Firm-Level Social Returns to Education[€]

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Abstract: Do workers benefit from the education of their co-workers? We examine this question first by introducing a model of learning, which argues that educated workers may transfer part of their general skills to uneducated workers, and then by examining detailed matched employer-employee panel data from Portugal. We find evidence of large firm-level social returns (between 14% and 23%), much larger than standard estimates of private returns, and of significant returns accruing to less educated workers but not to their more educated colleagues.

Keywords: Education Spillovers, Matched Employer-Employee Data, Endogenous Growth.

JEL codes: J24, J31, I20.

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1. Introduction

While the labour economics literature has devoted considerable attention to the estimation of *private* returns to education, relatively little is known about the social importance of education. However, from many points of view, *social* returns to education are the key parameter to take into account. For instance, a better understanding of whether education increases total output is of paramount importance in a number of policy questions.

Possibly the most important of these policy questions is how should education costs be split between the student and the taxpayer (see Gemmell, 1997). If, for instance, returns to education are only private, then the case for public subsidies for education is significantly eroded. Another question concerns the importance of education for economic growth. While some of the endogenous growth literature argues that investment in education can sustain positive growth rates of income per capita (Lucas, 1988), the empirical support for these views is far from clear (see the discussion in Krueger and Lindahl, 2001).

One explanation for the relatively small number of empirical studies of social returns to education lies in the demanding estimation strategy required. Firstly, as in many other areas of empirical research, one needs appropriate counterfactuals (for instance, Ashenfelter and Krueger, 1994, use twins to estimate private returns to education). Secondly, one needs exogenous variation in education (Harmon and Walker, 1995, draw on increases of school leaving age). Finally, the estimation of social returns to education (unlike private returns) may also have to deal with possible general equilibrium effects. For instance, if high- and low-skill 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 (Ciccone and Peri, 2006).

On top of the estimation hurdles described above, the few studies available on social returns to education have not yet reached any stylised fact, not even whether there are social returns, not to mention their magnitude. In particular, two of the most prominent papers, Acemoglu and Angrist (2000) and Moretti (2004a), find conflicting results, although drawing on similar data sets for the same country (the U.S.).

Acemoglu and Angrist (2000) draw on compulsory schooling laws (compulsory attendance and child labour laws) to identify the impact of average schooling on average wages in US states, finding insignificant external returns. On the other hand, Moretti (2004a) uses city demographic structures and the geographical presence of some colleges to find significant impacts of graduates on the wages of workers in the same city, particularly on those workers with lower levels of schooling.[1]

[1] These contrasting results also extend to other studies that look at the U.S. case but that do not address the endogeneity of schooling: Rauch (1993) finds positive, significant effects while Rudd (2000) documents insignificant effects. Using a new methodology, based on a "constant-composition" approach, Ciccone and Peri (2006) also find insignificant results. It should, however, also be mentioned that while the findings on *wage* social returns to education can be characterised as mixed, a more consistent and encouraging set of evidence has been found for other social domains where education may also matter. Studies focusing on productivity (Moretti, 2004b), crime (Lochner and Moretti, 2004), citizenship (Milligan et al., 2004), and intergenerational effects (Currie and Moretti, 2003) find positive and significant effects of education.

It is in the context of this emerging literature on social returns to education that the present paper makes its contribution. On the theoretical side, we present a model of learning. We argue that highly-educated workers may transmit part of their education skills to their low-education colleagues at the same firm. This assumption is intuitively appealing as it is likely that most of the education externalities that affect productivity (and, subsequently, pay) occur within firms: it is probably at the firm level that workers interact the most. Among other empirically testable results, our model shows that the existence of spillovers leads to a stronger relationship between wages and education at the firm level than at the individual level.

We then empirically examine this and other implications of the model, exploiting a panel of almost 5,000 Portuguese firms and their workers, followed for up to nine years. Focusing on firms, we not only address the same level of analysis considered in the theoretical model, but we also sidestep possible general-equilibrium effects induced by imperfect substitutability. Moreover, the longitudinal dimension of the data allows us to implicitly control for unobserved differences across firms, as in the twins literature, and we benefit from within-firm variation of education driven by the vigorous educational expansion experienced in Portugal. Finally, we also use lagged schooling and the lagged share of workers approaching retirement age as sources of exogenous variation of education.

Consistent with the model, we estimate social (firm-wide) returns to education above those commonly obtained in studies of private (individual) returns. These social returns are particularly high when focusing on specific firm/job-level cells, a less aggregated level of analysis where one would intuitively expect greater scope for spillovers. Again as predicted by the model, we also find that the less-educated workers benefit from increases in their firms' average schooling levels, unlike their more educated counterparts.

The remaining of the paper is structured as follows. The next section describes our model of learning. Section 3 presents the empirical methodology and the data. Section 4 describes the results, the robustness analysis and some extensions and discusses the implications of our findings. Section 5 concludes.

2. A Model of Learning

A firm produces a single output employing workers with various productivities and the number of workers is normalized to one unit. The worker's initial productivity y follows a density function in the interval [0,1], $f(y) = \beta y^{\beta-1}$, where $\beta > 0$.[2] If $\beta = 1$, the firm has a uniform distribution; if $\beta < 1$, the firm has fewer educated workers and more unskilled ones; if $\beta > 1$, we have an opposite case. We allow firms to have different β's.

