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Workers' Education, Externalities and Technology Adoption: Evidence From Plant-Level Production Functions*

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Abstract

Previous literature has attempted to identify human capital externalities by examining the relationship between a city's average wage and it's overall level of human capital. In this paper I propose a more direct approach. Using a unique worker–firm matched dataset, obtained by combining the Census of Manufacturers and the Census of Population, I focus on the productivity of manufacturing firms. I start by documenting a positive correlation between the productivity of manufacturing establishments in a given city and the average education outside the establishment in the same city. Further, I find that the external effect of education on productivity is larger for those plants in which the share of skilled workers is larger, suggesting that human capital externalities are more important in human–capital–intensive productions. I consider two explanations. The first is the presence of omitted variables that may raise both productivity and education. To investigate this possibility I exploit the longitudinal structure of the data, finding little evidence that omitted variables play a major role. A second explanation is that in cities with a better–educated labor force plants tend to be equipped with better technology. This explanation is consistent with a model in which learning takes place among workers, as suggested by several contributions to the theoretical literature. I find more support for the second hypothesis. In plants that are situated in cities with higher average education, both investment in computers and the fraction of new machinery to the total stock of machinery are larger, after controlling for a plant's characteristics. Furthermore, within a given city, the investment in computers in a particular plant is positively associated with the number of workers who use computers outside the plant.

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1 Introduction

Knowing the magnitude of the social return to education—the full set of benefits that education provides—is crucial for assessing the need for and effectiveness of public education policies. Most of the empirical research on the subject has been devoted to estimating the *private* return to education, i.e. the causal effect of schooling on an individual's earnings (Card 1999).

However, if education generates externalities, the private return underestimates the total benefits of education. Human capital externalities may arise if the presence of educated workers makes other workers more productive. For example, biotechnology firms are more productive if located near large research universities (Zucker, Darby & Brewer 1998). Similarly, patent citations are more likely to come from the same state or metropolitan area as the originating patent (Jaffe, Trajtenberg & Henderson 1993). But externalities may not be limited to high-tech industries. The adoption of new technologies among Indian farmers has been shown to depend not only on the farmer's human capital, but also on his neighborhood's adoption, so that farmers do not fully incorporate the social return to learning in making new adoption decisions (Foster & Rosenzweig 1995). Borjas (1995) finds that the economic success of the children of immigrants is a function not only of their parents' human capital, but also of the average human capital in the relevant ethnic group.

Despite significant policy implications, there is little systematic empirical evidence on the magnitude of the social return to education. Only recently have some papers attempted to estimate the size of the social return to education by comparing the wages of otherwise similar individuals who work in cities or states with different average levels of education (Rauch 1993, Moretti 1999, Acemoglu & Angrist 1999). The idea is that wage differences across local labor markets reflect productivity differences, which can in turn be explained by variation in the overall level of human capital.

In this paper I take a more direct approach to human capital externalities by focusing on the productivity of manufacturing plants. If externalities exist, we should find that plants located in areas with a high level of human capital can produce a greater output with the same inputs, or the same output with less inputs than otherwise similar plants located in areas with a low level of human capital. I document a positive correlation between the productivity of manufacturing establishments in a city and the average level of education there, after controlling for establishment characteristics. I consider two explanations. First, I investigate the presence of city–specific or establishment–specific unobserved factors that may affect both productivity and education in a given area. I address this possibility by exploiting the longitudinal structure of the data, and find little evidence that omitted variables play a major role.

I then investigate the possibility that plants located in cities with a better–educated labor force have better technology. This explanation is consistent with a model where learning takes place among workers, and more highly–skilled workers are more open to new technologies.[1] I find more support for the second hypothesis: investment in new technologies like computers is larger in plants located in cities with a higher average education.

I use a unique firms–workers matched dataset, obtained by combining the Census of Manufacturers with the Census of Population. The Census of Manufacturers contains detailed longitudinal information on *all* manufacturing firms in the US. I augment plant–level information from the Census of Manufacturers with information from the Census of Population on the distribution of education across metropolitan areas and industries.

To assess the magnitude of externalities, I estimate production functions where the inputs include the average education in the city within and outside the industry, as well as labor and capital. By focusing on productivity, instead of wages, I can test directly for the presence of externalities. In the absence of externalities, the level of education outside a plant should have no effect on productivity in the plant.

The key econometric issue in comparing firms across metropolitan areas with different levels of average education is the possible presence of unobserved factors that may raise productivity and attract a more educated labor force. The previous literature has used instrumental variables to address this problem (Moretti 1999, Acemoglu & Angrist 1999). In this paper, I exploit the panel structure of the data to abstract from permanent differences across plants and cities. By looking at how changes in average education outside a plant affect changes in productivity within the plant, I can control for all permanent factors that affect both productivity and

[1] Bartel & Lichtenberg (1987) show that more educated workers are have a comparative advantage in implementing new technologies. Knowledge diffusion plays a key role in many theories of firm–level dynamics and economic growth. Marshall (1890) is among the first to recognize that social interactions among workers create learning opportunities that enhance productivity. Lucas (1988) argues that long–run growth differences across countries may be explained by human capital externalities in the form of learning spillovers. In the typical learning model, individuals augment their human capital through pairwise meetings with more skilled neighbors during which they exchange ideas (Glaeser 1997, Jovanovic & Rob 1989).

average education and that might bias a simpler cross-sectional specification. To lessen any fear that average education may be endogenous because of transitory unobserved factors, I exploit the clustered structure of the data. I include state×year effects to absorb any unobserved time-varying characteristics of the state that could affect productivity and average education, such as public spending on infrastructure and education. I also include industry×year effects to absorb any differential trend in productivity across industries.

The results show that increases in the average education of workers employed outside the plant have a significant positive effect on productivity within the plant. Importantly, the effect is larger for plants where the share of skilled workers is large, suggesting that human capital externalities are more important for productions that are human–capital intensive. As one might expect, the effect also varies depending on the plant's connection with its local labor market. The effect is large for single-unit plants, and virtually zero for multi-unit plants.

Having addressed the issue of omitted variables, I turn to the hypothesis that technology is better in cities where the overall level of education is higher. Two measures of technology are available in the Census of Manufacturers: the level of investment in computers and the fraction of new machinery to the total stock of machinery. Results suggest that the average education in a local labor market outside a plant affects technology adoption inside the plant. Plants located in areas rich in human capital tend to invest more in computers and have a higher percentage of new machinery. Moreover, a plant's investment in computers is positively associated with the number of workers who use computers in the city outside the plant. Importantly, other types of investment that do not embody new technologies—such as investment in automobiles and trucks—do not seem to be correlated with average education.

The remainder of the paper is organized as follows. I start by presenting a simple theoretical framework (section 2). I then describe the matched Census of Manufacturers–Census of Population dataset (section 3). Section 4 presents estimates of the external effect of education on productivity, section 5 deals with the issue of measurement error, while section 6 adds separate estimates of the external effect for high–tech and low–tech plants. Finally, section 7 presents evidence on technology adoption. Concluding remarks are in section 8.

2 Theoretical Framework

Let's assume that technology can be described by a Cobb-Douglas production function:

$$y_{pjct} = A_{pjct} L^{\alpha}_{pjct} K^{\beta}_{pjct} \tag{1}$$

where y_{pjct} is added value (value of shipments - cost of materials) of plant p, belonging to industry j, in city c in year t; L_{pjct} is a quality of labor aggregate; K_{pjct} is capital; and A_{pjct} is a residual.[2] Workers differ in their skill level. Suppose that there are Z possible education levels. I assume that in the quality of labor aggregate, workers with different levels of education are perfectly substitutable inputs with different marginal products:

$$L_{pjct} = \theta_1 H_{1pjct} + \theta_2 H_{2pjct} + \ldots + \theta_Z H_{Zpjct} \tag{2}$$

where H_{zpjct} is the number of hours worked by individuals in education group z and θ_z is an education–group–specific productivity shifter that accounts for differences in human capital among workers:

$$\theta_z = exp(\delta S_z) \tag{3}$$

where S_z is the years of schooling of workers in group z and δ is the private return to education, generally believed to be about 0.08-0.1.

Up to this point, the specification is standard. (See, for example, Griliches (1970).) To introduce the externality in the model, I assume that the residual A_{pjct} is a function of average education outside the firm, as well as of unobservable factors at the plant, industry, city and year level:

$$\ln A_{pjct} = \gamma S_{-pjct} + \epsilon_p + \epsilon_j + \epsilon_t + \epsilon_{pc} + \epsilon_{jt} + \epsilon_{pjct} \tag{4}$$

[2] An alternative to the value added specification is a production function where the value of output is a function of labor, capital and materials. This specification is problematic given the potential endogeneity of materials. A second advantage of the value–added specification is that it can be derived from polar production functions: one in which the elasticity of substitution between materials and value–added in infinite; and one in which this elasticity of substitution is zero (Hellerstein, Neumark & Troske 1999). Hellerstein et al. (1999) experiment with both the value added specification and with a production function where materials are on the right hand side, and instrumental variables are used to account for endogeneity of materials. Results are similar in the two specifications.

where S_{-pjct} is the average education in all manufacturing plants in the city with the exception of plant p.[3] A simple test for the presence of externalities is to test whether γ is different from zero. In the absence of externalities from education, $\hat{\gamma}$ should be zero. If average education in a local labor market generates positive externalities—so that a rise in average education of the workforce outside the plant raises productivity in the plant—then $\hat{\gamma} > 0$.

