Using Social Norms to Estimate the Effect of Collective Bargaining on the Wage Structure

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January 20, 2011

JOB MARKET PAPER

Abstract: This paper examines the causal effect of collective bargaining on the structure of wages. Based on a detailed linked employer-employee data set, I estimate the Marginal Treatment Effects (MTE) as introduced by Heckman and Vytlacil (2005) and then recover the Average Treatment Effect (ATE) and the Average Treatment Effect on the Treated (ATET) for the level of wages. In addition, using the approach of Chen and Khan (2010), I estimate the ATE of collective bargaining on the dispersion of wages. This is the first paper for Germany providing causal evidence of collective bargaining accounting for selection on unobservables. The results suggest that the union wage effect is close to zero; the ATE equals 0.8 log points. In contrast, collective bargaining has a diminishing effect on inequality. The ATE on wage inequality yields that collective bargaining lowers the conditional standard deviation of log wages by 26%. Thus, while the causal impact of unions on the level of wages is very small, unions are indeed successful in compressing the wage structure. This may indicate that unions concentrate their efforts on reducing wage inequality.

Keywords: Union wage effect, Wage compression, Collective Bargaining, Marginal treatment effects, Pairwise matching

JEL-Classification: J31, J51, J52

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1 Introduction

The presence of unions can impose costs on employers, most importantly by altering the wage structure. If a positive union wage premium is not mirrored by productivity advantages of employees covered by a collective bargaining agreement compared to workers not being covered, then this may lead firms to reduce employment. This may also be the case if collective bargaining does not allow for sufficient wage flexibility.

A substantial body of research has documented a positive union wage differential as well as a lower dispersion of wages among workers which are employed in firms applying collective bargaining agreements. For the U.S., the U.K., and Canada the union wage premium in 2001 was between 15 log points (U.S. and Canada) and 5 log points (U.K.), while the residual variance of log wages appeared to be about 30% lower in the unionized sector (see e.g. Card et al., 2003). However, the union wage premium has not been stable over time and has been decreasing during the last decades (Blanchflower and Bryson, 2004). In fact, DiNardo and Lee (2004) challenge the finding of a positive union wage premium altogether. Using a sharp regression discontinuity design approach and comparing U.S. firms where unions barely won a union certification election (giving the union the right to bargain over wages with the employer) with firms where such an election was barely lost, the authors note that the impact of unionization in recent years on the average wage has been very close to zero.[1] Frandsen (2010), using similar but more extensive data, corroborates this result, but finds at the same time that unions are successful in reducing wage inequality.[2] Thus, low wage workers use unions to improve their wage position at the expense of high wage workers.

For Germany, estimates for the (conditional) union wage premium are about 3-6 log points and an extensive literature has documented the wage compressing effect of collective wage bargaining (Burda et al., 2008; Antonczyk et al., 2010, and the literature cited therein). However, while for the U.S. some attempts to estimate the causal effect of collective bargaining on the structure of wages have been carried out, to the best of my knowledge there does not exist any study focussing on Germany which estimates heterogeneous treatment effects while accounting for the possible endogeneity of *collective wage bargaining*. This is most likely due to a lack of credible instrumental variables in the commonly used data sets (Dustmann and Schönberg, 2009).

[1] In the U.S. case, unionization does not in all cases lead to collective wage bargaining agreements, but is in general a necessary first step towards reaching such an agreement. This is in contrast to the German case, where such elections do not take place. Hence, transferring the regression discontinuity design approach for estimating the union wage premium for Germany is impossible.

[2] Both studies concentrate on the private sector, where unionization rates have been declining over the recent decades. In contrast, U.S. unionization rates in the public sector have actually been increasing (Card, 2001). The overall union density in the U.S. in 2001 was about 14%, with higher rates in the public sector (37%) and lower rates in the private sector (9%) (Blanchflower and Bryson, 2004).

This paper estimates the causal effect of collective bargaining agreements on the wage structure for the case of Germany. Using a large linked employer-employee data set, which is supplemented with two sets of instrumental variables, I allow for the general case of heterogeneous treatment effects when estimating several treatment effects on both the level and the dispersion of wages. The present paper contributes to the literature by addressing the following questions: What is the causal impact of collective bargaining on the level of wages? What is the effect of collective bargaining on wage inequality? In what direction do selection effects work, i.e. can positive selection of workers into the unionized sector of the economy explain the positive union wage premium observed unconditionally? Moreover, do workers whose unobserved earning skills are more dispersed systematically select themselves into either one of the two bargaining regimes, thereby biasing the results for the effect of collective bargaining on wage inequality?

To answer these questions, I use the 2001 cross-section from the German Structure of Earnings Survey. This is a linked employer-employee data set, which is very well suited for the purposes of this paper, since it provides very precise information on hourly wages and a rich set of covariates on both workers and establishments characteristics. In particular, information is available on whether a collective wage bargaining agreement applies to a worker. The German system of collective wage bargaining differs in several aspects from those in the anglo-saxon countries. Traditionally in Germany, wages are determined by collective bargaining between unions and employers' associations ("Arbeitgeberverband") at the industry level (sectoral collective contract or "Flächentarifvertrag").[3] These agreements are only binding for firms belonging to such an employers' association.[4] Discrimination against non-union-members is legally forbidden, thus all employees in a firm recognizing a bargaining contract – and not only union members in this firm – benefit from the outcome of the collective agreements. Moreover, a firm may recognize a bargaining contract without being legally required to do so. This implies that coverage rates are much higher than the union density among employees (Fitzenberger et al., 2010).[5] In addition, union wages are likely to act as (sector-specific) minimum wages (in the unionized part of the sector), while payment above union wages is common (Dustmann and Schönberg, 2009).

When estimating the union wage premium, one is likely to be confronted with selection

[3]Bargaining at the firm or plant level ("Firmentarifvertrag" or "Betriebsvereinbarung") – which is much closer to the prevalent system in the U.S. or the U.K. – also exists but covers a much smaller share of employees and firms and is not being considered in the present study.

[4]This stands in contrast to other European countries, such as Spain and Italy, where sectoral agreements are automatically extended to all firms in the industry.

[5]In 2001, about 70% of the employees are covered by a collective wage bargaining agreement, while union membership was about 20% percent (Fitzenberger et al., 2010). This is in stark contrast to the U.S. case, where the differential between coverage and membership is typically less than 2% (Card et al., 2003).

bias. There may be selection – both on observable and unobservable characteristics – from the employer's side as well as from the worker's side. That is, perhaps only highly profitable firms join an employer's association for then to apply collective wage bargaining contracts. Likewise, it could be the case that only very productive workers decide to join firms which apply collective wage bargaining contracts. For Germany, Fitzenberger et al. (2008) suggest positive selection of covered firms and negative selection of covered workers regarding observable characteristics. Considering unobservables, there can be "classical" selection, i.e. that "more able" workers select themselves into covered establishments.[6] More generally, it is possible that individuals may decide to join these firms based, at least partly, upon expected idiosyncratic gains, which are observed by the individual but not by the data-analyst, leading to the case of *essential heterogeneity*. That is, employees react to the treatment differently *and* selection into treatment is based upon this knowledge. Furthermore, just as for the level of wages, the selection process of either firms or workers into the collective wage bargaining regime may lead to a situation where unobserved earning skills are more diversified e.g. among the group of workers who collectively bargain over wages with the employers. In this case, even comparing the unconditional wage dispersion or the residual variances between the treated and the untreated is not sufficient to estimate the causal effect of the treatment *collective bargaining* on wage inequality.

This paper investigates the causal relationship between the wage structure and collective wage bargaining. Allowing for *essential heterogeneity*, I apply a marginal treatment effects approach in order to recover several treatment effects (ATE, ATET, LATE) for the level of wages. In addition, using a pairwise matching procedure, I estimate the ATE of collective wage bargaining on wage inequality. Both methods make use of two sets of instrumental variables in order to provide the necessary exogenous variation for the treatment variable *collective wage bargaining*: these are share variables of religious affiliation and the historic gross union density from 1961; both variables are measured at the district level.[7] These instrumental variables influence the propensity to apply collective wage bargaining agreements and are plausibly exogenous.[8] In particular, several studies on Germany (Becker and Wößmann, 2009; Cantoni, 2009; Arruña, 2010) find no direct impact of religious affiliation on economic outcomes. Controlling for the industry structure in 2001, it is credible that the only channel through which the historic gross union

[6]Guertzgen (2010), concentrating on a selective set of sectors, applies panel estimation techniques and compares (relatively few) firms which switch their bargaining status with firms staying in their original bargaining regime. Her results suggest that the union wage premium is statistically not significant.

[7]The historic gross union density is approximated by using membership rates of the largest German trade union; the "IG Metall". This union covers broadly the metal producing sector. In order to minimize possible confounding effects, certain industries, which are possibly directly influenced by this union, are excluded from the analysis.

[8]These data stem from data sources outside the linked employer-employee data and are merged to the original data, as described in detail in the next section.

density affects the present structure of wages is by providing variation in the propensity to apply collective wage bargaining agreements.

The results suggest that the causal effect of unions on the level of wages is very close to zero; the Average Treatment Effect (ATE) equals 0.8 log points, which is about 3 log points below standard OLS estimates and slightly below the estimate for the Average Treatment Effect on the Treated (ATET). This indicates that workers positively select themselves into establishments applying collective wage bargaining agreements based on unobservable characteristics and that the selection is based upon the knowledge of idiosyncratic gains. The observable characteristics explain about half of the unconditional 7.3 log points union wage premium. Thus, positive selection based upon observable characteristics also takes place. Comparing the unconditional standard deviations of log wages between the two bargaining regimes shows that it is 18% lower for covered workers. Controlling for observable characteristics via standard OLS raises this effect to 20%. Finally, the ATE on wage inequality is even larger, suggesting that the causal impact of collective bargaining on inequality lowers the dispersion of wages by 26%. Thus, unions are successful in compressing the wage structure. In addition, workers whose observable as well as unobservable characteristics involving high wage dispersion select themselves into the collective bargaining regime. Overall the results suggest that the major part of the positive union wage premium documented so far in the literature is explained by selection effects. In contrast, unions seem to be successful in compressing the wage structure. This may indicate a concentration of efforts by unions towards a less dispersed wage distribution in the unionized part of the economy.

The remainder of the paper proceeds as follows: Section 2 describes the data. The third section presents the econometric approaches for estimating the causal effect of collective wage bargaining on both the level and the dispersion of wages. Section 4 introduces the instrumental variables. The empirical results are discussed in section 5. Section 6 provides concluding remarks. The appendix contains detailed descriptive statistics of the data.

2 Data and Basic Empirical Facts

The present study uses the 2001 cross-section of the German Structure of Earnings Survey (GSES; "Gehalts- und Lohnstrukturerhebung"), a large mandatory linked employer-employee data set, which is very reliable due to its compulsory character.[9] The data set is based on a random sample from basically all German firms in the private sector with at least ten employees. Certain industries, such as the educational and the health sector,

[9]This and other waves of the GSES have been frequently used to analyze wage differences across bargaining regimes. See among others Gerlach and Stephan (2006); Heinbach and Spindler (2007); Fitzenberger et al. (2008); Burda et al. (2008); and Antonczyk et al. (2010).

are excluded. Sampling weights are provided in order to make the sample representative for all employees potentially covered by this survey.

