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

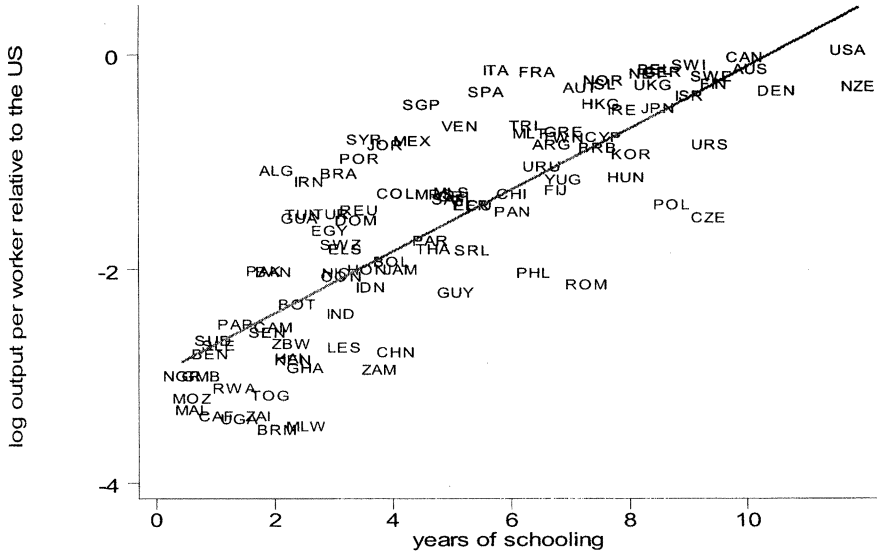
1. *Introduction*

The effect of human capital on aggregate income is of central importance to both policymakers and economists. A tradition going back to Schultz (1967) and Nelson and Phelps (1966) views the human capital of the workforce 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's (1988) model, worker productivity depends on the aggregate skill level, whereas Romer (1990) suggests that societies with more skilled workers generate more ideas and grow faster. More generally, many economists believe that cross-country income disparities are due in large part to differences in human capital (e.g., Mankiw, Romer, and Weil, 1992). Figure 1 plots the logarithm of output per worker relative to the United States for 103 countries against average years of schooling in 1985. Consistent with this view, the figure shows a strong correlation between output per worker and schooling. In fact, the bivariate regression line plotted in Figure 1 has an R^2 of 65%.[1]

We thank Alexis Leon, Chris Mazingo, and Xuanhui Ng for excellent research assistance, and our discussants Mark Bils and Cecelia Rouse for their comments. Thanks also go to 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 2000 NBER Macroeconomics Annual Conference, the 1999 NBER Summer Institute, University College London, Cornell University, the University of Maryland, and the University of Toronto for helpful discussions and comments. Special thanks to Stefanie Schmidt for advice on compulsory-schooling data.

1. Data on output per worker are from Summers and Heston (1991), with the correction due to Hall and Jones (1999). Education data are from Barro and Lee (1993). See Krueger

Figure 1 LOG OUTPUT PER WORKER AND YEARS OF SCHOOLING ACROSS COUNTRIES



The line shows the fitted OLS relationship. The slope coefficient is 0.29, and the standard error is 0.02.

A simple calculation suggests that for education to raise income as steeply as suggested by Figure 1, there must be large human-capital externalities. To see this, note that the *private return to schooling,* i.e., the increase in individual earnings resulting from an additional year of schooling, is about 6–10% (e.g., Card, 1999). If the *social return to schooling,* i.e., the increase in total earnings resulting from a one-year increase in average schooling, is of roughly the same magnitude, then differences in schooling can explain little of the cross-country variation in income. More specifically, the difference in average schooling between the top and bottom deciles of the world education distribution in 1985 is less than 8 years. With social returns to schooling around 10%, we would expect the top-decile countries to produce about twice as much per worker as the bottom-decile countries. In fact, the output-per-worker gap is approximately 15. Put differently, a causal interpretation of Figure 1 requires

and Lindahl (1999) for a detailed analysis of the cross-country relationship between education and income.

human-capital externalities on the order of 25–30%, approximately three times as large as the private returns to schooling.[2]

Human-capital externalities are important for education policy as well as for cross-country income differences. Current education policies are often justified on the basis of at least modest externalities. Nevertheless, there is little empirical work estimating human-capital externalities. Moreover, even as a theoretical matter, it is not clear whether social returns should exceed private returns. Despite the emphasis on human-capital externalities in recent growth models, education may also play a signaling role (e.g., Spence, 1973; Lang and Kropp, 1986). If schooling has signaling value, social returns to education can be less than private returns. In the extreme case where schooling does not increase human capital but is only a signal, aggregate income is unchanged when all workers increase their schooling by one year, so social returns are zero. Social returns may also be less than private returns if some other factor of production is inelastically supplied.

Rauch (1993) is the first attempt to estimate human-capital externalities. His results suggest there are externalities on the order of 3–5%, though he also reports some considerably larger estimates. Rauch's estimates are driven by differences in average schooling across cities. But higher incomes might cause more schooling instead of vice versa. Cities with greater average schooling may also 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 the average schooling level in an individual's state. An ideal instrument for average schooling would affect the schooling of the majority of workers in a given area. Differences in compulsory attendance laws and child labor laws in U.S. states between 1920 and 1960 provide such variation.

State compulsory attendance laws and child labor laws, which we refer to together as *compulsory schooling laws* (CSLs), generate an attractive natural experiment for the estimation of human-capital externalities (or *external returns*) for a number of reasons. First, while these laws were determined by social forces operating in states at the time of passage, the CSLs that affected an individual in childhood are not affected by future wages. Childhood CSLs are therefore exogenous to adult

2. The slope of the line in Figure 1, 0.29, corresponds to social returns of 34% ($e^{0.29} - 1 \simeq 0.34$). The difference between top- and bottom-decile countries implies social returns on the order of 40%. To rationalize Figure 1, we therefore need human-capital externalities of 25–30% on top of the 6–10% private returns.

wages. Second, although in principle CSLs may be correlated with omitted factors that also affect schooling and future wages, we provide evidence suggesting this is not a problem. Omitted variables related to family background or tastes would likely induce correlation between CSLs and college attendance as well as secondary and middle schooling. The results below show that CSLs affected schooling exclusively in middle-school and high-school grades, suggesting that omitted factors do not bias estimates using CSLs as instruments. A third consideration is that 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 United States in the twentieth century.

The baseline results in the paper use samples of white men aged 40–49 from the 1960–1980 Censuses, though some results use 1950 and 1990 data and samples of men aged 30–39. We focus on the 1960–1980 Censuses because the Census schooling variable changed in 1990. Also, we show below that it is important to control for private returns correctly by instrumenting for individual schooling when estimating external returns. The 1960–1980 Censuses include information on quarter of birth, which can be used as an instrument for individual schooling as in Angrist and Krueger (1991). We start with men in their 40s because they are on a relatively flat part of the age–earnings profile. This makes it easier to control for the effect of individual education on earnings, and facilitates the use of quarter-of-birth instruments for individual schooling. Finally, blacks are excluded because blacks in these cohorts experienced marked changes in school quality (see, e.g., Welch, 1973; Margo, 1990; or Card and Krueger, 1992a).

