Union Decline and the Coverage Wage Gap in Germany†

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Abstract
Using linked employer-employee data, this paper estimates the effect of collective bargaining coverage on wages over an interval of continuing decline in unionism. Unobserved firm and worker heterogeneity is dealt with using two establishment sub-samples, comprising collective bargaining joiners and never members on the one hand and collective bargaining leavers and always members on the other, each in combination with subsets of worker job stayers. The counterfactuals are then reversed for robustness checks. Joining a sectoral agreement is found always to produce higher wages, while leaving one no longer produces wage losses if the transition is to a firm agreement. Leaving a firm agreement to non-coverage also leads to wage reductions, while joining one from non-coverage seems decreasingly favourable. The reverse counterfactuals yield correspondingly smaller estimates (in absolute value) of wage development than reported for the initial counterfactuals. Finally, although small, the union wage gap persists.
[147 words]

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1. Introduction

Most information on the union-nonunion differential in Germany – strictly, the wage gap between covered and uncovered workers – pertains to the 1990s or early 2000s. Yet, as is widely known, German unions have been in retreat both during and subsequent to these intervals. It is the very breadth of decline that makes investigation of the more recent interval more compelling. First, union density has fallen sharply. The decline can be dated from the mid-1980s, the sudden rise in membership after 1989 proving to be little more than a diversion. Accordingly, union density declined from 36 per cent in 1991 in the aftermath of unification to 19.3 per cent in 2009 (see Fitzenberger, Kohn, and Wang, 2006; Addison, Schnabel, and Wagner, 2007; Binspinck, Dribbusch, and Schulten, 2010). By way of qualification, Hirsch and Schnabel's (2011) measure of union bargaining power, based on a right-to-manage model of collective bargaining, suggests that the fall off in union strength occurred mostly after 1999, remaining fairly stable in the 1990s despite a fairly uniform drop in density over the entire period.

Second, overall collective bargaining coverage as a share of employment fell in western Germany (eastern Germany) from 76 (63) per cent in 1998 to 65 (51) per cent in 2009 (see Binspinck, Dribbusch, and Schulten, 2010).[1] The corollary was a continuing rise in the bargaining free sector.

Third, the decline in collective bargaining coverage was not deflected by a growth in "orientation" in the uncovered sector, nor was it to receive any support from the "extension principle." Orientation refers to a process whereby uncovered firms claim to shadow the terms of sectoral agreements. The coverage of orienting firms in the private sector rose from 17.9 per cent of employment in 2000 to 22.4 per cent in 2010. This rising trend only partially compensated for the decline in *sectoral* bargaining: the share of employees covered by sectoral agreements fell from 59.9 to 49.3 per cent over the same interval. Moreover, these are simple frequencies. The wages paid by orienting firms have been shown to lie well below those set under collective bargaining (see Addison et al., 2012). As far as the extension of collective agreements to employees and employers not bound by the relevant sectoral agreement is concerned this, too, has evinced pronounced decline. For example, considering just the extension of primary collective agreements under the 1949 Collective Agreement Act, their number fell from 408 (or 5.4 per cent of all such agreements) to just 245 (1.5 per cent) in 2009 (Bispinck, Dribbusch, and Schulten, 2010).

Finally, German sectoral collective bargaining has been buffeted by decentralization in the form of opening clauses and pacts for employment and competitiveness (see, respectively, Bispinck, 2004; Seifert and Massa-Wirth, 2005). Abstracting from the issue of whether the process has been destabilizing or fragmenting – so-called "internal erosion" (on which, see Hassel, 1999, inter al.) – the very extensive contractual innovations in question involved such elements as hardship clauses allowing firms in economic difficulties to deviate from sectorally-agreed provisions, temporary cuts in pay to safeguard jobs, the introduction of profit-related pay substituting for previously guaranteed elements of the remuneration package, and concession bargaining more generally (see, for example, Haipeter and Lehndorff, 2009).

The very scale of these developments in Europe's largest economy makes Germany an interesting case for consideration. One primary interest is of course macroeconomic in scope. Specifically, has increasingly decentralized bargaining produced greater flexibility (i.e. improved) responsiveness of wages to their underlying determinants thereby benefiting employment, or, by analogy with concerns expressed after the collapse of the Swedish model, have the advantages associated with a coordinated system of bargaining been so compromised as to lead to the opposite outcome?[2] There is also a European-wide dimension to these developments, given the role of Germany in shaping EU social policy and institutions.

Our own concerns are less far ranging given the unsettled nature of research into the union premium, let alone its course through time. The focus of the present treatment is, then, to discover what has been happening to the union wage gap in the first decade of the 2000s. It offers a critical albeit partial first step in the analysis of the consequences of the decline in unionism. It is partial because an estimate of average wage differences does not inform us about the distribution of wages. A small premium may be consistent with a large effect in the lower reaches of the distribution. Even if declines in the wage gap are unlikely to be undone by distributional effects, the latter require explicit consideration in future research. But, to repeat, basic knowledge of the effect of unions on average wages is itself underdeveloped and remains the basis of the present inquiry.

The paper proceeds as follows. We first review the state of play on the union wage gap, drawing upon the German (and, briefly, the U.S.) literature. We then outline our unique dataset, namely the cross sectional version of the linked employer-employee data supported by the German Institute for Employment Research. Our analysis is based on two (three-year) clouds of data at each end of the sample period 2000-2010, exploiting changes in establishment collective bargaining status over time. Our estimates of union wage effects are obtained by comparing the wage growth of workers employed by plants joining or leaving collective bargaining with that of workers employed by establishments that did not change their collective bargaining status – never members and always members, respectively. (These counterfactuals are then reversed for robustness.) Our main finding is that joining a sectoral agreement is found always to produce higher wages, while leaving one no longer produces wage losses if the transition is to a firm agreement. Leaving a firm agreement to non-coverage also leads to wage reductions, while joining one from non-coverage seems decreasingly favourable. The reverse counterfactuals in turn yield correspondingly smaller estimates (in absolute value) of wage development than reported for the initial counterfactuals.

2. A Review of the Literature on the Collective Bargaining Premium

Recent studies of the magnitude of the collective bargaining premium in Germany use either the German Structure of Earnings Survey (GSES) or the Linked Employer-Employee Dataset of the Institute for Employment Research (LIAB). The thrust of studies using the GSES has been upon distributional issues, while those based on the LIAB have more often (though not exclusively) considered the effects of collective bargaining on wages and rent sharing.[3] Furthermore, the GSES studies lack a longitudinal capacity, limiting inferences that can be made about causality. Note, finally, that the wage gap in Germany refers to coverage rather than membership since German constitutional law – specifically Article 9 of the Basic Law (*Grundgesetz*) – does not allow collective agreements to discriminate against non-union members.