Without learning, the firm's total product would be $\bar{y} = \int_0^1 yf(y)dy = \beta/(1+\beta)$. However, through learning, there is a positive externality among workers. A worker y's productivity can be

[2] Our results hold for other distributions, e.g., $f(y) = \beta(1-y)^{\beta-1}$, or $f(y) = 2 - \beta + 2(\beta - 1)y$ for $0 \le \beta \le 2$, or an "olive" shaped distribution, $f(y) = 2y/\beta$ for $y \le \beta$, and $f(y) = 2(1-y)/(1-\beta)$ for $y > \beta$, where $\beta \in (0,1)$.

enhanced by an amount of $\theta(z - y)$, through learning from a more productive worker z ($> y$), where $\theta > 0$. We assume the same θ for all firms.

The opportunity of learning depends on the number of type z workers available per worker y. Hence a worker y's productivity increase due to internal learning from type z workers is equal to $\theta(z - y)f(z)/f(y)$. His total productivity gain due to learning is the sum of his learning gains from all more productive workers in the firm, i.e.,

$$\Delta y = \int_y^1 \theta(z-y)f(z)dz / f(y) \tag{1}$$

His effective productivity is then denoted by $x = y + \Delta y$. The firm's total productivity is the sum of all workers' effective productivity, i.e., $X = \int_0^1 (y+\Delta y)f(y)dy$. Given the density function $f(y) = \beta y^{\beta-1}$, we find this total productivity as follows (see Appendix A for the proof):

$$X = \bar{y}\left(1 + \frac{0.5\theta}{2-\bar{y}}\right) \tag{2}$$

Therefore, provided that $\theta > 0$, X, the total effective output, is larger than $\bar{y}$, the initial productivity. Now we consider how the workers' wages are determined. We assume the wage payment is the only cost, and denote the average wage by $\bar{w}$ (equivalent to the total wage since the workers are normalized to one unit). Let p be the output price. The total profit is $pX - \bar{w}$. We assume that $\bar{w}$ is determined by collective bargaining between the union and the firm, with the bargaining powers of the union and the firm being α and $1 - \alpha$ respectively.[3] For simplicity, we assume that the reservation prices for the union and the firm are both zero. Hence the Nash bargaining solution of the average wage should maximize the following function:

$$L = \bar{w}^{\alpha}\left[p\bar{y}\left(1 + \frac{0.5\theta}{2-\bar{y}}\right) - \bar{w}\right]^{1-\alpha} \tag{3}$$

After the total wage is determined, we assume that each worker's wage is proportional to his initial productivity, i.e., $w = y\bar{w}/\bar{y}$. Given equation (3), we can solve the Nash bargaining solution and find the average and individual workers' wages (see Appendix B for the proof). These solutions imply:

$$ln\bar{w} = C + ln\bar{y} + ln\left(1 + \frac{0.5\theta}{2-\bar{y}}\right),\quad lnw = C + lny + ln\left(1 + \frac{0.5\theta}{2-\bar{y}}\right) \tag{4}$$

Given equation (4) we can evaluate the impact of $\bar{y}$ on $ln\bar{w}$ using firm level data, and evaluate the impact of y on lnw, using individual workers' data within a firm holding $\bar{y}$ constant.

In our empirical study, we use years of education as approximate measure for the initial productivity because a worker's initial productivity y is usually unobservable. We assume that it is a linear function of his years of education e, $y = a + be$, where $b > 0$. This relation is assumed

[3] There is considerable empirical evidence of rent sharing in the labour market – see Blanchflower et al 91996) or Arai (2003), amongst others.

for all firms. For any firm, the average initial productivity $\bar{y} = a + b\bar{e}$, where $\bar{e}$ is the average years of education within the firm.

These relations allow us to test the existence of learning effect. According to (4), $\partial nw/\partial e = b/y$. When we estimate the private returns of education in one firm, the result would be the workers' average private returns, E(b/y). If there is no learning, $\theta = 0$. (4) implies $\partial n\bar{w}/\partial\bar{e} = b/\bar{y}$.

Given any distribution, $1/\bar{y}$ is always smaller than the expected value of $1/y$. Hence, when $\theta = 0$, our estimated $\partial nw/\partial e$ should be higher than that of $\partial n\bar{w}/\partial\bar{e}$. If we observe the opposite case, it must be due to the existence of learning, i.e., $\theta > 0$.

If our estimated $\partial nw/\partial e$ is smaller than that of $\partial n\bar{w}/\partial\bar{e}$, the next question is how this difference is related to θ. When θ is small, $ln[1 + 0.5\theta/(2 - \bar{y})]$ can be approximated by a first-order Taylor expansion as $0.5\theta/(2 - \bar{y})$. Therefore, we have $\partial n\bar{w}/\partial\bar{e} = b/\bar{y} + 0.5b\theta/(2 - \bar{y})^2$. Then, the difference between estimated $\partial n\bar{w}/\partial\bar{e}$ and $\partial nw/\partial e$ would be equal to $b/\bar{y} - E(b/y) + 0.5b\theta/(2 - \bar{y})^2$. Since the sum of the first two terms is negative, the difference between $\partial n\bar{w}/\partial\bar{e}$ and $\partial nw/\partial e$ indicates the lower limit of the schooling effect.

