

# **Change to Compulsory School Attendance and the Returns to Schooling**

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## **INTRODUCTION**

Does education affect earnings? Enormous amounts of attention have been given to answering this seemingly simple question. However, the problem is convoluted because a determinant of the years of education an individual achieves comes from an unobservable variable, innate ability. To eliminate bias, Angrist and Krueger (1991) pioneered the use of compulsory schooling laws (CSLs) as instruments for educational achievement.

In the first half of the twentieth century, secondary schooling in the United States experienced incredible growth. A period known as “the high school movement” occurred from 1910 to 1940. The fraction of youths enrolled in public and private U.S. secondary schools increased from 18 to 71 percent and the fraction graduating nationwide soared from 9 to 51 percent (Goldin 1998). Additionally, Americans born in 1930 attended college less than 25 percent of the time, by 1970 this figure would rise to over 60 percent, meaning more and more Americans are attending college (Goldin and Katz 2008). While there are numerous factors contributing to rising educational attainment, prior research found changes to CSLs led to significant increases in educational attainment (Lang and Kropp 1986; Acemoglu and Angrist 2000; Llera-Muney 2002).

These state-level changes to CSLs can be conceptualized as a natural experiment, the instrumental variable (IV) treatment of education corrects for the assumed bias in the ordinary least square (OLS) estimator (Angrist and Pischke 2009, Ch. 4). 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 (Stephens and Yang 2014).

Prior research suggests that a “common trends” assumption is unlikely to hold in this context, as 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 creation within states over time as is typically thought to identify these models (Stephens and Yang 2014). To examine the importance of the common trends assumption Stephens and Yang (2014) use samples from the 1960-1980 US censuses to estimate the benefits of schooling across a wide range of outcomes including, wages, occupational status, unemployment, and divorce. Using a baseline specification which includes state of birth and year of birth effects, they find statistically significant causal effects of increased schooling. This indicates one more year of required schooling leads to better life outcomes: increased wages, higher occupational scores, lower unemployment, and lower divorce rates. They introduce an interaction term to the baseline model in which the year of birth effects vary across the four U.S. census regions of birth. The estimates, for these life outcomes, become insignificant and even “wrong-signed” in the second model.

Their findings indicate results from the commonly-used baseline specification are driven by differences between regions as opposed to variation within states over time, suggesting

differences between Northern, Southern, Eastern, and Western states may play a larger role than state-level variation. For example, Card and Krueger (1992) find gains in school quality had important effects on the wages of men who were born during this time. These gains in school quality occurred more rapidly in the Southern states. Bleakley (2007) finds that eradication of hookworm, which affected 40 percent of Southern children, improved schooling outcomes and subsequent adult wages.

The IV estimates presented by Stephens Jr. and Yang (2014) lack consistency and robustness relative to their OLS counterparts. The IV estimates fail to converge in large samples, are highly sensitive to the inclusion of additional control variables, such as the added interaction term in the second model, and standard errors remain large despite large sample size.

This paper examines the validity of the inclusion of the interaction terms by testing the overidentifying restrictions in the second model compared to those in the first model. Additionally, and more importantly, this paper examines the model design used by Stephens and Yang (2014). Their results are counterintuitive and not in line with previous literature showing positive returns to education using IVs (Oreopoulos 2006; Angrist and Krueger 1991; Brunello, et al 2009). The IV estimates lack consistency and robustness relative to their OLS counterparts. The IV estimates fail to converge in large samples, are highly sensitive to the inclusion of additional control variables, such as the added interaction term in the second model, and standard errors remain large despite large sample size.

The authors suggest their results are 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 (LATE). If the 2SLS estimates are to be interpreted as a LATE, then their results are likely biased. The bias is generated by the “always-takers”, i.e. individuals who would have achieved higher levels of education without the increased schooling requirement change. Since 87.79 percent of individuals in the sample remains in school beyond the required years, the majority of the sample belongs to a sub-population for which the average treatment effect of the CSL is expected to be zero.

## **DATA**

Aligning with the majority of previous literature examining uses of CSLs as instruments for estimating the benefits of schooling, this paper uses data from the 1960-1980 U.S. Censuses of Population (Stephens and Yang 2014). Census data comes from the IPUMS, which houses various sample sizes of the census data. Data from the 1960 census utilized in the paper is the 1% microdata sample. Similarly, the 1970 data uses two 1% microdata samples and the 1980 data is the 5% microdata sample. The datasets are appended together, and sample weights applied. For replication purposes, this analysis is limited 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 (Stephens and Yang 2014; Oreopoulos 2006; Lleras-Muney 2005). When using CSLs as instruments, studies use a cutoff of 1954 because of the major changes in compulsory schooling laws that occurred following the Brown vs Board of Education decision in 1954. Following Stephens and Yang

(2014), this analysis focuses on “Whites” since they find no evidence supporting the efficacy of compulsory schooling laws for “Blacks” in the sample. The demographic information required 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.

