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Contract Innovation in Germany: An Economic Evaluation of Pacts for Employment and Competitiveness

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Abstract
Pacts for employment and competitiveness are an integral component of the ongoing process of decentralization of collective bargaining in Germany, a phenomenon that has been hailed as key to that nation's economic resurgence. Yet little is known about the effects of pacts on firm performance. The evidence largely pertains to employment and is decidedly mixed. The present paper investigates the association between pacts and six outcome indicators using a framework in which the controls comprise establishments that negotiated over pacts but failed to reach agreement on their implementation. An extensive set of robustness checks are run to test the sensitivity of the key findings of the model. There is no suggestion of pacts negatively impacting any of the selected measures of establishment performance. Rather, the evidence points to some positive short- and medium-run effects on firm average wages and possibly employment and innovation as well.

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1. Introduction

Company-level pacts for employment and competitiveness, or in-plant alliances, are a feature of a contractual innovations in a number of western European countries (e.g. Sisson and Artiles, 2000). They are an aspect of the decentralization of collective bargaining, conventionally associated with the forces of globalization. However, if pacts are to be described as concession bargaining they are a version with a *quid pro quo* from management unlike the archetypal U.S. variant of the 1980s.

German pacts for employment and competitiveness (*Bündnisse für Arbeit und Wettbewerbsfähigkeit*) are distinctive in that they are an outgrowth of pronounced decline in unionism and sectoral collective bargaining coverage in that nation since the early 1990s (Addison *et al.* 2014) against a backdrop of the perceived inflexibility of sectoral bargaining. One response to this inflexibility was to take the form of *opening clauses*, allowing firms to deviate from the normatively binding terms of collective agreements. Pacts were however to outgrow opening clauses and have been described as “fast becoming part of a new ‘normal’ regulatory instrument” at a time when “collective bargaining standards are becoming guidelines that give firms considerable leeway to come to company-specific solutions” (Seifert and Massa-Wirth 2005: 238). Pacts are then no longer limited to companies in crisis, although it is also true that general opening clauses that can be used independently of the specific economic situation have become more common. However, if we are observing something of a paradigm shift in company level strategies regarding flexibility, there is still controversy as to whether such decentralization is in fact ‘organized’ or not (Haipeter and Lehndorff 2009).

The present paper will eschew consideration of the problems of articulation and control in the bargaining process and instead focus on six (micro) economic outcomes: wages, employment, investment, productivity, innovation, and survivability. Further, we offer a different counterfactual to that typically used in the standard difference-in-differences treatments. Our counterfactual is made up of those establishments in which negotiations over a pact were initiated but not brought to successful fruition. Since we have no way of knowing whether those establishments that signed a pact were actually on the brink of not signing an agreement *and* whether those that did not do so were sufficiently close to concluding an agreement, we shall necessarily have recourse to an extended set of robustness checks that control for a variety of factors, including the introduction of alternative control groups and different estimation techniques. We also present a simulation exercise in which participation of establishments in the selected treatment and control groups is randomized.

To anticipate our results, there seems to be no evidence of pacts negatively impacting any of the selected measures of establishment performance. Rather, the results suggest the presence of positive short- and medium-run effects on firm average wages and possibly on employment and innovation too.

2. Econometric evidence[1]

Econometric evidence on the *effects* of pacts is patchy. In two early studies of employment using the 2003 Works Council Survey, Hübler (2005a, 2005b) reports that establishments that had signed pacts or planned to do so in the near future had a significantly lower probability of stable or rising employment than did plants without a pact (see also Hübler 2006). However, instrumenting pacts by the estimated probability of either implementing or planning a pact confirmed the negative employment result for projected pacts alone.[2] Hübler also argued that there was a time pattern in employment effects. These ultimately became positive in the very long run, even if in practice the expiry of agreements served to limit such gains.

A distinctly less positive set of outcomes is reported by Bellmann *et al*. (20088) in an analysis using the IAB Establishment Panel (see section 4) data for the sample period 2004 to 2007. The authors draw a distinction between expected and realized employment changes as the association between the introduction of a pact and the development of expected and realized employment is likely to be interdependent. Abstracting from type of pact (either crisis or preventative), the authors pooled cross-section time-series linear probability estimates show a clear negative association between pacts and both employment measures. Instrumenting pacts corroborated these results. Finally, the authors deploy a matching estimator and their difference-in-differences estimates fail to indicate a significant treatment effect (i.e. there is no difference in employment outcomes between establishments with pacts and their non-innovating counterparts). There is again strong evidence of negative selection: establishments with inferior projected and actual employment change use pacts. Neither type of pact nor pact component dislodges the negative employment effects of pacts.

On this evidence, then, there is nothing to suggest that pacts stabilize let alone increase employment. However, if we are to depict this study period as constituting *normal times*, a subsequent/parallel study by Bellmann and Gerner (2012a) of the

Establishment Panel for the years 2006-2009, again using a difference-in-differences estimator, reports that the adoption of pacts is associated with smaller employment losses. No such beneficial effect on employment *growth* was discernible before the onset of the crisis, leading the authors to conclude that pacts helped establishments to stabilize their employment during the crisis.[3]

The two remaining pact studies reviewed here consider firm-provided further training and firm investments in physical capital. Although OLS cross-section results using the Employment Panel, 2003-2007, point to heightened continuous training investments under pacts, Bellmann and Gerner's (2012b) parametric difference-in-differences regression models with matching and semiparametric difference estimates suggest that there is no disparity in training incidence between establishments with pacts and their counterparts, while pointing to a positive selection effect (firms offering more training tend to adopt pacts). Further, pacts are reported not to have positive causal effects on training intensity either.

Results from a more extensive study of investments in physical capital are at best suggestive since apart from explicit commitments on locational investments the effects of pacts are most often statistically insignificant. Using data from the Establishment Panel, 2001-2010, Bellmann *et al.* (2015) provide OLS, IV, and difference-in-differences estimates of a capital growth model that point to little significant impact on investment. The exception is net investment, where the impact is negative. Even if more positive results are obtained when a distinction is made between the adoption of a pact and the contract period of a pact, the bottom line of this inquiry is that pacts do not serve as instruments of growth of the firm's capital stock but rather contribute to its consolidation and modernization.[4]

3. Modelling strategy

As mentioned earlier, pacts are mutual accords between management and workforce representatives geared to the resolution of company-specific problems related to employment and competition. This means that a pact P will come into existence (i.e. $P=1$) whenever (a) some negotiation is actually carried out and (b) an effective agreement is reached.[5] In turn, a given negotiation will be successful if some variable V exceeds a certain threshold or cutoff point c, where V is a function of exogenous firm-level characteristics in the set X. Presumably X has also an impact on Y, the outcome variable. More importantly, the unobserved determinants of both Y and V are likely to be correlated, as are P and the error term in the outcome equation.

Formally, we have the following reduced-form dummy endogenous variable model:

$$Y = b * P + XB + e, \quad (1)$$

$$\text{with } P = 1[V > c], \quad (2)$$

$$\text{and } V = X\psi + u. \quad (3)$$

In this framework, the OLS estimate of b is obtained by simply running an ordinary linear regression on equation (1), which is equivalently given by $E[\ Y|\ X = x, P = 1] - E[\ Y|\ X = x, P = 0]$. An unbiased estimate will then require that the following condition is met (see DiNardo and Lee 2004):

$$E[\ e|V > c] - E[e|V \leq c] = 0. \quad (4)$$

In general, condition (4) will not be satisfied, unless there is a particular sample design, that is, a mechanism by which any selected unit has an identical chance of being on either side of the cutoff point c. Then, in the neighbourhood of the cutoff point, the status P depends only on X, which makes e and P uncorrelated.

In our implementation, we know for sure that if workers and management are successful in their negotiations there will be a pact. We also know that in our data there is no contamination from 'no-shows' or 'crossovers', by which we mean that the non-treated group will never contain any unit qualified for treatment since it does not make sense having a successful negotiation and not signing a pact. Nor for that matter will any non-qualified (for treatment) unit ever be treated because there is simply no pact agreement to be signed.

However, we cannot test whether those establishments with a pact were actually on the brink of not signing one, nor whether those establishments that failed to sign a pact were sufficiently close to actually reaching an agreement. We simply observe whether or not the negotiations were successful, not having access to the votes that might have been cast during the negotiation process. Accordingly, any parsimonious model that simply regresses Y on P with no other covariates will have to be based on the assumption that establishments with and without a pact do not differ in observables in an obvious manner. Under this assumption we would end up with the simple regression model $Y = a + b * P + e$ or, with the addition of an establishment subscript i,

$$Y_i = bP_i + e_i, \quad (5)$$

where $P_i = 1$ if $V_i > c$ and $P_i = 0$ if $V_i \leq c$.[6]

Model (5) gives an immediate measure of the raw outcome difference across establishments with and without a pact. Not wishing to rely exclusively on the assumptions of this model, however, we will present various regression approaches that do control for the presence of a relevant set of covariates, either within a single or simultaneous equation framework, while at the same time experimenting with different types of control groups (including those having a basis in propensity score matching).