Furthermore, given our assumption, $b/\bar{y} - b/y$ is equal to $b^2(e - \bar{e})/(a + be)(a + b\bar{e})$. Its expected value is close to zero if b/a is small. In this case, the difference between estimated $\partial n\bar{w}/\partial\bar{e}$ and $\partial nw/\partial e$ would be close to the schooling effect.

In the following sections, we will show that our empirical research indeed finds larger estimated average returns than those of private ones, indicating the existence of internal schooling.

3. Empirical Approach

Based on the theoretical model of learning, our empirical work is implemented by aggregating individual-level Mincer (1974) equations to the firm level. This follows from the predictions derived from the model as to how average, firm-level wages vary with average, firm-level education.

As mentioned in the introduction, it is critical to draw on data that present enough variability over time in educational attainment. This concern about variability motivated our use of data for a country and a period that document large upgrades in the schooling of its workforce: during the 1990s, Portugal experienced a substantial educational catching-up of its labour force. In our data, presented below, the average years of schooling increased by about 17% over a period of nine years, from 5.9 in 1991 to 6.9 in 1999.[4]

[4] These are very low average schooling figures for a European country. They correspond to an average school leaving age of the Portuguese workforce of around 12 or 13. Compulsory schooling was only four years of schooling (schooling leave age of 12) until the early 1960's. The considerable increase observed over the period is in part due to the rise of the minimum level of schooling from six to nine years that occurred on 1986. Additionally, the legal constraints that prevented the expansion of private universities

A second concern mentioned in the introduction is that education variability is exogenous with respect to wage determination. While our consideration of firm fixed effects allows the educational attainment of workers in each firm to be correlated with all time-invariant factors (observed or unobserved) that influence wages at that same firm, our estimates would become inconsistent if any of the variables or parameters mentioned above varied over time.[5]

Moreover, there are additional sources of endogeneity bias, not outlined by the model but of practical relevance, which can emerge from the interplay between workforce adjustment and time-varying shocks that also affect earnings. For instance, firms experiencing an increasing demand for their products may hire younger and more educated workers and, simultaneously, due to rent sharing, increase the earnings of both stayers and/or entrants above the market benchmark. This would lead to spurious positive correlation between education and wages and thus bias upward the education coefficient in a firm-level wage equation. Moreover, measurement error may also bias downward that coefficient, as it typically attenuates the estimate towards zero, also in panel data models (Griliches and Hausman, 1985).

Given these different and conflicting possibilities, we sought to derive consistent estimates using instrumental variables. The first instrument is the firm's lagged education level. As firms keep 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, as we control for current education and firm fixed effects.

Our second instrument is the lagged share of workers that are of retiring age in each firm-year. The intuition here is as follows: As workers reach their retirement age, they will sooner or later leave the firm (retirement is, in general, not compulsory in Portugal), typically being replaced by younger and, by force of the above-described expansion of the education system, more educated workers. So a larger share, in period t-1, of workers that will qualify for retirement in period t should be positively correlated with firm-average education in period t. Moreover, as for the case of lagged education, we find no reasons for this lagged share of retirement-age workers to directly affect the current level of firm wages.

There is additional exogenous variability related to this instrument, as the retirement thresholds varied differently for different types of cohorts over the 1990's in Portugal. A law issued in 1991 determined that retirement age would be adjusted gradually over the decade for women, as until then it had stayed at 62 (while it was 65 for men). Specifically, it was decided that the women's

were lifted in the same period, allowing for a large increase in enrolment in such institutions, as until then the demand for university education clearly exceeded its supply by public institutions. (Pereira and Martins (2001) describe these and other developments of the Portuguese education system in greater detail.)

[5] This is a possibility that may affect the study of Barth (2002), who also looks at firm-level returns to education. Drawing on the longitudinal dimension of his Norwegian worker-level data, but assuming education variation to be exogenous, he finds a significant effect of the establishment average level of education on workers' wages. See also Battu et al (2003), who find significantly positive effects in a cross-section study of British establishments, proxying firm average education from the distribution of workers across different occupations.

retirement age should converge gradually to the men's level, increasing by six months every year, starting at 62 years and six months in 1993 until reaching men's retirement age of 65 years in 1998 (see Martins et al. (2007) for an analysis of the impact of increasing retirement ages based on this reform). Our instrument takes this legislative change into account.[6]

Before concluding this subsection, we wish to highlight a possible concern with the method pursued in the paper: it may be that the new workers of higher education hired by firms have different unobservable characteristics than those of workers hired on previous occasions. In this case, differences in earnings over time could be attributable to such differences in unobservable characteristics among stayers, leavers and entrants. Our estimate of the social return could then be biased, as it would capture unobservable factors potentially correlated with education.