The ϵ's are unobservable factors, potentially correlated with S_{-pjct}. In particular, ϵ_p captures unmeasured plant characteristics that affect productivity and do not change over time, such as the quality of machines, patents, management quality, and the culture within the firm, etc. It is possible that more productive firms are located in areas with a higher level of average education, for reasons independent of human capital externalities. The term ϵ_j captures fixed industry characteristics; ϵ_{pc} captures all permanent factors that make a plant-city match particularly productive, such as a harbor, a railway, the weather, a city's institutions, and the presence of a research university, etc. Note that since a plant's location is fixed, plant fixed effects absorb both ϵ_p and ϵ_{pc}. However, the specification in equation 4 makes explicit the fact that firms are not randomly distributed across cities. Just as workers in a standard Roy model locate in cities where their characteristics are best rewarded, so firms locate in cities that maximize their productivity. Ignoring unobservable factors that make a plant-city match productive may bias the results. Finally, the terms ϵ_t and ϵ_{jt} represent unobservable technological shocks at the national and industry level, respectively; and ϵ_{pjct} is idiosyncratic noise.

In logs, the production function becomes

$$\ln y_{pjct} = \gamma S_{-pjct} + \alpha \ln L_{pjct} + \beta \ln K_{pjct} + \epsilon_p + \epsilon_j + \epsilon_t + \epsilon_{pc} + \epsilon_{jt} + \epsilon_{pjct} \tag{5}$$

Inserting equation 2 into 5 and using a first–order Taylor approximation, equation 5 can be rewritten as

$$\ln y_{pjct} \approx \gamma S_{-pjct} + \alpha\delta S_{pjct} + \alpha \ln H_{pjct} + \beta \ln K_{pjct} + \epsilon_p + \epsilon_j + \epsilon_t + \epsilon_{pc} + \epsilon_{jt} + \epsilon_{pjct} \tag{6}$$

where S_{pjct} is the average number of years of schooling among workers in the plant and

[3] An alternative is to define S_{-pjct} as the average education in *all* plants in the city with the exception of plant p. The reason for choosing average education in manufacturing over average education in all industries is that workers are likely to interact more with other workers in industries that are "close" than in industries that are very different. Average education in manufacturing is thus better suited to capture externalities for manufacturing workers than average education in all industries.

$H_{pjct} = \sum_z H_{zpjct}$ is total number of hours worked in the plant.[4]

In this paper, variants of equation 6 are estimated. The coefficient of interest is γ. The longitudinal structure of the data is exploited in order to absorb permanent unobserved heterogeneity. In particular, by including plant, industry and year effects, the terms ϵ_p, ϵ_j, ϵ_t and ϵ_{pc} are absorbed. Industry×year and state×year effects are also included, in order to abstract from unobservable factors that may affect changes in both productivity and average education.

A limitation of this framework is the assumption that workers with different skill levels have different marginal product but are perfectly substitutable. This assumption can be relaxed by separating labor inputs along occupational lines. In particular, production workers, M, and nonproduction workers, N, can enter the production function separately: $y_{pjct} = A_{pjct} M^{\alpha}_{pjct} N^{\eta}_{pjct} K^{\beta}_{pjct}$. Equation 14 then becomes

$$\ln y_{pjct} \approx \gamma S_{-pjct} + (\alpha+\eta)\delta S_{pjct} + \alpha \ln H^M_{pjct} + \eta \ln H^N_{pjct} + \beta \ln K_{pjct} + \epsilon_p + \epsilon_j + \epsilon_t + \epsilon_{pc} + \epsilon_{jt} + \epsilon_{pjct} \quad (13)$$

where H^M_{pjct} and H^N_{pjct} are the hours worked by production and nonproduction workers respectively.

One worry is that the establishment–specific productivity shocks, ϵ_{pjct}, may not be idiosyncratic, but may persist over time. This could be captured by allowing the component of the

[4] To see that equation 5 can be approximated by 6, consider a first order Taylor approximation of equation 2 around $\theta_1 = 1, \theta_2 = 1, \theta_3 = 1, ...\theta_Z = 1$:

$$\begin{aligned} \ln L_{pjct} &= \ln(\sum_z \theta_z H_{zpjct}) && (7) \\ &\approx \ln(\sum_z H_{zpjct}) + \sum_z \frac{H_{zpjct}}{H_{pjct}}(\theta_z - 1) && (8) \\ &\approx \ln(\sum_z H_{zpjct}) + \sum_z \frac{H_{zpjct}}{H_{pjct}} \ln \theta_z && (9) \\ &= \ln H_{pjct} + \delta S_{pjct} && (10) \end{aligned}$$

where $\frac{H_{zpjct}}{H_{pjct}}$ is the fraction of workers in the plant who belong to education group z. To see that equation 9 is indeed an approximation of 8, consider a first order Taylor expansion of $\frac{H_{zpjct}}{H_{pjct}} \ln \theta_z$ around $\theta_z = 1$:

$$\begin{aligned} \frac{H_{zpjct}}{H_{pjct}} \ln \theta_z &\approx \frac{H_{zpjct}}{H_{pjct}} \ln 1 + \frac{H_{zpjct}}{H_{pjct}}(\theta_z - 1) && (11) \\ &= 0 + \frac{H_{zpjct}}{H_{pjct}}(\theta_z - 1) && (12) \end{aligned}$$

Equation 10 is obtained by inserting the definition of θ_z from equation 3 into 9.

error term to be serially correlated: $\epsilon_{pjct} = \rho\epsilon_{pjct-1} + u_{pjct}$. The presence of a serially correlated error term can be a problem when using data with observations in consecutive years. But the presence of a serially correlated error term is unlikely to pose a major problem for the results presented here, because the empirical analysis of this paper uses observations that are ten years apart. Most of any serially correlated shock is likely to have disappeared after ten years.

The specification of equation 6 is similar to the one that has been used to estimate the social return to public infrastructure, such as roads and other form of public capital. The typical model incorporates public capital expenditures at the state or metropolitan area level into the traditional production function as an exogenous input along with labor and private capital (see, for example, Eberts (1986)).

3 The Matched Workers–Plants Sample

In this paper, I estimate externalities from education that arise in local labor markets. The definition of a local labor market I adopt here is that of metropolitan area. Although other definitions of labor markets are possible, metropolitan areas have the advantage of being economic units more homogeneous than states or countries.[5]

The data used in this study come from a match between plant records from the Census of Manufacturing and worker records from the Census of Population. The Census of Manufacturers is a longitudinal dataset that covers the universe of manufacturing establishments with one paid employee or more. The unit of observation is the plant. A company operating at more than one location is required to file a separate report for each location. I use the Census of Manufacturers data for 1982 and 1992. Two important advantages of the Census of Manufacturers are its panel structure, allowing for the absorption of permanent unobserved heterogeneity that could bias simpler cross-sectional specifications, and a sample size large enough to allow a disaggregation of the data by metropolitan area.

Although the Census of Manufacturers contains detailed information on plants' inputs and outputs, the number of years of schooling of workers is not reported. For the data on education I had to turn to the 1980 and 1990 Censuses of Population. I match the Census of Manufacturers with the Census of Population using narrowly defined city–industry cells. I assign each plant

[5]Previous literature has focused on metropolitan areas (Rauch 1993, Moretti 1999) or states Acemoglu & Angrist (1999).

in the Census of Manufacturers and each worker in the Census of Population to a city–industry cell based on the metropolitan area code and a 3-digit industry definition. For each city and industry, I then calculate the average education both within and outside the industry. The average years of schooling outside the plant, S_{-pjct}, and inside the plant, S_{pjct} are thus approximated by the average years of schooling outside the industry, S_{-jct}, and inside the industry, S_{jct}, respectively. Equation 6 becomes

$$\ln y_{pjct} \approx \gamma S_{-jct} + \alpha\delta S_{jct} + \alpha \ln H_{pjct} + \beta \ln K_{pjct} + \epsilon_p + \epsilon_j + \epsilon_t + \epsilon_{pc} + \epsilon_{jt} + \epsilon_{pjct} \tag{14}$$

The matching is not perfect, because in reality there is often more than one plant in each city-industry cell. However, the matching is correct *on average*, as average education in each cell is an unbiased estimator of education in the plant. The Data Appendix provides a detailed description of the two samples and the matching algorithm.

The matched sample is a balanced panel with 40,281 plants. Descriptive statistics for the matched sample are reported in table 1. Value added is derived by subtracting the total cost of materials (including materials, supplies, fuel, electric energy, cost of resales, and contract work) from the value of shipments (including resales) and other receipts, and adjusting the resulting amount by the next change in finished products and work-in-progress inventories between the beginning and the end of the year. The average plant added value is about $9.4 million in 1992.[6] Capital is measured as the sum of the end–of–year book value of buildings and machinery.[7] The average plant has 73 production workers and 37 non-production workers in 1982. The corresponding figures for 1992 are slightly smaller. The number of hours worked by production workers is stable in the two years, while the numbers of hours worked by non-production workers is larger in 1992 than in 1982.[8] The average hourly wage in the plant, obtained by dividing the total wage bill by the total number of hours worked, is $13.7 in 1992. By comparison, the average wage among manufacturing workers in the 1992 March Current Population Survey is 14.5.

