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Opening Clauses in Collective Bargaining Agreements: More Flexibility to Save Jobs?*

Tobias Brändle† and Wolf Dieter Heinbach ‡

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Abstract

Collective bargaining agreements have been said to decrease deployment since the work of Calmfors and Driffill (1988). We investigate empirically whether opening clauses, flexible elements that have been introduced to reduce the decline in coverage, can indeed minimise this effect and increase job growth in covered firms. Using representative data on German establishments, the IAB Establishment Panel, in combination with data on opening clauses from the IAW, and performing propensity score matching to control for selectivity bias, we find that the existence of opening clauses has significantly negative effects on job destruction rates and that it increases job growth by approximately 0.73% per year. However, it does not seem the case that firms with explicit knowledge of opening clauses anticipate their increased flexibility, since they do not have higher job creation rates. As regards the actual application of opening clauses, our results do not show additional effects.

Keywords: collective bargaining, opening clauses, job flows, propensity score matching

JEL code: J51, J63, C21

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1 Introduction

During the recent recession, the German labour market seems to have performed relatively well. Nonetheless, many economists and politicians are still discussing its rigid institutional setting (OECD 2009). In fact, industry-level collective bargaining agreements (CBAs) are still predominant in the German bargaining system (Ellguth and Kohaut 2010), while, in the literature on industrial relations, CBAs have been accused of decreasing employment since the seminal paper of (Calmfors and Driffill 1988). However, recent work by Addison et al. (2007) and Addison et al. (2009) shows the share of collective coverage to be declining.

The main critique of CBAs focuses on their inflexibility and the enactment of a wage floor (Kohaut and Schnabel 2003b). In deed, Fitzenberger et al. (2008), for example, show that collective coverage increases wages and decreases the wage dispersion, and that this effect increases with net union density, i.e. more powerful unions. To counter the critique and the decline in coverage, the bargaining partners in Germany have reacted by introducing various measures of decentralisation and flexibility into the system of wage setting. These include employment pacts, temporary work agencies, and short time work; see, for example, Antoni and Jahn (2009) or Hübler (2005). We turn to one of these measures which has so far not been analysed in detail, namely *opening clauses*,[1] and investigate empirically whether such clauses have a significant effect on job growth in German firms. Additionally, we examine whether opening clauses primarily affect job creation or job destruction.

The concept of job flows has played an important role in labour economics since Davis et al. (1996). Summarising newer developments in the literature on job flows, Haltiwanger et al. (2008) and Bassanini and Marianna (2009) find that size and industry affiliation explain most of the differences in job flows across firms, but not all. As regards the remainder, there is a vast literature on labour market institutions, which mainly aims to explain cross-country differences in job flows.[2] The impact of CBAs on job flows has been analysed in the so-called union employment literature. Most studies suggest a negative impact of 3% lower job growth per year.[3]

[1] Also referred to as *hardship* or *opt-out clauses* (Gürtzgen 2009) in the following we will continue to use the term opening clauses.

[2] See, for example, Joseph et al. (2004) for wage rigidities; Pissarides (2000) for unemployment benefits and labour taxes; Addison and Teixeira (2003) for employment protection; Kugler and Pica (2008) for interactions between those. With respect to Germany, recent studies analyse whether works councils affect job flows; see Addison and Teixeira (2006) or Jirjahn (2008).

[3] Wooden and Hawke (2000) analyse this for Australia; Long (1993) and Walsworth (2010) for Canada; Addison and Belfield (2004), Blanchflower et al. (1991), Bryson (2004) and Machin and Wadhwani (1991) for Britain; Bronars et al. (1994) and Leonhard (1992) for the United States. The German literature has so far been restricted to wage effects, for example Burda et al. (2008), Fitzenberger et al. (2007) and Stephan and Gerlach (2005).

To our knowledge this is the first paper to analyse the impact of opening clauses on job flows. Opening clauses allow firms to lower their employees' wages in the event of negative shocks, and we expect this to have two distinct effects on job flows. First, firms applying opening clauses should reduce job destruction, because they can afford to keep a larger number of employees. Equally, they might reduce job creation if the economical situation seems to be improving. Hence, their job reallocation should be lower. Second, firms not applying opening clauses could increase job creation, because they anticipate their increased flexibility. However, if the economic situation worsens, the application of opening clauses will prevent job destruction from increasing and we will see higher job reallocation for these firms. Overall, we expect higher job growth because firms can more efficiently choose an optimal level of employment.[4]

For our empirical estimation, we use data from the Establishment Panel (EP) of the Institute for Employment Research (Institut für Arbeitsmarkt und Berufsforschung, IAB). We combine this data set with a survey by the Institute of Applied Economic Research (Institut für Angewandte Wirtschaftsforschung, IAW) on opening clauses in CBAs in the manufacturing sector in West Germany, and use propensity score matching, a programme evaluation method, to isolate a causal effect of opening clauses on job flows. This is necessary in order to overcome a possible selection bias, because firms with different flexibility requirements may opt into different bargaining regimes.

The rest of the paper is structured as follows. Chapter 2 will shortly summarise the institutional setting of the German system of industrial relations with a focus on opening clauses. In chapter 3 we will present our data and econometric model. Chapter 4 presents the results together with our robustness checks, while chapter 5 concludes.

2 Institutional Background

As the German system of industrial relations differs to some extent from that of other OECD countries, we will briefly describe the setting in which our empirical investigation takes place. In particular, we will focus on the role of opening clauses. In large parts of the German labour market, particularly in manufacturing, two pillars shape the institutional setting or labour relations: regional, industry-wide CBAs (*Flächentarifverträge*) between trade unions and the respective employers' associations, and co-determination inside firms between work councils and managers.

