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

Richard Uhrig†

Click here for the latest version of my Job Market Paper

October 31, 2021

Abstract

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 Urban school districts gradually repealed these fees and funded public education through local taxes.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

Keywords: free public education, rate bills, primary school attendance

JEL Codes: N31, I22, H75

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

†Department of Economics, University of California Santa Barbara, 2127 North Hall, Santa Barbara, CA. Email: rauhrig@ucsb.edu

1 Introduction

Between the late 19th and early 20th centuries, the United States became one of the most educated societies in the world. 89.6% of children aged 5-14 were enrolled in public schools by 1910, compared to only 54.6% enrolled in public schools in 1830 (Lindert, 2004). Over this time period, public schools became free to attend through the abolition of rate bills, or tuition payments, shifting the burden of education funding from students and their families to society. Eventually, primary school attendance became mandatory through compulsory schooling laws (or CSL's). This paper analyzes the impact of state-level rate bill abolition laws, which prevented public schools from charging tuition, on attendance in rural areas. Although these 19th century rate bills were small relative to income, I find that their abolition led to increased attendance, implying that even low levels of tuition can hinder the diffusion of education.

There is limited evidence on the effect of rate bills and compulsory schooling laws on attendance in the United States. The 1826 imposition of a rate bill between 25 cents and 2 dollars per class in New York City led to a 13% drop in attendance (Cubberley, 1919). For those more advanced courses and higher associated tuition, the drop in attendance was above 90%. Go (2015) studies the effect of rate bill abolition in Connecticut and finds that larger initial rate bills, and therefore larger decreases at abolition, led to larger increases in attendance when the state banned tuition requirements in 1868. On the other hand, Landes and Solmon (1972) find that early versions of CSL's did not increase enrollment or attendance. More recent studies find that CSL's in the early 20th century likely had positive but statistically insignificant effects on educational attainment (Acemoglu and Angrist, 2000; Lochner and Moretti, 2004; Lleras-Muney, 2005; Black, Devereux and Salvanes, 2008).[1] While previous research has focused on general attendance trends, compulsory schooling laws, and single-state rate-bill analyses, this is the first paper to study the effects of broad state-level rate bill abolition policies on attendance in a wide range of states.

Using data from the United States Decennial Censuses of 1850 through 1880, I investigate the effect of state-level rate bill abolition laws on attendance in rural areas using a staggered adoption difference-in-differences framework. These policies were effectively imposed upon rural areas, via state legislatures or referenda, by urban areas, which had already repealed rate bills prior to these state-level bans. I find that

[1] As Black, Devereux and Salvanes (2008) points out, serial correlation in the treatment variable (in this case, the level of compulsory schooling) implies that standard errors should be clustered at the state level rather than the state-year level, which washes away much of the statistical significance in the literature.

the abolition of rate bills increased rural attendance by 7.2 percentage points for individuals below the age of seven when the laws were passed. These results imply an increased average educational attainment of 0.54 years,[2] approximately equal to the effect of compulsory schooling laws estimated in Black, Devereux and Salvanes (2008)[3] and approximately 16% of the increased educational attainment driven by the expansion and diffusion of the American high school in the early 1900's (Goldin, 1998). The effects on young children persist as they age, indicating that the abolition of rate bills increased attendance for children in late primary school and thus provided the foundation for the "high-school revolution" documented by Goldin (1998). These results suggest that the presence of tuition for post-secondary schools in the United States and primary and secondary schools in some developing countries, even at low levels, may depress attendance and educational attainment.

Increased attendance in response to rate bill abolition is somewhat surprising for a few reasons. Rate bills were not expensive, even for the time period. In New York state, average tuition for the full 18-week academic year in 1841-1842 was only 0.3% the annual wages of a non-farm worker (Go and Lindert, 2010). While the marginal student's family presumably earned less than the average non-farm wage, this statistic demonstrates the small magnitude of rate bills at this time, and one might expect that such small charges would not be an obstacle to school attendance. Additionally, tuition and user fees were not the only costs associated with primary schools at this time. Child labor laws were extremely rare in the 19th century, and some children worked in both rural and urban areas.[4] Therefore, non-attendance may be driven by the opportunity cost of children working rather than the pecuniary cost of rate bills. The elimination of rate bills only altered one aspect of the decision for marginal school attendees but still led to increased attendance. Lastly, public schools charging rate bills were not the only options for children seeking primary education. Some states and municipalities also provided pauper schools free of charge to those individuals on lower rungs of the socioeconomic ladder, which were most likely to be deterred by rate bills in public schools. For the wealthy, parents may choose to send children to private schools. In fact, "[rate bill] abolition was accompanied generally by a decreased attendance at private schools" (Cubberley,

[2]This number is calculated based on a 7.2 percentage point increase in attendance over ten years for approximately 75% of the population.

