Seniority and job stability: A quantile regression approach using matched employer-employee data

first draft
04-08-2006

*Bernhard Boockmann**

Susanne Steffes[‡]

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

Abstract: Employment durations and job mobility are explained by a number of theoretical approaches such as job matching or human capital theory. These models may, however, apply to different degrees at different durations in the employment spell. As a consequence, individual and firm-level determinants of job durations may change as job tenure increases. Standard empirical techniques such as hazard rate analysis cannot deal with this complexity. In this paper, we apply censored quantile regression techniques. Our results give some support to the job matching model: individuals with a high risk of being bad matches exhibit higher exit rates initially, but the effect fades out over time. The results also confirm the effect of firm-level institutions such as works councils on the timing of separations. By contrast, the influence of human capital variables such as qualification and job position decreases, which is inconsistent with the notion of increasing match-specific rents due to human capital accumulation.

JEL-Codes: J62, J63, C41

Key Words: Job Durations, Mobility, Matching, Human Capital, Quantile Regression

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

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

1 Introduction

Data on job mobility and employment durations reflect a variety of different economic processes. Approaches like search, job matching and human capital theory as well as the economic analysis of institutions such as legal employment protection and works councils entail specific predictions concerning the reasons for voluntary and involuntary job mobility. Although these reasons differ a lot according to the theories it is difficult to separate them empirically. For instance, according to all these approaches the separation probability is expected to be high at the beginning of a spell and should decrease with employment duration. Furthermore, job matching as wells as human capital theory predict high skilled workers to have longer job durations. Therefore, with an ordinary job duration analysis it is difficult to investigate the impact of these theories (see Boockmann and Steffes, 2005).

However, the degree to which these theories apply may vary with employment duration. For instance, job matching theory starts with the notion that the quality of the employer-employee-match cannot be observed ex ante but can only be experienced. This means that the uncertainty relating to the quality of the match will gradually disappear and job matching theory should no longer explain separations of long-standing employment relationships. Therefore, whereas the prediction of the impact of certain observable determinants is similar with respect to the whole employment distribution there could be differences according to different quantiles in the following sense:

> „It may be that particular demographic characteristics are more important early in a job than later or vice versa“ (Farber, 1994: 574).

Among demographic characteristics, age and education are likely to be the most important. Their effects on short job durations could be explained by job-matching but for longer durations the focus should be on human capital theory. Furthermore, the fact that characteristics influence mobility and stability of jobs differently at different points of time in the employment spell may also be true for employer-related variables. Thus, firm-level institutions such as firing rules, employee representation and firm-specific training programmes may not inhibit probation and sorting and, therefore, may have little effect on job stability within

the first months of the employment spell. After a minimum tenure, however, they protect workers from dismissal or make job changes less valuable. Likewise, certain personnel policies such as the use of fixed-term contracts is likely to increase mobility from early on in the employment spell but may stabilise employment among those matches that have "survived" the sorting process (Boockmann and Hagen, 2005).

Most studies analyzing job mobility use firm-level data. At the worker level, job durations have been analysed using techniques such as linear regressions on the stock of job durations (Mumford and Smith, 2004; Gerlach and Stephan, 2005) or, more appropriately, duration analysis (Bellmann et al., 2000; Bender et al., 2000; Bergemann and Mertens, 2002; Boockmann and Steffes, 2005; Grotheer et al., 2004; Wolff, 2004). All of these approaches do not allow for changes in the effects of the covariates on job durations. In this paper, we investigate job durations using censored quantile regression. Quantile regression recently has been used by researchers in studying unemployment durations (Fitzenberger and Wilke, 2005; Machado and Portugal, 2002; Koenker and Bilias, 2001). To our knowledge Horowitz and Neumann (1987) is the only study in which employment durations have already been estimated with quantile regression. A particular problem – as opposed to unemployment durations – is that many spells are long-lasting, so that the degree of censoring is high. Using employer-employee data for an interval of seven years, the proportion of censored spells is about 45 per cent. Nevertheless, using the Buchinsky algorithm, we find that numerical problems are not prohibitive and that convergence is almost always achieved. A drawback of the methodology is that we cannot use information on time-varying influences on job durations. This precludes us from testing the implications of some theories which rest on the time-varying nature of the mobility process.

Our database is the German Linked Employer-Employee Dataset of the IAB, the research institute of the Federal Employment Agency. We base our results on a flow sample of 36,500 employment spells of male workers in West Germany. On the individuals' side the data consists of administrative data. In addition, we have detailed annual information on establishment characteristics from a large-scale survey, the IAB establishment panel.

The remainder of the paper is structured as follows. In the next section, we elaborate on the idea that the relative importance of different theories changes over the employment spell, and propose a number of testable predictions. Next, we introduce the data. Some descriptive statistics point to the need of accounting for job durations in estimating the influence of certain variables on job exits. Section 4 contains the methodology and empirical results are presented in section 5. Some concluding remarks sum up the main implications of our findings and discuss their relevance for interpreting the results known from the literature.

2 Hypotheses on the determinants of employment durations

Different theories exist to explain job mobility. Our idea is that these theories apply differently at different durations in the employment spell. The theories we can build on are job matching theory, human capital theory and the analysis of labour market institutions. The first theory should have an impact on the lower quantiles of the distribution because at the beginning of an employment spell the match quality has to be screened by the employer as well as the employee. The other two theories should exhibit a variable impact on the whole distribution because the amount of human capital as well as the impact of institutions change over time.

First, considering job matching theory, the quality of the match between worker and employee is seen as an experience good (Jovanovic, 1979a; Mortensen, 1988). It is unknown at the start of the employment spell and is only revealed after the worker has been employed for some time. If the expected quality of the match turns out to be below a threshold level, the match is dissolved. Hence, separations reflect the degree of initial mismatch in new hirings whereas it is unimportant whether the employee quits or experience a layoff.[1] In a recent paper Moscarini (2003) develops further the model of Jovanovic (1979a) integrating the model of equilibrium unemployment of Mortensen and Pissarides (1994). The main result with respect to tenure is that the distribution of separation rates is hump shaped thus leading to an increase at the beginning of the employment spell and to a decrease after job quality is known. Moscarini (2003) differs between high and low skilled workers and states that the former

1 Jovanovic (1979a) considers only the case of quits but states that the theory could be transformed to the case of both – quits and layoffs.

exhibit ceteris paribus longer durations because their opportunity costs are higher and therefore they prefer a mismatch to unemployment.

However, the amount of initial mismatches may differ according to employer and worker characteristics. There may be some firm characteristics, such as company age or the introduction of new technologies that have a systematic influence on the uncertainty attached to the quality of the match at the moment when the employment contract is signed.[2] For instance personnel policies of large firms are often known outside the firm whereas those of small firms are normally unknown to outsiders. Therefore, high information asymmetry should be positive correlated with the probability of a mismatch. Boockmann and Hagen (2005) find longer job durations for persons who began with a fixed-term contract (FTC) and later changed into regular employment. There are two possible reasons for this finding: on the one hand, firms which need more time to analyse the match quality than the regular probation period allows (or which do not invest ex ante in a selection process) could use fixed-term contracts as a screening instrument (Varejão and Portugal, 2003). Then, separation probability should be reduced during the duration of the contract, be high at the end and be constant for those workers who change into regular employment. This is our expectation according to matching theory. On the other hand, according to dual labour market theory, fixed-term contracts could be used to adjust employment to the economic cycle and therefore be a complement to long job durations. This means that job durations should be longer for higher quantiles in firms which offer fixed-term contracts (Capelli and Neumark, 2001).

Furthermore, individual characteristics can be used to analyse matching theory. High skilled workers and those in high job positions could accept a bad match to avoid unemployment (Moscarini, 2003). Additionally, firms generally invest more in the selection of high skilled personnel and therefore the probability of a bad match should be less for them. In the case of young workers, less information is available on their ability for certain types of work both for the employer and the worker him-/herself. Hence, low age will have a larger influence early in the employment spell than later, when the uncertainty has been

2 Clearly, companies' and workers' decisions as to whether to conclude employment relationships with high inherent risks are endogenous. However, it may still be profitable for companies to engage in a certain amount of screening due to the presence of fixed-term contracts and probation periods.

resolved. A similar argument applies for persons who have been unemployed or out-of-the-labour force for a long period of time (Arranz and García-Serrano, 2003).

Second, according to human capital theory, workers mobility decreases over the employment spell due to the accumulation of job-specific human capital (Becker, 1964; Jovanovic, 1979b). To the extent that this capital is not transferable across jobs, specific human capital creates match-specific rents. These rents provide a disincentive to mobility. Since this effect rises with the accumulation of human capital, it is much lower initially in the employment spell than after a few years of tenure. Although the accumulation of human capital is positive it is decreasing over time that means the effect is positive but not increasing anymore after a while (Mortensen, 1988).

