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Laws, Educational Outcomes, and Returns to Schooling: Evidence from the First Wave of U.S. State Compulsory Attendance Laws

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The nineteenth and twentieth century saw two waves of state schooling laws. The first wave focused on children to age 14 and the second wave focused on high school. Using the full count 1940 census and a new coding of state laws, this paper provides new estimates of the effects of the first wave of laws. The analysis focuses on cohorts of prime working age between 1910 and 1940. IV estimates of returns to schooling range from 0.067 to 0.077. Quantile IV estimates show the returns were largest for the lowest quantiles, and were generally monotonically decreasing for higher quantiles.

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Introduction

The nineteenth and twentieth century in the U.S. saw two nationwide waves of state-level schooling requirements. The first wave generally focused on the educational attainment of children up to age 14, and the second wave focused on high school attendance. The second wave has been the focus of a substantial literature, which has both examined the causal effects of the laws on years of schooling and the returns to these additional years (Lleras-Muney 2002; Goldin & Katz 2011; Stephens and Yang 2014). The first wave has received considerably less attention, even though it laid the foundation for the second wave. Further, individuals subject to first wave laws were prime age workers during 1910-1940, a period of rapid transition in the American economy.

Did the implementation and expansion of this first wave of schooling laws increase educational attainment? If so, what were the returns associated with the additional years of schooling? These questions matter not only for historical analysis, but for other developing nations seeking to draw on the historical experience of the United States.¹ Drawing on the full count 1940 census and a new detailed coding of state compulsory attendance, continuation school, and child labor laws, this paper provides new estimates of the effects of laws on the number of years of schooling and the returns to schooling. As with prior work that exploits variation in state schooling requirements based on state and year of birth (e.g., Angrist and Krueger 1991; Acemoglu and Angrist 2000;

¹ An increasing literature on compulsory attendance in parts of the less developed world outside the United States has emerged in recent years (e.g., examination of Turkey in Kirdar et al. 2018; examination of Argentina in Edo et al. 2017). The historical setting of the United States in expanding compulsory attendance through common school ages is likely to be informative for these modern legal developments, although perhaps more relevant would be the effect of adopting compulsory high school attendance.

Lleras-Muney 2002; Oreopoulos 2006; Stephens and Yang 2014), the analysis is limited to native-born individuals. Both for comparability with much of the prior literature and in recognition of the fact that many institutional factors (e.g., Jim Crow laws, lagging school quality for blacks, Rosenwald schools, and differing employment opportunities across gender and racial lines for teenagers, such as cultural norms against work outside of the home for women) must be carefully addressed to accurately assess the impact of schooling and child labor laws on the economic outcomes of blacks and women, the sample is further restricted to white males.² Thus, the analysis focuses on native-born white men from the 1885-1912 birth cohorts who are age 27 to 54 when the 1940 Census was enumerated.

The key source of identifying variation in this paper is the number of years of schooling a child is required to attend. This schooling requirement is calculated for an individual from a given state born in a specific year based on the prevailing compulsory schooling, child labor, and continuation schooling laws throughout their schooling years. The information on schooling laws builds on work by previous authors (Lleras-Muney 2002 and Goldin and Katz 2011) by extending the coding of laws back to earlier time periods (pre-1910). For the post-1910 period, the coding of the laws builds on the work of Stephens and Yang (2014) by revisiting original session laws and reconciling differences between these sources and the coding of the schooling and child labor laws found in prior studies.

² In this work, we extend the diversity of the analysis to nativity status for white males. Future work will examine the impact of schooling laws for other groups. This is not to minimize the importance of studying the effect of laws across gender and race, but rather to give those groups their full due. For an example along these lines examining Rosenwald schools, see Aaronson and Mazumder (2011).

The 2SLS estimates of the returns to schooling found here range from 0.067 to 0.077. Excluding Southern born men for whom the trend in schooling laws and educational attainment was growing most rapidly (Stephens and Yang 2014), the IV estimate is only slightly smaller, at 0.064. Unlike the prior literature, this paper also presents quantile IV estimates to examine the impact of additional schooling across the earnings distribution. The results show that the returns to schooling are largest for the lower quantiles with estimates of 0.14 and 0.11 at the 25th and 50th quantiles, respectively. However, the estimated returns generally decrease monotonically for higher quantiles, e.g., falling to -0.00 for the 75th quantile.

The paper contributes to multiple literatures. The first is the literature on the returns to schooling. A number of papers including Angrist and Krueger (1991), Oreopoulos and Salvanes (2011), and Stephens and Yang (2014) have primarily used second wave schooling laws to estimate returns to schooling. For example, Oreopoulos and Salvanes (2011) estimate effects for the 1899-1982 cohorts and Stephens and Yang (2014) estimate effects for the 1905-1954 cohorts. This paper provides new estimates for the 1885-1912 cohorts which were primarily affected by the first wave of schooling laws.

The second is the literature on the effects of schooling laws on years of schooling. This paper is closest to Lleras-Muney (2002), Goldin and Katz (2011), and Lleras-Muney and Shertzler (2015) although it is distinct from these papers in important ways: i) a focus on earlier birth cohorts, ii) use of the full count 1940 census instead of the much smaller IPUMS samples; iii) use of a new detailed coding of state compulsory attendance, continuation school, and child labor laws which

builds on and extends prior datasets; and iv) use of a more flexible specification for estimating the effects of the laws. Even with these differences, this paper finds small but significant effects of schooling laws on years of schooling as have been found in the prior literature.

The paper is set out as follows. The next section provides background on the development of compulsory attendance, continuation schooling, and child labor laws in the United States. The following two sections discuss the data and the empirical methodology, respectively. The paper then turns to the empirical findings before concluding.

1. Compulsory Attendance, Child Labor, and Continuation Schooling Laws

Compulsory attendance laws commonly stated an age at which students had to begin attending school, an age at which they could leave, and a minimum number of weeks a child had to attend. Initial laws often used ages eight and fourteen as the entry and exit ages, respectively, and these ages were lowered and raised over time. Many states exempted children for a variety of reasons including those who had attended for a certain number of years or those who had completed a specific schooling course (e.g., “the common school course”). Furthermore, numerous states changed these exemptions over time. The online Data Appendix gives further detail and contains examples of the laws.

Compulsory attendance laws were often complemented with child labor laws that exempted working children from full-time school attendance. These labor laws let employed children stop attending school before the exit age found in the compulsory attendance law, usually after the child

had attained a certain level of education. Children were typically allowed to leave school to work at age fourteen. A few states had earlier ages, but most of these states later raised the age at which a working child was allowed to leave school. Child labor laws affecting school attendance were one of many different kinds of laws targeting employed children. For example, many states used age-based restrictions for employment in variety of industries, restricted work hours, and regulated working conditions in a variety of ways. This paper focuses on child labor laws that exempted children from full-time school attendance.

States often required these working children to attend continuation school, meaning part-time or evening school that supplemented their employment.³ Like the compulsory attendance laws, continuation school laws required attendance until a certain age, and often exempted children with a certain number of years of education.

The resources and mechanisms provided to enforce the laws varied. At the most effective level, a state might provide for truant officers with specific funds set aside to pay their wages. States without truant officers tasked teachers or the existing police with enforcement, or allowed taxpayers to sue local school districts for non-enforcement. Some states provided no enforcement mechanism in the initial law but strengthened it as time passed. States also varied on whether the law was required to be published, what levels of fines were imposed upon violation, and whether imprisonment of parents was explicitly provided for in the law.⁴

³ See Appendix Figure 1, showing the requirements of continuation schooling laws by state.

⁴ See Katz (1976) and Ensign (1921) (discussing enforcement).

Figure 1 shows when initial compulsory attendance laws, child labor laws affecting school attendance, and continuation schooling laws went into effect across the United States. The first compulsory attendance law was passed in Massachusetts in 1852.⁵ Attention to and debate about schooling heightened in 1871, as the Republican Party kicked off a “public school crusade.”⁶ By 1900 almost all states outside the South had compulsory attendance laws and by 1920 every state had a compulsory attendance law.⁷ Following the enumeration of the 1900 Census, public awareness that one-quarter of children ages 10 to 15 were employed led to a flurry of state level child labor reforms (Puerta 2011). As child labor laws were revisited, they began to integrate with compulsory attendance laws, such as exempting working children from school attendance in some states. Relatedly, while only a handful of states had continuation school laws before 1911, this number increased rapidly in the late 1910s due to the vocational education requirements of the Smith-Hughes Act of 1917 and additional public focus on child labor following World War I

⁵ Compulsory *attendance* laws for purposes of this paper are not to be confused with the variety of compulsory *instruction* laws that dated back to colonial times. For example, colonial instruction laws tasked town selectmen to ensure parents were raising literate children but did not mandate school attendance. See Cook (1912) and Ensign (1921).

⁶ See McAfee (1998).

⁷ Although 1918 is the traditional date for the passage of the last compulsory attendance law (in Mississippi), it was not until 1920 when all the Southern states had non-local option laws. See the online Data Appendix for further notes on county option laws and statewide adoption. For simplicity, the District of Columbia will be referred to as a state.

(Mayman 1933).⁸ Appendix Figure 1 graphs the changes in years required attendance, by state, for the 1885 to 1912 birth cohorts.

Even allowing for some inflation of self-reported schooling, it is clear that the laws were unlikely to have been binding for most children.⁹ The majority of children in the earliest cohorts were already completing between six and eight years of schooling, which were the typical numbers of years that students would complete by age fourteen. These relatively high levels of schooling -- even with voluntary attendance -- are consistent with historical evidence that laws were only passed after most children were already attending school (Landes and Solmon 1972; Clay, Lingwall, Stephens 2012).

2. Data

Attendance, Child Labor, and Continuation Schooling Laws

Data on compulsory attendance laws, child labor laws, and continuation schooling laws were gathered from the session laws of individual states. This new database captures each compulsory

⁸ Continuation schooling laws were of two types: either mandating that districts provide these schools (typically upon reaching a threshold for the minimum number of eligible students) or giving districts the option of setting up these schools and requiring attendance. Goldin and Katz (2002) combine both types of laws in their empirical analysis and designate such laws as “permissible.” Following their approach, the analysis in this paper uses “permissible” continuation schooling laws. The Smith Hughes Act required “... that at least one-third of the sum appropriated to any state for the salaries of teachers of trade, home economics, and industrial subjects shall, if expended, be applied to part-time school over fourteen years of age who have entered upon employment.”

⁹ As Goldin (1999) notes, respondents were not always accurately reporting educational attainment.

attendance age limit, years of schooling exemption, child labor exemption, continuation school requirement, and continuation school exemption in force in each state and year from 1880 to 1930. Our work builds on prior compilations in the United States Bureau of Education (various years), Goldin and Katz (2002), Lleras-Muney (2002), Moehling (1996), Eisenberg (1988), and Stephens and Yang (2014).¹⁰ The dataset attached in the online Data Appendix provides the dates, requirements, and original source for each law.

