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The financing of adult learning

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1 Introduction

Adult learning is high on the policy agenda of many governments, of the EU and of organisations like the OECD. It is easy to observe that high skilled people do better in the labour market than low skilled people, and that countries with more high skilled workers are more prosperous than countries with fewer high skilled workers. Since not everyone leaves the initial education phase with a high skill level and since acquired skills may depreciate (for technical or economic reasons), there may be a need for adult learning.

There is a widely spread belief that current participation levels in adult learning are below their optimal levels. While there are theoretical reasons for firms and workers to under-invest in learning (poaching, holdup), there are at the same time also various ways to circumvent these reasons. What the optimal investment level is, and whether current investment levels fall short of this, is hard to tell, and we have to rely on indirect evidence. One indication of underinvestment is a high return on investment. But this merely moves the problem, since it turns out to be rather difficult to estimate returns on adult learning. Other indications of underinvestment may come from policy experiments, which presumably relax binding restrictions. If these interventions increase learning participation substantially, this can be seen as evidence of underinvestment.

The remainder of this report consists of four parts. Section 2 interprets available information on participation in training and informal learning, and concludes that the current EU benchmark of participation in education and training of 15% in the last four weeks as measured in the ELFS is not very informative. Section 3 reviews the evidence on current public policy practices regarding adult learning. It concludes that with regard to tax instruments and sectoral training funds there is a clear lack of rigorous impact evaluation studies that can inform and guide policymakers. With regard to subsidies

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some rigorous impact evaluation studies exist and these show a surprisingly coherent picture. It turns out that training subsidies come with a very high deadweight loss, even when targeted at low skilled workers. This casts doubt on the often-expressed concern of underinvestment in adult learning. Section 4 elaborates on various training arrangements that may explain why there is no underinvestment. One insight from empirical analyses of these arrangements is that parties are less selfish than standard economic theory assumes. Many people have reciprocal attitudes. A large enough level of reciprocity amongst the workforce is enough to solve any underinvestment problem. Survey data suggest that this is indeed the case. Section 5 reviews the evidence on returns to training and finds that there are no extremely high returns. This is consistent with efficient investment levels.

2 Participation in adult learning in EU-countries

The belief that participation in adult learning in the EU is too low is based on the fact that participation in education and training as measured in the European Labour Force Survey is under 9%, whereas the official target is a participation rate of 15%. This section reflects on how training participation is measured in the ELFS and how sensible it is as a measure to evaluate the success of policies to raise adult learning.

The ultimate variable of interest is the level of knowledge and skills (human capital) of the work force. Training participation is just an input in the production of human capital. By analogy of formal education where we typically measure the highest level attended or completed, it would probably be preferable to measure the stock of training incorporated in the workforce (cf. Leuven, 2004). This requires an accounting scheme that keeps track of the amount of training invested in each worker (or at least by age cohort) and the depreciation of it. While this approach might be feasible for individual firms, training is typically measured in surveys as a flow during a specific period. But even when training is measured as a flow, important measurement issues remain. These are related to the heterogeneity of training. Training varies in length, intensity, level and the specificity of what is taught (is it only useful in the worker's current firm, the current industry, or everywhere?). Despite the multidimensionality of training, in many analyses training is just measured as a binary indicator which is equal to one if training took place in some reference period (4 weeks, 12 months, since the last interview), and is otherwise equal to zero.

Another measurement issue is that training is in most cases measured as participation in formal training courses or activities. This ignores informal learning activities, which in most cases can lead to the same increase in knowledge and skills as formal training.

The European benchmark of adult learning is that in the year 2020, 15% of adults aged 25-64 should participate in adult learning. Participation rates for this benchmark are measured by the ELFS, which asks about participation in taught learning activities (formal and non-formal education) in the 4 weeks prior to the survey. Two aspects of this operationalisation are worth discussing; the short reference period of 4 weeks and the restrictiveness of the types of learning activities included.

The length of the reference period has obviously a large impact on the measured training incidence. With a reference period of 4 weeks the ELFS occupies an extreme position. It is more common to employ a reference period of a longer period like 12 months (as in IALS or AES) or since the previous interview (as in the ECHP, which implies a reference period of 21 months). The advantage of a relatively short reference period is that recall bias will be modest. A disadvantage of a short reference period is that short training spells will be underrepresented. To see this, consider the following example. Assume that in a certain country 50 percent of the workforce participates in training each year. Assume that half of this 50 percent has a training spell of 6 months and the other half has a training spell of 4 weeks. If all training spells are spread out uniformly across the year, a reference period of one month will measure training incidence equal to 0.146, of which is 14% has a duration of one month. A reference period of 12 months will instead measure training incidence equal to 0.5, of which 50% has a duration of one month. This example shows that the training measure included in the ELFS underreports short training spells when at least some fraction of the training spells lasts more than 4 weeks.

Consistent with other operationalisations of training, the ELFS only covers formal and non-formal education and training but ignores informal learning. While informal learning is probably hard to measure, it might be an equally efficient learning mode as more formal forms of training. The interesting Commission staff working document "Progress towards the common European objectives in education and training" reports about informal learning methods (Chapter III.1.4). The reference period of learning activities in the AES is one year. The average share of people 25-64 years old that engage in informal learning activities amounts to 0.47, with a high 0.84 in Slovakia and a low 0.21 in Greece. The main sources of information are: family members, friends and colleagues (0.19), printed materials (0.35) and computers (0.27). While informal learning decreases with age, the profile is fairly flat; while 51% of the 25-34 year olds participate, this is 38% for the 55-64 year olds. Also, while informal learning increases with formal qualifications, still 28% of those with a lower secondary degree participate in informal learning.

