Evaluation of Further Training Programmes with an Optimal Matching Algorithm

Eva Reinowski*, Birgit Schultz* and Jürgen Wiemers**

JEL-Classification: C14, C41, H43, J68
Keywords: Evaluation, Further Training, Optimal Full Matching, Duration Analysis

1. Introduction

Microeconomic evaluation studies try to assess the effectiveness of a country's active labour market policy. The proclaimed objective of labour market programmes is the improvement of the chances of individuals to find regular employment. However, the outcome of such programmes is uncertain. Basically, participation in a labour market programme can have three possible outcomes: the probability of employment can either increase, decrease or remain unchanged. Evaluation studies aim at quantifying the effect of participation in a labour market programme on the probability of employment.

Previous studies on the impact of labour market programmes in Germany established different effects depending on the data used, the period observed, and the methods applied. Most studies are based on the East German Labour Market Monitor from 1990 to 1994, the Labour Market Monitor Saxony-Anhalt and the German Socio-Economic Panel. The problem of selection bias is approached by applying different methods: Pannenberg (1996), Hübler (1997, 1998) and Kraus, Puhani and Steiner (1999) use parametric models and consider observable heterogeneity. Fitzenberger and Prey (1998, 2000) additionally use a non-parametric difference-in-difference method to correct for unobservable heterogeneity. Hujer and Wellner, 2000 evaluate the effect of further training by means of a hazard rate model for matched samples. Other studies apply matching methods, sometimes supplemented by difference-in-difference or parametric models

* Halle Institute for Economic Research (IWH), Kleine Märkerstr. 8, D-06108 Halle. E-mail: Eva.Reinowski@iwh-halle.de, Birgit.Schultz@iwh-halle.de.
** Institute for Employment Resarch (IAB), Regensburger Str. 104, D-90478 Nürnberg. E-mail: Juergen.Wiemers@iab.de.

(see Bergemann et al., 2000, 2001, 2004, Eichler and Lechner, 2000, 2001 Hübler, 1998, Lechner, 1998). Simulation studies using different methods show that matching and the difference-in-difference method yield best results with regard to removing observable and unobservable heterogeneity (Hujer, Caliendo and Radić, 2001). Recent studies based on matching methods tend to result in negative or insignificant effects of further training programmes.[1]

However, the literature rarely analyses whether the effect of participation in a programme is influenced by individual characteristics, economic environment or the organisational design of training measures. Therefore, the aim of this study is to evaluate the employment effects of further training programmes for Saxony between 1990 and 2001 for different subgroups representing individual characteristics as well as some aspects of the economic environment.

Our methodological approach differs in three aspects from other studies. First, we follow the concept of perforated unemployment, that means the unemployment spell of participants includes the further training episode. Second, we use the pre-history of the employment status as an indicator of the employment probability before the start of the programme, in order to eliminate Ashenfelter's Dip. Third, we employ a matching algorithm which provides an optimal full assignment. The results of our evaluation study show a negative effect of participation in further training programmes.

The paper is organised as follows. Sections 2 and 3 give a short overview of the legal basis of further training programmes in Germany and the development of participation in East Germany and Saxony as well as the description of the data. Section 4 theoretically describes the fundamental problem of microeconomic evaluation and lists assumptions on the matching process and the resulting requirements for the data. Following we explain our selection of variables (section 5) and spells (section 6). Sections 7 and 8 present the matching approach and the model of duration analysis we employ for our empirical study. Results are presented in section 9 and section 10 concludes our paper.

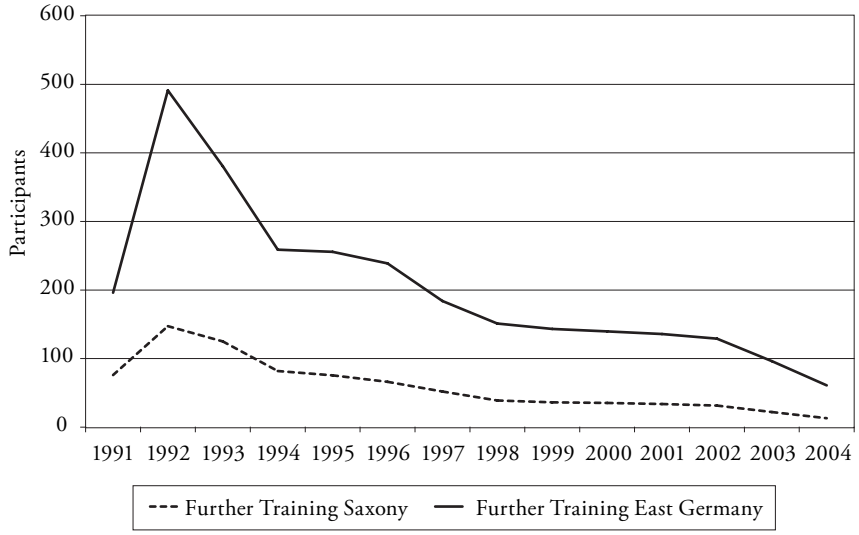
1 For an overview of evaluation studies in East Germany see also Hujer and Caliendo (2000).

2. Further Training in East Germany – Especially in Saxony

Further training programmes belong to the most important programmes of active labour market policy in East Germany. They intend to integrate unemployed persons into the labour market by promoting vocational qualifications. Further training programmes include vocational re-training measures and the extension or adaptation of vocational skills. Such further training measures can last up to 24 months for re-training in a new profession and three to eight months for extension or adaptation programmes. Participants can get a subsistence allowance (Unterhaltsgeld) if they are entitled to unemployment benefits or assistance. Local employment offices assign private training centres or schools to carry out further training programmes. The local employment office also selects the unemployed persons to take part in further training measures.

The importance of further training programmes in East Germany and Saxony can be seen from the number of participants (Figure 1). The maximum is in 1992 with an annual average of about 500,000 persons and 150,000 persons, respectively. In the following years the number of participating persons steadily declined to currently about 60,000 in East Germany.

Figure 1: Participants in Further Training Programmes in East Germany and Saxony from 1991 to 2004 (in Thousands)



Source: Bundesanstalt für Arbeit

3. Data Description

We base our evaluation of active labour market programmes on the Micro Census of Saxony in January 2000, January 2001 and January 2002. The Census offers the required data to satisfy the first assumption: it includes demographic characteristics as well as information on the employment history.[2] The Saxon data base is linked with the German Micro Census in as much as it is carried out three times per year with the similar questions and the similar procedure as the German one. A fraction of 0.5% of all households in Saxony are committed to participate, resulting in 10,000 households per census. All persons in these households (approx. 15,000 participants) are interviewed. It is obligatory to answer the questions of the Micro Census. A household can participate at most three times in the census, implying partial rotation of the participants.

In contrast to the German Micro Census, the Saxon Census includes quarterly information on participants' employment history since 1989. Due to the partial rotation, this information is available only once per person. The complete individual employment history can be reconstructed using quarterly information from the three censuses used. Our sample covers the period from the first quarter of 1989 until the fourth quarter of 2001. It includes spells of unemployment and participation in active labour market policies (ALMP), where it is possible to have more than one spell per person. There are no similar datasets for other East German federal states.

There are three possible sources of inaccuracies in our information on unemployment spells. First, since interviewed persons have to report retrospective information, they might give an incorrect sequence of their various spells or a wrong classification of their employment status, especially when the survey period extends far back into the past. Second, since the data frequency is quarterly, there is no information available on the exact time of a status change. The status change could have occurred in the same quarter it is reported or in the quarter before. Finally, short spells within a quarter cannot be observed.

2 Heckman and Smith (1999) show, that including employment history in addition to demographic characteristics is very important to control for selection bias.

4. The Microeconomic Evaluation Problem

Microeconomic evaluation is based on the model of potential outcomes.[3] It identifies the impact of labour market programmes on individual employment opportunities by comparing the outcome of a treated person with the probable outcome for the hypothetical case of non-treatment. The potential outcome can be defined for instance as personal income, unemployment duration or duration of future employment.

A direct estimation is impossible because the treatment outcome and the non-treatment outcome cannot be observed for a person simultaneously. In this sense, the fundamental evaluation problem is a missing data problem.

For a causal interpretation of the individual treatment effects it is necessary to satisfy the *SUTVA* (stable unit treatment assumption). It requires independence of individual treatment effects, i.e. the programme effect for each participant must not be affected by the treatment of other persons. This excludes indirect effects on the regional labour market or the whole economy[4] and permits the estimation of average treatment effects to overcome the fundamental evaluation problem independent of size and composition of the treated population group. The average effect of treatment on the treated indicates the expected outcome for persons who received treatment compared to the hypothetical situation of non-treatment. Therefore, a group of non-treated persons with – on average – the same relevant observable and unobservable characteristics as the participation group has to be found. If this is not exactly possible the estimation results will be distorted by a selection bias.

One of the most popular methods to overcome the problem of selection bias is a matching procedure. The basic idea is that the outcome of a well chosen group of non-treated persons is a good proxy for the counterfactual outcome as long as the persons in both groups have the same observable characteristics.

