Hidden Gains: Effects of Early U.S. Compulsory Schooling Laws on Attendance and Attainment by Social Background*

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Abstract

Research on early compulsory schooling laws finds minimal effects on attendance, but fails to investigate heterogeneous effects. Similarly, research proposes limited contexts in which expansion policies can increase equality, but has difficulty separating policy and cohort effects. Capitalizing on within-country variation in timing of early compulsory laws, passed 1852-1918, I ask whether they improved equality of school attendance or educational attainment by class, nativity, and race. Based on census data, compulsory laws increased equality of attendance and attainment, particularly among young men in the North, where the laws reduced class and race gaps by over 20%. Early compulsory schooling laws provided "hidden gains," missed in previous analyses, suggesting policies that raise minimum schooling can increase educational equality in certain contexts.

Keywords: educational inequality; social stratification; compulsory school; educational attainment; regression discontinuity

Introduction

Politicians often note the importance of expanding education for economic growth and competition. At the same time, they suggest the government can drive that expansion. In January 2012, for example, President Obama called for states to extend compulsory education to age 18. Furthermore, many education policies explicitly attempt to increase equality in schooling – whether in terms of access, quantity, or quality. Recent examples include No Child Left Behind and the Texas Top Ten Percent Admission Law (Texas House Bill 588).

Despite these current efforts, most historical research finds little effect of government intervention on educational expansion. There are arguments, for example, that individual interests (Goldin and Katz 2008), industrialization and urbanization (Baker 1999), or demand for workers with new or specific skills (e.g., Trow 1961; Clark 1961) drove expansion in the 19th and early 20th Centuries. On a broader scale, education may expand due to population growth, initial education levels, or nation-building efforts, not as a function of state policies (Meyer et al. 1977; 1979; 1992). These authors suggest specific government policies – including compulsory education laws – play little role in educational expansion.

A competing argument emphasizes the role of the state in shaping and expanding the educational system (Archer 1979; Steffes 2012). For example, Rubinson (1987) suggests the U.S. political system encouraged mass education and made the school system less stratified than in other countries. Walters (2000) stresses the importance of an implicit (Northern) American school policy: meet growing demand by providing spaces and teachers for all. In each of these cases, the state plays an important if subtle role in educational expansion.

Compulsory schooling laws, which require school-age children to attend school for a certain amount of time, are a clear effort to expand schooling. Most existing discussions of (especially early) U.S. compulsory schooling laws suggest they had little effect on attendance

(Landes and Solmon 1972; Edwards 1978; Goldin and Katz 2011; Tyack 1976; Lleras-Muney 2005; Katz 1976). Yet little research has examined early census data to test this claim and no existing studies investigate heterogeneous effects, which may explain previous null findings.

Meanwhile, reforms intended to equalize educational opportunity often show limited success. Whether examining trends in educational inequality or specific policies aimed at increasing equality of access to schooling, evidence suggests "persistent inequality" despite educational reform (Blossfeld and Shavit 1993) or different effects depending on context (Raftery and Hout 1993; Torche 2005; Post 1994; Shavit and Westerbeek 1998). Because there is no within-country variation in the timing of the policies, however, these analyses cannot separate cohort and policy effects.

To summarize, existing research suggests government attempts to expand education are largely ineffective; rather, educational expansion is driven by other factors. Early U.S. compulsory schooling laws, which explicitly attempted to increase education levels, apparently yielded little impact on expansion. At the same time, existing research leaves a largely pessimistic view of the equalizing potential of educational reform, except in certain contexts.

Combining these two perspectives may yield different results. Research on the effects of early compulsory schooling laws – the first law in each state requiring children in a given age range to attend school for a minimum number of weeks each year – has not addressed the possibility of different effects by social background. Because the laws were geared primarily at lower class children, estimating only average treatment effects makes null effects more likely. Similarly, studies of equalization efforts have not examined early U.S. compulsory laws, which should increase equality of attendance and attainment. The laws aimed to increase attendance at the elementary level, where previous research suggests policies may hold the greatest equalizing potential (Post 1994; Shavit and Westerbeek 1998). Thus, capitalizing on within-country variation in the timing of compulsory attendance laws and North-South differences at the time (Post 1994; Walters 2000), the 19th century U.S. allows further investigation of the contexts in which expansion policies can increase educational equality.

Combining two previously separate literatures – educational equality and U.S. compulsory schooling laws – I ask whether early compulsory laws improved educational equality by social background, including class, nativity, and race. Missed in previous studies that fail to investigate effects on equality, I find that compulsory attendance laws provided "hidden gains," making social background less important for school attendance. Results indicate a non-trivial equalizing effect of early compulsory laws on attendance (and attainment by race) and question the contexts in which reform can increase educational equality. Below, I provide brief reviews of U.S. compulsory schooling laws, research on those laws, and research on policy efforts to equalize education, followed by hypotheses, methods and data, and results.

Compulsory Schooling Laws

Compulsory school attendance laws began with Massachusetts in 1852. Other states in the North followed more quickly than the South, but by 1918 (when Mississippi passed the law) all states had made attendance compulsory. The year school attendance became compulsory in each state is shown in supplementary Table S1. Prior to these first compulsory attendance laws, only certain youth working in particular types of jobs were required to attend school. Compulsory attendance laws did not include any requirement to complete a certain number of grades, only to attend school at specified ages. Although most states required attendance from ages 8 to 14, the length varied slightly by state. For example, Connecticut and Minnesota required attendance from ages 8 to 16, New Hampshire from 6 to 16, and Rhode Island and Wisconsin from 7 to 15.

Compulsory laws aimed to achieve universal school attendance and were primarily directed at lower class and immigrant families who did not already send their children to school. For example, the Commissioner of Education (1891: 493) reports, "It must be borne in mind that the law applies to children of tender years, whose right it is to have schooling. If the misfortune or shiftlessness of parents has resulted in poverty, shall the burden of this fall upon young children?" Opposition to compulsory schooling reportedly came "from the lawless and criminal classes; from the idle and shiftless; from those who take no interest in the education of their children, or care nothing for them but to get work out of them; and, of course, from those who have felt the penalties of the law" (1891:520). A dissertation (Perrin 1896) and speech (Moore 1902) from the period portray 19th century compulsory laws as aimed at lower class and immigrant children, whose parents do not appreciate the value of education. As the above examples illustrate, compulsory schooling laws aimed to override "neglectful" parents and increase attendance among lower class and immigrant youth.

While previous research suggests the laws had no effect on school attendance (Landes and Solmon 1972; Goldin and Katz 2011), historical documents offer a mixed review: some state and city administrators saw no effect while others claim effective implementation. While the term "dead letter" is often used to describe compulsory laws, the New York City superintendent reported in 1890 that the law was enforced and effective (Commissioner of Education 1891:499). An Illinois administrator noted, "In the city of Chicago thousands of children have been brought into the schools through its agency. The same thing is also true in other towns and cities of the state" (Commissioner of Education 1891:505).

Historical debates indicate that compulsory laws carried enough weight for citizens to take them seriously. Discussing Connecticut's compulsory schooling law, the Commissioner of Education (1891:492) quotes a Connecticut School Report: "There are many parents who assert what they call their rights of governance. Such think it is unjust that they must send their children to school, and thus lose the profit of their labor." Objections to the law were outlined by the secretary of the state of Connecticut (Commissioner of Education 1891:493) as follows: 1) "Such a law would create a new crime." 2) "It interferes with the liberty of parents." 3) "It arrogates new power to the government." and 4) "It is un-American and unadapted to our free institutions." As these debates about compulsory laws illustrate, they were not insignificant at the time. Nevertheless, the historical claims are almost entirely devoid of statistical evidence and descriptions of the laws could merely reflect the rhetorical needs of the speaker at the time.

A Brief Review of Research on U.S. Compulsory Laws

Economic arguments suggest rapid educational expansion in the early 20th century largely reflects individual decisions and high demand for educated workers, not policy changes such as compulsory laws (Goldin and Katz 2008). Similarly, while research on later changes to compulsory laws find important effects (Lleras-Muney 2005; Oreopoulos 2006; Oreopoulos et al. 2006; Edwards 1978), the few studies of late-19th- and early-20th-century compulsory schooling laws find that they had little influence on the expansion of school attendance (Landes and Solmon 1972; Goldin and Katz 2011). Investigating the effect of later compulsory laws on secondary schooling from 1910 to 1939, Goldin and Katz (2011) suggest that, at best, the laws accounted for six or seven percent of the increase. This study, however, does not study equalizing effects of early compulsory laws on elementary school attendance.

In the most relevant study, Landes and Solmon (1972) use state-level census data to examine the effects of early laws and find that they did not increase attendance. Several limitations, however, motivate further research. First, Landes and Solmon focus primarily on 1870 through 1890. A more thorough analysis would include data from before attendance was required in the first state (Massachusetts in 1852) to after it was required in the last state (Mississippi in 1918). Thus, analyses should include data from 1850 through 1920. Second, although Landes and Solmon (1972) use first-differences to try to estimate the effect of a change in state compulsory law on a variety of school enrollment and attendance variables, they do not pool censuses. They only examine changes in enrollment between two censuses at a time (1870-1880 and 1880-1890). State adoption of compulsory laws was fairly slow, so identification rests on a small sample and leaves their analysis with low power, increasing the likelihood of null results. Finally, their approach does not allow the use of state and year fixed effects to account for these differences. A better approach would be to pool censuses, estimating the effect of compulsory laws on enrollment rates with state and year fixed effects.

Furthermore, a recent study contradicts arguments that early compulsory laws were ineffective (Puerta 2009). Using a more precise county-level difference-in-difference approach, limited to counties along a state border separating those with and without a change in compulsory attendance law between censuses, Puerta (2009) finds that compulsory laws increased absolute attendance rates by about seven percent.

Regardless of their effect on absolute attendance, however, compulsory laws may have shifted the distribution of attendance or attainment by social background, increasing equality of opportunity. Such shifts in the distribution of attendance would be hidden in previous analyses that fail to investigate heterogeneity. For example, because compulsory laws were primarily aimed at lower class youth, a simple average treatment effect could miss equalizing effects by class. In addition, if the laws were passed as growth in attendance was slowing – in an effort to push the reluctant into school – research could find null or even negative effects. In this case, growth in attendance would appear to decline after the compulsory laws, even if the laws were effective in promoting attendance among the targeted, lower class youth.

