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

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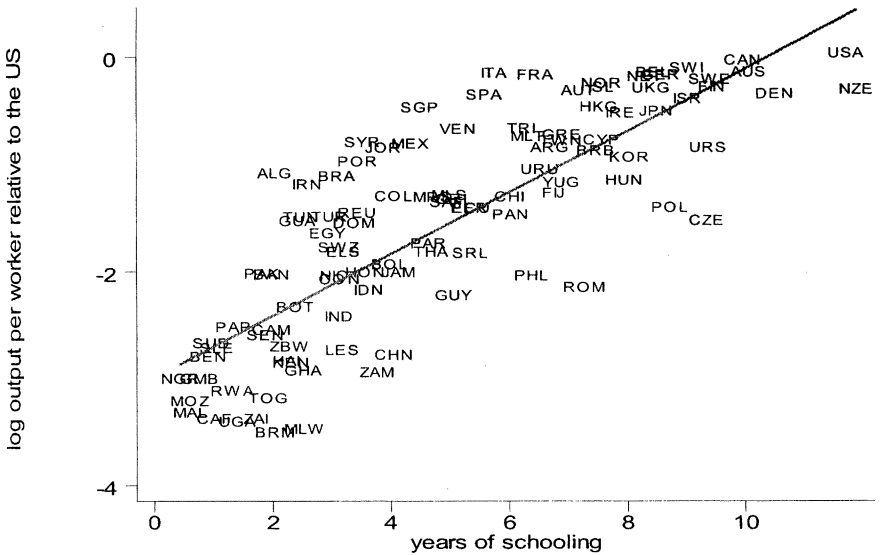
## *1. Introduction*

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%.<sup>1</sup>

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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.<sup>2</sup>

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% ( $e^{0.29} - 1 \approx 0.34$ ). 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.<sup>3</sup> 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 externalities 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

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

where  $H$  is a measure of aggregate human capital,  $E$  is the expectation operator,  $B$  is a constant, and  $\rho$  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  $\rho$ , the parameter  $\delta$  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

$$\ln W_i = \ln B + \delta \ln H + \nu \ln h_i. \quad (2)$$

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  $\beta$  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

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

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  $\beta$  of output, the wage income of individual  $i$  is given by  $W_i = \beta((1-\beta)\alpha H/R)^{\alpha/(1-\alpha)} h_i^\nu$ . Taking logs, this is

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

where  $c$  is a constant and  $\alpha/(1-\alpha)$  and  $\nu$  are positive coefficients.<sup>4</sup>

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.<sup>5</sup>

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,  $\theta_i$ , 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  $\psi_i$  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  $\epsilon_\eta$  is the elasticity of the function  $\eta$ . 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  $\psi_i$  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  $\bar{S}$ .<sup>6</sup> 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  $\ln H = \ln E[\exp(\nu \eta s_i)] \approx c_0 + c_1 E[\eta s_i] \approx c_2 + c_3 E[s_i]$ . 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  $\eta_i$  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  $H$ .

$$\ln W_{jt} \approx \gamma_0 + \gamma_1 \bar{S}_{jt} + \gamma_2 \eta_j s_i + u_{jt} \quad (9)$$

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  $\gamma_1$ . 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,  $\zeta_i$ , from living in state 1 instead of state 2, where  $\zeta_i$  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  $B = \beta[(1 - \beta)\alpha/R]^{1/(1-\alpha)}$ ). 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

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

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  $\zeta$  greater than  $\zeta(h, \Delta v, \Delta r)$  would prefer to live in state 1 when the rent differential is  $\Delta r$ . Denoting the distribution of human capital by  $F(\cdot)$ , and exploiting the fact that  $\zeta_i$ 's are independent across individuals, housing markets clear when

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

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. Firms pay (unadjusted) wages and, in equilibrium, receive the same return to physical capital in both states.<sup>7</sup> 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.<sup>8</sup> 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

$$Y_{ijt} = X_{ijt}\mu + \delta_j + \delta_t + \gamma_1 \bar{S}_{jt} + \gamma_2 s_{jt} + u_{jt} + \epsilon_i \tag{12}$$

where  $Y_{ijt}$  is the log weekly wage,  $u_{jt}$  is a state-year error component, and  $\epsilon_i$  is an individual error term. The vector  $X_{ijt}$  includes state-of-birth and year-of-birth dummies, and  $\delta_j$  and  $\delta_t$  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  $\gamma_{2i} \equiv \gamma_2 \eta_{it}$ , while the coefficient on average schooling,  $\gamma_1$ , 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.<sup>9</sup>

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.<sup>10</sup>

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,  $s_{it}$ , also complicates the interpretation of OLS estimates. To see this, consider an “atheoretical” regression of  $Y_{ijt}$  on both  $s_{it}$  and  $\bar{S}_{jt}$ , which for purposes of illustration is assumed to have constant coefficients and a cross-section dimension only:

$$Y_{ijt} = \mu^* + \pi_0 s_{it} + \pi_1 \bar{S}_{jt} + \xi_{ijt}, \quad \text{where } E[\xi_{ijt} s_{it}] = E[\xi_{ijt} \bar{S}_{jt}] = 0. \quad (13)$$

Now, let  $\rho_0$  denote the coefficient from a bivariate regression of  $Y_{ijt}$  on  $s_{it}$  only, and let  $\rho_1$  denote the coefficient from a bivariate regression of  $Y_{ijt}$  on  $\bar{S}_{jt}$  only. Note that  $\rho_1$  is the two-stage least squares (2SLS) estimate of the coefficient on  $s_{it}$  in a bivariate regression of  $Y_{ijt}$  on  $s_{it}$  using a full set of state dummies as instruments. Appendix A.1 shows that

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

where  $\phi = 1/(1 - R^2) > 1$ , and  $R^2$  is from a regression of  $s_{it}$  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  $s_{it}$ , 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  $s_{it}$  and unobserved earnings potential, we have  $\rho_1 < \rho_0$  and the appearance of negative external returns.<sup>11</sup>

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.<sup>12</sup>

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

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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 top-code 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.<sup>13</sup>

