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COMPULSORY EDUCATION AND THE BENEFITS OF SCHOOLING

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ABSTRACT

Causal estimates of the benefits of increased schooling using U.S. state schooling laws as instruments typically rely on specifications which assume common trends across states in the factors affecting different birth cohorts. Differential changes across states during this period, such as relative school quality improvements, suggest that this assumption may fail to hold. Across a number of outcomes including wages, unemployment, and divorce, we find that statistically significant causal estimates become insignificant and, in many instances, wrong-signed when allowing year of birth effects to vary across regions.

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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).

These state-level changes in schooling requirements are used as instrumental variables to examine the impact of increased schooling on a wide range of outcomes including wages, mortality, incarceration, and the social returns to schooling (Acemoglu and Angrist 2000; Lochner and Moretti 2004; Lleras-Muney 2005; Oreopoulos 2006). Identification of these effects is achieved by exploiting variation in the timing of the law changes across states over time such that different birth cohorts within each state have different compulsory schooling requirements. Key to this identification strategy, typically implemented in specifications that include state of birth and year of birth fixed effects, is that all other changes which occur across states during this period are uncorrelated with the law changes, educational improvements, and the outcomes under investigation.

Prior research suggests that such a "common trends" assumption is unlikely to hold in this context. For example, Card and Krueger (1992a,b) find that gains in school quality, which improved much more rapidly in the Southern states, had important effects on the adult wages of men who were in school during this period. Bleakley (2007) finds that eradication of hookworm, which affected 40 percent of Southern children before the intervention, improved schooling outcomes and subsequent adult wages. Aaronson and Mazumder (2011) find that the construction of Rosenwald schools had significant effects on the schooling attainment and cognitive test scores of rural Southern Blacks. Thus, standard estimates of the benefits of increased schooling may be driven by a variety of factors that had disproportionate effects across the regions of the U.S. rather than by variation within states over time as is typically thought to identify these models.

In this paper, we examine the importance of the common trends assumption for estimates of the benefits of schooling when using schooling laws as instruments. In samples commonly used in the prior literature (the 1960-1980 U.S. Censuses) and across a number of outcomes including wages, occupational status, unemployment, and divorce, we find statistically significant causal effects of increased schooling when using the baseline specification which includes state of birth and year of birth effects. However, these estimates become insignificant and, in many instances, "wrong-signed" when using specifications in which the year of birth effects vary across the four U.S. Census regions of birth. While our first stage estimates of the impact of schooling laws on educational attainment are somewhat weakened by allowing regional differences in the year of birth effects, our findings are not simply due to a weak first stage as we still find F-statistics on the excluded instruments that substantially exceed conventional weak instrument thresholds.

Our findings indicate that results from the commonly-used baseline specification are driven by differences between regions as opposed to variation within states over time. By using region-specific year of birth effects, we remain agnostic as to the exact reason why the results change so dramatically when we implement a relatively minor, but substantively important, modification to the baseline specification. We present additional results in which we adjust the baseline specification by adding Card and Krueger's (1992a) school quality measures which vary at the state of birth/year of birth level. Again we find that many of the previously significant estimates become insignificant and/or wrong-signed suggesting that differential relative improvements in school quality across states, which are not accounted for in the baseline specification, may be one possible explanation for our findings.

2 Data

As with the majority of the literature which examines uses schooling laws as instruments for estimating the benefits of schooling, we use data from the 1960-1980 U.S. Censuses of Population. We limit our analysis to native-born individuals ages 25 to 54 across these three Census years which encompasses the 1905 to 1954 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.[1] The evidence on the efficacy of compulsory schooling laws is far more substantial for these cohorts than for more recent birth cohorts.[2] Our analysis focuses on Whites since we find no evidence supporting the efficacy of compulsory schooling laws for Blacks in our sample.[3] 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 when growing up. Years of schooling is measured as the highest grade completed. The log weekly wage is calculated by dividing annual wages by weeks worked.[4]

2.1 Schooling Laws

Multiple aspects of state compulsory schooling laws and child labor laws are used to determine the minimum years of school that a child is required to attend (Acemoglu and Angrist 2000; Goldin and Katz 2003). Compulsory schooling laws specify an entry age by which the child is required to attend school and a drop out age at which the child can choose to unconditionally stop attending school. There are two primary types of exceptions frequently written into schooling laws that allow children to stop attending school before the drop out age. The first type of exception allows children to stop attending if they have completed a specified number of schooling years.[5] The second type of exception allows children to be excused from school attendance if they have secured employment and have also reached both minimum age and years of schooling requirements. States typically

[1] 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.

[2] 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.

[3] After we adjust the specification to account for differential trends across regions, we find that many of the first stage estimates of the relationship between schooling laws and educational attainment for Blacks are wrong-signed and, in some cases, statistically significant. This result holds for all of the codings of the schooling laws that we examine. In addition, the F-statistics on the excluded instruments fall well below the conventional weak instrument threshold. These results are available from the authors.

[4] We follow Acemoglu and Angrist (2000) in the construction of the variables using the procedures discussed in the Appendix of their paper.

