Individual and Plant-level Determinants of Job Durations in Germany

first draft
26-09-2005

*Bernhard Boockmann**
Susanne Steffes[‡]

Centre for European Economic Research (ZEW)
P.O. Box 10 34 43
D-68034 Mannheim

Abstract: We examine job durations of German workers using a linked employer-employee dataset. The descriptive evidence suggests that firm characteristics have a substantial influence on the job exit rate. However, the amount of dispersion in durations is not substantially lower at the firm level than for the sample as a whole, pointing to the presence of segmentation between long and short employment spells within establishments. Using the Cox partial likelihood estimator, we then examine the determinants of job exit. There is some evidence that neglecting firm characteristics biases the coefficients of individual-specific variables. Extension of the model to a competing risks framework shows that both individual and firm-level characteristics differ greatly in their impact on job exit to different destination states.

JEL-Codes: J62, J63, C41

Key Words: Job durations, job exit, tenure, linked employer-employee data

Acknowledgements: We wish to thank a number of persons at the Forschungsdatenzentrum of the IAB, and Holger Alda in particular, for their intensive support with the data, and Ralf Sacchetto for competent research assistance.

* Tel.: 0621 / 1235-156, Fax: 0621 / 1235-225, E-mail: boockmann@zew.de. [‡] Tel.: 0621 / 1235-281, Fax: 0621 / 1235-225, E-mail: steffes@zew.de.

1 Introduction

How long workers stay in their jobs is of central importance for individual work histories, employers' personnel policies, and the functioning of the labour market in general. Only recently, however, have employer-employee-data been made available to analyze job durations from both sides of the employment relationship. These data allow us to address a number of new questions.

First, both individual and firm-specific reasons for job exit can be analysed. One the one hand, job-to-job transitions and job exits to other destination states depend on individual characteristics such as age or education. On the other, firms also differ with respect to employment duration, for instance due to different needs of workforce adjustment. Whether a firm uses redundancies or adjusts by means of an internal labour market depends, in turn, on firm characteristics like size or industry and institutions such as works councils. Firm characteristics may also give rise to churning, i.e. separations that are not due to net reductions in the number of workers. Analyzing the determinants of job duration is likely to lead to biased results if either firm or individual characteristics are left out of the analysis.

Second, observed heterogeneity in tenure can be separated into heterogeneity within or across firms. Are short employment spells found in one type of firms and long employment durations in another, or do most firms have long-term as well as short-term workers? In other words, how much less heterogeneity is there in employment durations at the firm level than at the level of the whole economy?

Our data, taken from the German Linked Employer-Employee Dataset of the IAB (LIAB) longitudinal model, comprise all jobs started in a panel of 4200 establishments in East and West Germany during the period from 1996 to 2001. A special feature of the data is that we have not only linked company and person information, but can estimate the whole tenure distribution (for up to 6 years) within each of the establishments in the dataset. A problematic feature is that in almost all cases, only one employment spell per person is represented in the data, which prohibits the use of flexible models for individual-level heterogeneity.

Using German data, job durations have been studied by Bender et al. (2000), Bellmann et al. (2000), Bergemann and Mertens (2002), Gerlach und Stephan (2005) and Wolff (2004). Of these studies, only Bender et al. (2000) and Gerlach und Stephan (2005) use linked employer-employee data. Bender et al. (2000) use combined register and survey data similar to the LIAB data on which

this study is based. They conclude that characteristics of the employer (for instance, employer size or training opportunities) have a significant influence on job exit. The main result of Gerlach und Stephan (2005) using data from a Salary and Wage Structure Survey is that elapsed job duration is longer in firms with collective contracts. Internationally, there are a few studies which take the effects of both individual and establishment characteristics on tenure into account. According to Bronars and Famulari (1997), individual characteristics and company fixed effects explain roughly the same proportion of the total variance in employment durations in the United States. A similar result is obtained by Mumford and Smith (2004). Hence, there is evidence that individual employer's characteristics have an important impact on job durations. Dohmen and Pfann (2003) base their investigation on data from one large company so that time variation in firm-level characteristics can be used to identify their effects on job durations.

The distinction between tenure distributions within and across firms is related to the question whether there is labour market segmentation between long-lasting employment relationships and short-term jobs. Is this kind of segmentation occurring *within* companies, where a core workforce is protected against job losses by the employment of a marginal workforce (Abraham, 1988; Saint-Paul, 1996), or is it a phenomenon that is mainly visible in different job tenure averages in different firms? Institutions protecting insiders such as legal dismissal protection or works councils could give rise to segmentation within companies. This might explain the observation that in countries with weak employment protection such as the United States tenure is more homogenous across the working population, while overall mobility rates are not much different from countries with strong employment protection.

Some evidence for a sorting effect of workers with particular characteristics into different companies is found by Mumford and Smith (2004). Shorter individual tenure of females disappears once workplace effects are allowed. A similar finding is obtained by Gerlach and Stephan (2005). However, neither of these papers directly measures heterogeneity in job durations across and within firms. Moreover, both studies Bronars and Famulari (1997) analyze whether there is within or across establishment variation in wage growth and find across firm heterogeneity. Because of a positive correlation between wages and job durations they conclude that tenure is longer in firms with higher wage growth. However, all these studies use cross-sections of data and use elapsed

tenure as the dependent variable while not accounting for right-censoring and length-bias. By contrast, we use a flow sampling scheme and observe workers over a maximum period of 6 years (1996 to 2001). We only deal with employment relationships started during this period to avoid problems of left-censoring.

The paper is structured as follows. In the next section, we describe the data base in detail and explain how job durations are obtained. In section three, the sample is defined and descriptive statistics are given. We also provide descriptive evidence on within- and across-firm heterogeneity in job durations in this part of the paper. Estimation methods and independent variables are introduced in section 4. The following section contains the estimation results, while some conclusions are drawn in the final section of the paper.

2 Data description

2.1 Basic features of the LIAB data

The database of our study is the German LIAB, a linked employer-employee dataset which has recently become available to researchers at the Institut für Arbeitsmarkt- und Berufsforschung (IAB) (see Bellmann et al., 2002). The LIAB combines administrative data on employees with employer data from a large-scale representative survey of plants, the IAB Establishment Panel. This annual survey contains data on 16,000 establishments. The LIAB is exhaustive on the number of workers covered within the establishment sample. Using the systematic introduced by Abowd and Kramarz (1999), the LIAB is a dataset representative for firms and their respective workers, and based on both data of administrative origin and statistical survey data.

The employee part of the LIAB is the employment statistics register (Beschäftigtenstatistik) of the Federal Employment Agency (see Bender and Haas 2002). This administrative data record is based on all declarations of employers to the German social insurance institutions and has been collected since 1973. Misreporting is a summary offence and can in grave cases even be persecuted as a criminal offence. Therefore, the reliability of the data is high, although this assessment may be qualified for individual variables such as education (see Fitzenberger et al., 2005). The data contains daily information on the beginning and end of all employment relationships covered by the social

security system. Other forms of employment are not recorded in the data; this concerns, in particular, civil servants (Beamte), marginal work remunerated below a monthly income threshold, and employment in a foreign country. Self-employed individuals (together with unpaid family workers) are also not included in the statistics. As a whole, the employment statistics register covers about 80 per cent of total employment.

The Employment Statistics also contains information on a number of characteristics relating to the person or the job. Daily gross wages are included; however, the gross wage is top-censored at the earnings ceiling of the mandatory pension system (currently, 5,150 € per month in West Germany and 4,350 € in East Germany). Furthermore, there is information concerning age, sex, nationality, broad educational groups and profession (three-digit level). Most importantly for the purpose of matching, the data contain an establishment identifier which is also used in the employee survey that contains the establishment information of the LIAB.

The Employment Statistics are combined with data on periods of unemployment. This information is obtained from the benefit recipient data (Leistungsempfängerdatei) of the Federal Employment Agency. Hence, spells of unemployment are only recorded if the unemployed person receives unemployment benefits and/or participates in active labour market policies. In other cases, there is no information about the employment status. With this in mind, complete employment biographies of all employees from the early 1970s through (presently) 2002 can be constructed.

The IAB-Establishment Panel is an annual representative survey of establishments conducted by the Institute of Employment Research (IAB) (more information can be found in Bellmann, 2002, and Kölling, 2000). Thus, the unit of observation is not the company as a legal entity but the plant or site where the economic activities are carried out. The survey started in 1993 with more than 4,000 establishments (West Germany only), was extended to the East in 1996 and currently comprises almost 16,000 establishments in the whole of Germany. The sample is stratified according to the number of employees (obtained from the employment statistics register) and industry. While the Establishment Panel is an almost complete survey of large establishments, the probability of inclusion in the sample drops to roughly one per cent for small establishments with 1 to 5 employees.

The Establishment Panel contains numerous questions about employment, personnel policy, performance, investments and legal structure. Information on the number and composition of staff, the number of hirings and separations, working time and basic facts on industrial relations and other characteristics is provided annually. Other questions are asked only in individual years or in longer intervals. Most information relates to the situation on June 30th of each year. Employment flows can be obtained for the first six months of each year. For other information, the reference period can be one year or longer.

The employer data is matched with the (augmented) employment data through the establishment identifier and a variable indicating the year of the interview or the year in which a person was employed in the particular establishment. Since combining information on all workers who ever worked in a survey establishment would yield a dataset too large to be handled by standard computer hardware, two versions of the LIAB have been developed and made available to researchers at the IAB.[1] There is a cross-section version with employer-employee-data containing all persons employed on June 30th of each year in an establishment participating in the survey. This version contains no historical data but only the information of this particular day. The second version, the LIAB longitudinal version, contains the employment history from January 1st, 1991, of persons who have been employed for at least one day in an establishment of the IAB-Establishment Panel during the period from January 1st, 1996 to December 31st, 2001. Due to the longitudinal character of our research question, we use the second version in the present study.

The number of establishments used is limited to establishments having valid interviews from 1999 to 2001. This means that worker separations due to plant closures cannot be observed in the data. Furthermore, establishments where the number of employees reported in the survey deviated by more than a certain threshold level from the number inferred from the Employment Statistics are dropped.[2] This leaves us with about 2,100 plants in West Germany and more than 3,100 plants in East Germany; the East German dataset is then further reduced to 2,100 establishments by a random draw (Alda, 2004b: 12). If a firm was interviewed in the years 1996 to 1998 too, the data is also available. Overall,

1 Rules for accessing the data can be found at http://fdz.iab.de/. For a description of the two versions see Alda et al. (2005).

2 The thresholds are 10 per cent for establishments with more than 100 employees and increase to 40 per cent for establishments with less than five employees (Alda, 2004c: 3ff.).

the data provide a representative sample that covers 55.3 per cent of West German and 35.0 per cent of Eastern German establishments and 56.9 per cent of West German and 50.0 per cent of Eastern German employees (Alda et al., 2005: 15f).

2.2 Constructing job durations from the LIAB data

In the following, we define an employment spell as the period from the beginning until the end of an employment relationship within a particular establishment. In the original data, employment spells are recorded as a number of sub-spells. This is due to the fact that employers not only report the beginning and end of employment relationships to the insurance institution, but also changes in income and changes in insurance status. In addition, an annual report has to be given at the end of each year. Therefore, sub-spells reach over 365 days at most. If a person interrupts the employment relationship without formally terminating it, such as in cases of parental leave, the firm has to continue reporting on this person and only the wage is set to zero. Due to the variety of reasons for reports to the social security system, employment spells often consist of many more than one sub-spell, so that these sub-spells must be joined.

Because the beginning and the end of employment spells cannot be obtained directly from the data, we have to generate this information. Additional information is needed on the employment state before the current spell and the destination state after the spell. In the following, we describe the method to define these variables in detail, because these are the main variables of our study. For the sake of simplicity we start with the end of a spell and the definition of the destination states.

In our study there are four destination states: unemployment, non-employment, change to a new employer or no further information. The current spell ends in a failure event if the individual moves to unemployment, non-employment or to a new employer and the current employer stated “end of employment” in the report to the insurance institution. The current spell is right censored if we cannot observe the individual any more or if we cannot define a failure (due to the fact that the employer did not state “end of employment”). An end of the employment relationship could also be assumed whenever the establishment identifier changes. Unfortunately, however, the plant identifier often changes although the individual continues working in the same workplace. This happens, for instance, when the legal identity of the employer changes.

There are a large number of cases in which the establishment identifier changes without the end of the employment relationship being recorded in the data. This shows that the Federal Employment Agency assigns new identifiers whenever there is doubt about the identity of the establishment. Therefore, we rely, in addition, on the employers' declarations.

Concerning destination states, unemployment periods are difficult to define because, as mentioned above, the data only contains information for the time a person receives income transfers by the German federal labour office. Because not all unemployed workers qualify for unemployment insurance (UI) and because of sanctions temporarily suspending benefit payments (e.g., in cases of quits), individuals can be unemployed without receiving UI benefits (Fitzenberger and Wilke, 2004; Wilke, 2003). We define unemployment as UI benefit receipt at least for one day within 60 days after the end of the previous employment spell. In this way, most individuals experiencing a benefit suspension should still be counted as unemployed.

A job-to-job change is defined as a separation followed by an employment spell within 60 days after the end of the previous employment spell. We hypothesize that in these cases the new employment relationship was already known when the previous job ended. A special case occurs if the employee returns to the same employer after some time (recall). This can be due to various reasons, such as employment breaks during the winter season and others. We define a recall as a separation and subsequent return to the same employer within at most 90 days. Recalls may be combined with periods of UI benefit receipt. With 90 days, the maximum duration for the transition to be judged as recall is longer than the corresponding interval for job-to-job transitions. This reflects the idiosyncratic relationship between the worker and the employer.[3]

If a person did not receive benefits, did not move to another employer within 60 days after the current employment and did not return to the current employer within 90 days, the destination state is defined as non-employment. Under this category we subsume individuals who are out of labour force, search for a new job (i.e., are unemployed), become self-employed or move to a foreign country. In the following the definition of the destinations states are summarized:

3 In further research we are going to test whether we should take more or less than 90 days.

Unemployment	→	receives unemployment benefits for at least one day within 60 days after separation, is not employed with current employer for at least 90 days after separation
Non-employment	→	is not employed with current employer for the next 90 days after separation, receives no unemployment benefits and is not employed with another employer for at least 60 days after separation
Job-to-job transition	→	takes up employment with another employer within 60 days after separation

To determine the beginning of a spell we proceed similarly, although we do not use the information on the reported end of employment. The state "no observation" is generated in cases of individuals who were not observed for at least one year before the start of the observation period on January 1st, 1996.

