Electoral Democracy at Work

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Abstract

We show that an institutional change designed expressly to increase democracy 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, the frequency of walkouts, and it decreased voluntary resignations by about a third. In contrast, it had no effect on firms' economic or financial outcomes in the short-to-medium run. 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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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.[1]

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 further than application of fundamental principles and rights at work (such as freedom of association and collective bargaining).[2] 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.

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

[1]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.

[2]This contrasts with the detailed agenda of structural economic reforms usually called for by these same institutions.

which unions could be recognized as bargaining agents at firm,[3] 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 used to be discretionary. The reform imposed instead that a union delegate had attracted at least 10% of the workers' votes on her name.

The 2008 reform primarily introduces new procedures, consistent with the Schumpeter's definition of democracy as "competition for political leadership" (Schumpeter, 1950). However, its roots are both instrumental and cultural: on the one hand, it aims at boosting the legitimacy of unions, and on the other hand, it is an additional step of the historical extension of citizenship since the French Revolution, summarized by Jean Auroux the French ministry of Labor in 1982: "Like citizens in the City, workers must be citizens in their firm". 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 introduction of such elections may support a substantive form of democracy at work (Cohen, 1996; Levin-Waldman, 2010). Indeed, it 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. Putting an end to bureaucratic conservatism, the electoral condition to become a union delegate can reduce the distance between local union leaders and workers and can boost the emergence of new leaders or push the entrenched leaders to innovate. The mere fact to elect their leaders can also in its own increase workers' willingness to participate in collective action. Finally, by 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

[3]To be recognized for bargaining, a union had to get a least 10% of the vote cast at these elections.

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 first find that the democratic rules introduced in 2008 increased "social capital" or "common ideology" à la Dunlop (1958) 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.

Second, combining administrative data on workers' flows (exhaustive for all firms above 50 employees) with the election dates, we adopt the same identification strategy and find a large negative effect of the reform on voluntary resignations, which are reduced by about a third in 2011. This goes together with 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. 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.

Finally, we leverage exhaustive administrative data on firms balance sheets to examine the effect of the reforms on firms' economic and financial outcomes and do not find any significant effect two to three years after its application.

We distinguish three main possible explanations for the observed effects of regular free elections: (i) they induced the emergence of a new generation of union representatives, (ii) they pushed existing representatives to put more effort due to the competitive pressure these elections generate, and (iii) they were sufficient in their own to make unions more legitimate bargaining partners. All firms in our sample eventually faced elections and are therefore subject to competitive pressures, making the second explanation not fully identifiable. Additionally, we do not detect changes in the observable characteristics of local union leaders due to the reform. This suggests a direct effect on trust of institutional change: free regular elections in their own, as a new, salient, and perhaps more legitimate way of appointing union representatives, were sufficient to increase stakeholders' satisfaction.

Introducing such 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 informal criteria (e.g. mutual recognition by social partners), minimal membership at national level or on historical privilege, as in France before the reform.[4] Even in countries that do hold elections, they are not always organized in such a way as to encourage competition and direct electoral legitimacy of unions dele-

[4]For example, Germany has no specific rules for defining union representativeness while Belgium has a membership criteria that *de facto* excludes all unions but three historical ones.

gates. 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.[5] In addition, negotiators are directly appointed by the union. 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, the revitalization of the labor movement is explored by a vast literature in sociology of organizations and in political science (see Murray (2017) for a review). This literature however does not focus on public policy and institutional reforms but rather on innovations in internal organization and strategies of unions. The main concern is the oligarchy and conservatism in goals and tactics of unions. According to the seminal Michels (1915) "Iron low of oligarchy" and Jenkins (1977), union organizations face bureaucratization over time. 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 (Kremer and Olken, 2009). 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 considered to be a key solution to overcome the oligarchy (Edelstein and Warner, 1976; Freeman and Medoff, 1984). In particular, more democratic internal functioning of unions is seen as essential to foster unionization (e.g. Fiorito et al. (1988) on the U.S., Lévesque et al. (2005) on Quebec). In an influential paper based on U.S. 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. The contribution of our work with respect to this literature is to focus on democracy within the firm rather than within unions and, perhaps more importantly, to provide well-identified quantitative evidence on how more democratic rules can affect

[5] Decertification is possible but rare in practice, and in that case the loss of representativeness for a given union results in a de-organizing of the workplace rather than the gain of representativeness for a competing union (as it is the case in France).

unionization. As far as we know, our study is the first to provide such evidence, examining also the causal effect of more democratic rules on employers' attitudes towards unions, and more generally on labor-employer cooperation.

Second, in terms of methods, the paper closely relates to the recent literature that exploits reforms and/or size-dependent variations of workers' rights of representation in corporate boards in Germany (Kim et al., 2018; Jäger et al., 2021), Norway (Blandhol et al., 2020), or Finland (Harju et al., 2021). Using convincing identification strategies, these papers typically find zero or small positive effects of codetermination–as it is practiced in the above countries–on workers' wages and firm economic outcomes (see Jäger et al. (2022) for a survey). These conclusions are in line with ours. However, we find a large effect on voluntary job separations (in contrast to Harju et al. (2021)) and, based on rich survey data, we are able to show that, while having no immediate effect on economic performance, changes in labor market institutions can affect substantially employment relations and firm-worker cooperation.

A key feature that distinguishes our work from the above papers and more generally from most of the empirical literature evaluating the effect of institutional changes[6] is that we do not focus on a change in workers' representation or bargaining rights, but rather on a change in the rule for appointing union negotiators. This justifies our emphasis on democracy as a way to revitalize unions and could explain why our findings somewhat differ from those obtained in other papers. These findings also exemplify that giving workers representation rights may not be enough if one does not make sure that workers get ways to be involved in what their unions do.

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.

[6] Ellwood and Fine (1987) examines for example the effect of right-to-work laws in the U.S. and shows that it significantly reduced flows into unionism through organizing. An interesting feature of our own work, in this respect, is the demonstration that a targeted reform changing unions appointment rules 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.

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 them 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.[7]

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 their

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

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 at this stage.[8] 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 introduced a free market 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.

[8]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 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.2. The survey is also extended with information on workplace size in 2008 (prior to the reform) coming from an administrative source, the *Déclaration Annuelles de Données Sociales*. We use it in balancing and robustness tests.

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.[9] 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.2).

The DMMO/EMMO dataset. The DMMO (Données sur les Mouvements de Main-d'Oeuvre) has exhaustive quarterly data on gross worker flows (hirings and separations, excluding temporary help workers) for establishments with 50 employees or more. The data is broken down by type of flow. The EMMO (Enquête sur les Mouvements de Main-d'Oeuvre) has identical information on a representative sample of establishments with less than 50 employees. We use the DMMO-EMMO data to compute establishment-level measures of voluntary resignation rates by year or semester for years 2010 and 2011.[10]

The FARE dataset. FARE is a compilation made by the French national statistical office (INSEE) of the firm tax returns. It contains firms' balance sheets and financial statements, allowing us to construct measures of average wages, the labor share, investments, economic performance (value-added per worker and TFP) or financial performance (ROE or ROA) at the firm level for years 2010 and 2011. Details on the construction of these variables are provided in Appendix B.5.

The MARS dataset 2009-2012. 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 to 31 December 2012. The administration exploits

[9]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.

[10]See details on DMMO/EMMO data construction in Appendix B.4. Before 2009, all establishments are untreated and after 2012, they are all treated. Hence, we need to focus on years 2010 or 2011 to be able to compare treated and control establishments with our RDD design.

this dataset to compute union representativeness at industry/branch and national level and made it publicly available recently. The full dataset has been made publicly available recently. We exploit 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 REPONSE survey. Our empirical strategy (see next section) requires, for each employer and worker in the 2011 REPONSE 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.1. For the worker side, the approach differs slightly. They all filled out the REPONSE 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 elections during that period. For all other establishments, we consider the latest relevant election date before 1 April 2011.

Latest staff election before observing workers flows or economic outcomes. Outcomes in the DMMO/EMMO and in FARE are not measured at a specific date but for a given period of time, either in 2010 or 2011. We use the same algorithm as above to determine the latest staff election before the beginning of the period of interest. Additionally, we remove in our baseline specifications firms for which this election occurred before January 1st 2009 and that have another election *during* the period of observation. This is because those firms become subject to the new legal regime during the period of observation and are therefore partially treated (see details in Appendix B.4).

An additional complexity with the firm tax returns is that outcomes are measured at firm level, while elections are held at establishment level. In practice, however, many multi-establishment firms hold all their establishment-level staff elections at the same date, so that there is no ambiguity on which election date should be used to measure if a firm is treated or control. We keep in our sample all those firms, as well as all mono-establishment firms. Relatedly, in the DMMO/EMMO data, we group together and consider as a single entity all establishments within the same firm that have their election at the same date. This is because these establishments cannot be considered as independent observations since they coordinate their elections (see details in Appendix B.4).

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 2010 or 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.[11] As a consequence, for workplaces where elections had already been held, the last election date D_j before an outcome is measured 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, and in 2010 or 2011 using administrative data DMMO/EMMO or FARE), 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 $\tau = \mathbb{E}[Y_j(1) - Y_j(0)|D_j = 1^{st}\ Jan\ 2009]$, the causal impact of the reform on Y at the cut-off date. τ 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.

When measuring outcomes with REPONSE11, 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.

[11]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:

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

where $y_{j,2011}$ is the outcome of interest measured in 2011 (between January and June for employers) in firm j and ϵ_j is a residual term. β 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.[12] 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 REPONSE11 (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. Finally, our data allows us to observe for all firms that had an election after January 1st 2009, the actual date at which they should have held their election. We exploit this information to construct the expected election date for all firms for which this is possible, and use it as an alternative running variable. As we will explain, this robustness check allows us to show that our results are not driven by firms strategically delaying their election dates.

The local bandwidths for the estimation are determined endogenously for each out-

[12] Q has no constant term, as no such term could be identified separately from β.

come. 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 bandwidth and (ii) bias-corrected estimates computed following Calonico et al. (2014) as well as their associated robust standard errors and p-values.[13]

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. For survey outcomes, we use 4 bins to the left and 12 to the right of the cut-off, 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 in REPONSE 2011. 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 REPONSE11, we exclude workplaces in being for less than five years in 2011 (3.5% of the initial sample).[14] Second, workplaces that hold staff elections every two years should have had their latest election date under the new regime ($D_j > 1^{st}\ Jan$ 2009) when observed in the first semester of 2011 in REPONSE11. 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.

[13] Estimates and standard errors obtained using the Stata command *rdrobust*.

[14] Workplaces older than five years should have had at least two elections before REPONSE11, 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.

Sample restrictions in DMMO/EMMO and FARE. We apply the same restrictions as above. Regarding firm age, we use the information on firms' date of creation and exclude those created in 2006 or later. The final samples consist of 12,304 observations on resignation rates observed in DMMO/EMMO for the first semester of 2011 and 18,306 observations on firms' economic outcomes observed in FARE in 2011.[15]

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 observed in 2010 or 2011 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 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[16], 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.

[15] Final sample sizes in FARE and DMMO are much smaller than the total number of French firms with 10 employees or more (around 150,000). This is for various reasons: DMMO/EMMO is not exhaustive for firms below 50 employees, we add to discard firms holding workplace-level elections at different dates in FARE, we remove young firms and firms where elections occur every two years, and, most importantly, several firms have no candidates for elections and do not hold them.

[16] Actually, almost no elections are held in July or August, typically vacation months.

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 hard in a democratic country like France. First, 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 with the authorization of the labor administration, 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 1st 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. Tables 2a and 2b provide descriptive statistics on observable workplace characteristics and checks for discontinuities at the cut-off in both REPONSE2011 and the much larger sample DMMO/EMMO.[17] Except for being an establishment with more than 1,000 employees in the DMMO/EMMO, none of the estimated discontinuities are statistically significant at the 10% level, suggesting that in our framework manipulation was not a major issue. To strengthen this claim, we use in robustness checks an alternative running variable that cannot be affected by firms strategically delaying their election after the application date of the reform, and show that results are similar.

[17] The corresponding graphs on the DMMO/EMMO sample are provided in Appendix Figures C1 and C2.

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. We then turn to the results for workers' flows and economic or financial outcomes. Finally, 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.[18]

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 *Délégation 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

[18] We systematically report the number of observations in the optimal bandwidth used for the estimation of the regression function. The bandwidth sizes used for the estimation of the regression function and the bias correction are also provided for our main outcomes respectively in Figures E3 and E4.

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)[19], and Table 3 panel B (second row) show 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 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

[19]Versions of this important figure with more bins are provided in Figure C3 in appendix C.

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 Béroud (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:[20]

- 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%).

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).[21] 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

[20] 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.

[21] Variants of Figure 4 with more bins are shown in Figure C4 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.

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 Béroud (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.[22] 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). 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 improves stakeholders' perceptions of unions: the effect is large

[22]The questions used to measure these perceptions are detailed in the data Appendix.

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 Conflicts

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' questionnaire 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.[23] 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. Statistical non-significance could also reflect heterogeneity in the impact of the reform. We explore this issue in the sub-section 5.2.

At this stage, we do not exclude the possibility that the reform may have prompted increased participation in work stoppages.[24] 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.

4.4 Voluntary resignations

The study of resignations offers additional insight into workplace climate. We first focus on resignations for the first semester of 2011, that is the period at which survey outcomes are also measured. Table 6, panel a, reveals that the workplaces applying the reform regime had a significantly lower resignation rate during that period (2% against 3%). This reduction by a third of resignation is also illustrated on Figure 5. It is robust to including as controls in the analysis workplaces that had an election during the first semester of 2011 and might therefore be partially treated (Table 6, panel a, second row). A new legal procedure to terminate job contracts by mutual agreement was introduced in France in 2008 and may partly substitute for voluntary resignations. The reform effect on quits is still observed when these separations by mutual agreement are added up to resignations (third row).

Looking at other time periods, we find that the firms applying the new legal regime kept reducing quits in the second semester of 2011, and were already doing it in the

[23] 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.

[24] 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%.

first and second semester of 2010. The effects in 2010 are however quantitatively slightly smaller and are not statistically significant. We finally provide an additional balancing test for workplaces observed in the first semester of 2011: we compare (when observed) their resignation rate prior to the reform, that is in the first semester of 2008. Estimates are not statistically significant, and if anything, they go in the opposite direction to those observed post reform. This placebo test is reassuring.

Together, the results for resignations are consistent with the thesis that direct democracy at the 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.

4.5 Economic and financial outcomes

Table 6, panel b, provides LATE of the reform on economic performance, financial performance, wages, and investment in 2011.[25] None of the estimated effects is statistically significant. More specifically, we can reject an increase in value-added per worker larger than about 10% and an increase in average wages larger than about 8% due to the reform. We can also reject that the reform had large short-term effects on investment or on financial performance. These conclusions are very similar to that of Jäger et al. (2021) who reject with a similar (or slightly lower) precision an effect of codetermination in Germany on wages, productivity and capital per worker. In particular, our estimates suggest that the reform did not trigger large disinvestments as predicted by the hold-up view (Grout, 1984). Conclusions are similar when outcomes are measured in 2010 (results not shown) or when we use alternative specifications (e.g. controlling for covariates, as there some small imbalances around the cutoff in covariates observed in 2011). These results illustrate how reforms of social dialogue may substantially affect employment relations in short to medium run while having no or only limited effects on firms' economic and financial outcomes.

[25]Balancing tests for covariates observed in the FARE data are presented in Table C1. We observe that the number of firms in the construction and market services sectors are not balanced around the cutoff. Other covariates are balanced.

4.6 Falsification tests and robustness of RDD estimates

Positive and significant effects of the reform have been found for six main outcomes at workplace level: union recognition, unionization rate, employers' perceptions of unions, employees' perceptions of unions, work stoppages, and voluntary resignations (in the first semester of 2011). 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 REPONSE11 survey, and robustness checks have been run for both. 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.