However, there is some indirect evidence that this possibility does not affect our results, particularly for the medium- and large-sized firms considered here. Indeed, these firms (unlike smaller ones) typically set up expensive human resource departments that engage in long and meticulous recruitment processes, targeting and assessing worker characteristics that are unobservables for the labour econometrician. Since only good matches, from the firm and worker points of view, are likely to be stable, new hires should be comparable to their senior colleagues. Moreover, 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 in their firms.

3.1. Data

We use a large matched employer-employee panel, "Quadros de Pessoal" [QP, Personnel Records], which covers the universe of Portuguese firms with at least one employee. This data source is based on a compulsory survey administered by Portugal's Department of Employment. A large set of variables, concerning both firm and worker characteristics, is collected, including identifiers for each firm and each worker. These identifiers allow for both firms and workers to be followed over time. Moreover, as the survey is also to be used for inspection purposes, so that the Department of Employment can monitor each firm's compliance with different aspects of Portugal's relatively restrictive labour law, particular care is placed on the reliability of the survey.

In a first step, the analysis in this paper draws on a representative sample of 80% of all firms for each year between 1991 and 1999. We also use information about all workers for each of the firms sampled. Given that we want to focus on firms that are likely to have hiring policies as consistent as possible over time, and that we believe that such policies are positively correlated with firm size, we use in our analysis only those firms that are "large" enough, defined here as a size of at least 50 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 four out of the nine years available.

[6] There is also some anecdotal evidence supporting the unanticipated nature of this new law. It is argued that the discontentment it created among those female workers forced to work up to three more years than they initially expected contributed to the downfall of the government that enacted that law at the 1995 general elections.

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 in the wage equation. This procedure leaves us with 4,830 firms and 27,994 firms-year (more than 90% of the original number of firms-year), representing more than 5.9 million workers-year (and, on average, about 213 workers per firm-year).

The descriptive statistics, presented in Table 1, indicate an average schooling attainment across all firms of 6.5 years and an average hourly wage of 3.75 euros per hour (1999 prices). Consistently with our assumption about educational expansion, we find in separate calculations that the educational attainment at each firm increases on average by about 2.2% over two contiguous periods.

4. Results

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

$$y_{it} = \beta_1 educ_{it} + X_{it}'\beta_2 + \alpha_i + \tau_t + \varepsilon_{it} \qquad (5)$$

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: average experience and average tenure (and their squares), the share of female workers, and size (log number of workers). α_i is the firm fixed effect, τ_t the year dummy, and ε_{it} denotes the error term.

Table 2 presents the first set of 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 term, we find large estimates of returns to education, at .18 and .14, respectively.

In the fixed effects specification (3rd column), the estimated return falls considerably, but is still statistically significant and economically relevant, at .05 (5%). Moreover, the Hausman test strongly rejects the null that the difference in the random and fixed effects coefficients is not systematic (the p-value is less than .0005), thus favouring the fixed effects specification. On the other hand, this fixed-effect return is also below most of the equivalent estimates obtained in OLS analysis of private returns to education. Following our approach, these low returns suggest that, at best, there are no spillovers.

However, as discussed in Section 4, there are several reasons for the variability in firm-level education not to be exogenous as assumed in the fixed effects specification. Therefore, we now also instrument education in the fixed-effects model. The results, presented in Table 3, support the validity of the instruments. Firstly, both coefficients for the instruments in the auxiliary regression are statistically significant and positive (and the sign is as expected from our discussion before): the coefficient for lagged schooling in the auxiliary regression is .08 (with a p-value less than .0005) and the coefficient for the lagged share of workers of retirement age is 1.29 (p-value less than .0005). Secondly, the tests of instruments quality (see Bound et al, 1995) are also passed: the partial R^2 is reasonably large, at 0.013, and the F-statistic strongly rejects the null that the instruments are jointly equal to zero.

In the main equation, we find that the education coefficient almost triples with respect to the previous results, increasing from .05 to .133 (14.2%), while it is still precisely determined (p-value of .019). Moreover, the over-identification test is not rejected, with a test statistic of 1.3 (p-value of .25). This is a reassuring result although one has to bear in mind that over-identification tests typically have low power.[7]

These results are also encouraging as our estimate of a firm-level social return of 14.2% comfortably exceeds most international OLS estimates of private returns, including those for Portugal, a country which typically ranks at the top of the international distribution of those returns: for instance, Pereira and Martins (2001) estimate an OLS private return of between 8% and 11% over the 1991-98 period.[8] Moreover, we also computed individual-level returns to education with the same data that we use in this paper. Using different sub-samples and weights, we found an average return of approximately 10%, never exceeding 12% (results available upon request).

This relatively large gap between firm- and individual-level returns supports the idea that private returns are not irrelevant from the social point of view, as in signalling models, and that there is a considerable additional spillover effect on top of the private return. Moreover, since we have reasons to believe that rent sharing is an important feature of the Portuguese labour market (Martins, 2004), this higher firm-level estimate is precisely the result expected given the model's implications in this type of labour markets if productivity spillovers are relevant in practice.

