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The works council wage premium in Germany: a case of strategic discrimination? *

Clément Brébion[†]

Abstract

This article uses panel data to estimate the wage premium of works council members in Germany, the cooperative model of reference in industrial relations. The works council premium is positive in the manufacturing sector but negative in the service sector. Further analyses show that in both sectors, mandates primarily affect the wage of the most influential and assertive works councillors. These results likely reflect the strategic behaviour of employers, which is constrained by the sectoral level of collective bargaining institutionalization (which is strong in the manufacturing sector and weak in services). Overall, this article reveals a decline in the quality of labour-employer cooperation in Germany.

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Introduction

Firm-level worker representatives have the specificity to negotiate with their own employer. Because of this relation of hierarchical subordination, their wage trajectory is likely to depend on their employer's assessment of the benefit that such negotiations can bring to the company. At the firm level, the impact of mandates on individuals' earnings is therefore likely to reveal much about the 'black box' of collective bargaining, including the structure of incentives to run for a mandate. In times of changing industrial relations, as the scope of firm-level bargaining is widening in most countries (Baccaro and Howell, 2017), shop-floor representatives are gaining strategic importance for both employers and employees, and analyses of this relation are increasingly needed. However, while the economic literature has prolifically discussed the causes and effects of firm-level negotiations from the perspective of covered firms and their workers in the US and Western Europe (Breda, 2015; Doucouliagos et al., 2017; Jirjahn, 2017; Martins, 2019; Barth et al., 2020), research on the actors actually leading the negotiations is much scarcer.

In this article, I estimate the wage premium of works council members. The German setting is well suited for this study. Its model of industrial relations is renowned for its cooperative features on the shop floor, and has long garnered high praise from both national and foreign economic actors. The German model relies on two pillars: collective bargaining at the branch level between unions and employers' associations and plant-level negotiations involving managers and members of the works council (Betriebsrat). German works councils are traditionally released from negotiations over most conflicting distributional questions that take place at the branch level; meanwhile, they benefit from the strongest co-determination and co-management rights in the West.¹ As such, they are often considered the most promising collective organization in terms of rent generation at the firm level. However, despite the resilience of the formal structure of negotiations in Germany (Thelen, 2009), strong pressure

has been exerted on the institutions of collective bargaining since the reunification (Addison et al., 2016). As a result, the core of the German model is slowly shifting from the branch level to the firm level. The extent to which its cooperative features – especially the ability of works councils to generate rents – still apply today is therefore unclear. Assessing the impact of works council membership on wages is therefore a way to shed light on the quality of firm-level negotiations in the country. It fills a gap in the literature, and it introduces elements from the shop floor to the question of the demise of the cooperative model of reference in industrial relations (Hall and Soskice, 2001; Addison et al., 2017).

The baseline regression is an ordinary least squares (OLS) model with individual and time fixed effects that controls for union membership. Data come from the German Socio-Economic Panel (GSOEP) between 2001 and 2015. There is no firm identifier in the panel. To prevent job moves from biasing the analysis, for each individual, I restrict the sample to the longest of her working spells within a firm. Thus, the impact of mandates on wages is identified by individuals switching status within a firm (i.e., getting elected or leaving a mandate; hereafter, the ‘switchers’). Estimations show that the works council wage premium depends on the sector. In the manufacturing sector, individuals earn approximately 4.6% more during their mandate of works councillor than out of mandate. This is equal to a gain in gross monthly earnings of approximately €150 in 2015 euros or €7,200 for a typical 4-year mandate. Conversely, in the service sector, I find a penalty of 3.7% – that is, a decrease of €121 in gross monthly earnings or €5,808 for the full mandate. The results suggest that the relation vanishes after the mandate ends.

In the manufacturing sector, I further show that before they entered the council, the career trajectories of future worker representatives evolved more slowly than those of their colleagues. This negative selection reinforces the results and excludes an explanation of the premium in

terms of reverse causality. In the sector, switchers' earnings are 13.3% higher in office than what they would be if they had followed their pre-election trend. I then show that conditions imposed on employers by the Works Constitution Act are not sufficient to explain this very large effect. Taking them into account and clearing the negative selection bias leaves us with a residual premium of +5.9% – that is, a gain in gross monthly earnings of approximately €183 in 2015 euros or €8,784 for a typical 4-year mandate. I argue below that the source of this residual premium stems from firms' managerial strategy. Such a pre-trend analysis is not possible in the service sector due to data limitations, and I cannot formally exclude that reverse causality fully explains the negative relation between mandates and earnings in this sector. However, the following paragraph hints at an explanation in terms of employers' behaviour in the service sector as well.

Thus, I provide statistical evidence showing that, in both sectors, mandates primarily affect earnings of councillors who are the most influential and assertive, which I respectively proxy by using unionization and party identification (on the relation between the latter and assertiveness, see Greene (2004), Gerber et al (2012) or Bakker et al (2015)). These representatives, I explain, are the ones with the strongest capacity to boost or harm firms' profits. Employers therefore have an incentive to discriminate them in the first place, either positively or negatively depending on how institutionalized collective bargaining is in the sector.

This strategic discrimination from rational employers towards labour representatives is the best candidate to explain the different signs of the works council premium across sectors. Specifically, I argue that in the service sector, where the culture of collective bargaining is weak, negative discrimination is a feasible strategy to undermine opposition and to avoid sharing decision-making power. By contrast, in the manufacturing sector, where collective

bargaining is strongly entrenched and cannot be bypassed, positive discrimination is a way to ‘buy’ councillors’ cooperation to escape the constraints of the traditional German model of industrial relations. Secondary evidence from field research in political science strongly supports this reading of my results (Haipeter, 2011a, 2011b; Artus, 2013).

Related literature. Since Dunlop (1944) and Freeman (1976) respectively modelled unions as rent-seeking and rent-generating organizations, the empirical literature in industrial relations has chiefly tried to disentangle which of the two facets of collective bodies dominate in covered firms. Thus, a first vivid stream of research has estimated unions’ impact on employment, working conditions, wages or performance in covered firms (for a review, see Doucouliagos et al., 2017), while another has measured the impact of works councils on similar outcomes, mostly in Germany.²

Departing from the dominant interest of the field in the impact of collective bodies on covered firms and workers, only one stream of research has, to my knowledge, worked on the relation between firm-level representatives’ mandates and earnings (Breda, 2011, 2014; Bourdieu and Breda, 2016). It focuses on union delegates in France and finds that exerting such mandates is associated with an average wage penalty of approximately 10%. The penalty can reach 20% for delegates belonging to the most vehement unions. Bourdieu and Breda, who use cross-sectional data, suggest that the link could explain why few workers are running for these positions in France. Using this time panel data that allow causal analyses, I bring seminal evidence on the mandate-earning relationship found using the German cooperative model, which is one of the most influential references in industrial relations. Notably, my results are compatible with Bourdieu and Breda’s results in the sense that negative strategic discrimination is found where the quality and culture of employer-employee cooperation is traditionally weak (in France overall³ and in the German service sector).

The article is organized as follows. In the first section, I describe the institutional context and build two testable hypotheses in relation to the literature. In sections 2 and 3, I present the data and some descriptive statistics. I then turn to regressions and robustness checks that show that the works council premium differs according to the sector. In the last section, I present primary statistical estimations and secondary field evidence in political science to argue that the impact of works council mandates on wages is the result of strategic discrimination.

1 The institutional context

The traditional model. Modern industrial relations in Germany are structured around two pillars. The first pillar consists of industry-wide regional (or sectoral) collective bargaining that takes place every four years between trade unions and business associations. It sets the main rules conditioning the distribution of rent, including questions of wages, working conditions, working time and job classification. The agreements reached by these bodies are legally restrained to firms with membership in the signatory association and are rarely extended to the whole sector. Given their importance of the questions at stake, sectoral unions commonly organize large strikes at the time of renegotiations, particularly in the manufacturing sector.

The second pillar consists of firm-level bargaining between employers and works councils. When referred to by workers, employers in private firms with at least five permanent employees are required by law to facilitate the constitution of a works council and to bear its costs.⁴ The public sector also features works councils, but they have fewer powers. Works councils cannot strike deals on issues that are normally fixed by collective agreements. Confined to less conflicting questions of personal and social matters rather than to financial and economic decisions, councils are supposed to smooth relations between labour and employers at the plant level. Thus, the 2001 Works Constitution Act (WCA) (section 74) states that works councils

and management should cooperate and “refrain from activities that interfere with operations or imperil the peace in the establishment”. In particular, works councillors cannot call for strikes. Nevertheless, their influence should not be underestimated, and they still have some power resources to use in disagreements with management. They benefit from extensive information on firms’ strategic orientations, they are very well represented on the supervisory board⁵ and, for instance, have veto rights for some individual staff movements as well as co-management rights on overtime and plans for reduced working time. The following two paragraphs provide more information on the timetables and earnings of works councillors, which are key variables of the analysis.

Professional elections take place every four years.⁶ The number of works councillors per firm and, among them, the number to receive full release depend on the firm size (Table A1.1, in appendix 1). Councillors who are not fully released are expected to take the initiative on the amount of time needed to accomplish their duties and inform their employer in due time.⁷ In theory, as far as possible, any hour of representation must be spent during normal working hours. When impossible, overtime spent on a mandate should be converted to time off in the following 30 days and, as a last resort, be paid (WCA, section 37-2).

