The Effects of Public Sector Sponsored Training on Unemployment Duration in West Germany[1]

- A Discrete Hazard Rate Model based on a Matched Sample -

by

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First version: July 1998
Revised version: December 1998

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[1] Financial support from the Deutsche Forschungsgemeinschaft is gratefully acknowledged. The authors want to thank especially Hilmar Schneider for the permission to use his GAUSS library for the estimation of discrete hazard models, and Joachim Grammig for helpful comments and criticism. We would also like to thank Michael Lechner and participants of the session "Unemployment and Wage Inequality" at the Annual Congress of the German Economic Association 1997 in Bern, Switzerland, Barbara Petrongolo and participants of the session "Labour Market Programmes" at the 54th Congress of the International Institute of Public Finance 1998 in Cordoba, Argentina, participants of the Anual Congress of the German Statistical Association 1998 in Lübeck as well as participants of a seminar at the University of Munich for helpful comments on earlier versions of this paper. Furthermore we would like to thank Adrian Hallett for research assistance.

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Abstract

This study analyses the effects of public sector sponsored vocational training (PSVT) on individuals’ unemployment duration in West Germany for the period from 1985 to 1993. The data is taken from the German Socio-Economic Panel (GSOEP). To resolve the intriguing sample selection problem, i.e. to find an adequate control group for the group of trainees, we employ matching methods. These matching methods use the individual propensity to participate in training, which is obtained by estimating a panel probit model as the main matching variable. On the basis of the matched sample a discrete time hazard rate model is utilized to assess the effects of training participation on unemployment duration. Our results indicate that a significant positive effect on reemployment chances due to PSVT can only be expected for courses with a duration of no longer than six months. No significant positive effects on post-training reemployment chances where found for courses lasting longer than six months. In fact these PSVT courses are significantly less effective at increasing reemployment chances than those lasting no longer than three months.

Keywords: discrete hazard models, selection bias, matching methods

JEL classification: C40, J20, J64

1. Introduction

Since 1969 publicly sponsored vocational training (PSVT) in West Germany has been regulated by the Work Support Act ("Arbeitsförderungsgesetz", AFG). From then onwards high and almost yearly increasing amounts of public resources have been invested into the support of vocational training by the Federal Labor Office ("Bundesanstalt für Arbeit", BA). After the re-unification PSVT formed an even more important part of active labor market policy in East Germany. For example, in 1996 more than DM 8 bn (DM 7 bn) were spent by the BA for the support of vocational training in the western (eastern) part of Germany.

These high costs combined with increasing public budget deficits emphasize the need to evaluate whether individuals who participate in PSVT actually profit from their participation. PSVT aims to influence different post-training labor market outcomes such as employment rates, earnings and duration of employment or unemployment spells. Since the effect of training on earnings is mainly a result of its effect on employment rates, the question raises whether employment rates change because trainees keep their jobs longer or because they find jobs faster when unemployed. This study concentrates especially on the question whether former trainees manage to find jobs faster after becoming unemployed. Hence, the outcome of interest is the individual duration of unemployment.

From a theoretical point of view there are different opinions regarding the effects of PSVT on unemployment duration. Advocates of PSVT argue that PSVT helps to reduce human capital decay and thus improves job search skills. On the other hand, advocates of a pessimistic perspective claim that participation in PSVT during episodes of high unemployment may be regarded as a negative screening device for some employers and, as a result, may even increase unemployment durations.

To evaluate the causal effect of vocational training on any kind of outcome one has to contrast the situation of the participants after training with the counterfactual situation in the absence of training. Because the latter situation is only hypothetical, i.e. not observable, it needs to be estimated, based on the outcome of other individuals who did not receive training, members of a so-called control group. In an experiment, the construction of an adequate control group is completed by means of randomization at the data collection level. In Germany only non-experimental data sets are available. Hence, reliable evaluations of training effects have to consider possible sample selection effects arising from non-random participation in training. Especially in the USA strong research efforts were made to develop different econometric and statistical adjustment procedures for the case of non-experimental data (e.g. Rosenbaum and Rubin, 1983, 1985; Heckman and Robb, 1985). A comparison of their performance with results obtained in experimental evaluations brought mixed results. While particularly model based adjustment procedures hardly produced reliable estimates, varying widely and differing greatly from experimental estimates (LaLonde, 1986), matching methods performed better and were reasonably successful in replicating experimental results (Dehejia and Wahba 1995a,b; Heckman et. al., 1997).

Although the number of evaluations on the effects of PVST for Germany has rapidly increased recently, most of them focus on the eastern part.[3] While these studies differ with respect to the outcome they measure and the methods they apply the following short overview will reveal that this variety is far less present for the western part of Germany.

The study of Hujer and Schneider (1990) is based on the first four waves of the German Socio-economic Panel (GSOEP) covering the period 1983 to 1986. The authors estimate a parametric hazard rate model of a Weibull type and find a significant positive short run effect of PSVT on the reemployment probabilities of unemployed men. As their model does not explicitly account for the potential selectivity of participation in PSVT the results, however, have to be considered carefully. A recent paper by Prey (1997) examines the effects of PSVT on employment probabilities using the first ten waves of the GSOEP. The author uses a multivariate random effects probit model that endogenously accounts for participation in vocational training. To identify the presence of any remaining selection effects a pre-program test proposed by Heckman and Hotz (1989) is applied. Unfortunately the pre-program test indicates that the model is only partly successful in accounting for present selection effects. In this respect, the results draw a rather negative picture of PSVT as no positive employment effects for women and even negative effects for men were found. Finally, Staat (1997), who also uses the first 10 waves of the GSOEP, separately examines the effects of PSVT on unemployment and employment duration. Using the estimated probability of participation in training as an instrument in the hazard rate model to account for possible sample selection effects the author finds no significant effects of PSVT on unemployment and employment duration.

This study aims to contribute to the ongoing discussion of PSVT in West Germany. It is concerned with the effects of PSVT on individuals' unemployment duration for the period 1985 to 1993. In contrast to the existing studies for West Germany on the effects of PSVT which are all model based approaches we follow a different strategy. To resolve the intriguing sample selection problem, i.e. to find an adequate control group, we employ matching methods proposed by Rosenbaum and Rubin (1985) and Rubin (1991) and adapted by Lechner (1996) for the evaluation of PSVT in East Germany.[4] The matching procedure uses the individual propensity to participate in training, obtained by estimating a panel probit model as the main matching variable. Additionally, monthly pre-training employment status is incorporated to account for transitory shocks just prior to training. Using the resulting matched sample we then employ a discrete time hazard rate model when evaluating the impact of PSVT on unemployment duration. This seems necessary, because a simple comparison of trainees' and matched controls' post-training mean unemployment duration would neglect problems such as right censoring or the fact that the unemployment spells of trainees and controls do not necessarily have the same starting points.

The paper proceeds as follows. The next chapter presents some developments of PSVT and unemployment duration in West Germany. Chapter 3 describes the data base and presents descriptive statistics for several characteristics of the chosen sample. Chapter 4 is devoted to the

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3 For recent studies on publicly sponsored vocational training for East Germany see e.g. Fitzenberger and Prey (1996, 1997), Hübler (1997), Kraus et al. (1997) and Lechner (1996).

4 The only study for West Germany that uses matching methods to overcome the problem of sample selection is one by Hujer et. al. 1997b. Their study, however, evaluates a more heterogeneous type of vocational training, which includes private and public sector sponsored training.

construction of an adequate control group. First we give a short theoretical outline of the evaluation problem and the approach we apply to deal with it. We then turn to the estimation of the propensity score for participation in PSVT. Based on the estimated propensity score the application of the matching procedure follows. Chapter 5 deals with the outcome of interest, i.e. the impact of PSVT on the transition from unemployment to employment. Based on the matched sample constructed in chapter 4, we use a discrete time hazard rate model to evaluate the impact of PSVT. Chapter 6 summarizes the main findings.

2. Public sector sponsored training and unemployment duration in West Germany: Some stylized facts

Figure 1 depicts the development of average unemployment duration in West Germany during the period 1980 to 1996. On the one hand it emphasizes the significant increase of unemployment duration after the recession in the early eighties. On the other hand, even in the period of exceptionally strong growth during the early nineties, unemployment duration remained at a relatively high level. Note that the average duration is calculated at a specific date and hence also includes ongoing spells. Thus, due to the well-known length bias this statistic will typically overestimate the true average duration.

< Figure1 about here >

The support of vocational training ("Förderung der beruflichen Weiterbildung") by the Federal Labor Office (BA) regulated in the work support act (AFG) is one central instrument of active labor market policy in Germany. Figure 1 also depicts the development of the proportion of BA's expenditures for vocational training in West Germany as a percentage of BA's total expenditures. This is clearly an indicator of the importance that is attached to vocational training by the BA. Similar to the evolution of the average unemployment duration, yet a little delayed, there is a notable upward rise from 1983 to 1987. It is followed by a rather substantial decline after 1992 to a level of 12.5% in 1996 which is just a little higher than the level in 1980. This decline mainly results from a reform of the AFG in 1993 which led to financial restrictions and institutional changes. From 1993 onwards, the BA no longer supported training courses with very short durations. Moreover as we will see below, the BA tightened admission rules and increased it's focus on target groups with particular bad labor market prospects (e.g. long term unemployed).

In principle the BA supports three different types of vocational training. The first type is further training ("Fortbildung", FT) in an occupation the participant is already trained in, the second type is retraining ("Umschulung", RT) for a new occupation and the third is training to familiarize with a new occupation ("Einarbeitung", TFO). In the case of TFO the BA's support is a wage subsidy passed on to employers for providing on-the-job-training for those employees who need a long time to familiarize with a new job. In contrast to TFO the first two types of training (FT and RT) are typically off-the-job (classroom) courses. When certain conditions related to the employment history, the motivation and the personal situation of the applicant as well as to the possible success of the training course -in terms avoiding future unemployment and improving future reemployment chances- are met the BA gives financial support to the participant (see §36, §42, §§44-47 AFG). This support can cover the costs of the provision of the course as well as a maintenance allowance ("Unterhaltsgeld", UHG) in the range of 60% to 75% of previous net earnings. Figure 2 gives some information about the official numbers of entrants into the three different types of vocational training from 1985 to 1996 for West Germany. Note that FT always accounts for by far the largest part (73%-82%) of all entries followed by RT (11%-24%) and TFO (2%-12%). As already indicated above, the dramatic decline of entries in vocational training in 1993 is mainly driven by the canceled support for short term FT courses (regulated by AFG §41a) that lasted typically between 2 weeks and 6 weeks.

< Figure 2 about here >

Figure 3 gives some information on the individuals entering FT or RT courses. The tightening of the admission rules and a stronger focus on target groups with particularly bad labor market prospects in 1993 results in a strong increase in the share of participants who where unemployed before FT or RT (55% in 1993 to 88% in 1994). In contrast the share of women who participated FT or RT increased constantly over time (34% in 1985 44% in 1996). Finally figure 3 also shows that the share of individuals who received UHG during FT or RT rises especially from 1993 (52.7%) to1994 (80.5%).

< Figure 3 about here >

To obtain a rather homogeneous training effect our empirical analysis will focus on individuals who participated in FT or RT and who received UHG during the training course. Moreover, in order to ensure that these individuals are subject to similar selection rules by the BA, we will only consider participants who began their training no later than early 1993, i.e. before the first major tightening rules which originated in early May 1993. Thus in the following sections the term publicly sponsored vocational (PSVT) will always refer to the training definition above.

3. The data base

The sample used for the analysis is drawn from the German Socio-Economic Panel (GSOEP), a representative sample of the resident population. Starting in 1984, about 12000 individuals aged above 16 and belonging to nearly 6000 households have been interviewed on a yearly basis about subjects such as employment status, personal characteristics, education, various types of income etc.. From 1990 onwards, an additional sub-sample of just under 2000 East German households and 4500 individuals was added. Since our study is limited to West Germany, the latter sub-sample is disregarded in our analysis. For a detailed description of the GSOEP see Hanefeld (1987), Projektgruppe Sozio-ökonomisches Panel (1995) or Wagner et al. (1993).

To generate the outcome variable of interest in this paper, namely the individual's unemployment duration, we rely on the retrospective monthly employment calendar that gathers detailed information about the individuals labor force status in each month of the previous calendar year. In this questionnaire the individual has to distinguish between up to eight different labor force states for each month. The states include full-time employed, part-time employed, vocational education, registered unemployed, etc.. Information on participation in PSVT comes from a retrospective monthly income calendar that gives detailed information about the income sources in each month of the previous calendar year. Analogues to employment calendar the individual can give information on up to eleven different income sources for each month. Participation in PSVT is identified by the income information provided about maintenance allowance (UHG) during further training (FT) or retraining (RT).

Due to data restrictions and institutional arrangements (see section 2) we will consider PSVT courses that began during the time span between 1985 and early 1993. In order to control for labor market history before participation in PSVT and to measure unemployment duration after PSVT participation, we use information on all waves from 1984 to 1994.[5] To avoid the need to address early retirement issues our selected sample consists of individuals who were not older than 50 in 1985. Since we focus on the outcome unemployment duration we only consider individuals who had at least one unemployment spell during the time span 1985-1993. For methodological reasons, left censored unemployment spells have been excluded from our analysis (see e.g. Hujer and Schneider, 1996). Unemployment spells are completed if they end through a transition into employment, where the term employment comprises full-time and part-time employment and vocational education (i.e. apprenticeship, further training or retraining) in the retrospective employment calendar. Otherwise, unemployment spells are treated as right censored.