### *Schooling Laws*

CSLs specify an age an individual is required to attend school and/or a drop out age an individual can choose to unconditionally stop attending school. There are two primary types of exceptions frequently written into schooling laws allowing 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. 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 (Stephens and Yang 2014).

Following Stephens and Yang (2014), I construct four measures of compulsory school attendance. The first two measures were coded by Acemoglu and Angrist (2000): Compulsory Attendance ( $CA_{st}$ ) and Child Labor ( $CL_{st}$ ).

$$CA_{st} = \max\{Dropout\ Age_{st} - Enrollment\ Age_{st},\ Years\ of\ School\ Needed\ to\ Dropout_{st}\}$$

Compulsory attendance ( $CA_{st}$ ) is constructed by using a max function of two values. The first value is the difference between the dropout and enrollment age the individual faced in the state at the age of fourteen. The second value is years of completed schooling after which the

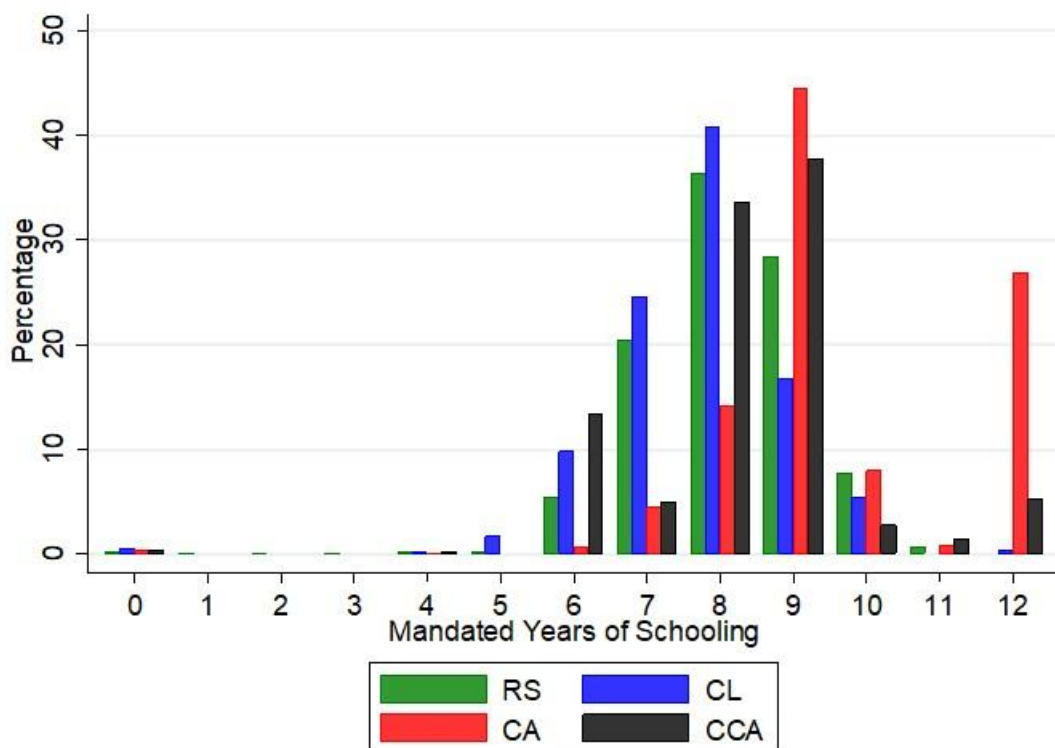
student can drop out without working. However, Goldin and Katz (2003) have noted the second term is an exception allowing the individual to leave before the dropout age, thus the max function should be a minimum function. Stephens and Yang correct this by constructing a Corrected Compulsory Attendance measure ( $CCA_{st}$ ) using a minimum function.

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

Child Labor ( $CL_{st}$ ) is constructed by using a max function of two values. The first value is the difference between the Work Permit and enrollment age the individual faced in the state at the age of fourteen. The second value is the number of schooling needed to receive a work permit.

The last measure constructed by Stephens and Yang is required schooling ( $RS_{st}$ ). This measure is more precise to timing of schooling requirement changes the individual faces. Unlike Acemoglu and Angrist (2000) measures, which collected data on these laws in five years intervals, Stephens and Yang determine the exact year in which every law changed. This was achieved using a number of additional secondary sources as well as the original legislation found in state session laws. For detailed instructions on how this measure is constructed see Stephens and Yang (2014).