The simplicity of this idea as well as the important fact that matching leaves the individual treatment effects completely unrestricted – that means robustness to heterogeneous treatment effects in the population – are the main reasons for its popularity. On the other hand, matching is highly demanding on the data at hand. The identifying assumption, the conditional independence assumption,

3 This model is also known as *Roy-Rubin-model.* For a detailed description see Heckman, LaLonde and Smith (1999), pp. 1877–1879.

4 See Fröhlich (2004), pp. 4–5 for a detailed discussion of this assumption and possible indirect effects.

requires that conditional on characteristics X the assignment to the treatment and the non-treatment group is independent of the potential outcomes. It is satisfied only if all variables that influence both the selection process and the potential outcome are used for matching. This also implies that all relevant characteristics must be observable. Since this is seldom the case, many studies use the difference-in-difference approach to handle heterogeneity in unobservable characteristics. The problems associated with this approach[5] can be avoided by using adequate proxy variables for the unobserved characteristics.

A further necessary condition for identifying an unbiased treatment effect is the common support condition,[6] which states that for each chosen X it must be possible to find both participants and non-participants. Both assumptions together are sometimes referred to as strongly ignorable treatment assignment.[7]

5. Choice of Variables

The selection of relevant variables for the analysis is derived from human capital theory and recent empirical studies.[8] Theory suggests decreasing investment into human capital with age, and labour market statistics show a negative influence of age on labour demand.[9] Another important factor for labour market behaviour is gender as it is obvious from the employment structure.[10] For the selection process gender may be important too, because the assignment to training measures depends on the fraction of men and women among the unemployed[11]. Therefore, gender and age are included into the matching process.

5 One of the most important problems is the choice of the reference time before the measure starts – it should be unaffected by the future participation and temporary heterogeneity of participants and non-participants. Furthermore, short-run results cannot be interpreted due to Ashenfelter's dip, the decrease of the employment probability before an ALMP-measure, and the mean reversion afterwards.

6 Heckman, Ichimura and Todd (1997) decompose the conventional bias measure into different components and show that failure of the common support condition (one component of the bias) results in a substantial increase of the bias.

7 See Rosenbaum and Rubin (1983), p. 43.

8 This variable selection procedure is also used e.g. in Hujer, Maurer and Wellner (1997), p. 13 or Christensen (2001), pp. 25–27.

9 The unemployment rate of persons of 55 to 60 years is 16.7%, in contrast to 11.5% for persons in the age bracket of 30 to 40. See Bundesanstalt für Arbeit (2004a), overview I/5.

10 The share of women in the total number of part time and low paid employment is 84.4% and 69.7%, respectively. See Bundesanstalt für Arbeit (2004b, 2004c).

11 See §8 SGB III.

Furthermore, we expect human capital to have a positive influence on the selection process for training[12] and on employment opportunities. To get quasi time-invariant information about formal education levels all persons who were younger than 25 at the beginning of the observation period (1989) were excluded from further analysis, because education is usually completed at the age of 25. If not, persons are not unemployed and hence not included in the sample. A problem could arise if persons continue their education after an unemployment period. If a previous participant has a higher qualification at the interview date than at the beginning of the considered unemployment period, it is possible that this person is matched with a – at matching time – higher qualified person. If a non-treated person continues education during unemployment, the person could be matched with a better-educated participant. Due to the selected sample we expect this problem will rarely occur and thus will not bias the estimation results in a systematic way.

Since other time-variant information, like income and family background, is not available for the matching time the estimated treatment effect will probably be biased. Moreover, these characteristics could follow different paths in the treatment and the non-treatment group. However, we assume that employment history can be used as a proxy for the time-variant characteristics in the matching process. Therefore, we generate the following employment history variables: the share of time spent in employment, non-employment and unemployment, as well as the frequency of changes into and the mean duration of employment, 'non-employment' and unemployment. Moreover, the labour market statuses for six quarters before matching are included.

Besides demographic characteristics and employment history, a similar economic environment of the compared persons is important for unbiased estimation results.[13] Therefore, information about the place of residence and the start of the considered unemployment spell are included additionally. The latter is necessary because of various changes of labour market policy and other economic factors during the observation time.

12 According to recent empirical studies, persons who completed an apprenticeship or any higher education are more likely to participate in vocational training. See e.g. Hujer, Maurer and Wellner (1997), p. 13 and Christensen (2001), p. 27.

13 Heckman, Ichimura and Todd (1997) analyse possible sources of biased estimation results. They identify a mismatch of labour market conditions across treatment group members and comparison-group members as one major source of bias.

6. Selection of Spells

Our aim is to compare the outcome of a treated person with the person's hypothetical outcome in case of non-treatment to answer the question whether participation can increase the probability to find employment, or whether participation does not influence employability, or whether participation even affects it negatively. In order to eliminate potential biases in the estimation of the treatment effect which cannot be handled by matching, it is necessary to select spells carefully.

We define our spells according to the concept of perforated unemployment[14], which means that the unemployment spell of participants includes the further training episode. A typical participation spell starts with the entry in unemployment in a specific quarter. After a few quarters unemployment is discontinued by a change into further training. Following the measure unemployment is continued. We regard the three periods as a whole. Thus, the only way to end a spell successfully is to change into employment. Not applying the concept of perforated unemployment would induce a selection bias. In this case spells of participation would start with the end of the measure and focus on unemployment duration after the training. At the beginning of the evaluation period most participants are already out of regular employment for a relatively long period of time. Accordingly, they have disadvantages on the labour market. If they would be compared with unemployment spells of non-participants whose unemployment period started recently the participation effect could not be estimated correctly.

We only select unemployment spells for the group of non-participants. For both groups, only spells of persons who have never participated in any ALMP-measure before the observation time are included. We also exclude all spells for persons older than 55 years, because these persons could probably use the policy to smooth their transition to retirement.

Two other sources of bias are an anticipation effect and a cohort effect, which make it difficult to find the correct treatment effect. Therefore, it is necessary to eliminate or to measure these effects.

Many studies observe a decrease in the probability of employment before participation in ALMP-measures. This effect was first observed by Ashenfelter[15] and is therefore referred to as Ashenfelter's Dip. The most popular explanation for this effect is that future participants anticipate their participation and therefore reduce their job search intensity.

14 See Büchel (1992).
15 See Ashenfelter (1978).

In Germany the legal requirements of taking part in an ALMP-measure could be a more important explanation of the dip, because only persons who are unemployed and entitled to unemployment benefits are allowed to participate in an ALMP-measure. The entitlement to unemployment benefits requires a minimum length of employment. This means most of the participants change from employment to unemployment and start a training measure after a few quarters. In our data, 92% of participants are employed one quarter before they change into unemployment and 80% of participants are unemployed less than four quarters before the start of the measure. Therefore, the cohort effect is a result of the selection of participants with specific labour market histories who are compared to a non-selected group of non-participants. This implies that participants and non-participants follow different employment paths. The employment quota of participants declines substantially before the start of the programme, whereas non-participants can have different employment histories. The only criterion is the registration of unemployment, a possible entitlement of unemployment benefits or an allowance to participate in an ALMP-measure are unknown.

A possibility to deal with both, the cohort and anticipation effect, is to match partners with similar employment histories so that participants and non-participants have the same employment probability before the ALMP-measure. In order to eliminate the effects, we only select non-participants as potential matching partners for every participant whose unemployment period is at least as long as the one of the participant before entering training. This selection procedure ensures that participants and non-participant follow similar employment paths until the start of the programme.

With this rules we select 850 participation spells for the matching procedure. In the case of non-participation 3,726 spells are available.

7. Application of the Matching Approach

The matching control group consists of individual counterfactual outcomes for each participant. These counterfactual outcomes are determined in this study as the outcome of one special non-participant who has similar relevant observed characteristics. This technique is commonly referred to as nearest neighbour matching[16] or nearest available pair matching.

16 For a short overview over different nearest neighbour matching approaches see Heckman, LaLonde and Smith (1999), pp. 1953–1954.

When using this approach, two central questions have to be answered: how to define similarity between participants and non-participants and how to make sure that every participant is assigned to a best non-participant?

One possible procedure is matching with replacement, where every participant is assigned to the closest non-participant irrespective of how often one non-participant is used as partner for participants. This technique contains the potential problem that only a few non-participants are used very often while other very similar non-participants are not considered. This may result in a rise of the variance of the estimated treatment effect.[17]

When the number of non-participants markedly exceeds the number of participants – which is the case in our study – matching without replacement is usually applied. Lechner (1998) improves a two-step procedure by Rosenbaum and Rubin (1985) by defining variable callipers for the so-called participation tendency. In the first step this single aggregated measure of similarity is used for pre-selection. In the second step additional characteristics for measuring similarity between a participant and possible partners are included. The deviation of these characteristics is not restricted.