This selection results in an unbalanced sample of 2013 individuals. There are 162 individuals who participated in at least one PSVT course that began between early 1985 and 1993 and for whom we observe valid information on the relevant covariates that are necessary to control for selection into training (trainees). The remaining 1851 individuals were not observed to participate in PSVT (non-trainees). All individuals observed contribute 3387 unemployment spells. 1090 of these spells are right censored. The average unemployment duration of all spells is 8.14 months, whereas the average duration of completed spells is 6.34 months. The 162 trainees took part in 198 PSVT courses. 76.5% of all trainees participated in only one course during the time span considered. The mean duration of all PSVT courses is 9.3 months and 78.2% of all PSVT courses are no longer than 12 months. Further detailed information on PSVT courses is given in Appendix A.

Table 1 gives information about the immediate pre- and post-training situation of trainees, focusing in particular on the two states employment and unemployment. It seems to be more likely that trainees exit PSVT in the same state as that from which the entered, i.e. 61% (55%) of those who where employed (unemployed) before PSVT remain employed (unemployed) after PSVT. Moreover focusing on the individuals who are in different states before and after training reveals that the share of those who switch from unemployment to employment (45%) is higher than from employment to unemployment (39%).

< Table 1 about here >

5 Information on employment status in 1983 (1993) comes form the retrospective calendar in 1984 (1994).

Figure 4 depicts the monthly share of PSVT participants that are unemployed for a time span of two

< Figure 4 about here >

years before and after the PSVT course. Clearly shown is that around 20% to 25% of future PSVT participants are typically unemployed up to approximately 12 months before the course begins. From then on the percentage unemployed increases up to more than 50% in the month just before training begins. In the month immediately after training we observe a share of unemployed PSVT participants that is about 10% lower than that of one month before training. In the following period the share continues to decline to about 15% in month 12 after training. Unfortunately this post-training decline may not simply be regarded as the result of a positive PSVT effect as we do not know how the post-training labor market situation of these PSVT participants would have been, had they not participated in PSVT. One can argue for example that the post-training decline may also be driven by other factors such as better demand conditions. Moreover it is not known whether possibly stronger search efforts without PSVT would not have lead to a similar post-training labor market situation as the one observed in figure 4. Obviously in order to identify a possible PSVT effect, information about individuals who did not participate in PSVT is required.

This brings us to the crucial evaluation problem when using non-experimental in contrast to experimental data. In non-experimental data sets we typically observe that non-trainees are not necessarily comparable to trainees. The descriptive comparison between non-trainees and trainees in table 2 reveals that this is also true for our sample. There are differences in characteristics such as nationality, education or employment status. When considering the year 1989 for example, trainees are on average younger, less likely foreigners, have a higher formal education level and a lower employment rate. Consequently, a simple comparison between mean unemployment duration (or rate) for trainees and non-trainees has to be considered carefully since it is subject to potential selection effects.

< Table 2 about here >

Furthermore if one looks at table 3 it becomes apparent, how sensitive the comparison of mean unemployment duration between trainees and non-trainees is with respect to the treatment of right censored spells. Looking only at completed spells would indicate an increasing effect while taking all spells into account would point to a very small reducing effect of PSVT on unemployment duration.

< Table 3 about here >

4. Constructing a control group using matching methods

4.1. The evaluation problem and identifying restrictions

When aiming to determine the impact of training programs on various kinds of outcome such as unemployment rates or unemployment duration, it is useful to define clearly what is actually meant by the impact of training. We try to do this by formulating the question of interest more precisely: *What is the average outcome gain to an individual who participates in training compared to his situation had he not participated?* Apparently, in order to answer this question one has to contrast the individual's situation after training with the corresponding situation of the *same* individual in the hypothetical case of not having participated in training, the so-called *counterfactual* situation. This points to the central problem of all training evaluations: They have to compare two situations which can never both occur. While one can observe the first situation, the latter is always hypothetical, i.e. unobservable. Hence, it has to be estimated based on the information of *other* individuals who did not participate in the training program, members of a so-called control group.

To formalize the evaluation problem, we base our analysis on the Roy-Rubin model (Roy, 1951; Rubin, 1974). In this model, there are two *potential* outcomes (Y_i^t, Y_i^c) for an individual *i*, where Y_i^t corresponds to the situation with training and Y_i^c without. The causal training effect for each individual is then defined as the difference between his/her potential outcomes (Y_i^t − Y_i^c).[6] The evaluation problem arises because we only observe either Y_i^t or Y_i^c, but never both, and hence cannot form the difference for any individual.

The parameter that receives the most attention in the evaluation literature and which is also considered in this study is the average training effect on the trained. It is defined as:

$$\alpha = E\left(Y^t - Y^c \middle| D=1\right) = E\left(Y^t \middle| D=1\right) - E\left(Y^c \middle| D=1\right), \tag{1}$$

where D is a dummy variable determining whether each individual participated in training ($D=1$) or not ($D=0$). The problem of not observing the counterfactual is now easily documented by the fact, that one only has information to estimate:

$$\alpha^e = E\left(Y^t \middle| D=1\right) - E\left(Y^c \middle| D=0\right). \tag{2}$$

It is obvious that α^e is a potentially biased estimator of the training impact of interest (α), since, $E\left(Y^c \middle| D=1\right)$, i.e. the unobservable average outcome of trainees, in the absence of training, does not necessarily equal $E\left(Y^c \middle| D=0\right)$, i.e. the observed average outcome of non-trainees. This inequality

[6] General equilibrium effects are ignored, so that the potential outcomes for a given individual are not affected by the training status of other individuals.

evidently arises if trainees and non-trainees have systematic differences in their individual characteristics as they do in our data set (see Section 3).

Carefully designed experiments are often viewed to be the only available procedure to overcome this evaluation problem and to obtain reliable estimates of the impact of training (see e.g. Ashenfelder and Card, 1985; LaLonde, 1986; Burtless and Orr, 1986). In an experiment, individuals who are eligible to participate in training are randomly assigned to a trainee group which participates in the training program, and a non-trainee group, that does not. Thus, the two groups do not systematically differ with respect to any of the relevant characteristics, except for having received training. As a result the difference in the outcome after training is only induced by the training program itself, i.e. the impact of training is isolated and there should be no selection bias. In formal terms the role of randomization is that the potential outcomes (Y^t and Y^c) are independent of the assignment to the training program (*D*). It follows:

$$E\left(Y^c \middle| D=1\right) = E\left(Y^c \middle| D=0\right). \tag{3}$$

Thus, the group of non-trainees can be used as an adequate control group to estimate the training impact α.[7]

As we are using non-experimental data we are forced to cope with the evaluation problem in a different way. We follow an approach introduced by Rubin (1977), which is, however, very much inspired by the conduction of an experiment. To construct an adequate control group even in a non-experimental setting, this approach is based on the identifying assumption that conditional on all relevant covariates (*Z*), the potential outcome without training (Y^c) is independent (denoted by $\perp\!\!\!\perp$) of the assignment to training (*D*):

$$Y^c \perp\!\!\!\perp D \mid Z = z. \tag{4}$$

If this assumption holds, then:

$$E\left(Y^c \middle| Z=z, D=1\right) = E\left(Y^c \middle| Z=z, D=0\right). \tag{5}$$

Rewriting the crucial term in (1), as:

$$E\left(Y^c \middle| D=1\right) = E_{Z|D=1}\left[E\left(Y^c \middle| Z=z, D=1\right) \middle| D=1\right], \tag{6}$$

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[7] Despite this advantage of an experimental approach some general and methodological problems related to these experiments exist which have been discussed in the literature. Especially ethical arguments against the random selection process and the practical difficulties of adequately conducting an experiment are most often mentioned. For a wider discussion of the relative advantages of experimental and non-experimental approaches see e.g. Burtless and Orr (1986), Björklund (1989), Heckman and Smith (1995).

and inserting equation (5), leads to:

$$E\left(Y^c \middle| D=1\right) = E_{Z|D=1}\left[E\left(Y^c \middle| Z=z, D=0\right) \middle| D=1\right]. \tag{7}$$

In principle an evaluation of (7), and thus α, on the basis of the group of non-trainees is now possible by conditioning on the distribution of all relevant covariates ($Z = z$) in the group of trainees. The implementation of conditioning is however limited in case of a high dimensional vector *z*. To deal with this dimensionality problem, Rosenbaum and Rubin (1983) suggest the use of the propensity score, i.e. the conditional probability of participating in training given the set of all relevant covariates, defined as $P(Z=z) \equiv P\left(D=1 \middle| Z=z\right)$. They show that if the potential outcome without training is independent of the assignment mechanism conditional on $Z = z$, then the conditional independence assumption can be extended to the use of the propensity score:

$$Y^c \perp\!\!\!\perp D \,\middle|\, P(Z=z). \tag{4'}$$

This leads to:

$$E\left(Y^c \middle| P(Z=z), D=1\right) = E\left(Y^c \middle| P(Z=z), D=0\right). \tag{5'}$$

The crucial term in (1) can now be written as:

$$E\left(Y^c \middle| D=1\right) = E_{P(Z)|D=1}\left[E\left(Y^c \middle| P(Z=z), D=0\right) \middle| D=1\right]. \tag{7'}$$

The major advantage of the identifying assumption (4’) is that it turns the estimation problem into a much easier task since one only has to condition on a univariate scale, i.e. on the propensity score.

In order to condition on the propensity score the next step has to be the estimation of this propensity score. This is done in subsections 4.2. and 4.3. by means of a panel probit model. Since we aim to construct an adequate control group that does not contain systematically different characteristics from those of the trainee group an appropriate way to condition on the estimated propensity score is to apply matching methods proposed by Rosenbaum and Rubin (1985) and Rubin (1991) (Subsection 4.4.).

4.2. A panel probit model for the estimation of the propensity score

A key ingredient of our empirical approach to overcome the evaluation problem is the estimation of the propensity score. In our case this estimation has to take into account that the starting dates of the PSVT courses vary over time among the participants, i.e. we are not evaluating one specific program that originated at one specific date, but we have different starting dates among the participants (for the distribution of the PSVT starting dates see appendix A, figure A.1.). This fact is important because if there are relevant time varying covariates which are related to the beginning of the training program, then they are not clearly defined for the non-trainees (Lechner, 1995). In order to deal with this problem all covariates ($Z = z_{it}$) refer to time *t*, i.e. the month of the questionnaire in each wave, while the dependent variable D_{it} is defined as the beginning of an actual training participation within the interval $(t, t+1]$. Thus the timevarying covariates are well defined for the non-trainees and always dated close to and prior to the beginning of a possible training participation which ensures a high explanatory power.[8]

For the estimation of the propensity score for participation in PSVT we propose a panel probit model which takes the following well known form:

$$D_{it}^{*} = z_{it}'\beta + u_{it} \qquad i = 1,\ldots,n;\ t = 1,\ldots,T, \tag{8}$$

where the *i* subscript indicates the individuals of a cross-section and the *t* subscript the time period. z_{it} is the *m*-dimensional vector of relevant pre-training covariates and β a corresponding parameter vector. u_{it} is an unobserved disturbance and D_{it}^{*} the latent continuous dependent variable. The relationship between D_{it}^{*} and the observed training participation D_{it} is given as follows:

$$D_{it} = I\left[D_{it}^{*} > 0\right] = I\left[z_{it}'\beta + u_{it} > 0\right], \tag{9}$$

where *I*, the indicator function, equals one if the expression in the brackets is true and zero otherwise. In matrix notation the model is written as $D_i = I\left[z_i'\beta + u_i > 0\right]$, where $z_i = \left(z_{i1},\ldots,z_{iT}\right)'$, $u_i = \left(u_{i1},\ldots,u_{iT}\right)'$, and D_i is the $(T \times 1)$-vector of observed training participation of individual *i*. To complete the specification let $s_i = \left(D_i, z_i\right)$ be a realization of *n* independent random draws of the joint distribution of a corresponding pair of random variables. Moreover, we assume that the error terms are jointly normally distributed $u_i \sim iid\ \ N(0,\Sigma)$ and independent of the explanatory variables z_i.[9]

To estimate the model above we adopt the Generalized Method of Moments (GMM) approach based on conditional moment restrictions as suggested by Newey (1990, 1993) for a general class of

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8 Note that the distance between the measurement of covariates and the possible beginning of a training participation may differ by between 1 and 12 months. This inaccuracy will be considered in the matching procedure where the precise starting month of the PSVT course for each trainee can be taken into account when choosing the optimal control (s). Lechner (1995, 1996) chooses a different solution to the problem of adequately dating time varying covariates when estimating the propensity score. He uses characteristics from the beginning of the observation period to explain all subsequent participation decisions and thus only needs to estimate a simple probit model. In this case however there is the problem that the distance between the time of covariates measurement and the possible beginning of a training course may become much larger (in our case between 1 month and 8 years). Thus of course the question arises whether these covariates are informative enough to explain a training participation up to 8 years later.

