

# Schooling Laws, School Quality, and the Returns to Schooling\*

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# Schooling Laws, School Quality, and the Returns to Schooling

## Abstract

Educational attainment increased sharply during the early 20th century as compulsory schooling laws became more stringent and school quality improved greatly. Causal estimates of the returns to schooling using state schooling laws as instruments may be biased if contemporaneous changes in school quality are ignored. We find that these estimates become insignificant and either close to zero or negative when we account for school quality. Additional analyses, motivated by stark regional differences in the relative gains in wages, schooling requirements, and school quality, show that these estimates are primarily identified by between-region differences rather than changes within states over time.

# 1 Introduction

The United States experienced a dramatic increase in educational attainment during the 20th century. While less than 25 percent of Americans born in 1930 attended college, over 60 percent of those born in 1970 did so (Goldin and Katz 2008). These gains were precipitated by the rapid expansion of secondary education, known as the “High School Movement,” which resulted in the high school graduation rate rising from 9 percent in 1910 to 51 percent in 1940 (Goldin 1998). Numerous factors contributed to the spread of high schools in the first part of the century including competition among localities to increase property values and a rising demand for educated workers (Goldin and Katz 2008). Prior research has also found that statewide policies such as compulsory schooling requirements and child labor restrictions led to significant increases in educational attainment (Lang and Kropp 1986; Acemoglu and Angrist 2000; Lleras-Muney 2002).

At the same time that high schools spread across the country, the quality of the education that students received was also changing. Along a number of quantifiable dimensions such as pupil/teacher ratios, the length of the school year, and relative teacher wages, the inputs into the schooling production function improved greatly during the first part of the 20th century (Card and Krueger 1992a). In a series of papers, Card and Krueger (1992a,b) find that gains in school quality had important effects on the adult wages of men who were in school during this period. They find that school quality improvements explain 20 percent of the increased returns to schooling for cohorts born in the 1920s and 1930s as well as 20 percent of the closing of the black-white wage gap among these cohorts. Card and Krueger also find that improving school quality led to increases in the levels of educational attainment among these cohorts. In fact, they note that “increases in school quality during the past century are associated with increases in years of schooling” (Card and Krueger 1992a, p.35). In related work they find that “virtually all of the increase in relative schooling of blacks is explainable by increases in relative quality” (Card and Krueger 1992b, p.192).

Figure 1 shows adult weekly wages, school drop out age, and select school quality measures for native white males born between 1910 and 1939, inclusive. Each panel displays the averages of these measures across U.S. Census regions for each birth cohort relative to men born in the

Northeast in the same year.<sup>1</sup> For example, Panel A of the Figure shows that while the average log weekly wage of white males born in the South in 1910 is 0.3 log points below that of men born in the Northeast, this regional gap had decreased to 0.13 log points for those born in 1939. Interestingly, most of the relative wage changes during this period are driven by Southern born men as there is little movement in relative wages for men born in the Midwest and West.<sup>2</sup> Paralleling these wage trends, Panel B indicates that the relative dropout age increased by nearly a full year for men in the South with little movement in the other regions.<sup>3</sup> Panels C and D show that school quality, as measured by pupil-teacher ratios and the length of the school term, was also rapidly improving in the South relative to the rest of the U.S. These figures, along with the findings by Card and Krueger (1992a,b), suggest that accounting for contemporaneous school quality improvements might be important when assessing the impact of schooling laws on wages. In addition, given that much of the observed relative wage movement occurs in the South, methods to identify the causal effect of schooling on wages need to carefully separate the growth in wages due to the schooling law changes from other forces, such as school quality improvements, that may have differentially impacted those born in the South.

In this paper, we examine the impact of including school quality when estimating the causal effect of schooling on wages using schooling laws as instruments. We use Card and Krueger's (1992a) quality measures which vary at the state of birth/year of birth level. The compulsory schooling law measures that are used most prominently in the literature are collected at roughly five-year intervals (Acemoglu and Angrist 2000). We introduce a new schooling law coding which relies on a wider range of sources including state session laws in order to provide a more precise timing of schooling law changes and find that these changes dramatically improve the first stage fit of the impact of schooling laws on completed schooling. Our coding of the laws also differs in other important ways including that we distinguish between the legal requirements for children to be able to stop attending school in order to work and the requirements of the child labor law which

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<sup>1</sup>Construction of the weekly wage, drop out age, pupil-teacher ratio, and term length measures used in this Figure are discussed below in the Data section of the paper.

<sup>2</sup>Absolute wages did increase by over a full log point, as measured at ages 40-49, between those born in 1910 and in 1939.

<sup>3</sup>The sharp change in the relative dropout age for the 1923 birth cohort is due to Pennsylvania's drop out age increasing from 16 to 17 in 1937.

may not apply when schools are in session.

Using the same sample of native born White males ages 40-49 from the 1960-1980 Censuses of Population that is used by Acemoglu and Angrist (2000), we find that including school quality measures dramatically reduces the explanatory power of the law instruments. While the instruments based on our new coding of the laws still produce F-statistics that are above the conventional weak instrument standard, the instruments most widely used in the literature fall well below this threshold. Regardless of the choice of instruments, the estimated causal effect of schooling on wages is insignificant and either close to zero and/or negative when accounting for school quality. These findings are in stark contrast to the prior literature which finds positive and significant effects of schooling on wages when using U.S. compulsory schooling laws as instruments.

As Figure 1 reveals, the majority of the relative movements over time in wages, schooling laws, and school quality is driven by changes in the South. Studies which estimate the causal effect of schooling using schooling law instruments typically include state of birth and year of birth fixed effects to account for differences in individuals born in different states and across different time periods.<sup>4</sup> While it is typically assumed that models which include state of birth and year of birth fixed effects are identified by variation within states over time, such an assumption is only correct if the counterfactual trend is common across all states. However, differential changes which occurred in the South during this period, such as the improvements in school quality (Card and Krueger 1992a) and the eradication of hookworm (Bleakley 2007), cast strong doubt on such an assumption. Thus, standard estimates of the returns to schooling may be driven by the improvements in the South relative to the rest of the country rather than by variation within states over time as is typically thought to identify these models.

We find that relative differences between regions are the driving force behind the positive and significant causal estimates of the returns to schooling. The causal estimates of the returns to schooling remain insignificant and either close to zero or negative when we adjust to specification along any of the following dimensions: measuring school quality at the region-year of birth level rather than the state-year of birth level, including region-year of birth indicators, and separately estimating the baseline specification for those born in the Southern states and those born in the

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<sup>4</sup>See, e.g., the survey of the literature by Oreopoulos and Salvanes (2011)

remaining U.S. states. Finally, we present a straightforward decomposition of the standard within-state of birth/within-year of birth estimator showing that it can be written as a weighted average of a) the within-state of birth/within-*region*-year of birth estimator which allows the year of birth fixed effects to differ by region of birth and b) the between-region estimator which is driven only by region-level changes over time. We find that the standard positive and significant estimate of the returns to schooling is driven entirely by differences between regions over time as opposed to variation within states over time which is typically thought to identify these models.<sup>5</sup> Overall, our findings suggest that changes in educational attainment driven by compulsory schooling laws had no effect on the subsequent earnings of the affected individuals.

