Indicative and Updated Estimates of the Collective Bargaining Premium in Germany

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Abstract
This study provides updated evidence on the union contract differential in Germany using establishment-wide wage data and two estimation strategies. It provides pairwise estimates of the union differential based on separate samples of collective bargaining leavers and joiners vis-à-vis the corresponding counterfactual groups. It is reported that average wages increase by 3 to 3.5 percent after entering into a collective agreement and decrease by 3 to 4 percent after abandoning a collective agreement. Excluding establishments that experience mass layoffs little influences these net findings, although such establishments record wage losses – statistically insignificant for joiners but up to 10 percent in the case of leavers, as compared with the counterfactuals. The backdrop to these new indicative estimates, which are properly conditioned on establishment size and industry affiliation, inter al., is one of wage stagnation and continuing union decline.

JEL Classification: J31, J51

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1. Introduction

The issue of the impact of institutions on wages, including unions and collective bargaining, has long been of interest in Germany as in other nations. Historically, that interest has focused on the covariation of institutions and macroeconomic outcomes; specifically, the relation between bargaining structure and wage inflation and unemployment, often addressed in a comparative context (see, respectively, Fitzenberger and Franz, 1999; Nickel et al., 2005). Altogether less interest has been accorded the effects of collective bargaining on individual wages or on wage dispersion. This differential research emphasis in part reflects data availability – namely, the absence until comparatively recently of good employer-employee linked data, especially those with a longitudinal capacity – and partly the distinctive nature of collective bargaining in Germany.

Arguably, the latter reason was more potent. Collective bargaining in Germany differs markedly from the Anglo-Saxon model. Sectoral or industry-level collective agreements between the relevant union and employers' association (*Flächentarifverträge*) have until recently been the uncontested norm. Under such (relatively centralized) agreements, collectively bargained wages and conditions are typically generalized to non-union members in covered firms, as constitutional considerations rule out discrimination between union members and non-members. In these circumstances, we may speak of a coverage effect resulting from membership of an employer's association that is party to the collective bargaining contract at industry/regional level.[1] Another distinctive facet of the German industrial relations architecture is that collective agreements can also be declared generally binding (i.e. to non-member firms and their employees) via an extension order (*Allgemeinverbindlicherklärung*) issued by the Ministry of Labor and Social Affairs under article 5 of the 1949 Collective Agreement Act (*Tarifvertragsgesetz*).[2] Moreover, there is also a policy of 'orientation' in Germany, namely, the professed tendency on the part of firms practicing individual (as opposed to collective) bargaining to 'align' the terms of their workers' contracts with those set under sectoral agreements, with the suggestion that the coverage of sectoral bargaining in Germany is understated. Taken in conjunction with the facts of historically high coverage, such phenomena not unnaturally directed research away from examination of a union contract differential. *Vulgo*: why compute the coverage premium when all workers are covered?

But times have changed. The distinguishing characteristic of German industrial relations in recent years has been the pronounced decline in sectoral bargaining. For example, between 2000 and 2008, sectoral bargaining coverage as a share of all establishments (employment) fell from 47.3 (57.3) percent to 35.4 (48.1) percent. Meantime, the share of uncovered establishments (employment) rose from 50.1 (35.9) percent to 61.9 (44.2) percent (Addison et al., 2012a).[3] This erosion has in turn served to redirect attention towards an examination of union wage effects. By the same token, there has also occurred a continuing decline in extension agreements from around 2.9 percent of all primary agreements in 2000 to 1.5 percent in 2008. And while there has been some increase in orientation with the growth of the bargaining-free sector, there is no suggestion either in terms of frequency or remuneration that the degree of 'compensation' is other than partial (Addison et al., 2012b). Both tendencies have further stimulated research interest in estimating contract differentials.

German workers can also be covered by firm-level agreements that are typically negotiated by the relevant industry union and the individual employer. Such agreements do of course more readily conform to the Anglo-Saxon pattern, and they expanded significantly in the 1990s (Hassel, 1999). Since then, however, their growth has faltered and today – again in weighted terms – such agreements are found in just 2.7 percent of establishments, accounting for some 7.7 percent of employment (Addison et al., 2012a). In the present treatment, and very pragmatically, we will aggregate over both types of collective agreement, sectoral and firm-level. But note that firm-level contracts may be expected to yield a coverage premium of a roughly similar order of magnitude, not least because they may involve the mutually-agreed application of existing union contracts at industry level. In similar vein, the increasingly decentralized nature of sectoral bargaining in recent years, associated with formally recognized (as well as illegal) deviations from the ruling industry-level agreement (see, inter al., Bispinck, 2004; Seifert and Massa-Wirth, 2005), might suggest that the distinction between sectoral and firm-level bargaining has become increasingly blurred. Both arguments inform our pragmatic treatment of collective bargaining as a composite.

Our longitudinal (unbalanced) firm-level data is extracted from the IAB establishment panel (*IAB-Betriebspanel*). We emphasize that the use of these data for the present exercise is novel. After all, there are other German data sets offering matched employer-employee information. The sister IAB linked employer-employee data set, or *LIAB*, for example combines data from the IAB *and* the Employment Register, while the German Structure of Earnings (GSES or *Gehalts- und Lohnstrukturerhebung*) survey not only contains information on firm-level bargaining coverage but also on that of each individual worker (though not on union membership, which has to be estimated from other data sets such as the German Socio-Economic Panel (GSOEP or *Sozio-ökonomisches Panel*). But neither data set is without blemish. In the case of the LIAB (and IABS which is a 2 percent sample of social security records), for example, the wage data are right censored at the highest level of earnings that are subject to social security contributions. The result is that studies often omit censored wage data – rather than seeking to impute them using a Tobit regression – thereby losing roughly one-eighth of the observations (e.g. Gartner et al., 2010). On the other hand, there are insufficient cases of individual worker mobility among sample establishments in the LIAB to permit the separate identification of unobserved individual and establishment-specific effects. For its part, the GSES has the advantages that hours of work are reported while earnings are not subject to truncation. That said, the GSES is a cross sectional data set and has no longitudinal capacity permitting the researcher to control directly for unobserved individual heterogeneity and thereby facilitating the identification of causal relationships.

The main goal of this study is to obtain indicative and updated estimates of the coverage premium that control for firm fixed effects and selection. The exercise is carried out using techniques that include matching models and separate samples of collective bargaining leavers and joiners and their corresponding counterfactual groups, against the backdrop of considerable flux in German labor institutions. In the process, despite the number of treatments, we obtain estimates of the adjusted union wage gap that fall within a narrow range. Moreover, such deviations as we observe for a mass layoff subsample of the data appear reasonable.

2. Background Literature

2.1 German Studies

Modern studies of the collective bargaining premium in Germany have used the administrative data sets noted above, namely the German Structure of Earnings Survey (GSES) or the linked-employer-employer data set of the Institute for Employment Research (LIAB) sometimes augmented by the IAB Employment Samples (IABS).[4] Of the two main data sets, analyses of the GSES have been the more common.

A useful starting point is the multi-level analysis of Stephan and Gerlach (2005), using linked employer-employee data for a regional subsample (Lower Saxony) of GSES firms in three separate cross sections for 1990, 1995, 2001. Results are reported for the impact of collective agreements on the hourly wage of an average worker employed in an average firm. In 1990 had that worker worked in an otherwise average firm, application of a sectoral (firm-level) agreement would have elevated the wage by 4 (3) percent. And somewhat higher returns to collective bargaining coverage are reported for the later sample years: 9 (12) percent in 1995 and 7 (11) percent in 2001. Stephan and Gerlach also report that the firm-specific rates of return to human capital are lower in firms with collective agreements than in companies with individual contracts in two out of three cross sections while the gender wage gap is lower than under individual bargaining throughout.[5]

Heinbach and Spindler (2007) take a different approach to the wage gap in analyzing the 1995 and 2001 waves of the GSES. They apply the Blinder-Oaxaca decomposition technique to analyze differences in mean log hourly wages between covered and uncovered workers. Specifically, the authors estimate how much of the total log wage difference (€1.15 or 0.143 log points in 1995 and €1.20 or 0.182 log points in 2001) can be explained by characteristics (the selection effect) and how much by coefficients (the bargaining or coverage effect) for three earnings regression specifications, where the first and most parsimonious model includes only human capital variables. It is found that the more parsimonious the specification, the more important is the characteristics or selection effect. That said, most of the increase in the total wage gap in 2001 is mainly explained by the bargaining effect.[6]

In addition to measuring coverage effects at firm level, the penultimate GSES study reviewed here also allows for individual coverage and union density, the latter being imputed from the German Socio-Economic Panel.[7] Using data from the 2001 GSES, Fitzenberger et al.'s (2008) OLS results indicate that firms that follow a collective agreement pay higher wages on average; specifically, the greater the share of workers in a firm covered by a collective contract, the higher are wages on average. The effects are somewhat larger for firm-level than sectoral agreements. But individuals subject to a collective agreement earn less cet. par. And the interaction effect with firm coverage is negative, so that on average a covered worker earns less than his/her uncovered counterpart in the same firm. That said, the effect of increasing firm coverage is positive for both types of individuals, it is just more positive for uncovered individuals who tend to be the more successful workers in the firm and tend not to be unionized. For its part, although union density (in the bargaining region) serves independently to *lower* wages, increases in density reduce or actually negate the negative effect of being a covered individual in a covered firm while reinforcing the positive effects of coverage at firm level. Finally, the individual bargaining coverage result has implications for the wage distribution, and one that receives support from the authors' separate quantile regression analysis; that is to say, the negative effect of individual coverage is stronger at higher quantiles of the conditional distribution (see also Burda et al., 2009).

