

# Electoral Democracy at Work

Philippe Askenazy and Thomas Breda\*

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## Abstract

We show that an institutional change designed expressly to heighten competition for the provision of union services can have a substantial effect on unionization and employment relations. We study a French reform of 2008 that introduced mandatory elections for representation of workers at firm, industry and national levels, putting an end to the oligopoly held until then by five historically established unions. Exploiting random variation in the reform's date of application in different private sector workplaces, we find that the reform increased union membership by around 8 percentage points and employers' trust in unions by 45 percent of a standard deviation. The reform also increased workers' trust in unions and the frequency of labor conflicts in manufacturing. Taken together, the results suggest that regular free elections can be an effective way to foster participation in unions and workers' ability to voice concerns, while at the same time making unions more legitimate bargaining partners for employers.

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

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

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\*Askenazy: Centre Maurice Halbwachs, Centre National de la Recherche Scientifique(CNRS) and ENS, philippe.askenazy@ens.psl.eu. Breda: Paris School of Economics, CNRS, thomas.breda@ens.fr. We are grateful to Alex Bryson, Gilbert Cette, Daniel Hamermesh, Vincent Pons and Ekaterina Zhuravskaya for their helpful comments on this manuscript. We thank seminar participants at Bristol University, CREST, OFCE, Aix-Marseille University, Paris Saclay University, Tinbergen Institute, the OECD, SIOE 2017 conference, the French Ministry of Finance 2018 conference and IZA worksop LMI 2020. We also thank the Dares at the French Ministry of Labor for giving us early access to administrative data on professional elections.

# Introduction

The quality of employment relations and the relative representativeness of trade unions are generally held to be significant determinants of a country’s business performance and inclusiveness. Cooperation between employers and employees can improve competitiveness (Aghion et al., 2011). In addition, a high membership rate improves unions’ position as legitimate partners for firms and so helps to foster labor-management cooperation. It can also increase workers’ bargaining power and reduce wage inequality (Card et al., 2004; DiNardo et al., 1996; Dustmann et al., 2009; Farber et al., 2018). In keeping with these familiar theses, enhancing social dialogue has become a central objective of policy makers and international organizations.<sup>1</sup>

The problem is that the paths to enhanced unionization (i.e. expanded union membership) and cooperation between workers’ representatives and employers remain largely unknown. While the decline in trade unionism is increasingly seen as a matter of concern for the representativeness of unions, especially given the increase in wage inequality in many developed countries, there is a dearth of practical policy solutions to revitalize the unions. The consensus among international institutions goes no farther than application of fundamental principles and rights at work (such as freedom of association and collective bargaining).<sup>2</sup> This lack of guidance is likely to be explained by the lack of clear empirical evidence in the academic literature, which mostly employs broad country-level comparisons between bargaining systems to explain the substantial international differences in unionization and cooperation (e.g. OECD (2018)). Such comparisons are rarely able to clearly identify specific channels conducive to higher unionization rates or improved labor-management cooperation. In a number of cases this comparative approach neglects the significant historical component of employment relations, which prevents effective regulations that may be in place in one country from being readily transferable to others.

The revitalization of labor movement is also explored by a vast literature in sociology of organization and in political science (see Murray (2017) for a review). The analysis does not focus on public policy and institutional reforms but rather on innovations in

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<sup>1</sup>A high-quality social dialogue was one of the four pillars of the concept of “decent work”, the prime standard of the International Labour Organization (ILO) for its centennial celebration in 2019. As such, it also forms one of the goals of the United Nations 2030 agenda for sustainable development.

<sup>2</sup>This contrasts with the detailed agenda of structural economic reforms usually called for by these same institutions.

internal organization and strategies of unions. The main concern is the oligarchy and conservatism in goals and tactics of unions. Following the seminal Michels (1915) “Iron law of oligarchy” and Jenkins (1977), over time, organizations face bureaucratization. Professionalized staff increases the distance between the members and the leaders. Then leaders alter the objectives and actions of the organization to keep their positions and to ensure the survival of the union. In that perspective, the revitalization can arise from new emergent, informal and innovative organizations, or from breaking the bureaucratic rigidity in entrenched unions. Keeping up and deepening democracy within the organization is believed as a key solution to overcome the oligarchy. But, in an influencing paper based on US case studies, Voss and Sherman (2000) show multiple paths for local union revitalization and argue that “the breakdown of bureaucratic conservatism paves the way for greater democracy and participation”, rather than the contrary.

We depart from these standard approaches and instead conduct a micro level study of the impact on unionization and labor-employer cooperation of an institutional reform of French labor relations enacted in 2008. The law mandated free elections to determine which unions could be recognized as bargaining agents at firm<sup>3</sup>, industry and national levels. These elections are repeated every two, three or four years and thus introduce permanent competition between unions. The reform did away with a situation in which five historically established trade unions essentially formed a legal cartel: they could always stipulate collective bargaining agreements for workers at the firm, industry and national levels (provided that they could designate volunteer representatives), while other unions faced stiff entry barriers. In addition, the appointment of the union negotiators (union delegates) at the workplace/firm level was discretionary. The reform imposes now that a union delegate should have attracted at least 10% of the workers’ votes on her name.

Drawing on the literature relating to the effects of free elections in politics (e.g., Wittman (1989); Besley et al. (2010)), we argue that the competition induced by such elections may be an efficient way to improve the quality of union representation and of the services provided to workers, and therefore to foster trade unionism and bolster workers’ trust in unions. In the bureaucratic conservatism perspective, the electoral condition to become a union delegate can reduce the distance between local union leaders and

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<sup>3</sup>To be recognized for bargaining, a union had to get a least 10% of the vote cast at these elections.

workers and can boost the emergence of new leaders or push the entrenched leaders to innovate. By also enhancing unions' legitimacy as bargaining partners, free elections may also increase employers' trust in unions. The chief contribution of this study is to test these simple predictions, which, to our knowledge, has never been done with relation to trade unions.

To evaluate the effects of the reform, we exploit the fact that implementation was gradual and its timing exogenous. In fact, the law instituted elections to determine which unions are legally recognized for bargaining, but free elections had already been held previously, to elect works council members or workers' delegates. The new regulations only became effective at the first post-enactment workplace election for councils or delegates. These elections must be held in all firms with more than 10 workers according to a pre-set frequency—usually every four years. This means that the date of election around the law's application date depend only on the date of the previous election at each firm, and so can be taken as random with respect to the reform, at least in firms old enough to have had elections in the past. The identification thus relies on a regression discontinuity design (RDD) in which the running variable is the date of the works council (or workers' delegate) election: we compare workplaces that had held elections slightly before and slightly after the reform became fully effective on 1 January 2009.

Using a unique dataset that combines a representative survey of both employers and workers at French establishments with more than 10 employees in 2011 and the exact dates of the elections according to administrative data, we find that the democratic rules introduced in 2008 increased “social capital” or “common ideology” à la Dunlop (1957) dramatically. That is, both employers' and workers' satisfaction and trust in unions measured in 2011 were much greater in the firms that had already applied the law—by about 45% of a standard deviation for employers and 30% for employees. Union coverage—i.e. the presence of at least one union recognized for bargaining—had jumped by 20 percentage points among the firms that were the earliest to apply the 2008 reform. Unionization rates increased by up to 8 percentage points. These local average treatment effects, obtained from the RDD described above, are very large, raising concerns on their validity. Accordingly, we backed them up with a study of macro trends in French union membership and employer-employee cooperation. The unionization rate rose from 9.7% in 2008 to 12.9% in 2016 among workers in the sample of private sector firms used

for our RDD estimates, while falling from 19.5% to 17.4% in the public sector, which was not affected by the reform. Similarly, we show that France is one of the countries that experienced the largest increase in the extent of cooperation between labor and employers (as reported by managers) between 2007 and 2016. The discussion provides further comparisons, all consistent with a substantial impact of the reform on unionization and trust in unions.

Finally, we find some evidence of a positive effect of the reform on the occurrence of moderate forms of conflict such as brief walk-outs, notably in manufacturing and construction. We interpret this as an expression of a stronger workers' voice in response to more democratic representation, consistent with Hirschman's analytical model of exit, voice and loyalty.<sup>4</sup> Interestingly, this increase in workers' willingness to voice their concerns is accompanied, according to our estimates, by a strong increase in employers' satisfaction and trust in unions; this indicates that absence of explicit labor conflict does not necessarily coincide with closer cooperation between workers and employers.

Heightening competition between unions through regular free elections could be taken into consideration by a number of other countries as a way of revitalizing trade unions, increasing their representativeness and strengthening social dialogue. Indeed, in many countries, unions' status as legal bargaining partners depends on quite informal criteria or on historical privilege, as in France before the reform. Even in countries that do hold elections, they are not always organized in such a way as to encourage competition. For example, union recognition in U.S. firms depends on a certification election for a single union, by the majority principle. There are no repeated elections, implying that once a union has gained recognition, it becomes entrenched, making it hard for potential entrants to compete. In Germany, there are regular elections for the representatives in works councils at private sector firms, but in practice the industry unions under the umbrella of the German Trade Union Confederation (DGB) have a quasi-monopoly in nominating the candidates.

**Related Literature.** Our paper relates to two main strands of the literature.

First, we add to the work on the sources of variations in the quality of employment relations. Typically, scholars have seen institutions and culture as the main forces driving

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<sup>4</sup>We find a large negative but not significant effect of the reform on voluntary resignations, our data sample being probably too small to study this convincingly.

these variations. Studies based on cross-country comparisons have tended to conclude either that cultural idiosyncrasies, including trust, play a major role (Black, 2005), or that institutional rules can account for both differences between countries and changes within them (Blanpain, ed, 2010).

From a different perspective, research on the diversity of capitalism (Hall and Soskice, 2003; Amable, 2003) has argued for complementarity between different types of institutions and put forth the thesis 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 and cooperation (e.g., Aghion et al. (2010)).<sup>5</sup> This literature typically studies the causes and long-run cultural consequences of major historical events (see Alesina and Giuliano (2015) for a review) and highlights the two-way causal relationship between culture and institutions. It also often relies on empirical comparisons of long-run evolution between 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 that in turn fuel (dis)trust, leading in the long run to multiple equilibriums.

A common characteristic of these studies is that they generally assume the impact of institutions on culture to be manifest and observable only in the long term. As such, they suggest that targeted changes in institutional rules may not be able to significantly shape union-management cooperation in a relatively brief time span. A contribution of the present study is the demonstration that this is actually possible. As far as we know, this is the first micro-level evaluation of the short-run impact of a switch to a more democratic system of worker representation. As such, our work also contributes to the literature on the benefits of democracy.

Second, we relate to two important strands of the literature on the determinants of unionization. One typically hypothesizes that the decision to join a union or to vote for unionization is a function of cost-benefit calculations (Pencavel, 1971); specifically, workers compare their current satisfaction with the wage and non-wage aspects of their job with expectations concerning how unions would change them (Farber and Saks, 1980).

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<sup>5</sup>At a micro level, there is some evidence consistent with an interplay between culture and institutions; for instance, Dill and Jirjahn (2017) find that in Germany, cooperation between management and works councils is less likely in firms owned by non-German stockholders.

The empirical evidence broadly supports these early models (Schnabel and 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., not being a union member in a high-unionization country) is individually costly. The second, more qualitative strand considers the possible union strategies (e.g., union structures, organizing techniques) that could lead to union revitalization (see Murray (2017) for a review). For example, more democratic internal functioning of unions is likely to be essential to foster unionization (e.g. Fiorito et al. (1988) on the U.S., Lévesque et al. (2005) on Quebec). A limitation of this work is that it offers only limited quantitative evidence on how institutional changes can affect unionization.

One exception is studies of right-to-work legislation in the U.S. stemming from the 1947 amendment to the Wagner Act. States were allowed to outlaw union shops, i.e. contract provisions requiring new employees to join the union and pay dues. Using state-level cross-section and panel data, Ellwood and Fine (1987) show that right-to-work laws significantly reduced flows into unionism through organizing.<sup>6</sup> An interesting feature of our own work, in this respect, is the demonstration that a targeted institutional change can have a substantial positive effect on unionization, whereas over the past four decades regulatory changes have mostly been conducive to the decline of unions. Another important contribution is our study of the quantitative effect of a specific institutional change on employers' attitudes towards unions, and more generally on labor-employer cooperation. Again, as far as we know this is the first such work.

**Organization.** The rest of the paper is organized as follows. Section 1 describes the relevant French institutions before and after the 2008 reform. The data are presented in section 2, and methods in section 3. The results are set out in section 4, and the mechanisms are discussed in section 5.

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<sup>6</sup>The more recent work assessing the effects of the statutory rights conferred on trade union learning representatives (ULRs) under the Employment Act of 2002 in the United Kingdom suffered from lack of suitable data and possible problems of endogeneity of the Act itself. These studies eventually reached conflicting conclusions (e.g. Wallis et al. (2005), McIlroy (2008)).

# 1 Institutional Settings

This section details the main changes introduced by the 2008 reform and their key implications. For a comprehensive description of the French system of employment relations and of the reform, see the Appendix A.1.

**Bargaining at workplace or firm level before and after the 2008 law.** The implementation of the 2008 reform in practice is shaped by the fact that three different types of worker representation mandates can coexist in French firms with more than 10 employees: works councils, workers' delegations, and union delegations. The members of the works council and the workers' delegates, both before and after the reform, are named at staff elections. However, prior to the reform, union delegates were non-elected volunteers designated by one of the five nationally representative unions.

The different types of worker representative had different prerogatives. In all covered workplaces/firms, the employer was required to inform workers' delegates and collect their views concerning several specific matters. Conversely, the delegates passed on individual grievances and collective demands concerning such matters as the organization of work (e.g., health and safety) or the application of higher-level collective bargaining agreements. In firms or workplaces with 50 employees or more, individual problems were still dealt with by delegates, but collective issues were mainly the prerogative of the works council (*comité d'entreprise*), which is chaired by the employer and whose functioning is more formally organized.

By contrast, formal collective bargaining is the province of the union delegates. When there are union delegates in a firm, only they are allowed to negotiate or sign legally binding collective bargaining agreements with the employer. Employers must negotiate with them at least once a year regarding wages, working conditions and employment.<sup>7</sup>

In this framework, the way union delegates are named is crucial for employers and unions alike. The first three rows of Table 1 summarize the main changes introduced by the reform in this regard. One obliges unions to select their delegates from candidates who won at least 10% of the votes in the first round of the staff elections. In other words, the reform did not introduce new elections for union delegates but forced unions to base

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



their choice on the elections already in being for other types of representatives. The second change was the opening of the first round of these elections to all trade unions, whereas previously only the five established unions could present candidates.<sup>8</sup> This put an end, *de facto*, to the legal cartel of the five established unions for the designation of both union delegates and other representatives. That is, the law heightened competition for the provision of union services at firm level.

**Industry-wide and national bargaining before and after the reform.** Before the 2008 reform, the same five historically established unions were also the only *de jure* collective bargaining partners in the 700 French economic branches and at national level. The 2008 reform ended this cartel by making representation dependent on the results of firm-level elections: to be representative and authorized to negotiate, a union had to win at least 8% of the votes cast in the first round of all firm-level staff elections in the branch or in the country (for national-level representation, see Table 1).

**The timing of the 2008 reform.** Following his election as president in May 2007, Nicolas Sarkozy asked the five established French unions and the employers' associations to start talks towards the modernization of industrial relations in France. After four months of bargaining, in April 2008 a "common position" was signed by the two largest employers' organizations and the two largest trade unions, namely CGT and CFDT.

Endorsed by the conservative government, the law for "the renovation of social democracy and working time" was enacted by Parliament in July 2008 and officially published on 21 August 2008. The reform adopted most of the points set out in the common position.

At the industry and national levels, starting 1 January 2009 union representation would be based on the results of firms' staff elections. Accordingly, elections from that date on have consequences not only at the firm but also at higher levels. However, it took four years before all firms had held elections under the new regime, so that changes in union representation at industry-wide and national level did not come until January 2013, based on the aggregation of firm-level votes in each industry and nationwide during the period 2009-2012.

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<sup>8</sup>Theoretically the elections are in two rounds, but the second round is held only if the first round has no candidates or too few candidates from unions or if the turnout is below 50%.

All the firms holding staff elections after 1 January 2009 applied the 10% threshold introduced by the August 2008 law to determine the local representativeness of union delegates. However, firms holding elections in September and October still applied the old rules, while those with elections in November or December 2008 may have used any of the two regimes. We return to this point in section 3.2 as it is key for our identification strategy.

## 2 Data

The empirical analysis is based on two main data sources.

**The REPONSE dataset: employer part.** Our first dataset is the French Ministry of Labor’s Workplace Employment Relations Survey for 2010-2011(REPONSE11), covering 4,023 non-agricultural business establishments with more than 10 employees. REPONSE11 is one of the leading sources of data on industrial relations in France. A management representative in each establishment completes a lengthy face-to-face interview relating mainly to work organization and industrial relations. These interviews were conducted between January and June 2011. The answers constitute the employers’ part of the survey, from which we retrieve information on the presence of union representatives, the unionization rate, employers’ opinion and relative trust of union and non-union representatives. The constructions of the outcome variables are detailed in Appendix B.1.