Difference-in-Discontinuity Design. An alternative and more direct way to examine if our estimates are driven by the fact that the cutoff date is the end of a calendar year is to use a kind of Difference-in-Discontinuity design, comparing the difference in outcomes between workplaces with elections before and after January 2009 to the difference between workplaces with elections before and after January of another calendar year. We implement this approach by estimating our main effects on a fixed window including 800 days (more than 2 years) on each side of the cutoff and including as controls both trimester of election fixed effects and the number of days between the election date and the closest January First. Our empirical specification is as follows:

$$y_{j,2011} = \alpha D_j + \beta \mathbb{1}(D_j > 1^{st}\ Jan\ 2009) + \gamma D_j \times \mathbb{1}(D_j > 1^{st}\ Jan\ 2009) + \Sigma_{t=1}^{4} \delta_t \mathbb{1}(Quarter(D_j) = t) + \kappa \times dist(D_j, Jan1^{st}) + \epsilon_j \quad (2)$$

The trimester of election fixed effects capture baseline discontinuities in the outcome between firms having elections at the beginning and the end of a calendar year. The distance between the election and the closest January First further allows differences in slopes between 1 January 2009 and 1 January of other calendar years.[26] Table E1 shows the results which confirm the insights from our falsification test based on an alternative cutoff on 1 January 2010: the reform effects on the occurrence of work stoppages are no longer statistically significant in this specification. We still observe however a significant conventional estimate for workers' participation to work stoppages. The estimated effect of the reform on workers trust is also not significant, while its effects on other main outcomes are still observed.

Using a proxy of the expected election date as running variable. Using the information in the MARS dataset on the date of the previous election, we can reconstruct the expected election date for all elections occurring after 1 January 2009 (see details in Appendix B.1). We replicate our main estimates using an alternative running variable that is still the latest staff election date when it occurred before 1 January 2009, but the expected latest staff election date for all elections occurring afterwards. Reassuringly, our main results (for employers trust, union coverage, unionization and voluntary resig-

[26]This distance is comprised between -183 and 182 and varies continuously when D_j is in the neighborhood of 1 January of any given year. It is however discontinuous at 1 July of any year, something we absorb with the trimester fixed effect. Note that a standard Difference-in-Discontinuity setting would typically involve comparisons around the same cutoff of individuals concerned and not concerned by a reform. In our case, we compare discontinuities around different cutoffs for the same workplace, making the empirical implementation a bit more cumbersome.

nations) are robust to this important alternative specification (Table E2). This shows that these results were not driven by firms strategically delaying their election.

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. Figures E1 and E2 provide conventional and 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 E2), 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 voluntary resignation in the first semester of 2011 always remains significant at the 5% level.

Varying bandwidth size. Figure E3 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 statistically 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.[27]

Controls and uniform kernel. Table E3 presents R.D. estimates for the main outcomes of interest with control variables added to the baseline specification. These controls include the variables used for the balancing checks (see Table 2). Panel A also adds 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 REPONSE11 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 addition of controls. However, point estimates tend to be slightly smaller and standard errors slightly larger when controls are included, implying that some results are no longer statistically significant. This is the case for the effect on the unionization rate which becomes only marginally significant. These results might reflect a lack of statistical power rather than a real identification issue. However, as they are not fully reassuring, we performed an additional check and included controls in specifications that use as running variable the proxy for the expected date of election described above. Results presented in Table E4 are in that case stronger and significant for the unionization rate, and we even find a large and significant effect of the reform on workers' trust in unions. Overall, our conclusions appear robust to the inclusion of controls. The observed differences from baseline are not surprising, since most effects cannot be estimated very precisely.

Table E5 finally presents R.D. estimates for the main outcomes of interest using a uniform instead of triangular kernel. All results are maintained except the impact of the reform on employees' trust that is no longer significant for the expanded sample of workers.

To summarize. These checks confirm that the 2008 reform had a positive impact on employers' trust, union recognition, unionization, and voluntary resignations in 2011.

[27]For the sake of completeness, Figure E4 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 E4 must accordingly be taken with caution.

The R.D. estimates for these variables are still significant in virtually all the checks. As for work stoppages, employer and worker responses are consistent and suggest a possible impact of the reform, but when a Difference-in-Discontinuity design is used the estimated effect becomes 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.

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 thesis that the reform may have induced a partial shift from entry/exit to voice/loyalty employment relations in France, and (iii) the channels through which the reform impacted on employers and employees.

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.[28] 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. If this were the case, one might expect smaller treatment effects for workplaces treated latter in time, inducing the slopes of the fitted lines on RDD graph to be *more*

[28]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.

downward-sloping to the right of the cut-off than they are to its left.[29] Examining Figures 2 to 5, one sees clearly that for the main outcomes the fitted lines have similar slopes on each side of the cut-off (usually flat or slightly negative). This suggests 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 REPONSE11) 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 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 C5).[30]

[29] To the right of the cut-off, the slopes of the fitted line on RDD graphs capture both the direct effect of the date at which the staff election is held and the effect of the time interval between an election and the moment when the outcomes are measured. On the left, they capture the same two effects plus how the treatment effect varies with the distance to the cut-off. Comparing the slopes hence provide suggestive evidence on variations in treatment effects with time.

[30] Unfortunately, the Eurobarometer dropped the specific question on trust in unions after 2010, pre-

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.[31] 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 7 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 government declined from 20.3% in 2008 to 17.4% in 2016, according to EU-SILC.[32] 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 employ-

venting its use in considering medium-run effects.

[31]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.

[32]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.

ees 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 7 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.1. In all cases, union membership in the REPONSE sample diminished between 2005 and 2008 and then turned up with the implementation of the 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 conductive 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 7 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, 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 7 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 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: despite more industrial actions, employers' opinion on unions improved. This is particularly clear in manufacturing and construction, where mobilizations are more common than in services (Table 8, panel A): our estimates suggest a significant increase both in the number of work stoppages, especially walkouts 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 mirrored in walkouts. The impact of the 2008 reform on resignations (see Table 6) is consistent with this interpretation.

From this standpoint, by introducing new procedures, the reform has not only delivered representative democracy at work but also fostered industrial democracy in a classical sense: workers are less afraid to join unions and voice their concerns. This shows that the introduction of procedural democracy can lead to more substantive forms of democracy (Cohen, 1996; Levin-Waldman, 2010).

5.3 Democracy, unions legitimacy and attractiveness

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 gives unions the incentive to improve the quality of their representation. 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.

In what follows, we first show that, while we indeed have a large expansion of union coverage, this extensive margin is unlikely to entirely drive our estimates: the reform also changed employment relations in workplaces where unions were already present. We then review three possible explanations for this change at the intensive margin: (i) incentives induced by competition for representation mandates, (ii) renewal of union representatives, and (iii) a direct effect of democracy as an appointment rule, all else being equal.

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 effect is likely to be driven by the incentives provided by introduction of electoral thresholds at the industry and national levels: to be representative at these levels, unions had incentives to organize and gain votes in new workplaces. To backup this claim, we show in Appendix F.1 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 RDD results for other outcomes (e.g. trust or unionization) are however unlikely to be driven entirely by the extensive margin response. To show this, we first focus on large workplaces that are almost all covered by unions and show that the reform still has an effect on trust or unionization among these workplaces (Table 8, panel A). Comparing trust or other outcomes in workplaces covered and not covered by unions, we then make naive calculations of the share of the reform's impact on these outcomes that can be accounted for by its impact on coverage. We typically find that the extensive margin may explain a quarter to a third of the total effect, hence leaving most of our estimates driven by workplaces where unions were already present. These arguments and the related calculations are detailed in Appendix F.2.

Incentives induced by competition? After the reform, representatives and unions have an incentive to perform better, because they know that they will face more consequential and competitive elections in the future. However, our estimates are unlikely to be strongly impacted by this effect. Indeed, in 2011, unions in both treated and control firms around the cutoff will face such consequential elections one or two years later. This being said, we cannot exclude completely a role for competitive pressures: it could be that these pressures are more salient to representatives that have already been elected once, the others not realizing yet the consequences of the new regime. The degree of competition is actually revealed to unions in firms that already had an election in the new regime, while in other firms, incumbent unions may not know yet if their mandates will be challenged by new entrants.

Change in the profile of union delegates? Post-reform elections may select different workers' representatives, shaking the local union oligarchy, and leading to the emergence of new generation. Because of the personal data protection regulation, the MARS dataset does not include individual information. However, when present, workers' representatives are interviewed in a third part of the REPONSE survey and provide their sociodemographic characteristics. Only one representative is interviewed in each establishment, and this representative should belong to the list that attracted most votes during the last professional elections. We know their age, gender, date of the first mandate and education. Even if the survey is not designed to cover a representative sample of union delegates, it can provide some hints regarding the effects of the reform on their profile.[33] Using the same RDD strategy as in the rest of the paper, Appendix Table C2 shows that union delegates in treated workplaces are more often women, slightly less experienced as representatives but also older and less educated. These differences are however not statistically significant and in all cases, they do not indicate a clear renewal of the profiles of union negotiators. 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.

A direct effect of democracy. The very fact of being allowed to vote may itself foster workers' participation in unions and hence their satisfaction with them. It can also make unions more legitimate bargaining partners for employers, hence increasing their trust. This is one of the key ideas of the literature on substantive democracy (see e.g. Cohen, 1996; Levin-Waldman, 2010). Our results are consistent with this explanation. Considering which workers are most affected by the reform provides further evidence. We may expect that the opportunity to vote should increase primarily the trust and participation of the workers that were initially the most distant from the unions, such as women or young workers. This is what we find in Table 8, panel B, which 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 and education. Overall, the effects tend to be 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 the conclusion that the reform was more beneficial

[33]About 30 percent of the workers' representatives could not be surveyed because the employer did not provide their contact or because the interviewers were not able to reach the representative at the address and phone provided by the employer.

to groups of workers initially less involved in unions. As argued, this could be because voting in its own affected these workers perceptions and behavior. But it could also be because elected unions were more sensitive to electoral incentives (our first explanation) and made more efforts toward these populations in order to be more representative of the "median voter". 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 also 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.

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 both employment relations and firm performance using survey and administrative data for the middle of the implementation period. This results in a rare micro-level evaluation of changes in the regulations governing employment relations.

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 work stoppages, greater workers' participation in them, and fewer voluntary resignations, suggesting that it helped workers to voice previously unspoken demands, in turn increasing their loyalty to the firm. In political philosophy terms, these findings support that a reform introducing a procedural democracy for union representativeness can turn into substantive democracy at work. Since both our treated and control firms will be eventually subject to free election, our results are unlikely to only reflect responses to competitive pressure induced by the prospect of elections. Instead, they may reflect either the emergence of a new generation of local union leaders in firms that already had elections, or the fact that allowing workers to vote for their representatives is sufficient to foster demand for and involvement in unions. Evidence suggests that this latter mechanism dominates.

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. While decades of massive national rallies and strikes against labor market and pension reforms did not reverse the decline of unions in France, the 2008 law provides an additional lesson that might be relevant in different institutional contexts: the workplace level may be more effective for building the legitimacy and the membership of unions than nation-wide campaigns.

Normatively, the absence of negative effects on economic performance combined with the increase in satisfaction with unions for both the workers they represent and the employers they bargain with lead us to conclude that the 2008 reform was a success. Repeated free workplace elections for union representation is therefore an interesting model that other countries may want to consider.

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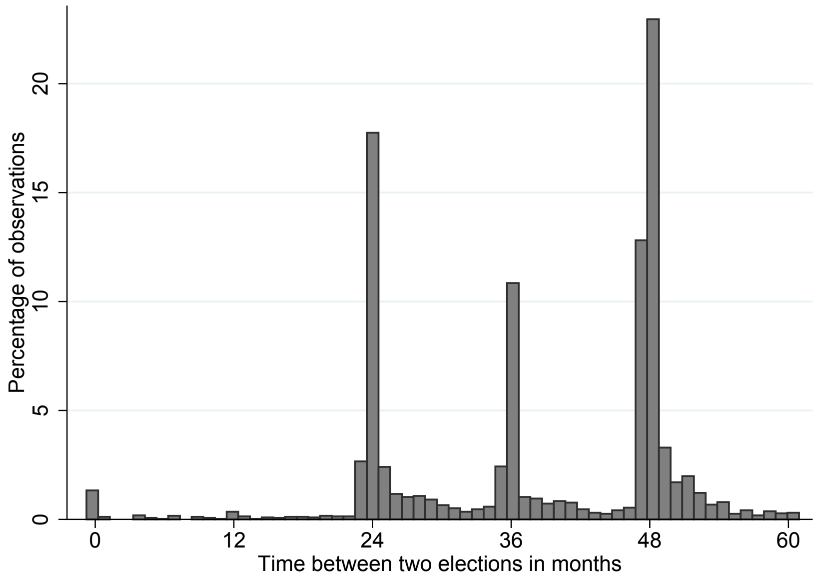
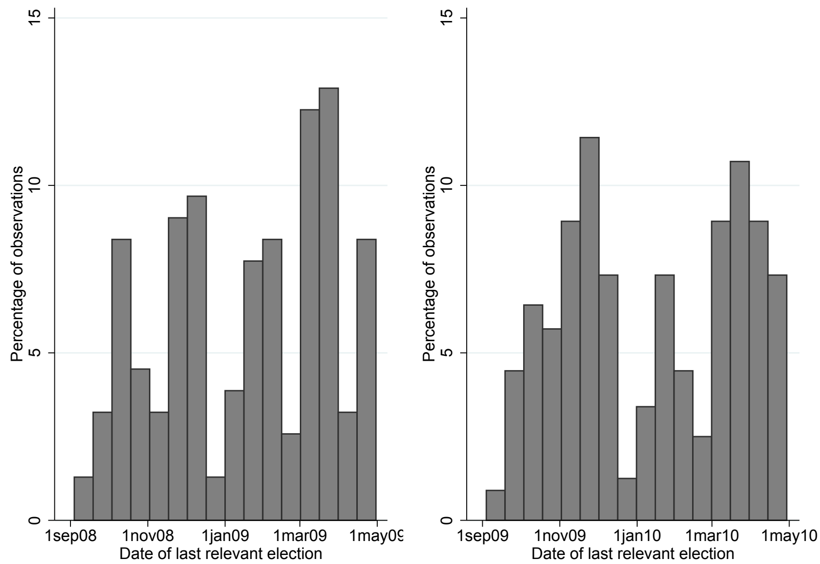
Figures and tables

Figure 1: Election dates

(a) Number of months between two consecutive elections



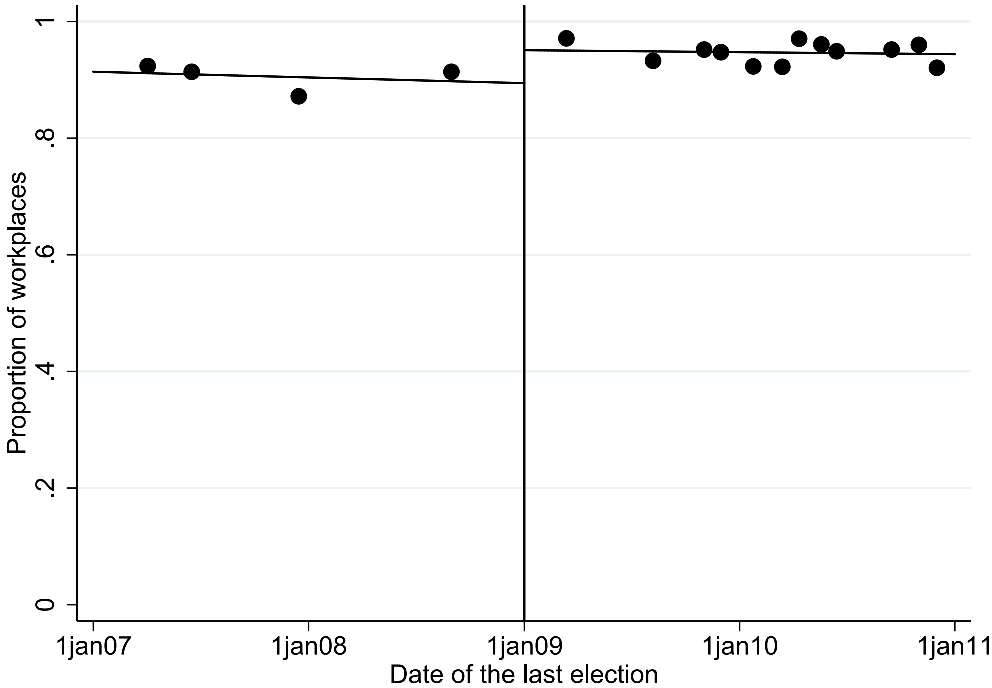
(b) Zooms around 1st January 2009 (cut-off date) and 1st January 2010