Ordinary least-squares (OLS) estimates using data from the 1960–1980 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% increase in average wages, over and above the roughly equal private returns. In contrast with the OLS estimates, instrumental variables (IV) estimates of external returns for men aged 40–49 in 1960–1980 are typically around 1–2%, and significantly lower than the corresponding OLS estimates. Adding data from the 1950 Census and/or data for men aged 30–39 yields slightly smaller and more precise estimates.[3] We therefore conclude there is little evidence for large external returns, though the results are consistent with modest external returns of 1–3%. The confidence intervals typically exclude human capital externalities greater than 5–6% and therefore rule out magnitudes in the

3. Adding data from the 1990 Census results in somewhat larger estimates of external returns, but this finding seems to be generated by problems with the schooling variable in the 1990 Census.

range of the OLS estimates. They also rule out magnitudes necessary to rationalize the steep relationship between schooling and output per worker observed in Figure 1. This implies that differences in average education are unlikely to be a major source of cross-country income differences.

A shortcoming of the approach used here is that it identifies local human-capital externalities only. We miss externalities that arise if, for example, more-skilled workers generate ideas used in other parts of the country. It should be noted, however, that most theories of externalties suggest an important local component (see, e.g., Glaeser et al., 1992, and Jaffe, Trajtenberg, and Henderson, 1993). Another limitation of estimation based on CSLs is that CSL variation mainly affects secondary education. A recent paper by Moretti (1999) explores the relationship between increasing numbers of college graduates and income in U.S. cities. Moretti finds sizable human-capital externalities. These results might be driven by greater externalities from college education, though they might also reflect differences in empirical strategy. In any case, externalities from high school are probably at least as important as externalities from college education; the bulk of twentieth-century U.S. human-capital accumulation is accounted for by changes in secondary schooling, as are most of the differences in schooling between high- and low-education countries.

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 (1970) 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). This notion also provides part of the motivation for Lucas's (1988) argument that human capital has important external returns. We refer to externality theories in this mold as *nonpecuniary* because the external effects work not 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

$$y_i = Ah_i^\nu,$$

where h_i is the human capital (schooling) of the worker, and A is aggregate productivity. So individual earnings are $W_i = Ah_i^\nu$.

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

$$A = BH^\delta \equiv B(E[h_i^\rho])^{\delta/\rho}, \tag{1}$$

where H is a measure of aggregate human capital, E is the expectation operator, B is a constant, and ρ determines how the human capital of different workers are aggregated into this measure. In Lucas's 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 $\rho \rightarrow \infty$. 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 in the production process. Individual earnings can be written as $W_i = Ah_i^\nu = BH^\delta h_i^\nu$. Therefore, taking logs, we have

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

If external effects are stronger within a geographical area, as seems likely in a world where human interaction and the exchange of ideas are the main forces behind the externalities, then equation (2) should be estimated using measures of H at the local level.

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 (1997a), 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 human capital, h_i, as given. 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

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

where $\alpha < 1$, $\nu \leq 1 - \alpha$. Since it is costly for the worker–firm pair to separate and find new partners in this economy, employment relationships generate quasi-rents. Wages will therefore be determined by rent sharing. Here, we simply assume that the worker receives a share β of the output, while the firm receives the remaining share, $1 - \beta$.

An equilibrium in this economy is a set of physical capital investments for firms. Firm f maximizes the expected profit function

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

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

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

where

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

is now the measure of aggregate human capital. Substituting (5) into (3), and using the fact that wages are equal to a fraction β of output, the wage income of individual i is given by $W_i = \beta\big((1-\beta)\alpha H/R\big)^{\alpha/(1-\alpha)} h_i^\nu$. 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 $\alpha/(1-\alpha)$ and ν are positive coefficients.[4]

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.[5]

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 $\delta = \alpha/(1-\alpha)$. A similar relationship also arises 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 of a role for H in individual wage determination weighs 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

4. As in Acemoglu (1996), human-capital externalities are additive in logs, so the marginal product of a more skilled worker increases when the average workforce skill level increases. Acemoglu (1998, 1999) discusses models in which log wage *differences* between skilled and unskilled workers increase with average skill levels.
5. In a frictionless world, firms maximize profits conditional on realized worker–firm matches instead of conditional on the expected match, and pay the full marginal product of the worker. In this case, firm j matched to worker i chooses capital $k_j = (\alpha h_i^\nu/r)^{1/(1-\alpha)}$, and worker i's wages is $\ln W_i = c' + [\nu\alpha/(1-\alpha)] \ln h_i$.

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

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

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

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 (1995).

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

$$\nu\theta_i[\eta(s_i) + s_i\eta'(s_i)] = \psi_i s_i, \tag{8a}$$

or

$$\eta'(s_i)\,(\epsilon_\eta^{-1} + 1) = \frac{\psi_i}{\nu\theta_i}, \tag{8b}$$

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 (1995). For example, if $\eta'(s_i) < 0$, those with high ψ_i get less schooling, and a marginal year of schooling is 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 the state average schooling $\bar{S}$.[6] Estimation can therefore be based on the following equation for individual i residing in state j:

6. In the pecuniary externality model, and in the nonpecuniary externalities model with $\rho = 1$, this approximation is natural. Specifically, we have $\ln H = \ln E[\exp(\nu\eta_i s_i)] \approx c_0 + c_1 E[\eta_i s_i] \approx c_2 + c_3 E[s_i]$. 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 reduces H, and with $\rho > 1$, greater variance increases H.

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

where $\bar{S}_{jt} = E_{jt}(s_i)$ is the 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 $\bar{S}_{jt}$ is correlated with average ability among workers in area 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 EFFECTS OF MIGRATION

Suppose that individuals choose to live in one of two states, indexed by $j = 1$ and 2, paying rent (user cost of housing) r_j in state j. Suppose also that i receives additional 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 his 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, assume that a random factor, v_j, also 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 $B = \beta[(1 - \beta)\alpha/R]^{\alpha/(1-\alpha)}$. 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 people 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 $F(\cdot)$, and exploiting the fact that ζ_i's are independent across individuals, housing markets clear when

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

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 encourages some (though not all) skilled workers to live in state 1. This is because increasing H_1 raises the wages of skilled workers by more than the wages of unskilled workers [recall that equations (2) and (6) are additive in logs]. Positive state-specific shocks to wages (i.e., $\Delta v > 0$) therefore attract more high-education workers to a state and raise average human capital via migration. This differential impact by schooling group generates positive correlation between average education and wages across states, potentially biasing OLS estimates of external returns.

