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Individual and Plant-level Determinants of Job Durations in Germany

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Non-technical summary

The duration of employment relationships is of substantial importance for both individual work histories and employers' personnel policies. Using German linked employer-employee-data, which have recently been made available, we analyse job durations from both sides of the employment relationship. We address two main questions. First, 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? Is there evidence for segmentation within companies, such that a core workforce is protected against job losses by the employment of a marginal workforce with short tenure on average, or do observed differences in tenure mainly stem from different tenure levels at different firms?

Our second question pertains to the influence of individual and firm characteristics on job tenure. While transitions from jobs depend on individual characteristics such as age or education, firm characteristics like size or industry and institutions such as works councils may also be important. Analysing the determinants of job duration is likely to lead to biased results if either firm or individual characteristics are left out of the analysis. We also investigate whether the role of worker and firm characteristics differs between transitions to different destination states, such as unemployment or a change to another employer.

From our results, we conclude that most of the heterogeneity in job durations is not due to individuals' employment in particular establishments. This is consistent with the view that long and short employment spells coexist at the firm level. The multivariate results show that low-skilled employees in low job positions have significantly shorter job durations, while there is little evidence that highly qualified employees are more mobile than others. 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. Firm size is of minor importance in this respect. Distinguishing between different destination states, we find that mobility to another job and exit to unemployment follow strikingly different processes.

Individual and Plant-level Determinants of Job Durations in Germany

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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 extent 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

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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 linked employer-employee-data been made available to analyse job durations from both sides of the employment relationship. These data allow us to address a number of questions which have so far not been sufficiently investigated.

First, both individual and firm-specific reasons for job exit can be analysed. While transitions from jobs depend on individual characteristics such as age or education, firms also differ with respect to employment duration of their workers, due to, for instance, 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 influence churning, i.e. separations that are not due to net reductions in the number of workers. Furthermore, the role of worker and firm characteristics is also likely to differ between transitions to different destination states. For instance, the presence of a works council may influence dismissals and individual quits independently and to different degrees. Analysing 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? 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 dual labour markets within companies.

Starting with the contributions of Burdett (1978) and Jovanovic (1979a,b), there has been much theoretical research on job durations in the context of the search and matching theory (see Mortensen and Pissarides, 1999 and Pissarides, 2000 for reviews). These models give an equilibrium interpretation to the observed patterns of job changes underscoring in particular, the simultaneous determination of wages and job durations. While it would, in principle, be highly desirable to estimate a structural model of job change, this comes at the cost of losing flexibility in accounting for left- and right-censoring, duration dependence and different exit states. In this paper, therefore, we model job exit by a reduced-form hazard rate model which does not include individual wages.

Our data, taken from the German Linked Employer-Employee Dataset of the IAB (LIAB) longitudinal model, comprise all jobs starting in a panel of 4,200 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), Grotheer et al. (2004), Schasse (1991) and Wolff (2004). Of these studies, only Bender et al. (2000), Gerlach und Stephan (2005) and Grotheer et al. (2004) use linked employer-employee data.

Bender et al. (2000) as well as Grotheer et al. (2004) use combined register and survey data similar to the LIAB, although the observation period is shorter in both cases. Both 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 the 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). Additionally, they find some evidence for a sorting effect of

workers with particular characteristics into different companies. Shorter individual tenure of females disappears once workplace effects are allowed. This agrees with findings obtained by Gerlach and Stephan (2005). 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.

However, neither of these papers directly measures heterogeneity in job durations across and within firms. Bronars and Famulari (1997) analyse 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 Institute for Employment Research (IAB) (see Alda et al., 2005). 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. Referring to the classification of linked employer-employee data introduced by Abowd and

Kramarz (1999), the LIAB is a dataset representative for firms and their respective workers, and based on both administrative 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 prosecuted 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 Register also contains information on a number of characteristics relating to the person or the job. 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 transfer receipt. 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 mid 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 (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 certain 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. 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. If a firm was interviewed in the years 1993 to 1998 too, the data is also available. This means that worker separations due to plant closures cannot be observed in the data. Hence, the LIAB longitudinal version contains a dataset with around 2,100 plants in West and around 2,100

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).

plants in East Germany.[2] Overall, the data is a sub-sample of the IAB Establishment Panel which is a representative 1 per cent sample of German establishments. Therefore, our data is representative and covers, if it is weighted, 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