The main GSES studies that also examine the wage gap in addition to distributional issues are: (a) Stephan and Gerlach (2005) who use a regional manufacturing subsample from Niedersachsen (Lower Saxony) of the GSES for the 1990, 1995, and 2001 waves; (b) Fitzenberger, Kohn, and Lembcke (2013) who use the full survey for 2001 but focus on prime-age male employees in West Germany; and (c) Antoncyzk (2010) who deploys the same GSES sample for 2001 but who seeks to account for the endogeneity of collective bargaining.[4] Stephan and Gerlach's multi-level model provides estimates of the wage of an average worker in an average firm applying individual contracts (i.e. a no collective bargaining regime), and of the differentials that would apply had that worker been employed in an otherwise average firm with either an industry collective agreement or a firm-level agreement. In 1990 the estimated wage premium was 4 per cent in the case of sectoral contracts and 3 per cent for firm-level contracts. Higher coverage premia are reported for the later sample years: respectively 9 and 12 per cent in 1995, and 7 and 11 per cent in 2001.[5, 6]

The study by Fitzenberger, Kohn, and Lembcke's (2013) is notable for its consideration of the wages of uncovered workers in covered firms and the role of (exogenous) union density in the relevant labour market segment in addition to firm coverage effects. The authors' OLS results indicate that firms applying 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 – and somewhat more so in the case of firm-level than sectoral agreements. For its part, an increase in union density reinforces the positive effects of higher coverage at firm level (while lowering wages in the uncovered sector). That said, individual coverage by a collective agreement in a covered firm shows a negative impact on the wage level, and the authors' separate quantile regression analysis shows that this effect is elevated at higher quantiles of the conditional wage distribution. In other words, collective bargaining coverage serves to reduce wage inequality – reminding us that the main thrust of analyses based on the GSES is upon distributional issues.[7]

Antonczyk (2010) seeks to measure the causal effect of collective bargaining in 2001 – actually, sectoral bargaining alone – on wages, using two instrumental variables measured at district level; specifically, the share of Protestants and Catholics (the expectation being that

Catholics are more likely to favour unionism), and historic gross union density (i.e. employed *and* unemployed union members as a share of employees). These variables are meant to provide exogenous variation in the treatment variable. Antonczyk reports the average treatment effect on wages or union wage effect, namely the expected gain from coverage of a randomly assigned individual with a given set of observable characteristics.[8] The upshot of this procedure is that the simple cet. par. coverage premium shrinks from 3.6 log points to just 0.8 log points, the bulk of the unadjusted differential seemingly reflecting the fact that individuals undergoing treatment have higher unobserved productivity than those undergoing the treatment.

Studies using the LIAB are sparse and, as noted earlier, are less concerned with distributional issues than with the effects of coverage on levels of wages and rents. The principal study is by Gürtzgen (2012), using data from the 1995-2008 LIAB for western German mining and manufacturing establishments. Gürtzgen presents OLS, establishment fixed effects, and spell fixed effects estimates of a wage equation where the dependent variable is the log daily wage. In the establishment fixed effects specification, the collective bargaining coverage premium is identified from establishments that change their contract status. The spell fixed effects specification first differences log wages within each individual-establishment cell, removing unobserved firm *and* worker heterogeneity. Raw coverage differentials of 20 log points for sectoral bargains and 29 log points for firm bargaining are reduced by 70 and 80 per cent, respectively, in the OLS specification with the full set of controls. More importantly, these much reduced coverage premia vanish once the non-random selection of firms into bargaining regime is controlled for and also under spell differencing designed to control for the possibility that plants changing their bargaining status might at the same time also experience a change in unobserved worker skills.

Finally, in the most contentious part of her analysis, Gürtzgen turns to the role of time-specific unobservables, the concern being that establishments that change their coverage status might be subject to different time-specific unobservables than are those maintaining the original contract status. To investigate this endogeneity issue, she adopts a trend-adjusted difference-in-difference estimator, analysing separate transitions from one bargaining regime to another (six cases in all) and allowing for differences in changes in time-specific shocks by subtracting the differential in wage growth in pre-transition intervals. Gürtzgen concludes from these comparisons of the wage growth of individuals experiencing a change in contract status with the wage growth of individuals employed by stable plants that there is no "true" wage effect of exiting sectoral bargaining to non-coverage. Although such individuals may experience wage losses, this outcome is indicative of the correlation of the transition with (more) negative demand shocks. The interpretation of firm collective bargaining transitions is altogether more nuanced, however, with some transitions leading to positive differentials and others giving rise to negative differentials, so that the small insignificant premium for firm-level collective bargaining reported in the main estimation exercise represents the net effect of these different influences. This latter part of Gürtzgen's analysis is rather speculative. The problem resides of course in implementing the adjusted difference-in-difference estimator (on which more below).

We conclude this section with some contextual remarks on the evolving U.S. literature. While the preponderance of American research has focused on the union membership premium – and also unlike the research reviewed here has continued to obtain some very large estimates of that particular premium using *both* cross sectional and longitudinal analysis of individual wage data[9]– some more recent studies offer a closer match with the German literature in focusing on the wage effects of establishments becoming unionized. We refer to studies of union representation elections comparing establishments in which unions became recognized by a close margin of the vote with those in which they barely lost, and where evidence of a discontinuous relation between the vote share and wages is deemed to be the causal impact of unionization. Such regression discontinuity studies by DiNardo and Lee (2004) and Frandsen (2012) produce German-type estimates of the wage gap, and attribute the results of past U.S. research using individual data to the contribution of unobserved firm heterogeneity.

Yet lest we draw premature conclusions about likely convergence in the two literatures, it should also be noted that recent U.S. studies in the more conventional event-study tradition still find large effects of new unionization on publicly-traded firms' equity values. Lee and Mas (2012), in particular, obtain estimates of lost market value attendant upon unionization that translate into a union coverage premium of 10 per cent. Importantly, these authors further argue that such equity losses are increasing in the union vote share in representation elections.

Against this backdrop, the present exercise which seeks to obtain estimates of the course of the union wage gap at a time of unambiguously declining union authority and controlling for unobserved firm and worker heterogeneity gains additional purchase.

3. The Dataset

The present study uses the LIAB Cross-Sectional Model Version 2 1993-2010 (LIAB QM2 9310) of the linked employer-employee data supported by the Institute for Employment Research/*Institut für Arbeitsmarkt- und Berufsforschung* (IAB). The LIAB data are created at the Research Data Center (*Forschungsdatenzentrum*, or FDZ) of the Federal Employment Agency/*Bundesagentur für Arbeit* by linking the establishment data from the annual waves of the IAB Establishment Panel (*Betriebspanel*) with information on individuals from the social security records of the German Federal Employment Agency.

The IAB Establishment Panel is a large-scale annual establishment survey that covers up to 16,000 establishments every year, beginning in 1993 in West Germany and extended in 1996 to the former East Germany. The participating establishments are surveyed on a large number of employment policy-related subjects. These include employment development, business policy and business development, collective bargaining, personnel structure and recruitment, remuneration, and working time. The survey is unique in Germany, since it is representative for all industries and establishment sizes nationwide and is conceived as a longitudinal survey. Therefore, it enables researchers to analyse developments over time and to conduct longitudinal studies of individual establishments as well. (For further information on the IAB Establishment Survey, see Fischer et al., 2009).

The information on individual workers in the LIAB dataset comes from the social security records of the German Federal Employment Agency and covers all employees of the establishments surveyed in the IAB Establishment Panel. Specifically, it includes both employees who are liable to social security and also employees who are marginally part-time employed (*geringfügig beschäftigt*). For these employees, several demographic characteristics such as gender, age, nationality, level of education, occupational group, employment status and place of residence are provided. Furthermore, the data contain the individual daily wage of an employee. The latter is measured with high accuracy by the authorities since this wage information is decisive in calculating an individual's social security payments.

In sum, the LIAB dataset is a unique data source for analysing both the supply side and the demand side the German labour market along various dimensions. Due to its coverage, it is one of the best-suited datasets for investigating the effects of collective bargaining coverage on the wages of individual workers in Germany. Several versions of the LIAB data including cross-sectional and longitudinal subsamples can be accessed for scientific purposes at the FDZ in Nuremberg.

