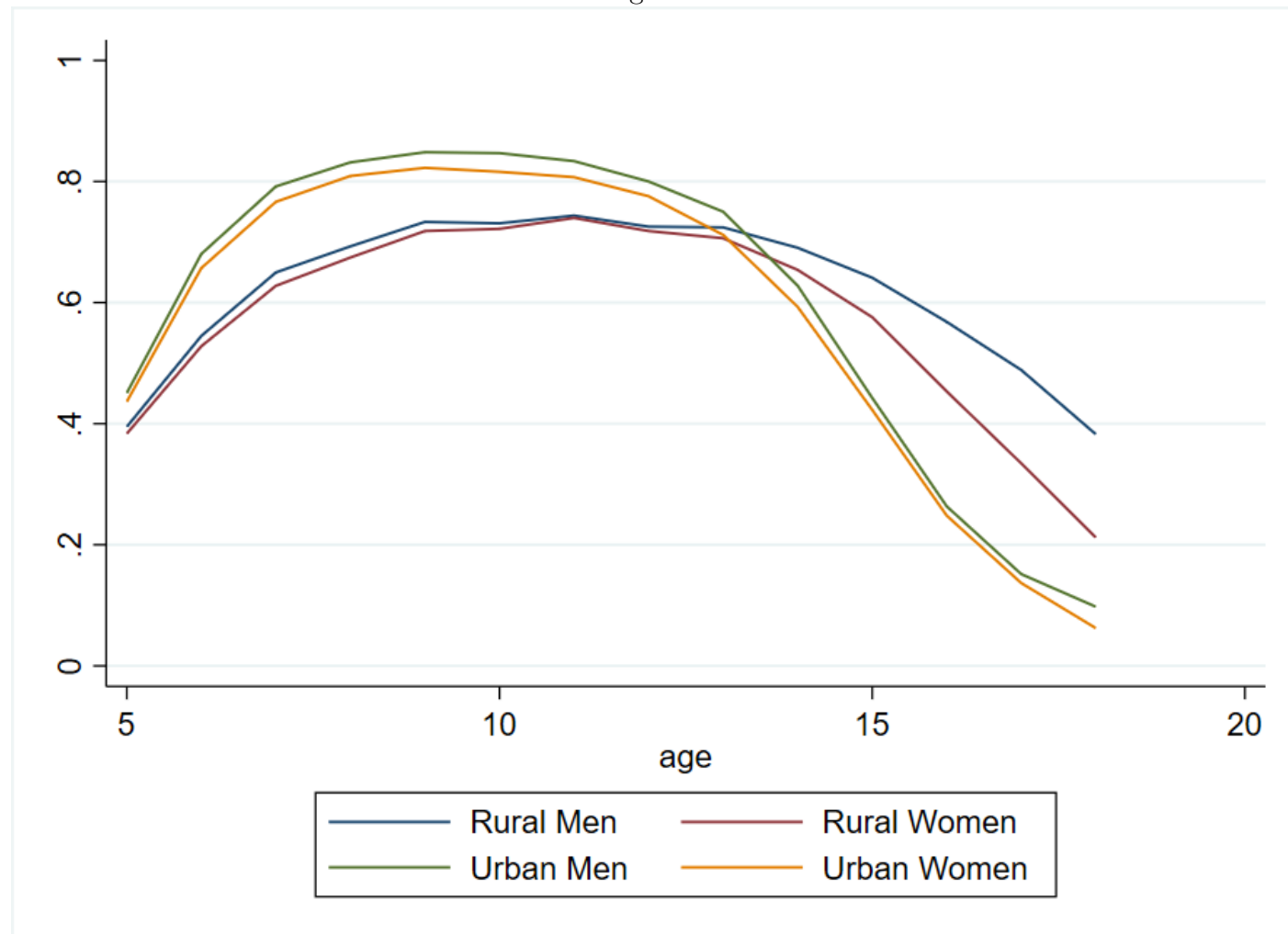
9   Figures

9.1   Map of Included States

Figure 1
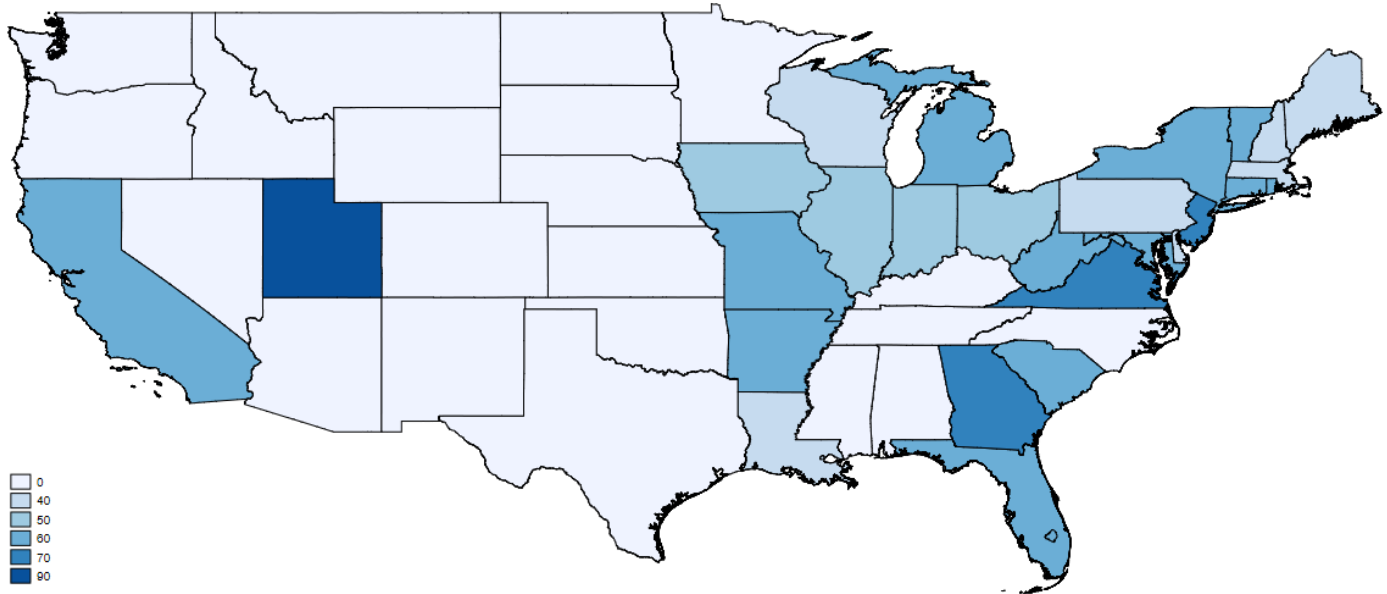
Source: Go (2009)

9.2 Attendance Rates by Age

Figure 2

Attendance Rates by Age, broken into subgroups by gender and urbanicity. Source: 1850 Census

A   Additional Tables and Figures

Table A.1: Staggered Adoption Differences in Differences - Contemporaneous Effect

	Rural	Urban	Rural 10+	Urban 10+
	prob(att)	prob(att)	prob(att)	prob(att)
Contemporaneous Treatment	0.0273	-0.0271	0.0331	-0.0190
(observed after law passed)	(0.0261)	(0.0150)	(0.0278)	(0.0141)
	[-0.038,0.097]	[-0.007,0.009]	[-0.003,0.107]	[-0.006,0.151]
Full Treatment	0.0466*	0.0254	0.0510*	0.0303
($age < 7$ when law passed)	(0.0202)	(0.0247)	(0.0206)	(0.0249)
	[-0.002,0.126]	[-0.016,0.124]	[0.003,0.112]*	[-0.024,0.123]
Partial Treatment	0.0111	0.0263	0.0130	0.0303
($11 > age > 6$ when law passed)	(0.0107)	(0.0138)	(0.0126)	(0.0149)
	[-0.011,0.055]	[-0.016,0.090]	[-0.015,0.060]*	[-0.002,0.098]
N	17,610,766	5,704,146	8,398,881	2,636,717

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Standard errors clustered at the state level in parentheses. Critical values are obtained from the t-distribution with 26 degrees of freedom. Column 1 shows the results from Equation 1 estimated on rural individuals between the ages of 5 and 14. Column 2 shows the results from Equation 1 estimated on urban individuals between the ages of 5 and 14. Column 3 shows the results from Equation 1 estimated on rural individuals between the ages of 10 and 14. Column 4 shows the results from Equation 1 estimated on urban individuals between the ages of 10 and 14. All regressions include state-by-age fixed effects and Census year-by-age fixed effects, as well as a constant term. No other controls are included.