**The REPONSE dataset: employee part.** The “employee” part of the survey is derived from a 2-page, 50-item questionnaire distributed by mail. The questionnaires were filled out by a core sample of 11,378 workers in a subset of 3,680 of the establishments that participated in the employer survey, plus an additional sample of 6,555 workers in 2,226 more establishments for which no workplace level information is available. The data includes the usual worker demographics, work organization, job satisfaction, union membership, opinion and trust of union and non-union representatives. The questionnaires were sent out in two rounds at the end of March and the end of May 2011 to a non-stratified random sample of employees who were already at the same workplace at the end of December 2009.

We conduct most of the analysis of the employee data at establishment level, first because the source of the variation we exploit is at this level and, second, in order to get results that are comparable between the employer and employee sides. Thus for all outcomes we construct the workplace-level average of workers' answers.<sup>9</sup> To facilitate comparison with employers' reactions to the reform, our baseline specifications are for the core sample of workers. The larger sample is used only for robustness or heterogeneity analysis. Most of the outcome variables are similar to those obtained from the employer part of the survey (see Appendix B.1).

**The MARS dataset.** When they hold staff elections, firms transmit to the administration a report including: (i) the date (day, month, year) and type (works council or workers' delegates) of the election; (ii) the date of the previous election of the same type; and (iii) the results. The MARS administrative dataset is the compilation of these reports from 1 January 2009 onward. The administration exploits this dataset to compute union representativeness at industry/branch and national level. The full dataset was not publicly available at the time of this study, but we obtained from the Ministry of Labor an extract of all reports made by establishments of REPOSE11 during the period 2009-2012, including those of the additional sample of employees. The extract contains the information on points (i) and (ii); that is, for all elections held during those years, the dates and the dates of the previous elections.

**Latest staff election before the REPOSE survey.** Our empirical strategy (see next section) requires, for each employer and worker in the 2011 REPOSE survey, knowing the exact date of the latest staff election before the interview (employers) or questionnaire response (workers). For employers, we simply retrieve from MARS the latest relevant staff election before their known interview date. While conceptually simple, this is not entirely straightforward in practice, owing to the formatting of the MARS dataset and a series of institutional exceptions. We detail our algorithm in Appendix B.2. For the worker side, the approach differs slightly. They all filled out the REPOSE questionnaires at unknown dates between 1 April and 22 July 2011 (end of data collection). So the analysis of workers' responses discards establishments that held staff

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<sup>9</sup>This also ensures that the results are not driven by the different sampling scheme for small workplaces or by variations across workplaces in the actual number of workers responding.

elections during that period. For all other establishments, we consider the latest relevant election date before 1 April 2011.

### 3 Empirical approach

We exploit the fact that the reform did not affect all firms/workplaces at the same time, which makes it possible to compare, in 2011, workplaces “treated” by the reform with those not yet treated, which form our control group.

#### 3.1 Sharp Regression Discontinuity Design

**Necessary assumptions and identification.** Our main identification strategy relies on the fact that the new conditions introduced by the law of 21 August 2008 only became effective in any given firm/workplace with the first staff election held after a given cut-off date. Actually, there is some ambiguity regarding this cut-off. It is certain that all elections after 1 January 2009 were conducted under the new regime, but firms holding elections between October and December 2008 may have applied either the old or the new regime. We ignore this issue for now and start by taking 1 January 2009 as a sharp cut-off date. This is formalized by the following assumption:

**Assumption 1:** All elections before 1 January 2009 were organized under the old regime, all those after that date under the new regime.

Assumption 1 implies that the assignment to treatment  $T_j$  (union representation and bargaining decided under the new scheme) in workplace  $j$  is  $T_j = \mathbb{1}(D_j > 1^{st} Jan 2009)$  where  $D_j$  is the date of the most recent staff election before workplace  $j$  was surveyed in REPONSE11. The procedure for dealing with ambiguity in the cut-off date is discussed in the next subsection.

The key feature that provides identification is that the dates of the elections around 1 January 2009 were set well before the law was enacted in August 2008, and in fact even before its content was known or could be anticipated. This is because staff elections at each firm/workplace with more than 10 employees are held according to a predefined

frequency: every two, three or four years.<sup>10</sup> As a consequence, for workplaces where elections had already been held, the last election date  $D_j$  before the REPONSE survey should only depend on previous election dates and be unrelated to the date of application of the reform. Clearly, this would not be the case if workplaces could shift their election dates forward or back in response to the reform. The next subsection makes it clear, however, that altering the election date is possible only in very strictly defined cases, such that they are unlikely to allow for endogenous response to the reform. This leads to our next assumption:

**Assumption 2:** The election date  $D_j$  for a firm/workplace  $j$  that had already held elections in the past can be taken as random with respect to the application of the new regime.

Assumptions 1 and 2 – which are justified in the next subsection – guarantee identification. For a given variable of interest  $Y$  (measured in the first semester of 2011 using REPONSE11), each workplace has two potential outcomes,  $Y_j(1)$  and  $Y_j(0)$ , corresponding, respectively, to the outcomes that would be observed under treatment and under control conditions. Denote  $\tau = \mathbb{E}[Y_j(1) - Y_j(0)|D_j = 1^{st} \text{ Jan } 2009]$ , the causal impact of the reform on  $Y$  at the cut-off date.  $\tau$  is identified and can be estimated using the workplaces that held elections just around the cut-off date using a sharp regression discontinuity design (RDD) in which the forcing variable is  $D_j$ .

This RDD strategy identifies middle-run effects of the reform, since the outcomes are measured in the first half of 2011, or 2-2.5 years 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 system, i.e. the workplaces that held elections under the old system just before the new one took its place and those that were the first to use the new regime after it was enacted. The reform may have affected early and late compliers differently, but this cannot be assessed with the RDD design.

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<sup>10</sup>By default, every four years, except where an industry-level or firm-level agreement shortens it to three or two years. Importantly, such agreements cannot reduce ongoing mandates and only apply to following ones.

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

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

where  $y_{j,2011}$  is the outcome of interest measured in 2011 (between January and June for employers) in firm  $j$  and  $\epsilon_j$  is a residual term.  $\beta$  estimates the effect of having held the last staff election under the new regime.  $P$  and  $Q$  are polynomials in  $D_j$ , capturing the fact that the date of the election per se can affect outcomes measured in 2011.<sup>11</sup> This is the case, for example, if perceptions of unions change in election periods (say, because unions are more active then).  $X_j$  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 REPOSE11 (employer part only).

We estimate variants of (1) with first-order polynomials on local bandwidths around the cut-off date. There are several options for doing this, and the results can be sensitive to the choice of “tuning parameters”. The main text uses the estimates that are most logical for our context, while the appendix offers robustness checks to show that the results are not driven by some particular methodological choice.

Our preferred specification does not include controls. It uses a standard triangular kernel, according greater weight to the observations closer to the cut-off date. For our main outcomes, the appendix also gives estimates including controls and based on a uniform kernel according equal weight to all observations in the bandwidth. A further robustness check provides estimates obtained after excluding the observations that are very close to the cut-off and may be driving the results. This “donut-hole” method is applied taking holes of various sizes. This is an important check in our context, because there is some uncertainty over the exact cut-off date and we cannot simply exclude the possibility of manipulation of the running variable just around the cut-off.

The local bandwidths for the estimation are determined endogenously for each outcome. By default, we use the MSERD bandwidths developed by Calonico et al. (2014) (or Calonico et al. (2019) when controls are included), as they limit potential bias the most, but the appendix also gives estimates for our main outcomes with alternative bandwidths. Finally, all the tables giving RDD results show both (i) conventional estimates and p-values obtained by estimating equation 1 by OLS on the endogenously determined

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<sup>11</sup> $Q$  has no constant term, as no such term could be identified separately from  $\beta$ .

bandwidth and (ii) bias-corrected estimates computed following Calonico et al. (2014) as well as their associated robust standard errors and p-values.<sup>12</sup>

In addition to the estimates, we offer graphical evidence of possible discontinuities in the main outcomes at the cut-off. This is done on a broad four-year window. On either side of the cut-off we group observations in equal-size bins (4 bins to the left and 12 to the right), so that we have about 125 observations in each bin. Variants using more bins are shown in the appendix for some of the main outcomes.

**Sample restrictions.** First of all, identification depends on the predetermination of the election dates around the reform implementation according to past election dates. This is obviously not the case for workplaces too new to have had elections before 2007. Using a categorical variable in REPOSE11, we exclude workplaces in being for less than five years in 2011 (3.5% of the initial sample).<sup>13</sup> Second, workplaces that hold staff elections every two years should have had their latest election date under the new regime ( $D_j > 1^{st} \text{ Jan } 2009$ ) when observed in the first semester of 2011 in REPOSE11. This means that they cannot be used to identify the impact of the 2008 reform. As these workplaces may even induce a discontinuity at the threshold (they only appear on the right-hand side), we remove them too from the analysis, retaining only workplaces holding elections every three or four years (about 83% of the sample of workplaces older than five years). With these restrictions, the final sample consists of 1911 workplaces: 502 held their most recent election before and 1409 after the cut-off date.

### 3.2 Election dates and threats to identification

**Cut-off date for the application of the reform law.** We have no direct information on whether employment relations in the workplaces of REPOSE11 are governed by the old or new statutory scheme. The new scheme was supposed to apply to all firms/workplaces that started to *prepare* for staff elections subsequent to 21 August 2008, while those that had already entered the pre-election preparation period before that date were to apply the old scheme. This preparation period generally lasted around

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<sup>12</sup>Estimates and standard errors obtained using the Stata command *rdrobust*.

<sup>13</sup>Workplaces older than five years should have had at least two elections before REPOSE11, so that the latest election date is indeed predetermined. However, they may still have had only one election if in the past they were too small for elections. Robustness checks limiting the sample to large workplaces show that our results are not in fact affected by this possible problem.

two months, with a legal minimum of 45 days. It started with a meeting between the bargaining partners that issued a pre-election protocol specifying the rules and date of the election. We accordingly assume that workplaces that had elections in late August<sup>14</sup>, September and October 2008 had begun preparations before the reform was passed and so applied the old scheme.

Elections held in November and December 2008 are more complicated to deal with. They are more likely to have entered their preparation phase after the summer break and therefore to have applied the new scheme. But some uncertainty remains, because the reform modified only the top layer of the labor law. As is common in French policy, the administration later provided a comprehensive interpretation of the law: a ministerial circular (*Circulaire d'Application*) dated 13 November 2008 but officially published in the *Bulletin of the Ministry of Labor* only on 30 December. Which scheme governed elections held in November and December is thus not entirely sure. We presume they are more likely to have been organized under the spirit of the old regime, and our baseline analyses accordingly put the cut-off date for the application of the new scheme at 1 January 2009.

A final reason for taking this cut-off date is that elections before that date did not count towards establishing the representativeness of labor unions at the industry and national levels. The administrative data on workplace- and firm-level staff elections that are used for that purpose (MARS – see previous section) only began to be collected on 1 January 2009. This means that one of the three major changes introduced by the reform regarding union representativeness went into effect precisely at our chosen cut-off.

By blurring the discontinuity at the cut-off, the uncertainty surrounding the application of the other provisions of the reform is likely to reduce our estimates, unless the workplaces that held staff elections in November and December deliberately selected either the new or the old scheme in such a way as to generate bias. The “donut hole” RDD specifications are used to check for this.

**Manipulation of election dates.** An essential assumption for the RDD design to work is that workplaces cannot select themselves into the treatment group by manipulating the election date. Unsurprisingly, on paper such manipulation is extremely hard in a democratic country like France. First, artificially extending the incumbents’

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<sup>14</sup>Actually, almost no elections are held in July or August, typically vacation months.



mandate is a clear breach of democracy, and it is strictly controlled. Current mandates can be extended only for a “reasonable period”, and only with the joint agreement of all the unions represented and the employer. Such broad consensus leaves little margin for strategic behavior, as it is virtually impossible that all stakeholders will gain (for additional details, see the Appendix A.2). Second, mandates cannot be shortened either, unless all the worker representatives step down or are fired simultaneously, again leaving practically no room for strategic behavior.

To see whether the legal interval between elections is observed in practice, Figure 1a plots the distribution of the average number of months between two consecutive elections (for all those registered in 2009-2012 at the workplaces of the REPONSE11 sample). The distribution peaks at 24, at 36 and at 48 months, i.e. the three possible legal intervals. These peaks – where the distance from the three legally mandated election dates is less than 30 days – count more than 60% of the registered elections. Other cases may constitute pure measurement error (likely due to errors made regarding the previous election date, which usually happened more than two years before), official changes to the election calendar corresponding to the institutional cases specified above and in Appendix A.2, or the need to repeat the election immediately owing to some procedural flaw. The small peak at zero in Figure 1a may reflect the first or the last of these cases.

The standard way to detect manipulation is to find a discontinuity in the density of the forcing variable around the cut-off (McCrary (2008) for continuous variables). However, this requires the forcing variable to be smoothly distributed in the absence of manipulation, a condition that does not have to be imposed in order to perform an RDD. Now, the distribution of our forcing variable (the date of the latest pre-survey election) is strongly seasonal, with almost no elections in July or August or between Christmas and New Year’s (see Figure B1 in Appendix). This prevents testing for discontinuity around the cut-off.

To check visually for strategic manipulation, Figure 1b shows the distribution of election dates around 1 January 2009 (cut-off date) and 1 January 2010. The two distributions are not perfectly comparable, but they do 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 signal of strategic manipulation of election dates would be discontinuities

in predetermined covariates at the cut-off: if there is strategic manipulation, then the workplaces where elections were postponed or brought forward in response to the reform are likely to differ in their observable characteristics (size, age, sector, region, etc.). In fact, employment relations and union coverage vary significantly according to firm size and sector, so that the distribution of these characteristics around the cut-off is likely to be affected by manipulation of election dates. Table 2 provides descriptive statistics on observable workplace characteristics and checks for discontinuities at the cut-off. None of the estimated discontinuities are statistically significant at the 10% level, suggesting that in our framework manipulation was not a major issue.

## 4 The main results

A reform that changes the conditions for union recognition in firms is likely to affect employment relations along three major axes: (1) workers’ representation, and in particular the prevalence of unions and union members; (2) how unions are perceived by employers and workers; and (3) workplace conflicts and social climate.

We first give the results of the baseline specification for each set of outcomes. Then, the last sub-section describes the robustness checks and falsification tests for all outcomes. The main tables systematically report the conventional and bias-corrected regression discontinuity (R.D.) estimates, along with the value of the interest variable just to the left of the cut-off.

### 4.1 Workers’ representation and union membership

**Works councils and workers’ delegates.** Our identification strategy requires restricting the analysis to workplaces for which we observe elections for workers’ delegates or works councils (or members of the so-called *DÃ©lÃ©gation Unique*, which combines the two). Using employers’ declarations, we start by verifying that the workplaces do in fact have this type of representation. Table 3 (first row) shows that this is the case for more than 93%. The absence of workers’ delegates or a works council in a few workplaces, as reported by employers, could reflect situations in which all the representatives had resigned and not been replaced, or else inaccurate employers’ statements.

R.D. estimates then indicate that workplaces that held staff elections after 1 January

2009 are around 10 percentage points more likely to still have workers' delegates or a works council when surveyed in 2011 (Figure 2 (a), and Table 3, panel A). This is consistent with the thesis that representatives elected after the reform were less likely to resign, although we cannot test this directly. However, the statistical significance of this estimated effect is poor, and no definitive conclusion should be drawn.

**Union recognition.** Workplaces that hold staff elections do not necessarily have unions recognized as bargaining partners. For union bargaining, at least one worker must have agreed to serve as union representative; and for elections held after the 2008 reform, this worker must have gotten at least 10% of the votes at the staff election. Before the reform, it was much easier for the five historically established unions to be represented. For union representation overall, the reform has driven opposite mechanisms. The 10% threshold introduces a barrier that may discourage workers from becoming union representatives. However, as the conditions for designation of non-incumbent unions have improved, they may be able to obtain more candidacies. Finally, the votes at workplace elections will count towards the representativeness of all unions industry-wide and nationally. This provision of the reform provides a powerful incentive for unions to find candidates at each and every firm, as the votes obtained even by losing unions count towards their industry and national total.

Figure 2(b)<sup>15</sup>, and Table 3 panel B (second row) shows that the reform has had a strong positive impact on union recognition: the probability of having at least one union as a recognized bargaining agent has jumped from under 60% to 80%.

Interestingly, this substantial effect depends mostly on the established unions (Table 3), suggesting that the incentives created by the contest for representativeness at higher levels outweighed the introduction of an entry barrier for these unions.

As regards new unions, the estimated effect of the reform, as expected, is positive. It is large in relative terms – the probability of being recognized jumps from around 9% to almost 20% – but it is not statistically significant at conventional levels.

**Multi-unionism.** By heightening the incentive for the historically established unions to participate in elections and eliminating barriers for challengers, the reform should be expected to boost multi-unionism. However, the 10% threshold automatically makes it

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<sup>15</sup>Versions of this important figure with more bins are provided in Figure C2 in appendix C.

harder for there to be a large number of unions (or coalitions) recognized as bargaining partners at any given workplace.

We check to see whether these direct consequences of the reform can be observed in the data. Table 3 shows some evidence that this is indeed the case, although the estimates are not significant at conventional levels and should be considered as merely suggestive. The probability of multi-unionism (at least two unions) in a workplace jumped by 11 percentage points after the reform, while the probability of having five or more unions dropped from 10 to less than 7 percent.

