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Regular Article

Property rights, sick pay and effort supply☆

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ABSTRACT

Direct evidence on variations in work incentives across different property rights systems remains scarce. This paper examines absenteeism among individuals employed in worker cooperatives—firms that are ultimately controlled by their workforce. By leveraging employment data matched with sick leave records and reform-induced variation in the generosity of Uruguay's statutory sick pay, we find that absenteeism differentially increased for individuals affected by the policy change and employed in cooperatives. The effect is driven by co-op members, hard-to-diagnose (and, hence, more prone to moral hazard reporting problems) musculoskeletal conditions and large cooperatives. Conventional firms used dismissals more intensely than cooperatives as a threat to keep absenteeism in check after the reform.

1. Introduction

Property rights play a crucial role in the process of economic development (Bardhan et al., 2000; Besley and Ghatak, 2010). Importantly, the allocation of control rights may affect workers' behaviour in the production process (Alchian and Demsetz, 1972; Hansmann, 1996; Hart and Moore, 1990; Dow and Putterman, 2000). However, direct evidence concerning individuals' incentives under different ownership structures remains rare.

Exploiting variation in the generosity of sick pay induced by recent policy changes in Uruguay, this paper studies the heterogeneous response of absenteeism 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 capital suppliers hire labour, appoint managers and have the right to appropriate the residual income. According to estimates included in the Second Global Report on Cooperatives and Employment (Eum, 2017), employment in or within the scope of cooperatives firms in their various forms accounts for 10% of the world's employed population. Cooperatives play a significant role in developing countries, with Latin America, Asia, and Africa accounting for 91% of the world's cooperative organisations, 86% of cooperative members, and 94% of the global workforce engaged in the cooperative sector.[1]

Measures of work effort are hard to observe, given the team-based nature of most production settings. While absenteeism does not capture variations in on-the-job effort, it serves as a useful proxy for worker

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[1] Cooperatives play a significant role in major emerging economies, such as India (Lal, 2023). In Latin America, the cooperative sector has experienced recent growth in countries like Colombia and Chile (Smith and Rothbaum, 2013). It has also gained visibility in manufacturing through worker buyouts (Ruggeri and Vieta, 2015; Pires, 2018; Dean, 2024) and in the context of land reform initiatives (Montero, 2022). Additionally, worker cooperatives have contributed to the economic development of certain European regions, such as the Basque Country and Emilia Romagna. Over several decades, cooperatives played a prominent role in the U.S. plywood industry (Pencavel, 2001).

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effort at the extensive margin. One significant advantage of focusing on absenteeism is that it can be consistently measured at the individual level across various sectors and firms. Absenteeism is 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 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 the London School of Economics and Political Science, rose 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, organise 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 et al., 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 high-effort norms and curb absenteeism in cooperative teams (Kandel and Lazear, 1992; Hamilton et al., 2003; Putterman, 2006; 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] 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 on 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 rose the probability of being absent from work in a given month by 1.6 percentage points more among treated individuals employed in cooperatives relative to those 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. These conclusions remain unchanged when we combine our basic DiD specification with non-parametric coarsened exact matching (Iacus et al., 2012) in order to improve the comparability between individuals employed in cooperatives and conventional firms in terms of observable characteristics. Our 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 merely 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 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

[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).

[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).

[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 et al., 2008; Ertan et al., 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).

motivated by plausibly 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 hired 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 involuntary job separations suggests that conventional firms used 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 in economics. First, we add to a long-standing literature examining how the assignment of control rights over productive assets affects workers' incentives (Alchian and Demsetz, 1972; Hart and Moore, 1998). Specifically, our paper relates to previous research on incentives in worker cooperatives (Sen, 1966; Kremer, 1997; Dow and Putterman, 2000; Putterman, 2006; Dow, 2018) and communal organisations (Abramitzky, 2008, 2009, 2011), and speaks to a series of studies examining the productivity of worker cooperatives vis-à-vis conventional firms (Berman and Berman, 1989; Craig and Pencavel, 1995; Fakhfakh et al., 2012; Pencavel, 2013; Monteiro and Straume, 2018; Young-Hyman et al., 2022; Benveniste, 2024). Most of these studies focus on developed countries and rely on firm-level measures of output or revenue per worker. Instead, our paper contributes to the relatively scant literature on cooperatives in developing countries (Banerjee et al., 2001; Sukhtankar, 2016; Montero, 2022; Lal, 2023). Moreover, our paper exploits detailed administrative data and a quasi-experimental setting to provide for the first time direct evidence of individuals' absence behaviour in worker cooperatives, an extensive-margin proxy of workers' effort.