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, it is differences in wages without cost-of-living adjustments that are relevant. Firm's pay (unadjusted) wages and, in equilibrium, receive the same return to physical capital in both states.[7] Thus, human-capital externalities are required if firms in the state with greater average education and higher wages are to be able to produce more and break even.

3. *Econometric Framework*

This section discusses instrumental-variables (IV) 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

$$Y_{ijt} = X_i'\mu + \delta_j + \delta_t + \gamma_1 \bar{S}_{jt} + \gamma_{2i} s_i + u_{jt} + \epsilon_i, \tag{12}$$

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 dummies, and δ_j and δ_t are state-of-residence and Census-

7. Firms producing nontraded goods may care only about local prices. But firms producing traded goods face the same prices and have to receive the same rate of return to physical capital. These firms must therefore have a more productive work force in high-wage states. Hence, as long as there are some firms producing traded goods in every state, average productivity has to be higher in states where wages are higher.
8. Brock and Durlauf (1999) survey non-IV approaches to estimating models with social effects.

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 at least two reasons for omitted-variables bias. First, economic growth may increase wages in a state, while also raising the demand for (or supply) of schooling. For example, state university systems often expand during cyclical upturns, and higher wealth levels typically increase investments in schooling. Alternatively, labor productivity and tastes for schooling in a state may change at the same time. These scenarios correspond to correlation between u_{jt} and the average cost of, or returns to, schooling in the theoretical model. To solve this problem, we construct instruments for $\bar{S}_{jt}$ 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 but are unlikely to be correlated with contemporaneous state-specific shocks, since they are derived from laws passed roughly 30 years before education and wages were recorded.[9]

In addition to generating exogenous variation in average education, the SOB-CSL instruments provide an attractive starting point because they are attached to individuals as opposed to states. We can therefore compare IV estimates of the individual returns to schooling using SOB CSLs with 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.[10]

A drawback of the SOB-CSL strategy is that it does not necessarily eliminate bias from state-specific wage shocks if there is substantial interstate migration in response. To see this, suppose that wages increase in, say, New York, and workers from out of state are attracted to New York. The model outlined above suggests more-educated workers may respond more to the pull of higher wages. Since more-educated workers are, on average, from states with more restrictive SOB CSLs, selective

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

10. A second reason we focus initially on the SOB-CSL instruments is that these instruments can be used without controlling for state of residence, a potentially endogenous variable due to migration.

migration by the more educated can cause these instruments to 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 current 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.

While omitted state–year effects are the primary motivation for these two IV strategies, the fact that one regressor, $\bar{S}_{jt}$, 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 $\bar{S}_j$, which for purposes of illustration is assumed to have constant coefficients and a cross-section dimension only:

$$Y_{ij} = \mu^* + \pi_0 s_i + \pi_1 \bar{S}_j + \xi_i, \qquad \text{where} \quad E[\xi_i S_i] = E[\xi_i \bar{S}_j] \equiv 0. \tag{13}$$

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 $\bar{S}_j$ 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.1 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 $\phi = 1/(1 - R^2) > 1$, and R^2 is 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 (averaging across all individuals within a state) corrects for attenuation bias due to measurement error in s_i, we 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 have $\rho_1 < \rho_0$ and the appearance of negative external returns.[11]

11. 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

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 IV strategy that treats both s_i and $\bar{S}_j$ 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. Appendix A.2 shows that if the instrument for individual schooling generates the same average return as would be generated using CSLs as instruments for individual schooling, the resulting IV estimates of social returns are consistent. Quarter-of-birth instruments, as in the work of Angrist and Krueger (1991), are therefore appropriate for individual schooling in our context because CSL and quarter-of-birth instruments both estimate individual returns for people whose schooling was affected by compulsory schooling laws. (In fact, we show below that, like quarter-of-birth instruments, CSLs changed the distribution of schooling primarily in the 8–12 range.)

4. *Data and OLS Estimates*

4.1 DATA SOURCES

The analysis begins with data for U.S.-born white males aged 40–49 from the 1960–1980 Censuses. 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. This reduces bias from age or experience effects when using quarter-of-birth dummies as instruments. Following the results using 1960–1980 data, we look at samples including data from the 1950 and 1990 Censuses. Because these censuses do not have 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 last set of results in the paper are for men aged 30–39. Men younger than 30 are excluded because many in this group have yet to finish school.[12]

The schooling variable for individuals in the 1950–1980 data is the

2SLS estimates of private returns to schooling using state dummies as instruments. Borjas (1992) discusses a similar problem affecting the estimation of ethnic-background effects.

12. Data are from the following IPUMS files (documented in Ruggles and Sobek, 1997): the 1% sample for 1960, Form 1 and Form 2 state samples for 1970 (giving a 2% sample), and the 5% 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% file. All regressions are weighted to population proportions. For additional information, see Appendix B.

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 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.[13]

The relevant labor market for the estimation of equation (12) is taken to be a state. Previous work on external returns in the United States has used cities, while macroeconomic studies of education and growth use 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 work with 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 our instruments 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 men aged 40–49 in all five censuses. The average age is constant across censuses, while average schooling increased by slightly less than a year between 1950 and 1960, and by slightly more than a year between 1960 and 1970, 1970 and 1980, and 1980 and 1990. 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 summarizes the extent of variation in average schooling 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 for men aged 40–49 from models with and without $\bar{S}_{jt}$, using

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

Table 1 DESCRIPTIVE STATISTICS

Variables	*1950*	*1960* (QOB Samples)	*1970* (QOB Samples)	*1980* (QOB Samples)	*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	609,852	626,511	729,695	16,659	72,344	161,029	376,479	103,184

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.

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. All standard errors reported in the paper are corrected for state–year clustering using the formula in Moulton (1986). Corrected standard errors are typically twice as large as uncorrected standard errors because of the group structure of some of the instruments and regressors.

OLS estimates of external returns for 1960–1980 imply that a one-year increase in state average schooling is associated with a 0.073 increase in the wages of all workers in that state. Using data from 1950–1980 generates an estimate of 0.061, whereas the 1950–1990 sample leads to an estimated external return of 0.072. These are similar to Moretti's (1999) estimates of external returns using within-city variation, which range from 0.08 to 0.13.[14] These OLS estimates of external returns are large, but substantially smaller than the external returns required to rationalize the relationship in Figure 1.

Interestingly, the external returns estimates from using single censuses are considerably larger than the estimates that control for state effects. This suggests that at least part of the relationship between average schooling and wages is due to omitted state characteristics. The remainder of the paper presents evidence on whether the association between state average schooling and wages reflects human-capital externalities.

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 3–6-year intervals from 1914 to 1965, with missing years interpolated by extending older data. For example, data for cohorts aged 14 in 1924–1928 come from a source for 1924. Sources for the CSLs are documented in Appendix B.

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

14. Rauch (1993) reports cross-section estimates around 0.05 using data from the 1980 Census. These estimates are not directly comparable with ours because Rauch's model includes occupation dummies and average experience.

