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ABSTRACT

Pacts for Employment and Competitiveness as a Role Model? Their Effects on Firm Performance

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 a wider set of outcomes – wages, employment, investment, productivity, innovation, and survivability – in a RDD framework where the controls comprise establishments that negotiated over pacts but failed to reach agreement on their implementation. An extensive set of simulations are run to test for robustness of the key findings of the model. There is no evidence of pacts negatively impacting any of the selected measures of establishment performance. Indeed, the positive effects reported for wages, productivity, and innovation are sustained in simulations.

JEL Classification: D22, J3, J41, J50, J53

Keywords: pacts for employment and competitiveness, opening clauses decentralization, concession bargaining, firm performance, regression discontinuity design, Germany

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I. 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 and not just Germany, the subject of the present study, even if the experience of the latter nation is highly distinctive.[1] They are a feature of the decentralization of collective bargaining, often referred to as *organized* or *coordinated* decentralization (but see below), and conventionally associated with the forces of globalization and Europeanization that have also brought about a corresponding shift in the design of labor policy from hard to soft regulation, also leaving much open to negotiation at lower levels (see Sisson, 2001). Pacts can be broadly described as mutual accords (*partnership agreements* in British parlance)[2] between management and workforce representatives geared to the resolution of company-specific problems related to employment and competition. But if they are to be described as concession bargaining they are a variant with a *quid pro quo* from management and hence wider in scope than the archetypal U.S. form of the 1980s (Cappelli, 1985) in which concessions largely came from the worker side alone.[3]

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 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; first in respect of working time in the 1980s until the mid-1990s (the price for the successful union campaign to reduce working hours), and for compensation since then. After an interval of informal/unsanctioned opening clauses on pay the system became institutionalized in the 2000s and the situations under which opening clauses could be used became the matter of formal agreement between the bargaining parties. Thus, companies seeking to apply hardship clauses permitting exemption from the sectoral contract wage had to prove their precarious economic condition to the bargaining parties at sectoral level, even if the details were typically negotiated at company level between management and works council. That being said, there is still a very real issue of whether such decentralization is in fact 'organized' or not. One view is that deviations from collective agreements to safeguard jobs have increasingly become instruments allowing for unspecified or restricted undercutting of standards agreed at industry or national levels – producing

deviant agreements – and that as a result there remain serious problems of articulation and control in the bargaining process (Haipeter and Lehndorff, 2009).

In some cases union/works council acquiescence to less favorable contract norms was connected to explicit pledges on the employer side such as reemployment guarantees or investment programs. Gradually this has become the norm and most opening clauses today have come to contain trade-offs. Observe however that term 'pact' is reserved for those agreements that are designated as such and are characterized by concessions from both bargaining partners at plant level. (Our data set automatically excludes agreements with unilateral concessions; see section IV, below.) No less important, pacts are 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 (Seifert and Massa-Wirth, 2005: 231-232). As a result, we are observing something of a paradigm shift in company level strategies regarding flexibility.

Not surprisingly, pacts have aroused considerable interest in Germany. The main focus has been upon the specifics of the case, namely the concessions made by workers and firms and their direct consequences. Also of importance have been evolving theoretical positions on the impact of decentralized collective bargaining on macroeconomic outcomes such as employment (cf. Berthold and Fehn, 1996; Fitzenberger and Franz, 1999) coupled with debate on microeconomic performance involving contract-theoretic considerations such as hold-up and governance linked to such changes in the architecture of industrial relations (e.g. Hübler and Jirjahn, 2003; Haucap and Wey, 2004). But in the absence of single-based expectations on the basis of theory, it follows that empirical evidence is at a premium.

Unfortunately, as we shall see, the evidence is also fragmented, with empirical exercises largely focusing on the employment outcome and, to a much lesser extent, on investments in physical and human capital. One goal of the present exercise, therefore, is to investigate a much wider range of outcomes, allowing possible combinations of outcomes to be considered. Specifically, six outcome indicators are considered: 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 also have recourse to simulations in which we randomize participation of establishments in the treatment and control groups. We also offer an alternative simulation exercise in which a random sampling probability is allocated to each unit in the original treatment and control groups and implement a weighted regression in which the weights are given by the inverse of that allocated probability. Both checks require the simulations to be centered around the prior estimates from the benchmark equation. To anticipate our results, we report that pacts have a non-negligible positive effect on wages, productivity, and innovation.

The structure of the paper is as follows. Section II provides more in the way of context on pacts, to include a review of the econometric literature. Section III outlines the modeling strategy. Section IV describes the dataset(s) used in this inquiry with special reference to the information contained on pacts and how such data may be operationalized (including the imputations required). Section V contains descriptive information on pacts. Section VI presents our detailed cet. par. results for different specifications of the regression discontinuity model together with the robustness checks. A brief summary concludes.

II. Background and Econometric Evidence

The descriptive literature on pacts provides useful background information on the German variant. In formulating a typology of pacts, two German-language studies by Rehder (2003) and Berthold, Brischke, and Stettes (2003) address what is similar to concession bargaining in the German experience and what is distinctive. Rehder offers a four-fold classification of pacts based on an analysis of the 100 biggest German undertakings. Two types of pacts involve compensation reduction (so-called *pacts for investment and compensation reduction* and *pacts for employment and compensation reduction*), and these are said to resemble American concession bargaining. The two other types – *pacts for investment and productivity enhancement* and *pacts for employment and worksharing* – in eschewing compensation reduction are held to be distinctive. Moreover, the latter dominate the former group, comprising 69 percent of the 149-pact sample investigated. Among other things, this study thus alerts us to pairs of outcomes such as rising wages and productivity. The more parsimonious typology of Berthold, Brischke, and Stettes (2003) differentiates between *adaptation* and *prevention* pacts based on a sample of 443 firms from the metal industry. The former are of the crisis variety and similar to Rehder's first two categories while the latter are forward looking arrangements

geared to improving competitiveness through internal mechanisms such as profit sharing and flexible working time accounts. We note parenthetically that in their investigation of the 2006 wave of the IAB Establishment Panel, Ellguth and Kohaut (2008) report that private-sector pacts seeking to strengthen competiveness are more frequent (48 percent) than those designed to prevent a crisis (at 41 percent).

In contrast to these two small-sample studies,[4] Seifert and Massa-Wirth (2005) examine pacts using the 2003 Works Council Survey conducted by the Institute of Economic and Social Research (*Wirtschafts- und Sozialwissenschaftliches Institut*) of the Hans-Böckler-Foundation, augmented by a mail survey of 15,000 firms. They examine the spread and content of pacts before offering a cet. par. analysis linking the economic situation of the firm to the contents of pacts. The first part of the analysis indicates that some 23 percent of firms with a works council and at least 20 employees had pacts in place; that 55 percent of pacts were concluded via a formal plant-level agreement (or *Betriebsvereinbarung*) between the works council and management; that pacts were by no means restricted to companies in economic difficulty [e.g. 39 percent (61 percent) of firms evaluated their company's order situation as either very poor or poor (good or very good) at the time when the pact was negotiated]; and that only 13 percent of pacts were one-sided agreements with no formal guarantees from management.

Having identified six composite concession categories on the part of employees[5] the authors' binomial logistic analysis indicated that these were mostly linked to the economic situation of the firm as proxied by dummies for a good/very good demand situation and a good/very good profit situation. Thus, for example, it was found that the an unfavorable/favorable order situation was the key factor determining whether a respective reduction or extension of working and operating hours was negotiated, while (poor) profits are linked to monetary concessions. (However, obvious problems in linking the economic situation of the firm to specific concessions include the lack of information on the intensity of concessions as well as the bluntness of the measures indexing the firm's financial position.) Two other rather interesting results of the exercise concern the insignificance of collective agreements and opening clauses: first, there is clear evidence of a trend toward increasing flexibility in the area of working time arrangements that is "detached from industry-wide collective regulation;" and, second, the bargaining parties at local level seemingly negotiate "temporal adjustments irrespective of industry-level opening clauses in union contracts" (Seifert and Massa-Wirth, 2005: 233).

The authors conclude that pacts have spread through large swathes of the German economy independent of the cycle and are currently deployed in weak and prosperous firms alike, confirming the findings of the small sample studies reviewed earlier. Furthermore, pacts are found to have outgrown opening clauses. In sum, Seifert and Massa-Wirth (2005: 238) observe that pacts "are 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."

Econometric evidence on the *effects* of pacts is as we have indicated patchy. Studies have tended to focus on single outcomes such as employment, investment, and training. Two broad datasets have been used in these inquiries, namely the WSI Works Council Survey of some 2,000 works and staff councils, mentioned earlier, and the nationally representative Establishment Panel (*IAB-Betriebspanel*) of the Institute for Employment Research of the Federal German Labor Agency (*Institut für Arbeitsmarkt- und Berufsforschung/IAB der Bundesagentur für Arbeit*) which is also the main dataset used in the present study. We shall devote most of our commentary to the latter since it permits the analyst to construct a panel with plants either having or not having a pact over a certain period of time (see also section IV).

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.[6] Using data on the previous duration of pacts, Hübler also argued that there was a time pattern in employment effects; specifically, employment effects that are positive initially, turn negative in the medium term, and ultimately become positive in the very long run (although only a few establishment enjoy the latter success as they typically have expired after 5 years).

