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Non-technical summary

In this study, we analyze the effects of individual and firm characteristics on job durations using censored quantile regression. We argue that censored quantile regression provides a better empirical framework for estimating the determinants of job durations than conventional hazard rate analysis in many cases. The main advantage of censored quantile regression is that the determinants of mobility are allowed to differ according to the position in the duration distribution. Therefore, quantile regression is an appropriate way to test the conjecture that 'particular demographic characteristics are more important early in a job than later or vice versa' (Farber, 1994: 574). We use this fact to investigate whether we can differentiate between approaches such as search and job matching theory, human capital theory and dual labor markets.

According to the job matching theory, the quality of an employment relationship is seen as an experience good. It is unknown at the start of the employment spell and is only revealed after the worker has been employed for some time, resulting in separations from the firm. Alternatively, human capital theory posits that specific human capital accumulated on the job creates match-specific rents which provide a disincentive to mobility. According to dual labor market theory, institutions such as employment protection enhance segmentation between long-term and short-term employment relationships. Overall, our findings provide some support for job matching and dual labor market theory while less evidence is found for effects of specific human capital.

Among the results, firm-specific coefficients vary markedly with seniority. Person-specific coefficients of variables such as age and education have reasonable magnitudes and are often constant over the duration distribution. The impact of a works council on job durations is high and significant as expected. It unfolds relatively slowly over the employment spell, suggesting that works councils' activities are targeted at workers with high seniority. Unexpectedly, the share of fixed-term contracts has a positive but decreasing impact on job durations. We also include wages, measured at the start of the employment spell. As expected from search theory, job durations are increasing monotonically with wages. However, the effect is strongly decreasing over the duration for high wage categories. This may be explained by the fact that wage gains on the job may differ much among workers with high entry wages, depending on whether workers are successful and offered promotion. Hence, the relevance of entry wages for mobility fades out gradually over time.

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Abstract: Job mobility and employment durations can be explained by different theoretical approaches, such as job matching or human capital theory or dual labor market approaches. These models may, however, apply to different degrees at different durations in the employment spell. Standard empirical techniques, such as hazard rate analysis, cannot deal with this problem. In this paper, we apply censored quantile regression techniques to estimate employment durations of male workers in Germany. 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. By contrast, the influence of human capital variables such as education and further training decreases with employment duration, which is inconsistent with the notion of increasing match-specific rents due to human capital accumulation. The results also suggest that the effects of certain labor market institutions, such as works councils, differ markedly between short-term and long-term employment, supporting the view that institutions give rise to dual labor markets.

JEL-Codes: J62, J63, C41

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

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1 Introduction

Data on job mobility and employment durations reflect a variety of different economic processes. Approaches like search theory, job matching theory, human capital theory and dual labor market theory all explain important and distinct aspects of job mobility. However, it is difficult to derive predictions that allow one to separate the theories empirically. For instance, according to all of these approaches, the separation probability is expected to be high at the beginning of a spell and to decrease with employment duration. Similarly, both job matching theory and human capital theory predict highly skilled workers to have longer job durations. Using methods such as proportional hazard models, it is difficult to assess the empirical validity of each of these theoretical approaches.[1]

In this paper, we argue that censored quantile regression provides a better empirical framework for estimating the determinants of job durations than conventional hazard rate analysis in many cases and is helpful to overcome some of the difficulties just mentioned. The main advantage of censored quantile regression is that the determinants of mobility are allowed to differ according to the position in the duration distribution. Therefore, quantile regression is an appropriate way to test the conjecture that 'particular demographic characteristics are more important early in a job than later or vice versa' (Farber, 1994: 574). Moreover, while different theories often do not differ in their predictions with respect to the *average* effect of covariates on the probability of exit, they sometimes differ markedly with respect to *relative* effects at different job durations. For instance, if the accumulation of specific human capital proceeds slowly, the effect of variables relating to specific human capital is likely to be stronger later than earlier in the employment spell. According to job matching, by contrast, any uncertainty relating to the quality of the match will gradually disappear. Hence, uncertainty should no longer explain dissolutions of long-standing employment relationships.

Another advantage of applying quantile regression is that effects of labor market institutions can be uncovered in more detail than otherwise possible. Firm-level institutions such as firing rules, employee representation and firm-specific training programs may protect workers from dismissal or make job changes less valuable after some time in the employment spell. However, they

1 There are some attempts to estimate structural models derived from specific human capital and matching theory, such as Nagypál (2006). This approach is a useful complement to the non-structural approach taken in our paper.

may not inhibit probation and sorting and, therefore, may have little effect on job stability within the first months of the employment relationship. Similarly, certain personnel policies such as the use of fixed-term contracts may increase mobility initially but may stabilize employment among those matches that have 'survived' the sorting process.

Quantile regressions have often been used by researchers analyzing unemployment durations (Fitzenberger and Wilke, 2006; Koenker and Bilias, 2001; Machado and Portugal, 2002). To our knowledge, however, Horowitz and Neumann (1987) is the only study in which employment durations have been estimated using quantile regression. A particular problem for estimating job durations – as opposed to unemployment durations – is that many spells are long-lasting, so that the degree of censoring is high. Using data for a period of seven years, the proportion of censored spells is about 45 per cent in our case. Nevertheless, we find that numerical problems are not prohibitive. A drawback of the methodology is that we cannot use information on time-varying influences on job durations. This precludes us from investigating hypotheses that rest on changes in observable quantities, such as the effect of current wages on job durations.

Most studies analyzing job mobility use either firm-level data or worker-level data with limited information on the firm side. In this paper, we use a matched employer-employee dataset with particularly rich information on firm-level characteristics. Furthermore, the data allows us to generate a flow sample which avoids biases due to left censoring. At the worker level, job durations have been analyzed using techniques such as linear regression on the stock of job durations (Bronars and Famulari, 1997; Gerlach and Stephan, 2005; Mumford and Smith, 2004) or, more appropriately, duration analysis based on inflows into employment (Abowd et al., 2006; Boockmann and Steffes; 2005; Dohmen and Pfann, 2004; Dostie, 2005). All of these approaches do not allow for changes in the effects of the covariates on job durations.

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 and institutions changes over the employment spell, and propose a number of testable predictions. In section 3, we introduce our data. Section 4 contains the methodology, while empirical results are presented in section 5. In the concluding remarks, we summarize the main implications of our findings.

2 Hypotheses on the determinants of employment durations

In this section, we show that there are strong reasons why the determinants of job exit may differ across the distribution of job durations. We highlight that the different theoretical accounts of the mobility process all imply that the effects of the determinants of mobility change with job durations, so that the use of quantile regression is required. Moreover, the predictions differ in important respects. This enhances the opportunities to empirically distinguish between different theoretical approaches. Table 1 summarizes our expectations about the coefficients of the explanatory variables.

Table 1: Expectations about effects of covariates

	Matching theory	Search theory	Human Capital theory	Dual labour markets
Firm-Size	+ ↓		+ ↑	
Investments in ICT	- ↓		+ ↑ or 0 →	
Further Training			+ ↑	
Education	+ ↓		+ ↑	
Job position	+ ↓		+ ↑	
Age	+ ↓		– ↑	
Job-to-job change	+ ↓			
Unemployed/out-of-labor	– ↓			
Recall	+ ↓		+ ↓	
Entry wage	+ ↓ (low q.)	+ →	+ ↑	
Share of FTC				– ↓ (low q.) + → (high q.)
Works council				+ ↑

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

Job matching

In the case of job matching theory, the quality of the match between worker and employer 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 falls below a reservation level, the match is dissolved. Hence, the number of

separations (both quits and layoffs[2]) reflects the degree of initial mismatch in new hirings. If there are costs to mobility, the number of separations changes in a hump-shaped fashion over time (Farber, 1994). Initially, few jobs are ended since little information has been gained. At long durations, all bad matches have been terminated. Most sorting activity takes place at intermediate durations.

According to matching theory, we expect that employer and worker characteristics influencing the amount of initial mismatch have relatively large effects at the beginning of the relationship but have no effects later in the employment spell. Some firm characteristics may have a systematic influence on the uncertainty attached to the quality of the match at the moment when the employment contract is signed and, hence, on subsequent job mobility.[3] For instance, personnel policies of large firms are often known to job applicants, while those of small firms are often unknown to outsiders. Consequently, one expects a higher degree of mismatch in the latter. Similarly, in firms having introduced new technologies, uncertainty about the kind of workers required is higher than elsewhere and sorting may be more intense.