[6] All monetary figures are in 1992 dollars.

[7] Because capital is measured by the original cost of purchase, it may contain measurement error.

[8] For production workers, both the number of workers and the number of hours worked is reported in the Census of Manufacturers. For non-production workers, the number of workers is known, but the number of hours worked is not reported. The number of hours of non-production workers is imputed by assuming that production and non-production workers in the same plant work the same number of hours per capita.

The multi–unit dummy indicates whether a plant belongs to a firm with more than one establishment. Multi–unit plants tend to be significantly larger than single-units ones. About a quarter of all plants are part of multi–unit firms. The fraction of new machinery to the total stock of machinery is the proportion of machinery bought in the previous five years. It is used as a measure of technological change. As plants in the sample age, this fraction falls from 0.37 in 1982 to 0.26 in 1992.

The bottom panel of table 1 reports summary statistics for the Census of Population variables. The average education in the industry is defined as the mean years of schooling of workers employed in the city and 3-digit industry the plant belongs to. The average education outside the industry is defined as the mean years of schooling of workers employed in the same city in all other manufacturing industries. A weighted average of average education within and outside the industry gives the average education of manufacturing workers in the city.

Since the true average education in each cell is unobserved, and I have to rely on estimates, measurement error is a concern. The last two rows in table 1 refer to the size of the city–industry cells. After eliminating cells with less than 10 workers, the average industry-city cell has 387 workers in 1990 (the maximum cell size being 9,655). The number of workers in each city outside the industry is much larger, averaging 12,848 in 1990 (the minimum being 118, and the maximum 44,710). These cell sizes suggest that the observed average education outside the industry is probably close to its true value, because it is estimated in samples that on average have almost 13,000 observations. On the contrary, average education within the industry is estimated in samples that are on average more than 30 times smaller and is likely to be noisier. Because measurement error has a known variance, its effect can be accounted for (see section 5).

In order to asses the quality of the matching, I have estimated plant–level wage equations. Hellerstein et al. (1999) have shown that plant–level wage equations represent the aggregation of individual–level wage equations over workers employed in a plant and hence should provide coefficients similar to the ones obtained from their individual–level counterparts. Although the focus of this paper is not on wages, plant–level wage equations provide an indirect test of the quality of the matching between the Census of Manufacturers and the Census of Population. If the matching is correct and measurement error is not too large, one would expect wage equation coefficients to be close to the ones usually found in the literature.

The estimated wage equations are shown in table A1 in the Appendix. Data on wages, from the Census of Manufacturers, are plant averages obtained by dividing the total wage bill by the number of hours worked. Data on workers are cell averages from the Census of Population. For example, "percentage female" is the fraction of women in the industry and city the plant belongs to. The coefficients in table A1 are similar to the ones found in the literature based on individual level regressions and the ones found Hellerstein et al. (1999), based on a plant–level regression. The coefficients on average education in the industry are 0.078 and 0.086 for 1992 and 1982, respectively. These coefficients are smaller but not qualitatively different from the standard cross–sectional estimates of the private return to education in the literature, although the 1992 estimates are usually found to be larger than the 1982 ones. Women and blacks are paid less, and older workers more, as expected. From this table I conclude that the matched sample contains some measurement error, but can reproduce standard individual level wage equation results.

4 Plant–Level Estimates of Production Functions

4.1 Cross–Sectional Estimates

Table 2 reports cross–sectional estimates of plant–level production functions. Columns 1 and 3 refer to a technology where workers with different skills are perfectly substitutable inputs with different marginal product depending on their education level (equation 6); column 2 and 4 refer to a technology where production and nonproduction workers are imperfect substitutes (equation 13). All models include capital, labor, a dummy equal one if the plant belongs to a multi–unit firm, industry dummies, and time–varying city characteristics to capture local labor market conditions that may be correlated with average education and productivity, including the percentage of immigrants, blacks, and females in the labor force. All labor inputs are measured in number of hours worked. The coefficient on average education outside the industry is about 0.06 and 0.045 in 1992 and 1982 respectively. The coefficient on average education within the industry is 0.039 in 1992 and 0.026 in 1982.

The fact that the coefficient on average education outside the industry is larger than the coefficient on average education within the industry does not imply that the private return to education is smaller than the external return to education. The coefficient on average education within the industry—which is $(\alpha+\eta)\delta$ in equation 13—is not directly comparable to the private return to education. First, the private return to education is δ, not $(\alpha + \eta)\delta$ (section 2). Second, the coefficient on average education within the industry is biased downward because of measurement error. The discussion in section 3 has suggested that measurement error in average education within the industry may be large, because average education within the industry is estimated using small city$\times$industry cells. On the contrary, average education outside the industry is precisely estimated and attenuation bias is small. The impact of measurement error is investigated in detail in section 5.

The sum of the coefficients on labor and capital is not statistically different from 1, suggesting that a constant return to scale can not be rejected. Finally, plants that are part of a multi–establishment corporation are more productive than similar one-unit plants. Interestingly, the coefficient on the multi-unit dummy raises from about 0.059 in 1982 to around 0.163 in 1992, suggesting that the importance of multi-unit status has more than doubled during the 1980s.

4.2 Longitudinal Specification

The coefficients on average education outside the industry may be biased if there are unobserved plant or city–specific factors that raise both average education and productivity in a city. In this section, I control for some of these unobservable factors by exploiting the longitudinal structure of the data. Table 3 reports estimates from specifications that include plant fixed effects. Plant fixed effects purge estimates of *permanent* plant and city unobserved factors that raise productivity and attract a better–educated labor force, and may bias cross-sectional estimates. Identification comes from *changes* in productivity and average education between 1982 and 1992.

Column 1 of table 3 refers to a specification in which workers are perfectly substitutable inputs. The coefficient on average education outside the industry is now 0.052, suggesting that a one year increase in average education outside the plant increases productivity inside the plant by 5.2%. The coefficient in column 2, where production and nonproduction workers are imperfect substitutes, is 0.057, slightly higher but statistically indistinguishable. The coefficient on average education within the industry is lower than the corresponding cross–sectional coefficient, because attenuation bias increases when plant fixed effects are included.[9]

The next two columns of table 3 relax some of the restrictions on the form of the production function. In the specification used in columns 1 and 2, the intercept of the production function is allowed to vary across plants, but the slope coefficients are constrained to be the same for all plants. In reality, however, it is possible that the relative importance of capital and labor varies across industries. If technology varies across industries, estimates in columns 1 and 2 are mispecified. To test whether a less restrictive production function yields different estimates, I allow the slope coefficients on capital and labor to vary by 2-digit industry. In particular, I estimate the following production function:

$$\ln y_{pjct} = \gamma S_{-jct} + (\alpha_j + \eta_j)\delta S_{jct} + \alpha_j \ln H^M_{pjct} + \eta_j \ln H^N_{pjct} + \beta_j \ln K_{pjct} + d_p + d_t + \epsilon_{pjct} \quad (15)$$

where d_p and d_t are plant and year dummies, respectively.[10] Column 3 shows that the estimates of equation 15 are similar to the ones obtained from the more restrictive specification that impose an equal slope across industries.

In column 4, the assumption of Cobb-Douglas technology is relaxed and a more general Translog production function is estimated. The coefficient on average education outside the industry is lower, but not statistically different from the one obtained from the corresponding Cobb-Douglas specification.

The results presented in tables 2 and 3 are based on a selected sample of plants that are observed both in 1982 and 1992. Not surprisingly, plants in the selected sample are significantly larger than plants in the population.[11] It is possible that the estimates based on the balanced longitudinal sample are different from population estimates. To investigate the magnitude of selection bias in the longitudinal sample, I re-estimate the model conditional on the predicted probability of selection.

I first take the population of manufacturing firms in 1982 and estimate the probability of selection by fitting a probit model where the dependent variable is a dummy equal 1 if the plant

[9]It is well known that in specifications that include fixed effects, the signal–to–noise ratio decreases if measurement error is classical (Griliches 1986). Krueger & Lindahl (1998) show that this can have severe consequences on the estimated coefficients in regressions based on macro data.

[10]Year effects absorb national shocks in productivity, such as the business cycle, technological progress, etc.

[11]For example, average number of workers for all plants in 1992 is 47.7 (104.4 in matched sample), value added is \$ 3,727,000 (\$9,412,000 in matched sample) and capital stock is \$ 3,145,000 (\$8,007,000 in matched sample).

exists both in 1982 and 1992 and no relevant variables are missing. The independent variables include plant size, a dummy equal one if the plant existed in 1972, and industry and state dummies. As one might expect, larger and older plants are more likely to be observed both in 1982 and 1992.[12] I then divide the sample of selected plants according to the probability of selection. Table 4 reports separate estimates of production function for plants with small, medium and high probabilities of selection. The three groups have similar size.[13] Only the coefficients on average education are reported in the table. The coefficient on average education outside the industry is large for plants with high and low probability of selection, and low for plants with medium probability of selection. Although the coefficient varies depending on the probability of selection, no clear pattern emerges that suggests that sample selection biases the estimates in one direction or another.