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

[4]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.

1919), implying some level of substitution between these two alternatives.[5] Other primary school options, especially pauper schools, meant that rate bills only applied to a subset of the overall population. However, these charges were sufficient to reduce attendance among marginal students that could not attend pauper schools or afford private schools. Thus, the striking result here is that even small fees can be large barriers to school attendance.

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 discusses 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 concludes.

2 Background

Rate bills were first implemented by school districts as education systems expanded in the 18th and 19th centuries as an alternative to increased government funding, thus sharing the cost of public school provision between the the local population writ large and the specific families of students utilizing the service.[6] The magnitude of rate bills varied across states and school districts but was generally small relative to incomes. According to Go and Lindert (2010), rate bills comprised over 75% of public school funding in the state of New York in 1825. In the case of New York City, for which Cubberley (1919) provides the most detail, rate bills were charged based on classes; more advanced courses like astronomy and bookkeeping required much higher rate bills than elementary classes on alphabet and spelling. The largest of these fees was $2, or the equivalent of approximately $52 in 2019. Go and Lindert (2010) estimates that tuition for 18 weeks of schooling in 1841-1842 was approximately 0.3% of annual wages for the average non-farm worker in New York state.[7] In that same school year, rate bills accounted for 41%

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

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

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

of school revenues, with the rest coming from public funding.

Over time, the general population thought of public provision of education as more important to society. As these attitudes shifted, school districts reduced rate bill requirements to better promote school attendance. By 1850, rate bills provided only around 35% of public school funding in New York state, a significant decrease relative to 1825. Tuition and fees were primarily replaced by local taxes and state subsidies for public schools, shifting the cost from those families utilizing public education to all of society. This change towards fully-funded schools rather than partial tuition began primarily in cities.[8] Urban areas typically repealed rate bills of their own volition in an attempt to increase school attendance and educational attainment, just as New York City had done in the 1830's. Rural areas, on the other hand, continued to charge tuition and vote against rate bill abolition until state legislatures and public referenda forced their hand.

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

[8] At least eleven cities in New York provided free schools before the state abolished rate bills in 1868: New York City, Buffalo, Hudson, Rochester, Brooklyn, Williamsburg, Syracuse, Troy, Auburn, Oswego, and Utica. Other major cities such as Baltimore, Charleston, Mobile, New Orleans, Louisville, Cincinnati, Chicago, and Detroit also provided free schools at least 25 years prior to state-level rate bill abolition (Cubberley, 1919).

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

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

3 Data

Information on rate bill abolition laws is collected from Go (2009), which draws on many different sources, including Cubberley (1919), Goldin and Katz (2008), Swift (1911), and Mead (1918). Table 1 shows the dates of passage for rate bill abolition as well as first instances of state-level compulsory schooling laws, which are drawn from Goldin and Katz (2008). Figure 1 displays a map of the United States, highlighting those states used in various specifications. For the primary analysis, I will include all 27 states for which rate bill abolition dates are available. I later restrict my sample of states to those 18 states, shown in light- and medium blue, in the American North and West to focus primarily on states that were less affected by the Civil War. Lastly, I restrict my sample to those 15 states for whom information on compulsory schooling is available, shown in light blue.

The American Civil War (1861-1865) occurred during the time period observed here, and the destruction of existing infrastructure in the American South likely had long-run effects on school attendance. This may problematic because many southern states abolished rate bills between the 1860 and 1870 Censuses, the same decade as the Civil War.[11] On the other hand, the American Civil War should not affect observed attendance in northern states because it was not ongoing during any Census year. While attendance everywhere may have changed during the war, only Confederate states experienced severe destruction that would have had long-lasting effects on school attendance. My results in the primary specification are robust to the exclusion of the American South, as is described in Section 5.2 and Table 5.

Due to lack of data on compulsory schooling in some states, I do not control for CSL's in the primary specification. This choice allows me to maintain 27 clusters rather than limiting the sample to 15 states. However, I check robustness on the restricted sample to account for their passage using two separate methods, which are detailed in Section 5.3. Results are presented in Table 6 and similar to the primary specification.[12]

[11] It is possible that the abolition of rate bills was imposed on some southern states as a condition for readmission into the United States, rather than enacted by their populations and/or legislatures. This would explain why so many southern states abolished rate bills at similar times. This would also reinforce the assumption that rate bill abolition was imposed upon relevant populations.

