FACULTAD DE CIENCIAS ECONÓMICAS Y DE ADMINISTRACIÓN

IECON INSTITUTO DE ECONOMÍA

UNIVERSIDAD DE LA REPÚBLICA URUGUAY

Property Rights and Effort Supply

Pablo Blanchard, Gabriel Burdín, Andrés Dean

INSTITUTO DE ECONOMÍA 02, 2023

Serie Documentos de Trabajo DT 01/2023

ISSN: 1510-9305 (en papel)

ISSN: 1688-5090 (en línea)

Agradecemos a José García-Louzao, Eduardo Montero, John Pencavel, y Arthur Silve por proporcionar valiosos comentarios sobre un borrador anterior de este documento. También agradecemos a los participantes de conferencias y seminarios en la Universidad de Siena, Universidad de Laval, Birkbeck, Bolonia, Escuela de Negocios de la Universidad de Leeds, FCS-UdelaR, IAFEP 2018 (Universidad de Lubjiana) y en el Taller 2021 "Transformaciones en la Gobernanza Cooperativa" (CREM - Universidad de Rennes). Este trabajo se realizó con apoyo financiero del programa grupos I+D de la Comisión Sectorial de Investigación Científica de la Universidad de la República. Agradecemos al Banco de Previsión Social por proporcionar los datos para este estudio. Todos los errores son nuestros.

Forma de citación sugerida para este documento: Blanchard. P, Burdín. G., Dean, A. (2023) “Property Rights and Effort Supply”. Serie Documentos de Trabajo, DT 01/2023. Instituto de Economía, Facultad de Ciencias Económicas y Administración, Universidad de la República, Uruguay.

Property Rights and Effort Supply

Pablo Blanchard *, Gabriel Burdín **, Andrés Dean***

Resumen

La evidencia directa sobre cómo varía la provisión de esfuerzo entre diferentes estructuras de propiedad sigue siendo escasa. En este artículo investigamos el comportamiento vinculado a las ausencias por enfermedad de las personas empleadas en cooperativas de trabajadores, es decir, en empresas que son propiedad y, en última instancia, controladas por su fuerza laboral. Utilizando datos de empleo combinados con los registros certificados de licencia por enfermedad y la variación exógena en la generosidad del régimen de licencia por enfermedad remunerada de Uruguay, mostramos que el ausentismo aumentó diferencialmente para las personas afectadas por la reforma y empleadas en cooperativas. El efecto es impulsado por miembros, ausencias tanto a corto como a largo plazo, condiciones musculoesqueléticas difíciles de diagnosticar (y, por lo tanto, más propensas a problemas de informes de riesgo moral) y cooperativas grandes.

Palabras clave: esfuerzo, ausentismo, baja por enfermedad, cooperativas, derechos de propiedad, equipos, riesgo moral.

Código JEL: I18, J22, J54

Abstract

Direct evidence on how effort provision varies across different ownership structures remains scant. We investigate the absence behaviour of individuals employed in worker cooperatives, that is, in firms owned and ultimately controlled by their workforce. Leveraging employment data matched with certified sick leave records and exogenous variation in the generosity of the Uruguayan paid sick leave regime, we show that absenteeism differentially increased for individuals affected by the reform and employed in cooperatives. The effect is driven by members, both short-term and long-term absences, hard-to-diagnose (and, hence, more prone to moral hazard reporting problems) musculoskeletal conditions, and large cooperatives.

Keywords: effort, absenteeism, sick pay, cooperatives, property rights, teams, moral hazard.

JEL Classification: I18, J22, J54

(*) Pablo Blanchard, IECON, Universidad de la República, Uruguay, correo electrónico: pablo.blanchard@fcea.edu.uy
(**) Gabriel Burdín, University of Leeds & IZA, correo electrónico: g.burdin@leeds.ac.uk
(***) Andrés Dean, IECON, Universidad de la República, Uruguay, correo electrónico: andres.dean@fcea.edu.uy

1 Introduction

The impact of property rights systems on workers' behaviour is a topic of perennial interest to economists and organisational scholars (Alchian and Demsetz 1972; Hart and Moore 1998; Hansmann 1996). However, direct evidence concerning individuals' effort responses under different ownership structures remains rare.

This paper fills this gap by assessing differences in absence behaviour, a proxy of effort choices, across individuals employed under two sharply distinct contractual arrangements: worker cooperatives and conventional investor-controlled firms. Worker cooperatives are enterprises in which the workforce has ultimate control rights (Dow 2003). Their members usually own and manage the company on a 'one person, one vote' basis, regardless of the amount of capital they supply to the cooperative. These organisational features stand in sharp contrast to those exhibited by conventional firms, in which outside owners hire labour, appoint managers and have the right to appropriate the residual income. Worker cooperatives are diffused in certain European regions, such as the Basque Country and Emilia Romagna. During specific periods, cooperatives also played a prominent role in certain sectors, such the US plywood industry (Pencavel 2001). Related arrangements, such as partial employee ownership schemes (e.g. ESOPs) and professional partnerships, are also common in the US and Europe.[1]

Measures of work effort are hard to observe, given the team-based nature of most production settings. Absenteeism is an important dimension of workers' behaviour that can be measured at the individual level. It represents a form of employee withdrawal behaviour that can be costly for firms and organisations. Firms may suffer from productivity losses and incur extra costs from employing temporary workers or from paying regular workers overtime in order to cover for absent employees (e.g. Herrmann and Rockoff 2012).[2] Interestingly, arguments concerning work incentives in cooperative firms date back to early economic writings.[3] For example, John Stuart Mill and Alfred Marshall highlighted potential advantages of worker cooperatives:

> "the general sentiment of the community, composed of the comrades under whose eyes each person works, would be sure to be in favour of good and hard working, and unfavourable to laziness, carelessness, and waste." (J. S. Mill, 1879, pp. 518-519).

> "[Cooperatives] render unnecessary some of the minor work of superintendence that is required in other establishments; for their own pecuniary interests and the pride they take

1. In the US, Kruse 2022 reports that about 20% of private sector employees own company stock. Roughly 12% of European companies (100+ employees) had put in place an employee ownership plan in 2015, ranging from 7% in The Netherlands to 25% in Belgium (Ligthart, Poutsma, and Brewster 2022)

2. Hensvik and Rosenqvist 2019 show that the extent of production disruptions due to absenteeism depends on firms' ability to find internal substitutes for absent workers.

3. Quotes are taken from Jones 1976.

> in the success of their own business make each of them averse to any shirking of work either by himself or by his fellow-workmen." (A. Marshall, 1964, pp. 254-255).

Instead, Sidney and Beatrice Webb, co-founders of London School of Economics and Political Science, raised concerns about the relationship between management and members in this type of firms:

> "The relationship set up between a foreman or manager, who has throughout the working day to give orders to his staff, and the members of that staff who, assembled in general meeting, criticise his action or give him directions, with the power of dismissing him if he fails to conform to their desires, has always been found to be an impossible one" (S. and B. Webb, 1920, p.166).

From the perspective of modern economic analysis, the impact of cooperative property rights on absence behaviour is theoretically ambiguous and remains an open empirical question. On the one hand, several explanations point to weaker work incentives and greater incidence of workers' absenteeism in cooperatives. First, cooperative teams may suffer from the classical free rider problem (Alchian and Demsetz 1972). This may be exacerbated by the *de facto* job security enjoyed by cooperative members, limiting the scope for using the threat of dismissal as a mechanism to keep shirking behaviour in check. Second, managerial discretion to impose sanctions and dissolve labour contracts may be more limited in cooperatives than in conventional firms (Hart and Moore 1998). Indeed, worker cooperatives are characterised by a dual-authority structure. Worker-principals appoint managers, set objectives and monitor the implementation of firm policies. In turn, managers, acting as quasi-principals, organize and monitor the production process and the actions of the workers. Interestingly, while workers have the power to dismiss managers, managers cannot replace workers without consulting the membership (Ben-Ner, Montias, and Neuberger 1993). Finally, egalitarian compensation policies implemented by cooperatives may induce negative selection of workers both at the bottom and the top of the ability distribution, distorting incentives of frontline workers and managerial quality (Kremer 1997; Abramitzky 2009; Burdin 2016).[4]

On the other hand, the fact that cooperatives rely more extensively on group-based profit sharing and on team-based work may mitigate absence behaviour driven by moral hazard. Profit-sharing makes workers residual claimants on the income stream resulting from the noncontractible effort supplied to the firm. This may provide an incentive to reduce absences, particularly in small cooperatives. Moreover, horizontal peer pressure and social emotions may help to save on monitoring inputs, sustain

4. Workers' experience in cooperatives may be more intense and stressful than in a conventional business as members have both production and decision-making responsibilities. This suggests that cooperatives, far from being idyllic workplaces, may be better described as "high-expectation, high-stress work systems" (Arando et al. 2015).

high-effort norms and curb absenteeism in cooperative teams (Putterman 1984;Kandel and Lazear 1992; Hamilton, Nickerson, and Owan 2003; Putterman 1984; Carpenter et al. 2009).[5] As the entire cooperative team suffers when one worker-member is absent from work, the returning team member can be exposed to informal group sanctions.[6] Although profit sharing provides weak incentives to work harder in large organisations, it might suffice to induce reciprocal workers to report each other for shirking (Carpenter, Robbett, and Akbar 2018). Finally, shirking on effort can be deterred in cooperative teams by relying on repeated game mechanisms as long as members expect to interact in the future and are sufficiently patient (Macleod 1984; Putterman and Skillman 1992; Dong and Dow 1993).

To shed light on this debate, our empirical analysis relies monthly employment history administrative records matched with unique individual-level information on certified sick leave over the period 2005-2013. We exploit variation created by a paid sick leave reform that increased the generosity of sickness insurance for certain workers in Uruguay. The reform gradually increased the sick pay cap, providing exogenous variation in sick leave compensation across individuals depending on their pre-reform wage. This setting allows us to use a difference-in-differences (DiD) approach, including heterogeneous treatment effects in order to capture the differential response of workers employed in cooperatives relative to individuals employed in conventional private-sector firms.

The analysis yields two basic results. First, we find that the increase in sick leave pay raised the probability of being absent from work in a given month by 1.6 percentage points more among treated individuals employed in cooperatives than among treated individuals employed in conventional firms. Second, the duration of sickness-related absence spells for treated cooperative members increased by 0.4 days relative to the other groups in a given month. In relation to the pre-reform situation of treated individuals employed in worker cooperatives, sickness absences in the extensive and intensive margins increased by 40% and 55%, respectively. Results from an event-study analysis suggest that the absence behaviour of these individuals was on a similar pre-reform trend relative to the other group. By excluding workers who switched between organisational forms during the period, we show that the results are not driven by non-random sorting into cooperatives due to the reform. The fact that we observe a similar trend in absence behaviour over a period of six years before the reform also suggests

5. Cooperative behaviour in public good games can be sustained by relying on social punishment (Fehr and Gächter 2000). However, peer sanctions may also be targeted at high-contributors (Herrmann, Thöni, and Gächter 2008; Ertan, Page, and Putterman 2009).

6. The cost to the organisation when a worker shirks by being absent and taking excessive paid sick leave may be less salient in the Uruguayan context as the Uruguayan regime has no experience rating sick leave insurance (i.e the payroll tax rate does not rise when more of the firm's workforce receives paid sick leave).

that a more general pattern of selection of absence-prone individuals into cooperative is unlikely to explain our findings.

Using our DiD framework, we explore several potential mechanisms that may account for the observed differences in absenteeism: (1) the differential shift in absence behaviour among treated individuals employed in cooperatives is explained by both short-term and long-term absences, suggesting that this type of firms not only face potential moral hazard problems but also facilitate greater take-up of sick leave motivated by genuine health problems; (2) the increase in absenteeism is entirely driven by cooperative members (no significant effects are obtained when the analysis is restricted to employees in conventional firms and cooperatives); (3) there is no differential increase in extended weekend absences ('Monday effect'); (4) the analysis of disease-specific behavioural responses reveals a differential increase in hard-to-diagnose (and more prone to moral hazard reporting problems) musculoskeletal conditions for treated individuals employed in cooperatives; (5) the dynamics of layoffs suggests that conventional firms use the threat of dismissal more actively than worker cooperatives as to keep absenteeism in check after the reform; and (6) the differential increase in absenteeism is entirely driven by individuals employed in medium-sized and large cooperatives, precisely where one would expect the dilution of work incentives to be more severe.[7].

Complementary survey-based evidence on worker supervision and managers' perceptions, collected before and after the reform, suggests more negative views on absenteeism and work ethics among managers of large cooperatives. Interestingly, small worker cooperatives do not seem to have experienced a similar erosion of work incentives. When the analysis is restricted to the subsample of small firms, our DiD estimates show no differential increase in absenteeism for individuals employed in worker cooperatives after the reform. Moreover, small cooperative exhibit lower supervision intensity than comparable conventional firms and extensively rely on mutual monitoring among coworkers as an alternative discipline device.

The paper contributes to different strands of research. First, we add to a long-standing literature examining moral hazard in team production and how the allocation of controls rights over productive assets affect workers' incentives (Alchian and Demsetz 1972; Holmstrom 1982; Macleod 1984; Macleod 1987; Putterman 1984). Our paper relates to previous research on incentives in communal organisations (Abramitzky 2008, 2009, 2011) and to a series of studies examining the productivity effect of worker cooperatives vis-à-vis conventional firms (Craig and Pencavel 1995; Fakhfakh, Pérotin, and Gago 2012; Pencavel 2013; Monteiro and Straume 2018; Young-Hyman, Magne,

7. Our results are consistent with recent qualitative evidence documenting problems of workplace absenteeism prior to the demise of the world's biggest industrial worker cooperative (Basterretxea, Heras-Saizarbitoria, and Lertxundi 2019)

and Kruse 2022). In a recent paper, Montero 2021 exploits exogenous variation in ownership rights induced by a land reform in El Salvador. Using a regression discontinuity design, he finds that cooperatives are less productive than conventional haciendas when producing cash crops, but more productive when producing staple crops. While previous studies rely on firm-level measures of productivity, our paper is one of the first attempts to provide direct evidence of individuals' effort provision in the form of absenteeism in worker cooperatives.

Second, this paper adds to the literature on sick leave insurance and absence behaviour (Henrekson and Persson 2004; Ziebarth and Karlsson 2010; Ziebarth 2013; Paola, Scoppa, and Pupo 2014; Ziebarth and Karlsson 2014; Pichler and Ziebarth 2017, Bryson and Dale-Olsen 2017; Marie and Vall-Castello 2022). While previous studies have focused exclusively on the U.S. and European countries, little is known about the incentive effects of paid sick leave reforms in less developed countries. Moreover, we contribute to understanding the role of firm organisation in moderating the interplay between sick leave insurance and workplace absenteeism (e.g. Bennedsen, Tsoutsoura, and Wolfenzon 2019). Previous research has analysed the effect of probationary periods (Ichino and Riphahn 2005) and sick leave reforms in the public sector (Paola, Scoppa, and Pupo 2014). According to these studies, workers' behaviour is sensitive to the level of employment protection, sick leave compensation and monitoring intensity. Interestingly, there is extensive evidence documenting greater job security in worker cooperatives compared to conventional firms (Burdín and Dean 2009; Pencavel, Pistaferri, and Schivardi 2006; Garcia-Louzao 2021). The fact that cooperative members "buy" an implicit long-term employment guarantee may have an effect on their absence behaviour. Indeed, our study shows that the impossibility of using dismissal threats as a discipline device seems to be an important channel behind the increase in absenteeism among individuals employed in worker cooperatives.

