Individual and Plant-level Determinants of Male Workers' Job Durations in Germany

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Abstract: We examine job durations of German workers using a linked employer-employee dataset. The influence of firm characteristics and the question whether we find selection into certain firms are our main issues. There is some evidence for a sorting process of certain worker groups into firms with long or short job durations. Therefore, not only within heterogeneity but also across heterogeneity occurs. An 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. Within and across sectors, companies and groups of workers, employment spells differ vastly with respect to duration. A number of theories, such as search and matching theory or human capital theory, have been used and further developed in order to explain this heterogeneity. These theories determine job duration from the interaction of characteristics of labour supply and demand. For empirical work, this means that data for both workers and their employers should be used. Only recently, however, have linked employer-employee-data been made available to analyse job durations from both sides of the employment relationship.

In this paper we specify and empirically estimate a reduced-form model of job exit using a linked employer-employee dataset. We do not specifically test one of the theories mentioned above. Instead, our paper is motivated by two empirical questions. First, what is the relative importance of firm- and worker-level characteristics for job durations? While transitions from jobs depend on individual characteristics such as age or education, firms also differ with respect to the 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.

Second, how do worker and firm characteristics interact? Do certain types of workers select into firms with high or low employment stability? If the effects of individual characteristics change when controlling for firm heterogeneity, this is evidence for a sorting process which leads to *across* company heterogeneity in job durations. Institutions protecting insiders such as legal dismissal protection or works councils could, by contrast, give rise to dual labour markets *within* companies. In any case, analysing the determinants of job durations is likely to lead to biased results if either firm or individual-level characteristics are left out from the analysis (Margolis, 1996; Abowd and Kramarz, 1999; Bender and von Wachter, 2006).

Starting with the contributions of Burdett (1978) and Jovanovic (1979a), 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.

In the same context human capital theory was developed to explain positive duration dependence with the accumulation of firm specific human capital (Jovanovic, 1979b). Finally Mortensen (1988) analyses the link between the two theories. Since the mid-1980s, researchers have developed approaches to estimating the effects of tenure on wages and vice versa applying methods to solve the endogeneity problem (for an extensive survey see Altonji and Williams, 2005). While it would, in principle, be highly desirable to estimate a structural model of job changes, this comes at the cost of losing flexibility in accounting for left- and right-censoring, duration dependence and different exit states. In this paper we therefore model job exit by a reduced-form hazard rate model which does not include individual wages.

Since job durations are driven by the decisions of employers and employees, it is interesting to distinguish between quits and layoffs and other forms of exit. There is empirical evidence for differences in tenure according to the type of separation (Booth et al., 1999). Although there is no information on the reason for an exit in our data, we can approximate the distinction between voluntary quits and involuntary layoffs by the the state into which the worker moves. As costs of unemployment are high even despite substantial benefit replacement ratios, workers moving into unemployment can be assumed to be separated involuntary in most cases. Conversely, individuals moving from one job to another are likely to do so voluntarily in order to increase their utility in the job. While this approximation is, of course, not complete (in particular, workers losing their jobs involuntarily may be hired by another firm without an intervening spell of unemployment), we expect some interesting differences in person and firm specific effects according to different destination states.

The empirical literature analyzing the determinants of job durations can be divided into studies using stock or flow samples as well as into studies based on worker or matched employer-employee data. Most survey datasets are stock samples because workers are asked when their current job began. This leads to a selection bias because short employment durations are underrepresented. An advantage of stock data is that longer maximum durations are observed as compared to flow data, making it easier to investigate changes of job durations over time. Accordingly, most existing studies are based on stock samples (Battu et al., 1999; Bronars and Famulari, 1997; Dohmen and Pfann, 2003; Gerlach and Stephan, 2005; Mumford and Smith, 2004).

In order to investigate the effects of firm characteristics on individual job durations and the relationship between firm and individual characteristics employer information is needed. Therefore, linked employer-employee data are most suitable for this research issue. With administrative data containing a firm identification number and some firm characteristics or with survey data containing observations of employees working in several establishments the estimation of fixed firm effects is also possible (Bronars and Famulari, 1997; Bellmann et al., 2000). However, the effect of time-varying firm-level characteristics cannot be accounted for in this way.

In Germany, two representative linked employer-employee datasets are available in order to analyze job durations. The first is the Salary and Wage Structure Survey used by Gerlach and Stephan (2005) analyzing the impact of collective contracts on job tenure. The second dataset, the German Linked Employer-Employee Dataset of the IAB institute (LIAB), matches administrative data to an annual representative establishment survey. In this paper, we form a flow dataset of employment spells from this data. Bender et al. (2000) and Grotheer et al. (2004) use different versions of the LIAB in order to analyze the determinants of individual and firm characteristics on job tenure. However, they do not investigate the relationship between firm heterogeneity and individual characteristics. Additionally, their definitions of job durations differ substantially from our definition. Bender and von Wachter (2006) analyse the impact of the selection of trainees into special firms on later job careers. They use data drawn from the Employment Statistics Register which contain some firm characteristics.

Among studies for other countries, only Mumford and Smith (2004) and Dostie (2005) use matched employer-employee data to analyze job tenure and the relationship between individual and firm effects. The data used in the former study is a cross section of the British Workplace Employee Relations Survey linking information of workplaces and their employees. This dataset is a stock sample and contains no completed durations. Dostie (2005) uses a flow sample obtained from French administrative data and applies simultaneous estimations of tenure and wages.

Our data, taken from 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.

The remainder of the paper is structured as follows. In the next section we describe our data in detail and explain how job durations are obtained. In addition, some descriptive statistics are given in this section. Estimation methods and independent variables are introduced in section 3. 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 LIAB has recently become available to researchers at the Institute for Employment Research (IAB) (see Alda et al., 2005). It 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. 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. The second version contains daily employment and benefit recipient information for the period 1991 to 2001. Due to the longitudinal character of our research question, we use the second version in the present study.

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). The survey started in 1993 with more than 4,000 establishments (West Germany only), Eastern Germany was included 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 number of establishments in the LIAB longitudinal data is limited to establishments having valid interviews from 1999 to 2001. If an establishment has been interviewed in the years 1993 to 1998, the data is available, too. The fact that establishment information must be available up to the end of the sample period means that worker separations due to plant closures cannot be observed in the data. The LIAB longitudinal version contains 2,100 plants in both East and West Germany. For our study, we use only firms with continous information on all relevant items in the years from 1996 to 2001. However, if a firm was not interviewed in one of the years from 1996 to 1998, information from the closest available year was used instead.

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 certain variables such as education (see Fitzenberger et al., 2005). The data contains daily information on all employment relationships covered by the social security system. Other forms of employment are not recorded in the data; this concerns, in

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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).

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 data. As a whole, the Employment Statistics Register covers about 80 per cent of total employment. It 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). 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 on the employment status. With this in mind, complete employment biographies of employees covered by the social security can be constructed.

The LIAB longitudinal version contains employment history information since 1991 and until 2001 for all individuals employed for at least one day between 1996 and 2001 in one of the Establishment Panel firms. In order to avoid a left-censoring bias, we include all employees who began an employment relationship between 1996 and 2001 in one of the sample establishments. The employer data is matched with the (augmented) employment data using an establishment identifier.

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 report a break. 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, this information has to be generated. For our purpose additional information on the former employment state and on the destination state after the current spell is needed. In the following, we describe the method of how to define these variables in detail. For the sake of simplicity we start with the end of a spell and the definition of the destination states.

In our study, five destination states can be distinguished: unemployment, non-employment, change to a new employer, recall and no further information. The current

spell ends in a failure event if two conditions apply. First, the individual is observed to move to unemployment, non-employment, is hired by a new employer or returns to the current employer. Second, the current employer stated the end of the employment relationship in the report to the insurance institution. However, in case of the employee moving into unemployment and not returning to the current employer within 90 days after the termination of the current spell, we do not require that the end of the employment relationship has been reported by the employer. 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 report the end of the employment relationship).