[5] In some instances these exceptions have both minimum age and completed years of schooling requirements.

have either the first or second type of exception; only in a handful of cases do states provide both types of exceptions.[6]

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 laws explicitly specify the requirements needed to work during school hours. In other situations, however, only the age at which the child can work when school is not in session is specified (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.

The compulsory schooling measures that primarily have been used in the prior literature are the two measures coded by Acemoglu and Angrist (2000). Their first measure is based only on the schooling attendance portion of the legal statutes. Compulsory attendance (CA_{st}) 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 the variables used to construct the measure are those prevailing in the individual's birth state when they were age fourteen.[7] The first quantity in the max function, the difference between the enrollment and drop out ages, 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. 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 drop out 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.

Their second compulsory schooling measure, child labor (CL_{st}), is based primarily on the child

[6]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.

[7]They assign laws as of age fourteen since most states require students to attend through age fourteen for the birth cohorts that they examine. Goldin and Katz use a similar approach except that they assign enrollment age based on the laws in place at age 6.

labor law and is computed as

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

The first term 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 term is the number of years of schooling needed to receive a work permit. A 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 a number of instances these age and/or schooling requirements are the same.

We construct a required schooling (RS_{st}) measure which accounts for any changes to the compulsory attendance and child labor laws that may occur during the child's school years.[8] For birth cohort t from state s, our measure is generated by iterating through ages six to 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.[9] By using this iterative process, we determine the number of years that the child would have been required to complete by the current age. In turn, we use this cumulative amount of required schooling to determine if the child is eligible for any school attendance exceptions at the current age. For each age as we iterate through the child's schooling years, if the student either has not reached their drop out age or is not eligible for an exception, we increment the number of required schooling years by one. Once the child either reaches their drop out age or meets the minimum age and/or years of schooling requirements to satisfy a schooling exception, we do not increment required schooling further unless there is a subsequent change in the schooling statutes.[10]

[8] Our measure also reflects additional data collection on the timing of schooling requirement changes. Whereas Acemoglu and Angrist (2000) collected data on these laws at roughly five-year intervals from secondary sources, we determine the exact year in which every law changed using a number of additional secondary sources as well as the original legislation found in state session laws. We verify each of the changes coded by Acemoglu and Angrist and by Goldin and Katz and, when necessary, reconcile differences between the two sets of codings. 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.

[9] 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 drop out age in our sample is eighteen.

[10] 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

Figure 1 displays the (weighted) distributions of alternative codings of the schooling laws for native-born Whites ages 25-54 in the 1960-1980 Censuses. The figure displays multiple histograms for the alternative codings of the schooling laws on the same x-axis to emphasize the contrast between the different measures. The modal number of required years of schooling based on our RS measure is eight with slightly more than half of the sample observations required to attend at least this modal amount. Compared to our RS measure, Acemoglu and Angrist's child labor law measure, CL, yields a lower number of compulsory years of schooling. Since eligibility for a work permit does not necessarily exempt the child from school attendance, 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 Figure 1 is quite different than that of either the RS or CL measures. As we noted above, however, 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 25 percent of individuals during this period were required to attend twelve years of schooling.[11] We create a corrected compulsory attendance measure (CCA) which replaces the max function with a min function in Acemoglu and Angrist's CA equation.[12] As shown in the Figure, this corrected measure substantially lowers the required years of school attendance.

As our results below show, the benefit of using our new coding of the compulsory schooling laws is that the first stage relationship between schooling attainment and schooling requirements is much stronger, as measured by the first stage F-statistic, than when using measures based on Acemoglu and Angrist's coding of the laws. As a result, our second stage estimates are also much more precisely estimated. However, our finding that changing the specification to account for differential trends by region yields small and insignificant causal estimates of the benefits of schooling does not depend upon the choice of the instrument that we use.

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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).

[11] A growing number of states during this period implemented an exception allowing children to leave prior to the drop out age if they have completed four years of high school. Five states had such laws in 1928 while twenty-one states had them in 1978.

[12] We use a min function when an exception exists. In the absence of an exception CCA is simply the difference between the drop out and enrollment ages.

2.2 School Quality

Our analysis examines how causal estimates of the benefits of schooling are affected when including region-specific year of birth effects. This approach remains agnostic to the exact mechanism by which these regional differences influence our findings. We present additional results in which we investigate one potential factor, state-level school quality. We use Card and Krueger's (1992a) school quality measures which they compiled from issues of the *Biennial Survey of Education* containing the results of surveys of state education departments performed by the U.S. Office of Education from 1918 to 1966. 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 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 the school quality data.[13] Since our oldest cohort was born in 1905, we also make use of school quality data beginning in 1911 which is available in earlier years of the *Survey*.[14] Furthermore, since our youngest cohort was born in 1954, we extend their data forward in time using various editions of the *Digest of Educational Statistics*.

3 Empirical Methodology

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

$$Outcome_{st,i} = \alpha Educ_{st,i} + \boldsymbol{\chi}_s + \boldsymbol{\delta}_t + \boldsymbol{\beta} X_{st,i} + \epsilon_{st,i} \tag{1}$$

where $Educ_{st,i}$ 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. Depending upon the exact sample composition, we also include a quartic in age, Census year indicators, and an indicator for gender to this baseline specification as part of $X_{st,i}$ in equation (1). To account for the aforementioned differences in trends across states, we also present specifications in which $X_{st,i}$

[13] We thank David Card and Alan Krueger for making these data available to us.