Following Stephens and Yang (2014), this paper calculates the years of required attendance for each state-year birth cohort. By using an iterative process that calculates whether attendance was required at each year of life, based on the age limits and exemptions in place in that year, the cumulative years each child was required to be in school is derived. The online Data Appendix describes the steps in this calculation, showing the interaction between the various exemptions and age limits regulating attendance. Continuation schooling requirements are determined similarly. Assuming children entered school at the required age, attended each required year, left to work

¹⁰ It is worth noting that states were only coded as having a law once the law (1) covered all counties or required counties to specifically opt-out, and (2) went into effect. The first issue arises because a number of Southern states passed laws merely *permitting* counties to enact compulsory attendance. Few counties actually made use of this law and passed compulsory attendance laws. These opt-in states later passed universal laws that covered all counties. The second issue arises because many states wrote “effective dates” into their laws different than the date of passage. With these coding conventions, the year of first passage for twelve states is later than dates conventionally used in the literature. See the online Data Appendix for further discussion, and the dataset attached in the online Data Appendix for complete sources.

once possible, and attended continuation school when required, this calculation gives the number of years each child was obligated to attend.¹¹

In the main specifications, compulsory school attendance and continuation schooling laws are treated together in our analysis, as these laws formed part of the same comprehensive statutory scheme for the education of children. Continuation schooling laws complemented work permit laws that excused working children from full-time school attendance, and often required part-time attendance through the exact age limits faced by unemployed children. Results from specifications that separate the effects of continuation school laws from those of compulsory attendance and child labor laws are presented in the Appendix.

Educational and Work Outcomes

The data are from the 1940 Preliminary Complete Count Census of Population Data available from IPUMS USA (Ruggles et al. 2015). The 1940 Census was the first census to ask about years of completed schooling and income. The census contains information on individuals' sex, race, state of residence, state of birth, and age. Information on parental birthplace is available for sample line individuals.¹² Given the focus on returns to schooling in this paper and for comparability for with much prior research, the sample is limited to native-born white males, leaving a sample size of

¹¹ Appendix Figure 1 plots the number of years of required schooling by year of birth for compulsory school attendance laws and for the combination of school attendance and continuation schooling laws for each state.

¹² The sample line is a five percent random subset of individuals in the 1940 Census for whom an additional set of questions was asked relative to the remaining population.

roughly 19 million.¹³

The census also contains information on highest grade attended and on wage income, but not on income from self-employment. Individuals were asked to report the highest grade completed. As Goldin (1999) notes, they were not always reporting accurately. “I have recently demonstrated that the 1940 census greatly overstates the proportion of Americans who were high school graduates.”¹⁴ To anticipate the estimation strategy, assuming the proportion of people in a given state overstating their education is either constant or trending similarly within a region, the overstatement should have limited impact on estimates of the effect of compulsory attendance laws on schooling outcomes. This issue is discussed further in section 4.

Our sample covers the 1885-1912 birth cohorts, who were 27-54 at the time of the 1940 Census.¹⁵

This age restriction is intended to capture prime age working men and exclude older men who might be retired or working part time. The initial estimates of the impact of schooling laws on educational attainment include all men in this age range, regardless of work status.

To estimate the returns to schooling, some additional restrictions were imposed. First, a “working men” sample is created limited to those who report both positive wage earnings and positive weeks worked in 1939. Second, due to the relatively high levels of agricultural employment during this

¹³ Future work will examine the effects of these laws and other educational policy interventions by race and gender.

¹⁴ Goldin (1999), p. S67.

¹⁵ Year of birth is calculated as 1940-age-1, because the census was taken in April and most individuals had not yet had their birthday. The 1940 Census does not include quarter of birth information that can be used to more accurately compute year of birth in other Censuses.

period, including working on one's own farm, earnings may be understated for a non-negligible subset of men. This is because the 1940 Census did not collect information on self-employment income. As such, a sample of "restricted working men" is used which further requires that the men worked at least 40 weeks in 1939, worked but were not self-employed, and did not live on a farm.

The dependent variable in our returns to schooling specifications is the log of weekly wages. The weekly wage is the ratio of annual wage and salary income to annual weeks worked. Both are measured for the prior calendar year, 1939, which had the typical labor market of the 1930s (see Cole and Ohanian 2002). Following Acemoglu and Angrist (2000), annual earnings are censored at the 98th percentile, and values above the 98th percentile are replaced with 1.5 times the 98th percentile value.

Summary statistics for these samples are presented in Table 1. The average age of men in the samples is between 38 and 39 and the average years of schooling is between 9 and 10. The distribution of required years of schooling shows that most common number of years was eight, followed by six and then seven. Of the observations in the nine or more years category, over 96 percent are required to attend school for nine years while the remainder have a ten year requirement.

3. Empirical Methodology

This paper uses an instrumental variables (IV) strategy to estimate the returns to schooling using

years of required schooling, based on state and year of birth, as an instrumental variable for reported educational attainment. OLS estimates of returns to schooling are typically regarded as being inconsistent, since years of schooling is generally considered to be a choice variable that is likely correlated with the error term in the weekly wage equation. For example, more motivated individuals tend to stay in school longer and, to the extent that motivation leads to higher levels of productivity, are paid more by employers. As such, returns to are typically estimated using empirical methods that account for the endogeneity of education (Card 1999).

The main specification for estimating the returns to schooling is

$$w_{isc} = \beta_0 + \beta_1 Educ_{isc} + \gamma_s + \alpha_{sc} + \varepsilon_{isc} \quad (1)$$

where w_{isc} is the log weekly wage of individual i , born in birth cohort c , and in state s , $Educ_{isc}$ is their years of education, γ_s is a vector of state fixed effects, and α_{sc} is a vector of birth cohort fixed effects for each of the four census regions. All estimates have standard errors clustered at the state-cohort level.¹⁶

Our first stage specification is

$$Educ_{isc} = \pi_0 + \pi_1 LawYears_{sc} + \theta_s + \tau_{sc} + u_{isc} \quad (2)$$

¹⁶ Appendix Table 11 shows results using alternative clustering at the state level only. Given the low first stage F- statistics with state-level clustering, we then provide weak-instrument IV estimates.

where $Educ_{isc}$ is the educational attainment of individual i , born in birth cohort c , and in state s , $LawYears_{sc}$ is the years of required attendance for someone born in state s in birth cohort c , θ_s is a vector of state fixed effects, and τ_{sc} is a vector of birth cohort fixed effects for each of the four census regions.

In the IV estimates presented below, $LawYears$ is included as a set of indicator variables because the effect of requiring one more year of schooling may not be linear throughout the distribution of required years of schooling. One to five years of required schooling are grouped together into a single dummy variable, because this primarily captures the initial adoption of laws. Older children at the time the laws are adopted may be subject to a just one or two years of required attendance. Separate dummy variables are included for six, seven, eight, and nine or more years of required schooling.

The effects of the laws are identified by variation in the timing of various statutes within state and region over time. Region by year of birth cohort fixed effects are included so the counterfactual is that the changes in state educational outcomes would have been the same as other states in the same region. Stephens and Yang (2014) use this approach to identification to examine educational outcomes in later censuses. This approach is also similar to Lleras-Muney (2002) and Goldin and Katz (2011) in using retrospective census data and including state and cohort fixed effects and regional controls.

A significant concern in the literature is that laws are being passed and educational outcomes are rising, but the laws are not causing educational outcomes to rise. This is central to Landes and Solmon's (1972) critique and has been investigated by other authors, notably Lleras-Muney (2002). Our estimation approach is designed to address this endogeneity by including both state fixed effects and region-cohort effects, so changes within state over time are identifying effects and states are being compared to other states in their region.

4. Results

Impact of Laws on Educational Attainment

Table 2 presents estimates of the impact of required schooling on educational outcomes. The Table uses three samples: i) all native-born white men, ii) native-born white men who reported positive earnings and positive weeks worked in the prior year ("working men" sample), and iii) a restricted sample which also excludes the self-employed, those who lived on a farm, and those who worked less than 40 weeks in 1939 ("restricted working men" sample). For each sample, as described above, the measure of required years of schooling is calculated based on the prevailing compulsory attendance, child labor, and continuation school laws.

As shown in Table 2, the relationship between schooling requirements and educational attainment is non-linear. For example, for working men in column (2), the effects of being required to attend one to five years and six years of schooling are 0.041 years and 0.047 years. The effects of being required to attend seven and eight years are 0.099 and 0.124, which are substantial increases

relative to six years. The effect of being required to attend nine years is 0.244, which is again a substantial increase relative to eight years.¹⁷ In comparison, the linear estimates would overestimate the effects of six, seven, and eight years of education and underestimate the effects of nine years of schooling.

Figure 2 provides detail showing where the schooling laws impacted the education distribution. The Figure shows results from regressing the categorical law variables on a series of binary outcomes for whether the individual completed at least X years of schooling, where X is shown in the horizontal axis. Thus, the Figure shows the impact of the laws on one minus the education CDF. Full results are presented in Appendix Table 1. Each line in the Figure shows the effect for a given categorical variable, while the estimates found at each point on the horizontal axis represents the results from a different regression.

The results in Figure 2 show that laws requiring six, seven, and eight years of schooling increase the number of individuals reporting they completed six, seven, or eight years of schooling. There are much smaller effects for lower and higher levels of reported education. This is what would be

¹⁷ While a schooling requirement of nine years may seem odd, as one year above a traditional eight-year common school course, and less than four additional years of high school, it arises in this dataset because of continuation school and several idiosyncratic state laws, such that required nine years for graduation from common school, rather than the usual eight. Around 5% of cohorts in our time period had laws which would have required children to attend past eighth grade before dropping out to work or otherwise be excused from school. These states are Illinois for the 1905-1912 cohorts, Indiana for the 1899-1912 cohorts, Nebraska for the 1904-1912 cohorts, New Jersey for the 1905-1912 cohorts, New Mexico for the 1884-1912 cohorts, Ohio for the 1905-1912 cohorts, Utah for the 1902-1912 cohorts, and Wyoming for the 1875-1891 cohorts. See Appendix Figure 1 for details on each state. As far as these arose from schooling years exemptions to work permit ages, anecdotal historical evidence suggests that exceptions to work permit ages were at least somewhat binding in this general time period (e.g., Children's Bureau 1928, 1932).

expected if the laws were binding. Laws requiring nine or more years of schooling increase the number of individuals who reported completing nine or ten years of schooling. Laws that only resulted in between one and five years of required education, mainly for cohorts in their pre-teenage years when the laws were adopted, have less of an effect.¹⁸

Table 3 summarizes the literature on the effects of compulsory attendance and related laws on educational outcomes. Similar to many – although not all – previous papers, our analysis finds statistically significant but small effects of laws. This paper differs from previous papers in that it uses a dramatically larger census sample, a more detailed coding of the laws regarding years of required schooling, a more flexible specification for estimating the effects of years of required schooling on reported schooling, and earlier birth cohorts.

Estimates of the Returns to Schooling

¹⁸ Appendix Figure 2 provides similar graphs that treat the requirements of compulsory attendance and child labor laws separately from those of the continuation school laws. The point estimates along with their standard errors are shown in Appendix Table 2. For compulsory attendance and child labor laws in Panel A of Appendix Figure 2, the pattern is similar to Figure 2. For continuation school in Panel B, one to two years required attendance has only small effects at any level of completed schooling, while three to four years required attendance has large effects on completion of nine to ten years of schooling, as expected from laws that often sought to provide some form of post-primary education for working teenagers. Goldin and Katz (2011) also find significant effects of continuation school laws on educational attainment after accounting for compulsory schooling and child labor laws. Notably in Appendix Figure 2, base schooling requirements (ignoring continuation school), appear to have spillover effects beyond the required years. Moving children into 8-9 legally required years of schooling increased attendance through completion of high school.