Table 3 DESCRIPTION OF CHILD LABOR AND COMPULSORY SCHOOLING LAWS

Year at Age 14 (Census Year)	*Earliest Dropout 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 Appendix B for sources and method.

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 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 also fallen over time.

Margo and Finegan (1996) show that in the 1900s child labor laws were at least as important as attendance restrictions for educational attainment, and the evidence in Schmidt (1996) suggests the same for 1920–1935.[15] This is probably because 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 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 was 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 collapse the CSLs into two measures that are highly related to educational attainment both conceptually and empirically.

Over 95 percent in the sample of men aged 40–49 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:

CL6 for $CL \leq 6$,
CL7 for $CL = 7$,
CL8 for $CL = 8$,
CL9 for $CL \geq 9$.

Similarly, for CA, the dummies are:

15. Edwards (1978), Ehrenberg and Marcus (1982), Lang and Kropp (1986), and Angrist and Krueger (1991) also present evidence that compulsory schooling laws affected schooling.

CA8	for	CA ≤ 8,
CA9	for	CA = 9,
CA10	for	CA = 10,
CA11	for	CA ≥ 11.

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, viz. 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. Results for men aged 40–49 with SOB CSLs are shown in Tables 4 and 5. Results using SOR CSLs and/or men aged 30–39 are similar, and are omitted to save space.

Table 4 reports estimates from regressions of individual schooling on CL7–CL9 and CA9–CA11, along with controls for Census-year effects, year-of-birth effects, and state-of-birth effects. For example, the entry in column 1 shows that in the 1950–1980 sample, men born in states with a child labor law that required 9 years in school before allowing work ended up with 0.26 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.

The right half of Table 4 shows that adding 1950 Census data to the sample leads to CSL effects similar to or slightly smaller than those estimated in the 1960–1980 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 0.17.

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 2 and 3, which plot differences in the probability that educational attainment equals 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 1960–1980 sample, with men exposed to the least restrictive CSLs as the reference group.

Figure 2 shows that men exposed to more restrictive child labor laws

Table 4 THE EFFECT OF STATE-OF-BIRTH COMPULSORY SCHOOLING LAWS ON INDIVIDUAL SCHOOLING

	Including State-of-Residence Controls				*Without State-of-Residence Controls*			
	1960–1980 (1)	*1950–1980* (2)	*1950–1990* (3)	*1980* (4)	*1960–1980* (5)	*1950–1980* (6)	*1950–1990* (7)	*1980* (8)
(a) Child Labor Laws								
CL7	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)
CL8	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)
CL9	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)
(b) Compulsory Attendance Laws								
CA8	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)
CA9	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)
CA10	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	609,852	626,511	729,695	376,479	609,852	626,511	729,695	376,479

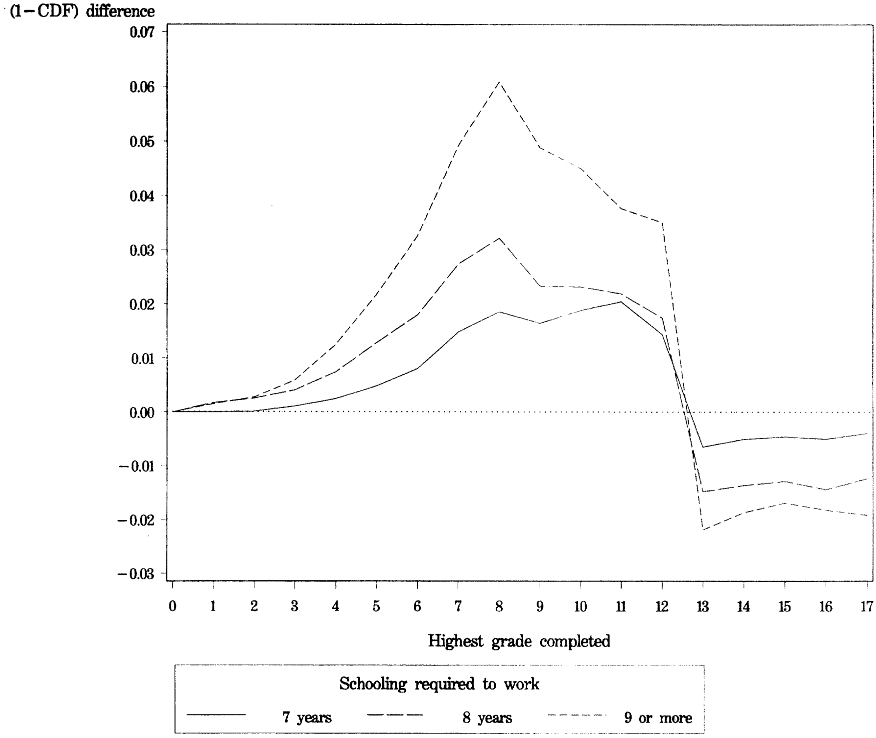
Notes: Standard errors corrected for state–year clustering are shown in parentheses. The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white males aged 40–49 in the Census year. All regressions contain Census-year, year-of-birth, and state-of-birth main effects.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

Table 5 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 (1)	*Completed 10 Years or Higher* (2)	*Completed 12 Years or Higher* (3)	*Completed 14 Years or Higher* (4)	*Completed 16 Years or Higher* (5)	*Completed 8 Years or Higher* (6)	*Completed 10 Years or Higher* (7)	*Completed 12 Years or Higher* (8)	*Completed 14 Years or Higher* (9)	*Completed 16 Years or Higher* (10)
Dependent-variable mean	0.908	0.747	0.617	0.249	0.167	0.884	0.695	0.562	0.226	0.151
(a) Child Labor Laws										
CL7	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)
CL8	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)
CL9	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)
(b) Compulsory Attendance Laws										
CA8	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)
CA9	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)
CA10	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 were 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.

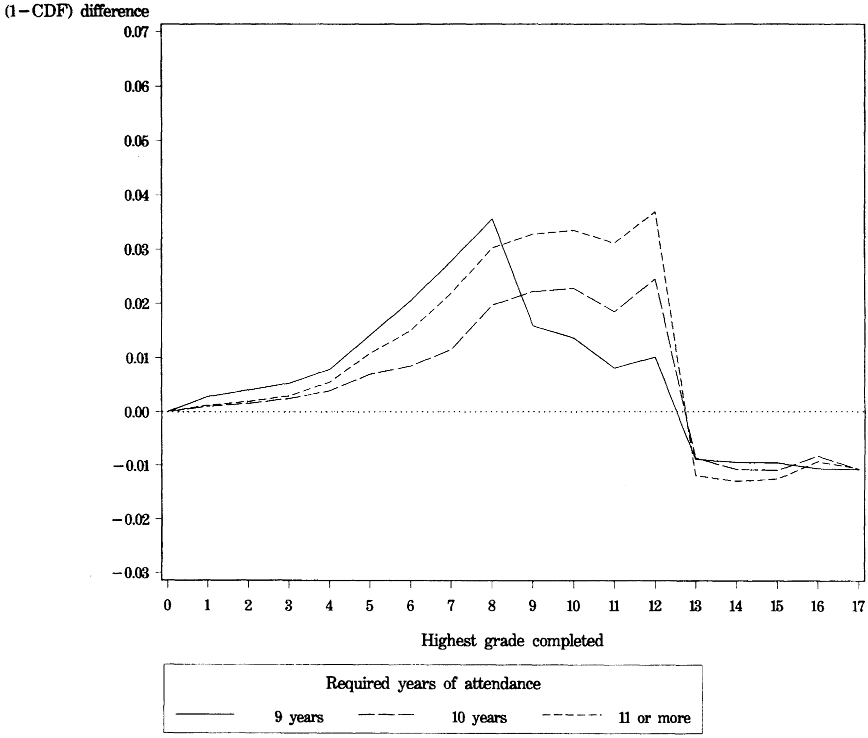
Figure 2 CDF DIFFERENCE BY SEVERITY OF CHILD LABOR LAWS