How plausible are these estimates of a zero return to schooling? Within the Local Average Treatment Effect (LATE) framework, the 2SLS estimates of the returns to schooling are the impact of an additional year of schooling on wages for those students who were induced by the increased schooling requirements to receive more education. Although high discount rates may lead these marginal students to forgo schooling in the absence of a schooling law, low private returns in the labor market from additional education also may cause these students to opt out of school. In fact, the recent empirical literature which estimates the returns to schooling using compulsory schooling law changes in a number of countries finds either small or zero returns. Black, Devereux, and Salvanes (2005) find returns to schooling of four and five percent for men and women, respectively, due to Norwegian schooling reforms in the 1960s. Devereux and Hart (2010) find that the 1947 schooling reform in the UK yields estimated returns of seven and zero percent for men and women, respectively. Meghir and Palme (2005) find an overall small and insignificant return to schooling due to Swedish schooling reforms in the 1950s although they do find evidence of heterogeneous returns by father's education level. Pischke and von Wachter (2008) find zero returns to schooling in Germany following a post-World War II schooling expansion and Grenet (2010) finds no returns to schooling following a 1967 education reform in France. Thus, our findings are comparable to the growing body of international evidence on the returns to education identified using schooling law

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<sup>5</sup>Among the prior literature, only Lleras-Muney (2002,2005) includes region-year of birth indicators in the analysis. An unpublished early version of Acemoglu and Angrist (2000) included region of birth linear trends when identifying the social returns to schooling although they find that the social returns are insignificant both with and without these trends in the specification.

reforms.<sup>6</sup>

## 2 Data

Our main analysis uses Acemoglu and Angrist's (2000) primary sample of White males from the 1960-1980 Censuses of Population.<sup>7</sup> This sample is restricted to men who are ages 40-49 at the time of the Census which corresponds to the 1910-1939 birth cohorts. These birth cohorts comprise a substantial subset of the birth cohorts that are typically found in studies which use compulsory schooling and child labor laws as instruments.<sup>8</sup> The evidence on the efficacy of compulsory schooling laws is far more substantial for these cohorts than for more recent birth cohorts.<sup>9</sup> For our analysis, the demographic information that we require from the Census is age, quarter of birth, and state of birth in order to determine both the prevailing schooling laws and the quality of schooling that the sample members faced while in school. Years of schooling is measured as the highest grade completed. The log weekly wage is calculated by dividing annual wages by weeks worked.<sup>10</sup>

### 2.1 School Quality

The school quality measures that we use are those collected by Card and Krueger (1992a,b). These data are compiled from issues of the *Biennial Survey of Education* which contains the results of surveys of state education departments performed by the U.S. Office of Education from 1918 to 1958. From the wealth of school quality information available in these reports, Card and Krueger focus on the pupil/teacher ratio, the length of the school term, and average teacher salaries. For each of these quality attributes, Card and Krueger create a single measure for each state of birth/year of

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<sup>6</sup>As with the U.S., the estimates in the Norway, Germany, and Sweden also rely in variation in the introduction of stricter schooling requirements across states. The estimates in the UK and France rely on nationwide policy changes and, as such, exploit regression discontinuity methods.

<sup>7</sup>We are able to exactly re-create their dataset and results based on the code provided on-line. The original dataset is available on-line at <http://econ-www.mit.edu/faculty/angrist/data1/data/aceang00>.

<sup>8</sup>E.g., Lleras-Muney (2005) uses individuals born from 1900 to 1925, Lochner and Moretti (2004) use men born in 1900 to 1960, and Oreopolous (2006) uses cohorts born in 1900 to 1961.

<sup>9</sup>See, e.g., Lleras-Muney (2002) and Goldin and Katz (2008). There has been virtually little research establishing a causal link between schooling laws and educational attainment for more recent years. Edwards (1978) finds little evidence that, when accounting for possible endogeneity, schooling laws affected educational attainment for cohorts in school between 1940 and 1960.

<sup>10</sup>These measures are all found in the Acemoglu and Angrist (2000) dataset. See the Appendix of their paper for a detailed discussion of the creation of these variables.

birth cohort by averaging the prevailing measures during the years in which that cohort was ages six to seventeen. We follow the same aggregation procedure for school quality using the data from the *Biennial Survey of Education*.<sup>11</sup> Since our oldest cohort was born in 1910, we also make use of school quality data beginning in 1916 which is available in earlier years of the *Survey*.<sup>12</sup>

## 2.2 Schooling Laws

The primary state schooling law codings that we use in this paper are from our own coding of the schooling and child labor laws. We began by using as a framework the same sources on schooling and child labor laws found in Acemoglu and Angrist (2000) and Goldin and Katz (2003). Many of these published sources are available at roughly five-year intervals. We then proceeded to determine the exact year in which every law changed using a number of additional secondary sources. When we were unable to locate a secondary source or an apparent change did not appear in a source, we made use of various state session laws in order to confirm the change in the law as well as the year in which it took place. In situations where the codings of Acemoglu and Angrist and those of Goldin and Katz differed, we also used these sources and state session laws to reconcile these differences. In addition, we have sought verify each of the schooling and child labor law changes that occurred within each state as coded by Acemoglu and Angrist and by Goldin and Katz.<sup>13</sup>

Multiple aspects of state compulsory schooling laws and child labor laws are typically used to determine the minimum years of school that a child is required to attend. Compulsory schooling laws specify an entry age (the variable *entryage* in our state schooling law dataset) by which the child is required to attend school as well as a drop out or exit age (*exitage*) at which the child can choose to unconditionally no longer attend school. There are two primary types of exceptions frequently written into schooling laws that allow children to stop attending school before the exit age. The first type of exception allows children to stop attending if they have completed a specified number of schooling years (*earlyyrs*).<sup>14</sup> The second type of exception allows children to be excused

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<sup>11</sup>We thank David Card and Alan Krueger for making these data available to us.

<sup>12</sup>We thank Jeff Lingwall for making these data available to us. To create relative teacher wages, we also obtained the same market wage measures from the sources found in the Appendix to Card and Krueger (1992a).

<sup>13</sup>A file containing the results of these comparisons and explaining differences between our codings and those found in prior work is available from the authors upon request.

<sup>14</sup>In some instances these exceptions have both a minimum age and a completed years of schooling requirement.

from school attendance if they have secured employment and have also reached both a minimum age (*workage*) and years of schooling requirement (*workyrs*). States typically either the first or second type of exception; only in a handful of cases do the schooling laws provide both an exception with a work requirement and an exception without a work requirement.<sup>15</sup>

Child labor laws specify the age and/or completed schooling requirement needed to be reached in order to enter the labor force. In some instances, these child labor laws would explicitly specify the requirements needed to work during school hours. In other situations, however, these laws only specify the age at which the child could work when school was not in session (i.e., outside of school hours or during vacations). Thus, having achieved the minimum age and/or amount of schooling needed for a work permit does not necessarily provide an exception to the compulsory schooling law. We only code those features of the child labor law which allow children to forgo schooling in order to work. These schooling exceptions are coded in the aforementioned *workage* and *workyrs* variables in our schooling law dataset.

### 2.3 Calculating Required Schooling

Since state schooling laws change throughout this period, we construct a dynamic required schooling measure which accounts for the various updates to the compulsory attendance and child labor laws that may occur during the child’s school years. We label our measure Required Schooling ( $RS_{st}$ ) to denote that it varies within states  $s$  across birth cohorts  $t$  due to changes in state laws over time. For a given birth cohort from a given state, we iterate through each age, from age six through age seventeen, to determine whether the child is required to attend school at that age based in the law that is place in that same year.<sup>16</sup> By having iterated through all previous ages, we are able to also determine the number of years that the child would have been required to complete by the current age. In turn, we use the amount of required schooling at all previous ages to determine if either of the school attendance exceptions based on age and/or completed schooling may be applicable to the child at the current age.

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<sup>15</sup>There are other types of exceptions to the compulsory schooling laws which frequently include economic need of the family, physical or mental handicap, and living too far from the nearest school. Consistent with the prior literature, we have not coded these exceptions to the compulsory schooling law.

<sup>16</sup>We begin at age six since there is no required entry age younger than six during this period. Similarly, the oldest that we need to check is seventeen since the oldest exit age in our dataset is eighteen.

For each age as we iterate through the child's schooling years, if the student either has not reached their exit age or is not eligible to leave school based on an exception, we increment the number of required schooling years by one. Once the child has acquired enough years of schooling to satisfy the first type of schooling exception, which does not require work, we do not increment required schooling at future ages unless there is a change in the schooling statutes.<sup>17</sup> Similarly, once the student meets the minimum age and/or years of schooling requirements for the second type of schooling exception, which requires the child to be employed, we do not increment required schooling at future ages. Finally, once the child reaches the exit age, which allows for the child to stop attending schooling without any conditions, we stop incrementing our required schooling measure.