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

<i>Variables</i>	<i>QOB Samples</i>				1990
	1950	1960	1970	1980	
<i>Covariates</i>					
Age	44.16 (2.87)	44.55 (2.88)	44.74 (2.90)	44.66 (2.94)	44.10 (2.84)
Individual education	9.67 (3.40)	10.52 (3.22)	11.59 (3.18)	12.62 (2.98)	13.70 (2.49)
<i>Regressors</i>					
State average education	9.94 (0.72)	10.65 (0.54)	11.52 (0.41)	12.46 (0.30)	13.10 (0.23)
Lowest state average education	7.87 [MS]	9.24 [MS]	10.45 [SC]	11.81 [KY]	12.62 [AR]
Highest state average education	11.18 [UT]	11.80 [UT]	12.38 [UT]	13.07 [DC]	13.74 [DC]
<i>Dependent Variable</i>					
Log weekly wage	4.06 (0.77)	4.64 (0.63)	5.17 (0.65)	5.90 (0.72)	6.44 (0.73)
<i>Instruments</i>					
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
<i>N</i>	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)
<i>(a) Private Returns</i>								
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
<i>(b) Private and External Returns</i>								
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.<sup>14</sup> 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

<i>Year at Age 14 (Census Year)</i>	<i>Earliest Dropout Age (1)</i>	<i>Latest Enrollment Age (2)</i>	<i>Minimum Schooling for Dropout (3)</i>	<i>Earliest Work Age (4)</i>	<i>Required Schooling for Work Permit (5)</i>
1914 (50)	15.31 (1.20)	7.49 (0.52)	1.90 (3.40)	11.00 (5.75)	1.70 (2.56)
1917 (50)	15.55 (0.89)	7.63 (0.49)	1.93 (2.74)	13.43 (1.98)	2.98 (2.66)
1921 (50)	15.69 (0.99)	7.42 (0.51)	4.28 (3.63)	13.94 (1.71)	4.19 (2.97)
1924 (60)	15.88 (0.97)	7.29 (0.57)	5.64 (3.64)	14.11 (1.33)	4.91 (3.04)
1929 (60)	15.97 (0.93)	7.30 (0.58)	5.66 (3.62)	14.16 (1.33)	5.31 (3.01)
1935 (70)	15.96 (0.94)	7.24 (0.55)	7.24 (3.73)	14.14 (0.76)	6.02 (2.67)
1939 (70)	16.16 (1.05)	7.16 (0.51)	7.29 (3.74)	14.15 (0.77)	6.01 (2.70)
1946 (80)	16.31 (0.63)	7.09 (0.53)	7.91 (4.00)	14.77 (1.16)	4.67 (3.37)
1950 (80)	16.27 (0.60)	7.08 (0.53)	7.94 (4.49)	15.03 (1.14)	3.51 (3.47)
1954 (80)	16.30 (0.63)	7.05 (0.52)	7.79 (4.65)	15.02 (1.20)	4.06 (3.67)
1959 (90)	16.25 (0.60)	7.05 (0.53)	7.40 (4.79)	15.19 (1.19)	3.49 (3.56)
1964 (90)	16.20 (0.60)	7.05 (0.54)	7.44 (4.79)	15.17 (1.22)	3.51 (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.<sup>15</sup> 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:

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

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:

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

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:

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

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.

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CA8	for	CA $\leq$ 8,
CA9	for	CA = 9,
CA10	for	CA = 10,
CA11	for	CA $\geq$ 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)
<i>(a) Child Labor Laws</i>								
CL7	0.095 (0.030)	0.117 (0.024)	0.173 (0.021)	0.050 (0.041)	0.105 (0.077)	0.115 (0.051)	0.175 (0.043)	0.062 (0.041)
CL8	0.124 (0.034)	0.130 (0.032)	0.213 (0.026)	0.132 (0.034)	0.120 (0.093)	0.119 (0.075)	0.202 (0.059)	0.143 (0.034)
CL9	0.259 (0.039)	0.220 (0.038)	0.398 (0.028)	0.167 (0.041)	0.269 (0.098)	0.225 (0.084)	0.410 (0.059)	0.182 (0.041)
<i>(b) Compulsory Attendance Laws</i>								
CA8	0.117 (0.027)	0.083 (0.025)	0.189 (0.020)	-0.011 (0.034)	0.103 (0.072)	0.068 (0.057)	0.171 (0.043)	-0.009 (0.034)
CA9	0.095 (0.034)	0.059 (0.036)	0.113 (0.020)	0.100 (0.044)	0.106 (0.085)	0.074 (0.077)	0.133 (0.063)	0.104 (0.045)
CA10	0.167 (0.038)	0.144 (0.036)	0.260 (0.028)	0.115 (0.037)	0.184 (0.103)	0.165 (0.085)	0.290 (0.063)	0.119 (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.

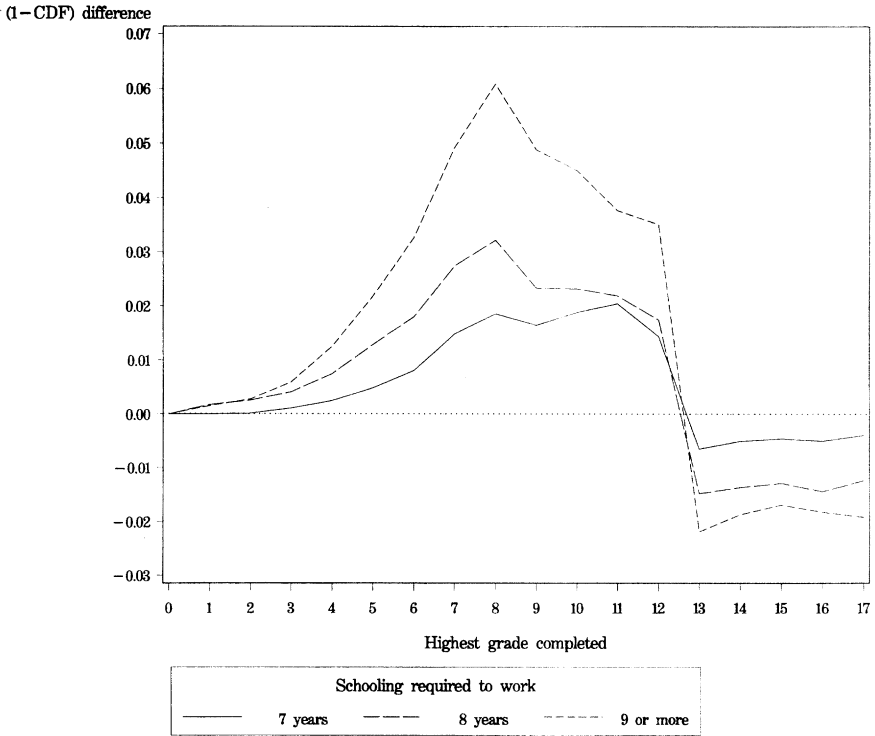
Table 5 THE EFFECT OF STATE-OF-BIRTH COMPULSORY SCHOOLING LAWS ON DISCRETE LEVELS OF SCHOOLING