However, all variables that capture match-specific rents should display a quantitatively more pronounced impact at higher quantiles of the duration distribution. At the level of the firm, the use of certain skill-intensive technologies and the offer of further training is also likely to directly influence the amount of job-specific human capital. Investments in further training paid by the employer are generally firm-specific and should therefore lead to longer job durations (Lazear, 2003). Investments in information and communication technology (ICT) are often followed by training investments for the employees who work with the new system and therefore should lead to similar results.

There is a well-known complementarity between match-specific human capital and formal education. Therefore, observables that capture the effects of job-specific rents are, at the level of the worker, all variables relating to education and skills. Thus, we expect a positive and increasing effect of education and job position on job durations. At the end of the duration distribution this effect should decrease. However, with a maximum duration of seven years this should not be observed with our data.

Finally, from the literature on employment protection and employee representation, there is strong empirical evidence for these institutions to increase job stability. Again, this finding applies to the average of all employees, irrespective of job tenure. However, employment protection often becomes

effective only after a certain amount of time (such as after a probation period), and in many countries there is a tendency that employment protection increases with tenure. Hence, workers with more seniority are better protected. Works councils are often believed to increase job stability due to two reasons. First, they may have a role in individual dismissals and redundancy procedures. Using this influence, works councils may slow down separation decisions. Second, works councils have a "voice" function, making companies more attractive to the workforce and thus reducing voluntary quits (Frick and Möller, 2003). If works councils are dominated by "insiders" with high job tenure, it is likely that the effect of works councils is concentrated on this constituency, while the effect is less present for workers with low tenure who, for various reasons, have less influence on the decisions of works councils (Boockmann and Hagen, 2003).[3]

Table 1 here

In table 1 we summarize our expectations about the effects of the covariates according to the theories. Generally, effects are expected to be higher at lower quantiles in the context of match quality whereas – according to human capital theory – effects are expected to rise with employment duration. Due to data restrictions the main observable institution – and from our sight the most interesting one – is a works council. However, we use this theoretical framework combined with the application of quantile regression to distinguish between the various theories explaining heterogeneous job durations.

3 Data and descriptive statistics

The database used in this study is the German LIAB, a linked employer-employee dataset.[4] The LIAB combines administrative data on employees with employer data from a large-scale representative survey of plants, the IAB Establishment Panel. This annual survey contains data on 16,000 establishments.

3 Ideally, the interaction between institutions and demand changes would also be considered. With works councils, demand changes may affect only workers at low tenure levels. Without works councils, all workers are affected by demand changes. However, this cannot be verified in the quantile regression framework because it cannot deal with time-varying covariates.

4 The data source is discussed in greater detail in Alda et al. (2005). Spell definitions and other properties of the dataset are discussed more closely in Boockmann and Steffes (2005).

The LIAB is exhaustive on the number of workers covered within the establishment sample. The employee part of the LIAB is the Employment Statistics Register (Beschäftigtenstatistik) of the Federal Employment Agency (Bender and Haas, 2002). The establishment part – the IAB-Establishment Panel – is a representative annual survey of establishments conducted by the Institute of Employment Research (Bellmann, 2002; Kölling, 2000). The unit of observation is not the company as a legal entity but the plant or site where the economic activities are carried out.

The longitudinal version of the LIAB currently contains establishments with interviews from 1993 to 2002. However, information on all workers in these establishments is available only from 1996, while worker information for previous years is limited to those workers still in employment in the survey establishments in 1996. In order to avoid sampling from a stock of workers, we restrict ourselves to workers having started their employment spells in the survey establishments after 1996. Furthermore, in order to restrict the amount of right-censoring, we only consider entries until the end of 1997.

We define an employment spell as the period from the beginning until the end of an employment relationship with a particular employer. The end of an employment relationship could be defined either by the change of the establishment identifier or by the employer report indicating the end of the employment relationship. We observe a large number of cases in which the establishment identifier changes without the end of the employment relationship being recorded in the data. It is likely that the plant identifier changes in a large number of these cases although the individual continues working in the same workplace.[5] This may happen, for instance, when the legal identity of the employer changes. In contrast to other studies such as Grotheer et al. (2004) and Bachmann (2005), we therefore rely on changes of the identifier as well as on employers' declarations.

The upper panel of table 2 briefly summarises the definition of the destination states (for details see Boockmann and Steffes, 2005). Episodes in which neither of these destination states can be verified are taken as censored at the last

5 This could be due to errors in the allocation of the identifiers by the local Federal Employment Agencies.

observed employment record. Hence, there are several possibilities why an employment spell may be censored. First, the end of the observation period (2002) is reached. Second, the plant identifier changes but there is no record on the end of the employment contract. Third, the employee is recalled to the previous employer more than 90 days after the end of the previous spell, in cases in which the end of the employment contract has not been reported. Recalls within 90 days after the last spell are defined as further employment with the same employer.

Table 2 here

The beginning of a spell is inferred in a similar way. The lower half of table 1 shows the definition of the origin states, which are also used as independent variables in the following. The state "no observation" is generated in cases of individuals not observed for at least one year before the start of the observation period on January 1st, 1996.

If an individual is employed with more than one employer at the same time, we only use the employment spell generating the highest income. Spells lasting only one day are dropped. We restrict the data to male workers in West Germany aged 25 to 52. The upper age limit is chosen in order to avoid confusion between job exit and early retirement. The lower limit excludes students who may have short-term employment spells during school and university holidays. In addition, we exclude employees working less than 15 hours a week during the whole employment spell, apprentices and home workers. Spells in the agricultural sector are dropped due to its high rates of seasonal and temporary employment which mark out this sector from the rest of the economy. Miners are dropped due to their extremely small numbers. All spells with missing covariate information are also eliminated from the data. These requirements leave us with a sample of 36,561 employment spells. 46 percent of these spells are censored. Of the censored spells, 86 percent are censored at the end of the observation period, 8 percent of censorings are followed by a spell with a new plant identifier and 6 percent are followed by a spell with the same plant identifier.

The Kaplan-Meier estimator of the empirical survival function is shown in figure 1. Median employment duration is at about 700 days. In figures 2 and 3, we

display Kaplan-Meier survival functions estimated separately for firm or individual characteristics. As expected, the survival functions are higher in large firms, in firms with works councils and in firms covered by a collective agreement. In addition, firms offering further training or investing in ICT exhibit higher survival rates. However, the curves are overlapping or even crossing at low durations. Among individual characteristics, results do not differ much across groups of workers. However, the survival functions of blue collar and white collar workers cross after about seven years of job duration. Summing up, estimation procedures allowing for different effects at different points in the tenure spell seem to be required to analyse job seniority and firm heterogeneity without imposing unduly restrictive assumptions on the effects of characteristics on exit rates.

Figures 1,2 and 3 here

4 Estimation technique

We use a censored quantile regression (CQR) approach to estimate establishment and individual effects on different quantiles of the job duration distribution. Although the most popular estimation technique for duration analysis is the Cox proportional hazard model this model implies some restrictions which we can avoid with quantile regressions. The main advantage is the possibility to estimate time dependent coefficients (Fitzenberger und Wilke, 2005; Koenker and Geling, 1999). With proportional hazard estimations the effects on the hazard rate are assumed to be the same over the whole distribution which could be a very strong assumption for some covariates. For instance Boockmann and Steffes (2005) find a positive significant effect of a works council on job durations. This is an important result according to the analysis of job stability. The question whether this positive effect is lower for short spells than for long spells is even more interesting. Additionally, in order to deal with right censoring the censored quantile regression approach is preferable.[6] Unfortunately, with CQR all covariates have to be time invariant which makes the analysis of some interesting relationships, e.g. business cycle effects on the duration distribution, impossible.

6 For a more detailed discussion about proportional hazard models versus quantile regression see Fitzenberger and Wilke (2005) and Koenker and Geling (1999).

Furthermore, in contrast to proportional hazard models the estimation of fixed firm effects is not possible using this model.