Table A.2: Staggered Adoption Differences in Differences - Heterogeneity by Gender

	All	Men	Women
	prob(att)	prob(att)	prob(att)
Full Treatment	0.0720**	0.0745**	0.0695**
($age < 7$ when law passed)	(0.0202)	(0.0203)	(0.0201)
	[0.026,0.128]***	[0.030,0.130]***	[0.026,0.125]***
Partial Treatment	0.0367	0.0378	0.0356
($11 > age > 6$ when law passed)	(0.0209)	(0.0211)	(0.0207)
N	17,610,766	8,987,577	8,623,189

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Standard errors clustered at the state level in parentheses. Critical values are obtained from the t-distribution with 26 degrees of freedom. Column 1 shows the results from Equation 1 estimated on rural individuals between the ages of 5 and 14. Column 2 shows the results from Equation 1 estimated only on rural men. Column 3 shows the results from Equation 1 estimated only on rural women. All regressions include state-by-age fixed effects and Census year-by-age fixed effects, as well as a constant term. No other controls are included.

Table A.3: Staggered Adoption Differences in Differences - Alternate Definition of Treatment

	Rural prob(att)	Urban prob(att)	Rural 10+ prob(att)	Urban 10+ prob(att)
Full Treatment	0.0721**	0.0003	0.0790**	0.0148
($age < 7$ when law passed)	(0.0202)	(0.0225)	(0.0232)	(0.0227)
	[0.029,0.127]***	[-0.050,0.074]	[0.031,0.142]***	[-0.038,0.087]
Partial Treatment	0.0398	-0.0004	0.0533*	0.0187
($age = 7$ when law passed)	(0.0201)	(0.0160)	(0.0227)	(0.0173)
Partial Treatment	0.0359	0.0024	0.0375	0.0155
($age = 8$ when law passed)	(0.0213)	(0.0167)	(0.0240)	(0.0157)
Partial Treatment	0.0399	0.0044	0.0438	0.0133
($age = 9$ when law passed)	(0.0212)	(0.0134)	(0.0220)	(0.0119)
Partial Treatment	0.0281	-0.0002	0.0316	0.0079
($age = 10$ when law passed)	(0.0234)	(0.0136)	(0.0240)	(0.0117)
N	17,610,766	5,704,146	8,398,881	2,636,717

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Standard errors clustered at the state level in parentheses. Critical values are obtained from the t-distribution with 26 degrees of freedom. Column 1 shows the results from Equation 3 estimated on rural individuals between the ages of 5 and 14. Column 2 shows the results from Equation 3 estimated on urban individuals between the ages of 5 and 14. Column 3 shows the results from Equation 3 estimated on rural individuals between the ages of 10 and 14. Column 4 shows the results from Equation 3 estimated on urban individuals between the ages of 10 and 14. All regressions include state-by-age fixed effects and Census year-by-age fixed effects, as well as a constant term. No other controls are included.

Table A.4: Staggered Adoption Differences in Differences - State Natives Only

	Rural prob(att)	Urban prob(att)	Rural 10+ prob(att)	Urban 10+ prob(att)
Full Treatment	0.0617**	-0.0056	0.0660*	0.0070
($age < 7$ when law passed)	(0.0214)	(0.0220)	(0.0253)	(0.0228)
	[0.016,0.126]**	[-0.051,0.069]	[0.014,0.144]**	[-0.042,0.085]
Partial Treatment	0.0293	0.0003	0.0330	0.0110
($11 > age > 6$ when law passed)	(0.0233)	(0.0133)	(0.0242)	(0.0127)
	[-0.020,0.088]	[-0.023,0.044]	[-0.022,0.097]	[-0.010,0.056]
N	15,094,635	5,043,825	7,009,657	2,284,139

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Standard errors clustered at the state level in parentheses. Critical values are obtained from the t-distribution with 26 degrees of freedom. Column 1 shows the results from Equation 1 estimated on rural individuals between the ages of 5 and 14, excluding those individuals that did not live in the same state of their birth. Column 2 shows the results from Equation 1 estimated on urban individuals between the ages of 5 and 14. Column 3 shows the results from Equation 1 estimated on rural individuals between the ages of 10 and 14. Column 4 shows the results from Equation 1 estimated on urban individuals between the ages of 10 and 14. All regressions include state-by-age fixed effects and Census year-by-age fixed effects, as well as a constant term. No other controls are included.

