The Effect of State-Level Rate Bill Abolition on School Attendance in the 19th Century United States *

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December 21, 2021

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 Urban school districts gradually repealed these fees and funded public education through local taxes.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, rate bills, primary school attendance

JEL Codes: N31, I22, H75

*The author would like to thank Kelly Bedard, Javier Birchenall, Peter Rupert, Antoine Deeb, Matthew Fitzgerald, Molly Schwarz, Ryan Sherrard, the UCSB Macroeconomics Research Group, the UCSB Econometrics Reading Group, the UCSB Applied Micro Economics Lunch, and the UC Davis Economic History Coffee Hour for their insightful discussion and comments on previous versions of the manuscript.

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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 (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 all of society. This shift predates compulsory schooling laws (CSL's), which eventually made primary school mandatory to attend. 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 hinder 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 and higher associated tuition, 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 educational attainment of 0.72 years for children educated in rural areas,[1] approximately equal to the effect of compulsory schooling laws estimated in Black, Devereux and Salvanes (2008)[2] and approximately 21% of the increased educational

[1]This number is calculated based on a 7.2 percentage point increase in attendance over ten years of childhood.

[2]However, these results for CSL's are statistically insignificant when clustering at the state level, driven by large standard

attainment driven by 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 authors calculations, rate bills in the relatively expensive American South for an entire academic year were only 2.5% of annual wages for farmers.[3] While the marginal student's family presumably earned less than the averages used here, these statistics demonstrates 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. Child labor laws were extremely rare in the 19th century, and some children worked in both rural and urban areas.[4] Therefore, non-attendance may be driven by the opportunity cost of children working rather than the pecuniary cost of rate bills. The elimination of rate bills only altered one aspect of the decision for marginal school attendees, leaving opportunity 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. For the wealthy, parents may choose 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.[5] Other primary school options meant that rate bills would have only inhibited a subset

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.

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

[4]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.

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

of individuals considering public schools. However, the removal of these charges still induced attendance among marginal students that could not attend pauper schools or afford private schools. 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. Relevant estimates for returns to education range from 3.8% (Goldin and Katz, 2000) to 10.6% (Duflo, 2001) per year. Therefore, the returns generated by an additional 0.72 years of educational attainment suggest increased income of between 2.7% and 7.6% for the rural populations induced to attend school by the abolition of rate bills. Thus, this shift in school funding contributed to the economic development of the United States in a significant way.

These policies could also induce structural change in the United States through three channels. First, high-education workers are more suited for non-agricultural types of work (Goldin and Katz, 2008). Second, increased productivity of the agricultural sector allows for "surplus" workers to shift into other sectors of the economy (Cao and Birchenall, 2013). 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 sets 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 may have been important in providing the foundation upon which the United States became the most educated country in the world in the late 19th and early 20th centuries.

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

(Harris, Stergachis and Ried, 1990; Choudhry et al., 2011; Baicker, Mullainathan and Schwartzstein, 2015; Gross, Layton and Prinz, 2020) show that small co-pays significantly reduce the probability a prescription is filled, even when the benefits of medication greatly outweigh the cost to the customer. 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 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.[6] The magnitude of rate bills varied across states and school districts but was generally small relative to incomes. According to Go and Lindert (2010), rate bills comprised over 75% of public school funding in the state of New York in 1825. 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, or the equivalent of approximately $52 in 2019. Go and Lindert (2010) estimates 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.[7] In that same school year, rate bills accounted for 41% of school revenues, with the rest coming from public funding.

[6]"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).

[7]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.

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 better promote school attendance. By 1850, rate bills provided only around 35% of public school funding in New York state, a significant decrease relative to 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 all of society. This change towards fully-funded schools rather than partial tuition began primarily in cities.[8] 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. Rural areas, on the other hand, continued to charge tuition and vote against rate bill abolition until state legislatures and public referenda forced their hand.

Indeed, when states began considering the abolition of rate bills throughout all school districts, urban areas were the primary drivers of this change.[9] In a 1849 referendum by the state of New York, the people voted to publicly fund schools. This referendum was reaffirmed by a second public vote the next year. In both cases, urban areas voted for rate bill abolition and rural areas voted for the status quo.[10] Despite these referenda, rural areas continued to charge tuition. The legislature for the state of New York finally abolished rate bills almost two decades later for those remaining communities that had continued to charge them. 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 had no qualms about rate bills, as they continued to charge tuition and fees well after cities provided primary schooling free of charge.