Dependent- variable mean	Results for 1950-1980									
	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)
	0.908	0.747	0.617	0.249	0.167	0.884	0.695	0.562	0.226	0.151
<i>(a) Child Labor Laws</i>										
CL7	0.019 (0.004)	0.019 (0.005)	0.014 (0.005)	-0.005 (0.006)	-0.005 (0.004)	0.031 (0.004)	0.014 (0.004)	0.009 (0.003)	-0.004 (0.004)	-0.004 (0.003)
CL8	0.032 (0.005)	0.023 (0.005)	0.018 (0.005)	-0.014 (0.007)	-0.014 (0.046)	0.033 (0.005)	0.019 (0.005)	0.016 (0.005)	-0.009 (0.006)	-0.010 (0.003)
CL9	0.061 (0.005)	0.045 (0.006)	0.035 (0.006)	-0.019 (0.007)	-0.018 (0.052)	0.065 (0.005)	0.034 (0.006)	0.024 (0.006)	-0.021 (0.007)	-0.007 (0.004)
<i>(b) Compulsory Attendance Laws</i>										
CA8	0.036 (0.004)	0.014 (0.004)	0.010 (0.004)	-0.009 (0.005)	-0.011 (0.004)	0.032 (0.004)	0.010 (0.004)	0.006 (0.004)	-0.010 (0.005)	-0.010 (0.003)
CA9	0.020 (0.004)	0.023 (0.005)	0.025 (0.005)	-0.011 (0.006)	-0.008 (0.005)	0.016 (0.005)	0.022 (0.005)	0.022 (0.005)	-0.011 (0.006)	-0.009 (0.005)
CA10	0.030 (0.005)	0.034 (0.006)	0.037 (0.006)	-0.013 (0.007)	-0.009 (0.005)	0.022 (0.005)	0.032 (0.006)	0.032 (0.005)	-0.010 (0.007)	-0.005 (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.



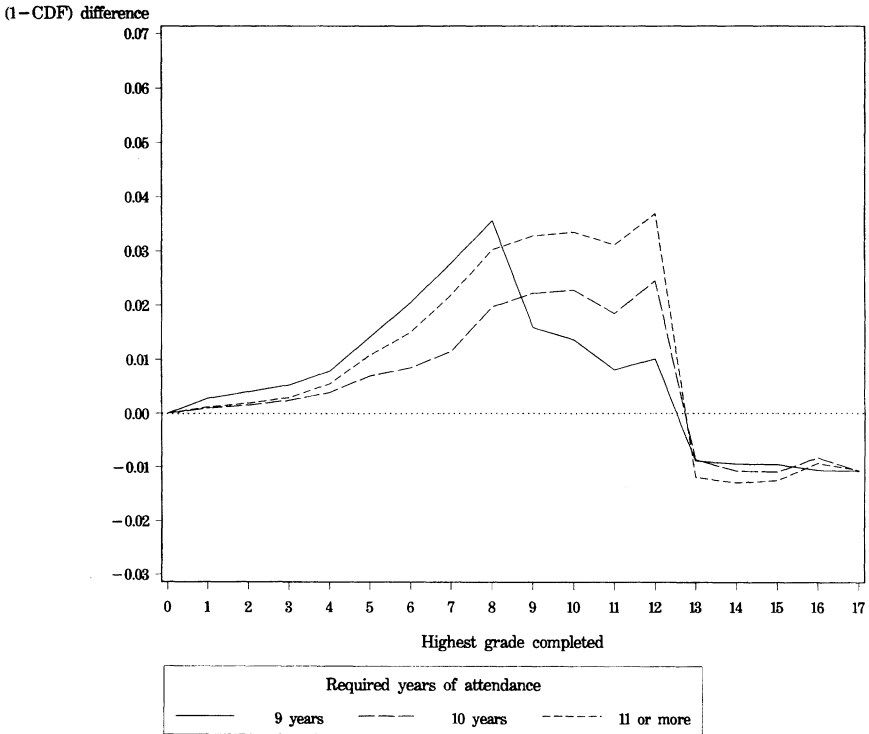
Figure 2 CDF DIFFERENCE BY SEVERITY OF CHILD LABOR LAWS



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.<sup>16</sup> 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 (1)	1980 (2)	1960–1970 (3)	1960–1980 (4)	1950–1980 (5)	1950–1990 (6)	1960–1980 (7)	1950–1980 (8)	1950–1990 (9)	
Including state-of-residence main effects	0.073 (0.012)	0.090 (0.016)	0.063 (0.017)	0.076 (0.034)	0.103 (0.038)	0.113 (0.018)	0.092 (0.044)	0.099 (0.052)	0.081 (0.023)	
No state-of-residence main effects	0.073 (0.012)	0.088 (0.016)	0.063 (0.017)	0.080 (0.064)	0.112 (0.060)	0.126 (0.027)	0.101 (0.088)	0.094 (0.086)	0.100 (0.040)	
N	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.<sup>17</sup>

### 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.<sup>18</sup>

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

	<i>Individual Schooling Endogenous</i>				<i>Individual Schooling Exogenous</i>			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Second-Stage Estimates</i>								
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 (0.028)	0.062 (0.025)	0.084 (0.028)	0.062 (0.025)
CL8	0.107 (0.035)	0.068 (0.031)	0.107 (0.035)	0.068 (0.031)
CL9	0.227 (0.036)	0.184 (0.034)	0.226 (0.035)	0.183 (0.034)
CA9	0.128 (0.026)	0.102 (0.023)	0.128 (0.026)	0.104 (0.030)
CA10	0.122 (0.030)	0.104 (0.029)	0.122 (0.030)	0.104 (0.029)
CA11	0.144 (0.038)	0.094 (0.036)	0.143 (0.038)	0.094 (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.<sup>19</sup>

### 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)
<i>(a) Results Using Child Labor Laws as Instruments</i>							
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)
<i>First Stage for State-Year Average Schooling</i>							
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      -0.018      0.021      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

	<i>Separate Private Returns</i>					
	<i>Baseline Results</i>		<i>By Census</i>		<i>By Census and State</i>	
	50–80 (1)	50–90 (2)	50–80 (3)	50–90 (4)	50–80 (5)	50–90 (6)
<i>(a) Results Using Child Labor Laws as Instruments</i>						
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)
<i>First Stage for State–Year Average Schooling</i>						
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)
<i>(b) Results Using Compulsory Attendance Laws as Instruments</i>						
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)
<i>First Stage for State–Year Average Schooling</i>						
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)
<i>(c) OLS Estimates</i>						
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