We use a linear quantile regression for duration data which is based on the accelerated failure time model

$$h(T_i) = x_i'\beta(\theta) + \varepsilon(\theta)_i$$

where the θ-quantile of $\varepsilon(\theta)_i$ conditional on x_i is zero and $h(.)$ is a strictly monotone transformation of the completed duration T_i of the spell i. The conditional quantile of this duration model can be written as

$$Q_{h(t|x)}(\theta) = x_i'\beta(\theta)$$

For this kind of regression several transformations can be chosen as long as they are preserving the ordering of the quantiles.[7] However, in this study we use the log-transformation as the most popular one and thus get

$$Q_{\log(T_i)}(\theta) = x_i'\beta(\theta)$$

Due to a high number of censored employment spells in our data we have to apply CQR in order to get unbiased estimates. This estimation method implies a strict demand on the structure of the data: the potential censoring point for uncensored data has to be known. The observed completed duration is given by

$$T_i = \begin{cases} T_i^* & \text{if spell is not censored} \\ C_i & \text{if spell is censored} \end{cases}$$

where T_i^* is the true duration and C_i is the observed duration if the true duration lasts longer. The restriction means that a potential C_i should be defined even if the true duration is known. However, the potential censoring point could be the end of the observation period which we take it to be in this study. Modifying the model according to the censoring yields

7 A very flexible transformation for duration analysis is the Box-Cox quantile regression (Fitzenberger et al., 2004).

$$Q_{\log(T_i)}(\theta) = x_i'\beta(\theta) \vee C_i$$

To estimate the CQR the distance function

$$\sum_{i=1}^{N} \rho_\theta(\log(T_i) - \min(x_i'\beta(\theta), C_i))$$

has to be minimized, where N is the absolute number of employment spells and ρ_θ is the so called check function $\rho_\theta(z) = z(\theta - I(z < 0))$ which weights the fitted values according to the quantile ($I(A)$ is the indicator function equalling one if A is true). A quantile regression without censoring is a special case with $C_i = +\infty$. Minimizing the distance function yields the CQR estimator $\hat{\beta}(\theta)$ which is $\sqrt{N}$-consistent and asymptotically normally distributed. For a discussion of the asymptotic distribution see Fitzenberger (1997).

Due to the fact that the distance function for CQR is not convex the calculation of the estimator is numerically difficult. In the literature several procedures to calculate the estimator were developed. Fitzenberger and Winker (1999) compare most of them according to their computational performance and come to the result that in case of a high censoring rate most of the algorithms imply difficulties. Therefore, we compared the algorithm developed by Buchinsky (1998) which is implemented in STATA and the one developed by Fitzenberger (1997) which is implemented in TSP. However, numerical results were found to be the same and thus we only present the coefficients and confidence intervals estimated using the former.[8] The Buchinsky algorithm is an iterative procedure where only those observations are used in the next step whose fitted values are below the individual potential censoring point. The process converges if all fitted values of the recent iteration step are below the censoring points. If the process does not converge, the best iteration step with the smallest objective function is used for the final estimation. Since standard errors may be biased, we use heteroscedastic robust estimates obtained by the pairwise bootstrap method (for details see Bilias et al., 2000). Due to high censoring rates at the end of the distribution we only estimate up to the 0.6-quantile.

8 Results of the comparison can be delivered by the authors on request.

5 Empirical Results

The main estimation results are reported in figure 4 (firm characteristics) and figure 5 (individual characteristics). Additional variables have been included in the estimation. Full results are presented in the appendix. Employees employed in firms with a works council exhibit between 40 and 75 percent higher job durations – the effect increases after the 0.2 quantile. Therefore, works councils might have a positive effect on job durations due to their "voice" function or due to their impact on redundancy policy even for employees with short job durations. However, the effect is much stronger for long job durations indicating an increasing employment protection with job seniority.

Figure 4 here

Job durations in firms which offer further training to their employees are higher on average. This effect oscillates with job seniority and is highest at the 0.5 quantile. Therefore, this result indicates that there is no relation between further training and seniority or the accumulation of job specific human capital respectively. However, we do not know from the data whether the observed individual receives further training or not. We do not present results concerning investments in information and communication technology because effects are not significant.

Unfortunately we cannot observe whether an employment contract is temporary or not. But we have information about the share of fixed-term employment within an establishment. The estimated impact of this share is significantly positive over the whole duration distribution. However, the coefficient decreases on average. This can be due to the fact that employees with fixed-term contracts are better protected against dismissals whereas employees with regular contracts experience lower protection at the beginning of their employment relationship. This effect diminishes after a while. However, the use of fixed-term employment seems to be a complement to long-lasting spells. This could be due to a better match quality for jobs that are kept after the end of a fixed-term contract. But it can also be due to a complement between short fixed-term contracts and long regular employment according to dual labour theory. Which of the two explanations prevails should be investigated in further research.

Boockmann and Steffes (2005) find that very large and very small firms exhibit significantly longer job durations but their estimated coefficients are only time constant. With quantile regression we can get a more precise expression of the impact of firm size on job durations. Our results show that in contrast to the smallest firms (reference group: less than 100 employees) the very large firms (more than 1000 employees) exhibit significantly longer job durations but the effect decreases with seniority and is negative after the 0.4 quantile. We find the same results for firms with 500 to 999 employees but here the effect is insignificant for the 0.3 and 0.4 quantile. For firms with between 100 and 199 employees employment durations are shorter and later on insignificant. All other firms exhibit more or less insignificant results. Summing up, on the one hand, internal labour markets do not play a crucial role in middle-sized firms. On the other hand, the impact of personal relation between the head of a firm and the employees, which is given in small firms, seems to be – mainly on the long-run – very important. Again, the impact of human capital theory seems to be neglectable in this context.

Figure 5 here

Next we discuss the effects of individual characteristics. Skilled employees with vocational training or university degree are longer employed with the same employer than unskilled persons. A reason for this result can be higher dismissal costs according to higher investments in initial training for high qualified jobs. Unexpectedly, the effect decreases for higher quantiles. Therefore, we cannot find positive effects of human capital accumulation according to seniority for high skilled employees. We find a similar result referring to the job position. Jobs for skilled blue and white collar workers exhibit higher job durations but again, the effects decrease over the duration distribution. However, we cannot confirm the expectation of higher match-specific rents for high qualified employees at high quantiles of the distribution.

Young workers are expected to exhibit shorter job durations on average according to a higher probability of initial mismatch. This phenomenon should decrease with seniority. Our results show that the older the workers at the beginning of the employment spell are the longer are their job durations. Only

the oldest observed group exhibit a few percent less than the 40 to 44 year olds. Indeed, the coefficients decrease with seniority indicating that age does not play a role for the separation probability of long employment spells.

Figure 6 here

The Kaplan Meier graphs in section 3 show a high impact of the employment history on survival probability. The interpretation of these coefficients must be done carefully because they could be correlated with unobserved individual characteristics and hence could be endogenous. Nevertheless, for the understanding of the determinants leading to such heterogeneous job durations one should not exclude this information if it is available. Individuals who came from unemployment have shorter job durations than the reference group (no observation before the current spell) but the effect is not significant at the first quantile (see figure 6). Persons not being employed for a longer time before the current employment spell begins exhibit the shortest job durations. The most interesting result concerns those who have been employed with the same employer more than 90 days before the current spell. At the beginning of the distribution their employment spells show about 20 percent less longevity but this effect becomes insignificant at higher quantiles. Therefore, persons with recalls obviously select themselves into two categories: 1) individuals who are employed several times with one employer but always for a short time; 2) individuals with a gap between two employment spells who stay with this employer for a longer time after the recall. Workers coming from another employer exhibit the longest job durations but this effect decreases over time as we have expected according to matching theory.

6 Conclusions

In this study we analyse the effects of individual and firm characteristics on job durations using censored quantile regression. The estimation procedure allows us to distinguish between the impact on short and long job durations. We use this fact to investigate whether we can differentiate between job matching and human capital theory explaining separation probabilities. Whereas predictions about the effects of observable characteristics on job durations are often similar according to both theories the expected strength at different points on the duration

distribution varies. Moreover, the linked employer-employee dataset we use allows us to analyze the impact of institutions like works councils and the share of fixed-term contracts on individual job stability and the respective development according to seniority.

Results show that the coefficients vary a lot with seniority and thus the proportional hazard assumption of the Cox-model can be rejected. Even within the relatively short observation period of maximal seven years we find extreme differences between the quantiles up to changes in sign. However, almost all coefficients turned out to have the expected sign indicating that no theory has to be called in question. Nevertheless, most of the covariates exert their impact early in the employment spell and are less pronounced later on. Especially, individual characteristics as education, job position and age exhibit this trend. Therefore, concerning matching and human capital theory the former seems to have the larger strength explaining job stability. One exception is further training which exhibits a constant positive effect on average and could be explained by human capital theory. Nevertheless, interpreting this coefficient is difficult because it says nothing about whether the observed individual received further training or not.

Considering the impact of a works council results are as expected. Further investigation of the effects concerning the business cycle as well as job destruction and job creation rates would be interesting in this context. Moreover, the results concerning the firm-size are interesting but hard to interpret. The share of fixed-term contracts has a positive but decreasing impact indicating that such contracts are used as a screening instrument. But again, we have no information about the kind of contract the observed worker has concluded with the employer.

