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Wage and Tenure Functions: Evidence from  
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## **ABSTRACT**

### **Unobserved Individual and Firm Heterogeneity in Wage and Tenure Functions: Evidence from German Linked Employer-Employee Data**

We estimate wage and job tenure functions that include individual and firm effects capturing time-invariant unobserved worker and firm heterogeneity using German linked employer-employee data (LIAB data set). We find that both types of heterogeneity are correlated to the observed characteristics and that it is therefore warranted to include individual and firm fixed effects in both the wage and the job tenure equation. We look into the correlation of the unobserved heterogeneity components with each other. We find that high-wage workers tend to be low-tenure workers, i.e. higher unobserved ability seems to be associated with higher job mobility. At firm level, there seems to be a trade-off between wages and job stability: High-wage firms tend to be low-tenure firms, which suggests that low job stability may be compensated by higher wages. High-wage workers seem to sort into low-wage/high-tenure firms. They seem to forgo some of their earnings potential in favour of higher job stability.

JEL Classification: C23, J31, J62, J63

Keywords: linked employer-employee data, unobserved worker and firm heterogeneity, tenure, wages

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# 1 Introduction

This paper is concerned with two key features of the employment relationship: pay and job stability. We estimate the determinants of wages and job tenure by taking into account unobserved individual and firm heterogeneity using linked employer-employee data. Our main interest in doing this is to analyze how the effects of unobserved individual and firm characteristics from the two equations interact.

With the availability of linked employer-employee data sets many researchers have investigated individual and firm effects in wage equations. In these studies the puzzle has emerged that there seems to be negative assortative matching, i.e. the individual and the firm wage effects seem to be negatively correlated. We argue that if the scope of the analysis is broadened by taking job stability into account, the negative correlation between worker and firm wage effect is not a puzzle any more, but it can be explained by the interaction between wages and job stability. Our main argument is that firms who provide stable jobs can pay lower wages and that high-wage workers, due to their higher earnings capacity, sort into these jobs. We therefore provide a new dimension in the discussion about the reasons of a negative correlation between individual and firm wage effects.

A further novelty of the paper lies in estimating tenure functions that control for unobserved worker and firm heterogeneity and by analyzing the association of individual and firm effects from tenure equations with those of wage equations. To our knowledge, individual and firm effects have not yet been simultaneously estimated in a job mobility equation.

We extend the research that has looked into whether high-wage workers are employed in high-wage firms by asking whether high-wage workers are high-tenure workers, whether high-wage firms are high-tenure firms, and whether high-wage workers work in high-tenure firms etc.<sup>1</sup> When estimating the two equations, we take the endogeneity of wages and tenure into account by formulating and estimating a simultaneous equations model.

The paper proceeds as follows. Section 2 develops expectations from a theoretical perspective about the association of individual and firm effects from wage and tenure

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<sup>1</sup>Abowd, Kramarz and Roux (2006) estimate simultaneous wage and mobility equations. While taking into account time-invariant unobserved person and firm heterogeneity in the wage equation, the mobility equation takes into account firm-specific effects but does not include person effects.

functions. Section 3 refers to related literature. The data set is described in section 4 and the model and estimation problems are discussed in section 5. Empirical results follow in section 6 and section 7 concludes.

## 2 Theoretical background

In this section we develop hypotheses about the interdependence of individual and firm effects in wage and tenure functions. The effects are those components of wages and tenure that are due to unobserved time-invariant individual and firm characteristics.

Individual and firm wage effects are often interpreted as reflecting person-specific and firm-specific productivity (Abowd et al. 2004) that is due to unobserved characteristics. These unobserved characteristics can be personal abilities and character traits for individuals and the quality of management or efficiency of production for firms. The firm-specific wage effect does not in the first place have to reflect productivity, but it may also reflect the firm's wage policy with respect to considerations of efficiency wages, implicit contracts or compensating wage differentials. The notion of efficiency wages suggests that a firm may decide to pay high wages as a motivation device (Akerlof 1982). Implicit contracts can include risk-sharing arrangements, according to which the employer pays a stable wage and insures the worker against market fluctuations (Malcomson 1999). The stable wage will in general be lower than the average spot market wage, reflecting that the worker pays an insurance premium. The theory of compensating wage differentials (Rosen 1986) states that in a competitive labor market undesirable job characteristics are compensated by higher wages. Firm wage differentials measured by the firm wage effect may therefore be due to differences of the firms according to their needs to motivate the workforce, insure the workforce against wage fluctuations or compensate the workforce for adverse working conditions.

In the discussion we will refer to "high-wage workers" and "high-wage firms", whereby we mean workers and firms with a high person and firm effect estimated from the wage equation, i.e. workers and firms with unobserved time-invariant characteristics that lead to higher wages.

The individual tenure effects can capture preferences for job stability or outside opportunities with different employers.

Firm tenure effects may capture the firm-specific need of fluctuation and adaptation of the skill-composition of their work-force or the cost of fluctuation.

In the following, when referring to "high-tenure workers" and "high-tenure firms" we mean workers and firms with a high person and firm effect estimated from the tenure equation, i.e. workers and firms with unobserved time-invariant characteristics that lead to higher tenure.

Based on these arguments on the meaning of individual and firm effects in wage and tenure functions we now discuss the interdependence of these effects.

## **2.1 Individual wage and individual tenure effect**

As discussed above, we take the individual wage effect as reflecting unobserved personal abilities. Asking how the individual wage and the individual tenure effect interact therefore means asking how individual abilities are related to the individual propensity of job stability and mobility. It is likely that the effect of unobserved abilities on job stability is similar to the effect of observed abilities, i.e. the level of education. On the one hand, more educated workers may be more mobile with respect to voluntary job mobility. They face a larger job market and search more efficiently and therefore receive more job offers (Mincer 1988). Sicherman (1990) argues that higher education also involves higher rates of planned mobility in the process of building an optimal career path. He finds evidence that the careers of better educated workers involve more movements within and across firms to higher positions. On the other hand, higher educated workers face lower risks of involuntary mobility.

Like education, unobserved abilities may also increase voluntary job mobility but decrease involuntary mobility, so that the overall association of unobserved ability with job mobility is a priori not clear. It is finally an empirical question whether high-ability workers are more or less mobile.

## **2.2 Firm wage and firm tenure effect**

The theory of compensating wage differentials implies that a firm that offers bad working conditions has to pay higher wages. One working condition that ranks as one of the the most important from workers' point of view is job security (Clark 2004). If workers in insecure jobs are compensated by higher wages, we would expect a negative correlation between the firm wage and firm tenure effect.

The opposite hypothesis can be expected if compensating differentials are not present and instead the labor market is segmented. Some firms may provide internal labor markets in which, at least for part of their work force, wages exceed market wages and the employment relationship is based on internal career paths and a long-term attachment to the firm. Empirical evidence on job stability and internal labor markets suggests that certain groups of workers benefit from stable employment relationships, while others are exposed to instable employment (Siebert and Addison 1991, Neumark 2000). Such existence of a segment with good jobs with high wages and job security, and another segment with bad jobs, i.e. with low wages and insecure jobs, would entail a positive correlation between the firm wage and the firm tenure effect.

## **2.3 Individual wage and firm tenure effect**

According to the argument of compensating differentials, firms with high fluctuation offer high wages and firms with more stable employment relationships offer lower wages. This implies that workers can buy job stability by accepting a lower wage. It seems straightforward that those workers who have a higher earnings potential are more willing to forgo some earnings in favor of job stability. From this point of view we expect high-wage workers to sort into high-tenure firms and hence a positive correlation between individual wage effect and firm tenure effect.

If the situation in the labor market is not described adequately by compensating differentials but by segmentation, then we expect high ability workers to be more likely to be employed in the good segment, where internal labor markets may provide stable jobs and high wages. The individual wage effect would then be positively correlated to the firm tenure effect. However, it seems that in the last years internal

labor markets became less important. The literature discusses a trend toward spot markets for labor (Levine et al. 2002). Gibbs and Hendricks (2004, p. 88), after analyzing personnel data from a large corporation, argue that formal salary systems act largely as a veil for the external labor market, imposing no real economic costs on this firm.

## 2.4 Individual wage and firm wage effect

In the literature, the person wage effect and the firm wage effect are understood as measures for the productivity of persons and firms. With this understanding, one would expect positive assortative matching. For example, when workers and firms are heterogeneous in their productive capacity, the assignment model of Becker (1973) implies positive assortative matching between workers and firms. Abowd et al. (2004) derive a model in which the productive capacities of workers and firms lead to separate linear individual and firm effects in a log wage equation. Positive assortative matching can then be tested empirically by computing the correlation of individual and firm effects measured from a log wage equation. A positive correlation is compatible with positive assortative matching.