[12] Only one state (Vermont) passed a compulsory schooling law prior to 1870. Eight more states passed CSL's in the decade between 1870 and 1880, but Landes and Solmon (1972) finds that these early versions of CSL's did not significantly affect enrollment or attendance. My results are robust to multiple methods of accounting for compulsory schooling laws.

I utilize the 1850-1880 Full Count Censuses from the Integrated Public Use Microdata Series (Ruggles et al., 2019). Key variables in the data include information on an individuals attendance status, age, race, gender, various measures of socio-economic status, urbanicity,[13] and state of birth. I restrict my dataset to native-born whites between the ages of 5 and 14 in rural areas.[14] Table 2 shows relevant summary statistics for each of the four groups of states in my dataset: those that abolish rate bills prior to 1850, between 1850 and 1860, between 1860 and 1870, and between 1870 and 1880.

In each Census year utilized here, the Census asked some variation of "Was the person at school within the last year?" for every person in the household.[15] Figure 2 plots the attendance rates by age for individuals between the ages of 5 and 18, broken down across dimensions of gender and urbanicity. Men and women attended school at similar rates, regardless of where they lived. Attendance in urban areas was higher than rural areas, likely due to children working rather than attending school (Cubberley, 1919).[16] In any case, rural children typically lagged behind urban ones in primary school attendance, possibly driven by the presence of rate bills, child labor, and/or differences in returns to education.[17]

4 Empirical Strategy

4.1 Identification

In order for the effect for rate bill abolition to be identified, I assume that the legislation and referenda that brought about these changes were not accompanied by other education reform policies. It is necessary that the only aspect changing with treatment is the existence of rate bills, rather than the availability of

[13] Individuals are considered to be rural if there are fewer than 2,500 people in the municipality.

[14] Children of other races or nationalities may have been subject to discrimination that prevented attendance regardless of any user fees charged. I choose ages 5 through 14 to focus on primary-school-age children.

[15] 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.

[16] I attribute the long tails on rural attendance to the structure of these Census questions. If rural individuals were to attend school for only part of the year, then they would continue to show up as attending. This is in contrast to urban children that might attend school full-time for fewer years before matriculating.

[17] 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.

education or the returns to education.[18] If this assumption were violated, then one would expect such legislation to affect attendance in urban areas as well as rural areas. Therefore, I use urban areas as a placebo test to check whether or not rate bill abolition laws led to changes in attendance in areas where rate bills had already been repealed. While I find strong, positive, statistically significant effects of rate bill abolition on attendance in rural areas, I estimate a null effect in urban areas. This provides some evidence reinforcing the assumption about other attendance-promoting policies; such policies would presumably affect urban areas as well, whereas rate bill abolition would only affect those places that had charged rate bills immediately prior to state-level abolition.

I also assume that individuals above the age of ten when these policies are implemented are unaffected by rate bill abolition. Presumably, the educational trajectory of an older individual is unlikely to be affected by minor changes in pecuniary costs, so the abolition of rate bills for the remaining years does not affect their attendance status going forward. This is consistent with evidence that attendance did not increase for children at older age groups unless these policies were implemented when they themselves were younger, shown in Table A.1 and A.2. If this assumption is violated, estimated coefficients could therefore be interpreted not as the true effect of rate bill abolition but instead as the effect of rate bill abolition on young children less the effect of rate bill abolition on older children, thus attenuating the results towards zero. My results are robust to various upper bounds on ages that would be affected by these policies.

Similarly, I assume that rural areas continued to charge rate bills until state-level policies abolished their use. Given the well-documented repeal of rate bills in urban areas and no corresponding evidence in rural areas, as well as the voting patterns in rural areas against rate bill abolition, I believe this to approximate the truth. If this assumption were violated, then estimated coefficients understate the true importance of rate bills as a barrier to attendance, since abolition would not be binding for the entire group and instead only affect that subset of rural areas that were forced to repeal rate bills by state-level policies.

[18]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.

4.2 Treatment

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

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

Over the four Decennial Censuses used here, most individuals fall into either the "fully-treated" or "fully-untreated" categories. 55.5% of students in my sample are fully treated, and 37.4% of individuals are are fully untreated. Therefore, this analysis is primarily a comparison between individuals on either extreme that either started school after these rate bill abolition policies took effect or attended school at a time such that they did not especially benefit from the abolition of tuition payments. It is therefore unsurprising that results are not sensitive to alternative age cutoffs for full and partial treatment, as the fully-treated and untreated groups remain the vast majority of the data.