However, firm characteristics play a crucial role explaining job stability and demand further research. Additionally, one may separate involuntary and voluntary mobility in order to analyze certain implications for employment policy. Empirically, this is a difficult task because in our data we cannot observe the reason for a separation. One might, for instance, differentiate results between expanding and shrinking establishments, with mobility in the latter being interpreted as involuntary. Furthermore, this strategy could be used to analyze

job destruction and creation rates and their development over the business cycle in order to investigate dual labour market theory.

References

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

Arranz, J.M.; C. García-Serrano (2003), The influence of previous labour market experiences on subsequent job tenure, Universidad de Alcalá Working Paper No. 17/03.

Bachmann, R. (2005), Labour market dynamics in Germany: Hirings, separations, and job-to-job transitions over the business cycle, Sonderforschungsbereich 649 Humboldt-Universität zu Berlin Discussion Paper No. 2005-045.

Becker, G.S. (1964), *Human Capital*, University of Chicago Press, Chicago.

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

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

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

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

Bergemann, A.; A. Mertens (2002), Job stability trends, layoffs and quits – an empirical analysis for West Germany, Paper provided by International Conferences on Panel Data in its series 10th International Conference on Panel Data, Berlin, July 5-6, 2002 with number C1-4.

Bilias, Y.; S. Chen; Z. Ying (2000), Simple resampling methods for censored regression quantiles, *Journal of Econometrics* 99, 373-386.

Boockmann, B.; T. Hagen (2005), Die Bedeutung befristeter Arbeitsverhältnisse für die Zugänge und den Verbleib in Beschäftigung, in: Kronauer, M.; G. Linne, *Flexicurity. Die Bindung von Sicherheit an Flexibilität*, 149-168.

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

Boockmann, B.; S. Steffes (2005), Individual and plant-level determinants of job durations in Germany, ZEW Discussion Paper No. 05-89.

Buchinsky, M. (1998), Recent advances in quantile regression models: a practical guideline for empirical research, *The Journal of Human Resources* 33(1), 88-126.

Capelli, P.; D. Neumark (2001), External job churning and internal job flexibility, NBER Working Paper 8111.

Farber, H.S. (1994), The analysis of interfirm worker mobility, *Journal of Labor Economics* 12(4), 554-593.

Fitzenberger, B. (1997), A guide to censored quantile regressions, in: Maddala, G.S.; C.R. Rao (eds.), *Handbook of Statistics Vol. 15: Robust Inference*, North-Holland, Amsterdam.

Fitzenberger, B.; R. Wilke (2005), Using quantile regression for duration analysis, ZEW Discussion Paper No. 05-65.

Fitzenberger, B.; R. Wilke; X. Zhuang (2004), A note on implementing box-cox quantile regression, ZEW Discussion Paper No. 04-61.

Fitzenberger, B.; P. Winker (1999), Improving the computation of censored quantile regressions, Beiträge zur angewandten Wirtschaftsforschung, Discussion Paper 568-99.

Frick, B.; I. Möller (2003), Mandated works councils and firm performance: Labor productivity and personnel turnover in german establishments, *Schmollers Jahrbuch* 123(3), 423-454.

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

Grotheer, M.; O. Struck; L. Bellmann; T. Gewiese (2004), Determinanten von Beschäftigungsstabilität. Chancen und Risiken von „Entrants“ im ost-westdeutschen Vergleich, in: Struck, O.; Köhler, C. (eds.), *Beschäftigungsstabilität im Wandel? Empirische Befunde und theoretische Erklärungen für West- und Ostdeutschland*, Rainer Hampp Verlag, München und Mering.

Horowitz, J.L.; G.R. Neumann (1987), Semiparametric Estimation of Employment Duration Models, *Econometric Reviews* 6(1), 5-40.

Jovanovic, B. (1979a), Job Matching and the theory of turnover, *Journal of Political Economy* 87(5), 972-990.

Jovanovic, B. (1979b), Firm-specific capital and turnover, *Journal of Political Economy* 87(6), 1246-1260.

Koenker, R.; Y. Bilias (2001), Quantile regression for duration data: a reappraisal of the Pennsylvania reemployment bonus expriments, *Empirical Economics* 26, 199-220.

Koenker, R.; O. Geling (1999), Reappraising medfly longevity: a quantile regression survival analysis, Discussion Paper Sonderforschungsbereich 373 Humboldt-Universität zu Berlin No. 53.

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

Lazear, E.P. (2003), Firm-Specific Human Capital: A Skill-Weights Approach, IZA Discussion Paper No. 813.

Machado, J.A.F; P. Portugal (2002), Quantile regression analysis of transition data, Banco de Portugal Working Paper No. 1-02.

Mortensen, D.T. (1988), Wages, separations, and job-tenure: on-the-job specific training or matching?, *Journal of Labour Economics* 6(4), 445-471.

Mortensen, D.T.; C.A. Pissarides (1994), Job Creation and Job Destruction in the Theory of Unemployment, Review of Economic Studies 61(3), 397-415.

Moscarini, G. (2003), Skill and Luck in the Theory of Turnover, Unpublished Manuscript, Yale University, Department of Economics.

Mumford, K.; P.N. Smith (2004), Job tenure in Britain: Employee characteristics versus workplace effects, *Economica* 71(282), 275-298.

Varejão, J., P. Portugal (2003), Why do firms use fixed-term contracts?, Faculdade de Economia Universidade do Porto Working Paper No. 2003-10.

Wolff, J. (2004), The duration of new job matches in east and west Germany, Discussion Papers in Economics No. 2004-10.

Figure 1: Kaplan Meier graph

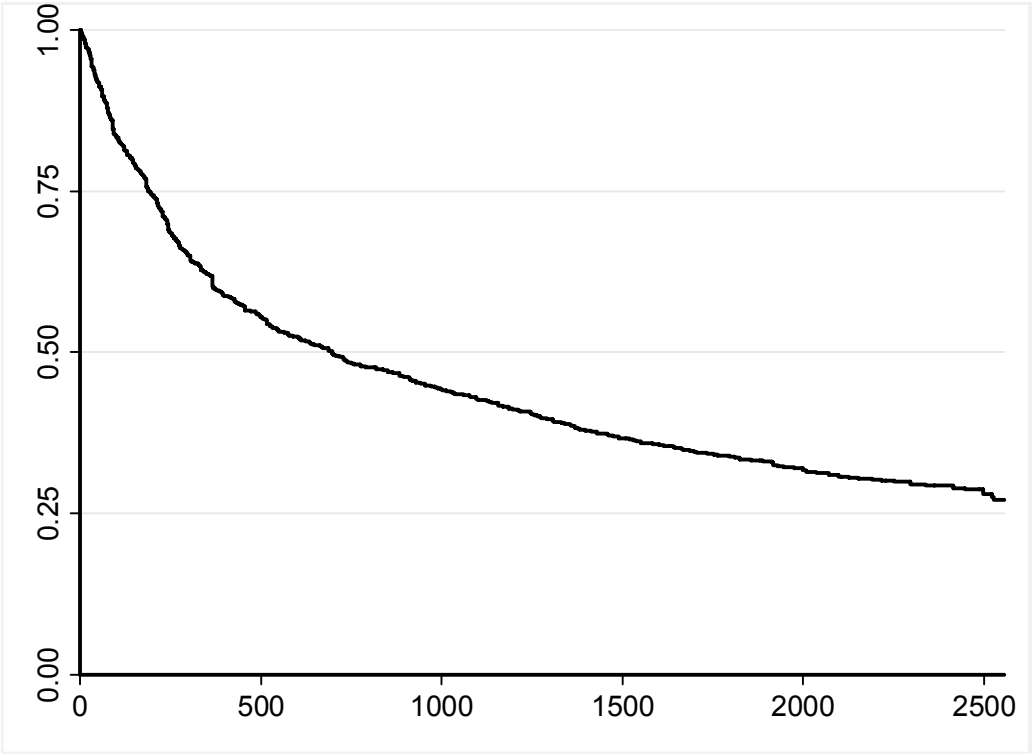
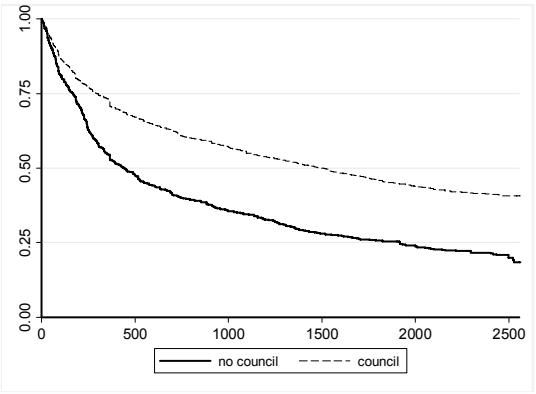
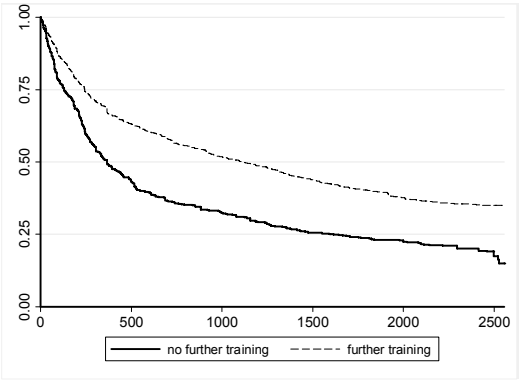