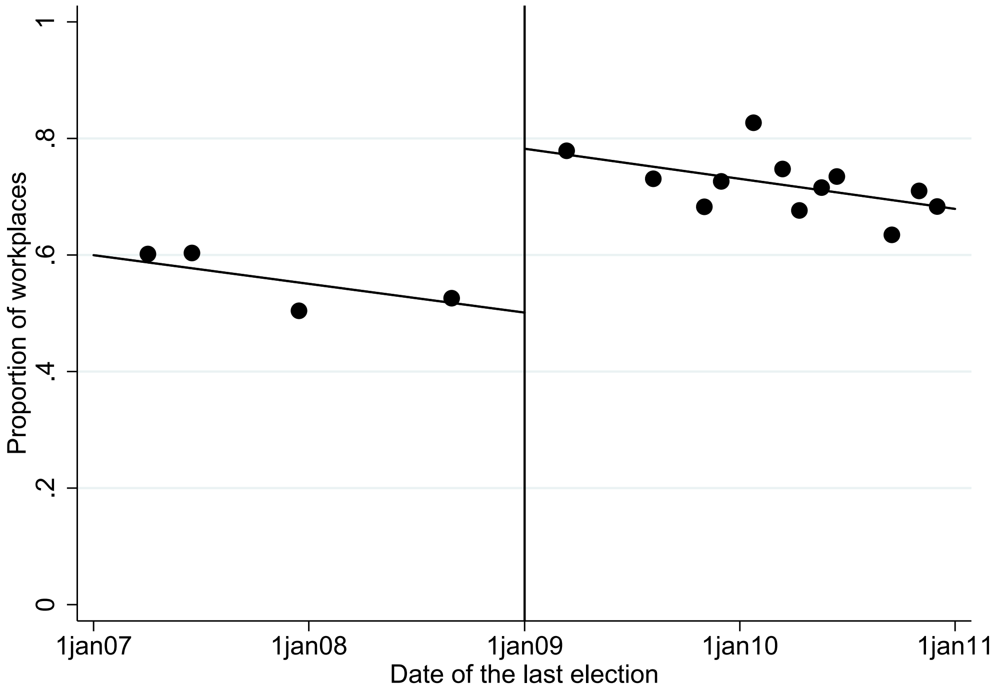
Source: Panel (a): MARS dataset, only establishment present in REPONSE11 which have registered an election during the period 2009-2012. Workplaces with more than 62 months between two consecutive elections are excluded. Panel (b): Our own computations from the MARS administrative dataset matched with REPONSE11 (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 REPONSE 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 (1st 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



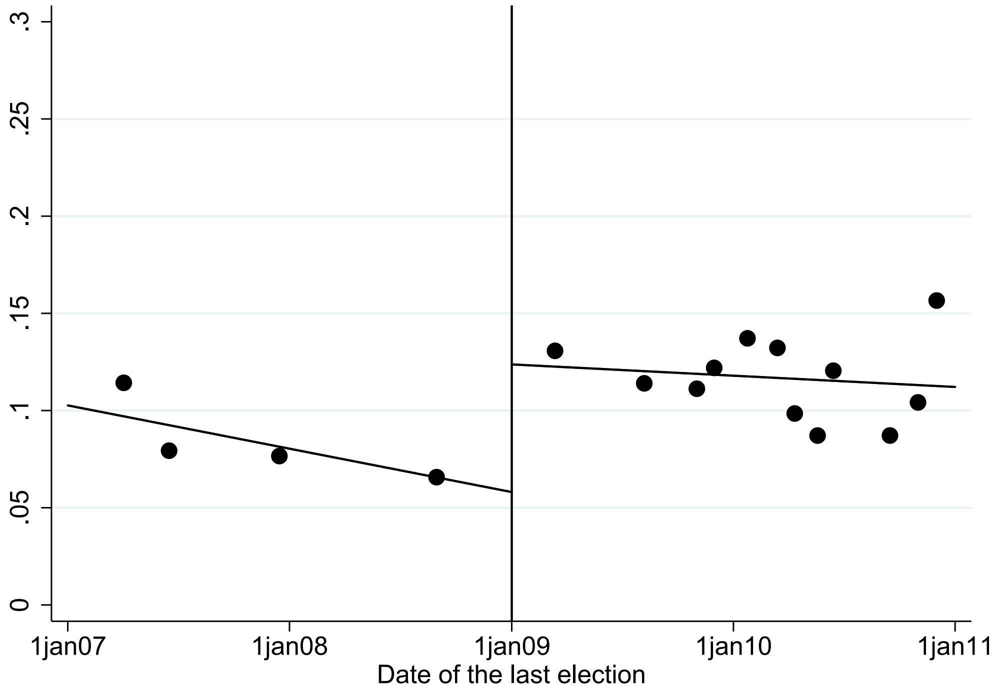
(b) At least one union recognized for bargaining



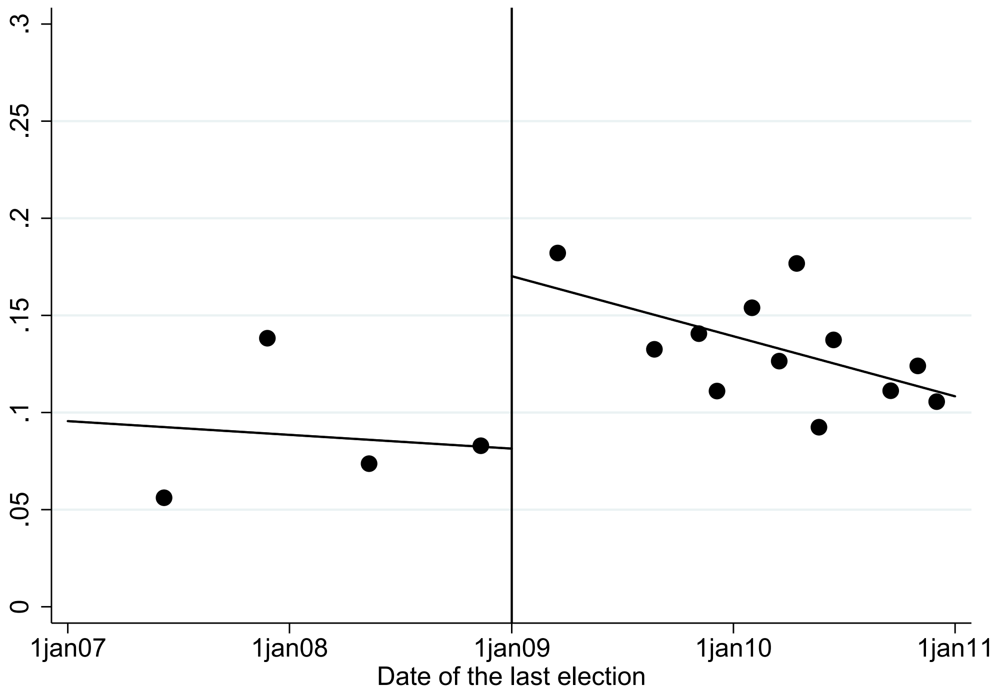
Notes: Each bin provides the mean of the interest variable for establishments experiencing their last staff election around the date of the bin; observations are split in 4 equal-size groups at the left of the cutoff date, and 12 equal-sized bins at the right of this cutoff. Lines represents the linear trend of the interest variable before and after the cutoff date. Workplaces younger than five years or having staff elections every two years are excluded.
Source: Our own computations from the MARS administrative dataset matched with REPONSE11 (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



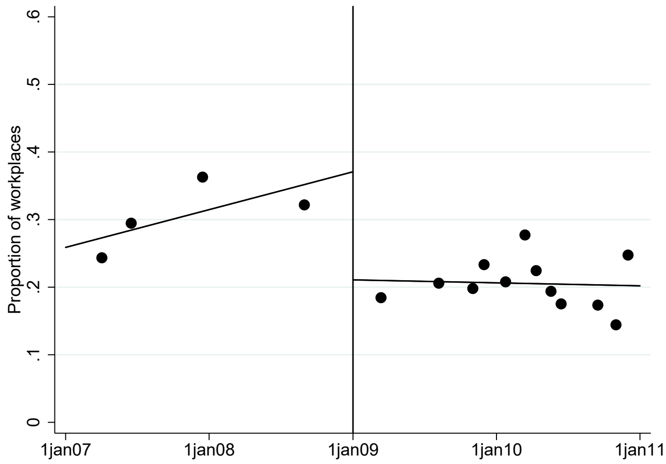
(b) Share of surveyed workers who declare to be union members (workplace average)



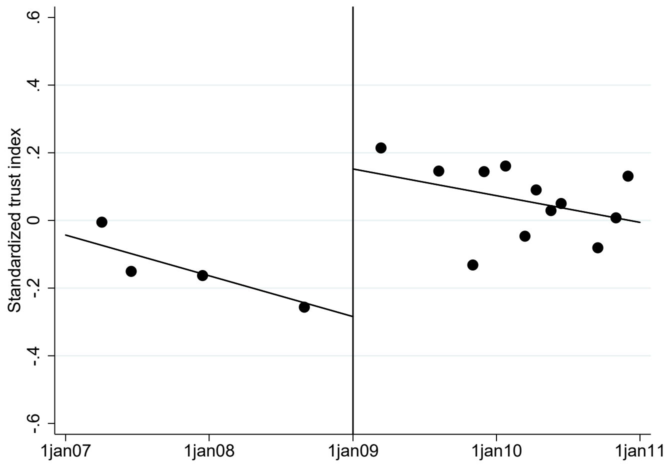
Notes: See Figure 2. The answers of individual workers are averaged by workplace; only workplaces for which an employer has been also surveyed are included (core sample).
Source: See Figure 2.

Figure 4: Impact of having a professional election under the new legal regime on employers' and employees' perceptions of unions in 2011

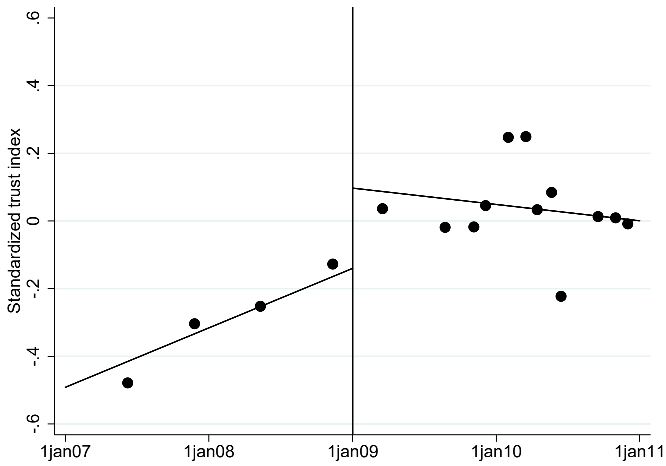
(a) Employer perceives unions representativeness as very weak



(b) Employers' trust in unions in their workplace

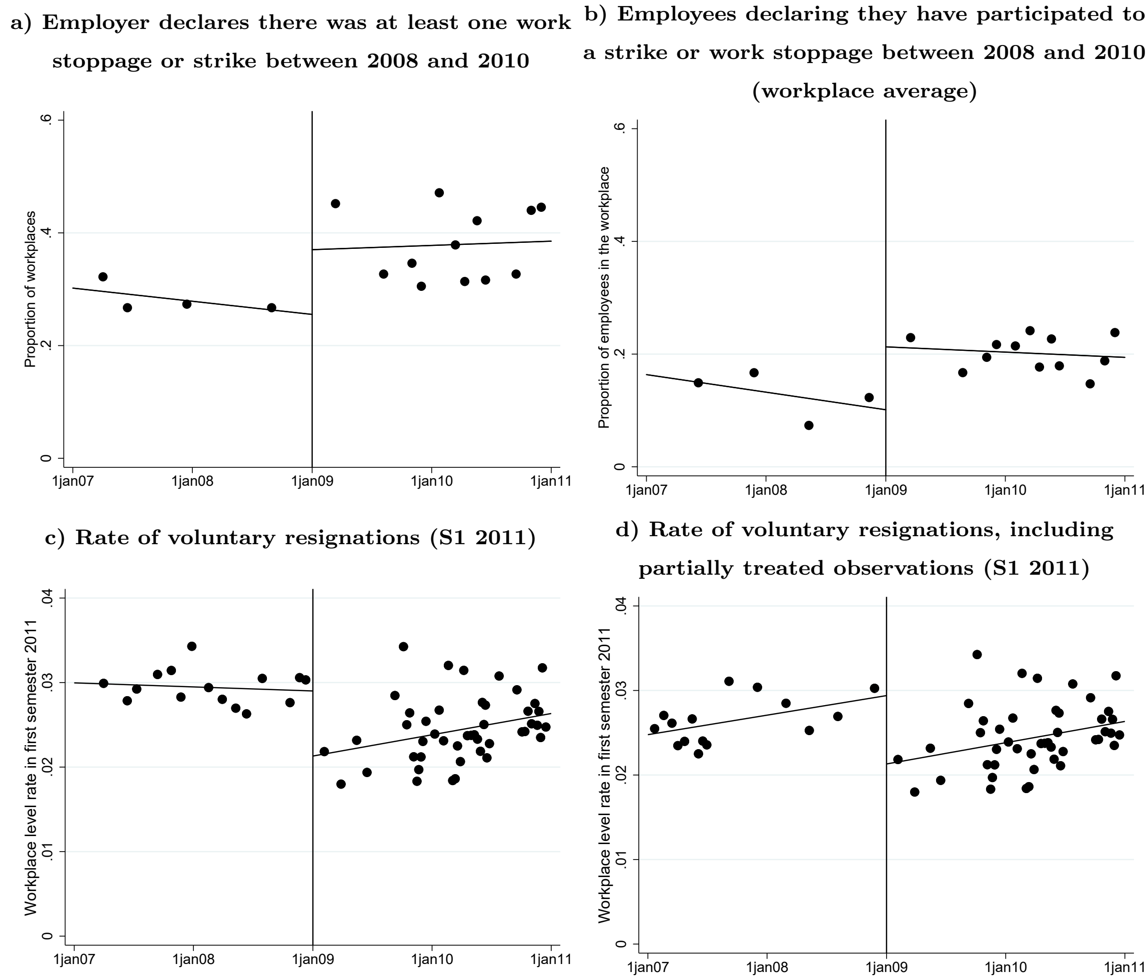


(c) Employees' trust in unions in their workplace (workplace average)



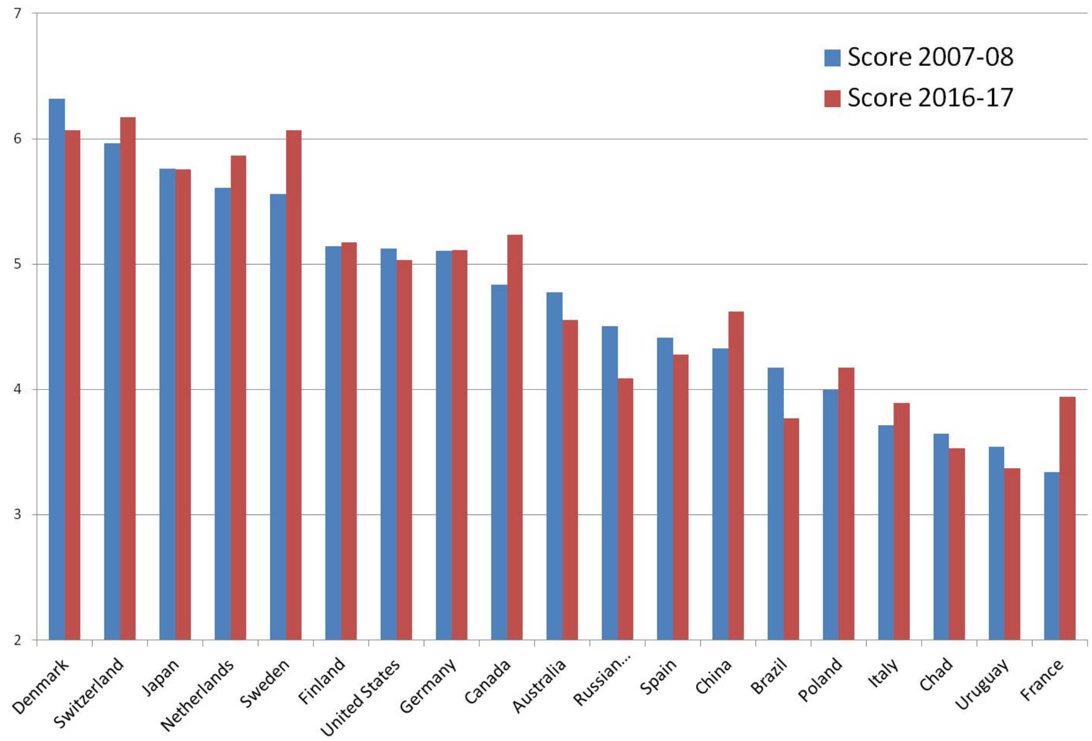
Notes and source: see Figure 2. The answers of individual workers are averaged by workplace; only workplaces for which an employer has been also surveyed are included (core sample).