Table A.5: Staggered Adoption Differences in Differences - Excluding Always-Treated States

	Rural prob(att)	Urban prob(att)	Rural 10+ prob(att)	Urban 10+ prob(att)
Full Treatment	0.0704**	0.0207	0.0768*	0.0401
($age < 7$ when law passed)	(0.0211)	(0.0184)	(0.0270)	(0.0211)
	[0.024,0.124]**	[-0.025,0.087]	[0.023,0.159]**	[-0.017,0.113]
Partial Treatment	0.0379	0.0000	0.0419	0.0153
($11 > age > 6$ when law passed)	(0.0222)	(0.0139)	(0.0243)	(0.0114)
	[-0.014,0.090]	[-0.026,0.032]	[-0.016,0.101]	[-0.007,0.046]
N	13,581,755	3,803,490	6,482,742	1,744,632

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Standard errors clustered at the state level in parentheses. Critical values are obtained from the t-distribution with 19 degrees of freedom. Column 1 shows the results from Equation 1 estimated on rural individuals between the ages of 5 and 14, excluding those states that adopted rate bill abolition laws prior to 1850. Column 2 shows the results from Equation 1 estimated on urban individuals between the ages of 5 and 14. Column 3 shows the results from Equation 1 estimated on rural individuals between the ages of 10 and 14. Column 4 shows the results from Equation 1 estimated on urban individuals between the ages of 10 and 14. All regressions include state-by-age fixed effects and Census year-by-age fixed effects, as well as a constant term. No other controls are included.
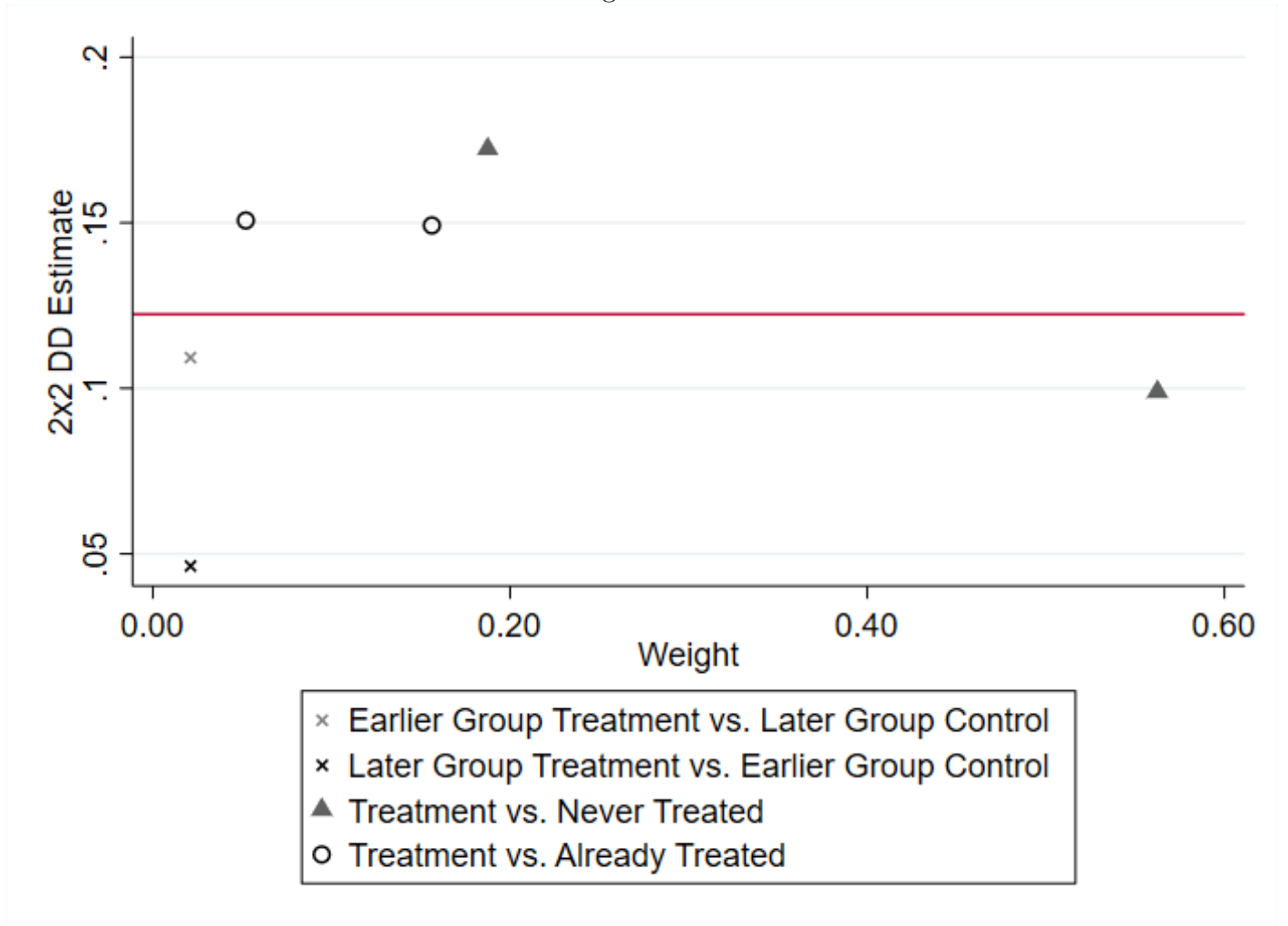
Table A.6: Staggered Adoption Differences in Differences - Sensitivity Analysis