The compulsory schooling laws that primarily have been used in the prior literature are those constructed by Acemoglu and Angrist (2000) based on their coding of the laws.<sup>18</sup> Whereas we use a dynamic coding of the laws based on the prevailing statutes at each age from six to seventeen, Acemoglu and Angrist use the laws that are prevailing in the child's state of birth when that child is age fourteen. Since most states require students to attend through age fourteen for the birth cohorts that they examine, the main concern from using this static approach is that the entry age law prevailing when a birth cohort is age fourteen may be different than the actual one that affected school entry six to eight years prior.<sup>19</sup>

Rather than creating a single measure of required schooling, Acemoglu and Angrist compute two separate measures of compulsory schooling. The first compulsory schooling variable they calculate is based only on the compulsory attendance portion of the legal statutes. Their compulsory attendance

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<sup>17</sup>As we mentioned above, the non-work schooling exceptions typically only specify a number of years of completed schooling to exemption from further attendance. In those cases in which this type of exception also has a minimum age requirement, it is nearly universally true that entry age plus the completed years of schooling equals or exceeds the minimum age requirement. That is, the minimum ages given for these schooling exceptions are virtually never binding. As such, we have not separately coded these ages as is also true with previous codings (Acemoglu and Angrist 2000; Lleras-Muney 2002,2005; Goldin and Katz 2003).

<sup>18</sup>Lleras-Muney (2002,2005) and Goldin and Katz (2003) have also coded compulsory schooling laws. However, in both cases, they only code the laws from 1910 through 1939 whereas Acemoglu and Angrist (2000) code these laws for 1910 through 1978.

<sup>19</sup>E.g., Goldin and Katz make use of entry age laws at age 6 while measuring the remaining schooling law variables at age 14.

( $CA_{st}$ ) variable for those born in state  $s$  in year  $t$  is computed as

$$CA_{st} = \max\{\text{Dropout Age}_{st} - \text{Enrollment Age}_{st}, \text{Years of School Needed to Dropout}_{st}\}$$

where, as previously mentioned, the variables used to construct the measure are those prevailing in the individual's birth state when they were age fourteen. The first quantity in the max function, the difference between the enrollment and drop out ages (which are analogous to our *entryage* and *exitage* variables), computes the minimum number of years that an individual needs to attend school without making use of any exceptions. The second term in the max function is years of completed schooling after which the student can drop out without working (which is analogous to our *earlyyrs* variable). As Goldin and Katz (2003) have noted, since this second term in the max function is an exception which allows the child to leave before the exit age, the correct calculation of  $CA$  would use a min function. We return to the implications of using the max versus the min function below.

The second compulsory schooling variable they calculate is based primarily on the child labor law. The child labor ( $CL_{st}$ ) variable is computed as

$$CL_{st} = \max\{\text{Work Permit Age}_{st} - \text{Enrollment Age}_{st}, \text{Education for Work Permit}_{st}\}$$

The first quantity in the max function is the difference between the age at which the child can receive a work permit and the age at which they must enter school. The second quantity in the max function is the number of years of schooling needed to receive a work permit. The use of the max function is appropriate in this instance since both requirements are necessary to receive a work permit. However, as we mentioned above, eligibility for a work permit does not necessarily exempt the child from school attendance (although in some instances, these age and/or schooling requirements are the same). As such, the variables in our schooling dataset (*workage*) and (*workyrs*) do not correspond to the work permit portion of the child labor statutes.

Figure 2 displays the (weighted) distributions of our required schooling ( $RS$ ) measure along with the two Acemoglu and Angrist measures ( $CL$  and  $CA$ ) using the aforementioned Acemoglu

and Angrist sample of native born white men ages 40-49 in the 1960-1980 Censuses. As shown in Panel A of the Figure, the modal number of required years of schooling is eight with roughly equal shares of individuals required to attend more or less schooling than this modal amount. Compared to our *RS* measure, Acemoglu and Angrist’s child labor law measure (Panel B) yields a lower number of compulsory years of schooling. As discussed above, having satisfied the minimum age and/or years of schooling to receive a work permit does not necessarily exempt the child from school attendance. Thus, it is not surprising that *CL* produces lower levels of compulsory schooling than our *RS* measure.

The distribution of Acemoglu and Angrist’s compulsory attendance (*CA*) measure shown in Panel C of Figure 2 is quite different than that of either the *RS* or *CL* measures. Recall from our discussion above, however, that this measure is calculated using a max function although it should instead be computed using a min function. In particular, the *CA* measure implies that over 20 percent of individuals during this period were required to attend twelve years of schooling during this period. A growing number of states during this period implemented an exception allowing children to leave prior to the exit age if they have completed four years of high school.<sup>20</sup> These exceptions were intended to allow those students who had completed high school at an early age to be excused from further schooling rather than, as implied by the computation of *CA*, forcing all children to become high school graduates.

### 3 Empirical Methodology

Following the prior literature, the wage equation that we estimate using two stage least squares (2SLS) is

$$\ln(\textit{Weekly Wage})_{st,i} = \alpha \textit{Educ}_{st,i} + \boldsymbol{\chi}_s + \boldsymbol{\delta}_t + \boldsymbol{\beta} X_{st,i} + \epsilon_{st,i} \quad (1)$$

where  $\textit{Educ}_{st,i}$  is the years of schooling of individual  $i$  born in state  $s$  in year  $t$  while  $\boldsymbol{\chi}_s$  and  $\boldsymbol{\delta}_t$  are vectors of state of birth fixed effects and year of birth fixed effects, respectively. Our baseline regressions do not include any additional regressors such as race, gender, or age in equation (1)

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<sup>20</sup>Five states had such laws in 1928, the year in which the youngest (1910) birth cohort in the sample was eighteen. Sixteen states had these exceptions as of the time that oldest (1939) sample birth cohort was eighteen while twenty-one states had them in 1978.

since the sample is restricted to White males and the set of year of birth dummies account for each of the possible ten age groups for each Census year.<sup>21</sup> The additional regressors that we include in some specifications, represented by the  $X_{st,i}$  in equation (1), are the school quality measures that we discussed above.<sup>22</sup>

The corresponding first stage equation that we estimate is

$$Educ_{st,i} = \pi CSL_{st} + \lambda_s + \theta_t + \nu X_{st,i} + \nu_{st,i} \quad (2)$$

where  $\lambda_s$  is a vector of state of birth fixed effects and  $\theta_t$  is a vector of year of birth effects. The  $CSL_{st}$  instruments are the schooling law variables that are represented by one of the aforementioned three sets of instruments ( $RS$ ,  $CA$ ,  $CL$ ). Since this equation includes both state of birth and year of birth fixed effects, the coefficients on the  $CSL_{st}$  instruments are identified by both variation in laws across states for each birth cohort as well as variation within states across birth cohorts.

Identifying the impact of compulsory schooling laws on education using equation (2) requires the strong assumption that, in the absence of the schooling laws, there would be a common national trend in education as captured by the birth cohort indicators,  $\theta_t$ . However, as shown above, not only did the education levels differ greatly across the U.S. at the beginning of the sample period, but so did the quality of education. Moreover, the greatest improvements in education occurred in the Southern U.S. which also contemporaneously experienced the largest increases in the required amount of schooling. Given that Card and Krueger (1992a,b) find that school quality improvements led to increased years of schooling, failing to control for school quality in equation (2) may impart an omitted variables bias on the estimates of the effectiveness of compulsory schooling laws and the returns to schooling.

When using a mutually exclusive set of binary indicators  $z_j, j = 1, 2, \dots, r$  as instruments for a

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<sup>21</sup>Recall that we only use men ages 40-49 in each Census year.