However, at least two possible explanations for negative assortative matching have been proposed (Barth and Dale-Olsen 2003). First, it may be that the firm effect captures the productivity effects of the capital endowment of firms if adequate control variables for capital are not available. If a very high capital productivity (e.g. high-quality machines) and a very high worker productivity (i.e. workers with high ability) are substitutes, then one would expect to observe a negative correlation. Second, if both parties cannot distinguish between the part of the productivity of a match that arises from the individual and from the firm, then only the sum of the two components matters for the search behavior. Once in a good paying match, an individual will reduce her search effort. More productive workers are likely to be in a good-paying job earlier and to stop searching earlier. Low productivity workers will search longer and therefore end up more likely in high-productivity firms (Barth and Dale-Olsen 2003, p. 10).



More elaborate models of labour markets with frictions (see Shimer 2005, Shimer and Smith 2000 and Postel-Vinay and Robin 2002) have different implications for positive or negative assortative matching depending on the assumptions.

We propose an additional explanation for a negative correlation between worker and firm fixed effects. This is based on understanding the firm wage effect not as an indicator of high productivity but as the price the firm has to pay to its workers because it does not offer stable employment relationships. Low-tenure firms must pay higher wages, high-tenure firms can pay lower wages. High-wage workers are likely to accept lower wages in favor of a higher firm tenure effect. From this perspective we expect a negative correlation between the individual wage and the firm wage effect. By including job stability into the discussion, we offer an explanation for negative assortative matching with respect to firm and person wage effects.

We have no independent theoretical argument for the association of the individual tenure effect with the firm wage and tenure effects. We consider these associations as consequences in the sense that once the correlations discussed in sections 2.1 - 2.4 are known, the sign of the remaining associations should follow logically. Analyzing the signs and significances of the remaining associations is a good test to whether there is an inner logic of our argument and whether the empirical results are consistent.

### **3 Related literature**

Several studies have estimated individual and firm effects in wage equations. In "High wage workers and high wage firms" Abowd, Kramarz and Margolis (1999) show that both person and firm effects are important, but that person effects are more important in explaining the variance in wages, as well as in explaining the firm size effect and the industry effect on wages. One key finding of Abowd, Kramarz and Margolis (1999) is that the correlation between individual and firm effects on wages is positive but not large (between 0.08 and 0.14), i.e. they find weak evidence for positive assortative matching. However, in that paper the authors use an

approximative method, the reliability of which they test in a later paper (Abowd, Creecy, Kramarz 2002). In that later paper they compute the exact least squares solution to the problem and find the importance of person effects confirmed, but the correlation of person and firm effects turns into a negative correlation.

Subsequent studies that have computed individual and firm effects in wage equations find negative correlations between the two effects (Andrews et al. 2006b, Alda 2006, Barth and Dale-Olsen 2003, Grütter and Lalive 2004, Goux and Maurain 1999). This has spurred a debate on economic (Abowd et al. 2004) and statistical (Andrews et al. 2006b) explanations for that (apparent) negative assortative matching. We are aware of studies that take into account individual heterogeneity in quit and separations equations using models of binary choice (Anderson and Meyer 1994, Frederiksen 2004) but not of studies that estimate individual fixed effects in job tenure equations. Unobserved firm heterogeneity in tenure and job duration models is taken into account in Mumford and Smith (2004) and Gerlach and Stephan (2005). These studies use linked employer-employee data, but the data sets are not constructed as panels, i.e. they do not allow for the estimation of individual effects alongside with the firm effects. Furthermore, again due to data restrictions, the studies are focused on elapsed tenure as a dependent variable, whereas the more appropriate measure is completed tenure if job stability is to be investigated. From the weak correlation of firm effects with observable person characteristics, Mumford and Smith (2004) conclude that there is no evidence that long-tenure workers sort into long-tenure workplaces.

Grotheer et al. (2004) analyse the determinants of job stability in German linked employer-employee data by estimating job duration models and taking into account the competing risks of unemployment versus job-to-job change. However, they do not control for unobserved heterogeneity. Boockmann and Steffes (2005) estimate job duration models taking into account competing risks in German linked employer-employee data. They control for unobserved firm heterogeneity but not for unobserved worker heterogeneity.

Abowd, Kramarz and Roux (2006) estimate simultaneous wage and mobility equations. They find that in 30-40% of the firms in their sample the individual effect from a wage equation reduces the probability of separations, while in 10% of the firms it increases separations. Other interesting findings include a negative correlation of

the intercepts from firm-specific wage and mobility functions (“high-wage firms are low-mobility firms”), and the finding that large firms tend to hire workers with low individual wage effects. While taking into account unobserved time-invariant person and firm heterogeneity in the wage equation, the mobility equation only includes time-invariant unobserved firm heterogeneity.

To date, there seems to be no investigation into the determinants of job tenure or job mobility that controls for both, unobserved individual and firm heterogeneity with respect to mobility. However, omitting one or the other is likely to produce biased estimates (Abowd, Kramarz and Margolis 1999).

Whether job insecurity is compensated by higher wages has been analyzed by studies that look into the existence of compensating wage differentials. In his overview on earlier studies of compensating wage differentials in the US, Brown (1980) concludes that the evidence in favor of compensating wage differentials is very limited. Among the studies he reviews, there are only two studies which take job insecurity into account (Taubman 1975 and Duncan 1976). Both studies find evidence in favor of higher wages in insecure jobs. In a study for Britain, McNabb (1989) also finds evidence in favor of wage differentials compensating for a lack of job security. Villanueva (2005), using German panel data, finds positive associations of wages with a number of job disamenities, however for job insecurity there seems to be no wage premium. Hübler and Hübler (2006) use German and British household data and find job insecurity (objective and subjective) to be negatively related to wages. The opposite result is found by Bonhomme and Jolivet (2006), who use European household panel data and estimate a large marginal willingness to pay for job security.

Our study differs from most of these studies in that we look into job stability at firm level, holding unobserved time-invariant heterogeneity of workers and firms constant. It is important to hold unobserved heterogeneity constant, because workers with a higher income potential are likely to buy job stability, which may ultimately lead to job security being related to higher wages and hide the actual trade-off.

## 4 Data

We use the West German sample of the first version of the longitudinal model of the German linked employer-employee data set LIAB provided by the Institute for Employment Research (IAB). Alda et al. 2005 give an overview of the LIAB data set. This LEE data set links the survey data of the IAB establishment panel to employee registry data from the employment and unemployment benefit registry of the German employment service. The individual characteristics that are provided by the registry data are few in comparison to the ample firm characteristics provided by the IAB establishment panel. A variable list and descriptive statistics of the variables we include into the analysis are provided in table 7 in the appendix.

In order to describe the data set design it is useful to define three types of firms. IAB firms are those that are covered by the IAB establishment panel survey, which started in 1993. A sub-group of these, namely all firms that have been part of the IAB establishment panel survey continuously at least from 1999-2001 or from 2000-2002 may be called LIAB firms. Thirdly, there are non-IAB firms, which are those that are not covered by the IAB establishment panel.

The employee side of the LIAB data version we use includes all persons that have been employed in any of the LIAB firms at least one day between 1996 and 2001. For all of those employees the complete set of employment and unemployment benefit spells between 1990 and 2002 is provided, regardless in which type of firm employment took place. This implies that there are many spells in non-IAB firms, i.e. spells for which apart from a firm identifier, no firm characteristics are available. It also follows from this design that, while for the LIAB firms between 1996 and 2001 practically the whole workforce is in the data set, for all non-LIAB firms there are typically only a few employees represented in the data. We therefore restrict our analysis to the set of LIAB firms.

As period of observation we chose 1996 to 2002 because for LIAB firms observed before 1996 it is not assured that the whole set of employees is in the data.

Using a sub-sample of firms reduces the chances to observe a worker who changes job in his new firm. The extent of observed worker mobility between firms is therefore limited. Out of about 446,000 workers we observe about 2,850 workers in more than one firm ("movers").

The employee data comes in the format of employment and unemployment spell data. In this analysis we exploit only the employment spells. Each firm in Germany has to notify the social security authorities at least once every year (typically at the end of the year) about who is employed and how much each employee has earned on average since the last notification. The earnings information, however, is reported only up to the social security contribution threshold and we therefore have right-censoring of the earnings variable. Apart from the wage, firms also report the education, sex, age, job position, profession, full-time status and other information of each worker (see variable list in table 7 in the appendix). A typical employment spell of a continuously employed person refers to the time period from the 1st of January to the 31st of December of a given year. In the case that a firm gives notifications during the calendar year separate spells are generated. There are several reasons for notifications, some of which are of more interest for our analysis (begin, end, interruption of employment etc.) and some of less interest (change of the health insurance company etc.). Some interesting events (change in the wage paid) give not rise to an extra notification, but they show up eventually in the next notification. The tenure variable has to be constructed from the longitudinal employment information that is available for each employee back to 1990. Employment relationships that began before 1990 are coded in the data as beginning on the 1st of January 1990. Likewise, for employment relationships that continue after 2002 we do not observe the end date. Consequently, we have left-censored and right-censored employment spells in the data.