Figure 5: Impact of having a professional election under the new legal regime on social conflicts and voluntary resignation



Notes and source: Each bin provides the mean of the interest variable for establishments experiencing their last professional elections around the date of the bin. In panels a) and b), 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. In panels c) and d), observations are split in 16 equal-size groups at the left of the cutoff date, and 48 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. In panel b), the answers of individual workers in REPONSE11 are averaged by workplace and only workplaces for which an employer has been also surveyed are included (core sample). In panels c) and d), we measure voluntary resignation rates at workplace level in the first semester of 2011. See details in Appendix B.
Source: Our own computations from the MARS administrative dataset matched with REPONSE11 (panels a and b) or matched with the DMMO/EMMO (panels c and d).

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

	Before the 2008 reform	*After the 2008 reform*
1) Who can participate to the first ballot of professional elections?	• 5 unions considered *de jure* representative for historical reasons • Other unions if they can prove they are representative in the firm (difficult in practice)	•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.
2) Which unions are eligible for firm-level bargaining?	• 5 unions considered *de jure* representative for historical reasons • Other unions if they can prove they are representative in the firm (difficult in practice)	•All unions that attracted at least 10% of vote casts at the first round of professional elections
3) Who can be appointed by eligible unions as union delegate for bargaining?	• Any worker in workplace/firm with 50+ employees; • An elected worker's delegates in firm with 11 to 49 employees	•Any worker who obtained at least 10% of vote casts at the first round of professional elections
4) Which unions are representative for bargaining at the industry level?	• 5 unions considered *de jure* representative for historical reasons • Other unions if they can prove (in court) their representativeness in many or major firms of the industry (rare in practice)	• Unions that attracted at least 8% of vote casts at the first round of all firm-level professional elections in the industry
5) Which unions are representative for bargaining at the national level?	• 5 unions considered *de jure* representative for historical reasons	• Unions that attracted at least 8% of vote casts at the first round of all firm-level professional elections in the country

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 bias-cor. estim.	Robust p val	Band. size (days)	N obs in band.
Panel A: REPONSE11 survey (S1 2011)						
Industries						
Manufacturing	1911	0.292	0.084	0.362	874	755
Construction	1911	0.054	0.061	0.198	842	701
Trade	1911	0.161	0.083	0.344	440	257
Market services	1911	0.320	-0.104	0.309	952	919
Non-market servicess	1911	0.173	-0.144	0.228	516	278
Workplace size groups (in December 2008)						
10-49 employees	1911	0.230	-0.043	0.663	844	703
50-249 employees	1911	0.445	0.015	0.902	686	553
250-999 employees	1911	0.270	0.064	0.529	888	788
More than 1000 employees	1911	0.054	-0.009	0.856	538	297
Workplace age (in 2011)						
5-9 years	1911	0.082	-0.166	0.129	386	229
10-19 years	1911	0.201	0.148	0.199	538	297
20-49 years	1911	0.452	-0.212	0.117	552	313
More than 50 years	1911	0.264	0.174	0.126	650	483
Paris region	1911	0.193	-0.071	0.464	628	434
Belongs to single-plant firm	1911	0.394	-0.099	0.434	686	553
Interviewee is a woman	1911	0.396	0.098	0.358	1006	981
Panel B: DMMO/EMMO S1 2011						
Industries						
Agriculture	12304	0.006	0.007	0.516	462	1795
Manufacturing	12304	0.286	-0.024	0.640	484	1844
Construction	12304	0.077	0.020	0.496	340	1388
Trade	12304	0.158	-0.009	0.799	408	1640
Market Services	12304	0.287	-0.043	0.447	440	1749
Non market services	12304	0.186	0.019	0.769	300	1183
Workplace/unit size groups (in December 2008)						
10-49 employees	12304	0.205	-0.019	0.663	576	2597
50-249 employees	12304	0.636	-0.035	0.534	494	1861
250-999 employees	12304	0.138	0.020	0.591	602	2878
More than 1000 employees	12304	0.021	0.039*	0.055	578	2597
Paris region	12304	0.215	-0.068	0.114	552	2203

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). The size of the bandwidth used for the estimation (but not the bias correction) and the number of observations in this bandwidth are also provided. In Panel A, the statistics are obtained using the REPONSE01 survey. In panel B, they are obtained using the DMMO/EMMO data for the first semester of 2011. Establishments of the same firm having their election the same day are aggregated. Those having elections during the period when outcomes are observed (partially treated units) are excluded. In both panels, workplace size comes from the DADS (see Appendix B.2 for details). 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.
Panel A: presence of workers' delegates, work councils and unions						
Workers' delegates or work council	0.933	0.876	0.090	0.106*	1911	919
			(0.055)	(0.064)		
At least one union recognized	0.659	0.578	0.213**	0.203**	1911	851
			(0.087)	(0.103)		
from historical unions only	0.645	0.568	0.186**	0.163	1911	909
			(0.086)	(0.100)		
from "new" unions only	0.109	0.087	0.098	0.115	1911	346
			(0.072)	(0.084)		
$\geq$ 2 unions recognized	0.440	0.391	0.118	0.114	1911	297
			(0.109)	(0.130)		
2 or 3 unions recognized	0.291	0.256	0.092	0.102	1911	569
			(0.090)	(0.109)		
5 unions or more recognized	0.058	0.102	-0.034	-0.039	1911	399
			(0.052)	(0.060)		
Panel B: unionization rate in the workplace						
Unionization rate (declared by employer)	0.106	0.056	0.078***	0.084**	1629	525
			(0.029)	(0.037)		
Share of workers union members (core sample of workers)	0.121	0.085	0.099**	0.097*	1586	657
			(0.042)	(0.052)		
Share of workers union members (larger sample of workers)	0.128	0.082	0.128***	0.143***	3042	940
			(0.035)	(0.041)		

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 size in days of the optimal bandwidths used for the estimation of the regression function and the bias of the regression are provided for the main outcomes respectively in Figures 7 and 8 of the appendix E.
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 REPONSE11. 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.
Panel A: Employers' perceptions						
Unions representativeness is very weak	0.245	0.383	-0.199**	-0.219*	1859	499
			(0.096)	(0.114)		
Trust in unions index	0.000	-0.240	0.458**	0.476**	1782	809
			(0.198)	(0.235)		
- Unions play a vital role	0.490	0.400	0.235**	0.253**	1878	537
			(0.100)	(0.122)		
- Unions provide a service	0.727	0.604	0.265***	0.289***	1849	523
			(0.088)	(0.104)		
- Unions interests not put ahead	0.414	0.368	0.144	0.178	1835	528
			(0.098)	(0.117)		
- Unions don't hinder running of firm	0.725	0.644	0.126	0.150	1858	547
			(0.095)	(0.113)		
Trust in workers delegate index	0.000	0.050	0.335*	0.403*	1862	462
			(0.192)	(0.230)		
Panel B: Workers' perceptions (core sample of workers)						
Trust in unions index	0.000	0.092	0.233	0.229	1453	188
			(0.268)	(0.328)		
- Unions play a vital role	0.635	0.667	0.120	0.146	1527	197
			(0.091)	(0.109)		
- Unions provide a service	0.697	0.700	0.089	0.106	1531	301
			(0.078)	(0.096)		
- Unions interests not put ahead	0.475	0.497	0.030	0.052	1508	208
			(0.101)	(0.119)		
- Unions don't hinder running of firm	0.714	0.757	-0.006	-0.007	1510	224
			(0.099)	(0.118)		
Trust in workers delegate index	0.000	0.562	0.008	0.082	1427	176
			(0.289)	(0.344)		
Panel C: Workers' perceptions (larger sample of workers)						
Trust in unions index	0.000	-0.002	0.275	0.319	2784	621
			(0.168)	(0.194)		
- Unions play a vital role	0.646	0.616	0.180***	0.210***	2938	406
			(0.068)	(0.079)		
- Unions provide a service	0.702	0.678	0.137**	0.155**	2946	555
			(0.060)	(0.071)		
- Unions interests not put ahead	0.469	0.503	-0.003	0.004	2892	1149
			(0.057)	(0.067)		
- Unions don't hinder running of firm	0.711	0.748	-0.006	-0.003	2883	923
			(0.054)	(0.065)		
Trust in workers delegate index	0.000	0.343	0.008	0.058	2717	357
			(0.193)	(0.225)		

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See notes of 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.
Panel A: Conflicts and social climate (declared by employer), quits						
Work stoppage	0.343	0.232	0.222**	0.260**	1911	422
(any kind)			(0.103)	(0.122)		
- Strike of 2 days or more	0.071	0.076	0.010	0.005	1911	586
			(0.051)	(0.061)		
- Intermittent strike	0.030	0.020	-0.016	-0.017	1911	399
			(0.020)	(0.022)		
- Strike of 1 day or less	0.213	0.169	0.094	0.121	1911	652
			(0.078)	(0.091)		
- Walkout	0.251	0.054	0.323***	0.361***	1911	282
			(0.092)	(0.101)		
Social climate	0.000	0.097	-0.290	-0.310	1910	453
			(0.192)	(0.229)		
Panel B: Workers' participation to work stoppages and job satisfaction						
Workplace averages on the core sample of employees:						
Participation to a work stoppage	0.178	0.135	0.103	0.110	1579	353
(any kind)			(0.066)	(0.085)		
Job satisfaction index	0.000	0.256	-0.029	0.027	1584	216
			(0.241)	(0.292)		
Workplace averages on the larger sample of employees:						
Participation to a work stoppage	0.190	0.173	0.045	0.050	3020	964
(any kind)			(0.048)	(0.057)		
Job satisfaction index	0.000	0.155	-0.108	-0.138	3033	668
			(0.159)	(0.191)		

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

Table 6: LATE of the reform on workers resignations and economic outcomes

	Sample Mean	Estim. left of thresh.	RD conv. estim.	RD BC estim.	N obs	N obs in band.
Panel A: Voluntary Resignations (from DMMO/EMMO)						
Voluntary resignations in S1 2011:						
- Baseline estimate	0.026	0.030	-0.010***	-0.011**	12302	2382
			(0.004)	(0.004)		
- Keeping partially treated obs.	0.025	0.031	-0.010***	-0.012***	16356	2586
			(0.004)	(0.004)		
- Adding quits by mutual agree.	0.030	0.038	-0.014***	-0.015***	12302	2511
			(0.004)	(0.005)		
Voluntary resignations in S2 2011	0.029	0.040	-0.010	-0.013*	15100	1656
			(0.006)	(0.007)		
Whole year 2011	0.053	0.076	-0.025**	-0.027**	11251	1474
			(0.011)	(0.013)		
Voluntary resignations in S1 2010	0.022	0.032	-0.007	-0.008	11116	1260
			(0.007)	(0.009)		
Voluntary resignations in S2 2010	0.027	0.038	-0.009	-0.010	12096	1257
			(0.008)	(0.010)		
Voluntary resignations in S1 2008	0.035	0.032	0.003	0.006	8776	2964
			(0.007)	(0.009)		
Panel B: Economic outcomes in 2011 (from FARE)						
Economic performance:						
- Log Value-added per empl.	4.103	4.113	-0.015	-0.028	17885	4560
			(0.050)	(0.060)		
- TFP	2.991	2.981	-0.032	-0.054	17586	2446
			(0.055)	(0.063)		
Wages:						
- Log wage bill per empl.	3.526	3.544	-0.013	-0.016	18135	4472
			(0.035)	(0.042)		
- Labor share	0.820	0.818	-0.005	0.002	18277	7156
			(0.033)	(0.039)		
Investments						
- Total investment	123.6	121.4	14.3	9.4	18305	2948
(in thousands €)			(16.641)	(18.935)		
- Investment over value-added	0.055	0.079	-0.029	-0.023	18277	6645
			(0.046)	(0.058)		
Financial performance:						
Profits	1002.0	1081.1	-36.7	-158.4	18306	2165
(in thousands €)			(383.900)	(428.748)		
Returns on Assets	0.036	0.037	-0.003	-0.002	18166	6324
			(0.012)	(0.015)		
Returns on Equity	0.247	0.317	0.162	0.137	18126	2513
			(0.142)	(0.160)		

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See notes of Table 3 for more details. Firms created after 2006 or having professional elections every two years are excluded. Unless otherwise stated, workplaces/firms having elections during the period when outcomes are observed (partially treated units) are excluded. The exact definition of each dependent variable is provided in Appendix B. For employment, we consider the number of full-time equivalent workers.

Table 7: Unionization rate (in %) in France 2005-2016: sectors affected and not affected by the 2008 reform

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

Notes: CEOs are excluded in all samples. Sources: REPONSE survey 2005 and Survey on *Sources de Revenu et Conditions de Vie* (SRCV) 2008, 2010, 2013 and 2016. From 2008 to 2013, SRCV was the official source for the French unionization rate. Statistics from SRCV are weighted to account for the population gender*age structure.
Panel B shows estimates after applying a propensity score reweighting to keep the distribution of workers' characteristics (age, age squared, gender, education in 8 groups, occupation in 10 groups, workplace size in 5 groups and sector in 15 groups) similar in 2010, 2013 and 2016 to their 2008 level. See details in the appendix 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 8: Heterogeneity of reform impacts

Panel A: Heterogenity according to firm characteristics

	At least one union recognized	Share of workers union members	Employer trust	Employee trust	Strike or work stoppage	Particpation to work stoppages
Workplace size						
100 employees	0.279*	0.176***	0.522	0.484	-0.227*	0.067
or less	(0.163)	(0.046)	(0.437)	(0.336)	(0.136)	(0.071)
more than	0.091	0.104	0.377*	0.173	0.284*	0.023
100 employees	(0.042)	(0.057)	(0.178)	(0.325)	(0.165)	(0.064)
Sector						
Trade and	0.241**	0.117*	0.452*	0.024	0.150	0.001
other Services	(0.113)	(0.066)	(0.240)	(0.284)	(0.107)	(0.062)
Manufacturing	0.180	0.103	0.553	0.970**	0.451**	0.407***
and construction	(0.192)	(0.098)	(0.479)	(0.479)	(0.221)	(0.143)

Panel B: Heterogenity according to workers' characteristics

	Share of workers union members		Employee trust		Participation to work stoppages	
	Mean	*Estimate*	*Mean*	*Estimate*	*Mean*	*Estimate*
Women	0.112	0.115**	0.071	0.142	0.166	0.100
		(0.048)		(0.208)		(0.077)
Men	0.133	0.084*	-0.081	0.187	0.201	0.054
		(0.048)		(0.168)		(0.055)
Age below median	0.101	0.126***	-0.071	0.250	0.179	0.088
		(0.048)		(0.164)		(0.055)
Age above median	0.156	0.081	0.044	0.031	0.207	-0.009
		(0.059)		(0.151)		(0.062)
Non tertiary Education	0.147	0.169***	0.040	0.132	0.217	0.005
		(0.047)		(0.161)		(0.063)
Tertiary Education	0.094	0.026	-0.083	0.448**	0.150	0.039
		(0.047)		(0.204)		(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

October 2022

List of Appendices

Appendix A Detailed institutional Settings

A.1 Insitutions 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.[A.1] 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).[A.2] 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.[A.3] 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

[A.1]For multi-establishement firms, there is representation at both the workplace and firm levels according to the same regulations (in terms of size thresholds, etc.).