2 For further information on the selection of the 4,200 establishments see Alda (2004b) and Alda (2004c).

end of the employment relationship could also be assumed whenever the establishment identifier changes. Unfortunately, however, the plant identifier sometimes changes although the individual continues working in the same workplace. This happens, for instance, when the legal identity of the employer changes. In our data this is only possible if a part of the establishment is legally separated from another part, the latter keeping the original identifier and remaining in the Establishment Panel. Because we observe a large number of cases in which the establishment identifier changes without the end of the employment relationship being recorded in the data, we assume errors in the allocation of the identifiers by the local Federal Employment Agencies. Therefore, we rely, in addition, on employers' declarations.[3]

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 (see Fitzenberger and Wilke, 2004; Lee and Wilke, 2005). 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 or recessions, individual reasons such as ill health and others. We define a separation and subsequent return to the same employer within at most 90 days as a continuation of the current employment spell. This may be combined with periods of UI benefit receipt. With 90 days, the maximum duration for the transition to be considered as a continuation of the current spell may be too short to capture seasonal work. In future research, the sensitivity of the result with respect to this definition will be explored.

3 This results in the fact that the number of right-censored spells is much higher in our study than in the study by Grotheer et al. (2004), who only rely on changes of the plant identifier. How the employer

If a person did not receive benefits, did not change job-to-job 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 destination states are summarized:

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 does not change from job-to-job for at least 60 days after separation
Job-to-job change	→	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, did not change from job-to-job 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, did not change from job-to-job during 60 days before employment
Job-to-job change	→	did change from job-to-job at most 60 days before employment
No observation	→	was not observed since January 1st, 1995

identifier changes mistakenly in individual's records will be investigated in further research.

3 Sample definition and descriptive statistics

3.1 Sample definition

We observe all employees of the sample establishments during 1996 to 2001. However, spells that 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 starting before 1996 and observed after 1996 is not representative of all spells starting 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. Despite the fact that we defined recalls within 90 days as continuation of employment we exclude the agricultural sector to avoid interruptions of spells due to seasonal work. 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.

Table 1 shows the sample employment spells according to previous employment and destination states. Due to the relatively short observation period, most of the spells are right-censored. In East Germany, more than 30 per

cent of all new relationships end in unemployment. In West Germany, the share of job-to-job changes is higher than the share of transitions into unemployment. Changes into non-employment do not occur very often. The same applies to the previous employment state, where a high share of recalls is striking. This can be due to the structure of our data set. The probability that a person is observed twice in two different establishments is relatively small. Moreover, individuals with recall are over-represented because we observe all new employment spells which started in the observation period in a particular establishment. However, other studies find a high share of recalls in the German labour market (see Mavromaras and Rudolph, 1995).

Table 1 here

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 $S(t)$ gives 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.[4] In West Germany, 50 per cent of male employees have left their employer after about 900 days. In East Germany, exit

4 We use the cross-sectional weights included in the IAB Establishment Panel for the Kaplan-Meier-estimations.

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. However, at the long end of the distribution, which we cannot observe, the reverse could be true (i.e. longer job durations for men).

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 especially in job creation schemes in East as compared to West Germany.[5] This result is also found in Grotheer et al. (2004) and draws attention to the very different labour market conditions in East and West Germany.

Survival rates for employees with different education levels are presented in figure 2.[6] 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. Grotheer et al. (2004) find the lowest job durations within the youngest age group in West Germany and within the oldest age group in East Germany. This

5 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.). However, on the basis of the Socio-Economic Panel (SOEP) Boockmann and Hagen (2006, Appendix 1) report that more than twelve per cent of East German females work in fixed-term contract employment (as opposed to close to seven per cent in the West), and the share of publicly subsidised work in fixed-term employment is 33 per cent in the East and less than three per cent in the West.

6 In the following Kaplan-Meier graphs, we distinguish between West and East Germany but not between the sexes.

discrepancy could be due to the fact that we excluded employees in vocational training and individuals above 52 years from the sample.

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 point out 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 to 99 and more than 1,000 employees have the highest survival rates. In addition, the curves for firms with more than 100 employees become flatter after two years in East Germany.

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 can be seen from 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. A drop in the survival rate after one year in East Germany only occurs for individuals moving 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 establishments

Next, we analyse whether heterogeneity occurs across or within establishments by comparing the distribution of job durations of the whole sample with the mean of the distributions within the firms. Figure 6 shows a stylised example of why a difference may arise. In the first graph the density function of job durations estimated for all employees in the sample is shown.[7] The difference between the 10 per cent and the 90 per cent quantile is assumed to be 1,200 days. In the second graph, the density functions for two of the sample firms are drawn. It is assumed that all firms differ in average job durations but that the variance is the same. Therefore, the mean of the distance between the 10 per cent and the 90 per cent quantiles is 600 days. Hence, in this example the heterogeneity of job durations is explained to a large part by different average job durations across firms.