<sup>22</sup>Prior papers have also included state of residence fixed effects in wage equations to account for differences in labor market conditions across states that might affect wages (E.g., Card and Krueger (1992a,b)). Including these regressors has very little effect on our estimates of  $\alpha$  across the various specifications we examine. These results are available from the authors.

single endogenous regressor  $x$ , Angrist (1988) shows that the 2SLS estimator can be written as

$$\hat{\alpha}^{2SLS} = \sum_{j=1}^r a_j \left( \frac{\bar{y}_j - \bar{y}_0}{\bar{x}_j - \bar{x}_0} \right) = \sum_{j=1}^r a_j \hat{\alpha}_j^{WALD} \quad (3)$$

where  $\bar{x}_j$  and  $\bar{y}_j$  are the average values of  $x$  and the outcome variable,  $y$ , respectively, for sample observations where  $z_j = 1$  and  $\bar{x}_0$  and  $\bar{y}_0$  are the corresponding averages for the excluded group (i.e., observations for which  $z_j = 0$  for all  $j = 1, 2, \dots, r$ ). Thus, the 2SLS estimator under these conditions can be written as a weighted sum of the pairwise Wald estimators,  $\hat{\alpha}_j^{WALD}$ , which are computed by separately comparing each of the  $r$  groups to the excluded group. The weights,  $a_j$ , can be shown to sum to one although each individual weight is not constrained to be non-negative.<sup>23</sup>

The formulation of the 2SLS estimator in (3) suggests another useful equation to estimate. The denominator of each pairwise Wald estimator,  $\bar{x}_j - \bar{x}_0$ , is the difference between the average value of  $x$  for the included group  $j$  and for the excluded group. These differences are simply the estimated coefficients on the mutually exclusive binary instruments  $z_j, j = 1, 2, \dots, r$  in the first stage regression. Analogously, the numerators of the pairwise Wald estimators,  $\bar{y}_j - \bar{y}_0$ , are the estimated coefficients on the instruments  $z_j, j = 1, 2, \dots, r$  in the reduced form regression of  $y$  on the  $z_j$ . Thus, to provide additional insight into our estimates of  $\hat{\alpha}^{2SLS}$ , we also estimate the reduced form regression of wages on the schooling law instruments

$$\ln(\textit{Weekly Wage})_{st,i} = \gamma \textit{CSL}_{st} + \boldsymbol{\rho}_s + \boldsymbol{\tau}_t + \mathbf{\Gamma} X_{st,i} + \varepsilon_{st,i} \quad (4)$$

where  $\boldsymbol{\rho}_s$  is a vector of state of birth fixed effects and  $\boldsymbol{\tau}_t$  is a vector of year of birth effects.

As we find below, some specifications yield F-statistics on the excluded instruments that fall into ranges at which instruments are conventionally considered to be weak. As is well-known, applying 2SLS with weak instruments will not only yield biased point estimates, but the standard 2SLS confidence intervals are incorrect (Nelson and Startz 1990; Stock and Staiger 1997). Given concerns with the bias in the 2SLS point estimates, we also report limited information maximum likelihood

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<sup>23</sup>As shown in Appendix 1 of Angrist (1988), the equation for the weights is  $a_j = [(\bar{x}_j - \bar{x})(\bar{x}_j - \bar{x}_0) n_j] / [\sum_{k=0}^r (\bar{x}_k - \bar{x})^2 n_k]$  where  $\bar{x}$  is the overall average of  $x$  and  $n_j$  is the number of observations with  $z_j = 1$ .

(LIML) estimates of the wage equation which are median unbiased even when the instruments are weak (Stock and Staiger 1997). Multiple methods have been developed for constructing confidence intervals with weak instruments including the Anderson-Rubin statistic (Anderson and Rubin 1949), a Lagrange multiplier statistic (Kleibergen 2002), and the conditional likelihood ratio (CLR) test (Moreira 2003). The CLR test is “nearly” optimal among these methods when the model has i.i.d. normally distributed errors (Andrews, Moreira, and Stock 2007). However, studies using schooling law instruments typically assume that the error terms are correlated among those born in the same state of birth/year of birth cell. Thus, in addition to the usual 2SLS confidence intervals, we report CLR confidence intervals that allow for clustering using the methods discussed in Finlay and Magnusson (2009).<sup>24</sup>

## 4 Results

### 4.1 First Stage Estimates

Our estimates of the first stage equation (2) are reported in Table 1. We present separate results for each of the three sets of instruments (*RS*, *CL*, and *CA*), both with and without the school quality measures. Estimates of the baseline specification found in columns (1), (3), and (5) yield statistically significant effects of the child labor and compulsory schooling laws on years of schooling.<sup>25</sup> The magnitudes of the point estimates are also consistent with more stringent laws leading students to remain in school longer. For example, the point estimates in column (1) indicate that relative to students who were required to obtain six or less years of schooling due to the prevailing schooling laws, those that were required to complete exactly seven years of schooling had their average years of schooling increase by nearly one-tenth of a year. The point estimate doubles in magnitude for those required to complete exactly eight years of schooling and the impact increases to over four-tenths

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<sup>24</sup>The CLR confidence intervals are computed using version 1.0.7 of the `-rivtest-` command for Stata (Finlay and Magnusson 2009). This command also allows for the use of inverse probability weights which are required since the sampling rate for publicly available Census varies over time: the 1960 Census is a 1 in 100 sample, the 1970 is 1 in 50, the 1980 is 1 in 20. We find qualitatively similar confidence intervals when we calculate Chernozhukov and Hansen’s (2008) Anderson-Rubin confidence intervals which also allow for clustering and sampling weights. Results using this alternative method of computing the confidence intervals are available from the authors.

<sup>25</sup>Our estimates in columns (3) and (5) reproduce those found in Acemolgu and Angrist (2000) in their Table 4, column (5), panels (a) and (b).

of a year of school for those needing to complete at least nine years of school. In addition, the F-statistics for the excluded instruments using the baseline specification are all above the conventional weak instrument threshold of 10. Finally, our *RS* instrument explains more variation in education than either the *CL* or *CA* instruments with an F-statistic that is more than twice as large as the one for the *CL* instrument and that is over three times as large as that of the *CA* instrument.

When we include the state of birth/year of birth school quality measures (columns (2), (4), and (6) of Table 1), the estimated impact of schooling laws on educational attainment changes dramatically. Consistent with Card and Krueger's (1992a) findings that school quality significantly affected completed years of schooling, we find that both lower pupil-teacher ratios and higher relative teacher pay have consistently strong, statistically significant effects while increasing the term length results in a positive but not always significant effect. Moreover, the inclusion of these school quality measures has a large effect on the estimated impact of the instruments. The magnitude of the estimated coefficients on the Required Schooling (*RS*) instrument indicators drops dramatically. The F-statistic on the excluded instruments, although falling as well, remains above the conventional weak instrument threshold while the estimates continue to show that increasing required schooling leads to more educational attainment. However, the partial R-squared falls by over two-thirds, from 0.00053 to 0.000015, indicating that a substantial amount of the variation in years of schooling previously being attributed to the instruments is now explained by the quality measures.

The magnitude of the *CL* estimates drop appreciably when including the school quality measures (column (4) of Table 1). The impact of having seven years of schooling falls by one-third, the impact of exactly eight years of schooling almost entirely disappears, and the impact of nine or more years falls by over one-half. Thus, the effect of increasing the schooling needed to obtain a work permit no longer has an estimated monotonic impact on completed schooling. In addition, the F-statistic falls by over fifty percent and is below the conventional weak instrument threshold.

The impact of including the school quality measures is far more dramatic on the compulsory attendance (*CA*) law estimates. As shown in column (6) of Table 1, none of the *CA* estimates is statistically significant when accounting for school quality with the coefficient on being compelled to attain nine years schooling actually becoming negative. Interestingly, the change in the

significance levels is driven entirely by the large reductions in the point estimates on the *CA* indicators as opposed to an increase in the standard errors of these estimates. The F-statistic for the compulsory schooling instruments is reduced to 1.5 and the partial R-squared falls by over eighty percent. Without relying on literature for inference with weak instruments, we cannot credibly draw inferences for the returns for schooling when using compulsory attendance instruments in this specification.

## 4.2 Estimates of The Causal Effect of Schooling on Wages

Table 2 presents our estimates of the causal effect of schooling on wages. Since we find that the F-statistics on the *CL* and, especially, *CA* instruments fall below the weak instrument threshold when we include the school quality measures, both the 2SLS estimates of the return to schooling and their confidence intervals are likely biased. To account for these concerns, we also present the LIML point estimates and the conditional likelihood ratio confidence intervals that we describe in the Empirical Methodology section.