These data allow for a very detailed analysis of the wage distribution because of the link between employer-specific information and employee information and because of its large size. Two further advantages of the GSES, standing in contrast to the IAB linked employer-employee data set (LIAB; used e.g. by Dustmann et al., 2009; Guertzgen, 2009), are that hours of work are reported and that earnings are neither truncated nor censored (Kohn and Lembcke, 2007; Antonczyk et al., 2010). In addition, the GSES provides precise information on whether an employee is covered by a sectoral bargaining agreement: Following Burda et al. (2008) and Antonczyk et al. (2010), I define a covered employee as anybody working in a covered establishment, i.e. an establishment that pays at least one percent of its employees according to a collective wage bargaining agreement.[10]

This study focuses on prime age male employees in West Germany.[11] Employees currently taking part in vocational training or an internship are dropped, as well as all employees younger than 25 or older than 55 years of age; virtually all men in the remaining core age group are attached to the labor force. I only analyze employees working full time, i.e. those paid at least 30 hours per week including overtime in October 2001. The final sample involves 210,471 employees (132,833 workers are covered and 77,638 workers are not covered) in 18,120 establishments. Calculating the weighted average of the binary treatment variable *collective wage bargaining* reveals that 71.2% of the workers are covered by such an agreement.

The wage is defined as October earnings including overtime pay and bonuses for Sunday or shift work, divided by hours paid in October including overtime hours.[12] For plausibility, I limit working hours to a maximum of 304 hours per month and the hourly wage to values between 4 and 70 euro per hour.[13] As outcome variable, the log gross hourly wage are used.

Table 1 provides detailed definitions of the variables of the data set; comprehensive descriptive statistics can be found in table 2.

[Tables 1 and 2 about here]

Employees working in establishments applying collective wage bargaining agreements

[10]The negotiated wages in the collective agreements act as minimum wage for non-covered individuals in covered firms, see Fitzenberger et al. (2008) for evidence along this line. Also note that the share of covered workers in covered establishments lies typically above 50% and most covered firms exhibit coverage rates above 90% (see again Fitzenberger et al., 2008).

[11]In light of the heterogeneity in wage trends between West and East Germany, (see e.g. Kohn and Lembcke, 2007; Gernandt and Pfeiffer, 2007; Orlowski and Riphahn, 2009), the analysis is restricted to West Germany.

[12]It is important to include premia as those are often regular and important wage components (Fitzenberger et al., 2008).

[13]Both bounds together correspond to less than 0.3% of the wage distribution in 2001.

are on average older and have higher tenure. The covered establishments tend to be larger[14] and the employees work less hours per month.[15] The raw union wage gap is slightly above 10 log points and wages are less dispersed for workers in covered firms. Figure 1 shows the distribution of wages by bargaining regimes. The distribution is steeper for the group for which a collective wage bargaining agreement does not apply, indicating a greater wage dispersion. Up to the 95% percentile the wage distribution for covered workers lies above the distribution for non-covered workers; above this point non-covered workers in fact earn higher wages. Table 3 summarizes the wages by skill group and bargaining regime. For all skill groups, the wages are on average higher for workers covered by a collective bargaining agreement.[16] In addition, the skill premia between medium- and low-skilled workers covered by collective wage bargaining agreements is lower than for workers not being covered, which perfectly is in line with the results by Dustmann and Schönberg (2009). Maybe somewhat surprisingly though, the skill-premia between the high- and the medium-skilled are equal across the two bargaining regimes. This could point into the direction of wage compression from below in the unionized sector of the economy.[17]

[Figure 1 and table 3 about here]

In order to further investigate differences in wage dispersion between the unionized and the non-unionized sector, it is helpful to describe where covered and uncovered workers actually find themselves in the unconditional wage distribution. Therefore, figure 2 plots the share of workers covered by collective wage bargaining agreements and those not covered along the deciles of the distribution of wages. Only at the lowest decile, the share of workers not being covered exceeds the share of workers being covered; i.e. more than half of the workers located at the very bottom of the overall wage distribution are not covered by a collective wage bargaining agreement. The largest shares of collective bargaining agreements are located in the upper middle of the wage distribution, between the median and the 90% quantile; this is where covered workers are strongly overrepresented.

[Figure 2 about here]

[14]The positive correlation between firm size and the probability of being covered by a collective bargaining agreement has also been established based upon the LIAB data set (see e.g. Guertzgen, 2010).

[15]Interestingly, for the U.S. Farber (1999) documents that the likelihood for firms to apply collective wage bargaining agreements is inversely related to the firm's size, standing in contrast to the case of Germany.

[16]As an exception, wages for those workers for which information on education is not available are slightly better of under individual bargaining.

[17]Guertzgen (2009) reports that wages of workers in firms applying collective bargaining contracts are much less sensitive to firm-level profitability, compared to firms in the non-unionized sector, which also suggests lesser wage dispersion.

3 Empirical Approach

This section introduces the empirical approaches to estimate the effect of the treatment *collective wage bargaining* on the (i) level of wages and on the (ii) dispersion of wages.

3.1 Collective wage bargaining and level of wages

I consider two possible treatment states for each individual. The treated state ($D = 1$), i.e. collective wage bargaining applies to an individual, and the non-treated state ($D = 0$), where collective wage bargaining does not apply, and the employer and the employee individually bargain over wages. Let Y_1 denote the wage of individual if treated and Y_0 his wage if not treated. These wages depend on a vector of observable characteristics X and an unobservable component U, varying by the treatment status, such that

$$\begin{aligned} (1) \quad Y_1 &= \mu_1(X) + U_1 \\ Y_0 &= \mu_0(X) + U_0 \end{aligned}$$

where μ_1 and μ_0 are general functions of the covariates X, which are observed by the analyst and the individual, while U_1 and U_0 are known by the individual but not the analyst, and in general $U_1 \neq U_0$. The individual gain from treatment $\Delta = Y_1 - Y_0$ contains two components: The Average Treatment Effect and the idiosyncratic gain from treatment $U_1 - U_0$, such that the treatment effect may vary across persons. In principle it can be the case that after controlling for X, the difference $Y_1 - Y_0$ is the same for all individuals. More likely however, it differs among individuals even after controlling for X, implying heterogeneous treatment effects.

The treatment dummy D equals 1 if the individual is treated and 0 otherwise. Using D and the fact that analysts observe either Y_1 or Y_0 for individuals, but never both at the same time, the above expressions can be summarized to

$$\begin{aligned} (2) \quad Y &= DY_1 + (1-D)Y_0 \\ &= D\mu_1(X) + U_1 + (1-D)\mu_0(X) + U_0 \\ &= \mu_0(X) + U_0 + D(\mu_1(X) - \mu_0(X)) + D(U_1 - U_0) \\ &= \mu_0(X) + DE(\Delta|X) + \underbrace{D(U_1 - U_0) + U_0}_{=\text{Error term}}. \end{aligned}$$

Assume that an individual can choose to either undergo treatment or not. Let D^* denote his net utility when being treated. The individual will undergo treatment if D^* is positive.

$$(3) \quad D^* = \mu_V(Z, X) - V, \quad E(V) = 0, \quad D = 1(D^* > 0)$$

where Z and X are observed factors, while V is unobserved and is independent of Z given X ($V \perp\!\!\!\perp (Z|X)$), as are U_1 and U_0. Z plays the role of an instrumental variable and provides exogenous variation in the choice of the agent to whether undergo treatment or not. $\mu_V(Z,X)$ denotes the expected value of gross utility when undergoing treatment, given the observed factors. The error terms in the outcome equation do not have to be independent of the error term in the choice equation, i.e. $(U_1, U_0) \not\perp\!\!\!\perp V|X$. Rewrite the treatment dummy D in the following way:

$$\begin{aligned} (4) \quad D &= 1[F_V(\mu_V(Z,X)) > F_V(V)] \\ &= 1[P(Z,X) > U_D] \end{aligned}$$

where F_V is the cumulative distribution function of V[18] and $P(Z,X) = F_V(\mu_V(Z)) = P(D=1|Z,X)$. Thus, the propensity score, which includes the exogenous variables z, is the probability of selecting treatment

$$\begin{aligned} (5) \quad P(z,x) &= Pr(D=1|Z=z, X=x) \\ &= Pr(P(Z,X) > U_D) \end{aligned}$$

Given the independence $V \perp\!\!\!\perp (Z|X)$, changing the instrumental variable Z in a certain direction shifts all individuals towards the same direction – this is the standard IV monotonicity assumption.[19]

Simply estimating the union wage premium via OLS would in general not yield a causal parameter. A standard regression model boils down to comparing the adjusted mean outcomes for the treated and the untreated, that is

$$\begin{aligned} & E(Y|X=x, D=1) - E(Y|X=x, D=0) \\ = \; & \mu_1(X) - \mu_0(X) + E(U_1|X=x, D=1) - E(U_0|X=x, D=0) \\ = \; & \mu_1(X) - \mu_0(X) + E(U_1 - U_0|X=x, D=1) \\ + \; & E(U_0|X=x, D=1) - E(U_0|X=x, D=0). \end{aligned}$$

This study focusses on the Average Treatment Effect (ATE) as well as the Average Treatment Effect on the Treated (ATET). Consider the ATE

$$ATE(x) = \mu_1(x) - \mu_0(x).$$

[18] It follows that $F_V(V) = U_D \sim Uniform(0,1)$.

[19] Vytlacil (2002) demonstrates that the LATE assumptions (independence and monotonicity) are equivalent to the assumption of an unobserved index crossing a threshold which defines selection into treatment. In particular, the author notes that "given the LATE assumptions, there always exists a selection model that rationalizes the observed and counterfactual data."

This is the expected gain of treatment for a randomly chosen individual with observable characteristics $X = x$. By taking the difference between the two potential outcomes in expression (1) we obtain $\Delta = Y_1 - Y_0 = ATE(X) + (U_1 - U_0)$. The second part is the idiosyncratic gain for a particular individual receiving the treatment. In a randomized experiment, $E(\Delta|X = x) = \mu_1(x) - \mu_0(x)$. The ATET is the expected gain for individuals undergoing treatment:

$$ATET(x) \quad = E(\Delta|X = x, D = 1) = \mu_1(x) - \mu_0(x) + E(U_1 - U_0|X = x, D = 1)$$

From this expression it can be inferred that the only reason why the ATE and the ATET differ is because the idiosyncratic gains differ across treatment status. If workers self-select into firms based on potential unobserved gains, essential heterogeneity occurs, which implies $U_1 \neq U_0$ and that the choice of treatment D and the difference between the idiosyncratic gains in both treatment statuses $(U_1 - U_0)$ are not statistically independent, so even conditional on X the effect of treatment may differ across individuals.[20]

The present study draws on the marginal treatment effect literature. Marginal treatment effects were first introduced by Björklund and Moffitt (1987) and further developed by Heckman and Vytlacil (1999, 2001, 2005).[21] These estimation techniques are particulary well suited for recovering various treatment effects in the case of response heterogeneity and self-selection. In the present study, individuals may select themselves into firms which either pay or do not pay wages which are collectively bargained over, based on their individual expected idiosyncratic gains or losses of working in such a firm. Likewise, firm owners may decide to either join or not join an employer's association for then to apply collective bargaining agreements. If these idiosyncratic gains are correlated with the treatment, the standard 2SLS model only identifies the average treatment effect for those individuals choosing treatment because the instrumental variable takes on certain values – these are the so-called compliers, who cannot be identified in general – and thus a Local Average Treatment Effect (LATE) is estimated. This LATE cannot be interpreted on its own as it is directly tied to the instrument(s), and in the general case of treatment effect heterogeneity neither equals the ATE nor the ATET.