A distinctly less positive set of outcomes is reported in Bellmann, Gerlach, and Meyer (2008) in an analysis using IAB Establishment Panel 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 the type of pacts (termed 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 corroborates these results with larger absolute regression coefficients being obtained for pacts. 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. Observe also that on this occasion 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.[7]

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 difference 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). Furthermore, pacts do not have positive causal effects on training intensity either. In sum, whatever concessions are agreed to in the form of wage moderation and working time, these do not appear to stimulate human capital formation.

Results from the more extensive study of investments in physical capital are at best suggestive since apart from specific measures (in particular, explicit commitments on locational investments) the effects of pacts are most often insignificant. Using data from the Establishment Panel, 2001-2010, Bellmann, Gerner, and Hübler's (2015) OLS, IV, and difference-in-differences estimates of a capital growth model suggest little significant impact on investment – other than for 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.[8]

Perhaps only in the opening clause literature is there (nuanced) evidence favoring the employment outcome under pacts. In one such study, Brändle and Heinbach (2013) have argued that firms applying opening clauses might have lower job destruction because they can maintain a larger number of employees in these circumstances and conceivably reduce job creation as well if the economic situation were to improve. By the

same token, it is also speculated that firms not applying opening clauses could increase job creation because they anticipate increased flexibility. Overall, higher job growth is predicted if firms can thereby more efficiently choose an optimal level of employment. Using IAB Establishment Panel data and information from the IAW (*Institut für Angewandte Wirtschafsforchung/*Institute of Applied Economic Research at the University of Tübingen) Data Set on Opening Clauses, the authors' propensity score analysis indicates that the *existence* of opening clauses in a sectoral collective agreement reduces job destruction and increases job growth, by -0.85 percentage points and by 0.73 percentage points, respectively. But no additional benefits stem from the *application* of opening clauses; indeed, the job growth rate is reduced. Furthermore, there is no suggestion that firms anticipate increased flexibility: the matching analysis shows that explicit knowledge of opening clauses does not lead firms to increase their hiring. In short, anticipation effects can seemingly be ruled out.

This, then, is the very mixed and limited evidence on the effects of pacts for employment and competitiveness. As we have seen, the main methodology has been propensity score matching and difference in differences. In what follows we propose a modified procedure for a different controls set comprising those establishments that tried but failed to reach an agreement on a pact with unions/employee representatives. In this sense we deploy a more robust treatment of self-selection using a regression discontinuity design. However, given that we lack the data to prove that sample variation in treatment is random we shall perforce have recourse to an extensive set of simulations to test for robustness.

III. Modeling

The RDD model and its assumptions

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.[9] In turn, a given negotiation will be successful if some variable V – called the *assignment variable* – 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 will have the following reduced-form dummy endogenous variable model:

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

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

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

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, for example, DiNardo and Lee, 2004: 1399):

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

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 neighborhood of the cutoff point, the status *P* depends only on *X*, which makes *e* and *P* uncorrelated, namely the regression discontinuity design (RDD) assumption.

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 will 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 the negotiations were successful or not, and do not have access to votes that might have been cast during the negotiation process. Our first set of indicative regression results will therefore use the strong assumption that the negotiation outcome is equally uncertain on the left and right margin of the threshold, hinted at by the favorable evidence reported below that the selected establishment-level covariates contained in *X* are fairly close across the treatment and control groups. Under this hypothesis, we end up with the simple regression model:

$$Y = b * P + e$$

or, adding the establishment subscript *i*,

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

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

In this scenario, identification of b relies on continuity of $E(e_i|V_i = v_i)$ in the vicinity of c (or continuity at $V_i = c$), given that:

$$\lim_{v \to c+} (E(Y_i|V_i = v_i) - \lim_{v \to c-} (E(Y_i|V_i = v_i) =$$
$$= [b + E(e_i|V_i = c)] - [E(e_i|V_i = c)] =$$
$$= b.$$

Description of the two sets of robustness checks to be used

By construction, all units in the estimation sample did participate in negotiations. But despite the evidence in favor of the *balanced covariates* hypothesis being satisfied in the data, we do not know whether the presumed *local* randomization is indeed effective. 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 indeed doomed to fail anyway), then the required randomization of participation is not satisfied, the selected units are not effectively comparable, and as a result the causal effect will not be identified.

Now, since the level of negotiation is unobservable, we have no indication of the actual fraction of establishments with a pact that were actually on the brink of signing/not signing an agreement. We circumvent this lack of information by running various simulation exercises in which we randomize participation of individuals in the treatment and control groups. 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 testing the robustness of the preliminary results obtained from running model (5) by randomly selecting in each group of treated and control units only a given fraction of the initial sample. This route has the advantage of offering a quick validation test if the simulation results are centered around the point estimates. In particular, we will interpret as a clear violation of the RDD assumption simulation results pointing to a negative effect. In an *ad hoc* manner we fixed the fraction first at 1/3 and then at 2/3 for both the control and treatment groups and repeat in each case the simulation 1,000 times. This exercise is referred to as *Simulation I*.

A second type of simulation exercise (*Simulation II*) was also carried out. In this case a random sampling probability was allocated to each unit in the original treatment and control groups, followed by a weighted regression in which the weights were given

by the inverse of the allocated probability. We ran this experiment 1,000 times. As in the previous exercise, if the simulation results are not centered around the point estimate, the validity of the RDD assumption is called into question. Clearly, in the proposed simulation exercises we go beyond simply testing the balanced covariates hypothesis by offering a distinct alternative robustness test, one that does not rely on observables.

IV. The Data Set

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, Kohaut, and Möller, 2014). Our observation window in particular covers the 2005-2013 interval, with the beginning period (i.e. 2005/2006) being dictated by the fact that the first IAB survey questionnaires containing a specific question on the existence of opening clauses and pacts are for 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 completeness. Currently, the sample stratification has a basis in 19 sectors, 10 establishment size classes, and the federal states. The sample encompasses more than 16,000 establishments.[10]

Unfortunately, the pact and opening clause variables are not available on a continuous (annual) basis. Specifically, establishments were asked about their pact status in 2006, 2008, 2009, and 2013 (and in 2005, 2007, and 2011 about opening clauses). At the outset, therefore, we confront three important data limitations: first, no time-series on pacts (or for that matter opening clauses) can be assembled without some form of imputation; second, even a simple cross-tabulation between pacts and opening clauses requires imputation of at least one of the two variables (as there is no single survey year in which the information on each is available); and, third, any sensible longitudinal analysis of pacts will necessarily include the 2006-2009 interval, with some modest imputation still required for 2007.[11]

Despite the obvious discontinuity in the raw dataset, the information available in the 2006 survey is very rich. In particular, it contains information on whether a company-level pact has been reached, the duration and term of the agreement, the type of agreement, and the degree to which efforts had been made to sign an agreement either in 2006 or even earlier. Specifically, question 41a of the 2006 IAB Establishment Panel

inquires 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 question 41a in the negative, the survey asks whether *there are currently any negotiations under way with a view to reaching an agreement* (question 41c); and whether, albeit unsuccessful, there had been past efforts to reach an agreement (question 41d). Based on question 41a, we then flag (as will be shown in more detail below) the treatment group, comprising all establishments with a pact (coded as *P=1*), while questions 41c and 41d allow us to define the control group (i.e. all establishments with both *P=0 and* unsuccessful negotiations in 2006 or earlier).

In order to study short- to medium-term effects, we obviously need to use the 2007, 2008, and 2009 surveys. Unfortunately, subsequent information on pacts in the 2008 and 2009 (and 2013) surveys is much less detailed. All we need to carry out the exercise is to ensure that pact status is correctly assigned in 2007, 2008, and 2009. Given that the follow up survey questionnaires include information on whether an agreement has been reached (questions 28a and 85 in the 2008 and 2009 questionnaires, respectively), while the 2006 survey includes the year of introduction and the term of the agreement, imputation of pact status in 2007 is straightforward. (If for some reason the variable is missing in 2008 or 2009, note that in most cases the information provided by the 2006 survey will be sufficient to allocate the pact status.) The 2006, 2007, and 2009 surveys allow us therefore to assemble a four-year (incomplete) panel covering the interval 2006-2009. 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.

For descriptive purposes, the 2006-2009 window is enlarged to include the information pertaining to 2005 and 2010-2013. The main goal is to describe the main variables – namely pacts, opening clauses, collective bargaining coverage, and worker representation – over a longer period than the core 2006-2009 interval. This procedure requires further imputation as it will be recalled that the information on pacts (and opening clauses) is not available on a continuous basis. In both cases, 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 or opening clauses) 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. By way of illustration, pact status in 2007 (P_{2007}) is imputed using the 2006 information on pact duration, conditional on the

pact status being the same in 2006 and 2008 (or 2009). 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 same 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. The advantage of the procedure is that we are able to replace an 'unknown' with some reliable information that is helpful in tracing the underlying developments of the selected time series, as described in section V below.

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 is always 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 $CB_{2012} = CB_{2011}$, where *OC* and *CB* are the acronyms for the opening clause and collective bargaining, respectively. For 2006, it is assumed that the same status obtains as in 2005 if there is no change in opening clause status over the (longer) interval from 2005 to 2007.[12]

We should further 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 be to condition *use of an opening clause* status in year *t* on collective bargaining in year *t-1* or *t+1*. Accordingly, we refrained from imputing this variable, and in section V we will therefore only address the corresponding raw data without making any imputation.

