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Working Paper

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ZEW Discussion Papers, No. 05-90 [rev.]

Provided in cooperation with:
Zentrum für Europäische Wirtschaftsforschung (ZEW)

Suggested citation: Gürtzgen, Nicole (2006) : Rent-Sharing and Collective Bargaining Coverage: Evidence from Linked Employer-Employee Data, ZEW Discussion Papers, No. 05-90 [rev.], http://hdl.handle.net/10419/24699

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Rent-Sharing and Collective Bargaining Coverage – Evidence from Linked Employer-Employee Data

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Economic Research

Discussion Paper No. 05-90

Rent-Sharing and Collective Bargaining Coverage – Evidence from Linked Employer-Employee Data

Nicole Gürtzgen

Non-technical summary: This paper analyses whether wages in Germany respond to firm-specific profitability conditions. Particular emphasis is given to the question of whether the sensitivity of wages to firm-specific rents varies with collective bargaining coverage. To address this issue, we distinguish sector-specific wage agreements, firm-specific wage agreements and wage determination without any bargaining coverage. Theoretical considerations lead us to expect the sensitivity of wages to firm-specific rents to be larger under firm-specific contracts than in non-covered firms. The same is likely to hold for industry-wide agreements, provided the bargaining parties make use of flexibility provisions, which recently have become a widespread element of central wage agreements. Since direct information on the use of flexibility provisions in firms subject to an industry-wide wage agreement is unavailable, we take our empirical findings as an indirect test of whether the use of such provisions is a quantitatively important phenomenon in Germany.

Using linked employer-employee data from the mining and manufacturing sector, our empirical analysis offers a remarkably consistent picture: We find evidence that individual wages are positively related to firm-specific quasi-rents, but this appears to be confined to the non-union sector and to firm-specific contracts. Industry-wide wage agreements, in contrast, appear to suppress rent-sharing at the firm level. While pooled OLS estimates yield a positive correlation between wages and quasi-rents under centralised contracts, estimates accounting for unobserved individual and establishment heterogeneity point to a coefficient of zero. Differenced GMM estimates accounting for the endogeneity of our profitability measure even point to a negative relationship between wages and firm-specific profitability under centralised contracts. This leads us to conclude that the lower responsiveness of wages to firm-specific conditions under centralised contracts is not simply due to a downward-bias caused by the endogeneity of quasi-rents. In examining the impact of collective bargaining coverage on the wage-profit relationship, our findings therefore suggest that centralised wage bargaining suppresses any positive responsiveness of wages to different profitability conditions, and that the use of flexibility provisions in central wage agreements appears to be empirically negligible. To reconcile this result with the fact that a considerable fraction of firms covered by a collective contract pay wages above the going rate, we conclude from our findings that such wages do not result from differences in profitability conditions, but rather reflect observable and unobservable differences in worker quality.

As to the importance of worker characteristics, the invariance of wages against firm-specific conditions is found to be largest for low- and medium-skilled blue-collar workers. This is consistent with our hypothesis that the extent of inter-firm wage compression under centralised contracts ought to be particularly pronounced among those workers who are likely to be covered by collective contracts. In non-covered establishments, we find medium-skilled and male workers to benefit to a larger extent from their employers' ability-to-pay than unskilled and female workers, which lends support to the hypothesis that rent-sharing in non-covered plants mainly results from the bargaining power of works councils.

Rent-Sharing and Collective Bargaining Coverage - Evidence from Linked Employer-Employee Data

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This version: November 2006

Abstract

Using a linked employer-employee data set, this paper analyses the relationship between firm profitability and wages. Particular emphasis is given to the question of whether the sensitivity of wages to firm-specific rents varies with collective bargaining coverage. To address this issue, we distinguish sector-specific wage agreements, firm-specific wage agreements and wage determination without any bargaining coverage. Our findings indicate that individual wages are positively related to firm-specific quasi-rents in the non-union sector and under firm-specific contracts. Industry-wide wage contracts, however, seem to suppress firm-level rent-sharing. While pooled OLS estimates yield a positive correlation between wages and quasi-rents under centralised contracts, estimates accounting for unobserved individual and establishment heterogeneity point to a coefficient of zero. Finally, GMM estimates using suitable lagged values as instruments indicate that this result appears to be robust to the endogeneity of quasi-rents.

Keywords: Rent-Sharing, Unions, Linked Employer-Employee Data
JEL Code: J31, J51, C23

*Centre for European Economic Research, Department of Labour Markets, Human Resources and Social Policy, L 7.1, 68161 Mannheim, Germany, E-Mail: Guertzgen@zew.de. I am grateful to Alfred Garloff, Michael Maier and Friedhelm Pfeiffer for helpful comments on an earlier version of this paper. I am particularly thankful to Holger Alda for help with the data at the Research Data Centre (FDZ) of the Federal Employment Services (BA) at the Institute for Employment Research (IAB), Nuremberg. The institutions mentioned are not responsible for the use of the data in this publication. Financial support from the German Science Foundation (DFG) under the Program "Potentials for more flexibility on heterogeneous labour markets" (Grant-No. PF 331/3-1) is gratefully acknowledged.

1 Introduction

The fact that profit making employers tend to pay higher wages than less profitable firms has long been recognised as a major source of employer-specific wage differentials. The existing literature offers competing explanations for a positive relationship between wages and firms' ability to pay, such as short-run frictions in a competitive labour market, efficiency wage mechanisms and union power. An empirical test of a positive wage-profit correlation is generally seen as an indirect test of the competitive labour market theory, since the latter is difficult to reconcile with a long-run correlation between wages and profits (see e.g., Blanchflower et al. 1996, Hildreth and Oswald 1997).

A large number of studies have attempted to quantify the impact of profitability conditions on wages. Early studies date back to Slichter (1950) who reports a positive correlation between wages and employers' ability to pay using industry data from U.S. manufacturing. Later work on inter-industry wage differentials documents persistent wage differentials across industries that appear to be correlated with industry profits (Dickens and Katz 1987, Krueger and Summers 1988, Katz and Summers 1989 and Blanchflower et al. 1996). Most of this work controls for systematic worker differences across industries by using individual data which are matched to industry-specific profitability measures. However, in relying on aggregate profit data these studies typically fail to account for a within-industry correlation between firm profits and wages. Studies using firm-level data overcome this problem, but in general do not control for worker quality (e.g., van Reenen 1996, Hildreth and Oswald 1997, Budd et al. 2005). As the focus of interest is generally on whether identical workers are paid higher wages in more profitable firms, an ideal data set would include linked information on both employers and employees. With this information at hand, it would be possible to account for non-random sorting of high quality workers into more profitable firms. Moreover, linked employer-employee data also allow researchers to control for unobserved worker and firm heterogeneity, provided such information is available in a longitudinal dimension. To date, only few studies have investigated the wage-profit relationship using matched worker-firm data. Examples include Margolis and Salvanes (2001), Arai (2003), Kramarz (2003), Nekby (2003) as well as Martins (2004).

Given the role unions may play in extracting rents, a further interesting question is how unions and the level of bargaining affect the extent of rent-sharing. Although an enormous volume of research has investigated the effects of unions and labour market institutions on inter-industry and skill wage differentials (e.g. Holmlund and Zetterberg 1991, Edin and Zetterberg 1992, Blau and Kahn 1996, Kahn 1998), much less work has been done on the effect of unions and the bargaining structure on the returns to firm-specific attributes such as profits. Moreover, while much of the empirical research on unions and wage differentials is based on cross-country comparisons, only few studies make use of intra-national variations in labour market institutions.[1] Clearly, such variations offer the advantage of avoiding the large amount of unobserved heterogeneity characterising cross-country comparisons.

This paper attempts to close this gap by exploring the linkages between individual wages, firm-specific profits and collective bargaining coverage using a large-scale German linked employer-employee data set. Our analysis of rent-sharing and collective bargaining in Germany is motivated by several reasons. First, the German case provides an instructive example for the co-existence of different bargaining structures. Until the early 1990s, wage determination was dominated by centralised wage bargaining between industry-specific unions and employers' associations. However, in the last decade, there has been a tendency towards decentralisation of wage determination, since firm-specific collective wage agreements as well as wage determination without any bargaining coverage have become more important (Hassel 1999, Ochel 2005). Even within centralised industry agreements, there have been numerous attempts to allow for more (downward) flexibility of wages by introducing opening and hardship clauses. Moreover, since bargained wages in centralised agreements merely represent a lower bound for wages, there is also sufficient room for upward flexibility. Given that recent decentralisation tendencies have introduced - at least formally - the possibility of adjusting wages to local conditions at the firm level, the main purpose of the paper is to shed light on the following questions: Do firm-specific contracts and flexibility provisions in centralised industry agreements allow for rent-sharing at the firm level? If so, does the extent to which wages respond to profits differ from that in firms without any bargaining coverage?

