Cornell University
ILR School

ILRReview

Volume 64 | Number 1 Article 5

9-30-2010

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

Bernhard Boockmann
Institute for Applied Economic Research (IAW), Bernhard.Boockmann@iaw.edu

Susanne Steffes
Centre for European Economic Research (ZEW), steffes@zew.de

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

Abstract
The authors examine job durations of German workers using the Linked Employer–Employee Data of the Institute for Employment Research (LIAB). Results indicate that exit rates are strongly influenced by firm characteristics, such as the existence of works councils and the opportunity for further training. The effects of these characteristics, however, are limited to jobs held by blue-collar workers or by those possessing vocational–educational skills. Changes in coefficients across specifications provide clear evidence for a sorting process whereby workers with expected long 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-level and firm-level characteristics differ greatly in their effects on worker job exits to unemployment and to new jobs. Evidence suggests that works councils decrease exits to both states, but only for blue-collar workers.

Keywords
Job durations, tenure, linked employer-employee data, works councils

WORKERS, FIRMS, OR INSTITUTIONS: WHAT DETERMINES JOB DURATION FOR MALE EMPLOYEES IN GERMANY?

BERNHARD BOOCKMANN AND SUSANNE STEFFES*

The authors examine job durations of German workers using the Linked Employer–Employee Data of the Institute of Employment Research (LIAB). Results indicate that exit rates are strongly influenced by firm characteristics, such as the existence of works councils and the opportunity for further training. The effects of these characteristics, however, are limited to jobs held by blue-collar workers or by those possessing vocation–educational skills. Changes in coefficients across specifications provide 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 effects on worker job exits to unemployment and to new jobs. Evidence suggests that works councils decrease exits to both states, but only for blue-collar workers.

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, we are interested in learning the relative importance of and relationship among worker characteristics, firm-level variables, and institutions for job duration. Empirical evidence demonstrates that individual attributes such as age or education affect job transitions; however, firms themselves 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 on worker exits is likely to differ with worker characteristics. For instance, works councils may be dominated by blue-collar workers, increasing job stability for only this group. To capture these

* Bernhard Boockmann is Scientific Managing Director at the Institute for Applied Economic Research (Institut für Angewandte Wirtschaftsforschung, IAW) in Tübingen, Germany, and Susanne Steffes is a researcher in Labor Economics, Personnel Management, and Social Security at the Centre for European Economic Research (ZEW) in Mannheim, Germany.

The data used in this paper may be accessed at the Research Data Center of the IAB in Nürnberg, Germany. Contact details are available at http://fdz.iab.de/. Copies of the computer programs used to generate the results presented in the paper are available from the authors on request via email at Bernhard.Boockmann@iaw.edu or steffes@zew.de.

Industrial and Labor Relations Review, Vol. 64, No. 1 (October 2010). © by Cornell University.
0019-7939/00/6401 $05.00

differences, we use a flexible parametric specification in our empirical estimations. In addition, we analyze the existence of sorting processes in which workers with long expected job durations are matched to firms offering stable employment and vice versa.

Second, we want to know whether these effects differ among exit states. A job-search model can tell us whether 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, its presence might increase employers' firing costs and thereby influence the productivity threshold (Mortensen and Pissarides 1994) below which layoffs are made. This threshold affects the dismissal rate and, if most laid-off workers experience a period of unemployment following dismissal, the number of job-to-unemployment transitions will be affected as well. Though the direction of the influence is the same in both cases, the two mechanisms themselves are distinct and the magnitude of that influence is likely to differ.

We investigate these issues on the basis of a German dataset which is more appropriate and contains more information than the datasets that have heretofore been available. Existing studies use stock data containing information that relates only to the current spell at the date of interview, 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 using stock data rather than flow data is that one is able to observe longer maximum durations in the former, 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, with only one-quarter of the spells being right-censored.

With some exceptions, such as Dostie (2005) and Mumford and Smith (2004), previous studies have used mostly data with little information on the firm side, such as worker-level administrative or survey data. Estimations based on this type of data yield unbiased coefficients if firm fixed effects are included (e.g. Bronars and Famulari 1997). The data often contain only one worker per firm, however, which means that fixed effects are not identified. Moreover, one can ascertain neither the effect of firm-level characteristics varying over time nor their interaction with individual covariates.