We have undertaken several modifications to adapt the data to fit our research purposes. In the first place, in order to improve the quality of the linkage between the survey data and the administrative data, we adopted the procedure that is followed by the FDZ for some of the longitudinal versions of the LIAB, erasing observations that exhibit a bad linkage quality. In the FDZ procedure, a link is defined as having a bad quality if the number of employees and apprentices that an establishment has reported in the IAB Establishment Panel deviates significantly from the number of employees and apprentices that is calculated from the administrative data. (For information on this procedure, see Jacobebbinghaus, 2008: 53f.)

Second, other modifications concern the key wage variable that is central to our analysis. In the LIAB data, the reported individual wage of a worker is the gross daily wage. Fringe benefits are included only if they are subject to social security. Since there exists an upper contribution limit in the German social security system – set annually for western and eastern Germany by the German government – the gross daily wages in these data are top-coded; in our dataset, this affects about 15 (10) per cent of the observations for western (eastern) Germany. We therefore imputed the wages above the contribution limit, using the procedure suggested by Gartner (2005). First, we estimated a Tobit regression of log daily wages on individual and establishment characteristics separately for both parts of the country and for each single year. Following Gartner (2005), we then constructed a truncated normal distribution by using the predicted values from the Tobit estimation as moments and by setting the lower truncation point equal to the contribution limit. Finally, we replaced censored wage observations by values randomly drawn from this truncated normal distribution. Furthermore, we deflated wages using the Consumer Price Index published by the German Federal Statistical Office; specifically, all wages are expressed in year 2000 values.

Third, because only a very broad measure of individual working hours is contained in the dataset – in particular, for part-time workers, whether working hours are less or greater than 18 per week – we restricted our analysis to full-time employed workers who are subject to social security. We further excluded those full-time workers who were recorded as receiving an implausibly low daily wage (of less than €16). In addition, we excluded observations from the following sectors/enterprises: agriculture, hunting, fishing and forestry, public administration, and not-for-profit entities.

Note that since we are using the cross-sectional version of the LIAB, we are unable to track a worker if he/she leaves one establishment for another that is not covered by the IAB Establishment Survey, or if he/she exits to non-employment. The same problem arises if a worker remains in an establishment that subsequently, and for whatever reason, no longer participates in the IAB Establishment Survey. Unfortunately, there is no way of circumventing this limitation of the data. Making use of one of the available longitudinal versions of the LIAB, or constructing our own LIAB-variant, would not suffice as the key information on the collective bargaining status of an establishment in a given year will not be available if the establishment fails to take part in the IAB Establishment Survey in that year. (The reader is referred to Heining, Scholz, and Seth, 2013, for more information on the LIAB dataset.)

4. Preliminary Data Analysis

Our analysis is based on two (three-year) clouds of data – annual observations for 2000-2002 and 2008-2010 – rather than the full 2000-2010 panel. Given the establishment rotation in the IAB survey, that disqualifies us in practice from following other than workers in permanent panel establishments over a relatively long period, little is lost from our more selective approach. Further, it has the advantage of contrasting in a possibly more direct way two presumably distinct periods, occurring at the beginning and at the end of the first decade of the present century, marked by a material decline in collective bargaining coverage and in union density (see Addison et al., 2011).

Table 1 presents the main longitudinal features of each subset of observations. Three main observations are in order. First, we observe approximately the same number of workers, in each sub-period: 2.4 million in 2000-2002 and 1.9 million in 2008-2010. Second, the share of individuals appearing in each and every year of each cloud is more or less the same: 29.9 per cent in the first interval, 32.9 per cent in the second. Finally, the percentage of individuals who are observed at least twice over the two intervals is also approximately constant: 50.9 and 55.8 per cent of the total, respectively. In sum, the two clouds of data are highly comparable both in terms of the number of workers being observed and in their longitudinal profile.

(Tables 1 and 2 near here)

In Table 2 we look at the longitudinal pattern of individuals observed at least twice over the two observation windows. Here we want to know whether workers stay in the same establishment or switch employers. Clearly, either in the case of workers who are observed in three consecutive years or for those who are observed in just two years (consecutively or otherwise), job stayers massively dominate in each sub-period: on average, only 1 in 100 workers observed at least twice are job movers. It is this very nature of the LIAB data that forces us to identify the union wage effect based on the wage development of job stayers, in conjunction with observed changes in establishment collective bargaining status, as further elaborated upon in section 5 below.

Table 3 indicates exactly how often establishments switch their collective agreement status, reporting both one-year and two-year transitions. In the interests of economy, we aggregate firm and sectoral agreements into a single category. We thus identify 'any type of collective agreement' here, be it firm-level or industry-wide in scope. (Examination of the two separate collective bargaining arrangements is remitted to Appendix Table 1.)

(Table 3 near here)

As shown in the table, between 2000 and 2001, for example, 424 out of 3,639 establishments (or 11.7 per cent) abandoned collective bargaining of either type while 441 out of 2,792 (15.8 per cent) joined a collective agreement from an initial state of no coverage. This means that the percentage of collective bargaining switchers in the total number of possible cases is 13.5 per cent (=[(424+441)/6,431]*100). There is therefore a considerable share of establishments whose collective bargaining status changes over time, comprising roughly equal numbers of joiners and leavers. Note further that approximately the same percentage of collective bargaining switchers is observed in 2001-2002, at 12.8 per cent. Similarly, for the years 2008-2009 and 2009-2010, the percentage of switchers is 11.5 and 6.8 per cent, respectively. Two-year transitions are slightly higher than the one-year transitions, at 14.6 and 10.6 per cent in the first and second sub-periods, respectively. Note that the two-year transition rates would be substantially higher than their one-year counterparts had the two samples been the same. They are not precisely because two-year transitions in practice require establishments to be in the sample for three consecutive years, with the implication that the corresponding sample tends to be populated by a substantially higher proportion of permanent panel stayers.

Our estimates of the collective bargaining wage gap were obtained by fitting an augmented Mincerian earnings function to the separate cross-sections of data. Specifically, we conditioned the wage gap on 24 (67) worker (establishment-) level covariates. The former included gender, age (and its square), years of service (and its square), citizenship status, education (6 levels), and occupation (12 levels). The latter comprised dummies for location, establishment age, the profit situation, the state of technology, works council status, firm size (and its square), and industry (40 2-digit), together with the share of female, fixed-term, foreign and skilled workers, and employee median age (see Appendix Table 1).

(Table 4 near here)

The results, shown in the first column of Table 4, indicate a positive wage gap of 7 to 14 per cent in favour of workers covered by sectoral agreements, relative to the comparison group of workers in non-covered establishments. This is a sizeable wage gap, consistent with some earlier OLS studies. Of some interest here is upward trend of this wage gap. Next, the evidence on sectoral versus firm-level agreements points to a wage gap favourable to the former: at 1.8 to 4.0 per cent. Finally, the third column of the table points unequivocally to higher earnings under any form of collective bargaining than under individual bargaining, the margin amounting to some 6.3 to 12.5 per cent.

We next place our discussion of wage formation in a longitudinal context, allowing for unobserved establishment and worker heterogeneity.