Of the GSES studies, only Antonczyk (2010) attempts to estimate the *causal* effect of (sectoral) collective bargaining on the wage structure for 2001, making use of two instrumental variables measured at district level – specifically, religious affiliation and union density. Antonczyk reports the average treatment effect (ATE) and the average treatment effect on the treated (ATET) for the level of wages, where the former is the expected gain from coverage of a randomly assigned individual with a given set of observable characteristics, and the latter is the idiosyncratic gain for the individual receiving the treatment. (He also investigates the effect of collective bargaining on wage inequality using pairwise matching.) Antonczyk's initial OLS regression of log wages on a dummy variable indicating whether an employee works in an establishment applying a collective agreement, suggests that wages for the treated are on average 7.3 log points higher than for the untreated. After controlling for the higher tenure of covered employees and the larger size of their employment unit, inter al., the coverage premium shrinks to 3.6 log points, pointing to positive selection into treatment based on observables. In turn, the ATE estimate or union wage effect is just 0.8 log points, while the ATET estimate is 1.6 log points. It is therefore concluded that individuals undergoing treatment have higher unobserved productivity and that they also profit from treatment (i.e. the idiosyncratic gains implied by the positive gap between the ATET and the ATE). Finally, Antonczyk reports that the small measured union effect on the wage level is consistent with a material effect on wage compression.

Studies using the LIAB are altogether less numerous and have been less concerned with distribution, with the principal exception of the influential contribution by Dustmann et al. (2009) that investigates *various explanations* for the growth in German wage inequality in the 1990s.[8] The most relevant LIAB study is by Gürtzgen (2007), using 1995-2002 data for mining and manufacturing establishments. Gürtzgen distinguishes between sectoral and firm-level bargaining for which the raw (real gross daily) wage differentials are 0.160 and 0.206 log points, respectively. After controlling for individual and establishment characteristics (e.g. the capital-labor ratio, per capita quasi-rents, and the presence of a works council) plus dummies for industry, region and time, the corresponding differentials are 0.045 and 0.074. These values rise somewhat – to 0.081 and 0.119 log points, respectively – once the contract arguments are interacted with all other RHS variables. These figures are for western Germany. The corresponding differentials for eastern Germany are 0.316 and 0.185, 0.131 and 0.051, and 0.068 and 0.009, respectively.

But Gürtzgen's interest lies in providing selectivity-adjusted estimates, controlling for the non-random selection of workers with unobservable skills and firms with unobservable attributes into the various contractual regimes. Insufficient switching of workers between firms in the sample does not allow her to identify the component contributions of unobserved worker and firm heterogeneity. Accordingly, her spell-differenced specification provides differentials net of both effects. Her estimates indicate an industry-level coverage premium in western (eastern) Germany of 0.023 (0.002), where the latter estimate is statistically insignificant. For its part, the firm-level premium shrinks in western Germany to -0.003 while rising to 0.020 in eastern Germany, and where this time the former estimate is statistically insignificant. Unlike studies using the GSES, however, there is no evidence from the spell-differenced regression (as opposed to the pooled OLS) that collective bargaining has much effect on the returns to observed worker attributes. The bottom line, therefore, is that there appears to be a small but statistically significant wage premium of around 2 percent for industry-level contracts in western Germany and a similar sized premium for firm-level contracts in eastern Germany.[9]

In sum, the extant literature presents an interesting series of snapshots as to the impact of collective bargaining on wages (and the wage structure) as of circa 2000.[10] There is every indication of a positive union coverage differential at this time, albeit likely well short of some of the initial GSES estimates. But the issue of the scale of the union premium is necessarily clouded both by profound changes in German industrial relations, namely the continuing decline in unionism and collective bargaining coverage since 2000, *and* by issues of causality. But before presenting our own updated estimates it is useful to contrast the German literature with U.S. findings. This will serve to underscore that the unsettled nature of estimates of the union wage gap is not confined to Germany, while also providing updated estimates based on plant-wide averages, albeit based on quasi-experimental methods.

2.2 U.S. Studies

Until very recently U.S. research has largely focused on estimating the effect of unionism using individual data. Such research has typically considered the effects of membership on wages.[11] A broad consensus – at least until most recently (see below) – has been an estimate of the union premium of around 15 percent, much higher than German estimates reviewed earlier.

Cross section estimates of the union gap have treated union membership status as either endogenous or exogenous. Studies using the former approach have attracted considerable controversy; initially because of the tremendous variation in estimates of the wage premium (e.g. Lewis, 1986; Hirsch and Addison, 1987: 123-127), and subsequently because of the difficulty of identifying the selection model/appropriate instruments for union status (Hirsch, 2004: 237-238). Such models are discussed in detail by Hirsch (2004: 238-241). Much of the research reflects U.S. data preoccupations associated with match bias in the imputation of earnings in the principal data set available to researchers – the Current Population Survey (CPS) – and reporting error resulting from the misclassification of union status. Hirsch's research in particular indicates that these two biases serve to materially lower estimates of the union premium (e.g. Hirsch and Schumacher, 2004).[12] They furthermore dominate any tendency toward diminution of the union wage premium through time. In short, there is little indication from such studies of any substantive narrowing in a union markup of around 15 percent on average (see also Blanchflower and Bryson, 2004).

Longitudinal analysis of the union premium is concerned with the correlation between unmeasured skill and union status which, if positive, will yield upwardly biased estimates of the union premium.[13] The U.S. debate has centered on whether unionized workers are likely to be systematically more or less skilled than nonunion labor. Here, there are two opposing influences. On the one hand, in being able to select from a queue for union jobs firms can avoid hiring workers in the lower tail of the distribution. On the other, workers from the opposite tail are unlikely to be in the queue because of wage compression.[14] Studies identifying a union wage effect from the wage change of individuals that switch union status – thereby controlling for unobserved individual heterogeneity, assuming that changes in status are exogenous – yield lower estimates of the union premium than those based on wage levels. But misclassification bias is a long-recognized issue here because although misclassification rates are small, so too is the incidence of switching. After correction for misclassification, a number of studies conclude that longitudinal estimates do not indicate that omitted ability bias has more than a rather modest impact on estimates of the union age gap (see, for example, Freeman, 1984; Card, 1996).[15]

More recent analyses of representation elections in the United States perhaps bear closer correspondence with the German literature. The starting point is DiNardo and Lee's (2004) comparison of establishments where unions became recognized by a close margin of the vote with those in which they barely lost, over the interval 1984-1999, employing a regression discontinuity methodology (see also Frandsen, 2012). That is, they estimated a discontinuity in the relationship between wages and the vote share at the 50 percent vote threshold, where evidence of a discontinuous relation between the vote share and wages is deemed to be the causal impact of unionization by eliminating any confounding selection and omitted variable biases. DiNardo and Lee reported small and mostly negative union wage effects – the largest positive wage effect within two standard errors of the point estimate being just 0.014. They also attempted to compute any union threat effect on wages using an event-study approach for those elections where unions lost and failed to gain recognition. Wages were relatively stable in the pre-election period and for up to 11 years after the election (where a 3 percent increase by year 3 could be ruled out). DiNardo and Lee explained their results as reflecting the (omitted) role of unobserved firm heterogeneity in studies using individual data.[16]

