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## The One Constant: A Causal Effect of Collective Bargaining on Employment Growth?

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# The One Constant: A Causal Effect of Collective Bargaining on Employment Growth?

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## ABSTRACT

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# The One Constant: A Causal Effect of Collective Bargaining on Employment Growth?\*

A large number of articles have analysed ‘the one constant’ in the economic effects of trade unions, namely that collective bargaining reduces employment growth by two to four percentage points per year. Evidence is, however, mostly related to Anglo-Saxon countries. We investigate whether a different institutional setting might lead to a different outcome, making the constant a variable entity. Using linked-employer-employee data for Germany, we find a negative correlation between being covered by a sector-wide bargaining agreement or firm-level contract and employment growth of about one percentage point per annum. However, the correlation between employment growth and collective bargaining is not robust to the use of panel methods. We conclude that the results of the literature using cross-section data might be driven by selection.

**JEL Classification:** J23 J52, J53, J63

**Keywords:** collective bargaining, employment growth, job flows, trade unions

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# 1 Introduction

What are the economic effects of trade unions on employment, i.e. does collective bargaining reduce employment? The neoclassical theory suggests that if wages equal marginal productivity and trade unions raise wages, firms will choose a lower quantity of labour, moving up along the (inverse) labour demanded schedule (Hammermesh 1993). However, if firms and trade unions bargain not only over wages, but also over employment, collective negotiations might increase employment beyond the level chosen by the firm if it determined employment unilaterally at the level at which its marginal revenue product equalled the wage (McDonald and Solow 1981). Furthermore, trade unions could increase labour productivity and boost employment in unionised plants by raising the quality of job matches or reducing turnover, such that the incentives to invest in firm-specific human capital may be larger.

Despite this theoretical ambiguity, empirical analyses at first sight provide a clear-cut picture and have uncovered what Addison and Belfield (2004) refer to as *the one constant* among the economic effects of trade unions: Unionism reduces employment growth by two to four percent per annum. This interpretation is based on data primarily from Anglo-Saxon countries which tend to be characterised by a pluralistic system of industrial relations and low collective bargaining coverage in the private sector.<sup>1</sup> Moreover, collective bargaining primarily takes place at the plant level.

Therefore, in this paper we inquire whether the negative employment effects observed for Anglo-Saxon countries are also present in a more cooperative and corporatist industrial relations system such as Germany, the largest economy in the European Union and fourth-largest in the world. It represents an interesting case for a number of reasons: First, while Germany has been called the *sick man of Europe* only about a decade and a half ago, its economy has been remarkably stable during the Great Recession. Nowadays, it is sometimes viewed as role model, outperforming its European competitors, especially with regard to the labour market (Dustmann et al. 2014). Second, collective bargaining coverage is still relatively high in Germany: sector-wide bargaining covers more than half of the workforce. If wages in a large part of the economy are resulting from collective bargaining, it will be easier to shift forward cost increases into prices, such that the employment effects of collective bargaining may be less pronounced than, for example, in Anglo-Saxon countries characterised by a lower level of coverage. The strong export-orientation of many German firms will further affect the employment consequences of collective bargaining. Exporting firms often have higher productivity levels than non-exporters. Therefore, detrimental employment effects may be less pronounced. However, if international competition

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<sup>1</sup>Australia is somewhat of an exception in the latter regard with a collective bargaining coverage of about 60 % (Visser 2016).

is more intense than domestic competition, exporting firms cannot shift forward higher wages into prices as easily as their non-exporting counterparts. This suggests that the employment effects of collective bargaining are aggravated for exporting plants. Third, there are also negotiations at the firm level for about 10% of employees. Accordingly, we can distinguish the employment effects of more and less centralised collective negotiations. A fourth reason why we look at Germany is that its labour market and institutional settings have undergone a number of changes recently. Following unification, unemployment was rather high at the dawn of the century and collective bargaining coverage has declined by about 15 percentage points over the last decades. Furthermore, a range of labour market reforms (Hartz IV laws etc.) have been enacted in the last 15 years. Fifth, we can utilise high quality linked-employer-employee panel data, namely the widely used LIAB data set from the Institute for Employment Research (IAB). Accordingly, we can look at various subgroups, analyse the interaction of particular features of the German labour market, such as works councils, with collective bargaining and scrutinise whether the relationship between collective bargaining and employment growth can be interpreted causally. Additionally, we can observe plants annually for a period of almost fifteen years, while previous contributions either rely on cross-sectional data or on substantially fewer observations per plant over time, which encompass larger time spans. Finally, if *the one constant* were to exist for Germany, its overall impact would be substantially larger than in economies exhibiting lower coverage.

Correspondingly, we contribute to the literature, first, by providing evidence for a major country, for which no such findings are available yet. Second, we go beyond most contributions by looking at the question of whether observed effects can be interpreted causally from a variety of vantage points. Finally, we distinguish different degrees of bargaining centralisation.

Our baseline findings indicate that collective bargaining is associated with a reduction in employment growth in German plants by one percentage point per annum, less than in those countries for which *the one constant* has yet been detected. Importantly, we observe no large differences between negotiations at the plant or at the sector level. When using the time dimension of the data, fixed-effects and difference-in-differences estimates provide no indication that employment growth is affected by the introduction or the abolition of collective bargaining. More elaborate estimation techniques, which account for endogeneity, fail to deliver valid results: Dynamic panel estimations and instrumental variables techniques provide no basis for a causal interpretation due to the rejection of the respective state-of-the-art test procedures. Therefore, our results suggest that the employment effects associated with collective bargaining in Germany are due to self-selection into bargaining regimes.

After having presented our motivation, we introduce the relevant literature in Section 2 and use Section 3 to characterise the institutional setting. Section 4 provides an overview of the data, presents descriptive evidence, and outlines the empirical methods we employ. The basic results as well as findings from various robustness checks are presented in Section 5. Section 6 concludes and puts our findings into perspective.

## 2 Related Literature

The *union employment literature* has been established on the basis of cross-sectional evidence, mainly for Great Britain or the United Kingdom. These studies generally employ data from Workplace Industrial Relations Surveys (WIRS) or Workplace Employee Relations Survey (WERS). Since the early 2000s, contributions tend to rely on repeated cross-sectional or panel data. Irrespective of the data utilised, a common and widely documented finding is that employment in unionised plants grows between 2 % to 4 % less per annum than in non-unionised plants.<sup>2</sup> A few analyses can establish such an effect for selected plants only (Machin and Wadhvani 1991), or cannot discern it at all (Blanchflower and Burgess 1996, Bryson and Dale-Olsen 2008). The result that employment is lower in British and UK plants covered by collective bargaining is mirrored by evidence from the United States (Leonard 1992, Bronars et al. 1994), Canada (Long 1993, Walsworth 2010, Walsworth and Long 2013),<sup>3</sup> and to some extent Australia (Wooden and Hawke 2000, Blanchflower and Burgess 1998).

The more recent contributions have increasingly used panel data. Furthermore, there are a few notable analyses from a methodological point of view. In particular, they employ a regression discontinuity design which utilises the fact that legal recognition of a trade union according to the United States National Labor Relations Act requires an election among the workforce. Specifically, these analyses focus on certification elections which were narrowly won or lost. While DiNardo and Lee (2004) find no impact of unionisation on hours of work in a large sample of establishments, the results for nursing homes by Sojourner et al. (2015) are in sharp contrast. Their estimates indicate that hours of work per resident (as a proxy for employment) decline dramatically because of union certification. As indicated by the authors, these findings may under- or overestimate the true impact of unionisation for at least two reasons: First, certification makes it more likely that collective negotiations are initiated afterwards, but is not equivalent to a subsequent unionisation. Second, even if a plant becomes unionised, the effects measured around the

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<sup>2</sup>See, inter alia, Blanchflower et al. (1991), Blanchflower and Burgess (1996), Booth and McCulloch (1999), Addison et al. (2000), Bryson (2004), Addison and Belfield (2004) and Bryson and Nurmi (2011).

<sup>3</sup>Walsworth and Long (2013) update the findings by Walsworth (2010) and observe negative effects of unionisation for large manufacturing plants and even positive effects for small service sector plants.

date of certification capture relatively short-run effects. While the contributions by DiNardo and Lee (2004) and Sojourner et al. (2015) have substantially advanced the analysis of causal effects of unionisation, their approach cannot be applied to Germany. Here, there are no certification elections or comparable events which randomly determine whether collective bargaining is established or not.

Going beyond Anglo-Saxon countries, Bryson and Dale-Olsen (2008) analyse Norwegian linked-employer-employee data. Employment growth is about 3-5 % lower in plants in which a union is recognised for the purpose of collective bargaining, compared to non-unionised plants, when correcting for survival bias. However, estimating a dynamic panel-data model and controlling for worker sorting, the study finds a positive effect of union density on both short-term and long-term employment.