5. Estimation strategy

Let us assume that the (log) gross daily wage for individual *i* in period t, y_{it}, is given by:

$$y_{it} = Z_{it}\beta + \delta U_{jt} + \lambda_t + \theta_i + \psi_j + \varepsilon_{it} \text{ ,} \tag{1}$$

where θ_i and ψ_j denote worker- and firm-specific time-invariant effects, respectively; Z_{it} is a vector of observed time-varying and time-invariant worker- and firm-level characteristics, as noted in section 3; λ_t is a time dummy; U_{jt} is a dichotomous variable indicating the collective agreement status of firm *j* (so that δ denotes the collective bargaining wage premium); and ε_{it} is the error term of the model. As is conventional, we assume $E(\varepsilon_{it}|Z_{it}, U_{jt}, \theta_i, \psi_j) = 0$.[10]

Given that each observation window (i.e. 2000-2002 and 2008-2010) comprises a 2-year interval, one possible route controlling for worker and firm heterogeneity is to take a 2-year difference from model (1), to obtain:

$$y_{it} - y_{it-2} = (Z_{it} - Z_{it-2})\beta + \delta(U_{jt} - U_{jt-2}) + (\lambda_t - \lambda_{it-2}) + (\psi_{jt} - \psi_{jt-2}) + (\varepsilon_{it} - \varepsilon_{it-2}). \tag{2}$$

Clearly, in following this approach we are aiming to capture some medium-term effect of collective bargaining coverage. Thus, using the sample of job stayers, for whom by construction we have $(\psi_{jt} - \psi_{jt-2}) = 0$, model (2) yields:

$$y_{it} - y_{it-2} = (Z_{it} - Z_{it-2})\beta + \delta(U_{jt} - U_{jt-2}) + (\lambda_t - \lambda_{it-2}) + (\varepsilon_{it} - \varepsilon_{it-2}). \tag{3}$$

In other words, given that individuals stay in the same firm, identification of δ is achieved via workers whose establishments have changed their status from *t-2* to *t*.

For movers, on the other hand, in general $(\psi_{jt} - \psi_{jt-2}) \neq 0$. This means that under the assumption that $(\psi_{jt} - \psi_{jt-2}) + (\varepsilon_{it} - \varepsilon_{it-2})$ is uncorrelated with $(U_{jt} - U_{jt-2})$, an OLS regression of model (2) will give an alternative estimate of the effect of collective bargaining coverage. Identification of δ in this case is via job movers whose establishments in *t-2* and *t* have the same coverage status vis-à-vis job movers whose establishments have changed their status. Unfortunately, as was described in section 2, the number of job movers is too small to allow us to pursue this approach. Our empirical strategy will therefore perforce rely solely on job stayers.[11]

Since we do not want to impose symmetry on the effects of an establishment leaving/joining a collective agreement, implementation of our difference-in-differences approach is carried out by running the selected models across separate subsamples of establishments. These comprise collective bargaining leavers and always members on the one hand, and collective bargaining joiners and never members on the other.

Our 2-year difference strategy of using the two groups of leavers and always members, on the one hand, and joiners and never members, on the other, and then regressing the changes in the wage outcome indicator on the corresponding change in collective bargaining status implicitly either assumes that any macro shock, proxied by a time dummy, has a similar impact on both treated and control groups (e.g. leavers and always members, respectively) or that the macro shock does not have any differentiated impact on the decision to leave/stay covered by a collective agreement in this particular case.

But one can obviously presume otherwise. Again taking the case of leavers versus always members, the beginning period characteristics may be such that, even after conditioning on the set of observables Z, selection into the treatment is not exogenous. In this case, δ in model (3) will tend to overestimate the causal effect of collective bargaining on worker earnings if, say, an adverse shock pressures covered establishments to leave an agreement rather than stay covered and where at the same time this shock has a negative impact on wages.

Had we observed, somewhere in the past, a group of establishments entirely similar to the group of leavers, and a group of establishments similar to the group of collective bargaining stayers, both confronting a similar macro shock, and where neither group had any possibility of changing its collective bargaining status in the pre-treatment interval, we would have been in a position to obtain a differential adjusted estimate of the causal effect of leaving collective bargaining. Formally, this modelling would necessarily entail the possibility that the macro effect is different across the treated (*T*) and control (*C*) groups. This would require λ_t in model (1) to be replaced by $k_i\lambda_t$, where $i \in g$ indicates that an individual belongs to group *C* or *T*.[12] In this case, the corresponding trend adjusted difference-in-differences estimator (*DADD*) after Bell, Blundell, and van Reenen (1999) is given by:

$$\hat{\delta}_{DADD} = \left\{(\bar{y}_{t_2}^{T} - \bar{y}_{t_1}^{T}) - (\bar{y}_{t_2}^{C} - \bar{y}_{t_1}^{C})\right\} - \left\{(\bar{y}_{t_*}^{T} - \bar{y}_{t_0}^{T}) - (\bar{y}_{t_*}^{C} - \bar{y}_{t_0}^{C})\right\}, \qquad (4)$$

where the second term in brackets is the difference-in-differences estimator obtained using the earlier interval (t_0, t_*); (t_1, t_2) is the selected treatment interval; and $\bar{y}_{t_j}^{T} = E[y_{it} - Z_{it}\beta | t = t_j, g = T]$ is the regression-adjusted mean outcome for the treated group. (And similarly for the control group *C*.) Note also that the 'unadjusted' difference-in-differences estimator, $\hat{\delta}_{DD}$, is given solely by the first term in brackets, which can in turn be obtained by using the regression model (3) for the sample of job stayers.

Unfortunately, the trend adjusted estimator in (4) is difficult to implement. In the first place, it is very difficult to find a similar business cycle somewhere in past. Here, it is not simply a matter of searching for some pre-treatment period with, say, a similar GDP growth rate; rather, it is necessary to find a similar interval in which a comparable set of treated and untreated establishments are subject to a similar shock. In this context, the selection of a (preceding) 2-year interval, for example, is insufficient. And in going further back in time, the likelihood of finding similar groups decreases as establishments will change their workforce structures and technologies over time. Accordingly, more lies behind any observed differential change in wages than just a differential macro shock. More fundamentally, computation of the 'trend' will require, in the (t_0, t_*) interval, the selection of two sets of firms neither of which is exposed to the treatment, that is, with no real chance of changing their collective bargaining status. This requirement is highly unlikely to be met in practice in our sample.

In short, although one might believe that the change in collective agreement status is not fully exogenous, the facts of the matter are that going beyond the 'unadjusted' difference-in-differences estimator is likely to rely upon even stronger assumptions. In our implementation, therefore, we do not attempt to correct for the possibility that the macro effect may be distinct over the treated and control groups. That said, we will seek to check the robustness of our results using alternative control groups, noting that even though treated and untreated groups may seem to have distinct observable characteristics, it does not necessarily follow that the two groups will respond differently to a given shock or, conversely, that a common set of characteristics will generate an identical propensity to change collective bargaining status.

6. Two-Year Differences

Table 5 presents estimates of the collective bargaining premium in the two-year difference formulation, using the subset of job stayers. Establishments are grouped into separate samples of sectoral and firm-level agreement leavers and joiners *and* their corresponding comparison groups of sectoral and firm agreement stayers (i.e. always members and never members), as indicated in the first four columns of the table. The table thus gives the 2-year effect of collective bargaining – either sectoral or firm-level agreements – on those individuals who do not switch jobs between *t-2* and *t* but who happen to be in establishments whose status has changed versus individuals whose establishments do not switch status. To repeat, identification of the union/collective bargaining effect is obtained via changes in an establishment's collective agreement status, given that workers stay in the same establishment over the selected interval.

(Table 5 near here)

As can be seen from the table, workers whose establishments leave a sectoral agreement for no coverage have their wages reduced by 0.7 per cent over the 2000-2002 interval, compared with those whose establishments remain covered by a sectoral agreement. The corresponding effect for the 2008-2010 period is -0.4 per cent. If, in turn, a firm *leaves* a sectoral agreement and becomes covered by a firm agreement, the effect is less pronounced in the first sub-period, at -0.4 per cent, and eventually reversed in the second sub-period, at +0.8 per cent.

