Firm-Level Social Returns to Education

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Abstract: Do workers benefit from the education of their co-workers? We investigate this question drawing on a panel of large Portuguese firms and their workers, using fixed effects and instrumenting average schooling in each firm-year with its lagged value and the lagged share of retirement-age workers. We find evidence of substantial firm-level social returns (at about 19%), much larger than standard estimates of private returns to education, and of sizeable returns accruing to less educated workers but not to their more educated colleagues.

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1. Introduction

The quality of research on returns to education, as that on many other topics in labour economics, depends crucially on the appropriateness of the counterfactual considered. In the case of private returns to education, one would ideally want to have information about the earnings of very similar individuals but that have different levels of education. For instance, one original strategy based on this idea studies differences in earnings and schooling of twins (Ashenfelter and Krueger, 1994).

Nonetheless, one concern with this approach is that it begs the question: why do similar individuals have different schooling levels? The possibility that schooling differences are not exogenous is, however, addressed by the stream in the literature that uses instrumental variables. One important example is Harmon and Walker (1995), who draw on the exogenous variation in schooling driven by the increase in the school leaving age in the UK.

Again, this approach is not free of criticism as the results obtained may be difficult to interpret or may be specific to the group affected. In the end, one would like to combine the positive aspects of each method. That would mean to contrast the earnings of similar individuals (or groups of individuals), whose different levels of education are driven by across-the-board exogenous forces.

These concerns are also important in the case of social returns to education, the topic examined in this paper and a parameter of paramount importance for public policy and macroeconomics research. Indeed, most recent research conducted on social returns to education has also paid attention to these matters, although with conflicting results. While Acemoglu and Angrist (2000) draw on compulsory schooling laws to identify the impact of schooling on average wages in US states, finding insignificant external returns, Moretti (2004) uses city demographic structures and the geographical presence of some colleges to find significant impacts of graduates on the wages of workers, particularly those with lower levels of schooling.[1]

However, the estimation of social returns to education in cities or regions involves an additional identification problem with respect to the case of private returns. This is due to the general-equilibrium effect of the increased supply of educated workers. The intuition is that, if educated and uneducated workers are imperfect substitutes, then an increased supply of the former will affect the prices of both types of workers even if spillovers do not exist (see Ciccone and Peri, 2002).

This problem motivated our analysis of social returns to education at the firm level, rather than in cities or regions, as most previous papers. Focusing on a less aggregated level of analysis, we are able to sidestep the general-equilibrium effects induced by imperfect substitutability. Moreover, it is also likely that most of the education externalities upon pay occur in firms, as it is probably there that workers of different skills interact the most.

Silva (2003), drawing on the same data set as the one used in this paper, tackles this matter focusing on displaced workers but examining county-level social returns to education. He finds very small externalities but his estimates may be affected by measurement error, as his county-level education variables do not include important categories of workers such as the self-employed or public servants. In any case, consistently with the results in this paper, he finds positive and very significant results for his controls for average firm education.

In our approach, we also consider the fixed-effect element, as in the twins research, using a panel of firms. Barth (2002) also looks at firm-level social returns to education: drawing on the longitudinal dimension of his Norwegian worker-level data, he finds a significant effect of the establishment average

[1] See also Dalmazzo and de Blasio (2003).

level of education on workers' individual wages. Similar results are obtained by Battu et al (2003), using a cross-section of British establishments and proxying firm average education from the distribution of workers across different occupations[2]

Secondly, the exogenous-variation aspect is drawn from the variability of the education levels of workers, as induced by the vigorous educational expansion that started before the first year in our Portuguese data. Additionally, we also instrument firm-level schooling, using lagged schooling and the lagged share of workers approaching retirement age in each firm-year. Finally, we exploit some features of our matched employer-employee panel to check the robustness of the findings.

The paper is structured as follows. The next section describes in greater detail our methodology, while Section 3 presents the data and some descriptive statistics. Section 4 presents the results: we find significant evidence of social returns and of spillovers to less educated workers. Section 5 concludes.

2. Methodology

The model that we have in mind involves a bargaining framework whereby rents are shared between employers and workers. We postulate that the admission of more educated workers improves the productivity of the less-educated workers. This may occur through better organisation, imitation effects or other factors: we regard this process as a black box and focus on assessing the empirical evidence about spillovers.

[2] Other papers look at productivity, not wages: see Moretti (2003).