The 2006-2009 panel allows us to further define the treatment and control groups. Specifically, the former comprises all the non-switchers with a pact active in 2006, which implies that all the units in the treatment group will belong to one of the following sequences: (*PP*..), (*PPP*.), (*P*.*P*.), (*PPPP*), (*PP*.*P*), (*P*.*PP*), and (*P*..*P*), where the first (last) element in the sequence indicates sample year 2006 (2009), P denotes the presence

of a pact (i.e. *P=1*), while a dot means that the status is missing. An alternative treatment group comprises all establishments that have a pact in 2006 but not necessarily after 2006. This group is of course larger than the main group of treated units and it is only assembled for comparison purposes, the presumption being that even if a pact is signed, say, for one or two years, it essentially represents a medium- to long-run commitment, so that it makes sense to look at 1-, 2-, and 3-years effects irrespective of the reported status in the post-2006 period.

All the units in the control group have a fixed status too. In other words, they have no pact in 2006 *and* no pact at all after 2006, but were involved in unsuccessful negotiations in 2006 or before. In this case, we have the sequences given by $(\bar{\bar{P}}\bar{\bar{P}}..)$, $(\bar{\bar{P}}\bar{\bar{P}}\bar{\bar{P}}.)$, $(\bar{\bar{P}}.\bar{\bar{P}}.)$, $(\bar{\bar{P}}\bar{\bar{P}}\bar{\bar{P}}\bar{\bar{P}})$, $(\bar{\bar{P}}\bar{\bar{P}}.\bar{\bar{P}})$, $(\bar{\bar{P}}.\bar{\bar{P}}\bar{\bar{P}})$, and $(\bar{\bar{P}}..\bar{\bar{P}})$, with $\bar{\bar{P}}$ denoting *P=0*. The control group does not contain establishments that negotiated pacts *after* 2006.

Our selected set of dependent variables comprises 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, labor productivity (measured by value added per FTE), and the innovation and business survival dummies, flagging, respectively, whether any product or process innovation was carried out by the establishment and whether an establishment remained in operation by 2009. (On which more below). Other (observed) establishment-level variables are the type of collective bargaining coverage (sectoral, firm-level, or no coverage) and the presence of a works council or other forms of staff representation, whose main trends are described in section V. To test the *balanced covariates hypothesis* we also collect information on an extended subset of variables including the shares of high skilled workers, those on fixed-term contracts, and part-timers, the presence of further training, expected business volume, the state of technical equipment, foreign ownership, exports as a share of sales, whether the establishment is individually owned and whether is a single firm or forms part of a multi-establishment entity, location (i.e. western or eastern Germany), industry affiliation, and establishment size.

Returning to the business survival variable, the first and last year in which an establishment is observed in the German Establishment Register (or *Betriebsdatei*) may in principle be used to determine the year of death of an establishment. (Establishments have a common establishment identifier in both the *Betriebsdatei* and the IAB Establishment Panel.) But this approach has its limitations because of misclassification

bias. For example, an ownership change can yield a change of the establishment identifier, so that a continuing establishment with a new identifier will be regarded as a newly-founded establishment. To overcome such measurement errors, Hethey and Schmieder (2010) have recently used worker flows to determine newly born and failed establishments, based on German administrative data (namely, the German Establishment History Panel or *Betriebs-Historik-Panel*). The basic idea is that when clustered worker inflows fall below a certain percentage of all worker inflows (say 30 percent) in the first year of an establishment identifier then the establishment can be called a new establishment and, equivalently in the case of an exiting establishment, an establishment death where clustered outflows are less than 30 percent of employment in the year before an exit. In this way, Hethey and Schmieder provide very credible information on entries and exits of establishments, inter al., between 1975 and 2004, so much so in fact that the Research Data Center of the Institute for Employment Research now offers regularly updated versions of these data. Information on establishment births and deaths currently covers the intervals 1975 to 2010 in the case of births and 1975 to 2009 for deaths. (For a detailed description of this material, see Hethey-Maier and Seth, 2010; Gruhl, Schmucker, and Seth, 2012.)

V. Pacts: Some Descriptive Statistics

Figure 1 presents the constructed (unweighted) time series on pacts (and opening clauses) over the full sample period, 2005-2013. 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 percent of all establishments were covered by a pact, and then declining to 3.7 percent in 2013. The imputed data on pacts for the interval 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 percent of establishments with opening clauses had signed a pact, as compared with their peak incidence of 30.8 percent in 2006.

As shown in Figure 2, the incidence of pacts in the presence of collective agreements is much higher than in their absence by approximately 6 times. In 2013, for example, the incidence across the two groups was 7.2 and 1.2 percent, respectively, or very roughly one half the corresponding levels in 2006. This pattern contrasts with a visible upward trend in the existence (and use) of opening clauses. Specifically, as a percentage of all establishments covered by a collective agreement, opening clause incidence increased by approximately 12 percentage points from 27.7 in 2005 to 39.3 in 2011. (The imputed data suggest an incidence rate of 38.5 percent by 2013.) Regarding the presence of pacts in establishments with works councils,[13] their presence is unexpectedly much higher when such an entity is present at the workplace. This incidence peaked at 20.3 percent of all establishments in 2009, declining to 11.2 percent in 2013.

Figure 3 shows the *use* of opening clauses in establishments with collective agreements as well as the incidence of pacts in these establishments. Pacts also declined on the latter basis – from 28.5 (in 2005) to 11.7 percent (in 2011) – despite a doubling in the use of opening clauses in collective agreements over the same interval to 28 percent. Further note that the use of opening clauses in establishments with opening clauses stood at 73.4 percent in 2011 (see the penultimate column of Appendix Table 1). Again recall our decision not to impute the use of opening clauses in the missing years.

[Figures 2 and 3 near here]

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 percent 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 percent. And, clearly, if collective agreements are distinguished by their absence, then the incidence of pacts falls even further to 10.9 percent. 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 percent 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 *Betriebsvereinbarung* or formal establishment agreement involving the works council and management as the main partners; a firm-level agreement between the trade union(s) and the employer (*Haustarifvertrag)*; a contract of employment (*Arbeitsvertrag*) agreed between individual employee and employer; a less formal verbal arrangement (*Mündliche Vereinbarung*); and, finally, a residual category (*Sonstige*).

Table 2 shows that the *Betriebsvereinbarung* category is the dominant type of pact, with an approximate 60 percent 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. The corresponding percentages, provided in the first two columns of the table, are 78.3 and 2.1 percent in the former case and 26.9 and 65.7 percent in the latter. 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; in the latter event, such individual contracts of employment dominate and represent some 40.8 percent of the total.

[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*.

VI. Findings

The Baseline RDD model

We begin the analysis by describing our selected treatment and control groups in Table 3. But note at the outset that the set of treated units is reduced from a total of 1,036 units in the 2006 raw sample to a maximum of 544. This reduction is due to the construction of our panel. Unfortunately, not all units observed in 2006 can be followed longitudinally, either because they have rotated out of the panel or because their status is missing or has changed after 2006. For its part, the control group contains a maximum of 144 units. Further, not all outcome variables are always observed; for example, while employment

can be observed in almost 100 percent of the cases, the labor productivity and real wage variables are far from being always observed.

[Table 3 near here]

Our expectation is that the underlying establishment characteristics are not systematically different across treatment and control groups – the balanced covariates hypothesis – and also that the base-year (i.e. 2006) outcomes for the two groups are not too far apart. Regarding the latter, there is a statistically significant difference in mean values in two cases (establishment-level real wages and employment) at the 0.01 level or better. The null is rejected at the 0.05 level in the case of labor productivity, while the null in the investment, innovation, and survival cases is more strongly rejected.[14] As far as the other observables are concerned we distinguish three subsets of variables: first, works council, sectoral agreement, and firm-level agreement; second, industry and establishment size; and third, all the other (18) variables. In the first subset, and unsurprisingly, the mean comparison two-tailed t-test confirms that establishments with worker representation, and covered by a firm agreement have a higher chance to have a pact, but not necessarily in the case of an establishment with a sectoral agreement; in the second, only in 1 out of 19 industries does the t-test rejects the null at the 0.01 level, while the mean in the case of the establishment size variables (a total of 6 dummies) is statistically different across the treatment and control groups in only 1 case, again at the 0.01 level or better. The null is rejected at the 0.05 level in two additional size categories. In 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). In the spirit of our preferred regression model (5), and although not all our priors are fully met, we will persist in our implementation using a restricted set of regressors (the most parsimonious possible) and only include as controls establishment size and industry affiliation. (A similar procedure is adopted by DiNardo and Lee, 2004, and Bradley, Kim, and Tian, 2015).

Table 4 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.

[Table 4 near here]

As shown in all (three) columns (1) of the table, the unadjusted mean difference is always positive and in 11 out of 15 cases (in the first five rows) the treatment effect is

statistically significant at conventional levels. The first major finding from this first set of run is, therefore, that there is no indication that pacts are detrimental to any of the selected outcomes; on the contrary, all the preliminary evidence does not exclude the likelihood of a positive impact on five out of six outcomes, possibly of sizeable magnitude. However, pacts do not have an impact on business survival 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 industry and establishment-size dummies (in column (2)), then 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)). By way of clarification, then, we have in column (3) the growth rate of the outcome variable Y^k as a function of pact status and, in column (4), as a function of pact status *and* the base-year outcome level. Given that we cannot de-mean the survival dummy, no results are given for columns (3) and (4) for survival. In the case of innovation, specification 3 gives the change in innovation status as the dependent variable, while in specification 4 the change in innovation status is also a function of the beginning-period (i.e. 2006) status.