	Oldest Partial Treatment = 9 prob(att)	Oldest Partial Treatment = 10 prob(att)	Oldest Partial Treatment = 11 prob(att)
Youngest Partial Treatment = 6	0.0734*** (0.0199) [0.031,0.130]***	0.0747*** (0.0206) [0.031,0.133]***	0.0760*** (0.0211) [0.031,0.135]***
Youngest Partial Treatment = 7	0.0708*** (0.0196) [0.029,0.125]***	0.0720*** (0.0209) [0.029,0.128]***	0.0731** (0.0207) [0.028,0.132]***
Youngest Partial Treatment = 8	0.0686** (0.0201) [0.027,0.122]***	0.0697*** (0.0217) [0.027,0.125]***	0.0707*** (0.0230) [0.026,0.127]***

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Standard errors clustered at the state level in parentheses. Critical values are obtained from the t-distribution with 26 degrees of freedom. Each column provides results for a different age for the Oldest Partial Treatment (OPT). Similarly, each row provides results for a different age for the Youngest Partial Treatment (YPT). Each cell entry shows the estimated coefficient on Full Treatment from Equation 1 for a given definition of Partial Treatment (and associated definitions of fully-treated and untreated individuals). For example, the upper-left entry shows the estimated coefficient when partially-treated individuals are those that are between the ages of 6 and 9 inclusive, rather than the definition used in the primary specification (and replicated in the middle entry of Table A.6) of between 7 and 10. All regressions include state-by-age fixed effects and Census year-by-age fixed effects, as well as a constant term. No other controls are included. Results are qualitatively similar across various definitions of treatment.
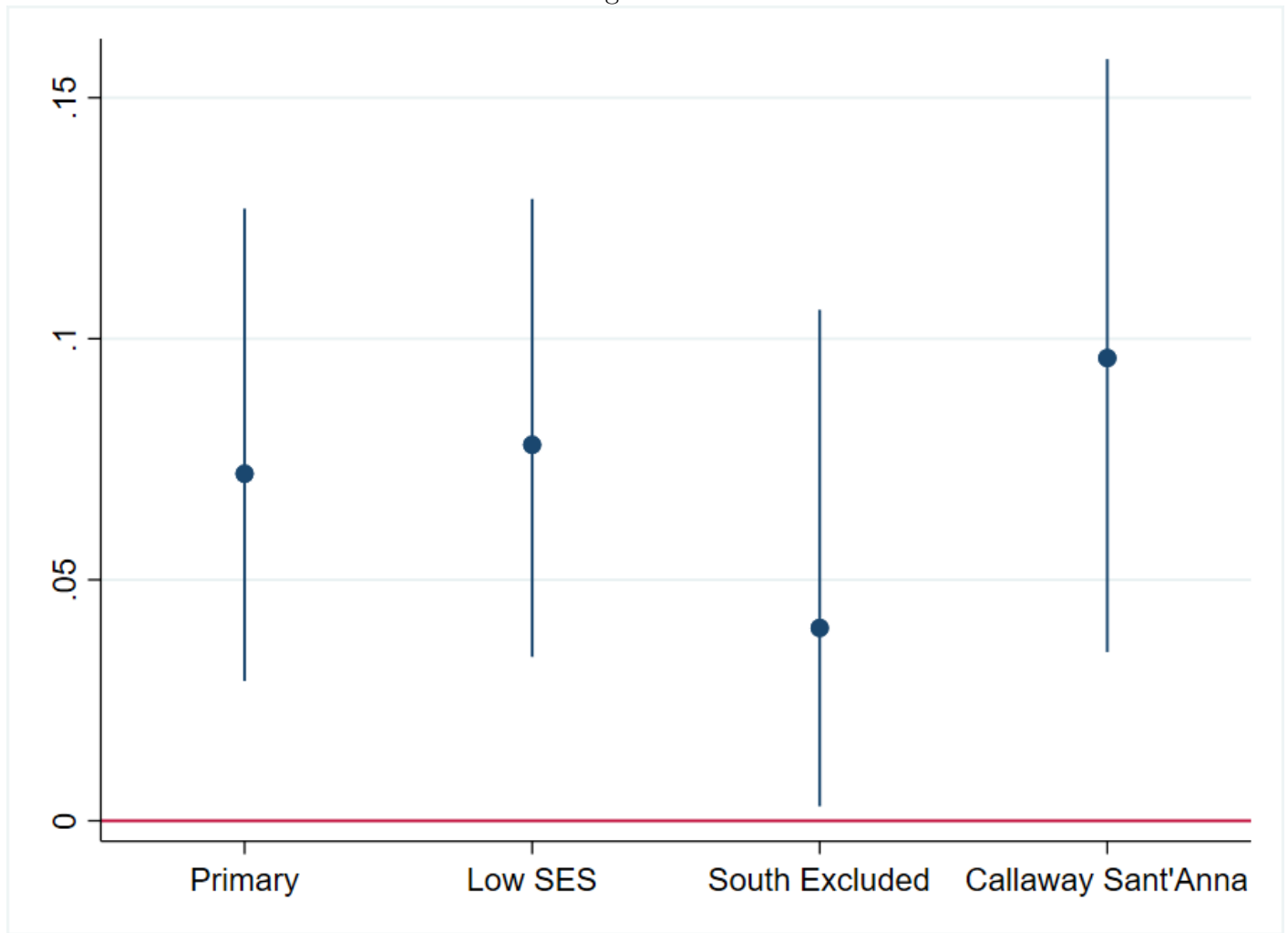
Figure A.1

Bacon Decomposition, via Goodman-Bacon (2021)