The AES also report participation in formal and non-formal education and training amongst 25-64 year olds with a reference period of one year instead of 4 weeks. Results

are reported in Boateng (2009). The EU average equals 36%, with a high 73% in Sweden and a low 9% in Hungary. Gender differences are small. The age profile is steeper than with informal learning. Now participation among 25-34 year olds doubles that of 55-64 year olds (0.45 vs 0.22).

Comparing data on training participation from the ELFS with data on training participation from the AES and data on informal learning also from the AES, makes clear that the official benchmark used by the EU is rather conservative. The short reference period gives the impression that training participation is a rare event, while the reality is that during a year more than one out of every three working aged people in the EU participates in training. Moreover, by only focusing on formal and non-formal education and training, the official measure does not capture that during a year one of every two working aged people in the EU engages in informal learning. These omissions lead to an underestimation of the total investment in human capital by working aged people. It is likely that this underestimation increases over time. This is the case if the length of training spells reduces over time and when informal learning activities gain importance.

3 Current approaches

The main approaches of public policy strategy regarding training are (1) tax instruments, (2) sectoral training funds and (3) various forms of subsidies, including vouchers and learning accounts. The next three subsections discuss each in turn.

3.1 Tax instruments

Various countries subsidise training participation through tax instruments, including Bulgaria, Czech Republic, Germany, Estonia, Spain, France, Hungary, the Netherlands, Austria, Finland and the UK (Cedefop, 2009, p.23). This can be done either by allowing firms to deduct training expenditures from the tax bill, or to allow individuals (workers) to deduct their training expenditures from their income tax. As firms' training expenditures are part of their normal operation costs, firms will normally be allowed to deduct such costs from their tax bill.

The only two rigorous impact evaluations that I am aware of pertain to the tax system in the Netherlands. Leuven and Oosterbeek (2004) exploited the feature that the Dutch tax scheme allowed firms to deduct an extra amount in case the training expenditures pertained to the training of workers older than 40 years. This policy created a discontinuity in firms' training costs at the age of 40. For a worker (just) over 40 years old, training is 14 percent cheaper than for a worker (just) under 40 years old. While the

policy was implemented with the aim to stimulate training participation among older workers, the empirical results suggest that this did not happen. Training participation among workers just above 40 is substantially above training participation among workers just below 40. This difference is, however, not the result of increased training rates among older workers but results from decreased training rates among younger workers. Apparently, training participation by workers just below 40 was postponed.

Leuven and Oosterbeek (2007) use two different approaches to study the deductibility of direct training expenditures from taxable income. The main challenge is to isolate the effect of tax deductibility of direct training expenditures from the (implicit) tax deductibility of opportunity costs of training investment and from the taxation of returns to training investments. The first method exploits differences in deductibility rates around kinks in the tax schedule. By choosing the intervals around the kinks such that average net wage rates are equal, they get rid of the tax deductibility of opportunity costs. They also show that future marginal tax rates for individuals who are above and below kinks in a given year are very similar. This eliminates differences in taxation of returns to training. Results based on this approach indicate that a 10 percentage point increase in the tax deductibility rate of direct training expenditures increase training participation by 0.33 percentage points (10 percent increase in training rate).

Their second method takes advantage of the 2001 tax reform, which implied a substantial change in marginal tax rates. Investment costs in 2000 were subject to the old tax code, while investment costs in 2001 were subject to the new tax code. Because returns to training materialise with some delay, returns to investments made in 2000 and 2001 were both subject to the new tax code. Accordingly, this method isolates changes in taxation of costs from changes in taxation of returns. It does not, however, isolate tax deductibility of direct training expenditures from tax deductibility of opportunity costs. This method identifies the joint effect of these two deductibility rates, and since these operate in the same direction, it will overestimate the effect of interest. Results based on this approach indicate that a 10 percentage point increase in the tax deductibility rate of training costs increase training participation by 0.8 percentage points (a 25 percent increase in training rate). The authors show that the ratio of the results from the two methods are informative about the ratio of the opportunity costs of training investments and the direct expenditures of training investments, implying that opportunity costs are 1.5 times as large as direct expenditures.

There is reason to believe the true effect of tax deductibility of direct training expenditures is somewhere in between the estimates from the two methods. To the extent that the first approach does not fully neutralise differences in the taxation of returns, the estimates based on this method underestimate the true effect. Moreover, this method assumes that individuals are fully aware of the marginal tax rate applicable to

their training expenditures. If this assumption does not hold for some individuals with incomes close to a kink, these individuals will not act on their tax treatment and their responsiveness will thus be zero. This also biases the estimate from the local identification method downwards.

On the other hand, the estimate from the reform method is interpreted as the joint effect of tax deductibility of direct training expenditures and tax deductibility of opportunity costs. The underlying economic model assumes that an individual's opportunity costs of an hour spent on training changes abruptly if this person's taxable income passes a kink in the tax schedule. For people who work full-time (as most people with incomes at least just below the first kink will do) and have little scope to adapt their working hours marginally, this assumption implies that these persons experience an abrupt change in the valuation of their leisure. To the extent that one is unwilling to believe this, a larger share of the effect estimate from the reform approach is attributable to the tax deductibility of direct training expenditures.

The reported effect sizes are evaluated at an average marginal tax rate equal to 0.4. If it is assumed that effects are constant over tax rates the low estimate of 0.3 percentage points change in training participation per 10 percentage point change in deductibility rate, suggests that abolishing the tax deductibility of direct training expenditures reduces the share of individuals who spend money on training for career purposes by almost one half: from 3 percent to 1.7 percent. Using the high estimate of 0.8 percentage points change in training participation per 10 percentage point change in deductibility rate, even suggests that without tax deductibility of direct training expenditures no one would spend money on training for career purposes. In any case, tax deductibility of direct training expenditures appears to be an effective instrument to enhance human capital accumulation. At a marginal tax rate of 0.4, every Euro invested by the government in the form of a tax deduction, leads to 0.75 to 1.5 euro's of private expenditures on training investments.