In other studies using the same data, an end of the employment relationship is 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 suspect errors in the allocation of the identifiers by the local Federal Employment Agencies. Therefore, we additionally rely on employers' declarations.[2]

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 illness, parental leave or sabbatical and others. We define a separation and subsequent return to the same

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2   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.

employer within 90 days at most as a continuation of the current employment spell. This may be combined with periods of UI benefit receipt. Furthermore, if an employer reported a break of the employment we define the next spell (if it really is at the same employer) as a continuation of the current spell independently of the duration of the break.

If a person did not receive benefits, did not change to another employer within 60 days after the end of current employment and did not return to the current employer, 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 table 1, the definition of the destination states is summarized.

Table 1 here

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 for individuals who were not observed for at least one year before the start of the observation period on January 1st, 1996.

2.3 Sample definition

Before using the data for analysis, some further adjustments had to be made. 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 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 285,701 employment spells, of which 98,211 spells are from establishments in East Germany.

Table 1 in the Appendix contains the sample employment spells according to previous employment and destination states and other descriptive statistics. Due to the relatively short observation period, many spells are right-censored. In West Germany, the share is 61 per cent and in East Germany it is 43 per cent (due to overall shorter spells). In East Germany, 35 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. The share of recalls is equally low in East and West Germany (about 1 per cent).

2.4 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 firms. These estimations give some first insights into the duration distribution. Furthermore, we can obtain a first impression of possible differences between individual or firm specific characteristics.

Figure 1 here

In figure 1 Kaplan-Meier survival curves are presented for West and East Germany and separately for some individual characteristics.[3] In West Germany, 50 per cent of male employees have left their employer after about 900 days. In East Germany, exit occurs faster: the median duration is about 650 days. In other studies the median duration of new employment spells is between one and one and a half years (Erlinghagen, 2002; Grotheer and Struck, 2003). The fact that East German survival rates are lower than in West Germany confirms the findings of Grotheer et al. (2004) and Wolff (2004) whereas they exhibit median durations of more than two years (due to data structure). Whereas the curve of West Germans is relatively smooth, the East German one exhibits a fall after exactly one year. This is due to the higher incidence of temporary employment especially in job creation schemes in East as compared to West Germany.[4] This result is also found in other German studies and draws attention to the very different labour market conditions in East and West Germany.

Next, we look at the survival curves for different age groups which show that younger workers exhibit shorter job durations. This confirms the findings of other studies (Grotheer et al., 2004; Grotheer and Struck, 2003). Although highly qualified workers are often expected to be more mobile we as well as a lot of international studies find higher mobility rates for unskilled workers (see for instance Holzer and Lalonde, 1999 for the USA; Dustman and Meghir, 2005 for Germany; Naticchioni and Panigo, 2004 for Italy).

In figure 2, Kaplan-Meier survival curves are drawn separately for firm characteristics. Whereas in West Germany survival rates are higher for larger

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

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

establishments, for East Germany the curves are crossing several times. Furthermore, we have information whether a firm participates in a collective agreement at the sector-level at the firm-level or not. In West Germany workers in firms with collective agreements have longer job durations independently of the level. In East Germany workers in firms without agreement have the shortest job durations. But there is a higher fall in the survival curve after exactly one year for individuals working in firms with firm-level agreements in comparison to others. If our suspicion is right and these workers are participants of active labour market programs, these jobs are mainly offered by firms with firm-level agreements. Firms with sector-level agreements exhibit the longest job durations. As we expected the unconditional effect of a works council is also clearly positive. However, the effects of firm-size and bargaining as well as codetermination on tenure could be highly correlated.

Figure 2 here

3 Estimation technique and independent variables

3.1 Estimation method

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 model is called a proportional hazard model because the baseline hazard is assumed to be shifted proportionately by the covariates. The independent variables are assumed to be exogenous with respect to the transition process.

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.).[5]

Although we include some firm characteristics in our model, firm-specific unobserved heterogeneity could also lead to a bias in estimation results. Hence, we account for firm-specific unobserved heterogeneity by stratifying the sample according to establishments in some of the estimations. The principle of the stratified partial likelihood estimator is to decompose the total likelihood into several sub-likelihoods specific for each stratum (Kalbfleisch and Prentice, 2002: 118f.; Lancaster, 1990: 268ff.; Ridder and Tunali, 1999). This means that a separate baseline hazard is assumed for each establishment, whereas the estimated parameters are assumed to be the same over all establishments. In this case, the coefficients of time-varying firm-specific variables are identified while those of time-constant variables are not identified.

While we allow for unobserved firm-specific effects in the stratified estimation, we do not include individual-specific fixed effects. 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

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5   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.

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 states. 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.). 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.[6]

3.2 Specification

In order to analyze individual and firm specific effects on tenure and the relationship between them, we estimate a specification with individual characteristics only and one which also includes firm characteristics. From the Employment Statistics Register we have demographic information (age, highest education, nationality) and job-level information (job position, occupation and daily wage). The wage is expected to have a positive impact on employment durations since reservation wages, and thus the probability to exit, increase with the current wage level. From human capital theory and empirical research, it is known that job durations also exhibit a positive impact on wages (see for instance Topel and Ward, 1992). Therefore, we exclude the wage information in order to avoid this endogeneity problem. From the Employment Statistics Register we are able to derive employment history information. We do not use the entire employment history since we only have data from 1991 onwards but we use the former employment state. On the firm side, we include information about the firm size, collective bargaining, works council, the age of the entity, the legal form, further training, investments in information and communication technology (ICT) and the sector. All these characteristics are expected to influence tenure and are available for all interviews. We also include year dummies and regional information like the federal

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

state and the local unemployment rate. In a further specification we stratify on the firm-level in order to control for establishment fixed effects. With the comparison of the first and the latter specification we are able to analyze the selection process of certain worker groups into certain firms (see Mumford and Smith, 2004). For instance, if the effect of high education is positive and quantitatively high in the first specification but lower or insignificant in the latter we can state a selection of high educated workers into firms with long job durations. Since we are interested in differences between person and firm specific interests according to the destination states, we include all variables in the competing risk specification and therefore do not control for firm fixed effects here. For a better overview we summarize all specifications in the following:

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

In table A1 in the appendix, the number of observations and failures as well as means and standard deviations of all covariates used in the estimations are listed. There are some structural differences between West and East Germany concerning age and education at the individual level and firm size at the firm-level. The rich pool of person and firm specific variables opens great possibilities for the estimation of the determinants of employment duration.

4 Empirical Results

In table 2 the results of the conventional Cox estimations are presented for East and West Germany. Table 3 shows own calculated hazard ratios of the works council effect on job position and firm size. In table 4 results of the competing risks model are shown. First of all, we discuss estimates of coefficients of individual and job specific variables and their robustness according to the consideration of firm specific effects (subsection 4.1). In subsection 4.2 we discuss the influence of firm characteristics and in subsection 4.3 the interaction effects are analysed. We tested the jointly significance of all interaction groups with likelihood-ratio tests and present the respective Chi-squared distributions table 5. In subsection 4.4 the competing risks model is dealt with. 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. Since

our focus is on job durations we state negative effects if tenure is shorter in comparison to the reference group (hazard ratios are significantly above 1). In order to analyse the hazard ratio of one group in comparison to another group (not the reference group) one has to divide the latter hazard ratio into the former.

4.1 Coefficient estimates for individual and job specific characteristics
Job-shopping theory suggests that young people move more frequently than older individuals because younger workers acquire information about their abilities while searching for a better match (Johnson, 1978; Viscusi, 1980). For the multivariate analyses, we generated age intervals to take non-linear effects into account. As expected, hazard ratios decline with age in West Germany. However, this is not the case in the East where only the oldest group exhibit slightly longer job durations. These findings, 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 in East Germany. They could, however, also reflect a higher risk of unemployment at older ages. In general, coefficients are only slightly influenced by the inclusion of firm variables or firm fixed effects.