The set of included covariates will be based on a mean difference test between treatment and control groups, while the simultaneous equation implementation is intended to test for possible interdependence across the selected establishment-level performance indicators. The two control groups alternative to that based on plants with abortive pact negotiations are given by units that simply do not have a pact on the one hand, and on those without a pact but with ongoing negotiations on the other. The base control group and the first of the two alternative controls groups just mentioned will be used for the matching exercise. In a final step, we shall provide a simulation exercise that randomizes participation in the treatment and control groups. After all, if establishments with a pact agreement in no case were at risk of not signing an agreement (or if establishments that were unable to reach an agreement were doomed to fail anyway), then the required randomization of participation is not satisfied, the selected units are not effectively comparable, and in consequence any causal effect will not be identified. In other words, given that we are not certain that all establishments in the treatment (control) group are indeed *near-winners* (*near-losers*) – although presumably some of them are – our strategy consists in randomly selecting in each group of treated and control units only a given fraction of the initial sample. This strategy has the advantage of offering a quick validation test as to whether the simulation results are indeed centred on the point estimates. In an *ad hoc* manner, we fixed the fraction first at one-third and then at two-thirds for both control and treatment groups and in each case repeated the simulation 1,000 times (our *Simulation I* exercise). We next allocated a random probability to each unit in the selected treatment and control groups and ran a weighted regression in which the weights were given by the inverse of the generated probability (*Simulation II*).

4. Raw data and the longitudinal panel construction

The relevant information for our empirical analysis is extracted from the IAB Establishment Panel, a nationally representative panel survey of establishments based on a stratified random sample of the population of all establishments with at least one employee covered by social insurance (see Ellguth *et al.* 2014). Specifically, our observation window for estimation purposes covers the 2006-2009 interval. However, as it is useful also to briefly describe the history of pacts – and opening clauses – we include data for 2005-2013 as well, the starting date here reflecting the fact that the first IAB survey questionnaires containing a specific question on the existence of opening clauses and pacts were 2005 and 2006, respectively. The information on pacts is of course critical in our analysis, while the information pertaining to opening clauses, given their role in the decentralization process, is included for contextual completeness.

Information on pacts was requested of establishments in 2006, 2008, and 2009 (and in 2013 as well). In particular, the data on pacts in the 2006 survey is very rich: it covers information on whether agreement on a company-level pact had been reached, the duration and term of that agreement, its type, and the degree to which efforts had been made to sign an agreement either in 2006 or even earlier. Subsequent information on pacts in the 2008 and 2009 surveys is rather less detailed. Nevertheless, all we need to construct our estimation sample is to ensure that pact status is correctly flagged in 2007, 2008, and 2009. Given that the follow-up survey questionnaires include information on whether an agreement had been reached (questions 28a and 85 in the 2008 and 2009 questionnaires, respectively), while the 2008 survey gives the year of introduction of the current agreement (question 28b), determining pact status in 2007 is immediate. That is, we have $P_{2007} = 1$ if the respondent answers that there is a pact in 2008 (in question 28a) and that the year in which the pact has been agreed is 2007 or before (question 28b); and, conversely, $P_{2007} = 0$ if the respondent indicates no presence of a pact in 2008 (question 28a) and also that such a pact did not exist in the past (question 29a). All other cases are discarded from the sample. Over this observation window, we further constrain all the sample units to be observed in 2006 *and* at least once in the 2007-2009 interval in consecutive observations.

As noted above, the 2006 survey contains information on whether negotiations on a pact were ongoing or had occurred in the past. Specifically, question 41a of the 2006 IAB Establishment Panel asks whether *the establishment has reached an agreement with employees regarding the safeguarding of jobs and/or locational competitiveness* (*Vereinbarung zur Beschäftigungs- oder Standortsicherung*). Next, for those responding to this question in the negative, the survey goes on to ask of the manager respondent first whether a pact *did exist in the past* (question 41b), second whether or not *there currently are any negotiations under way with a view to reaching an agreement* (question 41c), and third whether, albeit abortive, there had been any past *efforts to reach an agreement* (question 41d). Responses to these questions provide the basis for constructing our treatment and control groups.

Our (maintained) treatment group in particular is made up of all the 2006-2009 non-switchers with a pact active in 2006. This implies that in practice we have in the treatment group the pact-year sequences (*PPP.*) and (*PPPP*), where the first (last) element in the sequence indicates sample year 2006 (2009), *P* denotes the presence of a pact (i.e. *P=1*), and where a dot signifies that the status is not observed. All the units in the basic control group – we shall refer to it as CG1 – have a fixed status too. That is to say, they have no pact in 2006 *and* no pact at all thereafter, but *were* involved in unsuccessful negotiations in 2006 or earlier. In this case, we have the sequences given by ($\bar{P}\bar{P}\bar{P}$.) and ($\bar{P}\bar{P}\bar{P}\bar{P}$), with $\bar{P}$ denoting *P=0*. The description of alternative control groups is given below.

Regarding the construction of alternative control groups, we focus on two cases, both again extracted from the survey questionnaire. The first group, or CG2, is simply made up of all units with no pact in 2006 (i.e. those that have answered 'no' to question 41a); the second alternative group (CG3) is a subset of the basic control group CG1, as it contains only the units that have answered 'yes' to question 41c.

Returning to the purely descriptive part of the paper designed to provide historical context, the 2006-2009 window is enlarged to include the information pertaining to 2005 and 2010-2013. This procedure does however require some imputation as it will be recalled that the information on pacts is not available on a continuous basis. The procedure amounts to using all the available contiguous information. That is to say, if in a given year *t* an establishment is in the survey but the relevant question on pacts is not asked, then we use the information available in either *t-1*, *t-2*, …, and/or in *t+1*, *t+2*,…, subject to some additional condition. (As shown below, an identical procedure will be used to impute opening clause status, *OC*). In particular, for any year *t* in the interval 2010-2012, we use the rule $P_t = P_{2009}$, if $P_{2009} = P_{2013}$, combined with the information on 2006 pact duration, whenever available. For 2005, the imputation is based solely on the 2006 pact duration. In other words, imputation is always based on some longitudinal information. As a result, while the incidence rate in

2006, 2008, 2009, and 2013 is based on all surveyed units, for the years 2005, 2007, and 2010 through 2012 the computation is based on units that are required to have some longitudinal presence. The resulting information for the entire 2005-2013 interval is therefore unique albeit subject to limitations stemming from computations derived on the basis of varying sample size.

A similar procedure was applied to the imputation of opening clauses in 2007, 2008 through 2010, and 2012 through 2013, with a further adjustment being required to accommodate the fact that the duration of an opening clause is not available in the raw data. In this case, we assume some persistence in opening clause status conditional on the corresponding collective bargaining status being unchanged. (The question on collective bargaining status, *CB*, is always asked in the survey.) Thus, by way of illustration, for 2008 we use the rule $OC_{2008} = OC_{2007}$ if $CB_{2008} = CB_{2007}$, while for 2012 we have $OC_{2012} = OC_{2011}$ if $CB_{2012} = CB_{2011}$. For 2006, it is assumed that the same *OC* status obtains as in 2005 if there is no change in opening clause status over the (longer) interval 2005 to 2007.[7] Again, to repeat, neither imputation procedure (i.e. described in this and the preceding paragraph) is used in our cet. par. analysis, and is deployed only for descriptive, scene setting purposes.

Our dependent variables comprise six establishment-level outcomes: the employment level (given by the number of full-time employees, or FTE), the real wage per FTE, total investment per FTE, labour productivity (measured by value added per FTE), and innovation and business survival dummies. The two latter variables flag whether any product or process innovation was carried out by the establishment and whether an establishment remained in operation by 2009, respectively.

Our set of regressors comprise detailed establishment-level characteristics. They include type of collective bargaining coverage – sectoral, firm-level, or absence of coverage – and the presence of a works council or other forms of staff representation. We also collected information on the shares of highly skilled workers, those on fixed-term contracts, and part-timers, as well as the provision of further training. Additional arguments included the establishment’s expected business volume, exports as a share of sales, the state of its technical equipment, and several ownership measures (whether the establishment is a single firm or forms part of a multi-establishment entity, whether or not it is foreign-owned, and whether or not it is individually owned). The remaining regressors are the establishment’s geographic location (in either western or eastern Germany), industry affiliation, and employment size.