The figure shows the difference in the probability of schooling greater than or equal to the grade level on the X-axis. The reference group is 6 or fewer years of required schooling.

were 1–6 percentage points more likely to complete grades 8–12. For example, the top curve in Figure 2 shows that a person growing up in a state with the most restrictive child labor laws was about 6 percentage points more likely to have completed 8th grade than a person growing up with the least restrictive child labor laws. These differences decline at lower grades, and drop off sharply after grade 12. Figure 3 shows a similar pattern for compulsory attendance laws. These figures are encouraging in that they suggest that CSLs primarily shift the distribution of schooling in middle- and high-school grades. This is consistent with the notion that CSLs caused schooling changes, and not vice versa. Also, correlation between CSLs and omitted factors related to macroeconomic conditions, tastes for schooling, or family background would likely result in an association between more restrictive CSLs and

Figure 3 CDF DIFFERENCE BY SEVERITY OF COMPULSORY ATTENDANCE LAWS



The figure shows the difference in the probability of schooling at greater than or equal to the grade level on the X-axis. The reference group is 8 or fewer years of required schooling.

the proportion of the population attending college.[16] Therefore, Figures 2 and 3 suggest that CSLs are not correlated with omitted factors that affected schooling across the board.

Table 5 quantifies the CDF differences plotted in the figures for 1960–1980 and shows analogous results for the 1950–1980 sample. The table reports CSL coefficients in regressions of dummy variables for whether an individual completed the level of schooling indicated in the column heading. All of the positive estimates for grades 8–12 are statistically significant. The negative estimates at schooling levels above 12 are smaller and

16. 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.

Table 6 2SLS ESTIMATES OF PRIVATE RETURNS TO SCHOOLING

				CSL Instruments					
	QOB 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	0.090	0.063	0.076	0.103	0.113	0.092	0.099	0.081
	(0.012)	(0.016)	(0.017)	(0.034)	(0.038)	(0.018)	(0.044)	(0.052)	(0.023)
No state-of-residence main effects	0.073	0.088	0.063	0.080	0.112	0.126	0.101	0.094	0.100
	(0.012)	(0.016)	(0.017)	(0.064)	(0.060)	(0.027)	(0.088)	(0.086)	(0.040)
N	609,852	376,479	233,373	609,852	626,511	729,695	609,852	626,511	729,695

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 of 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.

less likely to be significant. The estimates also suggest that child labor laws shifted the distribution of schooling at younger grades more than compulsory attendance laws did. 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 Finegan's (1996) finding for the 1900s that child labor laws were more important for educational attainment than compulsory attendance laws.

For the most part, the CDF differences in the figures and in Table 5 are ordered by increasing severity, as would be expected if these differences reflect increasingly restrictive laws. For example, using 1960–1980 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.[17]

5.3 PRIVATE RETURNS TO EDUCATION

The CSL instruments 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 by correlation between the instruments and state average schooling. 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 6 reports two-stage least-squares (2SLS) estimates of the private returns to schooling using three different sets of instruments. Using 30 quarter-of-birth dummies (i.e., 3 quarter-of-birth dummies separately for each year of birth), the private return to schooling is estimated to be 0.073 (with a standard error of 0.012). 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.[18]

17. A final noteworthy feature of the figures is their similarity to CDF differences 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 Appendix A.2, IV estimates implicitly weight individual causal effects using CDF differences.

18. Bound, Jaeger, and Baker (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, reanalyses of

Estimates of private returns using CSL instruments in the 1960–1980 sample exceed those 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 0.076 (s.e. = 0.034). Using CA8–CA10 generates an estimate of 0.092 (s.e. =0.044), shown in column 7. Models estimated using CSL instruments without state-of-residence effects produce similar results. This last point is worth noting, since state of residence is a potentially endogenous variable.

The fact that quarter-of-birth and CSL instruments generate similar schooling coefficients in the 1960–1980 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. 2SLS estimates using quarter-of-birth instruments 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 0.073 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 1960–1980 sample.

6. *External Returns to Education*

6.1 RESULTS FOR 1960–1980

Table 7 reports estimates of external returns to education using data for 1960–1980. The bottom panel of Table 7 shows the first-stage relationship between SOB-CSL dummies and *average* schooling in 1960–1980

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these data by, among others, Chamberlain and Imbens (1996), Staiger and Stock (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.

data. These first-stage equations include year, year-of-birth, state-of-birth, and state-of-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 IV estimates reported in the top half of the table are from models that treat both s_i and $\bar{S}_{jt}$ as endogenous. Using quarter of birth and child labor laws as instruments generates a private return of 0.074 (s.e. = 0.012) and an external return of 0.003 (with s.e. = 0.040). This is considerably smaller, though less precise, than the corresponding OLS estimate of external returns. The 90% confidence interval for external returns, [−0.065, 0.066], excludes the OLS estimate of 0.073 (see Table 2). Using compulsory attendance laws as instruments generates somewhat higher external returns. These are not significantly different from the corresponding OLS estimates, but still considerably lower at 0.017 (s.e. = 0.043).

Using both sets of CSL dummies as instruments generates a more precisely estimated external return of 0.004 (s.e. = 0.035). The 90% confidence interval for this estimate is [−0.053, 0.061], 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 slightly more precise than estimates that do not use the interaction terms as instruments, showing external returns of 0.005 with standard error of 0.033.