Columns (2) though (4) of Table 4 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 effect, in 10 out of 45 cases the estimated coefficients are positive and statistically significant, while in the remaining (35) cases one cannot exclude a zero impact on performance. But, to repeat, in no case there is evidence of any negative impact. For completion, we note that introducing the industry and establishment size dummies separately – in column (2) the results are obtained after jointly adding the two sets of variables – we confirm that the treatment effect is indeed sensitive to the introduction of establishment size dummies, but not to industry affiliation. Since in Table 3 the null in the subset of industry dummies is rejected in only 2 out of 19 cases (1 at the 0.01 level or better), this result is highly predictable.

The full specification in column (4) of the table also allows us to identify three important regularities: first, a 2-year effect of about 8 percent on an establishment's average wage; second, a 3-year effect on average establishment productivity of approximately 20 percent; and, finally, a 1-year effect on innovation of some 11 percent. These are non-negligible results that do suggest that decentralization introduced by the

pact mechanism has somehow created a distinct combination of higher wages on the one hand, and higher innovation and higher worker productivity on the other.

[Table 5 near here]

The results in Table 5, obtained from using the *alternative* treatment group comprising all establishments with a pact in 2006 but not after 2006, confirm with one exception the regularities found in Table 4. Thus, the 2-year effect on establishment average wage of approximately 8 percent is confirmed, as well as a somewhat reduced impact on innovation of around 8 percent (rather than the 11 percent in Table 4). No causal effect on productivity was found in this case. Again, there was no indication of negative effects of pacts.

Robustness Checks

The specifications in Tables 4 and 5 test whether the results are robust to establishment-size and industry controls, on the one hand, and to the introduction of the corresponding base-year outcome, either as an explained or explanatory variable. Implicitly, it is assumed that either the covariates are sufficiently balanced or, above all, that the RDD assumption is satisfied. We now present the results of the robustness exercises described in section III in which we randomize participation of individuals (i.e. establishments) in the treatment and control groups. Given that the findings obtained in Tables 4 and 5 are qualitatively identical, we shall focus on the robustness of the results reported in Table 4.

Also in the interests of economy, we propose to illustrate our simulations using only specifications 3 and 4 – except in the case of survival, where we deploy specification 2 alone. Our results are given in Table 6. For each outcome indicator, we replicate in column (A) of the table the point estimate of the treatment effect first reported in column (4) of Table 4. Then, columns (B), (C), and (D) give the mean estimate of the same coefficient and its standard deviation (in parentheses) from 1,000 runs in *Simulation I* (Cases 1/3 and 2/3) and *Simulation II*, respectively. Recall that in *Simulation I* we fix the fraction of selected participants at 1/3 and 2/3, respectively, while in *Simulation II* we randomly allocate a sample probability and then run a weighted regression.

[Table 6 near here]

Our priors seem to be comfortably met. First of all, the mean obtained from the selected 1,000 iterations maintained with one exception the sign of the point estimate. Second of all, the standard deviation is comparatively smaller (in relation to the mean) when the point estimate is statistically significant. For example, the coefficient of

variation in the case of the innovation outcome in the fifth row of the table for the 1-year effect is 0.76, whereas it is always greater than 2 in the preceding four rows. Third of all, the mean tends to be closer to the point estimate when the null that the coefficient is zero is rejected. The results of the simulation exercise in Table 6 are therefore consonant with the findings from Table 4; in particular, it is confirmed that the simulation results are well centered around the prior point estimates. Moreover, this finding does not seem to hinge on a particular set of runs or simulation method, as it is apparent that both the mean(s) and standard deviation(s) are very stable across experiments.

[Table 7 near here]

This pattern of results is replicated in Table 7, which tests the sensitivity of the point estimates earlier reported in column (3) of Table 4. Note that although the results obtained in columns (3) and (4) of Table 4 are not qualitatively different, for completeness we decided to test separately the sensitivity of the findings based on specification 3. Thus, by way of a summary: (a) the simulation mean maintains the sign of the point estimate (with one exception); (b) whenever the point estimate is statistically significant, the standard deviation is relatively small; and (c) the dispersion of the mean across simulations is relatively small. Overall, there is again no material evidence to indicate that pacts impact any of the selected outcomes negatively.

[Figure 4 near here]

We provide further evidence in favor of these findings in Figure 4. In presenting the simulation histograms, the main aim is to assess the extent to which the simulation results are centered around the point estimate. For expositional convenience, we select three outcomes where statistically significant treatment effects are obtained, namely the 2-year effect of pacts on wages, the 3-year effect on productivity, and the 1-year effect on innovation. These three cases are illustrated in panels (a) through (c) of the figure, where we present histograms of the entire distribution. To improve readability, in all histograms we insert a 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.

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. Finally, the simulation results are well centered around the point estimate. The histograms produced for all other cells in Tables 6 and 7 follow the same pattern and are available

upon request. Also, 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 share in the (0, 1) interval.

VII. Conclusions

Pacts for employment and competitiveness are just one aspect of an unprecedented decentralization of collective bargaining from sectoral level to shop floor level. This decentralization is widely held to be the key to the resurgence of the German economy, a position most forcefully argued by Dustmann et al. (2014). Decentralization in the limit embraces a growth in *individual* bargaining between firms and their workers, and this change has been tied to more moderate wage developments. Decentralization that falls short of this, with local negotiations still largely being conducted within the framework of sectoral bargaining – often but not uncontroversially described as *organized* decentralization – has not generally been credited with more moderate wage agreements (Ochel, 2005), even if greater wage flexibility and wage dispersion have unambiguously accompanied this development.

Dustmann et al. (2014) argue that wage restraint and decreasing real wages, and with them falling unemployment, can be allied not only to a sharp decline in the share of workers covered by collective agreements (i.e. *deunionization*) but also to the increase in opening clauses that have strengthened the role of firm-based works councils in wage determination vis-à-vis to unions. The latter presumption still awaits formal testing at the local level.

We have not examined opening clauses in the present treatment other than tangentially most obviously because our test procedure has a basis in a comparison of negotiations that (can be assumed to have) narrowly failed or succeeded, and in the case of opening clauses we lack any information on negotiations other than at sectoral level. 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 our outcome indicators in the baseline model supported this presumption; three of which, namely *higher* wages, enhanced productivity, and improved innovation survived our robustness checks. Given the likely diversity in firm behavior and the short time period examined we consider these to be strong results.

Pacts for employment and competitiveness are likely to be of increasing importance in the future and their performance should be more closely monitored than hitherto across a full range of outcomes and, importantly, pairs of outcomes. Key issues here are likely to be the association between employment and investment (because of the suggestion in very recent research of a substitutive relationship between employment and the real capital stock) and the factors contributing to effectiveness or otherwise of pacts (including type of collective bargaining coverage). A different although perennial research topic is whether sectoral bargaining can survive what often amount to extensive opt-outs of this type and the consequences for macro performance of an implied more fundamental shift in the collective bargaining infrastructure.

Endnotes

1. The international experience with *pacts for employment and competitiveness* is charted in Sisson et al. (1999), Sisson and Artiles (2000), and Freyssinet and Seifert (2001). Note that this particular phrase is used to distinguish such local contractual innovations from *social pacts*, namely tripartite agreements between employers' organizations, trade unions, and government covering developments in wages, social security, and public expenditure (see, for example, Hassel, 2009).

2. On the British experience, see Bacon and Samuel (2009).

3. While recognizing that pacts may encourage regime competition and ultimately lead to what might be described as the *Americanization* of industrial relations in Europe, pacts were not designed to undermine the position of employee representatives as some would allege to be the case with concession bargaining in the United States.

4. See also Zagelmeyer (2000, Chapter 8; 2010) for individual company case studies.

5. Namely, reduction of working hours, extension of working and operating hours, monetary concessions, working time flexibility, organizational flexibility, and socially acceptable redundancies.

6. Hübler also examined the impact of different pact components, reporting that the promotion of further training and working time extensions benefited employment while work reorganization, wage cuts, and working time reductions had a contrary effect. For its part, the introduction of working time accounts left employment unaffected.

7. However, if we are to depict this study period as constituting *normal* times, a subsequent 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. The qualification that needs to be entered here is that during the period of the Great Recession employment barely fell and unemployment barely rose. Burda and Hunt (2011) have argued that this outcome in large part reflected a low rate of hiring in the preceding expansion.

8. 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).

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

10. Changes in industrial classification (in 2009) were accommodated using the methodology described in Addison et al. (2015).

11. Clearly, any panel covering the 2009-2013 interval implies a considerable 'extension' of the imputation procedure as the question on pacts is missing from the 2010 though 2012 waves of the dataset.

12. The above discussion is necessarily a summary description; the code for the entire procedure is available from the authors upon request.

13. 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.

14. Observe, however, that in order to reduce the presence of any potential bias our regression model does include the base-year outcome in the set of regressors; see columns (4) of Table 4, below.