4.3 Estimating Equation

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

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

where the outcome variable Y_{aist} is the attendance status of an individual i observed at age a in state s and Census year t, γ_{as} is a state-by-age fixed effect, δ_{at} is a year-by-age fixed effect, and ε_{aist} is the error term. Year-by-age fixed effects improve upon the combination of age- and year-fixed effects by allowing attendance differences between ages to vary across the four Census years considered here.[19] Similarly, state-by-age fixed effects improve upon state fixed effects because they account for differences in attendance rates by age for each state, rather than simply accounting for the overall attendance level with no regard for how attendance differs by age.[20]

Errors are clustered at the state level (thus allowing for correlation in the error terms between individuals in the same state across all Census years) due to serial correlation in the treatment variable, as is prescribed by Bertrand, Duflo and Mullainathan (2004). Therefore, I obtain my critical values from the t-distribution with $G-1=26$ degrees of freedom rather than from the normal distribution to account for the low number of clusters. Additionally, I utilize the wild bootstrap with Rademacher weights as prescribed in Cameron, Gelbach and Miller (2008) to construct a 95% confidence interval for the effect of state-level rate bill abolition on education.[21]

5 Findings

5.1 Main Results

Column 1 of Table 3 shows the results of my primary specification. I find that rural children at younger ages when rate bill abolition laws come into effect are 7.2 percentage points more likely to attend school than the control group. The estimated coefficient is statistically significant at the 1% level when critical values are taken from the t-distribution with $G-1=26$ degrees of freedom to account for the low number of clusters. Confidence intervals from the wild bootstrap are shown below the standard errors in Table 3. The estimated coefficient remains significant at the 5% level using this more conservative methodology.

Column 3 of Table 3 shows the results of my primary specification including only children at least ten

[19] My findings are robust to including age- and Census-fixed effects instead of birth-year fixed effects.

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

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

years old when they are observed in the Census. The coefficient is qualitatively similar to that estimated on the full primary-school-age population and statistically significant using both critical values from the t-distribution with $G-1=26$ degrees of freedom and the wild bootstrap. This indicates that positive effects on attendance persist for children later in primary school, as opposed to being concentrated in younger children.[22] As was previously mentioned, primary schooling is effectively a prerequisite to secondary school. Therefore, by increasing school attendance for children between the ages of 10 and 14 that were closest to secondary school, this policy provided the foundation for the "high-school revolution" in the early 20th century, which has been documented by Goldin (1998).

Similarly, Column 1 of Table 4 shows the results of my primary specification including only children of lower socio-economic status.[23] These results are qualitatively similar to those presented in Table 3. This is unsurprising given that a large fraction of the school-age population falls below the threshold for low socio-economic status set here. The similarity in results also indicates that these policies really were effective at inducing marginal students to attend primary school; one would have expected students from high-SES families to receive significant education regardless of the presence of rate bills/

The results presented here suggest that small rate bills were a big obstacle to primary school attendance. A 7.2 percentage point increase in attendance for rural children between the ages of 5 and 14 implies an average increase in educational attainment by 0.54 years for the native white population.[24] This figure can be considered a lower bound because of the underlying assumptions discussed in Section 4.1. Specifically, the classification of treatment as affecting all rural individuals means that some individuals considered treated in this analysis are unaffected by these policies. Therefore, the inclusion of these observations as treated would attenuate the estimated effect back towards zero. Additionally, this estimate on educational

[22]Note that many younger children were already attending schools prior to these policies, as is shown in Figure 2. If one is to think of the marginal student that drops out of school to work, this would be more likely for students at least ten years old than those younger students, especially if the older individuals had already attained some level of education prior to entering the labor force.

[23]Low socio-economic status is defined as the maximum occupation score in the household under 40. Occupation score is calculated as the median income, in hundreds of 1950 US dollars, for a given occupation. For reference, a farmer has an occupation score of 14, below the threshold for low socio-economic status as I have defined it. Note that the vast majority of children are classified as low socio-economic status in this datatset; the median value is 16 and 88% have an occupation score below 40.

[24]This number is calculated based on a 7.2 percentage point increase in attendance over ten years for approximately 75% of the population.

attainment relies on the assumption that increased primary school attendance did not induce more students to receive secondary and post-secondary education. However, given that primary schooling is effectively a prerequisite to attending secondary school, it is possible that promoting primary school attendance through rate bill abolition also lead to increased secondary school attendance.