We define separations (terminations of employment relationships) by (i) interruptions of employment with the present employer of longer than 30 days (i.e. the separation is followed by a recall), (ii) changes of the employer identifier (i.e. the separation is followed by a job-to-job move) and (iii) no subsequent employment spell recorded (i.e. the separation is followed by unemployment or inactivity). It follows from our definition of a separation that a worker can have several employment spells with the same employer. About 13% of workers in the data set have experienced a recall<sup>2</sup>.

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<sup>2</sup>Evidence of Mavromaras und Rudolph (1995) based on the same underlying data source, albeit for the time period before 1990, shows that 12% of all newly started employment relationships in Germany are recalls. According to their findings, recalls occur mostly in sectors with seasonal

The firm data has an annual panel format and the survey is recorded around the 30th of June of each year. For all employee spells that begin before the 30th of June of a given year we match the firm data from that year's survey. For the spells beginning after the 30th of June we match the firm data of the following year's survey. We configure two versions of the data set. The first version keeps several observations per employer-employee match and therefore captures the variation of time-varying characteristics during the match. There is at least one observation per year, but if there are several notifications during a year with economically interesting changes, then we keep these as different observations. We could establish an annual panel by choosing as the annual observation the spell that refers to the 30th June. But by doing this we would lose variation in the data and we would lose short employer-employee matches that last for periods of less than a year if they do not extend up to the 30th of June of a given year. We cannot afford to lose employer-employee matches because they may concern movers and therefore contribute to the identification of firm effects. Therefore we keep the single spells as the level of observation. Because we now have several observations per employment relationship, the tenure variable in this version of the data set is elapsed tenure.

The second version of the data set is one where we only keep one observation per employment relationship. The tenure variable in this case captures completed tenure. In order to retain one observation per employment relationship we retain either the value of a variable at the end of the employment relationship (tenure, wage, age), its mean value (firm size, export share, sum of investments, business growth) or its mode (most categorial variables). We replace some indicator variables by an indicator that captures whether the variable has been affirmative at least once during the employment relationship (we do so for indicators with respect to investment into IT, use of part-time work and use of fixed-term contracts). The incidence of recalls mentioned above implies that workers can have several employment relationships with the same employer, i.e. also "stayers" (those who are observed only at one employer) can have several observations of completed tenure<sup>3</sup>.

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fluctuations, and are more frequent for blue collar workers in the case of men and part-time workers in the case of women.

<sup>3</sup>When estimating wage equations it is doubtful whether one should start to count tenure from zero after a recall, because a worker is unlikely to lose all firm-specific human capital during the interruption. But when estimating job stability it seems justified to count recalls as separate

We estimate wage and tenure equations in both versions of the data set. Version one of the data set is our preferred sample for the wage equation, because there is more variability in the data, and version two for tenure equations, because the variable of interest is completed tenure.

In order to take possible biases in the estimation of tenure into account, we introduce further sample restrictions. Due to the design of the data set we worry about left- and right-censored employment relationships. We eliminate left-censored spells by restricting the estimations in both versions of the data set to employment relationships that began after the 1st of January 1990<sup>4</sup>.

The remaining right-censored employment spells cause interruption bias (Salant 1977), i.e. we observe elapsed tenure instead of completed tenure. In version one of the data set we deliberately estimate elapsed tenure and therefore do not adjust for this bias<sup>5</sup>. In version two, however, we principally observe completed tenure. Only those employment relationships that are ongoing after 2002 are right-censored. In order to minimize this problem we restrict estimations in version two of the data set to all non-censored employment relationships plus all right-censored employment relationships of workers older than 55 years. We argue that for this age group, elapsed tenure is very close to completed tenure.

We restrict our sample to full-time workers, because there is no information on the hours worked in the data set and wages of part-timers are therefore not comparable between workers, and for part-timers the information on the job position (blue-collar / white-collar) is missing. We restrict the minimum age to 16 and base the analysis on employees in regular employment defined as employment subject to social security contributions. With respect to missing values in the person and firm characteristics 

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employment relationships because employment relationships that are characterised by recalls are instable employment relationships. They may be so due to firm decisions, i.e. in the case of seasonal workers, or due to employee decisions, i.e. in the case of motherhood leaves.

<sup>4</sup>Due to the data set design, employment relationships that begin between 1990 and 1996 can only be observed if they are ongoing after 1996. Therefore one could argue that we over-sample long job durations. On the other hand, keeping the employment relationships that started between 1990 and 1996 allows us to observe uncensored completed job durations up to 13 years. Basing the analysis only on those employment relationships that started after 1996 would reduce the maximum observed tenure to 7 years.

<sup>5</sup>A number of studies estimate elapsed tenure, e.g. Mumford and Smith (2004) and Gerlach and Stephan (2005).

we base the analysis on the complete cases.

Table 1 gives an overview of the 2 estimation samples. In our preferred sample for the wage equation about 446,000 person effects and 740 firm effects are identified. In the preferred sample for the tenure estimation these are about 250,000 person effects and 550 firm effects.

## 5 Model and Estimation

### 5.1 Non-linearity and fixed effects

We estimate the determinants of wages and tenure alongside with individual and firm effects that may be correlated with the observables.

Concerning the choice of our estimation method we are in a dilemma of choosing between non-linear models and of estimating and predicting individual and firm fixed effects. Some aspects of our estimation problem call for non-linear models. First, as described in the preceding section there is right-censoring of the wage variable which calls for a censored normal regression. Second, in order to analyse job stability with our tenure variable we would preferably want to employ a model of duration analysis with censoring. There are some duration models that can be expressed as linear models in the logarithm of duration and estimated by a linear regression model. Apart from leading to inefficient estimates as compared to the maximum likelihood method on a non-linear model, this procedure does not lend itself readily to take into account the right-censoring of employment spells (Lancaster 1990, pp.219).

The dilemma comes along as we intend to estimate explicitly person and firm fixed effects in a very large linked employer employee data set. Unfortunately, non-linearity poses a problem in this endeavour for two reasons.

First, in many cases fixed effects methods applied to non-linear models lead to inconsistent estimates due to the incidental parameter problem (Neyman and Scott 1948, Greene 2003, pp. 690). In some non-linear models the unobserved heterogeneity term can be eliminated by conditional, marginal or partial likelihood methods. This is the case in the conditional logit model (see for example Greene 2003, p. 698) and in some duration models (Lancaster 1990, pp.263). For censored dependent variables



Honoré (1992, 1993) proposes two estimation methods. Even though these different models do allow to find consistent estimates for the structural parameters, they do not allow to estimate the fixed effects themselves, which is our aim. Furthermore, these methods are developed to take into account one fixed effect and they do not seem to have been extended to include several effects, e.g. person and firm effects. This may in some cases actually be impossible, because even in the linear case there does not seem to be an algebraic transformation that allows to sweep-out and to recover both person and firm effects at the same time<sup>6</sup>.

Second, the maximum likelihood estimator of non-linear models typically does not lead to an explicit set of normal equations, but it is solved through iterative methods. Iterative maximum likelihood estimation has a disadvantage when compared to least squares estimation in very large data sets. With large data sets it can be necessary to decompose the estimation problem. The least squares estimator can be decomposed observation-wise (Ritchie 1995), but this is not possible with the maximum likelihood estimator when it is solved iteratively.

These reasons restrict our analysis to linear models that are estimated by least squares techniques<sup>7</sup>. But at the same time we try to take into account the problems of possible biases of the tenure equations by an appropriate choice of the estimation sample (see previous section)<sup>8</sup>.

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<sup>6</sup>If it was only to sweep-out the heterogeneity one could estimate a fixed effect for each unique worker-firm match and thus only have one effect to control for. However, recovering the person and firm effect from this match effect is not possible (Andrews, Schank, Upward 2006a).

<sup>7</sup>When it comes to the trade-off of taking into account non-linearity versus fixed effects, it may in some cases be more important to take into account fixed-effects. Although in a different context, Ferrer-i-Carbonel and Frijters (2004) find that including fixed-effects affected their results more strongly than taking into account the ordinal character of the dependent variable.

<sup>8</sup>In this paper we ignore the right-censoring of the wage variable. Other options would be dropping the censored cases or imputing the censored values by a censored normal regression. This model is a non-linear model where we would not be able to take into account the unobserved heterogeneity. While the first solution is not optimal because it can lead to sample selection biases, the second is not optimal, because if unobserved heterogeneity is important, the imputation model is biased.

## 5.2 The estimation of person and firm effects

We estimate a fixed effects model of the form

$$y = X\beta + D\theta + F\psi + \epsilon, \quad (1)$$

where the dependent variable  $y$  is either the wage rate or tenure,  $X$  ( $N^* \times K$ ) is the design matrix of time varying characteristics;  $F$  ( $N^* \times J$ ) is the design matrix for the firm effect; and  $D$  ( $N \times N$ ) is the design matrix for the person effect. By estimating the model as a fixed effects model we allow for an arbitrary correlation between the individual and firm effects and the observed time varying characteristics.  $N^*$  is the number of person-years in the dataset,  $J$  is the number of firms,  $N$  is the number of persons, and  $K$  is the number of time varying regressors. The coefficient vector  $\beta$  captures the effects of observed time-varying worker and firm characteristics (including time effects). Our main quantities of interest are the unobserved individual effects  $\theta$  and the unobserved firm effects  $\psi$ .