4.3 Unobserved Transitory Productivity Shocks

The specifications in table 3 are robust with respect to permanent plant and city heterogeneity. But the fixed effects estimator may still be biased if there are transitory unobserved factors that affect both changes in average education and changes in productivity.

Some of these transitory factors, however, do not pose significant problems when the analysis is limited to manufacturing plants. For example, the variation in cost of living across cities may be a problem for industries that produce non–tradable goods, if prices of non-tradable goods tend to be higher in cities where both the cost of living and average education are higher. But heterogeneity in cost of living is not a problem when looking only at manufacturing, because manufacturing output is traded and its price determined in the national or international market.

Since plant location can not be changed, it is likely that location decisions are more based on the permanent characteristics of an area than on transitory factors. When plant fixed effects are controlled for, most unobserved heterogeneity that may attract more productive plants to an area with more education and cause spurious correlation is likely to be absorbed.

To lessen any fear that the fixed effect is biased by time–varying unobserved heterogeneity, I exploit the clustered nature of the data. In column 1 of table 5, I include state×year effects

[12] The coefficient on plant size, as measured by 1982 number of employees is 0.00026 (0.00001); the coefficient on the dummy equal one if the plant existed in 1972 is 0.424 (0.006).

[13] The specification adopted is the one where technology is Cobb-Douglas and production and non–production workers are imperfect substitutes (column 2 of table 3).

that absorb all state specific transitory shocks. For example, it is possible that states that invested more on public infrastructures during the 1980s also spent more on public education and training, thus raising both human capital and the firms' productivity. Similarly, it is possible that some parts of the South, where productivity was low at the beginning of the 1980s for historical reasons, caught up with the rest of the country during the 1980s, and that this modernization process itself attracted a better educated labor force. This type of transitory unobserved shock would bias the fixed effects estimates of table 3 upward.

The specification that includes state×year effects yields consistent estimates if transitory productivity shocks are similar across cities in the same state. Identification comes from changes in productivity and average education across cities in the same state. State×year effects have virtually no effect on estimates, suggesting that state–specific productivity shocks do not play a significant role in explaining the relation between productivity and average education.

The specification in column 2 allows for industry–specific transitory shocks. It is likely that productivity changes differ across industries, due to different rates of technological progress and other industry–specific unobservable factors. By including industry×year effects, this potential source of bias is absorbed. In column 2, the coefficient on average education outside the industry is 0.044.

The specification in column 3 pushes identification to the limit by including city×year effects. The coefficient on education outside the industry becomes implausibly large, and very imprecise. The reason is that when city×year effects are included, almost all variation in education outside the industry is absorbed. The model is identified only because cell size varies across industries.

Columns 4 to 6 of table 5 generalize the specification of columns 1 and 2 to a Translog production function. When state×year effects are included in column 3, the coefficient on average education outside the industry drops to 0.031, but it is not statistically different from the corresponding coefficient in column 1. When industry×year effects are included in column 4, the coefficient on average education outside the industry is virtually the same as the one in column 2.

I conclude that during the 1980's, plants located in areas where the level of education of the workforce increased became more productive than similar plants located in areas where the level of average education had stagnated. This increased productivity does not seem to be spurious because it remains significant when state– and industry–specific transitory shocks are controlled for. According to the estimates in table 5, an increase of one year in average education outside the industry is associated with a productivity increase between 3.9% and 5.1%. This productivity increase occurred over a ten year period. To help interpreting the magnitude of the coefficient, it should be considered that the mean increase in S_{-jct} between 1982 and 1992 was only 0.42 years. An increase in average education of 0.42 years would have caused an increase in productivity between 1.5% and 2.1% over a ten–year period.

4.4 Single–Unit and Multi–Unit Plants

In this section, I investigate how the external effect of average education on productivity varies by type and size of plant. In particular, I am interested in whether there is any difference between single-unit plants and plants that belong to large multi–establishment firms. Single–unit plants are likely to be more sensitive to the level of human capital in the local labor market than large firms with establishments in several locations. For example, it is likely that many decisions that affect the productivity of a General Motors establishment in, say, St. Louis are made in Detroit. I therefore expect that the changes in the education of the work force in a local labor market have a larger impact on the productivity of small single–unit plants than they do on large multi–establishment corporations.

Columns 1 and 2 of Table 6 report estimates of the productivity effect of changes in average education by multi-unit status. The specification refers to a Cobb-Douglas technology where production and nonproduction workers are imperfect substitutes, and includes plant and year effects.[14] Multi–unit plants are larger than single–unit ones. The average multi–unit plant has 309 employees, while the average single-unit plant has only 31. The coefficient on average education outside the industry is 0.077 for single–unit plants and virtually zero for multi–unit plants. This confirms that the productivity of establishments that are part of larger corporations is less sensitive to the level of human capital in local labor markets than is the productivity of single-unit plants.

Columns 3, 4 and 5 of Table 6 report separate estimates for small, medium and large plants. Plants are divided into three groups of equal size depending on whether their labor force is 13

[14] Coefficients on hours of production and nonproduction workers and capital are not reported, but are available on request.

or less, between 14 and 44, or 45 or more. Estimates in the first row of columns 3, 4 and 5 display a sharp contrast between small and medium plants on one side, and large firms on the other. The coefficient on average education outside the industry is large for small and medium plants, but drops to virtually zero for large plants. These results seem consistent with the notion that smaller firms are more sensitive to local conditions than larger ones are.

4.5 A Specification Test: Does Average Physical Capital Matter as Well?

In previous sections I have shown that increases in the overall level of human capital in a city are associated with increases in productivity. In this section I provide an indirect specification test by repeating the analysis substituting average human capital outside the industry with average physical capital outside the plant. If the results in the previous sections are spurious, or if they can be explained by agglomeration effects other than human capital externalities, then average physical capital outside the plant may enter the plant production function significantly. On the contrary, if the results in the previous sections are capturing only human capital externalities, there is no reason why physical capital in one plant should affect productivity in other plants.

Table 7 reports estimates of the same specification used in column 2 of table 3. Two measures of physical capital are used. In row 1 I include the log of average physical capital outside the plant. In row 2 I include the log of average capital *per worker* outside the plant. Results in rows 1 and 2 are obtained from two separate regressions.

Cross-sectional estimates in columns 1 and 2 suggest that average capital has a significant effect on productivity, although the signs in 1982 and 1992 are the opposite. When plant fixed effects are included, however, the coefficient on average capital becomes insignificant, suggesting that plant-level heterogeneity may be biasing cross-sectional results. When state$\times$year effects are added, the coefficients drop to virtually zero. These results confirm that physical capital outside the firm does not have an effect on productivity similar to the one generated by human capital. They are consistent with a model where externalities from education arise from workers' learning and interactions, as opposed to a model where average education captures standard agglomeration effects.

5 Accounting for Measurement Error

Estimates of average education within the industry are unbiased and consistent estimates of average education within the plant. But in finite samples, these estimate may differ from the true level of average education. In section 3, it was pointed out that the estimates of average education within an industry are obtained from small city×industry cells and may contain noise. (The average city×industry cell size in 1990 is 387). On the contrary, estimates of average education outside an industry are significantly more precise, because they are obtained from cells that are on average more than thirty times larger.

Measurement error in average education within the industry has two effects. First, it biases down the coefficient on average education within the industry. This is the standard attenuation bias. In all the regression presented so far the estimated coefficient on average education within the industry is quite low, partially because of attenuation bias. This type of bias is not a primary concern here, because the coefficient on average education within the industry is not the coefficient of interest.

Second, and more importantly, the coefficient on average education outside the industry may pick up some measurement error in average education within the industry, and thus may be biased upward. How large this source of bias is depends on the correlation between average education inside and outside the industry, and on the variance of measurement error. Consider a simplified version of equation 14:

$$\ln y_{pjct} = \gamma_1 S_{-jct} + \gamma_2 S S^*_{jct} \tag{16}$$

Assume that observed average education within the industry, S_{jct}, is equal its true value, S^*_{jct}, plus classical measurement error: $S_{jct} = S^*_{jct} + v_{jct}$. Because the variance of measurement error is known, it is possible to estimate the magnitude of the asymptotic bias on the coefficient on average education outside industry (Griliches 1986):

$$plim(\hat{\gamma}_1 - \gamma_1) = (\frac{cov(S_{jct}S_{-jct})var(v_{jct})}{var(S_{jct})var(S_{-jct}) - cov(S_{jct}S_{-jct})^2})\gamma_2 \approx 0.03\gamma_2 \tag{17}$$

where the probability limit is taken for the number of industry–city–time cells going to infinity, keeping constant the dimension of cells.[15] Equation 17 suggests that measurement error in

[15] If the size of cells goes to infinity, measurement error bias disappear.

average education within the industry is unlikely to induce a large bias in the coefficient on average education outside the industry. In a multivariate regression, however, the asymptotic bias formula is more complex, and the bias may be larger.[16]

A second, more direct way to assess the effect of measurement error is based on restrictions on parameters derived from the theoretical model. From equation 13, we know that the coefficient on average education within the industry should be equal to labor share times the private return to education, $(\alpha + \eta)\delta$. The actual estimates in tables 3 to 5 are lower because of attenuation bias. Since labor share and private return to education are known, it is possible to assess the effect of measurement error in average education within the industry on the coefficient on average education outside the industry by constraining the coefficient on average education within the industry to be equal to $(\alpha + \eta)\delta$.