Our results show that differences in mobility between different parts of the labor market are substantial. 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

We use a theoretical framework for modeling the probability of job exit based on the search and matching approach pioneered by Burdett (1978) and Jovanovic (1979).[1] In the simplest formulation, employed workers will take any job that pays better than their current one. The probability of workers leaving their 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 earn a high current wage and less likely to exit their current job. 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.

[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.

We can extend the model by adding involuntary dismissals. Most often, dismissals are taken as exogenous. Under this assumption, dismissals are determined by stochastic productivity shocks. If we also consider profit maximization by firms, and thus, move from partial equilibrium for workers to general equilibrium, it is important to include hiring costs (see Mortensen and Pissarides 1994). In this case, retaining a given worker may be valuable for the firm because it preserves the option of employing that 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 that influence search frictions become important determinants of job mobility.

The Jovanovic (1979) model assumes asymmetric information at the start of a new employment relationship. The quality of the match is uncertain initially but becomes an experience good as time passes. Its value increases with tenure, that is, until all information is available for both employer and employee or until the match comes to an end. With increasing outside options, such as 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. In comparison with other countries, the German labor market is highly regulated. For example, 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 though thresholds in terms of establishment size exist (Koller 2005). Below, 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 5 employees, workers are legally entitled to establish a works council. In practice, however, employees in small establishments often do not take the initiative to set one up. 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 of the notification of dismissal. In addition, the works council can make certain 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.

Operating at the industry level, a second

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

[3] Moscarini (2005) as well as Pries and Rogerson (2005) brought 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 2007). For a discussion of structural and reduced-form approaches, see Cahuc and Zylberberg (2004: 146).

[5] Another German institution of worker participation, employee representation on supervisory boards, has no direct effect on hiring and firing practices and is, therefore, not discussed here.

institution affecting the German labor market is the collective agreement. This is generally negotiated between the union and the employer's association and is mandatory for all employees whose employer is a member of an employers' association. In 2003, 70 percent of employees in western Germany[6] and 47 percent of employees in eastern Germany were covered by collective agreements. Since the late 1990s, however, the coverage rate has declined in western Germany (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 adjusting working hours or reducing 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, which can lead the firm to adjust employment instead of wages in bad times, thereby decreasing 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 the surveyed establishments. We use the longitudinal version I of the LIAB, which contains daily employment and benefit recipient information for the period 1991–2001.

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 containing valid interviews from 1999 to 2001.[7]

The employee part of the LIAB consists of the Employment Statistics Register of the Federal Employment Agency. This administrative data record is based on declarations of employers to German social insurance institutions. Depending on the circumstances, misreporting is legally treated either as an infraction (i.e., a violation of a regulation less serious than a criminal act) or as a criminal offense; 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.[8] 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 an unemployment benefit is received 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.

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

[6] By western Germany we refer to the federal states that were part of the Federal Republic of Germany before unification in 1990. By eastern Germany we refer to those federal states that were part of the German Democratic Republic.

[7] Worker separations due to plant closures cannot be observed in our data.

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

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.[9] We assume the end of an employment spell occurs if two conditions apply. The first occurs when the individual is observed to move into unemployment, or non-employment, or is hired by a new employer. The second occurs when 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.[10]

One advantage of these data is that they enable us to observe which of four employment states a worker moves into after the end of a spell: unemployment, new employment, recall to previous employment, 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 benefits from the German Federal Employment Agency. Since not all unemployed workers qualify for unemployment insurance (UI), and benefits may be temporarily suspended in the case of sanctions, individuals can be unemployed without receiving UI benefits (see 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 (new employment) is defined as a separation followed by an employment spell that commences within 60 days of the end of the previous one. It is likely that in most of these cases, the worker already knew about the new employment relationship when the previous job ended. A third employment state that may occur is one in which the employee is recalled to the previous employer because of fluctuating demand, temporary illness, or other reason. 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. An 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. Further, we use a separate category for employees who are likely to be in their first job. This category consists of individuals under the age of 30 who were starting their first spell in the data after 1996 but were not observed between 1991 and 1995. Table 1 summarizes the definitions of the exit and origin states.

