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Workers, Firms, or Institutions: What Determines Job Duration for Male Employees in Germany?

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Non-technical summary

How long workers stay in their jobs is of central importance for individuals' work histories, employers' personnel policies and the functioning of the labor market. Within and across industries, companies, institutional regimes and groups of workers, employment spells differ vastly in duration. Using German linked employer-employee-data we analyse job durations from both sides of the employment relationship. We address two main questions. First, what is the relative importance of worker characteristics, firm-level variables and institutions for job duration, and how are they related to each other? 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? A second question is whether effects differ between different exit states namely between job-to-job and job-to-unemployment changes.

Kaplan–Meier survivor functions show that more than fifty percent of all new employment relationships end after in the first two years. The estimation results indicate huge differences between individuals but also between establishments with different characteristics. We conclude that persistence in individual mobility behavior is much reduced when firm-level heterogeneity is accounted for. Blue collar workers tend to select into long-tenure firms as compared to white collar workers. Positive effects of works councils and further training on job duration are high for blue collar skilled or semi-skilled workers but non-existent for white collar employees. Competing risks analysis shows that mobility to another job and exit to unemployment follow strikingly different processes. For instance having a university degree lowers the unemployment hazard but increases the likelihood of a job-to-job change.

Das Wichtigste in Kürze

Die Betriebszugehörigkeit ist eine wichtige Größe im Hinblick auf die persönliche Erwerbshistorie, personalpolitische Maßnahmen eines Arbeitgebers und den Arbeitsmarkt insgesamt. Innerhalb und zwischen Sektoren, Unternehmen, verschiedenen Institutionen und bestimmten Beschäftigungsgruppen gibt es große Unterschiede in der durchschnittlichen Betriebszugehörigkeit. Mit verknüpften Beschäftigten- und Betriebsdaten analysieren wir den Einfluss beider Seiten eines Beschäftigungsverhältnisses auf Beschäftigungsdauern. Zwei Fragen stehen im Mittelpunkt der Analysen. Erstens, wie groß ist der Einfluss von individuellen Charakteristika, Betriebsinformationen und Institutionen auf die Betriebszugehörigkeit und wie stehen diese im Verhältnis zueinander? Gibt es Segmentierungsprozesse, so dass in einem Betrieb eine bestimmte Gruppe von Beschäftigten gegen Jobverluste abgesichert ist, weil eine andere Gruppe mit durchschnittlich kurzen Beschäftigungsdauern Schwankungen in der Gesamtbeschäftigung auffängt? Oder kommen unterschiedliche Beschäftigungsdauern hauptsächlich durch die Heterogenität zwischen Betrieben zustande, so dass sie durch Betriebscharakteristika erklärt werden können? Zweitens wird die Frage beantwortet, ob die Effekte unterschiedlich sind, wenn man zwischen direkten Arbeitgeberwechseln und Übergängen in Arbeitslosigkeit differenziert.

Kaplan-Meier Überlebensfunktionen zeigen, dass über fünfzig Prozent der neu begonnenen Beschäftigungsverhältnisse nach zwei Jahren wieder beendet sind. Mit proportionalen Cox Schätzungen finden wir große Unterschiede zwischen Individuen aber auch zwischen Betrieben mit unterschiedlichen Charakteristika. Der Einfluss individueller Charakteristika wird erheblich reduziert, wenn Betriebsinformationen mit berücksichtigt werden. So selektieren sich Arbeiter im Vergleich zu Angestellten in Betriebe mit langen Beschäftigungsdauern. Positive Effekte, die durch Betriebsräte und Weiterbildungsangebote generiert werden, wirken vor allem auf die Mobilität von Arbeitern aber kaum auf die von Angestellten. Weiterhin finden wir, dass sich die Effekte auf direkte Übergänge von einem zum nächsten Arbeitgeber sehr von Übergängen aus Beschäftigung in

Arbeitslosigkeit unterscheiden. Ein Universitätsabschluss verringert zum Beispiel die Wahrscheinlichkeit, in Arbeitslosigkeit zu wechseln, erhöht aber die direkten Arbeitgeberwechsel.

Workers, Firms, or Institutions: What Determines Job Duration for Male Employees in Germany?

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Note: This Discussion Paper is an extensively revised version of the ZEW Discussion Paper No. 05-89 "Individual and Plant-Level Determinants of Job Durations in Germany".

Abstract: We examine job durations of German workers using linked employer–employee data. Our results indicate that exit rates are strongly influenced by firm characteristics. The effects of some of these characteristics, however, are limited to particular job positions or skill groups. There is clear evidence for a sorting process whereby workers with long expected job durations are matched to firms offering stable employment (and *vice versa*). 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. Among the substantive results, it would appear that works councils decrease exit both to unemployment and to new jobs, but do so only for blue collar workers.

JEL-Codes: J62, J63, C41

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

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How long workers stay in their jobs is of central importance for individuals' work histories, employers' personnel policies and the functioning of the labor market. Within and across industries, companies, institutional regimes and groups of workers, employment spells differ vastly in duration. In this paper, we estimate a reduced-form model of job exit using a linked employer–employee dataset.

Our paper is motivated by two empirical questions. First, what is the relative importance of worker characteristics, firm-level variables and institutions for job duration, and how are they related to each other? Empirical evidence shows that transitions from jobs depend on individual attributes such as age or education, but firms also differ with respect to the employment duration of their workers (Battu *et al.,* 2002; Bronars and Famulari, 1997; Dohmen and Pfann, 2003; Gerlach and Stephan, 2005; Mumford and Smith, 2004). Firm-level industrial relations, such as the presence of shop-floor employee representation, may influence the number of quits and layoffs (Addison and Teixeira, 2006; Addison *et al.,* 2001, Frick, 1996). The impact of institutions is likely to differ with worker characteristics. For instance, works councils may be dominated by blue collar workers, increasing job stability only for this group. To capture these differences, we use a flexible parametric specification in our empirical estimations. In addition, we analyze the existence of sorting processes such that workers with long expected job durations are matched to firms offering stable employment (and *vice versa*).