With only one exception, no studies investigate potential heterogeneous effects by race, class, or nativity status. Puerta uniquely investigates heterogeneous effects by race, finding greater educational gains for black than white youth, but he does not study changes in equality by class or nativity status. Furthermore, the laws could have had different effects by gender, particularly if they influenced attendance through opportunity costs (Fishlow 1966). Young men had more employment opportunities around the turn of the century and, therefore, higher opportunity costs, so they could show a stronger response to the compulsory laws.

I improve on all of the above limitations of existing research by pooling 1850-1920 census data, capitalizing on state variation in the timing of the first compulsory law, and investigating heterogeneous effects by class, race, and nativity status, separately by gender.

A Brief Review of Research on Equalizing Policies

Research on educational equality has found little evidence to support claims – such as those by politicians – that policy changes can improve equality of educational opportunity. Blossfeld and Shavit (1993), for example, combine analyses of an impressive 13 countries over time spans covering a variety of educational changes, yet find an overarching pattern of "persistent inequality." These and many single-country studies (e.g., Park 2004; Pong 1993; Gerber and Hout 1995), however, investigate *trends* in equality rather than attempting to identify the effects of a specific policy.

In contrast, Raftery and Hout (1993) and Torche (2005) study particular policy changes, finding maintained or even increased inequality, but limit analyses to one country. Without within-country variation in policy timing, these studies rely on changes across cohorts to identify policy effects. They cannot separate policy and cohort effects. That is, analyses relying on cohort change cannot rule out the possibility that apparent policy effects reflect unrelated cohort changes. Cohort analysis is common in educational stratification (Shavit and Blossfeld 1993; Shavit and Westerbeek 1998; Mare 1980, 1981), but Post (1994) and Torche (2005) highlight its limited ability to identify causal effects.

Other studies offer more hope for educational reform. Whether relying on data from multiple countries (Breen 2010; Breen et al. 2009; Jonsson et al. 1996) or policy changes within a single country (Shavit and Westerbeek 1998), there is some evidence of increasing equality in countries throughout Europe. Methodological differences (ordered logit as opposed to the Mare transition model [1980, 1981]) could explain why their results differ from those of Blossfeld and Shavit (Breen et al. 2009). Alternatively, because the rising educational equality largely reflected increasing access to secondary school (Breen et al. 2009), it suggests government policy could improve educational equality through secondary expansion. Consistent with this argument, expansion policies in Hong Kong increased equality of secondary school attendance (Post 1994). Similar to other policy analyses (Raftery and Hout 1993; Torche 2005), however, Post (1994) cannot separate policy effects from unrelated changes over time because there was no within-country variation in timing.

A complicating argument suggests the equalizing potential of expansion policies depends on the context. Evidence suggests expansion can increase inequality at higher levels in contexts where education is free to expand (Raftery and Hout 1993; Torche 2005), but can decrease inequality at lower levels of education or in contexts where the education system is tightly controlled by the state (Post 1994; Shavit and Westerbeek 1998). Post (1994) suggests expansion policies are less likely to generate equalizing effects in demand-driven systems such as the Northern U.S. Comparing compulsory effects in the U.S. North and South will provide more insight into contexts in which expansion policies can increase educational equality.

To summarize, while most research examines trends in inequality across cohorts with inconsistent results, a few studies examine specific policy effects. Single-country policy studies, however, face a key methodological limitation: there is no within-country variation in the timing of the policy, so they cannot separate cohort and policy effects. Capitalizing on within-country variation in the timing of early U.S. compulsory attendance laws, I examine the effects of this expansion effort on educational equality, net of cohort and state differences.

While Post (1994) and others (Goldin and Katz 2008; Baker 1999; Trow 1961; Clark 1961) suggest that (at least Northern) U.S. educational expansion is largely demand-driven and unrelated to state policy, others emphasize the role of the state in shaping and expanding the U.S. educational system (Steffes 2012; Rubinson 1987; Walters 2000). Did early U.S. compulsory schooling laws contribute to expansion and equality of elementary school attendance or attainment? Combining two largely distinct areas of inquiry – on compulsory laws and policy-driven educational equality – I ask whether early compulsory schooling laws increased equality of school attendance and attainment by social background in the U.S.

Why Compulsory Laws Might Equalize Attendance by Class: Theory and Hypotheses

Theory suggests compulsory laws could have been an effective equalizing policy. As Fishlow (1966: 427) suggests, compulsory laws should offset opportunity costs, which fall unequally by class. Lower class families with fewer resources are less able to forego the wages their children could earn in the labor market. By increasing the costs of non-attendance (e.g., through fines or social stigma), compulsory schooling should make attendance more likely among lower class youth and increase equality. If this argument is correct, effects should be stronger for boys, who have higher earning potential and opportunity costs than girls.

Relative risk aversion (RRA) theory offers an alternative explanation for a link between compulsory laws and equality of attendance. People invest in more schooling to prevent downward mobility. As more families send their children to school, it raises the incentive of other families to send their children as well, so they can attain a lifestyle similar to their parents. While lower class and foreign families may have avoided schooling their children before the laws, afterwards they would risk disadvantage if their children did not attend. Stronger equalizing effects for boys could also be consistent with RRA, if families feel that a lack of schooling is more jeopardizing for men than women. Either of these explanations could generate an equalizing effect of compulsory laws; I do not attempt to distinguish between them.

As Hout et al. (1993:48) note, the size of the educational system must expand in order for opportunity to increase. There is disagreement, however, about how control of expansion is related to the equalizing potential of a policy. Post (1994) suggests expansion policies in tightly state-controlled systems are more likely to generate equalizing effects than in areas with demand-driven education systems such as the Northern U.S. Unlike the South and Europe, Northern states expanded education to meet growing demand (Walters 2000); local administrators provided spaces and teachers for all. Schools in the South, in contrast, faced greater demand for school attendance than they could (or chose to) meet, particularly among black and poor white youth (Anderson 1988). Despite greater state control of educational expansion, which facilitated equality when coupled with compulsory laws in Hong Kong (Post

1994), efforts to maintain racial inequality (i.e. Jim Crow laws) likely hampered the equalizing potential of compulsory laws in the post-Civil War South. Thus, because school attendance was not made available to all who desired it, compulsory schooling should hold less equalizing potential (particularly by race) in the South than in the North, where expansion met demand.

Based on the points above, hypotheses include:

1) compulsory schooling laws should increase equality of school attendance by social background (class, nativity, and race) in the late 1800s and early 1900s;
2) compulsory schooling laws should increase equality of educational attainment;
3) the laws should show a more robust effect on young men, who had greater opportunity costs of school attendance or higher risk of downward mobility from non-attendance than young women; and
4) compulsory laws in non-Southern and early adopting states should show more robust equalizing effects, particularly by race, than in Southern states.

Data and Methods

Below I provide details about the analyses and data used to examine the equalizing effects of compulsory attendance laws by class, nativity, and race.[1]

<u>Attendance Analysis</u>

Individual census data provide school attendance, father's occupation, nativity, race, state of residence, and other background information. Analyses use census data from 1850 to 1920, which includes information before the first and after the last law. State data based on ICPSR (1970) Historical Census Data as well as original census tables provide measures of state characteristics which serve as controls for factors which may be related to the timing of the compulsory law.[2] These state level controls help address concerns that the compulsory schooling laws were endogenous to educational expansion or equality of school attendance.

The sample is limited to school-age youth ages 6 to 13 who were related to the head of household in 1850 to 1920 censuses.[3] Combining state of residence, year of state compulsory attendance law (Table S1), and census year, a dummy variable for compulsory school assignment indicates whether an individual is required to attend school in a given census year.

School attendance is measured with an indicator for whether a child attended school within the last year. Ideally attendance would be measured with more detail because daily school attendance was well below the number of students enrolled or registered. For example, Lassonde (1996) notes about antebellum New Haven that only around 2/3 of those registered actually attended on a given day. However, the attendance indicator is not a bad measure given the flexibility of compulsory requirements. The laws generally required attendance for 12 weeks, only 6 of them consecutive. Intermittent attendance, permitted by the law, may not have been captured with other questions. While this measure of attendance offers a more valid measure of whether a child obeyed the compulsory law, its validity still depends on accurate reporting. If families lie about a child's school attendance to give the appearance of obeying the law, census data would provide an invalid measure of attendance. It is unclear, however, what families would have to gain by reporting inaccurate information to a census taker, since truant officers relied on school rather than census information and compulsory requirements were stricter than the census question. However, I also examine effects of the laws on grades of school completed (in the 1940 census), which should echo attendance results and reduce possible concerns.[4]

I use four different measures of social background: father's occupational status; father's occupational category; nativity status; and race. Individuals are coded as foreign born if their birth place was outside the U.S. and are coded as non-white if they indicated they are any race other than white. Father's occupation is measured using both continuous scores and occupation categories. IPUMS census data provide socioeconomic index (SEI) scores standardized to 1950 for all of the census years. SEI score is based on the median income and educational attainment associated with each occupation among men in 1950. There were many changes in occupational distributions from the late 1800s to 1950, but standardized scales provide consistent and complete scores for all occupations across multiple census years.[5]

Categorical occupational measures follow the EGP classification system, widely used in social mobility studies (Erikson and Goldthorpe 1992; Hout 1989; Torche 2008), which classifies occupations according to how difficult it is for employers to monitor performance and specificity of required skills (Goldthorpe 2000). Rather than create an algorithm to classify occupations myself, I use Morgan and Tang's (2007; also used in Torche 2008) coding scheme to assign census occupation codes to five EGP categories.[6] Results based on the categorical analysis are consistent with those using SEI score and are shown in the appendix.