Figure 6 here

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 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 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 in order 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 firms. To overcome this problem, averages are calculated as follows:

7 The hypothetical curves are drawn for the sake of the example only, and do not represent the distribution of real job durations.

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

where $j = 1,...,m$ denotes establishments and the quantile difference $\overline{r}_{\alpha,10}$ is measured in days. Establishments 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). To ensure that spells enter with the same weights in the overall and firm-averaged quantiles, the average in equation (2) is weighted by the number of spells in the establishment, where n_α is the number of spells with duration $t \geq \hat{q}_{\alpha}$ and n_j is the number of spells in establishment j with duration $t \geq \hat{q}_{j\alpha}$.[8]

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-firm variation in job durations. Clearly, taking the average only over the establishments 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 firms 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 2 shows the 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 firm-specific quantiles. The numbers of establishments that reach the lower quantile are displayed in parentheses. For instance, the numbers in the first three rows and the first column

8 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.

imply that 406 days pass between the points of time when 10 and 50 per cent of all West German male workers employed in the 903 firms used for the analysis have left their job. By contrast, on average over these firms, only 315 days pass between the 10th and the 50th percentile. Thus, the within-sector dispersion is 22 per cent lower than the overall dispersion in the sample and a considerable part of the heterogeneity seems to be due to firm effects.

Table 2 here

However, the other results in the table do not support the proposition that within-firm 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 or 40 per cent quantiles are considered while the reduction is lower for the 30 per cent quantile. In one instance, the dispersion even increases if the average over establishments is taken. However, the reductions in dispersion are relatively low, ranging between two and 29 per cent.

To summarise, it appears that most of the heterogeneity in job durations is not due to individuals' employment in particular establishments. Rather, the overall dispersion of job durations is reproduced to a large extent in a similar dispersion 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 the coefficients of 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 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. The expression (4) is only calculated for periods in which a transition is actually made. The regression constant and the baseline hazard $\lambda_0(t)$ cancel out from the formula.

If censoring is independent, as we assume in the following, right-censored spells 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.).[9]

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

9 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.

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 $\lambda_j^0(t)$ 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 independence 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 joint distribution of person-specific and firm-specific unobservables and covariates in

order to obtain consistent estimates (see Abowd et al., 1999). A consistent estimator would require that the unobserved person-specific effects are distributed independently from the covariates and the firm-specific effects. This implies, for instance, 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.[10]

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,

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

- 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.

On the one hand, 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. Both would lead to shorter job durations. We expect foreign nationals from non-EU-countries to have shorter job durations than others because they are often employed in unstable jobs. Employment history may also be an important determinant of job durations but the interpretation of the effects is difficult. On the one hand persons who often change their jobs and are unemployed or out of the labour force in between could be stigmatized and therefore only receive offers for unstable jobs. On the other hand employment history is expected to be highly correlated with unobserved heterogeneity. Therefore, including this variable leads to an endogeneity problem which can be solved with fixed-effects estimations. Nevertheless, we use these variables to control for individual heterogeneity. We only condition on a small part of individual's employment history, namely the employment state before the

current spell. Finally, six broadly defined occupational categories are included in the specification.[11]

Controlling for firm-specific heterogeneity helps to reduce a bias in the coefficients of the individual-level variables resulting from 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 where employment can be adjusted within an internal labour market. The same applies to firms with various establishments where employees could change between them. Unfortunately we cannot observe those changes. 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 more frequently 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

11 On the basis of a Classification of Occupations provided by the Federal Statistical Office (Statistisches Bundesamt), all 369 occupations contained in the Employment Register have been accumulated so that 6 occupational groups remained (Statistisches Bundesamt, 1992).

more job stability.[12] On the other hand, a firm with collective agreements cannot compensate negative shocks by adjusting wages and therefore has to adjust employment. 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, familiar with these technologies, 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.[13]

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 3 to 6. 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

12 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.

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

consideration of firm-specific effects (subsection 5.1). In subsection 5.2 we discuss the influence of firm characteristics. The competing risks model, fourth and fifth columns in tables 3 to 6, is dealt with separately in subsection 5.3. We used sampling weights in all estimations. All specifications 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 determinant 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 (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.

Tables 3-6 here

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). One explanation may be 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 concerning job position with the results 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 non-EU 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. 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, with respect to endogeneity 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 agrees with the results of Mumford and Smith (2004) who find a positive effect of the local unemployment rate on average workplace tenure due to decreased quit rates. 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 Grotheer et al. (2004) who find that including the works council variable in the estimation leads to less clear firm size effects than are expected according to univariate analyses. 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.[14] Overall, we cannot find robust evidence for internal labour markets providing more job stability within a firm in the first six years of employment.

