NOTES

SCHOOLING EXTERNALITIES, TECHNOLOGY, AND PRODUCTIVITY: THEORY AND EVIDENCE FROM U.S. STATES

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Abstract—The literature on schooling externalities in U.S. cities and states is rather mixed: positive external effects of average education levels are hardly found while positive externalities from the share of college graduates are more often identified. We propose a simple model to reconcile this mixed evidence. Our model predicts positive externalities from increased college education and negligible external effects from high school education. Using compulsory attendance/child labor laws, push-driven immigration of highly educated workers, and the location of land-grant colleges as instruments for schooling attainments, we test and confirm the model predictions with data on U.S. states for the period 1960–2000.

I. Introduction

Schooling is a valuable private investment as it increases the returns to hours worked of individuals. Moreover, as highly educated workers promote the development and adoption of better technologies, schooling may have large positive effects (externalities) on the productivity of all factors (total factor productivity or TFP). The cross-country empirical evidence based on development accounting methods (for example, Hall & Jones, 1999) identifies very significant positive correlations between measures of average schooling and measures of TFP across countries. Similarly, the growth literature has found positive effects of higher average schooling on growth (Temple, 1999; De la Fuente & Domenech, 2001, 2006). However, cross-country empirical analysis is unlikely to identify how much of the correlation between human capital and TFP is the result of an externality and how much is due to common determinants such as institutions and social infrastructure. This is mainly because it is very hard to find a genuinely exogenous shift in schooling levels across countries and to track its effects on TFP in a cross-country analysis. On the other hand, the empirical research based on state or city data within the United States that uses credibly exogenous instruments for the variation of schooling (such as schooling laws or the presence of public colleges) shows mixed results on the external effects of schooling. While Moretti (2004) finds large TFP effects of an increase in the *share of college graduates* in U.S. cities, Acemoglu and Angrist (2000) and Ciccone and Peri (2006) do not find any significant TFP effects of an *increase in average schooling* across U.S. cities and states.

Beginning with Rauch (1993), the framework traditionally adopted to analyze the external effects of schooling on TFP considers average schooling as a sufficient statistic to evaluate human capital's private and external returns. This strategy, however, neglects two well-established facts highlighted in the literature on cross-country income differences (Caselli & Coleman, 2006) and the literature on technological adoption and growth (Acemoglu, 1998, 2002; Acemoglu & Zilibotti, 2001). First, workers with different educational levels are not perfect substitutes in production and the relative wages of college- and high school–educated workers are affected by their relative supply (for example, Katz & Murphy, 1992; Angrist, 1995; or Ciccone & Peri, 2005). Thus, it seems appropriate to model two factors of production, skilled and unskilled workers, as imperfectly substitutable. Second, the presence of highly skilled workers seems to foster the adoption of skill-complementary (or skill-biased) technologies. The 1980s and the 1990s in the United States witnessed a substantial increase in the college–high school wage premium together with an increase in relative college–high school employment (Katz & Murphy, 1992; Autor, Katz, & Krueger, 1998; Autor, Katz, & Kearney, 2008). Furthermore, the relative wages of highly educated workers, as well as the relative supply of these workers, were much higher in rich (developed) countries in the year 2000 than in poor (developing) countries (Caselli & Coleman, 2002, 2006). These facts are consistent with systematic skill-biased technology adoption in economies with higher shares of educated workers (Acemoglu, 1998, 2002). Consequently, we allow for different technologies to have different degrees of complementarity to skills so that the two types of workers, the less educated and the highly educated, adopt different (skill-specific) technologies in order to maximize their productivity.

This paper revisits the issue of schooling externalities on U.S. data taking into account the two above-mentioned facts. The novel contributions of the paper are twofold. First, we introduce a model of regional economies, representing U.S. states, where two types of technologies exist: one complementary to highly educated workers and another complementary to less educated workers. Using this simple model and parameter values from the literature, we simulate the effects of an exogenous increase in high school education vis-à-vis an increase in college education. These two shifts have quantitatively very different impacts on productivity, with only college education having a sizable external effect. Second, we test the model on data for U.S. states for the period 1960–2000 using the method developed in Ciccone and Peri (2006). An important feature of our identification strategy is that we can construct exogenous shifters of the number of years of high school and college education per worker to be used as instruments. Compulsory schooling laws, in place between 1920 and 1970 and introduced at different times in different states (see Acemoglu & Angrist, 2000), provide an exogenous shifter of the number of years of high school per worker across states. Additionally, a measure based on the push-driven immigration of highly educated foreign-born to U.S. states and a measure based on a state's population living close to land-grant colleges provide reasonably good instruments for the state variation in the number of years of college per worker. Reconciling previous evidence (Moretti, 2004; Acemoglu & Angrist, 2000; Ciccone & Peri, 2006) and in accordance with the simulated predictions of the model, we estimate that one extra year of college per worker increases the state's TFP by a very significant 6%–9%, while one extra year of high school per worker increases the state's TFP by an insignificant 0%–1%.

The rest of the paper is organized as follows. Section II describes the model, derives its equilibrium conditions, and uses it to simulate the external (TFP) effects of an increase in schooling due to both an increase in high school education and an increase in college education. Section III uses Ciccone and Peri's (2006) constant composition approach and U.S.

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state-level data for the period 1960–2000 to *estimate* the effects of increased schooling. Finally, section IV provides concluding remarks.

II. The Model

Our model combines elements of Yeaple (2005) in a framework similar to that developed in Acemoglu (1998, 2002) and Acemoglu and Zilibotti (2001). We consider a closed economy, representing a U.S. state, that produces two locally consumed goods. An extension of the model to a two-state open economy setup with costly trade can be found in Iranzo and Peri (2006), with the results concerning schooling externalities being qualitatively similar to the simpler model developed here.[1] Unlike Acemoglu (2002) and Acemoglu and Zilibotti (2001) who model directed technological change, we do not try to explain the creation of new technologies but rather take the technological menu as given. In this framework, the schooling distribution of a state will affect the combination of technologies used and thus the state's sectorial composition. The main question we want to answer is the following: What are the external returns accruing to a state from an increase in the schooling level of its workers? In particular, are there external returns from increases in college education and/or from increases in high school education?

A. *Framework and Equilibrium*

Consider the economy of a state where each consumer receives one unit of utility from consuming the composite good C described by the following CES aggregator:

$$C = \left[(1-\beta) Y^{\frac{\theta-1}{\theta}} + \beta X^{\frac{\theta-1}{\theta}} \right]^{\frac{\theta}{\theta-1}}. \tag{1}$$

The parameter θ measures the elasticity of substitution between a homogeneous good Y and a differentiated good X, which is in turn described by the following CES aggregator:

$$X = \left(\int_0^N x(\upsilon)^{\frac{\sigma-1}{\sigma}} d\upsilon \right)^{\frac{\sigma}{\sigma-1}}, \tag{2}$$

where $\upsilon \in [0,N]$ denotes the particular variety of X, σ is the elasticity of substitution between varieties, and N is the total number of varieties. We assume that the varieties of X are closer substitutes for each other than they are for the homogeneous good Y, that is, $\sigma > \theta > 1$.

Taking good Y as the numeraire and denoting with $p(\upsilon)$ the price of variety υ, the demands for good Y and for each variety υ of good X are respectively

$$Y = (1-\beta)^{\theta} \frac{E}{P} \left(\frac{1}{P} \right)^{-\theta}, \quad x(\upsilon) = \left(\frac{s(P_X)E}{P_X} \right) \left(\frac{p(\upsilon)}{P_X} \right)^{-\sigma}, \tag{3}$$

where $P_X = [\int_0^N p(\upsilon)^{1-\sigma} d\upsilon]^{\frac{1}{1-\sigma}}$ is the unit price of composite good X, $P = [\beta^{\theta} P_X^{1-\theta} + (1-\beta)^{\theta}]^{\frac{1}{1-\theta}}$ is the overall price index, E is aggregate expenditure, and $s(P_X) = (\beta^{\theta} P_X^{1-\theta})/[\beta^{\theta} P_X^{1-\theta} + (1-\beta)^{\theta}]$ is the share of aggregate expenditure devoted to good X.