Sample Definition and Descriptive Statistics

We analyze employment durations of male workers only since employment histories of females are often determined by interruptions for maternity leave. With our data, we cannot observe precisely whether a woman leaves the labor market solely because of the birth of a child. Moreover, it is not possible to analyze the determinants for employment durations of mothers or factors such as the availability of child care, a partner's income, and so on, because we do not have this information.

In order to avoid bias due to left-censoring, we restrict our analysis to all employment spells that commenced in the

[9] In order to avoid having many short spells due to seasonal factors, we define two successive employment spells with the same employer (even if we observe an unemployment spell in between) as one spell if reemployment takes place within a maximum of 90 days. If an observed spell is interrupted for reasons such as parental leave, illness or sabbatical, we also define the whole period as one employment spell (independently of the duration of the interruption).

[10] 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, we cannot be certain 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.

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 and 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 an end of the employment relationship
Job-to-job change	Worker takes up employment with another employer within 60 days after separation and has recorded an end of the employment relationship
Recall	Worker takes up employment with the same employer after more than 90 days after separation and has recorded an end of the employment relationship
b) Previous employment states	
Unemployment	Worker received unemployment benefits for at least one day during 60 days before hiring and 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 experiences a time gap of more than 90 days in successive employment with the same employer, previous spell ended with recorded end of relationship, worker received no unemployment benefits during 60 days before hiring and did not change from job to job during 60 days before employment
Job-to-job change	Worker changed from job to job a maximum of 60 days before employment
First employment	Worker was not observed since January 1, 1991 and was not older than 30 years at the first observed spell between 1996 and 2001

sample establishments between 1996 and 2001. We restrict data to persons aged 25 to 52 to exclude individuals in vocational training and those who 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 (less than 15 hours per week), vocational training, or working from home. All spells with missing covariate information are eliminated. These requirements leave us with a sample of 249,313 employment spells, 88,202 of which are from eastern Germany.[11]

[11] The number of spells according to the different exit states is reported in Table 2. Summary statistics of all covariates can be made available by emailing requests to the authors.

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)

Kaplan–Meier survivor functions shown in graph form in Panel (a) of Figure 1 offer a first descriptive impression of job durations in the sample.[12] There are striking differences between eastern and western Germany, underlining the importance of separate analyses. Exit occurs more slowly in the western part of Germany. In the East, there is a substantial drop in the survivor function after exactly one year. This is probably due to the higher incidence of temporary employment in eastern Germany, especially in the job creation schemes used widely during the observation period.[13]

Separate analyses according to firm characteristics demonstrate that in western Germany, survival rates are higher for larger establishments (Panel (b)). However, the ordering is less clear in eastern Germany. Consonant with the findings of Gerlach and Stephan (2005), workers in firms with collective agreements at the firm or industry level have longer job durations in western Germany (Panel (c)). In the East, the difference occurs between firms that do or do not adhere to an industry wage agreement. Most strikingly, median durations are two to three times longer if the establishment has a works council (Panel (d)). Because 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:

$$\lambda(t) = \lim_{\Delta t \to 0} \Delta t^{-1} P[t \leq T < t + \Delta t \mid T \geq t]$$