[4]We do not refer to wages in detail. They are most likely to fall; although, according to Fitzenberger and Franz (1999), the introduction of opening clauses initially increases wages in collectively covered firms, because unions demand an equivalent for the increased flexibility. However, this wage level effect does not seem to play a major role here, because the majority of opening clauses were introduced before the timespan of our data (Heinbach and Schröpfer 2007).

Collective contracts are based on the Collective Bargaining Agreement Act (*Tarifvertragsgesetz*, TVG), which regulates the content, conclusion and termination of labor contracts. They can be negotiated according to §§1,2 TVG between unions and either single employers (firm contracts) or employers' associations (CBAs) and are legally binding for all union members and member firms, but generally extended to all employees. Working conditions negotiated in collective contracts serve as minimum standards. Firms can always choose to deviate from these contracts in favour of employees, but not at their expense (*Günstigkeitsprinzip*).[5] Firms are free to choose whether they want to become a member of an employers' association, bargain at the firm level, or with each individual worker separately – but once they choose to bargain collectively, §§3 and 4 TVG regulate the termination of CBAs by establishing strict after-effect clauses (*Nachwirkungsprinzip*).[6] Therefore, leaving collective coverage seems not to be a viable option for more flexibility, at least not in the short run (Traxler et al. 2001).

German legislation officially prohibits plant-specific agreements between work councils and management to bypass collective agreements. Nonetheless, plant-specific agreements can play a crucial role in wage determination (Hübler 2005). Therefore, unions have started to make collective agreements more flexible by a) relocating more bargaining competence to the firm level via works councils and employment pacts,[7] and b) introducing possibilities for firms to deviate from CBAs in bad economic circumstances through opening clauses. The latter are parts of CBAs (§4 TVG) and specify when and to what extent firms are allowed to reduce working conditions to below the normally binding standards (Kohaut and Schnabel 2007). They are typically restricted in magnitude and time, and rely on the agreement of the responsible union, works council, or employees. Hardship clauses can be applied if a firm proves that job losses can be prevented through their adoption. There also exist clauses for small businesses that allow them to undercut CBAs to a certain extent, in order to make collective coverage more attractive for them. Heinbach (2005) records data on CBAs in West German manufacturing and concludes that opening clauses exist for the majority of CBAs. Heinbach and Schröpfer (2007) additionally find that opening clauses are highly heterogeneous in their contents and application. To make opening clauses more operable, they distinguish three types: opening clauses on working time, opening clauses on compensation, and other, more firm-specific opening clauses. Opening clauses on working time were dominant until the mid 1990s, whereas opening clauses on compensation have been introduced into most CBAs since then.

[5] In fact, Jung and Schnabel (2009) and Kohaut and Schnabel (2003a) report that some 30-40% of firms, depending on the industry, pay above the collectively agreed pay scale, granting employees on average 8% more.

[6] Exemptions from this rule exist if the German Federal Ministry of Labour declares a central collective contract to be universally valid (§5 TVG).

[7] See Hübler and Jirjahn (2003), Massa-Wirth and Seifert (2005), and Waas (2005).

3 Data and Econometric Model

3.1 Data

Our empirical analysis is based on the IAB Establishment Panel. The IAB has conducted this survey each year since 1993 for West Germany and since 1996 for East Germany in personal interviews with higher management staff. The EP is a representative 1% sample of all establishments[8] in Germany with at least one employee covered by social insurance, and contains about 7% of all German employees. It is stratified randomly over industries and firm size classes and comprises over 15,000 establishments per year. There is information on firm characteristics, such as size, turnover, ownership, investment volume, number of jobs and economic prospects; and on labour market institutions, such as collective agreements, works councils, government subsidies, and active labour market policies. For more information, see the IAB website (www.iab.de) or Fischer et al. (2009). We are able to make use of this (valuable) data set through controlled remote data access via FDZ (*Forschungsdatenzentrum*).

In the panel version of the survey, the IAB corrects for panel attrition and panel mortality by adding establishments that match lost observations in terms of their representativeness. However, to compute job flows, we can make use of the fact that for each firm we observe both the current number of jobs and last year's. We can therefore use the cross-section of the survey and increase the number of observations. As the EP measures the numbers of jobs in each firm on June 30th of each year, we cannot observe job flows that happen throughout the year (gross job flows). We follow Davis and Haltiwanger (1992) in computing (net) job growth rates, jgr, as the difference in the number of jobs in years t and $t-1$, divided by the average number of jobs for each establishment j:

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

where x_{jt} and x_{jt-1} represent the number of jobs in the respective year. Accordingly, we compute job creation, jcr, job destruction, jdr, and job reallocation rates, jrr, as the positive or negative part, or as the absolute value of the difference in the number of jobs, respectively.

In our study we use waves 2000 to 2007 of the survey. In addition to a standard annual cohort of questions, there are varying topics of interest not carried out every year. In waves 2005 and 2007 the EP has questions about opening clauses. Establishments respond as to whether they know if opening clauses exist in their firm-specific or collective bargaining agreement, whether they use such clauses, and which type of opening clause

[8] While the IAB EP refers to establishments, we synonymously use the term *firms* in our paper.

they use. Apart from being only available for two years, Kohaut and Schnabel (2007) analyse these questions and find that a large number of firms do not know if opening clauses actually exist.