[8] At least eleven cities in New York provided free schools before the state abolished rate bills in 1868: 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).

[9] "Cities demanded educational progress, and were determined to have it, regardless of cost" (Cubberley, 1919).

[10] "[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).

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, which are drawn from Goldin and Katz (2008). Figure 1 displays a map of the United States, highlighting those states used in various specifications. For the primary analysis, I include all 27 states for which rate bill abolition dates are available.[11]

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,[12] and state of birth. I restrict my dataset to native-born whites between the ages of 5 and 14 in rural areas.[13] 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.[14] 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, likely due to children working rather than attending school (Cubberley, 1919).In any case, rural children typically lagged behind urban ones in primary school attendance, possibly driven by the presence of rate bills,[15] child labor, and/or differences in returns to education.[16]

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

[12] 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.

[13] 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. Urban areas are used as a placebo test since rate bill abolition was not binding to areas that already provided free public schools.

[14] 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.

[15] As is mentioned above, public schools were typically free in urban centers even prior to state-level abolition.

[16] 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 for 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 only aspect changing with treatment is the existence of rate bills, rather than the availability of education or the returns to education.[17] If this assumption were violated, then one would expect such legislation to affect attendance in urban areas as well as rural areas. Therefore, I use urban areas as a placebo test to check whether or not rate bill abolition laws led to changes in attendance in areas where rate bills had already been repealed. 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 provides some evidence reinforcing the assumption about other attendance-promoting policies; such policies would presumably affect urban areas as well, whereas rate bill abolition would only affect those places that had charged rate bills immediately prior to state-level abolition.

I also assume that individuals above the age of ten when these policies are implemented are unaffected by rate bill abolition. Presumably, 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. 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 and A.2. If this assumption is violated, estimated coefficients could 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. My results are robust to various upper bounds on ages that would be affected by these policies.

Similarly, I assume that rural areas continued to charge rate bills until state-level policies abolished 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, I believe this to approximate the truth. If this assumption were violated, then estimated coefficients understate the true

[17] 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.

importance of rate bills as a barrier to attendance, since abolition would not be binding for the entire group and instead only affect that subset of rural areas that were forced to repeal rate bills by state-level policies.

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 $PartialTreatment = 0$. Of course, 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 8.4% of individuals are considered partially treated; 66.1% of children in my sample are fully treated, and 25.6% 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 at a time such that they did not especially benefit from the abolition of tuition payments. It is therefore unsurprising that results are not sensitive to alternative age cutoffs for full and partial treatment, 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 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.[18] 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 with no regard for how attendance differs by age.[19]

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 education.[20]

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

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

[20] The potential pitfalls of a low number of effective clusters have been well documented by Carter, Schnepel and Steigerwald (2017) and MacKinnon and Webb (2017).

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. 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 5% 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.[21] The null results of the placebo test suggest that state-level rate bill abolition is truly driving the results observed in rural areas.

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 later in primary school, as opposed to being concentrated in younger children.[22] As was previously mentioned, primary schooling is effectively a prerequisite to secondary school.

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

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

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 1 of Table 4 shows the results of my primary specification including only children of lower socio-economic status.[23] These results are qualitatively similar to those presented in Table 3. However, these estimates are statistically different from those for only high socio-economic status individuals, presented in Table A.3.

5.2 The American South

The American Civil War (1861-1865) occurred during the time period observed here. While no Decennial Censuses were collected while the conflict was ongoing, it is possible that school attendance in southern states was affected in the aftermath of the American Civil War, either negatively due to the destruction of infrastructure or positively due to changes in norms, opportunities for farm labor, and returns to education. Many southern states passed rate bill abolition laws during the same decade as the conflict, further complicating the issue. On the other hand, 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-lasting 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.[24] Results are presented in Table 5 and are qualitatively similar to those results presented for the full sample of 27 states.[25] I still find large, statistically significant increases in rural attendance, both for children

[23] 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.

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

[25] 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.

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 such increases in urban areas.[26]

5.3 Compulsory Schooling Laws

Some states also 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. Research from Landes and Solmon (1972) shows that these 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 scholl beyond what was required. Due to lack of data on compulsory schooling in some states, I do not control for CSL's in the primary specification. This choice allows me to maintain 27 clusters rather than limiting the sample to 15 states.

I check robustness on the restricted sample to account for their passage using two separate methods. First, I restrict my sample to those 15 states with CSL information and estimate the following equation:

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

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. Results for this adjusted analysis are displayed in Column 3 of Table 6.