[A.2]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.

[A.3]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*.[A.4] 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.[A.5] 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[A.6] 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.[A.7]

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

[A.4] 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.

[A.5] Bargaining on several other themes such as gender equality or union rights within the firm is also mandatory but at a larger frequency.

[A.6] 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.

[A.7] 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.[A.8]

[A.8] 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$^{\text{th}}$ 2008 law. This is because the content of the law was only know on April 9$^{\text{th}}$ 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 Construction of the date of the latest professional election before a given pre-specified date

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*).[A.9] 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 is crucial to recover election dates for elections that took place before 2009.

The August 20th 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.[A.10]

Our algorithm to construct the date of the latest professional election before a given date d_0 (the date when an employer is interviewed in the REPONSE survey in a given workplace or the beginning of an observation period in DMMO/EMMO and FARE) 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:

[A.9] 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.

[A.10] The data also includes information on partial elections. We discard them as the law exclude to use them to determine the representativeness of unions.

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 d_0.
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.

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).[A.11]

In the employer part of REPONSE11, d_0 is the date of interview of the manager. For the employee part, d_0 is set on April 1$^{\text{st}}$ 2011. In addition, for the employee part, workplaces for which there are election dates between April 1$^{\text{st}}$ and July, 22$^{\text{sd}}$, 2011 are removed unless these dates concern only elections for workers' delegates and there is a relevant election for work councils before April 1$^{\text{st}}$ 2011. The exact procedure used with the DMMO/EMMO and FARE data is detailed in subsections B.4 and B.5.

Construction of a proxy for the expected election date. The algorithm described above is also applied to construct a proxy for the expected election date that is used as an alternative running variable. To construct this proxy, we start by replacing all election dates in MARS by the date at which the election *should* have occurred. This is done by considering the time span in years between the previous and current election, rounding this time span to the closest integer(two, three or four years), and adding it to the previous election date. We then re-run the whole algorithm. When the most recent election appears to be after 1 January 2009, it will by construction be an expected date

[A.11] This last imputation has no impact on our results.

rather than actual one. In contrast, when the most recent election is before 1 January 2009, it will correspond to a former election date for one election in the data, and no changes to the date would have been made (because we cannot observe the date of an election that occurred before an election that itself occurred before 1 January 2009).

Explaining the distribution of the date of most recent election before the REPONSE employer survey. 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 in 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 3rd 2005. [A.12] This second change implied for example that, absent of firm- or industry-level agreement, workplaces that should have had elections in 2006, 2008 and 2010 only had elections in 2006 and 2010. The first one may also have had long-term consequences that contribute to explain the shape of the distribution in Figure 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.2 Variables of interest in REPONSE11

This section details the construction of the main control and 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.[A.13]

[A.12] These regulations did not change the length of ongoing mandates and only applied to subsequent mandates.

[A.13] See https://www.niesr.ac.uk/projects/employment-relations-britain-and-france

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

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 or 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.

Controls

Workplace size comes from the *Déclaration Annuelles de Données Sociales* (DADS) which are social security records including information on job contracts of all employees in all French firms. This information is used to reconstruct workplace size. The construction of other control variables is straightforward.

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 +/-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.[A.14] For consistency, we keep only SRCV for our analysis from 2008 to 2016, but using the working conditions survey in 2016 instead would not alter our qualitative conclusions of a declining (increasing) union membership in the public (private) sector over the studied period. SRCV also provides information on the size of the workplace, its industry and the tenure of the worker in this workplace. We thus use the SRCV 2008, 2010, 2013, 2016 for providing consistent trends of union membership from 2008 to 2016.

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

[A.14]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.

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 REPONSE 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 7).

The non-weighted share of workers in REPONSE 2005 that member of a union is 12.1%. However the REPONSE 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 REPONSE 2005 by the ratio between the unionization rate in SRCV2010 on a sample corresponding to the REPONSE11 sample (11.40%, see last row of panel A of Table 7) and the non-weighted unionization rate in the REPONSE11 employee survey *among workplaces with 20 employees or more only*

(11.29%). This calibration corrects both for observed differences between the REPONSE 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 7).

We have also used the declaration of employers regarding their workplace unionization rate to get an alternative estimate. These are obtained both in REPONSE 2011 and REPONSE 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. Diving 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 REPONSE11 *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 REPONSE11 (11.40%) and the estimate obtained for workplaces with more than 20 employees using the REPONSE11 *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 7 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 7. 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:
$w_{it} = sw_{it}$ if $t = 2008$
$w_{it} = sw_{it} * \frac{1-p_{it}}{p_{it}} / \frac{1-p_{t'}}{p_{t'}}$ 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 7, panel B the weighted average of the unionization rate in each year t' using w_{it} as weights.

B.4 Data construction in DMMO/EMMO

Unit of observation. We group together and consider as a single unit of observation all establishments within the same firm that have their election at the same date. This is because these establishments cannot be considered as independent observations since they coordinate their elections. Considering them separately leads to mass points in the running variable and unbalances in covariates (because e.g. 1,200 establishments belonging to the same large French bank have their election the same day) and prevents us from convincingly implementing the RDD design.

Periods of observation. In practice, in 2012, all firms will eventually be treated, while at the beginning of 2009, none of them are. This implies that we restrict our attention to years 2011 and 2010. For consistency with the survey outcomes that are measured during the first semester of 2011, we consider in baseline analysis quits during the first semester of 2011. In robustness checks, we present evidence for other semesters in 2010 and 2011.

Measures of quit rates. The DMMO/EMMO are establishment-level quarterly data. While DMMO is exhaustive for establishments above 50 employees, EMMO is a rotative survey for establishments below 50 employees. To increase the number of observations, we typically consider periods longer than a quarter (e.g. a semester or a year). This also limits the number of establishments where no quits and having a quit rate equal to 0, increasing our capacity to detect a possible effect of the reform on quits. Our baseline measure of quit rate in each unit of observation during a given semester is obtained by dividing the total number of quits during the semester (which is the sum of the quits in the two quarters corresponding to the semester) by the average of the number of employees (eff_ref) working in the establishment during each of the two quarters of that semester. We construct measures of quit rates for 2010, S1 and S2 and 2011, S1 and S2. We similarly construct measures of the annual quit rate for years 2010 and 2011. We considered alternative measures such as the average of the quarterly quit rate over the quarters within the considered semester. We also used alternative measures of the quit rate by dividing by the number of employees at the beginning of the observation period rather than the average number of employees during the period. Results are robust to using all these alternative measures (results available upon request). In baseline specifications, we exclude a few observations with quit rates strictly larger than 1 as such mass flows of workers are unlikely to be related to the reform and may represent measurement error in the data. This trimming has no incidence on the final results. We finally perform a placebo test with the quit rate in the first semester of 2008 (i.e. prior to the reform). This rate is constructed similarly for all establishments observed in DMMO/EMMO in the first semester of 2011 that are also observed in the first semester of 2008.

Covariates. Almost all covariates used for balancing tests and in RDD specifications with controls are directly retrieved from the DMMO/EMMO data for the period of observation. The only exception is establishment size which is potentially endogenous to the reform. For this reason, for each establishment observed in the DMMO/EMMO during a given time period (e.g. 2011, S1), we retrieve its size in 2008 (prior to the reform) in the establishment-level DADS data, to which we also have access. The establishment-level DADS data provides administrative information on workers characteristics aggregated at establishment level from social security records. It is exhaustive, allowing us to recover the size of all establishments in DMMO/EMMO prior to the reform.

Partially treated units. The running variable is the date of latest staff election before an outcome is observed. It is therefore specific to each time period considered. If we examine for example quits during the third trimester of 2010, we use as running variable the latest election before July 1st 2010. Our codes allow us to retrieve the date of the latest election before each prespecified date, so that they can be adapted to various time periods. An additional complexity with the administrative data sources is that outcomes are not measured at a specific date but observed during a given time period (a calendar year in FARE and a trimester in DMMO). Hence, firms having an election during that period, and their former election before January 1st 2009 are partly treated: we measure their outcomes around the period they were treated. In our baseline specifications, we exclude these firms but include them back in robustness checks. We do so by computing the latest staff election before the end date of the considered period and remove firms for which (i) this date is after the beginning of the observed period, and (ii) the latest election date before the beginning of the observed period is before January 1st 2009.

Other sample restrictions. We exclude establishments having elections every two years as in 2011 these establishments can only be at the right of the cutoff and are likely to induce noise in our estimates. We retrieve firm creation dates from FARE, and exclude establishments belonging to a firm created in 2006 or later (establishment whose firm creation date is unknown are kept).

B.5 Data construction in FARE

Unit of observation. Elections are held at establishment level, a priori preventing an analysis of firm level outcomes such as firm economic performance. However, in practice many firms hold their staff elections the same day in all their establishments. For these firms, there is no ambiguity on the date at which they will start to be subject to the new legal regime in all their establishments. In practice, we construct the running variable at establishment level, and only keep firms for which the running variable is the same in all their establishments. The final sample includes all mono-establishment firms and a significant share of multi-establishment ones.

Periods of observation. We examine years 2010 and 2011.

Measures of economic and financial outcomes. We describe below how the main variable we examine are constructed.

Wage Bill: These are all salaried treatments paid to employees, including bonuses and financial participation schemes. They are net of employer contributions.

Labor share: It is defined as the wage bill plus employer social security contributions divided by firm value-added.

Profits: These are accounting profits reported in the tax returns.

Return on Equity: Profit divided by the value of equity reported in tax returns.

Return on Assets: Profit divided by the value of assets reported in tax returns.

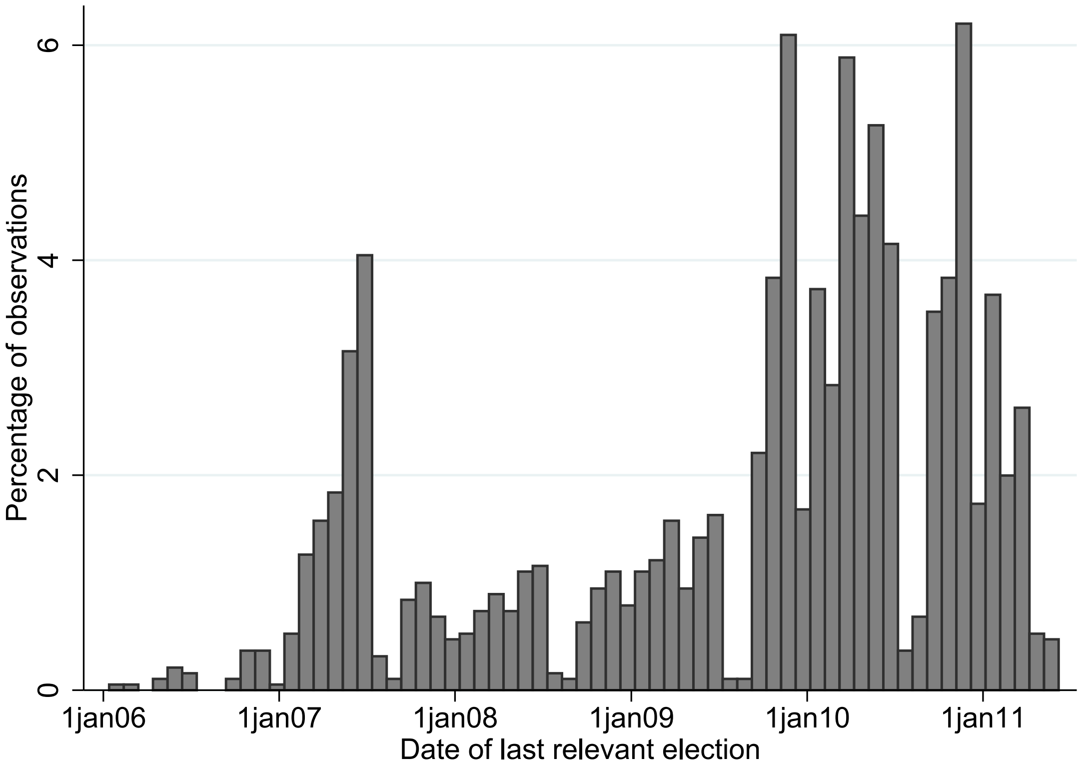
Total Factor Productivity (TFP): TFP is computed as the exponential of the residual of a regression of value added on tangible assets and FTE size (all variables in logarithm).

All constructed variables are windsorized at p01 and p99.

Partially treated units. We proceed as we do with the DMMO/EMMO data (see above).

Other sample restrictions. As with the DMMO/EMMO, we exclude firms whose establishments have elections every two years and firms created in 2006 or later.

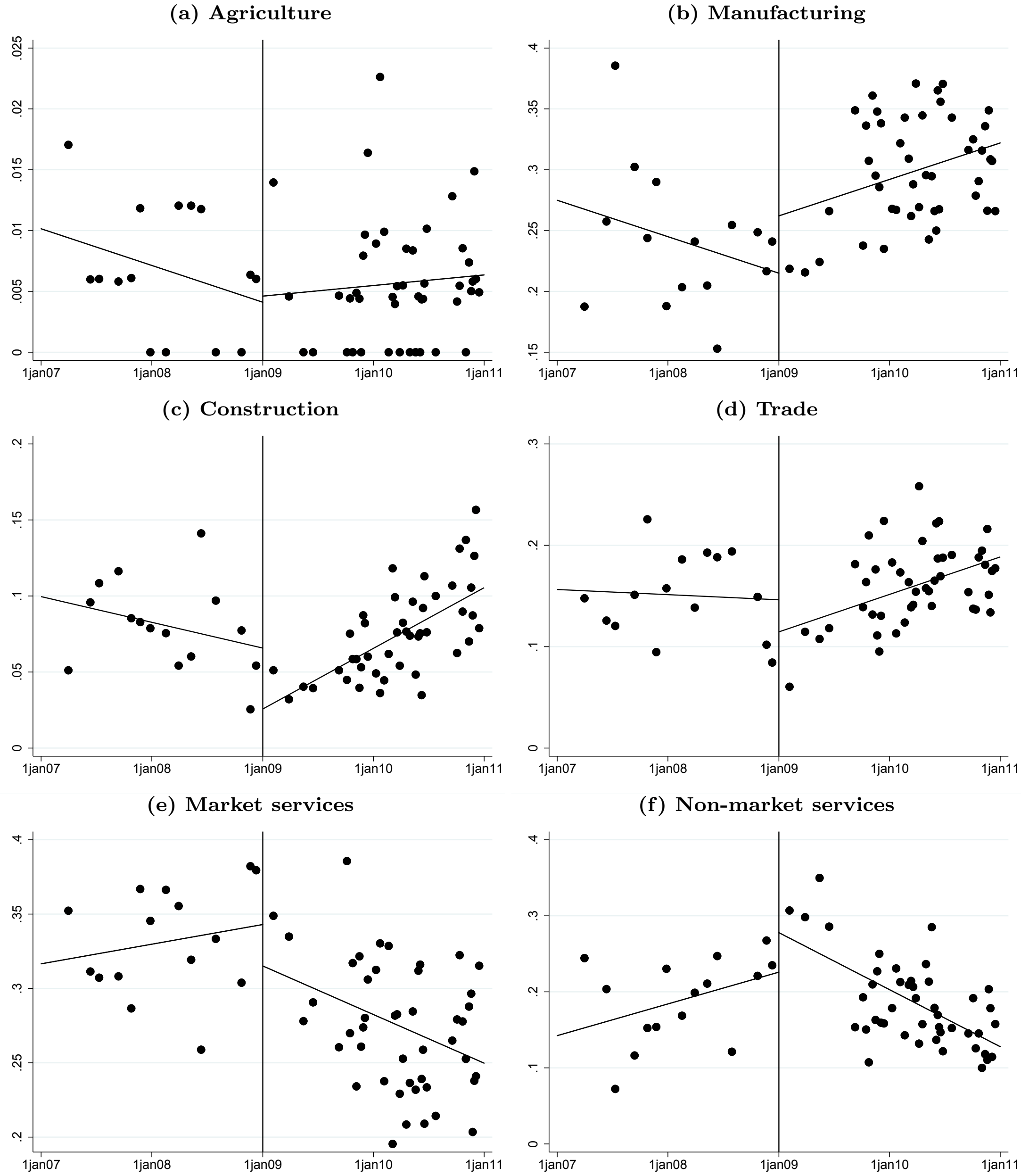
Figure B1: Distribution of the date of most recent election before the REPONSE employer survey