A second question is whether effects differ between exit states. It can be easily derived from a job search model that the determinants of job-to-job mobility are different from those that influence transitions into unemployment. For instance, the presence of a works council might, on the one hand, increase job satisfaction, reduce on-the-job searching and decrease the number of job-to-job transitions. On the other hand it might increase employers' firing costs and thereby influence the productivity threshold (Mortensen and Pissarides 1994) below which layoffs are made. This affects the dismissal rate and, if most laid-off workers experience a period of unemployment

following dismissal, the number of job-to-unemployment transitions. While the direction of the influence is the same in both cases, the two mechanisms are distinct and the magnitude of the influence is likely to differ.

These issues are investigated on the basis of a German dataset which is more appropriate and contains more information than the datasets previously available. Existing studies use stock data containing information which relates only to the current spell at the date of interview, but not to completed job durations. This leads to selection bias because the distribution of unobservables differs in the stock of ongoing and in the flow of newly started jobs. An advantage of stock data is that longer maximum durations are observed than in the case of flow data, making it easier to investigate changes of job durations over time. In the flow data we use, however, about three-quarters of all new employment relationships end within the observation period, while only one-quarter of the spells are right-censored.

With some exceptions such as Dostie (2005) and Mumford and Smith (2004), previous studies mostly use data with little information on the firm side, such as worker-level administrative or survey data. Estimation based on this type of data yields unbiased coefficients if firm fixed effects are included (e.g. Bronars and Famulari, 1997). However, the data often contain only one worker per firm, which means that fixed effects are not identified. Moreover, the effect of firm-level characteristics varying over time and their interaction with individual covariates cannot be ascertained in this way.

Our results show that differences in mobility between different parts of the labor market are large. Institutions such as works councils and the availability of further training play a pronounced role in reducing mobility within the market, and furthermore, they interact with individual characteristics. Including institutions and firm characteristics in a flexible way is, therefore, essential for modeling job durations.

Theoretical background

A theoretical framework for modeling the probability of job exit is provided by the search and matching approach pioneered by Burdett (1978) and Jovanovic (1979a).[1] In the simplest formulation, an employed worker will take any job that is better paid than his current one. The probability of a worker's leaving current employment for another job within a given period is then one minus the cumulative distribution function of wage offers arriving in each time period, evaluated at the current wage. In a framework with many periods, workers who have spent more time on the labor market will have received more wage offers. They are thus more likely to have a high current wage and less likely to exit their current jobs. As a consequence, time in the labor market or age are important explanatory variables for job mobility and current wages are endogenous to time spent in the labor market.

Extending this model to allow for involuntary dismissals, these are typically taken as exogenous, so that the probability of being dismissed is determined by a stochastic productivity shock. Moving from partial equilibrium for workers to general equilibrium, Mortensen and Pissarides (1994) also include hiring costs. In this case, retaining the worker may be valuable for the firm because it preserves the option of employing the same worker in the next period. At a given wage, this yields a reservation level of productivity. If the stochastic productivity shock falls below this threshold, the worker is dismissed. In this way, institutions influencing search frictions become important determinants of job mobility.

The Jovanovic (1979) model assumes asymmetric information at the start of a new employment relationship. Match quality is an experience good. Its value increases with tenure

1 These models have been extended and transformed from partial equilibrium into general equilibrium models (Mortensen and Pissarides, 1994). They are used for explaining macroeconomic phenomena such as equilibrium unemployment, wage distributions and mobility rates. Pissarides (1994) and Burdett and Mortensen (1998) placed on-the-job search models into this equilibrium framework.

until all information is available for both employer and employee or until the match comes to an end. With increasing outside options, e.g. higher wages offered by other employers or a high job finding rate, on-the-job search increases, leading to shorter job duration. However, with rising job duration, the likelihood that an outside offer will be higher than the match-specific rents created by reduced uncertainty about match quality declines, affecting job duration positively.[2] The empirical specification of the job exit probability must, therefore, allow for duration dependence.[3] The basic regularities predicted by the partial-equilibrium search and matching framework are captured in our empirical specification by a reduced-form approach using hazard rate analysis.[4]

The role of institutions for job durations

Within the framework outlined above, institutions influence job exit through their impact on hiring and firing costs. The German labor market is highly regulated by international standards. Wages and working hours are fixed by collective agreements, mandatory job protection and employee representation are strict, and many other regulations exist (Addison *et al.*, 2001; Gerlach and Stephan, 2005; Frick, 1996). Most regulations are mandatory for all employers, although thresholds in terms of establishment size exist (Koller, 2005). In the following, we focus on company-level institutions that vary across the establishments in our sample.

Works councils are the main institution of shop-floor worker representation in Germany.[5] In establishments with at least five employees, workers are legally entitled to establish a works

2 Another reason for job-specific rents is accumulation of job-specific human capital.

3 Moscarini (2005) as well as Pries and Rogerson (2005) bring together the Jovanovic (1979) and the Mortensen and Pissarides (1994) model. For recent surveys see Rogerson *et al.* (2005), or Yashiv (2007).

4 Based on employer–employee data, there have also been attempts to estimate structural models (e.g. Jolivet *et al.*, 2006, Nagypál, 2005). For a discussion of structural and reduced-form approaches, see Cahuc and Zylberberg (2004: 146).

5 Another German institution of worker participation, the presence of employee representatives in supervisory boards, has no direct effect on hiring and firing practices and is, therefore, not discussed here.

council. In practice, however, employees in small establishments often do not take the initiative to set up one. In establishments with a works council, voluntary separations may decrease if the council gives employees a voice', leading to higher job satisfaction. At the same time, involuntary separations may be affected if the works council increases separation costs. Participation rights of works councils are regulated in detail by the Works Constitution Act and the Dismissal Protection Law. Articles 102-104 of the Works Constitution Act grant consultation rights in dismissal cases. Works councils can raise objections within one week from the notification of dismissal. In addition, the works council can make suggestions in order to stabilize employment which must be answered by the employer. According to Article 112 of the Works Constitution Act, the works council has a right to participate in drawing up a social plan in the case of mass redundancies.