9 In order to ensure identification only one main-diagonal element of Σ has to be set to unity. However, as we are only interested in scaled estimates of β we assume that all main diagonal elements of Σ equal one (e.g. Avery et al., 1983).

nonlinear estimators and adapted to the panel probit model for example by Bertschek and Lechner (1998) and Inkmann (1997). We use a particular variant of this estimator that is based on a nonparametric estimation of the optimal set of instruments. As shown by the latter authors in Monte Carlo studies this estimator turns out to have better properties than its potential competitors, namely the widely used maximum likelihood (ML) estimator with equicorrelated residuals (known as random effects specification) or an estimation based on the Simulated Maximum Likelihood (SML) technique. Bertschek and Lechner (1998) find that this particular GMM estimator has good small sample properties and only tiny efficiency losses when compared to the ML estimation with equicorrelated residuals. A main advantage of the GMM estimator over the ML estimator is, that the imposed restrictions do not depend on parameters of the intertemporal error covariance matrix (Σ). Thus, although ML has the advantage of being more efficient in the case of a correctly specified Σ matrix it has the crucial disadvantage of being non-robust with respect to misspecification of the true intertemporal error covariance matrix (Avery et al., 1983).[10] Inkmann (1997), who compares the GMM with the SML estimator, finds slight efficiency gains from SML when the underlying model is correctly specified. However in case of multiplicative heteroscedasticity or when heteroscedasticity over time is ignored, the GMM estimator turns out to be more robust than SML. Moreover in order to make SML computationally tractable it is a common practice to impose certain restrictions on Σ. As pointed out by Bertschek and Lechner (1998), this is another drawback of SML since the consistency of SML has not yet been proven for the case of a misspecified Σ matrix.

The proposed GMM estimator of the panel probit model builds upon the following conditional moment restrictions (Bertschek and Lechner, 1998):

$$E\left[\rho(s_i,\beta_0)\middle|z_i\right]=0\,, \tag{10}$$

where

$$\rho(s_i,\beta_0)=\left(\rho_{i1},\ldots,\rho_{iT}\right)',$$

$$\rho_{it}=D_{it}-E\left(D_{it}\middle|z_{it}\right),$$

$$E\left(D_{it}\middle|z_{it}\right)=\Phi\left(z_{it}'\beta_0\right),$$

and Φ denotes the c.d.f. of the standard normal. Note that these conditional moment restrictions imply that $\rho(s_i,\beta_0)$ is uncorrelated with all functions of z_i. Let $A(z_i)$ denote a ($R \times T$)-dimensional matrix of functions of z_i, called instruments. Then the unconditional moment restrictions

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10 As noted by Avery et al. (1983) if Σ is incorrectly constrained to have equal off-diagonal elements, as assumed by the random effects specification, then ML yields inconsistent estimators. The only exception to this is the probit ML assuming independent errors, i.e. the off-diagonal elements are constrained to be zero.

$$E\left[A(z_i)\rho(s_i,\beta_0)\right]=0 \tag{11}$$

are satisfied by (10) and the law of iterated expectations. As shown by Newey (1990, 1993) there is an optimal, i.e. variance minimizing, set of instruments A^* which is given by:

$$A^*(s_i,\beta_0,\Sigma_0)=C\cdot D(s_i,\beta_0)'\,\Omega(s_i,\beta_0,\Sigma_0)^{-1}, \tag{12}$$

where

$$\Omega(s_i,\beta_0,\Sigma_0)=E\left[\rho(s_i,\beta_0)\rho(s_i,\beta_0)'\middle|z_i\right],$$

$$D(s_i,\beta_0)=E\left[\frac{\partial\rho(s_i,\beta_0)}{\partial\beta'}\middle|z_i\right],$$

and C is any nonsingular matrix. Using the optimal set of instruments a GMM estimator of the true parameter vector β_0 results from minimizing the quadratic form of the sample moments of the corresponding unconditional moment restrictions:

$$\hat{\beta}=\underset{\beta\in B}{\arg\min}=\left[\frac{1}{n}\sum_{i=1}^{n}A^*(s_i,\beta,\Sigma)\rho(s_i,\beta)\right]'P\left[\frac{1}{n}\sum_{i=1}^{n}A^*(s_i,\beta,\Sigma)\rho(s_i,\beta)\right], \tag{13}$$

where P is a suitable positive semi-definite matrix of weights and B some set of feasible values for β. Under suitable regularity conditions the asymptotic distribution of β^* is given by

$$\sqrt{n}(\beta^*-\beta_0)\xrightarrow{d}N\left(0,\left(E\left[D(s_i,\beta_0)'\,\Omega(s_i,\beta_0,\Sigma_0)^{-1}D(s_i,\beta_0)\right]\right)^{-1}\right). \tag{14}$$

Note that since the column dimension of A^* equals m, the number of parameters to be estimated, the asymptotic variance of β^* does not depend on the choice of the weighting matrix P.[11]

A difficulty of this GMM estimator is the construction of a consistent estimator $\hat{A}^*$, since the optimal instruments A^* depend on the unknown off-diagonal elements of the true intertemporal error covariance matrix (Σ_0). In order to circumvent this problem Newey (1990, 1993) proposes the

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11 In order to make the objective function invariant to nonsingular transformations of the optimal instruments and thus improve computation Newey (1993) suggests to use $\hat{P}^*=\left(\frac{1}{n}\sum_{i=1}^{n}\hat{A}^*\hat{A}^{*\prime}\right)^{-1}$ where $\hat{A}^*$ is a consistent estimator of A^*.

use of nonparametric methods like series approximation or nearest neighbor estimation to obtain a consistent estimate of $\Omega\left(s_i, \beta_0, \Sigma_0\right)$. Following Bertschek and Lechner (1998) and Inkmann (1997), we apply the latter approach and estimate $\Omega\left(s_i, \beta_0, \Sigma_0\right)$ by:

$$\hat{\Omega}\left(s_i, \hat{\beta}_1, k\right) = \sum_{j=1}^{n} w_{ij} \rho\left(s_i, \hat{\beta}_1\right) \rho\left(s_i, \hat{\beta}_1\right)' , \tag{15}$$

where $\hat{\beta}_1$ denotes a consistent first stage GMM estimate based on the scores of the pooled ML estimator (see Avery et al., 1983) and w_{ij} describes a weighting function. Let the observations be ordered according to their distance $\Psi_{ij} = \psi_{ij} \psi_{ij}'$ to observation i where $j = 1$ denotes the observation i itself and $\psi_{ij} = \left[\left(z_{1i} - z_{1j}\right)\hat{\beta}_1, \ldots, \left(z_{Ti} - z_{Tj}\right)\hat{\beta}_1\right]$. Then the weighting function w_{ij} assigns positive weights to those observations that belong to the k nearest neighbors of individual i and zero weights to all other individuals. We will use the following uniform weighting function:[12]

$$w_{ij} = \begin{cases} 1/k & 2 \leq j \leq k \\ 0 & j = 1, j > k. \end{cases} \tag{16}$$

It remains to choose the number of nearest neighbors k, i.e. the smoothing parameter in the weight function. Newey (1993) suggests a cross validation function that is based on the difference between true $\Omega\left(s_i, \beta_0, \Sigma_0\right)$ and its k-nearest neighbor estimate:

$$C\hat{V} = tr\left[Q \sum_{i=1}^{n} R\left(s_i, \hat{\beta}_1\right) \hat{\Omega}\left(s_i, \hat{\beta}_1, k\right) R\left(s_i, \hat{\beta}_1\right)'\right], \tag{17}$$

where

$$R\left(s_i, \hat{\beta}_1\right) = \left\{\hat{A}^* \left[\rho\left(s_i, \hat{\beta}_1\right)\rho\left(s_i, \hat{\beta}_1\right)' - \hat{\Omega}\left(s_i, \hat{\beta}_1, k\right)\right]\right\} \hat{\Omega}\left(s_i, \hat{\beta}_1, k\right)^{-1},$$

and Q represents any positive definite matrix. The optimal k minimizes the cross validation function (17). Since the evaluation of the cross validation function for all possible k would be very time consuming a straightforward way to obtain an optimal k is to compute (17) only for a given sequence k, e.g. $k = 0.05n, \ldots, 0.95n, n$.

A main drawback of using a balanced panel is that it leads to an efficiency loss, because the

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[12] Smoother versions are quadratic or triangular weights. However, the choice of the uniform weight function relies on results of Bertschek and Lechner (1998) who compare these different weight functions and do not obtain any serious differences in the estimations results.

information of the incomplete observations is not used for the estimation. This problem is especially severe if the further aim is to construct a matched sample of a size which is determined by the low number of trainees. Suppose that a variable $r_i = (r_{i1}, \ldots, r_{iT})'$ indicates whether the observation *i* is observed in wave *t* $(r_{it} = 1)$ or not $(r_{it} = 0)$. If we assume that the observations are missing at random we do not require the specification of the attrition process. Hence the conditional moment restriction is not affected by r_i (see Lechner and Breitung, 1996):

$$E\left[\rho(s_i, \beta_0) \middle| z_i, r_i = 1\right] = E\left[\rho(s_i, \beta_0) \middle| z_i, r_i = 0\right] = E\left[\rho(s_i, \beta_0) \middle| z_i\right] = 0. \tag{18}$$

The extension of the GMM estimator above to an unbalanced panel is straightforward, because the same conditional moment restriction can be used as for the balanced panel.

4.3. Estimation results for the propensity score

Our selection of potentially relevant covariates relies on theoretical hypotheses related to human capital theory. Moreover we have to consider the institutional arrangements which are relevant for the employment agencies when choosing PSVT participants. According to the AFG applicants for PSVT are selected on the grounds of their education, employment history, job specific qualifications, other qualifications, personal situation, motivation with respect to a PSVT course and motivation with respect to a future employment. Finally, an effort is made to proceed in accordance with relevant variables suggested in other empirical studies of training participation (e.g. Blundell et al., 1994). Table 4 gives the results of our panel probit estimation.

Theory suggests that investment in human capital and hence participation in training decreases with age. Our estimation results exhibit a concave age influence with a maximum at approximately age 31 (Age/10, $(\text{Age}/10)^2$). The hypothesis that men have better access to training due to discrimination against women could not be confirmed (Female). Foreign nationality (Foreigner) is found to have a significant negative effect. This result is in line with the hypothesis that minority groups have poor access to training programs. On the other hand the positive effect of being disabled could result from the fact that one main goal of the AFG is the support of target groups with particularly bad labor market prospects. The hypothesis that family related variables might influence training probability due to a greater marginal value of non-market time could be verified for marital status (PartHH). However, this is not true for women with children (Female×KS, Female×KM, Female×KL), irrespective of the children’s age.

The hypothesis that earlier accumulated human capital positively influences investment into new human capital could only partly be verified. The variable that indicates whether the individual has completed an apprenticeship (Lehre) shows that earlier accumulated “vocational“ human capital positively affects investment into new “vocational“ human capital. In contrast a high school degree (Abitur) indicating a higher “school specific“ human capital negatively affects the probability of participating in PSVT.

Evidently, participation in PSVT is also related to the actual labor market history since this can reflect a special need to participate in training. Along the lines of Heckman and Smith (1997) it is observed that labor force status dynamics seem to play a role in the participation process. While the status of being unemployed (Unemployed) during the month of the questionnaire does not affect the participation decision, different temporal labor force dynamics patterns do provide significant information about the participation probability. Individuals who have been out of the labor force for at least the last eleven months prior to the month of the questionnaire (OLF→OLF) have a significantly reduced training participation probability compared to those who were employed for at least the last eleven months prior to the month of the questionnaire.[13]

No evidence is found that the status employed in the month of the questionnaire (Employed) and in particular variables associated with the job position (BluCollar, WhiCollar) affect participation in PSVT. However working in the occupation originally educated for (JobEduc) or an increased job tenure (JobTenure) seems to provide job safety that reduces the PSVT participation probability.

As emphasized by Heckman et al. (1997), variables that predict job seeking might be very important determinants of training participation. Hence variables related to future plans regarding employment were included. Individuals who are not currently employed and are seeking an employment in the future (FutEmpDes) are most likely to participate in a PSVT course. In contrast individuals who only wish to become employed part time in the future have a reduced participation probability (FutPartTime). Moreover, individuals who are currently highly satisfied with life in general (SatisLife) are less likely to participate in a PSVT course.

Finally some macroeconomic factors that might affect PSVT participation were considered. Heckman et al. (1997) point out that differences in regional labor market conditions typically influence training participation decisions. By including an indicator that captures the number of vacancies and unemployed at the state level (RegSituation) we were able confirm this hypothesis.[14]

< Table 4 about here >

4.4. Application of matching methods

The aim of our matching method is to select for each trainee non-trainees, that resemble him/her as accurately as possible in terms of pre-training characteristics and thus to achieve a conditional independence between the potential outcome without training (Y^c) and the decision to participate in training (D). If this is done correctly, we obtain a matched sample consisting of trainees and controls

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[13] The labor force status pattern is defined by looking backwards in time starting in the month of the questionnaire (t) and ending in t minus 11 months. The second status in each pattern is the status in t and the first status is the most recent prior status within the indicated time period (11 months prior t). Hence "Unm→Emp" indicates an individual employed at the month of the questionnaire (t), but whose most recent preceding labor force status within the prior 11 months was unemployment. Repeated patterns such as "Unm→Unm" indicate individuals with the same labor force status from t minus 11 to the month of the questionnaire.

[14] In a different specification we also incorporated yearly dummies to control for the effect of business cycles. However a Wald test for joint restrictions rejected the significance of these variables.

who, on average, - similar as in a randomized experiment - do not systematically differ in any relevant characteristics. Thus, we eliminate the need to take the selection process into training into account when estimating its impact on unemployment duration. A detailed description of our matching procedure is given in appendix B.