3.2 Sectoral training funds

Sectoral training funds (STFs) are a common phenomenon in several EU countries. STFs typically work through levying a payroll tax from the companies in the sector. Companies that provide training to their workers can then claim expenses related to this training from the fund. Details regarding the payroll tax, the type of training and the type of costs that qualifies for reimbursement vary across STFs and across countries.

A detailed descriptive analysis together with the opinions of experts can be a valuable source of information. It should, however, be accompanied with a rigorous

empirical analysis before policy recommendations can be made. Without solid empirical analyses any claim about the advantages or disadvantages of STFs is mere speculation.

3.3 Subsidies, vouchers and ILAs

The literature offers some careful evaluations of interventions that aim to encourage participation in adult learning through subsidies, including vouchers and ILAs.

3.3.1 Subsidies Participation in adult training can be stimulated through direct subsidies which reduce the private costs of such training. By restricting eligibility the subsidies can be targeted to certain groups, such as low skilled workers. This is exactly what has been done in England when Employer Training Pilots (ETP's) were established. This program has been carefully evaluated by Abramovsky et al. (2011).

Abramovsky et al. (2011) show that the ETP had no statistically significant effect on the take-up of training among eligible employers and employees in the first 3 years of the program. This implies that the program was associated with very high levels of deadweight (i.e., training that would have been undertaken even in the absence of the ETP).[1]

In 2006, the German state of North Rhine-Westphalia (NRW) implemented a subsidy of voucher scheme that reduced training costs for establishments with up to 250 employees by 50% per course up to a maximum of 750 Euro. The program was co-financed by the European Social Fund. Both employees and firms could receive the voucher. Only individuals who did not participate in training in the year of application and in the previous year were eligible. This restriction did not apply to training courses subsidized by the program. The program only subsidized work-related training courses that were offered by accredited training institutions. Görlitz (2010) evaluated the impact of this program on:

(i) Training incidence at the establishment level (the share of establishments that finance employee training either by covering direct costs of training or by providing training during working hours);
(ii) Training intensity at the establishment level (relating the number of trained workers to the size of the establishment);
(iii) The skill structure of participants (relating the number of participants with no vocational degree to the overall number of participants).

To estimate the impact of the program on each of these outcome variables, Görlitz uses difference-in-differences designs. In the first design she exploits that the policy was implemented in NRW and not in other German states. In the second design she exploits

[1] See Falch and Oosterbeek (2011) for a more detailed discussion of this paper.

that only establishments with less than 250 employees are eligible, so that she can compare establishments with less than 250 employees with establishments with more (than 300) employees. In a third design, the author combines the two dimensions in a triple difference framework.

The findings of this study are robust across the different approaches and show that the share of establishments that participates in training increased by 4 to 6 percentage points due to the program. Relative to a base incidence level of 0.65, this is an increase of almost 10%. At the same time the results show no impact on the intensity of training conditional on participation. This means that the establishments that invest in training, the share of the employees that receive training (around 0.35) is not affected by the program. The share of low skilled participants (around 0.11) is also not affected by the program. All these estimates relate to short-term effects where establishments knew that the policy would be in place for 1.5 years. This biases the effects upwards if firms were trying to make maximum use of the policy.

Görlitz does not provide an estimate of the deadweight loss of the program. This is only possible when one is willing to make additional assumptions. If we assume that the take-up of the subsidy was the same among establishments that started to invest in training after the program was launched as among the establishments that already invested in training before the program started, the results imply that out of every 10 vouchers, 9 are used for workers that would also have received training in the absence of the program, while 1 voucher is used for a worker who would not have received training in the absence of the program. To get one additional low skilled worker into a training course, even requires 30 vouchers. All in all these results imply a high deadweight loss, in the vicinity of 90%.

3.3.2 Vouchers Under the so-called GI Bill, veterans of war are entitled to attend up to 45 months of education during a 10-year period after their active duty. They are entitled to receive an allowance if they attend an accredited schooling or training program. The allowance may be used either to meet the direct schooling costs or to cover costs of living. Veterans were thus effectively granted training vouchers in the spirit of the voucher scheme proposed by Levin (1983). Many newly established courses have been approved in relation to the GI Bill. Many of these courses were geared towards low-ability veterans, and these courses are believed to have a positive effect on earnings. This contradicts the belief that the voucher mechanism leads to the supply of inferior quality.

Bound and Turner (2002) and Turner and Bound (2003) have looked at the effects of the GI Bill on educational attainment of veterans. Using variation in service during World War II between cohorts, Bound and Turner (2002) estimate significantly positive

effects on years of college completed and on the probability of college completion. Turner and Bound (2003) show that this has been accompanied by a widening of the gap in educational outcomes between African-Americans and others.

3.3.3 Individual Learning Accounts Individual learning accounts (ILAs) encourage savings for education while providing vouchers to people interested in pursuing training. An ILA is a base amount of resources set aside for an individual to use for his or her learning. ILAs can be used to develop knowledge, skills and abilities that increase their human capital.

An ILA initiative ran in the Netherlands since 2001. It involves eight pilot projects, each serving up to 150 people. The project includes contributions from learners, employers, and the state. State contributions are budgeted at about $400 per learner; employers contribute about $130-$400 per learner. The pilots have been confined to particular training fields. Renkema (2006) conducted an in-depth study of the effect of ILAs on recipients' educational intentions. To this end, he focused on two sectors: elderly care and technical installation services. In the first sector he fails to find any effect at all, for the second sector he reports modest positive impacts on intentions; the experimental condition of respondents accounted for only 5 percent of the variation of educational intention, compared to 27 percent for age and 10 percent for prior participation.