Second, very few studies have been undertaken on the relationship between wages and profits in Germany and, to our knowledge, there is no study that uses matched worker-firm data. Hübler and König (1998) and Klodt (2000) use data from the 'Hannover Establishment Panel'. They report a significant positive impact of profits on average firm wages, but do not allow the effect to vary with bargaining coverage. In own recent work, we use data from the *IAB-Establishment Panel* and find wages to be positively related to establishment profits. However, this appears to be true only for uncovered establishments, since we fail to detect any positive relationship between wages and local profitability conditions in plants that are subject to a collective wage agreement - irrespective of whether the agreement is industry or firm-specific (Guertzgen 2005). However, like any other analysis using such aggregate data, these results are subject to the limitations of establishment or firm-level data. First, there may be unobserved worker heterogeneity,

[1]Exceptions are the studies by Hartog et al. (2002), Cardoso and Portugal (2005) and Card and de la Rica (2006), who use intra-national variations in the bargaining structure to analyse the impact of the bargaining structure on the wage level and on the returns to worker attributes.

which is unlikely to be fully captured by establishment level data. Second, aggregate data generally provide a rather crude measure of wages. The *IAB-Establishment Panel* only offers information on the reported wagebill exclusive of fringe benefits or bonus payments. Whenever rent-sharing takes the form of such supplemental payments, the use of these data will clearly entail an understatement of the true wage-profit relationship.

In this paper, we investigate the relationship between wages and profitability using the IAB Linked Employer-Employee data set (LIAB) which combines data from the *Employment Statistics Register* and the *IAB-Establishment Panel*. This data set is especially useful for our purposes since it enables us to match individual data with establishment-specific information on value added and collective bargaining coverage. A particular advantage is the exact information on earnings generated by the administrative nature of the *Employment Statistics Register*. In our estimation strategy, we first focus on simple static pooled Ordinary Least Squares (POLS) estimates. The OLS estimations serve as a benchmark case and will be modified in various respects. First, we will address the possibility of unobserved individual time invariant factors. Second, we will consider both establishment- and individual-level unobserved heterogeneity by estimating differenced spell fixed-effects models. A final problem concerns the endogeneity of our profitability measure, since wages and profits are simultaneously determined. To address this problem, we will instrument profits using differenced GMM-estimators according to Arellano and Bond (1991) and Arellano and Bover (1995).

The main results can be summarised as follows: We find evidence that individual wages are positively related to local profitability conditions. However, this only seems to be true for wage determination in the non-union sector and under firm-specific contracts. For establishments covered by an industry-wide wage contract, pooled OLS estimates yield a positive correlation between wages and quasi-rents, while estimates accounting for unobserved individual and establishment heterogeneity point to a coefficient of zero. Finally, GMM estimates using suitable lagged values as instruments for our profitability measure indicate that this result appears to be robust to the endogeneity of quasi-rents. In examining the impact of collective bargaining coverage on the wage-profit relationship, our findings therefore support the notion that centralised wage bargaining largely suppresses any positive responsiveness to local profitability conditions.

The remainder of the paper is organised as follows: the institutional background is presented in Section 2. Section 3 outlines the general empirical model and derives testable hypotheses about the degree of rent-sharing under different bargaining set-ups. Section 4 describes the data set and the main variables used in the subsequent analysis. Section 5 presents the results from the pooled OLS, fixed-effects and differenced GMM estimations. The final Section 6 concludes.

2 Is there any scope for firm-level rent-sharing in Germany?

The German system of wage bargaining is usually characterised as medium centralised, with regional and industry-wide collective wage agreements being the predominant form of wage determination (Calmfors and Driffill 1988, Soskice 1990, OECD 2004). Such central wage agreements are negotiated between an industry-specific trade union and an employers' association. They are legally binding on all firms which are members of the respective employers' association and on all employees who are members of the relevant trade union. Although strictly speaking the negotiated wage only applies to union members, member firms generally extend the wage settlement to non-unionised employees as well.[2]

Given the predominance of centralised wage bargaining, there appears to be little scope for rent-sharing at the firm level in Germany. A closer look at the German system of wage determination, however, shows that the situation is much more subtle. Since the early 1990s, the clear trend in German industrial relations has been towards more decentralised forms of wage determination (see e.g. Hassel 1999, Kohaut and Schnabel 2003). This tendency is driven by three major developments. First, the number of firm-specific collective wage agreements negotiated between an individual firm and an industry-specific trade union has increased markedly since the beginning of the 1990s. Second, wage determination without any bargaining coverage is growing in importance. In firms which are not covered by a collective agreement wage determination either takes the form of individual wage contracts or of plant-specific agreements (*Betriebsvereinbarungen*) between works councils and the management.[3] Third, there is a tendency even within centralised wage agreements to allow for more flexibility at the firm-level. In recent years, contractual opt-out clauses or hardship clauses have become a widespread element of central agreements. While opening clauses delegate issues that are usually specified in the central agreement, such as working-time and pay-conditions, to the plant-level, hardship clauses enable firms to be exempted from the centralised agreement if they are close to bankruptcy. In general, the adoption of such clauses requires the approval of the collective bargaining parties (Hassel 1999, Ochel 2005). Moreover, since bargained wages in centralised agreements merely represent a lower bound for wages, there is also sufficient

[2]The reason is that non-unionised employees who would receive a lower wage may be expected to join the union anyway in order to benefit from the higher union wage. Moreover, central wage agreements may also apply to non-member firms and their employees if the agreement is declared to be generally binding by the Federal Ministry of Labour.

[3]According to the German Works Constitution Act, works councils are not allowed to negotiate about issues that are normally dealt with in collective agreements, even in firms that are not parties to a collective agreement. In practice, however, works councils may be expected to play a crucial role in wage determination (see e.g. Hassel 1999, Hübler and Jirjahn 2003).

room for upward flexibility.[4] To sum up, recent decentralisation tendencies in Germany have introduced - at least formally - the possibility of adjusting wages to local conditions at the firm level. However, at this point it is worth noting that the extent to which this potential has really been exploited still remains to be examined empirically. For example, even though contractual opening and hardship clauses have become an important (formal) element of centralised agreements, empirical evidence on the use of such clauses is rather sparse.[5]

3 Empirical Model and Testable Hypotheses

In order to quantify the relationship between wages of individual workers and their employers' ability-to-pay, we consider a wage equation taking the following form:

$$\ln w_{it} = \mu + \beta_\pi \cdot \pi_{j(i,t)t} + \gamma \cdot \mathbf{x}'_{it} + \delta \cdot \mathbf{u}'_i + \eta \cdot \mathbf{w}'_{j(i,t)t} + \rho \cdot \mathbf{q}'_{j(i,t)} + \lambda_t \cdot D_t + \alpha_i + \phi_{j(i,t)} + \epsilon_{it} \quad (1)$$

There are $i = 1,..., N$ individuals, and $N^* = \sum T_i$ total worker-year observations. As we use individual data that are matched to establishment-level data, $j(i,t)$ refers to the establishment which employs individual i at time t, with $j = 1, ..., J$. The dependent variable, $\ln w_{it}$, is the individual log daily wage. The explanatory variable of main interest is $\pi_{j(i,t)t}$, measuring (time-varying) establishment-specific per-capita profitability.[6] $\mathbf{x}'_{it}$ represents a vector of time-varying individual covariates with a coefficient vector γ, while $\mathbf{u}'_i$ denotes a vector of individual time-constant characteristics with a coefficient vector δ. Similarly, $\mathbf{w}'_{j(i,t)t}$ and $\mathbf{q}'_{j(i,t)}$ represent time-varying and time constant $j-$level covariates with coefficient vectors η and ρ. α_i and $\phi_{j(i,t)}$ denote individual and establishment-specific unobserved heterogeneities. Finally, industry dummies are included to capture industry-specific factors, such as the overall level of industry demand and the degree of competition. Time dummies D_t are included to capture common macroeconomic shocks, and ϵ_{it} is a white-noise error term.

Since the emphasis of our analysis is on the impact of collective bargaining coverage on the sensitivity of wages to local profitability conditions, the coefficient β_π is specified to depend on the contract-type:

$$\beta_\pi = \beta_0 + \beta_{\pi\_CENT} \cdot CENT_{it} + \beta_{\pi\_FIRM} \cdot FIRM_{it}, \quad (2)$$

[4] Using data from the *IAB-Establishment Panel*, Bellmann et al. (1998) find about 50 per cent of all establishments in western Germany that are covered by a collective contract pay wages above the going rate.

[5] One exception is the study by Franz and Pfeiffer (2003), who analyse this issue based on an employer-survey of about 800 German firms. Their results indicate that only 18 per cent of those employers that are covered by a collective contract which allows for hardship clauses make use of such provisions.

[6] Particularly in case of multi-plant firms, it might be argued that firm-level profitability provides a more appropriate measure than establishment-level profitability. However, we only have access to the establishment-level measures, which we take as a proxy for firm-level profitability.

where $CENT$ is a dummy taking the value of unity if an establishment is subject to an industry-wide collective wage contract and $FIRM$ takes on the value of unity if an establishment is covered by a firm-specific contract.

Bargaining power considerations suggest the sign of β_{π_FIRM} to be positive, i.e. the sensitivity of wages to local profits is likely to be larger under firm-specific contracts than in uncovered establishments. An important argument is that firm-specific contracts in Germany are concluded by industry-specific unions. This distinguishes German firm-specific collective wage agreements from similar wage agreements in other countries, such as those in the U.K., where firm-specific unions bargain independently from each other (see e.g. OECD 2004). For this reason, the bargaining power of works councils determining wages in uncovered establishments may be expected to be considerably lower than that of an industry-wide union which determines wages under firm-specific contracts. This prediction is reenforced by the fact that the wage bargaining process under firm-specific contracts is highly coordinated by an industry-wide union, whereas it is completely uncoordinated in uncovered plants. While the bargaining parties in uncovered plants have an incentive to cut wages in order to gain a larger share of industry demand, this competitive mechanism completely disappears with an industry union (see Guertzgen 2005). This leads us to expect an industry union to capture a larger share of rents under firm-specific contracts than, say, works councils in uncovered establishments.