For Germany, direct evidence on the employment effects of collective bargaining is scarce. Empirical work has focused on plant-level co-determination and occasionally included a dummy variable indicating the existence of collective bargaining as a control variable. In particular, Addison and Teixeira (2006) find that collective bargaining has an insignificant or positive impact on employment growth. Jirjahn (2010) presents coefficients not significantly different from zero using OLS, and negative and marginally significant coefficients in a treatment effects model that controls for the endogeneity of works councils. Finally, Gralla and Kraft (2018) present negative but mostly insignificant effects of collective agreements on firm-level employment growth. They find significant effects on hires and dismissals, however. In none of these investigations, firm-level and industry-wide negotiations are distinguished. This contrasts with a study by Hirsch et al. (2010), which shows that neither industry-level nor firm-level collective bargaining is associated with a change in separation rates.

### 3 Institutional Background

In Germany, the Collective Agreement Act (Tarifvertragsgesetz, TVG) basically allows firms to choose whether wages and other working conditions are determined individually with each employee, locally with a union at the plant level, or centrally by joining an employers' association. In large parts of the economy, especially in the manufacturing sectors, firms belong to an employers' association (Arbeitgeberverband), which bargains with a sectoral union to set minimum working conditions. The outcome of such negotiations are sector- (or industry-) wide multi-employer agreements, which are often concluded in one region and then extended with possibly slightly modified terms to other areas. Accordingly, only a few of these contracts are truly national. In 2014 such *sector-wide bargaining agreements (SBAs)* applied to 53 % (36 %) of all employees in West (East)



Germany (Ellguth and Kohaut 2015). At the plant level, works councils typically monitor the enforcement of an SBA and provide for a voice for employees. Co-determination at the plant level by works councils covered about 43 % (33 %) of all private sector employees in West (East) Germany in 2014 (Ellguth and Kohaut 2015). Works councils have extensive co-determination rights with respect to personnel policy and although generally forbidden to explicitly bargain over wages, have also been shown to raise them (Addison et al. 2010). Works councils can affect pay scales, dismissal behaviour as well as organisational issues and have a significant impact on firm behaviour. As a consequence of the interaction with sector-wide bargaining agreements, works council activities have been shown to be beneficial in covered firms (Braendle 2017). While in decline, this dual system of industrial relations still covers the majority of employees (Addison et al. 2011, Ellguth and Kohaut 2015). This is the case because collective contracts are usually applied to all employees in a covered firm, not only to union members.<sup>4</sup> Therefore, collective bargaining coverage is much higher than union density, which has declined in recent years, from 25 % in 2000 to below 18% in 2013 (Visser 2016). Note, finally, that coverage in the public sector is more than 30 % higher than in the private sector.

In contrast to Anglo-Saxon countries, only a small minority of less than 3 % of firms bargain with unions directly at the firm level, covering around 8 % of all employees. These negotiations usually involve sector-union representatives and not (only) union members working at the firm itself. Therefore, such *firm-level contracts (FLCs)* are not the same as firm-level agreements signed by a works council, which can only cover issues not dealt with in collective contracts, unless specifically stipulated otherwise. If FLCs are tailored more closely to productivity and product market outcomes, employment consequences of collective bargaining may be weaker than in the case of SBAs. Alternatively, it may be the case that sectoral agreements allow firms to shift forward wage costs more easily into prices such that firm-level bargaining has stronger consequences. In addition, we know that firms signing collective bargaining contracts themselves (FLC) differ from those which adhere to a sectoral agreement (SBA). They are, for example, more likely to be exporters, to have a works council, to be smaller and to have signed pacts for employment and competitiveness (Addison et al. 2013, 2017b). Whether such differences translate into a greater or lower likelihood of bargaining over wages and employment instead of wages only is not known yet. Similarly, it is an open issue whether unions have more power in plants covered by FLCs than in those subject to SBAs, as approximated by union membership in these plants.

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<sup>4</sup>Among others, Fitzenberger et al. (2013) discuss various reasons and consequences of this practice.

In consequence, several types of bargaining regimes co-exist in Germany: individual wage determination; firm-level contracts, which are quite heterogeneous; and sector-wide bargaining agreements, which can, in addition, contain a varying number of flexibility provisions allowing to deviate from minimum standards under certain circumstances (Braendle and Heinbach 2013). Given these differences between firm-level and sectoral negotiations, it is important to control for the nature of collective bargaining. However, it would be somewhat speculative to predict which type of contract affects employment growth more strongly.

Additionally, about 50 % of the firms that are not formally a member of an employers' association refer to sector-wide bargaining agreements when they determine wages and working conditions with their employees (*Tariforientierung*, Addison et al. 2015b). Finally, it is noteworthy that about 40 % of firms which have signed a collective agreement pay wages in excess of the amount stipulated in these contracts (Jung and Schnabel 2011). This feature is more widespread in case of SBAs and often referred to as wage cushion.

While signing any collective agreement immediately affects wages and working conditions, replacing it by individual contracts takes more time. In particular, the regulations of an SBA or FLC continue to apply until a new contract has been bargained with each employee, which can take up to several years (labelled *Nachwirkungsprinzip*, §3.3 and §4.5 TVG). Therefore, leaving collective bargaining is unlikely to increase wage flexibility in the short run.

## 4 Empirical Procedure, Data and Descriptive Statistics

### 4.1 Estimation Procedure

**Empirical Model:** Following the *union employment literature*, we estimate a job growth model in the following (reduced) form:

$$y_{jt} = \beta_k \cdot union_{kjt} + \delta \cdot X'_{jt} + \alpha_j + \mu_t + \epsilon_{jt}$$

where  $y_{jt}$  is the employment growth rate for plant  $j$  at time  $t$  and  $union_{kjt}$  is our variable of interest, namely an indicator variable taking the value of one when a plant  $j$  is covered by a sector-wide bargaining agreement ( $k=1$ ) or a firm-level contract ( $k=2$ ) at time  $t$  and zero otherwise.

We add individual-specific and plant-specific control variables in  $X_{jt}$  (as detailed in the next section), as well as plant size classes, industry and regional fixed effects to our regression. The unobserved time effect  $\mu_t$  is treated as fixed between plants and estimated via time indicator variables to cover macroeconomic developments or general time trends. Finally,  $\epsilon_{jt}$  represents an idiosyncratic error term. We account for the repeated observation of plants over time using cluster-robust standard errors at the plant level.

**Discussion on Identification:** We determine the parameters  $\beta_k$  using pooled ordinary least squares (OLS) as a reference point, since the (early) *union employment literature* predominantly uses cross-sectional data. We check the robustness of the results, for instance, by using weighted least squares, restricting the sample to balanced panels, and investigating whether survival bias plays a role. These estimates ignore unobserved heterogeneity that is simultaneously correlated with collective bargaining coverage and employment growth, especially unobserved factors determining selection into firm or sectoral bargaining. Moreover, during the observation period, an unprecedented decline in sectoral bargaining coverage took place. Employers opted out of coverage or chose to align working conditions to sectoral agreements without being legally obliged to adhere to them (*Tariforientierung*). This development is well documented, but not well understood (Addison et al. 2017a, Oberfichtner and Schnabel 2017). It is unclear, for example, whether firms leaving bargaining coverage may be doing so because they are finding it increasingly difficult to pay the collectively bargained wage rates.<sup>5</sup>

An estimation via OLS relies on selection on observables, but unobserved heterogeneity could be correlated with both the dependent variable and independent variables. Thus, the estimated coefficients would capture a causal as well as a selection impact. The latter could cause the effect we can measure to be biased. Subsequently, we thus use static panel estimators to control for time-invariant heterogeneity. In the reduced form model,  $\alpha_j$  captures plant-specific unobserved heterogeneity (as well as potentially time-invariant control variables).<sup>6</sup> Identification in the panel dimension using a within-group estimator (or fixed-effects model) relies on observable changes in the bargaining status of plants.<sup>7</sup>

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<sup>5</sup>This might occur if larger, more productive firms play a dominant role in determining the sectoral rate. However, especially during the 2000s, the bargained rates were quite low, often below inflation rates.

<sup>6</sup>As both the dependent and the independent variable of interest are measured at the plant level, we use a two-way-error component model. It delivers the same results as a three-way-error component model controlling for spell-fixed-effects (Andrews et al. 2006). The correlation between plant size and our variables of interest would bias results if we estimate the effect on the worker level.

<sup>7</sup>We could relax the strict exogeneity assumption, namely that the independent variables of interest (as well as the fixed-effects) are uncorrelated with the time-varying part of the error term:  $E[\epsilon_{jt} | \text{union}_{kjt}] = 0$ . This would be done using internal instruments in dynamic panel estimators (GMM-diff and GMM-sys), which also acknowledge the time-dependency of our variables of interest. We have also tested the use of external instruments (two-stage-least-squares estimations). These should account for firm and worker sorting into collective coverage. However, all potential instruments fail to be either valid or relevant (or

Suppose that a collective bargaining agreement is established or terminated in year  $t$  and we observe a change in the employment growth rate prior and subsequent to year  $t$ . Then, the estimated coefficients of the variables indicating the collective bargaining regime can be interpreted as identifying a causal effect of collective negotiations, if any further changes affecting employment growth in the plant under consideration are due to alterations in control variables or time-invariant in a plant-fixed-effects specification.

