The Impact of Training on Unemployment Duration in West Germany

- Combining a Discrete Hazard Rate Model with Matching Techniques -

by

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ABSTRACT

In this study we are concerned with the impact of vocational training on the individual's unemployment duration in West Germany. The data basis used is the German Socio-Economic Panel (GSOEP) for the period from 1984 to 1994. To resolve the intriguing sample selection problem, i.e. to find an adequate control group for the group of trainees, we employ matching methods which were developed in the statistical literature. These matching methods uses as the main matching variable the individual propensity score to participate in training, which is obtained by estimating a random effects probit model. On the basis of the matched sample a discrete time hazard rate model is utilized to assess the impact of vocational training on unemployment duration. Our results indicate, that training significantly raises the transition rate of unemployed into employment in the short but not in the long run.

Keywords: discrete hazard models, selection bias, matching techniques

JEL classification: C40, J20, J64

1. Introduction[1]

In recent years the labor market policy debates in Germany regarding the value of training programs have become increasingly controversial. On one hand the persistently high level of unemployment rates stresses the necessity of training programs in helping unemployed former participants to find jobs faster as well as employed participants to retain their jobs longer. The main hypotheses are that participation in training helps to reduce the human capital decay, improves the job search skills during unemployment and strengthens work habits during employment. On the other hand, it is often doubted whether the positive effects of training programs outweigh their costs. Moreover, advocates of a pessimistic perspective even argue that participation in training programs during episodes of high unemployment may be seen as a negative screening factor for some employers and may therefore have a negative impact on the individual's employment performance.

In order to justify or reject training as a helpful part of active labor market policy, empirical studies investigate the effectiveness of training programs on different individual outcomes. Typically such outcomes are employment or unemployment rates, earnings, wages and duration of employment or unemployment spells. Since the effect of training on earnings is primarily 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. Our study concentrates especially on the question whether former trainees manage to find jobs faster when becoming unemployed. Hence, the outcome of interest is the individual duration of unemployment.

To evaluate the causal effect of training programs 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. When choosing or constructing this control group, different adjustment procedures are applied to ensure that trainees and controls are identical with respect to all relevant characteristics except for not having obtained training. In experimental evaluations, the construction of an adequate control group is done by means of randomization at the level of data collection. In studies where non-experimental data are used, different econometric adjusting or statistical matching procedures are applied to artificially construct or find an adequate control group. Failure to adjust for discrepancies between trainees and controls may lead to substantially biased judgments about the

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effect of the training program. Such a bias is known in econometric literature as *sample selection bias*.

The fact that existing empirical studies for Germany on the effects of training programs vary with respect to the outcome they measure, the types of training they evaluate, the econometric or statistical methods they apply and the data they use, makes a comparison difficult.[2]

HUJER/SCHNEIDER (1990) analyze the effects of training on the reemployment probabilities of unemployed in West Germany using the first four waves of the German Socio-economic Panel (GSOEP) covering the time period form 1983 to 1986. They apply a continuous time duration model that accounts for unobserved heterogeneity. The estimated results indicate a significant positive short run effect of training whereas a long run effect of training was not detected.

BECKER (1991) examines the impact of vocational training programs on changes on career ladders. The sample on which his estimations are based on consists of West German people who belong to three different cohorts and was drawn from data of the German Life History Study (MAYER/BRÜCKNER (1989)). The author applies a continuous time duration model to estimate independently the effects of training on up- and downward changes on the career ladder. The results imply, that successful training programs, i.e. if a certification was assigned, raise the promotion probabilities significantly.

For the case of East Germany HÜBLER (1994) examines the interactions between participation in training and job search activities as well as possible impacts of these activities on working time. The estimation is based on the first four waves of the Labor Market Monitor for East Germany from 1990 to 1991. To address the first question, the author uses a bivariate probit model and with respect to the second question he introduces an additional equation for hours of work, thereby extending the model to a selection model with doubled endogenous switch. The results obtained for the first model point to a causal effect of training activities on search activities but not vice versa. Whereas training within a firm reduces the search activities of women it has no significant effect on search activities of men, while on the other hand training outside of the firm has no significant effect for both, men and women. Finally, the results of the selection model indicate that training activities reduce the hours of work.

BÜCHEL/PANNENBERG (1994) and PANNENBERG (1995) are concerned with the relation between on-the-job training, firm-internal career ladders and income changes in West Germany using data from the GSOEP from 1984 to 1991. The relation of on-the-job training and movements on internal career ladders is estimated by means of a bivariate probit model. To estimate the effect of on-the-job training on earnings a fixed effect model was utilized. The results indicate that on-the-job training

[2] For a wider overview of empirical studies related to the impact of training, see BJÖRKLUND (1989), DOLTON (1994) and HUJER/MAURER/WELLNER (1996)

has positive effects on the promotion probability as well as on earnings. The study of PANNENBERG (1995) additionally finds positive on-the-job training effects on reemployment prospects when using a discrete time duration model.

STEINER/KRAUS (1995) are interested in the effects of subsidized temporary jobs (Arbeitsbeschaffungsmaßnahmen, ABM) on re-employment probabilities in East Germany. Based on the first six waves of the Labor Market Monitor form 1990 to 1992 they separately evaluate the re-employment chances of unemployed and participants of ABM using a discrete time duration model that allows for unobserved heterogeneity. Simulations based on a comparison of the estimated transition rate into employment for "reference" individuals of both groups indicate nearly no effect of ABM for men and even a negative impact for women. However the authors state that these results depend heavily on the selected reference individuals and therefore emphasize the sample selection problem typically involved in treatment estimations.

The study of PISCHKE (1994) for West Germany using data of the GSOEP from 1986 to 1989 evaluates the impact of on-the-job-training on wages. Using a fixed effect model the author finds no not noticeable wage gains due to training.

A crucial problem of all studies mentioned so far is that they do to explicitly address the selection effects that plague every empirical study that uses nonexperimental data. In this respect the studies of FITZENBERGER/PREY (1995, 1996) and LECHNER (1995, 1996A, 1996B) differ. Although they suggest two different estimation strategies, they have in common the strong focus on the correction of possible selection effects.

FITZENBERGER/PREY (1995) explore the effects of training on employment probability in East Germany. The latest study, FITZENBERGER/PREY (1996), additionally incorporates the training effects on hourly wages. In the first study the authors use the first six waves of the Labor Market Monitor covering the time span 1990 to 1992 and in the later they use five waves form 1990 to 1994. They distinguish between training within the firm, in an external institution and whether training was subsidized by the public sector, or not. In order to account for possible sample selection effects and panel attrition they utilize a dynamic simultaneous random effects model consisting of an employment, wage, qualification, attrition and initial condition equation. To identify the presence of any remaining selection effects before training the authors rely on a pre-program test proposed by HECKMAN/HOTZ (1989). Unfortunately the applied test procedure indicates that their model is only successful in taking account of selection effect for some qualification measures. In this respect, the authors find that training in an external institution tends to have positive effects on employment if it is financed by the public sector and on wages otherwise. Training programs which are provided inside of the firm seem to have a positive impact on wages, but no impact on employment.

In contrast to the studies presented so far, all relying on econometric models, LECHNER (1995, 1996A, 1996B) applies an approach which was developed in the statistical literature and is often referred to as the "Rubin Causal Model" or the "potential outcomes approach" (HOLLAND (1986)).

This approach has a close resemblance to experimental evaluations since it tries to extend the ideas of the experimental framework to a non-experimental context. To obtain an adequate control group, the author applies a statistical matching algorithm, that selects those from the group of non-trainees, who are most similar to the trainees with respect to the estimated propensity to participate in training as well as pre-training employment status. Based on such a sample LECHNER (1995, 1996A, 1996B) then estimates the training effects as the difference between various post-training outcomes such as unemployment, employment rates and earnings of trainees and matched controls. All three studies of LECHNER use the first five waves of the GSOEP for East Germany from 1990 to 1994. However, they focus on different types of training. LECHNER (1995) examines off-the-job training and finds no positive employment or income effects. The results of LECHNER (1996A) point to a non-existing training impact of public sponsored training programs. With respect to on-the-job training, LECHNER (1996B) detects large positive income but no employment effects.