Lechner's (1998) assignment process is to randomly order the participants, successively find the closest non-participant from the particular sub-sample and remove the matched pair from the pool of considered persons. Each participant for which no similar non-participant can be found is excluded from further analysis. This is a standard procedure in the empirical literature.[18]

The application of any matching procedure without replacement raises several questions if one non-participant is the best partner for more than one participant. Who should be assigned to this non-participant: the first drawn participant, the closest, or the participant who has no alternative partners? The standard procedure assigns the first drawn participant. The disadvantages of this random choice are the risk of not finding adequate partners for the later drawn participants and therefore losing observations, and additionally it cannot be ensured that the best possible assignment is found. The former problem may not be important if the sample size is sufficiently large. Since we divide the sample of participants into various sub-samples in this study, we however cannot ignore this problem. Thus, a procedure is desirable that guarantees not to lose observations due to the design of the assignment process and simultaneously ensures to find the best possible assignment result.

17 See Lechner (2000), p. 9.

18 For applications see e.g. Christensen (2001) or Gerfin and Lechner (2002).

In finite samples the importance of some characteristics for the participation decision and employment prospects may differ, i.e. persons with identical propensity scores may have dissimilar labour market prospects due to the fact that characteristics affect their participation decision and employment chances not to the same degree. Fröhlich (2004) recommends to use the principal covariates affecting the outcome or a so-called augmented propensity score for matching. Furthermore, using a symmetric metric, matching by use of the propensity score would lead to an undesirable asymmetry, when the propensity score is close to 0 or 1.[19]

Because of the finite sample size of the sub-samples we cannot use the propensity score as the only distance measure. Therefore, in this study we apply a one-step balancing-score matching, that uses personal characteristics as well as the participation tendency. The included characteristics are differently scaled. It is pointed out in statistical literature that to measure different scaled covariates with one and the same distance measure is inappropriate.[20] Regarding e.g. quantitative covariates as qualitative ones or vice versa, results in loss of information from the data or an overvaluation of the qualitative variables. The most common way to construct aggregated distance measures is a two step procedure. In a first step scale-specific distance measures are quantified. Then, after a suitable standardisation of the specific distances, the distances are weighted with the number of the included variables in each distance measure.[21]

Similarity between participant i and non-participant j in participation tendency and metric variables is measured by the Mahalanobis distance[22]

$$MD_{ij} = [(\hat{I}_i, X_i) - (\hat{I}_j, X_j)]' \, \Sigma^{-1} [(\hat{I}_i, X_i) - (\hat{I}_j, X_j)], \tag{1}$$

where X_i and X_j are the $n \times 1$-vectors of the considered covariates, $\hat{I}_i$ denotes the estimated participation tendency and Σ^{-1} the inverse of the covariance matrix of $(\hat{I}, X_i)$. The Mahalanobis distance has the advantage that potential correlations between the covariates are accounted for by including the inverse of their covariance matrix.[23]

19 See Lechner (1998), p. 115.
20 See e.g. Opitz (1980), pp. 50–51 for further details.
21 See e.g. Kaufmann and Pape (1996), p. 453.
22 The participation tendency is treated as a metric variable because normal distribution can be assumed. See Lechner (1998), p. 115.
23 See Kaufmann and Pape (1996), p. 450.

This distance measure contains the following variables (included in vector X): age, start of the unemployment spell, share of time spent in employment and unemployment as well as mean duration of employment and unemployment.

We estimate the participation tendency $\hat{I}_i$ as the latent variable of the index function of a probit model. In this estimation we include demographic variables (gender, age and human capital) by default. Indicators for the economic environment (start of the considered unemployment spell and place of residence) and for the employment history (share of time spent in employment/unemployment, mean duration of employment/unemployment and labour market statuses for six quarters before matching) enter the estimation only in the case they improve the model. To measure similarity in nominally scaled variables the generalised matching coefficient:[24]

$$MC_{ij} = \frac{1}{m} \sum_{p=1}^{P} m_p \sigma(z_{pi}, z_{pj}) \tag{2}$$

with

$$\sigma(z_{pi}, z_{pj}) = \begin{cases} 1 \text{ if } z_{pi} = z_{pj} \\ 0 \text{ otherwise} \end{cases} \tag{3}$$

is applied. The number of covariates under consideration is denoted by P. Covariate z_p has m_p different values. The total sum of values over all covariates is given by

$$m = \sum_{p=1}^{P} m_p .$$

Having this type of matching coefficient it is possible to measure similarity allowing for different numbers of values in the covariates.

The included variables (covariates z) are: gender, human capital, place of residence and labour market status for each of six quarters before matching.

Our aggregate distance measure is constructed as a weighted average of the Mahalanobis distance and the generalised matching coefficient:

$$M_{ij} = \frac{1}{a+b} [a(1 - MC_{ij}) + \alpha b MD_{ij}], \tag{4}$$

24 See Kaufmann and Pape (1996), p. 446 for a detailed description.

where a and b denote the number of metrically and nominally scaled covariates, respectively. The factor α ensures that the medians of both distance measures, $(1 - MC_{ij})$ and MD_{ij}, are equal. In our study it proved inappropriate to use the number of the included variables as the only weighting factors, because in this case the impact of the nominally scaled variables on the aggregate measure is dominated by that of the metric variables. This results in significant differences between matched participants and non-participants in the nominally scaled covariates. Therefore, we extended the standard procedure by including the weighting factor α. Thus, we achieve the desired similarity between participants and non-participants in all considered covariates.[25]

For the assignment process we use the Hungarian algorithm, which is known from graph theory and linear optimisation. The algorithm was introduced by Kuhn (1955) to solve the classical assignment problem. The basic idea is to update the edge weights of a bipartite graph with appropriate vertex potentials[26] so that a complete Matching with zero weight exists in the resulting sub-graph.

This iterative process requires a complete bipartite graph with left and right vertices of the same size and nonnegative edge weights. The solution process is as follows: The first step is to construct a sub-graph by choosing a potential for the left vertices so that edges with zero weight arise. In the second step a matching with maximum number of edges is searched for in this sub-graph. This is done by an iterative improvement of an initial matching along a prior labelled path. If the result of this improvement process is a complete matching this is an optimal matching with minimum overall weight.

If the matching is incomplete, the minimum weight of all edges with labels on the left side and no labels on the right side is the new vertex potential. The edge weights are updated with this potential and a new sub-graph results, where a new search for a matching with maximum number of edges starts until an optimal matching with minimum overall weight is found.[27]

The requirement of nonnegative edge weights is fulfilled by the choice of the aggregate distance measure, which has exclusively positive distance values. Obviously, implementing this algorithm avoids the problem of losing observations due to the design of the assignment process and yields an optimal result.

25 No significant differences in means and distributions of the covariates between participants and non-participants are found for all (sub-) samples.

26 A vertex potential is the valuation of the vertices with real numbers to allow for manipulations of the edge weight without changing the optimal solution.

27 For a detailed description of this assignment algorithm see e. g. Bazaraa et al. (1990), pp. 499–508.

To check the quality of the matching result, we test if differences in the means and distributions of the characteristics in the treatment and the non-treatment group arise.[28] No significant differences – neither of the means nor the distributions of the covariates – between both groups are found for all (sub-) samples after matching. Table 1 on pages 600 f. shows the tests for the whole sample as an example.[29]

8. Duration Analysis

One possible indicator for the impact of labour market programmes is the change in the duration a person is unemployed. Usually it is adequate to compare the means of the matched participation and the non-participation outcome. However, a simple comparison of average participants' and non-participants' unemployment durations is not the appropriate approach for three reasons: the main reason is the existence of censored spells, i.e. unemployment durations that are not finished at the interview time. Second, the unemployment spells start in different periods. Thus, labour market conditions may vary between different persons. The third problem is the change in the composition of the groups, because some persons take up employment and are not considered for the whole observation period. This is why the distribution of characteristics in the participants' and the non-participants' groups may differ over time.

One possible approach to deal with this kind of problems is to apply a survival analysis. The outcome variable here is the unemployment duration until an observed person changes into employment, the so called survival function. To estimate the survival function $S(t) = P(T > t)$, we use the Kaplan-Meier-estimator:[30]

$$\hat{S}(t) = \prod_{j|t_j \leq t} \frac{n_j - d_j}{n_j}, \tag{5}$$

where n_j denotes the number of individual spells at risk at time t_j and d_j the number of failure spells at t_j. In our study only the change of the initial status

28 Differences in means are checked by t-tests, for the distributions we applied KS-tests (for metrical variables) and chi-square-tests (nominally scaled variables), respectively.

29 The tests for all the sub-samples are presented in Reinowski, Schultz and Wiemers (2004), Table A.2 in the Appendix.

30 See Kaplan and Meier (1958), p. 463.

of unemployment to employment is defined as a failure (and thus, the unemployment spell is completed). All other unemployment spells are considered as censored.

9. Results

The aim of the study is to evaluate the effects of further training on the individual unemployment duration of different groups of persons representing individual characteristics and some aspects of the economic environment. We analyse the whole sample as well as the sub-sample of long-term unemployed persons. Additionally, we divide our sample in different sub-samples by gender, education, age, beginning of the unemployment period and duration of the measures.