Unemployment	→	received unemployment benefits for at least one day during 60 days before hiring, was not employed with current employer for at least 90 days before hiring
Non-employment	→	was not employed with current employer for at least 90 days before hiring, received no unemployment benefits for at least 60 days before hiring, was not employed with another employer for at least 60 days before hiring
Recall	→	was employed with current employer for more than 90 days before hiring, received no unemployment benefits during 60 days before hiring, was not employed with another employer during 60 days before employment
Job-to-job transition	→	was employed with another employer at most 60 days before employment
No observation	→	was not observed since January 1st, 1995.

3 Sample definition and descriptive statistics

3.1 Sample definition

We observe all employees of the sample establishments during 1996 to 2001. However, spells started earlier in these establishments are only incompletely contained in the data, since many will have dissolved by January 1st, 1996. Hence, we face a problem of left-censoring: the sample of employment spells started before 1996 and observed after 1996 is not representative of all spells started in the establishment. In the following, therefore, we use only information on jobs started within the six-year interval defined above.

Although we have data on all episodes of employment and unemployment during 1991 and 2001 (the latter only in cases of UI benefit receipt), we only use those episodes for which we have corresponding establishment data. Therefore our analysis is based on the years 1996 to 2001. If a firm was not interviewed in one of the years from 1996 to 1998, information from the next year available was used instead. Alternatively, we could have dropped firms for which we do not have data in every year but this would have induced too much loss of data.

If an individual is employed with more than one employer at the same time, we only use the employment spell generating the highest income. Spells lasting only one day are dropped, too. We restrict data to persons aged 25 to 52 in order to drop short-term employment spells during school and university holidays, and to avoid confusion between job exit and early retirement. In addition, we exclude employees working less than 15 hours a week, apprentices and home workers. This means that spells with at least one sub-spell of part-time work below 15 hours, vocational training or home work are dropped. Spells in the agricultural sector are dropped due to its high rates of seasonal and temporary employment which mark out this sector from the rest of the economy. Miners and female master craftsmen are dropped due to their extremely small numbers. All spells with missing covariate information are also eliminated from the data. These requirements leave us with a sample of 563,934 individuals and 1,384,065 employment spells. Of these episodes, 616,722 are from female workers and 528,420 concern establishments in the new Länder of the East.

3.2 Kaplan-Meier estimations

With a non-parametric Kaplan-Meier estimation it is possible to estimate survival rates for the whole sample as well as for different groups of individuals and to

obtain a first impression of possible differences between individual- or firm-specific characteristics. The survivor function $S(t)$ is the probability of surviving up to time *t*. The formula for the estimated survivor function is the following:

$$\hat{S}(t) = \prod_{j|t_j \le t} \left(\frac{n_j - d_j}{n_j} \right), \tag{1}$$

where n_j is the number of persons employed at time t_j and d_j is the number of failures at time t_j. The product is over all observed failure times less than or equal to *t* (see, for instance, Cleves et al. 2002).

Figure 1 here

In figure 1 Kaplan-Meier-survival-curves are presented separately for men and women and West and East Germany. In West Germany, 50 per cent of male employees have left their employer after about 900 days. In East Germany, exit occurs faster: the median duration is about 650 days. The median duration for West German women is 1,100 days and for East German women 1,900 days. The longer median job durations of women are in contrast to results of recent studies for Germany (Bergemann und Mertens, 2002; Gerlach und Stephan, 2005). A reason may be that, according to our definitions, employment spells continue even in cases of taking a sabbatical or during parental leave, as well as after recall. This tremendously increases the tenure of women.

Whereas the curves of West Germans are relatively smooth, the East German curves show a fall after exactly one year. In the Eastern Länder more than 10 per cent of female and more than 5 per cent of male spells end precisely after one year. This is due to the higher incidence of temporary employment in job creation schemes in East as compared to West Germany.[4] This draws attention to the very different labour market conditions in East and West Germany.

4 Unfortunately we cannot identify those jobs in the data but in the year 2003 75 per cent of all persons in job creating programmes participating employees were employed in East Germany (Bundesagentur für Arbeit, 2004: 116f.).

Survival rates for employees with different education levels are presented in figure 2.[5] It is obvious that the higher the skill group the longer is tenure. Furthermore, one can observe a higher fall after one year for less skilled employees in East Germany. This can also be observed for low skilled employees in West Germany, which again supports the interpretation that these falls are due to subsidised employment in active labour market measures.

Figure 2 here

In figure 3 we look at the survival curves for different age groups. As one would expect, the survivor curves are monotonically ordered, with young employees having the lowest and the oldest age group the longest employment durations.

Figure 3 here

One of the main issues of this paper is whether heterogeneity of employment durations is due to heterogeneity within or across firms. With Kaplan-Meier estimates for different firm-sizes we can show differences in the unconditional survival rates between firms. In figure 4, Kaplan-Meier survival curves for West and East German employees are drawn separately for firm-size categories. Whereas in West Germany survival rates are higher for larger establishments, in East Germany firms with 20-99 and more than 1,000 employees have the highest survival rates. During the first year and again after about 700 days the probability of loosing the job is the highest in firms with 1-19 employees. In between, firms with 100-999 employees have very low survival rates because they show a high fall after one year.

Figure 4 here

There should be differences in the shape of survival rates concerning destination states if the decision to end an employment relationship depends on different circumstances. As we can see in figure 5, survival curves for employees moving to unemployment are more convex, especially in the first two years of employment as compared to job-to-job transitions. We can clearly see that the

5 In the following Kaplan-Meier-graphs we make a distinction between West and East Germany but not between the sexes.

drop in the survival rate after one year in East Germany only occurs if a person moves into unemployment. This could be due to one-year job creating programmes for employees who are unemployed before and after. The survival curve of movers into non-employment decreases in the first year and is almost horizontal later on. However, only a low proportion of observed employees move into non-employment. The differences in the shape of the survival curves for different destination states indicate that a competing risks model separating between exit states may be appropriate in multivariate estimation.

Figure 5 here

3.3 Heterogeneity of job durations within and across firms

To distinguish heterogeneity in job durations within and across firms empirically, we first estimate quantiles of the distribution function $f(t)$. The differences between quantiles are then interpreted as measures of heterogeneity in the distribution of employment durations. We compare the overall heterogeneity of durations in the sample to the within-firm and within-sector heterogeneity. The latter is obtained by estimating the quantiles separately and then taking a weighted average (weighted by the number of spells in the firm or sector available at the quantile date). Unlike the quantile differences for the whole sample, the differences of theses averages do not contain the between-firm heterogeneity. We restrict ourselves to estimating the differences between the 10 per cent quantile and the 50, 40 and 30 per cent quantiles, respectively.

To calculate the quantiles of the distribution function, we use the Kaplan-Meier estimator $\hat{S}(t)$ again, to take into account right-censoring properly. The main problem is that, due to the relatively small observation period, the quantiles may not be available for some sectors or firms. To overcome this problem, averages are calculated as follows:

$$\begin{aligned} \overline{r}_{\alpha,10} &= \overline{q}_{\alpha} - \overline{q}_{10}^{(\alpha)} \\ &= \sum_{j=1}^{m} \frac{n_{j\alpha}}{n_{\alpha}} \hat{q}_{j\alpha} - \sum_{j=1}^{m} \frac{n_{j10}^{(\alpha)}}{n_{10}^{(\alpha)}} \hat{q}_{j10}^{(\alpha)} \quad , \end{aligned} \tag{2}$$

where $j = 1,...,m$ denotes units (either firms or sectors) and the quantile difference $\overline{r}_{\alpha,10}$ is measured in days. Units with an α-quantile $\hat{q}_{j\alpha} = \{t \mid \hat{S}_j(t) = 1-\alpha\}$ outside the range of observations are excluded from the calculation of both $\overline{q}_\alpha$ and $\overline{q}_{10}^{(\alpha)}$. Hence, every $\overline{r}_{\alpha,10}$ is based on a different set of observations, denoted by (α). The average in equation (2) is weighted by the number of spells in the unit, where n_α is the number of spells with duration $t \geq \hat{q}_\alpha$ and n_j is the number of spells in unit j with duration $t \geq \hat{q}_{j\alpha}$.[6]

The average quantile differences $\overline{r}_{\alpha,10}$ are then compared to the quantile difference $r_{\alpha,10}$ obtained over the whole sample for the same set of observations. If the unit averages are shorter than the quantile differences for the whole sample, this indicates that the sample duration distribution is more dispersed than the unit-specific duration distribution, indicating that some of the heterogeneity arises due to the between-unit variation in job durations. Clearly, taking the average only over the units for which the quantiles are available may have an impact on the results, since individuals in these units have shorter job durations than the sample average. Therefore, interpretation of the results should not be extended to units with long durations. Another limitation is that due to the short observation period, the highest quantile is the 50 per cent quantile. Obviously, heterogeneity at the long end of the duration distribution may differ in its composition from heterogeneity at other durations.

Table 1 shows the sector and establishment averages of the quantile differences as well as the quantile differences estimated by a single Kaplan Meier survivor function over the same samples. It also contains the percentage reduction of the interquantile range induced by averaging over unit-specific quantiles. The numbers of sectors or establishments that reach the lower quantile are displayed in parentheses. For instance, the numbers in the first three rows and the first column imply that 786 days pass between the points of time when 10 and 50 per cent of all West German male workers employed in the nine sectors used for the analysis have left their job. By contrast, on average over the nine sectors, only 510 days pass between the 10th and the 50th percentile. Thus, the within-

6 The first number is obtained by performing a Kaplan-Meier estimation over the observations in all units with an α-quantile inside the observation period.

sector dispersion is 35 per cent lower than the overall dispersion in the sample and a considerable part of the heterogeneity seems to be due to sector effects.

Table 1 here

However, the other results in the table do not support the proposition that within-sector dispersion is vastly lower than the dispersion of job durations in the whole economy. A sizeable reduction in the quantile differences is only achieved if the 50 per cent quantile is considered while the reduction is lower for the other quantiles. In some instances, the dispersion even increases if the average over sectors is drawn.

If, in addition to sector heterogeneity, the distribution of tenure differs across firms, we should find more deviation for firms than for sectors. Indeed, up to the 40 per cent quantile dispersion is higher for the average over establishments. However, the reductions in dispersion are still relatively low, ranging between five and 28 per cent. In East Germany, the results suggest that there is no heterogeneity in female tenure across sectors but heterogeneity across establishments can to a certain extent be found.

To summarise, it appears that most of the heterogeneity in job durations is not due to individuals' employment in particular sectors or establishments. Rather, the overall dispersion of job durations is reproduced to a large extent in a similar dispersion at the sectoral and even at the firm level. Having said this, the reduction of dispersion achieved by accounting for establishment differences is not completely negligible. Therefore, observed or unobserved firm heterogeneity still needs to be accounted for in multivariate estimation.

4 Estimation technique and independent variables

4.1 Estimation technique

We estimate job durations by the semi-parametric Cox proportional hazard model (Cox 1978). This model is flexible because the baseline hazard is not estimated and therefore no assumptions about the shape of the hazard are imposed. Furthermore, the model allows stratified estimation which is important in order to take unobserved firm-specific heterogeneity into account. To estimate different covariates according to destination state, the Cox model can be extended to the independent competing risks model.

The hazard rate for individual i (i=1,…, n_j) employed in firm j (j=1,…,m) at time t, written as $\lambda_{ij}(t)$, is

$$\lambda_{ij}(t) = \lambda^0(t)\exp[z_{ij}(t)'\theta], \qquad \text{with } z_{ij}(t)'\theta = x_i(t)'\beta + w_j^1(t)'\gamma^1 + w_j^2{}'\gamma^2, \qquad (3)$$

where $x_i(t)$ are (time-varying as well as time-constant) individual-specific characteristics, $w_j^1(t)$ are firm-specific time-varying and w_j^2 are time-invariant firm-specific characteristics. The θ, β, γ^1 and γ^2 are the parameters to be estimated. The model is called a proportional hazard model because the baseline hazard $\lambda^0(t)$ is assumed to be shifted proportionately by the covariates. The independent variables are assumed to be exogenous with respect to the transition process. Abstracting from right-censored observations, the likelihood function of the unstratified model is

$$L_u(\theta) = \prod_{j=1}^{m}\prod_{i=1}^{n_j} \frac{\exp[z_{ij}(t)'\theta]}{\sum_{k=1}^{m}\sum_{l\in R_k(t)} \exp[z_{kl}(t)'\theta]}, \qquad (4)$$

where k is a further index for firms and l an index for individual spells. In the likelihood contributions, the product of which is taken in (4), $R_k(t)$ is the set of employment spells in establishment k being at risk at time t. In the following, it is referred to as the risk set in establishment k. Summing over establishments, we obtain all spells that are ongoing at time t. Thus, in the denominator we have the probability that any of the persons in the sample exits from employment at time t, while the numerator gives the probability that individual i (i. e. the individual actually making the transition) exits from firm j at time t. The regression constant and the baseline hazard $\lambda_0(t)$ cancel out from this formulation.