The central conditioning, respectively matching variable is the estimated propensity score ($z'\hat{\beta}$). As stressed by Heckman et al. (1998) a successful application of matching methods is only possible within the range of common support of the distribution of the propensity score of the trainee and non-trainee group. Obviously this requires a large a-priori overlap between the densities of $z'\hat{\beta}$ for the trainees and non-trainees. A comparison of the distributions of $z'\hat{\beta}$ for each year reveals that despite the mass of the distribution of the non-trainees being to the left of the trainees' a large overlap indeed exists (see appendix B, figure B.1.).

The estimation of the propensity score incorporates characteristics that are all based on the date of the yearly interview. Potentially important differences like a particularly bad labor market situation for trainees just prior to the PSVT course, as emphasized by Lechner (1996) for East Germany or by studies like Ashenfelder and Card (1985) or Card and Sullivan (1988) for the US, thus might not be captured adequately. To account for these differences we extend our matching procedure to include a set of variables obtained from the retrospective calendar which describes the monthly employment history of a trainee just prior to the beginning of the PSVT course.

In principal it seems straightforward to match each trainee with only the closest non-trainee (one to one sampling). However, neglecting the other non-trainees leads to a small sample size and thus low degrees of freedom when estimating the impact of PSVT on the unemployment duration. To increase efficiency one can extend the one to one sampling to an oversampling by repeating the matching procedure to incorporate information about other non-trainees who also closely resemble the trainees with respect to the relevant matching variables. Of course when matching further non-trainees to each trainee the drawback arises that the match quality and thus bias reduction probably declines in comparison to the one to one sampling.

In order to choose an adequate control group, we compare the matching quality of the one to one sampling (OTOS) with four different oversampling procedures, namely OVS1, OVS2, OVS3 and OVS4, where the number stands for the repetitions of the matching algorithm. Table 5 demonstrates the quality of the five different matching procedures for selected characteristics in the month of the questionnaire prior to PSVT. A comparison between randomly selected non-trainees and trainees reveals significant differences especially in the propensity score, in some socio-demographic

< Table 5 about here >

variables and in job and employment related variables. In contrast, the distributions of the characteristics for all five matched control pools resemble the group of trainees very closely, and no significant differences were detected. In the two final rows of table 5 summary statistics, the median and mean of absolute standardized differences, compare the five matching procedures regarding the matching quality (bias reduction) for all variables considered in the matching algorithm. As expected they rank the matching procedures OTOS OVS1 and OVS2 in the first three positions, while OVS3 and OVS4 take up the last positions. Figure 5 explains this ranking by taking a closer look at the performance of the different matching procedures regarding the employment history in the 12 months prior to PSVT. Comparing random non-trainees and trainees reveals that as training approaches the trainees exhibit significantly higher unemployment and lower employment rates. A look at the performance of the five matching procedures reveals that only OTOS and OVS1 are able to completely remove this discrepancy in the employment history. Given these results the following analysis will be based on the OVS1 control group, as it promises efficiency gains when compared to the OTOS control group as well as a match quality which is very close to the match quality of the OTOS control group (see MedSD and MeaSD statistic in table 5). Additional descriptive statistics of the matching performance of the OVS1 control group are given in appendix B, table B.1.

< Figure 5 about here >

< Figure 6 about here >

5. Publicly sponsored vocational training and unemployment duration

5.1. Duration analysis based on a matched sample

So far we have considered the evaluation problem that arises from selection into PSVT. Via our matching procedure a matched sample was generated consisting of 89 trainees and 177 controls who on average -similar to a randomized experiment - do not systematically differ with respect to any of the relevant characteristics. 126 post-training unemployment spells are observed for the trainees and 239 for the controls. An obvious way to assess the impact of PSVT on unemployment duration is to compare the average post-training unemployment duration of trainees and controls.

Yet, as Ham and LaLonde (1990, 1996) demonstrate in an experimental framework which eliminates the need to control for selection into training by randomization, the estimation technique to evaluate an unbiased training effect depends heavily on the particular outcome of interest. If the outcome of interest is the employment or unemployment rate, it suffices to simply compare trainees' and controls' post-training average employment or unemployment rates. However, if one looks at the unemployment duration, as we do, there are three reasons, why a simple comparison of trainees' and controls' average duration would be insufficient and would lead to potentially biased estimates of the training effect:

- The first problem with simply comparing the trainees' and controls' average duration is the existence of right censored spells. A comparison of average unemployment duration fails to take into consideration that right censored spells are still ongoing and not observed until they have finished.

- A second problem is that trainees' and controls' unemployment spells do not necessarily originate at the same time and consequently time varying characteristics and demand conditions might differ.

- Finally, even if the unemployment spells of trainees and controls were to originate at the same date, e.g. the first period after training, the following problem would still exist: Assume that in this period trainees and controls on average do not systematically differ in their characteristics with the exception of having participated in training. Then the difference between the share of trainees and controls leaving unemployment would be an unbiased estimate of the training effect on reemployment chances in the first period. In the second period, however, the average characteristics of those trainees and controls who are still unemployed do not necessarily equate (as they did in the first period). If for example, a positive training effect allows relatively more trainees than controls with bad labor market characteristics to be able to exit unemployment, then the group of unemployed trainees in the second period will tend to have better labor market characteristics than the corresponding group of unemployed controls. Hence the difference in transition rates between trainees and controls in the second and subsequent periods of the initial unemployment spells reflects not only the effect of training but also the fact that now the trainees on average have better labor market characteristics than the controls.

These issues emphasize that even with a matched sample it is required to additionally rely on an econometric model. An appropriate approach which considers right censoring, and takes into account other observable and unobservable characteristics than training which also influence the unemployment duration, is a hazard rate model.

5.2. A discrete time hazard rate model

Hazard rate models are concerned with the observation *i*'s instantaneous rate of leaving a certain state of interest (here: unemployment) per unit time period at *t*: $\lambda_i(t) = \lim_{dt \to 0^+} P\left(t \le T_i < t + dt \middle| T_i \ge t\right) \cdot \left(dt\right)^{-1}$, i.e. the hazard or transition rate. T_i, the duration time, is a continuous non-negative random variable with realization *t*. The probability of survival up to *t* is given by the corresponding survivor function $S_i(t) = \exp - \int_0^t \lambda_i(u)\,du$.

The derivation of the particular hazard rate model utilized in this study begins with the well-known and widely used Mixed Proportional Hazards model for continuous time (e.g. Elbers and Ridder, 1982) which is based on a model proposed by Cox (1972):[15]

$$\lambda_i\left(t \middle| x_i(t), \omega_i\right) = \lambda_0(t) \exp\left(x_i'(t)\beta + \omega_i\right). \tag{19}$$

It relates the hazard rate to a set of explanatory variables $x_i(t)$ and a so-called baseline hazard rate λ_0 which is independent of individual characteristics as it gives the hazard rate for $\exp(0)$. In order to take into account unobserved heterogeneity and thus to avoid spurious duration dependence, a

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15 For a wider overview on discrete-time hazard models and their empirical applications see Hujer et al. (1996).

random variable ω_i, that, by assumption, is not correlated with the covariates, is also included. β is the vector of coefficients to be estimated. In contrast to parametric models (e.g. the Weibull hazard rate model) λ_0 is estimated non-parametrically in order to prevent a possible source of misspecification. To assess the distribution of the heterogeneity component ω_i, Heckman and Singer (1984) propose the use of non-parametric methods, because of sensitivity of parameter estimates to specific parametric distributions. However, Trussell and Richards (1985) point out, that much of the parameter instability found by Heckman and Singer (1984) might be a result of their parametric baseline hazard. They emphasize that in the context of a non-parametrically specified baseline hazard rate, a parametric specification for the distribution of the heterogeneity would also seem appropriate.

Up to now, it has been assumed that time is observed continuously. Yet, as the duration in the GSOEP data is only available on a monthly basis, it is not adequate for us to apply a model based on the notion of continuous time. When using continuous time models with grouped duration data, a term used by Kiefer (1988), parameter estimates could be meaningless due to the existence of ties, i.e. equal durations for different observations (e.g. Kalbfleisch and Prentice, 1980, Cox and Oakes, 1984). Assuming that duration data are grouped into *J*+1 intervals with the *j*-th interval defined as $\left[t_j, t_{j+1}\right)$, $j = 0,1,\ldots J$, the discrete hazard rate for an arbitrary *j* given the set of covariates $x_i(t)$ is defined in terms of the survivor function as:

$$h_i\left(j \middle| x_i(t)\right) = P\left[T_i < t_{j+1} \middle| T_i \geq t_j, x_i(t)\right] = \frac{S_i\left(t_j\right) - S_i\left(t_{j+1}\right)}{S_i\left(t_j\right)}. \tag{20}$$

If we assume that changes in the covariates $x_i(t)$ only occur at the lower bounds of each interval *j*, i.e. the covariates are constant within each interval, then the survivor function that corresponds to (19) takes the following form:

$$\begin{aligned} S_i\left(t_j \middle| \omega_i\right) &= \exp\left(-\sum_{m=0}^{j-1} \int_{t_m}^{t_{m+1}} \lambda_0(u) \exp\left(x_i'\left(t_m\right)\beta\right) \exp\left(\omega_i\right) du\right) \\ &= \exp\left(-\exp\left(\omega_i\right) \sum_{m=0}^{j-1} \exp\left[\left(x_i'\left(t_m\right)\beta\right) + \gamma_m\right]\right), \end{aligned} \tag{21}$$

where $\gamma_m = \ln \int_{t_m}^{t_{m+1}} \lambda_0(u) du$. To obtain $S_i\left(t_j\right)$ we assume that $\exp\left(\omega_\mathrm{i}\right) = \tau_\mathrm{i}$ follows a gamma distribution with mean one and variance σ^2. Let $\mathrm{f}\left(\tau_i\right)$ denote the corresponding density function, then the integration with respect to τ_i leads to:

$$\begin{aligned} S_i\left(t_j\right) &= \int_0^\infty \exp\left(-\tau_i \sum_{m=0}^{j-1} \exp\left[\left(x_i'\left(t_m\right)\beta\right) + \gamma_m\right]\right) \mathrm{f}\left(\tau_i\right) d\tau_i \\ &= \left(1 + \sigma^2 \cdot \sum_{m=0}^{j-1} \exp\left[\left(x_i'\left(t_m\right)\beta\right) + \gamma_m\right]\right)^{-\sigma^{-2}}. \end{aligned} \tag{22}$$

To derive the resulting likelihood function define a dummy variable δ_i indicating whether the *i*-th spell is right censored ($\delta_i = 0$) or not ($\delta_i = 1$). For a sample of *N* spells we then obtain:

$$L\left(\gamma,\beta,\sigma^2\right) = \prod_{i=1}^{N}\left[\left(S_i\left(t_j\right)\cdot S_i\left(t_{j+1}\right)^{-1} - 1\right)^{\delta_i}\cdot S_i\left(t_{j+1}\right)\right]. \tag{23}$$

Inserting (22), rearranging terms and taking logarithms leads to the following log-likelihood:

$$\begin{aligned} l\left(\gamma,\beta,\sigma^2\right) = \sum_{i=1}^{N}\ln\Bigg\{ & \delta_i\cdot\left[1+\sigma^2\cdot\sum_{m=0}^{j-1}\exp\left(x_i'\left(t_m\right)\beta+\gamma_m\right)\right]^{-\sigma^{-2}} \\ & +\left(1-2\delta_i\right)\cdot\left[1+\sigma^2\cdot\sum_{m=0}^{j}\exp\left(x_i'\left(t_m\right)\beta+\gamma_m\right)\right]^{-\sigma^{-2}}\Bigg\}. \end{aligned} \tag{24}$$

Similar models have already been applied by Meyer (1990) and Narendranathan and Stewart (1993) to assess the impact of unemployment benefits on unemployment duration.

5.3. Estimation results

Because this study focuses on the estimated effect of PSVT on reemployment probabilities and the empirical specification of the hazard rate model for unemployment duration has been extensively discussed in earlier work the complete estimation results have been referred to appendix C.

In principle the training effect could be measured by a variable that equals one if the individual participated in a PSVT course and zero if not (TR). However, we tried to determine whether any heterogeneous effects of PSVT are related to the duration of the PSVT course. This might be of particular interest as very short term courses (between 2 and 6 weeks) were no longer supported by the Federal Labor Office (BA) after 1992 (see section 2). Two indicator variables have been included to capture PSVT courses that lasted longer than 6 months (TR_Dur7+) or 4 to 6 months (TR_Dur4-6). Consequently the reference category refers to short term courses lasting no longer than three months. Besides considering differences in the PSVT course duration an attempt is made here to take into account any influence the post-training timing of the unemployment spell might have on the effectiveness of the PSVT course to be considered. In this respect, the distance between the end of the PSVT course and the beginning of the post-training unemployment spell may be a relevant factor. Therefore two additional indicator variables were defined. The first indicator variable focuses on unemployment spells that begin within the time period of between two and twenty four months after PSVT has concluded (TR_2-24) while the second indicator variable refers to unemployment spells that begin no earlier than two years after the PSVT course ends (TR_25+). Note that the reference category to these two post-training timing variables covers unemployment spells that take place immediately after the PSVT course, i.e. if the trainee becomes unemployed in the first month after the PSVT course ends.

To answer the question whether participation in PSVT leads to a significant average improvement in reemployment chances table 6 presents the nine possible composed PSVT effects. We find a significant positive PSVT effect on reemployment chances for short and medium term courses, i.e. courses lasting no longer than 6 months duration, if the unemployment spell begins within two to twenty four months after the PSVT course has concluded. A significant (at a 10% level) positive PSVT effect for unemployment spells that begin immediately after the course ends could only be found for short term courses lasting no longer than 3 months.