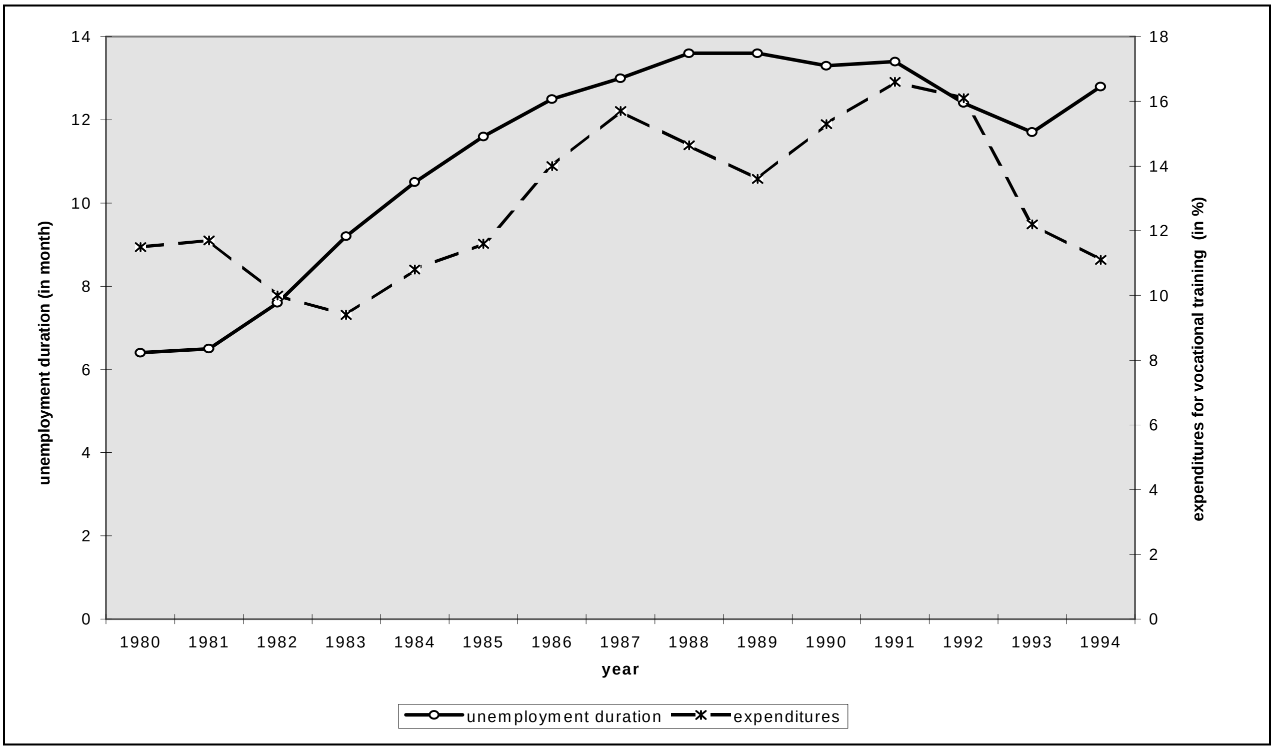
We are concerned with the impact of training, in particular vocational training, on individuals' unemployment duration in West Germany. The data basis used in this study is the GSOEP for the period from 1984 to 1994. To resolve the already mentioned sample selection problem, i.e. to find an adequate control group, we - like LECHNER (1995, 1996A, 1996B) - employ matching methods proposed by ROSENBAUM/RUBIN (1985) and RUBIN (1991). These matching methods uses as the main matching variable the individual propensity to participate in training, obtained by estimating a random effects probit model, and additionally incorporates monthly pre-training employment status to account for transitory shocks just prior to training as well as information on regional labor market conditions to include potential demand side effects. Using the resulting matched sample we then however emphasize the necessity to estimate a discrete time hazard rate model when evaluating the impact of vocational training on unemployment duration. This seems necessary because a simple comparison of post-training mean unemployment duration of trainees and matched controls 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. Our results draw a positive picture of training in the sense, that training raises the transition rate of unemployed into employment in the short as well as in the long run.

The paper proceeds as follows. The next chapter presents some features and developments of the West German labor market before and during the time span considered in our evaluations. Chapter 3 describes the data base and presents descriptive statistics for several characteristics of the chosen sample. Chapter 4 is devoted to the construction of an adequate control group. First we give a short theoretical outline of the evaluation problem and the strategy we apply to deal with it. We then turn to the estimation of propensity score which is followed by the application of the matching methods. Chapter 5 deals with the outcome of interest, i.e. the impact of vocational training on the transition from unemployment to employment. Therefore we introduce a discrete time hazard rate model. On the basis the of sample constructed in chapter 4, we use the hazard rate model to evaluate the impact of vocational training. Chapter 6 summarizes the main findings.

2. The Labor Market in West Germany: Some Stylized Facts

Figure 1 shows the development of the average unemployment duration before and during the period under investigation in West Germany. It emphasizes on the one hand 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.

Figure 1:
Average Unemployment Duration and Portion of BA's Expenditures for Vocational Training and Retraining in West Germany of BA's Total Expenditures

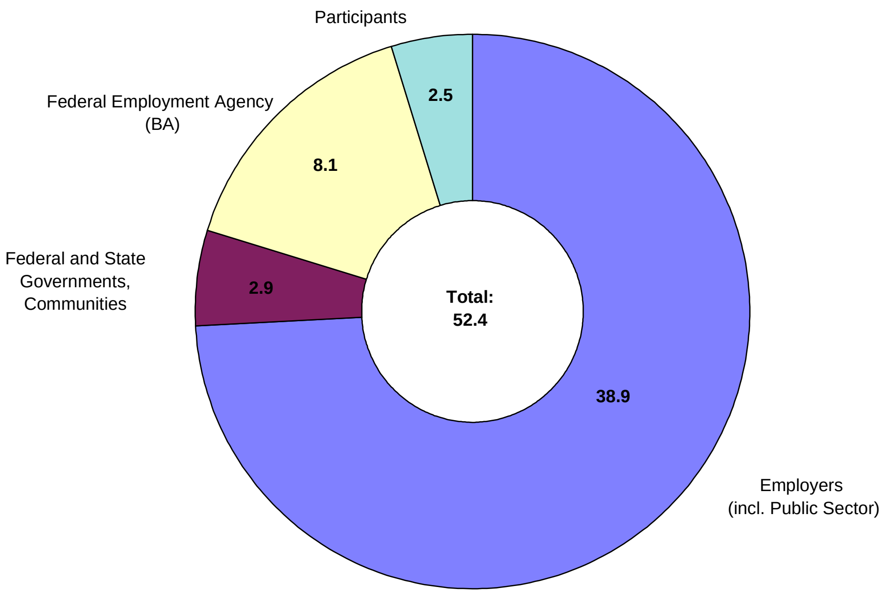


Source: BUNDESANSTALT FÜR ARBEIT: Berufliche Weiterbildung (1988, 1993, 1995); Amtliche Nachrichten der Bundesanstalt fur Arbeit (1996)

One central instrument of active labor market policy in Germany, regulated by the Work Support Act ("Arbeitsmarkförderungsgesetz"), is the support of vocational training ("Förderung der beruflichen Ausbildung"), and further vocational training and retraining ("Fortbildung und Umschulung") especially by the Federal Labor Office ("Bundesanstalt für Arbeit", BA). Either the BA itself provides training or, as in a larger number of cases, financial support is given for those currently unemployed or in danger of becoming unemployed. *Figure* 1 also depicts the development of the portion of BA's expenditures for vocational training and retraining in West Germany on BA's total expenditures. This is clearly an indicator for 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 form 1983 to 1987. It is followed by a rather substantial decline after 1991 to a level of 11.1% which approximately equals that of 1980. This decline is also a result of the reunification, since the BA had to support large labor market programs in East Germany.

Figure 2 contains the structure of the total volume of expenditures for training in West Germany in 1988.

Figure 2: Training Expenditures in West Germany 1988 (billion DM)



Source: INSTITUTS DER DEUTSCHEN WIRTSCHAFT (1990)

Apparently the Federal Labor Office is far from being the most important patron of vocational training in Germany. 74.2% of total training expenditures in West Germany in 1988 were paid by employers (including those in the public sector). Estimates by ALT/SAUTER/TILLMANN (1994) for 1991, which, however, also include East Germany, indicate that the public sector contributes a fifth of this share. The 2.9 billion DM financed by federal respectively state governments and communities include items like expenditures for adult education centers (“Volkshochschulen“). More than half of the costs paid by the participants themselves cover travel costs and costs for learning materials (ALT/SAUTER/TILLMANN (1994)). All in all more than 50 billion DM or 2.4% of the gross national product were spent for training in 1988.

3. The Data Basis

The sample used for the analysis is drawn from the German Socio-Economic Panel, a representative sample of the resident population. Starting in 1984, about 12000 individuals aged above 16 and belonging to nearly 6000 households were interviewed on a yearly basis about subjects such as employment status, personal characteristics, education, various types of income etc.. From 1990 on, 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/BURKHAUSER/BEHRINGER (1993).

To generate the individual's unemployment duration, we rely on the retrospective monthly "calendar" that gathers detailed information on 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, registered unemployed, education, etc.. Information on participation in training comes from two special surveys in 1989 and 1993, where individuals were asked about their last three *vocational* training courses that were either completed in the previous three years or are still ongoing. Hence, our analysis is limited by the corresponding time span form 1986 to 1993. However, since we also generated variables regarding employment history, we additionally utilized cross-sectional information from the years 1984 and 1985. Furthermore, information on unemployment duration in 1993 comes from the retrospective calendar in 1994.

Our selected sample consists of individuals who all had at least one unemployment spell during the time span 1986-1993 and who gave valid information in the special surveys on a possible participation at any vocational training courses. Due to methodological reasons, left censored spells have been excluded form our analysis (HUJER/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 in the retrospective calendar. Otherwise, unemployment spells are treated as right censored. This selection results in a sample of 1180 individuals who contribute 1835 unemployment spells. 720 of these spells are right censored. The average unemployment duration of all spells is 8.91 months, whereas the average duration of completed spells is 6.09 months.