Table 2 here

According to human capital theory we expect highly educated workers to have more general human capital and thus to be more mobile. On the other hand, more skilled workers are more able to acquire firm specific human capital which, in turn, delays job changes. Furthermore, there is empirical evidence for a selection of low-skilled employees into unstable jobs. Results for West Germany 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).[7] Additionally, we find some sorting effects of workers with A-level plus vocational training and with university degree into firms with long job durations.[8] The findings are in contrast to Battu et al. (2002), Gerlach and Stephan (2005) and Mumford and Smith (2004) who find shorter job durations for high-skilled employees. However, they analyse the effects of very long job durations whereas our observation period covers only six years. Dostie (2005) also finds a positive effect of years of schooling on job tenure. In East Germany, education does not play a crucial role concerning job durations. Only workers with vocational training are found to select themselves into establishments with short durations.

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7   Due to the inclusion of interactions of the education dummies with a dummy for further training, we discuss the effects of the second column in subsection 4.3.

8   We observe a selection process if an effect is significant in the first specification but is insignificant or diminishes in quantity in the stratified model (or vice versa).

Employment history may also be an important determinant of job durations but the interpretation of the effects is difficult. Lagged employment states could be endogenous since employment history is likely to be highly correlated with unobserved characteristics. Therefore, we use these variables to control for individual heterogeneity. As expected, the previous employment status seems to matter a lot for job stability (see also Booth et al., 1999 and Battu et al., 2002). Individuals who started their job from unemployment or non-employment exhibit a significantly higher job exit rate in comparison to the reference group (no observation). However, in the stratified model the effects become insignificant for West Germans. Surprisingly, workers who have been recalled to their previous employer exhibit the lowest hazard rates. Unfortunately, we cannot observe what they did meanwhile but it seems that we successfully excluded breaks due to seasonal work. Workers who move from job-to-job exhibit shorter job durations than the reference group but there is a sorting effect in East Germany.

Concerning job position, the reference group is unskilled blue collar workers. Skilled blue collar workers exhibit significantly lower job exit rates but in West Germany there is a sorting process of this group into firms with low job durations.[9] Although white collar workers exhibit significantly lower hazard rates in all specifications and regions they seem to select themselves into firms with high job durations. Finally, controlling for unobserved firm heterogeneity the durations of skilled blue and white collar workers are similar and clearly longer in comparison to the reference group. Master craftsmen exhibit the longest job durations in East Germany but for West Germany the effect is not significant in the stratified model. Men who are part-time workers have shorter job durations than full-time workers. The findings concerning the job position are consistent with the results of Gerlach and Stephan (2005) and Mumford and Smith (2004).

With respect to occupation and nationality we find partly high selection effects of certain groups into firms with high or low job durations. 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.[10] However, the local labour market does not have an impact on job durations in the stratified model.

Summing up we can state that there is heterogeneity in job durations which can be explained by individual and job specific characteristics. However, comparing the results with and without firm fixed effects, we find that accounting for these effects is important for the impact of a number of individual-level variables on tenure. This fact implies two important things: firstly, estimation results in which firm effects are not

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9   Again, due to the inclusion of interactions of the job position dummies with a dummy for works council, we discuss the effects of the second column in subsection 4.3.

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

taken into account are probably biased; secondly a high amount of heterogeneity in job durations are covered by firm fixed effects.

4.2 Coefficient estimates for firm specific characteristics

Controlling for firm specific heterogeneity helps reducing the bias in the coefficients of the individual-level variables resulting from selection into firms with long or short employment durations. Only some of the firm specific characteristics are included in the stratified specification, since only coefficients of time-varying variables are identified in this model. Furthermore, like in the previous section some characteristics are also interacted and therefore should be discussed jointly with the interaction effects. The covariates further training, works council and firm size are affected by this.

Investments into ICT often imply the necessity of special training which should lead to longer job durations due to investments in firm specific human capital of the workforce. Additionally, investments may reflect high profits, which in turn could lead to fewer separations. On the other hand, investments could also lead to higher turnover rates if older employees are not able (or not willing) to operate with new technologies and new employees, familiar with these technologies, are hired. Our results show that the influence of investments into ICT comes out consistently positive.

We have very detailed information about collective bargaining and, therefore, are able to discuss different interesting influences. 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. Gerlach and Stephan (2005) expect collective contracts to have a positive impact on job tenure because higher wages are an incentive for employees to stay with the firm. The firm anticipates this and invests more in firm specific training which again leads to more job stability. On the other hand one could suspect that a firm with collective agreements cannot compensate negative shocks by adjusting wages and therefore has to adjust employment. Therefore, tenure should be shorter in firms with sector-level collective bargaining at least if there is a negative shock. A firm paying wages above the collectively set level has some leeway of reducing them in bad times instead of reducing employment and, therefore, tenure is expected to be longer in these firms.[11] With respect to region, there are mixed results on the effects of collective agreements on job stability. In East Germany, employees exhibit longer durations if they are employed in firms with sector-level agreements. This confirms the expectations

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11 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.

of Gerlach and Stephan (2005). The change from no or a sector-level agreement to a firm-level agreement has a positive effect on job durations which in turn confirms our hypothesis. In West Germany such a change leads to shorter job durations. The difference between East and West Germany is due to structural differences in the collective bargaining behaviour of establishments. In their empirical analysis, Gerlach and Stephan (2005) find that workers in Lower Saxony have significantly higher job durations in firms with collective contracts. Moreover, 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 bias.

The legal form of the establishment is also of primary importance. In particular, public corporations and other legal forms like associations (the second only in West Germany) have far lower job exit rates than enterprises under private proprietorship. This could indicate differences in personnel policy according to the necessity of competitiveness. 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. However, the impact of the establishment's age is insignificant among West German men whereas new establishments have the lowest hazard rates in East Germany.[12] 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.

4.3 Interaction effects

According to Lazear (2003), firms invest in firm specific human capital but not in general human capital. For this reason, the firm has a crucial interest to retain this capital and to reduce quits. Hence, we expect positive effects of further training on tenure. Importantly, this effect should be higher for skilled workers if they are more able to acquire firm specific human capital. Unfortunately, we cannot observe whether an individual obtains further training. We only have the information whether a firm generally offers further training. In order to analyse the impact for certain worker groups we included interactions of the education variables with a dummy for further training. The overall effect of vocational training is not significant in the second specification. In the stratified model the effect is significantly negative in East Germany. Therefore, firms who introduce further training exhibit shorter tenure which is in contrast to our theoretical expectations. Concerning the specification with interactions we find that in West Germany, further training increases exit rates among workers with A-level but no vocational training. In East Germany we find the opposite. Furthermore, in East Germany workers with vocational training exhibit longer tenure in

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12 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.

firms with further training. All other variables are not significant indicating that further training has a low impact on individual job durations.[13]

Table 3 here

The presence of a works council is expected to lead to longer employment durations due to the fact that one of a works council's legal competences concerns dismissal procedures. Moreover, a 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). In a specification without interaction effects (not shown here) 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). We want to go a step further and answer the following question: which worker groups and which firms are affected by a works council? If for instance a works council mainly represents blue collar workers the exit voice function should be higher for this group. Therefore, we included interactions of the job position dummies and the works council dummy. Additionally, we did the same with the firm size dummies because almost all large firms in Germany have works councils and thus the covariates could be correlated. Since we are interested in the impact of a works council on a certain worker or firm size group we calculated the effects and present them in table 3. Due to some insignificant coefficients the calculated effects should be interpreted carefully.