Columns 1 and 2 in table 8 report estimates of the coefficient on average education outside an industry when the coefficient on average education within the industry is constrained to be 0.056. If the private return to education δ is 0.08—a common finding in the literature—and labor share $\alpha + \eta$ is 0.69—as in table 3 and 4—then the coefficient on average education within the industry should be 0.056. Results in the top panel of table 8 suggest that when the coefficient on average education within the industry is constrained to be 0.056, the coefficient on average education outside the industry falls from 0.057 to 0.046 in the specification that includes plant fixed effects (column 1). All other controls are included in the regression, but coefficients are not reported. In the specification that includes both plant and state$\times$year effects, the coefficient falls from 0.051 to 0.044 (column 2). Columns 3 and 4 report results obtained by assuming that the private return to education is 0.09, instead of 0.08. The estimates do not change significantly.

The bottom panel of table 8 reports estimates from the constrained regression for single–unit plants. The coefficient on average education outside industry in the constrained regression is lower than the corresponding coefficient in table 6, but it is still statistically significant.

[16] When plant effects are included, measurement error bias increases further, because the signal to noise ratio decreases (Griliches 1986).

6 Human Capital and High–Tech Plants

The effect of average education on productivity may vary by industry. It is possible that externalities from education play a larger role in human capital intensive industries, such as the high-tech ones, and more generally in those industries where the pace of innovation is fast. This hypothesis is consistent with the observation that the high-tech sector is relatively more important in cities where the labor force is well educated, even after controlling for education in the high-tech sector itself.

This is illustrated in figure 1, where average education in all industries except high-tech is plotted against the share of employment in high-tech industries for 282 cities. Data are from 1990 the Census of Population. High–tech industries are identified using the definition proposed by the American Electronic Association (1997) based on 45 4–digit SIC codes.[17] A weighted OLS fit is superimposed. The figure shows a positive correlation between the high–tech sector share of employment and average education outside high–tech across 282 cities. The weighted OLS slope coefficient is 0.025 (0.003).[18] The coefficient is not sensitive to the presence of outliers like San Jose, where the share of high–tech employment is 21.8%, more than four times the national average.

To investigate the possibility that spillover effects vary across industries, I estimate a model where average education outside the industry is interacted with 13 2–digit industry dummies. All the remaining coefficients are constrained to be the same across industries. Table 9 reports the coefficients on the interacted term.[19] The industry with the highest coefficient is Professional and Scientific Equipment, where an increase in average education generates a 6.9% increase in productivity. The industry with the lowest such effect is Petroleum and Coal, where the coefficient is 3.2% only. Most coefficients are not significantly different from the average.

Overall, no clear pattern emerges from the industry disaggregation, with traditional industries like Leather having a higher coefficient than Machinery and Computing. One problem with these results is that the 2–digit SIC industry classification may be too rough to isolate human

[17] The definition includes computers and office equipment, consumer electronics, communication equipment, electronic components, semiconductors, industrial electronics, photonics, defense electronics, electromedical equipment, software and computer related services, telecommunication services.

[18] Weights are the number of observations in each city.

[19] The excluded industry is Fabricated Metals. Fabricated Metals is chosen because it has a coefficient on average education outside the industry similar to the average one. Some industries do not appear because there are no valid observations.

capital intensive industries. For example, Machinery and Computing includes plants producing farm equipment such as corrals and stalls—hardly human capital intensive products—as well as computers. For this reason, I estimate a model where the coefficient on average education outside the plant is allowed to vary depending on the fraction of wages paid to non–production workers in the plant. The share of non–production wages is an indicator of how important human capital is in the plant (Berman, Bound & Griliches 1994). For plants where a large share of labor costs goes to non–production workers, human capital is probably a more important factor of production than in plants where a large share of labor costs goes to production workers.

Plants are divided in four groups: plants where wages paid to non–production workers amount to 25% of total wages or less (6068 plants), between 25% and 50% (24951 plants), between 50% and 75% (8041 plants), and more than 75% (1221 plants). A regression is estimated where indicators for the four groups are interacted with average education outside the industry. The coefficient on average education outside the industry is shown in table 10. Coefficients on other variables are similar to previous specifications and are not reported. For plants where non–production workers receive less than 50% of total wages, the coefficient is about 0.050. The coefficient is larger for plants where non–production wages are between 50% and 75% of the total, and even larger for the group where non production wages are more than 75% of the total. This finding is consistent with the notion that human capital spillovers have a larger effect on productions that are intensive in human capital, as measured by the share of wages paid to non–production workers.

7 Technology Adoption and Investment in Computers

A possible explanation for the finding that average education outside a plant raises productivity in the plant is that plants located in areas with more human capital are faster to introduce new technology and are therefore more productive. Technology adoption among farmers has been shown to depend both on the farmer's human capital and on his neighborhood's adoption (Foster & Rosenzweig 1995). Whether this type of learning process takes place among US manufacturing firms is an open question.

Marshall (1890) is often quoted as arguing that social interactions among workers in the same industry and location create learning opportunities that enhance productivity. More recently, an influential paper by Lucas (1988) suggests that long–run growth differences across countries can be explained by human capital externalities in the form of learning spillovers. In subsequent models of learning, individuals augment their human capital through exchanges of ideas in meetings with more skilled neighbors (Glaeser 1997, Jovanovic & Rob 1989).

The correlation between average education and productivity could also arise because human and physical capital are complementary, or because new technologies and human capital are complements (Acemoglu 1996, Acemoglu 1998). According to this model, plants have more capital and better technology in areas where the supply of human capital is abundant.[20]

I consider two measures of technology adoption available in the Census of Manufacturers: the level of investment in computers and the fraction of new machinery to the total stock of machinery. Plants were asked to divide the total investment in new machinery into three categories: purchases of computer and peripheral data–processing equipment, expenditures for vehicles designed for highway use, and other investment in machinery. The top panel in table 11 reports results from a regression of investment in computers on average education and other controls. The question on computer investment was asked only to the subset of plants that are part of the Annual Survey of Manufacturers, thus reducing the number of plants for which all relevant variables are not missing in 1982 and 1992 to 3936. Estimates suggest that higher average education outside the industry is associated with larger expenditures on computers. The effect is larger in the specification that includes plant effects than it is in the cross–sectional specifications. A one standard deviation increase in average education outside the industry is associated with a $850,000 increase in investment in computers, according to the fixed effects specification. Given the small sample size, however, the coefficients are not precisely estimated.

Table 11 also reports results from a similar regression where investment in automobiles and trucks is the dependent variable (column 4 to 6). Results on automobiles and trucks provide an

[20] Acemoglu (1996) shows that if firms and workers find each other via random matching and breaking the match is costly, human capital externalities will arise naturally even without learning or technological externalities. The intuition is simple. The privately optimal amount of schooling depends on the amount of physical capital a worker expects to use. The privately optimal amount of physical capital depends on the education of the workforce. If a group of workers in a city increases its level of education, firms in that city, expecting to employ these workers, would invest more. Because search is costly, some of the workers who have not increased their education work with more physical capital and earn more than similar workers in other cities.

indirect specification test. The purchase of automobiles and trucks by a plant is not usually considered adoption of new technology. Finding that investment in automobiles is associated with average education would cast some doubts on the interpretation of the regressions, suggesting that correlation between investment in computers and average education could be spurious. Estimates in table 11 suggests that, unlike investment in computers, investment in automobiles is not correlated with the average education outside the industry.

Previous literature has documented that the probability that a worker uses a computer is positively correlated with years of schooling (Autor, Katz & Krueger 1998). Given the results in the top panel of table 11, it is natural to ask whether a plant's investment in computers is correlated with the share of computer users in the city outside the plant.

I calculate the share of computer users for each city and industry using the Current Population Survey (CPS). Following Autor et al. (1998), I calculate city–specific computer use frequencies from the 1989 October School Enrollment Supplement to the CPS as the weighted fraction of currently employed workers age 16-65 who answered yes to the question "Do you use a computer directly at work?" within Standard Metropolitan Statistical Area (SMSA).[21] According to this definition, about 37.0% of workers used a computer at work in 1989. Consistent with findings in Autor et al. (1998), table 12 shows that cities with a better educated labor force have a larger share of computer users. In cities that belong to the first quartile of average education distribution, 30.9% of workers uses a computer. This percentage increases monotonically as average education increases, and reaches 41.4% in cities in the top quartile.[22]

The bottom panel in table 11 reports estimates of a regression of investment in computers in 1992 on the fraction of computer users outside the industry and other controls.[23] Results for 1982 are not available because the CPS didn't start collecting data on computer use until 1984.[24] Results suggest that computer use outside the plant may have a positive effect on investment in computers inside the plant, although the sample size is small and the coefficient is imprecisely estimated (column 1). A one standard deviation increase in the share of computer

[21] Observations for which SMSA is not identified are excluded. 45,872 are used to calculate frequencies of computer use. Only 219 metropolitan areas are identified in the CPS.