Column 2 of Table 3 shows the results of Equation 1 estimated on urban areas, which provide a useful placebo test. Note that rate bills were typically repealed in urban areas by the time these state-level abolition policies were passed, so state-level rate bill abolition should not have any impact on urban attendance. Estimated coefficients for urban areas are close to zero and statistically insignificant, in stark contrast to that in rural areas.[25] These coefficients remain statistically insignificant when Equation 1 is estimated only on urban children above the age of nine and urban children of lower socio-economic-status.[26]

5.2 The American South

The American Civil War took place from 1861 through 1865. While no Decennial Censuses were collected while the conflict was ongoing, it is possible that school attendance in southern states was affected in the aftermath of the American Civil War. Many such states passed rate bill abolition laws during the same decade as the conflict, further complicating the issue. Therefore, I replicate the estimation of Equation 1 for the 18 non-southern states. Results are presented in Table 4. The exclusion of nine states does not qualitatively affect the findings presented earlier. I still find large, statistically significant increases in rural attendance as a result of the rate bill abolition policies and no such increases in urban areas.

5.3 Compulsory Schooling Laws

Some states also passed early forms of compulsory schooling laws in the late 19th century, requiring students to attend school up to a certain age or for a certain number of years. Research from Landes and

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

[26] Surprisingly, the wild bootstrap would indicate that coefficients are statistically significant at the 5% level for urban children at least ten years old, as is shown in Column 4 of both Tables 3 and 4. However, in both cases, the confidence interval either barely includes the estimated coefficient or does not include it at all. Regardless, effects in urban areas are much closer to zero than those estimated in rural areas on the same population.

Solmon (1972) shows that these laws were not effective at increasing attendance. I do not have information on their passage for the entire sample of the states, so my main specification does not account for these laws.[27] To check the robustness of this result to the inclusion of compulsory schooling laws, I restrict my sample to those 15 states with CSL information, and estimate the following equation:

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

Results for this adjusted analysis are displayed in Column 3 of Table 5.

In addition, I restrict my sample to the same 15 states, truncate my data to only include the 1850 through 1870 Decennial Censuses, and again estimate Equation 1. Only one state had passed an CSL prior to 1870, so this truncated regression should better isolate the effect of rate bill abolition from compulsory schooling policies. Results for this adjusted analysis are displayed in Column 4 of Table 5. No matter how compulsory schooling laws are accounted for, the results are qualitatively similar to the estimation of Equation 1 on all four Census years, either on the entire sample of 27 states or the restricted sample of 15 states with CSL information.

5.4 Other Robustness Checks

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

[27] Including all 27 states allows me to maintain a higher number of clusters in my primary specification.

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

Results are presented in Table A.3. This specification is slightly more flexible than Equation 1 in that it allows for different treatment effects for those individuals that were already school-aged when these rate bill abolition policies came into effect and are therefore considered partially-treated. However, the disadvantage of this specification is that there are very few individuals in each partially-treated group, since so many individuals are either fully-treated or untreated. Therefore, the coefficients for each age of partial treatment are imprecisely estimated.

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

6 Conclusion

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

This analysis is subject to some limitations, primarily driven by the available data on this subject. I rely entirely on state-level policy changes rather than more granular rate bill information at the schools district level. Additionally, I am unable to control for inter- and intra-state mobility, especially that which may be caused by changes in rate bill policies over time. This paper also relies on attendance as it is marked in the 1850 through 1880 Censuses, which is an indicator variable rather than a measure of how much an individual has attended school both in the past year and throughout their childhood. Lastly, I am unable to comment on overall educational attainment other than back-of-the-envelope calculations, since the United States Census did not ask about completed educational attainment until 1940, well after rate bills were abolished throughout the country.

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

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

References

Acemoglu, Daron, and Joshua Angrist. 2000. "How large are human-capital externalities? Evidence from compulsory schooling laws." *NBER Macroeconomics Annual*, 15: 9–59.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How much should we trust differences-in-differences estimates?" *The Quarterly Journal of Economics*, 119(1): 249–275.

Black, Sandra E, Paul J Devereux, and Kjell G Salvanes. 2008. "Staying in the classroom and out of the maternity ward? The effect of compulsory schooling laws on teenage births." *The Economic Journal*, 118(530): 1025–1054.