< Table 6 about here >

An explanation for the weakness of the immediate effect could be that it implicitly focuses on individuals who already entered training as (possibly long term) unemployed while the stronger significant positive effects discussed above (on unemployment spells that originate within 2 to 24 months after the PSVT course ends) implicitly capture the influence of PSVT on individuals who entered training as employed, but directly threatened by unemployment (e.g. these individuals already know their date of firing). In this sense, PSVT seems to be more effective on post-training reemployment chances, if it is completed in a preventive way, i.e. during a still ongoing employment episode and not once the individual is already in an ongoing unemployment episode. Finally, for short and medium courses, no significant effects were detected for unemployment spells that begin no earlier than two years after the PSVT course ends. A reason for this might be that the human capital built up during PSVT decays with time after training.

Long term courses lasting more than 6 months did not prove at all effective in increasing post-training reemployment chances. In fact these courses are significantly (at a 10 % level) less effective than courses lasting no longer than 3 months (see appendix C, table C1, variable TR_Dur7+). Moreover, the effect of these courses on unemployment spells that begin no earlier than 24 months after PSVT ends is significantly (at a 10% level) negative. A possible explanation might be that a long period of absence from the labor market due to off the job (classroom) training, could be regarded as a negative screening device by employers, thus offsetting the positive effect of new human capital. Given the last result it seems questionable whether the post 1992 reduction of financial support by the Federal Labor Office, in particular for very short term further training courses ("Fortbildung"), helped to improve the labor market situation in West Germany.

To illustrate the combinations of the estimated PSVT effects figures 7 and 8 present two simulations based on the survivor function which gives the probability of still being unemployed after month *t*. In figure 7 we focus on the composed effect of a PSVT course that lasted no longer than 3 months on an unemployment spell that originated within 2 to 24 months after training. We compare two individuals, who became unemployed for the first time in January 1988 and

< Figure 7 about here >

are equal in all characteristics except for the fact that one has completed a PSVT course that lasted no longer than 3 months within 2 to 24 months prior to January (trainee) and the other has never participated in PSVT (control).[16] The survivor function for the control lies considerably above the

[16] The other characteristics are defined as follows: The individuals live in the state of Nordrhein-Westfalen. Both are

one for the trainee. Looking at the difference between the two survivor functions reveals significant differences (at a 5% level) due to the PSVT course from month 3 onwards. For example the control's probability of still being unemployed after month 4 is 76.9%, while the corresponding probability for the trainee is 48.2%.

Figure 8 illustrates the effect of an increased PSVT course duration. We choose the same trainee as in figure 7 and compare his situation (having participated in a course that lasted no longer than 3 months, i.e. a short term PSVT course) with an individual equal in all characteristics except for having participated in a PSVT course that lasted longer than 6 months (i.e. a long term PSVT course). The positive effect of the long term PSVT course is far lower than that of the short term PSVT course. Looking at the difference between the two survivor functions also reveals significant differences from month 3 onwards, however only at a 10% level. For example the difference in the survivor function in month 6 is 21.6%.

< Figure 8 about here >

6. Conclusion

In this paper we assess the impact of public sector sponsored vocational training on unemployment duration in West Germany for the period 1985 to 1993. Because we are using a non-experimental data set, the GSOEP, an important factor in obtaining reliable results is to overcome the intriguing sample selection problem. In order to construct an adequate control group we rely on a matching procedure. This procedure uses as a main matching variable the propensity to participate in training which was estimated by means of a panel probit model. As a comparison between the trainee and matched control group shows, the matching procedure eliminates the systematic differences that exist between random non-trainees and trainees. We emphasize that even while basing analysis on a matched sample, it remains necessary to use a discrete hazard rate model to evaluate the impact of training on the subsequent unemployment duration. Our results indicate that a significant positive effect on reemployment chances due PSVT can only be expected for courses with a duration of no longer than six months. No significant positive effects on post-training reemployment chances where found for courses lasting longer than six months. In fact PSVT these courses are significantly less effective at increasing reemployment chances than those lasting no longer than three months. Given the last result it seems questionable whether the post 1992 reduction of financial support by the Federal Labor Office in particular for very short term (between two and six weeks) further training courses ("Fortbildung") helped to improve the labor market situation in West Germany.

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males, possess a German nationality, are not disabled and are not living together with a partner. They have no high school degree (Abitur) or university degree, but have completed an apprenticeship. At the beginning of the unemployment spell they are 33 years old. Prior to the unemployment spell they were employed. The replacement ratio is 0.5.

Appendix

Appendix A: Variables and descriptive statistics for the PSVT courses

Table A.1 gives the definition of all variables used in this paper.

< Table A.1 about here >

There are 162 individuals who participated in at least one PSVT course that began between early 1985 and 1993. Table A.2 shows that more than 75% of these individuals only participated in one course during the time span considered:

< Table A.2 about here >

For these 162 individuals, we observe 198 PSVT courses. 169 of these courses are not right censored. Figure A.1 gives information about the start and end dates of the courses separated for all courses and uncensored courses only. Observe the high number of courses starting (ending) in January (December). This could be the result of the so-called heaping effect that arises from the retrospective design of the income calendar in the GSOEP leading to a disproportionately high number of spells ending in December and starting in January. See e.g. Hujer and Schneider (1996), Kraus and Steiner (1998) and Torelli and Trivellato (1993) for different heaping adjustment procedures in hazard rate models.

< Figure A.1 about here >

Figure A.2 gives information about the durations of PSVT courses. The mean (std.) of all PSVT course durations is 12.4 (7.4) and 10.3 (7.0) for the uncensored sample. 78.2% (77.5%) of all (the uncensored) PSVT courses are no longer than 12 months. Again, the high number of PSVT courses that are exactly 12 months long could be a result of the above mentioned heaping effect.

< Figure A.2 about here >

Appendix B: Matching procedure, requirements and additional results for the OVS2 matching procedure

The matching procedure is based on the one proposed by Rosenbaum and Rubin (1985) and Rubin (1991) and applied by Lechner (1995, 1996).

The steps of the matching procedure are as follows:

1. Divide the individuals into two separate groups called trainees and non-trainees according to whether they have participated in PSVT during the time span 1985-1992 (trainee group) or not (non-trainee group).

2. Randomly select a trainee (denoted by *i*) from the trainee group. If this trainee participated in more than one PSVT course take the earliest one as being relevant for the following steps. If we observe a post-training unemployment spell for this trainee go to step 3. If not, this trainee will be removed and not further considered in the estimation, and step 2 has to be repeated.

3. Based on the estimated panel probit model compute the propensity score $z_{it}'\hat{\beta}$ and the variance $\mathrm{Var}(z_{it}'\beta)$ for the trainee i in wave *t*, where *t* refers to the month of the questionnaire prior to the beginning of the PSVT course. Construct the interval $z_{it}'\hat{\beta} \pm c\sqrt{\mathrm{Var}(z_{it}'\hat{\beta})}$ for this trainee, and choose *c* such that one obtains a 90%-confidence interval around $z_{it}'\hat{\beta}$.

4. Find observations in the non-trainee group (denoted by *j*), whose first unemployment spell during the post-training period (of the trainee *i*) is a fresh spell and that obey $z_{jt}'\hat{\beta} \in \left(z_{it}'\hat{\beta} \pm c\sqrt{\mathrm{Var}(z_{it}'\hat{\beta})} \right)$ in wave *t*.

5. a. If there is no non-trainee lying between the given limits of the confidence interval, trainee i will not be considered further and step 2 has to be repeated.

   b. If there is only one non-trainee between the given limits of the interval go to step 6.

   c. If there is more than one observation in the confidence interval proceed as follows: Compute additional match variables related to monthly pre-training employment status and a subset of variables already included in the estimation of the propensity score. Denote these variables as a_{it} and a_{jt}. Evaluate the distance $d(j,i) = (z_{jt}'\hat{\beta}, a_{jt})' - (z_{it}'\hat{\beta}, a_{it})'$ between each non-trainee *j* and trainee *i*. Choose the non-trainee who is the "dosest neighbor" of the trainee *i* in terms of the Mahalanobis distance, defined as: $md(j,i) = d(j,i)' C^{-1} d(j,i)$, where *C* is the estimated sample covariance matrix of $(z'\hat{\beta}, a)'$ in the group of non-trainees in wave t.

6. Remove the trainee and non-trainee (now matched control) from their respective groups. If there

are any observations left in the trainee group, start again with step 2.

Along the lines of Lechner (1995, 1996) we use the unbounded score $z_{it}'\hat{\beta}$ instead of the bounded propensity score $\Phi\left(z_{it}'\hat{\beta}\right)$ as the main matching variable. Due to the location and symmetry of the distribution of $z_{it}'\hat{\beta}$ the use of $\Phi\left(z_{it}'\hat{\beta}\right)$ would lead to an undesirable asymmetry when $\Phi\left(z_{it}'\hat{\beta}\right)$ is close to 0 or 1. In contrast to Lechner (see section 4.2.) we estimated the propensity score via a panel probit model. Thus, although in principle a trainee could be matched to a non-trainee at *t* different times it seems straightforward to choose the last questionnaire prior to training participation as the relevant matching date for each trainee (Step 3). Note that if we find no non-trainee with a propensity score in the range of the trainee's individually varying confidence interval (i.e. we have a lack of common support, see. Section 4.4) we do not further consider this trainee in our training evaluation (Step 5. a). In order to control for monthly pre-training employment history we include the unemployment and employment status in the last month before training as well as the average unemployment and employment status for the last 4 and 12 months before training as additional match variables. To further enhance the matching quality, the matching procedure can also directly incorporate a subset of variables that were used for the propensity score estimation. We included as a further variable only Foreigner. Since we focus on the outcome unemployment duration we require to observe for each trainee and non-trainee at least one post-training unemployment spell (Step 2 and Step 4). An initial condition problem (see Ham and LaLonde, 1990, 1996) that would arise when comparing post-training unemployment spells of trainees with controls' that are already in progress when training ends, makes us restrict the matching procedure to controls whose first post-training unemployment spell is a fresh spell (i.e. the starting point of the unemployment spell is dated after the end of the corresponding trainee's PSVT course). In the case of one to one sampling each trainee is only matched to the closest non-trainee. Thus the matching procedure is finished when for every valid trainee one non-trainee (now control) has been found. In the case of oversampling the matching procedure can be repeated further times in order to find further non-trainees for each valid trainee.

Figure B.1 depicts the distributions of the propensity scores $z'\hat{\beta}$ for the non-trainees and trainees and reveals that the requirement of a large overlap for every year indeed exists.

< Figure B.1 about here >

Table B.1 gives further information about the quality of the OVS2 matching procedure.

< Table B.1 about here >

Appendix C: Further results of the estimates for the hazard rate model for the transition unemployment ⇒ employment.

Note that when interpreting these variables, one has to take into account, that they can only describe effects that are relevant for the matched sample under consideration, not, for example, for all individuals in our sample who became unemployed. Estimations based on all unemployed indicate more significant effects regarding the socio-demographic variables (e.g. Hujer and Schneider, 1996, Hujer et al., 1997a).

First we examine possible time-dependency in the hazard rate. To avoid any parametric assumptions the baseline rate is modeled by means of a dummy variable for each month since the spell begin (Base...). The first month is used as a reference level. This was carried out up to month 5. After month 5 the number of observations were too small to use monthly dummies. Month-groups were therefore constructed in such a way that the number of completed durations in each month did not become too small for estimation purposes. Considering the estimated baseline dummies we only a find significant positive effects (at a 10% level) for month four on reemployment chances.

Regarding the usual socio-demographic characteristics we only find a significant positive effect (at a 10% level) for the age 26-40 years and a negative effect for the status foreigner. No significant effects were found for a disability status. Men did not have a significant higher reemployment probability than women (Female) and variables related to family background (Female×Kids, PartHH) did not prove to have a significant impact on the chances of being re-employed either.

Education does not seem to have a significant effect on the reemployment probability (Abitur, Lehre, Diplom). A possible explanation for the insignificance of these rather formal individual skill level variables might be that employers conceive them to be preliminary screening devices. The final selection of applicants might be based on more detailed information such as interviews, evaluations from previous employers, specific grades or work related tests.

Our results reveal a significant influence of variables describing past employment history. The positive effect of the number of unemployment spells during the last three years (NoUneSp3) as well as previous employment (PrvEmployed) could be an indication of frictional unemployment with short unemployment spells and better reemployment chances. In contrast, the negative impact of the cumulated unemployment duration in the last 3 years (DurUneSp3) on reemployment probability reflects long-term and structural unemployment. The theoretical explanation for this detrimental effect might be that employers conceive the duration of unemployment as a negative screening device. A further possible explanation is that as unemployment duration increases, the decay of firm specific human capital decreases above-average. As a result reemployment chances decline. An increasing replacement ratio, defined as the relationship between the level of unemployment benefits and last labor market gross income received, has significant negative reemployment probabilities (ReplacementRatio). This stands in line with basic search theory.

