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Training, Wages and Employment Security: An Empirical Analysis on European Data

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Training, Wages and Employment Security:

An Empirical Analysis on European Data[§]

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Mars 04, 2005

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Abstract :

We use data from the European Community Household Panel (ECHP) to assess the effects of employee training on the individual labour market performance of different labour market groups in EU countries. We find significant training wage premia only in the case of young or highly educated employees. By contrast training appears to have a strong impact on employment security in the case of both older and low-educated workers. To reconcile this apparent contradiction, we need to take into account that, as standard in the literature, wage premia are estimated on a truncated sample including only employed workers. Due to downward wage rigidity, those workers who are unable to maintain their productivity are more frequently laid–off — rather than experiencing a wage fall and be retained in employment — and thereby excluded from our sample.

Résumé :

Nous utilisons des données du Panel Communautaire des Ménages (ECHP) pour évaluer les effets de la formation continue sur la performance des salariés sur le marché du travail. Nous trouvons des primes salariales à la formation significatives seulement dans le cas d'individus jeunes ou avec une qualification supérieure. En revanche, la formation parait avoir un impact important sur la sécurité de l'emploi dans le cas des travailleurs âgés et peu instruits. Ce contraste est seulement apparent : comme il est courant dans la littérature, les primes salariales sont estimées sur un échantillon tronqué composé seulement des travailleurs qui ont un emploi. Toutefois, du fait de la rigidité à la baisse des salaires, les travailleurs qui ne sont pas en mesure de conserver la même productivité se retrouvent plus souvent sans emploi et donc exclus de notre échantillon.

Keywords: Training wage premia, Job Security, Wage Compression, ECHP.
JEL Classification: J24, J28, J31

Introduction

Viewed from the point of view of employment policy, the rationale behind stressing the importance of lifelong upgrading of skills and competences is threefold. First, technological and structural changes render jobs and skills obsolete at such a rate that the slow renewal of the labour force through the entry of young qualified workers might not suffice to satisfy the demand for new qualifications, thereby increasing the risk of skill shortages that, in a global economy, may depress employment (OECD, 1994, chapter 7). Second, people with low qualifications face higher unemployment prospects or, in countries where they can price themselves into jobs, a higher risk of being persistently in low pay and often in poverty. Policies for initial education and adult training can, therefore, be seen as complementary to making-work-pay policies and job-search assistance as regards to "minimising the number of people who do not attain and maintain the skills required to command earnings that bring them above the poverty threshold" (OECD, 1999, p.12). Third, as skills become outdated more quickly than workers retire from the labour force, there is a strong risk of older workers losing their current jobs, while lacking the competencies to move into new jobs. Indeed, since increasing labour market participation of older workers has become a policy priority of many industrialised countries, "promoting access to training for all regardless of age and developing lifelong learning strategies, in particular workplace training for older workers" (European Commission, 2004, p. 46) has gained paramount attention.

It can be argued, however, that there is still little empirical evidence that can support the policymaker's emphasis on adult learning. For instance, the evidence on the impact of government funded training programmes for the unemployed is mixed,[1] the evidence on the impact of training for employed workers is essentially limited to its *average* effect on wages and productivity, while only few studies look at the relationship between employee training and employment security, and their results are somewhat inconclusive due to selection bias.[2] Furthermore, the fact that, while workers' participation in education and training is relatively high in certain countries, the number of hours of training received by each participant is much smaller than those received by full-time students enrolled in front-end education might cast doubts on how much a marginal improvement in training provision can affect labour market performance, in general, and, more specifically, individual and aggregate employment perspectives. Finally, deadweight and efficiency are seldom considered in the policy discourse.

This paper is a very partial attempt to contribute to bridge this gap. We use data from the European Community Household Panel to try to assess the effects of adult education and training on the individual

[1] See for example Martin and Grubb (2001) and Layard (2004).

