

# 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 U.S. municipalities paid small user fees, known as rate bills, for their children to attend public schools. Urban school districts gradually repealed these fees and funded public education through local taxes, but rural areas continued to charge tuition until state-level policies abolished the practice for public schools. Using United States Census data and a staggered adoption difference-in-differences approach, I show that 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

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# 1 Introduction

During the late 19th century, the United States became one of the most educated countries in the world. According to [Lindert \(2004\)](#), 90.6% of all children between the ages of 5 and 14 were enrolled in primary school in 1880. This was the highest rate in the world, with only three other countries within 15 percentage points of the United States. Between 1830 and 1880, the fraction of children aged 5-14 enrolled specifically in public school increased from 54.6% to 80.0% ([Lindert, 2004](#)). Although many people may associate the rise in mass schooling with the inception of compulsory schooling laws (CSL's), this shift actually occurred prior to effective attendance-mandating legislation.<sup>1</sup> Why did primary school enrollments increase so dramatically in the United States and not elsewhere? What policies contributed to this surge?

One possible contributor to increased primary school attendance was state-level policies that prevented public schools from charging tuition. Although public schools had been heavily subsidized, many school districts levied small fees, known as rate bills, until the late 19th century.<sup>2</sup> While most major cities repealed rate bills and provided free primary education of their own volition, many rural municipalities continued to charge tuition until state-level rate bill abolition laws banned the practice for public schools. Revenue from abolished rate bills was typically replaced by state funding, thus shifting the pecuniary cost of education from students and their families to society writ large. Historical evidence suggests that this change was imposed on rural areas via state legislatures or referenda. [Cubberley \(1919\)](#) states that “Cities demanded educational progress, and were determined to have it, regardless of cost.” It remains an open question as to what extent rate bills inhibited educational attainment, and, consequently, to what extent their removal contributed to the increase in enrollment during the 19th century. In order to better understand how the United States became a leader in education and the role of free schools in this transition, this paper analyzes the impact of state-level rate bill abolition laws on attendance in rural areas.

I employ a staggered adoption difference-in-differences specification using data from the United States Decennial Censuses of 1850 through 1880 and rate bill abolition data for 27 states. These policies were effectively imposed upon rural areas, suggesting that rate bill abolition was an exogenous

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<sup>1</sup>Compulsory schooling laws are discussed in greater detail in Section 5.3 and were generally ineffective prior to 1880.

<sup>2</sup>The magnitude of rate bills is discussed in greater detail in Section 2.

shock to those school districts that explicitly chose to charge tuition until they were forced to abandon the practice. I restrict the sample to native-born non-Hispanic white Americans in order to minimize potential confounding factors such as the abolition of slavery and changes in population composition with respect to immigration. Since rate bill abolition dates are only available for 27 states, I utilize the wild bootstrap to account for the low number of clusters, which can otherwise lead to over-rejection of the null hypothesis.

I find that rate bill abolition increased rural attendance by 7.2 percentage points for individuals below the age of seven when the laws were passed. There is no evidence for a corresponding effect in urban areas, suggesting that the observed effect in rural areas is driven by differential exposure to rate bills prior to their abolition.<sup>3</sup> These effects are especially strong for individuals from low socio-economic status families, for whom low levels of tuition may have been a greater obstacle to education. The positive effects on attendance persist for individuals observed above the age of nine, indicating that rate bill abolition induced children to continue attending through late primary school and thus contributed to the foundation for the “high-school revolution” documented by Goldin (1998).

Back of the envelope calculations suggest that this increase in attendance translates to an additional 0.72 years of average educational attainment for children in rural areas.<sup>4</sup> Estimates for returns to education generally range from 4.3% (Goldin and Katz, 2000) to 14.8% (Oreopoulos, 2006) per year.<sup>5</sup> Based on this large literature, the returns generated by an additional 0.72 years of educational attainment imply increased income of between 3.1% and 10.7% for the rural populations affected by rate bill abolition. This increase in income highlights the direct economic significance of free public schools.

Rate bill abolition may have also contributed to the structural change of the United States economy during the 19th century. It has been well-documented that education is instrumental for agricultural productivity (Schultz, 1978; Huffman, 2001; Goldin and Katz, 2000), which is especially relevant to the rural population during this time period. Increased productivity of the agricultural sector induces workers to reallocate to other sectors of the economy (Cao and Birchenall, 2013; Emerick, 2018), thus

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<sup>3</sup>Specifically, this supports the assumption that rate bill abolition was not accompanied by other education reform policies, as other reforms presumably would have affected both rural and urban children.

<sup>4</sup>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.

<sup>5</sup>Table 7 summarizes the literature on annual returns to education.

encouraging structural change. In addition, highly-educated workers are more suited for non-agricultural occupations (Goldin and Katz, 2000), and increased education is associated with higher innovation and research (Toivanen and Väänänen, 2016).<sup>6</sup> This suggests that additional education induced by rate bill abolition contributed to development and structural change as the United States evolved from an economy based on agriculture to one based on industry.

The existing literature has found mixed evidence for 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 courses 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, Fishlow (1966a) finds that changes in enrollment between 1830 and 1840 were comparable in Massachusetts, which abolished rate bills in 1826, and New York, which abolished rate bills in 1867, thus suggesting “that the legislated abolition of rate bills had little impact on aggregate school enrollment” (Goldin and Katz, 2009). This paper extends the literature on free schools beyond these specific contexts and exploits the staggered abolition of rate bills across many states, with a focus on the exogenous implementation in rural areas.

This paper also contributes to the literature on prices around zero. Some educators and policymakers in the 19th century were worried about social marketing, the idea that “[that] which costs nothing is considered as worth nothing” (Connecticut State Board of Education, 1868). However, in the context of modern post-secondary education, Denning (2017) shows that post-secondary attendance is more sensitive to decreased tuition when the price is already close to zero. In addition, various papers in health economics have found 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). My results provide further evidence that small prices, such as the rate bills charged to families of children attending public schools, can inhibit behaviors, thus suggesting that decision-makers

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<sup>6</sup>The results of Goldin and Katz (2000) specifically suggest that education induces 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, which implies reallocation from the former to the latter.

do not place less value on goods and services available for free (Cohen and Dupas, 2010).

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

Public schools were originally formed in the American colonies to provide formal primary education to the general population. Lindert (2004) states that “[s]tarting 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.” As the United States expanded west, the Land Ordinance of 1785 required that new towns provide and maintain schools to educate the local population (Goldin and Katz, 2003). Rate bills became a popular tool for school districts to increase revenue as education systems expanded in the 18th and 19th centuries, as opposed to increased property taxes, 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).

The magnitude of rate bills varied across states and school districts but was generally small relative to incomes.<sup>7</sup> 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.<sup>8</sup> 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

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<sup>7</sup>Goldin and Katz (2003) states that “rate bills often covered just part of the total cost of schooling, and occasionally only a small part.”

<sup>8</sup>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.

bills than elementary classes on alphabet and spelling. The largest of these fees was \$2, equivalent to approximately \$52 in 2019. Even in the American South, where rate bills were larger and wages were lower, I calculate that tuition for a full academic year was only 2.5% of average annual income for agricultural workers, highlighting the fact that rate bills represented small pecuniary costs to attend public schools.

The exact structure of rate bills also varied across school districts. [Goldin and Katz \(2003\)](#) document that “these tuition payments were charged for days in attendance exceeding some number of days that were provided by [most communities] free of direct charge. In [other communities] they were levied for the full term.”<sup>9</sup> In some cases, rate bills were not charged to families that could not afford them ([Connecticut State Board of Education, 1868](#)). However, this was the exception rather than the norm; in general, “children from indigent families who were not able to afford the fees were immediately banned from public schooling” ([Go, 2009](#)). This suggests that public schools were excludable; children could not attend without paying tuition.<sup>10</sup>

Over time, the general population thought of public provision of education as more important to society. As these attitudes shifted, school districts gradually reduced rate bill requirements to encourage attendance. By 1850, rate bills provided only about 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 as 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](#)

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<sup>9</sup>This is discussed in greater detail in Section 6 with regard to how the estimated effects on attendance translate to educational attainment.

<sup>10</sup>If rate bills were not charged to the poorest families, it is still likely that their existence was an obstacle to the children of those families due to the stigma associated with rate bill abatement ([Connecticut State Board of Education, 1868](#)). In either case, the existence of rate bills would have inhibited attendance, whether the reason was stigma or inability to pay.

Katz, 2003).<sup>11</sup> In addition to providing free schools at the local level, urban areas attempted to impose rate bill abolition on entire states.<sup>12</sup> On the other hand, rural areas continued to charge tuition after major cities provided primary schooling free of charge. Rural preference of rate bills is also exhibited by their voting patterns: rural voters in New York overwhelming chose the status quo in multiple referenda before the state legislature finally abolished rate bills in 1867 (Cubberley, 1919). Eventually, state legislatures and public referenda forced rural school districts to provide schools without tuition requirements. This rural resistance to rate bill abolition and state-level imposition of free schools forms the basis of my empirical strategy.