Of course, the individual's reemployment probability is also affected by demand side conditions. Thus an indicator that captures a potential mismatch between labor demand and supply on regional labor markets (RegSituation) was included. Findings indicate that worse regional labor market conditions increase unemployment duration. Dummy variables to capture the typical seasonal pattern over the course of a year were also incorporated. As expected, the reemployment probabilities rise significantly during the usual spring time stimulation of the labor market (SPRING). No significant effect could be detected for the summer season (SUMMER). The DECEMBER-variable requires special attention, since it not only aims to take into account the slack demand during the winter time. The retrospective design of the employment calendar in the GSOEP leads to a disproportionately high number of spells ending in December (Hujer and Schneider, 1996). Hence the December dummy also intends to capture this so-called heaping effect at the end of the year. Considering these two influences our results indicate, that the negative seasonal effect which we would expect for the winter time is offset by described heaping effect. Torelli and Trivellato (1993) criticize this kind of correction for heaped responses as it does not intend to correct for the true underlying spell duration. In principle this argument seems reasonable. A study by Kraus and Steiner (1998) for the GSOEP evaluates different heaping adjustment procedures and shows that different ways of incorporating heaping effects hardly affect the coefficients of the explanatory variables. Kraus and Steiner compare the specification applied in this study with one that explicitly derives an empirical heaping function through a comparison between GSOEP data and data published by the Federal Labor Office. As the authors conclude, a rough procedure such as the one applied in our study does "not lead to any important differences in estimation results and has the great advantage of facilitating estimations of more complicated duration models."

< Table C.1 about here >

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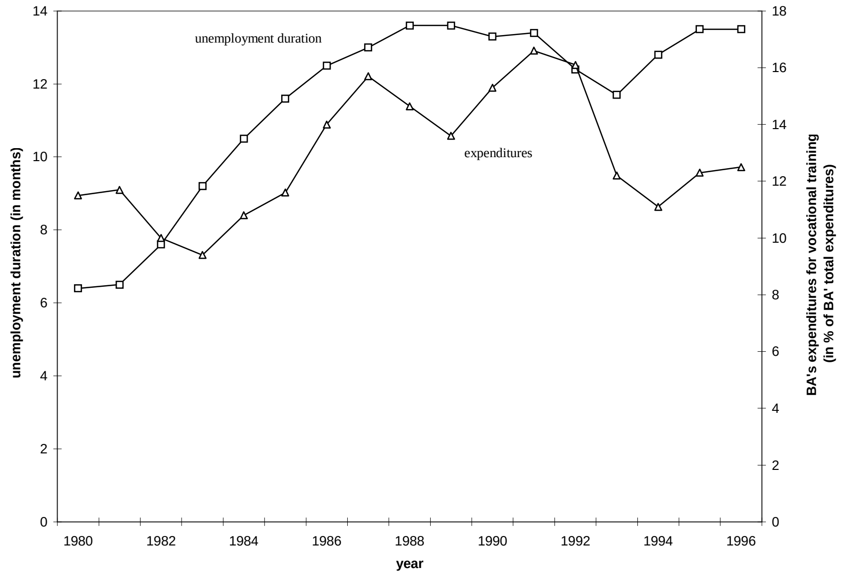
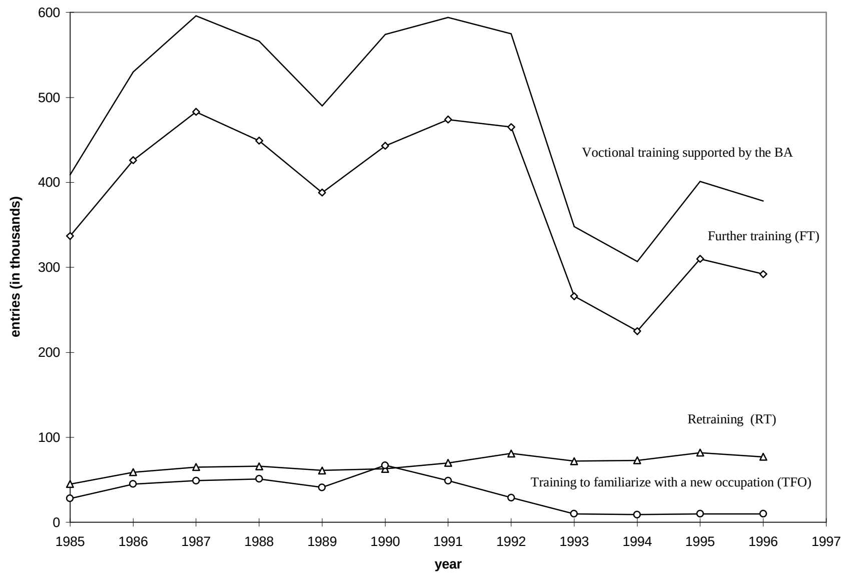
Fig. 1: Average unemployment duration and proportion of BA's expenditures for vocational training as a percentage of BA's total expenditures (West Germany, 1980-1996)



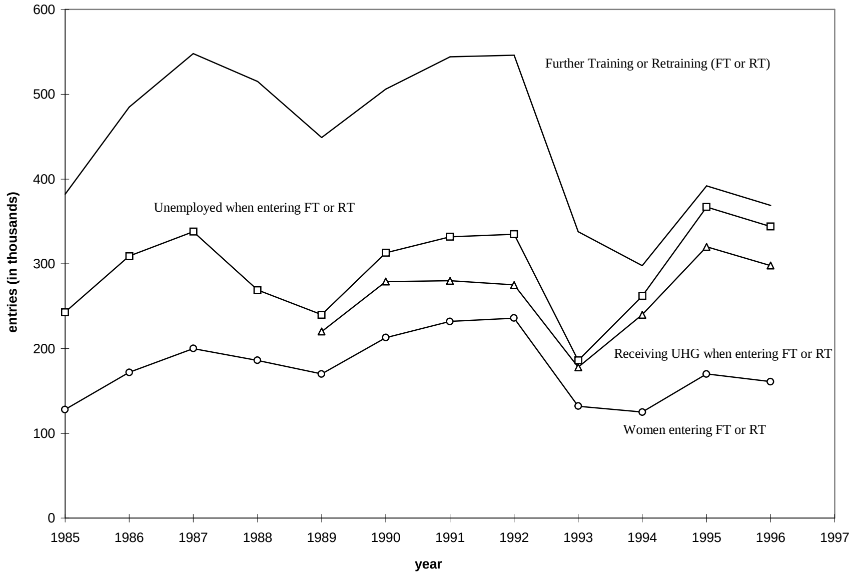
Sources: BA (1988, 1993, 1995, 1996b, 1997a,b).

Fig. 2: Entries into different types of vocational training supported by the BA (West Germany, 1985-1996)



Sources: BA (1990, 1992, 1995, 1996a, 1997a).

Fig. 3: Entries of women, previously unemployed individuals, and UHG recipients in FT or RT



Sources: BA (1990, 1992, 1995, 1996a, 1997a). Note that UHG here only covers “Großens UHG“.

Fig. 4: Share of unemployed PSVT participants before and after PSVT.

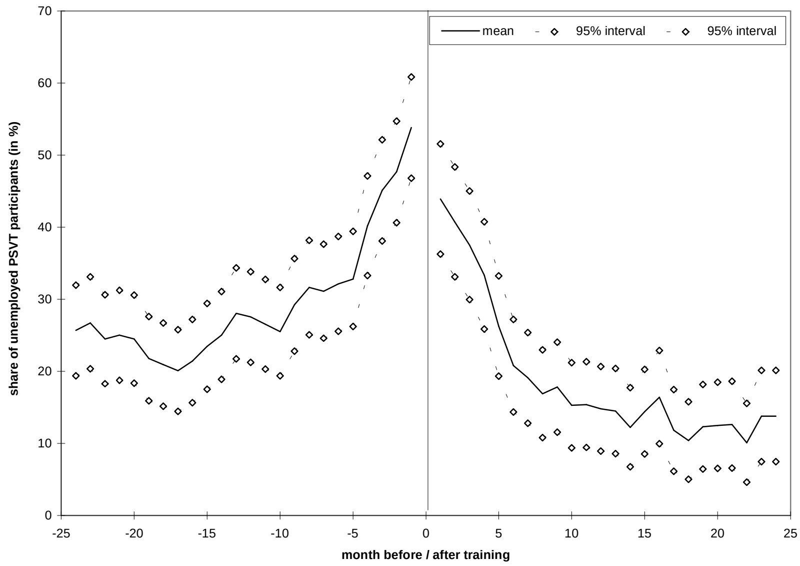
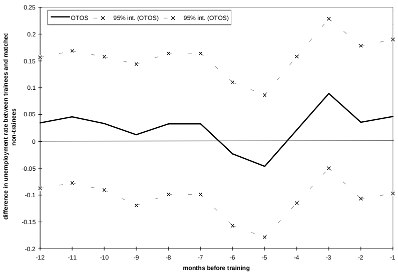
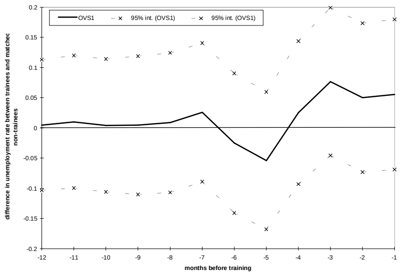


Fig. 5: Difference in pre-training unemployment between trainees and OTOS-, OVS1-, OVS2-, OVS3-, OVS4- and random non-trainees.

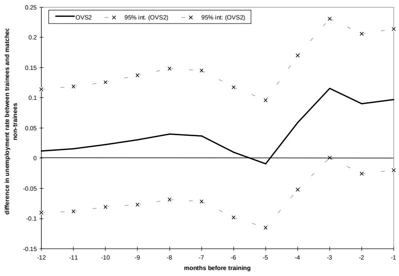
Trainees vs. OTOS-non-trainees



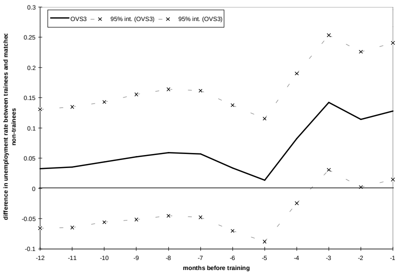
Trainees vs. OVS1-non-trainees



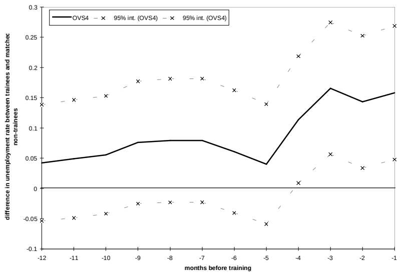
Trainees vs. OVS2-non-trainees



Trainees vs. OVS3-non-trainees



Trainees vs. OVS4-non-trainees



Trainees vs. all non-trainees

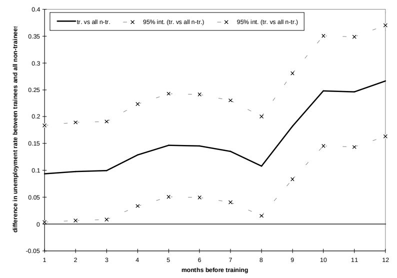
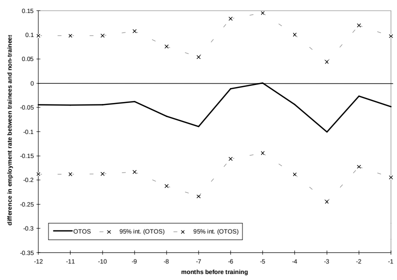
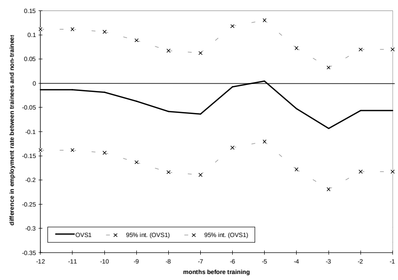


Fig. 6: Difference in pre-training employment between trainees and OTOS-, OVS1-, OVS2-, OVS3-, OVS4- and random non-trainees.