[2] See Bishop (1997) and Ok and Tergeist (2003).

labour market performance of different labour market groups in EU countries. Although we find that employee training has a clear impact on wage growth only in the case of young or highly educated employees, it appears to have a stronger impact on employment security — at least insofar as it is perceived by the workers — in the case of both older and low-educated workers. To reconcile this apparent contradiction, we need to take into account that training wage premia are estimated on a censored sample including only employed workers. Due to the existence of downward wage rigidity, one can expect that those workers who are unable to maintain their productivity (due, for instance, to skill obsolescence) are more frequently laid–off — rather than experiencing a fall in wages and be retained in employment — and thereby excluded from our sample. In particular, it can be conjectured that, in the case of older workers, training enables employers to *match* individual productivity with constant individual wages and therefore *retain* the worker. Conversely, workers not receiving training are more likely to enter non–employment because their productivity has fallen below their wage. This argument can be generalised to all low-productivity workers and suggests that, for those people who find it more difficult to price themselves into jobs, training allows attaining and maintaining the competences required to match productivity and wages, thereby sustaining their employment prospects. Once foregone income due to unemployment spells is taken into account, it turns out that training positively affects earnings at any age and level of educational attainment.

The paper is organised as follows: Section 1 sets forth a simple empirical model for evaluating the effect of training on individual wages and subjective perceptions of employment security. Section 2 provides some details on the data, while Section 3 reports the estimation results. Few concluding remarks are contained in the final section.

1. Empirical framework

The general empirical model used in this paper can be considered an extension of that proposed by Loewenstein and Spletzer (1998). Denote with V_{ijt} the value for the worker *i* of a job match with the firm *j* at time *t*. In the simplest case this value can be seen as the stream of expected revenues that the worker *i* can obtain from being employed in firm *j* at time *t*. In a narrow sense we can think of this value as the current wage. However, more generally, this value may also include the worker's valuation of his/her employment security. Our objective is to estimate the effect on V_{ijt} of the *stock* of previously taken training courses.

Whatever the precise definition of V_{ijt}, which will depend on the specific empirical application, let us assume that it can be written as

$$V_{ijt} = \beta \mathbf{X}_{it} + \gamma T_{ijt}^{c} + \delta T_{ijt}^{p} + y_t + \mu_i + \nu_{ijt} + \varepsilon_{it} \qquad [1]$$

where $\mathbf{X}_{it}$ is a vector of time-varying individual characteristics, T_{ijt}^{c} is the stock of training taken while working for current, T_{ijt}^{p} the stock of training taken while working for previous employers, while y_t, μ_i, ν_{ijt} are year (or country per year) effects, individual fixed effects and job-match-specific effects (with ν_{ijt} taking value ν_{ij} if the worker *i* has a job with firm *j* at time *t* and 0 otherwise), respectively, and ε_{it} is a standard random disturbance.

Assuming that [1] is valid is equivalent to ruling out time–variant heterogeneity, which is not due to observable characteristics (such as the training stock), the job-match or a serially uncorrelated random disturbance. The inclusion of an individual fixed effect in the empirical specification allows identifying the coefficient of all stock variables (such as training) for which only changes within the sample period are observable (depreciation is ruled out for convenience). However, if in addition match-specific effects are included in the empirical specification, the impact of T_{ijt}^{p}, being invariant within each specific job-match, cannot be identified. If we were interested only in returns to training within the job match, then this would not be a problem. However, in the presence of employers' market power, within-job returns to training might be particularly low and not sufficiently informative. Furthermore, it is interesting to know whether skills are transferable across jobs.

Loewenstein and Spletzer (1998) show that if $\gamma < \delta$ then estimating [1] by omitting match specific effects (but including individual fixed effects) would yield an estimate $\hat{\delta} < \delta$, provided that dummies for the number of job changes are included in the specification. Equivalently, the same result can be obtained by estimating model [1] in first differences using OLS, omitting match–specific effects and including a dummies for job change. Conversely, to obtain unbiased estimates of γ, we instrument the stock of training taken with the current employer with the difference of this variable from its job–match–specific mean, since $\mathrm{corr}((T_{ijt}^{c} - \overline{T}_{ij}^{c}), \nu_{ij}) = 0$ by construction ($\overline{T}_{ij}^{c}$ denoting the job-match-specific mean of T_{ijt}^{c}).

