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## Electoral Democracy at Work

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JEL Codes: J51, J52, J58

Keywords : Union Representativeness, Democracy, Unionization, Social  
Capital

# Electoral Democracy at Work

Philippe Askenazy and Thomas Breda\*

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## Abstract

We evaluate the short- to medium-run effects on unionization and employers' and workers' trust in unions, of an important reform of French employment relations in 2008. This reform made the conditions for union recognition more liberal and democratic after 2008 in private firms with 11 employees or more. The law gave equal chances to all unions to be recognized for bargaining, putting an end to the quasi-monopoly given to five historical unions until then. The law also introduced votes and minimal electoral requirements to obtain union recognition. These new regulations only became fully effective at the first firms' work councils elections happening after January 1<sup>st</sup>, 2009. Those elections occur within each firm according to a pre-defined frequency—usually every four years—, so that election dates only depend on former election dates, and can be considered as quasi-random with respect to the application date of the new law, at least in firms that are old enough. The identification thus relies on a regression discontinuity design in which the running variable is the firms' work councils election date: we compare in early 2011 firms that had those elections just before or just after January 1<sup>st</sup>, 2009. We find that the democratic rules introduced in 2008 quickly improved employers' satisfaction and trust towards unions by around 45% of a standard deviation. Union coverage and membership at the establishment level also increased strongly due to the reform and work stoppages became more likely. Together, these results suggest that the introduction of electoral democracy in French firms managed to improve workers' participation in unions and their ability to voice concerns while also improving employers' opinions of unions.

**Keywords:** Union Representativeness, Democracy, Unionization, Social Capital.

**JEL codes:** J51, J52, J58.

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# Introduction

Cross-country differences in the quality of employment relations and their changing nature attract a vast literature and political concern. Several scholars and institutions such as the OECD or the World Economic Forum stress the huge heterogeneity of cooperation in labor-employer relations (Figure 1), which affects firm, and eventually country competitiveness (Aghion et al. 2011). The current well performing European economies—Scandinavian countries or Germany—and also the US, the UK and Japan, enjoy cooperative labor-employer relations according to the employers. By contrast, Italy, France and Eastern European countries are consistently ranked among countries where these relations are the most uncooperative. Union membership rates exhibit massive differences across developed countries as well. If we exclude countries with the Ghent system (Scandinavia, Belgium) where unionization is mechanically high, they range from more than one third in Italy to about 10% in France, the United-States or South-Korea, and even less in some eastern countries.

Employment relations not only shape business performance but also social inclusiveness. On top of their cooperative labor-employer relations, Scandinavian countries have strong representative unions and high unionization rates. The strength of unions, or their bargaining power, usually proxied using the unionization rate, affects rent sharing (Breda 2015), and eventually inequality (Freeman et al. n.d.). Declining trade union membership and union coverage are known to be two important drivers of rising wage inequality in the United States (DiNardo et al. 1996, Card et al. 2004, Western & Rosenfeld 2011, Farber et al. 2018) or Germany (Dustmann et al. 2009).

Based on these well-known observations, improving social dialogue has become a central objective of policy makers and international organizations. A high-quality social dialogue is one of the 4 pillars of the concept of decent work, the flagship of the International Labor Organization (ILO) for its century in 2019. As such, it composes one of the goals of the United-Nations 2030 agenda for sustainable development.

Despite this widespread interest, the paths for enhancing unionization and cooperation between workers' representatives and employers remain largely unknown. The consensus among international institutions does not go beyond the application of fundamental principles and rights at work (e.g., freedom of association, right to collective bargaining). This is likely to be explained by the absence of clear-cut empirical evidence in the academic literature, which mostly relies on broad cross-country comparisons of bargaining systems (e.g., OECD (2018)). Such comparisons are rarely able to convincingly identify specific channels conducive to higher unionization or cooperation. They also tend to neglect the strong historical component of employment relations which prevents the regulations in place in a given country to be immediately transferable to others.

In contrast, the historical dimension of the union movement is given a central role in

the large and more qualitative literature in industrial relations that studies the possible union strategies (*e.g.*, union structures, organizing techniques) that could lead to union revitalization (see Murray (2017) for a review). A limit of this literature, however, is that it does not investigate empirically the potential impacts of changes in the institutional framework.

In this paper, we depart with these standard approaches by studying the impact on labor-employer cooperation and unionization of a major French reform that made the conditions for union recognition at the firm, industry and national levels, more democratic after 2008. The poor cooperation between workers and employers and low unionization rate in France fueled a large consensus for this reform whose main objective was to improve labor unions representativeness.<sup>1</sup>

The reform had three major aspects. First, it gave equal chances to all labor unions to be recognized for bargaining in private sector firms, putting an end to the quasi-monopoly that was given to five historical unions until then. Second, it introduced a minimal electoral requirement of 10% of vote casts at firm professional elections (for work councils or workers representatives) to obtain union recognition and appoint union delegates at the firm level, whereas the five historical unions were considered *a priori* representative before the reform and could appoint any worker as their legal delegate in charge of bargaining. Third, the reform also put an end to the monopoly given to historical unions to bargain at the industry and national levels as it made recognition at these levels dependent on the aggregate vote casts over all firms that held professional elections in the industry or country.

In a nutshell, the reform introduced more democracy and electoral competition to obtain the right to represent workers at all levels. Assuming the existence of a sovereign salaried people, the reform draw an homology between social democracy and political democracy. This competitive electoral democracy at work contrasts with institutions or practices in numerous countries where it could therefore be considered. For example, in Germany, there are repeated elections for the members of work councils in private sector firms. However, there is *de facto* a monopoly of the industry unions under the umbrella of the German Trade Union Confederation (DGB) implying that a vast majority of elected representatives at the firm level belong to the DGB. In the U.S., there are no elected representatives and union recognition at the firm level is based on a majority principle that leads to either no union or one recognized union. Several unions can try to organize a certification election and obtain recognition in a given firm. However, there are no repeated elections: once a union is recognized, it becomes entrenched, making it hard for potential entrants to compete.

We study how the French reform has impacted employment relations, in particular

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<sup>1</sup>The reform was initiated by the French two main unions and employers' organizations, and endorsed by a conservative government.

union coverage, unionization, how unions are perceived and trusted by employers and workers, and conflicts. Bérout and Yon have already explored these questions in an analysis based on a limited number of detailed *ex-post* monographies in selected industries (Bérout & Yon 2014, Yon & Bérout 2013). Based on interviews with human resources managers and union representatives, they stress a large heterogeneity among the studied cases but conclude that on average the reform improved the recognition of unions by employers as a relevant partner whereas it did not seem to affect workers' appraisal of unions nor unionization. Here we propose a more comprehensive and quantitative approach. To do so, we exploit the fact that it was implemented gradually following an exogenous timing. The new regulations introduced by the reform only became effective at the firms' first professional election (to elect work council members or workers' delegates) following the application date of the law. Those elections occur within each firm with 11 workers or more according to a pre-defined frequency—usually every four years—so that election dates around the application date of the new law only depend on former election dates, and can be considered as quasi-random with respect to the reform, at least in firms that have been in existence for long enough. The identification thus relies on a regression discontinuity design in which the forcing or “running variable” is the firms' work council (or delegate) election date: we compare workplaces in early 2011 that had run those elections slightly before and slightly after the reform became entirely effective on January 1<sup>st</sup> 2009.

Using a unique dataset which combines a large representative survey towards both employers and workers of French establishments with more than 10 employees in 2011 and the exact dates of the elections collected by the French administration, we find that the democratic rules introduced in 2008 increased “social capital” or “common ideology” à la Dunlop (1957): namely, both employers' and workers' satisfaction and trust in unions measured in 2011 are much higher in firms that already applied the new regulations—by about 45% of a standard deviation for employers and 30% of a standard deviation for employees. Union coverage has jumped by 20 percentage points and unionization rate by around as much as 9 percentage points for the firms that were the first to apply the 2008 reforms. These local average treatment effects are very large, and we discuss how they can be reconciled with the French macro trends in unionization and union coverage. Finally, we find a positive effect of the reform on the occurrence of light forms of conflicts such as walk outs. We interpret this result as a stronger expression of workers' voice in response to the introduction of more democracy for the representation of workers, consistent with Hirschman's model of Exit, Voice and Loyalty.<sup>2</sup> Interestingly, this increase in workers' willingness to express their concerns goes along with the strong estimated increase in employers' satisfaction and trust in unions, confirming that the absence of explicit social conflicts does not necessarily reflect of high cooperation between workers and employers.

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<sup>2</sup>We find a non-significant negative effect of the reform on voluntary quits. Our data sample is however too small to properly study quits.

We directly contribute to the academic literature on the sources of variations in the quality of employment relations. Institutions and culture are typically seen as the two major forces driving these variations. Studies based on cross-country comparisons have either concluded that cultural idiosyncrasies, including trust, play a major role (Black 2005), or that institutional rules can account for both differences between countries and changes (Blanpain 2010).

From a different perspective, the research on the diversity of capitalism (Hall & Soskice 2003, Amable 2003) highlights the complementarity between different types of institutions and supports the idea that industrial relations are part of global models of capitalism. Recent research in economics has highlighted the interplay between institutions and cultural traits such as trust or cooperation (e.g., Aghion et al. (2010)).<sup>3</sup> This strand of the literature typically studies the causes and long run cultural consequences of major historical events (see Alesina & Giuliano (2015) for a review) and highlights the two-way causal effect between culture and institutions. It also often relies on empirical comparisons of the long-run evolutions of countries or large regions (see for example Acemoglu et al. (2011) or Acemoglu et al. (2019) on the effects of institutions or democracy on growth). Focusing on the institutional origins of cooperation, Aghion et al. (2011) argue that (dis)trust determines institutional choices which in turns fuel (dis)trust, leading to multiple equilibriums in the long run.

A common point between the aforementioned papers is that they usually assume and can only show a long term impact of institutions on culture. As such, these papers raise skepticism that targeted changes in the institutional rules governing the action of labor unions can significantly curve the cooperation between employers and unions in the short-term. An important contribution of the present study is to show that this is not the case. To our knowledge, we provide the first micro-level evaluation of the short-run consequences of a switch to a more democratic system for the representation of workers. As such we also contribute to the literature on the benefits of democracy.

The rest of the paper is organized as follows. Section 1 reviews the literature on the determinants of the desire of, and satisfaction towards unions. Section 2 provides the relevant details on the French institutions before and after the 2008 reform. Data are described in section 3, and methods in section 4. Results are then presented in section 5, and followed by a last section including discussion and conclusion.

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<sup>3</sup>At a micro level, some evidence is also consistent with an interplay between culture and institutions; e.g. Dill & Jirjahn (2017) find that in Germany, work councils and managers are less likely to cooperate in firms owned by non-German stockholders.

# 1 Related Literature

Workers' desire of and satisfaction towards unions attract a vast literature in social sciences. A first strand explores the individual determinants of the decision to join a union or to vote for unionization. These decisions are usually hypothesized to be a function of costs and benefits (Pencavel 1971); specifically, in a voting process, workers compare their current satisfaction regarding wage and non-wage aspects of their job, with expectations about how unions would change these job aspects (Farber & Saks 1980). Empirical evidence broadly supports these early models (Schnabel & Wagner 2005). In the 1980s, Booth (1985) and Naylor (1989) also showed that unionization and strikes could be rationalized as social customs in models in which departing from the social norm (e.g., being a union member in a high-unionization country) is individually costly.

The strategy of unions can also improve the satisfaction towards unions and membership. Findings in different institutional and social environments suggest that a more democratic internal functioning of unions is a key ingredient (e.g. Fiorito et al. (1988) on the U.S., Lévesque et al. (2005) on Quebec). We complement this literature on the determinants of individual unionization by studying the effect of mandatory electoral democracy for the appointment of union delegates.

At a macro level, union membership is correlated with institutional variables and cultural factors such as workplace representation, Ghent unemployment insurance or a society's social capital (e.g. Ebbinghaus et al. (2011)). Of course, several omitted variables can drive such correlation, making it impossible to isolate specific aspects of employment relations systems that are likely to affect unionization. Similarly, when a global governmental shift—for instance the Thatcher's war against unions—clearly impacts unionization, the causal effect of changes in isolated parameters in existing models of industrial relations is also impossible to assess.

The Right-to-Work legislation in the U.S. which resulted from the amendment of the Wagner act in 1947 is one exception. States got the power to pass laws that outlaw the union shop, a contract provision that requires new employees to join and pay dues to the union. Using state-level cross-section and panel data, Ellwood & Fine (1987) show that the passage of a right-to-work law reduced significantly flows into unionism through organizing. More recently, the attempts to evaluate the effects of the statutory rights conferred on trade union learning representatives (ULRs) under the 2002 British Employment Act suffered from a lack of suited data and concerns regarding the endogeneity of the Act; they eventually reached conflicting conclusions (e.g. Wallis et al. (2005), McIlroy (2008)).

To the best of our knowledge, this existing literature on policy changes explores rarely the consequences of institutions on the employers' attitudes towards unions, and more generally on labor-employer cooperation. Rather, institutions (and their reforms) are

often seen as driven by the (changing) nature of labor relations, culture and social preferences. For example, the Wagner act may be considered as an application of American ideals of democracy, representation, and freedom of choice (e.g. Bodie (2008)). In Sweden, there is no separately elected structure along the lines of the works councils which exist in many other European countries. Only unions represent employees at work. This institutional context is said to be the result of the high trust between the parties. Unions' strength is not built on legal requirements, but instead results from high union membership and the commitment of unions to find alternatives to industrial action (Adlercreutz & Nyström (2015)). We depart from these approaches by looking instead at how a targeted institutional change can affect the nature of labor relations.

## 2 Institutional Settings

We give here a brief overview of employment relations in France, before presenting in more details the institutional changes introduced by the 2008 law as well as the context and timing in which it was prepared, announced and enacted.

**General organization of employment relations in the French private sector.** In the French private sector, industrial relations are organized at three main layers: workplace/firm, industry (called branch) and national. Despite one of the lowest union membership rate among OECD countries—around 10% in the private sector—, unions are key players and most French workers are covered by collective agreements.

At the national level, employers' and representative workers' organizations are consulted on future labor regulations and can also bargain over any relevant issues. If some large unions and employers' organizations reach a bilateral agreement called a "common position" or a national inter-industry agreement, the government is incited to include their propositions into the legislative process.

At the industry level, employers' organizations and representative unions meet a few times a year to update former agreements. They discuss all aspects of pay (e.g., the pay scales prevailing in the industry), benefits (e.g., sickness absence compensation) and working conditions (e.g, shift work). When they reach an agreement, it is extended to all firms in the industry by the government providing that it complies with the labor law.

At the firm or workplace level, the French system separates the consultation process from the bargaining process. The 2008 reform has almost exclusively affected the later.

Until 2016, the French multi-level collective bargaining system respects on most topics the "hierarchy of norms" which implies that industry-level (firm-level) collective agreements must be more favorable to workers than the law (industry-level agreements).

**Consultation at workplace or firm level and professional elections.** In work-

places and firms with 10 workers or less, there is no formal representation of workers. Consultation and information of workers is however mandatory in all workplaces and firms with 11 employees or more.<sup>4</sup> Until 2017, it was done with either *workers' delegates* only (in workplaces and firms with 11 to 49 employees) or both *workers' delegates* and a *work council* (in workplaces and firms with 50 employees or more).<sup>5</sup> In all covered workplaces/firms, the employer has the duty to inform workers' delegates and collect their views on several predefined matters. Conversely, these delegates relayed individual and collective claims concerning for example work organization (e.g., health and safety) or the application of higher-level collective agreements. In firms/workplaces with 50 employees or more, workers' delegates keep dealing with individual problems while collective issues were mainly the prerogative of the work council (*comité d'entreprise*) which is chaired by the employer and whose functioning is more formally organized.

Workers' delegates and part of the members of the work council are elected during two distinct elections that we call "professional elections". A worker can be candidate at both elections (which are usually run simultaneously in workplaces and firms with 50 employees or more). In several small workplaces or firms however, the employer does not organize elections (voluntarily or not), or there are no candidates among workers, implying that there is no worker representation at all.

To understand the exact implication of the 2008 reform, one needs to understand the functioning of professional elections. Depending on workplace or firm size, there is a predefined legal number of seats for *workers' delegates* and elected members at the *work council*. These seats are attributed in two rounds. Only workers endorsed by an *ex ante representative* union can be candidates at the first round. Candidate unions present ordered lists of names for the election. Workers vote for one list, and are allowed to cross the names of people they do not want to see elected. Seats are then allocated to unions proportionally to their vote casts, and within unions to workers according to the number of votes obtained on their name. A second round is only organized if there was no (or not enough) candidates from ex-ante representative unions in the first round or if the ballot turnout was below 50%. In that case, candidates not endorsed by a union can apply to the election.

**Bargaining at workplace or firm level before and after the 2008 law.** Collective bargaining is possible in all firms with 11 employees or more. Until 2017, it is done almost exclusively with unions through their *union delegates*.<sup>6</sup> When there are *union*

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<sup>4</sup>For multi-establishment firms, there is representation at both the workplace and firm levels according to the same regulations (in terms of size thresholds, etc.).

<sup>5</sup>In 2016 and 2017, several major changes in employment relations were introduced. In particular workers' delegates and work councils were merged in 2017. For simplicity, we do not describe in detail the new regulations that apply since 2016.

<sup>6</sup>Elected workers' delegates may bargain and sign agreements with the employers only when there is no union delegate and only on very restricted topics from which wage bargaining is explicitly excluded.

*delegates* in a firm, the employer has the duty to negotiate at least once a year with them regarding wages, working conditions and employment.<sup>7</sup> The negotiations can lead to legally-binding collective agreements.

The crucial changes introduced by the 2008 law at firm-level concern the design of the elections, the appointment of union delegates and the definition of representative unions. Table 1 synthesizes the union recognition rules before and after the law.

Under the previous regulation, the representativeness of a union was not connected to the results of the workplace elections. A union was considered to be representative in the firm or workplace if 1) it was an affiliate of one of the five trade unions<sup>8</sup> designated in a decree published in 1966 granting them representativeness, or 2) if it had been recognized as representative by the employer or by a judge. The criteria that judges were required to apply were the age of the union, its membership, its compliance with republican values and its patriotic behavior during the Second World War.<sup>9</sup>

These criteria gave a non-democratic prerogative to the five historical trade unions: they were *de jure* representative in all workplaces or firm with 11 employees or more and could appoint any voluntary worker as their union delegate. In workplaces/firms with 50 employees or more, they could do so without any constraint, even if zero votes were cast for them in the workplace or firm elections. In workplaces/firms having between 11 and 49 employees, unions however had the constraint to choose their delegate among elected workers' delegates, implying that there were already a small indirect link between election results and recognition for bargaining in these smaller workplaces/firms.

Before the 2008 law, these five *de jure* representative unions also had a substantial advantage during professional elections as they were the only one to be *ex ante* representative: only workers endorsed by them could be candidates in the first round of elections. Non-affiliated workers or workers endorsed by another union and could be elected if and only if a second round was organized, that is if there was no (or not enough) candidates from ex-ante representative unions in the first round or if the ballot turnout was below 50%.

The new law revamped the criteria of representativeness and the election process. Basically, conditions for being a candidate in the first round of the elections were relaxed, and representativeness is now based on the election results. Since the 2008 reform, any union that has more than two years of existence, that complies with republican values and financial transparency and that covers the industry and the geographic zone of a firm can endorse candidates for the first ballot of the elections in this firm. The key change

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<sup>7</sup>Bargaining on several other themes such as gender equality or union rights within the firm is also mandatory but at a larger frequency.

<sup>8</sup>CGT was created in 1895, FO which resulted from scission of a significant block from the CGT in 1947, CFDT and CFTC resulting from a split the Christian union created in 1919 and the CGC born in 1944.

<sup>9</sup>Unions were banned by the Vichy government during the Second World War; most of them remained active clandestinely and played a crucial role within the Resistance.

is then that a union is representative for bargaining at the firm or workplace level if and only if at least 10% of the votes are cast for it in this ballot. Finally, union delegates must be chosen among the candidates in the workplace elections who attract at least 10% of the vote on their name.

The last change introduced by the 2008 reform at the firm-level concerns the conditions under which collective agreements signed by representative unions and the employer are considered legally binding. These conditions were also made more democratic. Before the reform, firm-level collective agreements were considered legally binding as soon as they were signed by one representative union in the firm. This means that the five historical unions could sign legally-binding agreements with the employer against the will of virtually all workers (except the union delegate) and/or in cases where they had almost no local support in the firm. The 2008 reform put an end to this situation by making legally binding only the agreements signed by a union or a group of unions that collected more than 30% of the vote casts at the first round of professional elections.<sup>10</sup>

#### **Bargaining at industry and national levels before and after the 2008 law.**

Before the 2008 reform, the same five historical unions were also *de jure* representative to bargain in the 700 French branches and at the national level. Other unions were excluded from the negotiations at these levels. The 2008 reform has put an end to this monopoly of representation by making representativeness dependent on the results of firm-level elections: to be representative and allowed to negotiate, unions had to attract at least 8% of vote casts at the first round of all firm-level professional elections in the concerned branch or in the country (for national-level representativeness).

**The timing of the 2008 reform.** In the wake of its election as French president in May 2007, Nicolas Sarkozy asked the five historical French unions and employers' associations to bargain over the modernization of industrial relations in France. In April 2008, a "common position" was signed by the two largest employers' organizations and the two largest trade unions CGT and CFDT. This position proposed to base the representativeness of unions at each bargaining level on firm-level election results. The common position faced the opposition of the 3 smaller historical unions (FO, CGC union for managers, CFTC), which feared that they may not reach the minimal vote thresholds necessary to remain representative at the firm, sectoral and even national levels.

Endorsed by the conservative government, the law for "the renovation of social democracy and working time" was voted by the parliament in July 2008, just 4 months after the common position was revealed, and officially published on August, 21<sup>st</sup> 2008. It followed

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<sup>10</sup>A first electoral barrier was actually introduced in 2004: from that date, groups of unions gathering more than 50% of vote casts were allowed to start a procedure to contest an agreement and ultimately invalidate it.

most of the points raised by the common position.

At the industry and national levels, the representativeness of unions started to be based on the results of firms' professional elections from January 1<sup>st</sup> 2009 onwards. This means that all elections from January 1<sup>st</sup> 2009 have consequences not only at the firm level, but also at higher levels. However, four years were necessary to guarantee that all firms had an election under the new legal regime, so that changes in the representativeness of unions at the industry and national levels only started to occur in January 2013 based on the aggregation of all vote casts in each industry and in the entire country.<sup>11</sup> New changes at the national and industry levels occurred again in January 2017 at the end of a second electoral cycle of four years (2013-2016).

All firms that had professional elections after January 1<sup>st</sup> 2009 used the 10% of vote casts threshold introduced by the August 2008 law to determine the local representativeness of union delegates. In contrast, firms that had professional elections in September and October were still using the old rules, while firms that had elections in November or December 2008 may have used any of the two legal regimes. We return to this point in section 4.2 as it is key for our identification strategy.

### 3 Data

The empirical analysis is made using two main sources of data.

**The REPONSE dataset: employer part.** The first dataset is the Ministry of Labor's 2010-2011 French Workplace Employment Relations Survey (REPONSE11) of 4,023 business establishments with more than 10 employees in the non-Agricultural business sector. REPONSE11 is one of the main sources of data on industrial relations in France. A management representative completes a long face-to-face interview based on a 120 pages questionnaire in each establishment, and replies to a large number of questions covering notably the organization of work, the technologies used, and industrial relations. These face-to-face interviews have taken place between January and June 2011. The replies of the management representative constitute the employer part of the survey, from which we retrieve information on the presence of union representatives, the unionization rate, employers' opinion and trust towards union and non-union representatives (see below).

**The REPONSE dataset: employee part.** Then, the "employee" part of the survey comes from a 2-pages 50-questions questionnaire received by mail. These questionnaire have been filled by a core sample of 11,378 workers in the establishments that

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<sup>11</sup>For multi-establishment firms, only firm-level elections are considered. Elections for workers' delegates are only used when there are no available election for work council members. When a firm has more than one election for either workers' delegates or work council members during an electoral cycle of four years, the most recent election is considered in priority.

participated to the employer part of the survey, and an additional sample of 7,158 workers in 2,482 establishments for which no workplace-level information is available. Collected information includes workers' usual demographics, work organization, job satisfaction, union membership, opinion and trust towards union and non-union representatives.

The questionnaires have been sent in two rounds at the end of March and the end of May 2011 to a non-stratified random sample of 10 (5) employees with more than one year of tenure in establishments having 21 or more (between 11 and 20) workers.<sup>12</sup> We perform most analyses at the establishment level (i) because the source of variation we exploit is at the establishment-level (2) to get results that are comparable on the employer and worker sides. Therefore, for all outcomes, we construct the workplace-level average of workers' answers.<sup>13</sup> To ease comparisons with employers' reactions to the reform, our baseline specifications focus on the core sample of workers, and we only use the larger sample for robustness or heterogeneity analysis.

**The MARS dataset.** When they hold a professional election, firms are required to transmit to the administration a report which includes (i) the date (day, month and year) and type (work council members or workers' delegates) of the of the election, (ii) the date of the previous election of the same type, and (iii) the results of the election. The MARS administrative dataset is the compilation of these reports from January 1<sup>st</sup> 2009 onward. It is in principle not available to researchers but we managed to obtain from the Ministry of Labor an extract of all reports made by establishments of REPOSE11 during the period 2009-2012 and those of the additional sample of employees. The extract contains information (i) and (ii), i.e. for all elections held during the period 2009-2012, their date and the date of the previous election of the same type.

**Latest professional election before the REPOSE survey.** Our empirical strategy (see next section) requires to know, for each employer and worker in the 2011 REPOSE survey, the exact date of the latest professional election before she has been interviewed (employers) or filled a questionnaire (workers). For employers, we simply retrieve from MARS the latest relevant professional election before their known interview date. While conceptually simple, this is not entirely straightforward in practice due to the formatting of the MARS dataset and a series of institutional exceptions. We detail our algorithm in Appendix E. The approach is slightly different for workers. They have all filled the REPOSE questionnaire at an unknown date between April 1<sup>st</sup>, 2011, and July, 22<sup>nd</sup>, 2011. We therefore discard establishments that had professional elections during that period for the analysis of workers' responses. For all other establishments, we

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<sup>12</sup>Filled questionnaires stopped being collected on July, 22<sup>nd</sup>, 2011. We use this information to retrieve the relevant election date (see below).

<sup>13</sup>This also avoids the results to be driven by the different sampling scheme for small workplaces or variations across workplaces in the actual number of workers responding to the survey.

consider the latest relevant election date before April 1<sup>st</sup>, 2011.

**Opinion and trust towards Unions in REPONSE11.** Employers and workers are asked to what extent they agree or disagree with the following statements on unions:<sup>14</sup>

- Trade unions play a vital role in representing employees
- Trade unions provide a service to employees
- Trade unions put their own demands and interests ahead of those of the employees
- Trade unions hinder the running of the enterprise

The possible responses are on a 4-point Likert scale from Completely agree to Completely disagree, with also the possibility to answer “Don’t know”.

Because there is little independent information contained in the four different questions we combine them into a single trust/satisfaction index computed as the sum of the two first questions minus the sum of the two last ones. The index is then standardized to have a mean of 0 and a standard deviation of 1. It is our main outcome of interest. The employer questionnaire contains an additional question on the “representativeness of trade unions in general terms” (from very weak to very strong on a 4-point Likert scale) which we also exploit.

The REPONSE survey also contains a similar series of questions on staff representatives (rather than trade unions):

- The staff representatives convey the wishes of employees accurately
- During negotiations, the staff representatives take account of the economic opportunities open to the company
- During negotiations, the staff representatives influence the management’s decisions
- Employees are able to defend their own interests directly

The possible responses are again on a 4-point Likert scale from Completely agree to Completely disagree, with also the possibility to answer “Don’t know”. We combine them in a standardized index of trust/satisfaction towards staff representatives. As the reform only concerns unions and appointed union representatives, it should not impact directly staff representatives, and we use this second series of questions to run placebo tests.

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<sup>14</sup>The description of the questions is based on a translation of the REPONSE questionnaires from French to English made jointly by a team of eight researchers and two professional editors, see Amossé et al. (2016) for further details.

## 4 Empirical approach

We take advantage of the fact that the August 21<sup>th</sup> 2008 reform did not hit all firms/workplaces at the same time. This feature allows us to compare in 2011 workplaces “treated” by the reform with workplaces that have not been treated yet and can serve as a control group.

### 4.1 Sharp Regression Discontinuity Design

**Required assumptions and identification.** Our main identification strategy relies on the fact that the new conditions introduced by the August 21<sup>th</sup> 2008 law started to become effective in a given firm/workplace at the first professional election occurring after a given cut-off date. There is actually some ambiguity regarding this cut-off date and firms that had elections between October and December 2008 may have either applied the old or the new regime. We are sure however that all elections after January 1<sup>st</sup> 2009 were done under the new regime and we start by taking this date as the cut-off date  $c$ . Doing so, we make a first assumption:

**Assumption 1:** All elections before January 1<sup>st</sup> 2009 were organized under the old legal regime, whereas all those after that date were covered by the new regime.

Assumption 1 implies that the assignment to treatment  $T_j$  (union coverage and bargaining decided under the new legal scheme) in workplace  $j$  is  $T_j = \mathbb{1}(D_j > 1^{st}Jan2009)$  where  $D_j$  is the date of the most recent professional election before workplace  $j$  was surveyed in REPOSE11. The way we deal with ambiguity in the cut-off date is discussed in the next subsection.

The key feature that provides identification is that the dates of professional elections around January 1<sup>st</sup> 2009 were set much before the August 21<sup>th</sup> 2008 law was passed, and also before its actual content was known or could be anticipated. This is because professional elections occur within each firm/workplace with more than 10 employees according to a predefined frequency: two, three or four years. As a consequence, for workplaces that already had elections in the past, the last election date  $D_j$  before the REPOSE survey should only depend on former election dates and be unrelated to the reform application date. This would not be true, however, if workplaces can delay or advance their election dates in response to the reform. We show in the next subsection that changing the date of an election can only be done in very specific cases which are such that they are unlikely to allow for endogenous responses to the reform. We therefore make the following assumption:

**Assumption 2:** The election date  $D_j$  for a firm/workplace  $j$  that already had elections in the past can be considered as random with respect to the date at which the new legal regime applies.

Assumptions 1 and 2, which we defend in the next subsection, guarantee identification. For a given variable of interest  $Y$  (measured in the first semester of 2011 using REPOSE11), each workplace has two potential outcomes,  $Y_j(1)$  and  $Y_j(0)$ , corresponding, respectively, to the outcomes that would be observed under the treatment or control conditions. Denote  $\tau = \mathbb{E}[Y_j(1) - Y_j(0) | D_j = 1^{st} Jan 2009]$ , the causal impact of the reform on  $Y$  at the cut-off date.  $\tau$  is identified and can be estimated using workplaces that had elections just around the cut-off date from a sharp regression discontinuity design (RDD) in which the forcing variable is  $D_j$ .

This RDD strategy identifies middle-run effects of the reform as outcomes are measured in the first semester of 2011, that is two to two years and a half after the cut-off date. Such estimates are Local Average Treatment Effects (LATE) in the sense that they are only valid for the last non-compliers and the first compliers with the new legal system, that is the workplaces that applied the old system just before the new one took place and the workplaces that were the first to apply the reform just after it was passed. The reform may have impacted differently early and late compliers, something we cannot assess with the RDD design.

**Estimation.** The estimation typically relies on models of the type:

$$y_{j,2011} = P(D_j) + \beta \mathbb{1}(D_j > 1^{st} Jan 2009) + Q(D_j) * \mathbb{1}(D_j > 1^{st} Jan 2009) + X_j + \epsilon_j \quad (1)$$

where  $y_{j,2011}$  is the outcome of interest measured in 2011 (between January and June for employers) in firm  $j$  and  $\epsilon_j$  a residual term.  $\beta$  estimates the effect of having held the last professional election under the new legal regime.  $P$  and  $Q$  are trends capturing the fact that the date of the election in itself can affect outcomes measured in 2011.<sup>15</sup> This is the case for example if perceptions towards unions change around election periods (e.g. because unions are more active in those periods).  $X_j$  is a set of exogenous control variables, which may not necessarily be included, such as workplace size, age and industry, or the exact month of the interview in the REPOSE11 (for the employer part only).

We estimate variants of (1) with polynomials of order 1 on local bandwidths around the cut-off date. There are several options to do this, and results are sometimes sensitive to the choice of “tuning parameters”. We show in the paper the estimates that are the most logical regarding our context, and provide several robustness checks in the appendix to show that results are not driven by a specific methodological choice.

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<sup>15</sup> $Q$  has no constant term, as such a term could not be identified separately from  $\beta$ .

Our preferred specification does not include controls and uses a standard triangular kernel, giving more weight to the observations that are closer to the cut-off. For our main outcomes, we also show in the appendix estimates when controls are included and based on a uniform kernel which weights all observations in the bandwidth equally. As an additional robustness check, we also provide estimates obtained after excluding the observations that are very close to the cut-off and may drive the results. This is the so-called “donut-hole approach” that we perform with holes of various sizes. This is an important check in our context because there is some uncertainty on the exact location of the cut-off and we cannot fully discard manipulation of the running variable just around the cut-off.

The local bandwidths on which the estimation takes place are determined endogenously for each outcome. By default, we use the MSERD bandwidths developed by Calonico et al. (2014) (or Calonico et al. (forthcoming) when controls are included), as they limit potential bias the most, but we also provide estimates based on alternative bandwidths for our main outcomes of interest in the appendix. Finally, in all Tables showing RDD results, we provide both (i) conventional estimates and p-values obtained from estimating equation 1 by OLS on the endogenously determined bandwidth, and (ii) bias-corrected estimates computed following Calonico et al. (2014) as well as their associated robust standard errors and p-values.<sup>16</sup>

In addition to estimates, we provide graphical evidence regarding possible discontinuities on our main outcomes at the cut-off. This is done on a large four-year window. On the left (right) side of the cut-off, we group observations in four (twelve) equal-size bins, so that we have approximately 125 observations in each bin on each side of the cut-off. Variants of these graphs with more bins are also shown in the appendix for some of our main outcomes.

**Sample restrictions.** First, the identification relies on the fact that the election date around the reform implementation is predetermined because it depends on past election dates. This is of course not the case for workplaces that are too young to have had elections before 2007. Based on a categorical variable in REPOSE11, we therefore remove from the sample workplaces that are younger than five years in 2011 (representing 3.5% of the initial sample).<sup>17</sup> Second, workplaces that have professional elections every two years should have had their latest election date under the new legal regime ( $D_j > 1^{st} Jan 2009$ ) when observed in the first semester of 2011 in REPOSE11. This implies that they cannot be used to identify the impact of the 2008 reform. As these workplaces may even induce a

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<sup>16</sup>Estimates and standard errors are obtained using the Stata command *rdrobust*.

<sup>17</sup>Workplaces older than five years should have had at least two elections before REPOSE11, implying that the latest election date is indeed predetermined. These workplaces may still have had only one election if they were too small in the past to organize elections. We will present robustness checks for large workplaces to show that our results are not affected by this potential issue.

discontinuity at the threshold (they only appear on the right), we remove them from the analysis sample, so we keep only workplaces having elections every three and four years (about 83% of the sample of workplaces older than five years). After these restrictions, our final sample includes 1911 workplaces. 502 had their most recent professional election before the cut-off date, while 1409 had it after.

## 4.2 Election dates and threats to identification

**Cut-off date for the application of the August 21<sup>th</sup> 2008 law.** We do not have direct information on whether it is the old or new statutory scheme that governs employment relations in workplaces of REPOSE11. The new legal schemes is supposed to be applied by all firms/workplaces that started to *prepare* for professional elections after August 21<sup>th</sup> 2008. Conversely, firms/workplaces that already entered the pre-election preparation period before August 21<sup>th</sup> 2008 are supposed to apply the old scheme. This pre-election preparation period lasts about two months and no less than 45 days. It starts with a preparatory meeting between the firm/workplace social partners, resulting in the production of a pre-election protocol which states the rules governing the election and its date (which has to be published at least 45 days in advance). As a consequence of these institutional rules, we assume that workplaces which had elections in late August<sup>18</sup>, September and October 2008 started to prepare these elections before the reform was passed on August 21<sup>th</sup> 2008 and applied the old scheme.

Elections held in November and December 2008 are more difficult to deal with. They are more likely to have entered their preparation phase after the summer break and therefore to apply the new scheme. However, some uncertainty remains because the August 21<sup>th</sup> 2008 reform only modified the upper layer of the labor law. As is common in French policy, the administration provided a comprehensive interpretation of the law. The August 21<sup>th</sup> 2008 reform was followed by one ministerial circular (*Circulaire d'Application*) dated November 13<sup>th</sup> 2008, but officially published on December 30<sup>th</sup> 2008 in the Bulletin of the Ministry of Labor. As a consequence, there remains some uncertainty on the scheme governing elections in November and December 2008. We think they are more likely to have been organized under the spirit of the old regime and have set in our baseline analyses the cut-off date for the application of the new scheme on January 1<sup>st</sup> 2009.

A final reason to use January 1<sup>st</sup> 2009 as cut-off date is that we know for sure that elections before that date did not count to establish the representativeness of labor unions at the industry and national levels. The administrative data on workplace- and firm-level professional elections that are used to establish this representativeness (the MARS data-see previous section) only started to be collected on January 1<sup>st</sup> 2009. This means

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<sup>18</sup>Almost no workplaces hold elections in July or August, a period during which workers tend to be on vacations.

that one of the three major changes introduced by the August 2008 law regarding union representativeness applied sharply at our chosen cut-off date.

By making fuzzy the discontinuity at the chosen cut-off date, the uncertainty on the application of the other legal provisions of the reform is likely to reduce our estimates, unless workplaces that held elections in November-December intentionally selected the new or the old scheme in a way that generates biases. The “donut hole” RDD specifications will be used to check for this.

**Manipulation of election dates.** A key assumption for the RDD design to work is that workplaces cannot select themselves into treatment by manipulating the election date. Unsurprisingly such manipulations are on paper extremely hard to conduct in a democratic country like France. First, extending the mandate of incumbents may constitute an obvious breach of democracy, and it is strictly controlled. Mandates can only be extended for a “reasonable period” if all representative unions and the employer jointly agree to do so. This required consensus leaves limited room for strategic behavior (because it is virtually impossible that all actors gain from it, see further details in Appendix A). Second, for each worker representative elected during professional elections, a deputy representative is also elective, so that if a representative steps down or leaves the firm, there is no need to organize immediately an election. As a consequence, an election can only be advanced if all worker representatives step down or are fired, which again leaves limited room for strategic behavior.<sup>19</sup>

To study if the legal time span between two elections is respected in practice, Figure 2 plots the distribution of the average number of months between two consecutive elections (for all election registered over the period 2009-2012 in workplaces of the REPOSE11 sample). The distribution exhibits peaks at 24, 36 and 48 months, corresponding to the three possible legal time laps between two elections. These peaks concern more than 60% of registered elections for which the distance to the three expected election dates is smaller than 30 days. Other cases may correspond to pure measurement error (especially regarding the registration of the previous election date), official modifications of the election calendar corresponding to the institutional cases specified above and in Appendix B, or the necessity to reorganize immediately the election due to a procedural flaw. The small peak at zero on Figure 2 may reflect the first or the last of these cases.

The standard approach to detect manipulation is to look for a discontinuity in the density of the forcing variable around the cut-off (McCrary (2008) for continuous variable).

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<sup>19</sup>Even if election dates could be easily manipulated, only a few firms could have done so in response to the August 21<sup>th</sup> 2008 law. This is because the content of the law was only known on April 9<sup>th</sup> 2008. It resulted from a negotiation phase between social partners at the national level whose outcome could not be predicted before that date. This implies that only workplaces that started to prepare elections after April 2008 and should have held them before January 2009 could have been tempted to manipulate their election date *in response* to the reform.

However, such an approach requires the forcing variable to be smoothly distributed in the absence of manipulation, a condition that is not necessary to perform a RDD, but that is usually verified in standard RDD settings. Figure 3 plots the distribution of our forcing variable (the date of the latest election before the REPOSE11 survey) and shows that it has a strong cyclical pattern, preventing us from testing for discontinuity around the cut-off.

Panel (a) of Figure 3 show that election dates are very seasonal, with almost no elections during July and August and that elections in 2010 are strongly over-represented. This is explained by several factors. First, workplaces that have elections every three years are more likely to have had their most recent election before REPOSE11 in 2010 than 2007 or early 2008. Second, as the REPOSE interviews take place in the first semester of 2011, there are only few workplaces that had an election in 2011 before this survey. The distribution in Figure 3 is finally driven by historical reforms that had long-run consequences on the election periods. In particular, the default time span between two elections was extended from one year to two years in 1993, and then from two years to four years on August 3<sup>rd</sup> 2005.<sup>20</sup> This second change implied for example that, absent of firm- or industry-level agreement, workplaces that should have had elections in 2006, 2008 and 2010 only had elections in 2006 and 2010. The first one may also have had long-term consequences that contribute to explain the shape of the distribution in Figure 3, but that are not a direct threat for our identification strategy providing that workplaces cannot deviate from the pre-established election calendar in response to the reform or for other reasons correlated with the impact of the reform.

To check for strategic manipulation, panel (b) of Figure 3 depicts the distribution of election dates around the 1<sup>st</sup> January 2009 (cut-off date) and the 1<sup>st</sup> January 2010. The two distributions are not perfectly comparable. However, they have the same profile just around the 1<sup>st</sup> of January of each year, suggesting that nothing special happened around our cut-off date.

Another way to test for possible strategic manipulation of election dates is to look for discontinuities in predetermined covariates at the cutoff: if any, the workplaces that may have postponed or advanced their election in response to the reform are likely to differ in terms of observable characteristics such as their size, age, sector, region, etc. Indeed, employment relations and union coverage vary strongly by, *e.g.*, firm size and sector whose distribution around the cutoff is therefore likely to be impacted in case of manipulation of election dates. Table 2 provides descriptive statistics on workplace observable characteristics and checks for discontinuities at the cutoff for these variables. Estimated discontinuities are all not statistically significant at the 10% level, suggesting that manipulation in our context was limited.

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<sup>20</sup>These regulations did not change the length of ongoing mandates and only applied to subsequent mandates.

## 5 Main results

A reform that changes the conditions of union recognition in firms is likely to affect employment relations along three major dimensions: (1) workers' representation and more specifically the prevalence of unions and union members, (2) how unions are perceived by employers and workers, and (3) workplace conflicts and social climate. We study these outcomes in turn. We start with the results from our baseline specifications for these three sets of outcomes and then summarize the results of robustness checks and falsification tests for all outcomes in a last subsection. In the main tables, we systematically provide the conventional and bias-corrected regression discontinuity (R.D.) estimates, as well as the value of the interest variable just at the left of the cutoff.

### 5.1 Workers' representation and union membership

**Work councils and workers' delegates.** Our identification strategy requires to restrict the analysis to workplaces for which we observe elections for workers' delegates or work council members (or members of the *Délégation Unique*, which is a merger of the two). Using employers' declarations, we start by checking that workplaces in our sample indeed have this type of representation. Table 3 (first row) shows that this is the case for more than 93% of them. The absence of workers' delegates or of a work council declared by employers in a few workplaces may be explained by the fact that all elected representatives have resigned and were not replaced, or by measurement error in employers' declarations.

R.D. estimates then indicate that workplaces that had professional elections after January 1<sup>st</sup> 2009 are around 10 percentage points more likely to still have workers' delegates or a work council when surveyed in 2011 (Figure 4, panel A, and Table 3). This is consistent with the idea that representatives elected after the reform were less likely to resign, something we cannot test directly. However, the estimated effect is not statistically significant, and no definitive conclusion should be drawn.