Table 4 presents OLS and IV estimates of the returns to schooling for the two groups of working men reported in Table 2: the working men sample (native-born white men who reported positive earnings and weeks worked last year) and the restricted working men sample (native-born white men who reported positive earnings, worked at least 40 weeks in 1939, working but were not self-employed, and did not live on a farm). In the Appendix we consider additional outcomes such as employment and participation in the labor force.¹⁹ Each regression includes state of birth and region by year of birth fixed effects. The first stage F-statistics, testing the joint null hypothesis for the excluded instruments, are shown at the bottom of the table.²⁰ All of the F-statistics exceed conventional levels. The OLS estimates of the returns to schooling (columns 1 and 3) are quite comparable to those found for similar specifications in subsequent Census years (Acemoglu and Angrist 2000; Stephens and Yang 2014). Because the results for the working men and restricted working men samples are similar, the remaining tables show results for the working men sample only.²¹ Estimates of the returns to schooling (columns 2 and 4) are approximately 8% and 7% and are quite similar to the corresponding OLS estimates.²² It is worth noting that the impact on

¹⁹ Following the prior literature, sample selection corrections to account for labor force participation decisions are not implemented when estimating wage equations for prime age white males. As reported in Appendix Table 3, there is a modest impact of schooling on employment outcomes which suggests that participation decisions are unlikely to confound the estimates of the returns to schooling.

²⁰ The first stage regressions are shown in Table 2.

²¹ Results for the restricted working men sample are broadly similar, and can be found in Appendix Table 4.

²² As an interesting point of comparison, we also have estimated the returns to schooling using the previously available IPUMS 1% 1940 Census sample (Ruggles et al. 2015). The first stage F-statistic indicates the need to account for weak instruments. As such, Appendix Table 5 reports both the LIML estimate and confidence intervals that are constructed using Moreira's (2003)

wages is from reported years of attendance only, as no population-level data on *quality* of those school years exists. Administrative data from the period seem to indicate that quality was generally increasing during this time period, such as the number of teachers per student, funding per student, and so on.²³

Further insight on the effect of the laws can be gained from examining returns to schooling across the 1940 earnings distribution. Figure 3 shows returns to schooling across the distribution of (log) weekly wages by plotting the results of quantile IV regressions for every fifth quantile using a 25 percent random subset of the working men sample.²⁴ Panel A of Appendix Table 7 shows the complete set of quantile IV results for this sample. The instruments are the required years of schooling categorical variables used in Table 4, columns 2 and 4.

The returns to schooling are positive across the weekly wage distribution and are largest for the bottom half of the distribution. The returns are relatively flat at about 0.15 for the 10th to the 45th quantiles, decline monotonically to the 80th quantile, remain roughly constant near zero through the 90th quantile, and then drop again at the 95th quantile. The substantially larger estimates for

conditional likelihood ratio test, which are computed using version 1.0.7 of the `-rivtest-` command for Stata (Finlay and Magnusson 2009). The point estimates are twice as large as those found in Tables 2 and 4 although the confidence intervals are quite broad.

²³ For example, the reports of the U.S. Office of Education during this period regularly reported state-level data on school finance, teachers per student, and so on. For notes on these data, see Cano-Urbina and Lochner (2019).

²⁴ A 25 percent random subset of the working men sample, which still contains almost 3.4 million observations, is the largest sample for which the computationally intensive quantile IV estimator could reasonably be implemented. The first stage, OLS, and IV estimates shown in Appendix Table 8 for the 25 percent random sample are comparable to those for the full working men sample found in Tables 2 and 4.

those at the bottom of the earnings distribution indicates that schooling laws played a role in reducing income inequality as measured in the 1940 Census.²⁵ In addition, since the wage structure recorded in the 1940 census was typical of the years before World War II, larger returns to schooling for those in the lowest quantiles of the earning distribution may help explain the pre-1940 decrease in wage inequality.

Table 5 summarizes the literature on the returns to schooling. In contrast to some papers and consistent with others, this paper finds positive and significant returns to schooling. This paper differs from previous papers in that it uses a larger census sample, a more detailed coding of the laws regarding years of required schooling, a more flexible specification for estimating the effects of years of required schooling on reported schooling, and earlier birth cohorts. It also differs in its focus on the quantile IV.

A relevant comparison for these findings is the recent paper by Stephens and Yang (2014) which uses white men in similar age ranges from the 1960-1980 Censuses but finds insignificant, and even wrong-signed, estimates of the returns to schooling. It is important to note that birth cohorts examined in the current paper were affected along different margins than those examined by Stephens and Yang. As is shown in Figure 2, for the 1885-1912 birth cohorts examined in this paper, the schooling laws primarily had an impact at later stages of common school in years six to

²⁵ Chernozhukov and Hansen (2006) use quantile IV to estimate the returns to schooling across the weekly wage distribution of the using quarter of birth as an instrument and the Angrist and Krueger (1991) sample from the 1980 Census. They find the returns to schooling are rapid declining up to the 40th quantile, decline somewhat more through the 60th quantile and are relatively flat beyond that.

eight. Indeed, none of the Southern U.S. states had compulsory schooling laws for the oldest of these birth cohorts while the youngest birth cohorts experienced laws in virtually all states. For the 1905-1954 birth cohorts examined by Stephens and Yang, compulsory schooling was in place for nearly every birth cohort, and most of the adjustments to schooling laws affected education at higher levels of school.

As noted above, our main specification includes state fixed effects and region-year fixed effects. Much of the prior literature only includes national year effects which may overlook important differential trends across the U.S. We examine the robustness of our findings across different specification choices for the trends in Appendix Table 9. The estimates shown in columns (3) and (4) of this Table uses a specification which replaces the region-year fixed effects with state-specific quadratic time trends.²⁶ The resulting 2SLS estimates are somewhat larger than in our baseline specification, yielding a return to schooling of 0.127 as opposed to 0.077. In the final two columns of this Table, we include the results from implementing the standard choice of including state fixed effects and year fixed effects. The smaller 2SLS estimate (0.017) arising from using this specification in contrast to what we find from using our baseline model, indicates the importance of accounting for differential trends in wages and education across regions in our analysis.

Effects on Groups of Specific Interest

²⁶ Because we are using cross-sectional data, year of birth fixed effects cannot be separately identified from age fixed effects. The well-established concave relationship between earnings and age (e.g., diminishing returns to experience) appears, when plotted, as a concave pattern in the region-year fixed effects. As schooling requirements became more stringent over time, properly accounting for this earnings-age relationship is key for correctly identifying the returns to schooling. State linear trends will not address this concern which is why we present estimates that control for state quadratic trends.

This section discusses results for two groups of specific interest: the full sample excluding the South, and results from the Sample Line Sample of the 1940 Census, which allows an examination of the effect of the laws by nativity. Table 6 show the effect of the laws on schooling for these samples, and Table 7 shows the returns to schooling.

Stephens and Yang (2014) show that the estimated returns to schooling in the 1960-1980 Censuses for white men becomes statistically insignificant and, in some cases, negative in sign when allowing the year of birth fixed effects to vary by Census region. They suggest that the rapid catching up of the South in terms of schooling requirements and school quality, amongst other things, yields the positive and significant estimates of the returns to schooling when not allowing the fixed effects to vary by region. As a result, the specifications used throughout this paper include region by year of birth fixed effects.

Nonetheless, one concern is that the South alone might be driving the results because of its rapid adoption of compulsory attendance laws during this time period. Table 6, columns (1) and (2) re-estimate the first stage regressions in Table 2 but exclude men born in the South. The estimated coefficients are in some cases no longer significant. This reflects the fact that the variation within and across states is now smaller. The coefficients on seven, eight and nine or more years of schooling remain positive and significant. The magnitudes of the coefficients for eight years range from 0.087 to 0.097, whereas in Table 2 they ranged from 0.124 to 0.149. Table 7, columns (1) and (2) shows that the estimated returns to schooling for the non-South sample remain quite similar to the full sample results in Table 4.

The historical literature (Tyack 1974, Ralph and Rubinson 1980) and a recent paper by Lleras-Muney and Shertzer (2015) also raise questions about the extent to which laws were binding for children with foreign-born parents and for foreign-born children. Lleras-Muney and Shertzer examine the impact of contemporaneous schooling and child labor laws on children ages six to sixteen using the 1910-1930 Censuses. They estimate the effects of the laws on enrollment and employment for three groups: native-born children with native-born parents, native-born children with at least one foreign-born parent, and foreign-born children. The effects of the laws are larger for foreign-born children than for either of the two native-born children groups and were nearly identical for both native-born groups of children. The identification strategy implemented here, which uses state of birth to examine the impact of schooling and child labor laws on adult outcomes, can only distinguish between the two native-born groups. As such, the foreign-born are excluded from the analysis.

Parental birthplace is only available for sample line individuals – the five percent of the Census who answered the long form questionnaire. Following Lleras-Muney and Shertzer (2015), the sample line sample is divided into two groups: those for whom both parents are native-born and those who have at least one foreign-born parent.

Table 6, columns (3) and (4) uses parental birthplace information to examine whether the impact of schooling and child labor laws on educational attainment differs between those working men with native-born parents and those with a foreign-born parent. The estimates are only positive and significant for those with native-born parents. For those with a foreign-born parent, the regressions

yield generally insignificant results. Overall, the results consistently show that schooling laws increased educational attainment of native-born men with native-born parents while the findings for native-born men with a foreign-born parent are more tenuous.²⁷

Given the insignificant findings for the impact of required schooling on native-born men with foreign-born parents in Table 6, Table 7 columns (3) and (4) provide returns to schooling for native-born white men with native-born parents in the sample line. The returns are similar to those in Table 4 (the full sample) and Table 7 columns (1) and (2).²⁸

5. Discussion and Conclusion

This paper presents new estimates of the effects of the first wave of state compulsory attendance, child labor, and continuation school laws on years of schooling and new IV and quantile IV estimates of returns to schooling for native-born white men in the 1885-1912 birth cohorts. The results shed light on two questions surrounding early twentieth century economic development in the United States: How effective were early schooling laws? And, what were the returns to schooling for the cohorts affected by these laws?

²⁷ Lleras-Muney and Shertzer (2015) examine the effects of compulsory schooling, child labor, continuation schooling and English-only laws on years of schooling reported by immigrant and native-born men (in online appendix Table IV) in the 1940 census. For both groups they find only child labor laws, which governed the age at which a work permit could be obtained, had a statistically significant effect on schooling. For native-born men, they do not separately report estimates for men with foreign-born and native-born parents. It is worth noting that their results cover the 1904-1924 birth cohorts, which are later than the birth cohorts examined in this paper, and that their 1940 census data is from the 1% IPUMs sample.

²⁸ Appendix Figure 3 provides the quantile IV estimates, which parallel those in Figure 3.

The paper first demonstrates the efficacy of the first wave of schooling laws. The effect of having laws effectively requiring completion of primary school (such as seven or eight years of schooling) was to increase education by about 0.1 year. Given the large number of children affected by these laws across the United States, these gains represent a policy victory for states. Relative to the prior literature, these estimates use the full count 1940 census, a more detailed coding of schooling laws, a more flexible specification for estimation, and earlier birth cohorts.

The paper then uses these laws as instruments to estimate the returns to schooling. The 2SLS estimates of returns to schooling of 0.067 to 0.077 are statistically significant and within the ranges typically found for later cohorts. Notably, these estimates hold while controlling for region-specific trends in school attendance, a problem that has hampered many prior estimates.

In addition to these key findings, the paper also examines the returns to schooling across the earnings distribution. The quantile IV results show returns to those at the bottom of the earnings distribution for the 1885-1912 cohorts were substantially larger than for those at the top of the earnings distribution. A number of sources including Goldin and Katz (1999, 2001, 2008), Kopczuk, Saez, and Song (2010), and Lindert (2000) present detailed evidence on the decrease and then increase in earnings inequality over the course of the twentieth century. Skill ratios fell in manual trades from 1907 to 1929. Wage differentials in manufacturing declined across most industries between 1890 and 1940, as did the ratio of clerical worker wages to production workers. The ratio of white collar to blue collar earnings fell from 1910 to 1960. For these earlier cohorts, who were of prime working age between 1910 and 1940, increased returns to schooling for workers at the bottom of the wage distribution may have been one factor in the decline in inequality.