On the other hand, as we mentioned in the introduction, evidence of a stronger relationship between average schooling and average earnings than between individual schooling and individual earnings does not necessarily, in general, imply positive spillovers. At least in the context of more aggregate units of analysis, such as regions, imperfect substitution between educated and uneducated workers may also induce such result. In the case of such larger units of analysis, educational expansion may increase the earnings of uneducated workers not because they become more productive but just because they become scarcer.

Nonetheless, as we explained before, it seems unlikely that this general-equilibrium effect will be relevant in firms, unlike in cities or regions, for instance. Indeed, the model assumes that wages for workers of different skills will be constant from the firms' point of view, since individual

[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 (for men). These findings are available upon request.

[8] 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%. See also Martins and Pereira (2004), who present OLS and quantile regression results for comparable micro datasets covering sixteen Western countries. Portugal tops the international distribution, with a return at the mean of about 11%. Moreover, we also find similar results when estimating returns to education at the individual level using the sample we use for the firm-level analysis.

firms, as small units, cannot affect prices – wages may only change for the less skilled to the extent they benefit from learning spillovers and become more productive.

We now test these assumptions by contrasting how does pay for the more and less educated workers evolve as a function of average education in their firm. Our empirical approach is as follows: First, in order to make our estimates of the firm-level return to education less affected by the impact of entrants, we consider only workers that have been in the firm for at least 36 months. This period is, in general, the time threshold at which employment contracts have either to become permanent or be terminated. Workers with levels of tenure of 36 months or more will thus have a stronger degree of bargaining power, allowing them to benefit from any spillover that may occur, unlike those workers with temporary contracts.[9]

For these workers-stayers, we then consider two alternative thresholds between "educated" and "uneducated" workers, which are, for each firm-year, the respective mean and median levels of schooling. After that, we separate workers whose education levels exceed or are below each threshold and aggregate their characteristics (schooling, experience, etc) for each firm-year, after which we run similar regressions as before. This approach should result in clearer estimates of the spillovers, as we focus on the impact of average education (determined by both stayers and entrants) on educated and less educated stayers separately.

Our empirical model is an extended version of (5), now including the characteristics of each subset of workers (educated and less educated) plus the previously-used control for the average schooling across all workers:

$$y_{ijt} = \beta_1 educ_{it} + \beta_2 educ_{ijt} + X_{ijt}'\beta_3 + \alpha_i + \tau_t + \varepsilon_{it} \qquad (6)$$

y_{ijt} is the logarithm of the average earnings of workers of type j (educated or uneducated) in firm i in period t (that have been in the firm for at least 36 months). As before, $educ_{it}$ is the average level of schooling years of the workers of firm i in period t, regardless of whether they are stayers or new hires. 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 two education thresholds (mean and median).[10] More interestingly, we find for the main equation that the impact of firm average schooling is much greater for the uneducated stayers than for their educated counterparts. For instance, taking the mean-education threshold, an increase in firm average education of one year significantly increases uneducated workers wages by 0.024. The equivalent increase for educated workers is only 0.008 and not significant. Moreover, the same pattern is obtained for the median threshold, with a wage

[9] Using a sub-sample of the present data set and the same tenure threshold, Martins (2008) finds in his study of rent sharing that high-tenure workers benefit almost twice as much from firm rents than their low-tenure colleagues.

[10] 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 positive skewness of the distribution of schooling within firms.

increases for the uneducated workers of 0.033 and an insignificant wage increase for the educated workers.

On the other hand, this pattern is reversed if we look at the impact of each group's own schooling. While this impact is not significant for the uneducated workers, the return is significant and ranges between 0.075 and 0.077 for educated stayers.[11] Overall, these results are consistent with the model and, in particular, with the existence of spillovers for the less educated, as these uneducated workers benefit from the schooling of their co-workers, while the educated workers do not.

These results are also important in that they go against an alternative explanation, unrelated to spillovers, for the higher returns to education uncovered at the firm level in this paper. This alternative explanation is based on non-linear and, in particular, convex returns to education, which indeed have been documented for the Portuguese case (see Pereira and Martins, 2001). Under such non-linearities, returns at the firm level could exceed those at the individual level, as the former returns at the firm level, in within-firm estimations, can be more than proportionately driven by the inflow of more educated workers – who benefit from higher individual returns to education. However, the existence of spillovers to the less educated workers, documented in this subsection, indicates that, at the very least, the higher firm-level returns are not only a result explainable by non-linearities.[12]

Finally, as further evidence of robustness, we have replicated the analysis above for groups of firms of different sizes (results not shown but available upon request). We found returns always above 10% and some evidence that larger firms exhibit larger returns. This may suggest that the "spilloverability" of education is positively affected by firm size.

4.1. Extension

In this sub-section, we replicate our previous analysis of equation (6) but considering now information aggregated at different job levels within each firm, rather than at the firm level, as before. Our motivation for this exercise is that the model would predict a stronger spillover effect in this case, as there is greater scope for spillovers between educated and less educated workers within a job level, rather than across all job levels: learning is likely to be less non-excludable across job levels than within job levels.

[11] It should be mentioned that, in this specific approach, the instruments for retirement shares are generally not significant (and in some cases have negative signs). The over-identification test is, however, passed in all specifications.