We therefore supplement the information on opening clauses from the EP with additional regional and industry-specific information gathered from a national archive on CBAs (*Tarifregister*), including the exact types and years of introduction of opening clauses. For an overview of this survey, the IAW Data Set on Opening Clauses, see Heinbach (2005) or Heinbach and Schröpfer (2007). For this analysis, we classify each CBA into two different categories: "no opening clauses" and "opening clauses on working time and/or compensation". We then use information from the German Structure of Earnings Survey (GSES), an official source of statistics on establishments from the manufacturing sector in Germany, to know which CBA applies to a firm in a certain region and industry. We combine the two data sets, aggregate to the collective bargaining area level, and classify a collective bargaining area as belonging to one of the two categories for opening clauses if at least 80% of the covered establishments are in the same category. If this threshold is not met, we assign a third category, "some opening clauses". We add this information to that of the EP such that each firm in the EP is classified into one of three categories, depending on its regional location and industry affiliation.[9]

Table 1: Prevalence of Opening Clauses and Job Flow Rates

Variable	N. of obs.	Share	**Job reallocation**		**Job creation**		**Job destruction**		**Job growth**	
Opening clauses by IAW-classification										
Some	3,442	29.59%	.1390	(.2040)	.0639	(.1498)	.0751	(.1697)	-.0111	(.2467)
Do not exist	711	8.43%	.1548	(.2021)	.0731	(.1725)	.0817	(.1517)	-.0086	(.2545)
Exist	10,780	57.68%	.1175	(.1801)	.0515	(.1310)	.0659	(.1486)	-.0143	(.2146)
All CBAs	13,244	100%	.1275	(.1902)	.0573	(.1412)	.0701	(.1588)	-.0128	(.2286)
Opening Clauses, only IAB information										
Do not exist	1,680	61.20%	.1085	(.1706)	.0514	(.1227)	.0571	(.1411)	-.0056	(.2021)
Do exist	1,171	15.16%	.0880	(.1276)	.0525	(.1023)	.0354	(.0976)	.0170	(.1541)
Do not know	620	23.64%	.1476	(.2283)	.0726	(.1660)	.0750	(.1883)	-.0024	(.2719)
Not applied	549	49.04%	.0980	(.1258)	.0605	(.1109)	.0375	(.0899)	.0230	(.1579)
Applied	618	50.96%	.0784	(.1290)	.0447	(.0931)	.0336	(.1048)	.0110	(.1506)
Total	3,399	100%	.1308	(.2064)	.0644	(.1497)	.0663	(.1695)	-.0019	(.2443)

Note: Numbers represent means; standard deviations are in parentheses; information on IAW-classification restricted to manufacturing in West Germany 2000-2007; information in IAB restricted to years 2005 and 2007. Source: IAB Establishment Panel and IAW Data Set on Opening Clauses, own calculations (controlled remote data access via FDZ).

Table 1 shows the prevalence of opening clauses and job flows for different bargain-

[9]Further information on this procedure is laid out in Heinbach and Schröpfer (2008). For example, industry classification and regions in the EP do not perfectly overlap with collective bargaining areas.

ing regimes. As regards the prevalence of opening clauses, we find that for the majority of firms, 58%, opening clauses exist in their CBAs. For a large number of firms, 30%, we can at least say that some of their CBAs have opening clauses, and for only a small fraction of firms, 8%, we know that their CBAs do not have opening clauses. Using the IAB information only, we find that the majority of firms, 61%, state that their CBAs do not have opening clauses, while only a small number of firms, 15%, say the opposite. A relatively large share of firms, 24%, does not know about opening clauses in their CBAs. Kohaut and Schnabel (2007) already mention this fact, while Heinbach and Schröpfer (2008) address the striking difference in the prevalence of opening clauses depending on the usage of official statistics or firm surveys. We explain the gap in knowledge by looking at the fraction of firms that actually apply opening clauses. Out of those firms stating that they know opening clauses exist in their CBAs, the majority, 51%, do indeed use them. There is a significant level of heterogeneity between firms, i.e. most firms do not inform themselves about opening clauses, but those that do use them frequently. This line of reasoning concurs with Franz and Pfeiffer (2003), who find that firms differ in the way they set wages and in their need for flexibility. Hence, certain labour market institutions or measures face different levels of demand across heterogenous firms.

When looking at job flows, we see that firms with opening clauses have mostly lower job flow rates, compared to firms without. Using the IAW-classification, we compare firms whose CBAs have opening clauses with firms whose CBAs do not. We observe that job reallocation is 26% lower for firms with opening clauses, while job creation is 30% and job destruction 20% lower. Job growth is also lower, by about 66%. Using the IAB information only, we compare firms stating their CBAs have opening clauses to firms which say theirs do not. Again, job reallocation and job destruction are lower for the first group of firms, by about 19% and 38%, respectively. However, job creation is almost the same, while job growth is much higher for firms that claim to have access to opening clauses. Firms saying they actually apply opening clauses again have lower job flow rates. They have 20% lower job reallocation, 27% lower job creation, 11% lower job destruction, and 53% lower job growth rates.

Descriptive statistics, however, cannot identify causal effects. We know that firm size and industry affiliation have a large impact on job flows (Haltiwanger et al. 2008, Bassanini and Marianna 2009). We also recognise that the business outlook and other firm characteristics play an important role. Therefore, we need to control for covariates and for possible selection bias, a problem that has seldom been dealt with in the literature on job flows until now.

3.2 Econometric Model

For our econometric analyses, an estimation via Ordinary Least Squares would not be appropriate due to possible selection bias if firms are assigned non-randomly to programme and control groups, on the basis of their characteristics, as first pointed out by Rosenbaum and Rubin (1983). Cameron and Trivedi (2006) explain a number of econometric methods used to overcome the problem of endogeneity, for example instrumental variables or sample selection models. In this study, we use propensity score matching. While traditional matching methods try to compare observations that are "equal", or at least similar, concerning their characteristics, propensity scores address the "curse of dimensionality" by comparing observations by means of their probability of being in the programme group conditioned on observable characteristics.