Finally, the first wave of laws was the foundation upon which the second wave of schooling laws were built. Second-wave schooling laws requiring attendance at newly-established high schools across the country were central to the economic development of the United States from the mid-twentieth century onwards. The foundations of successful compulsory attendance policies were laid in the laboratories of late nineteenth and early twentieth century state primary school attendance policies and child labor laws.

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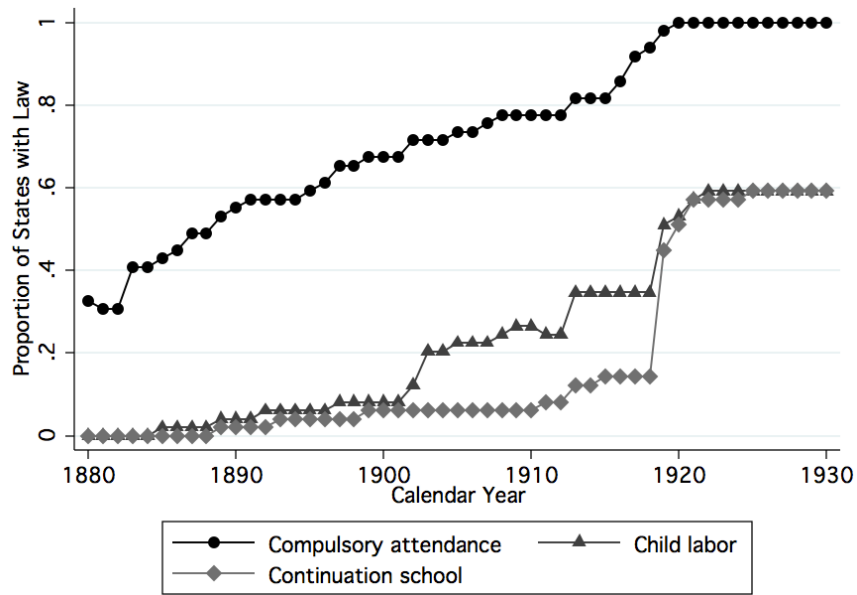
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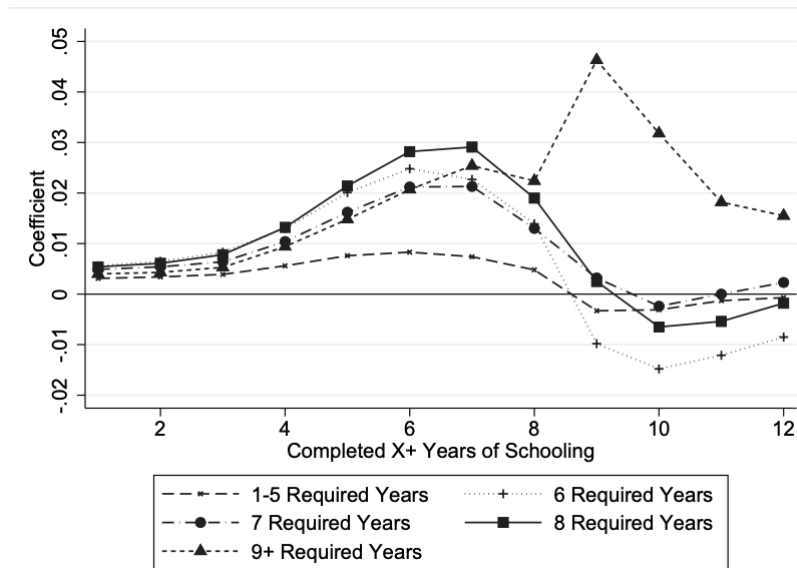
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Figure 1: Passage of State Laws Affecting School Attendance, 1880-1930



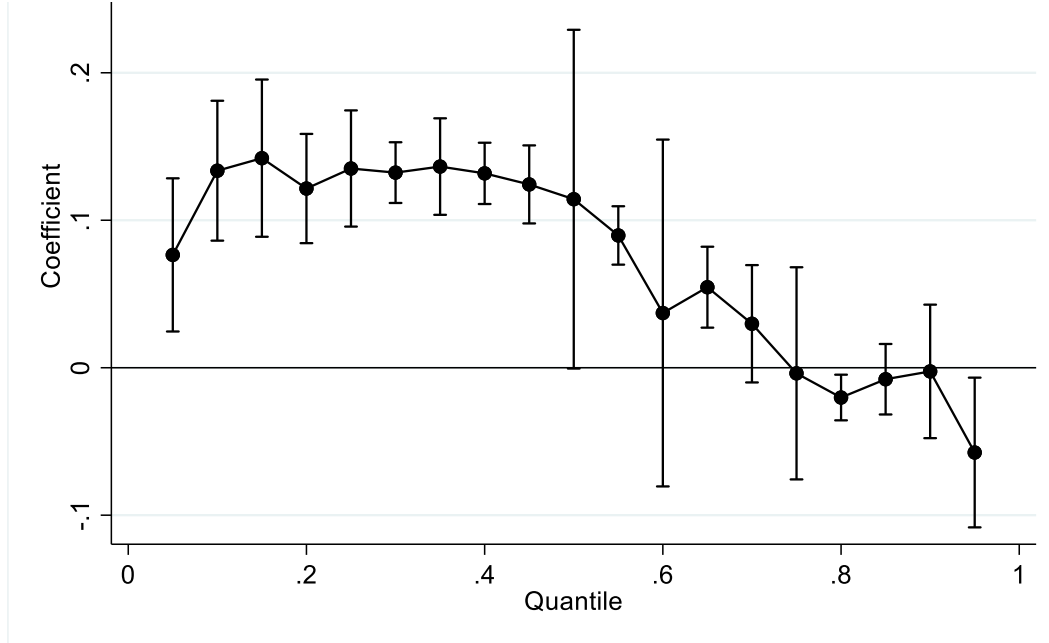
Notes: Figure shows proportion of states with a compulsory attendance law, child labor law exempting working children from compulsory attendance requirements, and continuation schooling law for working children in each calendar year. This figure shows the existence of a law on the books, not whether the law bound children in practice. See the dataset attached in the online Data Appendix for sources.

Figure 2: Effect of the Laws on Completed Years of Schooling, Native-Born White Men



Notes: Figure shows the coefficients from regressing completed years of schooling on laws using individuals who were born between 1885 and 1912, inclusive. The confidence intervals on each point can be obtained from Appendix Table 1, which shows the complete regression estimates. The outcome in each regression is a binary indicator for having completed at least as many years as shown on the axes. Results are shown for the all white men sample that has 19,055,381 observations. Each regression includes state of birth and region by year of birth fixed effects. See Appendix Table 1 for full results.

Figure 3: Quantile Instrumental Variable Regression Estimates of Returns to Schooling, Native-Born White Men



Notes: Each coefficient is from a separate regression, and error bars show 95% confidence intervals. Results are shown for a random 25% sample of the working men sample: all white men who reported positive earnings and positive weeks worked in 1939. This sample contains 3,352,676 observations. Each regression includes state of birth and region by year of birth fixed effects. See Appendix Table 7, Panel A for full results.

Table 1: Summary statistics

<i>Variables</i>	<i>Sample:</i>		
	<u>All Men</u>	<u>Working Men</u>	<u>Restricted Working Men</u>
Age	38.9	38.3	38.2
Years of Schooling	9.2	9.4	9.9
Log Weekly Wage		3.2	3.3
<i>Required Schooling:</i>			
Percent Zero Years	13.5	11.3	10.2
Percent One to Five Years	9.2	8.8	8.4
Percent Six Years	19.1	19.0	19.3
Percent Seven Years	16.9	17.0	17.8
Percent Eight Years	33.4	35.1	35.1
Percent Nine or More Years	7.9	8.8	9.2
N	19,055,381	13,410,703	8,731,805

Table 2: Effect of the Laws on Years of Schooling for Native-Born White Men

<i>Sample: Laws</i>	All Men (1)	Working Men (2)	Restricted Working Men (3)
<i>Required Schooling:</i>			
One to Five Years	0.041** (0.017)	0.043** (0.017)	0.024 (0.018)
Six Years	0.047** (0.021)	0.057*** (0.020)	0.044** (0.021)
Seven Years	0.099*** (0.022)	0.121*** (0.022)	0.124*** (0.023)
Eight Years	0.124*** (0.025)	0.149*** (0.024)	0.142*** (0.025)
Nine or More Years	0.244*** (0.029)	0.271*** (0.028)	0.219*** (0.029)
N	19,055,381	13,410,703	8,731,805

Notes: Each column in the table presents the results of a separate regression using individuals who were born between 1885 and 1912, inclusive. Results are shown for three samples: all native-born white men (Column 1), all white men who reported positive earnings and positive weeks worked in 1939 (Column 2), and all white men who reported positive earnings and worked at least 40 weeks in 1939, works but is not self-employed, and did not live on a farm (Column 3). Each regression includes state of birth and region by year of birth fixed effects. Standard errors are clustered by state and year. Significance is represented by *** at the .01 level, ** at the .05 level, and * at the .10 level.

Table 3: Literature Review of Effects of Laws on Educational Outcomes

Paper	Identification	Data	Sample	Time period	Effects of laws
Landes and Solmon (1972)	DD laws-time	Administrative attendance, enrollment	State-year averages from administrative data	1870-1880, 1880-1890	No
Eisenberg (1988)	Pre-post county analysis Iowa, Pennsylvania	Administrative attendance, enrollment, school quality	County-level averages from administrative data	1897-1908	Yes for Iowa and Pennsylvania
Margo and Finegan (1996)	DD laws-older and younger 14 year olds in 1900	1900 Census sample In school	1/760 Preston sample	1900	Yes for states with attendance + labor
Lleras Muney (2002)	DD Time series State FE, Year FE, Region x year FE	1960 Census sample Years of schooling	1/100 sample	1915-1939	Yes, all laws
Goldin and Katz (2011)	DD Time series State FE, Year FE, Region x TT	1960 Census sample Years of schooling 1910, 1920 working, in school Administrative	1/100 samples	1910-1939 (1896-1925 birth cohorts)	Yes, primarily continuation schooling, they note effects modest
Puerta (2011)	DD laws-time in border townships/counties	1850-1910 Census samples In school	1/100 – 1/20 samples	1850-1910	Yes, bigger for early (1850-1870) than late (1880-1910)
Lleras-Muney and Shertzer (2015)	DD Time series State FE, Year FE, State TT	1910, 1920, and 1930 Census samples, 1940 Census, WWII records	1/100 samples	1910-1930 (1904-1924 birth cohorts)	Yes, larger effects on enrollment for children born abroad than native-born children
Clay, Lingwall, Stephens (2020)	DD laws-time State FE, Year FE, Region x Year FE	Complete 1940 Census Years of schooling	Full census, white men	1899-1926 (1885-1912 birth cohorts)	Yes, effects modest and nonlinear.