Cameron, A Colin, Jonah B Gelbach, and Douglas L Miller. 2008. "Bootstrap-based improvements for inference with clustered errors." *The Review of Economics and Statistics*, 90(3): 414–427.

Carter, Andrew V, Kevin T Schnepel, and Douglas G Steigerwald. 2017. "Asymptotic behavior of at-test robust to cluster heterogeneity." *Review of Economics and Statistics*, 99(4): 698–709.

Cubberley, Ellwood Patterson. 1919. *Public education in the United States: A study and interpretation of American educational history; an introductory textbook dealing with the larger problems of present-day education in the light of their historical development.* Houghton Mifflin.

Denison, Edward F. 1985. "Trends in Economic Growth, 1929-1982." *Brookings, Washington DC*.

Goldin, Claudia. 1998. "America's graduation from high school: The evolution and spread of secondary schooling in the twentieth century." *The Journal of Economic History*, 58(2): 345–374.

Goldin, Claudia, and Lawrence Katz. 2008. "The race between technology and education." *Cambridge, MA: Harvard*.

Go, Sun. 2009. "Free Schools in America, 1850-1870: Who Voted for them, who got them, and who paid for them."

Go, Sun. 2015. "Free Elementary Schooling in Connecticut, 1866-1880." *Economic History*, 59(4): 49–75.

Go, Sun, and Peter Lindert. 2010. "The uneven rise of American public schools to 1850." *The Journal of Economic History*, 70(1): 1–26.

Landes, William M, and Lewis C Solmon. 1972. "Compulsory schooling legislation: An economic analysis of law and social change in the nineteenth century." *The Journal of Economic History*, 32(1): 54–91.

Lindert, Peter H. 2004. *Growing public: Volume 1, the story: Social spending and economic growth since the eighteenth century*. Vol. 1, Cambridge University Press.

Lleras-Muney, Adriana. 2005. "The relationship between education and adult mortality in the United

States." *The Review of Economic Studies*, 72(1): 189–221.

Lochner, Lance, and Enrico Moretti. 2004. "The effect of education on crime: Evidence from prison inmates, arrests, and self-reports." *American Economic Review*, 94(1): 155–189.

MacKinnon, James G, and Matthew D Webb. 2017. "Pitfalls when estimating treatment effects using clustered data." Queen's Economics Department Working Paper.

Marcotte, Dave E. 2019. "The returns to education at community colleges: new evidence from the Education Longitudinal Survey." *Education Finance and Policy*, 14(4): 523–547.

Marcotte, Dave E, Thomas Bailey, Carey Borkoski, and Greg S Kienzl. 2005. "The returns of a community college education: Evidence from the National Education Longitudinal Survey." *Educational Evaluation and Policy Analysis*, 27(2): 157–175.

Mead, Arthur Raymond. 1918. *The development of free schools In the United States as Illustrated by Connecticut and Michigan.* Teachers college, Columbia university.

Ruggles, Steven, Sarah Flood, Ronald Goeken, Josiah Grover, Erin Meyer, Jose Pacas, and Matthew Sobek. 2019. "IPUMS USA: Version 9.0 [dataset]."

Swift, Fletcher Harper. 1911. *A history of public permanent common school funds in the United States, 1795-1905.* H. Holt and Company.

7 Tables

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

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

Table 2: Summary Statistics in 1850

	Before 1850	1850-1859	1860-1869	1870 or later
% attending school	75.2	63.9	67.0	43.6
% female	49.3	48.9	49.2	49.0
% urban	17.7	5.3	14.9	6.5
age	9.3	9.2	9.3	9.3
N	1,059,576	1,066,815	1,367,436	431,562

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

The above table gives relevant summary statistics for school-age children in the 27 states considered here in 1850. Columns are organized by the decade in which the state abolished rate bills for students. I compare these groups across some observable dimensions.

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

Table 3: Staggered Adoption Differences in Differences

	Rural	Urban	Rural 10+	Urban 10+
	prob(att)	prob(att)	prob(att)	prob(att)
Full Treatment	0.0720**	0.0003	0.0790**	0.0147
($age < 7$ when law passed)	(0.0202)	(0.0224)	(0.0232)	(0.0226)
	[0.007,0.312]*	[-0.017,0.182]	[0.023,0.305]*	[0.010,0.147]*
Partial Treatment	0.0367	0.0015	0.0425	0.0140
($11 > age > 6$ when law passed)	(0.0209)	(0.0146)	(0.0219)	(0.0136)
	[-0.011,0.241]	[-0.011,0.108]	[-0.000,0.216]	[0.016,0.085]*
N	17,610,766	5,704,146	8,398,881	2,636,717