The results show a negative influence of further training on employment chances, with gradual differences in the analysed groups. For lack of space we present only the results for the whole sample and the gender sub-samples. When distinguishing between different times for the beginning of the unemployment period, we find interesting results. Therefore, we present especially these results in detail. The other sub-samples can be seen in Figures A.1 to A.8 in the Annex.

The Figures show the estimated survival function, i.e. the probability of being unemployed for each quarter after the beginning of the unemployment spell. The dashed line identifies participation, the solid line the situation of non-participation. Fine lines show the 95% confidence interval for both cases, participation and non-participation.[31] The Figures reveal that the influence of participation differs across our sub-samples.

As can be seen in Figure 2, over the whole sample the participation in further training has a negative influence on the employment probability. In case of non-participation 65% of the persons find a job within three quarters while in case of participation only 8% do. After twelve quarters about 45% of the participants are still not employed. In case they had not participated in the measure the rate of persons not employed would only be 16%.

31 The confidence intervals should not be used to draw inferential conclusions about the equality of median survival times for both groups, see Hosmer and Lemeshow (1999), p. 156.

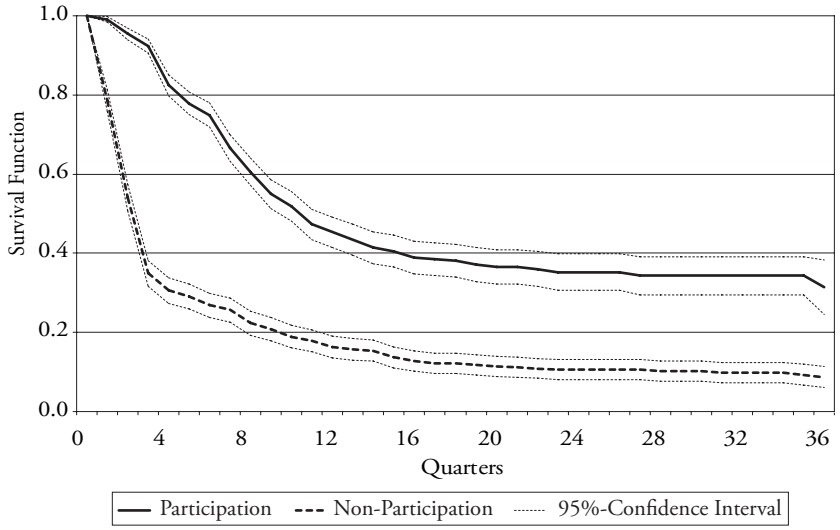
Table 1: Comparison of Means and Distributions of Participants (P) and Non-participants (NP) before and after Matching

Variable	Whole Sample							
	Before Matching				After Matching			
	P[c]	Mean NP[c]	Difference[d]	Distribution[e] Test Result[d]	P[c]	Mean NP[c]	Difference[d]	Distribution[e] Test Result[d]
Gender (male = 1)	0.382 (0.486)	0.534 (0.499)	–0.151 (0.000)	63.294 (0.000)	0.382 (0.486)	0.376 (0.485)	0.006 (0.803)	0.062 (0.803)
Start of Unemployment Spell	20.168 (11.348)	25.284 (12.421)	–5.115 (0.000)	5.736 (0.000)	20.168 (11.348)	20.272 (11.509)	–0.104 (0.852)	0.655 (0.784)
Completed Apprenticeship/Technician	0.816 (0.387)	0.835 (0.371)	–0.018 (0.201)	1.636 (0.201)	0.816 (0.387)	0.834 (0.372)	–0.018 (0.338)	0.918 (0.338)
University/College Degree	0.114 (0.318)	0.080 (0.272)	0.034 (0.002)	10.044 (0.002)	0.114 (0.318)	0.108 (0.311)	0.006 (0.699)	0.149 (0.700)
Age	36.425 (5.448)	38.344 (5.546)	–1.920 (0.000)	3.850 (0.000)	36.425 (5.448)	36.431 (5.332)	–0.006 (0.984)	0.461 (0.982)
Residence Chemnitz	0.362 (0.481)	0.417 (0.493)	–0.055 (0.003)	8.669 (0.003)	0.362 (0.481)	0.369 (0.482)	–0.007 (0.763)	0.91 (0.763)
Residence Dresden	0.404 (0.491)	0.362 (0.481)	0.041 (0.024)	5.110 (0.24)	0.404 (0.491)	0.400 (0.489)	0.004 (0.805)	0.610 (0.805)
Share of Time in Unemployment[a]	0.031 (0.090)	0.094 (0.194)	–0.064 (0.000)	3.624 (0.000)	0.031 (0.090)	0.039 (0.100)	–0.008 (0.923)	0.946 (0.333)
Share of Time in Employment[a]	0.905 (0.212)	0.865 (0.245)	0.040 (0.000)	2.317 (0.000)	0.905 (0.212)	0.909 (0.217)	–0.004 (0.674)	0.970 (0.303)
Share of Time in Non-Employment[a]	0.064 (0.188)	0.040 (0.159)	0.023 (0.000)	1.889 (0.002)	0.064 (0.188)	0.059 (0.188)	0.005 (0.595)	0.703 (0.706)
Mean Duration of Unemployment	0.640 (1.830)	0.958 (2.595)	–0.317 (0.001)	2.666 (0.000)	0.640 (1.830)	0.505 (1.866)	0.135 (0.127)	0.946 (0.333)

Variable	Whole Sample							
	Before Matching				After Matching			
	P[c]	Mean NP[c]	Difference[d]	Distribution[e] Test Result[d]	P[c]	Mean NP[c]	Difference[d]	Distribution[e] Test Result[d]
Mean Duration of Employment	13.747 (9.566)	15.564 (12.131)	–1.817 (0.000)	3.076 (0.000)	13.747 (9.566)	14.133 (9.504)	–0.386 (0.405)	0.800 (0.544)
Mean Duration of Non-Employment	0.955 (2.862)	0.602 (2.633)	0.353 (0.001)	1.889 (0.000)	0.955 (2.862)	0.94 (3.432)	0.015 (0.922)	0.703 (0.706)
Labour Market Status t – 1[b] (Employment = 1)	0.924 (0.266)	0.943 (0.231)	–0.020 (0.028)	4.807 (0.028)	0.924 (0.266)	0.926 (0.262)	–0.002 (0.854)	0.340 (0.854)
Labour Market Status t – 2[b] (Employment = 1)	0.908 (0.289)	0.922 (0.267)	–0.014 (0.169)	1.892 (0.169)	0.908 (0.289)	0.921 (0.269)	–0.013 (0.340)	0.912 (0.340)
Labour Market Status t – 3[b] (Employment = 1)	0.898 (0.303)	0.888 (0.316)	0.010 (0.397)	0.718 (0.397)	0.898 (0.303)	0.909 (0.287)	–0.011 (0.265)	0.675 (0.411)
Labour Market Status t – 4[b] (Employment = 1)	0.888 (0.315)	0.847 (0.360)	0.041 (0.002)	9.354 (0.002)	0.888 (0.315)	0.905 (0.293)	–0.017 (0.265)	1.242 (0.265)
Labour Market Status t – 5[b] (Employment = 1)	0.879 (0.326)	0.903 (0.297)	–0.024 (0.039)	4.269 (0.039)	0.879 (0.326)	0.893 (0.309)	–0.014 (0.360)	0.838 (0.360)
Labour Market Status t – 6[b] (Employment = 1)	0.878 (0.328)	0.895 (0.307)	–0.017 (0.154)	2.036 (0.154)	0.878 (0.328)	0.888 (0.315)	–0.01 (0.497)	0.461 (0.497)
Long Term Unemployed	0.280 (0.449)	0.442 (0.497)	–0.162 (0.000)	75.245 (0.000)	0.280 (0.449)	0.303 (0.460)	–0.023 (0.286)	1.139 (0.286)

a Time spent in the respective employment status relative to the time until the start of the considered unemployment spell
b "t – n" denotes the number of quarters until the start of the considered unemployment spell
c standard deviation in brackets
d *p*-value in brackets
e for metrical scaled variables KS-test; for nominal scaled variables chi-square test.

Figure 2: Survival Functions in Participation and Non-participation Case; Whole Sample



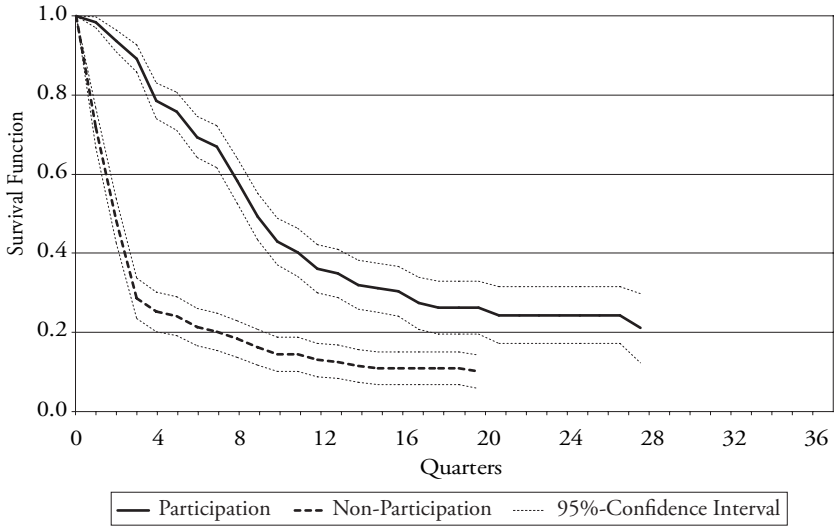
Source: Micro Census Saxony, own calculations.