Matching theory also has interesting implications for the effects of individual characteristics. In the case of young workers, less information is available on their ability for certain kinds of work. Hence, low age is expected to have a larger influence early in the employment spell than later, when the uncertainty has been resolved.[4] A similar argument applies for persons who have been unemployed or out of the labor force for some time (Arranz and García-Serrano, 2004). By contrast, in case of persons who had been previously employed by the same employer, the quality of the match should be known and, hence, job matching theory predicts a positive (but decreasing) coefficient. Lastly, it can be shown in a matching model that high-skilled workers and those in higher job positions have lower exit rates initially in the employment spell because they sustain a bad match longer than others. The reason is that their opportunity costs are higher and, therefore, they try to avoid periods of unemployment (Moscarini, 2003). The argument becomes less relevant later in the employment spell, so that an effect of skills should mainly be found at lower durations.

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2 Jovanovic (1979a) considers only the case of quits but the argument can be generalized to the case of both quits and layoffs.

3 Clearly, companies' and workers' decisions as to whether to conclude employment relationships with high inherent risks are endogenous. To the extent that variables like employment history are included, however, we capture some of the unobserved inherent riskiness of hirings.

4 This view is related to the job-shopping argument (Johnson, 1978).

Specific human capital

To the extent that specific human capital accumulated on the job is not transferable across employers, match-specific rents are created which provide a disincentive to mobility. Since this effect rises with the stock of human capital, mobility decreases over the employment spell (Becker, 1993; Jovanovic, 1979b). In contrast to matching theory, all variables that capture match-specific rents should display a quantitatively more pronounced impact at higher quantiles of the duration distribution. For instance, if match-specific human capital and formal education or job-related training are complementary, we expect a positive effect of education or training on job durations that is increasing with job tenure.[5]

At the level of the firm, the use of certain skill-intensive technologies and the provision of firm-financed further training should positively influence the amount of job-specific human capital. Investments into further training paid by the employer are generally firm-specific and should, therefore, lead to longer job durations (Becker, 1993; Lazear, 2003). Investments into information and communication technology (ICT) are often followed by the need of special training for the employees who work with the new system. If the technology is firm-specific the effect of investments in ICT is expected to increase with job duration. If not, the special training is an investment in general human capital and there should be no relationship with tenure.

On the basis of human capital theory, we expect durations to decline ceteris paribus with age at the start of the job. The reason is that employers and employees only invest in firm-specific human capital if they expect that the relationship will continue for some time. Moreover, it may be more difficult for older employees to accumulate human capital because the ability to absorb new information declines with age. As far as job-specific human capital is considered, the worker's employment history should not be important for job exit according to human capital theory. The only origin state which should affect tenure is an employment spell in the current firm. If recalled, the employee already possesses knowledge of firm-specific technical and organizational processes and, hence, starts with a positive amount of match-specific rents. To the extent that new employees catch up with this initial advantage, the positive

5 Due to the concavity of human capital accumulation commonly assumed in the literature, the strength of the effect is expected to decrease towards the end of the duration distribution. Given that we observe individuals at most over seven years, however, it is unlikely that we find strong evidence for decreasing marginal effects of education in our data.

effect on duration is bound to decrease over time.[6] This prediction is similar to the prediction of a matching model.

Search theory and the role of wages

According to search theory, tenure is expected to increase with wages (see, e.g., Mortensen and Pissarides, 1999). In the simplest case, workers obtaining wage offers from other employers will leave their current job if the outside wage is higher than the current wage. Thus, wages are a crucial variable for the determination of job durations. An important issue for specification is to which amount tenure is due to wage gains received during the current employment spell or due to the employer paying higher wages at all durations. In the former case, tenure depends on the current wage, while in the latter case it depends on the initial wage in the job.

While quantile regression cannot account for wage changes in the job, it is possible to analyze changes in the effect of entry wages over the duration of the employment contract. An impact of entry wages at the beginning of the job could easily be explained by search theory. If we find a significant effect at higher quantiles, this could be explained by search theory if current and entry wages are correlated. However, we would expect the effect to weaken with duration.

There is a well-known endogeneity problem with respect to wages in a tenure equation (Abowd et al., 2006; Altonji and Williams, 2005). According to human capital theory, the worker accumulates firm-specific human capital over time. Hence, wages depend positively on tenure (reversed causation). Therefore, the wage is excluded in some analyzes of job durations (e.g. Gerlach and Stephan, 2005; Mumford and Smith, 2004) or the endogeneity problem is not accounted for (e.g. Dohmen and Pfann, 2004). Some studies solve this problem using simultaneous or two-step estimation (e.g., Abowd et al., 2006). Unfortunately one cannot apply these techniques in censored quantile regression. Clearly, the exclusion potentially creates omitted variable bias if the wage is correlated with other observed or unobserved characteristics, such as productivity. While reversed causation is more relevant to current wages than for initial wages, job durations and entry wages could be linked due to unobserved characteristics. For instance, entry wages may proxy for certain skills that may

6 Firm-specific human capital can be depreciated during the non-working period. Because we only define employment spells as recalls in cases where the last employment spell has been with the same employer, depreciation should not have a great influence in our case.

also cause employer-employee matches to be more durable. Our strategy is to estimate specifications with and without wages. While the effect of wages on tenure must be interpreted with great care, it is instructive to see whether the coefficients of the other independent variables are sensitive with respect to the potential omitted variable and endogeneity biases.

Institutions and dual labor markets

There is strong empirical evidence that employment protection increases job stability (Gerlach and Stephan, 2005; Kugler and Pica, 2003). However, employment protection often becomes effective only after a certain amount of time, e.g. after a probation period. In the German case, tenure also increases employment protection because it is one of the criteria used by labor courts to judge whether a dismissal is 'socially justified'. Employment protection is often limited by the use of fixed-term contracts. Workers on fixed-term contracts are more likely to exit under two conditions: either the nature of the job or the companies' need for additional workers is temporary or fixed-term contracts are used as screening devices. On the basis of the screening mechanism, we expect to observe higher exit rates at lower quantiles and lower exit rates at higher quantiles in firms offering fixed-term contracts (Boockmann and Hagen, 2005; Capelli and Neumark, 2001). Fixed-term contracts could also be used to stabilize employment and insulate long-term workers from short-term adjustment needs.

There also exists, however, a reason why the use of fixed-term contracts may initially enhance job stability. According to the German labor law, separations before the expiry of a fixed-term contract are usually only possible by mutual agreement. Thus, it is often impossible for employers to dismiss fixed-term workers. Unfortunately, we do not observe the individual type of contract but only the proportion of workers having a fixed-term contract.

Another institution, apart from employment protection, that is often believed to increase job stability is works councils. One reason is that they may have a role in individual dismissals and redundancy procedures and may, thus, slow down separation decisions. Another is that works councils have a 'voice' function, making companies more attractive to the workforce and thus reducing voluntary quits (Frick and Möller, 2003). In either case, if works councils are dominated by 'insiders' with high job tenure, it is likely that their effect 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). Therefore, the effect of works

councils should increase over the duration distribution. In all of these cases, institutions create segmentation among companies' workforces into short-term and unstable employment on the one hand, and protected or voluntarily long-term employment, on the other.

3 Data and descriptive statistics

The database used in this study is the German LIAB, a linked employer-employee dataset provided by the Institute of Employment Research (IAB) of the Federal Employment Agency.[7] The LIAB combines administrative data on employees with employer data from a large-scale representative survey of plants, the IAB Establishment Panel. This annual survey contains data on 16,000 establishments. The LIAB is exhaustive on the number of workers covered within the establishment sample. 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 (Kölling, 2000).

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 employed by 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 spell is assumed if two conditions apply. First, the individual is observed to move into unemployment or non-employment or is hired by a new employer. Second, the current employer reports the end of the employment relationship to the insurance institution.[8] A change of the employer identifier

7 The data source is discussed in greater detail in Alda et al. (2005). The dataset used for estimations is discussed more closely in Boockmann and Steffes (2005).

8 However, in case of exit to unemployment we do not require that the end of the employment relationship is reported by the employer.

alone is insufficient as a definition of a job exit. The reason is that a change of identifier may occur although the individual continues to work in the same workplace, e.g. if the legal identity of the employer or the owner of the establishment change.[9] Hence, cases in which the end of the employment contract is not reported are treated as censored.

Table 2: Definitions of destination and origin states

Employment state	Definition
a) Destination states	
Unemployment	worker receives unemployment benefits for at least one day within 60 days after separation, is not employed with current employer for at least 90 days after separation
Non-employment	worker is not employed with current employer for the next 90 days after separation, receives no unemployment benefits and does not change from job-to-job for at least 60 days after separation and has recorded end of relationship
Job-to-job change	worker takes up employment with another employer within 60 days after separation and has recorded end of relationship
Recall	worker takes up employment with the same employer after more than 90 days after separation and has recorded end of relationship
b) Origin states	
Unemployment	worker received unemployment benefits for at least one day during 60 days before hiring, was not employed with current employer for at least 90 days before hiring
Non-empl. $\leq$ 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-empl. > 1 year	worker was not observed for at least 1 year before hiring
Recall	worker was employed with current employer for more than 90 days before hiring, previous spell ended with recorded end of relationship, received no unemployment benefits during 60 days before hiring, did not change from job-to-job during 60 days before employment
Job-to-job change	worker did change from job-to-job at most 60 days before employment
First employment	worker not observed since January 1st, 1991, not older than 30 years at the first observed spell between 1996 and 2001

The upper panel of table 2 briefly summarizes the definition of the destination states. Episodes in which neither of these destination states can be

9 In our data, this happens in a large number of cases. Other studies based on similar data, such as Bellmann et al. (2000) and Bachmann (2005), rely solely on changes of the identifier and, therefore, have far less cases of censoring.

verified are taken as censored at the last observed employment record. Recalls to the same employer within 90 days after the end of the previous spell are defined as uninterrupted employment. Recalls after that period are treated as a separate destination state if the end of the employment contract has been reported. More discussion on the definition of employment spells is contained in Boockmann and Steffes (2005).