2. The Data

We use longitudinal data from the 2003 release of the *European Community Household Panel* (ECHP). This survey provides a wealth of information on individual income and socio-economic characteristics for 15 EU countries and aims to be representative, both in cross-sections and longitudinally. Due to the common questionnaire, the information contained in the ECHP is, in principle, comparable

across countries, which is its main strength. Moreover, releases of the ECHP contain additional longitudinal data from other sources for certain countries — such as the German Socio-Economic Panel (SOEP) and the British Household Panel Survey (BHPS), whose questions are made comparable with those of the ECHP questionnaire.

The main question on vocational training (resp. formal education) in the ECHP is as follows "Have you at any time since January (year before the survey year) been in any vocational education or training (resp. formal education), including part-time and short-courses?". From this question, a dichotomous variable "participation in vocational training (resp. formal education)", which takes the value 1 if the individual responded "yes" and 0 if he/she responded "no", is constructed. Conditional on a positive answer, the individual is asked to report additional information on the last course only (including duration but, in the case of education courses, not including whether the course was paid for or provided by the employer). The distinction between formal education and vocational training is based on the categories used by national Labour Force Surveys.

In the year of the interview, the stock of vocational training and formal education is increased by 1 if the individual reported to have participated in one of them in the period covered by that interview. Each training stock is further decomposed in two aggregates: training taken with the current employer and training taken with previous employers. Due to the scattered nature of the information on course duration (with many missing values for many countries), start and end dates are not used for the analysis of this paper. This has two consequences. First, training reported in one interview is attributed to belong to the period between that interview and the previous one, although it might have been taken before the latter. This is equivalent to increasing the risk of false reporting, which, as shown by Frazis and Loewenstein (1999), is likely to bias returns towards zero. Second, training reported in one interview is considered to have been taken with the current employer at the time of the interview. If, at a given interview, the individual says he/she separated from the employer he/she was working for at the time of the previous interview, the training reported in previous interviews as training with the current employer is added to the stock of training taken with previous employers and the stock of training with the current employer is re-set to either 0 or 1 (depending on whether any training is reported in the current interview). Additional information on the data used is reported in the appendix.

3. Empirical Results

Training wage premia

There are various ways to estimate a training wage premium.[3] The simplest method, when longitudinal data are available, is to compare wage growth rates between two interviews for workers receiving/not receiving training between the same two interviews. This procedure already controls for time-invariant heterogeneity without resorting to sophisticated regression techniques.

As discussed above, however, workers employed by high-performing establishments (for example those belonging to more innovative firms) might receive more training and experience faster wage growth. We prefer, therefore, to decompose raw training premia straightaway into the premium to training taken with previous employers and the premium to training taken with the current employer — obtained by estimating the wage equation [1] in levels using individual fixed effects and correcting for match-specific heterogeneity[4] and for time-variant observable individual and firm characteristics (Table 1). Given that our interest is on labour market groups, we pool all countries together.[5]

The following results emerge from Table 1. Continuous education and training taken with previous employers have, in general, a positive impact on wages, while workers usually reap a lower premium while staying with the same employer. [6] The wage premium to participating in training in one previous year while already working for the current employer has an impact which is relatively homogeneous across groups (about 1%), with in most cases a lower premium to formal education than to vocational training. Although this finding is partially due to too few education spells in the sample, it might be also ascribed to the fact that adults enrol in general education to qualify for different jobs rather than to improve the competencies they can use within the same job or occupation. Finally, and most importantly, the impact of vocational training (excluding education) taken with previous employers is significant only in the case of relatively young and/or high educated workers.

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[3] The economic literature is crowded with empirical results on the issue. See Bishop (1997), Leuven (2003) and Ok and Tergeist (2003) for recent surveys.

[4] Match-specific effects on wage premia to training taken with current employers are eliminated by subtracting job-match-specific means from the stock of training taken with the current employer and using the latter as an instrument. A sensitivity analysis (not presented here) was undertaken by estimating wage equations with job-match fixed effects, and revealed that the two procedures give extremely close results as regards to training taken with the current employer.

[5] Estimating separate equations by country and labour market group would have implied very small samples in many cases.

[6] These findings are consistent with previous studies that typically find that the training premium increases in the aftermath of a job change (see Loewenstein and Spletzer, 1998, 1999, and Parent, 1999, for the United States, Fougère et al., 2001, for France, Blundell et al., 1999, and Booth and Bryan, 2002, for the United Kingdom, and Gerfin, 2003, for Switzerland). These papers tend to interpret this fact as evidence of employers' market power.

Table 1. **Estimates of training premia, by labour market group**

	Training taken with		Formal education taken with		Formal education or training taken with	
	Previous employers	Current employer	Previous employers	Current employer	Previous employers	Current employer
Total	1.19 *	1.11 *	5.28 *	0.91 *	2.65 *	1.22 *
	(0.3)	*(0.13)*	*(0.52)*	*(0.26)*	*(0.27)*	*(0.12)*
Gender						
Men	1.65 *	1.25 *	5.51 *	1.49 *	3.12 *	1.43 *
	(0.42)	*(0.18)*	*(0.74)*	*(0.39)*	*(0.37)*	*(0.17)*
Women	0.70	0.93 *	4.97 *	0.34	2.17 *	0.97 *
	(0.43)	*(0.18)*	*(0.73)*	*(0.35)*	*(0.39)*	*(0.17)*
Age						
25-34	2.13 *	1.55 *	6.21 *	1.41 *	4.40 *	1.65 *
	(0.54)	*(0.29)*	*(0.75)*	*(0.47)*	*(0.46)*	*(0.26)*
35-44	0.55	0.92 *	2.70 **	0.78 ***	0.83 ***	1.06 *
	(0.52)	*(0.22)*	*(1.05)*	*(0.45)*	*(0.47)*	*(0.21)*
45-54	0.56	0.71 *	1.47	0.17	0.81	0.72 *
	(0.78)	*(0.25)*	*(1.72)*	*(0.64)*	*(0.72)*	*(0.24)*
Educational attainment						
Less than upper secondary	1.09	1.29 *	2.58	0.64	1.39 ***	1.24 *
	(0.86)	*(0.34)*	*(2.04)*	*(0.9)*	*(0.79)*	*(0.32)*
Upper secondary	0.11	0.93 *	6.87 *	0.35	2.44 *	0.96 *
	(0.49)	*(0.2)*	*(0.81)*	*(0.43)*	*(0.44)*	*(0.19)*
More than upper secondary	1.43 *	0.95 *	3.03 *	0.95 *	1.97 *	1.10 *
	(0.47)	*(0.21)*	*(0.75)*	*(0.36)*	*(0.44)*	*(0.2)*

*, **, *** Statistically significant at 1% level, 5% level and 10% level, respectively. Standard deviation in parntheses.

Notes: Estimates of the wage premium (in percentage points) of participating in training in one additional year, obtained from the estimation of wage level equations controlling for individual fixed effects, age, age squared, tenure, tenure squared, firm size, public sector dummy, occupation, permanent contract dummy, log of hours worked, log of hours worked squared, the number of previous jobs, reason of last job change and interaction terms between country dummies, year dummies and date of interview. Wage premia to training and formal education are estimated through a specification that simultaneously includes both variables.

Should one conclude that education and training does not lead to a *durable* economic return for other categories, and particularly for those who have already lower earnings, greater employment insecurity as well as more imperfect access to training opportunities? This conclusion would be unwarranted. In fact, these returns are biased by the fact that the sample is censored: they are computed only for workers that are employed, excluding persons that are expelled from employment. Although estimating the effect of training on wages by restricting the sample to employed workers is standard in the literature, we can conjecture that, if selection into employment were taken into account, training would turn out having a positive impact on wages for all groups. To put it another way, these estimates do not take into account the impact of training on employment prospects and on containing the loss of income associated with unemployment spells. For this reason we turn now our attention to the effect of training on job security.

Training and the perception of employment security

In this paper, we focus on subjective perceptions of job security (measured on a 1-6 Likert scale).[7] Following the literature on job satisfaction, one could estimate a fixed effect linear model (Heywood et al., 2002) or a fixed effect logit model (Winkelmann and Winkelmann, 1998), by collapsing the measure of job security into a dichotomous variable. However, neither of these methods is ideal, since in the first case the qualitative (or at least double censored) nature of the data is not taken into account, while in the second case a great deal of information is thrown away. In this paper, we choose to follow the first route and estimate the model in first differences, using observations at relatively distant dates — two years. The advantage of estimating the model in first-difference is that we can perform a sensitivity analysis by checking that results are not due to heterogeneity of returns at different levels of initial employment security. Accordingly, Table 2 reports estimates by labour market groups, while Table 3 reports estimates by lagged levels of employment security.

Two clear facts seem to emerge from Table 2 and Table 3. First, vocational training taken with previous employers have a positive impact on the perception of job security of all categories of workers (with the exception of those with the highest educational attainment) and, essentially, all levels of employment security.[8] Given that these measures are partially forward-looking (that is, take into account the perceived risk of job loss), these results yield some support to the conjecture that returns to training might be positive even for those categories for which they do not show up in the wage level (conditional on being employed). Second, and more striking, training taken with previous employers has the greatest impact on perceived job security for those categories for which estimated wage premia are smaller. Conditional on changing job, for each year of previous training, employees without upper secondary qualification are estimated to increase their perceived job security by about 3%, and employees aged from 35 to 54 years by more than 2%, with no smaller effect when only vocational training (excluding education) is taken into account.[9]

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[7] There is an increasing interest in the economic and sociological literature for subjective measures of job security (See e.g. OECD, 1997, Schmidt, 1999, Green et al., 2000). Although these measures suffer from all problems typical of subjective data, they offer a synthesis of different aspects of employment security that is not possible with objective measures. Moreover, there seems to be a relatively good correspondence between subjective and objective measures of job insecurity.

[8] Table 2 reports also estimates for the impact of formal education only, which is insignificant. Beyond the usual caution due to the fact that few education spells are observed in the sample, it must be taken into account that the effect of education is likely to materialise only in the long-run. In the short-run, individuals who have got a better diploma often start new careers by accepting better paid temporary contracts.

[9] Care must be taken in interpreting these results, however, because the estimates might be biased due to the inclusion of the lagged level of perceived job security, which is endogenous. Nevertheless, a quick look at the data shows that perceived job security exhibits a clear pattern of mean reversal; therefore omitting the lagged level of

Table 2. **Estimates of the impact of training on security, by labour market group**