	<i>Baseline Results</i>		<i>Separate Private Returns by Census</i>		<i>Separate Private Returns by Census and State</i>	
	<i>50–80</i>	<i>50–90</i>	<i>50–80</i>	<i>50–90</i>	<i>50–80</i>	<i>50–90</i>
	(1)	(2)	(3)	(4)	(5)	(6)
<i>(a) Results Using Child Labor Laws as Instruments</i>						
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)
<i>First Stage for State–Year Average Schooling</i>						
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)
<i>(b) Results Using Compulsory Attendance Laws as Instruments</i>						
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)
<i>First Stage for State–Year Schooling</i>						
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)
<i>(c) OLS Estimates</i>						
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.<sup>20</sup>

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.<sup>21</sup> 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)
<i>(a) Results Using Child Labor Laws as Instruments</i>						
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)
<i>First Stage for State-Year Average Schooling</i>						
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)
<i>(b) Results Using Compulsory Attendance Laws as Instruments</i>						
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)
<i>First Stage for State-Year Schooling</i>						
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)
<i>(c) OLS Estimates</i>						
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 re-

turns 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:

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

where  $\tau_i \equiv s_i - \bar{S}_j$ . Since  $\tau_i$  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,

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

where  $\phi \equiv V(s_i)/[V(s_i) - V(\bar{S}_j)]$ . Solving for  $\pi_1$ , 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  $\gamma_1$ , 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  $\gamma_2^*$  is some average of  $\gamma_{2i}$ . 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  $\gamma_2^*$  allows us to use  $z_i$  as an instrument for  $\bar{S}_j$  in (15) to obtain a consistent estimate of  $\gamma_1$ ? The instrumental variables estimand in this case,  $\gamma_1^{IV}$ , is given by the Wald formula:

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

This shows that  $\gamma_1^{IV}$  estimates external returns to education consistently (i.e., equals  $\gamma_1$ ) if the adjustment for individual schooling uses the coefficient

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

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

Of course, we cannot use  $z_i$  to estimate both private and external returns, even though (16) appears to require this. But instruments based on quarter of birth can be used to estimate  $\gamma_2^*$ . 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

$$\gamma_2^* = \frac{E[Y_{ij} | q_i = 1] - E[Y_{ij} | q_i = 0]}{E[s_i | z_i = 1] - E[s_i | z_i = 0]} = \frac{E[\gamma_{2i} s_i | q_i = 1] - E[\gamma_{2i} s_i | q_i = 0]}{E[s_i | q_i = 1] - E[s_i | q_i = 0]}.$$

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

To see why  $\gamma_q^*$  should be close to  $\gamma_2^*$ , 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  $z_i = 0$ .<sup>23</sup> Angrist, Graddy, and Imbens (1995) show that

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

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

CSL instruments and quarter-of-birth instruments both estimate individual returns for people whose schooling is affected by compulsory schooling laws—i.e., individuals who would have otherwise dropped out of school. So the weighting functions  $P[s_i \leq \sigma | z_i = 0] - P[s_i \leq \sigma | z_i = 1]$  and  $P[s_i \leq \sigma | q_i = 0] - P[s_i \leq \sigma | q_i = 1]$  should be similar. In fact, Figure 2 shows that, like quarter-of-birth instruments, CSLs changed the distribution of schooling primarily in the 8–12 range. This suggests that  $\gamma_q^*$  and  $\gamma_2^*$  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  $\psi_{0i}$  or  $\psi_{1i}$  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

<i>Year</i>	<i>enroll_age</i>	<i>drop_age</i>	<i>req_sch</i>	<i>work_age</i>	<i>work_sch</i>
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;	Deffenbaugh;	Deffenbaugh;	Deffenbaugh;
		Schmidt	Schmidt	Schmidt	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	SCLS-1960	SCLS-1960	SCLS-1960	SCLS-1960
	Umbeck	Umbeck	Umbeck	Umbeck	Umbeck
1965	SCLS-1965	SCLS-1965	SCLS-1965	SCLS-1965	SCLS-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  $\text{work\_sch} = 6$ , but a work permit available at any age, so  $\text{work\_age} = \text{NR}$ ). 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  $\text{req\_sch} = \text{NR}$ , then  $\text{req\_sch} = 0$ ;

If  $\text{enroll\_age} = \text{NR}$  or  $\text{drop\_age} = \text{NR}$ , then  $\text{CA} = \max(0, \text{req\_sch})$ ;

If  $\text{enroll\_age} \neq \text{NR}$  and  $\text{drop\_age} \neq \text{NR}$  then  $\text{CA} = \max(\text{drop\_age} - \text{enroll\_age}, \text{req\_sch})$ .

If  $\text{work\_age} = \text{NR}$ , then  $\text{work\_age} = 0$ ;

If  $\text{work\_sch} = \text{NR}$ , then  $\text{work\_sch} = 0$ ;

If  $\text{enroll\_age} = \text{NR}$  then  $\text{CL} = \max(0, \text{work\_sch})$ ;

If  $\text{enroll\_age} \neq \text{NR}$  then  $\text{CL} = \max(\text{work\_age} - \text{enroll\_age}, \text{work\_sch})$ .

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

made for schooling. Education externalities also play a prominent role in the literatures on city formation and neighborhood effects, and, more generally, in discussions of income inequality. With the exception of leisure, education is no doubt the good most heavily subsidized by the government. Heckman and Klenow (1997) calculate that about 30% of the costs of an individual's schooling, at the margin, is absorbed by other persons' budgets through government subsidies. These policies are often rationalized on the basis of important external effects from increased schooling and school spending.

More exactly, Acemoglu and Angrist use an instrumental variables (IV) approach to examine the relationship

$$\ln w_i = \gamma_1 \bar{S} + \gamma_2 s_i, \quad (1)$$

where  $w_i$  and  $s_i$  refer to the wage and schooling for person  $i$ , and  $\bar{S}$  denotes average schooling for a broader group, whose schooling may have an external impact on person  $i$ .<sup>1</sup> For Acemoglu and Angrist, this group is persons living in the same U.S. state as person  $i$ .

I first discuss Acemoglu and Angrist's model interpretation of the parameter  $\gamma_1$  in equation (1) as capturing externalities from human capital. I then discuss why an OLS estimate of  $\gamma_1$  in equation (1) is problematic and briefly discuss the authors' IV approach. I then attempt to gauge the potential magnitude of  $\gamma_1$  on the basis of growth accounting.