Finally, returning to business survival, this variable is derived from Hethey and Schmieder (2010), who used worker flows to identify newly-born and failed establishments. The information on establishment births and deaths covers the intervals 1975 to 2010 in the case of births and 1975 to 2009 for deaths. A detailed overview of the dataset can be found in Hethey-Maier and Seth (2010) and Gruhl et al. (2012).

5. Pacts: Some descriptive statistics

We next describe our (unweighted) time series on pacts/opening clauses over the period, 2005-2013. The main purpose is to add context by charting the relevant trends in both constructed series over a sufficiently long period. We remit to the next section the regression results extracted from the incomplete panel of establishments observed over the shorter 2006-2009 interval.

Beginning with opening clauses, there is no sign of any strong upward trend in their existence across establishments. In effect, the actual data (i.e. the 2005, 2007, and 2011 figures) point to a slight increase in coverage of 1.8 percentage points, while the imputed data for 2012 and 2013 actually suggest a slight decrease in more recent years.

[Figure 1 near here]

Pacts in turn are clearly less frequent than opening clauses, reaching their peak in 2009 when 7.6 per cent of all establishments were covered, declining to 3.7 per cent in 2013. The imputed data on pacts over 2010-2012 perhaps indicate some modest growth but overall the data point to their having become less frequent. For their part, pacts in establishments with opening clauses have also become less common: by the end of the sample period 14.9 per cent of establishments with opening clauses had signed a pact, as compared with their peak incidence of 30.8 per cent in 2006.

As shown in Figure 2, the *incidence* of pacts is much higher in the presence of collective agreements than in their absence. In 2013, for example, the incidence across the two groups was 7.2 and 1.2 per cent, respectively, or very roughly one half the corresponding levels for 2006. This pattern contrasts with a visible upward trend in the existence of opening clauses: as a percentage of all establishments covered by a collective agreement, opening clause incidence increased by approximately 12 percentage points between 2005 and 2011. (The imputed data suggest an incidence rate of 38.5 per cent by 2013.) Pacts are also much more common in establishments with works councils,[8] peaking at 20.3 per cent of all establishments in 2009 before declining to 11.2 per cent in 2013.

[Figures 2 and 3 near here]

Figure 3 shows the *use* of opening clauses in establishments with collective agreements as well as the incidence of pacts in these establishments. Observe that the proportionate decline in the latter exceeded the increase in the former over the three years shown. The gap between 2008 and 2010 reflects our decision not to impute the use of opening clauses in the missing years.

Table 1 shows the sample probability of pacts, given works council and collective bargaining status. As indicated in the top left cell of the table, 32.4 per cent of establishments with a works council and a firm-level agreement have a pact. This percentage is much higher than obtains for the works council-sectoral bargaining combination, for example, at 16.5 per cent. And where collective agreements are distinguished by their absence, the incidence of pacts falls even further to 10.9 per cent. This pattern holds over time, although it seems to be the case that as pacts have become less common, the differences across the combinations shown in the first three columns of the table have become more muted. The hallmark of the situation in which works councils are absent is transparent: a very low incidence of pacts. In 2006, for example, the maximum sample probability of pacts was just 3.1 per cent in these circumstances.

[Table 1 near here]

A second issue has to do with the type of pact (i.e. its legal form) and the relationship with collective agreements and worker representation. The survey identifies five distinct types: a formal establishment agreement involving the works council and management as the main partners (*Betriebsvereinbarung*); a company agreement between the trade union(s) and the employer (*Haustarifvertrag)*; a contract of employment agreed between individual employee and employer (*Arbeitsvertrag*); a less formal verbal arrangement (*Mündliche Vereinbarung*); and, finally, a residual 'other' category (*Sonstige*).

Table 2 shows that the *Betriebsvereinbarung* category is the dominant type of pact, with an approximate 60 per cent share of the total. Interestingly, if an establishment is covered by both a works council and a sectoral agreement, a *Betriebsvereinbarung* is much more likely to occur than a *Haustarifvertrag*, the latter arrangement being much more likely for the firm-level agreement-works council combination. Unsurprisingly, if there is no collective agreement of any type, then an *Arbeitsvertrag* is much more likely to be encountered, especially in the absence of a works council.

[Table 2 near here]

Unfortunately, this disaggregation of pacts by type is only available for 2006. But based on the 2006 survey, there seems nevertheless to be some tendency towards pacts developing in situations where trade unions do not play the leading role (at least directly); that is, as the sole entity directly discharging pact implementation. Witness the domination of the *Betriebsvereinbarung* type of pact. In this sense, the decentralization process heralded by contract innovation might seem to be conducted at arms' length from trade unions. Offsetting any such trend, however, is the strong participation of trade unions in the negotiation of *Haustarifverträge*.

6. Findings

We begin our cet. par. analysis by further describing the treatment group and our preferred control group CG1. Note first that we have a maximum of 544 units in the set of treated units from a total of 1,036. This reduction in sample size reflects the fact that not all units observed in 2006 can be followed longitudinally, either by reason of their having rotated out of the panel or because their status is missing or has changed after 2006. For its part, CG1 contains a maximum of 144 units. Further, not all outcome variables are always observed; for example, while employment can be observed in almost all cases, there is significant slippage in respect of the labour productivity and real wage variables.

Given the procedures followed in the construction of the 2006-2009 panel (described in section 4), our expectation would be that the underlying establishment characteristics are not systematically different across treatment and control groups, and also that the base-year (i.e. 2006) outcomes for the two groups are not too divergent. As far as the outcomes are concerned, we found a statistically significant difference in mean values at the 0.01 level or better in two cases (establishment-level real wages and employment) and at the 0.05 level in a third (labour productivity). The null is not rejected for investment, innovation, and survival.

As far as the other observables are concerned, it is useful to distinguish between three subsets of variables: first, works council, sectoral agreement, and firm-level agreement; second, industry affiliation and establishment size; and, third, all the remaining (18) variables. In the first subset, the mean comparison two-tailed t-test, again not shown here, confirms that establishments with workplace representation, and covered by a firm agreement are more likely to have a pact, although this is not the case for establishments covered by sectoral agreements. As far as the second subset is concerned, in only 1 out of 19 industries does the t-test reject the null at the 0.01 level, while the difference in the means of establishment size as between the treatment and control groups is statistically significant in 3 out of 6 cases – one (two) at the 0.01 (0.05) levels. For the third subset, the null is rejected at the 0.01 level or better in 4 instances, and at the 0.05 level in a further 3 cases. (Information on all difference in means tests is available on request.) The set of variables suggested by these tests were used to specify our regression models, which also included conventional controls for industry and establishment size. Results for a parsimonious specification containing just the latter variables are addressed below as well (see DiNardo and Lee 2004; Bradley *et al*. 2015).

Table 3 presents the estimation results from implementing various model specifications, with each cell in a given column reporting the treatment effect on one of the six selected outcomes (viz. wages, employment, investment, productivity, innovation, and survival). By definition, column (1) gives the unadjusted or unconditional difference in means across establishments with and without pacts, 1-, 2-, and 3-years after 2006. The results are also separated by control group, although a unique treatment group is maintained across experiments. The controls are designated CG1 and CG2. It will be recalled that the latter is made up of all the units that simply have no pact in 2006 (nor over 2007-2009 for that matter). This is of course a much larger group than CG1, our preferred control group, since it does not require the included units to be involved in any negotiations – either currently or in the past. In fact, CG2 comprises several thousand units rather than several hundreds of units in the case of CG1. It will also be recalled that we further also experimented with a third control group (CG3) that contains all units without a pact but involved in ongoing negotiations. Since this group is a subset of CG1, as it excludes all the units with past negotiations, it is likely to be too small (as will be demonstrated below).[9]

[Table 3 near here]

As can be seen from all three of the columns designated (1) in the table, the unadjusted mean difference in the case of CG1 is always positive and in 13 out of 15 cases (in the first five CG1 rows) the coefficient is statistically significant at conventional levels. The first major finding from this first run is, then, that there is no indication that pacts are detrimental to any of the selected outcomes. Indeed, to the contrary, this preliminary evidence does not exclude the likelihood of a positive effect of pacts for five out of six outcome indicators. Only in the case of business survival – see the sixth row for CG1 – is there no discernible impact of pacts over this short sample period.

Refinements to specification (1) in the case of all outcomes other than survival are introduced in columns (2) through (4), firstly by adding the full set of selected establishment characteristics (in column (2)), next by 'de-meaning' the outcome variable and thus using the resulting growth rate as the dependent variable (column (3)), and finally by adding the base-year outcome to the set of regressors (column (4)). Note that in the case of innovation, we have in columns (3) the change in innovation status as the dependent variable, while in columns (4) the change in innovation status is also modelled as a function of the beginning-period (i.e. 2006) status. Given that the survival dummy is necessarily equal to 1 in the base year, no results are given for column (4). Lacking results in the comparator column (4), we do not report results for column (3) either.