### 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 (2003), Swift (1911), and Mead (1918). These data are only available for 27 states, all of which are included in the main analysis. 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, 2003; Clay, Lingwall and Stephens Jr, 2012; Snyder and Tan, 2005). Figure 1 shows which states are included and the variation in timing across states with regards to rate bill abolition.

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 individual’s attendance status, age, race, gender, occupation, various measures of socio-economic status (SES), urbanicity, current state, and state of birth.<sup>13</sup> In each Census year utilized here, the Census asked some variation

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<sup>11</sup>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).

<sup>12</sup>Cubberley (1919) notes that “[cities] would not tolerate the rate bill [anywhere in the state], and, despite their larger property interests, they favored tax-supported schools.”

<sup>13</sup>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. Section 5.7 discusses a robustness check with an alternative definition of urbanicity, for which results are qualitatively similar.

of “was the person at school within the last year?” for every person in the household.<sup>14</sup> I restrict the dataset to native-born whites between the ages of 5 and 14 in rural areas of the 27 states for which rate bill abolition data is available.<sup>15</sup>

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. Attendance rates are similar for males and females and higher in urban areas than rural areas. This may have been due to rural children working rather than attending school (Cubberley, 1919).<sup>16</sup> 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 differences in returns to education, may have contributed to the higher observed attendance rates in urban areas.

Table 2 shows relevant summary statistics for each of four groups of states: those that abolish rate bills prior to 1850, between 1850 and 1859, between 1860 and 1869, and later than 1869. Occupation scores and attendance rates are lower in rural areas than urban ones for each group of states. It is also noteworthy that most individuals in 1850 are considered rural by the Census definition of urbanicity, but this percentage changes over time; approximately 70% of the sample is rural in 1880, relative to 88% in 1850. This change highlights the importance of focusing only on the rural population, as aggregate shifts in attendance may be driven by compositional changes in urbanicity.

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<sup>14</sup>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. The Census did not distinguish between attendance at public and private schools. Therefore, changes in attendance over time reflect aggregate trends rather than compositional shifts from private institutions to public ones. According to Goldin and Katz (2003) and Clay, Lingwall and Stephens Jr (2012), these attendance figures can be reasonably interpreted as enrollment rates; I use the term “attendance” to mirror the format of the question, but it is worth noting the slight difference between the two. While a single day of school attendance in the previous year could induce a “Yes” response to these questions, this imperfect measure is consistent across the surveys. In addition, the inclusion of year-by-age fixed effects resolves any potential differences in measurement across Census years.

<sup>15</sup>Children of other races or nationalities may have been subject to discrimination that prevented attendance regardless of any user fees charged, and enslaved individuals were not included in the 1850 and 1860 Censuses. I choose ages 5 through 14 to focus on primary-school-age children and mirror the literature on compulsory schooling laws. Immigrants and non-white children make up 12.0% of the 5-14 population overall and 11.7% of the rural population. Urban areas are used as a placebo test since rate bill abolition was not binding to areas that already provided free public schools.

<sup>16</sup>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 to identify the effect of rate bill abolition on attendance, 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.<sup>17</sup> 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, since rate bill abolition does not have any empirical effect in areas that did not charge rate bills.

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, as is 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.

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<sup>17</sup>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.

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 (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, are both  $Treatment = 0$  and  $PartialTreatment = 0$  for individuals older than ten years old when these laws are passed. All individuals are considered untreated prior to rate bill abolition.

For example, Connecticut passed a rate bill abolition law in 1868. All individuals observed in Connecticut 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, 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 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:

$$Y_{aist} = \alpha + \beta_1 \text{Treatment}_{ast} + \beta_2 \text{PartialTreatment}_{ast} + \gamma_{as} + \delta_{at} + \varepsilon_{aist} \quad (1)$$

where  $Y_{aist}$  is the attendance status of an individual  $i$  observed at age  $a$  in state  $s$  and Census year  $t$ ,  $\gamma_{as}$  is a state-by-age fixed effect,  $\delta_{at}$  is a Census year-by-age fixed effect, and  $\varepsilon_{aist}$  is the error term.<sup>18</sup>

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 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 95% confidence intervals 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.<sup>19</sup>

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<sup>18</sup>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. My findings are robust to the inclusion of year and age fixed effects instead of year-by-age fixed effects. 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. My findings are robust to the inclusion of state fixed effects instead of state-by-age fixed effects.

<sup>19</sup>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 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.<sup>20</sup> 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 prior to state-level rate bill abolition, so these policies should not have any impact on urban attendance. Estimated coefficients for urban children are close to zero and statistically insignificant, in stark contrast to those in rural areas. The null results of the placebo test support the identifying assumption that rate bill abolition did not coincide with other attendance-promoting policies, as is discussed in Section 4.1; the fact that estimated coefficients are only positive and statistically significant for rural areas suggests that the results are indeed driven by the implementation of free schools at the state level rather than state-level trends or other policies.

Column 3 of Table 3 shows the results of Equation 1 including only children at least ten years old when they are observed in the Census. The results presented in Column 3 are qualitatively similar to

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<sup>20</sup>Table A.2 shows the results of Equation 1 estimated separately for males and females. The estimated coefficients for males and females are qualitatively similar; the estimated coefficient on rural males is slightly larger than that for females, but the two are not statistically different. This suggests that males and females were similarly affected by rate bill abolition. In a similar vein, Table A.10 shows the results of Equation 1 estimated separately for children observed at different ages. Treatment status is still assigned based on an individual's age when rate bills were abolished. Results are qualitatively similar to those presented in Column 1 of Table 3; there is no evidence for heterogeneity by observed age.

those in Column 1, indicating that the positive effects on attendance persist for children throughout primary school, as opposed to being concentrated in younger children.<sup>21</sup> Primary education is effectively a prerequisite to secondary school 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 \(1998\)](#).

Column 2 of Table 4 shows the results of my primary specification including only rural children of lower socio-economic status.<sup>22</sup> 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 statistically different from the coefficient estimated on low-SES individuals.<sup>23</sup> This suggests that rate bill abolition led to attendance gains for all rural children, but the effects were stronger for children of lower socio-economic status. This is unsurprising, as low pecuniary costs would have represented a greater obstacle to education for low-SES households.

## 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 identification. 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

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<sup>21</sup>Many younger children were already attending schools prior to these policies, as is shown in Figure 2. Students at least ten years old, on the other hand, would presumably be more likely to stop attending school, especially if they had already attained some level of education prior to entering the labor force.

<sup>22</sup>Low socio-economic status is defined as the maximum occupation score in the household under 19.5. 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.

<sup>23</sup>[Chow \(1960\)](#) describes a Chow Test for equality of coefficients from two separate regressions. [Toyoda \(1974\)](#), [Watt \(1979\)](#), [Honda \(1982\)](#), and [Ohtani and Toyoda \(1985\)](#) extend the Chow Test to a Wald test for equality of coefficients.

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.<sup>24</sup> Results are presented in Table 5 and are qualitatively similar to those results presented for the full sample of 27 states.<sup>25</sup> 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 find 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),<sup>26</sup> the literature concludes 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 (Landes and Solmon, 1972; Clay, Lingwall and Stephens Jr, 2012; Judd, 1918; Stambler, 1968; Tyack, James and Benavot, 1987).<sup>27</sup>

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<sup>24</sup>Excluded states are Arkansas, Florida, Georgia, Louisiana, Maryland, Missouri, South Carolina, Virginia, and West Virginia.

<sup>25</sup>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.

<sup>26</sup>Clark and Royer (2013) and Oreopoulos (2006) examine similar policies and outcomes in the United Kingdom.

<sup>27</sup>The first compulsory schooling law in Massachusetts was passed in 1852 but was not considered effective until 1890 (Deffenbaugh and Keesecker, 1935).

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

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

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. Information on compulsory schooling laws is drawn from [Go \(2009\)](#), [Goldin and Katz \(2003\)](#), [Clay, Lingwall and Stephens Jr \(2012\)](#), and [Snyder and Tan \(2005\)](#). Results are displayed in Column 2 of Table 6.

Second, I restrict the sample to the 25 states that first passed compulsory schooling legislation dates after 1870,<sup>28</sup> truncate the 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, 2020, 2022](#); [Goodman-Bacon, 2021](#); [Rios-Avila, Sant’Anna and Callaway, 2021](#)). The two-way fixed effects estimator identifies the weighted sum of all possible two-group two-period difference-in-differences 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 estimated coefficients.