The overall effect of a works council remains significantly positive in West Germany but is insignificant in East Germany including interactions. We find that a works council has a negative effect on tenure for white collar and part-time workers, the former effect being quantitatively low in West Germany. In East Germany the impact of a works council on blue collar workers is significantly positive. This is a notable result since the exit voice function seems to be valid for blue collar workers but not for white collar and especially not for part-time workers.

Generally, 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. Our results show that we cannot support the hypothesis "the larger the firm, the more stable the jobs". This is in contrast to Dostie (2005) who uses the capital stock as a proxy for firm size and finds positive effects on tenure. For West Germany most of the effects are significant but the sign changes a few times from one category to the next. Obviously, there is no monotone

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13 In a specification without interaction effects further training has positive effects in West and East Germany.

direction, neither for firms with works councils nor for firms without works councils. However, in both regions we find positive and quantitatively large effects of works councils on very small and on large firms. Moreover, we find significantly negative effects of small and large firms (with at least 300 employees) without works councils. Overall, we cannot find robust evidence for internal labour markets providing more job stability within a firm in the first six years of employment. Grotheer et al. (2004) find that including the works council variable leads to less clear firm size effects than are expected concerning the univariate analyses. Moreover, Mumford and Smith (2004) cannot find significant effects of firm size on the average tenure of a workplace in Great Britain. Our findings turned out to be more precise but still an interpretation is difficult.[14]

4.4 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 four destination states: unemployment, moving to another employer, recalls and non-employment. We display only results for the first two destination states. The specification is comparable with the specification of the second column of table 2 but the coefficients cannot be compared directly because in a competing risks model hazard ratios may not be interpreted qualitatively. In table 5, 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.

Table 4 and 5 here

The impact of age differs a lot between the destination states. While there are no significant effects for West Germans moving into unemployment, hazard ratios are significantly higher for East German men between 45 and 52 years in comparison to the youngest age group. This is probably due to the tight labour market in East Germany where reemployment chances of older workers are bad. The hypothesis "the older the employees the lower the job exit hazard" only holds for workers moving directly to

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

another employer. These findings are similar to those obtained by Wolff (2004) and confirm the job shopping theory of Johnson (1978).

Like in the estimations with one exit type there are as good as no raw education effects. We only find West German workers with vocational training and A-levels to be more protected against unemployment in comparison to all other workers especially those with vocational training. Curiously, further training has the opposite effect on those two education groups. In Germany vocational training was traditionally the professional education for young persons with O-level. In the recent years more and more persons with A-levels applied for vocational training. Generally they select themselves into special, mainly commercial, occupations displacing persons with O-level. Our findings could be an indicator for this selection process. Furthermore, Bender and von Wachter (2006) find a selection of less able apprentices into firms with low retention rates. Although they find the resulting wage loss to be caught up after a few years we possibly observe this initial sorting process. In the East there are no significant education effects and again we find no differences between high educated (university) and low educated employees.

The influence of the recent labour market state on tenure strongly depends on 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 a relatively short time. Wolff (2004) yields the same results but Bender et al. (2000) cannot find a significant impact on previous unemployment. Unexpectedly, employees who change into unemployment exhibit longer tenure if they had a recall. The impact of employment history on job-to-job transitions is less clear. While employees who even had been employed with the current employer have significantly lower hazard rates there are no significant effects for workers coming from another employer. Obviously, there is a special group of workers staying with their employers a long time after a break. More work should be done on the question which characteristics they have and in which firms they are employed. Conditioning on other covariates such as age and qualification, 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.

A high job position reduces the hazard rate of workers moving into unemployment but only in West German firms without a works council. There is no works council effect on workers moving into unemployment with respect to the job position. In East Germany skilled blue and white collar workers exhibit positive works council effects when exiting into unemployment. In contrast to this, part-time employees moving into unemployment are negatively affected from works councils in East Germany. This group also exhibits shorter tenure in firms with a works council if they move from job-to-job (in both regions). The same happens to white collar workers. The overall works council effect is positive for job-to-job changes but not significant for

changes into unemployment. Hence, there is no support for the exit voice function concerning a certain worker group but there could be a protection effect of works councils for East German blue and white collar workers.

In East German regions with high unemployment rates, job durations of men moving into unemployment are longer. Furthermore, we cannot find any significant differences between the destination states.

We expect some differences in the impact of collective agreements according to destination states. For instance there should be a negative impact of firm-level agreements in West Germany on exits into unemployment because a lot of firms changed the level in order to overcome economical problems. On the other hand, there could be a negative effect for job-to-job transitions in those firms because workers search more on the job due to lower wage increases. However, we only find two significant effects. In West Germany workers moving into unemployment exhibit longer job durations if the firm pays more than the tariff wage. In East Germany this only happens to workers with a job-to-job change in firms with sector-level agreements

Referring to the legal form the results show that the positive impact of public corporations and other legal forms is only significant for job-to-job transitions. The age of the establishment has no impact in West Germany but in East Germany job durations for those moving into unemployment are longer in very old firms. For job-to-job transitions tenure is shorter in these firms.

The impact of the firm size differs sometimes but not always according to the destination state. Moreover, the expected works council effect is not constant over the firm size groups and is even negative in some cases.

5 Conclusions

In this paper, we use a new linked employer-employee dataset to analyse the individual and firm specific determinants (and their relationship) of male worker`s job durations in Germany. Due to the flow sampling scheme and the relatively small time dimension of the data, we restrict the analysis to job durations of a maximum of six years. We are able to include a large number of firm-specific covariates and results show that almost all firm characteristics contribute to explain individual job durations. Moreover, we include interaction effects, which was not done before. A special focus of this paper is on the question whether we can observe a sorting mechanism of employees with certain characteristics into firms with long or short tenure. While Mumford and Smith (2005) using a stock sample also discussed this selection process our flow sample data seem to be more appropriate since short durations are not underrepresented. Furthermore we focus on the differences in the determinants according to the destination state of an exiting worker.

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 certain groups could be affected by persistence of unemployment or non-employment. We cannot find any effects of further training on job durations of low or high skilled while the medium skilled are strongly affected. In this context we find remarkable differences between workers with vocational training and A-levels and workers with only vocational training. Although job shopping seems to be popular across younger workers, there is no strong evidence that high qualified employees are more mobile than medium qualified. Among firm characteristics, institutional variables such as the firm's legal structure and the adherence to collective agreements, matter for job durations. Contrary to the descriptive evidence but in accordance with other studies, firm size effects are not monotonous. Additionally, the overall expected positive effects of a works council are negative for some firms. These effects also differ according to the destination state indicating that works councils carry out different functions concerning separation decisions.

While determinants of job durations differ between East and West Germany there seem to be sorting mechanisms according to job position, nationality, occupation and education in both regions. For instance a selection of blue collar workers into long tenure firms and the opposite for white collar workers can be observed. The competing risks framework shows that mobility to another job and exit to unemployment follow strikingly different processes. This is valid for individual as well as for firm characteristics.

All theory approaches explaining job durations take employees and employers decisions into account. Since linked employer-employee data have been only recently available in most empirical studies only individual characteristics are considered. With this study we contribute to the question whether firms differ according to their separation decisions and which are the characteristics which influence them. Implications for future research can be drawn from this. For instance, in order to test the impact of job and matching theory as well as human capital theory on job durations in a structural model firm specific covariates should be taken into account.

Additionally, in further research some of the assumptions that were made in our estimations should be relaxed. Most importantly, the assumption of independence between individual and firm-level heterogeneity should be avoided. 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 further issue is that covariates may affect exit probabilities differently at different durations. For instance, is the effect of a works council higher for longer or for shorter job tenure? These questions could be analyzed using a quantile regression approach.