[22] A regression of fraction of computer users to labor force in cities on average education in cities yields a coefficient equal 0.101 (0.009).

[23] Sample size in bottom panel differs from sample size in top panel because results in the top panel are based only on plants for which computer investment data are available both in 1982 and in 1992.

[24] Moreover, very few SMSAs are identified in the 1984 CPS.

users outside the industry is associated with a $267,000 increase in investment in computers.

Since only cross-sectional results are available, it is difficult to assess the direction of causality in the estimated relation. It is possible that there are unobserved city–specific factors that increase both computer use and computer investment. However, the fact that computer use outside the plant is uncorrelated with other types of investment, such as in automobiles and trucks (column 4), is consistent with the hypothesis that computer use causes computer investment, and not vice-versa.

A second measure of technology adoption available in the Census of Manufacturers is the fraction of new machinery to the total stock of machinery. I define new machinery as machinery that has been bought in the past five years. Although an imperfect measure of technical change, the fraction of new machinery to the total stock is likely to be correlated with the quality of capital in a plant. One important way new technologies such as microprocessors appear in manufacturing establishments is embedded in production equipment such as a robots or a computer–controlled machines (Bartel & Lichtenberg 1987, Cooper, Haltiwanger & Power 1997). Thus, plants with a higher percentage of new machinery are likely to have more advanced technology in place. To test whether technology adoption is faster in areas where average education is higher, I regress the fraction of new machinery on average education outside and within the industry and other controls.

Estimates are shown in table 13.[25] The cross–sectional specifications in columns 1 and 2 include 4–digit industry effects. Keeping the size of the plant and multi–unit status constant, plants located in areas with more human capital have higher fraction of new machinery in 1982 than plants located in areas with less human capital. The coefficient for 1992 is zero. But cross-sectional estimates can be biased by many confounding factors that affect the vintage of the machinery stock. In columns 3 to 5, plant fixed effects are included. The coefficient on average education outside the industry is positive, confirming that plants in areas with a more educated workforce have newer machinery.

Given the striking difference in the effect of average education on the productivity of multi–unit and single–unit plants (table 6), one might expect the effect of average education on the fraction of new machinery to differ as well. However, when the regression is separately esti-

[25] Sample size is only 31,070 because only plants for which there is data on the capital stock in 1977 can be included.

mated for single–unit and multi–unit establishments, the coefficient fails to show any significant difference.

8 Conclusion

Previous work has attempted to shed light on the magnitude of the social return to education by examining differences in education and wages across metropolitan areas. In this paper I take a more direct approach by focusing on the productivity of manufacturing establishments. I create a unique workers–firms dataset by matching plant–level productivity data from the Census of Manufacturers to data on workers' education from the Census of Population.

Focusing on the productivity of plants, instead of workers' wages, has two main advantages. First, by looking at quantities, instead of prices, I can perform a more direct test for the presence of externalities. Externalities are identified by comparing the productivity of otherwise similar plants located in metropolitan areas with different levels of average education. I account for unobserved factors that could affect both average education and productivity by including a rich set of plant, state×year, and industry×year effects. Results suggest that plants located in cities where the average education grew faster experienced larger increases in productivity than similar plants in cities where the average education grew more slowly. The average increase in the level of mean education outside a plant is associated with a 1% to 2% increase in productivity between 1982 and 1992. Importantly, the effect is larger for plants where the share of skilled workers is large, suggesting that human capital externalities are more important for productions that are human–capital intensive.

The second advantage of using firms data instead of workers data is that I can investigate possible explanations for the correlation between average education and productivity. I focus on the hypothesis that technology is better in plants located in areas where there is more human capital. Theoretical literature has suggested that this may be the case if learning takes place among workers, so that technology adoption is faster in cities where the labor force is more skilled.

Results in this paper suggest that, consistent with theoretical models, the quantity and quality of capital is indeed better in cities where the level of average education is higher.[26] In

[26]The focus in the paper is on quality of capital (technology). However, not only the quality of capital, but also the quantity is increasing in the level of human capital in local labor markets. In a regression of log capital

particular, technology adoption—measured by investment in computers and the fraction of new capital to the total stock of capital—is faster in cities with more human capital. Moreover, computer investment in a plant is positively associated with the percentage of workers who use computers in the same city outside the plant.

Without a randomized experiment, or a compelling natural experiment, it is difficult to be certain that the estimated correlations are causal. It is possible that estimates are driven by unobservable factors. However, these results are consistent with the finding in the previous literature that wages are higher in cities where the level of human capital is higher, after controlling for the private return to education (Rauch 1993, Moretti 1999). In view of the results in this paper, the positive correlation between human capital and wages, after controlling for the private return to education, may be explained by the presence of more and better capital in cities with a more highly educated labor force.

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on average education outside industry, average education inside industry, firm size, multi–unit dummy and plant effects, the coefficient on average education outside industry is 0.175 (0.026).

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DATA APPENDIX

The Census of Manufacturers covers the universe of manufacturing plants with 1 or more employees. It has 381,773 plants in 1982 and 348,385 in 1992. To build the balanced panel used in this paper, I first exclude all plants that do not appear both in 1982 and 1992 or for which some of the relevant variables are missing in at least one year.[27] About a quarter of the plants in the original sample have non-missing values in both years. I then assign each plant to an industry–city cell based on 3–digit SIC code and Standard Metropolitan Statistical Area (SMSA) code. Although 4–digit SIC codes are available, I choose 3-digit industries to maximize consistency with the Census of Population industry classification.

Since SMSA codes in different years are based on different definitions of metropolitan areas, I correct the 1992 SMSA code to be consistent with the 1982 definition. I delete all the SMSA that are new to the '92 sample and were not part of another SMSA in '82.[28] Because the metropolitan area definition was changed after 1982, I also redefine 1992 SMSAs to match the 1982 boundaries. I do this in two steps. First, I make the definition of counties consistent over time because some counties have changed their boundaries during the 1980s and there are coding errors in the Census of Manufacturers county code. To do so, I use a program written by Randy Becker and provided by CES. Only 5 urban counties are affected (they are located in Georgia, Virginia, Arizona, New Mexico and California). To make sure that all county changes have been captured, I use the County Group Equivalency. I find 7 more changes in Virginia counties that are not included in the CES program. Once I have a county code that is consistent over time, I use the County Group Equivalency files to identify SMSA boundary changes in the 1992 Census of Manufacturers. The computer code used for the matching is available on request. 263 SMSAs are identified in 1982 and 1992.

Average education by industry and city is estimated using the 1980 and 1990 Censuses of Population. To maximize sample size, I use the 5% version of the Public Use Microdata Sample (PUMS). The Census industry classification is not the SIC one, but has a similar level of detail as the 3-digit SIC codes. Using the name of the industry, I match the Census industry classification to the SIC one.

[27] I also exclude from the sample all plants that have capital or production hours or nonproduction hours equal zero. With Cobb–Douglas or Translog production function, output is zero for any plants where one of the inputs is zero.

[28] I also delete Dayton because it was combined with Springfield, OH and there isn't a good way to separate them and/or to define either one so that it resembles its form in 1982.

As in the Census of Manufacturers, metropolitan area definitions are not consistent across years. To make the 1990 SMSA codes consistent with the 1980 definition, I adopt a procedure consistent with the one described above for the Census of Manufacturers.[29] Years of education are assigned to the education codes used in 1990 Census following Table 1 in Kominsky and Siegel (1996). Since 1982 and 1992 are not Census of Population years, linear interpolation is used to estimate the average education for 1982 and 1992.[30]

Finally, plant–level data from the Census of Manufacturers are matched with data on average education by industry and city. To minimize the amount of measurement error in average education within the industry, I exclude all industry–city cells with less then 10 workers.[31] The resulting balanced panel sample has 40,281 plants in 1982 and 1992. This sample covers approximately 24% of average annual manufacturing employment over the period from 1982 to 1992. Summary statistics are in table 1.

In theory, the Worker Establishment Characteristics Database (WECD) could have been used instead of the sample used here. WECD matches Census of Manufacturers to Census of Population using a more restrictive algorithm that requires eliminating from the sample all observations located in cells with more than one plant (Hellerstein et al. 1999). But WECD is available only for 1992 and does not allow for longitudinal analysis. Since for the purpose of the present paper a one–to–one match is not necessary, I opted for a matching procedure that introduces less sample selection and allows for longitudinal analysis.

[29] I assign individuals a metropolitan area on the basis of two geographical identifiers, Public Use Microdata Areas (PUMAs) and metropolitan area codes. The finest geographic units identified in the 5% samples are PUMAs, which are arbitrary geographic divisions that contain no less than 100,000 people each. Most individuals who live in metropolitan areas are also assigned a metropolitan area identifier. However, some PUMAs straddle the boundary of two or more SMSAs and in these 'mixed' PUMAs an SMSA code is not assigned. These 'mixed' PUMAs are assigned a SMSA code on the basis of the County Group Equivalency files. The methodology used to assign SMSA codes and to match MSA across Censuses is identical to the one in Greenstone (1998) , who generously provided the computer code. If over 50% of the PUMA population is attributable to a single MSA, I then assign all individuals in that PUMA to the majority MSA. Since the MSA definition was changed after the 1980 Census, I redefine 1990 SMSAs to match the 1980 boundaries. The County Group Equivalency files are used to identify PUMAs that contain the affected counties in the 1990 Census. If the counties in question comprise more than half of the PUMAs population, all respondents are assigned to the pertinent SMSA. If more than 10% of a SMSAs 1990 population is affected by the boundary changes and is unrecoverable from the County Equivalency files, I drop the city from the analysis. Dayton and Springfield, Ohio are the only such cities. 282 SMSAs are identified in 1980 and 1990. The computer code for this assignment is available on request.