The sign of β_{π_CENT} cannot be predicted a-priori, since this depends on the fraction of firms making use of flexibility provisions in centralised wage agreements. Since our data lack explicit information on the use of such provisions, we will take our empirical findings as an indirect test of whether such provisions are really exploited. In this case, β_{π_CENT} might be expected to be positive (for the same reason as under firm-specific contracts). Conversely, testing $\beta_{\pi\_CENT} = -\beta_0$ provides a direct test of a complete invariance of wages against firm/establishment-specific conditions. Note that a rationale for why unions might favour a compressed intra-industry wage structure could be workers' demand for income insurance. The idea that wage compression might provide insurance against income risk has been taken up by several authors. Horn and Svensson (1986) show that union contracts may help to enforce implicit contracts between risk-averse workers and risk-neutral firms facing uncertainties over the business-cycle. Agell and Lommerud (1992) interpret wage compression across different skill groups as insurance against ex-ante uncertainties over skill endowments. Burda (1995) takes this approach further and analyses unions' reactions to changes in the distribution of uncertainties. Note that in our context, intra-industry wage compression provides insurance against two dimensions of uncertainties. First, wage compression between firms at a given point in time may reduce income risk if workers face uncertainties over the allocation to more or less profitable firms. Second, given that with a compressed intra-industry wage structure wage growth is likely to depend on changes in average sector performance, workers' wages in a given firm should also be sheltered against fluctuations in firm-level profitability over time.

Having derived hypotheses about the role of collective bargaining for the wage-profit relationship, it may also be useful to consider the importance of individual characteristics. The differential effects measured by the interaction terms ought to be particularly pronounced among those groups of workers who are likely to be covered by collective contracts. While our data contain information on collective bargaining coverage at the establishment level, they unfortunately lack explicit information on whether an individual worker is covered or not. There are a number of studies that analyse the determinants of individual union membership in Germany (e.g., Beck and Fitzenberger 2004, Goerke and Pannenberg 2004), but empirical evidence on individual collective bargaining coverage is rather scarce.[7] International empirical evidence suggests individual non-coverage to be particularly relevant among high-skilled managerial workers (see e.g. Hartog et al. 2002a). As a result, we expect the interaction effects to be stronger for low and medium-skilled blue-collar workers.

In uncovered establishments, a positive wage-profit correlation may result from the bargaining power of individuals and works councils on the one hand and from efficiency wage mechanisms on the other. The latter give rise to a positive correlation between wages and profits due to productivity enhancing effects. Such effects may arise, for example, from reductions in turnover and shirking (see e.g. Krueger and Summers 1988). Thus, efficiency wage considerations lead us to expect the wage-profit correlation to be particularly strong among the better educated, since those workers are more likely to accumulate firm-specific human capital and are less likely to be supervised than low-skilled workers. This raises the relative incentive for employers to pay above market-clearing wages in order to reduce turnover and shirking. Note that the supervision argument should also hold for white-collar as compared with blue-collar workers. The same conclusions can be drawn in case of rent-sharing as the result of individual wage bargaining power, because highly educated workers may be expected to have better outside options and higher bargaining power than low-skilled individuals. Finally, rent-sharing in uncovered establishments may also result from the bargaining power of works councils. Thus, the extent of rent-sharing ought to be larger for those groups of workers whose wages are likely to be affected by works councils. Empirical evidence on the presence of works councils generally suggests that the likelihood of codetermination increases with the share of male as well as skilled workers (see Addison et al. 1997, Hübler and Jirjahn 2003, Zwick 2004). This may be interpreted as a weak hint for wages of those groups being more likely to be influenced by works councils than those of female and low-skilled individuals.

[7]Note that individual union membership is not a necessary condition for individual bargaining coverage, since firms often extend the wage contract to non-member employees as well.

4 Data and Variable Description

The empirical analysis uses the IAB Linked Employer-Employee data set (LIAB) which combines data from the *IAB-Establishment Panel* and the *Employment Statistics Register*. The *IAB-Establishment Panel* is based on an annual survey of establishments in western Germany administered since 1993 by the research institute of the Federal Employment Services in Nuremberg (IAB - Institute of Employment Research). Establishments in eastern Germany entered the panel in 1996. The database is a representative sample of German establishments employing at least one employee who pays social security contributions. The survey data provide a great deal of information on establishment structure and performance, such as sales, the share of materials in sales and investment expenditures (see e.g. Bellmann et al. 2002). Moreover, the data set contains information on whether an establishment is covered by an industry-wide collective wage agreement, a firm-specific wage agreement or by no collective agreement at all.

The worker information comes from the *Employment Statistics Register* which is an administrative panel data set of all employees paying social security contributions (see e.g. Bender et al. 2000). The data are based on notifications which employers are obliged to provide for each employee covered by the social security system. These notifications are required whenever an employment relationship begins or ends. In addition, there is at least one annual compulsory notification for all employees who are employed on the 31^{st} December of each year. Due to its administrative nature, this database has the advantage of providing reliable information on daily earnings that are subject to social security contributions. The establishment and worker data sets contain a unique establishment identification number. This allows us to match information on all employees covered by the social security system with the establishments in the *IAB-Establishment Panel*.

The construction of the Linked Employer-Employee data set occurs in two steps: First, we select establishments from the establishment panel data set. From the available waves 1993 to 2001, we use the years 1995 to 2001, since detailed information on bargaining coverage is available only from 1995 onwards. Since information on a number of variables, such as sales and the share of materials in total sales are gathered retrospectively for the preceding year, we lose information on the last year. Moreover, we restrict our sample to establishments from the mining and manufacturing sector with at least two employees. We focus on these industries, since the introduction of opening and hardship clauses here has been particularly relevant in central collective wage agreements. These sectors therefore provide a particularly interesting case for testing the empirical relevance of the use of such clauses. As we apply dynamic panel data methods, only establishments with consistent information on the variables of interest (described below) and at least three consecutive time series observations are included in our sample. This results in a sample of 843 establishments with 3,498 observations, yielding an unbalanced panel containing establishment-observations with, on average, 4.15 years of data.[8]

In the second step, the establishment data are merged with notifications for all employees who are employed by the selected establishments on June 30^{th} of each year. From the worker data we drop observations for apprentices, part-time workers and homeworkers. To avoid modeling human capital formation and retirement decisions, we exclude individuals younger than 19 and older than 55. Moreover, since we consider only full-time workers, we eliminate those whose wage is less than twice the lower social security contribution limit. In order to be able to conduct first-differencing, we consider only those individuals for whom at least two consecutive time series observations are available. The final sample comprises 333,045 individuals in 821 establishments, yielding an unbalanced panel containing 3,361 establishment years and 1,305,705 individual observations with, on average, 3.92 years of data for each worker.[9]

The individual data include information on gross daily wage, age, gender, nationality, employment status (blue/white-collar), education (six categories)[10] and on the date of entry into the establishment. The latter is used to approximate tenure by subtracting the entry date from the ending date of the employer's notification which is available from the worker data. Note, however, that this proxy does not account for potential employment interruptions which might have occurred during this time span.

The dependent variable in the subsequent analysis will be the real gross daily wage. Since there is an upper contribution limit to the social security system, gross daily wages are top-coded. In our sample, top-coding affects about 12 per cent of all observations. To address this problem, we construct 36 cells based on education, gender and year. For each cell, a tobit regression is estimated with log daily wages as the dependent variable and individual and establishment covariates as well as industry dummies as explanatory variables (see Table 1 below). As described in Gartner (2005), right-censored observations are replaced by wages randomly drawn from a truncated normal distribution whose moments are constructed by the predicted values from the Tobit regressions and whose (lower) truncation point is given by the contribution limit to the social security system.

[8]Originally, the sample includes 2,897 establishments with consistent information on all the variables of interest. 12 observations were dropped due to suspected errors in the rent variable. These observations featured per-capita values of rents of above 1 million DM. This results in a sample of 2,891 establishments with a total of 6,404 observations. Only 843 of these feature at least three consecutive time-series observations.

[9]Note that the exclusion of certain individual groups entails a loss of 22 establishments.

[10]The categories are: No degree, vocational training degree, high school degree (*Abitur*), high school degree and vocational training, technical college degree and university degree. Missing and inconsistent data on education are corrected according to the imputation procedure described in Fitzenberger et al. (2006). This procedure relies, roughly speaking, on the assumption that individuals cannot lose their educational degrees.

After this imputation procedure, nominal wages are deflated by the Consumer Price Index of the German Federal Statistical Office normalised to 1 in 2000.