Figure 1 displays multiple histograms of the various codings of the CSLs. The histograms depict the weighted distributions of the alternative CSLs for native-born “Whites” ages 25-54 in the 1960-1980 U.S. Census. The RS measure has a modal number of required years of schooling of eight. Acemoglu and Angrist’s child labor law measure, CL, compared to the RS measure,



yields a lower number of required schooling years. This result is not surprising, given eligibility for a work permit does not exempt the child from school attendance.

The second Acemoglu and Angrist measure, compulsory attendance (CA), is quite different than both the RS and CL measures. As previously mentioned, this measure was calculated using a max function and Goldin and Katz (2003) illustrated this measure should be constructed using a minimum function. Figure 1 illustrates the drastic difference between the CA and alternative measures. The CA measure implies that over 25 percent of individuals during this period were required to attend twelve years of schooling, while no other measures implies more than 5 percent of individuals required to attend twelve years of schooling. The corrected compulsory

attendance measure (CCA), created by Stephens and Yang (2014) to correct the CA measure, substantially lowers the required years of school attendance.

### *School Quality*

Additional results are presented using Card and Krueger's (1992a) school quality measures, which are used as an alternative to the region-specific year of birth interaction term presented by Stephens and Yang (2014). These school quality measures were 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 create a single measure for each state of birth/year of birth cohort by examining pupil/teacher ratio, length of school term, and average teacher salaries. The school quality measure data was provided by Card and Krueger.

## **METHODOLOGY**

Following previous literature, the outcome equation I estimate using two stage least squares (2SLS) is

$$Y_{st,i} = \alpha Educ_{st,i} + x_s + \delta_t + \beta X_{st,i} + \varepsilon_{st,i} \quad (1)$$

Where  $Educ_{st,i}$  is the years of schooling of individual  $i$  born in state  $s$  in year  $t$  while  $x_s$  and  $\delta_t$  are vectors of state of birth fixed effects and years of birth fixed effects, respectively.

Depending upon the exact sample composition, I 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, I also present



specifications in which  $X_{st,i}$  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 (Stephens and Yang 2014).

The first stage equation estimated is

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

where  $\lambda_s$  is a vector of state of birth fixed effects and  $\theta_t$  is a vector of year of birth effects.  $CSL_{st}$  is the schooling law instruments which, for the primary analysis of this paper and extension, are based on the RS measure. 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 because the equation includes both state of birth and year of birth fixed effects. The RS instrument is specified using three indicators, RS7, RS8, and RS9, corresponding to being required to attend seven, eight, and nine or more years of schooling, respectively. These mandated years of schooling were chosen because the greatest percentage of observation fall this range. Results are also presented based the child labor measure (CL) and the corrected compulsory attendance measure (CCA).

Since the confidence intervals are constructed through an iterative process, the 2SLS estimates are generated by applying the Frisch-Waugh-Lovell theorem (Giles 1984) to save on computational time. First, the outcome of interest, endogenous regressor, and instruments are each regressed on the remaining exogenous variables. Then, 2SLS is applied to the residuals from these regressions which yields parameter estimates and conventional variances of the estimates that are equivalent to the standard implementation of 2SLS. Additionally, to be

conservative, Moreira's (2003) conditional likelihood ratio (CLR) test which is "nearly" optimal among methods to when using weak instruments under the assumption that the model has i.i.d. normally distributed errors is implemented. Most studies using schooling law as instruments typically assume that the error terms are correlated among those born in the same state of birth/year of birth cohort, so CLR confidence intervals are reports to allow for clustering.

## **REPLICATION RESULTS**

### *Impact of Schooling on Wages*

Replication of Stephens and Yang's results was successful. Table 1 examines the causal effect of schooling on log weekly wages to focus on how the first stage estimates are affected when changing the model specification and increasing the sample size. Similar to Acemoglu and Angrist's (2000) primary sample, results in columns (1) and (2) are presented for the sample of "White" males ages 40-49 from the 1960-1980 Censuses of Population. First stage and OLS estimates are presented with clustered-robust standard errors to allow for clustering in each state of birth and year of birth cohort. 2SLS estimates are reported with 95 percent confidence intervals based on Moreira's CLR test. Column (1) presents results based on the baseline model specification, which show OLS and 2SLS estimates of the returns to schooling are statistically significant and in line with results found in previous literature. First stage F-statistic is well above the conventional weak instrument threshold of 10, with a value of 42.76.