Good Y is produced by perfectly competitive firms with a constant return to scale technology, while each variety of X is produced under monopolistic competition using a common technology that requires a fixed cost, F_X, in the form of output that cannot be sold. This fixed cost can be considered a research/start-up cost to develop each variety. Each firm is the sole producer of a distinct variety and there is free entry in sector X.

Labor is the only factor of production. However, workers are not homogeneous but differ in their skills, which we measure by years of schooling. We index the education of a worker with the continuous variable $Z \in [0,1]$ and standardize the highest educational level, a PhD degree, to 1. Hence, $Z = 1$ corresponds to twenty years of schooling, the normal time to achieve a PhD, while high school graduation (achieved after twelve years in school) and college graduation (obtained after sixteen years of schooling) correspond to $Z = 0.6$ and $Z = 0.8$ respectively. The distribution of workers' education in the state is described by the cumulative density function $G(Z)$ and we define $W(Z)$ as the wage, in units of the numeraire, paid to a worker with education Z. For simplicity, the mass of workers in the state is standardized to 1. Workers of any educational level can produce either of the two goods.

Goods Y and X are produced using different technologies. We assume that in both sectors the productivity of a worker increases with her education, Z, but it increases more rapidly in sector X. Thus we will refer to sector X as the advanced or "high-tech" sector, whereas Y is the "traditional" sector. Consistent with these assumptions, we define the following production functions:

$$A_x(Z) = \exp(g_X Z), A_Y(Z) = \exp(g_Y Z), \text{ with } gx > gy > 0. \tag{4}$$

As mentioned above, we are not interested in explaining technological creation here. Therefore we assume the technological parameters g_Y and g_X are exogenous and common across states. Finally, the aggregate income of workers, expressed in units of the numeraire, equals aggregate expenditure, that is, $E = \int_0^1 W(Z)dG(Z)$.

With no trade between states, production and consumption of each good in a given state coincide and thus their relative price is determined within the internal market. Unit labor costs in each sector, denoted by $\hat{W}_Y$ and $\hat{W}_X$, are given by

$$\hat{W}_Y = W_Y(Z)/\exp(g_Y Z), \hat{W}_X(Z) = W_X(Z)\exp(g_X Z). \tag{5}$$

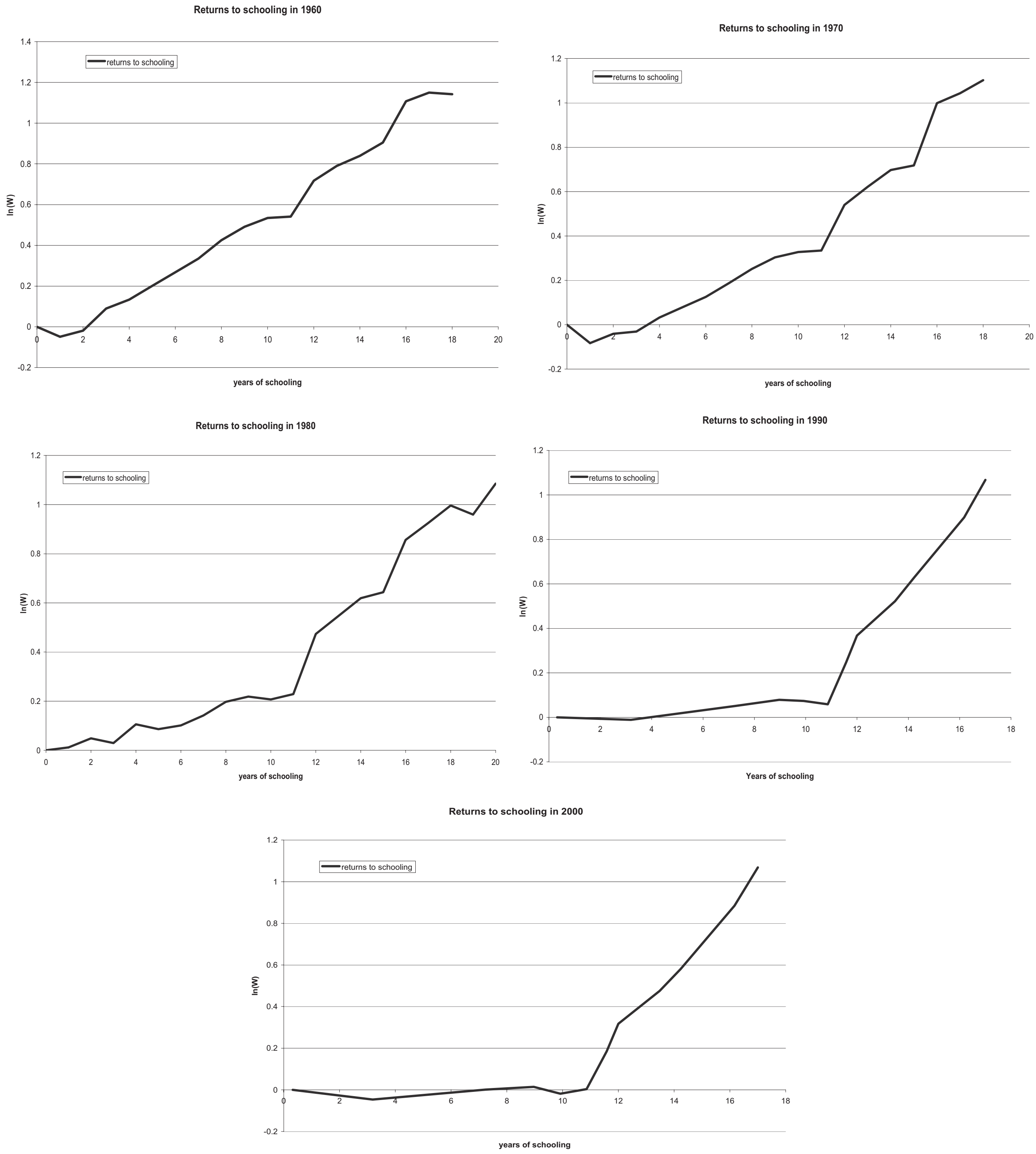
Perfect competition in sector Y implies that prices equal the marginal (and average) cost, that is, $\hat{W}_Y = 1$, while profit maximization and free entry in sector X yield unit prices equal to a markup on the marginal cost and a scale of production $x(\upsilon)$ proportional to the fixed cost:

$$p(\upsilon) = \tfrac{\sigma}{\sigma-1} \hat{W}_X, \quad x(\upsilon) = (\sigma-1) F_X \text{ for } \upsilon \in [0,N]. \tag{6}$$

In a perfectly competitive labor market the wage schedule adjusts in order to equalize the unit costs of all firms using the same technology. Moreover, workers choose to work in the sector in which they are paid the highest wage. Since highly educated workers have a comparative advantage using the advanced technology, there will be a threshold value $Z = \bar{Z}$, satisfying the condition $\hat{W}_X = \exp[(g_Y - g_X)\bar{Z}]$, such that workers with education $Z > \bar{Z}$ work in sector X while workers with $Z < \bar{Z}$ choose to work in sector Y. That is, the wage schedule can be expressed as

[1] In terms of results, the only difference between the closed- and open-economy versions rests on the magnitude of the externalities obtained. As we will discuss in section IIB, when states trade with each other the externalities spill over to states other than the one where schooling increases.

FIGURE 1.—LOG-WAGE SCHEDULES 1960–2000



Notes: Each graph plots the logarithm of the real weekly wage against years of schooling. These returns to years of schooling are the estimated coefficients obtained by regressing for each Census year the log weekly wages on years of schooling dummies and an additional set of individual controls. We included individuals between 16 and 65 years of age who worked at least one week in the previous year, earned some wage income, and did not live in group quarters.

$$W(Z) = \begin{cases} \exp(g_Y Z) & \text{if } 0 \leq Z \leq \bar{Z} \\ \hat{W}_X \exp(g_X Z) & \text{if } \bar{Z} \leq Z \leq 1. \end{cases} \quad (7)$$

Expression (7) implies that the log-wage schedule is a linear function of education Z with a kink at $\bar{Z}$. Given that $g_X > g_Y$, the slope (that corresponds to the returns to schooling) is larger for $Z > \bar{Z}$ than for $Z < \bar{Z}$. Figure 1 shows the empirical log-wage schedule for U.S. workers for each Census year between 1960 and 2000. It is clear that since 1970 the log-wage schedule reflects the shape predicted by the model, with a change in slope occurring in the vicinity of twelve years of schooling.