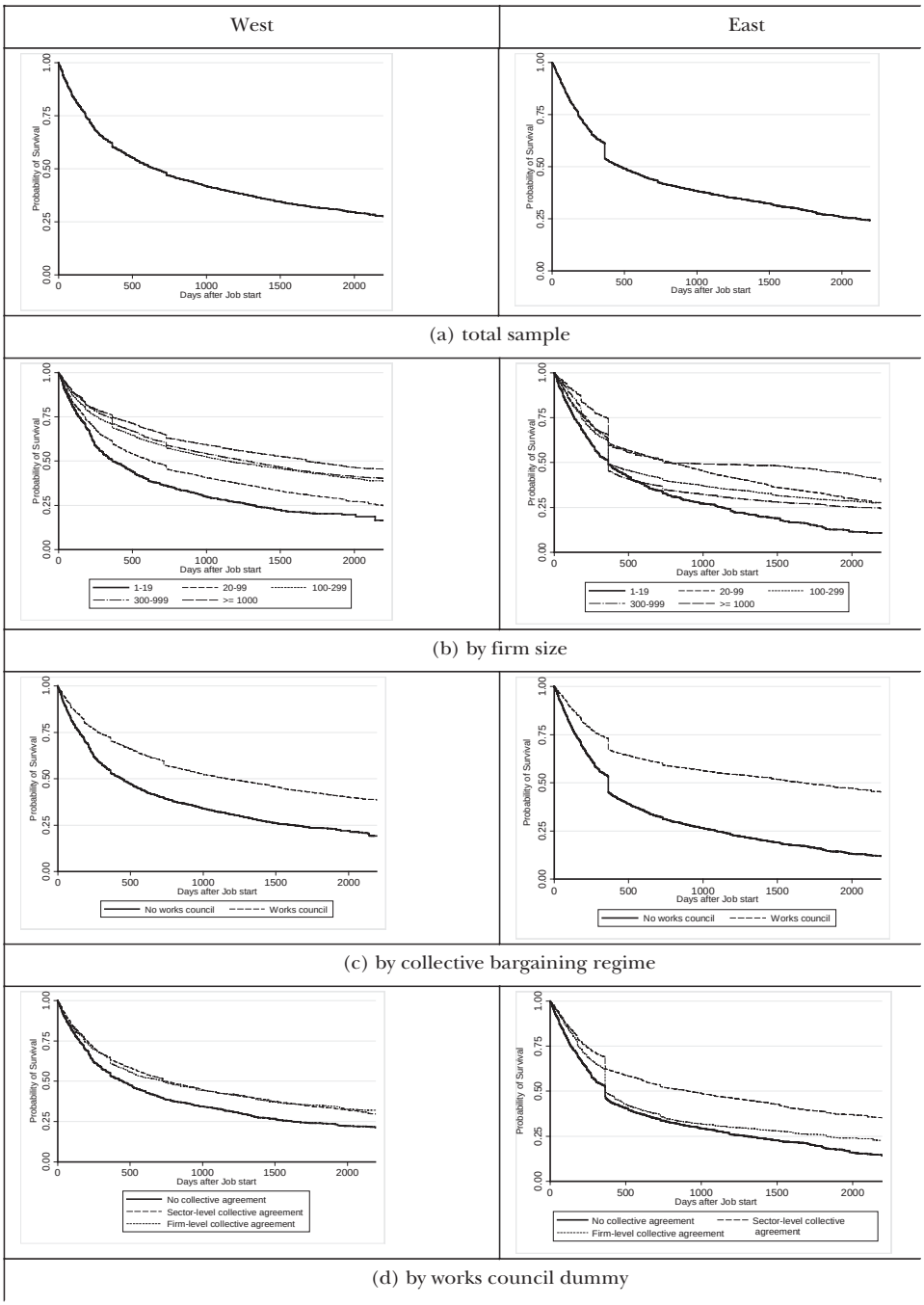
where t indicates time and T is the actual job duration. If job-to-job and job-to-unemployment exits follow the same processes, a single exit state model is appropriate. For individual i (i=1,…, n_j) employed in firm j (j=1,…,m) at time t, the hazard rate is specified as $\lambda_{ij}(t) = \lambda_0(t) \exp[z_{ij}(t)'\theta]$, with $z_{ij}(t)$ denoting a vector of individual- or firm-specific characteristics that may vary over time (including 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 $\lambda^k_{ij}(t) = \lambda^k_0(t) \exp[z_{ij}(t)'\theta^\kappa]$ and separate parameter vectors are 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

[12] Kaplan–Meier functions are useful insofar as 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.

[13] On average, the share of temporary employment in the stock of employees in the period 1995–2002 was 11.5 percent in eastern Germany, compared to 7.0 percent in western Germany. In eastern Germany, 33.6 percent of temporary jobs were subsidised by job creation schemes, as opposed to just 4.3 percent in western Germany (Boockmann and Hagen 2005: 156). Job creation schemes normally last for one year.