Figure 2: Kaplan Meier graphs separated for firm characteristics

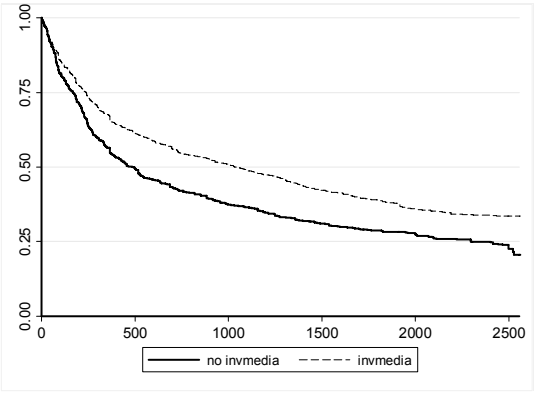
Works Council



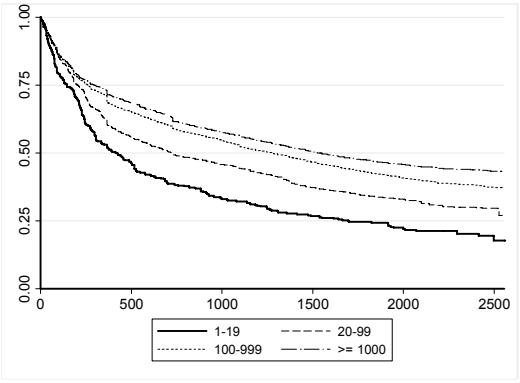
Further Training



ICT



Firm Size



Collective Agreement

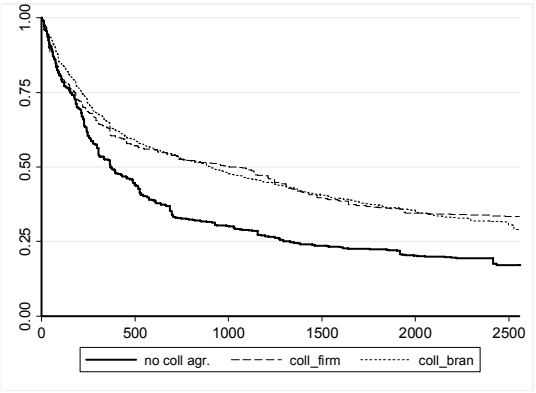
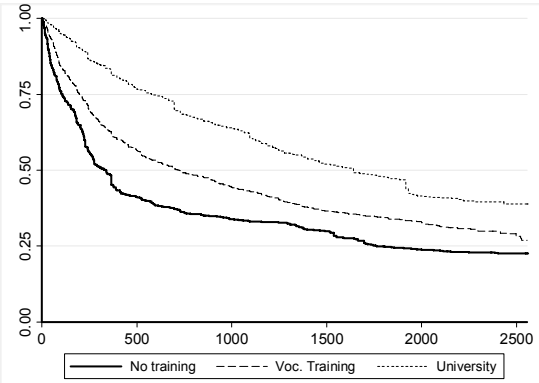
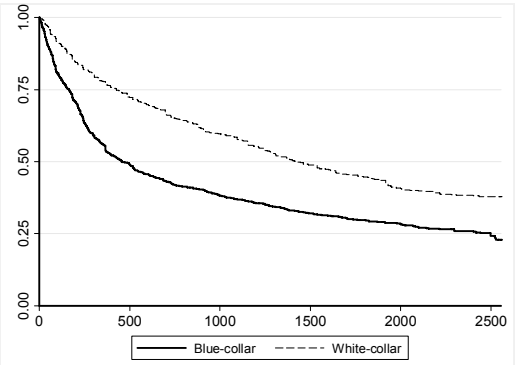


Figure 3: Kaplan Meier graphs separated for individual characteristics

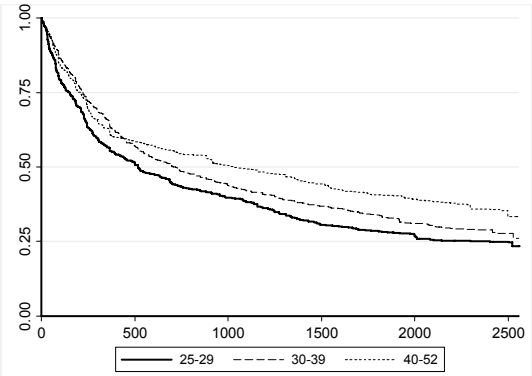
Education



Job Position



Age



Previous Employment Status

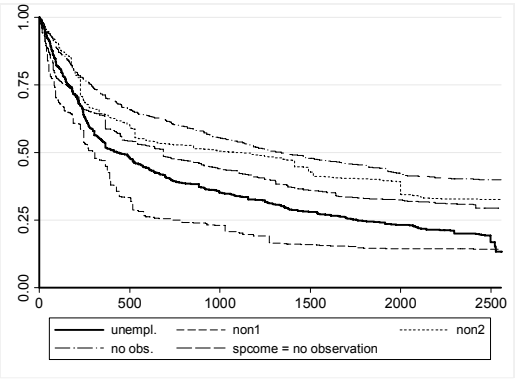
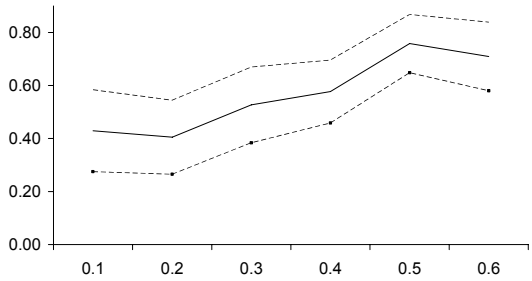
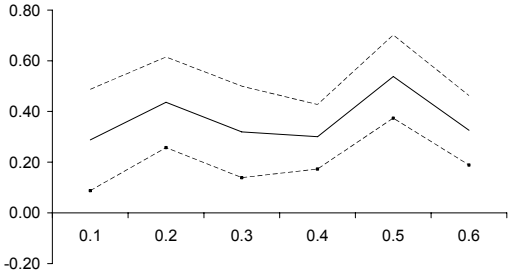