Analysis takes advantage of the staggered compulsory laws by state to estimate their effect among school-age youth by including fixed effects for state and cohort (in five-year intervals). Identification is based on changes in the laws across census years. The following logit model estimates the relationship between compulsory laws and the log odds of attendance by class. In equation 1, CompSchool indicates whether the youth is required to attend school, DadOcc is father's SEI score for individual i, X represents individual and state level measures at the given census year (including individual age up to a quartic and state measures related to the timing of the laws), and u and λ are indicators for state and cohort.

$$
\begin{aligned}
\text{logit}[\Pr(\text{InSchool}_{ijk}=1)] = a &+ \beta_1\text{CompSchool}_i + \beta_2\text{DadOcc}_i + \beta_3\text{Foreign}_i + \beta_4\text{Black}_i + \\
&\beta_5\text{CompSchool}_i * \text{DadOcc}_i + \beta_6\text{CompSchool}_i * \text{Foreign}_i + \\
&\beta_7\text{CompSchool}_i * \text{Black}_i + \beta_8\text{Foreign}_i * \text{DadOcc}_i + \\
&\beta_9\text{Black}_i * \text{DadOcc}_i + \beta_{10}\text{X}_i + u_j + \lambda_k
\end{aligned} \tag{1}
$$

Fixed effects enable identification of compulsory effects by father's class net of state and cohort differences. The parameters of interest are β_5, β_6, and β_7, which estimate the interaction between compulsory law and social background. If β_5 is significant and negative, it suggests the laws made school attendance more equal by class, expanding opportunity. β_6 and β_7 should be significant and positive if the laws made attendance more equal by nativity and race. I use the same model to investigate categorical effects, but indicators for father's EGP occupation category replace father's SEI in equation 1 and are interacted with compulsory school requirement. Logit estimates address the potential nonlinear relationship due to high rates of school attendance, but linear probability models show consistent results. Standard errors are corrected for state-level clustering in all models. To further rule out competing explanations, I also present models that limit the sample to individuals 10 years on each side of the compulsory cutoff (ages 4 to 23 at the time of the law in most states, which required attendance until age 14) and control for age at the time of the law.

Educational Attainment Analysis

I investigate effects on educational attainment using the 1940 census – the first census to include educational attainment. Data from older cohorts in this census partly address whether the laws had a similar effect on attendance and attainment.[7] The 1940 census is not ideal, because compulsory education may increase longevity. If education reduces mortality, as Lleras-Muney (2005) found for later extensions of compulsory school age requirements, individuals surviving until 1940 should under-represent those who were not required to attend school and yield an under-estimate of the effect of compulsory schooling on attainment. Nevertheless, 1940 census data provide an attenuated estimate of the relationship between compulsory laws and educational equality. Class background is not available for adults in the 1940 census, and I do not know whether foreign born individuals were in the U.S. when they were school age, so attainment analysis necessarily only examines compulsory effects by race.

$$\begin{aligned}\text{Highest Grade}_{ijk} &= a + \beta_1 \text{NonWhite}_i + \beta_2 \text{CompSchool}_i + \beta_3 \text{CompSchool}_i * \text{NonWhite}_i \\ &+ \beta_4 \text{X}_i + u_j + \lambda_k + \varepsilon_{ijk} \end{aligned} \quad (2)$$

Using an approach similar to the individual attendance analysis, state and cohort fixed effects are included in equation 2 predicting years of school attained. In this model, non-white indicates racial identification of individual *i*, CompSchool indicates whether the individual was required to attend school as a child based on state of current residence, X represents controls for individual age up to a quartic, and *u* and λ are indicators for state and cohort (in five-year intervals). Analyses using compulsory assignment based on state of birth yield similar results. The parameter of interest is β_3, which estimates the interaction between compulsory law and race. If β_3 is significantly positive, it suggests the laws increased equality of educational attainment. Standard errors are corrected for state-level clustering in all models.

As Mare (1980, 1981) noted, however, each year of education may not matter equally. Race or compulsory assignment may influence the likelihood of making certain educational transitions more than others. Lacking complete educational history data, I cannot use the Mare model of educational transitions. To investigate compulsory effects on level of educational attainment, I apply the same methods as Breen et al. (2009), who face the same limitation. Specifically, I use ordered logit models, which estimate the likelihood of completing a particular level of education. Compared to the Mare model, ordered logit is more parsimonious and is not sensitive to collapsing adjacent educational levels (which improves estimates that span long periods of time, such as this one) (Breen et al. 2009). A limitation is that ordered logit models do not allow social background or compulsory assignment effects to vary by level of education. Nevertheless, they offer a sensitivity check of whether compulsory effects are similar for both levels and years of education. The ordered logit models use the same sample and measures as the OLS models, but the dependent variable measures the highest level of education each individual attained based on highest grade completed. Levels identify those who have completed: less than grade four; at least four years, but less than elementary school (grades 4-7), elementary school (grade 8), some secondary school (grades 9-11), secondary school (grade 12), and more than secondary school (grades 13 or above).

To capitalize on the adult measurement of educational attainment, I set up a regression discontinuity (RD) design by limiting the sample to cohorts 10 years on each side of the compulsory cutoff (ages 4 to 23 in most states, which required attendance until age 14) and controlling for the assignment measure (age at the time of the law). Holding the assignment measure constant, RD assumes that individuals within a narrow window on each side of the law are similar except for whether they were required to attend school.

All analyses discussed above are shown separately by gender and run separately by region (North vs. South). Null effects in the South, however, could reflect the smaller number of Southern states. Therefore, I also investigate regional differences by comparing the effects of compulsory laws in all states versus the effects of laws in early-adopting states (28 non-Southern states which made attendance compulsory before 1895).

Descriptive statistics for individual level census data are provided in Table S2. The sample excludes those missing father's SEI score (10%) and state measures (1%). Census data cannot provide information about father's occupation among those who do not live with their father. Excluding these individuals could bias results if absence of father's information is related to compulsory assignment. For example, youth could have selectively moved out of their parents' home in anticipation of being required to attend school when the law passed. Alternatively, fathers of children who were required to attend school could have been more likely to leave their family (e.g., anticipating greater financial support requirements).

At the same time, excluding those without father's information could simply limit generalizability to children who lived with their father. For example, perhaps those who do not live with their father are less likely to attend school regardless of state law. In that scenario, the effect of the law would be weaker among those not living with their father, but excluding them would not affect estimated effects among those studied here.

To investigate the extent of potential bias, Table S3 compares samples with and without father's information. On average, young men and women without father information tend to have smaller families, fewer siblings, and lower maximum household SEI scores. They tend to be older, are more likely to be non-white, and are less likely to be in school or live on a farm. While nearly all are significant due to the large sample sizes, these differences are tiny and suggest minimal bias from excluding those with absent fathers. In addition, only 10% of cases are missing information about father's occupation (some of which could reflect unreported occupation rather than non-resident father), which limits the extent of any potential bias. Further, I only examine attendance among children ages 6 to 13, which makes it unlikely that individuals have left their parents' home and reduces concern about selection out of the sample. Most importantly, there is no difference in compulsory assignment between those with and without father's information, which suggests selective omission among those required to attend school is not a concern. Nevertheless, it is important to note that results could reflect bias due to selection and are only generalizable to children living with their father.

Table S4 includes 1940 census measures for attainment analysis. Although descriptive statistics show strong similarity for men and women, all analyses are conducted separately by gender. Supplementary Tables S2 and S4 show that attendance and attainment are indeed higher when school attendance was required. Regressions below attempt to isolate the effect of compulsory laws. State-level analyses are provided in the appendix.

Results

Table 1 shows results of attendance analyses by father's SEI, nativity, and race.

[Table 1 about here]

The interaction between compulsory school and father's occupational status is negative and significant (at $p<0.1$ or lower) for boys in all of the models, except when limited to the South. Based on the coefficients in Table 1, compulsory assignment reduced the association between father's SEI and likelihood of school attendance by 27% among boys in all states, 83% in early adopting states, and 25% in Northern states. The reduction does not reach significance among boys in Southern states, which suggests compulsory laws reduced the association between father's occupational status and individual attendance, but only among boys in the North. Thus, compulsory schooling made school attendance more equal by class background, but only outside the South. Among young women, this interaction is only significant when including all states. The interaction between compulsory school and nativity status never reaches significance for boys, suggesting the laws did not influence inequality by nativity. Among girls, this interaction also fails to reach significance except in the South (Model 8), where the negative interaction suggests compulsory laws increased inequality of attendance by nativity.

Finally, the interaction between compulsory assignment and race is significant and positive in every model, which suggests the laws significantly reduced the attendance gap by race. Depending on the states included, compulsory laws reduced the racial attendance gap by 19%-30% for boys and 25%-33% for girls. For both boys and girls, however, compulsory assignment appears to have increased racial equality more in the North than the South. In both cases, the interaction between compulsory assignment and race is larger among those in early compulsory states than those in Southern states. For boys, this interaction term is also larger in non-Southern than Southern states.[8]

The gender difference by class suggests the laws were less potent among young women (except non-white women). The stronger equalizing effects among young men are consistent with the argument that the laws reduced opportunity costs associated with school attendance. With lower earning potential, young women's opportunity costs may have been less influenced by compulsory laws. Gender differences could also be consistent with relative risk aversion theory. If women's life outcomes depended less on relative schooling (and more on marriage, for example), families may still have avoided sending them to school despite compulsory laws.

Figure 1, based on Model 2 in Table 1, illustrates the weaker relationship between father's SEI and the likelihood of school attendance among young men required to attend. Furthermore, the figure shows that compulsory assignment reduced attendance gaps by race. To further illustrate the relationship to class, Figure 2 shows average state attendance by father's SEI category over the number of censuses from passage of the compulsory law. There is a noticeable decline in the attendance gap between status groups in the censuses immediately before and after the compulsory law. In addition, variation in attendance rates among the status groups decreases with the law. Despite some variability before the law, after the law the three status groups remain quite close together with steadily increasing attendance, although children from lower SEI backgrounds still have slightly lower attendance rates.