Second, our study contributes to an important strand of research in development economics focusing on worker absenteeism (Banerjee and Duflo, 2006). A number of studies have explored absenteeism in various contexts within developing countries, such as in health and education services (Kremer et al., 2005; Chaudhury et al., 2006; Duflo et al., 2012), as well as in manufacturing sectors (Adhvaryu et al., 2024). Using unique worker-level administrative data from Uruguay, we contribute to the existing literature by providing the first comparative analysis of worker absenteeism across different types of firm ownership structures.

Finally, this paper contributes to the limited body of research examining labour supply responses to sick leave insurance in developing countries. While existing research has predominantly focused on the United States and European nations (Henrekson and Persson, 2004; Ziebarth and Karlsson, 2010; Ziebarth, 2013; Paola et al., 2014; Ziebarth and Karlsson, 2014; Pichler and Ziebarth, 2017; Bryson and Dale-Olsen, 2017; Böckerman et al., 2018; Marie and Vall-Castello, 2022), there is a paucity of evidence on the incentive effects of paid sick leave systems outside the developed world.[8] Moreover, we contribute to understanding the role of firm organisation and labour institutions in moderating the interplay between sick leave insurance and workplace absenteeism (e.g. Bennedsen et al. (2019)). Previous research has analysed the effect of probationary periods (Ichino and Riphahn, 2005), employment in the public sector (Paola et al., 2014) and trade union membership (Goerke and Pannenberg, 2015). 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 et al., 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.

More closely related to our paper, Goerke and Pannenberg (2015) study the effect of a reduction of statutory paid sick leave using self-reported survey data from Germany. They document 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 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.

The remainder of the paper is organised 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, and provides motivating evidence from a management survey. 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 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.[9,10] 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

[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 et al., 2019).

[8] A notable exception is Barone (2023), who uses administrative data from Chilean workers to analyse the behavioural responses to paid sick leave generosity and derives optimal paid sick leave contracts.

[9] In other words, there is some number between 0 and 0.7, call it replacement rate (r), such that when 0.7w > bmax, the compensation b = r*w. As w rises above (bmax/0.7), r declines.

[10] 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).

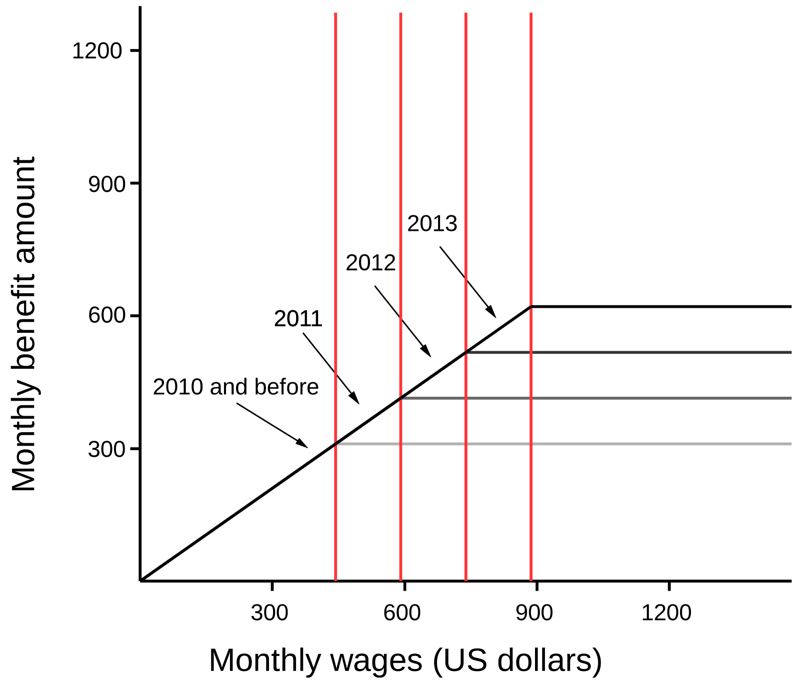

Fig. 1. Paid sick leave schedule before and after the reform.
Notes: Authors' elaboration based on provisions of the Sick Leave Insurance Law 18725 (December 2010). The graph shows the evolution of the schedule of the paid sick leave monthly benefit amount in nominal terms (USD) as a kinked function of previous earnings in Uruguay. Changes in the maximum benefit amount also apply to the benefit amount of ongoing spells.