The crucial problem when using non-experimental in contrast to experimental data is that trainees are not a random sample form the population of interest. The descriptive comparison between non-trainees and trainees in *table 1* reveals that this is also true for our sample. There are major differences, in characteristics such as nationality, education or employment status. On average trainees are younger, less likely foreigners, have a higher formal education level and more often working in the occupation they were educated for. Consequently, the simple comparison between mean unemployment duration for trainees and non-trainees has to be considered carefully since it is subject to potential selection effects. Furthermore 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 yields a far lower difference compared to the case where all spells are included.

Table 1: Selected Socio-economic Characteristics for Trainees vs. Non-Trainees

Variable	**non-trainees** *) (715 observations)	**trainees** *) (219 observations)
Socio-demographic characteristics (1988)		
Age	35.9 years	31.2 years
Male	54 %	52 %
Foreigner	41 %	12 %
Formal education characteristics (1988)		
Abitur	10 %	21 %
Lehre	46 %	72 %
Diplom	3 %	10 %
Characteristics related to employment (1988)		
Employed	62 %	71 %
Occupational status:		
Blue collar worker	42 %	20 %
White collar worker	13 %	38 %
Working in the occupation educated for	17 %	32 %
Average duration of unemployment spells (1986-93)		
Excluding right censored spells	6.49 months	4.97 months
Including right censored spells	9.77 months	6.11 months

*) Trainees are those who received vocational training at least once during the time span 1986-1993. Non-Trainees are those who never received vocational training during the time span 1986-1993.

4. Constructing a Control Group using Matching Methods

4.1. Evaluation Problem and Identifying Restrictions

When we aim to determine the impact of training programs on various kinds of outcomes such as employment rates or unemployment duration, it is useful to define clearly what is actually meant by impact of training. We try to do this by formulating the question of interest more precisely: *What is the average individual outcome gain of training participants compared to the situation in which they would not have participated in the training program?* Apparently, in order to answer this question one has to contrast the individuals' situation after training with the corresponding situation of the *same* individuals 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 respectively 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).[3] 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 anyone.

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 was exposed to training ($D = 1$) or not ($D = 0$). The problem of not observing the counterfactual is now documented easily by the fact, that one only has information to estimate:

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

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

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 absence of training, does not necessarily equal $E\left(Y^c \middle| D=0\right)$, i.e. observed average outcome of non-trainees. This inequality evidently arises if trainees and non-trainees differ systematically 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 on the impact of training.[4] In an experiment, individuals who are eligible training participants 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 differ systematically with respect to all relevant characteristics, except for the fact of having received training. The difference in the outcome after training is only induced by the training program itself, i.e. the impact of training is isolated and a selection bias does not exist. In formal terms the role of randomization is precisely that the potential outcomes (Y^t and Y^c) are independent of the assignment to the training program (*D*). It follows:

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

Thus, the group of non-trainees can be used as an adequate control group to estimate the training impact α.[5]

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*).

$$Y^c \perp\!\!\!\perp D \mid Z = z \qquad (4a)$$

If this assumption holds, then:

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[4] ASHENFELDER/CARD (1985), LALONDE (1986), BURTLESS/ORR (1986).

[5] Despite this advantage of an experimental approach there are, however, some general and methodological problems related to experiments which have been discussed in literature. Especially ethical arguments against the random selection process and practical difficulties to adequately conduct an experiment are most often mentioned. For a wider discussion of relative advantages of experimental and non-experimental approaches see e.g. BURTLESS/ORR (1986), BJÖRKLUND (1989) and HECKMAN/SMITH (1995) .

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

Rewriting the crucial term in (1), as:

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

and inserting equation (5a), leads to:

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

An evaluation of (7a), and thus α, on the basis of the group of non-trainees is in principal 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 problem of dimensionality, ROSENBAUM/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 conditional independence assumption extends to the use of the propensity score:

$$Y^c \perp\!\!\!\perp D \mid P(Z = z) \quad \text{(4b)}$$

This leads to:

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

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

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

The major advantage of the identifying assumption (4b) is that the estimation problem turns into a much more 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 subsection 4.2. by means of a random effects probit model. Since we aim to construct an adequate control group that does not systematically differ in the characteristics from the trainee group an attractive way to condition on the estimated propensity score is the application of matching methods proposed by ROSENBAUM/RUBIN (1985), RUBIN (1991) (Subsection 4.3.).

4.2. Estimating the Propensity Score

The estimation of the propensity scores has to deal with several problems. First we have to take into account the unbalanced nature of the sample. Secondly there is the problem, that the starting dates of the training programs vary over time among the participants. Hence, 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)). Finally one has to be careful to consider only covariates which influence the decision to participate in training (D) *as well as* the potential outcome without training (Y^c) *but* which are unaffected by the training program, i.e. exogenous. Thus the relevant covariates should be pre-training variables.

We assume that observations are missing at random and thus estimate an unbalanced random effects probit model for the eight waves with information on vocational training (1986-1993). In order to ensure that the covariates ($Z = z_{it}$) are dated prior to the beginning of the training program they refer to time *t*, while the dependent variable D_{it} is defined as the beginning of an actual training participation within the interval $(t, t+1]$. The relationship between the latent variable D_{it}^* and D_{it} is given as follows:

$$D_{it} = \begin{cases} 1 & \text{if } D_{it}^* > 0 \\ 0 & \text{otherwise} \end{cases}, \qquad i = 1, \ldots, N; \, t = 1, \ldots, T \qquad (8).$$

D_{it}^* is defined as a function of a set of relevant pre-training characteristics ($Z = z_{it}$) and a one-way error component $u_{it} = \mu_i + \varepsilon_{it}$.

$$D_{it}^* = z_{it}'\beta + u_{it} = z_{it}'\beta + \mu_i + \varepsilon_{it} \qquad (9).$$

μ_i captures an individual time-invariant effect and ε_{it} is an additional random component. Assuming $\mu_i \sim N(0, \sigma_\mu^2)$ and $\varepsilon_{it} \sim N(0, \sigma_\varepsilon^2)$ leads to the usual structure of the variance-covariance matrix of u_{it} (HSIAO (1996)). The contribution of each individual to the likelihood function can then be written as:

$$L_i = \int_{-\infty}^{\infty} \prod_{t=1}^{T} \left(\Phi_{it}\right)^{D_{it}\nu_{it}} \cdot \left(1 - \Phi_{it}\right)^{(1-D_{it})\nu_{it}} f(\mu_i) \mathrm{d}\mu_i \qquad (10).$$

$\Phi_{it} = \Phi_{it}\left(\left(z_{it}'\beta + \mu_i\right)/\sigma_\varepsilon\right)$ and $f(\mu_i)_i$ denotes the density function of μ_i. To exclude potential effects of past training programs on the actual participation we consider only the first observed training program. Thus the binary indicator ν_{it} does not only indicate whether the individual *i* is observed in wave t or not, but also whether the observed training participation is the first one or not.[6]

Our selection of potentially relevant covariates relies on theoretical hypotheses related to human capital theory and tries to be in accordance with variables suggested in other empirical studies of training participation (e.g. BLUNDELL/DEARDEN/MEGHIR (1994)).

Theory suggests that investment into human capital and hence participation in training decreases with age. Our estimation results exhibit a concave influence of age with a maximum at approximately age 31. The hypothesis that men (MALE) have better access to training due to discrimination of women could not be confirmed. We find that a foreign nationality (FOREIGNER) has a significant negative effect, a result that stands in line with hypotheses that minority groups have a poor access to training programs. A negative effect of being DISLABLED, however, was not detected. The hypothesis that variables related to family context might influence the training probability due to a greater marginal value of non-market time could only be verified for martial status (PARTHH) not, however, for children (KS KM KL). Our results concerning the variables that capture the formal education level confirm the assumption that human capital accumulated earlier positively influences the investment into new human capital. Individuals who have completed an apprenticeship (LEHRE) or who have a university degree (DIPLOM) participate significantly more often in vocational training.

Evidently, participation in vocational training is also related to the actual labor market status since this can reflect a special need to participate in training as well as different access possibilities to training programs. We find that employment (EMPLOYED) increases the training participation probability. This increase is even stronger if the individual is employed in the public sector (PUBLIC SECTOR). Moreover, the probability to participate in training rises with the firm size, i.e. large firms seem to provide more training than small firms (FIRMSIZE). Our results related to variables describing the specific job position show that individuals with jobs which require a high qualification participate more often in training (JOBQUALIF). The status "blue collar worker" (BLUCOLLAR) has a negative effect compared to "white collar worker" (WHICOLLAR) and the reference group. The fact that the individual is working in the occupation he was originally educated for (JOBEDUC) may provide a (deceptive) feeling of safety, as it lowers the probability of participation.