[Figures 1 and 2 about here]

In Table 1, the main effect of compulsory assignment is negative in most models, but generally only reaches significance in the South and is positive in the more precise models limited to those ten years on each side of the cutoff.[9] Consistent with previous research finding the laws did not increase overall attendance (Landes and Solmon 1972), this could reflect the large increases in attendance before school became required, with the laws aimed at reluctant truants. As shown in Figure 2, many high SEI and farm youth began attending before the law and enrollment growth declined or remained steady with the law for all except low SEI children. Thus, with state and cohort fixed effects, negative main compulsory estimates are not surprising, despite progress in attendance among lower class youth. By failing to examine heterogeneous effects of the laws, previous research missed effects among targeted youth.

Though not conclusive, North-South differences suggest compulsory effects may have been less effective in the South. In the largely agricultural economy, compulsory laws may have perversely increased opportunity costs for lower class youth if wages increased due to labor shortages. However, unmet demand for school places in the South (Walters 2000) could have prevented compulsory laws from increasing opportunity through competition for school access. Table S5 shows that the laws similarly increased equality by father's occupational category, with greater equalizing effects among boys and in early-adopting, Northern states.

To summarize, individual analyses suggest the laws reduced inequality in school attendance by class and race. Compulsory laws weakened the relationship between social background and school attendance, especially among young men in the North, where compulsory assignment reduced the class and race gaps by 25% and 30%, respectively. State level regression analyses, shown in the appendix, echo these equalizing effects.

School attendance was increasing throughout the late 1800s and early 1900s. As Hout et al. (1993) point out, as a given transition becomes nearly universal, inequality approaches zero. Importantly, however, attendance was not universal until about two censuses after the law (see Figure 2) and was generally around 70% (80% among high SEI youth) at the time of the law. There was substantial room for improvement, so results are not simply due to school attendance becoming universal.

[Table 2 and Figure 3 about here]

Educational attainment results (Table 2) suggest similar equalizing effects. Among both men and women in OLS and ordered logit models, attainment is substantially and significantly lower among non-whites. Net of state and cohort differences, individuals required to attend school completed slightly (but not always significantly) more years of school. Most importantly, however, compulsory assignment yielded a significantly greater boost in attainment for non-whites than whites. Figure 3 illustrates the interaction between compulsory school and race among women (Model 1 in Table 2). Among women who were never required to attend school as children, whites completed 3.4 more grades in school than non-whites on average. Among women required to attend school, however, this gap decreases by 22% to 2.7 grades. This equalizing effect of compulsory assignment holds in OLS models predicting grades completed and in ordered logit models predicting the conditional likelihood of completing education levels. The interaction terms remain at least marginally significant in all models, with only two exceptions. In both OLS and ordered logit models, women in early states and men in Southern states did not experience significant gains in racial equality.

Coupled with smaller interaction effects in Southern states for men and women, this null effect for men in the South is consistent with hypothesis 4 and suggests compulsory laws were less able to equalize educational attainment in the South than the North. Consistent with hypothesis 3, the null effect among women in early adopting states suggests the laws had a less robust effect on women, who had lower opportunity costs of education.

While class and nativity cannot be investigated, the positive interaction effects by race suggest compulsory schooling laws made educational attainment more equal by social background. Depending on gender and states included, compulsory assignment reduced the racial attainment gap by at least 6% outside the South (and as much as 53% among men in early adopting states). Thus, echoing attendance results, attainment results also suggest that compulsory laws increased educational equality, particularly among men outside the South.

Conclusion

Evidence suggests that early compulsory laws had different effects by class, nativity, and race, increasing educational equality. While previous studies find little evidence of absolute effects (Landes and Solmon 1972), I investigate heterogeneous effects by social background and find evidence of hidden gains. Supporting hypothesis 1, results show compulsory laws increased equality of school attendance by class and race (and by nativity status at the state level). Consistent with hypothesis 2, compulsory assignment increased equality of educational attainment by race. In both cases, supporting hypothesis 3, these equalizing effects were more robust among young men, suggesting the laws may have influenced attendance equality through opportunity costs or relative risk aversion.

Existing educational research offers a primarily pessimistic view of educational reform (Blossfeld and Shavit 1993; Rosenberg 1991; Raftery and Hout 1993; Torche 2005) or suggests expansion policies only decrease inequality in contexts where the education system is not demand-driven but tightly state-controlled (Post 1994). These studies, however, are limited in their ability to separate cohort and policy effects. Capitalizing on within-country variation in timing and net of cohort and state differences, I find the equalizing effects of early U.S. compulsory laws were more robust in the North than the South, consistent with hypothesis 4.

Contrary to Post (1994), my findings suggest that compulsory laws were more effective at increasing equality in the largely demand-driven system of the Northern U.S. (Walters 2000). My results could differ from Post's (1994) contention that expansion policies are more effective in tightly controlled systems for several reasons: methods (within-country variation, controlling for state and cohort differences); setting (Hong Kong vs. the U.S.); the discriminatory system of inequality in the post-Civil War South; or dimensions of inequality examined. Post investigates class inequality, not race or nativity. While I cannot tease out which of these explain the difference, my results appear to complicate existing arguments and suggest that expansion policies can yield different effects by both context and dimension of inequality. This complex three-way interaction could help explain previous evidence of persistent inequality and the apparent ineffectiveness of policy efforts. Further research should test this potential three-way relationship and tease out contexts and dimensions of inequality along which policy efforts can best increase educational equality.

Robust evidence of greater equality in attendance and attainment could of course mask persistence of inequality at important transition points, such as entry to secondary school (Mare 1981). Growth in the size of the system, for example, could encourage elites to maintain their relative advantage by attaining higher credentials (Collins 1971; Raftery and Hout 1993). However, results from ordered logit models (Table 2, Panel B) suggest compulsory assignment increased equality in level of educational attainment, which reduces concern about this type of inflation. Furthermore, in the late 1800s, simply attending school was an important transition. While this analysis cannot determine whether compulsory laws influenced equality at higher transition points, it supports existing research (Shavit and Westerbeek 1998) with evidence that the laws increased opportunity at lower levels of education, including school attendance. While Raftery and Hout (1993) point out that equality is expected as a level of education approaches saturation, attendance was hardly universal at around 70% (80% for high SEI youth) shortly before and after the laws.

Consistent with authors who emphasize the role of the state in shaping and expanding the U.S. educational system (Steffes 2012; Rubinson 1987; Walters 2000), I find that early U.S. compulsory schooling laws contributed to expansion and equality of elementary school attendance and attainment. Results provide consistent evidence of “hidden gains,” which were missed in previous research that only estimated average treatment effects of the laws. These hidden gains suggest reforms which raise the minimum level of schooling can increase equality at that level, at least in a developing context where schools expand to meet growing demand.

In January 2012, President Obama called for states to extend compulsory education in the U.S. to age 18. This analysis of 18^{th} and 19^{th} century U.S. data may hold limited relevance for the contemporary world, because of globalization or the current knowledge economy, for example. However, if states continue to meet rising space and teacher requirements and if skill demands continue to rise, results suggest raising the compulsory schooling age to 18 could increase educational equality by race and class.

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Tables and Figures

Table 1: Logit Coefficients from Individual Analysis of School Attendance by Class, Nativity, and Race

	In School							
	Young Men				Young Women			
	1	2	3	4	5	6	7	8
	All States	Early States 10-yr	Non-South	South	All States	Early States 10-yr	Non-South	South
Comp School	-0.134 +	0.244	-0.092	-0.246 *	-0.196 *	0.113	-0.179 +	-0.229 *
	(0.069)	(0.200)	(0.090)	(0.108)	(0.086)	(0.213)	(0.106)	(0.105)
Father's SEI	0.011 *	0.006 *	0.008 *	0.013 *	0.010 *	0.004 *	0.006 *	0.014 *
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Foreign	-1.302 *	-1.121 *	-1.218 *	-1.938 *	-1.001 *	-0.873 *	-0.858 *	-2.079 *
	(0.194)	(0.218)	(0.151)	(0.450)	(0.197)	(0.181)	(0.130)	(0.111)
Non-White	-1.186 *	-1.629 *	-1.455 *	-1.039 *	-1.166 *	-1.562 *	-1.131 *	-1.040 *
	(0.083)	(0.468)	(0.137)	(0.083)	(0.060)	(0.267)	(0.107)	(0.062)
Comp School * Father's SEI	-0.003 *	-0.005 *	-0.002 +	0.003	-0.002 *	-0.001	0.001	0.000
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
Comp School * Foreign	0.052	-0.150	0.162	-0.268	-0.054	-0.030	0.074	-0.375 *
	(0.163)	(0.297)	(0.150)	(0.223)	(0.142)	(0.292)	(0.132)	(0.134)
Comp School * Non-White	0.222 *	0.421 +	0.437 *	0.197 *	0.307 *	0.521 *	0.280 *	0.302 *
	(0.079)	(0.238)	(0.096)	(0.096)	(0.080)	(0.216)	(0.133)	(0.096)
N	302319	26955	196305	106014	295537	25872	192337	103200

* p<.05; + p<.10 Standard errors are adjusted for state level clustering.

Source: 1850-1920 IPUMS Census data. Sample includes 6-13 year old children related to head of household.

All models include controls for age up to a quartic, rural residence, group quarters residence, and time-varying state measures found related to the timing of early state laws: % manufacturing employment, % white illiteracy, % adjacent states with the law, % elderly, % foreign, years in union, manufacturing production per employee, and time varying % adjacent states and % elderly. Indicators for each state and cohort (in 5-year intervals) are included.

Early states made attendance compulsory before 1895. Models labeled 10-year are limited to 10 birth cohorts on each side of the compulsory cutoff and include controls for age at time of compulsory law and its interaction with compulsory assignment.

Results are similar by father’s occupational category, shown in appendix Table S5.