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FIGURE 1
Incidence of Pacts and Opening Clauses and their Relationship (in percent)

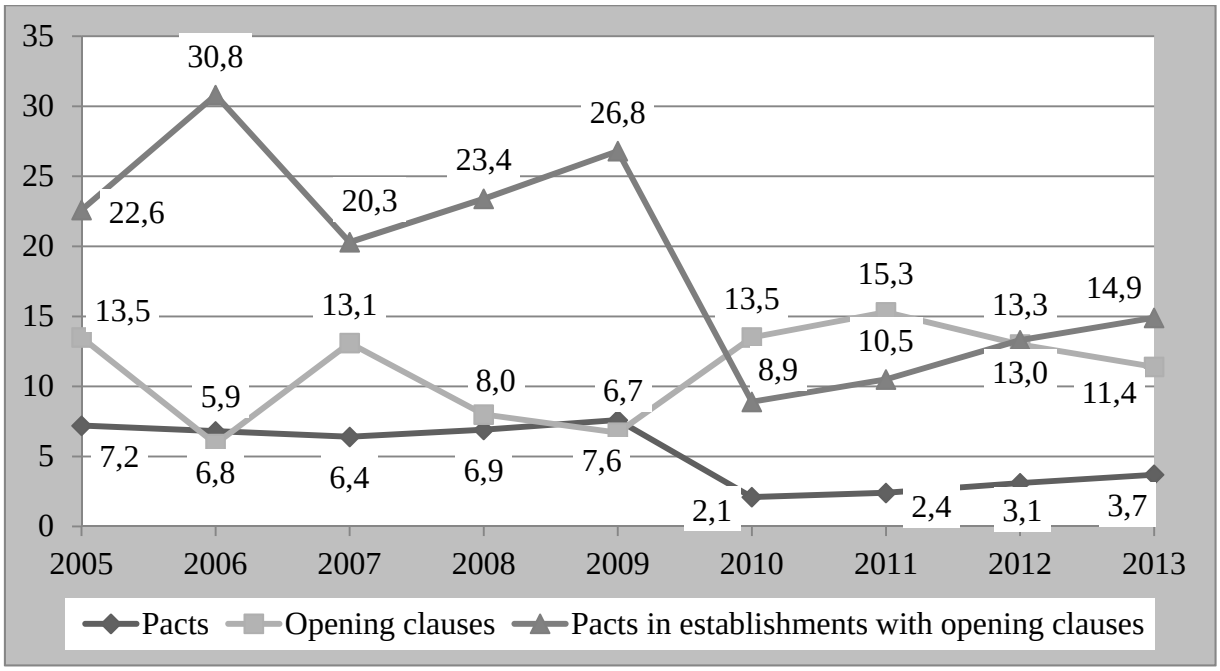


FIGURE 2
Pacts and Industrial Relations (in percent)

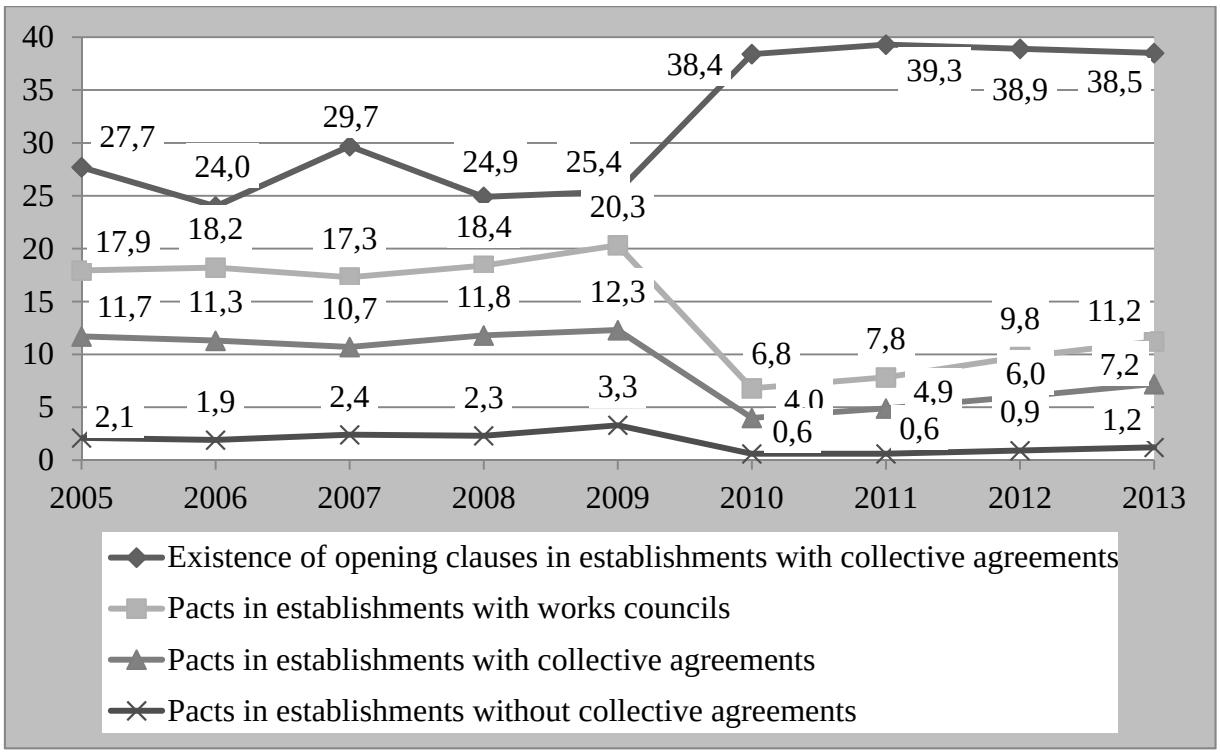


FIGURE 3
Pacts and the Use of Opening Clauses (in percent)