Collective agreements are generally negotiated at industry level between unions and employers' associations and are mandatory for all employees whose employer is a member of an employers' association. In 2003, 70 per cent of West German employees and 47 per cent of East German employees were covered by collective agreements. In West Germany, there has been a decline in the coverage rate since the late 1990s (Fitzenberger *et al.*, 2008; Schnabel 2005). The trend towards local bargaining was strengthened by the increasing use of opening clauses, allowing for deviations from the terms of the collective contract (Heinbach, 2007). Although German unions have no legally defined participation rights with regard to dismissal protection, the Works Constitution Act gives unions a wide range of information rights and their influence on the election of works councils is high. Very often, unions play a role in drawing up a social plan in the event of mass redundancies. Furthermore, they try to avoid reductions in employment by adjustment of working hours or reduced wage claims in collective agreements. Hence, one might expect higher job stability in establishments covered by collective agreements. At the same time, the presence of collective agreements may also lead to wage rigidity. This can lead the firm to adjust employment instead of wages in bad times, which decreases average job duration.

Data on Job Durations from the LIAB

The Linked Employer–Employee Data of the IAB Institute (LIAB) combine administrative data on employees obtained from social insurance files with employer data from an annual representative survey of 16,000 establishments. The data contain information on all workers employed in surveyed establishments. We use the longitudinal version I of the LIAB, which contains daily employment and benefit recipient information for the period from 1991 to 2001.

The firm side of the LIAB consists of information taken from the IAB Establishment Panel, the most extensive survey among firms in Germany. The number of establishments in the LIAB longitudinal data is limited to those having valid interviews from 1999 to 2001.[6]

The employee part of the LIAB is the Employment Statistics Register of the Federal Employment Agency. This administrative data record is based on declarations of employers to the German social insurance institutions. Depending on the circumstances, misreporting is either a summary or a criminal offense, and therefore the reliability of the data is high. The data contain daily information on all employment relationships covered by the social security system. Other forms of employment are not recorded in the data.[7] Overall, the Employment Statistics Register covers about 80 percent of total employment. The Register is further combined with data on periods of unemployment benefit receipt obtained from the Benefit Recipient Data of the Federal Employment Agency. Spells of unemployment are recorded only in cases where unemployment benefit is received and/or there is participation in active labor market programs. If we bear this in mind, the data allow us to construct complete employment biographies of those employees covered by the social security system.

6 Worker separations due to plant closures cannot be observed in our data.

7 This concerns self-employment, civil servants, marginal work remunerated below a monthly income threshold, unpaid family workers, and employment abroad.

The LIAB longitudinal version I covers 2,100 establishments in both the Western and the Eastern parts of Germany, and the employment histories from 1991 to 2001 of all individuals employed for at least one day between 1996 and 2001 in one of the sample establishments.

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.[8] As we define it, the end of an employment spell occurs if two conditions apply. First, the individual is observed to move into unemployment, or non-employment, or is hired by a new employer. Second, the current employer reports the end of the employment relationship to the insurance institution. The current spell is right-censored if either of these conditions does not hold, or if the end of the observation period is reached.[9]

One advantage of these data is the possibility of observing into which employment state a worker moves after the end of a spell. Four exit states are distinguished: unemployment, new employer, recall to the previous employer, and non-employment. Periods of unemployment are difficult to define because the data contain only information for the time during which a person receives unemployment benefit from the German Federal Employment Agency. Since not all unemployed workers qualify for unemployment insurance (UI), and benefits may be temporarily suspended in case of sanctions, individuals can be unemployed without receiving UI benefits (see

8 In order to avoid having many short spells due to seasonal factors, we define two successive employment spells within the same employer (even if we observe an unemployment spell in between) as one spell if the recall takes place 90 days after the end of the first period at maximum. If an observed spell is interrupted for reasons like parental leave, illness or sabbaticals, we also define the whole period as one employment spell (independently of the duration of the interruption).

9 Employers are obligated to report the end of an employment relationship. In some cases we observe a change in the employment state or in the employer identifier but no reported end of the relationship. In these cases, it is uncertain whether a job change actually occurred. Hence, we define these spells as right-censored. The exception is a move into unemployment because in this case, we have additional information relating to the start of benefit payments.

Fitzenberger and Wilke, 2004). We define unemployment as receipt of UI benefit for at least one day within 60 days after the end of the previous employment spell.

A job-to-job change is defined as a separation followed by an employment spell which commences within 60 days of the end of the previous one. It is likely that in most of these cases, the new employment relationship was already known about when the previous job ended. Another case is that the employee is recalled to the previous employer because of fluctuating demand, temporary illness or other reasons. We define the exit state as a recall if we observe a return to the same employer at least 91 days after the end of the current spell. The exit state is defined as non-employment if we observe neither subsequent employment nor a spell of unemployment within 60 days of the end of the current spell. This state comprises different situations such as unemployment without benefit receipt, inactivity or self-employment.

To determine the beginning of an employment spell, we proceed analogously, but we distinguish between short and long spells of previous non-employment. The former are defined as gaps of less than one year in a person's employment history. Furthermore, we use a separate category for employees who are likely to be in their first job. This category consists of individuals below 30 years of age who were starting their first spell in the data after 1996 but were not observed between 1991 and 1995. In Table 1, the definitions of the exit and origin states are summarized.

Table 1 here

Sample definition and descriptive statistics

We analyze employment durations of male workers only, because employment histories of females are often determined by interruptions for maternity leave. With our data we cannot observe exactly whether a woman leaves the labor market solely because of the birth of a child. Furthermore, it is

not possible to analyze determinants for employment durations of mothers, factors like availability of child care, partner's income, etc. because we do not have this information.

In order to avoid bias due to left-censoring, we restrict analysis to all employment spells which started in the sample establishments between 1996 and 2001. We restrict data to persons aged 25 to 52 to exclude individuals in vocational training or in work during university vacations, and to avoid confusion between job exit and early retirement. In addition, we exclude spells of employment that include episodes of part-time work (below 15 hours per week), vocational training or home working. All spells with missing covariate information are eliminated. These requirements leave us with a sample of 249,313 employment spells, of which 88,202 are from East Germany.[10]

Table 2 here

Kaplan–Meier survivor functions shown in graph form in panel (a) of Figure 1 give a first descriptive impression of job durations in the sample.[11] There are striking differences between East and West Germany, underlining the importance of separate analyses. Exit occurs more slowly in the Western part of Germany. In the East, there is a huge drop in the survivor function after exactly one year. This is probably due to the higher incidence of temporary employment in the East, especially in the job creation schemes used widely during the observation period.[12]

Figure 1 here

10 The number of spells according to the different exit states is reported in Table 2. Summary statistics of all covariates are made accessible by the authors on request.

