Is Short Training Short-Lived and Long Training Long-Lasting? A Multi-State Duration Analysis of the Dynamic Effects of Training Schemes for the Unemployed[1]

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Abstract: This study investigates the dynamic causal effects of short job-search training and traditional long-term training schemes for the unemployed on the exit rate to employment and the exit rate from subsequent employment back to unemployment. For this purpose, a multivariate duration model that deals with dynamically assigned treatments, selection on unobservables as well as heterogeneous treatment effects is estimated from rich administrative data for Germany. The results indicate that job-search training reduces average unemployment duration and increases average employment duration. In contrast, a participation in traditional further training increases both the expected unemployment duration and the expected employment duration. Treatment effects vary substantially with elapsed unemployment duration at program start. They are also heterogeneous across observed and unobserved characteristics of the participants.

Keywords: training, program evaluation, duration analysis, dynamic treatment effects, multiple treatments, active labor market policy

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1 Introduction

Since the mid-1990s, research and policy agendas in the OECD countries increasingly pay attention to identifying the most effective among the active labor market policies for the unemployed (see e.g. Martin and Grubb, 2001, and Kluve and Schmidt, 2002). In the microeconomic evaluation literature, job-search assistance programs that activate the unemployed in the short run often come off well (OECD, 2005, ch. 4, Kluve, 2006). In contrast, long-term training, that traditionally is the most important type of active labor market policy in many OECD countries, rates less well because positive gains only unfold after the completion of the program. In fact, participants in traditional further training programs tend to reduce their search activities while in training. Moreover, active job-search programs are inexpensive compared to traditional labor market training.

However, little is known on whether programs focusing on short-run targets such as improved search efficiency can increase employment stability in the long run. Job-search training may be inappropriate for job-seekers lacking important skills. From a policy perspective, it is important to know whether there is a need for substantial adjustments of the human capital and productivity of the unemployed or whether comparable employment effects can be achieved at less cost through short job-search training programs. A related important question in this debate is how the unemployed should be allocated to the different active labor market programs given that program effects vary with elapsed unemployment duration at program start and are heterogeneous across different participants.

In order to provide reliable empirical evidence on these issues, this study takes a comprehensive approach and analyzes the dynamic effects of job-search training and traditional further training on unemployment and employment in a multi-state duration model. The dynamic approach taken here is particularly well suited to study and compare the short- and long-run impacts of both types of training. I discuss treatment effects on the instantaneous exit rates out of unemployment and employment as well as the way they translate into changes in expected unemployment and employment duration. Moreover, my approach allows to answer the question when, during unemployment, programs should be assigned. This is of particular relevance with respect to traditional further training schemes, where negative short-run effects have to be weighed against positive long-run effects. Importantly, this paper also considers heterogeneous treatment effects, which allows to state precise policy conclusions with respect to specific groups.

The empirical analysis is based on administrative data for Germany. In fact, Germany is an interesting case to study because its recent reforms and developments in the field of labor market policy closely reflect the recommendations formulated in the international policy debate in the mid-1990s in view of high unemployment levels especially in the European countries (cf. European Commission, 2002, on the "European Employment Strategy" and OECD, 2006, on the "OECD Jobs Strategy"). In addition, Germany is a prototypical example of an economy with a comprehensive system of active labor market policy standing in contrast with the US where labor market policy interventions are typically less generous and occur on a small scale.[2]

In Germany, further training schemes have traditionally been the cornerstone of active labor market policy. During the recent years, however, the policy focus has shifted towards measures that activate the unemployed in the short run. In particular, short-term training schemes have gained in importance. These courses last only one month on average. The traditional long-term training schemes, on the contrary, have an average duration of nine months. Due to this difference in length short-term training costs per participant make up only a tenth of those of traditional training.

The difference in duration of the two program types also translates into a difference in contents and purpose. Traditional further training schemes impart a substantial amount of specific professional skills and operational techniques. Integration into employment is thus supposed to be achieved through an improvement of the productivity of the unemployed. In contrast, the human capital component of short-term training is rather limited. It basically provides skills that enhance job search. More importantly, short-term training is also employed to assess a job-seeker's abilities and his readiness to work.

Unlike in the US, social experiments are rather uncommon in the European countries. Researchers thus have to deal with the evaluation of ongoing programs. A typical feature of these is that the unemployed may potentially take part at any point in time during their unemployment spell. Hence, the decision whether to enrol or not is not a static problem, but the job-seeker is continuously at risk of participation as long as he remains unemployed. This setting allows to study a question that has been neglected so far in the US literature, i.e. the question how treatment effects depend on the elapsed unemployment duration at program start. In addition, the administrative data on which this study is based are organized as spells with start and end dates recorded at daily precision. It seems thus most natural to adopt an

[2]According to OECD (2006, ch. 7) average spending on active labor market policies per unemployed relative to per capita GDP is around 33% in Germany and 7% in the US.

econometric approach that fully exploits the information available on the timing of events in order to draw causal inference on the effects of training.

Abbring and van den Berg (2003a) provide formal identification proofs for the timing of events approach an extended version of which is applied in the present study. This approach jointly models the duration in an outcome state, i.e. unemployment, and the time until treatment start. It exploits the information on the timing of the treatment in order to identify the causal effect on the hazard rate out of unemployment in the presence of selection on unobservables. Intuitively, while unobserved determinants induce a global association between treatment and outcome process, a positive causal treatment effect gives rise to a quick succession of the realization of the moment of treatment and the exit to employment.

A fully dynamic framework has several advantages over methods that impose a static structure on the evaluation problem. On the one side with respect to the definition of treatment status, Fredriksson and Johansson (2003) have shown that a static evaluation approach, which stratifies individuals into a treatment and control group based on their treatment status observed within a fixed time window, yields biased treatment effects if the start of a treatment is the outcome of a stochastic process. On the other side concerning the evaluation of the outcome, a fully dynamic approach provides a more detailed understanding of how labor market programs work as it allows to disentangle the treatment effects on unemployment and employment duration. In contrast, a cross-sectional measure of the employment status at some given point in time after program participation does not allow this distinction. Further, a continuous time duration model avoids specification issues that arise as a consequence of discretization of inherently continuous dynamic features of the data. Finally, the framework proposed by Abbring and van den Berg (2003a) allows for selection on unobservables.

There exists a small literature employing an event history framework to evaluate active labor market policies.[3] Early examples include the papers by Ham and LaLonde (1996) and Eberwein et al. (1997). They analyze the effects of randomly assigned training programs for women in the US on subsequent unemployment and employment spells. While these papers have already highlighted the importance to study both unemployment as well as employment duration, they do not consider dynamically assigned treatments and the effect of the timing of a treatment.

[3] There exists a large literature on the microeconomic effects of active labor market policies that uses experimental or matching methods. Comprehensive surveys of this literature can be found in Heckman et al. (1999), Martin (2000), Martin and Grubb (2001), Kluve and Schmidt (2002) and Kluve (2006).

The more recent literature applies the timing of events approach by Abbring and van den Berg (2003a). There are a few papers that analyze the effects of job-search assistance in form of intensive counseling. The study by Crépon et al. (2005) evaluates different job-search programs in France. They find that these programs have favorable effects on unemployment duration as well as unemployment recurrence. Van den Berg and van der Klaauw (2006) analyze a social experiment in the Netherlands and find no significant impacts of counseling on unemployment duration. The paper of Weber and Hofer (2004) compares job-search programs to further training schemes in Austria. They find that active job-search programs reduce unemployment duration while further training increases it. Richardson and van den Berg (2006) study labor market training in Sweden. They find a positive effect on the exit rate to work when the time spent in training is not counted. The studies by Bolvig et al. (2003) for Denmark and Lalive et al. (2008) for Switzerland use duration models to evaluate the effects of different active labor market programs including training on welfare and unemployment spells, respectively. The only studies for Germany applying a fully dynamic approach are Hujer et al. (2006a, b).[4] They consider the effect of one type of training on unemployment duration. Hujer et al. (2006a) find that short-term training reduces unemployment duration in West-Germany, and Hujer et al. (2006b) find insignificant to negative effects of training schemes in East Germany.

This paper differs from the previous literature in the following important respects. First, it applies a framework that allows to separately analyze the dynamic treatment effects on unemployment and employment when treatments are dynamically assigned and selection into treatment depends on unobservables. Second, this paper takes a comprehensive approach in order to provide differentiated policy relevant evidence. In particular, I directly compare two types of training and consider heterogeneous treatment effects across observed and unobserved characteristics. Third, unlike many of the previous studies applying the timing of events approach, this study analyzes two outcomes: unemployment as well as employment duration. This is important in order to properly assess the long-run effectiveness of the programs.

The results in this paper indicate that job-search training reduces average unemployment duration and increases average employment duration. In contrast, a participation in traditional further training increases both the expected unemployment duration and the expected employment duration. There is also evidence for heterogeneous treatment effects across the characteristics of the participants. Further,

[4] Biewen et al. (2007) contains an overview of studies on labor market training in Germany. Evaluation studies that use the same database as the present one include Biewen et al. (2007) and Lechner and Wunsch (2007).

treatment effects vary widely according to the timing of treatment. The overall gains out of job-search training are highest when it is started early in the unemployment spell. The unemployment duration prolonging effect of traditional further training decreases with elapsed unemployment duration at program start. In fact, long-term training is only effective in the long run if it is not started too early in the unemployment spell. Simple back-of-envelope calculations suggest that job-search training schemes are cost effective whereas traditional further training schemes are not. In sum, these findings suggest that both programs should be carefully targeted in order to maximize potential benefits. This holds particularly for traditional further training where the net long-run effect is the sum of a negative effect on unemployment duration and a positive effect on employment duration.

The remainder of the paper is organized as follows. Section 2 summarizes the institutional context of training as part of active labor market policy in Germany. In section 3, I present the data and the analysis sample. Section 4 describes the identification strategy and the econometric implementation. Section 5 discusses the empirical results, and section 6 concludes.

2 The Role of Training in German Active Labor Market Policy

2.1 Institutional Background

The main goal of active labor market policy in Germany is to permanently reintegrate unemployed and persons at severe risk of becoming unemployed into employment. It comprises a variety of measures ranging from subsidized employment on the first and second labor market to training programs that adjust and enhance the qualifications of participants. In addition to these large scale programs, there also exist programs targeted to particular groups as e.g. youths, disabled persons, or long-term and elderly unemployed and schemes promoting business start-ups. Training schemes have traditionally dominated active labor market policy in Germany.

The legislation distinguishes three main types of training, further training (*Berufliche Weiterbildung*), retraining (*Berufliche Weiterbildung mit Abschluss in einem anerkannten Ausbildungsberuf*), and short-term training measures (*Trainingsmaß-*

nahmen und Maßnahmen der Eignungsfeststellung).[5,6] Whereas further training and retraining have kept their place in active labor market policy nearly unaltered since the 1970s short-term training has been reintroduced in 1998 after a similar program type had been abolished in the early 1990s.

The traditional further and retraining schemes differ by length as well as contents. Further training measures include advanced vocational training and refresher courses that impart specific professional skills and operational techniques in form of classroom or on-the-job training. They typically last between six to twelve months. Retraining is the most comprehensive training scheme. This program type provides a new vocational education degree within the German apprenticeship system and lasts two to three years. In general, it comprises periods of classroom training as well as internships. The newly reintroduced short-term training courses, on the contrary, last only several days to twelve weeks. Similar to the traditional further training schemes, they may take place either in classrooms or in firms. However, due to their shorter length their contents are more general. Typical examples of short-term training schemes include job application training, language courses and short-term internships. The aim of this type of training is twofold. On the one hand, it provides skills that improve and facilitate job search. On the other hand, it is employed to assess a job-seeker's abilities and his readiness to work or to participate in a further program.

To become eligible for any active labor market program job-seekers have to register at the local employment agency. This involves a counseling interview with the caseworker. Besides being registered as unemployed or job-seeker at risk of becoming unemployed, candidates for short-term training measures do not have to fulfil any additional eligibility criteria. As regards the traditional further and retraining measures, individuals are in principal only eligible if they also fulfil a minimum work criterion of one year and are entitled to unemployment compensation. However, there exist various exceptions to these requirements. The really binding criterion is that the training scheme has to be considered necessary by the caseworker in order for the job-seeker to find a new job. This is for instance the case if the employment

[5] Furthermore, there exist specific training schemes for youths and disabled persons as well as German language courses for returned settlers from former German settlements or refugees that may be counted among the training measures. As they are not analyzed in this paper they are omitted.

[6] In the empirical analysis, I focus on the distinction between traditional further and retraining schemes as opposed to short-term training. Thus, I shall neglect the distinction between further training and retraining and subsume both categories under the notion traditional further training or long-term training.

chances in the target occupation of a job-seeker are good but require an additional adjustment of his skills.

Before the so called Hartz-Reform in 2003, assignment into programs was to a large extent driven by the supply of courses (cf. Schneider et al. 2006). Labor agencies demanded in advance the amounts and contents of training courses from the providers and in turn committed themselves to fill them with participants throughout the year. The concrete allocation of unemployed to the different courses was strongly subject to the discretion of the caseworker. Blaschke and Plath (2000) report that private indicators of the caseworker like the composition of a group of participants in a particular course or his assessment of the motivation of the unemployed played an important role. Assignment into training programs often occurred at very short notice. In particular, it was characteristic for some types of training that participants did not start and finish a course together at a given date, but that new participants enroled into an ongoing scheme as soon as other participants left it, partly because they found a job (Blaschke and Plath, 2000). Anecdotal evidence in Schneider et al. (2006) suggests that ex post adjustments in form of belated assignments on very short notice were used to assure a high capacity utilization of booked courses.