To overcome the problems posed by the presence of essential heterogeneity, I use Marginal Treatment Effects, which can be used to recover the Average Treatment Effect as well as the Average Treatment Effect on the Treated. The Marginal Treatment Effect (MTE) is defined as the average treatment effect evaluated for the subpopulation which is indifferent between either receiving treatment or not, with characteristics $X = x$ and

[20] Basu et al. (2007) define *essential heterogeneity* as the case in which unobserved heterogeneity and selection into treatment based on unobserved gains is present.

[21] Heckman et al. (2006) and Basu et al. (2007) provide excellent illustrations of how to use these techniques in different economic applications.

$Z = z$. Thus, it can be interpreted as the limit version of the LATE. Formally, the MTE is defined as

$$
\begin{aligned}
(6) \quad MTE(x,z) &= E(\Delta|X=x, Z=z, D^*=0) \\
&= E(\Delta|\mu_V(z,x)=v) \\
&= \mu_1(x) - \mu_0(x) + E(U_1 - U_0|P(Z,X)=u_D).
\end{aligned}
$$

This expression shows that the MTE is identified over the support of $P(Z,X)$. Once the marginal treatment effects are estimated, they can be used to calculate specific weighted averages which identify several treatment effects (ATE, ATET, LATE) (see Heckman and Vytlacil, 2005). Table 4 summarizes these results. The parameter $ATE(x)$ simply integrates $MTE(x,u_D)$ over the entire support of u_D. $ATET(x)$ oversamples $MTE(x,u_D)$ for those individuals with low values of u_D, making them more likely to receive treatment based on unobservable characteristics.[22] In the following the steps of implementing the estimation procedure of the MTE are presented.

[Table 4 about here]

I rely upon the Local Instrumental Variable (LIV) (see e.g. Heckman and Vytlacil, 1999) estimator to first determine the MTE, which then is used to recover the treatment parameters of interest. Taking the conditional expectation of Y with respect to X and $P(Z,X)$ in equation (2) yields

$$
\begin{aligned}
(7)\, E(Y|X=x, P(Z,X) &= P(z,x)) \\
&= E(DY_1 + (1-D)Y_0|X=x, P(Z,X)=P(z,x)) \\
&= \mu_0(x) + P(z,x)(\mu_1(x)-\mu_0(x)) + E(U_0|P(Z,X)=P(z,x)) \\
&\quad + P(z,x)E(U_1 - U_0|P(Z,X)=P(z,x), D=1) \\
&= \mu_0(x) + P(z,x)(\mu_1(x)-\mu_0(x)) + K(P(z,x)),
\end{aligned}
$$

where $K(P(z,x))$ is a general function of the propensity score.

To identify the MTE, I take the derivative of $E(Y|X,Z)$ with respect to the propensity score $P(Z,X)$. This derivative is the LIV.

$$
\begin{aligned}
(8) \quad \frac{\partial}{\partial P(z,x)} E(Y|X=x, P(Z,X)=P(z,x)) &= E((Y_1 - Y_0)|X=x, u_D = P(z,x)) \\
&= \mu_1(x) - \mu_0(x) + \frac{\partial K(P(z,x))}{\partial P(z,x)} \\
&= MTE(x,u_D)
\end{aligned}
$$

[22] As the LATE parameter is not the focus of this study, I refer to Heckman et al. (2006) for a detailed discussion on the weights $\omega_{IV}^J(x,u_D)$.

In the empirical implementation, $K(P(z,x))$ in equation (7) is based on a polynomial of the propensity score. In particular, for the implementation of expression (7) the log wages are regressed on a constant, all covariates, the covariates interacted with the propensity score and a fourth order polynomial of the propensity score. The calculation of the MTE using expression (8) is then straight forward. In addition, as X represents a vector of covariates, its dimension is reduced by using demi-deciles of the linear predictor $X'\beta$ in expression (8), which are denoted as η_q. The treatment parameters are finally determined using the weights provided by table 4.[23]

3.2 Collective wage bargaining and wage dispersion

Now the approach to investigate the effect of collective wage bargaining on wage inequality is introduced. For that additional assumptions are required. First, assume that the error terms in expression (1) have the following form

$$\begin{aligned} U_1 &= \sigma_1(X)\epsilon_1 \\ U_0 &= \sigma_0(X)\epsilon_0. \end{aligned}$$

Hence, the potential outcomes are determined by a location-scale-shift model:

$$\begin{aligned} (9) \quad Y_1 &= \mu_1(X) + \sigma_1(X)\epsilon_1 \\ Y_0 &= \mu_0(X) + \sigma_0(X)\epsilon_0. \end{aligned}$$

where as before, X denotes the covariates and $\mu_d(X)$ and $\sigma_d(X)$ are the conditional location and the conditional scale of potential outcomes, respectively, by treatment status $d \in \{0,1\}$, and ϵ_1 and ϵ_0 are normalized error terms ($Var(\epsilon_1) = Var(\epsilon_0) = 1$). The causal effect of the treatment *collective wage bargaining* on wage inequality is investigated by considering the ratio of the two scale parameters, σ_1/σ_0. This answers the question of how much the dispersion of wages differs for the treatment group relative to the control group. Just as for the effect on the level of wages, identification of this scale ratio is not straightforward, as it would require to know both potential distributions of wages for each treatment status, which cannot be observed at once for the same individual. Simply comparing e.g. the conditional variances of wages does not necessarily yield the correct answer, as in general

$$(10) \quad \frac{Var[Y_1|X=x, D=1]}{Var[Y_0|X=x, D=0]} = \frac{\sigma_1^2(x)Var[\epsilon_1|D=1]}{\sigma_0^2(x)Var[\epsilon_0|D=0]} \neq \frac{\sigma_1^2(x)}{\sigma_0^2(x)}.$$

[23] To obtain e.g. the unconditional ATE, one can first integrate $MTE(u_D, \eta_q)$ over η_q and then over the full support of u_D. That is, $\mu_1(x) - \mu_0(x)$ is approximated by η_q, and $\frac{\partial K(P(z,x))}{\partial P(z,x)}$ is calculated by varying u_D between 0 and 1.

The last inequality sign arises from the fact that $Var[\epsilon_1|D=1]$ does not equal $Var[\epsilon_0|D=0]$ in general, as the distributions of ϵ_1 and ϵ_0 may vary by treatment status. One can think of a situation where unobserved earning skills are more diversified among the group of workers for which a collective wage bargaining agreement applies. In this case the above ratio does not necessarily estimate the causal effect of treatment on wage dispersion, rather it would understate the wage compression effect of collective bargaining.

To estimate the causal effect on wage dispersion, I draw on a method recently proposed by Chen and Khan (2010). Define $f_d(\epsilon_d, v)$ as the joint density function of the error term in the outcome equation (9) and the selection equation (3), which depends on the treatment status d. Under symmetry assumptions imposed on these error terms, Chen and Khan (2010) propose a pairwise matching estimator for calculating the ratio of the two conditional scale parameters in expression (9), $\sigma_1(x)/\sigma_0(x)$. These assumptions are

(*i*) *Conditional on* x *and* z, $f_d(\epsilon_d, V) = f_d(-\epsilon_d, -V)$ *for* $d = 0, 1$.

(*ii*) *Conditional on* V *and* z, *the unit random terms* ϵ_1 *and* ϵ_0 *in the outcome equation have the same distribution.*[24]

(*iii*) *Assume that the scale ratio* σ_1/σ_0 *is constant and that the set of symmetric pairs* (i, j), *one being treated and one being not treated and that satisfies* $p(x_i, z_i) + p(x_j, z_j) = 1$ *has positive density given the set of covariates.*[25,26]

The construction of symmetric pairs is the key for the implementation of this procedure. That is, for estimating this effect, I construct symmetric pairs of individuals, one who receives treatment and the other not being treated, such that their sum of probabilities of receiving treatment equals one; $p(x_i, z_i) + p(x_j, z_j) = 1$, for pairs of individuals i and j with covariates x and z and treatment statuses $d_i = 1$ and $d_j = 0$. The existence of such pairs is ensured by assumption (*iii*).[27] The intuition behind this estimation strategy is that under the above assumptions, such pairs with opposite treatment status share the same selection bias, so that the nuisance terms cancel out.

To deal with the possibility of heavy tails, which can occur when regarding wage distributions, I consider quantiles instead of variances for the estimation of the scale ratio. The conditional quantile functions for quantiles $\tau \in (0, 1)$ of the potential outcomes y_d are given by $q_\tau(y|d, z, x) = \mu_d(x) + \sigma_d(x) q_\tau(\epsilon_d|d, x, z)$. Define the interquantile spreads

[24] This equality assumption is rather strong. In particular, it rules out a general Roy model, where outcome and selection errors are correlated in opposite directions for the treated and the untreated.

[25] Angrist (2004) and Heckman and Vytlacil (2001) discuss similar symmetry conditions which link LATE with ATE for level effects.

[26] In the present study I assume the scale ratio to be constant. It is also possible to allow the scale ratio to vary with x and then to calculate the average scale ratio $E_X(\sigma_1(x)/\sigma_0(x))$.

[27] This assumption is satisfied if the propensity scores are continuously distributed (Chen and Khan, 2010).

IQ_τ from τ to $1-\tau$ by $IQ_\tau(y|d,x,z) \equiv q_\tau(y|d,x,z) - q_{1-\tau}(y|d,x,z)$.[28] The interquantile spreads by treatment status can be rewritten as

$$
\begin{aligned}
(11) \quad IQ_\tau(y_i|d_i=1,x_i,z_i) &= \sigma_1(x_i) IQ_\tau(\epsilon_{1i}|v_i \leq \mu_V(x_i,z_i)) \\
IQ_\tau(y_j|d_j=0,x_j,z_j) &= \sigma_0(x_j) IQ_\tau(\epsilon_{0j}|v_j > \mu_V(x_j,z_j)).
\end{aligned}
$$

Under the symmetry conditions invoked above, taking the ratio of the two expressions in (11) for symmetric pairs yields the scale ratio σ_1/σ_0.[29] Given the large sample size of the data at use, for the implementation I use a fine grid on the propensities for treatment to find such symmetric pairs as a practical approach.[30]

4 Instrumental variables

In order to implement the methods above and to estimate the causal relationship between the presence of collective wage bargaining agreements and the wage structure, this analysis uses two sets of instrumental variables, which are discussed next.

The first set of instrumental variables are shares of Catholic and Protestant inhabitants in 2001 at the district level.[31] West Germany consists of 319 such districts and for each district the two shares of the predominant Christian denominations (Catholic and Protestant) in Germany are provided. The weighted mean of this share variable indicates that 34% of the population considered are Protestant, 42% are Catholic, while the remaining 24% have either a different or no religion. There is a strong variation of these shares across Germany as well as within the German states, as can be inferred from figures 3 and 4, which illustrate the distribution of those shares across the districts of West Germany.[32] The share of Catholics across districts varies between 3% and 89%, the share of Protestants varies between 4% and 76%.