Institutionally, however, a change in coverage status is a complex process and may take time. On the one hand, if a collective contract is concluded, bargained wages and working conditions are likely to primarily affect future outcomes. On the other hand, if a collective contract is terminated, institutional regulations, especially after-effects clauses (*Nachwirkungsprinzip*, see Section 3), prevent wages and working conditions from being altered for up to several years. Therefore, doubts may arise as to whether a within-group estimator is sufficient to identify causality, since it mainly captures short-term effects and does not differentiate between introducing and abolishing collective coverage. We pursue alternative approaches to overcome this problem.

In Section 5.3 we first use the lags of our main independent variables and, for instance, consider whether a past alteration in the bargaining status has an effect on recent employment growth. Second, we analyse changes in collective bargaining coverage using indicator variables identifying plants that have introduced or terminated collective bargaining at one point of time during our observation period and then interact these indicators with the actual application of collective contracts (difference-in-differences approach). This allows us to control for selection effects of a change in bargaining status, and also to differentiate between introducing and abolishing collective coverage. We estimate the following model:

$$y_{jt} = \gamma_k \cdot \text{unionever}_{kj} + \beta_k \cdot \text{union}_{kjt} + \delta \cdot X'_{jt} + \alpha_j + \mu_t + \epsilon_{jt}.$$

The difference-in-differences estimations include time-invariant dummy variables if a plant has, at one point, introduced or abolished a bargaining agreement:  $\text{unionever}_{kj}$ .<sup>8</sup> The treatment effect is estimated using the  $\text{union}_{kjt}$  variables, which are equal to one if a plant, which has introduced or abolished a bargaining agreement, is covered by the agreement in the respective year.

Difference-in-differences estimation rely first and foremost on the common trend assumption. For this assumption to hold, it must be the case that the treated and control observations would have taken a similar development, had the treatment not taken place.

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both).

<sup>8</sup>In case of a panel fixed-effects estimation, the treatment indicator variables  $\text{unionever}_{kj}$  are contained in the firm-fixed effect,  $a_j$ . In this case, the difference-in-differences estimations are similar to a firm-fixed effects model but ignores either changes into or changes out of the treatment.

Since the treatment is, in our case, a firm-based decision to start or stop the application of collective contracts, there may be potential violations of that assumption. Again, we rely on selection on observables and on controlling time-invariant heterogeneity to account for these violations. Following Imbens and Wooldridge (2009), we discern two effects: the time-invariant indicator variable captures the selection (treatment group effect) into the treatment group of those plants, which at some point of time conclude or terminate a sector-wide bargaining agreement for the first time. The variable of interest captures the exposure to the ‘treatment’, indicating whether the plant was covered by a sector-wide bargaining agreement in period  $t$  (treatment effect).<sup>9</sup> The control group are those plants that always bargain individually with each employee throughout the observation period.

## 4.2 Data and Descriptive Statistics

**LIAB:** We use the the cross-section version 2 (LIAB QM2 9314) of the linked-employer-employee data set (LIAB) from the Institute for Employment Research in Nuremberg (Institut für Arbeitsmarkt- und Berufsforschung, IAB). The LIAB is created by linking official individual data from the IAB Employment Histories (IAB EH) with plant-level survey data from the IAB Establishment Panel (IAB EP).<sup>10</sup> We cover the years 2000 to 2014, the most recent data available. We add the employee information to the IAB Establishment Panel to employ covariates which describe the composition of the workforce, which is not directly available in the plant survey.

The IAB EH is based on information from social security records and therefore excludes civil servants (*Beamte*), students, and the self-employed, all of whom are not covered by this mandatory insurance scheme. Information comprises, inter alia, age, sex, nationality, occupation, education, and daily wages.<sup>11</sup> We restrict our analysis to individuals who work at least 50 % of the standard working time, earn more than 600 Euros per month, are aged between 15 and 65 and not classified as home workers or helping family members.

The IAB EP is a plant-level survey stratified over 10 plant size classes and 16 industries, based on the population of all plants in Germany with at least one employee subject to social security. Starting in 1993 for West Germany and 1996 for East Germany, the sample size has steadily increased to up to 16,000 plants per year. The survey covers about 1 % of all plants and about 7 % of all employees in Germany. It is conducted

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<sup>9</sup>We have also estimated the treatment effect using a fixed-effects difference-in-differences model. This, however, does not allow us to estimate a treatment group effect, because it is time-constant and would be omitted from a panel estimation.

<sup>10</sup>For a more detailed description of the LIAB, see Heining et al. (2014).

<sup>11</sup>The information on wages is very exact, but censored at the upper earnings limit for social security contributions. We lack precise information on individual working time.

via personal interviews with senior staff or personnel managers, and has a very high response rate as well as very low panel attrition. The questionnaire focuses on the plants' personnel structure, development and policy, and offers extensive information on plant characteristics.<sup>12</sup>

We restrict our sample to plants with at least five employees subject to social security and to plants for which we can observe at least five employees per plant in each year in the IEB. Furthermore, we drop plants from agriculture and mining as well as non-profit-organisations. We also exclude public administration plants, but not publicly owned plants that operate in a market environment.<sup>13</sup> Our data allows us to distinguish these different types of public sector plants and, thus, enables us to extend previous contributions.

**Collective Bargaining:** To assess the impact of union bargaining, we use plant-level information and distinguish whether a plant is covered by collective bargaining at the firm level between a sector union and the management of the company (firm-level contract, FLC) or at the sectoral level involving an employers' association (sector-wide bargaining agreement, SBA). Regarding comparability, firm-level contracts are institutionally most similar to the recognition for collective bargaining at the establishment level (United Kingdom).<sup>14</sup>

Table 1 presents the shares of employees and the number of plants covered by different bargaining regimes in our data. We replicate the stylised facts that sector-wide bargaining agreements are still the dominant regime in terms of employees, covering about 49 % of all employees in 2014. The share of covered employees has steadily fallen by about 12 percentage points, as also documented in other studies (Fitzenberger et al. 2013, Addison et al. 2015a).<sup>15</sup> The share of employees covered by firm-level contracts has been

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<sup>12</sup>Information contained in the IAB EP is obtained at the workplace, the firm, or some intermediate level. This variety makes the interpretation of results less straightforward. We can control, however, for the legal status of the reporting unit and whether it is a single plant firm or not. For further information on the IAB EP, see Ellguth et al. (2014).

<sup>13</sup>Individuals working in the public administration are usually covered by a state-wide bargaining contract or are civil servants (*Beamte*) for whom different legal regulations apply.

<sup>14</sup>The union employment literature employs various measures of union strength, depending on the institutional setting in the country and the data sets available. The most common measures are union density (the share of union members among all employees) (Blanchflower et al. 1991, Machin and Wadhvani 1991, Bronars et al. 1994, Dunne and MacPherson 1994, Addison et al. 2000, Wooden and Hawke 2000, Krol and Svorny 2007, Bryson and Dale-Olsen 2008) or union recognition for collective bargaining (Blanchflower et al. 1991, Machin and Wadhvani 1991, Leonard 1992, Blanchflower and Burgess 1998, Booth and McCulloch 1999, Addison and Belfield 2004, Bryson 2004, Bryson and Dale-Olsen 2008, Bryson and Nurmi 2011), while other authors use variations of these measures.

<sup>15</sup>The share of plants covered has experienced a similar development at a lower level, falling from 51 % in 2000 to only 41 % in 2014. Discrepancies with other findings stem from our sample restriction, for instance by disregarding small plants.

Table 1: Prevalence of Bargaining Regimes: Share of Employees and Number of Plants Covered

<b>Bargaining Re- gime</b>	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	Total
<b>Individual Wage Determination</b>																
Share of Employees (%)	30.82	29.08	31.01	30.78	31.93	33.53	34.76	37.32	37.83	36.57	37.88	40.59	40.90	41.72	42.84	33.69
No. of Plants	3,303	3,686	3,840	4,038	3,907	4,041	4,068	4,388	4,211	4,185	4,262	4,462	4,541	4,615	4,565	62,112
<b>Firm-Level Contract</b>																
Share of Employees (%)	7.78	8.49	8.08	8.72	8.01	8.74	9.56	8.65	8.69	10.26	8.80	8.14	8.59	9.56	8.37	8.99
No. of Plants	753	810	774	825	788	897	879	824	811	848	698	644	671	697	641	11,560
<b>Sector-wide Bargaining Agreement</b>																
Share of Employees (%)	61.40	62.44	60.91	60.49	60.06	57.73	55.69	54.03	53.48	53.18	53.32	51.28	50.51	48.72	48.79	57.32
No. of Plants	4,847	5,205	5,042	4,924	4,729	4,758	4,405	4,226	4,043	3,918	3,460	3,397	3,362	3,294	3,103	62,713
<b>Total</b>																
No. of Plants	8,903	9,701	9,656	9,787	9,424	9,696	9,352	9,438	9,065	8,951	8,420	8,503	8,574	8,606	8,309	136,385

*Note: Employment shares are calculated using representative sample weights.*

*Source: LIAB QM2 9314, Waves 2000 to 2014; own calculations (controlled remote data access via FDZ).*

relatively stable, such that individual bargaining has become more widespread. This development is partially reflected in our sample: 5,047 plants (11.19 % of all plants covering 11.48 % of all employees) conclude a collective contract (either SBA or FLC) during the time period of consideration, while 6,260 plants (13.20 % of all plants covering 13.78 % of all employees) leave collective coverage.<sup>16</sup> Hence, the decline in bargaining coverage may be due to new plants.