The study of Lee and Mas (2012) follows the much longer event-study tradition (e.g. Ruback and Zimmermann, 1984) and examines the effect of new unionization on publicly-traded firm's equity value, 1961-1999. The authors use a long panel – of up to 4 years before and after the representation election – of high frequency data on stock market returns for each firm in the sample. The event-study analysis revealed substantial losses in market value following a union election of $40,500 per unionized worker, which value is equated with a union premium of around 10 percent. The cumulative average returns of firms are found to be close to the benchmark portfolios matched on a firm's characteristics up to an election at which point the actual and the benchmark returns diverge. In addition to addressing the issue of how equity values respond to certification elections, the authors also estimate event-study models for elections with varying degrees of union support. There is a negative association between abnormal returns and vote share. Although there is no discernible discontinuity at the 50 percent union vote threshold, a greater than 60 percent share for example is associated with negative cumulative average returns in the range 20 to 30 percent. A formal regression discontinuity estimate of a union victory is statistically indistinguishable from zero, allowing these findings to be reconciled with DiNardo-Lee result without of course vindicating the regression discontinuity methodology.[17]

In sum, the U.S. research has been preoccupied with many of the same theoretical concerns as the German literature, even if the empirical studies reflect distinctive industrial relations structures. And many of the research findings are no less settled. As a final example, one might take the case of earnings inequality, briefly touched upon earlier and rather less commonly examined in the United States. Although on this occasion, the directional influence is the same – namely, an inverse correlation between unions and inequality (e.g. Card, Lemieux, and Riddell, 2004) – U.S. observers evince greater skepticism as to causality than their German counterparts (e.g. Hirsch, 2004: 256). Accordingly, there is disputation here as to what might happen to inequality among *union workers* were their unions to disappear – further complicating the computation of *nonunion* wages in the absence of unions.

3. The Dataset and the Raw Collective Bargaining Differential

We begin by briefly introducing the IAB Establishment Panel or *Betriebspanel*, a full description of which can be found in Fischer et al. (2009). This dataset is based on a stratified random sample of the population of all establishments with at least one employee covered by social insurance. Currently, the stratification has a basis in 19 industries and 10 employee size classes. As the Panel was set up to serve the needs of the Federal Employment Agency (*Bundesagentur für Arbeit*), fairly detailed information on the composition of the workforce and its growth trajectory constitutes an important part of the Panel questionnaire. Further questions cover wages as well as general information on the plant including its collective bargaining status. The Panel was initiated for western Germany in 1993 and extended to eastern Germany in 1996.

The unit of analysis is the establishment, although for convenience the terms 'establishment' and 'firm' will often be used interchangeably in what follows. Our unbalanced establishment panel covers the 2000-2008 interval, it being decided not to use survey data prior to 2000 and after 2008 because of changes in industry classification. In each year we have approximately 7 to 8 thousand establishments, after excluding those with always less than 5 employees, the non-for-profit sector, and agriculture. Establishments recording more than one change in collective bargaining status were also excluded from our sample (see section 5 below), as were those plants whose collective bargaining status was unreported.

Our firm-level wage variable is defined as the wage bill per full-time employee. This variable was obtained from three pieces of information extracted from the raw survey: first, the total wage bill, that is, the gross wages paid to workers, excluding social security contributions and holiday allowances; second, the total number of employees, excluding apprentices, temporary agency workers, and certain other residual categories; and, third, the number of part-time workers, all such information pertaining to end-June of the corresponding year. So as obtain the number of full-time equivalents, we assume two part-time workers are equivalent to one full-time worker.[18] As was noted earlier, one crucial advantage of the wage information contained in the nationally representative *Betriebspanel* is that it exactly reports the sum of all wages paid, without any right- or left-censoring. Nominal wages were deflated using the consumer price index throughout.

Given that the Establishment Panel was designed to facilitate labor market policy, its information on establishment characteristics is fairly detailed. We focus on a subset of these characteristics, including the shares of skilled, part-time, female, and fixed-term contract workers, and whether or not the establishment uses up-to-date technology, is owned by foreigners, is individually-owned, was founded before 1990, and engages in exporting. We also identify whether an establishment is a part of a multi-establishment entity and if it has a works council present. A variable capturing future sales – whether these are expected to be stable, increasing, or decreasing – is also deployed. In addition to these arguments, a full set of industry, sector, region, and establishment-size dummy variables complete the list of regressors, summary statistics on which are provided in Appendix Table 1.

(Tables 1 and 2 near here)

Descriptive information on wages by collective agreement type is summarized in Table 1. (The corresponding unweighted information on collective bargaining coverage is provided in Appendix Table 2.) Averaging over both types of collective agreement, for example, indicates a collective bargaining 'premium' of 24 percent in 2000, somewhat lower than the 35 percent wage gap obtaining in 2008. Since such raw premia can be expected to capture more than a true bargaining effect, we next condition the change in wages on the observed collective bargaining transitions.[19] For example, in comparing the change in real wages in two consecutive years for the categories of collective bargaining leavers and always-members, we should expect the wage change of leavers to fall below that of the comparison group of always members, and symmetrically for joiners versus never members. This information is displayed in Table 2. In 2000-2001, for example, the observed ratio of relative wage changes is surprisingly 1.0342 in favor of leavers. That is to say, leavers enjoyed larger increases than did always members. This result is contrary to what one might expect, and perhaps reassuringly the wage changes observed in 2002-2003, 2003-2004, and 2007-2008 more closely accord with our priors. As far as joiners (versus never members) are concerned, switching implies higher wages than staying uncovered in 2003-2004 through 2007-2008. (The corresponding raw transitions into and out collective bargaining are given in Appendix Table 3.) While interesting, this additional information on the implicit wage premium is very preliminary since we are not controlling for anything other than the change in collective bargaining status. We should also point out that examining real wage changes over two-year intervals – 2000-2002, 2002-2004, etc. – did not present any obvious improvement in the sense that joiners more consistently gained and losers more consistently lost. Indeed, the evidence was quite to the contrary. Our argument thus remains the same: a sufficiently-specified control function is required to address the issue, although we shall present a separate sub-analysis based on those establishments that experienced mass layoffs in a further control for firm heterogeneity.

With these preliminaries behind us we therefore turn to the formal modeling exercise.

4. Econometric Modeling of the Collective Bargaining Effect on Firm Wages

4.1 Regression Analysis (Difference-in-Differences)

Investigation of the effects of collective bargaining status on establishment wages involves speculation as to how (average) wages would have developed in the absence of the institution. Analysis of the problem therefore requires use of the standard Roy-Rubin model of potential outcomes (Roy, 1951; Rubin, 1974).

As a general framework, let us denote collective bargaining status by a binary variable U_{it} and assume that in a given year *t* establishment *i* is either covered by a collective agreement in which case we observe the corresponding average wage y_{1it}, or it is not covered and we observe y_{0it}. Further assuming that the outcome y_{0it} is a (linear) function of a time-invariant unobserved individual effect α_i, time-specific unobserved factors γ_t, and observed establishment characteristics X_{it}, where X is a vector row and β a vector column, we have

$$E(y_{0it}) = \alpha_i + \gamma_t + X_{it}\beta. \quad (1)$$

Under the assumptions that the expected outcome is independent of U_{it}, conditional on X_{it} ,

$$E(y_{0it}|\ \alpha_i,\ t,\ X_{it},\ U_{it}\ ) =\ E(y_{0it}|\ \alpha_i,\ t,\ X_{it}), \tag{2}$$

and that the causal effect of participation is additive and constant,

$$E(y_{1it}|\ \alpha_i,\ t,\ X_{it}\ ) =\ E(y_{0it}|\ \alpha_i,\ t,\ X_{it}) + \delta\ , \tag{3}$$

we can specify the general (unobserved effects) model as

$$y_{it} = \alpha_i + \gamma_t + \delta U_{it} + X_{it}\beta + \varepsilon_{it}, \tag{4}$$

where $E(\varepsilon_{it}|\ \alpha_i,\ X_{it},\ U_{it}\ ) = 0$.

Given that for collective agreement joiners (never members) $\Delta U_{it} = 1$ ($\Delta U_{it} = 0$), model (4) gives

$$E(\Delta y_{it}|\Delta\ U_{it} = 1\ ) - E(\Delta y_{it}|\Delta\ U_{it} = 0\ )$$

$$= \delta + E(\Delta\varepsilon_{it}|\Delta\ U_{it} = 1\ ) - E(\Delta\varepsilon_{it}|\Delta\ U_{it} = 0\ )$$

$= \delta$ (by condition (2)).

In particular, for *t = 1, 2*, and for the set of joiners and never members, model (4) becomes

$$y_{it} = \alpha_i + \gamma_1 + \gamma_2 d2_t + \delta U_{it} + X_{it}\beta + \varepsilon_{it}, \tag{5}$$

where $d2_t$ is a 1/0 dummy denoting *t=2* , while $U_{it} = 0$ for *t=1, 2* if establishment i is a never member and $U_{i1} = 0$ and $U_{i2} = 1$ if *i* is a joiner.