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



do not allow extensions like simultaneous estimations, such as those in Altonji and Williams 2005, or Topel and Ward 1992; therefore, we 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[14] 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 establishment or employee level that is 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 we have assumed a separate baseline hazard for each establishment while assuming the coefficients of the covariates to be the same. This is similar to the within-groups estimator in linear regression. In the stratified case, we can identify the coefficients of time-varying firm-specific variables but not those of time-constant variables varying only by stratum (i.e., establishment). 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 3,176 individuals (1.3 percent of all observations) have more than one spell in more than one establishment in the sample.

Coefficient Estimates For Individual and Job-Specific Characteristics

Table 3 presents the results of Cox estimations for workers in eastern and western Germany. In both cases, the first column is taken from estimation with individual-specific variables only, whereas 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, we can assess the importance of worker self–selection into certain firms (Mumford and Smith 2004). In order to facilitate the quantitative interpretation of the effects, the table displays hazard ratios.[15] Since 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 the results for individual and job-specific covariates at length.

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.[16] This effect, however, is not found for workers in eastern Germany. 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

[14] There is some correlation between variables such as works councils and collective agreements, but it is far from perfect.

[15] Hazard ratios are obtained by exponentiating the coefficients and indicating 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 percent if the covariate increases by one unit.

[16] The results are also consonant with the "job shopping" theory (Johnson 1978; Viscusi 1980), based on slightly different assumptions.

Table 3. Results from Cox Estimation of Job Duration

	Western Germany						Eastern 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 –Le vel	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)

Continued

Table 3. Results from Cox Estimation of Job Duration, Continued

	Western Germany						*Eastern 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.*
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	

Notes: Results are shown as hazard ratios (t-values in parentheses). They indicate the multiplicative effect on the probability of moving resulting from a unit change in the independent variable. 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.

changes. Segmentation theory predicts a selection of low-skilled workers into unstable jobs. The findings of previous empirical studies on this subject 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, both workers lacking vocational training or university education and 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 demonstrate that individual employment history is an important determinant of job duration (see also Booth et al. 1999; Battu et al. 2002). In fact, individuals entering employment from previous unemployment or non-employment states face a significantly higher risk of exit, compared to the reference group of job changers. When we account for firm heterogeneity, however, 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 federal state. It is lagged by one year because the unemployment rate may be endogenous to job exit.[17] Results show that the effect of local labor market conditions is weak.

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.[18] Among the time–varying covariates, we use a dummy indicating investment in information and communication technology (ICT) in the previous year. On the one hand, investment in 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 are unwilling) to apply new technologies, and firms hire new employees to replace them. As our estimates show, investment in 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 in their employees' specific human capital, they have an interest in reducing quits. As our results show, the expected negative effect of training on job exit for workers in western Germany does indeed occur. However, it becomes insignificant in stratified estimation. In the East, the negative effect is in fact reversed in the stratified estimation results. Our results also show that the presence of a works council leads to a significant reduction in the job exit hazard by more than 20 percent, a finding that is consistent with the broader range of literature on works councils (see, for example, the survey by Addison et al. 2004).

We distinguish between industry- and firm-specific collective agreements, the base groups being 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. For eastern Germany, there is some evidence that industry-level collective bargaining stabilizes employment, but even here the effect on the hazard is less than half as strong as the effect of works councils. For the West, the results are more mixed. Here, the exit rate is actually higher with firm-level collective agreements in comparison with the base group. However, only a small part of the workforce (11 percent) is in this group, and they are concentrated in big enterprises. Hence, the result should not be over-interpreted. As expected, wages above the bargained level lower the exit rate, suggesting that higher wages versus standard compensation make job-to-job changes less attractive.

[17] We eliminate a time trend from the regional unemployment series.

[18] The following variables either exhibit no variation over time or vary over time only for a small percentage of establishments: industry, federal state, firm size, legal form, establishment age, and works council.