Turning to the establishment variables, the main variables used in the subsequent empirical analysis are defined as follows. Following the majority of the rent-sharing literature (see e.g. see Abowd and Lemieux 1993, van Reenen 1996), establishment profitability, π, is measured by per-capita quasi-rents. We choose quasi-rents - defined as value-added minus the opportunity cost of labour - for two reasons. First, from a theoretical perspective quasi-rents may be interpreted as representing the 'pie' to be divided between the bargaining parties. Second, from an econometric perspective, the use of quasi-rents instead of profits enables us to circumvent the endogeneity problem induced by the accounting relationship between wages and profits. In particular, we construct per capita quasi-rents as the difference between annual sales, material costs and the alternative annual wagebill divided by establishment size, so that

$$\pi = \frac{SALES - MATERIALCOST - \overline{w} \cdot SIZE}{SIZE}. \tag{3}$$

Establishment size ($SIZE$) is calculated as the number of employees reported for the month June averaged over the present and preceding year. The alternative wagebill, $\overline{w} \cdot SIZE$, is defined as the annual wagebill which each firm would incur if it had to pay the average industrial wage. Thus, we approximate $\overline{w}$ by the weighted average of industry-specific wages for blue and white-collar workers (separately for western and eastern Germany), with the weights being the establishment-specific shares of those worker groups in the total work force.[11] The fractions of blue and white-collar workers are taken from the establishment data because the *Employment Statistics Register* provides the individual employment status only for full-time workers. All monetary values are expressed as real values by deflating them with a sector-specific producer price index normalised to 1 in 2000. Industry-specific price indices and wages are obtained from the German Federal Statistical Office and are matched to the establishment data on the basis of a two-digit sector classification.

Note that the profitability measure does not account for capital costs, because our data lack explicit information on such costs. However, we attempt to control for differences in capital intensities. As we do not directly observe the capital stock, we need to construct a proxy. We measure capital by using the perpetual inventory method starting from the capital value in the first observation year and using the information on expansion investment in the following years. The initial capital value is proxied by dividing investment

[11] We convert sectoral hourly industrial wages of blue collar workers into monthly wages by multiplying them with firm-specific average working time. Since information on average sectoral wages of white-collar workers is available only on a monthly basis, we are not able to adjust those wages for firm-specific average working time. Monthly values are converted into annual values by multiplying them with the factor 12.

expenditures in each establishment's first observation year by a pre-period growth rate of investment, g, and a depreciation rate of capital, δ.[12] Capital stocks in subsequent periods are calculated by adding real expansion investment expenditures.[13] To obtain real values, nominal investment expenditures are deflated by the producer price index of investment goods of the German Federal Statistical Office. The capital-labour ratio, K/L, is constructed by dividing the resulting capital proxy by establishment size. Finally, further establishment variables include the existence of a works council as well as information on industry-specific and firm-specific collective bargaining coverage.

Table 1: Summary statistics

Variable	Definition	Mean	Std.-Dev.	Mean	Std.-Dev.
		Individual level		Establishm. level	
Individual characteristics					
$\ln w$	Real log daily wage in DM	5.22	0.33	4.94	0.33
FEMALE	Female worker	0.19	0.39	0.24	0.22
AGE	Age in years	39.05	9.03	38.92	3.42
TENURE	Tenure in months	135.66	86.14	93.97	48.73
FOREIGN	Foreign worker	0.10	0.31	0.06	0.10
WHITECOLL	White-collar worker	0.37	0.48	0.32	0.22
VOCATIO	Vocational Degree	0.67	0.47	0.75	0.20
HIGHSCHOOL	Highschool Degree	$4.7e^{-03}$	0.07	$3.8e^{-03}$	0.02
VOC-HIGH	Voc. and Highschool Degree	0.03	0.16	0.02	0.05
TECHN-UNI	Technical Univ. Degree	0.06	0.24	0.05	0.08
UNI	University Degree	0.06	0.24	0.04	0.07
Establishment characteristics					
π	Per-capita quasi-rents	1.06	0.79	0.68	0.80
SIZE	Establishment size	6,680.86	12,430.56	550.89	2,075.87
CENT	Centralised agreement	0.88	0.31	0.62	0.44
FIRM	Firm-specific agreement	0.08	0.25	0.12	0.26
WCOUNCIL	Works council	0.97	0.15	0.64	0.47
K/L	Capital-labour ratio	1.94	4.01	2.25	12.3
EAST	Eastern Germany	0.15	0.35	0.43	0.49
Individuals		333,045			
Establishments				821	

Source: LIAB 1995-2001. 821 establishments, 333,045 individuals, 1,305,705 observations.
Note: Per-capita quasi rents and the capital-labour ratio are measured in 100,000 DM.
1 € corresponds to 1.95583 DM.

[12] This involves the assumption that investment expenditures on capital have grown at a constant average rate, g, so that the capital stock in the base year is $K_1 = I_0 + (1-\delta)I_{-1} + (1-\delta)^2 I_{-2} + \ldots = I_1 \sum_{s=0}^{\infty}[\frac{1-\delta}{1+g}]^s = I_1/(\delta+g)$. In particular, to calculate K_1, we set $\delta = 0.1$ and $g = 0.05$ (see Hempell 2002).

[13] More specifically, $K_t = K_{t-1}(1-\delta) + I_{t-1} = K_{t-1} + EI_{t-1}$, where K_t is the capital stock at the beginning of period t, i.e. at the end of period $t-1$, and EI_t are expansion investment expenditures in period t.

Table 1 presents summary statistics for the variables used in the subsequent analysis. The first two columns report statistics averaged over individuals, whereas the last two columns present statistics that are averaged over establishments. Note that both statistics partly differ substantially from each other due to the underlying distribution of establishment size. Because larger establishments pay on average higher wages and are more profitable in terms of per-capita quasi-rents, the underlying sample means are lower on the establishment level. Moreover, there are also considerable differences with respect to collective bargaining coverage. In particular, it can be seen that large establishments are much more likely to be covered by an industry-wide agreement, whereas small establishments are more likely to belong to the non-union sector. As a result, the overwhelming majority of individuals (88 per cent) are employed by an establishment that adopts an industry-wide agreement. The fraction of individuals in establishments that are subject to a firm-specific agreement amounts to 8 per cent. Finally, only 4 per cent of all individuals are subject to no agreement at all, even though the fraction of uncovered establishments amounts to about 26 per cent. Breaking down the sample into those individual observations covered by an industry-wide agreement, a firm-specific agreement and into those without any bargaining coverage reveals that wages are highest under industry-wide agreements and lowest without any bargaining coverage (see Table A1 in the Appendix). The variability in wages is higher for individuals without any bargaining coverage with a coefficient of variation of about 0.08 as compared with 0.06 and 0.07 for individuals who work in an establishment that is covered by a collective contract. Moreover, workers covered by firm-specific agreements are, on average, employed by more profitable firms, followed by those working in firms that are subject to an industry-wide agreement.

5 Results

5.1 Estimation Strategy

We first focus on a simple static pooled Ordinary Least Squares (POLS) specification of eq. (2), in which neither α_i nor $\phi_{j(i,t)}$ are controlled for. The POLS estimations serve as a benchmark case and will be modified in various respects: First, we control for individual unobserved heterogeneity to assess the extent to which unobservably more productive workers work in more profitable plants. Second, we address the possibility of unobserved plant-specific time invariant factors. Finally, we address the endogeneity of per-capita rents by using dynamic panel data methods.

5.2 Pooled OLS-Results

Table 2 reports the results from the POLS estimations of the impact of quasi-rents per worker on individual log wages. Quasi-rents are specified in levels rather than logs, since the use of logs would have required discarding all observations with negative quasi-rents.

In the first simplest model, which includes quasi-rents as the only explanatory variable, the estimate of quasi-rents per employee on the individual wage is 0.110. Adding individual characteristics increases the explanatory power of the model considerably (by a factor of more than six) and reduces the coefficient to 0.061, suggesting that almost 50 per cent of the correlation between rents and wages is due to systematic sorting of workers across firms (Model (2)). In particular, high-qualified workers appear to be associated with more profitable firms. The effects of rents on wages are further reduced when including other establishment characteristics, such as establishment size, bargaining coverage, the existence of a works council and the capital-labour ratio (Model (3)). Apart from the capital-labour ratio K/L, all control variables enter the regression with their expected sign and are all significant at the 1 or 5 per cent level. In line with earlier evidence, establishment size is found to have a significant positive effect on individual wages.[14] In the literature, a positive firm size effect is usually explained by differences in profitability conditions, capital equipment, worker quality and monitoring costs among others (see e.g. Oi and Idson 1999). As our specifications explicitly control for worker quality, the capital-labour ratio and quasi-rents, the establishment size variable may be interpreted as capturing some part of unobserved worker quality and technology differences.

The effects of quasi-rents on wages are further reduced after adding an east-west dummy, which is in accordance with less favourable economic conditions in eastern German establishments (Model (4)). Moreover, controlling for establishment location leads to a larger and more precise estimate of the capital-labour ratio on wages, indicating systematic differences in capital intensity across regions. Note that the coefficients on $CENT$ and $FIRM$ drop significantly in Model (4), which reflects the much lower extent of collective bargaining coverage among employers in eastern Germany.

Given the predominance of industry-level wage bargaining, it might be conceivable that the positive effect of quasi-rents on wages was primarily due to rent-sharing on the industry level. For this reason, we investigate whether the positive correlation is robust to the inclusion of 16 two-digit industry dummies (Model (5)). Controlling for industry affiliation increases the coefficient on rents even somewhat, suggesting that the sensitivity of wages to quasi-rents estimated by Models (4) and (5) mainly refers to within industry rent-sharing.

[14] For German evidence on employer size effects see e.g. Schmidt and Zimmermann (1991) and Gerlach and Hübler (1998).

