Voluntary Quits: Do Works Councils Matter? An Analysis of the Reform of the German Works Constitution Act 2001 *

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Abstract. Most of the literature on the effects of German works councils does not deal with the issue of potential endogeneity of works council existence. Exploiting exogenous variation in works council authority stemming from a 2001 reform of the German Works Constitution Act, I apply a regression difference-in-difference using establishment panel data. I find that increasing works council size and the introduction of one full-time councilor causally reduces the number of voluntary quits by about 30 percent. This decline is driven entirely by collective voice effects and there is no evidence for monopoly effects in place. Similar to the findings of previous research, the effect is significant only in establishments which are subject to a collective agreement. The results suggest that the effectiveness of works councils either heavily relies on the support of unions, or that works councils mainly serve as a guardian of collective agreements.

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1. Introduction

Due to search, vacancy and training costs, voluntary quits of employees can entail substantial costs to employers as well as to employees encountering a loss in employer-specific human capital or the threat of unemployment spells in between two jobs. Information asymmetries or a public good problem of enforcing good conditions of work can render separations arising from voluntary quits inefficient, which is why the determinants and causes of voluntary quits have for a long time been attracting the interest of research in economics. Bryant and Allen (2013) emphasize that, contrary to a common belief, remuneration is not one of the most decisive factors of voluntary quits, but rather overall job satisfaction, organizational commitment, the relationship with supervisors and advancement opportunities. The German Institute for Employment Research reports that 84 percent of employees in Germany suffered from deadline pressures, information overload, physical strains or unpleasant work environments at the workplace (Wolter et al.

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2016) and that these conditions increased the likelihood of job change considerations among employees. Especially when the labor market is tight, skilled labor is scarce or during high demand on the product market, the costs of turnover are high and employers have a strong interest in retaining employees. Dietz et al. (2013) find that quits are most prevalent in small and medium-sized establishments. They argue that training opportunities, larger internal labor markets and workplace representation and codetermination lead to relatively fewer quits in larger establishments.

There are various forms of institutions governing the relationship between employers and employees that influence the conditions of work and pay and that are suspected to influence conditions of work and pay, potentially affecting job satisfaction and quit behavior of employees.

This paper considers the reform of the German Works Constitution Act of 2001 which, among others, intended to strengthen the authority of works councils by raising the compulsory minimum number of councilors as well as the number of councilors which are granted paid release from work for certain establishment sizes only. Applying a difference-in-difference research design for the identification of causal effects and using data from the German Establishment Panel from 1998 through 2004, the change in size thresholds determining works council size is shown to have a significantly negative and causal effect on voluntary quits. Disentangling voice and monopoly effects, I find that the reduction in quits is a likely consequence of improved working conditions retaining workers and not a consequence of increased rent redistribution triggered by the enlargement of works councils. I argue, that the findings with respect to the effects of increasing works councils' power are likely to resemble the effects of works council existence and can at least qualitatively be interpreted in that way. In line with previous literature, significant effects are shown to exist only in establishments subject to collective agreements. The contribution to the empirical literature on the effects works councils is twofold. The paper is among the first to analyze the effect of one particular aspect, the enlargement of works councils, of the German 2001 reform of the Works Constitution Act. Moreover, the identification strategy takes into account the potential endogeneity of works council status and accordingly allows a causal interpretation of the findings, which is rare in the existing literature.

After decades of research on the effects of works councils, starting with rather negative evaluations of FitzRoy and Kraft (1985) and FitzRoy and Kraft (1987), in recent decades the pendulum has swung towards a more positive assessment of the economic effects of works councils. Notwithstanding that the empirical literature remains overall inconclusive, what most studies have in common is that they do not allow for a causal interpretation of the effects of workplace representation and codetermination, which is the novelty in this study.

The remainder of the paper is as follows: section 2 presents the institutional background of the German system of workplace representation and codetermination as well as the reform of the Works Constitution Act of 2001. Section 3 discusses theoretical effects of works councils on quits and provides an overview of the findings from previous empirical research. Section 4 describes the data and identification strategy. The estimation results as well as results from various robustness checks are presented in section 5 and section 6 provides a discussion of the results and points at potential limitations of the research.

Section 7 concludes.

2. Works Councils in Germany: Institutional Background

2.1. Employee Representation and Codetermination in Germany

When it comes to the effects of codetermination, the case of Germany is frequently analyzed. There are some distinctive features that make it commonly being considered the archetype of a system of workplace representation and codetermination. Although many southern European and some central and eastern European countries also feature dual systems of employee representation with unions and workplace organizations, the extent to which works councils have codetermination rights in addition to information and consultation rights is very limited and there typically exists a power imbalance in favor of the labor unions. The situation in Germany is characterized by unions, rather than works councils, engaging in collective bargaining, by the works councils' obligation not to engage in industrial action and their far-reaching rights and engagement in a variety of management decisions. In international comparison it appears to be the most comprehensive[1] and formalized system while strongly guided by the principle of cooperation. This seeming contradiction and the complementarity of unions and works councils has for decades been attracting interest in its functioning and the economic consequences. In the following, the institutional background of codetermination and the 2001 reform of the Works Constitution Act will be described.

The German system of worker representation is characterized by its dual, or complementary, structure. At the industry level, unions represent employees' interests by engaging in industrial disputes and participating in the collective bargaining process, which, among others, determines minimum wages for a wide range of sectors. In addition to unions at the industry level, works councils can be founded in every establishment with at least five employees of the age over 18 years, and if at least three of them are employed for a minimum of six months. Once established, there exists no statutory procedure to abolish a works council. The members of the council, called councilors, are elected every four years by the workforce and their number is based on the total number of employees in the establishment and determined by law. Additionally, the Works Constitution Act prescribes a minimum number of councilors who are granted paid leave from work[2], who work for the council on a full-time basis, which also depends on the number of employed persons in an establishment. All costs of council election, initiation and operation are borne by the employer. The tasks of the works councils comprise the initiation of measures serving the interests of employees[3] and the enforcement of regulations laid down in the labor code or collective agreements[4], preventing workplace discrimination, dealing with environmental aspects of the establishment and training measures as well as participating in personnel decisions such as hirings, firings or the transformation of employment contracts[5]. In practice, it serves as a means of individual and collective communication

[1]For a more contextual interpretation of German works councils as a role model for European codetermination, see Addison (2009).

[2]In the following, these will also be referred to as released councilors.

[3]Betriebsverfassungsgesetz (2001), 80, sect. 1(2)

[4]Betriebsverfassungsgesetz (2001), 80, sect. 1(1)

[5]Betriebsverfassungsgesetz (2001), 80, sect. 1(7) and sect. 1(9)

between employees and employers.[6]

The influence of works councils stems from the comprehensive statutory rights, which comprise information rights, consultation rights and codetermination rights. Works councils are granted comprehensive information rights which means that, with few exceptions, all information that is required for the work of the councils has to be provided either on a regular basis or upon request. These, among others, can comprise data on employees, information on hirings and firings, trainings, labor and environmental protection and works alteration. Consultation rights grant the works councils the opportunity of consulting with the employer. The codetermination rights are the most powerful tool works councils have in representing the employees' interests towards the management. The most important of these are the actual right of codetermination, the right of objection and the right to refuse consent. It should be noted that councils in practice have two ways of exerting power and influence: they can object on a case-by-case basis, depending on the respective issue, or they can arbitrarily oppose the management's plans in order to achieve concessions in future affairs or unrelated matters as well as simultaneously negotiate a variety of different aspects or measures in so-called bundling agreements.

Despite the frequently noticed decline in the coverage of works councils and collective agreements in the recent decades, in 2000, according to Schnabel and Kohaut (2003) still about 71% (55%) of all employees in West (East) Germany worked in establishments subject to a collective agreement and 54% (47%) in establishments with a works council, as Addison et al. (2003) state.

2.2. The Reform of the Works Constitution Act of 2001

The existence of work councils in Germany dates back to the Weimar Republic, when the first law governing the foundation of work councils in establishments with more than 20 employees was passed after a period of ongoing industrial conflict in 1920. Under the Nazi regime all forms of organization of workers, unions as well as work councils, were prohibited. After WWII, the Works Constitution Act (*Betriebsverfassungsgesetz, BetrVG*) was passed in 1952. It comprised the regulations regarding the initiation of councils, procedural regulations and rights and duties of works councils and employers. After a reform in 1972, the last major amendment of the Works Constitution Act took place in 2001[7] and came into effect in July 2001. The reform intended to strengthen the role of works councils and to adapt workplace codetermination to a modern labor market and works environment as well as to the emergence and proliferation of irregular forms of employment like agency work.

One component of the reform was the simplification of the council election procedure by lowering the requirements for the initiation of a works meeting which can decide on the foundation of a works council. This was intended to encourage the formation of councils especially in small and medium-sized establishments in which works councils rarely exist. The foundation of company-wide works councils embracing multiple establishment works councils was also simplified. Furthermore, the right to vote in the council elections was extended to agency workers with a tenure of at least three months in the respec-

[6]For a more detailed description of the role, rights and responsibilities of works councils in Germany,see Addison (2009), chapter 2.

[7]For the full text, see Bundesgesetzblatt (2001) (in German language).

tive establishment.[8] In order to prevent the excessive use of service contracts which hire workers for specific tasks on a fixed-term basis and are not subject to social security payments, the obligatory consent of the works council to such hirings was introduced. The law also intended to stipulate worker participation in the council work by forcing works councils to take up issues that are supported by at least five percent of the workforce. Secondly, the tasks of the works councils were complemented by matters of environmetal protection, promotion of work-life balance, and prevention of racism. Furthermore, the reform act obliged the management to consider job security measures proposed by the works council. Another important change concerned the composition of works councils: the amended law mandates that the minority sex in the establishment needs to be represented at least proportionally to its share in the workforce in councils with at least three councilors.