<sup>16</sup>These numbers may include 'frequent changers', that is, plants that change collective coverage back and forth. The IAB has recently established a policy that forces additional requests if, among other variables, the collective bargaining status of a plant has changed (Ellguth and Kohaut 2015). The panel data will cause the outcome to occur if there is significant measurement error in the switching of bargaining status. The coefficients will be biased towards zero in the presence of measurement error. To minimise the likelihood of reporting errors, we have interpolated the bargaining status of plants which repeatedly change their bargaining status back and forth. In a robustness checks we have also dropped these plants, for which frequent changes in union status occurs. There are, however, depending on the exact definition (for example when allowing only one change every three years), not enough observations to alter the results. That is, the results are the same when dropping these plants instead of interpolating their bargaining status.

**Employment Growth:** To measure plant-level employment growth, we use the concept of job flows. We compute employment growth rates according to Davis and Haltiwanger (1992) as the difference in the number of employees  $x$  in a plant  $j$  between year  $t$  and year  $t-1$ , divided by the average number of employees in both years:

$$jgr_{jt} = \frac{x_{jt} - x_{jt-1}}{(x_{jt} + x_{jt-1})/2}$$

Compared to conventional non-standardised growth rates, this measure has the advantage of being approximately normally distributed and bounded by 2 (-2) from above (below).<sup>17</sup> Some of the (newer) studies from the union employment growth literature have already used this measure, for example, Bryson and Nurmi (2011), Bryson (2004), Wooden and Hawke (2000), Walsworth (2010).

Table 2 presents descriptive statistics of job flow rates and especially job growth rates, differentiated by bargaining regime. Average employment growth ranges from one to three percentage points per year. Employment growth is larger in plants with individual wage determination due to a higher rate of job creation. Furthermore, plants covered by sector-wide bargaining agreements (SBAs) feature the lowest rate of job reallocation (the sum of either job creation or job destruction). Similar results can be seen in Gralla and Kraft (2018), which analyse the effects of works councils on hires and dismissals and also control for collective bargaining in their regression analyses. The differences between plants covered by firm-level contracts (FLC) and SBA are fairly small. Job destruction is about the same across bargaining regimes. The weighted job growth rates, depicted in the fifth and sixth columns of Table 2, are larger than the unweighted ones because small plants usually have higher job growth rates. The overall job growth rates of about three percentage points correspond to what happened in the macro-economy over the period.

While these numbers are qualitatively comparable to the ones presented in the union employment literature for Anglo-Saxon countries, they differ quantitatively. In particular, the difference in employment growth rates between unionised plants and non-unionised plants appears to be smaller in absolute terms in Germany. Furthermore, it is noteworthy that there is no discernible correlation between the level of collective bargaining - FLC versus SBA - and employment growth.

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<sup>17</sup>For the computation we use the contemporary information from the IAB EP questionnaire on the employment levels of the the last two years.



Table 2: Job Flows by Bargaining Regime

Bargaining Regime	Job Reallocation Rate	Job Creation Rate	Job Destruction Rate	Job Growth Rate (unw.)	Job Growth Rate (plant-w.)	Job Growth Rate (empl-w.)	N. of Obs.
Individual Wage Determination	0.11 (0.17)	0.07 (0.14)	0.05 (0.12)	0.02 (0.20)	0.05 (0.21)	0.05 (0.20)	63,921
Firm-Level Contract	0.10 (0.17)	0.05 (0.12)	0.05 (0.14)	-0.00 (0.20)	0.03 (0.22)	0.02 (0.17)	13,561
Sector-wide Bargaining Agreement	0.09 (0.14)	0.04 (0.11)	0.05 (0.11)	-0.00 (0.17)	0.02 (0.18)	0.02 (0.15)	71,644
Total	0.10 (0.16)	0.05 (0.13)	0.05 (0.12)	0.01 (0.19)	0.03 (0.20)	0.03 (0.17)	149,126

*Note: Numbers denote means, standard deviations in parentheses; Calculated using representative sample weights which control for plant size.*

*Source: LIAB QM2 9314, waves 2000-2014, own calculations (controlled remote data access via FDZ).*

**Covariates:** Similar to the *union employment literature* surveyed in Section 2, we include further covariates to control for (selection on) differences in observable characteristics. Regarding plant-level characteristics, we control for the existence of a works council, alignment to an SBA, the existence of a wage cushion (paying wages in excess of the collectively agreed upon level), investment activity, plant age, public or foreign ownership and organisational status (single plant, public listing, public corporation), as well as additional information on the workforce composition (share of open positions, temporary workers, as well as the job churning rate<sup>18</sup>).

We further include characteristics with a high share of item-non-response in some specifications. These variables might have an influence on employment growth, but they reduce sample size. We use the average working time for full-time employees, the share of exports, personnel and turnover outlook, firm-sponsored training, and a dummy indicating whether overtime was utilised. Furthermore, we control for productivity at the plant level, employing the log of total investments as well as the share of expansion investments on total investments. Additionally, we include fixed effects for the industry, the region, and the year of observation at the plant level. We offer a complete list of variables in the Appendix (see Table 6).

Using the linked-employer-employee data, we can incorporate individual characteristics aggregated at the plant-level. In particular, we use the share of female workers, and the shares of employee age groups in a plant. Additionally, we control for tenure employing categorical variables, as well as the share of employees with foreign nationality. We utilise

<sup>18</sup>Calculated as  $(\text{hires} + \text{separations} - |\text{hires} - \text{separations}|) / (\text{hires} + \text{separations})$ .

employee shares, distinguishing between unskilled, skilled and high-skilled workers;<sup>19</sup> as well as trainees and part-time workers. We further include the mean of employees' log daily gross earnings and the share of employees with an individual wage censored at the social security contribution ceiling.

Of course, it can be argued that several of the firm-level controls are potentially endogenous to union bargaining. At a more general level, one could also worry about workforce characteristics (since there is sorting of workers across firms with and without union bargaining). Therefore, we present estimates with and without different sets of control variables. In addition, we have marked the potentially endogenous control variables in Table 6.

## 5 The One Constant in Germany

### 5.1 Cross-sectional Evidence

We start by presenting cross-sectional evidence from pooled OLS models to ensure comparability with much of the (early) *union employment literature*. These baseline results are accompanied by robustness checks regarding data issues and validity in certain subsamples. We conclude with panel and difference-in-differences estimations.

**Baseline results:** Table 3 presents an overview of the effects of collective bargaining on employment growth in German plants. Due to space limitations, we only display the estimated coefficients relating to the variables of interest in the main text, while the entire set of results is reported in the Appendix (see Table 7). Specification (1) presents the raw differences in employment growth between plants with and without union bargaining. It indicates that the unconditional difference in average employment growth between covered and uncovered plants amounts to -2.3 percentage points per year.<sup>20</sup>

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<sup>19</sup>We use the imputation method supplied by Fitzenberger et al. (2006) to get increasingly consistent information.

<sup>20</sup>Differences from the descriptive results emerge from the use of sample weights. Incorporating survey weights (or at least a robustness check using them) is a necessary condition when working with the LIAB data. However, as Bossler et al. (2018) have shown, it is equivalent to employing the respective stratification variables (in our case federal state, economic sector and firm size class) as control variables. A number of our more sophisticated empirical approaches do not allow the simultaneous use of survey weights and cluster-robust standard errors. Therefore, we have checked that our baseline specifications are robust against the use of survey weights and prefer to rely on cluster-robust standard errors for the further analyses.