Taking then the first difference of (5), we have

$$\Delta y_{it} = \gamma_2 d2_t + \delta U_{it} + \Delta X_{it}\beta + \Delta\varepsilon_{it},$$

which is equivalent to

$$\Delta y_{i2} = \gamma_2 + \delta U_{i2} + \Delta X_{i2}\beta + \Delta\varepsilon_{i2}. \tag{6}$$

We have therefore the usual and important result that, at *t=2*, regressing Δy_{it} on a constant, ΔX_{it}, and ΔU_{it} one obtains the treatment effect, $\hat{\delta}$. Moreover, ignoring the ΔX_{i2} term, from model (6), we obtain the difference-in-differences (DD) estimate $\hat{\delta}_{DD}$, as follows $\hat{\delta} = \overline{\Delta y_{Joiner}} - \overline{\Delta y_{Never}} \equiv \hat{\delta}_{DD}$. In short, an estimate of δ can ultimately be obtained by the difference in the average wage change between the two groups (of joiners and never members).[20] *Mutatis mutandis* for *leavers* versus *always members*. All that is required here is maintenance of the adequate DD assumption, or $E(y_{0i2} - y_{0i1}|U_{i2} = 1) = E(y_{0i2} - y_{0i1}|U_{i2} = 0)$. This is equivalent to assuming the presence of a time-invariant individual effect in model (4).

In our empirical analysis of the 2000-2008 period, we will estimate model (6) using information on two consecutive years firstly in separate regressions and, secondly, in a pooled manner. The latter implementation simply regresses Δy_{it} on a constant, ΔX_{it}, and ΔU_{it}, where *t = 2, 3, …,9*, while ignoring any individual (establishment) history. The pooled version of the conditional difference-in-differences approach next described follows a similar strategy.

4.2 Conditional Difference-in-Differences (Matching)

Up to this point, we have obtained the effect of collective bargaining status on average wages after having simply entered the vector of covariates X into the unobserved effects model (4), the expectation being that the addition of a sufficiently large number of control variables will effectively purge the analysis of any correlation between unobservables and outcomes.[21] In the next step, however, rather than relying exclusively on a parametric model and a linear function form we will instead construct a matching control group, and then compute the difference in the average outcome across *participants* and *non-participants* to estimate the effect of participation (namely either separate act of joining or leaving).

The key point of the matching approach is to find, say, two units *i* and *i'* with the same probability of participation conditional on X – or the same propensity score $p(X)$ – such that one unit receives treatment and the other does not. The goal is therefore to randomize participation ex-post by selecting two groups of establishments – such as joiners and never members – with, presumably, an identical probability of joining collective agreements but which by mere *accident* are not all treated in the treatment period.

More formally, consider again the quadruple $(y_{0i}, y_{1i}, U_i, X_i)$, where U_i is the treatment dummy (i.e. $U_i = 1$ if *i* covered by a collective agreement and $U_i = 0$ if *i* is not covered), and y_{1i} and y_{0i} are the corresponding outcomes. X_i is a vector of *observables*. Let us further assume firstly that $E(y_{0i}|X_i, U_i = 1) = E(y_{0i}|X_i, U_i = 0) = E(y_{0i}|X_i)$ (i.e. conditional mean independence) and secondly that $p(X) < 1$.

Under these assumptions the average impact of treatment on the treated (*ATT*) is given by

$ATT^X \equiv E(y_{1i} - y_{0i}|U_i = 1)$

$= E\{E(y_{1i} - y_{0i}|U_i = 1, X_i)|U_i = 1\}$ (by iterated expectations)

$= E\{E(y_{1i}|U = 1, X_i) - E(y_{0i}|U_i = 1, X_i)|U_i = 1\}$

$= E\{E(y_{1i}|U_i = 1, X_i) - E(y_{0i}|U_i = 0, X_i)|U_i = 1\}$.

Simplifying the notation, we can write

$ATT^X = E(\delta_X|U_i = 1).$ (7)

Accordingly, if X_i is a discrete variable, we have

δ_X=$\sum_x \delta_x Prob(X_i = x)|U_i = 1,$

where δ_x is the treatment effect at a particular value, say $X_i = x$. (If, on the other hand, X_i is a continuous variable, one can use a stratification of X_i and work with as many groups as required.)

Alternatively, one may use the propensity score function. In this case, by the propensity score theorem, $y_{0i} \perp U_i|X_i$ implies $y_{0i} \perp U_i|p(X_i)$, giving

$ATT^{ps} \equiv E(y_{1i} - y_{0i}|U_i = 1)$

$= E\{E(y_{1i} - y_{0i}|U_i = 1, p(X_i))|U_i = 1\}$ (by iterated expectations)

$= E\{E(y_{1i}|U_i = 1, p(X_i)) - E(y_{0i}|U_i = 1, p(X_i))|U_i = 1\}$

$= E\{E(y_{1i}|U_i = 1, p(X_i)) - E(y_{0i}|U_i = 0, p(X_i))|U_i = 1\}.$ (8)

Clearly, ATT^{ps} in equation (8) resembles the average treatment effect on the treated (ATT^X) in (7). Having obtained a probit or logit estimate of $p(X_i)$, one either stratifies $p(X_i)$ and works with a certain number of groups of treated and untreated units or one attempts to find for every single treated unit the corresponding propensity score matched unit, or units. If for some participant we have $\hat{p}_i = 1$, then the impact of treatment will have to be redefined to comprise only the estimated impact of the treatment on the treated for those whose propensity scores lie within the common support region, $0 < \hat{p}(X_i) < 1$, discarding all those participants with $\hat{p}_i(X) = 1$. But even for a participant with a score in the support region it will not be always possible to find a perfect non-participant with exactly the same propensity score. The 'distance' between the propensity score of, say, participant *i* and the propensity score of the matched non-participant *j* can be then used as a weighting factor in the differenced outcome (see below).

In this general framework, it can be shown (see Smith and Todd, 2005) that a typical (cross-section) matching estimator is given by

$$\hat{\delta}_{ATT}^{ps} = \frac{1}{n_1} \sum_{i \in I_1 \cap S_P} [y_{1i} - \hat{E}\,(y_{0i} | U_i = 1, p(X_i)], \qquad (9)$$

where $\hat{E}(y_{0i} | U_i = 1, p(X_i) = \sum_{j \in I_0} W(i,j) y_{0j}$, I_1 is the set of participants, n_1 is the number of persons in the set $I_1 \cap S_P$, S_P being the support region, I_0 is the set of non-participants, and $W(i,j)$ gives the corresponding weights that will depend on the distance between $p(X_i)$ and $p(X_j)$.

However, it is probably more realistic to abandon the underlying (cross-section) identification assumption, that, after conditioning on X, the conditional mean independence is satisfied, and instead assume that a comparison between treated and matched untreated groups in a single year is not sufficient to capture all unobserved heterogeneity. Thus, under the hypothesis that not all systematic differences between participants ($D_i = 1$) and non-participants ($D_i = 0$)are captured by X, and assuming that (unobserved) characteristics are time-invariant, by differencing the outcomes over time one will be able to obtain an improved estimate of the treatment effect. Specifically, we obtain the difference in differences matching estimator for longitudinal data, namely

$$\hat{\delta}_{DD}^{ps} = \frac{1}{n_1} \sum_{i \in I_1 \cap S_P} \{(y_{1ti} - y_{1t'i}) - \sum_{j \in I_0 \cap S_P} W(i,j)(y_{0tj} - y_{0t'j})\}. \qquad (10)$$

This is an immediate extension of $\hat{\delta}_{ATT}^{ps}$ in equation (9) above that holds under the appropriate DD identification hypothesis

$E(y_{0it} - y_{0it'} | D_i = 1, p(X_i)) = E(y_{0it} - y_{0it'} | D_i = 0, p(X_i))$, with $t > t'$.[22]

As a first step in our matching approach, therefore, we will estimate a probit model in order to obtain the predicted probability of being treated, $\hat{p}(X)$, where X comprises an extended set of covariates. Once the predicted probability is obtained, we then apply different matching algorithms: nearest neighbor matching with replacement, radius matching, and kernel matching. Nearest neighbor matching with replacement consists of choosing for a control the (non-participant) unit with the nearest propensity score. In Radius matching one selects for the control group any match within a pre-selected distance. (In our case, this distance, *d*, is set at *d* = 0.01 and *d* = 0.005.) Finally, with kernel matching almost no establishment is excluded from the control group, with the corresponding weights depending on the propensity score distance. (See Caliendo and Kopeinig, 2008, for a survey of these various alternatives.)