**Union membership.** Has the reform, by allowing workers to elect their union representatives, fostered workers' sense of fit with unions and thus ultimately the likelihood of union membership? Here, two data sources are used. The first is employers' statements on the unionization rate at their workplace. From this source, we find a strong local average treatment effect (LATE): workplace-level union membership jumped from about 5% to 13% (Figure 3 and Table 3), panel A). This finding contrasts with the monographic works of Yon and Bérout (2013), in which human resource managers and union representatives (but not rank-and-file workers) were interviewed and did not report any upsurge in union membership.

While the REPONSE11 sample is designed to cover most business sectors, whereas the monographs are not, the apparent impact on union membership reported by our sample employers may be a statistical artifact. Or it might be owing to greater activism on the part of union members in these firms, so that employers mistakenly perceive them as more numerous. Our second data source is union membership status declared by the workers surveyed in REPONSE and averaged at the workplace level. These data avoid the foregoing caveats. The resulting measure of workplace-level union membership is constructed so as to be comparable to employers' statements. It cannot be biased by misleading employers' perceptions, but it is noisy, as it is based on only a handful of responses to the survey in each workplace. It also probably overestimates union membership and the overall impact of the reform, because most fixed-term and recently hired workers are not surveyed. Results based on this measure largely confirm the employers' declarations (Figure 3 and Table 3, panel B). Restricting the analysis to the core sample of workers (including only workplaces where employers also participated in the survey),

the magnitudes are similar, but only significant at the 10% level for the bias-corrected estimator. For the entire sample of workers, the estimated effects are even greater.

In conclusion, we find that the reform had a positive and significant effect on union membership in private workplaces with more than 10 workers. The estimated impact is very large, but the confidence interval is also very large in all specifications. The plausibility of these estimates is discussed in section 5.

## 4.2 Employers’ and employees’ perceptions of unions

We now turn to our second main question: has the reform improved employers’ and employees’ perceptions of and trust in labor unions? To this end, both employers and workers were asked how far they agree or disagree with a series of statements about unions:<sup>16</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 Totally agree to Totally disagree, plus “Don’t know”. Since the four different questions elicit little independent information, we merge them into a single trust/satisfaction index: the sum of the score for the first two statements minus the last two. The index is then standardized to have a mean of 0 and a standard deviation of 1. It is our main outcome of interest.

**Employers’ perceptions.** The employer but not the employee questionnaire has a question on the “representativeness of trade unions in general terms” (a 4-point Likert scale from very weak to very strong). Prior to the reform, almost 40% of employers considered unions’ representativeness to be very weak. This widespread feeling that unions are not representative could reflect the lack of direct democracy for electing delegates at firm and workplace level. Figure 4 and Table 4 panel A confirm this intuition, showing that the reform cut the probability of employers’ considering representativeness to be very weak in half (to about 20%).

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<sup>16</sup>The description of the questions is based on a public translation of the REPONSE questionnaires, see Amossé et al. (2016) and appendix B for further details.

We find an effect of the reform of about 45% of a standard deviation on the (standardized) index capturing employers’ positive perceptions of unions operating in the workplace (Figure 4 and Table 4, panel B).<sup>17</sup> To get estimates that can be interpreted as probabilities, again we converted the four-answer questions into binary variables, estimating the LATE of the reform for each. Table 4 shows LATE is positive for all four component items of the index. The probability of employers’ agreeing that trade unions play a vital role in representing employees or that they provide a service to employees increases by about 25 percentage points. Employers that have already applied the reform are also about 15 points less likely to say that the unions put their own demands and interests ahead of those of the employees or that they hinder the running of the enterprise (but these latter two effects are not statistically significant). These findings are consistent with the monographs of Yon and Bérout (2013), which show that representativeness based on elections “institutionalized” the bargaining unions and so enhanced their legitimacy in the employers’ eyes.

The reform would also appear to have improved employers’ perceptions of staff representatives by 30% to 40% of a standard deviation.<sup>18</sup> Staff representatives include not only union delegates but also workers’ delegates and members of works councils (who may or may not be union members). The question therefore jointly targets union representatives who have been affected by the reform and other worker representatives, not directly concerned. This may explain why the estimated LATE for this index is smaller than that for the index of employers’ perceptions.

**Employees’ perceptions.** Restricting the analysis to the core sample of workers, we find an effect equal to 23% of a standard deviation on workers’ perceptions of unions in their workplace, although it is not statistically significant (Figure 4(c) and Table 4, panel B). LATE estimated for the questions forming part of the index is usually positive but much smaller than on the employer side, and far from conventional levels of statistical significance.

Expanding the sample to include workplaces not covered in the employers’ survey, the estimated LATE increases to about 30% of a standard deviation (Table 4, panel C).

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<sup>17</sup>Variants of Figure 4 with more bins are shown in Figure C3 in appendix C. When no union is present, employers are still asked to answer the questions, but with reference to unions in general rather than at their workplace.

<sup>18</sup>The questions used to measure these perceptions are detailed in the data Appendix.

As in the case of employers, this result appears to be driven primarily by heightened perceptions on the part of employees that unions play a vital role and provide services to employees; the estimates for both variables are significant.

The results reported here indicate that the introduction of more direct democracy for union recognition has had a positive overall effect on stakeholders' perceptions of unions: the effect is large and positive for employers, and not negative for workers. There is some evidence that the reform had a positive effect on employees' perceptions, but this is based on a larger sample of workplaces for which we only know election dates, and most fixed-term and recently hired workers were not surveyed. Accordingly, this finding must be taken with caution.

### 4.3 Social climate

**Conflict and social climate as reported by employers.** Changing the conditions for union recognition is likely to affect the social climate and the likelihood of labor conflict through various channels. For instance, it could foster cooperation and thus reduce conflict; or conversely, increased union membership and sharper competition between unions might make them more aggressive.

The estimates reported in Table 5, panel A, suggest that the reform produced a deterioration in employers' perceptions of the quality of the social climate by around 30% of a standard deviation, but this effect is not statistically significant.

Consistent with this finding, workplaces where the last staff elections were held under the new regime are more likely to have experienced a work stoppage during the three-year period 2008-2010 (Figure 5 and Table 5 panel A, first row). According to our R.D. estimates based on employers' declarations, the probability of a stoppage doubles from around 25% to 50% due to the reform. This effect, which is statistically significant, appears to be driven entirely by walk-outs rather than strikes (Table 5). This last finding suggests that the reform encourages workers to make their voice heard more, but it does not engender harsher, more official forms of conflict. Most interestingly, this enhanced voice is accompanied by a better perception of unions on the part of employers.

**Conflict and job satisfaction reported by employees.** The workers' question-

naire includes a question on participation in work stoppages but does not distinguish between different types of stoppage. Here too, the restriction of the survey to employees who have worked at the establishment for at least fourteen months may generate overestimation of average participation.

We find that the average rate of participation in a work stoppage increased from around 15% before the reform to between 20% and 25% after it.<sup>19</sup> The R.D. estimates obtained are not statistically significant, either for the core or for the extended sample of workers (Table 5, panel B). But they are consistent with the employer survey, suggesting that the lack of significance may be due to statistical noise more than a real absence of effect. Statistical non-significance could also reflect heterogeneity in the impact of the reform. We explore this issue in the subsequent sub-section.

At this stage, we do not exclude the possibility that the reform may have prompted increased participation in work stoppages.<sup>20</sup>

We also checked the effect of the reform on workers' job satisfaction in general (Table 5) and on their satisfaction regarding various dimensions of their job (pay, training, working conditions, work environment). The estimated effect on these outcomes is practically nil for the core sample of employees and potentially slightly negative for the extended sample.

**Voluntary resignation.** The study of resignations offers additional insight into workplace climate. Our version of the REPOSE survey has been matched by the Ministry of Labor with administrative data on workplace entries and exits, which we use to measure the rate of voluntary resignations at each workplace in 2011 (number of resignations over average number of employees in that year). Table 5 reveals that the workplaces applying the reform regime had a substantially lower resignation rate in 2011 (around 5% against 7%). However, these effects are not statistically significant.<sup>21</sup> Yet while not fully conclusive, our results are consistent with the thesis that direct democracy at the

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<sup>19</sup>The variable here is not directly comparable with the prevalence of work stoppages, as there can be a stoppage that does not involve the entire workforce.

<sup>20</sup>Adjusting p-values for multiple hypotheses and testing the three estimates of stoppages (based respectively on employers' statements and on either core or expanded sample of workers' statements) by means of False Discovery Rate (FDR) controls, we still find the adjusted value (the q-value) relative to employer reports to be nearly 10%.

<sup>21</sup>Actually, we did not expect to find a significant effect. Given the size of the sample, it is impossible to detect anything less than an implausibly great impact.



workplace produces greater worker voice and fewer resignations. That is, the reform may have shifted employees' behavior from the economic entry/exit model to the voice, exit and loyalty model posited by Hirschman (1970). We will return to that interpretation in section 5.

## 4.4 Falsification tests and robustness of RDD estimates

Positive and significant effects of the reform have been found for five main outcomes at workplace level: union recognition, unionization rate, employers' perceptions of unions, employees' perceptions of unions, and work stoppages. This subsection sets out falsification tests and robustness checks. On unionization rates and work stoppages, estimates were obtained from both the employer and the employee parts of the REPOSE11 survey, and robustness checks have been run for both. In addition, the estimated impact of the reform on the social climate at the workplace was close to being statistically significant, so we ran robustness checks for this outcome as well, in order to see how significance changes with alternative empirical specifications. This leaves eight outcomes for which robustness and falsification tests have been conducted.

**Falsification tests.** A first falsification test is the investigation of possible discontinuities in predetermined covariates at the cut-off (Table 2). This test is complemented by the investigation of discontinuities in our eight main outcomes of interest at two placebo cut-offs: 1 January 2010 and 15 April 2009. The first of these is particularly important in allowing us to make sure that the main results are not affected by seasonal factors: that is, that for some reason unknown to us, having a staff election at the beginning rather than the end of a calendar year affects employment relations in a way that could be confounded with the impact of the reform. The results (Table D1 in Appendix) refute this thesis. Most R.D. estimates at the placebo cut-off on 1 January 2010 are close to zero and not statistically significant. An exception involves strikes and work stoppages between 2008 and 2010, for which we find positive and significant conventional and bias-corrected estimates, making the validity of the findings for this outcome questionable.

There is no other obvious placebo cut-off that stands out. We have chosen 15 April 2009 because it falls in a period in which many elections were held and is in the middle

rather than at the beginning of a month (a factor that is unlikely to play any role, but that can nevertheless be checked). For this cut-off, we find only non-significant estimates (Table D2). Note that the coefficients for occurrence of and participation in work stoppages are all negative.

**Donut-hole approach.** We take the “donut-hole” approach, i.e. excluding observations that are close to the cut-off before computing the R.D. estimates. This is an important check in our case, as we cannot entirely rule out the possibility that some elections were slightly delayed around the cut-off, or that some elections in November-December 2008 already applied some of the reform rules. Figure E6 provides biased-corrected R.D. estimates for our eight main outcomes, obtained after excluding 15 to 60 days on each side of the cut-off. The smallest donut-hole excludes workplaces that had elections between 16 December 2008 and 15 January 2009; the largest, those with elections between 1 November 2008 and 1 March 2009.

Compared to the baseline estimate (corresponding to a donut-hole radius of zero in Figure E6), excluding 15 days on each side slightly increases the magnitude of the R.D. estimates and does not alter their statistical significance. When the donut-hole is larger, the point estimates usually increase further but tend to become less precise. With 60 days excluded, the estimated effect of the reform on employers’ perceptions increases to almost one full standard deviation, but the estimate becomes so imprecise that it is no longer significant at the 5% level. For employees’ perceptions too, excluding observations around the cut-off increases the magnitude of the R.D. estimates, but they always remain non-significant. For other outcomes, the donut-hole approach with various radii tends to confirm our main results. In particular the impact of the reform on social climate remains non-significant.

**Varying bandwidth size.** Figure E7 provides conventional R.D. estimates for various bandwidths, defined as the number of days used on *each side* of the cut-off. The smallest bandwidth (200 days) corresponds to just under 7 months on each side of the cut-off; the largest one essentially embraces the entire sample (more than 2 years on each side). The optimal bandwidth is marked by the vertical dashed line. For the smallest bandwidth, the effects of the reform are very imprecisely estimated and usually not statis-

tically significant. Reassuringly, however, they do not differ greatly from those obtained using the optimal bandwidth. For all other bandwidths as well, the estimated R.D.s are usually close to those for the optimal bandwidth and tend to have the same level of statistical significance. Overall, conventional estimates for various bandwidths tend to corroborate the main results.<sup>22</sup>

**Controls and uniform kernel.** Tables E3 and E4 respectively present R.D. estimates for the main outcomes of interest with (1) control variables added to the baseline specification and (2) a uniform instead of triangular kernel. The controls performed in table E3 include the variables used for the balancing checks (see Table 2) and controls for workforce composition. Panel A has controls for the months of 2011 when the employer interviews were conducted, so as to capture any seasonal effect or, in combination with the running variable, the effect of the exact length of time between the last staff election and the REPOSE11 survey. Panel B includes controls for the mean characteristics of the workers surveyed, to make sure that the main results are not driven by respondents' demographics.

Most of the results are robust to the controls and change of kernels, with a few exceptions. First, with the controls the effect of the reform on work stoppages is 30% to 40% smaller and no longer statistically significant. Second, the unionization rates reported by employers and workers also become slightly smaller and only marginally significant. Third, with the uniform rather than triangular kernel the impact on employees' trust is no longer significant for the expanded sample of workers. This confirms that the evidence of an effect on workers' perceptions is only suggestive. Overall, these relatively small differences from baseline are not surprising, since most effects cannot be estimated very precisely.

**To summarize.** These checks confirm that the 2008 reform had a positive impact on employers' trust, union recognition and unionization. The R.D. estimates for these variables are still significant in all the checks. As for work stoppages, employer and

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<sup>22</sup>For the sake of completeness, Figure E8 also provides bias-corrected estimates for various bandwidths. Here again, the results of the baseline specification are confirmed. Note, however, that the bias correction provided by Calonico et al. (2014) is intrinsically related to the choice of bandwidth, so that setting the bandwidth manually impacts on the correction and can produce misleading results. The results depicted in Figure E8 must accordingly be taken with caution.

worker responses are consistent and suggest some impact of the reform, but when controls are included the estimated effect becomes a bit smaller and not significant, and one falsification test is failed. Finally, there is only evocative evidence of an effect of the reform on workers' perceptions of unions. The effect on the workplace climate is not robust.

## 5 Discussion and conclusion

Before concluding, three points in particular warrant special discussion: (i) the average and medium-run effects of the 2008 reform, (ii) the channels through which the reform impacted on employers and employees, and (iii) the thesis that the reform may have induced a partial shift from entry/exit to voice/loyalty employment relations in France.

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

The estimated local average treatment effects of the 2008 reform on union coverage, unionization and employers' and employees' trust in unions are quantitatively large. For unionization, the size of the effect is apparently at odds with the fact that the overall unionization rate did not change during the post-reform period (Pignoni, 2016). As to trust, the estimated local effect of the 2008 reform conflicts with the common proposition that social capital is hard to build in the short run.<sup>23</sup> We accordingly examine possible average and medium-run effects of the reform carefully, to determine whether they can be reconciled with our local estimates.

**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 immediately after the reform, engendering a discrepancy between LATE observed in the first semester of 2011 and average treatment effects.<sup>24</sup> For example, the new regime put unions in competition not only within firms

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<sup>23</sup>A French best-seller published in 2007 (Algan and Cahuc, 2007) suggested that France suffered from a general lack of trust, with a series of detrimental effects on society. Our estimates may also appear to clash with the widespread view that workplace-level employment relations in France are still non-cooperative.

<sup>24</sup>As LATE are observed about two years after the reform, formally we should compare them with the average treatment effect over all workplaces two years after treatment. This is not directly possible, however; what follows is a discussion of possible average treatment effects at different dates relatively

but also industry-wide and nationally. Losing recognition at these higher levels (especially national) could have had dramatic consequences for the major unions and their leaders and staff. Fear of losing their jobs may have prompted substantial extra effort from union employees to organize new firms and get as many votes as possible.

Suggestive evidence on average treatment effects can be gathered first by looking at the firms furthest from the cut-off date on RDD graphs. Of course, these firms are not perfectly comparable, because they held their staff elections at different dates and because the time interval between an election and the moment when the outcomes are measured is itself likely to affect the outcome. This temporal effect is captured by the slopes of the fitted lines in the RDD graphs. Examining Figures 2 to 5, one sees clearly that for the main outcomes the fitted lines have similar slopes to the left and to the right of the cut-off (usually flat or slightly negative). As there is no obvious reason why the distance from the previous staff election should affect treated and untreated workplaces very differently, we should find similar slopes at both sides of the cut-off, *unless* the effect of the reform on the first firms to apply it was much stronger than on later ones (in which case we should find a more sharply negative slope to the right). Hence, the fact that the fitted slopes are similar on the two sides offers evidence that the effects did not fade notably with time.