Using a logit model, we compute the propensity scores as the conditional probability that the CBA of a collectively covered firm has opening clauses, given the firm's characteristics.[10] We control for multiple observations of firms in the panel by using clustered standard errors. For the correct specification of the selection estimation we employ economic theory, experience from other empirical studies, and knowledge about institutions. We broadly follow Addison and Teixeira (2006) and Schnabel et al. (2006) and use a broad variety of variables to model the selection of firms into the programme group. An overview of all variables used in our binomial models can be found in Table 4 in the appendix. We present the propensity score estimations there as well (Table 5 in the appendix), displaying various specifications as robustness checks. We find there is a trade-off between the number of control variables included in the model and the number of observations we lose when incorporating more covariates due to item non-response. On the one hand, some studies, for example Strotmann (2006), advocate an intentional overparametrisation of the model to avoid a potential omitted variable bias. This can, however, lead to serious issues when some missing values are not randomly distributed, as Jensen and Rässler (2007), for example, point out for sensitive variables such as turnover or share of intermediates. We therefore carefully check for all our results if they are vulnerable to changes in the specification of the propensity score estimation.

To assess the quality of our propensity score estimations we use standardised bias tests to compare the means of the covariates between programme and control groups before and after the matching (Rosenbaum and Rubin 1985). We perform mean tests for all possible specifications and present them for one of these specifications (Table 6 in

[10] Theoretically, we could compare more than two groups at a time using non-covered establishments as an additional control group. This would imply using a multinomial logit model or an ordered logit model to compute the propensity scores. Caliendo and Kopeinig (2008) discuss this topic in more depth, but the more rigid assumptions of those models would make such an approach quite vulnerable and fragile.

the appendix).[11] The tests include differences in means for all characteristics both before and after the matching, the percentage bias and its reduction, and t-tests with the null hypothesis that the means of the covariates do not differ. They also include mean tests over all characteristics and the reduction in Pseudo-R^2.

After the matching we measure the average treatment effect on the treated observations (ATT), conditioned on the respective propensity scores, to identify the effect of opening clauses on job flows:

$$ATT_{PSM} = E_{P(X)|d=1}\left\{E[Y_1|d=1, P(X)] - E[Y_0|d=0, P(X)]\right\}, \tag{2}$$

where

$$E(Y_0|d=1, P(X)) = E(Y_0|d=0, P(X)). \tag{3}$$

In words: our estimator calculates the mean differences in job flows, Y, between programme and control group (collectively covered firms for whose CBAs opening clauses exist, $d = 1$, or do not exist, $d = 0$), inside the common support, and weighted by the propensity scores, $P(X)$ (Caliendo and Kopeinig 2008). In order for this estimator to be unbiased, it is of central importance that the conditional independence assumption (CIA) holds. The CIA states that systematic differences in outcomes between treated and comparison observations are only attributable to treatment, once controlled for the covariates, X: $Y_0, Y_1 \perp d|X$. We feel assured that the wealth of information in the IAB EP accounts for this crucial assumption.

Three further points are relevant. First, we use control observations multiple times, because in our data the programme group is relatively large, such that it is likely for one control observation to be the closest control observation, with regard to the propensity score, to more than one programme observation.[12] Second, we use different methods to compare programme observations with control observations. This should identify possible trade-offs between unbiasedness and efficiency and serve as a robustness check.[13] Third, due to the choice-based sample design of our data set, we use the odd ratios of the true propensity scores, as proposed by Heckman and Todd (2009).

[11] These tests are carried out with the Stata command *pstest* by Leuven and Sianesi (2003).

[12] This procedure can be critical if there is a bias in areas with only a small number of control variables. This is most likely to occur in the upper bound of the propensity score. We have checked this by using no-replacement as well as by looking at subsamples where this problem does not occur. For example, we do not find significantly different results when restricting firm size to up to 500 employees.

[13] Caliendo and Kopeinig (2008) show that in large data sets the different methods should theoretically yield similar results.

4 Empirical Findings

4.1 Estimation Results

Table 2 presents the effects of the existence of opening clauses on job flows. We present the results for binary OLS in column 2, OLS with covariates in column 3, and ATT in column 4. The upper half of the table shows the results for specification 1 of the propensity score estimation, where we use all covariates available, while the lower half shows the results for specification 4, where we use the most significant covariates and those that do not reduce sample size. In general we find that covariates and selection explain a large share of the differences in job flows between programme and control group. We do not find significant effects for the overparametrised model (specification 1). Instead, we lose 70% of our observations, mainly because firms do not reveal turnover, share of intermediates, or share of flexible workers, as mentioned by Jensen and Rässler (2007). We therefore discuss only the results of specification 4, for which we exclude all critical covariates. We find significant differences in job flows rates, which also have the expected signs. Firms whose CBAs contain opening clauses have 0.96 percentage points lower job reallocation rates, 0.85 percentage points lower job destruction rates, and 0.73 percentage points higher job growth rates. All effects are significant at the 5% level.[14] Only for job creation rates do we not find significant effects of opening clauses. As regards the lower job reallocation rates, it seems that opening clauses increase the matching quality of jobs, which is also indicated by lower job creation rates in the unmatched sample. The insignificant effect on job creation in the matched sample indicates that firms do not anticipate the increased flexibility, i.e. they do not hire a larger number of workers, in contrast to our theoretical expectations. On the other hand, the much lower job destruction rates support the notion that opening clauses fulfil their central objective of giving firms more flexibility to save jobs in harsh economic times. Taken together, the insignificant effect on job creation and the negative effect on job destruction lead to higher job growth rates in firms whose CBAs have opening clauses.

