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Private returns to education versus education spill-over effects

Or what co-workers account for!

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Abstract In this study we test whether education spill-over effects biase private returns to education. We newly investigate for such effects within narrowly defined occupational groups of a given firm using, going thus a step further than earlier studies analysing such effects at employer level or within regions. Using panel data from a Swiss employer-based labour force survey of 1996 we find clear empirical evidence for education spill-over effects on individual wages (2%). This result proved to be robust against an alternative explanation based on imperfect substitution between highly and less educated workers. Consequently, conventionally calculated private returns to education are shown to be biased upwards.

Keywords Workplace · Employer wage differentials · Human capital · Returns to education · Spillovers from education

JEL Classification D62 · J24 · J31

1 Introduction

Private returns to education measure the effect of an individual's own education on his or her wage. Conventionally calculated estimates of these returns,

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however, omit the fact that there may exist positive spill-over effects from one's own education to his/her co-workers' wages. Indeed, if more highly educated employees are working in a firm then there might be more opportunities for low-skilled workers to learn from higher-skilled co-workers.

Such positive spill-over effects from co-worker's education are particularly important as more educated people tend to segregate themselves into firms and jobs in which the average educational level is higher.[1] The higher educated then tend to benefit more from such human capital externalities. such positive externalities imply, then, that conventionally calculated private returns to education are biased upwards.[2]

The implications of these externalities, i.e., that returns to education may not be purely of a private nature, are the subject of analysis in a rather recent current of labour market literature.[3] So far, the evidence for such pecuniary education spill-over effects is mainly based on a positive correlation between wages and average education within regions or at the employer level. A clear identification of such education spill-over effects, however, is difficult at such aggregated levels. Such a positive correlation might just reflect other factors specific to this region or employer, which are positively correlated with the average educational level and which are difficult to control for. Taking the example of a firm, higher wages might well just reflect higher technology intensity of production and not be the result of higher incidence of knowledge transfer between co-workers as suggested by the notion of education spill-over effects (e.g., Hamermesh 1993; Bresnahan et al. 2002). Similarly, higher wages within a human capital intensive firm might reflect a wage policy aiming at minimising monitoring cost as stipulated by the efficiency wage theory. External effects of education might then be overestimated if any of these factors is omitted.

Therefore, in this study we go a step further than existing work, exploiting the specific features of the *Schweizerische Lohnstrukturerhebung*, a Swiss employer-based labour force survey. This data allows us to test for the impact of co-workers' education on an individual's wage within narrowly defined occupational groups of a given firm, after differencing out any confounding factor at the firm level. This more precise test for *education spill-over effects* is the

[1] See Kremer and Maskin (1996) for evidence from the USA, Britain and France, evidence for Switzerland is given in Sect. 3.

[2] Alternatively, larger positive externalities for higher educated persons may be considered as part of the private education return. This additional return takes then the form of a better opportunity to enter a firm with a highly educated workforce. In this case conventionally calculated private returns to education mainly overestimate the true private returns for persons having obtained only mandatory schooling. The overall effect for average educated is less clear while the the true returns for the higher educated are underestimated. In this study, however, private returns are defined as returns independent of co-workers' education keeping to the tradition of of standard human capital theory.

[3] Battu et al. (2001) find an effect of 4–22% per average education within workplaces using survey data for the UK. Barth (2000) finds an independent effect on own pay from the average level of education within an establishment between 1 and 4% per year of average education using matched employer–employee data from Norway.

first aim of our study.[4] Secondly, we investigate to what extent *private returns for education* are affected when such education spill-over effects are controlled for.

Looking at *external effects of education within occupational groups* exploits the fact that they are a natural control for closeness, and hence the frequency of professional interaction. Occupational closeness is also a measure for the compatibility of knowledge of co-workers, independently of the level of education. As an example, a software engineer might learn more from other software engineers than from other high skilled employees working in the management of the firm or the logistics department for instance, simply because they share the same problems at work and maybe even their office space. Similarly, the advice of such an engineer might also be demanded more frequently by a programmer and his or her assistant than the advice of a co-worker from a different occupational group. Managers or lawyers will themselves learn more often from their interactions with co-workers of their own occupational groups. This does not exclude that software engineers might give them useful advice, but is assumed to be less frequent. Hence, our main hypothesis is that employees working in an occupational group with a higher average educational level than otherwise identical employees will learn more working with them and thus earn higher wages.

However, the impact of differing technologies or wage policy at the firm level is not the only factor blurring the measurement of education spill-over effects. Additional alternative explanations will be tested in this study. First, unobserved heterogeneity of workers might bias the results. Hence, particular emphasis will be given to controlling thoroughly for the quality of workers and their education. Secondly, and even more importantly, imperfect substitution between high and low skilled workers as suggested by a conventional demand and supply model theoretically yields the same result for low skilled employees as education spill-over effects.

Following the explanation of imperfect substitution, the marginal productivity and hence the wage of the low skilled is a positive function of the number of highly educated colleagues and vice versa. Strongly simplified, the secretarial work done for a group of highly skilled engineers or the manager of a software firm might have a higher impact on aggregate productivity than the identical work done for a sales clerk working in a small supermarket where average education is low. The secretary in the software firm will thus earn a higher salary than the secretary of the sales clerk in the supermarket when working with a higher educated co-workers, even though the work is the same. In this sense, this effect is similar to the impact of a higher technology used in the firm on the productivity of the employees. In the case of education spill-over effects, on the contrary, there are learning effects of the employees assumed which should increase the productivity of an individual for longer than his employment spell

[4] The terms education spill-over effects, external effects of education and education externalities are used as mutual equivalents throughout the paper. Likewise, the terms firm, establishment and employer are used interchangeably.

at this firm. A secretary or software assistant is assumed to learn more from higher educated superiors in the software firm than when working in a supermarket.

In order to discriminate between these two explanations, we estimate the impact of co-workers' education on an individual's wage education for different educational levels. If a positive impact is found for the highly educated workers and not only for the less educated, we reject the explanation based on imperfect substitution but take the result as evidence for education spill-over effects.

Knowing whether there are education spill-over effects is important at many levels. First, the answer to this questions is important for an individual's education decision. If due to these spill-over effects the return to an additional year of education is lower than previously assumed and individuals are aware of it, they might prefer to invest their energy directly in searching a job within a human capital intensive working environment than increasing their education. More generally, if such education externalities exist, they can reasonably be assumed to affect a great variety of labour market processes, like the job search of individuals or the success of in-house training for instance, emphasising the need for micoeconomic theory to take them into account.

But knowing more about education spill-over effects will be equally important for economic policy. First, it would clearly strengthen the case for public subsidies for private education, as it is a well established fact that competitive markets underproduce in the presence of a positive externality (e.g., Hazlett 2000). Second, upgrading workers' skills in order to provide them with more flexibility in the labour market is an important aim of public policy given the changing structure of labour demand and high levels of unemployment. But upgrading workers' skills with formal education programs has generally proved rather difficult for workers with low educational levels. Hence, the concept of external effects of education would strengthen the case for public policy measures aiming at upgrading workers within the labour market, thus highlighting the need for empirical evidence in this field.

The rest of this study is organised as follows. Section 2 gives a more detailed overview on the relevant literature, in Sect. 3 the data used are described, in Sect. 4 the methodology and specification are detailed, in Sect. 5 the results are presented, and Sect. 6 concludes.