Second, I restrict my sample to the 13 states with known compulsory schooling legislation dates after 1870, [27] truncate my data to only include the 1850 through 1870 Decennial Censuses, and again estimate Equation 1. Results for this adjusted analysis are displayed in Column 4 of Table 6 and are qualitatively similar to those from the primary specification. 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, either on the entire sample of 27 states or the restricted sample with known CSL information.

[26]The difference between low- and high-SES individuals (presented in Table A.4 and A.5, respectively) is diminished when the South is excluded.

[27]Only Massachusetts and Vermont is known to have had a compulsory schooling law prior to 1870.

5.4 Other Robustness Checks

I consider an alternative empirical specifications in which partially-treatment 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:

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

Results are presented in Table A.6. This specification is slightly more flexible than Equation 1 in that it allows for different 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 so many individuals are either fully-treated or untreated. Therefore, the coefficients for each age of partial treatment are imprecisely estimated.

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.7. 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.

6 Economic and Historical Significance

The increased primary school attendance here corresponds to an extra 0.72 years of education for the rural population.[28] This estimate may understate the true effect on educational attainment for three reasons. First, it is possible that the estimated coefficients are attenuated towards zero for the reasons discussed in Section 4.1. Second, the Decennial Census asked whether or not an individual had attended school at any time in the previous year. Individuals attending part-time would answer yes to this question. 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 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. Third, only primary school age children are considered in this analysis. It is possible that finishing primary school would induce some individuals to continue their education in secondary and post-secondary manners. This continuation value is an indirect but potentially important aspect of the impact of these policies on educational attainment.

Duflo (2001) uses school construction in Indonesia in the 1970's to estimate returns to education. She finds that an additional year of education increased income by between 6.8 and 10.6 percent. This setting corresponds well 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 between an 4.9 and 7.6 percent increase in income.

Goldin and Katz (2000) estimate returns to education based on the 1915 State Census of Iowa, which was extremely rare in that it asked not just about contemporary school attendance but also educational attainment. The authors find that returns to education in the agricultural sector were approximately 3.8 percent per year, which translates to a 2.7 percent increase in income based on the additional 0.72 years estimated here. This estimate may be considered a lower bound for the gains from increased education because the marginal year of education at this time was in secondary school. If returns to education are

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

concave, then an additional year would have a higher impact (measured in percent terms) at the primary level than at the secondary level. Additionally, these estimates only account for within-occupation returns; if additional educational attainment also induces workers to switch out of agriculture to more productive occupations, then an annual return of 3.8 percent would understate the true effect.

Increased agricultural productivity is important in the history of the development of the broader economy of the United States. Multiple papers (Cao and Birchenall, 2013; Emerick, 2018) have shown 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 as well. 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 assume 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. Additional innovation would in turn require greater skills from workers, again highlighting the 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).[29] These policies also provided the foundation for the "high-

[29] 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.

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.

This analysis is subject to some limitations, primarily driven by the available data on this subject. I rely entirely on state-level policy changes rather than more granular rate bill information at the schools district level. Additionally, I am unable to control for inter- and intra-state mobility, especially that which may be caused by changes in rate bill policies over time. 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, since the United States Census did not ask 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. Previous studies (Denison, 1985; Goldin, 1998) have shown 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.

This research is relevant to today in two specific contexts. Some countries in the developing world are still undergoing or are yet to adopt these policies implemented in the United States during the 19th and 20th centuries. Places like India have recently abolished tuition payments for public schools, and some areas in Africa continue to charge students to attend public school. 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. In the United States, post-secondary education may provide an interesting parallel for primary schools in the 19th century. Post-secondary education is by no means compulsory, just as primary schooling was not compulsory when rate bill abolition was implemented, and community colleges charge low levels of tuition that are dwarfed by marginal returns to education (Marcotte et al., 2005; Marcotte, 2019). The results of this paper suggest that abolition of low tuition requirements and other fees may lead to large increases in primary school attendance in developing countries and post-secondary attendance in the United States.

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8 Tables

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

Table 1: Rate Bill Abolition Dates. Source: Go (2009)

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
% female	49.3	48.9	49.2	49.0
% urban	17.7	5.3	14.9	6.5
age	9.3	9.2	9.3	9.3
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 some observable dimensions.

Unfortunately, literacy was not recorded in the 1850 Census. Otherwise, it would be included in this table.