Under the case of positive spillovers, we assume that the increase in the average schooling level of a given firm generates rents. These arise since equivalent workers in firms whose co-workers are less educated will be less productive. A rent-sharing mechanism[3] will then lead to higher earnings, particularly for less-educated workers, those whose productivity stands to benefit the most from co-workers education.

Our empirical approach is implemented by aggregating individual-level Mincer (1974) equations to the firm level. Indeed, because firm-year characteristics are constant across all workers in each firm-year, we effectively only have one observation for each cell. Then, by drawing on a panel of firm-level data, we are able to examine the impact of changes in the average schooling level of workers in each firm on the average earnings of the same workers. Moreover, by considering fixed effects, we allow the educational attainment of workers in each firm to be correlated with time-invariant factors that influence wages at that same firm.

The identification strategy pursued is also motivated by two different assumptions. The first one is that firms have consistent hiring policies, so that, at each hiring event, firms target workers with unobservable characteristics similar to those of workers that were hired on previous occasions. This assumption matters because if firms were not to follow it, then the differences in earnings over time could be attributable to differences in the unobservable characteristics among stayers, leavers and

[3] Martins (2003) finds evidence of rent sharing in the Portuguese labour market, using firm fixed effects and exogenous variation in profits derived from interactions between exchange rates and export shares. Rent sharing is also found to be larger for workers that are more educated and more tenured. Other papers also document substantial levels of rent sharing in other labour markets, including Blanchflower et al (1996) for the US.

entrants. In this case, our estimate of the social return could be biased, as it would capture unobservable factors potentially correlated with education.

There is some indirect supporting evidence, particularly for large firms (those considered here) as these typically set up expensive human resource departments that engage in long and meticulous recruitment processes. Moreover, since only good matches, from the firm and worker points of view, are likely to be stable, new workers should be comparable to their senior colleagues.[4]

The second assumption is that there is enough variability over time in the firm-level educational attainment and that such variability is exogenous with respect to wage determination. This concern motivated our use of data for a country undergoing large upgrades in the schooling of its workforce. Indeed, during the 1990s, Portugal experienced a substantial educational catching-up of its population. In our data, the average years of schooling increased by almost 20%, from 5.3 in 1991 to 6.3 in 1999.[5]

The variation in schooling levels over time may however not be fully explained by educational expansion. It is possible that some firms adjust their workforces due to time-varying shocks that also affect earnings. For instance, firms experiencing an increasing demand for their products may hire

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[4] Barth and Dale-Olsen (2003) find corroborative evidence of "assortative matching" in Norwegian firms, in terms of a positive matching along observed and unobserved productivity characteristics between workers of different educational groups.

[5] This considerable increase is related to the rise of the minimum level of schooling from six to nine years that occurred since 1986. Additionally, the legal constraints that prevented the expansion of private universities were lifted in the same period, allowing for a large increase in enrolment in such institutions, as the demand for university education clearly exceeded its supply by public institutions. Pereira and Martins (2001) describe these and other developments in greater detail.

younger and more educated workers and, simultaneously, due to rent sharing, increase the earnings of both stayers and entrants above the market benchmark. This would bias upward the education coefficient.

Alternatively, firms facing negative demand shocks may simultaneously facilitate the early retirement of their senior workers and demand pay concessions from their remaining employees. Here the education coefficient would be downward biased. Moreover, measurement error will also bias downward that coefficient, as this bias attenuates the estimate towards zero, particularly in panel data models (Griliches and Hausman, 1985).

Given these two conflicting possibilities, we examine the net direction of the bias using instrumental variables. Firstly, we consider lagged education. As firms keep unchanged a large share of their workforce in each two subsequent periods, we expect there will be a significantly strong correlation between present and lagged education. However, lagged education is unlikely to have a direct role in current wages, particularly when controlling for fixed effects.

Our second instrument is the lagged share of workers that are 65 or more years old in each firm-year: as workers enter their retirement age (65 in Portugal), they will sooner or later leave the firm, typically being replaced by younger (and more educated) workers. As before, it is difficult to think why should this share affect directly the current level of firm wages.

3. Data

We use a large matched employer-employee panel, “Quadros de Pessoal”, that covers the universe of Portuguese firms with at least one employee. This data source is based on a compulsory survey administered by the Department of Employment. A large set of variables, concerning both firm and worker characteristics, is collected, including identifiers for each firm and each worker. The latter allow for both firms and workers to be followed over time.