Again, using control group CG1, columns (2) through (4) of Table 3 show that although both the statistical significance and magnitude of the point estimate are sensitive to model specification, one constant holds: in no case is there statistical evidence to suggest that pacts are harmful to any of the selected measures of establishment performance. In 5 out of 45 cases the estimated coefficients are positive and statistically significant, while in the remaining cases one cannot exclude a zero impact on performance. Again, in no case there is evidence of any negative impact on performance, while some positive effects on wages persist and there are perhaps some positive effects on innovation as well.

We should caution that specifications (2) through (4) are particularly demanding in terms of their data requirements. The use of an extended set of regressors – for example, specifications 2 through 4 involve approximately 40 right-hand side variables – further truncates the estimation sample given that some of the establishment characteristics are not continuously observed in the raw dataset. Inclusion of a substantial set of observables comes at the non-trivial cost of a reduced number of units in the already somewhat thin treatment and control groups. Specifically, in adding 13 regressors to establishment size and industry affiliation, sample size in columns (2) is reduced by more than 20 per cent. A presumption in favour of more parsimonious models and larger samples is strengthened when, as is the case here, the set of additional regressors (beyond industry and establishment size dummies) tend not to be statistically significant. (Indeed, in only 2 or 3 out of 13 variables are the corresponding coefficients statistically significant, typically at the margin.) In short, given the construction of the relevant groups in the present study, results based on more parsimonious models may offer more traction than fuller models offering gains in the quality of fit. By way of illustration, in Appendix Table 1 we present results generated by an experiment in which we work with the most parsimonious model containing only industry and size controls. Clearly, in this case we obtain a higher number of positive and statistically significant coefficients, that is to say, stronger evidence in favour of positive effects on wages, productivity (3-year effect) and innovation (1-year).

Returning to Table 3, for each outcome indicator, the second panel in each row reports the results from using CG2 rather than CG1. As mentioned above, CG2 offers an enhanced estimation sample that is roughly 20 times larger than CG1. But the results point to the self-same conclusion. That is to say, there is no evidence of a negative impact of pacts on any of the selected establishment performance indicators. Typically, whenever the coefficient is statistically significant, it tends to be higher than in the CG1 row, a result that is certainly due to the fact that treatment and control groups are in this case much further apart in terms of establishment size – consider the much reduced size of the coefficients once we control for establishment size in columns (2) through (4).

Results for a second alternative control group CG3 are predictably statistically weaker vis-à-vis CG1, as they are generated by an estimation sample that is some 25 per cent smaller. There is no evidence favouring negative effects of pacts on performance. In this case, however, neither is the any suggestion of any positive effects. These results are not reported here but are available from the authors upon request.

7. Robustness Checks

We provide in this final section the results from three different robustness exercises. The first examines whether the reported impact on the various outcomes is sensitive to joint estimation in a simultaneous equation framework, since the results in Table 3 are obtained in separate regressions. The second exercise uses propensity score matching in the selection of the treatment and control groups. The final application uses the simulation procedure described in section 4 to obtain pure randomization of establishment participation in the relevant groups.

Table 4 presents the results from the simultaneous equation estimation. In the interests of economy, we report results based on CG1 alone. Specifications (1) though (4) from Table 3 are again implemented in this new framework. The system is estimated using the *cmp* software for Stata developed by Roodman (2014). This package has the advantage of easily accommodating the presence of a binary variable in the set of dependent variables, specifically the innovation outcome Y5. Since business survival is a separate issue – the variable flags whether an establishment observed in 2006 remained in operation by 2009 – the system of simultaneous equations does not include Y6.

[Table 4 near here]

In the first place, note that only in the case of specification (1) are the rho statistics at the base of the table uniformly statistically significant different from zero, meaning that the interdependence across equations cannot be rejected. But note that, compared with the results in Table 3, the estimated coefficients are similar in both magnitude and statistical significance. The sole exception is the productivity equation (1-year effect). In the case of specifications (2) through (4), the rho statistics are not statistically different from zero in the very large majority of the cases; and whenever the pact coefficient is statistically significant the corresponding sign is unchanged and its magnitude largely unchanged. In sum, we would conclude that the results reported earlier in Table 3 do not seem to be sensitive to whether the five outcome equations are separately or jointly estimated.

The results based on propensity score matching are given in Table 5. The comparator is again Table 3, columns (2) through (4). But on this occasion two control groups are considered, CG1 and CG2, the latter to underline the point that good matching is indeed difficult to achieve. In the absence of observables, specification (1) is missing from this exercise.

[Table 5 near here]

The matching procedure implies a strong reduction in the actual number of establishments that are actually being compared. In the top left cell of the table, for example, we are left with only some 40 percent of the units reported in Table 3, and in most cases the reduction in units is more than this. Using observables to obtain comparable groups within the framework of the relevant treatment and control groups constructed here clearly has some deleterious effects on the number of statistically significant coefficients obtained after matching. Nevertheless, for CG1 in no case do we obtain any statistically significant *negative* impact of pacts on the selected performance indicators.

An interesting aspect of this exercise is revealed by matching the selected treatment group and CG2. As can be seen in the table, in more than 50 per cent of the cases the mean difference in observables between the two groups has not been sufficiently reduced after the matching (see the p-values of the chi2 statistic). This result suggests that CG2 is not a satisfactory comparator. In contrast, when using CG1 the null of the corresponding test is rejected in only 7 cases out of 45 and never at the 0.01 level.

Finally, we illustrate our simulation exercise. The goal here is to check whether the simulation generates mean values that are centred on the point estimates reported in Table 3. To this end we produce 1,000 runs in *Simulations I* and *II.* It will be recalled that in *Simulation I* we fix the fraction of selected participants from the treatment and controls at 1/3 and 2/3, respectively, while in *Simulation II* we randomly allocate a sample probability to each unit in those groups and then run a weighted regression.

[Figure 4 near here]

In the interests of economy, the single case selected is the 2-year effect on wages in columns (3) and (4) of Table 3. (Full simulation results are available from the authors upon request.) Specifically, we want to test the sensitivity of the point estimates 0.079 and 0.089, reported in the first row of the table (2-year effects), which are statistically significant at the 0.05 and 0.01 levels, respectively. As a quick summary of the experiment, we present in each scenario the corresponding histogram of the entire distribution out of the 1,000 simulation runs. To improve readability, in all histograms we insert a solid vertical line flagging the corresponding point estimate. The histograms are also vertically aligned, with specification 3 at the top and specification 4 at the bottom of Figure 4.

Clearly, specifications (3) and (4) yield very close distributions: compare in each panel the pairs of histograms vertically. Further, robustness of the results is independent of the simulation method: compare in each panel the three histograms horizontally. Also note that the simulation results are well centred on the point estimate.

Finally, given the results generated by *Simulation II*, in which participation is fully random, there seems to be no reason to suspect that the pattern of histograms would be radically different had we changed the 1/3 and 2/3 shares in *Simulation I* to virtually any other configuration (i.e. share in the (0, 1) interval).

8. Conclusions

Pacts for employment and competitiveness are one aspect of an unprecedented decentralization of collective bargaining from sectoral to shop floor level. This decentralization, together with the growth of individual bargaining between firms and their workers, is widely held to be the key to the resurgence of the German economy. Thus, Dustmann et al. (2014) have speculated that wage restraint and decreasing real wages, and consequently falling unemployment, can be allied not only to a sharp decline in the share of workers covered by collective agreements but also to the increase in opening clauses. Our focus has been upon decentralization of collective bargaining rather than deunionization, and on pacts rather than opening clauses. The latter have been addressed in a descriptive fashion only, most obviously because our test procedure has a basis in a comparison of negotiations that (can be assumed to have) narrowly failed or succeeded. Moreover, the local agreements that we do investigate are more likely to be the result of an integrative bargaining exercise or win-win situation for both sides. A number of outcome indicators in the baseline model supported this presumption, of which three in particular – higher wages, enhanced productivity, and improved innovation – are quite robust to our sensitivity tests. Given the likely diversity in firm behaviour and the short time period examined, we consider these to be rather strong results. For the future, the workplace outcomes of decentralized bargaining of all types need be more closely monitored than hitherto, as well as the consequences of the shift in the architecture of collective bargaining for macro performance (see Visser 2013; Addison 2015).

Notes

1. Here we do not consider the descriptive literature on pacts other than to note in passing two German-language studies by Rehder (2003) and Berthold et al. (2003) that offer a typology of pacts and address what is similar to concession bargaining in the German experience and what is distinctive, and also the large sample study by Seifert and Massa-Wirth (2005) of the evolution of pacts and the link between pact content and the economic situation of the firm.