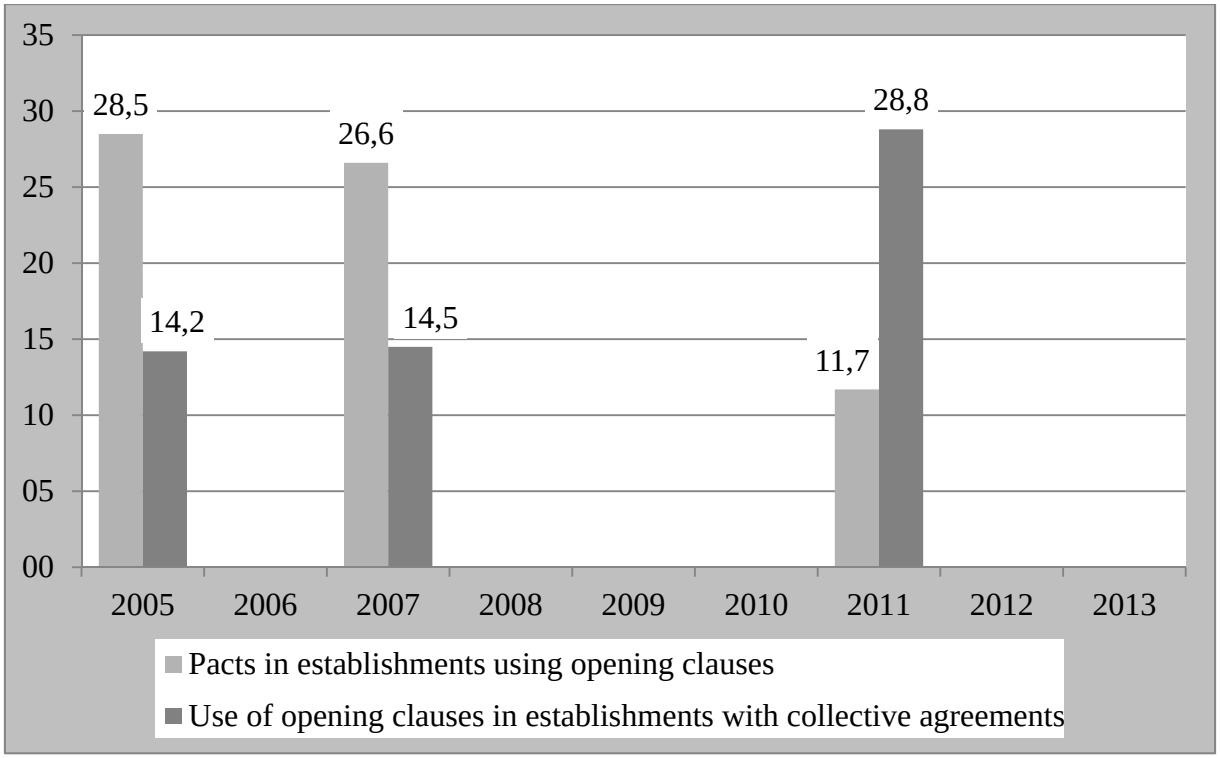
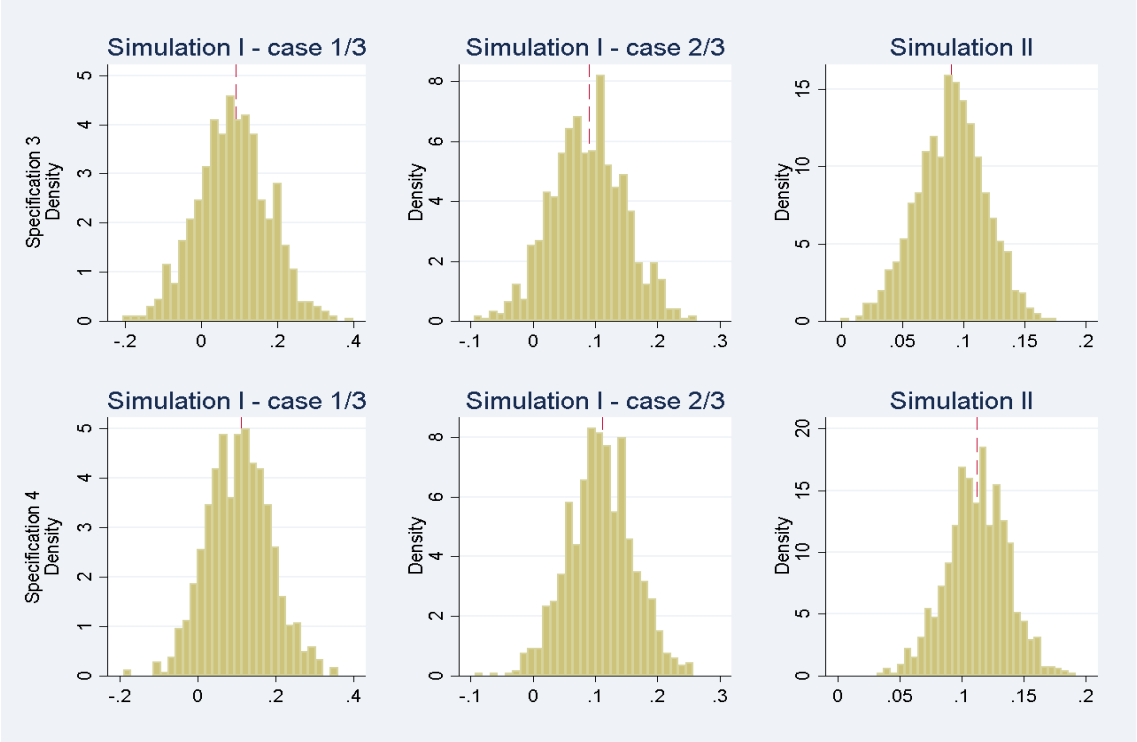
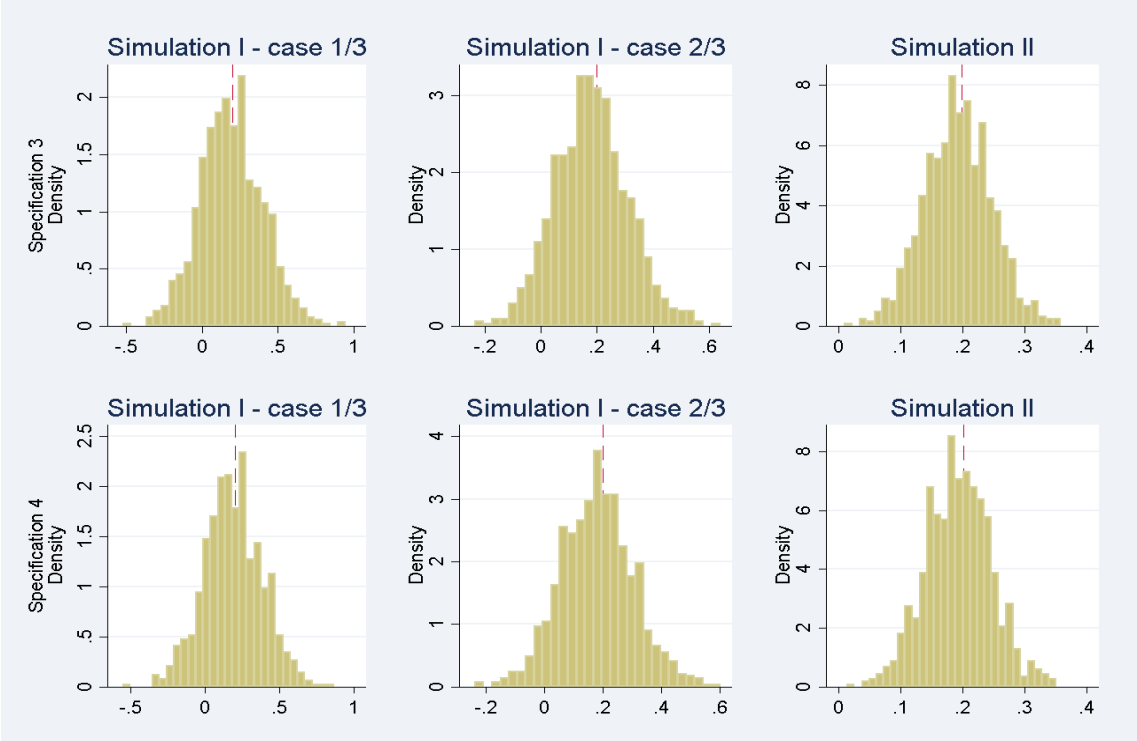


FIGURE 4
Simulation Histograms in Selected Outcomes

(a) 2-Year Effects on Wages



(b) 3-Year Effect on Productivity



(c) 1-Year Effects on Innovation

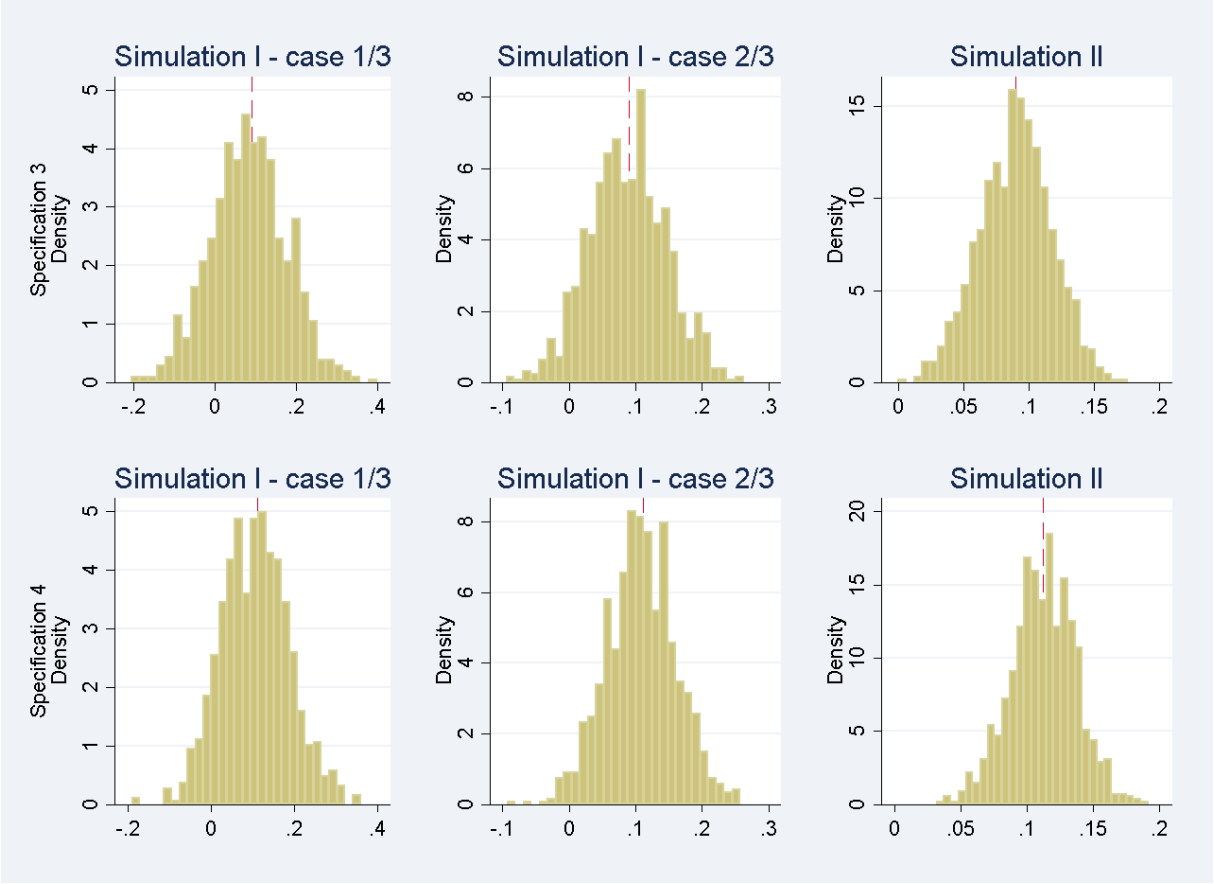


TABLE 1
Sample Probability of Pacts, given Works Council and Collective Bargaining Status (in percent)

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 percent of establishments with a works council and a firm-level agreement have a pact.

TABLE 2
Sample Probability of Type of Pact by Works Council and Collective Agreement Status (in percent)

	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 percent gives $\Pr(B|woco \cap fcb)$. The corresponding row total (i.e. 57.7 percent) gives $\Pr(B)$. The English translation of the pact type is given in the text.