	Training taken with		Formal education taken with		Formal education or training taken with	
	Previous employers	Current employer	Previous employers	Current employer	Previous employers	Current employer
Total	0.071 *	0.080 *	-0.002	0.032 **	0.068 *	0.081 *
	(0.018)	*(0.007)*	*(0.032)*	*(0.014)*	*(0.016)*	*(0.006)*
Gender						
Men	0.077 *	0.074 *	0.007	0.046 **	0.070 *	0.077 *
	(0.023)	*(0.009)*	*(0.041)*	*(0.021)*	*(0.021)*	*(0.009)*
Women	0.056 **	0.084 *	-0.020	0.017	0.057 **	0.082 *
	(0.028)	*(0.010)*	*(0.049)*	*(0.020)*	*(0.026)*	*(0.010)*
Age						
25-34	0.035	0.089 *	-0.010	-0.005	0.040 *	0.078 *
	(0.026)	*(0.012)*	*(0.039)*	*(0.022)*	*(0.023)*	*(0.011)*
35-44	0.096 *	0.063 *	-0.034	0.055 **	0.081 *	0.071 *
	(0.029)	*(0.011)*	*(0.062)*	*(0.023)*	*(0.027)*	*(0.010)*
45-54	0.122 **	0.084 *	-0.107	0.077 **	0.093 **	0.088 *
	(0.051)	*(0.013)*	*(0.104)*	*(0.035)*	*(0.046)*	*(0.013)*
Educational attainment						
Less than upper secondary	0.130 **	0.110 *	0.082	0.030	0.131 **	0.106 *
	(0.062)	*(0.018)*	*(0.113)*	*(0.053)*	*(0.056)*	*(0.018)*
Upper secondary	0.044	0.070 *	-0.048	0.014	0.032	0.068 *
	(0.027)	*(0.01)*	*(0.053)*	*(0.024)*	*(0.025)*	*(0.010)*
More than upper secondary	0.004	0.031 *	-0.029	0.002	0.000	0.028 **
	(0.027)	*(0.011)*	*(0.043)*	*(0.020)*	*(0.026)*	*(0.011)*

*, **, *** Statistically significant at 1% level, 5% level and 10% level, respectively. Standard deviation in parenthesis.

Notes: Estimates of the impact on the average employee's perception of job security (measured on a 1-6 Likert scale) of participating in some training in one additional year. The dependent variable is the two-year change in perceived job security. Estimates are obtained by OLS, adjusting standard errors for heteroskedasticity of unkown form. Data refer to employees working more than 15 hours per week and aged 25-54 years. Controls are: two year differences of age, age squared, tenure, tenure squared, log wage, log of hours worked, dummies for public/private employment, the number of previous jobs, lagged level of perceived job security, voluntary or involuntary separations in the two-year reference period and country per year dummies. Separate estimates for training and formal education are obtained by including both variables in the same specification.

Table 3. **Estimates of the impact of training on security, by lagged level of job security**

	Training taken with		Formal education taken with		Formal education or training taken with	
	Previous employers	Current employer	Previous employers	Current employer	Previous employers	Current employer
Lagged level of job security						
Not satisfied	0.264 *	0.133 **	-0.158	-0.211 ***	0.189 **	0.068
	(0.141)	*(0.114)*	*(0.098)*	*(0.057)*	*(0.085)*	*(0.054)*
2	0.111	0.030	-0.227 ***	-0.110	0.016	0.008
	(0.128)	*(0.094)*	*(0.083)*	*(0.044)*	*(0.074)*	*(0.042)*
3	0.240 *	0.094 *	-0.037	-0.040	0.186 *	0.072 *
	(0.088)	*(0.056)*	*(0.056)*	*(0.027)*	*(0.050)*	*(0.026)*
4	0.069	0.062 *	0.121 ***	0.078 **	0.099 *	0.074 *
	(0.065)	*(0.033)*	*(0.043)*	*(0.017)*	*(0.037)*	*(0.016)*
5	0.054 ***	0.069 *	0.056	0.087 *	0.067 **	0.081 *
	(0.053)	*(0.024)*	*(0.028)*	*(0.011)*	*(0.027)*	*(0.011)*
Fully satisfied	0.112 *	0.081 *	-0.015	0.019	0.109 *	0.081 *
	(0.062)	*(0.021)*	*(0.032)*	*(0.010)*	*(0.030)*	*(0.010)*

*, **, *** Statistically significant at 1% level, 5% level and 10% level, respectively. Standard deviation in parenthesis.