2. Hübler also examines the impact of different pact components, reporting favourable effects for some (e.g. working time extensions) but not others (e.g. work reorganization).

3. Note the qualification that during the Great Recession employment barely fell and unemployment hardly rose (see Burda and Hunt 2011).

4. For an ambitious simultaneous analysis of pacts, employment change, and real capital growth suggesting that pacts may be *bluff packages*, see Bellmann et al. (2014).

5. According to Zagelmeyer (2010), negotiations usually take 3 to 6 months.

6. In this scenario, observe that identification of b relies ultimately on the continuity of $E(e_i|V_i = v)$ at $v = c$, given that under continuity we have $\lim_{v\to c+}(E(Y_i|V_i = v) - \lim_{v\to c-}(E(Y_i|V_i = v) = [b + E(e_i|V_i = c)] - [E(e_i|V_i = c)] = b$.

7. The above discussion is necessarily a summary description; the code for the entire procedure is available from the authors upon request. We note that in 2005, 2007, and 2011 establishments were also asked whether they were currently making use of any existing opening clause. In this case, however, it would clearly be unreasonable to condition use of an opening clause status in year *t* on collective bargaining in year *t-1* or *t+1*.

8. This time series is closely mimicked by the corresponding time series on the presence of pacts in establishments with *some* type of worker workplace representation (i.e. both works councils proper and other staff representative bodies). The annual values for works councils exceed those for staff representation by less than 2 percentage points.

9. Construction of other alternative control groups is possible, but we refrain from reporting results based on such further experimentation as the set of tables would expand rapidly given the extended nature of our experiments that include different combinations of outcomes as well as short- and medium-run scenarios.

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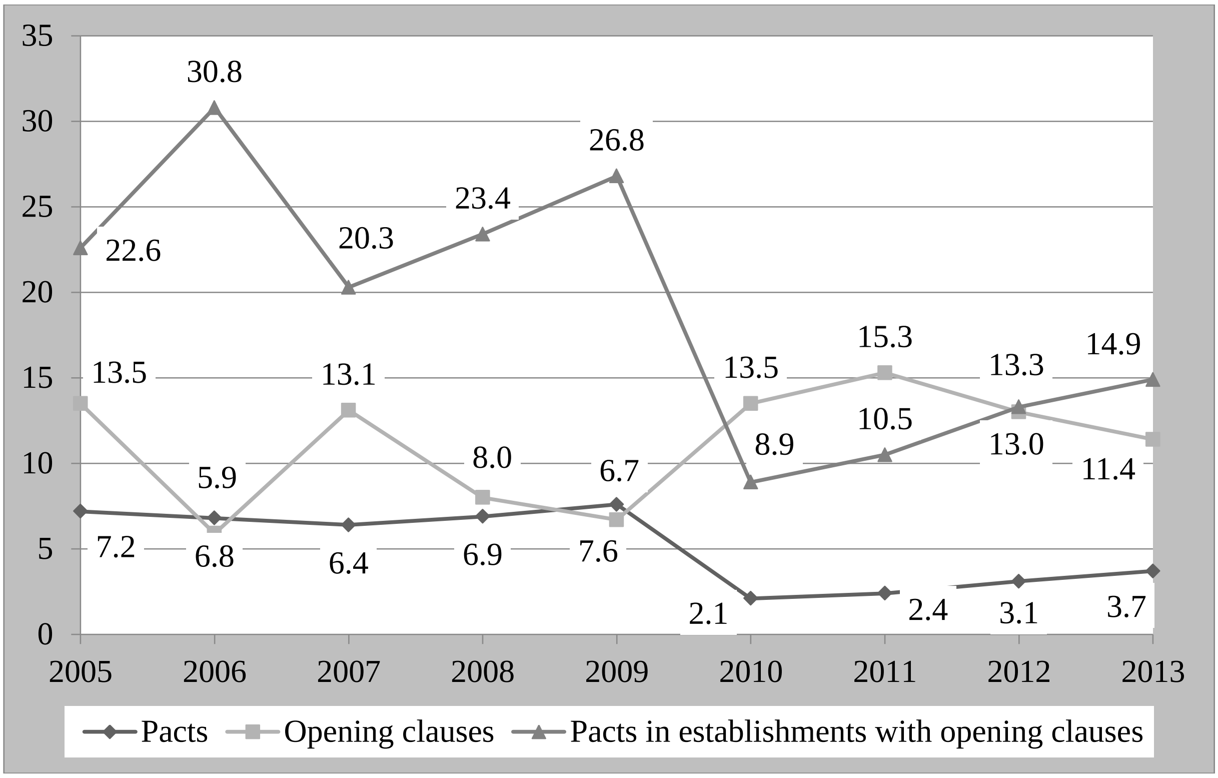
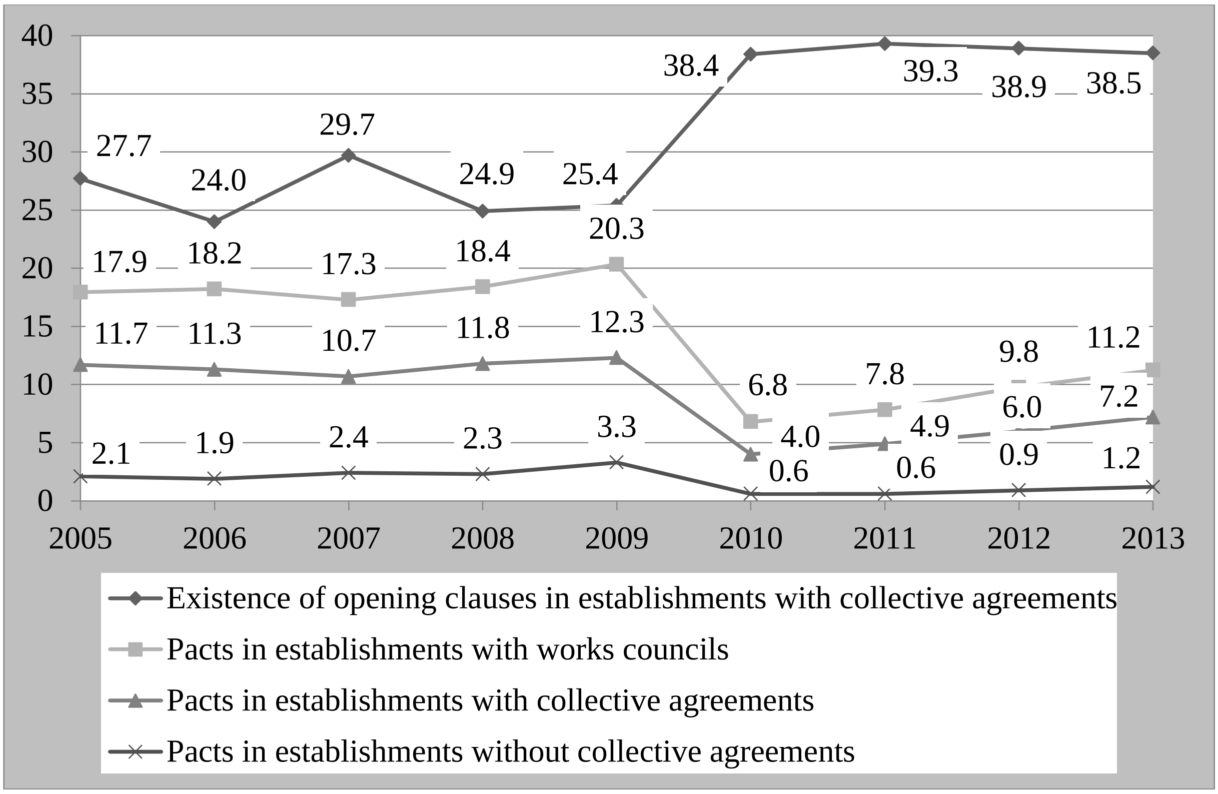
FIGURE 1
Incidence of Pacts and Opening Clauses and their Relationship (Per Cent).