Figure 4: Estimation results for firm characteristics

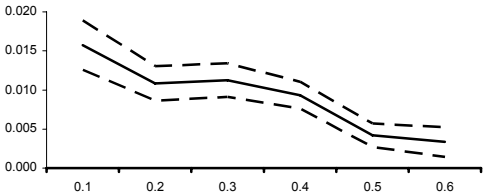
Works Council



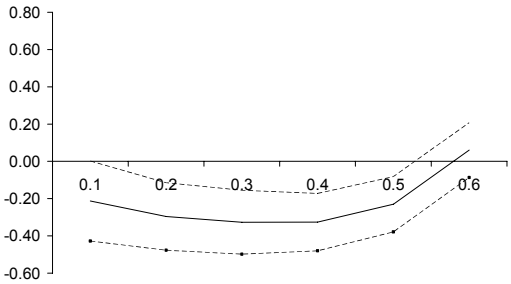
Further Training



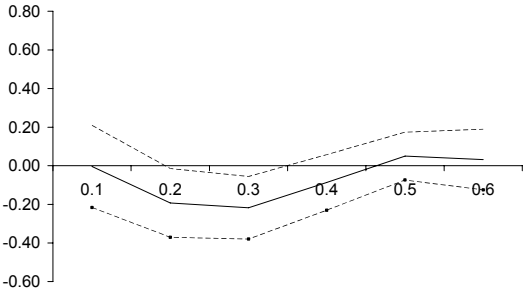
Share of Fixed-Term Contracts



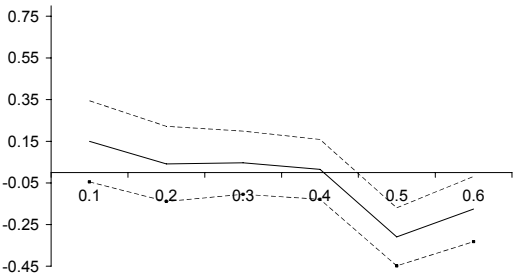
100-199 employees



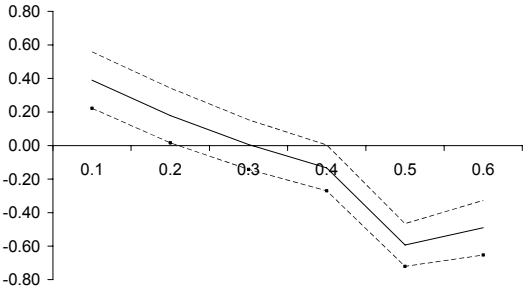
200-299 employees



300-499 employees



500-999 employees



>= 1000 employees

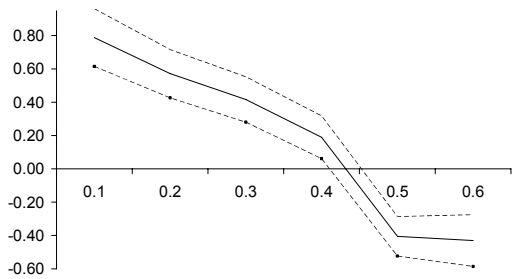


Figure 5: Estimation results for individual characteristics

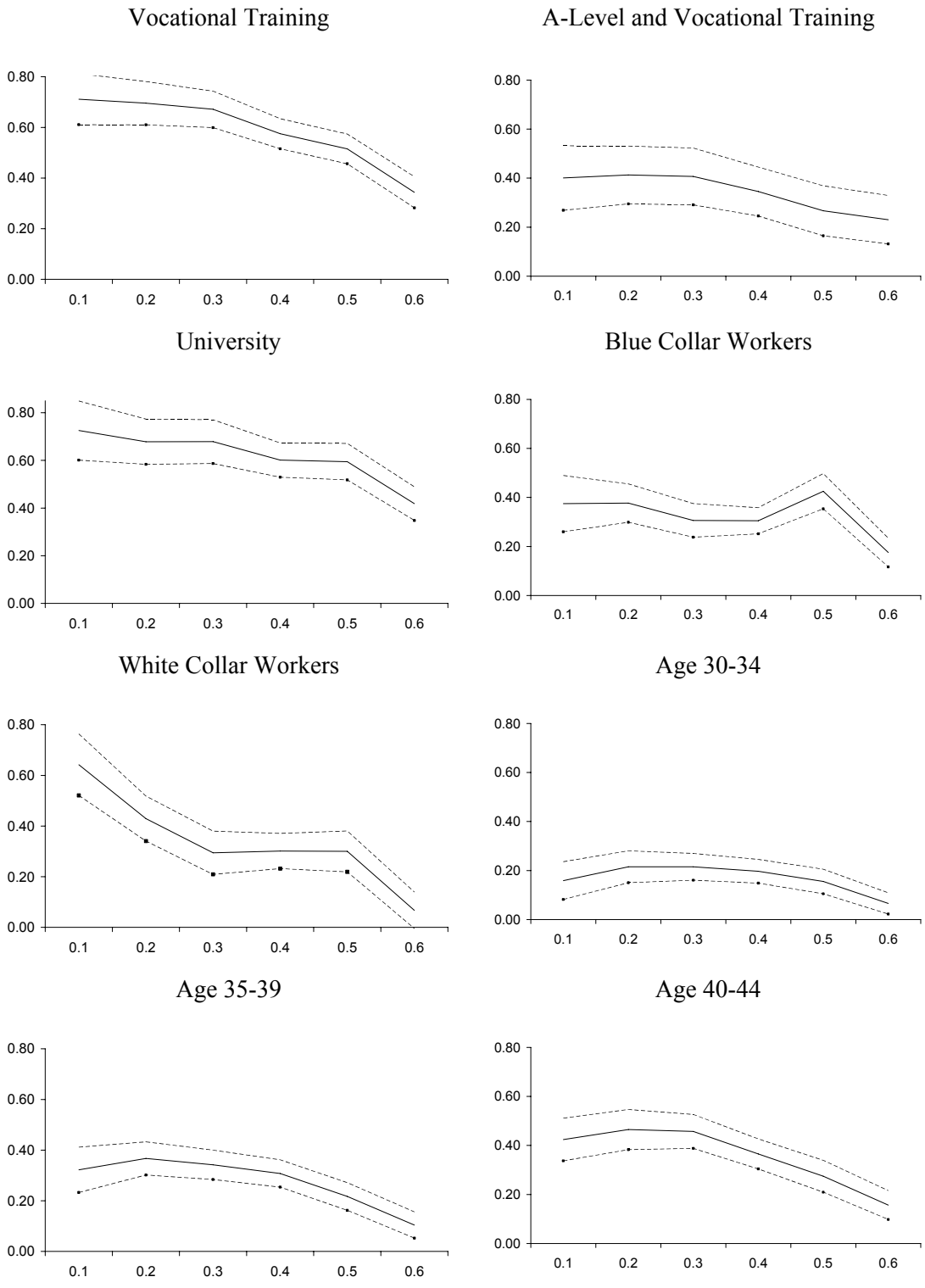


Figure 5 continued...

Age 45-52

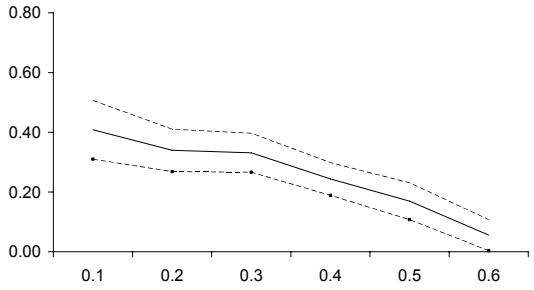
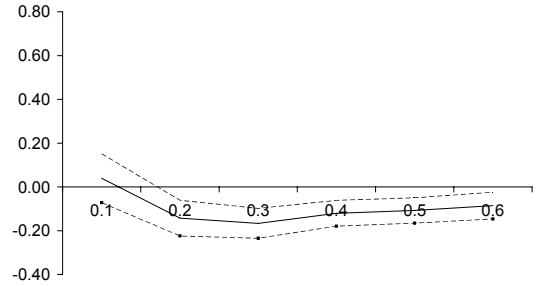
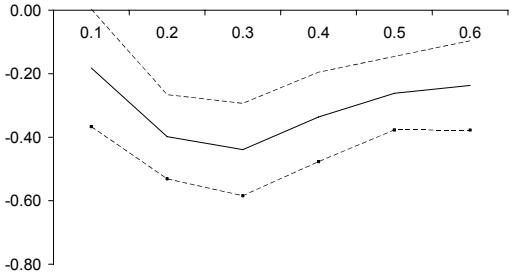


Figure 6: Estimation results for employment history

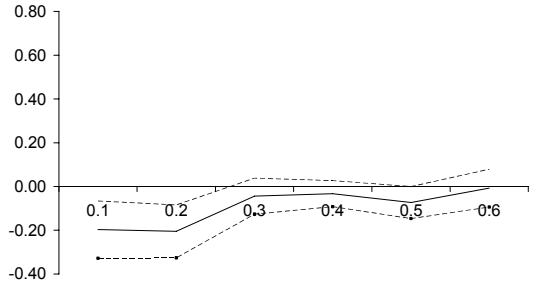
Unemployment



Non-Employment



Recall



Other Employer

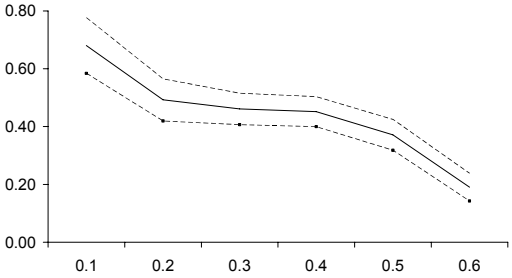


Table 1: Expectations about effects of covariates

	Matching	Human Capital	Institutions
Works Council			+ ↑
Further Training		+ ↑	
Investment in ICT	– ↓	+ ↑	
Share of FTC	+ while; – after		–; + later
Firm-Size	+ →	+ ↑	
Education	+ ↓	+ ↑	
Job position	+ ↓	+ ↑	
Age	+ ↓	– ↓	
Job-to-job	+ ↓		
Unemployed/out-of-labor	– ↓		
Recall	+ ↓	+ ↓	

Note: + positive effect, – negative effect, ↑ increasing over duration, ↓ decreasing over duration, → constant over duration.