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 - Low SES Only

	Rural	Urban	Rural 10+	Urban 10+
	prob(att)	prob(att)	prob(att)	prob(att)
Full Treatment	0.0783***	-0.0095	0.0870**	0.0033
($age < 7$ when law passed)	(0.0200)	(0.0208)	(0.0243)	(0.0245)
	[0.034,0.129]**	[-0.054,0.054]	[0.034,0.155]***	[-0.053,0.074]
Partial Treatment	0.0386	-0.0003	0.0457	0.0105
($11 > age > 6$ when law passed)	(0.0216)	(0.0193)	(0.0230)	(0.0198)
	[-0.010,0.088]	[-0.035,0.063]	[-0.008,0.101]	[-0.025,0.076]
N	10,992,025	754,181	5,294,987	361,536

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

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 low-SES 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 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	Urban	Rural: CSL on RHS	Rural: 1850-1870
	prob(att)	prob(att)	prob(att)	prob(att)
Full Treatment	0.0359	-0.0212	0.0515*	0.0511*
($age < 7$ when law passed)	(0.0185)	(0.0173)	(0.0185)	(0.0208)
	[0.001,0.100]*	[-0.055,0.037]	[-0.001,0.075]	[0.006,0.119]*
Partial Treatment	0.0047	-0.0194	0.0095	0.0034
($11 > age > 6$ when law passed)	(0.0174)	(0.0091)	(0.0192)	(0.0198)
	[-0.031,0.065]	[-0.040,0.006]	[-0.031,0.078]	[-0.049,0.075]
N	12,999,942	4,883,107	12,999,942	8,720,046

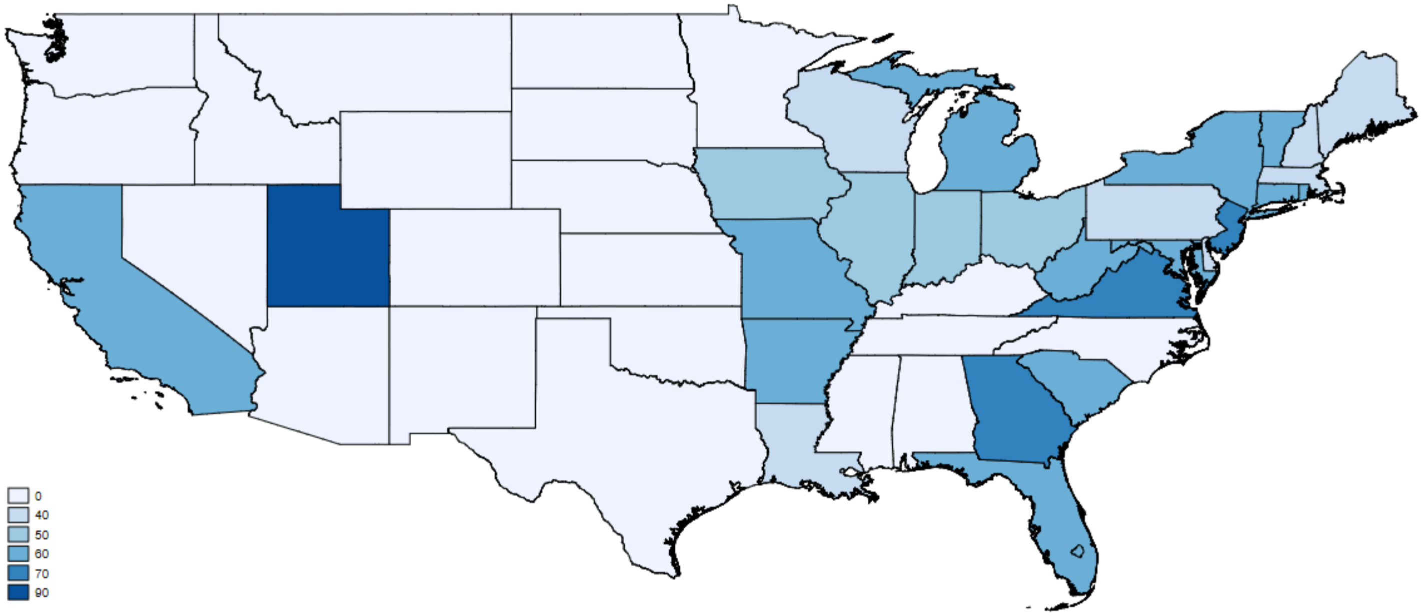
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Standard errors clustered at the state level in parentheses. Critical values are obtained from the t-distribution with 14 degrees of freedom (12 degrees of freedom for Column 4). 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 only in the 15 states for whom data on compulsory schooling laws is available. 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 2 estimated on rural individuals between the ages of 5 and 14. Column 4 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.

