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ABSTRACT

Quick Job Entry or Long-Term Human Capital Development? The Dynamic Effects of Alternative Training Schemes*

This study evaluates and compares the effectiveness of two alternative training schemes for the unemployed: short, job-search oriented training and long, human capital oriented training. We investigate the impact dynamics of these programs considering both unemployment and employment duration. Our analysis uses a rich administrative longitudinal data set for Germany where both programs were implemented at the same time. Our estimation strategy flexibly accounts for dynamic selection on observables and unobservables as well as heterogeneous treatment effects. The results indicate that short-term training schemes can be an effective tool to reduce unemployment in the long run, in particular if participation occurs early during unemployment. Long-term training schemes increase the average duration of employment spells more strongly than short-term training. However, they increase the expected unemployment duration if they are started early during unemployment. This negative short-run impact of long-term training is an important driver of its overall effectiveness.

JEL Classification: J64, C41, J68, I28

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

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1 Introduction

What is the best design of training schemes for the unemployed? Ideally, policy makers would like to devise schemes that lead to quick job entry as well as long employment spells. Existing training schemes typically stress either human capital development or labor force attachment. Long-term training schemes offer comprehensive instruction in occupational skills and operational techniques. While they focus on improvement in the productivity of the unemployed, they usually do not aim at rapid reemployment. Previous work shows that program participation reduces job finding rates of participants (lock-in effect), and positive employment effects only appear some time after completion of the program (Couch, 1992, Hotz et al. 2006, Fitzenberger et al. 2009, Lechner et al. 2009). Moreover, long-term training schemes are relatively expensive. In contrast, job search assistance programs comprise job readiness training and instruction in job search skills. They focus on quick job entry and are inexpensive. However, the limited set of skills provided may not be sufficient to improve employment stability in the long run.

Recent policy reforms implemented in 1996 in the US[2] and in the late 1990s to early 2000s in Canada and the European countries[3] place increased emphasis on short-term activation and work-first strategies. Yet evidence from existing research does not support the conclusion that either of the two approaches – quick job entry or long-term human capital development – outperforms the other in terms of employment rates, but shows rather mixed results (see Dyke et al., 2006, Hotz et al., 2006, Biewen et al., 2008)[4]. In fact, different programs may have distinct impacts on unemployment and employment spells of participants. Static comparisons of employment rates at some given point in time after program participation confound these differences and may thus lead to opposite conclusions regarding the relative effectiveness of one program compared to another.

The main contribution of this paper is to examine, from a dynamic perspective, how different training schemes affect the employment histories of trainees. Our focus is on comparison of short-term, job-search oriented training and long-term, human capital

[2]See e.g. Blank (2002) for a survey of the contents and aims of the Personal Responsibility and Work Opportunity Reconciliation Act.

[3]Further information on the recent policy trends and reforms in these countries can be found in European Commission (2002), and OECD (2005, ch. 4; 2006).

[4]For surveys comparing the effectiveness of different active labor market programs in the US as well as in other countries see e.g. Heckman et al. (1999), Martin and Grubb (2001), and Card et al. (2009). Meyer (1995) and Ashenfelter et al. (2005) survey experimental evaluations of job search programs conducted in the US.

oriented training, that were administered at the same time in Germany. We evaluate their effects on both unemployment and employment spells. We further examine how the impacts of training during unemployment vary with elapsed unemployment duration at program start and with time relative to the scheduled program end. Our analysis provides a detailed picture of the dynamics of training impacts as well as an assessment of the overall impacts of training that contrasts short-run and long-run effects. We have access to unique administrative data that contain highly accurate information on periods of employment, job search, benefit receipt, and program participation of individuals. The data also comprise a large variety of covariate information. This allows us to model the transitions into different labor market states and training programs in a very flexible way using a continuous time duration framework. Due to the particular richness of our data, identification of our estimated model does not rely on the functional form assumptions (i.e. proportionality between time and covariate effects) that are commonly imposed in the duration literature.

An important problem in the evaluation literature is the nonrandom sorting of individuals into programs. Our research design is based on the timing-of-events approach to program evaluation by Abbring and van den Berg (2003a). This approach allows for selection into training based on observables as well as unobservables. The identification strategy is based on the assumption that there is random variation in the timing of program participation during unemployment. This requires that the exact moment of program start is not perfectly anticipated at the individual level. Under this assumption the durations in unemployment until employment and until program participation can be modeled as a two equation system with correlated unobservables using identification results from the competing risks literature. By recognizing that the treatment affects the duration until employment only from the moment of program start onwards, the treatment effect can be traced out as the differential dependence between the durations until employment and until program participation that arises after program start. As we will show below, the assumption of random variation in program starts is plausible in the institutional setup underlying this study. In Germany, training programs may potentially take place at any point in time during unemployment and program assignment depends on short-term supply factors that are beyond the control of the job seekers and unknown to them. This modeling approach allows identification of the causal treatment effect from single spell data with time constant covariates under some functional form restrictions: in particular, proportionality of the hazard rate in its three components, elapsed time, observed covariates, and unobservables, and independence between observed and unobserved variables. These assumptions can be relaxed if multiple spells or time-varying regressors are available. In our analysis, we include both repeated spells per individual and time-varying covariates. This improves the robustness and credibility of our estimation results because in our case identification does not rely on these parametric assumptions.

A dynamic approach has several advantages over a static framework that is usually applied in the evaluation literature. First, it allows a separate analysis of the impact of training on unemployment and employment durations. Thus, it provides a more detailed understanding of how different training programs work.[5] Second, in many evaluation settings, as is the case here as well, program assignment is not a static decision problem but is dynamically related to the success of job search. Under these circumstances, an evaluation approach, which uses a static indicator for receiving treatment, yields biased treatment effects because it implicitly conditions on future outcomes.[6] A dynamic approach of the type we use here avoids this problem because it explicitly models the dynamic treatment assignment process. Third, the dynamic approach used in this paper allows – in addition – an analysis of the effect of the timing of program participation during unemployment. The optimal timing of treatment has been widely neglected in the literature although it is an important dimension of treatment effect heterogeneity. On the one hand, job finding rates typically vary with elapsed unemployment duration. On the other hand, the impact of training on the job finding probability varies over time. It is typically negative during program participation and positive thereafter. This suggests that job finding rates of participants in training are a complex function of time and may vary widely across this dimension. Finally, a continuous time duration model avoids specification issues that arise as a consequence of discretization of inherently continuous dynamic features of the data.

There exists a small literature using the timing-of-events approach to evaluate training or active job-search programs, see e.g. Crépon et al. (2005), Hujer et al. (2006a, b), Lalive et al. (2008), Richardson and van den Berg (2006), and Weber and Hofer (2004). This study extends the existing research by simultaneously taking into account the following important points. First, unlike many of the previous studies applying the timing-of-events approach, this study analyzes two outcomes: unemployment and employment duration. This is important for assessing and comparing the overall effectiveness of the programs, as they may impact differently on unem-

[5] This point is also put forward by Ham and LaLonde (1996) who use a duration framework to study the effects of a randomly assigned training program for disadvantaged women in the US on unemployment and employment spells of trainees.

[6] Fredriksson and Johansson (2008) analyze the biases of a static approach when treatment assignment is in fact dynamic.

ployment and employment duration. Second, this paper compares two alternative types of training within a unified context and allows for heterogeneous treatment effects across observed and unobserved characteristics. Third, due to data or computational restrictions, previous work typically used a rather limited set of regressors, subsuming a large part of the remaining selection effects in the one dimensional index of unobserved heterogeneity. Moreover, in these studies, treatment effects were typically identified through functional form restrictions. The mainstream, static literature on program evaluation, in contrast, aims at nonparametric identification and estimation of treatment effects. In particular, matching methods rely on rich and flexible specifications of the selection into treatment based on observables. However, unlike the dynamic event history approach to program evaluation, they do not allow for selection on unobservables. The empirical analysis in this paper resolves these trade-offs. We apply the dynamic evaluation approach to particularly rich data allowing us to model selection on observables in a highly flexible way as is common with matching methods, while at the same time accounting for selection on unobservables. In addition, since we use time-varying regressors and repeated spells per individual, our estimation results are not driven by functional form assumptions. Finally, this paper not only presents the instantaneous impacts of training on the exit rates out of unemployment and employment, but also the way treatment effects are reflected in changes in expected unemployment and employment duration. This allows us to translate the results into economically meaningful quantities that are of direct interest to economists and policy makers. The expected outcome durations are further used for cost-benefit calculations that give some guidance on the financial efficiency of the two programs.

The empirical analysis in this paper is based on a unique administrative data set for Germany. 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"). As in most other OECD countries, training schemes with a focus on human capital development have traditionally been the cornerstone of active labor market policy in Germany. However, recently the policy focus has shifted towards measures that activate the unemployed in the short run. Short-term training schemes have gained in importance. From 1999 to 2004, the period covered by our analysis, their share in all training schemes increased from 35% to 75% (see Appendix A, table A1). Short-term training schemes provide skills that facilitate job search and 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 (see Appendix A, table A2).

Our main findings are as follows. Short, job-search oriented training schemes can in fact be an effective and financially profitable tool to reduce unemployment in the long run, in particular if participation occurs early during unemployment. Long-term training schemes may increase the expected unemployment duration if they are started early during unemployment. This negative short-run impact of long-term training is an important driver of its overall effectiveness. Therefore, it should rather not be given to newly unemployed individuals.

The remainder of the paper is organized as follows. Section 2 gives an overview of the institutional setup and the enrolment into training. Section 3 explains important conceptual aspects of dynamic program evaluation. Section 4 describes the empirical strategy and its implementation. In section 5, we outline the construction of our analysis sample and provide a descriptive analysis. Section 6 discusses the empirical results, and section 7 concludes. The Appendix contains further information on the institutional context, the data source used, a description of the variables used in the estimation and the complete estimation results.

2 Institutional Setting

2.1 Training Programs Analyzed

Training schemes are the most important type of active labor market policy in Germany as in many other OECD countries.[7] In our analysis, we focus on the following two types of training schemes: human capital oriented long-term training and job-search oriented short-term training.

Long-term training schemes comprise a variety of programs ranging from advanced vocational training and refresher courses on specific professional skills and operational techniques to comprehensive retraining in a new vocational degree within the German apprenticeship system. The former typically last between six and twelve months whereas retraining takes two to three years. Training programs can take

[7]Further information on the regulation of training as well as some aggregate figures on the quantitative importance of training in the context of German active labor market policy can be found in Appendix A.

place either in classrooms, simulated workplaces, firms or a combination thereof. Typical examples of long-term training programs include training on marketing and sales strategies, computer assisted bookkeeping, operating construction machines, and specialist courses in specific legal fields.

Short-term training courses last a couple of days to twelve weeks. Similar to the long-term training schemes, they may take place off-the-job or on-the-job. However, due to their shorter length their contents are less occupation specific and the human capital component is limited. Typical examples of short-term training schemes include job application training, basic computer courses, language courses and short-term internships at a simulated or real workplace. 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.

2.2 Enrolment into Training

Training programs in the context of active labor market policy are offered continuously throughout the year. Registered job-seekers may participate in a program at any point in time during their unemployment spell. The job-seeker and the caseworker at the local employment agency meet repeatedly during the unemployment spell and discuss further search strategies and program participation. The unemployed may also just receive a written invitation to participate in a program. An assignment requires the approval of the caseworker who has to judge a participation as necessary in order to improve the employment prospects of the person under consideration. Participation is mandatory once a job-seeker has been assigned to a specific course or has received a training voucher. Non-compliance may entail a temporary suspension of benefits. However, job placement has priority over program participation. The unemployed are encouraged to continue job search at any time, even while participating in a training program.

For short-term training throughout the entire period considered in this paper and for long-term training 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). In an informal procedure, the employment agencies stipulated in advance the quantities and contents of the training courses to be supplied by the training providers for a given calendar year. They committed themselves to fill them with participants throughout the year. The actual 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. Some types of training schemes did not have fixed start and end dates in the sense that a group of participants started a course with a fixed scheduled duration together at some date that had been specified in advance. Rather, the schemes continued over an indefinite period of time and new participants enrolled for a varying scheduled duration determined by the caseworker as soon as previous participants left it (Blaschke and Plath, 2000). Anecdotal evidence in Schneider et al. (2006) suggests that belated assignments and referrals on very short notice were commonly used in order to assure a high capacity utilization of booked courses and to keep up job search incentives.

Since 2003, candidates for a long-term training program obtain a voucher that is valid for one to three months and that specifies a training field.[8] The candidate then redeems the voucher by choosing on his own a suitable course from a pool of certified courses. This reform intended to make the allocation of training programs more targeted and better tailored towards the needs of the unemployed. However, its original goals were impaired by important difficulties in the implementation of the new system. In particular, 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 (Kühnlein and Klein, 2003, Schneider et al., 2006). Therefore, potential participants faced a high uncertainty regarding the start of a chosen program. According to surveys conducted by the Federal Institute for Vocational Education and Training (BIBB) among training providers, in 2003 and 2004, 50% and 60%, respectively, of the offered courses had to be canceled shortly because of an insufficient number of participants (Paulsen et al., 2006).

To sum up, the allocation of unemployed to training programs largely depends on short-term criteria as well as private information of the caseworker. The unemployed typically does not know the starting date of a program in advance. In the following empirical analysis, we will exploit this feature of the assignment process for the identification of dynamic treatment effects.

[8]About 13% of the long-term training programs in our analysis sample start on or after the first of January 2003.

3 Conceptual Considerations

Why are treatments taken at different points in time not just a version of multiple treatments? Why look at unemployment and employment durations as outcomes? In order to clarify these questions, this section briefly outlines the concept of dynamic program assignment and the selection issues involved. Further, we explain in what respect an explicitly dynamic framework that also models outcomes in a dynamic way allows one to obtain additional insights in how active labor market programs work.

Consider first the following stylized institutional setup. Individuals dynamically move between the two labor market states unemployment and employment. While unemployed, they may be assigned to an active labor market program. In particular, the job-seeker and the caseworker at the local employment agency meet repeatedly during the unemployment spell. At any such occasion, the caseworker decides whether to assign his client to a program or to postpone participation to the future, waiting further how job search evolves. Somebody who has not participated, say, until day 80 of his unemployment spell may still enrol later. If, however, he starts a new job at day 81 he would not be eligible for participation anymore.

The above example is not specific to this paper but such an assignment process is indeed typical for many countries with comprehensive systems of ongoing labor market programs (cf. section 2.2 on Germany or Sianesi, 2004, on Sweden). Similar situations may also arise in social experiments: Assume as before that only unemployed individuals are eligible for treatment. At some baseline point in time unemployed individuals are randomly assigned to a treatment and control group, but the number of individuals in the treatment group exceeds the number of instantly available program slots.[9] In this case, randomization does only hold for the intention to treat but not for receiving treatment because those individuals in the treatment group starting a program at a later point in time are those who were unsuccessful in finding a job in the meantime.

These generic examples illustrate that program participation is often the result of a dynamic process that is linked to another dynamic process, here unemployment, that in turn is related to economic outcomes of interest in an evaluation study, e.g.

[9]Heckman et al. (1999) report that in several major social experiments conducted in the US, such as for the Job Training Partnership Act (JTPA), randomization could only be achieved with respect to the intention to treat. Because of time gaps between randomization and program start 30 to 50% of the individuals in the treatment group dropped out of the experiment before actually receiving treatment.

employment status or wages. In order to avoid an implicit conditioning on future outcomes when defining treatment and control group status, it is thus important to properly account for the dynamic nature of program participation (see Fredriksson and Johansson, 2008, for a formal analysis). In an empirical analysis, this can easily be done by using a time varying indicator for treatment status. If, for instance, one wants to study the effect of an active labor market program on the probability to leave unemployment, one would include a time varying dummy variable equal to one once the program has started. The coefficient on this variable would then correspond to the treatment effect.