2 Literature review

The contribution of an individual's education to his/her income is a thoroughly discussed subject. The economic theory at the micro level was largely dominated by Jacob Mincer's (1974) formulation of the log-linear wage-education relationship. Estimation of this model using an identical data specification show that each additional year of schooling of a given individual appears to raise wage earnings by 5–15% (e.g. Card 1999; Harmon et al. 2001). Critics of Mincer's model concentrated on possible selectivity problems due to differences in ability and time preferences (discount rates) of individuals of different educational levels. In principle, such omitted variables (e.g., inherent ability) could cause simple OLS estimates of wage equations to significantly overstate the return to education. But, surprisingly, little empirical evidence was found to support this view, leading to the conclusion that Mincer's standard model fits the data rather well.[5] However, this micro human capital literature completely ignored possible education spill-over effects on individual earnings.

The literature about external effects of education is sizeable. On the one hand, non-market externalities of schooling are shown to be substantial by Wolfe and Zuvekas (1995), among others. On the other hand, and more related to the pecuniary benefits of education, there is a second strand of literature, the recent neoclassical growth literature, which takes human capital explicitly into account, as in the work of Lucas (1988) and Romer (1990). The impact of education on income is modelled not only as an independent factor of production but also as a factor having an additional impact on the aggregate level. Nevertheless, finding robust empirical evidence for the positive impact of the level (externalities) and variation (factor input) of education on the per capita income growth of a country is a more challenging task, especially as measurement of education is a serious problem. Krueger and Lindahl (2000), for instance, review such models and problems associated with their estimation.[6] But their own estimations show that returns to schooling in cross-country models are indeed larger than those found using micro-level regressions, suggesting possible aggregate spill-over effects from higher educational levels. Moretti (2004a,b) finds robust evidence for significant education spill-over effects within cities in the USA. More precisely, Moretti (2004a) finds that a percentage point increase in the supply of college graduates raises high school drop-outs' wages by 1.9%, high school graduates' wages by 1.6%, and college graduates wages by 0.4%. Arguing correctly that imperfect substitution between high and low skilled workers as suggested by a conventional demand and supply model theoretically yields the same result for low skilled employees as education spill-over effects, he concludes that only the impact on the college graduates can be taken as evidence for such effects. Acemoglu and Angrist (1999), to the contrary, find little evidence for sizeable external returns to education when exploiting an exogenous variation in the length of compulsory schooling in US states. Arvanitis (1999), using data for Switzerland at the firm level, shows empirical evidence for a direct impact of human capital in related industries on the value added of a firm. It is, however, difficult to separate causal effects of education in such aggregated data, a fact also stressed by the authors of the above studies.

[5] Available micro evidence as summarised by Card (1999) and Harmon et al. (2003) is suggesting that the return to an additional year of education obtained for exogenous reasons like an increase of the compulsory years of schooling is more likely to be greater, not lower, than the conventionally estimated return to schooling. This result supports the idea that less educated individuals have a higher time preference (discount rates) than high educated individuals.

[6] For high technology industries an indirect causal link between human capital formation and productivity through the impact on research and innovation is empirically better documented, see Zucker et al. (1998) and Jaffe et al. (1993).

The literature, so far, has produced very few studies looking at evidence for education spill-over effects at the employee level, although measurement problems may be reduced with decreasing levels of aggregation. To date, such attempts concentrated on such effects at the employer level, finding a significant impact of average education on an individual's wage. Barth (2000) argues, for example, that external effects generally materialise through the learning/teaching process or through the organisational skills of the better educated co-workers.[7] Similarly to Barth (2000), Destré et al. (2001) also emphasise the learning/teaching channel, finding positive wage effects of the learning potential, proxied by co-workers' education and their tenure within firms. Battu et al. (2001) follow the idea, as suggested by Kremer (1993), that different educational levels measure skill incompatibility rather than skill complementarity. Based on this idea, they investigate whether education spill-over effects arise mainly within similar skill levels. Their results, nevertheless, do not confirm this hypothesis, and they conclude that education spill-over effects tend to materialise in a multidimensional way between workers of different educational levels above the mandatory schooling threshold.[8]

One general shortcoming of all these studies is that the precise identification of *education spill-over effects at firm level* is difficult due to various confounding factors. The observed correlation between average education within a firm and an individual's wage may just reflect other employer specific factors, such as differences in wage policy or the internal organisation structure of the firm. Efficiency wage policy aiming at minimising monitoring costs, for instance, could reasonably be assumed to be positively correlated with the share of high skilled employees, if monitoring costs increase with the complexity of work. Similarly, differences in the degree of introducing labour organisation flexibility and thereby gradually abandoning a Taylorist organisation, as suggested by Lindbeck and Snower (2000), may lead to differences in wage policies of firms. Furthermore, the physical capital or technology intensity chosen by the employer may also be positively correlated with the human capital intensity and the wage level of the firm. Finally, and of equal importance, imperfect substitution between high and low skilled workers as suggested by a conventional demand and supply model theoretically yield the same result for low skilled employees as education spill-over effects. In short, external effects of education might indeed be

7 It is important to note that these results are based on the assumption that some rent-sharing takes place in the labour market for workers to benefit from the productivity gain of the higher human capital of their co-workers. Otherwise, following the assumption of a competitive labour market without information and search cost, workers with low skilled co-workers would want to work with highly educated co-workers for any wage higher than their present wage and firms would prefer to employ them at any rate lower than their presently offered wages.

8 In a subsequent study restricted to two service sectors in the UK, Battu et al. (2004) found again robust evidence for education spill-overs effects at the workplace. However, the hypothesis of a complementary relationship between co-workers' human capital as suggested by Kremer (1993) was not confirmed by their results. Similarly, external effects for training were tested, but could not be found in this restricted sample.

overestimated if there are any omitted factors which are positively correlated with the educational level of employees.

The problem of firm heterogeneity has been addressed in earlier research in diverse ways. In Barth's study, the robustness of the results for capital intensity was tested, as the literature suggests a complementary relationship between these two factors of production. Battu et al. (2001) control for labour proportions of operating costs for similar reasons and for teamwork as a measure for the interaction intensity of co-workers working for the same employer, while Destré et al. (2001) control for the existence of an explicit wage policy. Despite the apparent need for controlling for such factors, the diversity and contingency of the factors included clearly highlight the importance of this identification problem at the firm level. Battu et al. (2001); Battu et al. (2004), indeed, acknowledge the need for looking at education spill-over effects on workers of the same team. Hence, none of these studies looks at spill-over effects of education at a more disaggregated level *within* the firm, thus controlling for unobserved heterogeneity of employers, industries and regions. Moreover, none of these studies analysing education spill-over at the employer level tested whether their measure of external effects of education did not merely reflect the impact of imperfect substitution between workers of different skills on the earnings of the lower skilled.

3 The methodology

As in the previous study the framework of analysis is given by Mincer's (1974) wage equation:

$$w_{ij} = X_{ij}\beta_{ols} + \epsilon_{ij}, \tag{1}$$

where $i = 1, \ldots, N$ and $j = 1, \ldots, J$ indices for individuals and firms, respectively. w_{ij} is the natural logarithm of hourly wages. X_{ij} is a vector of standard wage relevant individual characteristics: age, tenure, dummies for part-time work, foreign nationality, service sector activity, and, in particular, number of years of education (E_{ij}). β_{ols} is the corresponding vector of coefficients including the coefficient of the education variable, measuring *conventionally calculated private returns to education*, β^{E}_{ols}. The error term, ϵ_{ij}, is i.i.d. with mean zero and variance σ^2.