9 Figures

9.1 Map of Included States

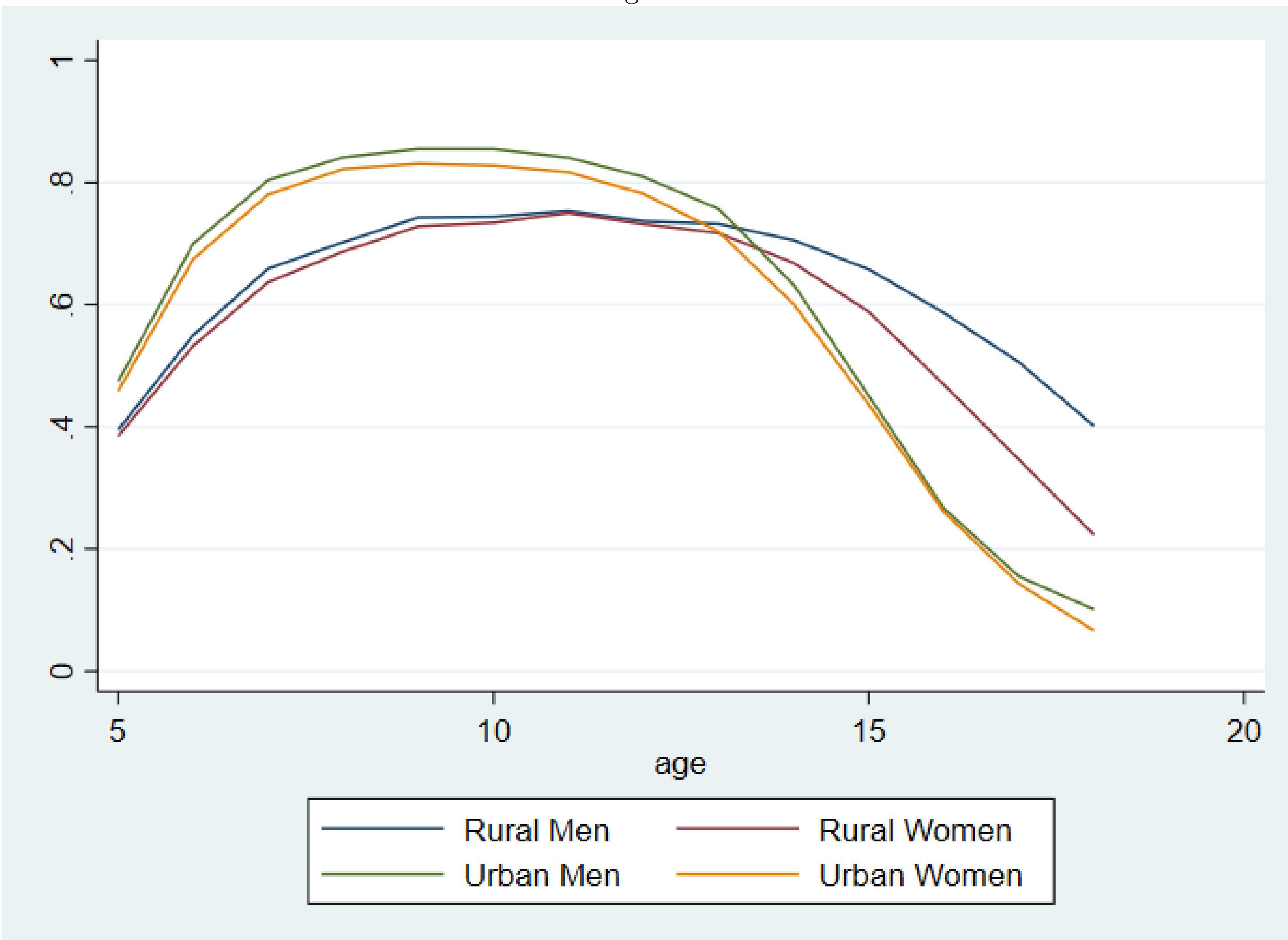
Figure 1



Source: Go (2009)