We further analyse the anticipation hypothesis using the IAB information on firms that know about opening clauses. In the upper part of Table 3, we do not possessing explicit knowledge about opening clauses to have any significant effects on job flows in the matched sample. We can observe significantly lower job reallocation, job creation,

[14] As Imbens (2004) points out, standard errors in propensity score matching should incorporate the variance from the selection estimation as well and be restricted to the common support, and are therefore underestimated. This can be dealt with an using approximation method (Lechner and Pfeiffer 2001) or by bootstrapping standard errors. However,Abadie and Imbens (2008) show that bootstrapping is not valid in nearest neighbour matching. We therefore use kernel matching and bootstrap our standard errors with 200 replications. For our calculations we use the commands *pscore* by (Becker and Ichino 2002) and *psmatch2* by (Leuven and Sianesi 2003).

Table 2: Existence of Opening Clauses: Effects on Job Flows

Variable	**Pooled OLS, no covariates**	**Pooled OLS, covariates**	**ATT**	N. of Obs
Specification 1				
Job Reallocation Rate	-.0220***	-.0080*	-.0064	4124
	(.0038)	(.0045)	(.0051)	
Job Creation Rate	-.0075***	-.0021	-.0019	
	(.0029)	(.0034)	(.0038)	
Job Destruction Rate	-.0145***	-.0059*	-0.0045	
	(.0031)	(.0035)	(.0041)	
Job Growth Rate	.0070	.0037	.0026	
	(.0046)	(0.0051)	(.0061)	
Specification 4				
Job Reallocation Rate	-.0233***	-.0109***	-.0096**	13140
	(.0025)	(.0031)	(.0031)	
Job Creation Rate	-.0067***	-.0022	-.0011	
	(.0017)	(.0020)	(.0021)	
Job Destruction Rate	-.0165***	-.0087***	-.0085**	
	(.0021)	(.0025)	(.0027)	
Job Growth Rate	.0098**	.0065**	.0073**	
	(.0030)	(0.0033)	(.0037)	

Note: OLS: cluster robust standard errors in parentheses; ATT computed using Kernel Density Matching; T-statistics for ATT computed using bootstrap (200 replications).
Source: IAB Establishment Panel and IAW Data Set on Opening Clauses, own calculations (controlled remote data access via FDZ).

and job destruction rates in the unmatched sample, but these effects almost completely wither once controlled for covariates and selectivity. As regards job growth rates, the effect increases (and changes the sign), but stays insignificant. These results indicate that explicit knowledge of opening clauses does not affect firms' job flows. Our theoretical considerations suggesting firms that explicitly know about opening clauses anticipate their increased flexibility and therefore increase hiring cannot be supported. Either firms already have enough flexibility – for example, because they pay wages above the bargained wage – or they do not gain more flexibility from opening clauses because they are forced to instantly apply them.

With regard to firms that apply opening clauses, we do not find significant effects either, as the lower part of Table 3 shows. Again, the covariates and selection into programme and control group explain many of the differences found in the unmatched sample, except perhaps with regard to job growth rates. We can therefore conclude that applying opening clauses does not support the positive effect on job growth which we find for the existence of opening clauses. As we know from Heinbach and Schröpfer (2007), the application of opening clauses in a firm is negotiated with the works council, the staff, or the responsible union. We also know from Hübler (2005) that employment pacts, which are also negotiated at the firm level, often restrict laying off employees. It could be the case that the application of opening clauses is also bound to such restrictions, such that

Table 3: Knowledge and Application of Opening Clauses: Effects on Job Flows

Variable	Pooled OLS, no covariates	Pooled OLS, covariates	ATT
Knowledge of opening clauses, specification 4			
Job Reallocation Rate	-.0275***	.0003	.0013
	(.0044)	(.0038)	(.0053)
Job Creation Rate	-.1389***	-.0048*	-.0030
	(.0036)	(.0044)	(.0038)
Job Destruction Rate	-.0136**	.0051	0.0044
	(.0036)	(.0033)	(.0044)
Job Growth Rate	-.0002	-.0095**	-.0075
	(.0053)	(0.0049)	(.0063)
Application of opening clauses, specification 4			
Job Reallocation Rate	-.0309***	.0007	-.0001
	(.0055)	(.0043)	(.0048)
Job Creation Rate	-.0134**	-.0062*	-.0057
	(.0035)	(.0030)	(.0033)
Job Destruction Rate	-.0074	.0069*	.0056
	(.0045)	(.0039)	(.0041)
Job Growth Rate	.0059	-.0130**	-.0013*
	(.0065)	(0.0054)	(.0077)
N. of obs.			3210

Note: OLS: cluster robust standard errors in parentheses; ATT computed using Kernel Density Matching; T-statistics for ATT computed using bootstrap (200 replications).
Source: IAB Establishment Panel, own calculations (controlled remote data access via FDZ).

opening clauses cannot affect employment directly, but only indirectly. Unfortunately, the data does not allow us to investigate this topic further.

Our results show that covariate effects and selection bias are very important in explaining the effects of opening clauses on job flows. We can also identify significantly negative effects of the existence of opening clauses on job destruction rates, which are in line with our theoretical considerations and the main policy objective of opening clauses. We do not find an anticipation effect if firms know about opening clauses. The application of opening clauses also has no additional significant effects on job flows.