Table 2: Regression results

Model	(1)	(2)	(3)	(4)	(5)	(6)
Pooled OLS regression results						
π	0.110***	0.061***	0.038***	0.026***	0.033***	0.100***
	(0.017)	(0.008)	(0.008)	(0.007)	(0.005)	(0.013)
π_CENT						-0.077***
						(0.014)
π_FIRM						-0.009
						(0.017)
SIZE/1000			0.019***	0.015***	0.009***	0.009***
			(0.003)	(0.003)	(0.003)	(0.003)
$SIZE^2/1000$			-.000***	-.000***	-.000**	-.000**
			(0.000)	(0.000)	(0.000)	(0.000)
CENT			0.127***	0.074***	0.077***	0.140***
			(0.032)	(0.029)	(0.027)	(0.031)
FIRM			0.098***	0.061**	0.073***	0.062**
			(0.034)	(0.028)	(0.026)	(0.027)
WCOUNCIL			0.154***	0.146***	0.115***	0.111***
			(0.024)	(0.018)	(0.021)	(0.020)
(K/L)			-0.001	0.002***	0.002***	0.002***
			(0.002)	(0.000)	(0.000)	(0.000)
EAST				-0.250***	-0.249***	-0.243***
				(0.017)	(0.015)	(0.014)
Individual controls	No	Yes	Yes	Yes	Yes	Yes
Ind.-/Time	No	No	No	No	Yes	Yes
Adj. R^2	0.087	0.580	0.641	0.689	0.708	0.712
Establishments	821	821	821	821	821	821
Individuals	333,045	333,045	333,045	333,045	333,045	333,045
Observations	1,305,705	1,305,705	1,305,705	1,305,705	1,305,705	1,305,705

Source: LIAB 1995-2001. Note: The dependent variable is the individual log daily wage. Standard errors are in parentheses and are adjusted for clustering at the establishment level. Individual control variables include gender, nationality, education (6 categories), a dummy for white-collar workers, tenure, tenure squared, age, age squared. Specifications (5) and (6) include 16 two-digit industry dummies and 5 time dummies. ***Significant at 1%-level, **Significant at 5%-level.

Finally, we are primarily interested in whether the rent-coefficient varies systematically with collective bargaining coverage. To investigate this issue, Model (6) includes interactions between collective bargaining coverage and rents. The results indicate that the extent to which wages react to local profitability conditions is significantly lower in establishments that are covered by a centralised wage agreement. However, the null hypothesis of $\beta_0 = -\beta_{\pi\_CENT}$ can be rejected at conventional levels, suggesting that the overall impact of rents on wages is still positive. In establishments that are covered by a firm-specific contract, wages do not appear to be less sensitive to rents as the coefficient on the interaction term is found to be insignificant.

5.3 Individual fixed-effects

Even though we have controlled for observable individual characteristics, it is conceivable that the positive effect of quasi-rents on individual wages is due to sorting of unobservably more productive workers into more profitable establishments. To assess the extent to which sorting affects our estimates, we next control for unobserved individual heterogeneity. First-differencing of eq. (2) sweeps-out the individual effect α_i:

$$\Delta \ln w_{it} = \beta_\pi \cdot \Delta\pi_{j(i,t)t} + \gamma \cdot \Delta\mathbf{x}'_{it} + \eta \cdot \Delta\mathbf{w}'_{j(i,t)t} + \rho \cdot \Delta\mathbf{q}'_{j(i,t)} + \lambda_t \cdot \Delta D_t + \Delta\phi_{j(i,t)} + \Delta\epsilon_{it} \quad (4)$$

Note that first-differencing also eliminates individual time-constant characteristics $\mathbf{u}_i$, so that the coefficient vector δ cannot be identified.[15] Model (1) and (2) in Table 3 report the individual first-differenced regressions results.[16] The specifications include the full set of time-varying covariates from Model (5) and (6) in Table 2. While Model (1) contains no interaction terms, Model (2) allows the coefficients to vary with collective bargaining coverage.

In Model (1), quasi-rents enter the equation with a positive sign, but the coefficient is not statistically significant. Interestingly, in Model (2), where the effect is allowed to vary with collective bargaining coverage, the coefficients are more precisely estimated. While the coefficient on quasi-rents is significantly positive for uncovered establishments, the effect is found to be significantly lower under centralised wage agreements. In contrast to the POLS results, a Wald test fails to reject the null $\beta_0 = -\beta_{\pi\_CENT}$ (with a p-value of 0.45), indicating that the overall effect of rents on wages is even zero under centralised contracts. For firm-specific contracts, the interaction term is found to be negative, but not significantly different from zero. Overall, the estimated effects of quasi-rents on wages are much lower than the POLS estimates. This finding is indicative of some systematic sorting of unobservably more productive workers into more profitable firms. Given that the POLS

[15] In our specification, individual time-constant covariates are gender and nationality.

[16] Note that the number of observations drops from 1,305,705 to 971,057 since we lose one observation for 331,442 individuals and two observations for those (1,603) whose time series exhibits a gap.

Table 3: Individual and spell first-differenced regression results

Model	*(1)*	*(2)*	*(3)*	*(4)*
	Individual fixed-effects		Spell fixed-effects	
$\Delta\pi$	0.005	0.016***	0.005	0.017***
	(0.004)	(0.003)	(0.003)	(0.003)
$\Delta\pi$_CENT		-0.014**		-0.014**
		(0.004)		(0.004)
$\Delta\pi$_FIRM		-0.003		-0.003
		(0.004)		(0.004)
ΔSIZE/1000	0.010**	0.011**	0.013	0.013
	(0.004)	(0.004)	(0.007)	(0.007)
ΔSIZE^2/1000	-.000	-.000	-.000**	-.000**
	(0.000)	(0.000)	(0.000)	(0.000)
ΔCENT	-0.003	0.004	-0.003	0.004
	(0.005)	(0.005)	(0.005)	(0.005)
ΔFIRM	-0.003	-0.003	-0.003	-0.003
	(0.006)	(0.007)	(0.006)	(0.007)
ΔWCOUNCIL	0.021***	0.022***	0.029***	0.022***
	(0.008)	(0.007)	(0.010)	(0.008)
Δ(K/L)	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
ΔEAST	-0.015***	-0.016***		
	(0.008)	(0.008)		
Establishments	821	821	821	821
Individuals	333,045	333,045	333,045	333,045
Observations	971,057	971,057	970,545	970,545
Adj. R^2	0.062	0.063	0.062	0.063

Source: LIAB 1995-2001.

Note: The dependent variable is the first-differenced individual log daily wage. Standard errors are in parentheses and are adjusted for clustering at the establishment level. Individual control variables include education (6 categories), a white-collar dummy, tenure, tenure squared, age, age squared. The models include 16 two-digit industry dummies (only individual fixed-effects) and 4 time dummies. ***Significant at 1%-level, **Significant at 5%-level.

upward-bias is found to be relatively larger under centralised agreements, sorting appears to play a major role for firms that are covered by a centralised wage contract. One possible explanation might be that centralised wage contracts lead to a more compressed wage structure across skill groups which causes firms to upgrade the quality of their workforce. For Germany, this is supported by evidence from Dustmann and Schönberg (2004) who find covered firms exhibit a more compressed wage structure and provide more training than uncovered firms. Note that this might lead to higher unobserved worker productivity in such firms and therefore to (relatively larger) upward biased estimates in the simple pooled OLS specification.

As regards the remaining establishment variables, in both specifications establishment size, the works council and the east-west dummy are found to be significantly different from zero and enter the equations with their expected sign. Presumably due to their low variability over time, the collective bargaining dummies and the capital-labour ratio are imprecisely estimated and are for the most part incorrectly signed.

5.4 Spell fixed-effects

Apart from unobserved individual heterogeneity, a further source of bias may be the presence of unobserved establishment effects that are correlated with our profitability measure. In our context, the presence of unobserved establishment heterogeneity may result from neglected capital costs in the quasi-rent measure as well as from differences in technological conditions[17] that are not captured by our control variables. In this case, consistent estimates of the parameters of interest may be obtained by taking differences within each individual-establishment combination (see Abowd et al. 1999). Andrews et al. (2005) label these combinations as individual-establishment-'spells'. Defining $\theta_s = \alpha_i + \phi_{j(i,t)}$ in eq. (2) as the unobserved spell-level effect for spell s, first-differencing of eq. (2) yields:

$$\Delta \ln w_{it} = \beta_\pi \cdot \Delta\pi_{j(i,t)t} + \gamma \cdot \Delta\mathbf{x}'_{it} + \eta \cdot \Delta\mathbf{w}'_{j(i,t)t} + \rho \cdot \Delta\mathbf{q}'_{j(i,t)} + \lambda_t \cdot \Delta D_t + \Delta\theta_s + \Delta\epsilon_{it} \quad (5)$$

Thus, first-differencing of eq. (2) removes θ_s, as long as differencing occurs within each spell. In addition to eliminating individual time-constant characteristics, first-differencing sweeps out time-constant establishment variables $\mathbf{q}'_{j(i,t)}$, so that the coefficient vector ρ cannot be identified either.[18] The extent to which the spell fixed-effects estimates differ from the individual fixed-effects results depends on the fraction of individuals who move between establishments within our sample. In the extreme case of no turnover between

[17] With respect to differences in technologies, firm-specific fixed effects capture e.g. production processes that provide firms with higher rents and which may require compensating wage differentials (e.g. processes involving dangerous work). Such differences might lead to a positive wage-rent correlation which would not be due to rent-sharing (see e.g. Margolis and Salvanes 2001).