Table 4: Returns to Schooling for Native-Born White Men

<i>Sample:</i> <i>Specification:</i>	Working Men		Restricted Working Men	
	OLS	2SLS	OLS	2SLS
<i>Laws</i>	(1)	(2)	(5)	(6)
Years of Schooling	0.084*** (0.001)	0.077*** (0.015)	0.067*** (0.000)	0.067*** (0.015)
First Stage F-Stat		30.0		22.3
N	13,410,703	13,410,703	8,731,805	8,731,805

Notes: Each column in the table presents the results of a separate regression of log weekly wages on years of schooling using individuals who were born between 1885 and 1912, inclusive. Results are shown for two samples: all white men who reported positive earnings and positive weeks worked in 1939 (Columns 1-2), and all white men who reported positive earnings and worked at least 40 weeks in 1939, works but is not self-employed, and did not live on a farm (Columns 3-4). The table contains OLS estimates of the returns to schooling (Columns 1 and 3) and IV estimates of the returns to schooling using indicators for required years of schooling that include continuation schooling requirements (Columns 2 and 4). The corresponding first stage estimates are found in Table 2. Each regression includes state of birth and region by year of birth fixed effects. Standard errors are clustered by state and year. Significance is represented by *** at the .01 level, ** at the .05 level, and * at the .10 level.

Table 5: Literature Review on Returns to Schooling

Paper	Identification	Data	Laws	Time period	Returns
Angrist and Krueger (1991)	IV time series	1970-1980 Censuses (1-5% samples)	Quarter of birth attendance	1920-1949 birth cohorts	Yes, IV larger than OLS
Staiger and Stock (1997)	IV time series	1980 Census (5% sample)	Quarter of birth attendance	1930-1949 birth cohorts	Yes, IV larger than OLS
Goldin and Katz (2000)	Cross sectional	1915 Iowa Census (2.5% sample)		1850-1897 birth cohorts	Yes, smaller for primary school, larger for high school/college
Oreopoulos and Salvanes (2011)	IV time series	1950-2000 Censuses (0.3-1% samples), ACS 2001-2007	Attendance	1899-1982 birth cohorts	Yes, IV larger than OLS
Stephens and Yang (2014)	IV time series State FE, Year FE, Region x Year FE	1960-1980 Censuses (1-5% samples)	Attendance	1905-1954 birth cohorts	No, if controls for Region x Year FE are included
Feigenbaum and Tan (2019)	Twin study	Full 1940 Census	None	1875-1920 birth cohorts (children ages 0-25 in 1900, 1910, and 1920)	Yes, around half of recent twin studies
Clay, Lingwall, Stephens (2020)	IV Time series State FE, Year FE, Region x Year FE	Full 1940 Census	Attendance, labor, continuation school	1885-1912 birth cohorts	Yes, OLS in line with previous studies (Card 1999), quantile IV positive for all quantiles, larger for lower quantiles

Table 6: Effect of the Laws on Years of Schooling for Selected Samples

<i>Sample: Laws</i>	(A) Full sample, excluding the South		(B) Sample Line Sample	
	All Men	Working Men	Parents Native- born	1+ Parent(s) Foreign-born
	(1)	(2)	(3)	(4)
One to Five Years	0.017 (0.028)	0.003 (0.026)	0.085** (0.041)	-0.115 (0.078)
Six Years	0.014 (0.027)	0.009 (0.024)	0.136*** (0.046)	-0.178** (0.079)
Seven Years	0.065** (0.029)	0.074*** (0.026)	0.217*** (0.050)	-0.043 (0.085)
Eight Years	0.087*** (0.031)	0.097*** (0.028)	0.233*** (0.051)	-0.039 (0.085)
Nine or More Years	0.205*** (0.035)	0.217*** (0.032)	0.421*** (0.059)	0.044 (0.097)
N	14,911,555	10,760,803	498,174	176,273

Notes: Each column in the table presents the results of a separate regression using individuals who were born between 1885 and 1912, inclusive. Results are shown for four samples: all white men excluding the South (Column 1), all working white men, excluding the South (Column 2), working white men with native-born parents who are in the sample line (Column 3), and working white men who are in the sample line with at least one parent born outside the United States (Column 4). Each regression includes state of birth and region by year of birth fixed effects. Standard errors are clustered by state and year. Significance is represented by *** at the .01 level, ** at the .05 level, and * at the .10 level.

Table 7: Returns to Schooling for Selected Samples

<i>Sample:</i>	(A) Full Sample Working Men, Excluding the South		(B) Sample Line, Native-Born	
	OLS	2SLS	OLS	2SLS
<i>Specification:</i>				
<i>Laws</i>	(1)	(2)	(3)	(4)
Years of Schooling	0.077*** (0.001)	0.064*** (0.016)	0.088*** (0.001)	0.091*** (0.024)
First Stage F-Stat		28.8		16.4
N	10,760,803	10,760,803	498,174	498,174

Notes: Each estimate in the table presents the results of a separate regression using individuals who were born between 1885 and 1912, inclusive. Results are shown for all white men who reported positive earnings and positive weeks worked in 1939. The table contains OLS estimates of the returns to schooling (Columns 1 and 3), and IV estimates of the returns to schooling using indicators for required years of schooling that includes continuation schooling requirements (Columns 2 and 4). The corresponding first stage estimates are found in Table 6, Columns 2 and 3. Each regression includes state of birth and region by year of birth fixed effects. Standard errors are clustered by state and year. Appendix Table 10 shows results for all sample line individuals. Significance is represented by *** at the .01 level, ** at the .05 level, and * at the .10 level.

Appendix: For Online Publication

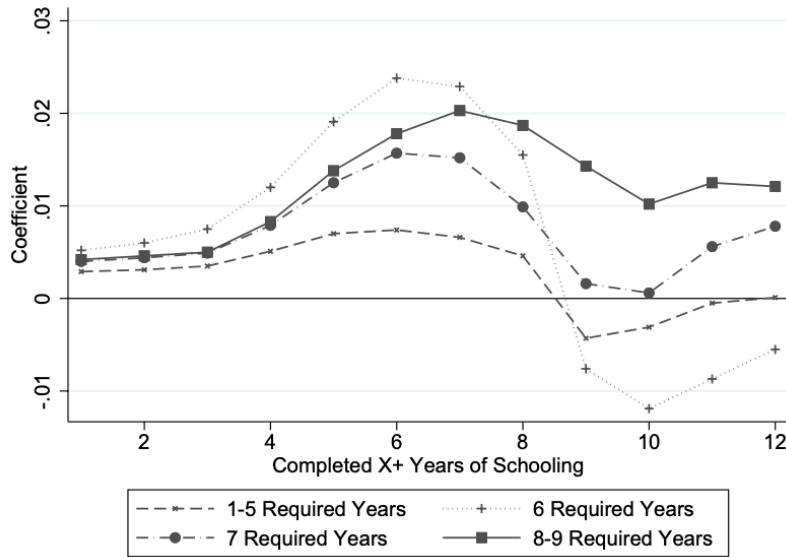
Appendix Figure 1: Required Years of Education by Year and State of Birth



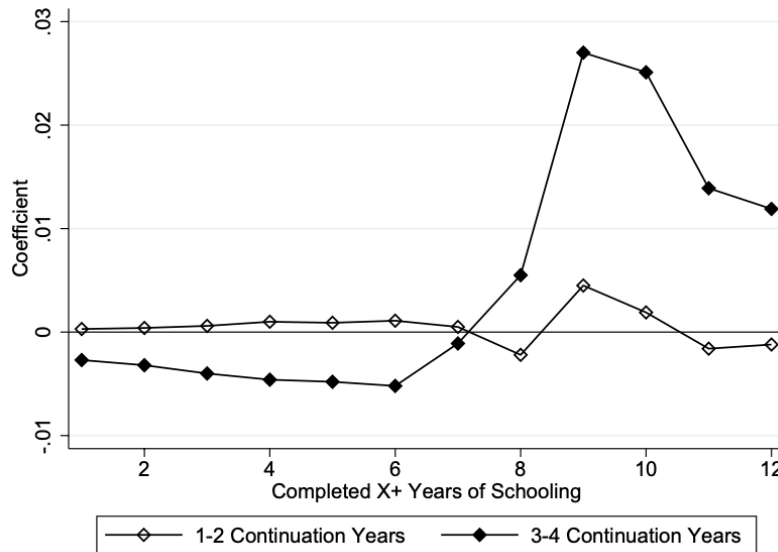
Notes: Figure shows the years of required education by year of birth (solid lines), according to state compulsory attendance laws and state continuation schooling laws (dashed lines). Years of required education are calculated for each birth cohort as described in the online Data Appendix. States are grouped by census division for comparison

Appendix Figure 2: Effect of the Laws on Completed Schooling – Separate Continuation School

Panel A: Compulsory Attendance Requirements

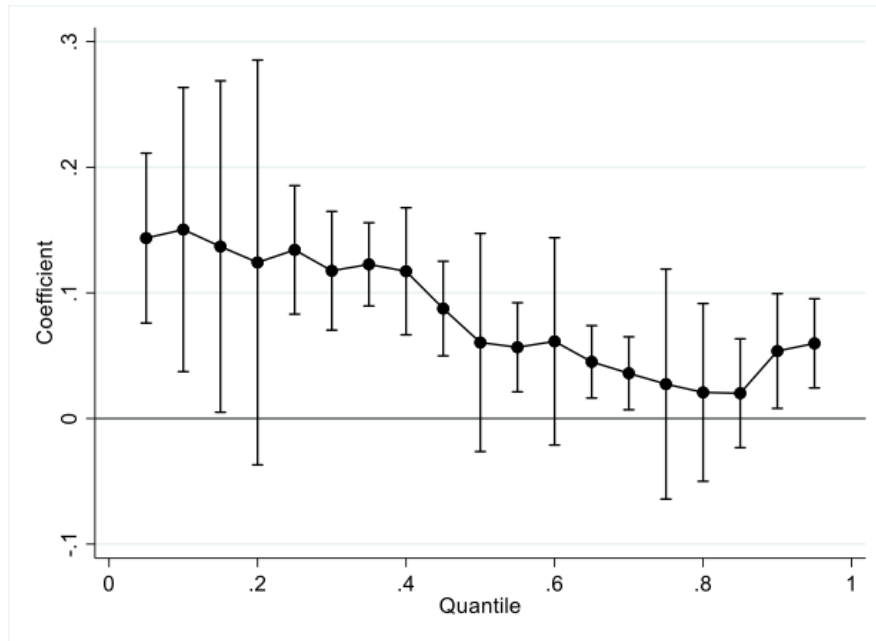


Panel B: Continuation School Requirements



Notes: Figures show the coefficients from regressing completed years of schooling on laws using individuals who were born between 1885 and 1912, inclusive. The outcome in each regression is a binary indicator for having completed at least as many years as shown on the axes. Results are shown for the all white men sample that has 19,055,381 observations. Each regression includes state of birth and region by year of birth fixed effects. See Appendix Table 2 for complete results.

Appendix Figure 3: Quantile Instrumental Variable Regression Estimates of Returns to Schooling, Native-Born Parents



Notes: Each coefficient is from a separate regression, and error bars show 95% confidence intervals. Results are shown using the working men sample: all white men who reported positive earnings and positive weeks worked in 1939. The Figure uses the 498,174 observations from the working men sample who are sample line individuals and have both parents that are native born. Each regression includes state of birth and region by year of birth fixed effects. See Appendix Table 7, Panel C for full results.