Since 2003, candidates for a training program obtain a voucher that is valid for one to three months and that specifies a training field. The candidate then redeems the voucher by choosing a suitable course from a pool of certified courses. In fact, the 2003 reform meant to make the allocation process more targeted and selective. However, potential participants were uncertain about the actual starting date because it turned out that training providers tended to collect vouchers until a sufficient critical number of participants was reached or they shortly canceled scheduled courses if there were too few participants (Schneider et al., 2006).

Participation in the training program is mandatory once the job-seeker has been assigned to a specific course or a training voucher, respectively. However, unemployed are generally encouraged to continue searching while in training. In particular, training providers are expected to assist the participants in their search. Dropping out of a program in order to take up a job does therefore not contravene the rule of mandatory participation.

Training costs as well as examination fees, traveling and child-care costs are covered by the employment agency. In addition, participants in further and retraining schemes typically draw subsistence payments that have the same amount as the unemployment compensation payments they would have otherwise received.[7] Participants in short-term, further or retraining who are not entitled to unemployment insurance payments may receive subsistence payments that are financed by the European Social Fund.

2.2 Quantitative Importance of Training

The further and retraining schemes have traditionally been the most important field of active labor market policy in Germany. Since 1998 there have been several reforms leading to a focus on measures considered particularly effective in activating the unemployed in the short run and in preventing long-term unemployment. Thus, allocation of resources was shifted away from the very comprehensive long-term training schemes to the short job-search training measures.

In fact, table 1 shows a decline in entries concerning the long-term training programs – in the Western Länder as well as in total Germany – whereas there is an increasing trend for short-term training programs. From table 2 it can be seen that the average monthly training costs per participant are slightly lower for job-search training courses (560 Euros on average) than for traditional further training measures (650 Euros on average). Most striking is the great difference in average duration of the courses, that is displayed in the second column for the respective year. While short-term training courses last on average one month, the duration of longer-term programs, where the average is taken over both further and retraining schemes, lies between eight and ten months. Under the budgetary pressure due to a persistently high unemployment rate and in light of these large differences in costs, the share of short-term training measures drastically increased in 2002 and, since 2003, this rise continues at the expense of the traditional longer-term measures. Of course, the higher training costs of the latter would be justified if they were associated to correspondingly higher gains.

— Insert table 1 about here. —

— Insert table 2 about here. —

[7] Unemployment compensation, in contrast to social assistance, is granted to individuals who are able and available to work or who participate in active labor market programs. Basically, unemployed who previously worked for at least twelve months within the last three years qualify for unemployment benefits. The amount and the entitlement period depend on the previous salary, age, and work experience. After expiration of their unemployment benefits unemployed individuals may receive the lower, means tested unemployment assistance. See table 2 below for information on average monthly expenditures per benefit recipient.

3 Data

3.1 The Integrated Employment Biographies Sample

The empirical analysis is based on an exceptionally rich administrative data base, the German Integrated Employment Biographies Sample (IEBS), that has only recently been made available by the Institute for Employment Research (IAB) of the German Federal Employment Agency. The IEBS is a 2.2% random sample from a merged data file containing individual records out of four different administrative registers.[8] It covers register data on employment subject to social security contributions, receipt of transfer payments during unemployment, job search, and participation in different active labor market programs. The basic population consists of all individuals who, during the reference period, have either held a job subject to social security contributions or have been registered as a benefit recipient, job searcher, or program participant at an Employment Agency. The data are constructed as an event history data set with start and end dates measured on a daily basis. An important feature of the data is that it contains parallel spells in the case of overlapping events. Moreover, the IEBS comprises a large set of variables that give a detailed picture on socio-economic, occupational and job characteristics, as well as on job search and contents of active labor market programs.

The Employment History and the Benefit Recipient History contain spells of employment and receipt of different types of unemployment benefits, respectively. The two data sources cover a time span ranging from January 1990 to December 2004 (employment) and June 2005 (benefits), respectively. The information on start and end dates as well as salaries and benefit payments is of high accuracy in these two files because it is directly relevant for the underlying administrative purposes. Furthermore, the information in the Employment and the Benefit Recipient History allows one to calculate the individual entitlement periods to unemployment benefits.[9]

The Program Participation History contains detailed information on participation in active labor market programs taking place in the period 2000 to mid-2005. An inspection of this data base shows that information on program participation in the second half of 1999 seems already quite complete, too. Furthermore, this data base allows to distinguish subsidized employment in the context of active labor market

[8] For a general description of the data see Jacobebbinghaus and Seth (2007) and Hummel et al. (2005). The version I use contains additional variables that are not publicly available.

[9] For the calculation of the claims, the present study relies on Plaßmann (2002) that contains a summary of the different regulations.

policy from regular employment.

The Job-Seeker Data Base contains information on job search episodes. In particular, it allows to distinguish whether an individual is registered as a job-seeker who may still hold a job or as an unemployed. Whereas the Employment and the Benefit Recipient History contain only a limited set of variables, the Job-Seeker Data Base includes a rich variety of information on personal characteristics (in particular education, family status and health condition), and information related to placement fields (e.g. qualification and experience in the target profession). The Job-Seeker Data Base contains all the records starting January 2000 to June 2005 and partly also those beginning before 2000 if the person in question keeps the same client number throughout.

3.2 Analysis Sample

For the estimation I use a sample of West German individuals aged 25 to 53 who experience a transition from regular, unsubsidized employment lasting three months or more to unemployment within the period July 1999 to December 2001. Unemployment (UE) is defined as non-employment with at least occasional contact with the employment agency that may consist either in receipt of some kind of unemployment compensation, a job search spell, or program participation. Unemployment spells are censored at the end date of the last contact with the employment agency if in the following three months no such contact persists. Transitions to other active labor market programs than training are also treated as independent censoring.

I consider two types of training programs: short-term training (ST) and the traditional further and retraining (FT). Thus, there exist three competing risks where the censoring mechanism works as follows. If a transition from unemployment towards short-term training (spell type UE to ST) occurs first, then the waiting time until further training (spell type UE to FT) is treated as censored at the time of entry into short-term training. The opposite is true if further training takes place first. If no transition towards training occurs before the termination of the unemployment spell (spell type UE to EM) – either due to a transition towards employment or because of censoring (cf. above) – then the waiting times until short-term training (UE to ST) and further training (UE to FT) are treated as censored at the termination date of the unemployment spell.

In addition, I do not only model unemployment duration but I also consider the subsequent employment duration (spell type EM to UE). Individuals in the sample may have multiple unemployment and employment spells if they experience multiple transitions between unemployment and employment.[10] The observation period lasts until the end of December 2004, and spells that do not end before that date are treated as censored. Overall the sample consists of 45,490 individuals and 327,302 spells. Tables 3 and 4 below give further details.

— Insert table 3 here. —

— Insert table 4 here. —

4 Econometric Implementation

4.1 Identification Strategy

Suppose that individuals dynamically move between the two labor market states unemployment and employment. While unemployed, they may take part in one of two types of training. Assignment into treatment is not a one shot decision, but occurs randomly during the unemployment spell. Thus, somebody who has not participated until day 80 of his unemployment spell may still enrol later. If, however, he starts a new job at day 81 he would not have participated at all. In this study, I want to analyze the dynamic short- and long-run effects of labor market training under dynamic treatment assignment. Formally speaking, the quantity of interest is the effect of the realization of the moment of treatment on the outcomes unemployment duration and subsequent employment duration. In order to estimate these causal effects, I adopt the timing-of-events approach proposed by Abbring and van den Berg (2003a). This approach takes its motivation from economic search theory. It is appropriate when the assignment into treatment of individuals who have not yet left the baseline state occurs randomly over time. A major advantage of this framework is that it avoids specification issues arising as a consequence of crude and ad hoc discretization of inherently continuous dynamic features of the data.[11]

[10] Any previous treatment is assumed to be irrelevant when an individual experiences a second unemployment or employment spell.

[11] Abbring and Heckman (2007) contains an overview over different approaches to the evaluation of dynamic treatment effects.

In particular, assume that one has access to a large data set including observations on realized durations of the outcome state unemployment and the waiting time until treatment. The model proposed by Abbring and van den Berg (2003a) has the following features. They consider a continuous time duration model where time until treatment start and unemployment duration constitute two competing risks. The two durations are allowed to depend on each other through dependent unobserved heterogeneity terms. Each duration has a mixed proportional hazards structure where the multiplicative components (i.e. the baseline hazard, the observed covariates, and the unobserved heterogeneity term) of each hazard may be specified nonparametrically. Additionally, the specification of the outcome duration contains a possibly time-varying and heterogeneous specification for the causal treatment effect that affects the outcome exit rate from the start of treatment on onwards.

Abbring and van den Berg's (2003a) identification strategy exploits the information on the timing of treatment. While selection effects driven by observable and unobservable determinants globally affect the exit rates from their beginning, the treatment effect only operates from the moment of program start onwards. Under the assumption that the moment of treatment is not anticipated at the individual level, the treatment and outcome process correspond until the treatment start to a standard competing risks model with dependent risks (cf. Heckman and Honoré, 1989, Lancaster, 1990). Intuitively, the treatment effect can then be traced out by comparing the realizations of the outcome duration and the waiting time until treatment, given that the moment of treatment has occurred, with those that would prevail if the treatment had not yet occurred (Abbring and van den Berg, 2004). It follows from competing risks theory that the bivariate model considered by Abbring and van den Berg (2003a) can be extended to a multiple competing risks and multiple treatments model (Abbring, 2006, Proposition 1). The basic identification results of Abbring and van den Berg (2003a) have been extended to models that incorporate a second outcome duration (Crépon et al., 2005), i.e. subsequent employment, and a mixed proportional treatment effect specification (Richardson and van den Berg, 2006).

Identification of the causal treatment effect presupposes access to single-spell data (i.e. one of each spell type per individual) and additionally relies on the following three fundamental assumptions: (i) proportional structure of the hazards, (ii) independence of observed covariates and unobserved heterogeneity terms, and (iii) conditional randomness of program starts at the individual level (no anticipation). However, exclusion restrictions are not required, but only independent variation of the treatment and the outcome hazards. In addition, no parametric functional form restrictions on the multiplicative components of the hazards need to be imposed.

In principle, assumptions (i) and (ii) can be relaxed if the data situation is more favorable than in the baseline case. With multi-spell data, the mixed proportional structure of the hazards can be relaxed and independence between observed and unobserved determinants is not required anymore, provided that the unobserved components are constant across spells of a given type and a given individual.[12] In fact, identification does not depend on the variation of the hazards with observed covariates anymore, but exploits the variation of the durations within individual and spell type, similar as in fixed effects panel data models (Abbring and van den Berg, 2004).[13] Time-varying covariates provide an additional useful source of exogenous variation generally facilitating identification (Heckman and Taber, 1994).[14]

The present analysis is based on a large administrative data base. In particular, I have a rich set of covariates that are updated at the start of each spell, among them some that vary over time within spells.[15] Thus, there is a great amount of regressor variation at the individual level as well as across the four equations that I am considering. Empirically, there arise exclusion restrictions as some regressors are significant in some equations but not in others. Moreover, I use multiple unemployment-employment cycles per individual. In fact, about half of the individuals in the sample experience two or more unemployment spells and about a third has two or more employment spells. Taken together, these data features give further credibility to my estimation results because they do not solely rely on the proportionality and independence assumptions.

Next, examine further the empirical content of assumption (iii). A first requirement is that there is variation of starting dates over elapsed unemployment duration. As can be seen from figure 1 the hazard rates into the two programs are positive over elapsed unemployment measured in months. The monthly hazard rate of short-term training varies between 0.5 and 2% and that of further and retraining lies between 0.1 and 1%. Second, this variation should persist at the individual level.

[12] This excludes lagged duration dependence.

[13] Formal proofs for the case of multi-spell data on a single risk or two competing risks can be found in Honoré (1993) and Abbring and van den Berg (2003a, b), respectively. The extension to the case of multi-spell multiple competing risks is obtained by repeatedly applying the identification results for the bivariate case (Abbring and van den Berg, 2003b). Abbring (2006, Proposition 3) discusses identification in the more general case when the distribution of initial states is not degenerate.

[14] Without time-varying regressors, one additionally needs finiteness of the mean of the unobservables or an assumption on the tail of their distribution for identification.

[15] See table 9 in the appendix for a description of the regressors used.

This means that conditional on observed and unobserved variables the moment of treatment should still be a random variable. At this stage, it is useful to distinguish anticipation effects from ex ante treatment effects, cf. Richardson and van den Berg (2006). Ex ante effects arise if the job-seeker knows that he has a high probability of entering a program and adjusts his search behavior accordingly. Ex ante effects do not invalidate the present analysis that aims at estimating the ex post treatment effect.