To determine the beginning of an employment spell, we proceed analogously, but we distinguish between short and long spells of previous non-employment (see lower panel of table 2). The former are defined as gaps of less than one year in a person's employment history. Furthermore, individuals below 30 years of age starting their first spell in the data after 1996 and not observed between 1991 and 1995 form a separate category because these employees are likely to be observed in their first job.

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. All spells with missing covariate information are also eliminated from the data. These requirements leave us with a sample of 31,941 employment spells, of which 44 per cent are censored. Means and standard deviations of all covariates are presented in table A1 in the appendix.

The Kaplan-Meier estimate of the empirical survival function is drawn in figure 1. The graph shows the unconditional quantiles at which the effects are estimated in the following. For instance, the 0.1 quantile is at 65 days, the 0.2 quantile at 178 days, and the 0.5 quantile (i.e., the median) is at 1307 days or 3.5 years. In figure 2 the hazard curve of our sample is shown. Farber distinguishes between job matching and human capital theory on the basis of initial increase in the job exit probability, which is in line with matching but not with human capital accumulation. In the graph, we do observe exactly this pattern since the hazard rate increases at the beginning and later on decreases again.

Figure 1: Kaplan Meier graph

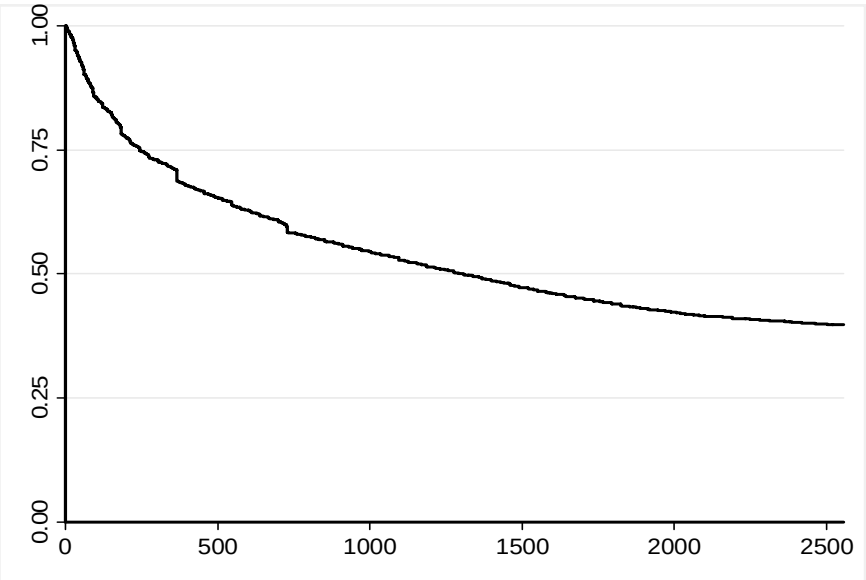
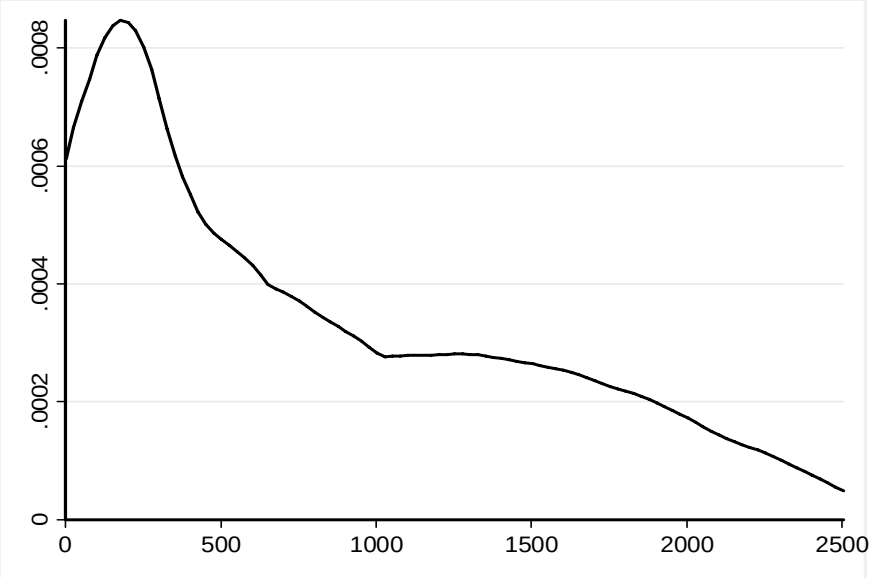


Figure 2: Hazard curve



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. Compared to standard techniques such as the Cox proportional hazard model, the main advantage is that coefficients may vary over the duration distribution (Fitzenberger und Wilke, 2006; Koenker and Geling, 2001). While quantile regression has been applied for a long time, the extension to censoring poses particular problems and has been developed only relatively recently (see

Fitzenberger, 1997). A drawback of using CQR is that only time-invariant covariates can be used. Hence, the influence of the business cycle or current wages cannot be ascertained using this methodology. Furthermore, in contrast to the proportional hazard model, the estimation of fixed firm effects is not possible using this model.[10]

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

$$h(T_i) = x_i'\beta(\theta) + \varepsilon(\theta)_i \tag{1}$$

where the θ-quantile of $\varepsilon(\theta)_i$ conditional on x_i is zero and $h(.)$ is a strictly monotonic transformation of the completed duration T_i of the spell i. The conditional quantile of model (1) can be written as

$$Q_{h(t|x)}(\theta) = x_i'\beta(\theta) \tag{2}$$

For this kind of regression, several transformations can be chosen as long as they preserve the ordering of the quantiles (Fitzenberger et al., 2004). In this study, we use the log transformation:

$$Q_{\log(T_i)}(\theta) = x_i'\beta(\theta) \tag{3}$$

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 exceeds the observed duration. Applying CQR requires that a potential C_i should be defined even if the true duration is known. In our case, it is natural to take the potential censoring point as the end of the observation period. Modifying model (3) by allowing for censoring yields

10 In principle, one could estimate fixed firm effects including firm dummies. However, due to the high number of firms in our sample, computational costs would be prohibitive in our case.

$$Q_{\log(T_i)}(\theta) = x_i'\beta(\theta) \vee \log(C_i) \tag{4}$$

The coefficients are estimated by minimizing an objective function. It consists of a weighted sum of the distances between the true log durations and the minimum of the conditional quantile and the censored log durations:

$$\sum_{i=1}^{N} \rho_\theta(\log(T_i) - \min(x_i'\beta(\theta), \log(C_i))), \tag{5}$$

where N is the absolute number of employment spells and ρ_θ is the so-called check function $\rho_\theta(z) = z(\theta - I(z<0))$, with $I(A)$ an indicator function equal to one if A is true. The check function weights the fitted values according to the quantile.[11] Minimizing the distance function (5) yields the CQR estimator $\hat{\beta}(\theta)$. It can be shown that the estimator is $\sqrt{N}$ -consistent and asymptotically normally distributed. For a discussion of the asymptotic distribution, see Fitzenberger (1997).

Due to right-censoring, the distance function for CQR is not convex. Hence, the calculation of the estimator is numerically difficult. Several procedures to calculate the estimator have been developed in the literature. They are compared by Fitzenberger and Winker (1999) with respect to their computational performance. These authors conclude that most of the algorithms encounter difficulties in case of a high censoring rate. Therefore, we compared coefficients obtained using the algorithm developed by Buchinsky (1998) implemented in STATA and the one developed by Fitzenberger (1997) implemented in TSP. Results were found to be numerically identical and thus we only present results from estimations with STATA.[12]

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

11 A quantile regression without censoring is a special case where $C_i = +\infty$.

12 The data can only be accessed at the Research Data Centre of the Federal Employment Agency, which uses STATA as a standard statistical software. Therefore, we first implemented the Buchinsky algorithm before comparing the results to those obtained using the Fitzenberger algorithm. Results of the comparison can be delivered by the authors on request.