11 Kaplan–Meier functions have the advantage that censored spells are taken into account in the risk group as long as they are observed. The cross-sectional weights of the IAB Establishment Panel are used for estimation.

12 On average over the period from 1995 to 2002, the share of temporary employment in the stock of employees was 11.5 percent in East Germany, as compared to 7.0 percent in West Germany. 33.6 percent of temporary jobs were

Separate analyses according to firm characteristics show that in West Germany, survival rates are higher for larger establishments (panel b). However, the ordering is less clear in East Germany. In line with the findings of Gerlach and Stephan (2005), workers in firms with collective agreements at firm or industry level have longer job durations in West Germany (panel c). In East Germany, the difference is between firms adhering or not adhering to an industry wage agreement. Most strikingly, median durations are two to three times longer if the establishment has a works council (panel d). As the effects of firm size, bargaining regime and employee representation are all likely to be correlated, these estimates must be interpreted with care. The graphs show, however, that mobility rates differ vastly between different parts of the German labor market.

Estimation technique and independent variables

The dependent variable in the following is the conditional hazard rate, which is defined as the instantaneous probability of exit from the current job: $\lambda(t) = \lim_{\Delta t \to 0} \Delta t^{-1} P[t \leq T < t + \Delta t \mid T \geq t]$, where t indicates time and T is the actual job duration. If job-to-job and job-to-unemployment exits follow the same processes, a single exit state model is appropriate. For individual i (i=1,…, n_j) employed in firm j (j=1,…,m) at time t, the hazard rate is specified as $\lambda_{ij}(t) = \lambda_0(t)\exp[z_{ij}(t)'\theta]$, with $z_{ij}(t)$ denoting a vector of individual- or firm-specific characteristics that may vary over time as well as a regression constant. The model is called a proportional hazard model because the baseline hazard $\lambda_0(t)$ is assumed to be shifted proportionately by the covariates (Kalbfleisch and Prentice, 2002: 95ff.). A competing risks framework is used if unemployment and a new job are treated as distinct destination states (denoted by k in the following). In this case, the hazard function is specific for each destination state, such that $\lambda_{ij}^k(t) = \lambda_0^k(t)\exp[z_{ij}(t)'\theta^k]$ and separate parameter vectors are

subsidised by job creation schemes, as opposed to 4.3 percent in West Germany (Boockmann and Hagen, 2005: 156). Job creation schemes normally last for one year.

estimated for each state (Kalbfleisch and Prentice, 2002: 251ff.). To estimate the model, we use the semi-parametric Cox partial likelihood estimator (Cox, 1972; Kalbfleisch and Prentice, 2002: 99ff.).

Using a wide range of person-, match- and firm-specific covariates as well as information on outside options, we include a great deal of information on the determinants of job durations. Because of the flow sample format and the information on exit states our data is predestined for survival analysis methods. These methods do not allow extensions like simultaneous estimations such as those in Altonji and Williams, 2005, or Topel and Ward, 1992. We therefore model job exit by a reduced-form hazard rate model that is not conditional on individual wages.

We estimate both a model with individual characteristics only and a model with individual and firm characteristics. The Employment Statistics Register contains demographic information (age, level of education, nationality) and job-level information (job position and occupation). Regarding previous employment states, we do not condition on the entire employment history but use only the immediately previous state. The coefficients on lagged employment states must be interpreted with care because they are likely to be correlated with unobserved worker characteristics. Since we cannot control for person-level fixed effects (see below), the estimated coefficients will represent both heterogeneity and lagged state dependence.

On the firm side, we include information on firm size, collective bargaining arrangements, works councils, age of establishment, legal status, availability of further training, investment in information and communication technology (ICT) and industry affiliation. We also include year dummies and regional information such as dummies for *Länder* (federal states) and the local unemployment rate.

Despite the wealth of data at our disposal, there may remain unobserved heterogeneity at the level of the establishment or employee correlated with the independent variables. We account for firm-level unobserved heterogeneity by stratifying the sample in some of the estimations (Kalbfleisch and Prentice, 2002: 118f.; Ridder and Tunali, 1999). This means that a separate

baseline hazard is assumed for each establishment, while the coefficients of the covariates are assumed to be the same. This is similar to the within-groups estimator in linear regression. In the stratified case, the coefficients of time-varying firm-specific variables are identified while those of time-constant variables are not identified. Including fixed effects at the person level is not feasible, because this would require multiple spells per person in the establishments included in our sample. However, only a tiny fraction of individuals has more than one spell in more than one establishment in the sample.

Coefficient estimates for individual and job-specific characteristics

Table 3 contains the results of Cox estimations for East and West German workers. In both cases, the first column is taken from estimation with individual-specific variables only, while the second and third columns add firm characteristics and firm fixed effects. Comparing the results from specifications with and without firm characteristics or fixed effects, the importance of self-selection of workers into certain firms can be assessed (Mumford and Smith, 2004). In order to facilitate the quantitative interpretation of the effects, the table displays hazard ratios.[13] Because our focus is on firm-specific effects and their interactions with worker characteristics as well as on the distinction between different exit states we do not discuss at length the results for individual and job-specific covariates.

Table 3 here

13 Hazard ratios are obtained by exponentiating the coefficients and indicate the ratio of the hazard for a one-unit change in the corresponding covariate (also see Cleves et al., 2002). A hazard ratio of 0.8 means that the hazard rate drops by 20 per cent if the covariate increases by one unit.