If censoring is independent, as we assume in the following, right-censored spells, i.e. non-ratification of conventions at the end of the observation period, can easily be accommodated by the partial likelihood technique by excluding them from the risk set at the time of censoring (see Lancaster, 1990: 250ff.).[7]

7 The case of dependent censoring arises if the likelihood of a later episode being censored depends on the length of the previous spell. See Wang and Wells (1998) for this case.

Although we include some firm characteristics in our model, firm-specific unobserved heterogeneity could also lead to a bias in estimation results. Hence, we account for firm-specific unobserved heterogeneity by stratifying the sample according to establishments in some of the estimations. The principle of the stratified partial likelihood estimator is to decompose the total likelihood into several sub-likelihoods specific for each stratum (Kalbfleisch and Prentice, 2002: 118f.; Lancaster, 1990: 268ff.; Ridder and Tunali, 1999). This means that a separate baseline hazard $\lambda_j^0(t)$ which depends on the firm-specific fixed effect μ_j is assumed for each establishment, whereas the estimated parameters are assumed to be the same over all establishments. The individual-specific hazard in the stratified model is

$$\lambda_{ij}(t) = \lambda_j{}^0(t)\exp[z_{ij}(t)'\theta] , \qquad \text{with } z_{ij}(t)'\theta = x_i(t)'\beta + w_j^1(t)'\gamma^1 . \tag{5}$$

In this case, the coefficients of time-varying firm-specific variables are identified while those of time-constant variables are not identified. For the stratified model, the partial likelihood is

$$L_S(\theta) = \prod_{j=1}^{m}\prod_{i=1}^{n_j}\frac{\exp[z_{ij}(t)'\theta]}{\sum_{l\in R_k(t)}\exp[z_l(t)'\theta]} . \tag{6}$$

In contrast to the unstratified model (4), there is an establishment-specific risk set which comprises all employment spells in the establishment that continue at duration *t*.

While we allow for unobserved firm-specific effects in (6), we do not include individual-specific effects in the current version of the paper. Handling individual fixed effects in a non-restrictive way is only feasible if there is more than one uncensored employment spell per person. This would require that information is available for both employment spells, so that both spells are from employers in the establishment panel. Given the relative size of the Establishment Panel to the whole economy and, in particular, the relatively small time span (see above), this would result in a highly selective sample. Assuming orthogonality between firm and person effects and between covariates and person effects, one could include person-specific effects as random effects by estimating

a frailty model as described by, for instance, Ridder (1989). However, the assumption of independence may be dubious. Moreover, computational limits render this solution infeasible. Hence, we present estimations without person effects in the following. This implies relatively stringent assumptions on the correlation of person-specific and firm-specific unobservables with the covariates in order to obtain consistent estimates (see Abowd et al., 1999). A consistent estimator requires that the unobserved person-specific effects are uncorrelated both with the covariates and with the firm-specific effects. This implies that companies with high tenure do not attract workers who desire long-term contracts.

The determinants of job durations are likely to differ according to destination state. For instance, as individuals reach higher ages, they may leave the labour force with higher probability but are less likely to make a transition to a new job. Therefore, we distinguish between exit states in some of the estimations. Again in order to keep things simple, we choose independent competing risks. This means that the destination-specific durations are distributed independently (see Lancaster, 1990: 99ff.; Kalbfleisch and Prentice, 2002: 247ff.). Under this assumption, we can write the destination-specific likelihood function as

$$L_U^d(\theta) = \prod_{j=1}^{m} \prod_{i=1}^{n_j} \frac{\exp[x_{ij}(t)'\theta^d]}{\sum_{k=1}^{m} \sum_{l \in R_k(t)} \exp[x_l(t)'\theta^d]}. \tag{7}$$

Here, all spells are included in the risk set for destination *d* at duration *t* unless they are observed to end with a transition to *d* at a date earlier than *t*, are censored before *t* or end with a transition to another state. A problem is that the coefficients from the competing risks model cannot be interpreted as the effects of the independent variables on the probability of exiting to the destination state in question (Thomas, 1996). Instead, the coefficients must be interpreted as the magnitude of the influences *relative* to staying in the initial state.[8]

8 One could also calculate marginal effects as proposed by Arntz (2005). However, in our case this would be computationally too burdensome.

4.2 Independent variables

Concerning the inclusion of covariates, we consider the following variants of the model:

- inclusion of individual-specific factors only,
- addition of firm-specific to individual-specific variables,
- addition of firm fixed-effects (time-constant firm-specific variables are not identified in this model and are, therefore, excluded),
- an independent competing risks version of the model with firm-specific and individual-specific variables.

In table A1 in the appendix, the number of observations and failures as well as means and standard deviations of all covariates used in the estimations are listed. There are some structural differences between West and East Germany concerning age and education at the individual level and firm-size at the firm-level. The rich pool of person- and firm-specific variables opens great possibilities to estimate the determinants of employment duration. Whereas some covariates only serve as control variables, there are some that are of interest from an economic point of view.

Concerning individual characteristics first, we include sex, age, education, occupation, profession, nationality and previous employment status. With the impact of age we can test whether young people move more frequently than older individuals. This is suggested by the job-shopping theory (Johnson, 1978; Viscusi, 1980), according to which younger workers acquire information while searching for a better match. For the multivariate analyses we generated age intervals to take non-linear effects into account.

We expect employees with low education who are in low job positions to be employed in unstable jobs. On the other hand, high-skilled employees are expected to be more mobile. Women and foreign nationals have been found to move with higher intensity than German men in other studies (e. g. Mumford and Smith, 2004). Employment history may also be an important determinant of job durations. Persons who often change their jobs and are unemployed or out of the labour force in between seem to be in a segment where jobs are very unstable. We only condition on a small part of individual's employment history, namely the employment state before the current spell. But we expect a high impact of

these variables on employment duration. Finally, six broadly defined occupational categories are included in the specification.[9]

Controlling for firm-specific heterogeneity ensures that estimates for the individual-level variables are not biased due to selection into firms with long employment durations and firms with short employment durations. Firm size is part of this context. We expect tenure to be higher in larger establishments because their employees are more flexible to move to another workplace within the establishment. Therefore, employment can be adjusted within an internal labour market. Codetermination should lead to longer employment durations due to the fact that one of a council's legal competences concerns dismissal procedures. Moreover, the works council could decrease the number of quits if, due to a collective voice function, workers are more satisfied with their jobs in companies in which a works council is established. Works councils may also influence the number of workers in temporary employment (Boockmann and Hagen, 2003). We have very detailed information about collective bargaining and, therefore, are able to discuss different interesting influences. Sector level collective bargaining traditionally is more wide-spread in West Germany where bargaining takes place at the firm level with a higher rate in East Germany (see table A1 in the appendix). This is due to the fact that many firms have left the employers' associations in the East during the 1990s. For establishments bargaining on the firm level one would expect longer tenure because unions are interested in stable jobs for their members as well as in wage increases. Bargaining at the firm level may make it easier to react to negative shocks by reducing wages instead of making workers redundant.

Due to the tight economic situation the share of firms paying more than the collectively agreed wages is lower in the new Länder. Gerlach and Stephan (2005) expect collective contracts to have a positive impact on job tenure because higher wages are an incentive for employees to stay with the firm. The firm anticipates this and invests more in firm-specific training which again leads to more job stability.[10] On the other hand, a firm with collective agreements cannot

9 On the basis of the International Standard Classification of Occupations (ISCO 88) provided by the ILO, all 369 occupations contained in the Employment Register have been accumulated so that 6 occupational groups remained.

10 From other studies, there is robust evidence that wages rise with tenure. If these tenure increases are in the form of payments above collectively agreed levels, an endogeneity problem may arise here (Abowd and Kang, 2002). Therefore, coefficients of this variable should be interpreted with care.

compensate negative shocks by adjusting wages and therefore has to adjust employment (Caballero and Hammour, 1994). Therefore, one would suspect that tenure is shorter in firms with sector-based collective bargaining at least if there is a negative shock. However, a firm paying wages above the collectively set level has some leeway of reducing them in bad times instead of reducing employment.

We expect older establishments to have longer job tenure because they have better established market positions and may have more experience in hiring adequate workers. The variable concerning further training is motivated by human capital theory. If a firm invests in further training for its employees, it has a crucial interest not to lose this human capital and to reduce quits. Therefore, we expect that further training has a positive impact on tenure. Something similar are investments in ICT because they often lead to the necessity of special training. On the other hand this can lead to higher turnover rates because older employees are not able or do not want to operate with new technologies and new employees, who even are experienced with it, are hired.

As mentioned above, some establishment information was extrapolated if in one year there was no interview. Most of the firm characteristics we use do not change over time, so this problem does not appear serious. We define 12 sector dummies and 9 dummies for intervals of the firm size distribution. As an indicator for the local labour market, we use a one year lag of the local unemployment rate because the unemployment rate at the time of the hazard could be endogenous.[11]

5 Empirical Results

The presentation of the results is organised according to groups of coefficients. Results are presented for men and women in East and West Germany in tables 2 to 5. The first three columns of each table differ in the degree to which firm-specific characteristics are taken into account. First of all, we discuss estimates of coefficients of individual-specific variables and their robustness according to the consideration of firm-specific effects (subsection 5.1). In subsection 5.2 we discuss the influence of firm characteristics. The competing risks model, fourth

11 Strictly speaking, we use the residuals of the time trend over the observation period to eliminate time effects.

and fifth columns in tables 2 to 5, is dealt with separately in subsection 5.3. All estimations contain industry and regional dummies which, however, are neither shown nor interpreted. All tables display hazard ratios in order to facilitate the quantitative interpretation of the covariate effects.

5.1 Coefficient estimates for individual-specific characteristics

An important influence of job exit is age. As expected, hazard ratios decline monotonically and significantly with age (except for East German men). Effects are also quantitatively large. For instance, the job exit hazard for a West German male worker aged 45 to 52 is 40 per cent lower than the hazard rate of a man aged 25 to 29. However, age effects are markedly less pronounced for women, for East German women in particular, and they are virtually non-existent for East German men. These findings – that are similar to other studies (Bellman et al., 1997; Gerlach and Stephan, 2005; Wolff, 2004) – could point to the importance of career interruptions and subsequent job shopping even at older ages after German reunification. They could, however, also reflect a higher risk of unemployment at older ages in East Germany. In general, coefficients are influenced only little by the inclusion of firm variables or firm fixed effects.

The results show that both vocational training and a university degree tend to reduce the job exit hazard as compared to the reference group (persons without vocational or professional training). This suggests that both forms of education facilitate the acquisition of firm-specific human capital which, in turn, retards job changes. This is in contrast to Gerlach and Stephan (2005) and Mumford and Smith (2004) who find lower job durations for high skilled than for low skilled workers. This could be due to the fact that our observation period covers six years whereas in these studies effects result from the influence of very long job durations. However, only among West German men does a university degree consistently lead to the highest job stability. Among East German women, in particular, job durations are longest among workers with vocational training. Throughout all population groups, workers with A-levels (Abitur) and vocational training but without a university degree do not have higher job stability than the baseline category of low-skilled workers. Overall, the effects of education are quite robust to the inclusion of firm variables.

Concerning job position, the reference group is unskilled blue-collar workers. Skilled blue-collar workers have significantly lower job exit rates in most population groups, the exception being West German women. This may be

explained by the fact that the number of unskilled blue-collar female workers is relatively low. In all four groups, white-collar workers have significantly lower hazard rates. The order of magnitude is high and exceeds the influence of the educational groups. However, the effect tends to weaken as firm-specific covariates are accounted for, pointing to the fact that particular workers are employed by firms with particular characteristics. Master craftsmen (due to the small number of occurrences for women, the coefficient is estimated only for men) have similar exit rates as white-collar workers. Part-time work increases job durations, but only for women, and in West Germany more so than in the East of the country. Whereas our findings are consistent with the results concerning job position of Gerlach and Stephan (2005) and Mumford and Smith (2004) the latter find negative effects for part-time workers estimating a model jointly for men and women.

Production workers do not uniformly have higher or lower exit rates than service workers (the baseline group). Moreover, the inclusion of firm-specific variables often changes the results with respect to this variable. A similar conclusion holds for nationality. Although it appears that citizens from other EU countries have lower and other foreigners higher job exit rates, hardly any significant effects remain in stratified estimation. This agrees with Mumford and Smith (2004) who conclude that individuals with certain characteristics, for instance the racial background, are sorted in establishments with low job durations.

Previous employment status seems to matter a lot for job stability. Individuals who started their job out of unemployment have a significantly higher job exit rate than persons who moved from one job to another. This is true, in particular, for women and East German workers. In these groups, the magnitude of the effect ranges from 50 to 85 per cent. It seems that these persons are selected into unstable jobs which can be a signal for state dependence. Employees who return to their employers after a period of non-employment have, by far, the longest employment durations. For women, parental leave can be an explanation, but not for men. Clearly, this result deserves further investigation in future research.

The local labour market seems to have an ambiguous impact on tenure. For West Germany the effect is not significant. In East Germany, the impact of the local unemployment rate (lagged by one year) is positive. This supports the hypothesis of Caballero and Hammour (1994) that tenure increases with negative

shocks because of a lower quit rate (also see Mumford and Smith, 2004). There is also indication of calendar time effects: with time, job stability seems to be somewhat decreasing.

Comparing the results with and without firm-level covariates or firm fixed effects, we find that accounting for these effects is important for the impact of a number of individual-level variables on tenure. In particular, coefficients for variables relating to the profession and nationality of the individual are highly sensitive to the inclusion or otherwise of firm-level information. Other variables, such as age and education, however, have a more robust influence on job durations.

5.2 Coefficient estimates for firm-specific characteristics

Apart from their effect on worker-level covariates, the influence of establishment characteristics is also interesting in its own right. Only some of them are included in stratified estimation, since only coefficients of time-varying variables are identified in this model. Among these variables, the influence of investment into ICT comes out consistently strong for male workers, but much less so for female employees. Lower job exit rates among men suggest a stronger complementarity between male than between female workers' firm-specific skills and establishment technology.