We consider a representative sample of 80% of these firms from the manufacturing sector (and all their workers) for each year between 1991-99. However, given that we want to focus on firms that have consistent hiring policies over time, we use in our analysis only those that are "large" enough, defined here as firms whose size is at least 100 workers. Moreover, since we need to examine each firm in several periods, we chose to select only those firms that are present in our data in at least five out of the nine years available.

As we also want to minimise measurement error, we drop firms-year in which more than 20% of workers have missing or incorrect information in the variables required. This procedure leaves us with 1,359 firms and 7,428 firms-year (more than 90% of the original number of firms-year), representing more than 2.2 million workers-year (and, on average, 298 workers per firm-year).

The descriptive statistics, presented in Table 1, indicate an average schooling attainment across all firms and years of 5.9 years and an average hourly wage of 3.46 euros per hour (1999 prices). Consistently with our assumption about educational expansion, we found in separate calculations that the educational attainment at each firm increases on average by 2.4% over two contiguous periods.

4. Results

Given the previous discussion, we consider the following wage equation:

$$(1) \qquad y_{it} = \beta_1 educ_{it} + X_{it}'\beta_2 + \alpha_i + \tau_t + \varepsilon_{it}$$

Here y_{it} is the logarithm of average real hourly earnings of firm i in period t. $educ_{it}$ is the average schooling years of the workers of firm i in period t. X_{it} is a set of average characteristics of those workers and their firm in that period: a quadratic of experience and tenure, a female dummy, and size (log number of workers). α_i is the firm fixed effect and τ_t the year dummy. ε_{it} denotes the error term.

Table 2 presents the results. For the benefit of generality, we also consider pooled OLS and random effects specifications. In these two cases, which assume orthogonality between schooling and the error, we find large estimates of returns to education, at .21 (22.9%) and .17 (18%), respectively.

In the fixed effects specification, the estimated return falls considerably, but is still statistically significant and economically relevant, at .07 (7.3%). Moreover, the Hausman test strongly rejects the null that the difference in the coefficients is not systematic (the p-value is less than .00005). This return is also below many of those estimates obtained in OLS analysis of private returns to education, suggesting that, at best, there are no spillovers.

However, as discussed in Section 2, the variability in education may not be exogenous. Therefore, we now consider the instruments in the fixed-effects model. The results, presented in Table 3, support their validity. Firstly, both coefficients are statistically significant and positive. Secondly, the tests of instruments quality (see Bound et al, 1995) are also passed.[6] In the main equation, we find that the

[6] The coefficient for lagged schooling in the auxiliary regression is .105 (with a p-value less than .0001) and the coefficient for the share of workers aged 65 or older is .265 (p-value of .069). Moreover, the partial R^2 is reasonably large, at 0.032, and the F-statistic rejects the null that the instruments are jointly equal to zero.

education coefficient more than doubles, increasing to .171 (18.7%), while it is still precisely determined (p-value less than .0005). Moreover, the over-identification test is not rejected, with a test statistic of .657 (p-value of .418).[7]

These results are encouraging as our estimate of a firm-level social return of 18.7% clearly exceeds most international OLS estimates of private returns, even for Portugal, which typically ranks at the top of such distribution.[8] This supports the idea that private returns are not irrelevant from the social point of view, as in signalling models, and that there is a considerably large additional spillover effect on top of the private return.

However, evidence of a stronger relationship between average schooling and average earnings than in worker-level studies does not necessarily imply positive spillovers. At least in the context of regions, imperfect substitution between educated and uneducated workers may also induce such result. In this case, educational expansion may increase the earnings of uneducated workers not because they become more productive but just because they become scarcer.

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[7] We have also considered different retirement-age thresholds (more than 60 or 63 years old) since early retirement applies in some cases. Our results remained largely unchanged. However, as expected, the strength of the instrument becomes weaker as we move farther from the 65 level. These findings are available upon request.

[8] Pereira and Martins (2001) estimate an OLS private return of between 8% and 12% in the 1991-98 period. Other studies include Vieira (1999), who follows the strategy of Harmon and Walker (1995) and documents IV estimates lower than the OLS ones, at around 5%, and Modesto (2003), who examines the self-selection involved in progressing or not from compulsory education and finds marginal returns at that stage not greater than 10%.