Finally, our paper contributes to the scant literature on worker voice and non-pecuniary dimensions of jobs, such as health outcomes. Arnold, Brändle, and Goerke 2018 find evidence of higher utilisation of sick leave in German firms with works councils. Exploiting exogenous changes in codetermination rules among Finnish firms, Harju, Jäger, and Schoefer 2021 find no effect of worker voice institutions on sickness-related absences.[8] Goerke and Pannenberg 2015 study the effect of a reduction of statutory paid sick leave using self-reported survey data from Germany. They find a positive relationship between trade union membership and sickness absence and a stronger reaction to the reduction in paid sick leave among union members than among non-members. As the German reform applied across the board to all private workers, their treatment group is entirely composed of private-sector workers and the control group

8. Blasi et al. 2010 find a positive association between employee ownership and absences in the US context.

comprises public-sector workers and self-employed workers. In this paper, we restrict the analysis to private sector workers employed both in worker cooperatives and in conventional enterprises. By relying on high-frequency administrative data, including information on the exact start and end date of each absence spell, our analysis is less affected by the kind of measurement errors that typically pervade survey data. Most importantly, the data allows us to extensively investigate the underlying channels through which the differential response of cooperative members manifests itself. Interestingly, while the studies mentioned above have analyzed institutional arrangements conveying limited power to workers, such as works councils and minority board-level representation, our paper contributes to understanding how the assignment of more extensive control rights to workers affects individual and firm outcomes.

The remainder of the paper is organized as follows. The next section describes the Uruguayan sick leave reform and provides contextual information on worker cooperatives. Section 3 explains the data and the identification strategy. Section 4 presents the main findings, provides evidence concerning identification assumptions and reports results from several robustness checks. Section 5 uncovers different mechanisms that may account for the differential behavioural response of individuals employed in worker cooperatives. Section 6 reports complementary survey evidence on supervision intensity and managerial perceptions about absenteeism and work ethics in cooperatives and conventional firms. Section 7 concludes.

2 Institutional context

2.1 Background on the Uruguayan paid sick leave reform

According to the sick leave legislation in Uruguay, a worker experiencing a sickness episode receives an amount b, which represents a constant replacement ratio (70%) of her last wage (w) up to a maximum benefit amount (bmax), where the replacement rate decreases.[9] The benefit cap is defined in terms of Bases de Prestación y Contribución (BPC), where BPC is the basic unit of measurement used to calculate different social benefits in the Uruguayan social security system.[10] Therefore, the sick leave pay is

9. The fact that the sick leave benefit is a kinked function of previous earnings makes the design of the Uruguayan system comparable to social insurance programs in developed countries, such as the Norwegian public sick leave (Bryson and Dale-Olsen 2019) and unemployment insurance in U.S. states (Landais 2015).

10. 1 BPC is equivalent to 3848 Uruguayan Pesos (USD 117/January 2018). Source: Banco de Prevision Social.

computed according to the following rule:

$$b = \begin{cases} 0.7w & \\ bmax & \text{if } 0.7w > bmax \end{cases} \tag{1}$$

To be eligible, the worker must have worked and paid social security contributions for at least 3 months in the year preceding the illness episode. As is common in other public sick leave regimes, a physician has to certify the worker's health condition. The worker is not entitled to any payment during the first three days of sick leave and can receive the benefit for a maximum of one year; the benefit may be extended for an additional year under special circumstances (Amarante and Dean 2017). The sick leave pay is not disbursed by the employer but by the public health insurance system. The program is funded from general taxation and social security contributions are paid by both employers and employees. In contrast to experience rating insurance systems, employers' payroll tax rates do not depend on the number of workers firms have had on sick leave in the past.

Before the reform, the benefit cap was 3 BPC. Therefore, those workers for whom 0.7w exceeded the threshold of 3 BPC received exactly 3 BPC as paid sick leave. Figure 1 describes the evolution of the paid sick leave schedule over the period analysed in this paper. As a result of the reform, the benefit cap gradually increased by 1 BPC per year starting from January 2011. By January 2013, the last year included in our study, the benefit cap had reached 6 BPC.[11] Figure 2 plots the evolution of the ratio between the benefit cap and the average wage before and after January 2011, confirming the sharp relative increase of the benefit cap. The spikes observed in the data correspond exactly to the reform schedule (January 2011, 2012 and 2013).

2.1.1 Worker cooperatives in Uruguay

Worker cooperatives are defined as enterprises where members jointly carry out the production of goods or services activities and have control over important economic decisions.[12] Usually, members jointly own and manage the firm on a "one person, one vote" basis regardless of their capital contribution and the residual is distributed among them according to a certain sharing rule.

In Uruguay, worker cooperatives are those firms that are legally registered as producer cooperatives (PCs) in which the employee-to-member ratio does not exceed 20%. These firms are allowed to hire salary employees but they must still comply with the legislated maximum percentage of hired workers in order to receive certain tax ad-

11. The reform was fully phased in by January 2015 when the benefit cap reached its current level of 8 BPC.

12. This section draws on Burdin 2016.

vantages – in particular, the exemption from paying the employer payroll tax to social security. The law also requires a minimum of six members to register a new cooperative firm.

Although their key organisational features are predetermined by law, worker cooperatives have discretion over a broad range of associational rules. With respect to governance structure, worke cooperatives must have a general workers' assembly that selects a council to supervise the daily operations (the council, in turn, usually selects the managers). Each member has only one vote, regardless of his capital contribution to the firm. Physical assets can be owned by their members either collectively or individually. Under collective ownership, members do not own tradable shares but enjoy the right to usufruct as long as they work in the firm. Under individual ownership, members own capital shares that vary with the firm's value. Most Uruguayan worker cooperatives operate under a collective ownership regime. As in other countries, membership markets are extremely rare in Uruguay: fewer than 10% of Uruguayan worker cooperatives are owned by their workforce through individual shares (Alves et al. 2012).

3 Data and identification

3.1 Data

Our empirical analysis is based on longitudinal individual-level administrative records from the Uruguayan social security system. The data were provided by Banco de Prevision Social, the agency in charge of social security affairs in Uruguay. Employers are obliged to deliver monthly information on their employees to the agency, which uses that information to calculate pension and social benefits. To conduct this study, we combine three different databases. First, we use monthly employment history data from a random sample of 300,000 individuals who were registered in the social security system for at least one month during the period 2005-2013. The structure of the data is an unbalanced panel of workers, containing information on wages, personal attributes of the worker (gender, age, tenure), and the firm in which she works (firm size, industry, region). Each worker-month observation is associated with a firm identification number so that job changes (or any other discontinuity in the individual's employment history) can be tracked. Moreover, we obtain similar employment history data for the universe of individuals employed in worker cooperatives. Finally, and crucially for the purpose of this study, we match individual-level records of certified sickness absences, including the start and end date of each sickness absence spell, and sick leave payment. Information on short sickness spells (fewer than 4 days) and diagnosis classified according to the International Classification of Diseases (ICD) is only available since 2010. For this reason, our investigation mainly focuses on spells of more than 3 days.[13]

We restrict the sample in several ways. First, we focus on workers employed in non-agricultural private firms, excluding public, rural and construction workers. Second, we only consider eligible individuals, i.e. those who made social security contributions for at least 3 months (or 75 days in the case of day labourers) in the year preceding the sickness spell. The final dataset is an unbalanced panel from January 2008 to December 2013, i.e. three years before and after the sick leave reform. Descriptive statistics for the final sample are presented in Table 1. The resulting sample includes, on average, about 36,965 individuals in each month. The total number of individual-month observations is 2,625,338, corresponding to 52,751 and 3,532 individuals employed in conventional firms and worker cooperatives, respectively. The composition of the two groups is different: individuals employed by worker cooperatives are older than those employed by conventional firms and, in the latter case, the percentage of small firms (less than 20 workers) is higher. Proportionately fewer women are employed by worker cooperatives than by conventional firms, particularly in the treatment group. On average, both the incidence and duration of sickness absences appear to be higher in cooperatives.

3.2 Identification

Before the reform, sick leave pay was subject to a benefit cap equivalent to 3 BPC. In other words, an individual for whom 70% of her total monthly earnings exceeded 3 BPC received exactly 3 BPC. As explained in section 2, the Uruguayan sick leave reform gradually increased this maximum benefit cap starting in January 2011. Our identification strategy exploits the exogenous increase in the generosity of paid sick leave for this group of workers. We compare the evolution of sickness absence (incidence and duration) between affected and unaffected workers according to their pre-reform earning level. Individuals earning up to 3BPCs remained unaffected by the reform and compose our control group. Instead, the treatment group comprises individuals earning an amount such that their sick leave pay would have been capped before the reform ($3BPC < 0.7w \leq 6BPC$). For these individuals, the reform increased the effective replacement rate of sick leave pay. To define treatment and control groups, we consider workers' total monthly earnings in November 2010, immediately before the reform came into force (January 2011).

13. As explained in the previous section, this is not a relevant limitation since spells fewer than 3 days old are not paid and were not affected by the reform.

We estimate the following triple difference-in-differences specification:

$$\begin{aligned} y_{it} = \alpha + \beta T_t + \gamma D_i + \delta Coop_{it} + \eta T_t \times Coop_{it} + \zeta D_i \times Coop_{it} + \theta D_i \times T_t + \\ + \phi D_i \times T_t \times Coop_{it} + \psi X_{it} + \tau_s + \omega_r + \epsilon_{it} \end{aligned} \tag{2}$$

where y_{it} either is an indicator for whether individual i experienced a sickness absence spell (lasting at least four days) in month t[14] or measures the number of days of sickness absence individual i took in month t, T_t is a post-reform dummy, D_i is the treatment group dummy, and $Coop_{it}$ is a dummy variable describing the worker cooperative status of individual i in month t. Sector τ_s and region ω_r fixed effects account for time-invariant permanent differences across 9 industries and 19 regions respectively. We also control for personal and firm-level characteristics (gender, age, tenure, firm size). Coefficient θ captures the general effect of the reform and coefficient ϕ, associated with the triple interaction, measures the differential effect for individuals employed in cooperatives. The model also includes all the corresponding two-way interactions. We estimate equation (2) by OLS, clustering standard errors at the individual level in order to account for serial correlation.

Figure 3 plots the evolution of the average sick leave benefit (in real terms) for both treatment and control individuals over time. The average sick leave pay increases in both groups. As sick leave pay is computed as a fixed fraction of the worker's total wage, this simply reflects the general increasing trend experienced by real wages in Uruguay during this period. More importantly, there is a differential increase in average sick leave pay for treatment workers starting in January 2011, suggesting that the reform hit the treatment group in the expected way. In Figure 4, we plot the fraction of workers in the treatment and control group affected by the sick pay cap before and after January 2011. As expected, the share of workers for whom the cap was binding was higher in the treatment group than in the control group before the reform, but decreased sharply after the reform.

Finally, in Figures 5 and 6, we plot the evolution of the incidence and duration of sickness absences for treated and control cooperative and conventional workers. Both figures show the evolution is similar in the pre-reform years for the four groups. Moreover, these figures reveal that treated workers employed in worker cooperatives react very differently to the sick leave reform starting in January 2011. While these figures provide preliminary visual evidence supporting the common time trend assumption, we report results from a formal event-study analysis in section 4.3.

14. If an absence spell spans over several months, the variable takes value 1 in each month.

4 Results

4.1 Exploratory analysis: fixed effects regressions

Before presenting the results of our main empirical exercise, we investigate the comparative absence behaviour of workers employed in worker cooperatives vis-à-vis employees in conventional firms by estimating a series of fixed-effects regressions. In this case, identification comes from the variability provided by workers who switch between organisational types during the period, under the assumption that sorting is driven by time-invariant characteristics. We count 1,746 switchers, which represents approximately 3% of the sample (454 workers moved from worker cooperatives to conventional firms and 1,292 made the reverse switch). The sample is restricted in the way explained in Section 3.1, except for the fact that we include all individuals regardless of their pre-reform wage.

Table 2 reports the corresponding estimates from fixed-effects regressions. We successively add controls for personal firm-level characteristics (age, tenure, firm size), year, industry, and region fixed effects. In all specifications, we cluster standard errors at the individual level. In columns (3) and (6), we report results from our preferred specifications, including region and industry-specific time trends. These estimates indicate the incidence of sickness-related absences in a given month is 1.3 percentage points higher for individuals employed in worker cooperatives compared with those employed in conventional firms. Moreover, workers employed in cooperatives spend 0.33 more days per month on sick leave compared to those employed in conventional firms. This difference is statistically significant at conventional levels.

4.2 Difference-in-differences estimates

Table 3 shows our main difference-in-differences estimates. The sample is restricted to control and treatment individuals, as defined in Section 3.2. We exploit the fact that individuals employed in worker cooperatives and conventional firms were exposed to an exogenous variation in the generosity of paid sick leave as a result of the reform. Columns 1-3 show the estimated coefficients for the incidence of sickness absences (extensive margin). In column (1) we include controls for individual- and firm-level attributes (sex, age, tenure, and firm size) and region and industry fixed effects. In column (2), we add industry- and region-specific time trends to control for time-varying shocks. In column (3), we restrict the sample to full-time workers aged 18-59.

The coefficient associated with the triple interaction term, which measures the differential effect of the reform for treated workers employed in worker cooperatives, is significantly positive in all specifications. Our estimates reported in Column (3) indicate that treated workers in cooperatives increased their probability of being absent from work in a given month by 1.6 percentage points in comparison to treated workers employed in conventional firms. This effect implies a 40% increase relative to the average pre-reform incidence of sickness absence among treated cooperative workers. Columns 4-6 report estimates considering the duration (in days) of sickness-related absences as the dependent variable. According to estimates reported in Column (6), which include industry- and region-specific time trends and restricts the sample to full-time workers aged 18-59, treated workers in cooperatives differentially increased absences by 0.4 days in a given month. The magnitude of the effect is sizeable, implying a 55% increase relative to the average pre-reform duration of sickness absence spells in that group.

4.3 Robustness checks and additional results

Event-study analysis. Our results indicate a differential intensification of absence behaviour among treated workers employed in worker cooperatives after January 2011. If the effect is due to the paid sick leave reform, we should not observe any differential pattern before 2011. Figure 7 and 8 report the results from an event-study analysis, showing the evolution of sickness-related absences over the years around the paid sick leave reform. Each estimated coefficient corresponds to the interaction between $T_t \times Coop_{i_t}$ and a full set of year dummies, where the coefficient for 2010 is normalized to zero. We do not find evidence of differential trends in workplace absences before 2011. The differential increase in sickness-related absences for treated workers employed in worker cooperatives becomes significant in 2012 and 2013.

Switchers. An important concern is that the reform may induce sorting of workers into cooperatives according to unobserved factors that may also affect their likelihood of sickness absence. We address this concern by restricting the analysis to a subsample of individuals who did not switch between conventional and worker cooperatives during this period. Our DiD estimates excluding job switchers are reported in columns 1-2 of Table 4. Treated workers in cooperatives increased their likelihood of being absent from work in a given month by 1.3 percentage points in comparison to other groups. The effect is significant at the 10% level. Duration increased by 0.316 days relative to the other groups, though the effect is not statistically significant at conventional levels (SE 0.195). This suggests that self-selection into worker cooperatives resulting from the paid sick leave reform cannot fully account for our results. Of course, we cannot rule out sorting effects in general. However, the fact that we observe a similar pre-reform trend in absence behaviour suggests that sorting pre-reform is unlikely.

Compositional changes. We perform additional DiD estimates using the balanced panel in order to control for workforce compositional changes. Estimates reported in Columns (3)-(4) of Table 4 restrict the sample to individuals observed for 24 consecutive months before and after the reform (balanced panel). We find a significant differential increase in absenteeism among treated workers in cooperatives relative to other groups. The magnitude of the effect is similar to our baseline estimates: the incidence of sickness-related absences increased by 1.5 percentage points and duration raised by 0.5 days in a given month.[15].

Individual fixed effects. We also control for time-invariant unobserved heterogeneity by estimating a difference-in-differences model with individual fixed effects. Results reported in columns (5)-(6) of Table 4 still indicate that the incidence and duration of sickness absence increased differentially among treated workers employed in cooperatives in relation to the other groups.[16]

Continuous Treatment. Our binary treatment indicator masks the fact that the increase in the generosity of paid sick leave after January 2011 did not affect individuals in the treatment group uniformly. As shown in Figure 1, the pre-reform benefit cap (3 BPC) gradually increased by 1 BPC per year from January 2011, reaching 6 BPC by January 2013. Workers earning less than 3 BPC just before January 2011 were not intended to be affected by the reform (never treated control group). Instead, all individuals earning above 3 BPC became treated in January 2011 (treatment group). Some of them, however, were also eligible to receive incremental "doses" in January 2012 and January 2013. To be more precise, the staggered intensification of the treatment worked as follows: (1) individuals earning 3-4 BPC *only* benefited from the initial sick pay cap rise in January 2011; (2) individuals earning 4-5 BPC *also* benefited from the second cap rise in January 2012; (3) finally, individuals earning 5-6 BPC were *also* eligible to benefit from an additional cap rise in January 2013. Hence, our treatment is multi-valued.

Following Ziebarth 2013, we take into account differences in treatment intensity by computing for each individual the (potential) reform-induced increase in statutory sick leave pay over the entire post-reform period relative to her pre-reform gross wage. Our measure of treatment intensity (dose) takes the value zero for workers in the control group and positive values up to 35% of workers' gross wage for those in the treatment group. On average, the potential sick leave benefit for treated workers increased by 19% of their gross wage due to the reform. Results are presented in columns (7)-(8) of Table 4. Consistent with our previous results using a discrete treatment indicator, the behavioural response to treatment intensity for workers employed in cooperatives is

15. We also estimate a more flexible DiD model interacting individual (gender, age, tenure) and firm-level characteristics (size, region, industry) with our Post-reform, treatment, and worker cooperative dummies. This model allows covariates to have a differential effect depending on time and individuals' treatment and cooperative status. Reassuringly, results are very similar to our baseline estimates (see Appendix Table A.1.1)

16. It is worth noting that in this case the effect is identified from within-individual change in their $D \times T$ and $D \times T \times Coop$ status over time.

significantly stronger relative to other groups.

In a recent paper, Callaway, Goodman-Bacon, and Sant'Anna 2021 identify crucial weaknesses of the DiD estimator in the presence of multiple time periods and variation in treatment intensity and timing of adoption. In particular, they identify a bias arising from a specific form of selection into different amounts of the treatment (selection-on-gains). Hence, to compare treatment effects across groups exposed to different dosage levels would require to make stronger assumptions than in the standard DiD framework. Unfortunately, there is no straightforward practical solution to this problem in the literature. As mentioned, our treatment group is composed of individuals who only benefited from the initial sick pay cap hike and individuals who also benefited from subsequent cap rises in January 2012 and January 2013. Therefore, we should assume that the treatment effect for the first group is the same as the treatment effect for the other group had they both benefited only from the first benefit cap rise. This would be violated if individuals have the discretion to set their pre-reform wages and, hence, dosage level based on their expected gains from the reform.

To further dig into this issue, we estimate a separate DiD model comparing individuals in the control group with individuals who experienced the same treatment intensity and timing. We focus on the group of individuals earning 3-4 BPCs just before January 2011, who only benefited from the first sick pay cap hike. Results reported in Appendix Table A.1.2 are qualitatively similar to our baseline estimates.

5 Mechanisms

Short-term vs. long-term absenteeism. The Uruguayan sick leave insurance system does not make any distinction between short- and long-term absences in terms of replacement rates and funding. However, the distinction might be important to understand the underlying mechanisms behind the differential response of individuals employed in worker cooperatives. Assuming that individuals on long-term sick leave are more prone to be seriously sick, it has been argued that standard labour supply responses driven by moral hazard might be more relevant for short-term rather than for long-term sickness absence. Following Ziebarth 2013, in a given month, we classify sickness-related absences originated in absence spells lasting more than 6 weeks as long-term absences. In our sample, long-term absences account for 53% of all absence days although they only represent 21% of all sickness cases.

In columns (1) and (2) of Table 5, we display estimates of equation (2) of the incidence of sickness absence for short- and long-term sickness spells, respectively. Results reported in column (1) indicate that the incidence of short-term absences for treated individuals employed in worker cooperatives increased by 0.4 percentage points relative to treated workers employed in conventional firm. We also find that long-term absences among individuals employed in cooperatives increased by 1 percentage points relative to the other group. The change in short-term absences suggests that the increase in workplace absenteeism in cooperatives after the reform is partly attributable to moral hazard problems. Interestingly, cooperatives also seem to facilitate greater take-up of long-term sick leave, presumably motivated by genuine health conditions.

Members vs. employees in worker cooperatives. As explained in section 2.2, worker cooperatives can also hire employees at market wages as do conventional firms. The distinction between members and employees in worker cooperatives is relevant in our context given the different incentive structure faced by the two types of workers, which in turn may affect their responses to the paid sick leave reform. In contrast to members, hired workers in cooperatives do not participate in strategic managerial decisions and do not have an ownership stake in the firm. Therefore, one could hypothesise that members and hired employees in worker cooperatives face different labour discipline environments. For instance, the threat of dismissal due to unsatisfactory job performance may be less credible in the case of members.[17]

In columns (3) and (5) of Table 5, we report DiD estimates comparing individuals employed in conventional firms and members of worker cooperatives, while columns (4) and (6) display estimates only comparing employees in conventional firms and worker cooperatives. Interestingly, the differential behavioural response of affected individual employed in worker cooperatives in terms of both incidence and duration of absence spells is entirely driven by the behaviour of cooperative members.

Extensive margin responses by disease-categories. In this section, we further investigate extensive margin responses of sickness absence to the paid sick leave reform by exploiting information on doctor-certified disease categories. Using medical diagnosis classified according to the International Classification of Diseases (ICD), we analyse six broad categories of diseases: musculoskeletal, infectious, respiratory[18] , mental, poisoning, and pregnancy complications. The anatomy of responses by certified disease categories may be informative of the underlying mechanisms behind individuals' behavioural responses in worker cooperatives. In particular, the comparison between labour supply adjustments for musculoskeletal (e.g. back pain) and infectious diseases has proved helpful in unpacking responses to paid sick leave in terms of shirking behaviour and contagious presenteeism (Pichler and Ziebarth 2017).

17. Interviews with managers of the world's biggest (and recently demised) industrial worker cooperative indicate that members' absenteeism was an important concern: *"The moment they became members, their sense of commitment just slipped away.(. . .) Being a member was almost like being in the public service. Absenteeism skyrocketed, especially on Mondays. I think it was a lack of commitment. And I think Human Resources should have come down harder on them"*(Basterretxea, Heras-Saizarbitoria, and Lertxundi 2019, p.592).

18. Respiratory diseases are part of a mixed category including both contagious and noncontagious diseases.

Information on disease categories for each sickness spell is only available from 2010 onward. Hence, we redefine our treatment and control groups and compare the pre-reform (2011-2012) and post-reform period (2013), exploiting the increase in the benefit cap that came into force in January 2013. Table 6 displays our disease-specific DiD estimates. We observe a differential increase in the incidence of musculoskeletal conditions for treated individuals in worker cooperatives relative to the other group. This category includes hard-to-diagnose conditions (e.g. back pain) and is more prone to moral hazard reporting problems.

Marginal utility of leisure: extended weekends absences. We further exploit the granularity of the data to see whether sickness absences in cooperatives are more frequent on days in which leisure may confer greater marginal utility. A crucial advantage of the data is that we know the precise start and end date of each sickness spell.[19]

As a first approximation, we investigate the existence of a "Monday effect." Figure 9 plots the distribution of sickness spells by day of first report.[20] If the start of a sickness spell is randomly distributed over the week, one should expect 20% of them to start on Monday. We observe that an excess proportion (5 percentage points) of spells started on Mondays. The pattern appears to be very similar for individuals employed in cooperatives and conventional firms. In Table 7, we report additional DiD estimates of the incidence of sickness spells by the day of first report. There are individuals with multiple absence spells in a given month. For this reason, estimates consider the day of first report of each absence spell in a given month. There is no evidence of a differential increase in extended weekend absences (Monday/Friday) for treated individuals employed in cooperatives compared to the other groups.

Labour discipline. We also investigate whether documented differences in absence behaviour between individuals employed in cooperatives and conventional firms could be explained by the use of more punitive labour discipline strategies in conventional firms. It is a well-established fact that worker cooperatives have more stable employment and destroy fewer jobs than conventional firms (Craig and Pencavel 1992; Pencavel, Pistaferri, and Schivardi 2006; Burdín and Dean 2009; Alves, Burdín, and Dean 2016). Union members are also less likely to lose their jobs than non-members, which, in turn, may explain why they react more strongly to variations in paid sick leave (Goerke and Pannenberg 2011, 2015). It is natural to think that a similar mechanism could be at work when employees have full bargaining power as in worker cooperatives.

19. The existence of the so-called "Monday effect" has been studied in the context of U.S. workers' compensation programs providing insurance against work-related injuries (Card and McCall 1996; Campolieti and Hyatt 2006). Related papers have analysed the impact of pubic holidays, weather conditions, sport events, and birthdays on absence behaviour (Böheim and Leoni 2019, Shi and Skuterud 2015, Thoursie 2004, Thoursie 2007).

20. In Appendix Figure A.1.1 and A.1.2, we report the distribution of sickness spells by disease categories and day of first report.

We adopt a similar DiD approach, comparing the evolution of dismissal rates between treated and control workers in both types of firms before and after the increase in sick leave pay. We identified dismissed individuals in each month by relying on both administrative information on the cause of separation (i.e. dismissal) and whether the individual was receiving unemployment benefits. In this way, we are able to restrict the analysis to layoffs, excluding other types of separations (voluntary terminations, retirement, etc.).

Table 9 shows estimates of equation (2) in which the dependent variable is a dummy indicating that a worker has been fired in the respective month. Our preferred estimates reported in column (2) indicate that the probability of being individually dismissed is 0.8 percentage points lower among treated workers employed in worker cooperatives relative to other groups. Considering the average pre-reform dismissal rate (1%), the magnitude of the effect is large. In column (3)-(5), we show that differences in the use of layoffs are explained by individuals employed in large firms and are larger for worker-members.

Figure 10 reports the results from an event-study analysis in which we track differences in dismissal rates before and after the paid sick leave reform. As in column (5) of 9, we consider individuals employed in conventional firms and members of worker cooperatives. Each estimated coefficient corresponds to the interaction between $T_t \times ConventionalFirm_{it}$ and a full set of year dummies, where the coefficient for 2010 is normalized to zero. The differential increase in dismissal rates for treated workers employed in conventional firms relative to cooperatives becomes positive and significant from 2011 onward. We observe broadly similar trends in the likelihood of dismissal before the reform, although there is a statistically significant violation of parallel pre-trends in 2008. Our analysis of the dynamics of layoffs is at least suggestive that conventional firms relied on more punitive labour discipline strategies than did cooperatives and were more prone to use the threat of dismissal after the reform.

Small vs. large firms. It has been argued that cooperative teams and profit sharing arrangements may suffer from weak work incentives (Alchian and Demsetz 1972). However, the extent of free riding may vary with the size of the team. Large teams may be particularly vulnerable to shirking behaviour (*1/n problem*). By contrast, in small teams, the dilution of incentives may be less severe and shrinking could be mitigated through mutual monitoring among members without relying on specialised supervisors. To check for this mechanism, in Table 8 we present additional estimates splitting the sample by firm size. We define small firms as those with less than 20 workers. The differential increase of absenteeism in cooperatives holds only for individuals employed in medium-sized and large firms, though differences appear to be larger in the subsample of large firms. In the next section, we report additional survey evidence on managers' perceptions about work ethics in both types of firms, confirming that large cooperatives were particularly affected by absenteeism after the reform.[21]

6 Worker supervision and managers' perceptions of work ethics: additional survey evidence

To further sharpen the interpretation of our findings, we provide additional firm-level survey evidence on managers' perceptions about absenteeism and work ethics collected before (2009) and after (2012) the reform. We collected information on a sample of roughly 400 Uruguayan firms per wave, including both worker cooperatives and conventional firms. By design, the comparison group of conventional firms mimics the sectoral and size distribution of cooperatives. In what follows, we restrict the analysis to firms that responded to the survey in both waves.

In Appendix Figure A.1.3 (Panel A), we report managers' responses to the following question: *¿Could you rank the most pressing human resource management problems faced by your company during the last year?* The evidence suggests that absenteeism is perceived as the main HRM problem in medium-large cooperatives. Moreover, concerns about absenteeism among managers of large cooperatives increased sharply between survey waves, coinciding with the implementation of the reform. In Figure A.1.3 (Panel B), we report responses to the following question: *What is your perception of work attitudes that predominate among individuals employed in your company?* This question is only available for the post-reform wave. The share of managers perceiving a low or very low work ethics in their companies is larger among managers of medium-large cooperatives. By contrast, poor work ethics does not seem to be a problem for small cooperatives. Altogether, survey evidence appears to be broadly consistent with our DiD estimates by firm size.

To further understand the distinct labour discipline environment of worker cooperatives, we report information on supervision intensity and monitoring mechanisms. In Figure A.1.4 (Panel A), we display the supervision intensity by firm size and organisational form. We define the supervision ratio as the number of supervisors divided by total employment.[22] In the case of small firms, supervision intensity appears to be lower in worker cooperatives than in conventional firms. By contrast, large firms

21. In Appendix A.1.1, we present a complementary empirical exercise comparing individuals' absence behaviour before and after a worker buyout, i.e. the conversion of a conventional firm into a worker cooperative. We distinguish worker buyouts of small and large firms. Interestingly, we only observe a significant increase in absenteeism after a worker buyout of a large firm.

22. Specifically, the questionnaire asks managers to report the number of workers performing supervision tasks. Following Wright 1995 and Jayadev and Bowles 2006, supervisors are defined as workers that have more than one subordinate and can make decisions regarding the tasks, the tools or procedures to be used, and the pace of work of their subordinates. They can also sanction (or cause to be sanctioned) with respect to pay, promotions or job termination.

exhibit roughly similar supervision ratios, regardless of their organisational form. Finally, we ask managers to report the main mechanism used by the firm to monitor and enforce work effort. In Figure A.1.4 (Panel B and C), we show that hierarchical monitoring by specialized supervisors (*"Verbal warnings from supervisors"*) is more common in conventional firms, while mutual monitoring among coworkers (*"Verbal warnings from coworkers"*) is more frequent in cooperatives. Interestingly, despite exhibiting a roughly similar supervision intensity, supervisors in medium-large cooperatives are perceived as less active in enforcing labour discipline than supervisors in conventional firms of similar size. Surprisingly, peer monitoring is also a relevant disciplinary mechanisms among medium-sized and large cooperatives. However, the documented differences in absence behaviour and perceived work ethics between cooperatives of different sizes suggest that peer monitoring constitutes a feasible substitute for hierarchical supervision only in the context of small cooperatives.

7 Conclusions

In this paper we aim at understanding individuals' effort choices, proxied by absence behaviour, across different organisational settings. Using monthly employment history data matched with individual-level sick leave records and exploiting an exogenous increase in the paid sick leave maximum cap in Uruguay, we compare the absence behaviour of individuals employed in worker cooperatives and in conventional firms. A worker cooperative constitutes a rather peculiar organisational setting in which worker-members have a stake in ownership and ultimately controlled managerial decisions.

We find a differential increase in absence behaviour among treated individuals employed in a worker cooperative relative to individuals employed in conventional firms. Differences between the two groups are driven by both short-term and long-term absences, members' behaviour, hard-to-diagnose conditions, and individuals employed in medium-sized and large cooperatives. We also find suggestive evidence that, relative to worker cooperatives, conventional firms employ dismissals more frequently as a disciplinary tool to reduce absenteeism after the reform. Small cooperatives did not suffer from a similar increase in absenteeism. Altogether, our findings indicate that conventional effort supply responses driven by moral hazard account for at least part of the differential increase in absenteeism among workers in cooperatives. Survey evidence on managers' perceptions suggests lower perceived work ethics in large cooperatives, where peer monitoring may be less feasible as an alternative labour discipline device.

The social welfare implications of individuals' behaviour under the two organisational settings are not straightforward. On the one hand, our findings suggest that a potential non-pecuniary benefit from cooperative membership could be a more discretionary utilisation of voluntary absences. This may come at a cost in terms of firm output, particularly in the context of large cooperative teams. On the other hand, conventional firms require the use of layoffs to enforce labour discipline and keep absenteeism under control. This entails potential negative externalities as firms do not fully internalise the consequences of layoffs for individual welfare and public finances. Moreover, workers may underutilize sick leave insurance, leading to potential problems of contagious presenteeism, reduced productivity, and additional costs to public health services. Further research could analyze how differences in absence behaviour map into productivity gaps between the two types of firms. The answer is not obvious as organisations may differ in their ability to replace absent workers and avoid disruptions in the production process.

References

Abramitzky, Ran. 2009. "The effect of redistribution on migration: Evidence from the Israeli kibbutz." *Journal of Public Economics* 93 (3): 498–511.

———. 2008. "The Limits of Equality: Insights from the Israeli Kibbutz*." *The Quarterly Journal of Economics* 123 (3): 1111–1159.

Alchian, Armen A., and Harold Demsetz. 1972. "Production, Information Costs, and Economic Organization." *The American Economic Review* 62 (5): 777–795.

Alves, Guillermo, Gabriel Burdin, Paula Carrasco, Andrés Dean, and Andrés Rius. 2012. "Empleo, remuneraciones e inversión en cooperativas de trabajadores y empresas convencionales: nueva evidencia para Uruguay," nos. 12-14.

Alves, Guillermo, Gabriel Burdín, and Andrés Dean. 2016. "Workplace democracy and job flows." *Journal of Comparative Economics* 44 (2): 258–271.

Amarante, Verónica, and Andrés Dean. 2017. "Estudio sobre el subsidio por enfermedad en el Uruguay." *CEPAL - Serie Estudios y Perspectivas* 32:1–64.

Arando, Saioa, Monica Gago, Derek C. Jones, and Takao Kato. 2015. "Efficiency in Employee-Owned Enterprises: An Econometric Case Study of Mondragon." *ILR Review* 68 (2): 398–425.

Arnold, Daniel, Tobias Brändle, and Laszlo Goerke. 2018. "Sickness Absence and Works Councils: Evidence from German Individual and Linked Employer–Employee Data." *Industrial Relations: A Journal of Economy and Society* 57 (2): 260–295.

Basterretxea, Imanol, Iñaki Heras-Saizarbitoria, and Aitziber Lertxundi. 2019. "Can employee ownership and human resource management policies clash in worker cooperatives? Lessons from a defunct cooperative." *Human Resource Management* 58 (6): 585–601.

Ben-Ner, Avner, John Michael Montias, and Egon Neuberger. 1993. "Basic Issues in Organizations: A Comparative Perspective." *Journal of Comparative Economics* 17 (2): 207–242.

Bennedsen, Morten, Margarita Tsoutsoura, and Daniel Wolfenzon. 2019. "Drivers of effort: Evidence from employee absenteeism." JFE Special Issue on Labor and Finance, *Journal of Financial Economics* 133 (3): 658–684.

Blasi, Joseph R., Richard B. Freeman, Christopher Mackin, and Douglas L. Kruse. 2010. "Creating a Bigger Pie? The Effects of Employee Ownership, Profit Sharing, and Stock Options on Workplace Performance." In *Shared Capitalism at Work: Employee Ownership, Profit and Gain Sharing, and Broad-based Stock Options,* 139–165. NBER Chapters. National Bureau of Economic Research, Inc.

Bryson, Alex, and Harald Dale-Olsen. 2017. *Does Sick Pay Affect Workplace Absence?* IZA Discussion Papers 11222. Institute of Labor Economics (IZA).

———. 2019. "The Role of Employer-provided Sick Pay in Britain and Norway." In *Health and Labor Markets,* 47:227–252. Emerald Publishing Limited.

Burdin, Gabriel. 2016. "Equality Under Threat by the Talented: Evidence from Worker-Managed Firms." *The Economic Journal* 126 (594): 1372–1403.

Burdín, Gabriel, and Andrés Dean. 2009. "New evidence on wages and employment in worker cooperatives compared with capitalist firms." *Journal of Comparative Economics* 37 (4): 517 –533.

Böheim, René, and Thomas Leoni. 2019. *Absenteeism on Bridging Days.* IZA Discussion Papers 12181. Institute of Labor Economics (IZA).

Callaway, Brantly, Andrew Goodman-Bacon, and Pedro H. C. Sant'Anna. 2021. *Difference-in-Differences with a Continuous Treatment.* Papers 2107.02637.

Campolieti, Michele, and Douglas E. Hyatt. 2006. "Further Evidence on the "Monday Effect" in Workers' Compensation." *Industrial and Labor Relations Review* 59 (3): 438–450.

Card, David, and Brian P. McCall. 1996. "Is Workers' Compensation Covering Uninsured Medical Costs? Evidence from the "Monday Effect"." *Industrial and Labor Relations Review* 49 (4): 690–706.

Carpenter, Jeffrey, Samuel Bowles, Herbert Gintis, and Sung-Ha Hwang. 2009. "Strong reciprocity and team production: Theory and evidence." *Journal of Economic Behavior Organization* 71 (2): 221 –232.

Carpenter, Jeffrey, Andrea Robbett, and Prottoy A. Akbar. 2018. "Profit Sharing and Peer Reporting." *Management Science* 64 (9): 4261–4276.

Craig, Ben, and John Pencavel. 1995. "Participation and Productiviy: A Comparison of Worker Cooperatives and Conventional Firms in the Plywood Industry." *Brookings Papers on Economic Activity* 26 (1995 Micr): 121–174.

———. 1992. "The Behavior of Worker Cooperatives: The Plywood Companies of the Pacific Northwest." *American Economic Review* 82 (5): 1083–1105.

Dean, Andres. 2019. "Un análisis del desempeño comparado de empresas recuperadas, otras empresas gestionadas por sus trabajadores y empresas convencionales en Uruguay." *Serie de Documentos de Trabajo del IECON* 2019 (11).

Dong, Xiao yuan, and Gregory K. Dow. 1993. "Does Free Exit Reduce Shirking in Production Teams?" *Journal of Comparative Economics* 17 (2): 472–484.

Dow, Gregory K. 2003. *Governing the Firm.* Cambridge Books. Cambridge University Press.

Ertan, Arhan, Talbot Page, and Louis Putterman. 2009. "Who to punish? Individual decisions and majority rule in mitigating the free rider problem." *European Economic Review* 53 (5): 495–511.

Fakhfakh, Fathi, Virginie Pérotin, and Monica Gago. 2012. "Productivity, Capital, and Labor in Labor-Managed and Conventional Firms: An Investigation on French Data." *ILR Review* 65 (4): 847–879.

Fehr, Ernst, and Simon Gächter. 2000. "Fairness and Retaliation: The Economics of Reciprocity." *The Journal of Economic Perspectives* 14 (3): 159–181.

Garcia-Louzao, Jose. 2021. "Employment and Wages over the Business Cycle in Worker-Owned Firms: Evidence from Spain." *British Journal of Industrial Relations* 59 (2): 418–443.

Goerke, Laszlo, and Markus Pannenberg. 2011. "Trade union membership and dismissals." *Labour Economics* 18 (6): 810 –821.

———. 2015. "Trade union membership and sickness absence: Evidence from a sick pay reform." *Labour Economics* 33:13 –25.

Hamilton, Barton H., Jack A. Nickerson, and Hideo Owan. 2003. "Team Incentives and Worker Heterogeneity: An Empirical Analysis of the Impact of Teams on Productivity and Participation." *Journal of Political Economy* 111 (3): 465–497.

Hansmann, Henry. 1996. *The Ownership of Enterprise.* Harvard University Press.

Harju, Jarkko, Simon Jäger, and Benjamin Schoefer. 2021. "Voice at Work." *CEPR Discussion Papers No. 15874* (March).

Hart, Oliver, and John Moore. 1998. *Cooperatives vs. Outside Ownership.* Working Paper, Working Paper Series 6421. National Bureau of Economic Research.

Henrekson, Magnus, and Mats Persson. 2004. "The Effects on Sick Leave of Changes in the Sickness Insurance System." *Journal of Labor Economics* 22 (1): 87–113.

Hensvik, Lena, and Olof Rosenqvist. 2019. "Keeping the Production Line Running Internal Substitution and Employee Absence." *Journal of Human Resources* 54 (1): 200–224.

Herrmann and Jonah E. Rockoff. 2012. "Worker Absence and Productivity: Evidence from Teaching." *Journal of Labor Economics* 30 (4): 749–782.

Herrmann, Christian Thöni, and Simon Gächter. 2008. "Antisocial Punishment Across Societies." *Science* 319 (5868): 1362–1367.

Holmstrom, Bengt. 1982. "Moral Hazard in Teams." *The Bell Journal of Economics* 13 (2): 324–340.

Ichino, Andrea, and Regina T. Riphahn. 2005. "The Effect of Employment Protection on Worker Effort: Absenteeism during and after Probation." *Journal of the European Economic Association* 3 (1): 120–143.

Jayadev, Arjun, and Samuel Bowles. 2006. "Guard labor." *Journal of Development Economics* 79 (2): 328–348.

Jones, Derek C. 1976. "British economic thought on association of laborers 1848–1974." *Annals of Public and Cooperative Economics* 47 (1): 5–36.

Kandel, Eugene, and Edward P. Lazear. 1992. "Peer Pressure and Partnerships." *Journal of Political Economy* 100 (4): 801–817.

Kremer, Michael. 1997. "Why are Worker Cooperatives So Rare?" *NBER Workin Paper No. 6118.*

Kruse, Douglas. 2022. "Does Employee Ownership Improve Performance?" *IZA World of Labor* 311.

Landais, Camille. 2015. “Assessing the Welfare Effects of Unemployment Benefits Using the Regression Kink Design.” *American Economic Journal: Economic Policy* 7 (4): 243–278.

Ligthart, Paul E. M., Erik Poutsma, and Chris Brewster. 2022. “The development of financial participation in Europe.” *British Journal of Industrial Relations* 60 (3): 479–510.

Macleod, Bentley. 1987. “Behavior and the organization of the firm.” *Journal of Comparative Economics* 11 (2): 207–220.

Macleod, W.B. 1984. *A theory of cooperative teams.* LIDAM Discussion Papers CORE 1984041. Université catholique de Louvain, Center for Operations Research and Econometrics (CORE).

Marie, Olivier, and Judit Vall-Castello. 2022. “Sick Leave Cuts and (Unhealthy) Returns to Work.” *Journal of Labor Economics.*

Monteiro, Natália P., and Odd Rune Straume. 2018. “Are Cooperatives more Productive than Investor-Owned Firms? Cross-Industry Evidence from Portugal.” *Annals of Public and Cooperative Economics* 89 (2): 377–414.

Montero, Eduardo. 2021. “Cooperative Property Rights and Development: Evidence from Land Reform in El Salvador.” *Journal of Political Economy* 0.

Paola, Maria De, Vincenzo Scoppa, and Valeria Pupo. 2014. “Absenteeism in the Italian Public Sector: The Effects of Changes in Sick Leave Policy.” *Journal of Labor Economics* 32 (2): 337–360.

Pencavel, John. 2013. “Worker cooperatives and democratic governance.” Chap. 24 in *Handbook of Economic Organization,* edited by Anna Grandori. Chapters. Edward Elgar Publishing.

———. 2001. *Worker Participation: Lessons from Worker Co-ops of the Pacific Northwest.* Russell Sage Foundation. Accessed February 6, 2023.

Pencavel, John, Luigi Pistaferri, and Fabiano Schivardi. 2006. “Wages, Employment, and Capital in Capitalist and Worker-Owned Firms.” *ILR Review* 60 (1): 23–44.

Pichler, Stefan, and Nicolas R. Ziebarth. 2017. “The pros and cons of sick pay schemes: Testing for contagious presenteeism and noncontagious absenteeism behavior.” *Journal of Public Economics* 156:14 –33.

Putterman, Louis. 1984. “On some recent explanations of why capital hires labor.” *Economic Inquiry* 22 (2): 171–187.

Putterman, Louis, and Gilbert L Skillman. 1992. “The role of exit costs in the theory of cooperative teams.” *Journal of Comparative Economics* 16 (4): 596–618.

Shi, Jingye, and Mikal Skuterud. 2015. “Gone fishing! Reported sickness absenteeism and the weather.” *Economic Inquiry* 53 (1): 388–405.

Thoursie, Peter Skogman. 2007. “Happy birthday! You’re insured! Gender differences in work ethics.” *Economics Letters* 94 (1): 141 –145.

———. 2004. “Reporting sick: are sporting events contagious?” *Journal of Applied Econometrics* 19 (6): 809–823.

Wright, Erik Olin. 1995. *Class Counts: Comparative Studies in Class Analysis.* Cambridge University Press.

Young-Hyman, Trevor, Nathalie Magne, and Douglas Kruse. 2022. “A Real Utopia Under What Conditions? The Economic and Social Benefits of Workplace Democracy in Knowledge-Intensive Industries.” *Organization Science* 0 (0).

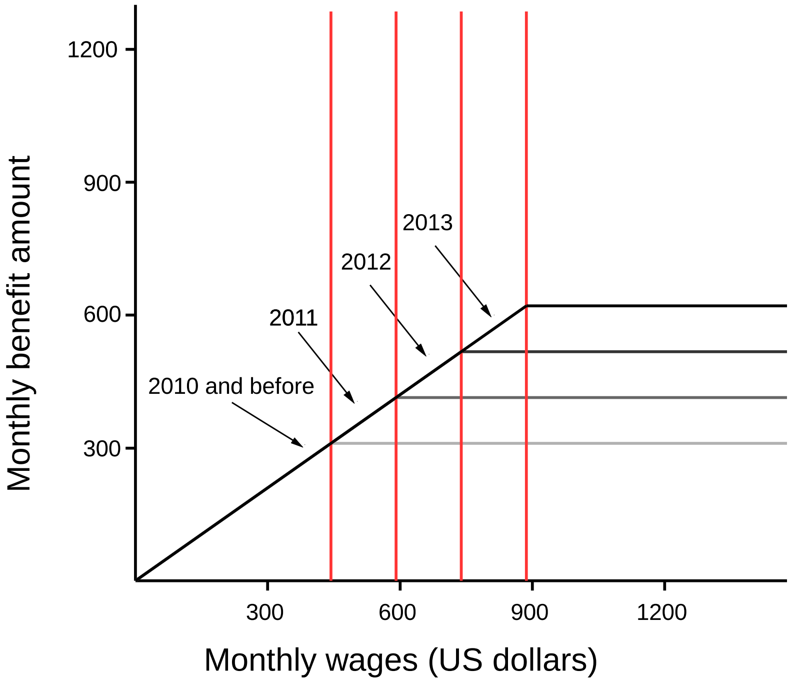
Ziebarth, Nicolas R. 2013. “Long-term absenteeism and moral hazard—Evidence from a natural experiment.” *Labour Economics* 24:277 –292.

Ziebarth, Nicolas R., and Martin Karlsson. 2010. “A natural experiment on sick pay cuts, sickness absence, and labor costs.” *Journal of Public Economics* 94 (11): 1108 –1122.

———. 2014. “The Effects of Expanding the Generosity of the Statutory Sickness Insurance System.” *Journal of Applied Econometrics* 29 (2): 208–230.

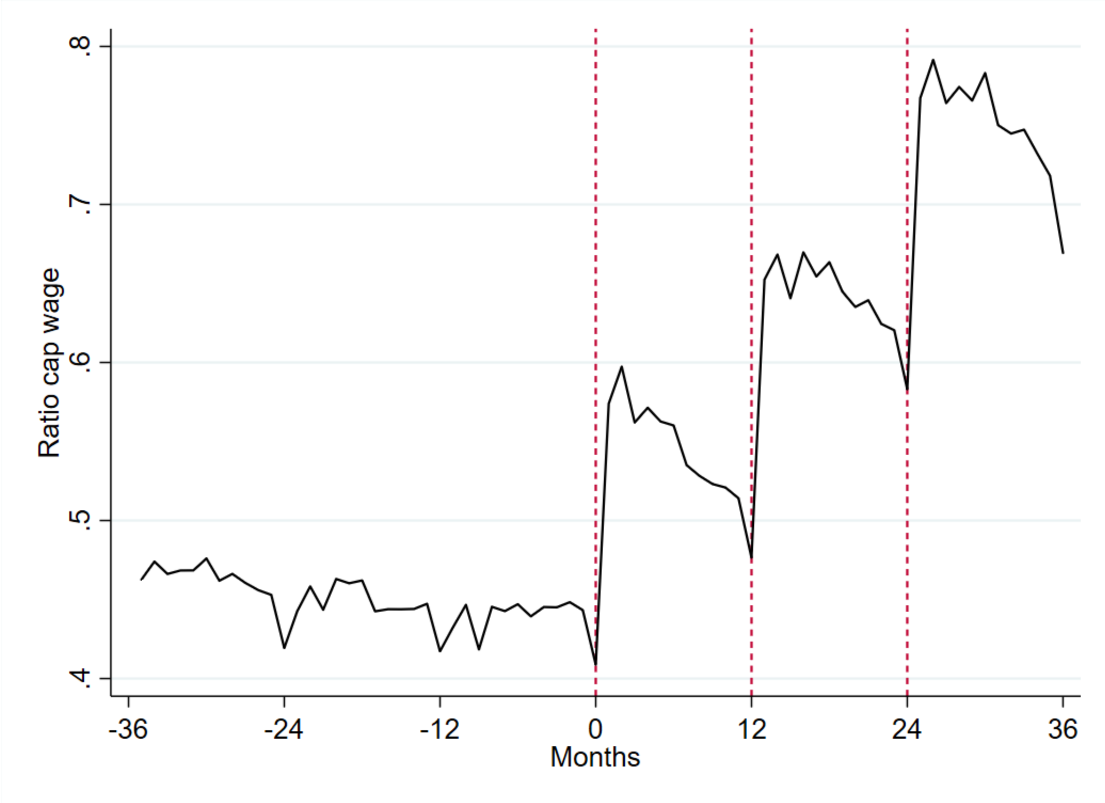
Figures and Tables

Figure 1: Paid sick leave schedule before and after the reform



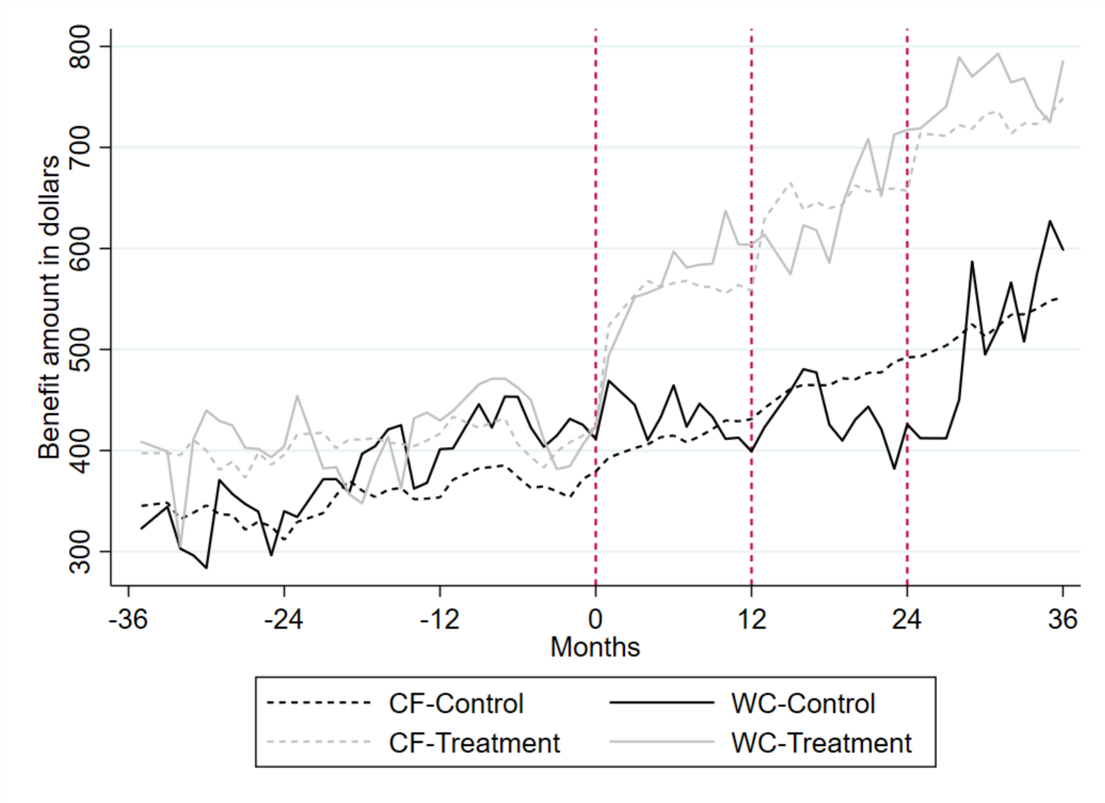
Notes: Authors' elaboration based on provisions of the Sick Leave Insurance Law 18725 (December 2010). The graph shows the evolution of the schedule of the paid sick leave monthly benefit amount in nominal terms (USD) as a kinked function of previous earnings in Uruguay. Changes in the maximum benefit amount also apply to the benefit amount of ongoing spells.

Figure 2: Ratio between paid sick leave benefit cap and average wage



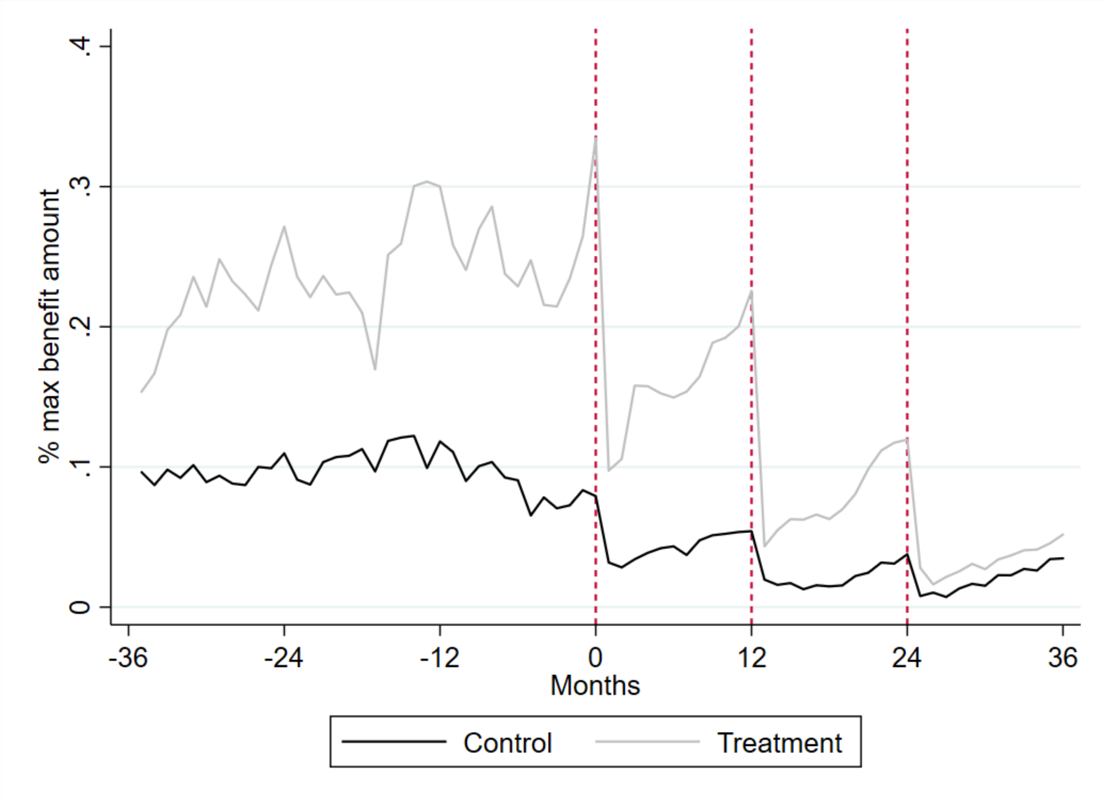
Notes: The graph shows the evolution of the sick pay cap relative to average wages.

Figure 3: Evolution of average paid sick leave by treatment status and organisational form



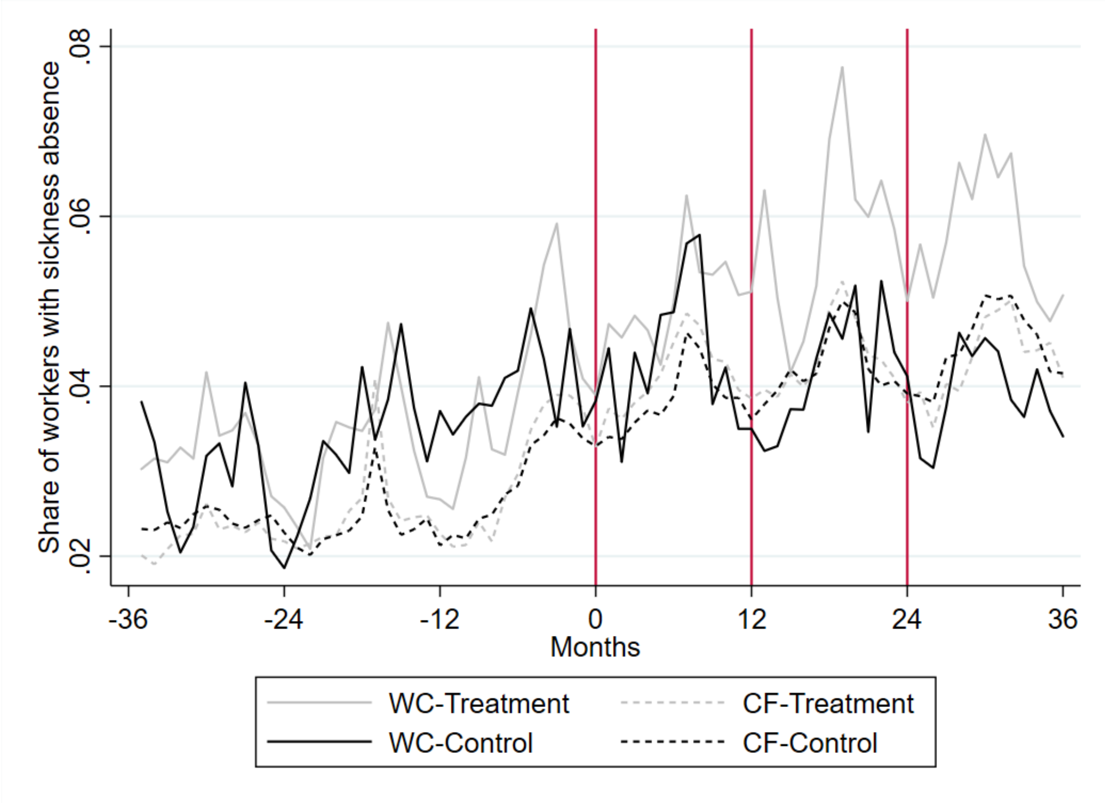
Notes: The graph displays the evolution of average sick pay for treatment and control groups in conventional firms (CF) and worker cooperatives (WC) before and after the reform.

Figure 4: Ratio of workers affected by the benefit cap



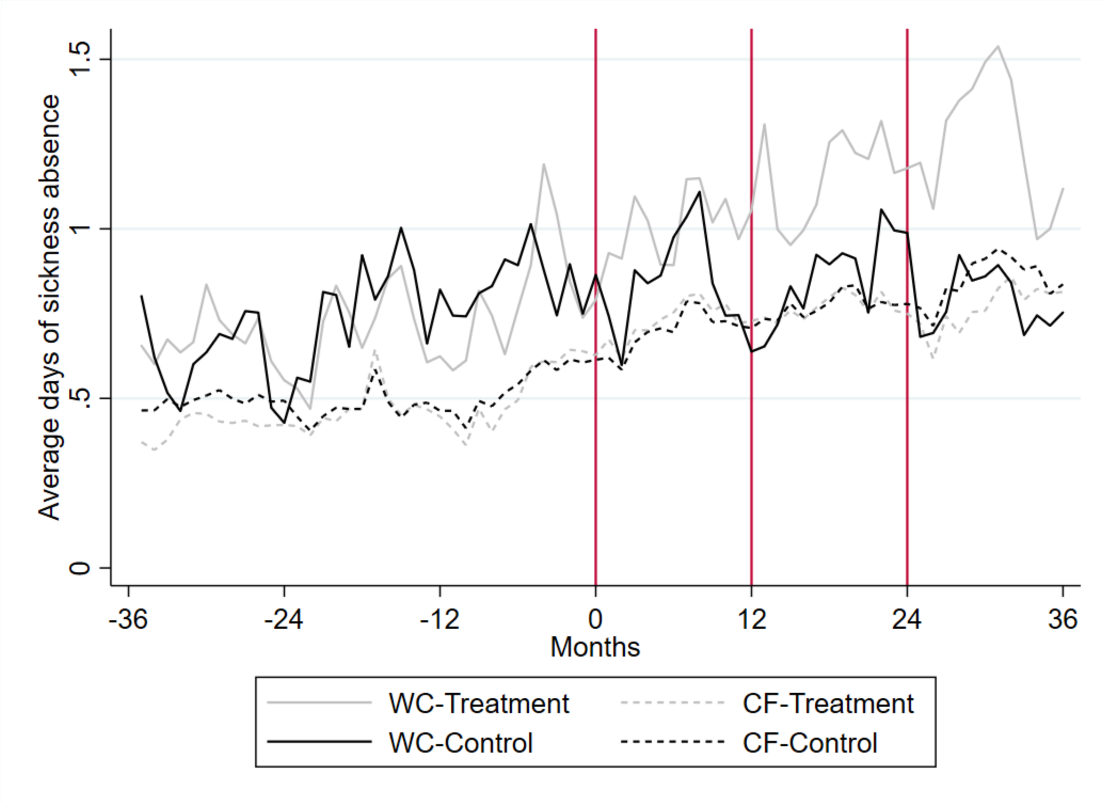
Notes:The graph displays the share of individuals affected by the sick pay cap in treatment and control groups before and after the reform.

Figure 5: Share of workers with sickness absence in each month



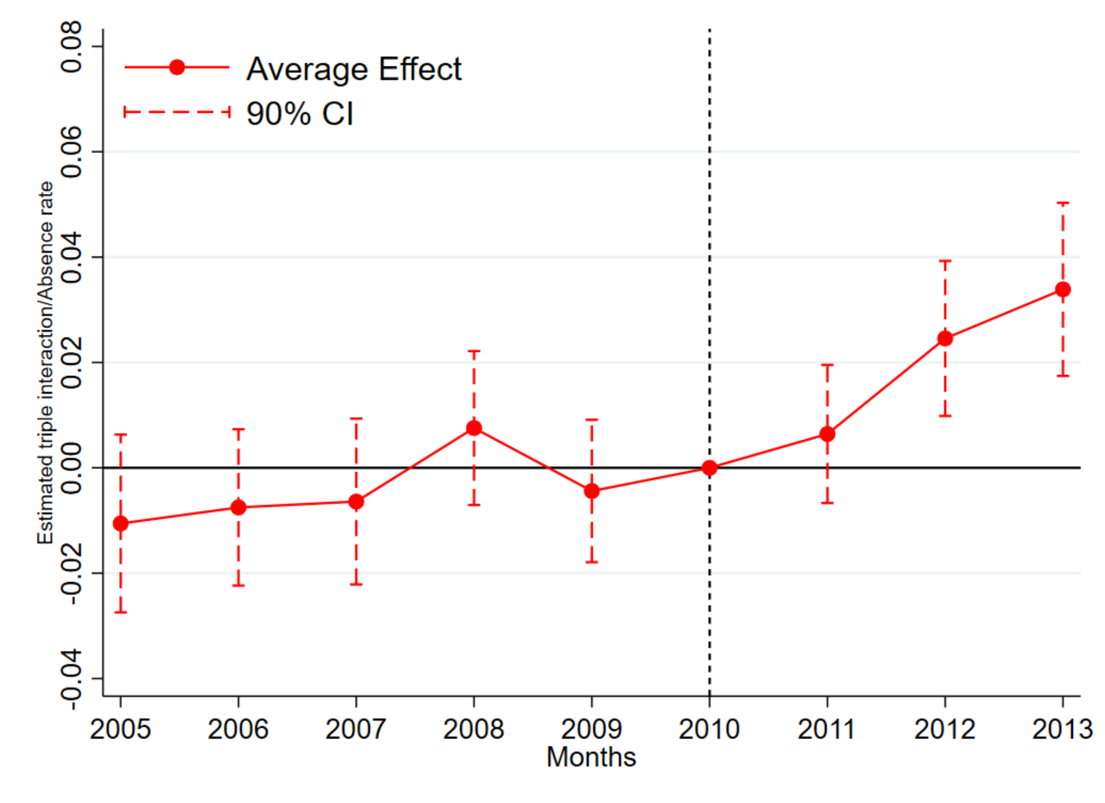
Notes: The graph displays the share of individuals with a sickness-related absence (lasting at least four days) in each month. The figure distinguishes treatment and controls in conventional firms (CF) and worker cooperatives (WC) before and after the reform.

Figure 6: Average duration of sickness absence spells (in days)



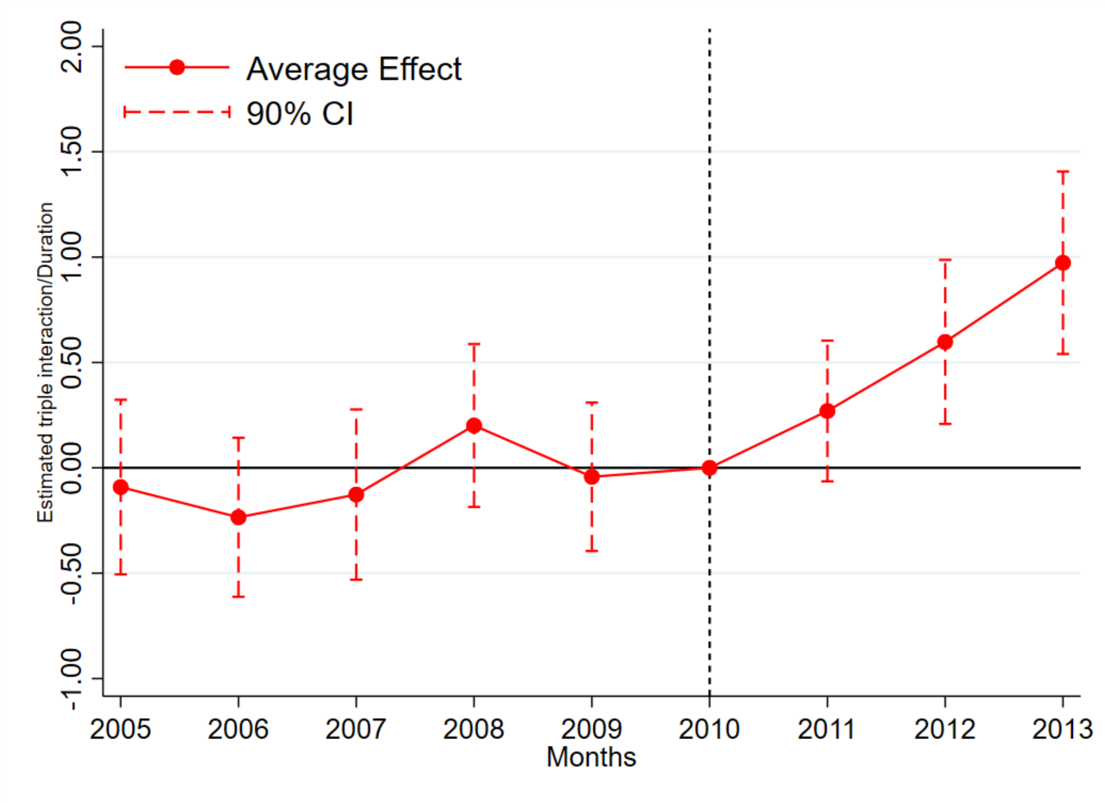
Notes: The graph displays the average duration (in days) of sickness-related absence spells in each month. The figure distinguishes treatment and controls in conventional firms (CF) and worker cooperatives (WC) before and after the reform.

Figure 7: Event-study analysis: incidence of sickness-related absence



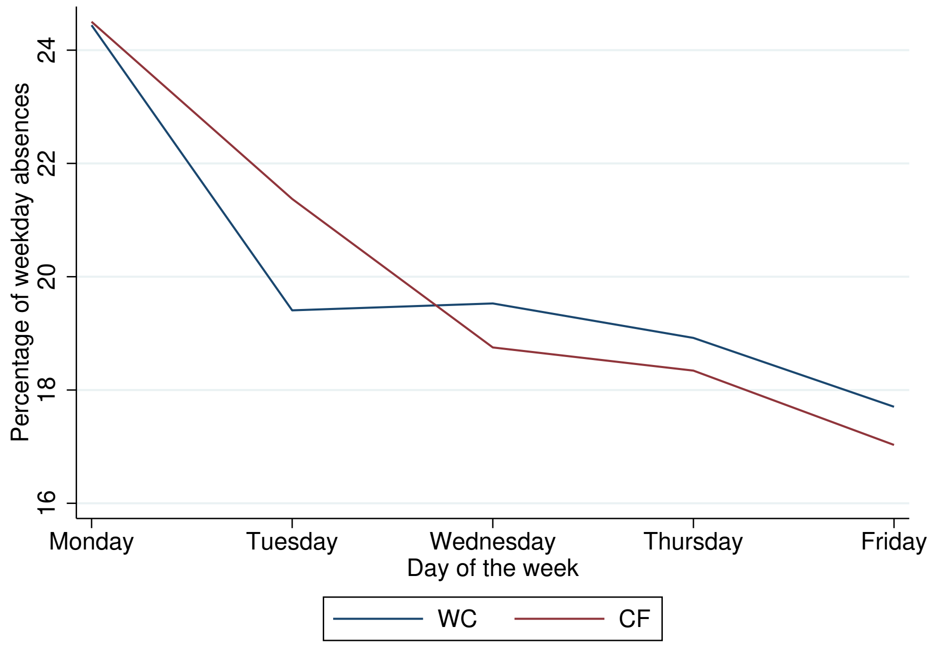
Notes: The figure shows event studies based on a triple DiD model as in Equation (2). Dependent variable: indicator for whether individual i experienced a sickness absence spell (lasting at least four days) in month t. The graph displays the estimated ϕ coefficient associated with the triple interaction term $D_i \times T_t \times Coop_{it}$, i.e. the heterogeneous effect by organisational form (employees in conventional firms vs. members in worker cooperatives). The standard errors are clustered at the individual level and the dash bars depict 90% confidence intervals.

Figure 8: Event-study analysis: duration of sickness-related absence



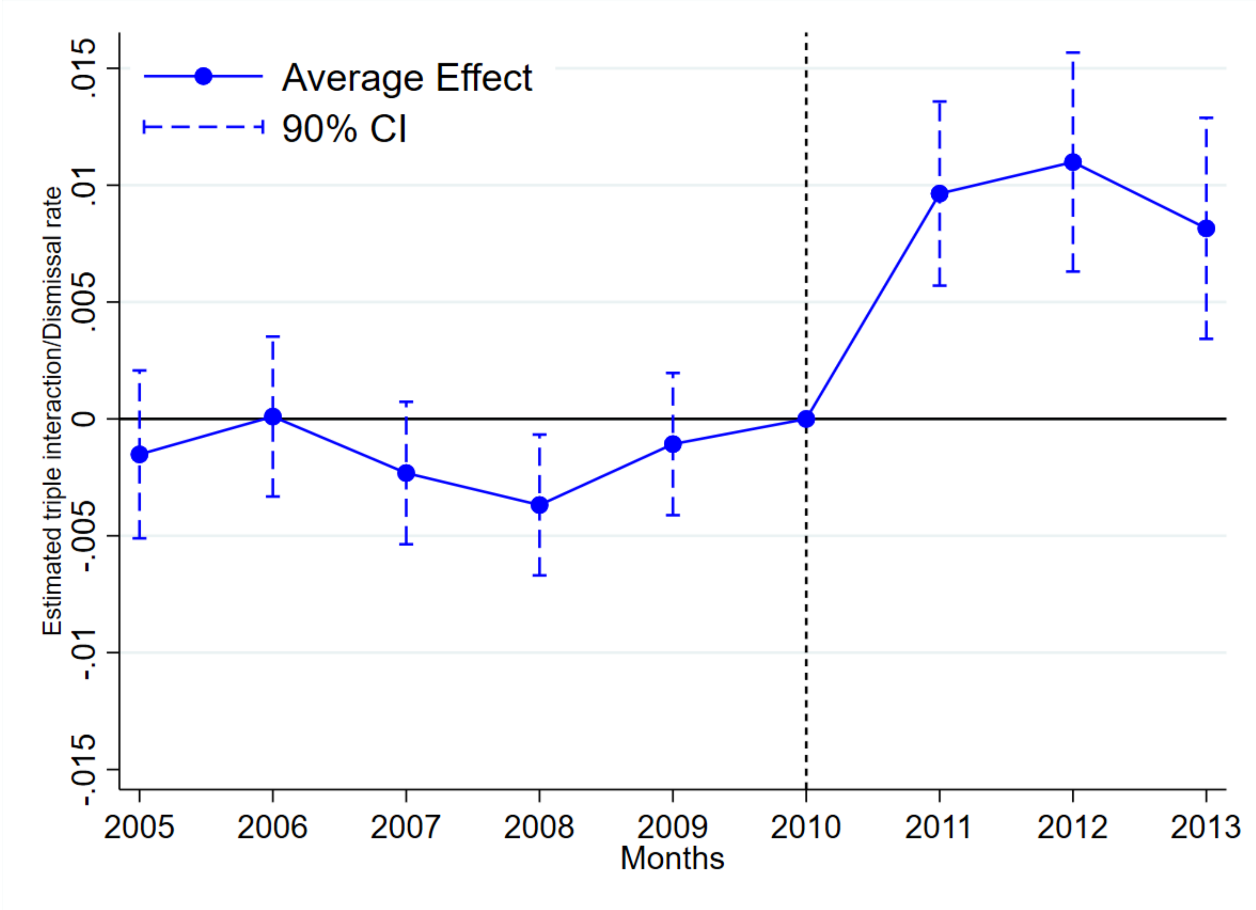
Notes: The figure shows event studies based on a triple DiD model as in Equation (2). Dependent variable: number of days of sickness absence individual i took in month t. The graph displays the estimated ϕ coefficient associated with the triple interaction term $D_i \times T_t \times Coop_{it}$, i.e. the heterogeneous effect by organisational form (employees in conventional firms vs. members in worker cooperatives). The standard errors are clustered at the individual level and the dash bars depict 90% confidence intervals.

Figure 9: Distribution of sickness-related absence spells by day of first report



Notes: The figure displays the distribution of sickness-absence spells by day of first report, distinguishing individuals employed in conventional firms (CF) and worker cooperatives (WC).

Figure 10: Event-study analysis: dismissals



Notes: The figure shows event studies based on a triple DiD model as in Equation (2), but substituting the dummy $Coop_{it}$ for the dummy $ConventionalFirm_{it}$ that takes the value 1 for conventional firms. Dependent variable: indicator for whether individual i experienced a layoff in month t. The graph displays the estimated ϕ coefficient associated with the triple interaction term $D_i \times T_t \times ConventionalFirm_{it}$, i.e. the heterogeneous effect by organisational form (employees in conventional firms vs. members in worker cooperatives). The standard errors are clustered at the individual level and the dash bars depict 90% confidence intervals.

Table 1: Descriptive statistics

	Pre-reform (2008-2010)				Post-reform (2011-2013)			
	Control		Treatment		Control		Treatment	
	CFs	Coops	CFs	Coops	CFs	Coops	CFs	Coops
Incidence of sickness-related absences (monthly)	0.03	0.03	0.03	0.04	0.04	0.04	0.04	0.06
Duration of absence spells (days)	0.50	0.75	0.47	0.73	0.77	0.83	0.76	1.15
Age	36.28	41.45	37.06	46.15	37.62	42.56	39.21	46.86
% Male	0.47	0.44	0.63	0.79	0.48	0.46	0.63	0.75
Tenure (years)	2.97	3.94	4.47	4.96	3.64	4.56	5.62	6.36
Average salary of the firm (log)	2.25	2.04	2.87	2.75	2.62	2.42	3.16	3.01
Number of workers (log)	2.76	3.24	3.73	3.46	2.97	3.53	3.83	3.51
% Part-time worker	0.22	0.47	0.07	0.12	0.22	0.50	0.08	0.16
% Small firms	0.84	0.74	0.72	0.73	0.82	0.71	0.69	0.67
% Manufacturing	0.16	0.16	0.18	0.07	0.15	0.15	0.17	0.08
Average observations by month	18,888	700	16,446	987	20,377	674	15,652	942

Notes: Authors' elaboration based on monthly employment administrative records. Uruguayan Social Security Agency (Banco de Prevision Social).

Table 2: Incidence and duration of sickness-related absence: fixed-effects regressions

	Incidence of sickness-related absence			Duration (days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Coop	0.017***	0.013**	0.013**	0.354***	0.345**	0.325**
	(0.005)	(0.006)	(0.006)	(0.128)	(0.139)	(0.139)
Observations	2,987,831	2,644,898	2,644,898	2,987,831	2,644,898	2,644,898
R-squared	0.161	0.162	0.162	0.199	0.197	0.198
Individual's controls	No	Yes	Yes	No	Yes	Yes
Industry-specific time trends	No	No	Yes	No	No	Yes
Region-specific time trends	No	No	Yes	No	No	Yes

Notes: Individual-level controls include age, male, tenure, firm size (log of total employment), 9 industry dummies, 19 regional dummies ("Departamentos"). Standard errors clustered at the individual level are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3: Difference-in-differences estimates

	Incidence of sickness-related absence			Duration (days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Reform × Treatment	0.002*	0.002*	0.001	0.036	0.035	0.023
	(0.001)	(0.001)	(0.001)	(0.027)	(0.028)	(0.032)
Post-Reform × Treatment × Coop	0.011*	0.014**	0.016**	0.292*	0.357**	0.415**
	(0.006)	(0.006)	(0.008)	(0.151)	(0.154)	(0.208)
Observations	2,395,433	2,395,433	1,719,958	2,395,433	2,395,433	1,719,958
R-squared	0.019	0.020	0.018	0.015	0.015	0.015
Individual's controls	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific time trends	No	Yes	Yes	No	Yes	Yes
Region-specific time trends	No	Yes	Yes	No	Yes	Yes
Only full-time workers aged 18-59 years	No	No	Yes	No	No	Yes

Notes: DiD estimates comparing treatment and control individuals. Estimates reported in columns 3 and 6 are restricted to workers aged 25-55 years old and employed full time. Coop equals 1 for individuals employed in a worker cooperative in a particular month and 0 otherwise. The post-reform variable equals 1 for years 2011-2013 (policy-on period) and 0 for years 2008-2010 (policy-off period). Individual-level controls include age, male, tenure, firm size (log of total employment), 9 industry dummies, 19 regional dummies ("Departamentos"). Standard errors clustered at the individual level are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: Robustness checks: switchers, balanced panel, individual FE, and treatment intensity