As mentionned at the beginning of Sect. 2 Mincer's model is strongly criticised for its failure to take into account an eventual selectivity bias due to the schooling decision of an individual. If schooling is not random but endogenous then the error term, ϵ_{ij}, capturing unobserved individual characteristics is correlated with the number of years of education (E_{ij}). Following the idea that more able individual choose to obtain higher education more frequently than less able individuals, this correlation is expected to be positive and conventionally calculated private returns to education, β^{E}_{ols}, are overestimated. However, differences in time preferences may act as a contradicting influence. It may be that less educated individuals choose to leave school and enter the labour market at an earlier age than the higher educated because they have a significantly higher preference for present as compared to future consumption. In this case the correlation between the error term, ϵ_{ij}, and the number of years of education (E_{ij}) is negative. Conventionally calculated private returns to education, β^{E}_{ols}, are then underestimated. Hence, ideally the Mincer's wage equation should be enlarged in order to take into account of these conflicting influences. This is, however, beyond the scope of this study. Moreover, the data set used is lacking appropriate information concerning ability or time preference of individuals approximated frequently by family background variables. This problem is mitigated somewhat by the fact that empirical evidence is available for both explanations leading finally to the conclusion that Mincer's wage equation fits the data rather well. Hence, although individual heterogeneity can not be controlled for throughly, Mincer's age equation is taken as a starting point.

We can, however, control for employer heterogeneity as a representative sample of employees is available for any firm in the data set. For that the wage equation (1) is generally enlarged by firm-specific variables not directly related to human capital, Z_j, like size, region and industry of the firm. In addition, weighted least squares estimation (*wls*) is used, due to the survey design. Our first model (*Model 1*) will thus be the following:

$$w_{ij} = X_{ij}\beta_{wls} + Z_j\eta_{wls} + \epsilon_{ij}, \tag{2}$$

where the estimate of private returns to education is, thus, measured by β^{E}_{wls}, the coefficient of the education variable in this equation.

Model 1 as a point of reference assumes that there are no external effects of education. Hence, an individual's wage depends only on his or her own individual education, β^{E}_{wls}, being indeed the true measure of private returns to education.

Against this reference model we will test in a first step *Model 2* where *education spill-over effects within firms* (*Model 2*) independent of occupational groups are assumed. For that we add controls for co-workers' education at the same firm but not within the same occupational group. We follow in that Barth (2000) and Battu et al. (2001). This will be done using a 2-step formulation of this wage equation following the spirit of the literature of inter-firm wage differentials introduced by Groshen (1991) and Abowd et al. (1999). First, it allows simultaneously and in an elegant way to control for the cluster structure of the data introduced by the two-stage sample survey design, see Peffermann and LaVange (1989) for a detailed discussion.[9] Second, this method has the advantage of differencing out any employer-specific factor. Using in addition robust estimation techniques like the Hubert-White sandwich estimator for the standard errors makes sure to take into account any remaining heteroscedasticity linked to firm

[9] Using appropriate weighting and clustering options, a 1-step wage estimation including individual (within-differences and averages) and firm characteristics were tested to give the same results, available from the author upon request.

size or industry affiliation. This will be important in a subsequent step when estimating the impact of co-workers education at occupational group.

More precisely, the Mincer wage equation is augmented to account for firm level fixed effects, as follows:

$$w_{ij} = \sum_{j=1}^{J} D_{ij}\gamma_j + X_{ij}\beta_w + \epsilon_{ij}, \tag{3}$$

where D_{ij} is a vector of dummy-variables indicating whether individual i works in firm j, and γ_j is the corresponding firm fixed effect. Equation (3) can be estimated by OLS which is equivalent to a regression of the deviation of each individual's log wage and X variables from their respective firm-specific means (e.g., Greene 1997, pp. 615–619). For technical reasons the second procedure is chosen. Furthermore, this method allows give an unbiased estimate of the firm fixed effect γ_j. For a consistent estimation of γ_j, however, a large number of observations for any given firm is required. Moreover, it is important to note that the resulting vector of coefficients β_w utilises the within-firm variation only.

The education spill-over effects within firms is then found when analysing the fixed effect γ_j which in fact measures the firm's pay premium. More precisely, the fixed effect γ_j is then expressed as a function of the firm-specific means of the explanatory variables of the wage equation, $\bar{X}_j$, including the average number of years of education $\overline{E}_j$ and other firm-specific variables, Z_j, like size, region and industry of the firm.

$$\gamma_j \ = \bar{X}_j\delta \ + Z_j\eta + v_j, \tag{4}$$

δ and η are the coefficient vectors to be estimated, including δ^E, the coefficient of average education at the firm, measuring the education spill-over effects at firm level we are intersted in. The error term, v_j, has expected value zero and variance σ_v^2. Moreover, v_j is independently distributed of the right-hand-side variables, $\bar{X}_j$ and Z_j. The error terms, ϵ, in Eq. (3) have also expected value zero, but are heteroscedastic with variance σ_v^2/n_j, n_j being the number of individuals working for firm j. In principle, the vector of coefficients δ and η could result from estimating Eq. (4). For the sake of computational simplicity, however, we calculate the between- and the within-firm estimators and estimate the vector of coefficients δ which we are interested in by subtracting the second from the first. Since the within- and between-firm variations are orthogonal and since both are uncorrelated with the respective X-variables, the variance of δ is simply the sum of the variances of β_w and β_b. See Wirz (2005) for a more detailed discussion.

Model 2 thus assumes that education spill-over effects arise at the firm level only. Hence, δ^E is expected to be statistically significantly different from zero. The resulting β_w^E is then the true measure of private returns to education corrected for spill-over effects at the firm level.

In a third step a new and more restrictive test is done with *Model 3*, where we allow for *education spill-overs to take their effect within the occupational group* over and above their effect at the firm level. If there is evidence found for such effects at more disaggregate level, we conclude that external effects of education indeed exist. In order to test that, the variable ECW_{ioj} is added to the vector of wage relevant individual characteristics (X_{ij}) as described in *Model 2*. ECW_{ioj} is the level of education of co-workers within the same occupational group o of individual i in firm j and is therefore the main variable of interest. To be precise, when speaking of co-workers of the same occupational group, only co-workers of the same firm are included. Workers of the same occupational group working in different firms are described as by "employees of the same occupational group". β_w is the corresponding vector of coefficients including β_w^{ECW} which measures finally the education spill-over effects within occupational group we want to test. The estimation procedure is identical to the one decribed for *Model 2*.

Model 3 finally assumes that education spill-over effects arise at the firm level and within occupational groups. Hence, δ^E and β_w^{ECW} are expected to be statistically significantly different from zero. Moreover, the resulting β_w^E is then the true measure of private returns to education corrected for spill-over effects at both levels.

All three models will be estimated in two specifications. *Model 1b, 2b, 3b* assume an identical wage structure for both gender. Whereas, *Model 1a, 2a, 3a* allow the wage structure of men and women to differ with respect to the slope coefficients. As an exception to that, co-workers' years of education and the dummy for no-coworker in occupational group are not interacted with gender for the sake of simplicity. Similarly, the firm effects are constrained to be equal between men and women.

Tests for alternative explanations, e.g. unobserved individual heterogeneity or imperfect substitution between high and low skilled workers, will be done with *Model 3* using information on education, tenure and detailed occupational classification.