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

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

Table 4: Staggered Adoption Differences in Differences - Low SES Only

	Rural	Urban	Rural 10+	Urban 10+
	prob(att)	prob(att)	prob(att)	prob(att)
Full Treatment	0.0738**	-0.0009	0.0818**	0.0153
($age < 7$ when law passed)	(0.0203)	(0.0231)	(0.0236)	(0.0233)
	[0.002,0.324]*	[-0.020,0.200]	[0.028,0.307]*	[0.006,0.155]*
Partial Treatment	0.0376	0.0009	0.0441	0.0145
($11 > age > 6$ when law passed)	(0.0211)	(0.0158)	(0.0224)	(0.0151)
	[-0.017,0.252]	[-0.014,0.115]	[0.002,0.227]*	[0.017,0.091]*
N	16,222,381	4,441,612	7,735,620	2,034,634

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

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

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

	Rural	Urban	Rural 10+	Urban 10+
	prob(att)	prob(att)	prob(att)	prob(att)
Full Treatment	0.0401*	-0.0181	0.0400*	0.0006
($age < 7$ when law passed)	(0.0190)	(0.0177)	(0.0179)	(0.0190)
	[-0.036,0.118]	[-0.064,0.165]	[0.001,0.112]*	[-0.024,0.140]
Partial Treatment	0.0096	-0.0180	0.0093	-0.0026
($11 > age > 6$ when law passed)	(0.0187)	(0.00884)	(0.0183)	(0.00625)
	[-0.033,0.091]	[-0.056,0.076]	[-0.004,0.089]	[-0.014,0.070]
N	13,307,070	5,015,349	6,344,541	2,315,178

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

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

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

	Rural	Urban	Rural: CSL on RHS	Rural: 1850-1870
	prob(att)	prob(att)	prob(att)	prob(att)
Full Treatment	0.0359	-0.0212	0.0515*	0.0532*
($age < 7$ when law passed)	(0.0185)	(0.0173)	(0.0185)	(0.0207)
Partial Treatment	0.0047	-0.0194	0.0095	0.0051
($11 > age > 6$ when law passed)	(0.0174)	(0.0091)	(0.0192)	(0.0202)
N	12,999,942	4,883,107	12,999,942	8,948,225

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

Standard errors clustered at the state level in parentheses. Critical values are obtained from the t-distribution with 14 degrees of freedom. Column 1 shows the results from Equation 1 estimated on rural individuals between the ages of 5 and 14 only in the 15 states for whom data on compulsory schooling laws is available. Column 2 shows the results from Equation 1 estimated on urban individuals between the ages of 5 and 14. Column 3 shows the results from Equation 2 estimated on rural individuals between the ages of 5 and 14. Column 4 shows the results from Equation 1 estimated on rural individuals between the ages of 5 and 14 using only data from 1850 through 1870, exluding 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.