## 2. *Interpreting the External Return to Schooling*

Acemoglu and Angrist discuss two distinct rationales for a positive  $\gamma_1$  in equation (1), that is, a positive effect of other persons' schooling on an individual's earnings. The first follows literature on cities (e.g., Rauch, 1993), growth (e.g., Lucas, 1988), and neighborhood effects (e.g., Borjas, 1995) by assuming that the human capital of others acts as a complementary input to your own labor through the exchange of ideas, making you more productive and increasing your wage. More novel, the authors consider a model of search that also leads to a causal increase in your earnings from operating in an economy with greater average human capital.

In the empirical work, the authors make equation (1) operational by measuring the broader group's human capital by schooling of other persons in the state of residence. Particularly for models based on externalities in production, it is not clear if the state of residence is the relevant economy. Ideas can probably be exchanged across state lines nearly as

1. This is a simplified depiction of Acemoglu and Angrist's more explicit equation (9).

easily as within. Suppose that the externality from human capital operates by increasing the adoption of technology because it effectively spreads the fixed costs of invention and innovation across a greater number of skilled workers who will make use of the technology. Provided the innovation can diffuse across states, the externality from human capital will not project on the level of schooling in a person's state of residence. This criticism is particularly relevant given that Acemoglu and Angrist's IV estimates do not suggest externalities at the state level.

Their model of search externalities from human capital can be briefly described as follows. A pool of workers and a pool of firms look to form matches. A worker brings his human capital  $h$  and a firm its physical capital  $k$  to a prospective match. If a match occurs, output equal to  $k^{\alpha}h^{1-\alpha}$  is produced. The key is that, regardless of how much capital the firm provides or how much human capital the worker possesses, this output is split with a fraction  $\beta$  going to the worker and a fraction  $1 - \beta$  going to the firm. This environment yields three implications:

1. There is underinvestment in both physical and human capital.
2. Because human and physical capital are complementary in production, firms invest in greater physical capital if they anticipate matching with a pool of workers with greater human capital.
3. Directly related to the second result, a worker's wage is increasing in the human capital of other workers in their search pool, as well as their own. This last result clearly rationalizes relating a worker's wage to other workers' schooling, as in equation (1).

The critical assumption in this story of search externalities is that the pie is split independently of how much each side brings to the match. This will not be the case, for instance, if there is directed search as in Acemoglu and Shimer (1999). More precisely, if a worker by building human capital and a resumé can gain access to job opportunities, then, given that firms can choose what set of workers to consider, a worker should be able to achieve higher earnings commensurate with the marginal product of the acquired human capital. This seems like a good description of the labor market, at least in developed economies. Potential employers ask for resumes, conduct interviews, call references, etc. Furthermore, job listings often quote a salary range, with starting pay depending on the education, experience, and other relevant characteristics of the applicant.

Acemoglu and Angrist's description of match externalities brings to mind another arena in which matching is important. Consider marriages that form between two persons. For convenience, I will refer to the two

persons as husband and wife. Suppose that the husband and wife share their household income independently of how much the husband produces and how much the wife produces—for simplicity, say equally. Then consumption for both husband and wife equal  $(w_{\text{husb}} + w_{\text{wife}})/2 = [f(s_{\text{husb}}) + f(s_{\text{wife}})]/2$ , where  $s_i$  is the schooling of member  $i$ , and  $f(s_i)$  is the earnings for a member with that schooling. Suppose schooling is determined prior to forming marriages. If matches form independently of a person's earnings potential and if students, in choosing when to leave school, do not show altruism to their future (unknown) spouse, then this model generates an important externality to schooling. As in Acemoglu and Angrist's setting, persons underinvest in schooling because they only internalize half of the gain in future earnings. In contrast to their setting, this externality does not show up in a wage equation such as equation (1).

But at least one of the assumptions above, that marriages form independently of a person's schooling, appears at odds with the evidence. Based on 22,102 households that were respondents in the 1980 to 1994 Consumer Expenditure Surveys, I projected years of schooling for wives on the schooling of their husbands, and vice versa. The results, with standard errors in parentheses, are

$$s_{\text{wife}} = 0.58s_{\text{husb}}, \quad s_{\text{husb}} = 0.70s_{\text{wife}}.$$

(0.005)                      (0.006)

So conditional on the husband having one more year of schooling, we should expect the household to have 1.58 more years; and conditional on the wife having one more year of schooling, we should expect the household to have 1.70 more years. Matching in marriage looks very directed. Thus individuals are able to obtain a total return on their schooling that is much of the total household gain. This should provide persons with incentive to obtain schooling, even if they are not concerned with providing for their future spouse. So even here, match externalities to schooling may not be very important.

### 3. *The OLS Relationship*

Estimating equation (1) by OLS, Acemoglu and Angrist find that one more year of schooling for a worker is associated with 7.3% higher earnings, with a standard error of less than 0.1%. (Here I focus on results for the sample period of 1960–1980.) But, more striking, one year of schooling of others in the worker's state, holding the worker's schooling constant, is also associated with 7.3% higher earnings, with a stan-

dard error of 1.6%. This estimate is comparable to, though slightly smaller than, estimates in Moretti (1999).

The authors are concerned that OLS estimates may provide an upward bias of the external effects of others schooling on a worker's earnings. I would like to expand on their discussion of this problem in Section 2.4 of their paper. They describe an economy with free migration. The externality from schooling drives up the price of the scarce resource, land, in areas with more schooling. The resulting higher cost of living in areas with more average schooling means that, even though there is an important productivity gain from living there, the market clears with no gains for additional workers to migrate to areas with more schooling. This is a standard view on the role of land pricing in allocating persons across locations in the presence of a productive or consumptive public good at a location (e.g., Roback, 1982).

For the present issue of measuring externalities from schooling, the concern is that any factor that results in higher productivity in location  $X$  will not only result in higher wages and higher costs of living at  $X$ , but will also attract workers with more schooling and greater unmeasured ability. The market equilibrium will tend to concentrate human capital on the most valuable land. As a result, the OLS relation showing higher earnings for those living in areas where others have more schooling need not reflect any structural external benefit of schooling. Instead, the results may show only that areas that are productive, with higher wages and higher living costs, attract workers with more schooling and greater unmeasured skills.

The notion that areas with higher levels of schooling also display higher housing prices is supported in the data. For 45 states I was able to relate the 1995 CPI for housing for the state's most populous city to average schooling level in the state. The housing cost is from the ACCRA Cost of Living Index; the state schooling levels were provided to me by Joshua Angrist.  $\bar{S}$  denotes average schooling for all male workers in the state;  $\bar{S}_{40-50}$  denotes average schooling for men aged 40 to 50. The relationship as estimated by OLS, with standard errors in parentheses, is

$$\begin{aligned} \ln(\text{housing CPI}) &= 0.10 \bar{S} && \text{or, alternatively,} \\ & (0.003) \\ &= 0.15 \bar{S}_{40-50} \\ & (0.003) \end{aligned}$$

Housing makes up about a third of the cost of living. So, allowing for the higher price of housing alone eats up about half of the wage gain, as measured by OLS, to moving to a state with more schooling.