The average wage in each state equals the per capita income and, because of the standardization of the employment mass to 1, it also equals the aggregate income:

$$E = \bar{W} = \int_0^{\bar{Z}} \exp(g_Y Z) dG(Z) + \hat{W}_x \int_{\bar{Z}}^1 \exp(g_X Z) dG(Z) \tag{8}$$

At the symmetric equilibrium the prices charged and the quantities produced by each firm are identical, and are given by

$$P_X = \left(\frac{\sigma}{\sigma - 1}\right) N^{\frac{1}{1-\sigma}} \hat{W}_X \text{ and } N = \frac{1}{\sigma F_X} \int_{\bar{Z}}^1 \exp(g_X Z) dG(Z). \tag{9}$$

As observed, all the endogenous variables of the model are a function of the skill cutoff level $\bar{Z}$ which, in turn, can be pinned down using the market-clearing conditions. Since individuals spend a share $s(P_X)$ of their income, $\bar{W}$, on good X the market-clearing condition for each variety of good X is[2]

$$\frac{s(P_X)\bar{W}}{N} = x(\upsilon)p(\upsilon), \upsilon \in [0,N]. \tag{10}$$

Substituting the equilibrium values of $x(\upsilon)$, $p(\upsilon)$, N, and $\bar{W}$ into equation (10) we obtain the following equilibrium condition that identifies $\bar{Z}$:

$$\int_0^{\bar{Z}} \exp(g_Y Z) dG(Z) - \left(\frac{1-\beta}{\beta}\right)^{\theta} \left(\frac{\sigma}{\sigma - 1}\right)^{\theta - 1} \left(\frac{\sigma F_X}{M}\right)^{\frac{\theta-1}{\sigma-1}} \exp[\theta(g_Y - g_X)\bar{Z}] \times \left(\int_{\bar{Z}}^1 \exp(g_x Z) dG(Z)\right)^{\frac{\sigma-\theta}{\sigma-1}} = 0. \tag{11}$$

This model can be compared with the standard two-skill model with skill-specific technology used in Acemoglu (2002) or Caselli and Coleman (2006). Let us define $L = \int_0^{\bar{Z}} dG(Z)$ as the total employment of less educated workers (employed in sector Y) and $H = 1 - L = \int_{\bar{Z}}^1 dG(Z)$ as the total employment of highly educated workers (employed in sector X). The average productivity of each group is $\bar{A}^L = [\int_0^{\bar{Z}} \exp(g_Y Z) dG(Z)]/L$ and $\bar{A}^H = [\int_{\bar{Z}}^1 \exp(g_X Z) dG(Z)]/H$. Accordingly, the average wages for less educated and highly educated workers are $\bar{W}^L = \bar{A}^L$ and $\bar{W}^H = \hat{W}_X \bar{A}^H$ respectively. Using this notation, condition (11) can be rewritten as

$$\frac{\hat{W}_X}{\hat{W}_Y} = \hat{W}_X = \Phi\left(\frac{\beta}{1-\beta}\right)\left(\frac{\bar{A}^H}{\bar{A}^L}\frac{H}{L}\right)^{-\frac{1}{\theta}} (\bar{A}^H H)^{\frac{\theta-1}{\theta(\sigma-1)}}, \tag{12}$$

with $\Phi = \left(\frac{\sigma-1}{\sigma}\right)^{\frac{\theta-1}{\theta}} \left(\frac{1}{\sigma F_X}\right)^{\frac{\theta-1}{\theta(\sigma-1)}}$. Expression (12) differs from the expression of relative wages (per unit of product) derived from a CES production function in the standard neoclassical model with perfect competition by the constant Φ and the term $(\bar{A}^H H)^{\frac{\theta-1}{\theta(\sigma-1)}}$. Substituting equation (12) into (9) we can also rewrite the price of the composite good X as follows:

$$P_X = \Gamma\left(\frac{\beta}{1-\beta}\right)\left(\frac{\bar{A}^H H}{\bar{A}^L L}\right)^{-\frac{1}{\theta}} (\bar{A}^H H)^{-\frac{1}{\theta(\sigma-1)}}, \tag{13}$$

where $\Gamma = \left(\frac{\sigma}{\sigma-1}\right)^{\frac{1}{\theta}} (\sigma F_X)^{\frac{1}{\sigma-1}}$. Expression (13) differs from its counterpart in the neoclassical model by the constant Γ and the term $(\bar{A}^H H)^{-\frac{1}{\theta(\sigma-1)}}$. Hence, in this model an increase in the supply of highly educated workers $(\bar{A}^H H)$ has the typical neoclassical (negative) effect on relative wages and prices but it also has an additional positive external effect captured by the terms $(\bar{A}^H H)^{\frac{\theta-1}{\theta(\sigma-1)}}$ and $(\bar{A}^H H)^{-\frac{1}{\theta(\sigma-1)}}$ respectively. In other words, following an increase in the supply of highly educated workers, the real wage of highly educated workers decreases less and the real wage of less educated workers increases more than what the neoclassical model would predict. Thus, there must be some extra real output accruing to the workers. Equations (12) and (13) also show that keeping the supply of highly educated workers $(\bar{A}^H H)$ constant, a change in the supply of less educated workers $(\bar{A}^L L)$ only has the neoclassical (relative supply) effects on $\hat{W}_X$ and P_X.[3] In short, the model presented here generates positive externalities from increases in the education of highly skilled workers (changes in the skill distribution above the threshold level $\bar{Z}$) and no external effects for improvements in the education of less skilled workers (changes in the distribution below $\bar{Z}$).

B. *Simulation of the Externalities*

We use the model to obtain testable predictions of the external (TFP) effect of different shifts in the educational distribution of workers within a state. In order to disentangle the external effect from the typical neoclassical (supply side) effect explained above, we use the constant composition approach developed by Ciccone and Peri (2006). Based on the "dual approach" to growth accounting, this method identifies changes in total factor productivity by measuring the changes in (real) factor prices (in this case wages, as labor is the only factor of production). Ciccone and Peri (2006) show that the increase in output due to the external effect can be approximated by the change in average wages, keeping the composition of skills constant.[4]

Iranzo and Peri (2006), to which we refer the reader for details, provide a full description of the calibration and simulation of the model. Here we just summarize the procedure and present the main simulated results concerning the externalities associated with increases in the level of high school and college education. Following the consensus estimates on the elasticity of substitution between high-skilled and low-skilled workers (that produce goods X and Y here), we use a value of θ equal to 1.5 (see, for example, Katz & Murphy, 1992; or Ciccone & Peri, 2005). The value of σ is equal to

[2] Walras's law ensures equilibrium in the market for Y as well.

[3] To be precise, a change in the skill distribution, below and above $\bar{Z}$, changes the equilibrium value of $\bar{Z}$ and hence indirectly the amount of effective skills of highly educated workers, $\bar{A}^H H$. Iranzo and Peri (2006) show that for the relevant changes of $\bar{A}^H H$ the indirect effect through $\bar{Z}$ is, however, negligible.

[4] See Ciccone and Peri (2006) for details on this method and Iranzo and Peri (2006) for a more detailed description of how the procedure is applied to this model.

2, consistent with the average estimate of the elasticity of substitution between differentiated tradable goods (Broda & Weinstein, 2006). The parameter β is chosen to be 0.65, corresponding to the expenditure share spent on advanced goods and services (including all consumption goods except for food, apparel, and personal services as from the Bureau of Labor Statistics, 2005). Finally, the technological parameters g_x and g_Y, which equal the returns to education in the two sectors, and F_X, the fixed setup costs for the advanced technology, are calibrated to match the average wage schedule for U.S. workers over the period considered—see figure 1. In particular, we calibrate g_Y and g_X to match the slopes of the wage schedule (or returns to schooling) above and below twelve years of schooling in 1980. Keeping in mind that one year of schooling corresponds to an increase in Z of 0.05, this yields $g_Y = 0.4$ and $g_X = 1.6$ respectively.