Appendix Table 1: Effect of the Laws on Completed Years of Schooling

VARIABLES	Years of School	Years of School	Years of School	Years of School	Years of School	Years of School	Years of School	Years of School	Years of School	Years of School	Years of School	Years of School
<i>Years of Schooling</i>	1+	2+	3+	4+	5+	6+	7+	8+	9+	10+	11+	12+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<i>Required Schooling:</i>												
One to Five Years	0.0031*** (0.0007)	0.0034*** (0.0008)	0.0039*** (0.0011)	0.0056*** (0.0016)	0.0076*** (0.0024)	0.0083*** (0.0030)	0.0074** (0.0033)	0.0048 (0.0030)	-0.0033 (0.0024)	-0.0031 (0.0023)	-0.0013 (0.0023)	-0.0007 (0.0021)
Six Years	0.0056*** (0.0008)	0.0065*** (0.0009)	0.0083*** (0.0012)	0.0130*** (0.0018)	0.0201*** (0.0029)	0.0248*** (0.0035)	0.0227*** (0.0039)	0.0139*** (0.0037)	-0.0098** (0.0030)	-0.0148*** (0.0027)	-0.0121*** (0.0026)	-0.0085*** (0.0023)
Seven Years	0.0049*** (0.0009)	0.0054*** (0.0010)	0.0064*** (0.0014)	0.0104*** (0.0020)	0.0162*** (0.0030)	0.0212*** (0.0036)	0.0213*** (0.0041)	0.0130** (0.0039)	0.0032 (0.0034)	-0.0024 (0.0031)	0.0000 (0.0029)	0.0023 (0.0027)
Eight Years	0.0054*** (0.0009)	0.0061*** (0.0011)	0.0078*** (0.0015)	0.0132*** (0.0021)	0.0214*** (0.0031)	0.0282*** (0.0038)	0.0291*** (0.0043)	0.0190*** (0.0042)	0.0025 (0.0039)	-0.0065** (0.0033)	-0.0054* (0.0031)	-0.0018 (0.0029)
Nine to Ten Years	0.0040*** (0.0011)	0.0043*** (0.0013)	0.0053*** (0.0017)	0.0094*** (0.0024)	0.0148*** (0.0034)	0.0207*** (0.0041)	0.0254*** (0.0046)	0.0224*** (0.0046)	0.0463*** (0.0056)	0.0318*** (0.0050)	0.0182*** (0.0044)	0.0155*** (0.0040)

Notes: Each column in the table presents the results of a separate regression using individuals who were born between 1885 and 1912, inclusive. The outcome in each regression is a binary indicator for having completed at least as many years as shown at the top of the column. Results are shown for the all white men sample that has 19,055,381 observations. Each regression includes state of birth and region by year of birth fixed effects. Standard errors are clustered by state and year. These results are shown graphically in Figure 2. Significance is represented by *** at the .01 level, ** at the .05 level, and * at the .10 level.

Appendix Table 2: Effect of the Laws on Completed Years of Schooling – Separate Continuation School

VARIABLES	Years of School	Years of School	Years of School	Years of School	Years of School	Years of School	Years of School	Years of School	Years of School	Years of School	Years of School	Years of School
<i>Years of Schooling</i>	1+	2+	3+	4+	5+	6+	7+	8+	9+	10+	11+	12+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<i>Required Schooling w/o Continuation Yrs.:</i>												
One to Five Years	0.0029*** (0.0007)	0.0031*** (0.0008)	0.0035*** (0.0011)	0.0051*** (0.0015)	0.007*** (0.0024)	0.0074** (0.0029)	0.0066** (0.0032)	0.0046 (0.003)	-0.0043 (0.0026)	-0.0031 (0.0024)	-0.0005 (0.0022)	0.0001 (0.002)
Six Years	0.0052*** (0.0008)	0.006*** (0.0009)	0.0075*** (0.0012)	0.012*** (0.0018)	0.0191*** (0.0028)	0.0238*** (0.0034)	0.0229*** (0.0038)	0.0155*** (0.0036)	-0.0076** (0.0033)	-0.0119*** (0.0029)	-0.0087*** (0.0025)	-0.0055*** (0.0023)
Seven Years	0.004*** (0.0009)	0.0044*** (0.0011)	0.0049*** (0.0015)	0.0079*** (0.0021)	0.0125*** (0.0032)	0.0157*** (0.0039)	0.0152*** (0.0042)	0.0099** (0.0039)	0.0016 (0.0039)	0.0006 (0.0035)	0.0056* (0.0031)	0.0078*** (0.0029)
Eight to Nine Years	0.0042*** (0.001)	0.0046*** (0.0012)	0.005*** (0.0016)	0.0083*** (0.0023)	0.0138*** (0.0034)	0.0178*** (0.0042)	0.0203*** (0.0046)	0.0187*** (0.0044)	0.0143*** (0.0047)	0.0102** (0.0041)	0.0125*** (0.0036)	0.0121*** (0.0033)
<i>Continuation School:</i>												
One to Two Years	0.0003 (0.0003)	0.0004 (0.0004)	0.0006 (0.0005)	0.001 (0.0008)	0.0009 (0.0011)	0.0011 (0.0014)	0.0005 (0.0016)	-0.0022 (0.0015)	0.0045* (0.0026)	0.0019 (0.0023)	-0.0016 (0.002)	-0.0012 (0.0018)
Three to Four Years	-0.0027*** (0.0004)	-0.0032*** (0.0005)	-0.004*** (0.0007)	-0.0046*** (0.0009)	-0.0048*** (0.0014)	-0.0052*** (0.0018)	-0.0011 (0.0021)	0.0055*** (0.002)	0.027*** (0.0033)	0.0251*** (0.0031)	0.0139*** (0.0026)	0.0119*** (0.0024)

Notes: Each column in the table presents the results of a separate regression using individuals who were born between 1885 and 1912, inclusive. The outcome in each regression is a binary indicator for having completed at least as many years as shown at the top of the column. Results are shown for the all white men sample that has 19,055,381 observations. Each regression includes state of birth and region by year of birth fixed effects. Standard errors are clustered by state and year. Significance is represented by *** at the .01 level, ** at the .05 level, and * at the .10 level.

Appendix Table 3: Impact of Schooling on Employment Outcomes

<i>Outcome:</i>	Panel A: OLS Estimates				
	Employed	Employed Excl. WPA	Unemployed	Unemployed Incl. WPA	Not in the Labor Force
<i>Mean of Dep Var.:</i>	0.886	0.842	0.063	0.109	0.055
<i>Laws</i>	(1)	(2)	(3)	(4)	(5)
Years of Schooling	0.010*** (0.0001)	0.016*** (0.0001)	-0.006*** (0.0001)	-0.013*** (0.0001)	-0.005*** (0.0000)
<i>Outcome:</i>	Panel B: 2SLS Estimates				
	Employed	Employed Excl. WPA	Unemployed	Unemployed Incl. WPA	Not in the Labor Force
<i>Laws</i>	(1)	(2)	(3)	(4)	(5)
Years of Schooling	0.012*** (0.0039)	0.027*** (0.0054)	-0.013*** (0.0033)	-0.028*** (0.0051)	-0.0001 (0.0024)

Notes: Each estimate in the table presents the results of a separate regression using the sample of all white men who were born between 1885 and 1912, inclusive. We present OLS estimates in Panel A and 2SLS estimates in Panel B. The outcome in column (1) is an indicator for being employed during the week of March 24-30, 1940. The outcome in column (2) is an indicator for being employed but excluding those on work relief from counting as employed (Goldin and Margo 1992). The outcome in column (3) is an indicator for being unemployed but only uses those men who are participating in the labor force in the analysis. The analysis in column (4) is the same as that found in column (3) except those on work relief are counted as being unemployed (Goldin and Margo 1992). The outcome in column (5) is an indicator for being out of the labor force. The sample of 19,055,381 observations in columns (1), (2), and (5) includes all white men in the relevant birth cohorts. The sample of 18,010,888 observations in columns (3) and (4) includes those who are in the labor force. The corresponding first stage estimates for columns (1), (2), and (5) are shown in column (1) of Table 2 and the corresponding first stage F-statistic is 25.3. The first stage estimates and F-statistic for the sample of men in the labor force used in columns (3) and (4) is 24.8. Each regression includes state of birth and region by year of birth fixed effects. Standard errors are clustered by state and year. Significance is represented by *** at the .01 level, ** at the .05 level, and * at the .10 level.

Appendix Table 4: Full Results for Restricted Working Men Sample

<i>Sample:</i>	<u>(A) Full sample, excluding the South</u>			<u>(B) Sample Line Sample, Native-Born</u>	
	Effect of Laws on Years of Schooling	Returns to Schooling		Returns to Schooling	
<i>Laws</i>		OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)
Years of Schooling		0.062*** (0.000)	0.059*** (0.017)	0.070*** (0.000)	0.091*** (0.020)
<i>Required Schooling:</i>					
One to Five Years	-0.025 (0.025)				
Six Years	-0.013 (0.024)				
Seven Years	0.065** (0.026)				
Eight Years	0.079*** (0.028)				
Nine or More Years	0.154*** (0.032)				
First Stage F-Stat			20.4		14.9
N	7,168,166	7,168,166	7,168,166	318,066	318,066

Notes: See notes to Table 2. Each column in the table presents the results of a separate regression using individuals who were born between 1885 and 1912, inclusive. Results are shown for all white men who reported positive earnings and worked at least 40 weeks in 1939, works but is not self-employed, and did not live on a farm. Each regression includes state of birth and region by year of birth fixed effects. Standard errors are clustered by state and year. Significance is represented by *** at the .01 level, ** at the .05 level, and * at the .10 level.

Appendix Table 5: Impact of Education on Earnings – 1% Sample of 1940 Census

	OLS	2SLS	LIML	1 st Stage
<i>Laws</i>	(1)	(2)	(3)	(4)
Years of Schooling	0.085*** (0.001)	0.164*** [0.068, 0.321]	0.172*** (0.054)	
<i>Required Schooling:</i>				
One to Five Years				0.096 (0.061)
Six Years				0.071 (0.066)
Seven Years				0.125* (0.073)
Eight Years				0.185** (0.077)
Nine or More Years				0.327*** (0.092)
First Stage F-Stat		4.2		

Notes: Each column in the table presents the results of a separate regression using individuals who were born between 1885 and 1912, inclusive. Results are shown for the previously available 1% micro sample of the 1940 Census constructed using the same criteria as the working men sample: all white men who reported positive earnings and positive weeks worked in 1939. This sample contains 142,173 observations. Each regression includes state of birth and region by year of birth fixed effects. Standard errors are clustered by state and year and are shown in parentheses. A 95% confidence interval for Moreira's Conditional Likelihood Ratio Test is shown in brackets. Significance is represented by *** at the .01 level, ** at the .05 level, and * at the .10 level.

Appendix Table 6: Reduced Form Estimates of Returns to Schooling

<i>Sample:</i> <i>Laws</i>	Working Men	Restricted Working Men
	(1)	(2)
<i>Required Schooling:</i>		
One to Five Years	0.015*** (0.003)	0.010*** (0.003)
Six Years	0.027*** (0.004)	0.018*** (0.003)
Seven Years	0.026*** (0.004)	0.018*** (0.004)
Eight Years	0.031*** (0.005)	0.022*** (0.004)
Nine or More Years	0.039*** (0.006)	0.029*** (0.005)
N	13,410,703	8,731,805

Notes: See notes to Table 4. We restrict the analysis to men working in 1939 who were born between 1885 and 1912, inclusive. Each regression includes state of birth and region by year of birth fixed effects. The dependent variable is log weekly wages. Significance is represented by *** at the .01 level, ** at the .05 level, and * at the .10 level.