Hidalgo et al. (2011) analyze the impact of training vouchers of 1000 Euros on the training participation and related outcomes of low-skilled workers in The Netherlands. To this end they exploit data from a randomized experiment that was conducted in four sectors with a majority of low-skilled workers.[2] Relative to a base training participation rate of 37 percent, receiving a voucher increases training participation by 5 percentage points after one year and up to 17 percentage points in the second year. Together with information about the number of vouchers redeemed, this implies a deadweight loss close to 60 percent. This means that more than half of the ILAs that were used would otherwise have been financed by private parties.[3]

Messer and Wolter (2009) and Schwerdt et al. (2012) analyze a program in Switzerland where ILAs were allocated randomly. Both papers use a sample of 2,437 individuals who had previously participated in the Swiss Labour Force Survey. This sample is more heterogeneous than the Dutch sample, including unemployed

[2] Doets and Huisman (2009) analyze the same experiment. These authors assign people who received the ILAs but in a follow-up survey report to be not aware of that, to the control group. This may bias the results since these unaware persons are probably also less motivated. Because of random assignment such less motivated people were already present in the control group, in which the share of these people is now doubled.

[3] More details of this study are provided in Falch and Oosterbeek (2011).

individuals, as well as workers with all levels of education. Different amounts of money (200 CHF, 750 CHF or 1500 CHF; equivalent to 124 Euros, 465 Euros and 930 Euros) to be used for a training course were randomly assigned to some individuals and others went to a control group. Messer and Wolter (2009) looked at the impact of these ILAs on training participation and at the deadweight loss it creates. They find that the 1500CHF-ILAs increased the participation in training by 6 percentage points (from a base of 34%). They find no impact of the smaller ILAs, after one year of the program. They report a deadweight loss of 60%, meaning that more than half of the redeemed ILAs used for training would have been paid by the individuals (or their employers) themselves. This share is remarkably close to the one found by Hidalgo et al. (2011).

In a follow-up paper, Schwerdt et al. (2012) added three additional outcomes: the impact on earnings, on employment status and on subsequent training after one year of the program. They analyze the impact of training on these outcome variables using the random assignment of the ILA as an instrument. It is debatable, however, whether the exclusion restriction is satisfied. This is not the case if ILAs affect the type of training courses people enrol in, and if the type of training has a separate effect on wages. Messer et al. also report the impact of the program directly on the same three outcomes and find no significant impact on earnings, employment status (employed or unemployed) or subsequent training.

3.3.4 Summarising The different studies that evaluate the impact of subsidies and ILAs on training participation reach a very similar conclusion: the deadweight loss is high. This implies that the costs of training an additional worker are very high. If we add to the deadweight loss of funding training that also without subsidy would have taken place, the excess burden of taxation, a conservative estimate is that one additional euro of training expenditures costs three euro's.

4 Private training arrangements

The dominant framework for the analysis of training is the model of Becker (1962), in which he introduces the distinction between general and specific training. Formalisations of this approach can be found in the work of Hashimoto (1981) and Hashimoto and Yu (1980). General training is defined as training that enhances the worker's productivity not only in the current firm, but to the same extent in all other firms as well. Specific training, on the other hand, is only useful in the current firm; it has no value in any other firm.

The important insight that can be gained from the distinction between general training and specific training is that an employee has to pay for all of the costs of general

training. The reason for this is that the employee will always be able to extract the entire return on the training. If the current employer is unwilling to pay for it, there will be another employer who is prepared to pay the worker the value of his marginal productivity. Hence, the current employer is unable to share the returns on the general training and for this reason will not be prepared to bear a part of the costs.

For specific training, the situation is different. The returns to a specific training course are conditional upon the worker and the firm staying together. If they separate, the investment in specific training becomes useless. For this reason, the two parties entertain strategic interactions with each other. According to Becker, the division of the gains and costs of specific training between worker and firm is determined by the probabilities of a quit and a lay off. If there is zero probability that the firm will dismiss the worker, the worker will receive the full return but also pay the full costs of the investment. If, at the other extreme, quitting is prohibited, then the firm will pay the entire burden of the training programme and also receive the full return. Leuven and Oosterbeek (2001) derive the expressions for intermediate (non extreme) cases.

In Becker's original formulation, the specificity of training is a technological characteristic; what matters is whether the human capital acquired during the training makes a worker more productive only in the current firm or potentially also in other firms. In an influential study, Acemoglu and Pischke (1999) have made clear that this is not the case when there are labour market imperfections due to for example minimum wage laws or the presence of unions. Under such conditions it can occur that training firms are still willing to pay for training that is also useful elsewhere. The reason is that due to the labour market imperfections, the worker can not claim the entire return on the training.

Underinvestment in firm-specific training can occur due to the so-called hold-up problem (Klein et al., 1978). After the costs are sunk, parties may appropriate a part of the other party's return by threatening to end the relationship. Parties that anticipate this opportunistic behaviour will underinvest in specific training.

There are various arrangements between private parties that can in principle solve the underinvestment in job-related training without any intervention from the government. Examples include: breach remedies (Edlin and Reichelstein, 1996), up-or-out contracts (Kahn and Huberman, 1988; Prendergast, 1993), unobservability of the investment (Gul, 2001) and sectoral training funds (Cedefop, 2008). In a series of papers, various of these remedies are tested in the setting of laboratory experiments (Oosterbeek et al., 2007a,b; Sloof et al., 2007a,b). The results of these experiments show that these contractual solutions can indeed alleviate underinvestment due to holdup. Before governments decide to intervene in the market of job-related training they might

want to examine whether any of these arrangements are in use in their country, and if not, why this is the case.