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 are omitted from the table but are available on request). In younger establishments, job durations are substantially longer in eastern Germany whereas the effect is insignificant for the western part. To interpret this finding, it is important to recall that job exits from companies that have shut down 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 these effects are relevant are the presence of a works council and the provision of further training by the enterprise. Table 4 lists hazard ratios similar to those in Table 2, but they are differentiated according to worker groups. They are taken from separate estimations which are otherwise identical to those displayed in Table 3.

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 including additional training is more productive for workers with vocational training but 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 workers in eastern Germany. These differences in the interaction effects between the East and the West are likely to be caused by the two different systems of education and training in place before 1990, which has resulted in different shares of workers in each category.

A second interaction occurs 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 types of separations of blue-collar workers. The results support this view. A significantly negative effect of works councils on job exit is only evident for blue-collar workers. In particular, the job exit probabilities for skilled blue-collar workers are reduced by 21 percent in western Germany and 24 percent in eastern Germany, whereas white-collar workers are not significantly affected. For part-time workers, the sign of the effect is actually reversed, suggesting that works councils increase labor market segmentation; this effect, however, is significant only for eastern Germany. All interaction effects are highly significant according to the likelihood ratio tests in the first two rows of Table 5.

Competing Risks

So far, we have assumed that the mechanism driving job exit is the same across all exit states. It is quite plausible, however, that the independent variables influence exit into different states in different ways. Using the definitions from Table 1, we distinguish among four exit states: unemployment, new employment (changing to another employer), recall, and non-employment. We display the results for the first two exit states only because the number of observations is very low with respect to the "recall" 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 those in the middle columns of Table 3. Likelihood-ratio tests of the null

Table 4: Interactions Between Firm and Worker Characteristics

	Effects of further training					***Effects** of a works council*			
	Western Germany		*Eastern Germany*			*Western Germany*		*Eastern 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)

Notes: Results are shown as hazard ratios (t-values in parentheses); t-values are calculated by the delta method. They indicate the multiplicative effect on the probability of moving, resulting from a unit change in the independent variable.

Table 5. Likelihood Ratio Test Statistics

Variables	*Western Germany*	*Eastern 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.

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 reveal 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. In line with the "job shopping" argument, however, the probability of a job-to-job change declines substantially with age. It is only about half as high for workers aged 45–52 than it is for workers aged 25–29 in western Germany. Possessing 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 it 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.

Collective bargaining arrangements are insignificant for both exit states except for job-to-job mobility in eastern Germany, where the wage differential between establishments bound or not bound by collective agreements are higher (Kohn and Lembcke 2007). Works councils substantially 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 eastern Germany. The effects of firm size are mostly insignificant (they not included here). The regional unemployment rate reduces the job-to-job hazard in the western part of the nation, most likely through a lower rate at which new job offers come in.

Conclusion

The use of linked employer–employee data dramatically 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 we have used are advantageous in that they represent a large proportion of German firms and their workers, that they can be arranged as a representative sample of the inflow into employment, and that they provide detailed information on a large number of firm-specific variables.

Substantively, our results offer a number of conclusions that previously have been unavailable. First, the differences in mobility between different parts of the labor market are large. Though 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 additional training on job durations are high for blue-collar skilled or semi–skilled workers but are non-existent for white-collar employees. These results imply that the activities of works councils are targeted to their traditional constituencies. Third, persistence in individual mobility behavior is greatly reduced when firm-level heterogeneity is accounted for. For instance, the effect of lagged unemployment is much smaller in a specification with firm fixed

Table 6. Results from Independent Competing Risk Estimation

	Western Germany				*Eastern 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)
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)

Continued

Table 6. Results from Independent Competing Risk Estimation Continued

	Western Germany				*Eastern 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.*
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 Chi^2	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). They indicate the multiplicative effect on the probability of moving resulting from a unit change in the independent variable. Dummies for nationality, occupation, industry, year, federal state, firm size, and legal form are included in all estimations but are not displayed. Results are made available by the authors on request.

effects. Other selection effects concern blue-collar workers, who tend to select into long-tenure firms, in contrast to white-collar workers. Fourth, competing risks analysis demonstrates 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, indicating that works councils not only increase employment protection but also reduce quits, for example, 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.