[Figures 3 and 4 about here]

Note that while some studies for the U.S. argue that religion is a *choice variable* (Lehrer, 2009), the affiliation to a certain religion in Germany is largely defined by family

[28] I consider the ratio of interquantile spreads instead of the ratio of variances, as the former are more robust against outliers.

[29] For the proof see Chen and Khan (2010), Appendix B.

[30] Experimenting with the number of grid points shows that the results are very robust. The results in the next section are based upon 200 grid points. If the sample size is small, Chen and Khan (2010) propose kernel estimation strategies to find a sufficient number of symmetric pairs.

[31] These data are available from the German Federal Statistical Office in Wiesbaden and have been merged with the GSES data. Information of religious affiliation on the individual level is not available and is therefore proxied by these share variables.

[32] As only West Germany is considered, the East German regions are left blank.

tradition and is rarely being switched systematically.[33] In addition, previous studies have argued that being either Protestant or Catholic does not have a direct impact on economic growth per se. Cantoni (2009) considers the case of Germany for the period 1300-1900 and finds no evidence for a direct link between religious affiliation and economic growth. The author thus concludes that there is no general attitude towards work which makes that e.g. Protestants exhibit a higher effort towards work, as one might infer from the seminal study by Weber (1904).[34] Becker and Wößmann (2009) consider cross-sectional data for Prussia for the year 1871. The authors do find that Protestants fared better than Catholics in economic terms, however, this can be entirely attributed to the higher literacy rates among Protestants at this point in time.[35] Hence, neither Cantoni (2009) nor Becker and Wößmann (2009) provide evidence of a specific *Protestant work ethic*, which could potentially confound the results in the present paper.[36,37]

How does religious affiliation influence either the employee's decision to work in an establishment applying a collective wage bargaining agreement or the employer's decision to join an employer's association, and then to apply such a collective wage bargaining agreement? One channel is traditional social norms: Employers of a certain religion might tend to decide to join an employer's association, as in certain regions this has traditionally been the case for members of this religion. There can be several of these *social norms* channels which are likely to depend on the interaction between region and religion.[38,39] For instance, in the Rhineland there is a strong tradition of a *Catholic social teaching*, which, broadly speaking, argues for solidarity and human dignity – and in particular promotes the idea that workers should have the right to form labor unions (Fleckenstein, 1999). This is likely to increase the propensity of Catholic employers to join an employer's association for then to apply collective wage bargaining agreements, as well as to increase the likelihood of catholic workers to join a union.

[33] In particular, it is observable that over time the numbers of individuals leaving the church is approximately evenly distributed across the two Christian denominations.

[34] Weber (1904) proposed a theory according to which, broadly speaking, Protestants work harder than Catholics, as worldly success was seen to be signal for salvation, which itself was predetermined. However, this theory was mainly coined for calvinism, the more ascetic form of Protestantism, which is predominant mostly in Switzerland and some parts of South-Western Germany. However, most Protestants in Germany are Lutheran.

[35] The authors argue that Protestants, compared to Catholics, had a higher likelihood of being literate, as a key ingredient of Protestantism is to be able to read an interpret the Bible on one's own.

[36] As an additional robustness check, the next version of this study will incorporate the analysis using historic shares of Protestants and Catholics, using census data from the 1950s.

[37] Arruña (2010) provides an international prospective and finds no differences regarding the "work ethic" between the two Christian denominations.

[38] The variable *region* indicates in which of the 10 states of West Germany (excluding Berlin) the establishment is located, where the smallest states Bremen and Saarland are merged with Lower Saxony and Rhineland-Palatinate, respectively.

[39] In addition, the share variables are also interacted with the variable *sector*, as similar arguments can be thought of for such interaction terms.

Another potential reason for how religious affiliation can influence the individual decision to work for an establishment applying a collective bargaining agreement may be rooted in the Nazi period: while during the postwar period the predominant unions were in general not adherent to specific religious groups, unions during the Weimar Republic were often either Catholic or Protestant. Just as parts of the Catholic church took an ambivalent position towards the Nazis, so did the Catholic workers' federation "South German Catholic Workers' Association (VSkA)" (Cremer, 1999).[40] It is conceivable that this partly sympathetic position of this union, prevalent in Bavaria and Baden-Württemberg, led to a sceptical view of unions in the aftermath of the second world war, and thus to a ceteris paribus lower propensity to apply collective wage bargaining agreements among Catholics, especially in the Southern states of Germany.

Moreover, Catholics may be more attracted by hierarchical structured organizations (as the Catholic church seems to follow stronger hierarchical structures compared to the Protestant church, see e.g. Putnam (1993)), such that Catholics could have a *lower* preference to work under a collective wage bargaining agreement. Indeed, Arruña (2010) argues that Protestants value institutions and mutual social control more than Catholics do. Again, the magnitude as well as the direction of how this latter channel influences the treatment variable probably varies by region. In sum, there are several possible channels through which religion can induce exogenous variation of the treatment variable. Therefore one should be agnostic about it and rely upon the linear correlation between these instruments and the treatment variable.

The second instrument I use is the approximated gross union density in 1961, which is also measured at the district level.[41] Several districts need to be aggregated when calculating these share variables, such that the 319 districts are merged to 287 areas. This is due to the fact that the administration of the unions sets up areas (*Verwaltungsstellen*) which are in charge of at least one district but may be in charge of several.[42] There are no data available for the state *Schleswig-Holstein*, which is therefore dropped from the analysis.[43] I approximate the union density by using data provided by the largest union in Germany in 1961, the "IG Metall". This union had 1,85 million members in 1961, accounting for 30% of all union members. It covers industries such as metal producing

[40] Adolf Hitler and his National Socialist Party seized power in 1933 and banned unions shortly afterwards. Before that, some unions were not complectly opposed against the ideas of the new regime.

[41] This variable is calculated using data which are directly obtained from the trade union "IG Metall". I choose the year 1961, because in this year a census took place in West Germany so that very reliable information regarding the total number of employees in each district is available. In addition, workers that were members of this union in 1961 have almost surely left the labor market by 2001, especially as I only consider workers in the age range between 25 years and 55 years.

[42] Regarding regional variables in this study, the smallest unit is *district*, followed by *area* and *region (Bundesland)*.

[43] This should, if at all, affect the results only in a very negligible way, as in 1961 the population accounted for less than 4% of the entire population in West Germany.

companies.[44] Figure 5 gives an impression of how this variable is distributed across the districts.

[Figure 5 about here]

How does this instrument provide exogenous variation for the treatment variable "collective wage bargaining"? The underlying assumption is that a high union density during the 1960s in certain regions has created a "culture" of unionism in these areas, which leads to ceteris paribus higher collective wage bargaining rates, even for industries which are not related to the "IG Metall". One can think of a situation where we observe two otherwise identical establishments from e.g. the service sector, where one is located in the formerly heavily industrialized and unionized Ruhr area in North-Rhine Westphalia, and the other is located near the Black Forest, a region which used to be less industrialized. As the latter region has less of a tradition of unionism, the propensity to apply collective wage bargaining should be lower for the establishment located in this region.[45]

The identification crucially relies on the assumption that conditional on all other covariates, the only channel how religion and the historic gross union density influence the outcome variable is through the worker's decision to work in an establishment which applies collective wage bargaining or the employer's decision to apply such a collective bargaining agreement. To minimize possible confounding effects with the instrument *gross union density* and to only capture the "culture" effect, I exclude certain industries from the analysis which are likely to be influenced by the "IG Metall". In summary, I thus assume that the gross union density from 1961 and the religious affiliation at the district level both influence nowadays attitude towards collective bargaining agreements, but that these variables have no direct effect on wages, conditioning on all other covariates.

5 Results

Based on the empirical framework introduced above, this section discusses the estimated specifications and then presents the empirical results.

The starting point is to estimate a probit model in order to predict the propensity for an employee to work in an establishment applying a collective wage bargaining agreement. These predicted treatment propensities are required for both the estimation of the

[44] In order to account for the industry structure in 1961, these estimated union densities have to be appropriately re-weighted.

[45] This idea is related to Holmes (2006), who, using U.S. data, provides evidence that unionism is contagious both across industries and time. The author observes that e.g. supermarkets which are located near places where formerly coal mines – which traditionally were most often unionized – existed have later in time a higher propensity to be unionized compared to otherwise identical supermarkets located in regions without such a history of unionism.

treatment effects at the mean and the effect of collective bargaining on the dispersion on wages. Table 5 summarizes the results of this regression. As covariates I include both firm and workers characteristics, as well as the share of Catholics and Protestants and the gross union density from 1961 and interaction terms of these instrumental variables with the covariates region and industry. Almost all of these terms are highly significant. Figure 6 gives an impression of the distribution of the predicted propensity scores for receiving the treatment *collective wage bargaining* for both treated and untreated workers; treated workers have a much greater propensity to receive treatment.

[Table 5 and figure 6 about here]

Next, using the predicted propensity scores, a test for the presence of essential heterogeneity is carried out, following Heckman et al. (2006). For that, log wages are regressed on all covariates, interaction terms between the covariates and the propensity score and a polynomial of second and higher order terms of the propensity score. Then it is tested whether the coefficients of the polynomial are jointly zero, using the appropriate chi-square statistics. Table 6 presents the results for different degrees of the polynomial, which show that the quartic terms are jointly significant. This is strong evidence for the presence of essential heterogeneity, i.e. individuals select themselves based on heterogeneous and unobserved gains in either of the two bargaining regimes. This implies that applying standard instrumental variable approaches, such as 2SLS, would not be informative neither on the ATE nor on the ATET.[46]

[Table 6 about here]

5.1 Collective wage bargaining and the level of wages

Now, I estimate the effect of collective wage bargaining on the level of wages. For comparison with the causal estimates, I start by simply regressing via OLS log wages on a dummy variable indicating if an employee works in an establishment applying collective wage bargaining, without including any additional covariates. Table 7 summarizes the results. The coefficient in this basic regression for the dummy variable indicating treatment equals 0.073, i.e. wages for workers being treated are on average 7.3 log points higher than for those being not treated. However, this coefficient does not take into account the fact that employees who work in establishments applying collective wage bargaining agreements have e.g. on average higher tenure and the firm size tends to be greater (see section 2); both covariates are likely to be positively correlated with wages.

[46] If only the first order term of the polynomial of the propensity score proved to be significant, then $\frac{\partial K(P(z,x))}{\partial P(z,x)}$ in equation (8) would not vary with $P(z,x)$ and thus the MTE would be constant across all margins and a constant treatment effect model would be sufficient to explain the data (see e.g. Basu et al., 2007).

[Table 7 about here]

In order to control for this correlation, the second regression includes the rich set of both individual and firm characteristics, which are described in the data section, as control variables. The coefficient halves to 3.6 log points, which is in line with results from the existing literature (see e.g. Kohn and Lembcke, 2007; Burda et al., 2008). This indicates that the observed characteristics of the treated account for half of the union wage premium observed unconditionally, and thus that a positive selection based on the observable characteristics takes place.