Results for age, represented by dummy variables for five age intervals, are highly significant. They point to the empirical validity of the Burdett (1978) model.[14] The effect is, however, not found for East German workers. According to human capital theory, highly educated workers have more general human capital and are, therefore, more mobile. At the same time, they are also better able to acquire firm-specific human capital, delaying job changes. Segmentation theory predicts a selection of low-skilled workers into unstable jobs. The findings of previous empirical studies are mixed (Battu *et al.*, 2002; Dostie, 2005; Holzer and Lalonde, 2000; Dustmann and Meghir, 2005; Mumford and Smith, 2004; Naticchioni and Panigo, 2004). In our estimations, workers without vocational training or university education, but also unskilled blue collar and part-time workers, are among the most mobile groups. The effect of university education vanishes when fixed effects are included, which points to a sorting process of these workers into firms with high job stability.

Our results show that individual employment history is an important determinant of job duration (see also Booth *et al.*, 1999; Battu *et al.*, 2002). Individuals entering employment from previous unemployment or non-employment face a significantly higher risk of exit as compared to the reference group of job changers. However, when we account for firm heterogeneity coefficients are reduced dramatically, indicating that these worker groups select into low-duration firms. As an indicator for local labor market conditions, we use the unemployment rate of the *Land* (federal state) concerned. It is lagged by one year because the unemployment rate may be endogenous to job exit.[15] Results show that the effect of local labor market conditions is weak.

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14 They are also in line with the 'job shopping' theory (Johnson, 1978; Viscusi, 1980) based on slightly different assumptions.

15 We eliminate a time trend from the regional unemployment series.

Coefficient estimates for firm and institutional characteristics

Coefficients for firm-level variables appear in the lower part of Table 3. Time-invariant firm characteristics are not included in stratified estimation because their coefficients are not identified. Among the time-varying covariates, we use a dummy indicating investment into information and communication technology (ICT) in the previous year. On the one hand, investment into ICT often requires specific training and, hence, should increase job durations. On the other hand, it may lead to higher turnover rates if employees are not able (or not willing) to apply new technologies and new hirings are made to replace them. As our estimates show, investment into ICT consistently reduces job exit rates.

The effect of training can be checked more directly by including a dummy variable indicating whether the firm does in fact offer further training to its employees. If firms invest into their employees' specific human capital, they have an interest in reducing quits. According to our results, there is the expected negative effect of training on job exit for West German workers. However, it becomes marginally insignificant in stratified estimation. In the East, the negative effect is actually reversed in the stratified estimation results.

As expected, our results show that the presence of a works council leads to significantly longer employment durations, a finding that is consistent with the wider literature on works councils (see, for instance, the survey by Addison *et al.*, 2004).

We distinguish between industry- and firm-specific collective agreements while the base groups are establishments not bound by collective agreements. We further include a dummy variable indicating whether the establishment pays higher wages than required by a collective agreement. The results are mixed. For East Germany, there is some evidence that collective bargaining stabilizes employment, while in the West we find the reverse effect for firm-level bargaining. As expected, wages above the bargained level lower the exit rate. This, however, could also be due to the omission of wages at the individual level from the specification.

We expect tenure to be higher in larger establishments which can use internal labor markets to adjust employment. In line with the results of Mumford and Smith (2004), this expectation is not borne out by the data: the relation fails to be monotonic, and the coefficients of the firm size group dummies are mostly insignificant (coefficients omitted from the printed output but available on request). Younger establishments show longer job durations in East Germany while the effect is insignificant for the Western part. To interpret this finding, it must be recalled that job exits from companies that have closed are not contained in the data. Hence, estimation is based only on successful company starts.

The effects of firm characteristics for different groups of workers

The effects of firm-level variables may differ according to types of workers. Matched employer–employee data allow us to estimate interaction effects between firm and worker characteristics. Two important explanatory variables for which this is relevant are the presence of a works councils and the provision of further training by the enterprise. Table 4 lists hazard ratios similar to those in Table 2, but split according to worker groups.

Table 4 here

Theoretically, the effects of further training depend on the education level of the workforce. If education and training are complementary, further training increases productivity and rents. Hence, persistence in the job should be higher for skilled workers. Alternatively, it may be that providing specific skills by further training is more productive for workers with vocational training, and less productive for university graduates whose skills are more general. Our empirical results support the latter hypothesis: further training decreases exit rates significantly only among workers with vocational training (the effect is, however, imprecisely estimated for East German workers).

A second interaction is that between works council and job position. If a works council mainly represents the interests of the majority of workers, which in most cases means blue collar workers, this group will benefit most from the council's 'voice' function. Moreover, a works council may pay more attention to dismissals and other separations of blue collar workers. The results are supportive of this view. A significantly negative effect of works councils on job exit can only be found for blue collar workers, while white collar workers are not affected. For part-time workers, the sign of the effect is actually reversed, suggesting that works councils increase labor market segmentation. However, the effect is significant only for East Germany. All interaction effects are highly significant, as the likelihood ratio tests in the first two rows of Table 5 show.

Table 5 here

Competing Risks

So far, we have assumed that the mechanism driving job exit is the same across all exit states. However, it is quite plausible that the independent variables influence exit into different exit states in different ways. Using the definitions from table 1, we distinguish between four exit states: unemployment, changing to another employer, recalls, and non-employment. We display results for the first two exit states only, because the number of observations is very low with respect to the 'recalls' state. Furthermore, the coefficients of the hazard into non-employment are hard to interpret because we do not know exactly what situation the worker moves into. The specification includes worker- and firm-level independent variables but does not consider interaction and firm fixed effects. The list of independent variables is thus the same as in the middle columns of Table 3. Likelihood-ratio tests of the null hypothesis of a single exit state versus the competing risks model clearly reject a single exit state, as shown in the lower half of Table 5.

Results in Table 6 show that the impact of most variables differs between exit states. For instance, age has little influence on the probability of exit to unemployment; if anything, it increases

the hazard. Consistent with the 'job shopping' argument, however, the probability of a job-to-job change declines significantly with age. Having a university degree lowers the unemployment hazard but increases the likelihood of a job-to-job change. Better job positions also protect against the risk of unemployment. Previous unemployment increases the likelihood of becoming unemployed again, but has a negative or no effect on job-to-job changes. In general, there is little consistent evidence that lagged employment states influence the probability of changing to another employer. Hence, past job changes do not lead to future employment mobility, nor does 'job shopping' result in more stable employment relationships further on in the individual's career.