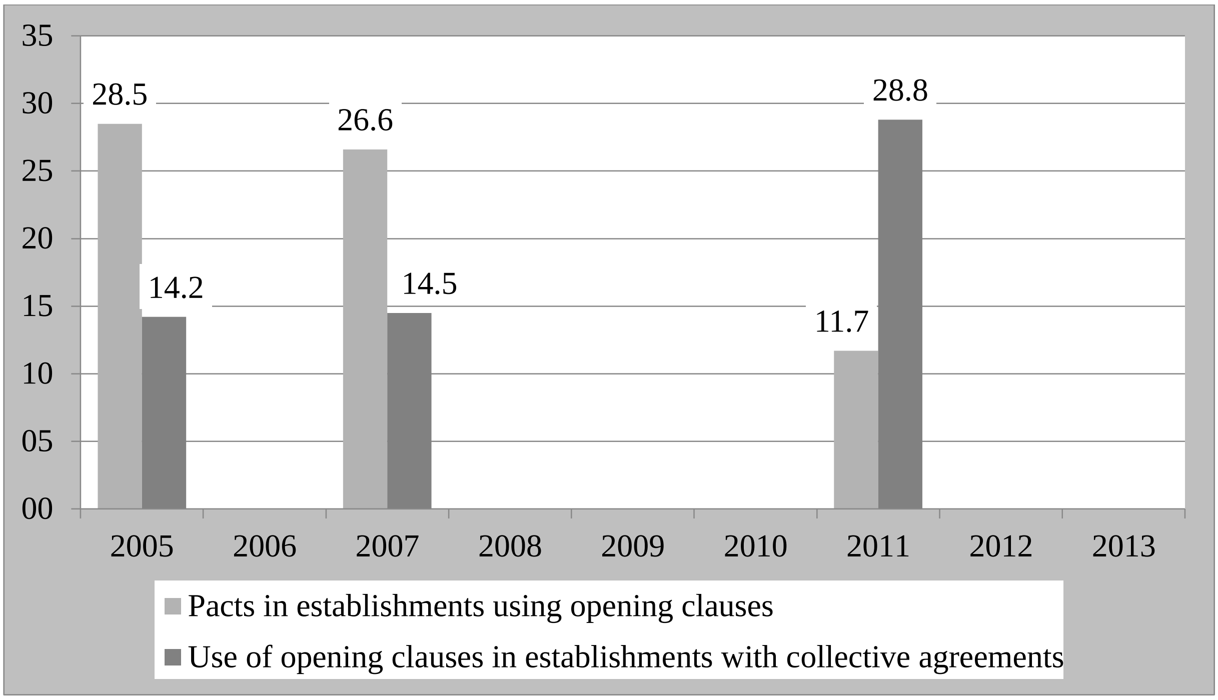
Source: IAB Establishment Panel, 2005-2013, and authors' own estimates.

FIGURE 2
Pacts and Industrial Relations (Per Cent).



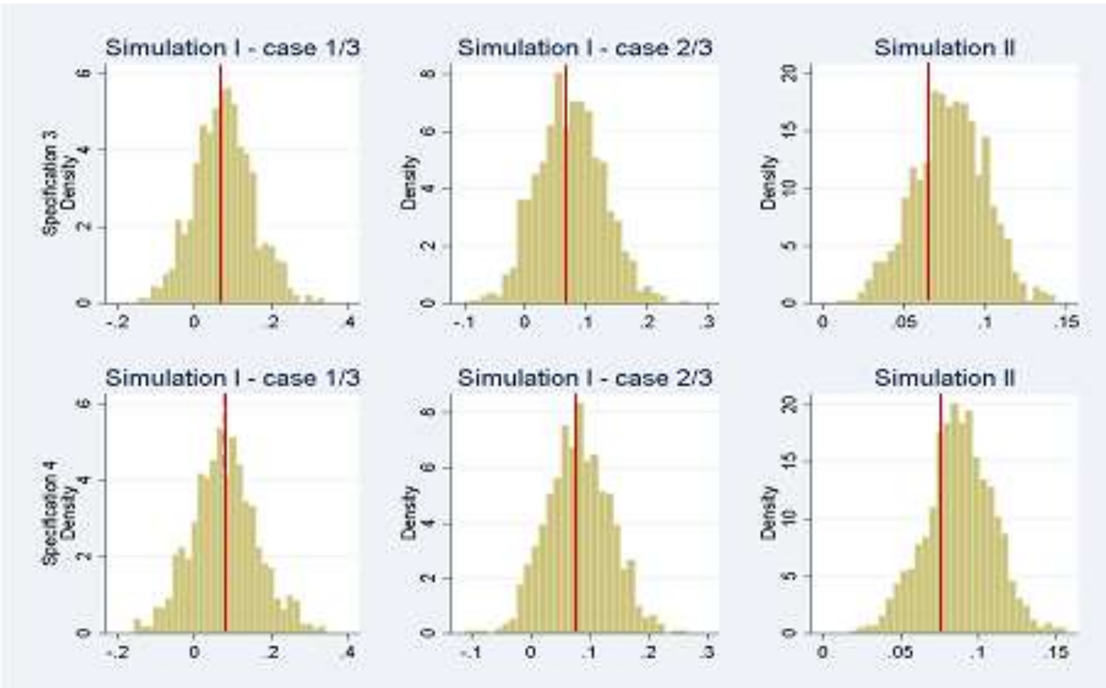
Source: IAB Establishment Panel, 2005-2013, and authors' own estimates.

FIGURE 3
Pacts and the Use of Opening Clauses (Per Cent).



Source: IAB Establishment Panel, 2005-2013, and authors' own estimates.

FIGURE 4
Simulation Histograms for the 2-Year Effect on Wages Case.



Notes: The vertical bar gives the point estimate reported in Table 4, columns (3) and (4), first row, 2-year effects. The simulation exercises are described in section 3. For descriptions of the specifications, see the notes to Table 4.

TABLE 1
Sample Probability of Pacts by the Works Council and Collective Bargaining Status of the Establishment (Per Cent)

Year	With a works council			Without a works council		
	Fcb	*Scb*	*Nocb*	*Fcb*	*Scb*	*Nocb*
2006	32.4	16.5	10.9	3.1	1.1	0.7
2008	27.9	18.5	10.3	3.5	2.5	1.2
2009	28.0	19.7	16.1	4.4	2.4	1.8
2013	15.1	11.6	7.1	4.3	1.1	0.5

Notes: The total number of establishments with non-missing information in 2006, 2008, 2009, and 2013 is 15,348, 15,353, 15,462, and 15,652, respectively. *Fcb*, *Scb*, and *Nocb* denote firm-level bargaining, sectoral bargaining, and absence of any collective agreement, respectively; for example, the top left cell indicates that 32.4 per cent of establishments with a works council and a firm-level agreement had a pact in 2006.
Source: IAB Establishment Panel.

TABLE 2
Sample Probability of Type of Pact by the Works Council and Collective Agreement Status of the Establishment (Per Cent)

	With a works council			Without a works council			Total
Type of pact	*Fcb*	*Scb*	*Nocb*	*Fcb*	*Scb*	*Nocb*	
Betriebsvereinbarung	26.9	78.3	70.5	0.0	20.0	10.2	57.7
Haustarifvertrag	65.7	2.1	0.0	66.7	2.9	0.0	18.8
Arbeitsvertrag	3.0	4.5	16.8	25.0	17.1	40.8	7.6
Mündliche Vereinbarung	0.4	1.4	2.1	8.3	8.6	20.4	2.4
Sonstige	3.7	11.8	9.5	0.0	48.6	24.5	11.2
Unknown	0.4	1.9	1.1	0.0	2.9	4.1	2.3
Total	100	100	100	100	100	100	100

Notes: The disaggregation by type of pact is only available for 2006. The top left cell combines *Betriebsvereinbarung* (B), works council, and *Fcb*; that is, 26.9 per cent gives $\Pr(B|woco \cap fcb)$. The corresponding row total (i.e. 57.7 per cent) gives $\Pr(B)$. The English translation of the pact type is given in the text.
Source: IAB Establishment Panel.