In legal terms, being a works councillor is not a job but an honorary position: elected representatives continue to work on the same contract they had before. Accordingly, two works councillors spending identical amounts of time on their mandate (fully released, for instance) will not earn the same amount if they used to receive different wages or bonuses prior to their election. For wage increases, “during his term of office and for one year thereafter [it should] not be fixed at a lower rate than the remuneration paid to workers in a comparable position who have followed the career that is usual in the establishment. The same shall apply to general benefits granted by the employer” (WCA, section 37-4). Non-respect of these terms is punishable by a term of imprisonment and a fine.

The recent transformation of the German model. The traditional model is deemed to maximize cooperation at the firm level and, thereby, works councils' capacity to generate rents. However, critically from the perspective of this article, four main sources of pressure have provoked a deep erosion of the system since the mid-1980s: (i) unionization rates have plummeted; (ii) the financialization of the economy has tilted the balance towards shareholders rather than stakeholders (Goyer, 2007); (iii) globalization has magnified employers' interest in flexibility over decision making (Müller-Jentsch, 2018); and (iv) the growing trade integration of the old Eastern bloc has increased threats of production shifts towards the East. In this context, employers have grown increasingly interested in bringing the core of industrial relations from the industrial or regional level back to the shop floor, and they have growing power resources to do so. Both internal and external erosion of the traditional German model of industrial relations have therefore materialized with a rising risk of strategic discrimination towards works councillors.

On the one hand, employers have massively abandoned their business associations to free themselves from applying sectoral agreements, resulting in an external erosion of the model.⁸ Notably, this move is particularly dramatic among firms with a works council. In these firms, the share of employees not covered by collective bargaining agreements (CBAs) tripled between 1996 and 2015 (Oberfichtner and Schnabel, 2019).⁹ On the other hand, a process of internal erosion materialized in firms with a works council that remained covered by branch-level agreements. First, unorganized workplace agreements signed between employers and their works council to deviate from sectoral CBAs have soared since the 1980s – however, *de jure*, they are null and void. Second, mostly to counterbalance the downward pressure on membership rates but also to limit recourse to this 'wildcat' cooperation, employers' associations and unions have been forced to engage with innovative institutional designs. These designs are of several types (more details in Brébion, 2020), but they all allow firms that are

members of branch-level business associations to deviate from CBAs under particular circumstances. [Table 1](#) shows that the trend is towards a deepening of this internal erosion.

[Table 1 about here]

The erosion of the German model of industrial relations implies that works councillors can no longer rely on the mandatory character of branch regulations and have to engage in bargaining over broader issues than in the traditional model. This is true regardless of whether pressure takes the shape of innovative institutional designs, calls for wildcat cooperation, or leads to firms leaving their business association. As a result, councillors' claims have gained in strategic importance, and management has increasing incentives to take actions to favour more collaborative members against radical ones at election time. In other words, the retrenchment of branch-level coordination is likely to foster (positive and negative) strategic discrimination towards works councillors.

Heterogeneity across sectors. Crucially for this analysis, the retrenchment of the German model does not affect all sectors to the same extent. Not mentioned yet, a decrease in the incidence of works councils has affected the service sector (- 20% of covered workers between 2000 and 2011) but not the manufacturing sector ([Table 2](#)). In 2011, 34% and 66% employees therefore worked in firms covered by works councils in the former and latter sectors, respectively. For our case, we should keep in mind that a lower incidence of works councils, and a negative trend in coverage, are likely to be the results of a stronger opposition of employers against the institution. The external erosion of branch-level collective bargaining shows a similar pattern across both sectors.¹⁰ However, a larger share of workers in the manufacturing sector remains covered.

Overall, of all employees working in firms with five or more employees in 2015, less than one third were covered by both mainstays of the German model in the service sector (Oberfichtner and Schnabel, 2019). In the manufacturing sector, the corresponding figure is close to 50% (ibid). Unionization rates also remain stronger in the manufacturing sector.

[Table 2 about here]

Building testable hypotheses. Our analysis seeks to estimate the effect of mandates on labour earnings. The literature helps us to frame two testable hypotheses, given the institutional context previously described.

First, Fairris and Askenazy (2010) mentioned that works councils need the involvement of standard workers in cooperation to implement rent-generative policies. In particular, workers must show willingness to provide bottom-up suggestions. However, the authors make it clear that goodwill of the workforce depends on the reward. Along these lines, two equilibria could appear depending on the capacity of collective organizations to ensure credible commitment from employers to share the surplus generated through cooperation.

In the first case, the works council can harm a firm's profits if the employer does not respect her commitments. Employees are therefore willing to cooperate, which produces some surplus ultimately shared between labour and employers. This is a 'win-win' situation in which works councillors are pivotal. Profit-maximizing employers may therefore offer councillors a specific reward to ensure that they 'properly' play their role. This first equilibrium is more likely to take place in high-value-added sectors where labour-employer cooperation can bring the largest surplus. Note also that the game is dynamic in the way that trust between actors is necessary to achieve rent generation: behaviour in past periods has consequences for the current quality of

cooperation. This ‘win-win’ equilibrium is therefore more likely to take place in sectors with a strong culture of cooperation.

Consider now a sector with low unionization rates, low coverage of sectoral CBAs and, overall, a weak culture of cooperation. Works councils have a low ability to organize the workforce, a limited ability to refer to branch unions in case the employer oversteps his authority and, in the end, little capacity to harm firm performance. In this context, works councils cannot sufficiently involve employers and the workforce in cooperation. They stick to rent-seeking actions, and employers lead councillor-busting policies. These situations are especially likely to take place in low-value-added sectors. There, employers have weak incentives to share powers since the most strategic variable is labour cost. This reasoning leads to the following hypothesis *H1*:

H1: The stronger the culture of cooperation and the larger unionization rates are, the larger the works council wage premium.

Given the heterogeneity between the manufacturing and service sectors previously described, one can derive hypothesis *H1.1*:

H1.1: The works council wage premium in Germany is expected to be larger in the manufacturing sector than in the service sector.

Another hypothesis can be framed based on the research by Breda (2011, 2014) and Bourdieu and Breda (2016). They explain that shop-floor representatives play two bargaining games with their employer at the same time: one through their mandate on behalf of their colleagues and another about their own career evolution (promotions, working conditions, etc.), like any other employee. Two Nash equilibria can result from these bargaining games. The first is a cooperative equilibrium, where the representative trades laxity in her positions as an elected delegate against particular improvements in her working conditions relative to her colleagues.

Conversely, a non-cooperative equilibrium will occur when the representative strongly negotiates for her colleagues. In this latter case, her employer could ensure that the delegate's career stagnates to deter further activism in the firm. This literature raises a new hypothesis to be tested in our setting:

H2: The works council wage premium is negatively correlated with the strength and assertiveness of representatives' claims on behalf of their colleagues.

2 Data

The two hypotheses that the previous section has produced are tested using data from the German Socio-Economic Panel (GSOEP). The database is a yearly survey representative at the household and individual levels (Haisken-DeNew and Frick, 2005). To my knowledge, it is the only database combining information on wages and works council membership in Germany. Both variables are available for the waves 2001, 2003, 2006, 2007, 2011 and 2015, which I therefore use for the main analysis.

Employees working in firms covered by a works council differ from the rest of the workforce in many observables ([Table A2.1](#) in appendix 2) and, expectedly, in a number of unobservables. A good control group for councillors can therefore not include uncovered workers. Information on works council coverage is available only for 2001, 2006, 2011 and 2016 ([Table 3](#)). I therefore need to approximate coverage status in 2003, 2007 and 2015. To do so, I assume that firms with no change in status between two consecutive waves with coverage information experienced no variation on the matter in the intervening time. This seems legitimate because works council elections normally take place once every four years. Coverage status for agents whose firm exhibits such a sequence can then be inferred.¹¹ This includes approximately 40% of the respondents in waves 2003, 2007 and 2015. Among this population,

approximately 75% work in a covered firm, compared to 65% in the other waves. The difference likely stems from the stronger average seniority in covered firms. However, note that the share of councillors among covered firms is approximately 7.9% in each wave (year-to-year t-tests for mean difference are never rejected at the usual thresholds). Some robustness checks will still be performed in [section 4](#) using an alternative sample built without these recoding assumptions.

Works councils depend *de facto* on unions: the latter supplies the former with expertise, and approximately two thirds of works councillors are also union members. The impact of the two variables on wages should therefore be disentangled. Information on union status is given in all years of interest except 2006 ([Table 3](#)). For the respondents who answered in both 2006 and 2007 without changing firms, I therefore approximate their status in 2006 by their status in 2007.¹² The other observations for 2006 are dropped.

[Table 3 about here]

I further restrict the sample to full-time workers¹³ (i.e., between 30 and 60 working hours per week), aged between 20 and 64 and employed on open-ended contracts in firms with more than 5 employees. Civil servants are dropped, as are apprentices and interns, voluntary workers, members of the military and workers in the agricultural sector or in the extractive industries.

There is no firm identifier in the panel. To ensure that the results are not driven by agents changing firms, for each individual, I restrict the sample to the longest of her working spells within a firm. As the main model of identification is an OLS with individual and time fixed effects, estimations are free of firm-intrinsic characteristics that are constant over time. To limit the risk of outliers driving the main results, I further trim the bottom and top 1% of the yearly distribution of the hourly gross wage (see hereafter). I finally drop individuals who are observed only once. The final unbalanced panel includes 8,323 observations from 2,530 respondents. On

average, a respondent is observed 3.3 times over 6.7 years (corresponding to the time span between the first and last observations).

The main dependent variable is the logarithm of the hourly gross wage. It is computed using the answers to the questions, “*How high was your income from employment last month?*” and “*how many hours [per week] do your actual working hours consist of including possible overtime?*”. Side regressions are also performed separately for the two variables. They show that the main result stems mostly from an evolution in monthly wages rather than an evolution in working hours. The distribution of earnings and wages according to works council status are given in [Table A2.2](#) in appendix 2 for individuals in the main sample and, among them, in [Table A2.3](#) and [A2.4](#) for workers in the private manufacturing sector and workers in the private service sector, respectively.¹⁴

All estimations – except the one comparing trends in earnings – include the following categorical control variables: age (4 categories), seniority (4), SES (9), firm size (6), the month of the interview (12) and a dummy to control for whether the number of weekly hours is specified in the working contract. A year fixed effect is also included. In the few regressions with no individual fixed effects, I also control for the level of education (6 categories), the firm sector (9 categories), and sex. Standard errors are clustered at the individual level in these latter cases.

3 Descriptive statistics

[Table 4](#) shows the incidence of works council and union memberships in the final sample. As previously mentioned, two thirds of works councillors are unionized in the main sample. In total, works council members account for 7.9% of the sample.¹⁵ Overall, one third of the sample is a member of a trade union.

[Table 4 about here]

Table A2.1 in appendix 2 displays the average values of different variables in the main sample according to the works council membership status (columns (3) and (5)). T-tests for mean difference are also provided in the last column. Works councillors earn, on average, approximately 0.80 euro per hour less than their colleagues – significant at the 1% level. This accounts for approximately one eighth of a standard deviation in the hourly gross wage distribution, or 4.4% of the average. This difference stems mostly from monthly earnings, although councillors do declare working approximately 25 minutes less per week. Interestingly, Tables A2.2 and A2.3 show that these relations do not apply similarly across sectors in the main sample. Works councillors actually earn approximately 0.50 euro per hour more than their colleagues in the private manufacturing sector and 1.90 euro per hour less than their colleagues in the private service sector. In the former, both monthly earnings and actual working hours drive this difference, whereas in the latter, monthly earnings are the sole driver.

More generally, works council members are, on average, older in age and seniority, less well educated and more often males than their colleagues. They also work more often in smaller firms; this is a mechanic consequence of the institutional rules (Table A1.1 in appendix 1). Statistics for individuals who identify the main effect in the OLS model with fixed effects are also displayed (column (4) of Table A2.1). They do not differ much from the average works councillor. If anything, their number of weekly hours is closer to that of non-elected workers. For information, extra columns show the same statistics for workers in the GSOEP before the main selecting procedures were applied (column (1)) and for workers in non-covered firms (column (2)).

4 Estimations

Baseline model. Table 5 displays the estimations of the baseline OLS regression with time and individual fixed effects, which is described in equation (1). The coefficient of interest, β , gives us the size of the works council premium. I control for union membership (U_M) and for the list of covariates ($X_{i,t}$) that was previously detailed in section 2.

$$\ln(w_{i,t}) = \beta \cdot WoCo_M_{i,t} + \gamma \cdot U_M_{i,t} + \Gamma \cdot X_{i,t} + \alpha_i + c_t + \epsilon_{i,t} \quad (1)$$

Column (1) of Table 5 shows that, overall, no association between works council mandates and wages can be found in Germany. However, when separately zooming in on economic sectors, it appears that works councillors in fact experience different situations depending on the sector. In the private manufacturing sector, works councillors earn 4.6% more on average during their mandate than before or after their election. This is equal to a gain in gross monthly earnings of approximately €150 in 2015 euros or €7,200 for a typical 4-year mandate. This sector shows a pattern different from the rest of the private sector: a non-significant penalty of approximately 2% is found in both the construction¹⁶ and the service sector. Similarly, the effect is non-significantly negative in the public sector.

Among private services, it is known that industrial relations in the financial sector are unique; the sector exhibits very high degrees of coverage of both sectoral CBAs and works councils, to an even larger extent than in the manufacturing sector. It also strongly differs from the manufacturing sector, where the relation between unions and works councillors is much stronger and where rights to open clauses and firm agreements are more widespread. Separate estimations for this sector are not robust due to the sample size and are therefore not displayed here.¹⁷ In the rest of the article, analyses of the private service sector will therefore never include banking and insurance.¹⁸ When limiting the estimation to the remainder of the services industry, the wage penalty that works councillors experience during their mandate proves stronger (3.7%)

and statistically significant at the 5% level (column (5)). In monetary terms, this is equal to a drop in gross monthly earnings of €121 in 2015 euros or €5,808 for a full mandate. The impact of all control variables for columns (1), (2) and (5) are displayed in [Table A3.1](#) in appendix 3. As a side note, the estimates in [Table 5](#) are identified by workers changing status (i.e., observed both in mandate and out of mandate). Useful information on their numbers according to the different specifications is given in the last appendix.

[Table 5 about here]

In the private sector, works councillors therefore receive a positive premium in sectors where the traditional German model of industrial relations is the most developed. Conversely, at its periphery, the premium is negative. This result is consistent with hypothesis *H1.1* elaborated in [section 1](#). It is interesting to compare the size of these effects with those found by Breda (2014) and Bourdieu and Breda (2016) in France. These authors find that, on average, French union delegates receive a 10% wage penalty. Overall, employers in Germany therefore better value works council mandates than employers in France value union delegation. However, the penalty in the service sector is in fact close to what the researchers found in France overall.

Estimates of the relation between works council membership and wages in [Table 5](#) are the resulting effect of changes in status when voted into and out of the works council. [Table A3.2](#) in appendix 3 disentangles the two status changes, presenting results separately for the private manufacturing sector and the private service sector (not including banking and insurance). To simplify, I separate agents who switch status at least once ('switchers') into two groups: respondents whose first change in status is to become a works councillor and respondents whose first change in status is to leave the works council. In both groups, I consider only observations that precede a second change in status if applicable. In columns (1) and (3), 'switchers' from

the second group as well as respondents who remain in office throughout the observed period are dropped from the sample. In these columns, the coefficient of interest is therefore estimated only with information from ‘switchers’ entering the treatment. Following the same principle, in columns (2) and (4), the coefficient of interest is estimated on agents switching out of the treatment. In both sectors, no significant difference can be observed between the two types of estimations, and the point estimates are similar to those in [Table 5](#). In other words, in both sectors, the association between membership and wages seems to occur throughout the mandate period and to vanish when individuals are voted out of the organization.

For union membership, [Table 5](#) shows an overall wage penalty of 1.6% that fully stems from the service sector, where the penalty spikes to 6.7%. It is difficult to compare this result with those found in the literature, as the vast majority of articles dealing with the issue measure the difference between members and non-members at a given time, generally within firms. Among others, Bourdieu and Breda (2016) find a penalty of 3.5% against union members in France, while Booth and Bryan (2004) show evidence of a non-significant impact in the UK. Using within-wage quantile regressions (but between firms), Eren (2009) measures a wage benefit of 9% in the US. As for Germany, in an OLS setting controlling for individual and workplace characteristics, Blanchflower and Bryson (2002) find a positive but non-significant union premium of 4%.

A works council premium despite unchanged working hours. In [Table 6](#), I lead regressions in a similar spirit as the baseline ones, but I do so separately using the actual number of weekly working hours and the log of the monthly gross wage as dependent variables. It appears that for ‘switchers’, works council membership and union membership affect incomes rather than working hours. Columns (1) and (2) of [Table 6](#) indeed show significant results of

very similar size as the results from [Table 5](#) for both the manufacturing sector and the service sector.

Note that the negative impact of works council membership on monthly incomes in the service sector should not be read as a wage drop in nominal terms. Earnings are indeed computed from the question: “How high was your income from employment last month?”. As such, it is expected that respondents include extras such as bonuses or premia in their answer. A drop in such earnings is a first possible explanation. A differentiated rate of promotion between councillors and their colleagues is also possible, as the combination of an average yearly wage growth in the sector of approximately 1.25% and flat wage evolutions for works councillors would be sufficient to generate an average disadvantage of 3.1% over a mandate.

No significant correlation can then be evidenced between works council or union memberships and working hours. However, if anything, the correlation tends to inflate the effects found on councillors’ incomes in both sectors. This result may seem unexpected. It is sometimes argued that time releases granted to councillors are not sufficient to fulfil their role and, more generally, that works councillors and union members spend extra time in meetings or in organizing the labour force. The present estimation tells us that either (i) these presuppositions are wrong; or (ii) individuals who identify the effect deal with work-related issues outside of working hours independently of whether they are in office or not.

[Table 6 about here]

Robustness checks. In this part, I perform two series of robustness checks. The first is motivated by the risks of measurement error stemming, on the one hand, from the self-declared feature of works council coverage and, on the other hand, from the recoding procedure explained in [section 2](#). To avoid these risks, I build an alternative sample that includes the longest job spell of all workers – observed at least twice – from large firms, as it is known that

there is a strong positive correlation between works council coverage and firm size. I select workers from firms with more than 200 employees. According to Addison et al (2017), the coverage rate in such firms reaches approximately 90% on average in Germany (Table A4.1 in appendix 4). Note as well that most of them are covered by sectoral collective agreements. The same restrictions as in the main sample apply otherwise.

This alternative sample includes 8,885 observations. Table A4.2 in appendix 4 shows that all conclusions previously mentioned regarding the association between works council membership and wages still apply to this sample with no strong change in nature or magnitude. Conversely, correlations between wages and union status described in the baseline analysis are slightly less robust. The effect becomes significant in the manufacturing sector and loses its significance in the service sector.

Causality or mere adverse selection? Having established a differentiated association between works council membership and income according to the sector, we now turn our attention to establishing the causality of the relation. Given my setting, some reverse causality could explain the results. This risk would materialize if agents experiencing an evolution in their wage different from ‘normal’ trajectories were more likely to run for elections because of this specific trend. However, to explain the main results, the difference in pre-trends should go in opposite directions in the manufacturing and service sectors. Intuitively, no obvious reason for this phenomenon stands out. Nonetheless, what follows applies a procedure that tests and treats for the risk of a pre-trend differential between ‘councillors-to-be’ and workers who will never hold office. The technique was previously used in the literature on taxation (Kleven et al., 2014) and minimum wages (Monras, 2019) and was most recently applied in urban economics (Garcia-López et al., 2020).¹⁹ Because of data limitations, the procedure is performed only in the manufacturing sector.

To use pre-trends of sufficient length, I build a new yearly sample (‘updated sample’ hereafter). It relies on the assumption that none of the respondents changed status twice between two waves of the main sample. This is a light assumption, as there is normally one election at most between them. I then predict works council status and works council membership in waves with no information on industrial relations – as long as individuals have remained the same in the two surrounding waves of the main sample. Concretely, if an individual was out of office in 2001, 2003, 2006 and 2007 and then observed in office in 2011 and 2015, I assume that he was also out of office in 2002, 2004 and 2005 and in office between 2011 and 2015; her observations between 2007 and 2011 are not used. The other restrictions specified in [section 3](#) apply. The ‘updated sample’ is the restriction of this dataset to never-elected workers (group A) and to ‘switchers’ who were first observed out of office for at least 2 waves and remained in office once elected (group B).

The test and treatment of the selection into the works councils follow three steps. First, I drop from the updated sample observations of group B individuals who are (non-strictly) posterior to their first time in office (i.e., observations between 2011 and 2015 in the previous example). I also drop observations from group A individuals that are strictly posterior to 2011. I therefore have a yearly unbalanced panel over 2001-2011 composed of unelected workers from two groups: ‘councillors to be’ (for whom the dummy variable $\mathbb{1}_{group_B_i}$ in equation (2) takes the value of 1) and workers who never declared holding office in the GSOEP ($\mathbb{1}_{group_B_i} = 0$). It includes about six observations per individual. I then estimate whether the trend in wages differs between those two groups. The OLS model with time and individual fixed effects is described in equation (2)²⁰:

$$\ln(w_{i,t}) = \psi * t + \delta * t * \mathbb{1}_{group_B_i} + \Gamma.X_{i,t} + \alpha_i + c_t + \mu_{i,t}; t \in \llbracket 2001, 2011 \rrbracket \quad (2)$$

Reverse causality materializes if δ is significantly non-null and is of the same sign as the baseline estimates of the works council premium. This is not the case. Whereas baseline estimates were positive in the manufacturing sector, the first column of [Table 7](#) shows that in the years preceding their election to the works council, the yearly pay rise of future works councillors was 1.9p.p. lower than that of their colleagues. The selection bias is therefore negative, and estimates from [table 5](#) actually underestimate the size of the works council premium in the manufacturing sector.

In a second step, to treat the selection bias and find the corrected premium, I estimate the impact of works council membership, taking into consideration the downward pre-trend representatives experience (in relative terms) before their election. Concretely, I estimate equations (3) and (4):

$$\ln(w_{i,t}) = \rho_i * t + \alpha_i + c_t + \mu_{i,t} ; t \in \llbracket 2001, 2011 \rrbracket \quad (3)$$

$$\ln(\widetilde{w}_{i,t}) = \beta \cdot WoCo_M_{i,t} + \Gamma \cdot X_{i,t} + \alpha_i + c_t + \epsilon_{i,t} ; t \in \llbracket 2001, 2015 \rrbracket \quad (4)$$

In equation (3), I regress the dependent variable on individual specific time trends and on individual and time fixed effects. Here, the sample is the same as in equation (2): it includes ‘councillors to be’ and workers never observed elected in the GSOEP. In equation (4), I use all observations of the ‘updated sample’ (that is, I bring back in-office observations of group B individuals and post-2011 observations of group A individuals). For each observation of the ‘updated sample’, I compute ‘residuals’ $\ln(\widetilde{w}_{i,t})$ as the difference between the outcome variable $\ln(w_{i,t})$ and the predicted values $\ln(\widehat{w}_{i,t})$ based on the estimation of equation (2). I can finally regress $\ln(\widetilde{w}_{i,t})$ on the baseline independent variables. The results appear in the second column of [Table 7](#), while the last column gives the coefficient of the baseline estimation for the same

sample. As expected, the real effect of becoming a works councillor in the manufacturing sector – i.e., after treating for the downward pre-trend – is larger than the baseline result. The point estimate is + 13.3%. This means that when in office, switchers’ earnings were 13.3% higher than what they would be if they had followed their pre-election trend. In terms of monetary gains given in 2015 euros, the premium reaches approximately €413 in gross monthly earnings, or approximately €19,824 for a typical 4-year mandate. This result therefore proves that, in the manufacturing sector, firms offer councillors a premium *because of* their mandate.

At this stage, I cannot exclude that the premium is a mechanical consequence of the Works Constitution Act, which constrains employers to inflate representatives’ wages so that they keep pace with their colleagues (see [section 1](#)). I therefore take a third step to test it – still using the full ‘updated sample’. I estimate equation (5), which is similar in all instances to equation (4), with the exception of the dependent variable for individuals in group B (switchers). For them, the dependent variable in equation (4) measured the difference between the observed logwage $\ln(w_{i,t})$ and a predicted value $\ln(\widetilde{w}_{i,t})$ computed by prolonging the pre-mandate behaviour of switchers’ earnings. In equation (5), I take into account the expected effect of the WCA by adding a term that compensates for the difference in pre-trends for in-mandate observations. This term is based on the average difference in pre-trends computed in equation (2). Hence, we have the following equation:

$$\ln(\widetilde{\widetilde{w}}_{i,t}) = \beta \cdot WoCo_M_{i,t} + \Gamma \cdot X_{i,t} + \alpha_i + c_t + \epsilon_{i,t}; t \in \llbracket 2001, 2015 \rrbracket \quad (5)$$

$$\text{with } \ln(\widetilde{\widetilde{w}}_{i,t}) = \begin{cases} \ln(\widetilde{w}_{i,t}) & \text{if } WoCo_M_{i,t} = 0 \\ \ln(\widetilde{w}_{i,t}) + \hat{\delta} * (t - T_i^0) & \text{if } WoCo_M_{i,t} = 1 \end{cases}$$

where $\hat{\delta}$ comes from the estimation of equation (2) and T_i^0 is the last year when councillors-to-be are observed out of mandate.²¹

The third column in [table 7](#) shows that there remains a works council premium of +5.9% after taking into account the legal requirement the WCA imposes on employers. This residual premium is the result of an unconstrained choice from employers that is likely driven by firms' managerial strategy. In monetary terms, it is equal to a gain in gross monthly earnings of approximately €183 in 2015 euros, or €8,784 for a typical 4-year mandate.

[Table 7 about here]

In this section, I estimated the impact of works council membership on wages. I obtained two main results: (i) As hypothesized in *H1.1*, works council membership and incomes are negatively associated in the service sector and positively associated in the manufacturing sector. (ii) In the manufacturing sector, the positive relation between mandates and earnings is not driven by a negative selection bias, and the legal requirements of the Works Constitution Act are not sufficient to explain it. Firms' managerial strategies are therefore likely to play a role.

5 Turning back to the context, a case of strategic discrimination

In this section, I introduce further elements that strengthen the interpretation of the works council wage premium as resulting from firms' managerial strategy in the manufacturing sector, but also in the service sector. I first show that mandates do not affect all councillors to the same extent: partisan and unionized councillors – who are expected to weight the most in negotiations – receive the largest (positive or negative) premium in both sectors. I then recall the context, and I rely on the political science literature to explain why some discrimination of opposite signs could be occurring in the manufacturing sector and in the service sector.

Partisan and unionized works councillors receive most of the premium. This analysis constitutes a test of hypothesis *H2*. To do so, I re-estimate the baseline regression, but this time, the dummy for works council membership is interacted with two types of political markers that respectively proxy councillors' influence and the assertiveness of their claims: union membership and party identification. The former is straightforward: unionized councillors can avail themselves of their union's support, which is useful to weight more during negotiations. The latter, party identification, may require a short explanatory development. It is measured by a dummy variable taking the value of 1 if the respondent identifies with a political party.²² More than an ideology or an attitude towards a political party, party identification reflects a strong sense of belonging and, more generally, a social identity (Greene, 2004). The variable was therefore chosen for what it says about individuals' personality traits. Thus, a stream of research has evaluated the impact of the 'Big Five' (extraversion, openness, agreeableness, conscientiousness, and neuroticism) on the strength of party identification (Greene, 2004; Gerber et al., 2012; Bakker et al., 2015; Aidt and Rauh, 2018). Consistently across these studies, extraversion is the trait that is most stably (and positively) associated with the strength of party identification.²³ Note that the effect of extraversion persists when controlling for individuals' ideological and policy preferences. Importantly, "extraverted individuals tend to be more sociable, gregarious, and assertive" (Gerber et al., 2012: 661). Overall, individuals who identify with a party – and for whom the dummy variable of interest takes the value 1 – are therefore "strong partisans [who] make up the most active and polarized citizens" (Clifford, 2017: 532).

Estimates are displayed in [Tables 8](#) and [9](#). They show that in both the manufacturing and service sectors, the works council premium, measured in terms of hourly gross wage, is fully concentrated on representatives that identify with a party. Their remuneration is clearly distinct from that of the other representatives, and this stems from differences in monthly wages rather than in working hours (though slightly less significantly so in the manufacturing sector).

Furthermore, in the manufacturing sector, unionized works councillors receive the whole premium observed in the baseline results. In the service sector, unionization does not significantly affect the penalty representatives endure in terms of earnings. The sample is unfortunately too small to lead triple interactions.

Unionization and party identification proxy councillors' influence and the assertiveness of their claims, respectively. Overall, they predict councillors' expected weight in negotiations. Taken together, the estimations led until now in this article therefore tell us that, in the manufacturing sector, employers positively discriminate works councillors with the strongest capacity to boost or harm firms' profits. In the service sector, despite the lack of a pre-trend analysis, the results suggest that employers negatively discriminate against these representatives. These elements do not fully validate hypothesis *H2*. Influence and assertiveness predict the size of the works council premium. However, their effect is mediated by the sectoral level of institutionalization of collective bargaining, which shapes employers' incentives regarding HR policies towards councillors. Our estimations suggest that in the manufacturing sector, employers' interest is to cooperate with the most influential and assertive councillors, as collective institutions are too strong for employers to avoid negotiations. Conversely, in the service sector, where the bargaining culture is weak, employers have both an incentive and the capacity to hinder the claims of these councillors. In other words, in this sector, negative discrimination would be a feasible strategy to undermine opposition and to avoid sharing decision-making power. Along these lines, the differentiated discrimination that I find seems to be strategic in the sense of Bourdieu and Breda. The political science literature backs up this reading of my results. I discuss it in the following subsection.

[Tables 8 and 9 about here]

Case studies from the political science literature shed light on our results – The manufacturing sector. Historically, the manufacturing sector has been at the core of the traditional model of industrial relations in Germany, with large coverage of sectoral agreements and a strong norm for firms to be part of business associations. For this reason, employers' demands for flexibility in sectoral CBAs have emerged most strongly in this sector when economic difficulties arose in the aftermath of reunification. The result was a first series of opt-out clauses that caused a large wave of wage restraint. In the early 2000s, branch-level associations agreed upon spreading further derogations by conditioning them on less-stringent economic requirements.²⁴ However, employees had already been affected by the first sequence of derogations and strongly opposed their firms' application of the most recent rights to deviate from sectoral CBAs (Haipeter, 2011b). Works councils' support for firm-level dialogue over the enactment of these derogations therefore gained strategic importance for employers.

Works councils' role is clearly established in Haipeter's study of 12 firms, of which half are from the metalworking industry and half from the chemical industry (ibid). He shows that in the early 2000s, works councils were forced to sit at the negotiating table due to employers' threat of imminent job cuts. Once the dialogue opened, they organized with their union and proved to be cooperative partners for employers willing to use their recent rights to derogate from CBAs. Concretely, in Haipeter's sample, works councils implemented two strategies. The first consisted of gathering employees to emphasize the risk that some of them would lose their jobs and therefore the need to stand unified against job losses. The second strategy was to organize short-term strikes to "channe[l] the critical attitude towards derogations into broad support for the works councils in their negotiations with management" (ibid, p. 689). Note that when this second strategy was applied, works councillors ended up keeping employees from intensifying their struggles once sufficient levels of support were achieved and some concessions from the management were obtained.

In Haipeter's words, works councils were therefore "recognized by management as competent and powerful negotiators. [...] In most cases, they have been able to regain a great deal of the power lost through the structural constraints management was able to impose on them" (ibid p. 687). Crucially for this paper, works councillors relied to a large extent on the support of their union. On the one hand, councillors needed some expertise on the topics at stake and considered derogations as part of the collective bargaining sphere that should be managed by unions. On the other hand, in a context of declining union density, IG Metall and IG BCE – the metalworking and the chemical trade unions – showed increasing interest in coordination at the firm level. They both privileged cooperation with works councils (to gain concessions from employers) over frontal opposition on negotiation rights (ibid). For this reason, unionized works councillors had a particular strategic importance within the works councils for employers as go-betweens with the union.

From these elements emerges the fact that in the manufacturing sector, both employers and works councils benefited by negotiating over derogations that employees initially rejected. In this case, it seems quite clear that works councillors were not the guarantors of employers' commitments to reach a win-win equilibrium along the lines of the theoretical reasoning of Fairris and Askenazy (2010). Rather, the positive impact of works council membership on wages in the sector should be interpreted as incentives, or rewards, for works councillors' investment in negotiations. Given the prime importance of unionized representatives among works councils, it further seems rational to target wage premia for them. This finding is fully in line with our statistical results.

The service sector. The service industries share a much weaker tradition of collective bargaining than the manufacturing sector (see [section 1](#) and [Table 2](#)). Decisions are more often considered to pertain to managers and firm holders, and works councils are more often

considered to exceed the normal prerogatives of employees in the former than in the latter (Nienhueser, 2009). Likewise, managers from the service sector classify work agreements as increasing the rigidity in the firm, which is not the case of their counterparts from the manufacturing sector (ibid). One can therefore expect employers to lead more actions against the establishment or the proper working of works councils in the service industries than in the manufacturing sector.

This is confirmed by the sole case study dealing with the service sector. Artus (2013) focuses on low-wage services and highlights managers' strong opposition to the formation of works councils and, once these councils are established, to councillors with the strongest claims. In these sectors, labour costs are at the core of firms' competitive strategies and are altered by councillors' demands (ibid). Moreover, in some cases, she notices the prevalence of a strong corporate identity. Employees pushing interests other than those of the company community are therefore castigated. Thus, managerial intrusion into the composition of works councils is common in the low-wage service sector. It can manifest through pro-management lists or corruption. Works councils therefore end up in a mix between pro-management members and more vehement delegates. The latter are more assertive in the concessions demanded from management and more often label their struggle as a "war" (ibid, p. 420).

Accordingly, the negative relation between works council membership and wages should be interpreted as proof of works council busting, likely driven by the most assertive councillors in the precarious service industries (the subsector constitutes a large chunk of the sample). This finding is in line with results from [Table 8](#). Note that one could expect pro-management councillors to receive a positive premium for their mandate, which is not observed in the data.

What about unionized works councillors? According to Artus, unions avoid conflictual relations with companies from the low-wage service sector and are satisfied if the "general works council chairperson [...] [is] a Ver.di member²⁵ and (at least) bothers to enforce the low-

wage sectoral collective agreement” (Artus, 2013: 421). The turnover is too strong in this sector to make engaging in long conflicts worthwhile. As a result, unions tend to appease demanding councillors rather than embolden them. Here as well, this finding seems in line with results from [table 9](#) showing no clear-cut penalty difference between unionized and non-unionized representatives.

Conclusion

In this article, I estimated the wage premium of works council members in Germany. This sheds light on the quality of shop-floor negotiations in the cooperative model of reference in industrial relations. My focus on the actors leading the negotiations contributes to the economic literature on collective organisations. To date this literature has mostly documented the impact of collective organisations on covered firms and their average worker in the US and Europe. To my knowledge, this article is the first of its kind to use panel data that allow causal claims regarding the mandate-earnings relationship.

Fixed-effect OLS estimations showed a differentiated impact of works council membership on earnings according to the sector. In the manufacturing sector, this impact is positive, and individuals earn approximately 4.6% more during their mandate of works councillors than out of mandate. In the service sector, this impact is negative, and the corresponding penalty is approximately -3.7%. I have also shown that in the manufacturing sector, the positive mandate-earnings relationship is not driven by specific wage trajectories of workers about to be elected to the works council or by the simple application of the Works Constitution Act. A residual premium of +5.9% remains after taking into account the effect of both the law and the negative selection bias in the manufacturing sector. Further regressions finally demonstrated that, both in the manufacturing and the service sectors, wage (dis-)advantages primarily affect the

earnings of the most influential and assertive councillors. Overall, my results suggest that the works council premium stems from firms' managerial strategy targeting elected representatives who are likely to have the most influence in negotiations. Whether employers strategically reward or, conversely, penalize these most influential representatives depends on the sectoral level of collective bargaining institutionalization. Second-hand sources from field research in political science support this reading.

My results also contribute to the literature of political economy that has focused on the erosion of the German system of industrial relations. To this literature I brought elements from the shop floor and showed that, in a context of decentralization of collective bargaining, the hierarchical relationship between firm-level representatives and their employer threatens the quality of labour representation in the country. In the manufacturing sector where the tradition of bargaining is well entrenched, decentralization of collective bargaining has enabled works councillors to regain their position of privileged social partner. However, this came at the expense of the quality of representation; my results suggested that employers increased the earnings of unionized works councillors who approved deviations to sectoral agreements despite the scepticism of most employees – including insiders. In the service sector, my estimates highlighted the obstacles to codetermination. If anything, the decentralization of collective bargaining has favoured the concentration of power in the hands of employers and increased the incentives for councillor-busting managerial strategies.

One policy implication of this article is that administrative controls on councillors' earnings could be usefully reinforced, and in particular in times of decentralization of collective bargaining. Reducing the hierarchical relation between firm-level representatives and their employer may also be an interesting avenue. One could for instance establish more explicit and stringent rules governing earnings of firm-level representatives. Another way forward might be for the branches to take over the payment of these representatives.

¹ Co-management indicates that members of the Betriebsrat benefit from veto power regarding the social consequences of strategic decisions (lay-offs, etc.), thereby forcing employers to find agreements beforehand. It is to be distinguished from co-determination, whereby worker representatives sit on the supervisory board with voting rights. On this difference, see Rebérioux (Forthcoming).

² Following FitzRoy and Kraft's series of papers (1985; 1987, 1990), most of the empirical research on the impact of works councils on covered firms and workers has focused on Germany. The most recent articles find a positive effect of works councils on firm performance that is boosted when the firm takes part in branch-level collective bargaining agreements (CBAs) (Hübler and Jirjahn, 2003; Wagner, 2008; Mueller, 2011; Jirjahn and Mueller, 2012; Brändle, 2017). The impact on wages is more ambiguous both in itself and when combined with coverage of CBAs (Addison et al., 2001, 2010; Hübler and Jirjahn, 2003; Kraft and Lang, 2008; Brändle, 2017). The impact on employment growth is negative in Addison and Teixeira (2006), but Jirjahn (2010) finds a positive impact after clearing the selection bias. Finally, Muehleman and Pfeifer (2016) find a positive impact of works councils (and of other forms of firm-level labour representation) on hiring costs.

³ According to the Global Competitiveness Index Historical Dataset of the World Economic Forum, employers' estimation of the quality of labour-employer relations in France is among the worst in the OECD.

⁴ In particular, this includes the cost of elections, works councillors' wages and training, and the cost of premises and equipment (2001 Works Constitution Act (WCA) (sections 20 and 40)).

⁵ E.g., half of the seats in the coal and steel industry, half minus one seat in other firms with more than 2,000 employees, and one third in those with 500 to 2,000 employees.

⁶ Since 2000, they should have taken place between 1 March and 31 May in 2002, 2006, 2010, 2014 and 2018 in all firms. Poll records are not available. Note that the turnover is important: according to Emmeler and Brehmer (2019), approximately 27% of all works councillors elected in 2014 had left their organizations by 2018.

⁷ A conciliation meeting can be held if the employer considers this representation time to be excessive.

⁸ CBAs are rarely extended to the whole sector, and business association membership is voluntary. A covered firm can therefore leave its business association and stop applying the CBAs that the association had signed.

⁹ From slightly less than 5% to more than 16%. The equivalent figures for firms with no works council are approximately 37% and 62%.

¹⁰ In both sectors, the coverage of sectoral CBAs dropped by approximately 20% between 2000 and 2011.

¹¹ Concretely, in 2003, I approximate the works council status by the status applicable in 2001 and 2006 if the firm did not change status and the worker did not change firms. For all three waves, I then drop observations that are not covered by a works council or for which the works council status could not be recovered.

¹² The method generates limited noise in the data: using the other waves, it can be shown that the yearly change in union membership affects approximately 5% of the sample.

¹³ The restriction is used because the evolution in working time after the election is not clear-cut for part-time workers. In particular, the stability brought by mandates may affect the results for simple mechanical reasons.

¹⁴ Banking and insurance are excluded from the service sector to be consistent with the main regressions.

¹⁵ This figure is larger than legal requirements (Table A1.1 in appendix 1). The criteria applied to build the main sample indeed over-select works councillors: e.g., they are rarely on short-term contracts or employed part time.

¹⁶ The construction sector is set aside in the rest of the analysis. As in the manufacturing sector, it shows high coverage by sectoral CBAs. However, as in the service sector, councillors have limited relations with their sectoral union, and works council coverage has been very low and exhibited a strong negative trend since the early 2000s.

¹⁷ If anything, they show a non-significant positive association between works council mandates and wages.

¹⁸ In the remainder of the article, the service sector is therefore composed as follows: 33% of the individuals work in trade, 21% in transport, 10% in services to industries and 36% in personal services.

¹⁹ The first-best and most-used solutions are unsuitable here: no source of exogenous variation explaining elections into or out of the works council is available (think of poll records), and graphical analyses would need longer sequences of observations of switchers' status after their election.

²⁰ Note that unionization does not appear in this equation because no reasonable assumption could be made on this variable in the waves with no information on industrial relations. It should not be problematic, as the baseline estimations have shown that unionization is not a significant predictor of wages in manufacturing (see Table 5).

²¹ As previously mentioned, I drop the observations lying between the last observation out of mandate and the first in mandate. I am therefore estimating a lower bound of the real residual premium driven by strategic discrimination.

²² The question I use in the GSOEP is as follows: "Many people in Germany lean towards one party in the long term, even if they occasionally vote for another party. Do you lean towards a particular party?"

²³ In the literature, the other 4 personality traits have either no impact on party identification or an ambiguous one.

²⁴ E.g., the Pforzheim agreements signed in 2004 in the metalworking industry rendered derogations possible provided that "jobs would be safeguarded or created as a result and they would help to improve competitiveness and the ability to innovate, as well as investment conditions" (Haipeter, 2011a: 184).

²⁵ Ver.di is the largest union of workers in the service sector.

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Main tables and figures

Table 1 – Share of German employees working in firms...

	... bound to an opt-out clause	...using an opt-out clause (when bound to one)
2005	39,7%	52,9%
2011	52,7%	77,0%

Source: Addison et al (2017:46)

Table 2 – Share of German employees working in firms...

	... covered by a works council			... covered by sectoral CBAs		
	All economy	Service sector	Manufacturing sector	All economy	Service sector	Manufacturing sector
2000	47,6%	41,4%	65,4%	59,1%	54,3%	63,9%
2011	42,4%	34,5%	65,9%	47,8%	43,4%	51,8%

Source: Addison et al (2017). The data comes from the IAB establishment panel. Establishments with fewer than 5 employees or from the agriculture sector, the extractive industries and the public corporations are excluded.

Table 3 – Availability of the main variables of interest in the German Socio-Economic Panel according to the wave

	2001	2003	2006	2007	2011	2015	2016
Coverage status of the firm	✓		✓		✓		✓
Works council membership	✓	✓	✓	✓	✓	✓	
Union membership	✓	✓		✓	✓	✓	

Source: German Socio-Economic Panel

Table 4 – Incidence of works council and union memberships in the final sample

		Member of a Trade Union		
		No	Yes	Total
Member of the Works Council	No	5408 65,0%	2260 27,2%	7668 92,1%
	Yes	218 2,6%	437 5,3%	655 7,9%
Total		5626 67,6%	2697 32,4%	8323 100,0%

Source: German Socio-Economic Panel, own calculations

Table 5 – Effect of works council and union memberships on the log hourly gross wage according to the sector (Baseline model)

	(1)	(2)	(3)	(4)	(5)	(6)
	Private sector					Public Sector
	All sectors	Manufacturing sector	Construction sector	Service sector --- (incl. banking and insurance)	Service sector ---- (no banking or insurance)	---- (no civil servant)
Member of the Works Council	0.002 (0.009)	0.046*** (0.017)	-0.020 (0.028)	-0.021 (0.017)	-0.037** (0.018)	-0.014 (0.016)
Member of a Trade Union	-0.016* (0.008)	-0.011 (0.014)	0.001 (0.022)	-0.063*** (0.018)	-0.067*** (0.019)	0.000 (0.014)
Individual Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,323	2,432	1,241	2,033	1,553	2,617
Adjusted R ²	0.854	0.835	0.826	0.899	0.901	0.828

Model: OLS with individual and time fixed effects.

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Source: German Socio-Economic Panel, own calculations

Table 6 – What drives the baseline results? Estimations on alternative dependent variables: the log monthly gross wage and the number of actual working hours

	Dependent variable: log of the monthly gross wage		Dependent variable: number of actual working hours	
	(1)	(2)	(3)	(4)
	Manufacturing sector	Service sector	Manufacturing sector	Service sector
Member of the Works Council	0.044*** (0.017)	-0.031* (0.017)	-0.119 (0.387)	0.312 (0.482)
Member of a Trade Union	-0.014 (0.013)	-0.062*** (0.018)	-0.117 (0.313)	0.191 (0.497)
Individual Fixed Effect	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes
Observations	2,432	1,553	2,432	1,553
Adjusted R ²	0.877	0.926	0.661	0.694

Model: OLS with individual and time fixed effects.

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Source: German Socio-Economic Panel, own calculations

Table 7 – Taking into account specific wage trajectories of ‘councillors-to-be’ and WCA requirements to highlight firms’ managerial strategy in the manufacturing sector

	Manufacturing sector	Manufacturing sector	Manufacturing sector	Manufacturing sector
Equation estimated	(2)	(4)	(5)	(1)
Dependent variable	Ln (hourly gross wage)	De-trended log wage	De-trended log wage - min. legal pay rise for councillors	Ln (hourly gross wage)
Pre-trend in the hourly gross wage	0.022*** (0.002)			
Pre-trend * Group B ⁺	-0.019*** (0.007)			
Member of the Works Council		0.133*** (0.021)	0.059*** (0.021)	0.040* (0.023)
Individual Fixed Effect	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes
Group A ⁺	✓	✓	✓	✓
Group B ⁺	✓	✓	✓	✓
Group C ⁺				
Observations	3,632	3,817	3,817	3,817
Adjusted R ²	0.852	0.816	0.817	0.856

Models: OLS with individual and time fixed effects.

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

⁺ Group A includes respondents never observed in office. Group B includes respondents first observed at least twice out of office before starting a mandate of works councillor. Group C includes all other respondents.

Note: In the first column, all observations falling between 2001 and 2011 are used for group A, and observations preceding first time in office are used for group B. In the three other columns, all observations from groups A and B falling between 2001 and 2015 are used. Note that both samples include observations falling outside of years 2001, 2003, 2006, 2007, 2011 and 2015. This explains why the total number of observations is larger than in [table 5](#). See the text for more details.

Source: German Socio-Economic Panel, own calculations

Table 8 – Effect of the interaction between works council membership and political steadfastness on the log hourly gross wage, the log monthly gross wage and the actual number of working hours

	Dependent variable: log hourly gross wage		Dependent variable: log monthly gross wage		Dependent variable: number of actual working hours	
	(1)	(2)	(3)	(4)	(5)	(6)
	Manufacturing sector	Service sector	Manufacturing sector	Service sector	Manufacturing sector	Service sector
Supports a Political Party	0.002 (0.010)	0.007 (0.011)	0.005 (0.010)	0.009 (0.011)	0.148 (0.227)	0.074 (0.299)
Member of the Works Council	0.017 (0.024)	0.007 (0.024)	0.022 (0.023)	0.014 (0.023)	0.228 (0.528)	0.317 (0.630)
Member of a Trade Union	-0.012 (0.014)	-0.067*** (0.019)	-0.014 (0.013)	-0.062*** (0.018)	-0.111 (0.313)	0.187 (0.496)
WoCo Member * Supports a Pol. Party	0.054* (0.030)	-0.080*** (0.028)	0.041 (0.029)	-0.081*** (0.026)	-0.651 (0.674)	-0.047 (0.732)
Individual Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,426	1,552	2,426	1,552	2,426	1,552
Adjusted R ²	0.836	0.902	0.878	0.927	0.660	0.695

Model: OLS with individual and time fixed effects.

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Note: WoCo stands for works council

Source: German Socio-Economic Panel, own calculations

Table 9 – Effect of the interaction between works council and union memberships on the log hourly gross wage, the log monthly gross wage and the actual number of working hours

	Dependent variable: log hourly gross wage		Dependent variable: log monthly gross wage		Dependent variable: number of actual working hours	
	(1)	(2)	(3)	(4)	(5)	(6)
	Manufacturing sector	Service sector	Manufacturing sector	Service sector	Manufacturing sector	Service sector
Member of the Works Council	-0.013 (0.030)	-0.033 (0.024)	-0.017 (0.029)	-0.008 (0.023)	-0.198 (0.669)	1.096* (0.633)
Member of a Trade Union	-0.017 (0.014)	-0.066*** (0.020)	-0.020 (0.014)	-0.052*** (0.019)	-0.125 (0.317)	0.513 (0.524)
WoCo Member * Union Member	0.085** (0.036)	-0.010 (0.035)	0.090*** (0.034)	-0.051 (0.034)	0.114 (0.795)	-1.769* (0.928)
Individual Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,432	1,553	2,432	1,553	2,432	1,553
Adjusted R ²	0.835	0.901	0.878	0.926	0.661	0.695

Model: OLS with individual and time fixed effects.

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Note: WoCo stands for works council

Source: German Socio-Economic Panel, own calculations

Appendix – additional tables and figures

Appendix 1: Number of full and partial releases of works councillors according to firm size

Table A1.1 – Number of works councillors according to the firm size

Number of employees	Number of works councillors	Number of works councillors fully released
5–20	1	0
21–50	3	0
51–150	5	0
151–200	7	0
201-300	7	1
301-500	9	1
501–600	9	2
601–900	11	2
901-1000	11	3
1001–1500	15	3
1501-2000	15	4
2001–2500	19	5
2500-3000	21	5
3001–3500	23	6
3500-4000	25	6
4001–4500	27	7
4501-5000	29	7
5001–6000	31	8
6001–7000	33	9
7001–8000	35	10
8001-9000	35	11
9001-10000		12
>9000	+2 per bracket of supplementary 3000 workers	
>12000		+1 per bracket of supplementary 2000 workers

Source: 2001 Works Constitution Act

Appendix 2: Descriptive statistics

Table A2.1 – Average values of different variables according to firms' and workers' status (see note).

	Workers in the SOEP	Workers in non- covered firms	WoCo members in covered firms	WoCo members in covered firms who switch status	Non-elected workers in covered firms	Difference (3) – (5)
	(1)	(2)	(3)	(4)	(5)	(6)
Hourly gross wage	14.93	13.84	17.41	17.59	18.20	-0.79***
Monthly gross wage	2,511.05	2,640.26	3,136.55	3,188.06	3,335.15	-198.61***
Actual working hours	37.72	44.29	41.95	42.17	42.35	-0.40*
Sex m=1 fem=2	1.49	1.33	1.29	1.28	1.33	-0.03*
Age of Individual	42.27	42.69	45.74	44.59	44.66	1.08***
Region W=1 E=2	1.22	1.31	1.24	1.21	1.23	0.01
Senioriy	10.35	11.03	17.10	16.83	16.22	0.88**
<u>Education</u>						
General Elementary	0.08	0.05	0.07	0.06	0.06	0.00
Middle vocational	0.51	0.59	0.55	0.54	0.49	0.06***
Vocational + Abitur	0.08	0.08	0.08	0.07	0.08	-0.00
Higher Vocational	0.08	0.09	0.07	0.10	0.10	-0.02**
Higher Education	0.22	0.18	0.23	0.22	0.26	-0.03*
Inadequate or no answer	0.03	0.01	0.01	0.01	0.01	-0.00
<u>Isco88 (1 digit)</u>						
Legislators senior officials and managers	0.05	0.08	0.06	0.05	0.06	-0.00
Professionals	0.14	0.11	0.17	0.17	0.18	-0.01
Tech. and associate professionals	0.24	0.22	0.28	0.30	0.27	0.01
Clerks	0.13	0.10	0.14	0.12	0.14	0.00
Service workers and shop and market sales workers	0.11	0.09	0.03	0.02	0.03	0.00
Craft and related workers	0.14	0.25	0.18	0.20	0.15	0.04**
Plant and machine operators and assemblers	0.09	0.09	0.07	0.09	0.12	-0.05***
Elementary occupations	0.08	0.05	0.05	0.04	0.05	0.01
Unknown	0.03	0.02	0.02	0.01	0.01	0.00
<u>Sector (1 digit)</u>						
Manufacturing	0.21	0.26	0.29	0.30	0.29	-0.01
Construction	0.14	0.20	0.13	0.14	0.16	-0.03**
Trade	0.16	0.22	0.10	0.09	0.06	0.04***
Transport	0.06	0.05	0.07	0.07	0.07	0.00
Bank Insurance	0.05	0.01	0.07	0.06	0.09	-0.03**
Other Services	0.37	0.25	0.35	0.34	0.33	0.02
Unknown	0.03	0.00	0.00	0.00	0.00	0.00
Public sector	0.22	0.06	0.30	0.29	0.32	-0.01
<u>Firm size</u>						
[5 ;20[0.18	0.37	0.03	0.03	0.02	0.01**
[20 ; 100[0.21	0.37	0.16	0.15	0.09	0.07***
[100 ;200[0.10	0.10	0.11	0.12	0.10	0.01
[200 ;2000[0.23	0.10	0.38	0.34	0.37	0.01
≥ 2000	0.23	0.04	0.31	0.34	0.40	-0.10***
Unknown	0.04	0.02	0.01	0.01	0.01	-0.00
Number of observations	47,756.00	3,826.00	655.00	428.00	7,668.00	

Source: German Socio-Economic Panel own calculations

Note: Column (1) includes all workers in firms with at least 5 workers excluding civil servants, voluntary workers and militaries as well as workers from the extractive industries and the agricultural sector. In the other columns, selection goes further: the sample is restricted to the longest spell observed within a firm of full-time workers employed on open-ended contracts and aged between 20 and 64. Column (2) shows statistics for the workers in firms not covered by a works council as opposed to columns (3), (4) and (5). Column (3) and column (5) are based on a split of the main sample of estimation. They respectively account for works councillors and non-elected workers. In between, column (4) gives information on the years of mandate of respondents observed both in and out of office in the main sample.