[18] Time-constant establishment variables are the east-west and the industry dummies.

sample establishments, spell and individual fixed-effects would yield the same results, and α_i and $\phi_{j(i,t)}$ could not be separately identified. Table 4 reports the distribution of the number of spells. The figures show that the majority of individuals (99.84 per cent) do not move between establishments, only 526 out of 333,045 workers (corresponding to 0.16 per cent) move from one sample establishment to another.[19]

Table 4: Movers and non-movers

	Individuals	Spell per Individual	Spells
Non-movers	332,519	1	332,519
Movers	524	2	1,048
Movers	2	3	6
All	333,045		333,573

Source: LIAB 1995-2001.

Columns (3) and (4) in Table 3 contain the results from the spell first-differenced regressions.[20] As expected, due to the tiny proportion of individuals who change their employer, the estimates do not substantially differ from the individual first-differenced estimates. As in specification (2), quasi-rents are found to exert a significantly lower impact on wages in establishments that are subject to a centralised wage contract (Model (4)). Similar to Model (2), a Wald test fails to reject the null of a zero rent-coefficient (with a p-value of 0.53). Although the point estimate of the interaction term for firm-specific contracts is negative, it is not significantly different from zero. Overall, our findings therefore suggest that centralised wage bargaining suppresses any wage dispersion due to diverging profitability conditions, whereas firm-specific contracts and no collective bargaining coverage allow wages to respond to local profits. Note that with spell level fixed-effects, unobserved heterogeneity captures both individual and establishment effects. An interesting issue would be to recover separate estimates of α_i and $\phi_{j(i,t)}$ and to examine whether unobservably better individuals work in establishments that are characterised by (unobservable) high-wage policies.

[19]The low proportion of movers is due to the fact that the establishment data are a sample of establishments, so that the probability of observing a worker moving from one sample establishment to another is very small. It is important to note that the low proportion of movers does not imply that our data set is restricted to very stable employment relationships as workers (and firms) may enter and exit the panel.

[20]Since differencing requires at least two consecutive time periods within each spell, we need to exclude 448 spells with only one observation per spell. The remaining number of spells is 333,125. Since one observation per spell is lost in first-differencing and 1,587 spells exhibit a gap in their time series, the number of obsevations drops to 970,545.

Table 5: Rent-sharing across various worker groups

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Female	Male	Blue-collar	White-collar	Low-skilled	Medium-skilled	High-skilled
$\Delta\pi$	0.009**	0.018***	0.017***	0.019***	0.008	0.016***	0.029***
	(0.004)	(0.004)	(0.003)	(0.004)	(0.006)	(0.003)	(0.008)
$\Delta\pi$_CENT	-.010	-.015**	-.016***	-.014***	-.010	-.013***	-.020**
	(0.005)	(0.004)	(0.005)	(0.005)	(0.008)	(0.004)	(0.009)
$\Delta\pi$_FIRM	0.003	-.004	-.007	0.000	-.002	-.003	-.003
	(0.006)	(0.005)	(0.004)	(0.006)	(0.008)	(0.004)	(0.009)
ΔSIZE/1000	0.011	0.013**	0.016**	0.009	0.019**	0.016***	0.000
	(0.011)	(0.007)	(0.007)	(0.007)	(0.009)	(0.007)	(0.005)
ΔSIZE^2/1000	-.000	-.000**	-.000**	-.000	-.000**	-.000**	-.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ΔCENT	0.002	0.005	0.002	0.012**	-.009	0.007	0.010
	(0.004)	(0.006)	(0.006)	(0.005)	(0.008)	(0.004)	(0.008)
ΔFIRM	-0.003	-0.003	-0.004	0.003	-0.019	0.001	0.007
	(0.005)	(0.008)	(0.007)	(0.006)	(0.011)	(0.005)	(0.007)
ΔWCOUNCIL	-0.015	0.026***	0.027***	0.007	0.000	0.024***	0.022**
	(0.010)	(0.006)	(0.007)	(0.008)	(0.022)	(0.007)	(0.009)
Δ(K/L)	-.000	0.001	0.000	-.000	0.005	0.001	-0.001
	(0.002)	(0.000)	(0.001)	(0.001)	(0.006)	(0.001)	(0.003)
Establishments	760	807	804	760	588	815	575
Observations	167,177	803,368	614,521	356,024	171,198	688,802	110,545
Adj. R^2	0.105	0.057	0.073	0.068	0.058	0.087	0.053

Source: LIAB 1995-2001.

Note: The dependent variable is the first-differenced individual log daily wage. All variables are spell first-differenced. Standard errors are in parentheses and are adjusted for clustering at the establishment level.

For individual control variables see Tables 2 and 3. All specifications include 4 time dummies.

***Significant at 1%-level, **Significant at 5%-level.

However, owing to the low proportion of movers in our sample we do not pursue this issue further, since for a large number of firms such an identification would have to rely on very little information to obtain estimates of the establishment effects.

5.5 The wage-profit correlation across various worker groups

Our earlier considerations on the individual determinants of rent-sharing suggested that the relationship between wages and quasi-rents might systematically differ across various worker groups. To test this notion, we additionally ran regressions separately by gender, occupation (blue-collar and white-collar workers) and skill-types. Table 5 reports the results for males and females and for blue and white-collar workers. Columns (1) and (2) show the results of the gender-specific regressions. For the female sample, we obtain a coefficient in uncovered establishments which amounts to about 50 per cent of the corresponding point estimate for males. Even though the difference is not statistically significant and may partly be attributed to gender differences in skill composition[21], the lower point estimate for females may be interpreted as weak evidence for a lower rent-extraction of women. Note that this is consistent with former evidence obtained by Arai (2001), Black and Strahan (2001), Nekby (2003) and Martins (2004) among others. Given that the intercept effect of works councils is much more pronounced among male individuals, this finding lends some support to the hypothesis that rent-sharing in uncovered establishments partly results from the local bargaining power of works councils which mainly extract rents on behalf of male workers. As to the interaction terms, the signs of the rent-coefficients exhibit the same pattern as in the pooled regressions. For each group, the null of $\beta_0 = -\beta_{\pi\_CENT}$ cannot be rejected (with p-values of 0.33 for males and 0.98 for females). Thus, industry-wide wage agreements appear to reduce inter-firm wage differentials both for men and women to a similar extent, indicating that the extent of inter-firm wage compression under centralised contracts is stable across both groups. Moreover, similar to the pooled regressions, the interaction terms for firm-specific contracts are found to be not significantly different from zero, and this result holds for either group.

Columns (3) and (4) report the results for blue and white-collar workers. First of all, the point estimate in uncovered establishments is slightly larger for white-collar workers. Even though the estimates do not significantly differ from each other, the higher point estimate may be interpreted as weak evidence for the efficiency wage hypothesis due to diverging supervision intensities. Comparing the intercept effect of works councils in column (3) and (4) shows that this effect is much more pronounced among blue-collar workers. This finding lends some support to the hypothesis that for this group of workers

[21] For example, the share of workers without any vocational degree is 28.2 per cent among female workers and 15 per cent among male workers.

rent-sharing in uncovered establishments partly results from the local bargaining power of works councils. The estimates of the interaction coefficients suggest that centralised contracts appear to reduce rent-sharing particularly among blue-collar workers. While the null of zero rent-sharing under centralised contracts cannot be rejected for blue-collar workers (with a p-value of 0.82), this hypothesis is to be rejected at conventional levels for white-collar workers. This finding confirms the hypothesis that inter-firm wage differentials of blue-collar workers are more likely to be compressed by centralised contracts than those of white-collar workers.

Columns (5) to (7) report separate regression results for different skill-types. As before, in uncovered establishments quasi-rents are found to exert a positive impact on wages. As hypothesized earlier, efficiency wage mechanisms and bargaining power considerations lead us to expect the relationship between wages and quasi-rents to be particularly pronounced among the better educated. This hypothesis is borne out by the estimates, which suggest the profit effect on wages to be larger among higher skill groups. The differential effect is particularly large for high-skilled workers and is found to be significant at the 10 per cent level as compared with the medium-skilled and at the 5 per cent level as compared with low-skilled workers. On the contrary, the estimates for low and medium-skilled workers do not significantly differ from each other. When choosing among the efficiency-wage and works council argument, the results do not appear to favour either of the two explanations. The reason is that, for both medium and high-skilled workers, the intercept effect of works councils is significantly different from zero and of similar magnitude. However, given that the extent of rent-sharing is found to be largest for the high-skilled, we interpret this result as evidence that the efficiency wage hypothesis appears to be somewhat more relevant for this group of workers as compared with their medium-skilled counterparts.

As to the interaction with bargaining coverage, the signs of the rent-coefficients exhibit the same pattern as in the pooled regressions. As before, the interaction effects for firm-specific contracts are found to be very small and not significantly different from zero, and this is true for each skill group. Industry-wide wage agreements seem to reduce inter-firm wage differentials for all skill groups, even though the interaction effect is found to be significant only for medium and high-skilled workers. The overall effect of quasi-rents on wages under centralised contracts is largest for high-skilled workers (with a point estimate of about 0.009). This may be interpreted as evidence for a more pronounced inter-firm wage dispersion among high-skilled workers as compared with their low and medium-skilled counterparts. Note that this finding is supportive of the notion that inter-firm wage differentials of high-skilled workers are less prone to be compressed by centralised wage agreements than those of low and medium-skilled workers, since the latter are more likely to be covered by collective contracts.