I proceed with estimating the LATE parameter applying standard 2SLS. The instruments are the same as in the first stage probit regression for the propensity score; the share of Protestants and Catholics at the district level and the gross union density as well as various interaction terms with other covariates, as described above. This regression yields a coefficient of 2.8 log points. Given this estimate one could suspect positive self-selection into treatment based upon unobservable characteristics, as the OLS estimate lies above this causal parameter.

In light of the test results presented by table 6, strongly indicating the presence of essential heterogeneity, I calculate the marginal treatment effects using equation (7) and (8), whereby a fourth order polynomial of the propensity score is used. Table 8 summarizes the results, while the detailed regression results are presented by table 9. Employing the formulas for the weights as described by table 4, I calculate the weighting schemes for the treatment effects. The resulting weighting schemes for the ATE and the ATET are depicted by figures 7 and 8, respectively. The ATE weights are simply the joint density of (η_q, u_D). The ATET weights are larger for small values of u_D, which make selection into treatment more likely.

[Tables 9 and 8 and figures 7 and 8 about here]

For comparison with the result obtained via 2SLS, I also estimate the LATE parameter using the marginal treatment effect approach. This calculation yields a slightly higher coefficient of 2.9 log points, which is however less precisely estimated. Nevertheless, the fact that the two estimates are very close to each other suggests that the model is well-specified.

Now I turn to the calculation of the ATE and ATET. The ATE estimate is 0.8 log points, which lies below the OLS and the LATE estimate. This is the expected gain of treatment for a randomly drawn person from the population. Again, this parameter is somewhat imprecisely estimated and is not statistically significant at any reasonable confidence level.[47]

[47] Interestingly, although concentrating on the manufacturing sector, which is excluded from the present study (see section 4), Guertzgen (2010) also finds that the union wage premium is statistically not

Finally, the ATET is estimated to equal 1.6 log points, which is not statistically different from zero either. Pointwise, this estimate lies above the ATE, which indicates that the idiosyncratic gains for individuals selecting into treatment are slightly positive. Thus, there are two effects. First, positive selection into treatment, based on unobservable characteristics, seems to take place. Individuals undergoing treatment have a higher productivity compared to individuals not being treated. Second, workers undergoing treatment profit – based on idiosyncratic gains – from receiving treatment compared to the situation in which they would not have been treated, as indicated by the positive difference between the ATET and the ATE.

It may come as a surprise that the estimate for the LATE lies above the ATE. If the instruments are interpreted as indicators for a particular social environment, then it is conceivable that employees who undergo treatment because these indicators pass a certain threshold in particular benefit from working under a collective wage bargaining agreement. One can for example think of a particular benevolent employer, who applies a collective wage bargaining contract because of certain *social norms*. Moreover, given the stark differences between the different treatment effects underlies the importance of using an econometrics framework which allows for heterogeneous treatment effects, as does the present study.

5.2 Collective wage bargaining and the dispersion of wages

The results for the treatment effect on wage dispersion are summarized in table 10. Unconditionally, the ratio of the two scale parameters equals 0.818, such that the dispersion for the treated is considerably lower than for the untreated. Just as for the level of wages, this estimate does not consider the observed differences in workers and firm characteristics between the treated and the untreated. To account for that, I calculate the ratio of the conditional scale parameters. This is the ratio of the residual standard deviations in an OLS regression, which equals 0.803. This indicates that the selection process is such that workers and firms whose observable characteristics are associated with larger wage dispersion are more likely to be covered by a collective bargaining agreement.[48]

Applying the pairwise matching procedure proposed by Chen and Khan (2010), using the predicted propensities from the first stage summarized in table 5, I obtain a ratio of 0.738, which is considerably below the estimate using the residuals from the OLS regression.

Thus, there is a strong negative causal effect of collective bargaining on wage disper-

significant. However, due to a different research design, she does not allow for heterogeneous treatment effects.

[48]This result regarding the selection process has also been documented by Dustmann and Schönberg (2009), using the LIAB data set.

sion. In addition, just as for the observable characteristics, workers whose unobservable characteristics involving high wage dispersion select themselves into the collective bargaining regime. This is a particular interesting results, as it stands in contrast to what one might expect when considering the Card's (1996) "hurdle-model", which in argues that these unobserved skills should be more compressed in the unionized part of the economy. However, as discussed in section 1 of this study, there are several aspects that differ between the Anglo-saxon and the Germany system of the collective bargaining regime, such that this result might not come as such a huge surprise in the end. One possible way for unions to compress the wage structure could be the suppression of using performance-pay schemes in the firms that apply a collective bargaining contract. Lemieux et al. (2009) report that in the U.S. 28% of the workers employed in the non-unionized receive wages which is at least partly based upon their performance, while this is only true for 14% of the unionized firms. Barth et al. (2008) provide similar evidence for Norway. While there exists certain evidence for this channel on the international level, for the case of Germany there seems to be no empirical evidence available. Further exploring this or other possibilities how unions actually manage to diminish wage inequality, which goes beyond the scope of this paper, therefore seems of particular interest.

Overall, while the results show a considerable effect of collective bargaining on the dispersion of wages, they strongly suggest at the same time that the causal effect of unions on the wage level is very close to zero. Why might that be? One interpretation is that unions put their emphasis not on the level of wages but on reducing wage inequality. Moreover, the level of wages may be taken as given by firms which are not covered by collective wage bargaining regimes, in order to be able to attract employees to work for them, which could also explain why the causal effect of unions on the level of wages is almost non-existent.

6 Conclusion

A large literature has documented a positive union wage premium and a wage compressing effect of collective bargaining. While for the U.S. some studies estimate the causal effect of collective bargaining on the structure of wages, the present study is the first to deliver such estimates for Germany.

This paper uses a large linked employer-employee data set, which is very reliable due to its administrative character and provides precise information on a rich set of both individual and firm characteristics. In particular, information on hourly wages and on whether an individual is covered by a collective wage bargaining agreement is included. The data show that in 2001 about 70% of the employees in Germany are covered by such a collective wage bargaining agreement, while the remaining 30% of workers bargain at the

individual level with employers over wages. In the case of collective bargaining, for the vast majority of cases unions and employers' associations negotiate wages at the industry level. The existing literature estimates that the (conditional) union wage premium equals 3 to 6 log points and many studies have documented the wage compressing effect of collective wage bargaining (Burda et al., 2008; Antonczyk et al., 2010, and the literature cited therein). However, due to a lack of credible instruments in the commonly used data sets, these estimates do not take into account that the treatment variable *collective wage bargaining* is likely to be endogenous.

Standard estimation techniques of the union wage premium can yield biased results for numerous reasons. First, selection bias is likely to occur. There may be selection from the employer's side as well as from the worker's side. This happens if for instance only highly profitable firms join an employer's association for then to apply collective wage bargaining contracts or, if only very productive workers decide to join firms which apply collective wage bargaining contracts. Furthermore, individuals may decide to join these firms based upon expected idiosyncratic gains, which are observed by the individual but not the analyst. This leads to the situation which Heckman and Vytlacil (2005) have termed *essential heterogeneity*. That is, employees react to the treatment differently *and* selection into treatment is based upon this knowledge. Again, similar arguments can be thought of for the employer's side. Thus in addition to the selection bias, the analyst is confronted with heterogeneous treatment effects. Moreover, just as for the level of wages, standard estimation techniques for the effect of collective bargaining on the dispersion of wages may yield biased estimates. For instance, it may be the case that unobserved earning skills are more diversified among the group of workers for which a collective wage bargaining agreement applies. Then, comparing the unconditional wage dispersion or the residual variances between the treated and the untreated does not necessarily yield the causal effect of *collective bargaining* on wage inequality.

The study by DiNardo and Lee (2004), considering the U.S., uses a sharp regression discontinuity design and comes to the conclusion that the union wage premium has been very close to zero over the recent decades. However, as institutions between the anglo-saxon countries and Germany differ, it is not possible to transfer such an estimation strategy to the German case. Therefore the present paper looks for instruments outside the commonly used data sets and merges these on a very fine regional level with the linked employer-employee data at use. This sets the stage for estimating the causal effect of collective wage bargaining on the structure of wages.

Using Marginal Treatment Effects as introduced by Heckman and Vytlacil (2005), I recover the Average Treatment Effect (ATE) and the Average Treatment Effect on the Treated (ATET) of *collective wage bargaining* for the level of wages. This method comes at the cost of larger standard errors, compared to more conventional 2SLS estimates.

However, in light of the stark differences between the point estimates of the causal treatment parameters as well as the estimate obtained via a standard OLS regression, paying this price seems justified. In addition, I employ a new estimation technique very recently proposed by Chen and Khan (2010) to estimate the ATE of collective wage bargaining on the dispersion of wages.

The results suggest that the union wage premium is close to zero; the ATE equals 0.8 log points, which is slightly below the estimate for the ATET. Moreover, I find that workers positively select themselves into establishments applying collective wage bargaining agreements based on observable and on unobservable characteristics. For the ATE on wage inequality I find that it is below the effect estimated via standard OLS; the residual standard deviation of log wages is about 20% lower among workers covered by a collective wage bargaining agreement, while the ATE even equals 26%. Both effects are stronger than what simply comparing unconditional standard deviations across the two bargaining regimes yields. This suggests two things: First, unions are successful in compressing the wage structure and second, workers whose observable as well as unobservable characteristics involving high wage dispersion select themselves into the collective bargaining regime. Overall the results thus provide evidence that unions, if at all, have only a little causal impact on the level of wages. In contrast, they are indeed successful in compressing the wage structure, suggesting that unions concentrate their efforts on diminishing wage inequality.