There are mixed results on the effects of collective agreements on job stability. Only for East German women, firms' adherence to collective agreements slows down job exit according to all coefficient estimates. The patchy evidence may be due to the fact that the inclusion of job security provision into collective agreements is far from uniform in German industries. Male workers are affected negatively in their job exit behaviour if the firm pays in excess of collectively agreed rates, as one would expect. However, the same is not true for women. These results are similar in stratified and unstratified estimation. The impact of collective contracts on tenure is the main issue in Gerlach and Stephan (2005). They find that workers in Lower Saxony have significantly higher job durations in firms with collective contracts. Moreover, male workers in establishments with firm level contracts exhibit the highest job stability. However they cannot control for the presence of a works council, which can lead to omitted-variable biases.

Our results provide some indication that further training prolongs tenure: according to the unstratified estimations, the hazard rate is reduced by about ten

per cent in establishments that offer training. However, this result is not obtained in stratified estimation, where inference is restricted to companies introducing or cancelling firm-sponsored training. Unfortunately, we have no information on individual training participation in the data. By contrast this is available for Mumford and Smith (2004) who find higher job exit rates in case of employer-provided training.

Among time-constant firm-level variables, the presence of a works council leads to significantly longer employment durations, a finding that is consistent with the large literature on works councils in Germany (Addison et al., 2001). The implied decrease in the hazard ranges from 15 to 25 per cent. The legal form of the establishment is also of primary importance. In particular, public corporations and other legal forms like associations have far lower job exit rates than enterprises under private proprietorship. This indicates differences in personnel policy according to the necessity of competitiveness. The effect is more pronounced in West Germany than in the East.

Interestingly, our results show that we cannot support the hypothesis "the larger the firm, the more stable jobs are". Only in West Germany, there appears to be a pattern such that firms below 100 employees have higher and large firms with more than 200 employees have lower job exit rates than the baseline category (firms with 100 to 200 employees). But even in this case, the clear ordering that appears in the Kaplan-Meier-graphs for West Germany (see figure 4) seems to be captured by other variables, such as legal ownership form or works council presence. This agrees with Bender et al. (1997) who find no significant firm-size effects apart from men who work in firms with less than 100 employees and change into unemployment exhibiting higher job exit rates. Moreover, Mumford and Smith (2004) cannot find significant effects of firm-size on the average tenure of a workplace in Great Britain. For East Germany even the Kaplan-Meier-curves are not as clear as one would assume. Estimation results for the new Länder exhibit the tendency that smaller firms with more than 20 employees have longer job durations than middle-sized companies in the reference group, especially for female workers. But results differ between sexes for firms with more than 200 employees.[12] Overall, we cannot find robust evidence for internal labour markets providing more job stability within a firm.

12 Quantile regressions could give more detailed information about the coherence of firm-size and job duration and should be part of further research.

The age of the establishment is insignificant among West German men whereas new establishments have the lowest hazard rates among all other groups.[13] This finding appears surprising at first sight but it needs to be recalled that the data comprises only companies that existed throughout the whole observation period. Therefore, job exits from companies that went bust are not contained in the data. In East Germany, firms founded before 1980 have positive effects of more than 20 per cent. This may reflect the fact that many of these establishments are state-owned.

5.3 Competing Risks

So far, we have assumed that the mechanism driving job exit is the same across all destination states. However, it is quite plausible that the independent variables influence exit into different destination states differently. For instance, highly skilled persons are not as likely to become unemployed as low-skilled workers, but due to their better chances on the external labour market, they can be expected to move more frequently from one job to another. With a competing risks model, we are able to separate the two effects. Using the definitions given in section 2, we distinguish between three destination states: moving to another employer, unemployment, and non-employment. We display only results for the first two destination states. They are contained in the fourth and fifth column of each table. These coefficients can be compared directly with the second column where the same set of covariates is used.

Table 6: LR Tests of competing risks versus single exit state

	Men		Women	
	West	East	West	East
∑ log likelihood of unrestricted model	-1,092,909	-512,068	-552,322	-404,280
Log likelihood of restricted model	-1,107,531	-517,940	-561,166	-409,707
Likelihood ratio test statistic Chi²	29,245	11,742	17,689	10,855
(degrees of freedom)	(68)	(63)	(67)	(62)

13 According to the structure of our data all firms should be established before 1996. Nevertheless some firms reported 1996 or later as "year of setting up" which can be due to changes in the ownership.

In table 6, we provide likelihood ratio tests of the null hypothesis of a single exit state versus the competing risks model. In all population groups, the single exit state model is clearly rejected in favour of the multiple-state model.

The impact of age differs a lot between the destination states as well as between employee groups. Whereas there are no significant effects for West Germans if they move to unemployment, the hazard ratios decline with age-intervals for East German women and increase for East German men. The hypothesis "the older the employees the lower the job exit hazard" holds only for all employee groups in the case of job-to-job transitions. By contrast, the risk of becoming unemployed is by and large independent of age. Our findings are supported by Wolff (2004) who gets similar effects. These results are an indicator that both theories job-shopping as well as loss of human capital during unemployment periods cannot be rejected.

The influence on tenure of the labour market state before the job was taken on depends strongly on the destination state. Obviously there is a strong relation between the original and the destination state and some employment histories seem to be persistent. Employees who came from unemployment have a high probability to return to unemployment after relatively short time. Again Wolff (2004) yields the same results but Bender et al. (1997) cannot find a significant impact of previous unemployment. The impact of employment history on job-to-job transitions is less clear. Whereas employees with recall have significantly lower hazard rates, coming from another employer leads to significantly higher hazard rates for West German women and significantly lower hazard rates for East German women. The coefficients for men are insignificant. Conditioning on other covariates such as age and qualifications, there appears to be no distinct group of job shoppers who frequently move from one employer to another. This is in contrast to Bender et al. (1997) who find higher hazard rates for men who move from job-to-job.

Better education and better job position reduce the risk of unemployment but do not retard job-to-job changes. In particular, employees with university degree generally have the lowest exit rates into unemployment. A striking difference is that in East Germany, more educated workers move more frequently from one job to another, while this is not true for West German workers. Referring to unskilled blue-collar workers, skilled blue- and white-collar workers have longer job durations if they move into unemployment afterwards. The selection of low-skilled individuals in partial labour markets with low job

stability is the reason for this result. The lagged unemployment rate increases the probability of exit into unemployment in the West but not in the East, while it makes job-to-job changes less likely among East German women.

Among the firm-level independent variables, the impact of firm-size is again found to be less ambiguous in West Germany. Here, being employed in a large firm clearly protects against unemployment. By contrast, there is little evidence that firm size has an impact on job-to-job changes. The works council effect has the same magnitude for both destination states. Hence, there is support both for the "voice" function of works councils preventing exits of dissatisfied workers from their employers as well as for the effect of works councils on employment protection. For West German men, both firm-sponsored training and the use of ICT decrease the job change probability. These effects are less clear for other population groups.

6 Conclusions

In this paper, we use a new linked employer-employee dataset to analyse the individual and firm-specific determinants of job durations in Germany. Due to the flow sampling scheme and the relative small time dimension of the data, we restrict the analysis to job durations of six years maximum. Significant differences between men and women and East and West Germany prove the necessity for performing separate analyses. As opposed to other studies, we find median durations to be longer for women than for men which may be due to the exclusion of young workers below the age of 25 as well as to our treatment of parental leave. It may also be due to other definitions such as recalls and unemployment, the effects of which are still unexplored given that the data have become available only recently. Due to a high rate of participants in job creation programmes in East Germany and possibly also due to unsubsidised temporary work, many employment contracts end precisely after one year.

A special focus of the paper is on the question whether there is heterogeneity in tenure between or across firms. Isolating within-firm dispersion in tenure durations from between-firm heterogeneity, we find that dispersion within firms is only moderately lower than dispersion over the whole sample: the interquantile range is reduced by only about 20 per cent by eliminating the between-firm dimension. This is consistent with the view that there is some segmentation into long and short employment spells at the firm level.

Coefficients of individual characteristics point to the presence of partial labour markets. Low-skilled employees in low job positions have significantly shorter job durations. In addition, effects of employment history call attention to the fact that these groups are affected by persistence of unemployment or non-employment. Although job shopping seems to be popular across younger workers, there is no strong evidence that high qualified employees are more mobile than others. A competing risks framework shows that mobility to another job and exit to unemployment follow strikingly different processes.

Among firm characteristics, institutional variables such as the firm's legal structure, the presence of a works council and, to a lesser degree, the adherence to collective agreements, matter for job durations. Works councils retard job exit both into unemployment and into another job, which gives support both to the "voice" function of worker participation as well as to its effect on employment protection. Contrary to the descriptive evidence but in accordance with other studies, firm size is of secondary importance.

Further research should relax some of the assumptions that were made in estimation. Most importantly, one should relax the assumption on the correlation of individual and firm-level heterogeneity. In accordance with Abowd and Kramarz (1999), person-level fixed effects could be included if estimation was restricted to individuals having more than one spell in the dataset. Is there a sorting process of long-tenure workers into long-tenure firms, similar to the sorting of high-wage workers into high-wage jobs? Since the stratified estimator can be used to control for firm-level fixed effects without having to estimate a large number of nuisance parameters, this can be accomplished relatively easily. However, estimating job exits on the movers only comes at the cost of having a selected sample. Moreover, no inference can be made as to the distribution of exit probabilities within firms.

A second issue is that covariates may affect exit probabilities differently at different durations. In this way, one could isolate the factors that to firm-level segmentation. For instance, do firms with a works council have more dispersion than firms without a works council, because insiders use shop-level participation to increase their own job stability at the expense of newly hired insiders? This issue seems highly relevant in view of the evidence for substantial within-firm heterogeneity in job durations found in this study.

References

Abowd, J.M.; C. Kang (2002), Simultaneous determination of wage rates and tenure, Manuscript.

Abowd, J.M.; F. Kramarz (1999), The analysis of labor markets using matched employer-employee data, in: Ashenfelter, O.; D. Card (eds.), *Handbook of Labor Economics*, Vol. 3, 2629-2710.

Abowd, J.M.; F. Kramarz,; D.N. Margolis (1999), High Wage Workers and High Wage Firms, *Econometrica* 67(2), 251-333.

Abaraham, K.G. (1988), Flexible staffing arrangements and employers' short-term adjustment strategies, in: Hart, R.A. (ed.), Employment, unemployment and hours of work, London, 288-311.

Addison, J.T.; C. Schnabel; J. Wagner (2001)**,** Works councils in Germany: Their effects on establishment performance, *Oxford Economic Papers* 53(4); 659-694.

Alda, H. (2004a), IAB-Datenreport Nr. 2: Die Verknüpfungsqualität der LIAB-Daten.

Alda, H. (2004b), IAB-Datenreport Nr. 4: Datenbeschreibung der Version 1 des LIAB-Längsschnittmodells.

Alda, H.; S. Bender; H. Gartner (2005), The linked employer-employee dataset of the IAB (LIAB), IAB Discussion Paper No. 6/2005.

Arntz, M. (2005), The geographical mobility of unemployed workers, ZEW Discussion Paper No. 05-34, Mannheim.

Bellmann, L. (2002), Das IAB-Betriebspanel - Konzeption und Anwendungsbereiche, *Allgemeines statistisches Archiv* 86(2), 177-188.

Bellmann, L.; S. Bender; U. Hornsteiner (2000), Job tenure of two cohorts of young German men 1979-1990: An analysis of the (West-)German Employment Statistic Register Sample concerning multivariate failure times and unobserved heterogeneity, IZA Discussion Paper No. 106.

Bellmann, L.; S. Bender; A. Kölling (2002), Der Linked Employer-Employee-Datensatz aus IAB-Betriebspanel und Beschäftigtenstatistik der

Bundesanstalt für Arbeit (LIAB), in: G. Kleinhenz (Hrsg.), Beiträge zur Arbeitsmarkt- und Berufsforschung Nr. 250, 21-29.

Bender, S.; A. Haas (2002), Die IAB-Beschäftigtenstichprobe, in: G. Kleinhenz (Hrsg.), Beiträge zur Arbeitsmarkt- und Berufsforschung Nr. 250 , 3-12,.

Bender, S.; D. Konietzka; P. Sopp (2000), Diskontinuität im Erwerbsverlauf und betrieblicher Kontext, *Kölner Zeitschrift für Soziologie und Sozialpsychologie* 52(3), 475-499.

Bergemann, A.; A. Mertens (2002), Job stability trends, layoffs and quits – An empirical analysis for West Germany, Manuscript.

Boockmann, B.; T. Hagen (2003), Works councils and fixed-term employment: Evidence from West German establishments, *Schmollers Jahrbuch* 123 (3), 359-381.

Bronars, S.G.; M. Famulari (1997), Wage, tenure, and wage growth variation within and across establishments, *Journal of Labor Economics* 15(2), 285-317.

Bundesagentur für Arbeit (2004), Arbeitsstatistik 2003 - Jahreszahlen, Sondernummer der Amtlichen Nachrichten der Bundesagentur für Arbeit 52, Nürnberg.

Caballero, R.J.; M.L. Hammour (1994), The cleansing effect of recessions, *American Economic Review* 84(5), 1350-1368.

Cleves, M.A.; W.W. Gould; R.G. Gutierrez (2002), An introduction to survival analysis using Stata, Stata Press, College Station, Texas.

Cox, D.R. (1972), Regression models and life-tables, *Journal of the Royal Statistical Society* 34, 187-202.

Dohmen, T.J.; G.A. Pfann (2003), Worker separations in a nonstationary corporate environment, *European Economic Review* 48, 645-663.

Fitzenberger, B.; R. Wilke (2004), Unemployment durations in West-Germany before and after the reform of the Unemployment Compensation System during the 1980s, ZEW Discussion Paper No. 04-24, Mannheim.