**Cross-country comparison of cooperation in labor-employer relations during the reform period.** Country-level trust in unions and labor-management cooperation are likely to be driven by a variety of factors, such as the global crisis and other institutional changes that occurred during our period. Nevertheless, if the average treatment effect of the reform is both comparable in magnitude to the LATE estimates and persistent over time, we should be able to find some evidence of it in macro series. And this is in fact the case, as is shown in Figure 6: of the 19 countries selected, France had the strongest increase in cooperation in labor relations between 2007-2008 and 2016-2017, as reported by managers. This is not due to our selection of countries. Among the 122 countries that participated in the World Economic Forum surveys in 2007-2008, 2010-2011 (middle of the application period and the year closest to REPOSE11) and 2016-2017 (four years after all firms had applied the reform), France had the lowest reported degree of cooperation in 2007-08 (behind a number of developing countries and

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close to the reform.

dictatorial regimes), improved to 112th in 2010-11 (having gained 10 places) and 97th in 2016-17 (no longer in the bottom quintile). The absolute changes in declared cooperation in France are the twentieth-greatest increase between 2007-08 and 2010-11 and the sixth-greatest jump over the entire period 2007-08 to 2016-17 (among these 122 countries). This development is certainly consistent with a large and persistent effect of the 2008 reform on labor-management cooperation.

Statistics on trust in unions based on the Eurobarometer and the World Values Survey are also consistent with a positive average effect of the reform on *workers'* trust, at least in the short run. And in fact among the 35 OECD countries, France showed the third-largest increase in overall popular trust in trade unions between 2005 and 2010 (Figure 4.9b in OECD (2017), reproduced as Appendix Figure C4).<sup>25</sup>

**Evolution of unionization in the private and public sectors.** The estimated LATE of around 8 percentage points on the unionization rate (which would amount to almost doubling it) does not gibe with the aggregated statistics provided by the Ministry of Labor, according to which unionization in France has been practically flat since the early 1990s (Pignoni, 2016). These paradoxical results can be reconciled by disaggregating the total unionization rate according to the sectors affected and not affected by the reform. This can be done using the French version of the European Survey of Income and Living Conditions (EU-SILC), the official source for the unionization rate in 2008 and 2010.<sup>26</sup> Unfortunately, until 2008, the question used to measure union membership in the ancestor of the French SILC survey was diluted in a series of questions concerning membership in various associations, resulting in an under-estimation of unionization (Pignoni, 2016). In the absence of alternative data sources, there are no reliable statistics on aggregate union membership prior to 2008. Consistent statistics can be obtained only from that date on.

The results are presented in the first panel of Table 6 on unionization rates from 2008 to 2016 in general government (not touched by the 2008 reform), in private sector workplaces with 10 or fewer employees (not affected) and in private sector workplaces with more than 10 employees (which were affected). Union membership in general gov-

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<sup>25</sup>Unfortunately, the Eurobarometer dropped the specific question on trust in unions after 2010, preventing its use in considering medium-run effects.

<sup>26</sup>The French working conditions survey is now the main source on unionization according to the French Ministry of Labor, thanks to its larger sample, which unlike EU-SILC and REPONSE also includes overseas départements.

ernment declined from 20.3% in 2008 to 17.4% in 2016, according to EU-SILC.<sup>27</sup> This is in patent contrast with the private sector workplaces concerned by the reform, where unionization rose from 9.0% in 2008 to 11.1% in 2016. A similar 2-point increase emerges if the sample is restricted to workers with at least one year’s seniority, in order to match the REPONSE2011 sample used for our RDD estimates. Computing a simple difference-in-differences shows an increment of 1.8 percentage points in union membership at the workplaces concerned by the reform by comparison with general government in the brief period 2008-2010, and of exactly 5 percentage points over the entire period 2008-2016. The change between 2008 and 2010 captures only part of the reform’s impact, as in the first two years practically half the private sector workplaces with more than ten employees had not yet implemented the reform. This may explain why the short-run change is smaller than the longer-run evolution and also much smaller than the LATE estimates (even though it remains within their 95% confidence intervals). The 5-percentage-point difference between the affected and unaffected workplaces in the longer run instead captures both the full effect of the reform on unionization and its persistence in the medium term.

Of course, this simple difference-in-differences could also be capturing other factors with differential impact in the public and private sectors, accounting for the divergent trends. To address this concern, we sought to estimate the unionization rate before the 2008 reform. The only source allowing construction of a series spanning that date is the REPONSE survey itself, which was also conducted in 2005. REPONSE covers only a part of the private sector and thus cannot be compared directly with the estimates obtained from EU-SILC in the private sector or for the whole economy. In addition, the 2005 REPONSE survey covered only workplaces with more than 20 employees. We have sought to make the estimates for 2005 comparable with the other statistics in Table 6 by multiplying the unionization rate in REPONSE 2005 by the ratio between the SILC estimate in 2010 for a sample corresponding to REPONSE11 (11.4%) and the estimate obtained for workplaces with more than 20 employees in the REPONSE11 employer survey. For additional justification of this calibration procedure, as well as alternative adjustments, see appendix B.2. In all cases, union membership in the REPONSE sample diminished between 2005 and 2008 and then turned up with the implementation of the

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<sup>27</sup>The French working conditions survey confirms the erosion of unionization in the public sector from 2013 to 2016 and suggests stability in the private sector.

2008 reform.

This rebound is all the more remarkable considering that unionization appears to have been declining or at best constant since the mid-1970s (Pignoni, 2016) and that in the private sector it is generally pro-cyclical or at most acyclical (see Schnabel (2003) for a review), which suggests that the global crisis from 2007 should actually have affected it negatively. The global crisis may have led, say, to a sharper economic downturn in the more highly unionized manufacturing sector and more difficult market conditions in the private sector, possibly less conducive to unionization. To control at least partially for these changes, we reproduce the trend in the unionization rate from 2008 to 2016 holding the distribution of the characteristics of workers and jobs constant as in 2008. This is done via propensity score reweighting (or “DFL reweighting”) as in Autor et al. (2008), adapting the seminal approach of DiNardo et al. (1996) (see technical details in appendix B.3). The results are affected only slightly, as panel B of Table 6 shows. This implies that the global crisis did not have a major effect on unionization rates and ensures, more in general, that the divergence between public and private sectors is not driven simply by different trends in workforce composition.

One final piece of evidence on the effect of electoral democracy on unionization can be garnered from a comparison between very small and larger private workplaces. Interestingly, given the French constitutional principle of equal citizenship in industry, the 2008 law mandated that workers in firms with 10 or fewer employees where staff elections are not held should nevertheless be counted to gauge the representativeness of unions at industry and national level. Talks between the social partners to devise consensual compliance with this legal requirement failed, so in the fall of 2010 a new law instituted a nationwide vote for workers in these small firms at the end of each four-year electoral cycle (i.e. in December 2012 and then in late 2016). This second law is comparable to the 2008 reform in two ways: it enables workers to participate in determining their representative unions and it gives an incentive to unions to expand their membership in workplaces where there is no official worker representation. Table 6 shows that unionization in small workplaces continued to decline from 2008 to 2010 but turned up in 2012. Again, this pattern is consistent with a positive impact of electoral democracy on unionization. In short, the observed trends in unionization in the sectors affected by electoral democracy at different points in time fully corroborate the idea that the impact on union



membership can be substantial.

## 5.2 Competition as an engine for union performance

In modern democratic regimes, citizens take part in deciding their representatives. Parties and politicians are in competition for public office at many levels (*e.g.*, city, region, nation). Their chances of gaining office through election or appointment at higher levels of representation depend at least indirectly on their performance at lower ones.

The kind of industrial democracy instituted by the French reform of 2008 shares these general features of free elections: workers participate in designating their union representatives, and the different unions compete at all levels. These key features could potentially explain the increase in unionization and in trust induced by the reform. Competition – the essential characteristic of electoral democracy – gives unions the incentive to improve the quality of their representation (intensive margin effect). It attenuates the risk of corruption and shakes the local union oligarchy by introducing an individual electoral condition for the appointment of union delegates. The linkage between local and higher levels of representation, further, encourages unions to field a candidate in every firm, including those where they are unlikely to win or were not present in the past (extensive margin effect). This effect is magnified by the free entry of new competitor unions.

There is additional evidence of such mechanisms. First, unions' electoral results at the industry and national levels are in keeping with the notion that they responded to the incentive created by the introduction of repeated elections. Second, unions are likely to have responded both at the extensive and at the intensive margin, as our RDD results cannot be explained entirely by the extensive margin effect alone (*i.e.*, the simple expansion of coverage). Finally, unions have probably become more inclusive due to the reform. This can be seen by examining the effect of the reform across different groups of workers, which offers insight into the drivers of the intensive margin effects.

**Competition, incentives and the electoral performance of the main unions at higher levels.** The aggregate results of the workplace elections suggest that the unions that had the most to lose or to gain from the introduction of electoral thresholds at the industry and national levels are those whose performance improved the most over time.

The unions with the strongest incentives are clearly those closest to the representation threshold. In each electoral cycle, these unions stand to win or lose representativeness in several industries and at national level. Three unions are in this position: the Christian union CFTC and the white collar union CGC, the smallest of the established unions, and UNSA, the largest non-established union. All of these gained in vote share. In contrast, the third largest union in terms of membership, FO, which has no direct competitor and is representative enough to be comfortably above the threshold, has lost votes since the reform. Similarly, the electoral performance of the two largest French unions, CGT and CFDT, has worsened slightly. Finally, some large non-established unions, such as SUD and UNSA, became representative in several industries, confirming that the 2008 reform removed some entry barriers. Overall, the electoral trends at industry and national levels, which we describe in greater detail in Appendix F, are in line with the thesis that the reform enhanced pluralism and that French trade unions responded positively to the new electoral requirements.

**Extensive versus intensive margin.** The substantial impact of the reform on union coverage indicates that unions did in fact respond at the extensive margin by seeking to organize new workplaces. This is more clearly visible from the LATE estimates of union coverage at workplaces of different sizes (Table 7, panel A): the effect is concentrated among workplaces with at most 100 employees. In these workplaces, the average coverage rate is 39% in our sample, while the estimated LATE is 28%, significant at the 10% level. The respective figures at workplaces with more than 100 employees are 86% and 9% (not significant at conventional levels).

The reform has a statistically significant effect on coverage in the trade and services sector, where the average coverage rate is 62%, but no significant effect in manufacturing and construction, where the rate is 73%. This tends to corroborate the notion that unions were more successful at organizing in workplaces where the initial coverage rate was low.

However, our RDD results are unlikely to be driven entirely by this extensive margin response. They accordingly suggest that unions also responded at the intensive margin by adapting their behavior at workplaces where they were already present before the reform. The observed impact at workplaces that were typically covered by unions (say, those with over 100 workers) is consistent with this argument. We find that in these workplaces there

is a positive effect both on employers' trust and on work stoppages, while no significant effects on our other outcomes are detected. This suggests that the effect on trust in and satisfaction with unions is not driven solely by coverage. A naive calculation of the share of the reform's impact on trust accounted for by the coverage effect points to the same conclusion. Employers in non-covered workplaces have a much worse perception of unions than their counterparts where at least one union is present, with a gap in the trust index of 50% of an s.d.<sup>28</sup> Assuming that this gap reflects a causal impact of union coverage on employers' perceptions (say, because local face-to-face collective bargaining improves employers' priors), we can estimate that the 21-percentage-point increase in union coverage induced by the reform (see Table 3) directly generated an increase of about 10% of a standard deviation ( $0.21 \times 50\%$ ) in employers' perceptions. This is less than a quarter of the total estimated effect on perceptions, again suggesting that the impact of the reform does not stem entirely from the extensive margin.<sup>29</sup>

Similar (non-causal) back-of-the-envelope calculations for other outcomes suggest that the extensive margin may increase workplace-level unionization (measured either by workers' or employers' statements) of around 2.5 percentage points, work stoppages by around 8% of a standard deviation workers' trust by 7% of a standard deviation, and workers' participation in work stoppages by less than 4 percentage points.<sup>30</sup> Despite depending on such a strong assumption, these calculations still suggest that the impact of the reform on union coverage, while substantial, does not entirely determine the effect on other outcomes.

**Change in union behavior and leaders.** The effects of the reform that are not accounted for by the change in coverage may result from qualitative improvement in union representation and services. The most direct indication of this is the increase in

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<sup>28</sup>When no unions are present, the REPONSE survey explicitly asks employers for their opinion of unions in general.

<sup>29</sup>The assumption that the gap reflects a causal impact of the reform is a strong one, and is likely to lead to an overestimation of the portion of the effect on trust that may be explained by the coverage effect. A plausible alternative explanation is that workers are afraid to accept union representation in workplaces that are hostile to unions (see Bourdieu and Breda (2017) for evidence of anti-union discrimination in France). If this kind of selection in fact occurs, the reform may simply have induced unions to organize more hostile workplaces, but without directly inducing a positive effect on trust there.

<sup>30</sup>These calculations depend on the strong assumption that there is a causal relation between union coverage and the raw gaps in outcomes between covered and non-covered workplaces. In reality, the effect could be either smaller or greater than the raw gap, depending on the sign of the selection effects. For most outcomes, intuition suggests that the raw gap is an upper bound of the causal impact, but this thesis cannot be proved.

workers' satisfaction. However, it can be objected that the very fact of being allowed to vote may itself foster workers' participation in unions and hence their satisfaction with them. Unfortunately, we do not have sufficient information to gauge the relative weights of these two explanations.

Considering which workers are most affected by the reform also suggests that the unions made an effort to improve the quality of their presence at the workplace. Theoretically, elections should encourage unions to represent the preferences or interests of the median worker, whereas previously it was in their interest to represent primarily their own members. Union members were more often men, older, higher-seniority, a bit less skilled and in slightly lower-grade jobs than the median (except for the white-collar CGC union). Hence, if unions responded to the incentive for inclusiveness, the magnitude of the observed effects as regards these types of worker should be smaller.

Table 7, panel B, shows (on the largest sample of workers) how the effect of the reform on reported union membership, trust in unions, and participation in work stoppages varies with gender, age, occupation and education. For all three outcomes, the effects are larger among women than men and among younger than older workers. The estimates are rarely statistically different from one another (or from zero), but they do point consistently to this conclusion, thus constituting suggestive evidence of an inclusive behavioral response on the part of unions. The heterogeneity of impacts according to job and skill levels is less straightforward. It could be that managerial employees are harder to organize and so responded less despite unions' efforts. As regards education, workers without post-secondary education appear to have become more unionized following the reform, while the university-educated strongly increased their trust in unions. This last result might be a sign of the pure effect of free elections, as more highly educated workers are known to favor more democratic systems more strongly, in general.

A second qualitative change that might be driven by the reform is the emergence of new generation of delegates (in the historical unions). Because of the personal data protection regulation, MARS does not include individual information. However, when present, workers' representatives are interviewed in the REPOSE survey. It covers only one representative for each establishment, and this representative belongs to the list which attracted most votes during the last professional elections. Mechanically, the vast majority of these representatives are linked to the 5 historical unions. We know

their age, gender, date of the first mandate, and if they are union delegates. Whatever the specifications (not reported), we find no evidence that the union delegates in the treated establishments are more female or younger. When correcting for the fact that elections in the treated group occurred later than in non-treated establishments, there is no significant differences in the date of the first mandate of the union delegates. Following the debate on the role of union leaders (e.g. Voss and Sherman (2000)), these limited evidences suggest that union may innovate when institutional incentives change, even if the same local oligarchy remains.

### 5.3 Exit, voice and loyalty

Industrial disputes are often taken as a sign of poor labor relations. However, the observed effect of the 2008 reform suggests that this association can be misleading. For if the number of labor conflicts increased, then the reform's positive impact on employers' opinion of unions may be associated a greater worker voice, expressed through less militant forms of industrial action. This is particularly clear in manufacturing and construction, where mobilizations are more common than in services (Table 7, panel A): our estimates suggest a significant increase both in the number of work stoppages and in the number of workers taking part in them.

These results may be interpreted in A.O. Hirschman's classic Exit/Voice/Loyalty framework of (Hirschman (1970)). Since workers too tend to trust unions more in the wake of the reform, their loyalty to them and to the firm is likely to increase. Eventually, they come to have a greater voice. The impact of the 2008 reform on resignations (see Table 5) is consistent with this interpretation.

From this standpoint, the reform may not only have delivered representative democracy at work but also a form of liberal democracy with multiple and new unions present at workplaces while trust in union representatives improved and freedom of expression was secured. This liberal democracy would seem to have induced classical industrial democracy as well: workers are less afraid to voice their concerns.

### 5.4 General conclusion

The reform that instituted electoral requirements for the designation of French union representatives in 2008 was implemented gradually by firms, owing to the exogenous

calendar of works council elections. Exploiting this feature of the reform, we identify its effects on employment relations using a survey of employers and employees conducted in the middle of the implementation period. This results in a rare micro-level evaluation of changes in the regulations governing employment relations (by contrast, there is an abundant cross-country comparative literature).

We find that the introduction of electoral democracy substantially increased union coverage and membership, employers' trust in unions, and to some extent workers' own trust in unions. The reform also induced more labor conflicts (work stoppages or strikes) and greater workers' participation in them, suggesting that it helped workers to voice previously unspoken demands. These effects can be driven by both supply and demand, which by definition are jointly affected by electoral democracy: allowing workers to vote for their representatives may be sufficient to foster demand for and involvement in unions; at the same time, elections introduce competition for the supply of union services at workplace level and create an incentive for each union to win as many votes as possible.

Taken together, our results also suggest that the reform induced better, more inclusive representation of workers and a shift towards employment relations governed by voice and loyalty rather than entry and exit. Interestingly, this change was accompanied by an increase in employers' trust in unions, which they viewed as more representative and more legitimate bargaining partners, even if we have no evidences of the emergence of a new generation of local union leaders in historical unions.