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Table 1: Definitions of destination and origin states

Employment state	Definition
a) destination states	
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 and has recorded end of relationship
Job-to-job change	takes up employment with another employer within 60 days after separation and has recorded end of relationship
Recall	takes up employment with the same employer after more than 90 days after separation and has recorded end of relationship
b) origin states	
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	not observed since January 1st, 1995

Table 2: only X's, X's and Z's, Stratified: Results from Cox estimation

	West Germany			East Germany		
	X's	X's & Z's	Stratified	X's	X's & Z's	Stratified
Age in years (reference group: 25-29)						
30-34	0.883	0.893	0.875	0.952	0.965	0.913
	(2.99)	(2.55)	(3.67)	(0.96)	(0.67)	(2.02)
35-39	0.826	0.853	0.853	0.980	1.027	0.908
	(4.46)	(3.54)	(4.14)	(0.42)	(0.51)	(1.99)
40-44	0.810	0.826	0.763	0.944	0.989	0.903
	(4.42)	(3.92)	(6.26)	(1.19)	(0.21)	(2.27)
45-52	0.654	0.672	0.691	0.912	0.971	0.893
	(8.56)	(7.46)	(8.44)	(2.08)	(0.64)	(2.70)
Education (reference group: no professional training)						
Vocational Training	0.868	1.035	0.826	0.923	1.144	0.828
	(3.44)	(0.47)	(4.88)	(1.69)	(1.18)	(5.03)
Voc. Training & A-Level	0.828	0.720	0.924	1.242	1.531	0.970
	(2.52)	(1.21)	(1.16)	(1.97)	(1.71)	(0.34)
University	0.696	0.759	0.810	0.938	0.954	0.896
	(5.99)	(1.45)	(3.66)	(0.93)	(0.29)	(1.64)
Previous employment state (reference group: no observation)						
Unemplomyent	1.143	1.149	0.962	1.476	1.425	1.282
	(2.51)	(2.36)	(0.76)	(5.58)	(4.38)	(3.25)
Non-employment	1.229	1.239	1.104	1.235	1.316	1.268
	(3.3)	(3.18)	(1.65)	(2.38)	(2.78)	(2.62)
Recall	0.498	0.586	0.567	0.391	0.467	0.602
	(8.65)	(6.61)	(8.02)	(10.16)	(7.58)	(5.46)
Job-to-job change	0.646	0.725	0.721	0.646	0.804	0.913
	(8.50)	(5.81)	(6.83)	(6.20)	(2.72)	(1.20)
Nationality (reference group: German)						
EU citizen	1.073	1.162	1.090	2.189	2.117	1.663
	(0.78)	(1.73)	(1.04)	(3.95)	(3.57)	(2.72)
No EU citizen	1.298	1.329	1.057	1.741	1.722	1.244
	(5.09)	(5.59)	(1.21)	(8.82)	(6.93)	(2.11)
Job position (reference group: no professional training)						
Skilled blue-collar	0.980	0.857	0.781	0.744	0.855	0.772
	(0.48)	(2.59)	(5.47)	(8.12)	(3.11)	(5.66)
White-collar	0.682	0.654	0.798	0.495	0.638	0.724
	(7.19)	(5.10)	(3.84)	(13.10)	(4.97)	(5.25)
Master craftsman	0.806	0.744	0.872	0.444	0.517	0.580
	(1.92)	(2.20)	(1.11)	(6.90)	(3.66)	(5.18)
Part-time worker	1.187	0.906	1.234	1.099	0.901	1.097
	(2.18)	(0.61)	(2.93)	(2.88)	(2.62)	(1.99)

Table 2 continued

Occupation (reference group: service workers)						
Production workers	0.900	1.040	1.077	1.203	1.303	1.091
	(2.75)	(0.80)	(1.40)	(4.21)	(5.43)	(2.22)
Technicians	0.869	0.950	0.958	0.983	0.951	0.956
	(2.73)	(0.89)	(0.81)	(0.27)	(0.73)	(0.78)
	0.689	0.780	2.160	1.523	1.462	1.290
	(0.87)	(0.64)	(4.35)	(5.73)	(5.14)	(4.12)
Others	(5.09)	(5.59)	(1.21)	(8.82)	(6.93)	(2.11)
	(8.5)	(5.81)	(6.83)	(6.20)	(2.72)	(1.20)
Calendar time (reference group: 1996, 2001)						
1997	0.934	0.831	0.921	0.847	0.818	0.822
	(1.4)	(3.58)	(1.78)	(3.28)	(3.57)	(3.94)
1998	0.951	0.893	1.030	0.700	0.665	0.797
	(0.57)	(1.25)	(0.30)	(6.13)	(6.65)	(3.87)
1999	0.966	0.924	1.055	0.866	0.786	0.919
	(0.46)	(1.01)	(0.63)	(2.58)	(4.15)	(1.54)
2000	1.114	1.086	1.080	0.946	0.940	0.929
	(1.7)	(1.27)	(1.09)	(1.20)	(1.32)	(1.80)
Local labour market						
Unemployment rate	0.973	0.941	0.905	0.915	0.962	0.979
	(0.41)	(0.90)	(1.29)	(3.44)	(1.34)	(0.83)
Investments in (reference group: no investment)						
ICT		0.920	0.880		0.887	0.893
		(2.32)	(2.89)		(3.65)	(2.94)
Further training		0.995	0.974		1.184	1.183
		(0.06)	(0.40)		(1.46)	(3.32)
Collective agreements (reference group: no collective agreement)						
Sector-wide		0.934	1.070		0.917	0.936
		(1.56)	(0.92)		(2.71)	(1.23)
Firm-level		1.054	1.283		0.954	0.882
		(0.86)	(2.37)		(1.21)	(2.47)
Wages > tariff		0.890	0.886		0.960	0.819
		(3.40)	(2.32)		(0.94)	(3.20)
Works council		0.748			0.948	
		(4.64)			(0.89)	
Legal form (reference group: individual firm)						
Partnership		0.989			1.027	
		(0.15)			(0.26)	
Private limited company		1.110			1.119	
		(1.70)			(1.91)	
Public limited company		1.060			0.938	
		(0.80)			(0.45)	

Table 2 continued

Public corporation	0.666 (4.26)	0.695 (4.88)
Other (e.g. association)	0.742 (3.39)	1.059 (0.83)
Firm size (reference group: 100-199 employees)		
≤19	1.267 (3.75)	1.196 (3.38)
20-49	1.020 (0.36)	0.909 (1.94)
50-99	1.281 (4.92)	1.029 (0.69)
200-299	0.861 (2.30)	1.015 (0.43)
300-499	1.425 (4.84)	1.142 (3.21)
500-999	0.923 (0.95)	1.221 (5.37)
≥ 1000	1.024 (0.24)	1.236 (4.56)
Year of setting up (reference group: 1981-1990)		
≤ 1980	1.028 (0.54)	0.989 (0.49)
1991-1995	0.918 (1.41)	0.961 (1.17)
≥ 1996	1.102 (1.09)	0.772 (5.28)
Interactions with works further training dummy		
A-levels	2.191 (9.66)	1.680 (4.2)
Vocational Training	0.863 (1.91)	0.753 (2.36)
Voc. Training & A-Level	1.428 (1.3)	0.711 (1.28)
University	1.140 (0.68)	0.919 (0.51)
Interactions with works council dummy		
Skilled blue-collar	0.918 (1.34)	0.670 (6.75)
White-collar	1.195 (2.45)	0.717 (4.25)
Master craftsman	0.765 (1.37)	0.760 (1.29)

Table 2 continued

Part-time worker	1.690			1.444		
	(3.02)			(5.77)		
≤19	0.621			0.805		
	(2.92)			(1.05)		
20-49	1.292			0.690		
	(3.32)			(3.76)		
50-99	0.878			1.035		
	(1.94)			(0.57)		
200-299	1.175			1.135		
	(2.19)			(2.61)		
300-499	0.722			0.996		
	(4.02)			(0.07)		
500-999	1.074			0.834		
	(0.82)			(3.11)		
≥ 1000	0.822			0.649		
	(1.98)			(6.85)		
Wald Chi²	1,136.82	4324.91	594.93	3,201.25	7619.65	551.41
Log likelihood	-1,110,059	-1,103,435	-464,503	-514,474	-510,234	-208,809
# Subjects		187,490			98,211	
# Obs.		513,179			235,986	
# Failures		73,904			56,132	

Note: Results are shown in hazard ratios; t-values are in parenthesis. Additional covariates are sectors and federal states.