different social benefits in the Uruguayan social security system.[11] Therefore, the sick leave pay is computed according to the following rule:

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

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.[12] Fig. 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.[13] Fig. 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). Importantly. the reform came into force after a swift approval by the Uruguayan Parliament (Law 18725), making anticipatory responses very unlikely. As shown below, this is further confirmed by the fact that the main outcome variables considered in the analysis show no pre-trends.[14]

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. 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 non-member employees but they must still comply with the legislated maximum ratio in order to receive certain tax advantages — 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, worker cooperatives must have a general workers' assembly that selects a council to supervise the daily operations (the council, in turn, usually selects the

[11] 1 BPC is equivalent to 3848 Uruguayan Pesos (USD 117/January 2018). Source: Banco de Prevision Social.

[12] Firms may top up the minimum statutory sick pay scheme described above by providing fringe benefits. In Uruguay, the provision of complementary social security benefits operated until 2011 through preferential regimes (*"cajas de auxilio"*) agreed between employers and unions in certain sectors and firms. Individuals receiving benefits on top of the statutory regime comprise only 1%–2% of the sample and do not alter our main findings (see Section 3.1 for further details). Of course we cannot rule out the existence of other complementary payment arrangements within worker cooperatives and conventional firms (e.g. through collective bargaining).

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

[14] According to parliamentaryrecords, the discussion and approval of the reform extended from late October to late December 2010.

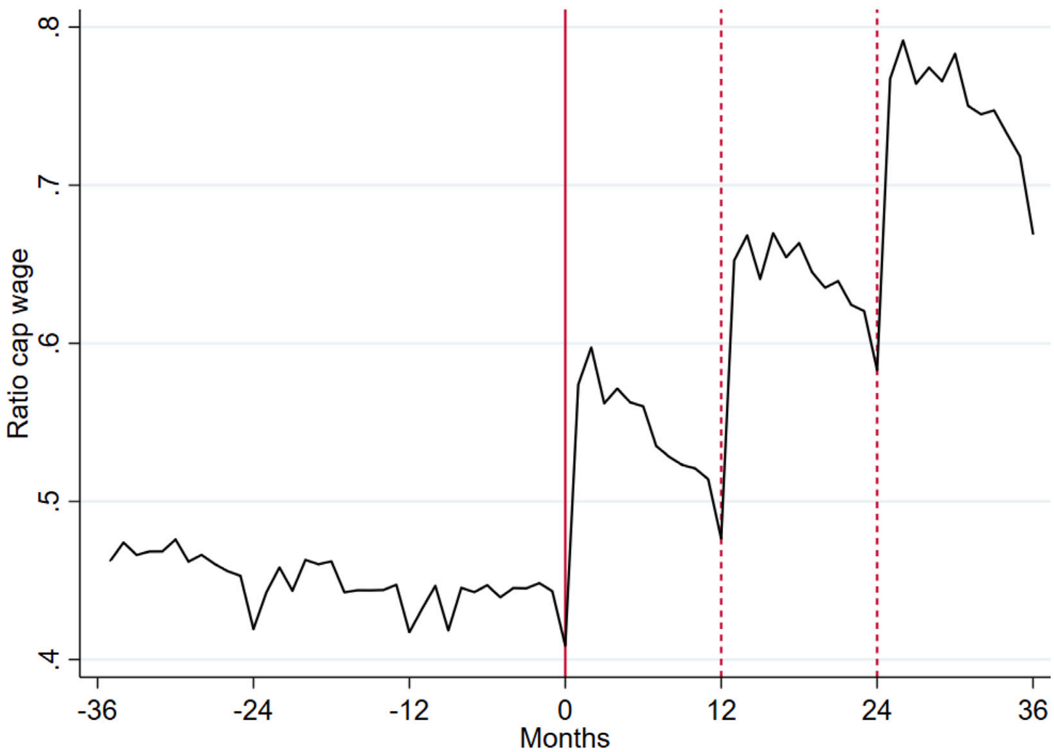

Fig. 2. Ratio between paid sick leave benefit cap and average wage.
Notes: The graph shows the evolution of the sick pay cap relative to average wages. $t = 0$ corresponds to January 2011, when the reform came into effect.