The evidence from workers whose establishments have joined sectoral agreements is stronger than that found for sectoral agreement leavers, at +0.7 and +1.1 per cent in 2000-2002 and 2008-2010, respectively, in the scenario where the initial state is of no coverage by any type of collective agreement. These gains are even larger if the transition is from firm-agreement coverage, at +1.0 and +2.3 per cent, respectively. Note that the latter result seems to contradict the evidence found for sectoral agreement leavers in the second row, last column, in the sense that from the perspective of worker wages it looks equally possible to have higher wages either from switching from sectoral to firm agreements or the other way around. Our preferred explanation for this apparent contradiction is that two sets of estimates might not be extracted from strictly comparable samples (on which more below).

The remaining four rows of the table examine the transitions between any type of coverage and firm agreements, on the one hand, and between any type of collective bargaining coverage and no coverage at all, on the other. Thus, in the fifth row, there is a reduction in wages for those workers whose establishments left a firm agreement to become 'uncovered', at -0.9 and -0.7 per cent in the two selected intervals, respectively; while joining a firm agreement from no coverage, in the sixth row, is increasingly less favourable to worker wages, at 2.4 per cent in 2000-2002, and 0.2 per cent (and statistically insignificant) in 2008-2010. Again, there seems to be no evidence of any close symmetry between leaving and joining firm agreements, which reiterates the possibility that the corresponding samples may not be strictly comparable. Alternatively put, although our results suggest that establishments under firm-level agreements are expected to generate lower worker wages if they switch to no coverage, and to generate higher wages if they switch from no coverage to firm agreements, the lack of symmetry in these estimated effects suggest again that, for example, the wages of workers in non-covered establishments will not be necessarily similar – all else constant – to the wage of a worker in an establishment with a firm agreement had the establishment been covered by this type of agreement. This is of course an important caveat that reminds us that our approach is quasi-experimental in nature, not a truly experimental exercise.

The last two rows offer perhaps more clear-cut results. Here we compare the situation of no coverage with any type of collective bargaining coverage and the evidence strongly points to a negative effect on wages after leaving a sectoral or firm agreement and a positive effect of joining any type of collective agreement. The respective losses and gains average -0.6 and +1.1 per cent, and with a clear decreasing tendency in both transitions. Again, there is no sign of a close symmetry in the effects of leaving and joining, but there is nevertheless a strong indication that it is better for workers to be associated with covered than non-covered establishments.

[Table 6 near here]

Table 6 further exploits the possibility raised in Table 5 that we are not fully controlling for unobserved establishment heterogeneity. The presumption here is that a sectoral agreement joiner, say, may more closely resemble a sectoral agreement stayer than a sectoral agreement 'never member.' Table 6 thus compares, for the same sample, sectoral agreement joiners with sectoral agreement stayers. Although one may question this new approach – since it seems eminently reasonable to suppose that a sectoral agreement joiner and a sectoral agreement never member share the same beginning period collective agreement status for non-arbitrary reasons – the strategy is worthwhile pursuing as a form of robustness check on our findings. Table 6 thus changes the counterfactuals not only for sectoral agreement changers but for all other coverage transitions as well.

And indeed the results are quite striking. Thus, even if one admits**,** on the evidence provided in Table 5, that leaving a sectoral agreement has a negative impact on worker wages (taking therefore as a comparison group the subset of always covered), it can be seen from the first row of Table 6 that the wage development for workers whose establishments left a sectoral agreement is nevertheless comparatively more favourable than is the case where workers are in an establishment never covered by any type of agreement. Relative to the latter group, there is indeed an *average* gain of 1.0 per cent in favour of the former.

The results in the second row of the table are more mixed, with the estimated effect for 2008-2010 indicating that whenever an establishment switches from a sectoral agreement to a firm-level agreement, worker wages go up at higher rate than the wages of those workers in establishments always covered by a firm agreement. This seems consistent with the evidence in Table 4 of a positive gap favourable to sectoral agreements relative to firm agreements. The condition for this interpretation is the assumption that sectoral agreements have a long-lasting effect on wage developments, one that cannot be totally offset even after two years.

The results for sectoral agreement *joiners* in the third row follow the same script: whenever an establishment joins a sectoral agreement, wages presumably go up (based again on the evidence provided by Table 5) but by less than would have occurred had the workers been in an establishment that remained consistently covered by a sectoral agreement over the corresponding sample period. Indeed, the loss amounts to 1.1 per cent, on average.[13] In turn, the results in the fifth row suggest that firm agreements have some long-lasting effects, too, as workers in establishments leaving firm agreements continue to receive higher wage increases than their counterparts in never covered plants. For its part, the evidence from the sixth row is more mixed, with a positive effect in the first sub-period and a negative effect in the second.

Next, turning to the aggregate category (i.e. coverage by any type of collective agreement), the seventh row shows that although the evidence from Table 5 would lead us to expect a fall in wages after an establishment leaves a collective agreement of either type, it remains the case that the wage change will still be comparatively more favourable than that obtained by workers in establishments never covered by a collective agreement. Indeed, an average wage gain of 0.9 per cent is anticipated as compared with the negative average value of -0.6 per cent recorded in Table 5.

Conversely, while workers in an establishment joining a collective agreement are expected to have, say, a 1 per cent increase in their wage over a period of two years relatively to those in an establishment never covered by any form of collective agreement (see the last row of Table 5), the corresponding results with the different counterfactual in the last row of the Table 6 offer a more qualified story. They show that the wage increase for joining plants **is** comparatively smaller than the wage increases received by workers in those always covered establishments. And if anything there is an increasing gap in this regard, amounting to some -1.3 per cent by the end of our sample period in 2008-2010.

Finally, in the interests of completeness, we present in Appendix Table 3 the results from implementing the spell fixed-effects case (see footnote 12 above). The table provides detailed results for the same counterfactuals as in Table 5; that is, it compares joiners with never members and leavers with always members. Here, we propose only to offer a brief summary of these findings.

Presumably, with Table 5 as our template, the estimated effects obtained in Appendix Table 3 should tend to be smaller in absolute value and perhaps even exhibit perverse signs given the essentially immediate or short-run effects captured by one-year changes. On the other hand, given that we are now using one-year differences, the number of observations is substantially larger as the number of establishments with two consecutive observations in the panel is much higher than the number of establishments with three observations in a row. (The former sample is roughly five times bigger than the latter.) Alternatively put, although we might expect the one-year effects to be somewhat messy, their statistical significance might not be that low. Even if one does not reject the hypothesis that the effects of, in this case, leaving an agreement should measurably increase through time the 'instantaneous' impact of the transition may yet be non-negligible.

And that does indeed seem to be the case. After one year, the effect of leaving either a sectoral agreement or a firm agreement or any collective agreement is negative, falling in the -0.4 to -2.1 per cent range (see the first, second, fifth, and seventh rows of the appendix table). In turn, the effects of joining (again either a sectoral, firm, or any type of collective agreement) are much less clear-cut, especially in the second sub-period, where the coefficients are negative or non-significant in all four possible cases (see the third, fourth, sixth, and eighth rows). (In the first sub-period the corresponding coefficients vary from 0.3 to 1.2 per cent.)

These results might lead us to conclude that the effects of an establishment leaving a collective agreement on wages are more rapid than the effects of joining. However, the number of perverse signs for worker wage changes in establishments that join a collective agreement would seem to devalue the one-year difference/spell fixed effects strategy. All things considered, then, we prefer to base our conclusions on the firmer ground of estimates derived from two-year differences.