[30] An alternative would have been to use averages obtained yearly from the Current Population Survey. Given the smaller sample size of the CPS, results obtained by interpolating Census averages turn out to be more precise than results obtained from CPS averages.

[31] The average number of workers in a cell is 546 in 1980 and 387 in 1990 (Table 1).

Table 1: Summary Statistics

	1982		1992	
	Mean	Std. Dev.	Mean	Std. Dev.
	(1)	(2)	(3)	(4)
Census of Manufacturers				
Added Value (x 1000)	8019.15	55307.88	9412.53	69566.91
Capital (x 1000)	7042.11	62003.42	8007.13	62502.58
Production Workers	73.35	338.48	68.09	275.56
Production Hours (x 1000)	139.95	632.99	139.10	557.77
Non-Production Workers	37.29	271.27	36.47	269.17
Non-Production Hours (x 1000)	71.90	522.03	74.45	561.48
Average Hourly Wage	13.54	5.50	13.73	5.40
Belong to Multi-Units Firm	0.25	0.43	0.29	0.45
Fraction of New Machinery	0.37	0.30	0.26	0.27
Number of Plants	40,281		40,281	
Census of Population				
Average Educat. Within Indus.	12.08	0.70	12.35	0.89
Average Educat. Outside Indus.	12.26	0.50	12.68	0.60
Within Industry Cell Size	546		387	
Outside Industry Cell Size	16,181		12,847	

NOTES: Monetary values are in 1992 dollars.

Table 2: Estimates of Production Functions: Cross-Sectional Specification

	1992		1982	
	Perfect Substitut.	Imperfect Substitut.	Perfect Substitut.	Imperfect Substitut.
	(1)	(2)	(3)	(4)
Average Educ. Outside Industry	0.060	0.061	0.045	0.046
	(0.008)	(0.008)	(0.008)	(0.008)
Average Educ. Within Industry	0.039	0.037	0.026	0.025
	(0.005)	(0.005)	(0.006)	(0.006)
ln All Workers	0.853		0.519	
	(0.005)		(0.009)	
ln Production Workers		0.510		0.353
		(0.007)		(0.008)
ln Non-Production Workers		0.337		0.158
		(0.008)		(0.006)
ln Capital	0.178	0.188	0.487	0.495
	(0.004)	(0.004)	(0.008)	(0.008)
Multi-Units	0.149	0.163	0.053	0.059
	(0.009)	(0.009)	(0.008)	(0.008)
Industry Effects	Yes	Yes	Yes	Yes
Time–Varying City Controls	Yes	Yes	Yes	Yes
R. sq.	0.89	0.89	0.91	0.90

Notes: Standard errors adjusted for clustering in parenthesis. The dependent variable is added value (value of production - cost of materials). All labor inputs are measured in number of hours worked. N = 40281.

Table 3: Estimates of Production Functions: Longitudinal Specification

	Cobb-Douglas, Perfect Subsitut. (1)	Cobb-Douglas, Imperfect Substitut. (2)	Cobb-Douglas, Imperfect Substitut. (3)	Translog, Imperfect Substitut. (4)
Average Educ. Outside Industry	0.052	0.057	0.055	0.053
	(0.022)	(0.023)	(0.022)	(0.022)
Average Educ. Within Industry	0.021	0.026	-0.001—0.072	0.025
	(0.005)	(0.005)		(0.005)
ln All Workers	0.720			
	(0.006)			
ln Production Workers		0.507	0.37—0.71	0.465
		(0.007)		(0.023)
ln Non-Production Workers		0.199	0.11—0.28	0.404
		(0.007)		(0.026)
ln Capital	0.184	0.190	0.13—0.23	0.220
	(0.005)	(0.005)		(0.020)
ln Product. Workers sq.				0.099
				(0.005)
ln Non-Produc. Workers sq.				0.076
				(0.003)
ln Capital sq.				0.029
				(0.002)
ln Produc.$\times$ ln Non-Produc.				-0.085
				(0.007)
ln Produc.$\times$ ln Capital				-0.069
				(0.005)
ln Non-Produc.$\times$ ln Capital				-0.048
				(0.005)
Multi-Units	0.031	0.043	0.040	0.033
	(0.012)	(0.013)	(0.012)	(0.012)
Establishment Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Technology Varies by Industry	No	No	Yes	No
Time–Varying City Controls	Yes	Yes	Yes	Yes
R. sq.	95.7	95.7	95.8	95.9

NOTES: Standard errors adjusted for clustering in parenthesis. The dependent variable is added value (value of production - cost of materials). All labor inputs are measured in number of hours worked. There are 40,281 plants, observed in 1982 and 1992.

Table 4: The Extent of Sample Selection in Longitudinal Sample

Probability of Selection	Small (1)	Medium (2)	Large (3)
Average Educ. Outside Industry	0.068	0.033	0.069
	(0.032)	(0.030)	(0.028)
Average Educ. Within Industry	0.021	0.033	0.024
	(0.011)	(0.010)	(0.009)
Establishment Effects	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes
Time–Varying City Controls	Yes	Yes	Yes
N	13,295	13,302	13,695

NOTES: Standard errors adjusted for clustering in parenthesis. The dependent variable is added value (value of production - cost of materials). All labor inputs are measured in number of hours worked.

Table 5: Estimates of Production Functions: Longitudinal Specification with Time-Varying Heterogeneity

	Cobb-Douglas			Translog		
	(1)	(2)	(3)	(4)	(5)	(6)
Average Educ. Outside Industry	0.051	0.044	0.130	0.039	0.040	0.053
	(0.027)	(0.021)	(0.165)	(0.023)	(0.020)	(0.161)
Average Educ. Within Industry	0.027	0.011	0.032	0.026	0.010	0.029
	(0.005)	(0.006)	(0.007)	(0.005)	(0.006)	(0.006)
ln Production Workers	0.507	0.504	0.507	0.465	0.462	0.465
	(0.007)	(0.007)	(0.005)	(0.023)	(0.022)	(0.017)
ln Non-Production Workers	0.199	0.196	0.199	0.405	0.407	0.405
	(0.007)	(0.006)	(0.004)	(0.026)	(0.027)	(0.017)
ln Capital	0.189	0.193	0.189	0.219	0.231	0.221
	(0.005)	(0.005)	(0.003)	(0.020)	(0.020)	(0.012)
ln Production Workers sq.				0.100	0.098	0.100
				(0.005)	(0.004)	(0.003)
ln Non-Production Workers sq.				0.076	0.074	0.075
				(0.003)	(0.003)	(0.003)
ln Capital sq.				0.029	0.030	0.029
				(0.002)	(0.002)	(0.001)
ln Production× ln Non-Production				-0.085	-0.079	-0.085
				(0.007)	(0.007)	(0.005)
ln Production× ln Capital				-0.069	-0.070	-0.069
				(0.005)	(0.005)	(0.003)
ln Non-Production× ln Capital				-0.048	-0.051	-0.048
				(0.005)	(0.005)	(0.003)
Multi–unit	0.043	0.036	0.042	0.033	0.026	0.033
	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)
Year×State Effects	Yes	No	No	Yes	No	No
Year×Industry Effects	No	Yes	No	No	Yes	No
Year×City Effects	No	No	Yes	No	No	Yes
Plant Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time–Varying City Controls	Yes	Yes	No	Yes	Yes	No
R. sq.	95.7	95.8	96.2	95.9	96.0	96.2

NOTES: Standard errors adjusted for clustering in parenthesis. The dependent variable is added value (value of production - cost of materials). All labor inputs are measured in number of hours worked. There are 40,281 plants, observed in 1982 and 1992.

Table 6: Estimates of Production Functions by Multi–Unit Status and Size: Longitudinal Specification

	Multi–unit		Size		
	No	Yes	Small	Medium	Large
	(1)	(2)	(3)	(4)	(5)
Average Educ. Outside Industry	0.077	-0.003	0.116	0.109	0.001
	(0.025)	(0.038)	(0.047)	(0.057)	(0.034)
Average Educ. Within Industry	0.021	0.040	0.025	0.019	0.022
	(0.007)	(0.014)	(0.013)	(0.018)	0.012
Plant Effects	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes
Time–Varying City Controls	Yes	Yes	Yes	Yes	Yes
Plant Controls	Yes	Yes	Yes	Yes	Yes
R. sq.	93.4	94.7	88.0	86.1	93.8
Number of Workers	31.0	308.8	1–13	14–44	>44

NOTES: Standard errors adjusted for clustering in parenthesis. The dependent variable is added value (value of production - cost of materials). Plant controls are hours of production and nonproduction workers, capital, a multi–unit dummy. There are 40,281 plants, observed in 1982 and 1992.