[14] 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).

contains either year of birth indicators that differ across the four U.S. Census regions of birth or the Card and Krueger school quality measures.[15]

The corresponding first stage equation that we estimate is

$$Educ_{st,i} = \boldsymbol{\pi} CSL_{st} + \boldsymbol{\lambda}_s + \boldsymbol{\theta}_t + \boldsymbol{\upsilon} X_{st,i} + \nu_{st,i} \tag{2}$$

where $\boldsymbol{\lambda}_s$ is a vector of state of birth fixed effects and $\boldsymbol{\theta}_t$ is a vector of year of birth effects. CSL_{st} represents the schooling law instruments which, for our primary analysis, are based on our RS measure of the required years of schooling. 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. We specify the RS instrument as three indicators, $RS7$, $RS8$, and $RS9$, corresponding to being required to attend seven, eight, and nine or more years of schooling, respectively. We also present results based on Acemoglu and Angrist's child labor measure (CL) and the corrected version of their compulsory attendance measure (CCA).

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). For the majority of our analysis, the first stage statistics are well above the conventional weak instrument thresholds. As such, we only present the 2SLS estimates below.[16] To be conservative with our inferences, we implement Moreira's (2003) conditional likelihood ratio (CLR) test which is "nearly" optimal among methods to construct confidence intervals when using weak instruments under the assumption that the model has i.i.d. normally distributed errors (Andrews, Moreira, and Stock 2007).[17] 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, we report CLR confidence intervals that allow for clustering using the methods

[15] 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 α across the various specifications we examine as is shown for our log weekly wage specifications in Appendix Table 1.

[16] The point estimates from using limited information maximum likelihood (LIML) are very similar to the 2SLS estimates shown here. We present LIML estimates for our log weekly wage specifications in Appendix Table 1.

[17] Alternative approaches for constructing confidence intervals with weak instruments include the Anderson-Rubin statistic (Anderson and Rubin 1949) and a Lagrange multiplier statistic (Kleibergen 2002).

discussed in Finlay and Magnusson (2009).[18]

4 Results

4.1 The Impact of Schooling on Wages

We limit our analysis in Table 1 to examining the causal effect of schooling on log weekly wages in order to focus on how the first stage estimates are affected by changing the model specification and the sample composition. The results shown in columns (1) and (2) of the Table limit the sample to Acemoglu and Angrist's (2000) primary sample of White males ages 40-49 from the 1960-1980 Censuses of Population.[19] The Table presents standard errors below their corresponding point estimates throughout except for the 2SLS estimates below which we report the 95 percent confidence intervals based on Moreira's CLR test. Column (1) shows that both the OLS and 2SLS estimates of the returns to schooling are statistically significant and of the usual magnitudes. The first stage F-statistic exceeds 42, well above the conventional weak instrument threshold of 10, while the first stage point estimates are consistent with monotonicity of the instrument because increases in the required years of schooling correspond to higher levels of schooling attainment.

Allowing for differential trends through the inclusion of region by year of birth indicators dramatically alters the causal estimates of the returns to schooling. As shown in column (2) of Table 1, these estimates becomes negative in sign and statistically insignificant. The first stage estimates fall in magnitude although they remain consistent with monotonicity of the instrument and are jointly significant with an F-statistic of 8.2. Corresponding to the reduction in the F-statistic, the CLR confidence interval is much larger than in column (1) and, although the estimate is insignificant, we are unable to rule out a rate of return to schooling of six percent.

Given the reduced precision of the estimate due to the inclusion of region by year of birth

[18]The CLR confidence intervals are computing 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 similar confidence intervals when we calculate Kleibergen's (2002) Lagrange Multiplier test and allow for clustering and sampling weights (Finlay and Magnusson 2009). Results using this alternative method of computing the confidence intervals are shown in Appendix Table 1 for our log weekly wage specifications.

[19]We are able to exactly re-create their dataset and results based on the code they provide on-line. The original dataset is available on-line at http://econ-www.mit.edu/faculty/angrist/data1/data/aceang00. This sample corresponds to the 1910-1939 birth cohorts.

indicators, we next increase the sample to include all White males between ages 25-54 in columns (3) and (4). Doing so dramatically increases the first stage F-statistics to 81.4 and 23.6, respectively, while the first stage estimates on the instruments remain very comparable in magnitude to those found in the first two columns. Moreover, the implications from including the region by year of birth indicators remain unchanged as the causal effect of schooling on wages is negative and insignificant. The most important difference is that the confidence interval on the causal estimate is reduced by the increased sample size such that we can rule out returns to schooling that exceed two percent.

We further increase the sample size in columns (5) and (6) by including White females. Roughly 40 percent of White women at these ages in our sample are not working so the results should be viewed with some caution due to possible sample selection. Strikingly, both the first stage estimates and causal estimates of the returns to schooling are quite similar to the male only sample. The first stage F-statistics increase in magnitude while the confidence intervals on the returns to schooling become slighly smaller. We again can rule out all but very small, positive returns to schooling.