Comparing Figures 3 and 4 demonstrates that the participation effect is negative particularly for women. While the non-participation curve of men and women is similar,[32] the participation in further training noticeably delays women's transition to employment compared to men. After four quarters 22% of male participants and 10% of female participants are employed. The ratio increases to about 55% and 39% for men and women, respectively after ten quarters. Over a longer time horizon the share of not employed female participants exceeds that of male participants (43% and 28%, respectively after twenty quarters).

Our results for three sub-samples, which describe different beginnings of unemployment spells, show a very interesting drift of participation effects with respect to the effectiveness of further training (Figures 5 to 7). This drift can be explained by a changing economic and legal basis during our observational period. Three different periods can be identified: the first period starts in 1989

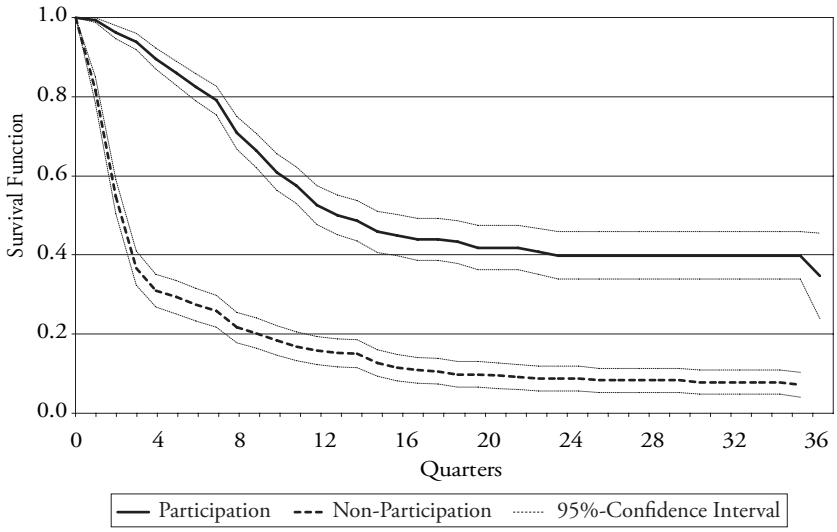
32 After four quarters 74% of the observed men and 68% of the observed women are employed; after 10 quarters the share is 84% and 80%, respectively.

Figure 3: Survival Functions in Participation and Non-participation Case; Men



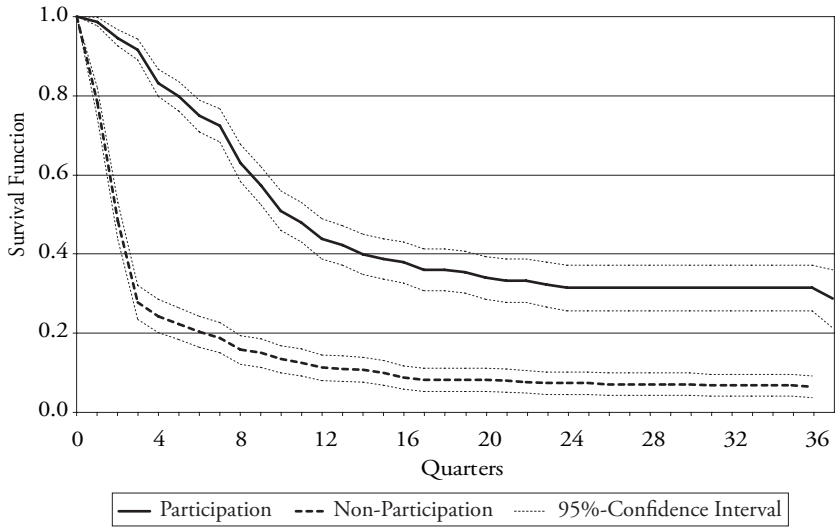
Source: Micro Census Saxony, own calculations.

Figures 4: Survival Functions in Participation and Non-participation Case; Women



Source: Micro Census Saxony, own calculations.

Figure 5: Survival Functions in Participation and Non-participation Case; Start of the Unemployment Spell until 1992



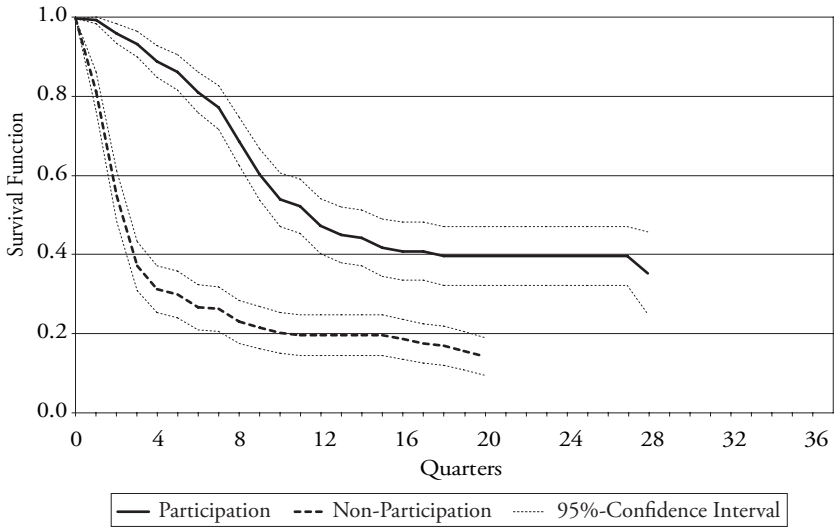
Source: Micro Census Saxony, own calculations.

and ends about 1992. This period is characterised by the transformation process in East Germany. One political answer to the changing conditions on the labour market was a large implementation of further training (see also Figure 1) which was mainly used to ease the pressure on the labour market. The implemented programmes were not differentiated regarding personal, regional or economic requirements.

The second period begins around 1993 and ends about 1996. Practice in the Federal Employment Office and Training Agencies began to change which led to a decreasing number of participants in training programmes. Therefore, it could have been easier to adjust the programmes to the labour demand requirements but de facto there was no major focus on integration of participants into regular employment. Instead Further Training was mainly used to extend the duration of unemployment benefits.

In the third period which starts around 1997 the training policy was modified by introducing the so called 'target group focussing'. Now subsidies on further training measures were primarily granted to specific target groups like long-term unemployed and older or younger persons without professional skills. Local

Figure 6: Survival Functions in Participation and Non-participation Case; Start of the Unemployment Spell between 1993 and 1996



Source: Micro Census Saxony, own calculations.

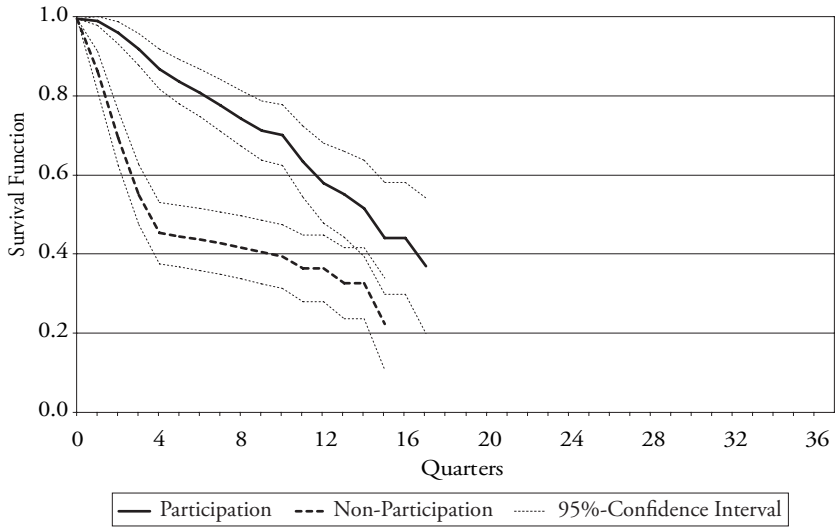
employment offices continued to plan training programmes but regional labour demand was not part of the consideration.

In all three periods participation in further training results in a prolongation of unemployment duration compared to the situation of non-participation. But there are some remarkable changes in the shape of the curves.

Especially during the first period until 1992 a very fast drop out of unemployment for non-participation can be observed (see Figure 5). A large divergence between the survival curves can already be noticed after three quarters. The survival curves begin to converge afterwards but in the long run the difference between the two remains at about 20%.