As mentioned before, we believe it is unlikely that this general-equilibrium effect will be relevant in firms, unlike in cities or regions. In any case, we shed some light on this possibility by contrasting the impact of firm education between educated and uneducated workers. For the educated, the overall effect of expanding firm education includes two effects which, if they exist, are likely to be of opposite sign (the lower return to education and the spillover). For the uneducated workers, the net effect comprises two effects of positive sign (the imperfect substitution and the spillover). Therefore, we expect to find a positive effect of average firm schooling at least for uneducated workers, as for educated workers the effect is ambiguous. (This test will, however, not be conclusive because the spillover need not be equal for educated and uneducated workers.)

We consider three different thresholds between "educated" and "uneducated" workers. For each firm-year, we calculate the mean and median levels of schooling and we consider the nine-years-of-schooling threshold. We then select workers whose education exceeds or is below each threshold and aggregate their characteristics (schooling, experience, etc) for each firm-year. Finally, in order to make our estimates of the social return to education less affected by the impact of entrants, we only consider workers that have been in the firm for at least 12 months. We therefore obtain cleaner estimates of the spillovers, as we focus on the impact of average education (determined by both stayers and entrants) on stayers only.

Our empirical model is an extended version of (1), now including the characteristics of each subset of workers plus the previously-used control for the average schooling across all workers:

$$(2) \qquad y_{ijt} = \beta_1 educ_{it} + \beta_2 educ_{ijt} + X_{ijt}'\beta_3 + \alpha_i + \tau_t + \varepsilon_{it}$$

y_{ijt} is the logarithm of earnings of workers of type j (educated or uneducated) in firm i in period t (that are also present in the firm in t-1). As before, $educ_{it}$ is the average level of schooling years of the workers of firm i in period t, regardless of whether they were also present in the firm in the previous period. X_{ijt} refer to the same set of average characteristics of the workers of type j in firm i in period t. The remaining variables have the same interpretation as before.

The results are presented in Table 4. With respect to the first-stage equations, we find little differences in the role of the instruments across the two sub-groups (educated and uneducated workers) and across the three thresholds.[9] More interestingly, we find for the main equation that the impact of average schooling is much greater for the uneducated stayers than for their educated counterparts. For instance, taking the mean-education threshold, an increase in average education of one year significantly increases uneducated workers wages by 0.056 (5.8%). The same increase for educated workers is only 1.1% and not significant. The same pattern is obtained for the other thresholds, with wage increases for the uneducated workers of 9.1% and 7.5% and insignificant wage increases for the educated workers.

On the other hand, this pattern is reversed if we look at the impact of each groups' own schooling. While this impact is not significant for the uneducated workers, the return ranges between 9.3% and 10.1% for their educated counterparts. Overall, these results are compatible with the existence of spillovers, as the uneducated workers benefit from the schooling of their co-workers, while the educated workers do not suffer from it. The latter result is consistent with spillover effects being of the same size, but opposite sign, as the effects of lower returns to education. Of course, another possibility is that the spillovers for the more educated are negligible.

[9] One exception is that the average schooling of the uneducated workers plays a greater role in explaining total average schooling than the average schooling of the educated workers. This is due to the large skewness of the distribution of schooling within firms.

5. Conclusions

We find evidence of social returns to education considerably larger than private returns. Whereas the latter typically do not exceed 10%, in OLS and other studies, we find firm-level social returns to education of about 19%. This result is obtained in a firm-level Mincer equation, controlling for firm fixed effects and other variables and instrumenting education with its lagged values and the lagged share of retirement-age workers. Our approach, motivated theoretically by a simple bargaining and rent-sharing model, also sidesteps important general-equilibrium effects that may bias estimates of social effects at cities or other more aggregate levels of analysis.

Consistently with this result, we also find evidence of significant wage spillovers to less-educated workers: their pay increases by 8% per extra year of education of workers in their firm. However, the group of educated workers does not seem to benefit from such spillovers, a result which is consistent with education spillovers being cancelled by lower returns to education or, more likely, with the absence of spillovers for the more educated.

These estimates exceed those obtained in other studies that find significant results, such as Barth (2002) and Moretti (2004). However, those papers examine Norway and the US, respectively, which are countries characterised by average levels of schooling attainment almost twice as big as that of the country considered here, Portugal. One may therefore speculate that spillovers to education decrease with education.