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 7 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 7 report estimates from models that treat individual schooling as exogenous and drop the quarter-of-birth instruments. The resulting estimates of external returns again offer little evidence of external returns, and are virtually indistinguishable from those in columns 1–4, though slightly more precise. Since treating individual schooling as exogenous has little effect on the estimates, the results presented

Table 7 2SLS ESTIMATES OF PRIVATE AND EXTERNAL RETURNS TO SCHOOLING–STATE-OF-BIRTH INSTRUMENTS, 1960–1980 AND MEN AGED 40–49

	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 (0.012)	0.074 (0.012)	0.075 (0.012)	0.060 (0.013)	0.073 (0.0003)	0.073 (0.0003)	0.073 (0.0003)	0.073 (0.0003)
External return to schooling	0.003 (0.040)	0.017 (0.043)	0.004 (0.035)	0.005 (0.033)	0.002 (0.038)	0.018 (0.042)	0.006 (0.033)	−0.011 (0.030)

First-Stage Estimates for State–Year Average Schooling

CL7	0.080		0.062	0.084		0.062
	(0.028)		(0.025)	(0.028)		(0.025)
CL8	0.107		0.068	0.107		0.068
	(0.035)		(0.031)	(0.035)		0.031)
CL9	0.227		0.184	0.226		0.183
	(0.036)		(0.034)	(0.035)		(0.034)
CA9		0.128	0.102		0.128	0.104
		(0.026)	(0.023)		(0.026)	(0.030)
CA10		0.122	0.104		0.122	0.104
		(0.030)	(0.029)		(0.030)	(0.029)
CA11		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.

in the rest of the paper are from models where individual schooling is not instrumented.

Overall, the results in Table 7 suggest that the association between state average schooling and wages found in Table 2 is unlikely to be due to human-capital externalities alone. Furthermore, they indicate a total social return of around 8–9% (7% private return plus 1–2% external return). This is clearly too small to rationalize the steep relationship between average schooling and output per worker found in Figure 1.

6.2 ADDITIONAL ESTIMATES USING 1960–1980 DATA

Estimates of external returns using child labor laws as instruments (CL7–CL9) change little when the basic specification is modified. The first column of Table 8 shows the results of allowing the private return to schooling to vary by census year. Time-varying returns may be important, since the literature on wage inequality suggests the private returns to schooling have been changing (see, e.g., Katz and Murphy, 1992). Imposing a constant private return across years may lead to misleading estimates of external returns. In practice, allowing private returns to vary by year generates an estimated external return of 0.007 (s.e. = 0.036) with CL instruments, close to the baseline estimate in Table 7. Allowing private returns to vary by state as well as year generates a negative external return of −0.024 (s.e. = 0.039), reported in column 2. The corresponding estimates using compulsory attendance instruments, reported in the bottom panel of Table 8, are 0.021 and −0.018.

Many of the studies in Card's (1999) survey of research on the returns to schooling report IV estimates that exceed OLS estimates. To illustrate the consequences of a higher private return for estimates of external returns, Table 8 also shows estimated external returns from models imposing a private return of 0.08 or 0.09 (i.e., using $Y_{ijt} - 0.08s_i$ or $Y_{ijt} - 0.09s_i$ as the dependent variable). Not surprisingly, the estimated external returns in this case are even smaller than the baseline estimates in Table 7. With private returns of 9%, for example, the external return is estimated to be −0.018 (s.e. = 0.039) with SOB-CL instruments, and 0.010 (s.e. = 0.043) with SOB-CA instruments.

Columns 5–7 of Table 8 show external return estimates using SOR CSLs as instruments for state average schooling instead of SOB CSLs. These estimates are of interest in that, as noted in Section 3, they are less subject to bias from endogenous migration. Column 5 reports estimates corresponding to those in Table 7, while columns 6 and 7 are for models allowing private returns to vary by year and by state and year. The CL estimates are larger using SOR CSLs, while the CA estimates are smaller. The differences are not large enough, however, to suggest significant bias due to migration when using SOB-CSL instruments.[19]

6.3 ADDING 1950 AND 1990 DATA

Individual schooling must be 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, though in practice the estimates of external returns for 1960–1980 are not sensitive to the exogeneity assumption. A second and potentially more serious problem is that schooling is a categorical variable in the 1990 Census, different from the earlier highest-grade-completed measure. We must therefore use an imputed years-of-schooling measure for 1990.

Table 9 reports estimates of external returns in the extended samples (still for men aged 40–49). Using child labor laws as instruments generates small positive or zero estimates of external returns with 1950–1980 data. These estimates are more precise than those using 1960–1980 data only. In column 1, for example, the estimated external return is 0.009 with a standard error of 0.025. As before, using compulsory attendance laws as instruments leads to somewhat 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 these CSLs and schooling. For example, in column 1, CA9 has a larger coefficient than either CA10 or CA11.

In contrast with the results using 1950–1980 data, adding data from the 1990 Census leads to statistically significant positive estimates of external returns when child labor laws are used as instruments. Column 2 shows an external return of 0.048 with a standard error of 0.02. Allowing separate private returns by census year leads to an even larger external return of 0.074 with the CL instruments. In contrast, CA instruments do not generate significant estimates of external returns in the 1950–1990 sample. Results using SOR CSLs in the expanded samples are reported in Table 10. These show small and insignificant external returns in the 1950–1980 sample, but—as in Table 9—some of the estimates using CL instruments in the 1950–1990 sample are positive and significant.

The relatively large and significant external return estimates using CL instruments in 1950–1990 data may signal a change in the external value

19. Another possible source of bias in the estimates in Tables 7 and 8 is changing school quality. But school quality is associated with higher average wages, so omission of these variables cannot 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 (1992b) leads to more negative estimates, though also less precise, than reported in Table 7.

Table 8 2SLS ESTIMATES OF EXTERNAL RETURNS TO SCHOOLING: ADDITIONAL RESULTS FOR MEN AGED 40–49

	With State-of-Birth Instruments				*With State-of-Residence Instruments*		
	Private Returns Separate by Census (1)	*Private Returns Separate by Census and State* (2)	*Private Returns =0.08* (3)	*Private Returns =0.09* (4)	*Baseline Estimates* (5)	*Private Returns Separate by Census* (6)	*Private Returns Separate by Census and State* (7)
(a) Results Using Child Labor Laws as Instruments							
External return to schooling	0.007 (0.039)	−0.024 (0.039)	−0.006 (0.038)	−0.018 (0.039)	0.048 (0.033)	0.051 (0.034)	0.015 (0.033)
First Stage for State–Year Average Schooling							
CL7	0.083 (0.028)	0.080 (0.026)	0.084 (0.028)	0.084 (0.029)	0.124 (0.046)	0.123 (0.046)	0.120 (0.043)
CL8	0.104 (0.034)	0.100 (0.031)	0.107 (0.035)	0.107 (0.035)	0.158 (0.053)	0.157 (0.052)	0.154 (0.049)
CL9	0.223 (0.035)	0.210 (0.032)	0.227 (0.036)	0.227 (0.035)	0.402 (0.063)	0.399 (0.062)	0.390 (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							
CA9	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)
CA11	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 9 2SLS ESTIMATES: ADDITIONAL SAMPLES WITH STATE-OF-BIRTH INSTRUMENTS FOR MEN AGED 40–49