9.2 Attendance Rates by Age

Figure 2



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

A Additional Tables

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

	Rural	Urban	Rural 11+	Urban 11+
	prob(att)	prob(att)	prob(att)	prob(att)
Contemporaneous Treatment	0.0273	-0.0271	0.0342	-0.0170
(observed after law passed)	(0.0261)	(0.0150)	(0.0278)	(0.0139)
Full Treatment	0.0466*	0.0254	0.0517*	0.0309
($age < 7$ when law passed)	(0.0202)	(0.0247)	(0.0204)	(0.0244)
Partial Treatment	0.0111	0.0263	0.0135	0.0331*
($11 > age > 6$ when law passed)	(0.0107)	(0.0138)	(0.0143)	(0.0157)
N	17,610,766	5,704,146	6,544,064	2,045,437

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

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 - Contemporaneous Effect, Low SES Only

	Rural prob(att)	Urban prob(att)	Rural 11+ prob(att)	Urban 11+ prob(att)
Contemporaneous Treatment	0.0271	-0.0299	0.0346	-0.0188
(observed after law passed)	(0.0263)	(0.0153)	(0.0281)	(0.0149)
Full Treatment	0.0486*	0.0268	0.0543*	0.0332
($age < 7$ when law passed)	(0.0206)	(0.0257)	(0.0210)	(0.0250)
Partial Treatment	0.0122	0.0284	0.0152	0.0364*
($11 > age > 6$ when law passed)	(0.0110)	(0.0152)	(0.0145)	(0.0175)
N	16,222,381	4,441,612	6,024,103	1,572,870

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

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 low-SES 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.3: Staggered Adoption Differences in Differences - High SES Only

	Rural	Urban	Rural 10+	Urban 10+
	prob(att)	prob(att)	prob(att)	prob(att)
Full Treatment	0.0571*	0.0018	0.0618**	0.0160
($age < 7$ when law passed)	(0.0209)	(0.0227)	(0.0220)	(0.0228)
	[0.015,0.118]**	[-0.048,0.078]	[0.017,0.127]**	[-0.036,0.089]
Partial Treatment	0.0340	0.0025	0.0368	0.0153
($11 > age > 6$ when law passed)	(0.0208)	(0.0140)	(0.0215)	(0.0128)
	[-0.008,0.091]	[-0.021,0.047]	[-0.006,0.097]	[-0.006,0.059]
N	6,618,741	4,949,965	3,103,894	2,275,181

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

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 high-SES 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 - Low SES Only, South Excluded

	Rural prob(att)	Urban prob(att)	Rural 10+ prob(att)	Urban 10+ prob(att)
Full Treatment	0.0454*	-0.0257	0.0438*	-0.0117
($age < 7$ when law passed)	(0.0193)	(0.0178)	(0.0183)	(0.0227)
	[0.005,0.115]*	[-0.060,0.030]	[0.005,0.105]*	[-0.062,0.058]
Partial Treatment	0.0133	-0.0254	0.0126	-0.0149
($11 > age > 6$ when law passed)	(0.0201)	(0.0134)	(0.0197)	(0.0126)
	[-0.029,0.084]	[-0.060,0.010]	[-0.030,0.080]	[-0.048,0.021]
N	7,993,373	661,282	3,861,848	315,797

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

Standard errors clustered at the state level in parentheses. Critical values are obtained from the t-distribution with 17 degrees of freedom. 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.5: Staggered Adoption Differences in Differences - High SES Only, South Excluded

	Rural prob(att)	Urban prob(att)	Rural 10+ prob(att)	Urban 10+ prob(att)
Full Treatment	0.0305	-0.0171	0.0336	0.0017
($age < 7$ when law passed)	(0.0187)	(0.0178)	(0.0178)	(0.0192)
	[-0.004,0.093]	[-0.053,0.046]	[-0.001,0.091]	[-0.037,0.071]
Partial Treatment	0.0053	-0.0162	0.0057	-0.0001
($11 > age > 6$ when law passed)	(0.0167)	(0.0086)	(0.0165)	(0.0057)
	[-0.024,0.065]	[-0.035,0.007	[-0.023,0.066]	[-0.011,0.016]
N	5,313,697	4,354,067	2,482,693	1,999,381

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

Standard errors clustered at the state level in parentheses. Critical values are obtained from the t-distribution with 17 degrees of freedom. 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.6: 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.7: Staggered Adoption Differences in Differences - State Natives Only

	Rural	Urban	Rural 10+	Urban 10+
	prob(att)	prob(att)	prob(att)	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)
	[0.018,0.123]**	[-0.051,0.070]	[0.015,0.142]**	[-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)
	[-0.020,0.087]	[-0.022,0.044]	[-0.022,0.099]	[-0.010,0.055]
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.