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Appendix

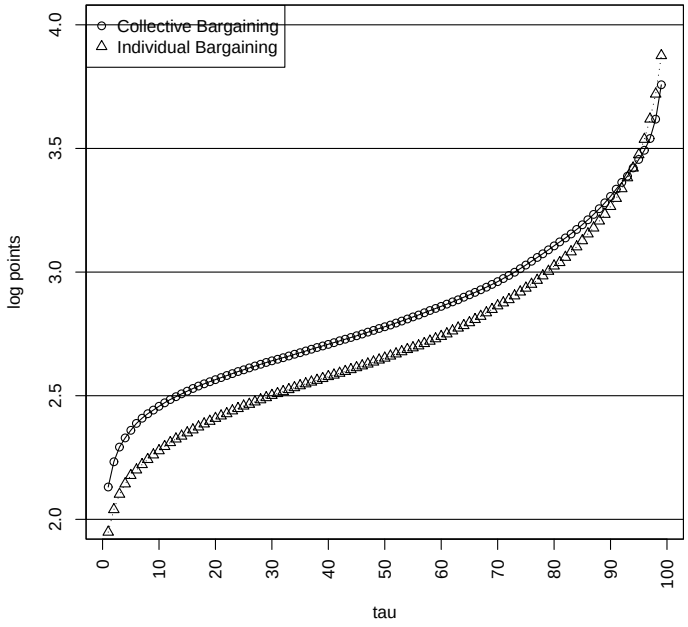

Figure 1: Wage distribution by bargaining regime

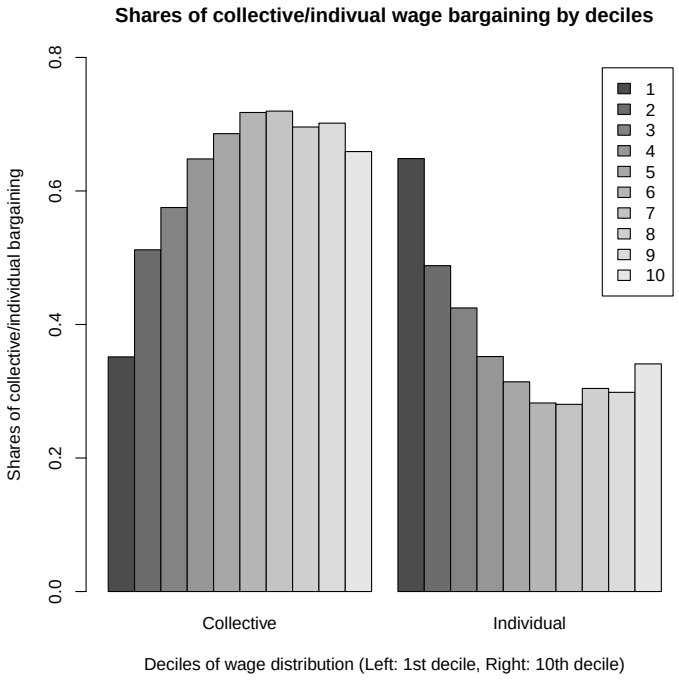

Figure 2: Shares of bargaining regimes by deciles of wage distribution

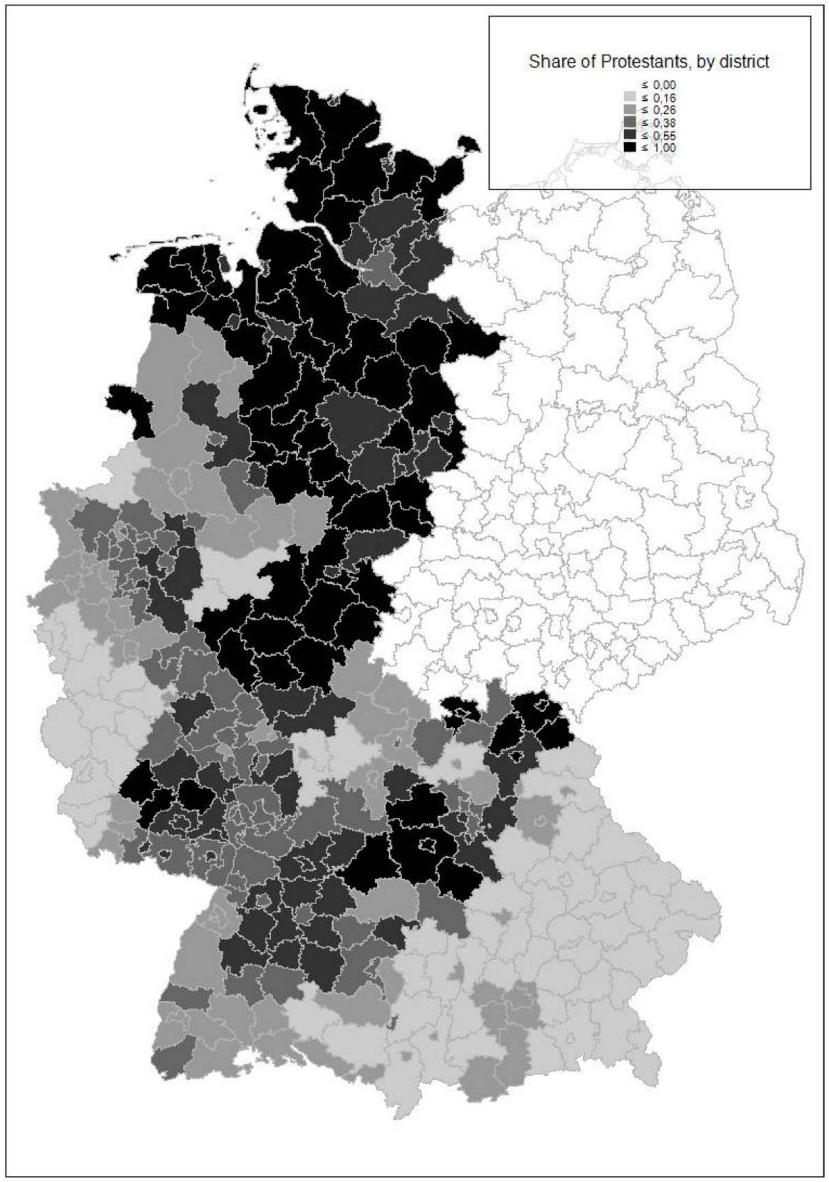

Figure 3: Shares of Protestants by district, (West Germany)

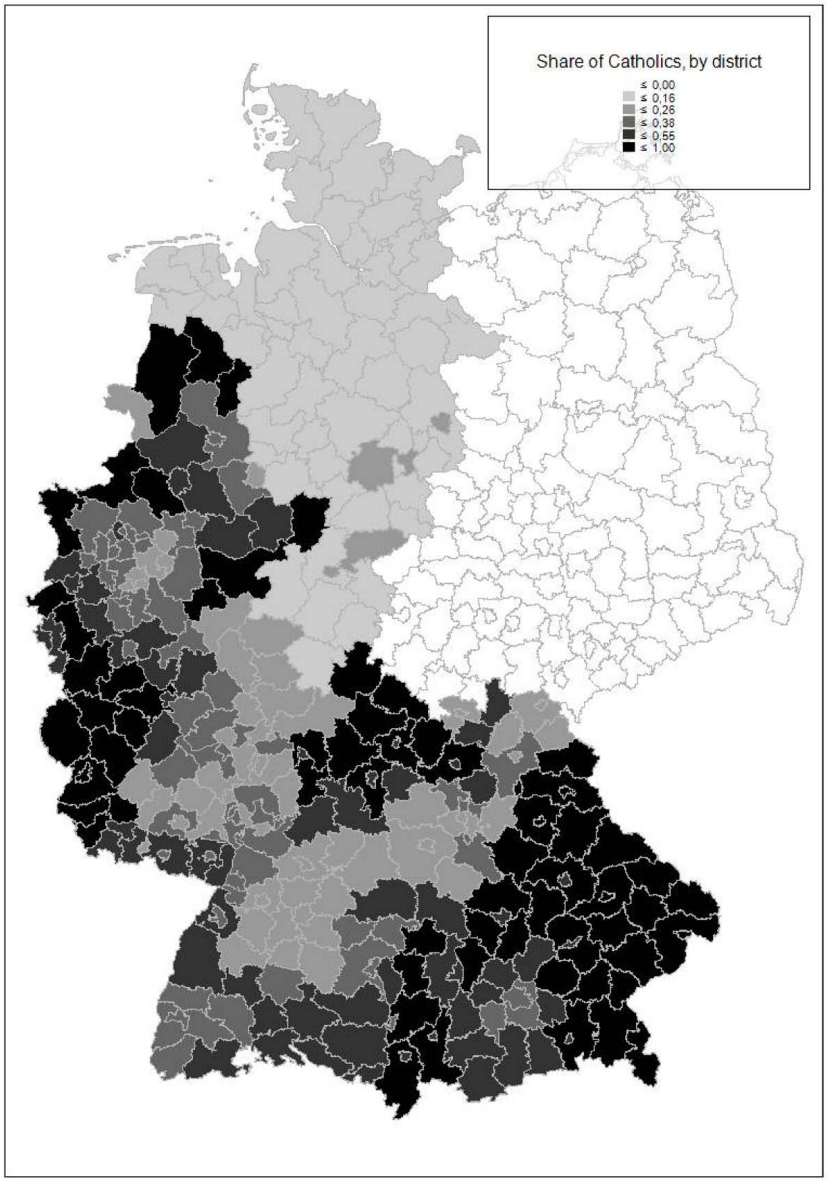

Figure 4: Shares of Catholics, by district, (West Germany)

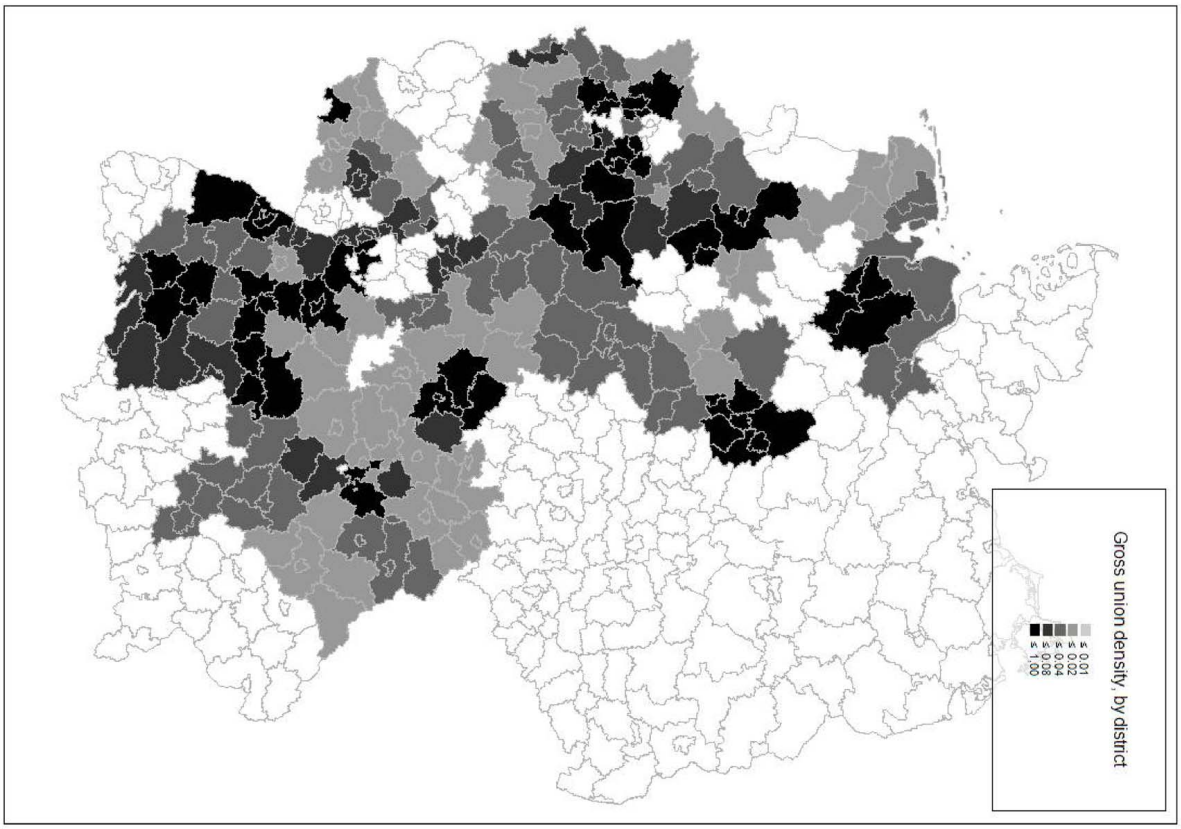

Figure 5: Gross union density, 1961, by district, (West Germany)