	Excluding switchers		Balanced panel		Individual Fixed Effects		Treatment intensity	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Incidence	Duration	Incidence	Duration	Incidence	Duration	Incidence	Duration
Post-Reform × Treatment	0.002*	0.030	0.003**	0.054	-0.001	-0.058**		
	(0.001)	(0.028)	(0.001)	(0.037)	(0.001)	(0.029)		
Post-Reform × Treatment × Coop	0.013*	0.316	0.015*	0.462**	0.013**	0.325**		
	(0.007)	(0.195)	(0.008)	(0.223)	(0.006)	(0.162)		
Post-Reform × Treatment Intensity							0.005	0.087
							(0.005)	(0.119)
Post-Reform × Treatment Intensity × Coop							0.054**	1.376**
							(0.022)	(0.596)
Observations	2,269,160	2,269,160	915,511	915,511	2,395,433	2,395,433	2,395,433	2,395,433
R-squared	0.019	0.015	0.019	0.015	0.191	0.238	0.020	0.015
Individual's controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes						
Industry-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: DiD estimates comparing treatment and control individuals. In columns 1-2, we report estimates excluding individuals who switched between worker cooperatives and conventional firms. In columns 3-4, we restrict the analysis to the balanced panel (individuals with continuous work history 24 month before-after January 2011). In column 5-6, we report estimates including individual fixed effects. In column 7-8, we report estimates using a treatment intensity indicator instead of a binary one. Coop equals 1 for individuals employed in a worker cooperative in a particular month and 0 otherwise. The post-reform variable equals 1 for years 2011-2013 (policy-on period) and 0 for years 2008-2010 (policy-off period). Individual-level controls include age, male, tenure, firm size (log of total employment), 9 industry dummies, 19 regional dummies ("Departamentos"). Standard errors clustered at the individual level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Heterogeneous effects and mechanisms: short-term vs. long-term absences, members vs. employees

	Incidence of sickness-related absence				Duration (days)	
	(1)	(2)	(3)	(4)	(5)	(6)
	Short-term absences	Long-term absences (>6 weeks)	Only members in worker coops	Only hired workers in worker coops	Only members in worker coops	Only hired workers in worker coops
Post-Reform × Treatment	0.001	0.001	0.001	0.004***	0.013	0.013
	(0.000)	(0.001)	(0.001)	(0.001)	(0.030)	(0.030)
Post-Reform × Treatment × Coop	0.004*	0.010*	0.020***	-0.009	0.552***	-0.356
	(0.002)	(0.005)	(0.006)	(0.013)	(0.167)	(0.344)
Observations	2,395,433	2,395,433	2,159,708	2,056,824	2,159,708	2,083,876
R-squared	0.010	0.011	0.018	0.026	0.014	0.014
Individual's controls	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes
Region-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes

Notes: DiD estimates comparing treatment and control individuals. In columns 1-2, we report estimates considering short-term and long-term absences, respectively. In columns 3 and 5, we restrict the analysis to employees in conventional firms and members of worker cooperatives. In columns 4 and 6, we restrict our DiD estimates to employees in both types of firms. Coop equals 1 for individuals employed in a worker cooperative in a particular month and 0 otherwise. The post-reform variable equals 1 for years 2011-2013 (policy-on period) and 0 for years 2008-2010 (policy-off period). Individual-level controls include age, male, tenure, firm size (log of total employment), 9 industry dummies, 19 regional dummies ("Departamentos"). Standard errors clustered at the individual level are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6: Difference-in-differences estimates: incidence of sickness absence by disease categories

	(1) Musculoskeletal	(2) Infectious	(3) Respiratory	(4) Mental	(5) Poisoning	(6) Pregnancy complications
Post-Reform × Treatment	0.000	0.000	0.001**	0.000	-0.000	0.002
	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)
Post-Reform × Treatment × Coop	0.009**	0.000	0.002	0.004	0.004	-0.001
	(0.004)	(0.001)	(0.002)	(0.003)	(0.003)	(0.006)
Observations	853,293	847,206	849,994	848,461	849,816	261,784
R-squared	0.007	0.001	0.002	0.004	0.001	0.005
Individual's controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes
Region-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes

Notes: DiD estimates comparing treatment and control individuals. Estimates reported in columns 6 are restricted to female workers. Coop equals 1 for individuals employed in a worker cooperative in a particular month and 0 otherwise. The post-reform variable equals 1 for 2013 (policy-on period) and 0 for years 2011-2012 (policy-off period). Individual-level controls include age, male, tenure, firm size (log of total employment), 9 industry dummies, 19 regional dummies ("Departamentos"). Standard errors clustered at the individual level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Difference-in-differences estimates: day of first report

	(1) All	(2) Monday	(3) Tuesday	(4) Wednesday	(5) Thursday	(6) Friday	(7) Monday-Friday	(8) Tuesday-Thursday
Post-Reform x Treatment	0.001**	0.000	0.000	0.000	0.000*	0.000	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Post-Reform x Treatment x Coop	0.003*	0.001	0.000	0.001	-0.000	0.000	0.001	0.001
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	2,395,433	2,360,160	2,359,079	2,358,199	2,358,049	2,357,537	2,341,699	2,347,034
R-squared	0.010	0.002	0.002	0.002	0.002	0.002	0.005	0.007
Individual's controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: DiD estimates comparing treatment and control individuals. Coop equals 1 for individuals employed in a worker cooperative in a particular month and 0 otherwise. The post-reform variable equals 1 for 2011-2013 (policy-on period) and 0 for years 2008-2010 (policy-off period). Individual-level controls include age, male, tenure, firm size (log of total employment), average firm wage (in logs), 9 industry dummies, 19 regional dummies ("Departamentos"). Standard errors clustered at the individual level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Difference-in-differences estimates by firm size

	Incidence of sickness-related absence			Duration (days)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Small firms	Medium firms	Large firms	Small firms	Medium firms	Large firms
Post-Reform × Treatment	-0.002*	-0.002	0.006**	-0.071**	-0.008	0.185**
	(0.001)	(0.002)	(0.003)	(0.033)	(0.057)	(0.074)
Post-Reform × Treatment × Coop	0.009	0.020**	0.034**	0.199	0.445*	0.823**
	(0.007)	(0.010)	(0.015)	(0.177)	(0.259)	(0.403)
Observations	1,184,625	584,913	625,895	1,184,625	584,913	625,895
R-squared	0.006	0.011	0.023	0.005	0.010	0.021
Individual's controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes
Region-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes

Notes: DiD estimates comparing treatment and control individuals. Coop equals 1 for individuals employed in a worker cooperative in a particular month and 0 otherwise. The post-reform variable equals 1 for years 2011-2013 (policy-on period) and 0 for years 2008-2010 (policy-off period). Individual-level controls include age, male, tenure, firm size (log of total employment), 9 industry dummies, 19 regional dummies ("Departamentos"). Standard errors clustered at the individual level are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 9: Difference-in-differences estimates: probability of being dismissed

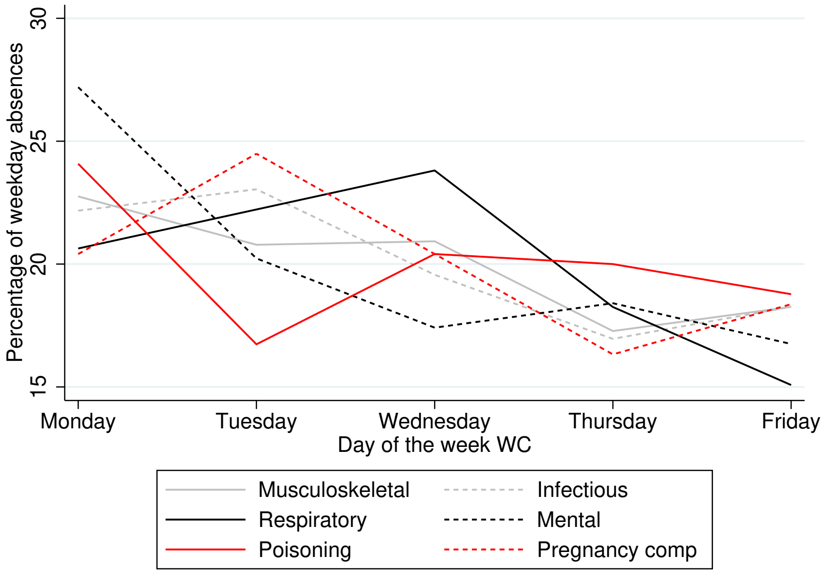
			Small Firms	Large Firms	Only members in worker coops (18-59 years)
	(1)	(2)	(3)	(4)	(5)
Post-Reform × Treatment	0.002***	0.002***	0.001**	0.006***	0.002***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
Post-Reform × Treatment × Coop	-0.007***	-0.008***	-0.002	-0.020***	-0.011***
	(0.001)	(0.001)	(0.001)	(0.004)	(0.002)
Observations	2,362,933	2,362,933	1,169,451	616,779	2,040,093
R-squared	0.005	0.005	0.004	0.010	0.004
Individual's controls	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Industry-specific time trends	No	Yes	Yes	Yes	Yes
Region-specific time trends	No	Yes	Yes	Yes	Yes

Notes: DiD estimates comparing treatment and control individuals. Dependent variable: indicator for whether individual i experienced a layoff in month t. Coop equals 1 for individuals employed in a worker cooperative in a particular month and 0 otherwise. The post-reform variable equals 1 for 2011-2013 (policy-on period) and 0 for years 2008-2010 (policy-off period). Individual-level controls include age, male, tenure, firm size (log of total employment), 9 industry dummies, 19 regional dummies ("Departamentos"). Standard errors clustered at the individual level are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

APPENDIX

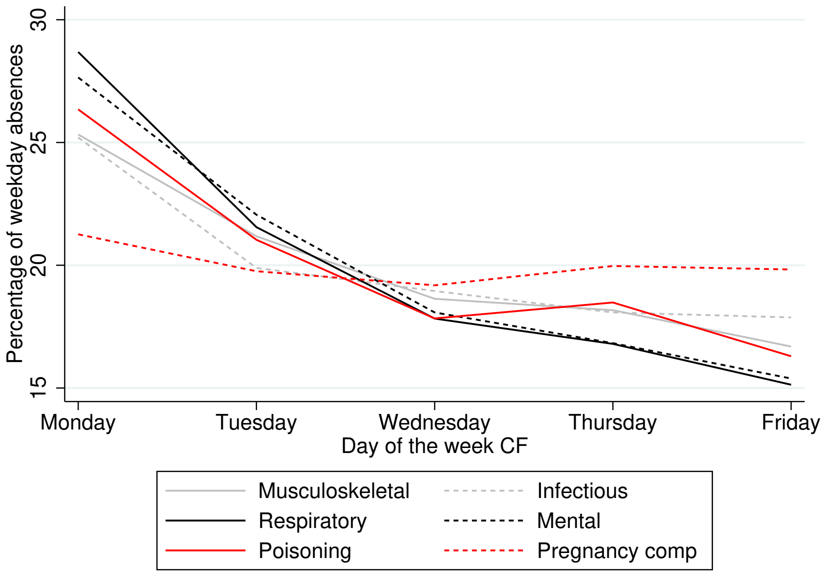
A.1 Additional Results: Figures and Tables

Figure A.1.1: Distribution of sickness-related absence spells by day of first report and disease category (Worker Cooperatives)



Notes: The figure displays the distribution of sickness-absence spells by day of first report and disease category for individuals employed in worker cooperatives (WC).

Figure A.1.2: Distribution of sickness-related absence spells by day of first report and disease category (Conventional firms)



Notes: The figure displays the distribution of sickness-absence spells by day of first report and disease category for individuals employed in conventional firms (CF).

Table A.1.1: Robustness checks: flexible DiD specification

	(1) Incidence	(2) Duration
Post-Reform x Treatment	-0.000	-0.006
	(0.001)	(0.030)
Post-Reform x Treatment x Coop	0.012**	0.302*
	(0.006)	(0.154)
Constant	0.000	-0.115
	(0.015)	(0.203)
Observations	2,395,433	2,395,433
R-squared	0.020	0.016

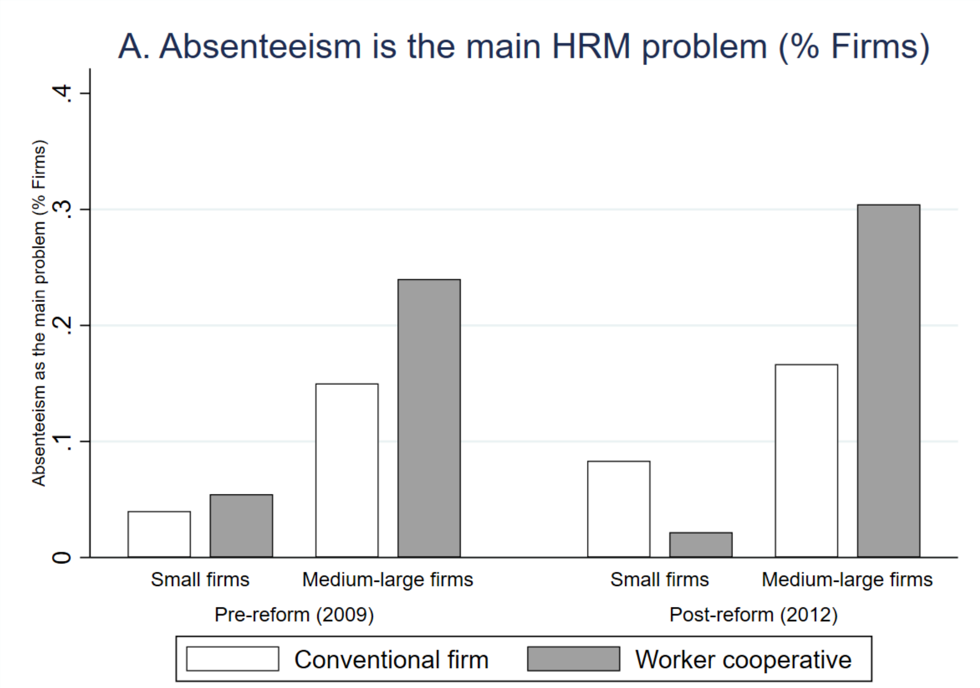
Notes: DiD estimates comparing treatment and control individuals. Coop equals 1 for individuals employed in a worker cooperative in a particular month and 0 otherwise. The post-reform variable equals 1 for years 2011-2013 (policy-on period) and 0 for years 2008-2010 (policy-off period). Individual-level controls include age, male, tenure, firm size (log of total employment), 9 industry dummies, 19 regional dummies ("Departamentos"). Covariates are interacted with treatment status, cooperative status and post-reform period dummies. Standard errors clustered at the individual level are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

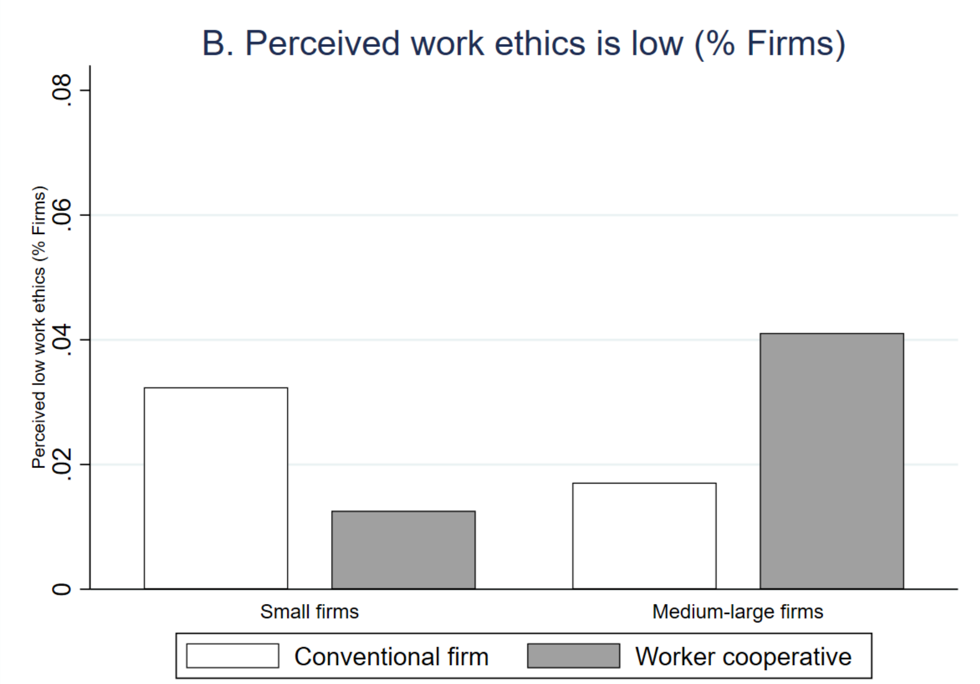
Table A.1.2: Robustness checks: DiD estimates (Controls vs. 3-4 BPCs)