4.1 Contractual solutions

Promotion contracts According to Milgrom and Roberts (1992), promotions serve two roles in an organisation. First, they help assign people to the positions where they can best contribute to the organisation's performance and success. Second, promotions serve as incentives and rewards. These conceptually distinct roles give rise to two different types of promotion contracts: up-or-stay contracts and up-or-out contracts. Up-or-stay contracts never waste the skills of those not promoted but may provide insufficient incentives to invest in skills. Up-or-out contracts can always induce investment in skill acquisition but may waste the skills of those not promoted. Oosterbeek et al. (2007a) designed an experiment to study this trade-off, which they study in the laboratory. Under up-or-out contracts, parties behave almost exactly as theory predicts. Workers invest in skills but the workers that cannot be promoted are laid off. In contrast, under up-or-stay (and stay-or-stay) contracts, results differ markedly from theoretical predictions. While theory predicts that workers will not invest, it turns out that they do so rather frequently. This deviation can be explained by reciprocity (see also Subsection 4.2). Workers who invest in skill and nonetheless turn out to be of low productivity are often rewarded for their investment, which in turn makes it worthwhile for workers to have made the investment.

Bargaining Oosterbeek et al. (2007b) study experimentally whether employers or workers should invest in specific training. In their setup, workers have an alternative trading opportunity that takes the form of either an outside option or a threat point. Theory predicts that with outside options, employers have (weakly) better investment incentives than workers do and should therefore be the investing party. With threat points, employers and workers are predicted to invest the same. The results of this study are, by and large, in line with these predictions. Due to offsetting inefficiencies in the bargaining stage, however, realised inefficiencies are remarkably similar across the different situations considered.

Unobservability Standard theory predicts that holdup can be alleviated by making specific investments unobservable; private information creates an informational rent that boosts investment incentives. Empirical findings, however, indicate that holdup is attenuated by fairness and reciprocity motivations. Private information may interfere with this, as it becomes impossible to observe whether the investor behaved fair or not. In that way unobservability could crowd out an informal fairness/reciprocity mechanism.

Sloof et al. (2007a) report on a laboratory experiment to investigate this issue empirically. Their results are in line with standard predictions when there is limited scope for social preferences. But with sufficient scope for these motivational factors, unobservability does not boost specific investments.

Breach remedies Breach remedies may serve an important role in protecting specific investments. The following three breach remedies are commonly used in practice and received considerable attention in the theoretical literature:

1. Liquidated damages: the breacher has to pay a fixed amount—specified in the initial contract—to the victim of breach.

2. Expectation damages: the breacher has to pay the amount that makes the victim equally well off as under contract performance.

3. Reliance damages: the breacher compensates the victim such that the latter is equally well off as before the contract had been signed.

Theory predicts that expectation damages and reliance damages protect too well and induce overinvestment, either through complete insurance against potential separation (expectation damages and reliance damages) or the possibility that breach is prevented by increasing the damage payment due to the investment made (reliance damages only). Sloof et al. (2003) report on an experiment designed to address whether these two motives show up in practice. In line with theoretical predictions, they find that overinvestment does not occur under liquidated damages. In the case of expectation damages, the full-insurance motive indeed appears to be operative. In the case of reliance damages, both motives are at work, as predicted. This analysis shows that payback contracts effectively eliminate the underinvestment problem and can even lead to investment levels above the efficient levels.

Hoffman (2012) studies such training contracts in the U.S. trucking industry where they are widely used, focusing on data from one leading firm. Exploiting two plausibly exogenous contract changes that introduced penalties for quitting, he confirms that training contracts significantly reduce quitting. To analyse the optimal design of training contracts and their welfare consequences, he develops and estimates a structural learning model with heterogeneous beliefs that accounts for many key features of the data. The estimation combines weekly productivity data with weekly subjective productivity forecasts for each worker and reveals a pattern of persistent overconfidence whereby many workers believe they will achieve higher productivity than they actually attain. If workers are overconfident about their productivity at the firm relative to their outside option, they will be less likely to quit and more likely to sign training contracts.

Counterfactual analysis shows that workers' estimated overconfidence increases firm profits by over $7,000 per truck, but reduces worker welfare by 1.5%. Banning training contracts decreases profits by $4,600 per truck and decreases retention by 25%, but increases worker welfare by 4%. Despite the positive effect of training contracts on profits, training may not be profitable unless some workers are overconfident.

4.2 Reciprocity

The studies that examine the working of contractual solutions to holdup by means of laboratory experiments also point to the importance of reciprocity. The term reciprocity refers to parties' inclination to reward friendly actions and to retaliate unfriendly actions, even when this rewarding or retaliation is costly. In the context of training this can take the form of not taking full advantage of one's bargaining position if the other party made a specific investment. Leuven et al. (2005) confirm the importance of reciprocity for training decisions using survey data.

They propose a theoretical model that differs from the standard model in which parties are supposed to behave opportunistically and in which reciprocity is ignored. In their model a firm invests the socially optimal amounts in general and specific training if the worker is sufficiently motivated by reciprocity. A reciprocal worker may be willing to give the firm a full return on its investment. They bring their model to the data and present empirical evidence that strongly supports the proposed mechanism. Workers with a reciprocal attitude are much more likely to participate in work-related training than workers who lack this attitude.

Using responses to the question "If someone does something that is beneficial to you, would you be prepared to return a favour, even when this was not agreed upon in advance?" workers' reciprocal attitudes are measured on a three-point scale:

- 'Low' (13.4%)
- Intermediate (60.8%)
- High (25.8%).