Our results using the baseline specification are shown in columns (1), (3), and (5) of Panel A in Table 2. For all three instrument sets, the causal impact of schooling is positive and significant with the magnitudes well within the range found in the prior literature.<sup>26</sup> Since the F-statistic is well above the conventional weak instrument threshold in these three columns, there is little concern with finite sample bias in either the 2SLS point estimate or with using the 2SLS confidence intervals for inference. In fact, for these specifications, the 2SLS and LIML estimates are nearly identical to each other while the 2SLS confidence intervals are quite similar to the CLR confidence intervals shown a few rows below.

The reduced form regression results shown in Panel B of Table 2 for the baseline specification are also consistent with a causal interpretation for the impact of schooling laws on wages. When using either the *RS* or *CL* instrument sets (columns (1) and (3)), the reduced form results show that increasing the required amount of schooling leads to higher wages. Furthermore, when combined with the corresponding first stage estimates found in Table 1, all of the pairwise Wald estimates

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<sup>26</sup>E.g., see Card (1999) for a survey of the literature. The estimated magnitudes of the 2SLS results in columns (3) and (5) exactly reproduce the findings in Acemolgu and Angrist (2000) shown in their Table 6, columns (4) and (7).

found in Angrist’s formula for the 2SLS estimator (equation (3)) all indicate that stricter schooling laws yield higher wages. The reduced form results for the *CA* instrument set do not exhibit the same monotonic pattern in the baseline specification (column (5)) but the 2SLS and LIML estimates, nonetheless, are positive and significant.

When we add the school quality measures to the specification, the causal estimates of the return to schooling are greatly diminished. Both estimators produce insignificant results when using the *RS* instruments with the 2SLS estimate of the return to schooling falling to 0.029 while the LIML estimate is 0.010 (column (2) of Table 2). Furthermore, the CLR confidence interval is centered near zero. We yield similar findings when using the *CL* instruments (column (4)) except that the point estimates become negative and the CLR confidence interval lies mainly on the negative side of zero. For both of these instrument sets, all of the reduced form coefficients on the instruments are negative and insignificant after accounting for school quality. All of these results indicate that there is no significant effect of schooling on wages that is induced by the schooling law changes.

Interestingly, when including the school quality measures, the *CA* instruments yield rather large and significant causal estimates of the return to schooling (column (6) of Table 2). However, there are two caveats to this finding. First, the first stage F-statistic of 1.7 suggests that the results should be interpreted with much caution. The CLR confidence interval includes only positive numbers with a lower bound of 0.13 and an upper bound that exceeds 10.<sup>27</sup> Second, and substantially more troubling, the reduced form estimates found in Panel B of the Table yields negative coefficients on all of the *CA* instruments with two of the three estimates being statistically significant.<sup>28</sup> Thus, coupled with the aforementioned concerns about the construction of the *CA* instrument, we are rather hesitant to interpret the positive and significant estimate of the return to schooling in column (6) as a credible empirical finding.

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<sup>27</sup>We stopped the iterative procedure used to produce the CLR confidence interval at 10, or a return to schooling of 1000%.

<sup>28</sup>The reduced form estimates on the three included schooling law variables shown in column (5) of Table 2 are -0.015, -0.017, and -0.005 while the corresponding first stage estimates shown in Table 1 are -0.032, 0.008, and 0.024. Thus, the three pairwise Wald estimators are, approximately, 0.47, -2.1, and -0.21 which yields a rather broad range of estimates which are weighted (unequally) when generating the 2SLS estimate.

### 4.3 The Importance of Region of Birth

We next turn to investigating why including school quality measures in the specification yields insignificant causal estimates of the return to schooling. The empirical specifications that we estimate are typically thought to identify the parameters of interest by relying on the variation that occurs within states over time. One possible objection to our analysis is that by measuring school quality at the state of birth/year of birth level we have eliminated much of the within-state variation available to identify the effect of education on wages.

To address this concern, we estimate an alternative specification in which we measure school quality at the U.S. Census region of birth/year of birth level rather than at the state of birth/year of birth level. This approach still accounts for the stark differences in school quality across regions shown in Figure 1. However, at the same time, these measures allow the analysis to retain all of the within-state variation in wages, schooling attainment, and schooling laws.

The results of using region of birth/year of birth school quality measures are shown in column (1) of Table 3. Using the *RS* instruments, we yield similar findings as when include school quality measured at the state of birth/year of birth level (column (2) of Tables 1 and 2). As shown in Panel A, both the 2SLS and LIML estimates are close to zero and the CLR confidence interval is also centered near zero. The first stage estimates, shown in Panel B, are nearly identical to those presented above except that the first stage F-statistic falls slightly to 9.7. Although not shown here, for both the *CL* and *CA* instrument sets, including region of birth/year of birth school quality measures yields results very similar to those that we find when including state of birth/year of birth school quality measures.<sup>29</sup>

Of course, there are numerous changes occurring in the United States during this period beyond those related to schooling that differentially impacted the regions of the country such as the substantial changes in the industrial mix and the ending of Jim Crow laws. To provide a more general specification to capture these differences, we replace the region of birth/year of birth school quality measures with a complete set of indicators for each region-year of birth group used in our analysis. As shown in column (2) of Table 3, we yield negative but insignificant causal estimates

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<sup>29</sup>These results are available from the authors upon request.

of the return to schooling. The first stage F-statistic falls slightly to 8.2 although the pattern of the estimated coefficients shown in Panel B is still consistent with stricter schooling laws leading to higher levels of education even if it does not yield higher wages.

Since the causal estimate of the return to schooling changes from being positive and significant (column (1) of Table 2) to negative and insignificant (column (1) of Table 3) when adding region of birth/year of birth indicators, it raises the question of whether the former findings are identified by differences between regions rather changes within states over time as is commonly assumed when using the baseline specification. One approach to investigating this issue is to estimate the baseline specification separately by region of birth. In columns (3) and (4) we separate the sample between those born outside of the Southern states and those born in the Southern states. For the non-Southern born sample, the estimated return to schooling is negative and significant (Panel A) even though the first stage estimates are consistent with stricter schooling laws leading to higher educational attainment and yield a large first stage F-statistic of 32.8 (Panel B). The causal estimate is positive but insignificant for the Southern born sample, although the first stage F-statistic is quite small (1.6) rendering inference rather difficult as can be seen by the width of the resulting CLR confidence interval. Comparing the 2SLS return to schooling in the full sample using the baseline specification is 0.095 to the corresponding estimates -0.020 and 0.052 found using the two regional sub-samples suggests that between-region differences may be an important factor in identifying the returns to schooling.

#### 4.4 The Role of Between-Region Differences

In order to explicitly demonstrate the role of between-region differences in the identification of the 2SLS estimate, we apply the standard within-between decomposition (e.g., Greene 2008). For the two variable regression of  $y_{gt}$  on  $x_{gt}$  using group-level panel data, where  $g$  indexes group and  $t$  indexes year, recall that the pooled OLS estimator can be decomposed into a weighted average of the within and between estimators. We denote  $\hat{\psi}_{Total}$  as the estimator of  $\psi$  in the pooled regression model

$$y_{gt} = \sigma + \psi x_{gt} + u_{gt}$$

$\hat{\psi}_{Between}$  as the estimator of  $\psi$  in the between-group regression

$$\bar{y}_g = \sigma + \psi \bar{x}_g + \bar{u}_g.$$

and  $\hat{\psi}_{Within}$  as the estimator of  $\psi$  in the within-group regression

$$y_{gt} - \bar{y}_g = \psi (x_{gt} - \bar{x}_g) + u_{gt} - \bar{u}_g.$$

The resulting decomposition of the pooled estimator is (e.g., Greene (2008, p.191-2))

$$\hat{\psi}_{Total} = F_{Within} \cdot \hat{\psi}_{Within} + (1 - F_{Within}) \cdot \hat{\psi}_{Between}$$

where  $F_{Within}$  is the fraction of the total variation in  $x_{gt}$  that occurs within-group.