Finally and most important for this research, the amendment did not only strengthen the power of works councils qualitatively by giving it more responsibilities and rights, but also quantitatively by increasing the total number of councilors as well as the number of councilors which need to be released from work. This change did not apply to all establishments, but only to establishments with certain sizes of the workforce and the arbitrariness of the threshold values gives rise to the identification strategy described in section 4. Table 1 presents the number of councilors and releases before and after the reform.[9] Whereas the size of works councils increased by two in the affected establishments, the number of paid releases was raised by one for selected establishment sizes.[10] Establishments with 201 to 300 employees were not only affected by the increase in the number of councilors, but they were also required to grant paid leave to one councilor for the first time, whereas before the reform the threshold for the first release was 300 employees. Figure 1 and 2 show the prescribed minimum number of councilors and released councilors depending on the number of regularly employed persons before and after the reform. The dark bars reflect the situation before the amendment in 2001 and the gray shaded parts reflect the increases in the number of councilors and councilors with paid leave.

Whereas the increase of works councils was supposed to happen after the forthcoming universal council elections in 2002, additional releases were supposed to happen immediately after the amendment came into effect in July 2001. Although the parliamentary debates took place only in April 2001 after the Federal Government had published the draft bill, reforming the Works Constitution Act has been a recurring issue in the political discussion during the years preceding the amendment. The intensive discussion was initially triggered by a publication of a proposed draft bill of the Works Constitution Act by the German Trade Union Confederation (*Deutscher Gewerkschaftsbund - DGB*), see DGB (1998). Following this initiative, other confederated union organizations published competing drafts and thus, fueled the debate on modernizing the works constitution.

[8] The reason for this provision is that these workers are employed legally by temporary work agencies whose potential works councils cannot exert influence with respect to the working conditions in the hiring establishments.

[9] It also shows the numbers stemming from a proposal of the German Trade Union Confederation of 1998. The comparison suggests that the law was modeled closely after this proposal.

[10] The reason why the number of councilors was increased by an even number is due to the fact that works councils always consist of an odd number of councilors in order to avoid ties in simple majority votings.

3. Effects of Works Councils on Quits: Theory and Evidence

Works councils represent all employees in an establishment and can theoretically act as monopolists of labor by raising wages above the competitive level. In the German institutional setting, where unions and works councils are complementary institutions with different responsibilities, unions engage in industry level collective bargaining and wage agreements, if applicable. However, it is conceivable that works councils generate rents for workers in a similar manner as unions do. Addison et al. (2001) find that the existence of works councils is associated with higher wages and lower profitability.[11] However, they also explicitly acknowledge that the councils' rent-seeking behavior is constrained by the collective bargaining process at the sectoral level. Using the IAB Establishment Panel, Bellmann and Kohaut (1999) find a significantly positive relationship between works council status and per-capita wages in East Germany only. Jirjahn (2003) finds a positive association of works council existence and wages, the effect being stronger for establishments not subject to collective agreements. It should be noted that not considering collective agreements may lead to spurious results and conclusions, since the existence of works councils and collective agreements are correlated and accordingly the effect of the latter might be erroneously attributed to works councils. Despite the fact that most theoretical considerations suggest a positive association between works councils and wages, the opposite may also occur, for instance during downturns, when wage cuts are necessary to secure the continuation of the firm and employment. In such a situation, works councils may lower the workers' resistance to wage cuts by means of mediation.[12]

As Freeman and Medoff (1979) argue, the range of potential effects of worker representation is not limited to monopoly effects. In the context of employee-employer communication and potentially beneficial effects of worker representation on firms, Freeman and Medoff (1979) take up the phrase of collective voice or institutional response (view), of which the term voice dates back to Hirschman (1970), who used it in the context of customer (dis-)satisfaction. In the collective voice framework, unions or works councils provide workers with a means of expressing needs, dissatisfaction and preferences and may prevent workers from choosing their exit option.[13]

Consequently, the term voice effect refers to changes in establishment and employment outcomes related to communicating and enforcing workers' non-monetary preferences and proposals for organizational restructuring, whereas monopoly effect denotes a distribution of rents from employers to employees effected by workplace codetermination. These are the definitions which most closely follow the meaning in Freeman and Medoff (1979). It is also necessary to regard the two concepts in terms of economic efficiency. In case of monopoly effects only, works councils are inefficient since they lead to higher wages and consequently too low levels of employment. When both effects are at work, the

[11] Higher wages alone are not sufficient to find the monopoly view confirmed, since they could be the effect of a productivity increase potentially induced by works councils, which needs to be controlled for.

[12] Especially when the primary objective of works councils is securing jobs as opposed to generating additional rents for the employees, this is likely to be the case.

[13] Taking into account that working and workplace conditions constitute a public good and are potentially subject to the classical free-rider problem, employees may not individually communicate their needs and preferences towards the management. As in the classical free-rider problem, the individual reluctance to openly engage in an improvement of working conditions will result in a suboptimal provision of these. Moreover, individual critique or suggestions might be perceived as a risk to the individual, increasing the probability of dismissal or other negative consequences.

effect on efficiency depends on the relative size of monopoly and voice effect. If there are only voice effects in place, works councils are plausibly pareto-improving, by mitigating the negative consequences of market failure due to the public good problem of working conditions and of the employment contract being "necessarily and inherently incomplete as well as unaviodably controversial" (Frick 1996: 409).

A reduction in quits results in lower search and vacancy costs which increases productivity and does not require firm-specific training of new workers which would temporarily reduce productivity. To the degree that the collective voice view applies, Freeman and Medoff (1979) argue that the detrimental effects of works councils on profitability caused by the monopoly status of works councils or unions may be partially, fully or more than fully offset by the works councils' role as collective voice, preventing voluntary quits and fostering productivity. If the two views and effects are considered jointly, it remains ambiguous whether works councils are beneficial or harmful to firm performance and this theoretical ambiguity accordingly calls for an empirical assessment. The effect on quits is less ambigious, since monopoly and voice effects are both expected to reduce voluntary quits.

Sadowski et al. (1995) find that works councils are associated with a 2.4 percentage points lower rate of voluntary quits, which is of a similar order of magnitude as the 1.6 percentage points Frick (1996) reports. Since they do not report the average quit rate in their sample, the relative size of the effect cannot be evaluated. Nevertheless, even for an unusually high quit rate of up to 10 percent, the effect appears to be substantial in magnitude. Using data from the Socio-economic Panel, Grund et al. (2016) find that works councils are associated with a 1.2 percentage point lower quit rate, which corresponds to a 27 percent difference in voluntary quits between establishments with and without a works council, respectively. Pfeifer (2007) reports that the existence of a works council is associated with 20 percent fewer voluntary quits, whereas the difference is 30 percent when works councils and collective agreements co-exist. Boockmann and Steffes (2010) show that works councils are associated with longer job duration of male workers, with a decrease in the job exit hazard of more than 20 percent. Applying a matching approach using data from the IAB Establishment Panel, Addison et al. (2004), do, however, not find a statistically significant causal effect of works councils on quits.[14] Hirsch et al. (2010) find that works councils are associated with a separation rate which is 1.5 percentage lower than in the absence of works councils, which in turn corresponds to 13 percent. Disentangling monopoly and voice effects, they confirm the existence of the monopoly effect for all types of workers, whereas the voice effect is heterogenous across different kinds of workers and only significant for male workers with low tenure. Both Addison et al. (2004) and Hirsch et al. (2010) use the separation rate as a measure for voluntary quits. The separation rate also includes dismissals, exits to retirement and deaths and therefore, the effect on voluntary quits is likely to be different from the effect reported. Pfeifer (2011a) provides some additional descriptive evidence in favor of the monopoly view of works councils. Distinguishing between works councils with different degrees

[14]They compare establishments in which a works council has been founded with establishments without a works council for the years 1996-2000. Since the initiation of a works council is a relatively rare event, their analysis is based on approximately 30 pairs of observations and the insignificance of all coefficient estimates might be caused by the small sample size. The insignificant estimate of the effect of works councils on the quit rate is positive, though.

of consensuality, statistically significant rent-sharing effects are reported. It should be noted that, except for the the paper of Addison et al. (2004), the designs of the studies mentioned do not allow for a causal interpretation of the effects. Since works councils are not automatic, i.e. they need to be initiated by the workforce, their existence cannot be considered exogenous. To the degree that the initiation of a works council is correlated with establishment-specific unobservable characteristics that simultaneously affect voluntary quits, this endogeneity problem may lead to a biased estimation. Furthermore, the examined literature looks at the effect of the mere existence of works councils, but not the size, composition or number of full-time councilors that potentially determine the assertiveness and power of councils and the size of the effects accordingly.

From the theoretical considerations and the empirical findings, the following hypotheses with respect to voluntary quits of workers can be derived:

> Hypothesis 1: The release of one councilor reduces the number of voluntary quits in establishments.

This hypothesis is based on the monopoly and collective voice view of works councils. Both rent-generating and working condition-improving councils potentially reduce the number of quits. It is assumed that the release of councilors increases the assertiveness and the authority of works councils, particularly in case of the first release. A negative causal effect on voluntary quits does, however, not imply that both effects are at work or that both effects are of equal magnitude, from which the second hypothesis is derived:

> Hypothesis 2: In the presence of collective agreements concluded by labor unions, the negative effect of works councils on voluntary quits is mainly driven by the voice effect.

As mentioned before, in Germany collective agreements are negotiated by labor unions and trade unions. These usually comprise sectoral minimum wages, working time regulations and general working conditions. In this institutional setting, the influence of works councils on wages is likely to be limited and the effect of works councils on voluntary quits is then caused mainly by an improvement of individual- and establishment-, but not of industry-specific working conditions. Notwithstanding, not all firms are subject to collective agreements and the relative size of the effects may differ between firms with and without a collective agreements.

The complementary structure of German worker representation and workplace codetermination further raises interest in the effect of the coexistence of works councils and collective bargaining. Pfeifer (2011b) mentions several conceivable ways of how works councils and membership in a trade union or collective agreements can interact. Firstly, unions may directly support the work of works councils by providing advice or sharing other resources. Secondly, works councils could improve the enforcement of collective agreements. With respect to rent-seeking activities, it is often argued that collective agreements at the industry level reduce the degree of distributional conflicts at the establishment level. The industry level of collective bargaining and the limited bargaining power of works councils could lead to a reduction of works council rent-seeking and a greater involvement in productivity-enhancing activities. From this point of view, the interaction of works councils and industry-level collective agreements could reinforce the effects and in particular increase productivity. A number of studies seem to empirically confirm this view with respect to productivity and profits. With regard to fluctuation, Frick and Möller (2003) find that the effect of works councils on personnel turnover is negative throughout, but larger in establishments subject to a collective agreement. Looking at voluntary quits, Pfeifer (2011b) finds a negative effect of works councils exclusively in establishments subject to a collective agreement.