Since exact matching is not feasible in practice, another crucial aspect of this approach is to assess its quality with respect to the similarity of the two groups – and the closer the characteristics between treated and comparison groups the better – and the variance of the estimated treatment effect. As far as the quality of the match is concerned, there are two main alternative measures available in the literature: the standardized bias (SB) and the t-test in mean differences. The former takes the difference in sample (after treatment) means in the treated and matched control groups, standardized by the corresponding sample variances, to yield $(\bar{X}_1 - \bar{X}_0)/[\sqrt{((V_1(\bar{X}_1) - V_0(\bar{X}_0))/2}$, where $\bar{X}_j(V_j)$ is the mean (variance) in group *j* = 0, 1. In practice, 0.05 is often taken as the critical value, so that a successful matching will always imply SB<0.05. The t-test is a standard test on the null that the mean of each included covariate is the same across the two groups. In turn, our preferred route to estimate the variance of average treatment effects will be to use the method of bootstrapping, which amounts to re-estimating the results R times and therefore to R bootstrap samples and R estimated average treatment effects. The distribution of these means approximates the sampling distribution of the population mean, which allows us then to compute the bootstrapped standard errors of the treatment effect.

5. Results

We focus on the private, for-profit sector and on establishments with at least five employees. The employment size cut-off is imposed to control for the presence of works councils – five employees being required to trigger the formation of a works council – and to avoid excessive volatility with respect to collective bargaining coverage associated with very small establishments.[23] Further, since collective bargaining status is not always reported in the IAB survey (this is true for 10 percent of the cases in a given year), we consider only those units for which this status is always provided in two consecutive years. As a result, we lose some 20 percent of all establishments. In short, we opted not to impute collective bargaining status in these missing cases. Finally, multiple switchers – otherwise includable units with more than one change in status over the sample period – were also dropped from the sample. This excision was applied because we suspect that most such cases are the result of faulty coding. (Nevertheless, results based on the sample of all units including those with imputed collective bargaining status and those with multiple collective bargaining transitions are available from the authors upon request. We found no material changes in the results as a result of their incorporation.)

5.1. Difference-in-Differences: Findings

Table 3 presents the effect of *joining* a collective agreement by separate one-year transition periods, from 2000-2001 through 2007-2008. In panel (a) of the table, we report the results of a regression in which the changes in wages at establishment level are a function of a dummy variable denoting establishment transition status (i.e. as a joiner or never member), with all control variables in first differences. In panel (b) the control variables are in base-year levels. Table 4, which charts the effect of *leaving* a collective agreement versus remaining covered by one, is organized in similar fashion. And in the last column of each table is given the results from pooling the observations from all separate one-year transition samples. Here we are respectively comparing the wage development of joiners (leavers) versus never members (always covered), assuming that unobserved macro effects on wage growth that are presumably different in each transition period can be captured by year dummies. Again observe that wages are expressed in real terms throughout.

(Tables 3 and 4 near here)

Two main findings stand out. First, switching in or out of collective bargaining implies *on average* a change in wages of 3 to 3.5 percent, positive for joiners and negative for leavers (see the last columns of Tables 3 and 4). Second, with a few exceptions, the evidence based on separate samples scarcely provides any statistically significant effect of collective agreement transitions on establishment wages. This latter result turns out to be quite relevant and likely indicates why analysts have been unable to obtain at establishment level a robust collective bargaining premium using single-year transitions. Indeed, random selection of any one cell from the first eight columns of Table 3 or Table 4 yields either implausible estimates of the premium derived from joining/leaving a collective agreement of any type or, more likely, statistically insignificant estimates. In short, the strategy of enlarging the sample as much as possible by including all joiners and leavers (and corresponding control groups) in a single, pooled sample is crucial.

Our use of a relatively large set of regressors makes it more likely that a proper control function has been used in the difference-in-differences (OLS) regression. But whether we are using proper control groups can only be addressed in a matching framework.

5.2. Conditional Difference-in-Differences (Matching): Findings

Tables 5 and 6 present alternative estimates of the effects of entering into or abandoning a collective agreement based on propensity score matching. Note that the nearest neighbor results are more or less the same as those discussed below, but since the quality of the match is always lower we focus on the two other variants of the model. Further note that the evidence for separate transition periods is now omitted. In the pooled cases presented here, observe that the treatment group is again made up of all joiners/leavers that happen to join/leave in any single year, 2001-2008. For its part, the group of (matched) untreated establishments is made up of all units that in any two consecutive years did not switch into (out of) collective bargaining. By way of a caveat, since an establishment *j* is a joiner (leaver) after being a never member (always member) for some time, if *j* is in the observation window for more than one year prior to the event of joining there is a possibility that the self-same establishment might be at once a joiner (leaver) and a member of the control group. Having identified such cases, we checked the sensitivity of our results to their exclusion. We can confirm that our findings were not materially affected, the results being available from the authors upon request.

(Table 5 near here)

As is apparent, the conditional difference-in-differences estimates based on propensity score matching of the treated and untreated groups are strikingly close to the regression DD results. That is to say, the effect of joining/leaving is both symmetric and again in the 3 to 4 percent range for the pooled case. For the kernel matching case in Table 5, we see that the collective bargaining premium obtained using the sample of joiners and never members lies between 3.2 and 3.5 percent, very close to the DD estimates in Table 3. In turn, the estimated collective bargaining wage premium using leavers and always members in the second row is now slightly higher than before, at 3.8 percent. All wage premia are statistically significant at either the .01 or .05 level.

(Table 6 near here)

In Table 6 we present results from using a different matching algorithm, namely radius matching. For each sample – joiners versus never members and leavers versus always members – the treatement effect is again strikingly in the 3 to 4 percent range. As a peripheral issue, note that the radius caliper methodology is out-performed by the kernel technique in respect of individual year transitions as virtually none of the treatment effects is statistically significant.

An observation on the quality of the matching between treated and untreated groups might usefully be added. Without exception, the mean standardized bias (the *mba* row in Tables 5 and 6) was substantially reduced after matching. This means that the difference in mean characteristics across treated and untreated groups has been reduced or, equivalently, that the groups have, after matching, approximately the same observed characteristics. Specifically, after matching, the *mba* statistic is always smaller than 5 percent in the pooled case.[24]

Finally, we also provide the pseudo-R^2 for the propensity score/probit model run after matching the treated and untreated groups. As in all cases the pseudo R^2 statistic tends to be very low, it follows that after matching on (observed) firm characteristics the treatment is fairly at random. The LR statistic in the last row of each panel also shows that after matching we cannot reject the null that the set of regressors in the propensity score probit is not jointly statistically significant. And although not reported in Tables 5 and 6, the number of off-support units is always modest, never exceeding 5 percent of the total number of untreated units either in the case of joiners or leavers.

5.3. Robustness: Results for the Subsample of Mass Layoffs

We now examine the robustness of our results for a sub-sample of establishments in which the reduction in the workforce exceeded a certain threshold, deemed collective dismissals. The selected threshold is based on §17 of the 1951 *Kündigungsschutzgesetz* (KSchG or Employment Protection Act). Under this section, formal notification to the Federal Employment Agency is required whenever an employer plans to lay off, within a period of 30 days, (a) 5 employees for all establishments with more than 20 and less than 60 employees, (b) 10 percent of the workforce or more than 25 employees where the number of employees is at least 60 and less than 500, and (c) 30 or more employees if the establishment has at least 500 employees.

Note that we do not have information on intra-annual employment changes; rather, we can only observe employment changes at establishment level from year *t* to year *t+1*. But using exactly the same estimation sample as in Tables 3 and 4, 7 percent of all collective bargaining leavers are flagged as mass layoffs. This compares with 11 percent among the group of always members. The corresponding estimates for joiners and never members are 6 percent and 4 percent, respectively. In short, mass layoffs are roughly the same in percentage terms among leavers and joiners, but they are much less common among never members than for always members.[25]

Next, we re-ran model (6). The results are presented in Table 7, but only for the pooled case given the limited number of annual mass layoffs yielded by our sample. Assuming that mass layoffs might provide a less contaminated sample – in the sense that endogenous *worker* separations are less of a problem – the results in Table 7 suggest that the wage effect of leaving a collective agreement is probably larger than the -3.5 percent effect found in Table 4. Indeed, the estimated effect for the mass layoff sample is between -8.2 and -9.9 percent (see column (2) of table 7). For the subsample of joiners (and never members), shown in column (1) of table and among whom mass layoffs were less common, the results are necessarily weaker than those in column (2) and we do not find any statistically significant evidence of the presence of any collective bargaining wage effect in this case. Furthermore, this result for joiners is not unexpected as short-run effects are likely to dominate: if a given establishment is laying off a sizeable proportion of its workforce, wage gains are unlikely over a one year horizon.