Concerning the influence of employment history, we find that individuals who were employed in the last two years (EMP2YRS), have an increased training participation probability. As emphasized by HECKMAN ET AL. (1997), variables that predict job-seeking might be very important determinants of

[6] A computational problem when evaluating the likelihood function is the requirement to integrate with respect to the random effect μ_i. This is done by means of Gauss-Hermitian quadrature techniques as suggested by BUTLER/MOFFITT (1982).

Table 2:
Maximum Likelihood Estimates for Participation in Vocational Training
Unbalanced Random Effects Probit Model for 1986-1993

Variable	**Coefficient**	**t-value**
Intercept	-3.90847	-7.18711
Age/10	0.75361	2.61093
$Age^2/100$	-0.12187	-3.11552
Male	0.08912	1.16303
Foreigner	-0.52003	-4.98330
PartHH	-0.16877	-1.99221
Disabled	-0.19711	-1.11530
KS	-0.04461	-0.69390
KM	0.02658	0.29943
KL	0.00947	0.12185
Abitur	0.16763	1.44119
Lehre	0.33885	3.83434
Diplom	0.35752	2.16622
Employed	0.84813	3.70264
WhiCollar	0.23596	1.88561
BlueCollar	-0.27038	-2.02562
JobTenur	-0.00777	-0.87609
JobQualif	0.58957	5.70104
JobEduc	-0.23364	-2.55465
Firmsize	0.08234	2.22522
Public Sector	0.19857	1.91024
FutEmpImm	0.95383	4.99613
Emp2yrs	0.32520	2.25819
SpecQuest	-0.38534	-10.23693
σ_μ	0.73413	6.22750
N	3131	
Log-Likelihood	-1421.87142	

training participation. Hence we included a variable related to future plans regarding employment Individuals who are currently not employed and are seeking for an employment in the near future (FUTEMPIMM) are most likely to participate in a training measure.

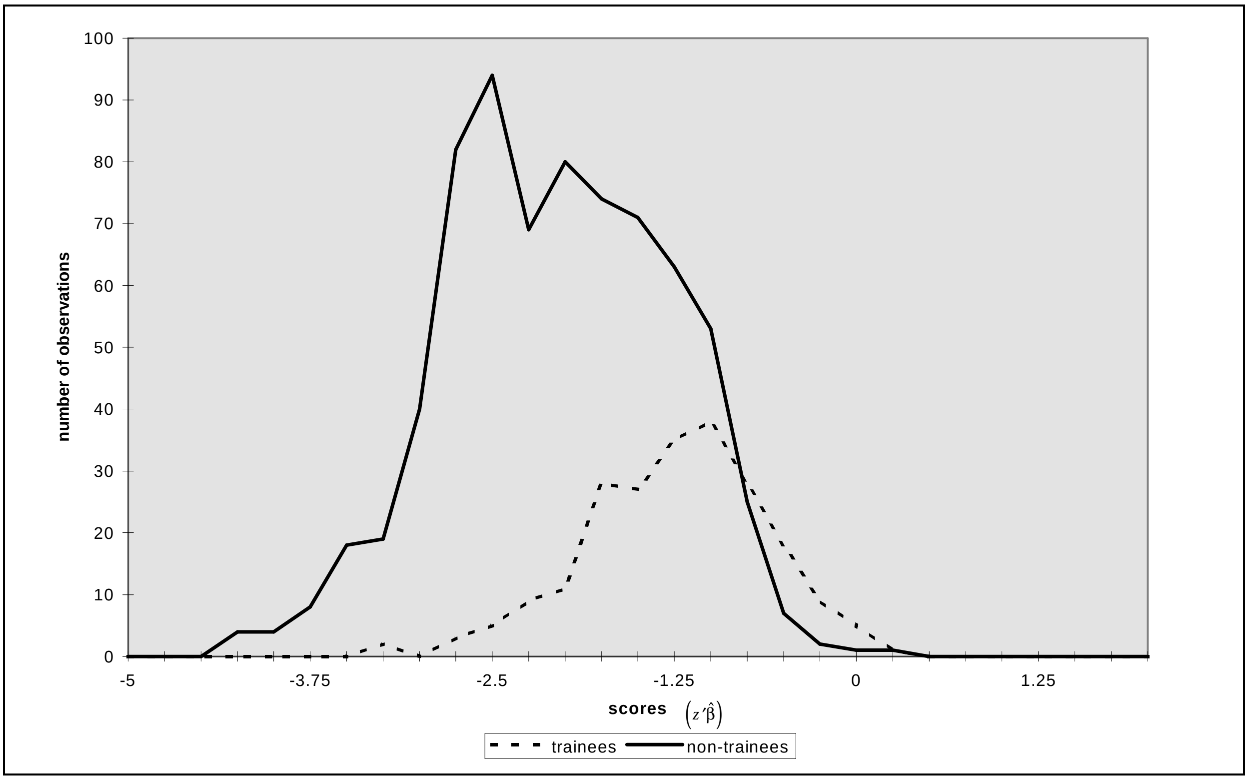
Finally we include a "technical" variable to account for the specific (retrospective) nature of the questionnaire concerning vocational training (SPECQUEST). This variable exhibits a negative "memory"-effect, i.e. an increasing distance between the current wave and the following special questionnaire makes the observation of a training participation less likely.

4.3. Application of Matching Methods

The aim of matching methods is to select for each trainee non-trainees, that resemble him/her as good 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 who on average - similar as in a randomized experiment - do not differ systematically in all relevant characteristics. Thus, we eliminate the need to take into account the selection process into training when estimating its impact on unemployment duration.

The central conditioning, respectively matching, variable is the estimated propensity score ($z'\hat{\beta}$). As stressed by HECKMAN ET AL. (1996) a successful application of matching methods is only possible inside the range of common support of the distribution of the propensity scores 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 for $z'\hat{\beta}$ for every year reveals that a large overlap indeed exists, despite the fact that the mass of the distribution of the non-trainees is to the left of the trainees'. *Figure 3* demonstrates this for the year 1988.

Figure 3: Distribution of $z'\hat{\beta}$ *for Trainee and Non-Trainee group in 1988*



The estimation of the propensity score incorporates yearly characteristics only. Potentially important differences like a particularly bad labor market situation of trainees just prior to the training program as emphasized by LECHNER (1995, 1996A, 1996B) for East Germany or by studies for the US like ASHENFELDER/CARD (1985) or CARD/SULLIVAN (1988) might not be adequately captured. Therefore we extend our matching procedure by a set of variables obtained from the retrospective calendar which describe the monthly pre-training employment history. We include the employment status in the last month before training as well as the average employment status in the last 4 and 12 months before training. Moreover HECKMAN ET AL. (1996) point out that accounting for differences in regional labor market conditions might also be essential in constructing an adequate control group. Hence we include an indicator that captures the number of vacancies and unemployed on state level (ANSPINDEX). To further enhance the matching quality, the matching procedure also directly incorporates the subset of variables that were used for the propensity score estimation and proved to be significant. These variables are: AGE, FOREIGNER, LEHRE, DIPLOM, EMPLOYED, JOBQUALIF, FIRMSIZE, FUTEMPIMM and EMP2YRS. Finally due to an initial condition problem (HAM/LALONDE (1990, 1996)) that would arise when latter comparing *post*-training unemployment spells of trainees with the controls that are already in progress when training ends, we 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 training).

In principal it seems straightforward to match each trainee only with 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 later estimating the impact of training on unemployment duration. Hence, in order to obtain a larger sample size we applied a matching procedure that matches each trainee to more the one non-trainee ("oversampling"). A detailed description of our matching procedure which is a variant of the one proposed by ROSENBAUM/RUBIN (1985), RUBIN (1991) and applied by LECHNER (1995, 1996A, 1996B) is given in the appendix B.[7]

Table 3 demonstrates the quality of our matching procedure. A comparison between randomly

Table 3: Comparison between Trainees, Controls and Random Non-Trainees for selected Characteristics in the Month of Questionnaire prior to Training Entry ("oversampling")

Variable	**random non-trainees** (113 observations) (means, share)	**controls** (218 observations) (mean, share)	**trainees** (113 observations) (mean, share)
$z'\hat{\beta}$	-2.48 *	-1.73	-1.69
Age (years)	34.7	30.0	32.3
Male	60	56	50
Foreigner	41 *	16	15
Disabled	8	4	4
PartHH	68 *	51	54
Abitur	5 *	13	17
Lehre	44 *	65	70
Diplom	2	5	7
Employed	71	78	78
WhiCollar	11 *	37	43
BlueCollar	47 *	21	16
JobTenure (years)	4.93	3.48	4.37
JobQualif	33 *	65	65
JobEduc	16 *	34	32
Firmsize (employed)	705	627	475
PublicSector	5	12	10
FutEmpImm	14	20	20
Emp2yrs	9	12	14
AnspIndex	11.81	12.75	12.71