Table 3: Unemployed, Job-to-job: Results from Cox estimation

	West Germany		East Germany	
	Unemployed	Job-to-job	Unemployed	Job-to-job
Age in years (reference group: 25-29)				
30-34	0.878	0.963	1.049	0.875
	(1.55)	(0.60)	(0.64)	(1.54)
35-39	0.944	0.838	1.144	0.879
	(0.69)	(2.75)	(1.85)	(1.39)
40-44	1.129	0.747	1.115	0.849
	(1.29)	(4.03)	(1.62)	(1.67)
45-52	0.962	0.499	1.247	0.587
	(0.44)	(9.14)	(3.52)	(5.88)
Education (reference group: no professional training)				
Vocational Training	1.261	0.913	1.046	1.533
	(1.99)	(0.75)	(0.39)	(1.62)
Voc. Training & A-Level	0.365	0.814	1.347	1.725
	(2.01)	(0.69)	(0.74)	(1.09)
University	0.742	0.714	0.881	1.555
	(1.32)	(0.99)	(0.66)	(1.27)

Table 3 continued

	West Germany		East Germany	
	Unemployed	Job-to-job	Unemployed	Job-to-job
Previous employment state (reference group: no observation)				
Unemployment	3.246	0.922	2.886	0.980
	(8.89)	(0.87)	(9.39)	(0.12)
Non-employment	1.201	1.208	1.082	1.352
	(1.00)	(1.76)	(0.47)	(1.51)
Recall	0.656	0.420	0.407	0.494
	(2.22)	(6.83)	(5.68)	(3.36)
Job-to-job change	1.019	1.037	1.161	0.984
	(0.14)	(0.42)	(1.25)	(0.10)
Nationality (reference group: German)				
EU citizen	0.648	1.095	0.463	2.557
	(2.41)	(0.62)	(1.40)	(2.09)
No EU citizen	1.361	0.957	0.879	1.673
	(3.46)	(0.53)	(0.84)	(2.30)
Job position (reference group: no professional training)				
Skilled blue-collar	0.711	1.015	0.917	0.846
	(3.34)	(0.15)	(1.38)	(1.87)
White-collar	0.566	0.817	0.658	0.610
	(3.74)	(1.61)	(3.89)	(3.49)
Master craftsman	0.539	1.153	0.549	0.487
	(2.51)	(0.64)	(2.88)	(2.38)
Part-time worker	0.872	0.634	0.946	0.644
	(0.49)	(1.80)	(1.11)	(4.13)

Table 3 continued

Occupation (reference group: service workers)				
Production workers	1.248	0.998	1.437	0.990
	(2.68)	(0.02)	(5.46)	(0.10)
Technicians	1.250	0.853	1.100	0.915
	(1.80)	(2.11)	(0.92)	(0.86)
	0.637	0.517	1.823	0.792
	(1.02)	(1.83)	(7.12)	(1.24)
Others	(3.46)	(0.53)	(0.84)	(2.30)
	(0.14)	(0.42)	(1.25)	(0.10)
Calendar time (reference group: 1996, 2001)				
1997	1.180	0.704	0.931	1.010
	(1.78)	(4.37)	(0.97)	(0.09)
1998	0.940	0.939	0.939	0.582
	(0.40)	(0.51)	(0.77)	(4.28)
1999	1.011	1.067	1.081	0.767
	(0.08)	(0.62)	(1.06)	(2.12)
2000	0.964	1.445	1.161	1.061
	(0.32)	(4.05)	(2.31)	(0.66)
Local labour market				
Unemployment rate	1.069	0.913	0.907	1.043
	(0.59)	(1.02)	(2.66)	(0.68)
Investments in (reference group: no investment)				
ICT	0.932	0.849	0.899	0.879
	(1.09)	(3.06)	(2.55)	(2.05)
Further training	1.149	0.833	1.049	1.462
	(1.31)	(1.67)	(0.42)	(1.28)
Collective agreements (reference group: no collective agreement)				
Sector-wide	0.945	0.978	1.003	0.772
	(0.72)	(0.33)	(0.06)	(3.85)
Firm-level	1.171	0.944	1.014	0.848
	(1.40)	(0.63)	(0.30)	(1.83)
Wages > tariff	0.871	0.930	0.953	0.973
	(2.15)	(1.45)	(0.77)	(0.37)
Works council	0.963	0.697	1.030	0.676
	(0.36)	(3.61)	(0.40)	(2.99)
Legal form (reference group: individual firm)				
Partnership	0.925	1.158	0.973	1.267
	(0.57)	(1.18)	(0.20)	(1.38)
Private limited company	1.074	1.225	1.115	1.230
	(0.70)	(1.96)	(1.41)	(1.89)
Public limited company	0.956	1.191	1.067	0.682
	(0.35)	(1.54)	(0.46)	(1.81)

Table 3 continued

Public corporation	0.738	0.700	0.894	0.381
	(1.82)	(2.83)	(1.13)	(6.03)
Other (e.g. association)	0.955	0.712	1.238	0.816
	(0.33)	(2.54)	(2.44)	(1.33)
Firm size (reference group: 100-199 employees)				
1-19	1.719	0.995	1.134	1.414
	(5.05)	(0.05)	(1.82)	(3.65)
20-49	1.190	0.962	0.811	1.150
	(1.86)	(0.48)	(3.39)	(1.40)
50-99	1.393	1.291	0.948	1.272
	(3.62)	(3.53)	(1.07)	(2.58)
200-299	1.020	0.872	0.987	0.890
	(0.19)	(1.36)	(0.32)	(1.34)
300-499	2.202	0.885	1.184	0.829
	(6.05)	(1.03)	(3.61)	(1.39)
500-999	0.816	1.094	1.173	0.906
	(1.22)	(0.77)	(3.60)	(0.96)
≥ 1000	0.702	0.879	1.283	0.837
	(1.91)	(0.81)	(4.33)	(1.40)
Year of setting up (reference group: 1981-1990)				
≤ 1980	1.159	0.870	0.684	1.562
	(1.60)	(1.92)	(2.05)	(2.00)
1991-1995	0.979	0.893	0.922	0.952
	(0.19)	(1.36)	(1.89)	(0.71)
≥ 1996	0.905	1.169	0.621	1.093
	(0.65)	(1.23)	(6.86)	(1.04)
Interactions with further training dummy				
A-level	1.440	1.664	0.794	1.102
	(1.31)	(4.72)	(0.81)	(0.22)
Vocational Training	0.686	1.044	0.793	0.694
	(3.10)	(0.35)	(1.90)	(1.21)
Voc. Training & A-Level	2.451	1.262	0.667	1.004
	(1.71)	(0.79)	(0.99)	(0.01)
University	0.893	1.391	0.773	0.835
	(0.48)	(0.99)	(1.30)	(0.49)
Interactions with works council dummy				
Skilled blue-collar	1.023	0.882	0.607	0.861
	(0.22)	(1.24)	(6.70)	(1.23)
White-collar	0.979	1.333	0.420	1.525
	(0.16)	(2.64)	(7.97)	(2.88)
Master craftsman	0.726	0.782	0.644	1.297
	(0.95)	(0.88)	(1.83)	(0.70)