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.[13]

5 Estimation results

Results for estimations including the entry wage[14] are reported in figure 3 (firm characteristics), figure 4 (individual characteristics) and figure 5 (wages). The curves display per cent effects relative to a reference group.[15] Individual coefficient estimates can be found in table A2 in the appendix. Additional variables like collective bargaining structure, industry, nationality, country and year of hiring are included in the estimations but results are not shown in the graphs. For the purpose of comparison, results for a specification without wages are included in table A3 in the appendix.

The impact of firm characteristics

Among firm characteristics, the effect of works councils is particularly interesting. At all quantiles, employees in firms with a works council experience significantly longer job durations. Quantitatively, the effect is much stronger for long job durations. At the 0.6 quantile (i.e. for workers with approximately five years of job tenure), job durations are predicted to be 60 per cent higher in firms with works councils. This suggests that the works councils' activities are mainly targeted at high-seniority workers. If wages are excluded, the effect of works councils is even higher, and the rising profile over quantiles is even more pronounced. This indicates a positive correlation between works councils and entry wages over the whole duration distribution. It suggests that, at least to some degree, the effect of works councils on durations is due to works councils' effects on wages. A question we cannot disentangle using our methodology is whether the increase in the effect of works councils is due to their impact on the

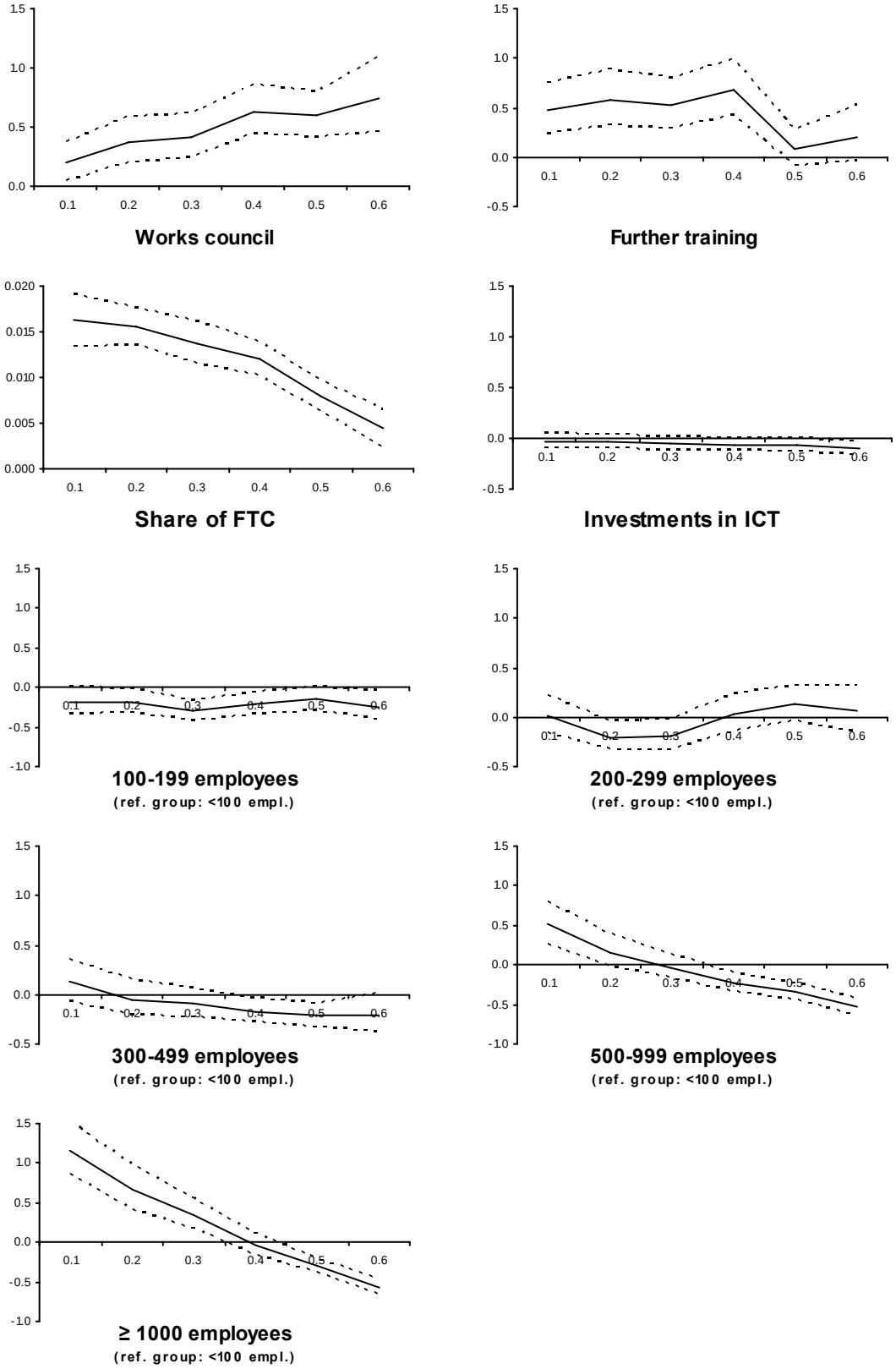
13 When estimating higher quantiles, the number of cases in which the fitted values were higher than the potential censoring points was very high at all iteration steps. Therefore, the results are not displayed here.

14 Since wages are right censored at the social security contribution ceiling in the data, we imputed censored wages according to the procedure by Gartner (2005).

15 They are calculated as one minus the exponentiated coefficients.

wage trajectory in the job (Addison et al., 2006) or due to other mechanisms, such as improved working conditions or higher employment protection.

Figure 3: Estimation results of firm characteristics (wages included)



Investments in information and communication technology (ICT) have a negative impact on long job durations. Coefficients are insignificant up to the 0.5 quantile and are significantly negative at the 0.6 quantile. Without including the entry wage even the coefficient at the 0.5 quantile is negative. Therefore, the investments done by the firms we observe in the sample are not related to firm-specific human capital.

The use of fixed-term contracts is measured at the firm level as the share of fixed-term contracts in total employment in the establishment. As mentioned earlier, individual contract status in not available in the LIAB. The estimated impact of the share of fixed-term contracts is significantly positive at low quantiles but decreases over the quantiles. Quantitatively, a one percentage point increase in the share of fixed-term contracts is associated with a 1.6 per cent increase in job durations at the 0.1 quantile. The fact that employment stability increases with the share of fixed term contract appears to be counter-intuitive at first sight. In particular, it cannot be explained by the fact that long-lasting employment relationships are stabilized by the use of temporary work, because the effect is only present for short employment durations. However, it is quite possible that the decreasing effect over quantiles could be due to the legal difficulty of dissolving a fixed-term contract prematurely. Running a regression without wage information results in smaller estimated effects at all quantiles, indicating that the overall positive effect of fixed-term contracts is higher in low-wage firms.

Firm size is expected to have a positive influence on job durations.[16] Our results show that in comparison to the reference group of small firms with less than 100 employees, large firms with more than 1000 employees exhibit 115 percent longer job durations at the lowest quantiles. However, the effect decreases with seniority and is actually negative after the 0.4 quantile. At the highest observed quantile, the effect is about minus 60 percent. A similar pattern is found for firms with 500 to 999 employees; here, the effect is insignificant for the 0.2 and 0.3 quantiles. For firms with between 100 and 199 employees, employment durations are significantly shorter beyond the 0.2 quantile. All other firm size dummies are more or less insignificant. The effects are somewhat higher in estimations without wages. These results indicate that the argument that internal labor markets stabilize employment in large firms needs to be

16 The usual argument is that internal labor markets facilitate job changes within large firms. However, Boockmann and Steffes (2005) find that the effect is small and does not increase monotonically with firm size. Mumford and Smith (2004) even find no significant effects of workplace size.

qualified in two important respects. First, it applies only to very large firms. Second, it is valid only for relatively short durations. Overall, our findings are inconsistent with internal labor markets. They are consistent with job matching theory if screening takes longer in larger firms due to particular difficulties in observing match quality if the number of workers is large.

Job durations in firms providing further training are about 50 per cent higher than in other firms. However, the effect becomes much smaller and statistically insignificant at the 0.5 quantile.[17] In this sense, investments into further training have no long-run impact on job stability. This finding contradicts the view that training facilitates the accumulation of specific human capital. It is much more in line with matching theory. Firms offering further training may also require a highly specialized workforce. Separations may be more costly in this case, and it may take longer to complete the process of sorting of good and bad matches.