The study measures training participation by response to the following question: 'Did you spend time following a course/training for purposes of your work or career opportunities during the past 12 months?' Of the 3127 respondents in the sample who held a job, 1393 (45%) gave an affirmative answer to this question.

Workers with a highly reciprocal attitude have a higher training rate than workers with a low reciprocal attitude. This is shown in the table below. This difference in training incidence between high and low reciprocal people is large, and hardly changes when control variables are taken into account. The training rate of reciprocal people is

50%, meaning that half of the reciprocal people participated in at least one training course in the past 12 months. Even among non-reciprocal people the training rate is around 35%, implying that one third of these people participated in a training course in the past 12 months. If current training participation is independent of past training participation, this means that a typical reciprocal (non-reciprocal) worker is trained every second (third) year.[4]

Table 1. Training participation rates by level of reciprocal attitude

	Training		
Level of reciprocity	All	Firm-sponsored	Without firm support
Low	0.356	0.325	0.075
Intermediate	0.441	0.404	0.100
High	0.502	0.475	0.105

Source: Leuven et al. (2005), p.144.

The results of Leuven et al. (2005) are important. The results confirm insights from laboratory experiments which point to the fact that many people are less selfish and opportunistic than standard economic theory assumes. The standard economic model predicts underinvestment in training because parties anticipate that others will behave opportunistically. But if many people (workers and firms) are less opportunistic than standard theory assumes, and if these people are aware of this, then underinvestment in training is less likely to occur. The results in Leuven et al. (2005) point in this direction. It would be worthwhile to replicate this study using other data (different period, different country). If replication studies confirm that reciprocal attitudes are indeed associated with higher training probabilities, the notion of underinvestment in training might be a misconception.

5 Returns to adult learning

The empirical analysis of returns to (formal) schooling has been a very active area of research. The question is interesting and important, and the problem is challenging because of deep endogeneity issues; people self-select into different schooling levels on the basis of characteristics that are not fully observed by the researcher. Moreover, schooling levels (or years) are measured with error.

[4] These figures pertain to the Netherlands, but training rates are not very different in this country than in other European countries.

Ideally one would like to use an experimental design in which people are randomly assigned to different schooling levels. This is clearly not feasible in practice and therefore researchers have looked for circumstances that somehow mimic this ideal design. Some very ingenious approaches have been used to estimate return to schooling. Examples include studies that use policy changes such as the gradual increase of the minimum school leaving age or regulations regarding the school starting age. Other researchers have relied on data from (identical) twins. The assumption is then that twins – who are otherwise identical – are randomly assigned to different schooling levels. The first wave of research in this area has focused in returns to schooling in terms of income. Later studies have also looked at wider benefits of schooling, such as health outcomes and crime.

The empirical literature on the return to training or adult learning is much less developed as the literature on the returns to (formal) schooling. There are good reasons for that. First, it is much harder to come up with sources of exogenous variation that mimic random assignment of training than with sources of exogenous variation that mimic random assignment of schooling. Such a source of exogenous variation should have a significant effect on the probability that a worker receives training (or that a firm organises training) and should at the same time have no direct impact on the relevant outcome (productivity or earnings). The literature has not been very successful in identifying such sources of variation. Second, as was already mentioned above, training is much more difficult to measure than years of schooling. This makes measurement error issues even more salient.

In principle we are interested in the impact of training investment on relevant outcomes for the parties involved; earnings for workers and productivity or profit for the firm. Since it is hard if not impossible to measure the productivity of individual workers, most of the literature has confined itself to attempting to estimate the impact of training on workers' earnings.[5]

A first approach to correct for endogeneity bias is to augment the wage equation with a Heckman-type selection correction term which results from a first-stage training participation equation (Lynch, 1992 and Veum, 1995 are early examples of this approach). The difficulty with this approach is twofold. First, the parametric selection models estimated in the literature are restrictive in the sense that they make an assumption about the distribution of the unobservables. Second, and more importantly, it is very hard to find variables which affect training participation and have arguably no direct effect on wages. The problem of finding such credible exclusion restrictions also

[5] I'm not aware of any convincing study that attempts to estimate wider benefits of training in terms of improved health and crime reduction. Given the focus of training courses on work-related issues such benefits seem less relevant.

hampers the application of an instrumental variable (IV) approach. In other words: it has been problematic to find variables that mimic the ideal experiment with random assignment of training participation.

Heckman's approach has been used recently by Budria and Telhado Pereira (2007) to estimate wage returns to training in Portugal. They use data from the Portuguese Labor Force Survey in 1998 and 2000. Training is measured as any training scheme during the worker's career. Only 5.2% of the sample claims to have participated in training. This is a surprisingly low number, especially given that individuals with the lowest education level are excluded from the sample. As excluded variables the authors use "having a second job" and "resided abroad". The authors assume that these variables affect training participation (which they do) but have otherwise no impact on wages. Or, to put it differently, that these variables are as good as randomly assigned to different workers. This is not very convincing.

OLS regressions return coefficients for training participation of 0.127 for men and 0.084 for women. The selection corrected treatment effects reported in this study are equal to 0.303 for men and 0.375 for women. According to these results, training participation boosts wages by more than 30 percent. Surprisingly, wage returns are at least twice as large for short training spells than for long training spells. This makes it hard to believe that this paper really identifies the return to an investment. (Why would anyone participate or offer a long training spell if a short spell has much larger returns?)