Table 3 continued

Part-time worker	1.471	2.441	1.253	2.648
	(1.26)	(3.38)	(2.95)	(5.66)
1-19	0.561	0.525	0.618	0.801
	(2.02)	(2.37)	(1.85)	(0.60)
20-49	1.019	1.495	0.961	0.489
	(0.14)	(3.59)	(0.33)	(3.50)
50-99	0.779	0.763	1.224	0.678
	(2.17)	(2.71)	(2.72)	(2.99)
200-299	0.849	1.201	1.137	1.442
	(1.36)	(1.63)	(2.20)	(3.33)
300-499	0.362	1.198	0.842	1.193
	(7.01)	(1.39)	(2.72)	(1.20)
500-999	1.175	0.959	0.767	1.447
	(0.94)	(0.34)	(3.90)	(2.79)
≥ 1000	0.989	0.948	0.611	1.207
	(0.06)	(0.34)	(6.00)	(1.26)
Wald Chi²	4,924.10	2,062.15	7,531.22	1,622.09
Log likelihood	-382,323.63	-454,229.47	-290,913.47	-141,081.14
# Subjects	187,490		98,211	
# Obs.	513,179		235,986	
# Failures	21,808	31,648	34,208	12,920

Note: Results are shown in hazard ratios; t-values are in parenthesis. Additional covariates are sectors and federal states.

Table 4: Effects of the works council on certain worker or firm group

| | West Germany | | | East Germany | | |
	All exits	Un-employ-ment	Job-to-job	All exits	Un-employ-ment	Job-to-job
Skilled blue collar	0.802	0.727	0.606	0.783	0.662	0.688
White collar	1.368	0.554	1.137	1.049	0.637	1.688
Master craftsman	0.769	0.392	0.473	1.471	1.173	1.800
Part-time worker	1.395	1.283	2.682	1.603	1.324	2.781
1-19	0.367	0.326	0.368	0.672	0.545	0.383
20-49	0.947	0.856	1.084	0.759	1.186	0.287
50-99	0.512	0.559	0.412	1.006	1.291	0.360
200-299	1.020	0.832	0.960	1.118	1.152	1.095
300-499	0.379	0.164	0.943	0.872	0.711	0.973
500-999	0.870	1.440	0.612	0.683	0.654	1.080
≥ 1000	0.600	1.409	0.752	0.525	0.477	0.975

Note: In order to get the hazard ratio of the works council effect of a certain group we did the following calculation: we multiplied the interaction hazard ratio with the raw works council hazard ratio and divided this through the raw hazard ratio of the respective worker or firm group. We only included the overall works council effect in the calculation if it is significant. We did not estimate the standard errors of the transformed hazard ratios.

Table 5: Likelihood-ratio tests: Chi² Distribution

	West	East
Competing risks	34,131 (81)	14,776 (76)
Interactions with education	539.4 (4)	108.5 (4)
Interactions with job position	428 (4)	558.54 (4)
Interactions with firm size	593.6 (6)	240.14 (6)

Note: Degrees of freedom in parenthesis.

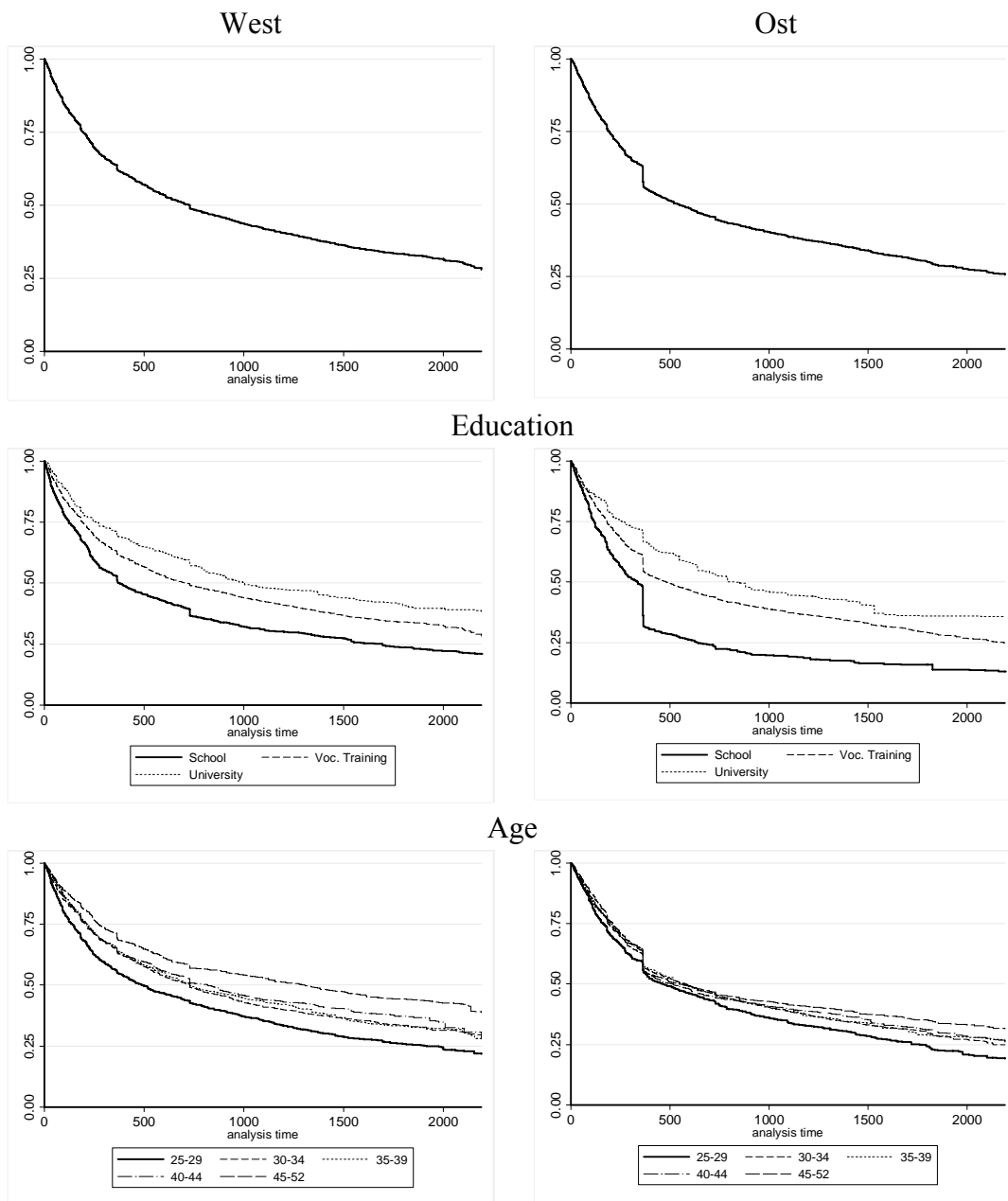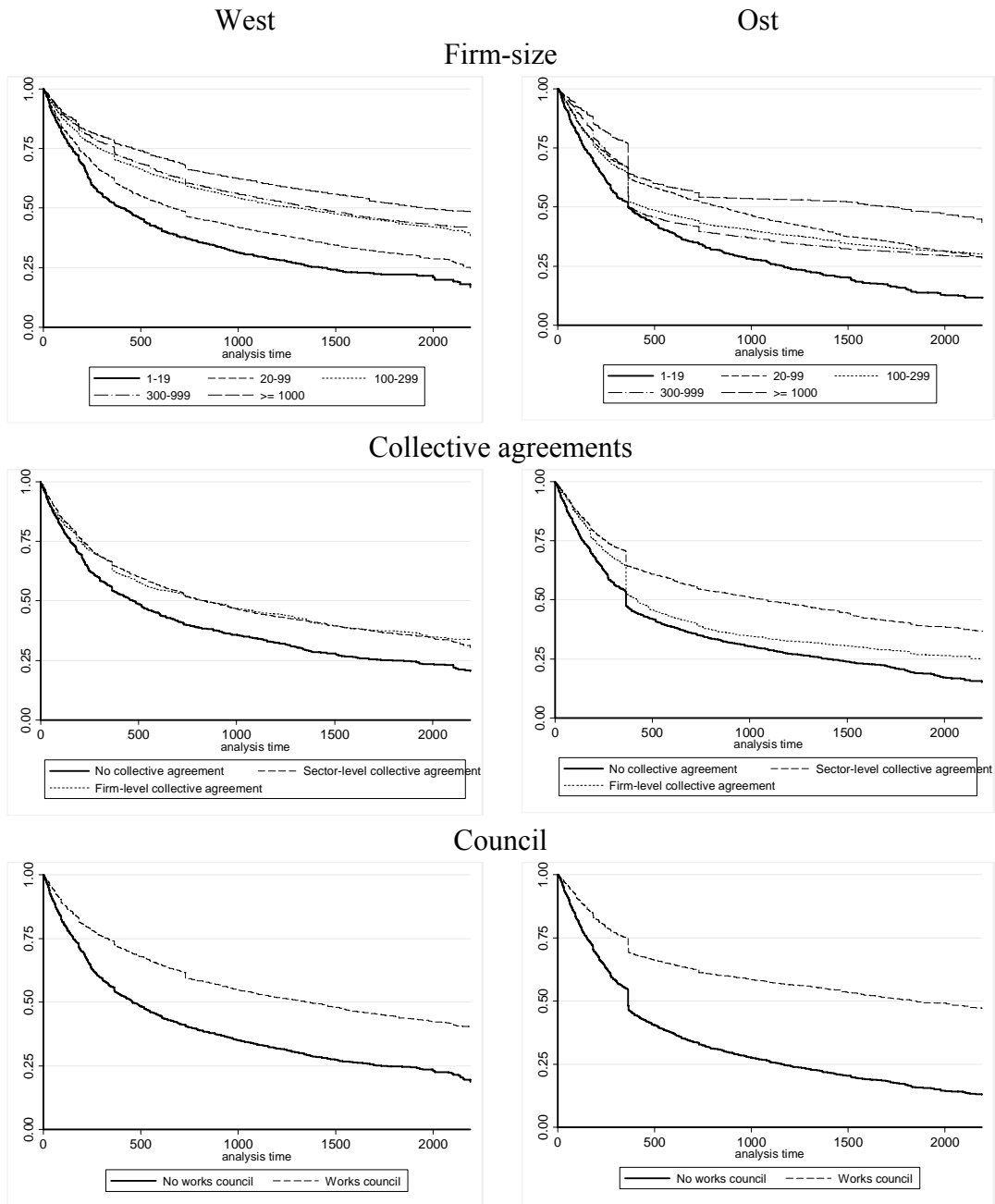**Figure 1: Kaplan-Meier-curves by age and education**