Table 3: Collective Bargaining and Employment Growth: Results from Pooled Ordinary Least Squares

Variables	(1)	(2)	(3)	(4)	(5)	(6)
Sector-wide Bargaining Agreement	-0.0231*** (0.0011)	-0.0227*** (0.0012)	-0.0139*** (0.0018)	-0.0101*** (0.0017)	-0.0086*** (0.0019)	-0.0064*** (0.0018)
Firm-Level Contract	-0.0233*** (0.0020)	-0.0237*** (0.0020)	-0.0129*** (0.0023)	-0.0071*** (0.0023)	-0.0077*** (0.0025)	-0.0063*** (0.0024)
Firm-Level Variables	No	No	Some	Some	Some	All
Individual-Level Variables	No	No	No	Yes	Yes	Yes
Dummy Variables	No	Yes	Yes	Yes	Yes	Yes
N. of Observations	149,288	149,288	149,288	149,288	113,983	113,983
N. of Clusters	34,285	34,285	34,285	34,285	27,456	27,456
F-Statistic	241.72	40.60	71.31	90.63	74.78	104.80
R squared	0.00	0.01	0.04	0.06	0.06	0.10
Aikaike Criterion	-77665.36	-79146.45	-82944.94	-86120.36	-78874.48	-83815.21

*Note:* Standard errors clustered at the plant level in parentheses; fixed effects: plant size classes, industries, regions and years; other control variables: as Table 7 in the Appendix; significance levels: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .  
*Source:* LIAB QM2 9314, Waves 2000 to 2014; own calculations (controlled remote data access via FDZ).

In specification (2), we add fixed effects or indicator variables for plant size, industry, region, and years, labelled dummy variables for simplicity: the results do not change qualitatively.<sup>21</sup> However, introducing plant-level control variables in specification (3) and plant-level averages of individual-level control variables in specification (4) reduces the coefficients of interest substantially. When both sets of variables are incorporated, the estimated impact of SBAs (FLCs) on employment growth drops to about 1.0 (0.7) percentage points.

Specification (6) furthermore incorporates observation-sensitive plant-level control variables, while specification (5) checks for sample selection by running the model of specification (4) on the sample of specification (6). While there is no evidence of such selection issues with regard to FLCs, a comparison of specifications (4) and (5) cannot completely rule out this possibility for SBAs. The inclusion of further plant-level control variables in specification (6) reduces the quantitative impact of collective bargaining on employment growth to about -0.6 percentage points per annum. In further robustness checks, we have included only strictly exogenous variables, as defined in Table 7. These are variables which cannot be changed by collective bargaining status, such as firm age. Our analyses have shown that this does not alter the effects of our variables of interest.

<sup>21</sup>We have also estimated a model using industry\*region interaction fixed effects (not depicted in Table 3), since the latter could help distinguishing between collective bargaining and the potentially confounding factors associated with product markets (since, as noted, above sectoral agreements tend to be negotiated at the regional level). Our results do not change (and are available upon request).

The explanatory power of the model is satisfactory. While the inclusion of firm-level variables influences the coefficients of interest most, the inclusion of individual-level covariates adds to the R squared and further improves the model, which is an advantage of using linked- employer-employee data. As the coefficients of interest shrink with the inclusion of covariates, this indicates that firm- and worker-sorting on observables play an important role in explaining differences in employment growth between covered and uncovered plants.

**Discussion and Interpretation:** In summary, we also observe a significant, negative correlation between collective bargaining and employment growth in Germany, when using a comparable methodology to that which has predominantly been employed in the early *union employment literature*. The effect is much smaller in absolute terms than the impact found for Anglo-Saxon countries, but sizeable in relative terms (that is about 30 %).<sup>22</sup> Interestingly, the effect of bargaining on employment growth does not depend on the degree of centralisation of negotiations.<sup>23</sup> If sectoral wage negotiations imply that most firms of an industry incur a wage increase, they will be able to shift forward the resulting cost increase to a greater degree than in the case of firm-level bargaining. This is because the output price is more likely to be fixed from the perspective of a single firm. This line of argument suggests that the employment effects of collective bargaining are more pronounced in case of FLC. Our findings are not consistent with this view. A number of reasons can be put forward to explain why no differential effects can be observed for SBA and FLC. If trade unions take into account that the labour demand elasticity is higher at the firm level, they will be more moderate in their wage demands. Bargaining at the firm level usually involves trade unions, which are also predominantly active at the sectoral and national level, so that, if trade unions ensure common wage adjustments across most firms in an industry, irrespective of whether this is due to a FLC or SBA, all firms will have the same possibility to shift forward cost increases via higher output prices. Firm-level negotiations make it easier to negotiate not only over wages, but also over employment (efficient bargaining model). If this leads to higher employment at a given wage, FLCs may generate advantageous employment consequences. While we cannot test these potential explanations with our data, the above arguments clarify that the expectation of differential employment effects of collective bargaining at the firm and sectoral level may be less compelling when considering the issue more thoroughly.

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<sup>22</sup>The large relative effect is caused by the relatively weak overall level of employment growth in Germany during the observation period.

<sup>23</sup>While the coefficients in our preferred specification (4) seem to differ in size, an F-test with the null hypothesis of similar coefficients cannot be rejected on any reasonable significance level (p-value 0.1048).

**Effect Heterogeneity:** When analysing labour market institutions, effect heterogeneity is a major concern. Institutions often have different consequences in different environments and their impact may interact with the effects of other institutions. Therefore, we have looked at the employment effects of collective bargaining separately for the private and public sector, manufacturing and services, West and East Germany, as well as for exporting and medium-sized companies.

Table 8 in the Appendix summarises the results of the OLS estimations, comparable to specification (4) of Table 3. In the public sector, collective bargaining is conducted at a more centralised level between sector-level unions and the federal government or an association of the German federal states. Hence, the adverse employment consequences may be less pronounced. We indeed find that collective bargaining is not associated with lower employment growth.<sup>24</sup> These public sector results are interesting, but should not be over-interpreted, since collective bargaining may work rather differently: the extension of collectively agreed wage rates is a standard procedure in that sector.

Additionally, it has been argued that industrial relations institutions have a more robust impact in regions and sectors where unions are traditionally strong (Kohaut and Schnabel 2003). This is particularly the case in manufacturing and in Western Germany, while unions were rendered ineffective during the German Democratic Republic in the eastern part of the country. If unions are stronger in some sectors and regions, the detrimental employment effects of collective bargaining will be more pronounced. We find, however, no support for this hypothesis. The coefficients of our variables of interest do not differ significantly from each other in the respective sub samples, with one exception. FLCs in East Germany are no longer significantly associated with lower employment growth, which might be due to the small numbers of observations, however.

Next, we analyse two special groups of plants. First, exporting plants are, on the one hand, more exposed to international competition and might, therefore, be more severely affected by collectively bargained wages. On the other hand, they are usually more productive than non-exporting plants and might therefore be able to cope with higher wages more easily. Our findings suggest that the two potential effects cancel out. Second, we look at medium-sized plants, which are of particular interest to policy makers focusing on the German 'Mittelstand'. We see that the effects of collective bargaining on employment growth tend to be slightly weaker for these plants, a result that concurs with recent

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<sup>24</sup>In the IAB EP, the public sector is narrowly defined by industry classification as plants belonging to the public administration. We additionally view plants (1) with either a budget as business volume, (2) which identify themselves as public corporations, or (3) employ public servants (*Beamte*) as belonging to a widely defined public sector. For these plants, collective coverage is higher than for the whole sample, at about 60%. Because of the wide definition of the public sector, we can observe, however, enough uncovered observations.

findings by Walsworth and Long (2013) for Canada.<sup>25</sup>

To summarise: The findings depicted in Table 8 indicate that collective bargaining is consistently associated with a reduction in employment growth in the case of industry-level negotiations (SBA) by about 1 percentage point. The effects of firm-level contracts (FLC) are estimated slightly less precisely for the various sub samples, but indicate a comparable quantitative effect. The only exception is the public sector, for which we observe no collective bargaining effect on employment growth.

## 5.2 Robustness Checks

There are many factors which could drive our results, regarding data choice for instance, macroeconomic conditions, survival bias or institutional interactions. Subsequently, we shortly report the findings of robustness checks concerned with these issues.

**Data Issues:** In our panel, large plants are over-represented. To ensure external validity of our results, we have also estimated weighted regressions. The estimated coefficients (*not documented*) are similar to those depicted in Table 3. More specifically, they suggest that the effect of SBAs on employment growth tends to be more pronounced for large plants, while the effects of FLCs might be larger in small plants. Furthermore, we have verified whether the use of an unbalanced panel drives the results. Accordingly, we consecutively restricted our sample to plants that stay in the sample for a certain period of time. The original results generally hold with some exceptions (results available upon request). Finally, our dependent variable, yearly employment growth, can be calculated in different fashions. We could use leads or lags of the dependent variable or, alternatively, yearly employment levels instead of the retrospective information in the recent wave, which is subject to small inconsistencies. Re-estimating the models using these alternative definitions reduces the size of coefficients of our variables of interest, but not necessarily the significance levels. If it does, it is mostly due to the reduced sample and not due to changes in specifications.