Given that 70 per cent of all uncovered establishments in our sample are located in eastern Germany, a further concern might be that the pattern of responses to local profitability conditions is driven by systematic regional differences in wage formation. To investigate this issue, we ran separate regressions for establishments in eastern and western Germany. The regressions yielded coefficients of 0.005, -0.001 and 0.011 for western Germany and 0.017, -0.021 and -0.010 for eastern Germany (for no-coverage, centralised contracts and firm-specific contracts, respectively). Even though the eastern sample is much smaller than the western sample (125,089 versus 845,456 observations), the coefficients for eastern establishments are all significantly different from zero, whereas except for the interaction term on firm-level contracts the estimates for western Germany are found to be insignificant. This exercise leads us to conclude that centralised contracts seem to suppress inter-firm wage differentials in either region, while the profit-sensitivity of wages in uncovered establishments is much more pronounced in eastern Germany.

5.6 Endogeneity bias

Even though the use of quasi-rents instead of profits mitigates the endogeneity problem induced by the negative accounting relationship between wages and profits, our profitability measure might still be endogenous. A first source of bias is a standard simultaneity bias which occurs if wages, output and quasi-rents are jointly determined. In general, the direction of bias can go either way and largely depends on the underlying relationship between output and employment. If there are, for example, decreasing returns in the use of labour, high wages will cause quasi-rents per worker to increase, and this will induce an upward-bias in the estimates of the rent-coefficient (see Abowd and Lemieux 1993). Second, because alternative wages and individual wages are likely to be positively correlated, there will always be some source of downward-bias. The potential endogeneity of the profitability measure raises the question as to whether the pattern of previous results holds if the endogeneity of quasi-rents is accounted for. An important concern is that the invariance of wages against local profitability conditions under centralised contracts is simply caused by a downward-bias due to the endogeneity of quasi-rents. Following Budd et al. (2005), we address this problem by applying the differenced Generalized Methods of Moments (GMM) estimator as proposed by Arellano and Bond (1991). By exploiting available moment conditions around the error term, this estimator instruments endogenous variables with suitable lagged values. In the first-differenced version of eq. (1) any first-differenced endogenous variable, Δx_{it}, is correlated with the error term, Δu_{it}. In the absence of second-order correlation in the error term, x_{it-2} and earlier lags will provide suitable instruments for Δx_{it}, since they will be uncorrelated with Δu_{it}. Note that in our specifications, not only quasi-rents but also their interactions with collective bargaining coverage are likely to be endogenous variables.

Apart from instrumenting endogenous variables by their lagged values in $t-2$ and earlier, the differenced GMM estimator provides an appropriate treatment of predetermined variables which are assumed to be uncorrelated with u_{it} and u_{it+1}, but are correlated with u_{it-1}. As first-differencing causes such variables to become correlated with the error term Δu_{it}, they are instrumented by lagged values in $t-1$ and earlier time lags. In particular, we allow establishment size and the capital-labour ratio to be predetermined in order to capture potential feedback effects from wages in period t on those covariates in subsequent periods. To test the validity of the moment conditions, we present the Sargan/Hansen test of overidentifying restrictions. This test statistic calculates the correlation of the error terms with the instrument matrix and has an asymptotic χ^2 distribution under the null that the moment conditions are valid. Moreover, we report diagnostics for second-order serial correlation of the error terms (testing the null of no second-order serial correlation).

Column (1) in Table 6 holds the results of the differenced GMM estimates.[22] Turning to the main variables of interest, the signs of the rent-coefficients exhibit a similar pattern as the individual and spell fixed-effects estimates in Table 3. While the rent-coefficient is significantly positive for uncovered establishments, wages appear to be less sensitive to rents if establishments are covered by a centralised wage agreement. The overall effect under centralised agreements even appears to be negative, since a Wald test rejects the null of $\beta_0 = -\beta_{\pi\_CENT}$ at conventional levels. Interestingly, the interaction term for firm-specific contracts is estimated to be significantly positive. Note, however, that the overall performance of the GMM estimates turns out to be rather unsatisfactory, since the specification obviously fails to pass the test of overidentifying restrictions and the AR(2) test. The sign of the AR(2) test statistic provides evidence of a negative second-order serial correlation of the (differenced) error terms, suggesting that specification (1) misses out some important unexplained dynamic effects. One possible explanation is that the sluggish adjustment of wages might introduce autoregressive dynamics into wage determination, so that lagged wages should be included as an explanatory variable in the regression (see e.g. Hildreth and Oswald 1997). If this were the case, the omission of the lagged differenced wage would induce a negative correlation between the composite differenced error term including the lagged differenced wage, $\Delta\nu_{it} = \Delta u_{it} + \alpha \cdot \Delta \ln w_{it-1}$, and u_{it-2}, with α denoting the autoregressive coefficient.

To investigate this issue further, we ran an additional specification including the lagged dependent variable as an explanatory variable (reported in column (2)). Since first-differencing causes the lagged differenced wage, $\Delta \ln w_{it-1}$, to become correlated with the differenced error term, $\Delta \ln w_{it-1}$ needs to be instrumented using lagged values in $t-1$ and earlier lagged values. The estimates in column (2) show that the lagged endogenous variable enters the regression with its expected positive sign and is significant at the 1

[22]Because of the low mobility of individuals between sample plants, we confine the presentation to the individual first-differenced estimates.

Table 6: GMM results

Model	*(1)* GMM	*(2)* GMM	*(3)* SYS-GMM
$\Delta \ln w(t-1)$		0.021^{***}	0.345^{***}
		(0.008)	(0.005)
$\Delta\pi$	0.032^{***}	0.020^{***}	0.066^{***}
	(0.003)	(0.003)	(0.003)
$\Delta\pi *$CENT	$-.067^{***}$	$-.044^{***}$	$-.060^{***}$
	(0.003)	(0.003)	(0.004)
$\Delta\pi *$FIRM	0.008^{***}	0.027^{***}	$-.027^{***}$
	(0.003)	(0.003)	(0.004)
ΔSIZE/1000	0.005^{***}	0.011^{***}	0.002^{***}
	(0.000)	(0.001)	(0.000)
ΔSIZE^2/1000	$-.000^{***}$	-.000	$-.000^{***}$
	(0.000)	(0.000)	(0.000)
ΔCENT	0.034^{***}	0.025^{***}	0.092^{***}
	(0.002)	(0.002)	(0.003)
ΔFIRM	-0.020^{***}	$-.022^{***}$	0.063^{***}
	(0.002)	(0.002)	(0.003)
ΔWCOUNCIL	0.017^{***}	0.022^{***}	0.072^{***}
	(0.000)	(0.000)	(0.000)
Δ(K/L)	0.001^{***}	0.001^{***}	0.006^{***}
	(0.000)	(0.000)	(0.000)
Δ(EAST)	$-.047^{***}$	$-.042^{***}$	$-.185^{***}$
	(0.003)	(0.003)	(0.002)
Sargan-$\chi^2(k)$	11,791.74 (69)	6,544.07 (75)	14,126.70 (106)
($p-$value)	0.00	0.00	0.00
AR(2)	-3.49	1.40	5.13
Individuals	333,045	282,002	333,045
Observations	971,057	636,409	971,057

Source: LIAB 1995-2001.

Note: The dependent variable is the first-differenced individual log daily wage. Heteroscedasticity robust standard errors are in parentheses. For remaining covariates see Table 3. Results are reported for the one-step GMM-estimator. ***Significant at 1%-level. **Significant at 5%-level.

per cent level. Moreover, the diagnostic tests indicate that this model is clearly preferable over specification (1). The specification passes the AR(2) test and the Sargan test statistic is considerably decreased, even though it still fails to confirm the validity of all moment restrictions. This may indicate that some of the covariates should not be treated as exogenous or predetermined variables. Treating establishment size and the capital-labour ratio as endogenous instead of predetermined variables decreased the Sargan test somewhat (from $\chi^2\,(75) = 6,544.07$ to $\chi^2\,(63) = 5,990.85$), but still led to a rejection of the validity of all moment restrictions. Note that a possible explanation for the poor performance of the Sargan test might relate to the existence of heteroskedasticity since simulation results of Arellano and Bond (1991) indicate that the Sargan test tends to reject too often in this case.

A further concern with Model (2) is that the autoregressive coefficient of 0.02 appears to be implausibly low, suggesting that Model (2) still fails to capture the full extent of autoregressive dynamics.[23] Given this low point estimate, column (3) reports results using the System-GMM (SYS-GMM) estimator as proposed by Arellano and Bover (1995). This estimator is motivated by the problem that lagged levels of a variable are likely to be weak instruments for the equation in first-differences if the individual time series exhibits near unit root properties. Closer inspection of the time-series properties of the explanatory variables reveals that particularly the size variable and the capital-labour ratio appear to be close to a random walk.[24] The SYS-GMM estimator exploits additional moment conditions for the equation in levels using lagged differences as instruments in the levels equation. In particular, predetermined variables are instrumented by contemporaneous first-differences in the levels equation, whereas endogenous and lagged dependent variables are instrumented by lagged first-differences (Bond 2002). The estimates in column (3) show that the coefficient on the lagged wage turns out to be considerably larger than the differenced GMM estimate, suggesting that the latter might be severely downward biased. However, the diagnostic tests indicate that the specification fails to pass the test of overidentifying restrictions and the AR(2) test. Even though the SYS-GMM estimates appear to perform rather unsatisfactorily, the pattern of the rent-coefficients in column (1) through (3) clearly indicates that the point estimates under centralised contracts become increasingly larger once the full extent of the autoregressive dynamics is accounted for. In Model (3), a Wald test even rejects the null of a zero rent-coefficient under centralised contracts (with a p-value < 0.01). However, the overall point estimate of about 0.006 turns out to be very small.