But what about nonrandom selection into programs? In order to illustrate the additional selection issues at play in a dynamic setup, abstract from selection bias due to observed and unobserved differences across treatment and control group and focus on selection across time. In particular, consider the case that randomization into treatment occurs sequentially at different points in time for a given cohort of individuals who survive in unemployment until that time.[10] At any given randomization date a fraction of those still unemployed at that date are randomly selected for participation in a program that starts immediately. Those individuals not receiving treatment at that day may be randomized into treatment at a later date. Thus, a comparison of outcomes between those treated at a given day and those not treated at that day would yield the effect of being treated now versus possibly later (i.e. the effect of treatment versus waiting).[11] However, with sequential randomization one cannot identify the effect of being treated at some later date versus at an earlier date for those who survive until the later date. In fact, if the treatment increases the probability of leaving unemployment, those already treated at an earlier date but still unemployed at the later date are a more negatively selected group than those untreated until the later date. Furthermore, treatment effects associated with different starting dates are estimated from the changing population of survivors at each date and are therefore not comparable across starting dates. This also means that one cannot causally compare treatment effects associated with different start-

[10] Sequential matching techniques mimic a sequential randomization into treatment. Applications include Sianesi (2004, 2008), Dyke et al. (2006), Lechner and Wiehler (2007) or Fitzenberger et al. (2008). Biewen et al. (2007) use such an approach to evaluate the different training schemes administered in the context of German active labor market policy.

[11] Crépon et al. (2009) consider a setup that corresponds to a slightly different randomization protocol than the one described here. At a given randomization date t treatment starting dates t_s are drawn for each individual still unemployed at that date, such that $\{t_s : t_s \geq t\}$. In this setting, it is possible to identify the effect of treatment at $t = t_s$ versus no treatment on the probability of survival in unemployment for those who are still unemployed at t because the randomization protocol implies that those receiving treatment at $t > t_s$ randomly drop out of the control group at their future treatment start.

ing dates. In order to causally analyze the effect of receiving treatment at different points in time, one needs to devise a model for the selection into different labor market states and treatment over time. Event history models provide an appropriate framework to model a dynamic evaluation problem. With rich enough data they allow a flexible modeling of the dynamic selection into treatment that may be based on observables as well as unobservables, under minimal or no parametric assumptions. Furthermore, they would also allow to pursue a fully structural analysis based on economic search theory.

There is an important additional benefit to using a duration framework. It allows one to analyze the separate impact of a program on unemployment and employment spells.[12] This is important in order to learn how a program generates a certain effect on the employment rate. A higher employment rate can be due to the treatment increasing the exit rate out of unemployment or decreasing the exit rate out of employment or both. This distinction is not possible with employment rates even if individuals are randomly assigned into treatment and control groups at some baseline point in time, because the composition of those in employment at a given later point in time depends on the treatment status, unless the treatment effect is zero. In fact, if the treatment increases the probability to leave unemployment, then the individuals in the treatment group who have experienced at least one transition to employment at a given point in time after treatment are a more negatively selected subgroup than those from the control group, as the treatment helps unemployed with otherwise low reemployment chances to get a job.[13]

4 Estimation Strategy

4.1 Identification

Our evaluation approach builds on the timing-of-events framework by Abbring and van den Berg (2003a).[14] They consider a continuous time duration model where the transition rates from untreated unemployment into program participation and into

[12]Bergemann et al. (2008) study the effects of training on the outcome states employment and non-employment. They combine propensity score matching with a conditional difference-in-differences approach where the non-employment and the employment hazard are specified as linear probability models. However, this approach does not allow them to model the dynamic selection into the different outcome states, but is conditional on being in a given state.

[13]See Ham and LaLonde (1996) for a detailed illustration of this point.

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

employment are jointly modeled as two competing risks. Extending the literature on dependent competing risks models, they show that the causal effect of entering a program can be identified semi-parametrically from single spell data with time constant regressors.

The timing-of-events approach exploits variation in the timing of treatment in order to identify causal treatment effects. Intuitively this approach works as follows. The realization of a treatment date is only observed if an individual is still in open, i.e. untreated, unemployment by that time. Otherwise the duration until program start is censored at the moment in which the individual exits to employment. Similarly, the realization of the time spent in untreated unemployment is only observed if an individual exits to employment before being assigned to a program. Otherwise the duration in untreated unemployment with destination employment is censored at the moment in which the individual enrols into the program. Thus, until treatment start, treatment and outcome process – i.e. the waiting times in untreated unemployment until program start and until employment, respectively – are two competing risks and identification results known from the literature on competing risks models can be applied.[15] In particular, dynamic selection into treatment based on unobservables can be modeled by allowing for a nonzero correlation between the unobservables of the two risks. In a next step, the treatment effect can be traced out as the differential dependence between treatment and outcome process arising after the program has started. This amounts to comparing the realization of the total unemployment duration until exit to employment of individuals who enroled in a program at a given point in time during their unemployment spell with that of individuals who did not (yet) enrol at that point in time. Individuals enroling at some later point in time contribute to the non-treatment outcome up to their own program start. Their duration in untreated unemployment with destination employment is censored when their program starts.

In this paper, we consider an extended version of the framework described above. We model the dynamic assignment process of two treatments, i.e. short-term and long-term training. We consider the impacts of training on the ongoing unemployment spell and the subsequent employment spell and allow for heterogeneous treatment effects that are a function of observed covariates and unobserved heterogeneity.[16]

[15]See Heckman and Honoré (1989) and Lancaster (1990) on the main identification results for competing risks models.

[16]Formal identification proofs for these extensions can be found in Abbring (2006, Proposition 1) on the multiple competing risks and multiple treatment model, Crépon et al. (2005) on a model including treatment effects on subsequent employment duration, and Abbring and van den Berg (2003a) and Richardson and van den Berg (2006) on heterogeneous treatment effects.

Moreover, we have access to particularly rich data containing time-varying regressors and repeated spells per individual, such that our estimated model is identified under much weaker assumptions than a more basic model with time constant covariates and single spell data.

In the timing-of-events framework, treatment assignment is conceived as a dynamic stochastic process. This requires first that there is sufficient variation of program starts over elapsed unemployment duration. This can be verified by plotting the distribution of program starts in the data. In section 5.2, we present evidence that starting dates of programs do vary over elapsed unemployment duration in our data. Second, at the individual level, program assignment needs to have a random component that remains after conditioning on elapsed time, observable and unobservable variables in the model. This holds if the exact treatment date is not known in advance by the potential participants.[17] This requirement has to be substantiated by the institutional circumstances that lead to program participation. As discussed in section 2.2, the institutional setting underlying this analysis is such that the allocation of unemployed to training programs is determined by short-term supply factors that are beyond the control of the unemployed and unknown to him (e.g. short-term allocation decisions of the caseworker aiming to ensure a high capacity utilization, or whether the number of other participants in a chosen course exceeds a critical threshold for the course to take place). The job-seeker learns about his participation only very shortly before or at the actual start of the program. Moreover, job-seekers are encouraged to continue searching for a new job at any time, even while they are participating in a program. There is also no evidence for Germany that job-seekers systematically like or dislike (the idea of) participating in a training program. Therefore, we are convinced that the so called 'no-anticipation' assumption holds in our analysis.

The basic version of the timing-of-events model by Abbring and van den Berg (2003a) imposes two important functional form restrictions: (i) a proportional structure of the hazards, where dependence on elapsed time, observed covariates and unobserved heterogeneity enter the specification multiplicatively,[18] and (ii) independence between observed covariates and unobserved heterogeneity terms. However, in our study these assumptions are not crucial for identification because our data

[17] This does however allow for the possibility that the job-seeker knows that he has a low or high probability of entering a program and adjusts his search behavior accordingly. This is commonly referred to as the ex ante treatment effect. Our analysis focuses on ex post treatment effects though.

[18] Within each of the multiplicative components, no parametric functional form restrictions need to be imposed.

situation is more favorable than in the baseline case of single spell data and time constant covariates.

Assumption (i) of proportionality can be relaxed if the set of conditioning variables includes time-varying regressors, as they provide additional exogenous variation generally facilitating identification (Heckman and Taber, 1994, McCall, 1996, and Brinch, 2007).[19] In particular, Brinch (2007) establishes that, if the model includes covariates that vary over time and across observations, the dependence of the hazard on elapsed time and observed regressors is nonparametrically identified. Thus, only separability between observed and unobserved components is needed. The intuition for this is as follows. Variation of covariates over time provides exclusion restrictions in the sense that, if one assumes that the current hazard depends only on current values of the regressors, then any observed dependence of the survival probability on past regressor values comes through the unobserved heterogeneity (Brinch, 2007, Eberwein et al., 1997). In this paper, we include several time-varying regressors in each of the four hazard rates we model. For instance, the hazard rate from unemployment to employment includes dummy variables indicating the closeness to the expected expiration date of unemployment benefits, dummies indicating the different seasons of the year, and for the treatment effects dummy variables indicating different time intervals before and after the planned end date of the program.[20]

Furthermore, neither assumption (i) nor (ii) need to be imposed with multi-spell data, i.e. multiple observations within individual and spell type. 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 (Abbring and van den Berg, 2003a). 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 to fixed effects panel data models (Abbring and van den Berg, 2004).[21] In this paper, we use multiple unemployment-employment cycles per individual. About half of the individuals in

[19]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 (Heckman and Taber, 1994).

[20]See Appendix C for a detailed description of the regressors used.

[21]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.

the sample experience two or more unemployment spells and about a third has two or more employment spells. Taken together, these data features enhance the robustness and credibility of our estimation results because identification does not rely on the functional form assumptions required in the basic model with single spell data and time constant covariates.

Multivariate duration models do not require formal exclusion restrictions, but only independent variation of the different hazard rates with the regressors. In our application this requirement should be fulfilled easily since the four hazard rates we model represent conceptually distinct labor market states and destinations. Furthermore, we consider a very large set of covariates reflecting many different individual and job characteristics, caseworker assessments and the influence of calendar time. Empirically, we find that some regressors are significant in some equations but not in others. This suggests that there is sufficient independent variation in the regressor effects across equations, which is important in order to identify the dependency structure of the different risks.

4.2 Modeling the Hazard Rates

For an inflow sample into unemployment, we 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 (ST) and long-term training (LT). 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 intensities for the waiting times until the start of the two training programs are 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:

$$\theta_1(t|x(t), v_1) = \lambda_1(t) \cdot \exp(x(t)\beta_1) \cdot \prod_{p=2}^{3} \exp[\delta_p(\centerdot) \cdot \mathbb{1}(t \geq t_p^n, P = p)] \cdot v_1$$

where the function $\delta_p(\centerdot)$ corresponds to the treatment effect of participating in training type $p = ST$, LT and $\mathbb{1}(\centerdot)$ is the indicator function that is equal to one from the beginning of program $P = p$ onwards. The treatment effect is modeled as a function of elapsed unemployment duration t, elapsed unemployment duration at program start t_p^n, planned program duration t_p^x, observed covariates and unobserved heterogeneity, i.e. $\delta_p(\centerdot) = \delta_p(t, t_p^n, t_p^x, x, v_{\delta_p})$.

Finally, the hazard rate while employed equals:

$$\theta_4(t|x(t), v_4) = \lambda_4(t) \cdot \exp(x(t)\beta_4) \cdot \prod_{k=2}^{3} \exp[\gamma_p(\centerdot) \cdot \mathbb{1}(P = p)] \cdot v_4$$

where $\gamma_p(\centerdot)$ is the treatment effect. Analogously to the unemployment hazard, $\gamma_p(\centerdot)$ is a function of observed covariates and unobservables, i.e. $\gamma_p(\centerdot) = \gamma_p(x, v_{\gamma_p})$.

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

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

4.3 Modeling Unobserved Heterogeneity

In order to avoid functional form assumptions on the joint distribution of the unobservables, 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, we adopt a two-factor loading model where the two underlying factors, w_1 and w_2, are assumed to be independent:

$$v_m = \exp(\alpha_{m1} w_1 + \alpha_{m2} w_2), \; m = 1, \ldots, 4, \delta_{ST}, \delta_{LT}, \gamma_{ST}, \gamma_{LT},$$

where the indices $m = 1, \ldots, 4$ correspond to the four hazards and the indices $m = \delta_{ST}, \delta_{LT}, \gamma_{ST}, \gamma_{LT}$ to the treatment effects of short- and long-term training on the unemployment and the employment hazard, respectively. Each of the two factors follows a discrete distribution with two masspoints.[22] This specifica-

[22]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. Monte Carlo evidence presented in Heckman and Singer (1984) and Gaure et al. (2007) suggests that the interpretation should rather focus on summary measures of the distribution of unobservables such as the mean, the variance or correlations. 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 and to produce accurate predictions of the duration distributions.

tion 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 $A_m = (\alpha_{m1}, \alpha_{m2}),\ m = 1, \ldots, 4, \delta_{ST}, \delta_{LT}, \gamma_{ST}, \gamma_{LT}$. Then the variance-covariance matrix of the unobserved heterogeneity terms is given by $\mathrm{Var}(\ln(v)) = A\,\mathrm{Var}(w)\,A'$. Such a factor loading specification requires some normalization in order to be identified. We normalize the two fundamental factors to have support on $\{-1, 1\}$ and in addition constrain α_{22} to equal zero.[23]

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:[24]

$$f_i = \prod_{k=1}^{4}\prod_{j=1}^{N_{ik}} \theta_k(t_{ijk}|x_{ij}(t_{ijk}), v_{ik})^{c_{ijk}} \exp[-\int_0^{t_{ijk}} \theta_k(\tau|x_{ij}(\tau), v_{ik})d\tau]$$

where N_{ik} is the number of spells that individual i spends in state k, t_{ijk} is the time spent in the jth observation period in state k, and c_{ijk} is a censoring indicator that equals one if the jth observation period in state k ends with a transition. Since we allow for nonzero correlations of the unobserved heterogeneity terms in the four transition intensities and the treatment effects, 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 vector of unobserved heterogeneity terms, ν, is:

$$L_i = \int_0^{\infty} \prod_{k=1}^{4}\prod_{j=1}^{N_{ik}} \theta_k(t_{ijk}|x_{ij}(t_{ijk}), \nu_k)^{c_{ijk}} \exp[-\int_0^{t_{ijk}} \theta_k(\tau|x_{ij}(\tau), \nu_k)d\tau]dG(\nu).$$

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

[23]Note that all hazards contain an intercept.

[24]In order to simplify the notation the treatment effects and the corresponding unobserved heterogeneity terms are kept implicit in the following.

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.[25] 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 such 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.[26] The final specifications presented in section 6 involve the estimation of 355 parameters.

5 Data

5.1 Analysis Sample

The empirical analysis is based on an exceptionally rich administrative database, the German Integrated Employment Biographies Sample (IEBS), that has recently been made available by the Research Data Center 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.[27] It comprises 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 period 1990 to mid-2005, 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 states. This allows us e.g. to distinguish regular employment from employment spells in the context of active labor market policy. Moreover, the IEBS comprises a large set of variables that give a detailed picture of socio-economic, occupational and job characteristics, as well as

[25]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.

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

[27]See Appendix B for further details.

of job search and contents of active labor market programs.

From this data set, we extract 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 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 active labor market programs other than training are also treated as independent censoring.

We consider two types of training programs: short-term training (ST) and traditional long-term training (LT). Thus, an untreated unemployed is exposed to three different risks: being assigned to a short-term training scheme (spell type unemployment, UE, to ST) or a long-term training program (spell type UE to LT) or finding a new job (spell type UE to employment, EM). Until the realization of the first transition this corresponds to a competing risks model with three destination states. However, unlike in a standard competing risks model, the two other risks are not in all cases censored at the realization of the first transition. A participation in training affects the exit rate to employment from the start of the program onwards. If no transition towards training occurs before the termination of the unemployment spell – either due to a transition to employment or because of censoring – then the waiting times until short-term training and long-term training are treated as censored at the termination date of the unemployment spell. If a transition from unemployment towards long-term training occurs first, then the waiting time until short-term training is treated as censored at the time of entry into long-term training. However, the waiting time until long-term training is *not* censored if the transition to short-term training occurs first. Instead, it is allowed to change from the start of the short-term training program onwards. This asymmetry between short-term and long-term training is motivated by the fact that short-term training is also used to assess the professional skills of an unemployed and to define a suitable reintegration plan, which may entail participation in a long-term training program.[28] In contrast, the probability that an unemployed who has already participated in a long-term training scheme is referred to an additional program is very small.