**Macroeconomic Conditions:** We also have undertaken two robustness checks regarding the business cycle and recession effects. First, we have omitted the Great Recession years, i.e. 2009 and 2010. Second, we have split the sample by GDP growth per year, that is between years with favourable economic conditions, i.e. high GDP growth, and years with poor economic conditions, i.e. low GDP growth. When omitting the years 2009 and 2010,

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<sup>25</sup>These findings also address a potential common support problem in certain parts of the size distribution. A formal examination, for instance by some sort of propensity score matching estimation, will therefore not alter the result.

our results are unaffected. Similarly, splitting the sample in the way outlined above yields comparable estimates for both sub samples. Accordingly, there is no evidence that the effects of collective bargaining agreements on employment growth vary over the business cycle. Instead, our findings are consistent with the view that such contracts exhibit enough flexibility to accommodate adverse demand shocks. This evaluation is also mirrored by the fact that the German labour market was extraordinarily stable during the crisis 2009 and 2010 and no significant change in unemployment could be observed during these years.

**Plant Closures:** If union bargaining (negatively) influences employment growth, this is likely to have a (positive) effect on plant closures. As a consequence, measuring employment growth on a panel of surviving plants may bias upwards the estimated coefficients (Blanchflower et al. 1991, Bryson 2004). Therefore, we follow an approach used by Bryson and Nurmi (2011) to model sample selection, controlling for plant survival using a two-step Heckman selection model.<sup>26</sup>

The results from Table 4 indicate that there might exist a (negative) selection of plants covered by a sector-wide bargaining agreement into survival. In our preferred specification (4), which includes all control variables available for the entire sample, plants covered by an SBA have an 8.1 % lower probability to survive into the next panel year. We can see correlated error terms (significant rho), non-independent equations (significant Wald-tests), and a quite large selection term. However, the effect is so small that it does not change the coefficients of collective bargaining in the employment equation. They are still significantly different from zero and similar in magnitude to the pooled OLS results. Therefore, negative employment effects of collective bargaining are not driven by more frequent closures of covered plants, but by reduced employment growth in continuing plants.

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<sup>26</sup>In the panel version of the IAB EP, we can observe if and when plants cease to operate. This is the case for 1,557, or 1.7 %, of all plants in our sample. For this estimation, we use a sub sample of the data until 2010, since we cannot track the existence of plants in more recent waves. We use this information to model the selection of collectively covered plants into survival. A dummy variable indicates whether a plant survives the observation period (one) or not (zero). We adjust the estimates of our pooled OLS regression by a selection equation estimating the probability of survival using a linear probability model. We estimate survival and employment growth jointly by maximum likelihood, weighted by the sampling probability from the first stage (Heckman 1979). Identification relies on the restriction that plants founded after 1990 are less likely to survive, but if they do, their employment growth does not systematically differ from older plants (Guertzgen 2010).

Table 4: Collective Bargaining and Employment Growth: Controlling for Firm Survival

Variables	(1)	(2)	(3)	(4)	(5)	(6)
<b>Job Growth Rate</b>						
Sector-wide Bargaining Agreement	-0.0223*** (0.0013)	-0.0241*** (0.0014)	-0.0116*** (0.0021)	-0.0078*** (0.0021)	-0.0082*** (0.0024)	-0.0062*** (0.0023)
Firm-Level Contract	-0.0242*** (0.0023)	-0.0285*** (0.0024)	-0.0164*** (0.0028)	-0.0105*** (0.0028)	-0.0083*** (0.0031)	-0.0074** (0.0031)
<b>First Stage: Firm Survival</b>						
Sector-wide Bargaining Agreement	-0.0156 (0.0220)	-0.0522** (0.0240)	-0.0492 (0.0335)	-0.0814** (0.0339)	-0.0847* (0.0450)	-0.0780* (0.0455)
Firm-Level Contract	0.0227 (0.0398)	-0.0904** (0.0422)	-0.0468 (0.0480)	-0.0606 (0.0482)	-0.1126* (0.0632)	-0.1152* (0.0640)
Share of old Plants by District	0.2796*** (0.0499)	0.1498 (0.1214)	0.0833 (0.1233)	0.0733 (0.1249)	0.1056 (0.1718)	0.1148 (0.1730)
Share of Trainees	0.1690 (0.1213)	0.2482* (0.1410)	0.0446 (0.1344)	0.0412 (0.2194)	0.1415 (0.3453)	0.0729 (0.3539)
Firm-Level Variables	No	No	Some	Some	Some	All
Individual-Level Variables	No	No	No	Yes	Yes	Yes
Dummy Variables	No	Yes	Yes	Yes	Yes	Yes
$\rho$	-0.0144 (0.0107)	-0.0151*** (0.0039)	-0.0198*** (0.0038)	-0.0199*** (0.0038)	-0.0215*** (0.0067)	-0.0367*** (0.0089)
$\sigma$	-1.6845*** (0.0082)	-1.6898*** (0.0082)	-1.7044*** (0.0083)	-1.7386*** (0.0083)	-1.7932*** (0.0095)	-1.8045*** (0.0095)
N. of Observations	96451	96451	96451	96451	65284	65284
N. of Cluster	26659.00	26659.00	26659.00	26659.00	18721.00	18721.00
Chi squared	313.59	1224.38	2733.70	5162.74	4078.89	5398.71
$\rho$	-0.01	-0.02	-0.02	-0.02	-0.02	-0.04

Note: Standard errors clustered at the plant level in parentheses; fixed effects: plant size classes, industries, regions and years; other control Variables: as in specification (4) of Table 7; significance levels: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Source: LIAB QM2 9314, Waves 2000 to 2010; own calculations (controlled remote data access via FDZ).

**Institutional Diversity:** The institutional setting in Germany is relatively complex. Therefore, we extend our analysis by differentiating the exact wage-setting regime. Specifically, we distinguish plants which do not sign a collective bargaining agreement, but adhere to its content (*Tariforientierung*), and plants which are covered by a collective agreement, but pay an additional wage cushion i.e., wages above the contractually stipulated level. Both groups of firms have more scope to adjust wages in response to negotiated increases and may exhibit higher employment growth than those which are bound more closely to the content of collective contracts. The results (available upon request) show that paying a wage cushion does not drive the negative employment effects of collective negotiations. The same is true with respect to paying wages in alignment with the regulation of an SBA, without legally being obliged to do so (*Tariforientierung*). These findings suggest that employment growth is not affected by the actual magnitude of wages, but by collective bargaining coverage per se.



The second institutional diversity relates to works councils. It may be the case that works councils mitigate the negative correlation between employment growth and collective bargaining, particularly in the presence of SBAs. In a further robustness check, we distinguish between plants without a works council and those in which such an institution exists. Our results (also available upon request) reveal quite similar associations between collective bargaining and employment growth for both types of plants. Hence, we can conclude that the negative effect of sectoral bargaining agreements can be found independently of works council existence.<sup>27</sup> The upshot of the robustness analyses of this section is that the negative relationship between collective bargaining and employment growth of about 1 percentage point per year is neither generated by data issues, survival bias, nor institutional features of the German system of industrial relations. Furthermore, while we know that firms signing FLCs differ from those that adhere to SBAs, the robustness checks reveal no consistent pattern of how these differences affect the employment consequences of collective bargaining.

### 5.3 (No) Causal Evidence

**Panel Estimations:** The coefficients of interest of the plant-fixed effects specifications are shown in columns (1) and (2) of Table 5. We find that the effects of collective bargaining on employment growth are not statistically different from zero. Therefore, we obtain no evidence that changes in collective bargaining coverage cause an increase or a reduction in employment growth in the short-run.<sup>28</sup>

The fixed-effects estimator might suffer from an identification problem if the introduction or abolition of a collective contract takes time to materialise. Furthermore, in absence of exogenous variation in bargaining status, the results require that employment growth or unobserved variables are not the reason why firms change their bargaining status (endogenous switching).<sup>29</sup> The panel analysis therefore rests on the assumption that the small number of switchers across bargaining status are representative of the impact of collective bargaining as a whole.<sup>30</sup> Consequently, we are cautious about the interpretation of the study's panel results.

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<sup>27</sup>Moreover, it is evident from the full model presented in Table 7 in the appendix that works councils have a negative effect on employment growth, which is at least as large as the collective bargaining effects. While not our focus, this negative employment growth effect is compatible with the impact found by Addison and Teixeira (2006) and Gralla and Kraft (2018), though it is not with the findings by Jirjahn (2010).

<sup>28</sup>We have also estimated random-effects models and correlated random effects models. The coefficients are similar to the OLS case. The results are available upon request.

<sup>29</sup>There are no sources of exogenous switching in the German case, such as changes in the collective bargaining law which could be used as a natural experiment.

<sup>30</sup>A total of 11% of all plants commence to negotiate working conditions collectively, while 13% of all plants leave collective bargaining on average. This includes, however, frequent changes.