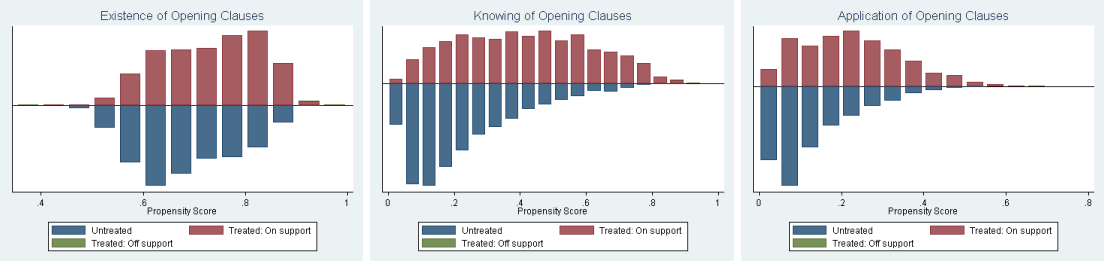
4.2 Robustness Checks

We are aware of the fact that belonging to a programme group itself can have effects on the counterfactuals, i.e. the control group. The decision of a firm to bargain collectively might have an influence on its neighbouring firm (either inciting it to also bargain collectively or to react in a different manner). This is not the case for opening clauses. For some reason, opening clauses are not of great importance for most firms, as the results of Kohaut and Schnabel (2007) show. In addition, they are of limited magnitude and time (Heinbach and Schröpfer 2007). Finally, contrary to a firm's decision to bargain collectively, the

decision to introduce opening clauses is effectively made by unions.[15] This decision is mainly driven by politics and industry characteristics, for example a sector's vulnerability to shocks and the average need for flexibility. Hence, we do not expect the existence of opening clauses to have an effect on the counterfactuals, especially not at the firm level. Furthermore, one firm's knowledge of the existence of opening clauses cannot be observed by its competitors. Consequently, we also do not think that knowing about opening clauses has an effect on the counterfactuals. However, the decision to apply opening clauses is made by the firm (not by the union) and can be observed by competitors. However, as we know from Heinbach and Schröpfer (2007), for example, firms cannot decide on this matter on their own. They have to consult the responsible union, works council, or the staff.

In order to ensure the robustness of our estimations, we use different subsamples, namely excluding large firms (as mentioned above) and using firms that face restructuring or that have recently changed their collective bargaining regime. We also match firms conditioned on their (two-digit) industry affiliation, their firm size class, and a corresponding business outlook, because all these covariates have been shown to determine job flows (before). We find our estimates to be robust against these alterations.

Figure 1: Distributions of the Propensity Scores and Common Support.



Note: Propensity Scores are estimated according to specification (4). Observations in the programme group are depicted in the upper half of each figure, observations in the control group in the lower half.
Source: IAB Establishment Panel and IAW Data Set on Opening Clauses, own calculations (controlled remote data access via FDZ).

Figure 1 displays the distribution of the propensity scores. We can observe that there is a wide range of common support for all variables. For all groups there are very few programme observations that lie outside the common support, which also holds over various specifications of the probit model. As regards regions of the propensity score where there are relatively few programme or control observations for one group in comparison to the other group, respectively, we check the sensitivity of our results in this respect. We estimate the propensity scores on subsamples, where observations which probably have

[15] In fact, it is also made by the respective employers' association, but we assume employers are usually in favour of more flexibility.

very high propensity scores (such as large firms) are excluded. This does not significantly change our ATT estimations. We also use no-replacement-1-to-1 matching, where control observations are only used once and find that our results are robust.

As a more general robustness check, we present OLS estimates, controlling for the same covariates as in our propensity score estimation along our ATT estimates. Recent work by Pfeifer (2009) shows that sometimes the differences in coefficients between the two methods are fairly small, and therefore advocates opting for the more simple method, OLS. In our case, Table 2 shows that the coefficients are indeed different, such that we conclude that there is selection bias and OLS is not appropriate to use.

5 Conclusion

In this paper we study the impact of opening clauses on job flows in German establishments. We are interested in whether opening clauses lead to higher job creation and lower job destruction levels as a result of greater flexibility for collectively covered firms, and overall to higher job growth, because this is the intention of the social partners that implemented them. After laying out theoretical considerations about how opening clauses can influence job flows, we explain the institutional setting in Germany, especially the legal foundations of opening clauses. We present our data and illustrate the importance of adding supplementary information on opening clauses. We see that opening clauses are widespread, but also that there is still an information problem on the firm side. Descriptive statistics further indicate that job flows differ between firms with and without opening clauses. Because of possible selection bias, we decide to use propensity score matching to identify causal effects.

We lay out our empirical strategy and show that there are indeed selection effects that might bias OLS estimation. We find that our theoretical assumptions can be partly confirmed. The existence of opening clauses has significantly negative effects on job reallocation and job destruction and significantly positive effects on job growth. Firms whose CBAs have opening clauses have on average .073 percentage points higher job growth. However, we find that explicit knowledge of opening clauses or their application has no additional effects and, moreover, that there is no effect of opening clauses on job creation. Hence, firms do not seem to anticipate their increased flexibility. We discuss the robustness of our estimates and find that they are robust to most alterations of the econometric procedure, except overparametrisation.

Our research contributes to the literature on labour market institutions and their impact on job flows. It is important to recognise that the German system of industrial relations has changed during recent decades such that it provides firms with enough

flexibility through many different measures. In addition, we think that opening clauses can be an effective tool to reduce job destruction and increase job growth. We can therefore only advise that their range and magnitude should be expanded and that firms should be made more aware of this valuable means of saving existing jobs and creating new ones.