7. Conclusions

That over the last two decades collective bargaining coverage has declined, and that the trend persists, seems to be beyond dispute. Much less clear-cut, however, is the impact of this decline on wage development. Indeed, the literature lacks a critical value: an updated estimate of the union/collective bargaining premium. This is provided in the present treatment which covers a period of a near standstill in German wages.

Whatever the reasons behind the erosion of collective bargaining coverage, we would not anticipate an elevated union wage premium, since unions should have become weaker rather than stronger. As a matter of fact, joining any type of agreement from a position of non-coverage has proven decreasingly favourable to wages, while the reverse transition has become decreasingly unfavourable. On the other hand, and looking at the two types of collective agreements separately, leaving sectoral agreements to non-coverage does involve losses (albeit decreasing), while joining a sectoral agreement from non-coverage entails wage gains at a slightly increasing rate. The concatenation of these results obviously implies that workers in establishments that have switched to firm agreements from non-coverage are gradually losing the wage advantage.

Our results are not directly comparable with those of previous studies because of differences in methodology. Nevertheless, they are consistent with the findings of studies seeking to tackle the causality issue in that the collective bargaining premium is smaller than more conventional estimates. More importantly of course, and unlike the former, the present study is able to chart *movements* in the collective bargaining premium over the course of the first decade of the 2000s. And in terms of broad movements into and out of collective bargaining, these changes are in the main consistent with the decline in union influence implied by diminishing coverage over that interval.

That being said, the transitions between firm and sectoral collective agreements do not seem to offer any clear-cut conclusions, with indications that establishments switching from firm to sectoral agreements tend increasingly to register wage gains, while at the same time switching from sectoral to firm agreements seems also to be increasingly beneficial. This apparent contradiction seems to be due to the lack of comparability in the selected estimation samples, which is not altogether surprising given the non-experimental nature of our exercise.

More interesting are the results generated by the reverse counterfactuals. Here, the most important finding is that although we generally expect workers to have higher wages after their establishments join a collective agreement, and lower wages after leaving, the gains – or losses – tend to be smaller (in absolute terms) if one compares the treated group (i.e. joiners or leavers) with never members or always members, rather than with the initially selected control groups of always members and never members. This finding confirms the presence of some persistence in the effects of collective bargaining coverage, an anticipated result given the rules governing the German industrial relations as described in section 2. Against this backdrop, we distinctly prefer our two-year estimates to any estimate based on a one-year spell fixed effects procedure.

Endnotes

1. The link between density and coverage by a collective agreement is noted in Section 2.

2. A related issue is whether the erosion of the industrial relations architecture might lead to 're-stabilization from above' and hence greater involvement of the nation state in wage fixing via national minimum wages and/or heightened use of extension provisions.

3. The main exception is the study by Dustmann, Ludsteck, and Schönberg (2009), which examines *a number of explanations* for the growth in German wage inequality in the 1990s, including changes in collective bargaining.

4. See also the interesting analysis of the 1995 and 2005 waves of the GSES by Heinbach and Spindler (2007), using the Machado-Mata decomposition technique.

5. Similar results for a different state (Baden-Württemberg) are reported by Bechtel, Mödinger, and Strotman (2004).

6. Stephan and Gerlach also report that the rates of return to human capital as well as the gender wage gap are lower in firms with collective agreements than in companies with individual contracts.

7. See also Fitzenberger and Kohn (2005), and Antonczyk, Fitzenberger, and Sommerfeld (2010).

8. He also reports the average treatment effect on the treated – or idiosyncratic gain for the individual receiving the treatment – which is roughly twice the average treatment effect.

9. See the excellent survey by Hirsch (2004).

10. Note that applying OLS to model (1), as was done in Table 3 in a purely cross-section fashion, is equivalent to assuming away worker and firm unobserved heterogeneity – or, alternatively, that ω_{it} is not correlated with U_{jt}, where $\omega_{it} = \theta_i + \psi_j + \varepsilon_{it}$.

11. It would also be possible to use the raw annual data and run the spell fixed-effects version of model (1). In this case, by first-differencing within each spell (only consecutive observations on job stayers are useable for estimation), we have $\Delta\theta_i = 0$ and $\Delta\psi_j = 0$, and therefore model (1) becomes $\Delta y_{it} = \Delta Z_{it}\beta + \delta\Delta U_{jt} + \Delta\lambda_t + \Delta\varepsilon_{it}$, where Δ denotes the first difference operator. For completeness, we will comment on the results from estimating this model in section 6. Note that by computing 3- and 4-year differences, for example, we would have both a substantial reduction in the number of workers (as the number of establishments with four

and five consecutive observations is much smaller than the number of establishments with two) *and* a sharply increasing proportion of large establishments in the total number of establishments with available data. (Model (3) in turn forces us to use only those establishments observed in three consecutive years.)

12. Clearly, the difference-in-differences model in (3) does not contemplate any such differential macro effect. Indeed, it assumes k_T=k_C.

13. The results in the fourth row of the table suggest that joining a sectoral agreement from an initial state of having a firm agreement is more favourable to wage development than being always covered by a sectoral agreement. This finding contradicts the estimates in the fourth row of Table 5. The probable reason is sample size, which is quite different in the two experiments likely invalidating the comparison.

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Table 1
Longitudinal Pattern of Observed Workers

2000-2002				
Year of observation			Number of workers	Number of establishments
2000	2001	2002		
Yes	Yes	Yes	721,321	6,279
Yes	Yes	No	327,829	6,687
Yes	No	Yes	48,503	4,210
Yes	No	No	549,528	9,192
No	Yes	Yes	127,890	5,207
No	Yes	No	142,360	5,972
No	No	Yes	491,841	7,236
			Total=2,409,272	
2008-2010				
2008	2009	2010		
Yes	Yes	Yes	627,027	6,858
Yes	Yes	No	260,129	6,649
Yes	No	Yes	77,968	3,901
Yes	No	No	461,133	9,060
No	Yes	Yes	98,996	5,563
No	Yes	No	93,982	5,715
No	No	Yes	288,995	7,652
			Total=1,908,231	

Note: A given worker necessarily populates one of the seven distinct patterns, but their establishments are not necessarily distinct.

Table 2
Longitudinal Pattern of Workers Observed at Least Twice Over the Observation Window

Workers with three consecutive observations					
	Year				
Profile	2000	2001	2002	Number of workers	Number of establishments
1	A	A	A	716,844	5,222
2	A	A	B	2,168	1,441
3	A	B	B	2,283	1,680
4	A	B	C	26	68
	2008	2009	2010		
1	A	A	A	623,732	6,081
2	A	A	B	1,524	1,122
3	A	B	B	1,754	1,291
4	A	B	C	17	51
Workers with two consecutive observations					
	2000	2001	2002		
5	A	A		325,473	6,176
6	A	B		2,356	1,735
7		A	A	126,439	4,754
8		A	B	1,451	1,207
	2008	2009	2010		
5	A	A		258,710	6,282
6	A	B		1,418	1,134
7		A	A	98,133	5,233
8		A	B	863	826
Workers with two non-consecutive observations					
	2000	2001	2002		
9	A		A	43,142	2,672
10	A		B	3,695	1,869
	2008	2009	2010		
9	A		A	74,832	2,799
10	A		B	2,092	1,202

Note: A, B, and C are establishment identifiers.