The assumption under which we estimate the model is that the error term is orthogonal to all regressors, including the individual and firm effects. This implies that the matching of workers to firms does not systematically depend on the shocks incorporated in  $\epsilon$ , i.e. that mobility is exogenous (see Grütter (2006) for a discussion of this assumption and an estimation strategy that relaxes the assumption).

Unfortunately, there seems to be no simple algebraic transformation to sweep out individual and firm effects as for example the within-transformation ("time-demeaning") in the fixed effects model. It is possible, however, to include the firm effects as dummy variables and to sweep-out the person effects by the within-transformation. The within-transformation consists of subtracting the person mean from each observation. This eliminates the person effect. The transformed model may be written as

$$\tilde{y} = \tilde{X}\beta + \tilde{F}\psi + \tilde{\epsilon}. \quad (2)$$

When estimating this mixture of the within-transformation and the dummy variable model we encounter the problem that the design matrix becomes too large to fit the available computer memory (see also Andrews et al. 2006a, Abowd et al. 2002 and Grütter 2006 for methods to estimate person and firm effects in large data sets). The design matrix of the time-demeaned model ( $\tilde{X}, \tilde{F}$ ) has dimension ( $N^* \times (K+J)$ ).

In our largest sample we have approximately  $N^* = 1,500,000$ ,  $K = 50$  and a number of identifiable firm effects of  $J = 750$ . Consequently, the design matrix has 1.2 billion cells ( $1,500,000 \cdot 800$ ). Assuming that 4 bytes of memory are needed to store each cell, the memory requirement amounts to 4.8 GB. However, the cross-product matrix  $(\tilde{X}, \tilde{F})'(\tilde{X}, \tilde{F})$ , which is eventually needed for estimation has only a dimension of  $800 \times 800 \approx 640,000$  and requires memory of below 3 MB<sup>9</sup>. In Cornelißen (2006) it is developed how to construct  $(\tilde{X}, \tilde{F})'(\tilde{X}, \tilde{F})$  without creating the  $F$  part of the design matrix. In our case, using this procedure lowers the memory requirement from 4.8 GB to below 500 MB.

Firm effects are identified through the mobility of workers between firms. Of the 1,904 firms in our largest sample, only 770 firms have "movers", i.e. workers that are observed in more than one firm of the data set. No firm effects can be estimated for the 1,134 firms without movers. The 770 firms with movers are divided into 30 groups of firms, which are defined such that firms within one group are connected by worker mobility, but firms of different groups are not connected by worker mobility (see Abowd, Creedy and Kramarz (2002) for an algorithm to determine the groups). If  $N_g$  is the number of persons in a group, and  $J_g$  the number of firms, then in each group  $N_g - 1$  person effects,  $J_g - 1$  firm effects and a group mean are identified. The  $J_g - 1$  firm effects and  $N_g - 1$  person effects within each group are arbitrary in the sense that they depend on which person and firm in each group serves as the reference. Therefore, person and firm effects should not be compared between different groups. One can normalise person and firm effects within each group by subtracting the mean person and the mean firm effect respectively, so that they sum to zero within each group and represent deviations from the group mean. Even after normalisation it is in our view not straightforward to compare person and firm effects between groups, because it is questionable whether a firm effect of +1 in a group with a very high group mean means the same as a firm effect of +1 in a group with a low group mean. Therefore, we base our subsequent analysis of the person and firm effects only on effects out of the same group. For this purpose we use the largest group which contains the majority of the observations. In our largest sample this group contains 88% of the observations and 701 of the 740 identified

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<sup>9</sup>But the  $X$  part of the design matrix will still require memory. Using the numbers given above, this would be an additional 300 MB

firm effects.

After the estimation of the person and firm effects we can study the correlations of the effects among each other. Under the assumption that individual and firm effects are not correlated with the other regressors, Andrews et al. (2006b) and Abowd et al. (2004) show that  $Corr(\theta, \psi)$  is biased downwards if there is true positive assortative matching. With arbitrary correlation between the unobserved heterogeneity and the other regressors, the sign of the bias cannot be determined a priori, but it is an empirical question. The formulae to compute the exact bias when there is arbitrary correlation between unobserved heterogeneity and observed characteristics require the inversion of an  $N^* \times N^*$  matrix (Andrews et al. 2006b), which is computationally not feasible with the size of our data set. Andrews et al. (2006b) propose the alternative of assuming that the observable regressors are uncorrelated with the unobservable heterogeneity. This assumption does not really fit the framework of a fixed effects estimation where one explicitly allows for such correlation. Therefore we prefer not to compute the bias under this assumption but to exploit a different finding of Andrews et al. (2006b). They show that the bias in the estimation of the correlation decreases if the number of movers increases. We therefore compute the correlations based on a sub-set of all firm effects that are identified by at least 2 or at least 5 movers. Requiring a larger number of movers per firm would reduce our sample too much. We furthermore also use only those individual effects of persons for whom we have at least 2 observations, because person effects of persons who have only one observation are poorly estimated.

### 5.3 Selection of explanatory variables

For a number of explanatory variables we assume that they influence both wages and tenure. First of all, these include the somewhat scarce information on individual characteristics available in our data set. We include age, education, occupational status and profession as explanatory variables for both wages and tenure. We exclude sex and nationality because they are time-constant and cannot be estimated alongside with the individual fixed effects<sup>10</sup>.

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<sup>10</sup>An alternative would be Hausman and Taylor's (1981) estimator, which models the correlation of unobserved effects with observed regressors explicitly and includes random effects that capture

We also include a number of firm characteristics in the wage and tenure equation. These are firm size, business expectations, investments into IT, the investment sum, the use of fixed-term work, the application of collective contracts and the existence of a works council. We expect these regressors to have an effect on both wages and tenure.

Wages and job stability are likely to be determined simultaneously (Abowd and Kang 2002). Wages usually increase with the duration of the employment relationship as specific human capital is accumulated. On the other hand, the wage development is likely to be a determinant of the continuation of the employment relationship. The payment of high wages reduces quits and, if it can be interpreted as the quality of the match or as a measure of productivity, it is also likely to reduce layoffs. Simultaneity biases the estimates if both equations are estimated separately as single equations. Therefore we estimate wages and tenure simultaneously by 2SLS (Wooldridge 2002, eqs. 5.18-5.19, p. 91). We include the individual and firm fixed effects in both stages. At the second stage we adjust the standard errors as described in Wooldridge (2002, eq. 5.25, p.95).

For the simultaneous estimation we need suitable instrumental variables (IVs) which we find using a mixed strategy driven partly by empirical considerations and partly by theoretical considerations.

We identified a broader set of IVs by running separate equations for wages and tenure. Those variables that were not significant in one equation but had a clear influence in the other were chosen as IVs<sup>11</sup>. This resulted in using the regressors firm provides training, firm uses old technology, firm uses overtime, past worker mobility (no. of job changes divided by experience) and firm reorganization as IVs for tenure. As IVs for wages this resulted in the share of male employees, the share of part-

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remaining unobserved factors under the assumption that they are not correlated with the observables. That model allows the inclusion of time constant regressors. We do not use this estimator, because it has a crucial difficulty: it is based on the assumption that we can distinguish regressors that are correlated with the unobserved effects from those that are not.

<sup>11</sup>We recognize that this is not a formal test since if the correct specification is a an IV model, then the single equation (non IV) model is misspecified. But given that there is no formal test of the validity of the instrument, our procedure at least offers a sense of the patterns in the data.

time workers, the share of white collar workers, the mean age of a firm's employees and the firm being situated in South Germany. This broader set of IV variables is used for a first IV specification. The reason for this procedure is that we are in a dilemma to choose the number of instruments. Too few and too many instruments are not optimal. On the one hand we have to search for the largest number of valid instruments following the GMM strategy and Andrews (1999). On the other hand, Donald and Newey (2001) suggest choosing instruments by minimizing MSE. This helps to reduce misleading IV inferences that can occur with too many instruments.

For a second IV specification we retained only one IV for wages and one for tenure. From the broader set of IVs chosen empirically we retained only those that appealed to us most from theoretical grounds. These are past worker mobility as IV for tenure and part-time work as IV for wages. The latter consists of two variables, a dummy which indicates whether part-time work is used, and the share of part-time employees at firm level. While the dummy captures the fixed costs associated with using part-time, the share captures the variable costs.

We define past worker mobility, the IV for tenure, as the number of past job changes divided by labor market experience. In the fixed effects specification, only the within variation of past job mobility identifies its effect on tenure. In other words, individual differences in the average propensity to change jobs are held constant by the individual fixed effect. The within variation of past mobility then marks the life-cycle profile of job mobility, which is likely to move from being more mobile to being more stable. The more past mobility an individual has accumulated, the more likely it has reached a more stable phase. After controlling for individual fixed effects, we therefore expect a positive effect of past mobility on job stability (tenure). We exclude the past mobility variable from the wage equation, because the life-cycle profile of earnings is already accounted for by including age.