The shape of the curves can be explained by the developments in the first period described above. Since the participants in further training programmes had a large share in the total number of unemployed persons in this period, it is possible that programmes affected the regular labour market. Thus, the fundamental assumption for microeconomic evaluation, the SUTVA, may be violated. In this case an additional macroeconomic analysis would be appropriate, but this is beyond the scope of this paper.

Figure 7: Survival Functions in Participation and Non-participation Case; Start of the Unemployment Spell from 1997



Source: Micro Census Saxony, own calculations.

As can be seen in Figure 6, in the second period from 1993 to 1996 effects of further training are similar to those in the first period. Participants and their hypothetical counterparts changed slightly slower into employment. A possible explanation for this difference is that target group focussing was gradually implemented then. Therefore, persons with lower employment chances often participated in training programmes. In the long run, the gap between both survival curves is nearly the same as in the first period.

Figure 7 shows that the survival functions changed considerably in the third period since 1997. The survival curve of participants is relatively linear, unlike the respective curve for the second period. Instead of a fading out, the participants' survival function becomes even steeper after the tenth quarter. Moreover, the non-participation survival curve shows a slower decline from the third quarter than in the period before and has a concave instead of a convex shape afterwards. The shape of both curves implies a smaller difference between the participation and non-participation outcome. We cannot observe the further development of the survival functions, because the observation time ends already after 17 quarters.

This change relative to the previous period may be a result of a more rigid implementation of target group focussing. We can also observe this trend in our data, e.g. the share of long-term unemployed persons changed from 24% in the first period to nearly 33% in the third period. In other target groups we cannot identify changes due to our selection of spells.

The results for the third group could be taken as a hint that further training is more successful if policy is focussed on specific target groups. This may indicate the direction to improve the effectiveness of training programmes.

In our analysis of the whole sample and the above described sub-samples we find a negative influence of further training on employment chances, with gradual differences in the analysed groups. These results are slightly worse than those of other recent evaluation studies which find insignificant effects (Fitzenberger, 2001, 2004, Lechner, 2000).

10. Conclusions

In this study we have evaluated the employment effects of further training programmes for Saxony between 1990 and 2001. Our methodological approach differs in three aspects from other studies in the literature. First, we follow the concept of perforated unemployment which implies that the duration of the programme is included in the total time of unemployment. This approach improves the comparability of the situation of participation and the hypothetical situation of non-participation. Second, we use the prehistory of the employment status. The structure and duration of employment and unemployment periods is used as an indicator of the probability of changing into employment before the start of the programme. Thereby we avoid heterogeneity between participants and non-participants and at the same time we eliminate Ashenfelter's Dip. Third, we employ the Hungarian algorithm for matching, which provides an optimal full assignment. This technique avoids the problem of losing observations due to the design of the assignment process and yields an optimal result as is required for an appropriate assignment procedure.

Since in the literature analyses of whether the effect of participation in a programme is influenced by individual characteristics or economic environment are rarely found, we evaluated the employment effects of further training programmes for different sub-samples representing individual characteristics as well as some aspects of the economic environment. The results of our evaluation show a negative effect of participation in further training programmes – with gradual differences in the sub-samples. These results are similar to the findings of other evaluation studies.

This can be interpreted as a first indication that the employment prospects of the participants are influenced by personal characteristics, economic environment and the organisational design of training measures. Further research should focus on institutional factors like entrance requirements, the subjects of the courses, their adjustment to regional demand, practical work experience during the measure. With this information it would be possible to detect potentially successful measures.

Annex

Figures A.1–A.8: Survival Functions in Participation and Non-participation Case for the Sub-samples

Figure A.1: Younger than 40

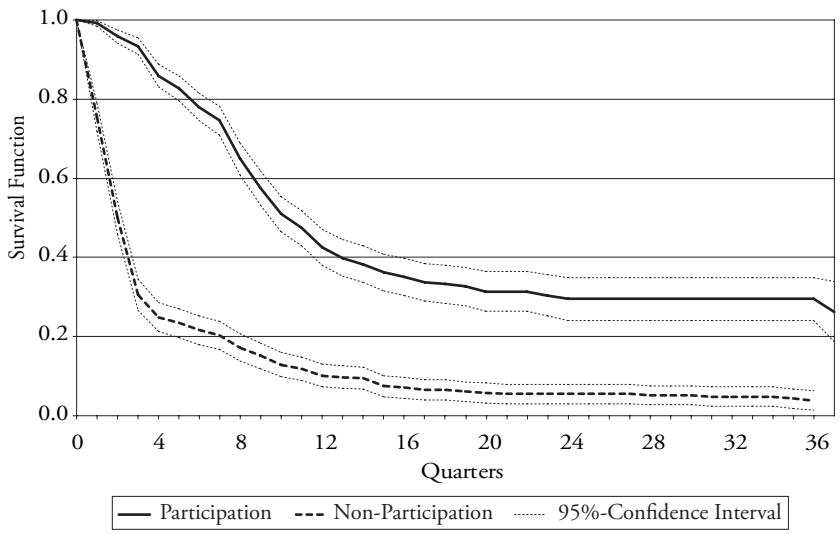


Figure A.2: Older than 40

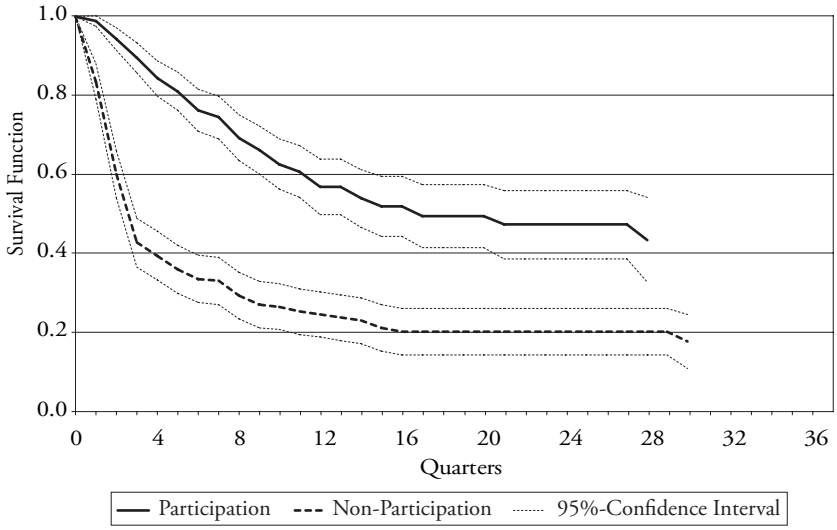


Figure A.3: Skilled

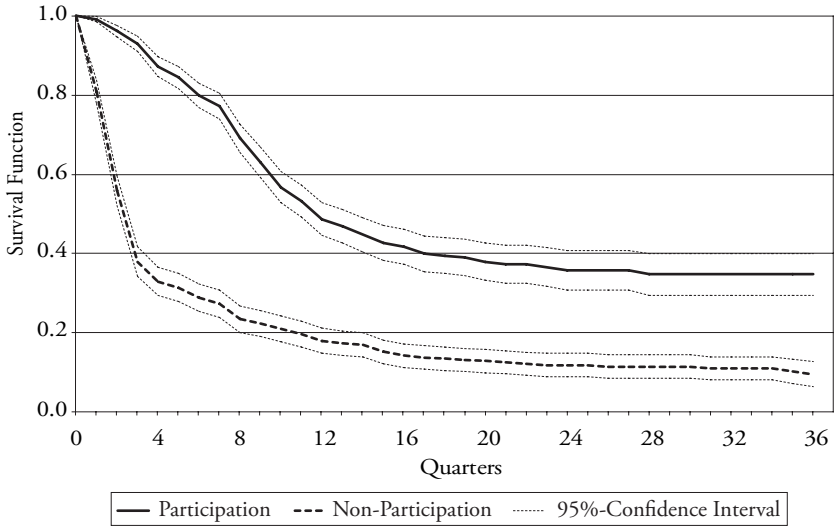


Figure A.4: High Skilled

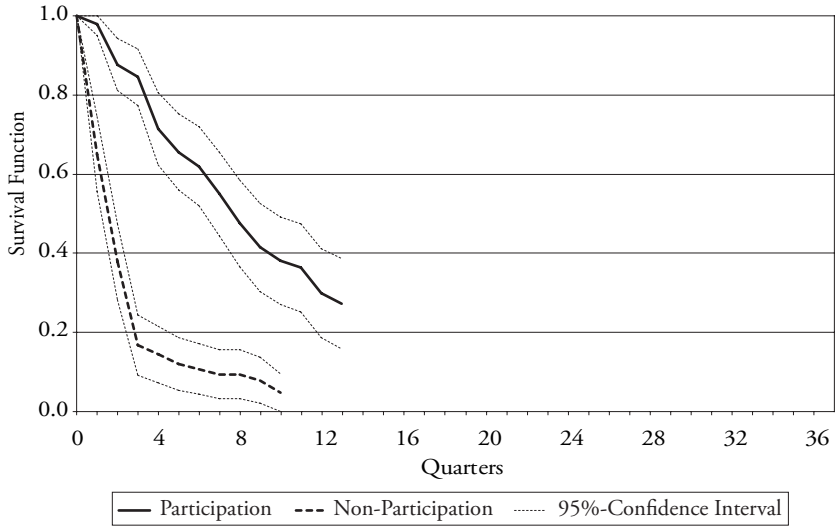


Figure A.5: Measure Shorter than 4 Quarters

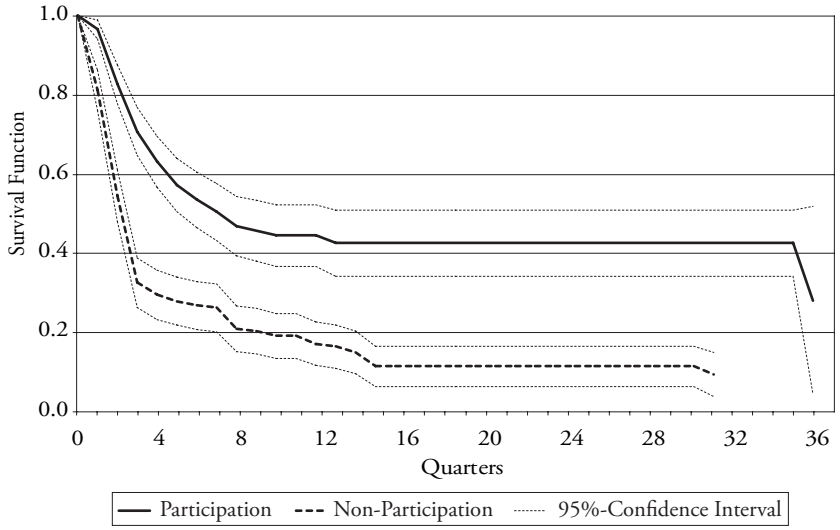


Figure A.6: Measure 4 to 7 Quarters

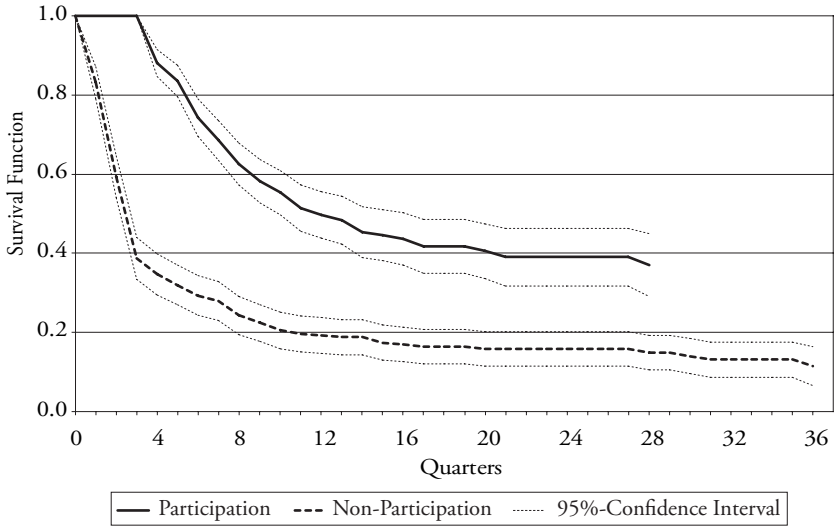


Figure A.7: Measure Longer than 7 Quarters

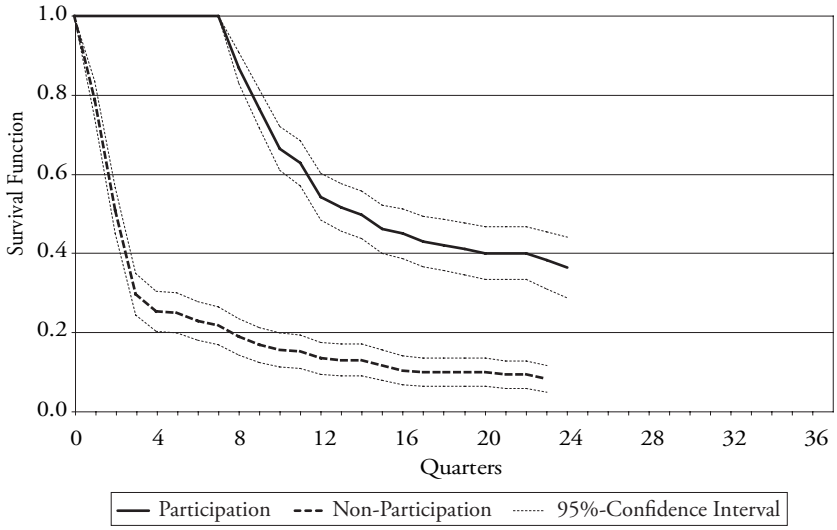
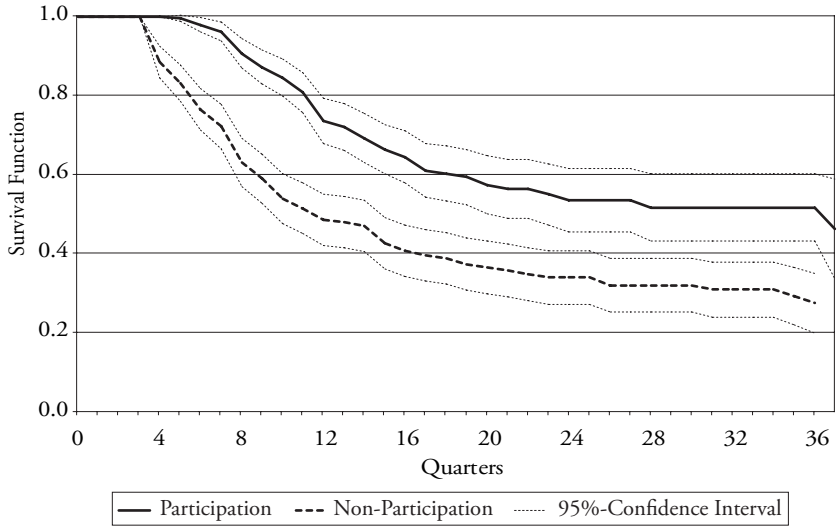


Figure A.8: Long Term Unemployed



References

Allison, P. (1984), "Event History Analysis. Regression for Longitudinal Event Data", *Sage University Paper Series on Quantitative Applications in the Social Sciences 07-046*, Beverly Hills: Sage Publications.

Ashenfelter, O. (1978), "Estimating the Effect of Training Programs on Earnings", *Review of Economics and Statistics*, 60, pp. 47–57.

Bazaraa, M., J. Jarvis and H. Sheraldi (1990), "Linear Programming and Network Flows", 2nd ed. New York: John Wiley & Sons.

Bergemann, A., B. Fitzenberger, B. Schultz and S. Speckesser (2000), "Multiple Active Labor Market Policy Participation in East Germany. An Assessment of Outcomes", *Konjunkturpolitik*, Beiheft Nr. 51, pp. 195–243.

Bergemann, A., B. Fitzenberger and S. Speckesser (2001), "Evaluating the Employment Effects of Public Sector Sponsored Training in East Germany: Conditional Difference-in-Differences and Ashenfelter Dip", Discussion Paper, Mimeo, Mannheim, http://www.vwl.uni-mannheim.de/ls_oek/forschung_index. htm.

Bergemann, A., B. Fitzenberger and S. Speckesser (2004), "Evaluating the Dynamic Employment Effects of Training Programs in East-Germany Using

Conditional Difference-in-Difference", *ZEW-Discussion Paper No. 04-41*, Zentrum für Europäische Wirtschaftsforschung GmbH, Mannheim.

Büchel, F. (1992), „Die Qualität der Wiederbeschäftigung nach ununterbrochener und nach ‚perforierter' Langzeitarbeitslosigkeit", *Beiträge aus der Arbeitsmarkt- und Berufsforschung*, 162.

Bundesanstalt für Arbeit (2004a), „Arbeitsmarkt in Zahlen. Strukturanalyse", http://www1.arbeitsamt.de/hst/services/statistik/200312/iiia4/st4-vorld.pdf (2004–1–13).

Bundesanstalt für Arbeit (2004b), „Beschäftigte. Gesamtübersicht", http://www1.arbeitsamt.de/hst/services/statistik/aktuell/iiia6/sozbe/q6001d.xls (2004–1–13).

Bundesanstalt für Arbeit (2004c), „Geringfügig entlohnte Beschäftigte. Gesamtübersicht", http://www1.arbeitsamt.de/hst/services/statistik/aktuell/iiia6/ sozbe/gq6001d.xls (2004–1–13).

Christensen, B. (2001), „Berufliche Weiterbildung und Arbeitsplatzrisiko: Ein Matching-Ansatz", *Kieler Arbeitspapier Nr. 1033*, Institut für Weltwirtschaft, Kiel.

Eichler, M. and M. Lechner (2000), "Some Econometric Evidence oh the Effectiveness of Active Labour Market Programmes in East Germany", *The Davidson Institute Working Paper No. 318*, The William Davidson Institute, Michigan.