(Table 7 near here)

We turn in conclusion to the results of the regression exercise once we net out the mass layoffs. The findings are provided in the bottom half of Table 7 (net sample). They show that the evidence earlier reported in Tables 3 and 4 is insensitive to the exclusion of mass layoffs. In other words, whenever the employment changes are not too dramatic (specifically, below the collective dismissals threshold), we have the key result that the wage effect of collective bargaining coverage is symmetric and around 3.5 percent. In this context – and given the relatively small number of mass layoffs in the sample and the difficulty in identifying them in practice – we have no strong reason to seriously question the results in Tables 3 and 4. The possible caveat is that whenever there is a sizeable reduction in the workforce, leaving a collective agreement is likely to imply a larger reduction in average wages than the benchmark loss of 3.5 percent indicated in Table 4.

6. Conclusions

Notwithstanding the steadily increasing number of studies seeking to determine the earnings impact of unionism and collective bargaining in Germany, the magnitude of the union wage premium is unsettled – no less so than in the United States. Be it due to the lack appropriate wage data, the difficulty of constructing a proper control group, or assembling a sufficient time series, the range of estimates of the adjusted union contract differential surveyed in section 2.1 as of circa 2001s too wide for comfort.

The principal goal of the present inquiry has been to derive selection-adjusted estimates of the effect of collective bargaining on average wages at the level of the firm, and thereby to inform the debate on the scale of the wage premium. That is, we have sought to obtain solid indicative estimates extracted from a sufficiently representative sample of the economy based on a comparatively long and updated time frame.

It is fully recognized that the effect of collective bargaining on wages may be expected to differ across individuals and even by type of collective agreement. But there are enough reasons to sustain a broader focus at this stage. And that has been to provide a reasonable ballpark estimate of the wage gap of a covered establishment vis-à-vis its uncovered counterpart. To this end, we have aggregated pragmatically across two types of collective bargaining – sectoral and firm-level agreements – against the backdrop of the pronounced decline in collective bargaining as a whole. Clearly, these collective agreements differ much more from individual contracts than from each other, and have arguably become closer with the decentralization of sectoral bargaining.

What are our findings? In the first place, we confirm that unionism/collective bargaining still attracts a wage premium. The size of the markup is in the 3 to 4 percent range. This estimate is well below average gains of 10 to 12 percent reported in some of the German cross-section literature. Technical issues apart, our results are prima facie consistent with the (ongoing) decline in and decentralization of collective bargaining. Secondly, we found symmetric effects: firms leaving collective bargaining experienced wage reductions that are very similar in magnitude to the wage gains recorded by joiners. Observe that this outcome was not imposed by any particular methodology and strengthens our central finding as to the magnitude of the wage gap. That said, by way of qualification, different results are found for a small subset of the data. Specifically, for those firms engaging in mass layoffs modest wage losses are experienced upon joining a collective agreement and fairly heavy losses when abandoning a collective agreement vis-à-vis the counterfactuals. Nevertheless, although mass layoffs clearly merit separate identification, netting out such establishments scarcely dented our central finding of a 3 to 4 percent contract premium.

Finally, of course, the present study has looked at just one aspect of bargaining impact. Left open has been its effect on the *wage structure*. A small union premium on average might mean that unions have devoted more of their energies to narrowing differentials. Historically, there is some suggestion in the German (*and* U.S.) literature of strong positive effects at lower reaches of the wage distribution. But if the small average differential charted here is indicative of a reduction in union bargaining power, the narrowing process observed in earlier years may itself have been reversed. Investigation of this issue is an important topic for future research.

ENDNOTES

1. But not of an individual union membership effect on wages, the focus of much of the Anglo-Saxon empirical literature.

2. Other elements of extension machinery in Germany are the Posted Workers Act (*Entsendegesetz*) of 1996 and the Act on Minimum Working Conditions (*Mindestarbeitsbedingungengesetz*) of 1952, both of which pieces of legislation were extended in 2009.

3. No less important has been the decline in union density. Over the same interval, union density fell from 24.6 percent to 19.6 percent (Bispinck et al., 2010).

4. For earlier studies using the German Socio-Economic panel (GSOEP) that focus on changes in the wage structure, see OECD, 1996; Steiner and Wagner, 1998. It is widely acknowledged that the GSEOP measures wages with much less precision than in administrative data where misreporting is subject to severe penalties.

5. Companion studies by Gerlach and Stephan (2006a; 2006b) also point to lower wage dispersion under collective than individual bargaining. See also the regional study of Bechtel et al. (2004).

6. Heinbach and Spindler also assess the distributional effects of bargaining using quantile regression decomposition techniques. For both sample years it is reported for all three models that the bargaining effect is highest in the lower parts of groups' wage distributions and decreases with increasing wages, although by 2001 the bargaining effect is reported in all parts of the wage distribution. In common with all other GSES studies, Heinbach and Spindler's analysis suggests that bargaining reduces inequality.

7. For separate studies of the union density-wage nexus, see, inter al., Fitzenberger and Kohn (2005); Büttner and Fitzenberger (1998).

8. Up to 28 percent of the increase in inequality in the lower end of the wage distribution at this time is attributed to union decline.

9. That said, Gürtzgen also provides an analysis of the wage consequences of transitioning between contract types based on trend-adjusted difference-in-difference estimators that might suggest that the former estimate is downwardly biased and the latter effect confounded by wage losses experienced by those joining sectoral agreements.

10. An exception is the very recent study by Antonczyk et al., (2010) that tracks wage inequality up to 2006.

11. There is almost no U.S. work on the effects of coverage on the wage gap, not only because of the relatively few covered nonunion members in the Unites States but also for data reasons including misclassification. What evidence there is, points to a much smaller coverage than membership premium (e.g. Schumacher, 1999; Budd and Na, 2000), although the jury is still out on the causes (see Hirsch, 2004: 257-258).

12. The scale of the problem is as follows, given an estimate of the union premium in 2001 of .13 log points using the full CPS sample comprising workers with and without imputed earnings. Correcting for match bias (by excluding those with imputed earnings) raises the wage premium to .18 log points, while correcting for misclassification bias (of just 2 percent) increases it to .24 log points.

13. In the event of a positive correlation between unmeasured skills and unionization, a movement from a nonunion (union) to a union (nonunion) job should entail a small wage gain (loss).

14. See also Wessels' (1994) argument that upgrading on the part of employers may not be expected on theoretical grounds.

15. Hirsch (2004: 254-255) further notes that longitudinal estimates that control for skill groups have broadly similar union gap estimates by skill. This is the result of two opposing forces: positive selection at the lowest skill levels and negative section at the highest levels. After sorting, so the argument runs, wage effects vary little by skill group.

16. They also allude to the lingering imprecision of the union treatment effect in longitudinal studies using household-level data where randomly chosen individuals are presumed as moving from a randomly chosen nonunion employer to a randomly chosen union employer.

17. Note that the regression discontinuity methodology is unable to provide a counterfactual for the set of elections where the large majority of workers vote in favor of unionization. The counterfactual in the event-study approach is what would have happened in the absence of any representation election.

18. Although there is no information in the *Betriebspanel* on hours worked at the individual (worker) level, the share of part-time workers by different categories of working hours is available. We experimented with various strategies for allocating part-timers and concluded that the rule 'two part-timers equal one full-timer' adequately represented these data.

19. There are transitions into and out of works councils as well. But these movements are much less frequent and we will here simply treat works council status as an additional control variable.

20. It follows from model (6) that $\hat{\gamma}_2 = \overline{\Delta y_{Never}}$ (Wooldridge, 2002: 283-284). These results show a more general result that $\hat{\delta}_{DD}$ can ultimately be obtained from a general unobserved

model (4), and that, for *T=2* (that is, a panel with two years), first differences and difference-in-differences yield the same estimate of the treatment effect.

21. An alternative to a full set of control variables is the use of a more parsimonious equation in which the estimated propensity score term, $\hat{p}_i$, with $p_i = p(X_i) \equiv Prob(U_i = 1|X_i)$, is added to the linear regression of Δy_{it} on a constant, ΔX_{it}, and ΔU_{it}. Indeed, Rosenbaum and Rubin (1983) advocate adding an additional term $U_i(\hat{p}_i - \hat{\mu}_p)$, where $\hat{\mu}_p$ is the sample average of $\hat{p}_i$. Unfortunately, it is not obvious which approach is more appropriate (Woodridge, 2002: 619-620).