The use of part-time work as an IV for wages is justified by the argument that due to fixed labor costs per employee, a firm that employs a higher share of part-time workers is likely to have higher wage costs per employee. These higher costs are likely to be partly shared by full-time employees and generally reduce the wage level of the firm. We expect no direct effect of using part-time work on the job stability of the full-time workers at the firm (recall that only full-time workers are in the

sample).

The choice of explanatory variables in our tenure equation is comparable to the specifications of the job duration models in Grotheer et al. 2004 and Boockmann and Steffes 2005<sup>12</sup>.

## 6 Results

Table 2 reports results of the estimated wage equation in the preferred sample for the wage estimation. The table presents 4 specifications. Model (1) is estimated by pooled OLS, model (2) is a fixed effects estimation including individual and firm effects. Models (3) and (4) are simultaneous equations models, e.g. the endogeneity of tenure is taken into account through IV estimation. Most coefficient estimates of the wage equations are according to expectations. We only mention a few effects that surprised us: Somewhat against our expectations is the steepness of the cubic function of age, reaching its maximum at about 25 years of age<sup>13</sup>. An unexpected result is that in the pooled regressions good business expectations and IT investments seem to reduce wages. However, after controlling for fixed effects these unexpected results vanish.

A further result is that, after controlling for fixed effects, firms with a higher share of part-time work pay lower wages to their full-time staff (only full-time workers are in the sample). Due to considerable fixed-costs per employee, firms that use part-time work have higher wage costs than firms that employ the same labor input only in terms of full-time work. We suspect that some of that cost is shifted onto full-time workers in the form of lower wages.

A further comparison of the pooled wage estimation with the fixed effects wage estimation (first two columns of table 2) reveals that introducing person and firm fixed-effects into the estimations influences all coefficients. Most coefficients change only in magnitude but remain qualitatively the same. Some influences become in-

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<sup>12</sup>The differences are that we do not include part-time work, the local unemployment rate, year of setting-up of the firm and pay above tariff wages in the tenure equations, while they do not include technology, sum of investments, business expectations, overtime use and past mobility. The remaining regressors are similar.

<sup>13</sup>Our sample consists only of "young" employment relationships. Probably the effect of age on wages in these employment relationships is different than the effect of age on wages in general.

significant. This is the case for IT investments, the use of part-time and fixed-term work (but not the share of part-time work), sectoral-level collective contracts, the presence of a works council and the shares of male and white-collar employees. That some influences become insignificant can be expected, because the fixed effects model identifies coefficients by using only the within-person and within-firm variation. This is usually much lower than the overall variation, and it is therefore more difficult to identify statistically significant effects.

Table 3 reports the estimation of completed tenure. As in the preceding table, the four models are pooled OLS, a single equation fixed effects estimation and two simultaneous equations fixed effects estimations. In the latter, the endogeneity of wages is taken into account.

Tenure increases with wages. In the single equations estimates (columns 1 and 2) the age coefficients suggest that job stability as measured by completed tenure reaches a maximum at the age of 22. This contradicts the theory of job shopping (Johnson 1978), according to which young job market entrants first change jobs more often and later become increasingly stable. In the IV estimations (model 3 and 4) the estimates are more in line with the job shopping theory. Job stability reaches a minimum at the age of 27 and rises thereafter.

A further result is that higher education and higher occupational status lead to more mobility. This only shows up in the IV specifications (models 3 and 4) but not in the single equation estimates (models 1 and 2).

In general, including fixed effects (moving from model 1 to model 2) leaves many effects qualitatively unaffected. An exception is the effect of whether the firm trains its work force. While in the pooled regression training seems to destabilize employment, in the fixed effects regression it seems to stabilize employment. However, in the IV estimations the effect is again negative, but also considerably smaller in magnitude and not statistically significant. It seems that after accounting for fixed effects and for the endogeneity of wages training tends to be associated somewhat higher worker mobility. An explanation for this may be that better trained workers have better outside options.



Many firm characteristics have no statistically significant effects in the IV estimates. This reflects the cost of IV estimation in terms of greater variances of the estimates. Effects that stay statistically significant in the IV estimates include IT investments, the investment sum and outsourcing activities, which all have stabilizing effects on employment in these estimates.

In tables 8 and 9 in the appendix the regression results for wages and tenure in the respective non-preferred samples are presented. However, it must be kept in mind that the non-preferred samples are conceptually different from the preferred samples. The non-preferred tenure sample includes elapsed tenure instead of completed tenure and therefore measures job stability less adequately. The non-preferred wage sample includes only wages at the moment of separation. For some regressors, also their value at separation has been retained, while for others the mean value over the whole employment relationship (firm size, export share, sum of investments, business growth), the mode (most categorial variables) or as an indicator variables that captures whether the variable has been affirmative at least once during the employment relationship (investment into IT, use of part-time work and use of fixed-term contracts). Due to these differences, deviations in coefficient estimates between the preferred and the non-preferred samples are to be expected. Nevertheless most coefficients are qualitatively similar in the two samples.

Tables 4 and 5 report the correlations of individual and firm effects estimated from the wage and tenure functions presented above. We report partial correlation coefficients holding the observed characteristics age, sex and nationality constant, because these characteristics are taken up by the person and firm effects<sup>14</sup>. The correlations are based on firms with at least two movers and persons with at least two observations.

In the sample with completed tenure the signs of the correlations are equal across all of the three estimations (table 4). In the sample with elapsed tenure, the results

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<sup>14</sup>Even though age is included as a regressor, its variation between individuals is taken up by the person effect.

are different in the single equation estimation, but in the IV estimations, the signs of the correlation are the same as in the completed tenure estimation (table 5). The signs and the magnitude of the correlations are very similar when we base them on the sample of firms that had at least five movers (results not reported) instead of at least two movers. The results therefore appear quite robust. Whether we chose the IV specification with more or with less instruments does not affect our results. The results can be summarized as follows (the correlations in parentheses are taken from the last column of table 4):

- 1.) High-wage workers are low-tenure workers (-0.65).
- 2.) High-wage firms are low-tenure firms (-0.52).
- 3.) High-wage workers sort into high-tenure firms (0.14).

From these results it follows logically and is confirmed empirically that

- 4.) High-wage workers sort into in low-wage firms (-0.50).
- 5.) High-tenure workers sort into low-tenure firms (-0.23).
- 5.) High-tenure workers sort into high-wage firms (0.21).

(Whereby "high-wage" and "high-tenure" are shortcuts for high wage and tenure effects of unobserved time-invariant heterogeneity.)

The results are consistent with the hypotheses that workers with better unobserved abilities are more mobile workers, that firms that offer unstable employment relationships have to pay higher wages, and that high-wage workers forgo some of their earnings potential by sorting into low-wage firms that offer more stable employment.

Result 2, that low-wage firms are high-tenure firms, appears not to be in line with the findings of Abowd, Kramarz and Roux (2006), who find that low-wage firms are high-mobility firms. However, Abowd, Kramarz and Roux (2006) do not control for individual fixed effects in their mobility equation. Their results are therefore not

comparable to ours. As we show below, if we do not control for unobserved effects and observed effects at the same time, the negative correlation between firm wage and firm tenure effect vanishes. There is therefore no inconsistency between the results.

Result 4 is consistent with the findings of Alda (2006), Andrews et al. (2006b) and Abowd et al. (2002). The fact that the correlation of individual and firm effects from the same equation is likely to be biased (Andrews et al. 2006b, Abowd et al. 2004) casts some doubt on whether the negative correlations in results 4 and 5 are really due to negative assortative matching, or whether they are statistical artefacts.<sup>15</sup> However, the two results fit the overall picture from the cross equation correlations very well, which may give some confidence that results 4 and 5 represent true associations and no statistical artefacts.

In table 6 we show how the correlations between the firm and person effects from wage and tenure equations change with varying degrees of control variables. The first column of the table shows the correlations if we control for both observed and unobserved effects. This is the repetition of column 1 from table 4. Column 2 shows the correlations when we control only for unobserved effects (i.e. we estimate model (1) without  $X$ ). In column 3 we report the correlations from estimations that control only for observed effects (i.e. when estimating the person effects we estimate equation (1) without  $F$ , and when estimating the firm effects we estimate it without  $D$ ). In column 4 we report the correlations without including any controls, which is equivalent to correlating raw person and firm means of wages and tenure. We see that as long as we include no controls or control only for observables, all correlations are positive (columns 3 and 4 of table 6). As soon as we control for unobserved factors, the individual and firm effects from the same equation (first and second line of the table) become negatively correlated. The negative correlations between individual wage and individual tenure effect, as well as between firm wage and firm tenure effect, however, only appear in column 1. They are a result of controlling

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<sup>15</sup>Andrews et al. (2006b) and Abowd et al. (2004) estimate the size of the biases in their estimations under simplifying assumptions and find that the size of the bias is not large enough to turn the negative correlations into positive ones.

for both, observables and unobserved heterogeneity. That may be a reason why previous studies, which have not controlled for unobserved heterogeneity have not found a trade-off between wages and job stability at firm level. This underlines the importance of controlling for unobserved heterogeneity.