The age of the establishment is insignificant among West German men whereas new establishments have the lowest hazard rates among all other groups.[15] 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. The specification is comparable with the specification of the second column but the coefficients cannot be compared directly because in a competing risks model hazard ratios may not be interpreted qualitatively.

Table 7 here

In table 7, 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.

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

15 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.

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 similar to those obtained by Wolff (2004). 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 but again one should take the endogeneity problem into account. Employees who came from unemployment have a high probability to return to unemployment after relatively short time. Wolff (2004) yields the same results but Bender et al. (2000) 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. (2000) 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 show that it is important to perform 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 long and short employment spells coexist at the firm level. The reasons for this coexistence, however, need to be explored in further research. In particular, is there an endogenous segmentation of workers with similar characteristics, or do different job durations at the firm level arise due to different worker characteristics?

The coefficients estimated for 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 could be 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 of independence between 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. 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 lead 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.

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Table 1: Shares of destination and previous employment states (in per cent)

	Men		Women	
	West	East	West	East
Destination state				
Unemployment	10.63	31.82	12.16	35.94
Non-employment	5.45	3.99	6.23	2.62
New employer	15.73	12.17	13.04	6.84
Censored	68.19	52.01	68.57	54.60
Previous employment state				
Unemployment	21.56	49.03	20.73	49.53
Non-employment	6.13	4.12	6.17	2.38
Recall	18.62	12.00	26.45	22.48
Job-to-job transition	45.14	28.53	34.99	20.42
No observation	8.56	6.31	11.66	5.19

Table 2: Estimated interquantile ranges (in days)

Men	West			East		
	$\alpha = 50$	$\alpha = 40$	$\alpha = 30$	$\alpha = 50$	$\alpha = 40$	$\alpha = 30$
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		
	$\alpha = 50$	$\alpha = 40$	$\alpha = 30$	$\alpha = 50$	$\alpha = 40$	$\alpha = 30$
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.

Table 3: 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 3 (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 3 (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 4: 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 4 (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 4 (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 5: 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 5 (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 5 (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 6: 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 6 (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 6 (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.

Table 7: 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 Chi^2	29,245	11,742	17,689	10,855
P-values (degrees of freedom)	0.000 (68)	0.000 (63)	0.000 (67)	0.000 (62)