Appendix Table 7: Quantile Instrumental Variable Regression Estimates of Returns to Schooling

	Quantile Point Estimates																		
	5 th	10 th	15 th	20 th	25 th	30 th	35 th	40 th	45 th	50 th	55 th	60 th	65 th	70 th	75 th	80 th	85 th	90 th	95 th
Years of Schooling	<i>A. Working Men Sample (25% subsample)</i>																		
	0.077	0.134	0.142	0.122	0.135	0.132	0.136	0.132	0.124	0.114	0.090	0.037	0.055	0.030	-0.004	-0.020	-0.008	-0.003	-0.058
	(0.027)	(0.024)	(0.027)	(0.019)	(0.020)	(0.011)	(0.017)	(0.011)	(0.014)	(0.059)	(0.010)	(0.060)	(0.014)	(0.020)	(0.037)	(0.008)	(0.012)	(0.023)	(0.026)
Years of Schooling	<i>B. Working Men Sample - Sample Line Individuals</i>																		
	0.095	0.120	0.119	0.112	0.111	0.095	0.098	0.028	0.058	0.046	0.036	0.020	0.030	0.017	-0.004	-0.018	-0.001	0.001	-0.008
	(0.056)	(0.031)	(0.077)	(0.030)	(0.039)	(0.022)	(0.016)	(0.149)	(0.050)	(0.033)	(0.015)	(0.026)	(0.022)	(0.018)	(0.026)	(0.019)	(0.025)	(0.025)	(0.053)
Years of Schooling	<i>C. Working Men Sample - Native Parents Only Sample Line Individuals</i>																		
	0.144	0.150	0.137	0.124	0.134	0.118	0.123	0.117	0.088	0.061	0.057	0.061	0.045	0.036	0.027	0.021	0.020	0.054	0.060
	(0.035)	(0.058)	(0.067)	(0.082)	(0.026)	(0.024)	(0.017)	(0.026)	(0.019)	(0.044)	(0.018)	(0.042)	(0.015)	(0.015)	(0.047)	(0.036)	(0.022)	(0.023)	(0.018)

Notes: Each estimate in the table presents the results of a separate quantile IV regression of log weekly wages on years of schooling, using laws as an instrument for years of schooling. We restrict the analysis to men working in 1939 who were born between 1885 and 1912, inclusive. Results are shown from the working men sample: all white men who reported positive earnings and positive weeks worked in 1939. Panel A uses a random 25% sample of the working men sample and contains 3,352,676 observations. Panel B uses the 674,447 observations from the full working men sample who are sample line individuals. Panel C uses the 498,174 observations from the full working men sample who are sample line individuals and have both parents that are native born. The instruments are the combined years of required compulsory attendance and continuation school indicators used in Table 4, Columns 2 and 4. Each regression includes state of birth and region by year of birth fixed effects.

Appendix Table 8: Impact of Education on Earnings – 25% Working Men Sample

	OLS	2SLS	1 st Stage
<i>Laws</i>	(1)	(2)	(3)
Years of Schooling	0.084*** (0.001)	0.077*** (0.020)	
<i>Required Schooling:</i>			
One to Five Years			0.029 (0.020)
Six Years			0.036 (0.022)
Seven Years			0.090*** (0.025)
Eight Years			0.126*** (0.027)
Nine or More Years			0.237*** (0.032)
First Stage F-Stat		18.8	

Notes: Each column in the table presents the results of a separate regression using individuals who were born between 1885 and 1912, inclusive. Results are shown for a random 25% sample of the working men sample: all white men who reported positive earnings and positive weeks worked in 1939. This sample, which is the same sample that is used in the quantile IV estimates shown in Figure 3 and Panel A of Appendix Table 7, contains 3,352,676 observations. Each regression includes state of birth and region by year of birth fixed effects. Standard errors are clustered by state and year. Significance is represented by *** at the .01 level, ** at the .05 level, and * at the .10 level.

Appendix Table 9: Alternative Specifications, Native-Born White Men, Working Men Sample

<i>Identification:</i>	Region by YOB FE		State Quadratic YOB Time Trends		YOB FE	
	<i>Specification:</i> OLS	2SLS	OLS	2SLS	OLS	2SLS
<i>Laws</i>	(1)	(2)	(3)	(4)	(5)	(6)
Years of Schooling	0.084*** (0.001)	0.077*** (0.015)	0.084*** (0.001)	0.127*** (0.017)	0.084*** (0.001)	0.017* (0.010)
State of Birth FE	Y	Y	Y	Y	Y	Y
First Stage F-Stat		30.0		11.1		43.5
N	13,410,703	13,410,703	13,410,703	13,410,703	13,410,703	13,410,703

Notes: Each column in the table presents the results of a separate regression of log weekly wages on years of schooling using individuals who were born between 1885 and 1912, inclusive. Results are shown for all white men who reported positive earnings and positive weeks worked in 1939. The table contains OLS estimates of the returns to schooling (odd Columns) and IV estimates of the returns to schooling using indicators for required years of schooling that include continuation schooling requirements (even Columns). Columns (1) and (2) repeat the results from Table 4 Columns (1) and (2) for ease of comparison. Each regression includes state of birth fixed effects. Standard errors are clustered by state and year. Significance is represented by *** at the .01 level, ** at the .05 level, and * at the .10 level.

Appendix Table 10: Returns to Schooling for Native-Born White Men, All Sample Line

<i>Sample:</i>	(A) All Sample Line – Full Sample		(B) All Sample Line – Including Foreign-born Indicator	
	OLS	2SLS	OLS	2SLS
<i>Laws</i>	(1)	(2)	(3)	(4)
Years of Schooling	0.083*** (0.001)	0.045 (0.027)	0.084*** (0.001)	0.034 (0.025)
First Stage F-Stat		12.6		15.6
N	674,447	674,447	674,447	674,447

Notes: Each estimate in the table presents the results of a separate regression using individuals who were born between 1885 and 1912, inclusive. Results are shown for all white men who reported positive earnings and positive weeks worked in 1939. We present OLS estimates of the returns to schooling (Columns 1 and 3), and IV estimates of the returns to schooling using indicators for required years of schooling that includes continuation schooling requirements (Columns 2 and 4). Each regression includes state of birth and region by year of birth fixed effects. Standard errors are clustered by state and year. Significance is represented by *** at the .01 level, ** at the .05 level, and * at the .10 level.

Appendix Table 11: Returns to Schooling for Native-Born White Men, With State Clustering

<i>Sample:</i> <i>Specification:</i>	Working Men		Restricted Working Men	
	OLS	2SLS	OLS	2SLS
<i>Laws</i>	(1)	(2)	(3)	(4)
Years of Schooling	0.084*** (0.003)	0.077 [-0.1,0.27]	0.067*** (0.002)	0.067 [-0.1,0.12]
First Stage F-Stat		4.0		3.9
N	13,410,703	13,410,703	8,731,805	8,731,805

Notes: This Table shows Table 4 with two adjustments. First, standard errors are clustered at the state level, rather than the state-cohort level. Second, for the IV regressions a 95% confidence interval for Moreira's Conditional Likelihood Ratio Test is shown in brackets. Each column in the table presents the results of a separate regression of log weekly wages on years of schooling using individuals who were born between 1885 and 1912, inclusive. Results are shown for two samples: all white men who reported positive earnings and positive weeks worked in 1939 (Columns 1-2), and all white men who reported positive earnings and worked at least 40 weeks in 1939, works but is not self-employed, and did not live on a farm (Columns 3-4).

**Data Appendix to *Laws, Educational Outcomes, and Returns to Schooling:*
*Evidence from the Early Twentieth Century United States***

Karen Clay, Jeff Lingwall, and Melvin Stephens Jr.

State-Level Compulsory Attendance, Child Labor, and Continuation Schooling Laws

The attached STATA file contains the compulsory attendance, child labor, and continuation school age limits and exemptions for each state between 1880 and 1930. This builds on the work of many others, in particular Stephens and Yang (2014) and the 1910-1939 compilation in Goldin and Katz (2007), which expanded work by Angrist and Acemoglu (2000), Adriana Lleras-Muney, and Stephanie Schmidt. This dataset extends these previous codings to 1880, but differs in some ways to accommodate additional characteristics of the laws. For example, (1) we code the variable *earlyyrs_condition*, which was the lowest age at which an exemption to a maximum age limit could be used by achieving a certain level of schooling, (2) we also code *contyrs*, the number of years required to be exempt from continuation school, and (3) we studied state histories to code specific year requirements for when many state laws exempted after completion of “common school.” These sources are noted in *reference_courseofstudy*. Please see *State Specific Notes on Laws* below for nuance on how we coded issues in particular states.

The dataset has ten variables:

1. *entryage*: The age at which children had to enter school.
2. *exitage*: The age at which all children could stop attending school.
3. *earlyyrs*: The years of completed schooling that allowed an exemption to *exitage*.

4. *earlyyrs_condition*: The earliest age at which an *earlyyrs* exemption could be used. For example, the unconditional exit age might be 16 (*exitage* = 16) with the ability to drop out after six years of schooling (*earlyyrs* = 6), provided the child was at least 14 (*earlyyrs_condition* = 14).
5. *workage*: The age at which employed children could stop attending school.
6. *workyrs*: The schooling required for a *workage* exception to apply.
7. *contage*: The age working children stopped attending continuation school.¹
8. *contyrs*: The schooling required to be exempt from continuation school.
9. *reference_agelimit*: A reference to the state session law or statutory compilation in which the law is found. (With limited exceptions, the dates and age limits are drawn directly from the state session laws, which are accessible through *HeinOnline's* historical state law [database](#).) For states with laws prior to 1880, this references the source of the age limits existing in 1880.
10. *reference_courseofstudy*: A reference indicating the length of a typical primary school course when *earlyyrs* or *workyrs* required completion of primary school.

Most of the laws had exceptions for poverty, distance from school, or by special order of a local judge or school board. As with prior codings, we ignore these and other case-specific exemptions, as the data available are insufficient to reasonably calculate when they might

¹ States varied in the scope of their continuation schooling laws. Most states with these laws required the creation of continuation schools, contingent on city size or number of eligible children in the district. A minority of states in this time period—Connecticut (through 1918 outside large cities), Indiana, and Ohio (through 1921)—gave districts the option of creating schools and requiring attendance. The attached STATA file treats both types of law as requiring continuation school attendance.

apply. When a law was optional at the county level, or passed county by county, we use the date the law either applied statewide or required counties to opt-out rather than opt-in. Where one age applied to cities and towns and another to the rest of the state, we use the age limit applicable to the state generally. These are complex laws, and special cases sometimes required discretionary coding. Details on initial laws in particular states are given below.

The variables *earlyyrs*, *workyrs*, and *contyrs* are coded as “99” when the laws required literacy rather than a specific number of years of school. When these variables used “completion of primary school” or similar language as the requirement, we code the law as “99X”, where X is the length of a typical primary school course in the state.

State-Specific Notes on Laws

Alabama We code the initial attendance law as beginning in 1917, because the 1915 statute specified the law would take effect “on and after the first day of October, 1917.”²

Arizona An early attendance law was passed in 1875, but was repealed in 1883.³ The conventional date of the first law is 1899, when the law was re-implemented.⁴

² 1915 Ala. Acts 534.

³ 1883 Ariz. Sess. Laws 50; 1875 Ariz. Sess. Laws 40.

⁴ 1899 Ariz. Sess. Laws 14.

Arkansas We code the initial law as beginning in 1917, because an earlier 1909 law did not apply to 44 of 75 counties (including Pulaski County, the most populous county and location of Little Rock).⁵

Delaware Different law applied for Wilmington versus the rest of the state for 1919-1952. We code the laws that applied outside Wilmington.