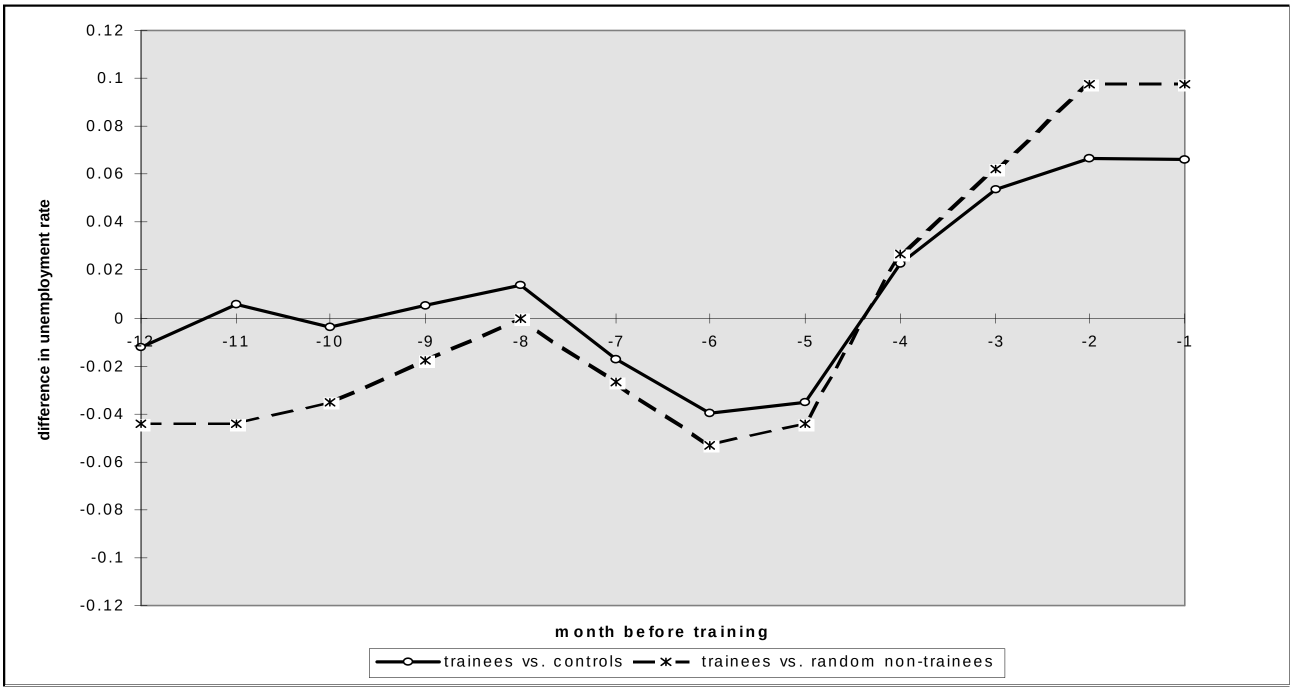
* denotes 95%-significance of difference in sample means

selected non-trainees and trainees reveals significant differences especially in the propensity score education, some socio-demographic variables and job related variables. In contrast, the distribution of characteristics of the matched controls resembles the group of trainees very closely, and no significant differences in mean were detected.

[7] Results of the "one-to-one-sampling" are presented in the appendix C.

With respect to differences in the monthly pre-training employment status *Figure 4* shows that trainees compared to random non-trainees have an increased unemployment rate just prior to the training program. Looking at the difference in unemployment rate between controls and trainees reveals that our matching algorithm is able to reduce this upward shift.

Figure 4:
Difference of Pre-Training Unemployment Rate between
Trainees, Controls and Random Non-Trainees ("oversampling")



5. Training and Unemployment Duration

5.1. Duration Analysis based on a Matched Sample

So far we have considered the evaluation problem that arises due to the selection into training. Via our matching procedure we generated a matched sample that consists of 113 trainees and 218 controls who on average -similar as in a randomized experiment - do not systematically differ in all relevant characteristics. We observe 160 *post*-training spells for trainees and 295 for controls. An obvious scheme to obtain the impact of training on unemployment duration is to compare the average *post*-training unemployment duration of trainees and controls.

Yet, as HAM/LALONDE (1990, 1996) demonstrate, even in an experimental framework which eliminates the need to control for the selection into training, 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, as we do, at the unemployment duration, we find 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 neglects the information that right censored spells are still ongoing and not observed until their ending.

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

- Finally, suppose that even if the unemployment spells of trainees and controls origin at the same time, e.g. the first period after training, the following problem still exists. Only in this first period trainees and controls do not on average systematically differ in their characteristics except for having participated in training. Beyond there, however, the average characteristics of those trainees and controls who are still unemployed do not necessarily equal (as in the initial groups). If for example, a positive training effect provokes that relatively more trainees with bad labor market characteristics than controls are 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.

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 controls for other observable and unobservable characteristics than training which also influence the unemployment duration is the application of 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*: $\lambda_i(t) = \lim_{dt \to 0^+} P\left(t \leq T_i < t + dt \middle| T_i \geq t\right) \cdot \left(dt\right)^{-1}$, i.e. the hazard or transition rate. T_i, the duration time, is a continuous non-negative random variable where t it's realization. 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 is this study starts off with the well-known and widely used Proportional Hazards model for continuous time as proposed by COX (1972):[8]

$$\lambda_i\left(t \middle| x_i(t)\right) = \lambda_0(t) \exp\left(x_i'(t)\beta\right) \qquad (11).$$

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 $\exp(0)$. β is the vector of coefficients to be estimated. In contrast to parametric models (e.g. Weibull hazard rate model) the form of λ_0 is not restricted to a particular specification. Instead the baseline hazard rate is estimated non-parametrically.

In model (11) it is assumed that all individual heterogeneity is captured by $x_i(t)$, and any unobservable heterogeneity is not considered. As ELBERS/RIDDER (1982) and KIEFER (1988) point out, failure to control for unobserved heterogeneity may lead to spurious duration dependence. In order to take into account unobserved heterogeneity we extend the Cox model to a Mixed Proportional Hazards model:

$$\lambda_i\left(t \middle| x_i(t), \omega_i\right) = \lambda_0(t) \exp\left(x_i'(t)\beta + \omega_i\right) \qquad (12)$$

where ω_i is a random variable that is not correlated with the covariates by assumption. An intriguing problem of including an additional random term for unobserved heterogeneity is the

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[8] For a wider overview on discrete-time hazard model and their empirical applications see HUJER/MAURER/WELLNER (1996).

specification of its distribution. HECKMAN/SINGER (1984) propose non-parametric methods to assess the distribution, because of sensitivity of parameter estimates to specific parametric distributions. However, as TRUSSELL/RICHARD (1985), MEYER (1990) and NARENDRANATHAN/STEWART (1993) point out, one difficulty of this approach is the fact that the number of mass points is not predetermined but has to be assessed using an iterative procedure. They emphasize that in the context of a non-parametrically specified baseline hazard rate, a parametric specification for the distribution of the heterogeneity seems to be appropriate as well.

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 are possibly useless due to the existence of ties, i.e. equal durations for different observations (KALBFLEISCH/PRENTICE (1980), COX/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:

$$h_i\left(j \middle| x_i(t)\right) = P\left[T_i < t_{j+1} \middle| T_i \geq t_j, x_i(t)\right] = \frac{S_i\left(t_j \middle| x_i(t)\right) - S_i\left(t_{j+1} \middle| x_i(t)\right)}{S_i\left(t_j \middle| x_i(t)\right)} \qquad (13).$$

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 (12) takes the following form:

$$\begin{aligned} S_i\left(t_j \middle| x_i\left(t_j\right), \omega_i\right) &= \exp\left(-\sum_{m=0}^{j-1} \int_{t_m}^{t_{m+1}} \lambda_0(u) \exp\left(x_i'(t_m)\beta\right) \exp\left(\omega_i\right) du\right) \\ &= \exp\left(-\exp\left(\omega_i\right) \sum_{m=0}^{j-1} \exp\left[\left(x_i'(t_m)\beta\right) + \gamma_m\right]\right) \qquad (14). \\ \text{where } \gamma_m &= \ln\left(\int_{t_m}^{t_{m+1}} \lambda_0(u) du\right) \end{aligned}$$

To obtain $S_i\left(t_j \middle| x_i\left(t_j\right)\right)$ we assume that $\exp\left(\omega_i\right) = \tau_i$ follows a gamma distribution ($\Gamma\left(\tau_i\right)$) with mean one and variance σ^{2}. The integration with respect to the gamma distribution leads to:

$$
\begin{aligned}
S_i\left(t_j \middle| x_i\left(t_j\right)\right) &= \int_0^\infty \exp\left(-\tau_i \sum_{m=0}^{j-1} \exp\left[\left(x_i'(t_m)\beta\right) + \gamma_m\right]\right) d\Gamma(\tau_i) \\
&= \left(1 + \sigma^2 \sum_{m=0}^{j-1} \exp\left[\left(x_i'(t_m)\beta\right) + \gamma_m\right]\right)^{-\sigma^{-2}}
\end{aligned} \qquad (15).
$$