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 related spells (fewer than 4 days) and diagnosis classified according to the International Classification of Diseases (ICD) is only available since 2010 when health providers started to inform the social security agency about all sickness-related spells regardless of their length. For this reason, our investigation mainly focuses on spells of more than 3 days.[15]

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. Third, as explained in Section 2.1, we exclude individuals who receive complementary sick pay benefits on top of statutory ones via special regimes (*"cajas de auxilio"*). These cases comprise only 1%–2% of individuals in our sample.[16] 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 A1 and Table A2. 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 3532 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

[15] Before 2010, in order to get access to paid sick leave, workers in Montevideo (where half of the Uruguayan population lives) had to go in person to the social security agency and present the certificate signed by a physician. Obviously, workers had no incentives to report absences shorter than four days for which they do not get paid. That explains the lack of records for sick leaves shorter than four days prior 2010. To check whether the exclusion of very short spells affects the results, we report additional DiD estimates considering all spells during the period 2010–2013. Our main findings are robust to this restriction (see Appendix Table A6).

[16] Our main results are not driven by this restriction (see Appendix A7).

Table 1
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
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: All specifications include individual fixed effects. 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.

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.

Figure A1 in Appendix compares the distribution of log monthly wages between individuals employed in worker cooperatives and conventional firms. On average, individuals employed in Uruguayan worker cooperatives earn higher wages compared to those in conventional firms. However, this difference is largely attributable to differences in observable characteristics. Previous research indicates that the residual earning premium, after accounting for personal attributes, is modest—approximately 2% (Burdin, 2016; Dean, 2024). It is important to note, as detailed below, that our study focuses on comparisons among individuals within a limited earnings range, leveraging the variation introduced by the sick pay reform.

3.2. Motivating evidence

Before presenting our main identification strategy and results, we provide two key pieces of motivating evidence. First, we offer descriptive evidence comparing absenteeism rates between workers in worker cooperatives and those in conventional firms. Second, we present survey data on managerial perceptions of work ethics, absenteeism behaviour, and worker supervision mechanisms in both types of firms, collected around the time of the reform.

Absenteeism Gap between Worker Cooperatives and Conventional Firms. Before presenting our main identification strategy and results, we provide descriptive evidence comparing the levels of absenteeism between workers employed in worker cooperatives and those in conventional firms. In this case, we leverage variation provided by workers who switch between organisational forms during the period, under the assumption that sorting is driven by time-invariant characteristics. We count 1746 switchers, which represents approximately 3% of the sample (454 workers moved from worker cooperatives to conventional firms and 1292 made the reverse switch). The sample is restricted in the way explained above, except for the fact that we include all individuals regardless of their pre-reform wage. Table 1 reports the corresponding estimates from fixed-effects regressions. We successively add controls for personal and 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.

Managers' Perceptions of Work Ethics. To further motivate the empirical analysis and the investigation of the underlying mechanisms, we collected firm-level survey data on managers' perceptions of absenteeism and work ethics, gathered before (2009) and after (2012) the reform. The survey includes responses from a sample of approximately 400 Uruguayan firms per wave, encompassing both worker cooperatives and conventional firms. By design, the comparison group of conventional firms matches the sectoral and size distribution of cooperatives. For consistency, we restrict the analysis to firms that participated in both survey waves.

We first characterise the labour discipline environment in worker cooperatives, presenting descriptive evidence on supervision intensity and monitoring mechanisms. Figure A6 (Panel A) displays supervision intensity by firm size and organisational form, measured as the ratio of supervisors to total employment.[17] In small firms, supervision intensity is lower in worker cooperatives than in conventional firms. However, in large firms, supervision ratios are roughly comparable across organisational forms. Managers also reported the primary mechanisms used to monitor and enforce work effort. Figures A6 (Panels B and C) reveal that hierarchical monitoring by specialised supervisors (e.g., ''Verbal warnings from supervisors'') is more prevalent in conventional firms, whereas mutual monitoring among coworkers (e.g., ''Verbal warnings from coworkers'') is more common in cooperatives, regardless of firm size. Interestingly, although supervision intensity is roughly similar across medium-to-large firms regardless of organisational form, supervisors in cooperatives are perceived as less relevant in enforcing labour discipline compared to their counterparts in conventional firms.[18]