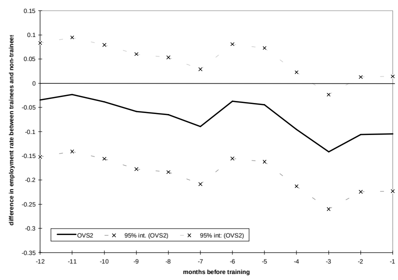
Trainees vs. OTOS-non-trainees



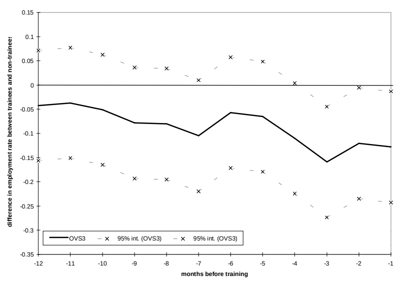
Trainees vs. OVS1-non-trainees



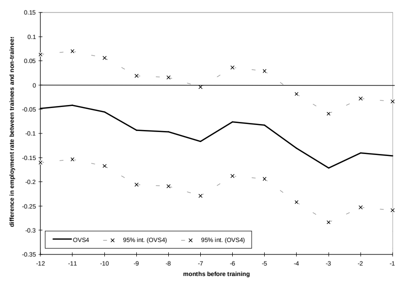
Trainees vs. OVS2-non-trainees



Trainees vs. OVS3-non-trainees



Trainees vs. OVS4-non-trainees



Trainees vs. all non-trainees

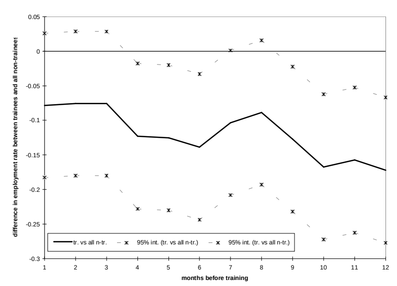


Fig. 7: Simulated survivor functions and difference in survivor functions for a trainee in a short term PSVT course vs. a control

Survivor functions | Difference in survivor functions

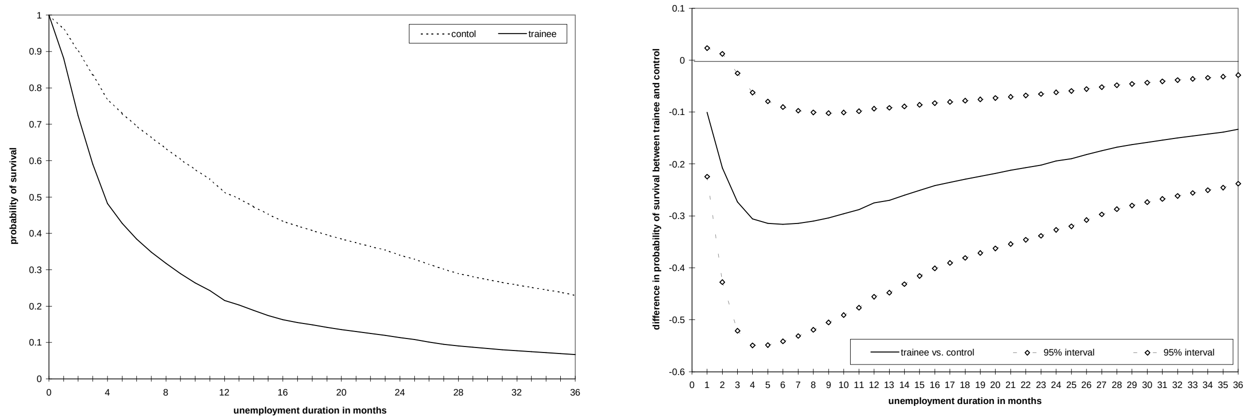


Fig. 8: Simulated survivor functions and difference in survivor functions for a trainee in a short term PSVT course vs. a trainee in a long term PSVT course

Survivor functions | Difference in survivor functions

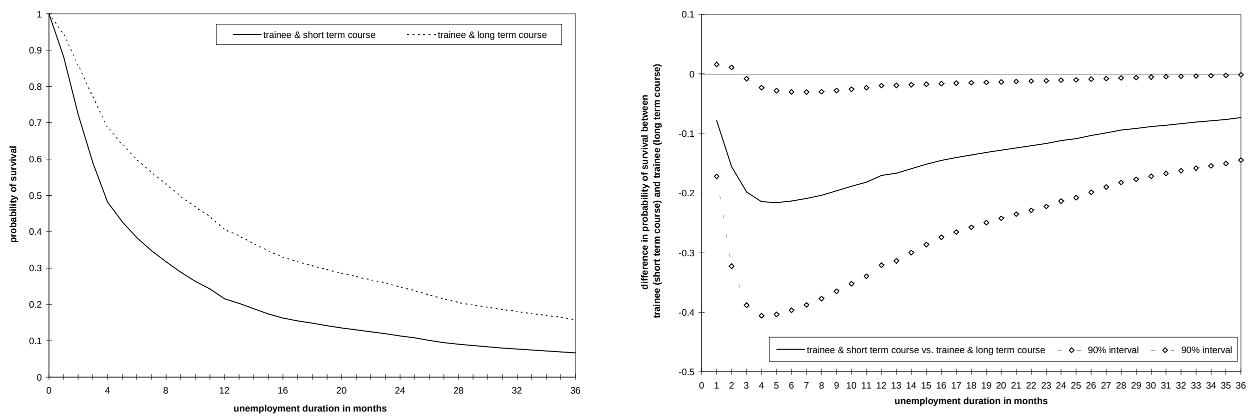
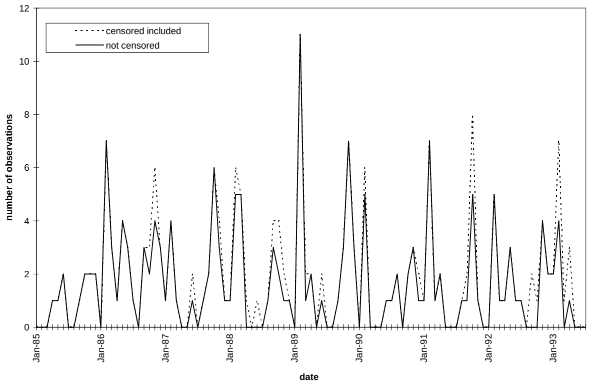


Fig. A.1: Distribution of PSVT start and end dates

PSVT start dates



PSVT end dates

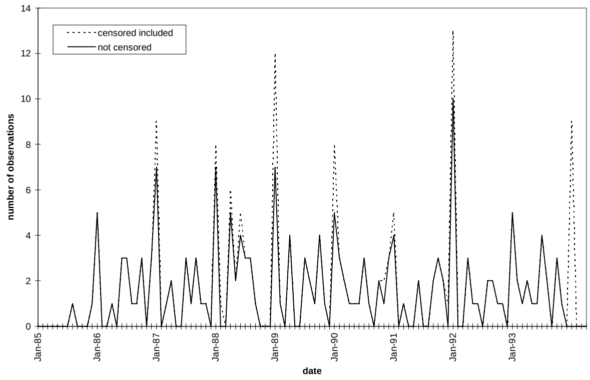
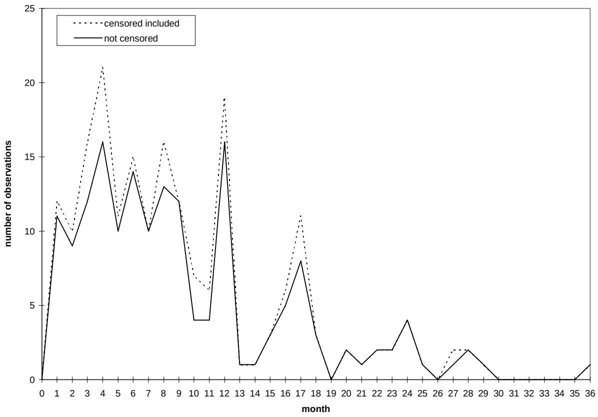


Fig. A.2: Distribution of PSVT durations and the empirical distribution function of PSVT durations

Distribution of PSVT durations



Empirical distribution function of PSVT durations

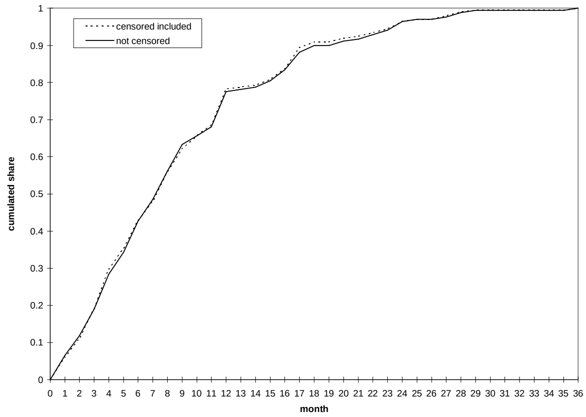
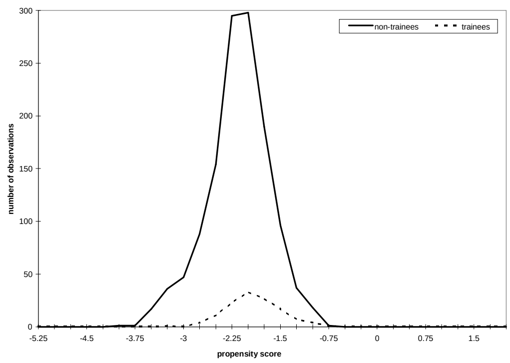
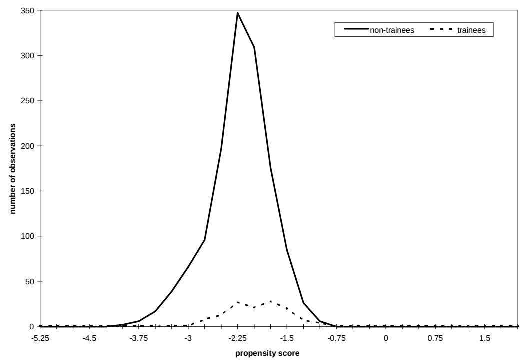


Fig. B.1: Distribution of the propensity scores for the trainee and non-trainee group in 1985-1992

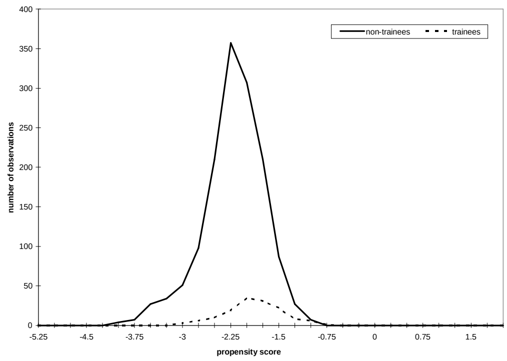
1985



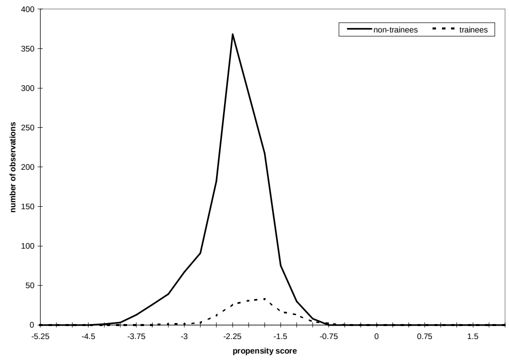
1986



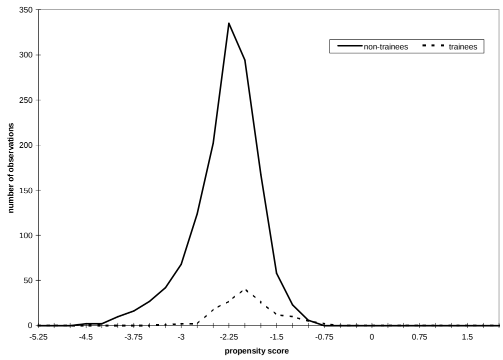
1987



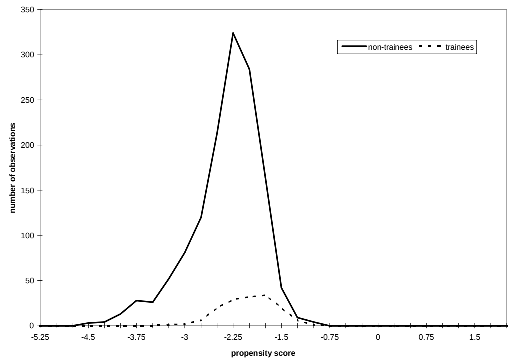
1988



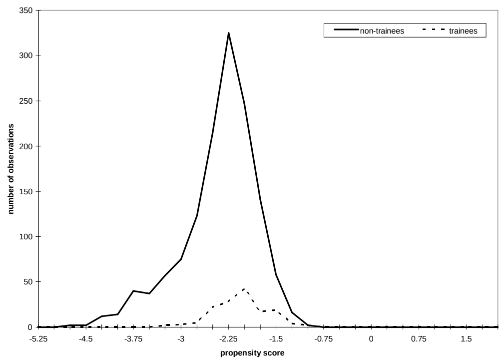
1989



1990



1991



1992

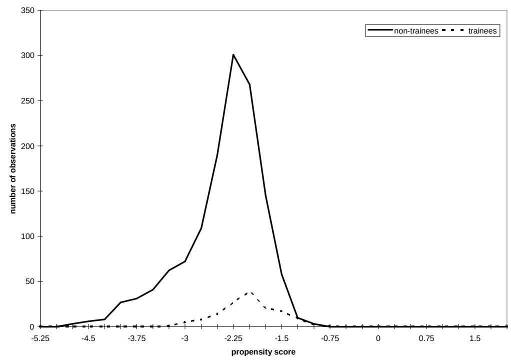


Table 1: Entry and exit states of PSVT courses

Entry from \ Exit into	Employment	Unemployment	Sum
Employment	37 (61%)	24 (39%)	61 (100%)
Unemployment	36 (45%)	44 (55%)	80 (100%)
Sum	73	68	141

Note: For the remaining 57 courses the corresponding trainees were not observed either to enter PSVT as employed or unemployed or to leave PSVT as employed or unemployed.

Table 2: Means and shares for selected socio-economic characteristics for trainees vs. non-trainees in 1989

Characteristics	Non-trainees (1376 individuals)	Trainees (146 individuals)
Socio-demographic characteristics (1989)		
Age (years)	32.2s	29.5
Male (%)	52	56
Foreigner (%)	38	19
Formal education characteristics (1989)		
High school degree (%)	13	15
Apprenticeship (%)	51	67
University degree (%)	5	8
Characteristics related to employment (1989)		
Employed (%)	68	62
Job tenure (years)	2.7	2.0

Note: Trainees (non-trainees) are those individuals who are observed in 1989 and who participated in PSVT at least once (did not participate in PSVT) during the time span 1985-1993.