Notes: Estimates of the impact on the average employee's perception of job security (measured on a 1-6 Likert scale) of participating in some training in one additional year. The dependent variable is the two-year change in perceived job security. Estimates are obtained by OLS, adjusting standard errors for heteroskedasticity of unkown form. Data refer to employees working more than 15 hours per week and aged 25-54 years. Controls are: two year differences of age, age squared, tenure, tenure squared, log wage, log of hours worked, public/private employment, the number of previous jobs, voluntary or involuntary separations in the two-year reference period and country per year dummies. Separate estimates for training and formal education are obtained by including both variables in the same specification.

job security would have probably induced an even greater bias. The application of instrumental variable techniques is made complex here by the lack of obvious instruments and is left to future research.

As conjectured above, the fact that training seems to have a stronger impact on employment security than on wages (conditional on being employed) in the case of older prime-age workers can be easily explained through the effect of skill obsolescence on individual wages and productivity: in the presence of downward wage rigidity, skill obsolescence compresses the wedge between productivity and wage, thereby increasing the risk of job loss without affecting the wage level conditional on keeping the job. In this case, training is required to maintain workers' competences so that their productivity will match their wage. If the wage structure is compressed, a similar argument can be generalised to all low productive workers (including, potentially, those with little or no qualifications). For instance, if the minimum wage is relatively high, a greater chance of being employed constitutes the main benefit from training for workers whose productivity would otherwise not match the minimum wage under all possible contingencies (Agell and Lommerud, 1997).

Overall, these results suggest that those workers, who do not seem to benefit from training through greater observed wages, can benefit from training by securing more stable employment prospects through lower job loss risk and/or greater chances to be re–employed quickly and in less precarious jobs. This is particularly the case for those categories (such as low–educated older workers) for whom their productivity–wage gap is more likely to be increasingly compressed – as they age – by companies' personnel policies and/or institutional arrangements (such as minimum wages). Once foregone income due to non–employment spells is taken into account, training premia for all groups are likely to be large.

Concluding remarks

James Heckman has argued that "in evaluating a human capital investment strategy, it is crucial to consider the entire policy portfolio of interventions together (training programmes, school-based policies, school reform, and early interventions) rather than focusing on one type of policy in isolation from the others. […] We cannot afford to postpone investing in children until they become adults, nor can we wait until they reach school age — a time when it may be too late to intervene. Learning is a dynamic process and is most effective when it begins at a young age and continues through to adulthood" (Heckman, 2000, p.50). This caveat has an irreplaceable importance for policy guidance. Nevertheless, as noted by Blundell (2000), Heckman's remarks do not imply that later interventions have no pay-off. There are indeed several reasons why systematic provision of front-end formal education and training preceding entry to the labour market is increasingly insufficient. This paper has provided evidence that training has a positive impact on individual labour market performance, yielding some support to the idea that it might be desirable to flank early interventions with policies for adult learning.

APPENDIX

The analysis of this paper is limited to individuals aged from 25 to 54 years. Due to data availability a person is defined as employed if he/she works at least 15 hours per week. Moreover, employee's gross hourly wages are computed from gross monthly earnings in the main job at the date of the interview, by dividing them by 52/12 and by usual weekly hours of work. Overtime pay and hours are included. The question on employment security in the ECHP is as follows: "How satisfied are you with your present job in term of job security?". Replies are quantified on a 1-6 Likert scale from not satisfied to fully satisfied. The median reply in the sample is 4, while the mode is 5. Finally, descriptive statistics of covariates are reported in Table A1.