Then, we report direct evidence on managers' perceptions on work ethics and absenteeism. In Appendix Figure A5 (Panel A), we present managers' responses to the question: ''Could you rank the most pressing human resource management problems faced by your company during the last year?'' The data indicates that absenteeism was perceived as the primary HRM challenge among medium-to-large cooperatives. Notably, concerns about absenteeism among managers of large cooperatives intensified significantly between survey waves, aligning with the timing of the reform. In Figure A5 (Panel B), we show responses to the question: ''What is your perception of the work attitudes that predominate among individuals employed in your company?'' (available only in the post-reform wave). Managers of medium-to-large cooperatives reported a higher prevalence of low or very low work ethics compared to smaller cooperatives, where poor work ethics does not appear to be an important issue.

[17] The survey asked managers to report the number of employees performing supervisory roles. Following Wright (1995) and Jayadev and Bowles (2006), supervisors are defined as workers with at least one subordinate and authority over tasks, tools or procedures, work pace, and the ability to impose or recommend sanctions, including pay adjustments, promotions, or terminations.

[18] Conceptually, monitoring on-the-job effort should be distinguished from keeping track of and responding to absenteeism. Perhaps supervisors are tasked with watching and giving warnings about effort on the job, but there are personnel managers, rather than the supervisors, who are tasked with enforcing policies about absenteeism.

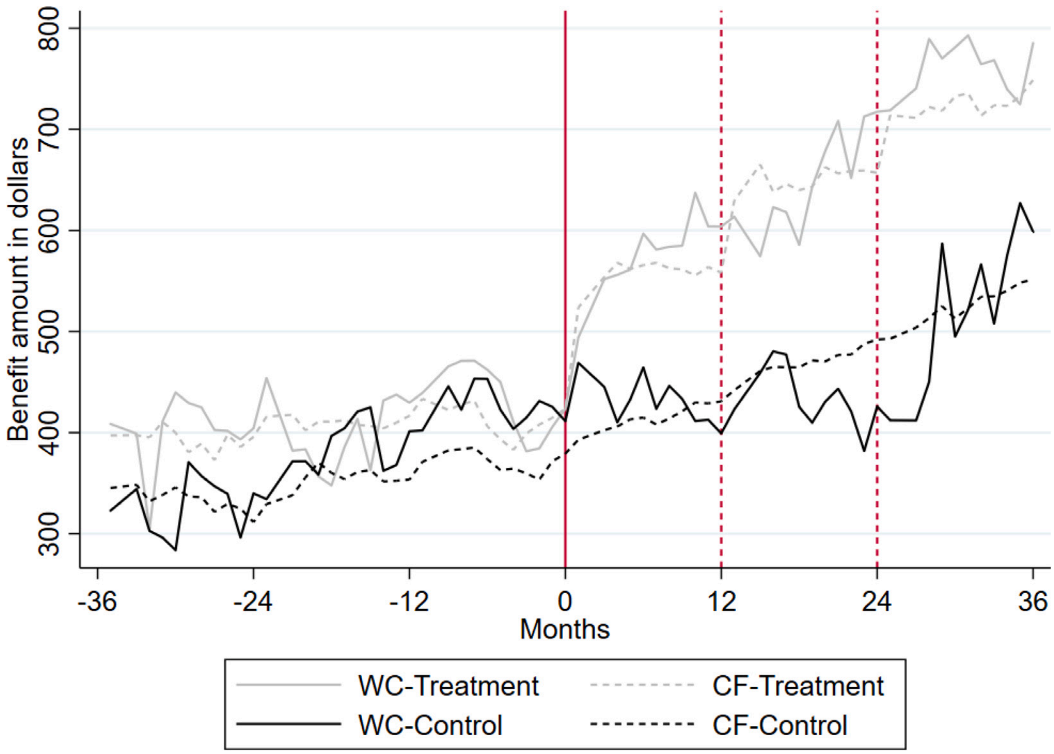

Fig. 3. Evolution of average paid sick leave by treatment status and organisational form.
Notes: The graph displays the evolution of average sick pay for treatment and control groups in conventional firms (CF) and worker cooperatives (WC) before and after the reform. $t = 0$ corresponds to January 2011, when the reform came into effect.

3.3. 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' average monthly earnings in 2010, the year immediately before the reform came into force (January 2011).

We estimate the following difference-in-differences specification:

$$y_{it} = \alpha + \beta T_t + \gamma D_i + \delta Coop_{it} + \eta T_t \times Coop_{it} + \zeta D_i \times Coop_{it} + \theta D_i \times T_t + \phi D_i \times T_t \times Coop_{it} + \psi X_{it} + \tau_s + \omega_r + \epsilon_{it} \quad (2)$$

where y_{it} either is an indicator for whether individual i experienced a sickness absence spell (lasting at least four days) in month t[19] or measures the number of days of sickness absence individual i took in month t, T_t is a post-reform dummy that equals one in and after January 2011 and zero otherwise, D_i is the above-defined treatment group dummy, and $Coop_{it}$ is a dummy variable indicating that individual i is employed in worker cooperative in month t.[20] 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 Eq. (2) by OLS, clustering standard errors at the individual level in order to account for serial correlation.[21]

Fig. 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 treated workers starting in January 2011, suggesting that the reform hit the treatment group in the expected way. In Fig. 4, we plot the fraction of workers in the treatment

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

[20] Initially, we pool all workers employed in worker cooperatives, including both members and employees. In Section 5, we report separate estimates for these two groups.

[21] Concerns about the effect of serial correlation and unobserved group shocks on the reliability of standard errors in a DiD setting have led researchers to implement different strategies (Bertrand et al., 2004; Donald and Lang, 2007; Conley and Taber, 2011). In Appendix Table A3, we check the robustness of our baseline estimates to alternative procedures, such as clustering standard errors at the industry × region level (178 clusters), computing wild bootstrap standard errors (Cameron et al., 2008; MacKinnon and Webb, 2018; Roodman et al., 2019) and implementing (Donald and Lang, 2007) two-step correction procedure. Results are qualitatively similar to our main estimates.

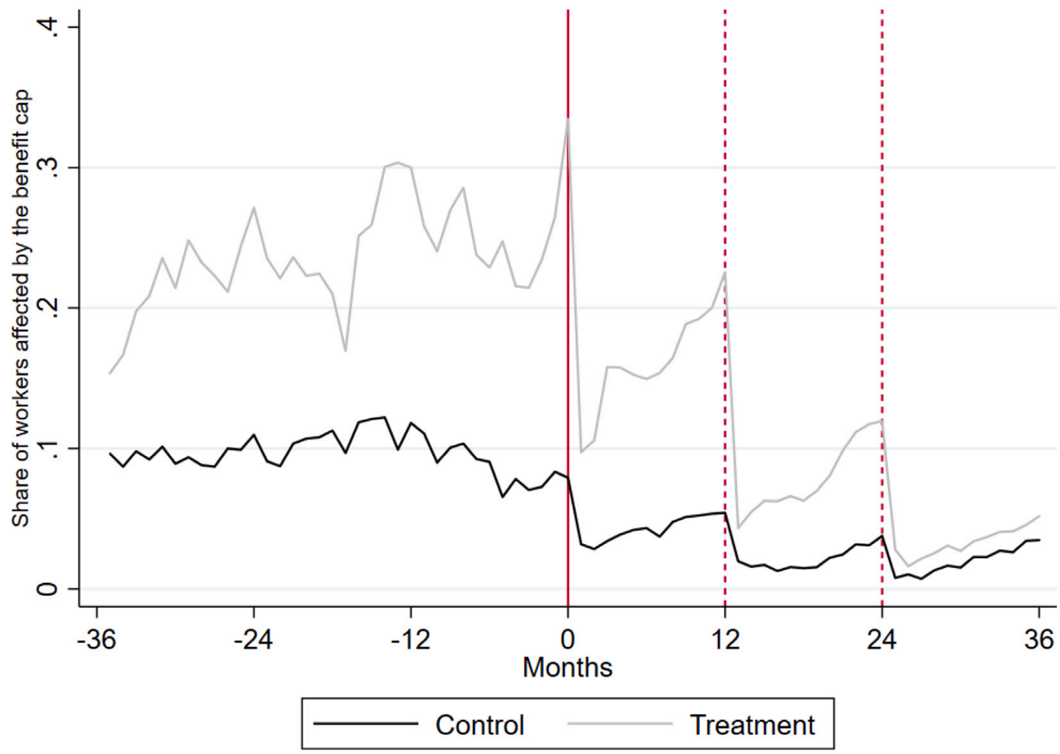

Fig. 4. Ratio of workers affected by the benefit cap.
Notes: The graph displays the share of individuals affected by the sick pay cap in treatment and control groups before and after the reform. $t = 0$ corresponds to January 2011, when the reform came into effect.