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 shows that the weights are positive for every two-group two-period comparison, which suggests that negative weights do not bias the main results. Second, I estimate coefficients using the estimator proposed by [Callaway and Sant’Anna \(2021\)](#). The estimated Average Treatment Effects on

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<sup>28</sup>Only Massachusetts and Vermont had passed compulsory schooling laws prior to 1870.

the Treated 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 the results towards zero.

## 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 thresholds 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 changes over time, in terms of both attendance age profiles and overall attendance levels, estimated coefficients could still be spuriously positive and statistically significant due to these broad time trends. If this were the case, then randomized treatment timing may provide similar results to the primary specification using the true treatment assignment, 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 specification 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:

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

Results are presented in Table A.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 this, 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 have migrated 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.<sup>29</sup>

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<sup>29</sup>It is unlikely that rate bill abolition induced within-state rural-urban migration because these policies made rural areas relatively more attractive for families of potential students. Urban-rural migration is not concerning due to the general trend of urbanization at this time.

Seven of the 27 states for which rate bill abolition data are available passed such legislation prior to the sample period here. I estimate Equation 1 without these seven states and present the results in Table A.5. The results are qualitatively similar to those in Table 3, suggesting that the inclusion of always-treated states does not inhibit estimation and inference of the staggered adoption difference-in-differences results.

The Census classifies individuals as urban if there are at least 2,500 inhabitants in the same municipality. Approximately 25% of the white native-born school-age population is considered urban over the four Census years. As a robustness check, I consider an alternative threshold of 100,000 individuals in order to exclude only the largest urban centers from the rural sample. Under this alternative definition, only 11% of the sample is considered urban. Table A.7 shows the results from the primary specification using the Census and alternative definitions of urbanicity; estimated coefficients are slightly lower, presumably due to the inclusion of individuals unaffected by state-level rate bill abolition, but qualitatively similar. Inference is unaffected, as the results remain statistically significant at the 1% level using both the  $t$ -distribution and the wild bootstrap.

Table A.8 replicates Table 4 using the professional status of an occupation rather than the occupation score assigned to it.<sup>30</sup> Results are qualitatively similar but somewhat stronger than those presented in Table 4. Specifically, the difference between the estimated coefficients for laborers and professionals is larger and more statistically significant than those for low socio-economic status households and high socio-economic status households. This suggests that the differences in the effects between these two groups are not driven by the exact definition of high and low socio-economic status.

The sample in the primary specification does not include children born outside the United States. However, it is possible that even children of immigrants, rather than just immigrants themselves, may have been subject to various obstacles that prevent school attendance, such as discrimination or language barriers. Table A.9 replicates Table 3 using only children for whom every individual in the household was born in the United States. Results are qualitatively similar to those using the main sample; if anything, estimated coefficients are further away from zero and more statistically significant for rural individuals than the primary specification.

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<sup>30</sup>The classification of “professional” occupations is available upon request.

Finally, Appendix B and Appendix C describe two additional robustness checks. Appendix B discusses and employs a triple difference-in-differences framework using urban, high socio-economic status households, and professional households as controls groups. Appendix C replicates the primary analysis with additional controls for the number of schools, the number of teachers, and funding for schools. Although there are various limitations with both of these two alternative approaches, the results are qualitatively similar to the main specification.

## 6 Economic and Historical Significance

The increase in primary school attendance shown in Column 1 of Table 3 translates to an extra 0.72 years of average education for the rural population.<sup>31</sup> This estimate may understate the true effect on educational attainment for three reasons. First, as is discussed in Section 4.1, estimated coefficients may be attenuated towards zero for various reasons. 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. If the shift from no attendance to part-time attendance is less than the shift from part-time attendance to full-time attendance, then the 0.72 additional years would be an underestimate.<sup>32</sup>

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<sup>31</sup>Individuals are 7.2 percentage points more likely to attend for any year in a 10-year period.  $0.072 \times 10 = 0.72$

<sup>32</sup>It is possible for 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, 2003).

Third, this analysis only considers the direct effect on primary-school-age children. 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 a large direct effect on wages. Table 7 shows the estimated annual returns to education from various papers. In general, these estimated returns are positive and statistically significant, ranging from 4.3% to 14.8%.<sup>33</sup> For example, [Duflo \(2001\)](#) exploits rapid school construction in Indonesia in the 1970's and finds that an additional year of education increased income by 10.6 percent.<sup>34</sup> Other papers exploit either cross-sectional variation or CSL's and find qualitatively similar results. Based on the range of estimates from 4.3% to 14.8%, an additional 0.72 years of educational attainment would lead to a 3.1% to 10.7% increase in income.

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 by inducing 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 occupations in other sectors as well as agriculture.

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<sup>33</sup>This range excludes the estimate from [Stephens Jr and Yang \(2014\)](#), which is an outlier in the literature at -0.3%.

<sup>34</sup>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 the marginal year of education was at the primary school level.

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 agriculture and other sectors to make better use of contemporary modern techniques and technologies.

Lastly, increased primary school attendance was crucial as the United States became one of the most-educated countries. By 1880, the primary school enrollment rate was approximately 90.6%, higher than every country in the world ([Lindert, 2004](#)).<sup>35</sup> 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, 2003](#)). 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 rural children that started school after abolition laws took effect. [Cubberley \(1919\)](#) was indeed correct in asserting that “the [rate bill] 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.

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<sup>35</sup>This figure considers children between the ages of 5 and 14 enrolled in either public or private schools. 80.0% of children between the ages of 5 and 14 were enrolled specifically in public schools.

These results have various implications for the modern education systems. Tuition requirements for primary and secondary schools persist in many developing countries. India 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.

Primary schooling in the 19th century also provides an interesting parallel for post-secondary education in the United States today. 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.

The structure of the data limits this analysis. This paper relies on attendance as it is marked in the 1850 through 1880 Censuses, thus capturing only changes at the extensive margin of school attendance. Similarly, I am unable to comment on overall educational attainment other than back-of-the-envelope calculations due to the lack of data on educational attainment and prevalence of part-time schooling. Future work should further investigate the impact of free schooling in modern contexts on attendance and educational attainment, as well as other outcomes such as wages and social mobility.

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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
% occupation score < 19.5	42.9	66.7	53.1	55.1
% occupation score < 19.5 (rural only)	49.8	69.6	60.5	58.1
$N$ (1850 only)	1,059,576	1,066,815	1,367,436	431,562
$N$	5,929,667	6,836,93	8,356,345	2,191,969

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, but “the first available literacy statistics of 1840 testify... more than 90 percent of white adults achieved this minimum competence” ([Fishlow, 1966b](#)).

Table 3: Staggered Adoption Difference-in-Differences

	(1)	(2)	(3)	(4)
Sample	Rural	Urban	Rural 10+	Urban 10+
Full Treatment	0.0720***	0.0003	0.0790***	0.0147
( <i>age</i> < 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 > <i>age</i> > 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]
<i>N</i>	17,610,766	5,704,146	8,398,881	2,636,717
Untreated Outcome Mean	58.3%	68.1%	65.9%	73.3%

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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 Difference-in-Differences - Heterogeneity by Socioeconomic Status

	(1)	(2)	(3)	(4)
Sample	All	Low SES	High SES	Difference
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	0.0046
( $11 > age > 6$ 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	
Untreated Outcome Mean	58.3%	56.4%	61.9%	

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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 3 and tests for statistical significance of these difference via a Wald-type Chow Test (Chow, 1960; Toyoda, 1974; Watt, 1979; Honda, 1982; Ohtani and Toyoda, 1985). 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 Difference-in-Differences - Excluding American South

	(1)	(2)	(3)	(4)
Sample	Rural	Urban	Rural 10+	Urban 10+
Full Treatment	0.0401**	-0.0181	0.0400**	0.0006
( <i>age</i> < 7 when law passed)	(0.0190)	(0.0177)	(0.0179)	(0.0190)
	[0.005,0.107]**	[-0.054,0.045]	[0.007,0.099]**	[-0.038,0.073]
Partial Treatment	0.0096	-0.0180*	0.0092	-0.0026
(11 > <i>age</i> > 6 when law passed)	(0.0187)	(0.0088)	(0.0183)	(0.0063)
	[-0.026,0.072]	[-0.037,0.005]*	[-0.027,0.071]	[-0.016,0.013]
<i>N</i>	13,307,070	5,015,349	6,344,541	2,315,178
Untreated Outcome Mean	71.8%	70.9%	77.8%	74.2%

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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 Difference-in-Differences - Compulsory Schooling Laws

	(1)	(2)	(3)
Sample	Rural	Rural: CSL on RHS	Rural: 1850-1870
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
Untreated Outcome Mean	58.3%	58.3%	58.4%

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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.