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 ($\delta_i = 1$). k_i is either the interval, in which an event can be observed or the censoring interval. We assume that spells censored in the k_i-th interval are censored at time t_{k_i}. For a sample of *N* spells we then obtain:

$$
L(\gamma, \beta, \sigma^2) = \prod_{i=1}^{N} \left[\left(\frac{S_i\left(t_{k_i} \middle| x_i\left(t_{k_i}\right)\right) - S_i\left(t_{k_i+1} \middle| x_i\left(t_{k_i+1}\right)\right)}{S_i\left(t_{k_i} \middle| x_i\left(t_{k_i}\right)\right)} \right)^{\delta_i} \cdot S_i\left(t_{k_i} \middle| x_i\left(t_{k_i}\right)\right) \right] \qquad (16).
$$

Inserting (15), rearranging terms and taking logarithms leads to the following log-likelihood (MEYER (1990)):

$$
\begin{aligned}
l(\gamma, \beta, \sigma^2) = \sum_{i=1}^{N} \ln \Bigg\{ & \left[1 + \sigma^2 \sum_{m=0}^{k_1 - 1} \exp\left(x_i'(t_m)\beta + \gamma_m\right) \right]^{-\sigma^{-2}} \\
& - \delta_i \left[1 + \sigma^2 \sum_{m=0}^{k_1} \exp\left(x_i'(t_m)\beta + \gamma_m\right) \right]^{-\sigma^{-2}} \Bigg\}
\end{aligned} \qquad (17).
$$

Similar models have already been applied by MEYER (1990) and NARENDRANATHAN/STEWART (1993) to assess the impact of unemployment benefits on unemployment duration.

5.3. Estimation Results

In order to estimate the hazard model, we have to specify the baseline rate and a vector of covariates. The baseline rate is modeled by means of a dummy variable for each month since spell begin (BASE...). The first month is used as reference level. This was done up to month 6. After month 6 the numbers of observation are too small to use monthly dummies. We constructed month-groups in such a way that the numbers of completed durations in each month did not become too small for the estimation purpose.

The importance of accounting for unobserved heterogeneity due to the problem of spurious duration dependence becomes apparent if one compares the estimated coefficients of the baseline dummies in models with and without unobserved heterogeneity (the results for the latter are given in the appendix D). The latter model suggests that there is a significant negative duration dependence, i.e.

a decreasing reemployment probability beyond month six. This effect was not detected for a model with unobserved heterogeneity (*Table 4*). The signs of the baseline dummies beyond month six still become negative, but they are insignificant.

Table 4:
Maximum Likelihood Estimates for Transition Unemployment ⇒ Employment
Discrete Hazard Rate Model with Unobserved Heterogeneity

Variable	**Coefficient**	**t-value**
Constant	-5.5014	-6.4692
Base02	0.1474	0.6187
Base03	0.4054	1.4156
Base04	0.1123	0.3007
Base05	0.0828	0.2112
Base06	0.5821	1.3835
Base0712	0.1024	0.2234
Base1320	0.1503	0.2283
Base21+	-0.0688	-0.0814
TRSHORT	0.7732	2.5484
TRLONG	0.3535	1.6192
Age -21yrs	3.7414	4.6125
Age 22-39yrs	3.0763	4.2984
Age 40-54yrs	2.6284	3.8120
Male	0.4761	2.5218
Foreigner	-0.2225	-0.9436
Disabled	-0.4402	-1.0489
PartHH	0.5172	2.3438
Kids	-0.5004	-1.9514
Abitur	0.0205	0.0714
Lehre	0.1108	0.4920
Diplom	0.2818	0.6515
PrvEmployed	0.3335	1.5346
NoUneSp3	0.4505	3.4118
DurUneSp3	-0.5616	-2.5183
ReplacementRatio	-0.3465	-0.9481
AnspIndex	-0.0738	-3.3479
December	0.3223	1.5014
Spring	0.4490	2.7567
Summer	-0.4535	-1.7951
Ln(σ^2)	-0.3513	-0.5886
Number of spells	455	
Log-Likelihood	-805.33	
LR-Test χ^2 (df)	125.55 (21)	

Concerning usual socio-demographic characteristics, we find that minority groups do not have a significant lower reemployment chance (FOREIGNER, DISABELD). Men prove to have a higher reemployment probability than women (MALE). Additionally younger individuals have higher chances of finding a new job (AGE...). Finally variables related to the family background indicate that children (KIDS) reduce, while a marriage or a partnership (PARTHH) increase the chances of being re-employed.

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 as a preliminary screening factor. The final selection of applicants might be built up on more detailed information like 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) could be an indication for frictional unemployment with short unemployment spells and with 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 factor and hence long term unemployed are stigmatized. A further potential explanation is that as unemployment duration increases, the decay of firm specific human capital decreases above-average. As an effect reemployment chances decline. An increasing replacement ratio, defined as relation between the level of unemployment benefits and last labor market gross income has no significant negative reemployment probabilities (REPLACEMENT RATIO). This stands in contrast to search theory.

Of course, the individual reemployment probability is also influenced by demand side conditions. Thus we included an indicator that captures a potential mismatch between labor demand and supply on regional labor markets (ANSPINDEX). We find that worse regional labor market conditions increase unemployment duration. We also incorporated dummy variables to capture the typical seasonal pattern over the course of a year. As expected, the reemployment probabilities rise significantly during the usual spring time stimulation of the labor market (SPRING). For the summer season we find the expected negative sign but the variable is insignificant (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 employment calendar in the GSOEP leads to a disproportionate high number of spells ending in December (HUJER/SCHNEIDER (1996)). Hence the December dummy also intends to capture this so-called heaping effect at the end of the year. TORELLI/TRIVELLATO (1993) criticize this 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 KRAUSS/STEINER (1996) for the GSOEP that evaluates different heaping adjustment procedures shows that different ways of incorporating heaping effects hardly affects the coefficients of the explanatory variables. They 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 concludes, 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."

In order to measure the impact of participation in training we include two dummy variables, separately capturing the short-run (TRSHORT) and the long run effect (TRLONG). We find that training has a positive effect on reemployment probabilities in the short run but not in the long run: A completed vocational training course within one year prior to unemployment reduces expected unemployment duration significantly. In contrast to the "formal" educational variables, which we interpreted as preliminary screening factors for the employer, vocational training seems to provide unemployed with more "relevant" human capital which positively distinguishes them from other unemployed when searching for a job. To illustrate this short run training effect in a more detailed way *figures 5* and *6* present simulations. We compare two unemployed individuals, who entered unemployment in January and are equal in all characteristics except for the fact that one has completed a training course (trainee) within twelve month prior to January and the other has never participated at training (control). *Figure 5* plots the hazard rate. For instance, looking at the sixth month the control has a hazard rate of 19.4 %, whereas the trainee's hazard rate exceeds by 18.0%.