TABLE 3
Mean Difference in Establishment-Level Observables in the Base-Year (i.e. 2006) between Selected Treatment and Control Groups, Two-Tailed *t*-Test

Variables	Treatment		Control		Mean difference	p-value
	Mean	s.d.	Mean	s.d.		
Outcomes						
Number of full-time employees (FTE)	5.48	1.63	4.93	1.79	0.55	0.0005
Real wage per FTE	7.95	0.33	7.82	0.43	0.13	0.0002
Total investment per FTE	8.43	1.61	8.24	1.45	0.19	0.2724
Labor productivity	15.88	0.74	15.68	0.90	0.20	0.0333
Innovation	0.76	0.43	0.74	0.44	0.02	0.6075
Business survival	0.96	0.21	0.96	0.20	-0.002	0.9106
Works council	0.92	0.28	0.81	0.39	0.11	0.0002
Sectoral agreement	0.63	0.48	0.62	0.49	0.01	0.9177
Firm-level agreement	0.25	0.43	0.11	0.32	0.14	0.0004
Industry						
Agriculture, hunting and forestry, fishing	0.00	0.00	0.01	0.08	-0.007	0.0511
Mining, electricity, energy, and water	0.04	0.20	0.04	0.20	-0.002	0.9351
Manufacture of food products	0.02	0.13	0.03	0.18	-0.017	0.2279
Manufacture of commodities	0.04	0.20	0.04	0.20	-0.002	0.9351
Manufacture of durables	0.14	0.35	0.15	0.36	-0.003	0.9164
Manufacture of investment- and consumer goods	0.27	0.44	0.24	0.43	0.031	0.4594
Construction	0.03	0.16	0.03	0.18	-0.007	0.6405
Wholesale and sales, maintenance, repair of motor vehicles	0.04	0.20	0.06	0.23	-0.016	0.4203
Retail trade	0.03	0.17	0.03	0.17	0.003	0.8397
Transport and warehousing	0.06	0.24	0.01	0.08	0.056	0.0072
Information and communication	0.01	0.11	0.02	0.14	-0.008	0.4718
Hotel business and gastronomy	0.00	0.06	0.01	0.08	-0.003	0.5931
Financial and insurance services	0.05	0.21	0.04	0.20	0.006	0.7686
Industrial services	0.06	0.23	0.03	0.17	0.027	0.1830
Education	0.02	0.15	0.03	0.17	-0.004	0.7805
Human health	0.07	0.26	0.10	0.30	-0.026	0.2966
Other services	0.02	0.13	0.02	0.14	-0.003	0.8396
Activities of membership	0.01	0.07	0.01	0.12	-0.008	0.2896
Public administration	0.09	0.28	0.10	0.31	-0.017	0.5397
Establishment size						
1 to 9 employees	0.02	0.13	0.08	0.27	-0.06	0.0003
10 to 19 employees	0.03	0.18	0.07	0.26	-0.04	0.0641

20 to 49 employees	0.10	0.31	0.04	0.20	0.06	0.0205
50 to 249 employees	0.31	0.46	0.38	0.49	-0.07	0.1284
250 to 999 employees	0.36	0.48	0.34	0.47	0.02	0.5846
At least 1000 employees	0.17	0.38	0.10	0.30	0.07	0.0321
Individually owned	0.01	0.11	0.06	0.25	-0.05	0.0001
Further training	0.96	0.20	0.89	0.31	0.07	0.0034
Expected business volume development:						
Unchanged	0.50	0.50	0.38	0.49	0.12	0.0141
Increase	0.29	0.45	0.31	0.46	-0.02	0.6211
Decrease	0.15	0.35	0.25	0.44	-0.10	0.0027
Uncertain	0.07	0.25	0.06	0.23	0.01	0.6218
State of the technical equipment:						
State-of-the-art	0.20	0.40	0.12	0.32	0.08	0.0345
Rather new	0.49	0.50	0.51	0.50	-0.02	0.7037
Medium or worse	0.31	0.46	0.37	0.48	-0.06	0.1857
Share of part-time workers	0.15	0.20	0.15	0.20	-0.00016	0.9934
Share of fixed-term contract workers	0.05	0.08	0.07	0.14	-0.0188	0.0359
Share of high-skilled workers	0.14	0.18	0.12	0.14	0.0213	0.1978
Single-establishment firm	0.41	0.49	0.53	0.50	-0.12	0.0075
Domestic ownership	0.67	0.47	0.67	0.47	0.00	0.9316
Foreign ownership	0.12	0.33	0.17	0.37	-0.05	0.1494
Other ownership	0.21	0.41	0.17	0.37	0.04	0.2670
Western Germany	0.67	0.47	0.68	0.47	-0.01	0.9003
Share of exports	0.20	0.28	0.21	0.29	-0.001	0.9659

Notes: The treatment and control groups are defined in section IV of the text. They comprise a maximum of 544 and 144 establishments, respectively. No characteristic is missing in more than 5 percent of the cases, except *Further training* and *Share of exports*, which are missing in 8 and 29 percent of the cases, respectively. The outcome variables *Real wage per FTE* and *Total investment per FTE* are missing in approximately 14 and 23 percent of the cases, while *Labor productivity* is missing in 40 percent of the cases. The results in the penultimate column are derived from a mean comparison (two-tailed) *t*-test across groups. Thus, for a given variable x_k, the null hypothesis is given by *difference* = mean (x_k^T) – mean (x_k^C) = 0, against the alternative *difference* $\neq$ 0, where the superscripts *T* and *C* denote treatment and control groups, respectively. All outcome variables are in logs except the *Innovation* and *Business survival* dummies.

TABLE 4
1-, 2-, and 3-Year Treatment Effects of Company-Level Pacts on Selected Outcomes

	1-year effect				2-year effect				3-year effect			
Outcome	(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	+0.179) *** (0.047)	+0.130*** (0.045)	+0.090* (0.048)	+0.112*** (0.041)	+0.117** (0.053)	+0.047 (0.052)	+0.038 (0.059)	+0.041 (0.049)	+0.101* (0.059)	+0.053 (0.060)	+0.025 (0.069)	+0.044 (0.059)
Y6-survival									-0.002 (0.019)	+0.001 (0.020)		
Base-year	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
'De-meaned'	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Size dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Industry dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes

Notes: The estimation sample comprises an unbalanced panel in which establishments are observed over the 2006-2009 window. Headings (1) through (4) denote specifications 1 through 4, respectively. For the 1-year effect, in column (1), the reported coefficients are obtained from running OLS on model (5) in the text, that is, $Y_{i,2007}^{k} = a + bP_i + e_i$, where $k = 1, \ldots, 6$ denotes the corresponding outcome variable. The model in column (2) adds establishment size and industry dummies (i.e. 6 and 19 dummies, respectively) to the right-hand-side of the equation. In columns (3) and (4) the dependent variable is given by $(Y_{i,2007}^{k} - Y_{i,2006}^{k})$. Finally, in column (4), the set of right-hand-side variables include the $Y_{i,2006}^{k}$ term. The specifications for the 2- and 3-year effects are similar, with $Y_{i,2007}^{k}$ being replaced by $Y_{i,2008}^{k}$ and $Y_{i,2009}^{k}$, respectively. The dependent variables are in logs, except for innovation and business survival, which are 1/0 dummies. The treatment and control groups are defined in section IV of the text. Standard errors are in parentheses. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.1 levels, respectively.

TABLE 5
1-, 2-, and 3-Year Treatment Effects of Company-Level Pacts on Selected Outcomes for the Alternative Treatment Group

Outcome	1-year effect				2-year effect				3-year effect			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Y1-wage	+0.115*** (0.039)	+0.098 (0.035)	+0.030 (0.024)	+0.044* (0.023)	+0.134*** (0.042)	+0.130*** (0.038)	+0.061** (0.029)	+0.081*** (0.027)	+0.089* (0.048)	+0.063 (0.044)	+0.012 (0.034)	+0.030 (0.031)
Y2-employment	+0.199 (0.149)	+0.064 (0.048)	+0.006 (0.018)	+0.007 (0.018)	+0.176 (0.163)	+0.076 (0.055)	+0.005 (0.026)	+0.007 (0.026)	+0.240 (0.186)	-0.005 (0.065)	-0.050 (0.035)	-0.048 (0.034)
Y3-investment	+0.224 (0.187)	+0.224 (0.176)	-0.032 (0.152)	+0.028 (0.140)	+0.193 (0.224)	+0.149 (0.218)	-0.231 (0.215)	-0.154 (0.200)	+0.162 (0.251)	+0.112 (0.238)	-0.156 (0.231)	-0.140 (0.206)
Y4-productivity	+0.155 (0.101)	+0.145 (0.094)	+0.024 (0.060)	+0.039 (0.058)	+0.079 (0.123)	+0.080 (0.118)	+0.092 (0.090)	+0.095 (0.088)	+0.101 (0.128)	+0.073 (0.122)	+0.107 (0.102)	+0.122 (0.097)
Y5-innovation	+0.097** (0.047)	+0.089** (0.044)	+0.062 (0.047)	+0.078* (0.040)	+0.044 (0.052)	+0.013 (0.048)	+0.004 (0.056)	+0.005 (0.047)	+0.009 (0.060)	+0.002 (0.056)	+0.002 (0.066)	+0.001 (0.056)
Y6-survival									+0.009 (0.016)	+0.007 (0.016)		
Base-year	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
'De-meaned'	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Size dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Industry dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes

Notes: See Notes to Table 4. The alternative treatment group is defined in section IV of the text.