— Insert figure 1 about here. —

Anticipation effects, on the contrary, arise if the job-seeker knows the exact starting date of the program before it is observable to the econometrician. Unless individuals are indifferent between searching in open unemployment and participating in a program, anticipation of the exact starting date of the treatment may pose a problem in an empirical analysis. It is clear that, as long as only actual starting dates of programs are recorded in the data and not the date at which the job-seeker obtained the information that he would enrol in a program in the near future, estimated treatment effects may be biased. However, if invitations to training programs are given at short notice it is likely that the job-seeker deos not have enough time or capacities to react. Alternatively, if the arrival rate of job-offers is low even an instantaneous adjustment of the job-seeker's search behavior will remain without consequences. Overall, potential biases should be small if the time span between the first knowledge of program start and actual program start is small relative to typical values of the time to treatment and outcome durations (Richardson and van den Berg, 2006).[16] As discussed in section 2.1, the allocation of unemployed to training programs greatly depends on factors that are known only shortly in advance by the job-seeker, as e.g. short-term allocation decisions of the caseworker or whether the number of other participants in a chosen course exceeds a critical threshold for the course to take place. Moreover, even if small anticipation effects exist, they may well cancel each other out. There is indeed no evidence for Germany that there are more individuals who dislike participating than those who enjoy it. Therefore, I am confident that estimated treatment effects should not be significantly affected by biases due to anticipation of the exact moment of treatment.

[16] Note that anticipation effects may also occur if a job-seeker has private knowledge about a future job offer that is independent of his participation in a training program.

4.2 Modeling the Hazard Rates

For an inflow sample into unemployment, I model the hazard rates from unemployment to employment and to training as well as from employment to unemployment. Two different types of training programs are considered: short-term training and traditional medium- to long-term training. If an individual experiences multiple unemployment or employment spells throughout the observation period all of them are retained. Analysis time is measured in days.

Let $x(t)$ denote a row-vector of observed time constant as well as time-varying covariates, β_k the parameter vector of spell type k, $k = 1, \ldots, 4$, v_k the unobserved heterogeneity term, and $\lambda_k(t)$ the baseline hazard. The conditional transition intensity for the waiting time until the start of one of the two training programs is then:

$$\theta_k(t|x(t), v_k) = \lambda_k(t) \cdot \exp(x(t)\beta_k) \cdot v_k, \; k = 2, \, 3.$$

Similarly, the hazard rate from unemployment to employment is:

$$\theta_1(t|x(t), v_1) = \lambda_1(t) \cdot \exp(x(t)\beta_1) \cdot \prod_{k=2}^{3} \exp[\delta_k(\bullet) \cdot I(t \geq t_k, t_k = \min(t_1, t_2, t_3))] \cdot v_1$$

where the function $\delta_k(\bullet)$ corresponds to the treatment effect of participating in training type k and $I(\bullet)$ is the indicator function. In the benchmark model the treatment effect is modeled as a function of time only, i.e. $\delta_k(\bullet) = \delta_k(t|t_k)$. The heterogeneous effects and the mixed proportional treatment effect model also consider dependence on observed and unobserved determinants, i.e. $\delta_k(\bullet) = \delta_k(t|t_k, x)$ and $\delta_k(\bullet) = \delta_k(t|t_k, x, v_{\delta_k})$, respectively.

Finally, the hazard rate while employed equals:

$$\theta_4(t|x(t), v_4) = \lambda_4(t) \cdot exp(x(t)\beta_4) \cdot \prod_{k=2}^{3} \exp[\gamma_k(\bullet) \cdot I(t_k = \min(t_1, t_2, t_3))] \cdot v_4$$

where $\gamma_k(\bullet)$ is the treatment effect. Analogously to the unemployment hazard, $\gamma_k(\bullet) = \gamma_k$ is a constant treatment effect in the benchmark model and a function depending on observed covariates in the heterogeneous effects model $\gamma_k(\bullet) = \gamma_k(x)$. The mixed proportional treatment effect model additionally allows for dependence on unobservables $\gamma_k(\bullet) = \gamma_k(x, v_{\gamma_k})$.

For the baseline hazards, I use a flexible piecewise constant specification:

$$\lambda(t) = \exp(\sum_d \lambda_d I(t_d < t \leq t_{d+1})).$$

4.3 Modeling Unobserved Heterogeneity

In order to avoid unnecessary parametric functional form restrictions, it is common to model the unobserved terms as a discrete masspoint distribution that in principle allows to approximate any arbitrary discrete or continuous distribution (Heckman and Singer, 1984). In particular, I adopt a two factor loading model where the two underlying factors, w_1 and w_2, are assumed to be independent:

$$v_k = \exp(\alpha_{k1}\, w_1 + \alpha_{k2}\, w_2),\ \ k = 1, \ldots, 4.$$

Each of the two factors follows a masspoint distribution with two masspoints.[17] This specification has the advantage that, while maintaining computational tractability, it imposes no restrictions on the covariance matrix of the four unobserved heterogeneity terms. Let $w = (w_1, w_2)'$ and A be the matrix of factor loadings with rows $A_k = (\alpha_{k1},\, \alpha_{k2}),\ \ k = 1, \ldots, 4$. Then the variance-covariance matrix of the unobserved heterogeneity terms is given by $\mathrm{Var}(\ln(v)) = A\,\mathrm{Var}(w)\,A'$. It is well known that such a factor loading specification requires some normalization in order to be identified. I normalize the two fundamental factors to have support on $\{-1, 1\}$ and in addition I constrain α_{21} to equal zero.[18]

4.4 The Likelihood Function

Conditional on the observed covariates and the unobserved determinants the joint density of the four durations for individual i is given by:

[17] Neither the latent factors nor their (number of) masspoints should be given a concrete interpretation. The latent factors just represent one dimensional indexes of model determinants that are unobserved by the econometrician and therefore not precisely known. Further, Heckman and Singer (1984) suggest that a relatively small number of support points for the unobserved heterogeneity terms suffices to get reliable estimates of the parameters of the observed determinants.

[18] Note that all hazards contain an intercept. Crépon et al. (2005) also use this normalization. In fact, this normalization is more convenient from a programming perspective than the alternative way to assume a block diagonal matrix for the factor loadings and to fix one of the factor loadings on each factor to one while leaving the locations of the masspoints and $\mathrm{Var}(w)$ unrestricted.

$$f_i = \prod_{k=1}^{4} \prod_{j=1}^{N_{ik}} \theta_k(t_{ijk}|x_{ij}(t_{ijk}), v_k)^{c_{ijk}} S_k(t_{ijk}|x_{ij}(t_{ijk}), v_k)$$

where N_{ik} is the number of spells that individual i spends in state k, c_{ijk} is a censoring indicator that equals one if the jth observation period in state k ends with a transition, and $S_k(t_k|x(t_k), v_k) = \exp[-\int_0^{t_k} \theta_k(\tau|x(\tau), v_k)d\tau]$ denotes the survivor function. Since the unobserved heterogeneity terms may in general be correlated, the likelihood function is not separable by individual and spell type but only at the individual level. Thus, the individual likelihood contribution conditional on observed covariates and integrated over the unobserved heterogeneity terms is:

$$\mathfrak{L}_i = \int_0^{\infty} \prod_{k=1}^{4} \prod_{j=1}^{N_{ik}} \theta_k(t_{ijk}|x_{ij}(t_{ijk}), \nu_k)^{c_{ijk}} S_k(t_{ijk}|x_{ij}(t_{ijk}), \nu_k) dG(\nu_1, \nu_2, \nu_3, \nu_4).$$

In order to determine suitable specifications for the four hazard rates, first a univariate mixed proportional hazards model, with unobserved heterogeneity modeled as a discrete masspoint distribution, is fitted for each. Starting values for the coefficients are chosen based on an iterative procedure. First, a piecewise constant exponential model without unobserved heterogeneity is fitted. Then, starting values for the parameters involving the mixing distribution are determined through a grid search. The parameter vector yielding the highest log likelihood is retained as initial vector for the final optimization that uses a modified Newton-Raphson algorithm with analytic first and second derivatives. The use of analytic derivatives considerably speeds up the estimation that – in spite of the large data size and the great variety of covariates considered – altogether takes only a couple of minutes per run. The covariates and their functional forms are chosen separately for each hazard based on single and joint significance and the value of the log likelihood function. The width and number of intervals for the baseline hazards vary over the four hazards as well. They are selected according to the shape of Kaplan-Meier estimates of the hazard rates and additional criteria as significance. Finally, starting from the optimal specifications for the single hazards, the multivariate mixed proportional hazards model is estimated also using a modified Newton-Raphson algorithm with analytic first and second derivatives.[19] The final specifications presented in the next section involve the estimation of 239 to 273 parameters.

[19] All the estimations are carried out with Stata MP Version 9 and its matrix language Mata.

5 Empirical Results

The estimation is based on an inflow sample into unemployment. The econometric model comprises two treatments, i.e. short-term and traditional further training, and two outcomes, i.e. unemployment and employment. The unemployment and employment duration as well as the waiting times until treatment start are modeled as a multivariate mixed proportional hazards model that is estimated by maximum likelihood methods. All four hazards contain an intercept. Additionally, the specifications contain categorical and continuous time constant as well as time-varying variables. Unobserved heterogeneity is modeled as a two factor loading specification composed of two independent factors. Each latent factor follows a discrete mass-point distribution with support $\{-1, 1\}$. The factor loading of the first hazard (i.e. UE to EM) on the second factor is normalized to zero.[20]

5.1 Benchmark Model

Previous work on the employment effects of training programs (e.g. Biewen et al., 2007) has shown that in the short run training programs typically exhibit substantial negative employment effects. This is due to the so called lock-in effect that arises because, while in training, participants have a lower exit rate to employment than comparable non-participants. In the benchmark model, treatment effects on the exit rate to work are therefore modeled as a function of elapsed time since program start. In particular, I use a piecewise constant specification to model the time dependence of the two treatment effects $\delta_k(t - t_k) = \sum_d \lambda_d I_d(t - t_k \geq t_d)$, $k = 2, 3$. The time intervals have been selected through a specification search that sequentially aggregated intervals where no or only little variation of the treatment effect over time was found. In the employment to unemployment equation the treatment effect corresponds to a dummy variable that equals one if a participation in the respective treatment occurred during the preceding unemployment spell.

Table 5 displays the marginal effects of training on the log exit rates to employment and unemployment, respectively.[21] The results show that the treatment effects on the unemployment hazard do in fact vary significantly with elapsed unemployment duration. The impact of short-term training (ST) on the hazard to employment

[20] The complete estimation results are given in table 10 in the appendix. Table 9 in the appendix contains the variable descriptions.

[21] The complete estimation results can be found in table 10 in the appendix.

is strongest right after program start – i.e. the escape rate out of unemployment is increased by $(exp(.34)-1)\cdot 100\% = 40\%$ during the first 7 weeks after program start – and then declines until it becomes insignificant one year after program start. After two years it becomes positive again. This last rise is likely attributable to a second training program that the unsuccessful unemployed may have completed by then. As regards the marginal effect of participating in traditional further training (FT), it turns out that during the first year participation substantially lowers the exit rate to work by $(exp(-.75)-1)\cdot 100\% = -53\%$. But after this lock-in period the treatment effect turns positive and increases further until two and a half years since program start, raising the escape rate out of unemployment by 172%. The treatment effect of traditional further training on the exit rate from employment back to unemployment is about twice as large in absolute terms than that of short-term training. The escape rate out of employment is lowered by 8.6% for participants in short-term training and by 16.5% for those in long-term training. Hence, employment is more stable for participants in long-term training than for those in short-term training.

— Insert table 5 about here. —

5.2 Heterogeneous Effects Model

As a next step, I investigate whether the treatment effects vary across observable characteristics. Allowing for heterogeneous treatment effects results in a considerable increase of the log likelihood function (cf. second column of table 10).[22] Table 6 shows the specifications for the heterogeneous treatment effects. Introducing heterogeneity across observed characteristics does not change the dynamic patterns of the treatment effects in the unemployment equation qualitatively (upper panel of table 6). As regards the employment hazard (lower panel of table 6), the base effect of traditional further training is hardly affected as well (it goes from -.18 to -.17). However, the corresponding effect of short-term training is reduced by one third in absolute terms (changing from -.09 to -.06) and has become insignificant. Thus, the treatment effect of short-term training on the exit rate to unemployment can be fully explained by the observed characteristics of the participants. This suggests that, at least with respect to employment duration, the assignment of short-term training schemes should be targeted towards job-seekers exhibiting those characteristics that make a participation beneficial for them.

[22]The log likelihood increases from $-847,431$ to $-847,357$. The corresponding likelihood ratio test statistic is 148.1 and comprises 26 constraints. Thus, the null hypothesis of homogeneous treatment effects can be rejected at any conventional significance level.

In fact, there are interesting differences in treatment effects across the characteristics of the participants. The effect of traditional further training is significantly larger in absolute terms for women than for men. Women participating in long-term training have a 9% higher exit rate out of unemployment and a 13% lower exit rate out of employment than men. However, there is no evidence for significant gender differences regarding short-term training. The positive marginal effect of age on the unemployment hazard for participants in further training diminishes as age increases and turns negative at the age of 43. This means that older participants benefit less from traditional further training when the outcome is unemployment duration. As regards the age effects of short-term training, there is some evidence for a concave profile for the unemployment hazard as well, but the coefficients of age and age squared are insignificant. There is no evidence for heterogeneous effects across age with respect to the exit rate from employment back to unemployment. Thus, once employed there are no differences in treatment effects for individuals of a different age. While disabled persons fare better with short-term training than with traditional further training, individuals with health constraints benefit more from traditional further training. In particular, there is a strong employment duration reducing effect of health constraints for participants in short-term training. The exit rate out of employment is 56% higher for somebody treated with short-term training who has health constraints, whereas that of a nonparticipant with health constraints is 44% higher, where both comparisons are relative to a healthy nonparticipant. Either training program is more beneficial to foreigners compared to Germans when the outcome is the exit rate to work. Foreigners participating in short-term training have a 10.5% higher escape rate out of unemployment than their German counterparts and individuals treated with further training holding a foreign nationality have a 18.6% higher unemployment hazard than German participants. This suggests that training schemes that improve search efficiency and provide signals to employers in form of accredited certificates are particularly effective in order to reintegrate foreigners back into employment who, without training, face higher placement difficulties than Germans.[23] However, being a foreigner tends to offset the employment prolonging base effect of traditional further training. Finally, there is only limited treatment effect heterogeneity across educational degrees. Holding a university or technical college degree tends to offset the negative effect of a participation in further training on the exit rate from employment to unemployment. There are no significantly heterogeneous treatment effects of educational degrees for short-term training in the employment hazard and for both types of training in the

[23]Note that the unemployment hazard for foreign nonparticipants is 3.5% lower than that of Germans.

unemployment hazard.