The impact of individual characteristics

As opposed to firm-level variables, the effects of individual characteristics are relatively constant over the job duration distribution. Compared to the reference group of employees with vocational training, individuals who only had school education (either completed A-levels or below) have significantly shorter job durations.[18] The effects, a 35 or 65 per cent reduction, respectively, remain roughly constant over the duration distribution. Employees with university degree have lower job durations up to the fourth quantile, implying that high skilled male employees are more mobile than semi-skilled workers. However, the effect is not present later in the employment spell. The effect on university graduates is the only one which depends qualitatively on the inclusion or exclusion of entry wages. If they are excluded, there is no significant influence of university education over the first five quantiles, while there is a significantly positive effect at the sixth quantile. White collar and skilled blue collar workers have consistently higher job durations than unskilled blue collar workers. In general, the inclusion of entry wages shifts the effects of education and job position more or less constantly over the tenure distribution. This indicates that

17 Estimations without entry wages produce similar results.

18 An explanation for the finding for individuals with A-levels could be that these are students working during university holidays. The same could hold for employees with vocational training and A-levels, who also have lower job durations. Although many of these cases should be excluded from the data by setting the lower age limit to 25, we cannot identify these employment relationships as long as they are covered by the social security system.

entry wages are a good proxy for general human capital to the extent that it is correlated with the skill level and the job position. The fact that the effects of education or skills do not increase over the duration distribution may be interpreted as evidence against the specific human capital model, if skilled and better educated workers are in a better position than the unskilled to accumulate human capital.

Figure 4: Estimation results of individual characteristics (wages included)

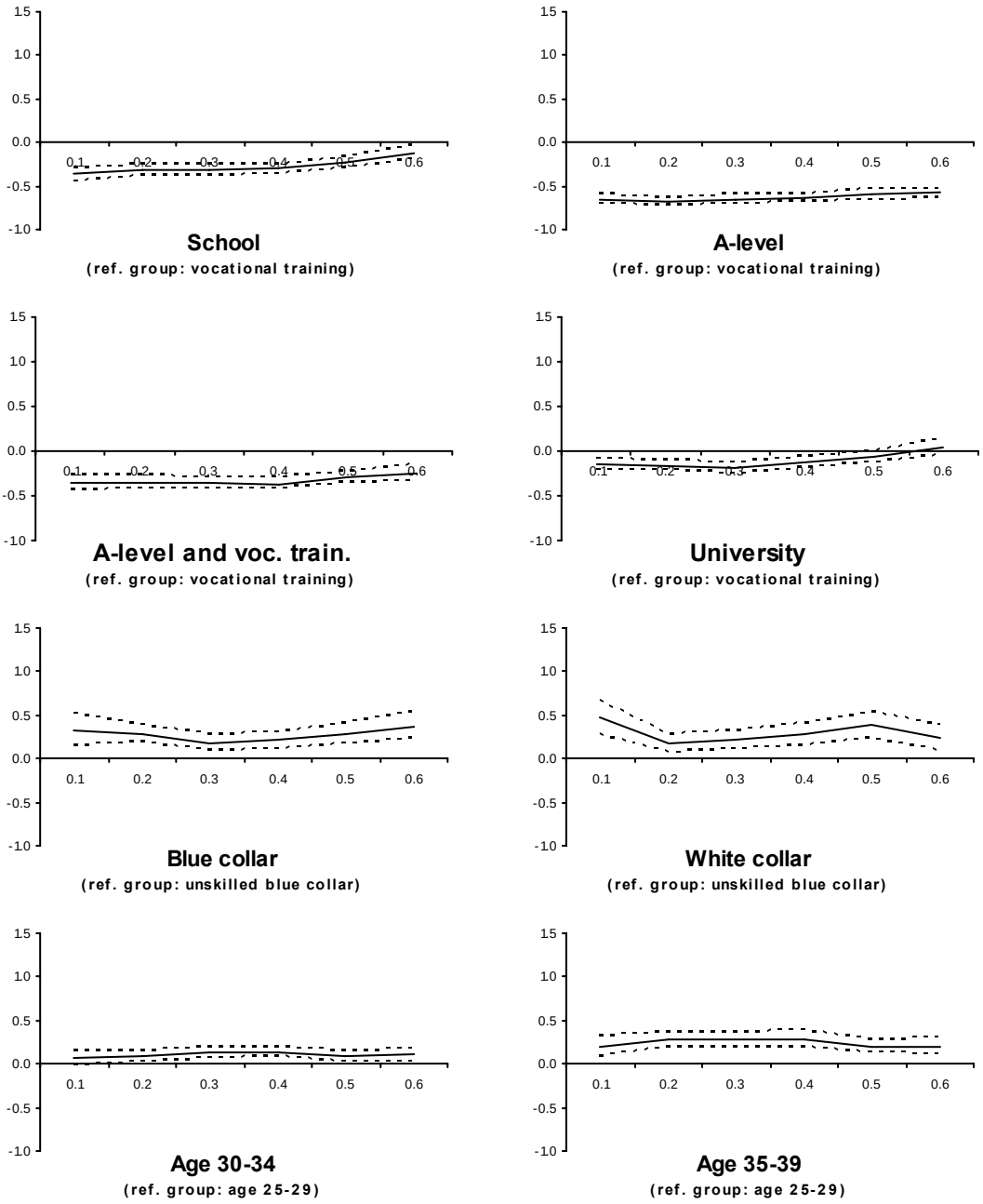
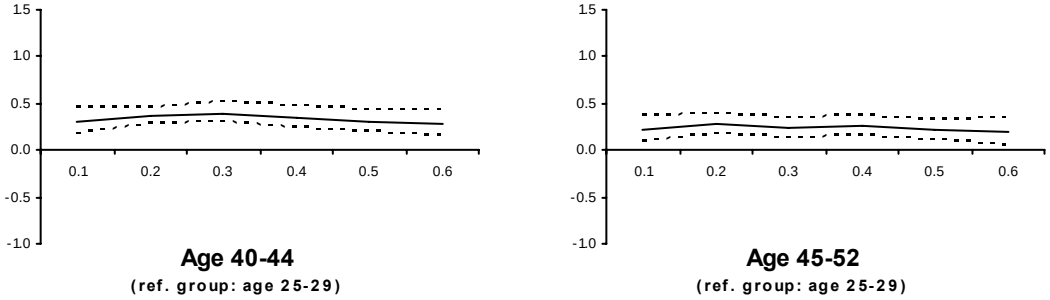


Figure 4 continued…

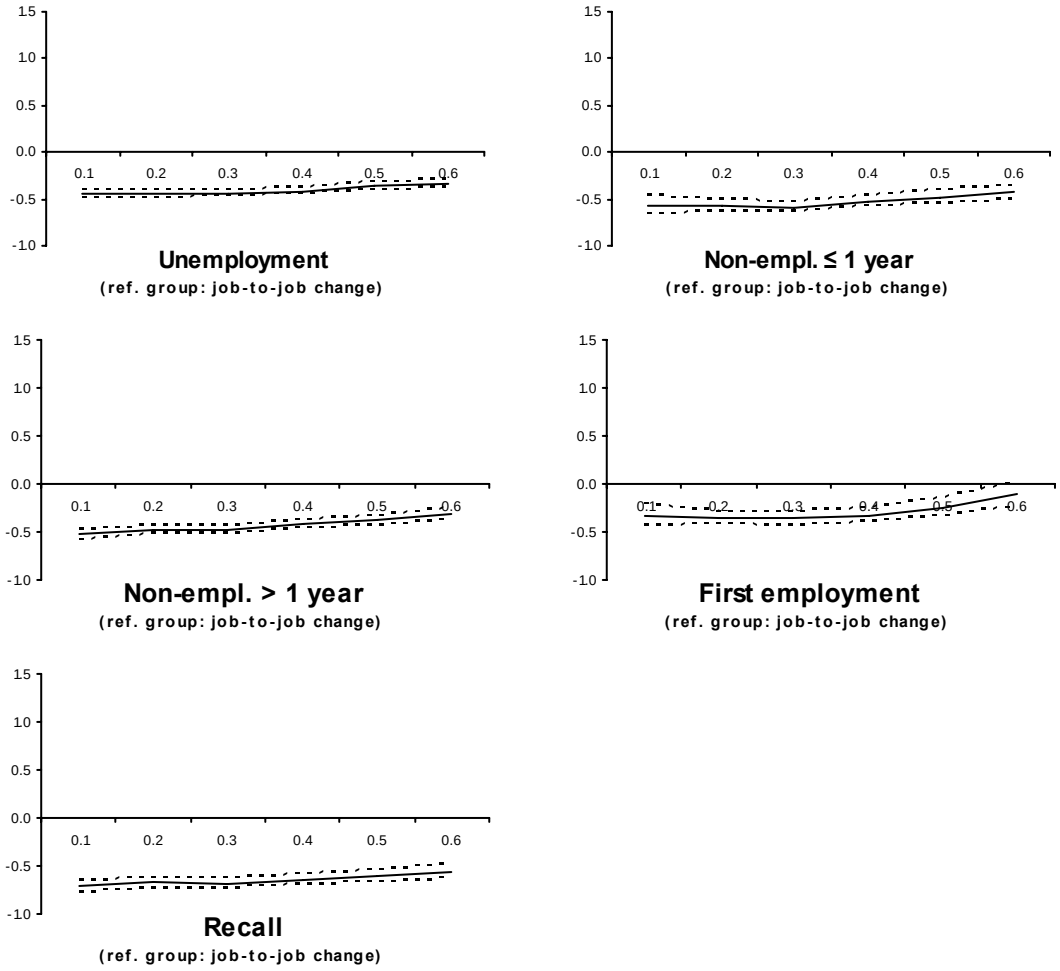


We expect young workers to exhibit shorter job durations according to a higher probability of initial mismatch. This phenomenon should decrease with seniority. According to the human capital view, we expect older workers to have shorter durations because building specific human capital pays less for them. This effect should be more marked the higher job durations. The results show that age (measured at the beginning of the employment spell) increases job durations up to the age of 40 to 44. From then, there is a slight decline in job stability. In estimations without wages, the effects are a bit higher but the slopes are decreasing at higher quantiles. The coefficients are quite similar between quantiles. Taken together, there is only little evidence in favor of human capital theory.