Using the overall U.S. schooling distribution of the labor force over five groups (school dropouts, high school dropouts, high school graduates, college dropouts, and college graduates) in 1980 as the starting point, we then conduct the following two experiments. First, we shift 6% of the labor force from the lowest educational group (school dropouts) to the next group (high school dropouts). This shift matches the average reduction per decade in the lowest educational group (from 28% of the employed in 1960 to 4% in 2000) and to a large extent mirrors the effect of the compulsory schooling laws used by U.S. states. In the second experiment we consider an equivalent increase in average schooling (0.36 years) obtained instead by augmenting the educational level in the upper part of the schooling distribution. To that end, we increase the share of college graduates by 10 percentage points, moving people out of the college dropouts group. Our simulations show significant TFP effects when college education increases, but practically no effect when high school education increases. In particular, in the model with no trade, a one-year increase in average high school attendance barely has an effect on the state's TFP (1%), while a comparable increase in college education has an external effect of 8.9%. This localized TFP effect is reduced if X is traded (with a cost) across states. In effect, the version of the model with trade, developed in Iranzo and Peri (2006), shows that the external effect partially spills over, via trade, to other states. The externality gets more diffuse (and thus the local impact of schooling on TFP is smaller) the lower trade costs are. In simulations with an *ad valorem* trade cost for good X of 100%, the localized externality due to an increase in college education is 5.3% per one extra year of schooling, while the externality due to an increase in high school education is only 0.5% per extra year of schooling. With *ad valorem* trade costs equal to 50%, we still obtain a localized externality of 3.25% for one extra year of college per worker and only 0.27% from one extra year of high school per worker. In sum, from the different simulations performed using reasonable ranges of parameter values, we obtain external effects between 0.25% and 1% for one extra year of high school per worker and between 3% and 9% for one extra year of college per worker.

III. Empirical Evidence from U.S. States, 1960–2000

A. *Methodology and Data*

The data used in the empirical analysis come mostly from the Integrated Public Use Microdata Series (IPUMS) of the U.S. Censuses 1960–2000 (Ruggles et al., 2005).[5] The construction of the TFP changes for each U.S. state over the four decades 1960–2000 follows the two-stage procedure developed in Ciccone and Peri (2006). In the first stage we regress the logarithm of the real weekly wage for individual i in state s and Census year t on a set of dummies capturing individual characteristics and a set of dummies that saturate the schooling by experience space in 32 cells combining four schooling groups and eight experience groups.[6] The regressions, run separately for each Census year t and state s, are estimated by weighted least squares using the individuals' weights provided by the Census. The set of dummies is chosen so that the "cleaned" estimated wage for each education-experience group corresponds to white, U.S.-born, married, male workers. In the second stage we use the cleaned wages of each schooling-experience group by state and year and the employment distribution by schooling-experience to construct the constant composition average wage for each state and decade. We use the initial skill composition for each decade and denote as $\Delta \ln w^{cc}_{st}$ the change in the logarithmic (percentage) constant composition average wage for state s in decade t which, as explained, captures the percentage change in TFP for the state over the decade. Using this measure we analyze the relation between the changes in TFP and the changes in schooling across U.S. states. As illustrated in equations (12) and (13), the external effects of skills on TFP depend upon the supply of effective skills of more educated workers ($\bar{A}^H H$) and less educated workers ($\bar{A}^L L$). These, in turn, are monotonic functions of the aggregate skill level of each group, which we define as $\bar{Z}^L = \int_0^{\bar{z}} ZdG(Z)$ and $\bar{Z}^H = \int_{\bar{Z}}^{1} ZdG(Z)$. As total employment is standardized to 1, $\bar{Z}^L$ and $\bar{Z}^H$ measure, respectively, the years of schooling of less and more educated workers relative to total employment. We construct an empirical counterpart to those measures as follows. We compute the "years of high school per worker" (denoted as $\overline{school}^{HS}$) as the years of schooling of workers with at most a high school diploma divided by the total number of workers. This is our proxy for $\bar{Z}^L$. Similarly, we compute the "years of college per worker" (denoted as $\overline{school}^{COLL}$) as the years of schooling of workers with college education divided by the total number of workers. This is the proxy for the average skills of more educated workers ($\bar{Z}^H$).[7] Notice that a one-year increase in either of these two variables represents an increase in overall average schooling of one year. Hence the estimated effects of these variables on TFP are comparable with each other and are also comparable to the simulated effects described in section IIB.

Figure 2 reports the percentage change in TFP, measured as $\Delta \ln w^{cc}_{st}$, against the change in years of high school per worker, $\Delta\overline{school}^{HS}_{st}$, for fifty U.S. states plus DC over two decades (1980–1990 and 1990–2000) pooled together. Figure 3 reports, for the same sample,

[5] We select only individuals between 16 and 65 years of age who worked at least one week in the previous year, earned some wage income, and did not live in group quarters.

[6] The educational groups are the four traditionally used in the labor literature: $H_1 = [0, 12)$ for high school dropouts, $H_2 = [12, 13)$ for high school graduates, $H_3 = [13, 16)$ for college dropouts, and $H_4 \geq 16$ for college graduates. The experience groups are eight groups of five-year intervals ranging between 0 and 40 years.

[7] The terms "years of high school per worker" and "years of college per worker" may seem inaccurate. After all, the group of workers with at most a high school degree also attended years of elementary school and the group with college education also attended elementary and high school. However, since we identify the externalities from inter-Census changes, the differences in the two variables between Censuses is indeed mostly due to years of high school attendance for the first group and years of college attendance for the second group.