In positive terms, the substantial effects revealed by this evaluation show that changing a historically rooted employment relations system is possible even in the short run, and even in a country characterized by conflictual labor relations. Consistently, recent anecdotal evidence suggests a quick and effective bargaining process at the workplace or firm level in the private sector for adapting work organization and compensation packages to the Covid-19 crisis, the conflict at Amazon being more the exception than the rule today in France.

Normatively, can we conclude that the 2008 reform was a success and that democracy at work is desirable? If one judges unions by the satisfaction of the workers they represent and the employers they bargain with, the answer is clearly in the affirmative. If instead one wishes to judge unions by their effect on economic performance, this study does not allow any conclusion, and further research would be needed. We believe that even

without knowing how the reform has affected economic performance, the effect on stakeholders' satisfaction is sufficient to conclude that repeated free workplace elections for union representation is an interesting model that other countries may want to consider.

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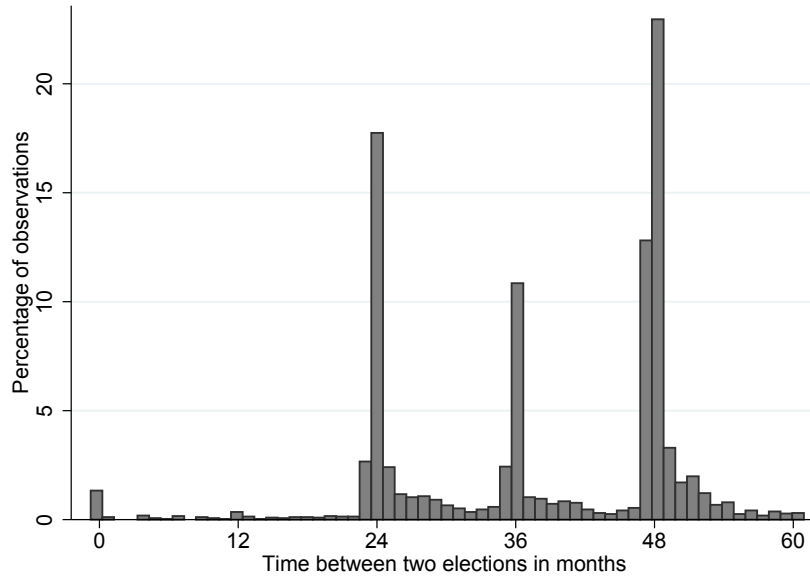
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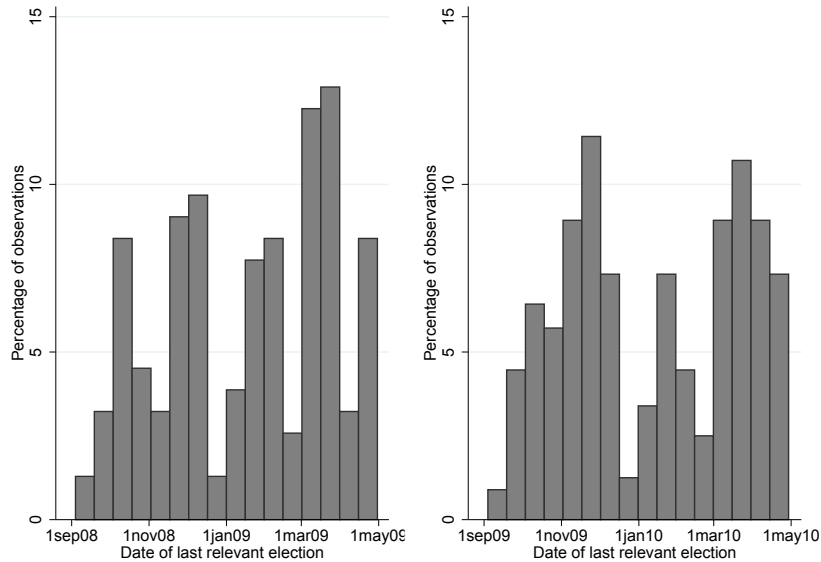
# Figures and tables

**Figure 1: Election dates**

(a) Number of months between two consecutive elections



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

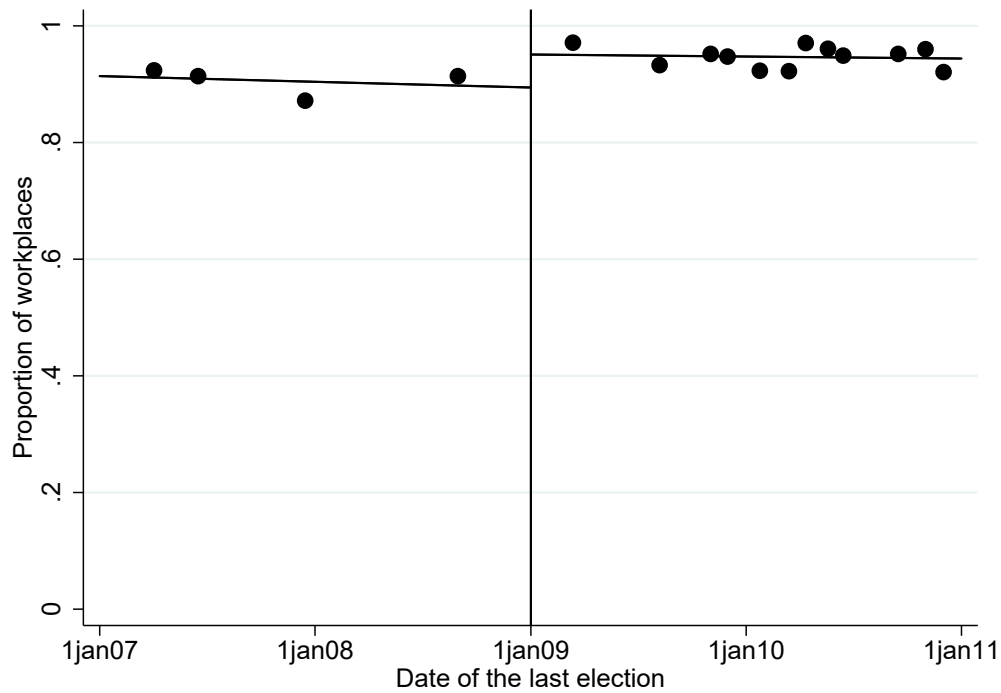


*Source:* Panel (a): MARS dataset, only establishment present in REPOSE11 which have registered an election during the period 2009-2012. Workplaces with more than 62 months between two consecutive elections are excluded. Panel (b): Our own computations from the MARS administrative dataset matched with REPOSE11 (see Appendix B).

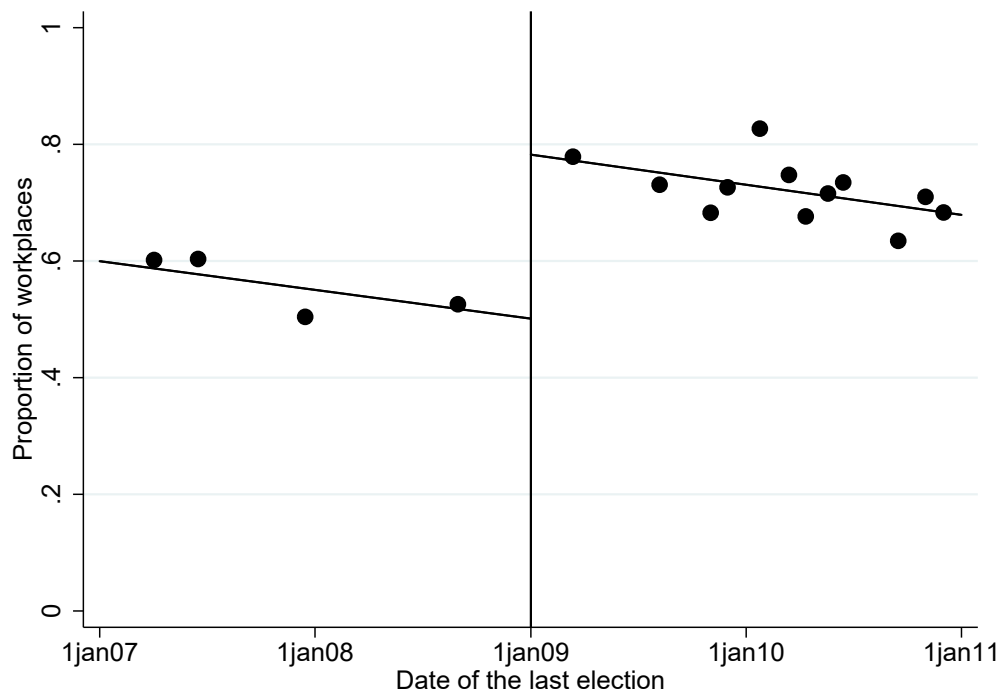
*Notes:* Panel (a): The figure represents the distribution of the length of time (in months) between all elections registered during the period 2009-2012 and the declared date of the preceding election. Partial elections have been removed. Panel (b): The figure represents the distribution of dates for the latest professional election before the REPOSE survey was done in early 2011. Workplaces younger than five years or having professional elections every two years are excluded. The distribution is shown around the application date of the 2008 reform (1<sup>st</sup> January 2009) and around the same date one year latter. See Figure B1 for the distribution over a larger time window.

**Figure 2:** 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



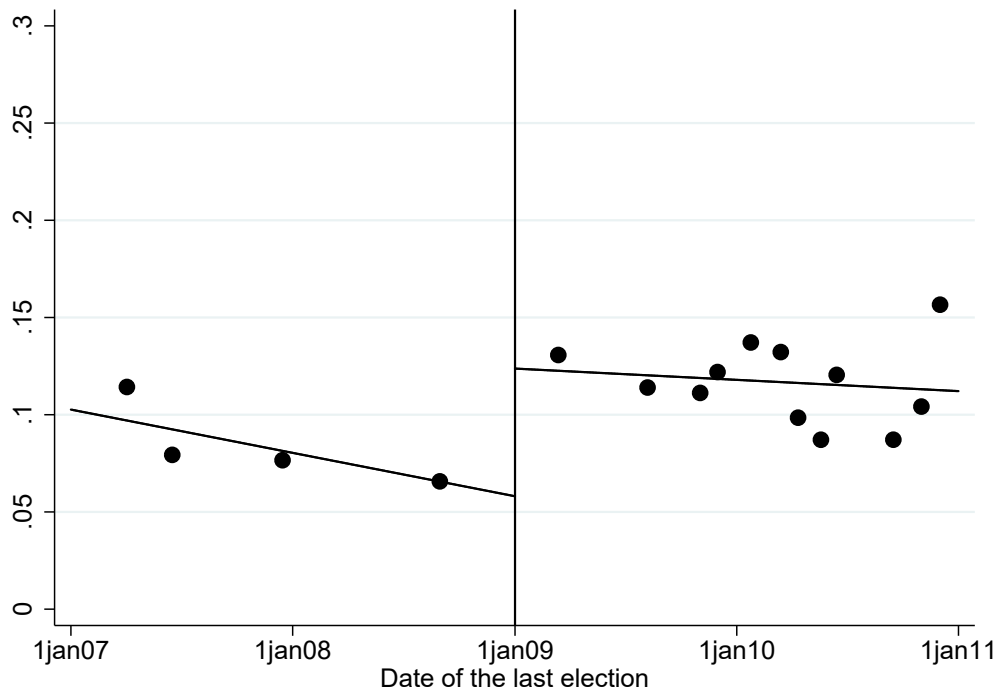
(b) At least one union recognized for bargaining



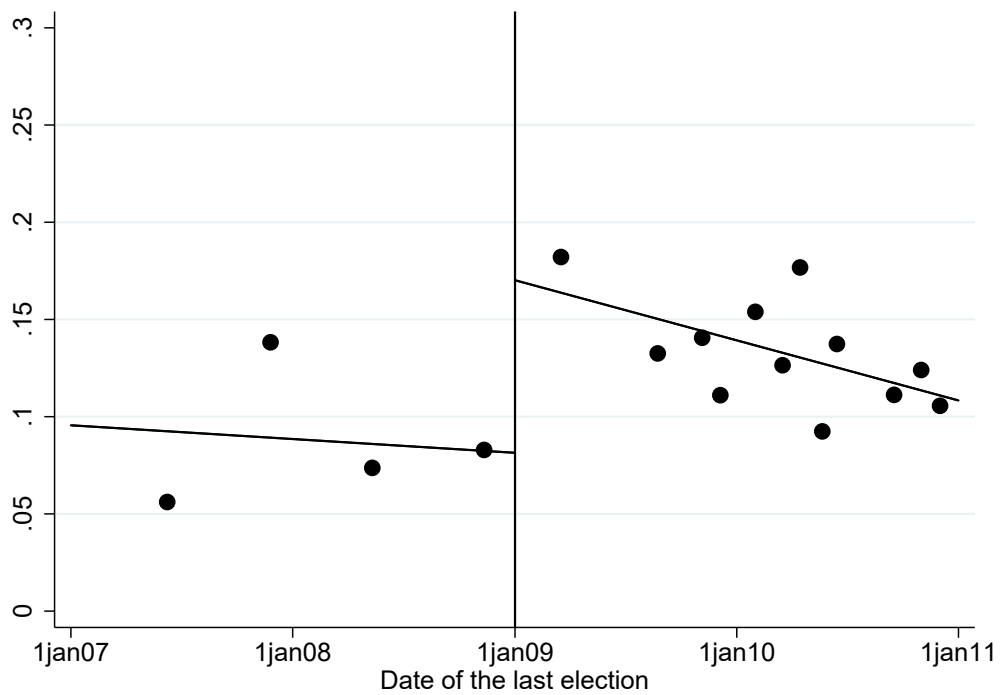
*Notes:* Each bin provides the mean of the interest variable for establishments experiencing their last professional elections around the date of the bin; observations are split in 4 equal-size groups at the left of the cutoff date, and 12 equal-sized bins at the right of this cutoff. Lines represent 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 B).

**Figure 3:** 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)

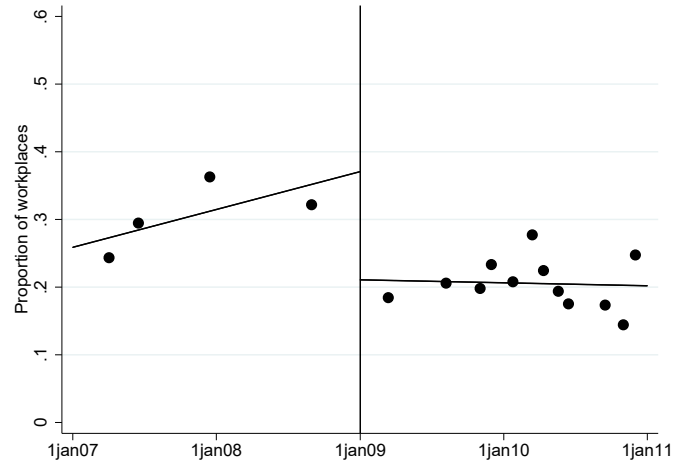


*Notes:* See Figure 2. The answers of individual workers are averaged by workplace; only workplaces for which an employer has been also surveyed are included (core sample).

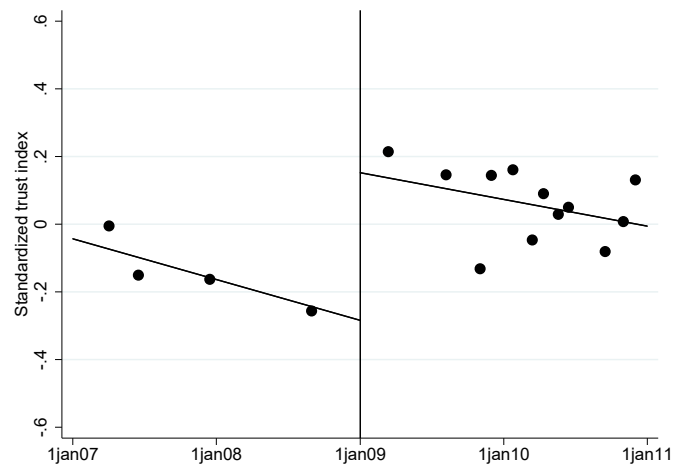
*Source:* See Figure 2.