Table 5: Collective Bargaining and Employment Growth: Results from Panel and DiD-Estimations

Method Variables	Plant-Fixed Effects		DiD Introduction		DiD Abolishment	
	(1)	(2)	(3)	(4)	(5)	(6)
Sector-wide Bargaining Agr.	0.0002 (0.0024)	-0.0015 (0.0032)				
Firm-Level Contract	0.0039 (0.0033)	0.0025 (0.0037)				
DiD-Treatment Group Effect			-0.0013 (0.0051)	-0.0024 (0.0051)	-0.0037 (0.0032)	0.0014 (0.0042)
DiD-Treatment Effect			-0.0111* (0.0065)	-0.0046 (0.0074)	-0.0096** (0.0039)	-0.0113** (0.0047)
Always Covered by SBA			-0.0147*** (0.0022)		-0.0141*** (0.0021)	
Firm-Level Variables	No	Some	Some	Some	Some	Some
Individual-Level Variables	No	Yes	Yes	Yes	Yes	Yes
Dummy Variables	No	Yes	Yes	Yes	Yes	Yes
N. of Observations	149,288	149,288	106,694	45,443	112,706	51,455
N. of Clusters	34,285	34,285	28,396	13,440	29,237	14,281
F-statistic	0.85	.	65.94	.	68.84	39.17
R squared (within)	0.00	0.07	0.06	0.07	0.06	0.07
$\rho$	0.50	0.70				

Note: Standard errors clustered at the plant level in parentheses; fixed effects: plant size classes, industries, regions and years; other control variables: as in specification (4) of Table 7; significance levels: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Source: LIAB QM2 9314, Waves 2000 to 2014; own calculations (controlled remote data access via FDZ).

As a further potential remedy for identification in the panel dimension, we have included lagged values of the independent variables into the fixed-effects models. This could accommodate for the fact that collective contracts take time to have an impact when first signed, and until their employment consequences have evaporated, subsequent to their termination. The results are available upon request and show that the coefficients remain statistically zero.

In sum, estimation via panel estimators indicates that there is at least no short-run causal effect of a change in collective coverage on employment growth. Also, these estimates may, at least partly, be driven by sample selection, for instance the differences between the OLS and the panel estimators are caused by differences between plants that are always covered and plants that are never covered.

**Difference-in-Differences Estimations:** In the difference-in-differences approach, we focus on plants that first conclude or terminate a collective agreement and disregard plants that have changed bargaining status multiple times.<sup>31</sup> We present two models, depending on whether we disregard all plants that are covered by a collective contract throughout the entire observation period or whether we take this group into consideration using an additional indicator variable (Gralla and Kraft 2018).

Our findings, depicted in the right part of Table 5, indicate the existence of a potentially negative selection into changes of collective bargaining status. The treatment group effect is not significantly different from zero in any specification. This means that plants which change their bargaining status do not have a lower or higher employment growth than uncovered plants before the treatment happens. When we include plants that are always covered in column (3), we see that such plants are also characterised by significantly smaller employment growth when compared to plants which never have a collective contract. The same holds in model (5).

Turning to the actual treatment effect, we occasionally obtain significant results. Plants which have abolished an SBA seem to have lower employment growth after the abolishment. Taken at face value, these findings suggest a positive employment impact of retaining collective bargaining coverage. The results no longer hold, however, if we use a fixed-effects difference-in-differences model: the treatment effects are insignificant once we control for observable characteristics. Furthermore, the findings from difference-in-differences specifications suggest that there is no causal effect of establishing a collective contract on employment growth. The effects are robust to placebo tests, which place the theoretical treatment one to two years ahead or after the actual treatment has taken place (results are available upon request). The negative correlation observed in OLS-specifications is, at least partly, due to a selection effect, which is represented in the significantly negative coefficient for always covered plants.

To conclude, static panel data models and their extensions do not allow us to identify a causal effect of collective bargaining on employment growth in German plants. Instead, the (relatively small) differences observed in OLS-specifications are potentially caused by selection bias and/or endogenous switching into or out of collective bargaining contracts. We can identify some of this bias by selection on observables and time-invariant heterogeneity. Further research may be needed to analyse what might account for this type of bias. It is unclear, for example, why firms with a declining level of employment should join an employer association (SBA) or negotiate a firm-level contract (FLC).

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<sup>31</sup>This avoids putting the introduction and the abolition of a collective contract council quantitatively on the same level, as it happens in fixed-effects or first-differences models. We rely on results for SBAs, because there are not enough plants switching into or out of firm-level contracts. If we pool the two types of contracts together, the results are the same as for SBAs only.

## 6 Conclusion

In a frequently cited paper, Addison and Belfield (2004) argue that *the one constant* characterises the relationship between unionism and establishment performance: Annual employment growth is significantly lower in unionised plants, relative to their non-unionised counterparts. In this paper, we focus on Germany, the largest economy in Europe and a country with extraordinary labour market performance in recent years. Hence, we complement the existing literature with evidence for a more corporatist industrial relations system, in which collective bargaining takes place at the sectoral level for more than 50% of all employees and, additionally, at the firm level for a sizeable minority of plants. We can utilise the linked-employer-employee data set (the LIAB), which allows us to investigate whether the relationship between collective bargaining and employment growth depends on the degree of bargaining centralization. Moreover, we can analyse the issue of causality in a variety of ways.

Using the cross-sectional variation, we observe that annual employment growth in plants covered by collective bargaining is about 1 percentage point less than in uncovered plants, controlling for a host of firm- and workforce-specific determinants. This effect is remarkably robust, since it can be found in various sub samples, when taking into account, inter alia, the (1) endogeneity of firm survival, (2) over-representation of larger plants in the data set, (3) existence of works councils, (4) and also when allowing for the fact that collective agreements may not be binding. Finally, we observe no systematic differences between plants for which wages and working conditions are determined by sector-wide bargaining agreements (SBAs) and plants bound by firm-level contracts (FLCs).

Although the estimated employment growth differs substantially, being about 3 percentage points for Anglo-Saxon countries and 1 percentage point in Germany, the subsequent back-of-the-envelope calculations put this differential into perspective. Employee bargaining coverage in Germany was about 60 % in 2014 (see Table 1), while the figures suggest a value of around 20 % in Anglo-Saxon countries. Taking the absolute number of jobs not created in covered plants would then be broadly the same in Germany and a (hypothetical) similarly sized Anglo-Saxon country. Alternatively, we can relate collective bargaining effects to absolute employment growth rates of about 3 % annually over the period under investigation in Germany (see weighted job growth rate in Table 2). A collective bargaining difference of 1 percentage point then implies that employment in covered plants grew 30% less than in the entire economy.<sup>32</sup> Therefore, we could conclude that,

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<sup>32</sup>Assuming a union effect of three percentage points in Anglo-Saxon countries, the average employment growth would have to be around 9% for the relative difference to be the same. Creating methodologically and quantitatively comparable evidence across countries on the employment effects of collective bargaining may, hence, constitute a topic for future analysis.

although there is not *the one constant* in absolute terms, a negative association between employment growth and collective coverage can be found for Germany as well.

Relating our findings to those of earlier analyses on Germany, which focus on other issues and are described at the end of Section 3, indicates a heterogeneous picture. Addison and Teixeira (2006) and Gralla and Kraft (2018) do not consistently find negative employment growth effects of collective bargaining coverage. At least our estimations based on cross-sectional variations, almost universally indicate detrimental consequences.

Going beyond many studies on the employment effects of unionism for Anglo-Saxon countries, our data also allows us to investigate whether the observed correlation can be interpreted causally from a variety of methodological angles. *The one constant* could only justify policy consequences if causality could be established convincingly. This, however, proves to be difficult. Within-variation fails to identify a significant (short-term) causal effect from changing bargaining status, while possible instrumental variables and dynamic panel estimations cannot be applied due to rejections in state-of-the-art test procedures. Although identifying causal effects is furthermore hampered by the institutional features of the German industrial relations system, insights from difference-in-differences models suggest, if anything, the existence of (negative) selection into collective bargaining, both on observable and unobservable characteristics. Consequently, we observe a negative correlation, but find no causal effect of collective bargaining on employment growth in Germany. More robust evidence on the (non-)existence of causal effects of collective bargaining certainly belongs to the list of imminent research questions.