REFERENCES

Addison, John T., Claus Schnabel, and Joachim Wagner. 2001. "Works Councils in Germany: Their Effects on Establishment Performance." *Oxford Economic Papers*, Vol. 53, No. 4, 659–94.

Addison, John T., Claus Schnabel, and Joachim Wagner. 2004. "The Course of Research into the Economic Consequences of German Works Councils." *British Journal of Industrial Relations*, Vol. 42, No. 2, 255–81.

____, Lutz Bellmann, Claus Schnabel, and Joachim Wagner. 2004. "The Reform of the German Works Constitution Act: A Critical Assessment." *Industrial Relations*, Vol. 43, No. 2, 392–420.

Addison, John T., and Paulino Teixeira. 2006. "The Effect of Works Councils on Employment Change." *Industrial Relations*, Vol. 45, No. 1, 1–25.

Altonji, Joseph G., and Nicolas Williams. 2005. "Do Wages Rise with Job Seniority? A Reassessment." *Industrial and Labor Relations Review*, Vol. 58, No. 3, 370–97.

Battu, Harminder, Robert McMaster, and Michael White. 2002. "Tenure and Employment Contracts—An Empirical Investigation." *Journal of Economic Studies*, Vol. 29, No. 2, pp. 131–149.

Boockmann, Bernhard, and Tobias Hagen. 2005. *Befristete Beschäftigungsverhältnisse – Brücken in den Arbeitsmarkt oder Instrumente der Segmentierung?* Baden–Baden: Nomos.

Booth, Alison L., Marco Francesconi, and Carlos Garcia-Serrano. 1999. "Job Tenure and Job Mobility in Britain." *Industrial and Labor Relations Review*, Vol. 53, No. 1, 43–70.

Bronars, Stephen G., and Melissa Famulari. 1997. "Wage, Tenure, and Wage Growth: Variation within and across Establishments." *Journal of Labor Economics*, Vol. 15, No. 2, 285–317.

Burdett, Kenneth. 1978. "A Theory of Employee Job Search Quit Rates." *American Economic Review*, Vol. 68, No. 1, 212–220.

____, and Dale T. Mortensen. 1998. "Wage Differentials, Employer Size, and Unemployment." *International Economic Review*, Vol. 39, No. 2, 257–273.

Cahuc, Pierre, and André Zylberberg. 2004. *Labor Economics*. Cambridge, MA: The MIT Press.

Cleves, Mario, William W. Gould, and Roberto G. Gutierrez. 2002. *An Introduction to Survival Analysis Using Stata*. College Station, TX: Stata Press.

Cox, David R. 1972. "Regression Models and Life–Tables." *Journal of the Royal Statistical Society*, Vol. 34, No. 2, 187–202.

Dohmen, Thomas J., and Gerard A. Pfann. 2003. "Worker Separations in a Nonstationary Corporate Environment." *European Economic Review*, Vol. 48, No. 3, 645–63.

Dostie, Benoit. 2005. "Job Turnover and the Returns to Seniority." *Journal of Business and Economic Statistics*, Vol. 23, No. 2, 192–99.

Dustmann, Christian, and Costas Meghir. 2005. "Wages, Experience and Seniority." *Review of Economic Studies*, Vol. 72, No. 1, 77–108.

Fitzenberger, Bernd, Karsten Kohn, and Alexander C. Lembcke. 2008. "Union Density and Varieties of Coverage: The Anatomy of Union Wage Effects in Germany." ZEW Discussion Paper No. 08–012, Mannheim.

____, and Ralf Wilke. 2004. "Unemployment Durations in West–Germany before and after the Reform of the Unemployment Compensation System during the 1980s." ZEW Discussion Paper No. 04–24, Mannheim.

Frick, Bernd. 1996. "Co-determination and Personnel Turnover: The German Experience." *Labour*, Vol. 10, No. 2, 407–30.

Gerlach, Knut, and Gesine Stephan. 2005. "Individual Tenure and Collective Contracts." IAB Discussion Paper No. 10, Nuremberg.

Heinbach, Wolf D. 2007. "Wages in Wage-Setting Regimes with Opening Clauses." *AStA – Wirtschafts– und Sozialstatistisches Archiv*, Vol. 1, No. 3, 233–45.