## 7 Conclusion

Our aim was to learn about unobserved individual and firm heterogeneity in wage and tenure functions. We have estimated individual and firm effects that capture time-invariant unobserved heterogeneity in wage and tenure equations and looked into the correlation of the unobserved heterogeneity components with each other.

We have circumvented the computer memory restrictions that are often encountered when identifying a large number of individual and firm effects in linked employer-employee data sets by applying a memory-saving way to compute the full least squares solution of the problem. Thus we could estimate all identified firm effects in our sample, which are about 740 firm effects.

Our findings can be summarized as follows: Workers with higher abilities are more mobile. High-tenure firms are low-wage firms. They seem to ask workers to pay an insurance premium in exchange for job stability. High-wage workers sort into high-tenure firms. High-wage workers seem to use their income potential to buy job security. High-tenure and low-wage workers, on the other hand, tend to be matched with the opposite type of firms, i.e. with low-tenure and high-wage firms. (Whereby "high-wage" and "high-tenure" are shortcuts for high wage and tenure effects of unobserved time-invariant heterogeneity.)

It follows from this sorting mechanism that high-wage workers work in low-wage firms. This negative assortative matching, which has also been found in other empirical work, has been regarded as a puzzle. By looking at both, job stability and wages, our explanation for this is that low-wage firms offer job stability, and therefore it is rational for high-wage workers to forgo some of their wage potential by choosing a low-wage firm and thus buying job stability.

One way of refining this research in further work is to construct proxies for voluntary and involuntary mobility and analyze whether unobserved effects differ between the two types of mobility.

# Tables

Table 1: Overview of estimation samples

Sample:	Sample version 1	Sample version 2
Level of observation:	Notification	Employment relationship
Restriction:	Begin of employment relationship after 1st January 1990.	Begin of employment relationship after 1st January 1990 and right-censored employment spells in 2002 of over 55 year-olds only.
Preferred for:	Wage equation	Tenure equation
Observations:	1,532,526	295,196
No. Persons:	445,800	250,548
thereof movers:	2,851	1,423
thereof >1 obs.:	341,652	33,458
No. Firms:	1,904	1,851
thereof with movers	770	594
connected groups	30	45
Identified firm effects	740	549

Table 2: Wage Equations (preferred sample)

	(1)	(2)	(3)	(4)
	Pooled OLS	FE	FE, IV (1)	FE, IV (2)
N	1532526	1532526	1532526	1532526
Tenure	0.023 (217 .4)	0.025 (102 .6)	0.029 (5 .4)	0.034 (5 .8)
Age	0.089 (77 .7)	0.120 (44 .3)	0.117 (28 .6)	0.113 (26 .5)
Age <sup>2</sup> /100	-0.178 (-61 .1)	-0.227 (-32 .9)	-0.230 (-29 .2)	-0.235 (-29 .3)
Age <sup>3</sup> /1000	0.118 (49 .3)	0.142 (25 .2)	0.145 (21 .4)	0.150 (21 .7)
Voc. Training	0.078 (93 .2)	0.077 (16 .1)	0.077 (16 .1)	0.077 (16 .1)
Voc. Training and A-levels	0.123 (70 .5)	0.086 (9 .3)	0.086 (9 .3)	0.087 (9 .5)
University	0.262 (186 .5)	0.171 (18 .9)	0.172 (18 .9)	0.173 (19 .0)
Skilled blue-collar	0.079 (81 .7)	0.013 (4 .2)	0.013 (4 .0)	0.013 (3 .9)
White collar	0.155 (95 .0)	0.081 (17 .6)	0.081 (17 .1)	0.080 (16 .8)
Log firm size	0.013 (42 .0)	0.039 (15 .5)	0.038 (15 .2)	0.043 (16 .3)
Business expectations	-0.009 (-3 .3)	0.010 (3 .8)	0.010 (3 .9)	0.011 (4 .4)
IT investments (dummy)	-0.005 (-5 .0)	0.000 (-0 .3)	0.000 (-0 .1)	0.002 (1 .1)
Investments (in 10 mill. Euros)	0.002 (34 .2)	0.001 (18 .0)	0.001 (17 .5)	0.001 (17 .6)
Firm uses part-time work	-0.005 (-2 .8)	0.000 (-0 .2)	0.000 (-0 .2)	0.000 (0 .1)
Share part-time	0.126 (25 .5)	-0.188 (-13 .4)	-0.188 (-13 .4)	-0.197 (-14 .6)
Firm uses fixed-term work	-0.002 (-2 .4)	0.000 (-0 .2)	0.000 (-0 .4)	0.001 (0 .9)
Sector-level coll. contract	0.018 (11 .2)	0.002 (0 .7)	0.002 (0 .7)	0.001 (0 .5)
Firm-level coll. contract	0.032 (17 .1)	0.006 (2 .1)	0.006 (2 .2)	0.006 (2 .1)
Works council	0.142 (93 .7)	0.002 (0 .5)	0.002 (0 .5)	0.002 (0 .6)
Training			IV for tenure	-0.017 (-1 .6)
Old technology			IV for tenure	0.002 (2 .8)
Overtime			IV for tenure	
Share males	0.397 (149 .3)	-0.004 (-0 .3)	-0.006 (-0 .4)	-0.063 (-4 .6)
Share whitecollar	0.104 (49 .6)	-0.012 (-1 .2)	-0.012 (-1 .2)	
Mean age	-0.002 (-12 .2)	-0.008 (-21 .3)	-0.008 (-17 .9)	
Mobility			IV for tenure	IV for tenure
South Germany	0.005 (7 .1)	0.011 (4 .3)	0.012 (4 .2)	0.018 (6 .0)
Outsourcing			IV for tenure	-0.001 (-0 .7)
Hiving-Off			IV for tenure	-0.002 (-1 .4)
Insourcing			IV for tenure	-0.001 (-1 .0)
Shut-down part of firm			IV for tenure	-0.005 (-2 .6)

Note: Year, sector and profession dummies included. T-values in parentheses. Reference categories are: No vocational training, unskilled blue-collar, no collective contract.

Table 3: Tenure Equations (preferred sample)

	(1)	(2)	(3)	(4)
	Pooled OLS	FE	FE, IV (1)	FE, IV (2)
N	295196	295196	295196	295196
Wage	0.006 (87 .8)	0.003 (19 .0)	0.213 (8 .9)	0.208 (10 .6)
Age	0.462 (64 .0)	0.641 (9 .9)	-3.053 (-5 .2)	-2.945 (-5 .6)
Age <sup>2</sup> /100	-1.043 (-56 .4)	-1.340 (-8 .2)	5.624 (4 .3)	5.410 (4 .5)
Age <sup>3</sup> /1000	0.780 (51 .8)	1.039 (8 .1)	-3.823 (-3 .8)	-3.659 (-4 .0)
Voc. Training	0.123 (19 .9)	0.067 (1 .1)	-0.308 (-0 .8)	-0.297 (-0 .7)
Voc. Training and A-levels	-0.086 (-6 .5)	0.035 (0 .3)	-0.828 (-1 .1)	-0.819 (-1 .1)
University	0.078 (6 .9)	0.150 (1 .1)	-2.472 (-2 .8)	-2.445 (-2 .8)
Skilled blue-collar	0.142 (18 .2)	0.268 (3 .6)	0.181 (0 .4)	0.175 (0 .4)
White collar	0.095 (7 .6)	0.176 (1 .6)	-1.244 (-1 .7)	-1.230 (-1 .7)
Log firm size	0.078 (24 .4)	1.009 (18 .8)	0.515 (1 .5)	0.454 (1 .3)
Business expectations	1.227 (50 .1)	2.837 (25 .9)	-0.334 (-0 .4)	-0.171 (-0 .3)
IT investments (dummy)	0.888 (95 .0)	0.985 (32 .3)	0.484 (2 .4)	0.615 (3 .2)
Investments (in 10 mill. Euros)	0.011 (23 .1)	0.018 (6 .6)	0.024 (1 .4)	0.034 (2 .0)
Firm uses part-time work	0.610 (41 .0)	0.960 (19 .3)	0.755 (2 .4)	IV for wage
Share part-time			IV for wage	IV for wage
Firm uses fixed-term work	0.646 (72 .9)	1.016 (33 .1)	0.247 (1 .2)	0.283 (1 .4)
Sector-level coll. contract	0.047 (3 .9)	0.448 (7 .1)	-0.019 (-0 .1)	-0.047 (-0 .1)
Firm-level coll. contract	-0.038 (-2 .6)	-0.031 (-0 .4)	-0.733 (-1 .5)	-0.486 (-1 .0)
Works council	0.554 (44 .2)	0.499 (6 .1)	-0.278 (-0 .5)	-0.031 (-0 .1)
Training	-1.076 (-20 .8)	3.164 (11 .1)	-0.518 (-0 .3)	-0.707 (-0 .5)
Old technology	-0.032 (-8 .8)	-0.175 (-11 .2)	0.020 (0 .2)	0.005 (0 .1)
Overtime	-4.964 (-90 .9)	-13.761 (-50 .7)	0.675 (0 .3)	
Share males			IV for wage	-0.810 (-0 .4)
Share whitecollar			IV for wage	
Mean age			IV for wage	
Mobility	-29.873 (-95 .4)	-4.792 (-5 .0)	6.207 (1 .0)	6.185 (1 .0)
South Germany			IV for wage	0.649 (1 .2)
Outsourcing	0.534 (75 .1)	0.577 (23 .9)	0.552 (3 .6)	0.564 (3 .7)
Hiving-Off	0.267 (27 .6)	0.340 (9 .7)	-0.077 (-0 .3)	-0.080 (-0 .4)
Insourcing	0.215 (26 .9)	0.570 (19 .9)	0.307 (1 .7)	0.276 (1 .5)
Shut-down part of firm	0.480 (48 .9)	0.398 (12 .7)	0.104 (0 .5)	0.117 (0 .6)

Note: Year, sector and profession dummies included. T-values in parentheses. Reference categories are: No vocational training, unskilled blue-collar, no collective contract.