— Insert table 6 about here. —

5.3 Mixed Proportional Treatment Effect Model

Last, I estimate a model where the treatment effects are allowed to depend on unobservables as well. Thus, the function for the treatment effect entering the unemployment hazard, is now specified as $\delta_k(t|t_k, x, v_{\delta_k}) = \lambda_{\delta_k}(t - t_k)exp(x\beta_{\delta_k})v_{\delta_k}$, $k = 2, 3$. This can be called a mixed proportional treatment effect model (Richardson and van den Berg, 2006). Similarly, the treatment effect on the employment hazard now corresponds to $\gamma_k(x, v_{\gamma_k}) = exp(x\beta_{\gamma_k})v_{\gamma_k}$. Since unobserved heterogeneity is modeled as a factor loading specification, introducing unobserved heterogeneity in the treatment effect equations corresponds to allowing for two further nonzero factor loadings on each of the two fundamental factors.

The estimation results displayed in the last column of table 10 indicate again a significant improvement of the log-likelihood function compared to the previous model.[24] This suggests that unobserved heterogeneity is in fact an important feature of the treatment effects. In general, the estimated coefficients of the observed and unobserved heterogeneity components that were already present in the previous model do not change systematically. As regards the treatment effects, the duration dependence patterns and differences across observed characteristics remain qualitatively the same. Quantitatively, in the employment hazard, the negative base effect of a participation in short term training is halved (from previously -.06 to -.03). However, the corresponding effect of traditional further training changes only marginally (from previously -.17 to -.16) and remains significant.

Table 7 reveals interesting correlation patterns between the unobserved heterogeneity terms of the four hazards and the four treatment effects. As expected, the unobservables affecting unemployment and employment duration, respectively, are negatively related with a correlation coefficient of -.38. This means that there are certain types of individuals who are likely to experience longer unemployment durations as well as shorter employment spells relative to others with the same observable characteristics but more favorable unobservable traits. In this sense, one can speak of “bad risk” individuals who exhibit both lower (re-)employment chances and

[24]The log likelihood increases from $-847,357$ to $-847,289$ and four additional parameters are introduced.

higher job instability and more able and/or more motivated "good risk" individuals for whom the opposite holds.

The correlation between unemployment duration and the waiting time until long-term training is .72, while that between unemployment duration and the waiting time until short-term training is -.53. Thus, unemployed whose exit rate to work is relatively high have a low hazard into short-term training, but a high one into traditional further training. This suggests that there may be some sort of cream skimming on the part of the caseworker resulting in individuals facing a higher risk of becoming long-term unemployed being less likely assigned to the more expensive long-term training schemes, but rather to the inexpensive short-term training. Likewise, the propensity to enrol into either type of training is higher for "good risk" individuals whose exit rate out of work is relatively low (cf. the correlation coefficients of -.58 in row 6, column 4, and -.91 in row 6, column 5 in table 7). The strongly negative correlations between the unobserved components in the treatment effects on unemployment and the unemployment hazard (-.76 for ST and -.95 for FT) indicate, however, that the "bad risk" unemployed are those who potentially benefit most from either training type, when the outcome is unemployment. The strongly negative correlation between the treatment effect of FT on employment and the unemployment hazard (-.98) suggests on the other hand that the employment prolonging effect of traditional further training is particularly high for the "good risk" unemployed.

The correlation coefficients between the waiting time until short-term training and the treatment effects of both types of training on unemployment are positive (.96 for ST and .77 for FT). Also, the propensity to enrol into traditional further training is negatively correlated with the treatment effects of both types of training on the employment hazard (.92 for ST and .84 for FT). The waiting time until traditional further training and its treatment effect on unemployment are negatively correlated (-.47), and the treatment effects of traditional further training on unemployment and employment are positively correlated (.87). Overall, these correlation patterns fit the above observation of a positive selection of unemployed among the participants, because the "good risk" individuals exhibiting a higher disposition to enrol into a program also gain more, as regards short-term training in terms of unemployment reduction and with regard to long-term training in terms of employment prolongation.

Finally, the correlation between the two hazards into training is slightly positive but insignificant. This suggests that the decision of referral to one program type is made independently from that for the other.

— Insert table 7 about here. —

5.4 Summing it all up: Expected Outcome Durations, the Long-Run Unemployment Rate, and the Timing of Treatment

So far, I have discussed treatment effects on the hazard rates out of unemployment and employment. Another way to analyze treatment effects in an event history framework is to study the expected unemployment duration for a given elapsed time until treatment start. This allows to conceive how the treatment effects accumulate over time and how these accumulated effects vary with starting date. Likewise, one can compute the expected employment duration according to the treatment status in the previous unemployment spell. Comparing expected unemployment and employment durations allows to assess short-run and long-run impacts of program participation. Furthermore, the long-run fraction of time spent in unemployment is useful to get an idea of the overall effectiveness as well as the cost effectiveness of the programs.[25]

In the absence of episode splitting, the expected unemployment duration would simply equal the reciprocal of the hazard rate that does not depend on elapsed unemployment duration in the exponential model. With time varying covariates and the piecewise constant baseline hazard and treatment effects, the expected unemployment duration corresponds to

$$\begin{aligned}\mathrm{E}[T_1|\{x(t)\},v] &= \int_0^\infty S_1(\tau|x(\tau),v)d\tau = \sum_{d=1}^\infty \int_{t_{d-1}}^{t_d} S_1(\tau|x(\tau),v)d\tau \\ &= \sum_d \frac{1}{\theta_1[t_d|x(t_d),v]} \cdot \{S[t_{d-1}|x(t_d),v] - S[t_d|x(t_d),v]\}\end{aligned}$$

where $\{x(t)\}$ denotes the entire covariate process including the baseline hazard, $x(t_d)$ refers to the covariates in time interval d, and v represents the unobserved heterogeneity affecting unemployment duration and in the mixed proportional treatment model also the treatment effects. The expected employment duration is computed analogously.

[25] Eberwein et al. (2002) give an overview over different possibilities to summarize treatment effects in duration models. See also Lancaster (1990, ch. 1 and 5).

In order to evaluate the expected outcome duration, the covariates are set to the values of a fictitious, but representative person in the inflow sample. Specifically, I assume that this person is a German aged 38 at the start of unemployment and 39 at the beginning of employment, married with children, living in one of the middle Länder of Germany, holding the highest high school degree (Abitur) and a vocational training degree, previously employed as a whitecollar service worker, with a salary in the second quartile, entitled to twelve months of unemployment benefits, considered as having relevant vocational qualification by the caseworker, and starting their unemployment spell in the first quarter of the year 2000 and the employment spell one year later.[26] The unobserved heterogeneity terms are set to their mean values. The expected unemployment and employment duration are then computed for a man and a woman with the above mentioned characteristics.

Figure 2 depicts the expected unemployment duration according to the treatment status and the waiting time until program start. The top panel refers to the benchmark model, the middle to the heterogeneous effects model and the bottom panel to the mixed proportional treatment effect model. The left column is for the representative man, the right for the woman. In each graph, the vertical distance between two curves corresponds to the treatment effect in terms of the difference in expected unemployment duration, where the latter is computed as the integral of the survivor function from day zero to infinity. Consider e.g. the top left graph of figure 2. It can be seen that the representative man stays on average 433 days in unemployment when not participating in a training program.[27] Equivalently, this number can be interpreted as the expected unemployment duration that would arise if he participates in training at some given starting date but the treatment effect is hypothetically set to zero. The vertical distance between the "No-training"-line and the ST-line measures the reduction in average unemployment duration that can be achieved through a participation in short-term training starting at some given day of unemployment. Similarly, the vertical distance between the FT-line and the "No-training"-line shows the difference in expected unemployment duration associated with participation in traditional further training compared to nonparticipation.

— Insert figure 2 about here. —

[26] All other (categorical) variables included in the specification not mentioned here are set to zero.

[27] The median is only about 150 days for the representative man, that is the distribution of unemployment durations is considerably right skewed. However, I prefer to discuss the mean instead of the median, because the former is of interest for a cost-benefit analysis.

Overall, the graphs reveal that, compared to nonparticipation, short-term training reduces average unemployment duration while traditional further training tends to increase it at least for early program starts. These effects are larger the earlier participation occurs during the unemployment spell. In fact, the mean unemployment duration of the representative man can be reduced, depending on the specification, by 79 to 125 days if short-term training occurs during the first three months of unemployment. Depending on the underlying model, a participation in traditional further training starting in the first three months of unemployment increases the mean unemployment duration by 119 (benchmark model) to 237 days (mixed proportional treatment effect model), and by 63 to 156 days when traditional further training starts in months four to six of unemployment. The pictures for the representative woman are qualitatively similar. The "No training"-line is shifted down by approximately 85 days and the vertical spread of the ST-line is somewhat compressed.

In order to better understand these patterns, recall that the unemployment hazard exhibits negative duration dependence.[28] Also, the treatment effect of short-term training on the unemployment hazard is highest right after program start and decreases thereafter. Traditional further training, on the contrary, reduces the unemployment hazard during the first year since program start and increases it strongly from the third year onwards. This means that the positive impact of short-term training and the negative lock-in effect of long-term training are strongest for somebody taking part early in their unemployment spell. Thus, the mean unemployment duration is shorter for earlier ST-starts than for later starts, and the mean unemployment duration of FT-participants decreases with elapsed unemployment duration at program start.

In order to get an idea of the long-term impact and the overall effectiveness of the programs, it is useful to complement their effects on average unemployment duration with those on subsequent average employment duration. Table 8 displays the mean employment duration by treatment status in the preceding unemployment spell.[29] It turns out that participation in either program considerably increases subsequent employment duration. In particular, a representative man can expect an increase of 2.5 months (MP treatment effect model) to six months (benchmark model) in

[28]In fact, negative duration dependence of unemployment hazards can be viewed as a stylized empirical fact that holds for many countries (cf. e.g. Machin and Manning, 1999).

[29]The distribution of employment durations is also considerably right skewed. According to the benchmark model the median employment duration without training is 991 days for the representative men.

subsequent employment duration after a participation in short-term training and an increase of one to 1.5 years after taking part in traditional further training. The effects on expected employment duration for the representative woman are similar in the benchmark model where the treatment effects are not allowed to vary across observable characteristics. However, according to the heterogeneous effects and the mixed proportional treatment effect model, that do account for gender differences in the treatment effects, the increase in expected employment duration for the representative female participant is 50-75% larger than that of the male.

— Insert table 8 about here. —

In sum, the above findings suggest that the fraction of time spent in unemployment of a participant in short-term training is reduced in the long run because short-term training reduces unemployment duration and increases employment stability at the same time. As regards traditional further training the sign of the total effect is ambiguous as on the negative side long-term training increases unemployment duration, while on the positive side it increases subsequent employment duration. Therefore, I also calculate the fraction of time spent in unemployment by relating the expected unemployment duration to the sum of the expected unemployment and employment duration. This gives the long-run unemployment rate that is depicted in figure 3 for the representative man and woman as a function of participation status and starting date of the program. The interpretation of this figure runs analogous to that of figure 2: the vertical distance between two lines represents the treatment effect associated with a participation at some given day of unemployment, except that this time the vertical difference measures the percentage point change in the long-run unemployment rate.

— Insert table 3 about here. —

As expected, figure 3 shows that a participation in short-term training reduces the long-run unemployment rate, and in fact the effect is larger the earlier a participation occurs during the unemployment spell. Traditional further training, in contrast, is only effective in the long run if it is not started too early during the unemployment spell. From a policy point of view, it becomes clear that the timing of a treatment is an important aspect of active labor market policy. Short training programs that do not lock the participants into the program should preferably be assigned very early in the unemployment spell when the absolute gain out of a participation is highest. Long training programs, in contrast, should not be assigned too early in the unemployment spell in order to avoid that participants are locked into the program while their chances to find a job on their own are highest. The results also show that the treatment effects for men and women differ considerably. Hence, an optimal assignment policy needs to carefully consider the costs and benefits of a program as a function of elapsed unemployment duration and the characteristics of the potential participant, and then target participation accordingly.

In order to get an idea of the cost-effectiveness of the programs, consider the following back-of-envelope calculations. Recall from table 2 that a job-search training course costs on average 560 Euro and a traditional further training course 5850 Euro. The labor office pays on average 1050 Euro unemployment compensation per month for an unemployed entitled to unemployment benefits. Thus, abstracting from all other costs and gains associated with unemployment and employment, a job-search training scheme will be cost effective if it saves in the long run more than 16 days of unemployment per participant entitled to unemployment benefits. A traditional further training course will be cost effective if it saves at least six months of unemployment in the long run.