A second, more often used approach is to estimate the wage return to work-related training using fixed-effects regressions. This estimator, which is similar in spirit to taking first differences of the before-and-after training log wages, purges permanent individual effects from the estimating equation. Examples of studies that follow this approach include Barron et al. (1993), Booth (1993), Frazis and Loewenstein (2005), Greenhalgh and Stewart (1987), Lynch (1992), Parent (1999) and Veum (1995).

The fixed-effects estimator produces unbiased estimates when the unobserved individual effects are permanent. It is conceivable, however, that, apart from selection based on fixed individual observables and unobservables, selection into training also has dynamic aspects that provide an additional potential source of bias. Consider, for example, the case where individuals decide to take training because their earnings are temporarily low; faster earnings growth is then expected to occur among the trainees even in the absence of training participation. More generally, fixed effects estimations do not recover causal relationships if wage growth is different for trainees and non-trainees. It should be noted that fixed-effect estimates of wage returns to training are

typically smaller than standard OLS estimates, suggesting that fixed-effect estimates at least partially eliminate selection bias.[6]

Krueger and Rouse (1998) have access to detailed information from two firms in the US, one in the manufacturing sector and one in the service sector. They employ a fixed effects approach to examine the impact of a workplace education program in these companies on a broad range of outcome variables, including earnings, turnover, performance awards, job attendance, and subjective performance measures. The program is targeted towards low skilled workers and subsidised by the federal government. Classes were held on-site, typically met twice per week for 2 hours, and were taught in five 8–12-week sessions. Employees were paid their regular wages during class time. The authors find a small, positive impact of 0.5 percent of the program on earnings at the manufacturing company but an insignificant impact at the service company. Trainees were equally likely to exit the company as non-trainees. They also find that the training had a positive association with the incidence of job bids, upgrades, performance awards, and job attendance.

A recent study using the fixed effects approach to estimate the wage returns to training in Europe is Albert et al. (2010). The authors use different waves of the ECHP to estimate the wage return to various indicators of training participation in France, Germany, Italy, Portugal, Spain and the UK. They start with presenting estimates from OLS wage equations. With the exceptions of France and Germany, they find for all countries significantly positive coefficients for training participation. The size of the coefficients is in most cases in the vicinity of 10 percent and does not vary with the exact training measure. Using fixed effects changes the results dramatically. With the exception of some results for Italy, all coefficients are no longer significantly different from zero. This leads the authors to conclude that "participation in training (whatever the type) does not have any significant effect on wages" (p.329).

Estimates of the wage returns to private-sector training are typically quite high. As an illustration take the estimates from Frazis and Loewenstein (2005), who use the NLSY dataset and present a careful and thorough analysis of these data. They estimate various specifications, and their preferred estimate that takes into account heterogeneity in wage growth is a rate of return in the region of 40–50% for one full-time week of training. The estimated wage return to a training spell of a median length of 60 hours equals 2.5%. Using the EOPP data, the authors find a wage return of 5% for a median training spell of 38 hours. These findings are consistent with those of Barron et al.

[6] This is also what Haelermans and Borghans (2011) conclude in their meta-analysis. Yet, they report weighted averages of all studies, with and without correction for selection bias, as "the" wage return to on-the-job training.

(1993) and Loewenstein and Spletzer (1999).[7] Estimated returns are also high with data from other sources and countries. Bartel (1995) using company data, for example, finds that one day of training increases wages by 2%, which in her data is equivalent to a rate of return of 60%. Blundell et al. (1996) report returns to training incidence (zero-one dummy variable) for men in the UK in the region of 8% using OLS estimations, 9% for fixed-effects estimations, and 7% for IV estimations. The returns for women are even higher.

These results illustrate the fact that for a variety of datasets and countries the estimated returns to private-sector training are substantial. Moreover, the returns to private-sector training are very high compared to, for example, the returns to education. The return to a year of full-time education is around 10%, where in contrast the literature often finds returns of at least 3% for a week of private-sector training. This raises the question whether these estimates are indeed causal effects.[8]

Leuven and Oosterbeek (2008) follow a different approach to estimate the returns to private-sector training. They use OLS as a benchmark result that does not correct for selectivity on unobservables. They then compare these results with estimates based on our approach that takes the concept of random assignment literally. Their idea is to narrow down the comparison group to those non-participants who did not participate due to some random event. They achieve this by using the information obtained through two specially designed survey questions. The first is whether there was any training related to work or career that the respondent wanted to attend but did not do so. The second asks whether this non-participation was due to some random event such as family circumstances, excess demand for training places, transient illness, or sudden absence of a colleague. Leuven and Oosterbeek argue that respondents who give an affirmative answer to both questions are a more appropriate comparison group. If the random event acts as random assignment, this approach gives an estimate of the effect of treatment on the treated. An attractive feature of this approach is that it can be implemented using a single cross-section.

Using naive OLS Leuven and Oosterbeek find a return that is very similar in magnitude to what other studies have found, a wage increase of 9.5% for participating in one training course (with median duration of 40 hours) during the past 12 months. Restricting the comparison group to workers who wanted to participate in training but did not do so reduces the estimated return to 6.3%. When the comparison group is

[7] Lynch (1992) and, to some extent, Parent (1999) find lower returns using the NLSY data. According to the analysis in Frazis and Loewenstein (2005) this is due to the linear specification of training, which leads to a serious underestimation of the return to training.

[8] There are, of course, exceptions. Some studies find smaller returns, typically for continental European countries (e.g. Pischke, 2001; Goux and Maurin, 2000), although other studies find larger returns for the same countries (e.g. Fougére et al., 2001; Kuckulenz and Zwick, 2003).

further restricted to those workers who wanted to participate in training but did not do so due to some random event, the point estimate of the wage return to training equals 1%, and is statistically insignificant. The credibility of the proposed strategy is supported by the fact that narrowing down the comparison group to those who wanted to participate makes them more comparable to participants in terms of observed individual characteristics. Subsequently restricting the comparison group to those who did not participate due to some random event makes the comparison group also more similar in the characteristics of the (planned) training events.