**Figure 4:** Impact of having a professional election under the new legal regime on employers' and employees' perceptions of unions

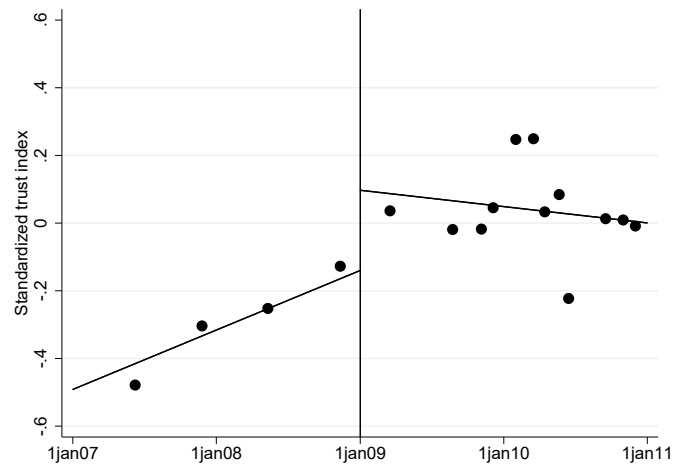
(a) Employer perceives unions representativeness as very weak



(b) Employers' trust in unions in their workplace



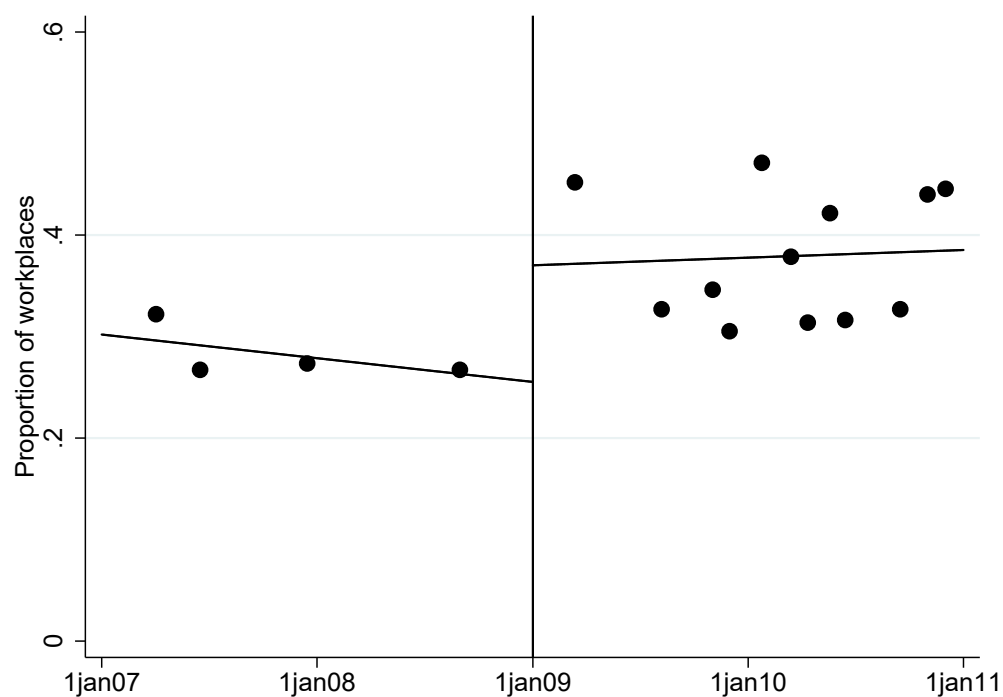
(c) Employees' trust in unions in their workplace (workplace average)



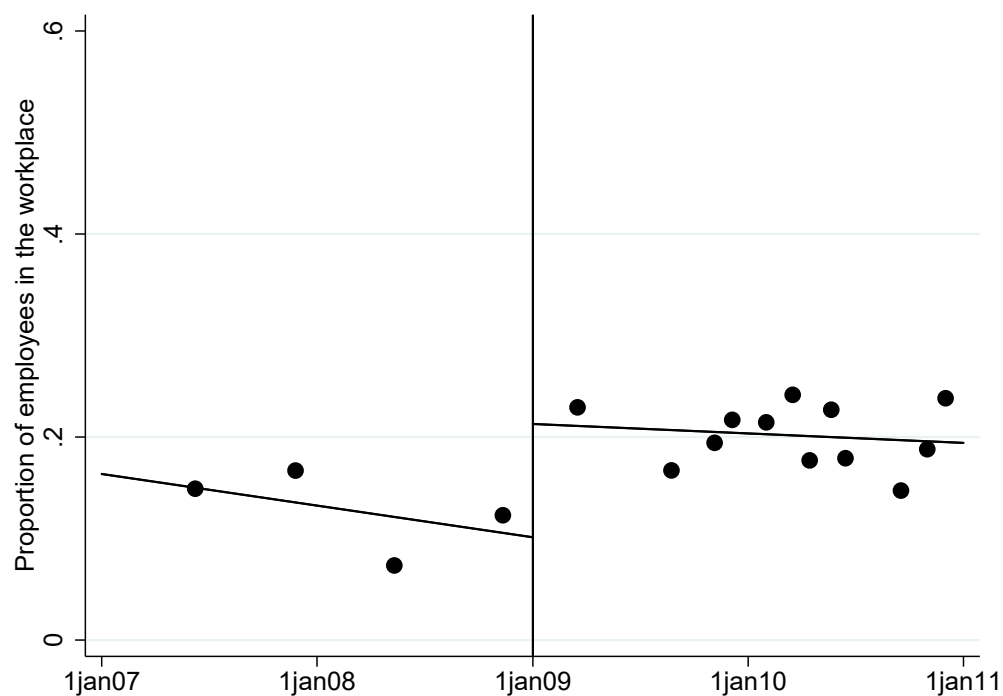
*Notes and source:* see Figure 2. The answers of individual workers are averaged by workplace; only workplaces for which an employer has been also surveyed are included (core sample).

**Figure 5:** 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

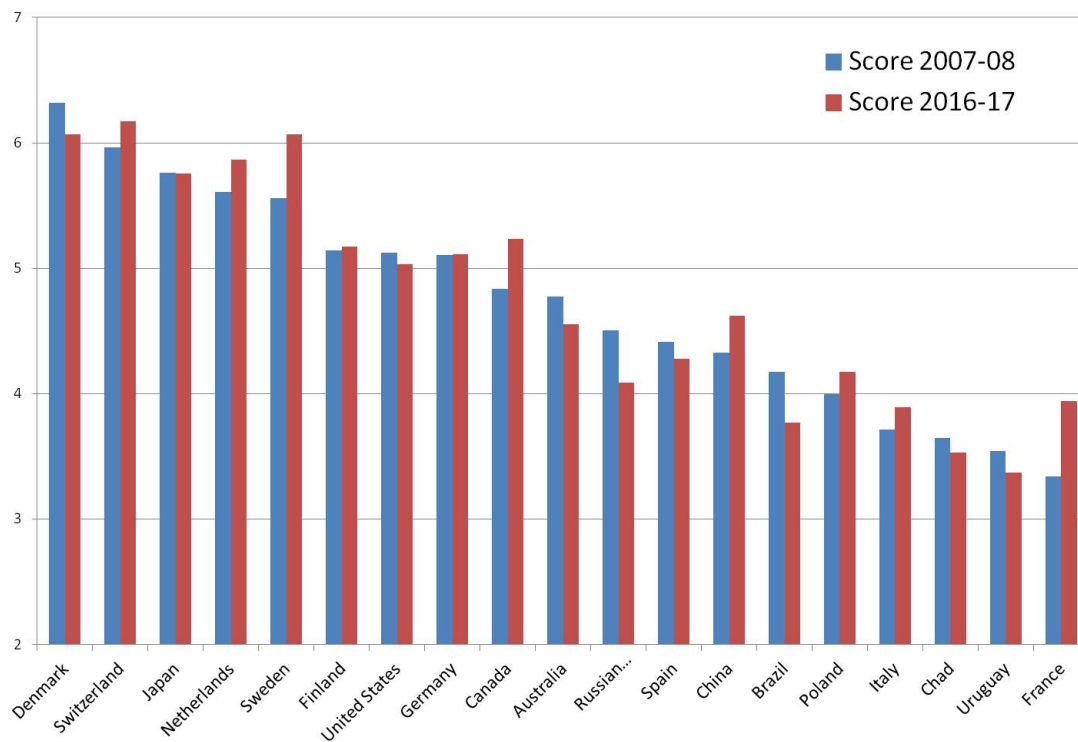


(b) Employees declaring they have participated to a strike or work stoppage between 2008 and 2010 (workplace average)



*Notes and source:* see Figure 2. The answers of individual workers are averaged by workplace; only workplaces for which an employer has been also surveyed are included (core sample).

**Figure 6:** 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.



**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	0.355	765
Construction	1911	0.054	0.045	0.197	701
Trade	1911	0.161	0.057	0.344	257
Market services	1911	0.320	-0.094	0.309	919
Non-market services	1911	0.173	-0.115	0.229	278
<i>Workplace size groups (in december 2008)</i>					
10-49 employees	1911	0.230	-0.050	0.677	707
50-199 employees	1911	0.375	-0.109	0.351	580
200-999 employees	1911	0.341	0.153	0.103	851
More than 1000 employees	1911	0.054	-0.011	0.856	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	483
Paris region	1911	0.193	-0.053	0.460	433
Belongs to single-plant firm	1911	0.394	-0.084	0.435	553
Professional elections every 3 years	1911	0.216	0.114	0.345	339
Professional elections every 4 years	1911	0.689	-0.003	0.868	399
Interviewed manager is a woman	1911	0.396	0.069	0.359	999

*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	0.876	0.090 (0.055)	0.106* (0.064)	1911	919
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	0.186** (0.086)	0.163 (0.100)	1911	909
from "new" unions only	0.109	0.087	0.098 (0.072)	0.115 (0.084)	1911	346
$\geq 2$ unions recognized	0.440	0.391	0.118 (0.109)	0.114 (0.130)	1911	297
2 or 3 unions recognized	0.291	0.256	0.092 (0.090)	0.102 (0.109)	1911	569
5 unions or more recognized	0.058	0.102	-0.034 (0.052)	-0.039 (0.060)	1911	399
<b><i>Panel B: unionization rate in the workplace</i></b>						
Unionization rate (declared by employer)	0.106	0.056	0.078*** (0.029)	0.084** (0.037)	1629	525
Share of workers union members (core sample of workers)	0.121	0.085	0.099** (0.042)	0.097* (0.052)	1586	657
Share of workers union members (larger sample of workers)	0.128	0.082	0.128*** (0.035)	0.143*** (0.041)	3042	940

*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	-0.199** (0.096)	-0.219* (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	523
- 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	0.126 (0.095)	0.150 (0.113)	1858	547
Trust in workers delegate index	0.000	0.050	0.335* (0.192)	0.403* (0.230)	1862	462
<b><i>Panel B: Workers' perceptions (core sample of workers)</i></b>						
Trust in unions index	0.000	0.092	0.233 (0.268)	0.229 (0.328)	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 (0.078)	0.106 (0.096)	1531	301
- 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 (0.099)	-0.007 (0.118)	1510	224
Trust in workers delegate index	0.000	0.562	0.008 (0.289)	0.082 (0.344)	1427	176
<b><i>Panel C: Workers' perceptions (larger sample of workers)</i></b>						
Trust in unions index	0.000	-0.002	0.275 (0.168)	0.319 (0.194)	2784	621
- Unions play a vital role	0.646	0.616	0.180*** (0.068)	0.210*** (0.079)	2938	406
- Unions provide a service	0.702	0.678	0.137** (0.060)	0.155** (0.071)	2946	555
- Unions interests not put ahead	0.469	0.503	-0.003 (0.057)	0.004 (0.067)	2892	1149
- Unions don't hinder running of firm	0.711	0.748	-0.006 (0.054)	-0.003 (0.065)	2883	923
Trust in workers delegate index	0.000	0.343	0.008 (0.193)	0.058 (0.225)	2717	357

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	586
- Intermittent strike	0.030	0.020	-0.016 (0.020)	-0.017 (0.022)	1911	399
- Strike of 1 day or less	0.213	0.169	0.094 (0.078)	0.121 (0.091)	1911	652
- Walkout	0.251	0.054	0.323*** (0.092)	0.361*** (0.101)	1911	282
Social climate	0.000	0.097	-0.290 (0.192)	-0.310 (0.229)	1910	453
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)	0.190	0.173	0.045 (0.048)	0.050 (0.057)	3020	964
Job satisfaction index	0.000	0.155	-0.108 (0.159)	-0.138 (0.191)	3033	668

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%	11.3%	11.2%	<i>Partly</i>
Public sector		20.3%	19.5%	19.3%	17.4%	<i>No</i>
Private sector		7.1%	7.5%	8.3%	8.8%	<i>Partly</i>
Private sector, workplaces with 10 employees or less		3.8%	2.8%	3.3%	3.9%	<i>Partly after 2012**</i>
Private sector, workplaces with more than 10 employees		9.0%	10.0%	10.5%	11.1%	<i>Yes</i>
Private sector, same sample as for RDD estimates	12.2%	9.7%	11.4%	11.7%	12.9%	<i>Yes</i>
<i>Panel B: estimates adjusted by DFL reweighting to maintain workforce characteristics at their 2008 level</i>						
All employees		10.6%	11.3%	10.7%	10.9%	<i>Partly</i>
Public sector		20.3%	20.7%	18.9%	17.0%	<i>No</i>
Private sector		7.1%	8.2%	7.8%	8.3%	<i>Partly</i>
Private sector, workplaces with 10 employees or less		3.8%	3.2%	3.5%	3.6%	<i>Partly after 2012**</i>
Private sector, workplaces with more than 10 employees		9.0%	10.0%	10.4%	11.1%	<i>Yes</i>
Private sector, same sample as for RDD estimates		9.7%	11.2%	11.4%	12.2%	<i>Yes</i>

*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 B.3.

\* 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 B.3).

\*\* 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* (0.163)	0.176*** (0.046)	0.522 (0.437)	0.484 (0.336)	-0.227* (0.136)	0.067 (0.071)
more than 100 employees	0.091 (0.042)	0.104 (0.057)	0.377* (0.178)	0.173 (0.325)	0.284* (0.165)	0.023 (0.064)
<i>Sector</i>						
Trade and other Services	0.241** (0.113)	0.117* (0.066)	0.452* (0.240)	0.024 (0.284)	0.150 (0.107)	0.001 (0.062)
Manufacturing and construction	0.180 (0.192)	0.103 (0.098)	0.553 (0.479)	0.970** (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	0.112	0.115** (0.048)	0.071	0.142 (0.208)	0.166	0.100 (0.077)
Men	0.133	0.084* (0.048)	-0.081	0.187 (0.168)	0.201	0.054 (0.055)
Age below median	0.101	0.126*** (0.048)	-0.071	0.250 (0.164)	0.179	0.088 (0.055)
Age above median	0.156	0.081 (0.059)	0.044	0.031 (0.151)	0.207	-0.009 (0.062)
Non managers	0.148	0.136** (0.058)	0.056	0.335** (0.155)	0.221	0.077 (0.053)
Managers	0.105	0.075 (0.057)	-0.271	0.181 (0.229)	0.155	0.038 (0.063)
Non tertiary Education	0.147	0.169*** (0.047)	0.040	0.132 (0.161)	0.217	0.005 (0.063)
Tertiary Education	0.094	0.026 (0.047)	-0.083	0.448** (0.204)	0.150	0.039 (0.060)

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.

Appendix to

# Electoral Democracy at Work

Philippe Askenazy and Thomas Breda

December 2020

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# Appendix A Detailed institutional Settings

## A.1 Institutions before and after the 2008 reform

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 workplaces 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>A.1</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>A.2</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". These elections occur every four years, unless an industry-level or a firm-level agreement reduces this frequency to three or two years.<sup>A.3</sup> 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

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<sup>A.1</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>A.2</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>A.3</sup>Industry- or firm-level agreements changing the frequency of professional elections cannot apply to ongoing mandates which cannot be reduced by such agreements (in which case, our identification strategy would not be valid).

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 (detailed description).** 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>A.4</sup> When there are *union 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>A.5</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>A.6</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>A.7</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

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<sup>A.4</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.

<sup>A.5</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>A.6</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>A.7</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.

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 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>A.8</sup>

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<sup>A.8</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.

## A.2 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).

Even if all actors coordinated for manipulating the election dates, 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.

## Appendix B Data constructions

### B.1 Outcomes of interest

This section details the construction of the main outcome variables from the REPONSE dataset. The description of the questions is based on a translation in English of the REPONSE questionnaires made jointly by a team of British and French researchers and professional editors.<sup>A.9</sup>

#### Two measures of union membership

*Unionization rate.* Employers were asked “In your estimation, roughly what proportion (%) of employees are union members in your Establishment/Firm”. If the employer did not give a number, the interviewer asked: “Would that be: Less than 5%; 5 to 10%; 11 to 20%; more than 20%; don’t know, does not want to say?”

We thus have access to two types of information, a percentage or a bracket. Two out of three employers answered a percentage. To build a unique variable, when the employer provided a bracket, we assign to her workplace the mean of the union membership over employers who gave a percentage in the same bracket.

We checked that estimations of the impact of the reform on union membership using the sample restricted to workplaces where employers were able to give the exact proportion of union members are comparable: estimates in this case are actually slightly higher; and coefficients are still statistically significant at the 5% threshold.

*Share of workers union members.* A second source consists in the union membership status declared by the workers surveyed in 2011 for REPONSE. These workers were already in the same workplace 31 December 2009. The question was Do you belong to a trade union? Yes; No, I never have; No, but I used to. We averaged their answers at the workplace level to build the variable.

#### Elected representative and unions recognized

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<sup>A.9</sup>See <https://www.niesr.ac.uk/projects/employment-relations-britain-and-france>

*Presence of workers' delegates or work council.* The employer is asked "What elected workforce representation bodies are present at the moment:

- Workforce delegates Yes/no
- Single staff delegation (*Délégation unique*) Yes/no
- Work council Yes/no"

If the employer answered yes to one these three sub-questions, the variable takes the value 1, otherwise 0.

*Number of union recognized for bargaining.* The variable is based on the information from 3 questions. The employer is first asked if there is any trade union delegate. If she answered no, we assign the value 0. If she answered yes, the next questions give an exact count of the number of union with a delegate so recognized for bargaining. The interviewer asked first "Which trade unions are represented by a trade union delegate: CFDT Yes/No; FCE-CGC Yes/No; CFTC Yes/No; CGT Yes/No; CGT-FO Yes/No; Solidaires Yes/No; Unsa Yes/No; Other trade unions Yes/No". If she answered yes to the last sub-question, the interviewer asked "How many other trade unions are represented by a trade union delegate".