Column (2) of Table 1 presents results for the model specification allowing for differential trends through the inclusion of region by year of birth indicators. The inclusion of these

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)
OLS	0.0731 (0.0005)	0.0730 (0.0005)	0.0632 (0.0004)	0.0630 (0.0004)	0.0682 (0.0003)	0.0682 (0.0003)	0.0671 (0.0004)	0.0691 (0.0004)
2SLS	0.0955 [0.0640, 0.1260]	-0.0200 [-0.1630, 0.0600]	0.0973 [0.0800, 0.1170]	-0.0140 [-0.0660, 0.0210]	0.1054 [0.0830, 0.1230]	-0.0025 [-0.0580, 0.0160]	-0.0086 [-0.0310, 0.0010]	0.0188 [-0.0970, 0.0850]
First stage:								
RS7	0.0948 (0.0359)	0.0399 (0.0352)	0.0972 (0.0362)	0.0466 (0.0269)	0.0793 (0.0332)	0.0293 (0.0221)	0.2119 (0.0560)	0.0329 (0.0277)
RS8	0.2241 (0.0324)	0.07199 (0.0352)	0.2681 (0.0283)	0.1352 (0.0238)	0.2458 (0.0256)	0.1358 (0.0192)	0.4181 (0.0371)	0.0664 (0.0261)
RS9	0.4041 (0.0399)	0.1767 (0.0432)	0.4495 (0.0331)	0.2173 (0.0289)	0.4060 (0.0285)	0.2220 (0.0234)	0.5744 (0.0417)	0.1156 (0.0284)
F (First stage instruments)	42.76	8.22	81.36	23.63	91.73	40.57	67.25	6.34
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 x 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
Observations	609,852		2,166,387		3,680,223		2,566,127	1,114,096

Notes: Standard errors, presented in parentheses, allow for correlation of the error terms within each state of birth/year of birth cell. The confidence intervals based on Moreira's CLR test, which are reported in brackets below the 2SLS estimates, allow for correlation of the error terms within each state of birth /year of birth cell.

indicators dramatically alter the 2SLS estimates of returns to schooling, yielding negative and statistically insignificant estimates. First stage estimates fall to 8.22, below the conventional weak instrument threshold. The CLR confidence intervals are much larger than in the baseline model specification in column (1) and, a positive rate of return to schooling of six percent cannot be ruled out.

Next, the sample size is increased to include all “White” males between ages 25-54 in columns (3) and (4). The increased sample size has a drastic effect on the first state F-statistics for both model specifications, increasing to 81.36 and 23.63 respectively. While both model specifications exceed the conventional weak instrument threshold for the F-statistic, the drastic difference between the two should be noted. Including the region by year of birth indicators remain unchanged as the causal effect of schooling on wages is negative and insignificant, however, the confidence interval on the causal estimate is reduced by the increased sample size such that returns to schooling that exceed two percent can be ruled out. Following the results shown in column (1), the 2SLS estimates of the baseline model are positive and statistically significant at the five percent level.

The sample size is further increased in columns (5) and (6) to include “White” females. These results should be viewed with caution because roughly 40 percent of the women in the sample are not working. Similar to the previous samples, F-statistics increase for both model specifications, but the baseline model far exceeds the model including the region by year of birth indicators, results for causal estimate in the baseline model are positive and statistically significant at the five percent level, and the casual estimates in the second model are negative and statistically insignificant.

It is clear including region by year of birth effects dramatically changes the estimates. This suggests results using the baseline model may be driven by regional variation rather than by changes within states over time. This point is further explored in columns (7) and (8), where the sample of all “Whites” ages 25-54 is separated by those born in Southern states and non-Southern states using the baseline model. For the non-Southern born shown in column (7), the

F-statistic exceeds the conventional weak instrument threshold, but causal estimates of returns to schooling are negative and statistically insignificant. For the Southern born in column (8), the F-statistic is below the conventional weak instrument threshold at 6.34 and the causal estimate of the returns to schooling is positive but statistically insignificant. When comparing these results to the pooled sample in column (5), the separated results indicate regional differences are a source of variation.

Results are also presented based on Acemoglu and Angrist coding for child labor (CL) and a corrected version of their compulsory attendance (CA) instrument, called corrected compulsory attendance (CCA) in Tables A1 and A2, respectively. Similar results are found when using the CL instrument compared to the RS instrument; when including region by year of birth indicators causal estimates of the returns to schooling are statistically insignificant, although first stage F-statistics grow increasingly larger as the sample size increases. Results for CCA, presented in Table A2, show causal estimates of the returns to schooling are positive and fall within conventional ranges, the first stage estimates fail to demonstrate evidence of monotonicity. Across all specifications using “Whites” ages 25-54, the impact on educational attainment of being required to attend nine years of schooling is larger than being required to attend ten or more years of schooling. Additionally, 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. Due to this, Stephens and Yang do not find the results using the CCA instrument as dependable estimates of the return to schooling. Overall, these findings indicate that, although there is a strong first stage relationship between schooling laws and

educational attainment, there is no evidence of a causal effect of schooling on wages after including region by year of birth indicators.