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Tables

Table 1 - Descriptive Statistics

Variable	Mean	Std. Dev.	Min	Max
Log Hourly Wage	1,241	0,410	0,381	3,258
Schooling	5,925	1,354	0,810	13,602
Experience	23,888	5,852	6,866	41,759
Female	0,475	0,295		
Log Size	5,408	0,648	4,605	8,967
1991	0,105			
1992	0,104			
1993	0,112			
1994	0,107			
1995	0,127			
1996	0,118			
1997	0,116			
1998	0,109			
1999	0,103			

Notes:
Log (Firm) Size is measured in terms of number of workers.
Hourly wages are measured in 1999 euros.
The number of observations (firms-year) is 7,428

Table 2 - Results

	Pooled OLS		Random Effects		Fixed Effects	
	Coeff.	**St. Error**	**Coeff.**	**St. Error**	**Coeff.**	**St. Error**
Schooling	0,206	0,002	0,166	0,003	0,070	0,006
Experience	0,052	0,004	0,056	0,005	0,039	0,006
Experience^2	-0,001	0,000	-0,001	0,000	-0,001	0,000
Female	-0,322	0,010	-0,347	0,017	-0,177	0,035
Log Size	0,067	0,004	0,061	0,006	0,011	0,009
Adj. R^2	0,745					
Within R^2					0,256	
Between R^2					0,761	
Overall R^2					0,668	
Firms-year	7.428		7.428		7.428	
Firms	1.359		1.359		1.359	

Notes:

All regressions include a quadratic on tenure and year dummies.

The Hausman test about the difference between the random and fixed effects models is strongly rejected.

Table 3 - Results, Fixed Effects and Instruments

	Coeff.	St. Error
First Stage		
Lagged Schooling	0,105	0,008
Share of 65 and over	0,265	0,146
Adjusted R^2	0,603	
Partial R^2	0,032	
F-statistic	69,01	(P-value= 0,000)
Main Equation		
Schooling	0,171	0,039
Experience	0,065	0,013
Experience^2	-0,001	0,000
Female	-0,089	0,039
Log Size	0,042	0,014
Within R^2	0,180	
Between R^2	0,766	
Overall R^2	0,715	
Overid. Test Statistic	0,657	(P-value= 0,418)
Firms-year	6.220	
Firms	1.345	

Notes:
Both equations consider the same additional variables, as in Table 2.

Table 4 - Results, Different Sub-Samples

	Mean Education		Median Education		9 Years Schooling	
	Coeff.	**St. Error**	**Coeff.**	**St. Error**	**Coeff.**	**St. Error**
Uneducated workers (stayers)						
First Stage						
Group Average Schooling	0,564	0,013	0,674	0,013	0,728	0,021
Lagged Total Average Schooling	0,163	0,009	0,123	0,008	0,183	0,009
Share of 65 and over	0,235	0,152	0,143	0,140	0,258	0,159
Main Equation						
Total Average Schooling	0,056	0,027	0,087	0,035	0,072	0,024
Group Average Schooling	-0,004	0,017	0,000	0,026	-0,038	0,021
Overall R^2	0,607		0,656		0,591	
Overid. Test Statistic	0,533	(P-value= 0,465)	0,132	(P-value= 0,716)	0,233	(P-value= 0,629)
Firms-year	6.219		6.219		6.179	
Firms	1.345		1.345		1.342	
Educated workers (stayers)						
First Stage						
Group Average Schooling	0,120	0,005	0,128	0,005	0,021	0,009
Lagged Total Average Schooling	0,167	0,010	0,157	0,010	0,205	0,010
Share of 65 and over	0,267	0,167	0,203	0,166	0,270	0,179
Main Equation						
Total Average Schooling	0,011	0,029	0,040	0,033	-0,013	0,024
Group Average Schooling	0,091	0,005	0,089	0,005	0,096	0,005
Overall R^2	0,727		0,654		0,436	
Overid. Test Statistic	1,364	(P-value= 0,243)	9,502	(P-value= 0,002)	7,734	(P-value= 0,005)
Firms-year	6.213		6.208		6.179	
Firms	1.345		1.344		1.342	

Notes:
Both equations consider the same additional variables as in Table 2, although now they refer to each specific subset of workers (educated and uneducated), and not to the entire firm.
For each period, only workers present in the firm in the current and previous period ("stayers") are considered, except in the total average schooling variable.