Table 2: Coefficients from Individual Analysis of Educational Attainment by Race, 1940 Census

Panel A: OLS Coefficients – Predicting Grades of School Completed

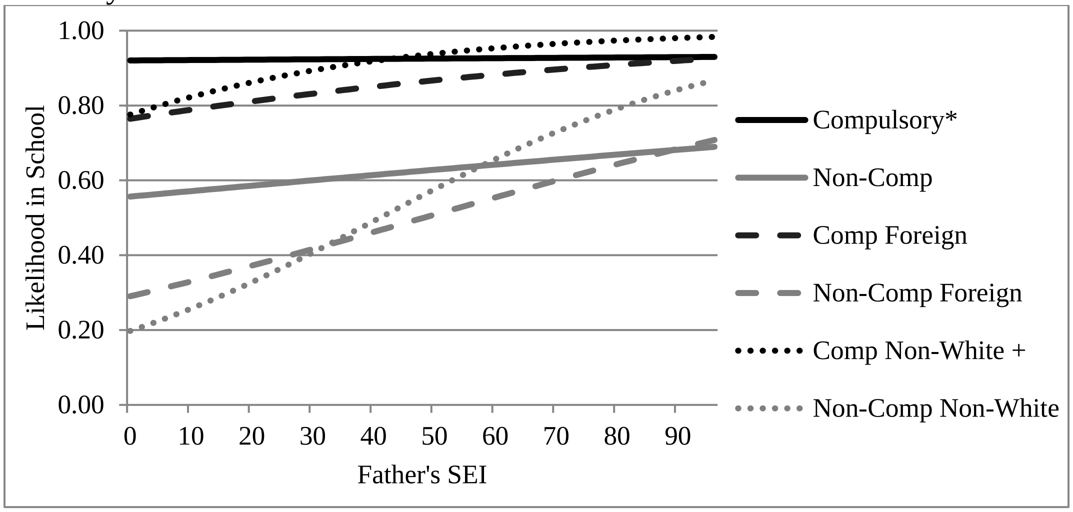
	Highest Grade Completed			
	Men			
	All (Age 25+)	Early States 10-yr RD	Non-South	South
Comp School	0.139	0.034	0.097	0.610 *
	(0.091)	(0.128)	(0.092)	(0.180)
Non-White	-3.239 *	-4.452 *	-2.762 *	-3.390 *
	(0.145)	(0.748)	(0.127)	(0.172)
Comp School*Non-White	0.199 +	2.343 *	0.599 *	-0.177
	(0.119)	(0.617)	(0.107)	(0.116)
N	314431	11582	211363	103068
	Women			
Comp School	0.142 *	0.194 +	0.145 +	0.394 *
	(0.077)	(0.097)	(0.072)	(0.176)
Non-White	-3.426 *	-3.970 *	-2.870 *	-3.569 *
	(0.135)	(0.745)	(0.135)	(0.155)
Comp School*Non-White	0.750 *	1.231	0.883 *	0.449 *
	(0.107)	(0.835)	(0.137)	(0.128)
N	319922	12693	215099	104823

Panel B: Ordered Logit Coefficients – Predicting Level of School Completed

	Education Level Completed			
	Men			
	All (Age 25+)	Early States 10-yr RD	Non-South	South
Comp School	0.087 +	0.050	0.073	0.263 *
	(0.048)	(0.068)	(0.050)	(0.081)
Non-White	-1.928 *	-2.712 *	-1.769 *	-1.843 *
	(0.087)	(0.476)	(0.091)	(0.097)
Comp School*Non-White	0.160 *	1.325 *	0.452 *	0.030
	(0.070)	(0.420)	(0.072)	(0.061)
N	314431	11582	211363	103068
	Women			
Comp School	0.098 *	0.119 *	0.099 *	0.177 *
	(0.044)	(0.060)	(0.043)	(0.088)
Non-White	-2.096 *	-2.331 *	-1.937 *	-1.970 *
	(0.084)	(0.472)	(0.092)	(0.094)
Comp School*Non-White	0.471 *	0.776	0.705 *	0.332 *
	(0.077)	(0.525)	(0.096)	(0.068)
N	319922	12693	215099	104823

* p<.05; + p<.10 Source: 1940 Census. All models include controls for age up to a quartic and indicators for each state and cohort (5-year intervals) not shown. Standard errors are adjusted for state level clustering and individuals are weighted to represent the population. RD models control for age at the time of the law (the assignment variable) and its interaction with compulsory assignment.

Figure 1: Predicted School Attendance by Father's SEI, Nativity, and Race: Young Men in Early States



Based on Model 2 in Table 1; 10-year-old boy in New York (non-compulsory) or New Hampshire (compulsory).

The association between father's SEI score and likelihood of school attendance is significantly weaker ($p<.05$) for boys required to attend school. Non-white boys have a stronger association between father's SEI and attendance, but compulsory assignment increases the likelihood of attendance more strongly for non-white than white boys ($p<0.1$). Compulsory assignment increases equality by class and race, but not nativity status in this model. Three-way interactions between compulsory assignment, father's SEI,and foreign or non-white are not significant.

Figure 2: State School Attendance by SEI Category – Young Men in All States

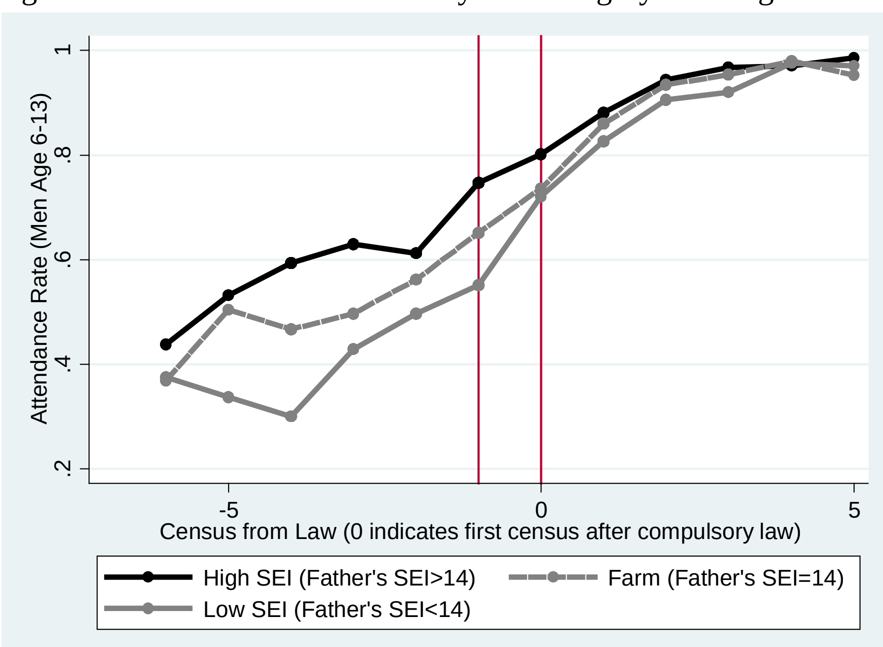


Figure 2 traces mean state attendance rates (of boys ages 6-13) separately by father's SEI category over the number of censuses from the time of the compulsory attendance law. The vertical lines represent the censuses immediately before and after the compulsory law. There was a large class gap in attendance rates before the law (approximately 0.2). That gap narrowed to less than 0.1 in the census immediately after the compulsory law and remained small or non-existent for at least five decades.

Figure 3: Educational Attainment by Compulsory School and Race – Women in All States

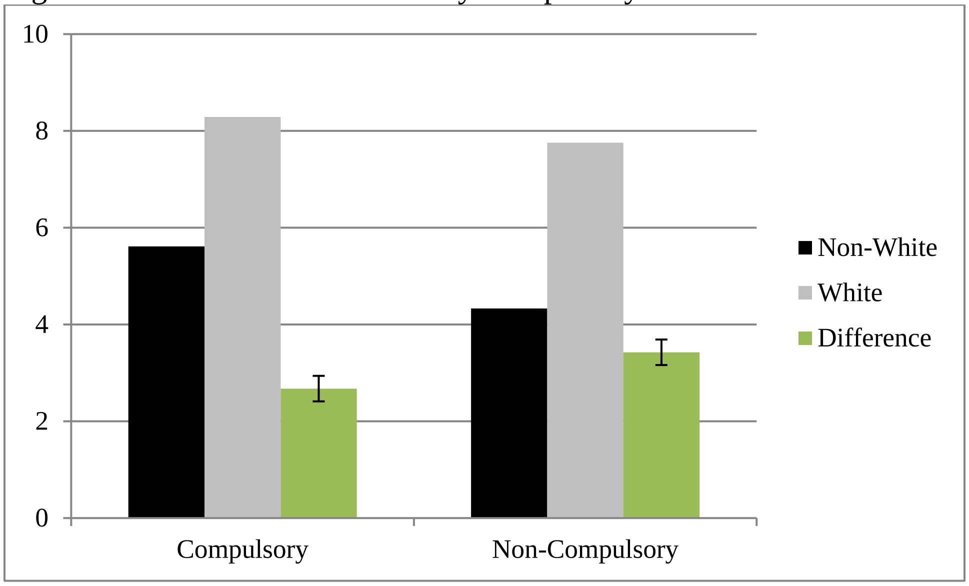


Figure 3 is based on Model 1 in Table 2, Panel A for women in all states.

There are significant educational attainment differences between white and non-white women, whether or not they were required to attend school. Furthermore, educational attainment is higher among both white and non-white women who were required to attend school. The racial attainment gap, however, is significantly lower among those required to attend school, suggesting that compulsory laws reduced inequality in educational attainment by race.