Florida A 1915 law was an “opt in” law, which no county implemented prior to 1918.⁶

Georgia We code the initial attendance law as beginning in 1917, because the 1916 statute specified “the provisions of this Act shall become operative on the first day of January in the year nineteen hundred and seventeen.”⁷

Kentucky Different laws applied for cities and counties between 1908-1919. We code the county laws.

Louisiana A 1910 law applied only to the Parish of Orleans.⁸ In 1916 compulsory attendance was applied statewide.⁹

Maryland Baltimore city differs from elsewhere through at least 1946. We code the non-Baltimore law.

Michigan In 1895-1904 different law applied in cities versus counties. We code the county laws.

⁵ 1909 Ark. Acts 706.

⁶ 1915 Fla. Laws 61; HARRY GARDEN CUTLER, 1 HISTORY OF FLORIDA, PAST AND PRESENT, HISTORICAL AND BIOGRAPHICAL 180 (1923) (noting the first county to adopt the law in 1918, and that by 1919 the law applied statewide).

⁷ 1916 Ga. Laws 103.

⁸ 1910 La. Acts 366.

⁹ 1916 La. Acts 59.

Mississippi A 1918 law was an “opt in” law, which was not effective.¹⁰ The 1920 law was an “opt out” law, and as of 1921 only four counties had opted out.¹¹

Nebraska City vs non-city laws were different from 1907-1921. We code the non-city laws.

New York Small and large districts faced different laws through 1927. We code the small district laws.

North Carolina North Carolina passed compulsory attendance for a few select counties in 1901 and 1903, and in 1907 an “opt in” law was passed that applied across the state.¹² As of 1910 the state reported that “[o]nly a few districts have as yet availed themselves of this law and adopted compulsory attendance.”¹³ The 1913 law required attendance statewide.¹⁴

South Carolina Voluntary at the district level from 1915-1918. We code as no law before 1919.

¹⁰ 1918 Miss. Laws 312; BIENNIAL REPORT AND RECOMMENDATIONS OF THE STATE SUPERINTENDENT OF PUBLIC EDUCATION TO THE LEGISLATURE OF MISSISSIPPI FOR THE SCHOLASTIC YEARS 1919-1920 AND 1920-1921, at 11 (1921) (comparing 1920 to 1918).

¹¹ *Id.*

¹² 1907 N.C. Sess. Laws 1284; 1903 N.C. Sess. Laws 1011, 1030, 1036, 1152; 1901 N.C. Sess. Laws 988.

¹³ BIENNIAL REPORT OF THE SUPERINTENDENT OF PUBLIC INSTRUCTION OF NORTH CAROLINA TO GOVERNOR W.W. KITCHIN FOR THE SCHOLASTIC YEARS 1908-1909 AND 1909-1910, at 44 (1910).

¹⁴ 1913 N.C. Sess. Laws 267.

Tennessee In 1905, the state began to pass laws county by county through a tortured population-based method based on the 1900 census.¹⁵ In 1913 the law applied statewide.¹⁶

Texas An early compulsory attendance law was passed during the 1870s, but was soon repealed.¹⁷ Compulsory attendance was revisited in 1915, with a law that went into effect in 1916.¹⁸

Virginia A 1908 law was “opt in,” which was not widely adopted.¹⁹ The law was applied statewide in 1918.²⁰

Washington An initial statewide attendance law was passed in 1871 and repealed in 1873.²¹ A second attendance law was passed in 1877, which did not apply to small localities.²² In 1886 the law applied statewide.²³

¹⁵ *E.g.*, 1905 Tenn. Pub. Acts 1040 (implementing compulsory attendance in, e.g., “counties of this State having a population of no less than ... 12,890 ... and not more than ... 12,900, according to the Federal Census of 1900”).

¹⁶ 1913 Tenn. Pub. Acts 19.

¹⁷ 1876 Tex. Gen. Laws 199; 1870 Tex. Gen. Laws 113.

¹⁸ 1915 Tex. Gen. Laws 92 (noting the law would take require attendance “beginning September 1, 1916”).

¹⁹ 1908 Va. Acts 640; ANNUAL REPORT OF THE SUPERINTENDENT OF PUBLIC INSTRUCTION OF THE COMMONWEALTH OF VIRGINIA WITH ACCOMPANYING DOCUMENTS: SCHOOL YEAR 1916-1917, at 57-118 (1918) (noting county-level implementation of compulsory attendance).

²⁰ 1918 Va. Acts 752.

²¹ 1873 Wash. Sess. Laws 419; 1871 Wash. Sess. Laws 29.

²² 1877 Wash. Sess. Laws 278 (applying to “any city, town or village containing more than four hundred inhabitants”).

²³ 1886 Wash. Sess. Laws 27.

Calculating Years of Required Schooling

The required years of attendance for each birth cohort in each state can be calculated by iterating through the dataset. In each year of a child's life, this calculation asks whether attendance was required based on the laws in that year and the cohort's accumulated years of required attendance. Specifically, at each age 5 to 17 of a child's life, we ask

- (1) Is the child's age between the *entryage* and the *exitage* in effect in that year?
- (2) Does an exemption to *exitage* apply?
 - a. Has the child already been required to attend for sufficient years to qualify for an *earlyyrs* exemption? Or,
 - b. Is the child's age equal to or greater than the *workage*? If so, has the child been required to attend for sufficient years to satisfy the *workyrs* requirement?
- (3) If a *workage* exemption applies, is the child's age less than *contage*? If so, has the child completed sufficient schooling to be exempt from continuation school if a *contyrs* exemption exists?

For example, Alabama passed its first compulsory attendance law in 1915, which went into effect in 1917. The law required attendance between ages 8 and 16 (*entryage* = 8 and *exitage* = 16). In 1919, the state added an exception for children who had completed primary school (which took six years of schooling), so long as the child was at least 14 (*earlyyrs* = 996 and *earlyyrs_condition* = 14). Children could also leave school to work at age 14, so long as they had a fourth-grade education (*workage* = 14 and *workyrs* = 4). The Table below highlights each year the 1902-1909 birth cohorts are coded as required to attend school. It also shows the total years of required schooling for a child in each cohort.

Data Appendix Table 1: Attendance Requirements for the 1902-1909 Alabama Birth Cohorts

<u>Calendar Year</u>	<u>Birth Cohort</u>							
	1902	1903	1904	1905	1906	1907	1908	1909
	<u>Cohort's Age in Each Year</u>							
1916	14	13	12	11	10	9	8	7
1917 (first law in effect)	15	14	13	12	11	10	9	8
1918	16	15	14	13	12	11	10	9
1919 (law modified)	17	16	15 ¹	14	13	12	11	10
1920	18	17	16	15	14	13	12	11
1921	19	18	17	16	15	14 ²	13	12
1922	20	19	18	17	16	15	14	13
1923	21	20	19	18	17	16	15	14 ³
Years Required Attendance:	1	2	3	4	4	4	5	6

Notes: A child is coded as required to attend school in each highlighted cell. The bottom row shows the total years of required attendance for each birth cohort.

¹ A child born in 1904 is coded as required to attend school in 1919, at age 15, despite the age 14 *workage* exemption, because they had not been required to complete the *workyrs* four years of schooling. Similarly, a child born in 1905 is coded as required to attend at ages 14 and 15, and a child born in 1906 is coded as required to attend at age 14.

² A child born in 1907 was required to attend for four years (1917-1920), and so would qualify for the *workage* exemption at age 14, in 1921.

³ A child born in 1909 was required to attend six years of school (1917-1922, at ages 8-13). At age 14, the child would be exempt under the six years *earlyyrs* exemption. The child would also be exempt under the *workage* exemption, because they had more than four years of required attendance.

Example Compulsory Attendance Laws

*Oregon's 1889 compulsory attendance law*¹

Section 1. Every parent, guardian, or other person in this State having control or charge of a child or children between the ages of eight and fourteen years shall be required to send such child or children to a public school for a period of at least twelve weeks in each school year, of which at least eight weeks' school be consecutive, unless the bodily or mental condition of such child or children has been such as to prevent his or her or their attendance at school or application to study for the period required, or unless such child or children are taught in a private school or at home in such branches as are usually taught in primary schools, or have already acquired the ordinary branches of learning taught in the public schools; *provided*, in case a public school shall not be taught for the period of twelve weeks, or any part thereof during the year, within two miles by the nearest traveled road of the residence of any person within the school district, he or she shall not be liable to the provisions of this Act.

Section 2. Any parent, guardian or other person having control or charge of any child or children failing to comply with the provisions of this Act shall be liable to a fine of not less than five dollars nor more than twenty-five dollars for the first offense, nor less than twenty-five dollars nor more than fifty dollars for the second and each subsequent offense, besides the cost of the prosecution.

Section 3. It shall be the duty of the directors and clerk of each school district to make diligent effort to see that this law is enforced in their respective districts.

¹ 1889 Ore. Acts 111.

Section 4. Justices of the peace shall have concurrent jurisdiction with the circuit court in all prosecutions under this Act.

Section 5. Inasmuch as many children are now permitted to remain away from school without cause and to their great detriment, this law shall take effect and be in force from and after its approval by the Governor.

Approved February 25, 1889.

*Alabama's 1919 compulsory attendance law*²

ARTICLE 15. SCHOOL ATTENDANCE.

Section 1. Every parent, guardian or other person in the State of Alabama having control or charge of any child or children between the ages of eight (8) and sixteen (16) years shall be required to send such child or children to a public school or to a private, denominational or parochial school, taught by a competent instructor, and such child or children shall attend school for the entire length of the school term in every scholastic year, provided that the county or city board of education, as the case may be, shall have power to reduce the period of compulsory attendance to not less than one hundred (100) days for any individual school, unless the school term for any school is less than one hundred (100) days, in which event the period of compulsory attendance shall be for the entire school term. The period of compulsory attendance shall commence at the beginning of the

² 1919 Ala. Acts 615.

school, unless otherwise ordered by the county or city board of education, as the case may be, with the approval of the State board of education.

Section 2. Any child or children fourteen (14) years of age and upward, who have completed the elementary course of study or the equivalent thereof, or who are legally and regularly employed under the provisions of the Child Labor Law,³ shall be exempt from the provisions of this Article, and in case there be no public school within two and one-half miles by the nearest traveled road of any person between the ages of eight (8) and sixteen (16) years, he or she shall not be subject to the provisions of this article, unless public transportation within reasonable walking distance is provided.

Section 3. Any child or children who are physically or mentally incapacitated for the work of the school are exempt from this article, but the school authorities shall have the right, and they are hereby authorized, when exemptions under the provisions of this article are claimed by any parent, guardian or other person having control of any child or children, to require from a practicing physician a properly attested certificate, that such child or children should not be required to attend school for some physical or mental condition which renders attendance impracticable or inexpedient.

Sections 4-8 [describes duties of attendance officers]

³ The child labor law of 1919 provided for legal employment at age 14 so long as the child had a fourth-grade education. 1919 Ala. Acts 867.

Section 9. It shall be unlawful for any parent, guardian, or other person having control of a child to fail to send such child to school as required by the provisions of this article, and any parent, guardian, or other person violating the provisions of this article, shall be guilty of a misdemeanor, and shall be fined in a sum of not less than five (\$5) dollars, nor more than fifty (\$50.) dollars, and may be committed to the county jail for a term of not to exceed thirty (30) days. All fines collected shall be paid into the county or city treasury, as the case may be, and it is hereby made the duty of all city attorneys in their respective cities, and of all county and circuit solicitors for the respective counties and districts to prosecute all complaints filed and actions brought under this article.

Sections 10-15 [describes treatment of truants, state reporting, and school vacations]