[12] Further support for our findings can be found in the results of Silva (2003), which draws on the same data used here to study *county-level* social returns to education, adopting an empirical approach based on displaced workers that move to different counties. Unlike in our paper, he generally finds small or insignificant externalities. However, his estimates may be affected by measurement error, as county-level education variables obtainable from the QP data set cannot include important categories of workers, such as the self-employed or public servants, not to mention individuals outside the labour force. In any case, in one specification which also controls for differences in education across counties, Silva (2003) documents positive and significant results for average firm education (Table 5, page 45).

The QP data include information on eight types of job levels, ranging from top managers to apprentices and including, in decreasing hierarchical level, intermediary managers, supervisors, highly-skilled professionals, skilled professionals, semi-skilled professionals, and non-skilled professionals. This specific range of job levels, unchanged over the period covered, has to be adopted by all firms that submit their information to the Department of Employment and is thus generally comparable both across and within firms. The descriptive statistics for the resulting new data are presented in Table 5. (We consider only the seven job levels above apprenticeships, as the latter level presents considerable measurement error.) Notice the large increase in the number of observations, from 27,994 firms-year (Table 1) to 177,662 job-levels-firm-year. Notice also the increase in (unweighted) average education, as the thinner job levels (with fewer workers) typically include more educated individuals.

We then regress log average wages in each job-level/firm/year cell on the mean characteristics of that cell, considering also cell fixed effects and instrumenting education in a similar way as before. The results are presented in Table 6 and indicate a significant and precisely estimated return to average education of 0.209 (23%). This finding is consistent with our expectations under the framework of the learning model since it is considerably larger than our estimate for the firm-level analysis.

Other reassuring results are that the over-identification test is passed (p-value of 0.45) and the F-statistic of the instruments is very large. However, the indicators of instrument quality are not as good as before: the coefficient of retirement shares is not significant and the partial R^2 statistic is relatively low.[13]

4.2 Implications

Before concluding, we discuss in this subsection some implications of our findings. First, we provide a more directly interpretable measure of the economic impact of learning and its spillovers, as derived in this paper, by computing some simple, back-of-the-envelope estimates of how much that type of schooling affects wages. For this exercise we consider a spillover effect of 7%, conservatively halfway between the 14% derived in our first estimation and the 23% obtained for job-level cells, after subtracting 10%, the latter figure corresponding approximately to the modal estimate of the OLS private returns to education for Portugal (see the references above).

We then borrow from the Lester range methodology, as discussed in the rent-sharing literature, and work out the percentage wage gain of an hypothetical worker that moved from a firm at the bottom of the distribution of average firm-level education (as proxied by the 10th percentile, which corresponds to 4.5 years of schooling) to a different firm at the top of the same distribution (90th percentile: 9.3 years of schooling). The two firms would have precisely the same average

[13] One explanation for these latter findings is that average education at each job-level cell is subject to job upgrading processes which are not much affected by retirement-related forces. Moreover, measurement error is likely to be more acute within job-levels taken separately than together in firms, for instance because firms may occasionally change their coding practices (as to how to allocate each given worker to a job level, for instance). Promotions may also negatively affect the strength of the retirement instrument.

characteristics, except for the education of their workforce. For that spillover of 7%, the resulting wage gain would be 34%, a figure that can be regarded as considerably large.

What further implications can a figure of this size have? It may be relevant for research that seeks to understand the increasing levels of (within) wage inequality observed in some countries, including the U.S. and the U.K., particularly during the 1980's (Katz and Autor, 1999). For instance, a process of increasing education dispersion within firms (which may or not have corresponded to the case of those countries) would, according to the model and assuming that productivity spillovers lead to wage spillovers, increase (within) wage inequality. This is because workers in firms with more educated workers would see their wages increase unlike workers in firms whose workforce's average educational attainment stays unchanged.

As to policy implications, our findings support the case for the public funding of (higher) education. Its benefits fall not only on the individuals that acquire those skills directly at schools but also on those persons that are "spilled over" at work. With respect to the evaluation of different labour market types, our findings are however less standard. Indeed, our results indicate that there will be less learning under competitive markets, as competition prevents employers from fully benefiting from their hiring of educated workers.

Finally, we also derive some results about the scope for education to generate endogenous growth, which are however less straightforward. On the one hand, the external benefit of education increases with the levels of education. On the other hand, increasing levels of education imply that the relative share of individuals that benefit from those external effects is increasingly smaller. External effects would then disappear in a possible long-run scenario in which all individuals have similarly high levels of education.

5. Conclusions

We contribute to the literature on social returns to education by putting forward a model of learning – in which workers learn from the schooling of their colleagues in the same firm – and testing empirically some of its implications. This model is shown to lead to a stronger relationship between wages and education at the firm level than at the individual level, at least in non-monopsonistic labour markets. The gap between the individual and firm level results is also shown to depend positively on the size of the education spillover. The empirical results are then based on the estimation of Mincer firm-level wage equations applied to a large Portuguese matched employer-employee panel, controlling for firm fixed effects and instrumenting firm average education.