Since allowing region by year of birth effects dramatically changes our estimates, it suggests that results using the baseline model are driven by variation between regions rather than by changes within states over time. To further explore this point, in the final two columns of Table 1 we present results in which we separately estimate the baseline model for those born in the Southern U.S. and those born outside of the South. For the non-Southern born shown in column (7), we continue to find a very strong first stage relationship including a large F-statistic. However, we find a negative and nearly statistically significant estimate of the return to schooling. For the Southern born, the first stage estimates are consistent with monotonicity of the instrument although the F-statistic is only 6.3. The resulting causal estimate of the returns to schooling are rather small although the confidence interval is relatively wide given the lack of a strong first stage. When compared with the 2SLS estimate of 0.105 for the pooled sample found in column (5), the separate results by region confirm that between regional differences are an important source of variation used when implementing the baseline specification to identify these models.

We examine the robustness of our results across a number of dimensions in Appendix Table 1. Panel A of the Table shows two additional sets of results using the same specifications as in Table 1. First, we present estimates from using Limited Information Maximum Likelihood (LIML) and find quite similar estimates as when using 2SLS. Given that we find relatively large first stage F-statistics, the similarities between the 2SLS and LIML estimates is not surprising. Second, we construct 95 percent confidence intervals using Kleibergen's (2002) Lagrange Mulitiplier (LM) test and, also unsurprisingly, find intervals that are comparable to those based on Moreira's CLR test. Panel B of the Table presents results in which we include indicators for the respondent's state of residence. Across all specifications, including these indicators moves the point estimates slightly in the negative direction although our conclusions are qualitatively unchanged.

We also present results based on the Acemoglu and Angrist codings of the instruments in the Appendix for the same samples that are used in Table 1. As shown in Appendix Table 2, we yield a similar set of findings when using the child labor (CL) instrument in that including the region by year of birth indicators yields insignificant estimates of the returns to schooling although the first stage generates relatively large F-statistics when using the larger samples.[20] Given the aforementioned concerns with Acemoglu and Angrist's compulsory attendance (CA) instrument, we present results in Appendix Table 3 using the corrected compulsory attendance measure (CCA) shown in Panel D of Figure 1.[21] Although the causal estimates of the returns to schooling all fall within conventional ranges, the first stage estimates fail to demonstrate evidence of monotonicity.[22] While not shown here, estimates of the corresponding reduced form equation (i.e., regressing the log weekly wage on the instruments) typically produce one or more negative point estimates on the instruments. As such, we do not find the results from using the CCA instruments as credible estimates of the returns to schooling.[23] Overall, our findings in this section indicate that, while a strong first stage relationship between schooling laws and schooling attainment remains intact, there is no evidence of a causal effect of schooling on wages after including region by year of

[20]Following Acemoglu and Angrist, we use three indicators to represent the CL measure corresponding to seven, eight, and nine or more required years of schooling.

[21]We use three indicators to represent the CCA measure corresponding to eight, nine, and ten or more required years of schooling. The findings are robust to alternative specifications of the CCA measure.

[22]Across all specifications that use Whites ages 25-54, the impact on schooling attainment of being required to attend nine years of schooling is at least twice as large as being required to attend ten or more years of schooling. Furthermore, across all specifications which include region by year of birth indicators, the impact on schooling of being required to attend eight years of school is negative and (marginally) significant.

[23]As Goldin and Katz (2003) note, the compulsory schooling and child labor laws were intended to be complementary when written by state legislatures. The seemingly puzzling first stage estimates from using the CCA instrument most likely reflects the problem of measuring schooling requirements by only using the compulsory schooling laws given that our results using the RS instruments are consistent with monotonicity.

birth indicators. The fact that we find very small and/or wrong-signed estimates of the returns to schooling when separately estimating the baseline specification for Southern and non-Southern born individuals makes clear that the standard intuition that these models are primarily identified by variation within-states over time is incorrect.

4.2 The Impact of Schooling on Additional Outcomes

While the estimates of the impact of increased schooling on wages has been a topic of longstanding interest among economists, Oreopoulos and Salvanes (2011) note in their recent survey of the literature that there is a growing literature examining the effect of schooling across a variety of domains outside of the labor market including, but not limited to, health, crime, and family formation. To understand whether accounting for differential year of birth trends across regions more broadly affects causal estimates of schooling impacts, we examine a number of additional outcomes available in the Census that are presented in Table 2 of the Oreopoulos and Salvanes (2011) survey. Furthermore, we present results separately for males and females since we examine multiple labor market outcomes and roughly 40 percent of women in our sample are not in the labor force.[24]

Our findings from examining these additional outcomes are shown in Table 2 again using our required schooling (RS) instrument. The first three rows present the results for labor market outcomes: log weekly wages, log occupational score, and an indicator for being unemployed.[25] The OLS estimates in column (1) are all significant and in the expected direction with more schooling leading to increased wages, higher occupational prestige, and less unemployment. The 2SLS estimates in the column (2) using the baseline specification yields estimates in the same direction, all of which are statistically significant. After including region by year of birth indicators (column 3), the 2SLS estimates all switch sign and become statistically insignificant.