West   Ost

Education

Age

Figure 2: Kaplan-Meier-curves by firm size

West   Ost

Firm-size

Collective agreements

Council

Appendix

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

Variable	Men			
	West		East	
	# spells		# spells	
Sub-spells	513,179		235,986	
Employment spells	187,490		98,211	
Destination				
Unemployment	21,808 (12)		34,208 (35)	
Non-employment	18,429 (10)		8,347 (9)	
New employer	31,648 (17)		12,920 (13)	
Recall	2,019 (1)		657 (1)	
Censored spells	113,586 (61)		42,079 (43)	
	Mean	Std.dev	Mean	Std.dev
Come from				
Unemployment	0.194	0.395	0.423	0.494
Non-employment	0.053	0.224	0.039	0.194
Recall	0.133	0.340	0.088	0.283
Job-to-job transition	0.514	0.500	0.367	0.482
No observation	0.106	0.308	0.083	0.275
Individual-specific				
Education				
No school, O-level	0.157	0.363	0.051	0.220
A-Level	0.026	0.159	0.005	0.073
Vocational training	0.548	0.498	0.711	0.453
Vocational training and A-levels	0.050	0.219	0.026	0.160
University	0.220	0.414	0.207	0.405
Nationality				
German				
EU	0.023	0.151	0.002	0.046
Non-EU	0.089	0.284	0.022	0.146
Job-specific				
Job position				
Blue collar unskilled	0.328	0.470	0.164	0.370
Blue collar skilled	0.212	0.409	0.371	0.483
White collar	0.410	0.492	0.326	0.469
Master craftsman	0.010	0.098	0.015	0.122

Part-time more than 15 hours	0.036	0.187	0.124	0.329
Age				
25-29	0.259	0.438	0.173	0.378
30-34	0.264	0.441	0.196	0.397
35-39	0.198	0.398	0.197	0.398
40-44	0.135	0.341	0.184	0.387
45-52	0.145	0.352	0.250	0.433
Profession				
Production	0.437	0.496	0.505	0.500
Technical	0.140	0.347	0.100	0.301
Services	0.418	0.493	0.383	0.486
Others	0.005	0.074	0.012	0.108
Firm-specific				
Sector				
Insurance, credit	0.050	0.219	0.053	0.224
Transport, communication	0.039	0.193	0.029	0.168
Trade, repair	0.059	0.236	0.020	0.141
Construction	0.062	0.241	0.088	0.284
Mining, energy, water	0.024	0.153	0.026	0.159
Finish of raw materials	0.294	0.455	0.131	0.337
Capital goods	0.215	0.411	0.171	0.376
Consumer goods	0.096	0.295	0.068	0.251
Services for firms	0.045	0.208	0.069	0.253
Other services	0.072	0.258	0.155	0.362
Non-profit organization	0.019	0.137	0.099	0.298
Regional authorities, social insurances	0.025	0.156	0.092	0.289
Firm-size				
1-19	0.011	0.102	0.026	0.158
20-49	0.023	0.149	0.051	0.221
50-99	0.038	0.192	0.096	0.295
100-199	0.054	0.226	0.156	0.363
200-299	0.060	0.238	0.131	0.338
300-499	0.089	0.285	0.158	0.365
500-999	0.150	0.357	0.196	0.397
≥ 1000	0.575	0.494	0.185	0.388
Bargaining				
Council	0.914	0.280	0.678	0.467
Sector coll. agreement	0.784	0.412	0.595	0.491
Firm collective agreement	0.129	0.335	0.161	0.368
Wage > tariff	0.614	0.487	0.132	0.339
Legal form				
Individual firm	0.010	0.101	0.028	0.164

Partnership	0.065	0.246	0.027	0.161
Private limited partnership	0.440	0.496	0.581	0.493
Public limited partnership	0.350	0.477	0.063	0.243
Public Corporation	0.104	0.305	0.187	0.390
Other (e.g. association)	0.031	0.174	0.114	0.318
Year of setting up				
≤ 1980	0.814	0.389	0.028	0.164
1981-1990	0.058	0.235	0.211	0.408
1991-1995	0.089	0.284	0.642	0.479
≥ 1996	0.039	0.193	0.119	0.324
Further training: yes/no	0.958	0.201	0.872	0.334
Investments in ICT	0.863	0.344	0.731	0.443
Country				
Berlin	0.075	0.263		
Schleswig-Holstein	0.025	0.157		
Hamburg	0.055	0.227		
Lower Saxony	0.091	0.288		
Bremen	0.014	0.116		
North Rhine-Westphalia	0.281	0.450		
Hesse	0.097	0.296		
Rhineland-Palatinate/Saarland	0.045	0.207		
Baden-Württemberg	0.121	0.326		
Bavaria	0.196	0.397		
Brandenburg			0.194	0.395
Mecklenburg-Vorpommern			0.179	0.384
Saxony			0.203	0.402
Saxony-Anhalt			0.220	0.414
Thuringia			0.204	0.403

Note: per cent in parentheses.