### *Impact of Schooling on Additional Outcomes*

A survey of the literature done by Oreopoulos and Salvanes (2011), which includes health, crime, and family formation, shows there is a growing literature examining the impact of

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 (mean = 5.21)	0.0630 (.0004)	0.0973 [0.0080, 0.1170]	-0.0140 [-0.0660, 0.0210]	0.0228 [-0.0170, 0.0480]
2. Log occupational score (mean = 3.34)	0.0364 (.0002)	0.0211 [0.0140, 0.0280]	-0.0011 [-0.0230, 0.0170]	0.0142 [-0.0060, 0.0270]
3. Unemployed (mean = 0.33)	-0.0050 (.00007)	-0.0064 [-0.0091, -0.0018]	0.0003 [-0.0062, 0.0132]	0.0098 [0.0009, 0.0253]
4. Divorced (mean = 0.047)	-0.0018 (.00006)	-0.0064 [-0.0098, -0.0020]	-0.0063 [-0.0175, 0.0036]	-0.0125 [-0.0212, 0.0021]
5. In mental institution (mean = 0.003)	-0.0008 (.00002)	0.0003 [-0.0009, 0.0015]	0.0008 [-0.0022, 0.0045]	0.0004 [-0.0025, 0.0032]
6. In jail (mean = 0.004)	-0.0009 (.00002)	0.0006 [-0.0007, 0.0019]	0.0016 [-0.0016, 0.0050]	0.0005 [-0.0024, 0.0035]
Outcomes for white females ages 25-54:				
7. Divorced (mean = 0.064)	-0.0008 (.00009)	-0.0041 [-0.0082, 0.0003]	0.0062 [-0.0009, 0.0164]	0.0014 [-0.0051, 0.0088]
8. In mental institution (mean = 0.002)	-0.0006 (.00003)	0.00004 [0.0009, 0.0012]	-0.0010 [-0.0026, 0.0014]	-0.0003 [-0.0019, 0.0015]
Fixed effects:				
State of birth	Yes	Yes	Yes	Yes
Year of birth	Yes	Yes	Yes	Yes
Census year	Yes	Yes	Yes	Yes
Region x year of birth	No	No	Yes	No
School quality controls	No	No	No	Yes

Notes: The 2SLS results shown in this table are from specifications using the three RS instruments (RS7, RS8, RS9). The standard errors, shown in parentheses, allow for correlation of the error terms within each state of birth/year of birth cell. The confidence intervals based on Moreira's CLR test, which are reported in brackets below the 2SLS estimates, allow for 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; Row 7-8, 2,563,971.

schooling across a variety of domains. To further examine whether accounting for differential year of birth trends across regions more broadly affects causal estimates of schooling impacts, Table 2 presents additional outcomes found in the survey as the dependent variable of interest.

Results are presented separately for males and females and use the RS instrument. OLS estimates presented in column (1) of Table 2, for the first three rows are in the expected direction and all statistically significant. Results in column (2) use the baseline specifications and again are in the expected direction and statistically significant. Column (3) includes region by year of birth indicators and yields negative causal estimates of the returns to schooling and are statistically insignificant. Across all outcomes presented in Table 2, including region by year of birth indicators yields no evidence of a causal effect of increased schooling, albeit with no statistical significance.

#### *Role of School Quality*

Column (4) in Table 2 presents results using Card and Krueger's (1992a) school quality measures in conjunction with the baseline specification. These measures include pupil/teacher ratios, length of the school year, and relative teacher wages. The baseline specification presented in column (2) found appropriately signed and statistically significant 2SLS estimates, however, when school quality measures are included the estimates are statistically insignificant and for half of the outcomes become wrong-signed compared to their OLS counter parts. This indicates school quality gains might be a contributing factor explaining why allowing for differential trends by region of birth yields insignificant estimates on the effect of schooling on these outcomes examined. However, school quality could be correlated with other factors.

Differences in economic growth rates or varying degrees of development across regions may be correlated with school quality, which indicates school quality may not be the underlying mechanism.