Notes: The figure represents the distribution of dates for the latest professional election before the REPONSE 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 REPONSE11

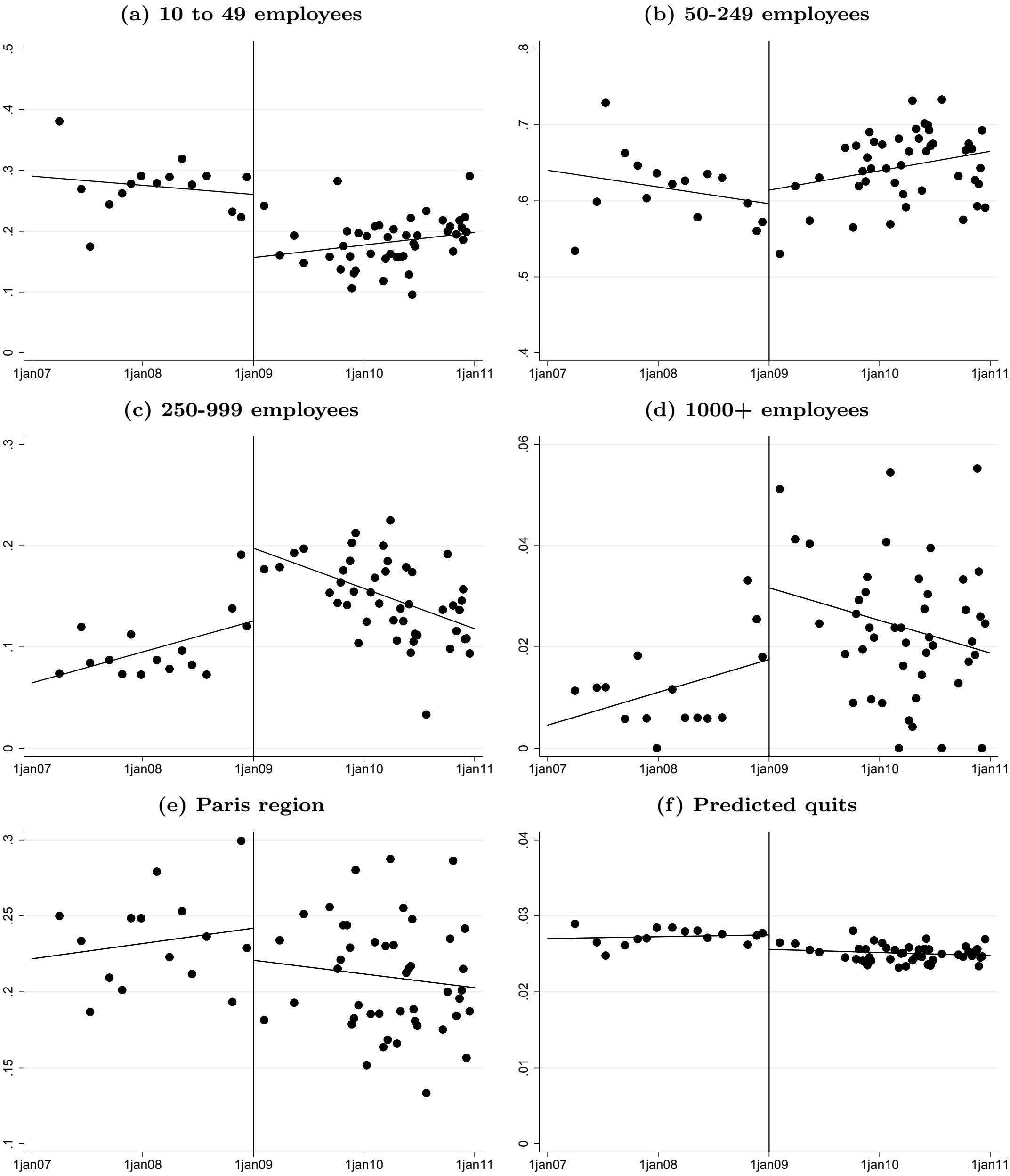
Appendix C Additional Figures and Tables

Figure C1: RD estimates for workplace industry in DMMO/EMMO



Notes: The Figure shows discontinuities in workplace industry around the cutoff date. Each bin provides the mean of the interest variable for establishments experiencing their last staff elections around the date of the bin; observations are split in 16 equal-size groups at the left of the cutoff date, and 48 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 staff elections every two years are excluded.
Source: DMMO/EMMO data (see Appendix B).

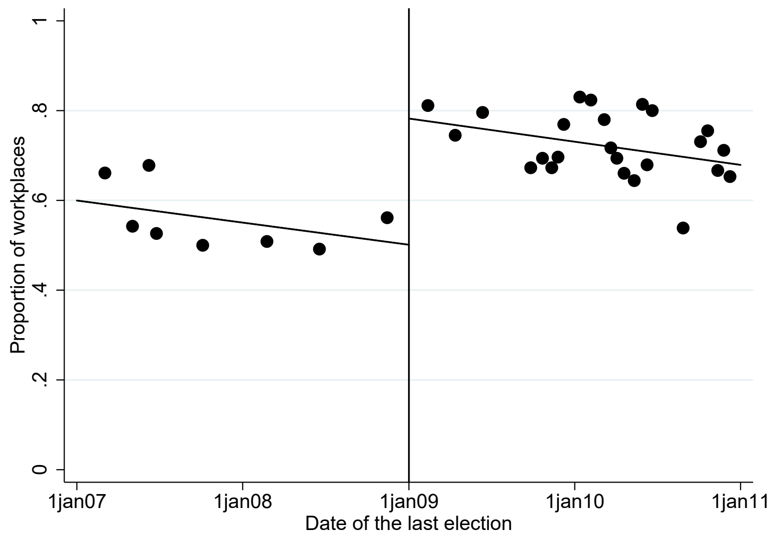
Figure C2: RD estimates for workplace size and region and quits predicted by covariates in DMMO/EMMO



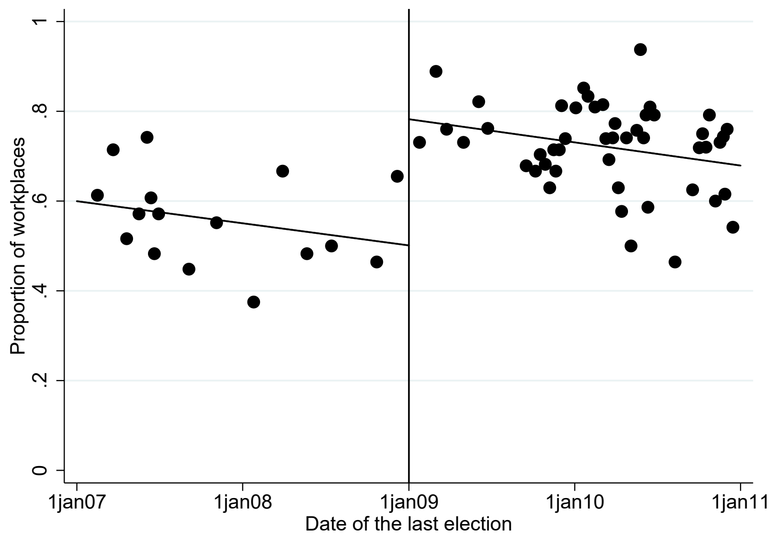
Notes: The Figure shows discontinuities in workplace size in late 2008 (subfigures a to d), a dummy for the workplace being in the Paris region (subfigure e), and quits in the first semester of 2011 predicted by workplace industry, size and region (subfigure f) around the cutoff date. Each bin provides the mean of the interest variable for establishments experiencing their last staff elections around the date of the bin; observations are split in 16 equal-size groups at the left of the cutoff date, and 48 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 staff elections every two years are excluded.
Source: DMMO/EMMO data (see Appendix B).

Figure C3: Impact of having a professional election under the new legal regime on union coverage in 2011: graphs with more bins

(a) 32 bins



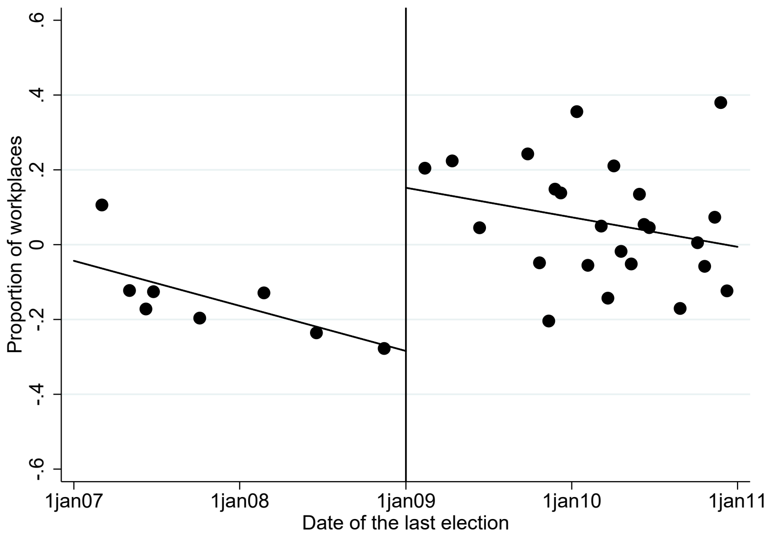
(b) 64 bins



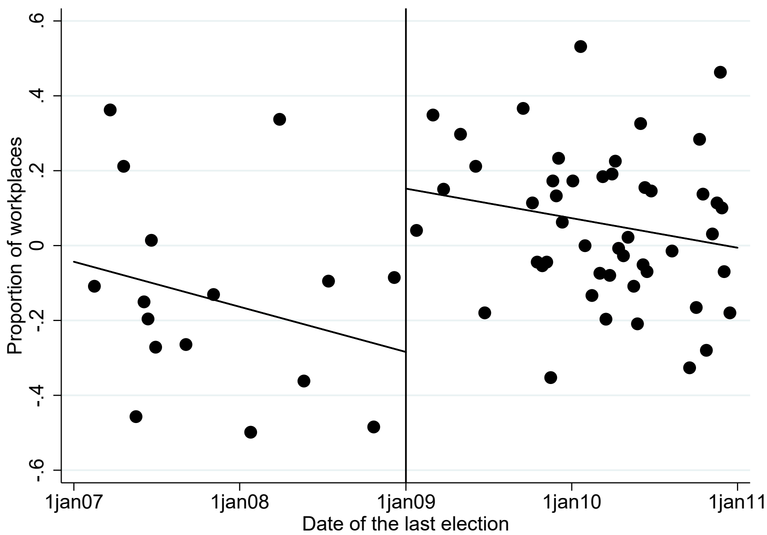
Notes: Union coverage is a workplace-level variable for having at least one union recognized for bargaining in the workplace. Observations are split in 8 (pane A) or 16 (panel B) equal-size groups at the left of the cutoff date, and 24 (panel A) or 48 (panel B) equal-sized bins at the right of this cutoff.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

Figure C4: Impact of having a professional election under the new legal regime on employers' perception of unions in 2011: graphs with more bins

(a) 32 bins

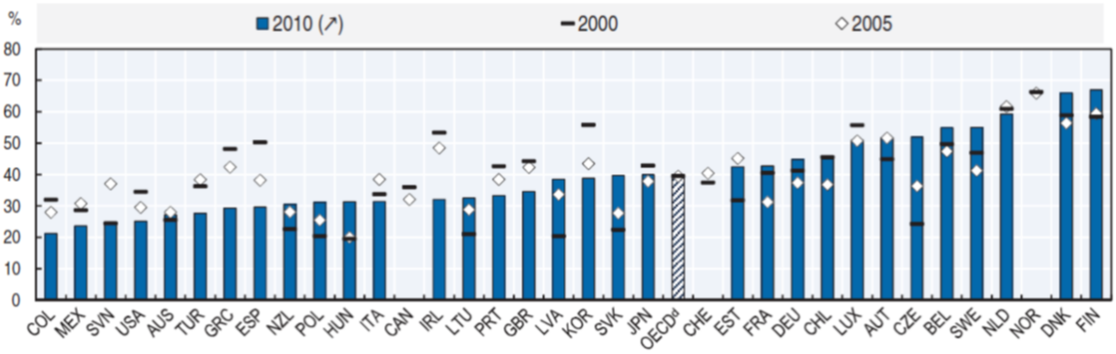


(b) 64 bins



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

Figure C5: 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.

Table C1: Descriptive statistics and analysis of discontinuities for covariates in firm accounts data (FARE 2011 and 2010)

	N obs	Mean	RD bias-cor. estim.	Robust p val	Band. size (days)	N obs in band.
Panel A: FARE 2011						
Industries						
Agriculture	18306	0.001	0.002	0.272	178	1090
Manufacturing	18306	0.280	-0.033	0.403	558	3516
Construction	18306	0.113	-0.064**	0.027	542	3296
Trade	18306	0.204	-0.006	0.876	452	2616
Market services	18306	0.322	0.082*	0.071	550	3422
Non market services	18306	0.080	0.024	0.477	452	2633
Workplace/unit size groups						
10-49 employees	18147	0.574	-0.024	0.616	508	2924
50-249 employees	18147	0.350	-0.015	0.736	506	2896
250-999 employees	18147	0.066	0.027	0.322	460	2647
More than 1000 employees	18147	0.010	0.006	0.521	736	6670
Panel B: FARE 2010						
Industries						
Agriculture	16541	0.001	0.002	0.235	204	1413
Manufacturing	16541	0.269	0.024	0.724	224	1537
Construction	16541	0.109	-0.048	0.263	274	1713
Trade	16541	0.208	-0.029	0.597	226	1537
Market services	16541	0.332	0.002	0.973	248	1617
Non market services	16541	0.081	0.053	0.211	270	1703
Workplace/unit size groups						
10-49 employees	16485	0.580	0.002	0.972	312	1852
50-249 employees	16485	0.344	-0.044	0.470	316	1886
250-999 employees	16485	0.066	0.051	0.222	236	1577
More than 1000 employees	16485	0.010	-0.001	0.961	194	1339

Notes: The Table reports in different rows the sample number of non-missing observations and sample mean for the main firm-level covariates, as well as bias-corrected RDD estimates and their associated robust p-values following Calonico et al. (2014). The size of the bandwidth used for the estimation (but not the bias correction) and the number of observations in this bandwidth are also provided. The statistics are obtained using the FARE data for year 2011 in panel A and the FARE data for year 2010 in panel B. Firms having elections during the period when outcomes are observed (partially treated units) are excluded. 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 C2: LATE of the reform on the profile of Union Delegate interviewed in the third part of the REPONSE survey

	Sample Mean	Estim. left of thresh.	RD conv. estim.	RD BC estim.	N obs	N obs in band.
Union delegate (UD) is a woman	0.293	0.230	0.224	0.309	659	88
			(0.197)	(0.226)		
UD age	48.065	47.417	2.861	2.776	659	81
			(4.544)	(5.398)		
UD has tertiary education	0.337	0.535	-0.308	-0.277	658	96
			(0.208)	(0.246)		
First year UD took a mandate	1997.303	1995.881	3.351	4.029	657	81
			(5.028)	(5.857)		

Notes: Estimates are based on the REPONSE worker representative survey which is used to examined the profile of the interviewed union delegates depending on the date of the last staff election. Only representatives declaring being a union delegate in the survey are kept. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See notes of Table 3 for more details.