> Hypothesis 3: The effect of works councils on voluntary quits through the monopoly effect is stronger in establishments which are not subject to collective agreements.

This hypothesis follows directly from the consideration that with a collective agreement, most of the conflict about the distribution of rents is carried out at the industry, but not at the establishment level. The willingness or capability of employers to grant rents to employees may also vary considerably conditional upon collective agreements. Taking into account that collective agreements do not only govern the remuneration, but also govern general working conditions, the following hypothesis shall be tested:

> Hypothesis 4: The effect of works councils on voluntary quits through the voice effect is weaker in establishments which are not subject to collective agreements.

The same argument as with respect to hypothesis 3 could apply and works councils be considered as constrained to matters that are not regulated elsewhere, e.g. by collective agreements. To the degree that collective agreements contain employee-friendly regulations regarding non-pecuniary conditions of work, the voice function of works councils might be obsolete. However, taking into account time and resource constraints of works councils, it is also plausible that the absence of wage negotiations at the establishment level allows for more engagement with respect to its voice function. Accordingly, the effect via the voice channel is expected to be more prevalent in establishments with a collective agreement, which is supported by the analysis of the general effects of works councils as found in Frick and Möller (2003) concerning fluctuations and in Pfeifer (2011a) concerning productivity, as well as by the reasoning of Freeman and Lazear (1995). Furthermore, the absence of the distributional conflict at the establishment level may improve the relationship between works councils and management and facilitate the improvement of working conditions through voice.

The multiplicity of plausible arguments with respect to voice and monopoly effects of works councils and its potential interaction with collective agreements once again emphasizes the necessity of an empirical analysis of the causal effects. The causal identification strategy and the data used for estimation are presented in the following section.

4. Empirical analysis

4.1. Data [15]

I am using the IAB Establishment Panel which is, to the best of my knowledge, the only representative and large establishment dataset for Germany. Since 1993 in West and 1996

[15] The data analyzed stems from the IAB Establishment Panel, waves 1998-2004. The data was accessed on-site at the Research Data Centre of the Federal Employment Agency at the Institute for Employment Research. The research is registered under the project number fdz1348.

in East Germany, respectively, it is annually conducted for the Institute of Employment Research (IAB) of the German Federal Employment Agency and the reference day for reporting is June 30. It consists of 8000 to 16000 observations of establishments per year which are randomly selected from the Federal Employment Agency's register of employment. The sample is disproportionately stratified across industries, federal states and establishment sizes. Large establishments are overrepresented, which makes it particularly feasible for the analysis of the policy change with respect to medium-sized and large establishments.[16] The Establishment Panel mainly focuses on topics of employment, personnel policies and training, participation in active labor market policies, the overall economic situation, investments, innovations and technological change. In Germany, it is the only representative large-scale employer dataset containing information on the works council status. The sample analyzed consists of all establishments with 151-250 employees with a persistent works council from 1998-2004 and no change in the collective agreement status during this period. Table 2 reports descriptive statistics for the whole sample as well as separately for the chosen treatment and control group. The average number of regularly employed persons is around 199 and the average number of quits in one year is around 2.7, which corresponds to a quit rate of 1.4 percent. Of all 2638 establishments in the sample, 83 percent are subject to a collective agreement and this share does not differ across the treatment and control group.[17]

4.2. Identification strategy

The empirical analysis of the causal effects of works councils bears some methodological pitfalls, one being the potential endogeneity of works council status. Although works councils in Germany are mandatory, they need to be initiated by the workforce and as such they do not exist in all establishments. The concern is that the initiation and consequently the existence of works councils might be a function of establishment-specific unobservable characteristics that also affect the outcome of interest, voluntary quits. In that case, the problem of endogeneity does not allow for a causal interpretation. Methods to identify the causal economic effects of works councils comprise matching techniques, as well as the exploitation of quasi-experimental settings.

The discontinuous changes in the threshold number of employees determining the number of councilors and councilors with paid release from work provide a suitable setting for a quasi-experimental study design. I argue that the effect of increasing works councils leads to an increase of assertiveness and power and shows qualitatively similar effects as the existence of works councils. This is not an inconceivable assumption, considering that councilors often perform their work after regular working time and are subject to time and knowledge or experience constraints. The assumption is more likely to hold for the release of the first councilor from work than for merely increasing the number of councilors or released councilors. Whereas the latter may lead to coordination problems or increased heterogeneity within the councils, potentially jeopardizing assertiveness and consentaneity, the initial release of one full-time councilor is expected to unambiguously strengthen the stand of the works council.

I apply a difference-in-difference (DiD) estimation strategy to analyze the causal effect

[16] For a detailed methodological description, see Kölling (2000) and Fischer et al. (2009).

[17] The share of establishment with collective agreement is somewhat higher than in the population, probably since the sample is restricted to establishments with works councils.

of increasing works councils on voluntary quits. Establishments with 201-250 employees serve as the treatment group. The treatment comprises the first release of a full-time councilor from work[18] in July 2001, as well as an increase in the number of councilors from seven to nine, which happened in the context of the next council elections in the first half of 2002. Establishments with 151-200 employees serve as the control group. As can be seen also from Table 1, these establishments were not affected neither by an increase nor by a release. Other changes induced by the reform, as described in section 2.2 do not threaten the identification, since no other provision affected these two groups in a different manner. That nothing else that could have affected voluntary quits changed at the time of the reform differently for these two groups of establishments, is one of the prerequisites for the validity of the interpretation of the difference-in-difference (DiD) method. The difference in the differential development of voluntary quits between treated and untreated establishments can then be interpreted as the causal effect of the treatment on the outcome, voluntary quits. Another necessary assumption for a correct parametric estimation in the context of this DiD identification strategy is that in the absence of the regulatory change, the number of quits would have followed the same time trend for both groups. The common trend assumption is fundamentally untestable, but the graphical evidence in Figure 3 roughly supports the claim of common trends in the unconditional outcome variable for the selected treatment and control group. Moreover, there is no reasonable explanation for a potential violation of the common trend assumption for the treatment and control group determined by an arbitrary size threshold. The effect uncovered by DiD is the average treatment effect on the treated.

Lechner (2011) lists additional assumptions that are required in order to render the DiD identification strategy internally valid. Besides the assumption that the treatment does not affect the general equilibrium or untreated establishments other than directly by means of the treatment, which Manski (2013) labeled the individualistic treatment response assumption, exogeneity of the treatment is required. Exogeneity is usually assumed in the context of legislative changes and if the possibility or incentives for self-selection into treatment and control group are not affected by the change. One way to rule out self-selection is by comparing transitions between treatment and control group before and after the time of the treatment. Such a comparison does not reveal an increase or decrease in the number of threshold transitions after the reform. Furthermore, self-selection into the control group, e.g. by dismissals and deferred or suspended hirings, is most likely to occur in case of establishments with a number of employees close to the threshold.[19] Taking this into account, the robustness checks presented in chapter 5 contain the results from an analysis for a subsample, that does not comprise establishments close to the threshold of 200 employees. Finally, it needs to be ruled out that there is an effect of the treatment on the pre-treatment outcome. If this assumption is violated the DiD method is still applicable, however, some treatment effect might be observed prior to the actual treatment, potentially as an anticipation of expected better working conditions or higher wages.

[18]The reform also introduced the possibility of releasing several councilors from work on a part-time basis. Unfortunately, the data at hand does not contain information on actual full-time or part-time releases and there is no information on the extent to which this is used in practice. However, it is plausible that the release of two part-time councilors has similar effects as the release of one full-time councilor.

[19]This is due to the cost of deviating from the previously optimal number of employees. The closer the number of employees is to the treatment threshold of 200 employees, the more likely will the anticipated expected cost of an increased works council exceed the cost of deviating from the original stock of labor.

The basic specifications of the regression equation are

$$y_{it} = \boldsymbol{X_{it}\beta} + \delta_1 treat_i + \delta_2 post_t + \gamma treat_i \times post_t + \eta_i + \epsilon_{it} \tag{1}$$

$$\begin{aligned} y_{it} =& \boldsymbol{X_{it}\beta} + \delta_1 treat_i + \sum_{t=1998}^{2000} \zeta_t year_t + \sum_{t=2002}^{2004} \zeta_t year_t \\ &+ \sum_{t=1998}^{2000} \gamma_t year_t \times treat_i + \sum_{t=2002}^{2004} \gamma_t year_t \times treat_i + \eta_i + \epsilon_{it} \end{aligned} \tag{2}$$

where y_{it} is the number of voluntary quits of establishment *i* in year *t*, $\mathbf{X}_{it}$ contains establishment characteristics such as business volume, the total number of employees and industry dummies. In order to account for time-invariant unobserved heterogeneity, the model is estimated with establishment-fixed effects η_i. In equation (1), the coefficient of interest, the treatment effect, is given by γ. Equation (2), instead of using a dummy variable for the time after the reform, $post_t$, contains treatment-year interactions in order to analyze the evolution of the treatment effect during the years after the reform, which not only allows for testing the assumption of no effect on the pre-treatment outcome, but also for the study of the treatment effect over time.

5. Results

Column (1) of Table 3 reports the results from the estimation of the baseline specification. Control variables in the regression comprise the natural log of sales, total number of employees in the establishment a dummy indicating whether the establishment is bound by a collective agreement[20], a dummy for West Germany and nine industry dummys. In the affected establishments, granting paid leave to one councilor and the increased council size reduced the number of voluntary quits by about 0.9. Taking into account the mean of voluntary quits in the treatment group, this amounts to a reduction of 30 percent. The effect is statistically significant at the 1%-level and its magnitude is similar to the one found by Boockmann and Steffes (2010), larger than Hirsch et al. (2010) and presumedly smaller than the result of Sadowski et al. (1995). Note that the above mentioned studies do not study the effect of the 2001 Works Constitution Act reform, but analyze the association between quits or fluctuation with the general existence of works councils, which even more stresses the substantial magnitude of the effect.