There is also evidence that persons who live in states with more average schooling display above-average human capital, even if they themselves do not have above-average schooling. That is, more schooling in a state is correlated with higher unmeasured abilities. In 1979 the National Longitudinal Survey of Youth (NLSY) gave its respondents the Armed Forces Quantitative Test (AFQT). The AFQT scores are understood to reflect acquired knowledge as well as innate intelligence. For 5629 respondents I can project their AFQT on their own schooling as well as average schooling in their state or residence. The result is

$$\text{AFQT}_i = 5.82 s_i + 4.34 \bar{S}.$$

(0.18)      (0.44)

So a person's test score projects nearly as much on the average schooling in one's state as on their own schooling.

This suggests that an OLS estimate of the key parameter  $\gamma_1$  in equation (1) will be biased upward, as the projection of wages on  $\bar{S}$  may reflect the higher unmeasured ability, as reflected in the AFQT score, rather than an externality. For workers in the NLSY data set, 4.34 extra points of AFQT score is associated with 2.0% higher earnings (with standard error of 0.2%), controlling for individual schooling. This would explain about one-third of the OLS estimate of  $\gamma_1$ .

Another interpretation is that growing up in a state with more average schooling is what actually causes AFQT to be higher in those states after controlling for an individual's level of schooling. Thus this again would point to an important externality from schooling, though occurring through learning as opposed to production or search as in Acemoglu and Angrist's interpretation.

#### 4. Discussion of Acemoglu and Angrist's IV Results

Given concerns that an OLS estimate of the schooling externality is upward biased, Acemoglu and Angrist pursue an IV estimator based on state regulations that restrict young persons' ability to drop out of school or work before a certain age. They document that more stringent restrictions in a state are clearly associated with more years of high-school attendance. Looking at their Table 7, columns (4) and (8), upon instrumenting for schooling, they continue to find a private return to a year of schooling of about 7%. But now they find an external effect from state-wide schooling roughly equal to zero, with a standard error of about 3%.

I see their work as very valuable. As discussed above and in their introduction, an assumption of positive human-capital externalities plays an

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important role in economic theorizing as well as in public policies. Yet, empirically the question is very open. The presumptive positive externalities are supported by a small body of empirical work (e.g., Rauch, 1993; Moretti, 1999) in which causality is difficult to decipher. Secondly, I see their IV estimator as a natural attack on the problem. I believe it should move one's prior quite clearly toward a fairly small or no external effect of schooling, unless, as in my case, that result is already close to your prior.

At the same time, I would note a few limitations. First of all, the standard error associated with their IV point estimate is sizable at about 3%. Thus a 95% confidence interval would include an external return to schooling of 6%, which is smaller, but of the same order of magnitude as, the OLS estimate. A 90% confidence interval includes an external return to schooling of 3%.

Secondly, their experiment in raising years of schooling is very specific. The increased years in schooling generated by their instruments are associated with keeping boys in high school who would prefer to leave. It may be that external benefits from such enforced schooling are smaller than could be garnered by encouraging college attendance through tuition subsidies. On the other hand, the relevant sample in this paper are boys who were in high school in approximately the years 1930 to 1950. Choosing to drop out of high school during that period was much more common than it is today.

Finally, there may be important externalities from schooling not reflected in higher wage rates for others. My example above of the return to schooling benefiting a future spouse is a possible example. It is often argued that increased education makes citizens better voters, though I could never follow the reasoning. A cursory reading of Dickens's *Oliver Twist* suggests the external benefits in lower crime from keeping young men in a monitored setting such as a school or a prison. Related to this, Lochner (1999) calculates that the social benefits from reduced crime associated with men graduating from high school are at least \$7000 (1996 dollars), and perhaps considerably more.

### *5. Limiting the Magnitude of Schooling Externality by Means of Growth Accounting*

Given the difficulty in constructing arguably valid instruments to estimate the return to schooling, and especially the external return to schooling, it is worthwhile attacking the problem from other directions as well. I consider one direction based on examining the growth-accounting implications of schooling externalities. I will argue that externalities of the size estimated by OLS in the authors' Table 2 are implausibly large.

Given the rapid rise in schooling levels in the United States and worldwide, externalities of that magnitude would constitute an unreasonable fraction of measured growth in total factor productivity (TFP) in recent decades.

Annualized growth rates for TFP for the United States for 1950 to 1997, and for subperiods, are given in the first row of the following table:

	1950–1997	1950–1970	1970–1997
$g_{TFP}$	1.13%	1.78%	0.65%
Schooling	8.9→13.4	8.9→10.7	10.7→13.4
Adj. $g_{TFP}$ ( $\gamma_1 = 0.073$ )	0.66%	1.34%	0.16%

The TFP numbers come from the U.S. Bureau of Labor Statistics. (They are available at Web site [stats.bls.gov/mprhome.htm](http://stats.bls.gov/mprhome.htm).) These growth rates in TFP already account for the increased schooling in the workforce and changes in workforce composition in terms of experience and gender, as discussed by Jorgenson (1995). The adjustment for increased schooling reflects a private return to schooling, as estimated in wage regressions of the Mincer (1974) type. But it does not consider an external return to schooling above the Mincerian return. TFP growth averages a little more than 1.1% per year for 1950 to 1997. As is well known, TFP growth has been much slower in the latter portion of the postwar period, averaging 0.65% per year for 1970 to 1997.

In the second row of the above table I report the growth rate in average years of schooling in the working-age population. (I calculate this for earlier years from historical statistics derived from the Current Population Surveys. 1997 values are available at the BLS Web site cited above.) Schooling attainment has grown rapidly, by about a year of attainment per decade. This growth has not subsided.