Table 4: Partial correlation of unobserved effects in dataset version two (holding age, sex, nationality constant)

	FE	FE, IV (1)	FE, IV (2)
IW - FW	-0.44	-0.47	-0.50
IT - FT	-0.59	-0.21	-0.23
IW - IT	-0.10	-0.65	-0.65
FW - FT	-0.17	-0.53	-0.52
IW - FT	0.22	0.12	0.14
IT - FW	0.11	0.20	0.21

IW: individual wage effect  
IT: individual tenure effect  
FW: firm wage effect  
FT: firm tenure effect

Note: Correlations are based on firms with at least two movers (326 firms) and persons with at least two observations.

Table 5: Partial correlation of unobserved effects in dataset version one (holding age, sex, nationality constant)

	FE	FE, IV (1)	FE, IV (2)
IW - FW	-0.51	-0.51	-0.51
IT - FT	-0.69	-0.51	-0.59
IW - IT	0.01	-0.68	-0.55
FW - FT	-0.02	-0.62	-0.50
IW - FT	-0.03	0.27	0.21
IT - FW	0.02	0.29	0.26

IW: individual wage effect  
IT: individual tenure effect  
FW: firm wage effect  
FT: firm tenure effect

Note: Correlations are based on firms with at least two movers (610 firms) and persons with at least two observations.



Table 6: Partial correlation of unobserved effects with varying control variables

	(1)	(2)	(3)	(4)
IW - FW	-0.44	-0.46	0.24	0.32
IT - FT	-0.59	-0.78	0.30	0.38
IW - IT	-0.10	0.07	0.11	0.25
FW - FT	-0.17	0.03	0.34	0.60
IW - FT	0.22	0.06	0.03	0.12
IT - FW	0.11	0.02	0.07	0.22
Unobserved effects included	✓	✓	-	-
Observed regressors included	✓	-	✓	-
IW: individual wage effect			FW: firm wage effect	
IT: individual tenure effect			FT: firm tenure effect	

Note: Partial correlations based on data set version two, FE single equation model, holding age, sex and natinoality constant. Correlations are based on firms with at least two movers (326 firms) and persons with at least two observations.

# Appendix

Table 7: Summary statistics

Variable Name	Remarks	Sample 1		Sample 2	
		Mean	S.d.	Mean	S.d.
<b>Tenure</b>	-	3.88	3.09	2.70	2.87
<b>Log tenure</b>	-	0.83	1.29	0.19	1.52
<b>Wage</b>	Daily wage	92.45	32.15	80.81	42.20
<b>Log wage</b>	-	4.46	0.45	4.23	0.72
<b>Age</b>	-	37.78	9.92	38.21	11.99
<b>Age<sup>2</sup>/100</b>	-	15.26	7.98	16.04	9.90
<b>Age<sup>3</sup>/1000</b>	-	6.55	5.12	7.30	6.56
<b>Voc. Training</b>	Dummy: Individual has completed a vocational training / apprenticeship.	0.61	0.49	0.56	0.50
<b>Voc. Training and A-levels</b>	Dummy: Individual has completed a vocational training and A-levels ("Abitur").	0.04	0.20	0.04	0.21
<b>University</b>	Dummy: Individual has completed a University degree.	0.12	0.33	0.10	0.30
<b>Reference: no vocational training and no A-levels and no University degree</b>					
<b>Skilled blue-collar</b>	Individual is in skilled blue-collar job position	0.23	0.42	0.20	0.40
<b>White collar</b>	Individual is in whitecollar job position	0.41	0.49	0.40	0.49
<b>Reference: unskilled blue collar job position</b>					
<b>Skilled manual</b>	Occupation classification dummies	0.16	0.37	0.14	0.35
<b>Technical</b>	"-"	0.11	0.32	0.09	0.28
<b>Unskilled services</b>	"-"	0.10	0.30	0.11	0.31
<b>Skilled services</b>	"-"	0.02	0.15	0.03	0.16
<b>Semi-Professional</b>	"-"	0.04	0.21	0.05	0.23
<b>Professional</b>	"-"	0.02	0.14	0.02	0.15
<b>Unskilled administrative</b>	"-"	0.05	0.21	0.06	0.23
<b>Skilled administrative</b>	"-"	0.16	0.37	0.15	0.36
<b>Manager</b>	"-"	0.02	0.14	0.02	0.13
<b>Reference: unskilled manual</b>					
<b>Log firm size</b>	Log no. of workers per firm	7.05	1.48	6.83	1.52
<b>Business expectations</b>	Ordinal index of firm's business expectations ranking from -1 to +4.	0.02	0.12	0.01	0.10
<b>IT investments (dummy)</b>	Dummy: firm invested into IT	0.88	0.33	0.90	0.30
<b>Investments (in 10 mill. Euros)</b>	Firm's investment sum	4.09	8.95	3.04	7.00
<b>Firm uses part-time work</b>	Dummy	0.96	0.19	0.97	0.17
<b>Firm uses fixed-term work</b>	Dummy	0.86	0.35	0.88	0.33
<b>Sector-level coll. contract</b>	Dummy: firm covered by sector-level collective bargaining agreement	0.85	0.35	0.85	0.36
<b>Firm-level coll. contract</b>	Dummy: firm covered by firm-level collective bargaining agreement	0.10	0.31	0.10	0.30
<b>Reference: firm not covered by collective bargaining agreement</b>					
<b>Works council</b>	Dummy: Firm has works council	0.94	0.24	0.91	0.29

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Variable Name	Remarks	Sample 1		Sample 2	
		Mean	S.d.	Mean	S.d.
<b>Training</b>	Dummy: Firm provides training to its work force	0.95	0.11	0.93	0.13
<b>Old technology</b>	Ordinal index of firm's technology coded from 1 (state of the art) to 5 (outdated)	1.98	0.70	1.99	0.69
<b>Overtime</b>	Dummy: Firm uses overtime	0.90	0.10	0.87	0.12
<b>Share males</b>	Share of male workers in firm	0.72	0.24	0.69	0.25
<b>Share part-time</b>	Share of part-time workers in firm	0.09	0.11	0.09	0.12
<b>Share whitecollar</b>	Share of whitecollar workers in firm	0.35	0.23	0.35	0.24
<b>Mean age</b>	Mean age of firm's workers	40.15	2.71	39.93	2.94
<b>Mobility</b>	Number of past job moves divided by work experience	0.0013	0.01	0.0022	0.01
<b>South Germany</b>	Dummy: South Germany	0.43	0.49	0.42	0.49
<b>Insourcing</b>	Dummy: firm has insourced parts of ist activity	0.06	0.23	0.11	0.31
<b>Shut-down part of firm</b>	Dummy: firm has shut down parts of ist activity	0.02	0.16	0.07	0.26
<b>Outsourcing</b>	Dummy: firm has outsourced parts of ist activity	0.06	0.24	0.15	0.35
<b>Hiving-Off</b>	Dummy: firm has hived off parts of ist activity	0.05	0.22	0.07	0.26
<b>Reference: no restructuring of parts of activity</b>					
<b>Year 1997</b>	Dummy	0.11	0.31	0.10	0.30
<b>Year 1998</b>	Dummy	0.13	0.33	0.13	0.34
<b>Year 1999</b>	Dummy	0.17	0.37	0.17	0.38
<b>Year 2000</b>	Dummy	0.17	0.38	0.17	0.37
<b>Year 2001</b>	Dummy	0.19	0.39	0.18	0.39
<b>Reference: 1996 and 2002 (two year dummies left out because age included in FE regression)</b>					
<b>Agriculture and forrestry</b>	Sector dummy	0.002	0.04	0.003	0.05
<b>Mining and energy</b>	..	0.02	0.15	0.04	0.19
<b>Ressource processing</b>	..	0.19	0.39	0.16	0.37
<b>Investments goods</b>	..	0.39	0.49	0.33	0.47
<b>Consumption goods</b>	..	0.06	0.23	0.07	0.25
<b>Construction</b>	..	0.02	0.13	0.03	0.16
<b>Retail</b>	..	0.04	0.21	0.05	0.22
<b>Logistics and Communications</b>	..	0.06	0.23	0.05	0.22
<b>Credit and banking</b>	..	0.03	0.17	0.03	0.16
<b>Insurance</b>	..	0.01	0.10	0.01	0.11
<b>Restauration and hotel</b>	..	0.02	0.14	0.02	0.15
<b>Education and publishing</b>	..	0.03	0.16	0.03	0.18
<b>Health sector</b>	..	0.07	0.25	0.08	0.28
<b>Liberal professions</b>	..	0.02	0.13	0.02	0.14
<b>Other services</b>	..	0.005	0.07	0.01	0.09
<b>Reference: Manufacturing</b>					