Finally, if robust evidence is found for such education externalities, we expect the estimate of private return to education corrected for these spill-overs, β_w^E, to be lower than conventional estimates, β_{wls}^E. Due to the fact that the segregation of highly educated workers into human capital intensive firms paying higher wages might be considered a part of the returns to individual education, this estimate should tend to be a lower bound. But in any case, the comparison between β_{wls}^E and β_w^E gives a first indication of the importance of the bias arising if external effects are ignored.

4 Data and descriptive statistics

Data structure This study uses linked employer-employee data from for the year 1996 from the *Schweizerische Lohnstrukturerhebung (LSE)*. The LSE is a biannual employer-based survey conducted by the *Bundesamt für Statistik (BFS)*, the Swiss Federal Statistical Office, in order to monitor the level and structure of wages in Switzerland.[10] This data represent a 10% sample of esta-

[10] See also Wirz (2005).

blishments in Switzerland stratified by industries and size classes and gives detailed information on workers' compensation, jobs, and firm characteristics.[11,12] Small firms in the survey have to report on all their employees, while large firms report only on a part of their workforce. This implies that LSE is representative at the job-level. As many firms choose to give information about a larger share of employees than indicated by the survey design initially, these survey weights have to be taken into account in summary statistics representing the total employed workforce.

In this study the terms firm and establishment are used as mutual equivalents. It is important to note, however, that within this terminology two separate firms may belong to a same unknown company. Still, a given firm may have several geographical locations or outlets of its business. For the purpose of our analysis we define firms having their business locations within only one of the seven regions. This is based on the assumption that outlets of the same firm working in the same region do share some centralised services like training, technical support and the like. Put differently, we assume that there is interaction between the employees of a given firm working in the same region. By redefining the firms in that way the number of firms increases somewhat. The estimation method applied differences as described in Sect. 3 controls thoroughly for heteroscedasticity due to enployer characteristics (industry, region, groups of firms) and any clustering of data at firm or higher aggregation levels.

Sample definition To increase the homogeneity of the firm and employee data sample we restrict our sub-sample to the private sector and consider only firms with at least six observations. For the same reasons some industries/sectors, such as agriculture, forestry and related services, other mining and quarrying, health and social work, and private sector educational facilities were excluded. These are small industries, where the number of observations was too limited to yield reliable results. This restricts our sample to 6,753 firms and 408,008 employees.

Descriptive statistics of total sample A look at the first two columns of Table 1 indicates the characteristics of this representative sample of the employed workforce in Switzerland. Employees have on average 12 years of education, are around 40 years old and two-thirds of employees work in service sector related occupations.[13] Overall, this is not surprising for a highly developed country. But, rather high in international comparison are the share of employees with foreign nationalities (0.31) and the share of part-time workers (0.21). The former is due to high immigration as well as a restrictive naturalisation policy.[14]

[11] The LSE reports standardized gross hourly wage rates not accounting for possible overtime pay.

[12] Register data (BUR, Schweizerisches Betriebs- und Unternehmensregister).

[13] The LSE does the highest level of completed education rather than years of schooling, see Weber et al. (2001) for a description of the Swiss educational system. Based on the work of Weber (1998) average years of schooling applying to each education level by gender are approximated, see Table 10 in the Appendix for details.

[14] Most foreigners have a Swiss education certificate, indicating that they grew up in Switzerland.

Table 1 Descriptive statistics by human capital intensity of the firm

	Mean	SD	Subsamples		
			Average years of education (E_j) in firm		
			10.5–11.5 Mean	11.5–12.5 Mean	12.5–13.5 Mean
Hourly wage rate	30.63	12.32	26.42	30.40	33.91
Hourly wage rate (log)	3.36	0.26	3.22	3.37	3.48
Firm fixed effect (log)	0.00	0.19	−0.06	0.00	0.05
Employee's years of education	12.10	1.32	11.02	12.10	12.84
Education information missing, share	0.20	–	0.15	0.16	0.21
Age	39.41	5.12	39.94	39.62	36.64
Tenure	8.92	4.31	8.90	9.21	9.36
Service occupation, share	0.59	–	0.51	0.56	0.61
Occupation information missing, share	0.11	–	0.01	0.16	0.08
Non-Swiss nationality, share	0.31	–	0.42	0.26	0.21
Part-time, share	0.21	–	0.22	0.20	0.16
Women, share	0.36	–	0.40	0.36	0.28
Co-workers' years of education in occupational group, average	10.42	2.93	9.94	10.33	11.16
No co-workers, share	0.10	–	0.08	0.12	0.11
Firm size (log)	3.84	3.85	4.03	3.54	4.06
Firm size information missing, share	0.04	–	0.04	0.06	0.05
Number of observations (firms)	6,753		1,129	2,268	1,558

Data weighted by the number of observations within the firm and the survey inclusion probability
Source: LSE(1996)

The latter is due to a combination of a high labour market participation rate of women, high number of working hours of standard labour contracts, and rather unfavourable institutional arrangements concerning child care and maternity leave in Switzerland (similar to Anglo-Saxon countries like the USA or the UK). These factors, reflecting the heterogeneity of the labour force, are taken into account in our model specifications. As a general characteristic of the Swiss labour market, unions are rather weak, membership and coverage are low, and employment protection measures are not very far-reaching. Despite this fact, Switzerland has low and rather stable wage inequality similar to other continental European countries, explaining why conventionally measured private returns (7%) are at the lower bound in international comparisons, as we will see in Sect. 5.

Descriptive statistics by human capital intensity of the firm The next three columns in Table 1 show the same worker and firm characteristics by increasing human capital intensity of the firm. This is measured by the variable average years of education of the firm. The ranges of this variable for the low, medium, and high human capital intensity categories are 10.5–11.5, 11.5–12.5, and 12.5–13.5 years, respectively. The employer wage premia vary between −0.06 and 0.05 log points over this range of employers and highlight the large wage impact of the human capital intensity of the firm, confirming earlier studies. The employer wage premia result from a fixed effect wage equation, and hence are corrected for human capital characteristics of the individual employees. For most of the remaining characteristics the differences are small. Generally employees in firms with high human capital intensity tend to be a few years younger, have slightly more tenure besides being better educated. Furthermore, Swiss citizens and male employees are overrepresented in these high wage firms. Logically, the average educational level of the co-workers within the same occupational group increases with the average educational level of the firm. Finally, it is interesting to see that firm size does not appear to be correlated with the human capital intensity of the firm.

Human capital intensity of occupational groups Table 2 lists the 24 occupational groups as defined in the LSE which will allow us to determine the wage effect of co-workers' education within occupational groups, the main focus of this study. As shown by the mean and the standard deviation of the employees years of education by occupational group, all skill levels of jobs and hence employees of all educational levels are found within most of these groups.[15,16] This allows for sufficient variation in co-workers' education at the occupational level.

Employees without co-workers in their group Particular care will be given to the treatment of employees working alone in their occupational group. These individuals tend to have higher educational levels than comparable employees with co-workers working in a different firm. This becomes visible in Table 1, where co-workers' education within an occupational group is shown to be lower (1–2 years) than the aggregate education at the firm level.

However, the share of such individuals working in isolation is quite uniform across all human capital intensity levels at around 10% of the employees (Table 1). There is more variation of the share of these individuals across occupational groups as shown in the last column of Table 2, although this share is mostly lower than 0.10. Exceptions are the occupational groups related to the management of the firm, its administrative tasks (groups 20, 21, 22, 24). This probably reflects the fact that in small firms only one person is in charge of these key tasks. This gives the overall impression that, albeit not important as its share within the sample, such a 'solitary status' within their occupational groups might hide a non-random selection process. A precise analysis of the selection process into such "solitary status" or alternatively into jobs with co-workers in the same occupational group is contingent on a more comprehensive data set than the LSE. In particular, a variable explaining this selection but which is not correlated with wages is lacking. Consequently, endogenizing or instrumenting such a selection process in a statistical and economically reliable way is not possible.