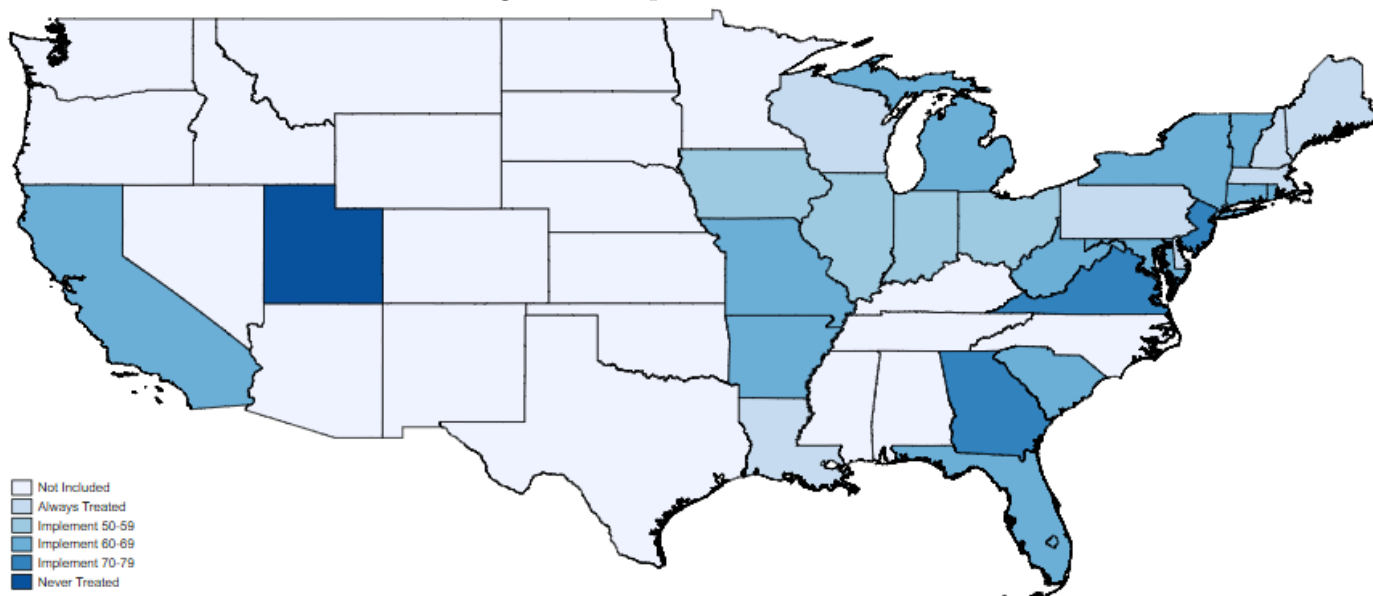
Table 7: Annual Returns to Education

Paper	Context	Annual Returns
Angrist and Krueger (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. 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

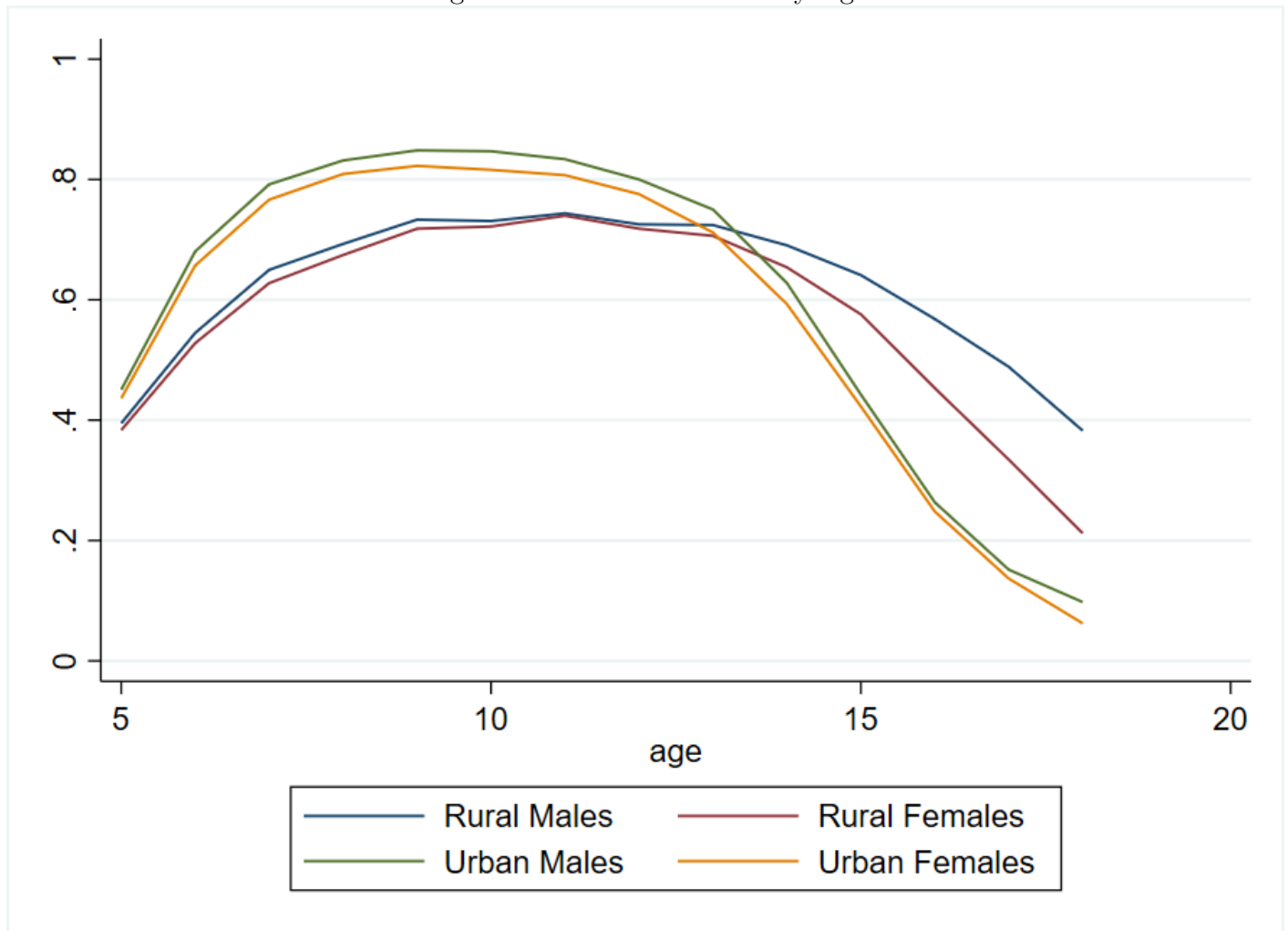
Figure 1: Map of Included States



Source: [Go \(2009\)](#)

Figure 1 shows the states that are included in the analysis and highlights when various states abolished rate bills. The unshaded states are not included in the analysis because their rate bill abolition dates are unknown. The lightest blue states abolished rate bills prior to 1850 and are therefore considered “always-treated.” Darker shades are associated with later decades of abolition. Utah abolished rate bills in 1890 and is thus considered “never-treated” in the analysis.

Figure 2: Attendance Rates by Age



Attendance Rates by Age, broken into subgroups by gender and urbanicity.

Source: 1850 Census, [Ruggles et al. \(2019\)](#)

Figure 2 shows the attendance rates by age for individuals between the ages of 5 and 18, broken down across dimensions of gender and urbanicity. Males and females attend at similar rates. Urban attendance is higher than rural attendance for younger ages. Rural attendance declines more slowly than urban attendance after the age of 13. This is presumably driven by part-time attendance, which the Census does not distinguish from full-time attendance.