Fitzenberger, B.; A. Osikominu; R. Völter (2005), Imputation rules to improve the education variable in the IAB Employment Subsample, ZEW Discussion Paper No. 05-10, Mannheim

Gerlach, K.; G. Stephan (2005), Individual tenure and collective contracts, IAB Discussion Paper No. 10/2005.

Johnson, W.R. (1978), A theory of job shopping, *Quarterly Journal of Economics* 92, 261-277.

Kalbfleisch, J.D.; R.L. Prentice (2002), The statistical analysis of failure time data, John Wiley & Sons, Hoboken, New Jersey.

Kölling, A. (2000), The IAB-Establishment Panel, *Schmollers Jahrbuch* 120(2), 291-300.

Lancaster, T. (1990), The econometric analysis of transition data, Cambridge Univ. Press.

Mumford, K.; P.N. Smith (2004), Job tenure in Britain: Employee characteristics versus workplace effects, *Economica* 71, 275-298.

Ridder, G. (1989), Attrition in multi-wave panel data, Memorandum from Institute of Economic Research, Faculty of Economics, University of Groningen.

Ridder, G.; I. Tunali (1999), Stratified partial likelihood estimation, *Journal of Econometrics* 92(2), 193-232.

Saint-Paul, G. (1996), Dual labour markets: A macroeconomic perspective, MIT Press.

Thomas, J. (1996), On the interpretation of covariate estimates in independent competing-risks model, *Bulletin of Economic Research* 48(1), 27-39.

Viscusi, W. (1980), A theory of job shopping: A Bayesian perspective, *Quarterly Journal of Economics* 94, 609-614.

Wang, W.; M.T. Wells (1998), Nonparametric estimation of successive duration times under dependent censoring, *Biometrika* 85, 561-572.

Wilke, R. (2003), Semiparametric estimation of regression functions under shape invariance restrictions, ZEW Discussion Paper No. 03-64, Mannheim

Wolff, J. (2004), The duration of new job matches in East and West Germany, University of Munich, Department of Economics, Discussion Paper No. 360.

Table 1: Estimated interquantile ranges (in days)

Men	West			East		
	$\alpha = 50$	$\alpha = 60$	$\alpha = 70$	$\alpha = 50$	$\alpha = 60$	$\alpha = 70$
Total sample ($r_{\alpha, 90}$)	786	422	216	471	297	198
Average over sectors	510	390	250	372	296	196
($\overline{r}_{\alpha,90}$)	(9)	(10)	(11)	(9)	(10)	(11)
Per cent reduction	-0.35	-0.08	+0.16	-0.21	0.00	-0.01
Total sample ($r_{\alpha, 90}$)	406	281	170	306	224	150
Average over	315	218	172	248	212	147
establishments ($\overline{r}_{\alpha,90}$)	(903)	(1105)	(1311)	(880)	(1007)	(1156)
Per cent reduction	-0.22	-0.22	+0.01	-0.19	-0.05	-0.02

Women	West			East		
	$\alpha = 50$	$\alpha = 60$	$\alpha = 70$	$\alpha = 50$	$\alpha = 60$	$\alpha = 70$
Total sample	952	533	277	595	247	246
Average over sectors	621	462	316	617	334	238
	(9)	(10)	(11)	(10)	(10)	(10)
Per cent reduction	-0.35	-0.13	+0.14	+0.04	+0.35	-0.03
Total sample	434	305	207	273	273	214
Average over	377	257	178	193	196	171
establishments	(743)	(937)	(1129)	(747)	(864)	(1018)
Per cent reduction	-0.13	-0.16	-0.14	-0.29	-0.28	-0.20

Note: Number of sectors and establishments included in the estimation are given in parentheses. The total number of establishments is 1777 for West German men, 1554 for East German men, 1714 for West German women and 1536 for East German women. The number of sectors is 12.

Table 2: Results from Cox estimation, West Germany, male workers

	only X's	X's & Z's	Stratified	Unemployed	Job-to-job
Age in years (reference group: 25-29)					
30-34	0.859	0.856	0.838	0.896	0.884
	(-3.58)	(-3.48)	(-4.64)	(-1.44)	(-2.00)
35-39	0.792	0.806	0.828	0.956	0.773
	(-5.20)	(-4.61)	(-4.71)	(-0.57)	(-4.06)
40-44	0.780	0.776	0.754	1.080	0.676
	(-4.92)	(-4.94)	(-6.25)	(0.87)	(-5.45)
45-52	0.604	0.611	0.627	0.916	0.465
	(-9.73)	(-8.85)	(-9.95)	(-1.05)	(-9.89)
Education (reference group: no professional training)					
Vocational	0.905	0.922	0.863	1.016	0.878
training	(-2.25)	(-1.77)	(-3.74)	(0.23)	(-1.88)
Voc. training &	0.878	0.960	0.934	0.852	0.885
A-level	(-1.62)	(-0.46)	(-0.96)	(-0.91)	(-1.14)
University	0.678	0.752	0.800	0.671	0.876
	(-6.06)	(-4.07)	(-3.69)	(-3.14)	(-1.36)
Job position (reference group: unskilled blue-collar)					
Skilled blue-	0.937	0.848	0.739	0.725	1.042
collar	(-1.51)	(-3.62)	(-6.68)	(-4.47)	(0.58)
White-collar	0.658	0.700	0.743	0.542	0.990
	(-7.38)	(-5.77)	(-4.81)	(-5.47)	(-0.11)
Master	0.767	0.706	0.789	0.449	1.228
craftsman	(-2.27)	(-2.96)	(-1.79)	(-3.61)	(1.17)
Part-time worker	1.129	1.195	1.104	1.040	1.158
	(1.51)	(2.04)	(1.32)	(0.29)	(1.10)
Occupation (reference group: service workers)					
Production	0.890	1.056	1.058	1.185	1.042
workers	(-2.95)	(1.03)	(1.02)	(2.12)	(0.54)
Technicians	0.858	0.961	0.958	1.290	0.813
	(-2.76)	(-0.64)	(-0.74)	(2.01)	(-2.75)
Others	0.587	0.553	2.096	0.419	0.371
	(-1.20)	(-1.04)	(4.32)	(-1.64)	(-1.79)
Nationality (reference group: German)					
EU citizen	0.907	0.907	0.846	0.548	1.110
	(-0.88)	(-0.84)	(-1.54)	(-2.87)	(0.70)
No EU citizen	1.305	1.265	1.072	1.429	0.998
	(5.02)	(4.52)	(1.44)	(4.28)	(-0.03)
Previous employment state (reference group: no observation)					
Unemployment	1.341	1.326	1.155	3.478	0.923
	(4.86)	(4.28)	(2.63)	(10.15)	(-0.85)

Table 2 (continued)

Non-employment	1.419	1.400	1.234	1.261	1.368
	(4.85)	(4.36)	(3.06)	(1.32)	(2.80)
Recall	0.517	0.597	0.595	0.803	0.466
	(-7.68)	(-6.12)	(-7.30)	(-1.45)	(-6.38)
Job-to-job change	0.769	0.855	0.861	1.157	1.059
	(-4.46)	(-2.51)	(-2.85)	(1.16)	(0.65)
Calendar time (reference group: 2001)					
1996	1.099	1.021	1.042	0.985	0.771
	(1.51)	(0.33)	(0.55)	(-0.16)	(-2.59)
1997	1.197	1.086	1.203	1.029	0.785
	(3.24)	(1.43)	(2.02)	(0.30)	(-2.91)
1998	1.131	1.019	1.137	0.820	0.930
	(2.27)	(0.33)	(1.10)	(-2.08)	(-0.96)
1999	1.161	1.072	1.208	0.896	1.061
	(3.03)	(1.40)	(2.06)	(-1.31)	(0.90)
2000	1.341	1.276	1.256	0.934	1.375
	(6.22)	(5.14)	(3.41)	(-0.77)	(5.53)
Local labour market					
Unemployment rate	1.002	1.020	0.994	1.069	0.988
	(0.27)	(2.36)	(-0.16)	(4.66)	(-0.99)
Investments in (reference group: no investment)					
ICT		0.870	0.847	0.912	0.815
		(-3.90)	(-3.78)	(-1.53)	(-4.09)
Further training		0.916	0.937	0.968	0.835
		(-2.11)	(-0.93)	(-0.50)	(-3.02)
Collective agreements (reference group: no collective agreement)					
Sector-wide		0.928	1.114	0.881	0.993
		(-1.63)	(1.42)	(-1.72)	(-0.10)
Firm-level		1.007	1.286	1.109	0.873
		(0.10)	(2.30)	(0.99)	(-1.49)
Wages > tariff		0.931	0.893	0.940	0.954
		(-2.05)	(-2.02)	(-1.05)	(-0.96)
Works council		0.788		0.812	0.802
		(-6.72)		(-3.53)	(-4.47)
Legal form (reference group: individual firm)					
Partnership		0.943		0.861	1.057
		(-0.77)		(-1.19)	(0.47)
Private limited company		1.089		1.041	1.193
		(1.37)		(0.42)	(1.75)
Public limited company		1.078		0.956	1.148
		(1.04)		(-0.39)	(1.26)

Table 2 (continued)

Public		0.673		0.623	0.768
Corporation		(-3.81)		(-2.77)	(-1.95)
Other (e.g.		0.708		0.753	0.811
association)		(-3.49)		(-1.95)	(-1.40)
Firm size (reference group: 100-199 employees)					
≤ 4		1.180		1.706	0.894
		(1.16)		(2.66)	(-0.45)
5-19		1.123		1.449	0.945
		(2.07)		(4.28)	(-0.70)
20-49		1.076		1.190	1.050
		(1.75)		(2.66)	(0.82)
50-99		1.208		1.222	1.197
		(5.35)		(3.38)	(3.73)
200-299		0.984		0.962	1.024
		(-0.52)		(-0.76)	(0.52)
300-499		1.018		0.983	1.004
		(0.58)		(-0.34)	(0.09)
500-999		1.052		1.053	1.085
		(1.62)		(1.00)	(1.87)
≥ 1000		0.885		0.804	0.859
		(-3.17)		(-3.38)	(-2.91)
Year of setting up (reference group: 1981-1990)					
≤ 1980		0.934		1.091	0.817
		(-1.28)		(1.01)	(-2.79)
1991-1995		0.922		0.966	0.890
		(-1.33)		(-0.34)	(-1.42)
≥ 1996		1.020		0.909	1.052
		(0.21)		(-0.64)	(0.40)
Wald Chi²	1,282.47	3,487.60	705.16	3,777.32	1,720.82
Log likelihood	-1,112,796.6	-1,107,531.2	-472,819.21	-431,834.14	-496,835.69
# Subjects			200,279		
# Obs.			529,293		
# Failures		63,701		21,282	31,496

Note: Results are shown in hazard ratios; t-values are in parentheses below. Additional covariates are countries and sectors.

Table 3: Results from Cox estimation, East Germany, male workers

	only X's	X's & Z's	Stratified	Unemployed	Job-to-job
			Age in years (reference group: 25-29)		
30-34	0.943	0.957	0.908	1.100	0.808
	(-1.13)	(-0.81)	(-2.11)	(1.27)	(-2.61)
35-39	0.974	1.008	0.897	1.187	0.810
	(-0.52)	(0.15)	(-2.20)	(2.27)	(-2.40)
40-44	0.934	0.976	0.904	1.187	0.758
	(-1.38)	(-0.47)	(-2.17)	(2.49)	(-2.95)
45-52	0.923	0.972	0.886	1.325	0.532
	(-1.75)	(-0.58)	(-2.67)	(4.23)	(-7.34)
			Education (reference group: no professional training)		
Vocational	0.893	0.895	0.821	0.869	1.129
training	(-2.30)	(-2.27)	(-5.07)	(-2.53)	(1.07)
Voc. training &	1.078	1.091	0.965	0.902	1.645
A-level	(0.63)	(0.72)	(-0.37)	(-0.78)	(2.31)
University	0.858	0.875	0.898	0.676	1.410
	(-2.11)	(-1.71)	(-1.55)	(-3.65)	(2.33)
			Job position (reference group: unskilled blue-collar)		
Skilled blue-	0.744	0.789	0.788	0.762	0.909
collar	(-8.42)	(-6.06)	(-5.28)	(-5.65)	(-1.41)
White-collar	0.483	0.525	0.658	0.433	0.706
	(-13.22)	(-10.76)	(-6.63)	(-9.95)	(-3.24)
Master	0.466	0.512	0.569	0.443	0.821
craftsman	(-6.19)	(-5.15)	(-5.24)	(-5.71)	(-0.91)
Part-time worker	1.068	1.066	1.075	1.040	0.848
	(2.10)	(1.69)	(1.56)	(0.94)	(-1.67)
			Occupation (reference group: service workers)		
Production	1.169	1.136	1.057	1.177	0.960
workers	(3.69)	(2.59)	(1.37)	(2.68)	(-0.47)
Technicians	1.010	0.943	0.966	1.020	0.867
	(0.14)	(-0.85)	(-0.58)	(0.21)	(-1.41)
Others	1.561	1.453	1.294	1.701	0.803
	(6.21)	(5.39)	(4.07)	(6.89)	(-1.18)
			Nationality (reference group: German)		
EU citizen	1.751	1.841	1.299	0.889	2.848
	(1.93)	(2.08)	(0.88)	(-0.23)	(2.48)
No EU citizen	1.290	1.336	1.026	0.933	1.549
	(2.95)	(3.18)	(0.23)	(-0.46)	(2.27)
			Previous employment state (reference group: no observation)		
Unemployment	1.807	1.587	1.520	3.103	0.933
	(7.56)	(5.64)	(5.39)	(10.58)	(-0.46)

Table 3 (continued)