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A Appendix

Table 4: Operationalisation and Summary Statistics of Covariates

Variable	**Operationalisation**	Observations	Mean	Std. Dev.	Minimum	Maximum
Firm size	Dummy variables for 5 firm size classes					
	alternatively: logarithmised employment, logarithmised business volume	123,336	3.38	1.84	0	10.83
	additionally: logarithmised employment and logarithmised business volume squared	123,336	14.89	14.33	0	117.29
Labour productivity	Logarithmised (turnover - intermediates) divided by number of employees	99,682	11.43	1.15	0	19.28
Capital productivity	Dummy variable, technical condition of assets (1 new, 0 old)	10,7021	.66	.47	0	1
Value Creation	Share of intermediates on turnover	83,021	.49	.23	.01	3.6
Investment activity	Share of investments on turnover	12,336	.30	.42	0	1
	alternatively: Dummy variable (1 investments made, 0 no investments made)	122,633	.66	.47	0	1
Export activity	Share of exports on turnover	123,336	.05	.16	0	1
	alternatively: Dummy variable (1 positive exports, 0 zero exports)	123,336	.69	.46	0	1
Business outlook	Dummy variable (0 poor to mediocre business outlook, 1 good or very good business outlook)	87,748	.33	.47	0	1
Turnover outlook	Index variable (1 risen turnover, 2 stagnated turnover, 3 fallen turnover)	113,115	2.00	.70	1	3
Employment outlook	Index variable (1 risen employment, 2 stagnated employment, 3 fallen employment)	112,928	2.04	.55	1	3
Insourcing activity	Dummy variable (1 yes, 0 no)	122,283	.03	.17	0	1
Outsourcing activity	Dummy variable (1 yes, 0 no)	123,336	.04	.20	0	1
Wage level	Logarithmised firm wage sum divided by number of employees	107,065	7.41	.68	2.19	10.59
Paying more than the bargained wage	Dummy variable (1 yes, 0 no)	67,431	.38	.48	0	1
Qualification levels	Share of skilled employees	123,336	.59	.28	0	1
	Share of highly skilled employees	123,336	.16	.21	0	1
	Share of female employees	123,128	.42	.30	0	1
Work council	Dummy variable (1 work council exists, 0 does not exist)	121,068	.40	.49	0	1
Employment Pact	Dummy variable (1 employment pact exists, 0 does not exist)					
Flexible employment	Share of flexible employees	123,336	.08	.15	0	1
Training	Share of apprentices and trainees	123,321	.05	.09	0	1
Hiring activity	Dummy variable (1 vacancies, 0 no vacancies)	123,218	.20	.40	0	1
Subsidiary	Dummy variable (1 independent, 0 subsidiary)	120,992	.70	.45	0	1
Legal form	Dummy variable (1 publicly listed, 0 otherwise)	121,690	.69	.46	0	1
Otrigin of firm	Dummy variable (1 spin-off, 0 otherwise)	123,336	.34	.47	0	1
Foreign ownership	Dummy variable (1 in foreign ownership, 0 otherwise)	118,120	.05	.22	0	1
Public ownership	Dummy variable (1 in public ownership, 0 otherwise)	118,120	.10	.30	0	1
Firm age	Dummy variable (1 younger than 1990, 0 older than 1990)	123,336	.41	.49	0	1
	alternatively: Age in years up to 18	121,690	14.21	5.28	0	18
Craft	Dummy variable (1 member of a craft association, 0 not member)	113,362	.25	.43	0	1
Industry	Dummy variables for 40 different industries					
Region	Dummy variables for 10 different regions					
Year	Dummy variables for all years following 2000					

Table 5: Propensity Scores for Existence of Opening Clauses: Probit Estimation

Variables	(1)	(2)	(3)	(4)
Log of firm size	0.0193	-0.0823	0.0137	-0.0779
	(0.1118)	(0.0658)	(0.0715)	(0.0647)
Log of firm size squared	0.0040	0.0134*	0.0066	0.0141**
	(0.0120)	(0.0072)	(0.0077)	(0.0071)
Log of turnover per employee	0.0673			
	(0.0620)			
Share of intermediates	0.3204*			
	(0.1657)			
New firm	-0.2249	-0.2277		
	(0.1988)	(0.1459)		
Firm age	-0.0200	-0.0157	-0.0032	0.0009
	(0.0169)	(0.0116)	(0.0048)	(0.0042)
Single firm	-0.0469	-0.1066*		
	(0.0913)	(0.0551)		
Legal form	0.0407	0.1059*	0.1786***	0.0999*
	(0.0924)	(0.0552)	(0.0596)	(0.0547)
Foreign ownership	-0.0631	0.0058		
	(0.1274)	(0.0818)		
Public ownership	1.3526***	1.1886***	0.9076***	1.2251***
	(0.4144)	(0.2237)	(0.2478)	(0.2238)
Share of export	0.1796	0.3532***	0.3959***	0.3475***
	(0.2018)	(0.1238)	(0.1391)	(0.1209)
Investment activity	0.0329	0.0396		
	(0.0683)	(0.0387)		
Technology	0.0909		0.0516	
	(0.0624)		(0.0435)	
Business outlook	0.0639		0.0939**	
	(0.0564)		(0.0408)	
Turnover outlook	0.0189		-0.0322	
	(0.0347)		(0.0246)	
Employment outlook	-0.0688		-0.0993***	
	(0.0484)		(0.0338)	
Hiring activity	0.0400	0.0757*		
	(0.0700)	(0.0399)		
Share of women	0.2393		0.2899**	
	(0.2120)		(0.1326)	
Share of flexible workers	0.1410	0.5360**		
	(0.3849)	(0.2086)		
Share of skilled workers	-0.3413**	-0.1515	-0.1367	-0.2081**
	(0.1587)	(0.0935)	(0.1065)	(0.0895)
Share of highly skilled workers	-0.7065**	-0.1934	-0.2749	-0.2807
	(0.3077)	(0.1887)	(0.2203)	(0.1847)
Share of trainees	-0.4457	-0.0402		
	(0.4186)	(0.2362)		
Works council	0.1879*	0.0693	0.1213*	0.0669
	(0.1015)	(0.0588)	(0.0679)	(0.0575)
Log of wage sum	-0.1501*		-0.2497***	
	(0.0799)		(0.0455)	
Craft	-0.0738	-0.1382***	-0.1870***	-0.1559***
	(0.0844)	(0.0510)	(0.0563)	(0.0496)
Paying more than the bargained wage	-0.0197	-0.0482		
	(0.0624)	(0.0388)		
Employment pact	0.1723			
	(0.1140)			
Constant	1.2091	1.0100***	2.4601***	0.7007***
	(0.8102)	(0.2696)	(0.3859)	(0.1599)
N. of obs.	4124	12931	7498	13140
Likelihood	-2274.65	-7356.09	-4309.90	-7481.59
Chi^2	111.62	158.37	215.25	144.06
Pseudo R^2	0.06	0.04	0.05	0.04
AIC	4605.29	14750.19	8653.81	14985.19

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: IAB Establishment Panel and IAW Data Set on Opening Clauses, own calculations (controlled remote data access via FDZ).