Table 2 presents results for some non-labor market outcomes also found in the Oreopoulos and Salvanes (2011) survey. Rows 4 and 7 examine the impact of education on divorce, separately for men and women, respectively. While our baseline 2SLS estimates indicate that increased schooling reduces divorce for both men and women, we find different results by gender when we include

[24] Estimates using pooled samples of men and women yield results qualitatively similar to those shown below.

[25] The occupational score available in the IPUMS Census samples ranks occupations based on the median income for each occupation in the 1950 Census.

region by year of birth effects. The estimated impact for men remains negative and is marginally significant. However, the impact on women, which is also marginally significant, is wrong-signed and of the same absolute magntitude as the male estimate. Rows 5 and 8 present results using an indicator for being in a mental institution as the dependent variable. While the OLS estimates are negative and significant, the 2SLS are insignificant across all specifications. Finally, in row 6 we examine the impact on the probability of being incarcerated for men and find positive and insignificant effects across all 2SLS specifications.[26] Across all of the outcomes in Table 2, allowing for differential trends across regions yields no evidence of a causal effect of increased schooling.

4.3 The Role of School Quality

The results presented thus far show that, across a number of outcomes, allowing for differential region by year of birth effects yields an insignificant, and in many cases wrong-signed, estimate of the causal effect of increased schooling. As we discussed above, one possible mechanism behind these findings is the closing of the gap in school quality across states, as documented by Card and Krueger (1992a) in Table 1 of their paper for cohorts born between 1920 and 1949. Measuring school quality as pupil/teacher ratios, length of the school year, and relative teacher wages, they find that the relative gains in quality were particularly sharp for the Southern states.

Column (4) of Table 2 presents results which add the three Card and Krueger school quality measures, which vary by state and year of birth, to the baseline model. For all of the outcomes in which we find a statistically significant effect in the baseline specification (column 2), we find an insigificant effect when we include the school quality measures and in half of these cases the resulting point estimates are wrong-signed. Thus, differences in the relative gains in school quality across states might be one contributing factor that explains why allowing for differential trends by region of birth yields insignificant estimates for the effect of schooling on the outcomes we examine here.[27] Of course, school quality may simply be correlated with other factors, such as differential rates of economic growth and development across regions, such that these findings are

[26]Less than 0.02 percent of White women in the sample are in jail so we do not examine this outcome for this group.

[27]We have also estimated specifications in which we create school quality measures which are averaged as the region-year of birth level rather than the state-year of birth level. We find that including these regional level measures yields results that are quite comparable to those shown in column (4) of Table 2.

not definitive evidence that school quality in and of itself is an underlying mechanism. Rather, we present the results using school quality measures as one possible explanation for understanding why the inclusion of year of birth effects that differ by region yields insignificant estimates of the causal effects of schooling.

5 Discussion

Our findings show no evidence of benefits to additional schooling using variation generated by compulsory schooling laws. As has been emphasized most recently by Carneiro, Heckman, and Vytlacil (2011), the 2SLS estimate is not necessarily a consistent estimate of the average population effect of additional schooling when the benefits of education are heterogenous. Rather, the 2SLS estimates of the benefits of schooling are the impact of an additional year of schooling for those students who were induced by the instrument to receive more education, i.e., a local average treatment effect (Imbens and Angrist 1994). Although high discount rates may lead 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. Therefore, it may not be surprising that we find no evidence of benefits from additional schooling, particularly for wages, among students who receive more schooling due to changes in compulsory schooling laws.

As a point of comparison, the recent empirical literature which estimates the returns to schooling using compulsory schooling law changes outside of the U.S. finds either small or zero returns. Since, unlike in the U.S., these reforms affected a substantial share of the population, we view many of these studies as providing compelling evidence of the impact of compulsory education. 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 heterogenous 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 (2013) finds no returns to schooling following a 1967 education reform in France. Although the identification strategies in these studies either exploit variation across states, similar to the U.S., or variation induced by a national reform, our estimates of the rate of return to schooling are comparable to the growing body of international evidence on the returns to education.

While the aforementioned papers examine changes in schooling requirements along the intensive margin, i.e., adjustments to an existing compulsory schooling law, recent work by Clay, Lingwall, and Stephens (2012) investigates the introduction of state compulsory schooling laws in the United States. Using both administrative and census data, they find that U.S. state schooling laws first introduced after 1880 significantly increased attendance, enrollment, and educational attainment. They also find marginally significant estimates of the return to schooling ranging from 11%-14% using data from the 1940 Census. Taken in conjunction with our findings, these results imply that the wage benefits from compulsory schooling laws occurred primarily in grammar school where students acquired the most fundamental skills.

As Oreopoulos and Salvanes (2011) highlight, the use of schooling law changes as instruments for educational attainment has been extended into a number of domains outside of the labor force, several of which that are beyond the scope of this paper. For example, recent papers on the health impacts of increased schooling have found significant benefits in the UK (Clark and Royer, Forthcoming) and the U.S. (Lleras-Muney 2005). In fact, Lleras-Muney does indeed account for region by year of birth effects in her analysis. Our findings suggest that future research using U.S. schooling law reforms should carefully account for the differential changes occuring across the U.S. during this period with school quality being but one of many possible confounding factors. At the very least, given the dramatically different results generated by estimating the models separately by Census region, these studies should investigate whether their findings are robust to comparable sample splits.