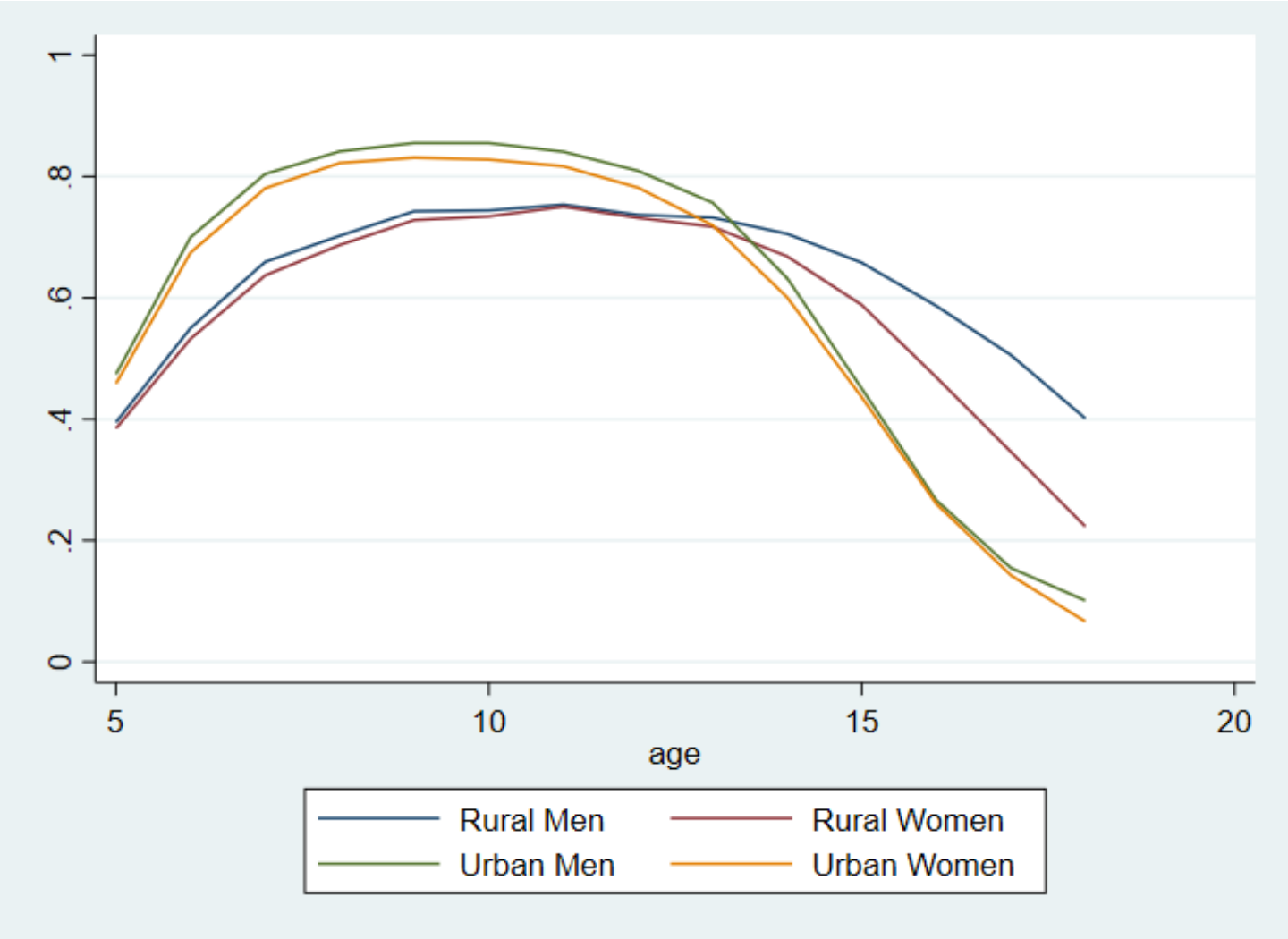
8 Figures

8.1 Map of Included States

Figure 1

8.2 Attendance Rates by Age

Figure 2



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

A Additional Tables

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

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

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

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

Table A.2: Staggered Adoption Differences in Differences - Contemporaneous Effect, Low SES Only

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

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

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

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

	Rural prob(att)	Urban prob(att)	Rural 10+ prob(att)	Urban 10+ prob(att)
Full Treatment	0.0721**	0.0003	0.0790**	0.0148
($age < 7$ when law passed)	(0.0202)	(0.0225)	(0.0232)	(0.0227)
	[0.002,0.317]*	[-0.021,0.191]	[0.024,0.291]*	[0.009,0.143]*
Partial Treatment	0.0398	-0.0004	0.0533*	0.0187
($age = 7$ when law passed)	(0.0201)	(0.0160)	(0.0227)	(0.0173)
Partial Treatment	0.0359	0.0024	0.0375	0.0155
($age = 8$ when law passed)	(0.0213)	(0.0167)	(0.0240)	(0.0157)
Partial Treatment	0.0399	0.0044	0.0438	0.0133
($age = 9$ when law passed)	(0.0212)	(0.0134)	(0.0220)	(0.0119)
Partial Treatment	0.0281	-0.0002	0.0316	0.0079
($age = 10$ when law passed)	(0.0234)	(0.0136)	(0.0240)	(0.0117)
N	17,610,766	5,704,146	8,398,881	2,636,717

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

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

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

	Rural	Urban	Rural 10+	Urban 10+
	prob(att)	prob(att)	prob(att)	prob(att)
Full Treatment	0.0617**	-0.0056	0.0660*	0.0070
($age < 7$ when law passed)	(0.0214)	(0.0220)	(0.0253)	(0.0228)
Partial Treatment	0.0293	0.0003	0.0330	0.0110
($11 > age > 6$ when law passed)	(0.0233)	(0.0133)	(0.0242)	(0.0127)
N	15,094,635	5,043,825	7,009,657	2,284,139

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

Standard errors clustered at the state level in parentheses. Critical values are obtained from the t-distribution with 17 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.