Following the approach of Hirsch et al. (2010), column (2) of Table 3 reports the results from a regression including a measure of productivity and wages, the natural log of value added per worker and the wage sum per capita for the month June, in order to account for potential indirect effects of the monopoly mechanism. The authors argue that if productivity and wages are controlled for, the reported coefficient of the treatment effect reflects the mere voice effect and the presence of monopoly effects would lead to a smaller coefficient than in column (1). Since the effect is not smaller than in the baseline model, there is no evidence for monopoly effects leading to a reduction of voluntary quits. Columns (3)

[20] The collective agreement can either be a sectoral agreement or a company wide agreement.

and (4) show the results from the augmented regression distinguishing the treatment effect by collective agreement status. Whereas there is no significant treatment effect found for establishments without a collective agreement, the treatment reduced voluntary quits by approximately one in establishments which are subject to a collective agreement and the effect is significant at the 1%-level in all specifications.[21] In neither establishments with, nor without a collective agreement there is evidence for monopoly effects. This finding is in line with Frick and Möller (2003) and Pfeifer (2011b) who found that the effects of works councils are more pronounced in case of the co-existence of works councils and collective agreements.

Analyzing the course of the treatment effect in more detail, Table 4 presents estimates from a regression that contains treatment-year interactions instead of the interaction of the treatment indicator with a post-reform dummy. Before the reform, all treatment effects are insignificant, which is crucial for the validity of the causal identification of the effects. On average, the treatment reduced the number of quits by 1.1 in 2002 compared to 2001 and the estimated coefficient is statistically significant at the 5%-level. The difference between the effect in 2003 and 2001 is 1.63 and statistically highly significant. The difference between the estimates of the treatment effect in column 5 and 6 is statistically not significant, which suggests that only voice effects are at work. The treatment effect in 2004 is insignificant in all specifications, which provides evidence for a merely temporary effect on voluntary quits. A similar result is described by Grund et al. (2016), who find that the formation of works councils initially increases job satisfaction, but this effect vanishes within not more than five years after formation.[22] If the dependent variable is the quit ratio instead of the number of quits,[23] the results are fairly similar: the treatment reduces the quit ratio by 0.3 percentage points, which corresponds to a reduction of about 25 percent in the mean quit rate of 1.3 percent in the group of treated establishments.[24]

Several robustness checks have been carried out, the results of which shall be discussed in the remainder of the section. One potential source of error could occur when employers misreport the number of employees in the Establishment Panel. Instead of the exact number, respondents may report rounded numbers, which would pose a threat to the chosen identification strategy that is relying on the sharp threshold of 200 employees. The size distribution of establishments appears to be uniform and there is no evidence for bunching at the threshold. Another, more serious, threat is self-selection into control or treatment group. Self-selection into the treatment group is unlikely to occur, since establishments can voluntarily grant works councils a higher number of members or paid leave. Selection into the control group by dismissals or suspended hirings, however, could cause the effects to be overestimated. In order to avoid this potential pitfall, the models have been estimated with a reduced sample exempting all establishments with a number of employees close to the threshold of 200 employees. Tables 5 and 6 report the results from the estimation based on the sample of establishments with 150-195 employees and 205-250

[21]The treatment effect for establishments with a collective agreement is the sum of the coefficients of the variables Treat × Post and Treat × Post × Coll. Agr. Statistics and p-values from an F-test on joint significance are reported in the table notes.

[22]Due to restrictions in the data they use, they cannot observe the exact duration of the reversion of the effect.

[23]That is defined as the ratio of the number of quits in a given year to employment.

[24]The results are reported in Tables 12 and 13.

employees. In addition, Tables 7 and 8 show the results from the estimation based on a sample of establishments with 150-190 and 210-250 employees. The estimated coefficients are significant and of a similar magnitude as before, which suggests the validity of the results.[25] [26] Moreover, a change in the number of transitions between treatment and control group after the report could suggest self-selection induced by the reform. There is no such changed pattern being observed in the data. Table 9 shows the results of a placebo test of specifications (1)-(4) with the treatment being predated to the year 2000. The treatment effect is insignificant, as can be seen from the respective values of the F-statistic.[27]

Since the dependent variable takes non-negative integers and about 30 percent of observations in the sample display zero voluntary quits, in addition to the fixed effects OLS regression, a fixed effects Poisson regression model for count data has been estimated.[28] [29] The effects suggested by the OLS and Poisson fixed effects regressions are virtually the same[30] and also the levels of statistical significance remain almost unchanged. Another issue is the number of employees, which is the variable that assigns establishments to the treatment. According to the Works Constitution Act, this number contains all regularly employed persons in an establishment[31], but working owners are explicitly exempted. The results are insensitive to the small deviations in the size of the workforce caused by regarding working owners. None of the conducted robustness checks puts the validity of the findings into question.

6. Caveats and Discussion

One recurrent concern in the context of granting paid release to members of works councils is the danger of potential non-compliance with the threshold provisions.[32]Apart from some reportedly confidential anecdotal evidence which Koller et al. (2008) mention and

[25]The size thresholds for these subsamples have been chosen in order not to overly drastically reduce the sample size. Apart from that, any self-selection was likely to occur as a result of anticipated cost of increased works councils exceeding costs of deviating from the previous number of employees, which becomes the less likely, the greater the deviation is.

[26]The relevant establishment size refers to the number of employees and does not regard working owners. In order to account for small discrepancies caused by counting working owners as employees when reporting the number of employed persons, I have subtracted these from the size of the workforce in an additional robustness check. The results are robust with repect to this modification.

[27]The same analysis has been conducted for the observation period 1998-2002, and another analysis has been conducted with the reform predated to 1999 for the observation period 1998-2000. In all these analyses, the treatment effects are insignificant. The results of all robustness checks not reported in the appendix are available from the author upon request.

[28]Wooldridge (2010) shows, that the violation of the central assumption of the Poisson regression model, the mean-variance equality, does not lead to an inconsistent estimator for and that inference based on robust standard errors is valid.

[29]The obtained coefficients for the treatment effects are reported in Tables 10 and 11. The detailed results are available from the author upon request.

[30]In order to be able to directly compare the magnitude of the coefficients of the treatment effect, the coefficients need to be multiplied by the conditional mean of the dependent variable. For the sake of an easier interpretation, only OLS results are being reported and discussed in the text of the paper.

[31]This threshold regulation is based on the number of persons, and not based on the number of full-time equivalents.

[32]This regards establishments with a smaller number of council members and releases as well as establishments voluntarily exceeding the minimum requirements specified in the Works Constitution Act.

which is difficult to quantify and to use in empirical research, Mohrenweiser and Backes-Gellner (2010) provide survey-based evidence for a considerable degree of non-compliance of establishments with the provisions of the Works Constitution Act with respect to releases. The authors base their conclusions on a works council survey of the Institute for Research on Small- and Medium-sized Establishments.[33] Taking into account the extremely low response rate of less than seven percent, the risk of selection into response requires caution when interpreting the results.[34] The seeming extent of non-compliance might resemble that responding "good" employers in establishments below the size threshold are more likely to voluntarily grant paid release or works councils may relinquish additional rights granted by the reform act. In that case, the actual degree of non-compliance would be lower than suggested in that survey and emphasized by the authors. Unfortunately, with the data from the IAB Establishment Panel, the extent of non-compliance cannot be quantified and evaluated in this paper. Even if the IAB Establishment Panel contained information on the actual number of released councilors, it remains questionable whether employers would reveal unlawful practices.

Besides intentional non-compliance, there can be other circumstances preventing employers or works councils from complying with the changed works council size and release requirements. Behrens (2003) argues that the adaption of the new regulations may require some time and accordingly compliance may not be immediate. This view could be supported by the significance and magnitude of the coefficients of the treatment-year interactions for the years 2002 and 2003 reported in Tables 4, 6, 8, and 11. However, it is not possible to unambiguously state whether the evolvement of the effects over time is due to delayed compliance or to a delayed effect itself.

The remainder of this section will discuss the findings reported in section 5. Although mostly in line with previous literature, little is known about the determinants of the relative size of voice and monopoly effects and the persistence of the effects of works councils on voluntary quits. One potential explanation for the relative magnitude of the effects reported, especially in comparison with the findings of e.g. Boockmann and Steffes (2010) or Hirsch et al. (2010), who are analyzing the consequences of works council status, is the endogeneity of works council existence. The effect in previous studies is likely to be underestimated if works council existence was negatively correlated with unobserved establishment characteristics that are associated with voluntary quits, for instance an uncooperative management or a bad work environment. Due to these factors, the formation of works councils could be inhibited or workers could be afraid of negative consequences or sanctions in case the formation of a works council is initiated, as Lücking (2006) describes. That could imply that works councils are more likely to exist in good establishments, which could explain the greater magnitude of the effect in this causal analysis, where the treatment as opposed to works council status, is assumed to be exogenous.[35]

Another remaining question refers to the lack of evidence for monopoly effects in this study. It can plausibly be argued that in establishments with a collective agreement distri-

[33] *Institut für Mittelstandsforschung* (author's translation)

[34] Although this is a works council survey, it was sent directly to employers, who were asked to forward it to the works councils, and there might be some extent of employer selection in forwarding or not forwarding the surveys.