[23]Studies based on firm-level data report autoregressive coefficients in the range of 0.2 to 0.6 (van Reenen 1996, Hildreth and Oswald 1997), and comparable studies using matched worker-firm data report coefficients in the range of 0.4 (Guiso et al. 2006).

[24]SYS-GMM estimates of a simple AR(1)-process yield a coefficient of about 0.94 for establishment size and of 0.91 for the capital-labour ratio.

Taken together, the GMM estimates appear to preserve the pattern of results obtained by the fixed-effects estimates and point to a remarkably stable pattern of the responsiveness of wages to profits. In particular, the estimates indicate that the effect of rents on wages is significantly larger under firm-specific contracts and in uncovered establishments as compared with establishments that are covered by an industry-level contract. However, when comparing firm-level contracts with uncovered plants, the results are less clear-cut. While the differenced GMM-estimates in Table 6 appear to confirm our hypothesis that firm-level contracts should enable strong sector unions to skim off a larger share of rents than works-councils in uncovered establishments, the SYS-GMM estimates point to a negative interaction coefficient. Overall, the established pattern partly corroborates our results from recent work on the basis of establishment level data where we failed to detect any significantly positive relationship between wages and establishment-specific quasi-rents under centralised contracts. However, in finding a positive amount of rent-sharing under firm-specific contracts, the present results stand in contrast to our earlier findings from the establishment-level estimations, which pointed to the complete insensitivity of wages to local conditions under firm-specific contracts (Guertzgen 2005). Note that this difference may partly be attributed to the more precise and encompassing information on wages in the LIAB data, where wages are measured inclusive of fringe-benefits or bonus payments.

Given the coefficients of 0.015 to 0.066 and mean quasi-rents per employee of 0.73, the elasticity of individual wages with respect to quasi-rents is of the magnitude 0.01 to 0.048 in uncovered plants. In establishments subject to a firm-level contract, elasticities range from 0.016 to 0.057 (with coefficients ranging from 0.013 to 0.047 and a mean value of 1.22). In terms of the economic significance of the estimates, our elasticities imply that a doubling of per-capita quasi-rents raises wages by about 1 to 4.8 per cent in uncovered plants and by 1.6 to 5.7 per cent under firm-level contracts. Moreover, calculating the share of variance in the distribution of wages due to the variability in quasi-rents, it can be shown that the variability in per-capita rents explains about 2.9 to 12.7 per cent of the variability in log wages in uncovered plants and about 3.5 to 12.5 per cent under firm-level contracts.[25]

It is interesting to note that our estimated elasticities are all within the range found in other studies on rent-sharing using linked employer-employee data: Margolis and Salvanes (2001) find elasticities between 0.002 and 0.03 for France and corresponding values of 0.006 between 0.01 for Norway. The relative magnitude of these elasticities largely

[25]This calculation is performed under the assumption that 95 per cent of the mass of a symmetric distribution is within plus or minus 2 standard deviations of the mean. Given the descriptive statistics in Table A1 the contribution of the variability of quasi-rents to the variability of log wages can then be calculated as:
$\frac{\beta_\pi(\overline{\pi}+2\sigma_\pi)-\beta_\pi(\overline{\pi}-2\sigma_\pi)}{(\overline{\ln w}+2\sigma_{\ln w})-(\overline{\ln w}-2\sigma_{\ln w})} = \frac{\beta_\pi \cdot \sigma_\pi}{\sigma_{\ln w}}$ (see e.g. Margolis and Salvanes 2001).

reflects differences in bargaining institutions in both countries, with firm-level bargaining prevailing in France and a two-ladder system with sector-level bargaining and subsequent firm-level negotiations being predominant in Norway. A similar system prevails in Sweden, which is consistent with comparable estimates obtained by Arai (2003), who reports an elasticity of 0.01. Finally, Martins (2004) reports elasticities ranging from -0.031 to 0.078 for Portugal, which is characterised by a mixed bargaining system of sectoral, single-firm and multi-firm contracts.

6 Summary and Conclusions

The aim of this paper was twofold: First, we have addressed the question of whether German wages respond to firm-specific profitability conditions and second, we looked at whether the sensitivity of wages to firm-specific rents depends on collective bargaining coverage. Theoretical considerations lead us to expect collective contracts either to suppress firm-level rent-sharing or to lead to a larger extent of rent-sharing relative to uncovered firms. Under industry-wide agreements, the latter hypothesis depends on the extent to which the bargaining parties exploit flexibility provisions which have recently become a widespread element of central wage agreements. Since direct information on the use of flexibility provisions under industry-wide wage agreements is unavailable in our data set, we take our empirical findings as indirect evidence of whether the use of such provisions is a quantitatively important phenomenon in Germany.

Using linked employer-employee data from the mining and manufacturing sector, our empirical analysis offers a remarkably consistent picture: Individual wages are found to be positively related to quasi-rents, but this seems to be confined to the non-union sector and to establishments subject to firm-specific contracts. Industry-wide wage contracts, however, appear to be associated with a significantly lower responsiveness of wages to local profitability conditions. While pooled OLS estimates yield a positive correlation between wages and quasi-rents under centralised contracts, estimates accounting for unobserved individual and establishment heterogeneity point to a coefficient of zero. Moreover, the pooled OLS upward-bias is found to be relatively larger under centralised contracts. This finding is indicative of the presence of unobserved factors that are positively related with profits and impact positively upon wages, and which are particularly relevant under centralised contracts. One such factor may be that a compressed wage structure under centralised wage contracts causes firms to upgrade the quality of their workforce. This might lead to higher unobserved worker productivity in such firms and therefore to (relatively larger) upward biased estimates in the pooled OLS specification. Differenced GMM and SYS-GMM estimates accounting for the endogeneity of our profitability measure preserve the pattern of results obtained by the pooled OLS and fixed-effects estimates.

In examining the impact of collective bargaining coverage on the wage-profit relationship, our findings therefore suggest that centralised wage bargaining is associated with significantly lower responsiveness of wages to firm-specific profitability conditions. We interpret this finding as evidence that the use of flexibility provisions in central wage agreements appears to be empirically negligible. Even though firms may pay wages above the going rate and may make use of opt-out clauses, the potential for positive adjustments to local profitability conditions seems to be largely unused. To reconcile this result with the fact that a considerable fraction of firms covered by a collective contract pay wages above the going rate, we conclude from our findings that such wages do not arise from more favourable profitability conditions, but rather reflect observable and unobservable differences in worker productivity.

Consistent with our hypotheses that the extent of inter-firm wage compression under centralised contracts ought to be particularly pronounced among those workers who are likely to be covered by collective contracts, we find the wages of low and medium-skilled as well as blue-collar workers to be most insensitive to local profits. In uncovered establishments, we find that skilled and white-collar workers benefit to a larger extent from their employers' ability-to-pay than do unskilled and blue-collar workers. Moreover, male workers receive a larger share of rents than their female counterparts. These findings lend support to the hypothesis that rent-sharing in uncovered plants may result from the bargaining power of works councils and efficiency wage mechanisms.

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A Appendix

A.1 Summary Statistics by Bargaining Coverage:

Variable	Mean	Std.-Dev.	Mean	Std.-Dev.	Mean	Std.-Dev.
	Centralised contract		Firm-level contract		No coverage	
Individual characteristics						
ln$w$	5.26	0.31	5.19	0.36	4.93	0.41
FEMALE	0.17	0.38	0.18	0.39	0.26	0.44
AGE	39.07	8.75	39.35	8.67	39.16	8.59
TENURE	146.34	85.95	129.03	84.75	94.22	71.74
FOREIGN	0.11	0.31	0.06	0.23	0.06	0.24
WHITECOLL	0.36	0.48	0.41	0.49	0.32	0.47
VOCATIO	0.67	0.47	0.70	0.46	0.70	0.46
HIGHSCHOOL	$4.5e^{-03}$	0.07	$3.0e^{-03}$	0.06	$3.8e^{-03}$	0.06
VOC-HIGH	0.03	0.17	0.03	0.18	0.03	0.16
TECHN-UNI	0.06	0.23	0.06	0.24	0.06	0.23
UNI	0.06	0.23	0.05	0.23	0.05	0.21
Establishment characteristics						
π	1.09	0.87	1.22	0.96	0.73	0.79
SIZE	8,493.49	14,149.43	1,855.77	1,841.42	640.56	768.29
WCOUNCIL	0.98	0.14	0.97	0.16	0.72	0.45
K/L	1.75	2.19	4.00	11.36	2.20	4.86
EAST	0.11	0.31	0.25	0.44	0.48	0.50
Individuals	299,585		39,943		22,672	
Establishments	582		185		310	
Observations	1,152,080		105,640		47,985	

Table A1: Summary statistics by bargaining coverage

Source: LIAB 1995-2001.

Note: Per-capita quasi rents and the capital-labour ratio are measured in 100,000 DM.
1 € corresponds to 1.95583 DM. All variables are averaged over individual observations.