Holzer, Harry J., and R.J. LaLonde. 2000. "Job Change and Job Stability among Less–skilled Younger Workers." In Card, David, and Rebecca M. Blank, eds., *Finding Jobs: Work and Welfare Reform*, 125–129. New York: Russell Sage Foundation.

Johnson, William R. 1978. "A Theory of Job Shopping." *Quarterly Journal of Economics*, Vol. 92, No. 2, 261–77.

Jolivet, Grégory, Fabien Postel-Vinay, and Jean-Marc Robin. 2006. "The Empirical Content of the Job Search Model: Labor Mobility and Wage Distributions in Europe and the US." *European Economic Review*, Vol. 50, No. 4, 877–907.

Jovanovic, Boyan. 1979. "Job Matching and the Theory of Turnover." *Journal of Political Economy*, Vol. 87, No. 5, 972–90.

Kalbfleisch, Jack D., and Ross L. Prentice. 2002. *The Statistical Analysis of Failure Time Data*. Hoboken, NJ: John Wiley & Sons.

Kohn, Karsten, and Alexander Lembcke. 2007. "Wage Distributions by Bargaining Regime." *AStA Wirtschafts– und Sozialstatistisches Archiv*, Vol. 1, No. 3, 247–261.

Koller, Lena. 2005. "Arbeitsrechtliche Schwellenwerte – Regelungen an der Schwelle zur Unüberschaubarkeit." Friedrich–Alexander–Universität Erlangen–Nürnberg, Lehrstuhl für Arbeitsmarkt– und Regionalpolitik. Diskussionspapier Nr. 40.

Mortensen, Dale T., and Christopher A. Pissarides. 1994. "Job Creation and Job Destruction in the Theory of Unemployment." *Review of Economic Studies*, Vol. 61, No. 3, 397–415.

Moscarini, Giuseppe. 2005. "Job Matching and the Wage Distribution." *Econometrica*, Vol. 73, No. 2, 481–516.

Mumford, Karen, and Peter N. Smith. 2004. "Job Tenure in Britain: Employee Characteristics versus Workplace Effects." *Economica*, Vol. 71, No. 282, 275–98.

Nagypál, Eva. 2007. "Learning by Doing vs. Learning About Match Quality: Can We Tell Them Apart?" *Review of Economic Studies*, Vol. 74, No. 2, 537–66.

Naticchioni, Paolo, and Demian Panigo. 2004. "Employment Protection, Job–Tenure and Short Term Mobility Wage Gains: A New Explanation for the Italian Case." CEPREMAP Working Paper No. 0402, Paris.

Pissarides, Christopher A. 1994. "Search Unemployment with On-the-job Search." *Review of Economic Studies*, Vol. 61, No. 3, 457–75.

Pries, Martin, and Richard D. Rogerson. 2005. "Hiring Policies, Labor Market Institutions and Labor Market Flows." *Journal of Political Economy*, Vol. 113, No. 4, 811–39.

Ridder, Geert, and Insan Tunali. 1999. "Stratified Partial Likelihood Estimation." *Journal of Econometrics* Vol. 92, No. 2, 193–232.

Rogerson, Richard D., Robert Shimer, and Randall Wright (2005). "Search-Theoretic Models of the Labor Market: A Survey." *Journal of Economic Literature*, Vol. 43, No. 4, 959–88.

Schnabel, Claus. 2005. "Gewerkschaften und Arbeitgeberverbände: Organisationsgrade, Tarifbindung und Einflüsse auf Löhne und Beschäftigung." *Zeitschrift für Arbeitsmarktforschung*, Vol. 38, No. 2/3, 181–96.

Topel, Robert H., and Michael P. Ward. 1992. "Job Mobility and the Careers of Young Men." *Quarterly Journal of Economics*, Vol. 107, No. 2, 445–79.

Viscusi, W K. 1980. "A Theory of Job Shopping: A Bayesian Perspective." *Quarterly Journal of Economics*, Vol. 94, No. 3, 609–14.

Yashiv, Eran. 2007. "Labor Search and Matching in Macroeconomics." *European Economic Review*, Vol. 51, No. 8. 1859–895.