A number of studies (e.g. Booth et al., 1999; Battu et al., 2002) find a high impact of the employment history on the survival probability.[19] Compared with the reference group of employees who changed from one employer to another, all other employees exhibit significantly shorter durations over the whole observed distribution. This is the expected result according to matching theory. Moreover, the effect decreases over time, which is also in line with matching theory. The prediction that recalls to the previous employer result in longer job duration is, however, not borne out by the data. In fact, recalls end earlier than all other employment spells. Unobserved match-specific heterogeneity, such as the seasonal nature of some employment relationships, is probably responsible for the tendency towards repeated short-term jobs. Finally, the impact of employment history is only little influenced by wages.

19 These coefficients must be interpreted carefully because previous employment states could be correlated with unobserved individual characteristics. However, for the understanding of the determinants leading to heterogeneous job durations, employment history provides highly valuable information that should not be dismissed.

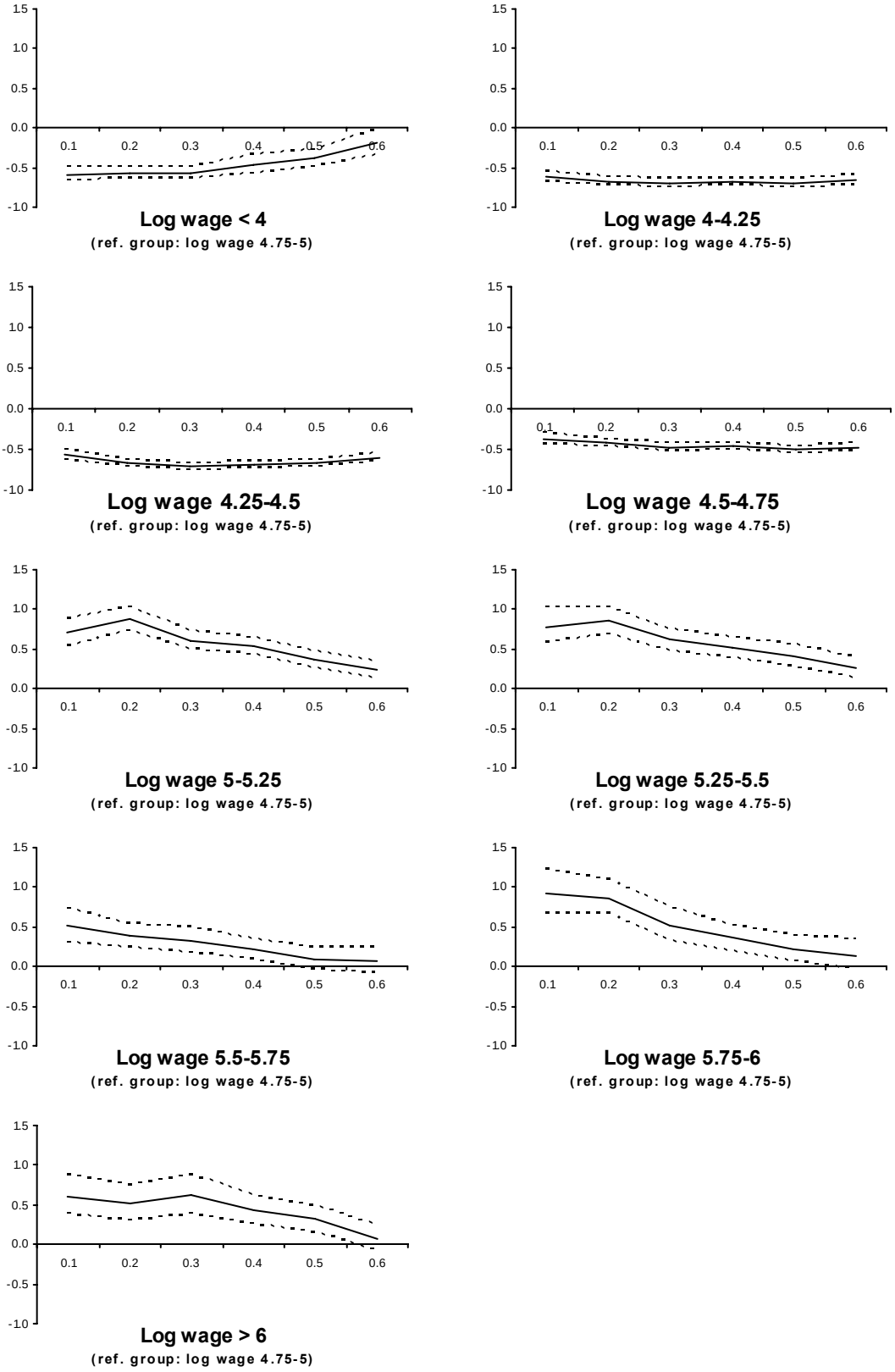
Figure 5: Estimation results of employment history (wages included)



The impact of entry wages

Figure 6 shows effects of the wage (measured as dummies for log wage brackets) on job durations. Wages refer to the first year during which an employment spell was observed. As expected from search theory, job durations are increasing monotonically with wages. However, there is a remarkable asymmetry below and above the middle wage bracket. While the effect is constant over the duration distribution for low wages, it is decreasing for the high wage categories and becomes insignificant for the 0.6 quantile. Some part of this difference may be explained by different wage trajectories depending on entry wages. An employee who starts with a low wage is unlikely to move up the wage distribution, while a worker with a high entry wage may be on a constant or a rising wage trajectory, depending on the specific capital accumulated on the job. In the second case, the effect of initial wages on current job duration is attenuated.

Figure 6: Estimation results of entry wages



6 Conclusions

In this study, we have analyzed the effects of individual and firm characteristics on job durations using censored quantile regression. The estimation procedure allows us to distinguish between the impact of observed characteristics on short and long job durations. Whereas predictions concerning the average effects of observable characteristics on job durations are often similar according to different theories, the expected impact at different points of the duration distribution varies. We use this fact to investigate whether we can differentiate between approaches such as search and job matching theory, human capital theory and dual labor markets. In addition, the comparison between models with and without entry wages gives some interesting insights into potential biases of the estimators. Furthermore, 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 its development with seniority.

A number of specific findings deserve to be highlighted. First, firm-specific coefficients vary markedly with seniority. For instance, large firms have relatively more job stability initially than small firms, possibly since they are better known among job applicants. But the effect fades away with duration and even becomes negative at higher points in the distribution. Second, person-specific coefficients, such as those for age and education, are often constant over the duration distribution. If anything, the effect of education becomes weaker with tenure. Furthermore, variables such as the previous employment state lose some of their strength with increasing tenure. Third, if entry wages are included, almost all of the estimated coefficients of individual-specific variables are weakened due to the correlation of characteristics and wages. In addition, some firm characteristics, such as the share of fixed-term contracts or the presence of a works council, turn out to be correlated with entry wages. Fourth, the coefficients of entry wages are positive as expected but decrease towards zero over quantiles in high wage groups. Low wage groups exhibit more or less constant effects. While entry wages may be endogenous to job durations, the problem is less serious than for current wages where a reversed causality from tenure to wages exists. The inclusion of entry wages in tenure estimations is, therefore, appropriate in order to avoid biases due to omitting the wage information.

Overall, there is some support from the results for job matching theory. Lower exit rates in large firms at the beginning of a match are one example. The decline in the effect of employment history is also consistent with job matching theory. By contrast, little evidence is found for effects of specific human capital. In particular, the effect of vocational training on job stability declines at higher durations. So does the provision of further training by the employer. Both results are in contrast to human capital theory if education or training and specific human capital are complements.

The impact of a works council on job durations is high and significant as expected. It unfolds relatively slowly over the employment spell, suggesting that works councils' activities are targeted at workers with high seniority. Thus, there is support for the view that institutions may reinforce labor market segmentation. Unexpectedly, the share of fixed-term contracts has a positive but decreasing impact on job durations. Further research with data having information on the contract type at the individual level is required to shed more light on this result. It would also be interesting to have more detailed information on other personnel policies.