	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						
CL7	0.173 (0.024)	0.165 (0.019)	0.170 (0.023)	0.162 (0.019)	0.158 (0.020)	0.145 (0.016)
CL8	0.126 (0.036)	0.144 (0.027)	0.123 (0.035)	0.139 (0.027)	0.113 (0.031)	0.121 (0.022)
CL9	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						
CA9	0.133 (0.028)	0.172 (0.019)	0.130 (0.027)	0.168 (0.019)	0.118 (0.023)	0.143 (0.015)
CA10	0.106 (0.037)	0.167 (0.028)	0.105 (0.036)	0.164 (0.027)	0.096 (0.031)	0.139 (0.022)
CA11	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	626,510	729,695	626,510	729,695	626,510	729,695

Notes: Standard errors corrected for state–year clustering are reported in parentheses. Estimates of external 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 10 2SLS ESTIMATES: ADDITIONAL SAMPLES WITH STATE-OF-RESIDENCE INSTRUMENTS FOR MEN AGED 40–49

	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	0.016 (0.028)	0.044 (0.022)	0.024 (0.028)	0.054 (0.022)	−0.007 (0.026)	0.016 (0.023)
First Stage for State–Year Average Schooling						
CL7	0.215 (0.035)	0.185 (0.031)	0.213 (0.035)	0.183 (0.031)	0.202 (0.031)	0.174 (0.027)
CL8	0.142 (0.054)	0.128 (0.045)	0.142 (0.054)	0.127 (0.044)	0.134 (0.049)	0.116 (0.039)
CL9	0.430 (0.068)	0.452 (0.048)	0.426 (0.067)	0.449 (0.047)	0.401 (0.061)	0.409 (0.043)
(b) Results Using Compulsory Attendance Laws as Instruments						
External return	0.007 (0.045)	−0.0004 (0.032)	0.014 (0.046)	0.020 (0.031)	−0.017 (0.043)	−0.029 (0.033)
First Stage for State–Year Schooling						
CA9	0.192 (0.043)	0.247 (0.030)	0.190 (0.042)	0.244 (0.030)	0.177 (0.038)	0.218 (0.026)
CA10	0.147 (0.075)	0.198 (0.056)	0.145 (0.074)	0.195 (0.056)	0.137 (0.067)	0.171 (0.049)
CA11	0.145 (0.063)	0.254 (0.046)	0.143 (0.063)	0.251 (0.045)	0.136 (0.057)	0.229 (0.040)
(c) OLS Estimates						
External return	0.061 (0.010)	0.072 (0.008)	0.076 (0.010)	0.094 (0.007)	0.038 (0.009)	0.057 (0.007)
N	626,511	729,625	626,511	729,625	626,511	729,625

Notes: Standard errors corrected for state–year clustering are reported in parentheses. Estimates of external 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.

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 effect of individual schooling is poorly controlled. Measurement error in the 1990 schooling variable could generate a problem of this type.[20]

To check whether measurement problems could be responsible for the 1950–1990 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 reestimated external returns in 1960–1980, 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 0.024 instead of the estimate of 0.003 reported in Table 7. Similarly, using CA instruments generates an external return of 0.034 instead of 0.017 with the better-measured schooling variable. This suggests that the higher external returns estimated with 1990 data are due to changes in the education variable in 1990.

6.4 RESULTS FOR MEN AGED 30–39

The last set of results is for men in their 30s. Since this group has a steep age–earnings profile, quarter of birth is confounded with age effects (Angrist and Krueger, 1991). Individual schooling is therefore treated as exogenous in this younger sample. With individual schooling exogenous, 1950 Census data can be included. 1990 data are omitted, however, because of the problems discussed above.

Columns 1–3 of Table 11 reports results for men aged 30–39 in 1950–1980, while results for a larger sample pooling men aged 30–49 appear in columns 4–6. The top panel shows results using CL instruments, while the bottom panel is for CA instruments (coded as SOB CSLs). The first-stage relationships are also reported in the table. They show significant effects of CSLs on the average schooling of men aged 30–39, very similar to those for men aged 40–49 reported in the bottom panel of Table 7. The baseline estimate using CL instruments in the younger sample, reported in column 1, is close to 0, with a standard error of 0.023. CA instruments

20. Note, however, that the measurement error in the 1990 schooling variable is not classical. Kane, Rouse, and Staiger (1999) discuss the implications of nonclassical measurement error for IV estimates. A detailed description of the schooling variables used here appears in Appendix B.

21. This exercise uses the IPUMS variable EDUCREC, which provides a uniform categorical schooling measure for the 1940–1990 Censuses.

Table 11 2SLS ESTIMATES OF EXTERNAL RETURNS TO SCHOOLING, 1950–1980

	Aged 30–39			*Aged 30–49*		
	Baseline Results (1)	*Separate Private Returns by Census (2)*	*Separate Private Returns by Census and State (3)*	*Baseline Results (4)*	*Separate Private Returns by Census (5)*	*Separate Private Returns by Census and State (6)*
(a) Results Using Child Labor Laws as Instruments						
External return	0.002 (0.023)	0.028 (0.022)	−0.018 (0.022)	0.011 (0.020)	0.030 (0.020)	−0.007 (0.023)
First Stage for State–Year Average Schooling						
CL7	0.070 (0.030)	0.069 (0.029)	0.067 (0.024)	0.128 (0.023)	0.125 (0.022)	0.116 (0.019)
CL8	0.137 (0.037)	0.133 (0.037)	0.123 (0.030)	0.136 (0.032)	0.132 (0.032)	0.121 (0.040)
CL9	0.284 (0.037)	0.278 (0.037)	0.254 (0.031)	0.285 (0.030)	0.279 (0.030)	0.252 (0.024)
(b) Results Using Compulsory Attendance Laws as Instruments						
External return	−0.006 (0.028)	0.017 (0.027)	−0.030 (0.026)	0.022 (0.027)	0.041 (0.027)	−0.006 (0.030)
First Stage for State–Year Schooling						
CA9	0.202 (0.027)	0.198 (0.027)	0.180 (0.022)	0.162 (0.025)	0.158 (0.024)	0.142 (0.027)
CA10	0.156 (0.032)	0.153 (0.032)	0.137 (0.026)	0.127 (0.029)	0.125 (0.028)	0.111 (0.020)
CA11	0.230 (0.039)	0.225 (0.039)	0.205 (0.032)	0.161 (0.035)	0.157 (0.034)	0.142 (0.040)
(c) OLS Estimates						
External return	0.081 (0.009)	0.095 (0.009)	0.054 (0.009)	0.071 (0.009)	0.087 (0.009)	0.048 (0.009)
N	812,864	812,864	812,864	1,439,375	1,439,375	1,439,375

Notes: The table reports results for men aged 30–39 and a pooled sample of men aged 30–49. Standard errors corrected for state–year clustering are reported in parentheses. Estimates of external returns to schooling use dummies for child labor and compulsory attendance laws as excluded instruments. Individual schooling is treated as exogenous. All regressions contain individual-schooling, Census-year, year-of-birth, state-of-birth, and state-of-residence main effects as well as a quartic function of potential experience. Compulsory schooling laws are assigned according to the laws in effect in the individual's state of birth when he was 14.

generate a less precisely estimated external return of −0.006. Estimates that allow private returns to vary by year are larger, but those from models allowing private return to vary by state and year are negative. Pooling age groups leads to similar estimates. Overall, the results for men aged 30–39 are consistent with the results for men in their 40s, showing no evidence of significant external returns. Once again, the estimated confidence intervals exclude returns above 5–6% percent.