In addition, we model the subsequent employment duration (spell type EM to UE)

[28] In the sample, about 9% of the participants in short-term training later on enrol in traditional further training.

in order to study how it is affected by a participation in training during the previous unemployment spell. Individuals in the sample may have multiple unemployment and employment spells if they experience multiple transitions between unemployment and employment. Each unemployment-employment cycle has the structure described above. A treatment is assumed to affect only the ongoing unemployment spell and the directly following employment spell. Thus regarding earlier treatments, the model specification rules out lagged occurrence dependence in the unemployment spell and higher order lagged occurrence dependence in the employment spell. The observation period lasts until the end of December 2004, and ongoing spells are censored at that date.

5.2 Descriptive Analysis

Overall the sample consists of 45,420 individuals and 326,608 spells. Tables 1 and 2 below give further details. There are 8,485 transitions into short-term training and 5,388 transitions into long-term training (cf. table 1). Table 2 shows that about half of the individuals in the sample experience multiple unemployment spells and about a third have more than one employment spell.

— Insert table 1 here. —

— Insert table 2 here. —

Figure 1 displays the Kaplan-Meier estimates of the probability to start a program at some given day in elapsed unemployment conditional on survival in untreated unemployment until that date. It can be seen that the daily hazard rate into short-term training varies between 0.015 and 0.056% and that of long-term training lies between 0.003 and 0.037%.

— Insert figure 1 here. —

We also calculate Kaplan-Meier estimates of the probability to survive in unemployment and employment, respectively, beyond the elapsed duration given on the abscissa by treatment status. These survivor functions are given in figure 2. These estimates do not correct for selection on observables or unobservables. Treatment status during unemployment is a time-varying variable meaning that future participants contribute to the non-treated survivor function until they start training. In the left panel of figure 2, it can be seen that the curve for the treated with long-term training lies above the one for the non-treated and that the area between the two lines has the shape of a lens. This suggests that initially individuals enroling into long-term training leave unemployment at a slower rate than non-treated individuals, which is reflected in a widening of the vertical distance between the two curves. As the share of treated who have completed their training program increases with elapsed unemployment duration the vertical distance between the curves narrows again, meaning that treated individuals now leave unemployment at a faster rate than non-treated. For short-term training there is no such strong evidence for a lock-in effect. Even at short elapsed unemployment durations the survivor functions of participants in short-term training and non-treated are very close. At higher elapsed durations, the fraction of treated with short-term training still unemployed is clearly smaller than that of non-treated or treated with long-term training.

— Insert figure 2 here. —

The right panel of figure 2 shows the survivor functions in employment by treatment status in the preceding unemployment spell. At short elapsed durations, individuals treated with short-term training exit employment at a faster rate than non-treated or former participants in long-term training. The latter have the most stable employment relationships. In fact, the vertical distance to the survivor functions of treated with short-term training and non-treated first increases and then remains largely stable at higher elapsed durations. The descriptive analysis thus suggests that long-term training may have a positive effect on employment duration but at the same time seems to strongly increase unemployment duration. Short-term training, in contrast, tends to reduce unemployment duration but seems to lead to smaller gains in employment duration than long-term training. However, it is important to stress that these patterns reflect a mixture of causal and selection effects. It remains to be seen to what extent they persist after accounting for selection into treatments and into employment and unemployment over time.

Turning again to the dynamics of treatment effects during unemployment, we also computed the hazard rate out of unemployment based on estimates of the Kaplan-Meier survivor function. The left panel of figure 3 shows the hazard rate obtained using the total population of entrants into unemployment, whereby those starting short-term training or long-term training within the first 182 days of unemployment are classified as treated from the moment of program start onwards. Those entering later are censored at the entry into program. The right panel of figure 3 shows the hazard rate estimated on the subset of individuals surviving at least 365 days in open unemployment. This time individuals beginning a training program between day 365 and 547 of unemployment are classified as treated from the moment of program start onwards. Those entering from day 548 onwards are again censored at the moment of program start. In particular, comparing the figure on the right to that on the left, it seems that the lock-in effect, i.e. the initial reduction of the hazard rate of participants relative to nonparticipants, is completely absent for participants in short-term training and less pronounced for those in long-term training. Also, the post-participation increase of the hazard rate for former participants of long-term training seems to be higher in the right figure. On a purely descriptive basis, however, one cannot tell whether this evidence can indeed be interpreted as dynamic variation of treatment effects with starting date. In fact, the population of individuals surviving until day 365 is likely systematically different from the total population starting an unemployment spell, and in addition selection into treatment may change over time.

— Insert figure 3 here. —

6 Results

In this section, we first present the treatment effects of short-term and long-term training on the exit rates out of unemployment and employment. In a next step, we look at how these instantaneous effects translate into changes in the expected unemployment and employment duration.[29]

6.1 Instantaneous Impacts of Training on the Hazard Rates

Treatment effects during unemployment are modeled as a function of elapsed unemployment duration at program start t_p^n, $p = ST$, LT, elapsed time since program start, t_p, planned program duration, t_p^x, individual characteristics such as age and gender, and unobserved heterogeneity.[30] The treatment effects during employment are modeled using indicators of training status in the preceding unemployment spell,

[29]The complete estimation results are given in table D1, column 3, in Appendix D. The lower panel of table D2 in Appendix D contains the correlations of the unobserved heterogeneity terms in the four hazards and the treatment effects. Table C1 in Appendix C contains the variable descriptions.

[30]Unobserved heterogeneity is modeled as a two-factor loading specification composed of two independent factors. Each latent factor follows a discrete masspoint distribution with support $\{-1, 1\}$.

one for short-term and one for long-term training, and interactions of these variables with individual characteristics and unobserved heterogeneity.[31] The estimated coefficients associated with the treatment effects are displayed in table 3.

— Insert table 3 about here. —

Duration Dependence Patterns of Treatment Effects

The evidence contained in section 5.2 as well as previous evaluations of training programs suggests that treatment effects vary with elapsed time since program start. During participation, participants typically search less intensively for a new job than comparable nonparticipants. Thus, one would expect to find a lower exit rate to employment for participants in training at least in the short run. In order to allow for duration dependent treatment effects in an event history framework, it is common to model treatment effects as a function of elapsed time since program start. However, if program durations vary across participants, it is impossible to infer precisely how search behavior changes during and after program participation based on this approach. Therefore, we model the duration dependence of treatment effects using time relative to the planned program end. In particular, we introduce time-varying dummies indicating different time intervals before and after expected program completion. The time intervals have been selected through a specification search that sequentially aggregated intervals where no or only little variation of the treatment effect was found. For short-term training we found some of these dummies interacted with gender to be significant.

In order to simplify the interpretation, figure 4 graphically illustrates the duration dependence patterns of the treatment effects on the hazard rate out of unemployment for a hypothetical person with all individual characteristics set to the reference category, and unobserved heterogeneity terms set to the mean.[32] The figure confirms the hypothesis that participation reduces search intensity when the scheduled end of the program is still far ahead. For short-term training the hazard rate of the hypothetical person is reduced by 43% when the planned end lies more than 15 days ahead. For the hypothetical participant in long-term training expecting to

[31]We also estimated a model that included interaction effects indicating a participation in both short-term and long-term training. However, these terms were not significant neither in the unemployment nor the employment hazard.

[32]The reference category is male, German, aged 38, no disability or health constraints, educational degree different than vocational education, long-term training starting at day 110 of unemployment. The percentage values of the treatment effects are obtained by applying $(\exp(\cdot)-1)\cdot 100$ to the values given in table 3.

complete his program in more than 360 days, the search intensity decreases by 94%. The hazard rates of participants recover towards the planned program end and exceed that of comparable non-participants shortly before and after the scheduled program completion date. For the hypothetical participant in short-term training the treatment effect peaks at 86% within the first month after expected program completion and for the hypothetical participant in long-term training it reaches a maximum of 113% within the first three months after expected program completion. While the positive impact of short-term training fades away very quickly, that of long-term training is much more lasting, turning zero only one and a half years after the scheduled program end for the hypothetical person depicted in figure 4. Finally, note that both programs reduce the exit rate out of subsequent employment for the hypothetical person considered here. The reduction is 12% for short-term training and 17% for long-term training (see table 3).

— Insert figure 4 about here. —

Heterogeneity of Treatment Effects Across Individual Characteristics

Table D1 in Appendix D includes estimation results for a specification that excludes unobserved heterogeneity in the treatment effects (column 2 of table D1). The log-likelihood for that model is $-840,349.68$, whereas the log-likelihood of our preferred specification, that includes eight additional parameters for the unobservables in the treatment effects, is $-840,321.05$ (column 3 of table D1). This suggests that unobserved heterogeneity is an important feature of the treatment effects. Furthermore, there are interesting differences in treatment effects across observed characteristics of the participants. We find for long-term training programs that treatment effects increase with elapsed time at program start (cf. the terms involving $\ln(t^n_{LT})$ in the right column of table 3).

The effect of long-term training is significantly larger in absolute terms for women than for men. Women participating in long-term training have a $(exp(.2)-1)\cdot 100 = 22$ percentage points higher exit rate out of unemployment and a twelve percentage points lower exit rate out of employment than male participants. Regarding short-term training, the lock-in effect is somewhat more pronounced for female compared to male participants. The short-run impact on the exit rate to work immediately after the expected program end is lower for women than men, but the medium- to long-run impact is higher. As regards the exit rate out of employment, there are no significant gender differences in the treatment effect for short-term training. Female participants in long-term training exhibit a 12 percentage points lower exit rate out of employment than otherwise identical male participants.

Treatment effects during unemployment vary also across age. The treatment effect of long-term training exhibits a concave profile across age. The turning point is at age 30. This means that older participants benefit less from human capital oriented 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. There is no evidence for heterogeneous treatment effects across age on the exit rate out of employment. Thus, once employed there are no differences in treatment effects for individuals of a different age.

Disabled persons benefit particularly from short-term training, while individuals with health constraints fare better with long-term training than with short-term training. In particular, former trainees in short-term training who faced strong health constraints during their job search have a higher exit rate out of employment than trainees with no health problems. The exit rate out of employment is 69 percentage points higher for somebody treated with short-term training who had health constraints affecting placement.

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 percentage points higher escape rate out of unemployment than their German counterparts. Individuals treated with human capital oriented training holding a foreign nationality have a 19 percentage points 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 to reintegrate foreigners back into employment. However, having a foreign nationality tends to offset the employment prolonging effect of long-term training. Finally, there is only little effect heterogeneity across formal educational degrees when the outcome is unemployment, and no effect heterogeneity when the outcome is employment.

6.2 Impacts on Unemployment and Employment Duration and the Timing of Treatment

So far, we have focused on the instantaneous impacts of training on the hazard rates out of unemployment and employment. Another way to analyze treatment effects in an event history framework is to examine the expected unemployment duration by treatment status as a function of elapsed unemployment duration at treatment start.[33] This allows us to study how the treatment effects accumulate over time and how these accumulated effects depend on the starting date. Likewise, one can compute the expected employment duration according to the treatment status in the previous unemployment spell. Contrasting expected unemployment and employment durations allows us to compare short-run and long-run impacts of program participation.[34]

In the following, we focus on the truncated expectation and present results for three different truncation points. With time-varying covariates and piecewise constant baseline hazards and treatment effects, the truncated expected unemployment duration corresponds to

$$\begin{aligned} \mathrm{E}[T_1|\{x(t)\}, v, T_1 \leq t_D] &= \left(\int_0^{t_D} \tau f_1(\tau|x(\tau), v) d\tau\right) / \left(1 - S_1(t_D|x(t_D), v)\right) \\ &= \left[\sum_{d=1}^{D} \int_{t_{d-1}}^{t_d} S_1(\tau|x(\tau), v) d\tau - t_D S(t_D)\right] / [1 - S_1(t_D|x(t_D), v)] \end{aligned}$$

where $\{x(t)\}$ denotes the entire covariate process including the baseline hazard and treatment effect, $x(t_d)$ refers to the covariate process until the beginning of time interval d, $d = 1, \ldots, D$, and v represents the unobserved heterogeneity terms affecting unemployment duration and the treatment effects. $S_1(t|x(t), v) = \exp[-\int_0^t \theta_1(\tau|x(\tau), v) d\tau]$ is the survivor function. t_D denotes the maximum duration until which integration is to be performed. Here, we set t_D in turn to 1826 days (= five years), 3653 days (=10 years), and 7305 days (=20 years). The time horizon of five years reflects a conservative choice that lies within the support of our data, whereas the truncation points of 10 and 20 years extrapolate beyond the sampling frame. The truncated expected employment duration, $\mathrm{E}[T_4|\{x(t)\}, v, T_4 \leq t_D]$, is computed analogously. Furthermore, we consider the median of the nontruncated distribution of employment spells. The median duration $\{t : S_4(t) = 0.5\}$ is calculated as follows: Determine the time interval d for which $S_4(t_{d-1}) \geq 0.5 > S_4(t_d)$ and solve $0.5 = S_4(t_{d-1}) \exp[-\int_{t_{d-1}}^{t} \theta_4(\tau) d\tau]$ for t. This yields $t = \ln[0.5/S(t_{d-1})]/\theta_{4,d} + t_{d-1}$, where $\theta_{4,d}$ denotes the hazard rate evaluated at the covariate and baseline hazard values in time interval d.

In order to evaluate the mean and median outcome durations, the covariates are set to the values of a fictitious, but representative person in the inflow sample. Specifically, we assume that this person is a German aged 38 at the start of unemployment

[33] We consider the unconditional expectation that does not condition on remaining unemployed until the program starting date under consideration.

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

and 39 at the beginning of employment, married with children, living in the German federal states of Hesse, holding the highest high school degree (Abitur) and a vocational training degree, previously employed as a whitecollar service worker, with a salary in the third quartile, entitled to twelve months of unemployment benefits, considered as having relevant vocational qualification by the caseworker, and starting his or her unemployment spell in the first quarter of the year 2000 and the employment spell one year later.[35] The planned program durations are set to 26 and 245 days for short-term and long-term training, respectively. 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 5 depicts the truncated expected unemployment duration according to the treatment status and the waiting time until program start. The top panel assumes a time horizon of five years, the middle panel of ten years, and the bottom panel of 20 years. The left column refers to the representative man, the right to the woman. In each graph, the vertical distance between two curves corresponds to the treatment effect in terms of the difference in the expected unemployment duration. Consider e.g. the top left graph of figure 5. The representative man stays on average 224 days in unemployment when not participating in a training program. Equivalently, this number can be interpreted as the expected unemployment duration that would arise if he participates in training at some given starting date plotted on the abscissae but the treatment effect is hypothetically set to zero. The vertical distance between the "No-training"-line and the line referring to short-term training (ST) 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 plotted on the abscissae. Similarly, the vertical distance between the line referring to long-term training (LT) and the "No-training"-line shows the difference in expected unemployment duration associated with participation in long-term training compared to nonparticipation.

— Insert figure 5 about here. —

A comparison of the different rows of figure 5 shows a stable pattern across the different time frames. This indicates that most of the mass of the distribution of

[35]All other (categorical) variables included in the specification not mentioned are set to the reference category. The original estimation includes time-varying dummies indicating different seasons of the year. In the simulation, we assign each of these variables a time-constant value representing their share in a given calendar year. This way we obtain expected outcome durations that are seasonally adjusted.

unemployment spells lies to the left of the smallest truncation point, i.e. five years, and allowing for a longer time horizon therefore has only little effect on the results.[36] Overall, the graphs reveal that, compared to nonparticipation, short-term training tends to reduce average unemployment duration while long-term training tends to increase it. These effects are the 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 time frame considered, by 20 to 30 days if short-term training occurs during the first three months of unemployment. A participation in long-term training starting in the first three months of unemployment increases the mean unemployment duration of the representative man by up to four months, and by a somewhat more than two months when it is started after three months in unemployment. The patterns for the representative woman are qualitatively similar. The expected unemployment duration in the absence of training is 228 days in the top right graph. The unemployment reducing effect of short-term training is slightly stronger and the adverse effect of long-term training is substantially weaker compared to the representative man. In particular, a participation in short-term training can reduce the expected unemployment duration by up to 37 days. A participation in long-term training within the first three months of unemployment increases the expected employment duration by 68 to 86 days maximally.