Table 3: Mean duration of unemployment spells (1985-93) for trainees vs. non-trainees

	Non-trainees	Trainees
Excluding right censored spells	6.24 months (1987 unemployment spells)	6.98 months (310 unemployment spells)
Including right censored spells	8.19 months (3010 unemployment spells)	7.72 months (377 unemployment spells)

Note: Trainees (non-trainees) are those individuals who participated in PSVT at least once (did not participate in PSVT) during the time span 1985-1993.

Table 4: Results for the estimation of the propensity score of PSVT: GMM estimation of an unbalanced panel probit (1985-1992)

Variable	Coefficient	Standard error
Intercept	-3.86645**	0.53602
Age/10	1.41192**	0.29294
$(Age/10)^2$	-0.23378**	0.04570
Female	0.10773*	0.05551
Female×KS	-0.15012	0.10730
Female×KM	-0.19026	0.15495
Female×KL	-0.15780	0.14641
Foreigner	-0.28692**	0.08067
Disabled	0.17804**	0.08086
PartHH	-0.10259*	0.05547
Abitur	-0.23402**	0.10655
Lehre	0.20205**	0.05711
Diplom	0.15007	0.14762
SatisLife	-0.07340**	0.01014
FutEmpDes	0.53175**	0.26263
FutPartTime	-0.37862**	0.13253
Unemployed	0.09808	0.08712
Employed	0.33269	0.28844
WhiCollar	0.09589	0.12434
BlueCollar	0.03248	0.11797
JobTenure	-0.03539**	0.01715
JobEduc	-0.36376**	0.10634
Unm→Emp	-0.14992	0.09125
OLF→Emp	-0.17353	0.15647
Emp→Unm	-0.14473	0.09959
Unm→Unm	0.06997	0.09798
OLF→Unm	-0.09848	0.15557
Emp→OLF	0.10337	0.10875
Unm→OLF	-0.07917	0.15498
OLF→OLF	-0.31847**	0.15671
RegSituation	0.00660**	0.00303
Wald tests of joint restrictions	χ^2	p-val.
H_0: coefficients of labor force status pattern = 0 $(\chi^2(7))$	17.291	1.562
H_0: all slope coefficients = 0 $(\chi^2(29))$	257.422	0.000
McKelvey-Zavoina R^2	0.220	
Cross-validated k	843	
Number of individuals	2013	

Note: ** denotes significance at a 5% level.
* denotes significance at a 10% level.

Table 5: Comparison between trainees, random non-trainees, controls from the four oversampling procedures (OVS1, OVS2, OVS3, OVS4) and from the one to one sampling (OTOS) for selected characteristics in the month of the questionnaire prior to PVST

	trainees (89)	matched non-trainees					all (random) non-trainees (1851)
		OTOS (89)	OVS1 (177)	OVS2 (263)	OVS3 (349)	OVS4 (433)	
Variable	mean, share in %	mean, share in %	mean, share in %	mean, share in %	mean, share in %	mean, share in %	mean, share in %
$z'\hat{\beta}$	-2.01	-2.03	-2.03	-2.04	-2.05	-2.05	-2.36**
Foreigner	16.9	22.5	22.6	23.2	24.1	23.6	36.8**
Age/10	2.8	2.8	2.9	2.9	2.9	2.8	3.0**
PartHH	43.8	46.1	49.8	49.1	51.9	52.0	56.3**
SatisLife	7.3	7.1	7.1	7.2	7.2	7.2	8.0**
FutEmpDes	43.8	40.6	42.5	41.5	40.4	39.3	30.1**
Unemployed	32.7	31.7	32.0	32.0	29.4	27.8	17.4**
BlueCollar	24.7	28.1	27.4	26.6	29.8	30.9	35.0**
JobTenure	1.4	1.4	1.5	1.4	1.4	1.6	2.4**
Median of absolute standardized differences (MedSD)	-	11.0	11.4	20.0	24.7	28.5	49.8
Mean of absolute standardized differences (MeaSD)	-	10.5	10.8	18.2	22.6	26.8	50.6

Note: ** denotes a significant difference in sample means at a 5% level.
The absolute standardized difference in percent is $100 \cdot |\bar{x}_i - \bar{x}_{(i)}| / ((s_i^2 + s_{(i)}^2)/2)^{1/2}$, where for each variable, $\bar{x}_i$ (s_i^2) and $\bar{x}_{(i)}$ $(s_{(i)}^2)$ are the means (variances) in the trainee and matched (or random) non-trainee group. The median and mean are taken with respect to the variables used in the matching procedure, i.e. the propensity score, foreigner and the variables that are used to control for the monthly pre-training employment history (see appendix B).

Table 6: Results for composed PSVT effects on transition unemployment ⇒ employment

Coefficient (Standard error)	PSVT course duration		
	Short term course	Medium term course	Long term course
The unemployment spells begins			
– in the first month after PSVT ends	0.8952*	0.4992	0.0799
	(0.4627)	(0.4464)	(0.3060)
– within 2 to 24 months after PSVT ends	1.2152**	0.8192**	0.3999
	(0.4991)	(0.3726)	(0.3499)
– no earlier than 24 months after PSVT ends	-0.0387	-0.4347	-0.8540*
	(0.4855)	(0.5075)	(0.4668)

Note: ** denotes significance at a 5% level.
* denotes significance at a 10% level.
Short term course refers to a course duration of no longer than 3 months, medium term course to a course duration of 4 to 6 months and long term course to a course duration of more than 6 months.

Table A.1: Definition of Variables

Variable	Description
Training variables	
TR	1 if the individual participated in a PSVT course.
TR_2-24	1 if the PSVT course ended earlier than one month and no later than twenty four months prior to the unemployment spell begin.
TR_25+	1 if the PSVT course ended later than twenty four months prior to the unemployment spell begin.
— reference category: if the PSVT course ended in the month prior to the unemployment spell begin.	
TR_Dur4-6	1 if the PSVT course lasted longer than three but no longer than six months.
TR_Dur7+	1 if the PSVT course lasted longer than six months.
— reference category: if the PSVT course lasted no longer than three months.	
Baseline dummy variables — reference category is first month of spell duration	
Basexx	1 if current month is month xx since spell begin.
Basexx-yy	1 if current month is one of the months xx to yy since spell begin.
Basexx+	1 if current month is month xx or higher since spell begin.
Seasonal variables	
Spring	1 if current month is February, March or April.
Summer	1 if current month is June or July.
December	1 if current month is December.
Macroeconomic and regional labor market indicator	
RegSituation	Defined as the quotient between the number of unemployed and vacancies in the state in which the individual has his place of residence.
Age variables	
Age/10	Age divided by 10.
$(Age/10)^2$	Age squared and divided by 100.
Age dummy variables — reference category is 41 years or older.	
Age –25yrs	1 if individual is 25 years or younger.
Age 26–40yrs	1 if individual is 26 years or older, but younger than 41.
Other socio-demographic variables	
Female	1 if individual is female.
Female×KS	1 if individual is female and has children aged up to 6 years.
Female×KM	1 if individual is female and has children aged 7 to 10 years.
Female×KL	1 if individual is female and has children aged 11 to 15 years.
Female×Kids	1 if individual is female and has children aged up to 15.
Foreigner	1 if individual is not a German.
PartHH	1 if individual is married or living together with his/her partner.
Disabled	1 if individual is disabled.
Abitur	1 if individual has Abitur or Fachhochschulreife (comp. to high school degree).
Lehre	1 if individual has completed an apprenticeship.
Diplom	1 if individual has a university degree or a degree from a Fachhochschule.
SatisLife	Satisfaction with life in general (0 = totally dissatisfied; 10 = totally satisfied).

Table A.1: Definition of Variables (contd.)

Variables related to current employment status	
Unemployed	1 if individual is currently unemployed.
Employed	1 if individual is currently employed.
Occupational Status — reference category are apprentices and self-employed.	
WhiCollar	1 if individual is currently employed and has white collar status.
BluCollar	1 if individual is currently employed and has blue collar status.
JobTenure	years of affiliation with current employer.
JobEduc	1 if individual is working in the occupation he/she was originally educated for.
Variables related to the labor force status pattern: They are defined by looking backward in time starting in the month of the questionnaire and ending eleven months earlier. The first status is the most recent prior status within the indicated time period. Thus Emp→Unm refers to an individual who was unemployed at the month of the questionnaire but whose most recent labor force status during the preceding eleven month was employment. Repeated pattern such as Unm→Unm indicate that the individual had the same labor force status in the month of the questionnaire and in all of the elven preceding months. The reference category is Emp→Emp (if individual remained employed for all twelve month).	
Unm→Emp	1 if individual switched from unemployment to employment.
OLF→Emp	1 if individual switched from out of the labor force to employment.
Emp→Unm	1 if individual switched from employment to unemployment.
Unm→Unm	1 if individual remained unemployed for all twelve months.
OLF→Unm	1 if individual switched from out of the labor force to unemployment.
Emp→OLF	1 if individual switched from employment to out of the labor force.
Unm→OLF	1 if individual switched from unemployment to out of the labor force.
OLF→OLF	1 if individual remained out of the labor force for all twelve months.
Variables related to future plans regarding employment	
FutEmpDes	1 if individual is currently not employed but wishes to be employed in the future.
FutPartTime	1 if individual is currently not employed but wishes to be employed in the future and is looking for a part time employment.
Variables related to employment history	
NoUneSp3	number of unemployment spells during the last three years (measured from spell begin).
DurUneSp3	cumulated number of unemployment months during the last three years (measured from spell begin and divided by 12).
PrvEmployed	1 if individual was previously, i.e. prior to the unemployment spell, employed.
ReplacementRatio	Level of unemployment benefits in relation to the last labor market gross income.

Table A.2: Number of PSVT courses per individual

number of PSVT courses	absolute frequency	relative frequency	cumulated relative frequency
1	124	0.765	0.765
2	31	0.191	0.956
3	7	0.043	1

Table B.1: Comparison between trainees, OVS2-non-trainees and all (random) non-trainees for all characteristics in the month of the questionnaire prior to PSVT

	Trainees (89)	Matched OVS2-non-trainees (177)	All non-trainees (1851)
Variable	mean, share in %	mean, share in %	mean, share in %
$z'\hat{\beta}$	-2.01	-2.03	-2.36**
Female	43.8	45.8	48.8
Female×KS	5.6	9.0	10.2*
Female×KM	3.4	5.1	4.6
Female×KL	4.5	6.2	7.6
Foreigner	16.9	22.6	36.8**
Age/10	2.8	2.9	3.0**
$(\text{Age}/10)^2$	8.6	9.2	10.4**
Disabled	5.6	2.3	5.0
PartHH	43.8	49.8	56.3**
Abitur	17.1	10.2	12.6
Lehre	55.3	55.6	47.3
Diplom	6.9	3.4	4.4
SatisLife	7.3	7.1	8.0**
FutEmpDes	43.8	42.5	30.1**
FutPartTime	6.7	5.1	5.3
Unemployed	32.7	32.0	17.4**
Employed	55.1	54.2	63.6
WhiCollar	21.3	18.7	17.7
BlueCollar	24.7	27.4	35.0**
JobTenure	1.4	1.5	2.4**
JobEduc	17.8	12.7	19.6
Unm→Emp	14.1	7.9	10.3
OLF→Emp	2.5	7.1*	7.2**
Emp→Unm	17.3	14.0	7.9**
Unm→Unm	10.3	11.5	4.6*
OLF→Unm	3.5	5.7	2.0
Emp→OLF	6.9	3.0	3.7
Unm→OLF	1.2	1.8	2.3
OLF→OLF	7.5	6.9	14.8**
RegSituation	14.5	13.6	12.8*

Note: ** denotes a significant difference in sample means at a 5% level.
* denotes a significant difference in sample means at a 10% level.

Table C1: Results for transition unemployment ⇒ employment: Maximum likelihood estimation of a discrete hazard rate model with unobserved heterogeneity

Variable	Coefficient	Standard error
Constant	-2.7642**	0.5541
Base02	0.2501	0.2559
Base03	0.4432	0.2997
Base04	0.5991*	0.3284
Base05	0.5062	0.3815
Base0608	0.4729	0.3940
Base09+	0.5988	0.5184
Age -25yrs	0.5147	0.3313
Age 26-40yrs	0.5073*	0.2877
Female	0.2967	0.2304
Female×Kids	-0.5241	0.3539
Foreigner	-0.7802**	0.2959
Disabled	-0.6704	0.8524
PartHH	-0.0205	0.2024
Abitur	0.3728	0.3238
Lehre	0.0594	0.2166
Diplom	-0.0894	0.4695
PrvEmployed	0.9983**	0.3377
NoUneSp3	0.4920**	0.1460
DurUneSp3	-0.8092**	0.2341
ReplacementRatio	-0.7934**	0.3467
RegSituation	-0.0742**	0.0221
December	0.4316*	0.2305
Spring	0.3126*	0.1847
Summer	-0.0026	0.2091
TR	0.8952*	0.4627
TR_Dur4-6	-0.3960	0.5134
TR_Dur7+	-0.8153*	0.4483
TR_2-24	0.3200	0.3867
TR_25+	-0.9339*	0.4975
Ln(σ^2)	-0.2598	0.4354
Likelihood Ratio test of joint restrictionst	χ^2	p-val.
H_0: all coefficients except intercept and base line = 0 $\left(\chi^2(24)\right)$	109.509	0.000
Log-Likelihood	-718.2883	
Number of spells	365	

Note: ** denotes significance at a 5% level.
* denotes significance at a 10% level.