Table 6 here

While collective bargaining arrangements are insignificant for both exit states, works councils reduce the transitions both to unemployment and to new jobs. Hence, there is empirical support for both effects: the increase in employment protection and the 'voice' function. Similarly, ICT investment and further training reduce both hazards, although the results are not as strong in the case of East Germany. The effects of firm size are mostly insignificant (not included). The regional unemployment rate reduces the job-to-job hazard in West Germany, most likely through a lower arrival rate of new job offers.

Conclusions

The use of linked employer–employee data strikingly increases the scope for analyzing labor market mobility and provides new knowledge concerning individual and firm-level determinants of employment durations. In this paper, we have addressed the sorting of employees into firms with long or short job tenure, differences between types of workers in determining the impact of firm-level characteristics, and the distinction between exit states such as new employment or unemployment. The data used have the advantage that they are representative of a large proportion

of German firms and their workers, can be arranged as a representative sample of the inflows into employment, and have detailed information on a large number of firm-specific variables.

Substantively, our results offer a number of conclusions that have previously been unavailable. First, the differences in mobility between different parts of the labor market are large. While some explanatory variables often used, such as firm size, do not account for the differences, institutions such as works councils and the availability of further training play a pronounced role in reducing mobility on the labor market.

Second, the positive effects of works councils and further training on job durations are high for blue collar skilled or semi-skilled workers but non-existent for white collar employees. The results imply that the activities of works councils are targeted to their traditional constituencies.

Third, persistence in individual mobility behavior is much reduced when firm-level heterogeneity is accounted for. For instance, the effect of lagged unemployment is much reduced in a specification with firm fixed effects. Other selection effects concern blue collar workers, who tend to select into long-tenure firms as compared to white collar workers.

Fourth, competing risks analysis shows that mobility to another job and exit to unemployment follow strikingly different processes. Among the findings for the firm-level variables, a works council slows down exit to both destinations. This suggests that works councils not only increase employment protection, but also reduce quits, e.g. by lobbying employers to provide better working conditions. It would be interesting to study the implications of these effects for long-term outcomes, such as profitability or firm entries and exits. By highlighting employer characteristics as a determinant of mobility, our study not only relates to the literature on individual labor market mobility but may also offer insights for the analysis of labor market institutions.

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Table 1: Definitions of Exit and Origin States

Employment state	Definition
a) Exit states	
Unemployment	Worker 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	Worker 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	Worker takes up employment with another employer within 60 days after separation and has recorded end of relationship
Recall	Worker takes up employment with the same employer after more than 90 days after separation and has recorded end of relationship
b) Previous employment states	
Unemployment	Worker 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 ≤ 1 year	Worker 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, was observed in the year before hiring
Non-employment > 1 year	Worker was not observed for at least 1 year before hiring
Recall	Worker was employed with current employer for more than 90 days before hiring, previous spell ended with recorded end of relationship, worker 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	Worker changed from job-to-job at most 60 days before employment
First employment	Worker not observed since January 1st, 1991, not older than 30 years at the first observed spell between 1996 and 2001

Table 2: Number of spells in the sample

	West # spells	East # spells
Number of employment spells	161,111	88,202
Exit state		
Unemployment (share)	20,810 (13)	33,649 (38)
Non-employment (share)	16,900 (10)	7,934 (9)
New employer (share)	30,246 (19)	12,344 (14)
Recall (share)	1,923 (1)	641 (1)
Censored spells (share)	91,232 (57)	33,634 (38)

Table 3: Results from Cox Estimation of Job Duration

	West Germany						East Germany					
	Individual-specific variables		Individual and firm-specific variables		Stratified estimation		Individual-specific variables		Individual and firm-specific variables		Stratified estimation	
Independent Variables	hazard ratio	t-Stat.	hazard ratio	t-Stat.	hazard ratio	t-Stat.	hazard ratio	t-Stat.	hazard ratio	t-Stat.	hazard ratio	t-Stat.
Age in years (reference group: 25-29)												
Age 30-34	0.900	(-2.63)	0.898	(-2.55)	0.871	(-3.89)	0.970	(-0.60)	0.976	(-0.47)	0.929	(-1.71)
Age 35-39	0.861	(-3.60)	0.866	(-3.30)	0.855	(-4.12)	0.985	(-0.31)	1.018	(0.35)	0.917	(-1.91)
Age 40-44	0.866	(-3.08)	0.861	(-3.11)	0.795	(-5.36)	0.959	(-0.90)	0.990	(-0.20)	0.922	(-1.91)
Age 45-52	0.696	(-7.29)	0.698	(-6.61)	0.705	(-7.75)	0.944	(-1.35)	0.984	(-0.37)	0.915	(-2.20)
Education (reference group: vocational training)												
Secondary school	1.095	(2.16)	1.054	(1.17)	1.105	(2.40)	1.063	(1.21)	1.038	(0.74)	1.171	(4.07)
A-Level	1.729	(7.38)	1.923	(7.26)	1.942	(8.75)	2.075	(5.75)	2.023	(6.06)	1.768	(5.07)
Voc. Training/A-Level	0.976	(-0.35)	1.065	(0.83)	1.100	(1.58)	1.283	(2.46)	1.280	(2.46)	1.163	(1.74)
University	0.776	(-5.08)	0.897	(-2.00)	0.998	(-0.04)	1.008	(0.15)	1.025	(0.42)	1.088	(1.42)
Job position (reference group: unskilled blue collar)												
Skilled blue collar	0.929	(-1.78)	0.846	(-3.86)	0.742	(-6.62)	0.764	(-7.85)	0.790	(-6.05)	0.791	(-5.29)
White collar	0.645	(-8.43)	0.690	(-6.52)	0.734	(-5.37)	0.469	(14.59)	0.537	(10.99)	0.705	(-5.76)
Master craftsman	0.735	(-2.71)	0.678	(-3.37)	0.804	(-1.84)	0.450	(-6.49)	0.493	(-5.38)	0.610	(-4.92)
Part-time worker	1.036	(0.45)	1.106	(1.13)	1.088	(1.21)	1.049	(1.53)	1.017	(0.49)	1.044	(0.96)
Previous employment state (reference group: job-to-job change)												
Unemployment	1.747	(17.21)	1.566	(13.22)	1.330	(8.62)	2.286	(26.35)	1.826	(18.05)	1.399	(10.24)
Non-empl. ≤ 1 year	1.959	(11.30)	1.750	(8.32)	1.557	(7.53)	2.031	(8.83)	1.764	(6.59)	1.501	(4.50)
Non-empl. > 1 year	1.763	(10.54)	1.597	(7.87)	1.473	(7.97)	1.897	(8.29)	1.644	(6.07)	1.310	(3.41)
Recall	1.883	(7.16)	1.621	(5.88)	1.144	(1.49)	2.162	(6.50)	1.837	(3.63)	1.596	(4.87)
First employment	1.826	(7.13)	1.628	(5.37)	1.444	(4.26)	1.409	(3.18)	1.169	(0.91)	1.118	(1.44)
Local labor market												
Unemployment rate	0.943	(1.51)	0.948	(-1.33)	0.985	(-0.34)	0.938	(-3.21)	0.977	(-1.04)	0.982	(-0.88)