Non-employment	1.409	1.417	1.444	1.135	1.450
	(3.38)	(3.33)	(3.74)	(0.78)	(2.18)
Recall	0.508	0.569	0.734	0.715	0.531
	(-6.95)	(-5.69)	(-3.41)	(-2.40)	(-3.61)
Job-to-job change	0.794	0.874	1.077	1.205	0.899
	(-2.92)	(-1.66)	(0.96)	(1.65)	(-0.76)
Calendar time (reference group: 2001)					
1996	0.924	0.986	1.112	0.994	0.866
	(-1.29)	(-0.23)	(1.53)	(-0.08)	(-1.24)
1997	1.109	1.124	1.046	1.062	1.079
	(2.20)	(2.44)	(0.94)	(1.02)	(0.86)
1998	0.781	0.860	0.938	0.876	0.708
	(-4.68)	(-2.85)	(-1.15)	(-2.01)	(-3.62)
1999	0.989	1.042	1.092	1.073	0.896
	(-0.22)	(0.82)	(1.64)	(1.16)	(-1.20)
2000	1.123	1.140	1.115	1.103	1.112
	(2.21)	(2.53)	(2.44)	(1.50)	(1.15)
Local labour market					
Unemployment rate	0.977	0.980	1.014	0.983	0.980
	(-2.89)	(-2.56)	(0.83)	(-1.68)	(-1.30)
Investments in (reference group: no investment)					
ICT		0.910	0.871	0.941	0.857
		(-2.91)	(-3.63)	(-1.53)	(-2.64)
Further training		0.905	1.136	0.852	0.979
		(-2.54)	(2.45)	(-3.42)	(-0.27)
Collective agreements (reference group: no collective agreement)					
Sector-wide		0.922	0.948	0.958	0.873
		(-2.41)	(-1.05)	(-1.02)	(-2.17)
Firm-wide		1.042	0.939	1.091	0.920
		(1.08)	(-1.25)	(1.97)	(-1.02)
Wages > tariff		0.904	0.818	0.898	0.910
		(-2.33)	(-3.08)	(-1.82)	(-1.30)
Works council		0.746		0.770	0.665
		(-9.92)		(-7.51)	(-7.65)
Legal form (reference group: individual firm)					
Partnership		1.024		0.982	1.135
		(0.23)		(-0.13)	(0.79)
Private limited company		1.084		1.087	1.115
		(1.41)		(1.14)	(0.97)
Public limited company		0.863		1.014	0.713
		(-1.16)		(0.10)	(-1.73)

Table 3 (continued)

Public		0.776		0.919	0.644
Corporation		(-3.26)		(-0.92)	(-2.66)
Other (e.g.		0.995		1.085	0.884
association)		(-0.07)		(0.98)	(-0.80)
Firm size (reference group: 100-199 employees)					
≤ 4		0.883		0.892	0.867
		(-0.74)		(-0.63)	(-0.49)
5-19		1.189		1.121	1.397
		(3.44)		(1.74)	(4.20)
20-49		0.857		0.799	1.041
		(-3.84)		(-4.47)	(0.53)
50-99		1.076		1.046	1.178
		(2.35)		(1.23)	(2.58)
200-299		1.125		1.054	1.256
		(4.40)		(1.68)	(3.91)
300-499		1.185		1.138	1.088
		(5.58)		(3.80)	(1.13)
500-999		1.120		1.021	1.328
		(3.80)		(0.61)	(4.49)
≥ 1000		1.050		0.971	1.398
		(1.30)		(-0.63)	(4.47)
Year of setting up (reference group: 1981-1990)					
≤ 1980		0.766		0.603	1.328
		(-2.00)		(-2.91)	(1.25)
1991-1995		0.924		0.934	0.927
		(-2.37)		(-1.68)	(-1.14)
≥ 1996		0.723		0.611	1.024
		(-6.27)		(-6.96)	(0.28)
Wald Chi²	3,195.51	6,035.77	543.28	6,411.72	1,513.56
Log likelihood	-521,762.12	-517,939.65	-215,219.8	-316,345.64	-157,189.32
# Subjects			104,455		
# Obs.			238,050		
# Failures		50,123		33,240	12,716

Note: Results are shown in hazard ratios; t-values are in parentheses below. Additional covariates are countries and sectors.

Table 4: Results from Cox estimation, West Germany, female workers

	only X's	X's & Z's	Stratified	Unemployed	Job-to-job
			Age in years (reference group: 25-29)		
30-34	0.807	0.822	0.799	1.006	0.792
	(-3.69)	(-3.31)	(-4.52)	(0.06)	(-2.69)
35-39	0.736	0.745	0.763	0.794	0.853
	(-4.66)	(-4.45)	(-5.33)	(-2.03)	(-1.53)
40-44	0.738	0.751	0.743	0.894	0.692
	(-4.24)	(-3.93)	(-5.26)	(-0.99)	(-3.20)
45-52	0.697	0.708	0.733	0.955	0.532
	(-5.28)	(-5.05)	(-5.60)	(-0.43)	(-6.19)
			Education (reference group: no professional training)		
Vocational	0.649	0.656	0.768	0.680	0.692
training	(-8.13)	(-8.03)	(-5.53)	(-4.66)	(-4.29)
Voc. training &	0.604	0.648	0.735	0.466	0.748
A-level	(-5.70)	(-4.73)	(-4.07)	(-4.66)	(-2.27)
University	0.599	0.660	0.824	0.574	0.860
	(-4.76)	(-3.70)	(-2.48)	(-3.59)	(-0.88)
			Job position (reference group: unskilled blue-collar)		
Skilled blue-	1.252	1.046	1.089	0.964	0.971
collar	(2.35)	(0.44)	(0.80)	(-0.24)	(-0.15)
White-collar	0.768	0.770	0.833	0.694	0.849
	(-3.71)	(-3.46)	(-2.44)	(-2.95)	(-1.34)
Part-time worker	0.711	0.723	0.820	0.630	0.712
	(-5.06)	(-4.39)	(-2.54)	(-4.01)	(-2.95)
			Occupation (reference group: service workers)		
Production	0.824	1.006	0.960	1.247	0.732
workers	(-3.46)	(0.09)	(-0.58)	(2.06)	(-2.56)
Technicians	0.867	1.001	0.886	1.098	0.856
	(-1.27)	(0.01)	(-1.19)	(0.49)	(-0.86)
Others	0.785	0.758	1.837	0.552	0.751
	(-0.60)	(-0.56)	(2.27)	(-0.76)	(-0.55)
			Nationality (reference group: German)		
EU citizen	0.710	0.741	0.809	0.606	0.747
	(-3.43)	(-3.00)	(-2.71)	(-2.36)	(-1.85)
No EU citizen	1.223	1.164	0.918	1.193	0.635
	(2.38)	(1.86)	(-1.40)	(1.71)	(-3.92)
			Previous employment state (reference group: no observation)		
Unemployment	1.803	1.772	1.567	3.991	1.188
	(9.37)	(8.58)	(8.39)	(14.38)	(1.52)
Non-	1.543	1.501	1.518	1.458	1.399
employment	(5.00)	(4.39)	(5.75)	(2.14)	(2.23)

Table 4 (continued)

Recall	0.639	0.678	0.699	0.772	0.550
	(-4.58)	(-4.02)	(-5.67)	(-1.90)	(-4.52)
Job-to-job	1.094	1.134	1.168	1.254	1.588
change	(1.41)	(1.88)	(2.83)	(2.07)	(4.20)
Calendar time (reference group: 2001)					
1996	1.262	1.258	1.501	1.460	0.662
	(2.77)	(2.68)	(4.99)	(2.72)	(-3.09)
1997	1.364	1.336	1.648	1.386	0.767
	(4.18)	(3.84)	(5.27)	(2.60)	(-2.47)
1998	1.232	1.194	1.778	1.052	0.871
	(2.74)	(2.29)	(4.62)	(0.41)	(-1.10)
1999	1.366	1.333	1.766	1.180	1.106
	(4.70)	(4.30)	(5.64)	(1.48)	(1.11)
2000	1.349	1.337	1.504	1.118	1.281
	(4.70)	(4.55)	(5.32)	(0.98)	(2.94)
Local labour market					
Unemployment	1.014	1.019	0.906	1.041	1.018
rate	(1.15)	(1.49)	(-2.37)	(2.26)	(0.92)
Investments in (reference group: no investment)					
ICT		0.955	0.794	1.004	0.955
		(-0.98)	(-4.20)	(0.06)	(-0.60)
Further training		0.970	0.940	0.898	1.062
		(-0.46)	(-0.59)	(-1.05)	(0.57)
Collective agreements (reference group: no collective agreement)					
Sector-wide		0.938	1.236	0.972	0.866
		(-1.04)	(2.41)	(-0.31)	(-1.47)
Firm-wide		0.964	1.145	0.942	0.834
		(-0.41)	(1.19)	(-0.51)	(-1.38)
Wages > tariff		1.091	1.014	0.899	1.252
		(1.74)	(0.19)	(-1.31)	(2.96)
Works council		0.852		0.857	0.823
		(-2.96)		(-1.92)	(-2.11)
Legal form (reference group: individual firm)					
Partnership		0.976		0.853	0.966
		(-0.23)		(-0.92)	(-0.19)
Private limited		1.034		0.965	1.151
company		(0.32)		(-0.24)	(0.84)
Public limited		1.249		1.000	1.301
company		(1.82)		(0.00)	(1.31)
Public		0.730		0.768	0.743
Corporation		(-2.68)		(-1.42)	(-1.63)

Table 4 (continued)

Other (e.g.		0.843		1.078	0.778
association)		(-1.56)		(0.45)	(-1.36)
Firm size (reference group: 100-199 employees)					
≤ 4		1.204		1.130	1.271
		(1.01)		(0.48)	(0.82)
5-19		1.111		1.234	0.997
		(1.44)		(1.84)	(-0.02)
20-49		1.102		1.088	1.284
		(1.77)		(0.95)	(3.14)
50-99		1.261		1.147	1.500
		(4.96)		(1.82)	(5.79)
200-299		1.185		1.245	1.120
		(3.85)		(3.27)	(1.53)
300-499		1.028		1.044	1.067
		(0.65)		(0.62)	(0.96)
500-999		0.978		0.943	1.067
		(-0.54)		(-0.85)	(1.04)
≥ 1000		1.027		0.941	1.093
		(0.55)		(-0.78)	(1.22)
Year of setting up (reference group: 1981-1990)					
≤ 1980		0.867		0.908	0.849
		(-1.90)		(-0.77)	(-1.49)
1991-1995		0.860		0.851	0.895
		(-1.74)		(-1.17)	(-0.92)
≥ 1996		0.676		1.038	0.442
		(-3.29)		(0.22)	(-3.94)
Wald Chi²	891.32	1,682.39	511.66	2,401.25	851.84
Log likelihood	-563,041.41	-561,166.43	-225,136.11	-224,902.3	-222,701.15
# Subjects			129,198		
# Obs.			326,352		
# Failures		40,612		15,705	16,853

Note: Results are shown in hazard ratios; t-values are in parentheses below. Additional covariates are countries and sectors.

Table 5: Results from Cox estimation, East Germany, female workers

	only X's	X's & Z's	Stratified	Unemployed	Job-to-job
			Age in years (reference group: 25-29)		
30-34	0.857	0.861	0.877	1.013	0.681
	(-2.94)	(-2.84)	(-3.20)	(0.21)	(-3.49)
35-39	0.848	0.857	0.830	1.056	0.561
	(-2.89)	(-2.67)	(-4.58)	(0.79)	(-5.13)
40-44	0.808	0.811	0.784	1.023	0.532
	(-3.88)	(-3.74)	(-5.76)	(0.37)	(-5.06)
45-52	0.780	0.779	0.763	0.999	0.396
	(-4.96)	(-5.02)	(-6.98)	(-0.01)	(-7.70)
			Education (reference group: no professional training)		
Vocational	0.889	0.891	0.744	0.901	0.887
training	(-1.96)	(-2.19)	(-7.43)	(-1.96)	(-0.80)
Voc. training &	0.966	1.029	1.016	0.856	1.800
A-level	(-0.32)	(0.26)	(0.16)	(-1.21)	(2.80)
University	0.954	1.037	0.911	0.792	1.688
	(-0.59)	(0.50)	(-1.36)	(-2.59)	(3.04)
			Job position (reference group: unskilled blue-collar)		
Skilled blue-	0.859	0.845	0.873	0.797	1.022
collar	(-2.79)	(-2.96)	(-2.48)	(-3.84)	(0.18)
White-collar	0.625	0.571	0.616	0.606	0.559
	(-6.97)	(-8.50)	(-10.49)	(-7.76)	(-4.95)
Part-time worker	0.965	0.848	0.923	0.884	0.777
	(-0.68)	(-3.20)	(-2.13)	(-2.51)	(-2.29)
			Occupation (reference group: service workers)		
Production	1.609	1.589	1.331	1.853	0.849
workers	(10.08)	(10.72)	(9.40)	(13.76)	(-1.51)
Technicians	1.302	1.063	1.010	1.262	0.680
	(4.00)	(0.87)	(0.14)	(3.37)	(-2.31)
Others	1.963	1.730	1.500	2.244	0.627
	(9.87)	(6.55)	(6.14)	(9.56)	(-1.66)
			Nationality (reference group: German)		
EU citizen	1.822	1.867	0.550	0.505	2.899
	(1.98)	(2.89)	(-1.37)	(-1.49)	(1.60)
No EU citizen	1.326	1.220	1.285	1.246	1.018
	(2.59)	(1.72)	(2.23)	(1.49)	(0.08)
			Previous employment state (reference group: no observation)		
Unemployment	1.846	1.693	1.499	3.178	0.803
	(7.45)	(6.37)	(6.28)	(13.36)	(-1.44)
Non-	0.831	0.976	1.149	0.788	1.019
employment	(-1.38)	(-0.19)	(1.40)	(-1.73)	(0.09)

Table 5 (continued)