The credibility of the approach is further supported by three replication studies, two using data from Germany and one using data from Switzerland. Görlitz (2011) uses data from the 2006 wave of a linked employer-employee data set called WeLL. This dataset covers over 6000 employees in 149 establishments. Fahr and Simons (2008) use the 2000 and 2003 waves of the "Berichtssystem Weiterbildung" which give snapshots of training behaviour of 19 to 64-year old Germans. Eymann (2012) uses data from the Swiss Labor Force Survey, which in 2003 offers a special module with regard to training. All three studies report results very similar to the results reported by Leuven and Oosterbeek (2008).

As we mentioned above, the interest in not only in the wage return to training but also, or perhaps even primarily in the impact of training on productivity or profit. A recent study of de Grip and Sauermann (2012) uses a randomised experiment to estimate the impact of work-related training on worker productivity. To identify the causal effects from training, they combine a field experiment that randomly assigns workers to treatment and control groups with panel data on individual worker performance before and after training. They find that participation in the training programme leads to a 10 percent increase in performance. This effect falls, however, quickly over time and after ten weeks the impact is no longer statistically significant. The major contribution of this study is that it shows the potential of identifying the impact of training on productivity in a clean experimental design. More studies of this sort are very much needed so that we get a complete picture of the effects of training.

Another strand of the literature uses large-scale data from across firms or industries. Firm productivity is then typically measured as value added or sales. Dearden et al. (2006) and Konings and Vanormelingen (2010) are recent examples using European data. These studies make use of panel data techniques and have to invoke much stronger assumptions than the previously discussed studies, to identify causal effects. Both studies find that the effects of training on firm productivity are about twice those on worker wages. This points to the fact that on average a substantial share of the training organised in firms is specific. Dearden et al. find that raising the proportion of workers in an industry who receive training by one percentage point increases value added per

worker in the industry by 0.6% and average wages by 0.3%. When controlling for various sources of worker heterogeneity, Konings and Vanormelingen find that the productivity premium for a trained employee is, on average, 17 percent.

The overall conclusion from this review of the literature is that the very large returns that the early studies on the impact of training have found are most likely attributable to unobserved differences between trained and untrained workers. Studies that use credible identification methods report rather modest estimates. This is also what one would expect at efficient investment levels.

6 Conclusion

In various documents the EU has expressed concerns that current levels of participation in lifelong learning activities fall short of the optimal levels. Accordingly it has formulated targets with regard to the desired participation levels of adults in learning activities.

The central message of this report is that there is no solid empirical basis for the EU's approach to lifelong learning. Briefly stated: The EU is convinced that current training levels are too low and has set target levels of training participation above the current participation levels. It also sees an active role for governments to achieve these higher training levels.

This report argues that there are no clear indications of underinvestment in training in the countries where solid evaluation studies have been conducted: (1) Policies aimed at stimulating training participation typically come with a very high deadweight loss, meaning that most of the subsidies go to training that would also have taken place otherwise; (2) Evidence suggests that firms and workers can and do engage in a variety of solutions to overcome the possible underinvestment problems due to holdup and poaching, and (3) Credible estimates of returns to training are typically rather low or equal to zero indicating that there is no underinvestment problem. Hence, on the basis of the available knowledge, there is no convincing case for government intervention. Of course, we do not know to what extent the available country evidence is representative for other EU countries with different settings; more research is called for on this.

Another message from this report is that the available knowledge base is rather limited. The EU has launched many studies (often through Cedefop) that lack a rigorous approach. While such studies may be a useful first step in the building of a knowledge base, it now seems time to make the next step. This step should consist of conducting a substantial number of rigorous impact evaluation studies to find out what works, and why (or why not), and in which circumstances. Textbook examples of the type of studies

that are needed are De Grip and Sauermann (2012), Messer and Wolter (2009) and Schwerdt et al (2012).

In that respect, much can be learned from the change that took place in development research. In the concluding chapter of their highly appraised book Poor Economics Banerjee and Duflo (2011) write "If we resist the kind of lazy, formulaic thinking that reduces every problem to the same set of general principles; if we listen to poor people themselves and force ourselves to understand the logic of their choices; if we accept the possibility of error and subject every idea, including the most apparently commonsensical ones, to rigorous empirical testing, then we will be able to construct a toolbox of effective policies but also to better understand why the poor live the way they do." (p.272)

Banerjee and Duflo describe many of their own studies in development economics that demonstrate that the clever use of randomised trials generates many new and useful insights about how to improve the position of the poor. There is little doubt that in the 10 years in which they conducted their research more has been learnt than during the many years that organisations like the World Bank evaluated interventions in developing countries using more traditional methods.

The revolution that took place in the analysis of the problems in developing countries is also needed in the analysis of lifelong learning strategies. Without a much broader basis of evidence obtained from rigorous empirical analyses government policies in the area of lifelong learning, while based on good intentions, have a high chance of being ineffective. it does not make sense to try to "develop a taxonomy / matrix of education and training at all levels and sectors set in relation to their returns and the potential beneficiaries (society, employer, individual), which could help policy makers to take informed decision on who should pay for/contribute to what kind of education and training".

The EU can play a leading role in the revolution needed to bring research regarding lifelong learning at a higher level: it can coordinate the formulation of a research agenda (probably in consultation with experts from EENEE) and it can invite some of Europe's excellent research groups in the field of empirical labour economics to submit research proposals in the field of lifelong learning strategies.

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