## Appendix A Additional Tables and Figures

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

	(1)	(2)	(3)	(4)
Sample	Rural	Urban	Rural 10+	Urban 10+
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
( <i>age</i> < 7 when law passed)	(0.0202)	(0.0247)	(0.0206)	(0.0249)
	[-0.002,0.126]*	[-0.028,0.124]	[0.003,0.112]**	[-0.024,0.123]
Partial Treatment	0.0111	0.0263*	0.0130	0.0303*
(11 > <i>age</i> > 6 when law passed)	(0.0107)	(0.0138)	(0.0126)	(0.0149)
	[-0.011,0.055]	[-0.003,0.090]*	[-0.015,0.060]	[-0.002,0.098]*
<i>N</i>	17,610,766	5,704,146	8,398,881	2,636,717
Untreated Outcome Mean	58.3%	68.1%	65.9%	73.3%

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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, with an additional indicator for if the law has been passed prior to the Census year, estimated on rural individuals between the ages of 5 and 14. Column 2 shows the results from the adjusted Equation 1 estimated on urban individuals between the ages of 5 and 14. Column 3 shows the results from the adjusted Equation 1 estimated on rural individuals between the ages of 10 and 14. Column 4 shows the results from the adjusted 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 Difference-in-Differences - Heterogeneity by Gender

	(1)	(2)	(3)
Sample	All	Males	Females
Full Treatment	0.0720***	0.0745***	0.0695***
( <i>age</i> < 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 > <i>age</i> > 6 when law passed)	(0.0209)	(0.0211)	(0.0207)
<i>N</i>	17,610,766	8,987,577	8,623,189
Untreated Outcome Mean	58.3%	58.8%	57.8%

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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 males. Column 3 shows the results from Equation 1 estimated only on rural females. 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 Difference-in-Differences - Alternate Definition of Treatment

	(1)	(2)	(3)	(4)
Sample	Rural	Urban	Rural 10+	Urban 10+
Full Treatment	0.0721***	0.0003	0.0790***	0.0148
( <i>age</i> < 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
( <i>age</i> = 7 when law passed)	(0.0201)	(0.0160)	(0.0227)	(0.0173)
Partial Treatment	0.0359	0.0024	0.0375	0.0155
( <i>age</i> = 8 when law passed)	(0.0213)	(0.0167)	(0.0240)	(0.0157)
Partial Treatment	0.0399*	0.0044	0.0438*	0.0133
( <i>age</i> = 9 when law passed)	(0.0212)	(0.0134)	(0.0220)	(0.0119)
Partial Treatment	0.0281	-0.0002	0.0316	0.0079
( <i>age</i> = 10 when law passed)	(0.0234)	(0.0136)	(0.0240)	(0.0117)
<i>N</i>	17,610,766	5,704,146	8,398,881	2,636,717
Untreated Outcome Mean	58.3%	68.1%	65.9%	73.3%

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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 Difference-in-Differences - State Natives Only

	(1)	(2)	(3)	(4)
Sample	Rural	Urban	Rural 10+	Urban 10+
Full Treatment	0.0617***	-0.0056	0.0660**	0.0070
( <i>age</i> < 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 > <i>age</i> > 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]
<i>N</i>	15,094,635	5,043,825	7,009,657	2,284,139
Untreated Outcome Mean	59.6%	68.6%	67.5%	73.9%

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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 Difference-in-Differences - Excluding Always-Treated States

	(1)	(2)	(3)	(4)
Sample	Rural	Urban	Rural 10+	Urban 10+
Full Treatment	0.0704***	0.0207	0.0768**	0.0401*
( <i>age</i> < 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 > <i>age</i> > 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]
<i>N</i>	13,581,755	3,803,490	6,482,742	1,744,632
Untreated Outcome Mean	58.3%	68.1%	65.9%	73.3%

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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 Difference-in-Differences - Sensitivity Analysis

	Oldest Partial Treatment = 9	Oldest Partial Treatment = 10	Oldest Partial Treatment = 11
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]***

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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. Similarly, each row provides results for a different age for the Youngest Partial Treatment. 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.

Table A.7: Staggered Adoption Difference-in-Differences - Alternative Definition of Urbanicity

	(1)	(2)	(3)	(4)
Sample	Rural	Rural <sup>alt</sup>	Urban	Urban <sup>alt</sup>
Full Treatment	0.0720***	0.0622***	0.0003	0.0012
( <i>age</i> < 7 when law passed)	(0.0202)	(0.0205)	(0.0224)	(0.0320)
	[0.029,0.129]***	[0.017,0.118]***	[-0.049,0.072]	[-0.067,0.122]
Partial Treatment	0.0367*	0.0293	0.0015	0.0092
(11 > <i>age</i> > 6 when law passed)	(0.0209)	(0.0198)	(0.0146)	(0.0243)
	[-0.006,0.087]*	[-0.011,0.079]	[-0.024,0.047]	[-0.041,0.096]
<i>N</i>	17,610,766	20,849,627	5,704,146	2,465,285
Untreated Outcome Mean	58.3%	59.3%	68.1%	67.9%

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

*alt*: alternative definition of urbanicity

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, using the Census definition of rural. Column 2 shows the results from Equation 1 estimated on rural individuals between the ages of 5 and 14, using the alternative definition where a municipality is considered “urban” if it has more than 100,000 inhabitants as measured in the Census. Column 3 shows the results from Equation 1 estimated on urban individuals between the ages of 5 and 14, using the Census definition of urban. Column 4 shows the results from Equation 1 estimated on urban individuals between the ages of 5 and 14, again using the alternative definition described above. 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.8: Staggered Adoption Difference-in-Differences - Heterogeneity by Professionalism

	(1)	(2)	(3)	(4)
Sample	All	Laborer	Professional	Difference
Full Treatment	0.0720***	0.0752**	0.0269*	0.0483***
( <i>age</i> < 7 when law passed)	(0.0202)	(0.0203)	(0.0144)	$\chi^2(1) = 15.54$
	[0.029,0.128]***	[0.031,0.130]***	[-0.003,0.064]*	p=0.000
Partial Treatment	0.0367*	0.0386*	0.0108	0.0278**
(11 > <i>age</i> > 6 when law passed)	(0.0209)	(0.0212)	(0.0145)	$\chi^2(1) = 4.89$
	[-0.006,0.087]*	[-0.006,0.090]*	[-0.018,0.052]	p=0.027
<i>N</i>	17,610,766	15,415,370	2,195,396	
Untreated Outcome Mean	58.3%	57.2%	68.9%	

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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 only children from non-professional households. Column 3 shows the results from Equation 1 estimated on only children from professional households. Column 4 shows the differences between the estimated coefficients in Column 2 and Column 3 and tests for statistical significance of these difference via a Wald-type Chow Test (Chow, 1960; Toyoda, 1974; Watt, 1979; Honda, 1982; Ohtani and Toyoda, 1985). 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.9: Staggered Adoption Difference-in-Differences - Non-Immigrant Households

	(1)	(2)	(3)	(4)
Sample	Rural	Urban	Rural 10+	Urban 10+
Full Treatment	0.0762***	0.0014	0.0847***	0.0115
( <i>age</i> < 7 when law passed)	(0.0215)	(0.0228)	(0.0247)	(0.0235)
	[0.029,0.135]***	[-0.051,0.072]	[0.031,0.157]***	[-0.044,0.083]
Partial Treatment	0.0397*	0.0024	0.0469*	0.0111
(11 > <i>age</i> > 6 when law passed)	(0.0231)	(0.0161)	(0.0240)	(0.0156)
	[-0.008,0.095]	[-0.029,0.051]	[-0.002,0.105]*	[-0.019,0.060]
<i>N</i>	13,580,416	2,049,970	6,575,708	1,003,960
Untreated Outcome Mean	57.0%	69.0%	64.5%	74.7%

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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 in households where every member was born in the United States. 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.10: Staggered Adoption Difference-in-Differences - Heterogeneity by Observed Age

	(1)	(2)
	Full Treatment	Partial Treatment
Observed at Age = 5	0.0748*** (0.0178)	
Observed at Age = 6	0.0655*** (0.0160)	
Observed at Age = 7	0.0559** (0.0203)	
Observed at Age = 8	0.0664*** (0.0233)	0.0378 (0.0280)
Observed at Age = 9	0.0690*** (0.0248)	0.0110 (0.0360)
Observed at Age = 10	0.0785*** (0.0254)	0.0406 (0.0295)
Observed at Age = 11	0.0810*** (0.0242)	0.0392 (0.0258)
Observed at Age = 12	0.0788*** (0.0252)	0.0408 (0.0261)
Observed at Age = 13	0.0769*** (0.0233)	0.0492** (0.0227)
Observed at Age = 14	0.0796*** (0.0212)	0.0433** (0.0189)
<i>N</i>	17,610,766	
Untreated Outcome Mean	57.0%	

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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 estimated coefficients on full treatment for each specific age. Column 2 shows the estimated coefficients on partial treatment. 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.