According to the mixed proportional treatment effect model, a complete unemployment-employment cycle lasts on average about six years. The representative man spends $72 \times 0.2 \approx 14.5$ months out of these six years in unemployment if he does not participate in any training program. First consider the cost-effectiveness of an early participation in job-search training. If the representative man enrols in short-term training during the first three months of his unemployment spell he will stay in total $72 \times 0.17 \approx 12$ months out of six years unemployed. The representative woman also saves 2.5 months in unemployment through participating in job-search training. The long-run effectiveness of short-term training declines with elapsed unemployment duration, but even for very late program starts a participant can save more than one month in unemployment in the long run. Hence, these figures suggest that short-term training is cost effective in the long run. Next consider the cost-effectiveness of a very late participation in traditional further training when its long-run effectiveness is highest. If the representative man starts training between day 642 and 735 the long-run fraction of time spent in unemployment decreases by 3.7%-points which is equivalent to 2.7 months out of six years. The representative woman saves 3.8 months in unemployment. Comparing these numbers with the six-months rule established above, traditional further training does not seem to be cost effective.[30]

6 Conclusion

In the recent years, the focus of active labor market policy has shifted away from the comprehensive long-term training schemes towards active job-search schemes putting more emphasis on reducing search frictions in the short run. Motivated by this policy reorientation, this study investigated the dynamic causal effects of short job-search training and traditional long-term training schemes in Germany on the exit rate to employment and the exit rate from subsequent employment back to unemployment. For this purpose, a multivariate duration model that dealt with selection on unobservables as well as heterogeneous treatment effects was estimated from rich administrative data.

The empirical results obtained in this study indicate significantly positive effects on the exit rate to employment and negative effects on the exit rate back to unemployment for both treatments. While the effects of job-search training on the unemployment hazard are highest shortly after program start and fade away over time, those of long-term training occur only after an initial lock-in period of one year. The effect of traditional long-term training on the subsequent employment hazard is, however, in absolute terms much larger than that of short-term training and highly significant. In fact, the baseline effect of short-term training on the exit rate to unemployment becomes insignificant when one accounts for heterogeneous treatment effects.

Participating in traditional further training has stronger beneficial effects for women than for men. There are concave age profiles of training effects on the hazard rate to work. But only those for traditional further training are significant. Treatment effects also vary by disability and health status. While disabled persons fare better with job-search training than with traditional further training, individuals with health constraints benefit more from long-term training. Either training program is

[30] Getting back to the heterogeneous treatment effects, I also calculated the long-run unemployment rate for foreign and disabled men and women with otherwise identical characteristics. It turns out that short-term training is even more effective for individuals with these characteristics than for the cases discussed above. Long-term training, on the contrary, is less effective for disabled persons and foreign men compared to the representative man and woman above. A female foreign participant gains more out of traditional further training than the representative woman above, but the long-run reduction of time spent in unemployment is still smaller than the cost effective amount.

more beneficial to foreigners compared to Germans when the outcome is the exit rate to work. However, being a foreigner tends to offset the employment prolonging effect of long-term further training. There is only little evidence for effect heterogeneity by educational degrees. Holding a university or a technical college degree tends to offset the employment prolonging effect of a participation in further training.

There arise interesting correlation patterns between the unobserved heterogeneity terms in the four hazards and the four treatment effects. In particular, the correlations between unemployment duration, the waiting times until job-search and traditional further training as well as the treatment effects of both types of training on unemployment and employment suggest that there may be some sort cream skimming on the part of the caseworker. "Bad risk" individuals facing a high risk of becoming long-term unemployed are less likely assigned to the more expensive long-term training schemes, but rather to the inexpensive short-term training. This selection rule does however seem rationale. In fact, the "bad risk" unemployed benefit more from either type of training when the outcome is unemployment duration. However, the "good risk" individuals gain more through a participation in long-term training in terms of job stability.

The instantaneous effects on the hazard rates translate into a reduction in average unemployment duration and an increase in average employment duration for job-search training, whereas a participation in traditional further training increases both the expected unemployment duration and the expected employment duration. Further, treatment effects vary widely according to the timing of treatment. The overall gains out of job-search training are highest when it is started early in the unemployment spell. The unemployment duration prolonging effect of traditional further training decreases with elapsed unemployment duration at program start. In fact, long-term training is only effective in the long run if it is not started before month three to six of unemployment. Simple back-of-envelope calculations suggest that job-search training schemes are cost effective whereas traditional further training schemes are not.

In sum, the results obtained in this study have the following general implications for the design of active labor market policies. First, short job-search training schemes seem in fact to be an inexpensive but nevertheless effective tool to reduce unemployment in the long run. Participation in a job-search training course costs only a small fraction of what has to be paid for a traditional further training course, while the long-run effectiveness of the short course is not so different from that of the long one. Second, there is scope for raising the efficiency of active labor market policies by considering the heterogeneity of the effects according to the elapsed unemployment duration and the characteristics of a potential participant.

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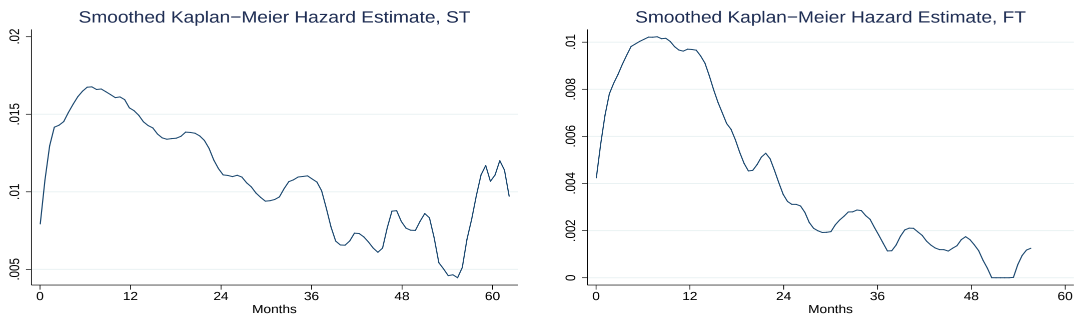
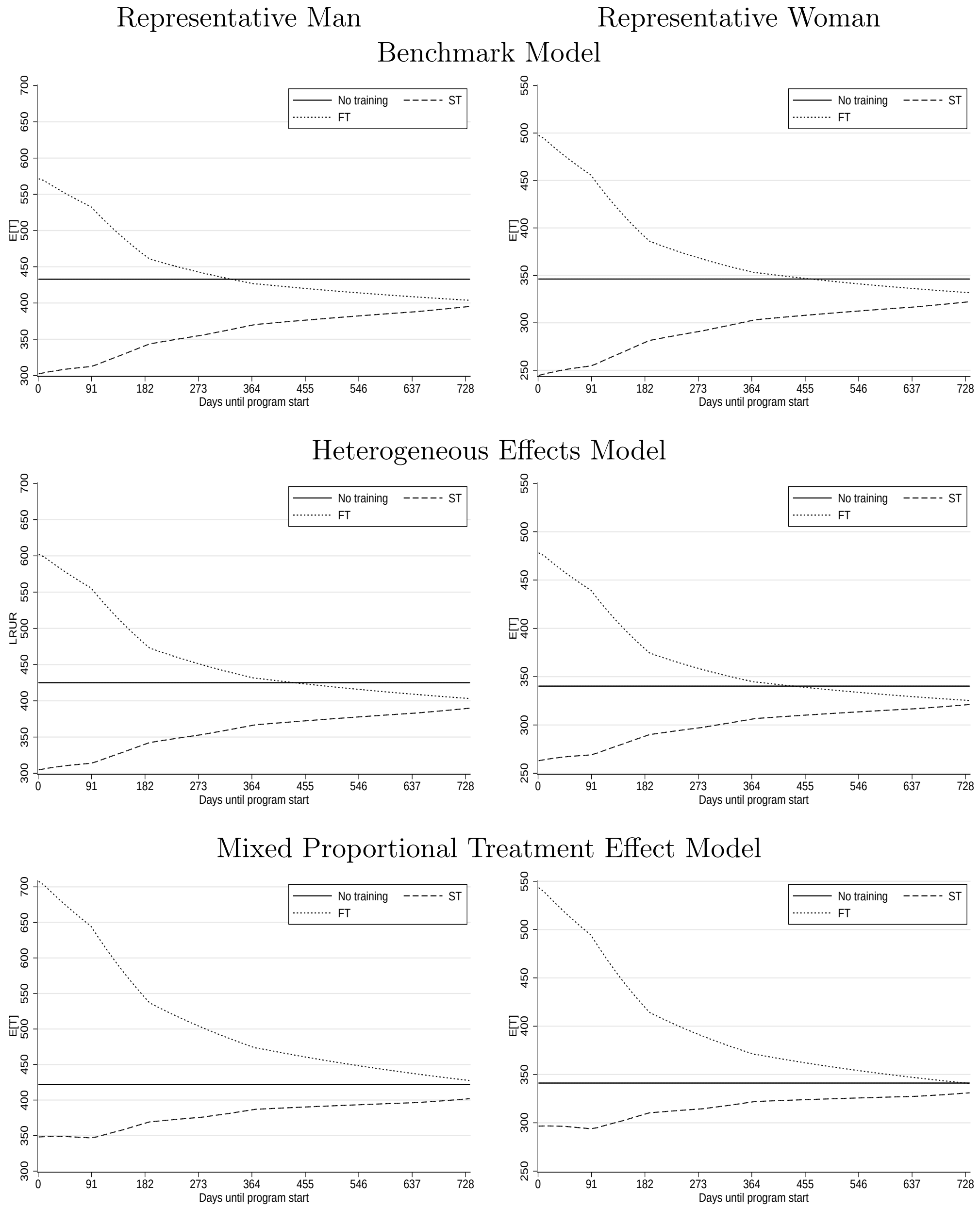
Figures

Figure 1: Smoothed Kaplan-Meier Estimate of the Hazard Rate into Program



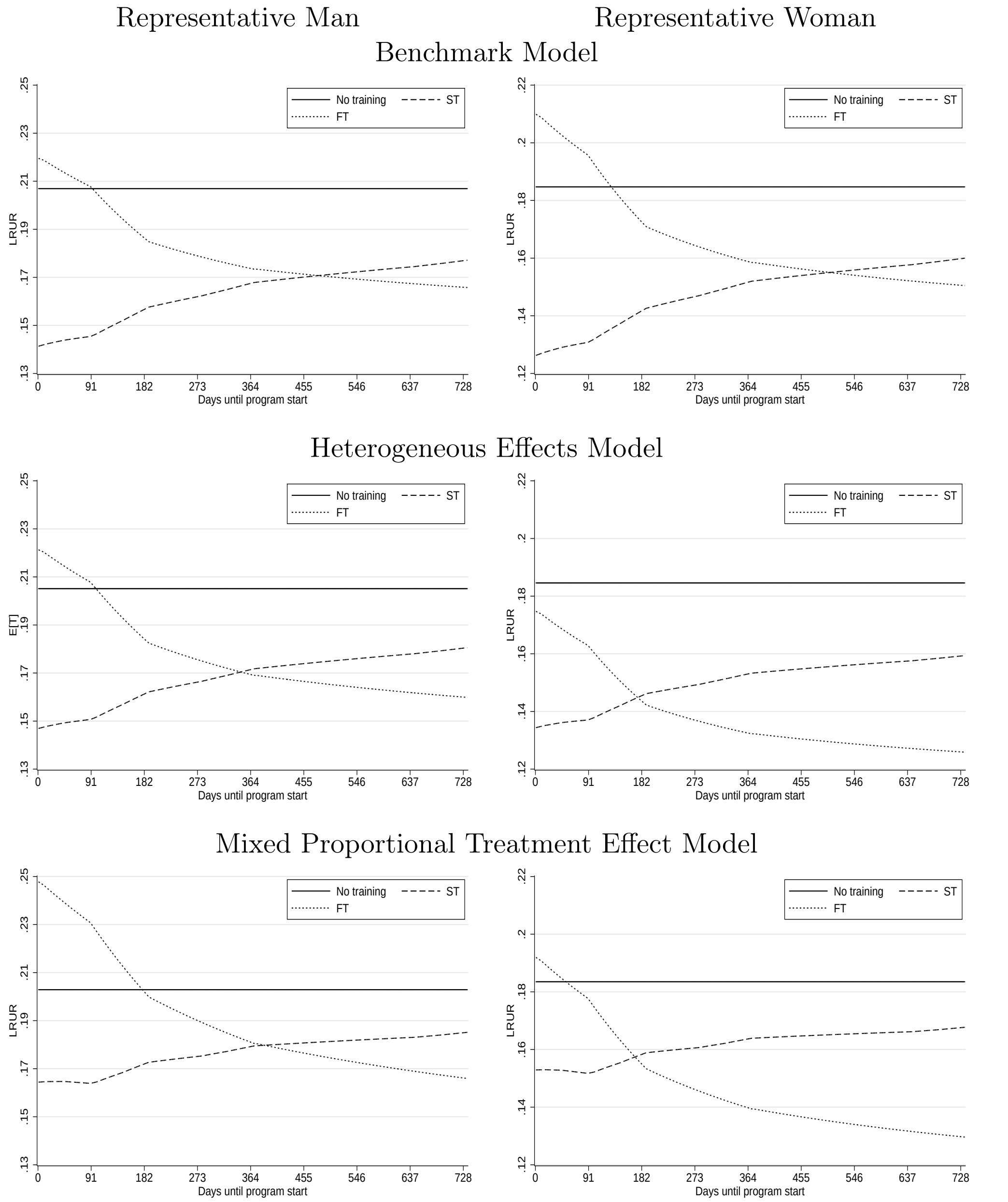
Notes: ST denotes short-term training, FT the traditional further training schemes. The bandwidth for the kernel smooth of the hazard rates is one month.