Figure A.1 shows the estimates and weights for each relevant 2-group 2-period difference-in-differences estimator in the staggered adoption specification used. Each of the estimated coefficients is positive, with the smallest around 0.05 and the largest above 0.15, and each of these estimates is given a weight greater than zero. This suggests that heterogeneous treatment effects do not bias the estimation or inference of the staggered adoption difference-in-differences framework in this context.
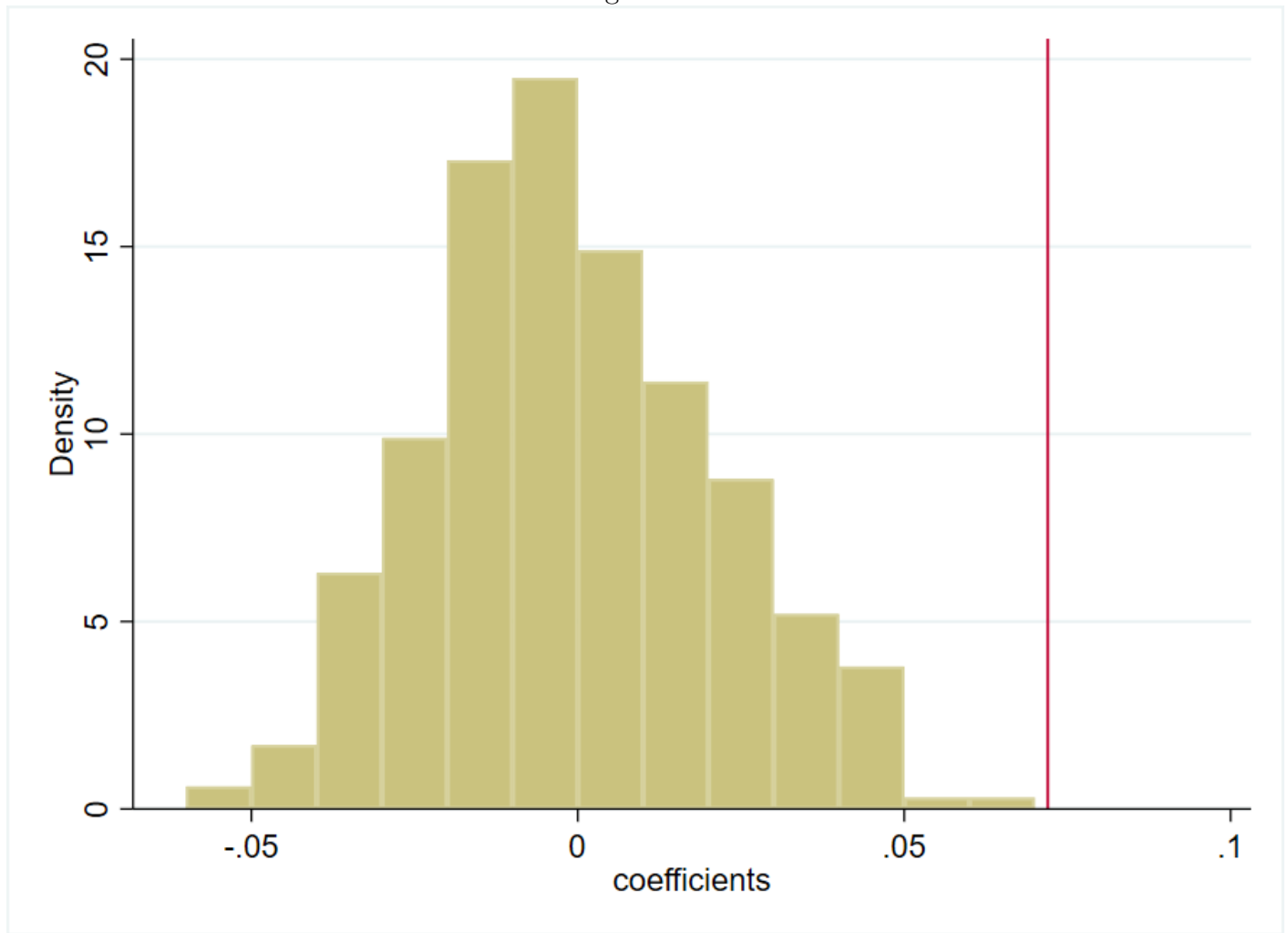
Figure A.2

Coefficient Plot

Figure A.2 shows the estimated coefficients from various specifications and samples. The first column shows the estimated coefficient from the primary specification. The second coefficient shows the estimated coefficient using only low socio-economic status individuals in rural areas. The third column shows the estimated coefficient when southern states are excluded. The fourth column shows the estimated coefficient using the estimator proposed in Callaway and Sant'Anna (2021).

Figure A.3

Randomization Test Histogram

Figure A.3 shows the estimated coefficients from 1000 replications of the primary specification with randomized treatment timing. For each replication, the year of treatment is drawn from the true distribution with replacement. The red vertical line illustrates the results from the primary specification when treatment is assigned using the realized adoption years, highlighting the statistical significance via this methodology.