Table 3
Two- and One-Year Establishment Collective Bargaining Transitions

One-year transitions			
	t+1=2001		
t=2000	Anycb=0	Anycb=1	Total
Anycb=0	2,351	441	2,792
Anycb=1	424	3,215	3,639
Total	2,775	3,656	6,431
	t+1=2002		
t=2001	Anycb=0	Anycb=1	Total
Anycb=0	2,046	270	2,316
Anycb=1	405	2,548	2,953
Total	2,451	2,818	5,269
	t+1=2009		
t=2008	Anycb=0	Anycb=1	Total
Anycb=0	3,385	412	3,798
Anycb=1	413	2,931	3,343
Total	3,798	3,343	7,141
	t+1=2010		
t=2009	Anycb=0	Anycb=1	Total
Anycb=0	3,340	106	3,446
Anycb=1	323	2555	2,878
Total	3,663	2,661	6,324
Two-year transitions			
	t+2=2002		
t=2000	Anycb=0	Anycb=1	Total
Anycb=0	1,829	301	2,130
Anycb=1	422	2,415	2,837
Total	2,251	2,716	4,967
	t+2=2010		
t=2008	Anycb=0	Anycb=1	Total
Anycb=0	2,776	223	2,999
Anycb=1	378	2,280	2,658
Total	3,154	2,503	5,657

Note: Anycb denotes the presence, or otherwise, of any collective bargaining – either sectoral or firm-level bargaining.

Table 4

OLS Wage Regressions, Summary Results

	Collective bargaining argument								
	Dummy variable equal to 1 if worker is in an establishment covered by a sectoral agreement; 0 if the establishment is not covered by any type of agreement			Dummy variable equal to 1 if worker is in an establishment covered by a sectoral agreement; 0 if the establishment is covered by a firm-level agreement			Dummy variable equal to 1 if worker is in an establishment covered by any type of collective agreement; 0 otherwise		
2000-2002									
	2000	2001	2002	2000	2001	2002	2000	2001	2002
δ	0.070***	0.075***	0.070***	0.018***	0.021***	0.022***	0.064***	0.069***	0.063***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	769,989	593,733	637,147	756,738	596,214	688,990	867,881	680,348	787,919
2008-2010									
	2008	2009	2010	2008	2009	2010	2008	2009	2010
δ	0.091***	0.115***	0.140***	0.035***	0.040***	0.039***	0.081***	0.102***	0.125***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	656,425	485,264	467,885	677,988	472,299	427,984	818,831	587,493	540,864

Notes: For each cross-section the fitted model is given by $y_i = Z_i\beta + \delta U_j + \omega_i$ [see model (1) in the text]. Standard errors are given in parenthesis.
***,* denote statistical significance at the .01 level.

Table 5
Estimates of the Collective Bargaining Premium, Two-Year Differences (2000-2002 and 2008-2010), Job Stayers

Sample				*Period: 2000-2002*		*Period: 2008-2010*	
Experiment	*Treatment and control groups*	*Collective bargaining status in t-2*	*Collective bargaining status in t*	δ	*N*	δ	*N*
Scb leavers vs. Scb stayers	*Treated group (leavers)*	*Scb=1*	*Anycb=0*	-0.007*** (0.0017)	375,397	-0.004** (0.0018)	289,320
	Control group (stayers)	*Scb=1*	*Scb=1*				
	Treated group (leavers)	*Scb=1*	*Fcb=1*	-0.004** (0.0014)	378,843	0.008*** (0.001)	295,414
	Control group (stayers)	*Scb=1*	*Scb=1*				
Scb joiners vs. Scb never members	*Treated group (joiners)*	*Anycb=0*	*Scb=1*	0.007*** (0.0017)	46,000	0.011*** (0.002)	66,228
	Control group (never members)	*Anycb=0*	*Anycb=0*				
	Treated group (joiners)	*Fcb=1*	*Scb=1*	0.010*** (0.0019)	57,847	0.023*** (0.002)	86,543
	Control group (never members)	*Fcb=1*	*Fcb=1*				
Fcb leavers vs. Fcb stayers	*Treated group (leavers)*	*Fcb=1*	*Anycb=0*	-0.009*** (0.0024)	54,849	-0.007** (0.003)	83,443
	Control group (stayers)	*Fcb=1*	*Fcb=1*				
Fcb joiners vs. Fcb never members	*Treated group (joiners)*	*Anycb=0*	*Fcb=1*	0.024*** (0.0019)	44,297	0.002 (0.002)	64,992
	Control group (never members)	*Anycb=0*	*Anycb=0*				
Anycb leavers vs. Anycb stayers	*Treated group (leavers)*	*Anycb=1*	*Anycb=0*	-0.009*** (0.0013)	447,074	-0.003** (0.002)	393,165
	Control group (stayers)	*Anycb=1*	*Anycb=1*				
Anycb joiners vs. Anycb never members	*Treated group (joiners)*	*Anycb=0*	*Anycb=1*	0.014*** (0.0013)	50,956	0.007*** (0.002)	69,194
	Control group (never members)	*Anycb=0*	*Anycb=0*				

Notes: The fitted model is given model (3) in the text. Anycb is a dummy variable equal to 1 if a worker is in an establishment covered by any type of collective agreement, 0 otherwise; Scb (Fcb) is a dummy variable equal to 1 if a worker is in an establishment covered by a sectoral (firm) agreement, 0 otherwise. Standard errors are given in parenthesis.
***,**,* denote statistical significance at the .01. .05, and .10 levels, respectively.

Table 6
Estimates of the Collective Bargaining Premium, Two-Year Differences (2000-2002 and 2008-2010) but with Different Counterfactuals, Job Stayers

Sample				*Period: 2000-2002*		*Period: 2008-2010*	
Experiment	*Treatment and control groups*	*Collective bargaining status in t-2*	*Collective bargaining status in t*	δ	*N*	δ	*N*
Scb leavers vs. Scb never members	*Treated group (leavers)*	*Scb=1*	*Anycb=0*	0.009*** (0.002)	46,317	0.011*** (0.002)	69,656
	Control group (never members)	*Anycb=0*	*Anycb=0*				
	Treated group (leavers)	*Scb=1*	*Fcb=1*	-0.001 (0.001)	61,863	0.010*** (0.002)	93,589
	Control group (never members)	*Fcb=1*	*Fcb=1*				
Scb joiners vs. Scb always members	*Treated group (joiners)*	*Anycb=0*	*Scb=1*	-0.009*** (0.002)	375,080	-0.013*** (0.002)	285,892
	Control group (always members)	*Scb=1*	*Scb=1*				
	Treated group (joiners)	*Fcb=1*	*Scb=1*	0.009*** (0.002)	374,827	0.021*** (0.002)	288,368
	Control group (always members)	*Scb=1*	*Scb=1*				
Fcb leavers vs. Anycb never members	*Treated group (leavers)*	*Fcb=1*	*Anycb=0*	0.004* (0.002)	42,749	0.007*** (0.002)	65,604
	Control group (never members)	*Anycb=0*	*Anycb=0*				
Fcb joiners vs. Fcb always members	*Treated group (joiners)*	*Anycb=0*	*Fcb=1*	0.014*** (0.002)	56,397	-0.015*** (0.003)	82,830
	Control group (always members)	*Fcb=1*	*Fcb=1*				
Anycb leavers vs. Anycb never members	*Treated group (leavers)*	*Anycb=1*	*Anycb=0*	0.007*** (0.001)	49,725	0.010*** (0.001)	73,234
	Control group (never members)	*Anycb=0*	*Anycb=0*				
Anycb joiner vs. Anycb always members	*Treated group (joiners)*	*Anycb=0*	*Anycb=1*	-0.002 (0.001)	448,305	-0.013*** (0.002)	389,124
	Control group (always members)	*Anycb=1*	*Anycb=1*				

Note: See notes to Table 5.