Table 6: Balancing Test and Matching Quality for Existence of Opening Clauses: Specification (1)

Variable	Sample	Mean Programme Group	Mean Control Group	% Bias	% Reduction	T-Test t	T-Test p>|t|
Log of firm size	Unmatched	4.2375	3.5836	35.8	19.14	0.000	
	Matched	4.2272	4.1594	3.7	89.6	1.47	0.142
Log of firm size squared	Unmatched	21.65	15.835	36.8	19.29	0.000	
	Matched	21.171	20.31	5.5	85.2	2.08	0.038
Log turnover per employee	Unmatched	11.776	11.621	19.6	10.06	0.000	
	Matched	11.797	11.78	2.2	88.9	0.83	0.404
Share of intermediates	Unmatched	.52004	.48487	17.2	8.71	0.000	
	Matched	.51462	.51916	-2.2	87.1	-0.87	0.386
New firm	Unmatched	.16883	.19528	-6.9	-3.80	0.000	
	Matched	.16091	.15996	0.2	96.4	0.10	0.921
Firm age	Unmatched	16.232	15.986	5.5	3.04	0.002	
	Matched	16.338	16.314	0.5	90.3	0.22	0.825
Single firm	Unmatched	.6739	.7902	-26.5	-14.01	0.000	
	Matched	.71172	.72343	-2.7	89.9	-1.00	0.318
Legal form	Unmatched	.74734	.6644	18.3	10.14	0.000	
	Matched	.75949	.75515	1.0	94.8	0.39	0.698
Foreign ownership	Unmatched	.11857	.07355	15.3	7.93	0.000	
	Matched	.12331	.1253	-0.7	95.6	-0.23	0.816
Public ownership	Unmatched	.01851	.00148	17.2	7.93	0.000	
	Matched	.00542	.00371	1.7	90.0	0.97	0.331
Share of exports	Unmatched	.16394	.09309	30.1	15.84	0.000	
	Matched	.18264	.17946	1.4	95.5	0.44	0.658
Investment activity	Unmatched	.76926	.70215	15.3	8.50	0.000	
	Matched	.81436	.80721	1.6	89.3	0.70	0.483
Technology	Unmatched	.65715	.62296	7.1	3.68	0.000	
	Matched	.66362	.65445	1.9	73.2	0.74	0.458
Business outlook	Unmatched	.34126	.27004	15.5	7.81	0.000	
	Matched	.37093	.35717	3.0	80.7	1.10	0.272
Turnover outlook	Unmatched	1.9587	2.0644	-14.7	-7.81	0.000	
	Matched	1.8984	1.9004	-0.3	98.1	-0.11	0.915
Employment outlook	Unmatched	2.089	2.123	-5.9	-3.07	0.002	
	Matched	2.0257	2.0427	-2.9	50.1	-1.12	0.264
Hiring activity	Unmatched	.272	.19865	17.4	9.28	0.000	
	Matched	.28252	.27843	1.0	94.4	0.35	0.727
Share of women	Unmatched	.22157	.19719	12.4	6.78	0.000	
	Matched	.20855	.21015	-0.8	93.4	-0.34	0.737
Share of flexible workers	Unmatched	.04874	.04966	-0.9	-0.49	0.624	
	Matched	.05009	.05422	-4.0	-345.2	-1.53	0.126
Share of skilled workers	Unmatched	.62425	.6346	-4.5	-2.47	0.014	
	Matched	.62579	.62927	-1.5	66.4	-0.61	0.544
Share of highly sk. workers	Unmatched	.09436	.10893	-10.9	-6.11	0.000	
	Matched	.09833	.09542	2.2	80.0	1.01	0.314
Share of trainees	Unmatched	.05686	.06549	-9.9	-5.62	0.000	
	Matched	.05874	.05888	-0.2	98.4	-0.07	0.942
Works council	Unmatched	.59744	.44604	30.7	16.64	0.000	
	Matched	.58672	.58219	0.9	97.0	0.35	0.724
Log of wage sum	Unmatched	7.7556	7.7109	8.1	4.32	0.000	
	Matched	7.7591	7.7602	-0.2	97.7	-0.08	0.939
Craft	Unmatched	.42622	.59098	-33.4	-17.54	0.000	
	Matched	.43259	.44196	-1.9	94.3	-0.73	0.468
Paying more	Unmatched	.63369	.61103	4.7	2.56	0.010	
	Matched	.66633	.66507	0.3	94.5	0.10	0.919
Employment pact	Unmatched	.09661	.05531	15.6	6.35	0.000	
	Matched	.12568	.10754	6.9	56.1	2.17	0.030
		Mean	Std. Dev.		Pseudo R2	LR chi2	p>chi2
Total	Unmatched	16.1555	10.1483		0.061	297.85	0.000
	Matched	1.8980	1.6410		0.003	24.36	0.610

Source: IAB Establishment Panel and IAW Data Set on Opening Clauses, own calculations (controlled remote data access via FDZ).

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