Figure A.1: Bacon Decomposition

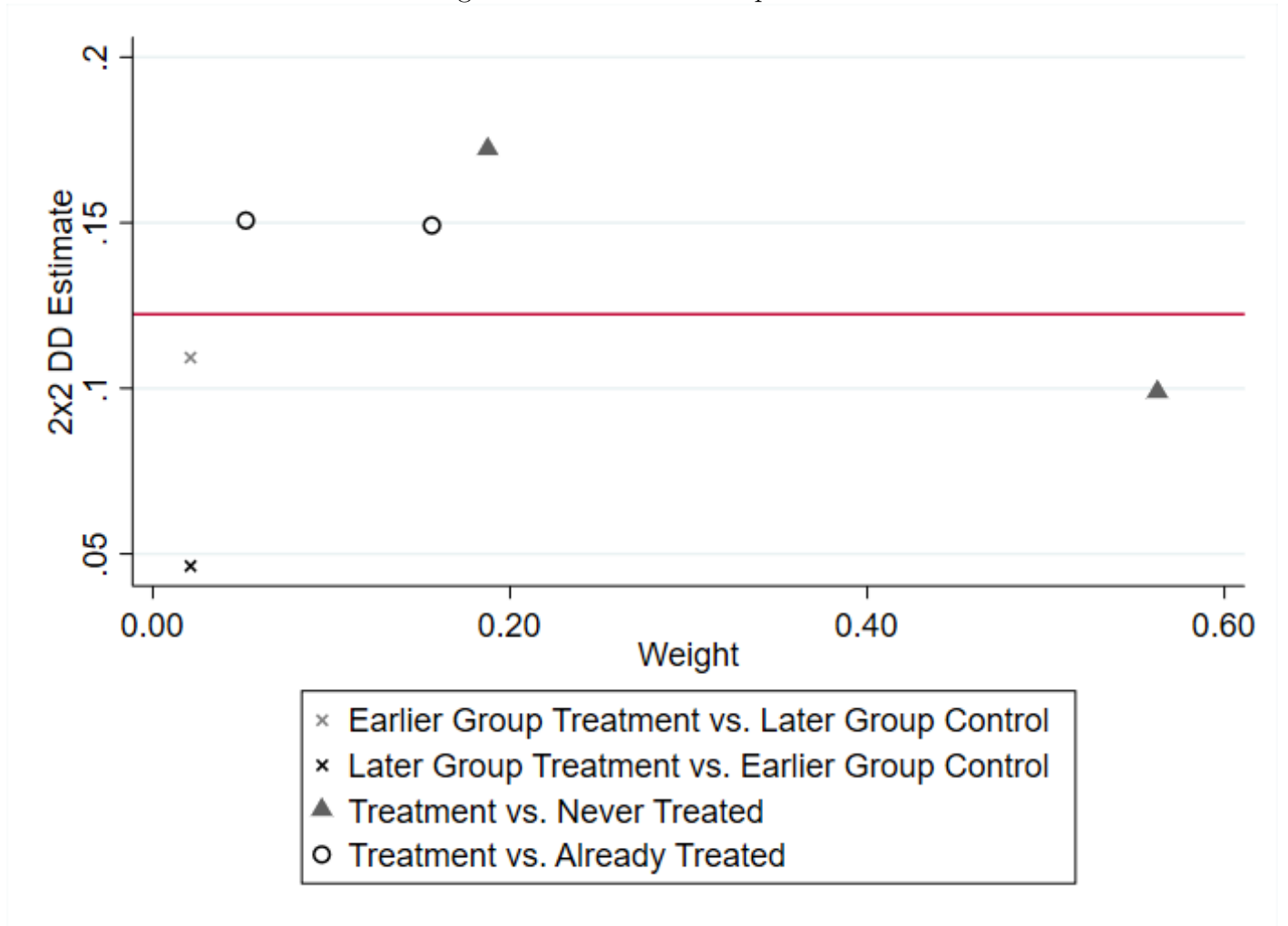


Figure A.1 shows the estimates and weights for each relevant two-group two-period difference-in-differences estimator in the staggered adoption specification used, calculated using a Bacon Decomposition (Goodman-Bacon, 2021). Each of the estimated coefficients is positive, with the smallest around 0.05 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 of Main Results and Robustness Checks

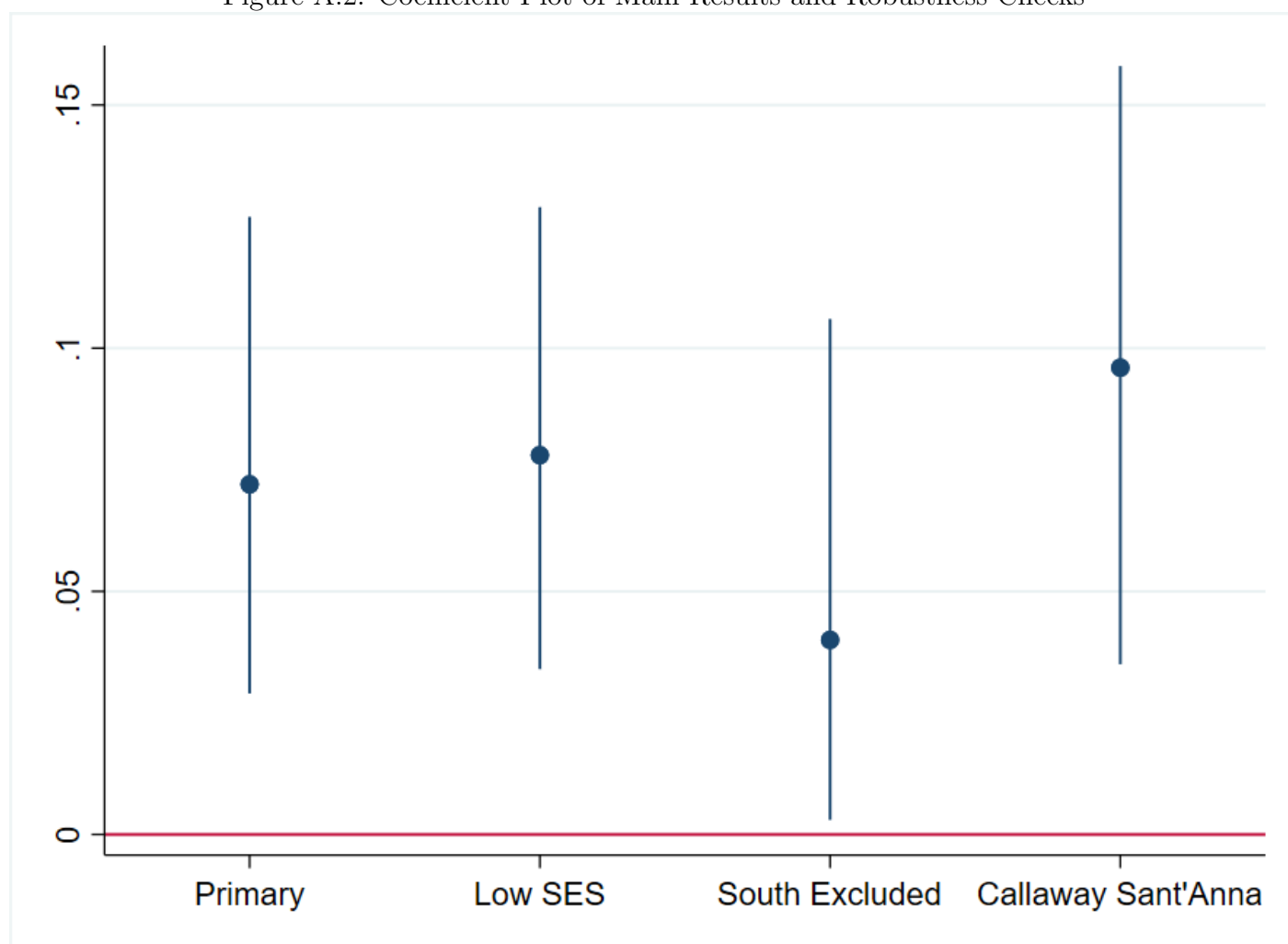


Figure A.2 shows the estimated coefficients from various specifications and samples. The first column shows the estimated coefficient from the primary specification. The second coefficient shows the estimated coefficient using only low socio-economic status individuals in rural areas. The third column 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 \(2021\)](#). For the first three estimates shown, bootstrapped confidence intervals are shown.