Table 3 continued

Investments in (reference groups: no investment)									
Inv. in ICT		0.909	(-2.81)	0.861	(-3.58)		0.916	(-2.81)	0.898	(-2.94)
Inv. in further train.		0.869	(-3.57)	0.900	(-1.65)		0.895	(-3.01)	1.147	(2.78)
Institutions (reference groups: no collective agreement, no works council)										
Collective agreement, industry-level		0.930	(-1.72)	1.087	(1.16)		0.904	(-3.20)	0.9160	(-1.70)
Collective agreement, firm-level		1.025	(0.44)	1.282	(2.43)		0.961	(-1.08)	0.907	(-1.96)
Wages > collective wage agreement		0.921	(-2.46)	0.888	(-2.30)		0.914	(-2.14)	0.833	(-3.06)
Works council		0.784	(-7.31)				0.765	(-8.87)		
Year of setting up (reference group: founded 1981-1990)										
Founded ≤ 1980		1.044	(0.85)				0.849	(-1.23)		
Founded 1991-1995		0.948	(-0.90)				0.918	(-2.57)		
Founded ≥ 1996		1.101	(1.11)				0.702	(-7.41)		
Wald Chi²	1,199	3,000		663		2,722	5,457		456	
Log likelihood	-1,090,882	-1,085,651		-457,174		-524,386	-520,796		-213,532	

Note: Results are shown as hazard ratios (t-values in parentheses). Dummies for nationality, occupation, industry, year, federal state ,firm size and legal form are included in all estimations but not reported. Results are made available by the authors on request.

Table 4: Interactions Between Firm and Worker Characteristics

	Effects of further training					Effects of a works council			
	West Germany		East Germany			West Germany		East Germany	
	hazard ratio	t-Stat.	hazard ratio	t-Stat.		hazard ratio	t-Stat.	hazard ratio	t-Stat.
Secondary school	0.938	(0.54)	1.332	(1.12)	Unskilled blue collar	0.787	(5.51)	0.973	(0.62)
A-Level	1.575	(0.68)	0.870	(3.83)	Skilled blue collar	0.792	(2.96)	0.758	(4.38)
Vocational training	0.825	(4.61)	0.468	(1.52)	White collar	1.287	(1.86)	0.990	(0.08)
Voc. Training/A-Level	1.250	(0.37)	0.619	(1.54)	Master craftsman	0.907	(0.33)	1.201	(0.44)
University	1.483	(0.91)	1.182	(0.65)	Part-time worker	1.480	(1.02)	1.432	(4.03)

Note: Results are shown as hazard ratios (t-values in parentheses), t-values are calculated by the delta method.

Table 5: Likelihood Ratio Test Statistics

	West Germany	East Germany
Training–education interaction	344 (4)	65 (4)
Works council–job position interaction	407 (5)	483 (5)
Competing risks	32,207 (78)	12,958 (73)

Note: Degrees of freedom in parentheses.