## **Perceptions of unions**

*Trust in union index.* Employers are asked: "In connection with trade unions, what do you think of the following statements? (If there are not trade unions in the establishment/enterprise: Give us your opinion of trade unions in general terms)

- 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 question is formulated almost similarly for workers ("What is your opinion of the following statements? (If there is no trade union within your establishment, please state

your general opinion)”) and the four statements are exactly identical to those provided to employers and listed above.

For both employers and employees, the responses are on a 4-point Likert scale from Completely agree to Completely disagree, with also the possibility to answer “Don’t know”.

The four different questions are combined into a single trust 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.

To get estimates that can be interpreted as probabilities, we have also constructed binary variables—somewhat disagree/completely disagree (0) versus completely agree/somewhat agree (1)—to summarize each of the four-answer questions asked to employers and workers.

*Union representativeness is very weak.* Employer were asked “In general terms and in your opinion, how representative are the following at present: very weak; weak; strong; very strong; don’t know”. Excluding “don’t know” observations, the variable is coded 1 if “very weak”, 0 otherwise.

*Trust in workers delegate index.* Surveyed workers were asked: “What is your opinion of the following statements? (If there are no staff representatives within your establishment, please state your general opinion)

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

The four different questions are combined into a single trust 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.



## **Social climate, work stoppages and job satisfaction**

*Social climate.* Employers were asked: “Would you say that the employee relations climate at the moment in your establishment/enterprise is?” The responses are on a 4-point Likert scale from Tense to Calm, with also the possibility to answer “Don’t know”. The index is then standardized to have a mean of 0 and a standard deviation of 1.

*Work stoppage.* This variable is captured via the question to the employers: “Which of the following forms of dispute has your establishment/enterprise experienced in the last 3 years (2008, 2009, 2010)?

- A walk-out
- Strike of less than two days
- Strike of two days or more
- Intermittent strike/Go-slow”

Note that for this specific question of the face-to-face interview, the employers could not answer “don’t know”.

*Participation to a work stoppage.* This binary variable is captured via workers’ answers to “Over the past three years, have you taken part in a work stoppage (strike, walk-out)?”.

*Job satisfaction index.* Workers were asked: “How do you feel about your job in general?” The responses are on a 4-point Likert scale: Not at all satisfied /Not very satisfied/Quite satisfied /Very satisfied. The index is then standardized to have a mean of 0 and a standard deviation of 1.

## **B.2 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.10</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.11</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 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 REPONSE survey.

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<sup>A.10</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.11</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.

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 REPONSE 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.12</sup>

The algorithm to determine the date of the last relevant election date before a worker has filled the REPONSE11 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>nd</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.

Figure B1 plots the distribution the dates of the latest election before the REPONSE11 survey for the full sample. It shows 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 REPONSE11 in 2010 than 2007 or early 2008. Second, as the REPONSE 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 B1 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>A.13</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

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<sup>A.12</sup>This last imputation has no impact on our results.

<sup>A.13</sup>These regulations did not change the length of ongoing mandates and only applied to subsequent mandates.

Figure B1, 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.

### B.3 Construction of consistent time series of unionization rate

#### 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  $\pm 0.2$  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.14</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.

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<sup>A.14</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.

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

**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.

**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:

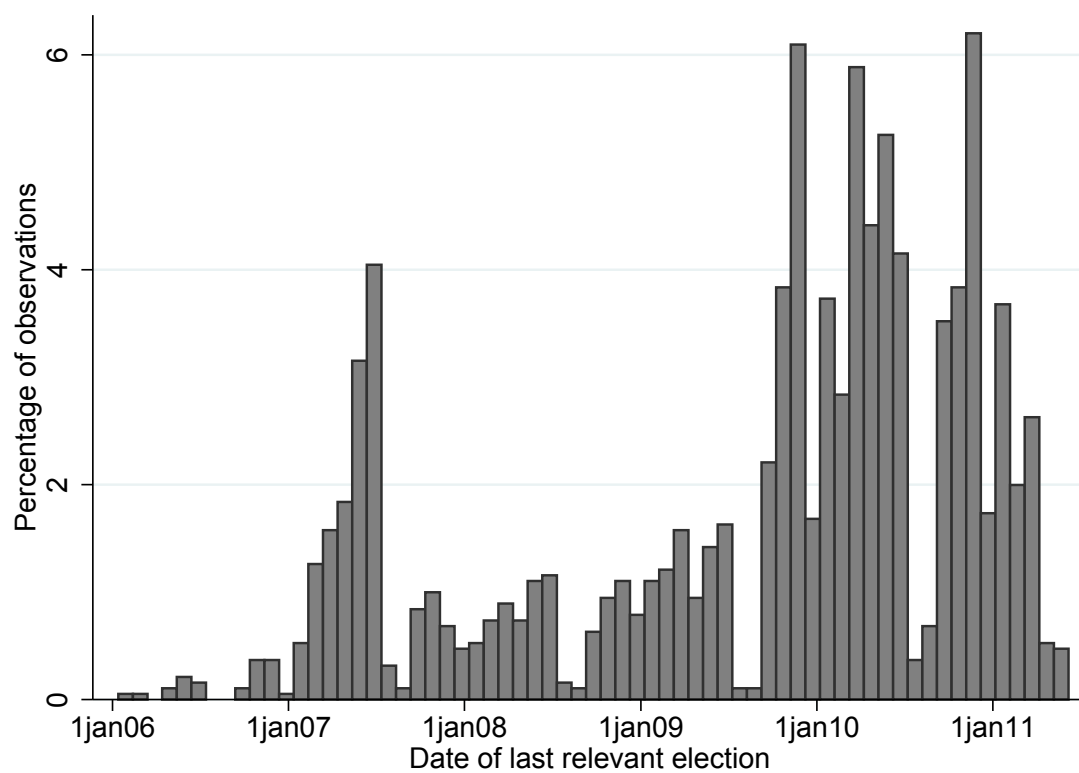
$$w_{it} = sw_{it} \text{ if } t = 2008$$

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

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.

**Figure B1:** Distribution of the date of most recent election before the RE-PONSE employer survey



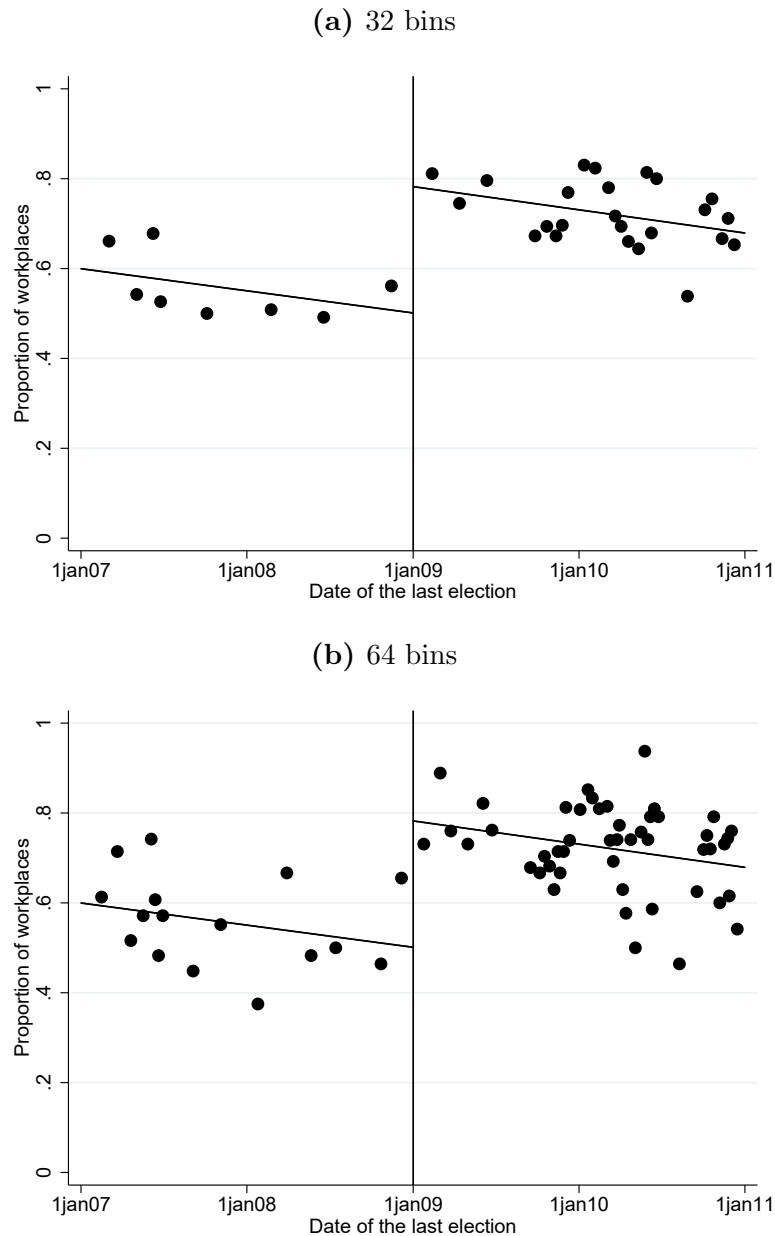
*Notes:* The figure represents the distribution of dates for the latest professional election before the RE-PONSE survey was done in early 2011. Workplaces younger than five years or having professional elections every two years are excluded.

*Source:* Our own computations from the MARS administrative dataset matched with RE-PONSE11



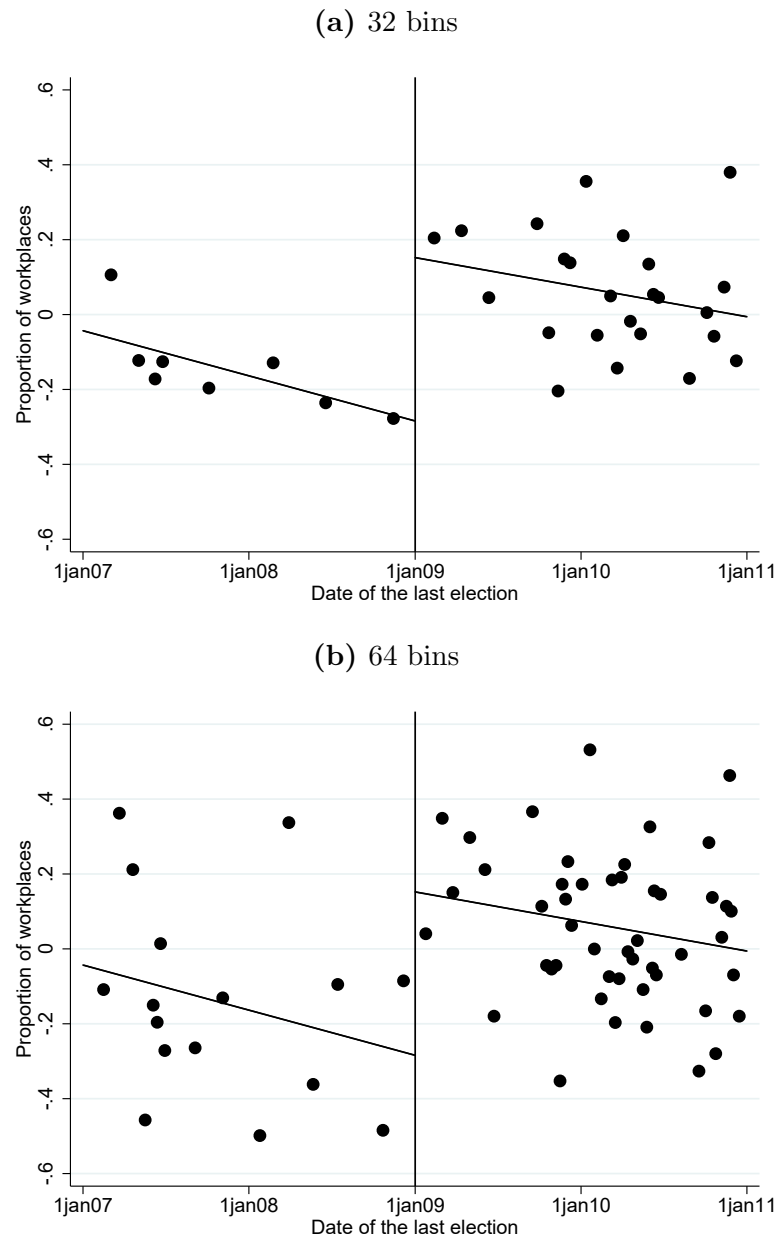
## Appendix C Additional Figures

**Figure C2:** Impact of having a professional election under the new legal regime on union coverage in 2011: graphs with more bins



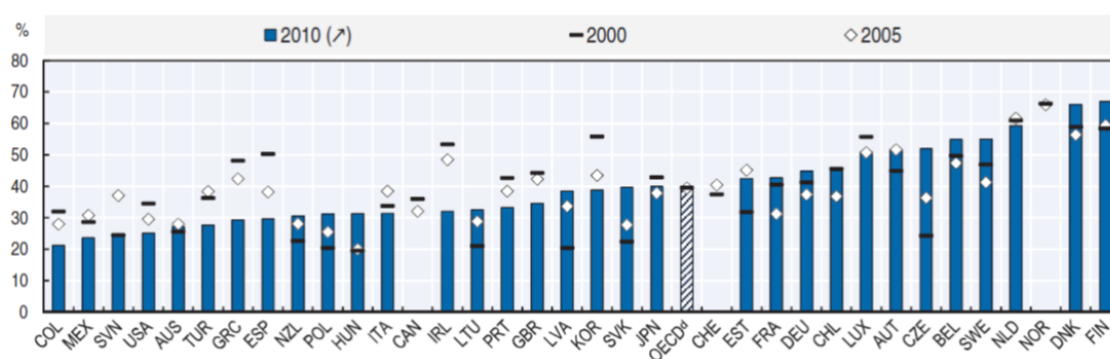
*Notes:* Union coverage is a workplace-level variable for having at least one union recognized for bargaining in the workplace. Observations are split in 8 (pane A) or 16 (panel B) equal-size groups at the left of the cutoff date, and 24 (panel A) or 48 (panel B) equal-sized bins at the right of this 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 C3:** Impact of having a professional election under the new legal regime on employers' perception of unions in 2011: graphs with more 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 C4: 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 D Falsification tests

**Table D1:** 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	0.106	0.130	0.018 (0.040)	0.013 (0.049)	1629	539
Work stoppage (any kind) (between 2008 and 2010)	0.343	0.293	0.190** (0.083)	0.205** (0.099)	1911	777
Social climate	0.000	-0.041	-0.041 (0.183)	-0.044 (0.216)	1910	774
<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	731
Trust in unions index	0.000	0.043	0.270 (0.186)	0.299 (0.224)	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	0.098	0.044 (0.036)	0.041 (0.044)	3042	1102
Trust in unions index	-0.000	-0.111	0.177 (0.168)	0.139 (0.208)	2784	988
Participation to a work stoppage	0.190	0.197	0.041 (0.038)	0.043 (0.046)	3020	1453

*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 D2:** 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	0.839	-0.059 (0.084)	-0.073 (0.099)	1911	589
Trust in unions index	0.000	0.314	-0.108 (0.196)	-0.156 (0.229)	1782	519
Unionization rate (declared by employer)	0.106	0.121	0.027 (0.030)	0.023 (0.035)	1629	587
Work stoppage (any kind) (between 2008 and 2010)	0.343	0.508	-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	0.035	-0.127 (0.219)	-0.080 (0.251)	1453	369
Participation to a work stoppage	0.178	0.251	-0.048 (0.075)	-0.048 (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	0.213	-0.031 (0.044)	-0.032 (0.051)	3042	1002
Trust in unions index	-0.000	0.145	-0.042 (0.188)	0.027 (0.226)	2784	860
Participation to a work stoppage	0.190	0.231	-0.026 (0.051)	-0.029 (0.061)	3020	1056

*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>th</sup> 2009. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . See notes of Table D1 for more details on the implementation of the RDD.

## Appendix E Robustness checks

**Table E3:** 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)	0.175* (0.089)	1898	577
Trust in unions index	0.000	-0.259	0.403** (0.196)	0.421* (0.232)	1770	431
Unionization rate	0.106	0.056	0.045* (0.027)	0.048 (0.033)	1617	501
Work stoppage (any kind) (between 2008 and 2010)	0.343	0.232	0.138 (0.093)	0.168 (0.102)	1898	314
Social climate	0.000	0.096	-0.261 (0.169)	-0.288 (0.196)	1897	491
<b><i>Panel B: Workers' main outcomes (core sample of workers)</i></b>						
Share of workers union members	0.121	0.087	0.088* (0.052)	0.085 (0.060)	1584	246
Trust in unions index	0.000	0.296	0.023 (0.220)	0.045 (0.259)	1452	172
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. (2019)). 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.