## **EXTENSION RESULTS**

### *Model Design Flaws*

Stephens and Yang's work using CSL as instruments for the returns to schooling contributed to the growing literature estimating the impact of an additional year of schooling. However, like the existing literature it is not without problems. Their results support the work of Acemoglu and Angrist (2001) of insignificant IV estimates leading to the conclusion that there is "no evidence of benefits to additional schooling" due to CSL. Insignificant IV estimates do encourage this interpretation, but this conclusion is counterintuitive.

The CSL indicators presented by Stephens and Yang (2014) lack consistency and robustness relative to their OLS counterparts, especially when introducing region by year of birth indicators into the model specification. IV estimates fail to converge in large sample sizes and standard errors remain large.

Table 3 presents results for the Sargan test of valid overidentifying restrictions and the Hausman test of OLS estimator consistency relative to IV estimates. The first four columns use the RS instrument and two main samples used throughout. Similar to Table 1, columns (1) and (3) use the baseline specification and columns (2) and (4) use the specification including region by year of birth indicators. Columns (2) and (5) are the only exceptions to finding invalid



Table 3 - Sargan and Hausman Test

Sample	RS CSL				CCA CSL			
	White males ages 40-49		White males ages 25-54		White males ages 40-49		White males ages 25-54	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLS	0.0731	0.0730	0.0632	0.0630	0.0731	0.0730	0.0632	0.0630
2SLS	0.0955	-0.0200	0.0973	-0.0140	0.1419	0.0920	0.1402	0.0858
Tests:								
Hausman	3.977	10.1401	44.1327	36.5163	17.6756	0.5494	147.689	3.3081
(p-value)	(0.0461)	(0.0015)	(0.0000)	(0.0000)	(0.0000)	(0.4586)	(0.0000)	(0.0689)
Sargan	0.9846	4.2997	18.2219	8.0163	0.657508	0.8975	12.733	17.86
(p-value)	(0.6112)	(0.1165)	(0.0001)	(0.0182)	(0.7198)	(0.6384)	(0.0017)	(0.0001)
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 x year of birth	No	Yes	No	Yes	No	Yes	No	Yes
Additional controls	None	None	Age, quartic, census yr	Age, quartic, census yr	None	None	Age, quartic, census yr	Age, quartic, census yr
Observations	609,852		2,166,387		609,852		2,166,387	

Notes: Standard errors, presented in parentheses, allow for correlation of the error terms within each state of birth/year of birth cell. The confidence intervals based on Moreira's CLR test, which are reported in brackets below the 2SLS estimates, allow for correlation of the error terms within each state of birth /year of birth cell. Wooldridge's extension of Sargan's test of overidentifying restrictions is used.

instruments. These columns have no rejection of Sargan's null of valid overidentifying

restrictions and rejection of Hausman's null of OLS estimator consistency relative to IV.

Table 3 again reveals what is acknowledged by Stephens and Yang in Table 1, that the inclusion of region by year of birth indicators creates large differences between the IV and OLS estimates and leads to insignificant IV estimates. This sensitivity raises questions about what the CSL represents. Regional variations may be entangled with the CSL. The inclusion of region by year of birth indicators to the model specification may introduce additional potential confounders, such as variation in required schooling for individuals who live in farming or

manufacturing economies and those who do not. This potential entanglement draws into the question the inclusion of region by year of birth indicators in the model.

Stephens and Yang acknowledge “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.” This is stated but the authors make no attempt to separate compliers from the always takers, despite a point made by Oreopoulos (2006) stating that empirical inconsistencies of the IV estimates may be generated by a negligible share of compliers in the sample. Since most people remain in school beyond the required years, the majority of the sample belongs to a sub-population for which the average treatment effect of the CSL is expected to be zero.

Table 4 breaks down the composition of CSL compliers, always takers, and defiers by each RS instrument used. Compliers are those individuals who leave school once they reach the required years of schooling. For the sample of “White” males ages 40-49, only 5.74 percent complies with the CSL. The share of compliers is even smaller for all “Whites” ages 25-54, with only 4.01 percent of the sample complying with the law.

In attempt to separate the compliers from the always takers, two sub samples are created. The “School” sub sample contains individuals who achieved 12 years or less of education and the “Postsecondary” sub sample contains individuals who achieved 13 years or more of education. Table 5 is a replication of Table 1 using the sub samples to test the claim of zero

Table 5 - The Effect of Schooling on Log Weekly Wages

Sample	White males ages 40-49		White males ages 25-54		All whites ages 25-54	
	School	Postsecondary	School	Postsecondary	School	Postsecondary
	(1)	(2)	(3)	(4)	(5)	(6)
OLS	0.0613 (0.0007)	0.0935 (0.0013)	0.0606 (0.0005)	0.0633 (0.0010)	0.0582 (0.0004)	0.0895 (0.0008)
2SLS	0.0970 [0.0570, 0.1340]	-0.0615 [-0.6527, 0.5297]	0.0682 [0.0495, 0.0869]	-1.3920 [-4.2222, 1.4381]	0.0923 [0.0717, 0.1129]	-0.6851 [-1.7470, 0.3768]
First stage:						
RS7	0.0483 (0.0388)	-0.0369 (0.0365)	0.0829 (0.0325)	-0.0112 (0.0202)	0.0854 (0.0290)	-0.0223 (0.0173)
RS8	0.2278 (0.0349)	-0.0424 (0.0320)	0.2970 (0.0288)	0.0004 (0.0178)	0.3056 (0.0263)	-0.0199 (0.0148)
RS9	0.3649 (0.0428)	-0.0553 (0.0360)	0.4678 (0.0340)	-0.0055 (0.0192)	0.4628 (0.0310)	0.0229 (0.0157)
F (First stage instruments)	39.46	0.83	98.39	0.37	120.48	0.74
Fixed effects:						
State of birth	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	None	None	Age, quartic, census yr	Age, quartic, census yr	Age, quartic, census yr, gender	Age, quartic, census yr, gender
Observations	399,428	210,424	1,283,824	882,563	2,259,859	1,420,364

Notes: Standard errors, presented in parentheses, allow for correlation of the error terms within each state of birth/year of birth cell. The confidence intervals based on Moreira's CLR test, which are reported in brackets below the 2SLS estimates, allow for correlation of the error terms within each state of birth /year of birth cell. The School sub-sample includes individuals achieving 12 years or less of education. The Postsecondary sub-sample includes individuals achieving 13 years or more of education.

average treatment effect for those who achieve beyond the required years of schooling. In each sample the “Postsecondary” sub sample yields negative and insignificant causal estimates of the returns to schooling. Additionally, the first stage F-statistics fall well below the conventional weak instrument threshold and first stage estimates fail to demonstrate evidence of monotonicity. The “Postsecondary” provides evidence to the claim of zero average treatment effect for school goers because a zero return to schooling cannot be ruled out.

The “School” sub sample yield F-statistics well above the conventional weak instrument threshold for the first stage estimates in each increasing sample size. The first stage estimates show evidence of monotonicity with increasing returns to school with each additional year. 2SLS estimates are statistically significant, however, these estimates do not converge as the sample increases. In contrast, the OLS estimates show clear signs of convergences as the sample increases.

## **CONCLUSION**

Stephens and Yang present evidence suggesting benefits to additional schooling using variation generated by compulsory schooling laws is zero or nonexistent. However, my findings suggest their model specifications may be flawed. Stephens and Yang acknowledge in Table 1, the inclusion of region by year of birth indicators creates large differences between the IV and OLS estimates and leads to insignificant IV estimates. This sensitivity raises questions about what the CSL represents. The inclusion of region by year of birth indicators to the model specification may introduce additional potential confounders, such as variation in required schooling for individuals who live in farming or manufacturing economies and those who do not. This potential entanglement draws into the question the inclusion of region by year of birth indicators in the model. Results in Table 2 with the Card and Krueger school quality measure also presents regional entanglement issues, as well as potentially being correlated with other factors and cannot be used as definitive evidence of zero returns to additional schooling using CSL instruments.

Stephens and Yang acknowledge “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.” Though this is acknowledged, no attempt is made to sort out defiers and always takers from the compliers, which invalidates the claim of the 2SLS estimates represent a local average treatment effect.

In an attempt to rescue the model, I create sub samples of the data for those more likely to have been induced by the CSL to achieve an additional year of schooling and those who would have gone on to achieve a higher level of education despite any CSL change. I find the CSL effect, for those achieving schooling years beyond the required years of schooling, is insignificant and I cannot rule out zero returns to schooling. For those likely to have been induced by the CSL treatment, I find the benefits to additional schooling for “White” males ages 25-54 is 6.8 percent.

For comparison sake, these results are in line with existing literature using CSL’s as instruments. 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. 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.

My findings suggest that variations in required years of schooling has a positive and statistically significant effect on log weekly wages for individuals induced by the treatment to

attend an additional year of schooling. However, the analysis done by Stephens and Yang indicates the baseline specification may not solely represent a local average treatment effect. The inclusion of regional variation interaction terms, along with the Card and Krueger school quality measures, indicate there may be other variables effecting the returns to education when using CSL's as instruments. It is possible individuals who live in more labor-intensive job markets, such as farming, mining, or manufacturing areas, may receive less benefit to an additional year of schooling. Instead the additional year of schooling may represent a lost year of job experience to those individuals. Future research using schooling law reforms should investigate a variety of potential confounding factors, including the prominent type of work available to workers in their region.