Although the empirical analysis of search, matching and human capital theory is still an issue for future research, this study shows that quantile regression can be a helpful instrument in this context, even if our data stretches only over a relatively short period of time. A similar application with a longer observation period could offer additional insights into the determination of job durations. It could also be helpful for addressing issues such as the changes in job stability over time of the impact of the business cycle on job durations.

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Appendix

Table A1: Means and standard deviations of covariates

	Mean	St.dev.		Mean	St.dev.
Firm-specific			***Individual-specific***		
Bargaining			*Education*		
Works council	.92	.27	Secondary school	.16	.36
Sector coll. agreement	.80	.40	A-level	.06	.23
Firm collective agreement	.15	.36	Vocational training and A-levels	.06	.24
Further training: yes/no	.96	.20	Vocational training	.49	.50
Investments in ICT	.81	.40	University	.23	.42
Share of FTC	6.76	14.65	*Job position*		
Firm-size			Blue collar unskilled	.36	.48
<100			Blue collar skilled	.16	.37
100-199	.05	.22	White collar	.41	.49
200-299	.06	.24	Master craftsman	.01	.08
300-499	.08	.27	Part-time	.06	.24
500-999	.12	.33	*Age*		
≥ 1000	.62	.49	25-29	.35	.48
Sector			30-34	.28	.45
Insurance, credit	.07	.25	35-39	.17	.37
Transport, communication	.05	.21	40-44	.10	.31
Trade, repair	.08	.27	45-52	.10	.29
Construction	.07	.25	*Profession*		
Mining, energy, water	.02	.14	Production	.40	.49
Finish of raw materials	.26	.44	Technical	.13	.34
Capital goods	.21	.41	Services	.46	.50
Consumer goods	.08	.28	Others	.01	.11
Services for firms	.04	.20	*Previous Employment status*		
Other services	.08	.28	Job-to-job change	.49	.50
Non-profit organization	.03	.16	Unemployment	.28	.45
Regional authorities, social insurances	.02	.13	Non-empl. ≤ 1 year	.04	.19
Country			Non-empl. > 1 year	.10	.30
Berlin	.07	.26	First employment	.05	.23
Schleswig-Holstein	.02	.15	Recall	.04	.19
Hamburg	.07	.25	*Log Wage*		
Lower Saxony	.07	.25	< 4	.04	.19
Bremen	.03	.16	4.0-4.25	.03	.16
North Rhine-Westphalia	.30	.46	4.25-4.5	.06	.24
Hesse	.07	.25	4.5-4.75	.17	.37
Rhineland-Palatinate/Saarland	.05	.21	4.75-5.0	.27	.44
Baden-Württemberg	.12	.33	5.0-5.25	.21	.41
Bavaria	.21	.41	5.25-5.5	.12	.33
			5.5-5.75	.06	.24
			5.75-6.0	.03	.17
			≥ 6.0	.02	.15

Table A2: Estimation results with wages

	Quantile 0.1			Quantile 0.2			Quantile 0.3			Quantile 0.4			Quantile 0.5			Quantile 0.6		
	Coef	low	up	Coef	low	up	Coef	low	up	Coef	low	up	Coef	low	up	Coef	low	up
Firm characteristics																		
Works council	0.18	0.04	0.32	0.32	0.18	0.46	0.35	0.22	0.48	0.49	0.36	0.62	0.47	0.35	0.59	0.56	0.38	0.74
Sector coll. agreement	-0.16	-0.27	-0.05	-0.10	-0.20	0.00	-0.10	-0.21	0.00	0.04	-0.06	0.13	0.03	-0.07	0.13	-0.03	-0.14	0.09
Firm collective agreement	-0.31	-0.45	-0.16	-0.24	-0.35	-0.12	-0.28	-0.40	-0.15	-0.18	-0.30	-0.06	-0.24	-0.37	-0.11	-0.12	-0.26	0.03
Further training	0.38	0.21	0.55	0.45	0.27	0.63	0.42	0.25	0.59	0.52	0.35	0.68	0.07	-0.10	0.24	0.19	-0.04	0.42
Investments in ICT	-0.03	-0.11	0.04	-0.03	-0.10	0.03	-0.05	-0.12	0.01	-0.06	-0.13	0.00	-0.07	-0.15	0.00	-0.11	-0.17	-0.04
Share of FTC	0.02	0.01	0.02	0.02	0.01	0.02	0.01	0.01	0.02	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.00	0.01
100-199	-0.20	-0.41	0.01	-0.20	-0.38	-0.02	-0.36	-0.54	-0.19	-0.24	-0.41	-0.06	-0.17	-0.33	0.00	-0.28	-0.51	-0.05
200-299	0.02	-0.16	0.20	-0.23	-0.41	-0.04	-0.22	-0.41	-0.02	0.03	-0.15	0.21	0.12	-0.03	0.28	0.06	-0.17	0.28
300-499	0.12	-0.07	0.31	-0.05	-0.24	0.13	-0.09	-0.25	0.06	-0.19	-0.34	-0.04	-0.25	-0.40	-0.09	-0.23	-0.49	0.02
500-999	0.41	0.24	0.59	0.15	-0.03	0.32	-0.04	-0.19	0.12	-0.25	-0.40	-0.10	-0.43	-0.58	-0.27	-0.76	-0.99	-0.54
≥ 1000	0.77	0.62	0.92	0.51	0.35	0.67	0.30	0.16	0.45	-0.04	-0.18	0.10	-0.36	-0.50	-0.23	-0.85	-1.07	-0.64
Sector	*Yes*			*Yes*			*Yes*			*Yes*			*Yes*			*Yes*		
Individual characteristics																		
Secondary school	-0.45	-0.57	-0.34	-0.39	-0.48	-0.30	-0.38	-0.46	-0.29	-0.36	-0.43	-0.29	-0.26	-0.35	-0.18	-0.13	-0.21	-0.04
A-levels	-1.08	-1.23	-0.92	-1.14	-1.26	-1.02	-1.05	-1.19	-0.90	-1.02	-1.15	-0.89	-0.91	-1.05	-0.76	-0.87	-1.00	-0.74
Voc. training and A-levels	-0.45	-0.57	-0.32	-0.43	-0.54	-0.32	-0.45	-0.55	-0.35	-0.46	-0.55	-0.36	-0.35	-0.45	-0.25	-0.28	-0.39	-0.16
Universitv	-0.16	-0.25	-0.08	-0.18	-0.24	-0.11	-0.22	-0.29	-0.14	-0.14	-0.21	-0.07	-0.06	-0.13	0.01	0.04	-0.05	0.14
Blue collar skilled	0.28	0.15	0.41	0.26	0.18	0.33	0.17	0.10	0.24	0.19	0.11	0.27	0.25	0.16	0.34	0.32	0.21	0.42
White collar	0.38	0.25	0.51	0.16	0.06	0.25	0.19	0.10	0.28	0.24	0.14	0.34	0.33	0.22	0.44	0.21	0.09	0.33
Master craftsman	0.09	-0.19	0.36	-0.17	-0.50	0.17	0.02	-0.21	0.25	0.50	-0.04	1.04	0.75	0.40	1.11	0.49	0.07	0.91