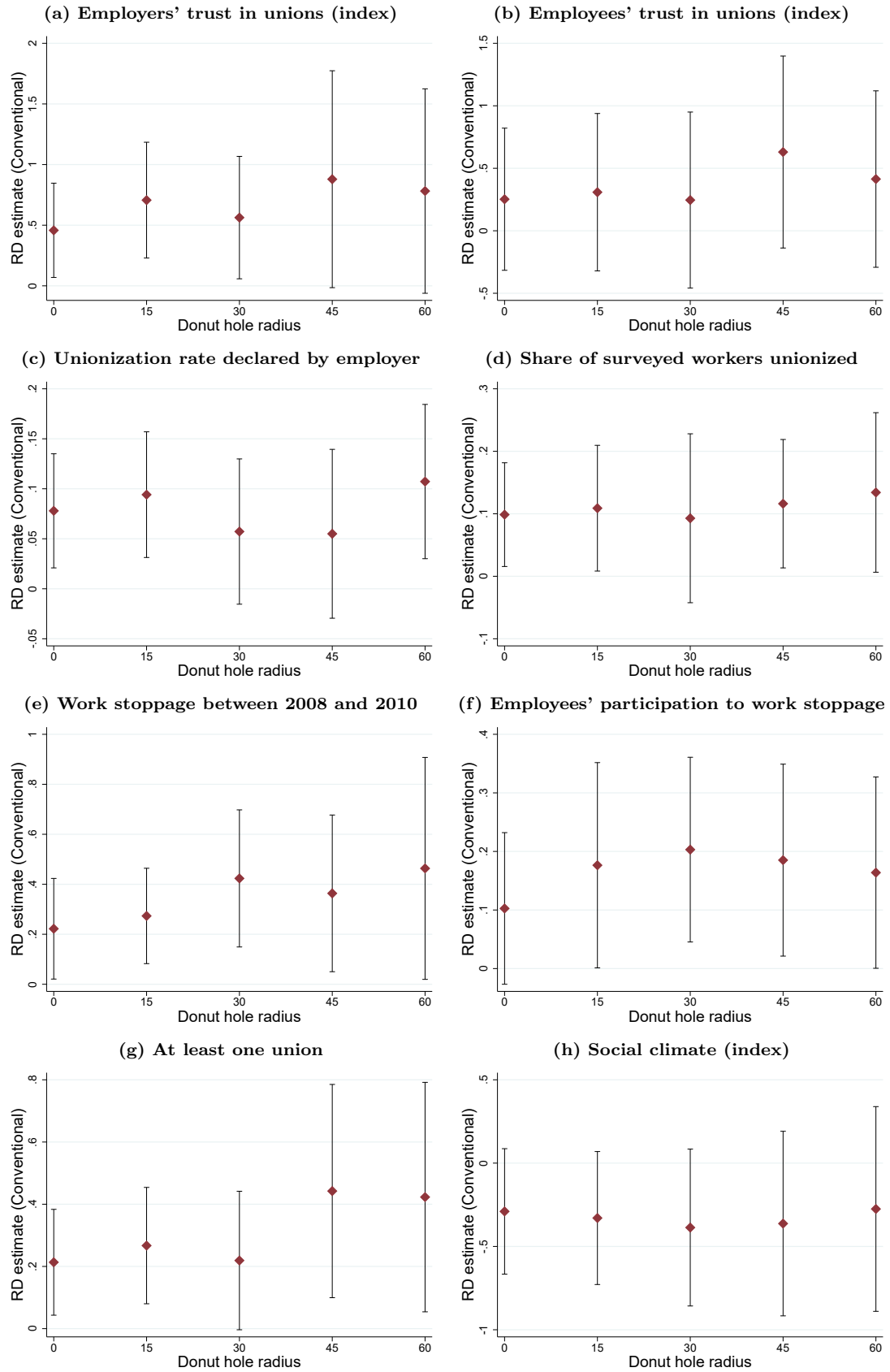
**Table E4:** RD estimates for main outcomes of interest when using a uniform kernel (instead of triangular) to construct the point estimator

	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	0.106	0.054	0.078* (0.041)	0.092** (0.045)	1629	204
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	0.073	0.144*** (0.044)	0.155*** (0.048)	3042	413
Trust in unions index	-0.000	-0.030	0.314 (0.198)	0.362 (0.221)	2784	356
Participation to a work stoppage	0.190	0.177	0.045 (0.056)	0.054 (0.062)	3020	420

*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.

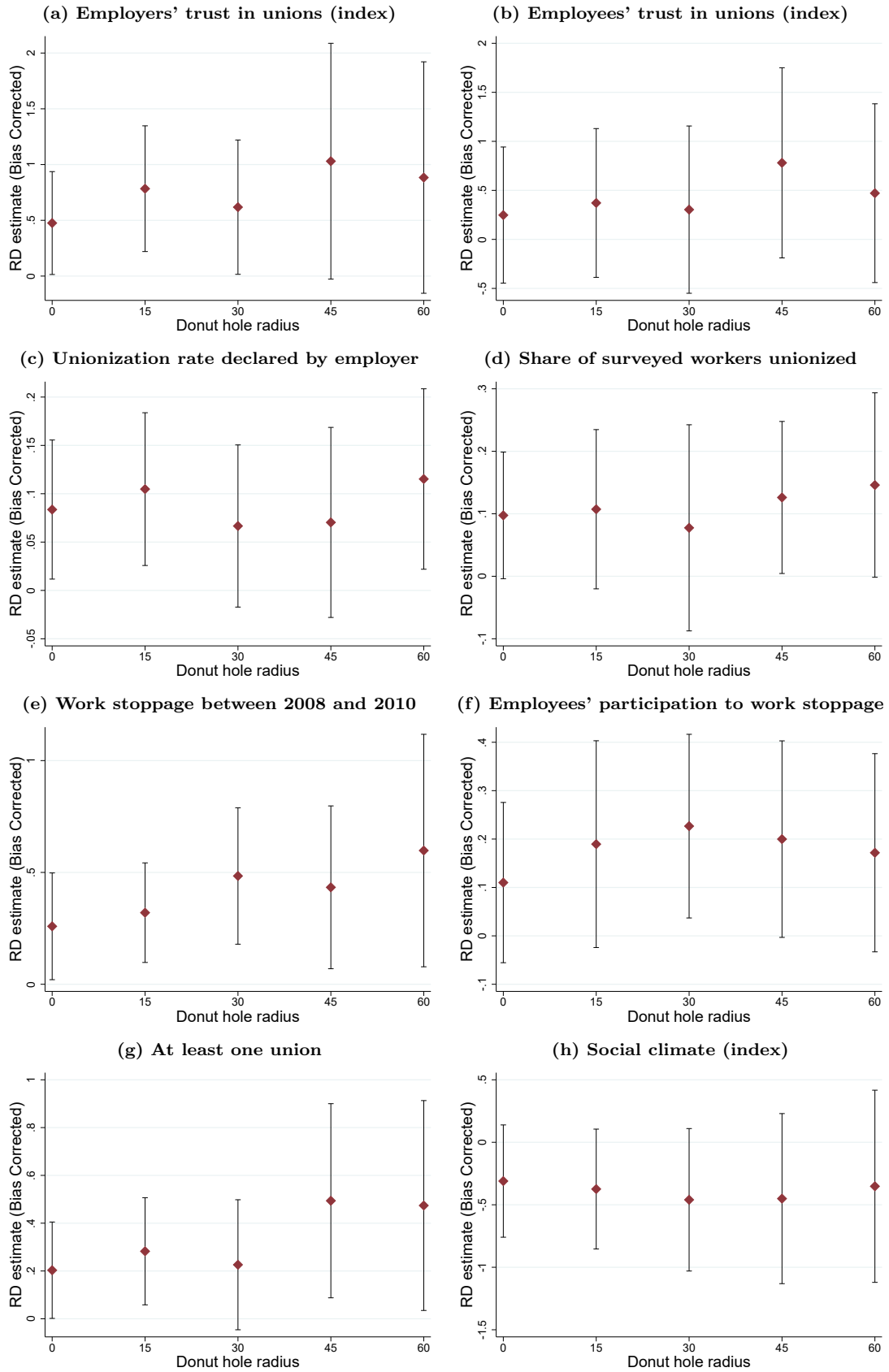
**Figure E5:** RD estimates (conventional) based on the donut hole approach for the eight main outcomes of interest



*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.

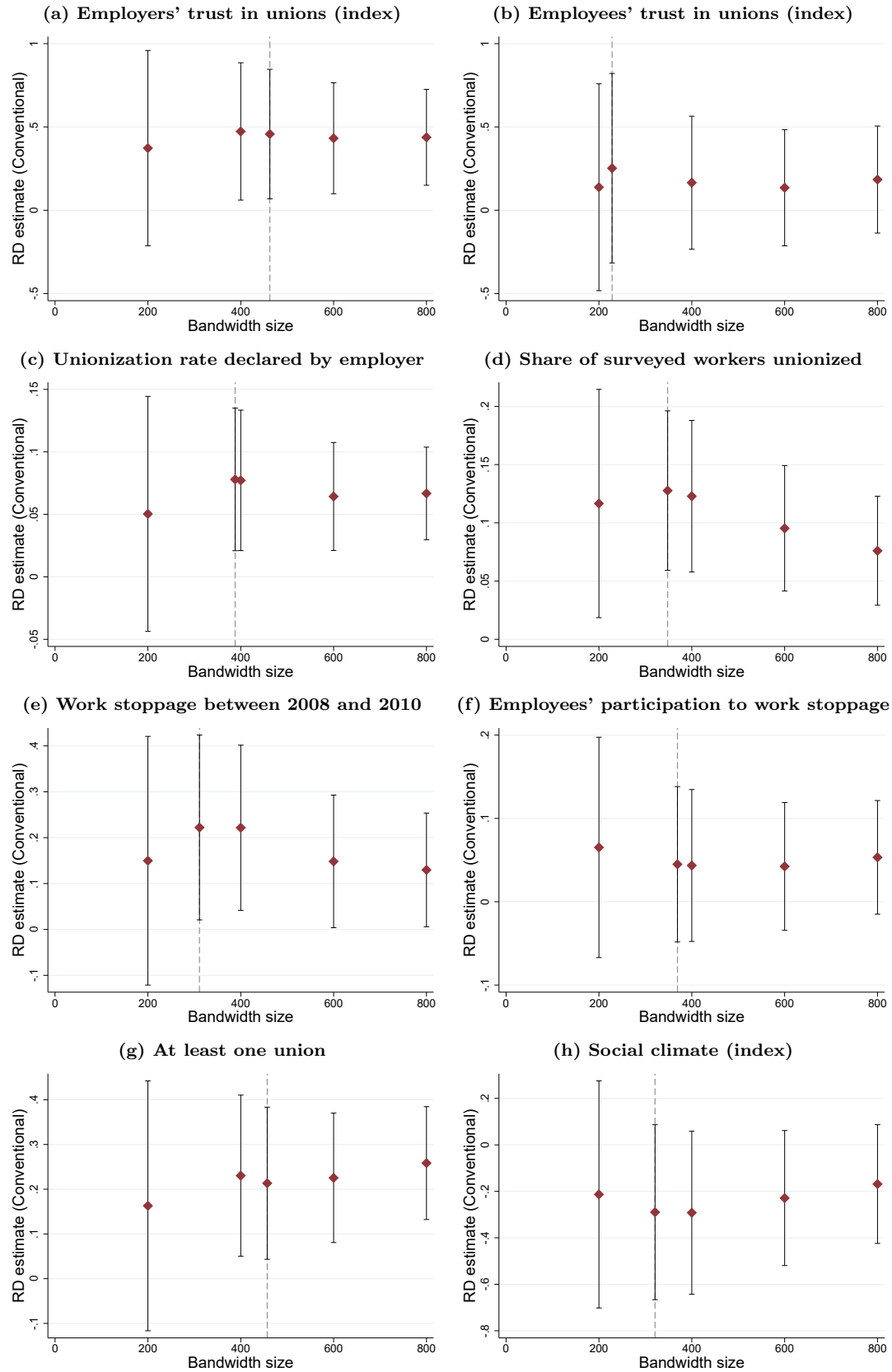


**Figure E6:** RD estimates (bias-corrected) based on the donut hole approach for the eight main outcomes of interest



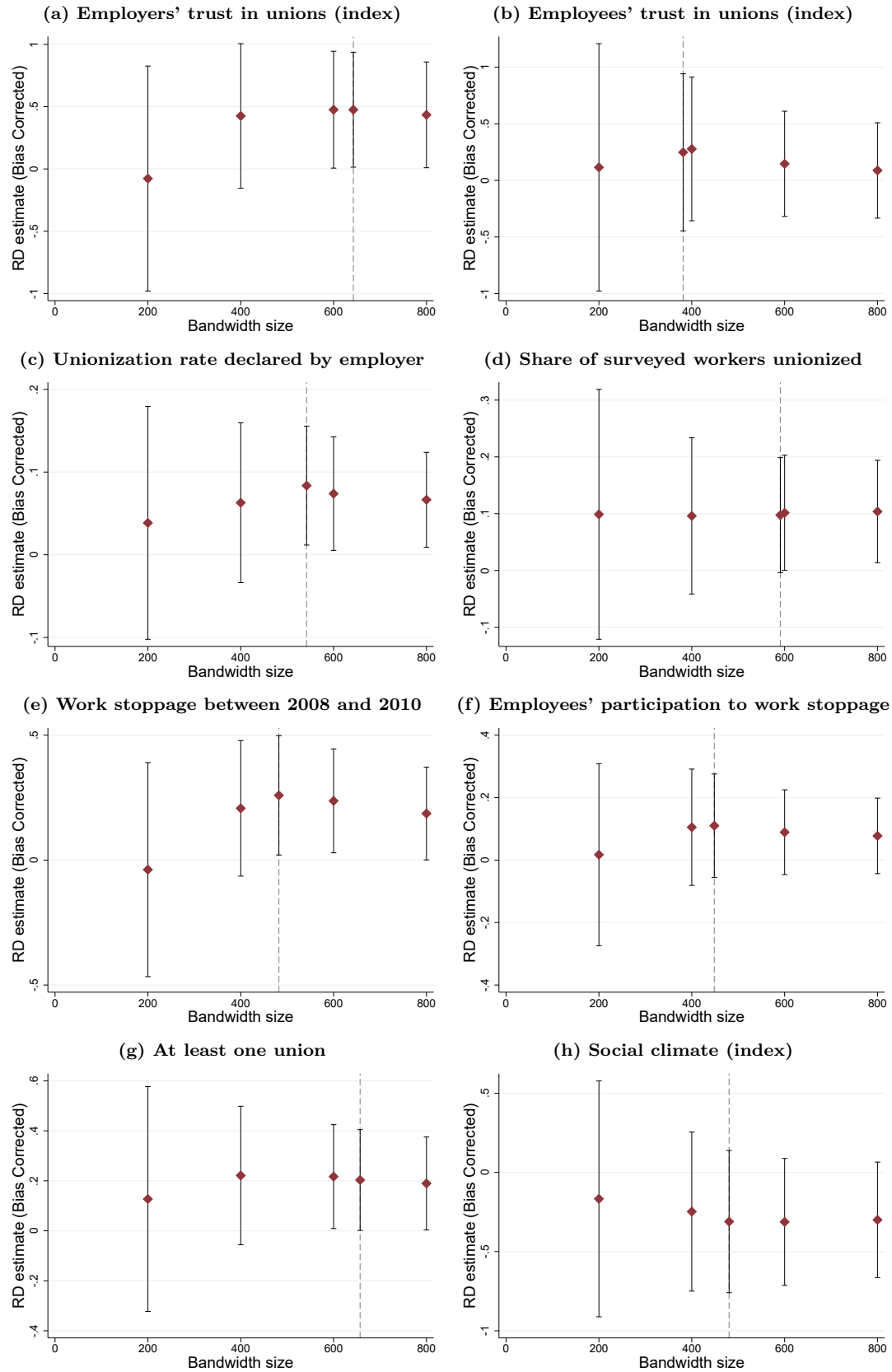
*Notes:* For each of the paper's eight main outcomes of interest, the Figure provides RDD bias-corrected estimates (red diamonds) and 95% *robust* confidence intervals (black vertical straight lines) obtained after removing 0 to 60 days on each side of the January 1<sup>st</sup> 2009 cutoff date ("donut 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 E7:** 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 E8:** 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 F   Evolution of the electoral performance of French unions at the industry and national 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 at 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 (2013-16) show that these unions continued to progress in the medium-run. In particular, the score of UNSA reached 5.4% nationally, and UNSA was recognized representative in 80 industries. These results illustrate that the 2008 reform induced more pluralism by removing barriers to entry for non-historical unions. They are also compatible with an incentive story. Indeed, UNSA is the only non-historical union that is large enough to compete for representativeness at the national level. It managed to make substantial progress to get closer to the 8% threshold necessary to obtain recognition.<sup>A.15</sup>

While limited, the evolution of the results of the historical unions provides additional evidence of incentive mechanisms. The two smallest historical unions which were both under the threat of being excluded from national bargaining clearly had the strongest incentives to compete for voters. The CGC (union of managers) and the Christian CFTC 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 respectively 10.7 and

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<sup>A.15</sup>The electoral results of UNSA during the ongoing electoral cycle 2017-2020 suggest that it will continue to progress and expand its presence. For examples, it became the main union in the RATP, the Paris public transport operator, and attracted one third of the votes for its first participation to professional elections at Mac Donald's France Services.

9.5 percent after the second cycle. However, they both lost their representativeness in hundreds of industries. By contrast, FO, the third French union but far behind the two leading ones, had no clear strategic incentive at the national-level as it could not lose its representativeness nor become leader. FO, eroded from 15.9% to 15.6% of vote casts.

Finally, the two largest (historical) unions 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. laundries, ski stations). The incentives to become leader did not prevent the total score of the two main unions to erode. 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.