Appendix: Supplementary Material

State Level Attendance Analysis

To supplement individual analyses, I estimate compulsory effects on attendance at the state level. Most state-year measures are gathered from ICPSR (1970) Historical Census Data 1790-1970. However, these data do not always include appropriate or consistent measures across censuses and are therefore supplemented with information from original census tables and a compilation of statistical abstracts (US Bureau of the Census 1975). Behrens et al. (2003) provide incarceration, male unemployment, and governor partisanship data.[10]

Attendance rates by class, nativity, and race are created from collapsed individual census attendance data (weighted to represent the population) to produce state attendance rates among school-age children by father's SEI score, father's occupation category, nativity, and race. For example, I construct average attendance rates among foreign and native boys in each state at each census year. I follow the same process by race and class category, to examine effects by class background, nativity, and race separately. Youth are divided into high (SEI>14), farm (SEI=14), and low (SEI<14) occupational status categories based on father's SEI score and into the five occupational categories for the analysis by EGP categories.[11]

Using state measures from 1850 to 1920, average state attendance rates are first graphed by census from the law and social background to illustrate how attendance rates changed with the law (Figure 2). Next, regressions including state and year fixed effects help identify whether compulsory schooling effects differ by social background.

$$\begin{aligned}\text{Attendance Rate}_{ijk} &= a + \text{DadOcc}_k + \beta_1 \text{CompSchool}_{ijk} + \beta_{2k} \text{CompSchool}_{ijk} * \text{DadOcc}_k \\ &\quad + \beta_3 X_{ijk} + u_i + \lambda_j + \varepsilon_{ijk} \qquad \text{(S1)}\end{aligned}$$

In equation S1, attendance rate is the proportion of youth enrolled in school in state *i* and year *j* for each class *k*; DadOcc includes indicators for each occupational category or status group;

CompSchool indicates whether a particular state has made school attendance compulsory at each year; X represents controls for factors related to the timing of compulsory schooling laws; and u and λ include indicators for each state and census year. The parameters of interest are β_{2k}, which estimate the relationship between a compulsory law and attendance for each class or status group. Separate regressions substitute foreign or non-white indicators for father's occupation. Results shown are weighted by state male and female school-age population size, but results are similar without weights. Table S6 includes descriptive statistics for the state level analyses, excluding the state-year observations with missing data (11%).

State-Level Results

Tables S7A and S7B show state level regression results. As in individual analyses, the interaction terms suggest the laws significantly reduced differences in school attendance by social background – whether measured in terms of father's occupational status or category, individual nativity status, or race. When class is measured categorically, the effect of compulsory laws on children of lower non-manual workers is not significantly different from the main effect on upper non-manual children (the omitted category). However, the laws significantly increased attendance of manual and agricultural classes more than children of upper non-manual fathers, with the largest effect on children of non-skilled manual workers. The laws significantly reduced attendance gaps by status, nativity, and race as well. Thus, as in the individual level analyses, attendance rates are more similar after the law than before. Contrary to individual analyses, however, results are similar by gender.

According to Raftery and Hout (1993), compulsory laws should only increase equality once attendance has reached saturation among upper class children. To address this possibility, I create a state-level measure of saturation – measured in each state at each census year, separately by gender – which indicates whether the state attendance rate among high SEI or upper non-manual boys or girls is at least 95%. Including this indicator as a control in state-level models yields similar results. In addition, I investigate whether any equalizing effects of the compulsory laws depend on saturation by including a three-way interaction between compulsory law, saturation, and class, nativity, or race (and all two-way interactions). In only two cases was there a significant three-way interaction (the three-way interactions of foreign and non-skilled manual background with compulsory and upper non-manual saturation), both occurring only when predicting attendance of girls but not boys. In all other cases, equalizing effects of the laws were unrelated to saturation of either upper non-manual or high SEI children. Thus, while the ability of compulsory laws to increase equality of attendance among girls in some cases depends on saturation among upper class girls, for the most part compulsory laws increased equality of attendance regardless of upper class saturation.

Model 5 in Table S7A represents the naïve approach, estimating the average treatment effect with no interaction terms. As in past research (Landes and Solmon 1972), this model suggests early compulsory laws had no effect on overall attendance rates for boys or girls. However, as shown in Models 1 through 4, this average treatment effect masks the equalizing effect of the laws – the "hidden gains" for lower class, foreign, and non-white children.

Table S7B compares state level results in Southern and non-Southern states. Outside the South, compulsory laws increased equality of attendance by occupational status, occupational category, nativity, and race among both boys and girls. In the South, however, compulsory laws did not increase equality by nativity. In addition, the laws had a smaller equalizing effect by race in the South, reducing the attendance gap by 64% (0.158 ÷ -0.245 = -0.64) in the North, but only 42% (0.083 ÷ -0.198 = -0.42) in the South. At the same time, compulsory laws often had a larger equalizing effect by father's occupational status and category in the North than the South. For example, among sons of fathers with low-SEI occupations, the attendance rate gap between them and sons of high-SEI fathers decreased about 67% (0.075÷-0.112=-0.67) with the laws outside the South compared to 50% (0.117÷-0.236=-0.50) in Southern states.

These results suggest that compulsory laws carried more robust equalizing potential in the North. The laws increased equality in both the North and the South, but along different dimensions. In the South, they promoted equality of attendance by class and race, but not nativity status. In the North, compulsory laws promoted equality along all dimensions, and in most cases more strongly than in the South. Thus, the same policy could increase equality more strongly in one context more than another (e.g., North vs. South) and along different dimensions across contexts. These results suggest the equalizing potential of an educational policy depends not just on context but also on the dimension of inequality examined.

Table S1: Year of First Compulsory School Attendance Law by State

State	**Compulsory School Year**
Massachusetts	1852
District of Columbia	1864
Vermont	1867
Michigan, New Hampshire, Washington	1871
Connecticut, New Mexico	1872
Nevada	1873
California, Kansas, New York	1874
Maine, New Jersey	1875
Wyoming	1876
Ohio	1877
Wisconsin	1879
Illinois, Montana, N./S. Dakota, Rhode Island	1883
Minnesota	1885
Nebraska, Idaho	1887
Colorado, Oregon	1889
Utah	1890
Pennsylvania	1895
Hawaii, Kentucky	1896
Indiana, West Virginia	1897
Arizona	1899
Iowa, Maryland	1902
Missouri, Tennessee	1905
Delaware, North Carolina, Oklahoma	1907
Virginia	1908
Arkansas	1909
Louisiana	1910
Alabama, Florida, South Carolina, Texas	1915
Georgia	1916
Mississippi	1918
Alaska	1929

Sources: U.S. Bureau of Education 1914: 10; U.S. Bureau of the Census 1924: 22; Steinhilber and Sokolowski 1966.

Table S2: Descriptive Statistics – Individual Level Analysis

	Young Men				Young Women			
	All Mean	Std. Dev.	Comp Mean	Non-Comp Mean	All Mean	Std. Dev.	Comp Mean	Non-Comp Mean
Individual Characteristics								
Comp School	0.58	0.49	1.00	0.00	0.58	0.49	1.00	0.00
In School	0.77	0.42	0.86	0.62	0.77	0.42	0.88	0.62
Father's SEI	21.83	18.84	23.82	18.67	21.99	18.96	24.21	18.88
Upper Non-Manual*	0.10	0.30	0.11	0.07	0.10	0.30	0.11	0.08
Lower Non-Manual*	0.04	0.20	0.05	0.02	0.04	0.20	0.06	0.02
Skilled Manual*	0.16	0.37	0.20	0.10	0.17	0.37	0.21	0.10
Non-Skilled Manual*	0.20	0.40	0.23	0.17	0.21	0.40	0.23	0.17
Agriculture*	0.50	0.50	0.41	0.64	0.49	0.50	0.39	0.63
Foreign	0.01	0.11	0.01	0.01	0.01	0.11	0.01	0.01
Non-White	0.10	0.30	0.07	0.16	0.10	0.30	0.07	0.16
Age	9.33	2.29	9.22	9.30	9.31	2.29	9.34	9.27
Rural	0.70	0.46	0.60	0.85	0.69	0.46	0.59	0.84
Group Quarters	0.00	0.02	0.00	0.00	0.00	0.02	0.00	0.00
Cohort (5-yr intervals)	1888	22.39	1901	1869	1889	22.33	1902	1870
Census Year	1896	22.26	1908	1877	1896	22.22	1910	1877
State Characteristics								
% Illiterate	0.11	0.11	0.07	0.18	Same as for young men			
% White Illiterate	0.07	0.05	0.05	0.10				
% Foreign	0.13	0.10	0.15	0.10				
% Manuf Empl	0.07	0.05	0.08	0.04				
Manuf Prod per Empl	3.49	2.69	4.60	1.68				
% Elderly	0.04	0.02	0.04	0.02				
% Adjacent States	0.58	0.43	0.86	0.14				
Democrat Governor‡	0.46	0.50	0.40	0.56				
Years since Union	81.19	33.60	90.18	66.92				
Incarceration Rate†	77.27	41.71	77.62	78.64				
% Non-White Prisoners§	0.25	0.22	0.20	0.32				
N	303293		191431	127753	296403		172631	123772
N*	299972		189931	125745	293032		171319	121713
N‡	302319		191431	126779	295537		172631	122906
N†	302934		191159	127650	296034		172375	123659
N§	291433		185686	120361	284485		168209	116276

Source: 1850-1920 IPUMS Census data. Sample includes 6-13 year old children related to head of household and excludes those missing father's SEI score (10%) or state measures (1%).

Table S3: Comparison of Individuals with and without Father's Occupation Information

	Young Men			Young Women		
	With Father Info	All		With Father Info	All	
	Mean	Mean	Difference	Mean	Mean	Difference
Individual Characteristics						
Comp School	0.58	0.58	0.00	0.58	0.58	0.00
In School	0.77	0.76	-0.01	0.77	0.77	-0.01
Foreign	0.01	0.01	0.00	0.01	0.01	0.00
Non-White	0.10	0.11	0.01	0.10	0.12	0.01
Age	9.33	9.35	0.03	9.31	9.34	0.03
Rural	0.70	0.69	0.00	0.69	0.69	-0.01
Group Quarters	0.00	0.00	0.00	0.00	0.00	0.00
Family Size	6.83	6.72	-0.11	6.84	6.73	-0.11
Number of Siblings	3.66	3.54	-0.12	3.66	3.54	-0.12
Max Household SEI	24.72	24.21	-0.51	24.91	24.43	-0.49
Birth Year	1886.49	1886.46	-0.03	1886.63	1886.59	-0.04
Not Working	0.94	0.94	0.00	0.98	0.98	0.00
Farm Resident	0.46	0.45	-0.02	0.45	0.44	-0.02
Cohort (5-yr intervals)	1888.46	1888.43	-0.03	1888.61	1888.57	-0.04
Census Year	1895.81	1895.81	0.00	1895.95	1895.94	-0.01
State Characteristics						
% Illiterate	0.11	0.12	0.00	Same as for young men		
% White Illiterate	0.07	0.07	0.00			
% Foreign	0.13	0.13	0.00			
% Manuf Empl	0.07	0.07	0.00			
Manuf Prod per Empl	3.49	3.48	-0.02			
% Elderly	0.04	0.04	0.00			
% Adjacent States	0.58	0.58	0.00			
Democrat Governor‡	0.46	0.46	0.01			
Years since Union	81.19	81.48	0.28			
Incarceration Rate†	77.27	77.67	0.41			
% Non-White Prisoners§	0.25	0.26	0.01			
N	303293	337907	10%	296403	330472	10%
N‡	302319	336770	10%	295537	329435	10%
N†	302934	337505	10%	296034	330058	10%
N§	291433	324095	10%	284485	316609	10%

Source: 1850-1920 IPUMS Census data. Sample includes 6-13 year old children related to head of household and excludes those missing state measures (1%).