In order to better understand these patterns, recall that the unemployment hazard exhibits negative duration dependence, i.e. the probability of exiting to employment decreases with elapsed unemployment duration. A participation in training basically leads to a proportional upward or downward shift of the exit rate to work whose amount varies over time. The treatment effect of short-term training on the unemployment hazard is highest around expected program completion and decreases thereafter. Long-term training, on the contrary, strongly reduces the unemployment hazard if the planned program end lies more than one month ahead and increases the hazard rate out of unemployment strongly from the moment of expected program completion up to one and a half years thereafter. This means that, in absolute terms, 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 his or her unemployment spell. Thus, the mean unemployment duration is shorter for earlier ST-starts than for later starts, and the mean unemployment duration of LT-participants decreases with elapsed unemployment duration at program start.

[36]The mean of the nontruncated distribution without training is 255 days for the representative man and the nontruncated median is 128 days.

In order to get an idea of the long-run impact of the programs, it is useful to compare their effects on unemployment duration with those on subsequent employment duration. Table 4 displays the truncated mean employment duration as well as the median employment duration by treatment status in the preceding unemployment spell. It can be seen that the choice of the truncation point has a considerable effect on the projected truncated mean employment duration and the treatment effect in terms of the difference in mean durations. This is due to the fact that, unlike for unemployment, very long spells are relatively common for employment. Therefore, we also include the median durations in table 4. A participation in any of the two training programs has a beneficial effect on the duration of subsequent employment spells. The employment prolonging effect is larger for long-term training than for short-term training and for the representative woman than for the representative man. At the median of the untruncated distribution, we find that a participation in long-term training increases the subsequent employment spell of the representative man on average by about nine months. For the representative female participant in long-term training this difference is 19 months. As regards short-term training, the effects on employment duration are smaller, with less pronounced gender differences. A participation in short-term training increases the subsequent employment duration at the median by 5.5 (8) months for the representative man (woman).

— Insert table 4 about here. —

The above discussion implies that the sign of the overall effect of long-term training is ambiguous. On the negative side, long-term training tends to increase the length of unemployment spells, while on the positive side it increases subsequent employment duration. Therefore, we 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 6 for the representative man and woman as a function of participation status and starting date of the program. The interpretation of this figure is analogous to that of figure 5: the vertical distance between two lines represents the treatment effect associated with a participation at some given day of unemployment plotted on the abscissae, except that this time the vertical difference measures the percentage point change in the long-run unemployment rate.

— Insert figure 6 about here. —

Figure 6 shows that a participation in short-term training reduces the long-run unemployment rate, and the effect is larger the earlier a participation occurs during the unemployment spell. The overall effectiveness of long-term training tends to increase with elapsed unemployment duration at program start. The net effect of long-term training on the long-run unemployment rate is only positive if it is not started too early. In the middle panel, for the representative man (woman) a participation in long-term training within the first six (three) months of unemployment increases the long-run unemployment rate.

In order to get an idea of the cost-effectiveness of the programs, consider the following back-of-the-envelope calculation. A short-term training course costs on average 560 Euro and a long-term training course 5850 Euro (cf. table A2 in Appendix A). The employment agency 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 short-term 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 long-term training course will be cost-effective if it saves at least 169 days (i.e. around six months) of unemployment in the long run.

Consider a time horizon of ten years (middle panel of figure 6). The representative man (woman) spends $120 \times 0.21 \approx 25$ months ($120 \times 0.20 \approx 24$ months) out of these ten years in unemployment if he (she) does not participate in any training program. First consider the cost-effectiveness of an early participation in short-term training. If the representative man enrols in short-term training during the first three months of his unemployment spell he will stay in total $120 \times 0.18 \approx 22$ months out of ten years unemployed. The representative woman also saves about three months in unemployment through participating in short-term training. Thus, an early participation in short-term training seems to be cost-effective for a person with the same characteristics as the representative man and woman. Moreover, although the overall effectiveness of short-term training declines with elapsed unemployment duration, its impact still exceeds the amount necessary for cost-effectiveness even if it is started only after two years of unemployment.

Next consider the cost-effectiveness of a participation in long-term training within the first three months of the second year of unemployment (days 366 to 457) when its long-run effectiveness flattens out. If the representative man starts training between day 366 and 457, the long-run fraction of time spent in unemployment decreases by about two percentage points which is equivalent to two months. The representative woman saves about three months in unemployment. Comparing these numbers with the six-months threshold established above, long-term training does not seem to be cost-effective for the fictitious, representative persons considered here. However, this latter conclusion depends on the time horizon chosen. The positive impact of training on employment duration increases with the length of the time horizon considered. This means that the rather negative impact of long-term training on unemployment duration becomes less important relative to its positive impact on employment duration. With a time horizon of 20 years, both programs are cost-effective for the representative man and woman.

6.3 Assessment of Overall Impacts

How do the treated individuals in our data fare with training and does training pay off in the long run? In order to answer these questions we computed the truncated mean unemployment and employment durations for every treated individual in the sample in the same way as for the representative persons above using a time frame of ten years.[37] We then analyze the effectiveness of training by calculating the fraction of participants who experience a shorter mean unemployment duration, a longer mean employment duration, and for whom a participation is cost-effective. A participation in training is considered to be cost-effective if the net reduction in time spent unemployed leads to savings in unemployment benefits that equal at least the training costs.[38]

Column 1 of table 5 displays the results from these calculations under the actual distribution of program starts. It can be seen that 69% of the participants in short-term training exhibit a reduction in mean unemployment duration. For 36% (27%) of the participants this reduction is at least three (six) months. The fraction of participants in long-term training experiencing a shorter mean unemployment duration is considerably lower with a value of 46%. As regards employment, a similar fraction of participants in short-term and long-term training, i.e. around 70%, experience longer employment spells through training, but the employment gains are more substantial for participants in long-term training. 42% of those participating in long-term training experience an increase in mean employment duration of at least 183 days, while the same holds for only 22% of the participants in short-term training. A participation in short-term training is cost-effective for 69% of the trainees, whereas this share amounts to 44% for long-term training.

[37]For this purpose, we randomly assigned each person a value of the latent factors using the estimated probabilities of the masspoints.

[38]We assume that one month of unemployment costs around 1050 Euro, a participation in short-term training 560 Euro and in long-term training 5850 Euro.

— Insert table 5 about here. —

In columns 2 to 5 of table 5, we present results for the hypothetical scenario that all individuals started training at the same day of unemployment given in the column header. The results for short term training in the upper panel indicate that, regardless of the starting date considered, in about 30% of the cases participation would always lead to longer unemployment spells. However, among the individuals for whom participation in short-term training has a beneficial effect on unemployment duration the positive effect can be further improved through targeting an early participation. If all individuals hypothetically started short-term training at day 30 of unemployment 45%, instead of 36% under the actual distribution of starting dates, would enjoy mean unemployment durations that were at least three months shorter compared to the situation of nonparticipation. The picture for long-term training goes the other way round. Columns two to five in the lower panel of table 5 show that through targeting a later starting date during unemployment the adverse effect of participation on unemployment duration can be mitigated. In the hypothetical scenario that all individuals enroled at day 180, mean unemployment durations would be shorter for 50%, compared to 37% in the scenario where everybody started at day 30.

6.4 Discussion

From a policy point of view, the above findings show that the timing of training during an unemployment spell is an important dimension of effect heterogeneity that should be taken into account in the design of active labor market policies. Given that the job-finding probability declines over time, 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 order to avoid that participants are locked into the program while their chances to find a job on their own are highest. However, the results do not imply that employment agencies should switch to a regime which assigns training slots as a deterministic function of elapsed unemployment duration. As the unemployed would eventually get to know this rule they would adjust their search behavior accordingly. Depending on whether they would like or dislike the outlook of participating at some given date in the future they would increase or decrease their search effort to make the desired outcome more likely.

7 Conclusion

In many advanced countries, the focus of active labor market policy recently shifted away from comprehensive training schemes aiming at long-term human capital development towards active job-search schemes emphasizing quick job entry. This study investigated and compared the dynamic causal effects of short, job-search oriented training and traditional long-term training schemes in Germany, where both program types were used at the same time. In order to provide a more detailed understanding of how these programs work, we analyzed their impacts on unemployment as well as employment spells, taking into account the role of the timing of participation during unemployment for the overall outcome.

We show that participants in training programs tend to reduce their search intensity during program participation (lock-in effect). These negative impacts of participating in a program are less pronounced and only temporary for short-term training, while the lock-in of participants in long-term training tends to be much deeper and longer, depending on the remaining time until the planned program end. The exit rates to employment of participants recover towards the expected completion date of the program. Positive impacts of training are highest right after the expected completion date. While those of short-term training quickly fade away, those of long-term training tend to persist over a long period.

We also find that treatment effects are heterogeneous across observed and unobserved characteristics of the participants. Both training programs have stronger beneficial effects for women than for men. Training impacts also vary significantly by age, health status, disability status, and nationality of participants. There is only little effect heterogeneity across formal educational degrees.

The instantaneous shifts in the exit rates out of unemployment and employment are reflected in corresponding changes in the expected unemployment and employment duration. While participating in short-term training tends to reduce the expected unemployment duration, long-term training may increase it. In contrast, long-term training tends to have a larger beneficial effect on the expected employment duration than short-term training. Taking into account the heterogeneity of treatment effects according to the timing of training during unemployment, the overall gains out of short-term training are highest when it is started early in the unemployment spell, while long-term training attains better outcomes when it is started after three to six (or even more) months of unemployment.

Simple back-of-the-envelope calculations suggest that short-term training schemes are likely cost-effective for the majority of participants in the sample (i.e. for 69% assuming a time horizon of ten years). Long-term training schemes do not tend to be cost-effective for the majority of participants (i.e. they are cost-effective for 44% assuming a time horizon of ten years). The cost-effectiveness of long-term training is sensitive to the starting date of training. It can be raised by avoiding assignments of newly unemployed.

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 oriented training schemes can be an effective and financially profitable tool to reduce unemployment in the long run, in particular if participation occurs early during unemployment. Participation in a short-term training course costs only a small fraction of what has to be paid for a more comprehensive human capital oriented training scheme, while the overall effectiveness of the short course tends to be no smaller than that of the long one. This is due to the fact that short-term training schemes, unlike long-term training programs, do not prevent participants from searching for new jobs for a long time. Second, because of their potentially substantial adverse effects on unemployment duration long-term training schemes should rather not be given to newly unemployed individuals who are likely to exit unemployment relatively quickly on their own. Third, although we find that the timing of training is an important driver of the overall effectiveness of both programs our results do not imply that an assignment regime in which all unemployed enrol automatically after a given time would be advantageous. The reason is that anticipation of future participation would likely change the search behavior of the unemployed.

From a conceptual point of view, this study highlights that time is an important dimension in evaluation analyses which can hardly be incorporated in experimental designs. A detailed analysis of the dynamics of program assignment and labor market outcomes allows a deeper understanding of how programs work. This is important for the optimal allocation of employment services. Dynamic outcomes may not just involve unemployment and employment durations, as considered here, but also e.g. transitions into and out of low income spells. Such an analysis is left for future research.

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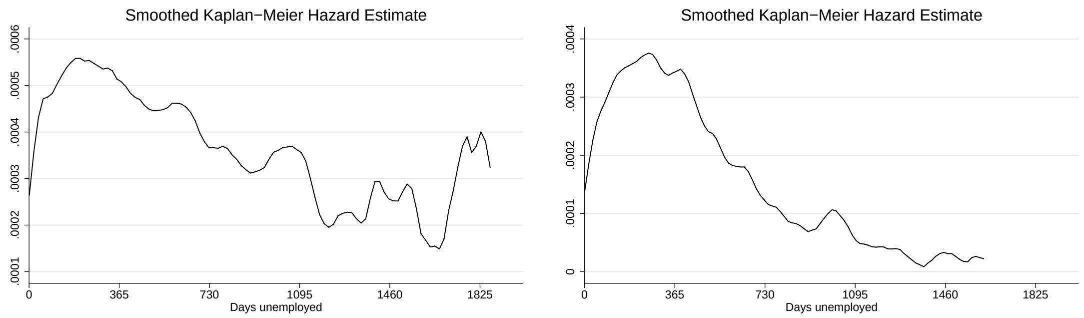
Figures

Figure 1: Hazard Rate into Training Programs

Short Training | Long Training



Notes: The bandwidth for the kernel smooth of the hazard rates is 30 days.

Figure 2: Survival in Unemployment and Employment by Treatment Status

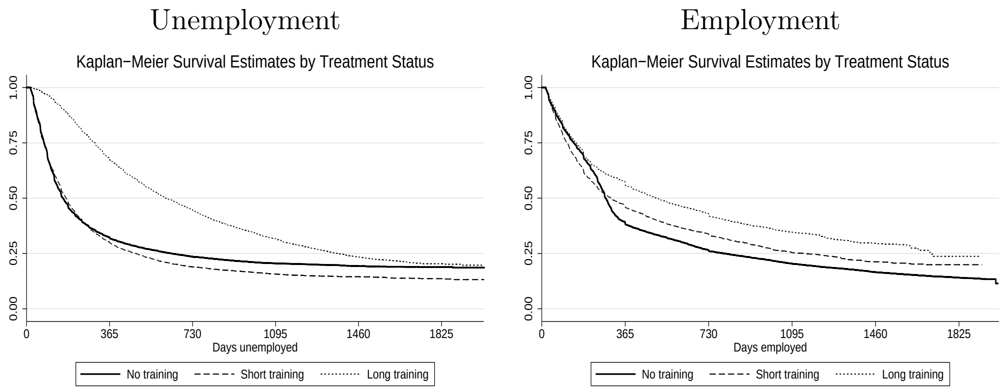
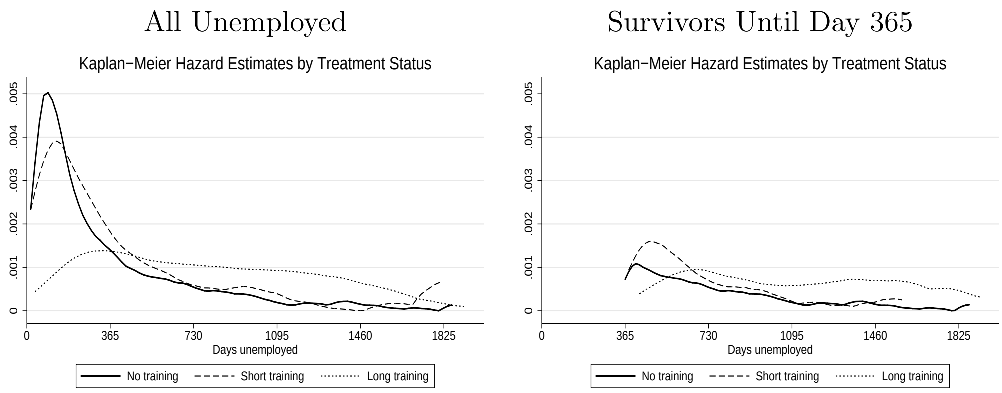
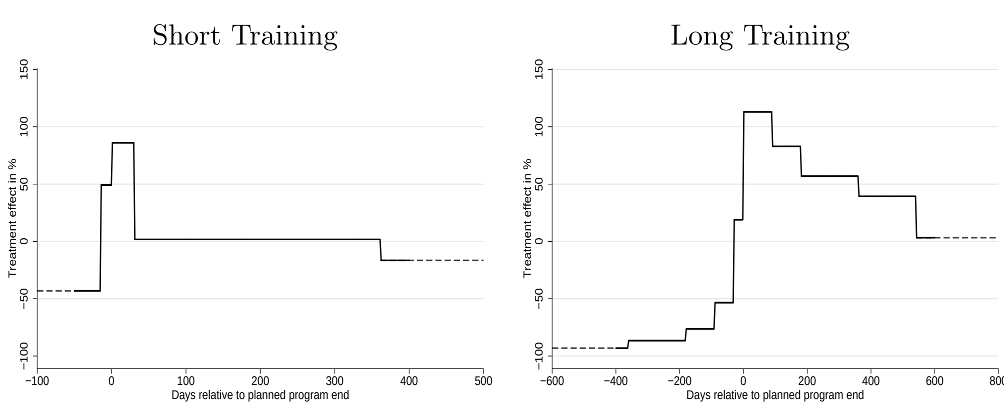


Figure 3: Hazard Rate out of Unemployment by Treatment Status



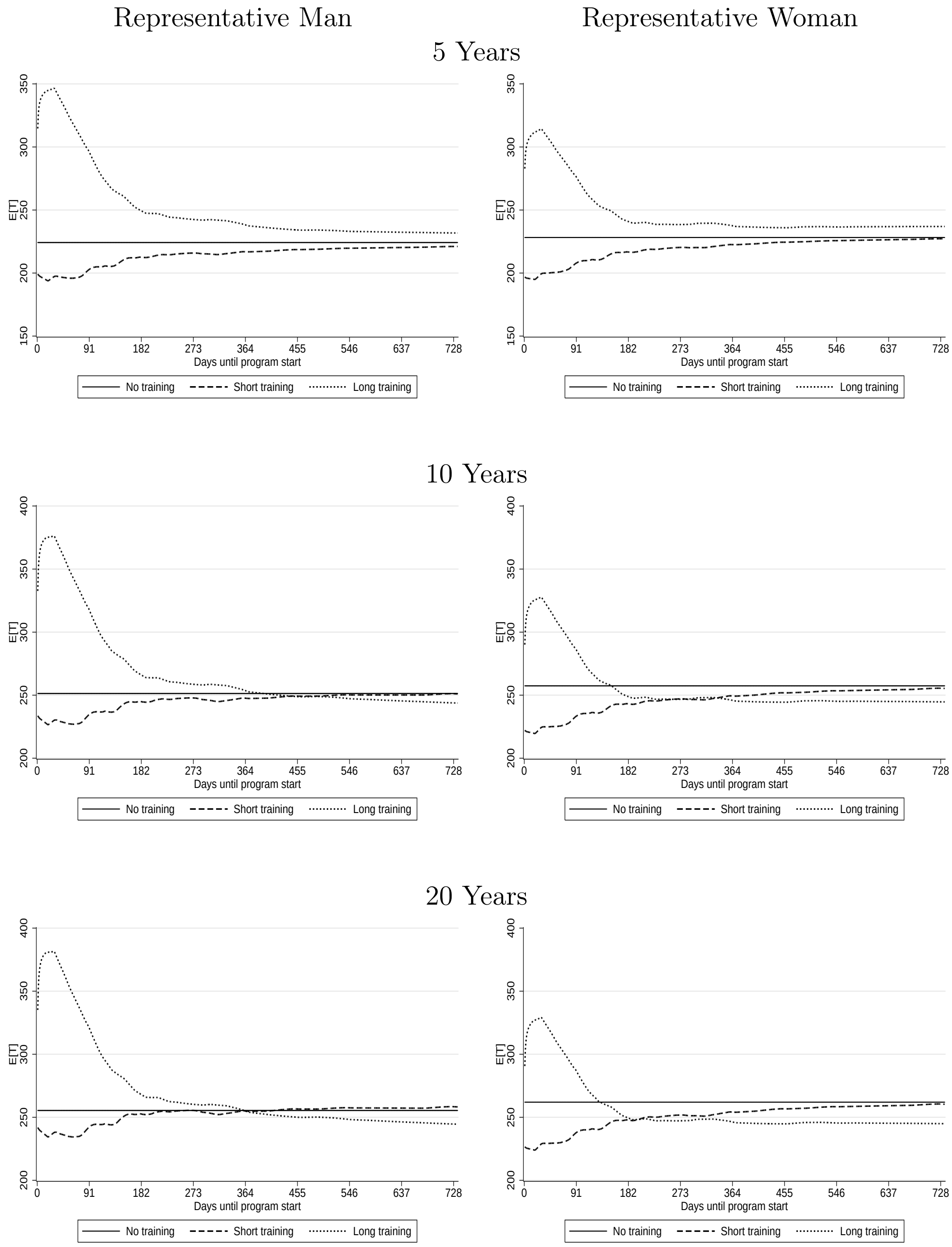
Notes: The bandwidth for the kernel smooth of the hazard rates is 30 days.

Figure 4: Treatment Effects on the Unemployment Hazard



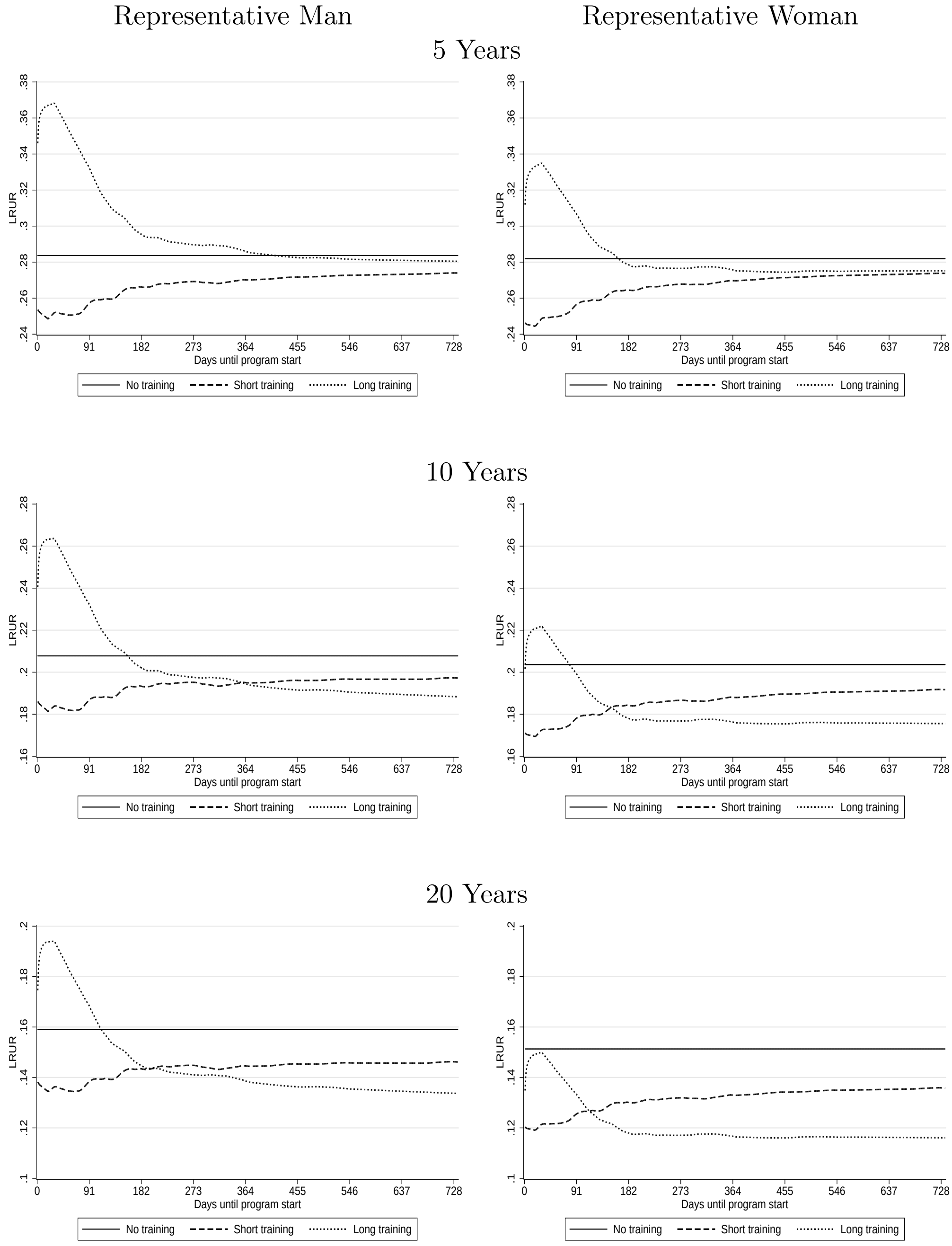
Notes: The treatment effects correspond to the effects for a 38 year old German man without disability or health restrictions, and an education different than vocational education degree. The starting date of long-term training is set to day 110 of unemployment. The unobserved heterogeneity terms are set to their means.

Figure 5: Truncated Expectation of Unemployment Duration by Starting Date of Program



Notes: The calculations are for a representative person. The three panels entitled five, ten, and 20 years indicate the different truncation points until which the integration is performed.

Figure 6: Long-Run Unemployment Rate by Starting Date of Program



Notes: The calculations are for a representative person. The long-run unemployment rate is computed as the truncated mean unemployment duration divided by the sum of the truncated mean unemployment and the truncated mean employment duration. The truncation points are five (top), ten (middle), and 20 years (bottom panel), respectively.

Tables

Table 1: Sample Size (Spells by Spell Type)

Spell type	Censored	Completed	Total
UE to EM	30,456	56,706	87,162
	34.94%	65.06%	100.00%
UE to ST	78,677	8,485	87,162
	90.27%	9.73%	100.00%
UE to LT	81,774	5,388	87,162
	93.82%	6.18%	100.00%
EM to UE	15,840	49,282	65,122
	24.32%	75.68%	100.00%
Total	206,747	119,861	326,608
	63.30%	36.70%	100.00%

Notes: UE to EM, ST, and LT denotes unemployment with destination state employment, short-term training, and long-term training, respectively. EM to UE denotes employment with destination state unemployment.

Table 2: Individuals by Number of Spells of a Given Type

Number of spells	Frequency	Percent	Cumulated
Spell type: UE to EM, ST, and LT			
1	23,705	52.19	52.19
2	11,494	25.31	77.50
3 or more	10,221	22.50	100.00
Total	45,420	100.00	
Spell type: EM to UE			
0	12,163	26.78	26.78
1	16,890	37.19	63.97
2	8,290	18.25	82.22
3 or more	8,077	17.78	100.00
Total	45,420	100.00	

Notes: UE to EM, ST, and LT denotes unemployment with destination state employment, short-term training, and long-term training, respectively. EM to UE denotes employment with destination state unemployment.

Table 3: Treatment Effects on the Hazard Rates

Short-Term Training		Long-Term Training	
	Coef. (SE)		Coef. (SE)
Treatment Effects on Hazard Rate from Unemployment to Employment			
$t^x_{ST} - t_{ST} > 15$	-0.763 (0.092)***	$t^x_{LT} - t_{LT} > 360$	-3.012 (0.137)***
$15 \geq t^x_{ST} - t_{ST} > 0$	0.203 (0.083)**	$360 \geq t^x_{LT} - t_{LT} > 180$	-2.339 (0.120)***
$0 \geq t^x_{ST} - t_{ST} > -31$	0.423 (0.076)***	$180 \geq t^x_{LT} - t_{LT} > 90$	-1.776 (0.108)***
$-31 \geq t^x_{ST} - t_{ST} > -361$	-0.181 (0.079)**	$90 \geq t^x_{LT} - t_{LT} > 30$	-1.097 (0.105)***
$t^x_{ST} - t_{ST} \leq -361$	-0.379 (0.101)***	$30 \geq t^x_{LT} - t_{LT} > 0$	-0.160 (0.103)
$(-31 \leq t^x_{ST} - t_{ST} > -361)\times$fem.	0.650 (0.111)***	$0 \geq t^x_{LT} - t_{LT} > -91$	0.517 (0.095)***
$(t^x_{ST} - t_{ST} \geq -361)\times$female	0.703 (0.138)***	$-91 \geq t^x_{LT} - t_{LT} > -181$	0.270 (0.105)***
		$-181 \geq t^x_{LT} - t_{LT} > -361$	0.117 (0.110)
		$-361 \geq t^x_{LT} - t_{LT} > -541$	-0.002 (0.130)
		$t^x_{LT} - t_{LT} \leq -541$	-0.301 (0.137)**
		LT$\times \ln(t^n_{LT})$	-0.096 (0.057)*
		LT$\times \ln(t^n_{LT})$ squared	0.020 (0.008)***
ST×female	-0.143 (0.054)***	LT×female	0.200 (0.043)***
ST×foreign	0.093 (0.036)**	LT×foreign	0.171 (0.047)***
ST×disabled	0.092 (0.041)**	LT×disabled	0.060 (0.025)**
ST×minor health constraints	0.099 (0.070)	LT×minor health constraints	0.132 (0.085)
ST×strong health constraints	0.340 (0.072)***	LT×strong health constraints	0.268 (0.088)***
ST×age/10	0.207 (0.224)	LT×age/10	0.486 (0.288)*
ST×age/10 squared	-0.035 (0.029)	LT×age/10 squared	-0.080 (0.038)**
		LT×vocational education	-0.206 (0.043)***
Treatment Effects on Hazard Rate from Employment to Unemployment			
ST	0.033 (0.072)	LT	0.062 (0.096)
ST×female	-0.023 (0.047)	LT×female	-0.133 (0.057)**
ST×foreign	-0.046 (0.067)	LT×foreign	0.227 (0.086)***
ST×ethnic German	-0.336 (0.146)**	LT×ethnic German	0.119 (0.142)
ST×strong health constraints	0.524 (0.101)***	LT×disabled	0.036 (0.015)**
Unobserved Heterogeneity in Treatment Effects During Unemployment			
factor loading on w_1	0.284 (0.077)***	factor loading on w_1	0.266 (0.084)***
factor loading on w_2	-0.004 (0.038)	factor loading on w_2	-0.139 (0.055)**
Unobserved Heterogeneity in Treatment Effects During Employment			
factor loading on w_1	-0.169 (0.088)*	factor loading on w_1	-0.307 (0.110)***
factor loading on w_2	0.105 (0.035)***	factor loading on w_2	0.089 (0.052)*
Probabilities			
	$\Pr(w_1 = 1)$	0.846 (0.013)***	
	$\Pr(w_2 = 1)$	0.301 (0.023)***	

Notes: ST and LT indicate participation in short-term and long-term training, respectively. During unemployment these variables are time-varying and equal to one from the moment of program start onwards. During employment these dummies equal one if a participation took place in the previous unemployment spell. t^n_p, $p = ST,\ LT$, denotes the starting date of short-term and long-term training, respectively, t^x_p the planned program duration, and t_p the elapsed time since program start. The variable age is centered around 38 years, $\ln(t^n_{LT})$ around the log of 110 days. Unobserved heterogeneity is modeled as a two factor loading specification. The two independent latent factors w_1 and w_2 are distributed with support $\{-1, 1\}$. *, ** and *** denote significance at the 10%-, 5%- and 1%- level, respectively. The complete estimation results are given in table D1, column 3, in Appendix D.

Table 4: Truncated Expectation and Median of Employment Duration in Days by Treatment Status

	5 Years	10 Years	20 Years	Median
		Representative Man		
No training	566	958	1350	716
Short training	586	1022	1509	887
Long training	595	1050	1585	985
		Representative Woman		
No training	581	1007	1469	839
Short training	603	1077	1656	1083
Long training	624	1149	1865	1413

Notes: The calculations are for a representative person. The first three columns display the truncated expectation with truncation at five, ten, and 20 years, respectively. The last column shows the median duration.

Table 5: Overall Impact Assessment for Treated Individuals in the Sample

	Actual	30	180	360	720
	Short-Term Training				
Shorter unemployed	0.693	0.699	0.694	0.697	0.678
– by 90 days or more	0.362	0.454	0.325	0.288	0.278
– by 183 days or more	0.265	0.271	0.262	0.260	0.260
Longer employed	0.705	0.705	0.705	0.705	0.705
– by 183 days or more	0.218	0.218	0.218	0.218	0.218
– by 365 days or more	0.192	0.192	0.192	0.192	0.192
Cost effective	0.694	0.697	0.692	0.691	0.686
	Long-Term Training				
Shorter unemployed	0.462	0.369	0.498	0.536	0.575
– by 90 days or more	0.314	0.276	0.352	0.331	0.282
– by 183 days or more	0.237	0.203	0.249	0.260	0.260
Longer employed	0.708	0.708	0.708	0.708	0.708
– by 183 days or more	0.427	0.427	0.427	0.427	0.427
– by 365 days or more	0.195	0.195	0.195	0.195	0.195
Cost effective	0.436	0.383	0.464	0.479	0.483

Notes: The rows labeled 'Shorter unemployed' contain the fraction of treated individuals in the sample who experience on average a shorter unemployment duration through participating in training. Likewise the rows labeled 'Longer employed' display the fraction of treated individuals who experience on average a longer employment spell after training. The rows labeled 'Cost effective' give the corresponding fraction of trainees for whom the participation is cost-effective in the sense that saved unemployment benefits due to shorter unemployment spells and/or longer employment spells exceed training costs. The calculations are based on truncated expectations of unemployment and employment durations, where the time horizon is ten years. The first column refers to the actual distribution of program starts, whereas the other columns refer to scenarios where all participants start at the same day of elapsed unemployment indicated in the column header.

Supplementary Appendix

A Further Institutional Information

A.1 Regulation of Training

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 labor market, job creation schemes on the second labor market to training programs that adjust and enhance the qualifications of participants. Employment subsidies aim at promoting either dependent employment or business start-ups. Job creation schemes provide employment opportunities in non-profit organizations for long-term and difficult-to-place unemployed. In addition to these large scale programs, there also exist programs targeted to particular groups as e.g. young or disabled persons. Active labor market policy is complemented by placement and advisory services, that are increasingly contracted out to private providers.

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 (*Trainingsmaßnahmen und Maßnahmen der Eignungsfeststellung*).[39] 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.

To become eligible for any active labor market program job-seekers have to personally 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 programs do not have to fulfil any additional eligibility criteria. As regards long-term training schemes, individuals are in principle 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

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

the job-seeker to find a new job. This is for instance the case if the employment chances in the target occupation of a job-seeker are good but require an additional adjustment of his skills.

Participation in the program is mandatory once the job-seeker has been assigned to a specific course or a training voucher, respectively. However, the unemployed are generally encouraged to continue searching while they are enroled 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 long-term training schemes typically draw subsistence payments that have the same amount as the unemployment compensation payments they would otherwise receive.[40] Participants in training programs who are not entitled to unemployment insurance payments may receive subsistence payments that are financed by the European Social Fund.

A.2 Aggregate Figures on Participation and Expenditures

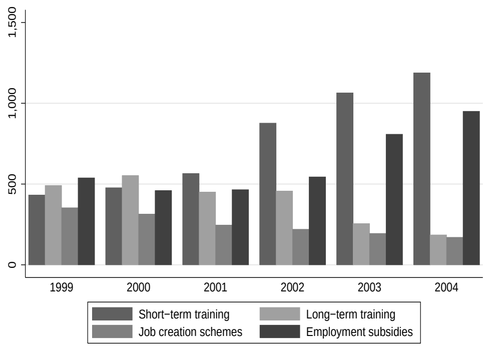
Long-term training schemes have traditionally been the most important field of active labor market policy in Germany. Since 1998 there have been several reforms leading to a focus on measures considered particularly effective in activating the unemployed in the short run and in preventing long-term unemployment. Thus, allocation of resources was shifted away from the very comprehensive long-term training schemes to the short-term training measures. In fact, figure A1 as well as table A1 show a decline in entries in the long-term training programs – in the Western Länder as well as in total Germany – whereas participation in short-term training increases over time.

From table A2 it can be seen that the average monthly training costs per participant are slightly lower for short-term training courses (560 Euros on average) than for traditional long-term training schemes (650 Euros on average). Most striking is the great difference in average duration of the courses, that is displayed in the lower

[40]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.

panel of table A2. While short-term training courses last on average one month, the mean duration of long-term training programs lies between eight and ten months. Under the budgetary pressure caused by 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, in 2003 and 2004, 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 with correspondingly higher gains.

Figure A1: Entries into Selected Active Labor Market Programs in Germany (in 1000)



Source: Bundesanstalt für Arbeit (2001a, 2003a), Bundesagentur für Arbeit (2005a); own calculations.

Table A1: Entries into Active Labor Market Programs in West Germany (in 1000)

	1999	2000	2001	2002	2003	2004
Training schemes	714	770	643	972	985	1,038
– long-term training	307	338	261	273	161	124
– short-term training	265	286	339	545	690	789
Employment subsidies	245	225	206	245	365	481
Placement & advisory services	286	279	296	375	640	1,398
Job creation schemes	96	89	73	63	39	42
Specific measures for youths	327	265	280	295	262	270
Other	231	296	370	345	17	175
Total	1,899	1,924	1,867	2,295	2,308	3,405

Source: Bundesanstalt für Arbeit (2001a, 2003a), Bundesagentur für Arbeit (2005a); own calculations.

Table A2: Average Monthly Expenditures and Program Durations

	1999	2000	2001	2002	2003	2004
	Average monthly expenditures (in Euro)					
Short-term training	602	580	570	658	538	421
Long-term 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
Long-term training	8.4	8.2	9.3	9.1	10.5	10.7

Notes: 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: Bundesanstalt für Arbeit (2000, 2001b, 2002, 2003b), Bundesagentur für Arbeit (2004, 2005b, 2005c); own calculations.

B Further Details on the Data Used

This study uses data from the IEBS, a 2.2% sample from the Integrated Employment Biographies (IEB) Version 4.02. These data are compiled by the Research Data Center of the German Federal Employment Agency.[41] The IEB is an integrated data base combining administrative records out of four different sources: the Employment History (*Beschäftigten-Historik*), the Benefit Recipient History (*Leistungsempfänger-Historik*), the Job-Seeker Database (*Bewerberangebot*), and the Program Participation History (*Maßnahme-Teilnehmer-Gesamtdatenbank*).

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.[42]

The Program Participation History contains detailed information on participation in active labor market programs taking place in the period 2000 to mid-2005. Comparing the entries into different programs in 1999 with the figures for later years shows that information on programs starting in 1999 seems to be already complete for most active labor market programs. Furthermore, this database allows to distinguish subsidized employment in the context of active labor market policy from regular employment.

The Job-Seeker Database 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 Database includes a rich variety of information on personal characteristics (in par-

[41] For descriptions of the data in German see Hummel et al. (2005), Zimmermann et al. (2007), and Oberschachtsiek et al. (2009). For information in English see Jacobebbinghaus and Seth (2007) as well as the web site of the Research Data Center of the Federal Employment Agency (http://fdz.iab.de/en.aspx). The data are subject to confidentiality regulations. A weakly anonymous version can be used on-site and via controlled remote access, a factually anonymous version is available as scientific use file, in which regional information, nationality and industry are provided at a more aggregated level. The analysis in this paper is based on a weakly anonymous version.

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

ticular education, family status and health), and information related to placement fields (e.g. qualification and experience in the target profession). The Job-Seeker Database 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.

C List of Variables Used in the Estimation

Table C1: Variable Definitions

Name	Definition
bhazXX	time-varying 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 categories: 1 25-29 years, 2 30-34 years, 3 35-39 years, 4 40-44 years, 5 45-49 years, 6 50 or older
age, agesq	age divided by ten, age squared divided by 100, centered around 38 years
f_age, f_agesq	age, 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	when unemployed with four categories: 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 dummy equal to one if person had health problems affecting placement within last two months of job search before start of employment spell
disabled	dummy equal to one if disabled
family	1 information missing; 2 living alone; 3 not married, but living together with at least one person; 4 lone 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 C1: 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 information missing, 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 agriculture 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
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)

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Table C1: Variable Definitions <continued>

Name	Definition
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
season1-season4	time-varying dummies indicating the current calender quarter
hasclaim	time-varying dummy equal to one if entitled to unemployment benefits and claim not yet expired (unemployment spells only)
totclaim, totclaimsq	time-varying variable, (square of) total entitlement period for unemployment benefits in months interacted with hasclaim, centered around twelve months (unemployment spells only)
ubendXX	time-varying dummy equal to one if the number of days until expiration of the unemployment claim is smaller or equal to XX days and larger than the number of days referring to the next interval dummy or greater than zero in the last interval (unemployment spells only)
lwquart1-lwaquart4	dummy variables indicating the quartile of last salary, additional category for information missing in employment 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
clwc, clws	variables indicating whether last salary is above (clwc) or below (clws) social security threshold
pst	time-varying dummy equal to one if participation in short-term training has started (unemployment spells only)
stend_XX	time-varying dummy variable equal to one if the number of days until scheduled program end of short-term training is smaller or equal to XX days and larger than the number of days referring to the next interval dummy; the last interval ends at the scheduled completion date (unemployment spells only)
stendXX	time-varying dummy variable equal to one if the number of days after scheduled program completion of short-term training is greater or equal to XX days and smaller than the number of days referring to the next interval dummy; the last interval is open ended (unemployment spells only)
pft	time-varying dummy equal to one if participation in long-term training has started (unemployment spells only)
ltend_XX	time-varying dummy variable equal to one if the number of days until scheduled program end of long-term training is smaller or equal to XX days and larger than the number of days referring to the next interval dummy; the last interval ends at the scheduled completion date (unemployment spells only)
ltendXX	time-varying dummy variable equal to one if the number of days after scheduled program completion of long-term training is greater or equal to XX days and smaller than the number of days referring to the next interval dummy; the last interval is open ended (unemployment spells only)

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Table C1: Variable Definitions <continued>

Name	Definition
lntft, lntftsq	time-varying, (square of) log of days unemployed at start of long-term training, positive if program has started otherwise zero, centered around 110 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 long-term training during previous unemployment spell (employment spells only)

Notes: If not noted otherwise, variables are time constant and refer to the start of a spell. In unemployment spells, job characteristics refer to the previous employment. Descriptions of additional interaction terms and aggregated categories are omitted if the content can be inferred from the variable name and the context. For example the variable industry67 is equal to one if industry is either category 6 or 7; a variable name starting with f_ indicates an interaction with the dummy female.

D Detailed Estimation Results

Table D1: Estimated Coefficients

	Specification 1	Specification 2	Specification 3
	Unemployment to Employment		
bhaz30	1.053 (0.019)***	1.137 (0.019)***	1.139 (0.019)***
bhaz50	1.056 (0.020)***	1.218 (0.020)***	1.221 (0.021)***
bhaz70	1.001 (0.020)***	1.237 (0.022)***	1.243 (0.022)***
bhaz90	1.199 (0.020)***	1.496 (0.022)***	1.504 (0.022)***
bhaz110	1.045 (0.022)***	1.382 (0.024)***	1.393 (0.024)***
bhaz130	0.676 (0.025)***	1.046 (0.027)***	1.058 (0.027)***
bhaz150	0.924 (0.024)***	1.319 (0.026)***	1.333 (0.027)***
bhaz170	0.692 (0.027)***	1.104 (0.029)***	1.120 (0.029)***
bhaz190	0.237 (0.032)***	0.662 (0.035)***	0.679 (0.035)***
bhaz210	0.515 (0.030)***	0.951 (0.033)***	0.968 (0.033)***
bhaz230	0.229 (0.025)***	0.677 (0.028)***	0.694 (0.029)***
bhaz290	-0.025 (0.034)	0.431 (0.036)***	0.449 (0.037)***
bhaz330	0.056 (0.044)	0.516 (0.046)***	0.533 (0.047)***
bhaz350	0.169 (0.044)***	0.603 (0.046)***	0.620 (0.046)***
bhaz370	-0.159 (0.031)***	0.264 (0.034)***	0.282 (0.035)***
bhaz460	-0.401 (0.036)***	0.043 (0.039)	0.062 (0.039)
bhaz550	-0.604 (0.034)***	-0.168 (0.038)***	-0.149 (0.038)***
bhaz735	-1.116 (0.036)***	-0.664 (0.041)***	-0.646 (0.042)***
female	0.089 (0.015)***	0.090 (0.018)***	0.089 (0.019)***
foreign	-0.176 (0.010)***	-0.178 (0.012)***	-0.179 (0.012)***

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Table D1: Estimated Coefficients <continued>

	Specification 1	Specification 2	Specification 3
ethnicgerman	0.106 (0.025)***	0.131 (0.034)***	0.130 (0.035)***
seasonwork	0.279 (0.018)***	0.299 (0.022)***	0.301 (0.023)***
whitecollar	-0.277 (0.054)***	-0.273 (0.064)***	-0.271 (0.064)***
bluecollar	-0.136 (0.054)**	-0.143 (0.064)**	-0.142 (0.064)**
parttime	-0.218 (0.054)***	-0.233 (0.064)***	-0.233 (0.064)***
area2	-0.116 (0.012)***	-0.133 (0.016)***	-0.132 (0.016)***
area3	-0.008 (0.014)	-0.004 (0.019)	-0.003 (0.019)
area4	0.157 (0.014)***	0.209 (0.018)***	0.211 (0.018)***
rtyp2	-0.126 (0.015)***	-0.146 (0.019)***	-0.150 (0.019)***
rtyp5	0.100 (0.012)***	0.119 (0.015)***	0.121 (0.015)***
education3	0.108 (0.012)***	0.172 (0.016)***	0.173 (0.016)***
education4	0.093 (0.027)***	0.140 (0.034)***	0.138 (0.034)***
schooling3	0.074 (0.015)***	0.116 (0.019)***	0.117 (0.020)***
schooling4	0.076 (0.021)***	0.113 (0.027)***	0.113 (0.027)***
occupation2	0.026 (0.020)	0.039 (0.024)	0.039 (0.024)
occupation3	0.297 (0.028)***	0.303 (0.038)***	0.295 (0.039)***
occupation4	0.154 (0.019)***	0.169 (0.022)***	0.171 (0.023)***
occupation5	0.146 (0.018)***	0.151 (0.022)***	0.155 (0.022)***
occupation7	0.054 (0.020)***	0.041 (0.024)*	0.043 (0.024)*
occupation8	0.085 (0.024)***	0.096 (0.028)***	0.098 (0.028)***
industry3	-0.155 (0.014)***	-0.172 (0.017)***	-0.170 (0.017)***
industry4	0.149 (0.015)***	0.185 (0.019)***	0.186 (0.019)***
industry67	-0.021 (0.013)*	-0.018 (0.015)	-0.016 (0.015)
agegroup2	-0.059 (0.014)***	-0.068 (0.017)***	-0.068 (0.017)***
agegroup3	-0.121 (0.014)***	-0.155 (0.018)***	-0.155 (0.018)***
agegroup4	-0.207 (0.018)***	-0.269 (0.023)***	-0.273 (0.023)***
agegroup5	-0.299 (0.018)***	-0.353 (0.023)***	-0.359 (0.023)***
agegroup6	-0.348 (0.024)***	-0.439 (0.032)***	-0.447 (0.032)***
f_agegroup4	0.063 (0.024)***	0.098 (0.029)***	0.098 (0.029)***
f_agegroup6	-0.147 (0.032)***	-0.165 (0.040)***	-0.164 (0.041)***
kids	0.058 (0.013)***	0.096 (0.017)***	0.099 (0.017)***
f_youngchild	-0.309 (0.025)***	-0.365 (0.030)***	-0.369 (0.030)***
f_kids	-0.047 (0.022)**	-0.108 (0.028)***	-0.107 (0.029)***
family3	-0.043 (0.023)*	-0.067 (0.028)**	-0.069 (0.029)**
family4	-0.045 (0.026)*	-0.082 (0.033)**	-0.083 (0.033)**
family5	0.022 (0.011)*	-0.006 (0.014)	-0.009 (0.015)
health3	-0.338 (0.020)***	-0.423 (0.025)***	-0.427 (0.026)***
health4	-0.519 (0.022)***	-0.626 (0.027)***	-0.632 (0.027)***
disabled	-0.025 (0.006)***	-0.038 (0.007)***	-0.038 (0.007)***
taredu3	0.045 (0.011)***	0.075 (0.014)***	0.077 (0.014)***
tarexp	-0.066 (0.015)***	-0.086 (0.018)***	-0.085 (0.018)***
endlastjob3	-0.108 (0.019)***	-0.111 (0.022)***	-0.110 (0.022)***
endlastjob4	0.087 (0.012)***	0.082 (0.015)***	0.082 (0.015)***

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Table D1: Estimated Coefficients <continued>

	Specification 1	Specification 2	Specification 3
prevtrans	0.242 (0.011)***	0.223 (0.012)***	0.224 (0.013)***
y1999q4	0.029 (0.024)	0.025 (0.027)	0.023 (0.027)
y2000q1	0.043 (0.024)*	0.055 (0.027)**	0.056 (0.027)**
y2000q2	-0.107 (0.027)***	-0.099 (0.030)***	-0.100 (0.030)***
y2000q3	-0.057 (0.026)**	-0.049 (0.029)*	-0.051 (0.029)*
y2000q4	-0.078 (0.024)***	-0.088 (0.027)***	-0.090 (0.027)***
y2001q1	-0.100 (0.024)***	-0.110 (0.027)***	-0.111 (0.027)***
y2001q2	-0.284 (0.026)***	-0.297 (0.030)***	-0.298 (0.030)***
y2001q3	-0.231 (0.025)***	-0.261 (0.029)***	-0.266 (0.029)***
y2001q4	-0.194 (0.023)***	-0.254 (0.026)***	-0.257 (0.027)***
y2002q1	-0.075 (0.029)***	-0.171 (0.033)***	-0.174 (0.033)***
y2002q2	-0.474 (0.037)***	-0.599 (0.042)***	-0.603 (0.042)***
y2002q3	-0.394 (0.035)***	-0.509 (0.040)***	-0.515 (0.040)***
y2002q4	-0.205 (0.027)***	-0.346 (0.031)***	-0.353 (0.031)***
y2003q1	-0.181 (0.029)***	-0.319 (0.033)***	-0.321 (0.034)***
y2003q2	-0.563 (0.040)***	-0.758 (0.046)***	-0.761 (0.046)***
y2003q3	-0.392 (0.038)***	-0.544 (0.043)***	-0.547 (0.043)***
y2003q4	-0.229 (0.029)***	-0.404 (0.033)***	-0.409 (0.033)***
y2004q1	-0.226 (0.032)***	-0.417 (0.037)***	-0.423 (0.037)***
y2004q2	-0.594 (0.049)***	-0.791 (0.054)***	-0.790 (0.054)***
y2004q3	-0.577 (0.055)***	-0.812 (0.060)***	-0.816 (0.060)***
y2004q4	-1.075 (0.093)***	-1.326 (0.095)***	-1.330 (0.095)***
lwquart2	0.064 (0.015)***	0.063 (0.017)***	0.062 (0.017)***
lwquart3	0.142 (0.015)***	0.152 (0.018)***	0.153 (0.018)***
lwquart4	0.287 (0.016)***	0.300 (0.019)***	0.299 (0.019)***
hasclaim	-0.437 (0.037)***	-0.436 (0.041)***	-0.435 (0.041)***
ubend60	0.174 (0.025)***	0.182 (0.026)***	0.180 (0.026)***
ubend30	0.296 (0.026)***	0.315 (0.027)***	0.313 (0.027)***
totclaim	0.071 (0.005)***	0.048 (0.006)***	0.048 (0.006)***
totclaimsq	-0.002 (0.000)***	-0.002 (0.000)***	-0.002 (0.000)***
season2	0.317 (0.011)***	0.394 (0.012)***	0.398 (0.012)***
season3	0.135 (0.013)***	0.214 (0.014)***	0.219 (0.014)***
season4	-0.325 (0.013)***	-0.253 (0.014)***	-0.248 (0.014)***
pst	0.569 (0.042)***	0.655 (0.045)***	0.423 (0.076)***
stend_16	-1.159 (0.069)***	-1.197 (0.070)***	-1.185 (0.069)***
stend_15	-0.197 (0.059)***	-0.244 (0.060)***	-0.220 (0.060)***
stend31	-0.561 (0.046)***	-0.569 (0.046)***	-0.604 (0.047)***
stend361	-0.720 (0.069)***	-0.748 (0.071)***	-0.802 (0.075)***
f_stend31	0.235 (0.068)***	0.231 (0.069)***	0.228 (0.068)***
f_stend361	0.279 (0.103)***	0.277 (0.106)***	0.280 (0.106)***
pst_female	-0.145 (0.053)***	-0.145 (0.055)***	-0.143 (0.054)***
pst_foreign	0.103 (0.034)***	0.088 (0.037)**	0.093 (0.036)**
pst_disabled	0.076 (0.040)*	0.090 (0.041)**	0.092 (0.041)**

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Table D1: Estimated Coefficients <continued>

	Specification 1	Specification 2	Specification 3
pst_health3	0.099 (0.065)	0.075 (0.071)	0.099 (0.070)
pst_health4	0.321 (0.067)***	0.320 (0.074)***	0.340 (0.072)***
pst_age	0.220 (0.210)	0.235 (0.227)	0.207 (0.224)
pst_agesq	-0.038 (0.027)	-0.040 (0.029)	-0.035 (0.029)
pft	0.859 (0.052)***	0.803 (0.058)***	0.517 (0.095)***
ltend_361	-3.306 (0.115)***	-3.479 (0.119)***	-3.435 (0.121)***
ltend_360	-2.682 (0.095)***	-2.789 (0.097)***	-2.762 (0.098)***
ltend_180	-2.161 (0.077)***	-2.220 (0.079)***	-2.199 (0.080)***
ltend_90	-1.481 (0.069)***	-1.530 (0.070)***	-1.520 (0.071)***
ltend_30	-0.542 (0.066)***	-0.590 (0.066)***	-0.583 (0.067)***
ltend91	-0.178 (0.058)***	-0.162 (0.058)***	-0.153 (0.059)**
ltend181	-0.361 (0.063)***	-0.330 (0.064)***	-0.306 (0.067)***
ltend361	-0.515 (0.090)***	-0.468 (0.091)***	-0.425 (0.096)***
ltend541	-0.869 (0.092)***	-0.795 (0.095)***	-0.724 (0.105)***
lntft	-0.095 (0.050)*	-0.081 (0.056)	-0.096 (0.057)*
lntftsq	0.017 (0.007)**	0.019 (0.007)**	0.020 (0.008)***
pft_female	0.177 (0.039)***	0.200 (0.043)***	0.200 (0.043)***
pft_foreign	0.169 (0.042)***	0.161 (0.046)***	0.171 (0.047)***
pft_disabled	0.038 (0.023)*	0.052 (0.025)**	0.060 (0.025)**
pft_health3	0.075 (0.077)	0.131 (0.084)	0.132 (0.085)
pft_health4	0.222 (0.080)***	0.263 (0.088)***	0.268 (0.088)***
pft_age	0.532 (0.261)**	0.471 (0.284)*	0.486 (0.288)*
pft_agesq	-0.089 (0.034)***	-0.078 (0.037)**	-0.080 (0.038)**
pft_education3	-0.171 (0.039)***	-0.211 (0.043)***	-0.206 (0.043)***
Intercept	-5.947 (0.076)***	-5.989 (0.092)***	-6.010 (0.092)***
	Unemployment to Short-Term Training		
bhaz40	-0.204 (0.036)***	-0.223 (0.036)***	-0.219 (0.036)***
bhaz90	-0.087 (0.037)**	-0.122 (0.038)***	-0.116 (0.038)***
bhaz150	0.017 (0.046)	-0.027 (0.046)	-0.020 (0.047)
bhaz190	-0.023 (0.034)	-0.075 (0.035)**	-0.067 (0.036)*
bhaz760	-0.212 (0.060)***	-0.266 (0.061)***	-0.258 (0.061)***
foreign	-0.148 (0.027)***	-0.148 (0.027)***	-0.148 (0.027)***
daqtiv	0.125 (0.028)***	0.130 (0.028)***	0.129 (0.028)***
seasonwork	-0.179 (0.048)***	-0.179 (0.048)***	-0.179 (0.048)***
rtyp3	-0.091 (0.031)***	-0.095 (0.031)***	-0.094 (0.031)***
rtyp4	-0.223 (0.044)***	-0.230 (0.044)***	-0.229 (0.044)***
rtyp5	-0.152 (0.035)***	-0.162 (0.035)***	-0.161 (0.035)***
occupation3	-0.091 (0.085)	-0.091 (0.085)	-0.090 (0.085)
occupation4	-0.037 (0.036)	-0.037 (0.036)	-0.037 (0.036)
occupation5	0.074 (0.044)*	0.074 (0.044)*	0.074 (0.044)*
occupation6	0.146 (0.042)***	0.148 (0.042)***	0.147 (0.042)***
occupation7	0.053 (0.049)	0.057 (0.049)	0.056 (0.049)

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Table D1: Estimated Coefficients <continued>

	Specification 1	Specification 2	Specification 3
occupation8	-0.060 (0.063)	-0.060 (0.063)	-0.060 (0.063)
industry3	0.289 (0.039)***	0.302 (0.039)***	0.299 (0.039)***
industry5	0.226 (0.040)***	0.236 (0.040)***	0.234 (0.040)***
industry6	0.198 (0.043)***	0.208 (0.043)***	0.206 (0.043)***
industry7	0.075 (0.047)	0.089 (0.047)*	0.086 (0.047)*
agegroup2	-0.034 (0.036)	-0.031 (0.036)	-0.032 (0.036)
agegroup3	-0.062 (0.036)*	-0.057 (0.036)	-0.058 (0.036)
agegroup4	-0.153 (0.038)***	-0.146 (0.038)***	-0.148 (0.038)***
agegroup5	-0.313 (0.043)***	-0.304 (0.043)***	-0.305 (0.043)***
agegroup6	-0.721 (0.056)***	-0.705 (0.056)***	-0.708 (0.056)***
family4	0.197 (0.049)***	0.200 (0.049)***	0.199 (0.049)***
family5	-0.155 (0.024)***	-0.155 (0.024)***	-0.155 (0.024)***
health3	-0.057 (0.042)	-0.049 (0.042)	-0.051 (0.042)
health4	-0.154 (0.043)***	-0.146 (0.043)***	-0.147 (0.043)***
disabled	0.094 (0.020)***	0.094 (0.020)***	0.094 (0.020)***
seekpt	-0.218 (0.042)***	-0.218 (0.042)***	-0.218 (0.042)***
taredu3	0.067 (0.025)***	0.060 (0.025)**	0.062 (0.025)**
taredu4	0.100 (0.059)*	0.094 (0.059)	0.096 (0.059)
tarexp	-0.034 (0.036)	-0.032 (0.036)	-0.032 (0.036)
earlycontact	0.141 (0.025)***	0.138 (0.025)***	0.139 (0.025)***
q2	0.066 (0.030)**	0.066 (0.030)**	0.066 (0.030)**
q3	0.138 (0.027)***	0.137 (0.027)***	0.137 (0.027)***
y20002001	0.336 (0.042)***	0.338 (0.042)***	0.338 (0.042)***
y2002	0.519 (0.050)***	0.540 (0.050)***	0.537 (0.050)***
y20032004	0.714 (0.047)***	0.743 (0.047)***	0.739 (0.047)***
lwquart2	0.065 (0.035)*	0.063 (0.035)*	0.064 (0.035)*
lwquart3	0.136 (0.035)***	0.132 (0.035)***	0.133 (0.035)***
lwquart4	-0.057 (0.038)	-0.061 (0.038)	-0.060 (0.038)
hasclaim	-0.144 (0.085)*	-0.131 (0.085)	-0.133 (0.085)
totclaim	0.038 (0.012)***	0.037 (0.012)***	0.037 (0.012)***
totclaimsq	-0.001 (0.000)*	-0.001 (0.000)*	-0.001 (0.000)*
season2	0.265 (0.031)***	0.258 (0.031)***	0.259 (0.031)***
season3	0.136 (0.032)***	0.128 (0.032)***	0.129 (0.032)***
season4	-0.078 (0.032)**	-0.082 (0.032)**	-0.082 (0.032)**
Intercept	-7.722 (0.125)***	-7.898 (0.132)***	-7.845 (0.132)***
Unemployment to Long-Term Training			
bhaz90	0.209 (0.044)***	0.206 (0.045)***	0.202 (0.045)***
bhaz140	0.275 (0.048)***	0.272 (0.050)***	0.267 (0.050)***
bhaz190	0.188 (0.044)***	0.182 (0.046)***	0.176 (0.047)***
bhaz370	-0.362 (0.058)***	-0.362 (0.060)***	-0.372 (0.061)***
daqtiv	0.360 (0.039)***	0.361 (0.039)***	0.361 (0.039)***
female	-0.032 (0.035)	-0.030 (0.035)	-0.030 (0.035)

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Table D1: Estimated Coefficients <continued>

	Specification 1	Specification 2	Specification 3
foreign	-0.193 (0.035)***	-0.195 (0.035)***	-0.194 (0.035)***
ethnicgerman	0.399 (0.076)***	0.398 (0.077)***	0.398 (0.076)***
seasonwork	-0.297 (0.065)***	-0.300 (0.065)***	-0.300 (0.065)***
bluecollar	-0.124 (0.050)**	-0.122 (0.050)**	-0.123 (0.050)**
whitecollar	0.107 (0.048)**	0.109 (0.048)**	0.108 (0.048)**
land2	0.319 (0.073)***	0.319 (0.073)***	0.320 (0.073)***
land3	0.097 (0.042)**	0.099 (0.042)**	0.098 (0.042)**
land6	0.092 (0.052)*	0.093 (0.052)*	0.092 (0.052)*
land7	0.204 (0.060)***	0.203 (0.060)***	0.203 (0.060)***
land8	0.215 (0.044)***	0.223 (0.044)***	0.219 (0.044)***
land9	0.152 (0.041)***	0.153 (0.041)***	0.152 (0.041)***
education4	-0.147 (0.081)*	-0.147 (0.081)*	-0.147 (0.081)*
schooling3	0.204 (0.048)***	0.207 (0.048)***	0.205 (0.048)***
schooling4	0.512 (0.061)***	0.517 (0.061)***	0.515 (0.061)***
occupation5	0.044 (0.049)	0.044 (0.049)	0.044 (0.049)
occupation6	0.434 (0.044)***	0.435 (0.044)***	0.435 (0.044)***
occupation7	0.168 (0.053)***	0.169 (0.053)***	0.169 (0.053)***
industry3	0.123 (0.037)***	0.122 (0.037)***	0.123 (0.037)***
industry4	-0.420 (0.056)***	-0.426 (0.056)***	-0.426 (0.056)***
industry7	-0.206 (0.041)***	-0.201 (0.041)***	-0.202 (0.041)***
agegroup2	-0.077 (0.043)*	-0.074 (0.043)*	-0.074 (0.043)*
agegroup34	-0.180 (0.040)***	-0.178 (0.040)***	-0.178 (0.040)***
agegroup5	-0.514 (0.053)***	-0.511 (0.053)***	-0.510 (0.053)***
agegroup6	-1.017 (0.066)***	-1.012 (0.066)***	-1.011 (0.066)***
f_youngchild	-0.151 (0.059)**	-0.156 (0.060)***	-0.153 (0.060)***
family3	0.145 (0.071)**	0.140 (0.071)**	0.141 (0.071)**
family4	0.447 (0.062)***	0.441 (0.062)***	0.442 (0.062)***
family5	0.058 (0.031)*	0.054 (0.031)*	0.055 (0.031)*
health4	-0.112 (0.055)**	-0.116 (0.055)**	-0.113 (0.055)**
seekpt	-0.334 (0.055)***	-0.340 (0.055)***	-0.339 (0.055)***
taredu4	-0.123 (0.076)	-0.127 (0.076)*	-0.126 (0.076)*
tarexp	-0.364 (0.043)***	-0.365 (0.043)***	-0.364 (0.043)***
earlycontact	0.328 (0.032)***	0.324 (0.033)***	0.324 (0.033)***
prevtrans	-0.210 (0.031)***	-0.198 (0.031)***	-0.200 (0.031)***
offben	-0.664 (0.236)***	-0.665 (0.236)***	-0.665 (0.236)***
q3	-0.096 (0.036)***	-0.095 (0.036)***	-0.095 (0.036)***
q4	-0.244 (0.036)***	-0.241 (0.036)***	-0.241 (0.036)***
y2000	-0.115 (0.043)***	-0.112 (0.044)**	-0.112 (0.044)**
y2001	-0.561 (0.047)***	-0.559 (0.047)***	-0.558 (0.047)***
y2002	-1.137 (0.069)***	-1.134 (0.069)***	-1.129 (0.070)***
y2003	-1.322 (0.078)***	-1.322 (0.078)***	-1.315 (0.079)***
y2004	-1.617 (0.117)***	-1.616 (0.118)***	-1.609 (0.118)***
lwquart2	0.073 (0.046)	0.071 (0.046)	0.071 (0.046)