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Table 1 - The Effect of Schooling on Log Weekly Wages

Sample:	White Males Ages 40-49		White Males Ages 25-54		All Whites Ages 25-54		Whites 25-54 Born: Non-South	Whites 25-54 Born: South
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLS	0.075	0.073	0.063	0.063	0.068	0.068	0.067	0.069
	(.0005)	(.0005)	(.0004)	(.0004)	(.0003)	(.0003)	(.0004)	(.0004)
2SLS	0.095	-0.020	0.097	-0.014	0.105	-0.003	-0.009	0.019
	[.064,.126]	[-.163,.060]	[.080,.117]	[-.066,.021]	[.083,.123]	[-.058,.016]	[-.031,.001]	[-.097,.085]
First Stage:								
RS7	0.095	0.040	0.097	0.047	0.079	0.029	0.212	0.033
	(0.036)	(0.035)	(0.036)	(0.027)	(0.033)	(0.022)	(0.056)	(0.028)
RS8	0.224	0.072	0.268	0.135	0.246	0.136	0.418	0.066
	(0.032)	(0.032)	(0.028)	(0.024)	(0.026)	(0.019)	(0.037)	(0.026)
RS9	0.404	0.177	0.449	0.217	0.406	0.222	0.574	0.116
	(0.040)	(0.043)	(0.033)	(0.029)	(0.028)	(0.023)	(0.042)	(0.028)
F (First Stage Instruments)	42.8	8.2	81.4	23.6	91.7	40.6	67.3	6.3
Fixed Effects:								
State of Birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region*Year of Birth	No	Yes	No	Yes	No	Yes	No	No
Additional Controls	None	None	Age Quartic, Census Yr	Age Quartic, Census Yr	Age Quartic, Census Yr, Gender	Age Quartic, Census Yr, Gender	Age Quartic, Census Yr, Gender	Age Quartic, Census Yr, Gender
N	609,852		2,166,387		3,680,223		2,566,127	1,114,096

Note - The standard errors, which are 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, which are reported in brackets below the 2SLS estimates, allow for the correlation of the error terms within each state of birth/year of birth cell.

Table 2 - The Effect of Schooling on Additional Outcomes

	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
Outcomes for White Males Ages 25-54:				
1. Log Weekly Wage	0.063	0.097	-0.014	0.023
(Mean=5.21)	(0.0004)	[.080,.117]	[-.066,.021]	[-.017,.048]
2. Log Occupational Score	0.036	0.021	-0.0011	0.014
(Mean=3.34)	(0.0004)	[.014,.028]	[-.023,.017]	[-.006,.027]
3. Unemployed	-0.005	-0.006	0.0003	0.0098
(Mean=0.033)	(0.00007)	[-.0091,-.0018]	[-.0062,.0132]	[.0009,.0253]
4. Divorced	-0.0018	-0.0064	-0.0063	-0.013
(Mean=0.047)	(0.00006)	[-.0098,-.002]	[-.0175,.0036]	[-.0212,-.0021]
5. In Mental Institution	-0.0008	0.0003	0.0008	0.0004
(Mean=0.003)	(0.0006)	[-.0009,.0015]	[-.0022,.0045]	[-.0025,.0032]
6. In Jail	-0.0009	0.0006	0.0016	0.0005
(Mean=0.004)	(0.00002)	[-.0007,.0019]	[-.0016,.0050]	[-.0024,.0035]
Outcomes for White Females Ages 25-54:				
7. Divorced	-0.0008	-0.0041	0.0062	0.0014
(Mean=0.064)	(0.00009)	[-.0082,.0003]	[-.0009,.0164]	[-.0051,.0088]
8. In Mental Institution	-0.0006	0.00004	-0.0010	-0.0003
(Mean=0.002)	(0.00003)	[-.0009,.0012]	[-.0026,.0014]	[-.0019,.0015]
Fixed Effects:				
State of Birth	Yes	Yes	Yes	Yes
Year of Birth	Yes	Yes	Yes	Yes
Census Year	Yes	Yes	Yes	Yes
Region*Year of Birth	No	No	Yes	No
School Quality Controls	No	No	No	Yes

Note - The 2SLS results shown in this table are from specifications using the three Required Schooling instruments ($RS7$, $RS8$, $RS9$). The standard errors, which are 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, which are reported in brackets below the 2SLS estimates, allow for the correlation of the error terms within each state of birth/year of birth cell. The sample sizes for the results shown in this table are: row 1, 2,166,387; row 2, 2,161,610; rows 3-6, 2,502,089; rows 7-8, 2,563,971.