Table 6: Results from Independent Competing Risk Estimation

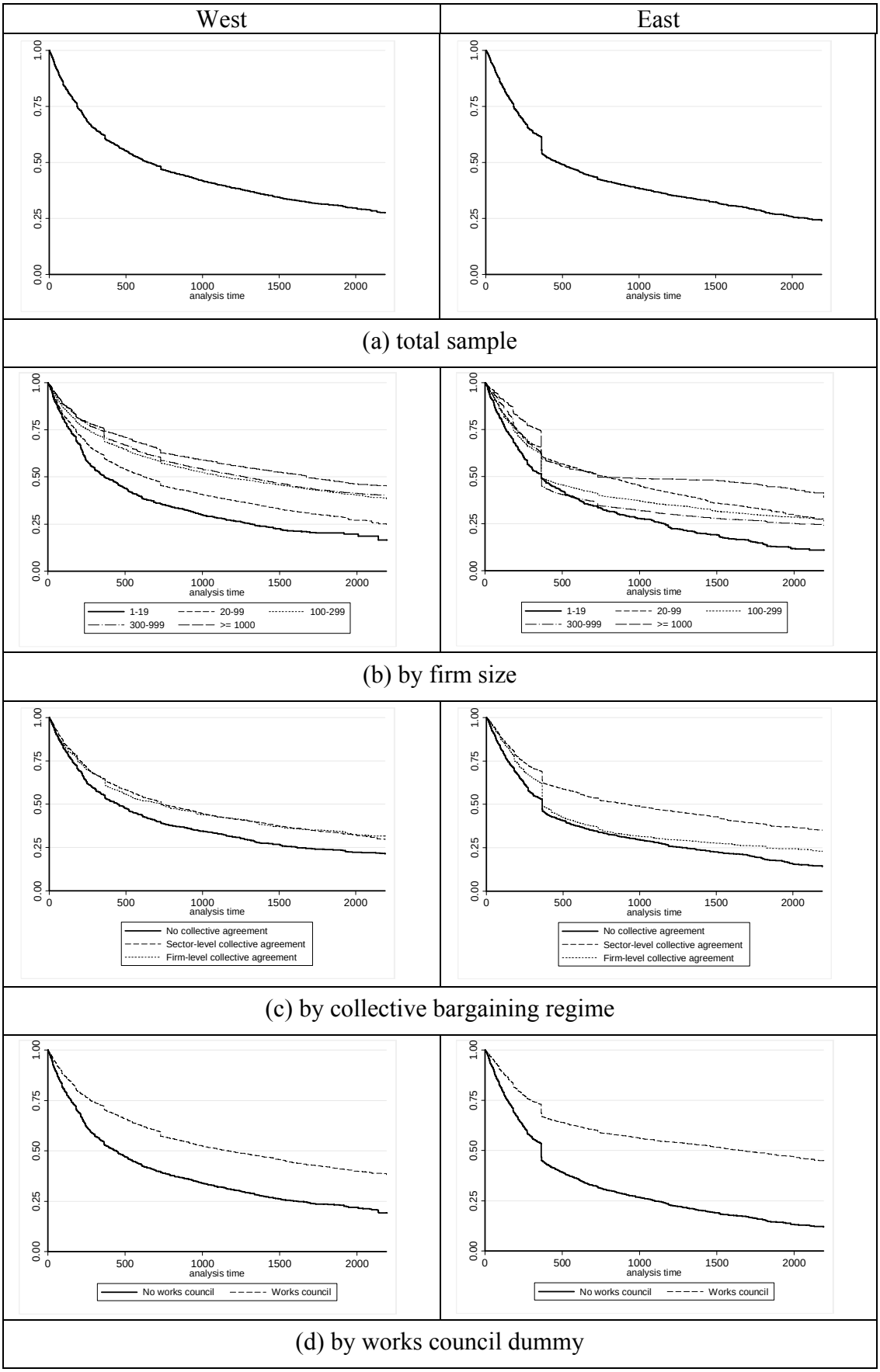
	West Germany				East Germany			
	Unemployment		Job-to-Job		Unemployment		Job-to-Job	
Independent Variables	hazard ratio	t-Stat.	hazard ratio	t-Stat.	hazard ratio	t-Stat.	hazard ratio	t-Stat.
Age in years (reference group: 25-29)								
Age 30-34	0.910	(-1.23)	0.944	(-0.93)	1.059	(0.79)	0.894	(-1.27)
Age 35-39	0.959	(-0.53)	0.825	(-3.01)	1.149	(1.95)	0.864	(-1.68)
Age 40-44	1.153	(1.57)	0.741	(-4.14)	1.126	(1.81)	0.826	(-2.09)
Age 45-52	0.969	(-0.37)	0.517	(-8.53)	1.265	(3.84)	0.591	(-6.15)
Education (reference group: vocational training)								
Secondary school	1.017	(0.23)	1.051	(0.69)	1.095	(1.65)	0.790	(-2.02)
A-Level	1.176	(0.66)	1.843	(4.93)	0.945	(-0.23)	1.296	(0.54)
Voc. Training/A-Level	0.796	(-1.31)	1.024	(0.26)	1.075	(0.57)	1.537	(2.16)
University	0.663	(-3.71)	1.033	(0.41)	0.840	(-2.01)	1.331	(2.83)
Job position (reference group: unskilled blue collar)								
Skilled blue collar	0.747	(-3.83)	0.977	(-0.33)	0.794	(-4.85)	0.874	(-1.94)
White collar	0.564	(-5.11)	0.908	(-1.15)	0.458	(-9.35)	0.606	(-4.86)
Master craftsman	0.502	(-3.07)	1.110	(0.61)	0.472	(-5.17)	0.610	(-2.30)
Part-time worker	0.964	(-0.23)	1.069	(0.50)	1.037	(0.88)	0.780	(-2.66)
Previous employment state (reference group: job-to-job change)								
Unemployment	3.097	(18.08)	0.877	(-2.41)	2.600	(18.42)	1.048	(0.78)
Non-empl. ≤ 1 year	1.290	(1.59)	1.214	(1.87)	1.138	(0.78)	1.465	(2.76)
Non-empl. > 1 year	0.818	(-1.56)	1.170	(1.76)	0.870	(-1.11)	1.347	(1.68)
Recall	0.863	(-0.47)	0.608	(-2.74)	0.683	(-1.68)	0.791	(-0.72)
First employment	0.767	(-1.08)	1.035	(0.25)	0.707	(-1.30)	0.868	(-0.46)
Local labor market								
Unemployment rate	1.111	(1.46)	0.845	(-2.74)	0.959	(-1.45)	1.040	(0.89)

Table 6 continued

Investments in (reference groups: no investment, respectively)								
Inv. in ICT	0.928	(-1.24)	0.826	(-3.75)	0.930	(-1.85)	0.885	(-2.04)
Inv. in further training	0.857	(-2.43)	0.844	(-2.89)	0.831	(-4.04)	1.008	(0.11)
Institutions (reference groups: no collective agreement, no works council, respectively)								
Collective agreement, industry-level	0.911	(-1.24)	0.976	(-0.37)	0.993	(-0.17)	0.750	(-4.41)
Collective agreement, firm-level	1.123	(1.12)	0.877	(-1.43)	1.037	(0.81)	0.798	(-2.71)
Wages > collective wage agreement	0.885	(-1.98)	0.965	(-0.73)	0.902	(-1.73)	0.950	(-0.72)
Works council	0.825	(-3.23)	0.781	(-5.08)	0.772	(-7.11)	0.678	(-7.19)
Year of setting up (reference group: founded 1981-1990)								
Founded ≤ 1980	1.149	(1.59)	0.885	(-1.69)	0.629	(-2.47)	1.387	(1.45)
Founded 1991-1995	1.005	(0.05)	0.898	(-1.31)	0.912	(-2.22)	0.873	(-2.04)
Founded ≥ 1996	0.962	(-0.26)	1.129	(0.97)	0.587	(-7.95)	0.965	(-0.43)
Wald Chi²	2,763		1,542		5,384		1,224	
Log likelihood	-383,597		-441,234		-298,510		-144,877	

Note: Results are shown as hazard ratios (t-values in parentheses). Dummies for nationality, occupation, industry, year, federal state, firm size and legal form are included in all estimations but not displayed. Results are made available by the authors on request.

Figure 1: Kaplan-Meier Survivor Functions of Remaining in the Job



(a) total sample

(b) by firm size

(c) by collective bargaining regime

(d) by works council dummy