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.[22]

Finally, in Figs. 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.

4. Main results

4.1. Difference-in-differences estimates

Table 2 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 1-4 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.[23]

4.2. Robustness checks and additional results

Matching. In Columns (4) and (8) of Table 2 we report additional

[22] In practice, the benefit cap (bmax) is applied on a monthly basis. There is a maximum amount of sick pay per month, and the scheme pays 0.7w, where w is the monthly wage, for up to a maximum number of sick days, and would then pay 0 for any additional days after bmax has been reached. Hence, the cap may not be binding for treated workers in certain instances. Nevertheless, for our intention-to-treat approach, the key consideration is that the cap was more likely to be binding for treated workers relative to controls prior to the reform, with this gap closing after the reform.

[23] In these baseline estimates, we cluster standard errors at the individual level. In Appendix Table A3, we report additional estimates clustering standard errors at the industry × region level (178 clusters) and computing wild bootstrap standard errors. Moreover, in Appendix Table A6 we show additional DID estimates for the period January 2010–December 2013 including very short spells of less than 4 days. In both cases, results are qualitatively similar to our baseline estimates.

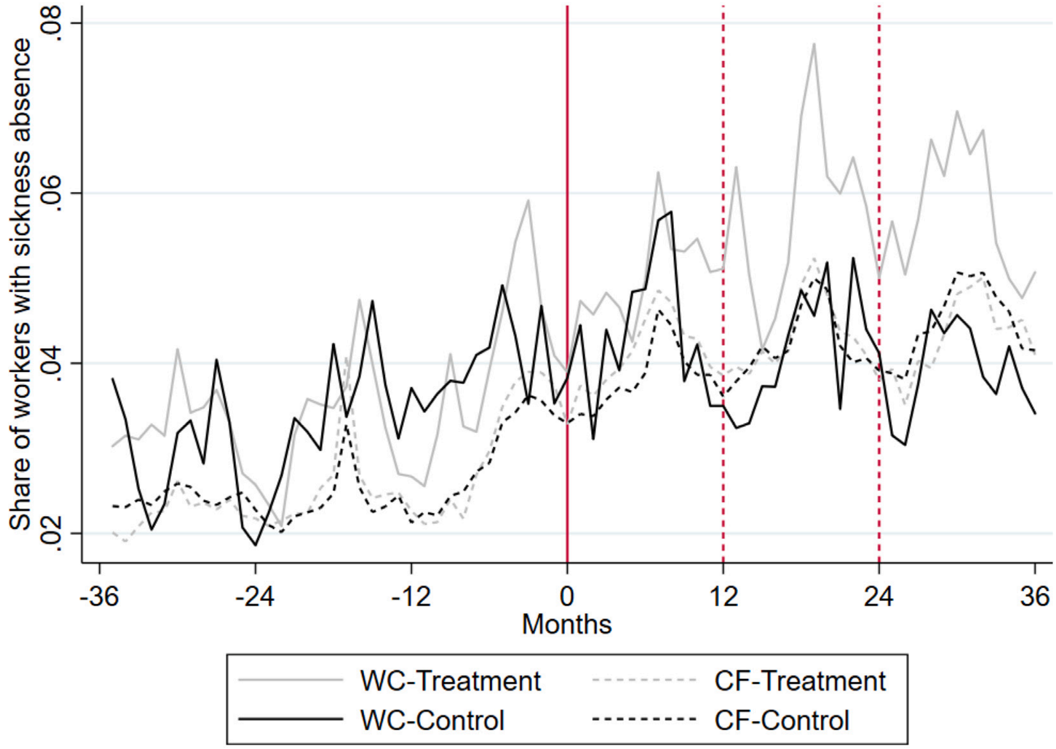

Fig. 5. Share of workers with sickness absence in each month.
Notes: The graph displays the share of individuals with a sickness-related absence (lasting at least four days) in each month. The figure distinguishes treatment and controls in conventional firms (CF) and worker cooperatives (WC) before and after the reform. $t = 0$ corresponds to January 2011, when the reform came into effect.