**Union recognition.** Workplaces that have professional elections do not necessarily have unions recognized for bargaining. For this to happen, a worker (at least) should have accepted to be union representative, and for elections taking place after the reform, she should have obtained at least 10% of the votes at professional elections. Before the reform, the five historical unions could be represented much more easily than other unions. In total, the reform has ambiguous effects on union recognition. The 10% threshold introduces a barrier to entry that may discourage workers to become a union representative. However, as the conditions of appointment have improved for non-historical unions, there may be more candidacies from them. Finally, votes gathered at elections will count to determine the representativeness of all unions at the branch and national levels. In a

context of uncertainty regarding the number of votes each union can get, this new rule provides a strong incentive for unions to find candidates for elections in each and any firm. This is because these votes will be added to their sectoral and national total number of votes even if the union loses the election.

Figure 4, panel B<sup>21</sup>, and Table 3 (second row) show that the reform has had a strong positive impact on union recognition. Namely, it has shifted the probability to have at least a union recognized for bargaining from a bit less than 60% to about 80%. Interestingly, this large effect is mostly driven by historical unions (Table 3). This suggests that the incentives provided by the representativeness criteria at higher levels of bargaining dominated the introduction of a barrier to entry for these unions.

Regarding new unions, we find that the estimated effect of the reform is positive as expected. It large in relative terms, with a jump in the probability to be recognized from around 9% to almost 20%, but it is not statistically significant at conventional levels.

**Multi-unionism.** By increasing incentives for historical unions to participate to elections and the end of barriers for challengers, the reform should boost multi-unionism. However, the introduction of the requirement to obtain at least 10% of votes mechanically limits the number of unions (or coalitions of unions) that can be represented in a workplace.

We check if these direct consequences of the reform can be observed in the data. Figure 4, panel C, and Table 3 show some evidence that it is indeed the case. Estimates are not significant at conventional levels and should only considered as suggestive. We find that the probability of multi-unionism (two unions or more) in a workplace jumped by 12 percentage points due to the reform. The estimated impact of the reform on the probabilities to have three or four unions (Table 3) is also positive, but highly not significant. By contrast, the probability to have five unions or more in a workplace dropped from 10 to less than 7 percent due to the reform.<sup>22</sup> The estimates are again not statistically significant, likely because our sample is too small to detect a drop in the proportion of workplaces having five unions or more which is only around 10% prior to the reform.

In total, there is suggestive evidence that the reform has increased the probability that a workplace has only one or a few unions recognized and decreased the probability to have a large number of unions recognized. Our sample size is however too small to draw any clear-cut conclusion on the extent and intensity of multi-unionism.

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<sup>21</sup>Versions of this important figure with more bins are provided in Figure B1 in appendix B.

<sup>22</sup>As RDD estimates are obtained on local bandwidth and based on a triangular kernel, they do not always exactly coincide with the estimates than can be inferred on the graphs where a four year window and a uniform kernel are used. This is the case here regarding the probability to have five unions or more in a workplace.

**Union membership.** By allowing workers to elect their union representatives, has the reform fostered workers' sense of fit with unions and ultimately their likelihood to be a union member? We study this using two data sources. The first is the employers' declarations regarding the unionization rate in their workplace. From this source, we find a strong LATE of the reform on workplace-level union membership, with a jump from about 5% to 14% (Figure 5, panel A and Table 3). This result contrasts with the monographies of (Yon & Bérout 2013) in which the interviewed human resource managers and union representatives (but not workers) did not mention a renewal of workers' participation to unions.

While the REPOSE11 sample is supposed to be representative whereas the monographies are not, the apparent large impact on union membership declared by employers on REPOSE11 may be a statistical artifact or result from the fact that on this sample union members become more active after the reform and are therefore perceived as more numerous by the employer after the reform, even though they are not. Using our second source of information, we reject these potential caveats. This source consists in the union membership status declared by the workers surveyed in REPOSE and averaged at the workplace level. The resulting measure of workplace-level union membership is constructed to be comparable to employers' declarations. It cannot be biased by misleading employers' perceptions, but it is noisy, as it is built from only a handful of workers responding to the survey in each workplace. Results based on this measure largely confirm employers' declarations (Figure 5, panel B and Table 3). When we restrict the analysis to the core sample of workers (including only workplaces where employers also participated to the survey), we find very similar magnitudes, albeit only significant at the 10% level. Estimated effects become even larger and highly significant when we focus on the whole sample of workers.

However, we conclude to a very large LATE of the reform on union membership in private workplaces with 11 or more workers, a result to which we will return in the next section.

## 5.2 Employers' and employees' perceptions of unions

We now turn to our second main question of interest: has the reform improved employers' and employees' perceptions of, and trust towards unions?

**Employers' perceptions.** We start by looking at the question regarding the representativeness of unions in general (only asked to employers). Prior to applying the reform provisions, almost 40% of employers considered unions' representativeness to be very weak. This widespread feeling that unions are not representative might reflect the absence of direct democracy for the appointment of their delegates at the firm and work-

place levels. Figure 6, panel A, and Table 4 confirm this intuition by showing that the reform has lowered the probability that employers consider the representativeness of labor unions to be very weak to about 20 percentage points, dividing it by about two.

We then find an effect of the reform of about 45% of a standard deviation (s.d.) on the (standardized) index capturing employers' positive perceptions of unions operating in the workplace (Figure 6, panel B, and Table 4).<sup>23</sup> To get estimates that can be interpreted as probabilities, we have also constructed binary variables from the four-answer questions entering this index, and estimated the LATE of the reform of each of them. Table 4 shows a positive LATE is found for all four items entering the index. The probability that employers agree with the claim that trade unions play a vital role in representing employees or that they provide a service to employees increases by about 25% percentage points due to the reform. Employers that have already applied the reform provisions are also about 15 percentage points less likely to declare that trade unions put their own demands and interests ahead of those of the employees or that trade unions hinder the running of the enterprise, these two estimated LATE being however not statistically significant. These findings are here consistent with the monographies of (Yon & Bérout 2013) which show that the representativeness based on elections "institutionalized" the bargaining unions and doing so increased their legitimacy from the point of view of employers.

The reform also seems to have improved employers' perception of staff representatives by 30 to 40% of a standard deviation. Staff representatives include union delegates but also workers' delegates and members of the work council (who can be unionized or not). The question therefore targets jointly union representatives who have been impacted by the reform, and other type of representatives who are not directly concerned by the reform. This may explain why the estimated LATE for this index is smaller than that for the index capturing how employers perceive unions.

The reform effect on employers' perceptions of unions is likely to be partly explained by its effect on union recognition. Indeed, the index capturing the perceptions of unions is 50% of a s.d. larger in workplaces where unions are recognized for bargaining. The effect of the reform on the probability to have a union recognized (20 percentage points) may in its own generate part of the effect on perceptions.<sup>24</sup> It is not possible to know what share of the effect on trust is driven by union recognition without making strong hypotheses on the trust levels and evolution of these levels in workplaces that become unionized due to the reform. When we focus only on workplaces where unions are recognized (not comparable before and after the reform), we get a robust R.D. estimate the reform effect on trust of 20% of a s.d. (not statistically significant), while the estimate is equal to 36%

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<sup>23</sup>Variants of Figure 6, panel B with more bins are provided in Figure B2 in appendix B. When there is no union in the workplace, employers are still asked to provide answers to the questions entering the index, but regarding unions in general rather than in their workplace.

<sup>24</sup>Around 10% of a s.d. if we assume that workplaces that become unionized due to the reform also experience employers' perceptions of unions jump by 50% of a s.d.

of a s.d. (statistically significant at the 10% level) when we instead control for union recognition (endogenous control). These results combined with the order of magnitude of the estimated effects of the reform on union recognition and employers' perceptions suggest that the latter is unlikely to be entirely driven by the former.

**Employees' perceptions.** When we limit the analysis to the core sample of workers, we find a 23% of a s.d. non-significant effect of the reform on workers' perceptions of unions in their workplace (Figure 6, panel C, and Table 4). Estimated LATE for the questions entering the index are usually positive but much smaller in magnitude than on the employer side, and far from conventional levels of statistical significance.

When the sample is expanded to also include workplaces that do not appear in the employers' survey, the estimated LATE increases to about 35% of a s.d. and becomes significant at the 10% level (Table 4).<sup>25</sup> As for employers, this result appears mostly driven by employers' improved perceptions that unions play a vital role and provide a service to employees; we find significant estimates for both variables.

We conclude from the results in this section that the introduction of more direct democracy for union recognition has had an overall positive effect on "stakeholders" perceptions of unions: the effect is large and positive for employers, while it is not negative for workers. There is limited evidence that the reform had a positive effect on workers' perceptions, but it is established on a larger sample of workplaces for which we only know election dates. This result should therefore be considered with prudence.

### 5.3 Social climate

**Conflicts and social climate declared by employers.** Changing the conditions of union recognition is also likely to affect the social climate and likelihood of conflicts in the workplace through differing channels. E.g. it may foster corporation and thus reduce conflicts; conversely, higher membership and increasing competition between unions may push them to be more aggressive.

Our estimates (Table 5) suggest the reform deteriorates the quality of the social climate perceived by the employer by around 30% of a s.d., this effect being however not statistically significant.

Consistently, we find that workplaces where the last professional elections occurred under the new legal regime are more likely to have experienced a work stoppage during the three-year period 2008-2010 (Figure 7, panel A, and Table 5, first row). According to our R.D. estimates based on employers' declarations, the probability to have experienced such events jumps from about 25% to around 50% due to the reform. This statistically

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<sup>25</sup>The corresponding figure is provided in Appendix B.

significant effect appears to be entirely driven by walkouts rather than strikes (Table 5), suggesting that the reform induces workers to voice more, without this additional voice leading to more official and harsher forms of conflicts. More interestingly, this additional voice induced by the reform goes along with a better perception of unions by employers.

**Conflicts and job satisfaction declared by employees.** The workers' questionnaire includes a question on the participation to a work stoppage, but does not make it possible to study different types of stoppages separately. We find that workers' workplace average participation to a work stoppage has increased due to the reform from around 15% to between 20 and 25%.<sup>26</sup> R.D. estimates obtained on the core and larger samples of workers are both not statistically significant. They are however consistent with results obtained from employers, suggesting that the absence of statistical significance might be more driven by statistical noise than by an absence of effect. With a note of caution, we conclude to a positive effect of the reform on the participation to work stoppages.<sup>27</sup>

In addition, we have checked the effect of the reform on workers job satisfaction in general (Table 5) and various sub-dimensions of job satisfaction (regarding pay, training, working conditions, ambiance at work). The estimated effect of the reform on these outcomes is close to zero.

**Voluntary quits.** The study of quits provides an additional insight on the climate within the workplace. Our version of the REPOSE survey has been matched by the Ministry of Labor with administrative information on workplace entries and exits. We use this data to measure the rate of voluntary quits in each workplace in 2011 as the total number of quits divided by the average number of employees in the workplace that year. Estimates in Table 5 reveal that workplaces that apply the new legal regime have a substantially lower rate of quits in 2011 (around 5% against 7%). These effects are however not statistically significant.<sup>28</sup> Even if not fully conclusive, our results are consistent with local direct democracy in the workplace inducing more voice and fewer exits, hence shifting the employees' behavior in the workplace from the more economic entry/exit model to the voice, exit and loyalty described by Hirschman (1970). We will return to that result in the next section.

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<sup>26</sup>The variable we look at here is not directly comparable to the prevalence of work stoppage as there can be a work stoppage in a workplace that do not concerns all employees.

<sup>27</sup>When we adjust p-values for multiple hypotheses testing across the three estimates of work stoppages (from employers' declarations, and from workers' declarations on either the core or larger sample of workers) using the False Discovery Rate (FDR) control method, we still find the adjusted p-value (q-value) for the estimated effect of the reform on work stoppages declared by employers to be just below 10%.

<sup>28</sup>Actually, we did not expect to identify a significant effect. Our sample size only makes it possible to detect very large impacts that would be hardly intuitive.

## 5.4 Falsification tests and robustness of RDD estimates

We have shown positive and significant effects of the reform on five main outcomes defined at the workplace level: union recognition, unionization rate, employers' perceptions of unions, employees' perceptions of unions, and work stoppages. We now provide falsification tests and robustness checks for these outcomes. Regarding unionization rates and work stoppages, we have estimates obtained from both the employer part and the employee part of the REPOSE11 survey. We provide robustness checks for both types of estimates. Finally, the estimated impact of the reform on social climate was close to be statistically significant and we also provide robustness checks for this outcome in order to see how statistical significance evolves when alternative empirical specifications are adopted. This leaves us with eight outcomes for which we check robustness and provide falsification tests.

**Falsification tests.** A first falsification test is the investigation presented in Table 2 of possible discontinuities at the cutoff in predetermined covariates. We now complement this test with the investigation of discontinuities in our eight main outcomes of interest at two placebo cutoffs: January 1<sup>st</sup> 2010 and April 15<sup>th</sup> 2009. The first cutoff is particularly important because it allows us to check that our main results are not driven by seasonality, i.e. the fact that, for a reason unknown to us, having a professional election at the beginning rather than the end of a calendar year impacts employment relations in a way that can be confounded with the reform impact. Results in Table C1 show that this is not the case. Most R.D. estimates at a placebo cutoff on January 1<sup>st</sup> 2010 are close to zero and not statistically significant. The only exception is the existence of strikes and work stoppages between 2008 and 2010 for which we find positive and significant conventional and bias-corrected estimates.

There is no obvious other placebo cutoff that stands out. We have chosen April 15<sup>th</sup> 2009 because it is in a period with many elections and in the middle rather than at the beginning of a month (something that is unlikely to play any role, but that we can nevertheless check). For this cutoff, we find only non significant estimates (Table C2).

**Donut-hole approach.** We start by applying the so-called “donut-hole” approach which consists in excluding observations that are close to the cut-off before computing R.D. estimates. This is an important check in our case as we cannot entirely exclude that some elections have been slightly delayed around the cut-off, or that elections in November-December 2008 already applied some of the new rules introduced by the August 2008 reform. Figure D5 provides biased-corrected R.D. estimates for our eight main outcomes obtained after removing 15 to 60 days on each side of the cut-off. The smallest hole excludes workplaces that had elections between December 16<sup>th</sup> 2008 and January 15<sup>th</sup>

2009, while the largest one excludes workplaces that had elections between November 1<sup>st</sup> 2008 and March 1<sup>st</sup> 2009.

Compared to the baseline estimate (corresponding to a donut hole radius of zero on Figure D5), excluding 15 days on each side of the cutoff tends to increase a little bit the magnitude of R.D. estimates and does not change their statistical significance. When the donut hole is increased, point estimates usually increase further but tend to become less precise. With 60 days excluded on each side of the cutoff, the estimated effect of the reform on employers' perceptions has for example increased to almost one s.d., but the estimate is so imprecise that it is not significant at the 5% level anymore. Regarding employees' perceptions, excluding observations around the cutoff also increases the magnitude of the R.D. estimates, but these estimates however always remain non-significant. For other outcomes, the donut hole approach with various hole radius comforts our main results. In particular the impact of the reform on social climate remains not statistically significant with a donut hole.

**Varying bandwidth size.** Figure D6 provides R.D. conventional estimates for various bandwidth sizes defined as the number of days used on *each side* of the cut-off. The smallest bandwidth of 200 days correspond to using a bit less than 7 months on each side of the cutoff, while the largest one basically includes the whole sample (more than 2 years on each side of the cut-off). The optimal bandwidth corresponds to the vertical dashed line. When the smallest bandwidth is used, reform effects are very imprecisely estimated and usually not statistically significant. Reassuringly, they are however not very different from the estimates obtained with the optimal bandwidth. For all other bandwidth sizes, the estimated R.D. are usually also close to those obtained with the optimal bandwidth and tend to have the same level of statistical significance. Overall, conventional estimates obtained on bandwidth of various sizes comfort the main results.<sup>29</sup>

**Controls and uniform kernel.** Tables D3 and D4 respectively present R.D. estimates for the main outcomes of interest when (1) control variables are added to the baseline specification, and (2) a uniform instead of triangular kernel is used. Controls in table D3 include the variables used for the balancing checks (see Table 2) as well as controls for workforce composition. Panel A includes controls for the months of 2011 at which the interview with employers took place in order to capture seasonal effect or, in combination with the running variable, the effect of the exact time laps between the last professional election and the REPOSE11 survey. Panel B includes controls for the

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<sup>29</sup>For the sake of completeness, Figure D7 also provides bias-corrected estimates for various bandwidth sizes. Here again, the results from our baseline specification are comforted. Note however that the bias-correction offered by Calonico et al. (2014) is intrinsically related to the choice of the bandwidth size and setting manually the bandwidth impacts the bias-correction and can lead to misleading results, implying that results in Figure D7 should be considered with caution.

surveyed workers mean characteristics to make sure our main results are not driven by the respondents' demographics.

Most of our results (or absence of results) are robust to the inclusion of controls and change in kernels with only a few exceptions. First, the effect of the reform on work stoppages becomes 30 to 40% smaller and no longer statistically significant when controls are included. Second, the unionization rates declared by employers and workers also become slightly smaller and only marginally significant when controls are included. Third, the impact of the reform on employees' trust is no longer significant on the larger sample of workers when we use a uniform kernel instead of a triangular one. This confirms that the evidence regarding an effect of the reform on workers perceptions of unions is only suggestive. Overall, these small differences with the baseline specifications are not surprising since most impacts cannot be very precisely estimated, with confidence intervals never very far from including zero.

**Wrapping up.** We conclude from these checks that the 2008 reform had a positive impact on employers' trust, union recognition and unionization. Estimated R.D. for these variables are indeed still significant in all our checks. Regarding work stoppages, the evidence from employers and workers responses are also consistent and suggest a certain impact of the reform (with the small caveat that the estimated effect becomes a bit smaller and not significant when controls are included). There is only evocative evidence of an effect of the reform on workers' perceptions of unions. The effect on social climate is however not robust.

## 6 Discussion and conclusion

This section discusses (i) the average and medium-run effects of the 2008 reform, (ii) the channels through which the reform has impacted employers and employees, and (iii) the idea that the reform may have induced a partial shift from an entry/exit to a voice/loyalty type of regulation of employment relations in France. It ends with a brief general conclusion.

### 6.1 Average and medium-run effects of electoral democracy

The estimated local average treatment effects of the 2008 reform on union coverage, unionization and employers' or employees' trust towards unions are quantitatively very large. Regarding unionization, these large magnitude appear at odd with the fact that the unionization rate remained constant in France during that period (Pignoni 2016). Regarding trust, the estimated local effect of the 2008 reform contrasts with the idea

that social capital is hard to build in the short run.<sup>30</sup> We therefore provide a careful examination of the possible average and medium run effects of the 2008 reform.

**Getting away from the cut-off.** By construction, local average treatment effects are based on the comparison of the first treated and the last non-treated workplaces. Facing new rules, unions may have over-reacted just after the reform was enacted, leading to a discrepancy between LATE observed in the first semester of 2011 and average treatment effects.<sup>31</sup> For example, the new legal regime was not only putting unions in competition within firms, but also at the sector and national levels. Loosing recognition at these higher levels (especially the national level) could have had dramatic consequences for unions and their top representatives or employees. The fear of loosing their jobs may have pushed union employees to a lot of initial extra efforts to organize new firms and collect as many votes as possible.

Suggestive evidence on average treatment effect can first be obtained by looking at firms further away from the cut-off date on RDD graphs. Of course, these firms are not directly comparable because they had their professional election at a different date and because the time laps between a professional election and the moment where the outcomes are measured is likely to affect these outcomes. The effect of this time laps is captured by the slopes of the fitted lines on RDD graphs. When looking at Figures 4 to 7, we see that for our main outcomes fitted lines have a comparable slope at the left and at the right of the cut-off (usually flat or slightly negative). As there is no obvious reason why the distance to the last professional election would affect very differently treated and non-treated workplaces, we may expect to observe similar slopes at each side of the cut-off, *unless* the effect of the reform on the first compliers was much stronger than its effect on latter ones (in which case we would get a more negative slope on the right). Hence, the fact that fitted slopes are similar on each side of the cut-off provides some evidence that effects were not strongly vanishing over time.

**Cross-country comparison of cooperation in labor-employer relations over the reform period.** The country-level evolution of trust in unions or labor-employer cooperation over the reform period is likely to be driven by several alternative factors, such as the global crisis or other institutional changes that happened at the same time. Nevertheless, if the average treatment effect of the reform is (i) comparable in magnitude

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<sup>30</sup>A French best-seller book published in 2007 (Algan & Cahuc 2007) suggested that France was suffering from a general lack of trust which had several detrimental impacts on the French society. Our estimates may also appear slightly at odds with the widespread view that France still suffers from a lack of trust in general and non-cooperative employment relations in particular.

<sup>31</sup>As LATE are observed about 2 years after the adoption of the reform, we should formally compare them with the average treatment effect (i.e., over all workplaces) two years after treatment. This is not directly possible and we provide here a discussion of what could be ATE at different points in time close to the reform

to the LATE estimates and (ii) persists over time, we may hope to find some evidence of it in macro series. This is indeed the case as shown in Figure 1: among the 19 countries, France is the one that had the highest increase in cooperation in labor-employer relations declared by managers over the period 2007-2008 to 2016-2017. This is not due to our selection of countries. Among the 122 countries that participated to the World Economic Forum survey in 2007-2008, 2010-2011 (middle of the application period of the reform and closest year to REPOSE11) and 2016-2017 (four years after all firms have applied the reform), France has the worst cooperation in labor-employer relations declared by managers (behind several developing countries and dictatorial regimes), while it ranks 112th in 2007-2008 (having gained 10 places) and 97th in 2016-2017 (not anymore in the bottom quintile). The absolute increase in declared cooperation observed in France is the 20th largest one over the period 2007-2008 to 2010-2011 and the sixth largest one over the whole period 2007-2008 to 2016-2017 (still among the same 122 countries). These observations are consistent with a large and persistent effect of the 2008 reform on cooperation.

Statistics on trust in unions based on the Eurobarometer and World Value Surveys are also consistent with a positive average effect of the reform on workers' trust, at least in the short-run. Indeed among the 35 OECD countries, France has experienced the third largest increase in the trust in trade unions among the total population between 2005 and 2010 (Figure 4.9b in OECD (2017) which we reproduce in Appendix Figure B3).<sup>32</sup>

**Evolution of unionization in the French private and public sectors.** The estimated LATE of the reform on the unionization rate of around 8 percentage points (which represents a doubling of the unionization rate) seems at odds with the statistics provided by the Ministry of Labor showing that unionization in France is almost flat since the early 1990s (Pignoni 2016). These seemingly paradoxical results can be reconciled by breaking down the evolution of the unionization rate in France by sectors concerned and not concerned by the 2008 reform. This can be done using the French version of the EU-SILC survey (Survey of Income and Living Conditions), which was the official source for the French unionization rate in 2008 and 2010.<sup>33</sup> Unfortunately, prior to 2008, the question used to measure union membership in the ancestor of the French SILC survey was diluted in a series of questions regarding membership to various associations, which led to an under-estimation of membership (Pignoni 2016). In the absence of alternative data sources, there is therefore no reliable statistics on union membership prior to 2008 and we can only provide consistent statistics from that date.

<sup>32</sup>unfortunately, the Eurobarometer abandoned after 2010 the specific question on trust in unions, preventing us to look at medium-run effects.

<sup>33</sup>The French working conditions survey is now the main source for unionization according to the French ministry of labor because it has a larger sample which includes, contrary to EU-SILC and REPOSE, overseas départements.

Results are presented in the first panel of Table 6 which shows the evolution of the membership rate from 2008 to 2016 in public administrations (not concerned by the 2008 reform) and in private sector workplaces with 10 employees or less (not concerned by the reform) and with more than 10 employees (concerned by the reform). Union membership steadily declined in public administrations from 20.3% in 2008 to 17.4% in 2016, according to EU-SILC.<sup>34</sup> The contrast is patent with the private sector workplaces concerned by the reform. In these workplaces, unionization steadily increased from 9.0% in 2008 to 11.1% in 2016. A similar 2 percentage point increase is found if we restrict further the sample to workers having at least one year of tenure, so that we match the REPOSE2011 sample on which RDD estimates are established. If we compute a simple difference-in-differences, we get a jump of membership in workplaces concerned by the reform relative to public administration of 1.8 percentage point over the short period 2008-2010, and of exactly 5 percentage point over the whole period 2008-2016. The relative evolution over the period 2008-2010 can only capture part of the reform effect as about half of private sector workplaces with more than 10 employees have not been affected by the reform yet. This may explain why this short-run evolution over two years is smaller than the long-run evolution and also much smaller than the LATE estimates (even though it remains in their 95% confidence intervals). The 5 percentage point long-run relative evolution between workplaces concerned and not concerned by the reform is however capturing both the full reform effect on unionization and its persistence on the medium-run.

Of course, our simple difference-in-differences can also capture several other factors that affect differently the public and private sectors, leading to diverging trends between them. To limit this concern, we have tried to estimate the unionization rate prior to the 2008 reform. The only data source allowing to build consistent series before and after 2008 is actually the REPOSE survey, as it also took place in 2005. These estimates are however only available for part of the private sector and should not be directly compared to the estimates obtained from SILC on similar samples. An additional challenge is that REPOSE in 2005 only includes workplaces with more than 20 employees. We provide an estimate for 2005 comparable with other statistics in Table 6 by multiplying the unionization rate in REPOSE 2005 by the ratio between the SILC estimate in 2010 for a sample corresponding to REPOSE11 (11.4%) and the estimate obtained for workplaces with more than 20 employees using the REPOSE11 employer survey. We provide more justification in appendix E for this calibration as well as alternative adjustments. In all cases, we find that union membership was higher in 2005 than in 2008 on the REPOSE sample and therefore reverted in 2008.

The observed reversal is all the more remarkable that unionization was probably declining or constant since the mid 1970s (Pignoni 2016) and is in general a-cyclical or

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<sup>34</sup>The French working conditions survey confirms an erosion of unionization in the public sector from 2013 to 2016 and suggests a stability in the public sector.

pro-cyclical in the private sector (see Schnabel (2003) for a review), which suggests that the global crisis may have affected it negatively from 2007 onward. This could be for example because it has led to a larger contraction of the more unionized manufacturing sector and to more difficult market conditions in the private sector that may be less conducive to unionization. To control for these changes to some extent, we reproduce the evolution of the unionization rate over the 2008-2016 period while holding the distribution of the characteristics of workers and their jobs to their 2008 level. We do so by propensity score reweighting (or “DFL reweighting”) as in Autor et al. (2008), adapting the seminal approach introduced by DiNardo et al. (1996) (see technical details in appendix F). Results, which are shown in panel B of Table 6, are only slightly affected. This suggests that the global crisis had no major effect on the unionization rate and more generally it ensures that the diverging trends between public and private sectors are not simply driven by a different evolution of the composition of the workforce in these sectors.

An additional (and final) piece of evidence regarding the effect of electoral democracy on unionization comes from the comparison of small and large private workplaces. Interestingly, because of the constitutional principle of equality before industrial citizenship, the 2008 law stated that workers in firms with 10 or less employees where professional elections are not organized, should be taken into account for the measure of representativeness of unions at the industry and national levels. Discussions between social partners to find a consensual solution failed. Eventually, in the fall 2010, a second law addressed this issue by organizing at the end of each 4-year electoral cycle (so first in December 2012 and then in late 2016) a national vote for workers in these small firms. This second law is comparable to the 2008 reform in two dimensions: it allowed workers to participate in the determination of their representative unions and it provided incentives to unions to expand in workplaces where there is no official worker representation. Table 6 shows that the decline of unionization in small workplaces continued from 2008 and 2010, but that from 2012 onward, a reverse trend was observed. This evolution is again consistent with a positive impact of electoral democracy on unionization. We conclude that the evolution of unionization in sectors concerned at different points in time by electoral democracy fully comforts the idea that it can have large effects on union membership.

## 6.2 Channels of influence

In many modern political democratic regimes in developed countries, citizens participate to the appointment of their representatives, which may foster their involvement in politics and avoid political apathy. Parties and politicians are also in competition for public offices at many levels (*e.g.*, city, region and state). Their chances of being elected or appointed at higher levels of representation depend at least indirectly to their performance at lower ones.

The type of industrial democracy introduced by the 2008 reform shares these general features of free elections: workers can participate to the appointment of their union representatives and unions are in competition at all levels. These fundamental aspects of electoral democracy could all potentially explain the overall impact of the 2008 reform. On the one hand, the higher levels of unionization, trust and social conflicts induced by the reform could be mostly driven by the mere fact to allow workers to choose their representatives through an election. On the other hand, competition, which is intrinsic to electoral democracy, provides incentives to unions to improve the quality of their representation. The link between the local and higher levels of representation also encourages unions to have candidates at elections in all firms, including those where they are unlikely to win or those where they were not present in the past. These channels of influence are hard to separate. Nevertheless, they hit unions, firms and workers differently according to their size and situation before the reform, something we now look at.

**Electoral performance of main French unions at higher levels.** Aggregated results of the workplace elections show that the reform was an important boost for non-historical unions. Two challengers already representative in some segments of public administrations strengthened in the private sector as well: Solidaires -the main union among civil servants belonging to the ministries of economy and finance- and UNSA - the main union in tribunals and prisons. At the national level, the non-historical unions attracted 12.1% of voters after the first 4-year cycle (2009-12) i.e. more than two out of the five historical ones. The national score of UNSA was 4.3%; it reached the threshold to become representative in 56 industries over a total of around 700. Solidaires attracted only 3.5% of the votes at the national level, but got a strong support in a dozen of industries, becoming for example the main union among journalists. Results from the second electoral cycle show that these unions continued to progress in the medium-run. The total score of the non-historical unions reaches 12.8% after the second cycle (2013-16), mainly driven by the UNSA score (5.4%). After this second cycle, UNSA was recognized representative in 80 industries. These results clearly illustrate that the 2008 reform induced more pluralism by removing barriers to entry for non-historical unions.

For the smallest historical unions, CGC (union of managers) and the Christian CFTC, the risk of becoming non-representatives in many industries and at the national level was large after the 2008 reform. They were initially opponents to the reform; after it passed, they strongly engaged to expand their audience at the workplace level. This strategy was partially successful. At the national level, after the first 4-year electoral cycle, they attracted respectively 9.4 and 9.3 percent of the vote casts. Then, they strengthened to gain resp. 10.7 and 9.5 percent after the second cycle. However, they both lost their representativeness in hundreds of industries (disable care homes, hairdressers, etc).

The two largest (historical) unions also compete for the leadership at the national level

in the private sector, so that they can claim to be the most legitimate social partner of employer associations and the government. The CGT won the first cycle while the CFDT won the second, becoming the largest union in the private sector in 2017 (26.4% versus 24.9% for the CGT). But even the CFDT lost its representativeness in some industries (e.g., butchers, laundries, ski stations). Finally, the fifth historical union, FO, eroded from 15.9% to 15.6% of vote casts.

While limited, the evolution of the results of the main national unions is compatible with an incentive story. The two smallest historical union who were both under the threat of being excluded from national bargaining clearly had the strongest incentives: they are the unions which progressed the most. UNSA is then the only non-historical union that is large enough to also compete for representativeness at the national level and we observe that it managed to make substantial progress to get closer to the 8% threshold necessary to obtain recognition.<sup>35</sup> By contrast, FO, which slightly declined, had no clear strategic incentive at the national-level as it could not lose its representativeness nor become leader. Finally, the incentives to become leader did not prevent the total score of the two main unions to erode slightly. This may be explained by the fact that these unions, which are on very different strategic lines, are fighting together to impose their model of unionism.

**Are reform effects only driven by the increase in union coverage?** The fact that the unions that had the highest incentives progressed the most suggests that the incentives provided by electoral competition partly explain the increase in union membership induced by the reform. Unions may have both put efforts to organize new workplaces or firms (extensive margin effect) and to better represent workers in workplaces where they were already present (intensive margin effect). The large reform impact on union coverage confirms the existence of an effect at the extensive margin. To see it more clearly, Table 7, panel A, provides LATE estimates for workplaces of different sizes and shows that the reform effect on union coverage is mostly concentrated among workplaces with 100 employees or less. In these workplaces the average coverage rate is 39% on our analysis sample and the estimate reform LATE is 28% (significant at the 10% level). In contrast, the average coverage rate and reform LATE among workplaces with more than 100 employees are respectively 86% and 9% (not significant at conventional levels). We also find that the reform has a statistically significant effect on coverage in the trade and services sector where the average coverage rate is 62%, while it has no significant effect in manufacturing and construction where the average coverage rate is 73%. These results are in line with the idea that unions were more successful at organizing the groups of workplaces where the initial coverage rate was the lowest (and therefore the margin of

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<sup>35</sup>The electoral results of UNSA in some major firms during the ongoing electoral cycle 2017-2020 suggest that it will continue to progress. For example, it became the main union in the RATP, the Paris public transport operator, which is historically highly-unionized.

improvement the highest).

The impact of the reform among groups of workplaces that are typically covered by unions (e.g., those with 100 employees or more) are informative about intensive margin effects. We see that in these workplaces, employers' trust and the prevalence of strikes or work stoppages is positively impacted, while we cannot detect significant effects on other outcomes of interest. This suggests that the effect of the reform on trust and satisfaction in unions is not entirely driven by its effect on coverage. Another way to reach this conclusion is to make a naive calculation of the share of the reform impact on trust that can be accounted for by its effect on coverage. Employers in non-covered workplaces have a much worse perception of unions than their counterparts in workplaces where at least a union is present: the gap in the trust index between these two groups is 50% of a s.d.<sup>36</sup> If we assume that this gap reflects a causal impact of union coverage on employers' perceptions (e.g., because experiencing face-to-face collective bargaining locally improves employers' priors towards unions), we can estimate that the 21 percentage points increase in union coverage induced by the reform (see Table 3) can directly generate an increase of about 10% of a s.d. ( $0.21 \times 50\%$ ) in employers' perceptions. This represents less than a quarter of the total estimated effect of the reform on these perceptions, suggesting again that the reform impacts are not entirely driven by the extensive margin.<sup>37</sup>

Similar (non-causal) back-to-the-envelope calculations for other outcomes suggest that the extensive margin may lead to an increase in workplace-level unionization (measured either from workers or employers declarations) of around 2.5 percentage points, an increase in work stoppages of around 8% of a s.d., an increase in workers' trust of around 7% of a s.d. and an increase in workers' participation to work stoppages of less than 4 percentage points.<sup>38</sup> Even if they rely on a strong assumption, these calculations suggest that the impact of the reform on union coverage, while large, does not drive entirely its effects on other outcomes.

**Change in union behavior and open democracy.** The effects of the reform that are not accounted for by the change in coverage can then result from an increase in the

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<sup>36</sup>The REPOSE survey explicitly asks employers to give their perceptions of unions in general when no unions are present in their workplace.

<sup>37</sup>The assumption that the gap in employers' perceptions between covered and non-covered workplaces reflects a causal impact is strong and likely to lead to overestimate the share of the effect of the reform on trust that may be explained by its effect on coverage. Indeed, another credible explanation for the gap in perception is that workers fear to engage in union representation in workplaces that are the more hostile to unions (see Bourdieu & Breda (2017) for evidence of anti-union discrimination in France). If such selection occurs, the reform may have induced unions to organize more hostile workplaces, without inducing directly a positive effect on trust in these newly organized workplaces.

<sup>38</sup>These calculations are made under the strong assumption that raw gaps in outcomes between covered and non-covered workplaces are informative about the causal effect of coverage on these outcomes. The causal effect can actually be both smaller or larger than the raw gap, depending on the sign of selection effects. For most outcomes, the intuition suggests that the raw gap is an upper bound for the causal impact, but this claim cannot be proved.

quality of union representation and services, or simply by the introduction of free elections. The higher satisfaction on workers' side is the most direct indication that the quality of union representation has improved. The issue with this interpretation is however that the fact to be allowed to vote may in itself foster workers' participation in unions and their satisfaction towards them. Unfortunately, we do not have sufficient information to clearly separate between these two types of explanations.

Indirect evidence that unions have changed their behavior and perhaps improved their quality may be obtained by looking at which workers are the most affected by the reform. The introduction of elections provides incentives to unions to represent the preferences or interests of the median worker in the workplace, while they had interest before to primarily represent their members. These members are more often men, they are older, have more tenure and tend to be a bit less skilled and in slightly lower-rank occupations than the median worker. Hence, if unions responded to the incentive to become more inclusive, we should observe lower effects for these workers.

Table 7, panel B, shows (on the largest sample of workers) how the effect of the reform on declared union membership, trust and participation to work stoppages varies according to workers' gender, age, occupation and education. For all three outcomes, we find larger effects for women than for men and for young workers than for old workers. Estimates are rarely statistically different from each other (nor from zero) but they consistently point to this conclusion, providing suggestive evidence of a behavioral response of unions toward more inclusiveness. Heterogeneity of impacts across occupations and skill levels are more contrasted. First, managers seem to have responded less to the reform than non managers. This could be because one of the five historical unions, the CGC, specifically represents managers. Such a segmentation may explain that other unions have not strongly targeted their efforts toward managers. It could also be that managers are the hardest to organize, so that they responded less despite unions' efforts. Regarding education, we see that workers without tertiary education become more unionized due to the reform while those with tertiary education strongly increased their trust in unions. This last result might be a sign of the pure effect of free elections as it is known that more educated workers tend to be more strongly in favor of more democratic systems generally speaking.

### **6.3 Exit, voice and loyalty**

Industrial disputes are often seen as the signal of deteriorated labor relations. The impact of the 2008 reform suggests that this association may sometimes be misleading. Indeed, social conflicts have increased due to the reform, showing that the reform impact on employers' trust in unions goes hand-in-hand with more voice mediated by light forms of industrial action. In contrast, This is particularly clear in the manufacturing and construction sectors where mobilizations are more common than in services (Table 7 panel

B): our estimates suggest a significant increase in both the number of work stoppages and the number of workers that take part of them.

These results may be interpreted in the classic Exit/Voice/Loyalty framework of Hirschman (1970). Since workers also tend to trust unions more due to the reform, their loyalty to them and the firm is likely to increase. Eventually, they voice more. The impact of the 2008 reform on quits (5) is consistent with this interpretation.

In that perspective, the 2008 reform may have not only delivered representative democracy at work but also a form of liberal democracy with multiple and new unions in the workplaces whilst trust in representatives improved and freedom of expression was secured. This liberal democracy seems to have induced industrial democracy in the classical sense: workers are not afraid to voice and express freely their concerns.

## 6.4 General conclusion

The 2008 French reform introducing electoral requirements for the appointment of union representatives has been implemented gradually in firms due to the exogenous calendar of their elections for work council members. Exploiting this feature of the reform, we identify its impacts on employment relations based on a representative employer-employees survey conducted in the middle of the reform implementation period. Doing so, we provide one of the rare micro-level evaluation of changes in employment relations regulations, contrasting with a large literature relying on cross-country comparisons.

We find that the introduction of electoral democracy has strongly increased union coverage and membership, employers' trust in unions and to some extent workers' trust in unions as well. The reform also induced more social conflicts (work stoppages or strikes) and a higher participation of workers in these conflicts, suggesting that it helped workers to voice latent and unspoken tensions. These effects can be driven by both supply and demand which are by definition jointly affected by electoral democracy. On the one hand, allowing workers to vote for their representative can be sufficient to foster their demand for unions and involvement in them. On the other hand, elections introduce competition for the supply of union services at the local level and incentives to gather as many votes as possible. All these mechanisms are likely to be at play.

Altogether, the results also suggest that the reform induced a better and more inclusive representation of workers, and a shift toward a system of employment relations regulated through voice and loyalty rather than entry and exit. Interestingly, this change goes hand-in-hand with an increase in employers' trust in unions which they consider as much more representative and more legitimate negotiation partners.

In positive terms, the large impacts uncovered in this evaluation prove that changing a strongly historically-rooted employment-relations system is possible even in the short-run, and even in a conflictual country.

Moving to more normative arguments, shall we conclude that the 2008 reform was a success and that democracy at work is desirable? If one wishes to judge unions from the joint satisfaction of the workers they represent and the employers they bargain with, the answer is clearly positive. If instead one wishes to judge unions based on their effect on economic performance, the present paper does not allow to conclude and further research is needed. We think that even without knowing the reform effects on economic performance, its effects on all stakeholders' satisfaction are compelling enough to conclude that repeated free elections for union representation in firms is an interesting model that other countries may want to consider.

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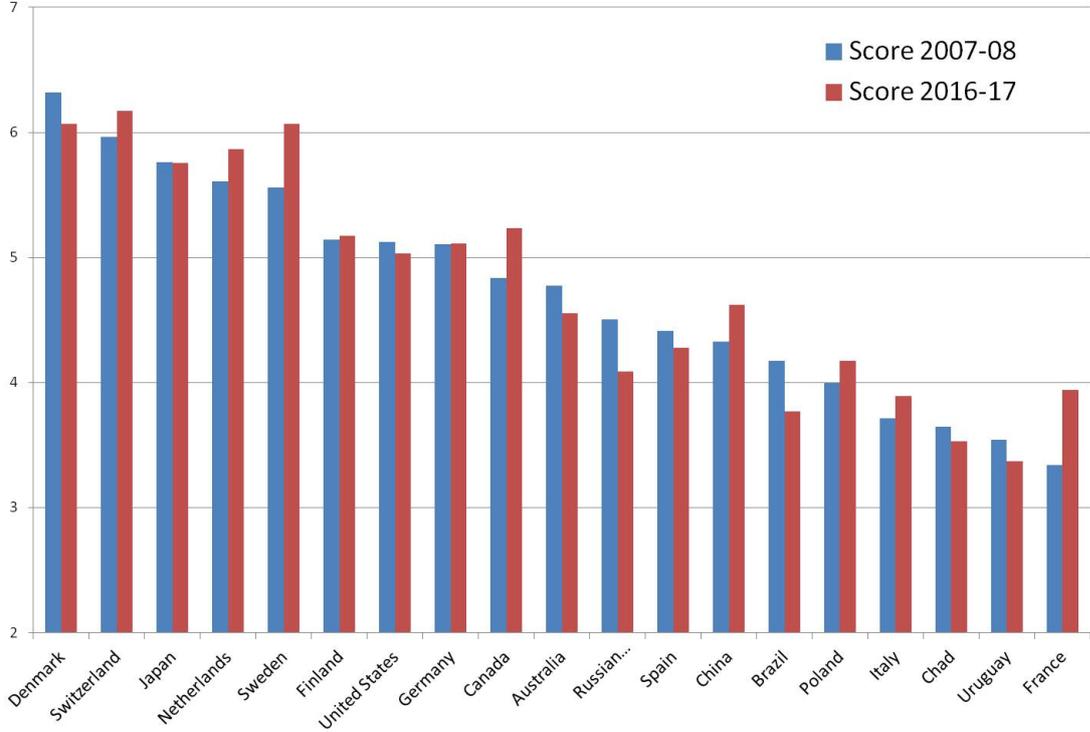
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# Figures and tables

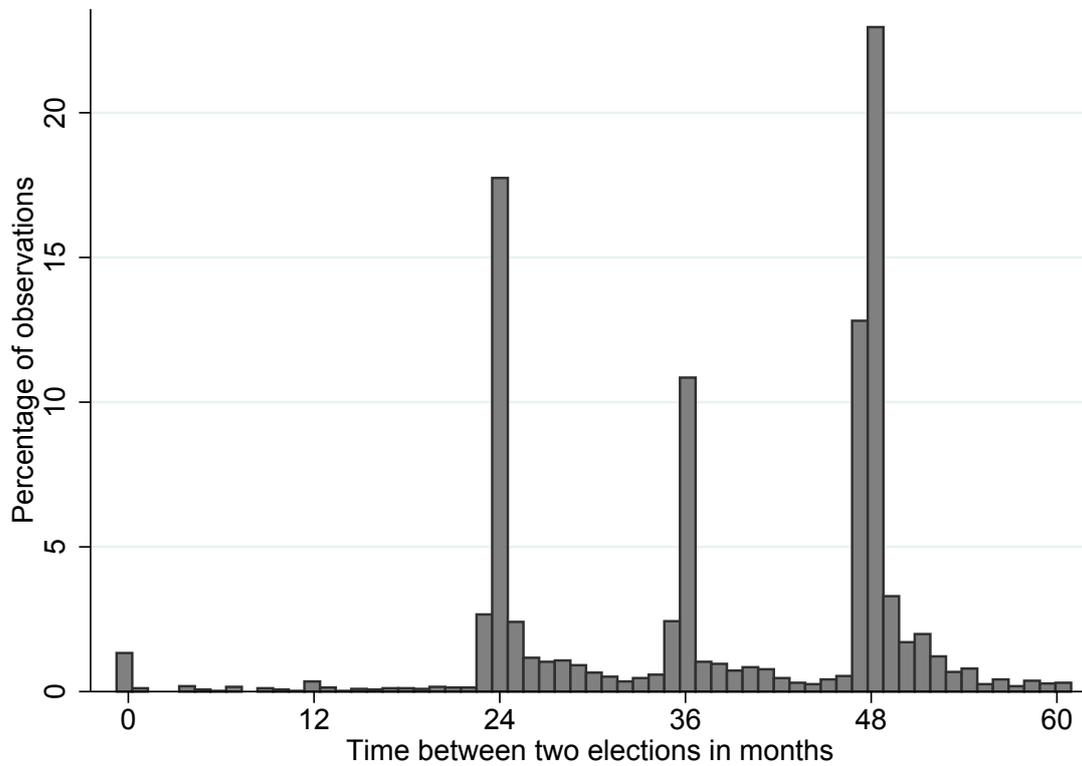
**Figure 1:** The Global Competitiveness Index-World Economic Forum. Cooperation in labour-employer relations in selected countries.



*Source:* World Economic Forum historical dataset. A rolling sample of managers is asked to quote from 0 -the least- to 7 -the best- the cooperation of labour-employer relations in their country.

*Note:* A selection of 19 countries out of 122 surveyed in both years are represented. In 2007-2008, France ranks last out of 128 countries in terms of this declared cooperation. In 2016-2017, France ranks 117 out of 145 countries in terms of this declared cooperation.

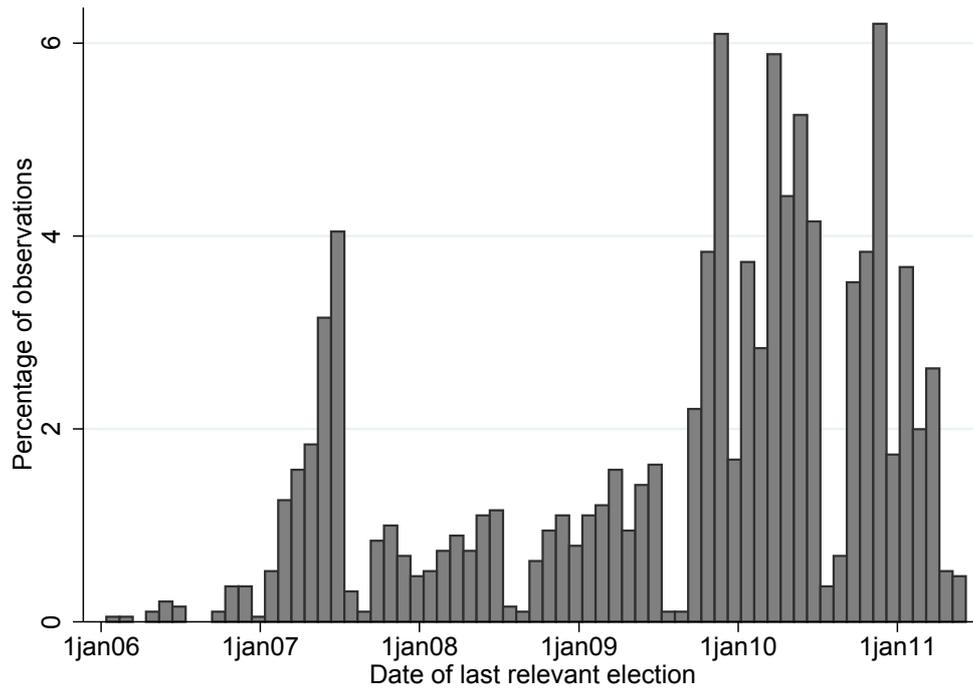
**Figure 2:** Number of months between two consecutive elections



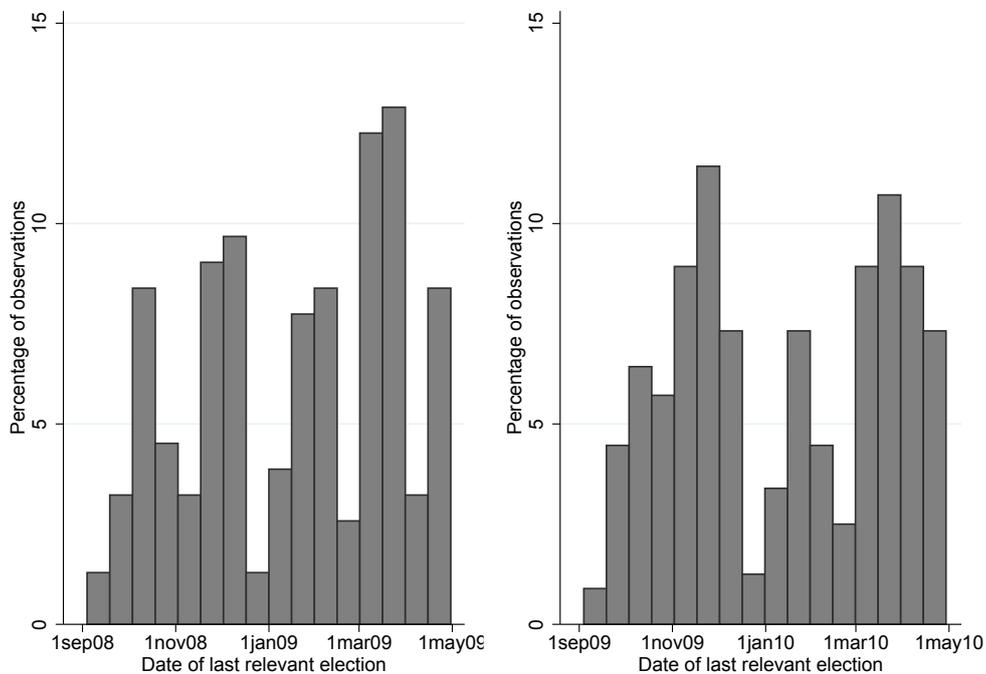
*Source:* MARS dataset, only establishment present in REPOSE11 which have registered an election during the period 2009-2012. Workplaces with more than 62 months between two consecutive elections are excluded.  
*Notes:* The figure represents the distribution of the length of time (in months) between all elections registered during the period 2009-2012 and the declared date of the preceding election. Partial elections have been removed.

**Figure 3:** Distribution of the date of most recent election before the RE-PONSE employer survey

(a) Whole period of interest



(b) Zooms around 1<sup>st</sup> January 2009 (cut-off date) and 1<sup>st</sup> January 2010

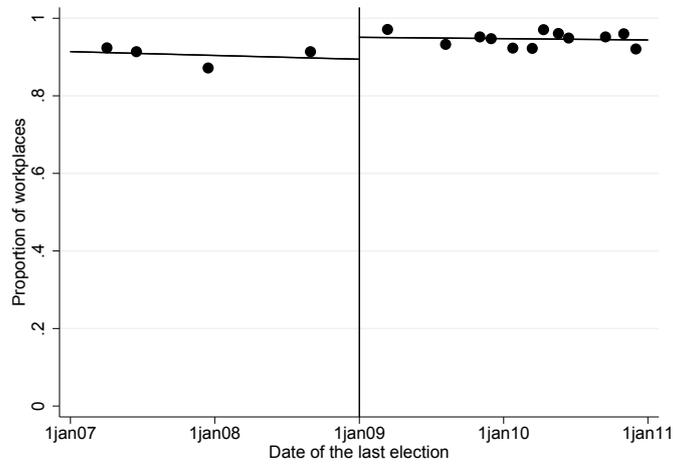


*Notes:* The figure represents the distribution of dates for the latest professional election before the RE-PONSE survey was done in early 2011. Workplaces younger than five years or having professional elections every two years are excluded.

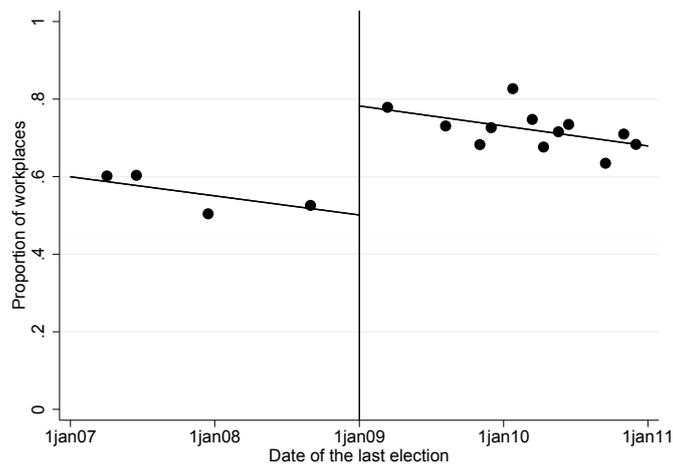
*Source:* Our own computations from the MARS administrative dataset matched with RE-PONSE11 (see Appendix A).

**Figure 4:** Impact of having a professional election under the new legal regime on workers' representation in 2011

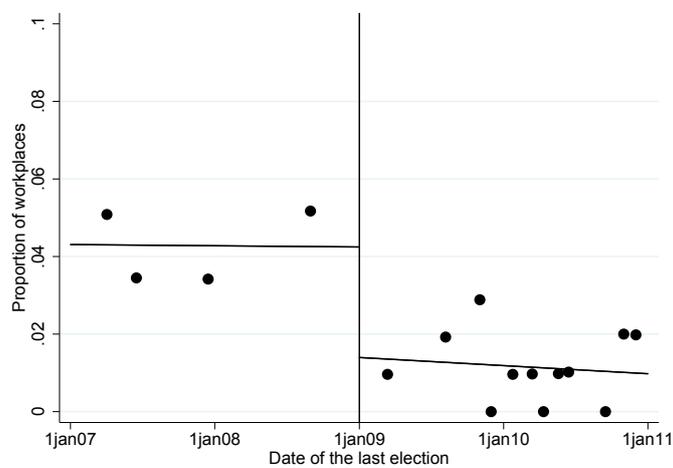
(a) Presence of workers' delegates or a work council



(b) At least one union recognized for bargaining



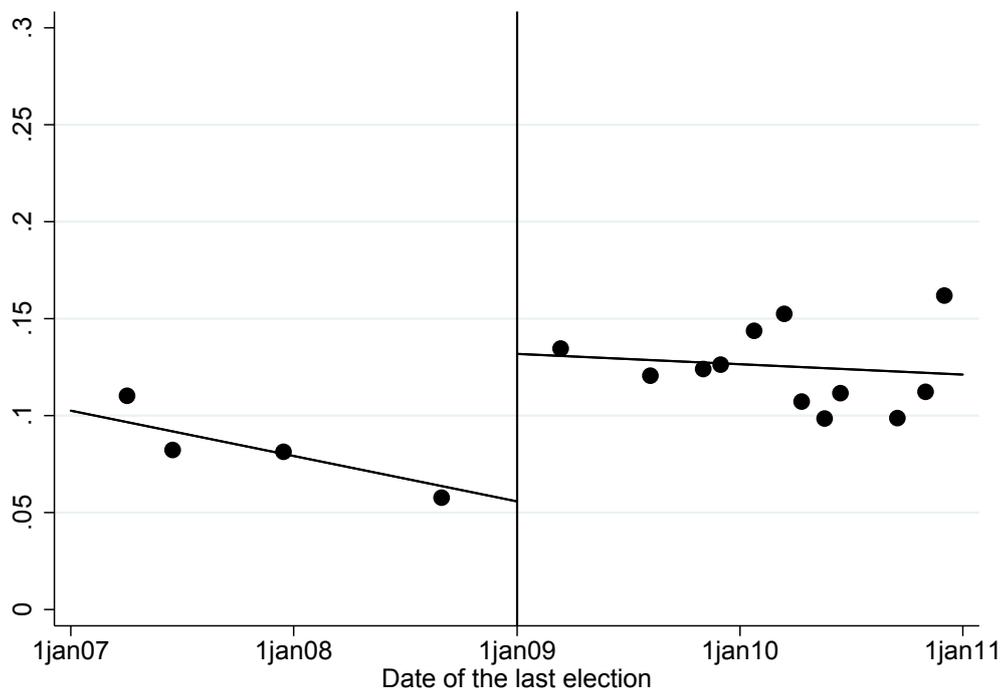
(c) Five or more unions recognized for bargaining



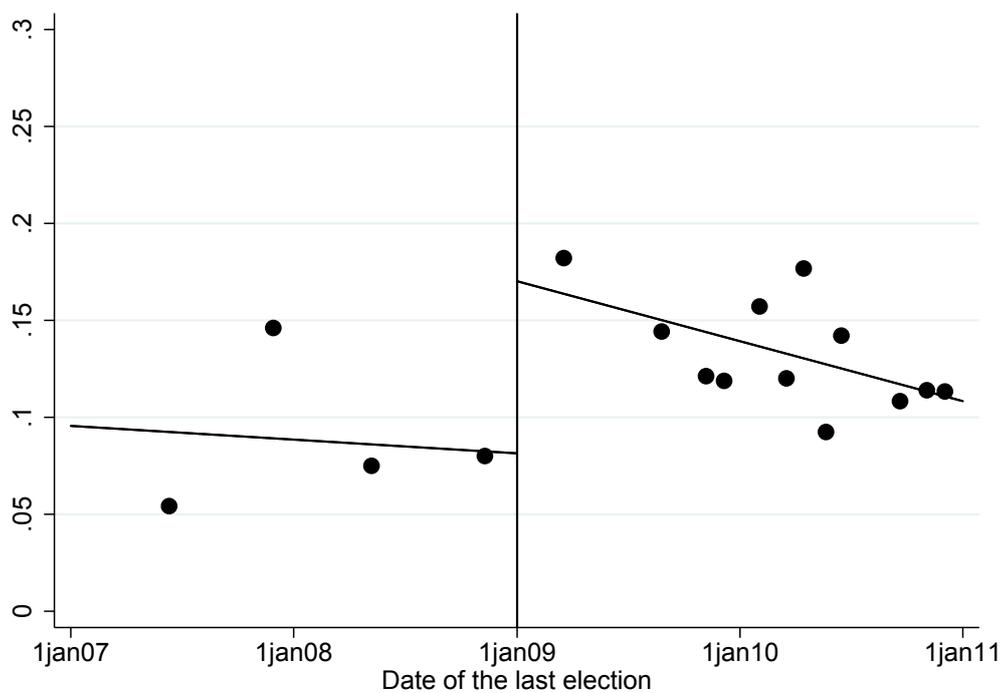
*Notes:* Each bin provides the mean of the interest variable for establishments experiencing their last professional elections around the date of the bin; observations are split in 4 equal-size groups at the left of the cutoff date, and 12 equal-sized bins at the right of this cutoff. Lines represent the linear trend of the interest variable before and after the cutoff date. Workplaces younger than five years or having professional elections every two years are excluded.  
*Source:* Our own computations from the MARS administrative dataset matched with REPOSE11 (see Appendix A).

**Figure 5:** Impact of having a professional election under the new legal regime on workplace-level unionization rate in 2011

(a) Unionization rate declared by the employer



(b) Share of surveyed workers who declare to be union members (workplace average)

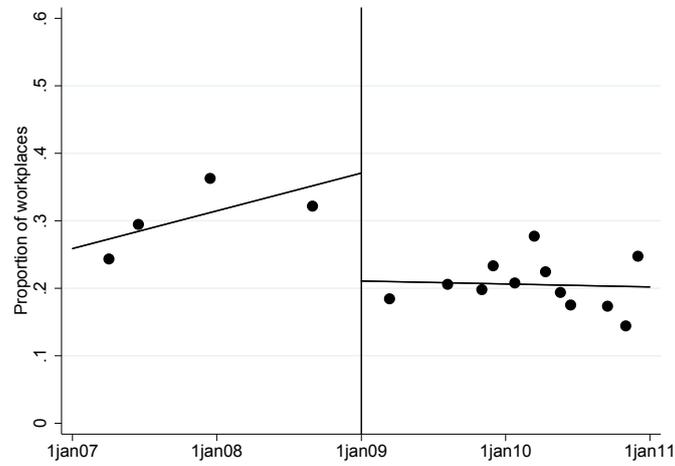


*Notes:* See Figure 4. The answers of individual workers are averaged by workplace; only workplaces for which an employer has been also surveyed are included (core sample).

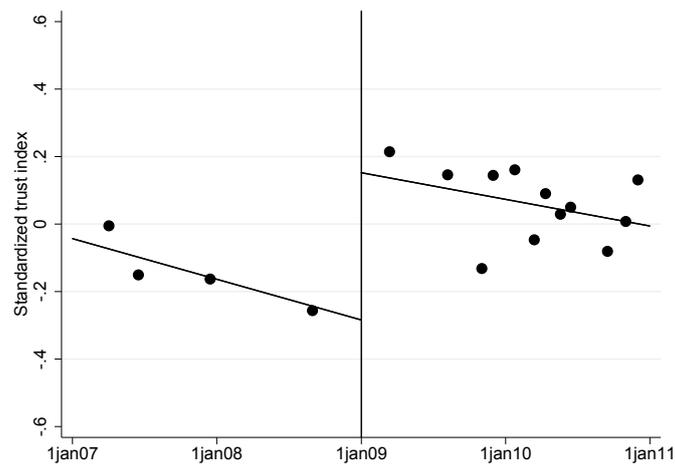
*Source:* See Figure 4.

**Figure 6:** Impact of having a professional election under the new legal regime on employers' and employees' perceptions of unions

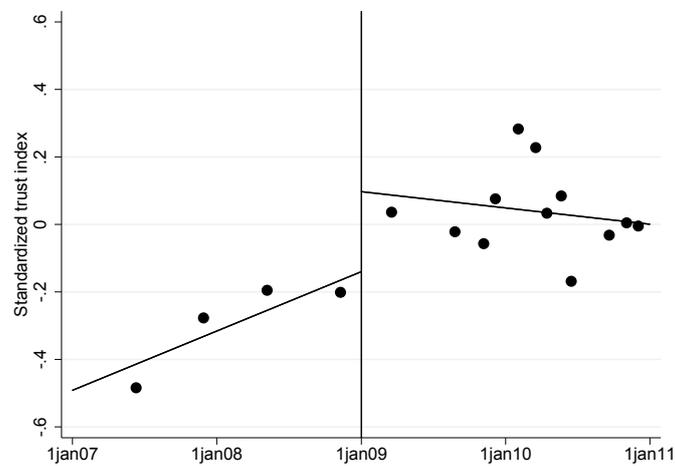
(a) Employer perceives unions representativeness as very weak



(b) Employers' trust in unions in their workplace



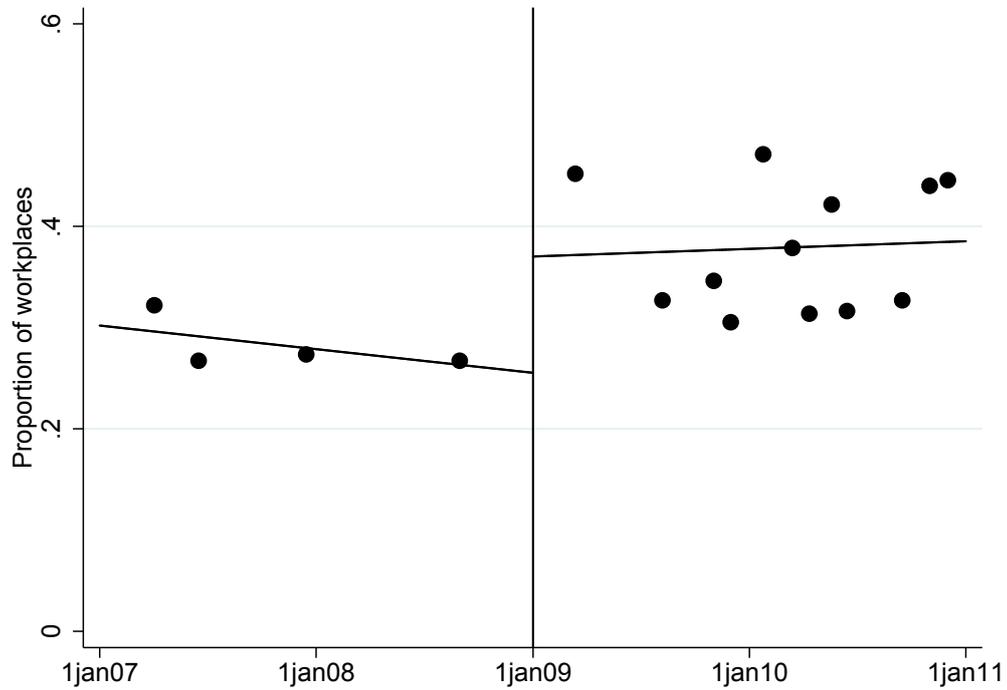
(c) Employees' trust in unions in their workplace (workplace average)



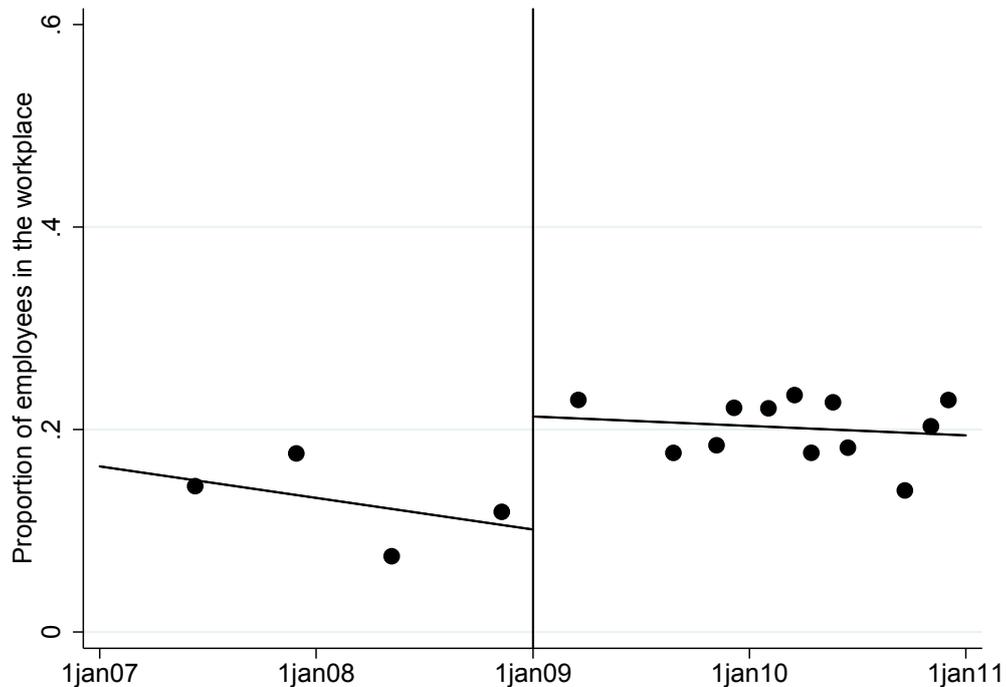
Notes and source: see Figure 4. The answers of individual workers are averaged by workplace; only workplaces for which an employer has been also surveyed are included (core sample).

**Figure 7:** Impact of having a professional election under the new legal regime on social conflicts

(a) Employer declares there was at least one work stoppage or strike between 2008 and 2010



(b) Employees declaring they have participated to a strike or work stoppage between 2008 and 2010 (workplace average)



*Notes and source:* see Figure 4. The answers of individual workers are averaged by workplace; only workplaces for which an employer has been also surveyed are included (core sample).

**Table 1:** Professional elections and union recognition rules at firm or workplace level before and after the 2008 reform

	<i>Before the 2008 reform</i>	<i>After the 2008 reform</i>
<b>1) Who can participate to the first ballot of professional elections?</b>	<ul style="list-style-type: none"> <li>• 5 unions considered <i>de jure</i> representative for historical reasons</li> <li>• Other unions if they can prove they are representative in the firm (difficult in practice)</li> </ul>	<ul style="list-style-type: none"> <li>• All unions older than 2 years that comply with republican values and financial transparency and are active in the sector and area of the firm/workplace.</li> </ul>
<b>2) Which unions are eligible for firm-level bargaining?</b>	<ul style="list-style-type: none"> <li>• 5 unions considered <i>de jure</i> representative for historical reasons</li> <li>• Other unions if they can prove they are representative in the firm (difficult in practice)</li> </ul>	<ul style="list-style-type: none"> <li>• All unions that attracted at least 10% of vote casts at the first round of professional elections</li> </ul>
<b>3) Who can be appointed by eligible unions as union delegate for bargaining?</b>	<ul style="list-style-type: none"> <li>• Any worker in workplace/firm with 50+ employees;</li> <li>• An elected worker's delegates in firm with 11 to 49 employees</li> </ul>	<ul style="list-style-type: none"> <li>• Any worker who obtained at least 10% of vote casts at the first round of professional elections</li> </ul>
<b>4) Which unions are representative for bargaining at the industry level?</b>	<ul style="list-style-type: none"> <li>• 5 unions considered <i>de jure</i> representative for historical reasons</li> <li>• Other unions if they can prove (in court) their representativeness in many or major firms of the industry (rare in practice)</li> </ul>	<ul style="list-style-type: none"> <li>• Unions that attracted at least 8% of vote casts at the first round of all firm-level professional elections in the industry</li> </ul>
<b>5) Which unions are representative for bargaining at the national level?</b>	<ul style="list-style-type: none"> <li>• 5 unions considered <i>de jure</i> representative for historical reasons</li> </ul>	<ul style="list-style-type: none"> <li>• Unions that attracted at least 8% of vote casts at the first round of all firm-level professional elections in the country</li> </ul>

*Notes:* Professional elections are used both prior and after the 2008 reform to elect workers' delegates and members of the work councils. These elections have two rounds. Only candidates supported by a union can apply at the first round. A second round with both unionized and non-unionized candidates is organized if less than 50% of the workers voted at the first round, or if there were less candidates than the number of available seats (or no candidates at all) at the first round. Workers' delegates and work councils only have the right to be informed and consulted about important matters by the employer. They are not officially allowed to bargain on wages or working conditions and to sign collective agreements. Only unions can do it through their official union delegates that have the right to bargain at least once a year with the employer.

**Table 2:** Descriptive statistics and analysis of discontinuities for covariates

	N obs	Mean	RD estimator	robust p value	N obs in bandwidth
<i>Industries</i>					
Manufacturing	1911	0.292	0.065	0.363	755
Construction	1911	0.054	0.045	0.198	702
Trade	1911	0.161	0.057	0.344	257
Market services	1911	0.320	-0.094	0.310	923
Non-market services	1911	0.173	-0.115	0.228	278
<i>Workplace size groups (in december 2008)</i>					
10-49 employees	1911	0.230	-0.050	0.662	702
50-199 employees	1911	0.375	-0.109	0.353	580
200-999 employees	1911	0.341	0.154	0.103	851
More than 1000 employees	1911	0.054	-0.011	0.855	297
<i>Workplace age (in 2011)</i>					
5-9 years	1911	0.082	-0.132	0.129	229
10-19 years	1911	0.201	0.111	0.196	297
20-49 years	1911	0.452	-0.169	0.118	313
More than 50 years	1911	0.264	0.129	0.126	482
Paris region	1911	0.193	-0.051	0.468	434
Belongs to single-plant firm	1911	0.394	-0.084	0.435	549
Professional elections every 3 years	1911	0.216	0.114	0.345	339
Professional elections every 4 years	1911	0.689	-0.008	0.897	416
Interviewed manager is a woman	1911	0.396	0.070	0.358	981

*Notes:* The Table reports in different rows the sample number of non-missing observations and sample mean for the main workplace-level covariates, as well as bias-corrected RDD estimates and their associated robust p-values following Calonico et al. (2014). To get RDD estimates, separate polynomials are fitted on each side of the threshold. A triangular kernel is used. The polynomial order is 1, and the optimal bandwidths are derived under the MSERD procedure separately for each dependent variable. There are no control variables.

**Table 3:** LATE of the reform on workplace-level workers' representation and unionization rate

	Sample Mean	Estim. left of thresh.	RD conv. estim.	RD BC estim.	N obs	N obs in band.
<b><i>Panel A: presence of workers' delegates, work councils and unions</i></b>						
Workers' delegates or work council	0.933	0.877	0.090 (0.055)	0.105 (0.064)	1911	924
At least one union recognized	0.659	0.578	0.213** (0.087)	0.203** (0.103)	1911	851
from historical unions only	0.645	0.568	0.185** (0.086)	0.163 (0.100)	1911	909
from "new" unions only	0.109	0.089	0.101 (0.072)	0.118 (0.084)	1911	375
≥ 2 unions recognized	0.440	0.391	0.119 (0.109)	0.115 (0.130)	1911	298
2 or 3 unions recognized	0.291	0.256	0.092 (0.090)	0.103 (0.109)	1911	574
5 unions or more recognized	0.058	0.102	-0.033 (0.052)	-0.038 (0.060)	1911	434
<b><i>Panel B: unionization rate in the workplace</i></b>						
Unionization rate (declared by employer)	0.112	0.049	0.089** (0.035)	0.093** (0.044)	1277	373
Share of workers union members (core sample of workers)	0.121	0.086	0.099** (0.042)	0.097* (0.052)	1586	650
Share of workers union members (larger sample of workers)	0.128	0.080	0.135*** (0.035)	0.151*** (0.042)	3100	925

*Notes:* The Table provides LATE of the 2008 reform estimated by RDD. There is one row for each relevant outcome variable. Both the RDD conventional estimator and its standard error (column 3) and the bias-corrected estimator and its associated robust standard error (column 4) are shown. For each estimate and its associated standard error, we recomputed p-values and used the standard convention: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . To get RDD estimates, separate polynomials are fitted on each side of the threshold. A triangular kernel is used. The polynomial order is 1, and the optimal bandwidths are derived under the MSERD procedure separately for each dependent variable. There are no control variables. The Table also provides the number of observation in the estimation bandwidth (column 6) as well as the value taken at the cutoff by the polynomial fitted on the left side of the RDD threshold (column 2).

The core sample of workers only includes workplaces for which an employer has been also surveyed while the larger sample includes all workplaces selected to take part to REPOSE11. Workplaces younger than five years or having professional elections every two years are excluded except on the larger sample of worker where this selection cannot be done.

**Table 4:** LATE of the reform on employers' and employees' perceptions of unions

	Sample Mean	Estim. left of thresh.	RD conv. estim.	RD BC estim.	N obs	N obs in band.
<b><i>Panel A: Employers' perceptions</i></b>						
Unions representativeness is very weak	0.245	0.383	-0.200** (0.096)	-0.220* (0.114)	1859	499
Trust in unions index	0.000	-0.240	0.458** (0.198)	0.476** (0.235)	1782	809
- Unions play a vital role	0.490	0.400	0.235** (0.100)	0.253** (0.122)	1878	537
- Unions provide a service	0.727	0.604	0.265*** (0.088)	0.289*** (0.104)	1849	526
- Unions interests not put ahead	0.414	0.368	0.144 (0.098)	0.178 (0.117)	1835	528
- Unions don't hinder running of firm	0.725	0.644	0.125 (0.094)	0.150 (0.112)	1858	558
Trust in workers delegate index	0.000	0.050	0.334* (0.192)	0.403* (0.230)	1862	467
<b><i>Panel B: Workers' perceptions (core sample of workers)</i></b>						
Trust in unions index	-0.000	0.089	0.236 (0.268)	0.231 (0.327)	1453	188
- Unions play a vital role	0.635	0.667	0.120 (0.091)	0.146 (0.109)	1527	197
- Unions provide a service	0.697	0.700	0.089 (0.079)	0.106 (0.096)	1531	300
- Unions interests not put ahead	0.475	0.497	0.030 (0.101)	0.052 (0.119)	1508	208
- Unions don't hinder running of firm	0.714	0.757	-0.006 (0.098)	-0.007 (0.117)	1510	224
Trust in workers delegate index	-0.000	0.564	0.005 (0.288)	0.078 (0.343)	1427	176
<b><i>Panel C: Workers' perceptions (larger sample of workers)</i></b>						
Trust in unions index	-0.000	-0.016	0.327* (0.172)	0.368* (0.197)	2835	555
- Unions play a vital role	0.647	0.612	0.189*** (0.069)	0.220*** (0.080)	2994	413
- Unions provide a service	0.702	0.676	0.137** (0.060)	0.154** (0.071)	3002	513
- Unions interests not put ahead	0.468	0.501	-0.001 (0.056)	0.007 (0.066)	2947	1168
- Unions don't hinder running of firm	0.711	0.742	0.004 (0.054)	0.012 (0.065)	2937	911
Trust in workers delegate index	-0.000	0.333	0.030 (0.189)	0.082 (0.221)	2766	361

Notes: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . See Table 3 for more details.

**Table 5:** LATE of the reform on work stoppages, social climate and job satisfaction

	Sample Mean	Estim. left of thresh.	RD conv. estim.	RD BC estim.	N obs	N obs in band.
<b><i>Panel A: Conflicts and social climate (declared by employer), quits</i></b>						
Work stoppage (any kind)	0.343	0.232	0.222** (0.103)	0.260** (0.122)	1911	422
- Strike of 2 days or more	0.071	0.076	0.010 (0.051)	0.005 (0.061)	1911	584
- Intermittent strike	0.030	0.020	-0.016 (0.020)	-0.017 (0.021)	1911	399
- Strike of 1 day or less	0.213	0.169	0.094 (0.078)	0.121 (0.091)	1911	651
- Walkout	0.251	0.054	0.323*** (0.092)	0.362*** (0.102)	1911	281
Social climate	-0.000	0.097	-0.290 (0.192)	-0.311 (0.229)	1910	463
Quits in 2011 (from admin data)	0.046	0.070	-0.022 (0.020)	-0.021 (0.024)	1450	512
<b><i>Panel B: Workers' participation to work stoppages and job satisfaction</i></b>						
<i>Workplace averages on the core sample of employees:</i>						
Participation to a work stoppage (any kind)	0.178	0.135	0.103 (0.066)	0.110 (0.085)	1579	353
Job satisfaction index	0.000	0.256	-0.029 (0.241)	0.027 (0.292)	1584	216
<i>Workplace averages on the larger sample of employees:</i>						
Participation to a work stoppage (any kind)	0.191	0.171	0.046 (0.047)	0.054 (0.057)	3078	976
Job satisfaction index	-0.000	0.163	-0.138 (0.158)	-0.172 (0.189)	3091	664

Notes: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . See Table 3 for more details.

**Table 6:** Unionization rate (in %) in France 2005-2016: sectors affected and not affected by the 2008 reform

	2005*	2008	2010	2013	2016	2008 reform applies?
<i>Panel A: estimates not adjusted for changes in workforce composition</i>						
All employees	10.6%	10.8%	10.8%	11.3%	11.2%	Partly
Public sector	20.3%	19.5%	19.3%	19.3%	17.4%	No
Private sector	7.1%	7.5%	7.5%	8.3%	8.8%	Partly
Private sector, workplaces with 10 employees or less	3.8%	2.8%	2.8%	3.3%	3.9%	Partly after 2012**
Private sector, workplaces with more than 10 employees	9.0%	10.0%	10.0%	10.5%	11.1%	Yes
Private sector, same sample as for RDD estimates	12.2%	9.7%	11.4%	11.7%	12.9%	Yes
<i>Panel B: estimates adjusted by DFL reweighting to maintain workforce characteristics at their 2008 level</i>						
All employees	10.6%	11.3%	11.3%	10.7%	10.9%	Partly
Public sector	20.3%	20.7%	20.7%	18.9%	17.0%	No
Private sector	7.1%	8.2%	8.2%	7.8%	8.3%	Partly
Private sector, workplaces with 10 employees or less	3.8%	3.2%	3.2%	3.5%	3.6%	Partly after 2012**
Private sector, workplaces with more than 10 employees	9.0%	10.0%	10.0%	10.4%	11.1%	Yes
Private sector, same sample as for RDD estimates	9.7%	11.2%	11.2%	11.4%	12.2%	Yes

*Notes:* CEOs are excluded in all samples. Sources: REPONSE survey 2005 and Survey on *Sources de Revenu et Conditions de Vie* (SRCV) 2008, 2010, 2013 and 2016. From 2008 to 2013, SRCV was the official source for the French unionization rate. Statistics from SRCV are weighted to account for the population gender\*age structure.

Panel B shows estimates after applying a propensity score reweighting to keep the distribution of workers' characteristics (age, age squared, gender, education in 8 groups, occupation in 10 groups, workplace size in 5 groups and sector in 15 groups) similar in 2010, 2013 and 2016 to their 2008 level. See details in the appendix F.

\* The unionization rate in 2005 is obtained from the REPONSE 2005 employee survey by calibration: We have multiplied the unweighted share of employees that are members of a unions by  $k$ , with  $k$  the ratio between the unionization rate obtained using SRCV in 2010 and that obtained using REPONSE11 for the same population as that of REPONSE 2005 (workplaces with more than 20 employees only). The goal of this operation is to make statistics comparable across surveys (see details in appendix F).

\*\* Because of the constitutional principle of equality before industrial citizenship, the 2008 law stated that workers in firms with 10 or less employees where professional elections are not organized should be taken into account for the measure of representativeness of unions at the industry and national levels. A national vote for workers in these small firms was organized in December 2012.

**Table 7:** Heterogeneity of reform impacts

**Panel A: Heterogeneity according to firm characteristics**

	At least one union recognized	Share of workers union members	Employer trust	Employee trust	Strike or work stoppage	Participation to work stoppages
<i>Workplace size</i>						
100 employees or less	0.279* (0.164)	0.176*** (0.046)	0.523 (0.437)	0.483 (0.335)	-0.227* (0.136)	0.066 (0.071)
more than 100 employees	0.091 (0.091)	0.068 (0.057)	0.377* (0.211)	0.090 (0.325)	0.284* (0.165)	0.084 (0.112)
<i>Sector</i>						
Trade and other Services	0.242** (0.114)	0.118* (0.066)	0.451* (0.240)	0.024 (0.283)	0.152 (0.106)	0.001 (0.062)
Manufacturing and construction	0.179 (0.192)	0.103 (0.098)	0.552 (0.502)	0.972** (0.479)	0.451** (0.221)	0.407*** (0.143)

**Panel B: Heterogeneity according to workers' characteristics**

	Share of workers union members		Employee trust		Participation to work stoppages	
	<i>Mean</i>	<i>Estimate</i>	<i>Mean</i>	<i>Estimate</i>	<i>Mean</i>	<i>Estimate</i>
Women	0.113	0.131*** (0.049)	0.071	0.235 (0.209)	0.167	0.127 (0.079)
Men	0.133	0.088* (0.048)	-0.080	0.185 (0.167)	0.203	0.055 (0.054)
Age below median	0.102	0.143*** (0.050)	-0.069	0.280* (0.162)	0.181	0.092* (0.054)
Age above median	0.155	0.071 (0.055)	0.043	0.060 (0.154)	0.208	-0.008 (0.061)
Non managers	0.148	0.137** (0.057)	0.058	0.383** (0.161)	0.223	0.070 (0.053)
Managers	0.105	0.086 (0.056)	-0.270	0.177 (0.227)	0.155	0.043 (0.062)
Non tertiary Education	0.147	0.184*** (0.045)	0.040	0.175 (0.161)	0.218	0.013 (0.061)
Tertiary Education	0.095	0.021 (0.046)	-0.082	0.436** (0.203)	0.150	0.033 (0.058)

*Notes:* \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Conventional RDD estimates and standard errors are reported. More details on RDD estimates are given in the notes of Table 3.

Panel A: Based on employers responses in columns 1, 3 and 5 and workers' responses in columns 2, 4 and 6 (core sample only, as firm characteristics are not available on the full sample).

Panel B: The full sample of workers is used in all analyses. The trust variable is standardized on the whole sample of workers. Averages of this standardized variable are then constructed at the workplace-level for each type of workers to obtain the dependent variables used in the RDD. Median age is 42 year old.

(For Online Publication)

Appendix to  
Democracy at Work

Philippe Askenazy and Thomas Breda

July 2019

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## Appendix A The legal conditions for changing the date of an election

The length of the mandate can be altered by changes in the frontier and the size of the firm or workplace but not through direct manipulations. First, if the firm is absorbed by another one, the length of the mandates are adapted so as the mandates end at the same date. Second, if the size of the firm becomes larger than 50-worker threshold, the employer has to organize the election for a work council. Since the elections of delegates and work councils should be simultaneous, the mandate of the workers' delegates has to be shortened.

Other main cases of changes in the date of the election require very special conditions and are under the strict supervision of the labor inspectorate (the *inspection du travail*, which ensures the respect of labor Law):

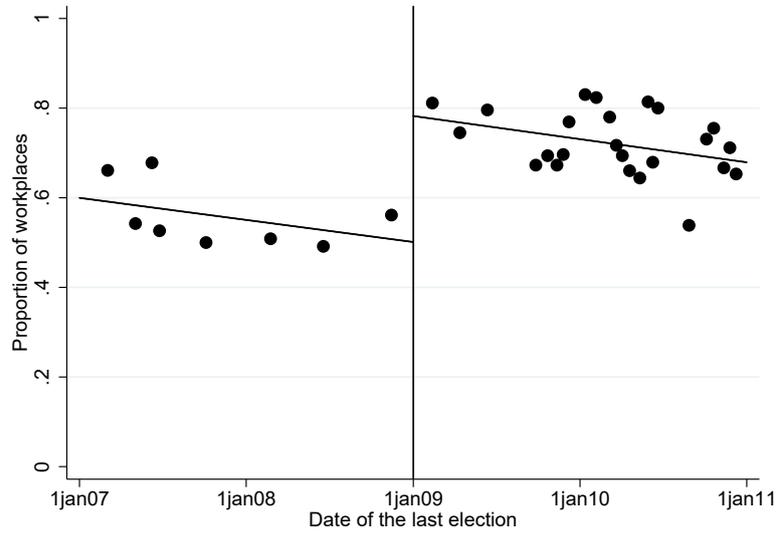
- The mandate can be shortened only if all elected workers resign or are fired simultaneously. Firing all elected workers is in practice impossible (except if the workplace closes). Indeed, these workers are protected by the law, and the employer can fire them only after the authorization of the labor inspection which checks there is no discrimination.

- The mandate can be extended but, here again, the conditions are precise and make a manipulation unlikely. All representative unions and the employer should unanimously agree to extend the current mandate for a "reasonable period" (some days up to some months) and objective motives. The extension agreement is transmitted to the labor inspection. In practice, unions and the employer do that because of exceptional circumstances linked to the material organization of the elections (e.g. a natural disaster).

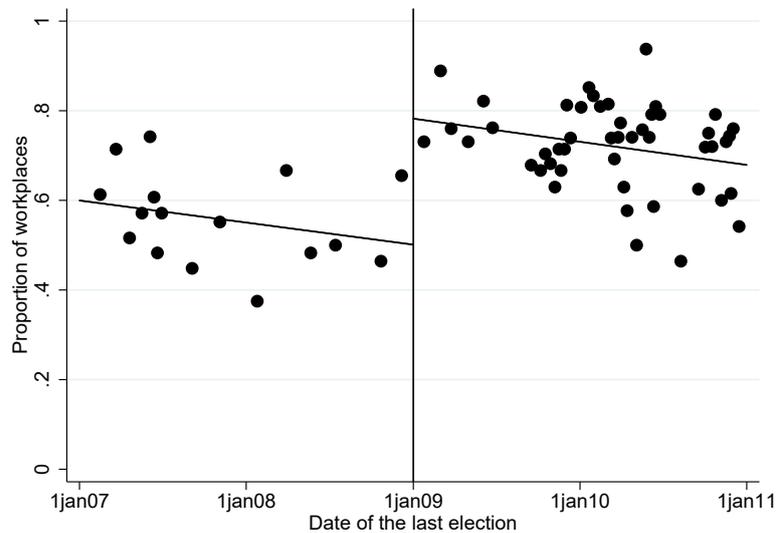
## Appendix B Additional Figures

**Figure B1:** Impact of having a professional election under the new legal regime on union coverage in 2011: graphs with more bins

(a) 32 bins



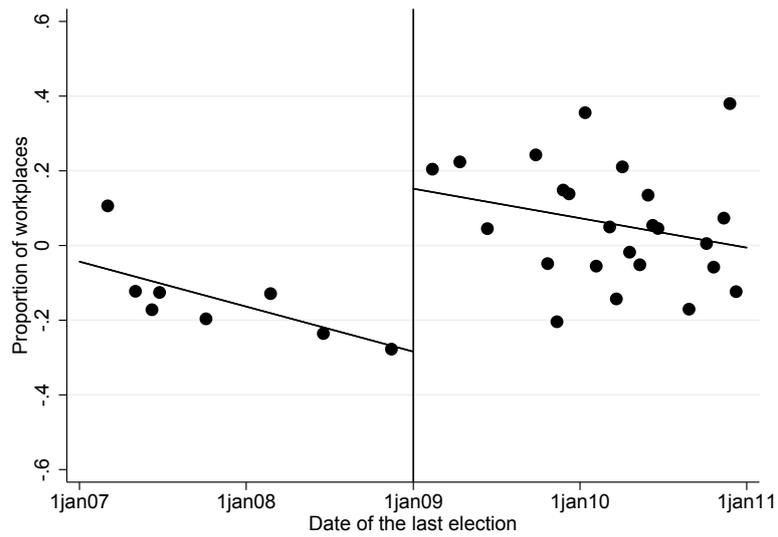
(b) 64 bins



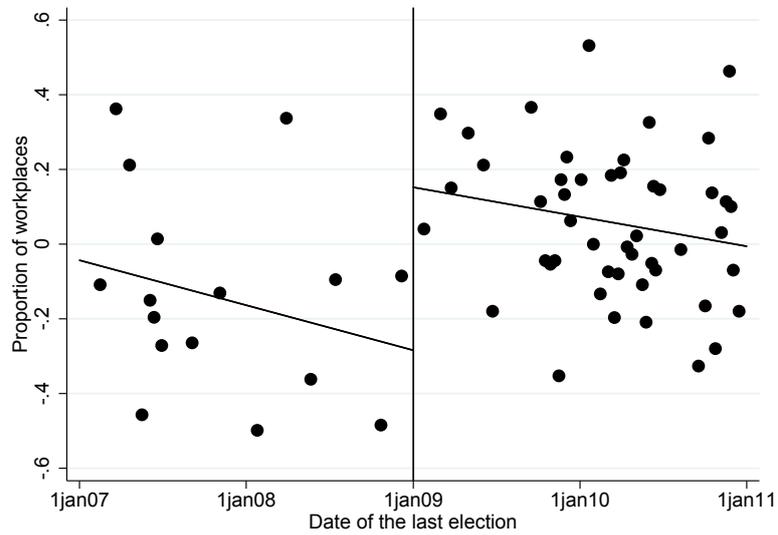
*Notes:* Union coverage is a workplace-level variable for having at least one union recognized for bargaining in the workplace. Observations are split in 8 (pane A) or 16 (panel B) equal-size groups at the left of the cutoff date, and 24 (panel A) or 48 (panel B) equal-sized bins at the right of this cutoff. Lines represents the linear trend of the interest variable before and after the cutoff date. Workplaces younger than five years or having professional elections every two years are excluded.

**Figure B2:** Impact of having a professional election under the new legal regime on employers' perception of unions in 2011: graphs with more bins

(a) 32 bins

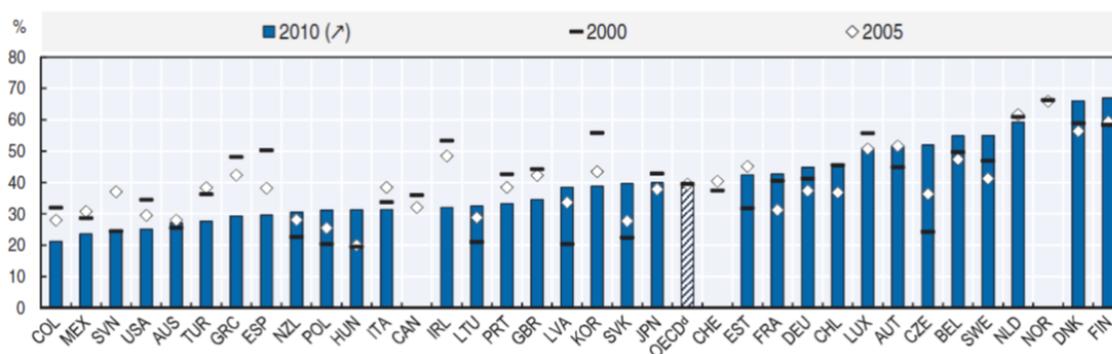


(b) 64 bins



*Notes:* The dependent variable is the standardized trust index. Observations are split in 8 (pane A) or 16 (panel B) equal-size groups at the left of the cutoff date, and 24 (panel A) or 48 (panel B) equal-sized bins at the right of this cutoff. Lines represents the linear trend of the interest variable before and after the cutoff date. Workplaces younger than five years or having professional elections every two years are excluded.

**Figure B3:** Trust in trade unions among total population



*Notes:* Percentage of persons (aged 15 or over) tending to trust trade unions for the European countries excepted Norway and Switzerland and percentage of persons (aged 15 or more) who are greatly or quit a lot confident in trade unions for all other countries, Norway and Switzerland.

Over the 35 OECD countries for which statistics are shown, France experienced the third largest increase between 2005 and 2010, just behind Sweden and The Czech Republic.

*Source:* Reproduction of Figure 4.9b in OECD (2017) based on Eurobarometer for all European countries (not including Norway and Switzerland) and World Values Survey (<http://www.worldvaluessurvey.org/WVSONline.jsp>) for all other countries.

## Appendix C Falsification tests

**Table C1:** RD estimates for main outcomes of interest for a fake reform applying on January 1<sup>st</sup> 2010

	Sample Mean	Estim. left of thresh.	RD conv. estim.	RD BC estim.	N obs	N obs in band.
<b><i>Panel A: Employers main outcomes</i></b>						
At least one union recognized	0.659	0.760	0.118 (0.084)	0.118 (0.103)	1911	647
Trust in unions index	0.000	0.141	0.065 (0.205)	0.088 (0.252)	1782	603
Unionization rate	0.112	0.138	0.010 (0.049)	-0.001 (0.061)	1277	405
Work stoppage (any kind) (between 2008 and 2010)	0.343	0.292	0.190** (0.083)	0.206** (0.099)	1911	774
Social climate	-0.000	-0.041	-0.041 (0.182)	-0.045 (0.216)	1910	777
<b><i>Panel B: Workers' main outcomes (core sample of workers)</i></b>						
Share of workers union members (from workers responses)	0.121	0.099	0.055 (0.046)	0.059 (0.055)	1586	735
Trust in unions index	-0.000	0.043	0.270 (0.185)	0.298 (0.223)	1453	694
Participation to a work stoppage	0.178	0.231	0.005 (0.058)	-0.006 (0.070)	1579	750
<b><i>Panel C: Workers' main outcomes (larger sample of workers)</i></b>						
Share of workers union members (from workers responses)	0.128	0.101	0.041 (0.035)	0.038 (0.044)	3100	1102
Trust in unions index	-0.000	-0.103	0.172 (0.165)	0.136 (0.205)	2835	1005
Participation to a work stoppage	0.191	0.196	0.041 (0.039)	0.043 (0.046)	3078	1472

*Notes:* The Table provides a placebo test for the LATE of the 2008 reform. For this placebo test, the RDD threshold is moved from January 1<sup>st</sup> 2009 to January 1<sup>st</sup> 2010. There is one row for each relevant outcome variable. Both the RDD conventional estimator and its standard error (column 3) and the bias-corrected estimator and its associated robust standard error (column 4) are shown. For each estimate and its associated standard error, we recomputed p-values and used the standard convention: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . To get RDD estimates, separate polynomials are fitted on each side of the threshold. A triangular kernel is used. The polynomial order is 1, and the optimal bandwidths are derived under the MSERD procedure separately for each dependent variable. There are no control variables. The Table also provides the number of observation in the estimation bandwidth (column 6) as well as the value taken at the cutoff by the polynomial fitted on the left side of the RDD threshold (column 2).

The core sample of workers only includes workplaces for which an employer has been also surveyed while the larger sample includes all workplaces selected to take part to REPOSE11. Workplaces younger than five years or having professional elections every two years are excluded except on the larger sample of worker where this selection cannot be done.

**Table C2:** RD estimates for main outcomes of interest for a fake reform applying on April 15<sup>th</sup> 2009

	Sample Mean	Estim. left of thresh.	RD conv. estim.	RD BC estim.	N obs	N obs in band.
<b><i>Panel A: Employers main outcomes</i></b>						
At least one union recognized	0.659	0.840	-0.058 (0.084)	-0.072 (0.099)	1911	582
Trust in unions index	0.000	0.314	-0.108 (0.195)	-0.156 (0.229)	1782	525
Unionization rate (declared by employer)	0.112	0.123	0.040 (0.034)	0.039 (0.040)	1277	441
Work stoppage (any kind) (between 2008 and 2010)	0.343	0.507	-0.093 (0.103)	-0.118 (0.122)	1911	604
Social climate	-0.000	-0.183	0.093 (0.173)	0.111 (0.215)	1910	919
<b><i>Panel B: Workers' main outcomes (core sample of workers)</i></b>						
Share of workers union members (from workers responses)	0.121	0.171	0.023 (0.062)	0.036 (0.072)	1586	431
Trust in unions index	-0.000	0.033	-0.126 (0.219)	-0.078 (0.251)	1453	368
Participation to a work stoppage	0.178	0.251	-0.048 (0.075)	-0.047 (0.090)	1579	448
<b><i>Panel C: Workers' main outcomes (larger sample of workers)</i></b>						
Share of workers union members (from workers responses)	0.128	0.222	-0.042 (0.045)	-0.045 (0.052)	3100	1002
Trust in unions index	-0.000	0.192	-0.129 (0.181)	-0.083 (0.220)	2835	967
Participation to a work stoppage	0.191	0.222	-0.021 (0.049)	-0.022 (0.059)	3078	1110

*Notes:* The Table provides a placebo test for the LATE of the 2008 reform. For this placebo test, the RDD threshold is moved from January 1<sup>st</sup> 2009 to April 15<sup>st</sup> 2009. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . See notes of Table C1 for more details on the implementation of the RDD.

## Appendix D Robustness checks

**Table D3:** RD estimates for main outcomes of interest when workplace control variables are included

	Sample Mean	Estim. left of thresh.	RD conv. estim.	RD BC estim.	N obs	N obs in band.
<b><i>Panel A: Employers main outcomes</i></b>						
At least one union recognized	0.659	0.571	0.170** (0.075)	0.176** (0.089)	1898	579
Trust in unions index	0.000	-0.259	0.403** (0.196)	0.421* (0.232)	1770	440
Unionization rate	0.112	0.050	0.054* (0.032)	0.055 (0.040)	1270	365
Work stoppage (any kind) (between 2008 and 2010)	0.343	0.232	0.138 (0.093)	0.168 (0.102)	1898	322
Social climate	-0.000	0.096	-0.259 (0.168)	-0.287 (0.196)	1897	506
<b><i>Panel B: Workers' main outcomes (core sample of workers)</i></b>						
Share of workers union members	0.121	0.087	0.087* (0.052)	0.085 (0.060)	1584	245
Trust in unions index	-0.000	0.299	0.018 (0.220)	0.040 (0.259)	1452	171
Participation to a work stoppage	0.178	0.134	0.061 (0.069)	0.050 (0.081)	1577	213

*Notes:* Workplace controls include variables used for balancing checks in Table 2: 5 sectors, 4 workplace size groups, 5 workplace age groups, Paris region, single-plant firm, professional election every 3 or 4 years, gender of the employer interviewed. In addition, we systematically include controls for the entire workforce composition: share of women, share of young workers and share of workers in the 4 main occupation groups. 6 dummies for the month of interview (January to June 2011) are also included in panel A only while controls for the mean characteristics of the workers interviewed (gender, age, education and occupation) are included in panel B only. Finally, workplace controls are not available on the larger sample of workers, so that we only present this robustness check on the smaller sample of workers.

There is one row for each relevant outcome variable. Both the RDD conventional estimator and its standard error (column 3) and the bias-corrected estimator and its associated robust standard error (column 4) are shown. For each estimate and its associated standard error, we recomputed p-values and used the standard convention: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . To get RDD estimates, separate polynomials are fitted on each side of the threshold. A triangular kernel is used. The polynomial order is 1, and the optimal bandwidths are derived under the MSERD procedure separately for each dependent variable and set of controls (see Calonico et al. (forthcoming)). The Table also provides the number of observation in the estimation bandwidth (column 6) as well as the value taken at the cutoff by the polynomial fitted on the left side of the RDD threshold (column 2).

The core sample of workers only includes workplaces for which an employer has been also surveyed. Workplaces younger than five years or having professional elections every two years are excluded.

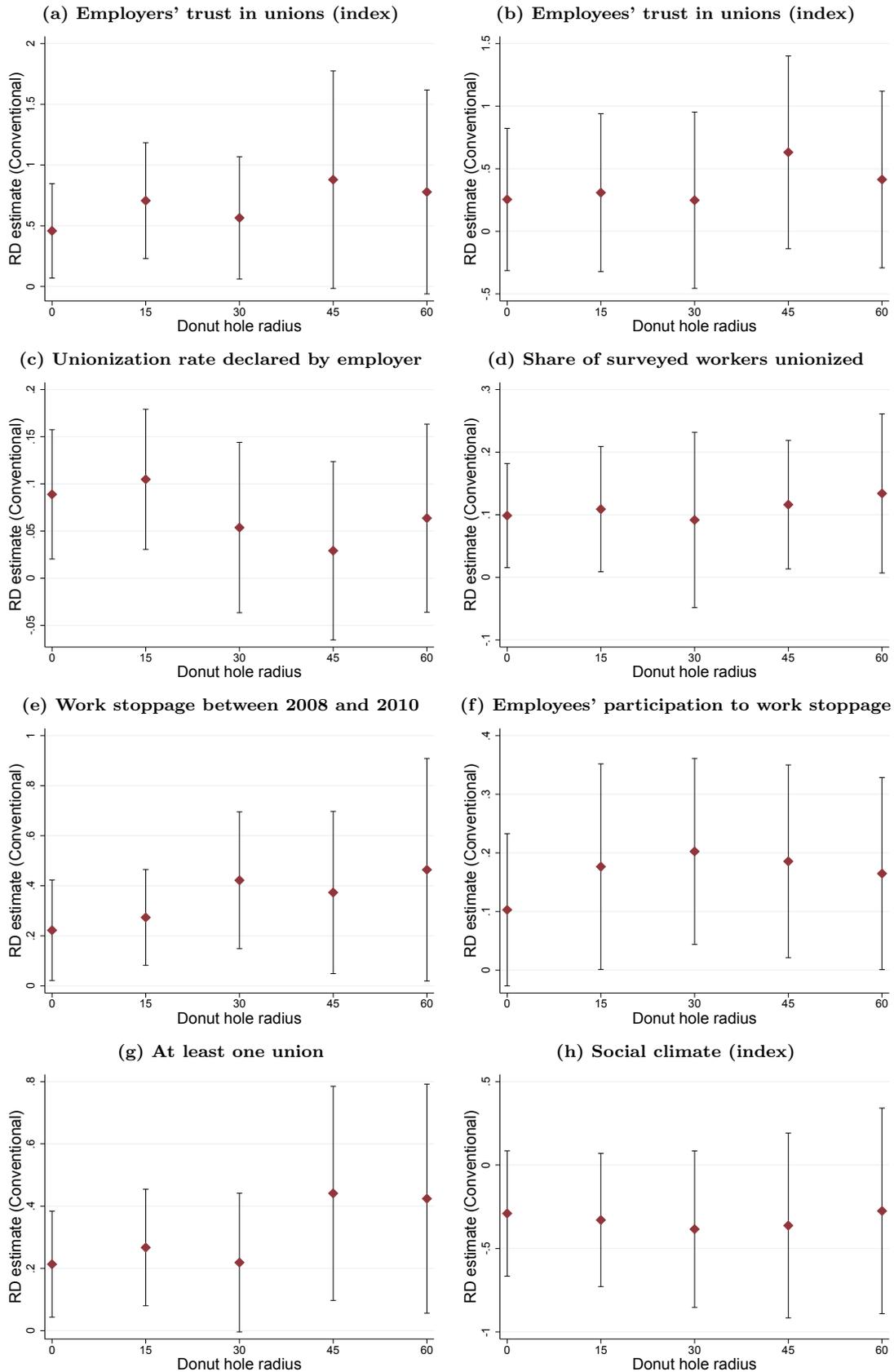
**Table D4:** RD estimates for main outcomes of interest when using a uniform kernel (instead of triangular) to construct the point estimator

	Sample Mean	Estim. left of thresh.	RD conv. estim.	RD BC estim.	N obs	N obs in band.
<b><i>Panel A: Employers main outcomes</i></b>						
At least one union recognized	0.659	0.533	0.295*** (0.093)	0.302*** (0.110)	1911	443
Trust in unions index	0.000	-0.254	0.494** (0.206)	0.525** (0.240)	1782	528
Unionization rate	0.112	0.045	0.095*** (0.034)	0.104** (0.043)	1277	258
Work stoppage (any kind) (between 2008 and 2010)	0.343	0.231	0.276*** (0.107)	0.305** (0.119)	1911	288
Social climate	-0.000	0.099	-0.360* (0.205)	-0.396* (0.230)	1910	287
<b><i>Panel B: Workers' main outcomes (core sample of workers)</i></b>						
Share of workers union members	0.121	0.081	0.112** (0.057)	0.102 (0.065)	1586	217
Trust in unions index	-0.000	0.129	0.135 (0.286)	0.188 (0.339)	1453	168
Participation to a work stoppage	0.178	0.137	0.100 (0.062)	0.106 (0.076)	1579	261
<b><i>Panel C: Workers' main outcomes (larger sample of workers)</i></b>						
Share of workers union members	0.128	0.054	0.169*** (0.046)	0.184*** (0.050)	3100	406
Trust in unions index	-0.000	0.045	0.243 (0.196)	0.275 (0.221)	2835	349
Participation to a work stoppage	0.191	0.144	0.075 (0.058)	0.082 (0.068)	3078	404

*Notes:* The Table provides LATE of the 2008 reform estimated by RDD. There is one row for each relevant outcome variable. Both the RDD conventional estimator and its standard error (column 3) and the bias-corrected estimator and its associated robust standard error (column 4) are shown. For each estimate and its associated standard error, we recomputed p-values and used the standard convention: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . To get RDD estimates, separate polynomials are fitted on each side of the threshold. A *uniform* kernel is used. The polynomial order is 1, and the optimal bandwidths are derived under the MSERD procedure separately for each dependent variable. There are no control variables. The Table also provides the number of observation in the estimation bandwidth (column 6) as well as the value taken at the cutoff by the polynomial fitted on the left side of the RDD threshold (column 2).

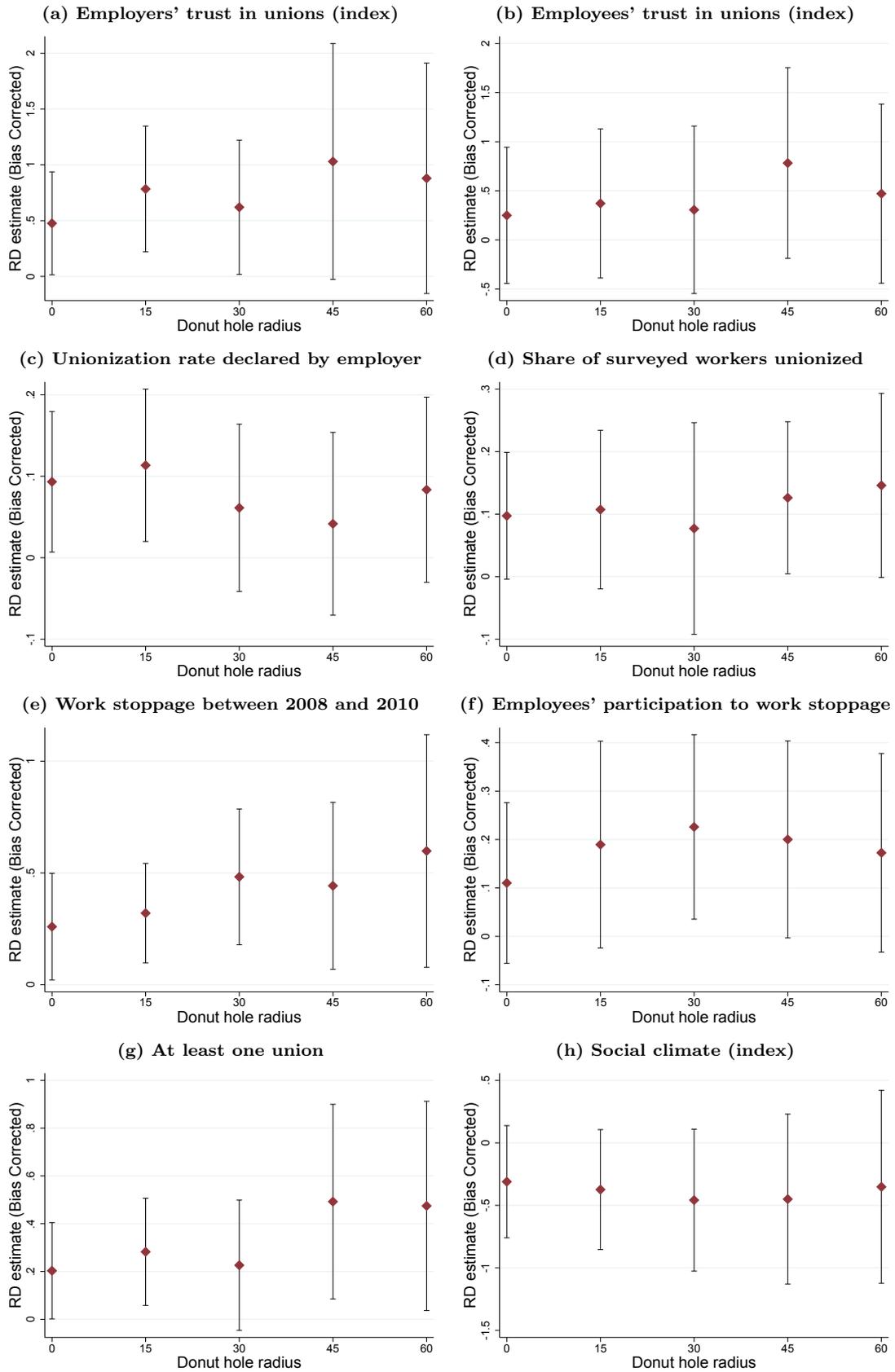
The core sample of workers only includes workplaces for which an employer has been also surveyed while the larger sample includes all workplaces selected to take part to REPOSE11. Workplaces younger than five years or having professional elections every two years are excluded except on the larger sample of worker where this selection cannot be done.