In the third and final row of the table, I adjust the Commerce Department's measure of TFP growth for an external return to schooling equal to 7.3% higher labor input for each additional year of schooling in the national workforce. The externality of 7.3% reflects the OLS estimate of  $\gamma_1$  from the authors' Table 2. I use a labor share of two-thirds to convert the effect of the externality on effective labor input to an effect on GDP. For 1950 to 1997 this reduces residual TFP growth from 1.13% to 0.66% per year. More striking is the period from 1970 to 1997. Adjusting TFP for the externality from schooling reduces it nearly to zero, viz., to 0.16% per year. Another way to state this is that more than three-fourths of TFP growth for 1970 to 1997 is attributable to the external benefits of rising

schooling. This strikes me as implausible. It leaves almost no room for a contribution of new approaches, new technologies, and new types of equipment to productivity growth over the past 30 years.<sup>2</sup>

Similar, and in fact stronger, statements apply if we look across a broader set of countries. Using the Mincerian approach to relate human capital to years of schooling and experience, as discussed in Bils and Klenow (2000), I calculated average annual TFP growth for 89 countries for the years 1960 to 1990. All specifications require that the private returns to schooling and experience be consistent with empirical estimates of Mincer equations across more than 50 countries. The specifications differ in whether the Mincerian return decreases in years of schooling. Depending on this choice, the growth rate of TFP, across the 89 countries, averages from 0.10% to 0.40% per year. Over the same 30 years, schooling attainment (based on Barro and Lee, 1996) grew by an average of 2.1 years, or 0.07 schooling years per year. Externalities from schooling consistent with the OLS estimate,  $\gamma_1 = 0.073$ , by themselves would yield a rate of growth in TFP of 0.34% per year. This falls very high in the range of total TFP growth of 0.10% to 0.40% per year, leaving no room for improvements in ideas and adoption of technologies as a source of worldwide growth from 1960 to 1990.

The upshot, I would argue, is that growth accounting suggests externalities from schooling that are no more than a fraction of the OLS estimate of  $\gamma_1$ . On the other hand, this type of exercise is certainly unable to rule out some smaller external effect of schooling.

I believe readers should take away from Acemoglu and Angrist's paper that external benefits as large as private returns are very unlikely. Furthermore, external benefits greater than about 40% of the private benefit (an external return of 3% on earnings for each extra year of aggregate schooling) are fairly unlikely. I would draw similar conclusions from these growth accounting exercises.

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2. An important caveat here is that if there has been very considerable unmeasured growth in the economy (as suggested by the Boskin Commission, 1996), then the TFP growth rates in the table are also understated. This would allow more room for a contribution from technological growth.

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## Comment

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One of the few areas on which many economists agree is that market failures justify government intervention, particularly when it comes to education. For example, in *Capitalism and Freedom*, Milton Friedman (1982 ed., pp. 85–86; original 1962) writes, “. . . government intervention into education can be rationalized on . . . the existence of substantial ‘neighborhood effects,’ i.e., circumstances under which the action of one individual imposes significant costs on other individuals for which it is not feasible to make him compensate them, or yields significant gains to other individuals for which it is not feasible to make them compensate him—circumstances that make voluntary exchange impossible.” Based on the belief, the subsidization (and provision in the case of K–12 education) of education is a major focus of government at all levels. Indeed, in 1997 direct expenditures on education by state and local governments accounted for over 7% of GDP.

And yet, because these neighborhood effects, or externalities, can be difficult to measure, we have precious little direct evidence that the

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social return to education does, indeed, differ from the private return. Previous authors have attempted to measure the social return to education by studying the effect of education on other outcomes such as crime and welfare dependence, but these studies are few and far between because of the difficulty of obtaining credible information on both education and the outcome of interest (which can vary considerably over a lifetime). In addition, it is difficult to design an analytic approach that credibly generates a consistent estimate of the causal effect of education on the outcome.

Another approach is to interpret the coefficient on an aggregate measure of schooling in a regression of individual wages on individual schooling and aggregate schooling as an estimate of the externalities to schooling. The interpretation is that, conditional on one's own schooling, if one earns more as the educational level of one's "neighbors" increases, it is because the others' education is generating positive externalities for the individual in question. This is the approach followed in this ambitious paper by Acemoglu and Angrist. Other papers using this approach have found large and statistically significant effects of aggregate schooling on one's own schooling, suggesting positive externalities to education.

However, this paper goes further than most of the previous literature. The authors attempt to address head-on the concern that aggregate schooling may be spuriously positively correlated with wages because economic growth may increase both wages and the supply of, or demand for, schooling, or because positive area-specific shocks may attract more "able" individuals to the geographic area under consideration (a U.S. state, in this paper). Therefore, they instrument for aggregate schooling, using (1) compulsory schooling laws in effect in the state in which the individual grew up during the time the individual was young and (2) compulsory schooling laws in effect in the state in which the individual currently resides during the time the individual was young. Identification comes from changes within states in compulsory schooling laws, the identifying assumption being that the legal changes are independent of (the residual of) the wages of the individual 30 years later.

As with most of Angrist's other work, this is a clever empirical strategy. A key question for identification is why and when states change their compulsory schooling laws. For example, are they changed in response to economic conditions? If so, do changes occur during times of economic prosperity or during economic downturns? I can imagine it going either way. On the one hand, during times of economic prosperity residents may be wealthier and, through an income effect, willing to increase the compulsory schooling age. On the other hand, perhaps

residents are willing to increase the compulsory schooling age during times of economic downturns when the opportunity cost of attending school is lower. And yet it will matter to the interpretation of the authors' results which "story" is more plausible. If one believes that the laws are changed during economic upswings (which may lead to other changes that affect future wages, such as an improvement in school quality), then the authors' estimates, as small as they are, may actually overstate schooling externalities. Conversely, if the laws are changed during downturns (and there are other changes that occur during that time that affect future wages), then the authors' estimates may understate education externalities. Either way, it would be useful to know more about the political economy of the decision to change compulsory schooling laws.

I would also like to have a better understanding of how the instruments work. The figures that the authors provide are quite useful, but I do not understand why the instruments have a negative (but not always significant) effect on completion of postsecondary education (as measured by 14 and 16 years or more of schooling). While most of the estimates are not statistically significant, they are consistently negative. A typical (naive?) sorting model would suggest that on an increase in the schooling of individuals at the lower end of the distribution, others would complete more schooling in order to differentiate themselves. However, the authors find that those not likely affected by the laws get *less* schooling. One possible explanation is that when states strengthen compulsory schooling laws, increased expenditures for secondary schooling are required. If the states do not increase their total expenditures on all education (including postsecondary education), it is possible that funds are shifted from postsecondary schooling to secondary schooling to pay for the increased numbers of students. This hypothesis may have implications for future wages that complicate their identification strategy.

So, what do Acemoglu and Angrist find? When the authors estimate their equation by OLS, they estimate private returns to schooling of about 7%, a tad lower than what most researchers estimate today, but in the same ballpark. They also estimate external returns to schooling of roughly the same magnitude (once state-of-residence effects have been included). With IV, the private returns to schooling do not change much, but the coefficient on the external returns falls to 1–2%.