[35] However, the opposite argument of works councils being more likely to be founded in bad establishments is conceivable, as well.

butional conflicts are not or only to a lesser extent carried out at the establishment level. Accordingly, the finding that significant treatment effects are observed in establishments with collective agreements only provides additional support to the evidence of exclusive voice effects in this group of establishments, as stated in Hypothesis 2. However, this argument does not explain why no monopoly effects are observed in establishments without collective bargaining. Freeman and Lazear (1995) hypothesize that for this reason works councils in establishments with a collective agreement have more capacities left to deal with non-monetary aspects of the employer-employee relationship and thus, their effect with respect to productivity and other outcomes might be more pronounced, which is the finding in most of the literature. This reasoning could explain why the voice effect was smaller in establishments without collective agreements, but it cannot explain the simultaneous absence of voice and monopoly effects in establishments without collective agreement coverage. The absence of monopoly effects in general is likely to be the result from the dual nature of the German system of employee representation and codetermination that does not assign wage and remuneration bargaining to works councils, but explicitly to unions. Acknowledging that works councils may still behave monopolistically to some extent and attempt to increase remuneration, one can say this behavior was more likely to occur in case of no collective agreement and potentially prevents those works councils from engagement with respect to other aspects like working conditions. This could explain the missing evidence of an effect of the treatment on voluntary quits by the voice mechanism in establishments without a collective agreement.

Alternatively, the actual role of works councils could differ from the classical view that works councils autonomously and actively act as a codetermination organ. If instead the councils acted as the guardian of treaties, in this case collective agreements, the observed difference in effects between establishments with and without collective agreements, respectively, would reflect the effect of a higher degree of enforcement of provisions stemming from collective bargaining. Finally, there may be considerable interactions between works councils and unions. Whether, as well as to which degree works councils and unions are in contact or collaborating, might depend on collective agreement coverage and the significantly negative treatment effect could consequently be the result of union support to works councils, as Pfeifer (2011b) argues. This support by unions can comprise financial means, the provision of expertise and experience as well as the establishment of contacts among works councils and accordingly increase the efficacy of works councils. However, the interactions of the treatment effect with collective agreement status should be interpreted with caution. As has been argued, the causal effect of works councils is assessed by looking at exogenous variation in the number of released councilors. Collective agreements, though, may not be fully exogenous in this analysis.

The insignificant coefficients of the treatment-year interactions for the year 2004 are somewhat puzzling. As can be seen from Table 5, the estimated treatment coefficient is still negative and of considerable magnitude, but statistically not significant. Accordingly, the initially very strong effect appears to decrease over time and there is a reversion of quits to the original level of 2001. This finding is in line with Grund et al. (2013), who find that the increase in employees' job satisfaction following the introduction of works councils is of a temporary nature and vanishes within six years after the introduction, which they attribute to unmet expectations of the workforce in the works council. If the job satisfaction or quitting behavior was driven by expectations rather than by actual actions and achievements of the works council, it would be likely to observe an effect already before the actual change in the Works Constitution Act in 2001, since the reform act has been discussed at length and with increasing intensity since the German Trade Union Confederation (DGB) proposed an amendment in 1998, after which the actual reform act was modeled. The question under which conditions and for which reasons the effects are temporary, is beyond the scope of this paper and is left for future research.

7. Conclusion

In the last decades, researchers have been extensively dealing with the evaluation of the effects of German works councils on the performance of firms and personnel fluctuation. Although the evidence is mixed, especially contemporary studies do not find negative impacts and often find positive effects of workplace codetermination. A caveat of these previous analyses is the neglection of the potential endogeneity of the works council status, which may bias the results and lead to spurious conclusions. This paper is among the first to analyze the effect of authority gains of works councils on voluntary quits. Using a reform of the German Works Constitution Act of 2001 which affected establishments of certain sizes by raising the minimum number of councilors and councilors with paid release, the difference-in-difference identification strategy allows the findings to be interpreted causally. It shows that the causal effect of strengthening works councils is negative, statistically significant and of considerable magnitude. In establishments with 201-250 employees, the legislative change with respect to the size of works councils and paid leave of councilors on average reduced the number of quits by 0.9, which corresponds to a reduction by 30 percent. The magnitude of the effect is in line with previous findings of the effect of works council existence.

Disentangling voice and monopoly channels of codetermination, I provide evidence that the effect on quits is exclusively driven by the collective voice function of works councils and not by productivity-constant wage increases as a result of monopoly effects. Comparing the effect across establishments with and without binding collective agreements reveals that establishments not subject to a collective agreement do not seem to be affected. This is also in line with previous findings of more pronounced effects of codetermination in case of co-existence with collective bargaining.

There is some evidence for a temporary nature of the effect of the legislative change on voluntary quits. Despite the fact that previous research obtained similar results, not much is known about the dynamics and potential impermanence of the effects of codetermination, yet. One limitation of the analysis is that it focuses on the effects of one particular aspect of the Works Constitution Act, namely the enlargement of works councils, and it remains uncertain to which degree the effects found resemble the effects of works council existence. Since the initial paid release of one full-time councilor is expected to substantially increase the power and to slacken time as well as coordination and knowledge constraints of councils, it is plausible that the results qualitatively apply to works council existence per se. The finding that voluntary quits are not only affected by works council status, but also by size and authority of works councils, is of particular relevance to employers considering a voluntary release of a councilor or opposing works council power, to employers in different institutional settings faced with the problem of employee retention, or during times of skills shortages.

Although frequently observed, little is known about the underlying mechanisms in which works councils, unions and collective bargaining interact in the German dual system of worker codetermination. Understanding these interactions remains both a task and a challenge for future industrial relations research.

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Appendix: Figures and Tables

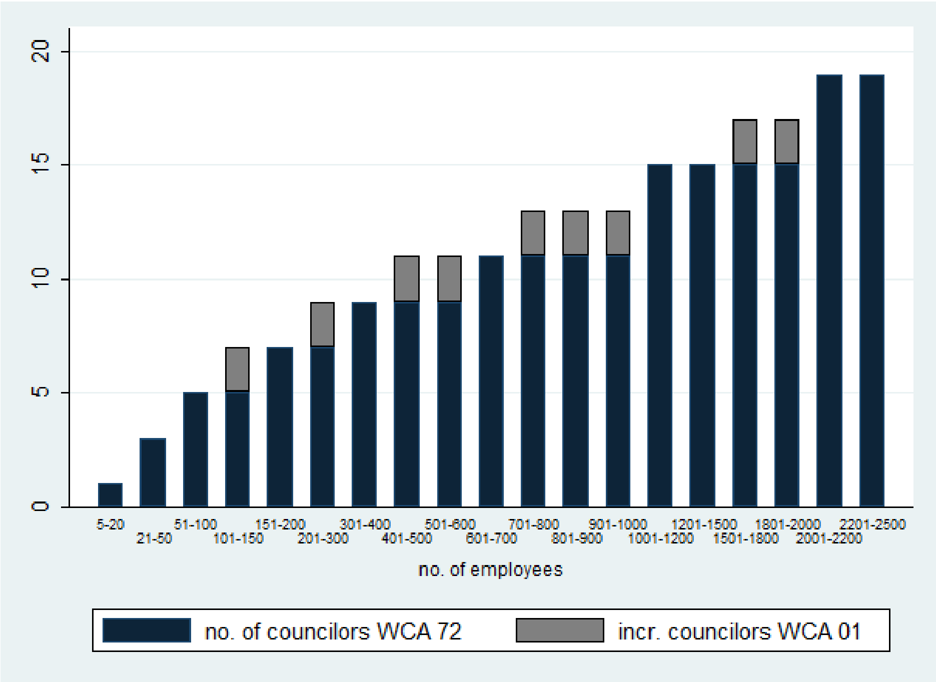

Figure 1. Number of councilors before and after the 2001 reform, by number of regularly employed persons in the establishment.

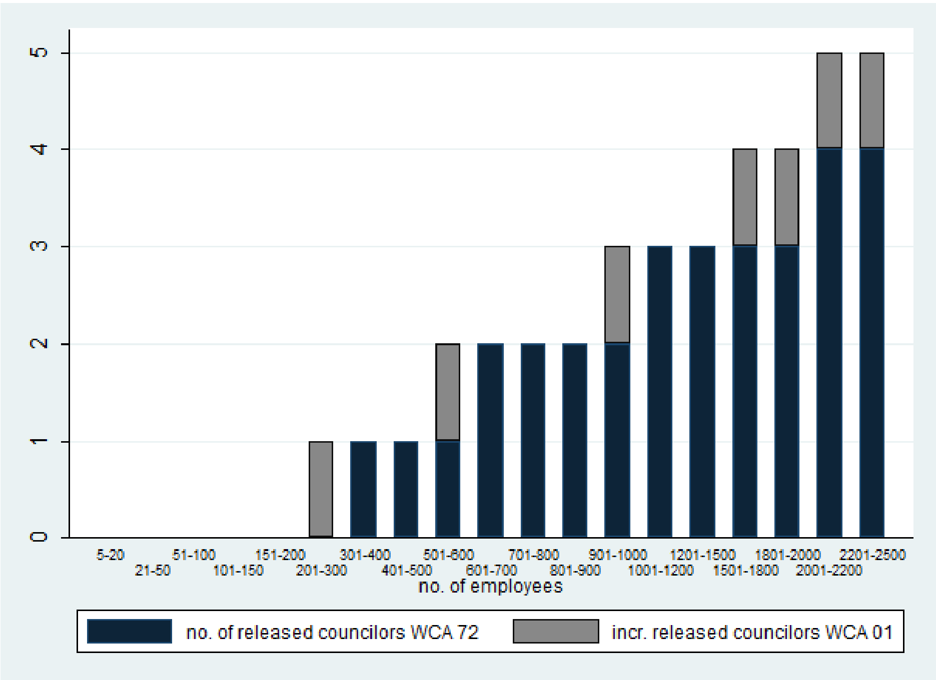

Figure 2. Number of released councilors before and after the 2001 reform, by number of regularly employed persons in the establishment.