Consistently with the predictions of the model, we found firm-level returns to education much above their individual-level counterparts. We also found evidence of significant wage spillovers to less-educated workers: their pay increases by 2% to 3% per extra year of education of workers in their firm. However, the subset of educated incumbent workers does not seem to benefit from such spillovers, a result which is again predicted by the model. The education spillover is also found to be stronger when examining job levels within firms, a more disaggregated level of analysis, which allows for stronger interactions among workers.

Taken as a whole, our evidence indicates that education has a significant external effect on productivity and wages within firms, implying social returns to education greater than private returns. More specifically, the results suggest that there is a multiplier effect in the provision of education, as its benefits are not only circumscribed to the individuals that invest in their own human capital but also on the workers that have not made that investment at school but then go on to interact with educated colleagues at their workplace. However, the scope of this multiplier effect to generate endogenous growth may be limited, as the external effects can only arise while there is dispersion in the schooling attainment of the labour force.

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Tables

Table 1 - Descriptive Statistics (Firm-Level Data)

Variable	Obs	Mean	Std. Dev.	Min	Max
Hourly earnings	27,994	3.75	2.47	0.92	87.16
Log Hourly Earnings	27,994	1.08	0.47	-0.11	4.41
Education	27,994	6.45	2.00	0.00	16.65
Experience	27,994	23.38	6.05	3.72	43.65
Experience^2	27,994	712.95	310.96	23.25	1942.44
Tenure	27,994	108.18	63.65	0.00	357.16
Tenure^2	27,994	231.36	218.91	0.00	1402.50
Female	27,994	0.42	0.30	0.00	1.00
Firm Size	27,994	213.41	615.13	50	29433
Age	27,994	35.83	5.72	18.51	53.70
Share Retirement Age	27,994	0.01	0.02	0.00	0.21
1991	27,994	0.10	0.29		
1992	27,994	0.10	0.30		
1993	27,994	0.11	0.31		
1994	27,994	0.11	0.32		
1995	27,994	0.12	0.33		
1996	27,994	0.12	0.32		
1997	27,994	0.12	0.33		
1998	27,994	0.12	0.32		
1999	27,994	0.10	0.31		
Lagged Education	23,164	6.38	1.98	0.00	16.65
Lagged Share Retirement	23,164	0.01	0.02	0.00	0.21

Notes: All variables are aggregated from worker-level data into firm-level data. Hourly earnings are measured in 2000 euros. Education indicates the number of years of schooling (based on the highest diploma obtained by the worker). Experience is Mincer experience (age-education-6) and is measured in years. Tenure is measured in months. Female is a dummy taking value one for women and zero for men. Firm size is measured in terms of the number of workers. Age is measured in years. “Share retirement age” is the percentage of the workforce of each firm-year that will reach retirement age in the following period (i.e. aged between 61 and 64 years, depending on the worker’s gender and the year being considered). 1991-1999 are year dummies. Lagged education and lagged share retirement are lagged values of each variable. Calculations done by the authors based on the “Quadros de Pessoal” data.

Table 2 – Results (Pooled OLS, RE and FE)

	Pooled OLS		Random Effects		Fixed Effects	
	Coeff.	**St. Error**	**Coeff.**	**St. Error**	**Coeff.**	**St. Error**
Schooling	0.178**	0.002	0.135**	0.003	0.050**	0.003
Experience	**0.072****	0.004	0.066**	0.002	0.035**	0.003
Experience^2	-0.001**	0.000	-0.001**	0.000	-0.001**	0.000
Female	-0.348**	0.009	-0.318**	0.009	-0.159**	0.021
Log Size	0.054**	0.004	0.047**	0.003	-0.003	0.005
Adj. R^2	0.7836					
Firms-year	27,994		27,994		27,994	

Notes:

All regressions include a **squared** tenure and year dummies.

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

* - significant at the 5% level

** - significant at the 1% level

Table 3 - Results, Fixed Effects and Instruments

	Coeff.	St. Error
First Stage		
Lagged Schooling	0.082**	0.005
Share of 65 and over	1.289**	0.246
Adjusted R^2	0.5186	
Partial R^2	0.0131	
F-statistic	8.58	(P-value= 0,000)
Main Equation		
Schooling	0.133**	0.019
Experience	0.066**	0.008
Experience^2	-0.001**	0.000
Female	-0.128**	0.017
Log Size	0.021**	0.007
Within R^2	0.5302	
Between R^2	0.2502	
Overall R^2	0.2529	
Overid. Test Statistic	1.307	(P-value= 0.253)
Observations	23,164	

Notes:
* - significant at the 5% level
** - significant at the 1% level

Table 4 - Results, Different Sub-Samples

	Mean Education		Median Education	
	Coeff.	St. Error	Coeff.	St. Error
Uneducated workers (stayers)				
First Stage				
Group Average Schooling	0.544**	0.011	0.596**	0.010
Lagged Total Average Schooling	0.168**	0.006	0.152**	0.006
Share of 65 and over	-0.170	0.279	-0.144	0.270
Main Equation				
Total Average Schooling	0.024**	0.011	0.033**	0.012
Group Average Schooling	-0.008	0.007	0.006	0.008
Overall R^2	0.3928		0.5062	
Observations	22,841		22,883	
Groups	4,824		4,828	
Educated workers (stayers)				
First Stage				
Group Average Schooling	0.021**	0.005	0.013**	0.005
Lagged Total Average Schooling	0.179**	0.006	0.188**	0.006
Share of 65 and over	-0.020	0.296	0.004	0.294
Main Equation				
Total Average Schooling	0.008	0.012	-0.003	0.012
Group Average Schooling	0.075**	0.002	0.077**	0.002
Overall R^2	0.7315		0.6504	
Observations	22,771		22,578	
Groups	4,820		4,804	

Notes:
All 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.