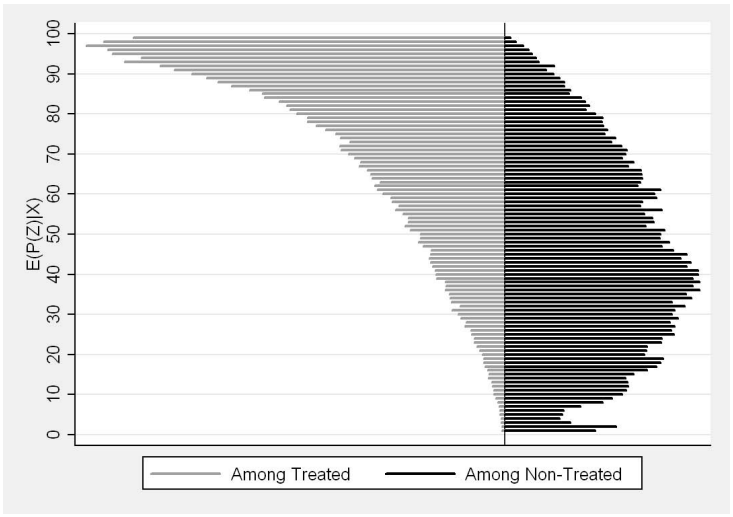

Figure 6: Propensities for Collective Wage Bargaining

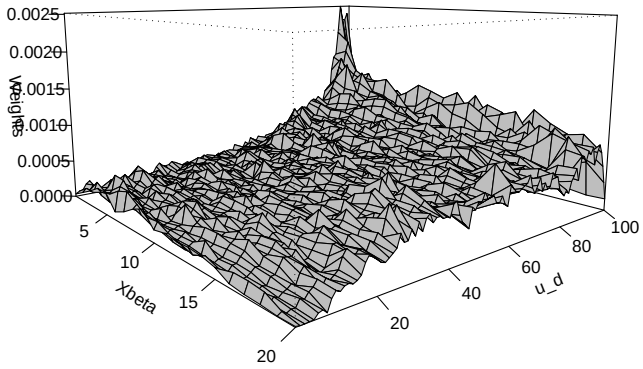

Figure 7: Weights for the Average Treatment Effect

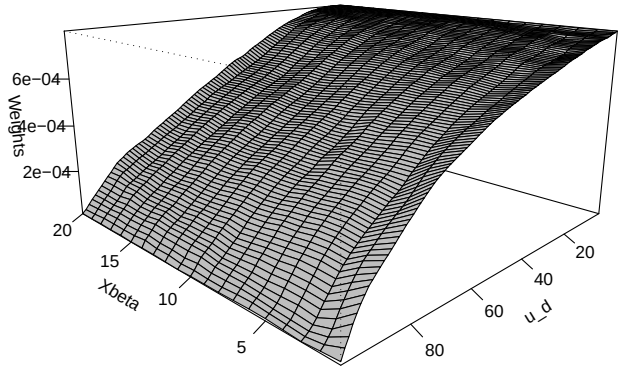

Figure 8: Weights for the Average Treatment Effect on the Treated

Table 1: Definition of Variables

Label	Description
Individual level	
Wage	Hourly wage
Log wage	Logarithmized wage
Age	Age in years
Tenure	Tenure in years
Low education	Low level of education: no training beyond a school degree
Medium education	Intermediate Level of education: vocational training
High education	High level of education: university or university of applied sciences
Education n/a	Missing information on the education level
Hours worked per month	Hours worked per month
Firm level	
HH	Firm is located in Hamburg
Lower Saxony, Bremen	Firm is located in Lower Saxony or Bremen
NRW	Firm is located in North Rhine-Westphalia
Hesse	Firm is located in Hesse
RLP, Saarland	Firm is located in Rhineland-Palatinate or Saarland
Baden-Württemberg	Firm is located in Baden-Württemberg
Bavaria	Firm is located in Bavaria
Share protestants	Share of protestants on district level
Share catholics	Share of catholics on district level
Share other or no religion	Share of other/no religion on district level
10 - 99 employees	Firm has between 10 and 99 employees
100 - 199 employees	Firm has between 100 and 199 employees
200 - 999 employees	Firm has between 200 and 999 employees
1000 - 1999 employees	Firm has between 1000 and 1999 employees
2000 - 9999 employees	Firm has between 2000 and 9999 employees
Mainly publicly owned	Firm is mainly public-owned (>50%)
Share male employees	Share of male employees
Manufact: Food	Manufacture of food products, beverages and tobacco
Manufact: Textiles	Manufacture of textile and textile products, leather and leather products
Manufact: Wood	Manufacture of wood and wood products
Publishing, printing	Publishing, printing and reproduction of recorded media
Manufact: Rubber, plastic	Manufacture of rubber and plastic products
Manufact: Non-metallic	Manufacture of other non-metallic mineral products
Manufact: n.e.c.	Manufacture not elsewhere classified
Electricity	Electricity, gas and water supply
Construction	Construction
Auto sales, repair	Sale, maintenance and repair of motor vehicles and motorcycles; retail sale of automotive fuel
Wholesale trade	Wholesale trade and commission trade except of motor vehicles and motorcycles
Retail trade	Retail trade, except from motor vehicles and motorcycles; repair of personal and household goods
Hotels, restaurants	Hotels and restaurants
Auxiliary transport	Supporting and auxiliary transport activities; activities of travel agencies
Post, telecommunications	Post and telecommunications
Real estate	Real estate activities; renting of machinery and equipment without operator
Data processing	Data processing and information systems
Research, other services	Research and development and other services

Note: As further controls we include: Age squared, tenure squared, and the interactions of age with education.

Table 2: Descriptive statistics by bargaining regime

Label	Collective wage bargaining		Individual wage bargaining	
	Mean	Stdd.	Mean	Stdd.
Individual level				
Wage	16.313	6.962	15.165	7.968
Log wage	2.792	.321	2.719	.393
Age	39.879	8.038	38.675	7.919
Tenure	10.939	9.330	6.508	7.222
Low education	.158	.365	.141	.348
Medium education	.686	.464	.623	.484
High education	.104	.305	.113	.317
Education n/a	.051	.221	.121	.326
Hours worked per month	167.985	17.043	175.237	18.163
Firm level				
HH	.021	.143	.045	.209
Lower Saxony, Bremen	.117	.322	.103	.305
NRW	.320	.466	.266	.442
Hesse	.082	.275	.108	.311
RLP, Saarland	.073	.260	.070	.255
Baden-Württemberg	.193	.395	.233	.423
Bavaria	.191	.393	.171	.376
Share protestants	.322	.154	.316	.150
Share catholics	.403	.204	.401	.207
Share other or no religion	.273	.111	.281	.121
10 - 99 employees	.290	.453	.641	.479
100 - 199 employees	.156	.363	.142	.349
200 - 999 employees	.340	.473	.174	.379
1000 - 1999 employees	.085	.279	.024	.154
2000 - 9999 employees	.127	.333	.017	.129
Share male employees	.776	.178	.729	.195
Manufact: Food	.057	.190	.053	.204
Manufact: Textiles	.034	.118	.018	.091
Manufact: Wood	.051	.174	.040	.171
Publishing, printing	.048	.165	.035	.156
Manufact: Rubber, plastic	.057	.190	.046	.210
Manufact: Non-metallic	.050	.172	.014	.120
Manufact: n.e.c.	.045	.157	.026	.159
Electricity	.055	.184	.002	.054
Construction	.147	.333	.116	.320
Auto sales, repair	.060	.196	.049	.195
Wholesale trade	.090	.256	.170	.366
Retail trade	.066	.209	.072	.241
Hotels, restaurants	.030	.102	.027	.131
Auxiliary transport	.050	.171	.051	.198
Post, telecommunications	.024	.070	.012	.044
Real estate	.026	.083	.023	.115
Data processing	.027	.085	.054	.226
Research, other services	.056	.186	.168	.374
No. of observations	132,833		77,638	

Note: All statistics are weighted by the inverse sampling probability.

Table 3: Wages by skill-level and bargaining regime

Label	Collective wage bargaining		Individual wage bargaining	
	Mean	Stdd.	Mean	Stdd.
Log hourly wage for low skilled	2.64	.25	2.44	.27
Log hourly wage for medium skilled	2.82	.30	2.71	.34
Log hourly wage for high skilled	3.29	.29	3.18	.36
Log hourly wage for skill level n/a	2.60	.32	2.63	.42
Skill premium medium- vs. low-skilled	.18	–	.27	–
Skill premium high- vs. medium-skilled	.47	–	.47	–

Note: All statistics are weighted by the inverse sampling probability. The skill premia are calculated as the difference between the log hourly wages.

Table 4: Treatment effects and estimands as weighted averages of the marginal treatment effect

Treatment Effects

$ATE(x) = E(Y_1 - Y_0|X = x) = \int_0^1 MTE(x, u_D)\omega_{ATE}(x, u_D)du_D = \int_0^1 MTE(x, u_D)du_D$
$ATET(x) = E(Y_1 - Y_0|X = x, D = 1) = \int_0^1 MTE(x, u_D)\omega_{ATET}(x, u_D)du_D$
$LATE(x) = E(Y_1 - Y_0|X = x, D(J(z)) - D(J(z')) = 1) = \int_0^1 MTE(x, u_D)\omega^J_{IV}(x, u_D)du_D$

Weights

$\omega_{ATE}(x, u_D) = 1$
$\omega_{ATET}(x, u_D) = \frac{Pr(P(z,x)>u_D|X=x)}{E(P(z,x)|X=x)}$
$\omega^J_{IV}(x, u_D) = \frac{E(J(Z)-E(J(Z))|P(Z,x))Pr(P(Z,x)>u_D)}{Cov(J(Z),D)}$

Note: $J(Z)$ is a scalar function of the vector Z, containing the instrumental variables.

Table 5: First Stage: Probit Estimates of Collective Bargaining Coverage.