Appendix Table 1
Two- and One-Year Establishment Transitions by Type of Collective Bargaining

Two-year transitions						
t=2000	t=2002	Fcb=1 (Scb=0)	Nocb=1 (Scb=0)	Scb=1	Total
Fcb=1	Scb=0	189	104	81	374
Nocb=1	Scb=0	77	1,829	224	2,130
Scb=1		88	318	2,057	2,463
Total		354	2,251	2,362	4,967

t=2008	t=2010	Fcb=1 (Scb=0)	Nocb=1 (Scb=0)	Scb=1	Total
Fcb=1	Scb=0	244	96	69	409
Nocb=1	Scb=0	51	2,776	172	2,999
Scb=1		83	282	1,884	2,249
Total		378	3,154	2,125	5,657

One-year transitions					
t=2000	t=2001	Fcb=1 (Scb=0)	Nocb=1 (Scb=0)	Scb=1	Total
Fcb=1	Scb=0	249	110	95	454
Nocb=1	Scb=0	99	2,334	331	2,764
Scb=1		113	305	2,758	3,176
Total		461	2,749	3,184	6,394

t=2001	t=2002	Fcb=1	Nocb=1	Scb=1	Total
		Scb=0			
Fcb=1	Scb=0	217	80	82	379
Nocb=1		70	2,030	196	2,296
Scb=1		69	319	2,180	2,568
Total		356	2,429	2,458	5,243

t=2008	t=2009	Fcb=1	Nocb=1	Scb=1	Total
		Scb=0			
Fcb=1	Scb=0	330	95	97	522
Nocb=1		95	3,368	307	3,770
Scb=1		128	310	2,376	2,814
Total		553	3,773	2,780	7,106

t=2009	t=2010	Fcb=1	Nocb=1	Scb=1	Total
		Scb=0			
Fcb=1	Scb=0	358	46	40	444
Nocb=1		22	3,283	82	3,387
Scb=1		27	180	2,130	2,337
Total		407	3,509	2,252	6,168

Appendix Table 2
Description of Variables

Variable	Definition
Individual level (variables stemming from the social security records)	
Log daily wage	Log of the daily wage of a full-time employee which is top coded due to the contribution limit in the German social security system
Log imputed daily wage	Log of the daily wage of a full-time employee; values above the contribution limit have been imputed using the procedure by Gartner (2005) (see data section for further information)
Female	Dummy=1 if female
Age	Age in years
Age squared	Age in years, squared
Foreign	Dummy=1 if foreign citizenship
Secondary / intermediate school leaving certificate without completed vocational training	Dummy=1 if yes
Secondary / intermediate school leaving certificate with completed vocational training	Dummy=1 if yes
Upper secondary school leaving certificate (general or subject-specific aptitude for higher education) without completed vocational training	Dummy=1 if yes
Upper secondary school leaving certificate (general or subject-specific aptitude for higher education) with completed vocational training	Dummy=1 if yes
Degree from specialized college of higher education	Dummy=1 if yes
College or university degree	Dummy=1 if yes
Unskilled manual occupation	Dummy=1 if yes
Skilled manual occupation	Dummy=1 if yes
Technician	Dummy=1 if yes
Engineer	Dummy=1 if yes
Unskilled service occupation	Dummy=1 if yes
Skilled service occupation	Dummy=1 if yes
Semiprofessional	Dummy=1 if yes
Professional	Dummy=1 if yes
Unskilled commercial and administrative occupation	Dummy=1 if yes
Skilled commercial and administrative occupation	Dummy=1 if yes
Manager	Dummy=1 if yes

Occupation unknown	Dummy=1 if yes
Tenure	Tenure in years
Tenure squared	Tenure in years, squared
Establishment level (variables stemming from the IAB Establishment Panel Survey)	
Establishment founded before 1990	Dummy=1 if yes
Profit situation in last fiscal year "good" or "very good"	Dummy=1 if yes. This information is derived from the establishment's reply to the question "Please give your assessment of the profit situation of your business in the last fiscal year (2007)". The five possible answers are: Profitability was "very good"; "good"; "satisfactory"; "sufficient"; "unsatisfactory".
Technical state of plant and machinery "state of the art" or "nearly state of the art"	Dummy=1 if yes. This information is derived from the establishment's reply to the question "How do you assess the overall technical state of the plant and machinery, furniture and office equipment of this establishment compared to other establishments in the same industry?" The five possible answers are: "state-of-the-art"; "nearly state-of-the-art"; "medium"; "nearly obsolete"; "obsolete".
Share of foreign workers	
Share of high-skilled and skilled workers	
Share of fixed-term workers	
Share of female workers	
Median age of the workforce	
Establishment covered by any collective bargaining agreement	Dummy=1 if yes
Establishment covered by a sector-level collective bargaining agreement	Dummy=1 if yes
Establishment covered by a firm-level collective bargaining agreement	Dummy=1 if yes
Existence of a works council	Dummy=1 if yes
Establishment size	Number of employees
Establishment size squared	Number of employees, squared
Industry (omitted category: machinery and equipment)	40 dummy variables
German federal state in which establishment is located (omitted category: North Rhine-Westphalia)	15 dummy variables

Appendix Table 3
Estimates of the Collective Bargaining Premium, Spell Fixed-Effects for 2000-2002 and 2008-2010, annual data, Job Stayers

Sample				*Period: 2000-2002*		*Period: 2008-2010*	
Experiment	*Treatment and control groups*	*Collective bargaining status in t-2*	*Collective bargaining status in t*	*δ*	*N*	*δ*	*N*
Scb leavers vs. Scb stayers	*Treated group (leavers)*	*Scb=1*	*Anycb=0*	-0.007***	1,341,088	-0.021***	1,022,416
	Control group (stayers)	*Anycb=0*	*Anycb=0*	(0.002)		(0.001)	
	Treated group (leavers)	*Scb=1*	*Fcb=1*	-0.007***	1,348,578	-0.002	1,034,444
	Control group (stayers)	*Fcb=1*	*Fcb=1*	(0.001)		(0.001)	
Scb joiners vs. Scb never members	*Treated group (joiners)*	*Anycb=0*	*Scb=1*	0.003*	190,094	-0.011***	273,148
	Control group (never members)	*Scb=1*	*Scb=1*	(0.002)		(0.002)	
	Treated group (joiners)	*Fcb=1*	*Scb=1*	0.001	198,341	-0.011***	215,575
	Control group (never members)	*Scb=1*	*Scb=1*	(0.001)		(0.002)	
Fcb leavers vs. Fcb stayers	*Treated group (leavers)*	*Fcb=1*	*Anycb=0*	-0.014***	185,519	-0.004*	203,057
	Control group (stayers)	*Anycb=0*	*Anycb=0*	(0.002)		(0.002)	
Fcb joiners vs. Fcb never members	*Treated group (joiners)*	*Anycb=0*	*Fcb=1*	0.012***	181,618	0.001	270,964
	Control group (never members)	*Fcb=1*	*Fcb=1*	(0.002)		(0.002)	
Anycb leavers vs. Anycb stayers	*Treated group (leavers)*	*Anycb=1*	*Anycb=0*	-0.010***	1,617,132	-0.018***	1,310,721
	Control group (stayers)	*Anycb=0*	*Anycb=0*	(0.001)		(0.001)	
Anycb joiners vs. Anycb never members	*Treated group (joiners)*	*Anycb=0*	*Anycb=1*	0.008***	206,589	-0.005***	287,286
	Control group (never members)	*Anycb=1*	*Anycb=1*	(0.001)		(0.001)	

Notes: The fitted model in first differences is given by footnote 11 in the text. See notes to Table 5.
Standard errors are given in parenthesis.
***,* denote statistical significance at the .01 and .1 levels, respectively.