* - significant at the 5% level
** - significant at the 1% level

Table 5 - Descriptive Statistics (Firm/Job-levels)

Variable	Cells	Mean	Std. Dev.	Min	Max
Hourly earnings	177,662	4.49	3.86	0.51	198.08
Log Hourly Earnings	177,662	1.24	-0.61	0.67	5.29
Education	177,662	7.43	3.36	0.00	17.00
Experience	177,662	23.82	10.30	0.00	76.00
Experience2	177,662	751.84	542.17	0.00	5776.00
Tenure	177,662	112.62	89.58	0.00	758.00
Tenure2	177,662	254.57	340.69	0.00	5745.64
Female	177,662	0.37	0.37	0.00	1.00
Firm Size	177,662	231.76	647.31	50	29433
Age	177,662	37.25	9.59	14.00	87.00
Share Retirement Age	177,662	0.02	0.08	0.00	1.00
1991	177,662	0.09	0.29		
1992	177,662	0.10	0.30		
1993	177,662	0.10	0.31		
1994	177,662	0.11	0.31		
1995	177,662	0.12	0.33		
1996	177,662	0.12	0.33		
1997	177,662	0.12	0.33		
1998	177,662	0.12	0.32		
1999	177,662	0.11	0.31		
Lagged Education	142,176	7.30	3.31	0.00	17.00
Lagged Share Retirement	142,176	0.02	0.08	0.00	1.00

Notes: see notes of Table 1. Now variables refer to each firm/job-level cell.

Table 6 - Results, Fixed Effects and Instruments (Job Levels)

	Coeff.	St. Error
First Stage		
Lagged Schooling	0.015**	0.002
Share of 65 and over	-0.062	0.052
Adjusted R^2	0.3503	
Partial R^2	0.0003	
F-statistic	6.86	(P-value= 0,000)
Main Equation		
Schooling	0.209**	0.037
Experience	0.069**	0.010
Experience^2	-0.001**	0.000
Female	-0.180**	0.010
Log Size	0.041**	0.005
Within R^2		
Between R^2	0.6853	
Overall R^2	0.6515	
Overid. Test Statistic	0.58	(P-value= 0.446)
Observations	142,176	

Notes:
Both equations consider the same additional variables, as in Table 2.
* - significant at the 5% level
** - significant at the 1% level

__Appendix A__:

First, following our definition, we can write the total productivity as:

$$X = \int_0^1 yf(y)dy + \int_0^1 f(y)\,[\int_y^1 \theta(z-y)f(z)dz/f(y)]dy = \bar{y} + \theta\int_0^1 dy \int_y^1 (z-y)f(z)dz \qquad \text{(A1)}$$

To evaluate the extra productivity due to internal learning, $\theta\int_0^1 dy \int_y^1 (z-y)f(z)dz$, we have

$$\int_0^1 dy \int_y^1 (z-y)f(z)dz = \int_0^1 dy \int_y^1 (z-y)\beta z^{\beta-1}dz = \int_0^1 dy[\frac{\beta}{1+\beta}z^{\beta+1} - yz^{\beta}]_y^1$$

$$= \int_0^1 (\frac{\beta}{1+\beta} - y + \frac{y^{1+\beta}}{1+\beta})dy = \frac{\beta}{1+\beta} - \frac{1}{2} + \frac{1}{(1+\beta)(2+\beta)} = \frac{\beta}{2(2+\beta)} \qquad \text{(A2)}$$

Substitute (A2) into (A1), and recall that $\bar{y} = \beta/(1+\beta)$, we get $X = \bar{y}\,[1 + 0.5\theta/(2 - \bar{y})]$.

__Appendix B__:

The second-order condition for maximizing L in (3) is guaranteed. The first-order condition $\partial L/\partial \bar{w} = 0$ implies

$$\alpha\{p\bar{y}\,[1 + 0.5\theta/(2 - \bar{y})] - \bar{w}\,] = (1 - \alpha)\bar{w} \qquad \text{(B1)}$$

Solving (B1), we get the unique Nash bargaining solution for the average wage:

$$\bar{w} = \alpha p \bar{y}\,(1 + \frac{0.5\theta}{2-\bar{y}}) \qquad \text{(B2)}$$

Taking log of (B2) and writing $ln\alpha p$ as C, we get $ln\bar{w} = C + ln\bar{y} + ln[1 + 0.5\theta/(2 - \bar{y})]$.

Since $w = y\bar{w}/\bar{y}$, we have $lnw = ln\bar{w} - ln\bar{y} + lny = C + lny + ln[1 + 0.5\theta/(1 - \bar{y})]$.