Appendix Table 1 - The Effect of Schooling on Log Weekly Wages

Sample:	White Males Ages 40-49		White Males Ages 25-54		All Whites Ages 25-54		Whites 25-54 Born: Non-South	South
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Without State of Residence Fixed Effects								
2SLS	0.095	-0.020	0.097	-0.014	0.105	-0.003	-0.009	0.019
LIML	0.096	-0.029	0.098	-0.016	0.107	-0.012	-0.015	0.007
CLR Conf. Int.	[.064,.126]	[-.163,.060]	[.080,.117]	[-.066,.021]	[.083,.123]	[-.058,.016]	[-.031,.001]	[-.097,.085]
LM Conf. Int.	{.064,.126}	{-.167,.061}	{.080,.117}	{-.066,.021}	{.082,.124}	{-.065,.022}	{-.032,.002}	{-.113,.095}
F (First Stage Instruments)	42.8	8.2	81.4	23.6	91.7	40.6	67.3	6.3
B. With State of Residence Fixed Effects								
2SLS	0.087	-0.041	0.090	-0.029	0.097	-0.013	-0.021	-0.001
LIML	0.087	-0.054	0.090	-0.031	0.099	-0.023	-0.029	-0.019
CLR Conf. Int.	[.056,.119]	[-.249,.046]	[.074,.110]	[-.086,.009]	[.075,.114]	[-.072,.006]	[-.045,-.011]	[-.160,.063]
LM Conf. Int.	{.056,.119}	{-.254,.048}	{.074,.110}	{-.086,.009}	{.075,.114}	{-.079,.011}	{-.047,-.010}	{-.219,.070}
F (First Stage Instruments)	38.5	6.2	77.9	20.8	90.6	36.6	63.6	5.4
Fixed Effects:								
State of Birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region*Year of Birth	No	Yes	No	Yes	No	Yes	No	No
Additional Controls	None	None	Age Quartic, Census Yr	Age Quartic, Census Yr	Age Quartic, Census Yr, Gender	Age Quartic, Census Yr, Gender	Age Quartic, Census Yr, Gender	Age Quartic, Census Yr, Gender
N	609,852		2,166,387		3,680,223		2,566,127	1,114,096

Note - Confidence intervals based on Moreira's Conditional Likelihood Ratio (CLR) Test, which are reported in brackets, allow for the correlation of the error terms within each state of birth/year of birth cell. Confidence intervals based on Kleibergen's Lagrange Multiplier (LM) test, which are reported in braces, allow for the correlation of the error terms within each state of birth/year of birth cell.

Appendix Table 2 - The Effect of Schooling on Wages
Child Labor Instrument

Sample:	White Males Ages 40-49		White Males Ages 25-54		All Whites Ages 25-54		Whites 25-54 Born: Non-South	South
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2SLS	0.080	-0.048	0.076	0.023	0.077	0.002	-0.005	-0.022
	[.033,.117]	[-.267,.039]	[.058,.106]	[-.036,.060]	[.067,.104]	[-.055,.021]	[.-055,.027]	[-.089,.021]
First Stage:								
CL7	0.105	0.045	0.137	0.104	0.128	0.075	0.217	0.090
	(0.032)	(0.028)	(0.025)	(0.019)	(0.020)	(0.015)	(0.034)	(0.019)
CL8	0.120	0.032	0.183	0.090	0.154	0.068	0.184	0.020
	(0.028)	(0.024)	(0.022)	(0.017)	(0.018)	(0.013)	(0.026)	(0.022)
CL9	0.269	0.109	0.337	0.164	0.323	0.158	0.360	0.132
	(0.038)	(0.033)	(0.028)	(0.020)	(0.024)	(0.017)	(0.033)	(0.023)
F (First Stage Instruments)	18.6	4.7	54.4	22.0	65.2	33.4	47.8	16.1
Fixed Effects:								
State of Birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region*Year of Birth	No	Yes	No	Yes	No	Yes	No	No
Additional Controls	None	None	Age Quartic, Census Yr	Age Quartic, Census Yr	Age Quartic, Census Yr, Gender	Age Quartic, Census Yr, Gender	Age Quartic, Census Yr, Gender	Age Quartic, Census Yr, Gender
N	609,852		2,166,387		3,680,223		2,566,127	1,114,096