Why might these results differ from those found by others? One explanation is that the estimates of the external return to schooling in the other papers are biased upward by omitted variables. Another is that the authors' empirical strategy identifies the social return to secondary education. In contrast, previous papers may identify the social return to

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other levels of education. For example, in a recent paper Enrico Moretti (1999) identifies social returns to postsecondary education that are positive and statistically significant. One can reconcile the results by concluding that there are minimal social returns to secondary education but large social returns to post-secondary education. A third explanation is that the other papers use a different level of aggregation in order to identify the external return to schooling. For example, many of the previous papers use the average level of schooling in a metropolitan area; Acemoglu and Angrist use the state. And yet, perhaps the metropolitan area is conceptually more appropriate, since it is closer to the level at which workers can meet and exchange ideas regularly. Similarly, it is not clear whether, conceptually, it is the schooling of *all* workers in the state (or metropolitan area) that makes a difference, or the schooling of individuals who would interact with the individual in production, such as those in the same industry or occupation. Given the results from the previous literature and the presumption in the field that externalities in education are present, it is important to understand why Acemoglu and Angrist estimate such small external returns.

Finally, suppose these results represent the truth. What do they imply for economic theory and/or public policy? On the one hand, they may be extremely important. For, even though the estimates are imprecise, Acemoglu and Angrist's results imply that the bulk of the return to secondary education is a private return. And yet, as I mentioned at the beginning, most economists and policymakers justify public subsidies to, and perhaps even provision of, elementary and secondary education by potential positive externalities. As some evidence of the commitment, state and local direct expenditures on K-12 education outpace those on higher education by almost 4 : 1. Should this public commitment to K-12 (or at least secondary) education be reconsidered?

On the other hand, the results may not be so important for policy today. One reason is that the sample includes only white men aged 40-49 from 1950 to 1980. Today's policymaking regarding high-school dropouts is focused on low-income youths and African-American and Hispanic youth. These coefficients may not apply to them. Most importantly, the approach followed by the authors captures a relatively narrow form of externality. From a policy perspective there are many others that may be equally or more important, such as the effects on tax revenues, government transfers, and criminal activity. As a reminder, recall the earlier evaluation of the federal Job Corps program, a training program targeted at low-income youths. In this evaluation, the increased earnings of participants only accounted for about one-half of the total benefits of the program. The other one-half was accounted for by reduced criminal activity



and reduced reliance on transfer programs. On net, as a result, there was a social benefit of the program once allowing for costs. This illustration is simply a reminder that when we consider externalities to schooling they come in many forms.

In all, I enjoyed the paper and commend the authors for attempting to tackle an extremely difficult and yet extremely important issue in economics.

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- Friedman, M. (1982, originally 1962). *Capitalism and Freedom*. Chicago: The University of Chicago Press.
- Moretti, E. (1999). Estimating the social return to education: Evidence from repeated cross-sectional and longitudinal data. Berkeley: University of California, Center for Labor Economics. Working Paper 22.

### *Discussion*

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In responding to Cecilia Rouse's comments, Daron Acemoglu said he was also puzzled by the finding that the instruments are negatively correlated with postsecondary educational attainment (that is, tougher compulsory K-12 attendance laws in a state are negatively correlated with subsequent college attendance). He agreed that investigating political-economy explanations for this correlation would be interesting, but was skeptical that these could imply significant biases in the estimates. Acemoglu also acknowledged the potential importance of directed search, as pointed out by discussant Mark Bills.

Joshua Angrist concurred with the discussants that the imprecision of the estimates unavoidably reduced the sharpness of their conclusions. He also agreed with Rouse that externalities might be stronger at the city than at the state level; but he noted that, as the instruments are available only at the state level, further disaggregation is simply not feasible. On the issue of potential selection bias, Angrist said one should remember that compulsory schooling laws were passed in the early twentieth century, at a time when many children left school to enter the labor market; at that time, school-leavers were not necessarily troublemakers who did badly in school and whose benefit from extra schooling might be smaller than average. Regarding policy implications, he noted that the absence of externalities does not necessarily justify cutting subsidies to education, as there are also distributional consequences.

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Benjamin Friedman suggested that it might be possible to exploit data on geographical mobility by state to get sharper estimates. In particular, we might expect the externality to be smaller in states with higher labor mobility. Acemoglu agreed in principle, but worried about adding more endogenous variables to the analysis; he also thought the approach might make more sense at the city level. Andrew Atkeson noted the high rates of mobility, both within and across countries, of educated workers; he argued that with sufficient migration the method of the paper would be unable to detect an externality. Acemoglu pointed out that 65% of the people between ages 45 and 49 are still living in their states on birth, so that mobility is far from complete; still, he conceded that 30–35% rates of migration might be enough to arbitrage away the externalities created by local schooling laws.

John Leahy said that it is not obvious that mobility reduces the externality. He cited Michael Kremer's O-ring theory, which implies that the ability to move increases external returns. On the subject of the validity of the instruments, Leahy wondered whether CSLs might not be correlated with urbanization, which differs across states and is highly persistent over time. If so, their exogeneity with respect to wages thirty years later might be questioned.

Valerie Ramey warned that one should be careful in using this type of estimate in cross-country comparisons, as doing so implicitly assumes constant returns in the externality. She pointed out that the economic implications of moving from average education of 9 years to 12 years might be very different from moving from a population that cannot read to one that can. The authors agreed with this comment.

About the negative correlation of the instruments with college attendance, Olivier Blanchard mentioned the possibility that states view their education budgets as fixed, so that if more is spent on high school then the subsidy to postsecondary education falls; in principle, at least, this is testable. Gregory Mankiw noted that, if Blanchard is right and if it is also the case that externalities differ by level of education, then the paper's findings are suspect. Acemoglu agreed that if Blanchard's hypothesis is right and if the returns to college education are much higher than the returns to attending high school, this paper would be underestimating the externality; but he thought it unlikely that the overall bias would be large.

Mankiw also raised the issue of how one should frame the null hypothesis: Is it, for example, that the externality is large enough to justify current education policy? From that perspective, if one-third of education costs are borne by taxpayers and the external effect is roughly half the private effect, then on the basis of this paper we cannot reject the

hypothesis that current policy is optimal. Ben Bernanke added that, since the wage measure is before tax, a marginal tax rate of about one-third already justifies the current level of subsidy. Mark Bils objected to Bernanke's conclusion on the ground that it ignores the fact that school subsidies themselves must be financed through distortionary taxation.