Young men and women without father information tend to have smaller families, fewer siblings, and lower maximum household SEI scores. They tend to be older, non-white, and less likely to be in school or live on a farm. While nearly all are significant due to the large sample sizes, these differences are tiny and suggest minimal bias from excluding those with absent fathers. Importantly, there is no difference by compulsory assignment.

Table S4: Descriptive Statistics: Educational Attainment – 1940 Census

	Men				Women			
	All Mean	Std. Dev.	Comp Mean	Non-Comp Mean	All Mean	Std. Dev.	Comp Mean	Non-Comp Mean
All; Age 25+								
Comp School	0.78	0.41	1.00	0.00	0.79	0.41	1.00	0.00
Highest Grade Completed	8.60	3.64	9.09	6.86	8.92	3.41	9.37	7.27
Education Level Completed	3.37	1.46	3.55	2.73	3.53	1.43	3.70	2.89
Non-White	0.10	0.30	0.07	0.21	0.10	0.31	0.08	0.20
Age	44.12	14.20	39.96	59.02	44.16	14.44	39.98	59.43
Cohort (5 yr intervals)	1898	14.17	1902	1883	1898	14.43	1902	1883
N	314431		245378	69053	319922		251099	68823
Early States 10-yr RD sample								
Comp School	0.74	0.44	1.00	0.00	0.73	0.44	1.00	0.00
Highest Grade Completed	7.87	3.36	8.02	6.36	8.32	3.02	8.43	8.03
Education Level Completed	3.05	1.31	3.09	2.95	3.25	1.26	3.28	3.14
Non-White	0.01	0.11	0.20	0.26	0.01	0.10	0.01	0.01
Age	70.90	6.67	35.09	56.14	71.25	6.82	68.94	77.65
Cohort (5 yr intervals)	1871	6.83	1907	1886	1871	6.95	1873	1864
N	11582		8604	2978	12693		9323	3370
Non-South								
Comp School	0.88	0.33	1.00	0.00	0.88	0.33	1.00	0.00
Highest Grade Completed	9.21	3.27	9.43	7.69	9.45	3.04	9.64	8.03
Education Level Completed	3.37	1.46	3.68	3.00	3.73	1.32	3.81	3.15
Non-White	0.04	0.20	0.03	0.12	0.04	0.20	0.03	0.11
Age	44.21	14.19	41.50	63.79	44.41	14.50	41.62	64.77
Cohort (5 yr intervals)	1898	14.17	1900	1878	1897	14.49	1900	1877
N	211363		185709	25654	215099		189269	25830
South								
Comp School	0.58	0.49	1.00	0.00	0.59	0.49	1.00	0.00
Highest Grade Completed	7.32	4.01	8.02	6.36	7.81	3.86	8.51	6.80
Education Level Completed	2.90	1.54	3.15	2.57	3.10	1.54	3.35	2.73
Non-White	0.22	0.42	0.20	0.26	0.23	0.42	0.22	0.26
Age	43.93	14.20	35.09	56.14	43.64	14.30	34.90	56.17
Cohort (5 yr intervals)	1898	14.18	1907	1886	1898	14.28	1907	1886
N	103068		59669	43399	104823		61830	42993

Source: 1940 IPUMS Census. Sample excludes foreign born and individuals under age 25 in 1940, who were born more than 10 years after the last compulsory law in 1918.

Table S5: Logit Coefficients from Individual Analysis of School Attendance by Father's Occupation Category, Nativity, and Race

	In School							
	Men				Women			
	1	2	3	4	5	6	7	8
	All States	Early States 10-yr	Non-South	South	All States	Early States 10-yr	Non-South	South
Lower Non-Manual	-0.128 *	-0.128	-0.069	-0.271 *	0.042	0.054	0.011	-0.035
	(0.049)	(0.095)	(0.052)	(0.065)	(0.062)	(0.162)	(0.099)	(0.058)
Skilled Manual	-0.362 *	-0.172 *	-0.214 *	-0.485 *	-0.258 *	-0.013	-0.079	-0.434 *
	(0.048)	(0.086)	(0.070)	(0.053)	(0.059)	(0.093)	(0.062)	(0.078)
Non-Skilled Manual	-0.628 *	-0.308 *	-0.441 *	-0.828 *	0.536 *	-0.195 *	-0.331 *	-0.771 *
	(0.060)	(0.098)	(0.067)	(0.059)	(0.066)	(0.087)	(0.055)	(0.081)
Agriculture	-0.551 *	-0.217 *	-0.280 *	-0.739 *	-0.480 *	-0.084	-0.166 *	-0.719 *
	(0.074)	(0.073)	(0.097)	(0.055)	(0.081)	(0.114)	(0.080)	(0.067)
Foreign	-0.961 *	-0.916 *	-0.899 *	-1.275 *	-0.819 *	-0.691 *	-0.724 *	-1.362 *
	(0.129)	(0.147)	(0.129)	(0.231)	(0.139)	(0.177)	(0.113)	(0.273)
Non-White	-1.001 *	-1.297 *	-1.182 *	-0.938 *	-1.002 *	-1.320 *	-0.981 *	-0.955 *
	(0.062)	(0.199)	(0.074)	(0.062)	(0.054)	(0.228)	(0.083)	(0.055)
Comp School	-0.377 *	-0.028	-0.235 *	-0.071	-0.336 *	0.058	-0.132	-0.179
	(0.087)	(0.225)	(0.093)	(0.204)	(0.101)	(0.245)	(0.111)	(0.137)
Comp*Lower Non-Manual	0.100 +	0.105	0.092	-0.020	-0.064	-0.284	0.019	-0.218 *
	(0.059)	(0.212)	(0.073)	(0.125)	(0.102)	(0.204)	(0.150)	(0.105)
Comp*Skilled Manual	0.183 *	0.213	0.135 *	-0.134	0.091	-0.001	-0.044	0.070
	(0.051)	(0.135)	(0.068)	(0.086)	(0.071)	(0.124)	(0.081)	(0.148)
Comp*Non-Skilled Manual	0.267 *	0.189	0.166 *	0.079	0.177 *	0.054	0.049	0.088
	(0.056)	(0.163)	(0.061)	(0.107)	(0.053)	(0.130)	(0.045)	(0.105)
Comp*Agriculture	0.168 *	0.283 *	0.066	-0.165	0.087 *	0.058	-0.081	-0.085
	(0.067)	(0.120)	(0.093)	(0.115)	(0.061)	(0.112)	(0.069)	(0.114)
Comp * Foreign	0.063	-0.104	0.174	-0.435 *	-0.049 *	-0.041	0.051	-0.396 *
	(0.164)	(0.284)	(0.151)	(0.208)	(0.134)	(0.285)	(0.132)	(0.126)
Comp * Non-White	0.239 *	0.388	0.454 *	0.180 +	0.325 *	0.589 *	0.273 *	0.310 *
	(0.078)	(0.252)	(0.101)	(0.095)	(0.078)	(0.219)	(0.129)	(0.094)
N	299014	26554	193984	105030	292180	25467	190023	102157

* p<.05; + p<.10. Standard errors are adjusted for state level clustering. Controls and sample are the same as in Table 1. Omitted category is upper non-manual. Compulsory laws reduced inequality for some class backgrounds among young men, but only outside the South. The laws significantly reduced gaps by race.

Table S6: Descriptive Statistics: State Level Analysis

	Young Men				Young Women			
	All Mean	Std. Dev.	Comp Mean	Non-Comp Mean	All Mean	Std. Dev.	Comp Mean	Non-Comp Mean
Comp School	0.58	0.49	1.00	0.00	0.59	0.49	1.00	0.00
In School	0.77	0.20	0.88	0.62	0.77	0.19	0.88	0.62
Father's SEI and Occup Cats								
High Father's SEI (>14)	0.36	0.48	0.44	0.24	0.35	0.48	0.44	0.23
Farming Father (SEI=14)	0.43	0.50	0.34	0.56	0.43	0.50	0.34	0.57
Low Father's SEI (<14)	0.21	0.41	0.22	0.19	0.21	0.41	0.22	0.20
Upper Non-Manual	0.10	0.30	0.11	0.07	0.10	0.30	0.11	0.08
Lower Non-Manual	0.04	0.20	0.06	0.02	0.04	0.20	0.06	0.02
Skilled Manual	0.17	0.37	0.21	0.10	0.17	0.38	0.22	0.10
Non-Skilled Manual	0.21	0.41	0.23	0.17	0.21	0.41	0.23	0.18
Agriculture	0.49	0.50	0.39	0.63	0.48	0.50	0.38	0.62
State Characteristics								
% Illiterate	0.11	0.11	0.06	0.18	Same as for young men			
% White Illiterate	0.07	0.05	0.04	0.10				
% Foreign	0.13	0.10	0.16	0.10				
% Manuf Empl	0.07	0.05	0.08	0.04				
Manuf Prod per Empl	3.48	2.68	4.78	1.69				
% Elderly	0.04	0.02	0.05	0.02				
% Adjacent States	0.58	0.43	0.91	0.14				
Democrat Governor†	0.45	0.50	0.38	0.56				
Years since Union	81	34	92	67				
Incarceration Rate‡	77.22	41.79	76.27	78.54				
% Non-White Prisoners§	0.25	0.22	0.20	0.32				
Year	1896	22	1910	1877				
N	851		405	446	847		403	444
N†	839			434	834			431
N‡	842		399	443	838		397	441
N§	825		399	426	819		397	422

Sources: 1850-1920 Census data; ICPSR (1970) Historical Census Data 1790-1970; US Bureau of the Census 1975; Behrens et al. 2003. School attendance and father's occupation measures are aggregated from IPUMS Census data, including 6-13 year old white children related to head of household and excluding foreign born age 8 or over.