**Figure D4:** RD estimates (conventional) based on the donut hole approach for the eight main outcomes of interest



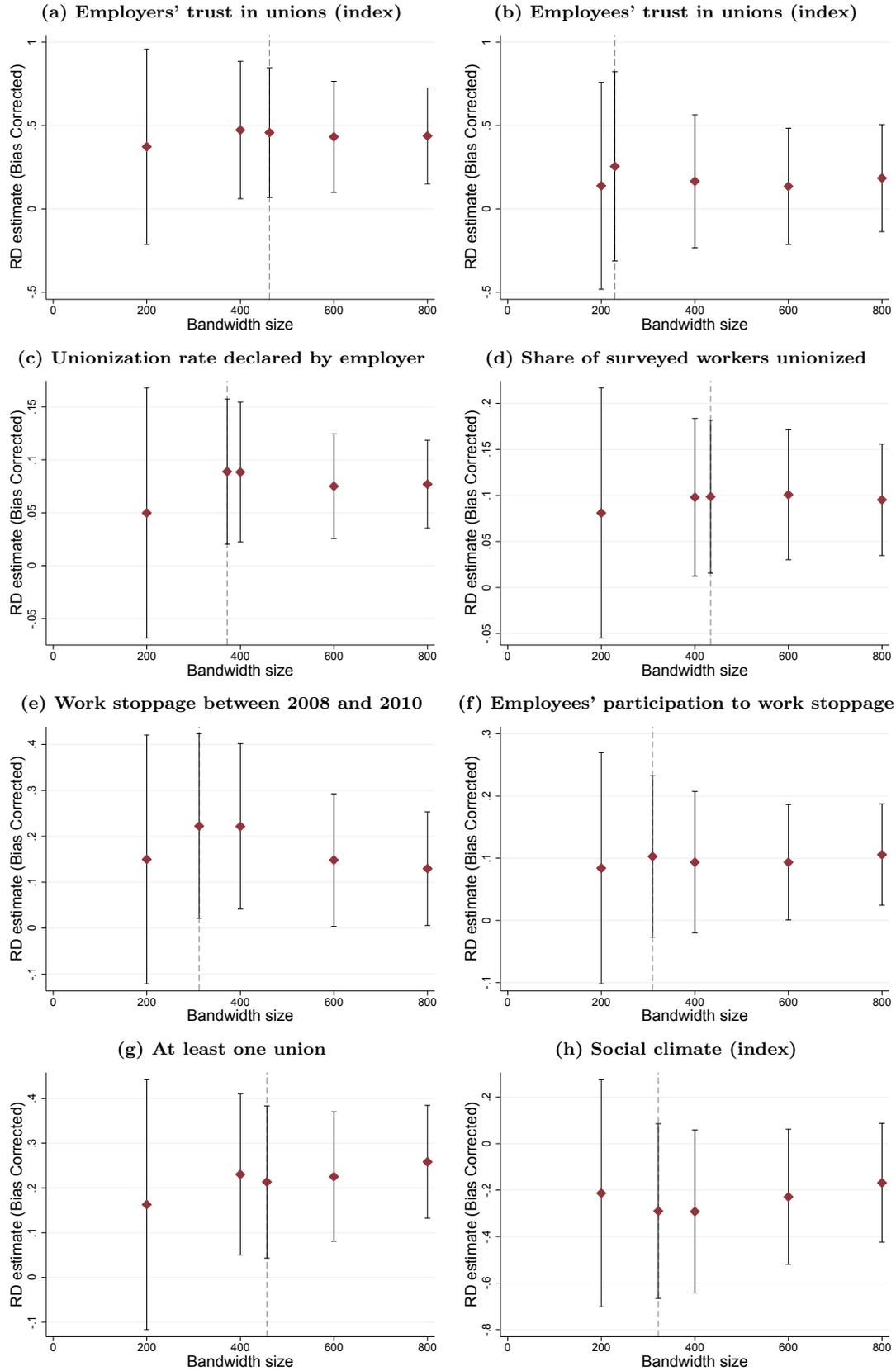
*Notes:* For each of the paper's eight main outcomes of interest, the Figure provides RDD conventional estimates (red diamonds) and their associated conventional 95% confidence intervals (black vertical straight lines) obtained after removing 0 to 60 days on each side of the January 1<sup>st</sup> 2009 cutoff date ("donut hole radius"). A donut hole radius of 0 day yields the baseline estimates provided in the paper when no observations are removed around the cutoff date.

**Figure D5:** RD estimates (bias-corrected) based on the donut hole approach for the eight main outcomes of interest



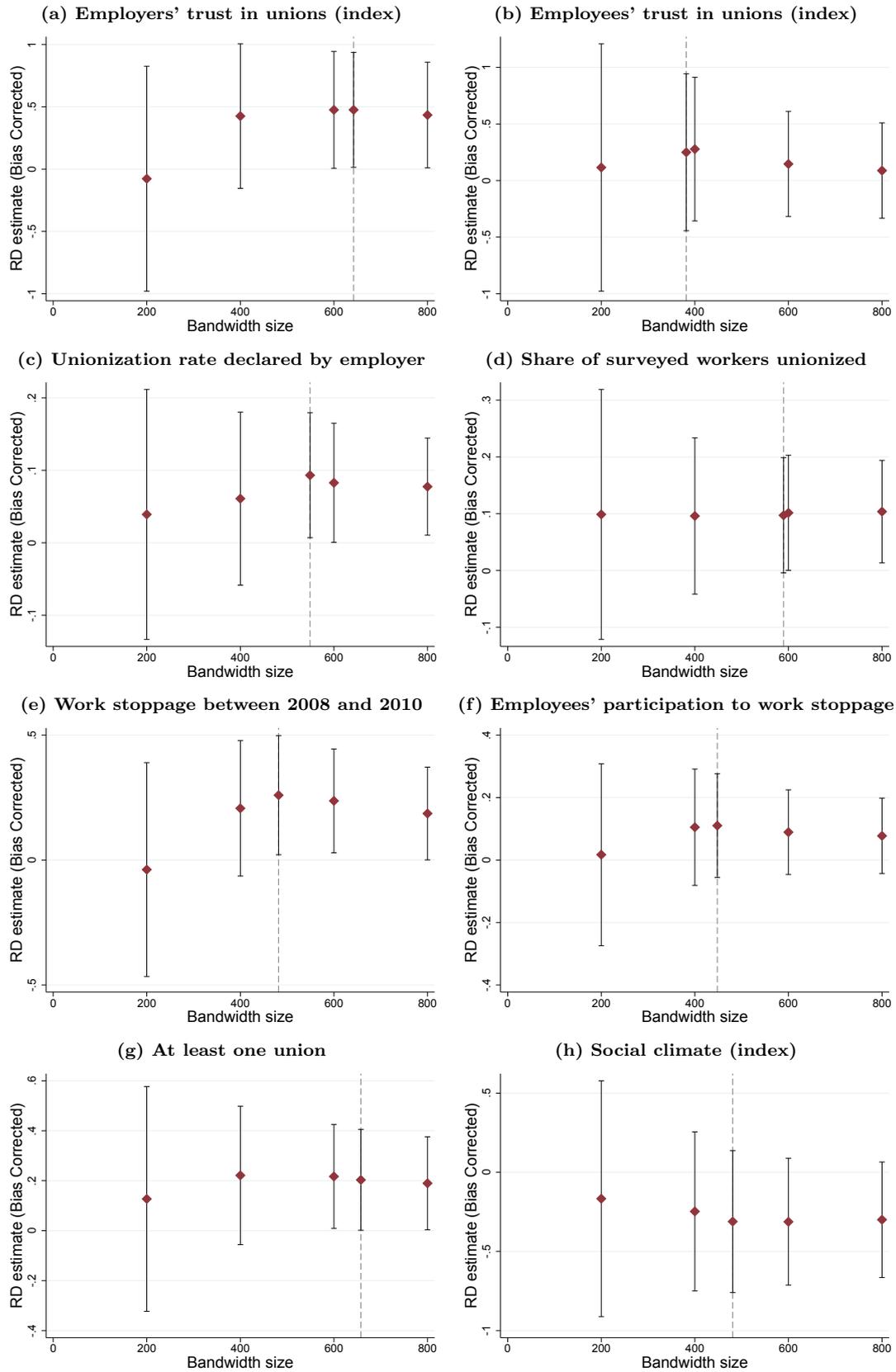
Notes: For each of the paper's eight main outcomes of interest, the Figure provides RDD bias-corrected estimates (red diamonds) and 95% *robust* confidence intervals (black vertical straight lines) obtained after removing 0 to 60 days on each side of the January 1<sup>st</sup> 2009 cutoff date ("donut hole radius"). A donut hole radius of 0 day yields the baseline estimates provided in the paper when no observations are removed around the cutoff date.

**Figure D6:** RD estimates (conventional estimator) for various bandwidth sizes for the eight main outcomes of interest



*Notes:* For each of the paper's eight main outcomes of interest, the Figure provides RDD conventional estimates (red diamonds) and their associated conventional 95% confidence intervals (black vertical straight lines) obtained on bandwidths of various size around the January 1<sup>st</sup> 2009 cutoff date. Results on bandwidths of 200 to 800 days on each side of the cutoff are shown. The vertical dashed line indicated the MSERD optimal bandwidth. The RDD estimate for this optimal bandwidth (corresponding to the baseline estimate given in the paper) is also provided.

**Figure D7:** RD estimates (bias-corrected) for various bandwidth sizes for the eight main outcomes of interest



*Notes:* For each of the paper's eight main outcomes of interest, the Figure provides RDD bias-corrected estimates (red diamonds) and 95% *robust* confidence intervals (black vertical straight lines) obtained on bandwidths of various size around the January 1<sup>st</sup> 2009 cutoff date. Results on bandwidths of 200 to 800 days on each side of the cutoff are shown. The vertical dashed line indicated the MSERD optimal bandwidth. The RDD estimate for this optimal bandwidth (corresponding to the baseline estimate given in the paper) is also provided.

## Appendix E Construction of the date of the latest professional election before the interview with an employer in REPONSE11

The administrative data on professional elections includes the minutes of all elections for workers' delegates, members of the work council, or members of the Unique Delegation of employees (Délégation Unique du personnel, which can replace and merge the remit of the workers' delegates and the work councils) that took place between 2009 and 2012. Those minutes are collected through standardized administrative forms that firms have to fill and send to the General Labor Services (*Direction Générale du Travail*).<sup>A.1</sup> Those forms include information on the type of the election (workers' delegates, work council members or Unique Delegation of employees), its date, and the results. For each election registered, the date of the closest former election of the same type is also registered. This information will be crucial to recover election dates for elections that took place before 2009.

The August 20<sup>th</sup> 2008 law provides precise guidelines regarding the elections that are eligible and those that are not to determine the representativeness of unions and their delegates. Elections for work councils are used in priority (typically in workplaces with more than 50 employees). In workplaces that have no work council, elections for the Unique Delegation of Employees are used instead. In workplaces that had neither work council nor Unique Delegation of Employees, elections of workers' delegates are finally used.<sup>A.2</sup>

Our algorithm to construct the date of the latest professional election before an employer is interviewed in the REPONSE survey in a given workplace is based on the institutional rules described above.

For each type of election (work council, Unique Delegation of Employees, workers' delegates), we start by identifying in the data the most relevant election date (if any) as

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<sup>A.1</sup>Some minutes may be missing if a firm has not sent to the central administration the standardized form. This explains that the election date cannot be recovered from the administrative data in some of the establishments in the REPONSE survey where the managers indicates that there was an election. Our robustness checks based on the year of the election declared by managers interviewed in the REPONSE survey are not subject to that selection and allow us to check that it does not affect the results.

<sup>A.2</sup>The data also includes information on partial elections. We discard them as the law exclude to use them to determine the representativeness of unions.

follows:

1. We code as “tentative dates” all registered dates and all registered dates of the former election of the same type for all elections registered between 2009 and 2012 in the administrative data, providing that they are anterior to the date of the known interview with the employer.
2. In each workplace, we take the latest “tentative date” as the date of the latest election of the considered type before the REPOSE survey.

The latest relevant election date is then obtained by aggregating the information on each type of election. In workplaces that had elections for work council, we take the election date obtained by the algorithm above. Otherwise, we switch to the election date calculated for the Unique Delegation of Employees, and then to that for workers’ delegates. For workplaces that had elections for work councils or Unique Delegation of Employees more than four years before the beginning of the REPOSE survey and more recent elections for workers’ delegate, we consider the later as the relevant election (assuming that the work council or Unique Delegation of Employees did not exist anymore).<sup>A.3</sup>

The algorithm to determine the date of the last relevant election date before a worker has filled the REPOSE11 questionnaire differs in two aspects. First, in step 1 above we keep all elections that are anterior to April 1<sup>st</sup> 2011. Second, workplaces for which there are election dates between April 1<sup>st</sup> and July, 22<sup>sd</sup>, 2011 are removed unless these dates concern only elections for workers’ delegates and there is a relevant election for work councils before April 1<sup>st</sup> 2011.

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<sup>A.3</sup>This last imputation has no impact on our results.

# Appendix F Construction of consistent time series of unionization rate

## F.1 Changes and problems with data sources over time and the SRCV survey

The European Union Statistics on Income and Living Conditions (EU-SILC) aims at collecting timely and comparable cross-sectional and longitudinal multidimensional microdata on income, poverty, social exclusion and living conditions. This system responds to a demand of the European Commission and is steered by Eurostat. The *Statistiques sur les ressources et conditions de vie* (SRCV) survey is the French part of the EU-SILC.

Every 2 or 3 years, SRCV includes an unambiguous question on union membership. It is used as the official source from 2008 to 2010 by the DARES (Direction of analysis, research and statistics of the French ministry of Labor), and as the joint source with the French survey on working conditions in 2013. This latter survey is now the preferred source for DARES because its sample is larger and covers overseas départements. The periods of collection of the two surveys are different: May and June for SRCV and from October (of the previous year) to June for the working conditions survey. Findings from both sources are quantitatively similar on the same perimeter (Metropolitan France): estimates of unionization rates in 2013 both in the private and public sectors differ by only +/-0.2 percentage points. In 2016, the difference between the two sources is larger. The official unionization rates obtained by the Dares from the working condition survey in the public and private sectors are respectively equal to 18.7 and 8.4% while they are equal to 17.44 and 8.79% in SRCV. We do not have a clear explanation for these discrepancies.<sup>A.4</sup> For consistency, we keep only SRCV for our analysis from 2008 to 2016, but using the working conditions survey in 2016 instead would not alter our qualitative conclusions of a declining (increasing) union membership in the public (private) sector over the studied period. SRCV also provides information on the size of the workplace, its industry and the tenure of the worker in this workplace. We thus use the SRCV 2008, 2010, 2013, 2016 for providing consistent trends of union membership from 2008 to 2016.

SRCV replaces the EPCV (Permanent survey on the life conditions of households)

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<sup>A.4</sup>They may be partly explained by the difference in collection periods and the very large social movement that occurred in May and June 2016 against the 2016 labor market reform.

that was used from 1996 to 2006 as the official source for union membership. This source is proved to strongly underestimate the union membership rate in France. The question about union membership was ambiguous and inconsistent with the French law. Individuals were asked if they were members of various types of “associations” such as an “association of parents”. Among the listed possible “associations” was “a *syndical* or professional group”. Belonging to “a *syndical* or professional group” was considered as union membership. However, unions and associations have distinct legal statuses in France; a “professional group” may more refer to a friendship club of bakers than a trade union; and a “*syndical* group” may stand more for a *conseil syndical*—an ownership board in a collective property—rather than a *syndicat* (*i.e.*, a labor union).

A variety of comparative databases still used these old inconsistent data. New official historical macro series include rough corrections done through simple calibrations (see Pignoni (2016) for details), as well as latest OECD series. Unfortunately, it is impossible to correct properly the biases in order to estimate pre-2008 trends by firm size or workers’ status.

## **F.2 Using the REPONSE 2005 survey to get an estimate of the unionization rate in the pre-reform period**

An alternative is to use the employee and employer REPONSE 2005 surveys. Surveyed workers answered an unambiguous question on union membership “Are you a member of an union? Yes or No”. The Dares provides sampling weights to correct for non-response and match the observable characteristics of the French workforce on the survey sample. Unfortunately, when building these weights the Dares aligned unionization rate in REPONSE 2005 to that in EPCV 2003 which has been proved to be wrong since then (see above). This implies that weighted statistics in REPONSE 2005 are not reliable, especially when it comes to measure the unionization rate which is by construction equal to the under-estimated one in EPCV 2003.

As a consequence, we had to rely on either non-weighted statistics on union membership from workers surveyed in REPONSE 2005 or weighted statistics based on employers *declared* union membership in their workplace.

Our preferred approach is to rely on non-weighted statistics on union membership. This is for two reasons: (i) the unionization rate estimated by employers in their workplace

is often missing and may be less reliable, and (ii) the non-weighted unionization rate on REPOSE 2011 is equal to 10.92%, which is reasonably close to the estimate obtained with SRCV 2010 on the same sample (11.40%, see Table 6).

The non-weighted share of workers in REPOSE 2005 that member of a union is 12.1%. However the REPOSE 2005 does not include workplaces having between 11 and 20 employees. Instead of recomputing all statistics based on SRCV on this sample, we multiply the non-weighted unionization rate in REPOSE 2005 by the ratio between the unionization rate in SRCV2010 on a sample corresponding to the REPOSE11 sample (11.40%, see last row of panel A of Table 6) and the non-weighted unionization rate in the REPOSE11 employee survey *among workplaces with 20 employees or more only* (11.29%). This calibration corrects both for observed differences between the REPOSE and SRCV surveys on a similar sample, and sample discrepancies. The final estimated unionization rate that would have prevailed in 2005 among workers with at least one year of tenure in workplaces with more than 10 employees is 12.21% (Table 6).

We have also used the declaration of employers regarding their workplace unionization rate to get an alternative estimate. These are obtained both in REPOSE 2011 and REPOSE 2005. We have used the workplace-level survey weights (which do not include any correction for unionization rates) to compute estimates of the total number of union members (obtained as the weighted sum of the number of union member in each workplace) and total number of workers in the population covered by the survey. Dividing the former value by the latter provides estimates of a unionization rate equal to 10.99% in 2005. We then apply a correction close to the one before, except that it corrects for discrepancies between estimated unionization rates in the REPOSE11 *employer* survey and the SRCV 2010 *employee* survey: we multiply the estimate of 10.99% by the ratio between the SRCV estimate in 2010 for a sample corresponding to REPOSE11 (11.40%) and the estimate obtained for workplaces with more than 20 employees using the REPOSE11 *employer* survey (11.05%). We finally get an alternative estimate of the unionization rate in 2005 equal to 10.72%. This second estimate is quite lower than the one presented in Table 6 but still larger than the estimated unionization rate in 2008. In all cases, our analyses conclude that unionization was declining between 2005 and 2008 among workers with at least a year of tenure in workplaces with more than 10 employees.

### F.3 Propensity score reweighting

We employ a variant of the kernel reweighting approach introduced by DiNardo et al. (1996), following (among others) Autor et al. (2008). We refer to these papers for theoretical details and only explain here how we implemented the technique.

Denote  $X_{it}$  for an individual  $i$  observed in year  $t$  the vector of individual and firm characteristics we wish to maintain at their 2008 level in subsequent years (age, age squared, gender, education in 8 groups, occupation in 10 groups, workplace size in 5 groups and sector in 15 groups). For each year  $t'$  in 2010, 2013 and 2016 we pool together data for 2008 and  $t'$ . We then construct an indicator variable  $T_{it}$  for an observation corresponding to year  $t'$  (rather than 2008) and run a weighted logit of  $T_{it}$  on  $X_{it}$  on each of the subsamples for which statistics are presented in Table 6. For the weighting, we use the sampling weights  $sw_{it}$  made available for each individual observation  $i$  in each SRCV survey. We then retrieve the individual-level predicted probability  $p_{it}$  of being in year  $t$  conditional on  $X_i$  (the propensity score) and construct individual weights  $w_i$  as follows:

$$w_{it} = sw_{it} \text{ if } t = 2008$$

$$w_{it} = sw_{it} * \frac{1-p_{it}}{p_{it}} / \frac{1-p_{t'}}{p_{t'}} \text{ if } t = t'$$

where  $p_{t'}$  is simply the (weighted) mean of  $T_{it}$ .  $p_{t'}$  captures the probability that an observation is observed in  $t'$  rather than in 2008 and enters the weight to cancel the fact that the propensity score also captures differences in sample sizes across years.

In each subsample of interest, we finally report in Table 6, panel B the weighted average of the unionization rate in each year  $t'$  using  $w_{it}$  as weights.