Figure 5: Simulated Hazard Rates for Trainee vs. Control

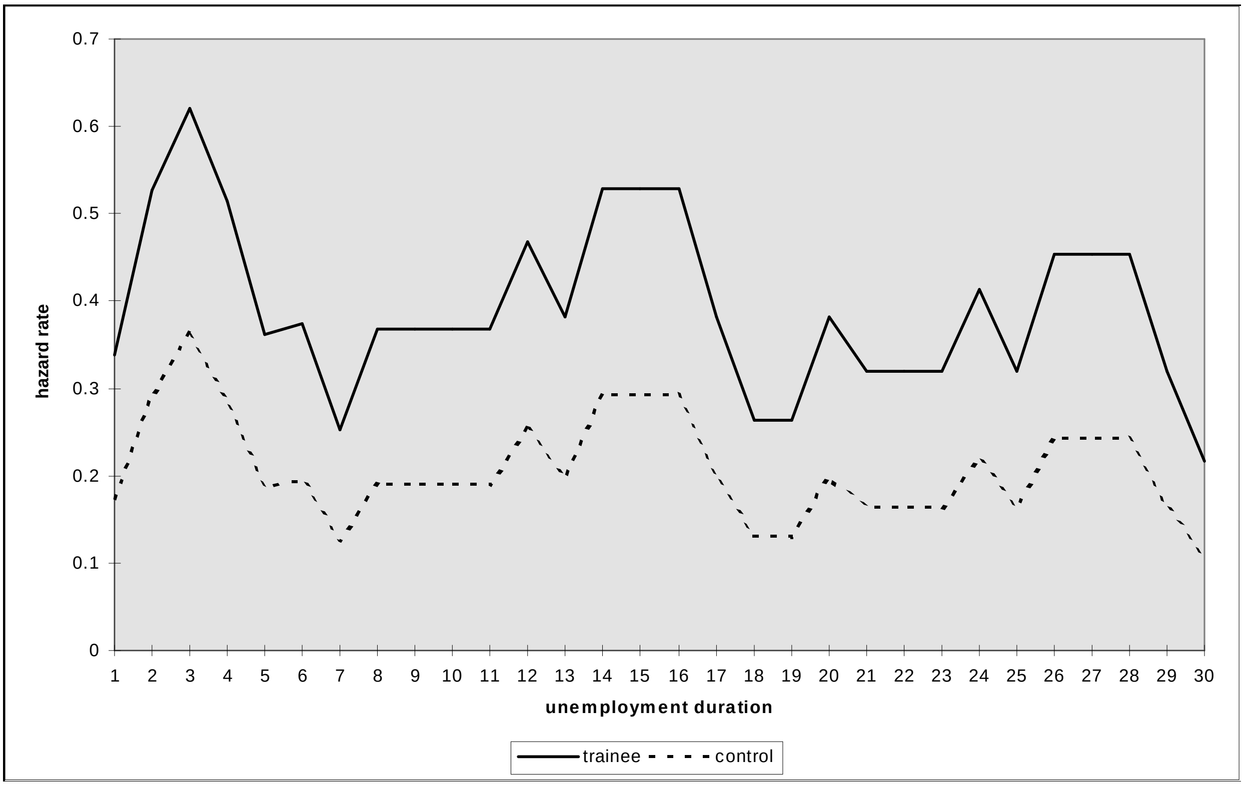
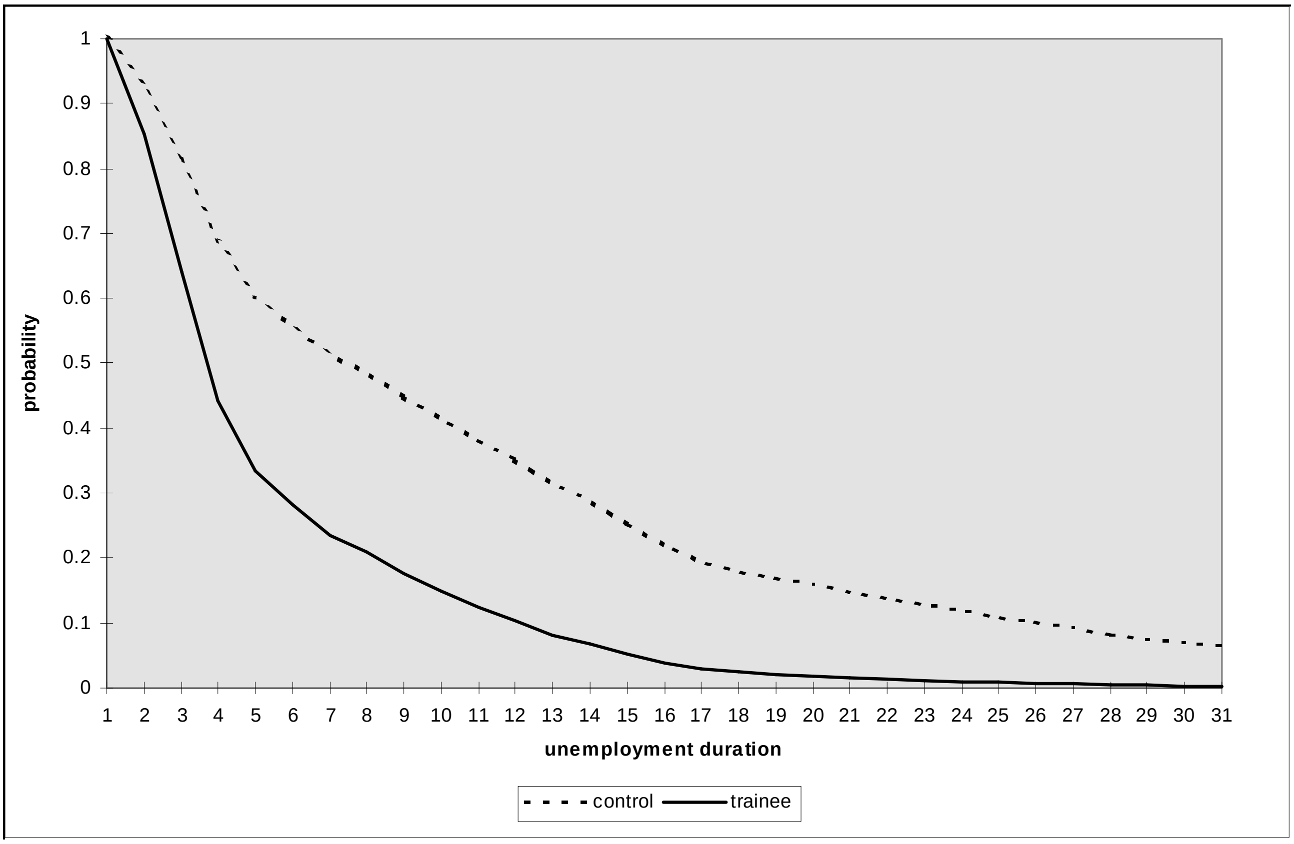


Figure 6 depicts the survivor function which illustrates the relationship of the hazard rates to long-term unemployment since it gives the probability of still being unemployed after month *t*. The survivor function for the control lies considerably above the one for the trainee. For the control the probability of still being unemployed after the twelfth month is 35.2%, while the corresponding probability for the trainee is 10.4%.

Figure 6: Simulated Survivor Function for Trainee vs. Control



6. Conclusion

In this paper we assess the impact of vocational training on unemployment duration in West Germany for the period from 1986 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 we estimated by means of a random effects probit model. As our 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 is necessary to use a discrete hazard rate model to evaluate the impact of vocational training on the subsequent unemployment duration. Our results indicate that participation in vocational training has a significant negative effect on unemployment duration in the short run but not in the long run.

Although our findings point to the importance of vocational training, it has to be kept in mind that the definition of vocational training measures used in this study includes courses that are privately financed as well as those subsidized by the Federal Labor Office. A natural way to deal with this heterogeneity would afford a further partitioning of training measures. This however is limited due to the small number of participants in the sample.

Appendix

Appendix A: Definition of Variables

Variable	Description
Training variables	
TRSHORT	1 if vocational training ended during the last twelve month prior to the spell begin
TRLONG	1 if vocational training ended earlier than twelve month prior to the spell begin
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
Variables related to the regional labor market	
AnspIndex	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
$(\text{Age}/10)^2$	Age squared and divided by 100
Age dummy variables — reference category is 55 years or older	
Age –21yrs	1 if individual is 21 years or younger
Age 22–39yrs	1 if individual is 22 years or older, but younger than 40
Age 40-54 yrs	1 if individual is 40 years or older, but younger than 55
Other socio-demographic variables	
Male	1 if individual is male
Foreigner	1 if individual is not a German national
PartHH	1 if individual is married or living together with his/her partner
Disabled	1 if individual is disabled
KS	number of children age up to 6 years
KM	number of children age 7 to 10 years
KL	number of children age 11 to 15 years
Kids	1 if individual has children age up to 15
Abitur	1 if individual has Abitur oder Fachhochschulreife (comp. to highschool degree)
Lehre	1 if individual has completed an apprenticeship
Diplom	1 if individual has a university degree or a degree of a Fachhochschule
SatisLife	Satisfaction with life in general (0 = totally dissatisfied; 10 = totally satisfied)

Appendix A: Definition of Variables (continued)

Variables related to current employment	
Employed	1 if individual is currently employed (full or part time)
Occupational Status	— reference category are apprentices and self-employed
WhiCollar	1 if individual is currently employed and has a white collar status
BluCollar	1 if individual is currently employed and has a blue collar status
JobTenure	years of affiliation with current employer
JobQualif	1 if individual is currently employed and the current job requires special instructional courses, a completed apprenticeship or a university degree
JobEduc	1 if individual is working in the occupation he/she was originally educated for
Firmsize	1 if firm has less than 20 employees or individual is self-employed
	2 if firm has 20 or more, but less than 200 employees
	3 if firm has 200 or more, but less than 2000 employees
	4 if firm has 2000 or more employees
Public Sector	1 if individual is working in the public sector
Variables related to future plans regarding employment	
FutEmpImm	1 if individual is currently not employed but wishes to be employed in the *near* future (i.e. immediately or next year)
Variables related to employment history	
Emp2yrs	1 if individual was employed sometime within the last two years
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
„Technical" variables	
SpecQuest	Years to the following special questionnaire on vocational training

Appendix B: The Matching procedure

This section gives detailed information on the matching procedure. It corresponds closely to the one proposed by ROSENBAUM/RUBIN (1985) RUBIN (1991) and applied by LECHNER (1995, 1996A, 1996B). Along the lines of LECHNER we use the unbounded scores $z'\hat{\beta}$ instead of the bounded propensity score $\Phi\left(z'\hat{\beta}\right)$ as the main matching variable. Due to location and symmetric of the distribution of $z'\hat{\beta}$ the use of $\Phi\left(z'\hat{\beta}\right)$ would lead to an undesirable asymmetry when $\Phi\left(z'\hat{\beta}\right)$ is close to 0 or 1. The steps of the matching procedure are as follows:

1. Divide the individuals in two separate groups of trainees and non-trainees according to whether they have participated in vocational training during the time span 1986-1993 (trainee group) or not (non-trainee group).