N indicates number of state-year-status group observations and excludes those missing state measures (11%).

Descriptive statistics for the state level data sets by EGP category, nativity status, and race are the same but with different numbers of observations (because there are 5 EGP categories, 2 nativity statuses, and 2 racial categories as opposed to 3 status categories).

Table S7A: Coefficients from State Analysis of Attendance Rate by Class, Nativity, and Race

Model		Attendance Rate					
		Young Men			Young Women		
		Coeff		Std Error	Coeff		Std Error
1 SEI	Farm (SEI=14)	-0.065	*	(0.007)	-0.060	*	(0.008)
	Low SEI (SEI<14)	-0.171	*	(0.009)	-0.159	*	(0.009)
	Comp School	-0.055	*	(0.011)	-0.064	*	(0.012)
	Comp School*Farm	0.043	*	(0.010)	0.040	*	(0.010)
	Comp School*Low SEI	0.123	*	(0.011)	0.113	*	(0.011)
	N (state-status group-years)	839			831		
2 EGP	Lower Non-Manual	-0.021		(0.020)	-0.002		(0.020)
	Skilled Manual	-0.074	*	(0.012)	-0.061	*	(0.012)
	Non-Skilled Manual	-0.149	*	(0.011)	-0.136	*	(0.011)
	Agriculture	-0.127	*	(0.010)	-0.117	*	(0.010)
	Comp School	-0.088	*	(0.013)	-0.086	*	(0.013)
	Comp School*Lower Non-Manual	0.015		(0.023)	-0.002		(0.022)
	Comp School*Skilled Manual	0.052	*	(0.014)	0.042	*	(0.014)
	Comp School*Non-Skilled Manual	0.110	*	(0.013)	0.099	*	(0.013)
	Comp School*Agriculture	0.087	*	(0.012)	0.080	*	(0.012)
	N (state-occupation group-years)	1371			1370		
3 Nativity	Foreign	-0.137	*	(0.019)	-0.126	*	(0.019)
	Comp School	-0.014	*	(0.011)	-0.018	*	(0.011)
	Foreign*Comp School	0.094	*	(0.025)	0.075	*	(0.025)
	N (state-nativity group-years)	532			535		
4 Race	Non-White	-0.212	*	(0.011)	-0.214	*	(0.010)
	Comp School	-0.023	*	(0.011)	-0.029	*	(0.011)
	Non-White*Comp School	0.119	*	(0.016)	0.141	*	(0.016)
	N (state-race group-years)	522			531		
5 Naïve	Comp School	-0.012		(0.011)	-0.016		(0.011)
	N (state-race group-years)	522			531		

* p<.05; + p<.10

Sources: 1850-1920 Census data; ICPSR (1970) Historical Census Data 1790-1970; US Bureau of the Census 1975; Behrens et al. 2003.

State level regression models in Table S7A include state and year fixed effects, as well as controls for state measures related to the timing of compulsory laws prior to 1895. Controlling for measures related to timing of the laws in all states gives the same results. States are weighted by the population of young men or women ages 6-13, but results are similar without weights.

Each model represents a separate regression. Model 5 shows the naïve approach with no interaction.

Table S7B: Coefficients from State Analysis of Attendance Rate in Non-Southern and Southern States by Class, Nativity, and Race

Model		Attendance Rate			
		Young Men		Young Women	
		Non-South	South	Non-South	South
1 SEI	Farm (SEI=14)	-0.022 *	-0.117 *	-0.020 *	-0.106 *
	Low SEI (SEI<14)	-0.112 *	-0.236 *	-0.107 *	-0.213 *
	Comp School	-0.029 *	-0.058 *	-0.041 *	-0.056 *
	Comp School*Farm	0.012	0.033 *	0.009	0.029 +
	Comp School*Low SEI	0.075 *	0.117 *	0.070 *	0.107 *
	N (state-status group-years)	533	306	526	305
2 EGP	Lower Non-Manual	-0.016	-0.046	-0.012	-0.012
	Skilled Manual	-0.043 *	-0.108 *	-0.027 *	-0.100 *
	Non-Skilled Manual	-0.092 *	-0.232 *	-0.077 *	-0.220 *
	Agriculture	-0.050 *	-0.212 *	-0.035 *	-0.206 *
	Comp School	-0.041 *	-0.105 *	-0.038 *	-0.111 *
	Comp School*Lower Non-Manual	0.014	0.028	0.010	-0.005
	Comp School*Skilled Manual	0.031 *	0.046 +	0.014	0.062 *
	Comp School*Non-Skilled Manual	0.063 *	0.142 *	0.049 *	0.138 *
	Comp School*Agriculture	0.029 *	0.090 *	0.013	0.098 *
	N (state-occupation group-years)	872	499	872	498
3 Nativity	Foreign	-0.158 *	-0.045	-0.146 *	-0.073
	Comp School	-0.015	-0.016	-0.021 *	-0.016
	Foreign*Comp School	0.124 *	-0.095	0.104 *	-0.089
	N (state-nativity group-years)	351	181	353	182
4 Race	Non-White	-0.245 *	-0.198 *	-0.231 *	-0.203 *
	Comp School	-0.012	-0.038 +	-0.020 *	-0.043 *
	Non-White*Comp School	0.158 *	0.083 *	0.157 *	0.109 *
	N (state-race group-years)	329	193	337	194

* p<.05; + p<.10

Sources: 1850-1920 Census data; ICPSR (1970) Historical Census Data 1790-1970; US Bureau of the Census 1975; Behrens et al. 2003.

State level regression models in Table S7B include state and year fixed effects, as well as controls for state measures related to the timing of compulsory laws prior to 1895. Controlling for measures related to timing of the laws in all states gives the same results. States are weighted by the population of young men or women ages 6-13, but results are similar without weights.

Each model represents a separate regression, predicting aggregate state attendance rates by category.

Endnotes

[1] While important, I do not investigate changes in the quality of schooling received (Lucas 2001). Potential compulsory effects on quality could counteract equalizing effects on attendance or attainment, but are beyond the scope of this paper.

[2] Preliminary analyses investigate state characteristics that best predict the timing of the first compulsory school attendance law in each state (results available upon request). These characteristics are controlled in this analysis. Among states with early (pre-1895) laws, these include: % white illiteracy, % manufacturing employment, % foreign, % elderly, % adjacent states with the law, years in the union, a governor from the Democratic Party, and manufacturing production per employee. Among all states, the best-fitting model includes an indicator for states in the Southern census region, % illiterate, % manufacturing employment, % non-white prisoners, and incarceration rate. For simplicity, all models control for measures best predicting early laws, but results are similar when controlling for measures that best predict the timing for all states.

[3] Compulsory laws most often applied to ages 8 to 14. This sample excludes older children because employment experience was valued for young adults, leaving little influence for schooling laws. A central goal of these laws was to prevent truant youth without jobs from being on the street. As long as they had some schooling, compulsory attendance was not likely enforced among older (over age 13), employed youth (Tyack 1974).

[4] Literacy is an alternate measure and would address questions of skills gained from schooling. However, literacy is not recorded for children in early censuses and is nearly universal among adults in the 1940 sample.

[5] Ganzeboom et al. (1992) note several advantages to continuous measures of occupational status and find that their international socio-economic index compares favorably with the classic Erikson, Goldthorpe, and Portacarero (EGP 1979) scheme. Specifically, Ganzeboom et al. suggest that: 1) some EGP categories are not internally homogeneous, with different mobility chances for some occupations in the same EGP category; 2) multivariate analysis is more feasible with continuous measures; and 3) log-linear analyses can scale EGP categories on one dimension and measure class distance without much loss of information. Nevertheless, there are also important reasons to incorporate categorical analyses (Wright 1980; 1985). For example, the high proportion of farming families could skew results using continuous measures. Analyses of both continuous and categorical measures provide a better proxy of class; categorical results are in the appendix.

[6] Morgan and Tang provide EGP categories for the 1980 and 1990 census occupation codes. The IPUMS census data include 1950 census occupation codes. I convert 1950 to 1990 census codes using the 1950 census, which includes standardized occupation codes for both 1950 and 1990. Like Morgan and Tang, I am unable to distinguish managers by how many workers they supervise; EGP category one may therefore include more low-level managers than appropriate. The early censuses do not include self-employment information; like Morgan and Tang's labor market earnings analysis, I am therefore unable to distinguish self-employed individuals and create no EGP category IV.

[7] When estimating effects of the laws in all states, the sample is limited to those at least age 25 in 1940, to drop as many irrelevant cases while still ensuring that there are some individuals in each state who were required to attend school during all of their school age years. For example, Mississippi was the last state to pass the law (in 1918). Individuals age 25 in 1940 would have been 3 when the law was passed if they lived in Mississippi, and required to attend from the time they became school age. Results are similar using different age cutoffs.

[8] Three-way interactions between compulsory law, father's SEI, and nativity or race are not significant (when including all two-way interactions) and are not included in the model.

[9] State and cohort indicators are higher for those required to attend school, so the predicted likelihood of attendance is higher among youth required to attend school, despite the negative estimate of compulsory assignment.

[10] Many thanks to Jeff Manza, Christopher Uggen, and Angela Behrens for generously sharing their data.

[11] Because there are 3 status groups (low, farm, and high SEI scores), there are 3 measures for each state in each census year. The 1890 individual census was burned in a fire, so observations from that year are missing. Therefore, there are at most 1,008 possible state-status-year observations (7 censuses x 3 status groups x 48 states – excluding Alaska and Hawaii). However, because several states were formed in the period from 1850 to 1920 (e.g., Arizona in 1912 and Oklahoma in 1907), data are not available for them in early years. The number of state-status-year observations is therefore less than 1,000.