7. *Concluding Remarks*

The returns to education are important 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%. However, private returns may be only part of the story. With positive external returns to education, private returns underestimate the economic value of schooling. On the other hand, if education plays a major signaling role, the total economic value of schooling may be less than suggested by private returns.

This paper exploits potentially exogenous variation in average schooling caused by changes in compulsory schooling laws in U.S. states. Census data from 1960–1980 generate statistically insignificant external-return estimates around 1% (mostly ranging from −1% to 3%). 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% 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 likely due to changes in the schooling variable in the 1990 Census.

On balance, the analysis here offers little evidence for sizable external returns to education, at least over the range of variation induced by changing CSLs. Moreover, while some of the estimates are positive, they are nowhere near large enough to rationalize the cross-country association between average education and average income documented in Figure 1 or even the cross-state (OLS) association documented in Table 2.

Some final caveats are in order. First, the standard errors associated with the estimates reported here lead to confidence intervals that include external returns of, say, 1–3%. External returns of this magnitude are sufficient to justify significant public subsidies for education. Second, our strategy identifies local effects, missing external returns that raise wages nationwide. Finally, our estimates are driven by changes in secondary schooling and not changes in higher education. Weak external returns to secondary school do not rule out the possibility of external returns to schooling at higher levels.

Appendix A. Mathematical Details

A.1 DERIVATION OF EQUATION (14)

Rewrite equation (13) as follows:

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

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

$$\rho_1 = \pi_0 + \pi_1,$$

$$\pi_0 = \frac{C(\tau_i, Y_{ij})}{V(\tau_i)}.$$

Simplifying the second line,

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

where $\phi \equiv V(s_i)/[V(s_i) - V(\bar{S}_j)]$. 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

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

What value of γ_2^* allows us to use z_i as an instrument for $\bar{S}_j$ 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:

$$\begin{aligned} \gamma_1^{\text{IV}} &= \frac{E[\tilde{Y}_{ij} \mid z_i = 1] - E[\tilde{Y}_{ij} \mid z_i = 0]}{E[\bar{S}_j \mid z_i = 1] - E[\bar{S}_j \mid z_i = 0]} \\ &= \gamma_1 + \left(\frac{E[\gamma_{2i} s_i \mid z_i = 1] - E[\gamma_{2i} s_i \mid z_i = 0]}{E[s_i \mid z_i = 1] - E[s_i \mid z_i = 0]} - \gamma_2^* \right) \\ &\quad \left(\frac{E[s_i \mid z_i = 1] - E[s_i \mid z_i = 0]}{E[\bar{S}_j \mid z_i = 1] - E[\bar{S}_j \mid z_i = 0]} \right). \end{aligned}$$

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

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

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 $\bar{S}_j$, we have

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

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 $\bar{S}_j$ 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

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

which is an average derivative with weighting function $P[s_{1i} \geq \sigma > s_{0i}] = P[s_i \leq \sigma \mid z_i = 0] - P[s_i \leq \sigma \mid z_i = 1]$. 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 $P[s_i \leq \sigma \mid z_i = 0] - P[s_i \leq \sigma \mid z_i = 1]$ and $P[s_i \leq \sigma \mid q_i = 0] - P[s_i \leq \sigma \mid q_i = 1]$ should be similar. In fact, Figure 2 shows 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

B.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:

22. In practice, we have more than one CSL instrument, so it may be possible to use CSLs to instrument s_i and $\bar{S}_{jt}$ simultaneously. Note, however, that because of the group structure of $\bar{S}_{jt}$ and the CSL instruments, the projection of s_i on the CSL instruments is almost identical to the projection of $\bar{S}_{jt}$ on the CSL instruments. This is not a problem with quarter-of-birth instruments, since they are independent of $\bar{S}_{jt}$.
23. These potential schooling choices can be described in terms of the theoretical framework. Suppose, for example, that $\eta(s_i) = \bar{\eta}$ and the CSL instrument changes discount rates from ψ_{0i} or ψ_{1i} as in Card (1995). Using (8), individual schooling choices would be $s_{0i} = \nu\theta_i\bar{\eta}/\psi_{0i}$ and $s_{1i} = \nu\theta_i\bar{\eta}/\psi_{1i}$.

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 U.S.-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 1950–1980, 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 questionnaire from the February 1990 CPS to construct averages for essentially the same Census categories. This generates a years of schooling variable roughly 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 1960–1980 data are limited to men born 1910–1919 in the 1960 Census, 1920–1929 in the 1970 Census, and 1930–1939 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.

B.2 CALCULATION OF AVERAGE SCHOOLING

Average schooling is the mean of GRADCAP by state and census year for all U.S.-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.

B.3 MATCH TO CSLs AND STATE AVERAGE SCHOOLING

The CSLs in force in each year from 1914 to 1972 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.

B.4 CSL VARIABLES

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

B.4.1 Sources The sources are collected in Table 12, in which

enroll_age is the maximum age by which a child has to enroll at school,
drop_age is the minimum age a child is allowed to drop out of school,
req_sch is the minimum years of schooling a child has to obtain before dropping out,
work_age is the minimum age at which a child can get a 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 (Section B.5).

B.4.2 Methods Data were drawn from the sources listed in Table 12. 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

Table 12 SOURCES OF CSL DATA

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	SCLS-1949	SCLS-1949	SCLS-1949	SCLS-1949
	Keesecker-1950	Keesecker-1950	Keesecker-1950	Keesecker-1950	Keesecker-1950
1954	Keesecker-1955	Keesecker-1955	Keesecker-1955	M	Keesecker-1955
1959	SCLS-1960	SLCS-1960	SLCS-1960	SLCS-1960	SLCS-1960
	Umbeck	Umbeck	Umbeck		Umbeck
1965	SLCS-1965	SLCS-1965	SCLS-1965	SCLS-1965	SLCS-1965
	Steinhilber	Steinhilber	Steinhilber	LLS	Steinhilber

source changes. In the table, M denotes missing, i.e., we found no source or reliable information for that variable in that year. Missing data were imputed by bringing older data forward. Intersource years were imputed and the data set expanded by bringing older data forward to make a complete set of five CSL laws for each year from 1914 to 1965.

The imputed data set contains either numerical entries or 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 grade or age requirements 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).

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Comment

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1. *Introduction*

Daron Acemoglu and Joshua Angrist attack the important and difficult problem of measuring external returns from an individual's schooling investment. As the authors discuss, much of the work that stresses human capital in growth relies on such externalities, as private returns to schooling are not nearly large enough to justify the claims of importance