FIGURE 2.—TFP CHANGES AND CHANGES IN YEARS OF HIGH SCHOOL PER WORKER

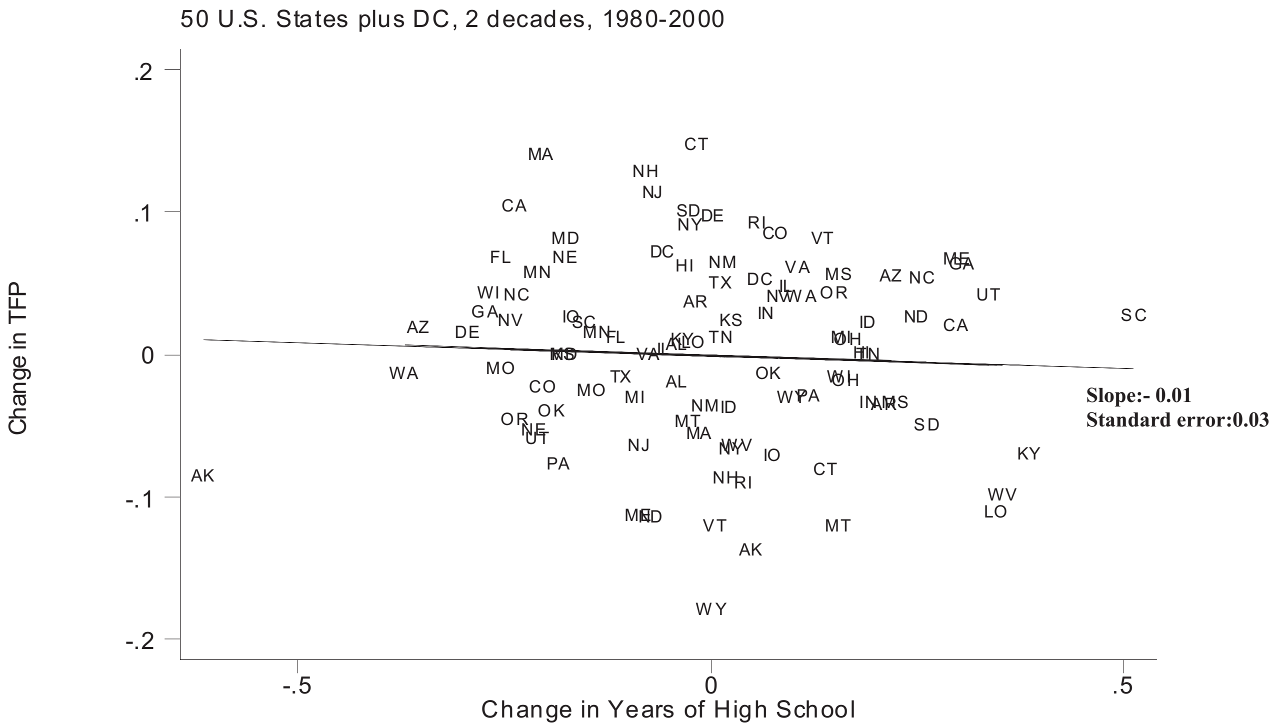


FIGURE 3.—TFP CHANGES AND CHANGES IN YEARS OF COLLEGE PER WORKER

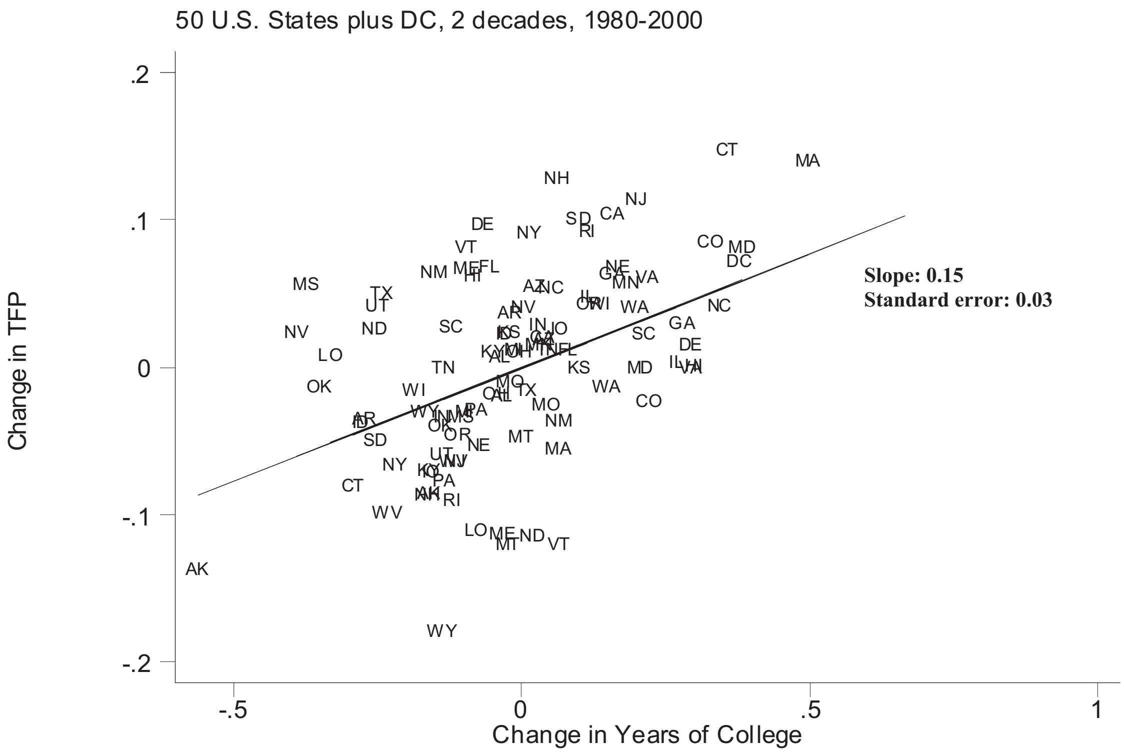


TABLE 1.— FIRST-STAGE REGRESSIONS: EFFECT OF CHILD LABOR (CL) AND COMPULSORY ATTENDANCE (CA) LAWS ON YEARS OF HIGH SCHOOL PER WORKER AND YEARS OF COLLEGE PER WORKER

Dependent variable:	Years of High School per Worker		Years of College per Worker	
Specification:	(1)	(2)	(3)	(4)
Share CA < 8	−1.17** (0.47)	−0.51 (0.43)	−0.60 (0.37)	−0.45 (0.37)
Share CA > 11	1.19** (0.35)	1.04** (0.34)	−0.43 (0.29)	−0.53 (0.29)
Share CL < 6	−1.31** (0.58)	−0.86 (0.52)	0.23 (0.43)	0.29 (0.43)
Share CL > 9	0.81** (0.30)	0.42** (0.28)	0.43 (0.27)	0.37 (0.27)
Region-specific effects	No	Yes	No	Yes
F-test of joint significance (p-value)[a]	9.84 (0.000)	6.52 (0.0001)	2.8 (0.03)	2.4 (0.05)
R^2	0.89	0.91	0.51	0.60
Observations	204	204	204	204

Notes: All regressions are in differences over decades and include decade fixed effects. In specifications (2) and (4) three regional dummies (East, South, and Midwest, omitting West) are included. Each column is a separate regression. Fifty U.S. states plus DC included over the period 1960–2000. Method of estimation: OLS with each observation weighted by the employment in the state-year. Heteroskedasticity-robust standard errors clustered by state are in parentheses.

**indicates a coefficient that is significant at the 5% confidence level.

a: Null hypothesis is that the explanatory variables have no power in predicting the dependent variable. The p-value is the confidence level at which the null hypothesis is rejected.

the change in TFP against the change in years of college per worker, $\Delta school_{st}^{COLL}$. In both graphs the variables are in deviations from the decade-specific average. While there is no correlation at all between TFP changes and increases in years of high school per worker across states (figure 2), we observe a strong and significant correlation between TFP changes and changes in years of college per worker (a 15% increase in TFP for an increase of one year of college education per worker). Although the scatter plots do not establish any causal relationship, they already convey the essence of our empirical findings: only increases in college education are associated with sizable and significant external TFP effects.[8]

B. *Instrumental Variables: Discussion and First Stage*

Rather than being the cause of higher TFP, highly educated workers might be attracted to states with highly productive sectors. Alternatively, the selection of highly educated workers to a state may be due to other (unobservable) characteristics of workers, resulting in a spurious correlation of TFP and schooling. In order to address these endogeneity issues, we adopt an instrumental variable strategy that uses three sets of state-specific determinants of schooling attainments based on supply rather than demand factors. Two of these instruments, compulsory schooling laws and proximity to land-grant colleges, have been used previously in the externalities literature, and here we introduce a new one as well: the push-driven immigration of highly educated foreigners across U.S. states. We discuss the construction and characteristics of each instrument next.

The compulsory attendance (CA) and child labor (CL) laws, in place between 1920 and 1970, affected the schooling level of several cohorts of Americans.[9] They were introduced at different times across states and implied different requirements, in terms of years of schooling, before one could access the labor market. We can identify the minimum years of schooling required by the state where an individual resided at age 14, and attach that requirement to each individual. Consequently, we can calculate the share of workers in each state for which the associated CA laws mandated less than eight years (CA < 8) and the share for which they required more than eleven years (CA > 11). We expect the first share to be associated with smaller values of the variable $school_{st}^{HS}$ and the second with higher values of that variable. Similarly, we use CL laws imposing between six and nine years of schooling to construct the dummies CL < 6 and CL > 9, as well as the corresponding measures of the share of workers in each state associated with the first dummy (for which we expect a negative impact on $school_{st}^{HS}$) and the share of workers associated with the second dummy (for which we expect a positive effect on $school_{st}^{HS}$). While presumably uncorrelated with productivity or the personal ability of workers across states, these four variables are correlated with the schooling levels of individuals, as stricter laws increased significantly the rate of attendance for the nineth, tenth, and eleventh grades, as well as the high school graduation rates of the states in which they were introduced. Table 1 shows the explanatory power of these four variables in predicting inter-decennial changes in years of high school per worker across states (columns 1 and 2). Notice that the coefficient on each variable has the expected sign (positive for CA > 11 and CL > 9 and negative for CA < 8 and CL < 6) and, for the most part, the coefficients are significant. Moreover, the F-test of overall significance always rejects the hypothesis that the instruments are jointly insignificant at the 1% confidence level. As a check, we use the same variables to predict the share of college graduates across states (columns 3 and 4). We obtain no significant coefficients and in this case one can never reject the null hypothesis of zero joint significance at the 1% confidence level (the F-statistic is always lower than 3). This shows that such schooling laws did not generically increase average schooling; they did so by increasing high school attendance rates and high school graduation rates. In other words, they shifted the schooling distribution in its "low" range only.