	(1) Incidence	(2) Duration
Post-Reform x Treatment	0.002	0.032
	(0.001)	(0.034)
Post-Reform x Treatment x Coop	0.014*	0.360*
	(0.008)	(0.213)
Observations	1,800,877	1,800,877
R-squared	0.022	0.017
Individual's controls	Yes	Yes
Year dummies	Yes	Yes
Industry-specific time trends	Yes	Yes
Region-specific time trends	Yes	Yes

Notes: DiD estimates comparing treatment and control individuals. Coop equals 1 for individuals employed in a worker cooperative in a particular month and 0 otherwise. The post-reform variable equals 1 for years 2011-2013 (policy-on period) and 0 for years 2008-2010 (policy-off period). Individual-level controls include age, male, tenure, firm size (log of total employment), average firm wage (in logs), 9 industry dummies, 19 regional dummies ("Departamentos"). Treatment group restricted to individuals who were only intended to benefit from the initial sick pay cap hike in January 2011. Standard errors clustered at the individual level are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

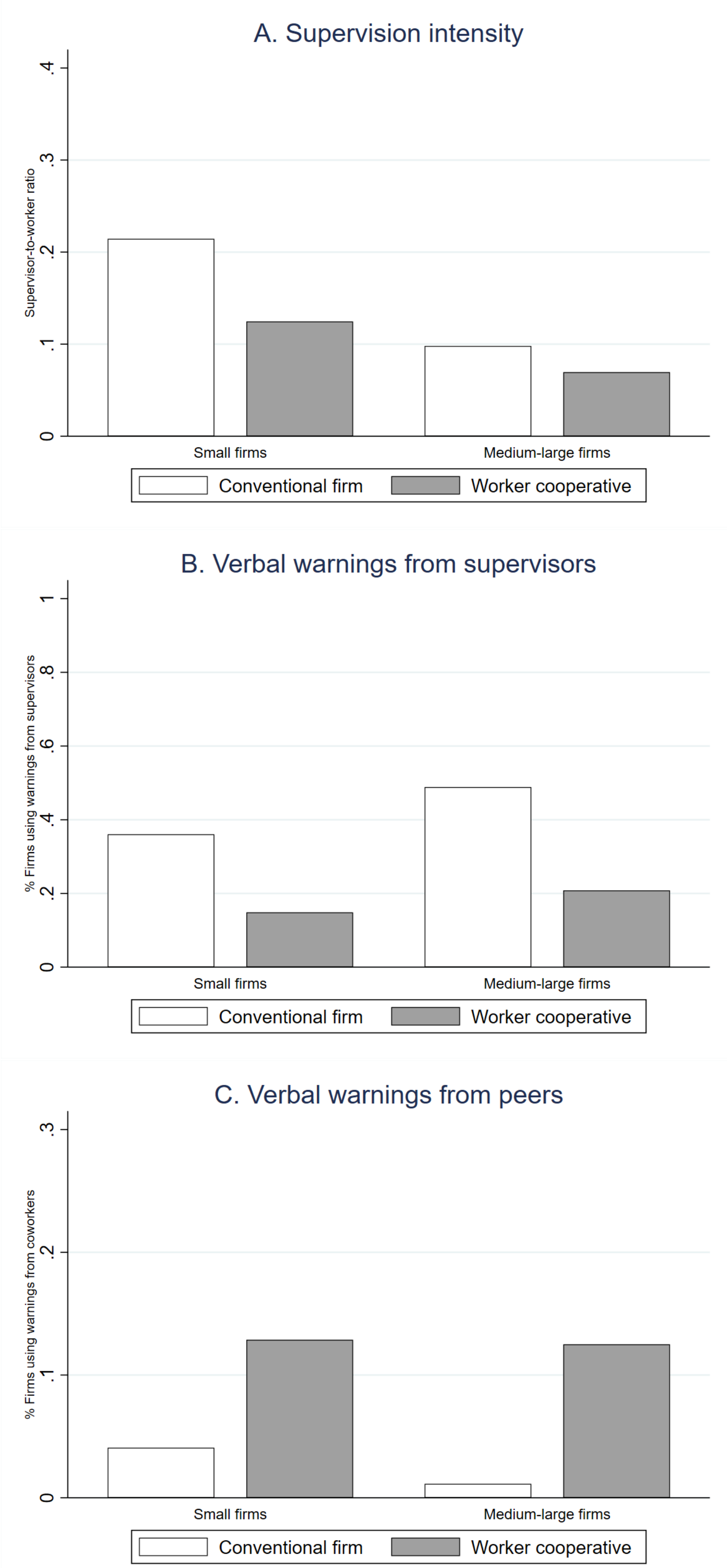
Figure A.1.3: Managers' perceptions about absenteeism by firm size and ownership





Notes: The figure displays the fraction of managers reporting absenteeism as the main HRM problem in the last year (Panel A) and the fraction of managers perceiving that work ethics is low or very low (Panel B). Data from pre-reform (2009) and post-reform (2012) waves of a survey to Uruguayan worker cooperatives and conventional firms of similar size and industry composition. The question on perceived work ethics was introduced in the post-reform wave of the survey. See Section 6 for further details.

Figure A.1.4: Worker supervision and disciplinary mechanisms by firm size and ownership



Notes: The figure displays the supervisor-to-worker ratio (Panel A), the fraction of firms indicating "Verbal warnings from supervisors" as the main disciplinary mechanism (Panel B) and the fraction of firms indicating "Verbal warnings from coworkers" (mutual monitoring) as the main disciplinary mechanism (Panel C). Pooled data from pre-reform (2009) and post-reform (2012) waves of a survey to Uruguayan worker cooperatives and conventional firms of similar size and industry composition. See Section 6 for further details.

Table A.1.3: Heterogeneous effects by age

	Incidence of sickness-related absence			Duration		
	(1)	(2)	(3)	(4)	(5)	(6)
	<35 years	35-49 years	49+ years	<35 years	35-49 years	49+ years
Post-Reform x Treatment	0.003**	0.003	-0.001	0.068*	0.052	-0.006
	(0.001)	(0.002)	(0.003)	(0.035)	(0.055)	(0.068)
Post-Reform x Treatment x Coop	-0.007	0.009	0.028***	-0.139	0.161	0.664**
	(0.008)	(0.012)	(0.011)	(0.177)	(0.329)	(0.284)
Observations	1,147,379	615,668	525,805	1,147,379	615,668	525,805
R-squared	0.020	0.023	0.022	0.016	0.018	0.017
Individual's controls	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes
Region-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes

Notes: DiD estimates comparing treatment and control individuals. Coop equals 1 for individuals employed in a worker cooperative in a particular month and 0 otherwise.The post-reform variable equals 1 for years 2011-2013 (policy-on period) and 0 for years 2008-2010 (policy-off period). Individual-level controls include male, tenure, firm size (log of total employment), 9 industry dummies, 19 regional dummies ("Departamentos"). Standard errors clustered at the individual level are reported in parentheses. *** p¡0.01, **p¡0.05, * p¡0.1

Table A.1.4: Heterogeneous effects by sector

	Incidence of sickness-related absence				Duration			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Manufacturing	Services	Transport	Others	Manufacturing	Services	Transport	Others
Post-Reform x Treatment	0.011***	-0.000	-0.003	-0.015***	0.250***	-0.009	-0.074	-0.413***
	(0.003)	(0.001)	(0.004)	(0.005)	(0.074)	(0.031)	(0.095)	(0.134)
Post-Reform x Treatment x Coop	0.007	0.015*	0.010	0.048**	0.256	0.278	0.300	1.298**
	(0.014)	(0.008)	(0.015)	(0.021)	(0.393)	(0.216)	(0.408)	(0.594)
Observations	519,070	1,515,733	240,486	120,144	519,070	1,515,733	240,486	120,144
R-squared	0.027	0.017	0.014	0.019	0.022	0.012	0.012	0.017
Individual's controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: DiD estimates comparing treatment and control individuals. Coop equals 1 for individuals employed in a worker cooperative in a particular month and 0 otherwise. The post-reform variable equals 1 for years 2011-2013 (policy-on period) and 0 for years 2008-2010 (policy-off period). Individual-level controls include age, male, tenure, firm size (log of total employment), 19 regional dummies ("Departamentos"). Standard errors clustered at the individual level are reported in parentheses. *** p¡0.01, **p¡0.05, * p¡0.1

Table A.1.5: Heterogeneous effects by gender

	Incidence of sickness-related absence		Duration	
	(1)	(2)	(3)	(4)
	Male	Female	Male	Female
Post-Reform x Treatment	0.003**	0.005**	0.039	0.112**
	(0.001)	(0.002)	(0.031)	(0.049)
Post-Reform x Treatment x Coop	0.014**	0.011	0.461***	0.129
	(0.006)	(0.011)	(0.170)	(0.296)
Observations	1,289,192	1,106,241	1,289,192	1,106,241
R-squared	0.009	0.023	0.008	0.018
Individual's controls	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Industry-specific time trends	Yes	Yes	Yes	Yes
Region-specific time trends	Yes	Yes	Yes	Yes

Notes: DiD estimates comparing treatment and control individuals. Coop equals 1 for individuals employed in a worker cooperative in a particular month and 0 otherwise. The post-reform variable equals 1 for years 2011-2013 (policy-on period) and 0 for years 2008-2010 (policy-off period). Individual-level controls include age, tenure, firm size (log of total employment), average firm wage (in logs), 9 industry dummies, 19 regional dummies ("Departamentos"). Standard errors clustered at the individual level are reported in parentheses. *** p¡0.01, **p¡0.05, * p¡0.1

A.1.1 Worker buyouts

As a complementary exercise, we compare absence behaviour before and after the conversion of a conventional firm into a worker cooperative, i.e. a worker buyout. The empirical identification of worker buyouts is not straightforward. For individuals employed in worker cooperatives, we have information about the previous firms at which individuals were employed before joining the worker cooperative.

Following Dean 2019, a worker cooperative that meets the following criteria is considered a worker buyout: (1) more than 50% of the founding members of the worker cooperative were previously employed at the same conventional firm; (2) that conventional firm reduced its workforce by at least 90% either before or in the first operational year of the newly created worker cooperative; (3) both the conventional firm that closed down and the new worker cooperative operate in the same industry. Previous research using similar criteria has identified 58 events of worker buyouts (Dean 2019). For this additional exercise, which is completely independent from our main DiD approach, we only consider 5 worker buyout events that occurred between 2005 and 2013, i.e. the time window for which we have information on sickness-related absences, and restrict the sample to 240 individuals who experienced the two organisational systems at the same firm.

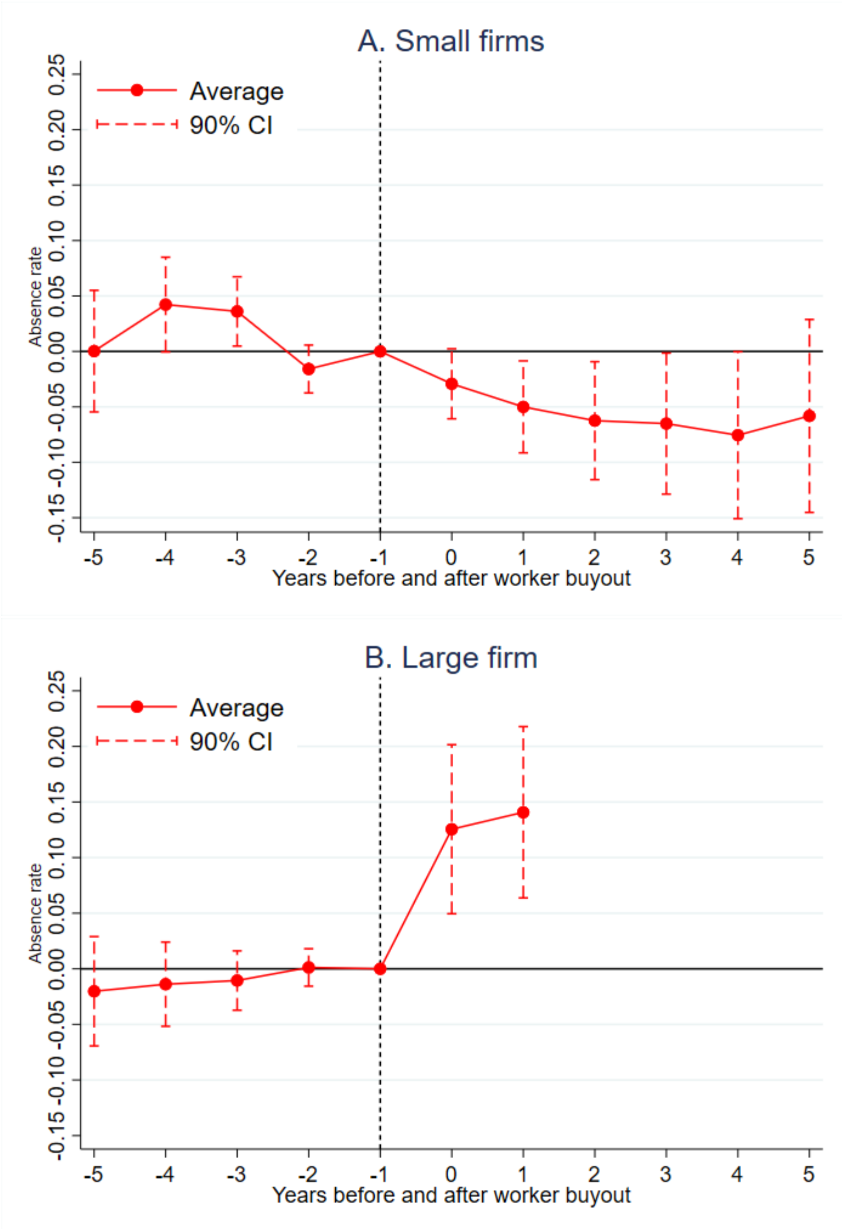
We divide the analysis according to the size of the firms: we observe four worker buyouts of small firms employing less than 20 workers and one worker buyout of a large firm (184 workers). In the latter case, as the buyout occurred in 2011, we can only track individuals under the new cooperative ownership structure for 2 years. To uncover patterns of absence behaviour around worker buyout events, we estimate models of the following form:

$$\mathrm{y}_{it} = \alpha_i + \sum_{j=-5}^{5} \theta_j WBO^j_{it} + \beta' X_{it} + \epsilon_{it}$$

where y_{it} either measures whether individual i experienced a sickness absence spell (lasting at least four days) in month t. X_{it} is a vector personal and firm-level characteristics. Our variables of interest are a series of dummy variables WBO^j_{it} indicating how many years j it has been since the worker buyout at a given time t. We further include individual fixed-effects α_i to account for time-invariant unobservable characteristics.

Figure A.1.5 displays the estimated coefficients of interest considering the year before the worker buyout as the baseline category. We distinguish the case of worker buyouts of small firms and the worker buyout of a large firm. Interestingly, we observe an asymmetric response of absence behaviour depending on firm size. In the case of the large firm, we find a significant increase in the incidence of sickness-related absences after the buyout (Panel B). By contrast, there is some evidence of a reduction in absenteeism for individuals who experienced a worker buyouts at small firms (Panel A). Although broadly consistent with our main analysis documenting differences between small and large cooperatives, these results should be interpreted cautiously, given the small number of cases.

Figure A.1.5: Incidence of sickness-related absences before and after a worker buyout



Notes: The figures displays the estimated coefficients associated with a vector of dummy variables WBO^{j}_{it} indicating how many years j it has been since the worker buyout at a given time t. See Appendix A.1.1