Appendix D Falsification tests

Table D1: RD estimates for main outcomes of interest for a fake reform applying on January 1st 2010

	Sample Mean	Estim. left of thresh.	RD conv. estim.	RD BC estim.	N obs	N obs in band.
Panel A: Employers main outcomes (REPONSE11)						
At least one union recognized	0.659	0.760	0.118	0.118	1911	647
			(0.084)	(0.103)		
Trust in unions index	0.000	0.141	0.065	0.088	1782	603
			(0.205)	(0.252)		
Unionization rate	0.106	0.130	0.018	0.013	1629	539
			(0.040)	(0.049)		
Work stoppage (any kind)	0.343	0.293	0.190**	0.205**	1911	777
(between 2008 and 2010)			(0.083)	(0.099)		
Panel B: Workers' main outcomes (core sample of workers, REPONSE11)						
Share of workers union members	0.121	0.099	0.055	0.059	1586	731
(from workers responses)			(0.046)	(0.055)		
Trust in unions index	0.000	0.043	0.270	0.299	1453	694
			(0.186)	(0.224)		
Participation to a work stoppage	0.178	0.231	0.005	-0.006	1579	750
			(0.058)	(0.070)		
Panel C: Workers' main outcomes (larger sample of workers, REPONSE11)						
Share of workers union members	0.128	0.098	0.044	0.041	3042	1102
(from workers responses)			(0.036)	(0.044)		
Trust in unions index	-0.000	-0.111	0.177	0.139	2784	988
			(0.168)	(0.208)		
Participation to a work stoppage	0.190	0.197	0.041	0.043	3020	1453
			(0.038)	(0.046)		
Panel D: Rate of voluntary resignations in S1 2011 (DMMO/EMMO)						
Voluntary resignations in S1 2011	0.026	0.024	0.002	0.001	12279	3491
			(0.004)	(0.005)		

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 1st 2009 to January 1st 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 REPONSE11. 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. For voluntary resignations, workplaces adopting the new legal regime during the first semester of 2011 (when the outcome is measured) are excluded.

Table D2: RD estimates for main outcomes of interest for a fake reform applying on April 15th 2009

	Sample Mean	Estim. left of thresh.	RD conv. estim.	RD BC estim.	N obs	N obs in band.
Panel A: Employers main outcomes						
At least one union recognized	0.659	0.839	-0.059	-0.073	1911	589
			(0.084)	(0.099)		
Trust in unions index	0.000	0.314	-0.108	-0.156	1782	519
			(0.196)	(0.229)		
Unionization rate (declared by employer)	0.106	0.121	0.027	0.023	1629	587
			(0.030)	(0.035)		
Work stoppage (any kind)	0.343	0.508	-0.093	-0.118	1911	604
(between 2008 and 2010)			(0.103)	(0.122)		
Panel B: Workers' main outcomes (core sample of workers)						
Share of workers union members	0.121	0.171	0.023	0.036	1586	431
(from workers responses)			(0.062)	(0.072)		
Trust in unions index	-0.000	0.035	-0.127	-0.080	1453	369
			(0.219)	(0.251)		
Participation to a work stoppage	0.178	0.251	-0.048	-0.048	1579	448
			(0.075)	(0.090)		
Panel C: Workers' main outcomes (larger sample of workers)						
Share of workers union members	0.128	0.213	-0.031	-0.032	3042	1002
(from workers responses)			(0.044)	(0.051)		
Trust in unions index	-0.000	0.145	-0.042	0.027	2784	860
			(0.188)	(0.226)		
Participation to a work stoppage	0.190	0.231	-0.026	-0.029	3020	1056
			(0.051)	(0.061)		
Panel D: Rate of voluntary resignations in S1 2011 (DMMO/EMMO)						
Voluntary resignations in S1 2011	0.026	0.016	0.004	0.005	12279	3491
			(0.003)	(0.003)		

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 1st 2009 to April 15st 2009. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See notes of Table D1 for more details on the implementation of the RDD. For voluntary resignations, workplaces adopting the new legal regime during the first semester of 2011 (when the outcome is measured) are excluded.

Appendix E Robustness checks

Table E1: RD estimates for main outcomes of interest using a Difference-in-Discontinuity Design

	Sample Mean	Estim. left of thresh.	RD conv. estim.	RD BC estim.	N obs	N obs in band.
Panel A: Employers main outcomes (REPONSE11)						
At least one union recognized	0.659	0.528	0.231***	0.149	1907	1828
			(0.064)	(0.095)		
Trust in unions index	0.000	-0.256	0.416***	0.394*	1778	1702
			(0.147)	(0.217)		
Unionization rate	0.106	0.064	0.058***	0.054*	1625	1557
			(0.019)	(0.029)		
Work stoppage (any kind)	0.343	0.270	0.072	0.097	1907	1828
(between 2008 and 2010)			(0.063)	(0.095)		
Panel B: Workers' main outcomes (core sample of workers, REPONSE11)						
Share of workers union members	0.103	0.077	0.092***	0.098**	1583	1503
(from workers responses)			(0.031)	(0.046)		
Trust in unions index	0.000	-0.040	0.098	-0.044	1452	1380
			(0.147)	(0.192)		
Participation to a work stoppage	0.139	0.109	0.096**	0.063	1576	1496
			(0.042)	(0.062)		
Panel C: Workers' main outcomes (larger sample of workers, REPONSE11)						
Share of workers union members	0.128	0.080	0.096***	0.115***	1923	1825
(from workers responses)			(0.030)	(0.043)		
Trust in unions index	-0.000	-0.131	0.181	0.060	1771	1683
			(0.150)	(0.205)		
Participation to a work stoppage	0.190	0.136	0.084**	0.050	1914	1816
			(0.040)	(0.059)		
Panel D: Rate of voluntary resignations in S1 2011 (DMMO/EMMO)						
Voluntary resignations in S1 2011	0.026	0.029	-0.008***	-0.011**	12282	12220
(partially treated excluded)			(0.004)	(0.003)		
Voluntary resignations in S1 2011	0.025	0.029	-0.008***	-0.012***	16328	16075
(partially treated kept)			(0.003)	(0.003)		

Notes: The estimation is performed on a large bandwidth including 800 days on each side of the cutoff. Trimester of election fixed effects are included as controls as well as the distance between the election date and the closest January First (see equation in main text). 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 observations 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. For voluntary resignations, workplaces adopting the new legal regime during the first semester of 2011 (when the outcome is measured) are excluded.

Table E2: RD estimates for main outcomes of interest using a proxy for the expected latest election date as running variable

	Sample Mean	Estim. left of thresh.	RD conv. estim.	RD BC estim.	N obs	N obs in band.
Panel A: Employers main outcomes (REPONSE11)						
At least one union recognized	0.656	0.580	0.211**	0.215**	1937	810
			(0.088)	(0.102)		
Trust in unions index	0.000	-0.193	0.469**	0.511**	1782	755
			(0.185)	(0.212)		
Unionization rate	0.105	0.065	0.086***	0.098***	1653	538
			(0.030)	(0.038)		
Work stoppage (any kind)	0.343	0.269	0.292***	0.322***	1937	331
(between 2008 and 2010)			(0.103)	(0.115)		
Panel B: Workers' main outcomes (core sample of workers, REPONSE11)						
Share of workers union members	0.103	0.097	0.085	0.084	1586	247
(from workers responses)			(0.057)	(0.068)		
Trust in unions index	0.000	-0.028	0.452	0.520	1448	202
			(0.281)	(0.332)		
Participation to a work stoppage	0.139	0.134	0.186**	0.212**	1576	235
			(0.085)	(0.102)		
Panel C: Workers' main outcomes (larger sample of workers, REPONSE11)						
Share of workers union members	0.130	0.102	0.089**	0.100**	3040	725
(from workers responses)			(0.037)	(0.045)		
Trust in unions index	-0.000	0.020	0.250	0.294	2769	379
			(0.205)	(0.240)		
Participation to a work stoppage	0.190	0.152	0.089	0.101	3015	453
			(0.060)	(0.071)		
Panel D: Rate of voluntary resignations in S1 2011 (DMMO/EMMO)						
Voluntary resignations in S1 2011	0.026	0.031	-0.010***	-0.011**	13029	2184
(partially treated excluded)			(0.004)	(0.004)		
Voluntary resignations in S1 2011	0.025	0.031	-0.011***	-0.012***	16565	3231
(partially treated kept)			(0.003)	(0.004)		

Notes: 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. When the date of the latest staff election before outcomes are measured is anterior to 1 January 2009, it is used as running variable. However, when this date is posterior to 1 January 2009, it is replaced by the expected date of the election which is constructed using the date of the previous election and information on mandate duration (see details in Appendix B.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 (see Calonico et al. (2019)). The Table also provides the number of observations 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 E3: RD estimates for main outcomes of interest when control variables are included

	Sample Mean	Estim. left of thresh.	RD conv. estim.	RD BC estim.	N obs	N obs in band.
Panel A: Employers main outcomes						
At least one union recognized	0.659	0.578	0.183**	0.193**	1911	765
			(0.072)	(0.085)		
Trust in unions index	0.000	-0.264	0.422**	0.454*	1782	513
			(0.195)	(0.232)		
Unionization rate	0.106	0.059	0.045	0.045	1629	301
			(0.034)	(0.042)		
Work stoppage (any kind)	0.343	0.227	0.177*	0.197*	1911	267
(between 2008 and 2010)			(0.098)	(0.113)		
Panel B: Workers' main outcomes (core sample of workers)						
Share of workers union members	0.103	0.090	0.072	0.067	1587	217
			(0.054)	(0.064)		
Trust in unions index	0.000	0.269	0.174	0.203	1455	179
			(0.229)	(0.271)		
Participation to a work stoppage	0.139	0.132	0.084	0.076	1580	249
			(0.064)	(0.077)		
Panel C: Rate of voluntary resignations in S1 2011 (DMMO/EMMO)						
Voluntary resignations in S1 2011	0.026	0.030	-0.009**	-0.010**	12302	2197
			(0.004)	(0.004)		

Notes: Workplace controls include variables used for balancing checks in Table 2: 5 sectors, 4 workplace size groups, 5 workplace age groups (in REPONSE11 only) , Paris region, single-plant firm (in REPONSE11 only), gender of the employer interviewed (in REPONSE11 only). In addition, 6 dummies for the month of interview (January to June 2011) are also included in panel A while controls for the mean characteristics of the workers interviewed (gender, age, education and occupation) are included in panel B. Since workplace controls are not available on the larger sample of workers in REPONSE11, 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. For voluntary resignations, workplaces adopting the new legal regime during the first semester of 2011 (when the outcome is measured) are also excluded.

Table E4: RD estimates for main outcomes of interest using a proxy for the expected latest election date as running variable and controlling for workplace characteristics

	Sample Mean	Estim. left of thresh.	RD conv. estim.	RD BC estim.	N obs	N obs in band.
Panel A: Employers main outcomes						
At least one union recognized	0.659	0.624	0.188**	0.187**	1653	365
			(0.081)	(0.093)		
Trust in unions index	0.000	-0.274	0.600***	0.656**	1543	219
			(0.232)	(0.276)		
Unionization rate	0.106	0.077	0.112**	0.119**	1399	185
			(0.049)	(0.058)		
Work stoppage (any kind)	0.343	0.300	0.176**	0.184*	1653	235
(between 2008 and 2010)			(0.089)	(0.101)		
Panel B: Workers' main outcomes (core sample of workers)						
Share of workers union members	0.103	0.093	0.054	0.047	1582	407
			(0.044)	(0.052)		
Trust in unions index	0.000	-0.003	0.613**	0.684**	1446	169
			(0.255)	(0.287)		
Participation to a work stoppage	0.139	0.135	0.129*	0.144*	1573	232
			(0.073)	(0.086)		
Panel C: Rate of voluntary resignations in S1 2011 (DMMO/EMMO)						
Voluntary resignations in S1 2011	0.026	0.030	-0.009**	-0.010**	12302	2197
			(0.004)	(0.004)		

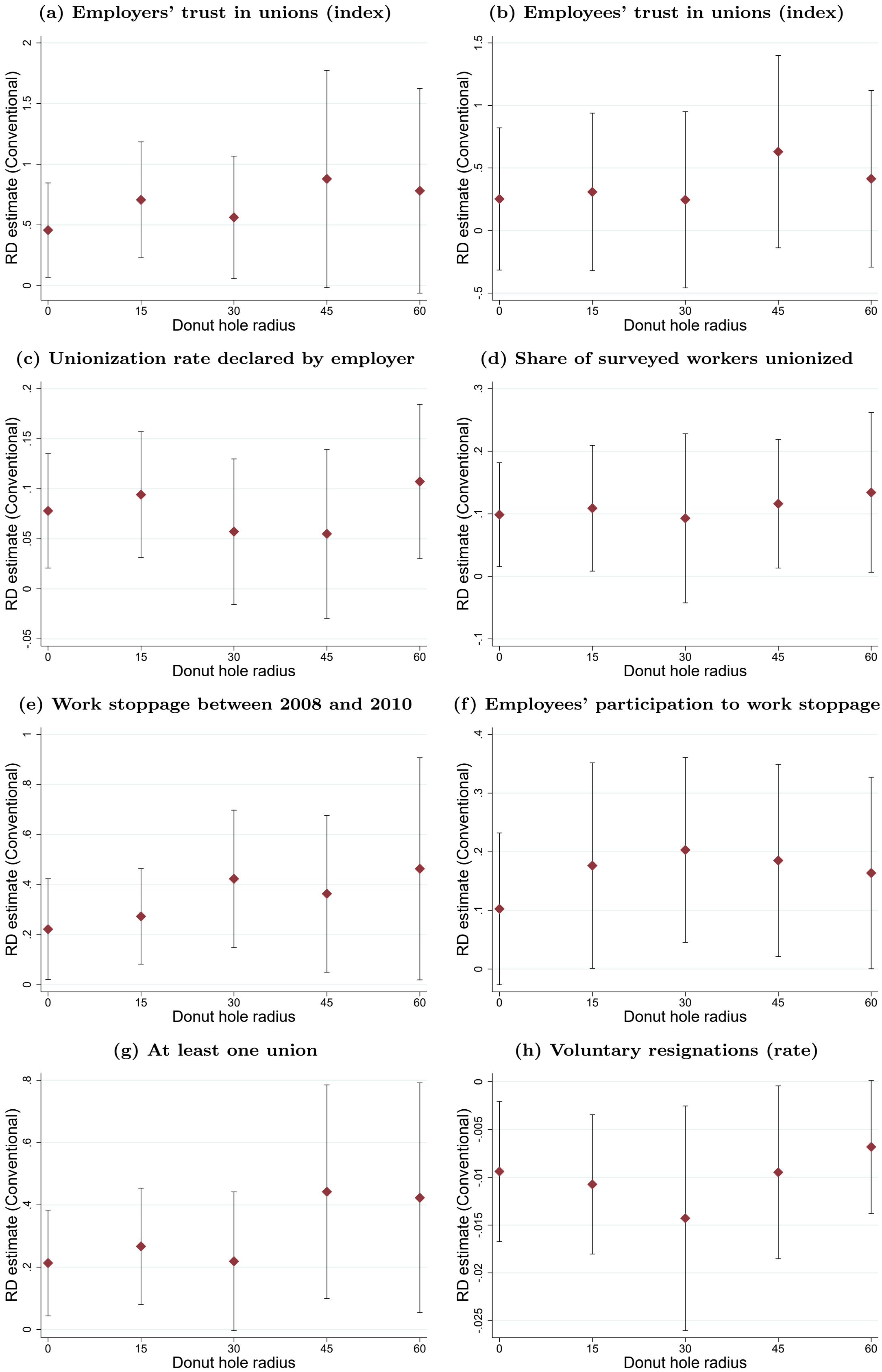
Notes: See notes of previous Table for included controls and additional details. The only difference with the previous Table is the running variable which is here a proxy for the expected date of the latest staff election. When the date of the latest staff election before outcomes are measured is anterior to 1 January 2009, it is used as running variable. However, when this date is posterior to 1 January 2009, it is replaced by the expected date of the election which is constructed using the date of the previous election and information on mandate duration (see details in Appendix B.1). 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)).