Figure 2: Expected Unemployment Duration by Starting Date of Program



Note: ST denotes short-term training, FT traditional further training.

Figure 3: Long-Run Unemployment Rate by Starting Date of Program



Note: ST denotes short-term training, FT traditional further training.

Tables

Table 1: Entries into Active Labor Market Programs (in 1000s)

	1999	2000	2001	2002	2003	2004
	Germany					
Qualification schemes	1,108	1,221	1,069	1,537	1,502	1,548
– traditional further training	491	552	450	456	255	185
– job-search training	432	477	565	877	1,064	1,188
Employment subsidies	538	459	465	544	808	950
Placement and advisory services	532	601	742	934	2,920	5,134
Job creation schemes	353	314	246	220	194	170
Specific measures for youths	244	263	265	294	389	408
Other	312	391	516	457	212	309
Total	3,087	3,249	3,304	3,985	6,025	8,520
	West Germany					
Qualification schemes	714	770	643	972	985	1,038
– traditional further training	307	338	261	273	161	124
– job-search training	265	286	339	545	690	789
Employment subsidies	245	225	206	245	365	481
Placement and advisory services	286	279	296	375	1,281	2,797
Job creation schemes	96	89	73	63	39	42
Specific measures for youths	181	193	191	210	262	270
Other	231	296	370	345	17	175
Total	1,753	1,852	1,778	2,210	2,949	4,803

Source: Bundesagentur für Arbeit (2001a, 2002a, 2003a, 2004a, 2005a).

Table 2: Average Monthly Expenditures and Program Durations in Germany

	1999	2000	2001	2002	2003	2004
	Average monthly expenditures (in Euro)					
Short-term training	602	580	570	658	538	421
Traditional further training	1,570	1,627	1,668	1,686	1,555	1,574
– subsistence allowance	1,093	1,152	1,178	1,188	1,156	1,150
– training costs	629	640	664	681	631	627
Unemployment benefits	1,132	1,160	1,189	1,185	1,261	1,313
Unemployment assistance	869	753	721	727	691	713
	Average program duration (in months)					
Short-term training	1.1	1.2	1.1	0.9	1.0	0.9
Traditional further training	8.4	8.2	9.3	9.1	10.5	10.7

Note: The upper panel contains the average monthly expenditures (in Euro) per participant/benefit recipient, the lower panel the average program duration in months. Expenditures on subsistence allowance, unemployment benefits and unemployment assistance include social security contributions. Source: Bundesagentur für Arbeit (2000, 2001b, 2002b, 2003b, 2004b, 2005b, 2005c).

Table 3: Sample Size (Spells by Spell Type)

Spell type	Censored	Completed	Total
UE to EM	30,490	56,844	87,334
	34.91%	65.09%	100%
UE to ST	78,817	8,517	87,334
	90.25%	9.75%	100%
UE to FT	82,619	4,715	87,334
	94.6%	5.4%	100%
EM to UE	15,883	49,417	65,300
	24.32%	75.68%	100%
Total	207,809	119,493	327,302
	63.49%	36.51%	100%

Table 4: Individuals by number of spells of a given type

Number of spells	Frequency	Percent	Cumulated
Spell type: UE to EM, ST, and FT			
1	23,720	52.14	52.14
2	11,517	25.32	77.46
3 or more	10,253	22.54	100.00
Total	45,490	100.00	
Spell type: EM to UE			
0	12,136	26.68	26.68
1	16,942	37.24	63.92
2	8,310	18.27	82.19
3 or more	8,102	17.81	100.00
Total	45,490	100.00	

Table 5: Estimated Treatment Effects in the Benchmark Model

	Coef.	t-Stat.
Unemployment to Employment		
$1 \leq t - t_{ST} < 46$	0.344	10.36
$46 \leq t - t_{ST} < 91$	0.298	7.72
$91 \leq t - t_{ST} < 361$	0.158	4.95
$361 \leq t - t_{ST} < 721$	0.067	1.20
$t - t_{ST} \geq 721$	0.348	4.31
$1 \leq t - t_{FT} < 361$	-0.747	-19.02
$361 \leq t - t_{FT} < 721$	0.059	1.02
$721 \leq t - t_{FT} < 901$	1.001	12.03
$t - t_{FT} \geq 901$	0.786	9.29
Employment to Unemployment		
ST	-0.091	-3.10
FT	-0.181	-4.39

Table 6: Estimated Treatment Effects in the Heterogeneous Effects Model

	Coef.	t-Stat
Unemployment to Employment		
$1 \leq t - t_{ST} < 46$	0.284	6.78
$46 \leq t - t_{ST} < 91$	0.242	5.19
$91 \leq t - t_{ST} < 361$	0.108	2.61
$361 \leq t - t_{ST} < 721$	0.027	0.44
$t - t_{ST} \geq 721$	0.318	3.78
st_female	-0.041	-1.10
st_foreign	0.100	2.77
st_ethnicgerman	0.066	0.60
st_disabled	0.120	2.57
st_health	0.001	0.09
st_age	0.283	1.22
st_agesq	-0.049	-1.63
$1 \leq t - t_{FT} < 361$	-0.856	-17.28
$361 \leq t - t_{FT} < 721$	-0.048	-0.75
$721 \leq t - t_{FT} < 901$	0.889	10.01
$t - t_{FT} \geq 901$	0.675	7.50
ft_female	0.090	1.96
ft_foreign	0.171	3.71
ft_ethnicgerman	-0.116	-0.97
ft_disabled	0.020	0.66
ft_health	0.057	4.15
ft_age	0.638	2.04
ft_agesq	-0.075	-1.82
Employment to Unemployment		
ST	-0.064	-1.38
st_female	-0.042	-0.88
st_foreign	-0.043	-0.91
st_ethnicgerman	-0.291	-2.04
st_disabled	0.000	0.00
st_health	0.511	4.74
FT	-0.170	-2.19
ft_female	-0.139	-2.27
ft_foreign	0.138	2.23
ft_ethnicgerman	0.212	1.47
ft_disabled	0.047	3.00
ft_health	-0.001	-0.01
ft_education3	-0.055	-0.84
ft_education4	0.221	1.66

Table 7: Estimated Correlation Coefficients between Heterogeneity Terms in the Log Hazard Rates and the Treatment Effects

	UE to EM	δ_{ST}	δ_{FT}	UE to ST	UE to FT	EM to UE	γ_{ST}	γ_{FT}
UE to EM	1							
δ_{ST}	-0.756 (0.133)***	1						
δ_{FT}	-0.951 (0.035)***	0.921 (0.091)***	1					
UE to ST	-0.532 (0.110)***	0.956 (0.079)***	0.767 (0.110)***	1				
UE to FT	0.722 (0.177)***	-0.092 (0.329)	-0.473 (0.232)**	0.203 (0.278)	1			
EM to UE	-0.379 (0.022)***	-0.319 (0.196)	0.075 (0.115)	-0.583 (0.107)***	-0.914 (0.104)***	1		
γ_{ST}	-0.385 (0.373)	-0.313 (0.434)	0.083 (0.417)	-0.577 (0.329)*	-0.917 (0.196)***	0.9999(.)	1	
γ_{FT}	-0.980 (0.095)***	0.612 (0.410)	0.872 (0.243)***	0.353 (0.465)	-0.844 (0.248)***	0.554 (0.400)	0.560 (0.514)	1

Note: Correlations are calculated based on $\text{var}(\ln v)$ using the estimated coefficients in table 10, last column. δ_{ST} and δ_{FT} represent the unobserved heterogeneity components associated with participation in short-term training and traditional further training, respectively, that affect the unemployment hazard. γ_{ST} and γ_{FT} denote the corresponding heterogeneity terms in the mixed proportional treatment effect model entering the employment hazard. Standard errors of the correlations are in parentheses and calculated using the delta method. *, ** and *** denote significance at the 10%-, 5%- and 1%-level, respectively.

Table 8: Expected Employment Duration in Days by Treatment Status

	No training	ST	FT
Representative Man			
Benchmark	1659	1836	2032
Heterogeneous Effects	1647	1769	2119
MP Treatment Effect	1658	1730	2149
Representative Woman			
Benchmark	1528	1692	1873
Heterogeneous Effects	1503	1694	2259
MP Treatment Effect	1518	1643	2288

Note: ST denotes short-term training, FT traditional further training.

Appendix

Table 9: Variable Definitions

Name	Definition
bhazXX	dummy equal to one if elapsed duration (in days) is greater than XX days and smaller than or equal the number of days referring to the next time interval dummy; the last interval is open ended
female	dummy equal to one if female
agegroup	age in 6 groups
age	age divided by ten
agesq	age squared divided by 100
f_agesq	agesq interacted with female
foreign	dummy equal to one if citizenship is not German
ethnicgerman	dummy equal to one if ethnic German, i.e. returned settler from former German settlements
education	1 information missing, 2 no degree, 3 vocational training degree, 4 university or technical college degree
schooling	1 information missing, 2 no schooling degree, 3 Hauptschulabschluss or Mittlere Reife/Fachoberschule (degrees reached after completion of the 9th or 10th grade), 4 Fachhochschulreife or Abitur/Hochschulreife (degrees reached after completion of the 12th or 13th grade)
health	1 no information available, 2 no health problems mentioned, 3 health problems, but considered without impact on placement, 4 health problems considered to have an impact on placement; when employed variable is a dummy equal to one if person had health problems affecting placement within last two months before beginning of employment spell
disabled	dummy equal to one if disabled
family	1 no information available, 2 living alone, 3 not married, but living together with at least one person, 4 single parent, 5 married
kids	dummy equal to one if person has at least one child
youngchild	dummy equal to one if person has children younger than 10 years

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Table 9: Variable Definitions <continued>

Name	Definition
seekpt	dummy equal to one if seeking only parttime job (unemployment spells only)
tarexp	dummy equal to one if caseworker considers job-seeker to have professional experience in target profession (unemployment spells only)
taredu	1 no information available, 2 caseworker considers job-seeker not sufficiently qualified for target profession, 3 considered with vocational qualification, 4 considered highly qualified (unemployment spells only)
endlastjob	1 if other reason and missing, 2 if termination of last employment by employer, 3 by employee, 4 fixed term contract (unemployment spells only)
land	10 categories indicating the West German Bundesländer (place of residence): 1 SH, 2 HH, 3 NI, 4 HB, 5 NW, 6 HE, 7 RP, 8 BW, 9 BY, 10 SL
area	West German Bundesländer aggregated into 4 categories (place of residence): 1 SH, NI, HB, HH; 2 NW, 3 HE, RP, SL; 4 BY, BW
rtype	classification of the districts of residence according to local labor market conditions into 5 groups
occupation	occupation (of last employment) in 8 categories: 1 missing; 2 elementary occupations; 3 skilled agric. and fishery workers; 4 craftsmen, machine operators and related; 5 service workers; 6 clerks; 7 technicians and associate professionals; 8 professionals and managers
industry	industry (of last employment) in 7 categories: 1 missing; 2 agriculture, forestry, fishing; 3 manufacturing; 4 construction; 5 trade and transport; 6 financial, renting and business; 7 other services
seasonwork	dummy equal to one if industry (of last employment) characterized by seasonal work
whitecollar	dummy equal to one if white-collar job

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Table 9: Variable Definitions <continued>

Name	Definition
bluecollar	dummy equal to one if blue-collar job
parttime	dummy equal to one if weekly hours worked less than full-time
earlycontact	dummy equal to one if already registered as job-seeker up to three months before beginning of current unemployment spell (unemployment spells only)
prevtrans	dummy equal to one if received some kind of unemployment insurance benefits in the three years preceding the current unemployment spell (unemployment spells only)
daqtiv	time-varying dummy equal to one in year of introduction of Job-AQTIV reform (i.e. in 2002) (unemployment spells only)
offben	time-varying dummy equal to one if temporarily off unemployment transfers because of sanctions (unemployment spells only)
q1, q2, q3, q4	dummy equal to one if spell starts in the first, second, third, fourth quarter of the year
y1999-y2004	year of starting date of spell
totclaim	original entitlement period for unemployment benefits in months (unemployment spells only)
hasclaim	dummy equal to one if originally entitled to unemployment benefits (unemployment spells only)
lnlwage, lnlwagesq	log of last real salary, square of log of last real salary if salary is below social security threshold, else zero (unemployment spells only)
clwc, clws	variables indicating whether last salary is above or below social security threshold (unemployment spells only)
lwquart	quartile of last salary (unemployment spells only)
lnwage, lnwagesq	log of first accepted real wage or square of log of first accepted real wage if salary is below social security threshold, else zero (employment spells only)
wc, ws	dummy variables indicating whether first salary is above or below social security threshold (employment spells only)
wquart	quartile of first salary (employment spells only)

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Table 9: Variable Definitions <continued>

Name	Definition
st_ptXX	dummy equal to one if elapsed duration (in days) since start of short-term training is greater or equal than XX days (unemployment spells only)
ft_ptXX	dummy equal to one if elapsed duration (in days) since start of further or retraining is greater or equal than XX days (unemployment spells only)
dst	dummy equal to one if participated in short-term training during previous unemployment spell (employment spells only)
dft	dummy equal to one if participated in further or retraining during previous unemployment spell (employment spells only)

Note: Unless otherwise noted above, all variables refer to the start of the spell. In unemployment spells, job characteristics refer to the previous employment. Descriptions of additional interaction terms and aggregated categories are omitted if the variable name is self-explaining. Continuous variables are centered around their mean across all spells.