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

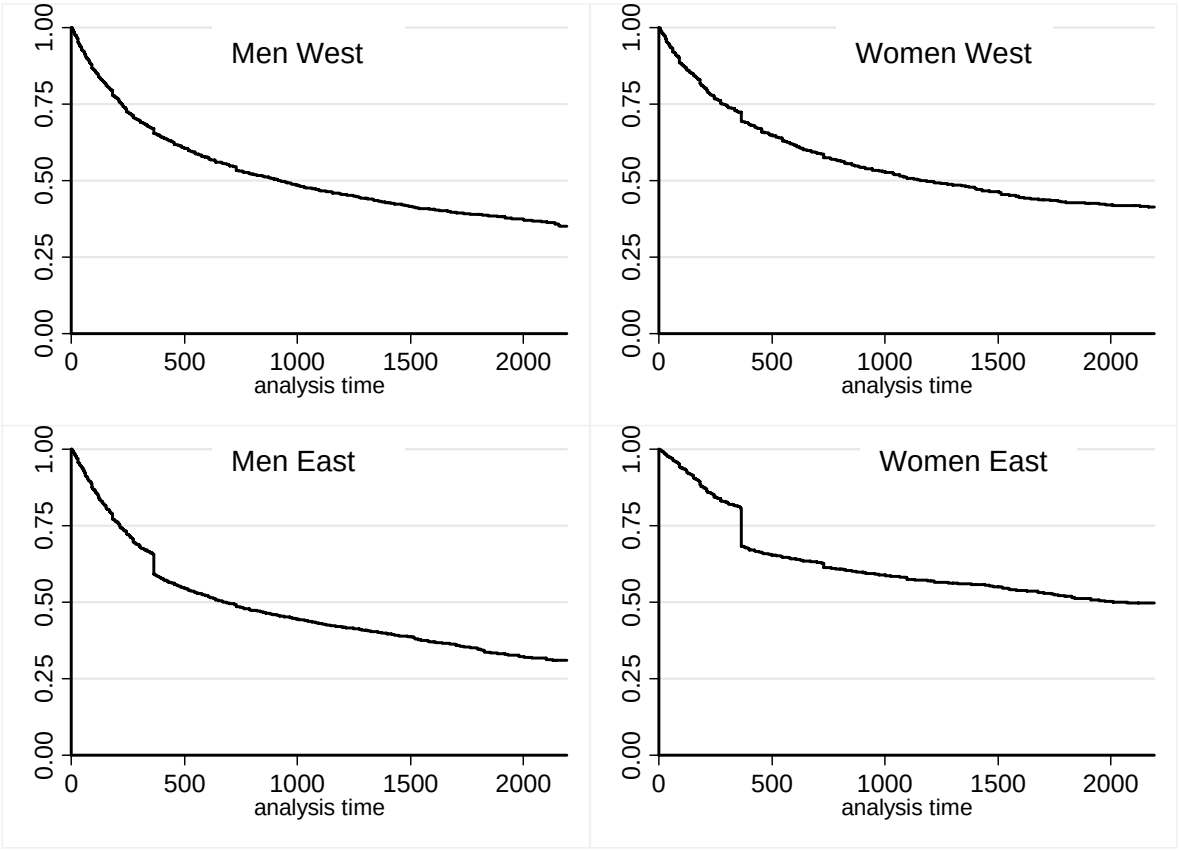


Figure 2: Kaplan-Meier-curves by educational level separated for West and East

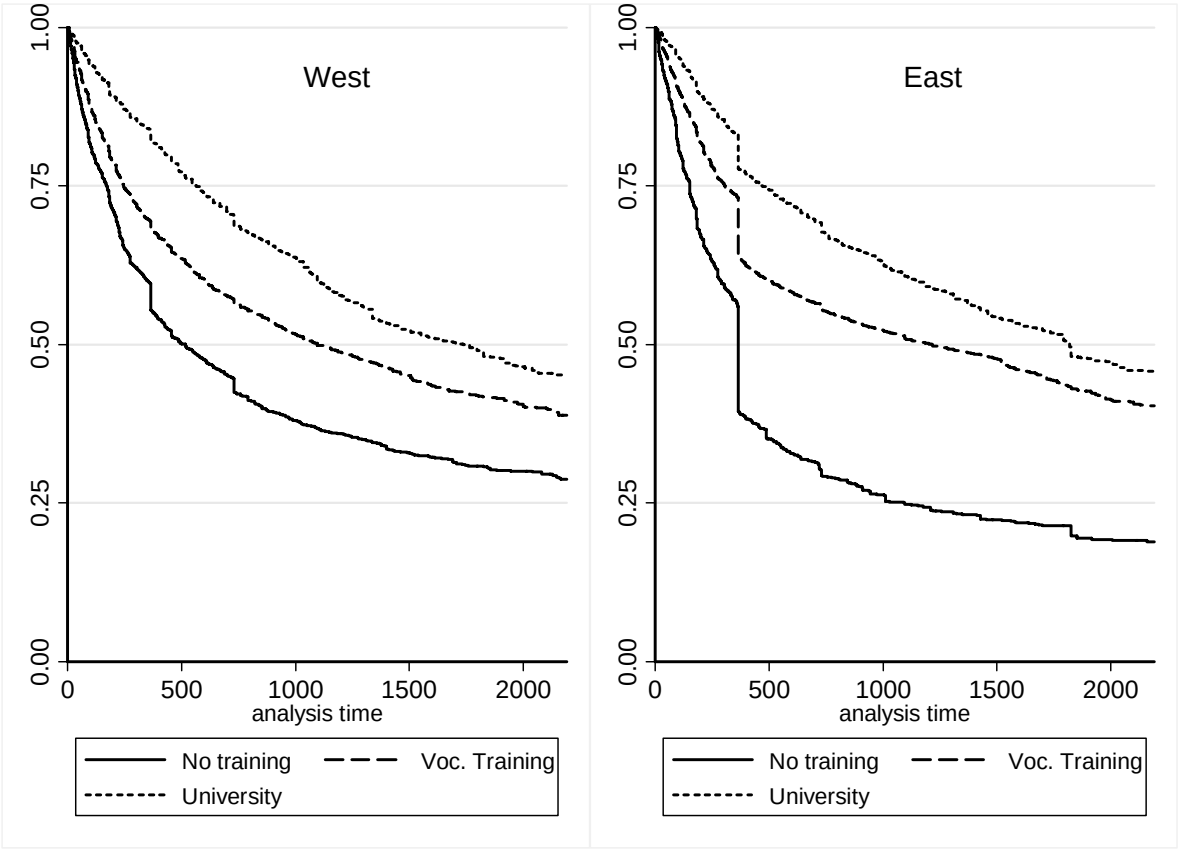