TABLE 6
Simulating the Impact of Pacts on the Different Outcome Indicators Results, Specification 4 (Specification 2 for Surviving)

Outcome	1-year effect				2-year effect				3-year effect			
	Point Estimate (from Table 4)	*Simulation I*		*Simulation II*	Point Estimate (from Table 4)	*Simulation I*		*Simulation II*	Point Estimate (from Table 4)	*Simulation I*		*Simulation II*
		Case 1/3	Case 2/3			Case 1/3	Case 2/3			Case 1/3	Case 2/3	
		Mean (s.d.)	Mean (s.d.)	Mean (s.d.)		Mean (s.d.)	Mean (s.d.)	Mean (s.d.)		Mean (s.d.)	Mean (s.d.)	Mean (s.d.)
	(A)	*(B)*	*(C)*	*(D)*	*(A)*	*(B)*	*(C)*	*(D)*	*(A)*	*(B)*	*(C)*	*(D)*
Y1-wage	+0.032	+0.023 (0.052)	+0.028 (0.034)	+0.032 (0.035)	+0.076***	+0.070 (0.063)	+0.072 (0.040)	+0.075 (0.042)	+0.017	+0.011 (0.072)	+0.013 (0.044)	+0.015 (0.050)
Y2-employment	+0.010	+0.006 (0.034)	+0.008 (0.023)	+0.008 (0.028)	+0.025	+0.019 (0.053)	+0.024 (0.033)	+0.026 (0.038)	-0.050	-0.047 (0.068)	-0.050 (0.042)	-0.050 (0.049)
Y3-investment	+0.025	0.014 (0.259)	+0.019 (0.164)	+0.021 (0.228)	-0.179	-0.191 (0.370)	-0.198 (0.233)	-0.181 (0.324)	-0.101	-0.126 (0.409)	-0.113 (0.241)	-0.102 (0.323)
Y4-productivity	+0.049	+0.047 (0.109)	+0.042 (0.067)	+0.047 (0.083)	+0.053	+0.051 (0.218)	+0.042 (0.129)	+0.052 (0.164)	+0.201**	+0.192 (0.195)	+0.182 (0.124)	+0.194 (0.145)
Y5-innovation	+0.112***	+0.107 (0.081)	+0.109 (0.052)	+0.113 (0.059)	+0.041	+0.047 (0.104)	+0.044 (0.064)	+0.041 (0.073)	+0.044	+0.039 (0.126)	+0.039 (0.079)	+0.043 (0.086)
Y6-survival									+0.001	+0.002 (0.037)	+0.001 (0.024)	+0.002 (0.028)

Notes: Columns *(A)* repeat the point estimates of the pact coefficients reported in Table 4. Columns *(B), (C),* and *(D)* give the mean estimate of the same coefficient and its standard deviation in parentheses from 1,000 runs in *Simulation I* (Cases 1/3 and 2/3) and *Simulation II*, respectively. The simulation exercises are described in section III. In the simulations, specification 4 is used for Y1-Y5; for the survival outcome specification 2 is used (for descriptions of the specifications, see the Notes to Table 4).

TABLE 7
Simulating the Impact of Pacts on the Different Outcome Indicators Results, Specification 3

Outcome	1-year effect				2-year effect				3-year effect			
	Point Estimate (from Table 4)	*Simulation I*		*Simulation II*	Point Estimate (from Table 4)	*Simulation I*		*Simulation II*	Point Estimate (from Table 4)	*Simulation I*		*Simulation II*
		Case 1/3	Case 2/3			Case 1/3	Case 2/3			Case 1/3	Case 2/3	
		Mean (s.d.)	Mean (s.d.)	Mean (s.d.)		Mean (s.d.)	Mean (s.d.)	Mean (s.d.)		Mean (s.d.)	Mean (s.d.)	Mean (s.d.)
	(A)	*(B)*	*(C)*	*(D)*	*(A)*	*(B)*	*(C)*	*(D)*	*(A)*	*(B)*	*(C)*	*(D)*
Y1-wage	+0.021	+0.009 (0.055)	+0.017 (0.036)	+0.020 (0.035)	+0.065**	+0.059 (0.063)	+0.061 (0.040)	+0.064 (0.043)	+0.044	-0.001 (0.010)	+0.001 (0.044)	+0.003 (0.052)
Y2-employment	+0.008	+0.004 (0.033)	+0.007 (0.023)	+0.007 (0.028)	+0.021	+0.016 (0.053)	+0.020 (0.033)	+0.022 (0.028)	-0.053	-0.049 (0.068)	-0.053 (0.043)	-0.053 (0.049)
Y3-investment	-0.013	-0.006 (0.277)	-0.011 (0.173)	-0.014 (0.250)	-0.187	-0.165 (0.379)	-0.184 (0.240)	-0.185 (0.344)	-0.043	-0.028 (0.433)	-0.033 (0.258)	-0.039 (0.356)
Y4-productivity	+0.042	+0.041 (0.110)	+0.038 (0.067)	+0.040 (0.083)	+0.056	+0.044 (0.218)	+0.043 (0.122)	+0.040 (0.164)	+0.199**	+0.197 (0.206)	+0.182 (0.131)	+0.193 (0.145)
Y5-innovation	+0.090*	+0.085 (0.093)	+0.089 (0.059)	+0.091 (0.069)	+0.038	+0.036 (0.119)	+0.040 (0.078)	+0.037 (0.086)	+0.025	+0.010 (0.157)	+0.016 (0.096)	0.024 (0.101)

Notes: See Notes to Table 6. Observe, however that specification 3 is used throughout in the present case.

APPENDIX TABLE 1
The Presence of Company-Level Pacts and Opening Clauses (in percent)

Year	Pacts	Opening clauses	Pacts in establishments with opening clauses	Opening clauses in establishments bound by a collective agreement	Pacts in establishments bound by a collective agreement	Pacts in establishments without collective agreements	Pacts in establishments with works council	Pacts in establishments using of opening clauses	Use of opening clauses in establishments bound by collective agreements	Use of opening clauses in establishments with opening clauses	Collective agreements
2005	7.2	13.5	22.6	27.7	11.7	2.1	17.9	28.5	14.2	51.4	52.9
2006	6.8	5.9	30.8	24.0	11.3	1.9	18.2				51.4
2007	6.4	13.1	20.3	29.7	10.7	2.4	17.3	26.6	14.5	48.8	48.9
2008	6.9	8.0	23.4	24.9	11.8	2.3	18.4				48.4
2009	7.6	6.7	26.8	25.4	12.3	3.3	20.3				48.0
2010	2.1	13.5	8.9	38.3	4.0	0.6	6.8				43.9
2011	2.4	15.3	10.5	39.3	4.9	0.6	7.8	11.7	28.8	73.4	42.8
2012	3.1	13.0	13.3	38.9	6.0	0.9	9.8				43.2
2013	3.7	11.4	14.9	38.5	7.2	1.2	11.2				42.2

Notes: The reported sample probabilities are based on the annual IAB Establishment Panel (*IAB-Betriebspanel*), 2005-2013, unweighted data. Questions on pact status are asked in the 2006, 2008, 2009, and 2013 waves of the IAB Establishment Panel; the questions on opening clauses in the 2005, 2007, and 2011 waves. The imputation procedures are described in the text.

APPENDIX TABLE 2
Sample Industries and their (2-digit) Components

Sample industry (19 sectors)	2-digit-industry (43 sectors)
Agriculture, hunting and forestry, fishing	Agriculture, hunting and forestry, fishing
Mining, electricity, energy, and water	Mining and quarrying Energy and water supply, water and scrap disposal, recycling
Manufacture of food products	Manufacture of food products
Manufacture of commodities	Manufacture of textiles and clothing, tanning and dressing of leather Manufacture of wood products, paper, print products
Manufacture of durables	Manufacture of chemicals, coke, refined petroleum products and nuclear fuel Manufacture of rubber and plastic products Manufacture of other non-metallic mineral products Manufacture of basic metals
Manufacture of investment and consumer goods	Manufacture of fabricated metal products (not including machinery and equipment) and structural metal products Manufacture of electrical equipment, office machinery and computers Manufacture of precision and optical equipment Manufacture of machinery and equipment Manufacture of motor vehicles, trailers and semi-trailers Manufacture of furniture, jewelry, musical instruments, sports goods, games and toys and other products Repair of machinery installation equipment
Construction	Building construction and civil engineering Building installation and building completion
Wholesale and sales, maintenance, repair of motor vehicles	Sales, maintenance and repair of motor vehicles Wholesale and commission trade
Retail trade	Retail trade, petrol stations
Transport and warehousing	Transport and warehousing also car parks, railway stations, additional carriage, postal-, courier-, express mail service
Information and communication	Publishing, film production, rental, distribution, broadcasting service, telecommunication; information technological services and information services
Hotel business and gastronomy	Hotel business and gastronomy
Financial and insurance services	Financial and insurance services
Industrial services	Real estate activities Legal and tax advice, accounting Administration, leadership of establishments, consulting Architecture and engineering offices, technical, physical, chemical support Research and development Marketing and market research, design, photography, translation Veterinary industry Renting and business activities Placement and temporary provision of labor Hawking, security agencies, landscaping, other economic services
Education	Education
Human health	Human health and social work
Other services	Recreational, cultural and sporting activities Repair of computers and consumer goods Other services (e.g. laundry/hairdressing)
Activities of membership	Activities of membership, religious and other organizations
Public administration	Public administration and defense, social security