How Large Are Human Capital Externalities? Evidence from Compulsory Schooling Laws*

Daron Acemoglu
MIT

Joshua Angrist
MIT

March 12, 2000

Abstract

Many economists and policy makers believe that education creates positive externalities. Indeed, average schooling in U.S. states is highly correlated with state wage levels, even after controlling for the direct effect of schooling on individual wages. We use variation in child labor laws and compulsory attendance laws over time and across states to investigate whether this relationship is causal. Our results show private returns to education that are around 7 percent, and external returns to education that are in the neighbourhood of 1-2 percent and not significantly different from zero.

Keywords: human capital externalities, returns to schooling, wage equations

JEL Classification: I20, J31, J24, D62, O15.

*We thank Alexis, Leon, Chris Mazingo and Xuanhui Ng for excellent research assistance. Paul Beaudry, Bill Evans, Bob Hall, Larry Katz, Enrico Moretti, Jim Poterba, Robert Shimer, and seminar participants at the Canadian Institute for Advanced Research, the 1999 NBER Summer Institute, University College London, the University of Cornell, the University of Maryland, and the University of Toronto provided helpful comments. Special thanks to Stefanie Schmidt for helpful advice on compulsory schooling data.

1 Introduction

The effect of human capital on aggregate income and economic growth is of central importance to both policy makers and economists. Many economists believe that the large disparities in income across countries are due to differences in human capital (e.g., Mankiw, Romer and Weil, 1992). A tradition going back to Schultz (1967) and Nelson and Phelps (1966) views the human capital of the work force as a crucial factor facilitating the adoption of new and more productive technologies (see Foster and Rosenzweig, 1996, for evidence). Similarly, many recent endogenous growth models emphasize the link between human capital and growth. For example, in Lucas' (1988) model, each worker's productivity depends on the aggregate skill level in the economy, while Romer (1990) suggests that societies with more skilled workers generate more ideas and grow faster.

For human capital to play such an important role, there must be human capital externalities. Many studies find that *the private return to schooling,* i.e., the increase in individual earnings resulting from an additional year of schooling, is about 6-10 percent in both developed and less developed economies. If *the social return to schooling*, i.e., the increase in total earnings resulting from a one-year increase in average schooling, is of the same magnitude, then differences in schooling can explain only a small fraction of cross-country differences in income. To see this, note that the difference in average schooling between the top and bottom deciles of the world education distribution in 1985 is about 9 years (data from Barro and Lee, 1995). So if the social returns to schooling were around 10 percent, we would expect the top decile countries to produce less than twice as much output per worker as the bottom decile countries. In practice, this output per worker gap is over 15.[1] Therefore, for education differences to play a more major role, we would need a substantial wedge between the social returns and the private returns; in other words, there must be significant *human capital externalities.*

Although many economists believe in—and public education policies are often justified on the basis of—such externalities, there is little empirical work estimating human capital externalities. Moreover, it is not clear whether social returns should exceed private returns, even as a theoretical matter. Despite the emphasis on human capital externalities in recent growth models, many economists believe in the signaling role of education (e.g.,

[1]Data on output per worker from Summers and Heston (1991), with the Hall and Jones (1998) correction. This calculation ignores differences in the quality of education across countries, for which there is relatively little reliable data.

Spence, 1967, Lang and Kropp, 1986); if schooling has signaling value, social returns to education may be less than private returns. In the extreme case where schooling does not increase human capital but is simply a signal, the external returns to education would be the negative of the private returns, so that social returns would be zero. Social returns may also be less than private returns if some other factor of production is inelastically supplied.

Rauch (1993) is the first study that estimates human capital externalities, and finds them to be on the order of 3-5 percent. However, Rauch uses differences in average schooling across cities, and cities with greater average schooling may have higher wages for a variety of other reasons. This highlights the fact that a major challenge in estimating the effects of education on income is identification. To solve this problem, we use instrumental variables to estimate the effect of both individual schooling and the average schooling level in an individual's state. An ideal instrument for average schooling should affect the schooling of the majority of workers in a given area. We argue that differences in compulsory attendance laws and child labor laws in U.S. states between 1920 and 1960 play this role.

State compulsory attendance laws and child labor laws, or more briefly Compulsory Schooling Laws (CSLs), generate an attractive "natural experiment" for the estimation of human capital externalities (*external returns*) for a number of reasons. Although these laws were determined by social forces operating in the states at the time of passage, the CSLs that affected an individual in childhood are not affected by future wages. We also show that CSLs affected schooling almost exclusively in middle school and high school grades. This suggests that CSLs are not correlated with omitted state-of-birth and cohort effects, since these effects would likely be related to college-going behavior as well. Finally, changing CSLs were part of the 1910-1940 "high school movement" that Goldin (1998) has argued was responsible for much of the human capital accumulation in the U.S. in the twentieth century.

The bulk of the empirical work in the paper uses samples of white men aged 40-49 from the 1960-80 Censuses, although some estimates are computed using an extended sample that includes 1950 and 1990 data. We focus on the 1960-80 Censuses because the census schooling variable changes in 1990. Moreover, we show that it is important to control for the effect of individual schooling (private returns) correctly in order to estimate the external returns to education. Our strategy is to instrument for individual schooling using quarter of birth instruments as in Angrist and Krueger(1991). 1960-80 Censuses are useful in this regard as well because they contain quarter of birth information. We exclude blacks from the estimation because cohorts of blacks in these data sets experienced marked changes in school quality (see, e.g., Card and Krueger, 1992a; Margo, 1990; Welch, 1973). The fact that men in their 40s are on a relatively flat part of the age-earnings profile also enables us to control more easily for the effect of individual education on earnings. First, quarter of birth works best as an instrument for the 40-49 age groups. Second, estimates using a wider age range potentially require control for potential labor market experience, and not just age or cohort. This adds endogenous variables since potential experience is a function of schooling. An obvious drawback to focusing on this group is that externalities may be more important for younger workers. On the other hand, there is nothing in the empirical literature or our OLS estimates to suggest that this is actually the case.

OLS estimates using data from the 1960-80 Censuses show a large positive relationship between average schooling and individual wages. A one-year increase in average schooling is associated with about a 7 percent increase in average wages, over and above the roughly equal private returns. In contrast with the OLS estimates, IV estimates of external returns in 1960-80 are typically around 1 or 2 percent, and significantly lower than the corresponding OLS estimates. Adding data from the 1990 Census results in somewhat larger estimates of external returns, but this finding seems to be generated at least in part by problems with the schooling variable in the 1990 Census. We therefore conclude that overall there is no strong evidence for large external returns from education, though our results are consistent with modest external returns in the range of 1 to 3 percent.[2]

A shortcoming of our approach is that it identifies local human capital externalities only. We miss externalities that arise if, for example, more skilled workers generate ideas that are used in other parts of the country. Although we cannot rule out such global external returns, human capital externalities are also likely to have an important local component. For example, many studies have previously found important interactions between industries in local geographic areas (e.g., Glaeser, *et al* 1992, Jaffe, *et al* 1993). Another limitation of our study is that variation in CSLs mainly affects high school graduation. So our strategy is unlikely to uncover human capital externalities driven by changes in the number of college graduates. A recent paper by Moretti (1999) explores the effect

[2]Also our results strongly suggest that there are no negative external returns to education as would be implied by signaling models of schooling.

of the fraction of the workforce with college degrees on income in U.S. cities, and finds sizable human capital externalities. Although other reasons for the differences between our two studies cannot be ruled out, one possibility is Moretti's focus on college graduates. Despite the potential importance of externalities from college education, the bulk of the human capital accumulation in the U.S. during the twentieth century is accounted for by changes in high school graduation, and differences in schooling between high and low education countries are mostly at the secondary schooling level. Human capital externalities created by high school graduates are therefore of central importance.

The next section lays out two simple economic models that show how human capital externalities can arise. These models are used to develop an estimation framework and to highlight the econometric issues involved in identifying the external returns to education. Section 3 discusses the data and reports OLS estimates from regressions on individual and average schooling. Section 4 describes the CSL instruments, Section 5 reports the IV estimates, and Section 6 concludes.

2 Theories of human capital externalities

Many different interactions can lead to human capital externalities. Here, we discuss two possibilities, and derive a simple theoretical relationship to be estimated.

2.1 Theories of nonpecuniary externalities

In *The Economy of Cities,* Jane Jacobs argued that cities are an engine of economic growth because they facilitate the exchange of ideas, especially between entrepreneurs and managers (see also Bairoch, 1988, especially chapter 20). This notion also provides part of the motivation for Lucas' (1988) argument that human capital has important external returns. We refer to externality theories in this mold as "nonpecuniary" because the external effects do not work through prices, but rather through the exchange of ideas, imitation, or learning by doing.

To discuss these ideas more formally, suppose that the output (or marginal product) of a worker, i, is

$$y_i = A \cdot h_i^{\nu}$$

where h_i is the human capital (schooling) of the worker, and A is aggregate productivity.

So individual earnings are $W_i = Ah_i^{\alpha}$. For now, take the human capital of workers as given.

The notion that the exchange of ideas among workers raises productivity can be captured by allowing A to depend on aggregate human capital. In particular, suppose that

$$A = BH^{\delta} \equiv \left[\int h_i^{\rho} di \right]^{\delta/\rho} \tag{1}$$

where H is a measure of aggregate human capital, B is a constant, and ρ determines how the human capital of different workers are aggregated into this measure. In the Lucas' model, $\rho = 1$, so what matters is average human capital in a society or city. Another possibility, discussed by Murphy, Shleifer, and Vishny (1991), is that the skills of the most talented individuals create externalities, in which case we have $\rho \to \infty$. Finally, Benabou (1996) proposes an equation similar to (1) with $\rho < 0$, so that inequality in the distribution of human capital depresses aggregate productivity. Acemoglu (1997b) derives a similar relationship with $\rho < 0$ from imperfect job matching.

For any value of ρ, the parameter δ measures the importance and sign of external effects. Individual earnings can be written as $W_i = Ah_i^{\alpha} = BH^{\delta}h_i^{v}$. Therefore, taking logs, we have:

$$\ln W_i = \ln B + \delta \ln H + \nu \ln h_i \tag{2}$$

If the external effects are thought to operate within a geographical area, as seems reasonable in a world where interactions and the exchange of ideas is the force behind the externalities, then equation (2) could be estimated using some measure of H in local areas.

2.2 Theories of pecuniary externalities

Marshall (1961) argued that increasing the geographic concentration of specialized inputs increases productivity since the matching between factor inputs and industries is improved. A similar story is developed in Acemoglu (1997b), where firms find it profitable to invest in new technologies only when there is a sufficient supply of trained workers to replace employees who quit. We refer to this sort of effect as a pecuniary externality since greater human capital encourages more investment by firms and raises other workers' wages via this channel. Here, we outline a related theory of pecuniary human capital externalities based on Acemoglu (1996).

Consider an economy lasting two periods, with production only in the second period, and a continuum of workers normalized to 1. For now, take the human capital workers as given, and denote it by h_i for worker i as before. There is also a continuum of risk-neutral firms. In period 1, firms make an irreversible investment decision, k, at cost Rk. Workers and firms come together in the second period. The labor market is not competitive; instead, firms and workers are matched randomly, and each firm meets a worker. The only decision workers and firms make after matching is whether to produce together or not to produce at all (since there are no further periods). If firm f and worker i produce together, their output is

$$k_f^\alpha \cdot h_i^\nu \tag{3}$$

where $\alpha < 1$, $\nu \leq 1 - \alpha$, and the worker receives a share β of this output as a result of bargaining.

An equilibrium in this economy is a set of schooling choices for workers and a set of physical capital investments for firms. Firm f maximizes the following expected profit function:

$$(1-\beta) \cdot k_f^\alpha \cdot E[h_i^\nu] - R \cdot k_f, \tag{4}$$

with respect to k_f. Since firms do not know which worker they will be matched with, their expected profit is an average of profits from different skill levels. The function (4) is strictly concave, so all firms choose the same level of capital investment, $k_f = k$, given by

$$k = \left(\frac{(1-\beta) \cdot \alpha \cdot H}{R} \right)^{1/(1-\alpha)} \tag{5}$$

where

$$H \equiv E[h_i^\nu]$$

is the measure of aggregate human capital. Substituting (5) into (3), and recalling that wages are equal to a fraction β of output, the wage income of individual i is given by $W_i = \beta \left(\frac{(1-\beta)\alpha H}{R} \right)^{\alpha/(1-\alpha)} (h_i))^\nu$. Taking logs, this is:

$$\ln W_i = c + \frac{\alpha}{1-\alpha} \ln H + \nu \ln h_i, \tag{6}$$

where c is a constant and $\frac{\alpha}{1-\alpha}$ and ν are positive coefficients.[3]

Human capital externalities arise here because firms choose their physical capital in anticipation of the average human capital of the workers they will employ in the future. Since physical and human capital are complements in this setup, a more educated labor force leads to greater investment in physical capital and to higher wages. In the absence of the need for search and matching, firms would immediately hire workers with skills appropriate to their investments, and there would be no human capital externalities.[4]

Nonpecuniary and pecuniary theories of human capital externalities lead to similar empirical relationships since equation (6) is identical to equation (2), with $c = \ln B$ and $\delta = \frac{\alpha}{1-\alpha}$. The same sort of relationship appears if more educated workers produce higher quality intermediate goods, and monopolistically competitive upstream and downstream producers locate in the same area. Thus, an empirical strategy based on relationships of this sort cannot distinguish between the types of externalities we have discussed. Nevertheless, lack of evidence for a role for H in individual wage determination would weigh against all of these mechanisms (at the least at the local level).

2.3 Estimating the external returns to education

The models discussed above are closed by a mechanism explaining individual education decisions. Suppose that an individual's human capital is given by

$$h_i = \exp(\eta_i \cdot s_i),$$

where s_i is worker i's schooling. Workers have unobserved ability $\eta_i = \theta_i \eta(s_i)$, which depends on an individual characteristic, θ_i, and also potentially on schooling. This dependence captures potential decreasing returns to individual schooling, as in Lang (1993).

Suppose also that a worker's consumption, C_i, is equal to his labor income, and that schooling is chosen by workers so as to maximize

$$\ln C_i - \frac{1}{2}\psi_i \cdot s_i^2. \tag{7}$$

[3] As in Acemoglu (1996), human capital externalitites are additive in logs, so the marginal product of a more skilled worker increases when the average workforce skill level increases. Acemoglu (1998, 1999) discuss models in which log wage *differences* between skilled and unskilled workers increase with average skill levels.

[4] In a frictionless world, firms maximize profits conditional on realized worker-firm matches instead of conditional on the expected match. In this case, firm j matched to worker i chooses capital $k_j = \left(\frac{\alpha h_i^{\nu}}{r}\right)^{1/(1-\alpha)}$ and worker i's wage is $\ln W_i = c' + \frac{\nu\alpha}{1-\alpha} \ln h_i$.

The parameter ψ_i is the cost of education for individual i and can be interpreted as a personal discount rate, along the lines of Card (1995a).

Individual schooling decisions will then be determined by maximizing (7), taking (6) as given. In both models, this yields equilibrium schooling levels satisfying

$$\begin{aligned} \nu\theta_i\left[\eta(s_i)+s_i\eta'(s_i)\right] &= \psi_i s_i, \text{ or} \\ \eta'(s_i)\left(\varepsilon_\eta^{-1}+1\right) &= \frac{\psi_i}{\nu\theta_i}, \end{aligned} \tag{8}$$

where ε_η is the elasticity of the function η. The population average return to optimally chosen schooling levels is $E[\nu\theta_i(\eta_i(s_i)+s_i\eta_i'(s_i))]$. But the average return for particular subpopulations interacts with discount rates in a manner noted by Lang (1993) and Card (1995a). For example, if $\eta'(s_i)<0$, those with high ψ_i will get less schooling, and a marginal year of schooling will be worth more to such people than the population average return.

Equations (2) and (6) provide the theoretical basis for our empirical work. Since H is unobserved, however, we approximate $\ln H$ by state average schooling $\overline{S}$.[5] Estimation can therefore be based on the following equation for individual i residing in state j:

$$\ln W_{ijt} \approx \gamma_0+\gamma_1\overline{S}_{jt}+\gamma_2\eta_i s_i+u_{jt} \tag{9}$$

where $\overline{S}_{jt}=E_{jt}(s_i)$ is average schooling in state j at time t, and u_{jt} captures other factors that affect wages in that state at time t. An important implication of equation (9) is that if $\overline{S}_j$ is correlated with average ability among workers in area j, $\overline{\eta}_j$, then OLS will not estimate γ_1. One reason for such correlation is the endogenous nature of educational choices. Another is selective migration.

2.4 Migration

Suppose that individuals choose to live in one of two states, index by $j=1$ and 2, paying rent (user cost of housing) r_j in state j. Suppose also that i receives additional

[5] In the pecuniary externality model, and the nonpecuniary externalities model with $\rho=1$, this approximation is natural. Specifically, we have

$$\ln H=\ln E[\exp(\nu\eta_i s_i)]\approx c_0+c_1E(\eta_i s_i)\approx c_2+c_3E(s_i).$$

The first step approximates the mean of the log with the log of the mean. The second step takes $E(\eta_i)$ and the covariance between η_i and s_i to be constant, unaffected by changes in average education. When $\rho\neq 1$ in the nonpecuniary externalities model, the variance of education will also matter. With $\rho<1$, greater variance would reduce H, and with $\rho>1$, greater variance would increase H.

utility, ζ_i, from living in state 1 instead of state 2, where ζ_i is an independent draw from the continuous distribution function $G(\zeta)$. This taste shock introduces some degree of heterogeneity in worker preferences regarding residential location.

We normalize the total housing stock of each state to 1, so that total population is fixed at 1 in each state. Individuals have to live and work in the same state. Rents will adjust to clear the housing market. The consumption of individual i when he lives in state j is the difference between his labor income and the rent, that is, $C_{ij} = W_{ij} - r_j$, where W_{ij} is his earnings when he lives and works in state j.

To facilitate the discussion, also assume that a random factor, v_j, affects wages in each state, so the earnings of individual i in state j are given by

$$W_{ij} = BH_j^{\delta}(h_i))^{\nu} + v_j,$$

(in the model of pecuniary externalities, $\delta = \alpha/(1-\alpha)$ and $B = \beta\left((1-\beta)\alpha/R\right)^{\alpha/(1-\alpha)}$). An individual with human capital h will be indifferent between living in state 1 and state 2 if he has $\zeta_i = \zeta(h, \Delta v, \Delta r)$, where

$$BH_1^{\delta}(h)^{\nu} + \zeta(h, \Delta v, \Delta r) + \Delta v - \Delta r = BH_2^{\delta}(h)^{\nu}, \tag{10}$$

with $\Delta r = r_1 - r_2$ and $\Delta v = v_1 - v_2$. This implies that among the individuals with human capital h, those with ζ greater than $\zeta(h, \Delta v, \Delta r)$ would prefer to live in state 1 when the rent differential is Δr. Denoting the distribution of human capital by $F(.)$, and exploiting the fact that ζ_i's are independent across individuals, housing markets clear when

$$\int G(\zeta(h, \Delta v, \Delta r))\, dF(h) = \frac{1}{2}, \tag{11}$$

i.e., when half of the population prefers state 1. Intuitively, $G(\zeta(h, \Delta v, \Delta r))$ is the fraction with human capital h who prefer to live in state 2, and the integral sums over all levels of education. Equation (11) determines the equilibrium rent differential between the two states.

One implication of this simple framework is that an increase in H_1 will encourage more skilled workers to live in state 1.[6] This is because increasing H_1 raises the wages of more skilled workers by more then the wages of less skilled workers (recall that equations (2) and (6) are *additive in logs*). Positive state-specific shocks to wages (i.e., $\Delta v > 0$) will therefore attract more high education workers to a state and raise average human capital

[6]Nevertheless, not all high-education workers will migrate to the high-education state.

via migration. This is a source of positive correlation between average education and wages in a state that we will have to control for in order to identify the external returns to education.

It is also interesting to note that because rents tend to be higher in the state with greater average education, observed wage differences exaggerate differences in living standards. Nevertheless, for our purposes, differences in wages without cost-of-living adjustments are relevant. Firms pay (unadjusted) wages, and, in equilibrium, receive the same return to physical capital in both states.[7] Thus, human capital externalities are required so that firms in the state with greater average education and higher wages can produce more and break even.

3 Econometric framework

This section discusses instrumental-variables strategies to estimate equation (9), the causal relationship of interest.[8] In practice, of course, there are many factors beside schooling that determine wages. An error term is therefore added to the estimating equation. Also, we adopt notation that reflects the fact that different individuals are observed in different years in our data. The resulting equation is

$$Y_{ijt} = X_i'\mu + d_j + d_t + \gamma_1 \overline{S}_{jt} + \gamma_{2i} s_i + u_{jt} + \varepsilon_i, \tag{12}$$

where Y_{ijt} is the log weekly wage, u_{jt} is a state-year error component, and ε_i is an individual error term. The vector X_i includes state-of-birth and year-of-birth effects, and d_j and d_t are state-of-residence and Census year effects. The random coefficient on individual schooling is $\gamma_{2i} \equiv \gamma_2 \eta_i$, while the coefficient on average schooling, γ_1, is taken to be fixed.

The most important identification problem raised by equation (12) is omitted variables bias from correlation between average schooling and other state-year effects embodied in the error component u_{jt}. The theoretical discussion suggests two basic reasons for omitted variables bias. First, economic growth may increase wages in a state, while also increasing the demand for (or supply) of schooling. For example, state university systems typically expand during cyclical upturns, and also, as families become wealthier, they may invest

[7]Firms producing nontraded goods may only care about local prices. But firms producing traded goods will tend to receive the same rate of return to physical capital. They must therefore have a more productive work force in high wage states, raising state average productivity.

[8]Brock and Durlauf (1999) survey non-instrumental variables approaches to estimating models with social effects.

more in schooling. In our model, this corresponds to a correlation between u_{jt} and the average cost of or returns to schooling in a state and year ($E_j(\theta_i)$ and $E_j(\psi_i)$). To solve this problem, we construct instruments for $\overline{S}_{jt}$ using CSLs effective in individuals states of birth at the time they were 14. These instruments are called state of birth CSLs (SOB-CSLs). Since roughly two-thirds of the people in our sample live in their states of birth, the SOB-CSLs are correlated with average schooling in states of residence. SOB-CSLs generate variation in average schooling levels unlikely to be correlated with these omitted factors: they refer to laws passed thirty years before we observe the individual in our sample, so they are clearly not affected by state specific shocks (and permanent state differences are taken out by state fixed effects).

In addition to generating exogenous variation in average education, the SOB-CSL instruments provide an attractive starting point because they are attached to individuals. We can therefore compare IV estimates of the individual returns to schooling using SOB-CSLs to other IV estimates using individual characteristics (such as quarter of birth). Human capital externalities should cause IV estimates of individual returns using SOB-CSLs to diverge from these other estimates. A second reason we focus initially on the SOB-CSL instruments is that these instruments can be used while controlling for state of birth, but without controlling for state of residence, a potentially endogenous variable due to migration.

The second source of omitted variable bias is selective migration by more educated workers. The theoretical framework suggests that positive state-specific wage shocks attract more skilled workers (e.g., those with relatively high θ_i or low ψ_i), and hence create a spurious positive association between wages and average schooling. The SOB-CSL instruments do not necessarily correct for this source of bias. To see this, suppose that wages increase in a state, say New York. In response, workers from out of state will want to move to New York, pushing up housing costs. Our migration model suggests that higher education workers may have a greater tendency to move because they gain more in absolute terms from a proportional increase in wages. But, since these higher education workers may be from states with restrictive SOB-CSLs, SOB-CSLs could be correlated with state-specific shocks.

To solve this problem, we create an alternative set of instruments based on state of residence (SOR-CSLs). These instruments assign CSLs to each individual according to the laws in effect in their state of residence 30 years before the year they are observed (i.e., approximately the time they were 14). SOR-CSLs are uncorrelated with contemporary state-specific shocks since they are (by construction) invariant to the population mix in a particular state. In practice, SOB-CSLs and SOR-CSLs lead to similar estimates of human capital externalities, suggesting that differences in migration patterns by state of birth are not important.[9]

While omitted state-year effects are the primary motivation for these two IV strategies, the fact that one regressor, $\overline{S}_{jt}$, is the average of another regressor, s_i, also complicates the interpretation of OLS estimates. To see this, consider an "atheoretical" regression of Y_{ij} on both s_i and $\overline{S}_j$, which for purposes of illustration is assumed to have constant coefficients and a cross-section dimension only:

$$Y_{ij} = \mu^* + \pi_0 s_i + \pi_1 \overline{S}_j + \xi_i; \text{ where } E[\xi_i s_i] = E[\xi_i \overline{S}_j] \equiv 0. \tag{13}$$

Now, let ρ_0 denote the coefficient from a bivariate regression of Y_{ij} on s_i only and let ρ_1 denote the coefficient from a bivariate regression of Y_{ij} on $\overline{S}_j$ only. Note that ρ_1 is the two-stage least squares (2SLS) estimate of the coefficient on s_i in a bivariate regression of Y_{ij} on s_i using a full set of state dummies as instruments. Appendix A shows that

$$\begin{aligned} \pi_0 &= \rho_1 + \phi(\rho_0 - \rho_1) \\ \pi_1 &= \phi(\rho_1 - \rho_0) \end{aligned} \tag{14}$$

where $\phi = \frac{1}{1-R^2} > 1$, and R^2 is the R-squared from a regression of s_i on state dummies. Thus, if *for any reason* OLS estimates of the bivariate regression differ from 2SLS estimates using state-dummy instruments, the coefficient on average schooling in (13) will be nonzero. For example, if grouping corrects for attenuation bias due to measurement error in s_i, we would have $\rho_1 > \rho_0$ and the appearance of positive external returns even when $\gamma_1 = 0$ in (12). In contrast, if grouping eliminates correlation between s_i and unobserved earnings potential, we would have $\rho_1 < \rho_0$, and the appearance of negative social returns.[10]

[9] The endogenous variable is state average schooling for all the residents, while the estimation sample is limited to men aged 40-49. The CSLs these men were exposed to are nevertheless highly correlated with overall average schooling in a state, since this sample contributes to the overall average, and because the CSLs of neighboring cohorts are correlated with the CSLs of the estimation cohort.

[10] The coefficient on average schooling in an equation with individual schooling can be interpreted as the Hausman (1978) test statistic for the equality of OLS estimates and 2SLS estimates of private returns to schooling using state dummies as instruments. Borjas (1992) discusses a similar bias in the estimation of ethnic-background effects.

The interpretation of OLS estimates is complicated even further when returns to education vary across individuals, as in our random coefficients specification, (12). Nevertheless, an instrumental variables strategy that treats both s_i and $\overline{S}_j$ as endogenous can generate consistent estimates of external returns. The key to the success of this approach is finding the right instrument for individual schooling. We argue that quarter of birth instruments as in the work of Angrist and Krueger (1991) are the right instruments for individual schooling in our context. This is because CSL instruments and quarter of birth instruments both estimate individual returns for people whose schooling is affected by compulsory schooling laws—i.e., individuals who would have otherwise dropped out of school. (In fact, we show below that, like quarter of birth instruments, CSLs changed the distribution of schooling primarily in the 8-12 range). We develop this point more formally in Appendix A.

4 Data and OLS estimates

4.1 Data sources

Most of the analysis uses an extract of U.S.-born white males aged 40-49 from the 1960-80 Census microdata samples. These samples were chosen because they include data on quarter of birth, and are limited to groups on the flattest part of the age-earnings profiles.[11] This reduces bias from age or experience effects when using quarter-of-birth dummies as instruments. We also look at samples that include data from the 1950 and 1990 Censuses. Because these censuses do not include quarter of birth, estimates using the extended sample must treat individual schooling as exogenous. A second problem with the 1990 data is that the schooling variable is categorical.

The schooling variable for individuals in 1950-80 data is highest grade completed, capped at 17 years to impose a uniform topcode across censuses. Average schooling in a state and year is measured as the average of the capped highest grade completed for the full sample of workers aged 16-64 (i.e., not limited to white men). The averages are weighted by individuals' weeks worked the previous year. For 1990 data, we assigned

[11] Data are from the following IPUMS files (documented in Ruggles and Sobek, 1997): the 1 percent sample for 1960, Form 1 and Form 2 State samples for 1970 (giving a 2 percent sample), and the 5 percent PUMS-A sample for 1980. The 1950 sample includes all Sample Line individuals in the relevant age/sex/race group, and the 1990 data are from the IPUMS self-weighting 1 percent file. Stacked regressions are weighted so that each year gets equal weight. For additional information, see Appendix B.

average years of schooling to categorical values using the imputation for white men in Park (1994). Average schooling in 1990 is the average capped value of this imputed years of schooling variable.[12]

The relevant "labor market" for the estimation of equation (12) is taken to be a state. Previous work on external returns in the U.S. has used cities, while macroeconomic studies of education and growth have used countries (see, e.g., Mankiw, Romer, and Weil, 1992; Barro and Sala-i-Martin, 1995; Benhabib and Spiegel, 1994; Bils and Klenow, 1998; Topel, 1999; or Krueger and Lindahl, 1999). We use states because all three PUMS samples record state of residence while the 1960 and part of the 1970 PUMS fail to identify cities or metropolitan areas. Since the instruments used here are derived from individuals' states of birth and not their cities of birth, little is lost from this aggregation.

Table 1 gives descriptive statistics for the extract. The average age is constant across censuses, while average schooling increases by slightly less than a year between 1950-60, and by slightly more than a year between 1960-70, 1970-80 and 1980-90. The mean of state average schooling, shown in the row below individual schooling, refers to the entire working age population. The standard deviation of average schooling indicates the extent of variation in this average across states. The next two rows record the lowest and highest average schooling. For example, in 1980 the lowest average education was 11.8 years, in Kentucky, while Washington, DC had the highest average education at 13.1. The last eight rows of Table 1 report the fraction in each census affected by child labor and compulsory attendance laws (coded as SOB-CSLs). We discuss these variables in detail in Section 5, below.

4.2 OLS estimates

OLS estimates of private returns are similar to those reported elsewhere, and do not change much with controls for average schooling. For example, the estimates show a marked increase in schooling coefficients between 1980 and 1990. This can be seen in Table 2, which reports OLS estimates of models with and without $\overline{S}_{jt}$, using pooled samples, and separately by census year. The pooled regressions include state of residence effects, year effects, year of birth effects, and state of birth effects. Regressions using the individual censuses omit state of residence effects. All standard errors reported in

[12] Only 1 percent samples are used for the calculation of averages. Alternative weighting schemes for measures of average schooling (e.g., unweighted) generated similar results.

the paper are corrected for state-year clustering using the formula in Moulton (1986). Corrected standard errors are as much as two times larger than uncorrected standard errors because of the group structure of some of the instruments and regressors.

OLS estimates of external returns for 1960-80 imply that a one-year increase in state average schooling is associated with a .073 increase in the wages of all workers in that state (recall that average schooling or first to all the residents in the state, not only to those between the ages of 40-49). Using data from 1950-80 generates an estimate of .061, whereas the 1950-90 sample leads to an estimated external return of .072. These are similar to Moretti's (1999) estimates of external returns using within-city variation, which range from .08 to .13. The estimates in Table 2 using single censuses are from models without state effects. The resulting coefficients on average schooling are considerably larger, suggesting that at least some of the relationship between average schooling and wages is due to omitted state characteristics.[13]

5 Compulsory schooling laws and schooling

5.1 Construction of CSL variables

The CSL instruments were coded from information on five types of restrictions related to school attendance and work permits that were in force at the time census respondents were aged 14. These restrictions specify the maximum age for school enrollment ($enroll\_age$); the minimum dropout age ($drop\_age$); the minimum schooling required before dropping out (req_sch); the minimum age for a work permit ($work\_age$); and the minimum schooling required for a work permit ($work\_sch$). Information was collected for every 3-6 years from 1914-65, and missing years were interpolated by extending older data. For example, data for cohorts aged 14 in 1924-28 come from a source for 1924. Sources for the CSLs are documented in the data appendix.

The five CSLs vary considerably over time and across states. This can be seen in Table 3, which reports the mean and standard deviation for each CSL component in the years for which we have CSL data. Statistics in the table are averages using micro data; that is, they weight state requirements using the sample distribution of states for each cohort. The data show that compulsory attendance requirements have generally been

[13] Rauch (1993) reports cross-section estimates around .05 using data from the 1980 Census. These estimates are not directly comparable to ours because Rauch's model includes occupation dummies and average experience.

growing more restrictive, with the maximum enrollment age falling and the minimum dropout age rising. The minimum age for work has also increased. The cross-section variability in age requirements for dropout and work permits has fallen over time.

Margo and Finnegan (1996) show that in the 1900s, child labor laws were at least as important as attendance restrictions for educational attainment, and the evidence presented in Schmidt (1996) suggests the same for 1920-1935.[14] This is probably because, historically, the main reason for leaving school was to work. We therefore combine the five CSL components into two variables, one summarizing compulsory attendance laws and one summarizing child labor laws. Compulsory attendance laws are summarized as the minimum years in school required before leaving school, taking account of age requirements. This is the larger of schooling required before dropping out and the difference between the minimum dropout age and the maximum enrollment age:

$$CA = \max\{req\_sch;\ drop\_age - enroll\_age\}$$

Similarly, child labor laws are summarized as the minimum years in school required before work is permitted. This is the larger of schooling required before receiving a work permit and the difference between the minimum work age and the maximum enrollment age:

$$CL = \max\{work\_sch;\ work\_age - enroll\_age\}$$

These variables combine the CSLs into two measures that are highly related to educational attainment both conceptually and empirically.

Over 95 percent of men aged 40-49 in both the 1960-80 and 1950-90 censuses have CL in the 6-9 range, while CA is concentrated in the 8-12 range, with almost no one in the "11" category. The distribution of CL and CA can therefore be captured using four dummies for each variable. For CL, the dummies are:

$$\begin{aligned} CL6 \text{ for } CL &\leq 6, \\ CL7 \text{ for } CL &= 7, \\ CL8 \text{ for } CL &= 8, \\ CL9 \text{ for } CL &\geq 9. \end{aligned}$$

[14] Edwards (1978), Ehrenberg and Marcus (1982), Lang and Kropp (1986), and Angrist and Krueger (1991) also present evidence that compulsory schooling laws affect schooling.

Similarly, for CA, the dummies are:

$$\begin{aligned} CA8 \text{ for } CA &\leq 8, \\ CA9 \text{ for } CA &= 9, \\ CA10 \text{ for } CA &= 10, \\ CA11 \text{ for } CA &\geq 11. \end{aligned}$$

Table 1 shows the fraction of individuals in our sample in each group when CL and CA are assigned according to the laws that were in effect in individuals' state of birth at the time they were 14 (i.e., SOB-CSLs). The distribution of SOR-CSLs is similar. In the empirical work, the omitted categories are the least restrictive groups for CL and CA, $CL6$ and $CA8$.

5.2 CSL effects on individual schooling

There is a large and statistically significant relationship between individual schooling and the CSL dummies, using both SOB-CSLs and SOR-CSLs. Results with SOB-CSLs are shown in Table 4. Results that use SOR-CSL are similar, and we omit these to save space.

Table 4a reports results from regressions of individual schooling on $CL7-CL9$ and $CA9-CA11$, along with year effects, year of birth effects, and state of birth effects. For example, the entry in column 1 shows that in the 1960-80 sample, men born in states with a child labor law that required 9 years in school before allowing work ended up with .26 more years of school completed than those born in states that required 6 or fewer years. The results are similar in models that do not include state-of-residence effects, which suggests that selective migration for workers affected by different types of compulsory schooling laws may not be a major issue in the data.

The right half of Table 4a shows that adding 1950 Census data to the sample leads to CSL effects similar or slightly smaller than those estimated in the 1960-80 data alone. Incorporating both 1950 and 1990 data leads to larger effects. Also, the relationship between CSLs and schooling is larger and more precisely estimated in samples that pool three or more censuses than in a sample using 1980 data only. For example, column 4 shows that with 1980 data alone, the effect of $CL9$, though still statistically significant, falls to .17.

Overall, the estimates reflect a pattern consistent with the notion that more restrictive laws caused higher educational attainment. This pattern can be seen in Figures 1 and 2, which plot differences in the probability that educational attainment is at or exceeds the grade level on the X-axis (i.e., one minus the CDF). The differences are between men exposed to different CSLs in the 1960-80 sample, with men exposed to the least restrictive CSLs as the reference group.

Figure 1 shows that men who were exposed to more restrictive child labor laws were 1-6 percentage points more likely to complete grades 8-12. These differences decline at lower grades, and drop off sharply after grade 12. Figure 2 shows a similar pattern for compulsory attendance laws. These figures are encouraging for us; they suggest that CSLs primarily shift the distribution of schooling in middle- and high-school grades. This is consistent with the notion that CSLs cause schooling changes, and not vice versa. In fact, if the laws were picking up omitted factors related to macroeconomic conditions, tastes for schooling, or family background, we would have expected to find more restrictive CSLs associated with a greater proportion of the population attending college as well as a greater proportion completing high school.[15]

Table 4b quantifies the CDF differences plotted in the figures for 1960-80 and shows analogous results for the 1950-80 sample. The table reports CSL coefficients in regressions of dummy variables for whether an individual has completed the level of schooling indicated in the column heading. All of the positive differences for grades 8-12 are statistically significant. The negative differences at schooling levels above 12 are smaller and less likely to be significant. The estimates in the table also suggest that child labor laws shifted the distribution of schooling at younger grades more than compulsory attendance laws. This too is consistent with a causal interpretation of the relationship between CSLs and schooling since child labor laws refer to lower schooling levels than compulsory attendance laws. Interestingly, we replicate Margo and Finnegan's (1996) finding for the 1900s that child labor laws have been more important for educational attainment than compulsory attendance laws.

For the most part, the CDF differences in the figures and in Table 4b are ordered by increasing severity, as would be expected if these differences reflect increasingly restrictive

[15] Up to 12th grade, the CSLs increase schooling above required levels. For example, $CL9$ makes high-school graduation more likely. This may reflect "lumpiness" of schooling decisions, peer effects, or the fact that our coding is imperfect. Lang and Kropp (1986) note that educational sorting might also lead people not affected directly by CSLs to change their schooling when CSLs change.

laws. For example, using 1960-80 data, the difference at grade 9 for men with $CL9 = 1$ exceeds the difference for men with $CL8 = 1$. This in turn exceeds the difference for men with $CL7 = 1$. Adding 1950 data leaves this pattern unchanged.

A final noteworthy feature of the figures is their similarity to differences in the CDF of schooling induced by quarter of birth (as reported in Angrist and Imbens, 1995). Like CSLs, quarter of birth changes the distribution of schooling primarily in the 8-12 grade range. This supports our claim that CSL instruments and quarter of birth instruments are likely to generate similar estimates of the private return to schooling, since, as explained in the Appendix, IV estimates implicitly weight individual causal effects using CDF differences.

5.3 Private returns to education

The SOB-CSL instruments have individual level variation, capturing differences in laws that individuals were exposed to in their youth. The results in Tables 4 and 5 show that they are an important determinant of individual schooling. So, in principle, they can be used as instruments for individual schooling in wage equations. On the other hand, if there are external returns to schooling, IV estimates of private returns using CSL instruments will be biased because the instruments will pick up the effect of state average schooling on earnings.[16] In fact, one simple test for external returns is to compare estimates using quarter of birth instruments, which are uncorrelated with average education, to estimates using CSL instruments.

Table 5 reports 2SLS estimates of the private returns to schooling using three different sets of instruments. Using 30 quarter of birth/year of birth dummies, the private return to schooling is estimated at .073 (with a standard error of .012). This is less than the Angrist and Krueger (1991) estimate from a similar specification using 1980 data only. Columns 2 and 3 show that the discrepancy is explained by the fact that 1960 and 1970 data generate smaller quarter-of-birth estimates than the 1980 sample.[17]

Estimates of private returns using CSL instruments in the 1960-80 sample exceed those

[16] Similarly, positive social returns may also bias IV estimates of private returns using aggregate distance instruments, as in Card (1995b).

[17] Bound, et al (1995) note that with many instruments, 2SLS estimates may be biased towards OLS estimates, and argue that this is a problem for some of the specifications reported by Angrist and Krueger (1991). However, re-analyses of these data by, among others, Chamberlain and Imbens (1996), Stock and Staiger (1997), and Angrist and Krueger (1995), suggest that using 3 quarter of birth dummies interacted with 10 year of birth dummies as instruments produces approximately unbiased estimates.

using quarter of birth instruments, though the differences are not large or statistically significant. The 2SLS estimate of private returns using $CL6 - CL8$ as instruments, reported in column 4, is .076 (s.e.=.034). Using $CA8-CA10$ as instruments generates an estimate of .092 (s.e.=.044), shown in column 7. Models estimated using CSL instruments without state of residence effects produce similar results.[18] This suggests once again that the endogeneity of state of residence (i.e., migration) may not be important in determining returns to schooling.

Although we present a more detailed analysis below, the fact that quarter of birth and CSL instruments generate similar schooling coefficients in the 1960-80 data already suggests that external returns are modest in this period. As noted above, significant external returns would likely lead to estimates of private returns that are biased upwards when using CSL instruments, since CSLs are correlated with average schooling. Quarter of birth instruments, on the other hand, are not subject to this bias.

Estimates that include data from 1950 and 1990 use only CSL instruments, and not quarter of birth. Adding 1950 data to the basic sample leads to somewhat larger estimates with CL instruments. Adding 1990 data as well leads to even larger estimates using CL instruments, and to a substantial increase in precision with both sets of instruments. On the other hand, the estimates using CA instruments are remarkably insensitive to the inclusion of 1950 and 1990 data.

Finally, it is noteworthy that the IV estimates using quarter of birth are very close to the OLS estimates for the same period; compare, for example, the estimates of .073 in column 1 of Table 5 and column 1 of Table 2. Thus, estimates of external returns that treat individual schooling as exogenous and endogenous should give similar results, at least for the 1960-80 sample.

6 External returns to education

6.1 Results for 1960-80 with SOB-CSLs

The first set of IV estimates use SOB-CSL dummies as instruments for average schooling, while also treating individual schooling as endogenous. The bottom panel of Table 6 shows the relationship between SOB-CSL dummies and *average* schooling in 1960-80 data. The first-stage equations include year, year of birth, state of birth, and state of

[18] The estimates are less precise without state of residence effects, which reflects the impact of state-year clustering on the corrected standard errors.

residence dummies. CSL effects are identified in these models because cohorts born in different years in the same state were exposed to different laws. The effect of SOB-CSL dummies on average schooling is similar to, though typically somewhat smaller than, the corresponding effect on individual schooling reported in Table 4. A moderately weaker relationship is not surprising since the average schooling variables refer to a broader group than our sample of white men in their 40s.

The instrumental variables estimates reported in the top half of the table are from models that treat both s_i and $\overline{S}_{jt}$ as endogenous. Using quarter of birth and child labor laws as instruments generates a private return of .074 (s.e.=.012) and an external return of .003 (s.e.=.040). This is considerably smaller, though less precise, than the corresponding OLS estimate of external returns. The 90 percent confidence interval for external returns, [-.065, .066], excludes the OLS estimate of .073 (see Table 2). Using compulsory attendance laws as instruments generates a somewhat higher external returns. These are not significantly different from OLS estimates, but still considerably lower at .017 (s.e.=.043). Note, however, that the first stage for average schooling using $CA9-CA11$ is not as sharply patterned as the first stage using $CL7-CL9$.

Using both sets of CSL dummies as instruments generates a more precisely estimated external return of .004 (s.e.=.035). The 90 percent confidence interval for this estimate is [-.053, .061], which again excludes the OLS estimate. Finally, column 4 reports results using both CL and CA dummies, and a full set of interactions between them, as instruments. This is useful because child labor and compulsory attendance laws may work together to encourage students to stay in school longer. The results in this case are somewhat more precise than estimates that do not use the interaction terms as instruments, showing external returns of 0.005 with standard error of .033.

Earlier we argued that it is important to use the "right" private return to adjust for individual schooling when estimating external returns. On the other hand, the IV estimates of private returns in columns 1-4 of Table 6 are remarkably close to the OLS estimates of private returns reported in Table 2. This suggests that estimates of external returns from models that treat individual schooling as exogenous may not be biased. Columns 5-8 in Table 6 confirm this. These columns report estimates from models that treat individual schooling as exogenous and drop the quarter of birth instruments. The estimates of external returns in columns 5-8 are indeed similar to those in columns 1-4, though slightly more precise. For example, the estimated external return using CL dummies is

.002 (s.e.=.038). With both sets of CSL dummies, the estimated external return is .005 (s.e.=.032), while adding interactions between the two sets of dummies generates slightly negative external returns with a standard error of .03. Overall, the results in Table 6 offer no evidence for large (e.g. in the order of 7 or 8 percent) external returns to education. Moreover, they suggest that treating individual schooling as endogenous is not central, so in the rest of the paper, we will treat individual schooling as exogenous. This will enable us to allow private returns to education to vary by state and year, and also expand our sample to include data from the 1950 and 1990 Censuses.

6.2 Additional estimates using 1960-1980 data

Estimates of external returns using child labor laws as instruments ($CL7 - CL9$) change little when we modify the specification. These are reported in the top panel of Table 7. In the first column of Table 7, we allow the private return to schooling to vary by census (in all of these regressions, individual schooling is treated as exogenous). This is important since the literature on wage inequality suggests that returns to schooling have been changing over time (see, e.g., Katz and Murphy, 1992, and also the results in Table 2). Allowing private returns to vary by year generates an estimated external return of .007 (s.e.=.036), quite close to our baseline estimate in Table 6. Private returns to schooling may also vary by state, for example, because some states have a greater supply of educated workers. Allowing private returns to vary by year and state generates a negative estimate for external returns, of -.024 (s.e.=.039), reported in column 2. The results are quite similar when we use compulsory attendance instruments as in the bottom panel. When returns to schooling vary by year, we estimate a small positive external return, .021, similar to the results in Table 6 using compulsory attendance laws. However, when we allow for private returns to vary by year and state in column 2, the external return is estimated to be negative, -.018.

If IV estimates of private returns were actually larger than OLS estimates, as suggested by results in some of the studies surveyed by Card (1999), then the estimates of external returns computed here would be biased upward (see Section 3). To illustrate the implications of a higher IV estimate of private returns, we estimated external returns imposing a private return of .08 or .09 (i.e., using $Y_{ijt} - .08s_i$ or $Y_{ijt} - .09s_i$ as the dependent variable). The estimated external returns in this case, reported in columns 3 and 4 with SOB-CSLs, are even smaller than the estimates in the first two columns. With private returns of 9 percent, for example, the external return is estimated to be -.018 (s.e.=.039) with CL instruments, and .010 (s.e.=.043) with CA instruments.[19]

The second half (columns 5-7) of Table 7 reports external return estimates using SOR-CSLs as instruments for state average schooling. The first column reports baseline estimates corresponding to those in Table 6, and the two other columns allow private returns to vary by year and by state and year. These estimates may be more robust to problems of migration. Migration would typically bias external return estimates upwards, so with significant migration of high education workers to high wage states, we would expect the estimates using SOR-CSLs to be even smaller than those using SOB-CSLs. In practice, the estimates using SOR-CSLs are broadly consistent with the earlier results. The CL estimates are larger using SOR-CSLs, while the CA estimates are somewhat smaller.

6.3 Results using 1950 and 1990 data

Individual schooling is treated as exogenous in analyses using 1950 and 1990 data, since there is no quarter of birth information in these data sets. In principle, this may lead to biased estimates, although in practice, the estimates of external returns for 1960-80 are not sensitive to the exogeneity assumption. A second and potentially more serious problem is that the schooling variable in the 1990 Census is categorical, with no direct information on highest grade completed, so we have to use an imputed years of schooling measure for 1990. Since individual schooling has to be treated as exogenous when using 1990 data, the resulting measurement error may lead to biased estimates of external returns (see the discussion in Section 3).[20]

Table 8 reports estimates of external returns in the extended samples. Using child labor laws as instruments generates small positive or zero estimates of external returns with 1950-80 data. These estimates are more precise than those using 1960-80 data only. In column 1, for example, the estimated external return is .009 with a standard error of .025. As before, using compulsory attendance laws as instruments leads to somewhat

[19] A possible concern with the estimates in Tables 6 and 7 is that CSL dummies are correlated with measures of school quality. But since school quality is associated with higher wages, the omission of these variables could not be responsible for the apparent lack of an external return to education. In fact, controlling for the school quality variables used by Card and Krueger (1992a) leads to more negative estimates, though also less precise, than in our baseline specification. It should be noted, however, that even conditional on quality variables, there is a clear first-stage relationship between CSLs and average schooling.

[20] A detailed description of the schooling variables used here appears in Appendix B.

larger estimates. But these estimates are less precise than those using CL instruments, and the first-stage relationships are not uniformly consistent with a causal interpretation of the correlation between CSLs and schooling. For example, in column 1, $CA9$ has a larger coefficient than both $CA10$ and $CA11$.

In contrast with the results using 1950-80 data, adding data from the 1990 Census leads to statistically significant positive estimates of external returns when child labor laws are used as instruments. The baseline estimate reported in column 2 shows an external return of .048 with a standard error of .02. Controlling for separate private returns by census year leads to an even larger external return of .074. Using CA instruments, in contrast, does not lead to significant estimates of external returns in the 1950-90 sample. Estimates with expanded samples using SOR-CSLs are similar to, but on the whole somewhat smaller than, those using SOB-CSLs. This can be seen in Table 9. In particular, estimates of external returns in the 1950-80 sample are small and all insignificant, while some of the estimates using the 1950-90 sample and CL instruments are positive and significant (though not when we allow private returns to vary by census and state as in column 6).

The relatively large and precise external return estimates for 1950-90 generated by CL instruments in both Tables 8 and 9 may signal a change in the external value of human capital. But this result could also reflect the switch to a categorical schooling variable in 1990. The econometric discussion in Section 3 highlights the possibility of spurious external return estimates when the impact of individual schooling is poorly controlled for. Measurement error in the 1990 schooling variable could generate a problem of this type.

To check whether measurement problems could be responsible for the 1950-90 results, we assigned mean values from the 1980 Census to a categorical schooling variable available in the 1960, 1970 and 1980 Censuses. This variable is similar to the categorical 1990 variable. We then re-estimated external returns in 1960-80 treating the imputed individual schooling variable as exogenous.[21] This leads to markedly larger estimates of external returns. For example, using CL instruments to estimate external returns with imputed schooling data generates an external return of .024 instead of the estimate of .003 reported in Table 6. Similarly, using CA instruments generates an external return of .034 instead of .017 with the better-measured schooling variable. These results suggest that the higher external returns estimated with 1990 data are likely due to changes in the way the education data were collected in 1990.

[21] This exercise uses the IPUMS variable EDUCREC, which provides a uniform categorical schooling measure for the 1940-90 Censuses.

7 Concluding remarks

Evidence on the returns to education has implications for both economic policy and economic theory. A large literature in labor economics reports estimates of private returns to education on the order of 6-10 percent. However, private returns may be only part of the story. If there are positive external returns to education, then private returns underestimate the economic value of schooling. Alternatively, if the signaling role of education is important, the total economic value of schooling may be less than that suggested by private returns.

In this paper, we exploit potentially exogenous variation in state average schooling caused by changes in compulsory schooling laws. Using 1960-80 data, we find statistically insignificant external return estimates, typically ranging from -1 to 3 percent. Adding data from 1950 leads to somewhat more precise estimates, without changing the basic pattern. Regressions using data from the 1990 Census, in contrast, generate statistically significant estimates of external returns of 4 percent or more with one set of instruments. This may reflect the increased importance of human capital after 1980. Further investigation, however, suggests that the larger estimates in samples with 1990 data are probably explained by changes in the schooling variable in the 1990 Census.

On balance, therefore, the analysis here offers little evidence for sizeable external returns to education, at least over the range of variation induced by changing CSLs. Nevertheless, the standard errors associated with our estimates are large, so it is impossible for us to detect plausible external returns, say in the order of 1 to 3 percent. In fact, many of our estimates are precisely in this range. Additionally, external returns may be generated by college education, but not by high school education. Since CSLs affect exclusively education at the high school grades, externalities to college education would not show up in our estimates. Finally, external returns may be more important for younger workers, whereas we have focused on 40-49-year-olds. Further investigation of different sources of variation in average schooling, externalities from college education, and external effects on younger workers seem fruitful areas for future research.

References

Acemoglu, Daron (1996), "A Microfoundation for Social Increasing Returns in Human Capital Accumulation," *Quarterly Journal of Economics* 111 [3], 779-804.

Acemoglu, Daron (1997a) "Training and Innovation in An Imperfect Labor Market," *Review of Economic Studies* 64, 445-464.

Acemoglu, Daron (1997b) "Matching, Heterogeneity and the Evolution of Income Distribution", *Journal of Economic Growth*, 2, 61-92.

Acemoglu, Daron (1998), "Why Do New Technologies Complement Skills? Directed Technical Change and Wage Inequality," *Quarterly Journal of Economics* 113 [4], 1055-1089.

Acemoglu, Daron (1999), "Changes in Unemployment and Wage Inequality: An Alternative Theory and Some Evidence," *American Economic Review*, 89, 1259-1278.

Angrist, Joshua, Kathryn Graddy, and Guido. Imbens (1995), "Nonparametric Demand Analysis with an Application to the Demand for Fish," NBER Technical Working Paper No. 178, April (forthcoming in *The Review of Economic Studies*).

Angrist, Joshua D. and Guido W. Imbens (1995), "Two-Stage Least Squares Estimates of Average Causal Effects in Models with Variable Treatment Intensity," *Journal of the American Statistical Association* 90(430), 431-42.

Angrist, Joshua D. and Alan B. Krueger (1991), "Does Compulsory School Attendance Affect Schooling and Earnings?" *Quarterly Journal of Economics* 106, 979-1014.

Angrist, Joshua D. and Alan B. Krueger (1995),"Split-Sample Instrumental Variables Estimates of the Return to Schooling," *Journal of Business and Economic Statistics* 13, 225-235.

Angrist, Joshua D. and Alan B. Krueger (1999),"Empirical Strategies in Labor Economics," Chapter 23 in O. Ashenfelter and D. Card, eds., *The Handbook of Labor Economics Volume III*, Amsterdam: Elsevier.

Bairoch, Paul (1988), *Cities and Economic Development: From the Dawn of History to the Present*, University of Chicago Press, Chicago, translated by Christopher Braider.

Barro, Robert J. and Jong-Wha Lee (1993). "International Comparisons of Educational Attainment," *Journal of Monetary Economics* 32, 363-94.

Barro, Robert and Xavier Sala-i-Martin (1995), *Economic Growth*, New York: McGraw Hill.

Becker, Gary (1993), *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education,* 3rd edition, Chicago: The University of Chicago Press.

Benabou, Roland (1996), "Heterogeneity, Stratification, and Growth: Macroeconomic Implications of Community Structure and School Finance," *American Economic Review* 86 [3], 584-609.

Benhabib, Jess and Mark M. Spiegel (1994). "The Role of Human Capital in Economic Development: Evidence from Aggregate Cross-Country Data," *Journal of Monetary Economics* 34, 143-73.

Bils, Mark, and P.J. Klenow (1998), "Does Schooling Cause Growth or the Other Way Around?," NBER Working Paper No. 6393.

Borjas, George (1992), "Ethnic Capital and Intergenerational Mobility," *Quarterly Journal of Economics* 107, 123-150.

Bound, John, David Jaeger and Regina Baker (1995),"Problems with Instrumental Variables Estimation when the Correlation Between the Instruments and the Endogenous Explanatory Variable is Weak," *Journal of the American Statistical Association* 90, 443-50.

Brock, William and Steve Durlauf (1999), "Interactions-Based Models," forthcoming in James Heckman and Edward Leamer, eds.,*The Handbook of Econometrics*, Amsterdam: Elsevier.

Card, David E. (1995a), "Earnings, Schooling and Ability Revisited," in Solomon W. Polachek, ed., *Research in Labor Economics*, Greenwich, CT: JAI Press.

Card, David E., (1995b), "Using Geographic Variation in College Proximity to Estimate the Return to Schooling," in L.N. Christofides, E.K. Grant, and R. Swidinsky, eds, *Aspects of Labor Market Behavior: Essays in Honour of John Vandekamp*, Toronto: University of Toronto Press.

Card, David E. (1999), "The Causal Effect of Education on Earnings," in O. Ashenfelter and D. Card, eds., *The Handbook of Labor Economics* Volume III, Amsterdam: Elsevier.

Card, David E. and Alan Krueger (1992a), "School Quality and Black-White Relative Earnings: A Direct Assessment," *Quarterly Journal of Economics* 107, 151-200.

Card, David E. and Alan Krueger (1992b), "Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States," *Journal of Political Economy* 100, 1-40.

Chamberlain, Gary and Guido W. Imbens (1996) Hierarchical Bayes Models With Many Instrumental Variables," National Bureau of Economic Research Technical Paper 204, September.

Edwards, Linda (1978) "An Empirical Analysis of Compulsory Schooling Legislation," *Journal of Law and Economics* 21, 203-22.

Ehrenberg, Ronald and Alan Marcus (1982), "Minimum Wages and Teenagers Enrollment-Employment Outcomes," *Journal of Human Resources* 27, 39-58.

Foster, Andrew and Mark Rosenzweig (1996) "Technical Change in Human Capital Return and Investments:Evidence from the Green Revolution" *American Economic Review*, 86, 931-953.

Glaeser, Edward L, Heidi Kallal, Jose Scheinkman, and Andrei Shleifer (1992) "Growth in Cities" *Journal of Political Economy*, 100 (6), 1126-52.

Goldin, Claudia (1998), "America's Graduation from High School: The Evolution and Spread of Secondary Schooling in the Twentieth Century," *Journal of Economic History* 58, 345-74.

Grossman, Gene and Elhanan Helpman (1991), *Innovation and Growth in the Global Economy,* Cambridge, MA, MIT Press.

Hall, Robert and Charles I. Jones (1998). "Why Do Some Countries Produce So Much More Output per Worker Than Others?," forthcoming *Quarterly Journal of Economics.*

Hausman, Jerry (1978), "Specification Tests in Econometrics," *Econometrica* 46, 1251-1271.

Jacobs, Jane (1970), *The Economy of Cities*, New York: Vintage Books.

Jaffe, Adam B., Manuel Trajtenberg, and Rebecca Henderson, "Geographic Localizaion of Knowledge Spillovers as Evidenced by Patent Citations" *Quarterly Journal of Economics*, CVIII (1993), 577-598.

Katz, Lawrence F. and Kevin M. Murphy (1992), "Changes in Relative Wages, 1963-1987: Supply and Demand Factors," *Quarterly Journal of Economics* 107(1), 35-78

Krueger, Alan, and Mikael Lindahl (1999), "Education for Growth in Sweden and the World," NBER Working Paper No. 7190, June.

Lang, Kevin (1993) "Ability Bias, Discount Rate Bias and the Return to Education," Boston University, Department of Economics, Mimeo.

Lang, Kevin, and David Kropp (1986), "Human Capital Versus Sorting: The Effects of Compulsory Attendance Laws," *Quarterly Journal of Economics* 101, 609-624.

Lucas, Robert (1988), "On the Mechanics of Economic Development," *Journal of Monetary Economics* 22, 3-42.

Mankiw, N. Gregory, David Romer, and David N. Weil (1992), "A Contribution to the Empirics of Economic Growth," *Quarterly Journal of Economics* 107, 407-37.

Marshall, Alfred (1961), *Principles of Economics,* London: MacMillan, for the Royal Economic Society.

Moretti, Enrico (1998), "Estimating the External Return to Education: Evidence from Repeated Cross-sectional and Longitudinal Data," University of California, Berkeley, Department of Economics, Mimeo.

Margo, Robert A. (1990), *Race and Schooling in the South: 1880-1950,* Chicago: University of Chicago Press.

Margo, Robert A., and T. Aldrich Finegan (1996), "Compulsory Schooling Legislation and School Attendance in Turn-of-The Century America: A 'Natural Experiment' Approach," *Economics Letters* 53, 103-10.

Moulton, Brent R. (1986), "Random Group Effects and the Precision of Regression Estimates," *Journal of Econometrics* XXXII, 385-397.

Murphy, Kevin, Andrei Shleifer and Robert Vishny (1991);"The Allocation of Talent: The Implications For Growth" *Quarterly Journal of Economics* Vol 106, 503-530

Park, Jin Heum (1994), "Estimation of Sheepskin Effects and Returns to Schooling Using the Old and New CPS Measures of Educational Attainment," Princeton Industrial Relations Section Working Paper No. 338, December.

Rauch, James E. (1993), "Productivity Gains from Geographic Concentration of Human Capital: Evidence from the Cities" *Journal of Urban Economics* 34, 380-400.

Romer, Paul M., "Endogenous Technological Change" *Journal of Political Economy,* IIC (1990), S71-S102.

Ruggles, Steven, and Matthew Sobek (1997), *Integrated Public Use Microdata Series Version 2.0, Volume 1: User's Guide*, Historical Census Projects, University of Minnesota, Department of History.

Schultz, Theodore, *The Economic Value Education*, Columbia University Press, New York (1967).

Schmidt, Stefanie (1996), *School Quality, Compulsory Education Laws, and the Growth of American High School Attendance, 1915-1935*, MIT Ph.D. dissertation.

Staiger, Douglas and James H. Stock (1997), "Instrumental Variables Regression with

Weak Instruments," *Econometrica* 65(3), 557-86.

Summers, Lawrence and Alan Heston (1991). "The Penn World Table (Mark 5): An Expanded Set of International Comparisons, 1950-1988," *Quarterly Journal of Economics*, 106, 327-368.

Topel, Robert (1999), "Labor Markets and Economic Growth" chapter in O. Ashenfelter and D. Card, eds., *The Handbook of Labor Economics* Volume III, Amsterdam: Elsevier.

Welch, Finis (1973), "Black-White Differences in Returns to Schooling," *American Economic Review* 63, 893-907.

Appendix A: Mathematical Details

A.1 Derivation of equation (14)

Rewrite equation (13) as follows

$$Y_{ij} = \mu^* + \pi_0 \tau_i + (\pi_0 + \pi_1)\overline{S}_j + \xi_i;$$

where $\tau_i \equiv s_i - \overline{S}_j$. Since τ_i and $\overline{S}_j$ are uncorrelated by construction, we have:

$$\begin{aligned} \rho_1 &= \pi_0 + \pi_1. \\ \pi_0 &= \frac{C(\tau_i, Y_{ij})}{V(\tau_i)}. \end{aligned}$$

Simplifying the second line,

$$\begin{aligned} \pi_0 &= \frac{C[(s_i - \overline{S}_j), Y_{ij}]}{[V(s_i) - V(\overline{S}_j)]} \\ &= \left[\frac{C(s_i, Y_{ij})}{V(s_i)}\right]\left[\frac{V(s_i)}{V(s_i) - V(\overline{S}_j)}\right] - \left[\frac{C(\overline{S}_j, Y_{ij})}{V(\overline{S}_j)}\right]\left[\frac{V(\overline{S}_j)}{V(s_i) - V(\overline{S}_j)}\right] \\ &= \rho_0\phi + \rho_1(1-\phi) = \rho_1 + \phi(\rho_0 - \rho_1) \end{aligned}$$

where $\phi \equiv \frac{V(s_i)}{V(s_i) - V(\overline{S}_j)}$. Solving for π_1, we have

$$\pi_1 = \rho_1 - \pi_0 = \phi(\rho_1 - \rho_0).$$

A.2. How to instrument for individual schooling?

To discuss this issue more formally, consider a simplified version of the random coefficient model (12), again with no covariates and no time dimension. Assume also that a single binary instrument is available to estimate γ_1, say z_i, a dummy for having been born in a state with restrictive CSLs. Finally, suppose we adjust for the effects of s_i by subtracting $\gamma_2^* s_i$, where γ_2^* is some average of γ_{2i}. In other words, subtract $\gamma_2^* s_i$ from both sides of (12) to obtain

$$\begin{aligned} Y_{ij} - \gamma_2^* s_i &\equiv \widetilde{Y}_{ij} \\ &= \mu + \gamma_1 \overline{S}_j + [u_j + \varepsilon_i + (\gamma_{2i} - \gamma_2^*) s_i]. \end{aligned} \tag{15}$$

What value of γ_2^* allows us to use z_i as an instrument for $\overline{S}_j$ in (15) to obtain a consistent estimate of γ_1? The instrumental variables estimand in this case, γ_1^{IV}, is given by the

Wald formula:

$$\begin{aligned}\gamma_1^{IV} &= \frac{E[\widetilde{Y}_{ij}|z_i=1]-E[\widetilde{Y}_{ij}|z_i=0]}{E[\overline{S}_j|z_i=1]-E[\overline{S}_j|z_i=0]} \\ &= \gamma_1 + \left[\frac{E[\gamma_{2i}s_i|z_i=1]-E[\gamma_{2i}s_i|z_i=0]}{E[s_i|z_i=1]-E[s_i|z_i=0]} - \gamma_2^*\right]\cdot\left[\frac{E[s_i|z_i=1]-E[s_i|z_i=0]}{E[\overline{S}_j|z_i=1]-E[\overline{S}_j|z_i=0]}\right].\end{aligned}$$

This shows that γ_1^{IV} estimates external returns to education consistently (i.e., equals γ_1) if the adjustment for individual schooling uses the coefficient:

$$\begin{aligned}\gamma_2^* &= \frac{E[\gamma_{2i}s_i|z_i=1]-E[\gamma_{2i}s_i|z_i=0]}{E[s_i|z_i=1]-E[s_i|z_i=0]} \\ &= \frac{E[(Y_{ij}-\gamma_1\overline{S}_j)|z_i=1]-E[(Y_{ij}-\gamma_1\overline{S}_j)|z_i=0]}{E[s_i|z_i=1]-E[s_i|z_i=0]}.\end{aligned} \tag{16}$$

In other words, the adjustment for effects of s_i should use the (population) IV estimate of *private returns* generated by z_i, once we subtract the effect of human capital externalities.

Of course, we cannot use z_i to estimate both private and external returns, even though (16) appears to require this. But instruments based on quarter of birth can be used to estimate γ_2^*. Let q_i denote a single instrument derived from quarter of birth, say a dummy for first quarter births. Since q_i is orthogonal to $\overline{S}_j$, we have

$$\gamma_q^* = \frac{E[Y_{ij}|q_i=1]-E[Y_{ij}|q_i=0]}{E[s_i|z_i=1]-E[s_i|z_i=0]} = \frac{E[\gamma_{2i}s_i|q_i=1]-E[\gamma_{2i}s_i|q_i=0]}{E[s_i|q_i=1]-E[s_i|q_i=0]}.$$

If $\gamma_q^*=\gamma_2^*$, the quarter-of-birth instrument provides an appropriate adjustment for private returns in (15).[22]

To see why γ_q^* should be close to γ_2^*, let $w_i(s_i)\equiv\gamma_{2i}s_i$, and note that $w_i'(s_i)$ is the causal effect of schooling on i's (log) wages with $\overline{S}_j$ fixed (see equation (12)). Also, let s_{1i} denote the schooling i would get if $z_i=1$, and let s_{0i} denote the schooling i would get if $z_i=0$.[23] Angrist, Graddy, and Imbens (1995) show that

$$\gamma_2^* = \frac{\int E[w_i'(\sigma)|\ s_{1i}\geq\sigma>s_{0i}]P[s_{1i}\geq\sigma>s_{0i}]d\sigma}{\int P[s_{1i}\geq\sigma>s_{0i}]d\sigma}, \tag{17}$$

[22]In practice, we have more than one CSL instrument, so it may be possible to use CSLs to instrument s_i and $\overline{S}_{jt}$ simultaneously. Note, however, that because of the group structure of $\overline{S}_{jt}$ and the CSL instruments, the projection of s_i on the CSL instruments is almost identical to the projection of $\overline{S}_{jt}$ on the CSL instruments. This is not a problem with quarter of birth instruments since they are independent of $\overline{S}_{jt}$.

[23]These potential schooling choices can be described in terms of the theoretical framework. Suppose, for example, that $\eta(s_i)=\overline{\eta}$ and the CSL instrument changes discount rates from ψ_{0i} or ψ_{1i} as in Card (1995a). Using (8), individual schooling choices would be $s_{0i}=\frac{\nu\theta_i\overline{\eta}}{\psi_{0i}}$ and $s_{1i}=\frac{\nu\theta_i\overline{\eta}}{\psi_{1i}}$.

which is an average derivative with weighting function $P[s_{1i} \geq \sigma > s_{0i}] = P[s_i \leq \sigma | z_i = 0] - P[s_i \leq \sigma | z_i = 1]$. In other words, IV estimation using z_i produces an average of the derivative $w_i'(\sigma)$, with weight given to each value σ in proportion to the instrument-induced change in the cumulative distribution function (CDF) of schooling at that point. Similarly, γ_q^* is a CDF-weighted average with s_{1i} and s_{0i} defined to correspond to the values of $q_{i.}$

CSL instruments and quarter of birth instruments both estimate individual returns for people whose schooling is affected by compulsory schooling laws—i.e., individuals who would have otherwise dropped out of school. So the weighting functions $P[s_i \leq \sigma | z_i = 0] - P[s_i \leq \sigma | z_i = 1]$ and $P[s_i \leq \sigma | q_i = 0] - P[s_i \leq \sigma | q_i = 1]$ should be similar. In fact, we show in Section 5 that, like quarter of birth instruments, CSLs changed the distribution of schooling primarily in the 8-12 range. This suggests that γ_q^* and γ_2^* capture similar features of the causal relationship between individual schooling and earnings.

Appendix B: Data sources and methods

1. Micro Data

The paper uses data from the 1950, 1960, 1970, 1980, and 1990 PUMS files. Census data were taken from the IPUMS system (Ruggles and Sobek, 1997). The files used are as follows:

1950 General (1/330 sample)
1960 General (1% sample)
1970 Form 1 State (1 % sample)
1970 Form 2 State (1% sample)
1980 5% State (A Sample)
1990 1% unweighted (a 1% random self-weighted sample created by IPUMS)

Our initial extract included all US-born white men aged 21-58. The 1950 sample is limited to "sample line" individuals (i.e., those with long-form responses). Our sample excludes men born or living in Alaska or Hawaii. Estimates were weighted by the IPUMS weighting variable SLWT, adjusted in the case of 1970 to reflect the fact that we use two files for that year (i.e., divided by 2). The weights are virtually constant within years, but vary slightly to reflect minor adjustments by IPUMS to improve estimation of population totals.

The schooling variable was calculated as follows: For 1950-80, the variable is HIGRADED (General), the IPUMS recode of highest grade enrolled and grade completed into highest grade completed. For the 1990 Census, which has only categorical schooling, we assigned group means for white men from Park (1994, Table 5), who uses a one-time overlap questionaire from theFeburary 1990 CPS to construct averages for essentially the same Census categories. This generates a years of schooling variable roughlt comparable across censuses (GRADCOMP). Finally, we censored GRADCOMP at 17 since this is the highest grade completed in the 1950 census. We call this variable GRADCAP.

The dependent variable is log weekly wage, calculated by dividing annual wages by weeks worked, where wages refer to wage and salary income only. Wage topcodes vary across censuses. We imposed a uniform topcode as follows. Wage data for every year for the full extract of white men aged 21-58 were censored at the 98th percentile for that year. The censoring value is the 98th percentile times 1.5. Weeks worked are grouped in the 1960 and 1970 censuses. We assigned means to 1960 categorical values using 1950 averages and we assigned means to 1970 categorical values using 1980 averages.

The analyses in the paper, including first-stage relationships, are limited to men with positive weekly wages. Analyses using 1960-80 data are limited to men born 1910-1919 in the 1960 Census, 1920-29 in the 1970 Census, and 1930-39 in the 1980 Census. Since year of birth variables are not available in the 1950 and 1990 censuses, analyses using those data sets are limited to men aged 40-49.

2. Calculation of average schooling

Average schooling is the mean of GRADCAP by state and census year for all US-born persons aged 16-64. For 1970, we used only the Form 2 State sample (a 1 % file) and for 1980 we used a 1% random subsample, drawn from the 5% State (A Sample) using the IPUMS SUBSAMP variable. The SLWT weighting variable was adjusted to reflect the fact that this leaves a 1% sample for each year. The averages use data excluding Alaska and Hawaii (residence or birthplace). Average schooling was calculated for individuals with positive weeks worked and weighted by the product of SLWT and weeks worked. Categorical weeks worked variables were imputed as described above.

3. Match to CSLs and state average schooling

The CSLs in force in each year from 1914-72 were measured using the five variables described in Section 4 of this appendix. For each individual in the microdata extract, we calculated the approximate year the person was age 14 using age on census day (not year of birth, which is not available in 1950 and 1990). The CSLs in force in that year in the person's state of birth were then assigned to that person. State average schooling was matched to individual state of residence and census year.

4. CSL variables

Data on CSLs were collected and organized by Ms. Xuanhui Ng, in consultation with us.

a. Table of sources

Year	*enroll_age*	*drop_age*	*req_sch*	*work_age*	*work_sch*
1914	Commissioner	Schmidt Commissioner	Schmidt	Schmidt	Schmidt
1917	Biennial	Biennial	Biennial	Biennial	Biennial
1921	Chart1-1921	Chart1-1921	Chart1-1921	Chart2-1921	Chart2-1921
1924	Chart1-1924	Chart1-1924	Chart1-1924	Chart2-1924	Chart2-1924
1929	M	#197	M	#197	#197
1935	Deffenbaugh;	Deffenbaugh; Schmidt	Deffenbaugh; Schmidt	Deffenbaugh; Schmidt	Deffenbaugh; Schmidt
1939	Umbeck	Umbeck	M	M	M
1946	SCLS-1946	SCLS-1946	SCLS-1946	SCLS-1946	SCLS-1946
1950	SCLS-1949 Keesecker-1950	SCLS-1949 Keesecker-1950	SCLS-1949 Keesecker-1950	SCLS-1949 Keesecker-1950	SCLS-1949 Keesecker-1950
1954	Keesecker-1955	Keesecker-1955	Keesecker-1955	M	Keesecker-1955
1959	SCLS-1960 Umbeck	SLCS-1960 Umbeck	SLCS-1960 Umbeck	SLCS-1960	SLCS-1960 Umbeck
1965	SLCS-1965 Steinhilber	SLCS-1965 Steinhilber	SLCS-1965 Steinhilber	SLCS-1965 LLS	SLCS-1965 Steinhilber

Notes: enroll_age is maximum age by which a child has to enroll at school.
drop_age is minimum age a child is allowed to drop out of school.
req_sch is minimum years of schooling a child has to obtain before dropping out.
work_age is the minimum age at which a child can get work permit.
work_sch is the minimum years of schooling a child needs for obtaining a work permit.

Source abbreviations are given with the references.

b. Methods

Data were drawn from the sources listed in the table of sources. In some cases sources were ambiguous or there were conflicts between sources for the same year. For resolution, we looked for patterns across years that seemed to make sense, and tried to minimize the number of source changes. In the source table, "M" denotes missing, i.e., we found no source or reliable information for this variable in this year. Missing data were imputed by bringing older data forward. Inter-source years were imputed and the data set expanded by bringing older data forward to make a complete set of 5 CSL laws for each year from 1914 to 1965.

The imputed data set contains either numerical entries or an "NR" indicating we found laws that appeared to impose no restriction (e.g., 6 years schooling required for a work permit, so *work_sch*=6, but a work permit available at any age, so *work_age*=NR). The algorithm for calculating required years of schooling for dropout and the required years of schooling for a work permit handles NR codes as follows:

If *req_sch*=NR, then *req_sch*=0;
If *enroll_age*=NR or *drop_age*=NR, then *ca*=max(0, *req_sch*);
If *enroll_age*≠NR and *drop_age*≠NR then *ca*=max(*drop_age*-*enroll_age*, *req_sch*).

If *work_age*=NR, then *work_age*=0;
If *work_sch*=NR, then *work_sch*=0;
If *enroll_age*=NR then *cl*=max(0, *work_sch*);
If *enroll_age*≠NR then *cl=max(work_age-enroll_age, work_sch*).

We coded a general literacy requirement without a specific grade or age requirement as NR. We coded a grade requirement of "elementary school" as 6, even though this was distinct from sixth grade in some sources (our dummies would group these requirements anyway).

5. References for Appendix B

[Deffenbaugh] Deffenbaugh, Walter S. and Keesecker, Ward, W., *Compulsory School Attendance Laws and Their Administration*, US Department of Interior, Office of Education, Bulletin 1935, No. 4, Washington: US GPO (1935).

[Keesecker-1950] Keesecker, Ward, W. and Allen, Alfred, C., *Compulsory School Attendance and Minimum Educational Requirements in the United States, 1950*, Federal Security Agency, Office of Education, Circular No. 278 (September 1950).

[Keesecker-1955] Keesecker, Ward, W. and Allen, Alfred, C., *Compulsory School Attendance and Minimum Educational Requirements in the United States*, US Department of Health, Education, and Welfare, Office of Education, Circular No. 440, Washington: US Department of Health, Education and Welfare (March 1955).

[Schmidt] Schmidt, Stefanie, R., *School Quality, Compulsory Education Laws, and the Growth of American High School Attendance*, 1915-35, MIT Ph.D. dissertation (1996).

[Steinhilber] Steinhilber, A.W. and Sokolowski, C.J., *State Law on Compulsory Attendance*, US Department of Health, Education and Welfare, Office of Education, Circular 793, Washington: US GPO (1966).

[Umbeck] Umbeck, Nelda, *State Legislation on School Attendance and Related Matters - School Census and Child Labor*, US Department of Health, Education and Welfare, Office of Education, Circular No. 615, Washington: US GPO (January 1960).

[Biennial] US Department of the Interior, Bureau of Education, *Biennial Survey of Education 1916-18*. Bulletin 1919, No. 90 (1921).

[Commissioner] US Department of the Interior, Office of Education, *Report of the Commissioner of Education for the Year Ended June 30, 1917*, Vol. 2, p. 69, Washington: US GPO.

[LLS] U.S. Department of Labor, Bureau of Labor Standards, *Summary of State Child Labor Laws*, Labor Law Series No. 3-A, Washington: US Department of Labor (September 1966).

[SCLS-1946] U.S. Department of Labor, Division of Labor Standards, *State Child-Labor Standards: A State-by-State Summary of Laws Affecting the Employment of Minors Under 18 Years Of Age*, Child Labor Series No. 2, Washington: US GPO (July 1946).

[SCLS-1949] _____, *State Child-Labor Standards: A State-by-State Summary of Laws Affecting the Employment of Minors Under 18 Years of Age*, Bulletin 114, Washington: US GPO (September 1949).

[SCLS-1960] _____, *State Child-Labor Standards: A State-by-State Summary of Laws Affecting the Employment of Minors Under 18 Years of Age*, Bulletin 158 (Revised 1960), Washington: US GPO (May 1960).

[SCLS-1965] _____, *State Child-Labor Standards: A State-by-State Summary of Laws Affecting the Employment of Minors Under 18 Years of Age*, Bulletin 158 (Revised 1965), Washington: US GPO (September 1965).

[Chart1-1921] US Department of Labor, Children's Bureau, *State Child-Labor Standards, January 1, 1921*," Chart Series No. 1, Washington: US GPO.

[Chart1-1924] _____, *State Child-Labor Standards*, January 1, 1924, Chart Series No. 1, Washington: US GPO.

[Chart2-1921] _____, *State Compulsory School Attendance Standards Affecting the Employment of Minors*, January 1, 1921, Chart Series No. 2, Washington: US GPO.

[Chart2-1924] _____, "State Compulsory School Attendance Standards affecting the employment of minors, January 1, 1924," Chart Series No. 2, Washington: US GPO.

[#197] _____ *Child Labor Facts and Figures (October 1929)* Bulletin 197, Washington: US GPO.

Table 1
Descriptive Statistics for Census IPUMS

Variables	1950	QOB Samples 1960	1970	1980	1990
Covariates					
Age	44.16	44.55	44.74	44.66	44.10
	(2.87)	(2.88)	(2.90)	(2.94)	(2.84)
Individual Education	9.67	10.52	11.59	12.62	13.70
	(3.40)	(3.22)	(3.18)	(2.98)	(2.49)
Regressors					
State Average Education	9.94	10.65	11.52	12.46	13.10
	(0.72)	(0.54)	(0.41)	(0.30)	(0.23)
Lowest State Average Education	7.87	9.24	10.45	11.81	12.62
	[MS]	[MS]	[SC]	[KY]	[AR]
Highest State Average Education	11.18	11.80	12.38	13.07	13.74
	[UT]	[UT]	[UT]	[DC]	[DC]
Dependent Variable					
Log Weekly Wage	4.06	4.64	5.17	5.90	6.44
	(0.77)	(0.63)	(0.65)	(0.72)	(0.73)
Instruments					
Percent Child Labor 6	0.45	0.23	0.19	0.05	0.03
Percent Child Labor 7	0.45	0.36	0.24	0.24	0.16
Percent Child Labor 8	0.10	0.36	0.50	0.41	0.37
Percent Child Labor 9+	0.01	0.05	0.07	0.31	0.44
Percent Compulsory Attendance 8	0.57	0.35	0.24	0.11	0.11
Percent Compulsory Attendance 9	0.40	0.53	0.44	0.44	0.44
Percent Compulsory Attendance 10	0.02	0.06	0.08	0.09	0.06
Percent Compulsory Attendance 11+	0.01	0.07	0.24	0.37	0.39
N	16659	72344	161029	376479	103184

Notes: Standard deviations are in parentheses. Bracketed entries in the ‘Lowest State Average Education’ and ‘Highest State Average Education’ rows are abbreviations indicating the state with the lowest and highest average schooling. All other entries are means. The data are from the Census IPUMS for 1960 through 1980, with the sample restricted to white males aged 40-49 in the Census year.

Table 2
OLS Estimates of Private and External Returns to Schooling

	1960-1980 (1)	1950-1980 (2)	1950-1990 (3)	1950 (4)	1960 (5)	1970 (6)	1980 (7)	1990 (8)
				A. Private Returns				
Private Return to Schooling	0.073 (0.0003)	0.068 (0.0003)	0.075 (0.0003)	0.055 (0.002)	0.069 (0.001)	0.076 (0.001)	0.075 (0.001)	0.102 (0.001)
State of Residence Main Effects?	YES	YES	YES	NO	NO	NO	NO	NO
				B. Private and External Returns				
Private Return to Schooling	0.073 (0.000)	0.068 (0.000)	0.074 (0.000)	0.055 (0.002)	0.068 (0.001)	0.075 (0.001)	0.074 (0.000)	0.102 (0.001)
External Return to Schooling	0.073 (0.016)	0.061 (0.004)	0.072 (0.003)	0.136 (0.017)	0.136 (0.016)	0.128 (0.021)	0.160 (0.027)	0.168 (0.047)
State of Residence Main Effects?	YES	YES	YES	NO	NO	NO	NO	NO
N	609852	626511	729695	16659	72344	161029	376479	103184

Notes: Standard errors corrected for state-year clustering are shown in parentheses. The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white males aged 40-49 in the Census year. All regressions contain Census year, year of birth, and state of birth main effects.

Table 3
Description of Child Labor and Compulsory Schooling Laws

Year at Age 14 (Census Year)	Earliest Drop Out Age (1)	Latest Enrollment Age (2)	Minimum Schooling for Dropout (3)	Earliest Work Age (4)	Required Schooling for Work Permit (5)
1914 (50)	15.31	7.49	1.90	11.00	1.70
	(1.20)	(0.52)	(3.40)	(5.75)	(2.56)
1917 (50)	15.55	7.63	1.93	13.43	2.98
	(0.89)	(0.49)	(2.74)	(1.98)	(2.66)
1921 (50)	15.69	7.42	4.28	13.94	4.19
	(0.99)	(0.51)	(3.63)	(1.71)	(2.97)
1924 (60)	15.88	7.29	5.64	14.11	4.91
	(0.97)	(0.57)	(3.64)	(1.33)	(3.04)
1929 (60)	15.97	7.30	5.66	14.16	5.31
	(0.93)	(0.58)	(3.62)	(1.33)	(3.01)
1935 (70)	15.96	7.24	7.24	14.14	6.02
	(0.94)	(0.55)	(3.73)	(0.76)	(2.67)
1939 (70)	16.16	7.16	7.29	14.15	6.01
	(1.05)	(0.51)	(3.74)	(0.77)	(2.70)
1946 (80)	16.31	7.09	7.91	14.77	4.67
	(0.63)	(0.53)	(4.00)	(1.16)	(3.37)
1950 (80)	16.27	7.08	7.94	15.03	3.51
	(0.60)	(0.53)	(4.49)	(1.14)	(3.47)
1954 (80)	16.30	7.05	7.79	15.02	4.06
	(0.63)	(0.52)	(4.65)	(1.20)	(3.67)
1959 (90)	16.25	7.05	7.40	15.19	3.49
	(0.60)	(0.53)	(4.79)	(1.19)	(3.56)
1964 (90)	16.20	7.05	7.44	15.17	3.51
	(0.60)	(0.54)	(4.79)	(1.22)	(3.57)

Notes: Standard deviations are in parentheses. All other entries are means. The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white men aged 40-49 in the Census year. See the data appendix for sources and method.

Table 4a
The Effect of State-of-Birth Compulsory Schooling Laws on Individual Schooling

	Including State of Residence Controls				Without State of Residence Controls			
	1960-1980	1950-1980	1950-1990	1980	1960-1980	1950-1980	1950-1990	1980
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				State-of-Birth Child Labor Laws				
CL 7	0.095	0.117	0.173	0.050	0.105	0.115	0.175	0.062
	(0.030)	(0.024)	(0.021)	(0.041)	(0.077)	(0.051)	(0.043)	(0.041)
CL 8	0.124	0.130	0.213	0.132	0.120	0.119	0.202	0.143
	(0.034)	(0.032)	(0.026)	(0.034)	(0.093)	(0.075)	(0.059)	(0.034)
CL 9	0.259	0.220	0.398	0.167	0.269	0.225	0.410	0.182
	(0.039)	(0.038)	(0.028)	(0.041)	(0.098)	(0.084)	(0.059)	(0.041)
				State-of-Birth Compulsory Attendance Laws				
CA 8	0.117	0.083	0.189	-0.011	0.103	0.068	0.171	-0.009
	(0.027)	(0.025)	(0.020)	(0.034)	(0.072)	(0.057)	(0.043)	(0.034)
CA 9	0.095	0.059	0.113	0.100	0.106	0.074	0.133	0.104
	(0.034)	(0.036)	(0.020)	(0.044)	(0.085)	(0.077)	(0.063)	(0.045)
CA 10	0.167	0.144	0.260	0.115	0.184	0.165	0.290	0.119
	(0.038)	(0.036)	(0.028)	(0.037)	(0.103)	(0.085)	(0.063)	(0.038)
N	609852	626511	729695	376479	609852	626511	729695	376479

Notes: Standard errors corrected for state-year clustering are shown in parentheses. The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white males aged 40-49 in the Census year. All regressions contain Census year, year of birth, and state of birth main effects. Compulsory Schooling Laws are assigned according to the laws in effect in the individual's state of birth when he was 14.

Table 4b

The Effect of State-of-Birth Compulsory Schooling Laws on Discrete Levels of Schooling

	Results for 1960-1980					Results for 1950-1980				
	Completed 8 Years or Higher	Completed 10 Years or Higher	Completed 12 Years or Higher	Completed 14 Years or Higher	Completed 16 Years or Higher	Completed 8 Years or Higher	Completed 10 Years or Higher	Completed 12 Years or Higher	Completed 14 Years or Higher	Completed 16 Years or Higher
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent Variable Mean	0.908	0.747	0.617	0.249	0.167	0.884	0.695	0.562	0.226	0.151
	State-of-Birth Child Labor Laws									
CL 7	0.019	0.019	0.014	-0.005	-0.005	0.031	0.014	0.009	-0.004	-0.004
	(0.004)	(0.005)	(0.005)	(0.006)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)
CL 8	0.032	0.023	0.018	-0.014	-0.014	0.033	0.019	0.016	-0.009	-0.010
	(0.005)	(0.005)	(0.005)	(0.007)	(0.046)	(0.005)	(0.005)	(0.005)	(0.006)	(0.003)
CL 9	0.061	0.045	0.035	-0.019	-0.018	0.065	0.034	0.024	-0.021	-0.007
	(0.005)	(0.006)	(0.006)	(0.007)	(0.052)	(0.005)	(0.006)	(0.006)	(0.007)	(0.004)
	State-of-Birth Compulsory Attendance Laws									
CA 8	0.036	0.014	0.010	-0.009	-0.011	0.032	0.010	0.006	-0.010	-0.010
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.003)
CA 9	0.020	0.023	0.025	-0.011	-0.008	0.016	0.022	0.022	-0.011	-0.009
	(0.004)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
CA 10	0.030	0.034	0.037	-0.013	-0.009	0.022	0.032	0.032	-0.010	-0.005
	(0.005)	(0.006)	(0.006)	(0.007)	(0.005)	(0.005)	(0.006)	(0.005)	(0.007)	(0.005)

Notes: Standard errors corrected for state-year clustering are shown in parentheses. All entries are OLS estimates from a regression of a dummy for having completed the indicated year of schooling on child labor law or compulsory attendance law dummies. All regressions also contain Census year, year of birth, state of birth, and state of residence main effects. The data are from the Census IPUMS for 1950 through 1980, with the sample restricted to white males aged 40-49 in the Census year. Compulsory Schooling Laws are assigned according to the laws in effect in the individual's state of birth when he was 14. The sample size for the 1960-1980 columns is 609,852; the sample size for the 1950-1980 columns is 626,511.

Table 5
2SLS Estimates of Private Returns to Schooling

	QOB Instruments			CSL Instruments					
				SOB-CL Instruments			SOB-CA Instruments		
	1960-1980	1980	1960-1970	1960-1980	1950-1980	1950-1990	1960-1980	1950-1980	1950-1990
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Including State of Residence Main Effects	0.073 (0.012)	0.090 (0.016)	0.063 (0.017)	0.076 (0.034)	0.103 (0.038)	0.113 (0.018)	0.092 (0.044)	0.099 (0.052)	0.081 (0.023)
No State of Residence Main Effects	0.073 (0.012)	0.088 (0.016)	0.063 (0.017)	0.080 (0.064)	0.112 (0.060)	0.126 (0.027)	0.101 (0.088)	0.094 (0.086)	0.100 (0.040)
N	609852	376479	233373	609852	626511	729695	609852	626511	729695

Notes: Standard errors corrected for state-year clustering are in parentheses. All entries are two-stage least squares estimates of private returns to schooling, using the excluded instruments indicated above and discussed in the text. The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white males aged 40-49 in the Census year. 'QOB' refers to the set 30 dummies interacting quarter of birth and year of birth. . 'SOB-CL' refers to a set of dummies indicating state and year specific child labor laws assigned according to the laws in effect in the individual's state of birth when he was 14. 'SOB-CA' refers to a set of dummies indicating state and year specific compulsory attendance laws assigned according to the laws in effect in the individual's state of birth when he was 14. All models contain Census year, year of birth, and state of birth main effects.

Table 6
2SLS Estimates of Private and External Returns to Schooling- State-of-Birth Instruments

	Individual Schooling Endogenous				Individual Schooling Exogenous			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Second-Stage Estimates				
Instrument Set	QOB & CL	QOB & CA	QOB, CA & CL	QOB, CA & CL, Interactions	CL	CA	CL & CA	QOB, CA & CL, interactions
Private Return to Schooling	0.074	0.074	0.075	0.060	0.073	0.073	0.073	0.073
	(0.012)	(0.012)	(0.012)	(0.013)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
External Return to Schooling	0.003	0.017	0.004	0.005	0.002	0.018	0.006	-0.011
	(0.040)	(0.043)	(0.035)	(0.033)	(0.038)	(0.042)	(0.033)	(0.030)
				First-Stage for State-Year Average Schooling				
CL 7	0.080		0.062		0.084		0.062	
	(0.028)		(0.025)		(0.028)		(0.025)	
CL 8	0.107		0.068		0.107		0.068	
	(0.035)		(0.031)		(0.035)		(0.031)	
CL 9	0.227		0.184		0.226		0.183	
	(0.036)		(0.034)		(0.035)		(0.034)	
CA 9		0.128	0.102			0.128	0.104	
		(0.026)	(0.023)			(0.026)	(0.030)	
CA 10		0.122	0.104			0.122	0.104	
		(0.030)	(0.029)			(0.030)	(0.029)	
CA 11		0.144	0.094			0.143	0.094	
		(0.038)	(0.036)			(0.038)	(0.036)	

Notes: Standard errors corrected for state-year clustering are reported in parentheses. All entries are two-stage least squares estimates of returns to schooling, using the excluded instruments indicated above and discussed in the text. 'QOB' refers to a set of dummies interacting quarter of birth and year of birth. 'CL' refers to a set of dummies indicating state and year specific child labor laws. 'CA' refers to a set of dummies indicating state and year specific compulsory attendance laws. These are assigned according to the laws in effect in the individual's state of birth when he was 14. The data are from the Census IPUMS for 1960 through 1980, with the sample restricted to white males aged 40-49 in the Census year. All regressions contain Census year, year of birth, state of birth, and state of residence main effects. The sample size for all columns is 609,852.

Table 7
2SLS Estimates of External Returns to Schooling: Additional Results

	With State-of-Birth Instruments				With State-of-Residence Instruments		
	Private Returns Separate by Census	Private Returns Separate by Census and State	Private Returns=.08	Private Returns=.09	Baseline Estimates	Private Returns Separate by Census	Private Returns Separate by Census and State
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	A. Results Using Child Labor Laws as Instruments						
External Return to Schooling	0.007	-0.024	-0.006	-0.018	0.048	0.051	0.015
	(0.039)	(0.039)	(0.038)	(0.039)	(0.033)	(0.034)	(0.033)
	First Stage for State-Year Average Schooling						
CL 7	0.083	0.080	0.084	0.084	0.124	0.123	0.120
	(0.028)	(0.026)	(0.028)	(0.029)	(0.046)	(0.046)	(0.043)
CL 8	0.104	0.100	0.107	0.107	0.158	0.157	0.154
	(0.034)	(0.031)	(0.035)	(0.035)	(0.053)	(0.052)	(0.049)
CL 9	0.223	0.210	0.227	0.227	0.402	0.399	0.390
	(0.035)	(0.032)	(0.036)	(0.035)	(0.063)	(0.062)	(0.058)
	B. Results Using Compulsory Attendance Laws as Instruments						
External Return to Schooling	0.021	-0.018	0.011	0.010	0.009	0.007	-0.031
	(0.043)	(0.043)	(0.042)	(0.043)	(0.053)	(0.054)	(0.054)
	First-Stage for State-Year Average Schooling						
CA 9	0.125	0.118	0.128	0.128	0.164	0.162	0.155
	(0.026)	(0.023)	(0.026)	(0.026)	(0.038)	(0.038)	(0.035)
CA10	0.120	0.112	0.122	0.122	0.161	0.159	0.151
	(0.030)	(0.027)	(0.030)	(0.030)	(0.059)	(0.058)	(0.055)
CA 11	0.141	0.134	0.143	0.143	0.207	0.205	0.199
	(0.037)	(0.034)	(0.038)	(0.038)	(0.055)	(0.054)	(0.051)
	C. OLS Estimates						
External Return to Schooling	0.079	0.044	0.069	0.063	0.073	0.079	0.044
	(0.016)	(0.016)	(0.017)	(0.017)	(0.016)	(0.016)	(0.016)

Notes: Standard errors corrected for state-year clustering are reported in parentheses. All entries are estimates of returns to schooling, using dummies for child labor laws or compulsory attendance laws as excluded instruments. The data are from the Census IPUMS. The sample is restricted to white males aged 40-49 in the Census year. All regressions contain individual education, Census year, year of birth, state of birth, and state of residence main effects. The first four columns use state-of-birth child labor laws or compulsory attendance laws as instruments, which are assigned according to the laws in effect in the individual's state of birth when he was 14. The last four columns use state-of-residence child labor laws or compulsory attendance laws as instruments, which are assigned according to the laws in effect in the individual's state of residence 30 years ago. The sample size for all columns is 609,852.

Table 8
2SLS Estimates: Additional Samples with State-of-Birth Instruments

	Baseline Results		Separate Private Returns by Census		Separate Private Returns by Census and State	
	50-80 (1)	50-90 (2)	50-80 (3)	50-90 (4)	50-80 (5)	50-90 (6)
			A. Results Using Child Labor Laws as Instruments			
External Return	0.009 (0.025)	0.048 (0.019)	0.023 (0.025)	0.074 (0.019)	-0.034 (0.025)	0.041 (0.021)
			First-Stage for State-Year Average Schooling			
CL 7	0.173 (0.024)	0.165 (0.019)	0.170 (0.023)	0.162 (0.019)	0.158 (0.020)	0.145 (0.016)
CL 8	0.126 (0.036)	0.144 (0.027)	0.123 (0.035)	0.139 (0.027)	0.113 (0.031)	0.121 (0.022)
CL 9	0.278 (0.039)	0.333 (0.026)	0.275 (0.039)	0.327 (0.026)	0.250 (0.034)	0.280 (0.022)
			B. Results Using Compulsory Attendance Laws as Instruments			
External Return	0.040 (0.038)	0.0006 (0.027)	0.053 (0.039)	0.038 (0.027)	0.017 (0.038)	-0.008 (0.029)
			First- Stage for State-Year Average Schooling			
CA 9	0.133 (0.028)	0.172 (0.019)	0.130 (0.027)	0.168 (0.019)	0.118 (0.023)	0.143 (0.015)
CA 10	0.106 (0.037)	0.167 (0.028)	0.105 (0.036)	0.164 (0.027)	0.096 (0.031)	0.139 (0.022)
CA 11	0.096 (0.042)	0.182 (0.029)	0.095 (0.041)	0.178 (0.028)	0.087 (0.036)	0.154 (0.023)
			C. OLS Estimates			
External Return	0.061 (0.009)	0.072 (0.006)	0.076 (0.009)	0.094 (0.007)	0.039 (0.008)	0.057 (0.004)
N	626510	729695	626510	729695	626510	729695

Notes: Standard errors corrected for state-year clustering are reported in parentheses. Estimates of social returns to schooling use dummies for child labor and compulsory attendance laws as excluded instruments. Individual schooling is treated as exogenous. The sample is restricted to white males aged 40-49 in the Census year. All regressions contain individual schooling, Census year, year of birth, state of birth, and state of residence main effects. Compulsory Schooling Laws are assigned according to the laws in effect in the individual's state of birth when he was 14.

Table 9
2SLS Estimates: Additional Samples with State-of-Residence Instruments

	Baseline Results		Separate Private Returns By Census		Separate Private Returns By Census and State	
	50-80	50-90	50-80	50-90	50-80	50-90
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Results Using Child Labor Laws as Instruments			
External Return	0.016	0.044	0.024	0.054	-0.007	0.016
	(0.028)	(0.022)	(0.028)	(0.022)	(0.026)	(0.023)
			First Stage for State-Year Average Schooling			
CL 7	0.215	0.185	0.213	0.183	0.202	0.174
	(0.035)	(0.031)	(0.035)	(0.031)	(0.031)	(0.027)
CL 8	0.142	0.128	0.142	0.127	0.134	0.116
	(0.054)	(0.045)	(0.054)	(0.044)	(0.049)	(0.039)
CL 9	0.430	0.452	0.426	0.449	0.401	0.409
	(0.068)	(0.048)	(0.067)	(0.047)	(0.061)	(0.043)
			B. Results Using Compulsory Attendance Laws as Instruments			
External Return	0.007	-0.0004	0.014	0.020	-0.017	-0.029
	(0.045)	(0.032)	(0.046)	(0.031)	(0.043)	(0.033)
			First Stage for State-Year Schooling			
CA 9	0.192	0.247	0.190	0.244	0.177	0.218
	(0.043)	(0.030)	(0.042)	(0.030)	(0.038)	(0.026)
CA 10	0.147	0.198	0.145	0.195	0.137	0.171
	(0.075)	(0.056)	(0.074)	(0.056)	(0.067)	(0.049)
CA 11	0.145	0.254	0.143	0.251	0.136	0.229
	(0.063)	(0.046)	(0.063)	(0.045)	(0.057)	(0.040)
			B. OLS Estimates			
C.						
External Return	0.061	0.072	0.076	0.094	0.038	0.057
	(0.010)	(0.008)	(0.010)	(0.007)	(0.009)	(0.007)
N	626511	729625	626511	729625	626511	729625

Notes: Standard errors corrected for state-year clustering are reported in parentheses. Estimates of social returns to schooling use dummies for child labor and compulsory attendance laws as excluded instruments. Individual schooling is treated as exogenous. The sample is restricted to white males aged 40-49 in the Census year. All regressions contain individual schooling, Census year, year of birth, state of birth, and state of residence main effects. Compulsory Schooling Laws are assigned according to the laws in effect in the individual's state of residence 30 years ago.

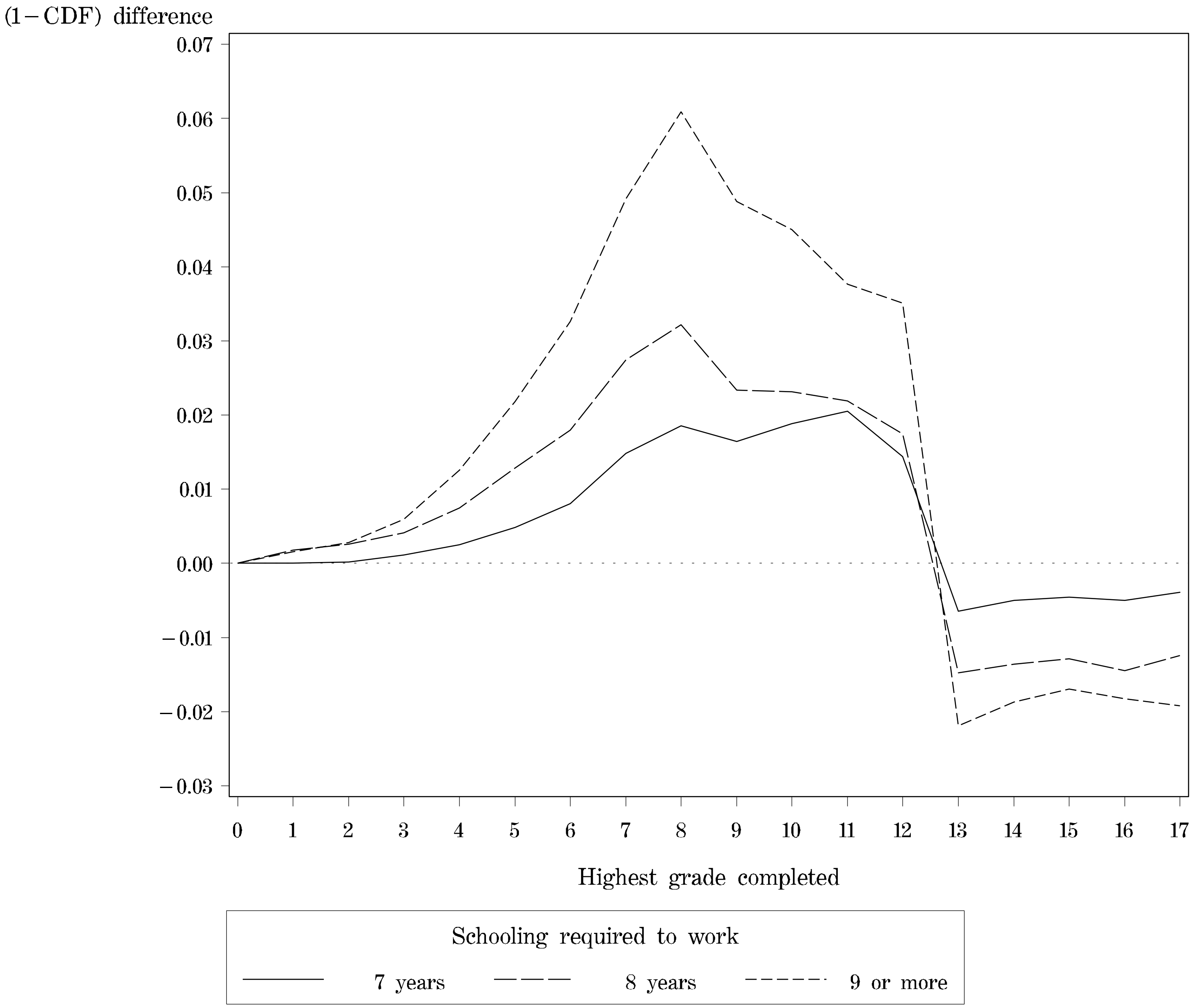

Figure 1. CDF difference by severity of child labor laws. The figure shows the difference in the probability of schooling at or exceeding the grade level on the X−axis. The reference group is 6 or fewer years required schooling.

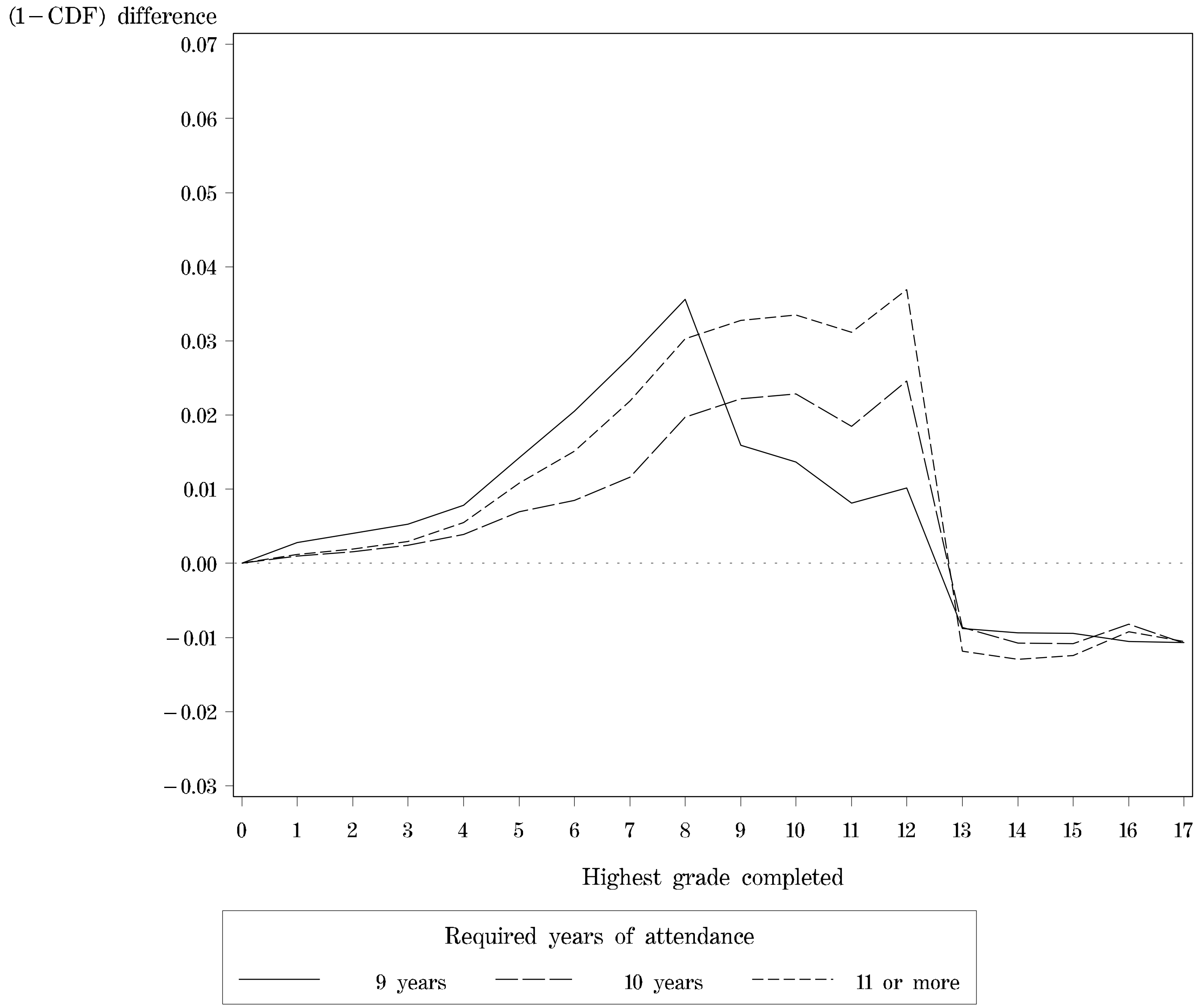

Figure 2. CDF difference by severity of compulsory attendance laws. The figure shows the difference in the probability of schooling at or exceeding the grade level on the X−axis. The reference group is 8 or fewer years required schooling.