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## APPENDIX

Table A1 - The Effect of Schooling on Wages Using the Child Labor Instrument

Sample	White males ages 40-49		White males ages 25-54		All whites ages 25-54		Whites 25-54 born:	
	(1)	(2)	(3)	(4)	(5)	(6)	Non-south	South
2SLS	0.0800 [0.0330, 0.1170]	-0.0477 [-0.2670, 0.0390]	0.0762 [0.0580, 0.1060]	0.0229 [-0.0360, 0.0600]	0.0771 [0.0670, 0.1040]	0.0021 [-0.0550, 0.0210]	-0.0049 [-0.0550, 0.0270]	-0.0225 [-0.0890, 0.0210]
First stage:								
CL7	0.1054 (.0317)	0.0447 (.0282)	0.1373 (.0245)	0.1039 (.0189)	0.1285 (.0204)	0.0753 (.0150)	0.217 (.0341)	0.0895 (.0193)
CL8	0.1204 (.0278)	0.032 (.0235)	0.1833 (.0216)	0.899 (.0165)	0.1538 (.0183)	0.0681 (.0128)	0.1843 (.0265)	0.0204 (.0222)
CL9	0.2688 (.0377)	0.1090 (.0327)	0.3372 (.0285)	0.1635 (.0204)	0.3232 (.0244)	0.1580 (.0167)	0.3601 (.0333)	0.1323 (.0232)
F (First stage instruments)	18.61	4.65	54.42	22.00	65.23	33.37	47.82	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 x 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
Observations	609,852		2,166,387		3,680,223		2,566,127	1,114,096

Table A2 - The Effect of Schooling on Wages Using the Corrected Compulsory Attendance Instrument

Sample	White males ages 40-49		White males ages 25-54		All whites ages 25-54		Whites 25-54 born:	
	(1)	(2)	(3)	(4)	(5)	(6)	Non-south	South
2SLS	0.1419 [0.0970, 0.1920]	0.0920 [0.0340, 0.1620]	0.1402 [0.1200, 0.1660]	0.0858 [0.0540, 0.1270]	0.1751 [0.1530, 0.2140]	0.0976 [0.0660, 0.1560]	0.1584 [0.1070, 0.2500]	0.0488 [-0.0370, 0.1130]
First stage:								
CCA8	0.0820 (.0290)	-0.0841 (.0303)	0.0989 (.0227)	-0.0360 (.0198)	0.0900 (.0197)	-0.0272 (.0156)	0.0167 (.0256)	0.0706 (.0251)
CCA9	0.2146 (.0288)	0.0656 (.0253)	0.2593 (.0210)	0.1008 (.0161)	0.2206 (.0174)	0.0967 (.0128)	0.1221 (.0241)	0.1254 (.0195)
CCA10	0.1927 (.0588)	0.0464 (.0421)	0.1244 (.0387)	0.0349 (.0258)	0.0920 (.0339)	0.0308 (.0214)	0.0114 (.0408)	0.1217 (.00365)
F (First stage instruments)	20.85	13.13	56.91	27.79	56.03	31.97	11.23	13.73
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 x 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
Observations	609,852		2,166,387		3,680,223		2,566,127	1,114,096

Table 4 - Composition of CSL Compliers, Always Takers, and Defiers

	RS7=1	RS8=1	RS9=1	Total
White males ages 40-49				
Untreated				3.54%
Less	5.13%	6.30%	10.73%	7.55%
Equal	3.20%	8.87%	4.43%	5.74%
More	91.67%	84.83%	84.84%	83.17%
N	119,309	234,934	234,018	609,852
White males ages 25-54				
Untreated				3.47%
Less	3.85%	5.13%	7.47%	5.84%
Equal	2.45%	7.22%	3.71%	4.47%
More	93.70%	87.66%	88.82%	86.21%
N	376,569	687,950	1,026,619	2,166,387
All whites ages 25-54				
Untreated				3.39%
Less	3.06%	4.14%	6.23%	4.80%
Equal	2.09%	6.28%	3.51%	4.01%
More	94.85%	89.58%	90.27%	87.79%
N	642,206	1,158,154	1,754,947	3,680,223

Notes: "RS" is the CSL constructed by Stephens and Yang (2014) for required schooling years. "Untreated" is the share of individuals not treated by CSL; "Less" is the share of individuals who achieved less schooling than the CSL requirement; "Equal" is the share of individuals who achieved the same level of schooling as the CSL requirement; "More" is the share of individuals who achieved more schooling than the CSL requirement.