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## **A Appendix**

### **A.1 Tables**

Table 6: Operationalisation and Summary Statistics of Covariates

Variable	Mean	Std. Dev.	Min	Max
<b>Variables of Interest</b>				
Job reallocation rate	0.10	0.16	0	1.94
Job creation rate	0.05	0.13	0	1.93
Job destruction rate	0.05	0.12	0	1.94
Job growth rate	0.01	0.19	-1.94	1.93
Sector-wide Bargaining Agreement	0.48	0.50	0	1
Firm-Level Contract	0.09	0.29	0	1
Collective Bargaining (SBA or FLC)	0.57	0.49	0	1
Introduction of a SBA at some Point	0.25	0.43	0	1
After Introduction of a SBA	0.29	0.45	0	1
<b>Individual Characteristics (Plant-level Averages)</b>				
+Female Employees	0.42	0.30	0	1
Employees with Foreign Origin	0.07	0.10	0	1
+Empl. with Tenure <5 Years	0.61	0.39	0	1
+Empl. with Tenure 6 to 10 Years	0.17	0.18	0	1
+Empl. with Tenure 10 to 20 Years	0.16	0.18	0	1
+Empl. with Tenure >20 Years	0.06	0.11	0	1
+Empl. aged between 18 and 29	0.20	0.16	0	1
+Empl. aged between 30 and 49	0.52	0.16	0	1
+Empl. aged between 50 and 65	0.28	0.16	0	1
+Flexible Employees	0.11	0.16	0	1
+Trainees	0.05	0.10	0	1
+Skilled Employees	0.69	0.28	0	1
+Highly-Skilled Employees	0.15	0.20	0	1
+Part-Time Employees	0.22	0.25	0	1
+Average Gross Daily Wage	67.51	29.15	1.19	198.16
+Dispersion of Gross Daily Wage	0.04	0.09	0	1
<b>Firm Level Characteristics</b>				
+Works Council	0.40	0.49	0	1
Orientation to SBA	0.23	0.42	0	1
Existence of Wage Cushion	0.25	0.43	0	1
+Share of Vacancies	0.02	0.06	0	1
+Share of Temporary Workers	0.06	0.14	0	1
+Churning Rate	0.06	0.16	0	13.01
+Investment Activity	0.74	0.44	0	1
+New Technical Assets	0.70	0.46	0	1
Firm Age (up to 20 Years)	16.84	10.72	0	39
Public Ownership	0.07	0.26	0	1
+Foreign Ownership	0.07	0.25	0	1
Single Firm	0.68	0.47	0	1
Listed Company	0.65	0.48	0	1
Public Sector	0.12	0.32	0	1
+Average Standard Working Time*	39.02	2.03	0	70
+Log. of Total Investments*	8.76	5.70	0	22.45
+Share of Expansion Investments*	0.22	0.34	0	1
+Share of Exports*	0.08	0.19	0	1
+Firm-Sponsored Training*	0.76	0.44	0	1
+Overtime Dummy*	0.73	0.45	0	1
Turnover Outlook (1 falling, 2 steady, 3 rising)*	2.06	0.70	1	3
Employment Outlook (1 falling, 2 steady, 3 rising)*	2.00	0.57	1	3
<b>Dummy variables</b>				
Sector:	9 dummy variables for different industries (approx. Nace1)			
Region:	12 dummy variables for German Laender (some combined)			
Firm size:	5 dummy variables for different firm size classes			
Year:	11 dummy variables for each year			

Note: 149,288 Observations; \* 113,983 Observations; + potentially endogenous control variables.

Table 7: Collective Bargaining and Employment Growth: Results from Pooled Ordinary Least Squares: Full Results

Variables	(1)	(2)	(3)	(4)	(5)	(6)
Collective Bargaining Agreement	-0.0231*** (0.0011)	-0.0227*** (0.0012)	-0.0139*** (0.0018)	-0.0101*** (0.0017)	-0.0086*** (0.0019)	-0.0064*** (0.0018)
Firm-Level Contract	-0.0233*** (0.0020)	-0.0237*** (0.0020)	-0.0129*** (0.0023)	-0.0071*** (0.0023)	-0.0077*** (0.0025)	-0.0063*** (0.0024)
Works Council			-0.0291*** (0.0015)	-0.0196*** (0.0015)	-0.0177*** (0.0016)	-0.0175*** (0.0015)
Orientation to a CBA			-0.0032** (0.0016)	-0.0008 (0.0016)	-0.0007 (0.0017)	-0.0018 (0.0016)
Wage Cushion			0.0017 (0.0013)	0.0015 (0.0013)	0.0003 (0.0014)	-0.0000 (0.0014)
Share of Vacancies			0.1088*** (0.0181)	0.0691*** (0.0180)	0.0596*** (0.0201)	-0.0106 (0.0195)
Share of Temp Workers			0.0293*** (0.0069)	0.0017 (0.0069)	0.0152** (0.0076)	0.0199*** (0.0073)
Churning Rate			0.0066 (0.0063)	-0.0151** (0.0064)	-0.0126* (0.0075)	-0.0119 (0.0073)
Investment Activity			0.0330*** (0.0013)	0.0322*** (0.0013)	0.0289*** (0.0014)	0.0024 (0.0044)
Modern Technical Assets			0.0168*** (0.0011)	0.0136*** (0.0011)	0.0130*** (0.0011)	0.0078*** (0.0011)
Firm Age			-0.0020*** (0.0001)	-0.0008*** (0.0001)	-0.0007*** (0.0001)	-0.0007*** (0.0001)
Public Ownership			-0.0099*** (0.0022)	-0.0063*** (0.0021)	-0.0050** (0.0023)	-0.0027 (0.0022)
Foreign Ownership			-0.0099*** (0.0023)	-0.0106*** (0.0023)	-0.0094*** (0.0024)	-0.0085*** (0.0023)
Single Firm			0.0030** (0.0012)	0.0051*** (0.0012)	0.0048*** (0.0013)	0.0049*** (0.0012)
Limited Firm			-0.0194*** (0.0013)	-0.0151*** (0.0013)	-0.0137*** (0.0014)	-0.0151*** (0.0014)
Public Sector Plant			0.0039* (0.0023)	0.0113*** (0.0023)	0.0090*** (0.0026)	0.0044* (0.0025)
Female Employees				-0.0117*** (0.0028)	-0.0105*** (0.0030)	-0.0116*** (0.0029)
Foreign origin				-0.0094 (0.0079)	-0.0190** (0.0083)	-0.0141* (0.0081)
Tenure: 6 to 10 years				-0.1058*** (0.0033)	-0.1094*** (0.0035)	-0.0941*** (0.0034)
Tenure: 11 to 20 years				-0.0731*** (0.0033)	-0.0808*** (0.0035)	-0.0640*** (0.0034)
Tenure: over 20 years				-0.0537*** (0.0056)	-0.0618*** (0.0060)	-0.0412*** (0.0058)
Age: between 30 and 49 years				-0.0695*** (0.0060)	-0.0567*** (0.0066)	-0.0450*** (0.0064)
Age: between 50 and 65 years				-0.1284*** (0.0062)	-0.1058*** (0.0066)	-0.0823*** (0.0063)
Other Employees				0.0199*** (0.0061)	0.0263*** (0.0066)	0.0284*** (0.0064)
Trainees				-0.1516*** (0.0096)	-0.1318*** (0.0108)	-0.1045*** (0.0103)

... Table 7 continued ...

Qualification: Skilled						
Qualification: High-Skilled						
Status: Part-Time Worker						
Mean of gross daily wages						
Employees at s.s.contribution limit						
Working Time						
Log. of total investments						
Share of expansion investments						
Share of Exports						
Firm-sponsored Training						
Overtime Dummy						
Rising Turnover Outlook						
Rising Employment Outlook						
Constant	0.0210***	0.0082	-0.0231***	0.0343***	0.0340*	-0.1018***
Dummy variables	No	Yes	Yes	Yes	Yes	Yes
N. of Observations	149,288	149,288	149,288	149,288	113,983	113,983
N. of Clusters	34,285	34,285	34,285	34,285	27,456	27,456
F-Statistic	241.72	40.60	71.31	90.63	74.78	104.80
R squared	0.00	0.01	0.04	0.06	0.06	0.10
Aikaike Criterion	-77665.36	-79146.45	-82944.94	-86120.36	-78874.48	-83815.21

Note: Standard errors clustered at the plant level in parentheses; fixed effects: plant size classes, industries, regions and years; significance levels: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Source: LIAB QM2 9314, Waves 2000 to 2014; own calculations (controlled remote data access via FDZ).

Table 8: Collective Bargaining and Employment Growth: Overview on Subsamples, OLS Results

	Public Sector		Region			Sector				Others
	Yes	No	West Germany	East Germany	Manufacturing	Services	Exporting Firms	20-200 Employees		
Sector-wide Bargaining Agreement	0.0026 (0.0086)	-0.0119*** (0.0018)	-0.0111*** (0.0022)	-0.0119*** (0.0029)	-0.0083*** (0.0026)	-0.0096*** (0.0023)	-0.0093*** (0.0031)	-0.0070*** (0.0024)		
Firm-Level Contract	0.0049 (0.0096)	-0.0074*** (0.0024)	-0.0092*** (0.0029)	-0.0056 (0.0036)	-0.0076** (0.0032)	-0.0062* (0.0033)	-0.0074** (0.0035)	-0.0047 (0.0032)		
Firm-Level Variables	Some	Some	Some	Some	Some	Some	Some	Some		
Individual-Level Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Dummy Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
N. of Observations	17,739	131,549	92,183	57,105	66,066	83,222	37,979	72,236		
N. of Clusters	3,896	31,347	22,900	11,613	14,012	20,310	10,446	17,831		
F-Statistic	8.38	87.73	59.48	43.55	53.57	49.07	34.16	49.53		
R squared	0.04	0.07	0.06	0.07	0.08	0.05	0.10	0.06		

Note: Standard errors clustered at the plant level in parentheses; fixed effects: plant size classes, industries, regions and years; other control variables: as in specification (4) of Table 7; significance levels: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Source: LIAB QM2 9314, Waves 2000 to 2014; own calculations (controlled remote data access via FDZ).