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Table D1: Estimated Coefficients <continued>

	Specification 1	Specification 2	Specification 3
lwquart3	0.150 (0.046)***	0.149 (0.046)***	0.148 (0.046)***
lwquart4	0.043 (0.049)	0.042 (0.049)	0.042 (0.049)
hasclaim	0.219 (0.045)***	0.193 (0.047)***	0.202 (0.048)***
ubend120	0.096 (0.064)	0.104 (0.065)	0.101 (0.065)
ubend90	0.222 (0.063)***	0.232 (0.063)***	0.230 (0.063)***
ubend60	0.069 (0.055)	0.082 (0.055)	0.079 (0.055)
season2	-0.100 (0.038)***	-0.099 (0.038)***	-0.100 (0.038)***
season3	0.067 (0.036)*	0.068 (0.037)*	0.067 (0.037)*
season4	-0.527 (0.042)***	-0.525 (0.042)***	-0.527 (0.042)***
pst	0.349 (0.042)***	0.348 (0.042)***	0.350 (0.042)***
Intercept	-7.570 (0.108)***	-7.667 (0.123)***	-7.655 (0.122)***

	Employment to Unemployment		
bhaz60	0.208 (0.017)***	0.291 (0.017)***	0.288 (0.017)***
bhaz120	-0.013 (0.019)	0.139 (0.020)***	0.133 (0.020)***
bhaz180	0.130 (0.022)***	0.329 (0.024)***	0.320 (0.025)***
bhaz210	0.087 (0.022)***	0.325 (0.025)***	0.316 (0.025)***
bhaz240	0.482 (0.021)***	0.763 (0.023)***	0.757 (0.024)***
bhaz270	0.701 (0.020)***	1.010 (0.023)***	1.008 (0.023)***
bhaz300	0.459 (0.024)***	0.783 (0.026)***	0.782 (0.026)***
bhaz330	0.152 (0.025)***	0.485 (0.028)***	0.485 (0.028)***
bhaz370	-0.456 (0.039)***	-0.105 (0.040)***	-0.104 (0.041)**
bhaz400	-0.545 (0.031)***	-0.177 (0.033)***	-0.175 (0.033)***
bhaz460	-0.621 (0.033)***	-0.235 (0.035)***	-0.232 (0.035)***
bhaz520	-0.571 (0.044)***	-0.173 (0.046)***	-0.168 (0.046)***
bhaz550	-0.725 (0.035)***	-0.318 (0.037)***	-0.311 (0.037)***
bhaz610	-0.527 (0.033)***	-0.116 (0.035)***	-0.109 (0.035)***
bhaz670	-0.488 (0.047)***	-0.074 (0.049)	-0.066 (0.049)
bhaz700	-0.244 (0.041)***	0.176 (0.043)***	0.183 (0.043)***
bhaz735	-0.981 (0.060)***	-0.552 (0.062)***	-0.543 (0.062)***
bhaz770	-0.951 (0.021)***	-0.488 (0.024)***	-0.477 (0.024)***
female	-0.092 (0.016)***	-0.088 (0.020)***	-0.085 (0.020)***
age	-0.062 (0.070)	-0.232 (0.088)***	-0.246 (0.090)***
agesq	0.014 (0.009)	0.038 (0.011)***	0.040 (0.011)***
f_age	-0.725 (0.121)***	-0.769 (0.151)***	-0.733 (0.153)***
f_agesq	0.083 (0.015)***	0.087 (0.019)***	0.082 (0.019)***
foreign	0.021 (0.015)	0.007 (0.019)	0.006 (0.019)
ethnicgerman	-0.057 (0.028)**	-0.106 (0.036)***	-0.109 (0.037)***
seasonwork	0.313 (0.041)***	0.379 (0.043)***	0.383 (0.043)***
land1	0.101 (0.021)***	0.084 (0.027)***	0.080 (0.027)***
land2	0.112 (0.029)***	0.145 (0.035)***	0.147 (0.036)***
land3	0.060 (0.014)***	0.060 (0.018)***	0.059 (0.018)***
land4	0.103 (0.046)**	0.120 (0.054)**	0.129 (0.054)**

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Table D1: Estimated Coefficients <continued>

	Specification 1	Specification 2	Specification 3
land6	-0.022 (0.018)	-0.040 (0.022)*	-0.040 (0.023)*
land8	-0.064 (0.016)***	-0.125 (0.020)***	-0.127 (0.020)***
land9	0.061 (0.012)***	0.064 (0.016)***	0.058 (0.016)***
schooling34	-0.035 (0.016)**	-0.038 (0.019)**	-0.039 (0.020)**
education3	-0.173 (0.011)***	-0.231 (0.014)***	-0.236 (0.015)***
education4	-0.303 (0.027)***	-0.344 (0.033)***	-0.350 (0.033)***
whitecollar	-0.099 (0.018)***	-0.112 (0.021)***	-0.115 (0.021)***
bluecollar	0.159 (0.016)***	0.177 (0.019)***	0.176 (0.019)***
occupation3	0.054 (0.034)	0.096 (0.045)**	0.083 (0.046)*
occupation4	0.022 (0.014)	0.036 (0.017)**	0.032 (0.017)*
occupation5	-0.035 (0.018)*	-0.037 (0.022)*	-0.039 (0.022)*
occupation6	-0.133 (0.019)***	-0.148 (0.022)***	-0.146 (0.023)***
occupation7	-0.088 (0.022)***	-0.097 (0.026)***	-0.096 (0.026)***
industry3	-0.112 (0.080)	-0.175 (0.085)**	-0.187 (0.086)**
industry4	1.112 (0.079)***	1.173 (0.083)***	1.165 (0.084)***
industry5	0.034 (0.075)	-0.009 (0.080)	-0.022 (0.080)
industry6	0.211 (0.078)***	0.180 (0.083)**	0.168 (0.083)**
industry7	-0.078 (0.079)	-0.157 (0.084)*	-0.167 (0.084)**
kids	-0.086 (0.012)***	-0.118 (0.014)***	-0.121 (0.015)***
family3	0.174 (0.026)***	0.244 (0.030)***	0.247 (0.030)***
family4	0.198 (0.029)***	0.314 (0.035)***	0.317 (0.035)***
family5	0.046 (0.011)***	0.113 (0.014)***	0.112 (0.014)***
health	0.245 (0.022)***	0.301 (0.025)***	0.308 (0.026)***
disabled	0.073 (0.003)***	0.088 (0.003)***	0.089 (0.003)***
lwquart12	-0.276 (0.021)***	-0.275 (0.024)***	-0.280 (0.024)***
lwquart3	-0.370 (0.021)***	-0.359 (0.025)***	-0.363 (0.025)***
lwquart4	-0.467 (0.021)***	-0.446 (0.025)***	-0.452 (0.025)***
q1	-0.213 (0.011)***	-0.201 (0.013)***	-0.205 (0.013)***
q3	-0.060 (0.013)***	-0.063 (0.014)***	-0.064 (0.014)***
y2000	0.101 (0.027)***	0.025 (0.030)	0.026 (0.030)
y2001	0.221 (0.027)***	0.151 (0.030)***	0.152 (0.030)***
y2002	0.401 (0.027)***	0.330 (0.031)***	0.330 (0.031)***
y2003	0.491 (0.028)***	0.419 (0.032)***	0.414 (0.032)***
y2004q1	0.431 (0.037)***	0.341 (0.041)***	0.332 (0.042)***
y2004q2	0.596 (0.036)***	0.516 (0.040)***	0.502 (0.040)***
y2004q3	0.698 (0.046)***	0.620 (0.050)***	0.614 (0.051)***
y2004q4	0.836 (0.060)***	0.781 (0.065)***	0.770 (0.065)***
season23	-0.492 (0.090)***	-0.454 (0.091)***	-0.459 (0.091)***
season4	0.613 (0.081)***	0.560 (0.082)***	0.558 (0.082)***
s2_seasonwork	-0.486 (0.054)***	-0.529 (0.055)***	-0.533 (0.055)***
s3_seasonwork	-0.305 (0.054)***	-0.385 (0.055)***	-0.388 (0.055)***
s4_seasonwork	0.236 (0.047)***	0.228 (0.048)***	0.224 (0.048)***
s2_industry3	0.233 (0.097)**	0.192 (0.098)*	0.195 (0.098)**

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Table D1: Estimated Coefficients <continued>

	Specification 1	Specification 2	Specification 3
s2_industry4	-1.299 (0.097)***	-1.344 (0.099)***	-1.349 (0.099)***
s2_industry567	0.337 (0.089)***	0.288 (0.090)***	0.292 (0.090)***
s3_industry3	0.337 (0.096)***	0.272 (0.097)***	0.276 (0.097)***
s3_industry4	-1.034 (0.095)***	-1.145 (0.097)***	-1.149 (0.097)***
s3_industry5	0.386 (0.089)***	0.321 (0.090)***	0.324 (0.090)***
s3_industry6	0.504 (0.093)***	0.437 (0.094)***	0.440 (0.094)***
s3_industry7	0.692 (0.094)***	0.628 (0.095)***	0.631 (0.095)***
s4_industry3	-0.095 (0.086)	-0.081 (0.087)	-0.080 (0.087)
s4_industry4	-0.875 (0.084)***	-0.911 (0.086)***	-0.918 (0.086)***
s4_industry5	-0.189 (0.078)**	-0.162 (0.079)**	-0.162 (0.079)**
s4_industry6	-0.328 (0.084)***	-0.300 (0.085)***	-0.300 (0.085)***
s4_industry7	-0.091 (0.086)	-0.052 (0.086)	-0.051 (0.087)
dst	-0.103 (0.026)***	-0.155 (0.030)***	0.033 (0.072)
dst_female	-0.056 (0.039)	-0.026 (0.045)	-0.023 (0.047)
dst_foreign	-0.073 (0.057)	-0.055 (0.064)	-0.046 (0.067)
dst_ethnicgerman	-0.300 (0.120)**	-0.348 (0.139)**	-0.336 (0.146)**
dst_health	0.478 (0.086)***	0.517 (0.097)***	0.524 (0.101)***
dft	-0.243 (0.034)***	-0.264 (0.042)***	0.062 (0.096)
dft_female	-0.108 (0.048)**	-0.118 (0.055)**	-0.133 (0.057)**
dft_foreign	0.203 (0.074)***	0.229 (0.083)***	0.227 (0.086)***
dft_ethnicgerman	0.132 (0.120)	0.163 (0.135)	0.119 (0.142)
dft_disabled	0.028 (0.013)**	0.030 (0.014)**	0.036 (0.015)**
Intercept	-6.182 (0.086)***	-5.955 (0.094)***	-5.977 (0.094)***
Factor loadings on first latent factor (w_1)			
UE to EM		-0.757 (0.017)***	-0.750 (0.018)***
δ_{ST}			0.284 (0.077)***
δ_{LT}			0.266 (0.084)***
UE to ST		0.221 (0.048)***	0.161 (0.051)***
UE to LT		0.086 (0.075)	0.090 (0.076)
EM to UE		0.040 (0.025)	0.037 (0.027)
γ_{ST}			-0.169 (0.088)*
γ_{LT}			-0.307 (0.110)***
Factor loadings on second latent factor (w_2)			
UE to EM		-0.408 (0.020)***	-0.393 (0.022)***
δ_{ST}			-0.004 (0.038)
δ_{LT}			-0.139 (0.055)**
UE to ST		0	0
UE to LT		-0.097 (0.042)**	-0.068 (0.048)
EM to UE		0.651 (0.019)***	0.620 (0.020)***
γ_{ST}			0.105 (0.035)***
γ_{LT}			0.089 (0.052)*

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Table D1: Estimated Coefficients <continued>

	Specification 1	Specification 2	Specification 3
	Probabilities		
$\Pr(w_1 = 1)$		0.862 (0.011)***	0.846 (0.013)***
$\Pr(w_2 = 1)$		0.273 (0.019)***	0.301 (0.023)***
Parameters	338	347	355
Log-L.	-842,376.54	-840,349.68	-840,321.05
Observations	2,893,445	2,893,445	2,893,445
Spells	326,608	326,608	326,608
Individuals	45,420	45,420	45,420

Notes: Specification 1 refers to an alternative model with no unobserved heterogeneity and specification 2 to an alternative model without unobserved heterogeneity in the treatment effects. Specification 3 is the main model discussed in the text. Standard errors are in parentheses. *, ** and *** denote significance at the 10%-, 5%- and 1%- level, respectively.

Table D2: Estimated Correlation Coefficients between the Individual Specific Effects in the Hazard Rates and the Treatment Effects

Specification 2

	UE to EM	UE to ST	UE to LT	EM to UE
UE to EM	1			
UE to ST	-0.820 (0.021)***	1		
UE to LT	0.009 (0.471)	0.565 (0.388)	1	
EM to UE	-0.611 (0.020)***	0.048 (0.030)	-0.797 (0.285)***	1

Specification 3

	UE to EM	δ_{ST}	δ_{LT}	UE to ST	UE to LT	EM to UE	γ_{ST}	γ_{LT}
UE to EM	1							
δ_{ST}	-0.822 (0.095)***	1						
δ_{LT}	-0.389 (0.224)*	0.845 (0.155)***	1					
UE to ST	-0.832 (0.022)***	1 (0.003)***	0.834 (0.136)***	1				
UE to LT	-0.224 (0.588)	0.739 (0.418)*	0.985 (0.121)***	0.726 (0.412)*	1			
EM to UE	-0.593 (0.023)***	0.028 (0.172)	-0.511 (0.213)**	0.048 (0.034)	-0.652 (0.458)	1		
γ_{ST}	0.308 (0.309)	-0.795 (0.206)***	-0.996 (0.034)***	-0.783 (0.208)***	-0.996 (0.063)***	0.584 (0.270)**	1	
γ_{LT}	0.589 (0.203)***	-0.945 (0.099)***	-0.974 (0.080)***	-0.938 (0.087)***	-0.919 (0.205)***	0.302 (0.245)	0.950 (0.135)***	1

Notes: Specification 2 refers to an alternative model without unobserved heterogeneity in the treatment effects. Specification 3 is the main model discussed in the text. Correlations are calculated based on var($\ln v$) using the estimated coefficients in table D1, third column. δ_{ST} and δ_{LT} represent the unobserved heterogeneity components associated with participation in short-term training and traditional further training, respectively, that affect the unemployment hazard. γ_{ST} an γ_{LT} denote the corresponding heterogeneity terms 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.

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