[15] Employers indicate the required skill level of job in a variable which takes values from 1 to 4 on a discrete scale.

[16] An exception is group 20 'Goal setting and defining of firm strategy', which is rated as requiring the highest skill level in a job.

Table 2 Education years by occupational groups

	Occupational group (OG)	Number of employees	Employees' years of education in OG		No. co-workers in OG
			Mean	SD	Share
10	Manufacturing and processing of products	62,629	11.1	1.82	0.015
11	Construction sector activities	11,815	11.7	1.71	0.013
12	Installation, operation, and maintenance of machinery and equipment	23,348	11.9	1.57	0.080
13	Restoration, craftsman's work	366	11.3	1.74	0.114
20	Goal setting and defining of firm strategy	6,409	14.6	2.12	0.462
21	Accounting and human resource management	12,780	12.8	1.66	0.386
22	Secretarial and administrative work	19,743	12.2	1.34	0.241
23	Other commercial, administrative work	28,264	12.3	1.63	0.155
24	Logistics, general management support	9,636	13.0	2.20	0.214
25	Assessment, consulting, authentication	19,594	13.7	2.28	0.073
26	Purchasing/sale of raw materials and investment goods	5,964	13.1	1.71	0.151
27	Sale of consumption goods and services	60,210	11.7	1.61	0.031
28	Research	9,224	14.9	2.56	0.090
29	Analysing, programming, operating	11,130	13.7	2.22	0.103
30	Planing, engineering, technical drawing, designing	11,955	13.8	1.87	0.110
31	Transportation of persons, goods and services	23,902	11.0	1.76	0.068
32	Security and protection services	1,869	12.2	1.72	0.064
33	Medical, care taking, and social work activities	626	13.2	2.25	0.086
34	Personal hygiene and clothes maintenance	1,214	11.7	1.23	0.048
35	Cleaning and public hygiene services	6,026	10.0	1.49	0.219
36	Pedagogical services	1,581	13.4	2.74	0.151
37	Catering and housekeeping services	16,997	10.8	1.82	0.013
38	Culture, information, entertainment, sport and free-time activities	2,383	13.2	2.44	0.040
40	Other activities	13,676	11.9	2.28	0.088
0	Missing information	46,667	12.1	2.14	0.007

Data weighted by the survey inclusion probability. Standard deviation are calculated within the occupational group of the total sample
Source: LSE(1996)

Table 3 Co-workers' education by education level of the employee

Aggregate education classification	Average education years of co-workers		No co-workers (share of employees)	Number of employees
	in firm	in OG		
Tertiary education level	13.4	14.6	0.152	28,555
Upper secondary level	12.6	13.0	0.169	28,924
Vocational education	12.1	12.2	0.111	173,953
Mandatory schooling and on the job training	11.3	10.6	0.048	93,128
Foreign education, MEI	13.1	11.7	0.028	83,448
Total number of observations (employees)				408,008

OG occupational group. Data weighted by the survey inclusion probability. Tertiary education level includes university and tertiary education of the vocational education system. Upper secondary level includes upper secondary education of vocational and non-vocational education system and teacher education. Vocational education is a professional education certificate, recognized as federal degree. On the job training is vocational education which is not recognized as federal degree. Foreign education is a non-Swiss education degree. *MEI* missing education information. See Tables 9 and 10 in the Appendix for a detailed overview
Source: LSE(1996)

Because of this we restrict the analysis of education spill-over effects within occupational groups to employees having co-workers and we do not compare them to individuals working alone in their occupational group. We do this by creating first a dummy variable, *DCOW*, indicating that the individual has *'co-workers'* and interacting it with co-workers' education at the occupational group level. Second we add a variable (DNCOW=1−DCOW) for individuals *'without co-workers'* in the same occupational group in the wage regressions.

Variables with missing information Missing information on education, occupation and firm size is taken into account using additional dummies, in the same way as we did for these individuals working in isolation, in order to keep the sample as large as possible. For the remaining variables, such missing information concerns only a few observations which are excluded. However, this lack of information is of particular importance to the education variable. Part of it is due to the fact that some foreign workers have (foreign) education that does not fit into the Swiss classification system. This is coded as missing. Rather than excluding these observations from the sample we treat individuals with missing observations on education as a separate education category (MEI). Overall the group with missing information on education is comparable to the other groups except for slightly lower wages and a little higher tenure. Nevertheless, this problem could affect the measure of co-workers' education within occupational group. In order to increase the reliability of this measure, this variable is defined to be missing if education information is available for only 20% or less of co-workers within the same educational group.

Educational segregation by firm and occupational groups Table 3 shows the average number of years of education of co-workers within the firm and within the occupational group (of the same firm) by aggregate educational level of the individual, see Table 11 in the Appendix for a precise definition. This gives a measure of the segregation by skill levels. Such educational segregation is even higher across occupational groups (second column) than across employers (first column). University graduates' co-workers have on average two years more education than co-workers of employees having followed only mandatory schooling. But, university graduates' co-workers within the same occupational group have even four more years of education than the corresponding co-workers of the lowest skilled. Employees without co-workers in the same occupational groups are mainly medium or high skilled (share between 0.11 and 0.17), and less often low skilled (0.05).

Overall, this first look at the data confirms the positive correlation of education with the employer wage premia and the strong segregation pattern of employees with different human capital across employers and occupational groups.

5 Results

5.1 Education spill-over effects within firms

5.1.1 Basic model

The results of the estimation of *Model 1a* in the first column of Table 4 show that the *conventionally calculated private return to an additional year of education*, β_{wls}, amounts to 0.071 log points in our overall sample. For detailed results of this equation see Table 9 in the Appendix. As differences in log wages indicate approximately relative differences this is equivalent to around 7%. This simplification will be used throughout the study. In *Model 1b*, which allows the wage structure to vary by gender, we find an almost identical estimate of around 0.072 log points for men, and a slightly lower estimate for women (0.067 log points). These results are at the lower bound of the private returns to education found for Switzerland in earlier studies as shown by Weber et al. (2001). This should be due to the more extensive specification of the wage equation.

In a second step we estimate *Model 2a* assuming now that *education spill-over effects arise within firms*. The results presented in the third column of Table 4 show that the coefficient δ^E, measuring these spill-over effects, amounts to 0.022 log points (or around 2%). The corrected *private return to education*, β_w^E, are at 0.060 log points or 6% around one percentage point lower than conventional returns. Introducing gender effects into the wage structure in *Model 2b*, the results are not altered noticeably (fourth column), the corrected return for women (0.053 log points) are also slightly lower than for men (0.063 log points). The quality of fit for these models, measured by the relevant R-square is around 50%, a very satisfactory fit for a cross-sectional wage equation estimation. The assumption of equality of employer-specific fixed effects is clearly rejected, based on the F-test statistic above 30.

Table 4 Education spill-over effects within firms

Estimation model: Dependent variable: log hourly wage	1a WLS	1b WLS	2a W/B	2b W/B
Employee's years of education:				
Both gender	0.071	–	0.060	–
	(0.001)**	–	(0.000)**	–
Men	–	0.072	–	0.063
	–	(0.001)**	–	(0.000)**
Women	–	0.067	–	0.053
	–	(0.001)**	–	(0.000)**
Average years of education in firm:				
Both gender	–	–	0.022	–
	–	–	(0.002)**	–
Men	–	–	–	0.022
	–	–	–	(0.003)**
Women	–	–	–	0.022
	–	–	–	(0.004)**
Number of observations (employees)	408,008	408,008	408,008	408,008
Number of observations (firms)	–	–	6,753	6,756
R–squared (standard)	0.526	0.535	–	–
R–squared (within)	–	–	0.469	0.487
F–test for firm fixed–effects	–	–	34.9	35.5

Coefficients denoted by ** are significant at 1% level. WLS shows the result from weighted least squares estimation. W/B indicates within and between estimation results from two-step estimation described in Sect. 3

Both estimation models include furthermore as individual variables: age, age squared, tenure, tenure squared, dummies for service or unclassified occupation, foreigner, women, part-time workers, additional dummies for individuals with missing information and for individuals with missing information for their own or co-workers' education or their activity and firm level controls for seven regions, seven industries and firm size. In Model 1a and 2a the wage structure of men and women is supposed identical but differs in Model 1b and 2b. The firm effects, however, are always restricted to be equal between men and women (Models 2a and 2b)

Source: LSE(1996)

These results confirm the findings from previous studies about private returns to education and the positive correlation between average education at the firm level and the employer wage premia. For Switzerland, this was shown previously by Ramirez (2000) and Wirz (2005). The estimate for education spill-over effects at firm level for Switzerland is at around the lower bound of the results for Norway (between 2 and 6%), but it is much lower than the estimates found for the UK (between 4 and 22%). For Switzerland the private return to education decreases substantially when corrected for external effects of education, by around one percentage point to 6% for both genders taken together.

5.2 Education spill-over effects within occupational groups

5.2.1 Basic model

A new and more restrictive test for external effects of education is developed in this study by analysing the correlation between wages and co-workers'

Table 5 Education spill-over effects within firms and occupational groups

Estimation model: Dependent variable: log hourly wage	3a W/B	3b W/B
Employee's years of education:		
Both gender	0.052**	–
	(0.000)	–
Men	–	0.054**
	–	(0.000)
Women	–	0.046**
	–	(0.000)
Co-workers' years of education in occupational group, average	0.041** (0.000)	0.040** (0.000)
No. co-workers in occupational group, dummy	0.604** (0.000)	0.588** (0.006)
Average years of education in firm:		
Both gender	0.020**	–
	(0.005)	–
Men	–	0.021**
	–	(0.005)
Women	–	0.019**
	–	(0.006)
Number of observations (employees)	408,008	408,008
Number of observations (firms)	6,753	67,53
R-squared (within)	0.482	0.499
R-squared (between)	0.550	0.557
F-test for firm fixed-effects	30.7	31.1

Coefficients denoted by ** are significant at 1% level. W/B indicates within and between estimation results from two-step estimation described in Sect. 3

Both estimation models include furthermore as individual variables: age, age squared, tenure, tenure squared, dummies for service or unclassified occupation, foreigner, women, part-time workers, additional dummies for individuals with missing information and for individuals with missing information for their own or co-workers' education or their activity and firm level controls for seven regions, seven industries and firm size

In Model 3a the wage structure of men and women is supposed identical but differs in Model 3b. In both models, however, the firm effects are restricted to be equal between men and women

Source: LSE(1996)

education within clusters of occupational groups at a given firm (*Model 3*). As discussed in Sect. 4, this measure has the advantage of not being affected by employer-specific effects. Theoretically, it can be argued that these additional education spill-over effects arise because workers within the same occupational group have a higher interaction frequency. With increasing closeness in professional activity the potential for learning from co-workers may also be higher.

The results in Table 5 show that there are indeed *education spill-over effects within occupational groups* over and above their *effects at the firm level*. More impressively, the effects within occupational groups (0.041 log points or around 4%) are twice the size of the effects at firm level (0.020 log points or around 2%). Introducing a gender-specific wage structure as in *Model 3b* displays a similar picture.

The coefficient of the dummy for no-coworkers in the occupational group confirms our *a priori* expectation that such individuals working in isolation are not a randomly chosen subsample of employees but clearly earn above average wages. This wage premia is, however, overestimated by the coefficient 0.604, as the impact of co-workers' average education years within occupational groups ($10.42 \times 0.041 = 0.427$) has to be taken into account. This impact is differenced out in the estimation of the co-workers' education years coefficient. Hence, the true wage premia of these individuals working in isolation is thus the difference between the average and the estimated coefficient which amounts to 0.177 log points ($= 0.604 - 0.427$). Intuitively, it seems indeed reasonable to expect that individuals with additional unmeasured qualifications such as professional training or a high degree of self-confidence and independent thinking are chosen for such autonomous positions. But in addition to that, the overrepresentation of employees of occupational groups related to the management of the firm in this subsample mentioned earlier should explain a large share of this wage premia.

Despite the fact that a formal causal relationship regarding a positive spill-over effect of one's co-workers' education on his/her wage cannot be established with this data, these findings clearly strengthen the case for the external effects of education. This, in addition, confirms the *a priori* expectation that closeness in professional activity matters for such effects. The estimate of an individual's *private return to education* corrected for education spill-over effects at firm and occupational group level amounts to 0.052 log points or around 5% for the total sample (0.054 log points for men and 0.046 log points for women). This estimate is about two percentage points lower than the conventional estimates of private returns to education.

5.2.2 Unobserved worker and education heterogeneity

So far, our estimates of external effects of education are based on the hypothesis of workers being homogenous in unobserved characteristics and that there is no systematic sorting of individuals within firms determined by unmeasured characteristics. In order to evaluate the impact of such unobserved heterogeneity as far as possible we perform several robustness tests in the following subsections using information on education, tenure and detailed occupational classification.

Sample of employees with mandatory schooling only Qualitative differences in education may arise above all when individuals differ in the subject and quality of their education, ending up working in different jobs. This may result in different wages for individuals having equal years of education and experience. Furthermore, differences in the quantity and quality of unmeasured formal training add to the problem. These problems are minimised by restricting the sample to employees who followed mandatory schooling only, because this education is of quite comparable quality across Switzerland and formal training of these employees is shown to be rather low (e.g., see Bundesamt für Statistik, 1998). It is important to note, however, that the measure of co-workers' education still takes into account the education of all employees.

Within this restricted sample equation (3) of *Model 3a* is reestimated. The results shown in the first column of Table 6 confirm that education spill-over effects within occupational groups arise also for this homogenous group of lowest skilled workers. The estimated spill-over effect (0.024 log points or around 2% per year of co-workers education) is, however, only half the size of the effect found for the overall sample (0.041 log points or around 4%). This may either indicate that a part of the effect for the whole sample reflects unmeasured differences in human capital or that the external effects are a function of the skill level. This could be the case if low-skilled workers get less training by co-workers, or their co-workers care less about the organisation of their work, or the lowest skilled are less capable of learning from their co-workers and their organisation of the work.

Correcting the wage impact of the characteristic 'no-coworkers in the occupational group' (0.294 log points) for the impact of co-workers' average education years within occupational groups (0.249=10.4×0.024), a premia of 0.0444 log points of the average wage of employees of the mandatory schooling level results. The "solitary worker premia" thus almost vanishes when only the subsample of the lowest educated is looked at. This confirms our earlier expectations that the wage premia of this subgroup estimated in the total sample is

Table 6 Education spill-over effects within occupational groups—employees with mandatory schooling only

Estimation model: 3a Dependent variable: log hourly wage	Total sample	Tenure up to 2 years	Tenure higher than 2 years
Co-workers' years of education in occupational group, average	0.024** (0.001)	0.003 (0.005)	0.026** (0.001)
No. co-workers in occupational group, dummy	0.294** (0.014)	0.103 (0.057)	0.308** (0.015)
Number of observations (employees)	68,360	7,614	60,746
Number of observations (firms)	3,146	1,303	2,967
R-squared (within)	0.290	0.165	0.297

Coefficients denoted by ** resp. * are significant at 1 resp. 5 % level. The results from within firm wage estimation are shown, see Sect. 3 for a detailed description

All estimation models include furthermore as individual variables: age, age squared, tenure, tenure squared, dummies for service or unclassified occupation, foreigner, women, part-time workers, additional dummies for individuals with missing information and for individuals with missing information for their own or co-workers' education or their activity besides the firm fixed effects. The wage structure of men and women is supposed identical

Source: LSE(1996)

not determined by the lowest educated employees but rather by the "professional experts" in some occupational groups at higher educational levels. The explanatory power of this specification, measured by the relevant *R*-square, is around 30%, which we consider satisfactory. The assumption of the equality of employer-specific fixed effects is also rejected for this restricted sample.

Impact of tenure In a second step, we look at the interaction of these eduction spill-overs with tenure within the same homogenous group of lowest skilled workers. The idea is that if these effects were to be explained by a systematic sorting process within firms by unmeasured individual characteristics, the tenure of the workers at a firm should help capture them. The underlying assumption is that a reasonable period of time (1–2 years at most) since the beginning of a job spell should be sufficient for an employer to detect the true productivity of an employee. On the other hand, if educational spill-over effects are found to arise later during a job spell with an employer, and furthermore if they are positively correlated with tenure, then the evidence speaks against unobserved individual heterogeneity affecting the results.

If external effects of education indeed seem to rise over time, as measured by their correlation with tenure, this speaks in favour of the explanation that learning from co-workers or the work organisation is enhanced by the high educational level of co-workers. But we still can not totally exclude other competing explanations. For instance, such a result may also reflect a rather slow matching process of employees, co-workers and jobs, where the quality of the matches are determined by unknown and random job and personal (individual's and co-workers') characteristics. In this process the wage gains from this matching process can be expected to increase with the complexity of the work approximated by the human capital intensity of the occupational group. In any case, nevertheless, these estimations give a first hint at the dynamics behind these effects.

Thus, in a next step, we split the sample of employees with mandatory schooling according to their length of tenure. The empirical results are shown in Table 8. Most strikingly, no education spill-over effects are detected in the sub-sample of employees with less than two years of tenure (second column). Contrary to that, significant effects are found for employees having more tenure than two years. Additional tests, not shown in Table 6 but available from the author upon request, showed that this effect is not simply caused by the larger sample size of the group of employees with tenure higher than two years. Restricting the sample to individuals with tenure up to only six years shows some evidence for external effects of education, but which are only one third of the figure estimated for individuals with medium or high levels of tenure (more than six years). Hence, education spill-over effects within occupational group clearly appear to be related to tenure. This finding speaks against the hypothesis of a systematic screening of more able individuals into such high skilled occupational groups from the beginning of a job spell as standard competitive labour market theory would suggest. An employment spell of minimum two years seems to be too long for simply detecting the true ability of a worker with low educational attainment level in the case of information problems. These results, furthermore, are consistent with earlier tests for external effects of education at employer level, modelling explicitly the impact of unobserved individual heterogeneity, as shown by Barth (2000).[17]

The finding that education spill-over effects arise predominantly in a sample of the stable workforce of the firm is, however, compatible with a rather slow matching process, which is more rewarding for an employee, the higher the educational level of his/her co-workers. A more precise determination of this matching or transmission mechanism of external effects of education is a topic for further research.

Impact of additional occupational controls The previous estimations showed that the estimate of education spill-over effects for the lower educated are substantially lower (2%) than the estimate for the total sample (4%). So far it is unclear whether this difference is due to the fact that these external effects of education are indeed higher for employees with higher education or whether other unmeasured factors account for it. In order to test this, we add in a last step more detailed controls (23 dummies) for the occupational groups in *Model 3*. These dummies capture wage differentials by occupational groups over and above the effect linked to the educational level of the co-workers within this group. More precisely, these controls will cancel some differences in individual's education (e.g., concerning the professional orientation) and in unmeasured characteristics like personal preferences for a given occupation. Similarly, they also control for the impact of a possible labour market segmentation along occupational lines.

In the first two column of Table 7, we see that adding these controls lowers the estimate of education spill-overs within occupational groups from 0.041 to 0.016 log points. This points to a significant impact on the result of unmeasured factors correlated with occupations. Most surprisingly, the education spill-overs measured at the firm level increase at the same time from 0.020 to 0.060 log points. This result needs further investigation, but if confirmed it would indicate that our previous estimates tend to be at the lower bound. The estimate for corrected private returns to education is still around 0.050 log points or 5%, surprisingly little affected by the additional occupational controls.

5.2.3 Imperfect substitution between high and low educated workers

In a last step, we try to clarify whether imperfect substitution between high and low skilled workers as suggested by a conventional demand and supply model explains the correlation of co-workers education and an individual's wage instead of external effects of education. We do this by estimating education spill-over effects within occupational groups separately by the three main

[17] However, our results seem to contradict the conclusions of Destré et al. (2001) which found evidence for the largest share of such learning effects on wages arise shortly after being hired. These substantial differences in the modelling of the external effects of education, albeit, do not allow a precise comparison of the results.

Table 7 Education spill-over effects within occupational groups—additional controls for sorting

Estimation model: Dependent variable: log hourly wage	3a W/B	
Number of occupational controls	2	23
Employee's years of education, both gender	0.052** (0.000)	0.050** (0.000)
Co-workers' years of education in occupational group, average	0.041** (0.000)	0.016** (0.000)
No co-workers in occupational group, dummy	0.604** (0.006)	0.233** (0.007)
Average years of education in firm, both gender	0.020** (0.005)	0.060** (0.005)
Number of observations (employees)	408,008	408,008
Number of observations (firms)	6,753	6,753
R-squared (within)	0.482	0.520
R-squared (between)	0.550	0.622
F-test for firm fixed-effects	30.7	26.2

Coefficients denoted by ** are significant at 1% level. W/B indicates within and between estimation results from two-step estimation described in Sect. 3

The estimation model includes furthermore as individual variables: age, age squared, tenure, tenure squared, foreigner, women, part-time workers, additional dummies for individuals with missing information and for individuals with missing information for their own or co-workers' education or their activity and firm level controls for seven regions, seven industries and firm size. The wage structure of men and women is supposed identical

Source: LSE(1996)

educational levels for both the original and the enlarged specification (2 or 23 occupational controls). These main educational levels are mandatory schooling, vocational education of the secondary level, and university education. For these restricted samples, only the within-firm estimation is done, as this is the main focus of our study.