Our second instrument, the presence of a college close to where a large share of the college-age population resides, affects the margin of college attendance and graduation across states. Proximity to college reduces the (material and psychological) costs of attending college, inducing some individuals to get further education.[10] Moretti (2004) uses the presence of a land-grant college in a metropolitan area as a

[8] The qualitative features of figures 2 and 3 do not depend on the choice of decades. For any decade (from 1960 to 1990) considered one by one or in groups, the correlation between changes in TFP and $\Delta school_{st}^{HS}$ is never significant and occasionally negative (between −0.03 and 0.025), while the correlation between TFP and $\Delta school_{st}^{COLL}$ is always positive and significant (between 0.08 and 0.16).

[9] First collected and used in Acemoglu and Angrist (2000), schooling laws have been used in several other papers as an instrument for schooling. See for instance Milligan, Moretti, and Oreopoulos (2004), Moretti and Lochner (2004), and Oreopoulos, Page, and Stevens (2006).

[10] Card (1995) found that the presence of a four-year college in the same labor market positively affected the probability that an individual attended college and graduated from it. Currie and Moretti (2003) used the same idea of college proximity to instrument mothers' education in analyzing the effect of the latter on children's health.

TABLE 2.— FIRST-STAGE REGRESSIONS: EFFECT OF LAND-GRANT COLLEGES AND OF IMPUTED COLLEGE-EDUCATED IMMIGRANTS ON YEARS OF COLLEGE PER WORKER AND YEARS OF HIGH SCHOOL PER WORKER

	Land-Grant College		Highly Educated Immigrants	
Dependent Variable:	(1) Years of College per Worker	(2) Years of High School per Worker	(3) Years of College per Worker	(4) Years of High School per Worker
People living within 100 km of a land-grant college as % of labor force, 1970	3.11* (1.70)	3.40* (2.05)		
People living within 100 km of a land-grant college as % of labor force, 1980	3.04* (1.71)	2.40* (1.20)		
People living within 100 km of a land-grant college as % of labor force, 1990	4.62** (1.51)	−2.40 (1.81)		
Imputed share of college-educated immigrants			9.23** (2.84)	−2.90 (2.2)
F-test of significance (*p*-value)[a]	6.91 (0.001)	6.16 (0.001)	10.5 (0.0001)	1.64 (0.21)
R^2	0.71	0.80	0.57	0.87
Observations	153	153	204	204

Notes: All regressions are in differences over decades and include decade fixed effects. Each column is a separate regression. Fifty U.S. states plus DC included over the period 1960–2000. Method of estimation: OLS with each observation weighted by the employment in the state-year. Heteroskedasticity-robust standard errors are in parentheses. Columns 1 and 2 report the first-stage regressions for the land-grant instrument, while columns 3 and 4 report the first-stage regressions for the push-driven immigration instrument.
**indicates a coefficient that is significant at the 5% confidence level, *significant at the 10% confidence level.
a: Null hypothesis is that the explanatory variables have no power in predicting the dependent variable. The *p*-value is the confidence level at which the null hypothesis is rejected.

predictor of its share of college educated. Land-grant colleges were established in the late 1800s to provide accessible higher education in each U.S. state. Hence their initial location is not correlated with returns to education in the late 1900s. Moreover, they are evenly distributed across the United States, and they became well-established, large institutions over time. Individuals living close to them are likely to have lower costs and thus a higher probability of attending them than others living farther away. For each state we calculate the percentage, in the working-age population, of the people between the ages of 14 and 21 living in counties within 100 kilometers (60 miles) of a land-grant college in every Census year (between 1970 and 1990).[11] We use this measure as a predictor of the change in the years of college education per worker in the following decade.[12] As the effect on college attendance was different across decades, we interact those shares with decade dummies. Table 2, column 1, shows the power of these instruments in predicting the (increases in) years of college education per worker. There is a positive and significant correlation between the instruments and the increase in years of college education per worker, and the correlation is stronger for the 1990s than for the 1980s and 1970s. For each 10% increase in the share of young residents living within 100 kilometers of a land-grant college in the 1990s, a state experienced 0.46 more years of college education per worker (column 1). The *F*-test of joint significance of the instruments is above 6, showing that overall there is a significant, though not overly strong, correlation. We then check whether the instrument also shifts education at the high school level (column 2). The effect is positive and significant in the 1970s and 1980s but insignificant (and negative) in the 1990s. Thus, because of the limited power of this instrument and its limited ability to isolate changes in college education at the state level, we develop and use an additional instrument.

Previous studies have used the geographically uneven residence of less educated Mexican immigrants in the 1960s, and the tendency of new immigrants to locate in the same state as previous immigrants, to construct instruments for the changes in the supply of less educated workers.[13] Similarly, we use the fact that immigrants from non-Hispanic countries (especially India, China, and Europe) are generally over-represented among college graduates while under-represented among high school graduates and college dropouts to construct an instrument for the years of college education per worker. Based on an "imputed" inflow of highly educated immigrants by state, the instrument is calculated as follows. Using 1960 as the reference year, we compute the number of foreign-born workers residing in each U.S. state that were born in one of 57 different foreign countries. We attribute to each national group in each state in 1960 the skill composition of that group nationwide. While the *initial share of highly educated* workers in a state is likely to be correlated with its sectorial composition, and possibly with its subsequent productivity changes, our instrument is based only on the *initial distribution of immigrants by nationality* and not by education. Hence we avoid possible state selection bias within certain educational groups. Taking the 1960 imputed number of college educated by country of origin and by state, we apply the inter-decennial national growth rates of the college graduate population from each of those 57 nationalities. Using these values we then compute the imputed share of foreign-born college graduates in total employment for each Census year. This methodology exploits the fact that certain countries (such as India and China) sent many of their college graduates to the United States during the period considered. Further, if there is a higher probability for the new immigrants to land and stay where previous immigrants from the same country (of any education level) already were (due to, for instance, networking, taste, or information reasons), then the imputed inflows of college educated will be correlated to the actual inflow of foreign-born

[11] These data were obtained from the *County and City Data Book,* U.S. Bureau of the Census (2000). We are grateful to Jordan Rappaport for sharing these data with us.

[12] We also used the number of potential students within 200 and 300 kilometers of a land-grant college, with similar results.

[13] See for instance Card (2001), Lewis (2004), and Peri and Sparber (2007).

TABLE 3.—OLS AND 2SLS ESTIMATES OF THE EFFECTS OF YEARS OF HIGH SCHOOL PER WORKER AND YEARS OF COLLEGE PER WORKER ON TFP

Method of Estimation	Ordinary Least Squares			Two-Stage Least Squares		
Specification:	(1) Basic	(2) Males Only	(3) 1980–2000	(4) Basic	(5) Males Only	(6) 1980–2000
$\Delta\overline{school}_{st}^{HS}$	0.013 (0.014)	0.024 (0.014)	0.01 (0.03)	−0.01 (0.01)	0.01 (0.015)	0.01 (0.03)
$\Delta\overline{school}_{st}^{COLL}$	0.069** (0.02)	0.084** (0.025)	0.156** (0.03)	0.06** (0.025)	0.056** (0.025)	0.12** (0.04)
Test of overidentifying restrictions (*p*-value)	Not Applicable	Not applicable	Not applicable	8.82 (0.19)	7.6 (0.27)	3.92 (0.70)
R^2	0.72	0.64	0.71	0.68	0.45	0.49
Observations	204	204	102	153	153	102

Notes: Dependent variable is the percentage TFP change by state and decade as measured by the cleaned constant composition real wage change, $\Delta\ln\omega_{st}^{cc}$, calculated as described in the main text. Each column is a separate regression. All regressions include decade fixed effects. Heteroskedasticity-robust standard errors, clustered by state, are in parentheses. Method of estimation for columns 1–3 is OLS with each observation weighted by the employment in the state-year using a panel of fifty U.S. states plus DC over four decades, 1960–2000. The method of estimation for columns 4–6 is 2SLS with each observation weighted by the employment in the state-year. We use as instrumental variables the imputed share of college-educated immigrants, the population share near a land-grant college, and CA-CL laws, as described in the main text. The sample is a panel of fifty U.S. states plus DC over the period 1970–2000. Specifications (1) and (4) are the basic ones using all individuals and years; specifications (2) and (5) include only male workers in the construction of the constant composition wage; specifications (3) and (6) include only the decade changes between 1980 and 2000.
**indicates a coefficient that is significant at the 5% confidence level.

college educated and, thus, to the total supply of college educated workers in the state.