Table 1. Mandated number of councilors and released councilors according to Works Constitution Act of 1972 and 2001, respectively

No. of employees	councilors WCA 1972	councilors DGB proposal	councilors WCA 2001	released councilors WCA 1972	released councilors DGB proposal	released councilors WCA 2001	Diff. no. of councilors	Diff. no. of released councilors
5-20	1	1	1	0	0	0	0	0
21-50	3	3	3	0	0	0	0	0
51-100	5	5	5	0	0	0	0	0
101-150	5	7	7	0	0	0	2	0
151-200	7	7	7	0	0	0	0	0
201-300	7	9	9	0	1	1	2	1
301-400	9	11	9*	1	1	1	0	0
401-500	9	11	11	1	2	1*	2	0
501-600	9	13	11*	1	2	2	2	1
601-700	11	13	11*	2	2	2	0	0
701-800	11	15	13*	2	2	2	2	0
801-900	11	15	13*	2	3	2*	2	0
901-1000	11	17	13*	2	3	3	2	1
1001-1200	15	17	15*	3	3	3	0	0
1201-1500	15	19	15*	3	4	3*	0	0
1501-1800	15	21	17*	3	4	4	2	1
1801-2000	15	23	17*	3	4	4	2	1
2001-2200	19	23	19*	4	5	5	0	1
2201-2500	19	25	19*	4	5	5	0	1

Notes: This table shows the mandated minimum number of councilors and released councilors according to Works Constitution Act 1972 and Works Constitution Act 2001. Gray shaded cells indicate changes by the reform legislation. Asterisks denote deviations in the legislation from the 1998 proposal of the German Trade Union Confederation (DGB).

Table 2. Sample characteristics

Variable	Minimum	Maximum	Mean and std. dev. Sample	Ctrl	Treat.	n Sample	Ctrl.	Treat.
West	0	1	0.669 (0.471)	0.650 (0.477)	0.690 (0.463)	2638	1398	1240
Employees	151	250	198.843 (28.965)	175.220 (14.775)	225.477 (14.137)	2638	1398	1240
Sales	14735	36800000000	91800000 (78300000)	60100000 (132000000)	127000000 (1130000000)	2638	1398	1240
Value added p.c.	82.640	148000000	439438 (3219100)	340154 (731455)	551373 (4629052)	2638	1398	1240
Wage sum (real) p. c. in 1000 Euro, June	0.248	17.404	2.342 (0.877)	2.316 (0.910)	2.370 (0.846)	2506	1326	1180
Collective agr.	0	1	0.833 (0.373)	0.830 (0.375)	0.836 (0.370)	2638	1398	1240
Voluntary quits	0	95	2.689 (4.589)	2.413 (4.495)	3 (4.676)	2638	1398	1240
Quit ratio	0	0.490	0.014 (0.023)	0.014 (0.025)	0.013 (0.021)	2638	1398	1240
Firings	1	230	10.236 (12.861)	9.381 (12.627)	11.202 (13.057)	2637	1398	1239
Profit situation (1-6)	1	6	3.260 (1.099)	3.281 (1.127)	3.235 (1.070)	289	153	136
Share of female workers	0	0.992	0.337 (0.253)	0.341 (0.256)	0.332 (0.250)	2632	1394	1238

Notes: IAB Establishment Panel 1998-2004. Variables value added per capita, quit ratio, share of female workers, wage sum (real) from own calculations. Only establishments with a persistent works council since 1998.

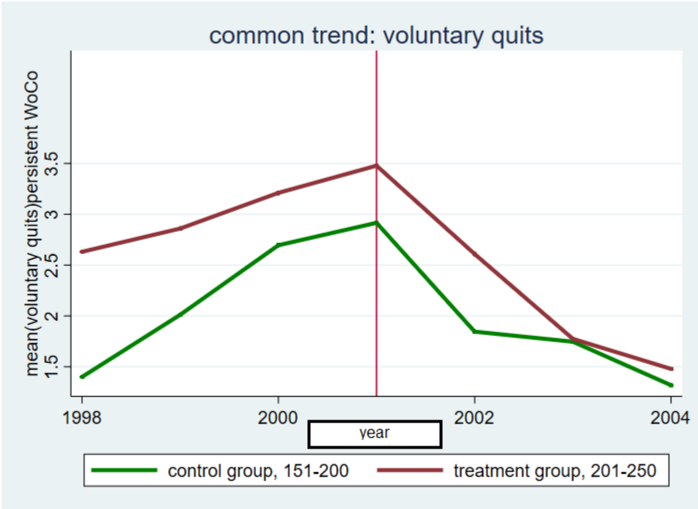

Figure 3. Development of unconditional mean of voluntary quits in treated and untreated establishments 1998-2004

Table 3. Regression results baseline model and model disentangling voice and monopoly effects.

	(1) baseline	(2) voice	(3) baseline coll. agr.	(4) voice coll. agr.
Treat	0.626*	0.631*	0.667**	0.677*
	(0.367)	(0.378)	(0.368)	(0.378)
Post	−1.796***	−1.774***	−1.822***	−1.796***
	(0.346)	(0.364)	(0.348)	(0.365)
Treat × Post	−0.888***	−0.923***	0.084	0.029***
	(0.301)	(0.303)	(0.519)	(0.532)
Coll. agr.	0.600	0.471	0.840**	0.717*
	(0.382)	(0.394)	(0.397)	(0.412)
Treat × Post × Coll. agr.			−1.137**	−1.120**
			(0.510)	(0.519)
log(Sales)	−0.072	0.675	−0.083	2.681
	(0.432)	(12.252)	(0.433)	(12.277)
West	−1.076***	−1.161***	−1.036***	−1.162***
	(0.374)	(0.406)	(0.373)	(0.406)
Employees	0.007	0.005	0.006	−0.007
	(0.007)	(0.063)	(0.007)	(0.063)
log(Value added p.c.)		−0.876		−2.903
		(12.542)		(12.564)
Wage sum p.c.		0.065		0.064
		(0.183)		(0.184)
Est. FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Industry dummys	yes	yes	yes	yes
R^2	0.061	0.061	0.063	0.063
Obs.	2638	2506	2638	2506
Establishments	1150	1090	1150	1090

Notes: Dependent variable is the number of voluntary quits. Standard errors clustered at the establishment level in parentheses. */ **/ *** denote significance at the 10%/5%/1% level, respectively. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (3) and (4) are 6.56 (0.002) and 6.82 (0.001), respectively. Wage sum per capita is measured in 1000 Euros.

Table 4. Regression results baseline model and model disentangling voice and monopoly effects with treatment effect, by collective agreement status and year.

	(5) baseline	(6) voice	(7) baseline coll. agr.	(8) voice coll. agr.
Treat	0.892	0.891	0.910	0.926
	(0.586)	(0.613)	(0.589)	(0.615)
Treat × 1998	0.082	0.203	−0.682	−0.180
	(0.676)	(0.711)	(2.082)	(2.187)
Treat × 1999	−0.629	−0.661	−0.257	−0.540
	(0.645)	(0.684)	(0.945)	(0.922)
Treat × 2000	−0.680	−0.702	−0.433	−0.530
	(0.622)	(0.654)	(1.135)	(1.160)
Treat × 2002	−1.123**	−1.183**	0.158	−0.105
	(0.487)	(0.502)	(0.656)	(0.670)
Treat × 2003	−1.626***	−1.662***	−0.706	−0.771
	(0.605)	(0.638)	(0.895)	(0.938)
Treat × 2004	−0.737	−0.716	−0.284	−0.108
	(0.629)	(0.652)	(0.945)	(0.994)
Treat × Coll.agr. × 1998			0.795	0.385
			(2.048)	(2.149)
Treat × Coll.agr. × 1999			−0.441	−0.140
			(0.863)	(0.834)
Treat × Coll.agr. × 2000			−0.287	−0.196
			(1.070)	(1.088)
Treat × Coll.agr. × 2002			−1.510**	−1.268**
			(0.588)	(0.585)
Treat × Coll.agr. × 2003			−1.044	−1.016
			(0.824)	(0.850)
Treat × Coll.agr. × 2004			−0.520	−0.728
			(0.835)	(0.871)
Coll. agr.	0.594	0.456	0.834*	0.689
	(0.379)	(0.392)	(0.439)	(0.460)
log(Sales)	−0.056	0.646	−0.081	2.198
	(0.430)	(12.267)	(0.432)	(12.406)
West	−0.960***	−1.028**	−0.903**	−1.009**
	(0.355)	(0.412)	(0.358)	(0.421)
Employees	0.008	0.006	0.007	−0.004
	(0.007)	(0.064)	(0.008)	(0.064)
log(Value added p.c.)		−0.814		−2.390
		(12.530)		(12.666)
Wage sum p.c.		0.052		0.054
		(0.184)		(0.184)
Est. FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Industry dummys	yes	yes	yes	yes
R^2	0.065	0.065	0.067	0.067
Obs.	2638	2506	2638	2506
Establishments	1150	1090	1150	1090

Notes: Dependent variable is the number of voluntary quits. Standard errors clustered at the establishment level in parentheses. */ **/ *** denote significance at the 10%/5%/1% level, respectively. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (7) for the year 2000, 2002, 2003 and 2004 are 0.66 (0.516), 5.33 (0.005), 4.10 (0.017) and 0.86 (0.424), respectively. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (8) for the year 2000, 2002, 2003 and 2004 are 0.60 (0.547), 4.64 (0.010), 3.87 (0.021) and 0.99 (0.373), respectively. Wage sum per capita is measured in 1000 Euros.

Appendix: Robustness Checks

Table 5. Regression results baseline model and model disentangling voice and monopoly effects, by collective agreement status. Sample of establishments with 150-195 and 205-250 employees.

	(1) baseline	(2) voice	(3) baseline coll. agr.	(4) voice coll. agr.
Treat	0.988	1.106*	1.036	1.173*
	(0.650)	(0.665)	(0.651)	(0.666)
Post	−1.727***	−1.696***	−1.746***	−1.709***
	(0.389)	(0.407)	(0.389)	(0.408)
Treat × Post	−0.701*	−0.761**	0.426	0.442***
	(0.361)	(0.370)	(0.518)	(0.534)
Coll. agr.	0.657	0.547	0.968**	0.899**
	(0.406)	(0.425)	(0.424)	(0.448)
Treat × Post × Coll. agr.			−1.362***	−1.464***
			(0.516)	(0.534)
log(Sales)	−0.405	8.163	0.413	11.179
	(0.406)	(12.252)	(0.409)	(12.381)
West	−1.221***	−1.425***	−1.173***	−1.434***
	(0.409)	(0.439)	(0.409)	(0.439)
Employees	0.006	−0.038	0.004	−0.056
	(0.010)	(0.063)	(0.010)	(0.064)
log(Value added p.c.)		−8.826		−11.873
		(12.472)		(12.606)
Wage sum p.c.		−0.044		−0.042
		(0.205)		(0.206)
Est. FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Industry dummys	yes	yes	yes	yes
R^2	0.059	0.060	0.062	0.063
Obs.	2364	2243	2364	2243
Establishments	1100	1045	1100	1045

Notes: Dependent variable is the number of voluntary quits. The sample comprises establishments with 150-195 and 205-250 employees. Standard errors clustered at the establishment level in parentheses. */ **/ *** denote significance at the 10%/5%/1% level, respectively. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification 3 and 4 are 4.71 (0.009) and 5.20 (0.006), respectively. Wage sum per capita is measured in 1000 Euros.