TABLE 3
1-, 2-, and 3-Year Treatment Effects of Company-Level Pacts on Selected Outcomes

Outcome		1-year effect (1)	1-year effect (2)	1-year effect (3)	1-year effect (4)	2-year effect (1)	2-year effect (2)	2-year effect (3)	2-year effect (4)	3-year effect (1)	3-year effect (2)	3-year effect (3)	3-year effect (4)
Y1-Wage	CG1	+0.176 *** (0.039) 578 obs.	+0.074 ** (0.037) 514	+0.024 (0.026) 489	+0.035 (0.025) 489	+0.195 *** (0.044) 445	+0.105 ** (0.044) 394	+0.079 ** (0.033) 375	+0.089 *** (0.032) 375	+0.154 *** (0.048) 347	+0.028 (0.052) 306	-0.004 (0.041) 291	+0.005 (0.040) 291
	CG2	+0.535 *** (0.030) 9,747 obs.	+0.079 *** (0.026) 7,717	+0.006 (0.018) 7,296	+0.023 (0.017) 7,296	+0.508 *** (0.033) 8,258	+0.082 *** (0.029) 6,483	+0.003 (0.022) 6,098	+0.026 (0.021) 6,098	+0.509 *** (0.037) 7,140	+0.044 (0.033) 5,629	-0.054 ** (0.027) 5,287	-0.024 (0.025) 5,287
Y2-Employment	CG1	+0.581 *** (0.158) 664 obs.	+0.036 (0.063) 581	+0.001 (0.021) 579	+0.002 (0.021) 579	+0.666 *** (0.174) 516	+0.054 (0.073) 453	+0.013 (0.030) 452	+0.014 (0.030) 452	+0.848 *** (0.202) 400	-0.058 (0.090) 352	-0.030 (0.037) 351	-0.031 (0.037) 351
	CG2	+2.507 *** (0.077) 11,230 obs.	+0.117 *** (0.026) 8,883	-0.010 (0.013) 8,823	-0.002 (0.013) 8,823	+2.613 *** (0.087) 9,484	+0.143 *** (0.030) 7,443	+0.006 (0.018) 7,398	+0.017 (0.018) 7,398	+2.715 *** (0.097) 8,230	+0.158 *** (0.035) 6,476	+0.006 (0.024) 6,442	+0.023 (0.023) 6,442
Y3-Investment	CG1	+0.358 * (0.191) 402 obs.	-0.065 (0.203) 359	-0.039 (0.194) 327	-0.059 (0.176) 327	+0.288 (0.240) 302	-0.126 (0.283) 272	-0.178 (0.266) 248	-0.221 (0.253) 248	+0.389 (0.256) 249	+0.039 (0.285) 223	-0.098 (0.265) 201	-0.196 (0.244) 201
	CG2	+0.479 *** (0.091) 5,546 obs.	+0.051 (0.097) 4,492	+0.057 (0.087) 3,724	+0.039 (0.079) 3,724	+0.455 *** (0.106) 4,803	+0.040 (0.116) 3,872	+0.122 (0.110) 3,096	+0.063 (0.099) 3,096	+0.488 *** (0.115) 4,144	+0.095 (0.124) 3,353	+0.173 (0.128) 2,665	+0.133 (0.114) 2,665
Y4-Productity	CG1	+0.218 ** (0.107) 309 obs.	+0.008 (0.108) 275	+0.045 (0.062) 249	+0.037 (0.061) 249	+0.153 (0.147) 236	+0.010 (0.173) 206	+0.080 (0.137) 182	+0.087 (0.138) 182	+0.229 * (0.137) 212	+0.018 (0.140) 190	+0.124 (0.103) 165	+0.121 (0.102) 165
	CG2	+0.587 *** (0.056) 5,828 obs.	+0.114 * (0.060) 4,531	+0.016 (0.040) 3,990	+0.035 (0.038) 3,990	+0.556 *** (0.064) 5,047	+0.061 (0.068) 3,917	-0.066 (0.055) 3,328	-0.033 (0.051) 3,328	+0.563 *** (0.065) 4,386	+0.096 (0.069) 3,419	-0.046 (0.060) 2,900	-0.003 (0.055) 2,900
Y5-Innovation	CG1	+0.161 *** (0.043) 494 obs.	+0.108 ** (0.043) 431	-0.028 (0.046) 429	-0.022 (0.044) 429	+0.110 ** (0.050) 396	+0.034 (0.054) 340	+0.050 (0.060) 301	+0.046 (0.057) 301	+0.096 * (0.057) 345	+0.088 (0.062) 302	-0.002 (0.068) 282	-0.001 (0.067) 282
	CG2	+0.371 *** (0.028) 9,325 obs.	+0.156 *** (0.030) 7,342	-0.048 * (0.027) 7,312	-0.050 * (0.027) 7,312	+0.325 *** (0.031) 8,079	+0.118 *** (0.034) 6,337	-0.058 * (0.032) 6,197	-0.060 * (0.032) 6,197	+0.335 *** (0.032) 7,114	+0.107 *** (0.034) 5,610	-0.038 (0.034) 5,497	-0.042 (0.033) 5,497

Y6- Survival	CG1								-0.002 (0.019) 687 obs.	+0.020 (0.023) 490		
	CG2								+0.093 ** (0.018) 13,992 obs.	+0.005 (0.019) 10,750		

Notes: The estimation sample comprises an unbalanced panel in which establishments are observed over the 2006-2009 window. Columns (1) through (4) denote separate specifications. For the 1-year effect, in column (1), the reported coefficients are obtained from running OLS on model (5) in the text, that is, $Y^k_{i,2007} = a + bP_i + e_i$, where $k = 1, \ldots, 4$ denotes the corresponding outcome variable; for $k = 5, 6$ (or outcomes Y^5 and Y^6) we run a probit model and report the corresponding marginal effects. The model in column (2) adds the following dummy variables to the right-hand-side of the base/raw specification: works council, firm-level agreement, the share of fixed-term contract workers, further training, single firm, expected business volume (4 dummies), state of technical equipment (3 dummies), establishment size (6 dummies), and industry (19 dummies). In columns (3) and (4) the dependent variable is given by $(Y^k_{i,2007} - Y^k_{i,2006})$. Finally, in column (4), the set of right-hand-side variables include the $Y^k_{i,2006}$ term. The specifications for the 2- and 3-year effects are similar, with $Y^k_{i,2007}$ being replaced by $Y^k_{i,2008}$ and $Y^k_{i,2009}$, respectively. The dependent variables are in logs, except for innovation and business survival, which are 1/0 dummies. The variables and the treatment and control groups are defined in section 4 of the text. Standard errors are in parentheses. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

TABLE 4

1-, 2-, and 3-Year Treatment Effects of Company-Level Pacts on Selected Outcomes, Simultaneous Equation Estimates

Outcome	1-year effect				2-year effect				3-year effect			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Y1- Wage	0.175*** (0.039)	+0.071** (0.036)	0.025 (0.025)	+0.036 (0.024)	0.198*** (0.044)	+0.107** (0.042)	0.078** (0.032)	+0.089*** (0.031)	0.150*** (0.048)	+0.024 (0.049)	0.0002 (0.039)	+0.008 (0.037)
Y2- Employment	0.581*** (0.158)	+0.036 (0.061)	0.001 (0.020)	+0.002 (0.020)	0.664*** (0.174)	0.054 (0.070)	0.013 (0.028)	+0.014 (0.028)	0.847*** (0.201)	-0.058 (0.085)	-0.031 (0.035)	-0.031 (0.035)
Y3- Investment	0.394** (0.188)	-0.043 (0.193)	-0.045 (0.184)	-0.063 (0.167)	0.259 (0.239)	-0.144 (0.277)	-0.117 (0.260)	-0.160 (0.252)	0.434* (0.250)	+0.004 (0.263)	-0.136 (0.245)	-0.216 (0.225
Y4- Productivity	0.120 (0.099)	-0.011 (0.098)	0.055 (0.057)	+0.044 (0.056)	0.175 (0.141)	+0.009 (0.157)	0.117 (0.124)	+0.113 (0.125)	0.155 (0.128)	+0.004 (0.126)	0.149 (0.090)	+0.151* (0.088)
Y5- Innovation	0.504*** (0.144)	+0.528*** (0.199)	-0.123 (0.213)	-0.110 (0.217)	0.306* (0.160)	+0.212 (0.223)	0.204 (0.265)	+0.181 (0.272)	0.295* (0.175)	+0.338 (0.236)	-0.024 (0.252)	-0.059 (0.251)
rho_12	0.372 ***	0.024	-0.117**	-0.114**	0.360***	0.083	-0.097*	-0.078	0.319***	+0.083	-0.239***	-0.222***
rho_13	0.305 ***	0.156**	0.094	+0.118*	0.242***	0.120	0.104	0.124	0.264***	+0.150*	-0.137	-0.080
rho_14	0.538 ***	0.449***	-0.081	-0.048	0.399***	0.308***	0.135	0.071	0.498***	+0.388***	-0.027	-0.038
rho_15	0.160 ***	-0.095	-0.072	-0.093	0.126*	0.152	0.032	-0.015	0.093	+0.123	0.028	-0.022
rho_23	0.234 ***	0.030	-0.012	-0.0003	0.165***	0.024	0.162*	0.194**	0.184***	+0.091	-0.078	-0.032
rho_24	0.096*	-0.040	-0.091	-0.085	0.049	-0.112*	-0.108	-0.074	0.070	-0.078	-0.138	-0.149*
rho_25	0.292 ***	0.069	-0.005	-0.005	0.256***	0.161	-0.106	-0.032	0.338***	0.054	-0.274***	-0.249***
rho_34	0.422 ***	0.275***	0.045	+0.074	0.553***	0.717***	0.463***	0.656***	0.377***	0.130	0.076	0.066
rho_35	0.185 ***	0.188**	-0.113	-0.138	0.195**	0.232***	-0.048	-0.043	0.070	+0.161	-0.090	-0.036
rho_45	0.148*	0.070	-0.026	-0.0006	0.079	0.078	0.106	0.303	-0.158*	-0.172	0.638***	0.834***
	666 obs.	581 obs.	581 obs.	581 obs.	519 obs.	455 obs.	454 obs.	454 obs.	402 obs.	354 obs.	354 obs.	354 obs.