Table 2: Definitions of destination and origin states

Employment state	**Definition**
a) destination states	
Unemployment	receives unemployment benefits for at least one day within 60 days after separation, is not employed with current employer for at least 90 days after separation
Non-employment	is not employed with current employer for the next 90 days after separation, receives no unemployment benefits and does not change from job-to-job for at least 60 days after separation and has recorded end of relationship
Job-to-job change	takes up employment with another employer within 60 days after separation and has recorded end of relationship
Recall	takes up employment with the same employer after more than 90 days after separation and has recorded end of relationship
b) origin states	
Unemployment	received unemployment benefits for at least one day during 60 days before hiring, was not employed with current employer for at least 90 days before hiring
Non-employment	was not employed with current employer for at least 90 days before hiring, received no unemployment benefits for at least 60 days before hiring, did not change from job-to-job for at least 60 days before hiring
Recall	was employed with current employer for more than 90 days before hiring, received no unemployment benefits during 60 days before hiring, did not change from job-to-job during 60 days before employment
Job-to-job change	did change from job-to-job at most 60 days before employment
No observation	not observed since January 1st, 1995

Appendix

Table A1: Means and standard deviations of covariates

	Mean	St.dev.		Mean	St.dev.
Firm-specific			***Individual-specific***		
Bargaining			*Education*		
Council	.93	.26	No training	.16	.37
Sector coll. agreement	.77	.42	Vocational training	.51	.50
Firm collective agreement	.16	.36	A-level	.05	.22
Further training: yes/no	.96	.20	Vocational training and A-levels	.06	.23
Investments in ICT	.85	.35	Universitv	.21	.41
Share of FTC	6.55	13.52	*Job position*		
Firm-size			Blue collar unskilled	.36	.48
<100	.06		Blue collar skilled	.18	.38
100-199	.05	.22	White collar	.39	.49
200-299	.05	.22	Master craftsman	.01	.08
300-499	.07	.25	Part-time	.06	.23
500-999	.13	.34	*Age*		
≥ 1000	.63	.48	25-29	.33	.47
Sector			30-34	.27	.44
Insurance, credit	.06	.24	35-39	.17	.37
Transport, communication	.05	.21	40-44	.11	.32
Trade, repair	.07	.26	45-52	.12	.32
Construction	.07	.25	*Nationality*		
Mining, energy, water	.03	.17	EU citizen	.02	.15
Finish of raw materials	.26	.44	Non-EU citizen	.09	.29
Capital goods	.21	.41	*Profession*		
Consumer goods	.09	.28	Production	.41	.49
Services for firms	.04	.19	Technical	.13	.34
Other services	.08	.27	Services	.44	.50
Non-profit organization	.02	.15	Others	.01	.11
Regional authorities, social insurances	.02	.14	*Previous Employment status*		
Country			Unemployment	.23	.42
Berlin	.07	.26	Non-employment	.04	.20
Schleswig-Holstein	.02	.15	Recall	.08	.28
Hamburg	.07	.26	Other employer	.44	.50
Lower Saxony	.07	.25	No observation	.21	.40
Bremen	.02	.15			
North Rhine-Westphalia	.31	.46			
Hesse	.07	.25			
Rhineland-Palatinate/Saarland	.05	.21			
Baden-Württemberg	.12	.32			
Bavaria	.21	.40			

Table A2: Estimation results for 0.1 and 0.2 quantiles

	Quantile 0.1			Quantile 0.2		
	Coef.	-2.5 %	+2.5 %	Coef.	-2.5 %	+2.5 %
Firm characteristics						
Council	0.43	0.27	0.58	0.40	0.27	0.54
Sector coll. agreement	-0.15	-0.29	-0.01	-0.06	-0.17	0.05
Firm collective agreement	-0.31	-0.48	-0.15	-0.33	-0.46	-0.20
Further training	0.29	0.09	0.49	0.44	0.26	0.61
Investments in ICT	-0.04	-0.13	0.04	-0.01	-0.07	0.05
Share of FTC	0.02	0.01	0.02	0.01	0.01	0.01
100-199	-0.21	-0.43	0.00	-0.30	-0.48	-0.11
200-299	0.00	-0.22	0.21	-0.19	-0.37	-0.01
300-499	0.15	-0.05	0.34	0.04	-0.14	0.22
500-999	0.39	0.22	0.56	0.18	0.02	0.34
≥ 1000	0.79	0.61	0.96	0.57	0.43	0.72
Insurance, credit	-0.28	-0.51	-0.04	-0.21	-0.38	-0.05
Transport, communication	-1.40	-1.68	-1.11	-1.24	-1.43	-1.05
Trade, repair	0.17	-0.07	0.41	0.21	0.03	0.39
Construction	1.91	1.68	2.15	1.75	1.59	1.91
Mining, energy, water	0.37	0.10	0.65	0.25	0.06	0.45
Finish of raw materials	1.23	1.01	1.45	1.35	1.21	1.49
Capital goods	-0.11	-0.32	0.10	-0.26	-0.40	-0.12
Consumer goods	1.09	0.84	1.34	1.17	1.00	1.34
Services for firms	0.44	0.19	0.70	0.57	0.33	0.81
Other services	-0.63	-0.88	-0.38	-0.46	-0.63	-0.30
Non-profit organization	0.85	0.50	1.20	0.90	0.66	1.14
Individual characteristics						
Vocational training	0.71	0.61	0.81	0.70	0.61	0.78
Voc. training and A-levels	0.40	0.27	0.53	0.41	0.29	0.53
Universitv	0.73	0.60	0.85	0.68	0.58	0.77
Blue collar skilled	0.37	0.26	0.49	0.38	0.30	0.46
White collar	0.64	0.52	0.76	0.43	0.34	0.52
Master craftsman	0.48	0.13	0.82	0.23	-0.03	0.48
Part-time	0.15	-0.02	0.32	-0.01	-0.14	0.11
30-34	0.16	0.08	0.24	0.22	0.15	0.28
35-39	0.32	0.23	0.41	0.37	0.30	0.43
40-44	0.42	0.34	0.51	0.46	0.38	0.55
45-52	0.41	0.31	0.51	0.34	0.27	0.41
EU citizen	-0.05	-0.20	0.10	-0.18	-0.33	-0.03
Non-EU citizen	-0.22	-0.33	-0.12	-0.28	-0.38	-0.18
Production workers	0.15	-0.03	0.34	0.50	0.28	0.72
Technicians	0.67	0.50	0.84	0.86	0.64	1.08
Others	0.40	0.24	0.57	0.62	0.41	0.83
Previous employment status						
Unemployment	0.04	-0.07	0.15	-0.14	-0.22	-0.06
Non-employment	-0.18	-0.37	0.00	-0.40	-0.53	-0.27
Recall	-0.20	-0.33	-0.07	-0.20	-0.33	-0.08
Job-to-job transition	0.68	0.58	0.78	0.49	0.42	0.57

Table A2 continued…

States						
Berlin	-0.47	-0.76	-0.18	-0.47	-0.64	-0.30
Schleswig-Holstein	-0.26	-0.54	0.02	-0.35	-0.52	-0.19
Hamburg	-0.25	-0.53	0.03	-0.30	-0.48	-0.12
Lower Saxony	0.15	-0.17	0.47	0.25	0.04	0.47
Bremen	-0.02	-0.28	0.23	-0.09	-0.24	0.07
North Rhine-Westphalia	0.21	-0.08	0.49	0.01	-0.17	0.20
Hesse	-0.39	-0.68	-0.10	-0.64	-0.84	-0.45
Rhineland-Palatinate/Saarland	-0.05	-0.31	0.21	-0.20	-0.36	-0.05
Bavaria	0.08	-0.18	0.35	0.02	-0.13	0.18
Year 1996	0.35	0.28	0.41	0.34	0.29	0.38
Constant	1.40	0.97	1.83	2.18	1.81	2.55
Min. sum of deviations	18683			27803		
# of observations	36552			36044		
Convergence	yes			yes		

Note: Confidence intervals are shown in second and third column.

Table A3: Estimation results for 0.3 and 0.4 quantiles

	Quantile 0.3			Quantile 0.4		
	Coef.	-2.5 %	+2.5 %	Coef.	-2.5 %	+2.5 %
Firm characteristics						
Council	0.53	0.38	0.67	0.58	0.46	0.70
Sector coll. agreement	-0.01	-0.11	0.08	0.04	-0.05	0.13
Firm collective agreement	-0.33	-0.45	-0.21	-0.30	-0.41	-0.20
Further training	0.32	0.14	0.50	0.30	0.17	0.43
Investments in ICT	0.01	-0.04	0.06	0.06	0.01	0.11
Share of FTC	0.01	0.01	0.01	0.01	0.01	0.01
100-199	-0.33	-0.50	-0.15	-0.33	-0.48	-0.17
200-299	-0.22	-0.38	-0.06	-0.09	-0.23	0.06
300-499	0.05	-0.10	0.20	0.01	-0.13	0.16
500-999	0.01	-0.14	0.15	-0.13	-0.27	0.00
≥ 1000	0.42	0.28	0.55	0.19	0.06	0.32
Insurance, credit	-0.10	-0.29	0.08	0.07	-0.11	0.25
Transport, communication	-1.08	-1.27	-0.89	-0.84	-1.02	-0.65
Trade, repair	0.41	0.24	0.58	0.59	0.41	0.76
Construction	1.75	1.59	1.90	1.73	1.58	1.89
Mining, energy, water	0.38	0.19	0.58	0.30	0.12	0.48
Finish of raw materials	1.52	1.37	1.67	1.66	1.51	1.82
Capital goods	-0.27	-0.42	-0.11	-0.19	-0.35	-0.03
Consumer goods	1.47	1.31	1.62	1.76	1.60	1.93
Services for firms	0.87	0.67	1.06	0.96	0.79	1.13

Table A3 continued…

Other services	-0.34	-0.51	-0.18	-0.26	-0.42	-0.09
Non-profit organization	0.85	0.63	1.07	0.91	0.70	1.12
Individual characteristics						
Vocational training	0.67	0.60	0.74	0.58	0.52	0.63
Voc. training and A-levels	0.41	0.29	0.52	0.35	0.25	0.44
Universitv	0.68	0.59	0.77	0.60	0.53	0.67
Blue collar skilled	0.31	0.24	0.37	0.30	0.25	0.36
White collar	0.29	0.21	0.38	0.30	0.23	0.37
Master craftsman	0.28	0.04	0.52	0.42	0.09	0.76
Part-time	-0.13	-0.26	0.00	-0.05	-0.15	0.06
30-34	0.21	0.16	0.27	0.20	0.15	0.25
35-39	0.34	0.28	0.40	0.31	0.25	0.36
40-44	0.46	0.39	0.53	0.37	0.30	0.43
45-52	0.33	0.27	0.40	0.24	0.19	0.30
EU citizen	-0.21	-0.35	-0.08	-0.15	-0.25	-0.06
Non-EU citizen	-0.21	-0.30	-0.12	-0.11	-0.19	-0.03
Production workers	0.60	0.44	0.75	0.72	0.53	0.91
Technicians	0.91	0.75	1.08	0.99	0.79	1.18
Others	0.71	0.56	0.87	0.84	0.65	1.03
Previous employment status						
Unemployment	-0.17	-0.23	-0.10	-0.12	-0.18	-0.06
Non-employment	-0.44	-0.58	-0.29	-0.34	-0.48	-0.20
Recall	-0.04	-0.13	0.04	-0.03	-0.09	0.03
Job-to-job transition	0.46	0.41	0.52	0.45	0.40	0.50
States						
Berlin	-0.61	-0.77	-0.46	-0.66	-0.81	-0.50
Schleswig-Holstein	-0.42	-0.57	-0.27	-0.42	-0.57	-0.27
Hamburg	-0.43	-0.58	-0.27	-0.37	-0.53	-0.21
Lower Saxony	-0.12	-0.28	0.03	-0.51	-0.66	-0.36
Bremen	-0.20	-0.34	-0.06	-0.23	-0.36	-0.09
North Rhine-Westphalia	-0.09	-0.25	0.07	-0.18	-0.33	-0.02
Hesse	-0.63	-0.82	-0.44	-0.57	-0.73	-0.41
Rhineland-Palatinate/Saarland	-0.32	-0.46	-0.18	-0.37	-0.52	-0.22
Bavaria	-0.14	-0.28	-0.01	-0.24	-0.38	-0.11
Year 1996	0.35	0.31	0.38	0.34	0.30	0.38
Constant	2.80	2.48	3.12	3.17	2.89	3.46
Min. sum of deviations	31835			31698		
# of observations	33848			30599		
Convergence	no			no		

Note: Confidence intervals are shown in second and third column.

Table A4: Estimation results for 0.5 and 0.6 quantiles

	Quantile 0.5			Quantile 0.6		
	Coef.	-2.5 %	+2.5 %	Coef.	-2.5 %	+2.5 %
Firm characteristics						
Council	0.76	0.65	0.87	0.71	0.58	0.84
Sector coll. agreement	-0.04	-0.15	0.07	-0.07	-0.16	0.03
Firm collective agreement	-0.44	-0.57	-0.31	-0.24	-0.36	-0.13
Further training	0.54	0.37	0.70	0.33	0.19	0.46
Investments in ICT	0.03	-0.03	0.09	0.01	-0.05	0.06
Share of FTC	0.00	0.00	0.01	0.00	0.00	0.01
100-199	-0.23	-0.38	-0.08	0.06	-0.09	0.21
200-299	0.05	-0.07	0.17	0.03	-0.12	0.19
300-499	-0.31	-0.45	-0.17	-0.18	-0.33	-0.02
500-999	-0.59	-0.72	-0.47	-0.49	-0.65	-0.33
≥ 1000	-0.40	-0.52	-0.29	-0.43	-0.59	-0.28
Insurance, credit	0.31	0.16	0.46	0.50	0.35	0.65
Transport, communication	-0.68	-0.83	-0.52	-0.63	-0.80	-0.46
Trade, repair	0.74	0.60	0.88	0.84	0.68	1.00
Construction	1.87	1.73	2.02	1.22	1.07	1.36
Mining, energy, water	0.29	0.13	0.44	0.50	0.34	0.66
Finish of raw materials	1.72	1.59	1.86	1.49	1.33	1.64
Capital goods	-0.08	-0.22	0.05	0.10	-0.04	0.23
Consumer goods	2.31	2.16	2.47	2.19	0.63	3.75
Services for firms	1.01	0.85	1.18	1.04	0.86	1.22
Other services	-0.18	-0.32	-0.03	-0.10	-0.24	0.04
Non-profit organization	0.75	0.59	0.91	0.62	0.32	0.92
Individual characteristics						
Vocational training	0.52	0.46	0.57	0.34	0.28	0.41
Voc. training and A-levels	0.27	0.16	0.37	0.23	0.13	0.33
Universitv	0.59	0.52	0.67	0.42	0.35	0.49
Blue collar skilled	0.43	0.35	0.50	0.18	0.12	0.23
White collar	0.30	0.22	0.38	0.07	0.00	0.14
Master craftsman	0.93	0.67	1.20	0.37	0.05	0.69
Part-time	0.05	-0.04	0.14	-0.10	-0.18	-0.01
30-34	0.16	0.11	0.21	0.07	0.02	0.11
35-39	0.22	0.16	0.27	0.10	0.05	0.16
40-44	0.27	0.21	0.34	0.16	0.10	0.22
45-52	0.17	0.11	0.23	0.06	0.00	0.11
EU citizen	-0.17	-0.28	-0.07	-0.10	-0.20	-0.01
Non-EU citizen	-0.07	-0.15	0.00	0.00	-0.05	0.05
Production workers	0.97	0.82	1.11	1.19	0.96	1.42
Technicians	1.11	0.95	1.27	1.36	1.13	1.59
Others	1.04	0.89	1.20	1.36	1.13	1.59

Table A4 continued…

Previous employment status						
Unemployment	-0.11	-0.17	-0.05	-0.08	-0.15	-0.02
Non-employment	-0.26	-0.38	-0.15	-0.24	-0.38	-0.10
Recall	-0.07	-0.15	0.00	-0.01	-0.09	0.08
Job-to-job transition	0.37	0.32	0.42	0.19	0.14	0.24
States						
Berlin	-0.02	-0.17	0.13	0.15	0.04	0.27
Schleswig-Holstein	0.40	0.19	0.60	0.33	0.14	0.53
Hamburg	0.17	0.01	0.32	0.30	0.15	0.44
Lower Saxony	0.33	0.17	0.50	0.36	0.22	0.50
Bremen	0.35	0.20	0.49	0.34	0.22	0.46
North Rhine-Westphalia	0.26	0.10	0.42	0.30	0.17	0.43
Hesse	0.01	-0.18	0.20	0.13	-0.06	0.33
Rhineland-Palatinate/Saarland	0.17	0.02	0.32	0.27	0.15	0.40
Bavaria	0.30	0.15	0.45	0.21	0.08	0.34
Year 1996	0.32	0.28	0.36	0.24	0.20	0.28
Constant	3.01	2.71	3.31	3.66	3.32	3.99
Min. sum of deviations	26864			22329		
# of observations	25844			23936		
Convergence	no			no		

Note: Confidence intervals are shown in second and third column.