Table 8: Wage equations (non-preferred sample)

	(1)	(2)	(3)	(4)
	Pooled OLS	FE	FE, IV (1)	FE, IV (2)
N	295196	295196	295196	295196
Tenure	0.041 (88 .7)	0.066 (32 .6)	0.083 (13 .1)	0.101 (10 .2)
Age	0.092 (24 .6)	0.343 (8 .8)	0.327 (8 .3)	0.305 (7 .5)
Age <sup>2</sup> /100	-0.199 (-20 .9)	-0.727 (-7 .4)	-0.689 (-6 .9)	-0.642 (-6 .3)
Age <sup>3</sup> /1000	0.144 (18 .6)	0.535 (6 .9)	0.508 (6 .5)	0.473 (5 .9)
Voc. Training	0.078 (24 .9)	0.034 (0 .9)	0.036 (0 .9)	0.038 (1 .0)
Voc. Training and A-levels	0.135 (20 .1)	0.052 (0 .7)	0.055 (0 .8)	0.060 (0 .9)
University	0.350 (60 .8)	0.093 (1 .2)	0.092 (1 .2)	0.091 (1 .1)
Skilled blue-collar	0.047 (11 .8)	-0.018 (-0 .4)	-0.024 (-0 .5)	-0.030 (-0 .7)
White collar	0.115 (17 .7)	0.133 (2 .0)	0.131 (2 .0)	0.129 (1 .9)
Log firm size	-0.004 (-2 .8)	-0.061 (-1 .9)	-0.080 (-2 .5)	-0.104 (-2 .9)
Business expectations	-0.066 (-5 .5)	0.233 (3 .9)	0.225 (3 .8)	0.199 (3 .4)
IT investments (dummy)	-0.026 (-6 .1)	-0.014 (-0 .8)	-0.026 (-1 .5)	-0.059 (-2 .7)
Investments (in 10 mill. Euros)	0.001 (4 .4)	0.001 (0 .4)	0.001 (0 .6)	0.001 (0 .6)
Firm uses part-time work	-0.021 (-2 .8)	-0.007 (-0 .2)	-0.022 (-0 .7)	-0.038 (-1 .2)
Share part-time	-0.071 (-3 .9)	-1.358 (-7 .1)	-0.971 (-4 .2)	-0.740 (-2 .8)
Firm uses fixed-term work	0.006 (1 .3)	-0.001 (-0 .1)	-0.015 (-0 .8)	-0.040 (-1 .8)
Sector-level coll. contract	0.056 (9 .1)	0.072 (1 .9)	0.059 (1 .6)	0.052 (1 .3)
Firm-level coll. contract	0.031 (4 .3)	0.112 (2 .5)	0.107 (2 .4)	0.095 (2 .1)
Works council	0.119 (22 .3)	-0.035 (-0 .7)	-0.032 (-0 .7)	-0.040 (-0 .8)
Training			IV for tenure	0.228 (1 .4)
Old technology			IV for tenure	-0.020 (-2 .1)
Overtime			IV for tenure	
Share males	0.390 (38 .8)	0.119 (0 .6)	0.218 (1 .1)	0.109 (0 .6)
Share whitecollar	0.065 (7 .8)	0.149 (1 .1)	0.196 (1 .4)	
Mean age	0.000 (0 .3)	-0.022 (-5 .2)	-0.017 (-3 .6)	
Mobility			IV for tenure	IV for tenure
South Germany	0.013 (5 .2)	0.069 (1 .4)	0.048 (0 .9)	0.035 (0 .7)
Outsourcing			IV for tenure	-0.027 (-1 .7)
Hiving-Off			IV for tenure	-0.029 (-1 .3)
Insourcing			IV for tenure	-0.025 (-1 .3)
Shut-down part of firm			IV for tenure	-0.045 (-2 .3)

Note: Year, sector and profession dummies included. T-values in parentheses. Reference categories are: No vocational training, unskilled blue-collar, no collective contract.

Table 9: Tenure equations (non-preferred sample)

	(1)	(2)	(3)	(4)
	Pooled OLS	FE	FE, IV (1)	FE, IV (2)
N	1532526	1532526	1532526	1532526
Wage	0.009 (239 .5)	0.005 (107 .9)	0.091 (24 .8)	0.052 (12 .1)
Age	0.595 (168 .1)	0.817 (121 .0)	0.094 (2 .8)	0.419 (11 .1)
Age <sup>2</sup> /100	-1.299 (-143 .3)	-1.386 (-80 .4)	-0.433 (-8 .0)	-0.855 (-15 .7)
Age <sup>3</sup> /1000	0.916 (122 .9)	0.984 (69 .8)	0.455 (12 .2)	0.689 (20 .5)
Voc. Training	0.104 (39 .7)	0.111 (9 .3)	-0.158 (-5 .7)	-0.036 (-1 .7)
Voc. Training and A-levels	-0.115 (-21 .1)	0.138 (6 .0)	-0.214 (-4 .2)	-0.059 (-1 .6)
University	-0.243 (-54 .4)	0.273 (12 .1)	-0.592 (-9 .8)	-0.205 (-3 .7)
Skilled blue-collar	0.065 (21 .5)	0.074 (9 .3)	0.015 (0 .9)	0.041 (3 .5)
White collar	-0.006 (-1 .2)	-0.006 (-0 .5)	-0.596 (-17 .0)	-0.333 (-9 .7)
Log firm size	0.002 (1 .6)	0.167 (26 .7)	-0.137 (-7 .4)	0.002 (0 .1)
Business expectations	0.046 (5 .4)	-0.080 (-11 .4)	-0.196 (-12 .6)	-0.106 (-11 .4)
IT investments (dummy)	0.041 (11 .5)	-0.062 (-18 .8)	-0.124 (-16 .8)	-0.086 (-16 .9)
Investments (in 10 mill. Euros)	0.002 (11 .1)	-0.003 (-18 .8)	-0.020 (-25 .4)	-0.012 (-14 .5)
Firm uses part-time work	0.051 (9 .6)	0.002 (0 .5)	-0.025 (-2 .4)	IV for wage
Share part-time			IV for wage	IV for wage
Firm uses fixed-term work	0.025 (7 .8)	-0.010 (-3 .1)	-0.074 (-10 .6)	-0.037 (-7 .4)
Sector-level coll. contract	0.036 (7 .3)	0.051 (8 .1)	0.013 (1 .0)	0.023 (2 .4)
Firm-level coll. contract	-0.111 (-19 .4)	-0.014 (-1 .9)	-0.111 (-7 .1)	-0.074 (-6 .4)
Works council	0.140 (24 .1)	-0.070 (-8 .0)	-0.194 (-10 .1)	-0.111 (-8 .0)
Training	0.346 (15 .3)	0.433 (14 .0)	0.475 (7 .3)	0.642 (14 .5)
Old technology	-0.024 (-16 .3)	0.001 (0 .9)	-0.003 (-1 .0)	-0.001 (-0 .4)
Overtime	-0.630 (-25 .0)	-0.080 (-2 .6)	0.793 (10 .7)	
Share males			IV for wage	0.195 (3 .6)
Share whitecollar			IV for wage	
Mean age			IV for wage	
Mobility	-45.902 (-242 .5)	-17.013 (-71 .9)	-9.035 (-15 .0)	-12.569 (-23 .9)
South Germany				0.041 (4 .1)
Outsourcing	-0.008 (-2 .1)	0.025 (8 .4)	0.008 (1 .2)	0.017 (3 .9)
Hiving-Off	-0.055 (-11 .9)	-0.016 (-4 .6)	-0.086 (-10 .7)	-0.059 (-9 .3)
Insourcing	-0.151 (-36 .2)	-0.053 (-15 .8)	-0.027 (-3 .7)	-0.040 (-8 .0)
Shut-down part of firm	-0.033 (-5 .5)	0.021 (4 .4)	0.045 (4 .4)	0.036 (5 .2)

Note: Year, sector and profession dummies included. T-values in parentheses. Reference categories are: No vocational training, unskilled blue-collar, no collective contract.

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