In multivariate settings, where the pooled regression model contains the vector of regressors,  $X_{gt}$ , which are multiplied by the parameter vector  $\Psi$ , the decomposition becomes

$$\hat{\Psi}_{Total} = \mathbf{F}_{Within} \cdot \hat{\Psi}_{Within} + (\mathbf{I} - \mathbf{F}_{Within}) \cdot \hat{\Psi}_{Between} \quad (5)$$

where  $\mathbf{F}_{Within} = [\mathbf{S}_{xx}^{Total}]^{-1} \mathbf{S}_{xx}^{Within}$ ,  $\mathbf{S}_{xx}^{Total}$  is the matrix of total sums of squares and cross products, and  $\mathbf{S}_{xx}^{Within}$  is the within-group sums of squares and cross products. The diagonal of  $\mathbf{F}_{Within}$  contains the fraction of the total variation occurring within-group for each of the regressors. Thus, in the multivariate context, the decomposition depends not only on the coefficients on the regressor of interest as well as the share of within-group variation in that regressor, but also upon the coefficients on the other regressors to the extent that they are correlated with the regressor of interest.

We use this decomposition to link our baseline specification, which includes state of birth and year of birth indicators (“within-state/within-year estimator”), to the specification which includes state of birth and region by year of birth indicators (“within-state/within-region-year estimator”, i.e., column (2) of Table 3). It is straightforward to decompose the standard within-state/within-year estimator into the within-state/within-region-year estimator and the between-state-region-year

estimator where the latter estimator is derived by estimating the baseline specification using state of birth/region-year of birth cell averages.<sup>30</sup> As we show below, this decomposition reveals that the 2SLS causal estimate of the returns to schooling using the standard within-state/within-year estimator are driven primarily by the between-region-year estimator rather than changes within states over time as is commonly believed.

We apply this decomposition to the first stage, reduced form, and 2SLS estimates of the returns to schooling. To simplify the exposition, we use a continuous version of the *RS* instrument rather than the three categorical variables used above. In this way, the relevant first stage and reduced form estimates are found in a single parameter for each specification, the coefficient on *RS*. Furthermore, since it is well-known that the 2SLS estimator can be written as the ratio of the first stage and reduced form estimates when using a single instrument, it clarifies what is driving the identification of the causal estimate.

Before proceeding, we should note that we implement the decomposition found in (5) by applying it to the weighted least squares (WLS) estimator. Since the Census samples come from years with different sampling schemes (1-in-100 in 1960, 1-in-50 in 1970, and 1-in-20 in 1980), our analysis must make use of the sampling weights. We perform our decomposition on the WLS specification by first transforming the variables in the specification by multiplying them by the square root of the sampling weight, applying OLS using the transformed variables, and then decomposing the regression.

Table 4 presents the results of applying the decomposition in (5) to the four U.S. Census regions using the single, continuous *RS* instrumental variable. Column (1) displays the fraction of the variation in *RS* from the standard within-state/within-year estimator that can be attributed to

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<sup>30</sup>It is well known that the standard regression of  $y_{st}$  on  $x_{st}$  when including state and year fixed effects is equivalent to the two-variable regression of the within-state/within-year transformations of these variables. This transformation, applied to a given variable  $w_{st}$ , is  $w_{st} - \bar{w}_{s.} - \bar{w}_{.t} + \bar{w}_{..}$  which is created by subtracting the state-average,  $\bar{w}_{s.}$ , and the year-average,  $\bar{w}_{.t}$ , from the original variable and then adding back the overall average,  $\bar{w}_{..}$ . Averaging this transformation at the region-year level yields the transformed variables for the between region regression,  $\bar{w}_{.t}^R - \bar{w}_{..}^R - \bar{w}_{.t} + \bar{w}_{..}$  where the  $R$  superscript denotes an average at the region-year level. The within transformation, which is created by differencing the previous two transformations, is therefore  $w_{st} - \bar{w}_{s.} - \bar{w}_{.t}^R + \bar{w}_{..}^R$ . Inspection of this last transformation reveals that the within transformation is equivalent to including state fixed effects (corresponding to the  $\bar{w}_{s.}$  term in this transformation) and region-year fixed effects ( $\bar{w}_{.t}^R$ ) in the regression of  $y_{st}$  on  $x_{st}$ . Since our Census sample contains multiple observations for each state of birth/year of birth cell and also requires the use of sampling weights, applying these transformations only yields an approximate decomposition. To yield an exact decomposition, we must therefore apply equation (5).

the two parts of the decomposition. Two-thirds of the variation in  $RS$  occurs within-state/within-region-year while the remaining one-third of the variation occurs between region-year. Thus, a sizable amount of weight in the decomposition will be placed on the between region-year estimator.<sup>31</sup>

The decomposition of the first stage estimate is shown in column (2) of Table 4. The within-state/within-year estimate on the continuous required schooling variable is 0.126. This estimate is more than twice as large as the result found when using the within-state/within-region-year estimator, 0.052. However, this smaller coefficient on  $RS$  when allowing the effect of year of birth to vary by region of birth is statistically significant and is consistent with our results in Table 3 which show that including region-year of birth indicators reduces the magnitude of the estimates on the  $RS$  indicators. Interestingly, the between region-year first stage estimate of 0.100 is nearly twice as large as the corresponding within-state/within-region year estimate. This result is striking since the between-region estimator is identified by the regional-level trends in schooling laws shown in Figure 1. Any factor that led to these time-varying differences between regions may have contributed to the significant result for the between-region first stage estimate. For example, the improvements in school quality (Card and Krueger 1992a) and the eradication of hookworm (Bleakley 2007) disproportionately favored relative increases in education levels among Southerners born during this period while their region contemporaneously experienced a relatively larger increase in required years of schooling.

The importance of between region-year variation in identifying the 2SLS estimate is quite apparent in the reduced form estimates shown in column (3) of Table 4. While the within-state/within-region-year coefficient on  $RS$  is negative and insignificant (-0.004), the between region-year estimator is positive and significant. Since the standard within-state/within-year estimator produces a positive and significant result, this finding is driven entirely by the between region-year differences in wages that are correlated with regional level schooling law differences rather than being due to changes in laws within states over time.

The 2SLS estimates in column (4) of Table 4 follow a similar pattern to the first stage and reduced form estimates shown in the preceding columns. The within-state/within-year 2SLS estimate

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<sup>31</sup>As we noted above, the decomposition (5) will also depend upon the estimates on the remaining regressors as well.

of the returns to schooling is 0.092 and is highly significant. However, the within-state/within-region-year estimate is negative while the between region-year 2SLS estimator yields a rather large estimate of the returns to schooling of 0.356. Although these results are expected given our findings in the previous two columns, relative to the common intuition that the estimated returns to schooling when using labor laws as instruments are identified by within-state changes over time, these results are rather surprising.

As a point of comparison, the final column in Table 4 presents a decomposition of the returns to schooling estimated using OLS. For this decomposition, it is the within-state/within-year variation in observed years of schooling that determines the weights in the decomposition rather than the variation of the transformed  $RS$  as in the prior columns. Over 99 percent of the variation in years of schooling occurs within-state/within-year whereas two-thirds of the variation in  $RS$  occurs within-state/within-year. The reason for this difference is that changes in schooling laws occur rather infrequently within states which leads to a larger amount of the variation in the schooling laws occurring between regions, especially given the lagging behind of the Southern U.S. states in strengthening their required years of schooling. Notice that estimates produced by the within-state/within-region-year estimator (0.073) and the between-region estimator (0.065) are similar. However, given that virtually all of the variation in observed years of schooling occurs within-region, the within-state/within-year estimates and within-state/within-region-year estimates are essentially identical.

Although Acemoglu and Angrist's primary sample of 40-49 year olds in the 1960 to 1980 covers a substantial subset of the birth cohorts (1910-1939) that have been examined in this literature, some studies which use schooling laws as instruments have included a wider range of cohorts.<sup>32</sup> Thus, we use a broader sample, also available from Acemoglu and Angrist, by expanding the age range in the 1960-1980 Census to cover prime age White males ages 25-54 which includes the birth cohorts from 1905 to 1954. Unlike when the age range is restricted to 40-49 year olds, using individuals ages 25-54 will include some birth cohorts in multiple Census years. To account for the repeated use of birth cohorts over Census years, we expand the specifications in equations (1), (2), and (4)

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<sup>32</sup>E.g., Lochner and Moretti (2004) use men born in 1900 to 1960. Oreopolous (2006) uses cohorts born in 1900 to 1961.

to include a quartic in age as well as indicators for the 1970 and 1980 Census years. The additional cohorts expand the sample size from 609,852 to 2,166,387.