Table 10: Estimated Coefficients

	Benchmark	Heterogeneous Effects	MP Treatment Effects
	Unemployment to Employment		
bhaz90	0.477 (0.011)***	0.477 (0.011)***	0.492 (0.011)***
bhaz190	-0.046 (0.016)***	-0.045 (0.016)***	-0.019 (0.017)
bhaz370	-0.588 (0.023)***	-0.585 (0.023)***	-0.557 (0.023)***
bhaz735	-1.386 (0.040)***	-1.380 (0.041)***	-1.355 (0.040)***
female	0.135 (0.018)***	0.135 (0.018)***	0.130 (0.018)***
foreign	-0.036 (0.012)***	-0.052 (0.012)***	-0.056 (0.012)***
ethnicgerman	0.110 (0.032)***	0.113 (0.033)***	0.112 (0.034)***
seasonwork	0.260 (0.021)***	0.261 (0.021)***	0.254 (0.021)***
whitecollar	-0.317 (0.063)***	-0.311 (0.063)***	-0.310 (0.063)***
bluecollar	-0.206 (0.063)***	-0.201 (0.062)***	-0.206 (0.063)***
parttime	-0.281 (0.063)***	-0.276 (0.063)***	-0.276 (0.063)***
area2	-0.144 (0.016)***	-0.142 (0.016)***	-0.138 (0.016)***
area3	-0.024 (0.018)	-0.023 (0.018)	-0.021 (0.018)
area4	0.171 (0.017)***	0.172 (0.017)***	0.171 (0.017)***

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Table 10: Estimated Coefficients <continued>

	Benchmark	Heterogeneous Effects	MP Treatment Effects
rtyp2	-0.176 (0.018)***	-0.178 (0.018)***	-0.173 (0.018)***
rtyp5	0.118 (0.015)***	0.118 (0.015)***	0.118 (0.015)***
education3	0.166 (0.015)***	0.167 (0.015)***	0.164 (0.015)***
education4	0.168 (0.033)***	0.168 (0.033)***	0.170 (0.033)***
schooling3	0.126 (0.018)***	0.124 (0.018)***	0.121 (0.018)***
schooling4	0.105 (0.026)***	0.103 (0.026)***	0.096 (0.026)***
occupation2	0.029 (0.024)	0.030 (0.024)	0.022 (0.024)
occupation3	0.311 (0.034)***	0.314 (0.034)***	0.306 (0.034)***
occupation4	0.166 (0.022)***	0.167 (0.022)***	0.158 (0.022)***
occupation5	0.141 (0.022)***	0.142 (0.022)***	0.133 (0.021)***
occupation7	0.054 (0.024)**	0.057 (0.024)**	0.053 (0.023)**
occupation8	0.102 (0.028)***	0.103 (0.028)***	0.099 (0.028)***
industry3	-0.147 (0.017)***	-0.147 (0.017)***	-0.140 (0.017)***
industry4	0.139 (0.018)***	0.137 (0.018)***	0.136 (0.018)***
industry67	-0.034 (0.015)**	-0.034 (0.015)**	-0.033 (0.015)**
agegroup2	-0.042 (0.016)**	-0.047 (0.017)***	-0.049 (0.017)***
agegroup3	-0.107 (0.017)***	-0.113 (0.018)***	-0.116 (0.018)***
agegroup4	-0.201 (0.021)***	-0.203 (0.022)***	-0.206 (0.022)***
agegroup5	-0.198 (0.021)***	-0.194 (0.022)***	-0.199 (0.022)***
agegroup6	-0.186 (0.030)***	-0.176 (0.030)***	-0.181 (0.030)***
f_agegroup4	0.086 (0.029)***	0.086 (0.029)***	0.087 (0.028)***
f_agegroup6	-0.176 (0.038)***	-0.175 (0.038)***	-0.177 (0.038)***
kids	0.070 (0.016)***	0.070 (0.016)***	0.069 (0.016)***
f_youngchild	-0.358 (0.029)***	-0.358 (0.029)***	-0.353 (0.029)***
f_kids	-0.101 (0.028)***	-0.102 (0.028)***	-0.103 (0.028)***
family3	-0.047 (0.028)*	-0.048 (0.028)*	-0.043 (0.028)
family4	-0.047 (0.032)	-0.048 (0.032)	-0.040 (0.032)
family5	0.015 (0.014)	0.014 (0.014)	0.016 (0.014)
health3	-0.370 (0.022)***	-0.375 (0.022)***	-0.367 (0.022)***
health4	-0.549 (0.024)***	-0.557 (0.024)***	-0.550 (0.024)***
disabled	-0.037 (0.007)***	-0.045 (0.007)***	-0.045 (0.007)***
taredu3	0.092 (0.014)***	0.091 (0.014)***	0.091 (0.014)***

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Table 10: Estimated Coefficients <continued>

	Benchmark	Heterogeneous Effects	MP Treatment Effects
tarexp	-0.071 (0.018)***	-0.078 (0.018)***	-0.079 (0.018)***
endlastjob3	-0.137 (0.022)***	-0.139 (0.022)***	-0.135 (0.022)***
endlastjob4	0.068 (0.014)***	0.068 (0.014)***	0.064 (0.014)***
prevtrans	0.195 (0.012)***	0.195 (0.012)***	0.191 (0.012)***
offben	0.097 (0.059)	0.095 (0.059)	0.098 (0.059)
y1999q4	0.076 (0.027)***	0.073 (0.027)***	0.068 (0.027)**
y2000q1	0.241 (0.027)***	0.238 (0.027)***	0.229 (0.027)***
y2000q2	0.065 (0.031)**	0.065 (0.031)**	0.057 (0.031)*
y2000q3	-0.044 (0.030)	-0.044 (0.030)	-0.051 (0.030)*
y2000q4	-0.040 (0.027)	-0.043 (0.027)	-0.050 (0.027)*
y2001q1	0.082 (0.027)***	0.080 (0.027)***	0.072 (0.027)***
y2001q2	-0.135 (0.030)***	-0.137 (0.030)***	-0.143 (0.030)***
y2001q3	-0.271 (0.029)***	-0.274 (0.029)***	-0.272 (0.029)***
y2001q4	-0.194 (0.027)***	-0.197 (0.027)***	-0.204 (0.027)***
y2002q1	0.006 (0.033)	0.001 (0.033)	-0.005 (0.033)
y2002q2	-0.476 (0.042)***	-0.481 (0.042)***	-0.473 (0.042)***
y2002q3	-0.554 (0.040)***	-0.560 (0.040)***	-0.559 (0.040)***
y2002q4	-0.288 (0.031)***	-0.294 (0.031)***	-0.303 (0.031)***
y2003q1	-0.108 (0.034)***	-0.113 (0.034)***	-0.115 (0.034)***
y2003q2	-0.596 (0.045)***	-0.602 (0.045)***	-0.597 (0.045)***
y2003q3	-0.544 (0.042)***	-0.550 (0.042)***	-0.549 (0.042)***
y2003q4	-0.333 (0.032)***	-0.338 (0.032)***	-0.347 (0.032)***
y2004q1	-0.178 (0.036)***	-0.182 (0.036)***	-0.187 (0.036)***
y2004q2	-0.578 (0.052)***	-0.583 (0.052)***	-0.582 (0.053)***
y2004q3	-0.770 (0.058)***	-0.776 (0.058)***	-0.780 (0.058)***
y2004q4	-1.683 (0.093)***	-1.693 (0.093)***	-1.702 (0.093)***
hasclaim	-0.170 (0.029)***	-0.174 (0.029)***	-0.178 (0.029)***
lwquart2	0.098 (0.017)***	0.099 (0.017)***	0.097 (0.017)***
lwquart3	0.212 (0.017)***	0.214 (0.017)***	0.211 (0.017)***
lwquart4	0.383 (0.019)***	0.384 (0.019)***	0.376 (0.019)***
totclaim	0.004 (0.004)	0.003 (0.004)	0.003 (0.004)
totclaimsq	-0.001 (0.000)***	-0.001 (0.000)***	-0.001 (0.000)***

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Table 10: Estimated Coefficients <continued>

	Benchmark	Heterogeneous Effects	MP Treatment Effects
st_pt1	0.344 (0.033)***	0.284 (0.042)***	0.287 (0.041)***
st_pt46	-0.046 (0.044)	-0.043 (0.044)	-0.066 (0.045)
st_pt91	-0.139 (0.042)***	-0.134 (0.042)***	-0.178 (0.043)***
st_pt361	-0.091 (0.057)	-0.081 (0.057)	-0.134 (0.058)**
st_pt721	0.280 (0.089)***	0.292 (0.089)***	0.270 (0.089)***
st_female		-0.041 (0.037)	-0.031 (0.036)
st_foreign		0.100 (0.036)***	0.117 (0.035)***
st_ethnicgerman		0.066 (0.110)	0.057 (0.107)
st_disabled		0.120 (0.047)**	0.122 (0.046)***
st_health		0.001 (0.013)	0.002 (0.013)
st_age		0.283 (0.233)	0.278 (0.226)
st_agesq		-0.049 (0.030)	-0.047 (0.029)
ft_pt1	-0.747 (0.039)***	-0.856 (0.050)***	-0.813 (0.043)***
ft_pt361	0.806 (0.050)***	0.808 (0.050)***	0.650 (0.050)***
ft_pt721	0.942 (0.081)***	0.938 (0.081)***	0.829 (0.081)***
ft_pt901	-0.215 (0.095)**	-0.214 (0.095)**	-0.330 (0.094)***
ft_female		0.090 (0.046)*	0.100 (0.042)**
ft_foreign		0.171 (0.046)***	0.189 (0.042)***
ft_ethnicgerman		-0.116 (0.119)	-0.122 (0.106)
ft_disabled		0.020 (0.031)	0.017 (0.028)
ft_health		0.057 (0.014)***	0.049 (0.012)***
ft_age		0.638 (0.314)**	0.581 (0.284)**
ft_agesq		-0.075 (0.041)*	-0.068 (0.037)*
Intercept	-6.104 (0.078)***	-6.098 (0.078)***	-6.072 (0.078)***
Unemployment to Short-Term Training			
bhaz40	-0.223 (0.036)***	-0.222 (0.036)***	-0.221 (0.036)***
bhaz90	-0.117 (0.038)***	-0.115 (0.038)***	-0.112 (0.038)***
bhaz150	-0.023 (0.046)	-0.020 (0.046)	-0.017 (0.047)
bhaz190	-0.145 (0.034)***	-0.142 (0.034)***	-0.137 (0.035)***
bhaz760	-0.453 (0.058)***	-0.451 (0.058)***	-0.446 (0.059)***
daqtiv	0.135 (0.028)***	0.134 (0.028)***	0.133 (0.028)***

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Table 10: Estimated Coefficients <continued>

	Benchmark	Heterogeneous Effects	MP Treatment Effects
seasonwork	-0.161 (0.044)***	-0.161 (0.044)***	-0.159 (0.044)***
rtyp3	-0.089 (0.031)***	-0.089 (0.031)***	-0.087 (0.031)***
rtyp4	-0.237 (0.044)***	-0.236 (0.044)***	-0.233 (0.044)***
rtyp5	-0.166 (0.035)***	-0.165 (0.035)***	-0.161 (0.035)***
occupation3	-0.240 (0.079)***	-0.240 (0.079)***	-0.239 (0.079)***
occupation6	0.157 (0.032)***	0.157 (0.032)***	0.154 (0.032)***
industry3	0.085 (0.029)***	0.084 (0.029)***	0.083 (0.029)***
industry4	-0.203 (0.037)***	-0.203 (0.037)***	-0.202 (0.037)***
agegroup2	-0.028 (0.036)	-0.027 (0.036)	-0.027 (0.036)
agegroup3	-0.053 (0.036)	-0.053 (0.036)	-0.053 (0.036)
agegroup4	-0.143 (0.039)***	-0.143 (0.039)***	-0.143 (0.038)***
agegroup5	-0.267 (0.044)***	-0.267 (0.044)***	-0.267 (0.044)***
agegroup6	-0.632 (0.057)***	-0.632 (0.057)***	-0.631 (0.057)***
family4	0.191 (0.050)***	0.190 (0.050)***	0.187 (0.050)***
family5	-0.166 (0.024)***	-0.166 (0.024)***	-0.167 (0.024)***
health3	-0.039 (0.042)	-0.040 (0.042)	-0.043 (0.042)
health4	-0.129 (0.044)***	-0.130 (0.044)***	-0.133 (0.044)***
disabled	0.093 (0.021)***	0.093 (0.021)***	0.092 (0.021)***
seekpt	-0.222 (0.042)***	-0.222 (0.042)***	-0.222 (0.042)***
taredu3	0.091 (0.024)***	0.091 (0.024)***	0.093 (0.024)***
taredu4	0.058 (0.052)	0.059 (0.052)	0.059 (0.052)
tarexp	-0.015 (0.036)	-0.014 (0.036)	-0.015 (0.036)
earlycontact	0.194 (0.023)***	0.194 (0.023)***	0.192 (0.023)***
q2	0.101 (0.029)***	0.101 (0.029)***	0.100 (0.029)***
q3	0.124 (0.026)***	0.124 (0.026)***	0.123 (0.026)***
y20002001	0.367 (0.042)***	0.367 (0.042)***	0.366 (0.042)***
y2002	0.639 (0.051)***	0.637 (0.051)***	0.628 (0.051)***
y20032004	0.841 (0.048)***	0.840 (0.048)***	0.829 (0.048)***
lwquart2	0.042 (0.035)	0.042 (0.035)	0.043 (0.035)
lwquart3	0.102 (0.035)***	0.103 (0.035)***	0.104 (0.035)***
lwquart4	-0.090 (0.037)**	-0.090 (0.037)**	-0.087 (0.037)**
totclaim	0.035 (0.005)***	0.035 (0.005)***	0.035 (0.005)***