Table 7: The Effect of Average Physical Capital Outside the Plant on Plant Productivity

	Cross-Section		Panel	
	1982	1992		
	(1)	(2)	(3)	(4)
(1) ln Average Capital Outside Plant	0.349	-0.150	0.012	0.007
	(0.067)	(0.065)	(0.017)	(0.017)
R sq.	90.3	89.4	95.7	95.7
(2) ln Average Capital Per Worker Outside Plant	1.871	-0.724	-0.005	-0.006
	(0.661)	(0.404)	(0.021)	(0.021)
R sq.	90.4	89.3	95.7	95.7
Plant Effects	No	No	Yes	Yes
Year×State Effects	No	No	No	Yes
Year Effects	No	No	Yes	Yes
Time–Varying City Controls	Yes	Yes	Yes	Yes
Plant Controls	Yes	Yes	Yes	Yes

NOTES: Standard errors adjusted for clustering in parenthesis. Coefficients in row 1 and 2 are from two separate regressions. The dependent variable is added value (value of production - cost of materials). Plant controls include average education within the industry, hours of production and nonproduction workers, capital, a multi–unit dummy. There are 40,281 plants, observed in 1982 and 1992.

Table 8: Estimates of Production Functions: Longitudinal Specification When Measurement Error is Accounted For

	Cobb-Douglas	Cobb-Douglas	Cobb-Douglas	Cobb-Douglas
	(1)	(2)	(3)	(4)
ALL				
Average Educ. Outside Industry	0.046	0.044	0.044	0.042
	(0.023)	(0.027)	(0.023)	(0.027)
Average Educ. Within Industry	0.056	0.056	0.062	0.062
(Constrained)	(-)	(-)	(-)	(-)
Implied Private Return to Educ.	0.08	0.08	0.09	0.09
	(-)	(-)	(-)	(-)
R. sq.	95.6	95.6	95.6	95.6
SIGLE–UNIT PLANTS				
Average Educ. Outside Industry	0.064	0.066	0.062	0.064
	(0.025)	(0.026)	(0.025)	(0.026)
Average Educ. Within Industry	0.056	0.056	0.062	0.062
(Constrained)	(-)	(-)	(-)	(-)
Implied Private Return to Educ.	0.08	0.08	0.09	0.09
	(-)	(-)	(-)	(-)
R. sq.	95.7	95.7	95.7	95.7
Plant Effects	Yes	Yes	Yes	Yes
State $\times$ Year Effects	No	Yes	No	Yes
Year Effects	Yes	Yes	Yes	Yes
Time–Varying City Controls	Yes	Yes	Yes	Yes
Plant Controls	Yes	Yes	Yes	Yes

NOTES: Standard errors adjusted for clustering in parenthesis. The dependent variable is added value (value of production - cost of materials). Plant controls are hours of production and nonproduction workers, capital, a multi–unit dummy. There are 40,281 plants, observed in 1982 and 1992.

Table 9: Estimates of External Effect of Education by Industry

Aver. Educat. Outside Ind.		0.056
		(0.022)
	× Lumber and Wood	-0.006
		(0.007)
	× Furniture	0.000
		(0.008)
	× Petroleum and Coal	-0.024
		(0.016)
	× Rubber and Plastic	0.002
		(0.002)
	× Leather	0.013
		(0.008)
	× Stone, Clay, Glass, Concrete	0.001
		(0.003)
	× Metals	-0.004
		(0.003)
	× Machinery and Computing	0.000
		(0.001)
	× Electrical Machinery	0.005
		(0.002)
	× Transportation Equipment	-0.001
		(0.002)
	× Professional and Scientific Eq.	0.013
		(0.003)
	× Miscellaneous	0.006
		(0.003)
Other Controls		Yes
Plant Effects		Yes
Year Effects		Yes
Time–Varying City Controls		Yes

NOTES: Standard errors adjusted for clustering in parenthesis. The dependent variable is added value (value of production - cost of materials). The excluded industry in the upper panel is Fabricated Metals. Other covariates are average education within the industry, hours of production and nonproduction workers, capital, a multi–unit dummy. There are 40,281 plants, observed in 1982 and 1992.

Table 10: Estimates of External Effect of Education by Fraction of Wages Paid to Non–Production Workers to Total Wages

Non–Production Wages to Total Wages	Coefficient on Average Education Outside Industry
< 25%	0.051
	(0.022)
25%–50%	0.050
	(0.021)
50%–75%	0.061
	(0.021)
> 75 %	0.088
	(0.022)
Other Controls	Yes
Plant Effects	Yes
Year Effects	Yes
Time–Varying City Controls	Yes

NOTES: Standard errors adjusted for clustering in parenthesis. The dependent variable is added value (value of production - cost of materials). The first column indicates the ratio of wages paid to non–production workers to total wage bill in plant. Other covariates are average education within the industry, hours of production and nonproduction workers, capital, a multi–unit dummy. There are 40,281 plants, observed in 1982 and 1992.

Table 11: The External Effect of Education and Computer Use on Innovation: Investment in Computers and Automobiles

	Invest. in Computers			Invest. in Automobiles		
	1992	1982	Panel	1992	1982	Panel
	(1)	(2)	(3)	(4)	(5)	(6)
(1) Education						
Average Educ. Outside Industry	826.4	112.2	1519.2	1.2	-25.9	-84.4
	(616.1)	(155.4)	(828.6)	(116.4)	(16.0)	(92.4)
Average Educ. Within Industry	142.4	70.8	237.2	114.4	17.2	128.4
	(424.3)	(64.6)	(271.0)	(86.8)	(12.4)	(54.4)
Multi–Unit	-2496.5	-202.2	-2059.3	-174.2	-59.9	-40.1
	(1112.9)	(51.5)	(1450.0)	(70.4)	(11.4)	(32.2)
ln Size	4979.9	430.7	1232.0	570.7	87.8	106.4
	(548.2)	(99.7)	(530.0)	(106.0)	(15.6)	(50.9)
R. sq.	24.5	18.6	53.2	15.9	14.1	50.6
N	3936	3936	3936	3936	3936	3936
(2) Computer Use						
Computer Use Outside Industry	4316.7			14.8		
	(3188.5)			(569.9)		
Multi–Unit	-1099.3			-97.3		
	(272.4)			(27.3)		
ln Size	3070.1			341.1		
	(348.1)			(53.5)		
R. sq.	21.1			9.8		
N	6047			6047		
Industry Effects	Yes	Yes	No	Yes	Yes	No
Plant Effects	No	No	Yes	No	No	Yes
Year Effects	No	No	Yes	No	No	Yes
Mean of Dependent Variable	3817.0	260.6		377.4	66.9	

NOTES: Standard errors adjusted for clustering in parenthesis. Investment in computers is defined as investment in computers and peripheral data processing equipment. Investment in automobiles is defined as investment in automobiles and trucks for highway use per worker. Computer use is the fraction of individuals who use a computer at work estimated by industry and city using 1989 October CPS.

Table 12: Computer Use and Average Education in 1990

Average Education in City	Fraction of Workers in City who Use a Computer
First Quartile	0.309
Second Quartile	0.330
Third Quartile	0.356
Fourth Quartile	0.414

NOTES: Data on computer use are from 1989 October CPS.

Table 13: The External Effect of Education on Innovation: Fraction of New Machinery to the Total Machinery Stock

	Cross-Section		Panel		
	1982	1992	All	Single–Unit	Multi–Unit
	(1)	(2)	(3)	(4)	(5)
Average Educ. Outside Industry	0.018	0.001	0.036	0.036	0.038
	(0.010)	(0.004)	(0.017)	(0.018)	(0.023)
Average Educ. Within Industry	-0.008	0.008	0.002	0.005	-0.003
	(0.006)	(0.002)	(0.004)	(0.005)	(0.007)
Multi–Unit	-0.029	-0.039	-0.048		
	(0.006)	(0.005)	(0.009)		
ln Size	0.015	0.041	0.076	0.065	0.107
	(0.001)	(0.001)	(0.003)	(0.004)	(0.005)
Industry Effects	Yes	Yes	No	No	No
Plant Effects	No	No	Yes	Yes	Yes
Year Effects	No	No	Yes	Yes	Yes
R. sq.	6.8	6.0	54.8	57.1	60.3
N	31070	31070	31070	31070	31070

NOTES: Standard errors adjusted for clustering in parenthesis. The dependent variable is the fraction of new machinery to the total stock of machinery.

Table A1: Plant–Level Wage Equations

	1992 (1)	1982 (2)
Average Education	0.078	0.086
	(0.004)	(0.006)
Percentage Female	-0.304	-0.449
	(0.029)	(0.026)
Percentage Black	-0.123	-0.026
	(0.056)	(0.058)
Average Age	0.011	0.012
	(0.001)	(0.001)
City Effects	Yes	Yes
R. sq.	8.8	8.3

NOTES: Standard errors adjusted for clustering in parenthesis. The dependent variable is log of average wage in the plant. There are 40,281 plants, observed in 1982 and 1992.

Figure 1: The Correlation Between The Share of Employment in High–Tech and Average Education Outside High–Tech in 282 Cities