Eichler, M. and M. Lechner (2001), "Public Sector Sponsored Continuous Vocational Training in East Germany: Institutional Arrangements, Participants and Results of Empirical Evaluations", in: Riphahn, R., D. Snower and K. Zimmermann (eds), *Employment Policy in Transition: The Lessons from German Integration for the Labour Market,* Berlin: Springer-Verlag, pp. 208–253.

Fitzenberger, B. and H. Prey (1998), „Beschäftigungs- und Verdienstwirkungen von Weiterbildungsmaßnahmen im ostdeutschen Transformationsprozeß: Eine Methodenkritik", in: Pfeiffer, F. and W. Pohlmeier (eds), *Qualifikation, Weiterbildung und Arbeitsmarkterfolg*, Baden-Baden: Nomos Verl.-Ges., pp. 39–95.

Fitzenberger, B. and H. Prey (2000), "Evaluating Public Sector Sponsored Training in East Germany", *Oxford Economic Papers*, 52, pp. 497–520.

Fröhlich, M. (2004): "Programme Evaluation with Multiple Treatments", *Journal of Economic Surveys*, 18, pp. 181–224.

Fröhlich, M. (2000), "Treatment Evaluation: Matching versus Local Polynomial Regression", *Discussion Paper No. 2000-17*, Department of Economics, University of St. Gallen, St. Gallen.

Gerfin, M. and M. Lechner (2002), "A Microeconomic Evaluation of the Active Labour Market Policy in Switzerland", *The Economic Journal*, 482, pp. 854–893.

Heckman, J., H. Ichimura and P. Todd (1997), "Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Program", *Review of Economic Studies*, 64, pp. 605–654.

Heckman, J., R. LaLonde and J. Smith (1999), "The Economics and Econometrics of Active Labor Market Programs", in: O. Ashenfelter and D. Card (eds), *Handbook of Labor Economics*, Vol. III, Amsterdam: Elsevier Science B.V., pp. 1865–2097.

Heckman, J. and J. Smith (1999), "The Pre-Program Earnings Dip and the Determinants of Participation in a Social Program: Implications for Simple Program Evaluation Strategies", *Economic Journal*, 109, pp. 313–348.

Hosmer, D. and S. Lemeshow (1999), "Applied Survival Analysis – Regression Modeling of Time to Event Data", New York: John Wiley & Sons.

Hübler, O. (1997), „Evaluation beschäftigungspolitischer Maßnahmen in Ostdeutschland", *Jahrbücher für Nationalökonomie und Statistik*, Bd. 216, pp. 21–44.

Hübler, O. (1998), „Berufliche Weiterbildung und Umschulung in Ostdeutschland – Erfahrungen und Perspektiven", in: F. Pfeiffer and W. Pohlmeier (eds), *Qualifikation, Weiterbildung und Arbeitsmarkterfolg*, Baden-Baden: Nomos Verl.-Ges., pp. 97–132.

Hujer, R. and M. Caliendo (2000), "Evaluation of Active Labour Market Policy: Methodological Concepts and Empirical Estimates", *IZA Discussion Paper No. 236*, Forschungsinstitut zur Zukunft der Arbeit, Bonn.

Hujer, R., M. Caliendo and D. Radić (2001), "Nobody Knows… How Do Different Evaluation Estimators Perform in a Simulated Labour Market Experiment?", Discussion Paper, http://much-magic.wiwi.uni-frankfurt.de/Professoren/hujer/ (2002–5–13).

Hujer, R., K.-O. Maurer and M. Wellner (1997), „Kurz- und langfristige Effekte von Weiterbildungsmaßnahmen auf die Arbeitslosigkeitsdauer in Westdeutschland: Eine empirische Analyse mit diskreten Hazardraten-Modellen", *Frankfurter Volkswirtschaftliche Diskussionsbeiträge No. 79*, Johann Wolfgang Goethe University, Frankfurt/Main.

Hujer, R. and M. Wellner (2000), "The Effects of Public Sector Sponsored Training on Individual Employment Performance in East Germany", *IZA Discussion Paper No. 141*, Forschungsinstitut zur Zukunft der Arbeit, Bonn.

Kaplan, E. and Meier, P. (1958), "Non-parametric Estimation from Incomplete Observations", *Journal of the American Statistical Association*, 53, pp. 457–481.

Kaufmann, H. and Pape, H. (1996), „Clusteranalyse“, in: Fahrmeir, L., A. Hamerle and G. Tutz (eds), *Multivariate statistische Verfahren*, 2nd ed. Berlin: de Gruyter, 437–536.

Klein, J. and M. Moeschberger (1997), "Survival Analysis. Techniques for Censored and Truncated Data", New York: Springer-Verlag.

Kraus, F., P. Puhani and V. Steiner (1999), "Employment Effects of Publicly Financed Training Programmes – The East German Experience", *Jahrbücher für Nationalökonomie und Statistik*, Bd. 219, pp. 216 –248.

Kuhn, H. (1955), "The Hungarian Method for Solving the Assignment Problem", *Naval Research Logistics Quarterly*, 2, pp. 83–97.

Lechner, M. (2000), "An Evaluation of Public Sector Sponsored Continuous Vocational Training Programs in East Germany", *The Journal of Human Resources*, 35, pp. 347–375.

Lechner, M. (1998), "Training the East German Labour Force. Microeconometric Evaluations of Continuous Vocational Training after Unification", Heidelberg: Physica-Verlag.

Opitz, O. (1980), *Numerische Taxonomie*, Stuttgart, New York: Fischer-Verlag.

Pannenberg, M. (1996), „Zur Evaluation staatlicher Qualifizierungsmaßnahmen (FuU) in Ostdeutschland: Das Instrument Fortbildung und Umschulung“, *Diskussionspapier Nr. 38*, Institut für Wirtschaftsforschung Halle, Halle.

Reinowski, E., B. Schultz and J. Wiemers (2004), "Evaluation of Further Training Programmes with an Optimal Matching Algorithm", *IWH Discussion Paper No. 188*, Institut für Wirtschaftsforschung Halle, Halle.

Rosenbaum, P. and D. Rubin (1983), "The Central Role of the Propensity Score in Observational Studies for Causal Effects", *Biometrika*, 70, pp. 41–55.

Rosenbaum, P. and D. Rubin (1985), "Constructing a Control Group Using Multivariate Matched Sampling Methods that Incorporate the Propensity Score", *The American Statistician*, 39, pp. 33–39.

SUMMARY

This study evaluates the effects of further training on the individual unemployment duration of different groups of persons representing individual characteristics and some aspects of the economic environment. The Micro Census Saxony enables us to include additional information about a person's employment history to eliminate the bias resulting from unobservable characteristics and to avoid Ashenfelter's Dip. In order to solve the sample selection problem we employ an

optimal full matching assignment, the Hungarian algorithm. The impact of participation in further training is evaluated by comparing the unemployment duration between participants and non-participants using the Kaplan-Meier-estimator. Overall, we find empirical evidence that participation in further training programmes results in even longer unemployment duration.

ZUSAMMENFASSUNG

In dieser Studie werden die Effekte von öffentlich geförderten Weiterbildungsmaßnahmen auf die individuelle Arbeitslosigkeitsdauer für verschiedene Gruppen von Personen evaluiert, die sich in ihren persönlichen Merkmalen und einigen ökonomischen Aspekten unterscheiden. Mit Hilfe des Mikrozensus Sachsen ist es möglich, zusätzliche Informationen über die Erwerbsgeschichte der Personen einzubeziehen, so dass Verzerrungen aufgrund unbeobachtbarer Heterogenität und des Ashenfelter's Dip vermieden werden. Um das Zuordnungsproblem zu lösen, wird als Matchingverfahren der ungarische Algorithmus eingesetzt. Der Einfluss der Teilnahme an Weiterbildungsmaßnahmen wird durch einen Vergleich der Arbeitslosigkeitsdauer zwischen Teilnehmern und Nichtteilnehmern mit Hilfe des Kaplan-Meier-Schätzers evaluiert. Die Ergebnisse der empirischen Untersuchung deuten darauf hin, dass die Teilnahme an Weiterbildungsprogrammen in allen betrachteten Personengruppen zu einer längeren Arbeitslosigkeitsdauer führt.

RÉSUMÉ

Cette étude évalue l'effet des programmes publics de formation continue sur la durée du chômage des individus, en tenant compte que les personnes se distinguent par leurs traits individuels et leur situation économique. Avec l'échantillon «Mikrozensus» de Saxe qui contient des informations sur l'histoire de l'activité professionnelle, il est possible d'éviter le biais dû à une hétérogénéité non-observée ainsi que le «dip de Ashenfelter». Afin de résoudre le problème de classement, on utilise l'algorithme hongrois comme méthode de «matching». L'effet de la participation à un programme public de formation continue est estimé en comparant avec un estimateur de Kaplan-Meier la durée de chômage des participants et des non-participants. Les résultats empiriques suggèrent que la participation dans un programme public de formation continue résulte en une prolongation de la durée de chômage.