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Table 10: Estimated Coefficients <continued>

	Benchmark	Heterogeneous Effects	MP Treatment Effects
totclaimsq	-0.001 (0.000)***	-0.001 (0.000)***	-0.001 (0.000)***
Intercept	-7.830 (0.072)***	-7.827 (0.072)***	-7.828 (0.072)***

Unemployment to Traditional Training			
bhaz90	0.225 (0.040)***	0.231 (0.040)***	0.249 (0.040)***
bhaz190	0.179 (0.047)***	0.191 (0.047)***	0.223 (0.049)***
bhaz370	-0.425 (0.059)***	-0.411 (0.060)***	-0.366 (0.063)***
female	-0.020 (0.037)	-0.020 (0.037)	-0.020 (0.037)
daqtiv	0.394 (0.043)***	0.393 (0.043)***	0.392 (0.043)***
foreign	-0.189 (0.034)***	-0.191 (0.034)***	-0.196 (0.035)***
ethnicgerman	0.374 (0.080)***	0.375 (0.080)***	0.375 (0.080)***
seasonwork	-0.326 (0.068)***	-0.326 (0.068)***	-0.326 (0.068)***
whitecollar	0.199 (0.041)***	0.199 (0.041)***	0.199 (0.041)***
land2	0.424 (0.082)***	0.423 (0.082)***	0.423 (0.082)***
land3	0.071 (0.046)	0.071 (0.046)	0.073 (0.046)
land6	0.138 (0.055)**	0.139 (0.055)**	0.141 (0.055)**
land7	0.180 (0.065)***	0.181 (0.065)***	0.183 (0.065)***
land8	0.291 (0.047)***	0.295 (0.047)***	0.305 (0.047)***
land9	0.215 (0.043)***	0.215 (0.043)***	0.217 (0.043)***
education4	-0.259 (0.071)***	-0.259 (0.071)***	-0.259 (0.071)***
schooling3	0.149 (0.050)***	0.150 (0.050)***	0.152 (0.050)***
schooling4	0.426 (0.064)***	0.426 (0.064)***	0.428 (0.064)***
occupation5	0.056 (0.052)	0.056 (0.052)	0.056 (0.052)
occupation67	0.397 (0.041)***	0.397 (0.041)***	0.396 (0.041)***
industry3	0.172 (0.038)***	0.171 (0.038)***	0.170 (0.038)***
industry4	-0.359 (0.058)***	-0.360 (0.058)***	-0.363 (0.058)***
agegroup2	-0.061 (0.047)	-0.062 (0.047)	-0.063 (0.047)
agegroup3	-0.160 (0.048)***	-0.161 (0.048)***	-0.162 (0.048)***
agegroup4	-0.146 (0.050)***	-0.147 (0.050)***	-0.148 (0.050)***
agegroup5	-0.323 (0.060)***	-0.324 (0.060)***	-0.325 (0.060)***
agegroup6	-0.702 (0.081)***	-0.703 (0.081)***	-0.703 (0.081)***
f_youngchild	-0.172 (0.065)***	-0.174 (0.065)***	-0.179 (0.065)***

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Table 10: Estimated Coefficients <continued>

	Benchmark	Heterogeneous Effects	MP Treatment Effects
family3	0.143 (0.076)*	0.143 (0.076)*	0.143 (0.076)*
family4	0.391 (0.068)***	0.391 (0.068)***	0.390 (0.068)***
family5	0.051 (0.034)	0.052 (0.034)	0.052 (0.034)
health4	-0.098 (0.058)*	-0.101 (0.058)*	-0.107 (0.058)*
seekpt	-0.328 (0.059)***	-0.326 (0.059)***	-0.327 (0.059)***
tarexp	-0.412 (0.045)***	-0.413 (0.045)***	-0.415 (0.045)***
earlycontact	0.341 (0.034)***	0.340 (0.034)***	0.338 (0.034)***
prevtrans	-0.247 (0.034)***	-0.251 (0.034)***	-0.254 (0.034)***
offben	-0.444 (0.237)*	-0.443 (0.237)*	-0.442 (0.237)*
q3	-0.123 (0.038)***	-0.125 (0.038)***	-0.129 (0.038)***
q4	-0.256 (0.038)***	-0.260 (0.038)***	-0.267 (0.038)***
y2000	-0.053 (0.046)	-0.055 (0.046)	-0.058 (0.046)
y2001	-0.503 (0.050)***	-0.508 (0.050)***	-0.514 (0.050)***
y2002	-1.020 (0.078)***	-1.035 (0.078)***	-1.056 (0.078)***
y2003	-1.109 (0.084)***	-1.125 (0.084)***	-1.149 (0.084)***
y2004	-1.430 (0.118)***	-1.445 (0.118)***	-1.469 (0.118)***
lwquart2	0.018 (0.048)	0.019 (0.048)	0.021 (0.048)
lwquart3	0.100 (0.049)**	0.102 (0.049)**	0.106 (0.049)**
lwquart4	-0.008 (0.052)	-0.006 (0.052)	-0 (0.052)
hasclaim	0.308 (0.093)***	0.301 (0.093)***	0.294 (0.093)***
totclaim	0.018 (0.013)	0.019 (0.013)	0.018 (0.013)
totclaimsq	-0.002 (0.000)***	-0.002 (0.000)***	-0.002 (0.000)***
Intercept	-8.002 (0.120)***	-7.989 (0.120)***	-7.984 (0.120)***
Employment to Unemployment			
bhaz180	0.745 (0.012)***	0.747 (0.012)***	0.746 (0.012)***
bhaz360	0.042 (0.018)**	0.045 (0.018)**	0.044 (0.018)**
bhaz720	-0.134 (0.024)***	-0.129 (0.024)***	-0.136 (0.024)***
female	0.073 (0.035)**	0.081 (0.035)**	0.078 (0.035)**
foreign	0.422 (0.014)***	0.420 (0.014)***	0.421 (0.014)***
ethnicgerman	-0.047 (0.036)	-0.042 (0.038)	-0.040 (0.038)
seasonwork	0.208 (0.023)***	0.209 (0.023)***	0.210 (0.023)***

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Table 10: Estimated Coefficients <continued>

	Benchmark	Heterogeneous Effects	MP Treatment Effects
area1	0.099 (0.016)***	0.100 (0.016)***	0.099 (0.016)***
land8	-0.143 (0.021)***	-0.143 (0.021)***	-0.141 (0.021)***
land9	0.032 (0.016)**	0.033 (0.016)**	0.033 (0.016)**
education3	-0.159 (0.014)***	-0.156 (0.014)***	-0.155 (0.014)***
education4	-0.212 (0.034)***	-0.228 (0.035)***	-0.227 (0.035)***
whitecollar	0.119 (0.023)***	0.120 (0.023)***	0.118 (0.023)***
bluecollar	0.314 (0.021)***	0.314 (0.021)***	0.312 (0.021)***
occupation3	0.100 (0.037)***	0.097 (0.037)***	0.095 (0.037)**
occupation4	0.078 (0.018)***	0.077 (0.018)***	0.077 (0.018)***
occupation5	-0.063 (0.023)***	-0.065 (0.023)***	-0.065 (0.023)***
occupation6	-0.126 (0.024)***	-0.126 (0.024)***	-0.125 (0.024)***
occupation7	-0.044 (0.027)	-0.046 (0.027)*	-0.045 (0.027)*
industry3	-0.204 (0.021)***	-0.204 (0.021)***	-0.203 (0.021)***
industry4	0.261 (0.022)***	0.264 (0.022)***	0.264 (0.022)***
industry5	-0.101 (0.018)***	-0.102 (0.018)***	-0.101 (0.018)***
industry7	-0.097 (0.021)***	-0.095 (0.021)***	-0.093 (0.021)***
kids	-0.147 (0.015)***	-0.148 (0.015)***	-0.150 (0.015)***
family3	0.276 (0.033)***	0.277 (0.033)***	0.278 (0.033)***
family4	0.318 (0.037)***	0.321 (0.037)***	0.319 (0.037)***
family5	0.125 (0.015)***	0.125 (0.015)***	0.123 (0.015)***
health	0.393 (0.028)***	0.364 (0.029)***	0.361 (0.029)***
disabled	0.067 (0.003)***	0.066 (0.003)***	0.065 (0.003)***
q1	-0.181 (0.013)***	-0.181 (0.013)***	-0.183 (0.013)***
q4	0.076 (0.017)***	0.076 (0.017)***	0.077 (0.017)***
y2000	0.046 (0.034)	0.047 (0.034)	0.053 (0.034)
y2001	0.134 (0.034)***	0.136 (0.034)***	0.142 (0.034)***
y2002	0.275 (0.035)***	0.277 (0.035)***	0.283 (0.035)***
y2003	0.329 (0.036)***	0.331 (0.036)***	0.336 (0.035)***
y2004q1	0.219 (0.044)***	0.220 (0.044)***	0.223 (0.044)***
y2004q2	0.324 (0.043)***	0.325 (0.043)***	0.327 (0.043)***
y2004q3	0.768 (0.052)***	0.769 (0.052)***	0.768 (0.052)***
y2004q4	1.060 (0.064)***	1.062 (0.064)***	1.061 (0.064)***

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Table 10: Estimated Coefficients <continued>

	Benchmark	Heterogeneous Effects	MP Treatment Effects
ws	-1.024 (0.068)***	-1.025 (0.068)***	-1.021 (0.067)***
wquart2	-0.110 (0.024)***	-0.110 (0.024)***	-0.109 (0.024)***
wquart3	-0.149 (0.030)***	-0.150 (0.030)***	-0.148 (0.030)***
wquart4	-0.206 (0.038)***	-0.207 (0.038)***	-0.204 (0.038)***
lnwage	-0.229 (0.029)***	-0.229 (0.029)***	-0.229 (0.029)***
lnwagesq	-0.019 (0.006)***	-0.019 (0.006)***	-0.019 (0.006)***
age	-0.441 (0.079)***	-0.438 (0.079)***	-0.430 (0.079)***
agesq	0.067 (0.010)***	0.067 (0.010)***	0.065 (0.010)***
f_agesq	-0.010 (0.002)***	-0.010 (0.002)***	-0.010 (0.002)***
dst	-0.091 (0.029)***	-0.064 (0.046)	-0.030 (0.049)
dst_female		-0.042 (0.048)	-0.032 (0.050)
dst_foreign		-0.043 (0.047)	-0.044 (0.048)
dst_ethnicgerman		-0.291 (0.143)**	-0.297 (0.149)**
dst_disabled		-0 (0.015)	0.008 (0.016)
dst_health		0.511 (0.108)***	0.544 (0.110)***
dft	-0.181 (0.041)***	-0.170 (0.077)**	-0.160 (0.081)**
dft_female		-0.139 (0.061)**	-0.134 (0.062)**
dft_foreign		0.138 (0.062)**	0.141 (0.063)**
dft_ethnicgerman		0.212 (0.144)	0.231 (0.148)
dft_disabled		0.047 (0.016)***	0.051 (0.016)***
dft_health		-0.001 (0.123)	0.029 (0.125)
dft_education3		-0.055 (0.066)	-0.062 (0.068)
dft_education4		0.221 (0.133)*	0.218 (0.136)
Intercept	-5.953 (0.084)***	-5.949 (0.084)***	-5.961 (0.084)***

Factor loadings on 1st latent factor (w_1)			
UE to EM	-0.624 (0.010)***	-0.628 (0.010)***	-0.658 (0.011)***
δ_{ST}			0.137 (0.036)***
δ_{FT}			0.440 (0.049)***
UE to ST	0.157 (0.030)***	0.148 (0.030)***	0.131 (0.031)***
UE to FT	-0.067 (0.051)	-0.093 (0.049)**	-0.152 (0.052)***
EM to UE	0.292 (0.018)***	0.300 (0.017)***	0.271 (0.018)***

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Table 10: Estimated Coefficients <continued>

	Benchmark	Heterogeneous Effects	MP Treatment Effects
γ_{ST}			0.047 (0.050)
γ_{FT}			0.139 (0.086)
		Factor loadings on 2nd latent factor (w_2)	
UE to EM	0	0	0
δ_{ST}			-0.119 (0.038)***
δ_{FT}			-0.142 (0.056)**
UE to ST	-0.223 (0.029)***	-0.222 (0.029)***	-0.208 (0.030)***
UE to FT	-0.194 (0.046)***	-0.163 (0.047)***	-0.146 (0.051)***
EM to UE	0.668 (0.012)***	0.668 (0.012)***	0.661 (0.013)***
γ_{ST}			0.112 (0.034)***
γ_{FT}			0.028 (0.059)
		Probabilities	
$Pr(w_1 = 1)$	0.469 (0.015)***	0.460 (0.015)***	0.467 (0.015)***
$Pr(w_2 = 1)$	0.482 (0 .013)***	0.484 (0.013)***	0.481 (0.013)***
Log L.	-847,430.86	-847,356.81	-847,288.55
Param.	239	265	273
Obs.	950,894	950,894	950,894

Note: The number of observations given in the table is obtained after episode splitting. This number corresponds to 327,302 unsplitted spells coming from 45,490 individuals. Standard errors are in parentheses. *, ** and *** denote significance at the 10%-, 5%- and 1%- level, respectively.