22. DD matching and pure regression DD estimates might not differ by much. For the cross-section case, this point can be illustrated in relatively straightforward manner by setting $y_i = y_{0i} + U_i(y_{1i} - y_{0i})$, with $y_{0i} = \mu_0 + v_{0i}$, $y_{1i} = \mu_1 + v_{1i}$, $\mu_0 = E(y_{0i})$, and $\mu_1 = E(y_{1i})$. Further assuming both $E(v_{0i}|X_i)$ = $E(v_{1i}|X_i)$ and the conditional mean independence assumption, we have $E(y_i|U_i, X_i) = \mu_0 + \alpha U_i + g_0(X_i)$, with $\alpha \equiv ATT$ and $g_{0i}(X_i) = E(v_0|X_i)$, the control function. If regression analysis explicitly requires these two assumptions plus additivity, matching has its own associated difficulties. Specifically, since there will be as many treatment effects as $X_i{}'$ s, perfect matches are not in general possible so that an explicit weighting rule is required.

23. We experimented with several establishment size filters, the main issue being whether we should allow an establishment to fall below the minimum number of five employees at any time. After testing alternative rules, we implemented the restriction that establishments have the 5-employee minimum in at least one year in the sample period. (Results based on different filters are available from the authors upon request.)

24. According to Rosenbaum and Rubin (1983), an *mba* of 20 percent is considered large, and one of less than 5 percent very good.

25. Especially for the group of small establishments there will be an overestimation of mass layoffs as the Establishment Panel does not allow us to distinguish between a voluntary quit and an employer-initiated separation. For larger firms, however, this is less of an issue because it is unlikely to be the case that, say, say, a 10 percent reduction in the workforce, is largely the result of voluntary quits.

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Table 1
Real Wage Bill Per Employee (in year 2000 Euros), Unweighted Data

Establishment type	2000	2001	2002	2003	2004	2005	2006	2007	2008
Firm collective bargaining	2,170.9	2,198.6	2,329.3	2,297.9	2,387.1	2,316.1	2,433.0	2,231.9	2,133.0
Sectoral collective bargaining	2,140.9	2,153.5	2,193.9	2,139.0	2,140.5	2,124.3	2,109.6	2,033.1	1,953.6
Any collective bargaining	2,144.6	2,158.9	2,209.4	2,158.3	2,169.4	2,149.4	2,152.9	2,060.3	1,979.2
No collective bargaining /individual bargaining	1,733.2	1,786.5	1,827.0	1,747.2	1,692.2	1,646.0	1,620.3	1,530.5	1,469.9
All	1,959.3	1,984.3	2,023.8	1,950.4	1,923.5	1,883.4	1,858.2	1,753.7	1,675.2
N	7,037	7,752	7,391	7,336	7,320	7,480	7,357	7,855	8,094

Notes: The reported figures are per full-time equivalent employee. The number of full-time equivalent workers is given by the sum of full-time workers plus 0.5 (part-time workers). Real wages, which reference the year 2000, were obtained using the inverse of the consumer price index as a deflator. See the text for full description of the dataset.

Table 2
Real Wage Changes for Collective Bargaining Leavers Versus Always Members and Joiners Versus Never Members, Consecutive Years, Unweighted Data

	2001	2002	2003	2004	2005	2006	2007	2008
(a) Leavers vis-à-vis always members								
2000	1.0342							
2001		1.0506						
2002			0.9428					
2003				0.9811				
2004					1.0300			
2005						1.0912		
2006							1.0144	
2007								0.9570
(b) Joiners vis-à-vis never members								
2000	0.9697							
2001		0.9617						
2002			0.9456					
2003				1.0505				
2004					1.0345			
2005						1.1189		
2006							1.0356	
2007								1.0554

Notes: See note to Table 1. The real wage change for a leaver (always member) j (j') is given by $w_{j,t+1}/w_{j,t}$ $(w_{j',t+1}/w_{j',t})$, where w denotes the real wage bill per full-time equivalent employee. The value in each cell is obtained by dividing the two ratios; and similarly for joiners versus never members. The values reported in panel (a) are based on panels (a) and (c) of Appendix Table 4, while panel (b) is obtained using panels (b) and (d).

Table 3
Difference-in-Differences Estimates of the Effect of Joining/Entering into a Collective Agreement, Separate One-Year Transitions and Pooled Samples

	2000-01	2001-02	2002-03	2003-04	2004-05	2005-06	2006-07	2007-08	POOLED
(a) Regressors in first differences									
$\hat{\delta}_{DD}$	0.008	0.000	-0.044	0.123***	0.047	-0.022	0.041	0.091***	0.035**
t-statistic	0.228	-0.011	-0.656	2.406	0.941	-0.237	0.835	2.980	2.204
N	1,999	2,279	2,123	1,688	2,700	2,720	2,847	3,223	19,579
(b) Regressors in base-year levels									
$\hat{\delta}_{DD}$	-0.,007	0.003	-0.054	0.132***	0.053	-0.045	0.039	0.090***	0.031***
t-statistic	-0.,203	0.059	-0.766	2.431	1.058	-0.462	0.804	2.805	1.869
N	2,004	2,281	2,129	1,693	2,711	2,724	2,857	3,226	19,625

Notes: The model specification is given by equation (6) in the text. In each column, *N* gives the number of establishments observed in the two corresponding (consecutive) years. The set of regressors includes all the variables described in Appendix Table 1, plus year dummies in the pooled case. In panel (a), the time-varying regressors are in first differences.
***, **, and * denote statistical significance at the .01, .05, and .10 levels, respectively.

Table 4
Difference-in-Differences Estimates of the Effect of Leaving/Abandoning a Collective Agreement, Separate One-Year Transitions and Pooled Samples

	2000-01	2001-02	2002-03	2003-04	2004-05	2005-06	2006-07	2007-08	POOLED
(a) Regressors in first differences									
$\hat{\delta}_{DD}$	-0.058*	-0.016	-0.076**	-0.076	0.017	-0.004	-0.036	-0.033	-0.035***
t-statistic	-1.787	-0.448	-2.095	-1.076	0.520	-0.088	-0.653	-1.433	-2.541
N	2,351	2,481	2,253	1,610	2,389	2,233	2,149	2,204	17,670
(b) Regressors in base-year levels									
$\hat{\delta}_{DD}$	-0.062*	-0.020	-0.063	-0.073	0.019	-0.006	-0.044	-0.033	-0.034***
t-statistic	-1.876	-0.559	-1.655*	-1.064	0.585	-0.130	-0.803	-1.374	-2.475
N	2,354	2,484	2,256	1,614	2,396	2,239	2,155	2,212	17,710

Note: See notes to Table 3.
***, **, and * denote statistical significance at the .01, .05, and .10 levels, respectively.

Table 5
Conditional Difference-in-Differences Estimates of the Effect of Joining *and* Leaving a Collective Agreement, with Kernel Matching, Pooled Sample

	(i) Regressors in first differences		(ii) Regressors in base-year levels	
	Coefficient	t-statistic	Coefficient	t-statistic
Collective bargaining joiners (Control group: never members)	0.0345**	2.16	0.0319**	2.05
	N=19,560 *mba*=0.02 *Pseudo* R^2=0.036 *LR*=1.95		*N*=19,606 *mba*=3.4 *Pseudo* R^2=0.02 *LR*=0.07	
Collective bargaining leavers (Control group: always members)	-0,0380**	-2,20	-0,0384***	-2,53
	N=17,650 *mba*=2.10 *Pseudo* R^2=0.04 *LR*=0.0		*N*=17,690 *mba*=2.18 *Pseudo* R^2=0.05 *LR*=0.0	

Notes: See notes to Table 3. *mba* indicates the mean standardized (absolute) bias in percentage, while the *pseudo*-R^2 and *LR* statistics are drawn from the propensity score regression (probit) run after matching the treated and untreated groups. The likelihood ratio (*LR*) tests the joint significance of all included variables in the probit regression.

***, ** denote statistical significance at the .01 and .05 levels, respectively.

Table 6
Conditional Difference-in-Differences Estimates of the Effect of Joining *and* Leaving a Collective Agreement, with Radius Matching (Caliper 0.01), Pooled Sample

	(i) Regressors in first differences		(ii) Regressors in base-year levels	
	Coefficient	t-statistic	Coefficient	t-statistic
Collective bargaining joiners (Control group: never members)	0.036**	2.26	0.033**	2.09
	N=19,560 *mba*=0.79 *Pseudo* R^2=0.06 *LR*=0.0		*N*=19,606 *mba*=0.91 *Pseudo* R^2=0.06 *LR*=0.0	
Collective bargaining leavers (Control group: always members)	-0.038***	-2.68	-0.036***	-2.50
	N=17,650 *mba*=1.07 *Pseudo* R^2=0.10 *LR*=0.0		*N*=17,690 *mba*=1.17 *Pseudo* R^2=0.10 LR=0.0	

Note: See notes to Table 6.