The results, shown in Table 8 present a very interesting picture. Clearly, the impact of co-workers' education within occupational groups is at least as high for secondary and tertiary education as for mandatory schooling. The impact is higher when the heterogeneity in earnings by occupational group is not taken out with the more detailed set of dummies. These results suggest that, as a lower bound, education spill-over effects within occupational groups amount to 0.017–0.025 log points or around 2% for all levels of education. If there are effects due to the imperfect substitution of high and low skilled workers, these are small and largely offset by these spill-over effects. In other words, this result is compatible with the idea that all employees become more productive when working with higher educated individuals through learning processes between them. The evidence does not confirm the idea that this effect simply reflects the fact that the less educated receive a higher wage when working for more educated and thus more productive individuals in the same occupational group than when they work for less educated co-workers. This result stands

Table 8 Education spill-over effects within occupational groups—by educational level

Estimation model: 3a Dependent variable: log hourly wage	Mandatory schooling		Vocational education degree		University degree	
Number of occupational controls	2	23	2	23	2	23
Co-workers' years of education in occupational group, average	0.024** (0.001)	0.017** (0.001)	0.054** (0.001)	0.025** (0.001)	0.050** (0.002)	0.023** (0.002)
No. co-workers in occupational group, dummy	0.294** (0.014)	0.204** (0.016)	0.742** (0.009)	0.335** (0.011)	0.885** (0.004)	0.339** (0.038)
Number of observations (employees)	68,360	68,360	173,953	173,953	16,509	16,509
Number of observations (firms)	3,146	3,146	5,946	5,946	1,444	1,444
R-squared (within)	0.290	0.357	0.437	0.478	0.452	0.525
R-squared (between)	–	–	–	–	–	–
F-test for firm fixed-effects	20.1	18.5	13.8	11.8	9.9	10.7

Coefficients denoted by ** are significant at 1% level. The results from within firm wage estimation are shown, see Sect. 3 for a detailed description

The estimation model includes furthermore as individual variables: age, age squared, tenure, tenure squared, dummies for service or unclassified occupation, foreigner, women, part-time workers, additional dummies for individuals with missing information and for individuals with missing information for their co-workers' education or their activity and firm level controls for seven regions, seven industries and firm size. The wage structure of men and women is supposed identical

Source: LSE(1996)

in contradiction to the results of Moretti (2004a) looking at data at city level. However, the results are also quite consistent with the idea that the higher educated benefit in fact more from education spill-overs due to their higher learning capacities. However, a precise determination of the respective size of the spill-over and substitution effects has to be left to further research.

Again the coefficient of the dummy for no-coworkers in the occupational group is strongly reduced after introducing additional occupational controls, from 0.604 to 0.233 log points in the total sample. This impact increases even more with the educational level, confirming thereby our initial assumptions that the selection process in such expert positions is not random and in particular correlated with occupations related to the management of the firm.

Hence, we find clear evidence for education spill-over effects within occupational groups. A lower bound for our estimate of these effects is around 2%, being remarkably similar to the estimate of education spill-over effects at firm level (2%). Private returns to education corrected for the impact of such effects are at around 5% substantially lower then the uncorrected returns at around 7%. This confirms our *a priori* expectation that conventionally calculated private returns are biased upwards.

6 Conclusions

In this study, we have attempted to identify the effects of *education spill-over effects* from an individual's co-workers on the conventional estimates of his/her *private returns to education*. Our results show clear evidence for such spill-over effects within occupational groups (around 2%) over and above the general firm level effects of similar size. These results have been tested to be robust to extensive controls for the quality of workers and their education. Further research based on panel data is needed to test whether these results based on cross-sectional data really show a true causal relationship. Nevertheless, the empirical evidence so far strongly hints at external effects of education arising within the stable workforce of a firm. Employees frequently changing their employer, on the contrary, seem not to be able to reap the benefits from their co-workers' education on their own wages.

Moreover, the results clearly speak against alternative explanations based on imperfect substitution between high and low educated workers yielding a similar impact on the wages of the low skilled. Contrary to this explanation, education spill-over effects were found to be at least as high for workers with secondary and tertiary education than for workers with mandatory schooling only.

Due to these education spill-over effects and the fact that more educated people segregate themselves to employers and into occupational groups with a higher average education, conventionally calculated private returns to education are shown to be biased upwards. We find a lower bound of purely private returns to education of 5% as compared to 7% calculated by conventional methods.

These results emphasise the need for microeconomic theory to take external effects of education into account. Moreover, they clearly strengthen the case for public subsidies for education, as such externalities risk leading to an under-production of human capital. More specifically, the results highlight the benefit of policies aiming at upgrading workers within the labour market.

Appendix

Table 9 Standard wage regression—detailed results

Estimation model: 1a	Weighted least squares		
Dependent variable: log hourly wage	Coef.	Std. Error	t-value
Years of education	0.071	(0.001)	111.53**
Foreign or missing education	0.765	(0.008)	95.80**
Age	0.044	(0.001)	62.63**
Age squared/100	–0.045	(0.001)	–51.93**
Tenure	0.008	(0.000)	21.20**
Tenure squared/100	–0.007	(0.001)	–5.87**
Manufacturing activity			
Service activity	0.105	(0.003)	39.61**
Other activity	0.065	(0.005)	11.95**
Activity information missing	0.098	(0.004)	24.26**
Swiss nationality			
Foreign nationality	–0.067	(0.002)	–29.08**

Table 9 continued

Estimation model: 1a	Weighted least squares		
Dependent variable: log hourly wage	Coef.	Std. Error	t-value
Working part-time	−0.059	(0.003)	−18.54**
Woman	−0.179	(0.003)	−70.58**
Lake of Geneva districts	−0.081	(0.004)	−20.73**
Bern, neighbouring districts	−0.094	(0.003)	−27.30**
Bale, neighbouring districts	−0.104	(0.003)	−32.64**
Zurich			
Eastern districts	−0.054	(0.003)	−17.98**
Central montain districts	−0.133	(0.003)	−39.11**
Southern district (Italian speaking)	−0.104	(0.006)	−17.68**
Horticulture, without agriculture	−0.064	(0.004)	−14.26**
Electricity, gas and water supply	0.028	(0.003)	−8.04**
Construction	−0.003	(0.004)	−0.79
Manufacturing			
Trade, repair, hotels and restaurants	−0.108	(0.003)	−32.90**
Transport, communication	−0.049	(0.008)	−6.55**
Financial intermediation	0.041	(0.005)	8.96**
Private households, extraterrit. org.	0.037	(0.004)	8.33**
Log establishment size	0.013	(0.001)	24.54**
Establishment size missing	0.013	(0.004)	3.00**
Constant	1.560	(0.015)	103.34**
Number of observations (employees)	408,008		
R-squared (standard)	0.526		

Source: LSE(1996)

Table 10 Education years by educational level

Level	Classification labels		Years assigned	
	German	English	Men	Women
1	Nicht schweizerische Ausbildungsgaenge	Foreign education	–	–
2	Obligatorische Schulbildung	Mandatory schooling	9.0	9.0
3	Unternehmensinterne Berufsausbildung	Vocational education, no recognised degree	10.5	10.5
4	Abgeschlossene Berufsausbildung	Vocational education, recognised degree	12.5	12.0
5	Matura, Berufsmaturitaet	High school degree	13.0	13.0
6	Lehrerpatent	Teachers' education	14.0	13.5
7	Hoehere Berufsausbildung	Higher professional schooling	14.5	14.0
8	Hoehere Fachschule/ Fachhochschule	Vocational tertiary education	15.5	15.0
9	Universitaets-, Hochschulausbildung	University	18.0	18.0

Source: Weber (1998)

Table 11 Aggregate classification

Level	Classification label (aggregate)	Levels of original classification
1	Foreign education, MEI	1 or missing information on education
2	Mandatory schooling	2, 3
3	Vocational education degree	4
4	Upper secondary	5, 6, 7
5	University	8, 9

Source: Weber (1998)

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