Table 6. Regression results baseline model and model disentangling voice and monopoly effects, by collective agreement status and year. Sample of establishments with 150-195 and 205-250 employees.

	(5) baseline	(6) voice	(7) baseline coll. agr.	(8) voice coll. agr.
Treat	1.441*	1.560*	1.466*	1.637*
	(0.865)	(0.897)	(0.872)	(0.904)
Treat × 1998	−0.513	−0.378	−1.293	−0.751
	(0.755)	(0.797)	(2.291)	(2.400)
Treat × 1999	−1.099	−1.141	−0.824	−1.124
	(0.722)	(0.773)	(1.062)	(1.044)
Treat × 2000	−0.876	−0.912	−1.173	−1.324
	(0.657)	(0.696)	(1.210)	(1.232)
Treat × 2002	−1.217**	−1.286**	−0.085	−0.314
	(0.538)	(0.555)	(0.700)	(0.705)
Treat × 2003	−1.538**	−1.599**	−0.447	−0.461
	(0.676)	(0.720)	(0.968)	(1.027)
Treat × 2004	−0.739	−0.791	0.041	0.385
	(0.727)	(0.762)	(1.057)	(1.121)
Treat × Coll.agr. × 1998			0.820	0.394
			(2.241)	(2.348)
Treat × Coll.agr. × 1999			−0.331	0.008
			(0.962)	(0.934)
Treat × Coll.agr. × 2000			0.365	0.509
			(1.122)	(1.133)
Treat × Coll.agr. × 2002			−1.376**	−1.185*
			(0.650)	(0.647)
Treat × Coll.agr. × 2003			−1.268	−1.332
			(0.844)	(0.895)
Treat × Coll.agr. × 2004			−0.952	−1.465
			(0.925)	(0.974)
Coll. agr.	0.652	0.539	0.889*	0.801
	(0.403)	(0.424)	(0.476)	(0.510)
log(Sales)	−0.414	7.871	−0.429	11.278
	(0.402)	(12.369)	(0.406)	(12.688)
West	−1.039***	−1.224***	−1.020**	−1.277***
	(0.394)	(0.450)	(0.401)	(0.461)
Employees	0.007	−0.035	0.006	−0.055
	(0.010)	(0.064)	(0.010)	(0.066)
log(Value added p.c.)		−8.541		−11.983
		(12.556)		(12.883)
Wage sum p.c.		−0.067		−0.068
		(0.207)		(0.209)
Est. FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Industry dummys	yes	yes	yes	yes
R^2	0.063	0.064	0.066	0.067
Obs.	2364	2243	2364	2243
Establishments	1100	1045	1100	1045

Notes: Dependent variable is the number of voluntary quits. The sample comprises establishments with 150-195 and 205-250 employees. Standard errors clustered at the establishment level in parentheses. */ **/ *** denote significance at the 10%/5%/1% level, respectively. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (7) for the year 2000, 2002, 2003 and 2004 are 0.90 (0.407), 4.19 (0.015), 3.57 (0.029) and 1.06 (0.346), respectively. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (8) for the year 2000, 2002, 2003 and 2004 are 0.90 (0.407), 3.80 (0.023), 3.43 (0.033) and 1.76 (0.173), respectively. Wage sum per capita is measured in 1000 Euros.

Table 7. Regression results baseline model and model disentangling voice and monopoly effects, by collective agreement status. Sample of establishments with 150-190 and 210-250 employees.

	(1) baseline	(2) voice	(3) baseline coll. agr.	(4) voice coll. agr.
Treat	1.151*	1.253*	1.198*	1.322*
	(0.683)	(0.709)	(0.685)	(0.711)
Post	−1.493***	−1.426***	−1.507***	−1.437***
	(0.402)	(0.427)	(0.402)	(0.427)
Treat × Post	−0.840**	−0.980**	0.307	0.175
	(0.421)	(0.433)	(0.598)	(0.612)
Coll. agr.	0.762*	0.674	1.062**	0.994**
	(0.447)	(0.481)	(0.463)	(0.504)
Treat × Post × Coll. agr.			−1.363**	−1.383**
			(0.581)	(0.587)
log(Sales)	−0.045	7.059	−0.055	9.492
	(0.369)	(13.937)	(0.375)	(14.030)
West	−1.097**	−1.296***	−1.049***	−1.297***
	(0.435)	(0.483)	(0.434)	(0.484)
Employees	0.000	−0.037	−0.002	−0.051
	(0.010)	(0.073)	(0.010)	(0.074)
log(Value added p.c.)		−7.226		−9.692
		(14.147)		(14.244)
Wage sum p.c.		0.148		0.147
		(0.218)		(0.219)
Est. FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Industry dummys	yes	yes	yes	yes
R^2	0.071	0.072	0.074	0.075
Obs.	2087	1978	2087	1978
Establishments	1030	981	1030	981

Notes: Dependent variable is the number of voluntary quits. The sample comprises establishments with 150-190 and 210-250 employees. Standard errors clustered at the establishment level in parentheses. */ **/ *** denote significance at the 10%/5%/1% level, respectively. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (3) and (4) are 4.25 (0.015) and 4.74 (0.009), respectively. Wage sum per capita is measured in 1000 Euros.

Table 8. Regression results baseline model and model disentangling voice and monopoly effects, by collective agreement status and year. Sample of establishments with 150-190 and 210-250 employees.

	(5) baseline	(6) voice	(7) baseline coll. agr.	(8) voice coll. agr.
Treat	1.461*	1.565*	1.482*	1.648*
	(0.847)	(0.891)	(0.859)	(0.908)
Treat × 1998	−0.422	−0.267	−1.073	−0.527
	(0.789)	(0.829)	(2.408)	(2.407)
Treat × 1999	−0.775	−0.801	−0.307	−0.675
	(0.771)	(0.838)	(1.203)	(1.176)
Treat × 2000	−0.482	−0.526	−0.644	−0.807
	(0.684)	(0.725)	(1.478)	(1.510)
Treat × 2002	−1.116*	−1.278**	0.178	−0.232
	(0.599)	(0.618)	(0.757)	(0.749)
Treat × 2003	−1.331*	−1.429*	−0.268	−0.408
	(0.692)	(0.733)	(1.132)	(1.132)
Treat × 2004	−0.990	−1.165	−0.165	0.012
	(0.693)	(0.736)	(1.167)	(1.232)
Treat × Coll.agr. × 1998			0.708	0.271
			(2.241)	(2.357)
Treat × Coll.agr. × 1999			−0.552	−0.141
			(1.109)	(1.080)
Treat × Coll.agr. × 2000			0.177	0.319
			(1.401)	(1.421)
Treat × Coll.agr. × 2002			−1.550**	−1.255*
			(0.691)	(0.673)
Treat × Coll.agr. × 2003			−1.234	−1.199
			(0.969)	(1.033)
Treat × Coll.agr. × 2004			−1.001	−1.474
			(1.036)	(1.104)
Coll. agr.	0.762*	0.668	1.030**	0.953*
	(0.448)	(0.481)	(0.523)	(0.572)
log(Sales)	−0.060	7.220	−0.076	9.954
	(0.373)	(14.291)	(0.379)	(14.699)
West	−1.038**	−1.215**	−1.004**	−1.239**
	(0.428)	(0.508)	(0.435)	(0.521)
Employees	0.001	−0.036	−0.001	−0.052
	(0.010)	(0.075)	(0.010)	(0.078)
log(Value added p.c.)		−7.410		−10.179
		(14.484)		(14.899)
Wage sum p.c.		0.131		0.128
		(0.220)		(0.222)
Est. FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Industry dummys	yes	yes	yes	yes
R^2	0.073	0.074	0.076	0.077
Obs.	2087	1978	2087	1978
Establishments	1030	981	1030	981

Notes: Dependent variable is the number of voluntary quits. The sample comprises establishments with 150-190 and 210-250 employees. Standard errors clustered at the establishment level in parentheses. */ **/ *** denote significance at the 10%/5%/1% level, respectively. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (7) for the year 2000, 2002, 2003 and 2004 are 0.26 (0.768), 3.64 (0.027), 2.58 (0.076) and 1.71 (0.181), respectively. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (8) for the year 2000, 2002, 2003 and 2004 are 0.28 (0.753), 3.23 (0.040), 2.52 (0.081) and 2.48 (0.085), respectively. Wage sum per capita is measured in 1000 Euros.

Table 9. Regression results baseline model and model disentangling voice and monopoly effects, by collective agreement status. Placebo analysis for the year 2000.

	(1) baseline	(2) voice	(3) baseline coll. agr.	(4) voice coll. agr.
Treat	0.377	0.383	0.410	0.422
	(0.351)	(0.365)	(0.349)	(0.364)
Post	−1.713***	−1.659***	−1.743***	−1.689***
	(0.382)	(0.397)	(0.384)	(0.399)
Treat × Post	−0.257	−0.293	0.910	0.820
	(0.373)	(0.394)	(0.718)	(0.732)
Coll. agr.	0.606	0.476	0.979**	0.848*
	(0.386)	(0.398)	(0.429)	(0.450)
Treat × Post × Coll. agr.			−1.350*	−1.298*
			(0.695)	(0.718)
log(Sales)	−0.115	−1.178	−0.111	0.553
	(0.429)	(12.402)	(0.428)	(12.667)
West	−1.300***	−1.358***	−1.261***	−1.353
	(0.373)	(0.395)	(0.375)	(0.394)
Employees	0.007	0.014	0.005	0.003
	(0.007)	(0.064)	(0.007)	(0.066)
log(Value added p.c.)		0.950		−0.787
		(12.683)		(12.941)
Wage sum p.c.		0.065		0.064
		(0.185)		(0.185)
Est. FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Industry dummys	yes	yes	yes	yes
R^2	0.058	0.057	0.061	0.060
Obs.	2638	2506	2638	2506
Establishments	1150	1090	1150	1090

Notes: Dependent variable is the number of voluntary quits. Standard errors clustered at the establishment level in parentheses. */ **/ *** denote significance at the 10%/5%/1% level, respectively. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (3) and (4) are 2.17 (0.114) and 1.92 (0.147), respectively. For the placebo analysis, the year of treatment has been predated to the year 2000. Wage sum per capita is measured in 1000 Euros.