Figure 3: Kaplan-Meier-curves by age group separated for West and East

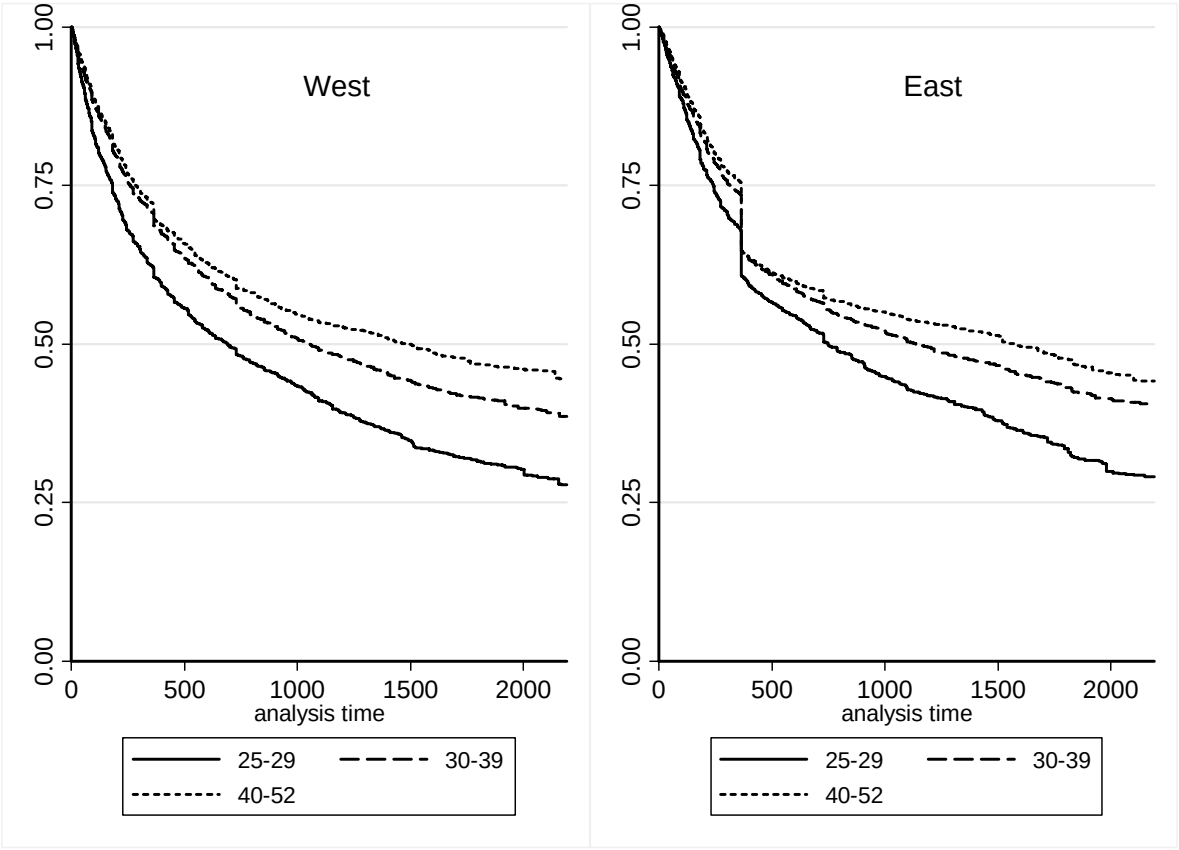


Figure 4: Kaplan-Meier-curves by firm size separated for West and East