Notes: See notes to Table 3. The system of equations includes performance outcomes Y1 through Y5. The variables, the treatment group, and the control group (CG1) are defined in section 4. *rho_jk* gives the correlation between the error terms in the *j* and *k* equations in the system. Row 5 reports the coefficients extracted from the corresponding probit estimation, rather than the marginal effect. The estimates were obtained using the *cmp* software for Stata (Roodman 2014). Standard errors are in parentheses. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

TABLE 5

1-, 2-, and 3-Year Treatment Effects of Company-Level Pacts on Selected Outcomes, Propensity Score Matching Estimates

Outcome		1-year effect			2-year effect			3-year effect		
		(2)	(3)	(4)	(2)	(3)	(4)	(2)	(3)	(4)
Y1- Wage	CG1	+0.140 (0.087) 220 obs. Chi2: 0.973	+0.038 (0.059) 219 0.809	+0.073 (0.054) 201 0.957	+0.026 (0.124) 159 0.058	+0.101 (0.110) 130 0.995	+0.0351 (0.091) 142 0.742	+0.020 (0.172) 98 0.902	+0.0001 (0.161) 95 0.969	-0.008 (0.130) 85 0.359
	CG2	+0.063*** (0.022) 7,214 obs. Chi2: 0.00002	+0.012 (0.015) 6,817 0.006	+0.012 (0.013) 6,818 0.014	+0.053* (0.030) 6,068 0.001	-0.013 (0.025) 5,706 0.698	+0.009 (0.020) 5,709 0.048	+0.015 (0.037) 3,899 0.523	-0.050 (0.032) 3,682 0.708	-0.042 (0.025) 3,680 0.832
Y2- Employment	CG1	-0.071 (0.318) 278 obs. Chi2: 0.967	+0.008 (0.040) 284 0.497	-0.003 (0.040) 259 0.293	-0.039 (0.424) 179 0.873	-0.002 (0.058) 172 0.691	+0.036 (0.074) 202 0.357	-0.195 (0.521) 129 0.344	+0.043 (0.107) 122 0.948	-0.016 (0.104) 127 0.217
	CG2	+0.088 (0.061) 8,291 obs. Chi2: 0.00001	-0.005 (0.011) 8,236 9.3×10^{-11}	-0.002 (0.011) 8,230 0.005	+0.170** (0.069) 6,970 0.0001	+0.001 (0.017) 6,920 0.001	+0.008 (0.019) 6,920 0.001	+0.114 (0.082) 4,513 0.086	+0.0003 (0.026) 4,477 0.259	+0.016 (0.026) 4,477 0.119
Y3- Investment	CG1	-0.223 (0.491) 137 obs. Chi2: 0.851	-0.053 (0.400) 106 0.369	-0.100 (0.550) 103 0.623	-0.619 (0.907) 87 0.092	-0.334 (0.970) 66 0.840	-0.268 (0.921) 59 0.022	+0.904 (1.066) 64 0.241	+0.191 (1.002) 53 0.318	+0.068 (1.048) 50 0.042
	CG2	-0.001 (0.141) 4,197 obs. Chi2: 0.232	+0.056 (0.107) 2,944 0.445	+0.067 (0.131) 2,956 0.395	+0.052 (0.208) 2,900 0.116	+0.120 (0.168) 2,385 0.0003	-0.006 (0.168) 2,392 0.589	+0.174 (0.195) 2,537 0.729	+0.073 (0.187) 2,091 0.329	+0.174 (0.187) 2,085 0.880
Y4- Productivity	CG1	-0.163 (0.359) 88 obs. Chi2: 0.915	-0.068 (0.180) 85 obs. 0.036	-0.073 (0.238) 81 0.612	-0.016 (1.154) 50 0.020	-0.099 (0.408) 41 0.536	-0.056 (0.509) 33 No test	+0.472 (0.691) 44 0.692	+0.018 (0.536) 43 0.046	+0.057 (0.539) 39 No test
	CG2	+0.068 (0.085) 2,869 obs. Chi2: 2.94×10^{-7}	+0.060 (0.062) 2,518 0.394	+0.069 (0.061) 2,525 0.090	-0.017 (0.147) 2,464 0.087	-0.020 (0.105) 2,074 0.153	-0.052 (0.105) 2,063 0.680	+0.100 (0.138) 2,141 0.860	+0.095 (0.096) 1,798 0.011	+0.025 (0.112) 1,806 0.470

Y5-Innovation	CG1	+0.014 (0.109) 188 obs. Chi2: 0.988	+0.094 (0.106) 171 0.525	+0.209* (0.108) 155 0.802	+0.008 (0.189) 124 0.572	+0.132 (0.231) 112 0.795	-0.024 (0.167) 101 0.640	-0.021 (0.176) 100 0.894	+0.058 (0.224) 101 0.664	-0.065 (0.205) 95 0.126
	CG2	+0.129*** (0.031) 6,870 obs. Chi2: 0.012	+0.074 (0.041) 6,835 0.415	+0.144*** (0.032) 6,838 0.112	+0.082* (0.039) 4,400 0.062	+0.037 (0.042) 4,303 0.005	+0.081* (0.043) 4,295 0.018	+0.109*** (0.043) 3,918 0.011	+0.026 (0.051) 3,822 0.00001	+0.053 (0.046) 3,830 0.004

Notes: See notes to Table 3. The results are based on radius matching with the distance *d* set at *d*=0.001, implemented using psmatch2 in Stata 14. Bootstrapped standard errors are in parentheses and the number of observations is given by the number of (on-support) units in the treatment and control groups. The chi2 statistic tests the joint significance of all included variables in the probit regression after matching. The null is no joint significance and the reported statistic is the corresponding p-value. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.

APPENDIX TABLE 1

1-, 2-, and 3-Year Treatment Effects of Company-Level Pacts on Selected Outcomes, Parsimonious Model

Outcome	1-year effect				2-year effect				3-year effect			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Y1- wage	+0.176*** (0.039)	+0.090*** (0.035)	+0.021 (0.024)	+0.032 (0.024)	+0.195*** (0.044)	+0.120*** (0.041)	+0.065** (0.029)	+0.076*** (0.029)	+0.154*** (0.048)	+0.055 (0.047)	+0.044 (0.035)	+0.017 (0.034)
Y2- employment	+0.581*** (0.158)	+0.090 (0.055)	+0.008 (0.019)	+0.010 (0.019)	+0.666*** (0.174)	+0.129** (0.064)	+0.021 (0.027)	+0.025 (0.027)	+0.848*** (0.202)	+0.018 (0.077)	-0.053 (0.034)	-0.050 (0.034)
Y3- investment	+0.358* (0.191)	+0.175 (0.185)	-0.013 (0.171)	+0.025 (0.156)	+0.288 (0.240)	-0.035 (0.247)	-0.187 (0.231)	-0.179 (0.218)	+0.389 (0.256)	+0.087 (0.254)	-0.043 (0.236)	-0.101 (0.214)
Y4- productivity	+0.218 ** (0.191)	+0.138 (0.101)	+0.042 (0.056)	+0.049 (0.056)	+0.153 (0.147)	+0.106 (0.147)	+0.056 (0.108)	+0.053 (0.108)	+0.229 (0.137)	+0.142 (0.135)	+0.199** (0.095)	+0.201** (0.095)
Y5- Innovation	+0.161 *** (0.043)	+0.112 *** (0.040)	-0.015 (0.041)	-0.015 (0.039)	+0.110 ** (0.050)	+0.042 (0.050)	-0.052 (0.051)	-0.052 (0.049)	+0.096 * (0.057)	+0.049 (0.056)	-0.045 (0.058)	-0.042 (0.057)
Y6- Survival									-0.002 (0.019)	+0.002 (0.020)		

Notes: See notes to Table 3. For the 1-year effect, in column (1), the reported coefficients are obtained from model (5), while the model in column (2) adds establishment size and industry dichotomous variables (6 and 19 dummies, respectively) to the right-hand-side of the equation. In columns (3) and (4) the dependent variable is given by $(Y^k_{i,2007} - Y^k_{i,2006})$. Finally, in column (4), the set of right-hand-side variables include the $Y^k_{i,2006}$ term. CG1 is the selected control group. The specifications for the 2- and 3-year effects are similar, with $Y^k_{i,2007}$ being replaced by $Y^k_{i,2008}$ and $Y^k_{i,2009}$, respectively. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively.