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Effects of Multi-unionism on Wage Bargaining - a Regression Discontinuity Approach

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Supervisor: Thomas Breda

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Abstract[1]

In this dissertation, I exploit newly-available data on French professional elections to investigate the effects of multi-unionism on wage bargaining outcomes in firms. The co-existence of several trade-unions – whether at the national, sector or firm level – is widespread in Western Europe and has amplified with the division of labor movements. However, its effects for both workers and employers are a priori ambiguous. First, a higher number of unions may result in coordination frictions, source of union paralysis or crises, but it may also result in increased union accountability. Second, this ambiguity is compounded by the "two faces of unions" highlighted by Freeman and Medoff [1979]. In this dissertation, I therefore assess the net effect of an additional (weak) union on wages and investigate whether the co-existence of several trade-unions in a firm appears to result in coordination frictions affecting wage bargaining. In response to the endogeneity issues faced by the literature on unions, recent works have had recourse to Regression Discontinuity Designs to causally identify the effects of unions, exploiting the discontinuities generated by minimum electoral requirements for unionization. I follow a similar strategy, exploiting a set of three discontinuities: French unions require minimum electoral scores of 10%, 30% and 50% to have the possibility of taking part in bargaining in a workplace, signing agreements alone, or vetoing agreements alone. While Regression Discontinuity Designs in electoral settings are widespread, I uncover a particularity of the setting studied: as a result of the small size of most elections, combined with an asymmetric distribution of election sizes, the standard requirement of absence of precise manipulation of the running variable is not a sufficient condition for valid identification. However, recourse to a "donut" approach appears to restore the validity of the identification. Despite a first stage which highlights jumps in treatment probability ranging from 30% to nearly 100%, I uncover no evidence that an additional bargaining union affects wage bargaining outcomes or the wage bargaining process. Although the minimum detectable effects would be too high to be credible, inspection of the trends suggested by the point estimates and comparison to the results from placebo specifications using pre-electoral outcomes suggest an absence of effect. However, this may be attributable to contradictory effects of coordination frictions and increased accountability, or to heterogeneous treatment effects, whereby the effects of an additional *weak* trade-union only provides lower bound for the effects of an additional strong union. Investigation of the effects on wage bargaining outcomes of an additional trade-union gaining the possibility to sign agreements without having to coordinate with other unions or, by contrast, to block the bargaining process, indeed appears, upon first inspection, to corroborate the predictions of a model whereby the co-existence of several trade-unions in a firm is a source of coordination frictions negatively affecting wages. However, further inspection suggests a more complex story, as these configurations do not appear to generate jumps in the probability that an agreement be signed or vetoed, while suggestive evidence nonetheless suggest that they are associated with improvements in employers' social climate perception and a decrease in mobilizations during wage bargaining.

[1] I would like to express my gratitude to my supervisor Thomas Breda for his guidance and support during the realization of this master's dissertation. I had access to the datasets I used for the purpose of this dissertation through the partnership between the Paris School of Economics' "Chaire Travail" and the DARES (the French Directorate for the Animation of Research, Studies and Statistics of the Ministry of Labor). I therefore would like to thank the Eric Maurin and "Chaire Travail" as well as Patrick Pommier, the departments DRPTT and SD-SEPEPF at the DARES for giving me the opportunity of working on these datasets, as well as for letting me work from the premises of the DARES. Finally, I would like to thank my parents and Aude for their support when writing this dissertation.

Contents

1 Introduction

Paralleling a wider movement in Western Europe (Visser [1987]), the decline of the manufacturing sector in France has led to a growing division of unionism through the development of white-collar and autonomous unions. Thus, while in the United-States, by reserving the exclusivity of bargaining to a single union, the Wagner Act of 1935 put an end to the coexistence of different unions in the same firm, multi-unionism is today the rule rather than the exception in Western Europe: 85% of the EU countries have a plurality of confederations (Akkerman [2008]), themselves divided in several unions. Occurring in a context of decline of unionism in the industrialized world (Rosanvallon [1998]), this development of pluralism has been described as a manifestation of the unsuitability of traditional unionism to a changing labour world. However, in a context of de-unionization where unions are increasingly perceived as ill-fitted to the defence of the interests of the workers, the question of whether increased pluralism contributes to solving the stakes faced by unionism or worsens them remains open: as in other political representation settings, pluralism in labour bargaining has the potential for increased electoral accountability, but also increased coordination frictions, and thus its implications appear a priori ambiguous consequences for both workers and employers.

On the one hand, assuming imperfectly-sighted unions, multi-unionism could be thought to result in coordination frictions between union leaders with different objectives, preferred strategies, ideologies, or simply personalities. This would, in turn, threaten the representation of workers' interests, in such a way that workers would lose from being represented by several unions. Assuming that bargaining between workers and employers is a zero-sum game, such a situation would benefit the firm at the expense of the workers. However, taking into account the fact that coordination frictions between unions may also result in a loss of time spent bargaining or more frequent crises leading to industrial disputes, one may also expect employers to be similarly negatively affected by the resulting coordination issues. Additionally, one should consider the second "face" of unions highlighted by Freeman and Medoff [1979], whereby, echoing Hirschman's "Exit, Voice and Loyalty" (Hirschman [1970]), unions contribute to preventing crises and exits by voicing workers' discontent, eventually leading to a better social climate and positively affecting productivity. In this light, one could expect employers to further lose from such coordination frictions between unions.

On the other hand, workers' representation may also benefit from increased accountability stemming from the increased competition between unions. Indeed, Ross (Ross [1948]), by contrast with Dunlop et al's model (Dunlop et al. [1944]) according to which a union's objective function is solely the wage bill, raised the possibility of misaligned incentives between unions and workers. He indeed highlights the fact that unions are political organizations. Recent literature has similarly highlighted the importance of possible misaligned incentives between workers and their representatives (Breda [2016]). In 1998, Rosanvallon had already highlighted that the introduction of elections meant to elect workers' representatives in French firms, which had initially been met with strong opposition by traditional unions, radically changed the way unions were being perceived and perceived themselves: previously considered as legitimate by essence, conceived as the emanation of workers' interests, French unions increasingly came to be perceived as representatives with an instrumental legitimacy solely contingent on their bargaining successes. In such a context, one may therefore think that an increase in union pluralism may result in increased union accountability, eventually benefiting workers. However, by extension of the "second face" of unions, this may also benefit employers, if better voicing of the diversity of workers' preferences allows for greater flexibility.

Thus, the questions of whether union pluralism benefits or harms workers and employers, and how, remain open. Although an extensive descriptive literature has highlighted that multi-unionism was frequently associated with industrial conflicts, straightforward interpretation of these observations is faced by a key endogeneity issue: multi-unionism is likely to arise in more conflict-prone workplaces. To solve this issue, I exploit in this dissertation newly-available data on all French professional elections held between 2009 and 2016 to take advantage of three institutional discontinuities through recourse to Regression Discontinuity Design. Although the question of the consequences of increased electoral accountability and possible redistributive implications would deserve more attention in future works, I focus on the net effect of multi-unionism on the average wage and further investigate the question of coordination frictions. I thus investigate the following questions: Do workers appear to gain or lose from the presence of more unions in a firm[2]? Can we detect evidence that the co-existence of several unions in the same workplace results in coordination frictions between unions, eventually affecting wage bargaining?

In 2008, in an attempt to respond to the decline of unionism, French legislators passed a law requiring that a union gather minimum 10% of the votes in the professional elections held in a firm to be recognized as "representative", thereby acquiring the right to bargain with the employer in the name of the workforce. Exploiting this first discontinuity thus allows me to capture how the presence of an additional (weak) trade-union with the possibility of taking part in bargaining affects wage bargaining outcomes. The net effect of an additional union on wages and economic profitability is a priori indeterminate, and so are its effects on the share of wages in the value-added and on the firm's value-added.

Although absence of precise manipulation of electoral scores is traditionally assumed to be a sufficient condition for identification in electoral Regression Discontinuity Designs, the asymmetric distribution in election sizes, compounded by a majority of small elections, results in a jump in firm size at the thresholds of interest. However, since election size is otherwise symmetrically distributed on opposite sides of the thresholds, recourse to "donut" Regression Discontinuity appears to restore the validity of the identification.

If multi-unionism solely caused coordination frictions between unions, thereby negatively affecting workers' representation, one would expect a negative effect on the share of wages in the value-added, conditional on an absence of effect on firm productivity itself. However, coordination frictions may both negatively (if they make crises more likely) or positively affect firm productivity (if union paralysis makes industrial action less likely). Thus, if coordination frictions affect firm productivity, the net effect on wages will be a priori unclear. Furthermore, if coordination frictions adversely impact workers' representation but also firm productivity, one might not witness a negative effect on the share of wages in the value-added. However, if this is the case, this would result in a negative effect on wages. However, as I argue below, although this is more unlikely, an increase in union accountability might result in a negative effect on the value-added, thereby driving a negative effect on wages. Thus, finding a negative effect on both wages and on the value-added cannot be straightforwardly interpreted as signalling coordination frictions adversely affecting workers' representation. Similarly, although this might seem unlikely, coordination frictions could result in a positive effect on value-added, eventually positively affecting wages. Nonetheless, as I argue below, increased union accountability is likely to have similar consequences.

By contrast, if the presence of an additional union in a firm results in improved representation efforts by unions, conditional on not finding an effect on firm productivity, one would expect a positive effect on the share of wages in value-added, resulting in a positive net effect on wages. However, owing to the "second" face of unions, increased electoral accountability might also positively affect firm productivity. If this were the case, one might not witness a positive effect on the share of wages in the value-added, although this should manifest itself by a positive net effect on wages. Nonetheless, as I argue above, although this may appear less likely, similar effects could also be attributable to coordination frictions. By contrast, although this might appear unlikely, as a result of the "first" face of unions, increased representation efforts by unions could negatively affect firm productivity (if more mobilizations negatively affects productivity), eventually resulting in a negative net effect on wages. However, as I argued above, similar effects could be the manifestation

[2] My focus on firm-level bargaining is motivated by the fact that, although, in the name of the "Hierarchy of Norms" principle, branch-level agreements theoretically prevail on firm-level agreements, these agreements have been shown (Breda [2015]) to be often obsolete as their majority is today less favourable to workers than national minimum wage dispositions. Furthermore, building upon the 1982 "Lois Auroux" which made annual firm-level wage bargaining compulsory, the 2015 "Rapport Combrexelle" has initiated new steps in a promotion of increasingly decentralized bargaining in France. However, while unions are traditionally perceived as particularly powerful in France, constituting key institutional actors at the national level (Rosanvallon [1998]), it has been suggested that this does not reflect their oftentimes weaker position in French firms, as suggested by the union wage premium being of solely 2%, far behind the wage premiums found in other countries (Breda [2015]). Thus, as legislators are inclined towards conferring increasing importance to firm-level bargaining, one may think this level of bargaining deserves greater attention.

of coordination frictions.

All things considered, it therefore appears that, if the presence of an additional union in a firm resulted in a negative effect on the share of wages in the value-added, conditional on not finding an effect on firm productivity, this would signal the presence of coordination frictions, whereby the presence of an additional union in a firm negatively affects workers' representation. By contrast, if it resulted in a positive effect on the share of wages in the value-added, conditional on not finding an effect on firm productivity, this would signal increased representation efforts by unions, as a result of increased accountability. However, alternative situations are less interpretable. Furthermore, since multi-unionism may result in *both* increased coordination frictions and increased representation efforts by unions, i.e. affect both the objective and resources of unions, if the two effects co-exist, they might offset one another. Hence, not finding any effect could not be interpreted as ruling out either hypothesis.

Furthermore, Regression Discontinuity Designs only identify local effects, corresponding to treatment effects on the firms in the boundary of the threshold used. Since one might expect the coordination frictions and electoral threat resulting from the presence of an additional *weak* union, as is the case when a union has only 10% of the votes, the results found would only provide a lower bound for the average effects exerted by the presence of an additional union.

The French institutional context however provides two additional discontinuities which I exploit to better understand how multi-unionism affects wage bargaining. These discontinuities allow me to better test for the presence of coordination frictions resulting from the co-existence of several unions in a firm, without restricting the analysis to the marginal coordination frictions exerted by a weak union.

First, a union which obtains more than 30% of the votes gains the possibility of signing agreements alone, as opposed to having to be part of a coalition of unions with more than 30% of the votes, thus resulting in a decreased need for coordination between unions. Thus, if the coexistence of several trade-unions in the same firm were a source of coordination frictions, negatively affecting the representation of workers' interests, one would expect a positive effect of a union reaching 30% of the votes on the share of wages in the value-added. If coordination frictions took the form of delayed but better-quality agreements however, this initial positive effect could be followed by a negative effect. Furthermore, if wage bargaining is not solely a zero-sum game, and if coordination frictions negatively affect firm productivity, one might expect a positive effect of such alleviated need for coordination on the value-added. In both cases, one would expect a positive net effect on wages, although no effect on the share of wages in the value-added (or even a negative effect) may be apparent if the increase in value-added is superior to the increase in wages. The magnitude of the effects found should be expected to decrease in the number of unions already above 30%, as the marginal effect of an additional union gaining the possibility to sign agreements alone should be low when other unions can already do so. Thus, if co-existing unions struggle to coordinate, one would expect a positive net effect of a new union reaching 30% of the votes on wages.

The last discontinuity I exploit comes form the fact that, if a union obtains more than 50% of the votes, it gains the possibility of vetoing agreements alone. In the presence of strong coordination frictions between unions, whereby competition between unions is harmful to workers' representation, one would a priori expect a negative effect on the share of wages in the value-added of a union's gaining the possibility of blocking agreements. However, if competition between unions results in greater representation effort, this could be followed by a positive effect on the share of wages in the value-added, if the vetoed agreement is followed by a more ambitious agreement.

Despite a first stage which highlights that when an additional union obtains 10% of the votes, this generates an increase ranging from 0.3 to nearly 1 in the number of unions present, exploiting the first discontinuity does not yield any evidence of a net effect on wages of an additional representative union. Despite a series of steps taken to increase the accuracy of estimates, such as recourse to log as well as a focus on growth rates and on residualized dependent variables, the minimum detectable effects are very high, systematically superior to the 2% union wage premium uncovered by Breda [2015] in the French setting. However, the inconclusive results do not seem to solely reflect this lack of accuracy: further inspection of the trends suggested by the coefficients, as well as comparison to pre-electoral differences in trends suggests an absence of effect. Estimating separately effects on the share of wages in the value-added and on the value-added by worker, as well as on features of the wage bargaining process such as signing of agreements and organization of strikes proves similarly inconclusive. However, in the presence of possible heterogeneous effects, whereby the impacts of small trade-unions, both in terms of coordination frictions and electoral competition, could be expected to only provide a lower bound for effects of an additional trade-union when all unions are equally-powerful, one should be wary not to extrapolate this result and conclude that unions coordinate perfectly in such a way that, even when numerous, they manage to act as one in the best interests of workers.

Initial results obtained when estimating the effects on wage bargaining outcomes of a union obtaining the possibility to sign agreements alone or veto them indeed appears to suggest that the co-existence of several unions in a firm affects wage bargaining through coordination frictions. Suggestive evidence in favour of a positive net effect on wages of an additional union gaining the possibility to sign agreements alone indeed appears to corroborate the above predictions. Similarly, evidence hinting at a negative effect of a union's obtaining the possibility to veto agreements on the share of wages in the value-added, resulting in a negative net effect on wages seems to corroborate these predictions.

However, the first effects on wage bargaining outcomes do not appear to be robust to choosing a narrower bandwidth. While the second effects are by contrast robust to bandwidth choice, they might appear to be partly attributable to pre-existing imbalances.

Investigation of intermediary channels appears to further shed doubts on the coordination frictions story: there indeed appears to be no effect of a union's obtaining the possibility to sign or veto agreements alone on the probability that agreements be indeed signed or vetoed. Nonetheless, suggestive evidence hints at a positive effect of the two configurations on employers' social climate perceptions, as well as a negative effect on union's mobilization during wage bargaining.

Although this would warrant further investigation and robustness checks, future research might want to explore the possibility that the effects on wage bargaining outcomes found are not the result of the actions of the union which obtains the possibility to sign or veto agreements alone, but rather by the response of other unions to this new configuration: when a union gains the possibility to sign agreements alone, this might result in demobilization by other unions, explaining employers' improved social climate perceptions. Could this explain the effects on wage bargaining outcomes suggested by an initial exploration? If demobilization positively affected firm productivity, this could in turn explain a positive net effect on wages. However, initial results had suggested a possible positive effect on the share of wages in the value-added, which appear difficult to reconcile with such an explanation. In the French institutional setting, when a union gains the possibility to veto agreements alone, it also implies that all alternative coalitions of unions lose the possibility to veto agreements. If realization that the majoritarian union can entirely determine wage bargaining resulted in resignation by the other unions, this could explain the evidence in favour of a demobilization, thereby explaining employers' improved social climate perceptions. Demobilization could also explain a positive effect on the value-added suggested by the results, although it remains unclear whether this effect should be entirely attributed to pre-existing imbalances in trends. Demobilization of non-majoritarian unions could in turn explain the evidence suggesting a negative effect on the share of wages in the value-added driving a negative net effect on wages.

This dissertation will be organized as follows. Section II will review the literature on unions and multi-unionism. Section III will put the contribution of my dissertation in perspective. Section IV will present the French institutional context, the data used and some summary statistics. Section V will detail the methodology used with a particular focus on tests of the identification assumption. Section VI will present the results obtained, first focusing on wage bargaining outcomes before turning to the wage bargaining process itself. Section VII will discuss the results and future perspectives to be explored.

2 Literature review: a causal identification challenge in the literature on unions

While a series of descriptive works in political science and in the industrial relations literature have explored pluralism in labour representation, the economic literature on unions has mainly focused on estimating the effects of unionization itself.

In this line, echoing Dunlop et al's model, according to which unions' objective is to maximize the wage bill (Dunlop et al. [1944]), most theoretical works have concluded in a positive effect of unions on wages (see Jarrell and Stanley [1990] for a meta-analysis). In the French context, a union wage premium of 2% was thus found (Breda [2015]). Following Dunlop et al, the wage bargaining has been interpreted as a zero-sum game, whereby, by increasing the wage bill, unions negatively affect firm profitability. However, Freeman and Medoff [1979], echoing Hirschman's "Exit, Voice and Loyalty", stated that unions were characterized by "two faces", highlighting that unions could positively affect firms' productivity, by allowing to voice discontent, thus preventing crises, and possibly increasing worker motivation, but also making increases in efficiency possible by exploiting rents in the heterogeneity of workers' preferences. The "Voice" of unions is however a double-edged sword: unions may negatively affect productivity by making work stoppages more likely. A series of studies by the "Harvard school" found positive effects of unions on productivity (Brown and Medoff [1978], Clark [1980] and Allen [1984] as reviewed in Laroche and Doucouliagos [2003]). Laroche and Doucouliagos [2003] carry out a meta-analysis of the existing literature measuring partial correlations between unionization and productivity and however conclude that, on average, there is no evidence of a negative association between unionization and productivity, despite evidence of a negative association between unionization and productivity growth, as well as a negative association between unionization and firm profitability (Laroche and Doucouliagos [2009]).

Most of this literature however relies on partial correlations, which raises two key issues. First, causal interpretation is frequently prevented by endogeneity: for instance, unions may be more likely to form in the firms where the rents they can capture are larger (selection bias), or in dysfunctional firms where conflicts pre-exist (reverse causality). The second issue stems from the main solution which has been used to limit the resulting biases, that is, controlling for a series of observables. Indeed, in the presence of equilibrium effects, whereby, for instance, unionization may affect both the share of wages in the value-added and the value-added itself, recourse to controls may prevent estimation of the full effect of unions.

To solve these issues, DiNardo and Lee [2002, 2004], Lee and Mas [2012] and Frandsen [2012] have exploited a minimum electoral score of 50% required for unionization in American firms, thereby running RDDs to measure the local effect of unionization on the above-mentioned economic outcomes (wages, firm profitability as captured by a firm's stock market value, the likelihood of firm closure) and found null or very small impacts, contradicting the common wisdom according to which the economic effects of unions – at least on wages – are major.

DiNardo and Lee [2002, 2004], Lee and Mas [2012] as well as Frandsen [2012]'s recent works have marked a turn in the literature on unionization towards causal identification. By contrast, in the literature on multi-unionism, covering a large extant of descriptive works in political science and industrial organization and a scarcer multivariate regressions economic literature, causal identification yet has to be attempted.

This literature is two-fold. The political science literature has mainly focused on cross-country comparisons or case studies, describing the association between multi-unionism and conflicts (see for instance Battista [1991], Clegg [1976], Galenson [1940], Gitlow [1952], Korpi and Shalev [1979], Krislov [1960], Ross [1960], Ross and Irwin [1951] as reviewed in Akkerman [2008]). The economic literature on the subject (Machin et al. [1993], Naylor [1995], Dobson [1997]) has, by contrast, only focused on the United-Kingdom, responding to earlier concerns with respect to the economic impacts of unions in 1980s' Thatcherite Britain.

Focusing on the most conflict-prone industries through surveys of employers and employees, Dobson found no evidence of a clear effect of multi-unionism on conflicts in British firms, thus concluding that, in contrast with the depictions of chaotic multi-union configurations, British multi-unionism was marked by a form of rationality, taking advantage of complementarities between different segments of the workforce. This second strand of the literature has otherwise mainly focused on economic outcomes, exploring effects on productivity and wages. Thus, while Dobson concluded in the absence of effects on productivity, Machin et al. found that workplaces with certain multi-union configurations were, controlling for a set of characteristics, characterized by higher wages.

However, the external validity of the explanation which Naylor advanced for Machin et al's finding may be thought not to extend to West European multi-unionism in general: just as Dobson's explanation for the absence of negative effects on conflicts and productivity, Naylor's explanation relies on the complementarity of craft-specific British unions, compounded by a pragmatic self-selection ("à la Roy") of unions into the configuration which benefits workers the most. By contrast, Akkerman [2008] highlights that, although 25% of the Western European countries where multiple union confederations coexist are characterized by a multi-unionism divided along professional lines (thus paralleling the British craft-specific workplace multi-unionism), 50% of these countries are characterized by a labour movement characterized along political lines. This is particularly the case in France, where the union movement is very politicized, the main unions rejecting the idea of division along professional lines in the name of universalism.

Thus, one may think that, by contrast with the British context, the French multi-unionism scene presents a greater potential for conflicts. Henceforth, one could expect the co-existence of several trade-unions in the same workplace to result in net losses for workers. Nevertheless, the recent literature has coined the question of misaligned incentives between union representatives and workers as particularly concerning in France in the absence of accountability mechanism incentivizing unions to represent workers' interests instead of their own (Breda [2016]). In such a context, pluralism in workers' representation might therefore have the potential for a better representation of workers' interests.

3 Contribution to the literature: RDD exploiting a new institutional discontinuity with newly-available data for causal identification

The existence of minimum electoral scores in professional elections (10%, 30% and 50%) required for a union to have the possibility to take part in bargaining, but also act alone when signing or vetoing agreements provides a solution to the endogeneity issues which have been encountered by the literature on multi-unionism. To this end, I exploit the newly-available MARS dataset ("Mesure de l'Audience et de la Représentativité Syndicale")[3] which includes the minutes of all professional elections held in France between 2009 and 2016. This allows me to reproduce a Regression Discontinuity methodology similar to the RDDs which DiNardo and Lee [2002, 2004], Lee and Mas [2012] and Frandsen [2012] use to estimate causal effects of unionization.

By using the minutes of professional elections recorded in the MARS dataset, I thus constructed rank-specific electoral scores (score of the first to tenth union). Using these scores as running variables to run RDDs exploiting these three thresholds allows me to capture, under the assumption that trade-unions - or firms - have no precise control on their electoral scores, the intention to treat effect (ITT) of having an additional (weak) trade-union which can take part in bargaining in a firm, as well as that of having an additional trade-union capable of signing agreements alone, but also of having a majoritarian trade-union capable of blocking the bargaining process (or of a coalition of unions losing the possibility of blocking an agreement signed by the largest union).

Exploiting the 10% representativity threshold only enables to capture a local effect, namely the effect of an additional weak union having the possibility of taking part in bargaining, thus providing a lower bound for the effects of multi-unionism, since one may expect the paralysis and competition effects from the part of weak trade-unions to be weaker than those exerted by stronger

[3] I had access at the to the MARS dataset, along with the BRN, FARE, Agrifin, Acemo and REPONSE datasets I have been using in this dissertation thanks to the convention between the Paris School of Economics' Chaire Travail and the DARES (the French Directorate for the Animation of Research, Studies and Statistics of the Ministry of Labor)

trade-unions. Accordingly, my results suggest that the effects - on wages, on firm productivity, but also on strikes or the signing of agreements - of an additional weak bargaining union is null. However, this may be thought to reflect heterogeneous effects, whereby the marginal effects of a weak union are very low. This would echo Lee and Mas [2012]'s finding that, while the local effect on firm profitability of unionization resulting from close race appears null, the average effect of unionization, as revealed by an event study, is much larger. Indeed, by completing this by a study of the 30% and 50% thresholds, I uncover suggestive evidence that, when a union gains the possibility to circumvent coordination with other unions to sign or veto agreements or, alternatively, when other unions lose the possibility of vetoing an agreement signed by the larger union, wage bargaining outcomes and the wage bargaining process are affected. These last results initially seem to corroborate the predictions from a model whereby the co-existence of several unions in a workplace is a source of coordination frictions, negatively affecting workers' representation. However, further investigation of their robustness would be warranted. Furthermore, investigation of intermediary mechanisms seem to suggest a more complex picture than this simple coordination frictions story: while these two configurations appear to have no effect on the probability that an agreement be signed or vetoed, contradicting interpretations in terms of coordination frictions, suggestive evidence hints at possible demobilizing effects and a positive effect on employers' social climate perceptions. Although greater investigation of the robustness of the results and channels at play would be desirable, I suggest that one may want to investigate the possibility that the effects found are driven by the response of other unions to a large union's monopolistic position.

Although recourse to RDDs in electoral settings is common, I uncovered a particularity characterizing the configuration studied here which, to the best of my knowledge, has not been encountered in other settings.

Indeed, by contrast with other electoral settings, professional elections are characterized by two distinctive features. First, most elections involve a small number of voters, in such a way that the running variables used cannot be thought of as purely continuous variables: by construction, since an electoral score corresponds to the ratio of two integers, their support includes some gaps. However, the particularities of the setting I study go beyond discreteness of the running variable (which has been studied by Kolesár and Rothe [2018]). Indeed, discreteness of the running variable is compounded by the asymmetric distribution of election sizes: although most elections involve only a small number of voters, a few elections involve a very large number of voters. The combination of both characteristics results in a Data-Generating-Process mechanically generating discontinuities in firm size at the thresholds of interest.

This implies that the firms exactly to the left of the threshold studied are not a valid counterfactual for the firms situated at the threshold, since they are characterized by a larger workforce size. However, since firm size is otherwise symmetrically-distributed on opposite sides of the thresholds, I follow Barreca et al [2011] and run "donut" RDDs by excluding observations at the threshold, thus restoring the validity of the identification.

4 Data, institutional background and summary statistics

4.1 Institutional setting and electoral data (MARS 2009-2016)

In 2008, French legislators passed a law which put an end to the "Présomption Irréfragable de Représentativité": while unions affiliated to the five historical confederations previously had the "de jure" right to speak in the name of the workers of a firm, they are now required to have obtained minimum 10% of the votes in the first round of professional elections[4] to have the possi-

[4]The scores correspond to the share of the validly-expressed votes - thus excluding spoilt and null ballots - obtained by the list affiliated to the corresponding union in the first round of the professional elections. This score is computed regardless of the number of voters, i.e. it is computed even if the "quorum" is not reached in such a way that the number of votes is too low for the election to be valid with respect to the election of representatives.

While for most unions, the electoral score is computed by dividing the number of votes it has received by the total number of votes across all colleges – regardless of whether the union has presented lists in all colleges -, certain bility to take part in bargaining. Aiming to ensure that unions speaking in the name of a workforce be representative, the reform instituted a second discontinuity, whereby, to have the possibility of signing agreements in the name of the workforce, a union is required to have obtained minimum 30% of the votes, or else sign along with a coalition of unions which obtained minimum 30% of the votes. These two requirements completed an earlier requirement of 50% of the votes necessary for a union or coalition to veto an agreement[5].

As I chose to focus on firm-level bargaining[6], I used the MARS dataset to construct firm-level electoral scores[7].

Recording the minutes ("Procès verbaux" in French) of all professional elections held in France between 2009 and 2016, the MARS dataset includes a total of 370,548 election minutes, resulting in a total of 64,312 firm-level election perimeters after dropping firms for which scores could not be computed with certainty. In each voting place, after the counting of votes, a separate minute is filled for each electoral college. It records among other information the other voting places concerned by the election, the institution and college concerned, the election date and planned mandate duration, as well as the total number of votes and the number of votes received by each list and the union to which this list is associated, thus making it possible to compute electoral scores as well as precisely determine the start and end date of each mandate.

Professional elections[8] are compulsory in all firms of eleven or more employees. However, 39.7% of the election minutes are characterized by a "carence", i.e. an absence of candidate, and some employers may not take the initiative of organizing elections - their firms being therefore absent from the dataset - if not asked to do so by employees.

A union's representativity - which determines whether it can bargain in the name of the workers, but also whether it needs to be part of a coalition to sign or veto an agreement - ends with the

unions, labelled as "catégoriel" are treated differently. They are meant to represent a certain category of workers and solely bargain or sign agreements in the name of the latter. Therefore, under two conditions, their representativity should be evaluated only in the colleges whose composition correspond to the workers it is meant to represent. This is notably the case of unions affiliated to the "Confédération Générale des Cadres" (CGC) meant to represent "cadres", but also of the Syndicat National des Journalistes (SNJ) meant to represent journalists and of the Syndicat National des Pilotes de Ligne (SNPL) meant to represent plane pilots. For this, a union must not present a list in other colleges and its written status has to explicitly state it is "catégoriel". This implies that the scores of the corresponding unions may suffer from measurement errors. Indeed, the data used does not contain information on the written statuses of a union. Therefore, I assumed that the second condition was fulfilled whenever the first one was fulfilled.

[5]Since the "Loi Travail" of the 8 August 2016, the 30% and 50% thresholds are calculated based on the votes received by *representative unions* only, that is by unions which obtained more than 10% of the votes.

[6]I initially planned to focus on establishment-level bargaining. However, as wages are mainly-determined at the firm level and as the data I was planning to use to measure economic outcomes at the establishment level (the establishment-level DADS) included many missing values, I eventually decided to focus on firm-level bargaining.

[7]Although, for the purpose of this dissertation, I focused on rank-specific scores, i.e. the score of the second to fifth union, I also constructed union-specific electoral scores. Related summary statistics, displaying the frequency of the presence of the main French unions in firms, as well as the frequency of the presence of each of these unions in different ranks can be found in Figures 7 in Appendix 8.1..

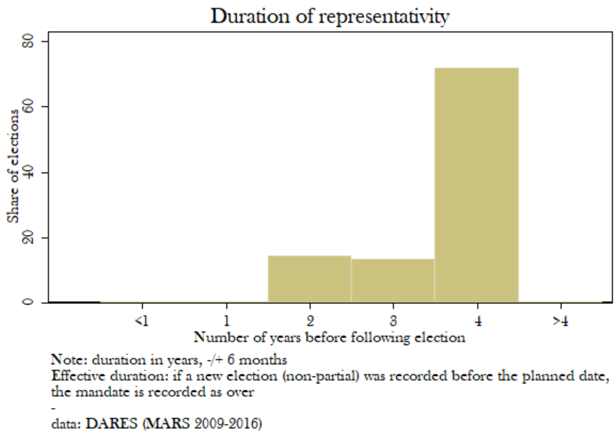
[8]These elections are initially meant to elect representative to three bodies (the "Comité d'Enterprise"/"Comité d'Etablissement" (CE), the "Délégués du Personnel" (DP) or the "Délégation Unique du Personnel" (DUP)) with consultative and information roles. When different bodies coexist, only the CE is used to compute union representativity.

Although the primary function of these elections is not the election of union representative, in the first round of this list system, each list must be associated to a union, in such a way that union representativity is computed using the results of the first round of these elections.

These three bodies may exist at four different levels: the "Unité Economique et Sociale" (UES) which groups together different firms, the firm itself, the geographically-distinct workplace ("establishment") or the "distinct establishment", a subset of the firm which may include several physical establishments and whose perimeter is bargained over. For their existence at a level, a workforce size criteria must be fulfilled at the corresponding level (minimum eleven employees for the existence of DP, 50 for the existence of a CE, between 50 and 200 for the existence of a DUP). Thus 27.8% of the election minutes correspond to multi-establishment perimeters, in such a way that computing electoral scores require the aggregation of the results reported in different election minutes. When the institutions of a multi-establishment firms are only defined at a lower level, firm-level representativity is computed by aggregating the votes received in all establishments. The possible coexistence of institutions at different levels in the same firm implies that a union may be representative at one level but not another, thereby having the right of taking part in bargaining only at the level where it is recognized as representative.

mandates of the representatives whose election was used to measure representativity. However, in the name of the principle of continuity of representativity, partial elections which correspond to 2.9% of election minutes (e.g. elections organized because the number of sitting representatives in a representative body had become insufficient) are not taken into account when computing union representativity. Nonetheless, some elections may be organized before the planned end of mandate and entail a renewal of representativity (e.g. if a UES is created, all elections must be scheduled at the same date, implying that some mandates may be shortened). While mandates in representative bodies, and thereby union representativity, theoretically last four years, they may be shorter in certain firms due to branch, firm or group agreements. As a result, as illustrated in Figure 1, only 72% of the mandates effectively last four years, while 14.5% last two years and 13.3% last two years.

Figure 1: Distribution of duration of representativity span (years +/-6 months)



This phenomenon is reflected in the imperfect four-year cyclicity of the data illustrated by Table 1.

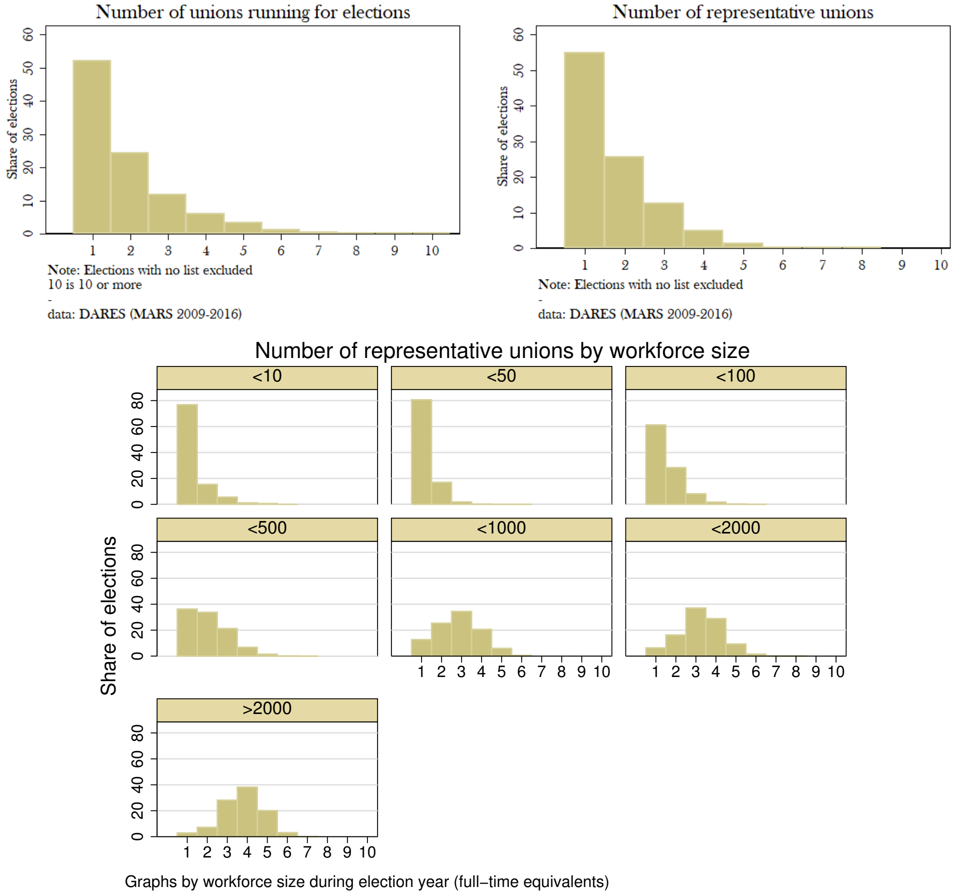
Table 1: Descriptive statistics: distribution of election years

Distribution of election years								
Year (start of representativity)	2009	2010	2011	2012	2013	2014	2015	2016
Number of firm level election perimeters *(used to compute union representativity only)*	**5,228**	**9,334**	**8,340**	**5,732**	**7,248**	**10,317**	**9,406**	**6,807**
Stems from:								
Number of election minutes recorded in MARS	32,406	62,768	51,255	33,759	46,840	65,562	45,172	32,784
Number of election minutes recorded in MARS *(carence and partial elections excluded)*	19,875	36,280	29,071	18,644	25,111	36,088	30,823	22,183
CE *(carence and partial excluded)*	7,321	12,581	9,973	6,830	8,728	12,639	10,821	7,566
DP *(carence and partial excluded)*	9,600	18,171	14,286	8,886	12,555	17,099	14,631	10,469
DUP *(carence and partial excluded)*	2,904	5,528	4,812	2,928	3,838	6,350	5,371	4,148
Note: data: DARES (MARS 2009-2016)								

Despite the legal obligation to hold professional elections, and albeit the institutional barriers to multi-unionism are very low (as long as it is supported by minimum 10% of the voters, a second union can bargain in the name of the workers), firm-level multi-unionism may at a first glance seem a limited phenomenon as it only concerns a restricted number of firms for a series of reasons. First, as previously mentioned, there is no legal enforcement of the organization of elections unless it is demanded by employees of a firm. Second, 39.8% of the recorded election minutes correspond to elections with no candidate. Third, as illustrated by Figure 2, out of the remaining elections, 52.2% are characterized by a single list running, resulting in close to 54.8% of elections leading to a single-union being representative at the firm level. Furthermore, out of the elections which led to several unions being representative in a firm, 57% are characterized by solely two unions, 28% by three unions, 11% by four unions and 3% by five unions, implying that less than 1% led to six or more representative unions.

However, Figure 2 also highlights that multi-unionism is far more widespread among large firms: although more than half of the elections (out of the election with candidates) result in single-union situations in the firms with less than 100 employees, multi-unionism concerns more than half of the firms with more than 100 employees and the larger the workforce size, the higher the number of representative unions tends to be.

Figure 2: Distribution of number of unions running and number of representative unions



4.2 Wage bargaining outcomes and economic performance (BRN 2007, FARE and Agrifin 2012-2017)

To build firm-level measures of wage bargaining outcomes and economic performance in the years following the elections, I used the FARE and Agrifin datasets provided by the INSEE which compile firm-level annual accountability data from fiscal sources[9]. Since I also use pre-electoral outcomes to assess the validity of the RD design and put the coefficients obtained in perspective, I also used data from the BRN 2007 database (the precursor of the FARE database). Out of the 40,097 firms for which I have retrieved electoral results from the MARS dataset, 32,225 are also covered by the FARE or Agrifin datasets, corresponding to a total of 49,591 elections.

I thus used as dependent variables firm level measures of the annual gross average wage[10], annual value-added by worker[11], annual share of the wage bill in the value-added[12] and annual economic profitability[13] one to three years after the election year, provided the corresponding year

[9]The Agrifin dataset covers firms from the agricultural and financial sectors.

[10]Expressed in thousands of euros and computed by dividing the annual gross value-added by the full-time equivalent workforce size recorded for the corresponding year.

[11]Expressed in thousands of euros and computed by dividing the annual gross value-added by the full-time equivalent workforce size recorded for the corresponding year.

[12]Replaced by missing values whenever the numerator or denominator is negative since negative wage bills – which occurred for 0.02% of the observations (using firm*year as observation level) – constitute obvious anomalies and since, although negative value-added – strictly negative for 2.5% and null for 0.4% of the observations – may realistically occur when a firm's intermediary consumption is superior to its production, the resulting negative ratio would not be interpretable as the share of the value-added captured by the workers.

[13]Measured by dividing the "excédent brut de production", i.e. the difference between the value-added and the wage bill, by the value of the long-term investments the corresponding year, and, for interpretability, counted as

was effectively part of the mandate. To maximize the statistical power available, I pooled together elections held on different years but converted all economic variables in 2009 terms using the INSEE inflation series and had recourse to year fixed effects.

Although normalizing the wage bill and value-added by firm size is not theoretically required for identification, in a context where identification requires restriction to a narrow bandwidth, thereby limiting the number of observations effectively available, such normalization can contribute to reduce the weight of outliers, thus gaining precision. As illustrated by Table 2[14], however, despite this normalization, these outcomes remain characterized by major outliers likely to greatly affect the accuracy of the estimates.

Table 2: Descriptive statistics: Economic outcomes, raw data

Descriptive statistics: raw data – 2007-2017, pooled							
Median	**Mean**	**Min**	**Max**	**Standard deviation**	**Nb. observations**		
Workforce size*							
54.5	195.5109	0	264,781	1,504.593	All	Workforce size of 0	Workforce size of 0 and strictly positive value-added
					289,751	12,260	10,293
Average annual gross wage**							
31.52	40.74	-7443.479	44,952.57	222.96	All	Strictly negative average wage	
					261,354	57	
Value added by worker***							
53.6550	84.4330	-117,314.1	175,407	971,095	All	Null value-added	Strictly negative value-added
					261,354	1,019	6,494
Share of wages in the value-added****							
0.5910	1.3406	0	55,327.25	118.4295	All	Missing because negative/null value-added	Missing because negative wage
					265,840	10,246	156
Economic profitability*****							
0.0896	-4,4966	-783,595	73,984.71	2,202.74	All	Missing because negative assets	
					270,255	44	

Notes: * in full-time equivalents for 2008-2016, average number of employees over the year for 2007
** obtained by dividing gross wage bill by workforce size in full-time equivalents for 2008-2016, by number of employees for 2007 –
Unit: thousands of euros, 2009 terms
*** value-added (production-intermediary consumptions) divided by workforce size in full-time equivalents for 2008-2017, by average annual number of employees for 2007
Unit: thousands of euros, 2009 terms
**** share of wages in value added = (annual gross wage bill)/(production-intermediary consumptions)
***** economic profitability = (value-added - gross wages + net taxes) / durable assets
Sources: INSEE (BRN 2007, FARE 2008-2017, Agrifin 2012-2017 – inflation series)

To further reduce the weight of outliers and increase precision, I thus used the log of the average wage and the log of the share of the wage bill in the value-added as dependent variables[15], as well as their growth rates or increases (in the case of the value-added by worker and economic profitability, since the growth rate of a negative value is not interpretable) compared to pre-election values and excluded all observations which lie in the bottom or top 1% of the distribution of the variables

missing whenever long-term investments are negative (which occurred for 0.01% of the observations), in such a way that economic profitability is systematically negative when the "excédent brut de production" is negative and only in such cases.

[14] A detailed yearly decomposition can be found in Tables 24 and 25 in Appendix.

[15] I however initially retained levels for the average value-added and economic profitability, since using log would imply ignoring negative values, but completed the analysis of effects on average gross value-added by using log and growth rates for the average gross value-added since negative values are less frequent than for economic profitability.

used, as well as observations characterized by a variance of any of these variables or of its workforce size (normalized by the mean value to avoid systematically-excluding large firms) within the top 1% percentile of the normalized variance distribution of the corresponding variable. Since extreme outliers are likely to reflect poor data quality or exceptional economic phenomena unrelated to multi-unionism and may therefore shed doubt on the interpretability of all the reported economic characteristics for the corresponding firm, I chose the conservative option of excluding all observations which, for any of the variables of interest, lied in the bottom 1%, top 1% of the distribution or were characterized by a normalized variance superior to the 99^{th} percentile. Excluding these observations results in 42,265 out of the initial 49,591 elections remaining available (corresponding to 27,821 out of 32,225 firms) and, as illustrated by the comparison of the descriptive statistics of the trimmed data in Table 3 to that of the raw data in Table 2, drastically reduces the influence of outliers on the mean. Table 26 in Appendix compares the growth and increases (during the year preceding the election, and up to three years after the election, compared to before the election) in these variables, before and after trimming outliers. It similarly highlights that trimming reduces the weight of the outliers on the average growth rates or changes in the variables studied.

Table 3: Descriptive statistics: Economic outcomes, trimmed data

Descriptive statistics: trimmed data used in the analysis – 2007-2017, pooled					
Workforce size*					
Median	**Mean**	**Min**	**Max**	**Standard deviation**	**Nb. observations**
56	166.9819	0	80,411	615.183	202,171
Average annual gross wage**					
31.1529	34.6962	0	1132.227	17.403	180,967
Value-added by worker***					
54.2160	64.9225	-3,406.866	1,764.28	49.655	182,042
Share of wages in the value-added****					
0.5850	0.6362	0	1827.557	4.462	185,844
Economic profitability*****					
0.1014	-0.0807	-19,150	17,373.43	79.374	186,344

Notes: Election year and years not part of mandate systematically excluded (except placebo)
* in full-time equivalents for 2008-2016, average number of employees over the year for 2007
** obtained by dividing gross wage bill by workforce size in full-time equivalents for 2008-2016, by number of employees for 2007 –
Unit: thousands of euros, 2009 terms
*** value-added (production-intermediary consumptions) divided by workforce size in full-time equivalents for 2008-2017, by average annual number of employees for 2007
Unit: thousands of euros, 2009 terms
**** share of wages in value added = (annual gross wage bill)/(production-intermediary consumptions)
***** economic profitability = (value-added - gross wages + net taxes) / durable assets
Sources: INSEE (BRN 2007, FARE 2008-2017, Agrifin 2012-2017 – inflation series)

4.3 Union presence and intermediary channels: agreements, strikes, social climate (Acemo 2009-2016 and REPONSE 2011, 2017)

To better understand the economic impacts of the multi-unionism configurations studied, I investigated the impacts of these configurations on the signing of agreements, recourse to veto, frequency and length of strikes, mobilization during the wage bargaining process and social climate.

To do so, I matched the MARS data with two other datasets which originate from surveys of employers on professional relations in their workplace, the "Enquête Acemo sur le Dialogue Social en Entreprise" (surveys 2009-2016 which cover professional relations from 2008 to 2015 since each wave covers the professional relations during the previous year) and the REPONSE (Relations Professionnelles et Négociations Salariales en Entreprise, Représentants de la Direction - 2011 and 2017). These datasets also enable me to measure union presence in workplaces and thereby realize first stages.

Although studying the effects of these different configurations on intermediary channels should a priori allow greater accuracy than when using more noisy outcomes of the wage bargaining process as dependent variables, both the Acemo and REPONSE datasets only cover representative subsamples of, respectively, French firms and French establishments[16], thereby limiting the statistical power available (out of the 32,225 firms for which the MARS dataset makes it possible to compute electoral scores, 15,433 appeared at least once in Acemo, 60% of them appearing only once and only 5% of them appearing in all waves, corresponding to approximately 3,000 firms for each wave of the Acemo surveys ; in the case of the REPONSE dataset, 4,699 establishments - out of the 136,142 establishments for which the MARS dataset makes it possible to compute electoral scores - appeared in both the REPONSE and MARS datasets).

The Acemo surveys are conducted every year. However, since surveyed firms are not systematically surveyed every year, to maximize the statistical power available, I did not distinguish between the different years of the mandate but computed the outcomes as proportions of the years for which the information was available (e.g. the proportion, out of all the years in the mandate for which the firm was surveyed, of the years when an agreement was signed).

By contrast, the REPONSE surveys are carried out every three years, in such a way that most questions concern indiscriminately what happened in the establishment over the last three years before the interview. However, this is not the case of the questions on the signing of agreements and organization of mobilizations during the wage negotiations which concern only the year preceding the interview, i.e. 2010 and 2016 (or the year of the last effectively-held wage negotiations - as indicated in the survey - in the case of the question on mobilizations in the 2017 wave). As the 2017 dataset includes the interview date, to improve the accuracy of the estimates, when measuring effects on other outcomes than the signing of agreements or organization of mobilizations during the wage bargaining process, I only kept observations for which the interview date was included in the mandate corresponding to the election used to compute electoral scores. By contrast, the interview date was absent from the 2011 data, but it included the date of the last professional elections, thus I used this information to only keep observations for which this coincided with the election date recorded in the MARS dataset. Out of the 4,699 establishments which appeared in both the MARS and REPONSE data, 4,062 fulfilled either of these conditions. When using the signing or vetoing of agreements or organization of mobilization during the wage bargaining process, I only kept observations for which 2010 or 2016 (or the years of the last effectively-held wage negotiations in the case of the question on mobilizations in the 2017 survey) was strictly included in the mandate, which corresponds to a total of 2,464 establishments.

When an information (e.g. the organization of a strike) was present in both the Acemo and REPONSE surveys, I therefore privileged recourse to the Acemo survey, since I can ensure that the outcome has only been measured over a period strictly included in the mandate, while doing so for REPONSE would have warranted restricting the sample to the establishments where unions' representativity span started strictly before 2008 or 2014 and ended strictly after 2011 or the 2017 interview date, thereby drastically restricting the sample size[17].

Thus, I used both the Acemo and REPONSE datasets to measure effects on the signing or vetoing of agreements[18], solely used the Acemo dataset to estimate effects on the holding of strikes but used the REPONSE dataset to measure effects on the holding of a mobilization during the wage bargaining process and the quality of social climate.

In the case of strikes, I used both the proportion of years during which a strike occurred and the "Journées Individuelles Non Travaillées" divided by the number of years – during the mandate - during which the firm was surveyed in Acemo, which I then divided by the number of voters in

[16] Since the REPONSE surveys are carried out at the establishment level, and since limiting myself to single-establishment firms would have further limited the sample size available, I used establishment-level electoral scores as running variables and therefore only studied effects on outcomes measured at the establishment level when using data from these surveys.

[17] This would have implied a restriction to the only 682 observations corresponding to four-year mandates starting in 2014 - as the elections held in 2008 are absent from the MARS data and, as, since they were held before the law ending the Présomption Irréfragable de Représentativité entered in application, could not lead to a discontinuity in union presence or union's signing rights.

[18] Starting from the 2013 Acemo survey, the questions on the signing of agreements do not discriminate between negotiations held at the level of the firm, establishment or "Unité Economique et Sociale", thus I restricted the sample to firms that were not part of a "Unité Economique et Sociale" for greater accuracy, but not to single-establishment firms since this would have substantially reduced the sample size and since most wage negotiations are not held at the establishment level.

the election as a normalization to build a proxy for strike length and importance. Indeed, while one may want to distinguish between effects on the probability of strike and on the length of strikes, the latter information cannot be measured per se as the Acemo surveys solely record the total number of "Journées Individuelles Non Travaillées", i.e. the number of days of strike over the entire year, multiplied by the number of employees taking part in each day of strike. If the proportion of voters taking part in a strike were identical in all strikes, dividing the number of "Journées Individuelles Non Travaillées" would therefore enable me to obtain a proxy for the number of days of strike, therefore preventing small sample bias from arising as a result of imbalances in firm size (and thereby in the number of strike participant and thus in the number of "Journées Individuelles Non Travaillées") on different sizes of the threshold. However, since the ratio of the number of strike participants over the number of voters in the last election will mechanically be higher in the firms where the strike gathers more support from employees, conditional on the same level of civic participation as measured by voter turnout, this proxy captures both the length of the strike and the support it receives from employees. Nonetheless, even after this normalization, there remain large outliers[19] likely to bias the results. Since using log to reduce the weight of outliers would have entailed exclusion of the firms where no strike occurred, thus resulting in a selection bias, I used levels but excluded the firms where the normalized number of days of strike was superior to the $99th$ percentile[20] (out of all the firms where a strike occurred over the period for which the information was recorded).

5 Methodology: Fuzzy Regression Discontinuity Design

5.1 10%, 30% and 50%: three local Intention to Treat Effects

Exploiting the 10%, 30% and 50% thresholds for bargaining, signing and vetoing allows me to identify the effect of an additional union *having the possibility* to take part in bargaining, sign agreements alone or vetoes them alone: when a union reaches one of these thresholds, one would expect a jump in the probability that it undertakes the corresponding action.

However, it does not ensure that the latter action is indeed undertaken by the union. Since professional elections are primarily meant to elect representatives in the CE, DP or DUP rather than union delegates, a union may not have any union delegate (whose role is to take part in bargaining) in a firm, despite having reached 10% of the votes. Similarly, a union may obtain 30% or 50% of the votes but nonetheless refrain from signing or vetoing agreements, or even have no union delegate in a firm. While treatment effects could have been recovered by instrumenting treatment status (= whether the union takes part in bargaining, whether it signs an agreement alone, whether it vetoes an agreement alone) by the assignment status (= whether the union's electoral score is superior to the threshold), the latter information are not available in the dataset used. In section 6.2, I however use the REPONSE and Acemo dataset to realize first stages verifying that a union's obtaining more than 10% of the votes indeed results in a jump in union presence.

In the case of bargaining, the effect of interest is the treatment effect rather than the intention to treat effect, i.e. the effect of the actual participation of an additional union in bargaining. With respect to the other two thresholds however, the intention to treat effects are the main effects of interest: to know whether the simultaneous presence of several trade-unions in the bargaining and signing process is a source of coordination issues, eventually affecting wage bargaining outcomes, we are more interested in whether a union's *having the possibility* to sign agreements alone (and therefore not needing to coordinate with other unions) or to veto agreements alone (and therefore able to block the signing process) eventually affects the wage bargaining outcomes.

[19] After normalizing by the number of voters and number of years over which the information is recorded, the maximum is 208,333 days, i.e. superior to one year, which suggests some measurement error. For comparison, out of the firms which experienced a strike (depending on the year, this concerns 19% to 32% of the sampled firms), the median normalized number of days of strike ranged from 0.13 to 0.43 depending on the year.

[20] Which corresponds to 20.75 normalized days of strike.

5.2 Econometric specification

To estimate these Intention to Treat Effects, I thus used the firm*election year as observation level[21], pooling all election years (2009 to 2016) together to increase the statistical power available and clustering the standard errors at the firm level (using heteroskedasticity-robust standard errors). Indeed, since a firm's wage policy is likely to be correlated over time and a union's score in the same firm is similarly likely to be correlated over successive elections, failing to account for clustered would result in risks of spurious small-sample correlations between the dependent variable and electoral scores, eventually over-estimating the accuracy of the estimates[22].

First focusing on the economic outcomes previously described, I thus used as dependent variables, log, levels or changes (growth rate or increase compared to the variable's value the year before the election) of the average gross annual wage, annual value-added by worker, share of annual wage bill in the annual value-added, annual economic profitability, one, two and three years after the election, provided the corresponding year was effectively included in the representativity span.

Investigating intermediary channels, I then used as dependent variables the signing of agreement[23], organization of strikes[24], normalized strike duration, quality of social climate[25].

Using as running variables the score of the first to fifth union, and as thresholds 10%, 30% and 50%, I thus estimate the following equation, with $\boldsymbol{\beta}$ capturing the Intention to Treat Effect (e.g. the effect of a second union having the possibility to take part in bargaining for $r = second$ and $C = 10\%$, or the effect of the first union having the possibility to sign agreements alone for $r = first$ and $C = 30\%$).

$$\begin{aligned} Y_{f,T+s} = \alpha + \boldsymbol{\beta}.\{score_{f,T,r} \geq C\} + \delta_1.(score_{f,T,r} - C)\{score_{f,T,r} \geq C\} \\ + \delta_2.(score_{f,T,r} - C)\{score_{f,T,r} < C\} + \gamma.X_{f,T} + \epsilon_{f,T+s} \end{aligned} \tag{1}$$

with:

$$f = firm \tag{2}$$
$$T = election\ year \qquad s \in \{1, 2, 3\} \qquad T + s \leq year\ when\ representativity\ span\ ends \tag{3}$$
$$C \in \{10\%, 30\%, 50\%\} \tag{4}$$
$$r \in \{first,\ second,\ third,\ fourth,\ fifth\} \tag{5}$$
$$|score_{f,T,r} - C| \leq bandwidth \tag{6}$$

Thus, $\boldsymbol{\beta}$ captures the effect on the dependent variable of a union's score being above the threshold, after controlling for the effect of the score itself on the running variable.

I model the relation between the score and the dependent variable as locally linear[26] (first in

[21] When studying intermediary outcomes using the REPONSE data in Section 6.2, I use the establishment*election year as observation level.

[22] Heteroskedasticity-robust standard errors tend to still over-estimate the precision of the estimates when the number of clusters is limited and clusters are unbalanced. However, since a firm can, at most, appear eight times in the data, and since 72% of the mandates last four years, the clusters tend to be small and balanced. This problem is more serious when using the establishment*year as observation level when using REPONSE data to study intermediary outcomes since one firm may count 1 (in the case of 71% of the firms) to 1,671 establishments.

[23] When using REPONSE, measured by an indicator variable equal to one if an agreement was signed, when using Acemo, measured both by an indicator variable equal to one if minimum one agreement on wages was signed during any of the year of the representativity span for which the firm appears in Acemo, and the proportion of these years when an agreement was signed.

[24] The share of the years (out of the years in the representativity span and for which the firm was surveyed in Acemo) when a strike was organized.

[25] Measured by a series of indicator variables equal to one if the respondent qualified the social climate as "good", "bad", or having "improved" or "worsened" during the three years prior to the interview.

[26] Except when using data from REPONSE when, due to the lack of observations - less than 30 on one side of the threshold, hence preventing inference - available, I sometimes use the full sample and have recourse to a polynomial of order 2.

a bandwidth of 5%, then in a bandwidth of 2%[27] to realize a robustness check), but allowing for the slope of that relation to be different on opposite sides of the threshold ($\delta_1 \neq \delta_2$)[28].

Under the identifying assumption that the running variable is locally random, and assuming that small variations in the score itself do not *directly* affect the dependent variable (e.g. that a union with 10.1% of the votes will not, as a result of its lower score, be more or less motivated than a union with 10.15% of the votes, thus behaving differently), the jump in the dependent variable captured by β will identify the treatment effect as the difference in the value of the dependent variable between firms at the threshold and those slightly below is not be driven by the electoral score (e.g. a more motivated union due to a higher score) nor any unobservable or observable affecting the electoral score (e.g. higher support for the union or higher motivation leading to more campaigning and therefore a higher score). Since the closer to the cut-off, the more plausible the assumption of local randomness and therefore the better the counterfactual, I have recourse to triangular kernels giving greater weight to the observations closest to the cut-off.

5.3 Identification

Since identification of these intention to treat effects relies on the treatment status (whether a union takes part in bargaining, can sign an agreement alone or veto an agreement alone) being a discontinuous function of a union's electoral score, in such a way that a jump in the outcomes at the treatment eligibility cut-offs can be attributed to the treatment, identification warrants the absence of other factors capable of causing a discontinuity in the outcome at the cut-off considered.

In a review of the literature on RD designs, Lee and Lemieux [2010] argue that, in the absence of another policy taking the same eligibility cut-off[29], a *sufficient* condition for this is the impossibility to *precisely* manipulate the running variable.

Through campaigning or pressures, a political actor (e.g. a trade-union or an employer) can exert some influence on electoral scores. Nonetheless, unless, when a union is within a one-vote distance from one of the cut-offs, one of these actors can knowingly determine the decisive vote, manipulation will not bias the coefficient estimated by a comparison of the outcomes of interests where unions with neighbouring scores fall on different sides of the cut-off. This leads Lee and Lemieux [2010] to conclude that the impossibility to precisely manipulate the running variable is a sufficient condition for the continuity assumption which Hahn et al [2001] have coined as guaranteeing identification in Rubin's potential framework. The lack of precise control over the running variable indeed appears to ensure that the potential outcomes (e.g. the average wage in the firm with one less or one more union which can take part in bargaining) are continuous in the running variable, in such a way that any discontinuity in the actual outcomes should be attributed to the treatment.

However, in the case of electoral scores, it must be noted that the absence of precise control over the running variable is necessary but not sufficient to ensure continuity of the potential outcomes at all values of the running variable, and therefore to ensure identification. As I will

[27] For both bandwidths, I report in Table 27 in Appendix the number of elections on each side of the thresholds for each pair of *running variable . threshold* and the number of observations effectively available for estimation of effects on the log of the average gross wage (one year before the election used as a placebo, one, two and three years after the election). With a bandwidth of 5%, the number of observations available ranges from 65 to 2485. With a bandwidth of 2%, it ranges from 33 (score of the second union around the 10% threshold) to 1022 (score of the second union around the 30% threhsold) observations.

[28] By contrast, while I control linearly for a set of pre-determined characteristics (year fixed effects and the number of votes) for greater accuracy, the slope of the relation between the dependent variable and these characteristics is assumed to be the same on opposite sides of the threshold.

[29] The 10% cut-off also results in a downward discontinuity in the presence of a "head of union section" (unions which fail to reach 10% of the votes can have a "head of union section" who cannot take part in bargaining but can campaign in view of the following elections), as highlighted by the first stages in Figure 13 in Appendix 8.4. However, since the only difference between the head of union section and the union delegate is that the latter can take part in bargaining, the effect of an additional bargaining union is identified.

This may seem more problematic in the case of the 50% discontinuity. Indeed, except for elections with several colleges where the number of seats per college is not proportional to the number of voters, a union with more than 50% of the votes is necessarily the majoritarian union in the institution whose election was used to compute union representativity. Since this might generate discontinuities in the decisions taken by this institution, it would be valuable in future works to check for robustness of the results to restriction to elections where the number of voters in the different colleges is not proportional to the number of seats in the corresponding colleges.

further detail in Section 5.3.1.3., since an electoral score is the ratio of two integers, while certain scores – such as 10% or 50%, but also 25% or 33.33333% - may be found in both small and large firms, other scores – such as 9.9999999% - can only occur in large firms. Thus, since the potential outcomes are likely to be correlated with firm size, firms where a union has obtained 9.9999999% of the votes are not a valid counterfactual for the firms where a union has obtained 10% of the votes since they are on average larger. However, they are a valid counterfactual for firms where a union has obtained 1.0000001% of the votes as the latter should be of equivalent size. Thus, the absence of precise control of the running variable is a sufficient condition for identification if one performs donut RDDs (as introduced by Barreca et al [2011]), excluding observations at the cut-off.

To assess the likelihood that actors might be able to precisely manipulate the running variable, one first needs some knowledge of the process determining the running variable. Can unions or employers precisely manipulate electoral scores in professional elections?

The press or unions regularly relay accusations of fraud during professional elections. However, unless a union knows exactly the number of votes it has received when committing a fraud, fraud is not sufficient to threaten the validity of the identification. The most frequent cases of fraud which featured in the press were situations where electronic voting made it possible for a union to usurp the identity of certain voters, thereby increasing its vote share. In such situations, there is no reason to assume that the cheating union knows the number of votes it has received, and therefore to believe that it can precisely manipulate the running variable. By contrast however, if the fraud occurs during the counting of the votes, it is theoretically possible for a union member to precisely manipulate the vote share of her (or a rival) union, thus threatening the validity of the identification.

Even in the absence of fraud, one might fear a lack of local randomness if workers (or, in situations with high levels of abstention, the few, possibly highly-motivated, workers actually voting) systematically vote for the same union. This might be more likely in small firms with limited workforce turnover where employees know one another than in larger firms. Nonetheless, a single undecided worker makes a larger difference in small than large firms, making it unclear whether identification is less likely to hold in the former or latter firms[30].

Thus, the question of whether electoral scores in French professional elections are locally random remains a priori open. Hence, testing some implications of the identification assumption can help ascertain that the above-listed configurations are rare enough not to contaminate the results.

In the following, I thus test for the presence of a discontinuity in the density of the running variable at the thresholds and imbalances in pre-determined characteristics (including the lagged value of the dependent variables) around the thresholds.

5.3.1 Tests: Density inspection: absence of manipulation of the running variable around the threshold?

5.3.1.1 Visual inspection

Before formally testing for the presence of discontinuities in the density of the running variables, I first realized a visual inspection of the density of the running variables: if some unions were successful in sorting themselves above a threshold, one would expect a peak in the density of the running variable at the threshold, preceded by a gap.

As illustrated by the histograms in Figures 3 to 8, none of the rank-specific electoral scores presents an exceptional peak at 10% or 30%, once compared to similar peaks occurring away from these cut-offs.

However, if manipulation were non-monotonic (e.g. if some union managed to manipulate her

[30] Even if such non-randomness occurred however, identification would a priori not seem greatly threatened since the effect identified would remain that of the treatments of interest but would turn into cumulated effects over several mandates. Nonetheless, if election outcomes are easy to predict in small firms with high abstention level, it might be possible for motivated union members to convince colleagues to take part in the election or vote for a certain list, although one can speculate as to whether union members would indeed attempt to have a score just above a certain threshold rather than campaign in order to attain the largest score possible.

score in order to reach one of these thresholds, while, for instance, some employer manipulated the score of an union to prevent her from reaching one of these thresholds), opposite manipulations could cancel out, in such a way that plotting the scores of distinct unions together would conceal the resulting union-specific peaks and gaps at the thresholds. Reassuringly, as illustrated by the union-specific histograms in Figures 9 and 10 (Appendix 8.2.1.), when plotting separately the distribution of the scores of the main trade-unions, none of the resulting distributions presents a peak or gap at 10% or 30%.

By contrast, the distribution of the score of the first-ranked union presents a clear peak at 50%, which one might interpret as a sign of manipulation. Suspicions of manipulation might be reinforced by the fact that the peak is preceded by a small gap and that the density of the scores to the right of the threshold is systematically higher than the density of the scores to the left of the threshold. The peak indeed appears to reflect some degree of manipulation as, once we exclude elections involving a pre-electoral alliance[31] (whereby two or more trade-unions may form a common list and decide ex-ante how the votes received by the list will be shared across unions, making it possible for unions to ensure 50% of the valid votes in the presence of a single list), it attenuates. However, even after excluding such elections, although the peak attenuates slightly, the previously evoked signs of manipulation remain.

Nonetheless, these signs may not necessarily reflect manipulation of the electoral score for two reasons.

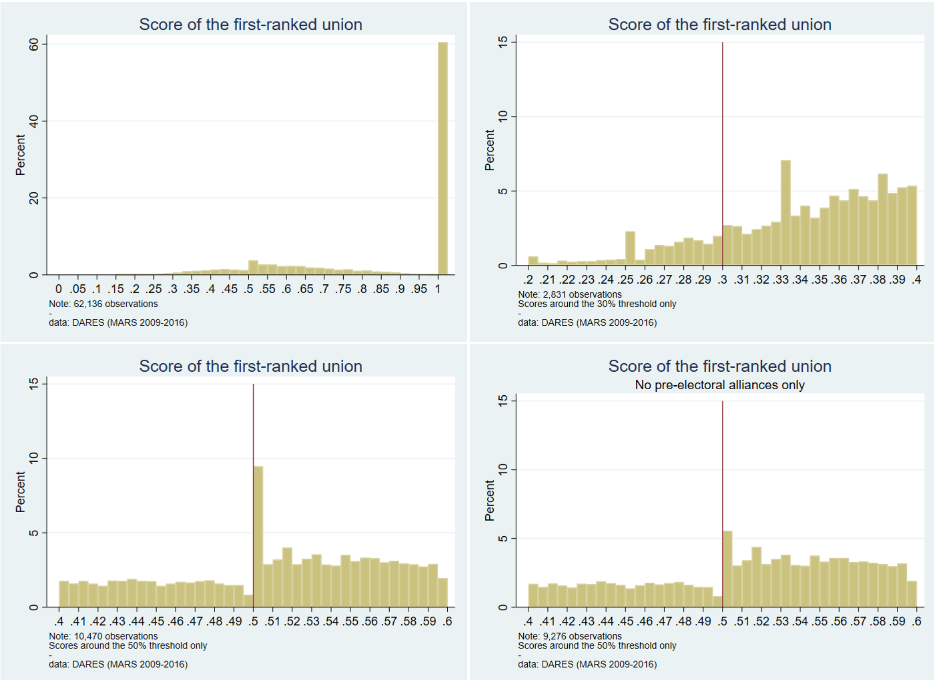
First, the density appears to be systematically higher to the right than to the left of the cut-off as a result of the construction of the variable itself: since it is the score of the first-ranked union, when only two unions run for the election, the union arriving below 50% necessarily arrives second and is therefore excluded from the histogram, explaining the missing mass observed. This explanation seems corroborated by the fact that, if one inspects the distribution of the scores of all union-specific scores around the 50% threshold (in Figure 8 in Appendix 8.2.1.), while a similar peak appears at 50%, the density of the scores to the left of the threshold is not systematically lower than their density to the right of the threshold.

Second, as I will further discuss in Section 5.3.1.3., the persistence of a peak at 50% after excluding pre-electoral alliances does not necessarily reflect the fact that some actor succeeds in precisely manipulating the electoral scores around these thresholds. Indeed, if one inspects the distribution of the score of the first-ranked union around the 30% threshold, one can observe similar peaks, albeit of lower amplitude, at 25% and 33.33%. This is likely to be at least partly attributable to the particularity of the running variable which I mentioned in 5.3.: since an electoral score is the ratio of two integers, such scores are more likely than their exact neighbours, and especially in a context where most elections involve only a limited number of voters. In any election with an even number of voters, a score of 50% is indeed theoretically possible, while the elections where a score of 50.1% could theoretically occur are rarer. Similarly, a score of 25% may occur in any election with a number of voters divisible by four, and a score of 33.3333...% may occur in any election with a number of voters divisible by three. Hence, if one assumed in a thought experiment that the number of votes received by a union were drawn from a uniform distribution, after 100%, 50%, would be the most likely score, 33.3333... % and 25% being slightly less likely, albeit more plausible than, for instance, 7%.

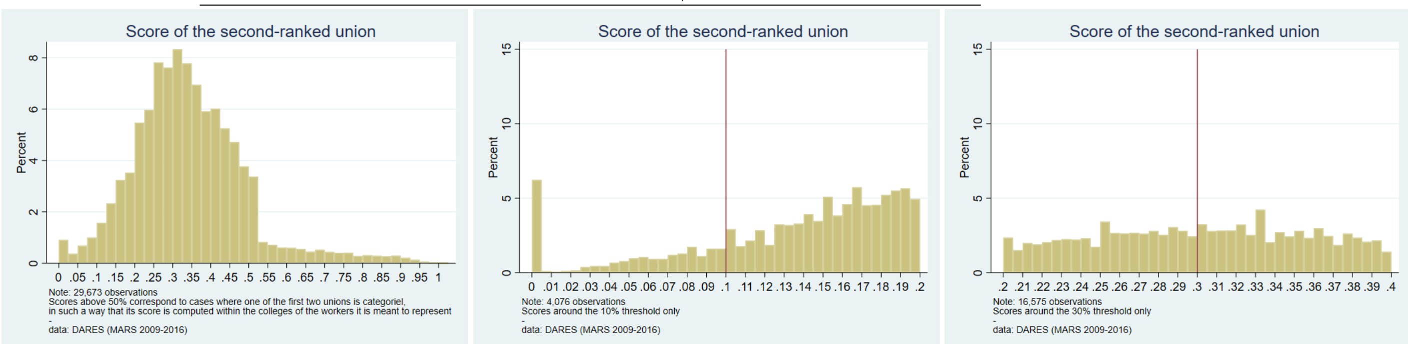
[31] Pre-electoral alliances concern 4.9% of all elections in the dataset.

Figure 3: Distribution of electoral scores, rank-specific

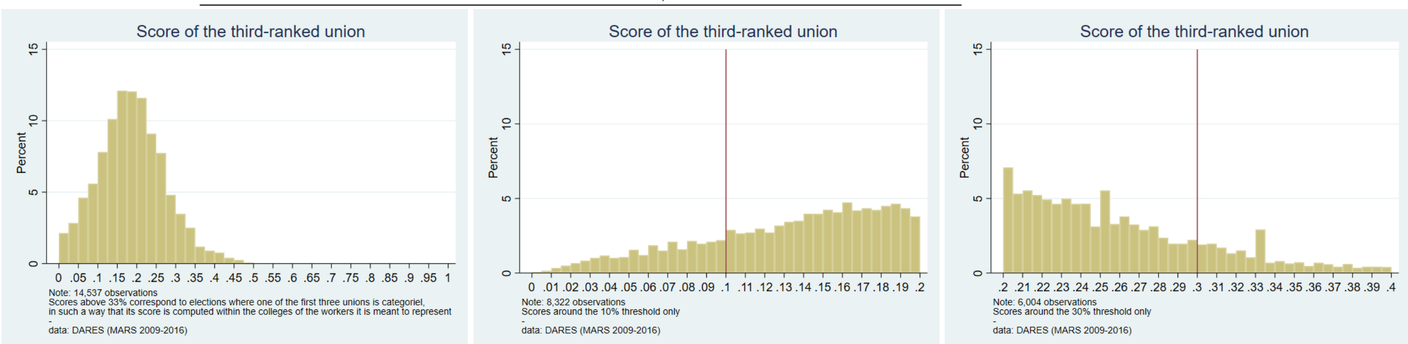
Score of the first-ranked union: overall, 30% and 50% thresholds



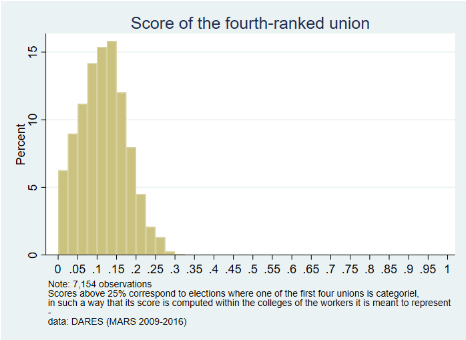
Score of the second-ranked union: overall, 10% and 30% thresholds

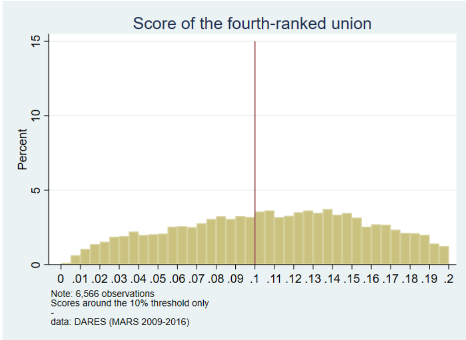


Score of the third-ranked union: overall, 10% and 30% thresholds

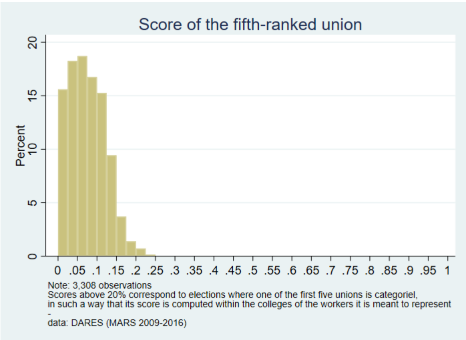


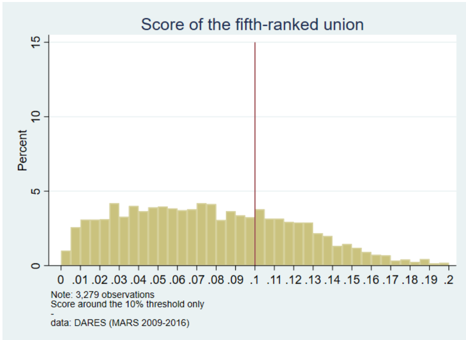
Score of the fourth-ranked union: overall and 10% threshold





Score of the fifth-ranked union: overall and 10% threshold





5.3.1.2 McCrary tests

To formally test for the statistical significance of the observed discontinuities, I completed this visual inspection by running McCrary tests (McCrary [2008]). The test first separates the data in bins and computes frequency counts for the different bins, before locally regressing bin density against bin mid-points on both sides of the cut-off, thus testing whether the difference of the log of the limit density on both sides of the cut-off is null.

As illustrated by the log discontinuity estimates I report in Table 4, although for the 10% and 30% thresholds, no discontinuity could be detected by a visual inspection of the density of the scores, for each of the three thresholds, the test very frequently detects statistically-significant peaks.

However, as the bottom half of the table illustrates, this phenomenon is not specific to the treatment eligibility cut-offs. Similarly frequent rejections (with positive estimates) indeed occur for scores of 33.3333%, 25% as well as, to a lesser extent, 12.5%. Rejection is by contrast very rare – (and only occurs at 5% and 10%) for arbitrary cut-offs of 63.12% and 17.23%. This corroborates the evidence from the histograms that, although small peaks could often be witnessed at 10% or 30%, such peaks were unexceptional compared to peaks at other cut-offs which do not allow eligibility to any treatment but correspond to an integer number of voters in a larger number of elections.

Table 4: Tests of continuity of the density of the running variable: McCrary discontinuity estimates

Discontinuity estimates: McCrary tests for manipulation of the running variables around cut-offs (treatment and placebo cut-offs)

Threshold:		All elections / exclusion of pre-electoral alliances	Running variable: score of:												
			First	Second	Third	Fourth	Fifth	Sixth	CGT	CFDT	FO	CFTC	UNSA	SUD	CGC
Treatment cut-offs	50%	*All*	1.337*** (0.047)	*N.A.*	*N.A.*	*N.A.*	*N.A.*	*N.A.*	0.650*** (0.054)	0.711*** (0.050)	0.744*** (0.073)	0.883*** (0.094)	0.369** (0.179)	0.488** (0.210)	0.971*** (0.148)
		Exclusion of alliances	1.143*** (0.053)	*N.A.*	*N.A.*	*N.A.*	*N.A.*	*N.A.*	0.337*** (0.059)	0.392*** (0.056)	0.455*** (0.085)	0.530*** (0.106)	0.412** (0.179)	0.290 (0.180)	0.486*** (0.171)
	30%	*All*	0.224** (0.084)	0.064** (0.026)	0.165** (0.077)	*N.A.*	*N.A.*	*N.A.*	0.120*** (0.044)	0.073* (0.043)	0.069 (0.055)	0.153** (0.271)	0.271* (0.140)	-0.147 (0.156)	0.149* (0.083)
	10%	*All*	*N.A.*	0.183** (0.089)	0.215*** (0.076)	0.081 (0.078)	0.121 (0.101)	-0.258 (0.242)	0.138** (0.053)	0.118** (0.052)	0.01 (0.050)	0.011 (0.062)	0.51*** (0.121)	0.125 (0.120)	-0.013 (0.065)
Placebo cut-offs	63.12%	*All*	0.089** (0.040)	*N.A.*	*N.A.*	*N.A.*	*N.A.*	*N.A.*	0.012 (0.063)	0.146** (0.061)	0.074 (0.097)	0.149 (0.120)	-0.065 (0.227)	0.288 (0.277)	0.106 (0.209)
	33.3333%	*All*	0.442*** (0.065)	0.111*** (0.026)	*N.A.*	*N.A.*	*N.A.*	*N.A.*	0.224*** (0.044)	0.128*** (0.043)	0.295*** (0.059	0.256*** (0.076)	0.073 (0.149)	0.205 (0.167)	0.205** (0.089)
	25%	*All*	0.720*** (0.152)	0.229*** (0.031)	0.191*** (0.046)	*N.A.*	*N.A.*	*N.A.*	0.188*** (0.046)	0.070*** (0.052)	0.070 (0.052)	0.286*** (0.071)	0.035 (0.125)	0.004 (0.154)	0.081 (0.075)
	17.23%	*All*	*N.A.*	-0.116** (0.054)	-0.008 (0.045)	-0.004 (0.079)	0.081 (0.29)	*N.A.*	-0.055 (0.048)	-0.065 (0.042)	-0.090* (0.050)	-0.114* (0.65)	-0.186 (0.123)	0.090 (0.137)	-0.009 (0.066)
	17%	*All*	*N.A.*	-0.187*** (0.055)	-0.015 (0.046)	-0.024 (0.076)	-0.109 (0.269)	*N.A.*	-0.106** (0.045)	-0.77* (0.043)	-0.118** (0.049)	-0.100 (0.065)	-0.087 (0.121)	0.061 (0.127)	0.031 (0.067)
	12.5%	*All*	*N.A.*	0.243*** (0.075)	0.105 (0.065)	0.114* (0.068)	0.037 (0.123)	0.064 (0.395)	0.038 (0.046)	0.094* (0.051)	0.118** (0.049)	0.121* (0.063)	0.277** (0.113)	0.125 (0.125)	-0.109 (0.069)

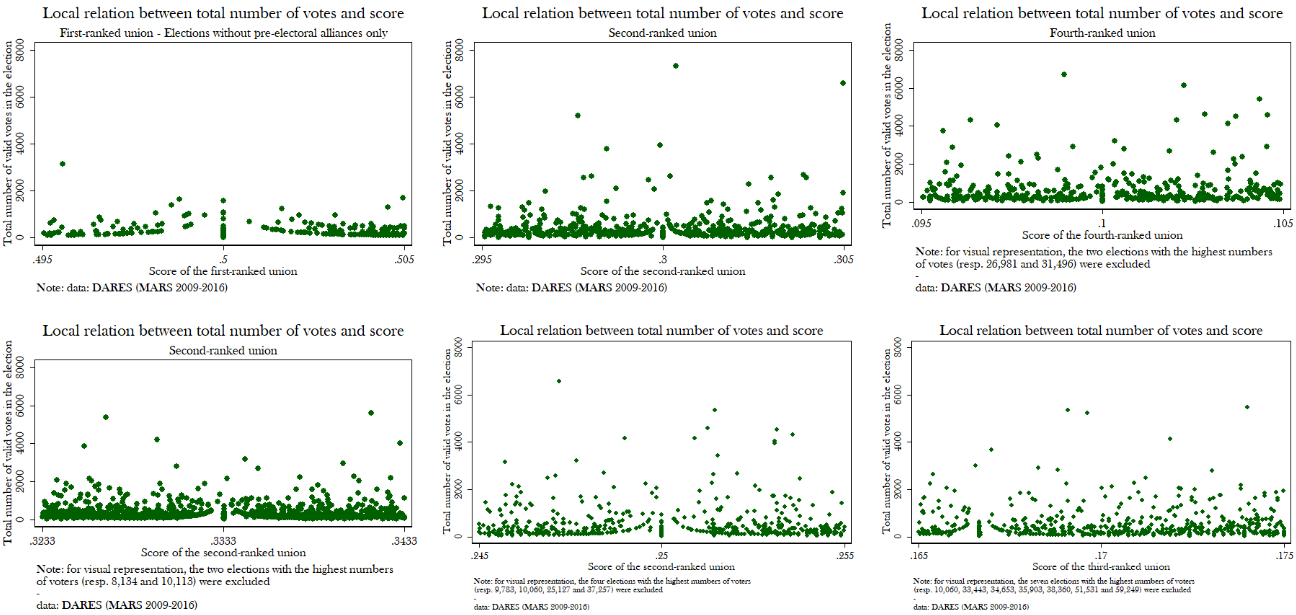
Legend:
- *Standard errors in parentheses*
- ** : p<0.1*
- *** : p<0.05*
- **** : p<0.01*
- *Blue: policy cut-offs*
- *Green/Orange gradient: for placebo cut-offs, degree to which rejection of the null is likely as a result of the score corresponding to round numbers of voters (orange: rejection likely / green: rejection unlikely)*
- *N.A: McCrary tests were not performed when the threshold lies in a boundary region of the running variable*

5.3.1.3 A DGP mechanically source of bunching

The levels of bunching detected therefore appear to be at least partly attributable to the particularity of the running variable whereby, even in the absence of manipulation, scores of 0.1, 0.3 and 0.5, but also 0.33333... and 0.25 are more likely than scores of 0.6312 or 0.1723.

As illustrated by Figure 4, plotting the number of valid votes against the rank-specific electoral scores indeed results in mountain-shaped forms around the cut-offs of interest: while scores of 10%, 30% and 50% may be found in elections with indiscriminately many or few voters, scores very close to the cut-off are only found in elections with high numbers of voters, and the closer to the cut-off, the higher the number of voters. As could be expected, similar shapes can be found around 25% and 33.333333% cut-offs, but not around 17%.

Figure 4: Local relation between total number of valid votes and electoral scores



Furthermore, as illustrated in Table 5, the majority of elections involve only small numbers of voters (71.21% of elections involved less than 100 voters). This reflects the fact that most firms in the sample have a small workforce size but is amplified by abstention. However, the distribution of the number of voters is positively-skewed: while the 99^{th} percentile of the distribution of the number of voters is 1,533, respectively 59,249, 65,602, 68,402 and 72,342 voters participated in the four largest elections.

Table 5: Distribution of workforce size and number of valid votes

Distribution of workforce size *(full-time equivalents, election year)*							
Workforce size	0-10	10-50	50-100	100-500	500-1000	1000-2000	2000/+
Share of firms	2.73%	41.80%	21.35%	27.47%	3.62%	1.88%	1.16%
Note: firms for which several elections have been recorded are counted only once Out of a total of 30,975 firms for which the full-time equivalent during the election year could be retrieved from the FARE database - data: DARES (MARS 2009-2016 and FARE 2009-2016)							
Distribution of numbers of valid votes *(out of all elections)*							
Number of votes	0-10	10-50	50-100	100-500	500-1000	1000-2000	2000/+
Share of firms	10.60%	41.62%	18.99%	23.74%	3.12%	1.29%	0.63%
Note: data: DARES (MARS 2009-2016)							

Thus, such scores as 50.00001% or 9.99999% can only arise in elections with large numbers of voters, which occur only rarely. By contrast, any election with respectively an even number of voters or a number of voters divisible by ten may theoretically see a union reach exactly 50% or 10% of the votes. As a result, the latter scores are far more likely to occur than their nearest neighbours.

Hence, the data-generating process itself appears to be a source of bunching, raising the question of whether the bunching detected by McCrary tests should be entirely attributed to this mechanical phenomenon or nonetheless reflect some degree of sorting.

Importantly, as mentioned in 5.3., this result however implies that, in the setting studied, the absence of precise manipulation of the electoral scores around the threshold is not a sufficient condition for identification: although this does not result from purposeful manipulation, the firms slightly below the cut-offs cannot be used as valid counterfactuals for the firms exactly at the cut-off, since, as highlighted by scatterplots in Figure 10, they are on average characterized by a higher number of voters and therefore tend to have a larger workforce size. However, since, as suggested by Figure 10, the relation between the number of voters and the electoral score is symmetric on both sides of the thresholds, provided there is no precise manipulation of the running variable, they should constitute a valid counterfactual for the firms exactly to the right of the threshold.

Thus, performing so-called "donut" RDDs, by excluding elections with scores exactly at the threshold can ensure that the treatment effects detected are not the result of this difference in workforce size.

To investigate whether this local relation between the total number of votes and electoral scores has the potential to lead to type 2 errors and could entirely explain the levels of bunching detected by the McCrary tests, I realized simulations to build intervals of levels of bunching which could arise from the data-generating process itself, in the absence of any manipulation[32].

To this end, I assumed that the number of votes received by a union could be locally approximated by a uniform distribution in a 5% neighbourhood of the thresholds of interest. Using the distribution of the number of valid votes in the sample, I thus drew from three distinct uniform distributions, for each election, three numbers of votes (respectively drawn from discrete uniform distributions defined over $[valid\,votes_{f,T}\,.\,5\%\,;\,valid\,votes_{f,T}\,.\,15\%]$, $[valid\,votes_{f,T}\,.\,25\%\,;\,valid\,votes_{f,T}\,.\,35\%]$ and $[valid\,votes_{f,T}\,.\,45\%\,;\,valid\,votes_{f,T}\,.\,55\%]$ with $valid\,votes_{f,T}$ the total number of validly expressed votes in the election organized in firm f in year T). For each election, doing this therefore yields three simulated electoral scores, respectively comprised between $5-15\%$, $25-35\%$ and $45-55\%$.

I repeated this process 10,000 times and performed, at each iteration, a separate McCrary test for each of the three thresholds. After excluding, for each threshold, the 5% lowest and highest t-statistics obtained, I derived the intervals which can be found in Table 6. For each threshold, these intervals therefore provide an approximate 95% coverage of levels of bunching (as proxied by McCrary t-statistics) which could be expected to arise naturally if electoral scores were drawn from a uniform distribution in a 5% bandwidth around the threshold.

[32] An alternative test would have consisted in running McCrary tests on large firms only to ascertain that the null is no longer rejected. Although I initially explored this option, since large firms are rare, attempting to do so resulted in an insufficient number of observations for inference.

Alternative tests for manioulation have been developed. For instance, noticing that the asymptotic properties warranted for inference are not met when performing McCrary tests on discrete running variables, Frandsen [2017] proposes an alternative test meant to detect bunching when the running variable is discrete. Other tests include Funke, Hirukawa [2017].

However, to my knowledge, none of these tests answers the problem I encountered here: they attempt to detect bunching, rather than attempting to detect *abnormal* bunching.

Indeed, although the problem encountered here is related to electoral scores not being a purely continuous variable, it differs from the inference issue noted by Frandsen [2017]: in a context of asymmetric distribution of election sizes, because an electoral score are not a purely continuous variables, and the size of the gaps in its support is determined by election size, the data-generating process is mechanically source of bunching. Hence, to ascertain whether a discontinuity should be interpreted as signalling manipulation of the running variable, the null tested should not be the absence of a discontinuity, but whether this discontinuity is normal, *conditional on the data-generating process.*

Table 6:

95% coverage intervals of McCrary test statistics obtained using simulated electoral scores			
Cut-off	10%	30%	50%
Interval	[15,290 ; 18,654]	[17,594 ; 21,104]	[49,616 ; 53,085]
Note: intervals obtained with 10,000 iterations, simulated scores obtained using a uniform approximation in a 5% bandwidth around the cut-off (drawing one score around each cut-off for each election)			

The test-statistics thus-obtained are very high, in such a way that all values of the obtained intervals would result in p-values of 0.

Although the obtained intervals thus suggest that the data-generating process could be a source of type 2 error, they do not enable me to conclude that the levels of bunching detected should be entirely attributed to the data-generating process. Further refining of the simulations would indeed be required to better mimic the DGP: the test statistics obtained with the original data indeed systematically fall below the lowest bound of the intervals obtained with the simulated data. Importantly, the test statistic obtained with the original data when considering the 50% threshold and failing to exclude pre-electoral alliances is 28.668, nearly twice smaller than the 49.616 test statistics which corresponds to the lowest bound of the interval obtained with simulated data. Excluding pre-electoral alliances however results in a lower test statistics, suggesting that pre-electoral alliances are indeed a source of manipulation.

Since comparing the test statistic obtained prior to the exclusion of pre-electoral alliances to the interval built with simulated data would fail to detect the resulting level of bunching as anomalous, it appears that additional tests are required to ascertain that the RD design is valid.

5.3.2 Test: Balance of pre-determined covariates

As mentioned in McCrary [2008], testing for the absence of discontinuity at the cut-off in the density of the running variable could fail to detect non-monotonous sorting, whereby some trade-unions sort above a threshold while others sort below that threshold. If trade-unions themselves might have little incentives to willingly sort below one of the cut-offs of interest, one could imagine that a rival union or an employer may have interest in manipulating the election in such a way as to prevent it to reach one of the cut-offs.

Inspecting the density of the electoral scores and realizing McCrary tests separately for union-specific scores, as I do respectively in Figures 8 to 10 (Appendix 8.2.1.) and in section 5.3.1.2. may contribute to alleviate such concerns.

However, since I failed to ascertain the absence of abnormal bunching, I realize a complementary test which has the potential of detecting non-monotonous sorting. I thus test for the continuity of pre-determined characteristics around the cut-offs studied.

With five running variables, three cut-offs and several baseline characteristics, running separate RDDs to test for the continuity of each baseline characteristic around each cut-off for every running variable would fall prey to multiple testing issue: imbalances in some baseline characteristics are likely to arise due to pure chance.

Since such imbalances would only threaten identification when sufficient to generate discontinuities in the outcomes studied however, I follow a strategy used in Card et al. [2007]. I first use a set of baseline characteristics to predict the wage bargaining outcomes studied. I then run RDDs, using the thus-obtained predicted outcomes as dependent variables, thereby checking for *discontinuities in the predicted outcomes.* Besides alleviating multiple testing issues, checking for discontinuities in predicted outcomes (rather than directly checking for discontinuities in the baseline characteristics) has the advantage of detecting discontinuities in unobservables if the latter are correlated with the baseline characteristics as well as with the outcomes of interests.

To predict the outcomes of interest, I thus run OLS regressions, using the log of workforce size before the election [33], year fixed effects and sector[34] fixed effects as explanatory variables. Although sector-specific conjunctures might play a key role in the determination of the variables of interest, I decided not to use interaction terms in order to avoid overfitting: including too many covariates could lead to risks of type one error, whereby a jump in a dependent variable resulting from the treatment would be attributed to a highly-specific combination of pre-determined characteristics. For the same reason, I also recoded the measure of sector provided by the INSEE (the NAF 88 nomenclature) which took eighty-two different values, using the NAF 10 nomenclature in which the sector takes ten different values[35].

I report in Table 7 below the p-values and R^2 of regressions of, respectively, the log of the average wage and its growth, the share of the wages in the value-added and its growth, the value-added by worker, its log, increase and growth rate, as well as the economic profitability of the firm, its log, increase and growth rate (each variable being measured respectively one, two and three years after the election, and growth rates and increases being measured with respect to the year preceding the election).

Table 7: Comparison of fit of OLS regressions - yellow: regressions selected to make the predictions used to test for the balance of pre-determined characteristics

COMPARISON OF SPECIFICATIONS

OLS regressions to predict the dependent variables used in the analysis and test for imbalance in characteristics which would threaten identification

		1-yr after election				2-yrs after election				3-yrs after election			
		Level	Log	Diff.	Growth rate	Level	Log	Diff.	Growth rate	Level	Log	Diff.	Growth rate
Average wage	p-value	0.0001	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.96
	R^2	0.0012	0.0685	0.0023	0.0014	0.0051	0.0719	0.0045	0.0016	0.0030	0.0662	0.0043	0.0006
	R^2-adj	0.0008	0.0681	0.0018	0.0009	0.0047	0.0714	0.0040	0.0011	0.0024	0.0656	0.0037	-0.0000
Share of wages in value-added	p-value	0.0004	0.0000	0.0017	0.0000	0.7364	0.0000	0.5269	0.0000	0.6098	0.0000	0.6500	0.2853
	R^2	0.0011	0.0966	0.0010	0.0014	0.0004	0.1001	0.0004	0.0015	0.0005	0.1014	0.0005	0.0007
	R^2-adj	0.0006	0.0962	0.0005	0.0010	-0.0001	0.0997	-0.0000	0.0010	-0.0001	0.1009	-0.0001	0.0001
Value-added per worker	p-value	0.0000	0.0000	0.0406	0.4195	0.0000	0.0000	0.0042	0.9438	0.0001	0.0000	0.3439	0.7101
	R^2	0.0032	0.0915	0.0007	0.0004	0.0062	0.0950	0.0011	0.0003	0.0018	0.0911	0.0007	0.0005
	R^2-adj	0.0028	0.0911	0.0003	0.0000	0.0057	0.0945	0.0006	-0.0002	0.0012	0.0905	0.0001	-0.00001
Economic profitability	p-value	0.0088	0.0000	0.7136	0.0562	0.2176	0.0000	0.8795	0.88337	0.0632	0.0000	0.9238	0.6594
	R^2	0.0008	0.0585	0.0003	0.0007	0.0006	0.0583	0.0003	0.0003	0.0009	0.0629	0.0003	0.0005
	R^2-adj	0.0004	0.0579	-0.0001	0.0002	0.0001	0.0577	-0.0002	-0.0002	0.0003	0.0622	-0.0003	-0.0001

Covariates used in regressions: sector and year fixed effects, log of workforce size the year before the election (or the most recent year before the election when information unavailable)
Indicated in yellow are the specifications eventually used to make predictions
-
Data: INSEE (FARE 2010-2017 and agrifin 2012-2017, inflation series)

A first teaching of Table 7 is that, overall, the pre-determined characteristics used only explain a small share – never more than 10% - of the variation in the variables studied[36]. This implies

[33] When available, I used the log of workforce size the year before the election. For 3,655 elections, workforce size the year before the election was missing although for 844 of these elections, an earlier measure of workforce size could be found in the FARE dataset. I hence replaced missing values by the log of workforce size the latest year before the election for which the information was available (not using information from years following the election to avoid endogeneity). Although recourse to log forces to drop firms which declared a workforce size of zero, I used the log to reduce the weight of outliers. Since workforce sizes of zero are likely to be frequently attributable to misreports, doing so may further improve the accuracy of the estimates.

[34] I applied a correction similar to that used for the workforce size, replacing the sector by that of the year the closest to the election when that information was not available for the year preceding the election. Since the sector is unlikely to be affected by collective bargaining in a short time span, using the sector indicated for a year following the election should not lead to any endogeneity.

[35] In future works, using cross-validation technics could enable recourse to interaction terms without running the risk of overfitting, as well as to a more precise measure of sectors.

[36] In Figure 11 in Appendix 8.3.2., I plot the predicted log of, respectively, the average wage, the share of wages in

that small imbalances in those pre-determined characteristics should not threaten identification. The R^2 obtained are especially low when using growth rates or increases as dependent variables. Unsurprisingly, since recourse to log transformations decrease the influence of outliers, they are larger when using log than levels. To minimize the risks of type two error, I therefore chose to focus on log specifications when realizing balance tests[37]. Output from the regressions used to make the predictions I use to test for the balance of pre-determined characteristics can be found in Tables 28 to 31 in Appendix 8.3.1.. To assess the magnitude of the coefficients in relative terms, standard deviations of the predicted variables can be found in table 107 in Appendix 8.9..

After excluding outliers following the procedure described in section 4.2., I first carried out a series of balance tests whose detailed results can be found in Tables 32 to 40 in Appendix 8.3.3. (using 10% and 30% as cut-offs), and Tables 8-10 (using 50% as cut-off) below.

Tables 32 and 34 in Appendix highlight small imbalances between firms where the second or third union obtained slightly more or slightly less than 10% of the votes: firms where the second-ranked union obtained 10% of the votes or slightly more are thus predicted to be characterized, three years after the election, by respectively an average gross wage lower by 4.55% and a gross value-added by worker lower by 7.67% than firms where the second-ranked union obtained slightly less than 10% of the votes (both significant at 10%). Similarly, firms where the third-ranked union obtained 10% of the votes or slightly more are predicted to be characterized by a gross value-added by worker lower by 7.73% than those where the third-ranked union obtained slightly less than 10% of the votes (significant at 5%).

While these imbalances could be thought to be attributable to multiple testing, the detected imbalances are more serious when focusing on the 50% threshold, corroborating the suspicions raised by previous tests. As illustrated by Table 8, when testing for the balance of pre-determined characteristics between firms where the first-ranked union obtained 50% of the votes or slightly more, and those where it obtained slightly less than 50% of the votes, all the coefficients are significant at minimum 10%, ten out of the twelve coefficients being significant at 1%: firms where the first-ranked union obtained 50% of the votes or slightly more are thus predicted, one to three years after the election, to be characterized by a lower average wage and value-added by worker (respectively lower by 3-4% and 6-8%) but higher (by 3-4%) share of wages in the value-added and far higher (by 22%) economic profitability than the firms where it obtained slightly less than 50% of the votes. When compared to the overall dispersion of the predicted variables (as captured by the standard deviation of each predicted variable in the sample after exclusion of outliers), this corresponds to differences of a magnitude ranging from 0.22 (column 8) to 0.66 (columns 10 and 11) standard deviations.

In the light of previous results, one would have expected these imbalances to attenuate with the exclusion of pre-electoral alliances. However, comparing Table 8 to Table 9 highlights that the estimated imbalances (except with respect to the predicted economic profitability) increase once excluding pre-electoral alliances. This suggests that, although the possibility for two unions to split equally all the valid votes mechanically results in a peak at 50%, the firms where this happens do not appear to be very different (at least in terms of observables or unobservables correlated with the observables used for prediction) from the firms where the first-ranked union obtained slightly

the value-added, the value-added by worker, and the economic profitability one year after the election against the actual value. This highlights that the predictions underfit rather than overfit: the predictions do not align on the bisector and have a plurimodal density. Figure 12 in Appendix 8.3.2. however highlights that the original data follows a unimodal distribution. This plurimodal density is likely to be attributable to recourse to a majority of indicator variables, hindering the continuity of the predictions. Indeed, in an earlier draft, using the establishment-level DADS to measure wages, I had had recourse to both OLS and a combination of random forest, kernel-support vector machine, OLS, ridge and lasso regressions (selected by cross-validation). Using cross-validation enabled recourse to interaction terms without running the risk of overfitting. This naturally led to more continuous predictions. However, while one could have feared that discontinuous predictions might lead to over-rejection of the null, the results from balance tests carried out when using OLS or machine learning predictions were similar.

[37] Although using the log of the value-added by worker and economic profitability may result in a selection bias by excluding all firms with negative value-added or economic profitability, since my present aim is to detect imbalances rather than identify these imbalances, I decided to use the predicted log of the value-added by worker and economic profitability when testing for the balance of pre-determined characteristics.

less than 50% of the votes.

By contrast, as illustrated by Table 10, after excluding the firms where the first union obtained exactly 50% of the votes, all point estimates decrease in absolute value and their standard errors increase, in such a way that only three out of the twelve coefficients remain significant: firms where elections did not involve a pre-electoral alliance and where the first union obtained slightly more than 50% of the votes are predicted to be characterized by a value-added by worker lower by 4.32% one year after the election (significant at 10%) and an economic profitability lower by 11% one and two years after the election (respectively significant at 1% and 5%) than those where it obtained slightly less than 50% of the votes.

As illustrated by Tables 33 and 35 in Appendix, after excluding elections with pre-electoral alliances[38], and elections where the running variables takes the cut-off, the point estimates corresponding to the previously-detected imbalances around the 10% threshold decrease in absolute value (and the significance of the coefficients similarly decreases). Since I used workforce size to realize the predictions used, given the local relation between the number of voters and electoral scores around the thresholds studied, it is unsurprising that excluding observations at the cut-off results in lower imbalances in predictions.

If one applies Holm's correction for multiple testing, considering all combinations of $running\ variable\,.\,threshold\,.\,variable\,of\,interest\,.\,year$ together, none of the detected imbalances remains significant after exclusion of outliers, pre-electoral alliances and of the elections where the running variable takes the cut-off value[39].

Nonetheless, the constancy of the sign of the coefficients may appear as the possible signal of systematic imbalances.

Since the same pre-determined covariates were used to predict the variable of interests respectively one, two and three years after the election, it is not surprising however, to see the sign of the estimated coefficient remain constant over the years for each combination of $running\,variable\,.\,threshold\,.\,variable\,of\,interest$.

One might note an a priori more worrying pattern from Tables 32 to 37 in Appendix: firms where the second, third or fourth union (and, at the exception of columns 3, 6 and 12, fifth union) obtained more than 10% of the votes are characterized by lower predicted wage and value-added but higher predicted share of wages in the value-added and economic profitability than the firms where the union of same rank obtained less than 10% of the votes. This constancy of sign might be interpreted as signalling a local non-linear relation of the variables of interest with respect to the electoral support for small trade-unions, resulting in biased estimation when the bandwidth chosen is too large. Indeed, when using a bandwidth of 0.01, this constancy of sign seemed to attenuate.

Thus, this constancy of sign should be kept in mind when interpreting the coefficients found when studying the effects of the treatment on the genuine variables of interest, and may suggest the need for a recourse to a narrower bandwidth as a robustness check.

In addition, although having excluded elections with pre-electoral alliances and scores at the cut-off reduces the point estimate of the previously significant coefficients, one may be worried to observe that none of these coefficients changes sign after the exclusion of these observations. Since, under the null of no imbalances, one could have expected some coefficients to change sign, this might hint at the persistence of non-detectable imbalances.

Thus, although after excluding elections with pre-electoral alliances and elections where the

[38] Although excluding elections where the running variable takes the cut-off value de facto implies excluding elections where unions could have used a pre-electoral alliance in such a way to obtain exactly 10%, 30% or 50% of the votes, I nonetheless decided to exclude elections with pre-electoral alliances since such elections are rare, therefore resulting in a limited loss of statistical power, and since forming alliances allows unions to precisely control their electoral score, thus introducing possible non-randomness away from the threshold but within the bandwidth, possibly biasing the estimates if pre-electoral alliances are the expression of a specific social climate.

[39] A total of 96 equations have been estimated, implying that, rejection of a null at 10% after applying Holm's multiple testing correction would warrant $p_s < \frac{1}{96-s}$ with $s = 0$ for the hypothesis corresponding to the lowest p-value, $s = 1$ for the hypothesis corresponding to the second lowest p-value, etc. Since $0.006 > 0.00104$, $0.014 > 0.00105$, $0.062 > 0.00106$, $0.067 > 0.0017$ and $0.087 > 0.00109$, none of the hypotheses is rejected.

running variable takes the cut-off value, there remains no statistically-significant sign of imbalances robust to multiple testing correction, the results from these tests should be kept in mind when interpreting the measured effects on wage bargaining outcomes since they might suggest the presence of small, non-detectable, imbalances.

Table 8: Test of balance of pre-determined characteristics - outliers excluded - First union, cut-off: 50%

Running variable: score of the first-ranked union Cut-off: 0.5						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	-0.0361***	-0.0373***	-0.0413**	-0.0806***	-0.0683***	-0.0840***
	(0.0128)	(0.0135)	(0.0163)	(0.0216)	(0.0229)	(0.0302)
Conventional p-value	.005	.006	.011	0	.003	.005
Obs left of cut-off	881	717	409	877	716	408
Obs right of cut-off	2150	1746	1062	2142	1738	1055
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.0470***	0.0387**	0.0471*	0.226***	0.226***	0.222***
	(0.0165)	(0.0177)	(0.0256)	(0.0363)	(0.0406)	(0.0564)
Conventional p-value	.004	.029	.066	0	0	0
Obs left of cut-off	903	761	464	670	562	349
Obs right of cut-off	2217	1870	1195	1698	1427	916
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects
and the log of workforce size before the election
Wage and value-added in 2009 terms
Outliers excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 9: Test of balance of pre-determined characteristics - outliers and pre-electoral alliances excluded - First union, cut-off: 50%

Running variable: score of the first-ranked union Cut-off: 0.5						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	-0.0476***	-0.0456***	-0.0484***	-0.0935***	-0.0820***	-0.102***
	(0.0137)	(0.0144)	(0.0180)	(0.0214)	(0.0239)	(0.0320)
Conventional p-value	0	.002	.007	0	.001	.001
Obs left of cut-off	795	650	368	791	650	368
Obs right of cut-off	1872	1518	924	1867	1513	918
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.0454***	0.0421**	0.0545**	0.205***	0.218***	0.222***
	(0.0153)	(0.0181)	(0.0262)	(0.0374)	(0.0424)	(0.0596)
Conventional p-value	.003	.02	.037	0	0	0
Obs left of cut-off	817	692	418	608	514	315
Obs right of cut-off	1926	1629	1040	1472	1246	795
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects
and the log of workforce size before the election
Wage and value-added in 2009 terms
Outliers and pre-electoral alliances excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 10: Test of balance of pre-determined characteristics - outliers, pre-electoral alliances and scores at the cut-off excluded - First union, cut-off: 50%

Running variable: score of the first-ranked union Cut-off: 0.5						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	-0.0239	-0.0242	-0.0244	-0.0432*	-0.0316	-0.0446
	(0.0146)	(0.0154)	(0.0192)	(0.0236)	(0.0270)	(0.0375)
Conventional p-value	.102	.116	.204	.067	.242	.234
Obs left of cut-off	795	650	368	791	650	368
Obs right of cut-off	1668	1345	806	1662	1339	801
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.0182	0.0138	0.0220	0.111***	0.113**	0.102
	(0.0177)	(0.0209)	(0.0316)	(0.0405)	(0.0459)	(0.0669)
Conventional p-value	.305	.511	.487	.006	.014	.129
Obs left of cut-off	817	692	418	608	514	315
Obs right of cut-off	1718	1450	911	1318	1113	701
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects
and the log of workforce size before the election
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances and scores at the cut-off (=donut) excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

5.3.3 Test: Placebo using lagged value of the dependent variable

Finally, I test for the absence of discontinuity in the outcomes of interest or their trends prior to the election. To do so, I run RDDs, using as dependent variables the log of the average gross wage, its growth rate, the log of the share of the wage bill in the value-added, its growth rate, the value-added by worker, its log, increase and growth rate, as well as the economic profitability, its log, its increase and its growth rate during the year prior to the election.

Just as for any pre-determined characteristic, a discontinuity in a variable of interest before the election, might arise due to pure chance. However, compared to small imbalances in other pre-determined characteristics, it would prevent interpretation of the coefficients: interpretation of post-electoral difference in the same variable as entirely attributable to the treatment would indeed probably attribute pre-existing differences to the treatment.

Since it appears that including elections with pre-electoral alliances as well as elections where the running variable takes the cut-off value results in imbalances in predicted outcomes, I directly excluded such observations. Since, in the following, I control for year fixed effects and the log of the number of voters to estimate treatment effects more precisely, I also control for year fixed effects and the log of the number of voters in order to use a specification comparable to that used in the rest of the analysis.

As illustrated by the Tables 24 to 31 in Appendix, the rare imbalances found[40] are likely to be attributable to multiple testing: once considering all combinations of $running\,variable\,.\,threshold\,.\,variable\,of\,interest$ and applying Holm's correction for multiple testing, none remains significant[41], although they should be kept in mind when interpreting the results, to avoid pre-existing imbalances being attributed to the treatment.

5.3.4 Recap

Since the firms where a union's score is exactly 10%, 30% or 50% are on average smaller than their immediate neighbours, including observations at the cut-off would threaten the validity of the identification, since the firms at the cut-off do not constitute valid counterfactuals for the firms exactly at the right of the cut-off. However, since the number of voters seems symmetrically distributed on opposite sides of the thresholds, excluding observations where the running variable takes the cut-off value should restore the validity of the identification.

In the following, I therefore systematically exclude elections where the running variable takes the cut-off value, as well as elections with pre-electoral alliances. Indeed, even after exclusion of observations at the cut-off pre-electoral alliances are susceptible to generate non-linearities in potential outcomes away from the cut-offs - but within the chosen bandwidths -, resulting in biased estimates.

The tests of balance of pre-determined characteristics and placebo tests I ran suggest that, despite recourse to donut and exclusion of pre-electoral alliances, owing to multiple testing, chance imbalances with the potential of resulting in spurious conclusions occur. Thus, in the following, I systematically analyze the results obtained when estimating effects on post-electoral outcomes in the light of the above results.

6 Results

6.1 Wage bargaining outcomes

6.1.1 Effects of an additional union: 10% threshold

Is the net effect on wages of an additional bargaining union positive or negative?

As I mentioned in introduction, the net effect of a new player in the bargaining game is a priori ambiguous, for both workers and the firm: more unions may result in coordination frictions, but also increased electoral competition and therefore increased accountability of union representatives. Although I mainly focus on consequences for workers, in the following, when uncovering evidence of effects on wage bargaining, I explore whether this seems to reflect zero-sum games.

[40] The growth rate of the share of the wages in the value-added one year before the election was higher by 10.8 percentage points (which corresponds to a 0.37 standard deviation difference when considering the distribution of the trimmed data only) in firms where the second union obtained slightly more than 10% of the votes compared to those where it obtained slightly less than 10% of the votes, the value-added by worker increased by an average 2.85 thousands euros more or 5.79 percent more in firms where the third union obtained slightly more than 10% of the votes as opposed to those where it obtained slightly less (both respectively 0.14 standard deviation differences), the gross average wage was 5.61% higher in firms where the fourth union obtained slightly more than 10% of the votes compared to those where it obtained slightly less (0.18 standard deviation difference), the economic profitability increased by 0.102 points less (a 0.18 standard deviation difference) in firms where the third union obtained slightly more than 30% of the votes, as opposed to those where it obtained slightly less, and finally the economic profitability was lower by 0.103 points (a 0.16 standard deviation difference) and increased by 0.413 points less (a 0.76 standard deviation difference) in firms where the first union obtained slightly more than 50% of the votes as opposed to those where it obtained slightly less.

[41] 96 equations have been estimated, leading to 6 p-values inferior to 0.1 but s.t. $p_0 = 0.004 > 0.00104$, $p_1 = 0.018 > 0.00105$, $p_2 = 0.038 > 0.00106$, $p_3 = 0.043 > 0.00107$, $p_4 = 0.08 > 0.00109$ and $p_5 = 0.085 > 0.0011$.

Furthermore, both coordination frictions and increased electoral competition may have contradictory effects on both the share of the value-added captured by the workers and firm productivity. As a result of coordination frictions, an additional union could be expected to result in a decreased share of wages in the value-added, as unions fail to coordinate. However, it could result in both positive (if limiting the number of mobilizations and work stoppages because of union paralysis) or negative (if coordination frictions make crises more likely, or if union paralysis results in a deteriorated social climate, and thereby a demotivated workforce) effects on firm productivity. In turn, while increased union accountability should be expected to increase wages, in proportion of the value-added, the "two faces of unions" highlighted by Freeman and Medoff [1979] imply that consequences in terms of value-added are ambiguous: better-functioning unions may both negatively and positively affect the value-added.

I therefore investigate the following questions. First, do workers gain or lose from an additional bargaining union? (as measured by the net effect of an additional union on the average gross wage) Second, can we detect evidence of coordination frictions? Third, can we detect evidence of increased union accountability?

As argued in the introduction, conditional on not finding an effect on firm productivity, a negative effect of an additional union on the share of wages in the value-added would signal coordination frictions, while a positive effect would signal increased representation efforts by unions.

However, owing to the existence of "two faces" of unions, both situations could positively or negatively affect firm productivity (e.g. coordination frictions might make crises more likely or, on the contrary, result in union paralysis making industrial action less likely ; increased representation efforts might positively affect firm productivity as unions better fulfill their "Voice" role, adverting crises, but if voicing discontent could similarly negatively affect firm productivity). Thus, alternative situations do not appear to be interpretable. Furthermore, since both effects may be at play and therefore offset one another, finding no evidence of an effect of an additional union on wage bargaining outcomes could not be used to rule out the presence of either effect.

6.1.1.1 First stage: effect of treatment assignment on treatment status

Since, despite having obtained 10% of the votes in the first round of the professional elections, a union might not take part in bargaining, I first realized first stages to verify that a union reaching 10% of the votes is associated with a jump in union presence in the corresponding workplace. I first realized first stages using union-specific scores (using the scores of the seven largest trade-unions), followed by first stages using rank-specific scores to verify that the jumps in union presence found were reflected by jumps in the number of unions.

To do so, I used both the REPONSE[42] and the Acemo datasets.

Since the REPONSE surveys are carried out at the establishment level, they only enable me to measure union presence at the establishment level and therefore, insofar as firms are concerned, solely enable me to verify the existence of jumps in union presence in single-establishment firms. By contrast, since the Acemo surveys are carried out at the firm level, they enable me to measure union presence at the firm level.

However, the measures of union presence available in the two surveys differ substantially: while using the REPONSE survey enables me to verify the presence of a jump in the probability that a union has a union delegate (whose role is to take part in bargaining), the Acemo surveys only enquires about "union presence", which may cover many instances where a union has no union delegate[43] and which are not conditional on a union having obtained 10% of the votes.

[42] As mentioned in section 4.3., while the 2017 survey includes the date of interview, the 2011 survey solely includes the year of the last professional elections. Since the REPONSE surveys only enquire about union presence in 2011 and 2017, I restricted the sample to firms for which, either elections occurred strictly before the year of the last election reported in the 2011 and representativity ended strictly after 2011, or those for which the election occurred before the interview date reported in the 2017 survey and the representativity ended after the interview date.

[43] A union is defined as present in the firm if it has a union delegate, but also if any elected representative (sitting in the CE or DUP, a DUP, or a representative sitting in another body, the CHSCT) belongs to that union or if a union has a "head of union section" in the firm. The latter are entities whose existence was made possible by the 2008 reform of unionism: if a union has not reached 10% of the votes, it may nonetheless remain present in the firm in the form of a "union section", allowing it to campaign to possibly become representative during the following

This difference explains the higher levels of "union presence" found when using the Acemo dataset than when using the REPONSE dataset (when using the Acemo dataset and considering all firms together, union presence is indicated in $60-80\%$ of the firms where the corresponding union obtained slightly less than 10% of the votes, while, when using the REPONSE dataset and considering all establishments, this concerns maximum 30% of establishments). However, it is interesting to note that, although having obtained 10% of the votes is not a necessary condition for union presence, it remains associated with a jump in the probabilities of "union presence" (a $10-30$ percentage points increase in the probability of union presence) albeit narrower than in the probability of the presence of a union delegate[44].

The only exception is the CGC ("Confédération Générale des Cadres"), for which no jump appears at 10%: since the CGC only represents a certain category of workers ("cadres"), under certain conditions (which I detail in footnote 2 in section 4.1.), only a subset of the expressed votes is considered to evaluate its representativity. However, since the data available does not enable me to ascertain whether all conditions are met, this results in measurement error. This suggests that analysis should be carried out when excluding observations for which the running variable (score of the first, second, etc) union pertains to that union, to check whether the resulting treatment effects are stronger[45].

Lastly, one should note that, whether using the REPONSE or the Acemo datasets, the measured jumps are more pronounced when restricting attention to single-establishment perimeters (i.e. to "distinct establishments" which effectively consist of a single physical establishment in the case of REPONSE, and to single-establishment firms in the case of Acemo): for instance, when using the REPONSE data to focus on single-establishment perimeters, the measured jumps correspond to a $60-90$ percentage points increase in the probability that a union delegate is present in the establishment, but this falls to $5-30$ percentage points when restricting the sample to multi-establishment perimeters.

This may be partly attributable to the fact that neither dataset allows a measure of union presence at the appropriate observation level in the case of multi-establishment election perimeters. The REPONSE survey indeed enquires about the presence of a delegate in the physical establishment which, when a "distinct establishment" consists of a series of physical establishment, may differ from union presence in the corresponding "distinct establishment". In turn, the Acemo survey enquires about the presence of a union "in the firm or in any of its establishments".

This may however also reflect greater measurement error[46], either of union presence, or of the electoral scores, in the case of multi-establishment perimeters. Measurement error with respect to union presence may be more likely in these settings since one can think that the risks of misreport by the respondent increases with the number of representativity layers (e.g. establishment, firm, UES) in the same workplace. Electoral scores may be similarly more subject to measurement error in multi-establishment firms: as I mentioned in section 4.1., the election minutes associated to such election perimeters include a list of "associated establishments" [47], however, when this list is very long, it may include some mistakes.

elections, but not to take part in bargaining.

[44] This corroborates the finding from additional first stages in Figure 13 Appendix 8.3. using the presence of head of union section as dependent variables which I realized using the REPONSE dataset and which highlight clear jumps in the presence of head of union sections within establishments, but narrower than the jumps in the presence of union delegates: although unions have the possibility of being present in a firm despite having received less than 10% of the votes, while they almost systematically have a union delegate when they have the possibility to, the remedial solution of having a head of union section when in the impossibility of having a union delegate is less frequently taken upon.

[45] Although this problem is likely to occur as well for the other "categoriel" unions, namely the SNJ ("Syndicat National des Journalistes"), the SNPL ("Syndicat National des Pilotes de Ligne") and the SNPNAC ("Syndicat National du Personnel Navigant de l'Aéronautique Civile"), since these unions are present in only 0.5% of the elections of the sample while a list associated to the CGC was present in 20% of the elections of the sample), they are unlikely to affect the results.

[46] Although a continuously-distributed measurement error affecting all electoral scores would entirely prevent identification through RDD, the first stages highlight that the measurement error encountered here should solely result in an attenuation bias: since measurement error only concerns a subset of observations, despite measurement error, a net jump in the probability of treatment status persists at the threshold, thus permitting identification.

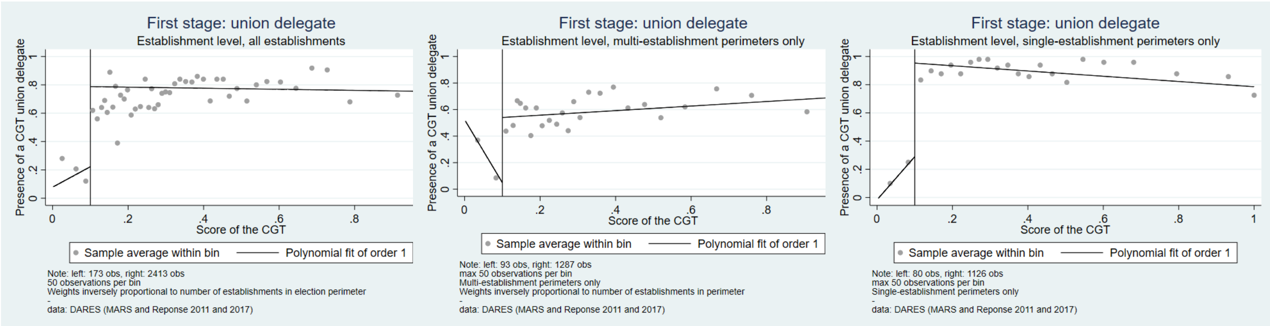
[47] "siret associés", thus designing the physical establishments. 27.8% of the election minutes thus refer to more than one physical establishment, the maximum being 895 establishments.

In the absence of certainty as to whether this discrepancy between single and multi-establishment first stages is a sign of measurement error of electoral scores, raising concerns of attenuation bias, I reproduced the analysis carried out in Section 6.1. by restricting the sample to single-establishment firms, in order to check whether the resulting treatment effects appear stronger than when including multi-establishment firms. However, since 39% of the elections in the sample correspond to multi-establishment firms[48] and since multi-unionism is more frequent among large firms, doing so results in a large reduction in sample size. As this did not appear to yield clearer results than the analysis carried out on the full sample, I decided to report the results obtained when pooling single and multi-establishment firms together.

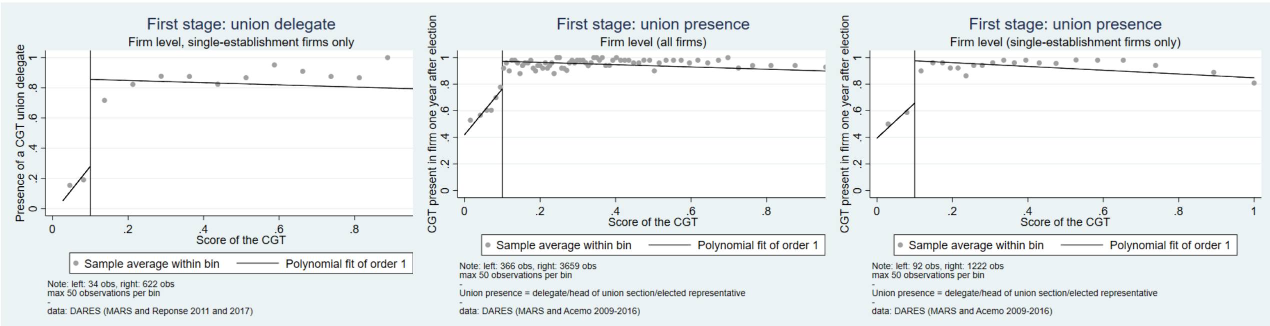
[48]This large proportion of multi-establishment firms may reflect both the fact that small firms - with more than 11 employees - may not hold elections despite the legal obligation, but also that I restricted the sample to elections for which minimum one list was running, and that the absence of list is more frequent among small firms.

Figure 5: Union-specific first stages

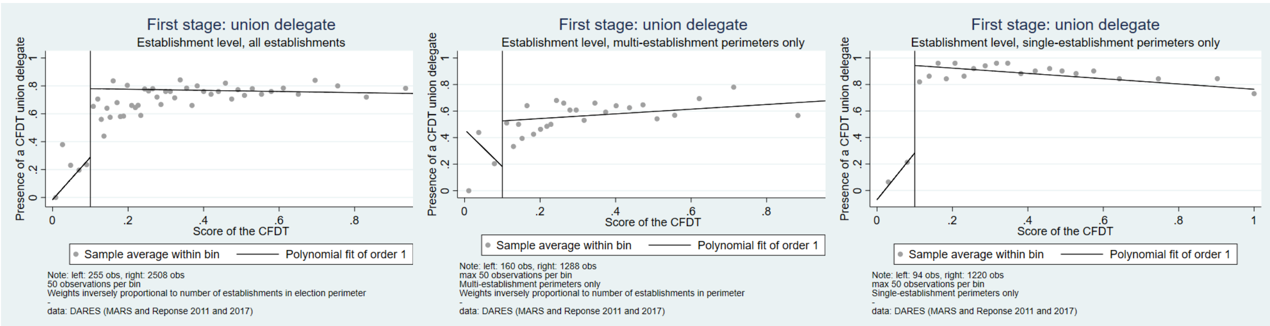
CGT, establishment level



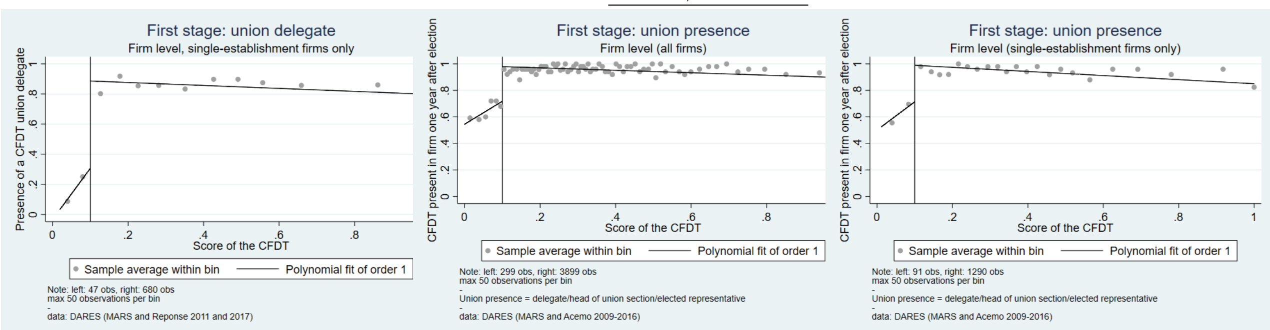
CGT, firm level



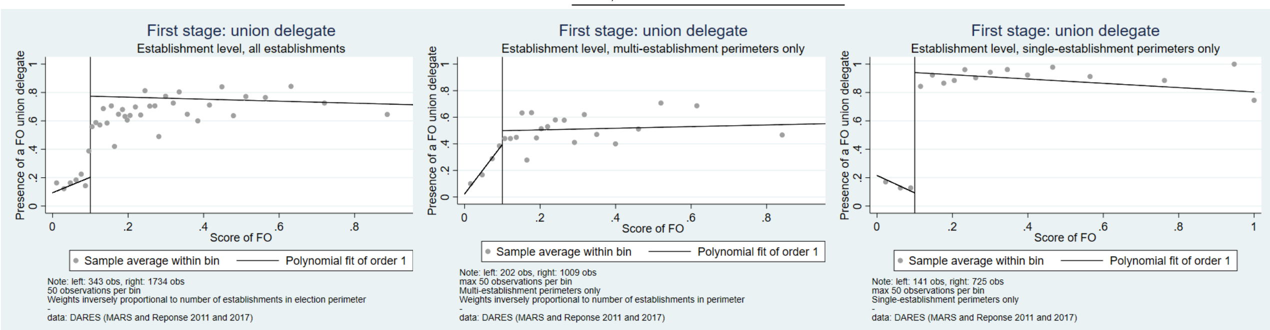
CFDT, establishment level



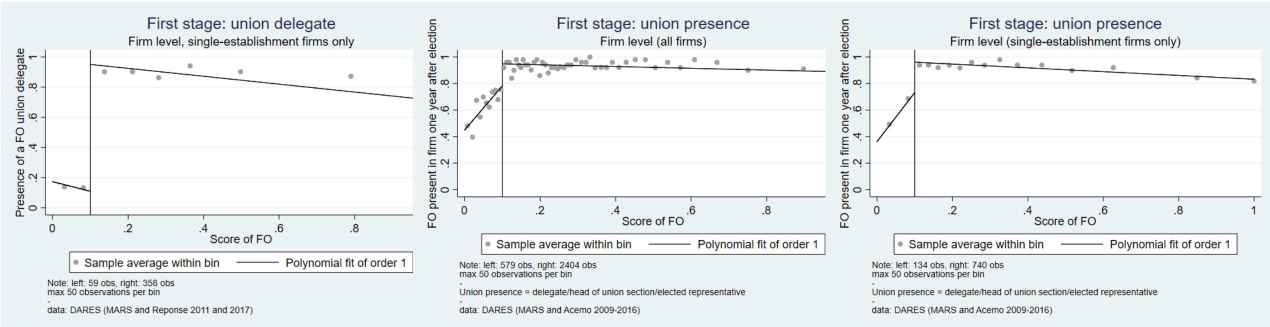
CFDT, firm level



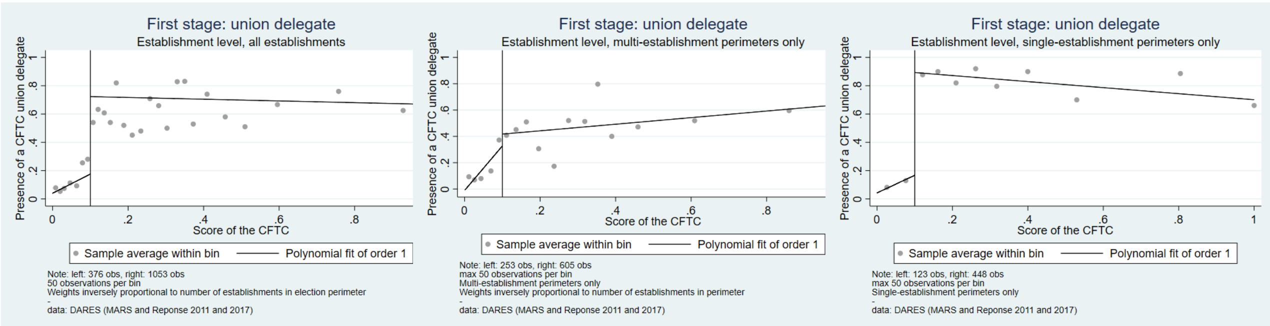
FO, establishment level



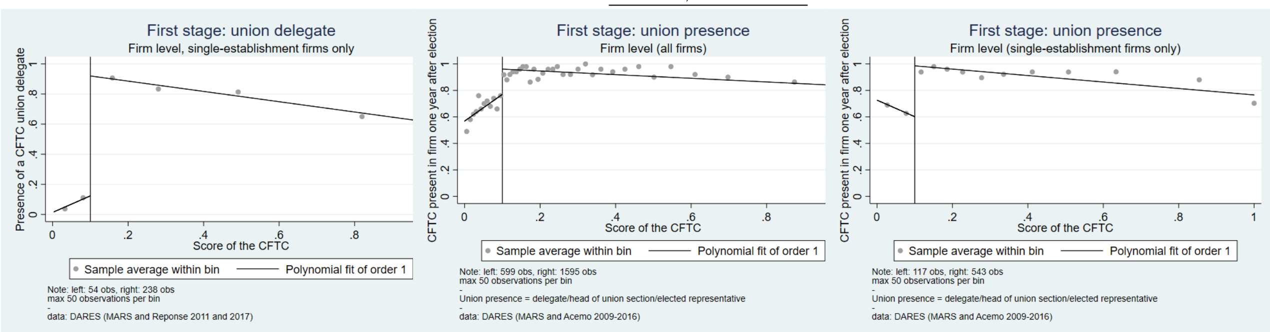
FO, firm level



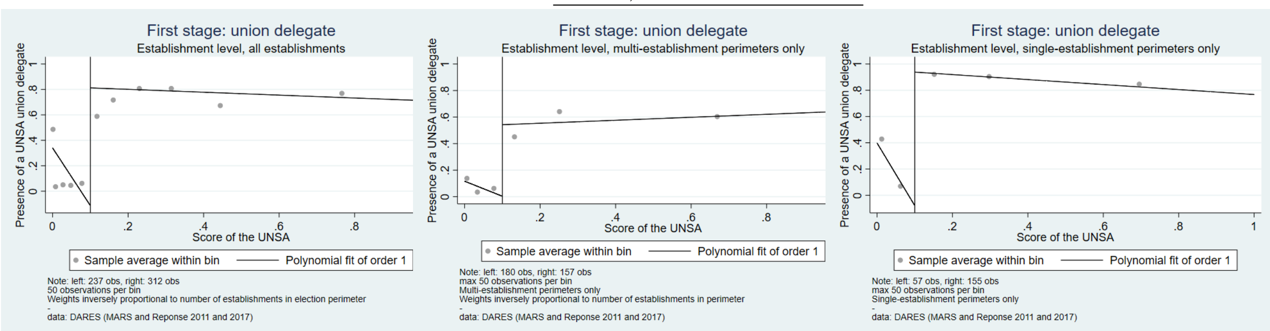
CFTC, establishment level



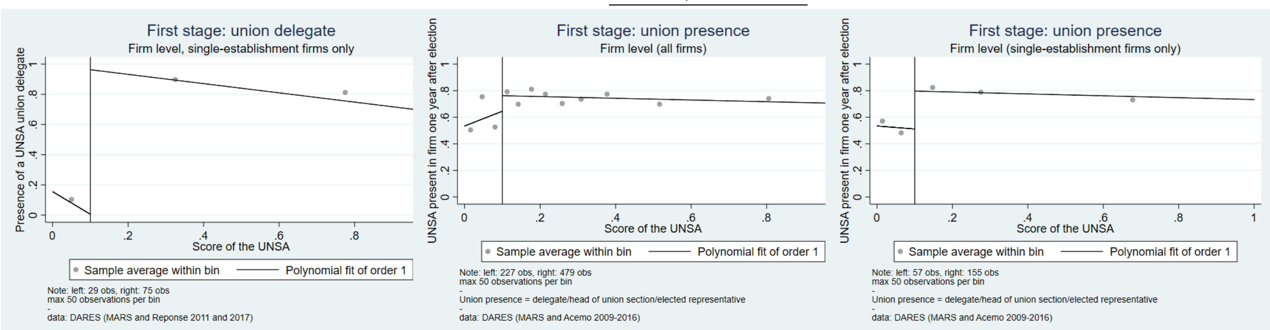
CFTC, firm level



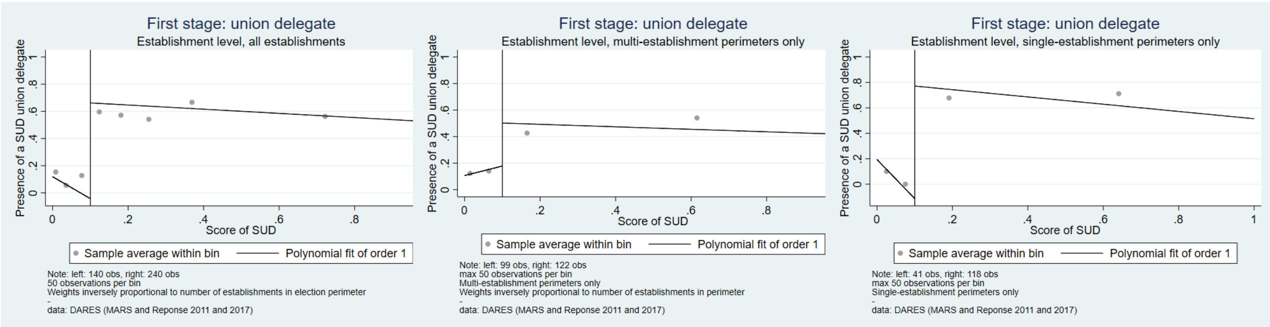
UNSA, establishment level



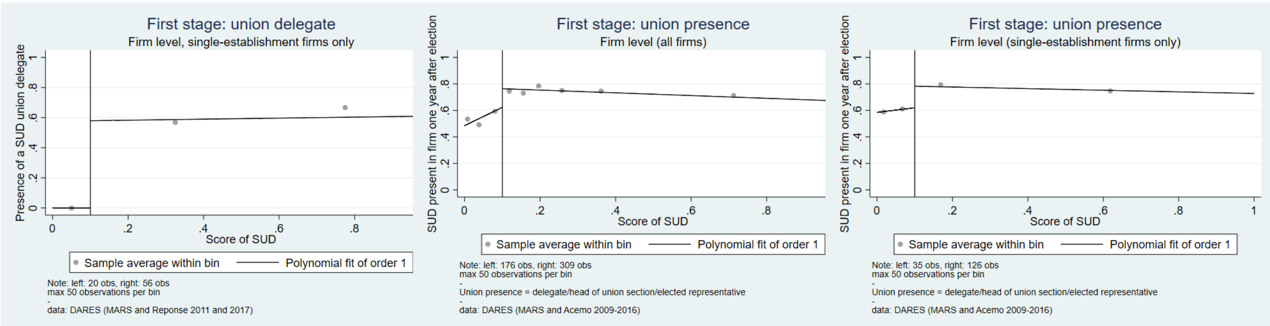
UNSA, firm level



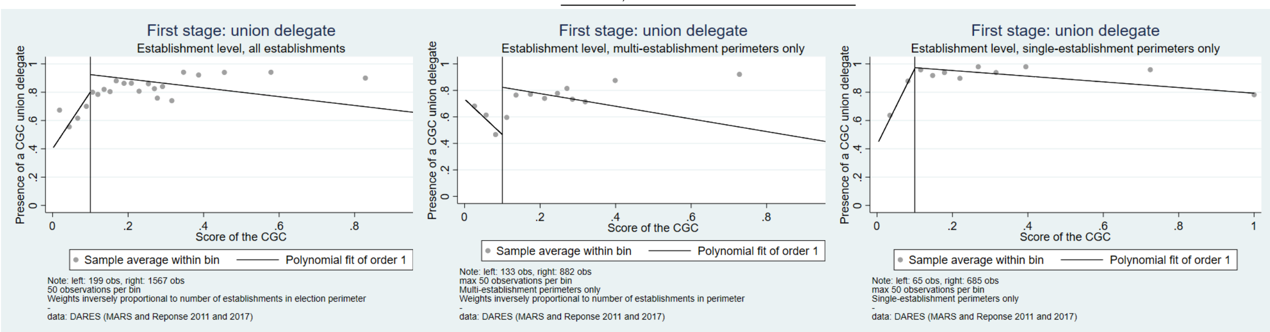
SUD, establishment level



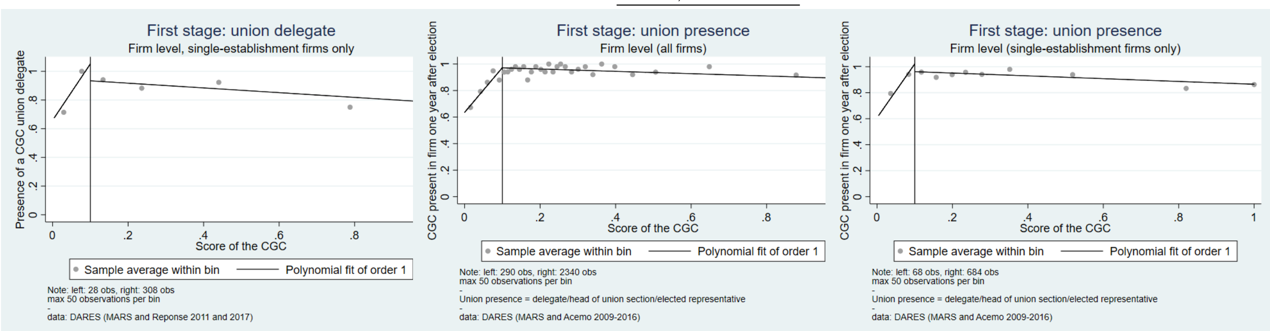
SUD, firm level



CGC, establishment level



CGC, firm level



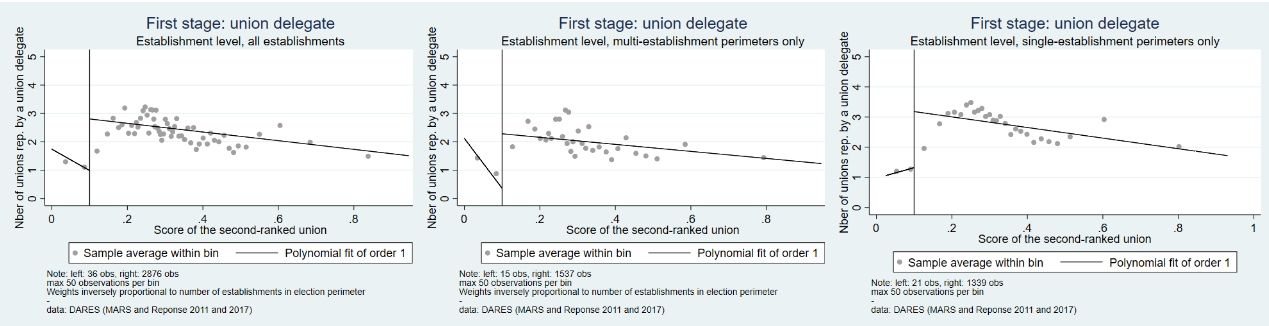
As illustrated by the rank-specific first stages in Figure 6 below, when using rank-specific running variables and the number of unions present as a dependent variable, the higher the rank, the narrower the jump: while when the firm-level score of the second or third union reaches 10%, the increase in union presence is close to 1, it respectively increases by 0.5 and 0.3 when the score of the fourth and fifth union reaches 10%.

This is likely attributable to the fact that, since, despite having more than 10% of the votes, a union may not be present in the firm, the higher the rank of the union whose score is used as a running variable, the more likely it is that minimum one of the unions with a higher score is absent from the firm.

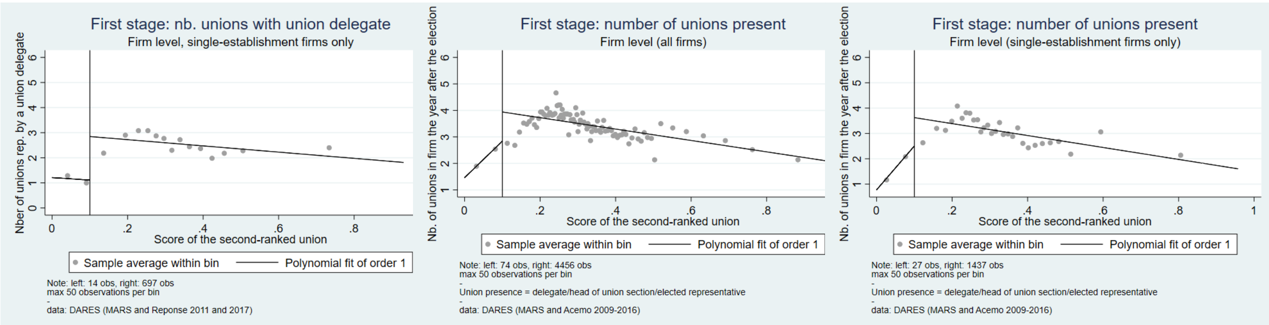
However, reassuringly, a positive jump in the number of unions present in the establishment or firm remains observable for all rank-specific running variables and, the higher the rank, the higher the corresponding levels. This implies that using these rank-specific scores as running variables should indeed enable identification of the treatment effects of higher numbers of unions, and that using the score of the fifth union as a running variable should permit identification of the effect of an additional union when the existing number of unions is higher than when using the score of the fourth union.

Figure 6: Rank-specific first stages

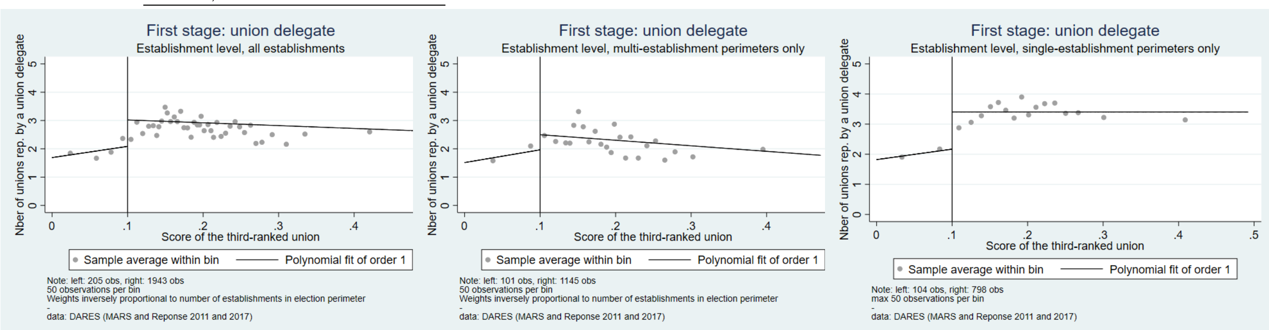
Second, establishment level



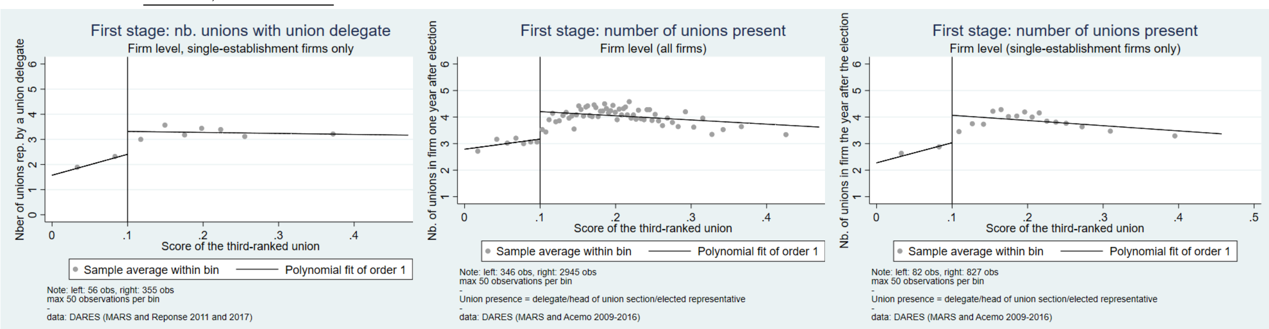
Second, firm level



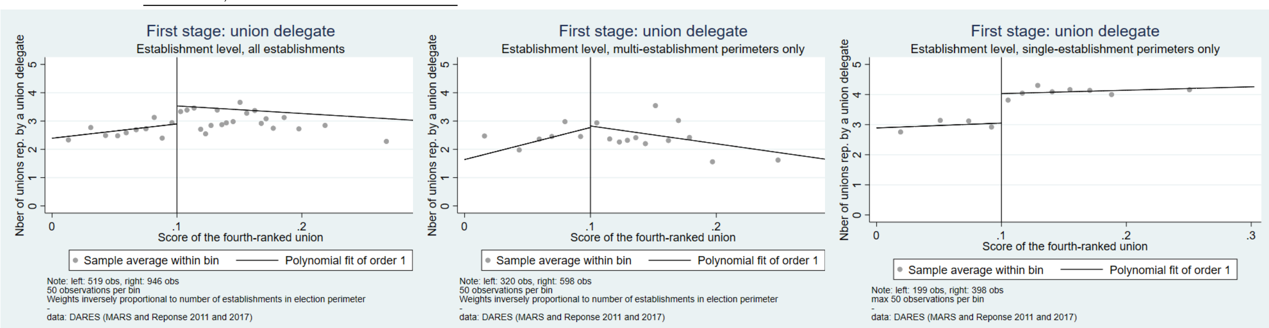
Third, establishment level



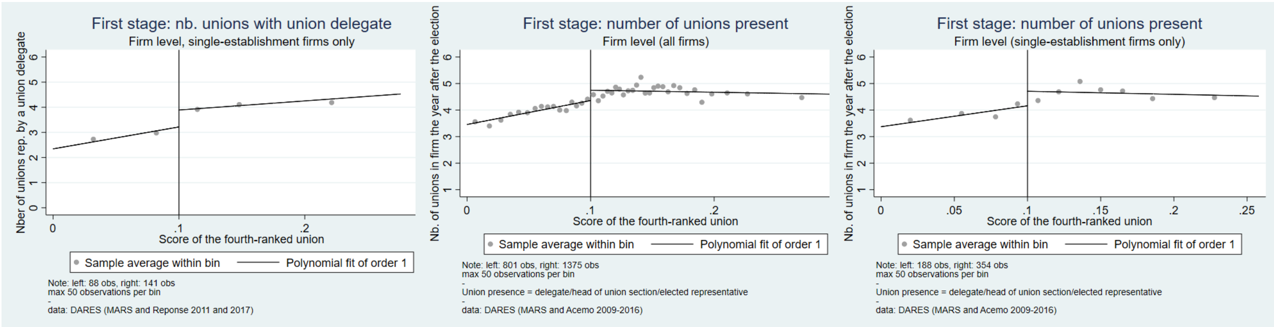
Third, firm level



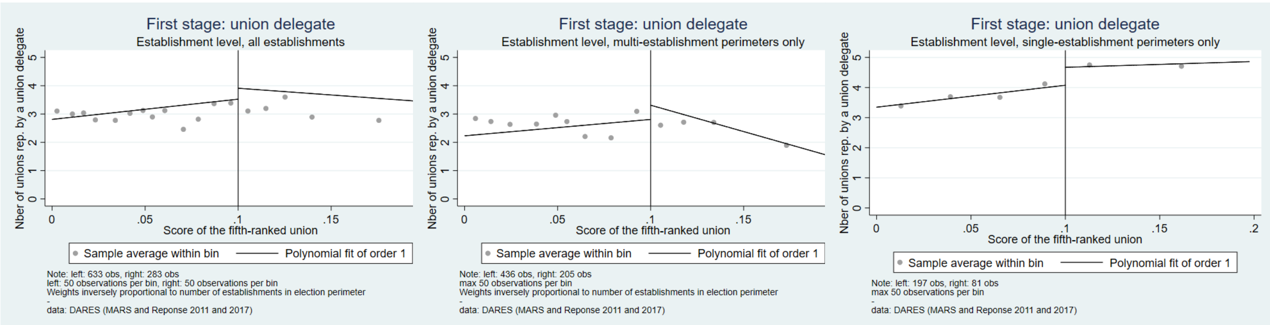
Fourth, establishment level



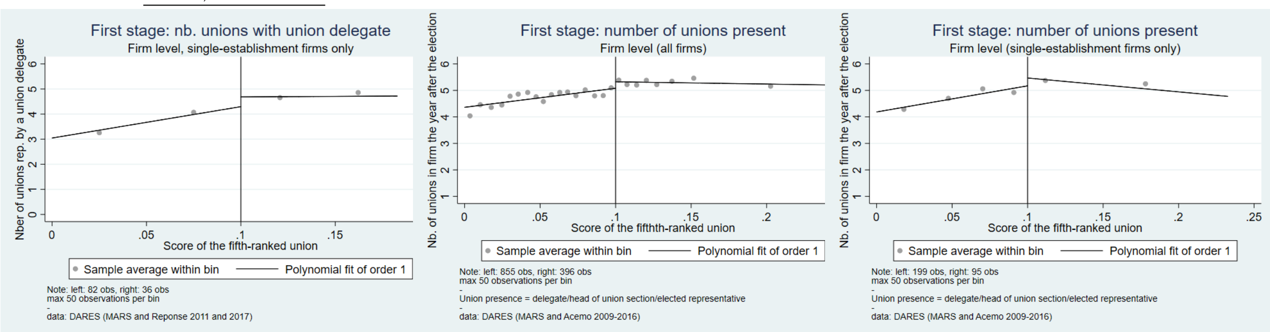
Fourth, firm level



Fifth, establishment level



Fifth, firm level



6.1.1.2 No effects on wages, share of wages in the value-added nor productivity?

When an additional union reaches 10% of the votes, this generates a jump in the number of unions with a delegate in the firm. Hence, comparing the average wage bargaining outcomes in the firms where a union obtained slightly more than 10% of the votes to the average bargaining outcomes in the firms where the union of corresponding rank obtained slightly less than 10% should identify Intention to Treat Effects of an additional bargaining union.

Can we detect a positive or negative net effect of an additional union on wages?

To answer this question, I first ran a series of RDDs around the 10% threshold, using as dependent variables the log of the average gross wage respectively, one, two and three years after the election. Using the score of the second, third, fourth and fifth-ranked union as running variables thus enables me to capture the Intention to Treat Effects of an additional union having the possibility to take part in bargaining in a firm.

In the light of the results from previous tests and first stages, I systematically exclude elections where the running variable takes the threshold value, elections with pre-electoral alliances and elections where the union of corresponding rank is the CGC[49]. To further increase accuracy, I also

[49] I also exclude the CGC when exploiting the 30% and 50% thresholds because the 2008 law did not explicitly state whether a "catégoriel" union could sign agreements alone. It was only clarified (even if received more than

exclude outliers following the procedure I described in Section 4.2. and control for the log of the number of voters (since it is locally tightly related to the running variable and, being correlated with workforce size, is also correlated with the outcomes studied) and year fixed effects[50].

Since the tests of the identification assumption carried out in section 5.3.2. suggested that identification could be threatened by the inclusion of elections where the running variable takes the threshold value and elections with pre-electoral alliances, I excluded these elections. Furthermore, as the first-stage in 6.1.1.1. indicates the absence of a jump in the presence of the CGC at the threshold, I also excluded elections where the CGC is the union of corresponding rank. Finally, to further increase precision, I excluded the outliers following the procedure described in section 4.2. and used the log of the wage as a dependent variable.

Although I omit the formulation "ceteris paribus" for brevity, all comparisons of the values taken by a dependent variable on opposite sides of a threshold are therefore ceteris paribus comparisons.

I report the resulting estimates in Table 11 below. To put the magnitude of the estimates in perspective and ascertain that potentially-detected effects do not result from a pre-existing trend, they are reported along with estimates obtained when using as dependent variables the growth rate of the gross average wage one year before the election and the log of the average gross wage one year before the election.

30% of the votes once considering all electoral colleges, these unions cannot sign agreements alone, unless these agreements only apply to the category of workers they are meant to represent) by the jurisprudence of the "Cour de Cassation" on the 2^{nd} July 2014.

[50] In the following, I systematically exclude the same observations and control for the log of the number of voters. However, starting from 6.1.1.2.1., I use residualized dependent variables and no longer control for year fixed effects as it results in multi-collinearity issues.

Table 11: Effect of an additional representative union on the average gross wage

	Cut-off: 0.1				
	Running variable: score of the second-ranked union				
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Growth rate of average wage 1-yr before	Log of average wage 1-yr before	Log of average wage 1-yr after	Log of average wage 2-yrs after	Log of average wage 3-yrs after
RD_Estimate	0.00141	0.0643	0.0407	0.00627	-0.0224
	(0.0267)	(0.0509)	(0.0532)	(0.0576)	(0.0745)
Conventional p-value	.958	.207	.445	.913	.764
Obs left of cut-off	230	252	234	188	124
Obs right of cut-off	572	616	555	450	297
	Running variable: score of the third-ranked union				
RD_Estimate	0.0123	0.0198	0.0138	0.0208	-0.0124
	(0.0140)	(0.0301)	(0.0307)	(0.0324)	(0.0462)
Conventional p-value	.377	.511	.654	.52	.789
Obs left of cut-off	644	719	663	532	323
Obs right of cut-off	1137	1245	1144	936	582
	Running variable: score of the fourth-ranked union				
RD_Estimate	0.00580	0.0498	0.00194	0.0343	0.0586
	(0.0151)	(0.0304)	(0.0289)	(0.0329)	(0.0443)
Conventional p-value	.701	.102	.947	.297	.186
Obs left of cut-off	785	888	809	668	442
Obs right of cut-off	911	1003	907	761	512
	Running variable: score of the fifth-ranked union				
RD_Estimate	-0.0415	0.00491	-0.0142	-0.00530	-0.0102
	(0.0460)	(0.0454)	(0.0463)	(0.0494)	(0.0639)
Conventional p-value	.367	.914	.759	.915	.874
Obs left of cut-off	478	534	483	424	279
Obs right of cut-off	325	357	328	279	184
Bandwidth	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: col1: growth during year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

From Table 11, it is apparent that none of the obtained coefficients is statistically-significant.

Upon first inspection, this could be thought to reflect a lack of accuracy due to the small sample size, amplified by the fact that the RDDs performed are fuzzy.

Indeed, since the slope of the relation between the dependent variable and the electoral score is estimated separately on opposite sides of the threshold, inference is not in the total number of observations in the bandwith, but the number of observations in the bandwidth to one side of the 10% cut-off only, resulting in 124 (score of the second union, column 5) to maximum 1,245 (score of the third union, column 2) observations available.

As a result, the minimum detectable effects (at 5%) range between changes in the average gross wage of the order of 5.78% (score of the fourth union, column 3 - corresponding to a 0.19 standard deviation increase once compared to the distribution of the log of the average wage one year after the election and excluding outliers) to 14.9% (score of the second union, column 5 - a 0.49 standard deviation increase). Remembering that the French union wage premium estimated by Breda [2015] is of only 2%, and that the first stages suggest jumps in the probability of treatment ranging of nearly 100% (when the score of the second or third union is used as a running variable), but also 50% and 30% (when the scores of the fourth and fifth union are used as running variables), it is apparent that the minimum detectable effects would indeed not be credible.

Henceforth, one should not, on the basis of the obtained p-values only, conclude in the absence of net effect of an additional union on wages: the statistical power available only permits ruling out particularly large, non-credible treatment effects.

Do the differences in trends suggested by the coefficients nonetheless suggest a consistent or interpretable pattern? As I detail below, further inspection does not appear to hint at any clear pattern or remarkable difference in trends, once considered in the light of pre-existing differences.

The results obtained for the second and third union could a priori be interpreted as suggesting the reversal of an initially negative trend. Indeed, the year before the election, both the growth rate and the log of the average gross wage was larger among firms where the second or third union obtained slightly less than 10% of the votes (as opposed to those where it obtained slightly more). After the election, the difference in logs narrows down, eventually attaining negative values three years after the election. However, one should note that, three years after the election, the difference between the log of the average wage in firms on opposite sides of the threshold is lower in absolute value than the difference before the election, suggesting that the post-electoral differences are hardly exceptional and might be attributable to sheer randomness.

By contrast, the amplitude of the difference between the log of the average gross wage in firms where the fifth union obtained slightly more than 10% of the votes and those where it obtained less is larger in absolute value (0.0102) three years after the election than before the election (0.00491). This could be thought to suggest a negative effect resulting in a reversal in the sign of the difference. Indeed, it was positive but close to null before the election (the average gross wage was 0.491% higher in firms where the fifth union obtained more than 10% of the votes, as opposed to those where it obtained less), but negative and of larger amplitude after the election (three years after, it was 1.02% lower in the former firms compared to the latter). However, one must observe that, the year before the election, the growth rate of the average gross wage was lower by 4.15 percentage points in the former compared to the latter firms. This suggests that, rather than the net effect of a fifth bargaining player on the average wage, this difference three years after the election might reflect a pre-existing trend.

The results obtained for the fourth union do not suggest any clear effect of a fourth bargaining player either. The log of the average gross wage was initially larger among firms where the fourth union obtained slightly more than 10%. After the election, this difference has initially narrowed down (one and two years after the election). This might be thought to indicate a possible initial reversal of trend since before the election, the growth of the average gross wage was higher in the former firms. However, three years after the election, the difference increased anew, shedding doubt on the validity of that interpretation as no clear pattern appears to emerge.

One could think that the absence of clear pattern could reflect contradictory effects on the share of wages in the value-added and on the value-added itself. However, Tables 49 and 50 in Appendix 8.5.1. illustrate that trying to separately identify treatment effects of an additional bargaining player on the share of wages in the value-added and on the value-added does not enable us to detect a clear pattern on either variable.

Indeed, only one of the coefficients obtained when running RDDs on measures of the dependent variables after the election is significant at 10% (the value-added by worker two years after the election was higher by 10% in firms where the fourth union obtained slightly more than 10% of the votes), and this may partly reflect a pre-existing positive trend whereby, before the election, the growth rate of the value-added by worker was higher in the former firms.

Otherwise, when considering effects on both dependent variables, the only cases where the magnitude of the differences found increases after the election as opposed to before seem to reflect pre-existing differences in trends (respectively the difference between the share of wages in the value-added in the firms where the second or fourth union obtained more as opposed to less than 10%, the difference in the log of the value-added by worker in the firms where the second union obtained more as opposed to less than 10% of the votes, the difference between the log of the value-added by worker in the firms where the fourth union has obtained more as opposed to less than 10%).

By contrast, all reversals of trend (the difference between the share of wages in the value-added in the firms where the third union obtained more as opposed to less than 10%, the difference between the value-added by worker in the firms where the third union has obtained less more as opposed to less than 10%) result in coefficients of lower amplitude after the election compared to before. This therefore sheds doubt on whether they should be attributed to the treatment rather than random economic fluctuations.

All things considered, these preliminary results therefore appear to suggest the absence of a net effect of an additional bargaining union on wages, but also the absence of any effect on the share of wages in the value-added or on the value-added. Indeed, the differences after the election are systematically either of lower amplitude than those before the election or, when of larger amplitude, seem to be the fruit of pre-existing non-parallel trends between the firms on opposite sides of the threshold.

6.1.1.2.1 Increasing the accuracy: residualized dependent variables and growth -

Before concluding in the absence of effect, I however carry out additional RDDs in an attempt to gain accuracy, following a two-fold approach: I both "residualize" the dependent variables, and as a consistency check, replace the dependent variables by their trends compared to before the election.

First, I follow an approach suggested in Lee and Lemieux (Lee and Lemieux [2009], p55 in the NBER working paper) to increase the accuracy of RDDs, by "residualizing" the dependent variables.

In a first step, I thus use pre-determined characteristics (election year and sector fixed effects, the log of workforce size before the election and the value of the variables of interest the year before the election) to predict the variables of interest (I use as dependent variables the log of the average wage after the election, the log of the share of the value-added after the election, the log of the value-added by worker after the election and the log of the electoral profitability after the election, since the R^2 obtained were systematically higher when using log than levels). The regression results can be found in tables 51 to 54 in Appendix 8.6.1.: unsurprisingly, including the value of the dependent variable before the election substantially increases the R^2 of the regressions, compared to the regressions I used to test for the balance of pre-determined characteristics.

In a second step, I use the residuals from these regressions as dependent variables in the RDDs I realize.[51] Doing so enables me to increase the power of the RDDs, since it focuses on explaining the part of the variables of interest which cannot be explained by pre-determined characteristics,

[51] Prior to this, I realized RDDs, verifying that, when excluding outliers as well as elections with pre-electoral alliances and elections where the running variable takes the cut-off value, the resulting predictions are balanced around the threshold. The resulting RDD outputs can be found in tables 55 to 62 in Appendix 8.6.2.

including the laged dependent variable. While I could have simply controlled for these characteristics in the RDDs, given the relatively small samples available due to recourse to local estimation, doing so could have resulted in irrelevant covariates, eventually decreasing the accuracy. By contrast, using the full sample to predict the variables of interests allows me to make more precise predictions. To assess the magnitude of the coefficients in relative terms, the standard deviations of these residuals can be found in table 107 in Appendix 8.9.. Since year fixed effects are used in the predictions from which I derive the residuals, in the following, I no longer control for year fixed effects when using residualized dependent variables.

Second, I also check whether the trends suggested by the residuals are consistent with the actual differences in the growth rates and changes in the variables of interest.

To do so, I run additional RDDs, using as dependent variables the growth rates (of the average gross wage, share of wages in the value-added, value-added by worker and economic profitability - the last two growth rates replaced by missing values when the value-added by worker or economic profitability takes null or negative values) and increases[52] (of the value-added by worker and economic profitability) of the variables of interest, compared to their value before the election.

I summarize in Table 12 below the results I thereby obtained when estimating effects of an additional bargaining union on the gross average wage (detailed RDD output can be found in tables 63 to 66 in Appendix 8.7.1.).

Table 12:

Estimated treatment effects: recap *(Detailed results in Appendix)*

Cut-off: 10%

Net effect on the average gross wage

Dependent variables:
- residual from a regression of the log of the average gross wage on pre-determined firm characteristics (sector and year fixed effect, log(workforce size before the election), log(average gross wage before the election))
- growth rate of the average gross wage compared to the year before the election

	Second		Third		Fourth		Fifth	
Dependent variable:	Residual	Growth	Residual	Growth	Residual	Growth	Residual	Growth
1-yr before	*N.A.*	0.00141 (0.0267)	*N.A.*	0.0123 (0.0140)	*N.A.*	0.00580 (.0151)	*N.A.*	-0.0415 (0.040)
1-yr after	0.0377 (0.0302)	0.0190 (0.0237)	-0.00473 (0.0171)	-0.0122 (0.0163)	-0.0124 (0.0168)	-0.0269 (0.0173)	-0.0216 (0.0270)	-0.0218 (0.0245)
2-yrs after	0.00583 (0.0361)	-0.0192 (0.0359)	-0.0130 (0.0195)	-0.0296* (0.0152)	0.0167 (0.0216)	0.00271 (0.0246)	-0.0174 (0.0297)	-0.0151 (0.0260)
3-yrs after	0.0121 (0.0514)	0.0452 (0.0704)	-0.0231 (0.0284)	-0.00479 (0.0269)	0.0654** (0.0305)	0.0449 (0.0339)	-0.0107 (0.0402)	-0.0148 (0.0316)

Note: data: DARES (MARS 2009-2016), INSEE (BRN 2007, FARE 2008-2017, agrifin 2012-2017, inflation series)

Despite these additional steps to increase the accuracy of the estimates, the minimum detectable effects (at 5%) would be too large to be credible. When using residualized dependent variables, they indeed range from changes in the average gross wage of the order of 3.42% (third union, one year after the election - corresponding to a 0.2 standard deviation difference when compared to the distribution of the residuals used and excluding outliers) to changes of the order of 10.28% (second union, three years after the election - corresponding to a 0.54 standard deviation difference). When using growth rates as dependent variables, they range from differences of the order of 3.04 percentage points (third union, two years after the election - a 0.13 standard deviation difference)

[52] As mentioned in section 4.2., although using log or growth rates rather than levels enables me to gain accuracy by reducing the weight of outliers, doing so may also bias the estimates in the case of the value-added and economic profitability. Indeed, since the latter variables can take negative values, having recourse to the log or growth rates introduces a selection bias, whereby firms with negative value-added or economic profitability are ignored. Comparing whether the estimates obtained when using residualized log, growth rates or increases as dependent variables are consistent can therefore help clear the question of whether the effects inferred are contaminated by such bias.

to differences of the order of 14.08 percentage points (second union, three years after the election - a 0.59 standard deviation difference).

Although two coefficients are significant at respectively 5% and 10% (in the firms where the fourth union obtained slightly more than 10% of the votes, the part of the log of average gross wage which cannot be explained by pre-determined characteristics was higher by 0.0654 compared to the firms where it obtained slightly less, while in the firms where the third union obtained slightly more than 10% of the votes, the growth rate of the average gross wage two years after the election was lower by 2.96 percentage points compared to the firms where it obtained slightly less), they are not robust to Holm's multiple testing correction[53].

Since the minimum detectable effects remain too large to be credible, I inspect anew, despite this lack of significance, the trends suggested by the point estimates. Doing so however corroborates the previous absence of clear pattern.

First, if one solely trusted the sign of the estimates, the effects of a third and fifth union would appear as negative (although, for the fifth union, it seems to reflect a pre-existing negative trend which has attenuated over time), while the effects of a second or fourth union would appear as mainly positive. Thus, arguing that the effect of an additional union when the number of unions is low is different from its effect when the number of unions is large would not suffice to account for these results.

Second, in the presence of a treatment effect measured over a larger time span (while the growth rate one year before the election corresponds to a growth rate over one year, the other three growth rates correspond to growth rates over a two, three and four years periods), one should expect coefficients of larger amplitude, unless the treatment effect fluctuates over time. By contrast, for most running variables, the amplitude of the coefficients does not appear to increase over time.

Finally, although for the second and fourth union, the magnitude of the coefficients is larger when considering the growth rate of the average gross wage after the election than when considering that before the election, possibly suggesting the presence of treatment effects, the sign of the estimates fluctuates and cannot be interpreted as reflecting the same cycle for both ranks.

6.1.1.2.2 Decomposition: no effect on share of wages in value-added nor productivity -

Does the apparent absence of a net effect on wages necessarily imply that the presence of an additional bargaining union has no effect on wage bargaining?

As previously-explored, the absence of clear net effects of an additional union on the average gross wage could however be the result of opposite effects on the share of wages in the value-added and effects on the value-added. To investigate this possibility, I therefore run additional RDDs, using as dependent variables the residualized log of the share of wages in the value-added and residualized log of the value-added by worker, as well as growth rate of the share of wages in the value-added and growth rate and increase of the value-added by worker.

The detailed results can be found in tables 67 to 74 in Appendix 8.7.1. and are summarized in Table 13 below.

[53]The corresponding p-values are respectively 0.032 and 0.052, to be respectively compared with benchmarks of 0.1/28=0.00357 and 0.1/0.27=0.00370 if one wishes to control for the 10% family-wise error rate).

Table 13:

Estimated treatment effect: recap *(Detailed results in Appendix)*

Cut-off: 10%

Effect on the share of wages in the value-added

Dependent variables:
- residual from a regression of the log of the share of wages in the value-added on pre-determined firm characteristics (sector and year fixed effect, log(workforce size before the election), log(share of wages in the value-added before the election))
- growth rate of the share of wages in the value-added compared to the year before the election

	Second		Third		Fourth		Fifth	
Dependent variable:	Residual	Growth	Residual	Growth	Residual	Growth	Residual	Growth
1-yr before (Placebo)	*N.A.*	0.113** (0.055)	*N.A.*	-0.0449 (0.0279)	*N.A.*	-0.0166 (0.0238)	*N.A.*	-0.00146 (0.0301)
1-yr after	-0.0127 (0.0391)	-0.00906 (0.0482)	-0.00468 (0.0222)	0.00596 (0.0288)	-0.00662 (0.0242)	-0.00304 (0.0267)	0.0234 (0.0269)	0.0241 (0.0298)
2-yrs after	-0.0134 (0.0431)	-0.0264 (0.0551)	-0.00598 (0.0327)	0.0232 (0.0439)	-0.0409 (0.0282)	-0.0285 (0.0364)	0.0331 (0.0354)	0.0618 (0.0458)
3-yrs after	0.0048 (0.0457)	0.00352 (0.051)	0.00404 (0.0401)	0.0362 (0.0556)	-0.0121 (0.0443)	0.0553 (0.0633)	-0.00595 (0.0408)	-0.0215 (0.0506)

Effect on the value-added by worker

Dependent variables:
- residual from a regression of the log of the value-added by worker on pre-determined firm characteristics (sector and year fixed effect, log(workforce size before the election), log(value-added by worker before the election))
- increase in the value-added by worker compared to the year before the election
- growth rate of the value-added by worker compared to the year before the election (only defined for positive value-added)

	Second			Third			Fourth			Fifth		
Dependent variable:	Residual	Increase	Growth	Residual	Increase	Growth	Residual	Increase	Growth	Residual	Increase	Growth
1-yr before (Placebo)	*N.A.*	-3.115 (4.290)	-0.0224 (0.0375)	*N.A.*	3.381** (1.713)	0.0688** (0.027)	*N.A.*	0.879 (1.865)	0.0229 (0.0312)	*N.A.*	-2.257 (3.710)	-0.0266 (0.0524)
1-yr after	0.0475 (0.0484)	-2.099 (3.665)	0.0671 (0.0541	-0.0095 (0.0270)	-2.094 (2.181)	-0.0473 (0.0362)	-0.0173 (0.0299)	-2.577 (2.367)	-0.0719* (0.0397)	-0.133 (0.0980)	-5.011 (4.110)	-0.0422 (0.0419)
2-yrs after	0.0286 (0.0587)	0.463 (5.225)	0.0492 (0.0647)	-0.0138 (0.0421)	-3.266 (3.747)	-0.0791 (0.0791)	0.0477 (0.0373)	0.305 (3.143)	-0.0290 (5.340)	-0.0523 (0.0483)	-1.968 (4.200)	-0.0155 (0.0482)
3-yrs after	-0.0106 (0.0703)	-2.483 (5.276)	0.0563 (0.0873)	-0.0185 (0.0541)	-7.963* (4.243)	-0.0545 (0.0678)	0.0896 (0.0553)	-1.040 (5.340)	-0.0108 (0.0740)	0.0255 (0.0666)	0.613 (5.939)	0.0185 (0.0591)

Note: data: DARES (MARS 2009-2016), INSEE (BRN 2007, FARE 2008-2017, agrifin 2012-2017, inflation series)

Despite the increased accuracy, previous results seem corroborated, in the sense that the estimates obtained do not appear to reveal clear or remarkable patterns.

First, let us start with the share of wages in the value-added. The only significant coefficient concerns differences prior to the election and, once again, no clear trend seems apparent.

Indeed, if one solely focused on the signs obtained, the effect of a second and fourth union would appear to be mainly negative while the effect of a third or fifth union would appear mainly positive. The fact that the signs alternate seems to rule out explanations in terms of nonlinear effects of multi-unionism. Furthermore, when using the score of the second, fourth and fifth union as running variables and using post-electoral measures of the share of wages in the value-added, the signs obtained appear to alternate over time. As this does not seem to reflect a common cycle, it reinforces suspicions that the signs obtained are attributable to chance.

Turning to the value-added by worker, there is no evidence of remarkable or consistent differences either.

Indeed, few coefficients are significant and the most significant coefficients correspond to differences before the election (the year before the election, annual gross value-added by worker increased on average by 3.381 more thousands euros in the firms where the third union obtained slightly more than 10% of the votes, as opposed to those where it obtained slightly less, corresponding to a growth rate higher by 6.88 percentage points when excluding the firms with negative value-added).

When focusing on the years after the election, the only two statistically-significant coefficients (three years after the election, the value-added by worker had increased by 7.963 thousands euros less in the firms where the third union obtained slightly more than 10% of the votes, and if one restricts herself to the firms with positive value-added, one year after the election, the growth rate of the value-added by worker – compared to the year before the election - was lower by 7.19 percentage points in the firms where the fourth union obtained slightly more than 10% of the votes) are only significant at 10% and therefore not robust to Holm's multiple testing correction.

Focusing on the trends suggested by the signs and amplitudes of the coefficients do not enable detection of remarkable patterns either.

Indeed, the sign of the coefficients obtained when using the growth of the value-added by worker since the year before the election would a priori suggest a positive effect of a second union obtaining 10% of the votes, but a mainly negative effect of the third, fourth or fifth union obtaining 10% of the votes.

However, most of this evidence appears dubious in the light of the results obtained when using residuals, increases and changes prior to the election as dependent variables.

Thus, for the second and fourth union, the signs of the coefficients obtained when using residuals or increases as dependent variables suggest a more ambiguous story.

In turn, in the case of the fifth-ranked union, before the election, the value-added by worker already followed a more negative trend in the firms where the fifth union obtained slightly more than 10% of the votes, suggesting that the negative effect found reflects pre-existing imbalances. Furthermore, all three specifications suggest a reversal of trend three years after the election.

Finally, while in the case of the third union, all three specifications, as well as their comparison to the difference in trends before the election would suggest a negative effect, the amplitude of the coefficients obtained for the increase in the value-added by worker or its growth rate after the election is of greater amplitude than the coefficient obtained when using the increase in value-added or its growth rate before the election in only one out of six specifications (the increase in the value-added by worker three years after the election), thus shedding doubt on the idea that the reversal in trend should be attributed to the effect of a third union rather than to a random economic fluctuation.

One might however want to compare the sign of the coefficients obtained when using as a dependent variable the growth rate of the share of wages in the value-added to the sign of the coefficients obtained when using as a dependent variable the growth rate of the value-added by worker the corresponding year. Indeed, when focusing on the years following the election, the resulting signs are opposed in nine out of twelve cases.

However, this should not be interpreted as signalling opposed effects of an additional union on the two variables, possibly explaining the failure to detect a clear net effects on wages.

Indeed, if one repeats the same process by comparing the sign of the coefficients obtained when using respectively as dependent variables the growth rate of the share of wages in the value-added before the election and the growth rate of the value-added by worker, the coefficients are of opposed sign in exactly three out of four cases. This suggests that, rather than reflecting ambiguous consequences of an additional union, the above-mentioned reflects a pre-existing wage stickiness, whereby, for instance, firms characterized by a lower increase – or a larger decrease - in their value-added are mechanically characterized by a larger increase – or lower decrease – in the proportion of the value-added represented by wages.

6.1.1.3 Recap: No effects of an additional union, or no effects of an additional *weak* union?

To recap, an additional union obtaining the possibility of taking part in bargaining does not seem to be associated with a clear net effect on wages. Although I did not report the results, restricting the sample to single-establishment firms, in the light of the results from the first stages, was similarly inconclusive.

Even after having recourse to residualization and using the growth of the average gross wage as dependent variable, the minimum detectable effects are far superior to the effects one could expect. Nonetheless, failure to reject the null should not be solely attributed to the lack of statistical power when carrying out a fuzzy RDD over relatively small samples: the lack of consistency in the signs obtained and the frequently lower or similar magnitude of the coefficients obtained when considering outcomes posterior, as opposed to anterior, to the election, suggests an absence of net effect on wages.

This absence of identifiable effect does not seem to come from an opposition between contradictory effects on the share of wages in the value-added and on the value-added itself. Indeed, there appears to be no consistent effect on either variable and the opposition between the measured effects on both variables is likely to reflect a more general climate of wage stickiness.

However, this should not be interpreted as implying that the simultaneous presence of several trade-unions in the same firm has no effect on wage bargaining. Indeed, as argued in Introduction, coordination frictions and increased representation efforts might have opposite effects, offsetting one another. Furthermore, the Intention to Treat Effects captured are only local, since they correspond to effects of an additional *weak* union with the possibility of taking part in bargaining. Meanwhile, one could expect the coordination frictions, as well as possibly electoral competition, exerted by an additional union, to be stronger when that additional union is stronger.

While no other threshold allows us to detect the effect of additional electoral competition, exploiting the 30% and 50% thresholds might be conducive to detecting coordination frictions.

Indeed, in the presence of coordination frictions, whereby unions struggle to coordinate on common bargaining claims and strategies, a union reaching 30% of the votes should be associated with a decrease in the effects of these coordination frictions, since no agreement between unions is necessary for an agreement with the employer to be signed.

By contrast, in the presence of major coordination frictions, one might expect a union reaching 50% of the votes to be associated with an increase in the effects of the latter frictions, since the majoritarian union gains the possibility to block negotiations.

6.1.2 Effects of a reduction in coordination frictions: 30% threshold

To investigate whether the co-existence of multiple unions in the same firm appears to result in coordination frictions between unions, thereby affecting the wage bargaining, I therefore turn to specifications using the scores of the first, second and third union as running variables, and comparing firms where the corresponding union obtained slightly more, as opposed to slightly less, than 30% of the votes.

6.1.2.1 Positive net effect on wages

If coordination frictions between unions were sufficient to affect wage bargaining, one would expect a positive net effect on wages of a union obtaining more than 30% of the votes.

Indeed, as a union gains the possibility to sign agreements alone, this reduces the need for coordination. One should a priori expect this to increase the probability that an agreement be signed, thereby increasing the share of wages in the value-added, unless this also results in an increase in firm productivity (as a result of a decreased risk of crises). In both situations however, the resulting net effect on wages should be expected to be positive. Although I had argued in the introduction that the effects of coordination frictions on firm productivity might be ambiguous, since coordination frictions might positively affect firm productivity, if resulting in union paralysis and thereby decreased recourse to industrial action, there indeed appears to be no reason to expect a negative effect on firm productivity of a union's not having to coordinate with others to sign agreements. However, if coordination efforts between unions resulted in delayed but more demanding agreements, one might expect this positive effect to be only short term, followed by a negative effect.

Does alleviating the need for coordination between unions have any net effect on the average gross wage? To answer this question, I run RDDs using the residualized log of the average gross wage and the growth rate of the average gross wage as dependent variables. I report the results obtained for the score of the first and second union in tables 14 and 15 below, while the results obtained when using the score of the third union as running variable can be found in table 75 in Appendix 8.7.2..

Table 14: Effect on average wage of the first union reaching 30% of the votes

Running variable: score of the first-ranked union Cut-off: 0.3			
	(1)	(2)	(3)
	Residual log of average wage 1-yr after	Residual log of average wage 2-yrs after	Residual log of average wage 3-yrs after
VARIABLES			
RD_Estimate	0.0262	0.0309	0.128**
	(0.0334)	(0.0359)	(0.0532)
Conventional p-value	.432	.389	.016
Bandwidth	.05	.05	.05
Obs left of cut-off	118	94	58
Obs right of cut-off	343	285	178
Kernel	Triangular	Triangular	Triangular

	(4)	(5)	(6)	(7)
VARIABLES	Growth rate of average wage 1-yr before	Growth rate of average wage 1-yr after	Growth rate of average wage 2-yrs after	Growth rate of average wage 3-yrs after
RD_Estimate	-0.0435	0.0488	0.0648	0.169
	(0.0349)	(0.0553)	(0.0757)	(0.112)
Conventional p-value	.213	.378	.392	.13
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	134	118	94	58
Obs right of cut-off	357	343	285	178
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
col 1-3: residual = actual value minus predicted value
col 1-3: Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4: growth during year before election
col 5-7: growth compared to year before election
-
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for, col 5-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 15: Effect on average wage of the first union reaching 30% of the votes

Running variable: score of the second-ranked union Cut-off: 0.3			
VARIABLES	(1) Residual log of average wage 1-yr after	(2) Residual log of average wage 2-yrs after	(3) Residual log of average wage 3-yrs after
RD_Estimate	0.0194*	0.0189	0.0412**
	(0.0110)	(0.0124)	(0.0164)
Conventional p-value	.077	.127	.012
Bandwidth	.05	.05	.05
Obs left of cut-off	2089	1704	1014
Obs right of cut-off	2212	1782	1082
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Growth rate of average wage 1-yr before	(5) Growth rate of average wage 1-yr after	(6) Growth rate of average wage 2-yrs after	(7) Growth rate of average wage 3-yrs after
RD_Estimate	-0.00381	0.0130	0.0136	0.0105
	(0.00921)	(0.0145)	(0.0144)	(0.0175)
Conventional p-value	.679	.37	.346	.547
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	2169	2089	1704	1014
Obs right of cut-off	2272	2212	1782	1082
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
col 1-3: residual = actual value minus predicted value
col 1-3: Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4: growth during year before election
col 5-7: growth compared to year before election
-
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for, col 5-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

As illustrated by tables 14 and 15 , most coefficients are not significant. However, as I further detail below for the first and second union, a focus on the trends suggested by the signs and amplitude of coefficients suggests that the residual and growth specifications both concur in hinting at a positive effect on average gross wages of a first or second union reaching 30% of the votes.

Indeed, when using the score of the first or second union as a running variable, the residualized log of the average gross wage one year, two years and three years after the election is systematically larger to the right than to the left of the threshold. This is corroborated by the results obtained when focusing on growth: firms to the right of the threshold have experienced a higher average gross wage growth after the election but lower before the election. Furthermore, with both specifications, the differences between firms on opposite sides of the 30% threshold widen over time. By contrast however, as illustrated by table 75 in appendix, the two specifications suggest no difference between firms where the third-ranked union obtained slightly more than 30% of the votes, in comparison to those where they obtained slightly less.

Let us start with the first union. One and two years after the election, the part of the average gross wage which cannot be explained by a regression on its value before the election and pre-determined characteristics was 2-3% higher (corresponding to $0.11 - 0.16$ standard deviation differences once compared to the overall dispersion in the residuals used) and, three years after the election, 12.8% higher (significant at 5% and corresponding to a 0.66 standard deviation difference) in the firms where the first union obtained slightly more than 30% of the votes, as opposed to those where it obtained slightly less.

Furthermore, while during the year preceding the election, the growth rate of the average gross wage was 4.35 percentage points lower (a 0.2 standard deviation difference) in the former compared to the latter, one year after the election, the growth rate of the average gross wage – compared to one year before the election – was 4.88 percentage points higher (a 0.23 standard deviation difference) in the former. This difference in trends continued up to three years after the election, since the growth of the average gross wage, compared to the year before the election, was respectively, two years after the election, 6.48 percentage points higher (a 0.29 standard deviation difference) in the former compared to the latter and, three years after the election, 16.9 percentage points larger (a 0.7 standard deviation difference) in the former compared to the latter.

However, one may be worried that the point estimates obtained appear especially large, especially once put in perspective with the fact that the union wage premium in France is of only 2% (Breda [2015]). This is especially true of the coefficients obtained when estimating effects three years after the election. One may therefore fear that these results are affected by outliers as a result of an excessively-limited sample size for estimation of the local linear polynomial (ranging from 58 to 343 observations). This small sample size reflects the fact that such multi-unionism configurations are extreme: in 99.2% of the elections in the sample, the first union has indeed at least 30% of the votes.

The effect of a second union reaching 30% of the votes may be expected to be lower, since the decrease in the need for coordination it implies is limited by the fact that there is necessarily already one union which can sign agreements alone. However, it can be more accurately estimated as situations where the second union has between 25 and 35% of the votes are approximately twenty times more frequent.

The obtained point estimates similarly suggest a positive net effect on wages of a second union obtaining 30% of the votes, although the estimates are of a lower amplitude than those obtained for the first union. The part of the average gross wage which cannot be explained by pre-determined characteristics was respectively 1.94 %, 1.89% and 4.12% (significant at 5%) higher one, two and three years after the election (respectively corresponding to 0.11, 0.1 and 0.21 standard deviation differences when compared to the overall dispersion of the residuals) in the firms where the second union obtained slightly more than 30% of the votes as opposed to those where it obtained slightly less.

The differences in growth rates on both sides of the threshold tell a similar story, although they may suggest that the positive effect on the growth rate might have resorbed three years after the election. Indeed, the difference in trends between the firms on opposite sides of the threshold was negative but close to null (corresponding to a 0.02 satndard deviation difference) the year before the election. By contrast, one, two and three years after the election, the average gross wage had grown by respectively 1.3, 1.36 and 1.05 percentage points more (respectively 0.06, 0.06 and 0.04 standard deviation differences) compared to the year before the election in the firms where the second union obtained slightly more than 30% of the votes, as opposed to those where it obtained slightly less.

These results suggest that, as one would have expected in the presence of coordination frictions between unions, enabling firms to sign agreements without having to form a coalition, and thereby alleviating the need for coordination in the bargaining process, eventually results in higher gross wages.

The absence of effect when the third-ranked union reaches 30% of the votes, in contrast to the first and second union, combined with the apparent larger effects when the first rather than the second union reaches 30% of the votes would suggest that, the larger the number of union with the possibility of signing agreements alone, the lesser the marginal effect of an additional union with the possibility of signing agreements alone as coordination frictions have already been alleviated.

As previously mentioned, one could think that coordination frictions might manifest themselves by delayed agreements, but eventually agreements of possibly greater benefit to the workers. Evidence in this respect appears more mixed.

On the one hand, the increasingly positive gaps found in the case of the first union would seem to contradict this assumption. When using the score of the second union as a running variable, the large and significant at 5% coefficient found when using the residualized log of the average wage three years after the election would seem further evidence against this hypothesis.

On the other hand however, the large amplitude of the coefficients obtained three years after the election when using the score of the first union as a running variable might be thought to be attributable to outliers in a small sample context. Furthermore, the more precisely-estimated coefficients obtained when using the score of the second union as a running variable appear to suggest a reversal of trend three years after the election.

6.1.2.2 Decomposition: positive or null effects on the share of wages in the value-added and productivity

While these results suggest a positive net effect on wages of alleviating the need for coordination between unions, this effect could theoretically be driven by both a positive effect on the share of wages in the value-added or a positive effect on the value-added by worker: as argued in the introduction, coordination frictions between unions may negatively affect the two "faces" of unions, by making representation of workers' interests more difficult but also making crises more likely.

To investigate which channel seems to drive this apparent positive net effect on wages, I therefore realize additional RDDs. I use as dependent variables the residualized log of the share of wages in the value-added, the residualized log of the value-added by worker, the growth rate of the share of wages in the value-added, that of the value-added by worker, as well as increases in the value-added by worker. I report the results obtained when using the scores of the first and second union as running variables in Tables 16 and 17 below, and those obtained when using the score of the third union as running variable in table 76 in Appendix 8.7.2..

Let us start with the share of wages in the value-added: does decreasing the need for coordination between unions appear to result in better representation of workers' interests, positively affecting the share of wages in the value-added?

Table 16: Effect on unexplained share of wages in the value-added of first or second union reaching 30% of the votes

	Cut-off: 0.3		
	(1)	(2)	(3)
	Residual	Residual	Residual
	log of share of wages	log of share of wages	log of share of wages
	in value-added	in value-added	in value-added
VARIABLES	1-yr after	2-yrs after	3-yrs after
	Running variable: score of the first-ranked union		
RD_Estimate	0.0312	0.0172	0.0422
	(0.0397)	(0.0570)	(0.0532)
Conventional p-value	.432	.763	.428
Obs left of cut-off	120	98	62
Obs right of cut-off	352	296	198
	Running variable: score of the second-ranked union		
RD_Estimate	0.0206	-0.00657	0.0279
	(0.0151)	(0.0161)	(0.0226)
Conventional p-value	.172	.684	.216
Obs left of cut-off	2155	1805	1147
Obs right of cut-off	2245	1894	1227
Bandwidth	.05	.05	.05
Kernel	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: residual = actual value minus predicted value
Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
Outliers, CGC, pre-electoral alliances excluded, donut
Log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

As illustrated by Table 16, using as dependent variable the residual of the log of the share of wages in the value-added might suggest a globally positive or null effect of the first and second unions reaching 30% of the votes on the share of wages in the value-added.

However, two years after the election, this effect seems null when using the score of the second union as a running variable: firms where the second union obtained more than 30% of the votes were characterized by a lower share of wages in the value-added (conditional on pre-determined characteristics) but the coefficient obtained corresponds to a difference of the order of 0.003 standard deviations. Similarly, the coefficient found two years after the election when using the score of the first union as a running variable is of lower magnitude than the coefficients obtained one and three years after the election (once compared to the dispersion in the residuals, it corresponds to 0.07 standard deviations, by contrast to 0.13 and 0.16 standard deviations one and three years after the election). This could possibly explain the lower coefficients found two years after the election in the case of the second union.

Although the residualized specifications would thus point to a mainly positive effect on the share of wages in the value-added, the evidence is mixed: no coefficient is significant, and one of the coefficients appears null, suggesting a temporary reversal of trend.

Let us now turn to the differences in the actual trends of the share of wages in the value-added on opposite sides of the threshold.

Table 17: Effect on growth of share of wages in value-added of first or second union reaching 30% of the votes

Cut-off: 0.3				
	(1)	(2)	(3)	(4)
VARIABLES	Growth rate of share of wages in value-added 1-yr before	Growth rate of share of wages in value-added 1-yr after	Growth rate of share of wages in value-added 2-yrs after	Growth rate of share of wages in value-added 3-yrs after
Running variable: score of the first-ranked union				
RD_Estimate	-0.0187	0.0104	-0.0229	0.0533
	(0.0385)	(0.0386)	(0.0783)	(0.0703)
Conventional p-value	.627	.788	.77	.448
Obs left of cut-off	133	120	98	63
Obs right of cut-off	354	355	299	201
Running variable: score of the second-ranked union				
RD_Estimate	0.0181	0.0276	-0.00943	0.0426
	(0.0159)	(0.0199)	(0.0180)	(0.0260)
Conventional p-value	.257	.166	.6	.101
Obs left of cut-off	2160	2165	1813	1155
Obs right of cut-off	2260	2256	1902	1236
Bandwidth	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: col1: growth during year before election
col 2-4: growth compared to year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017)

As illustrated by Table 17, using the growth of the share of wages in value-added since the year before the election confirms this temporary reversal of trends two years after the election[54]: the coefficients obtained two years after the election are negative. This implies that, while one year after the election, the share of wages in the value-added had increased by more in firms where the first or second union obtained slightly more as opposed to slightly less than 30% of the votes, two years after the election, the latter firms had more than caught up on the former but, one year later, a new reversal of trend occurred, in such a way that, compared to before the election, the share of wages in the value-added had increased by about $4-5$ percentage points more in the former firms.

Since the reversal of trend two years after the election appears to be solely temporary, it cannot

[54] Tables 78 and 79 in Appendix 8.7.2.1. highlight that this pattern persists when restricting the analysis to the firms with a mandate of four years in such a way that, conditional on data availability, the same population is observed one, two and three years after the election.

be reconciled with a story of coordination frictions resulting in better long-term outcomes. These results therefore shed doubts on the idea that the apparent positive net effects on wages originate in a positive effects on the share of wages in value-added, as a result of easier representation of the workers by unions.

Putting the results from the growth specification in perspective with the sign and amplitude of the coefficients obtained when using the growth of the share of wages in the value-added before the election as dependent variable may further shed doubt on whether the coefficients can be interpreted as signalling a positive effect on the share of wages in the value-added[55].

Indeed, on the one hand, the coefficient obtained when comparing firms where the first union obtained slightly more than 30% of the votes as opposed to slightly less is negative, suggesting a possible reversal of trend after the election. By contrast however, the coefficient obtained when comparing firms where the second union obtained slightly more than 30% of the votes as opposed to slightly less is positive, thus suggesting that the positive trend suggested by the coefficients may reflect a pre-existing difference in trends rather than a positive treatment effect on the share of wages in the value-added. This interpretation may seem reinforced by the fact that the amplitude of the coefficients found when considering the growth of the share of wages in the value-added after the election is comparable to the amplitude of the coefficients obtained when using the growth of the share of wages in the value-added before the election (before the election, the differences correspond to 0.06 standard deviations, while one and two years after the election, they range from differences of the order of 0.02 to 0.08 standard deviation ; they are however larger, being of the order of 0.1-0.13 standard deviation three years after the election).

All things considered, the evidence in favour of a positive effect on the share of wages in the value-added as the driver of the apparent positive effect on wages of an additional union capable of signing agreements alone therefore appears mixed.

Should the positive effect on wages suggested by tables 14 and 15 therefore be attributed to a positive effect on firm productivity? A positive effect could be explained by a decrease in conflicts, resulting in either less strikes or work interruptions, or an improved social climate resulting in increased productivity, as the agreement-signing process is made easier.

Results obtained when using the residualized log of the value-added by worker as well as the growth rates and increases of the value-added by worker are displayed in Tables 18 and 19 below.

[55] The lack of interpretability of this possible positive effect on the share of wages in the value-added as the driver of the positive net effect on wages may seem further reinforced by comparison of the above tables 16 and 17 to table 76 in Appendix 8.7.2.. Indeed, the coefficients obtained when using the score of the first or second union as running variables are similar to those obtained when using the score of the third union as running variable, while in the latter case, no positive effect on wages is detected.

However, the absence of a net positive effect on wages of the third union gaining the possibility of signing agreements alone can be reconciled with an apparent positive effect on the share of wages in the value-added by comparing the value-added by worker and its evolution on both sides of the threshold. Indeed, the residualized specification in table 77 in Appendix highlights that the residualized log of the value-added by worker one, two and three years after the election was systematically lower in the firms where the third union obtained slightly more than 30% of the votes as opposed to slightly less. While the growth specification in table 77 in Appendix confirms that, after the election, the value-added by worker systematically increased by less in the firms where the third union obtained more than 30% of the votes compared to those where it obtained slightly less, it also suggests that this reflected pre-existing non-parallel trends, since the growth rate of the value-added by worker was slightly more negative in the former firms the year before the election (although the fact that the increase in the value-added by worker itself was more positive in the former firms may shed doubts on the sign of the difference in trends before the election).

Table 18: Effect on value-added by worker of first union reaching 30% of the votes

Running variable: score of the first-ranked union
Cut-off: 0.3

VARIABLES	(1) Residual log of value-added by worker 1-yr after	(2) Residual log of value-added by worker 2-yrs after	(3) Residual log of value-added by worker 3-yrs after
RD_Estimate	0.00965	-0.00201	0.328
	(0.0500)	(0.0659)	(0.303)
Conventional p-value	.847	.976	.28
Bandwidth	.05	.05	.05
Obs left of cut-off	118	94	59
Obs right of cut-off	338	282	177
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Increase in value-added by worker 1-yr before	(5) Growth rate of value-added by worker 1-yr before	(6) Increase in value-added by worker 1-yr after	(7) Growth rate of value-added by worker 1-yr after
RD_Estimate	0.117	0.0191	-2.528	0.0479
	(3.995)	(0.0475)	(4.480)	(0.0716)
Conventional p-value	.977	.687	.573	.504
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	134	133	119	118
Obs right of cut-off	357	352	343	338
Kernel	Triangular	Triangular	Triangular	Triangular

VARIABLES	(8) Increase in value-added by worker 2-yrs after	(9) Growth rate of value-added by worker 2-yrs after	(10) Increase in value-added by worker 3-yrs after	(11) Growth rate of value-added by worker 3-yrs after
RD_Estimate	2.341	0.0331	8.961	0.192
	(4.504)	(0.0936)	(6.269)	(0.132)
Conventional p-value	.603	.724	.153	.147
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	95	94	60	59
Obs right of cut-off	287	282	180	177
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables:
col 1-3: residual = actual value minus predicted value,
predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4-5: increase/growth during year before election
col 6-11: increase/growth compared to year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for
col 4-11: year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 19: Effect on value-added by worker of second union reaching 30% of the votes

Value-added by worker
Running variable: score of the second-ranked union
Cut-off: 0.3

VARIABLES	(1) Residual log of value-added by worker 1-yr after	(2) Residual log of value-added by worker 2-yrs after	(3) Residual log of value-added by worker 3-yrs after
RD_Estimate	-0.00189	0.0226	0.0379
	(0.0176)	(0.0200)	(0.0341)
Conventional p-value	.915	.258	.266
Bandwidth	.05	.05	.05
Obs left of cut-off	2078	1687	1010
Obs right of cut-off	2185	1770	1074
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Increase in value-added by worker 1-yr before	(5) Growth rate of value-added by worker 1-yr before	(6) Increase in value-added by worker 1-yr after	(7) Growth rate of value-added by worker 1-yr after
RD_Estimate	-0.144	-0.00820	0.410	0.00193
	(1.157)	(0.0145)	(1.377)	(0.0226)
Conventional p-value	.901	.571	.766	.932
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	2180	2158	2105	2078
Obs right of cut-off	2284	2255	2230	2185
Kernel	Triangular	Triangular	Triangular	Triangular

VARIABLES	(8) Increase in value-added by worker 2-yrs after	(9) Growth rate of value-added by worker 2-yrs after	(10) Increase in value-added by worker 3-yrs after	(11) Growth rate of value-added by worker 3-yrs after
RD_Estimate	1.157	0.0246	0.952	0.000235
	(1.673)	(0.0272)	(2.254)	(0.0402)
Conventional p-value	.489	.364	.673	.995
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	1713	1687	1022	1010
Obs right of cut-off	1798	1770	1093	1074
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm leel in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables:
col 1-3: residual = actual value minus predicted value,
predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4-5: increase/growth during year before election
col 6-11: increase/growth compared to year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for
col 4-11: year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

The evidence in favour of a positive effect on firm productivity as a driver of the apparent positive effect on wages appears similarly mixed.

The residualized specifications would indeed suggest an initially null or slightly positive effect of both the first and the second union obtaining 30% of the votes (respectively of the order of 0.03 and 0.009 standard deviations one and two years after the election for the first union, and of the order of 0.006 standard deviations one year after the election for the second union), followed by a positive effect respectively three and two years after the election (respectively of the order of 1.312 standard deviation for the first union, and of the order of 0.07 and 0.11 standard deviations two and three years after the election for the second union).

Does putting this in perspective with the results obtained when considering differences in actual trends yield clearer evidence?

Doing so highlights that a more positive trend pre-existed (albeit the difference in increases found before the election is of a lower of magnitude - in absolute terms and once compared to the dispersion of the changes in value-added - than the differences found after the election: it is of the order of 0.005 standard deviation, by opposition to differences ranging from 0.09 to 0.33 standard deviations after the election) in the firms where the first union obtained slightly more than 30% of the votes as opposed to slightly less, shedding doubt on whether the positive effect found should be attributed to the attenuation of coordination frictions.

In the case of the second union, however, the point estimates seem to indicate a reversal of trend. Indeed, prior to the election, the growth of the value-added by worker had been lower in the firms where the second union obtained slightly more than 30% of the votes as opposed to those where it obtained slightly less. After the election, the sign of the difference in trends reversed. The amplitude of the coefficients however suggests that this reversal of trend is unexceptional, shedding doubts on whether it should be attributed to the treatment. Indeed, the amplitude of the coefficients obtained for growth rates is lower after the election compared to before the election in two out of three cases (being of the order of 0.1-0.2 percentage points compared to 0.8 percentage points). However, when using increases in the value-added by worker rather than growth rates as dependent variables, the amplitude of the coefficients obtained after the election is larger than that before the election (corresponding, before the election, to a difference of 0.007 standard deviations, by contrast to differences of the order of 0.017 to 0.046 standard deviations after the election).

Thus, evidence in favour of a positive effect on the value-added as the driver of the positive effect on wages appears similarly mixed as the evidence in favour of a positive effect on the share of wages in the value-added.

These last results may however reinforce the interpretation of the positive coefficients found when using measures of the share of wages in the value-added as reflecting increased representation efforts by unions. Indeed, in a context of increasing productivity and sticky wages, one would expect a mechanical negative effect on the share of wages in the value-added, contradicting the results in tables 16 and 17.

Accordingly, by comparing the sign of the coefficients obtained when using respectively the growth rate of the share of the wages in the value-added and the growth rate of the value-added by worker before the election as dependent variables, one can observe that the resulting signs are always opposed before the election, possibly reflecting a context of wage stickiness[56]. By contrast, after the election, the coefficients are conversely of same sign in four out of six cases[57].

Thus, although the evidence in favour of a positive effect on the share of wages in the value-added might not seem particularly compelling when considered alone, interpreting the coefficients in tables 16 and 17 in the light of those in 18 and 19 hints at a positive effect on the share of wages

[56] The results obtained when using the score of the third union as a running variable reported in tables 75 to 77 in Appendix similarly seem to reflect a context of wage stickiness: the measured net effects on wages appear null, but there seems to be a pre-existing more positive trend in productivity to the right of the threshold, which is systematically matched by a more negative trend in the share of wages in value-added, resulting in null net effects on wages.

[57] The signs are only opposed two years after the election, reflecting the dip in the coefficients obtained for the share of wages in the value-added two years after the election.

in the value-added which cannot be explained by value-added fluctuations alone.

6.1.2.3 Recap: an apparent positive net effect on wages but no clear source

To recap, both the residual and growth specifications appear to indicate a positive net effect on the average gross wage of the first or second union obtaining 30% of the votes, but null effect of the third union obtaining 30% of the votes. This suggests that easing the need for coordination between unions eventually benefits workers.

However, separate analysis of the effects on the share of wages in the value-added does not permit clearly decomposing this effect between effects on either variables. Indeed, suggestive evidence would suggest a mainly positive (or null) effect on both variables.

Notably, however, hints of a positive effect on the value-added by worker makes a positive effect on the share of wages in the value added more remarkable: it suggests that the latter effect is not solely the mechanical result of random value-added fluctuations in a context of wage stickiness but might reflect increased bargaining gains by trade-unions.

6.1.2.4 A win-win situations? Economic profitability

Under the possible simultaneous presence of positive effects on both the share of wages in the value-added and the value-added by worker, the effect on economic profitability is a priori ambiguous. One would indeed expect a positive effect if the positive effect on the share of wages in the value-added were low enough, but a negative one if the wage bill increased by more than the value-added.

Thus, while evidence suggests that workers gain from an additional union gaining the possibility of signing agreements, the question of whether this reflects a zero-sum game or whether the firm also gains from this configuration remains open. Indeed, as mentioned in introduction, decreasing the need for coordination between unions might benefit the firm, illustrating the "second face" of unions. If decreased tensions result in decreased strikes or work interruptions, or more simply better working relations and motivation, this could result in increased economic profitability if the resulting increase in productivity were sufficient to compensate the increase in the wage bill bargained.

Although the absence of strong evidence in tables 18 and 19 in favour of a positive effect on the value-added by worker suggests that this does not seem to be the case, I further investigated this hypotheses by using the residualized log of economic profitability and changes in economic profitability as dependent variables.

The detailed results, which can be found in tables 80 and 81 in Appendix highlight that evidence in favour of a positive or negative effect for the firm seems weaker than evidence in favour of a positive effect for the workers.

The signs obtained with the residualized specification would suggest an initially negative, followed by a positive effect (three years after the election) of the first or second union obtaining 30% of the votes. This could be interpreted as suggesting that when unions can sign agreements with greater ease, the initial gain perceived by workers is immediate, and they therefore initially gain at the expense of the firm, but this is followed by positive repercussions on the value-added. Such positive repercussions might result from decreased conflicts or improved social climate eventually benefiting the firm.

Actual trends differences in trends however tell different stories from the residualized specifications.

In the case of the second union, the signs and amplitude of the coefficients clearly suggest a negative trend: before the election, the trend in economic profitability was slightly more positive to the right than two the left of the threshold, but after the election, the signs reversed, and the magnitude of the coefficients amplified over the years. The fact that the coefficients obtained before the election are lower in absolute value than those obtained after the election further reinforces the hypothesis of a negative effect.

In the case of the first union, the picture is more ambiguous. Trusting the signs obtained when using growth rates would indeed suggest, contradicting the results of the residualized specification, an initially positive effect (two and three years after the election) followed by a negative effect. However, since, when using growth rates, firms with a negative economic profitability before or after the election are automatically excluded, resulting in a selection bias, this should be put in perspective with the results obtained when using inceases in economic profitability as dependent variables. The latter would approximately corroborate the results obtained with the residualized specification, since they suggest an initially negative (one year after the election) effect followed by positive effect (two and three years after the election, with an increase of 0.299 significant at 5% three years after the election). However, the very large magnitude of the coefficient obtained three years after the election might hint at a small sample bias, whereby the limited number of observations available (67 to the left of the threshold and 204 to the right) leads to results affected by outliers.

Hence, one considering the different specifications used, it does not seem possible to infer a positive or negative (even delayed) effect on a firm's economic profitability of a first or second union reaching 30% of the votes. This suggests that the workers are the main beneficiaries of the decrease in the need for coordination between unions.

6.1.2.5 Robustness check: a positive net effect on wages in question

Since the exact source of the detected positive net effect on wages does not appear clearly, and since magnitude of the point estimates obtained for the first union suggests spurious results affected by outliers, I realized a robustness check to verify whether the evidence in favour of a positive net effect on wages was robust to alternative bandwidth choices.

I thus ran the same RDDs anew, with a bandwidth of 0.02, to verify that the results were not driven by non-linearities in a too large bandwidth of 0.05. The results can be found in tables 82 and 83 in Appendix.

Overall, the evidence in favour of a positive net effect on wages is less obvious when using this bandwidth.

When using the score of the first union as running variable, while the point estimates still suggests a positive effect three years after the election, inference cannot be conducted as a result of a lack of observations (twenty-eight observations to the left of the threshold). When considering previous years, the overall evidence appears more mixed: both the residual and growth specifications point to a null net effect on wages one year after the election and negative two years after the election[58].

When using the score of the second union as running variable, the residualized specification indicates a substantial positive effect on the average gross wage three years after the election (a 5% increase – significant at 5%). One and two years after the election, the coefficients are of lower - and possibly more plausible - amplitude, albeit similarly suggesting positive effects (respectively corresponding to increases in the average gross wage of the order of 0.581% and 0.735%). By contrast, the growth specification however suggests a negative or null effect one, two, but also three years after the election (respectively a growth lower by 0.0707 percentage points one year after the election, lower by 0.583 percentage points two years after the election and only higher by 0.0183 percentage points three years after the election, by contrast to a growth higher by 0.022 percentage points one year before the election).

While this sheds doubt on the previous finding of a positive net effect of the second union reaching 30% of the votes, this does not entirely contradicts previous findings and interpretations.

[58] While this could be thought to parallel the previously-found negative effect of the second or third union obtaining 30% of the votes on the share of wages in the value-added two years after the election, using as dependent variables the residualized log of the share of wages in the value-added and growth rate of the share of wages in the value-added does not evidence a corresponding negative effect. Using the residualized log of the value-added by worker and growth or increase of the value-added by worker would rather suggest that this is attributable to a negative effect on the value-added by worker two years after the election. However, the results obtained when considering changes in value-added contradict this result as the coefficients obtained are positive.

Indeed, the previously-found coefficients were small, possibly reflecting the fact that the marginal effect of the second union reaching 30% of the votes in terms of reducing the need for coordination should be expected to be lower than the marginal effect of the first union reaching the same score. Although by contrast, one would expect to find stronger effects of the first union reaching 30% of the votes, situations where the first union has around 30% of the votes are very rare, resulting in a low statistical power.

6.1.3 Recourse to veto right? 50% threshold

Section 6.1.2. has suggested the possible presence of mild coordination frictions between unions, in such a way that enabling a union to sign agreements without having to coordinate with other unions seems to rapidly benefit workers and might eventually benefit the firm. However, since the evidence was only suggestive and is not robust to choosing a narrower bandwidth, the question of whether multi-union configurations are indeed affected by coordination frictions between unions remains open.

Alternatively, coordination frictions may materialize if, when given the possibility to do so, a union vetoes an agreement signed by other unions. This would result in a negative effect on the share of wages in the value-added, albeit possibly followed by a positive effect if such coordination processes resulted in delayed agreements eventually more beneficial to the workers.

Since, by obtaining 50% of the votes, a union gains the possibility of vetoing agreements alone, to further investigate the question of coordination frictions, I therefore run RDDs using a threshold of 50% and using the score of the first union as running variable.

6.1.3.1 Negative or null net effect on wages

I report in table 20 below the results obtained when using the residualized log of the average gross wage and the growth rate of the average gross wage as dependent variables.

Table 20: Effect on average wage of first union reaching 50% of the votes

Running variable: score of the first-ranked union
Cut-off: 0.5

VARIABLES	(1) Residual log of average wage 1-yr after	(2) Residual log of average wage 2-yrs after	(3) Residual log of average wage 3-yrs after
RD_Estimate	-0.00141	-0.00199	-0.00608
	(0.0169)	(0.0196)	(0.0261)
Conventional p-value	.934	.919	.816
Bandwidth	.05	.05	.05
Obs left of cut-off	735	605	342
Obs right of cut-off	1571	1272	765
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Growth rate of average wage 1-yr before	(5) Growth rate of average wage 1-yr after	(6) Growth rate of average wage 2-yrs after	(7) Growth rate of average wage 3-yrs after
RD_Estimate	0.00958	-0.0112	-0.0141	0.00879
	(0.0143)	(0.0202)	(0.0190)	(0.0223)
Conventional p-value	.503	.579	.457	.693
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	764	735	605	342
Obs right of cut-off	1622	1571	1272	765
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
col 1-3: residual = actual value minus predicted value
col 1-3: Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4: growth during year before election
col 5-7: growth compared to year before election
-
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for, col 5-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

The sign of the coefficients obtained with the residualized specification appear to suggest a negative net effect on wages of the first union obtaining the possibility of vetoing agreement. However, the estimated effects are close to null (of the order of 0.1 to 0.6%, corresponding to differences of a magnitude of 0.008, 0.011 and 0.032 standard deviations once compared to the overall dispersion of the unexplained log of average wage after the election), resulting in p-values systematically superior to 0.8 despite the relatively large sample size (from several hundreds to more than one thousands of observations depending on the side of the threshold considered).

Using the growth rate of the average gross wage nonetheless results in coefficients of a slightly larger amplitude, suggesting an initially negative effect one and two years after the election (of the order of 1 percentage point) but a positive effect three years after the election, thus hinting at a possible reversal of a negative trend (contradicting the apparent deepening of a negative trend suggested by the coefficient obtained in column 3). However, the amplitude of the estimated effects is hardly superior to that obtained when using the growth rate before the election (0.958 percentage points) as a dependent variable. Furthermore, the coefficients found before and after the election have a magnitude which systematically ranges between 0.04 and 0.06 standard deviation. The evidence therefore converges in sugesting that the differences between firms on opposite sides of the threshold may be attributable to random economic fluctuations rather than to the first union obtaining the possibility to veto agreements.

Thus, although, as one would expect in the presence of coordination frictions, the signs of the coefficients would suggest an initially negative (possibly followed by a reversal of trend three years after the election) net effect on wages, the limited amplitude of the coefficients suggests that this effect is either null or very small.

6.1.3.2 Decomposition: a zero-sum game between employer and workers?

To further investigate whether these results can be interpreted as signalling coordination frictions, I therefore run additional RDDs, decomposing these effects between effects on the share of the value-added received by the workers and effects on the value-added by worker.

The results obtained for the share of wages in the value-added which are detailed in table 21 below a priori seem to corroborate the coordination frictions story.

Table 21: Effect on share of wages in value-added of first union reaching 50% of the votes

Running variable: score of the first-ranked union Cut-off: 0.5			
	(1)	(2)	(3)
	Residual log of share of wages in value-added	Residual log of share of wages in value-added	Residual log of share of wages in value-added
VARIABLES	1-yr after	2-yrs after	3-yrs after
RD_Estimate	-0.0374	-0.0423	-0.0790*
	(0.0259)	(0.0335)	(0.0463)
Conventional p-value	.149	.206	.088
Bandwidth	.05	.05	.05
Obs left of cut-off	756	643	390
Obs right of cut-off	1615	1366	867
Kernel	Triangular	Triangular	Triangular

	(4)	(5)	(6)	(7)
	Growth rate of share of wages in value-added	Growth rate of share of wages in value-added	Growth rate of share of wages in value-added	Growth rate of share of wages in value-added
VARIABLES	1-yr before	1-yr after	2-yrs after	3-yrs after
RD_Estimate	-0.00203	-0.0330	-0.0501	-0.135*
	(0.0258)	(0.0304)	(0.0388)	(0.0807)
Conventional p-value	.937	.278	.196	.095
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	763	760	645	392
Obs right of cut-off	1615	1620	1369	871
Kernel	Triangular	Triangular	Triangular	Triangular

Standard clustered at the firm level errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables:
col 1-3: residual = actual value minus predicted value,
predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col4: growth during year before election
col 5-7: growth compared to year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for
col 4-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017)

Upon first inspection, both the residualized and growth specifications indeed concur in suggesting a negative effect of the first union reaching 50% of the votes on wages.

The residualized log of the share of wages in the value-added, one, two and three years after the election is indeed systematically lower in firms where the first union obtained slightly more than 50% of the votes as opposed to slightly less. The difference appears to amplify over time[59],

[59]This however contradicts the hypothesis that the coordination process eventually results in an agreement more favourable to the workers being signed after the first agreement was vetoed.

until reaching minus 7.9% (significant at 10%) three years after the election. Furthermore, when compared to the overall distribution of the residuals, this corresponds to magnitudes of respectively 0.17, 0.18 and 0.316 standard deviations (one, two and three years after the election).

The growth specification corroborates this negative trend. Point estimates indicate that, respectively one, two and three years after the election, the growth of the share of wages in the value-added compared to the year before the election was lower by 3.3, 5.01 and 13.5 (significant at 10%) percentage points in the former firms compared to the latter (corresponding to 0.1, 0.135 and 0.329 standard deviations differences in growth).

However, one should observe that the growth rate of the share of wages in the value-added before the election was already lower by 0.203 percentage points in the former firms the year before the election. The amplitude of this pre-electoral difference is lower than the amplitude of the differences found after the election (both in absolute and relative terms: before the election, the coefficient found indeed corresponds to a 0.008 standard deviation difference in the growth of the share of wages in the value-added). Nonetheless, one could think that the deepening difference in trends suggested by the growing amplitude of the coefficients reflects pre-existing non-parallel trends.

The results found when considering effects on the value-added by worker, which I display in table 22 below, a priori seem to reinforce this last interpretation.

Indeed, the more negative trend in the share of wages in the value-added to the right of the threshold seems to be mechanically explained by a pre-existing more positive trend in the value-added by workers in firms where the first union obtained slightly more than 50% of the votes as opposed to slightly less.

Table 22: Effect on value-added by worker of first union reaching 50% of the votes

Running variable: score of the first-ranked union Cut-off: 0.5			
VARIABLES	(1) Residual log of value-added by worker 1-yr after	(2) Residual log of value-added by worker 2-yrs after	(3) Residual log of value-added by worker 3-yrs after
RD_Estimate	0.0293 (0.0301)	0.0428 (0.0390)	0.0888 (0.0590)
Conventional p-value	.332	.272	.132
Bandwidth	.05	.05	.05
Obs left of cut-off	730	604	342
Obs right of cut-off	1561	1263	760
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Increase in value-added by worker 1-yr before	(5) Growth rate of value-added by worker 1-yr before	(6) Increase in value-added by worker 1-yr after	(7) Growth rate of value-added by worker 1-yr after
RD_Estimate	1.660 (1.796)	0.0285 (0.0273)	1.569 (2.218)	0.0245 (0.0361)
Conventional p-value	.355	.298	.48	.497
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	766	760	741	730
Obs right of cut-off	1630	1612	1584	1561
Kernel	Triangular	Triangular	Triangular	Triangular

VARIABLES	(8) Increase in value-added by worker 2-yrs after	(9) Growth rate of value-added by worker 2-yrs after	(10) Increase in value-added by worker 3-yrs after	(11) Growth rate of value-added by worker 3-yrs after
RD_Estimate	0.809 (3.038)	0.0111 (0.0485)	2.850 (4.564)	0.110* (0.0642)
Conventional p-value	.79	.819	.532	.087
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	610	604	348	342
Obs right of cut-off	1278	1263	772	760
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables:
col 1-3: residual = actual value minus predicted value,
predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4-5: increase/growth during year before election
col 6-11: increase/growth compared to year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for
col 4-11: year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

One, two and three years after the election, the residualized log of the value-added by worker was systematically larger (corresponding to differences in value-added by worker of the order of 2.93 to 8.88%) in the firms where the first union obtained more than 50% of the votes, with the amplitude of the difference increasing over time (both in absolute and relative terms: when compared to the overall dispersion of the residuals one, two and three years after the election, this corresponds to differences of a magnitude of 0.1, 0.138 and 0.26 standard deviations).

In a context of sticky wages, an increase in the value-added by worker would be mechanically associated with a decrease in the share of wages in the value-added. This seems to be corroborated by the different evolutions of the economic profitability in firms to the right and to the left of the cut-off, as illustrated by table 84 in Appendix[60]: the growth in economic profitability after the election has been more positive in the firms to the right of the threshold than firms to the left of the threshold.

One can doubt that this positive "effect" on firm productivity is causal, however. Indeed, using the growth rate or the increase in the value-added by worker as a dependent variable suggests that this reflects pre-existing non-parallel trends. Before the election, the firms where the first union obtained slightly more than 50% of the votes were already characterized by a faster-growing value-added by worker, compared to those where it obtained slightly less than 50% of the votes. Furthermore, the amplitude of the coefficients found after the election is unexceptional compared to that of the coefficients found before the election: once compared to the overall dispersion in the changes in value-added, one year before the election, the difference in increases in value-added corresponded to 0.083 standard deviations while, one, two and three years after the election, it ranged from 0.032 to 0.106 standard deviations.

6.1.3.3 Recap: a positive effect on value-added reflecting pre-existing trends but a distinct causal effect on the share of wages in the value-added?

To recap, upon first inspection, a union's obtaining the possibility to veto agreements signed by other unions seems to benefit the firm, possibly at the expense of the workers.

However, the amplitude of the estimated effects on the average gross wage is small, suggesting that the result might be spurious. Nonetheless, the amplitude of the coefficients found when estimating effects on the share of wages in the value-added and on the value-added by worker is larger, once compared to the overall dispersion of the data. This could be interpreted as suggesting that contradictory effects on the share of wages in the value-added and on firm productivity offset each other, resulting in a quasi-null net effect on wages.

However, the negative effect on the share of wages in the value-added found might be thought to reflect the mechanical response to an increase in firm productivity in a context of wage stickiness. Consideration of pre-electoral trends suggests that the larger increases in firm productivity to the right of the threshold are not causal, since a difference pre-existed and was of similar magnitude once compared to the overall dispersion of the data. This also echoes the results from the balance tests carried out in section 5.3.2.: Table 10 indicates that, even after excluding outliers, pre-electoral alliances and elections where the running variable takes the cut-off value, the predicted log of economic profitability was higher one, two and three years after the election (by $10-11\%$), two of the obtained coefficients being significant, respectively at 1 and 5%.[61].

Nonetheless, although different trends similarly pre-existed with respect to the share of wages in the value-added, the magnitude of the pre-electoral difference was lower than that of the post-electoral differences once compared to the overall dispersion of the data. By contrast, if the negative effect on the share of wages in the value-added were solely the mechanical consequence of an increase in firm productivity in a context of sticky wages, one would expect the negative effect on the share of wages in value-added to be immediate and possibly attenuate over time as

[60] The residual specification conversely suggests a *lower* unexplained log economic profitability in the firms to the right of the cut-off, as opposed to the firms to the left of the cut-off, resulting in inconsistent findings: since both specifications however concur in finding no net effect on wages, a negative effect on the share of wages in the value-added and a positive effect on the value-added, which mechanically should translate in a positive effect on the economic profitability.

[61] This imbalance explains the contradictory results found with respect to economic profitability when using residualized and growth specifications.

wage increases are bargained. Hence, the fact that, once compared to the overall dispersion of the data, the pre-electoral difference in the growth of the share of wages in the value-added was lower by more than one order of magnitude than the post-electoral differences, suggests that the post-electoral differences might not be entirely attributable to pre-existing imbalances in firm productivity growth.

Since the presence of pre-existing imbalances sheds doubt on whether the post-electoral differences should be interpreted causally, I carried out a robustness check by conducting the analysis within a bandwidth of 0.02 around the threshold. The results, which can be found in tables 85 to 88 in Appendix, highlight that most results (with respect to the share of wages in the value-added, the value-added by worker and economic profitability) are robust to the bandwidth chosen. While the estimated net effect on the average wage was negative or null when using a bandwidth of 0.05, with a bandwidth of 0.02, the residualized specifications would suggest a positive effect of larger magnitude, although the results from the growth specification are more ambiguous. Interestingly, one should observe that the order of magnitude of the pre-existing differences in trends with respect to firm productivity and the share of wages in the value-added appear robust to the bandwidth chosen.

However, since the tests of the identification assumption carried out in section 5.3.2. suggested more imbalances around the 50% threshold than other thresholds, one may wonder whether this might not reflect systematic sorting around the 50% threshold rather than the causal effect of the first union obtaining more than 50% of the votes.

6.1.4 Recap

To investigate the effects of multi-unionism on wages, I first compared firms where the second, third, fourth or fifth union obtained slightly more than 10% of the votes as opposed to slightly less, thereby gaining the possibility of taking part in bargaining. Despite residualizing the dependent variables and considering changes in the dependent variables, this does not evidence any effect of an additional union on wages, nor on the share of wages in the value-added or firm productivity.

The lack of statistical significance is unsurprising, since the minimum detectable effects tend to be large. However, further inspection of the trends and comparison of the magnitude of the coefficients when considering post-electoral outcomes to those obtained when considering pre-electoral outcomes suggests an absence of effect.

Nonetheless, one might think that this could be the consequence of opposite effects of increased coordination frictions and increased representation efforts. Alternatively, this could also be attributable to heterogeneous effects, whereby the effects of an additional union, whether in terms of increased accountability or increased coordination frictions, are very limited, compared to the average effect of an additional union.

To explore these possibilities, I decided to focus on the question of coordination frictions, exploiting the 30% and 50% thresholds for signing or vetoing agreements alone.

Upon first inspection, the results would hint at a positive effect on wages of the decreased need for coordination between union, as occurs when an additional union obtains 30% of the votes, thereby gaining the possibility to circumvent coordination to sign agreements. However, the source of this positive effect appears unclear and it is not robust to choosing a narrower bandwidth.

By contrast, the results found when estimating the effects of the first union gaining the possibility of vetoing agreements appear robust. However, while one would have solely expected adverse consequences for the workers, they suggest that the firm gains, possibly at the expense of the workers. Nonetheless, it remains unclear whether these results should be interpreted causally: although post electoral differences in productivity trends appear to mimic pre-electoral imbalances, differences in trends of the share of wages in the value-added sharpened after the election, in such a way that they cannot be entirely explained as the mechanical consequence of differences in productivity trends in a context of wage stickiness.

6.2 Intermediary channels: wage bargaining process and professional relations

6.2.1 Ambiguous results raising a series of questions: no effects of multi-unionism or effects conveyed through unexpected channels?

Although the results from section 6.1. do not permit uncovering clear effects on wage bargaining outcomes, one should not necessarily conclude in the absence of effects of multi-unionism on the wage bargaining process. Indeed, since the economic outcomes studied are by nature noisy, being determined by a plurality of factors beyond the wage bargaining process, and since one would not expect large treatment effects, the limited sample size entailed by recourse to RDDs may result in type 2 errors despite the steps I took to improve the accuracy of the estimates.

By contrast, if multi-unionism indeed affects the wage bargaining process, effects may be measured more precisely by focusing on features of this process itself, such as the frequency of signing of agreements, of veto, of strikes or other mobilizations and the perceived quality of the social climate.

The previous results raise a series of questions which I therefore now explore by considering the above outcomes, combining data from the Acemo sur le Dialogue Social en Entreprise and REPONSE datasets. I investigate three questions.

First, does the presence of an additional bargaining union with solely 10% of the votes appear to have no effect on any feature of the wage bargaining process?

Second, does the presence of a union obtaining the possibility of signing agreements alone result in an increase in the frequency of agreements on wages signed, or in the probability that an agreement be signed? Does the presence of a union obtaining the possibility of vetoing agreements alone result in an increase in the probability of veto, and thereby a decrease in the probability that an agreement on wages be signed and not vetoed?

Third, does a union obtaining the possibility of signing agreements alone, or of vetoing agreements alone, appear to have effects on other features of the bargaining process, possibly explaining the positive effects on the value-added by worker suggested by the previous results? (although the evidence in favour of positive effect of both configurations appear limited, since the effect of a union's obtaining 30% of the votes seemed to have a null or positive effect on value-added, while the larger productivity growth in firms where the first union obtained more than 50% of the votes might be attributable to pre-existing imbalances)

As already discussed, the presence of an additional small trade-union with the possibility of bargaining might have no effect on any step of the wage bargaining process despite substantial effects of multi-unionism in general: by virtue of identification being local, RDDs only enable identification of the effect of an additional trade-union with solely 10% of the votes. In most cases, this occurs when the first union has received a lot of support during professional elections, gathering almost systematically more than 30% of the votes and very frequently more than 50% of the votes. By contrast, one would expect the coordination frictions resulting from multi-unionism to be strongest when the different unions are similarly powerful, and the electoral threat represented by a new union to be similarly strongest when the first union is relatively weak. Hence, the RDDs carried out using the 10% threshold only permit identification of lower bounds for the effects of multi-unionism.

While one would a priori expect that a union's gaining the possibility to sign agreements alone would result in a positive effect on the probability that agreements be signed, the results from section 6.1.2.2. paint a more complex picture. Indeed, while initial results suggest a positive net effect on wages, the source of this effect – whether it is driven by a positive effect on the value-added by worker, on the share of wages in the value-added or a combination of the two – remains unclear, and this positive effect is not robust to the choice of a narrower bandwidth. Can we find any evidence that a union gaining the possibility of signing agreements alone affects the wage bargaining process? Does it result in an increase in the probability that agreements be signed, as one could expect, thus explaining a possible increase in the share of wages in the value-added? Could this affect the value-added by worker through some alternative channel such as decreased tensions?

Similarly, while one would a priori expect that a union's gaining the possibility to veto agreements alone should have a negative effect on the probability that an agreement be signed, thereby negatively affecting the share of wages in the value-added and thus the wages, the results from section 6.1.3. might suggest a less obvious story. As expected, a negative effect on the share of wages in the value-added is found. However, the sign of the resulting effects on the average wage is not robust to the bandwidth chosen. This reflects an unexpected difference in productivity trends, whereby the firms where the first union obtained more than 50% of the votes were characterized by greater productivity increases. While a negative effect on the share of wages in the value-added could have been interpreted as the direct consequence of recourse to veto, preventing wages from increasing, it may also partly reflect the mechanical consequence of the increase in value-added suggested by the results. Nonetheless, the question of whether the positive effect on the value-added by worker should be attributed to the first union gaining the possibility of vetoing agreements or is a spurious result entirely attributable to pre-existing imbalances remains open.

6.2.2 Intermediary channels: approach and results

As previously done when using wage bargaining outcomes as dependent variables, when using dependent variables built using the Acemo data, I also used values of the dependent variable the year before the election as dependent variable as a benchmark[62].

6.2.2.1 Additional weak bargaining union: no apparent effect on signing nor strikes

As illustrated by tables 89 and 90 in Appendix, the presence of an additional – relatively weak - union with the possibility to take part in bargaining does not seem to have a consistent effect on the probability that an agreement on wages be signed, nor on the probability that a strike be organized or on the length of strikes.

In the case of agreements, the coefficients obtained (using Acemo) when using the score of the second, third, fourth or fifth union are indeed never significant and of similar amplitude when using the signing of agreements after the election or before.

In the case of the proportion of years when a strike was held during the mandate – out of the years when the firm was surveyed in Acemo – and strike length, the evidence converges to an absence of effect. The amplitude of the coefficients obtained with the outcomes measured after the election is indeed similar to that of the coefficients obtained with for the outcomes measured before the election. Hence, the obtained coefficients are more frequently significant for the outcomes measured before the election than after. Second, although I did not report the results, the sign is frequently not robust to the bandwidth chosen.

6.2.2.2 30% and 50% thresholds: no straightforward effect on signing or recourse to veto

The results from sections 6.1.2. and 6.1.3. suggested some effects on wage bargaining outcomes of a union obtaining more than 30% or more than 50% of the votes. The most straightforward channels whereby one would expected these configurations to affect wage bargaining is an increase in the probability that an agreement be signed when a union reaches 30% of the votes, and an increase in the probability that an agreement be vetoed when it reaches 50% of the votes.

However, as illustrated by tables 91 to 95 in Appendix, none of these configurations appears to

[62]Since the sampled firms are not systematically the same from one year to the other, owing to a subsample of firms being surveyed, this cannot be straightforwardly interpreted as signalling pre-existing imbalances or changes in trends, but provides a benchmark for the amplitude of the coefficients.

affect the corresponding probability[63].

In contrast with what could have been expected, the presence of an additional union with the possibility of signing agreements alone does not seem to increase the probability that an agreement on wages be signed. While, when using REPONSE data (table 91 in Appendix - using bandwidths of 0.1 and 0.05 since recourse to bandwidths of 0.02 resulted in less than 20 observations available for estimation of the local linear polynomial when using the score of the first and third union as running variable), I obtained positive point estimates when using the score of the first and third union as running variables, none of the coefficients is significant. Taking advantage of the larger sample size of the Acemo dataset to use narrower bandwidths (0.05 and 0.02 in tables 92 and 93) further highlights that the sign of the coefficients obtained (when using an indicator variable equal to one if minimum one agreement is signed during the mandate, or the share of years when agreements one wages were signed), is not robust to the bandwidth chosen nor database used. Furthermore, the magnitude of the coefficients obtained is similar to the magnitude of the coefficients obtained when using an indicator variable equal to one if an agreement was signed the year before the election, corroborating the evidence in favour of an absence of treatment effect. The coefficients obtained when using the score of the second union as running variable are similarly respectively non-significant but negative when using the REPONSE data, and negative and significant when using the Acemo dataset, but seem to reflect an imbalance which pre-existed before the election in the latter case.

There appears to be no clear positive effect of the first union obtaining 50% of the votes on the probability of veto either. The point estimate obtained obtained when using REPONSE data (using the full sample and a polynomial of order 2 due to the limited sample size) would suggest a 14.7% increase in the probability of veto (table 94 in Appendix). However, the magnitude of the coefficient (a 1.42 percentage points increase in the share of years marked by a recourse to veto during the mandate) obtained when using the Acemo dataset and a bandwidth of 0.05 (table 95 in Appendix) is far lower. This last coefficient may still seem substantial in the light of the low frequency of veto: based on the Acemo data, it appears that, conditional on the holding of negotiations (and on the respondent knowing whether or not an agreement has been vetoed), every year, some agreement is vetoed in approximately $2-3\%$ of the firms of the sample. Nonetheless, the magnitude of the coefficient is inferior to the magnitude of the coefficient obtained when using recourse to veto prior to the election as dependent variable. Furthermore, using a bandwidth of 0.02 yields a negative coefficient.

Thus, while the results from sections 6.1.2. and 6.1.3. suggest that a union obtaining 30% or 50% of the votes might affect wage bargaining outcomes, and while one would expect these configurations to primarily affect the signing of agreement or recourse to veto, there appears to be no evidence in this direction.

6.2.2.3 Alternative channels: decreased mobilizations and improved social climate?

Although one would have expected that a union's reaching 30% or 50% of the votes would have primarily affected the probability that an agreement be signed or vetoed, this does not appear to be the case.

Should we conclude that the suggestive evidence of effects on wage bargaining outcomes found in 6.1.2. and 6.1.3. must be spurious, or could alternative channels explain these findings?

Although, due to the small sample sizes available, evidence is only suggestive, as illustrated by table 23 below and tables 99 to 106 in Appendix, additional results suggest that both configurations might affect alternative aspects of the bargaining process, namely the probability of mobilization

[63] However, the results obtained when taking into account the possibility of veto should be interpreted cautiously. The question on the recourse to veto was removed from the Acemo surveys in 2013, in response to concerns that it was not understood correctly by respondents who ignored the institutional requirements needed for unions to exert their veto right. This implies that the resulting variable may suffer from mismeasurement, and also severely restricts the sample size available. However, this lack of knowledge of the institutional context is, by itself, informative, in that it suggests that the practice of veto is relatively limited.

during wage bargaining and the quality of the social climate, as perceived by employers.

With respect to mobilizations, I report the results obtained when using bandwidths of 0.05 and 0.02 in table 23 below. They indeed suggests that a union's having obtained slightly more than 30% or 50% of the votes (as opposed to slightly less) is associated with a lower probability that a mobilization (of any sort, including recourse to a petition, the organization of strikes, etc) be organized during the process of wage negotiation.

Table 23: Effect of a union reaching 30% or 50% on the probability of mobilization during wage bargaining

	Bandwidths of 0.05 and 0.02			
Running variable	Score of first union	Score of second union	Score of third union	Score of first union
Cut-off	0.3	0.3	0.3	0.5
	(1)	(2)	(3)	(4)
VARIABLES	Mobilization during last wage nego condit. on nego	Mobilization during last wage nego condit. on nego	Mobilization during last wage nego condit. on nego	Mobilization during last wage nego condit. on nego
RD_Estimate	-0.209*	0.000903	-0.0104	-0.0220
	(0.119)	(0.0584)	(0.0849)	(0.0980)
Conventional p-value	.078	.988	.902	.822
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	81	502	221	212
Obs right of cut-off	192	442	85	254
RD_Estimate	-0.244	-0.102	-0.0537	-0.0217
	(0.156)	(0.0862)	(0.0851)	(0.172)
Conventional p-value	.118	.237	.528	.9
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	39	203	49	71
Obs right of cut-off	54	202	39	120
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Observation level: establishment, weights inversely-proportional to number of establishments in perim.
-
Dependent variables:
Missing if no wage nego. year before interview
defined only if mandate started before 2010 and finished after
-
Excluded observations:
Pre-electoral alliances, elections with score at the cut-off, CGC, excluded
-
Log of number of votes controlled for
-
Data: DARES (MARS 2009-2016, REPONSE 2011 and 2017, Representant de la Direction)

Although most point estimates are not significant and one may be worried that the results be spurious due to the small sample size available, the negative sign in all eighth but one (column 2, where the obtained coefficient is null) columns might suggest demobilizing effects of a union obtaining slightly more than 30% or slightly more than 50% of the votes, as opposed to slightly less.

Using a bandwidth of 5%, the results suggest that the first union obtaining 30% of the votes is associated with a 20% decrease in the probability of a mobilization during the wage bargaining process (significant at 10%). Similarly negative coefficients (although of a substantially lower amplitude, being of the order of 1-10 percent) are found when estimating the effect of the first union obtaining 50% of the votes or the third union's obtaining 30% of the votes. Although the effect found for the effect of the second union's obtaining 30% of the votes is null, it falls down to -10% when using a narrower bandwidth of 0.02.

One could have feared that the results might have reflected concavity of the probability of mobilization in the support for moderate to strong unions whereby, for instance, the stronger the union, the lesser the marginal effect of greater support on the probability of mobilization. The fact that the coefficients remain negative when using a bandwidth of 0.02, and that the magnitude of all but one (column 4) point estimates increases however suggest that this is not the case.

Could this decrease in mobilizations manifest itself by a decrease in strikes, thereby explaining possible positive effects on firm productivity?

To explore this question, I used the Acemo dataset to investigate whether these configurations seem associated with negative effects on strike frequency and strike length. However, as illustrated by tables 96 to 98 in Appendix, there does not seem to be a clear negative effect of these configurations on the proportion of years when a strike was held during the mandate, nor on strike length[64].

With a bandwidth of 0.05, point estimates would suggest a negative effect of the first union reaching 30% on both variables – a decrease by 13-15% (significant at 10% in the case of the share of years when a strike was held). However, this is not robust to choosing a bandwidth of 0.02 and effects appear more mixed (the signs obtained when estimating effects on strike frequency and strike length diverge and are not robust to bandwidth choice) in the case of second and third union.

While using a bandwidth of 0.05 or 0.02 would concur in suggesting a negative effect on strike frequency of the order of -6% when the first union reaches 50% of the votes, the sign of the effect on strike length is not robust to the bandwidth chosen and the amplitude of the coefficient obtained when using the probability of strike one year before the election as running variable is larger, highlighting that the differences found post-election are likely to be attributable to chance.

However, additional results which can be found in Appendix might suggest that a union obtaining 30% or 50% of the votes positively affects the social climate as perceived by the employer. I use three indicator variables as dependent variables, respectively equal to one if the respondent (employer representative) answered that the social climate in the establishment is good, if she answered that it has improved over the three years preceding the interview and if she answered that it has worsened over the three years preceding the interview.

As illustrated by tables 99 and 100 in Appendix, although none of the obtained coefficients is significant, the point estimates obtained when using bandwidths of 0.1, 0.05 or 0.02 would point to a positive effect of the first or second union obtaining 30% of the votes on the probability that the respondent answered that the social climate in the establishment is good, and negative effect on the probability that he answers that it has worsened over the three years leading to the interview. In the case of the second union, they also point to positive effect on the probability that the respondent answers that the climate has improved over the past three years, albeit in the case of the first union, this coefficient is negative. In the case of the third union, the evidence appears more mixed (table 101 in Appendix): although the sign associated to a worsened social climate is systematically negative, so is the sign associated to a good social climate, while the sign of the

[64] Although aggregate strike length might seem more relevant than strike frequency (as measured by the share of years - during the mandate - for which a strike was recorded) in terms of implications for firm productivity, one might fear a lack of accuracy resulting from outliers, motivating my decision to consider both outcomes.

coefficient associated to an improved social climate is not robust to the bandwidth chosen.[65]

Evidence in favour of a positive effect of an union reaching 30% of the votes appears more compelling when considering union-specific scores.

As illustrated by tables 102 and 103 in Appendix, distinguishing between the effect of the CGT, CFDT, FO, CFTC or UNSA[66] reaching 30% of the votes indeed suggests a positive effect on the social climate of most unions reaching 30% of the votes. Indeed, in four out of five cases, the coefficient associated to a good social climate is positive. Similarly, in four out of five cases, the coefficient associated to a worsened climate is negative (and significant at 10% for the CGT and FO). Although in three our of five cases, the coefficient associated with an improved social climate is by contrast positive, the magnitude of the negative coefficients (-0.00118, -0.0287 and -0.0995) is lower than the magnitude of the two positive coefficients (0.115 and 0.177). Furthermore, for two (the CGT and FO) out of these three unions for which the coefficient associated to an improved social climate is negative, the coefficient associated to a worsened social climate is however negative and of larger amplitude (-0.136 and -0.185).

Similarly, table 104 in Appendix would suggest a rather positive effect on the perceived social climate of the first union reaching 50% of the votes, although most coefficients are not significant. With all three bandwidths, the coefficient associated with a good social climate is positive, indicating a $7 - 13\%$ increase in the probability that the social climate is deemed "good" by the respondent. Similarly, the coefficient associated with an improved social climate is systematically positive or null, indicating a $0 - 12.8\%$ increase in the probability that the climate is perceived as having improved. Evidence appears slightly more mixed when considering whether the climate is perceived as having worsened, however, since the coefficient obtained is negative in only two (with bandwidths of 0.1 and 0.05) out of three cases.

I report union-specific results for the largest unions (CFDT, CGT, FO, CFTC) in tables 105 and 106 in Appendix, using a bandwidth of 0.5 and a polynomial of order 2 due to the small sample size, but reporting estimates with a bandwidth of 0.05 and polynomial of order 1 for the two largest unions (CFDT and CGT). These results similarly suggest mainly positive effects on the social climate. Indeed, when considering whether or not the social climate is perceived as "good", all but one coefficients are positive, and the only negative coefficient is approximately one order of magnitude lower (0.000675, as compared to 0.0395, 0.083, 0.0964) than the coefficients obtained for other unions. Although only half of the coefficients associated with the social climate having improved are positive, all coefficients associated with the social climate having worsened are by contrast negative.

6.2.3 Alternative channels: interpretations and conjectures

Can these results - namely, the apparent absence of effect of a union's reaching 30% or 50% of the votes on the probability that agreements be signed or vetoed, but a possible decrease in mobilizations during the wage bargaining and improvements in social climate - help make sense of the results obtained when studying the effects on wage bargaining outcomes of an additional union at 30% or 50%?

In the absence of effect of a union reaching 30% of the votes on the probability that agreements be signed, there appears to be no clear channel whereby the positive net effect on wages found when using a bandwidth of 0.05 could result from an increase in the share of wages in value-added. This suggests that the net effect on wages found might be spurious, as suggested by recourse to a bandwidth of 0.02, or alternatively that it must be driven by a positive effect on firm productivity.

[65] For the third union, the only results reported were obtained using a bandwidth of 0.05 since using a bandwidth of 0.02 resulted in less than thirty observations on both sides of the threshold.

[66] The number of observations for SUD was insufficient to carry out inference.

One could expect both a better social climate and decreased mobilizations to have a positive effect on the value-added by worker, if an improved social climate resulted in more motivated employees or better cooperation in the workplace, and if less mobilizations resulted in less work interruptions. Alternatively, an improved social climate may simply be the manifestation of less mobilizations: the answers reported correspond to employers' perceptions, hence one might expect decreased mobilizations, especially if taking the form of decreased strikes, to result in a better climate, as perceived by the employer.

However, interpretation of the decrease in mobilizations as the source of the differences in productivity trends which the results of sections 6.1.2. and 6.1.3. hint at may seem hindered by the absence of a clear negative effect on the probability or amplitude of strikes. Nonetheless, this may also reflect the fact that the configurations studied solely affect strikes and mobilizations organized in the context of wage negotiations. Given the small sample size, this might explain that the treatment effect on strikes fails to be detected.

A second puzzle remains, however: how could we explain that a union obtaining the possibility of signing agreements alone or vetoing agreements alone would result in an improved social climate and less mobilizations during wage negotiations, without affecting the probability that an agreement be signed or vetoed? And similarly, how could we explain the evidence in favour of a negative effect of the first union reaching 50% of the votes on the share of wages in the value-added (which, as previously argued, does not seem entirely attributable to differences in productivity trends) when no increase in recourse to veto is apparent?

One could have conjectured that, as unions get the possibility of signing agreements alone by reaching 30% of the votes, they mobilize less, since signing agreements enables unions to justify their existence[67]. However, this seems contradicted by the absence of a positive effect on the probability that an agreement be signed.

Alternatively, if unions have recourse to mobilizations as a way of publicly exerting pressure on other unions to convince them to enter a signing coalition, the absence of a need to form such a coalition may explain a decrease in mobilizations.

One might want to explore a distinct interpretation however, whereby the effects found would not be driven by the union which reaches 30% of the votes but by the *response of other unions*. Unions might free-ride and mobilize less when they know that another union is going to sign the agreement and that no coalition is needed for the agreement to be validated. This could be the case if signing agreements were perceived as making a compromise with the employer, negatively affecting the combative self-perception of the union.

The positive effect on the social climate of the first union obtaining the possibility of exerting a veto right may seem a priori more puzzling.

One would indeed expect recourse to veto right to worsen the relations between unions, thereby hindering the quality of the social climate. However, as previously highlighted, there appears to be no clear evidence in favour of an increased recourse to veto. Nonetheless, having a veto right may act as a deterrence tool, in such a way that the first union has the possibility to exert pressure on the negotiations without having to organize mobilizations: neither the union nor the employer may have interest in the first union having recourse to its veto right, as it may reflect badly on its image and on the relations with the employer. Thus, by using the threat of veto, the first union may impact negotiations in the direction it desires without needing to organize alternative mobilizations. However, this would be difficult to reconcile with the evidence in favour of a negative effect on the share of wages in the value-added: if the majoritarian union used its veto right as a deterrence tool, thus forcing employers to make more compromises, but never actually having recourse to veto, one would actually expect a positive effect on the share of wages in the value-added.

In this light, one may want to explore the possibility that the effect found is not driven by the union which receives the possibility of vetoing agreements, whether it exerts this right or uses it as a deterrence tool, but by the other unions.

Indeed, the 50% threshold used correspond to two distinct treatments: the union which reaches

[67]Rosanvallon [1998] thus argues that, in a context of decline of unionism, signing agreements is taken upon by unions as a way of justifying their existence.

50% of the votes gains the possibility of vetoing agreements alone, but *all alternative coalitions of unions also lose the possibility of vetoing agreements.* Thus, the results from section 6.1.3., suggesting that the firm gains at the expense of workers when the first union reaches 50%, may actually originate in a combative coalition of unions losing its veto right. Could this be reconciled by the evidence in favour of a negative effect on the probability of mobilization during the wage bargaining process? If, in response to the loss of their veto right, other unions attempted to influence the negotiations by exerting pressure through mobilizations, both findings might seem difficult to reconcile. Alternatively, however, this might result in resignation and thereby demobilization by these unions, accepting that the bargaining process will be led by the majoritarian union. This would, in turn, explain an improved social climate, as perceived by the employer.

Finally, this last explanation might explain an apparently puzzling result obtained when estimating union-specific effects of the first union reaching 50% of the votes. The results in table 105 in Appendix indeed suggest that the positive effects on social climate found are mainly driven by the CFDT. However, the latter is the most reformist of the union spectrum in France. As a result, an explanation of any effect of the CFDT's obtaining the right to veto in terms of effects of its actual recourse to veto or even in terms of deterrence would not seem credible. By contrast, this last explanation could echo criticisms of so-called "Syndicats Maison", whereby the signing of agreements by more conciliatory unions eventually benefits the firm at the expense of the workforce.

7 Discussion and future perspectives

In this dissertation, I run a series of RDDs, exploiting three institutional discontinuities to investigate how the coexistence of a plurality of unions in a firm affects the outcomes of the wage bargaining process. Provided a union has obtained respectively minimum 10%, 30% and 50% of the votes in the first round of professional elections in a firm, it can take part in bargaining, sign agreements alone or veto agreements alone. Using the score of the first to fifth union and exploiting these three discontinuities therefore allows me to capture the (local) Intention to Treat Effects of an additional union having the possibility to take part in bargaining, but also undertake to sign agreements alone, or by contrast block an agreement signed by others.

Although DiNardo and Lee [2002, 2004], Lee and Mas [2012] and Frandsen [2012] have similarly had recourse to RDDs to identify the causal effects of unionization, to the best of my knowledge, to this date, causal identification of the effects of multi-unionism had not yet been attempted.

The effects of an additional union is a priori ambiguous, both for the workers and employers. Coordination frictions between unions should be expected to negatively affect the representation of workers, thereby negatively affecting wages, but this negative effect could be offset by increased electoral competition resulting in increased union accountability. Furthermore, a positive or negative effect on workers' representation could be offset by effects on firm productivity. Considering the "two faces" of unions highlighted by Freeman and Medoff [1979], the effects on firm productivity of both coordination frictions and increased electoral accountability are a priori ambiguous. Indeed, if bargaining is considered as a zero-sum game between the workers and the firm, coordination frictions may benefit the employer at the expense of the workers, but if one takes into account the resources involved in bargaining and the risk that coordination friction leads to crises, coordination frictions may result in a net loss for both bargaining partners. Alternatively, coordination frictions may result in union paralysis, thereby resulting in decreased recourse to industrial action and positively affecting productivity. While considering labour relations as a zero sum game would similarly lead to conclude that more accountable unions would benefit the workers at the expense of the employer, a better representation of workers may prevent crises, result in a better social climate and better articulation of the interests of different actors, eventually making a net gain possible. But such a better representations might also rsult in more mobilizations, eventually negatively affecting productivity.

The questions I investigated were therefore twofold. First, what is the net effect of multi-unionism on wages? Second, can any evidence that coordination frictions between unions affect the wage bargaining be detected?

As illustrated among others by DiNardo and Lee [2002, 2004], Lee and Mas [2012] and Frandsen

[2012]'s works, RDDs in electoral contexts are increasingly popular, since they appear to enable identification under remarkably unrestrictive assumptions. The absence of precise manipulation of electoral scores is traditionally assumed to guarantee identification of the treatment effect of interest when running RDDs in electoral settings. By contrast, in the present context, characterized by asymmetrically-distributed election sizes and a majority of small elections, elections exactly to the left of the thresholds of interest cannot be used as valid counterfactuals for elections at the threshold, since the latter are, on average, characterized by a lower number of voters and therefore pertain to smaller firms. Since election sizes appear to be symmetrically-distributed around the thresholds of interest, however, recourse to "donut" RDD appears to restore the validity of the identification, as suggested by the results obtained when testing for the balance of pre-determined characteristics.

Despite a first stage which confirms that a union's gaining the right of representing workers by achieving 10% of the votes in a professional election effectively leads to a jump in union presence, and despite taking several steps to increase the accuracy of the estimates, I find no evidence of a net effect of an additional union on the average wage of workers, nor of effects on the share of wages in the value-added or on the value-added per worker. These inconclusive results do not seem to solely reflect the fact that the minimum detectable effects are high: the magnitude of the coefficients obtained when considering post-electoral outcomes is indeed comparable to that of the coefficients obtained when considering pre-electoral outcomes. Furthermore, the signs obtained when estimating the effects of a second, third, fourth and fifth union are not consistent nor seem to reflect different effects of an additional union in contexts with a low or high number of unions. An exploration of intermediary channels proves similarly inconclusive.

However, one should not conclude that multi-unionism has no effect on the bargaining process. First, the effects on wage bargaining outcomes of an increase in coordination frictions and of an increase in union accountability might offset one another. Second, as highlighted by Lee and Mas [2012]' contradictory finding of a large average effect of unionization on firm profitability despite a null effect of unionization resulting from close races, unionism may be characterized by heterogeneous effects, whereby the local effect of a weak union captured by the RDDs carried out should be expected to solely provide a lower bound for the effect of multi-unionism.

In this light, I therefore exploit the 30% and 50% thresholds to explore wage bargaining implications of the need for unions to coordinate with one another when several unions co-exist in the same firm.

Due to the limited sample size warranted by recourse to local estimation, compounded by the fact that one would not expect major effects of multi-unionism, evidence is only suggestive and relies on a comparison of the sign and magnitude of coefficients obtained when considering post-electoral and placebo pre-electoral outcomes. Although evidence is therefore only suggestive, initial results found when estimating the effect of a union obtaining the possibility to sign or veto agreements alone indeed seem to corroborate the hypothesis of coordination frictions between unions. They suggest that, when a union obtains the possibility of signing agreements alone, thereby alleviating the need for coordination between unions, this results in higher wages. By contrast, when a union gains the possibility to veto agreements signed by others, this seems to result in a lower share of wages in the value-added.

However, the results obtained when estimating the effects of an additional union gaining the possibility to sign agreements alone are not robust to recourse to a narrower bandwidth. While the results obtained when estimating the effects of a union gaining the possibility to veto agreements alone are robust to other bandwidths, they seem to be at least partly attributable to pre-existing imbalances.

In future works, further exploration of both the validity of the identification around the 50% threshold and the robustness of the effects found when a union reaches this threshold would be desirable.

Indeed, although the imbalances found are no longer significant once accounting for multiple testing, tests of the identification suggested imbalances around this threshold. To investigate the possibility of sorting around this threshold, I tested for the presence of an incumbency advantage, whereby a union systematically obtains slightly more than 50% of the votes. To do so, I followed

a strategy similar to that adopted used in Snyder [2005] (albeit simpler), using incumbency status as the dependent variable in a RDD using the score of the first union as running variable. The results, which can be found in table 108 in Appendix 8.10. however do not suggest the presence of an incumbency advantage. Further exploration of the sources of the imbalances detected around the 50% threshold when testing for the balance of pre-determined characteristics would however be desirable, to determine whether these imbalances seem indeed attributable to multiple testing or reflect non-randomness of electoral scores.
Additionally, even in the absence of such non-randomness, identification of treatment effects using the 50% threshold might be threatened by the presence of a double-discontinuity: except in elections with several colleges and where the number of voters in a college is not proportional to the number of seats in the corresponding college, the 50% discontinuity indeed coincides with possible discontinuities in the decisions taken by the institution (e.g. CE) whose election is used to measure union representativity. Indeed, if a union obtains 50% of the votes, this implies that the corresponding list similarly obtained 50% of the seats in this institution. Thus, in future works, it would be desirable to check robustness of the results to restriction of the sample to elections where both discontinuities do not coincide.

Furthermore, exploration of intermediary channels highlights that, when a union reaches 30% or 50% of the votes in a firm, this does not seem to generate a jump in the probability that an agreement be signed or vetoed.

Should this finding lead us to conclude that the effects suggested by an investigation of wage bargaining outcomes are spurious?

Alternative channels might nonetheless explain these results. Although evidence is only suggestive, it indeed seems to converge in suggesting that a union's obtaining of the possibility of signing or vetoing agreements alone is associated with less mobilizations during the wage bargaining process and a better social climate as perceived by the employers.

In this light, one may want to explore the possibility that the effects are not driven by the union which obtains the possibility of signing or vetoing agreements alone but, by contrast, by the response of other unions to this monopolistic position. When a union obtains the possibility of signing agreements alone, other unions might free-ride and refrain from signing agreements, resulting in a demobilization which might explain employers' perceived better social climate. If decreased mobilizations results in increased productivity, this could in turn be thought to explain the positive effect on wages suggested by inspection of wage bargaining outcomes, although evidence in favour of a positive effect on wages or of a positive effect on firm productivity does not appear very conclusive. Since, in order to veto agreements, a union or coalition thereof needs minimum 50% of the votes, when a union obtains the possibility of vetoing agreements alone, this also implies that all alternative coalitions of unions lose the possibility to do so. If a combative coalition of unions thus loses the possibility to veto agreements, resulting in resignation and demobilization, this could in turn explain the apparent effects on wage bargaining outcomes and on the wage bargaining process.

Before further exploring these explanations however, one might want to further investigate whether the suggested effects of a union reaching 30% and 50% of the votes are spurious. Although I analyzed the coefficients obtained in the light of trends the year before the election, as well as investigated robustness to the bandwidth chosen, additional steps should be considered in this view.

First, at previously mentioned, it would be desirable to further investigate the sources of the imbalances around the 50% threshold as well as verify that the results found are not attributable to the double discontinuity previously mentioned.

Second, analyzing the coefficients found in the light of longer pre-trends would be desirable to ensure that they do not seem attributable to chance economic fluctuations. Similarly, it would be desirable to put the magnitude of the coefficients obtained when using REPONSE data in perspective by comparing it to the magnitude of coefficients obtained when using a placebo sample, restricted to firms where the election used to compute scores occurred after the interview date. Doing so would help determine whether the evidence in favour of negative effects on mobilizations and positive effects on employers' social climate perceptions appears spurious.

Third, recourse to cross-validation techniques to realize more accurate predictions - without running the risk of overfitting - would permit gaining accuracy when using residualized dependent variables.

Fourth, investigation of effects on other features of the bargaining process, such as verifying whether the improved social climate only corresponds to employers' perceptions or also to workers and union representatives' perceptions, could contribute to a better understanding of the mechanisms at play.

Finally, although in this dissertation, I mainly focused on the question of coordination frictions, exploring whether the presence of an additional union appears to be a source of increased union accountability as a result of increased electoral pressure[68] and how this affects the distribution of wages also appears worthy of investigation.

Indeed, the initial conjecture that increased union accountability should manifest itself by an increase in unions' efforts to maximize the wage bill may seem overly simplistic in the light of recent works which have questioned Dunlop et al.'s idea that unions should be modelled as maximizing the wage bill.

In this line, Friedman [1950] had already highlighted that workers may sanction unions' excessively high wage demands as they weigh the resulting wage benefits against potential job security threats. In the French context where a possible self-selection into union representation by the most combative workers has been suggested to occur in response to union discrimination (Breda [2016]), one could therefore conjecture that increased union accountability could actually lead unions to moderate their wage demands.

Furthermore, Dunlop et al.'s conception of unions as maximizing the total wage bill overlooks the fact that, as the workforce is heterogeneous and as a union's incentives are not necessarily aligned with the workforce's, unionism may have redistributive implications. Unions may indeed not perceive themselves as meant to defend the aggregate interests of the workforce, but rather prioritize the interests of the worse-off workers. This would echo Frandsen [2012]'s finding that, while running RDDs in the American context does not enable identification of a significant effect of unionization from close races on the average wage, this conversely sheds light on large redistributive impacts from the top to the bottom of the wage distribution.

The redistributive consequences of increasing union accountability are however a priori ambiguous.

Indeed, one might expect increased electoral competition to offset such redistributive consequences, as unions prioritize the median voter rather than the worse-off workers. However, Rosanvallon [1998] also suggested that the introduction of electoral competition between unions could have led each union to further stress its own ideological specificities to justify its existence. Thus, in a context with more than two unions, increased electoral competition might result in union polarization rather than in an alignment of all unions on the defence of the interests of the median voter.

[68] Effects of increased electoral competition could be identified by distinguishing firms along some indicator of electoral competition, classifying them for instance based on the standard deviation of the electoral score – over past elections – of the most powerful union in order to proxy the extent to which an additional union may be thought to threaten the incumbent's status. Finding more pronounced effects of an additional union among the firms with above-median levels of electoral competition could thus suggest that multi-unionism results in increased electoral competition, and from there could be exploited to investigate in what directions unions modify their behaviour in response to an increase in electoral competition.

8 Appendix

8.1 Descriptive statistics

Figure 7: Main unions: frequency of presence and rank (observation level: election)

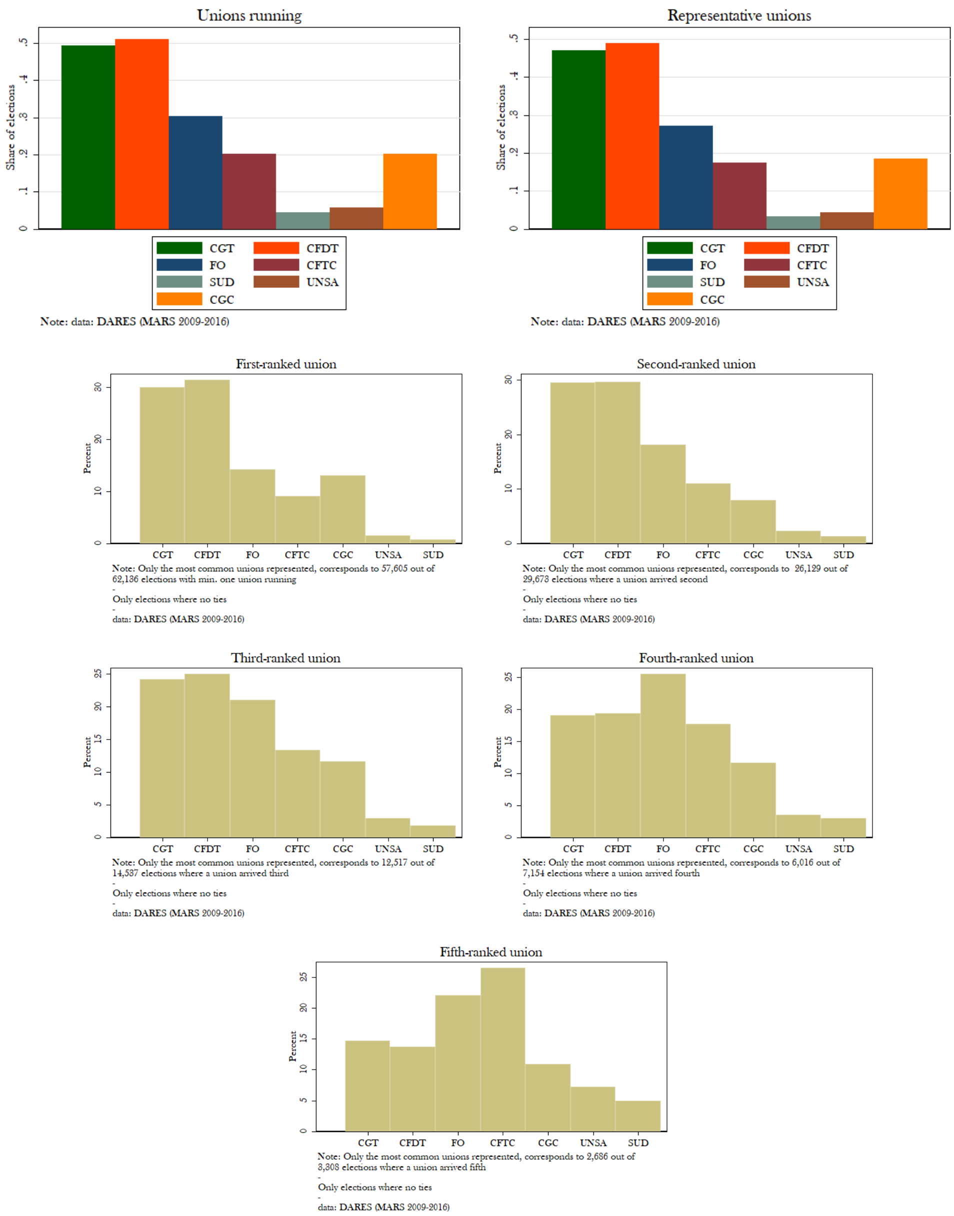


Table 24: Economic variables: annual decomposition: workforce size, wage, value-added by worker

Descriptive statistics: raw data								
Workforce size								
Year	Median	Mean	Min	Max	Standard deviation	Nb. obs	Nb. obs with workforce size of 0	Nb. obs with workforce size of 0 and strictly positive value-added
All years	54.5	195.5109	0	264,781	1,504.593	289,751	12,260	10,293
2007	59	239.0434	0	264,781	2,318.373	26,124	1,039	117
2008	55	180.5737	0	49,981	796.3406	25,992	146	132
2009	57	184.0524	0	53,133.5	784.4635	26,635	414	387
2010	57	189.9734	0	215,350	1510,489	27,477	212	184
2011	57.5	188.8948	0	206,505	1,451.083	28,062	17	13
2012	59.25	204.7381	0	233,911.5	1,627.106	28,719	17	13
2013	59	204.2465	0	232,716.8	1,623.672	28,544	16	10
2014	59.25	205.4685	0	227,036.5	1,603.66	28,334	16	8
2015	58	204.0421	0	206,406	1,497.963	28,671	724	455
2016	57	206.8036	0	203,296	1,525.935	28,015	550	333
2017	0	84.45962	0	40,286	725.6823	13,178	9,109	8,641

Note: in full-time equivalents for 2008-2017, average number of employees over the year for 2007
Sources: INSEE (BRN 2007, FARE 2008-2017, Agrifin 2012-2017)

Descriptive statistics : raw data							
Average annual gross wage							
Year	Median	Mean	Min	Max	Standard deviation	Nb. obs	Nb obs with strictly negative average wage
All years	31.52	40.74	-7443.479	44,952.57	222.96	261,354	57
2007	27.0	32.08	0	810.92	23.19	8,948	0
2008	32.18	49.92	-4.22	44,952.57	407.24	25,846	3
2009	30.82	37.35	-0.17	17,251.47	151.93	26,221	2
2010	31.36	39.15	-45.06	44,468.86	287.55	27,265	2
2011	31.42	39.42	-25.84	29,997.86	204.65	28,045	3
2012	31.38	40.69	-22.78	25,292.89	196.07	28,702	5
2013	31.43	38.86	-14.42	12,918.52	111.79	28,528	6
2014	31.67	39.19	-182.71	10,457.98	108.94	28,318	9
2015	31.98	39.96	-1,447.83	19,985.19	153.25	27,947	7
2016	32.54	46.02	-7,443.479	38,641.29	282.08	27,465	20
2017	32.69	37.47	0	4,910.243	78.62	4,069	0

Note: obtained by dividing gross wage bill by workforce size in full-time equivalents for 2008-2016, by number of employees for 2007 –
Unit: thousands of euros, 2009 terms
Sources: INSEE (BRN 2007, FARE 2008-2017, Agrifin 2012-2017 – inflation series)

Descriptive statistics : raw data								
Value added by worker								
Year	Median	Mean	Min	Max	Standard deviation	Nb. firms	Nb. firms with null value-added	Nb. firms with strictly negative value-added
All years	53.6550	84.4330	-117,314.1	175,407	971,095	261,354	1,019	6,494
2007	52.2923	87.5051	-3,885.61	62,385.27	926.372	8,948	6	205
2008	58.5018	108.6729	-3,406.866	147,967.2	1286.043	25,846	66	422
2009	52.9035	72.8101	-6,419.401	23,154.19	308.1216	26,221	286	475
2010	54.0286	77.4245	-11,146.46	125,462.6	804.546	27,265	61	511
2011	53.8915	85.3080	-41,157.28	142,091.6	1218.172	28,045	36	649
2012	52.7438	83.4246	-14,861.01	58,924.82	675.6588	28,702	36	795
2013	52.4091	79.4375	-117,314.1	129,536.3	1146.807	28,528	42	789
2014	52.4697	82.1040	-24,060.07	175,407	1121.856	28,318	54	849
2015	52.9697	81.3658	-60,461.01	105,205.2	855.5116	27,947	63	808
2016	54.3807	90.6642	-109,427.1	63,716.42	979.1849	27,465	364	830
2017	52.3560	76.8871	-13,975.02	20,437.89	547.3393	4,069	5	161

Note: value-added (production-intermediary consumptions) divided by workforce size in full-time equivalents for 2008-2017, by average annual number of employees for 2007
Unit: thousands of euros, 2009 terms
Sources: INSEE (BRN 2007, FARE 2008-2017, Agrifin 2012-2017 – inflation series)

Table 25: Economic variables: annual decomposition: share of wages in the value-added, economic profitability

Descriptive statistics : raw data								
Share of wages in the value-added								
Year	Median	Mean	Min	Max	Standard deviation	Nb. firms	Nb. Firms for which missing because negative/null value-added	Nb. Firms for which missing because negative wage
All years	0.5910	1.3406	0	55,327.25	118.4295	265,840	10,246	156
2007	0.5432	0.6827	0	551.7499	0.6827	8,854	284	0
2008	0.5648	0.6707	0	152.1183	2.2710	25,811	688	3
2009	0.5865	1.0962	0	2,173.696	23.0966	25,998	983	8
2010	0.5867	1.8378	0	2,2321.3	136.3972	27,058	889	6
2011	0.5876	0.7421	0	506.6899	3.9666	27,590	961	11
2012	0.5972	0.8237	0	910.3995	7.8133	28,105	1,080	19
2013	0.6011	1.6935	0	4,501.934	44.2298	27,990	1,050	23
2014	0.6041	1.5524	0	6,049.859	44.7685	27,765	1,114	27
2015	0.6036	2.9559	0	55,327.25	333.9422	27,524	1,146	23
2016	0.6023	1.0921	0	4,955.169	32.7199	26,598	1,413	27
2017	0.5944	0.8194	0	485.0618	5.6140	12,547	638	9

Note: share of wages in value added = (annual gross wage bill)/(production-intermediary consumptions)
Sources: INSEE (BRN 2007, FARE 2008-2017, Agrifin 2012-2017)

Descriptive statistics : raw data							
Economic profitability							
Year	Median	Mean	Min	Max	Standard deviation	Nb. observations	Nb. observations for which missing because negative assets
All years	0.0896	-4,4966	-783,595	73,984.71	2,202.74	270,255	44
2007	0.1562	0.4148	-207	444.1429	7,9610	8,976	0
2008	0.1318	0.8076	-34,990.63	22543.4	302.9364	25,932	1
2009	0.0982	2.2857	-12,475.5	35,171.18	307.9648	26,191	8
2010	0.1000	4.1508	-14,245.61	58,854.01	466.1216	27,328	1
2011	0.0916	2.0606	-19,836.27	45,551.8	449.9655	28,021	8
2012	0.0738	3.5613	-11,280.86	46,508.09	399.0424	28,702	3
2013	0.0771	0.7251	-10,092.02	27,031.34	233.2112	28,544	2
2014	0.0769	0.7230	-32,196.75	45,365.99	426.42	28,381	13
2015	0.0801	4.5662	-58,751.09	73,984.71	654.2082	28,129	4
2016	0.0783	-66.8619	-783,595	33,397.39	6,832.349	27,190	3
2017	0.0763	5.8444	-8,945.112	32,833.76	429.8557	12,861	1

Note: economic profitability = (value-added - gross wages + net taxes) / durable assets
Sources: INSEE (BRN 2007, FARE 2008-2017, Agrifin 2012-2017)

Table 26: Economic variable: growth, before and after trimming outliers

Growth rate of average annual gross wage since year before election

Number of years after election	Median	Mean	Min	Max	Standard deviation	Nb. observations
RAW DATA						
One	0.0135	14.4056	-0.9999	343,968.9	1,763.934	39,860
Two	0.0187	11.7852	-0.9992	308,778.3	1,827.076	32,071
Three	0.0203	3.1365	-0.9997	41,705.8	296.284	20,279
For placebo: growth rate of average annual gross wage wrt its value two years before election						
One year before election	0.0051	13.9725	-1	343,868.9	1,763.934	41,328
TRIMMED DATA						
One	0.0132	0.0261	-0.8580	8.7083	0.2137	34,429
Two	0.0183	0.0329	-0.8248	8.3760	0.2220	27,796
Three	0.0204	0.0368	-0.8113	6.8370	0.2376	17,656
For placebo: growth rate of average annual gross wage wrt its value two years before election						
One year before election	0.0049	11.0365	-0.8744	9.9289	0.2154	35,979

Note: only firms for which corresponding year is included in the mandate –
Unit: 100%, real terms
Sources: INSEE (BRN 2007, FARE 2008-2017, Agrifin 2012-2017 – inflation series)

Increase in value-added by worker since year before election

Number of years after election	Median	Mean	Min	Max	Standard deviation	Nb. observations
RAW DATA						
One	-0.0486	5.7740	-129,248.7	118,569.7	1,076.631	40,180
Two	-0.0837	3.0470	-129,263.2	146,022.3	1,336.36	32,355
Three	-0.3090	2.1348	-109,458.2	123,780.6	1,390.78	20,489
For placebo: increase wrt two years before election						
One year before election	-0.1282	-6.7211	-122,242.3	105,014.5	923.0492	41,601
TRIMMED DATA						
One	-0.0238	-0.2228	-391.5969	385.4259	23.236	34,659
Two	-0.0610	-0.2331	-322.8569	393.1738	25.806	27,987
Three	-0.2451	-0.3459	-332.6813	353 .0351	27.997	17,810
For placebo: increase wrt value-added by worker two years before election						
One year before election	-0.1455	-0.8100	-326.094	285.9326	20.221	36,190

Note: value-added (production-intermediary consumptions) divided by workforce size in full-time equivalents for 2008-2017, by average annual number of employees for 2007
Unit: thousands of euros, 2009 terms –
Only defined form firms for which corresponding year included in mandate (except placebo)
Sources: INSEE (BRN 2007, FARE 2008-2017, Agrifin 2012-2017 – inflation series)

Growth rate of share of wage bill in value-added since year before election

Number of years after election	Median	Mean	Min	Max	Standard deviation	Nb. observations
RAW DATA						
One	0.0107	7.7303	-1	281,140.3	1,398.446	40,504
Two	0.0159	8.3955	-1	261,473.5	1,423.564	33,755
Three	0.0183	0.7952	-1	4,325.6	41.910	22,330
For placebo: growth rate wrt share of wages in value-added to two years before election						
One year before election	0.0063	8.4444	-1	218,138.3	1,160.674	40,533
TRIMMED DATA						
One	0.0110	0.0473	-1	10.805	0.3337	35,728
Two	0.0156	0.0613	-1	9.766	0.3758	29,900
Three	0.0185	0.0697	-1	10.730	0.4117	19,859
For placebo: growth rate wrt two years before election						
One year before election	0.0064	0.0396	-1	11.815	0.2915	38,875

Note: share of wages in value added = (annual gross wage bill)/(production-intermediary consumptions) –
Only defined for firms for which corresponding year included in mandate (except placebo)
Unit: 100%
Sources: INSEE (BRN 2007, FARE 2008-2017, Agrifin 2012-2017)

Increase in economic profitability since year before election

Number of years after election	Median	Mean	Min	Max	Standard deviation	Nb. observations
RAW DATA						
One	-0.0065	-0.6796	-51,645.38	57,175.86	496.641	41,546
Two	-0.0099	-3.4263	-42,045.16	16,588.54	391.550	34,684
Three	-0.0123	-0.9595	-33,129.77	20,278.11	369.680	22,876
For placebo: increase wrt economic profitability two years before election						
One year before election	-0.0033	-0.8817	-46,493.93	35,898.5	479.633	41,468
TRIMMED DATA						
One	-0.0071	-0.3159	-11.1355	8.7320	0.5901	35,946
Two	-0.1052	-0.0443	-10.3753	10.3275	0.6171	30,108
Three	-0.0131	-0.0441	-7.1914	10.3985	0.6239	19,932
For placebo: increase wrt economic profitability two years before election						
One year before election	-0.0036	-0.0248	-12.070	9.1308	0.5413	36,086

Note: economic profitability = (value-added - gross wages + net taxes) / durable assets
Only defined for firms for which corresponding year included in mandate (except placebo)
Sources: INSEE (BRN 2007, FARE 2008-2017, Agrifin 2012-2017)

Table 27: Number of observations available for estimation of effects on the log of the average gross wage

NUMBER OF OBSERVATIONS AVAILABLE IN BANDWIDTH – BENCHMARK: LOG(AVERAGE WAGE)

		Threshold	10%								30%						50%	
		Union	Second		Third		Fourth		Fifth		First		Second		Third		First	
		Bandwidth	5%	2%	5%	2%	5%	2%	5%	2%	5%	2%	5%	2%	5%	2%	5%	2%
Number of observations (resp. left and right of threshold)	1	MARS	[425, 967]	[193, 345]	[1182, 1 959]	[538, 697]	[1341, 1586]	[587, 615]	[813, 559]	[283, 285]	[236, 555]	[126, 193]	[3697, 3994]	[1478, 1644]	[1402, 623]	[408, 330]	[1339, 2882]	[477, 1089]
	2	Placebo: -1-yr	[252, 616]	[119, 215]	[719, 1245]	[335, 442]	[888, 1003]	[383, 393]	[534, 357]	[178, 182]	[144, 397]	[78, 139]	[2381, 2485]	[947, 1022]	[851, 392]	[246, 211]	[828, 1770]	[301, 686]
	3	+1-yr	[234, 555]	[111, 182]	[663, 1144]	[301, 406]	[809, 907]	[341, 354]	[483, 328]	[160, 169]	[123, 359]	[63, 118]	[2171, 2316]	[863, 948]	[773, 355]	[227, 195]	[768, 1640]	[277, 627]
	4	+ 2 yrs	[188, 450]	[85, 151]	[532, 936]	[244, 341]	[668, 761]	[288, 299]	[424, 279]	[142, 142]	[103, 305]	[51, 104]	[1797, 1880]	[725, 773]	[646, 293]	[197, 157]	[645, 1332]	[231, 508]
	5	+ 3 yrs	[124, 297]	[55, 90]	[323, 582]	[143, 202]	[442, 512]	[182, 207]	[279, 184]	[90, 96]	[65, 199]	[33, 71]	[1082, 1157]	[432, 471]	[406, 178]	[135, 96]	[373, 812]	[137, 297]

Note: CGC, pre-electoral alliances, scores at the cut-off excluded – lines 2-5: outliers excluded
For years after the election, observations for which the representativity span ended before the corresponding year excluded
-
Line 1: observations available from MARS 2009-2016
Lines 2-5: observations available from MARS 2009-2016 and for which log(average wage) can be computed the corresponding year (from FARE 2008-2017 or Agrifin 2012-2017)

8.2 Identification

8.2.1 Density of the running variable: union-specific scores

Figure 8: Density around 50% threshold - union-specific

Figure 9: Density around 30% threshold - union-specific

Figure 10: Density around 10% threshold - union-specific

8.3 Identification: balance of pre-determined characteristics

8.3.1 OLS regressions used for prediction

Table 28: OLS regression to predict log of average wage

VARIABLES	(1) Log of average wage 1-yr after election	(2) Log of average wage 2-yrs after election	(3) Log of average wage 3-yrs after election
2010	0.00703	-0.00104	-0.0171*
	(0.0107)	(0.0104)	(0.00952)
2011	0.0238**	-0.00466	-0.00955
	(0.0104)	(0.00997)	(0.00944)
2012	0.0381***	0.0255**	0.0159
	(0.0108)	(0.0105)	(0.0107)
2013	0.0469***	0.0445***	0.0670***
	(0.00986)	(0.00999)	(0.0104)
2014	0.0359***	0.0637***	-0.0252*
	(0.0101)	(0.0104)	(0.0141)
2015	0.0657***	-0.00601	
	(0.0103)	(0.0164)	
2016	0.0379**		
	(0.0166)		
Industry	0.0725**	0.186***	0.233***
	(0.0332)	(0.0451)	(0.0771)
Construction	0.0617*	0.179***	0.221***
	(0.0338)	(0.0455)	(0.0774)
Trade/transport/hospitality	-0.0141	0.105**	0.152**
	(0.0332)	(0.0451)	(0.0771)
Info/communication	0.366***	0.504***	0.536***
	(0.0347)	(0.0470)	(0.0790)
Finance/insurance	0.430***	0.534***	0.536***
	(0.0398)	(0.0504)	(0.0826)
Real estate	-0.0175	0.0829*	0.169**
	(0.0363)	(0.0474)	(0.0781)
Scientific/technic and admin support	0.116***	0.242***	0.285***
	(0.0344)	(0.0461)	(0.0777)
Public admin/teaching/health/social	-0.230***	-0.0807*	-0.0105
	(0.0371)	(0.0481)	(0.0788)
Miscellaneous services	-0.0618	0.0870*	0.137*
	(0.0422)	(0.0505)	(0.0813)
Log(workforce size) year before election	0.0155***	0.0167***	0.0223***
	(0.00286)	(0.00287)	(0.00299)
Constant	3.370***	3.266***	3.211***
	(0.0355)	(0.0469)	(0.0780)
Observations	40,175	32,384	24,258
R-squared	0.069	0.072	0.066

Robust Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Average wage = gross annual wage bill divided by workforce size (full-time equivalents)
In 2009 terms
Year fixed effects = election year
Agriculture and 2009 excluded categories
Standard errors clustered at the firm level
-
Data: INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 30: OLS regression to predict log of value-added per worker

VARIABLES	(1) Log of value-added per worker 1-yr after election	(2) Log of value-added per worker 2-yrs after election	(3) Log of value-added per worker 3-yrs after election
2010	-0.00261	-0.0383***	-0.0337**
	(0.0151)	(0.0147)	(0.0148)
2011	-0.00955	-0.0455***	-0.0375***
	(0.0148)	(0.0142)	(0.0145)
2012	-0.0240	-0.0405**	-0.0263
	(0.0163)	(0.0159)	(0.0162)
2013	0.00749	-0.0122	0.0482***
	(0.0147)	(0.0144)	(0.0150)
2014	-0.00453	0.0154	-0.0640***
	(0.0146)	(0.0145)	(0.0246)
2015	0.0184	-0.0489*	
	(0.0148)	(0.0265)	
2016	-0.0570*		
	(0.0305)		
Industry	0.215***	0.217**	0.239**
	(0.0723)	(0.0841)	(0.104)
Construction	0.104	0.0990	0.129
	(0.0727)	(0.0845)	(0.104)
Trade/transport/hospitality	0.0699	0.0654	0.0876
	(0.0723)	(0.0841)	(0.104)
Info/communication	0.473***	0.497***	0.504***
	(0.0747)	(0.0864)	(0.107)
Finance/insurance	0.552***	0.569***	0.553***
	(0.0875)	(0.101)	(0.121)
Real estate	1.096***	1.094***	1.130***
	(0.0778)	(0.0886)	(0.108)
Scientific/technic and admin support	0.118	0.133	0.154
	(0.0732)	(0.0849)	(0.105)
Public admin/teaching/health/social	-0.252***	-0.195**	-0.165
	(0.0750)	(0.0864)	(0.106)
Miscellaneous services	-0.0843	-0.0700	-0.00514
	(0.0802)	(0.0914)	(0.110)
Log(workforce size) year before election	0.0440***	0.0415***	0.0478***
	(0.00402)	(0.00422)	(0.00452)
Constant	3.713***	3.750***	3.701***
	(0.0748)	(0.0863)	(0.106)
Observations	39,324	31,684	23,717
R-squared	0.091	0.095	0.091

Robust Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Average value-added by worker = gross annual value-added divided by workforce size (full-time equivalents)
In 2009 terms
Year fixed effects = election year
Agriculture and 2009 excluded categories
-
Data: INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 29: OLS regression to predict log of share of wages in value-added

VARIABLES	(1) Log of share of wages in value-added 1-yr after election	(2) Log of share of wages in value-added 2-yrs after election	(3) Log of share of wages in value-added 3-yrs after election
2010	0.00971	0.0391***	0.0157
	(0.00973)	(0.0112)	(0.0115)
2011	0.0329***	0.0421***	0.0262**
	(0.00974)	(0.0111)	(0.0111)
2012	0.0637***	0.0676***	0.0361***
	(0.0113)	(0.0124)	(0.0119)
2013	0.0435***	0.0610***	0.0250**
	(0.0106)	(0.0115)	(0.0113)
2014	0.0390***	0.0472***	0.00261
	(0.00970)	(0.0110)	(0.0125)
2015	0.0495***	0.0523***	
	(0.0100)	(0.0127)	
2016	0.0454***		
	(0.0140)		
Industry	-0.160***	-0.0714	-0.0575
	(0.0551)	(0.0596)	(0.0706)
Construction	-0.0540	0.0349	0.0503
	(0.0554)	(0.0599)	(0.0709)
Trade/transport/hospitality	-0.0984*	-0.00341	0.0123
	(0.0551)	(0.0597)	(0.0707)
Info/communication	-0.125**	-0.0429	-0.0114
	(0.0570)	(0.0617)	(0.0730)
Finance/insurance	-0.150**	-0.0964	-0.0728
	(0.0687)	(0.0749)	(0.0875)
Real estate	-1.135***	-1.071***	-1.040***
	(0.0610)	(0.0669)	(0.0758)
Scientific/technic and admin support	-0.0225	0.0593	0.0807
	(0.0553)	(0.0599)	(0.0709)
Public admin/teaching/health/social	-0.00177	0.0742	0.0876
	(0.0555)	(0.0600)	(0.0710)
Miscellaneous services	-0.00319	0.0892	0.0776
	(0.0610)	(0.0653)	(0.0751)
Log(workforce size) year before election	-0.0292***	-0.0263***	-0.0247***
	(0.00267)	(0.00291)	(0.00303)
Constant	-0.327***	-0.436***	-0.440***
	(0.0564)	(0.0612)	(0.0718)
Observations	40,885	34,085	26,343
R-squared	0.097	0.100	0.101

Robust Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Share of wages in value-added = annual gross wage bill divided by annual gross value-added
Year fixed effects = election year
Agriculture and 2009 excluded categories
-
Data: INSEE (FARE 2010-2017, agrifin 2012-2017)

Table 31: OLS regression to predict log of economic profitability

VARIABLES	(1) Log of economic profitability 1-yr after election	(2) Log of economic profitability 2-yrs after election	(3) Log of economic profitability 3-yrs after election
2010	-0.0343	-0.0990***	-0.0203
	(0.0337)	(0.0344)	(0.0347)
2011	-0.0961***	-0.0784**	-0.0121
	(0.0344)	(0.0340)	(0.0343)
2012	-0.0418	-0.0124	0.0515
	(0.0380)	(0.0379)	(0.0385)
2013	-0.0809**	-0.0258	0.0550
	(0.0343)	(0.0340)	(0.0340)
2014	-0.0771**	-0.0693**	-0.0558
	(0.0330)	(0.0329)	(0.0386)
2015	-0.102***	-0.192***	
	(0.0332)	(0.0388)	
2016	-0.125***		
	(0.0479)		
Industry	0.515***	0.473***	0.699***
	(0.0998)	(0.109)	(0.131)
Construction	1.183***	1.077***	1.314***
	(0.105)	(0.115)	(0.136)
Trade/transport/hospitality	0.803***	0.770***	0.968***
	(0.100)	(0.109)	(0.131)
Info/communication	0.990***	0.925***	1.158***
	(0.111)	(0.121)	(0.143)
Finance/insurance	0.263*	0.249	0.703***
	(0.142)	(0.154)	(0.180)
Real estate	-0.398***	-0.417***	-0.192
	(0.107)	(0.116)	(0.139)
Scientific/technic and admin support	1.154***	1.119***	1.346***
	(0.104)	(0.113)	(0.135)
Public admin/teaching/health/social	0.718***	0.658***	0.862***
	(0.102)	(0.112)	(0.134)
Miscellaneous services	0.639***	0.575***	0.738***
	(0.113)	(0.123)	(0.146)
Log(workforce size) year before election	-0.121***	-0.117***	-0.133***
	(0.00783)	(0.00838)	(0.00905)
Constant	-2.068***	-2.090***	-2.300***
	(0.106)	(0.114)	(0.136)
Observations	30,285	25,417	19,820
R-squared	0.058	0.058	0.063

Robust Standard errors clustered at the firm level in parentheses

*** p<0.01, ** p<0.05, * p<0.1

-

Economic profitability = value-added + net taxes – gross wages

In 2009 terms

Year fixed effects = election year

Agriculture and 2009 excluded categories

-

Data: INSEE (FARE 2010-2017, agrifin 2012-2017)

8.3.2 Quality of the predictions used to test balance of pre-determined characteristics

Figure 11: Predicted data plotted against original data

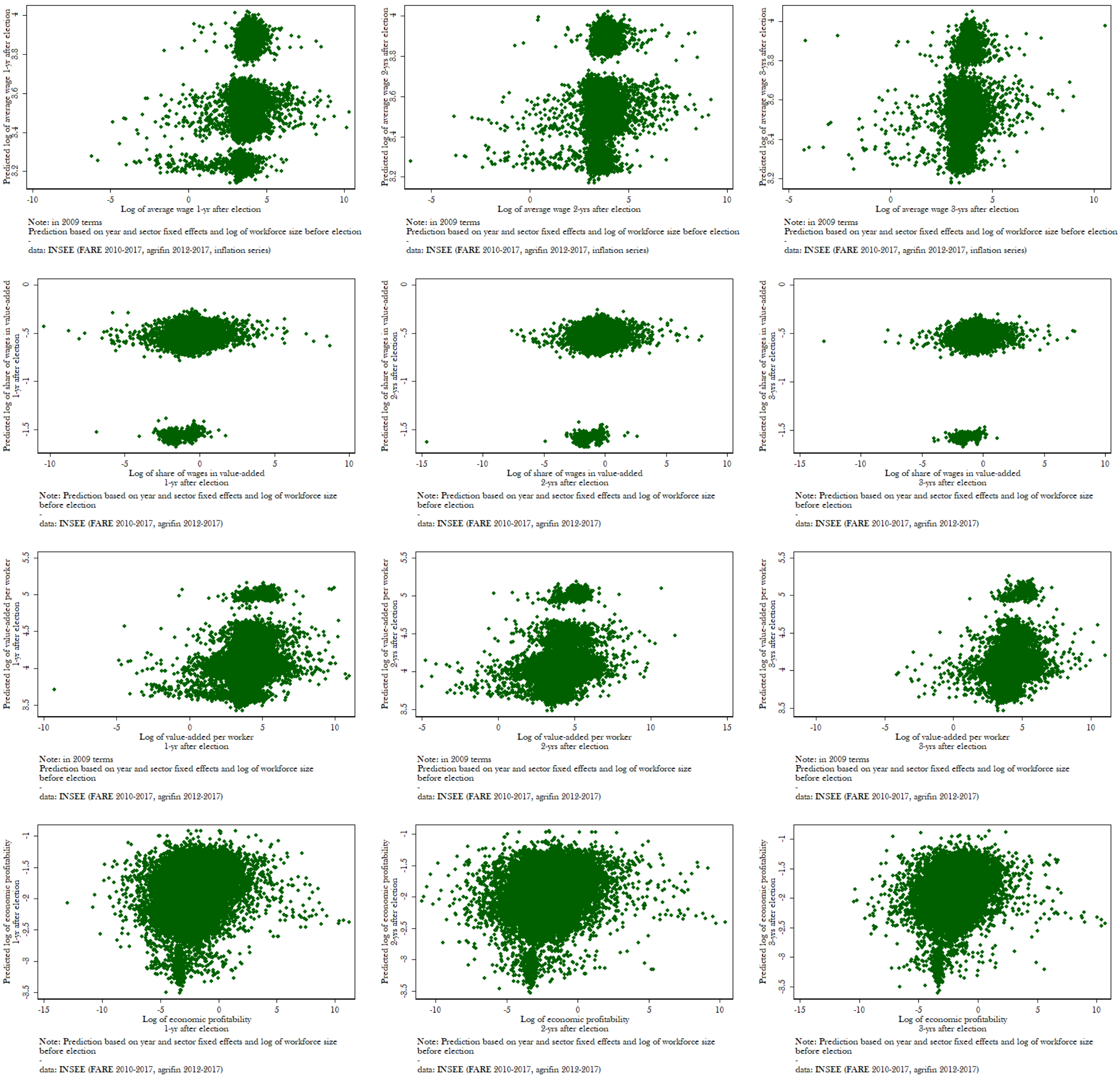
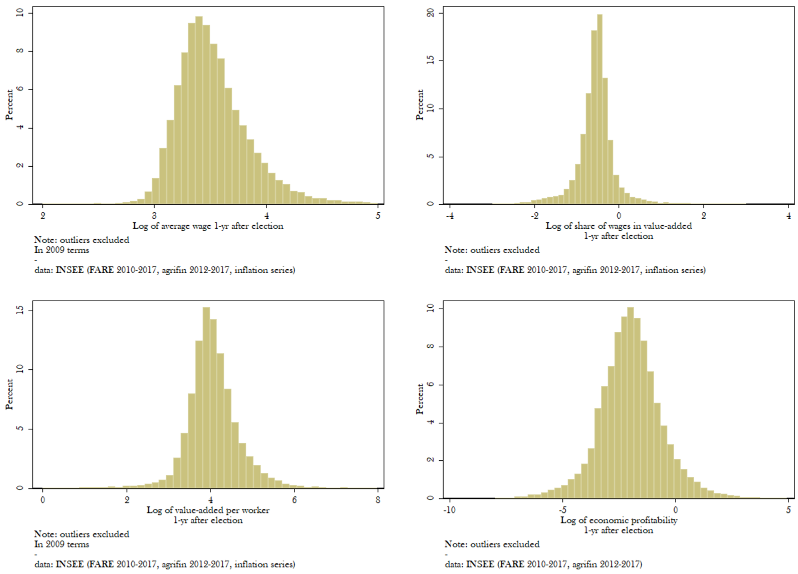


Figure 12: Density of original data (1 year after the election)



8.3.3 Tests of balance of pre-determined characteristics

Table 32: Test of balance of pre-determined characteristics: Second union, cut-off: 10%

Running variable: score of the second-ranked union Cut-off: 0.1						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	-0.0149	-0.0165	-0.0455*	-0.0263	-0.0405	-0.0767*
	(0.0205)	(0.0206)	(0.0246)	(0.0310)	(0.0329)	(0.0430)
Conventional p-value	.467	.423	.064	.397	.218	.074
Obs left of cut-off	265	210	135	264	208	133
Obs right of cut-off	643	520	339	642	519	338
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.0131	0.0220	0.0182	0.0619	0.0727	0.0641
	(0.0201)	(0.0214)	(0.0305)	(0.0531)	(0.0577)	(0.0796)
Conventional p-value	.513	.304	.55	.244	.207	.421
Obs left of cut-off	267	231	156	214	185	125
Obs right of cut-off	669	556	370	522	419	285
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects
and the log of workforce size before the election
Wage and value-added in 2009 terms
Outliers excluded
-
Standard errors clustered at the firm level
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 33: Test of balance of pre-determined characteristics - pre-electoral alliances and scores at the cut-off excluded (donut) - Second union, cut-off: 10%

Running variable: score of the second-ranked union Cut-off: 0.1						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	-0.0110	-0.00955	-0.0403*	-0.0221	-0.0323	-0.0670
	(0.0211)	(0.0209)	(0.0235)	(0.0328)	(0.0351)	(0.0445)
Conventional p-value	.668	.647	.087	.501	.356	.133
Obs left of cut-off	255	202	129	254	200	128
Obs right of cut-off	592	473	306	591	473	305
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.0120	0.0212	0.0142	0.0732	0.0856	0.0696
	(0.0217)	(0.0232)	(0.0332)	(0.0554)	(0.0615)	(0.0826)
Conventional p-value	.581	.362	.668	.186	.164	.399
Obs left of cut-off	256	223	150	207	177	120
Obs right of cut-off	616	507	332	483	382	255
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects
and the log of workforce size before the election
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances and scores at the cut-off (=donut) excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 34: Test of balance of pre-determined characteristics - Third union, cut-off: 10%

Running variable: score of the third-ranked union Cut-off: 0.1						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	-0.00516	-0.00712	-0.0182	-0.0213	-0.0351	-0.0773**
	(0.0120)	(0.0131)	(0.0155)	(0.0251)	(0.0290)	(0.0381)
Conventional p-value	.668	.587	.24	.396	.226	.043
Obs left of cut-off	813	639	383	807	636	382
Obs right of cut-off	1456	1185	708	1450	1182	705
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.0138	0.0282	0.0497	0.0216	0.0469	0.0904
	(0.0223)	(0.0263)	(0.0364)	(0.0436)	(0.0480)	(0.0635)
Conventional p-value	.535	.283	.172	.621	.33	.155
Obs left of cut-off	845	704	424	630	523	333
Obs right of cut-off	1506	1263	773	1161	969	585
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects
and the log of workforce size before the election
Wage and value-added in 2009 terms
Outliers excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 35: Test of balance of pre-determined characteristics - pre-electoral alliances and score at the cut-off (donut) excluded - Third union, cut-off:10%

	Running variable: score of the third-ranked union Cut-off: 0.1					
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	-0.00622	-0.00561	-0.0121	-0.0248	-0.0357	-0.0749*
	(0.0122)	(0.0133)	(0.0152)	(0.0267)	(0.0309)	(0.0402)
Conventional p-value	.61	.672	.426	.352	.248	.062
Obs left of cut-off	750	589	349	746	587	348
Obs right of cut-off	1283	1040	624	1277	1039	621
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.0165	0.0304	0.0539	0.0318	0.0595	0.0939
	(0.0242)	(0.0281)	(0.0395)	(0.0453)	(0.0503)	(0.0670)
Conventional p-value	.496	.28	.173	.483	.237	.161
Obs left of cut-off	779	653	385	588	487	304
Obs right of cut-off	1327	1110	682	1020	844	516
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects
and the log of workforce size before the election
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances and scores at the cut-off (=donut) excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 36: Test of balance of pre-determined characteristics - Fourth union, cut-off: 10%

Running variable: score of the fourth-ranked union Cut-off: 0.1						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	-0.000682	-0.00236	-0.00410	-0.0162	-0.0225	-0.0205
	(0.0114)	(0.0136)	(0.0171)	(0.0184)	(0.0213)	(0.0269)
Conventional p-value	.952	.862	.811	.379	.292	.447
Obs left of cut-off	1041	846	544	1037	845	541
Obs right of cut-off	1228	1004	625	1216	996	621
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.0160	0.0214	0.0175	0.0270	0.0490	0.0556
	(0.0138)	(0.0159)	(0.0177)	(0.0329)	(0.0374)	(0.0441)
Conventional p-value	.245	.176	.321	.411	.19	.207
Obs left of cut-off	1083	905	592	828	678	419
Obs right of cut-off	1276	1066	678	943	784	512
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects
and the log of workforce size before the election
Wage and value-added in 2009 terms
Outliers excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 37: Test of balance of pre-determined characteristics - Fifth union, cut-off: 10%

Running variable: score of the fifth-ranked union Cut-off: 0.1						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	-0.0144	-0.0118	0.00904	-0.0145	-0.0233	0.00182
	(0.0183)	(0.0195)	(0.0254)	(0.0292)	(0.0322)	(0.0421)
Conventional p-value	.431	.546	.722	.62	.469	.966
Obs left of cut-off	649	537	336	645	534	336
Obs right of cut-off	446	373	241	444	370	237
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.00179	0.00541	0.00650	0.0261	0.0157	-0.000253
	(0.0225)	(0.0269)	(0.0335)	(0.0528)	(0.0583)	(0.0720)
Conventional p-value	.937	.84	.846	.621	.787	.997
Obs left of cut-off	675	560	363	503	413	274
Obs right of cut-off	460	398	262	353	304	201
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects
and the log of workforce size before the election
Wage and value-added in 2009 terms
Outliers excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 38: Test of balance of pre-determined characteristics - First union, cut-off: 30%

Running variable: score of the first-ranked union Cut-off: 0.3						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	-0.0130	-0.0118	-0.0118	-0.0458	-0.0530	-0.0143
	(0.0230)	(0.0233)	(0.0298)	(0.0413)	(0.0449)	(0.0597)
Conventional p-value	.571	.612	.693	.268	.238	.811
Obs left of cut-off	225	192	119	224	191	119
Obs right of cut-off	553	457	280	550	455	279
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.0305	0.0417	0.00814	0.0822	0.122	-0.00550
	(0.0341)	(0.0391)	(0.0495)	(0.0721)	(0.0812)	(0.101)
Conventional p-value	.371	.287	.869	.254	.135	.956
Obs left of cut-off	235	196	129	183	154	102
Obs right of cut-off	571	480	315	426	352	232
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects
and the log of workforce size before the election
Wage and value-added in 2009 terms
Outliers excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 39: Test of balance of pre-determined characteristics - Second union, 30%

Running variable: score of the second-ranked union Cut-off: 0.3						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	0.00459	0.00313	0.00475	-0.00618	-0.0119	-0.00875
	(0.00733)	(0.00823)	(0.0104)	(0.0136)	(0.0157)	(0.0193)
Conventional p-value	.531	.704	.648	.649	.449	.65
Obs left of cut-off	2592	2113	1254	2581	2097	1249
Obs right of cut-off	2700	2165	1318	2679	2158	1311
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.0116	0.0160	0.0171	0.0226	0.0396	0.0108
	(0.0111)	(0.0131)	(0.0172)	(0.0257)	(0.0274)	(0.0366)
Conventional p-value	.296	.221	.323	.378	.147	.769
Obs left of cut-off	2675	2240	1422	2008	1696	1083
Obs right of cut-off	2756	2319	1485	2102	1769	1136
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects
and the log of workforce size before the election
Wage and value-added in 2009 terms
Outliers excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 40: Test of balance of pre-determined characteristics - Third union, cut-off: 30%

Running variable: score of the third-ranked union Cut-off: 0.3						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	-0.00451	0.00272	-0.00363	-0.00136	0.0148	-0.000279
	(0.0177)	(0.0200)	(0.0215)	(0.0276)	(0.0312)	(0.0352)
Conventional p-value	.799	.892	.866	.961	.635	.994
Obs left of cut-off	989	827	493	987	826	490
Obs right of cut-off	475	391	241	475	390	242
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	-0.00371	-0.00925	-0.0177	0.00287	-0.0458	-0.0129
	(0.0205)	(0.0240)	(0.0289)	(0.0490)	(0.0531)	(0.0678)
Conventional p-value	.856	.700	.54	.953	.389	.849
Obs left of cut-off	1023	884	561	768	668	418
Obs right of cut-off	492	418	268	382	322	209
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects
and the log of workforce size before the election
Wage and value-added in 2009 terms
Outliers excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

8.3.4 Placebo using lagged values of the variables of interest

Table 41: Placebo: lagged variable of interest used as dependent variable - Second union, cut-off: 10%

Running variable: score of the second-ranked union Cut-off: 0.1						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Log of average wage 1-yr before	Growth rate of average wage 1-yr before	Value-added by worker 1-yr before	Log of value-added by worker 1-yr before	Increase in value-added by worker 1-yr before	Growth rate of value-added by worker 1-yr before
RD_Estimate	0.0608	0.00502	9.173	0.0374	-2.872	-0.0394
	(0.0478)	(0.0247)	(6.298)	(0.0734)	(4.049)	(0.0427)
Conventional p-value	.203	.839	.145	.61	.478	.356
Obs left of cut-off	289	264	289	287	264	263
Obs right of cut-off	681	631	682	680	632	626
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Log of share of wages in value-added 1-yr before	Growth rate of share of wages in value-added 1-yr before	Economic profitability 1-yr before	Log of economic profitability 1-yr before	Increase in economic profitability 1-yr before	Growth rate of economic profitability 1-yr before
RD_Estimate	0.0138	0.108**	-0.0785	0.0388	-0.0631	1.960
	(0.0505)	(0.0520)	(0.0862)	(0.234)	(0.0718)	(3.407)
Conventional p-value	.784	.038	.362	.868	.38	.565
Obs left of cut-off	289	267	293	229	270	192
Obs right of cut-off	684	632	687	513	636	421
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: respectively growth rate and increase wrt values the year before the election
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances excluded, donut
-
Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2015, agrifin 2012-2015, inflation series)

Table 42: Placebo: lagged variable of interest used as dependent variable - Third union, cut-off: 10%

Running variable: score of the third-ranked union Cut-off: 0.1						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Log of average wage 1-yr before	Growth rate of average wage 1-yr before	Value-added by worker 1-yr before	Log of value-added by worker 1-yr before	Increase in value-added by worker 1-yr before	Growth rate of value-added by worker 1-yr before
RD_Estimate	0.0119	0.0114	0.0191	0.0546	2.850*	0.0579**
	(0.0271)	(0.0127)	(4.507)	(0.0523)	(1.551)	(0.0244)
Conventional p-value	.661	.368	.997	.297	.066	.018
Obs left of cut-off	839	753	842	835	758	748
Obs right of cut-off	1439	1316	1445	1431	1319	1303
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Log of share of wages in value-added 1-yr before	Growth rate of share of wages in value-added 1-yr before	Economic profitability 1-yr before	Log of economic profitability 1-yr before	Increase in economic profitability 1-yr before	Growth rate of economic profitability 1-yr before
RD_Estimate	-0.0396	-0.0353	0.00102	-0.0601	0.0409	0.576
	(0.0406)	(0.0264)	(0.0538)	(0.136)	(0.0528)	(0.358)
Conventional p-value	.33	.18	.985	.659	.438	.108
Obs left of cut-off	837	751	842	644	761	517
Obs right of cut-off	1434	1311	1445	1100	1325	907
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: respectively growth rate and increase wrt values the year before the election
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances excluded, donut
-
Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2015, agrifin 2012-2015, inflation series)

Table 43: Placebo: lagged variable of interest used as dependent variable - Fourth union, cut-off: 10%

Running variable: score of the fourth-ranked union Cut-off: 0.1						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Log of average wage 1-yr before	Growth rate of average wage 1-yr before	Value-added by worker 1-yr before	Log of value-added by worker 1-yr before	Increase in value-added by worker 1-yr before	Growth rate of value-added by worker 1-yr before
RD_Estimate	0.0561**	0.00561	0.458	0.0258	-0.953	-0.000549
	(0.0277)	(0.0140)	(4.033)	(0.0481)	(1.844)	(0.0283)
Conventional p-value	.043	.688	.91	.593	.605	.985
Obs left of cut-off	1023	908	1026	1018	912	901
Obs right of cut-off	1150	1046	1154	1140	1047	1035
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Log of share of wages in value-added 1-yr before	Growth rate of share of wages in value-added 1-yr before	Economic profitability 1-yr before	Log of economic profitability 1-yr before	Increase in economic profitability 1-yr before	Growth rate of economic profitability 1-yr before
RD_Estimate	0.0296	0.0248	0.0495	0.00629	0.0282	0.226
	(0.0356)	(0.0276)	(0.0591)	(0.145)	(0.0422)	(0.227)
Conventional p-value	.405	.368	.402	.965	.504	.319
Obs left of cut-off	1022	910	1025	766	918	617
Obs right of cut-off	1142	1042	1156	855	1062	703
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: respectively growth rate and increase wrt values the year before the election
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances excluded, donut
-
Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2015, agrifin 2012-2015, inflation series)

Table 44: Placebo: lagged variable of interest used as dependent variable - Fifth union, cut-off: 10%

Running variable: score of the fifth-ranked union Cut-off: 0.1						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Log of average wage 1-yr before	Growth rate of average wage 1-yr before	Value-added by worker 1-yr before	Log of value-added by worker 1-yr before	Increase in value-added by worker 1-yr before	Growth rate of value-added by worker 1-yr before
RD_Estimate	0.00823	-0.0483	-6.763	-0.0229	-3.188	-0.0355
	(0.0436)	(0.0425)	(7.113)	(0.0762)	(3.494)	(0.0485)
Conventional p-value	.85	.256	.342	.763	.362	.465
Obs left of cut-off	601	535	603	597	537	530
Obs right of cut-off	381	347	382	376	347	344
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Log of share of wages in value-added 1-yr before	Growth rate of share of wages in value-added 1-yr before	Economic profitability 1-yr before	Log of economic profitability 1-yr before	Increase in economic profitability 1-yr before	Growth rate of economic profitability 1-yr before
RD_Estimate	0.0423	-0.00403	-0.00738	0.190	-0.0656	0.203
	(0.0545)	(0.0280)	(0.0680)	(0.243)	(0.0562)	(0.322)
Conventional p-value	.438	.886	.914	.435	.243	.529
Obs left of cut-off	597	535	601	445	542	348
Obs right of cut-off	378	345	385	292	352	246
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: respectively growth rate and increase wrt values the year before the election
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances excluded, donut
-
Year fixed effects and log of number of votes controlled for
Standard errors clustered at the firm level
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2015, agrifin 2012-2015, inflation series)

Table 45: Placebo: lagged variable of interest used as dependent variable - First union, cut-off: 30%

Running variable: score of the first-ranked union Cut-off: 0.3						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Log of average wage 1-yr before	Growth rate of average wage 1-yr before	Value-added by worker 1-yr before	Log of value-added by worker 1-yr before	Increase in value-added by worker 1-yr before	Growth rate of value-added by worker 1-yr before
RD_Estimate	0.0312	-0.0132	0.690	-0.00283	2.116	0.0452
	(0.0458)	(0.0300)	(9.643)	(0.101)	(3.754)	(0.0431)
Conventional p-value	.495	.659	.943	.978	.573	.294
Obs left of cut-off	175	165	176	175	165	164
Obs right of cut-off	443	399	444	436	399	394
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Log of share of wages in value-added 1-yr before	Growth rate of share of wages in value-added 1-yr before	Economic profitability 1-yr before	Log of economic profitability 1-yr before	Increase in economic profitability 1-yr before	Growth rate of economic profitability 1-yr before
RD_Estimate	0.0369	-0.0274	-0.271	-0.185	-0.0128	-0.0747
	(0.0848)	(0.0361)	(0.183)	(0.349)	(0.0869)	(0.376)
Conventional p-value	.664	.448	.14	.595	.883	.842
Obs left of cut-off	174	164	176	136	166	114
Obs right of cut-off	439	396	446	334	401	269
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: respectively growth rate and increase wrt values the year before the election
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances excluded, donut
-
Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2015, agrifin 2012-2015, inflation series)

Table 46: Placebo: lagged variable of interest used as dependent variable - Second union, cut-off: 30%

Running variable: score of the second-ranked union Cut-off: 0.3						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Log of average wage 1-yr before	Growth rate of average wage 1-yr before	Value-added by worker 1-yr before	Log of value-added by worker 1-yr before	Increase in value-added by worker 1-yr before	Growth rate of value-added by worker 1-yr before
RD_Estimate	0.00756	-0.00584	1.482	0.0126	-0.503	-0.00798
	(0.0174)	(0.00860)	(2.644)	(0.0310)	(1.175)	(0.0141)
Conventional p-value	.665	.497	.575	.683	.668	.572
Obs left of cut-off	2651	2419	2660	2639	2430	2408
Obs right of cut-off	2741	2504	2752	2720	2516	2486
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Log of share of wages in value-added 1-yr before	Growth rate of share of wages in value-added 1-yr before	Economic profitability 1-yr before	Log of economic profitability 1-yr before	Increase in economic profitability 1-yr before	Growth rate of economic profitability 1-yr before
RD_Estimate	-0.00376	0.0163	-0.0293	0.00615	0.0117	0.127
	(0.0240)	(0.0150)	(0.0372)	(0.0900)	(0.0248)	(0.583)
Conventional p-value	.876	.278	.431	.946	.636	.827
Obs left of cut-off	2644	2411	2660	2036	2426	1707
Obs right of cut-off	2723	2492	2745	2083	2514	1727
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: respectively growth rate and increase wrt values the year before the election
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances excluded, donut
-
Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2015, agrifin 2012-2015, inflation series)

Table 47: Placebo: lagged variable of interest used as dependent variable - Third union, cut-off: 30%

Running variable: score of the third-ranked union Cut-off: 0.3						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Log of average wage 1-yr before	Growth rate of average wage 1-yr before	Value-added by worker 1-yr before	Log of value-added by worker 1-yr before	Increase in value-added by worker 1-yr before	Growth rate of value-added by worker 1-yr before
RD_Estimate	-0.0148	-0.00278	4.258	0.0738	0.887	-0.00662
	(0.0334)	(0.0239)	(6.172)	(0.0862)	(2.787)	(0.0327)
Conventional p-value	.659	.907	.49	.392	.75	.84
Obs left of cut-off	931	853	936	928	857	853
Obs right of cut-off	406	369	407	403	371	366
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Log of share of wages in value-added 1-yr before	Growth rate of share of wages in value-added 1-yr before	Economic profitability 1-yr before	Log of economic profitability 1-yr before	Increase in economic profitability 1-yr before	Growth rate of economic profitability 1-yr before
RD_Estimate	-0.0187	0.0421	-0.0935	-0.00353	-0.102*	-1.224
	(0.0526)	(0.0323)	(0.0814)	(0.183)	(0.0586)	(0.791)
Conventional p-value	.723	.193	.251	.985	.08	.122
Obs left of cut-off	927	854	936	703	862	589
Obs right of cut-off	403	367	406	317	370	265
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: respectively growth rate and increase wrt values the year before the election
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances excluded, donut
-
Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2015, agrifin 2012-2015, inflation series)

Table 48: Placebo: lagged variable of interest used as dependent variable - First union, cut-off: 50%

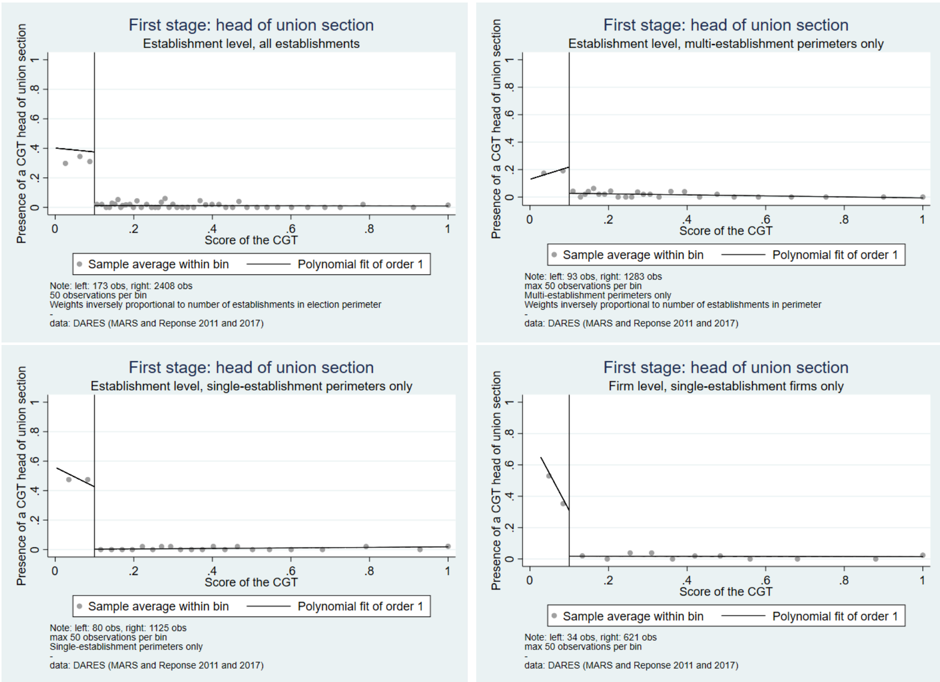
Running variable: score of the first-ranked union Cut-off: 0.5						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Log of average wage 1-yr before	Growth rate of average wage 1-yr before	Value-added by worker 1-yr before	Log of value-added by worker 1-yr before	Increase in value-added by worker 1-yr before	Growth rate of value-added by worker 1-yr before
RD_Estimate	0.0327	0.00934	4.405	0.0491	1.392	0.0273
	(0.0277)	(0.0138)	(4.080)	(0.0508)	(1.765)	(0.0262)
Conventional p-value	.239	.498	.28	.334	.43	.299
Obs left of cut-off	890	821	894	886	824	817
Obs right of cut-off	1866	1711	1874	1857	1719	1701
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Log of share of wages in value-added 1-yr before	Growth rate of share of wages in value-added 1-yr before	Economic profitability 1-yr before	Log of economic profitability 1-yr before	Increase in economic profitability 1-yr before	Growth rate of economic profitability 1-yr before
RD_Estimate	-0.0207	-0.00550	-0.103*	-0.413***	-0.0673	-4.034
	(0.0384)	(0.0249)	(0.0597)	(0.143)	(0.0462)	(5.109)
Conventional p-value	.59	.825	.085	.004	.146	.43
Obs left of cut-off	887	820	895	665	830	553
Obs right of cut-off	1859	1704	1869	1457	1714	1209
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: respectively growth rate and increase wrt values the year before the election
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances excluded, donut
-
Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2015, agrifin 2012-2015, inflation series)

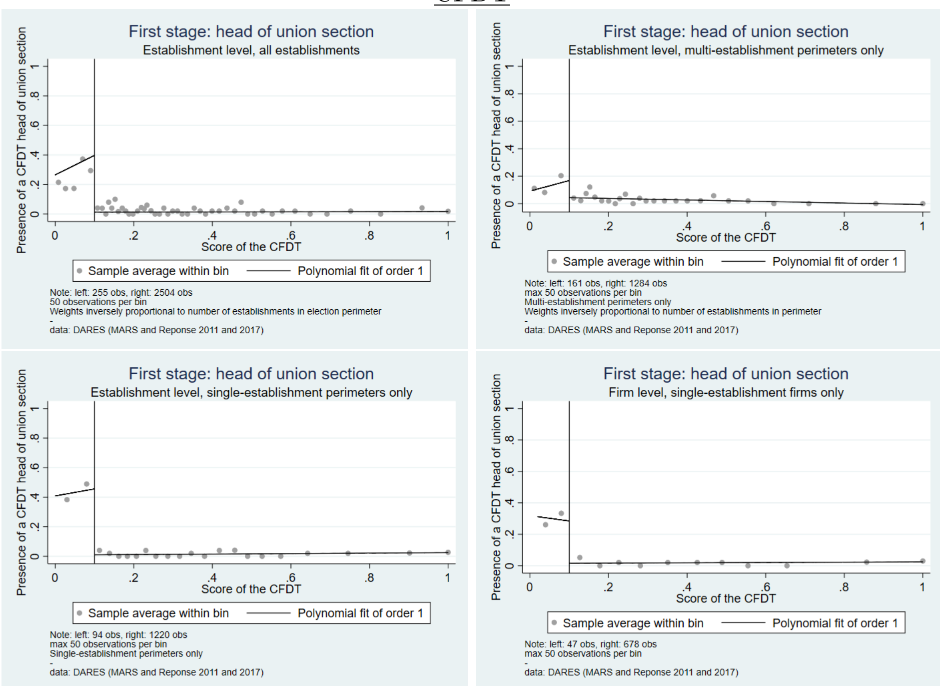
8.4 First stage: presence of a head of union section

Figure 13: First stage: presence of a head of union section, union-specific

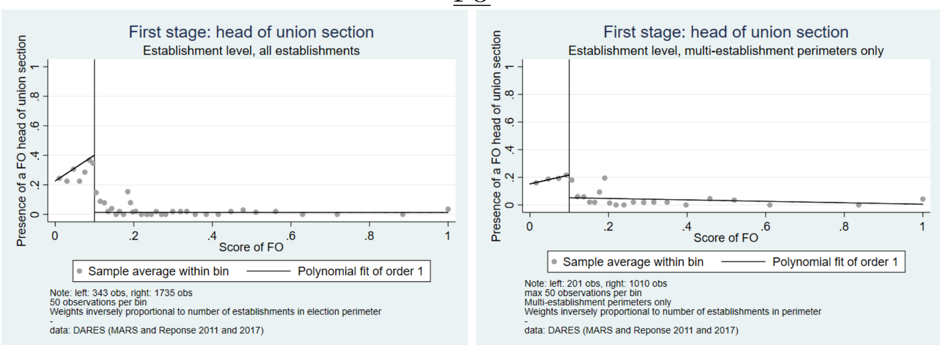
CGT

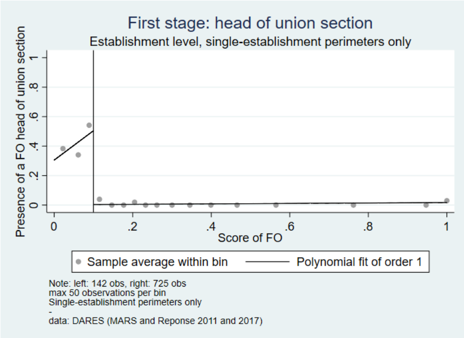


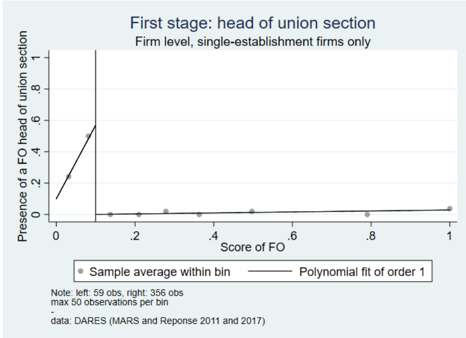
CFDT



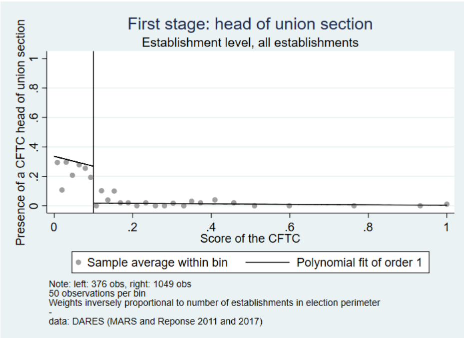
FO

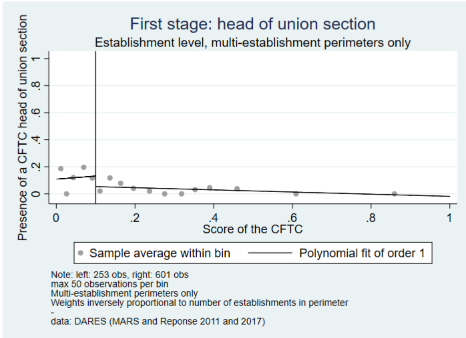


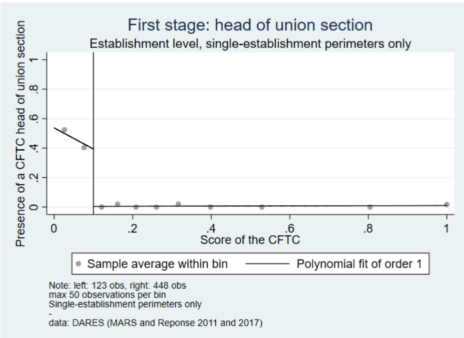

First stage: head of union section
Establishment level, single-establishment perimeters only
Presence of a FO head of union section
Score of FO
Sample average within bin
Polynomial fit of order 1
Note: left: 142 obs, right: 725 obs
max 50 observations per bin
Single-establishment perimeters only
data: DARES (MARS and Reponse 2011 and 2017)

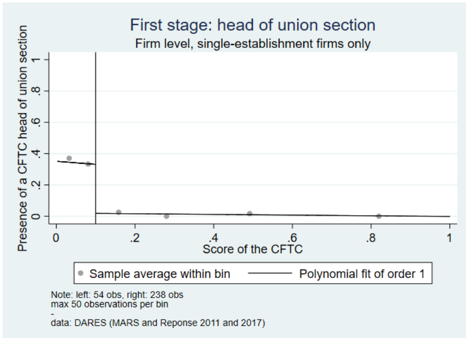

First stage: head of union section
Firm level, single-establishment firms only
Presence of a FO head of union section
Score of FO
Sample average within bin
Polynomial fit of order 1
Note: left: 59 obs, right: 356 obs
max 50 observations per bin
data: DARES (MARS and Reponse 2011 and 2017)

CFTC

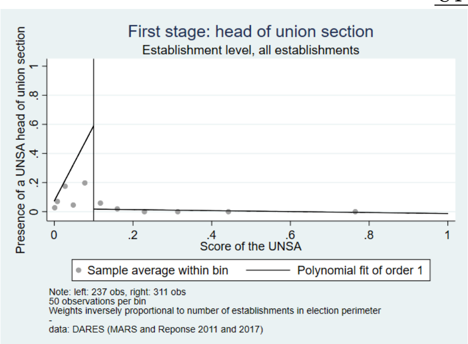

First stage: head of union section
Establishment level, all establishments
Presence of a CFTC head of union section
Score of the CFTC
Sample average within bin
Polynomial fit of order 1
Note: left: 376 obs, right: 1049 obs
50 observations per bin
Weights inversely proportional to number of establishments in election perimeter
data: DARES (MARS and Reponse 2011 and 2017)

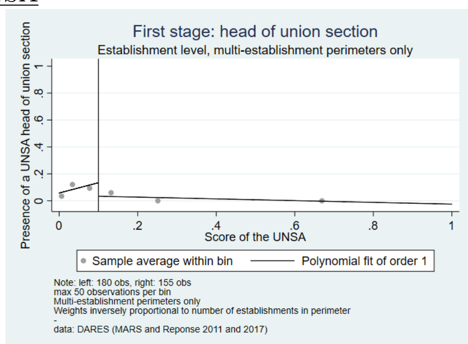

First stage: head of union section
Establishment level, multi-establishment perimeters only
Presence of a CFTC head of union section
Score of the CFTC
Sample average within bin
Polynomial fit of order 1
Note: left: 253 obs, right: 601 obs
max 50 observations per bin
Multi-establishment perimeters only
Weights inversely proportional to number of establishments in perimeter
data: DARES (MARS and Reponse 2011 and 2017)

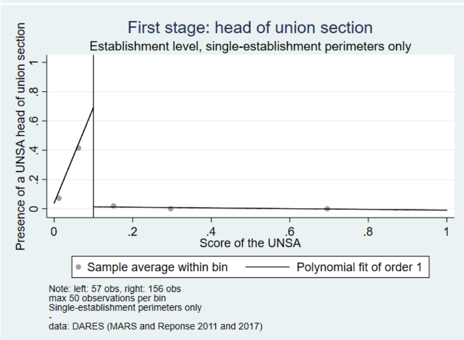

First stage: head of union section
Establishment level, single-establishment perimeters only
Presence of a CFTC head of union section
Score of the CFTC
Sample average within bin
Polynomial fit of order 1
Note: left: 123 obs, right: 448 obs
max 50 observations per bin
Single-establishment perimeters only
data: DARES (MARS and Reponse 2011 and 2017)

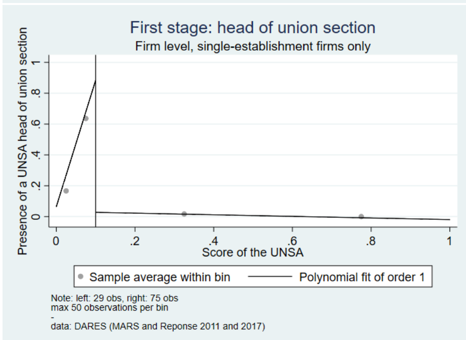

First stage: head of union section
Firm level, single-establishment firms only
Presence of a CFTC head of union section
Score of the CFTC
Sample average within bin
Polynomial fit of order 1
Note: left: 54 obs, right: 238 obs
max 50 observations per bin
data: DARES (MARS and Reponse 2011 and 2017)

UNSA

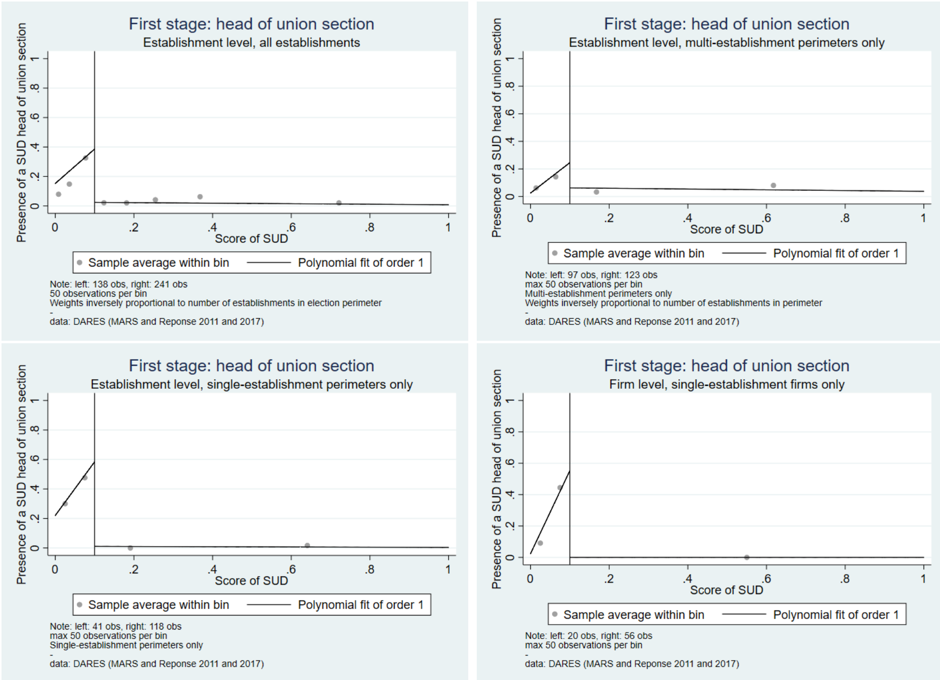

First stage: head of union section
Establishment level, all establishments
Presence of a UNSA head of union section
Score of the UNSA
Sample average within bin
Polynomial fit of order 1
Note: left: 237 obs, right: 311 obs
50 observations per bin
Weights inversely proportional to number of establishments in election perimeter
data: DARES (MARS and Reponse 2011 and 2017)


First stage: head of union section
Establishment level, multi-establishment perimeters only
Presence of a UNSA head of union section
Score of the UNSA
Sample average within bin
Polynomial fit of order 1
Note: left: 180 obs, right: 155 obs
max 50 observations per bin
Multi-establishment perimeters only
Weights inversely proportional to number of establishments in perimeter
data: DARES (MARS and Reponse 2011 and 2017)

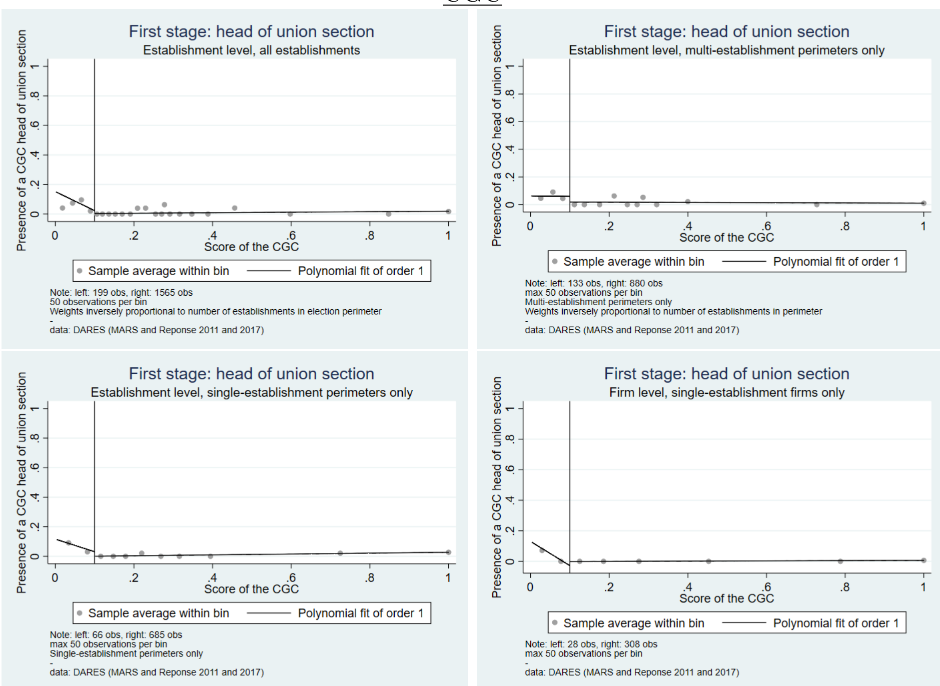

First stage: head of union section
Establishment level, single-establishment perimeters only
Presence of a UNSA head of union section
Score of the UNSA
Sample average within bin
Polynomial fit of order 1
Note: left: 57 obs, right: 156 obs
max 50 observations per bin
Single-establishment perimeters only
data: DARES (MARS and Reponse 2011 and 2017)


First stage: head of union section
Firm level, single-establishment firms only
Presence of a UNSA head of union section
Score of the UNSA
Sample average within bin
Polynomial fit of order 1
Note: left: 29 obs, right: 75 obs
max 50 observations per bin
data: DARES (MARS and Reponse 2011 and 2017)

SUD


First stage: head of union section
Establishment level, all establishments
Presence of a SUD head of union section
Score of SUD
Sample average within bin
Polynomial fit of order 1
Note: left: 138 obs, right: 241 obs
50 observations per bin
Weights inversely proportional to number of establishments in election perimeter
data: DARES (MARS and Reponse 2011 and 2017)
First stage: head of union section
Establishment level, multi-establishment perimeters only
Note: left: 97 obs, right: 123 obs
max 50 observations per bin
Multi-establishment perimeters only
Weights inversely proportional to number of establishments in perimeter
data: DARES (MARS and Reponse 2011 and 2017)
First stage: head of union section
Establishment level, single-establishment perimeters only
Note: left: 41 obs, right: 118 obs
max 50 observations per bin
Single-establishment perimeters only
data: DARES (MARS and Reponse 2011 and 2017)
First stage: head of union section
Firm level, single-establishment firms only
Note: left: 20 obs, right: 56 obs
max 50 observations per bin
data: DARES (MARS and Reponse 2011 and 2017)

CGC


First stage: head of union section
Establishment level, all establishments
Presence of a CGC head of union section
Score of the CGC
Sample average within bin
Polynomial fit of order 1
Note: left: 199 obs, right: 1565 obs
50 observations per bin
Weights inversely proportional to number of establishments in election perimeter
data: DARES (MARS and Reponse 2011 and 2017)
First stage: head of union section
Establishment level, multi-establishment perimeters only
Note: left: 133 obs, right: 880 obs
max 50 observations per bin
Multi-establishment perimeters only
Weights inversely proportional to number of establishments in perimeter
data: DARES (MARS and Reponse 2011 and 2017)
First stage: head of union section
Establishment level, single-establishment perimeters only
Note: left: 66 obs, right: 685 obs
max 50 observations per bin
Single-establishment perimeters only
data: DARES (MARS and Reponse 2011 and 2017)
First stage: head of union section
Firm level, single-establishment firms only
Note: left: 28 obs, right: 308 obs
max 50 observations per bin
data: DARES (MARS and Reponse 2011 and 2017)

8.5 Results - dependent variables in log or level

8.5.1 10% threshold

Table 49: Effect of an additional bargaining union (ITT) on the share of wages in the value-added

	Share of wages in the value-added Cut-off: 0.1				
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Growth rate of share of wages in value-added 1-yr before	Log of share of wages in value-added 1-yr before	Log of share of wages in value-added 1-yr after	Log of share of wages in value-added 2-yrs after	Log of share of wages in value-added 3-yrs after
	Running variable: score of the second-ranked union				
RD_Estimate	0.113**	-0.00408	0.00338	0.0141	0.0181
	(0.0550)	(0.0534)	(0.0586)	(0.0650)	(0.0786)
Conventional p-value	.04	.939	.954	.828	.818
Obs left of cut-off	233	252	235	204	143
Obs right of cut-off	573	619	578	479	320
	Running variable: score of the third-ranked union				
RD_Estimate	-0.0449	-0.0390	-0.00363	0.0269	0.0373
	(0.0279)	(0.0444)	(0.0449)	(0.0540)	(0.0717)
Conventional p-value	.108	.38	.935	.619	.603
Obs left of cut-off	642	717	687	585	351
Obs right of cut-off	1132	1239	1183	998	633
	Running variable: score of the fourth-ranked union				
RD_Estimate	-0.0166	0.00499	0.00241	-0.0736	-0.0310
	(0.0238)	(0.0391)	(0.0410)	(0.0453)	(0.0585)
Conventional p-value	.484	.898	.953	.104	.596
Obs left of cut-off	787	888	842	710	474
Obs right of cut-off	906	995	938	806	549
	Running variable: score of the fifth-ranked union				
RD_Estimate	-0.00146	0.0372	0.0476	0.0766	0.0113
	(0.0301)	(0.0565)	(0.0570)	(0.0650)	(0.0737)
Conventional p-value	.961	.511	.404	.239	.879
Obs left of cut-off	477	530	499	436	301
Obs right of cut-off	323	354	335	301	202
Bandwidth	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: col1: growth during year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017)

Table 50: Effect of an additional bargaining union (ITT) on the share of wages in the value-added

	Value-added by worker Cut-off: 0.1				
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Growth rate of value-added by worker 1-yr before	Log of value-added by worker 1-yr before	Log of value-added by worker 1-yr after	Log of value-added by worker 2-yrs after	Log of value-added by worker 3-yrs after
Running variable: score of the second-ranked union					
RD_Estimate	-0.0224	0.0575	0.0292	0.0149	-0.0526
	(0.0375)	(0.0774)	(0.0849)	(0.0981)	(0.117)
Conventional p-value	.551	.457	.731	.879	.653
Obs left of cut-off	229	250	233	185	123
Obs right of cut-off	567	615	555	450	298
Running variable: score of the third-ranked union					
RD_Estimate	0.0688**	0.0662	0.00689	-0.00433	-0.0414
	(0.0270)	(0.0576)	(0.0593)	(0.0727)	(0.0950)
Conventional p-value	.011	.25	.908	.953	.663
Obs left of cut-off	639	715	658	528	320
Obs right of cut-off	1123	1236	1138	935	579
Running variable: score of the fourth-ranked union					
RD_Estimate	0.0229	0.0444	-0.0102	0.120*	0.103
	(0.0312)	(0.0534)	(0.0575)	(0.0633)	(0.0857)
Conventional p-value	.462	.406	.859	.058	.228
Obs left of cut-off	779	884	806	667	438
Obs right of cut-off	900	993	896	753	506
Running variable: score of the fifth-ranked union					
RD_Estimate	-0.0266	-0.0190	-0.148	-0.106	-0.000668
	(0.0524)	(0.0792)	(0.118)	(0.0862)	(0.104)
Conventional p-value	.612	.811	.208	.221	.995
Obs left of cut-off	473	530	478	421	279
Obs right of cut-off	322	352	326	276	180
Bandwidth	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: col1: growth during year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

8.6 Balance of pre-determined characteristics, using lagged dependent variable in prediction

8.6.1 OLS regressions used for prediction

Table 51: OLS regression to predict log of wage - using lagged log of wage

VARIABLES	(1) Log of average wage 1-yr after election	(2) Log of average wage 2-yrs after election	(3) Log of average wage 3-yrs after election
Log(average wage) 1-yr before	0.589***	0.537***	0.503***
	(0.0233)	(0.0253)	(0.0254)
2010	0.0384***	0.0210**	0.00905
	(0.00880)	(0.00861)	(0.00851)
2011	0.0384***	0.00958	0.0112
	(0.00898)	(0.00844)	(0.00847)
2012	0.0423***	0.0342***	0.0324***
	(0.00925)	(0.00919)	(0.00934)
2013	0.0627***	0.0593***	0.0862***
	(0.00935)	(0.00923)	(0.0102)
2014	0.0512***	0.0786***	0.00657
	(0.00862)	(0.00896)	(0.0132)
2015	0.0742***	0.0132	
	(0.00894)	(0.0149)	
2016	0.0421***		
	(0.0149)		
Industry	0.132***	0.250***	0.287***
	(0.0286)	(0.0446)	(0.0717)
Construction	0.125***	0.248***	0.279***
	(0.0290)	(0.0449)	(0.0719)
Trade/transport/hospitality	0.0985***	0.217***	0.252***
	(0.0289)	(0.0448)	(0.0718)
Info/communication	0.231***	0.387***	0.413***
	(0.0299)	(0.0461)	(0.0733)
Finance/insurance	0.241***	0.370***	0.399***
	(0.0324)	(0.0477)	(0.0761)
Real estate	0.0960***	0.207***	0.257***
	(0.0314)	(0.0468)	(0.0726)
Scientific/technic and admin support	0.142***	0.268***	0.300***
	(0.0291)	(0.0450)	(0.0719)
Public admin/teaching/health/social	-0.00866	0.113**	0.142*
	(0.0304)	(0.0457)	(0.0727)
Miscellaneous services	0.0857**	0.217***	0.261***
	(0.0342)	(0.0473)	(0.0745)
Log(workforce size) year before election	0.0193***	0.0206***	0.0244***
	(0.00212)	(0.00220)	(0.00251)
Constant	1.204***	1.277***	1.354***
	(0.0913)	(0.104)	(0.120)
Observations	39,870	32,115	24,042
R-squared	0.443	0.413	0.364

Robust Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Average wage = gross annual wage bill divided by workforce size (full-time equivalents)
In 2009 terms
Year fixed effects = election year
Agriculture and 2009 excluded categories
-
Data: INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 52: OLS regression to predict log of share of wages in value-added - using lagged log of share of wages in value-added

VARIABLES	(1) Log of share of wages in value-added 1-yr after election	(2) Log of share of wages in value-added 2-yrs after election	(3) Log of share of wages in value-added 3-yrs after election
Log(share of wages in value-added) 1-yr before	0.613***	0.574***	0.579***
	(0.0192)	(0.0207)	(0.0198)
2010	-0.0324***	0.00132	-0.0224**
	(0.00774)	(0.00962)	(0.00993)
2011	-0.00470	0.00944	-0.00159
	(0.00787)	(0.00949)	(0.00960)
2012	0.0229**	0.0274**	0.000228
	(0.00897)	(0.0109)	(0.0103)
2013	0.0131	0.0261**	-0.00739
	(0.00907)	(0.0105)	(0.0103)
2014	-0.00918	0.00459	-0.0171*
	(0.00762)	(0.00938)	(0.0103)
2015	-0.00451	0.0199**	
	(0.00777)	(0.0100)	
2016	-0.000328		
	(0.0100)		
Industry	-0.100***	-0.0429	-0.0567
	(0.0365)	(0.0393)	(0.0613)
Construction	-0.0367	0.0308	0.00911
	(0.0368)	(0.0396)	(0.0615)
Trade/transport/hospitality	-0.0795**	-0.0130	-0.0196
	(0.0364)	(0.0394)	(0.0613)
Info/communication	-0.105***	-0.0533	-0.0451
	(0.0379)	(0.0410)	(0.0628)
Finance/insurance	-0.102**	-0.0742	-0.104
	(0.0435)	(0.0500)	(0.0725)
Real estate	-0.471***	-0.467***	-0.473***
	(0.0425)	(0.0478)	(0.0666)
Scientific/technic and admin support	-0.0524	0.00384	-0.00388
	(0.0367)	(0.0396)	(0.0614)
Public admin/teaching/health/social	-0.0511	0.00753	0.00148
	(0.0369)	(0.0397)	(0.0616)
Miscellaneous services	-0.0795**	0.00940	-0.0144
	(0.0401)	(0.0423)	(0.0638)
Log(workforce size) year before election	-0.0104***	-0.00921***	-0.00747***
	(0.00200)	(0.00228)	(0.00255)
Constant	-0.0663*	-0.163***	-0.138**
	(0.0386)	(0.0421)	(0.0631)
Observations	40,261	33,542	25,902
R-squared	0.453	0.382	0.361

Robust Standard errors clustered at the firm level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
-
Share of wages in value-added = annual gross wage bill divided by annual gross value-added
Year fixed effects = election year
Agriculture and 2009 excluded categories
-
Data: INSEE (FARE 2010-2017, agrifin 2012-2017)

Table 53: OLS regression to predict log of value-added by worker - using lagged log of value-added by worker

VARIABLES	(1) Log of value-added per worker 1-yr after election	(2) Log of value-added per worker 2-yrs after election	(3) Log of value-added per worker 3-yrs after election
Log(value-added by worker) 1-yr before	0.686***	0.645***	0.610***
	(0.0128)	(0.0145)	(0.0166)
2010	0.0809***	0.0307***	0.0367***
	(0.0111)	(0.0113)	(0.0121)
2011	0.0495***	0.00717	0.0170
	(0.0117)	(0.0112)	(0.0120)
2012	0.0265**	0.0129	0.0324**
	(0.0123)	(0.0123)	(0.0133)
2013	0.0555***	0.0407***	0.101***
	(0.0121)	(0.0119)	(0.0136)
2014	0.0668***	0.0805***	-0.00266
	(0.0110)	(0.0113)	(0.0197)
2015	0.0888***	0.00794	
	(0.0112)	(0.0191)	
2016	0.0242		
	(0.0216)		
Industry	0.231***	0.307***	0.315***
	(0.0454)	(0.0660)	(0.0849)
Construction	0.173***	0.234***	0.256***
	(0.0458)	(0.0665)	(0.0852)
Trade/transport/hospitality	0.194***	0.255***	0.257***
	(0.0455)	(0.0661)	(0.0850)
Info/communication	0.314***	0.418***	0.399***
	(0.0472)	(0.0679)	(0.0868)
Finance/insurance	0.304***	0.413***	0.451***
	(0.0526)	(0.0748)	(0.0935)
Real estate	0.501***	0.605***	0.677***
	(0.0488)	(0.0688)	(0.0872)
Scientific/technic and admin support	0.200***	0.278***	0.278***
	(0.0459)	(0.0664)	(0.0853)
Public admin/teaching/health/social	0.0827*	0.155**	0.138
	(0.0463)	(0.0668)	(0.0857)
Miscellaneous services	0.198***	0.240***	0.269***
	(0.0499)	(0.0694)	(0.0889)
Log(workforce size) year before election	0.0271***	0.0263***	0.0327***
	(0.00265)	(0.00294)	(0.00357)
Constant	0.885***	1.014***	1.114***
	(0.0697)	(0.0910)	(0.109)
Observations	38,712	31,155	23,309
R-squared	0.555	0.505	0.444

Robust Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Average value-added by worker = gross annual value-added divided by workforce size (full-time equivalents)
In 2009 terms
Year fixed effects = election year
Agriculture and 2009 excluded categories
-
Data: INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 54: OLS regression to predict log of economic profitability - using lagged log of economic profitability

VARIABLES	(1) Log of economic profitability 1-yr after election	(2) Log of economic profitability 2-yrs after election	(3) Log of economic profitability 3-yrs after election
2010	-0.0343	-0.0990***	-0.0203
	(0.0337)	(0.0344)	(0.0347)
2011	-0.0961***	-0.0784**	-0.0121
	(0.0344)	(0.0340)	(0.0343)
2012	-0.0418	-0.0124	0.0515
	(0.0380)	(0.0379)	(0.0385)
2013	-0.0809**	-0.0258	0.0550
	(0.0343)	(0.0340)	(0.0340)
2014	-0.0771**	-0.0693**	-0.0558
	(0.0330)	(0.0329)	(0.0386)
2015	-0.102***	-0.192***	
	(0.0332)	(0.0388)	
2016	-0.125***		
	(0.0479)		
Industry	0.515***	0.473***	0.699***
	(0.0998)	(0.109)	(0.131)
Construction	1.183***	1.077***	1.314***
	(0.105)	(0.115)	(0.136)
Trade/transport/hospitality	0.803***	0.770***	0.968***
	(0.100)	(0.109)	(0.131)
Info/communication	0.990***	0.925***	1.158***
	(0.111)	(0.121)	(0.143)
Finance/insurance	0.263*	0.249	0.703***
	(0.142)	(0.154)	(0.180)
Real estate	-0.398***	-0.417***	-0.192
	(0.107)	(0.116)	(0.139)
Scientific/technic and admin support	1.154***	1.119***	1.346***
	(0.104)	(0.113)	(0.135)
Public admin/teaching/health/social	0.718***	0.658***	0.862***
	(0.102)	(0.112)	(0.134)
Miscellaneous services	0.639***	0.575***	0.738***
	(0.113)	(0.123)	(0.146)
Log(workforce size) year before election	-0.121***	-0.117***	-0.133***
	(0.00783)	(0.00838)	(0.00905)
Constant	-2.068***	-2.090***	-2.300***
	(0.106)	(0.114)	(0.136)
Observations	30,285	25,417	19,820
R-squared	0.058	0.058	0.063

Robust Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Economic profitability = value-added + net taxes – gross wages
In 2009 terms
Year fixed effects = election year
Agriculture and 2009 excluded categories
-
Data: INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

8.6.2 Tests of balance of predetermined characteristics

Table 55: Test of balance of pre-determined characteristics: using lagged dependent variable for prediction - outliers and pre-electoral alliances excluded, donut - Second union, cut-off: 10%

Running variable: score of the second-ranked union Cut-off: 0.1						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	0.00170	-0.00670	-0.0581	-0.0409	-0.0451	-0.112
	(0.0357)	(0.0361)	(0.0422)	(0.0597)	(0.0647)	(0.0754)
Conventional p-value	.962	.853	.169	.493	.486	.137
Obs left of cut-off	253	200	129	252	198	128
Obs right of cut-off	591	472	305	589	471	304
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.0425	0.0496	0.0528	0.114	0.166	-0.0634
	(0.0367)	(0.0384)	(0.0513)	(0.193)	(0.206)	(0.219)
Conventional p-value	.247	.196	.304	.304	.42	.773
Obs left of cut-off	254	221	150	184	149	104
Obs right of cut-off	614	505	331	400	316	215
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects,
the log of workforce size before the election and lagged dep. var (before election)
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances and scores at the cut-off (=donut) excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 56: Test of balance of pre-determined characteristics: using lagged dependent variable for prediction - outliers and pre-electoral alliances excluded, donut - Third union, cut-off: 10%

Running variable: score of the third-ranked union Cut-off: 0.1						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	0.00373	0.0214	0.000713	0.00484	0.00679	-0.0481
	(0.0192)	(0.0195)	(0.0243)	(0.0433)	(0.0472)	(0.0611)
Conventional p-value	.846	.272	.977	.911	.886	.431
Obs left of cut-off	745	584	347	741	582	346
Obs right of cut-off	1275	1031	620	1268	1028	616
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	-0.00622	0.0196	0.0347	-0.118	-0.0631	0.133
	(0.0325)	(0.0356)	(0.0495)	(0.110)	(0.114)	(0.126)
Conventional p-value	.848	.581	.483	.483	.58	.292
Obs left of cut-off	774	648	382	515	425	258
Obs right of cut-off	1319	1100	676	892	735	444
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects,
the log of workforce size before the election and lagged dep. var (before election)
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances and scores at the cut-off (=donut) excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 57: Test of balance of pre-determined characteristics: using lagged dependent variable for prediction - outliers and pre-electoral alliances excluded, donut - Fourth union, cut-off: 10%

Running variable: score of the fourth-ranked union Cut-off: 0.1						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	0.0353*	0.0301	0.0381	0.0139	0.0331	0.0527
	(0.0193)	(0.0211)	(0.0269)	(0.0376)	(0.0407)	(0.0516)
Conventional p-value	.067	.154	.157	.711	.416	.307
Obs left of cut-off	899	727	466	894	726	463
Obs right of cut-off	1004	818	520	990	811	516
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.0251	0.0125	-0.00612	-0.0160	0.0299	-0.00197
	(0.0256)	(0.0271)	(0.0330)	(0.111)	(0.114)	(0.130)
Conventional p-value	.326	.645	.853	.853	.792	.988
Obs left of cut-off	935	778	506	633	509	312
Obs right of cut-off	1039	868	563	691	575	375
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects,
the log of workforce size before the election and lagged dep. var (before election)
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances and scores at the cut-off (=donut) excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 58: Test of balance of pre-determined characteristics: using lagged dependent variable for prediction - outliers and pre-electoral alliances excluded, donut

Running variable: score of the fifth-ranked union Cut-off: 0.1						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	0.0104	0.00957	0.0368	-0.0326	-0.0666	-0.0415
	(0.0309)	(0.0315)	(0.0387)	(0.0571)	(0.0584)	(0.0723)
Conventional p-value	.737	.761	.341	.568	.254	.565
Obs left of cut-off	519	434	274	516	433	275
Obs right of cut-off	332	275	181	327	270	176
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.0379	0.0510	0.0738	0.194	0.107	0.0440
	(0.0394)	(0.0422)	(0.0466)	(0.193)	(0.211)	(0.248)
Conventional p-value	.336	.226	.113	.113	.611	.859
Obs left of cut-off	539	448	298	355	284	191
Obs right of cut-off	336	295	197	232	200	135
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects, the log of workforce size before the election and lagged dep. var (before election)
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances and scores at the cut-off (=donut) excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 59: Test of balance of pre-determined characteristics: using lagged dependent variable for prediction - outliers and pre-electoral alliances excluded, donut - First union, cut-off: 30%

Running variable: score of the first-ranked union Cut-off: 0.3						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	-0.0135	-0.0130	0.00534	-0.0861	-0.0949	-0.128
	(0.0345)	(0.0354)	(0.0451)	(0.0833)	(0.0895)	(0.125)
Conventional p-value	.694	.713	.906	.301	.289	.308
Obs left of cut-off	145	118	74	145	118	75
Obs right of cut-off	378	309	193	372	305	191
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.0619	0.0680	0.116	0.325	0.262	-0.0907
	(0.0685)	(0.0749)	(0.0996)	(0.271)	(0.312)	(0.338)
Conventional p-value	.366	.364	.246	.246	.401	.789
Obs left of cut-off	148	122	78	102	83	55
Obs right of cut-off	391	324	215	259	204	136
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects,
the log of workforce size before the election and lagged dep. var (before election)
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances and scores at the cut-off (=donut) excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 60: Test of balance of pre-determined characteristics: using lagged dependent variable for prediction - outliers and pre-electoral alliances excluded, donut - Second union, cut-off: 30%

Running variable: score of the second-ranked union Cut-off: 0.3						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	0.00971	0.0125	0.0284*	0.0133	0.0165	0.0242
	(0.0122)	(0.0127)	(0.0163)	(0.0250)	(0.0270)	(0.0344)
Conventional p-value	.427	.324	.082	.595	.54	.481
Obs left of cut-off	2319	1883	1115	2306	1866	1110
Obs right of cut-off	2441	1956	1185	2413	1944	1177
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.00125	-0.00258	0.00648	0.0112	-0.000261	0.0130
	(0.0181)	(0.0194)	(0.0253)	(0.0746)	(0.0785)	(0.0988)
Conventional p-value	.945	.894	.798	.798	.997	.896
Obs left of cut-off	2391	2003	1262	1603	1337	834
Obs right of cut-off	2484	2088	1337	1648	1391	881
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects,
the log of workforce size before the election and lagged dep. var (before election)
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances and scores at the cut-off (=donut) excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 61: Test of balance of pre-determined characteristics: using lagged dependent variable for prediction - outliers and pre-electoral alliances excluded, donut - Third union, cut-off: 30%

Running variable: score of the third-ranked union Cut-off: 0.3						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	-0.0184	-0.000627	-0.00921	-0.00678	-0.00503	0.0359
	(0.0239)	(0.0261)	(0.0336)	(0.0530)	(0.0584)	(0.0786)
Conventional p-value	.441	.981	.784	.898	.931	.648
Obs left of cut-off	813	671	407	809	669	406
Obs right of cut-off	356	293	177	353	289	177
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	-0.0153	0.00290	-0.0493	-0.0794	-0.254*	-0.0767
	(0.0369)	(0.0395)	(0.0557)	(0.131)	(0.144)	(0.183)
Conventional p-value	.678	.942	.377	.377	.079	.676
Obs left of cut-off	841	721	458	553	469	298
Obs right of cut-off	368	311	199	253	211	137
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects,
the log of workforce size before the election and lagged dep. var (before election)
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances and scores at the cut-off (=donut) excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

Table 62: Test of balance of pre-determined characteristics: using lagged dependent variable for prediction - outliers and pre-electoral alliances excluded, donut - First union, cut-off: 50%

Running variable: score of the first-ranked union Cut-off: 0.5						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Pred. log of average wage 1-yr after	Pred. log of average wage 2-yrs after	Pred. log of average wage 3-yrs after	Pred. log of value-added by worker 1-yr after	Pred. log of value-added by worker 2-yrs after	Pred. log of value-added by worker 3-yrs after
RD_Estimate	-0.0201	-0.0126	-0.0318	-0.0224	-0.00727	-0.0828
	(0.0199)	(0.0203)	(0.0247)	(0.0400)	(0.0428)	(0.0541)
Conventional p-value	.312	.534	.198	.575	.865	.126
Obs left of cut-off	791	646	365	787	646	365
Obs right of cut-off	1655	1336	799	1645	1327	794
	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Pred. log of share of wages in value-added 1-yr after	Pred. log of share of wages in value-added 2-yrs after	Pred. log of share of wages in value-added 3-yrs after	Pred. log of economic profitability 1-yr after	Pred. log of economic profitability 2-yrs after	Pred. log of economic profitability 3-yrs after
RD_Estimate	0.00548	-0.000690	0.0346	-0.193*	-0.0632	-0.233
	(0.0283)	(0.0308)	(0.0424)	(0.112)	(0.120)	(0.163)
Conventional p-value	.846	.982	.414	.414	.599	.152
Obs left of cut-off	813	688	414	535	440	269
Obs right of cut-off	1702	1438	905	1175	981	608
Bandwidth	.05	.05	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables: predicted values obtained using year and sector fixed effects,
the log of workforce size before the election and lagged dep. var (before election)
Wage and value-added in 2009 terms
Outliers, pre-electoral alliances and scores at the cut-off (=donut) excluded
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2010-2017, agrifin 2012-2017, inflation series)

8.7 Results: residualized and growth specifications

8.7.1 10% threshold

Table 63: Effect of an additional union (ITT) on average wage - Second union, cut-off: 10%

Running variable: score of the second-ranked union Cut-off: 0.1		
VARIABLES	(1) Residual log of average wage 1-yr after	(2) Residual log of average wage 2-yrs after	(3) Residual log of average wage 3-yrs after
RD_Estimate	0.0377 (0.0302)	0.00583 (0.0361)	0.0121 (0.0514)
Conventional p-value	.212	.872	.814
Bandwidth	.05	.05	.05
Obs left of cut-off	220	174	114
Obs right of cut-off	532	426	279
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Growth rate of average wage 1-yr before	(5) Growth rate of average wage 1-yr after	(6) Growth rate of average wage 2-yrs after	(7) Growth rate of average wage 3-yrs after
RD_Estimate	0.00141 (0.0267)	0.0190 (0.0237)	-0.0192 (0.0359)	0.0452 (0.0704)
Conventional p-value	.958	.422	.594	.521
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	230	220	174	114
Obs right of cut-off	572	532	426	279
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
col 1-3: residual = actual value minus predicted value
col 1-3: Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4: growth during year before election
col 5-7: growth compared to year before election
-
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for, col 5-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 64: Effect of an additional union (ITT) on average wage - Third union, cut-off: 10%

Running variable: score of the third-ranked union
Cut-off: 0.1

VARIABLES	(1) Residual log of average wage 1-yr after	(2) Residual log of average wage 2-yrs after	(3) Residual log of average wage 3-yrs after
RD_Estimate	-0.00473	-0.0130	-0.0231
	(0.0171)	(0.0195)	(0.0284)
Conventional p-value	.782	.505	.416
Bandwidth	.05	.05	.05
Obs left of cut-off	636	500	301
Obs right of cut-off	1100	888	533
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Growth rate of average wage 1-yr before	(5) Growth rate of average wage 1-yr after	(6) Growth rate of average wage 2-yrs after	(7) Growth rate of average wage 3-yrs after
RD_Estimate	0.0123	-0.0122	-0.0296*	-0.00479
	(0.0140)	(0.0163)	(0.0152)	(0.0269)
Conventional p-value	.377	.456	.052	.858
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	644	636	500	301
Obs right of cut-off	1137	1100	888	533
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
col 1-3: residual = actual value minus predicted value
col 1-3: Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4: growth during year before election
col 5-7: growth compared to year before election
-
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for, col 5-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 65: Effect of an additional union (ITT) on average wage - Fourth union, cut-off: 10%

Running variable: score of the fourth-ranked union Cut-off: 0.1			
VARIABLES	(1) Residual log of average wage 1-yr after	(2) Residual log of average wage 2-yrs after	(3) Residual log of average wage 3-yrs after
RD_Estimate	-0.0124	0.0167	0.0654**
	(0.0168)	(0.0216)	(0.0305)
Conventional p-value	.462	.44	.032
Bandwidth	.05	.05	.05
Obs left of cut-off	778	624	403
Obs right of cut-off	875	717	467
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Growth rate of average wage 1-yr before	(5) Growth rate of average wage 1-yr after	(6) Growth rate of average wage 2-yrs after	(7) Growth rate of average wage 3-yrs after
RD_Estimate	0.00580	-0.0269	0.00271	0.0449
	(0.0151)	(0.0173)	(0.0246)	(0.0339)
Conventional p-value	.701	.12	.912	.186
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	785	778	624	403
Obs right of cut-off	911	875	717	467
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
col 1-3: residual = actual value minus predicted value
col 1-3: Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4: growth during year before election
col 5-7: growth compared to year before election
-
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for, col 5-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 66: Effect of an additional union (ITT) on average wage - Fifth union, cut-off: 10%

Running variable: score of the fifth-ranked union Cut-off: 0.1			
VARIABLES	(1) Residual log of average wage 1-yr after	(2) Residual log of average wage 2-yrs after	(3) Residual log of average wage 3-yrs after
RD_Estimate	-0.0216	-0.0174	-0.0107
	(0.0270)	(0.0297)	(0.0402)
Conventional p-value	.425	.557	.79
Bandwidth	.05	.05	.05
Obs left of cut-off	464	391	247
Obs right of cut-off	310	259	166
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Growth rate of average wage 1-yr before	(5) Growth rate of average wage 1-yr after	(6) Growth rate of average wage 2-yrs after	(7) Growth rate of average wage 3-yrs after
RD_Estimate	-0.0415	-0.0218	-0.0151	-0.0148
	(0.0460)	(0.0245)	(0.0260)	(0.0316)
Conventional p-value	.367	.373	.562	.641
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	478	464	391	247
Obs right of cut-off	325	310	259	166
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
col 1-3: residual = actual value minus predicted value
col 1-3: Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4: growth during year before election
col 5-7: growth compared to year before election
-
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for, col 5-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 67: Effect of an additional union (ITT) on share of wages in value-added - Second union, cut-off: 10%

Running variable: score of the second-ranked union Cut-off: 0.1			
	(1)	(2)	(3)
	Residual log of share of wages in value-added	Residual log of share of wages in value-added	Residual log of share of wages in value-added
VARIABLES	1-yr after	2-yrs after	3-yrs after
RD_Estimate	-0.0127	-0.0134	0.00480
	(0.0391)	(0.0431)	(0.0457)
Conventional p-value	.746	.755	.916
Bandwidth	.05	.05	.05
Obs left of cut-off	221	190	133
Obs right of cut-off	553	454	302
Kernel	Triangular	Triangular	Triangular

	(4)	(5)	(6)	(7)
	Growth rate of share of wages in value-added	Growth rate of share of wages in value-added	Growth rate of share of wages in value-added	Growth rate of share of wages in value-added
VARIABLES	1-yr before	1-yr after	2-yrs after	3-yrs after
RD_Estimate	0.113**	-0.00906	-0.0264	0.00352
	(0.0550)	(0.0482)	(0.0551)	(0.0510)
Conventional p-value	.04	.851	.631	.945
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	233	223	191	136
Obs right of cut-off	573	556	457	305
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
col 1-3: residual = actual value minus predicted value
col 1-3: Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4: growth during year before election
col 5-7: growth compared to year before election
-
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for, col 5-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 68: Effect of an additional union (ITT) on share of wages in value-added - Third union, cut-off: 10%

Running variable: score of the third-ranked union Cut-off: 0.1			
	(1)	(2)	(3)
	Residual	Residual	Residual
	log of share of wages	log of share of wages	log of share of wages
	in value-added	in value-added	in value-added
VARIABLES	1-yr after	2-yrs after	3-yrs after
RD_Estimate	-0.00468	-0.00598	0.00404
	(0.0222)	(0.0327)	(0.0401)
Conventional p-value	.833	.855	.92
Bandwidth	.05	.05	.05
Obs left of cut-off	661	553	330
Obs right of cut-off	1139	948	582
Kernel	Triangular	Triangular	Triangular

	(4)	(5)	(6)	(7)
	Growth rate of	Growth rate of	Growth rate of	Growth rate of
	share of wages	share of wages	share of wages	share of wages
	in value-added	in value-added	in value-added	in value-added
VARIABLES	1-yr before	1-yr after	2-yrs after	3-yrs after
RD_Estimate	-0.0449	0.00596	0.0232	0.0362
	(0.0279)	(0.0288)	(0.0439)	(0.0556)
Conventional p-value	.108	.836	.598	.515
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	642	664	558	335
Obs right of cut-off	1132	1144	954	586
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
col 1-3: residual = actual value minus predicted value
col 1-3: Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4: growth during year before election
col 5-7: growth compared to year before election
-
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for, col 5-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 69: Effect of an additional union (ITT) on share of wages in value-added - Fourth union, cut-off: 10%

Running variable: score of the fourth-ranked union Cut-off: 0.1			
	(1)	(2)	(3)
	Residual log of share of wages in value-added	Residual log of share of wages in value-added	Residual log of share of wages in value-added
VARIABLES	1-yr after	2-yrs after	3-yrs after
RD_Estimate	-0.00662	-0.0409	-0.0121
	(0.0242)	(0.0282)	(0.0443)
Conventional p-value	.784	.147	.785
Bandwidth	.05	.05	.05
Obs left of cut-off	809	667	436
Obs right of cut-off	904	762	505
Kernel	Triangular	Triangular	Triangular

	(4)	(5)	(6)	(7)
	Growth rate of share of wages in value-added	Growth rate of share of wages in value-added	Growth rate of share of wages in value-added	Growth rate of share of wages in value-added
VARIABLES	1-yr before	1-yr after	2-yrs after	3-yrs after
RD_Estimate	-0.0166	-0.00304	-0.0285	0.0553
	(0.0238)	(0.0267)	(0.0364)	(0.0633)
Conventional p-value	.484	.909	.433	.382
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	787	816	675	439
Obs right of cut-off	906	906	764	507
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
col 1-3: residual = actual value minus predicted value
col 1-3: Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4: growth during year before election
col 5-7: growth compared to year before election
-
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for, col 5-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 70: Effect of an additional union (ITT) on share of wages in value-added - Fifth union, cut-off: 10%

Running variable: score of the fifth-ranked union Cut-off: 0.1			
	(1)	(2)	(3)
VARIABLES	Residual log of share of wages in value-added 1-yr after	Residual log of share of wages in value-added 2-yrs after	Residual log of share of wages in value-added 3-yrs after
RD_Estimate	0.0234	0.0331	-0.00595
	(0.0269)	(0.0354)	(0.0408)
Conventional p-value	.384	.349	.884
Bandwidth	.05	.05	.05
Obs left of cut-off	481	404	269
Obs right of cut-off	314	279	182
Kernel	Triangular	Triangular	Triangular

	(1)	(2)	(3)	(4)
VARIABLES	Growth rate of share of wages in value-added 1-yr before	Growth rate of share of wages in value-added 1-yr after	Growth rate of share of wages in value-added 2-yrs after	Growth rate of share of wages in value-added 3-yrs after
RD_Estimate	-0.00146	0.0241	0.0618	-0.0215
	(0.0301)	(0.0298)	(0.0458)	(0.0506)
Conventional p-value	.961	.418	.177	.671
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	477	483	408	273
Obs right of cut-off	323	317	281	184
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
col 1-3: residual = actual value minus predicted value
col 1-3: Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4: growth during year before election
col 5-7: growth compared to year before election
-
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for, col 5-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 71: Effect of an additional union (ITT) on value-added by worker - Second union, cut-off: 10%

Running variable: score of the second-ranked union Cut-off: 0.1			
	(1)	(2)	(3)
VARIABLES	Residual log of value-added by worker 1-yr after	Residual log of value-added by worker 2-yrs after	Residual log of value-added by worker 3-yrs after
RD_Estimate	0.0475	0.0286	-0.0106
	(0.0484)	(0.0587)	(0.0703)
Conventional p-value	.326	.627	.881
Bandwidth	.05	.05	.05
Obs left of cut-off	219	172	113
Obs right of cut-off	530	425	279
Kernel	Triangular	Triangular	Triangular

	(4)	(5)	(6)	(7)
VARIABLES	Increase in value-added by worker 1-yr before	Growth rate of value-added by worker 1-yr before	Increase in value-added by worker 1-yr after	Growth rate of value-added by worker 1-yr after
RD_Estimate	-3.115	-0.0224	-2.099	0.0671
	(4.290)	(0.0375)	(3.665)	(0.0541)
Conventional p-value	.468	.551	.567	.215
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	230	229	223	219
Obs right of cut-off	573	567	537	530
Kernel	Triangular	Triangular	Triangular	Triangular

	(8)	(9)	(10)	(11)
VARIABLES	Increase in value-added by worker 2-yrs after	Growth rate of value-added by worker 2-yrs after	Increase in value-added by worker 3-yrs after	Growth rate of value-added by worker 3-yrs after
RD_Estimate	0.463	0.0492	-2.483	0.0563
	(5.225)	(0.0647)	(5.276)	(0.0873)
Conventional p-value	.929	.447	.638	.519
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	174	172	115	113
Obs right of cut-off	429	425	281	279
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables:
col 1-3: residual = actual value minus predicted value,
predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4-5: increase/growth during year before election
col 6-11: increase/growth compared to year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for
col 4-11: year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 72: Effect of an additional union (ITT) on value-added by worker - Third union, cut-off: 10%

Running variable: score of the third-ranked union
Cut-off: 0.1

VARIABLES	(1) Residual log of value-added by worker 1-yr after	(2) Residual log of value-added by worker 2-yrs after	(3) Residual log of value-added by worker 3-yrs after
RD_Estimate	-0.00950	-0.0138	-0.0185
	(0.0270)	(0.0421)	(0.0541)
Conventional p-value	.725	.743	.732
Bandwidth	.05	.05	.05
Obs left of cut-off	632	498	300
Obs right of cut-off	1093	885	529
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Increase in value-added by worker 1-yr before	(5) Growth rate of value-added by worker 1-yr before	(6) Increase in value-added by worker 1-yr after	(7) Growth rate of value-added by worker 1-yr after
RD_Estimate	3.381**	0.0688**	-2.094	-0.0473
	(1.713)	(0.0270)	(2.181)	(0.0362)
Conventional p-value	.048	.011	.337	.191
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	649	639	639	632
Obs right of cut-off	1138	1123	1109	1093
Kernel	Triangular	Triangular	Triangular	Triangular

VARIABLES	(8) Increase in value-added by worker 2-yrs after	(9) Growth rate of value-added by worker 2-yrs after	(10) Increase in value-added by worker 3-yrs after	(11) Growth rate of value-added by worker 3-yrs after
RD_Estimate	-3.266	-0.0791	-7.963*	-0.0545
	(3.747)	(0.0791)	(4.243)	(0.0678)
Conventional p-value	.383	.317	.061	.422
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	502	498	303	300
Obs right of cut-off	896	885	536	529
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables:
col 1-3: residual = actual value minus predicted value,
predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4-5: increase/growth during year before election
col 6-11: increase/growth compared to year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for
col 4-11: year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 73: Effect of an additional union (ITT) on value-added by worker - Fourth union, cut-off: 10%

Running variable: score of the fourth-ranked union Cut-off: 0.1			
	(1)	(2)	(3)
	Residual	Residual	Residual
	log of value-added	log of value-added	log of value-added
	by worker	by worker	by worker
VARIABLES	1-yr after	2-yrs after	3-yrs after
RD_Estimate	-0.0173	0.0477	0.0896
	(0.0299)	(0.0373)	(0.0553)
Conventional p-value	.564	.201	.105
Bandwidth	.05	.05	.05
Obs left of cut-off	773	623	400
Obs right of cut-off	862	710	462
Kernel	Triangular	Triangular	Triangular

	(4)	(5)	(6)	(7)
	Increase in	Growth rate of	Increase in	Growth rate of
	value-added	value-added	value-added	value-added
	by worker	by worker	by worker	by worker
VARIABLES	1-yr before	1-yr before	1-yr after	1-yr after
RD_Estimate	0.879	0.0229	-2.577	-0.0719*
	(1.865)	(0.0312)	(2.367)	(0.0397)
Conventional p-value	.637	.462	.276	.07
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	788	779	783	773
Obs right of cut-off	912	900	879	862
Kernel	Triangular	Triangular	Triangular	Triangular
	(8)	(9)	(10)	(11)
	Increase in	Growth rate of	Increase in	Growth rate of
	value-added	value-added	value-added	value-added
	by worker	by worker	by worker	by worker
VARIABLES	2-yrs after	2-yrs after	3-yrs after	3-yrs after
RD_Estimate	0.305	-0.0290	-1.040	-0.0108
	(3.143)	(0.0508)	(5.340)	(0.0740)
Conventional p-value	.923	.568	.846	.884
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	631	623	405	400
Obs right of cut-off	721	710	471	462
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables:
col 1-3: residual = actual value minus predicted value,
predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4-5: increase/growth during year before election
col 6-11: increase/growth compared to year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for
col 4-11: year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 74: Effect of an additional union (ITT) on value-added by worker - Fifth union, cut-off: 10%

Running variable: score of the fifth-ranked union
Cut-off: 0.1

VARIABLES	(1) Residual log of value-added by worker 1-yr after	(2) Residual log of value-added by worker 2-yrs after	(3) Residual log of value-added by worker 3-yrs after
RD_Estimate	-0.133	-0.0523	0.0255
	(0.0980)	(0.0483)	(0.0666)
Conventional p-value	.174	.278	.702
Bandwidth	.05	.05	.05
Obs left of cut-off	461	390	248
Obs right of cut-off	305	254	161
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Increase in value-added by worker 1-yr before	(5) Growth rate of value-added by worker 1-yr before	(6) Increase in value-added by worker 1-yr after	(7) Growth rate of value-added by worker 1-yr after
RD_Estimate	-2.257	-0.0266	-5.011	-0.0422
	(3.710)	(0.0524)	(4.110)	(0.0419)
Conventional p-value	.543	.612	.223	.313
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	480	473	465	461
Obs right of cut-off	325	322	312	305
Kernel	Triangular	Triangular	Triangular	Triangular

VARIABLES	(8) Increase in value-added by worker 2-yrs after	(9) Growth rate of value-added by worker 2-yrs after	(10) Increase in value-added by worker 3-yrs after	(11) Growth rate of value-added by worker 3-yrs after
RD_Estimate	-1.968	-0.0155	0.613	0.0185
	(4.200)	(0.0482)	(5.939)	(0.0591)
Conventional p-value	.639	.748	.918	.754
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	394	390	249	248
Obs right of cut-off	260	254	167	161
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables:
col 1-3: residual = actual value minus predicted value,
predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4-5: increase/growth during year before election
col 6-11: increase/growth compared to year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for
col 4-11: year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

8.7.2 30% threshold

Table 75: Effect on average wage of an additional union above 30% - Third union

Running variable: score of the third-ranked union Cut-off: 0.3			
	(1) Residual log of average wage 1-yr after	(2) Residual log of average wage 2-yrs after	(3) Residual log of average wage 3-yrs after
VARIABLES			
RD_Estimate	-0.00377 (0.0214)	-0.0221 (0.0246)	-0.00749 (0.0296)
Conventional p-value	.86	.37	.8
Bandwidth	.05	.05	.05
Obs left of cut-off	741	614	375
Obs right of cut-off	347	285	170
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Growth rate of average wage 1-yr before	(5) Growth rate of average wage 1-yr after	(6) Growth rate of average wage 2-yrs after	(7) Growth rate of average wage 3-yrs after
RD_Estimate	-0.00432 (0.0249)	0.0293 (0.0226)	-0.0202 (0.0261)	0.00617 (0.0244)
Conventional p-value	.862	.194	.44	.8
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	782	741	614	375
Obs right of cut-off	356	347	285	170
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
col 1-3: residual = actual value minus predicted value
col 1-3: Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4: growth during year before election
col 5-7: growth compared to year before election
-
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for, col 5-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 76: Effect on share of wages in value-added of an additional union above 30% - Third union

Running variable: score of the third-ranked union Cut-off: 0.3			
	(1)	(2)	(3)
	Residual log of share of wages in value-added	Residual log of share of wages in value-added	Residual log of share of wages in value-added
VARIABLES	1-yr after	2-yrs after	3-yrs after
RD_Estimate	0.0169	-0.00980	0.0637
	(0.0328)	(0.0379)	(0.0510)
Conventional p-value	.608	.796	.212
Bandwidth	.05	.05	.05
Obs left of cut-off	770	661	421
Obs right of cut-off	359	303	192
Kernel	Triangular	Triangular	Triangular

	(4)	(5)	(6)	(7)
	Growth rate of share of wages in value-added	Growth rate of share of wages in value-added	Growth rate of share of wages in value-added	Growth rate of share of wages in value-added
VARIABLES	1-yr before	1-yr after	2-yrs after	3-yrs after
RD_Estimate	0.0486	0.00456	-0.0358	0.0490
	(0.0335)	(0.0368)	(0.0446)	(0.0565)
Conventional p-value	.147	.901	.423	.386
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	783	772	663	423
Obs right of cut-off	354	362	303	195
Kernel	Triangular	Triangular	Triangular	Triangular

Standard clustered at the firm level errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables:
Dependent variables: col 1-3: residual = actual value minus predicted value,
predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col4: growth during year before election
col 5-7: growth compared to year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for
col 4-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017)

Table 77: Effect on value-added by worker of an additional union above 30% - Third union

Running variable: score of the third-ranked union
Cut-off: 0.3

VARIABLES	(1) Residual log of value-added by worker 1-yr after	(2) Residual log of value-added by worker 2-yrs after	(3) Residual log of value-added by worker 3-yrs after
RD_Estimate	-0.0688	-0.0229	-0.0720
	(0.0542)	(0.0512)	(0.0711)
Conventional p-value	.204	.655	.311
Bandwidth	.05	.05	.05
Obs left of cut-off	738	612	374
Obs right of cut-off	344	281	170
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Increase in value-added by worker 1-yr before	(5) Growth rate of value-added by worker 1-yr before	(6) Increase in value-added by worker 1-yr after	(7) Growth rate of value-added by worker 1-yr after
RD_Estimate	0.800	-0.00849	1.063	-0.117
	(2.893)	(0.0338)	(3.214)	(0.133)
Conventional p-value	.782	.801	.741	.381
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	786	782	748	738
Obs right of cut-off	358	353	350	344
Kernel	Triangular	Triangular	Triangular	Triangular

VARIABLES	(8) Increase in value-added by worker 2-yrs after	(9) Growth rate of value-added by worker 2-yrs after	(10) Increase in value-added by worker 3-yrs after	(11) Growth rate of value-added by worker 3-yrs after
RD_Estimate	-1.778	-0.155	-7.625	-0.510*
	(4.292)	(0.168)	(5.476)	(0.302)
Conventional p-value	.679	.355	.164	.091
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	619	612	380	374
Obs right of cut-off	285	281	171	170
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables:
col 1-3: residual = actual value minus predicted value,
predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4-5: increase/growth during year before election
col 6-11: increase/growth compared to year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for
col 4-11: year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

8.7.2.1 Robustness: only 4-yrs mandates, share of wages in value-added

Table 78: Effect on share of wages in value-added of first union above 30% - robustness check: restriction to firms with four-years mandate

Running variable: score of the first-ranked union Cut-off: 0.3			
	(1)	(2)	(3)
	Residual log of share of wages in value-added	Residual log of share of wages in value-added	Residual log of share of wages in value-added
VARIABLES	1-yr after	2-yrs after	3-yrs after
RD_Estimate	0.0143	-0.0520	0.0755
	(0.0437)	(0.0695)	(0.0642)
Conventional p-value	.744	.454	.24
Bandwidth	.05	.05	.05
Obs left of cut-off	75	60	44
Obs right of cut-off	221	186	141
Kernel	Triangular	Triangular	Triangular

	(4)	(5)	(6)	(7)
	Growth rate of share of wages in value-added	Growth rate of share of wages in value-added	Growth rate of share of wages in value-added	Growth rate of share of wages in value-added
VARIABLES	1-yr before	1-yr after	2-yrs after	3-yrs after
RD_Estimate	0.00222	-0.0256	-0.0953	0.0952
	(0.0495)	(0.0477)	(0.0921)	(0.0965)
Conventional p-value	.964	.591	.301	.324
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	81	75	60	45
Obs right of cut-off	228	223	188	143
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables:
col 1-3: residual = actual value minus predicted value
Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col4: growth during year before election
col 5-7: growth compared to year before election
Outliers, CGC, pre-electoral alliances, mandates below 4 yrs, excluded, donut
-
Year fixed effects (col 4-7) and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017)

Table 79: Effect on share of wages in value-added of second union above 30% - robustness check: restriction to firms with four-years mandate

Running variable: score of the second-ranked union Cut-off: 0.3			
VARIABLES	(1) Residual log of share of wages in value-added 1-yr after	(2) Residual log of share of wages in value-added 2-yrs after	(3) Residual log of share of wages in value-added 3-yrs after
RD_Estimate	0.0345* (0.0178)	-0.00637 (0.0196)	0.0235 (0.0253)
Conventional p-value	.053	.745	.353
Bandwidth	.05	.05	.05
Obs left of cut-off	1437	1198	887
Obs right of cut-off	1539	1284	976
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Growth rate of share of wages in value-added 1-yr before	(5) Growth rate of share of wages in value-added 1-yr after	(6) Growth rate of share of wages in value-added 2-yrs after	(7) Growth rate of share of wages in value-added 3-yrs after
RD_Estimate	0.0254 (0.0199)	0.0313 (0.0242)	-0.0160 (0.0221)	0.0333 (0.0305)
Conventional p-value	.201	.197	.469	.275
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	1466	1445	1204	893
Obs right of cut-off	1582	1547	1290	982
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables:
col 1-3: residual = actual value minus predicted value
Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col4: growth during year before election
col 5-7: growth compared to year before election
Outliers, CGC, pre-electoral alliances, mandates below 4 yrs, excluded, donut
-
Year fixed effects (col 4-7) and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017)

8.7.2.2 A win-win situation? Economic profitability

Table 80: Effect on economic profitability of first union above 30%

Economic profitability
Running variable: score of the first-ranked union
Cut-off: 0.3

VARIABLES	(1) Residual log of economic profitability 1-yr after	(2) Residual log of economic profitability 2-yrs after	(3) Residual log of economic profitability 3-yrs after
RD_Estimate	-0.219	-0.0234	0.523
	(0.220)	(0.211)	(0.372)
Conventional p-value	.319	.912	.16
Bandwidth	.05	.05	.05
Obs left of cut-off	83	68	43
Obs right of cut-off	227	182	125
Kernel	Triangular	Triangular	Triangular

VARIABLES	(5) Increase in economic profitability 1-yr before	(6) Growth rate of economic profitability 1-yr before	(7) Increase in economic profitability 1-yr after	(8) Growth rate of economic profitability 1-yr after
RD_Estimate	-0.0246	-0.108	-0.0435	0.848
	(0.0995)	(0.416)	(0.127)	(2.360)
Conventional p-value	.804	.796	.732	.719
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	135	91	122	83
Obs right of cut-off	358	236	361	228
Kernel	Triangular	Triangular	Triangular	Triangular

VARIABLES	(9) Increase in economic profitability 2-yrs after	(10) Growth rate of economic profitability 2-yrs after	(11) Increase in economic profitability 3-yrs after	(12) Growth rate of economic profitability 3-yrs after
RD_Estimate	0.0896	1.423	0.299**	-0.627
	(0.113)	(4.347)	(0.142)	(0.809)
Conventional p-value	.429	.743	.034	.438
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	101	68	67	43
Obs right of cut-off	302	183	204	127
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables:
col 1-3: residual = actual value minus predicted value,
predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4-5: increase/growth during year before election
col 6-11: increase/growth compared to year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for
col 4-11: year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 81: Effect on economic profitability of second union above 30%

Running variable: score of the second-ranked union Cut-off: 0.3			
VARIABLES	(1) Residual log of economic profitability 1-yr after	(2) Residual log of economic profitability 2-yrs after	(3) Residual log of economic profitability 3-yrs after
RD_Estimate	-0.00291 (0.0665)	-0.0135 (0.0772)	0.0422 (0.103)
Conventional p-value	.965	.862	.681
Bandwidth	.05	.05	.05
Obs left of cut-off	1444	1209	761
Obs right of cut-off	1479	1257	808
Kernel	Triangular	Triangular	Triangular

VARIABLES	(5) Increase in economic profitability 1-yr before	(6) Growth rate of economic profitability 1-yr before	(7) Increase in economic profitability 1-yr after	(8) Growth rate of economic profitability 1-yr after
RD_Estimate	0.0196 (0.0270)	0.155 (0.643)	-0.0379 (0.0350)	-0.371 (0.319)
Conventional p-value	.468	.809	.279	.245
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	2175	1534	2185	1450
Obs right of cut-off	2283	1555	2288	1483
Kernel	Triangular	Triangular	Triangular	Triangular

VARIABLES	(9) Increase in economic profitability 2-yrs after	(10) Growth rate of economic profitability 2-yrs after	(11) Increase in economic profitability 3-yrs after	(12) Growth rate of economic profitability 3-yrs after
RD_Estimate	-0.0346 (0.0434)	-0.0631 (0.450)	-0.0503 (0.0558)	-0.680 (0.868)
Conventional p-value	.425	.888	.368	.434
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	1834	1213	1165	765
Obs right of cut-off	1930	1260	1246	811
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables:
col 1-3: residual = actual value minus predicted value,
predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4-5: increase/growth during year before election
col 6-11: increase/growth compared to year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for
col 4-11: year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

8.7.2.3 Robustness: bandwidth of 0.02

Table 82: Effect on average wage of first union above 30% - robustness check: bandwidth of 0.02

Running variable: score of the first-ranked union Cut-off: 0.3			
	(1)	(2)	(3)
	Residual	Residual	Residual
	log of average wage	log of average wage	log of average wage
VARIABLES	1-yr after	2-yrs after	3-yrs after
RD_Estimate	0.00873	-0.0254	0.112*
	(0.0423)	(0.0448)	(0.0676)
Conventional p-value	.837	.571	.098
Bandwidth	.02	.02	.02
Obs left of cut-off	61	48	28
Obs right of cut-off	114	99	65
Kernel	Triangular	Triangular	Triangular

	(4)	(5)	(6)	(7)
	Growth rate of	Growth rate of	Growth rate of	Growth rate of
	average wage	average wage	average wage	average wage
VARIABLES	1-yr before	1-yr after	2-yrs after	3-yrs after
RD_Estimate	-0.0127	0.00137	-0.0142	0.105
	(0.0299)	(0.0538)	(0.0653)	(0.0982)
Conventional p-value	.671	.98	.828	.286
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	73	61	48	28
Obs right of cut-off	123	114	99	65
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
col 1-3: residual = actual value minus predicted value
col 1-3: Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4: growth during year before election
col 5-7: growth compared to year before election
-
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for, col 5-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 83: Effect on average wage of second union above 30% - robustness check: bandwidth of 0.02

Running variable: score of the second-ranked union
Cut-off: 0.3

VARIABLES	(1) Residual log of average wage 1-yr after	(2) Residual log of average wage 2-yrs after	(3) Residual log of average wage 3-yrs after
RD_Estimate	0.00581	0.00735	0.0500**
	(0.0166)	(0.0186)	(0.0252)
Conventional p-value	.727	.692	.047
Bandwidth	.02	.02	.02
Obs left of cut-off	833	694	406
Obs right of cut-off	905	733	440
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Growth rate of average wage 1-yr before	(5) Growth rate of average wage 1-yr after	(6) Growth rate of average wage 2-yrs after	(7) Growth rate of average wage 3-yrs after
RD_Estimate	0.00220	-0.000707	-0.00583	0.00183
	(0.0148)	(0.0203)	(0.0178)	(0.0260)
Conventional p-value	.882	.972	.744	.944
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	853	833	694	406
Obs right of cut-off	930	905	733	440
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
col 1-3: residual = actual value minus predicted value
col 1-3: Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4: growth during year before election
col 5-7: growth compared to year before election
-
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for, col 5-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

8.7.3 50% threshold

Table 84: Effect on economic profitability of first union above 50%

Running variable: score of the first-ranked union
Cut-off: 0.5

VARIABLES	(1) Residual log of economic profitability 1-yr after	(2) Residual log of economic profitability 2-yrs after	(3) Residual log of economic profitability 3-yrs after
RD_Estimate	-0.0538	-0.354***	-0.157
	(0.0957)	(0.119)	(0.164)
Conventional p-value	.574	.003	.337
Bandwidth	.05	.05	.05
Obs left of cut-off	493	404	250
Obs right of cut-off	1110	929	579
Kernel	Triangular	Triangular	Triangular

VARIABLES	(5) Increase in economic profitability 1-yr before	(6) Growth rate of economic profitability 1-yr before	(7) Increase in economic profitability 1-yr after	(8) Growth rate of economic profitability 1-yr after
RD_Estimate	-0.0629	-4.943	0.0558	1.855
	(0.0477)	(5.446)	(0.0426)	(1.756)
Conventional p-value	.187	.364	.191	.291
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	773	511	769	498
Obs right of cut-off	1624	1144	1628	1111
Kernel	Triangular	Triangular	Triangular	Triangular

VARIABLES	(9) Increase in economic profitability 2-yrs after	(10) Growth rate of economic profitability 2-yrs after	(11) Increase in economic profitability 3-yrs after	(12) Growth rate of economic profitability 3-yrs after
RD_Estimate	0.0308	0.501	0.0683	0.345
	(0.0565)	(0.787)	(0.0925)	(1.192)
Conventional p-value	.586	.525	.461	.772
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	650	408	392	252
Obs right of cut-off	1374	930	875	579
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables:
col 1-3: residual = actual value minus predicted value,
predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4-5: increase/growth during year before election
col 6-11: increase/growth compared to year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for
col 4-11: year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

8.7.3.1 Robustness: bandwidth of 0.02

Table 85: Effect on average wage of first union above 50% - robustness check: bandwidth of 0.02

Running variable: score of the first-ranked union
Cut-off: 0.5

VARIABLES	(1) Residual log of average wage 1-yr after	(2) Residual log of average wage 2-yrs after	(3) Residual log of average wage 3-yrs after
RD_Estimate	0.00768	0.0340	0.0239
	(0.0292)	(0.0382)	(0.0511)
Conventional p-value	.793	.374	.641
Bandwidth	.02	.02	.02
Obs left of cut-off	267	221	125
Obs right of cut-off	600	488	284
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Growth rate of average wage 1-yr before	(5) Growth rate of average wage 1-yr after	(6) Growth rate of average wage 2-yrs after	(7) Growth rate of average wage 3-yrs after
RD_Estimate	-0.00575	-0.0131	-0.0167	0.0289
	(0.0258)	(0.0238)	(0.0272)	(0.0373)
Conventional p-value	.824	.581	.538	.439
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	283	267	221	125
Obs right of cut-off	627	600	488	284
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
col 1-3: residual = actual value minus predicted value
col 1-3: Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4: growth during year before election
col 5-7: growth compared to year before election
-
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for, col 5-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 86: Effect on share of wages in value-added of first union above 50% - robustness check: bandwidth of 0.02

Running variable: score of the first-ranked union
Cut-off: 0.5

VARIABLES	(1) Residual log of share of wages in value-added 1-yr after	(2) Residual log of share of wages in value-added 2-yrs after	(3) Residual log of share of wages in value-added 3-yrs after
RD_Estimate	-0.0783	-0.0784	-0.201**
	(0.0490)	(0.0655)	(0.0918)
Conventional p-value	.11	.232	.028
Bandwidth	.02	.02	.02
Obs left of cut-off	275	234	135
Obs right of cut-off	622	524	321
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Growth rate of share of wages in value-added 1-yr before	(5) Growth rate of share of wages in value-added 1-yr after	(6) Growth rate of share of wages in value-added 2-yrs after	(7) Growth rate of share of wages in value-added 3-yrs after
RD_Estimate	-0.00958	-0.0587	-0.0543	-0.312*
	(0.0466)	(0.0621)	(0.0667)	(0.167)
Conventional p-value	.837	.345	.415	.062
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	284	275	234	135
Obs right of cut-off	624	623	525	323
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
col 1-3: residual = actual value minus predicted value
col 1-3: Predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4: growth during year before election
col 5-7: growth compared to year before election
-
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for, col 5-7: Year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 87: Effect on value-added by worker of first union above 50% - robustness check: bandwidth of 0.02

Running variable: score of the first-ranked union
Cut-off: 0.5

VARIABLES	(1) Residual log of value-added by worker 1-yr after	(2) Residual log of value-added by worker 2-yrs after	(3) Residual log of value-added by worker 3-yrs after
RD_Estimate	0.0639	0.0964	0.240**
	(0.0550)	(0.0720)	(0.111)
Conventional p-value	.245	.181	.03
Bandwidth	.02	.02	.02
Obs left of cut-off	264	221	125
Obs right of cut-off	596	484	283
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Increase in value-added by worker 1-yr before	(5) Growth rate of value-added by worker 1-yr before	(6) Increase in value-added by worker 1-yr after	(7) Growth rate of value-added by worker 1-yr after
RD_Estimate	1.467	0.0493	0.988	0.0167
	(2.843)	(0.0546)	(3.165)	(0.0605)
Conventional p-value	.606	.366	.755	.783
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	284	283	269	264
Obs right of cut-off	629	622	603	596
Kernel	Triangular	Triangular	Triangular	Triangular

VARIABLES	(8) Increase in value-added by worker 2-yrs after	(9) Growth rate of value-added by worker 2-yrs after	(10) Increase in value-added by worker 3-yrs after	(11) Growth rate of value-added by worker 3-yrs after
RD_Estimate	-1.838	0.00284	10.80	0.289**
	(4.517)	(0.0851)	(7.801)	(0.113)
Conventional p-value	.684	.973	.166	.011
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	222	221	126	125
Obs right of cut-off	489	484	287	283
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variables:
col 1-3: residual = actual value minus predicted value,
predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4-5: increase/growth during year before election
col 6-11: increase/growth compared to year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for
col 4-11: year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

Table 88: Effect on economic profitability of first union above 50% - robustness check: bandwidth of 0.02

Running variable: score of the first-ranked union Cut-off: 0.5			
VARIABLES	(1) Residual log of economic profitability 1-yr after	(2) Residual log of economic profitability 2-yrs after	(3) Residual log of economic profitability 3-yrs after
RD_Estimate	-0.156	-0.548**	0.0758
	(0.154)	(0.232)	(0.324)
Conventional p-value	.311	.018	.815
Bandwidth	.02	.02	.02
Obs left of cut-off	183	141	81
Obs right of cut-off	427	361	206
Kernel	Triangular	Triangular	Triangular

VARIABLES	(4) Increase in economic profitability 1-yr before	(5) Growth rate of economic profitability 1-yr before	(6) Increase in economic profitability 1-yr after	(7) Growth rate of economic profitability 1-yr after
RD_Estimate	-0.0531	-0.134	0.170***	1.237
	(0.0738)	(0.872)	(0.0651)	(2.625)
Conventional p-value	.472	.878	.009	.637
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	286	189	282	185
Obs right of cut-off	627	429	628	428
Kernel	Triangular	Triangular	Triangular	Triangular

VARIABLES	(8) Increase in economic profitability 2-yrs after	(9) Growth rate of economic profitability 2-yrs after	(10) Increase in economic profitability 3-yrs after	(11) Growth rate of economic profitability 3-yrs after
RD_Estimate	0.180**	0.624	0.122	1.289
	(0.0792)	(0.620)	(0.102)	(1.232)
Conventional p-value	.023	.314	.233	.296
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	236	142	136	81
Obs right of cut-off	530	362	325	206
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Dependent variables:
col 1-3: residual = actual value minus predicted value,
predictions using year and sector fixed effects, log of workforce size before election and lagged dep. var
col 4-5: increase/growth during year before election
col 6-11: increase/growth compared to year before election
Outliers, CGC, pre-electoral alliances excluded, donut
-
col 1-3: log of number of votes controlled for
col 4-11: year fixed effects and log of number of votes controlled for
-
Data: DARES (MARS 2009-2016) INSEE (FARE 2007-2017, agrifin 2012-2017, inflation series)

8.8 Results: Intermediary channels

8.8.1 10%

Table 89: Effect of additional union (ITT) on strike probability and strike length - cut-off: 10%

	Cut-off: 0.1			
	(1)	(2)	(3)	(4)
VARIABLES	Strike 1-yr before	Nb. days strike normalized by nb. votes 1-yr before	Share of yrs with strike during mandate	Nb. days strike after election normalized by nb. votes and nb. yrs recorded
	Running variable: score of the second-ranked union			
RD_Estimate	0.112	0.558*	-0.0112	-0.101
	(0.108)	(0.325)	(0.0893)	(0.176)
Conventional p-value	.296	.086	.9	.567
Obs left of cut-off	64	63	111	109
Obs right of cut-off	165	164	253	246
	Running variable: score of the third-ranked union			
RD_Estimate	0.0430	-0.0139	-0.0203	0.139*
	(0.0730)	(0.143)	(0.0563)	(0.0838)
Conventional p-value	.556	.923	.718	.098
Obs left of cut-off	292	286	423	421
Obs right of cut-off	569	556	782	777
	Running variable: score of the fourth-ranked union			
RD_Estimate	-0.0538	-0.0451	-0.0160	-0.0434
	(0.0636)	(0.130)	(0.0508)	(0.0899)
Conventional p-value	.397	.729	.753	.629
Obs left of cut-off	513	506	668	661
Obs right of cut-off	679	671	810	805
	Running variable: score of the fifth-ranked union			
RD_Estimate	0.108	0.370**	0.120**	0.00628
	(0.0802)	(0.181)	(0.0602)	(0.0676)
Conventional p-value	.18	.041	.045	.926
Obs left of cut-off	404	398	497	492
Obs right of cut-off	289	281	323	323
Bandwidth	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
Share of years with strike = share of the years for which info. recorded in Acemo,
up to 4-yrs after the election if the mandate did not end before
Number of days of strikes = number of days of strike*workers who participated ('journees individuelles non travaillees'),
normalized by number of years for which info. recorded in Acemo, and number of voters
-
Excluded observations:
Cols 2 and 4: 1% top outlier, among firms with positive number of days of strikes, CGC excluded
Pre-electoral alliances and elections with score at the cut-off, CGC excluded
-
Controlling for the log of the number of votes and year fixed effects
-
Data: DARES (MARS 2009-2016, Acemo sur le Dialogue Social en Entreprise 2009-2016)

Table 90: Effect of additional union (ITT) on agreement probability and agreement frequency - cut-off: 10%, Acemo

	Cut-off: 0.1			
	(1)	(2)	(3)	(4)
VARIABLES	Agreement on wages 1-yr before	Agreement on wages conditional on holding of nego 1-yr before	Share of yrs with agreement on wages during mandate	Share of yrs with agreement conditional on holding of nego during mandate
	Running variable: score of the second-ranked union			
RD_Estimate	0.0148	0.0896	0.00242	-0.0268
	(0.167)	(0.173)	(0.119)	(0.108)
Conventional p-value	.930	.604	.984	.803
Obs left of cut-off	55	45	92	69
Obs right of cut-off	140	119	216	184
	Running variable: score of the third-ranked union			
RD_Estimate	0.0468	0.0289	0.0528	0.0102
	(0.0791)	(0.0777)	(0.0607)	(0.0414)
Conventional p-value	.554	.71	.385	.806
Obs left of cut-off	257	234	374	348
Obs right of cut-off	483	453	661	623
Kernel	Triangular	Triangular	Triangular	Triangular
	Running variable: score of the fourth-ranked union			
RD_Estimate	0.0756	0.0642	-0.00398	0.0159
	(0.0697)	(0.0708)	(0.0529)	(0.0374)
Conventional p-value	.278	.365	.940	.67
Obs left of cut-off	436	416	552	528
Obs right of cut-off	557	534	676	654
	Running variable: score of the fifth-ranked union			
RD_Estimate	0.00250	-0.0150	0.00716	0.0372
	(0.0869)	(0.0870)	(0.0630)	(0.0374)
Conventional p-value	.977	.863	.91	.319
Obs left of cut-off	338	321	409	395
Obs right of cut-off	247	238	273	266
Bandwidth	.05	.05	.05	.05
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
Share of years with agreement signed = share of the years for which info. recorded in Acemo, up to 4-yrs after the election if the mandate did not end before
-
Excluded observations:
Firms which belonged to a UES, CGC excluded
Pre-electoral alliances and elections with score at the cut-off, CGC excluded
-
Controlling for the log of the number of votes and year fixed effects
-
Data: DARES (MARS 2009-2016, Acemo sur le Dialogue Social en Entreprise 2009-2016)

8.8.2 Agreements or veto: 30% and 50%

Table 91: Effect of additional union above 30% on agreement probability (bandwidths: 0.1, 0.05, 0.02), REPONSE

	Cut-off: 0.3		
	(1)	(2)	(3)
VARIABLES	Agreement signed last yr before interview	Agreement signed last yr before interview	Agreement signed last yr before interview
Running variable	Score of first union	Score of second union	Score of third union
RD_Estimate	0.139	-0.0694	0.0122
	(0.137)	(0.0673)	(0.129)
Conventional p-value	.311	.302	.924
Bandwidth	.1	.1	.1
Obs left of cut-off	55	429	343
Obs right of cut-off	185	382	59
RD_Estimate	0.0626	-0.0605	0.0280
	(0.171)	(0.0905)	(0.178)
Conventional p-value	.714	.504	.875
Bandwidth	.05	.05	.05
Obs left of cut-off	46	269	119
Obs right of cut-off	91	240	42
Kernel	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Observation level: establishment, weights inversely-proportional to number of establishments in perim.
-
Dependent variables:
Null if no agreement or no wage negotiations year before interview
defined only if mandate started before 2010 and finished after
-
Excluded observations:
Pre-electoral alliances, elections with score at the cut-off, CGC, excluded
-
Log of number of votes controlled for
-
Data: DARES (MARS 2009-2016, REPONSE 2011 and 2017, Representants de la Direction)

Table 92: Effect of first or second union above 30% on agreement probability (bandwidths: 0.05, 0.02), Acemo

	Cut-off: 0.3			
	(1)	(2)	(3)	(4)
VARIABLES	Agreement on wages 1-yr before	Agreement on wages conditional on holding of nego 1-yr before	Share of yrs with agreement on wages during mandate	Share of yrs with agreement on wages conditional on holding of nego during mandate
Running variable: score of the first-ranked union				
RD_Estimate	0.128	0.122	-0.00780	-0.0149
	(0.106)	(0.107)	(0.0816)	(0.0794)
Conventional p-value	.229	.254	.924	.851
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	118	117	129	126
Obs right of cut-off	224	217	265	258
RD_Estimate	0.0987	0.0987	0.108	0.104
	(0.145)	(0.145)	(0.107)	(0.104)
Conventional p-value	.496	.496	.312	.314
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	63	63	69	68
Obs right of cut-off	88	88	89	87
Running variable: score of the second-ranked union				
RD_Estimate	-0.105**	-0.113**	-0.156***	-0.160***
	(0.0528)	(0.0529)	(0.0411)	(0.0414)
Conventional p-value	.046	.032	0	0
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	806	747	1119	1052
Obs right of cut-off	760	694	1119	1048
RD_Estimate	-0.160*	-0.194**	-0.197***	-0.208***
	(0.0820)	(0.0813)	(0.0624)	(0.0629)
Conventional p-value	.051	.017	.002	.001
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	320	297	447	424
Obs right of cut-off	310	283	473	445
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
Share of years with agreement signed = share of the years for which info. recorded in Acemo, up to 4-yrs after the election if the mandate did not end before
-
Excluded observations:
Firms which belonged to a UES, CGC excluded
Pre-electoral alliances and elections with score at the cut-off, CGC excluded
-
Controlling for the log of the number of votes and year fixed effects
-
Data: DARES (MARS 2009-2016, Acemo sur le Dialogue Social en Entreprise 2009-2016)

Table 93: Effect of third union above 30% on agreement probability (bandwidths: 0.05, 0.02), Acemo

Cut-off: 0.3 Running variable: score of the third-ranked union				
	(1)	(2)	(3)	(4)
VARIABLES	Agreement on wages 1-yr before	Agreement on wages conditional on holding of nego 1-yr before	Share of yrs with agreement on wages during mandate	Share of yrs with agreement on wages conditional on holding of nego during mandate
RD_Estimate	0.00259	-0.00787	0.0649	0.00213
	(0.0941)	(0.0966)	(0.0756)	(0.0745)
Conventional p-value	.978	.935	.39	.977
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	355	330	450	427
Obs right of cut-off	174	163	212	204
RD_Estimate	0.197	0.229	0.0655	-0.0180
	(0.136)	(0.141)	(0.110)	(0.109)
Conventional p-value	.147	.104	.551	.868
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	98	90	127	120
Obs right of cut-off	89	84	105	102
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
Share of years with agreement signed = share of the years for which info. recorded in Acemo,
up to 4-yrs after the election if the mandate did not end before
-
Excluded observations:
Firms which belonged to a UES, CGC excluded
Pre-electoral alliances and elections with score at the cut-off, CGC excluded
-
Controlling for the log of the number of votes and year fixed effects
-
Data: DARES (MARS 2009-2016, Acemo sur le Dialogue Social en Entreprise 2009-2016)

Table 94: Effect of first union reaching 50% on probability of veto and on probability of agreement (bandwidths: 0.5 with poly. order 2 for veto, 0.05 for agreement), REPONSE

Running variable: score of the first-ranked union Cut-off: 0.5			
	(1)	(2)	(3)
VARIABLES	Veto condit. on nego last yr before interview	Agreement signed last yr before interview	No agreement or agreement vetoed last yr before interview
RD_Estimate	0.147	-0.0716	0.0409
	(0.119)	(0.125)	(0.0762)
Conventional p-value	.216	.566	.591
Bandwidth	.5	.05	.5
Obs left of cut-off	76	115	503
Obs right of cut-off	40	126	597
Kernel	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Observation level: establishment, weights inversely-proportional to number of establishments in perim.
-
Dependent variables:
One if disagreement, vetoed agreement, or no negotiations year before interview
defined only if mandate started before 2010 and finished after
-
Excluded observations:
Pre-electoral alliances, elections with score at the cut-off excluded
-
Log of number of votes controlled for
-
Data: DARES (MARS 2009-2016, REPONSE 2011 and 2017, Representants de la Direction)

Table 95: Effect of first union reaching 50% on probability of veto and on probability of agreement (bandwidths: 0.05 and 0.02), Acemo

Running variable: score of the first-ranked union Cut-off: 0.5				
	(1)	(2)	(3)	(4)
VARIABLES	Veto 1-yr before	Agreement on wages 1-yr before	Share of yrs with veto during mandate	Share of yrs with agreement on wages during mandate
RD_Estimate	0.0368	0.0326	0.0142	0.0314
	(0.0423)	(0.0754)	(0.0284)	(0.0578)
Conventional p-value	.385	.665	.618	.587
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	179	407	185	519
Obs right of cut-off	215	496	263	800
RD_Estimate	0.0790*	0.0693	-0.0117	0.00315
	(0.0439)	(0.127)	(0.0224)	(0.0987)
Conventional p-value	.072	.587	.603	.975
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	66	154	71	206
Obs right of cut-off	101	215	112	328
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
Share of years with agreement vetoed = share of the years for which info. recorded in Acemo, up to 4-yrs after the election if the mandate did not end before
-
Excluded observations:
Firms which belonged to a UES excluded, CGC excluded
Pre-electoral alliances and elections with score at the cut-off excluded
-
Controlling for the log of the number of votes and year fixed effects
-
Data: DARES (MARS 2009-2016, Acemo sur le Dialogue Social en Entreprise 2009-2012)

8.8.3 Strikes: 30% and 50%

Table 96: Effect of first or second union above 30% on strike frequency and length (bandwidths: 0.05, 0.02)

Cut-off: 0.3				
	(1)	(2)	(3)	(4)
VARIABLES	Strike 1-yr before	Nb. days strike normalized by nb. votes 1-yr before	Share of yrs with strike during mandate	Nb. days strike after election normalized by nb. votes and nb. yrs recorded
Running variable: score of the first-ranked union				
RD_Estimate	-0.0731	0.281	-0.149*	-0.132
	(0.0991)	(0.242)	(0.0804)	(0.164)
Conventional p-value	.46	.246	.064	.419
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	143	140	150	151
Obs right of cut-off	269	266	311	307
RD_Estimate	-0.00821	0.583	-0.123	0.0971
	(0.146)	(0.443)	(0.108)	(0.259)
Conventional p-value	.955	.188	.256	.707
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	75	74	77	78
Obs right of cut-off	105	105	103	102
Running variable: score of the second-ranked union				
RD_Estimate	0.0575	0.110	-0.0188	-8.84e-05
	(0.0481)	(0.113)	(0.0346)	(0.0608)
Conventional p-value	.232	.331	.587	.999
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	938	920	1275	1266
Obs right of cut-off	873	862	1274	1265
RD_Estimate	0.0349	-0.0618	-0.0373	0.0680
	(0.0763)	(0.104)	(0.0534)	(0.0716)
Conventional p-value	.648	.553	.486	.342
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	365	355	506	503
Obs right of cut-off	354	351	544	536
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
Share of years with strike = share of the years for which info. recorded in Acemo,
up to 4-yrs after the election if the mandate did not end before
Number of days of strikes = number of days of strike*workers who participated ('journees individuelles non travaillees'),
normalized by number of years for which info. recorded in Acemo, and number of voters
-
Excluded observations:
Cols 2 and 4: 1% top outlier, among firms with positive number of days of strikes,, CGC excluded
Pre-electoral alliances and elections with score at the cut-off, CGC excluded
-
Controlling for the log of the number of votes and year fixed effects
-
Data: DARES (MARS 2009-2016, Acemo sur le Dialogue Social en Entreprise 2009-2016)

Table 97: Effect of third union above 30% on strike frequency and length (bandwidths: 0.05, 0.02)

Cut-off: 0.3 Running variable: score of the third-ranked union				
	(1)	(2)	(3)	(4)
VARIABLES	Strike 1-yr before	Nb. days strike normalized by nb. votes 1-yr before	Share of yrs with strike during mandate	Nb. days strike after election normalized by nb. votes and nb. yrs recorded
RD_Estimate	-0.0127	0.0681	0.0145	-0.0739
	(0.0851)	(0.0877)	(0.0698)	(0.0791)
Conventional p-value	.881	.437	.835	.35
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	417	412	533	533
Obs right of cut-off	202	199	250	248
RD_Estimate	-0.0531	-0.0133	-0.0315	-0.192*
	(0.125)	(0.140)	(0.107)	(0.0980)
Conventional p-value	.67	.925	.768	.05
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	114	111	152	152
Obs right of cut-off	105	103	124	122
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
Share of years with strike = share of the years for which info. recorded in Acemo,
up to 4-yrs after the election if the mandate did not end before
Number of days of strikes = number of days of strike*workers who participated ('journees individuelles non travaillees'),
normalized by number of years for which info. recorded in Acemo, and number of voters
-
Excluded observations:
Cols 2 and 4: 1% top outlier, among firms with positive number of days of strikes,, CGC excluded
Pre-electoral alliances and elections with score at the cut-off, CGC excluded
-
Controlling for the log of the number of votes and year fixed effects
-
Data: DARES (MARS 2009-2016, Acemo sur le Dialogue Social en Entreprise 2009-2016)

Table 98: Effect of first union above 50% on strike frequency and length (bandwidths: 0.05, 0.02)

	Cut-off: 0.5			
	Running variable: score of the first-ranked union			
	(1)	(2)	(3)	(4)
VARIABLES	Strike 1-yr before	Nb. days strike normalized by nb. votes 1-yr before	Share of yrs with strike during mandate	Nb. days strike after election normalized by nb. votes and nb. yrs recorded
RD_Estimate	0.0596	0.162	-0.0590	0.0458
	(0.0687)	(0.116)	(0.0526)	(0.0489)
Conventional p-value	.386	.165	.262	.35
Bandwidth	.05	.05	.05	.05
Obs left of cut-off	402	393	518	515
Obs right of cut-off	487	480	788	787
RD_Estimate	0.0131	-0.150	-0.0565	-0.0172
	(0.118)	(0.140)	(0.0885)	(0.0525)
Conventional p-value	.912	.285	.523	.743
Bandwidth	.02	.02	.02	.02
Obs left of cut-off	151	150	205	205
Obs right of cut-off	212	210	324	324
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Dependent variables:
Share of years with strike = share of the years for which info. recorded in Acemo,
up to 4-yrs after the election if the mandate did not end before
Number of days of strikes = number of days of strike*workers who participated ('journees individuelles non travaillees'),
normalized by number of years for which info. recorded in Acemo, and number of voters
-
Excluded observations:
Cols 2 and 4: 1% top outlier, among firms with positive number of days of strikes,, CGC excluded
Pre-electoral alliances and elections with score at the cut-off, CGC excluded
-
Controlling for the log of the number of votes and year fixed effects
Standard errors clustered at the firm level
-
Data: DARES (MARS 2009-2016, Acemo sur le Dialogue Social en Entreprise 2009-2016)

8.8.4 Social climate: 30% and 50%

Table 99: Effect of first union above 30% on social climate perceptions (bandwidths: 0.01, 0.05, 0.02)

Cut-off: 0.3 Running variable: score of the first-ranked union			
	(1)	(2)	(3)
VARIABLES	Good social climate	Worsened social climate comp. to 3 yrs before interview	Improved social climate comp. to 3 yrs before interview
RD_Estimate	0.0140	-0.00745	-0.0628
	(0.102)	(0.108)	(0.120)
Conventional p-value	.47	.945	.6
Bandwidth	.1	.1	.1
Obs left of cut-off	98	98	98
Obs right of cut-off	350	343	343
RD_Estimate	0.104	-0.0871	-0.116
	(0.114)	(0.143)	(0.161)
Conventional p-value	.36	.543	.473
Bandwidth	.05	.05	.05
Obs left of cut-off	75	75	75
Obs right of cut-off	160	157	157
RD_Estimate	0.156	-0.0793	-0.0915
	(0.144)	(0.186)	(0.248)
Conventional p-value	.276	.669	.712
Bandwidth	.02	.02	.02
Obs left of cut-off	34	34	34
Obs right of cut-off	37	38	38
Kernel	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Observation level: establishment, weights inversely-proportional to number of establishments in perim.
-
Dependent variables:
Resp. quality and evolution of social climate over past 3 yrs,
defined only if mandate started before interview and finished after and min. 2 unions with delegates
-
Excluded observations:
Pre-electoral alliances, elections with score at the cut-off excluded
-
Log of number of votes controlled for
-
Data: DARES (MARS 2009-2016, REPONSE 2011 and 2017, Representants de la Direction)

Table 100: Effect of second union above 30% on social climate perceptions (bandwidths: 0.1, 0.05, 0.02)

	Cut-off: 0.3		
	Running variable: score of the second-ranked union		
	(1)	(2)	(3)
VARIABLES	Good social climate	Worsened social climate comp. to 3 yrs before interview	Improved social climate comp. to 3 yrs before interview
RD_Estimate	0.0594	-0.116**	0.0565
	(0.0503)	(0.0499)	(0.0589)
Conventional p-value	.238	.02	.338
Bandwidth	.1	.1	.1
Obs left of cut-off	666	655	655
Obs right of cut-off	550	545	545
RD_Estimate	0.0499	-0.111	0.130
	(0.0782)	(0.0766)	(0.0872)
Conventional p-value	.523	.146	.136
Bandwidth	.05	.05	.05
Obs left of cut-off	334	327	327
Obs right of cut-off	297	297	297
RD_Estimate	0.0941	-0.143	0.0881
	(0.133)	(0.126)	(0.138)
Conventional p-value	.479	.257	.525
Bandwidth	.02	.02	.02
Obs left of cut-off	133	130	130
Obs right of cut-off	140	140	140
Kernel	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Observation level: establishment, weights inversely-proportional to number of establishments in perim.
-
Dependent variables:
Resp. quality and evolution of social climate over past 3 yrs,
defined only if mandate started before interview and finished after and min. 2 unions with delegates
-
Excluded observations:
Pre-electoral alliances, elections with score at the cut-off excluded
-
Log of number of votes controlled for
-
Data: DARES (MARS 2009-2016, REPONSE 2011 and 2017, Representants de la Direction)

Table 101: Effect of third union above 30% on social climate perceptions (bandwidths: 0.1, 0.05, 0.02)

Cut-off: 0.3 Running variable: score of the third-ranked union			
	(1)	(2)	(3)
VARIABLES	Good social climate	Worsened social climate comp. to 3 yrs before interview	Improved social climate comp. to 3 yrs before interview
RD_Estimate	-0.0967	-0.0546	-0.150
	(0.133)	(0.109)	(0.132)
Conventional p-value	.466	.617	.254
Bandwidth	.1	.1	.1
Obs left of cut-off	401	395	395
Obs right of cut-off	66	65	65
RD_Estimate	-0.0863	-0.212	0.0362
	(0.208)	(0.177)	(0.189)
Conventional p-value	.679	.231	.848
Bandwidth	.05	.05	.05
Obs left of cut-off	121	120	120
Obs right of cut-off	45	44	44
RD_Estimate	0.312	-0.570**	0.413
	(0.323)	(0.288)	(0.310)
Conventional p-value	.334	.047	.183
Bandwidth	.02	.02	.02
Obs left of cut-off	28	28	28
Obs right of cut-off	20	20	20
Kernel	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Observation level: establishment, weights inversely-proportional to number of establishments in perim.
-
Dependent variables:
Resp. quality and evolution of social climate over past 3 yrs,
defined only if mandate started before interview and finished after and min. 3 unions with delegates
-
Excluded observations:
Pre-electoral alliances, elections with score at the cut-off excluded
-
Log of number of votes controlled for
-
Data: DARES (MARS 2009-2016, REPONSE 2011 and 2017, Representants de la Direction)

Table 102: Effect of additional union above 30% on social climate perceptions - union-specific, (bandwidth: 0.1)

	Cut-off: 0.3		
	(1)	(2)	(3)
VARIABLES	Good social climate	Worsened social climate comp. to 3 yrs before interview	Improved social climate comp. to 3 yrs before interview
	Running variable: score of the CGT		
RD_Estimate	0.0283	-0.136*	-0.0287
	(0.0766)	(0.0711)	(0.0908)
Conventional p-value	.712	.055	.752
Obs left of cut-off	278	272	272
Obs right of cut-off	255	253	253
	Running variable: score of the CFDT		
RD_Estimate	0.0211	-0.0576	0.115
	(0.0929)	(0.0849)	(0.0903)
Conventional p-value	.821	.497	.201
Obs left of cut-off	281	276	276
Obs right of cut-off	223	221	221
	Running variable: score of FO		
RD_Estimate	0.109	-0.185*	-0.0995
	(0.0927)	(0.103)	(0.118)
Conventional p-value	.238	.072	.399
Obs left of cut-off	210	209	209
Obs right of cut-off	144	139	139
Bandwidth	.1	.1	.1
Kernel	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Observation level: establishment, weights inversely-proportional to number of establishments in perim.
-
Dependent variables:
Resp. quality and evolution of social climate over past 3 yrs,
defined only if mandate started before interview and finished after and FO has delegate
-
Excluded observations:
Pre-electoral alliances, elections with score at the cut-off excluded
-
Log of number of votes controlled for
-
Data: DARES (MARS 2009-2016, REPONSE 2011 and 2017, Representants de la Direction)

Table 103: Effect of additional union above 30% on social climate perceptions - union-specific, (bandwidth: 0.1)

Cut-off: 0.3			
	(1)	(2)	(3)
VARIABLES	Good social climate	Worsened social climate comp. to 3 yrs before interview	Improved social climate comp. to 3 yrs before interview
Running variable: score of the CFTC			
RD_Estimate	-0.0657	0.117	-0.00118
	(0.133)	(0.143)	(0.161)
Conventional p-value	.621	.414	.994
Obs left of cut-off	99	99	99
Obs right of cut-off	120	121	121
Running variable: score of the UNSA			
RD_Estimate	0.0763	-0.113	0.177
	(0.216)	(0.212)	(0.233)
Conventional p-value	.724	.596	.447
Obs left of cut-off	41	41	41
Obs right of cut-off	31	31	31
Bandwidth	.1	.1	.1
Kernel	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Observation level: establishment, weights inversely-proportional to number of establishments in perim.
-
Dependent variables:
Resp. quality and evolution of social climate over past 3 yrs,
defined only if mandate started before interview and finished after and UNSA has delegate
-
Excluded observations:
Pre-electoral alliances, elections with score at the cut-off excluded
-
Log of number of votes controlled for
-
Data: DARES (MARS 2009-2016, REPONSE 2011 and 2017, Representants de la Direction)

Table 104: Effect of first union above 50% on social climate perceptions - (bandwidths: 0.1, 0.05, 0.02)

Running variable: score of the first-ranked union Cut-off: 0.5			
	(1)	(2)	(3)
VARIABLES	Good social climate	Worsened social climate comp. to 3 yrs before interview	Improved social climate comp. to 3 yrs before interview
RD_Estimate	0.132*	-0.117	5.72e-05
	(0.0690)	(0.0789)	(0.0872)
Conventional p-value	.057	.138	.999
Bandwidth	.1	.1	.1
Obs left of cut-off	312	309	309
Obs right of cut-off	331	328	328
RD_Estimate	0.113	-0.0357	0.0500
	(0.0927)	(0.116)	(0.134)
Conventional p-value	.223	.759	.709
Bandwidth	.05	.05	.05
Obs left of cut-off	163	161	161
Obs right of cut-off	170	169	169
RD_Estimate	0.0715	0.114	0.128
	(0.163)	(0.182)	(0.243)
Conventional p-value	.661	.529	.598
Bandwidth	.02	.02	.02
Obs left of cut-off	46	45	45
Obs right of cut-off	63	62	62
Kernel	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Observation level: establishment, weights inversely-proportional to number of establishments in perim.
-
Dependent variables:
Resp. quality and evolution of social climate over past 3 yrs,
defined only if mandate started before interview and finished after
-
Excluded observations:
Pre-electoral alliances, elections with score at the cut-off, CGC, excluded
-
Log of number of votes controlled for
-
Data: DARES (MARS 2009-2016, REPONSE 2011 and 2017, Representants de la Direction)

Table 105: Effect of union above 50% on social climate perceptions - union-specific, (bandwidths: 0.05, 0.5)

Cut-off: 0.5			
	(1)	(2)	(3)
VARIABLES	Good social climate	Worsened social climate comp. to 3 yrs before interview	Improved social climate comp. to 3 yrs before interview
Running variable: score of the CGT			
RD_Estimate	0.0782	-0.0446	-0.0678
	(0.222)	(0.212)	(0.207)
Conventional p-value	.724	.833	.743
Bandwidth	.05	.05	.05
Obs left of cut-off	80	79	79
Obs right of cut-off	71	70	70
RD_Estimate	0.0964	-0.0479	-0.0347
	(0.0847)	(0.0779)	(0.0872)
Conventional p-value	.255	.539	.691
Bandwidth	.5	.5	.5
Obs left of cut-off	1030	1013	1013
Obs right of cut-off	307	300	300
Running variable: score of the CFDT			
RD_Estimate	0.208*	-0.139	0.298*
	(0.119)	(0.149)	(0.177)
Conventional p-value	.08	.351	.092
Bandwidth	.05	.05	.05
Obs left of cut-off	98	98	98
Obs right of cut-off	70	70	70
RD_Estimate	0.0830	-0.126*	0.191**
	(0.0651)	(0.0729)	(0.0858)
Conventional p-value	.202	.084	.026
Bandwidth	.5	.5	.5
Obs left of cut-off	1013	996	996
Obs right of cut-off	331	324	324
Kernel	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Observation level: establishment, weights inversely-proportional to number of establishments in perim.
-
Dependent variables:
Resp. quality and evolution of social climate over past 3 yrs,
defined only if mandate started before interview and finished after and CFDT has delegate
-
Excluded observations:
Pre-electoral alliances, elections with score at the cut-off excluded
-
Log of number of votes controlled for
-
When bandwidth of 0.5, poly. of order 2, standard errors clustered at the firm level
-
Data: DARES (MARS 2009-2016, REPONSE 2011 and 2017, Representants de la Direction)

Table 106: Effect of additional union above 50% on social climate perceptions - union-specific, (bandwidth: 0.5)

	Cut-off: 0.5		
	(1)	(2)	(3)
VARIABLES	Good social climate	Worsened social climate comp. to 3 yrs before interview	Improved social climate comp. to 3 yrs before interview
	Running variable: score of FO		
RD_Estimate	0.0395	-0.00540	0.0459
	(0.110)	(0.110)	(0.136)
Conventional p-value	.719	.961	.735
Bandwidth	.5	.5	.5
Obs left of cut-off	770	758	758
Obs right of cut-off	142	139	139
	Running variable: score of the CFTC		
RD_Estimate	-0.00675	-0.0611	-0.0906
	(0.111)	(0.210)	(0.187)
Conventional p-value	.952	.771	.627
Bandwidth	.5	.5	.5
Obs left of cut-off	459	455	455
Obs right of cut-off	81	81	81
Kernel	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
-
Observation level: establishment, weights inversely-proportional to number of establishments in perim.
-
Dependent variables:
Resp. quality and evolution of social climate over past 3 yrs,
defined only if mandate started before interview and finished after and CFTC has delegate
-
Excluded observations:
Pre-electoral alliances, elections with score at the cut-off excluded
-
Log of number of votes controlled for
-
Poly. of order 2, standard errors clustered at the firm level
-
Data: DARES (MARS 2009-2016, REPONSE 2011 and 2017, Representants de la Direction)

8.9 Standard deviations (for interpretation of coefficients in tests of balance in 5.3.2. and in residualized specifications in 6.1.)

Table 107: Standard deviations of: predicted variables used in 5.3.2., residuals used in 6.1.

STANDARD DEVIATIONS (after exclusion of outliers)				
Standard deviations of the predicted variables used to test for balance of pre-determined characteristics in 5.3.2.				
	Predicted log(wage)	Predicted log(share of wages in value-added)	Predicted log(value-added by worker)	Predicted log(economic profitability)
1 year after election	0.13	0.16	0.22	0.34
2 years after election	0.12	0.17	0.22	0.34
3 years after election	0.12	0.17	0.21	0.36
Note: predictions obtained after an OLS regression of the dependent variable on the log of workforce size and year and sector fixed effects				

STANDARD DEVIATIONS (after exclusion of outliers)				
Standard deviations of the residuals used as dependent variables in 6.1.				
	Residual log(wage)	Residual log(share of wages in value-added)	Residual log(value-added by worker)	Residual log(economic profitability)
1 year after election	0.17	0.24	0.29	1.25
2 years after election	0.19	0.26	0.31	1.25
3 years after election	0.19	0.27	0.34	1.22
Note: residuals from a regression of the dependent variable on its value one year before the election, the log of workforce size and year and sector fixed effects				

8.10 Discussion: incumbency advantage?

Table 108: Test for incumbency advantage - 50% threshold

Running variable: score of the first union Cut-off: 0.5						
	(1)	(2)	(3)	(4)	(5)	(6)
	Incumbent union	Incumbent union	Incumbent union	Incumbent union	Incumbent union	Incumbent union
VARIABLES	(50% or more)	(>50%)	(50% or more)	(>50%)	(50% or more)	(>50%)
RD_Estimate	0.0180	-0.00125	0.0622	0.0379	0.157*	0.0911
	(0.0276)	(0.0311)	(0.0516)	(0.0596)	(0.0903)	(0.107)
Conventional p-value	.514	.968	.229	.525	.082	.393
Bandwidth	.05	.05	.02	.02	.01	.01
Obs left of cut-off	1391	1381	492	489	209	209
Obs right of cut-off	3574	2805	1811	1063	1148	406
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Standard errors clustered at the firm level in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Dependent variable: equal to 1 if first union had received (weakly/strongly) more than 50% of the votes during previous election
Elections where the first union was not the CGT, CFDT, FO, CFTC, UNSA or SUD excluded
col 2, 4, 6: score at the cut-off excluded
Log of number of valid votes and year fixed effects controlled for
-
Data: DARES (MARS 2009-2016)

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