The decompositions for the sample of 25-54 year olds are shown in Table 5. The fraction of the variation in the transformed  $RS$  variable that occurs between regions is 0.191, which is somewhat smaller than that found in Table 4 although, as before, it is far larger than the between-region variation in the transformed years of education of 0.002 for this sample. The first stage estimates of the impact of schooling laws on educational attainment are positive and statistically significant across all three specifications. The baseline within-state/within year estimates shown in the first row of the Table are slightly smaller than we found for sample of 40-49 year olds, although the estimates are highly significant. The 2SLS estimate of the returns to schooling becomes negative and significant once region of birth indicators are included in the analysis. Once again, the between-region estimates are positive and significant and much larger in magnitude than the within-state/within-region-year estimates and, as the decomposition shows, are the driving force behind the positive and significant baseline causal estimate of the returns to schooling.

Overall, these decompositions highlight why including school quality, even measured at the region-year level, has such a strong effect on the estimated return to schooling. The standard within-state/within-year estimates are greatly influenced by between-region variation as opposed to the within-state variation in schooling laws that is typically thought to be the important determinant of this identification strategy.

## 5 Discussion

As compulsory schooling laws increased the years of education required of U.S. children during the first part of the 20th century, the quality of the education obtained by these children was improving greatly. Estimates of the returns to schooling which use legislative changes as instruments typically ignore the contemporaneous changes in school quality which have been found to significantly affect educational attainment. Interestingly, the Southern U.S. states experienced both the largest gains in school quality as well as the greatest increases in required schooling during this period. We find that the positive and highly significant estimates of the returns to schooling when using these laws

as instruments become insignificant and close to zero and/or even negative once we account for the state of birth/year of birth school quality measures used by Card and Krueger (1992a,b). Even our new coding of the schooling laws, which dramatically increases the first stage fit of compulsory schooling laws, yields small and insignificant estimates of the returns to schooling.

Our additional analysis points to the importance of regional differences in explaining our findings. Including school quality measured at the region-year of birth level or including region-year of birth indicators yields insignificant estimates of a causal effect of schooling on wages even though the first stage relationship between compulsory schooling laws and completed years of schooling remains statistically significant. Furthermore, separately examining estimating the returns for the South and the Non-South yields small and insignificant results which casts doubt on interpretation that increases in schooling due to legal changes led to higher wages. Rather, as our decomposition of the standard within-state/within-year estimator demonstrates, the causal estimates of the returns to schooling are driven by differences between regions over time rather than changes within states.

This paper focuses on the importance of contemporaneous school quality changes and, more broadly, accounting for regional differences when estimating the returns to schooling using schooling laws as instruments. There are a number of other issues that have been raised in the related literature that we do not address in this paper but quickly note. The standard errors are typically clustered at the state of birth/year of birth level in this literature although, following Bertrand, Duflo, and Mullainathan (2004), many studies cluster at the state level when using specifications of this type. Although we follow the prior literature in our study and cluster at the state of birth/year of birth level, clustering does not affect the point estimates which are the focus of our analysis. In addition, previous research has questioned whether state-level school quality measures are in fact measuring only variation in school inputs across states and over time or if these measures are correlated with other time-varying factors (Loeb and Bound 1996). Whether school quality measures are taken as exogenous or endogenous does not affect our analysis since in either case the implications of our findings for using schooling laws as instruments for educational attainment are the same.

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**Table 1 - First Stage Estimates: The Impact of Schooling Laws on Education**

Instrument Set:	Required Schooling RS7, RS8, RS9 (1) (2)	Child Labor CL7, CL8, CL9 (3) (4)	Compulsory Attendance CA9, CA10, CA11 (5) (6)
<b>Instruments:</b>			
RS7/CL7/CA9	0.095 (0.036)***	0.105 (0.032)***	0.103 (0.025)***
RS8/CL8/CA10	0.224 (0.032)***	0.120 (0.028)***	0.106 (0.030)***
RS9/CL9/CA11	0.404 (0.040)***	0.269 (0.038)***	0.184 (0.032)***
<b>State/Year of Birth</b>			
<b>School Quality Controls:</b>			
Pupil/Teacher Ratio	-0.043 (0.006)***	-0.046 (0.006)***	-0.045 (0.006)***
Term Length	0.002 (0.002)	0.002 (0.002)	0.004 (0.002)**
Relative Teacher Pay	0.411 (0.073)***	0.517 (0.077)***	0.478 (0.076)***
F-Stat for Instruments	42.8	18.6	11.5
Partial $R^2$	0.00053	0.00021	0.00015
		7.4	1.7
		0.00007	0.00002

Note: Each column contains the results of estimating a variant of (2). All regressions include also state of birth and year of birth fixed effects. The standard errors reported in parentheses allow for the correlation of the error terms within each state of birth/year of birth cell. \*, \*\*, and \*\*\* represent significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

**Table 2 - Returns to Schooling: Causal and Reduced Form Estimates**

Instrument Set:	Required Schooling RS7, RS8, RS9 No Yes (1) (2)	Child Labor CL7, CL8, CL9 No Yes (3) (4)	Compulsory Attendance CA9, CA10, CA11 No Yes (5) (6)
<i>A. Causal Estimates of the Returns to Schooling</i>			
2SLS	0.095 [.064,.127]	0.029 [-.025,.084]	0.101 [.043,.159]
LIML	0.096 [.064,.127]	0.010 [-.069,.090]	0.106 [.038,.174]
CLR Conf. Int.	{.064,.126}	{-.056,.079}	{-.063,.199}
F (First Stage Excluded Instr.)	42.8	13.8	11.5
<i>B. Reduced Form Estimates</i>			
Instruments:			
RS7/CL7/CA9	0.013 (0.009)	-0.017 (0.008)**	0.015 (0.005)***
RS8/CL8/CA10	0.022 (0.008)***	-0.025 (0.007)***	0.00001 (0.007)***
RS9/CL9/CA11	0.041 (0.009)***	-0.019 (0.009)**	0.017 (0.006)***
F (Red. Form Instr.)	10.1	5.1	6.0
(P-Value)	(<0.0001)	(0.002)	(0.0005)
N	609,852	609,852	609,852

Note: Each column of Panel A contains both the 2SLS and LIML results from estimating a variant of (1). Each column of Panel B contains the corresponding results of estimating a variant of (4). All regressions include also state of birth and year of birth fixed effects. The standard errors reported in parentheses allow for the correlation of the error terms within each state of birth/year of birth cell. The confidence intervals based on Moreira's Conditional Likelihood Ratio Test shown in braces also allow for the correlation of the error terms within each state of birth/year of birth cell. \*, \*\*, and \*\*\* represent significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

**Table 3 - The Importance of Regional Differences  
Using Required Schooling Instruments (RS7, RS8, RS9)**

Sample:	All U.S. (1)	All U.S. (2)	Non-Southern Born Only (3)	Southern Born Only (4)
<i>A. Causal Estimates of the Returns to Schooling</i>				
2SLS	0.011	-0.020	-0.018	0.052
LIML	0.001	-0.029	-0.025	0.030
CLR Conf. Int.	{-.100,.081}	{-.163,.060}	{-.087,.007}	{<-10.0,>10.0}
Additional Regressors:	Region-Year of Birth School Quality	Region-Year of Birth Indicators	None	None
<i>B. First Stage Estimates</i>				
Instruments:				
RS7	0.017 (0.038)	0.040 (0.035)	0.060 (0.048)	0.078 (0.048)
RS8	0.065 (0.034)*	0.072 (0.032)**	0.268 (0.037)***	0.025 (0.050)
RS9	0.174 (0.044)***	0.177 (0.043)***	0.455 (0.051)***	0.082 (0.060)
F (First Stage Excluded Instr.)	9.7	8.2	32.6	1.6
N	609,852	609,852	422,385	187,467