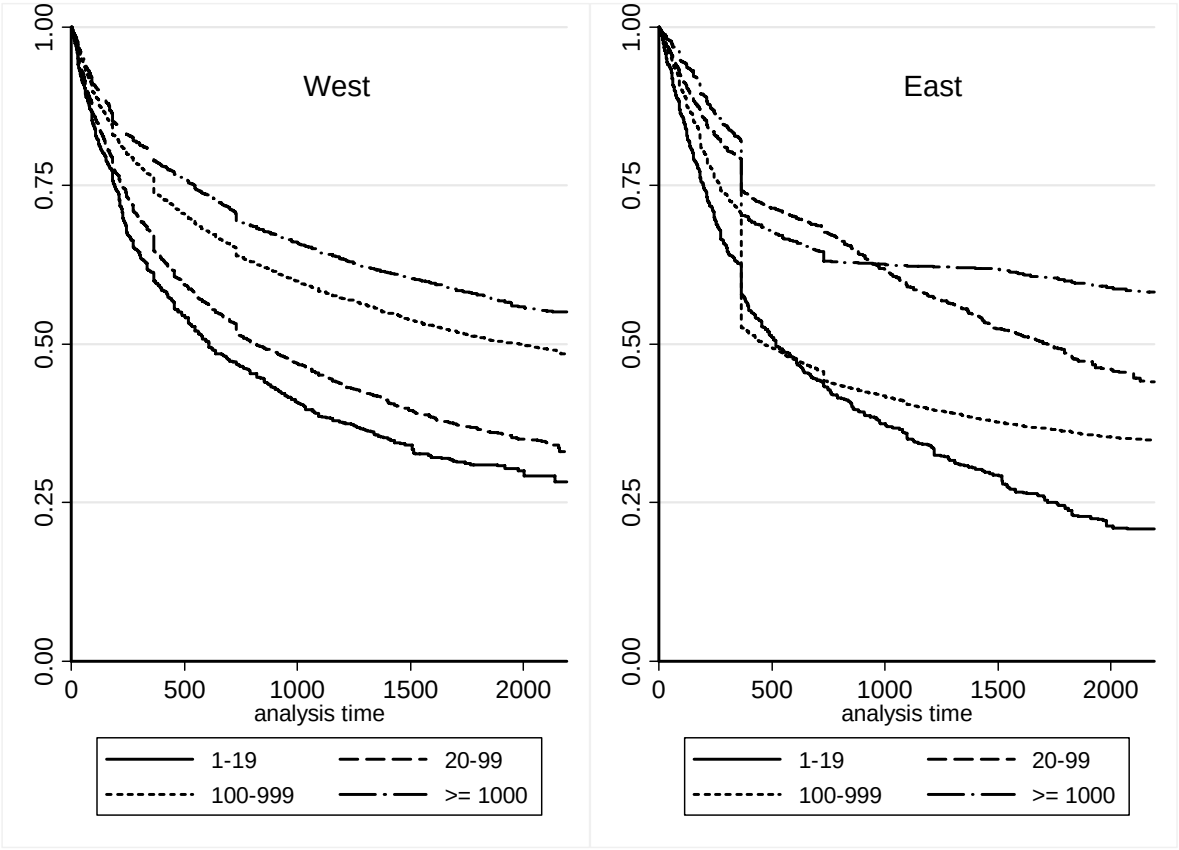


Figure 5: Kaplan-Meier-curves by destination state separated for West and East

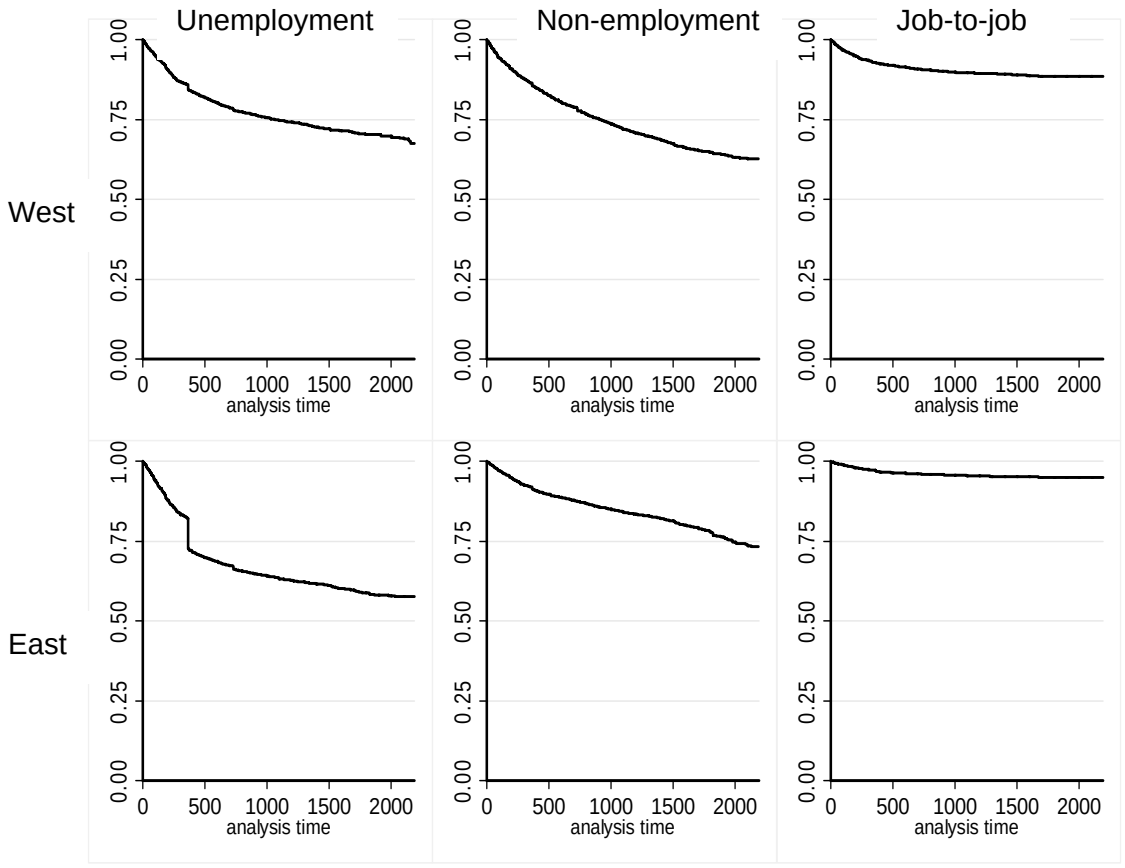
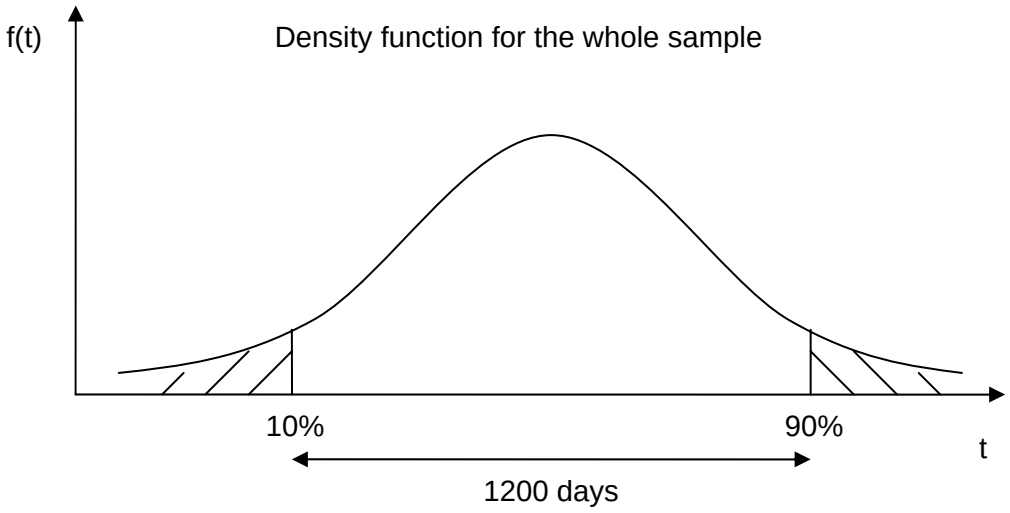