Columns 3 and 4 of table 2 show the predictive power of the constructed share of college-educated immigrants on years of college per worker and on years of high school per worker respectively. This instrument has strong predictive power (very significant *t*-statistics and *F*-test) for the years of college per worker, whereas it has no power at all in predicting the years of high school per worker. In other words, it only shifts the schooling distribution at the high end of the schooling range. This implies that identification across states does not come simply from differences between high-immigration and low-immigration states but from the specific nationalities that are associated with high levels of education and their geographic distribution across states.[14]

C. Estimates

Table 3 reports the estimation results of the following regression:

$$\Delta\ln w_{st}^{cc} = \alpha_t + \beta_1\Delta\overline{school}_{st}^{HS} + \beta_2\Delta\overline{school}_{st}^{COLL} + \varepsilon_{st}, \tag{14}$$

where α_t are Census year fixed effects. Each variable measures the change in the fifty U.S. states plus DC over each decade of the 1960–2000 period, and ε_{st} denotes uncorrelated zero mean errors. Each observation is weighted by the employment in the state and standard errors are clustered at the state level. In columns 1–3 we use ordinary least squares to estimate the externalities from years of high school per worker (first row) and years of college per worker (second row), and columns 4–6 report the two-stage least squares estimates using the three sets of instruments: the CA-CL laws, the proximity to land-grant colleges, and the imputed share of college-educated immigrants.[15] We include all the instruments in order to maximize their power. The estimates reported in column 1 show that an increase of one year of college per worker is associated with an almost 7% increase in the state's TFP, while a one-year increase in high school per worker is associated with an insignificant 1.3% increase in TFP. Similar results (with a slightly larger college externality) are obtained if we use male workers only to calculate the constant composition wages (column 2), whereas when we restrict the sample to the two most recent decades (column 3) the strength of the externality from college education increases. In particular, specification (3) shows a 15% externality resulting from one extra year of college per worker for the 1980–2000 period, while the years of high school per worker still have a statistically insignificant coefficient of 0.01. These correlations are qualitatively and quantitatively consistent with the model presented and simulated above. As in the simulations, the external effects of high school education are never above 1%–2% of TFP. With respect to college education, the estimated external effects are between 7% and 9% for the whole sample, consistent with the simulations (described in section IIB) that showed localized externalities between 5% and 9%.

Specification (4) reports the 2SLS counterpart to the estimates in equation (1). The TFP effect from one extra year of high school per worker is −1%, though insignificantly different from 0, while the TFP effect of one extra year of college per worker is 6%, significantly different from 0. Specification (5), obtained using male workers only, shows a 1% insignificant TFP effect of one extra year of high school per worker and a significant 5.6% effect from one extra year of college per worker. Relative to the OLS estimates, the IV estimates are smaller by 1–3 percentage points. This supports the notion that demand-driven TFP growth might have biased the OLS estimates upwards. Moreover, the 2SLS estimates are even closer than the OLS estimates to the simulated effects from the model with costly trade (see section IIB). Specification (6) restricts the regression to the most recent decades, 1980–2000. In line with the OLS estimates, the TFP impact of one extra year of college is larger (12%), although more imprecisely estimated, and the effect of high school education is still insignificantly different from 0. Despite the large value of the externality associated with college education, once we account for its standard deviation (4%) the estimate is not significantly different from the simulated values. We should also keep in mind that restricting the analysis to the last two decades reduces considerably the power of the instruments, and hence increases the standard error and the potential weak-instrument bias.[16] Finally, we test for the exogeneity of the instruments using the test of overidentifying restrictions as described

[14] The power of imputed college-educated immigrants as an instrument for years of college per worker across U.S. states is not as strong as the power of imputed less educated immigrants in predicting the share of workers with no degree. Iranzo and Peri (2006) present a series of comparisons and establish that the instrument based on push-driven immigration is more effective for less educated workers than for highly educated workers because of the second group's greater mobility and weaker tendency to co-locate. Yet, it is still a powerful enough instrument.

[15] The 2SLS estimation is performed for the period 1970–2000 only. We omit the 1960s because data on the college-age population by county in 1960 (used to construct the land-grant instrment) is not available from the *County and City Data Book.*

[16] Several other specifications are estimated and reported in Iranzo and Peri (2006). The results obtained are robust to the omission of outliers, to

Table 4.— Controlling for Changes in Human Capital and Technology Driven by Initial Sector Composition

Method of Estimation	Ordinary Least Squares				Two-Stage Least Squares			
Specification:	(1) Basic	(2) Males Only	(3) Control Based on 20% Most Productive Sectors	(4) Control Based on 50% Most Productive Sectors	(5) Basic	(6) Males Only	(7) Control Based on 20% Most Productive Sectors	(8) Control Based on 50% Most Productive Sectors
$\Delta\overline{school}_{st}^{HS}$	0.013 (0.013)	0.025 (0.015)	0.02 (0.011)	0.016 (0.023)	−0.01 (0.02)	0.005 (0.01)	0.005 (0.02)	0.005 (0.025)
$\Delta\overline{school}_{st}^{COLL}$	0.06** (0.025)	0.062** (0.020)	0.055** (0.025)	0.066** (0.021)	0.11** (0.04)	0.11** (0.04)	0.050* (0.028)	0.048** (0.024)
Change in the share of college graduates driven by initial sector composition	1.4 (0.8)	0.70 (1.02)			1.4 (2.2)	2.2 (1.8)		
(Share of gross state product in top productive sectors, 1960) × (productivity growth of sectors)			2.3 (1.6)	0.55 (0.33)			2.5 (1.5)	0.60** (0.31)
Observations	204	204	204	204	153	153	153	153

Notes: Dependent variable is the percentage TFP change by state and decade as measured by the cleaned constant composition real wage change, $\Delta \ln \omega_{st}^{cc}$, calculated as described in the main text. All regressions include decade fixed effects. The observations correspond to state-decade changes. Heteroskedasticity-robust standard errors clustered by state are in parentheses. The method of estimation for columns 1–4 is OLS with each observation weighted by the employment in the state-year. The sample is a panel of fifty U.S. states plus DC over four decades, 1960–2000. Columns 5–8 are estimated using 2SLS with each observation weighted by the employment in the state-year. As instrumental variables we include the imputed share of college-educated immigrants, the population share near a land-grant college, and CA-CL laws, as described in the main text. The period included is 1970–2000.

**indicates a coefficient that is significant at the 5% confidence level, *significant at the 10% confidence level.

in Woolridge (2002). The test statistic, reported in the third row of table 3, is distributed as a *chi*-squared with 6 degrees of freedom under the null hypothesis (that no instrument enters the estimating equation directly).[17] We cannot reject the null hypothesis of exogenous instruments at any significant confidence level.

The available IV estimates of average schooling externalities from previous studies using CA-CL as exogenous shifters of schooling (Acemoglu & Angrist, 2000; Ciccone & Peri, 2006) are mostly within the range obtained in table 3 for high school externalities (−1 to + 1%) and tend to be statistically insignificant. The existing estimates of externalities from college-educated workers (Moretti, 2004) are around 1% for each 1% increase in the share of college graduates. Assuming that the increase in college graduates corresponds to a decrease in high school graduates of the same amount, Moretti's estimates for the 1980s and 1990s imply an external effect of 25% for each extra year of college per worker. Our estimates for that period are around 12%, about half the size of the effect estimated by Moretti.[18]