Note: Each column of Panel A contains both the 2SLS and LIML results from estimating a variant of (1) using the Required Schooling indicators (RS7, RS8, RS9) as instruments. Each column of Panel B contains the corresponding results of estimating a variant of (2). All regressions also include state of birth and year of birth fixed effects. The standard errors reported in parentheses allow for the correlation of the error terms within each state of birth/year of birth cell. The confidence intervals based on Moreira's Conditional Likelihood Ratio Test shown in braces also allow for the correlation of the error terms within each state of birth/year of birth cell. \*, \*\*, and \*\*\* represent significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

**Table 4 - Decomposition Using Continuous Required Schooling (RS) Instrument**

Estimator	Fraction of Variation in			OLS	
	Within-State/ Within-Year <i>RS</i> (1)	First Stage (2)	Reduced Form (3)	2SLS (4)	Returns <sup>†</sup> (5)
A. Within-State/ Within-Year		0.126***	0.012***	0.092***	0.073***
B. Within-State/ Within-Region-Year	0.666	0.052***	-0.004	-0.082	0.073***
C. Between Region-Year	0.334	0.100***	0.036***	0.356***	0.065***

Note: This table presents first stage, reduced form, and 2SLS estimates of the returns to schooling using a single instrument, a continuous version of the Required Schooling (*RS*) Instrument. Column (1) contains the fraction of the variation in the within-state/within-year transformed *RS* that is attributed to each corresponding row. Column (2) contains the coefficients on *RS* in different specifications of the first stage, column (3) contains the coefficients on *RS* in different specifications of the reduced form, column (4) contains the coefficients on education in the 2SLS estimates the returns to schooling, and column (5) contains OLS estimates of the wage equation. Estimates in the rows labelled “Within-State/Within-Year” are from regressions that use individual-level data and include state of birth and year of birth fixed effects. Estimates in the row labelled “Within-State/Within-Region-Year” are from regressions that use individual-level data and include state of birth and region-year of birth fixed effects. Estimates in row labelled “Between Region-Year” are from regressions that use region-year averaged data. Although standard errors are not shown in the Table to conserve space, \*, \*\*, and \*\*\* represent significance at the 10 percent, 5 percent, and 1 percent levels, respectively. N=609,852.

† For the transformed years of education variable which is the regressor in the OLS Returns column, the share of the variation that occurs within-state/within-region-year is 0.999.

**Table 5 - Decomposition Using Continuous Required Schooling (RS) Instrument  
Expanded Sample Ages 25-54**

Estimator	Fraction of Variation in			OLS	
	Within-State/ Within-Year (1)	First Stage (2)	Reduced Form (3)	2SLS (4)	Returns† (5)
A. Within-State/ Within-Year		0.078***	0.0063***	0.081***	0.063***
B. Within-State/ Within-Region-Year	0.809	0.036***	-0.0025	-0.070**	0.063***
C. Between Region-Year	0.191	0.087***	0.015***	0.167***	0.032***

Note: This table presents first stage, reduced form, and 2SLS estimates of the returns to schooling using a single instrument, a continuous version of the Required Schooling (RS) Instrument. Column (1) contains the fraction of the variation in the within-state/within-year transformed RS that is attributed to each corresponding row. Column (2) contains the coefficients on RS in different specifications of the first stage, column (3) contains the coefficients on RS in different specifications of the reduced form, column (4) contains the coefficients on education in the 2SLS estimates the returns to schooling, and column (5) contains OLS estimates of the wage equation. Estimates in the rows labelled “Within-State/Within-Year” are from regressions that use individual-level data and include state of birth and year of birth fixed effects. Estimates in the row labelled “Within-State/Within-Region-Year” are from regressions that use individual-level data and include state of birth and region-year of birth fixed effects. Estimates in row labelled “Between Region-Year” are from regressions that use region-year averaged data. Although standard errors are not shown in the Table to conserve space, \*, \*\*, and \*\*\* represent significance at the 10 percent, 5 percent, and 1 percent levels, respectively. N=2,166,387.

† For the transformed years of education variable which is the regressor in the OLS Returns column, the share of the variation that occurs within-state/within-region-year is 0.998.

Figure 1 – Trends in Wages, Dropout Age, and School Quality

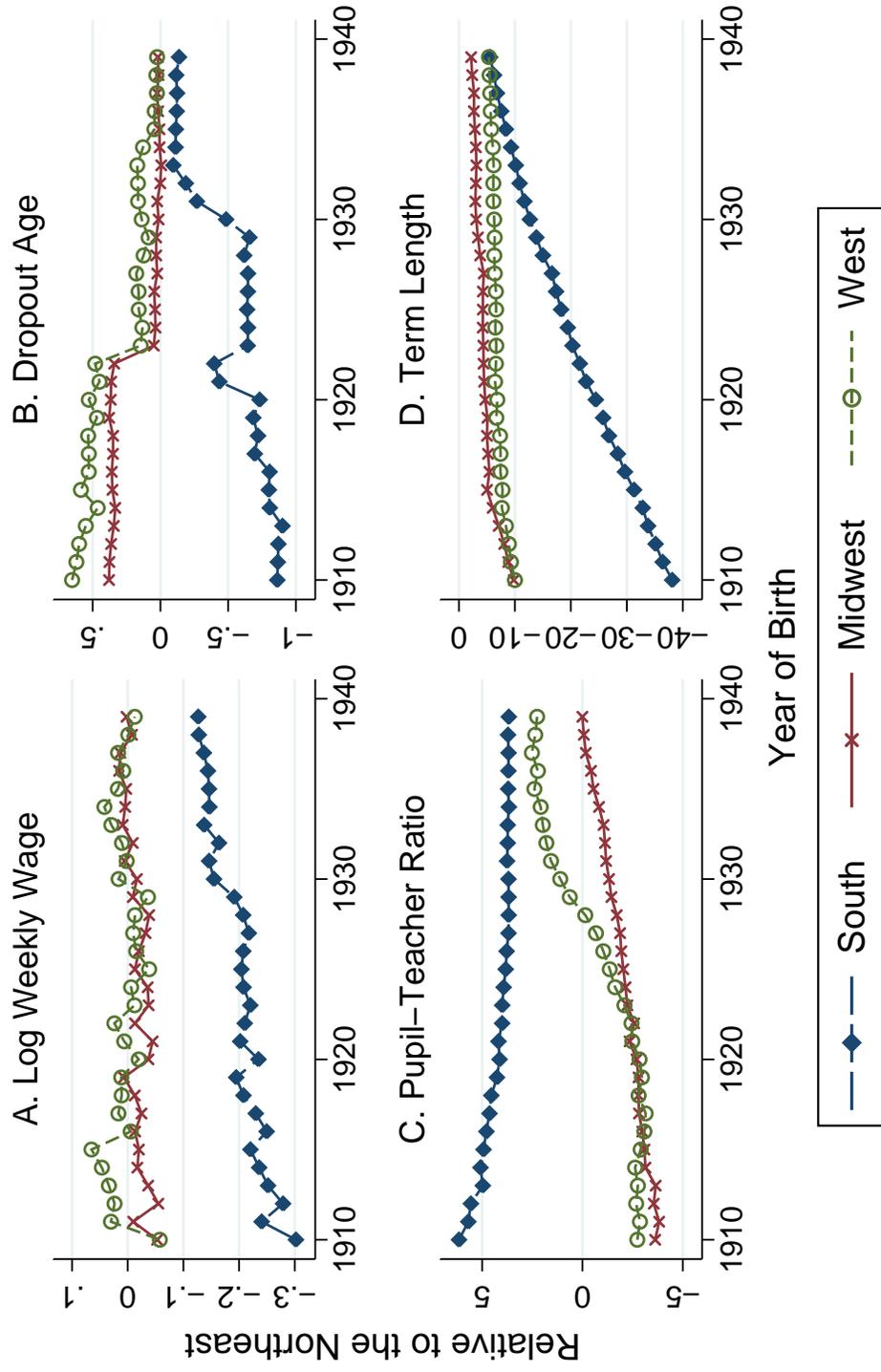


Figure 2 – Comparison of Schooling Law Codings