Table A2 continued…

Part-time	0.59	0.44	0.75	0.49	0.37	0.62	0.51	0.38	0.64	0.58	0.45	0.71	0.65	0.52	0.77	0.56	0.42	0.70
Age 30-34	0.06	-0.02	0.14	0.08	0.03	0.14	0.12	0.06	0.18	0.13	0.08	0.19	0.09	0.03	0.15	0.10	0.03	0.16
Age 35-39	0.18	0.09	0.28	0.25	0.18	0.31	0.25	0.18	0.32	0.26	0.19	0.32	0.19	0.12	0.26	0.18	0.11	0.26
Age 40-44	0.27	0.17	0.37	0.31	0.24	0.38	0.34	0.26	0.41	0.30	0.22	0.38	0.27	0.18	0.36	0.25	0.15	0.36
Age 45-52	0.20	0.09	0.31	0.25	0.17	0.33	0.21	0.12	0.29	0.23	0.15	0.31	0.19	0.11	0.28	0.17	0.05	0.29
Occupation	*Yes*			*Yes*									*Yes*			*Yes*		
Origin state																		
Unemployment	-0.58	-0.66	-0.50	-0.59	-0.65	-0.52	-0.59	-0.64	-0.53	-0.55	-0.60	-0.49	-0.45	-0.52	-0.39	-0.40	-0.46	-0.34
Non-empl. $\leq$ 1 year	-0.84	-1.05	-0.63	-0.87	-1.02	-0.71	-0.88	-1.01	-0.74	-0.74	-0.87	-0.61	-0.65	-0.78	-0.52	-0.57	-0.70	-0.44
Non-empl. > 1 year	-0.74	-0.86	-0.63	-0.64	-0.72	-0.56	-0.64	-0.72	-0.56	-0.54	-0.62	-0.46	-0.48	-0.56	-0.40	-0.37	-0.46	-0.29
First employment	-0.41	-0.57	-0.24	-0.44	-0.53	-0.34	-0.45	-0.56	-0.34	-0.40	-0.52	-0.28	-0.28	-0.40	-0.15	-0.12	-0.26	0.01
Recall	-1.25	-1.43	-1.06	-1.12	-1.28	-0.97	-1.14	-1.28	-1.00	-1.01	-1.15	-0.88	-0.93	-1.07	-0.79	-0.81	-0.95	-0.66
Others																		
Year 1996	0.16	0.10	0.22	0.11	0.07	0.15	0.06	0.01	0.11	0.03	-0.01	0.08	-0.01	-0.06	0.04	0.01	-0.05	0.06
Constant	4.28	3.83	4.72	4.85	4.50	5.19	5.06	4.73	5.40	4.73	4.39	5.08	5.58	5.24	5.93	6.05	5.53	6.57
States	*Yes*			*Yes*									*Yes*			*Yes*		
Min. sum of deviations	15222.76			22131.38			25002.75			24497.27			20806.83			16525.02		
# of observations	31504			29807			26805			23385			19321			16129		
Convergence	Yes			No			No			No			No			No		

Note: Confidence intervals on the 5 %-level are shown in the second and third columns.

Table A3: Estimation results without wages

	Quantile 0.1			Quantile 0.2			Quantile 0.3			Quantile 0.4			Quantile 0.5			Quantile 0.6		
	Coef	low	up	Coef	low	up	Coef	low	up	Coef	low	up	Coef	low	up	Coef	low	up
Firm characteristics																		
Works council	0.37	0.21	0.53	0.39	0.23	0.55	0.55	0.42	0.67	0.59	0.47	0.71	0.79	0.65	0.93	1.03	0.89	1.18
Sector coll. agreement	-0.14	-0.29	0.00	-0.06	-0.18	0.05	0.03	-0.08	0.13	0.09	-0.02	0.20	-0.03	-0.15	0.09	-0.09	-0.21	0.03
Firm collective agreement	-0.26	-0.44	-0.08	-0.23	-0.37	-0.09	-0.25	-0.38	-0.13	-0.20	-0.33	-0.07	-0.45	-0.59	-0.30	-0.25	-0.41	-0.09
Further training	0.31	0.10	0.51	0.42	0.25	0.60	0.39	0.22	0.57	0.53	0.37	0.70	0.11	-0.09	0.31	0.18	-0.04	0.40
Investments in ICT	-0.06	-0.15	0.03	-0.02	-0.09	0.06	-0.05	-0.12	0.01	-0.03	-0.10	0.04	-0.13	-0.21	-0.05	-0.17	-0.25	-0.09
Share of FTC	0.01	0.01	0.02	0.01	0.01	0.02	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.00	0.01	0.00	0.00	0.00
100-199	-0.17	-0.40	0.06	-0.26	-0.44	-0.07	-0.35	-0.54	-0.17	-0.33	-0.50	-0.17	-0.41	-0.58	-0.23	-0.35	-0.55	-0.16
200-299	-0.01	-0.23	0.20	-0.20	-0.40	-0.01	-0.14	-0.32	0.04	0.06	-0.12	0.24	0.13	-0.03	0.29	0.10	-0.09	0.29
300-499	0.12	-0.08	0.32	-0.05	-0.25	0.15	-0.01	-0.16	0.15	-0.06	-0.22	0.10	-0.44	-0.60	-0.27	-0.26	-0.47	-0.05
500-999	0.42	0.23	0.61	0.17	-0.01	0.35	0.00	-0.16	0.15	-0.31	-0.45	-0.17	-0.73	-0.88	-0.58	-0.89	-1.08	-0.70
≥ 1000	0.86	0.68	1.05	0.59	0.42	0.76	0.42	0.27	0.56	0.04	-0.09	0.17	-0.55	-0.68	-0.41	-0.95	-1.11	-0.78
Sector	*Yes*			*Yes*			*Yes*			*Yes*			*Yes*			*Yes*		
Individual characteristics																		
Secondary school	-0.59	-0.70	-0.48	-0.50	-0.60	-0.40	-0.51	-0.60	-0.43	-0.41	-0.49	-0.33	-0.35	-0.43	-0.26	-0.16	-0.25	-0.07
A-levels	-1.18	-1.32	-1.03	-1.24	-1.38	-1.11	-1.22	-1.35	-1.09	-1.22	-1.34	-1.10	-1.06	-1.19	-0.92	-0.84	-0.98	-0.69
Voc. training and A-levels	-0.32	-0.45	-0.20	-0.35	-0.47	-0.23	-0.36	-0.47	-0.26	-0.36	-0.45	-0.26	-0.26	-0.39	-0.13	-0.20	-0.32	-0.08
University	-0.01	-0.09	0.07	-0.04	-0.10	0.03	-0.06	-0.13	0.01	-0.01	-0.08	0.05	0.08	0.00	0.16	0.15	0.07	0.24
Blue collar skilled	0.40	0.27	0.52	0.43	0.33	0.52	0.29	0.21	0.38	0.30	0.23	0.38	0.49	0.39	0.60	0.50	0.39	0.61
White collar	0.67	0.55	0.80	0.64	0.54	0.73	0.60	0.50	0.69	0.52	0.42	0.61	0.67	0.56	0.77	0.52	0.41	0.64
Master craftsman	0.47	0.17	0.76	0.40	0.20	0.60	0.46	0.11	0.82	0.85	0.45	1.26	1.29	0.96	1.62	0.76	0.26	1.25

Table A3 continued…

Part-time	0.29	0.12	0.46	0.21	0.08	0.34	0.14	0.01	0.27	0.24	0.11	0.37	0.33	0.21	0.45	0.17	0.04	0.30
Age 30-34	0.17	0.10	0.25	0.17	0.10	0.24	0.17	0.11	0.23	0.19	0.12	0.25	0.14	0.08	0.20	0.11	0.05	0.18
Age 35-39	0.35	0.25	0.44	0.38	0.31	0.46	0.34	0.26	0.41	0.33	0.26	0.41	0.25	0.18	0.33	0.20	0.13	0.27
Age 40-44	0.45	0.35	0.55	0.49	0.41	0.58	0.44	0.35	0.52	0.38	0.30	0.46	0.31	0.23	0.40	0.23	0.14	0.32
Age 45-52	0.42	0.31	0.52	0.40	0.31	0.49	0.34	0.26	0.42	0.25	0.17	0.32	0.25	0.17	0.34	0.18	0.08	0.28
Occupation	*Yes*			*Yes*									*Yes*			*Yes*		
Origin state																		
Unemployment	-0.73	-0.82	-0.65	-0.83	-0.91	-0.76	-0.85	-0.91	-0.78	-0.72	-0.78	-0.66	-0.56	-0.62	-0.49	-0.45	-0.53	-0.38
Non-empl. $\leq$ 1 year	-1.00	-1.17	-0.83	-1.07	-1.24	-0.91	-1.05	-1.20	-0.90	-0.89	-1.03	-0.75	-0.70	-0.87	-0.53	-0.56	-0.72	-0.40
Non-empl. > 1 year	-0.84	-0.95	-0.72	-0.93	-1.03	-0.83	-0.88	-0.97	-0.79	-0.70	-0.79	-0.62	-0.51	-0.59	-0.43	-0.39	-0.47	-0.31
First employment	-0.59	-0.73	-0.45	-0.69	-0.81	-0.57	-0.68	-0.79	-0.57	-0.62	-0.74	-0.51	-0.38	-0.49	-0.26	-0.16	-0.28	-0.04
Recall	-1.19	-1.34	-1.04	-1.32	-1.47	-1.17	-1.31	-1.47	-1.16	-1.22	-1.35	-1.09	-1.02	-1.16	-0.87	-0.85	-1.01	-0.69
Others																		
Year 1996	0.26	0.20	0.32	0.23	0.19	0.28	0.22	0.17	0.27	0.18	0.14	0.23	0.16	0.12	0.21	0.13	0.08	0.18
Constant	3.20	2.79	3.61	3.82	3.43	4.20	4.10	3.72	4.48	4.33	3.96	4.69	4.41	4.00	4.81	4.06	3.62	4.49
States	*Yes*			*Yes*									*Yes*			*Yes*		
Min. sum of deviations		15739			22983			25998			25348			20553			15905	
# of observations		31586			29750			27003			23763			19039			15698	
Convergence		Yes			No			No			No			No			No	

Note: Confidence intervals on the 5%-level are shown in the second and third columns.