Figure A.3: Randomization Test Histogram

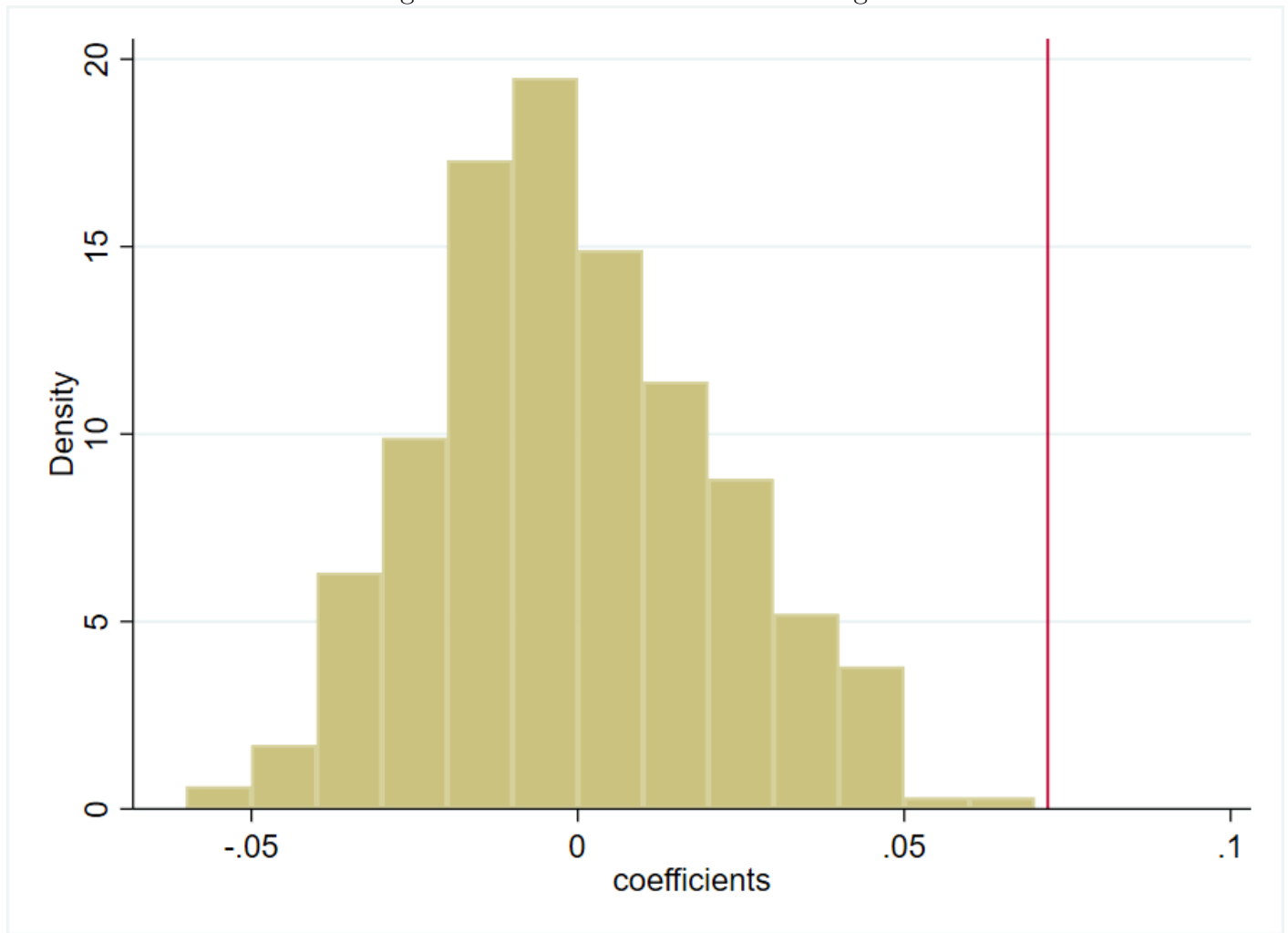


Figure A.3 shows the estimated coefficients from 1000 replications of the primary specification with randomized treatment timing. For each replication, the year of treatment for each state is drawn with replacement from the true distribution of years. The red vertical line illustrates the results from the primary specification with true treatment assignment, highlighting the statistical significance via this methodology; the estimated coefficient is greater for the true years of rate bill abolition than any of the 1000 redrawn replications.

## Appendix B Triple Difference-In-Differences

As an alternative to the primary specification, I use a triple difference-in-differences specification to explicitly exploit the variation between groups with supposedly different levels of pre-abolition exposure to rate bills. I consider three different dimensions for this third level of variation: urbanicity, family socioeconomic status as measured by occupation score, and professionalism of the parental occupation(s). This robustness check has various advantages and disadvantages relative to Equation 1 in Section 4.3.

While the added level of variation allows for a direct comparison between children that presumably were and were not deterred by rate bills, this alternative relies on the additional assumption that rate bill abolition had no effect at all on the control group considered. This assumption is more realistic in some cases than others, as is discussed in greater detail below. Despite the additional assumption, results are qualitatively similar to those presented in Table 3, Table 4, and Table A.8.

### B.1 Urbanicity

The first comparison considered here exploits the rural-urban split between exposure to rate bills prior to their abolition at the state level; many cities had already repealed rate bills prior to state-level abolition and are thus considered a control in this triple difference-in-differences specification. I estimate the following equation:

$$Y_{aist} = \alpha + \beta_1 Treatment_{ast} + \beta_2 PartialTreatment_{ast} + \beta_3 Rural_{aist} + \beta_4 Rural_{aist} \times Treatment_{ast} + \beta_5 Rural_{aist} \times PartialTreatment_{ast} + \gamma_{as} + \delta_{at} + \varepsilon_{aist} \quad (4)$$

where  $Y_{aist}$  is the attendance status of an individual  $i$  observed at age  $a$  in state  $s$  and Census year  $t$ ,  $\gamma_{as}$  is a state-by-age fixed effect,  $\delta_{at}$  is a Census year-by-age fixed effect, and  $\varepsilon_{aist}$  is the error term. Errors are clustered at the state level. The coefficient  $\beta_3$  captures rural-urban differences in attendance, and the coefficients  $\beta_1$  and  $\beta_2$  capture changes in attendance among urban individuals in response to rate bill abolition. The coefficients of interest  $\beta_4$  and  $\beta_5$  show the changes in attendance among rural individuals, relative to urban individuals, in response to rate bill abolition. Although the prevalence of free schools was documented in many cities, this specification explicitly assumes that the true effect of rate bill abolition should be equal to zero in urban areas. This is a stronger assumption than is required

in the main specification, in which the sample is restricted to only rural individuals.

Column 1 of Table B.1 shows the results of Equation 4 estimated on the full sample. The coefficient of interest  $\beta_4$  is positive and statistically significant, indicating that rate bill abolition led to large increases in rural areas relative to urban areas. This is presumably due to the fact that cities had repealed rate bills and provided free public schools prior to state-level legislation. These results are qualitatively similar to those presented in Table 3.

## B.2 Socio-Economic Status

The second comparison considered here exploits differences in socio-economic status. This specification relies on the assumption that high-SES individuals were unaffected by rate bills, as the low pecuniary costs should not have inhibited their attendance. I estimate the following equation:

$$\begin{aligned}
Y_{aist} = & \alpha + \beta_1 Treatment_{ast} + \beta_2 PartialTreatment_{ast} + \beta_3 LowSES_{aist} \\
& + \beta_4 LowSES_{aist} \times Treatment_{ast} + \beta_5 LowSES_{aist} \times PartialTreatment_{ast} \\
& + \gamma_{as} + \delta_{at} + \varepsilon_{aist}
\end{aligned} \tag{5}$$

where  $Y_{aist}$  is the attendance status of an individual  $i$  observed at age  $a$  in state  $s$  and Census year  $t$ ,  $\gamma_{as}$  is a state-by-age fixed effect,  $\delta_{at}$  is a Census year-by-age fixed effect,  $LowSES_{aist}$  is an indicator variable equal to one if the maximum occupation score of the household is less than 19.5,<sup>36</sup> and  $\varepsilon_{aist}$  is the error term. Errors are clustered at the state level. The coefficient  $\beta_3$  captures differences in attendance between low socio-economic status households and high socio-economic status households, and the coefficients  $\beta_1$  and  $\beta_2$  capture changes in attendance among high-SES individuals in response to rate bill abolition. The coefficients of interest  $\beta_4$  and  $\beta_5$  show the changes in attendance among low-SES individuals, relative to high-SES individuals, in response to rate bill abolition.

Column 2 of Table B.1 shows the results of Equation 5 estimated on both rural and urban individuals, whereas Column 3 of Table B.1 shows the results of Equation 5 estimated on only rural individuals. In both cases, the coefficient of interest  $\beta_4$  is positive and statistically significant, indicating

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<sup>36</sup>This is the same definition used earlier in the paper. Approximately 60% of rural individuals are considered low-SES by this metric.

that rate bill abolition led to large increases in attendance among low socio-economic status individuals relative to high socio-economic status individuals. This is presumably due to the fact that low socio-economic status households were more likely inhibited by the small pecuniary costs to attend primary school. These results are quantitatively similar to those presented in Table 4, which shows that rate bill abolition had positive effects on attendance in rural areas regardless of socio-economic status but that these results were stronger for low-SES individuals.