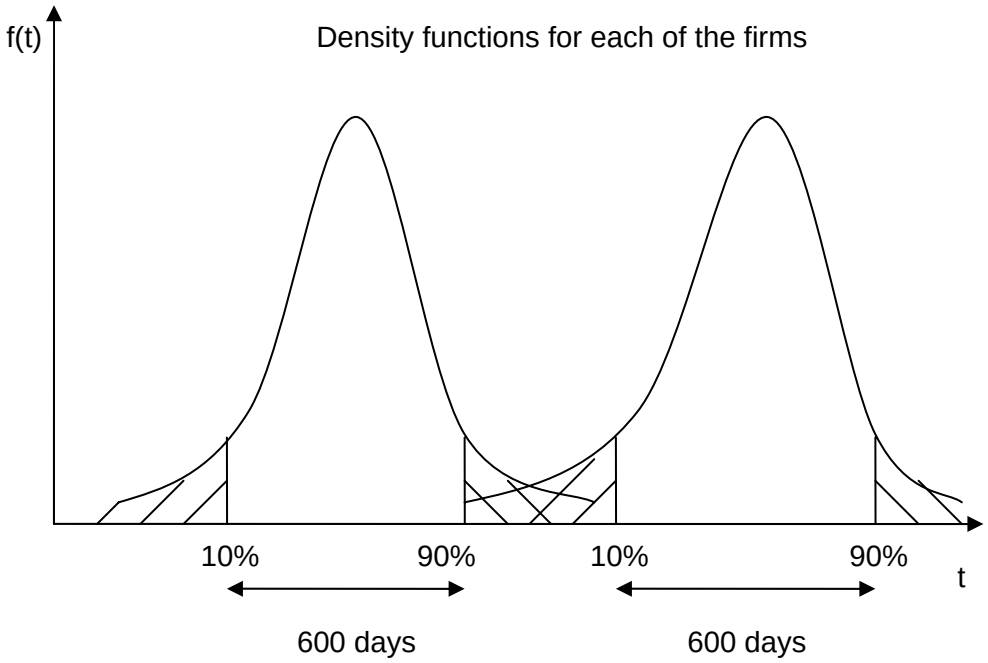


Figure 6: An illustration for interquantile differences at the firm level and for the whole sample





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