Table A2.2 – Distribution of the dependent variables in the main sample

		Number of observations	Mean	Standard deviation	Minimum	Maximum
Works councillors	Hourly gross wage	655	17,4	6,0	6,8	43,5
	Monthly gross wage		3137	1151	1176	8638
	Actual working hours		41,9	4,7	30,0	60,0
Non-elected workers	Hourly gross wage	7668	18,2	6,7	6,2	50,6
	Monthly gross wage		3335	1401	1140	11500
	Actual working hours		42,4	5,1	30,0	60,0

Source: German Socio-Economic Panel, own calculations

Table A2.3 – Distribution of the dependent variables in the main sample (private manufacturing sector)

		Number of observations	Mean	Standard deviation	Minimum	Maximum
Works councillors	Hourly gross wage	187	18,9	6,1	7,4	40,2
	Monthly gross wage		3390	1264	1400	8638
	Actual working hours		41,4	4,7	35,0	60,0
Non-elected workers	Hourly gross wage	2245	18,4	6,5	6,5	50,6
	Monthly gross wage		3341	1359	1143	10000
	Actual working hours		41,9	5,3	30,0	60,0

Source: German Socio-Economic Panel, own calculations

Table A2.4 – Distribution of the dependent variables in the main sample (private service sector, excluding banking and insurance)

		Number of observations	Mean	Standard deviation	Minimum	Maximum
Works councillors	Hourly gross wage	157	15,3	5,6	8,0	40,1
	Monthly gross wage		2812	1130	1200	8000
	Actual working hours		42,6	5,2	30,0	60,0
Non-elected workers	Hourly gross wage	1396	17,2	7,2	6,6	47,2
	Monthly gross wage		3235	1556	1140	11000
	Actual working hours		43,4	5,7	30,0	60,0

Source: German Socio-Economic Panel, own calculations

Appendix 3: Details and further analysis of the baseline regressions

Table A3.1 – Details of the baseline regression for the main samples of interest

	All sectors		Manufacturing sector		Service sector ---- (no banking or insurance)	
	Coefficient	Std. Err.	Coefficient	Std. Err.	Coefficient	Std. Err.
Member of the Works Council	0.002	(0.009)	0.046***	(0.017)	-0.037**	(0.018)
Member of a Trade Union	-0.016*	(0.008)	-0.011	(0.014)	-0.067***	(0.019)
<u>Age Category (ref: 20-35 y.o.)</u>						
36-43 y.o.	0.028***	(0.009)	0.059***	(0.017)	0.017	(0.018)
44-50 y.o.	0.033***	(0.012)	0.062***	(0.024)	0.041	(0.026)
51-64 y.o.	0.021	(0.016)	0.058*	(0.032)	0.019	(0.034)
<u>Seniority (ref: [0; 6.2[)</u>						
[6.2;11.9[0.046***	(0.007)	0.058***	(0.014)	0.027*	(0.014)
[11.9;20[0.064***	(0.010)	0.062***	(0.020)	0.040*	(0.022)
>= 20	0.054***	(0.015)	0.058**	(0.029)	0.028	(0.034)
<u>Isco88 (ref: Legislators senior officials and managers)</u>						
Professionals	-0.007	(0.012)	-0.002	(0.024)	0.023	(0.022)
Technicians and associate professionals	-0.017	(0.011)	0.019	(0.023)	-0.018	(0.021)
Clerks	-0.032**	(0.013)	-0.002	(0.029)	-0.043*	(0.024)
Service workers and shop and market sales workers	-0.006	(0.021)	0.121	(0.158)	-0.005	(0.029)
Craft and related workers	-0.019	(0.015)	0.011	(0.026)	0.008	(0.040)
Plant and machine operators and assemblers	-0.018	(0.016)	0.010	(0.026)	-0.050	(0.049)
Elementary occupations	-0.004	(0.018)	0.038	(0.031)	-0.036	(0.036)
Unknown	-0.077**	(0.036)	-0.115*	(0.063)	-0.008	(0.063)
<u>Firm size (ref: [5 ;20[)</u>						
[20 ; 100[0.010	(0.021)	0.012	(0.070)	-0.057	(0.053)
[100 ;200[0.024	(0.021)	0.075	(0.070)	-0.075	(0.054)
[200 ;2000[0.011	(0.021)	0.050	(0.069)	-0.065	(0.053)
>= 20	0.011	(0.021)	0.069	(0.070)	-0.089*	(0.053)
Unknown	0.054	(0.038)	0.162*	(0.088)	-0.145*	(0.083)
No working hour agreement	0.012	(0.015)	0.034	(0.027)	0.021	(0.032)
<u>Survey year (ref: 2001)</u>						
2003	0.087***	(0.005)	0.076***	(0.010)	0.098***	(0.012)
2006	0.128***	(0.007)	0.102***	(0.013)	0.155***	(0.015)
2007	0.132***	(0.008)	0.113***	(0.015)	0.157***	(0.017)
2011	0.225***	(0.010)	0.203***	(0.020)	0.250***	(0.023)
2015	0.344***	(0.013)	0.326***	(0.026)	0.387***	(0.030)
<u>Month of interview (ref: Jan)</u>						
February	-0.008	(0.008)	-0.008	(0.014)	-0.036**	(0.017)
March	-0.014*	(0.008)	-0.012	(0.015)	-0.031*	(0.017)
April	-0.002	(0.009)	-0.001	(0.017)	-0.015	(0.019)
May	-0.016	(0.010)	-0.012	(0.018)	-0.022	(0.021)
June	-0.009	(0.011)	-0.008	(0.020)	-0.026	(0.024)
July	-0.011	(0.013)	-0.019	(0.026)	-0.029	(0.027)
August	-0.006	(0.015)	-0.006	(0.028)	0.012	(0.033)
September	-0.002	(0.018)	0.024	(0.036)	-0.060*	(0.035)
October	0.043	(0.035)	0.090	(0.069)	-0.030	(0.091)
Individual Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,323		2,432		1,553	
Adjusted R ²	0.854		0.835		0.901	

Model: OLS with individual and time fixed effects;

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1;

Source: German Socio-Economic Panel, own calculations

Table A3.2 - Differentiation of the baseline effects between entrance and exit from the works council. Dependent variable: log hourly gross wage

	(1)	(2)	(3)	(4)
	Manufacturing sector	Manufacturing sector	Service sector	Service sector
Member of the Works Council	0.041* (0.022)	0.083** (0.035)	-0.038 (0.024)	-0.062* (0.036)
Member of a Trade Union	-0.016 (0.015)	-0.026* (0.015)	-0.068*** (0.021)	-0.067*** (0.023)
Individual Fixed Effect	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes
Group 1 (enter WoCo) ⁺	✓		✓	
Group 2 (leave Woco) ⁺		✓		✓
Group 3 (always in WoCo) ⁺				
Group 4 (never in WoCo) ⁺	✓	✓	✓	✓
Observations	2,242	2,134	1,413	1,343
Adjusted R ²	0.842	0.843	0.908	0.900

Model: OLS with individual and time fixed effects.

Standard errors in parentheses are clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

Note: WoCo stands for works council

⁺ I separate agents who are observed at least once as a works councillor into three groups. Among the 'switchers', group 1 includes respondents whose first change in status was becoming a works councillor, group 2 includes respondents whose first change in status was leaving the works council. Group 3 includes respondents always observed in office. Respondents never observed in office are part of the group 4

Source: German Socio-Economic Panel, own calculations

Appendix 4: Robustness checks

Table A4.1 – Share of German employees working in firms with more than 250 employees covered by ...

	... sectoral collective agreements	... firm-level collective agreements	... a works council
2000	76.6%	12.8%	92.4%
2001	75.2%	14.6%	92.0%
2002	77.8%	12.1%	93.7%
2003	74.7%	13.9%	92.2%
2004	76.8%	12.4%	92.7%
2005	76.3%	12.6%	91.5%
2006	73.3%	14.0%	89.2%
2007	73.6%	12.1%	89.3%
2008	70.0%	13.3%	88.9%
2009	67.1%	15.6%	89.5%
2010	69.9%	14.2%	88.9%
2011	70.1%	13.0%	89.0%

Source: Addison et al (2017).

Table A4.2 – Alternative sample - workers in firms with more than 200 employees. Dependent variable - log hourly gross wage

	(1)	(2)	(3)	(4)	(5)	(6)
	Private sector					Public Sector
	All sectors	Manufacturing sector	Construction sector	Service sector --- (incl. banking and insurance)	Service sector ---- (no banking or insurance)	---- (no civil servant)
Member of the Works Council	-0.001 (0.011)	0.035** (0.018)	0.007 (0.031)	-0.046** (0.022)	-0.062** (0.024)	-0.017 (0.020)
Member of a Trade Union	-0.013 (0.009)	-0.024* (0.014)	-0.012 (0.023)	-0.012 (0.019)	-0.023 (0.021)	0.001 (0.017)
Individual Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,885	2,624	1,386	2,484	1,889	2,391
Adjusted R ²	0.860	0.843	0.841	0.896	0.896	0.823

Model: OLS with individual and time fixed effects.

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Source: German Socio-Economic Panel, own calculations

Appendix 5: Number of individuals identifying the main effects

Table A5.1 – Number of individuals identifying the main effect (i.e. switching status) according to the regression model and the sample

	All sectors	Manufacturing Sector	Service Sector (excl. banking and insurance)	Public Sector
Baseline regression	279	86	65	79
Robustness Check - Large firms	230	86	48	56
Robustness Checks - Enter WoCo		60	44	
Robustness Checks - Exit WoCo		26	21	

Note: WoCo stands for works council

Source: German Socio-Economic Panel, own calculations

Table A5.2 – Number of individuals identifying the main effect in the baseline regression according to their union and political status when first observed

	Manufacturing Sector	Service Sector (excl. banking and insurance)
Total number of individuals identifying the main effect in the baseline regression ...	86	65
... of which, N were unionized when first observed	59	23
... of which, N supported a political party when first observed	52	35

Source: German Socio-Economic Panel, own calculations

Table A5.3 – Number of individuals in group B of table 7

Manufacturing Sector
29

Source: German Socio-Economic Panel, own calculations