Table A.1. Descriptive statistics of covariates

Panel A. Wage regressions sample

	Number of observations *	Mean/ Percentage	Standard deviation
Number of job changes	151646	0.11	0.31
Of which: % involuntary	9299	6.13	-
Age	151646	38.84	8.17
Job tenure (months)	151646	106.92	80.61
Hours worked	151646	39.02	8.86
Formal education	6289	4.15	-
Training course	27527	18.15	-
Employed in the public sector	49857	32.92	-
Permanent contract	133324	87.92	-
Women	64290	42.39	-
Educational attainment:			
Less than upper secondary	47167	31.54	-
Upper secondary	62735	41.95	-
More than upper secondary	39635	26.51	-
Firm size:			
None	809	0.53	-
1-4	14960	9.87	-
5-19	23818	15.71	-
20-49	18593	12.26	-
50-99	12693	8.37	-
100-499	17647	11.64	-
500 or more	13269	8.75	-
Occupations:			
Legislators, senior officials and managers	9169	6.05	-
Professionals	22537	14.86	-
Technicians and associate professionals	24704	16.29	-
Clercks	24731	16.31	-
Service workers and shop and market sales workers	16990	11.20	-
Skilled agricultural and fishery workers	1893	1.25	-
Craft and related trade workers	23769	15.67	-
Plant and machine operators and assemeblers	13772	9.08	-
Elementary occupations	14081	9.29	-
Country:			
Austria	9672	6.38	-
Belgium	8096	5.34	-
Denmark	9991	6.59	-
Finland	12081	7.97	-
Germany (SOEP)	21312	14.05	-
Ireland	8190	5.40	-
Italy	21213	13.99	-
Netherlands	19822	13.07	-
Portugal	16562	10.92	-
Spain	18161	11.98	-
United Kingdom (BHPS)	6546	4.32	-

* Number of 1s in the case of dichotomic variables.

Table A.1. **Descriptive statistics of covariates** *(Cont)*

Panel B. Job security regressions sample

	Number of observations *	Mean/ Percentage	Standard deviation
Two-year differences of age squared	76599	156.81	30.55
Two-year differences of log wage (x 100)	76599	9.87	0.27
Two-year differences of log of hours worked (x 100)	76599	-0.44	0.17
Two-year differences in job tenure (months)	76599	16.55	30.57
Number of job changes	76599	0.07	0.26
Formal education	2234	2.92	-
Training course	14887	19.43	-
At least one voluntary separation	4090	5.34	-
At least one involuntary separation	4120	5.38	-
Transition Public/Private sector:			
Public to private sector	2409	3.14	-
No change	72208	94.27	-
Private to public sector	1982	2.59	-
Two-year differences of the number of previous jobs:			
Same job	67594	88.24	-
One new job	7556	9.86	-
Two new jobs	1449	1.89	-
Women	31488	41.11	-
Educational attainment:			
Less than upper secondary	25369	33.33	-
Upper secondary	30310	39.82	-
More than upper secondary	20431	26.84	-
Country:			
Austria	4629	6.04	-
Belgium	5103	6.66	-
Denmark	7302	9.53	-
Finland	5059	6.60	-
Greece	2528	3.30	-
Ireland	4016	5.24	-
Italy	13887	18.13	-
Netherlands	12958	16.92	-
Portugal	11068	14.45	-
Spain	8404	10.97	-
United Kingdom	1645	2.15	-

* Number of 1s in the case of dichotomic variables.

The ECHP release used in this paper contains data from 1994 to 2000. Although, in principle, the ECHP covers 15 European Union countries, the country sample in the different analyses is chosen on the basis of data availability. Luxembourg and Sweden never appear in the analysis – due to the small sample size for the former and the absence of longitudinal data for the latter. SOEP and BHPS sources are preferred for Germany and the United Kingdom, respectively, since data from ECHP sources on these two countries are not available after 1996, and exact interview dates are not available in German ECHP data. Nevertheless, due to a change in the BHPS questionnaire, starting in 1998, only the waves 1998-2000 are used for the United Kingdom. Furthermore, data for Austria are not available in 1994 and data for Finland are not available in 1994 and 1995. In addition, observations for certain countries and certain years are excluded from the sample due to the lack of time-series comparability of wage data – notably, 1995 for Austria, 1994 and 1997-2000 for France, 1994-1996 for Greece, 2000 for Ireland, and 1994 for Spain. Finally, employment security data are not available for Germany.

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