2. Randomly select a trainee (denoted by i) form the trainee group. If this trainee participated in more than one vocational training course choose the earliest one as the relevant for the following steps.
   If the we observe for this trainee a *post*-training unemployment spell go to step 3. Else, this trainee will be removed and not further considered in the estimation, and step 2 has to be repeated.
3. Based on the estimated random effects probit model compute the propensity score $z_{it}'\hat{\beta}$ and the variance $\mathrm{Var}\left(z_{it}'\beta\right)$ for the trainee i in wave t, where t refers to the month of questionnaire prior to the beginning of vocational training. Construct the interval $z_{it}'\hat{\beta} \pm c\sqrt{\mathrm{Var}\left(z_{it}'\hat{\beta}\right)}$ 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 $z_{jt}'\hat{\beta} \in \left(z_{it}'\hat{\beta} \pm c\sqrt{\mathrm{Var}\left(z_{it}'\hat{\beta}\right)}\right)$ in wave t.

5. 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.
   If there is only one non-trainee between the given limits of the interval go to step 6.
   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 $d\left(j,i\right) = \left(z_{jt}'\hat{\beta}, a_{jt}\right)' - \left(z_{it}'\hat{\beta}, a_{it}\right)'$ between each non-trainee j and trainee i. Choose that 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 their are any observations left in the trainee group, start again with step 2.

In case of matching each trainee only with the closest non-trainee ("one-to-one-sampling") the matching procedure is finished if for every valid trainee **one** non-trainee (control) is found. In case of a "oversampling" the matching procedure is repeated a second time in order to find a **second close** non-trainee for each valid trainee.

Appendix C: Results of the "one-to-one-sampling"

Table C1: Comparison between Trainees, Controls and Random Non-Trainees for selected Characteristics in the Month of Questionnaire prior to Training Entry ("one-to-one-sampling")

Variable	**random non-trainees** (113 observations) (mean, share)	**controls** (113 observations) (mean, share)	**trainees** (113 observations) (mean, share)
$z'\hat{\beta}$	-2.48 *	-1.69	-1.69
Age (years)	34.7	30.0	32.3
Male	60	58	50
Foreigner	41 *	14	15
Disabled	8	4	4
PartHH	68 *	53	54
Abitur	5 *	16	17
Lehre	44 *	64	70
Diplom	2	7	7
Employed	71	79	78
WhiCollar	11 *	42	43
BlueCollar	47 *	18	16
JobTenure (years)	33 *	66	65
JobQualif	4.93	4.09	4.37
JobEduc	16 *	37	32
Firmsize (employed)	705	606	475
PublicSector	5	12	10
FutEmpImm	14	20	20
Emp2yrs	9	12	14
AnspIndex	11.81	13.19	12.71

* denotes 95%-significance of difference in sample means

Figure C1: Difference of Pre-Training Unemployment Rate between Trainees, Controls and Random Non-Trainees ("one-to-one-sampling")

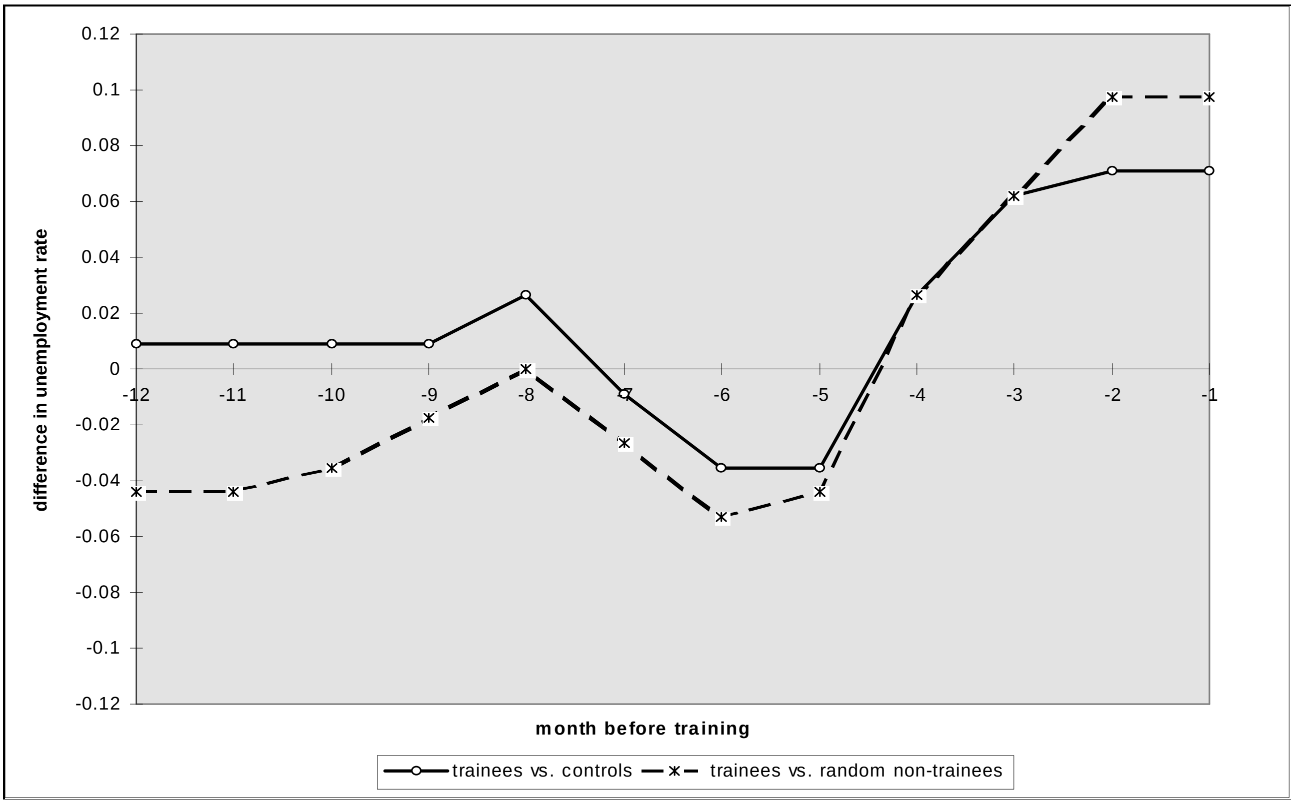


Table C2: Maximum Likelihood Estimates for Transition Unemployment ⇒ Employment Discrete Hazard Rate Model with Unobserved Heterogeneity (based on "one-to-one-sampling")

Variable	**Coefficient**	**t-value**
Constant	-5.8727	-4.9011
Base02	0.5347	1.6767
Base03	0.7446	1.8129
Base04	0.2928	0.5221
Base05	0.7578	1.2865
Base06	1.3116	2.0777
Base0712	0.8556	1.1672
Base1320	1.5090	1.4824
Base21+	1.5150	1.0641
TRSHORT	1.2186	2.6345
TRLONG	0.5165	1.4708
Age -21yrs	4.2229	3.7139
Age 22-39yrs	3.7637	3.6579
Age 40-54yrs	3.2643	3.3034
Male	0.9865	3.0881
Foreigner	-0.2764	-0.7482
Disabled	-0.3796	-0.6414
PartHH	0.6984	1.9937
Kids	-0.4632	-1.2036
Abitur	-0.5338	-1.1697
Lehre	-0.3440	-0.9464
Diplom	0.3287	0.4754
PrvEmployed	0.2924	0.9368
NoUneSp3	0.3921	1.7521
DurUneSp3	-0.3670	-1.0184
ReplacementRatio	-0.7245	-1.2403
AnspIndex	-0.0988	-3.0902
December	0.2489	0.8848
Spring	0.2172	1.0408
Summer	-0.3130	-1.0567
Ln(σ^2)	0.4825	1.0133
Number of spells	307	
Log-Likelihood	-549.9084	
LR-Test χ^2 (df)	75.98 (21)	

Appendix D:

Maximum Likelihood Estimates for Transition Unemployment ⇒ Employment
Discrete Hazard Rate Model without Unobserved Heterogeneity

Variable	**Coefficient**	**t-value**
Constant	-4.9177	-7.1607
Base02	0.0007	0.0035
Base03	0.1407	0.6699
Base04	-0.2361	-0.9105
Base05	-0.3244	-1.1309
Base06	0.0990	0.3721
Base0712	-0.5715	-2.5929
Base1320	-0.7658	-2.6422
Base21+	-1.3695	-3.3296
TRSHORT	0.3841	1.9461
TRLONG	0.2310	1.4889
Age -21yrs	3.0089	4.7955
Age 22-39yrs	2.5342	4.2639
Age 40-54yrs	2.2440	3.7276
Male	0.3234	2.3038
Foreigner	-0.2190	-1.2022
Disabled	-0.1851	-0.7220
PartHH	0.3706	2.3212
Kids	-0.2954	-1.7738
Abitur	-0.0488	-0.1998
Lehre	0.1432	0.8254
Diplom	0.2845	0.8256
PrvEmployed	0.2151	1.3293
NoUneSp3	0.3470	3.9300
DurUneSp3	-0.4114	-2.3306
ReplacementRatio	-0.1858	-0.7113
AnspIndex	-0.0564	-3.4585
December	0.3524	1.7797
Spring	0.4337	2.9747
Summer	-0.4562	-1.9436
Number of spells	455	
Log-Likelihood	-807.1034	
LR-Test χ^2 (df)	122.00 (21)	

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