Covariate	Coefficient	Robust Std. Err.	Covariate	Coefficient	Robust Std. Err.
Intercept	-.397	.125	Catholic·Manufact: Food	-3.427	.178
Age	-.003	.004	Protest.·Manufact: Food	-4.767	.239
Age^2	.000	.000	Catholic·Manufact: Textiles	-2.785	.279
Tenure	.038	.001	Protest.·Manufact: Textiles	-1.488	.330
$Tenure^2$	-.000	.000	Catholic·Manufact: Wood	-3.489	.203
Low education	-.103	.085	Protest.·Manufact: Wood	-3.825	.223
High education	-.738	.094	Catholic·Publishing, printing	-2.004	.157
Education n/a	-.577	.094	Protest.·Publishing, printing	.097	.232
Age·Low education	.003	.001	Catholic·Manufact: Rubber, plastic	-4.318	.160
Age·High education	.013	.001	Protest.·Manufact: Rubber, plastic	-3.527	.214
Age·Education n/a	.010	.001	Catholic·Manufact: Non-metallic	-1.740	.200
Manufact: Food	3.865	.155	Protest.·Manufact: Non-metallic	-2.119	.253
Manufact: Textiles	2.677	.240	Catholic·Manufact: n.e.c.	-3.030	.193
Manufact: Wood	3.442	.151	Protest.·Manufact: n.e.c.	-2.998	.251
Publishing, printing	1.619	.128	Catholic·Electricity	6.986	.289
Manufact: Rubber, plastic	3.762	.139	Protest.·Electricity	3.714	.308
Manufact: Non-metallic	2.589	.173	Catholic·Construction	-2.348	.121
Manufact: n.e.c.	1.312	.144	Protest.·Construction	-1.525	.166
Electricity	-2.104	.181	Catholic·Auto sales, repair	-2.817	.161
Construction	2.685	.101	Protest.·Auto sales, repair	-2.043	.225
Auto sales, repair	3.009	.133	Catholic·Wholesale trade	-2.660	.131
Wholesale trade	2.114	.104	Protest.·Wholesale trade	-2.188	.175
Retail trade	2.318	.149	Catholic·Retail trade	-2.601	.177
Hotels, restaurants	2.858	.183	Protest.·Retail trade	-1.384	.247
Auxiliary transport	1.405	.111	Catholic·Hotels, restaurants	-2.607	.240
Post, telecommunications	-.230	.239	Protest.·Hotels, restaurants	-1.743	.310
Real estate	.800	.186	Catholic·Auxiliary transport	-1.401	.139
Data processing	.385	.153	Protest.·Auxiliary transport	-1.440	.183
Region HH	-.779	.031	Catholic·Post, telecommunications	1.530	.376
Region Lower Sax., Bremen	.153	.100	Protest.·Post, telecommunications	2.155	.422
Region Rhine.L-Palat.	-.065	.100	Catholic·Real estate	-1.027	.250
Region Hesse	3.402	.185	Protest.·Real estate	.463	.341
Region Baden Württemberg	.419	.031	Catholic·Data processing	-2.158	.200
Region Bavaria	-.601	.091	Protest.·Data processing	.231	.279
Size 10-99	-1.579	.015	Gr. union dens.	2.764	.499
Size 100-199	-1.042	.016	Gr. union dens.·Region HH	-1.928	.518
Size 200-999	-.614	.015	Gr. union dens.·Region Rhine.L-Palat.	-15.467	.933
Size 1000-1999	-.395	.019	Gr. union dens.·Region Hesse	-18.367	.013
public	1.394	.024	Gr. union dens.·Region Baden Württemberg	1.565	.413
Protest.	1.508	.154	Gr. union dens.·Region Bavaria	5.001	.545
Catholic	1.639	.122	Gr. union dens.·Manufact: Food	- .098	.977
Protest.·Region Lower Sax., Bremen	-.338	.155	Gr. union dens.·Manufact: Textiles	-4.345	1.161
Protest.·Region Rhine.L-Palat.	.386	.140	Gr. union dens.·Manufact: Wood	.917	.743
Protest.·Region Hesse	-3.930	.256	Gr. union dens.·Publishing, printing	.208	.827
Protest.·Region Baden Württemberg	-3.036	.207	Gr. union dens.·Manufact: Rubber, plastic	-7.253	.787
Protest.·Region Bavaria	.127	.136	Gr. union dens.·Manufact: Non-metallic	1.586	.983
Catholic·Region Lower Sax., Bremen	-.083	.118	Gr. union dens.·Manufact: n.e.c.	.003	.965
Catholic·Region Rhine.L-Palat.	-.229	.136	Gr. union dens.·Construction	-3.147	.600
Catholic·Region Hesse	-3.909	.203	Gr. union dens.·Auto sales, repair	-7.871	.783
Catholic·Region Baden Württemberg	-2.408	.156	Gr. union dens.·Wholesale trade	.033	.600
Catholic·Region Bavaria	.643	.112	Gr. union dens.·Retail trade	-1.112	.856
			Gr. union dens.·Hotels, restaurants	-6.340	1.214
			Gr. union dens.·Transport	-4.228	.824
			Gr. union dens.·Auxiliary transport	6.749	.693
			Gr. union dens.·Post, telecommunications	-2.822	1.358
			Gr. union dens.·Real estate	-4.489	1.084
			Gr. union dens.·Data processing	-6.967	.748

Note: The omitted categories are: Education: Medium education; Region: North Rhine-Westphalia; Size: 2000-9999 employees, Industry: Research, other services.

Table 6: Tests of linearity of conditional expectation $E(Y|P(Z,X),X)$

	Specifications of the polynomial of the propensity score			
	Linear	Quadratic	Cubic	Quartic
	coefficient (std. error)	coefficient (std. error)	coefficient (std. error)	coefficient (std. error)
p	.967	.672	.721	.361
	(.068)	(.079)	(.086)	(.126)
p^2		.226	.108	1.258
		(.031)	(.027)	(.310)
p^3			.073	-1.494
			(.015)	(.409)
p^4				.744
				(.192)
L-R chi-square statistic	–	52.74	6.06	14.93

Note: Standard errors rely upon 200 bootstrap replications.

Table 7: Detailed Regression Results for OLS and 2SLS

	OLS with no control variables		**OLS with control variables**		**2SLS**	
Covariate	Coefficient	Robust Std. Err.	Coefficient	Robust Std. Err.	Coefficient	Robust Std. Err.
Intercept	2.717	.001	1.811	.001	1.819	.001
Covered	.073	.001	.036	.001	.028	.010
Age			.040	.000	.039	.000
Age^2			-.000	.000	-.000	.000
Tenure			.012	.000	.012	.000
$Tenure^2$			-.000	.009	-.000	.011
Low education			-.051	.011	-.052	.017
High education			.152	.014	.149	.018
Education n/a			.091	.013	.088	.000
Age·Low education			-.004	.000	-.004	.000
Age·High education			.007	.000	.007	.000
Age·Education n/a			-.004	.000	-.004	.006
Manufact: Food			-.016	.004	-.012	.008
Manufact: Textiles			-.001	.006	.003	.006
Manufact: Wood			.246	.004	.250	.005
Publishing, printing			.046	.004	.049	.007
Manufact: Rubber, plastic			.025	.004	.030	.006
Manufact: Non-metallic			.019	.004	.022	.006
Manufact: n.e.c.			.170	.005	.175	.006
Construction			.047	.003	.052	.007
Auto sales, repair			.044	.004	.049	.005
Wholesale trade			.090	.003	.093	.007
Retail trade			.026	.003	.029	.009
Hotels, restaurants			-.150	.006	-.145	.005
Auxiliary transport			-.020	.004	-.017	.015
Post, telecommunications			.020	.010	.025	.008
Real estate			.170	.007	.172	.006
Data processing			.267	.004	.266	.006
Region HH			.099	.004	.096	.003
Region Lower Sax., Bremen			-.066	.002	-.066	.003
Region Rhine.L-Palat.			.017	.002	.016	.003
Region Hesse			-.037	.003	-.037	.003
Region Baden Württemberg			.015	.002	.014	.003
Region Bavaria			-.028	.002	-.028	.006
Size 10-99			-.114	.003	-.119	.004
Size 100-199			-.084	.003	-.087	.004
Size 200-999			-.044	.003	-.045	.005
Size 1000-1999			-.015	.004	-.016	.004
Public			-.056	.004	-.053	.024

Note: The omitted categories are: Education: Medium education; Region: North Rhine-Westphalia; Size: 2000-9999 employees, Industry: Research, other services.

Table 8: Effect of collective wage bargaining on level of wages

	Average effect	Robust standard error
OLS with no control variables	0.073***	0.002
OLS with control variables	0.036***	0.001
IV 2SLS	0.028***	0.010
LATE	0.029	0.026
ATE	0.008	0.029
ATET	0.016	0.025

Note: Standard errors for LATE, ATE, and ATET rely upon 200 bootstrap replications. Standard errors for OLS and IV 2SLS are calculated using White-Huber formula. ***: Significant at the 1% level, **: Significant at the 5% level, *: Significant at the 10% level.

Table 9: Estimation of equation (7)

Covariate	Coefficient	Robust Std. Err.	Covariate	Coefficient	Robust Std. Err.
Intercept	1.594	.070	Propensity score (Ps)	.361	.126
Age	.054	.003	Ps^2	1.258	.310
Age^2	-.000	.000	Ps^3	-1.494	.409
Tenure	.024	.001	Ps^4	.744	.192
$Tenure^2$	-.000	.000	Ps·Age	-.020	.004
Low education	-.141	.031	Ps·Age^2	.000	.000
High education	.075	.038	Ps·Tenure	-.021	.001
Education n/a	.122	.038	Ps·$Tenure^2$	.000	.000
Age·Low education	-.004	.000	Ps·Low education	.214	.043
Age·High education	.009	.001	Ps·High education	.048	.053
Age·Education n/a	-.004	.001	Ps·Education n/a	-.134	.075
Manufact: Food	-.116	.016	Ps·Age·Low education	-.000	.001
Manufact: Textiles	.012	.027	Ps·Age·High education	-.002	.001
Manufact: Wood	-.052	.014	Ps·Age·Education n/a	.002	.001
Publishing, printing	.155	.018	Ps·Manufact: Food	.160	.025
Manufact: Rubber, plastic	.008	.012	Ps·Manufact: Textiles	-.013	.036
Manufact: Non-metallic	.08	.020	Ps·Manufact: Wood	.142	.022
Manufact: n.e.c.	-.032	.016	Ps·Publishing, printing	.140	.027
Electricity	.232	.024	Ps·Manufact: Rubber, plastic	.067	.021
Construction	.053	.015	Ps·Manufact: Non-metallic	-.071	.028
Auto sales, repair	.008	.018	Ps·Manufact: n.e.c.	.085	.025
Wholesale trade	.090	.011	Ps·Electricity	-.049	.030
Retail trade	.014	.016	Ps·Construction	-.003	.023
Hotels, restaurants	-.163	.027	Ps·Auto sales, repair	.063	.027
Auxiliary transport	-.104	.012	Ps·Wholesale trade	.009	.020
Post, telecommunications	.183	.035	Ps·Retail trade	.030	.026
Real estate	.238	.020	Ps·Hotels, restaurants	.019	.040
Data processing	.257	.011	Ps·Auxiliary transport	.145	.019
Region HH	.079	.013	Ps·Post, telecommunications	-.225	.050
Region Lower Sax., Bremen	-.116	.009	Ps·Real estate	-.126	.031
Region Rhine.L-Palat.	.035	.009	Ps·Data processing	-.055	.031
Region Hesse	-.088	.008	Ps·HH	-.028	.020
Region Baden Württemberg	-.030	.008	Ps·Lower Sax., Bremen	.071	.011
Region Bavaria	-.021	.008	Ps·Region Rhine.L-Palat.	-.022	.013
Size 10-99	-.065	.030	Ps·Region Hesse	.070	.011
Size 100-199	-.075	.028	Ps·Region Baden Württemberg	.086	.010
Size 200-999	.026	.026	Ps·Region Bavaria	-.000	.011
Size 1000-1999	.071	.029	Ps·Size 10-99	-.031	.036
Public	.154	.036	Ps·Size 100-199	.022	.032
			Ps·Size 200-999	-.084	.028
			Ps·Size 1000-1999	-.093	.031
			Ps·Public	-.228	.038

Note: The omitted categories are: Education: Medium education; Region: North Rhine-Westphalia; Size: 2000-9999 employees, Industry: Research, other services.

Table 10: Effect of collective wage bargaining on dispersion of wages

	Average effect	Robust standard error
Unconditional ratio of standard deviations	0.818***	—
Ratio of residual standard deviations	0.803***	—
Average scale ratio using pairwise-matching	0.738***	0.017

Note: Standard errors rely upon 200 bootstrap replications. ***: Significant at the 1% level, **: Significant at the 5% level, *: Significant at the 10% level.