Table E5: 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.
Panel A: Employers main outcomes						
At least one union recognized	0.659	0.533	0.295***	0.302***	1911	443
			(0.093)	(0.110)		
Trust in unions index	0.000	-0.254	0.494**	0.525**	1782	528
			(0.206)	(0.240)		
Unionization rate	0.106	0.054	0.078*	0.092**	1629	204
			(0.041)	(0.045)		
Work stoppage (any kind)	0.343	0.231	0.276***	0.305**	1911	288
(between 2008 and 2010)			(0.107)	(0.119)		
Panel B: Workers' main outcomes (core sample of workers)						
Share of workers union members	0.121	0.081	0.112**	0.102	1586	217
			(0.057)	(0.065)		
Trust in unions index	-0.000	0.129	0.135	0.188	1453	168
			(0.286)	(0.339)		
Participation to a work stoppage	0.178	0.137	0.100	0.106	1579	261
			(0.062)	(0.076)		
Panel C: Workers' main outcomes (larger sample of workers)						
Share of workers union members	0.128	0.073	0.144***	0.155***	3042	413
			(0.044)	(0.048)		
Trust in unions index	-0.000	-0.030	0.314	0.362	2784	356
			(0.198)	(0.221)		
Participation to a work stoppage	0.190	0.177	0.045	0.054	3020	420
			(0.056)	(0.062)		
Panel C: Rate of voluntary resignations in S1 2011 (DMMO/EMMO)						
Voluntary resignations in S1 2011	0.026	0.030	-0.008*	-0.009*	12302	1479
(partially treated excluded)			(0.004)	(0.005)		
Voluntary resignations in S1 2011	0.025	0.030	-0.008***	-0.009***	16356	4683
(partially treated kept)			(0.003)	(0.003)		

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 REPONSE11. 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.

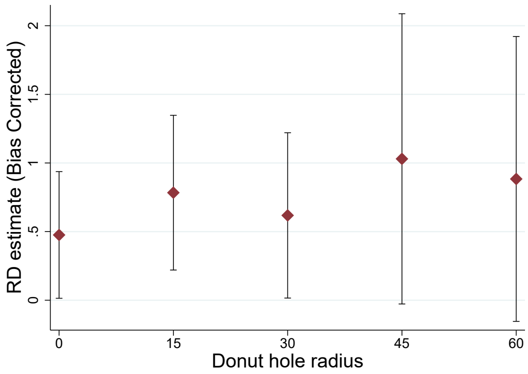
Figure E1: RD estimates (conventional) based on the donut hole approach for the eight main outcomes of interest



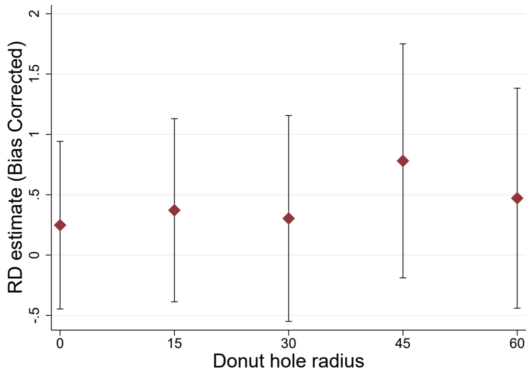
Notes: For each of the paper's eight main outcomes of interest, the Figure provides RDD conventional estimates (red diamonds) and their associated conventional 95% confidence intervals (black vertical straight lines) obtained after removing 0 to 60 days on each side of the January 1st 2009 cutoff date ("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 E2: RD estimates (bias-corrected) based on the donut hole approach for the eight main outcomes of interest

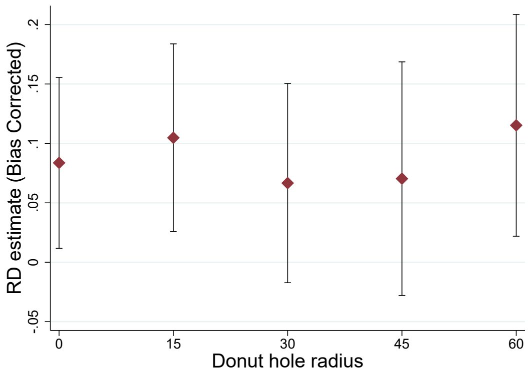
(a) Employers' trust in unions (index)



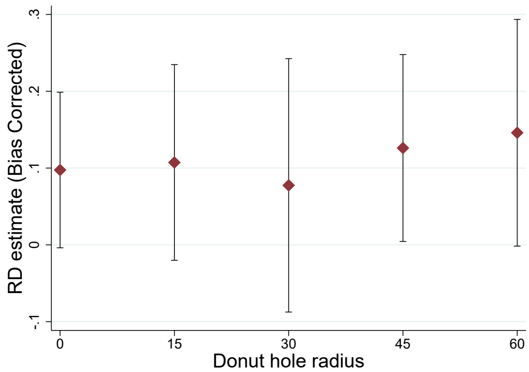
(b) Employees' trust in unions (index)



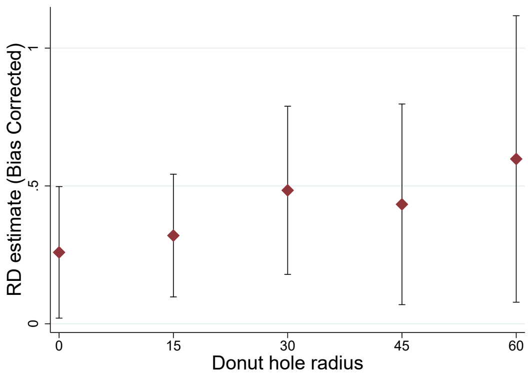
(c) Unionization rate declared by employer



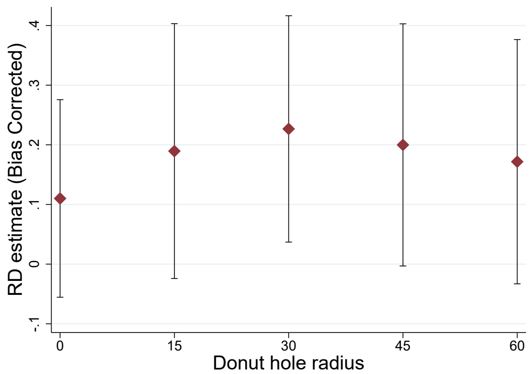
(d) Share of surveyed workers unionized



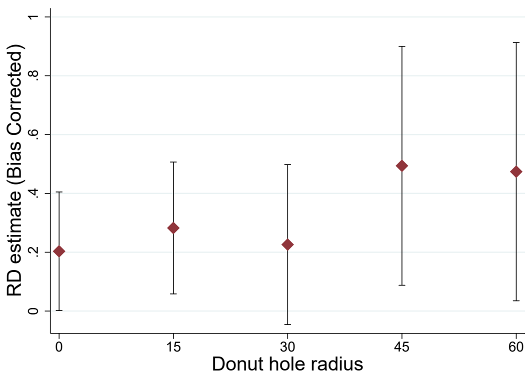
(e) Work stoppage between 2008 and 2010



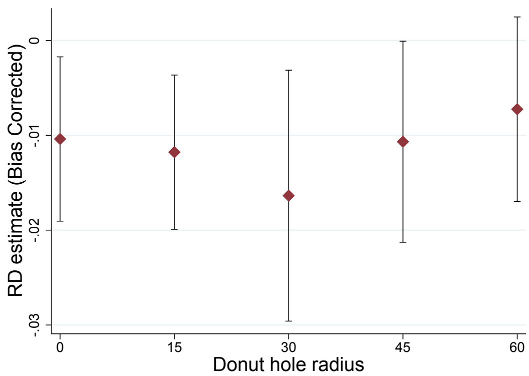
(f) Employees' participation to work stoppage



(g) At least one union

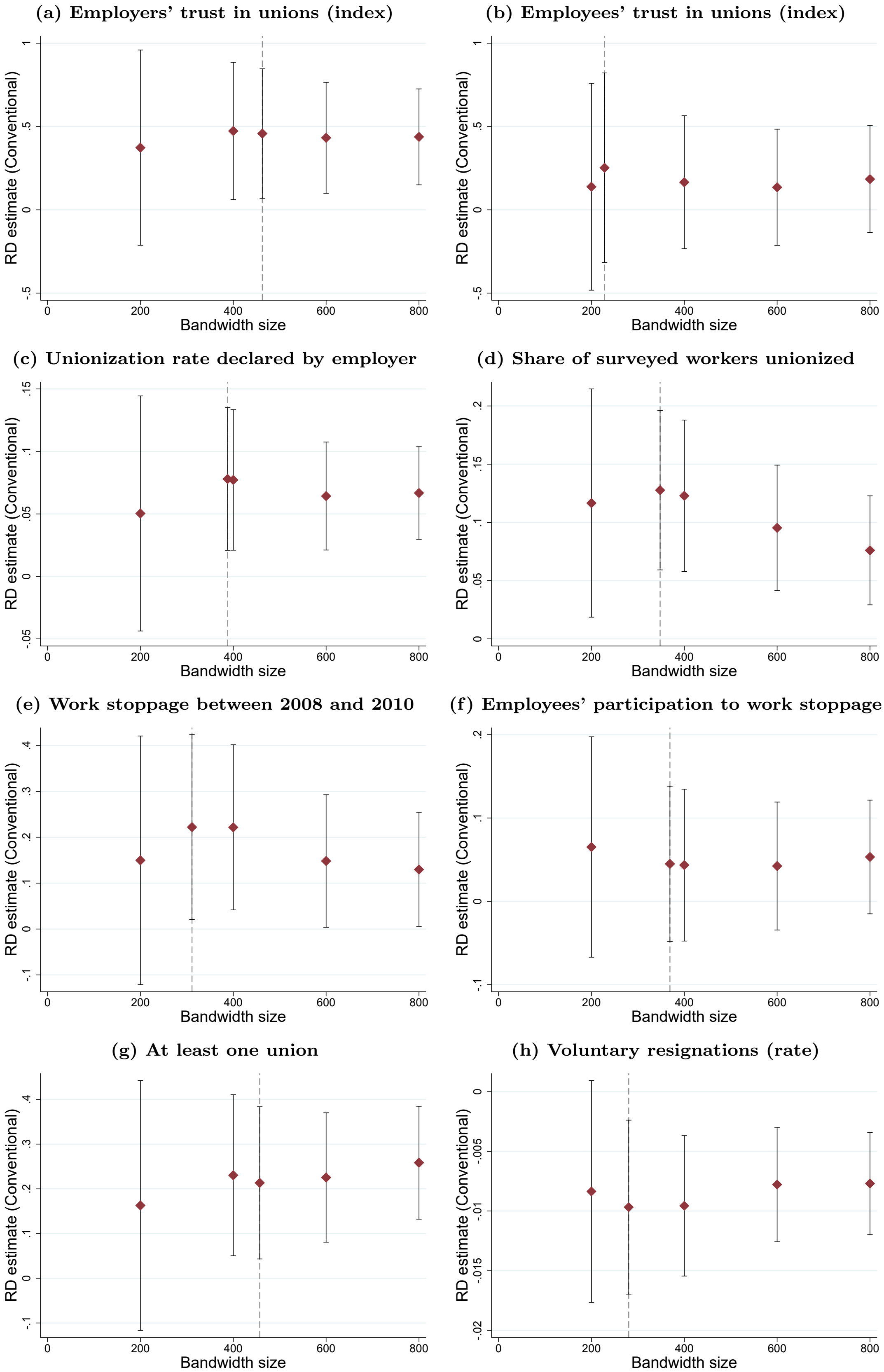


(h) Voluntary resignations (rate)



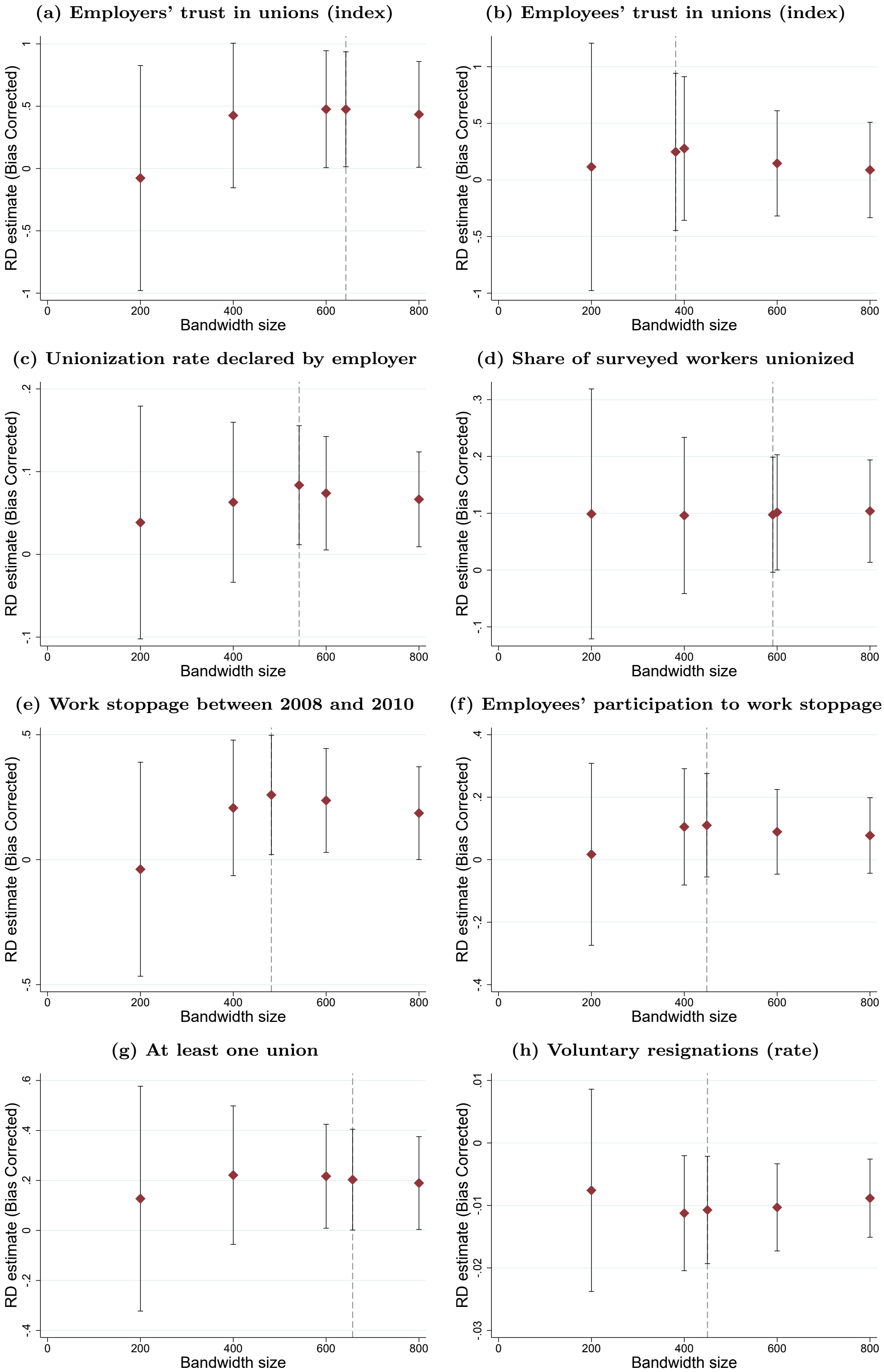
Notes: For each of the paper's eight main outcomes of interest, the Figure provides RDD bias-corrected estimates (red diamonds) and 95% *robust* confidence intervals (black vertical straight lines) obtained after removing 0 to 60 days on each side of the January 1st 2009 cutoff date ("donut hole radius"). 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 E3: 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 1st 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 conventional estimate given in the paper) is also provided.

Figure E4: 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 1st 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 bandwidth used *for the bias correction*. The RDD estimate for this bandwidth (which does not correspond to the baseline estimate given in the paper) is also provided.

Appendix F Incentives from higher-levels and effects at the extensive margin

F.1 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.[A.15]

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

[A.15] 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.

9.3 percent of the vote casts. Then, they strengthened to gain respectively 10.7 and 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.

F.2 Details on extensive versus intensive margin effects

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 8, 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 a s.d.[A.16] 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*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.[A.17]

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) by 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. It may similarly decrease the quit rate in S1 2011 by 0.2 to 0.3 percentage points. These effects typically represent a quarter to a third of our baseline estimates. They are based on calculations that 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 cannot be proved. Nevertheless, these calculations are suggestive that the impact of the reform on union coverage, while substantial, does not entirely determine the effect on other outcomes.

[A.16] When no unions are present, the REPONSE survey explicitly asks employers for their opinion of unions in general.

[A.17] 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.