### B.3 Professionalism of Occupation

The third comparison considered here exploits differences in professionalism of occupations in the household. This specification relies on the assumption that children in households with professional occupations were unaffected by rate bills, as these households likely emphasized education and such low pecuniary costs should not have inhibited their attendance. I estimate the following equation:

$$\begin{aligned}
Y_{aist} = & \alpha + \beta_1 Treatment_{ast} + \beta_2 PartialTreatment_{ast} + \beta_3 NotProfessional_{aist} \\
& + \beta_4 NotProfessional_{aist} \times Treatment_{ast} \\
& + \beta_5 NotProfessional_{aist} \times PartialTreatment_{ast} + \gamma_{as} + \delta_{at} + \varepsilon_{aist}
\end{aligned} \tag{6}$$

where  $Y_{aist}$  is the attendance status of an individual  $i$  observed at age  $a$  in state  $s$  and Census year  $t$ ,  $\gamma_{as}$  is a state-by-age fixed effect,  $\delta_{at}$  is a Census year-by-age fixed effect,  $NotProfessional_{aist}$  is an indicator variable equal to one if no member of the household holds a professional occupation,<sup>37</sup> and  $\varepsilon_{aist}$  is the error term. Errors are clustered at the state level. The coefficient  $\beta_3$  captures differences in attendance between laborer and professional households, and the coefficients  $\beta_1$  and  $\beta_2$  capture changes in attendance among professional individuals in response to rate bill abolition. The coefficients of interest  $\beta_4$  and  $\beta_5$  show the changes in attendance among laborer individuals, relative to professional individuals, in response to rate bill abolition.

Column 4 of Table B.1 shows the results of Equation 6 estimated on both rural and urban individuals, whereas Column 5 of Table B.1 shows the results of Equation 6 estimated on only rural individuals.. The coefficient of interest  $\beta_4$  is positive and statistically significant in both cases, indicating that rate bill abolition led to large increases in attendance among children in laborer households relative

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<sup>37</sup>The classification of “professional” occupations is available upon request.

to children in professional households. This is presumably due to the fact that non-professional families were more likely inhibited by the small pecuniary costs to attend primary school, as well as the fact that they may have emphasized education less than professional households. These results are quantitatively similar to those presented in Table A.8.

Table B.1: Triple Difference-in-Differences

	(1)	(2)	(3)	(4)	(5)
	Urbanicity	SES	SES	Professional	Professional
Full Treatment	0.0201	0.0364*	0.0555***	-0.0010	0.0070
( $age < 7$ w.l.p.)	(0.0144)	(0.0191)	(0.0190)	(0.0122)	(0.0167)
Partial Treatment	0.0266	0.0301	0.0382*	0.0051	0.0049
( $11 > age > 6$ w.l.p.)	(0.0175)	(0.0189)	(0.0210)	(0.0147)	(0.0186)
Rural	-0.0298				
	(0.0268)				
Low SES		-0.0319**	-0.0233*		
		(0.0149)	(0.0121)		
Non Professional				-0.0898***	-0.1070***
				(0.0300)	(0.0263)
Full Treatment	0.0452**				
X Rural	(0.0188)				
	[0.012,0.107]**				
Partial Treatment	0.0012				
X Rural	(0.0187)				
	[-0.042,0.065]				
Full Treatment		0.0361***	0.0247**		
X Low SES		(0.0108)	(0.0105)		
		[0.014,0.062]***	[0.003,0.050]**		
Partial Treatment		0.0001	-0.0009		
X Low SES		(0.0126)	(0.0111)		
		[-0.031,0.032]	[-0.029,0.021]		
Full Treatment				0.0669***	0.0714***
X Non Professional				(0.0239)	(0.0229)
				[0.018,0.137]***	[0.018,0.134]***
Partial Treatment				0.0286	0.0350*
X Non Professional				(0.0177)	(0.0172)
				[-0.003,0.086]*	[-0.001,0.082]*
$N$	23,314,912	23,314,912	17,610,766	23,314,912	17,610,766
Untreated Outcome Mean	58.3%	56.9%	56.4%	58.5%	57.2%

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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 4 estimated on individuals between the ages of 5 and 14. Column 2 shows the results from Equation 5 estimated on the same sample. Column 3 shows the results from Equation 5 estimated on only rural individuals. Column 4 shows the results from Equation 6 estimated on all individuals between the ages of 5 and 14. Column 5 shows the results from Equation 6 estimated on only rural individuals. 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.



## Appendix C Controls for Availability of Education

The availability of educational services was also increasing over this time period. This appendix attempts to take these changes into account by including controls for various factors that make up the “supply” of education: teachers, schools, and public funding. In addition to the data described in the main body of the paper, this section utilizes state-level statistics on the number of schools, number of teachers, and total public funding for schools (De Bow, 1853; Edmunds, 1866; Walker, 1872; Walker and Seaton, 1883).

There are three primary limitations with including these controls. First, the data included for schools, teachers, and funding are collected at the state level. Therefore, it is not possible to focus specifically on the provision of education in rural areas. Given that cities were growing faster than rural areas during the second half of the 19th century, both due to internal rural-urban migration and immigration from other countries, it may have been the case that observed increases in schools, teachers, and funding disproportionately favored urban areas.

In a similar vein, only statistics on public provision of education are included, as data on private schools and academies are incomplete over this time period. If the number of private schools is changing at a different rate than the number of public schools, then including controls for only public schools paints an incomplete picture of the actual availability of education. This is especially problematic given that the treatment was associated with changes in demand for private schools: “[rate bill] abolition was accompanied generally by a decreased attendance at private schools” (Cubberley, 1919).

Third, the inclusion of these controls on the right hand side implies that aggregate school attendance rates have no impact on the public provision of schools and teachers. This is a strong assumption; it seems more likely that teachers, schools, and funding per student are equilibrium objects that take into account the fraction of students that wish to attend public school. It is therefore possible for some degree of reverse causality, in which case the statistics included on the right hand side of Equation 7 may in fact be bad controls.

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

$$Y_{aist} = \alpha + \beta_1 Treatment_{ast} + \beta_2 PartialTreatment_{ast} + \psi \mathbb{X}_{st} + \gamma_{as} + \delta_{at} + \varepsilon_{aist} \quad (7)$$

where  $Y_{aist}$  is the attendance status of an individual  $i$  observed at age  $a$  in state  $s$  and Census year  $t$ ,  $\gamma_{as}$  is a state-by-age fixed effect,  $\delta_{at}$  is a Census year-by-age fixed effect, and  $\varepsilon_{aist}$  is the error term.  $\mathbb{X}_{st}$  is a series of control polynomials for the three factors that describe the “supply” of education.

Table C.1 shows the results from Equation 1 and Equation 7. Column 1 replicates the primary specification on all 27 states. Column 2 shows the results of the primary specification using only the 24 states for which data is available in all four Census years; the estimated coefficient is smaller but remains statistically significant at the 1% level. Columns 3 through 5 show the estimated coefficients of treatment from Equation 7, including linear, quadratic, and cubic controls for each of the three measures described divided by the school-age population in the Census. In each case, the estimated coefficient is statistically significant at the 10% level via the  $t$ -distribution with 23 degrees of freedom.

Despite the limitations of this alternative specification, the results are similar to those presented in the main body of the paper. The estimated coefficients for treatment are smaller and less statistically significant than those in Table 3. This is partially driven by a difference in sample selection; the exclusion of three states both reduces the estimated coefficient before additional controls are included and reduces the degrees of freedom used to calculate critical values for inference. Therefore, the primary specification presented in the main body of the paper is preferred to this alternative.

Table C.1: Staggered Adoption Difference-in-Differences - Availability Controls

	(1)	(2)	(3)	(4)	(5)
Sample	Full Sample	Limited	Limited	Limited	Limited
Full Treatment	0.0720***	0.0550***	0.0313*	0.0319*	0.0323*
( <i>age</i> < 7	(0.0202)	(0.0163)	(0.0154)	(0.0172)	(0.0169)
when law passed)	[0.029,0.128]***	[0.019,0.102]***	[-0.004,0.075]*	[-0.010,0.079]	[-0.007,0.085]
Partial Treatment	0.0367*	0.0199	0.0073	0.0052	0.0114
(11 > <i>age</i> > 6	(0.0209)	(0.0176)	(0.0145)	(0.0136)	(0.0120)
when law passed)	[-0.006,0.087]*	[-0.017,0.063]	[-0.027,0.044]	[-0.027,0.042]	[-0.017,0.044]
Linear Controls			X	X	X
Quadratic Controls				X	X
Cubic Controls					X
<i>N</i>	17,610,766	16,445,344	16,445,344	16,445,344	16,445,344
Untreated Y Mean	58.3%	62.1%	62.1%	62.1%	62.1%

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Standard errors clustered at the state level in parentheses. Critical values are obtained from the  $t$ -distribution with 26 degrees of freedom for Column 1 and 23 degrees of freedom for Columns 2 through 5. Column 1 shows the results from Equation 1 estimated on rural individuals between the ages of 5 and 14 in all 27 states. Column 2 shows the results from Equation 1 estimated on rural individuals between the ages of 5 and 14 in 24 states, excluding Utah, Virginia, and West Virginia. Column 3 shows the results from Equation 1 estimated on the same population with linear controls for teachers per population, schools per population, and funding per population. Column 4 shows the same using linear and quadratic controls. Column 5 shows the same using linear, quadratic, and cubic controls. All regressions include state-by-age fixed effects and Census year-by-age fixed effects, as well as a constant term.