Estimating the Effect of Training on Unemployment Duration in West Germany

– A Discrete Hazard-Rate Model with Instrumental Variables –

by

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ABSTRACT

We estimate a semiparametric single-risk discrete-time duration model to assess the effect of vocational training on the duration of unemployment spells. The data basis used in this study is the German Socio-Economic-Panel (GSOEP) for West Germany for the period from 1986 to 1994. To take into account a possible selection bias actual participation in vocational training is instrumented using estimates of a random-effects probit model for the participation in qualification measures. Our main results show that training does have a significant short term effect of reducing unemployment duration but that this effect does not persist in the long run.

Keywords: discrete hazard models, training, selection bias, instrumental variables

JEL classifications: C41, J20, J64

I. Introduction[1]

In view of persistent high unemployment and – at least during the eighties – lengthening unemployment durations in most developed countries and the ever accelerating technical and technological change affecting society in general and the labour market in particular, education and on-the-job resp. off-the-job training are more and more often considered to be of utmost importance for individual labour market prospects. At the same time, firms and public institutions are reducing their expenditures, including those for qualification measures, to save costs and lower budget deficits.

In Germany, the budget of the *Bundesanstalt für Arbeit* (Federal Employment Agency), which is responsible for the payment of unemployment benefits and for the implementation of active labour market policy, including the financing of further vocational training (*Fortbildung*) and retraining (*Umschulung*), is critically reviewed by politicians. In addition, an amendment of the Work Support Act (*Arbeitsförderungsgesetz*) is currently being discussed which – among other aims – should improve the reemployment prospects of those unemployed by encouraging the use of training measures (DEUTSCHER BUNDESTAG (1996), p.144). This trade-off, the need to lower unemployment while being compelled to cut back public expenditures, explains the great interest in the evaluation of the effectiveness of training programs in general.

In principle, there are various outcome variables that could serve as an indicator for the impact of training. The empirical literature so far mainly evaluates effects on individuals' income or employment probabilities but other outcomes like employment duration (as an indicator of employment stability), unemployment duration or promotion/seniority effects are also important.

Despite the notable public interest, there are only a few recent empirical studies for Germany concerned with the impact of training and most of them concentrate on East Germany.[2,3]

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1 The authors want to thank especially Dr. Hilmar Schneider, Institute for Economic Research Halle, for the permission to use his GAUSS library for the estimation of discrete hazard models. We also profited from his helpful comments and suggestions and those of Dr. Joachim Grammig and Ralf Grossmann, both University of Frankfurt.

2 For a more detailed discussion, also including studies for other countries, see DOLTON (1994) and HUJER/MAURER/WELLNER (1996B).

FITZENBERGER/PREY (1995) estimate a simultaneous random-effects probit model based on the Arbeitsmarktmonitor (Ost), a panel labour market survey run by the Federal Employment Agency in East Germany. Their data allow them to distinguish between two types of training: training within and training outside a firm. For the period from 1989 to 1992 their results indicate that training outside the firm has a positive effect on the employment probability, whereas training within the firm only has a positive short run effect, the long run effect is actually negative. Refining their first model by also looking at wage effects and further differentiating training measures, **FITZENBERGER/PREY (1996)** find mostly positive effects on employment or wages.

LECHNER (1995) concentrates his analysis of training effects on the evaluation of off-the-job training. Using the German Socio-Economic Panel (GSOEP) for East Germany 1990-1994 and a statistical approach that incorporates a matching procedure his results show no robust positive effects of training on employment and income. However, his results are based on a fairly small number of trainees. In two later papers, **LECHNER (1996A, 1996B)** evaluates public sector sponsored and enterprise related vocational training. Basically, his earlier results carry over with the exception that enterprise related vocational training seems to have a positive effect on earnings. These all are short term effects, long term effects cannot yet be observed, as LECHNER (1995, p.55) stresses.

Although not strictly dealing with training, the study by **STEINER/KRAUS (1995)** should be mentioned as well as it tries to answer a similar question. The authors examine, whether participation in employment schemes (*Arbeitsbeschaffungsmaßnahmen*), which have been an important instrument of labour market policy in East Germany so far, has any reemployment effects. Also using data from the Arbeitsmarktmonitor the authors find a positive short-run effect for men and a negative effect for women.

The results of all the studies reviewed so far cannot simply be transferred to the case of West Germany as the situation in East Germany is still very different from that in the western part because of the on-going transformation process after reunification in 1990. As mentioned before

[3] The main reason for the relative popularity of East Germany as opposed to West Germany is that the labour market situation since reunification in 1990 has rapidly declined in East Germany and qualification measures were a major counteraction used by the Federal Employment Agency. Thus, data sets for East Germany show a much higher proportion of participants than those for West Germany.

there are only a few recent studies for West Germany that try to assess the impact of training. The analysis of **HUJER/SCHNEIDER (1990)** is based on the first four waves of the GSOEP for West Germany covering the time period from 1983 to 1986. They estimate a parametric hazard model of a Weibull type and their results show a positive short run reemployment effect of training for unemployed men. However, their study is severely limited by the number of observations. **BECKER (1991)** examines three cohorts of West Germans using a simple Cox-model and notices positive effects for successfully completed training programs on seniority. **PISCHKE (1994)** conducts a detailed analysis of vocational training activities and effects for West Germany based on the GSOEP for 1986-1989. His results show that prior participation in vocational training fails to have any significant effect on wages. In a very comprehensive study for both West and East Germany, **PANNENBERG (1995)** analyses the effects of training on different outcomes like income, reemployment probabilities and mobility within and between firms and finds mostly positive effects for both regions.

However, all four studies for West Germany fail to take into account the intriguing problem of *sample selection bias* that affects every empirical study of (training) program effects that uses nonexperimental data: The groups of program participants (trainees) and non-participants may not be random samples from the population of interest. If, for instance, typically people, who experience longer unemployment spells, participate in a training program, a simple post program comparison of mean unemployment durations of trainees and non-trainees is likely to underestimate the true program effect.

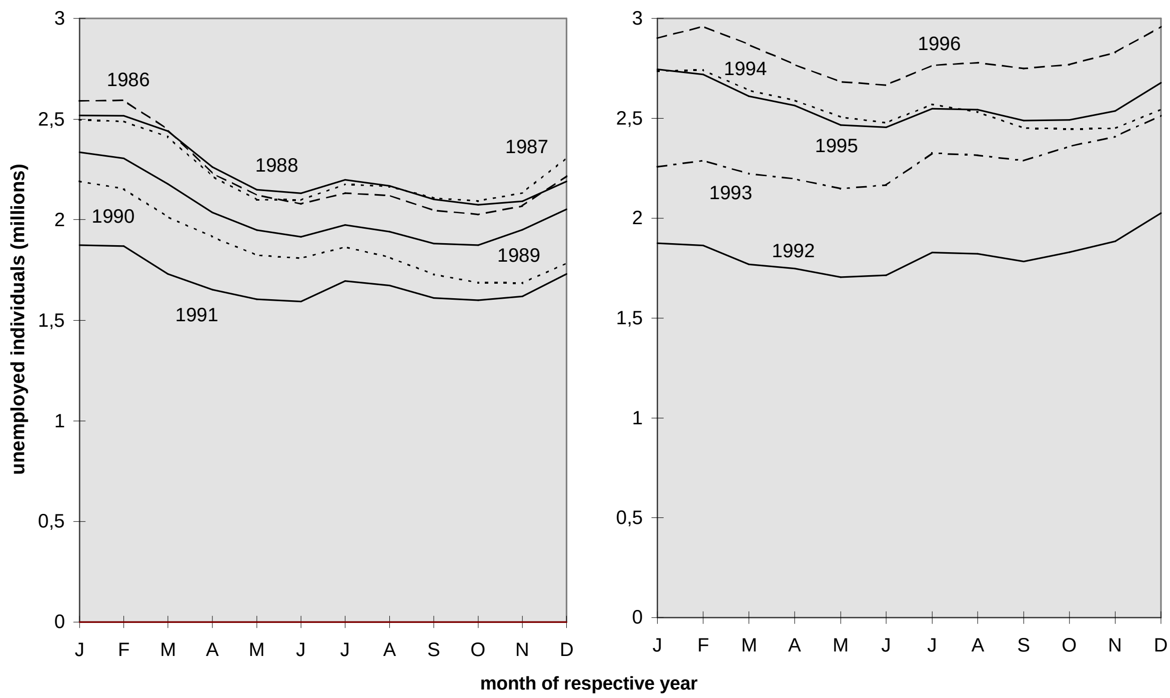
Concluding this short overview, one can say that there is still a great need for evaluations of training effects in West Germany. In the remainder of this paper, we will try to assess these effects. Our study is based on the Socio-Economic Panel (GSOEP) for West Germany and spans the time period between 1986 and 1993. As the lengthening of unemployment durations is one of the main labour market problems we concentrate our analysis on the reemployment effect of training, i.e. the effect of qualification measures on the duration of unemployment. The outcome "unemployment duration" as opposed to employment probabilities, for instance, has the additional advantage of providing a continuous longitudinal information. We estimate a semi-parametric single-risk discrete time hazard model for the transition from unemployment into employment. This method is also used e.g. by MEYER (1990), NARENDRANATHAN/STEWART (1993) and more recently HUJER/SCHNEIDER (1996). To take into account the sample selection problem we propose an instrumentation of actual participation in a training measure using the estimates from a random-effects probit model for participation in training.

The remainder of the paper is organised as follows: The next section gives some stylized facts about the labour market and qualification measures in Germany. Methodological aspects, i.e. the econometric models used in this paper and the sample selection problem, are discussed in section 3. The empirical analysis can be found in section 4. There, we describe the data basis of our analysis and the process of generating the instrumental variables and present the results of the estimation of the training impact on unemployment duration. The last section contains a summary and an outlook for future work.

II. Some Stylised Facts about West German Labour Markets and Qualification Measures

Figure 1 shows the monthly level of unemployment in West Germany for 1986 to 1996. For every year the typical seasonal variation can readily be observed. In the first three years of the observation period in this paper the level of unemployment changed little. In contrast, the years 1989, 1990 and 1991 show a quite significant drop in unemployment figures mainly resulting from German reunification. As this special effect died away in later years and the negative reunification influences began to emerge, unemployment rose almost continuously with the increase during 1992 and 1993 being quite dramatic.

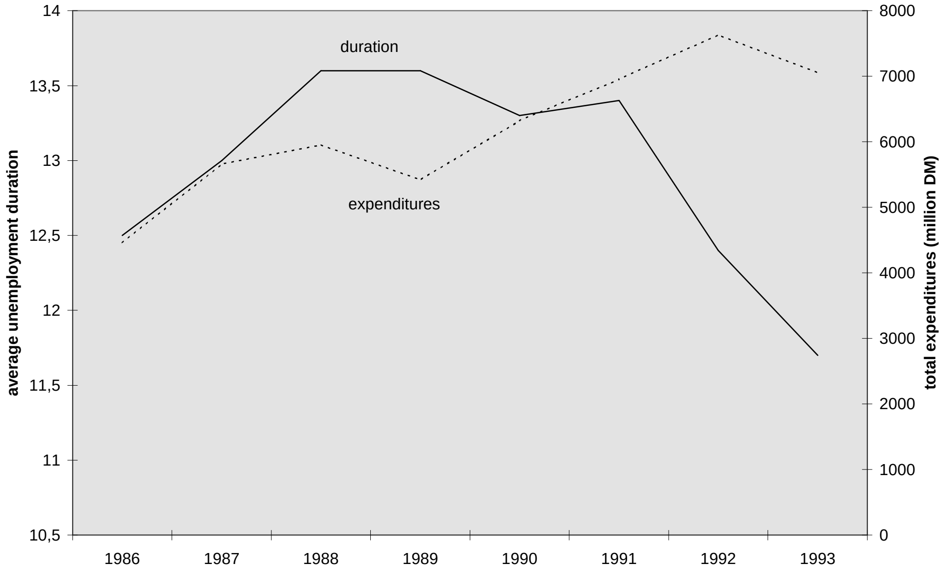
Figure 1: Unemployment in West Germany 1986-1996



Source: BUNDESANSTALT FÜR ARBEIT: Amtliche Nachrichten der Bundesanstalt für Arbeit, various issues.

Figure 2 presents the development of the average of the outcome variable of interest in this paper during the study period in West Germany. Comparing the average unemployment duration with the results from figure 1 it seems that the average duration is lagging behind the level of unemployment. For example, whereas unemployment fell during 1990 and 1991, unemployment durations decreased only well after 1990. Note that the average durations also include ongoing unemployment spells so that this statistic will be influenced by the relative inflows in and outflows out of unemployment. One central instrument of active labour market policy in Germany that is provided by the Work Support Act is the financing of vocational training, and further vocational training and retraining in particular, through the Federal Employment Agency for those currently unemployed or in danger of becoming unemployed. Therefore, *figure 2* also has the development of total expenditures of the Federal Employment Agency for vocational training and retraining in West Germany. It can be argued that expenditures are a good indicator for the scale of vocational training thus provided. At least for the years after 1990 we observe a significant increase in vocational training that coincides with a decrease in mean unemployment duration. But, of course, this does not allow any causal interpretations.

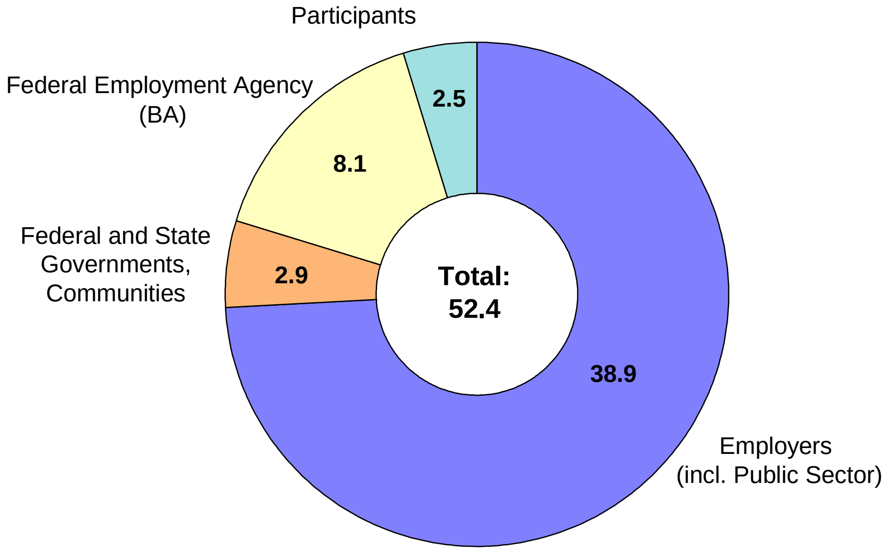
Figure 2:
Average Unemployment Duration and
Expenditures of the Federal Employment Agency for Vocational Training and Retraining
(West Germany only)



Source: BUNDESANSTALT FÜR ARBEIT (1994), p.77; BUNDESANSTALT FÜR ARBEIT (1995), p.72; BUNDESANSTALT FÜR ARBEIT (1996), p.87

The Federal Employment Agency is far from being the most important patron of vocational training in Germany. As *figure 3* shows, 74.24% of total training expenditures in West Germany in 1988 were due to employers (including those in the public sector). Estimates by ALT/SAUTER/TILLMANN (1994) for 1991 indicate that the public sector contributes a fifth of that share. However, this estimate is based also on figures from East Germany. The 2.9 billion DM paid for by federal resp. state governments and communities include items like expenditures for adult education centres (*Volkshochschulen*). More than half of the costs paid by the participants themselves are travel costs and costs for learning materials (ALT/SAUTER/TILLMANN (1994), p.78). All in all more than 50 billion DM or 2.4% of the gross national product were spent for training in 1988.

Figure 3: Training Expenditures in West Germany 1988 (billion DM)



Source: INSTITUT DER DEUTSCHEN WIRTSCHAFT (1990), p.7.

III. Methodological Considerations

III.1 The Discrete Hazard-Rate Model

The dependent variable we are interested in is the duration of time an individual spends in the state of unemployment. However, the problem of censoring prohibits the straight use of duration as the dependent variable in an econometric model. There are a number of spells who were already in process at the beginning of data collection. For those spells the actual duration is unknown because the true inception of the spell generally cannot be observed (*left censoring*). Also, at the end of the observation period (or at the point where an individual drops out of the study) one does not necessarily observe a transition out of unemployment (*right censoring*). Hence, one analyses hazard rates instead of durations.

Basically, hazard models are concerned with observation i's instantaneous rate of leaving a certain state of interest (here: unemployment) per unit time period at t (LANCASTER (1990, p.7)): $\lambda_i\left(t \middle| x_i\right) = \lim\limits_{dt \to 0+} P\left(t \le T_i < t + dt \middle| t \le T, x_i\right) \cdot (dt)^{-1}$, i.e. the *hazard or transition rate*.[4] In the simple case of continuous hazard models the duration T_i spent by observation i in the state of interest is said to be a continuous random variable. The probability of survival to t is given by the corresponding *survivor function* $S_i\left(t \middle| x_i\right) = \exp - \int_0^t \lambda_i(u)du$.

However, 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)). It is then necessary to formulate the model in discrete time.

To specify the discrete hazard model, we consider the case where individual duration data are grouped into J intervals with the j-th interval defined as $\left[t_j, t_{j+1}\right),\ j = 0,1,\ldots J$. For an arbitrary j

[4] Generally, observations may either be spells or individuals. In a single spell framework, however, this distinction is redundant.

the discrete hazard rate $h_i\left(j \mid x_i(t)\right)$ is defined as the probability that a spell ends before t_{j+1}, given that it has lasted at least until t_j and a set of time-varying covariates $x_i(t)$:[5]

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

To further specify the discrete hazard rate-model we use the *Mixed Proportional Hazards Model* as a starting point, a well known and widely applied model for continuous time transition data that is based on a model proposed by COX (1972):[6]

$$\lambda_i\left(t \mid x_i(t), v_i\right) = \lambda_0(t)\exp\left(x_i'(t)\beta + v_i\right) \qquad (2),$$

where λ_0 is the so called *baseline hazard* as it gives the hazard rate for $\exp(0)$ and β is the vector of the coefficients to be estimated. To avoid a possible source of misspecification we estimate λ_0 nonparametrically. v_i is a time-invariant and observation-specific error term that is not correlated with the covariates by assumption. It controls for unmeasured heterogeneity across observations to prevent spurious time dependence (ELBERS/RIDDER (1982)).

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, and substitute $\gamma_j = \ln \int_{t_j}^{t_{j+1}} \lambda_0(u)du$, the discrete time survivor function corresponding to (2) is given by:

$$\begin{aligned} S_i\left(t_j \middle| x_i\left(t_j\right), v_i\right) &= \exp\left(-\sum_{m=0}^{j-1}\int_{t_m}^{t_{m+1}} \lambda_0(u)\exp\left(x_i'\left(t_m\right)\beta + v_i\right)du\right) \\ &= \exp\left(-\exp\left(v_i\right)\sum_{m=0}^{j-1}\exp\left(x_i'\left(t_m\right)\beta + \gamma_m\right)\right) \end{aligned} \qquad (3a).$$

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5 For a thorough survey on discrete hazard models see, for instance, HUJER/MAURER/WELLNER (1996A).

6 The name of the model derives from COX's (1972) original Proportional Hazards model where the hazards for two individuals with vectors of covariates x_1 and x_2 are in the same ratio for all t (LANCASTER (1990)), which is a quite strong assumption. This property of the model vanishes if individual covariates are allowed to vary over time.

HECKMAN/SINGER (1984) propose nonparametric methods to assess the distribution of the heterogeneity component v_i. They show that parameter estimates are sensitive to different parametric assumptions regarding the distribution of v_i. Yet, as TRUSSELL/RICHARDS (1985) point out, much of the parameter instability found by HECKMAN/SINGER (1984) might be the result of their parametric baseline hazard. Therefore, when estimating $\lambda_0(t)$ nonparametrically, the functional form of the heterogeneity distribution may as well be unimportant. NARENDRANATHAN/STEWART (1993) compare a two mass point mixing model using the Heckman-Singer procedure with a normal mixture model and get very similar results. Thus, in our model we assume that $\tau_i = \exp(v_i)$ is a gamma-distributed random variable with mean one and variance σ^2. If $f(\tau_i)$ denotes the corresponding density function we obtain the following expression for the survivor function (LANCASTER (1979)):

$$\begin{aligned} S_i\left(t_j \middle| x_i(t_j), \tau_i\right) &= \int_0^\infty \exp\left(-\tau_i \sum_{m=0}^{j-1} \exp\left(x_i'(t_m)\beta + \gamma_m\right)\right) f(\tau_i)\, d\tau_i \\ &= \left[1 + \sigma^2 \sum_{m=0}^{j-1} \exp\left(x_i'(t_m)\beta + \gamma_m\right)\right]^{-\sigma^{-2}} \end{aligned} \tag{3b}$$

To derive the resulting likelihood function define a dummy variable δ_i, indicating whether the i-th observation is right-censored ($\delta_i = 0$) or not ($\delta_i = 1$). k_i is either the interval, in which an event for individual i can be observed ($\delta_i = 1$), or the censoring interval ($\delta_i = 0$). For a sample of N observations the likelihood function then is:

$$L(\gamma, \beta, \tau_i) = \prod_{i=1}^{N} \left[1 - S_i\left(t_{k_i+1} \middle| x_i(t_{k_i+1}), \tau_i\right) \cdot S_i\left(t_{k_i} \middle| x_i(t_{k_i}), \tau_i\right)^{-1}\right]^{\delta_i} \cdot S_i\left(t_{k_i} \middle| x_i(t_{k_i}), \tau_i\right) \tag{4.}$$

Inserting (3b), rearranging terms and taking logarithms we have for the log-likelihood (MEYER (1990)):

$$\begin{aligned} l(\gamma, \beta, \sigma^2) = \sum_{i=1}^{N} \ln \Bigg\{ &\left[1 + \sigma^2 \cdot \sum_{m=0}^{k_i - 1} \exp\left\{x_i'(t_m)\beta + \gamma_m\right\}\right]^{-\sigma^{-2}} \\ &- \delta_i \left[1 + \sigma^2 \cdot \sum_{m=0}^{k_i} \exp\left\{x_i'(t_m)\beta + \gamma_m\right\}\right]^{-\sigma^{-2}} \Bigg\} \end{aligned} \tag{5.}$$

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

III.2 The Fundamental Evaluation Problem: Sample Selection Bias

The aim of any evaluation of (training) program effects is to assess the difference between the level of the outcome variable at time t for a given participant having received training at some prior date and the level of that variable at time t for the *same* individual without participation. A problem arises because, naturally, the latter situation is a hypothetical one and we cannot directly observe the corresponding outcome level. The level of the outcome variable without participation is only available for non-trainees. If both the group of participants in the program and the group of nonparticipants are random samples from the population of interest a consistent estimate of the average treatment effect could be obtained by comparing the expected level of the outcome variable for the two groups.

This is the case for data based on social experiments where the applicants for a training program are *randomly* selected into a group of actual participants and a control group of non-participants. The recent study of HAM/LALONDE (1996), for instance, uses such an experimental data set. In nonexperimental settings as ours, however, it is possible that both groups are nonrandom samples from the population, i.e. trainees may be different from non-trainees just *because* they are trainees. Trainees might be more aware of the importance of training (MOFFITT (1991), p.294), they may be better educated, they may have experienced a 'shock' (e.g. a decline in earnings (ASHENFELTER/CARD (1985)) or employment probabilities (CARD/SULLIVAN (1988))) prior to participation, etc. Hence, the selection process into training may depend on observable as well as unobservable characteristics. In both cases this will result in a stochastic dependence between the dummy variables for the actual participation that are included in the vector of covariates $x_i(t)$ and the heterogeneity component v_i in (2) (HECKMAN/ROBB (1985), p.162-163).

As a consequence, nonexperimental methods are often considered to be less reliable than evaluations based on an experimental design (e.g. BJÖRKLUND (1989), HAM/LALONDE (1996)). However, experimental designs also have specific problems. Apart from ethical reservations, these problems are, for instance,

- the possibility of a *substitution bias* as it cannot be ensured that members of the control group do not participate in an alternative program (HECKMAN/SMITH (1995));
- that subsequent employment and unemployment spells do not have to be random subsets of the experimental samples (HAM/LALONDE (1996));
- the fact that typically all left censored spells belong to control group members (*initial condition problem*; HAM/LALONDE (1996)).

Since the GSOEP is a nonexperimental data set, we are forced to cope with the potential sample selection problem. The solution applied by FITZENBERGER/PREY (1995, 1996), for instance, is to simultaneously estimate the equation for participation in training and the outcome equation(s) of interest. LECHNER (1995, 1996A, 1996B) favours a different approach as he constructs a control group of non-trainees who are as similar to his sample of trainees as possible with regard to relevant characteristics.[7] In this study we are using an instrumental variable approach that is also suggested by MOFFITT (1991), i.e. we are substituting actual participation with a variable that is correlated with actual participation but not with v_i. We propose the *propensity* (likelihood) to participate in a training program as a suitable variable.

III.3 Econometric Model for Generating the Instruments

The propensities are the result of the estimation of an unbalanced, random-effects probit model with the actual training participation of individual i in wave t, Q_{it}, as the dependent variable:

$$Q_{it} = \begin{cases} 1 & \text{if} \quad Q^*_{it} \geq 0 \\ 0 & \text{else} \end{cases}, \qquad i = 1, \ldots, N\,;\ t = 1, \ldots, T \tag{6}$$

where the latent variable Q^*_{it} is defined as a function of a vector of exogeneous variables, z_{it}, and an one-way error component $\varepsilon_{it} = \mu_i + \kappa_{it}$. μ_i captures the individual-specific effect and

[7] This method of „matching“ trainees and non-trainees with regard to relevant characteristics is utilised in a separate paper (HUJER/MAURER/WELLNER (1997)).

$\mu_i \sim N\left(0, \sigma_\mu^2\right)$, κ_{it} is the "true" error component and $\kappa_{it} \sim N\left(0, \sigma_\kappa^2\right)$, and the usual assumptions about the structure of its variance-covariance-matrix are made (e.g. HSIAO (1996), p.412-413):

$$Q_{it}^* = z_{it}'\xi + \varepsilon_{it} = z_{it}'\xi + \mu_i + \kappa_{it} \qquad (7).$$

Conditioning on μ_i and defining a dummy variable ω_{it}, that equals one if individual i is present at wave t, we obtain the following likelihood function:

$$L = \prod_{i=1}^{N^*} \left\{ \int_{-\infty}^{+\infty} \prod_{t=1}^{T} \left(\Phi_{it}\right)^{Q_{it}\omega_{it}} \left(1 - \Phi_{it}\right)^{(1-Q_{it})\omega_{it}} f\left(\mu_i\right) d\mu_i \right\} \qquad (8)$$

where N^* is the number of individuals, $\Phi_{it} = \Phi\left(\left(z_{it}'\xi + \mu_i\right) / \sigma_\kappa\right)$ and $f\left(\mu_i\right)$ is the density function of μ_i. To simplify the computation we use the Gaussian quadrature formula as suggested by BUTLER/MOFFITT (1982).

IV. Empirical Analysis

IV.1 Description of the Data Basis

The German Socio-Economic Panel (GSOEP) is a panel study collected in the Federal Republic of Germany since 1984.[8] In the year of German reunification, 1990, an additional subsample for the eastern part of Germany, i.e. the former German Democratic Republic, was added. As our study is limited to West Germany this subsample was excluded from our analysis.

The spell data set used in the analysis comes from the individual employment histories that are collected retrospectively for the previous calendar year. During each interview, the individual is presented a list of possible employment states (full-time employed, part-time employed, unemployed, etc.) and is asked to specify for each month of the previous calendar year which of the listed states are applicable for that month. Information on training activities stem from special questionnaires regarding *vocational training* activities in the *previous three years* and were collected two times since 1984, in 1989 (wave F) and in 1993 (wave J). As there is no later information on vocational training than 1993 and because of the three year time frame of the questionnaire we are only able to use data for the years 1986 to 1993 in this study. Because the information on the individual employment histories is collected retrospectively for the previous year, this means that the spell data set is based on waves D (1987) to wave K (1994). All other, cross-sectional information come from waves C (1986) to J (1993).

To model the participation in training the set of covariates for the hazard model includes two dummy variables, separately capturing the short-run or transitory and the long-run or permanent effect of participation:

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[8] The SOEP and its concept have been widely described see, for instance, HANEFELD (1987) or PROJEKTGRUPPE SOZIO-ÖKONOMISCHES PANEL (1995). A good source in English is WAGNER/BURKHAUSER/BEHRINGER (1993), who, though concentrating on a special English language version of the Public use file, do also give information on the SOEP in general.

TR_S: participation in a vocational training measure during the last twelve months prior to spell begin (1 = yes, 0 = otherwise)

TR_L: individual has participated in vocational training sometime between thirteen and thirty-six months before spell begin (1 = yes, 0 = otherwise)

As both variables cover a retrospective time period of three years altogether, we use the first three years (1986-1988) of the spell sample to avoid an initial condition problem regarding the two most important regressors.

As our analysis concentrates on the duration of unemployment spells the selected sample consists of all individuals who had at least one unemployment spell. They contribute all their unemployment spells between 1989 and 1993. The standard assumption in hazard rate models is that multiple unemployment spells of the same individual are independent observations. Left censored spells have been excluded from our analysis for methodological reasons (HUJER/SCHNEIDER (1996)). A spell is completed if it ends through a transition into employment, where the term employment covers the states of full-time and part-time employment in the GSOEP calendar, otherwise it is treated as right-censored. Descriptives for the resulting spell data set can be found in *table 1*.

Table 1: Descriptives for Sample of Unemployment Spells

absolute number:	
individuals	827
spells	1114
completed spells	555
mean duration in months: [a)]	
all spells	5.16
spells of trainees [b)]	3.86
spells of non-trainees [b)]	5.38

a) completed spells only

b) spells of trainees are spell with TR_S = 1 and/or TR_L = 1; all other spells are spells of non-trainees

The descriptive analysis of the spell data set shows that at the most 34.70% of the individuals contribute more than one unemployment spell between 1989 and 1993. 49.82% of the spells in the data set are completed, the rest are right-censored. Prior participation in a qualification measure seems to have a negative influence on the duration of unemployment. However, the difference in the mean duration of spells of trainees and non-trainees must be examined carefully for two reasons: First, it neglects the additional information gained through an analysis of right-censored spells and in the second place it might be subject to selection bias as outlined above.

Table 2 presents some characteristics for trainees and non-trainees in 1993. Though it is restricted to only a few and only *observable* characteristics, this simple comparison already shows that there are significant differences between both groups: Individuals who participated in vocational training in 1993 are younger, more satisfied with their life in general and are better educated. In addition, German nationals and employed persons are more likely to have participated in vocational training in 1993.

Table 2: Individual Characteristics for Trainees vs. Non-Trainees (1993) [a)]

	trainees	non-trainees
number of individuals	100	1996
mean age (years) *	32.52	38.82
males (%)	0.55	0.52
foreigners (%) *	0.13	0.34
current satisfaction with life in general [b)],*	7.30	6.75
education/occupational skills (%):		
Abitur (high school degree) *	0.31	0.13
Lehre (apprenticeship)	0.65	0.58
Diplom (university degree) *	0.18	0.06
employed (%) *	0.80	0.57

a) Cross section for people with at least one unemployment spell 1983-1993. Trainees are those individuals who participated in a vocational training measure in 1993. Non-trainees did not participate in such a measure.

b) Satisfaction is measured on a scale from 0 to 10 (0 = totally dissatisfied, 10 = totally satisfied).

* denotes 95%-significance of difference in sample means

IV.2 Generating the Instrumental Variables

As the short descriptive analysis in the preceding subsection shows, sample selection is indeed an important issue in our data set. Therefore, we use instrumental variables for actual participation in a vocational training measure to control for the possibility of a selection bias in the specification of the hazard rate model. The dummy variables TR_S and TR_L in the vector of covariates $x_i(t)$ in log-likelihood function (5) that represent prior participation in vocational training are replaced with the *propensity* of having participated. To obtain the propensities we estimate the unbalanced, random-effects probit model outlined in subsection III.3 for the eight waves with information on vocational training activities (1986-1993).

BLUNDELL/DEARDEN/MEGHIR (1994, p.3) identify important determinants of training, also suggested by other studies, like age, sex, caring for children, belonging to minority groups (e.g. foreigners or disabled people), educational degrees, occupational status and certain job characteristics. The relevance of some of these determinants has already been confirmed by the descriptive analysis underlying table 2. The choice of regressors in our empirical specification is mainly motivated by these results. However, we augmented the set of job characteristics. We also wanted to test the hypothesis that certain aspects of future plans of those not currently employed are important factors for the decision to participate in vocational training.

The anticipated effects of the more important variables are as follows: Human capital theory leads us to expect a negative influence of age because the period where the investment can pay off declines with increasing age. The same reason, namely a limitation of pay-off opportunities, also makes a negative effect of part-time employment sound reasonable. Discrimination should be the driving force behind gender and minority group effects. Variables related to the familiy context might influence the participation probability through their effect on marginal value of non-market time (BLUNDELL/DEARDEN/MEGHIR (1994, p.20)). Employment status and firm size are important factors with respect to accessibility of training, i.e. employed individuals and individuals in larger firms generally have a better access to vocational training. It could be argued that the better people are educated and the higher their occupational status the greater the probability of participation for the reasons of awareness regarding the relevance of training and the relevance itself. Wealth effects could also be reflected in these variables (BLUNDELL/DEARDEN/MEGHIR (1994, p.20)).

Previous employment history is controlled for by a dummy variable that equals one if the individual has been employed sometime within the last two years. As the special questionnaire on vocational training is collected retrospectively for the previous three years it is subject to a memory bias, i.e. the number of participations in a given year is decreasing with the distance of that year to the relevant special questionnaire. A special variable controls for this bias. Exact definitions and descriptions of all variables used in this paper are given in *table A.1* in the appendix.

Table 3 shows the estimation results which are broadly consistent with the effects identified by BLUNDELL/DEARDEN/MEGHIR (1994). The age profile shows that age has a negative effect beyond the age of 32. However, sex (MALE) and caring for children (KS, KM, KL) turn out to be insignificant. Minority status is important with respect to nationality (FOREIGNER), but not with respect to disability (DISABLED). Also, job tenure does not have the negative effect found in other studies (JOBTENURE). The additional variables included in our analysis are plausible as well: Blue collar workers have a lower probability of participating in vocational training than white collar workers or the reference group (WHICOLLAR, BLUCOLLAR). Individuals with jobs that require a certain degree of previous experience or knowledge are more likely to participate (JOBQUALIF). These jobs may also be those with the greatest need to stay in touch with technological progress, for example. Yet, the fact of working in an occupation one was originally educated for may provide a (deceptive) feeling of safety as it lowers the probability of participation (JOBEDUC). Individuals not currently employed are most likely to participate in a training measure if they are looking for employment in the near future (FUTEMPIMM) and do not explicitly want to work part-time (FUTPARTTIME).

These estimation results are used to compute the propensity for individual i to participate in a vocational training measure in wave t, $z'_{it}\hat{\xi}$. These propensities in turn are used to obtain instruments for the variables of interest, TR_S and TR_L, denoted TR_S* and TR_L* respectively: TR_S* is the maximum of the propensities of the current and the previous year, TR_L* is the maximum of the propensities of the preceding three years, in each case measured with regard to the beginning of the respective spell. We use the propensity rather than the probability of participating because of the greater variation in the propensity.

Table 3:
Maximum Likelihood Estimates for Participation in Vocational Training
Unbalanced Random-Effects Probit for 1986-1993

Variable	Coefficient	t-value
Constant	-4.87399	-10.14761
Age/10	1.15264	4.75078
$(Age/10)^2$	-0.18016	-5.50048
Male	0.05331	0.75811
Foreigner	-0.57477	-6.31847
PartHH	-0.03662	-0.52753
Disabled	-0.05043	-0.36206
KS	-0.05159	-1.00383
KM	-0.09026	-1.27089
KL	0.07993	1.30082
Abitur	0.06172	0.57511
Lehre	0.34264	4.53386
Diplom	0.43635	3.04987
Employed	1.19240	4.07986
WhiCollar	-0.10979	-1.13758
BluCollar	-0.59529	-5.65472
JobTenure	-0.00448	-0.58912
JobQualif	0.67372	8.13176
JobEduc	-0.24058	-3.42499
Firmsize	0.10210	3.52332
Public Sector	0.30508	3.82953
FutEmpDes	0.48695	1.54715
FutPartTime	-0.26169	-1.91767
FutEmpImm	0.90079	5.60821
Emp2yrs	0.23499	2.05908
SpecQuest	-0.24492	-12.13688
σ_μ/σ_κ	0.82660	17.63083
N	3131	
Log-Likelihood	-2531.71680	

IV.3 Main Estimation Results

In this section we finally assess the impact of training on unemployment duration by means of the discrete hazard rate model outlined in subsection III.1 above. We use the instrumental variables that were generated in the last subsection to correct for the possible selection bias. In order to confirm that the specification with instrumental variables is indeed preferable to the specification with "exogeneous" training variables, a Hausman-Test for a parsimonious discrete hazard-rate model without unobserved heterogeneity has been performed following WHITE (1982). With a χ^2-test statistic of 178.5554 and 15 degrees of freedom we reject the exogeneous specification in comparison to the instrumented one. The complete specifications and estimation results are given in *table A.2* in the appendix.

With these results in mind we now turn to the specification of the semiparametric discrete hazard-rate model with a gamma-distributed heterogeneity component as specified in log-likelihood function (5) and substitute the dummy variables for actual participation in vocational training, TR_S and TR_L, with their instruments, TR_S* and TR_L*.

To nonparametrically estimate the baseline hazard and controlling for the time-dependency of the hazard rate the specification includes dummy variables for the respective month(s) of spell duration (BASE$\cdots$). As has been depicted in *figure 1*, the labour market shows a seasonal pattern which should be taken into account to model market restrictions. Thus, we include a set of corresponding dummy variables. It should be mentioned that the DECEMBER-variable, besides controlling for the typical winter slump on the labour market, will also capture heaping effects that result from the fact that the employment calendar in the GSOEP is spanning the period from January to December of the previous year. Thus, the December is a natural join between the employment calendars of consecutive interviews and individuals typically let their spells end in that month of the year (HUJER/SCHNEIDER (1996), p.63). TORELLI/TRIVELLATO (1993, p.205-206) criticise the inclusion of dummy variables in order to explicitly control for such heaping effects. As the true end (and beginning) of the respective spell cannot be identified from the data at hand their argument seems reasonable. However, the results of KRAUS/STEINER (1996) for the GSOEP show that different ways of incorporating heaping effects hardly affect the coefficients of the explanatory variables. In particular, they compare – among other specifications – the inclusion of estimates for the heaping probabilities derived by comparing their data with inflow and outflow data from official labour market statistics and the dummy variable approach.[9] KRAUS/STEINER (1996, p.23) therefore propose the use of dummy variables, as it "has the great advantage of facilitating estimation of more complicated duration models."

Additional variables include usual socio-demographic characteristics like age (AGE···), nationality (FOREIGNER), disability status (DISABLED) and qualification (ABITUR, LEHRE, DIPLOM). It is widely accepted that younger, native, non-disabled, better educated and more qualified individuals have higher chances of finding a new job. We also control for sex (MALE) and family context (PARTHH). The inclusion of a variable measuring individual's current satisfaction with life in general (SATISLIFE) is causally related to the assumption that it is highly correlated with the individual's motivation and self-commitment in finding a new job. Public employment agencies are often considered to be rather inefficient. In addition, a substantial commitment of the individual itself could be an important screening factor of firms when considering applicants. The variable is based on a scale from 1 to 10 with 1 being totally dissatisfied and 10 being totally satisfied and corresponds to a question in the yearly GSOEP questionnaire.

Likewise, the situation of the individual in the past, i.e. prior to the unemployment spell, and his future plans are relevant determinants of unemployment duration. The number (NOUNESP3) and cumulated duration (DURUNESP3) of unemployment spells in the past and the employment status prior to the unemployment spell (PRVEMPLOYED) control for the individual's employment history. Duration (PRVTENURE) and occupational status (PRVWHICOLLAR, PRVBLUCOLLAR) further characterise a possibly preceding employment. To allow for the possibility that an individual is only registered as being unemployed in order to reap unemployment benefits but, in fact, is not looking for a new employment, a dummy variable for plans for a future employment is included (FUTEMPDES). As it might be much more difficult to find a part-time job than a full-time job, a dummy variable covering the case that the individual is explicitly looking for a part-time occupation is also part of the set of covariates (FUTPARTTIME). The importance of the level of unemployment benefits for unemployment duration is the focus of many empirical studies. For Germany, lately STEINER (1994) and HUJER/SCHNEIDER (1996) find significant whereas the results of WURZEL (1993) show insignificant effects. We allow for an

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9 Parameterisation of the heaping process as proposed by TORELLI/TRIVELLATO (1993) failed because of numerical difficulties due to the small number of observations in particular duration groups (KRAUS/STEINER (1996, p.12-13).

influence of the level of unemployment benefits by inclusion of the replacement ratio defined as the level of unemployment benefits in relation to the last labour market income.

To test for remaining sample selection effects in their model, FITZENBERGER/PREY (1995) include a dummy variable that follows the idea of the preprogram test of HECKMAN/HOTZ (1989, p.366). HECKMAN/HOTZ (1989) analyse sample selection issues in the context of a particular training program[10] and advocate the use of a dummy variable that equals one if an individual is a future participant and zero if it is from the control group of non-participants. The estimated coefficient for that dummy variable „should not be statistically significantly different from zero for any correctly specified selection-correction model“ (HECKMAN/HOTZ (1989), p. 366). Incorporation of such an ideal dummy variable (which will be called *HH-dummy* throughout the rest of this paper) represents a simple way to test for sample selection effects and we thus implement this idea in our model as well. However, the empirical application of this dummy variable is not without problems, especially in the context of longitudinal studies as ours. The distinction between trainees and non-trainees for a particular program as in HECKMAN/HOTZ (1989) is not difficult. It is clear, at least afterwards, who participated in that program and who not. In a longitudinal survey, however, one observes a number of people who participate in a great variety of training programs and some who do not participate in any program *during the observation period*. The key problem is if the latter are true controls or not.

If it is the single program that matters for sample selection issues – and nobody can deny that for sure – one would ideally have to include a separate HH-dummy for every program in the data that equals one if an individual will participate in that program in the future. But, neither is it very likely that more than one individual will participate in a given program nor can individual programs be identified. Of course, the sheer number of different programs will also prohibit this practice.

If it is participation in a training measure in general that matters for sample selection issues, however, it suffices to include a single HH-dummy that equals one if an individual participates in any training program in the future (and has not done so in the past). This is done in FITZENBERGER/PREY (1995). Yet, in this case we have two other problems: First, it remains unknown if

[10] The example they use is the National Work Demonstration project implemented in the U.S. in 1976/1977.

someone, who has not participated in a program during the period of observation, has not participated in the past or will not do so in the future and, consequently, should be counted as a trainee for sample selection purposes. Thus, members of the „control group“ (HH-dummy=0) can be actual „future trainees“ (HH-dummy=1) and vice versa. Second, especially if the dummy variable is conditioned on the past, the dummy variable will be time dependent as there will be less persons, who will participate in the future and who have not done so in the past, the nearer the end of the period of observation. Hence, a part of the effect of the HH-dummy is a pure time-effect. This second problem is the motivation for the inclusion of a variable (MONTHSEND) that equals the number of months from spell begin till the end of the period of study and should capture this time effect. Indeed, our estimations showed sensitivity of the significance of the HH-dummy to inclusion/exclusion of this variable.

The HH-dummy has further problems, namely its own possible endogeneity (FITZENBERGER/PREY (1996), p.20), the underlying assumption of time-constancy of selection effects (LECHNER (1995), p.63) and the issue of testability in general (LECHNER (1995), p.5). Considering the delicacy of the selection problem it remains unclear if it actually can be adequately represented by a somehow inaccurate variable. One should keep those caveats in mind when interpreting the coefficient of this dummy variable. We try to alleviate the problems described above by introducing year-specific HH-dummies[11] (TRHH$\cdots$) and controlling for time effects with the variable MONTHSEND. Thus, we think that inclusion and interpretation of the HH-dummies is still helpful. However, we would not go as far as rejecting or accepting a certain model or specification solely on the significance or insignificance of the HH-dummy variable.

Table 4 has the estimation results. Considering time dependency, a long unemployment duration of more than 26 months significantly reduces the transition probability. The earlier a spell lies within the observation period the higher the transition probability. The seasonal effects for spring and the month of December have the expected signs and are significant. Thus, the usual stimulation of the labour market during spring time is reflected in our sample as well. For the winter time one would expect to find a negative effect but this is more than offset by the described heaping effects. The effect for summer does have the expected sign but is insignificant.

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[11] We could not obtain a reliable estimate for a HH-dummy variable for 1993 because of the small number of relevant training participations in that year.

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

Variable	Coefficient	t-value
Constant	-5.9309	-10.3590
Base02	0.1723	1.1785
Base03	0.0041	0.0226
Base04	0.0301	0.1456
Base05	-0.0812	-0.3552
Base06	0.2654	1.1421
Base07-12	-0.2973	-1.2659
Base13-18	-0.5158	-1.5550
Base19-26	-0.7100	-1.7158
Base27+	-2.1086	-2.8755
MonthsEnd	0.0105	3.1237
Spring	0.3202	2.9690
Summer	-0.0431	-0.3148
December	0.3288	2.2550
Age –21yrs	1.4988	4.4895
Age 22–39yrs	1.2424	4.0426
Age 40-54 yrs	0.9689	3.1724
Male	0.1428	1.1163
Foreigner	-0.3425	-2.4157
PartHH	-0.0685	-0.6011
Disabled	-0.3507	-1.6354
Abitur	-0.1909	-1.0107
Lehre	0.0212	0.1827
Diplom	0.0646	0.2614
SatisLife	0.0787	3.2184
TR_S*	0.3737	2.7822
TR_L*	-0.1607	-1.1940
TRHH89	-0.4103	-1.1694
TRHH90	0.4740	1.3230
TRHH91	0.5859	1.1670
TRHH92	0.5202	1.0705
NoUneSp3	0.4232	5.0186
DurUneSp3	-0.5026	-3.7432
PrvEmployed	0.4885	1.3459
PrvWhiCollar	0.1879	0.5257
PrvBluCollar	0.1980	0.5674
PrvTenure	-0.0200	-0.4810
FutEmpDes	1.8636	6.8886
FutPartTime	-0.2136	-1.3362
ReplacementRatio	-0.5177	-2.4235
Ln(σ^2)	-1.4068	-1.9893
Log-Likelihood	-1771.9027	
LR-Test χ^2 (df)	469.3498 (30)	

Our initial hypothesis that younger, native and non-disabled persons have a better chance of finding a new job is confirmed by our estimates, even though the coefficient for the disability dummy is significant on the 10%-level only. The effects of sex, of currently living together with a partner and – more unexpectedly – of the education/qualification variables are not different from zero. The insignificance of the latter variables, however, can result from the fact that these variables are also important determinants for the participation in vocational training and the propensities for participation already control for these effects. A higher level of satisfaction with life in general indeed has a significant positive effect, so that our hypothesis about the role of motivation in determining unemployment duration is confirmed if our variable is indeed a good proxy for this unmeasurable factor.

Individuals who have already experienced unemployment spells in the past are less likely to find a new job if cumulated unemployment duration is considered. The number of past unemployment spells, however, has a positive and significant effect. Initially, one would have expected a negative sign of the respective parameter but it should be taken into account that for a given period of time duration and absolute number of spells are inversely related. The estimated parameter for a preceding employment has the correct sign but is insignificant as incidentally are the parameters of all other variables related to this employment. If the individual actually wishes to take up a new employment this, of course, has a positive and significant effect on the transition probability. This, too, can be viewed as a manifestation of motivation. Explicitly looking for a part-time employment again has the expected sign but is insignificant. Finally, the level of benefits as expressed through the replacement ratio has a negative significant effect and, thus, would support demands for lowering unemployment benefits in order to reduce unemployment.

Turning to the two variables of greatest interest in this study (TR_S*, TR_L*) we find that participation in vocational training has a positive effect on the hazard rate in the short run but not in the long run: A vocational training within one year prior to unemployment reduces expected unemployment duration whereas earlier training measures have no influence. To better assess the short run impact of training, *figure 4* depicts the hazard rates for two individuals who entered unemployment at the beginning of 1990. One individual is very likely to have participated in a vocational training course in 1989 ("trainee"), the other, however, is very unlikely to have done so ("non-trainee"), i.e. the trainee (non-trainee) individual was given the mean propensity of all trainees (non-trainees) in the sample for the short run variable. For the long run variable both were given the mean propensity of all non-trainees in the sample. [12] Initially, the hazard rate of the trainee is by 5.5%-points greater than that of the non-trainee. The difference soon reaches its maximum of 8.1%-points in February 1990, the second month. It then declines with the level of the hazard rate which in turn decreases with increasing unemployment duration.

Figure 4:
Hazard Rates for Trainee vs. Non-Trainee

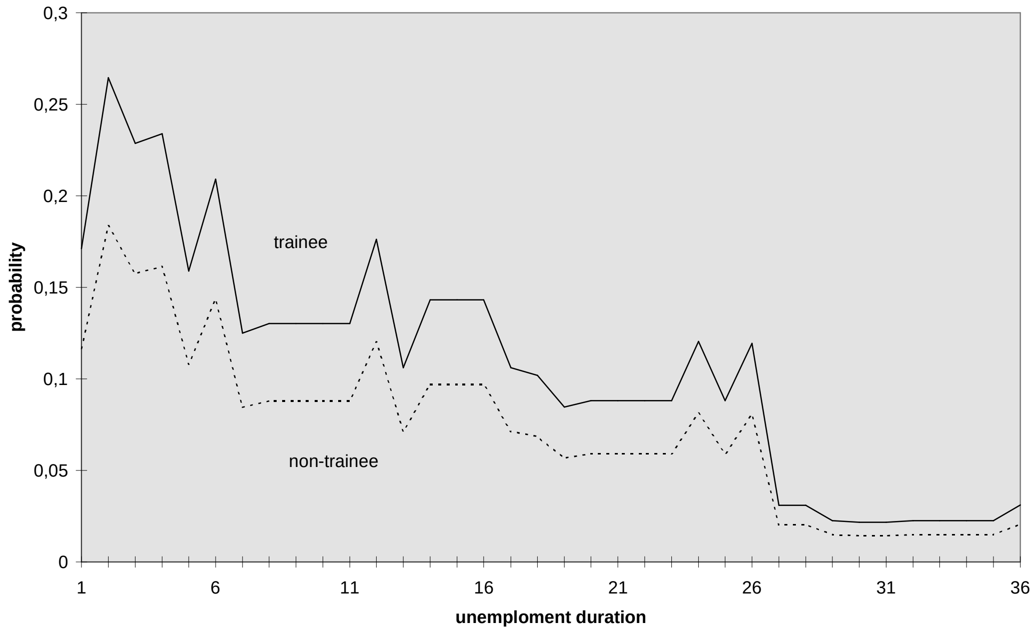

Figure 5a gives the unconditional probabilities that the spell ends in the respective month. The trainee has higher exit probabilities up to the sixth month and lower ones from the sixth month onwards. As the exit probabilities for all months must sum up to one for each individual this change is not surprising. The cumulated exit probabilities in *figure 5b* show that in each month the trainee has nonetheless a higher probability of having experienced a transition into employment.

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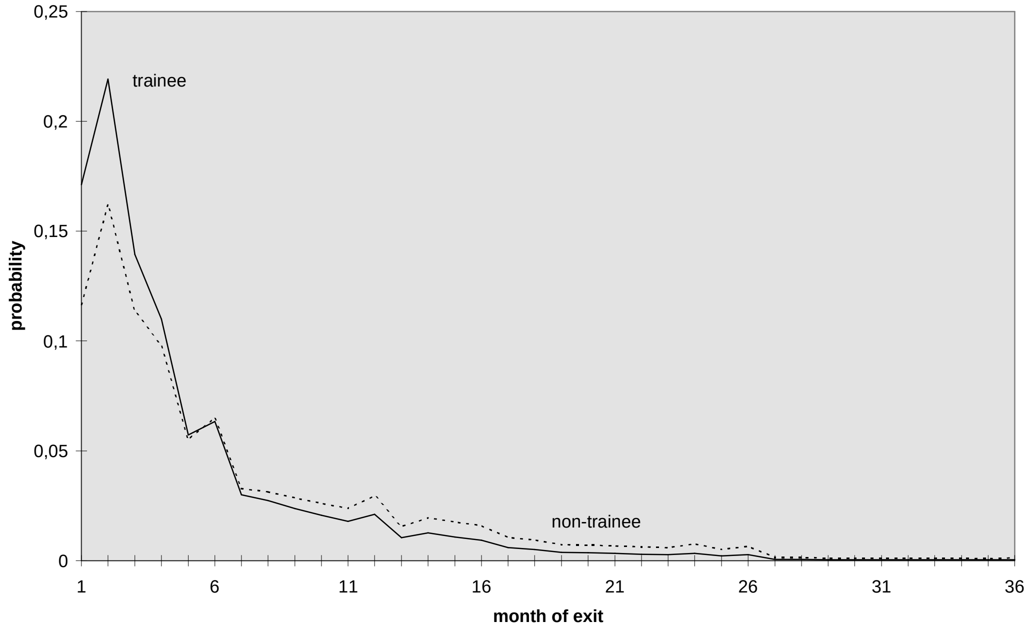
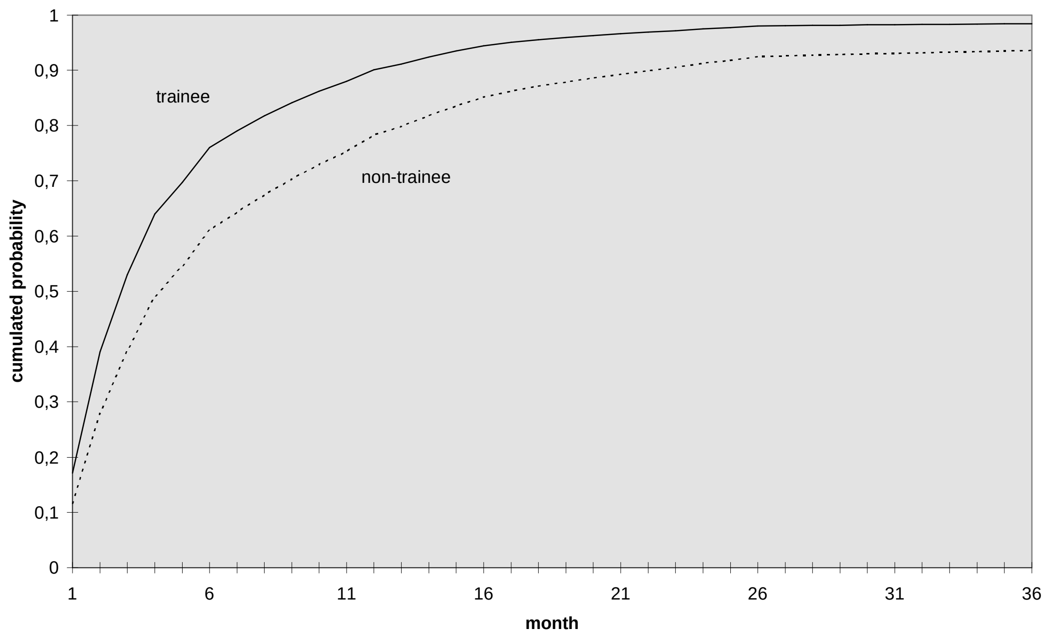
[12] The individuals are defined as follows. Both individuals were previously unemployed during the whole year of 1987. At the beginning of the current unemployment spell they are both 30 years old. Both are males, German nationals and not disabled. They are currently living together with a partner, have Abitur and a university degree but not a completed apprenticeship. Their satisfaction with life in general is above average (7 out of 10). They were previously employed as white collar workers during 1988 and 1989. Both individuals are currently looking for a full-time employment. The replacement ratio is 0.5. All socio-demographic variables with the exception of age are assumed to be constant over the spell. Furthermore, it is assumed that both individual do not participate in any vocational training measures in the future.

The above results would have to be considered with care if they were still subject to a sample selection bias. However, the HH dummy variables are all insignificant. If they indeed are a valid method for testing for a remaining sample selection effect, this means that our results are not biased in that respect.

Figure 5:
Unconditional Exit Probabilities (a) and Cumulated Unconditional Exit Probabilities (b)
for Trainee vs. Non-Trainee



(a)



(b)

IV. Conclusion

In this paper we estimate a discrete hazard model with gamma distributed unmeasured heterogeneity for the transition from unemployment into employment to assess the impact of training on unemployment duration in West Germany. As the insignificant coefficients for the HH-dummy variables and the results of the Hausman-test indicate, the instrumental variable approach is indeed able to correct for sample selection problems. Our results show that prior participation in vocational training has a significant negative effect on unemployment duration in the short but not in the long run. They correspond to the findings of a positive training effect in a separate analysis using matching techniques (HUJER/MAURER/WELLNER (1997)).

The lack of a significant long term effect may be due to a possibly considerable heterogeneity of vocational training measures in the GSOEP data. Looking at the possible response categories for the institution organising the training measure, for instance, the current employer or chamber of commerce is listed as well trade unions, churches or adult education centres. It may be doubted if an employer recognises training measures in the latter three institutions as a relevant vocational qualification (see e.g. INSTITUT DER DEUTSCHEN WIRTSCHAFT (1990), p.7). Consequently, the effect of „true“ vocational training courses might be underestimated. Unfortunately, due to the small number of participations in the sample a further partitioning of training measures is not a very promising strategy.

As LANCASTER (1990, p.107) points out the transition from unemployment into employment may depend on transition probabilities into other possible destination states like non-employment or training. Thus, a competing risks model that takes into account these dependencies should be an interesting alternative, especially if this would simultaneously explain the selection into training measures as in GRITZ (1993).

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Appendix

Table A.1: Definition of Variables

Variable	Description
Training variables	
TR_S	1 if individual participated in vocational training within 12 months prior to spell begin
TR_L	1 if individual participated in vocational training within 13 to 36 months prior to spell begin
TR_S*	instrument for TR_S: maximum of the propensities of the current and the previous year as measured from spell begin
TR_L*	instrument for TR_L: maximum of the propensities of the preceding three years, as measured from spell begin
TRHHxx	1 if individual is participating in vocational training in the future, has not participated in the past three years and the current year is 19xx
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
Age variables	
Age/10	Age divided by 10
$(Age/10)^2$	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
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)
ReplacementRatio	Level of unemployment benefits in relation to the last gross labour market income

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

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		
FutEmpDes	1	if individual is currently not employed but wishes to be employed in the future
FutPartTime	1	if individual is currently not employed but wishes to be employed in the future and is looking for part-time 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
Occupational Status — reference category are apprentices and self-employed		
PrvWhiCollar	1	if individual was previously employed and had a white collar status
Prv BluCollar	1	if individual was previously employed and had a blue collar status
PrvTenure	duration of previous employment in months divided by 12	
„Technical“ variables		
SpecQuest	No. of years until year before the next special questionnaire on vocational training	
MonthsEnd	number of months from spell begin till end of period of study	

Table A.2:
Comparison of Maximum Likelihood Estimates for Transition Unemployment ⇒ Employment
Exogeneous vs. Instrumented Specification
Discrete Hazard-Rate Model

	Exogeneous Specification		Instrumented Specification	
Variable	Coefficient	t-value	Coefficient	t-value
Constant	-2.6857	-23.8400	-1.6011	-10.2412
Base02	0.0596	0.4379	0.0784	0.5770
Base03	-0.2097	-1.3499	-0.1804	-1.1622
Base04	-0.2311	-1.3720	-0.2049	-1.2183
Base05	-0.3730	-1.9620	-0.3352	-1.7631
Base06	-0.0824	-0.4558	-0.0244	-0.1351
Base07-12	-0.7525	-5.2622	-0.6524	-4.5555
Base13-18	-1.1413	-5.5990	-1.0005	-4.8970
Base19-26	-1.5000	-5.2741	-1.3156	-4.6106
Base27+	-2.9483	-5.0288	-2.8219	-4.8129
Spring	0.3622	3.5979	0.3845	3.8258
Summer	-0.0236	-0.1766	-0.0203	-0.1522
December	0.2604	1.8337	0.2548	1.7944
NoUneSp3	0.5804	10.8057	0.5350	9.8159
DurUneSp3	-0.5481	-5.1255	-0.4842	-4.5156
TR_S(*)	0.0899	0.4723	0.4939	4.7741
TR_L(*)	0.5467	3.7928	-0.0031	-0.0287
Log-Likelihood	-1934.7640		-1900.9979	
LR-Test χ^2 (df)	143.6272 (7)		211.1594 (7)	
Hausman-Test χ^2 (df)	178.5554 (15)			