D. *Robustness Checks*

The initial sector composition of a state, interacted with sector-specific productivity growth and human capital intensity, may be responsible for part of the estimated correlation between college education and TFP, which means that our estimates of schooling externalities could be biased upward. We address this potential problem by explicitly controlling for the effect of the initial sector composition on the demand for highly educated workers and on the productivity growth of the state. Specifications (1), (2), (5), and (6) in table 4 report the estimation of equation (14), including an "imputed" increase in the share of college-educated workers across states driven by their initial sector composition. We begin with the composition of employment in each state in 1960 over 41 different sectors,[19] and then apply, for each sector in each state, the growth rate in college graduate employment experienced by that sector nationwide in each of the decades considered. Summing across industries for each state and year and dividing by total workers, we obtain a "sector-driven" imputed share of college graduates in the labor force. This measure proxies for the demand-driven increase in college-educated workers in each state. This variable is significantly correlated with the actual change in years of college per worker (the correlation coefficient is 0.73), indicating that demand factors are important determinants of the changes in a state's college-educated workers. Consequently, including it as a control allows us to be more confident that the estimated coefficients on the variable $\Delta\overline{school}_{st}^{COLL}$ isolate a supply-driven externality rather than a demand- (sector-) driven correlation. The OLS estimates of the externalities (columns 1 and 2) are fairly similar to those in table 3: the TFP effect of college education is around 6% and no externality from high school education is found. The IV estimates of college externalities are somewhat larger (around 11%) and less precise, but still compatible with the simulated values. We should keep in mind that, given the correlation between the control and the instruments, collinearity reduces the precision of the IV estimates. In any case, though, the inclusion of the control does not eliminate or reduce the size and significance of the college externality and it does not change the insignificance of the high school externality either. Restricting the sample to males only (specifications 2 and 6) does not produce any significant changes in the estimates. Alternatively, we can control for demand-driven productivity growth at the state level. Using data on gross state product by sector over the years 1963–1997 (from the

different methods of constructing the constant composition wage, and to the selection of different decades.

17 The degrees of freedom are determined by the number of instruments, eight in this case (four CA-CL shares, three land-grant college variables, and one imputed immigrants' college share), minus the number of endogenous variables (two).

18 Moretti (2004) is aware of the very large size of his estimated externalities (see his discussion on page 195). Differences with our estimates may arise from his choice of cities, rather than states, and differences in the set of instruments used.

19 The classification used is close to a two-digit classification, based on the variable IND1950 in the Census, and follows the one used in Hanson and Slaughter (2002).

Bureau of Economic Analysis), and merging them with employment data from the Census from 1960 to 2000, we compute the output per worker in 41 industries in 1960, 1970, 1980, 1990, and 2000.[20] Then we calculate the share of gross state product in 1960 accounted for by the top 20% and 50% most productive sectors. We interact these shares with the productivity growth of those industries over each of the following decades and include the resulting variables as controls in specifications (3), (4), (7), and (8) of table 4 (fourth row). The presence of a large initial share of highly productive industries may induce high TFP growth in a state, particularly in decades when productivity growth of those industries is large (hence the interaction). The possibility of such productivity growth attracting highly educated workers may induce omitted variable bias in the regressions. Table 4 shows the OLS (columns 3 and 4) and IV estimates (columns 7 and 8) of the externalities when we control for this demand-driven productivity growth. The correlation between this type of growth and the increase in college education (and with the immigration-based IV) is small (never larger than 0.2) and the OLS and IV estimates are not very different from those in table 3. The IV estimates of the college education externalities are around 5% and the high school education externalities are never higher than 0.5%.

IV. Conclusions

This paper analyzes the connection between high school and college education and TFP using a simple model and a new empirical strategy. The nature of the technology in the model is such that below a certain education level (estimated to be around twelve years of schooling) increases in schooling have low private as well as social returns. Above that threshold, however, education has large private and social returns as it is associated with the adoption of modern technology that increases the variety of goods produced and hence results in overall TFP gains. A simple calibration of the model to U.S. state data shows that the increase in secondary education had very small effects on TFP (less than 1% for a one-year increase of high school education per worker), while comparable increases in college education had external effects between 3% and 9%. Using compulsory attendance and child labor laws as instruments for years of high school per worker and proximity to land-grant colleges and nationality-based immigration of college graduates as instruments for years of college per worker across U.S. states, we are able to empirically estimate these effects. The empirics confirm the insignificant external effect of increased high school education and large positive effects of increased college education on TFP. Both the model and the empirical results reconcile the mixed findings on human capital externalities previously found in Acemoglu and Angrist (2000), Moretti (2004), and Ciccone and Peri (2006).

[20] We use GSP in 1963 to proxy for GSP in 1960, and that in 1997 to proxy for 2000 GSP.

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BEGGAR THY NEIGHBOR? THE IN-STATE, OUT-OF-STATE, AND AGGREGATE EFFECTS OF R&D TAX CREDITS

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Abstract—The proliferation of R&D tax incentives among U.S. states in recent decades raises two questions: (i) Are these tax incentives effective in increasing in-state R&D? (ii) How much of any increase is due to R&D being drawn away from other states? This paper answers (i) "yes" and (ii) "nearly all." The paper estimates an augmented R&D factor demand model using state panel data from 1981 to 2004. I estimate that the long-run elasticity of in-state R&D with respect to the in-state user cost is about −2.5, while its elasticity with respect to out-of-state user costs is about +2.5, suggesting a zero-sum game among states.

I. Introduction

Over the past two decades, R&D tax credits offered by U.S. states have become widespread and increasingly generous. This phenomenon is illustrated in figure 1, which plots from 1981 to 2006 both the number of states offering R&D tax credits and the average effective credit rate among those states.[1] The process began with Minnesota in 1982, one year after the introduction of the U.S. federal R&D tax credit. As of 2006, 32 states provided a tax credit on general, company-funded R&D, and the average effective credit rate has grown approximately fourfold over this period to equal roughly half the value of the federal effective credit rate.[2,3] In fact, a number of states' R&D credits are considerably more generous than the federal credit.

The proliferation of state R&D credits raises two important questions. First, are these tax incentives effective in achieving their stated objective, to increase private R&D spending within the state? Second, insofar as the incentives do increase R&D within the state, how much of this increase is due to drawing R&D away from other states? This latter question is particularly important given recent U.S. court decisions on the constitutionality of state business credits (discussed further in section V).

There has been surprisingly little empirical research on either of these questions. Most work on R&D tax incentives has investigated the effectiveness of the federal R&D credit. Studies in this area generally follow the approach of estimating the elasticity of R&D with respect to its price (user cost), and exploiting panel data variation across firms, industries, or countries.[4] These studies, which generally find a statistically significant R&D cost elasticity at or above unity, are frequently cited in debates over the efficacy of state R&D credits.

It is not at all clear, however, that inferences based on existing firm-, industry-, or country-level data, which report only nationwide R&D expenditures for the unit of observation, can be extended to the state level. R&D may be mobile across states so that the cost of R&D in other states can affect how much R&D is performed in any one state. Thus, the "net" or "aggregate" R&D elasticity with respect to the cost of R&D, for a given state, is actually the difference between (the absolute value of) the elasticity with respect to the cost of performing R&D within the state and the elasticity with respect to the cost of performing R&D outside of the state.

This paper addresses the two questions posed above by estimating an augmented version of the standard R&D factor demand model using a two-way fixed-effects estimator with state panel data from 1981 to 2004. An appealing aspect of using state-level information to identify the elasticities of R&D with respect to in-state and out-of-state costs is that state-level variation in the user cost of R&D is driven entirely by variation in R&D tax credits and corporate income tax rates, both of which are arguably exogenous to firms' contemporaneous R&D decisions.[5]

II. Data

State and federal R&D credits offer corporations credits against income tax liability based on the amount of qualified research done by the corporation within the state or country, respectively. U.S. states generally follow the federal Internal Revenue Code (IRC) definition of qualified research: the wages, materials expenses, and rental costs

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[1] The effective credit rate corresponds to the k^e_{it} term defined in section II.

[2] The statutory rate for the U.S. federal R&D tax credit is 20%. However, since the credit itself is considered taxable income, the effective credit rate is 20%(1−0.35) = 13%, using 0.35 as the corporate income tax rate.

[3] The sizable jump in the average credit rate in 1990 was because many states piggyback on the federal definition of the R&D base amount (explained in section II below), and this was changed in 1990 from a moving-average base to a fixed-period base, which increases the effective credit rate.

[4] See, for example, Hall (1993), Swenson (1992), and Berger (1993) for firm-level studies; Baily and Lawrence (1995) and Mamuneas and Nadiri (1996) for industry-level studies; and Bloom, Griffith, and Van Reenen (2002) for a country-level study.

[5] This approach of using tax code changes as natural experiments has been employed in the investment literature (see, for example, Cummins, Hassett, & Hubbard, 1994).

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