Recall	0.383	0.400	0.425	0.545	0.353
	(-8.31)	(-8.18)	(-11.03)	(-3.94)	(-5.94)
Job-to-job	0.445	0.573	0.824	0.578	0.945
change	(-8.92)	(-6.24)	(-2.70)	(-5.13)	(-0.38)
Calendar time (reference group: 2001)					
1996	0.870	0.951	0.979	1.078	0.507
	(-2.10)	(-0.72)	(-0.36)	(1.04)	(-3.76)
1997	1.035	1.077	1.040	1.099	0.629
	(0.67)	(1.37)	(0.98)	(1.70)	(-3.08)
1998	0.737	0.833	0.910	0.832	0.704
	(-5.28)	(-3.13)	(-1.97)	(-2.75)	(-2.73)
1999	1.079	1.116	1.156	1.143	0.854
	(1.28)	(1.84)	(3.68)	(1.96)	(-1.30)
2000	1.111	1.144	1.047	1.075	1.074
	(1.91)	(2.42)	(1.21)	(1.16)	(0.60)
Local labour market					
Unemployment	0.953	0.980	0.991	0.992	0.911
rate	(-6.21)	(-2.54)	(-0.64)	(-0.81)	(-4.95)
Investments in (reference group: no investment)					
ICT		0.967	0.943	0.955	1.056
		(-1.09)	(-1.94)	(-1.35)	(0.66)
Further training		0.887	1.134	0.864	1.018
		(-2.92)	(3.02)	(-3.44)	(0.14)
Collective agreements (reference group: no collective agreement)					
Sector-wide		0.876	0.872	0.917	0.727
		(-3.28)	(-2.80)	(-1.88)	(-3.52)
Firm-wide		1.067	0.850	1.163	0.789
		(1.56)	(-3.89)	(3.32)	(-2.35)
Wages > tariff		1.010	0.923	0.903	1.178
		(0.16)	(-1.14)	(-1.39)	(1.22)
Works council		0.783		0.791	0.775
		(-7.38)		(-6.12)	(-3.59)
Legal form (reference group: individual firm)					
Partnership		1.376		1.408	1.251
		(2.03)		(1.86)	(0.82)
Private limited		1.120		1.144	1.135
company		(1.08)		(1.05)	(0.62)
Public limited		1.318		1.359	0.911
company		(1.75)		(1.54)	(-0.30)
Public		0.932		1.066	0.790
Corporation		(-0.59)		(0.42)	(-1.06)

Table 5 (continued)

Other (e.g.		1.050		1.226	0.832
association)		(0.43)		(1.47)	(-0.83)
Firm size (reference group: 100-199 employees)					
≤ 4		0.907		0.853	0.829
		(-0.61)		(-0.83)	(-0.57)
5-19		0.779		0.771	0.897
		(-3.22)		(-2.72)	(-0.77)
20-49		0.660		0.654	0.860
		(-8.38)		(-6.92)	(-1.51)
50-99		0.876		0.877	1.008
		(-3.86)		(-3.56)	(0.10)
200-299		0.961		0.978	0.933
		(-1.70)		(-0.86)	(-1.10)
300-499		0.943		0.906	1.120
		(-2.33)		(-3.72)	(1.56)
500-999		0.963		0.968	1.083
		(-1.59)		(-1.21)	(1.28)
≥ 1000		0.719		0.716	0.692
		(-8.00)		(-7.73)	(-3.24)
Year of setting up (reference group: 1981-1990)					
≤ 1980		0.688		0.681	0.858
		(-2.63)		(-2.16)	(-0.95)
1991-1995		0.985		0.982	1.062
		(-0.38)		(-0.39)	(0.72)
≥ 1996		0.602		0.548	1.065
		(-7.20)		(-5.91)	(0.48)
Wald Chi²	5,118.27	7,163.75	1,184.29	7,986.27	1,170.73
Log likelihood	-412,241.87	-409,707.26	-170,924.57	-293,829.1	-81,508.027
# Subjects			130,002		
# Obs.			290,370		
# Failures		59,019		46,723	8,896

Note: Results are shown in hazard ratios; t-values are in parentheses below. Additional covariates are countries and sectors.