Table 10. Treatment effects for baseline model and model disentangling voice and monopoly effects, comparison of OLS and Poisson coefficients

	Estimated model	(1) baseline	(2) voice	(3) baseline coll. agr.	(4) voice coll. agr.
Treat × Post	OLS	−0.888***	−0.923***	0.084	0.029
	Poisson	−0.362***	−0.365***	0.078	0.076
Treat × Post × Coll. agr.	OLS			−1.137**	−1.120**
	Poisson			−0.528***	−0.531***

Notes: Dependent variable is the number of voluntary quits. Standard errors are robust. */ **/ *** denote significance at the 10%/5%/1% level, respectively. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (3) for OLS and Poisson are 6.56 (0.002) and 14.45 (0.001), respectively. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (4) for OLS and Poisson are 6.56 (0.002) and 20.08 (0.000), respectively.

Table 11. Treatment effects for baseline model and model disentangling voice and monopoly effects, comparison of OLS and poisson coefficients

	Estimated model	(5) baseline	(6) voice	(7) baseline coll. agr.	(8) voice coll. agr.
Treat × 1998	OLS	0.082	0.203	−0.682	−0.180
	Poisson	0.257	0.344	0.285	0.372
Treat × 1999	OLS	−0.629	−0.661	−0.257	−0.540
	Poisson	−0.128	−0.130	0.065	−0.035
Treat × 2000	OLS	−0.680	−0.702	−0.433	−0.530
	Poisson	−0.171	−0.181	−0.151	−0.181
Treat × 2002	OLS	−1.123**	−1.183**	0.158	−0.105
	Poisson	−0.344**	−0.356**	0.101	0.053
Treat × 2003	OLS	−1.626***	−1.662***	−0.706	−0.771
	Poisson	−0.596***	−0.589**	−0.110	−0.104
Treat × 2004	OLS	−0.737	−0.716	0.084	−0.108
	Poisson	−0.278	−0.269	0.022	0.090
Treat × Post × Coll. agr. × 1998	OLS			0.795	0.385
	Poisson			−0.034	−0.044
Treat × Post × Coll. agr. × 1999	OLS			−0.442	−0.140
	Poisson			−0.232	−0.112
Treat × Post × Coll. agr. × 2000	OLS			−0.287	−0.196
	Poisson			−0.016	0.005
Treat × Post × Coll. agr. × 2002	OLS			−1.510**	−1.268**
	Poisson			−0.541***	−0.499***
Treat × Post × Coll. agr. × 2003	OLS			−1.044	−1.016
	Poisson			−0.555***	−0.552**
Treat × Post × Coll. agr. × 2004	OLS			−0.520	−0.728
	Poisson			−0.368	−0.456

Notes: Dependent variable is the number of voluntary quits. Standard errors are robust. */ **/ *** denote significance at the 10%/5%/1% level, respectively. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (7) for the regression for the year 2000, 2002, 2003 and 2004 are 0.66 (0.516), 5.33 (0.005), 4.10 (0.017) and 0.86 (0.424), respectively. Statistics and corresponding p-values from χ^2-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (7) for the poisson regression for the year 2000, 2002, 2003 and 2004 are 0.81 (0.666), 12.56 (0.002), 9.41 (0.001) and 2.59 (0.274), respectively. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (8) for the OLS regression for the year 2000, 2002, 2003 and 2004 are 0.60 (0.547), 4.64 (0.010), 3.87 (0.021) and 0.99 (0.373), respectively. Statistics and corresponding p-values from χ^2-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (8) for the poisson regressions for the year 2000, 2002, 2003 and 2004 are 0.86 (0.650), 12.08 (0.002), 8.12 (0.017) and 3.23 (0.198), respectively.

Appendix: Additional Results for Online Publication

Table 12. Dependent variable quit ratio: Regression results baseline model and model disentangling voice and monopoly effects.

	(1) baseline	(2) voice	(3) baseline coll. agr.	(4) voice coll. agr.
Treat	0.0026	0.0026	0.0028	0.0028
	(0.0018)	(0.0019)	(0.0018)	(0.0019)
Post	−0.0093***	−0.0092***	−0.0094***	−0.0093***
	(0.0018)	(0.0019)	(0.0018)	(0.0019)
Treat × Post	−0.0033*	−0.0034**	−0.0016	−0.0014
	(0.0015)	(0.0015)	(0.0026)	(0.0026)
Coll. agr.	0.0033*	0.0026	0.0045**	0.0038*
	(0.0020)	(0.0020)	(0.0021)	(0.0021)
Treat × Post × Coll. agr.			−0.0057**	−0.0056**
			(0.0024)	(0.0025)
log(Sales)	−0.0002	−0.0166	−0.0002	−0.0066
	(0.0023)	(0.0638)	(0.0023)	(0.0637)
West	−0.0026	−0.0025	−0.0024	−0.0025
	(0.0019)	(0.0022)	(0.0019)	(0.0022)
Employees	0.0000	−0.0001	0.0000	0.0000
	(0.0000)	(0.0003)	(0.0000)	(0.0003)
log(Value added p.c.)		0.0160		0.0059
		(0.0656)		(0.0655)
Wage sum p.c.		0.0003		0.0003
		(0.0010)		(0.0010)
Est. FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Industry dummys	yes	yes	yes	yes
R^2	0.050	0.048	0.051	0.050
Obs.	2638	2506	2638	2506
Establishments	1150	1090	1150	1090

Notes: Dependent variable is the quit ratio. Standard errors clustered at the establishment level in parentheses. */ **/ *** denote significance at the 10%/5%/1% level, respectively. Coefficients and standard errors are rounded at the fourth decimal. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (3) and (4) are 5.08 (0.006) and 5.08 (0.006), respectively. Wage sum per capita is measured in 1000 Euros.

Table 13. Dependent variable quit ratio: Regression results baseline model and model disentangling voice and monopoly effects with treatment effect, by collective agreement status and year.

	(5)	(6)	(7)	(8)
Spec.	baseline	voice	baseline coll. agr.	voice coll. agr.
Treat	0.0033	0.0032	0.0034	0.0033
	(0.0028)	(0.0029)	(0.0028)	(0.0030)
Treat × 1998	0.0022	0.0029	−0.0009	0.0014
	(0.0032)	(0.0034)	(0.0097)	(0.0102)
Treat × 1999	−0.0020	−0.0021	0.0003	−0.0009
	(0.0031)	(0.0033)	(0.0044)	(0.0044)
Treat × 2000	−0.0031	−0.0031	−0.0014	−0.0018
	(0.0031)	(0.0032)	(0.0056)	(0.0056)
Treat × 2002	−0.0043*	−0.0045*	0.0018	0.0007
	(0.0023)	(0.0024)	(0.0031)	(0.0032)
Treat × 2003	−0.0063**	−0.0064**	−0.0012	−0.0015
	(0.0031)	(0.0032)	(0.0044)	(0.0046)
Treat × 2004	−0.0013	−0.0011	−0.0013	0.0021
	(0.0032)	(0.0033)	(0.0047)	(0.0047)
Treat × Coll.agr. × 1998			0.0031	0.0014
			(0.0095)	(0.0100)
Treat × Coll.agr. × 1999			−0.0028	−0.0014
			(0.0040)	(0.0039)
Treat × Coll.agr. × 2000			−0.0020	−0.0015
			(0.0051)	(0.0052)
Treat × Coll.agr. × 2002			−0.0072***	−0.0061**
			(0.0027)	(0.0027)
Treat × Coll.agr. × 2003			−0.0058	−0.0056
			(0.0040)	(0.0041)
Treat × Coll.agr. × 2004			−0.0032	−0.0039
			(0.0038)	(0.0040)
Coll. agr.	0.0032*	0.0025	0.0046**	0.0038
	(0.0020)	(0.0020)	(0.0023)	(0.0024)
log(Sales)	−0.0001	−0.0168	−0.0002	−0.0091
	(0.0023)	(0.0636)	(0.0023)	(0.0639)
West	−0.0018	−0.0017	−0.0015	−0.0015
	(0.0019)	(0.0023)	(0.0019)	(0.0023)
Employees	0.0000	0.0001	0.0000	0.0000
	(0.0000)	(0.0003)	(0.0000)	(0.0003)
log(Value added p.c.)		0.0164		0.0086
		(0.0653)		(0.0656)
Wage sum p.c.		0.0003		0.0003
		(0.0010)		(0.0010)
Est. FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Industry dummys	yes	yes	yes	yes
R^2	0.053	0.053	0.056	0.054
Obs.	2638	2506	2638	2506
Establishments	1150	1090	1150	1090

Notes: Dependent variable is the quit ratio. Standard errors clustered at the establishment level in parentheses. */ **/ *** denote significance at the 10%/5%/1% level, respectively. Coefficients and standard errors are rounded at the fourth decimal. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (7) for the year 2000, 2002, 2003 and 2004 are 0.59 (0.553), 4.83 (0.008), 2.80 (0.061) and 0.42 (0.657), respectively. Statistics and corresponding p-values from F-test on joint significance of (Treat × Post) and (Treat × Post × Coll. Agr.) in specification (8) for the year 2000, 2002, 2003 and 2004 are 0.51 (0.602), 3.92 (0.020), 2.64 (0.072) and 0.54 (0.583), respectively. Wage sum per capita is measured in 1000 Euros.