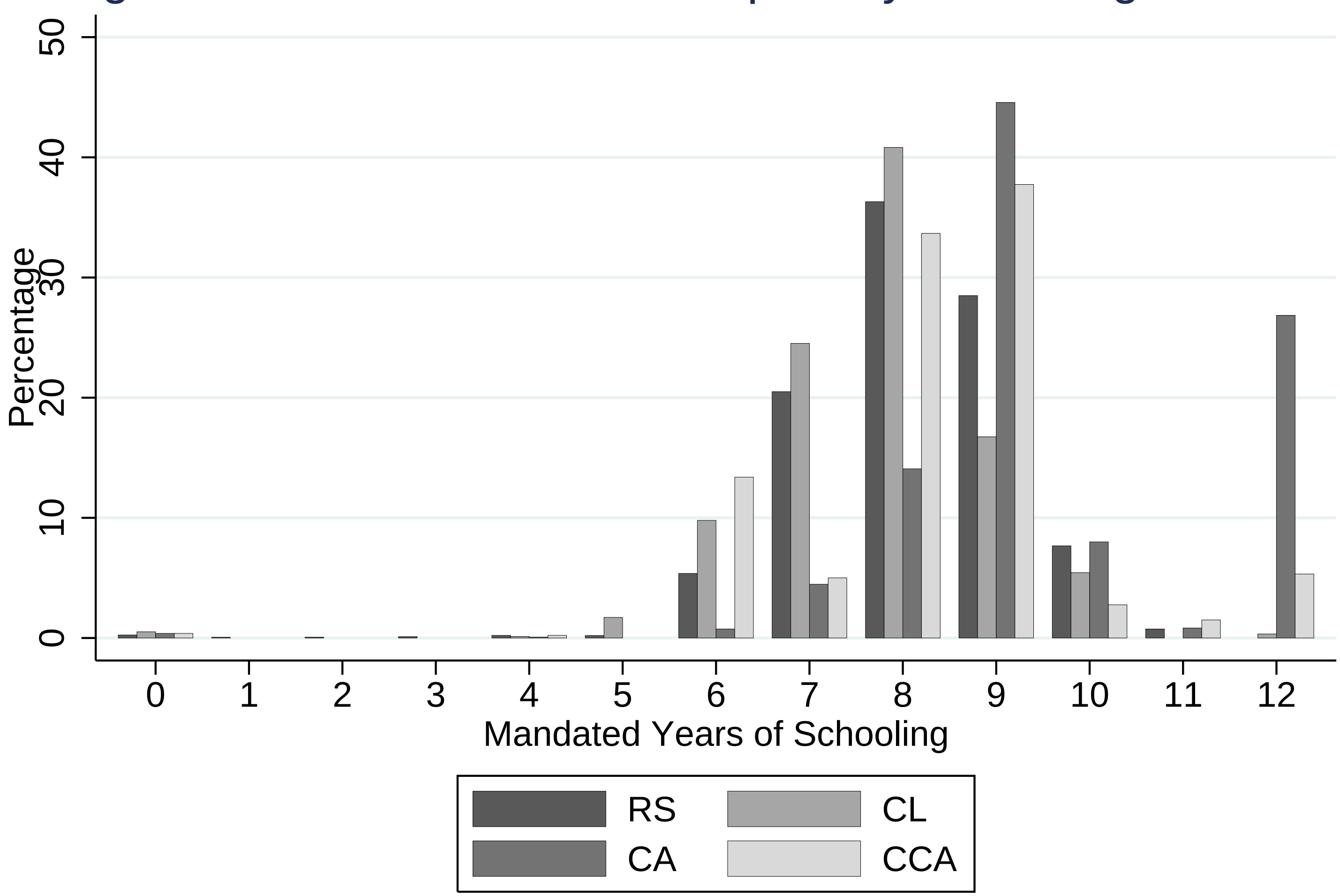
Note - The standard errors, which are reported in parentheses, allow for the correlation of the error terms within each state of birth/year of birth cell. The confidence intervals based on Moreira's Conditional Likelihood Ratio Test, which are reported in brackets below the 2SLS estimates, allow for the correlation of the error terms within each state of birth/year of birth cell.

Appendix Table 3 - The Effect of Schooling on Wages
Corrected Compulsory Attendance Instrument

Sample:	White Males Ages 40-49		White Males Ages 25-54		All Whites Ages 25-54		Whites 25-54 Born: Non-South	South
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2SLS	0.142	0.092	0.140	0.086	0.175	0.098	0.158	0.049
	[.097,.192]	[.034,.162]	[.120,.166]	[.054,.127]	[.153,.214]	[.066,.156]	[.107,.250]	[-.037,.113]
First Stage:								
CCA8	0.082	-0.084	0.099	-0.036	0.090	-0.027	0.017	0.071
	(0.029)	(0.030)	(0.023)	(0.020)	(0.020)	(0.016)	(0.026)	(0.025)
CCA9	0.215	0.066	0.259	0.101	0.221	0.097	0.122	0.125
	(0.029)	(0.025)	(0.021)	(0.016)	(0.017)	(0.013)	(0.024)	(0.020)
CCA10	0.193	0.046	0.124	0.035	0.092	0.031	0.011	0.122
	(0.059)	(0.042)	(0.039)	(0.026)	(0.034)	(0.021)	(0.041)	(0.037)
F (First Stage Instruments)	20.9	13.1	56.9	27.8	56.0	32.0	11.2	13.7
Fixed Effects:								
State of Birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region*Year of Birth	No	Yes	No	Yes	No	Yes	No	No
Additional Controls	None	None	Age Quartic, Census Yr	Age Quartic, Census Yr	Age Quartic, Census Yr, Gender	Age Quartic, Census Yr, Gender	Age Quartic, Census Yr, Gender	Age Quartic, Census Yr, Gender
N	609,852		2,166,387		3,680,223		2,566,127	1,114,096

Note - The standard errors, which are 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, which are reported in brackets below the 2SLS estimates, allow for the correlation of the error terms within each state of birth/year of birth cell.

Figure 1 – Distributions of Compulsory Schooling Measures



Note - This figure displays the distribution of the number of years of schooling mandated by each of the four schooling law measures described in the text. These distributions are constructed using native-born Whites ages 25-54 in the 1960-1980 Censuses and by applying sampling weights.