Figure 1: Kaplan-Meier-curves separated for men and women and West and East

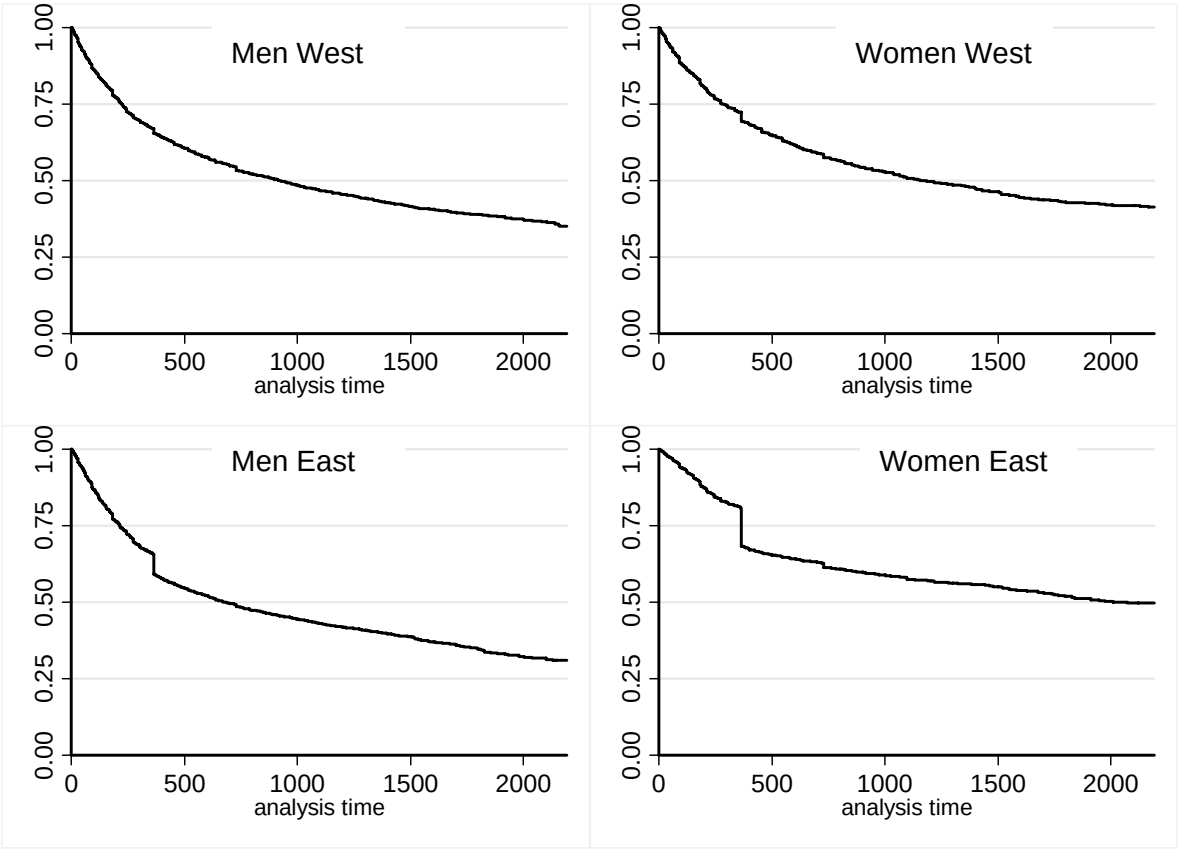


Figure 2: Kaplan-Meier-curves by educational level separated for West and East

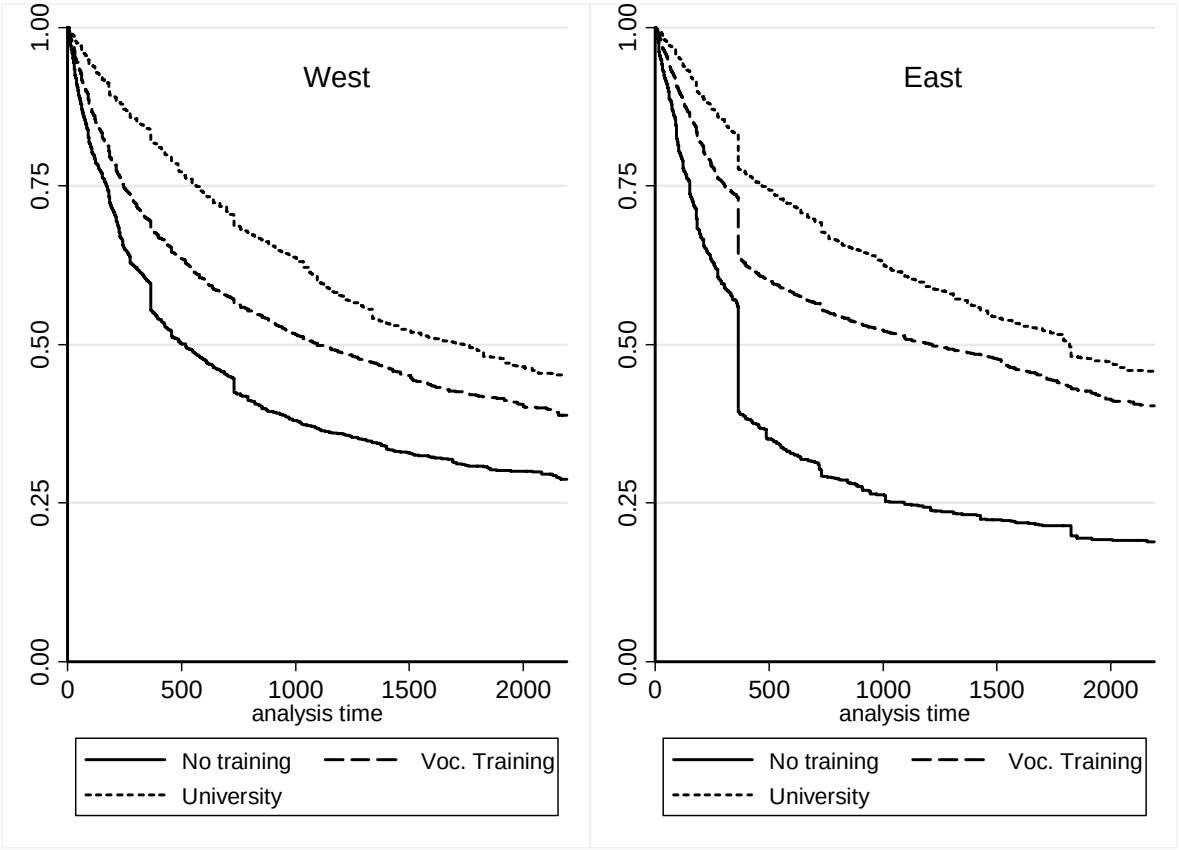


Figure 3: Kaplan-Meier-curves by age group separated for West and East

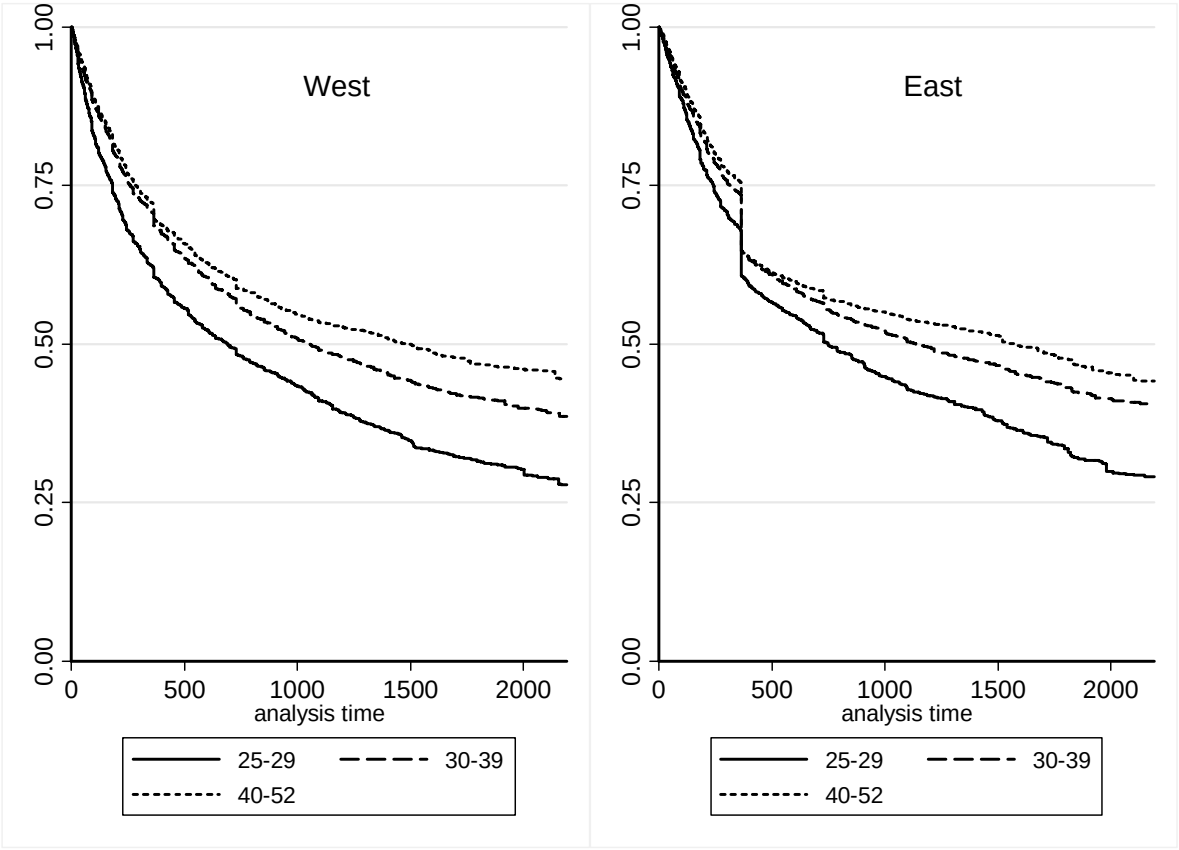


Figure 4: Kaplan-Meier-curves by firm size separated for West and East

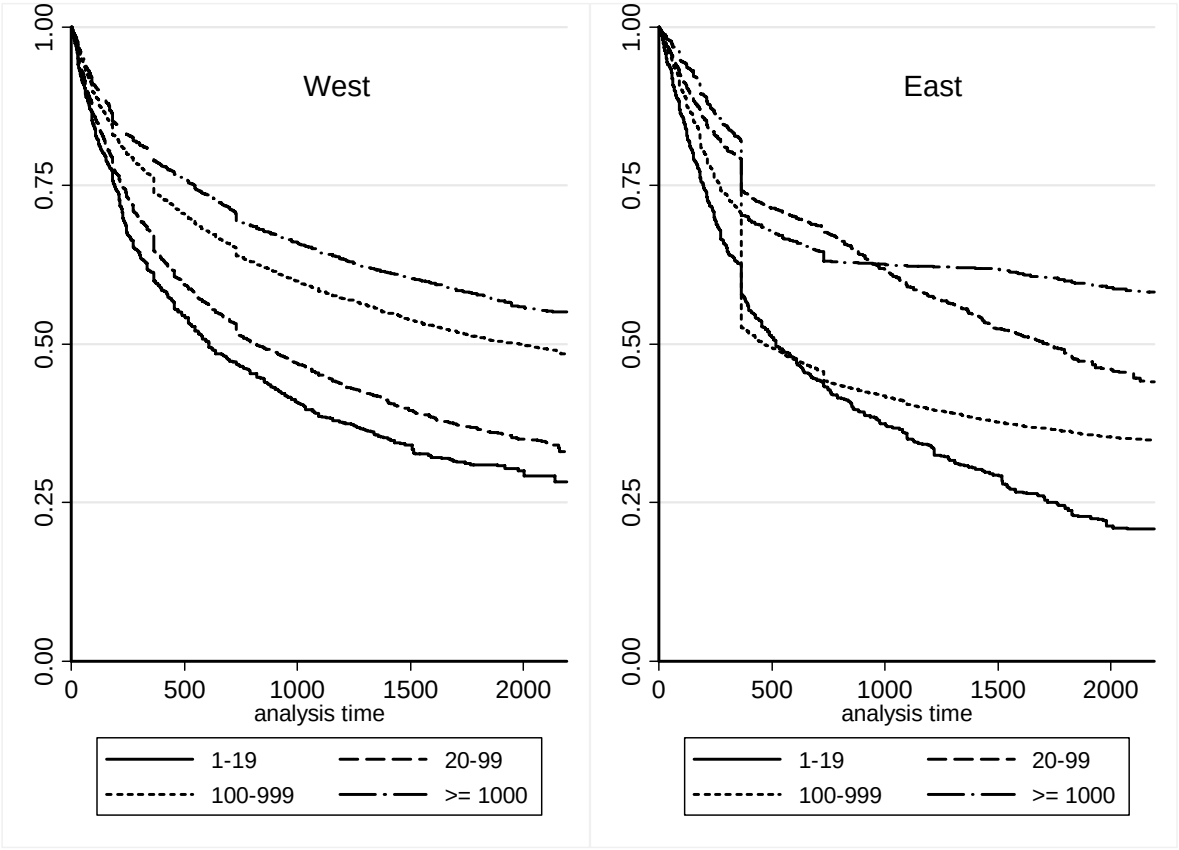
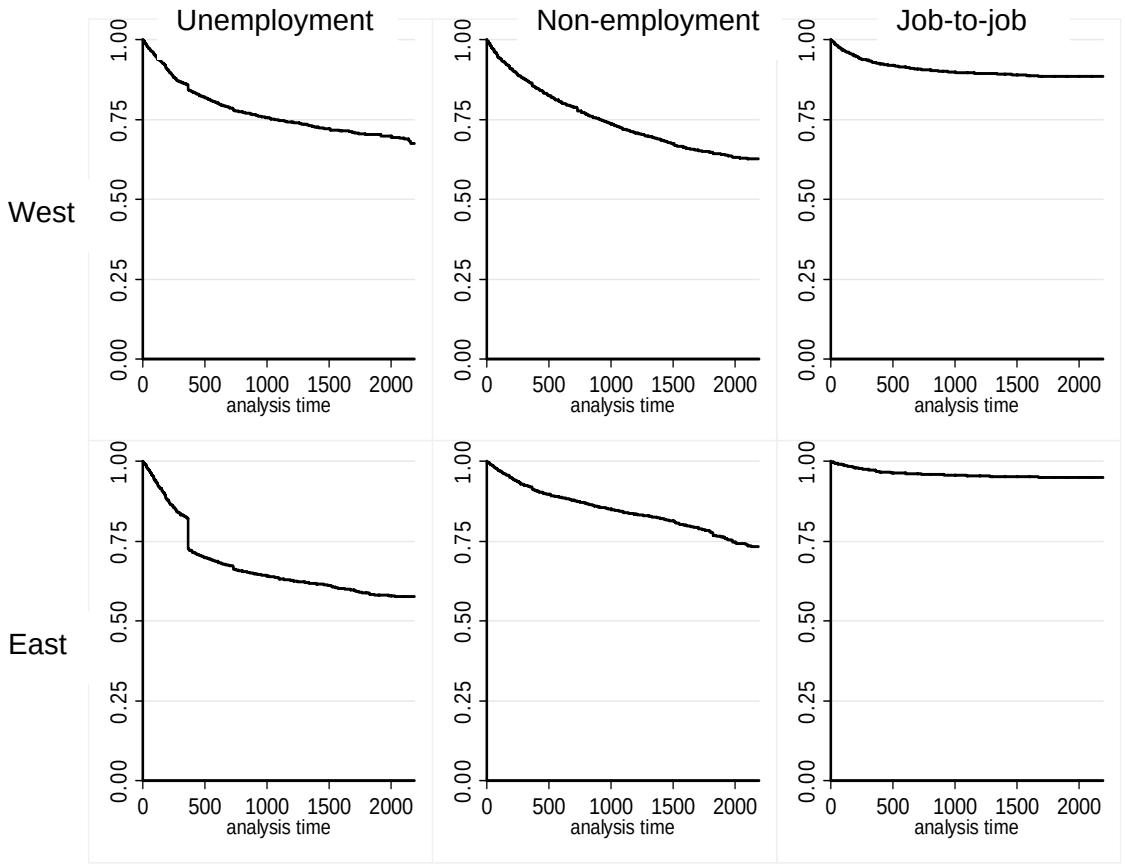


Figure 5: Kaplan-Meier-curves by destination state separated for West and East



Appendix

Table A1: Number of observations and failures, means and standard deviations of covariates

Variable	Men				Women			
	West # spells		East # spells		West # spells		East # spells	
Observations	529,293		238,050		326,352		290,370	
Destination								
Unemployment	21,282		33,240		15,705		46,723	
Non-employment	10,923		4,167		8,054		3,400	
New employer	31,496		12,716		16,853		8,896	
	Mean	Std.dev	Mean	Std.dev	Mean	Std.dev	Mean	Std.dev
Come from								
Unemployment	0.194	0.396	0.423	0.494	0.193	0.395	0.437	0.496
Non-employment	0.050	0.218	0.038	0.192	0.053	0.224	0.023	0.150
Recall	0.176	0.381	0.114	0.317	0.237	0.425	0.212	0.409
Job-to-job transition	0.482	0.500	0.346	0.476	0.373	0.484	0.258	0.438
No observation	0.098	0.297	0.079	0.270	0.144	0.351	0.070	0.256
Individual-specific								
Education								
No training	0.187	0.390	0.056	0.230	0.217	0.412	0.051	0.220
Vocational training	0.554	0.497	0.715	0.451	0.567	0.496	0.782	0.413
Vocational training and A-levels	0.049	0.216	0.026	0.160	0.080	0.272	0.033	0.177
University	0.210	0.408	0.202	0.402	0.136	0.342	0.135	0.341
Job position								
Blue collar unskilled	0.329	0.470	0.164	0.370	0.181	0.385	0.120	0.324
Blue collar skilled	0.220	0.414	0.378	0.485	0.028	0.164	0.083	0.276
White collar	0.397	0.489	0.321	0.467	0.481	0.500	0.483	0.500
Master craftsman	0.010	0.098	0.015	0.121				
Part-time more than 15 hours	0.036	0.185	0.121	0.326	0.306	0.461	0.313	0.464

Table A1 (continued)

Age								
25-29	0.188	0.391	0.134	0.341	0.184	0.387	0.131	0.338
30-34	0.268	0.443	0.194	0.395	0.274	0.446	0.214	0.410
35-39	0.220	0.414	0.205	0.404	0.220	0.414	0.212	0.408
40-44	0.153	0.360	0.190	0.393	0.155	0.362	0.188	0.391
45-52	0.171	0.376	0.276	0.447	0.167	0.373	0.255	0.436
Profession								
3 Production	0.449	0.497	0.508	0.500	0.197	0.398	0.238	0.426
4 Technical	0.136	0.343	0.099	0.298	0.041	0.198	0.043	0.203
5 Services								
6 Others	0.005	0.072	0.012	0.107	0.002	0.048	0.006	0.077
Nationality								
German								
EU	0.023	0.151	0.002	0.046	0.024	0.153	0.001	0.029
Non-EU	0.089	0.285	0.021	0.145	0.072	0.258	0.005	0.072
Firm-specific								
Sector								
Insurance, credit	0.063	0.243	0.010	0.099	0.110	0.313	0.024	0.152
Transport, communication	0.068	0.251	0.043	0.204	0.056	0.231	0.012	0.110
Trade, repair	0.044	0.206	0.028	0.166	0.075	0.263	0.031	0.172
Construction	0.023	0.150	0.095	0.293	0.002	0.048	0.007	0.082
Mining, energy, water	0.024	0.154	0.026	0.160	0.010	0.100	0.010	0.101
Finish of raw materials	0.153	0.360	0.087	0.281	0.078	0.268	0.034	0.182
Capital goods	0.406	0.491	0.199	0.399	0.195	0.396	0.060	0.237
Consumer goods	0.043	0.203	0.049	0.216	0.050	0.218	0.050	0.219
Services for firms	0.032	0.177	0.049	0.216	0.033	0.180	0.027	0.161
Other services	0.095	0.294	0.201	0.401	0.276	0.447	0.326	0.469
Non-profit organization	0.012	0.107	0.065	0.246	0.040	0.196	0.111	0.314
Regional authorities, social insurances	0.036	0.185	0.148	0.355	0.074	0.262	0.308	0.462
Firm-size								
0-4	0.001	0.027	0.002	0.043	0.001	0.031	0.002	0.039
5-19	0.010	0.098	0.024	0.152	0.010	0.098	0.010	0.102
20-49	0.022	0.148	0.052	0.221	0.022	0.148	0.039	0.195
50-99	0.037	0.190	0.097	0.296	0.039	0.195	0.058	0.234
100-199	0.053	0.224	0.157	0.363	0.052	0.222	0.118	0.322

Table A1 (continued)

200-299	0.059	0.236	0.131	0.337	0.067	0.250	0.135	0.342
300-499	0.088	0.283	0.158	0.365	0.110	0.312	0.170	0.376
500-999	0.149	0.356	0.195	0.396	0.173	0.378	0.232	0.422
≥ 1000	0.581	0.493	0.185	0.388	0.526	0.499	0.235	0.424
Bargaining								
Council	0.916	0.277	0.682	0.466	0.912	0.283	0.722	0.448
Sector coll. agreement	0.788	0.409	0.599	0.490	0.765	0.424	0.679	0.467
Firm collective agreement	0.127	0.333	0.160	0.367	0.125	0.331	0.140	0.347
Wage > tariff	0.621	0.485	0.132	0.339	0.453	0.498	0.076	0.266
Legal form								
Individual firm	0.010	0.100	0.028	0.164	0.007	0.086	0.013	0.111
Partnership	0.064	0.245	0.027	0.161	0.059	0.235	0.014	0.119
Private limited partnership	0.438	0.496	0.583	0.493	0.338	0.473	0.388	0.487
Public limited partnership	0.359	0.480	0.064	0.245	0.265	0.441	0.026	0.161
Public Corporation	0.099	0.299	0.187	0.390	0.263	0.440	0.395	0.489
Other (e.g. association)	0.030	0.171	0.112	0.315	0.068	0.252	0.163	0.370
Year of setting up								
≤ 1980	0.820	0.384	0.027	0.163	0.812	0.391	0.033	0.180
1981-1990	0.057	0.233	0.215	0.411	0.049	0.216	0.240	0.427
1991-1995	0.087	0.281	0.643	0.479	0.116	0.320	0.608	0.488
≥ 1996	0.036	0.185	0.115	0.319	0.024	0.152	0.118	0.323
Further training: yes/no	0.958	0.200	0.873	0.333	0.967	0.177	0.887	0.316
Investments in ICT	0.865	0.342	0.732	0.443	0.853	0.354	0.719	0.449
Country								
Berlin	0.073	0.260			0.118	0.323		
Schleswig-Holstein	0.025	0.156			0.028	0.166		
Hamburg	0.058	0.233			0.047	0.212		
Lower Saxony	0.091	0.288			0.092	0.288		
Bremen	0.013	0.114			0.014	0.119		
North Rhine-Westphalia	0.282	0.450			0.274	0.446		
Hesse	0.095	0.294			0.086	0.280		
Rhineland-Palatinate/Saarland	0.049	0.215			0.032	0.177		
Baden-Württemberg	0.120	0.325			0.136	0.343		

Table A1 (continued)

Bavaria	0.194	0.396			0.173	0.378		
Brandenburg			0.195	0.396			0.186	0.389
Mecklenburg-Vorpommern			0.179	0.384			0.151	0.358
Saxony			0.204	0.403			0.208	0.406
Saxony-Anhalt			0.219	0.414			0.285	0.452
Thuringia			0.203	0.402			0.169	0.375