***, ** denote statistical significance at the .01 and .05 levels, respectively.

Table 7
Difference-in-Differences Estimates of the Effect of Joining and Leaving a Collective Agreement, Pooled Samples

	Estimates of the Effect of Joining (1)	Estimates of the Effect of Leaving (2)
A: Sample of mass layoff establishments		
(a) Regressors in first differences		
$\hat{\delta}_{DD}$	-0.016	-0.082*
t-statistic	-0.278	-1.763
N	716	1,665
(b) Regressors in base-year levels		
$\hat{\delta}_{DD}$	-0.023	-0.099**
t-statistic	-0.365	-2.111
N	719	1,670
B: Net sample (i.e. all establishments except mass layoffs)		
(a) Regressors in first differences		
$\hat{\delta}_{DD}$	0.036**	-0.032***
t-statistic	2.180	-2.245
N	18,863	15,998
(b) Regressors in base-year levels		
$\hat{\delta}_{DD}$	0.032**	-0.031**
t-statistic	1.897	-2.149
N	18,906	16,033

Notes: See notes to Table 3.
***, **, and * denote statistical significance at the .01, .05, and .10 levels, respectively.

Appendix Table 1
Variable Description and Summary Statistics

Variable	Mean	Standard deviation
Full time equivalent of employees (time-varying)	163.309	947.102
Share of females (time-varying)	0.373	0.296
Share of skilled workers (time-varying)	0.634	0.289
Share of part-time workers (time-varying)	0.165	0.225
Share of fixed-term workers (time-varying)	0.042	0.111
State of technology in use: good (1=yes, time-invariant)	0.671	0.470
Founded before 1990 (1=yes, time-invariant)	0.539	0.498
Foreign ownership (1=yes, time-invariant)	0.075	0.264
Single establishment firm (1=yes, time-invariant)	0.736	0.441
Individually-owned establishment (1=yes, time-invariant)	0.281	0.450
Works council (1=yes, time-invariant)	0.308	0.462
Export in previous year (1=yes, time-invariant)	0.269	0.443
Expected business volume development in the current year compared to previous year:		
Expected to remain constant	0.482	0.500
Expected to increase	0.272	0.445
Expected to decrease	0.246	0.431
Industry:		
Manufacture of food products	0.038	0.192
Manufacture of textiles and clothing, tanning and dressing of leather	0.012	0.109
Manufacture of paper products, printing, publishing	0.021	0.144
Manufacture of wood products	0.019	0.137
Manufacture of chemicals, coke, refined petroleum products and nuclear fuel	0.024	0.154
Manufacture of rubber and plastic products	0.020	0.139
Manufacture of other non-metallic mineral products	0.020	0.139
Manufacture of basic metals	0.024	0.151
Recycling	0.004	0.059
Manufacture of fabricated metal products and structural metal products	0.042	0.200
Manufacture of machinery and equipment	0.045	0.208
Manufacture of motor vehicles, trailers and semi-trailers	0.018	0.132
Manufacture of other transport equipment	0.008	0.088
Manufacture of electrical equipment, office machinery and computers	0.027	0.162
Manufacture of precision and optical equipment	0.021	0.142
Manufacture of furniture, jewellery, musical instruments, sports goods, games and toys and other products	0.013	0.113
Building of complete constructions or parts	0.049	0.215
Building installation and building completion	0.060	0.238
Sales, maintenance and repair of motor vehicles and motorcycles; retail service of automotive fuel	0.039	0.193
Wholesale and commission trade	0.052	0.222
Retail trade, repair of personal and household goods	0.087	0.282
Transport	0.048	0.213
Communication	0.004	0.060
Central banking	0.007	0.082
Insurance and pension funds	0.010	0.100
Computer and related activities	0.021	0.145

Research and development	0.008	0.090
Legal, accounting, book-keeping and auditing activities, advertising, market research	0.038	0.192
Real estate activities	0.016	0.126
Renting and business activities	0.065	0.246
Hotels and restaurants	0.046	0.208
Education	0.011	0.104
Health, veterinary and social work activities	0.047	0.212
Sewage and refusal disposal, sanitation and similar activities	0.005	0.072
Recreational, cultural, and sporting activities	0.008	0.089
Other services	0.025	0.157
Establishment size:		
1-9 employees	0.313	0.464
10-19 employees	0.145	0.352
20-49 employees	0.182	0.386
50-249 employees	0.229	0.420
250-999 employees	0.100	0.300
1000 or more employees	0.030	0.171
Region:		
Schleswig-Holstein	0.037	0.190
Hamburg	0.029	0.167
Niedersachsen	0.069	0.253
Bremen	0.057	0.231
Nordrhein-Westfalen	0.105	0.306
Hessen	0.058	0.233
Rheinland-Pfalz	0.045	0.208
Baden-Württemberg	0.078	0.269
Bayern	0.073	0.260
Saarland	0.039	0.194
Berlin	0.064	0.244
Brandenburg	0.064	0.244
Mecklenburg-Vorpommern	0.061	0.240
Sachsen	0.082	0.274
Sachsen-Anhalt	0.064	0.245
Thüringen	0.075	0.264
Year:		
2001	0.104	0.305
2002	0.104	0.305
2003	0.109	0.311
2004	0.113	0.317
2005	0.118	0.323
2006	0.121	0.327
2007	0.115	0.319
2008	0.123	0.329

Appendix Table 2
Collective Bargaining Coverage (Unweighted Data)

		2000	2001	2002	2003	2004	2005	2006	2007	2008
Firm-level Agreement	Coverage rate	7.42	6.97	6.75	6.66	6.94	7.54	8.10	7.43	7.85
	No. estabs.	549	565	519	510	522	561	579	539	555
	Average no. employees	318	467	480	436	428	417	470	395	435
Sector-level Agreement	Coverage rate	50.73	48.95	48.44	46.23	46.01	44.92	42.73	41.90	41.05
	No. estabs.	3,754	3,968	3,725	3,539	3,462	3,341	3,055	3,038	2,901
	Average no. employees	314	306	*309*	*282*	336	337	329	314	302
Any Collective Agreement (Firm- and Sector-level Agreements)	Coverage rate	58.15	55.91	55.19	52.89	52.95	52.46	50.83	49.33	48.90
	No. estabs.	4303	4533	4244	4049	3984	3902	3634	3577	3456
	Average no. employees	315	326	330	302	348	348	351	327	324
	N	7,400	8,107	7,690	7,655	7,524	7,438	7,149	7,251	7,067

Appendix Table 3
Transitions into and out of Collective Bargaining, Establishments Observed in Two Consecutive Years, Unweighted Data

	2000->01	2001->02	2002->03	2003->04	2004->05	2005->06	2006->07	2007->08
(a) Sector-level Agreement								
1-->1	2,636	2,704	2,622	2,674	2,638	2,498	2,415	2,382
1-->0	270	227	200	223	241	260	224	303
0-->0	2,878	3,177	3,119	3,684	3,885	4,100	4,364	4,836
0-->1	232	155	158	213	150	138	188	273
(b) Firm-level Agreement								
1-->1	252	279	256	262	292	300	294	306
1-->0	168	110	115	121	115	109	152	133
0-->0	5,481	5,748	5,617	6,291	6,363	6,444	6,629	7,200
0-->1	115	126	111	120	144	143	116	155
(c) Any Collective Agreement								
1-->1	3,063	3,165	3,072	3,132	3,148	3,008	2,936	2,870
1-->0	263	155	121	148	138	159	149	254
0-->0	2,518	2,844	2,831	3,377	3,552	3,758	4,029	4,424
0-->1	172	99	75	137	76	71	77	246

Appendix Table 4
Average Real Wages before and after Collective Bargaining Transition, Unweighted Data (Any Type of Collective Agreement)

	2001	2002	2003	2004	2005	2006	2007	2008
(a) 1-->0 (leavers)								
2000	1.089529							
2001		1.126072						
2002			0.9892491					
2003				1.021349				
2004					1.058279			
2005						1.149984		
2006							1.04605	
2007								0.9948533
(b) 0-->1 (joiners)								
2000	1.067516							
2001		1.067734						
2002			1.010619					
2003				1.115022				
2004					1.093552			
2005						1.212154		
2006							1.076664	
2007								1.149686
(c) 1-->1 (always members)								
2000	1.053465							
2001		1.07184						
2002			1.049254					
2003				1.041052				
2004					1.027456			
2005						1.053826		
2006							1.031231	
2007								1.03952
(d) 0-->0 (never members)								
2000	1.100853							
2001		1.110248						
2002			1.068743					
2003				1.06137				
2004					1.057104			
2005						1.083362		
2006							1.039649	
2007								1.089377

Note: The reported values are given by $w_{j,t+1}/w_{j,t}$, where $w_{j,t}$ denotes the real wage bill per full-time employee in establishment *j* in year *t*.