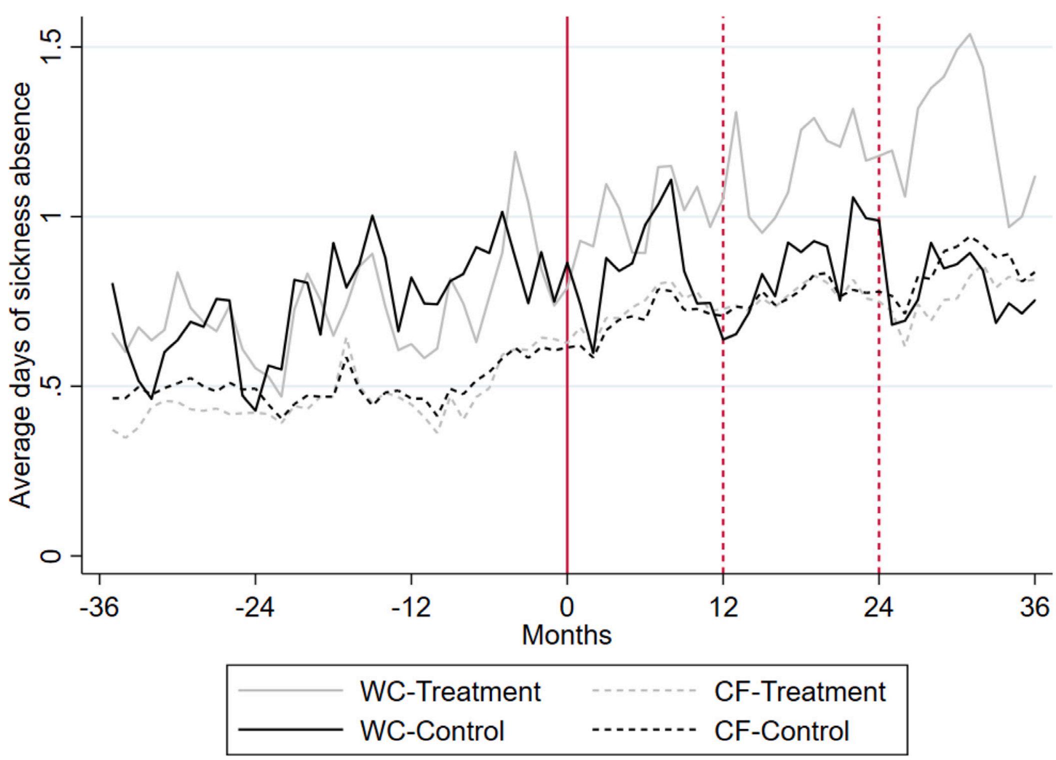

Fig. 6. Average duration of sickness absence spells (in days).
Notes: The graph displays the average duration (in days) of sickness-related absence spells in each month. The figure distinguishes treatment and controls in conventional firms (CF) and worker cooperatives (WC) before and after the reform. $t = 0$ corresponds to January 2011, when the reform came into effect.

Table 2
Difference-in-differences estimates.

	Incidence of sickness-related absence				Duration (days)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Reform × Treatment	0.002*	0.002*	0.001	−0.004	0.036	0.035	0.023	−0.085
	(0.001)	(0.001)	(0.001)	(0.003)	(0.027)	(0.028)	(0.032)	(0.071)
Post-Reform × Treatment × Coop	0.011*	0.014**	0.016**	0.021*	0.292*	0.357**	0.415**	0.445*
	(0.006)	(0.006)	(0.008)	(0.009)	(0.151)	(0.154)	(0.208)	(0.242)
Observations	2,395,433	2,395,433	1,719,958	1,505,081	2,395,433	2,395,433	1,719,958	1,505,081
Individual's controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific time trends	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Region-specific time trends	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Only full-time workers aged 18–59 years	No	No	Yes	Yes	No	No	Yes	Yes
Matching	No	No	No	Yes	No	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"). Mean incidence (duration) of sickness-related absences for treated individuals employed in worker cooperatives pre- reform: 0.04 (0.73). "Matching" refers to a re-weighted DiD estimation of a coarsened exact-matched sample of individuals employed in worker cooperatives and conventional firms. Standard errors clustered at the individual level are reported in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

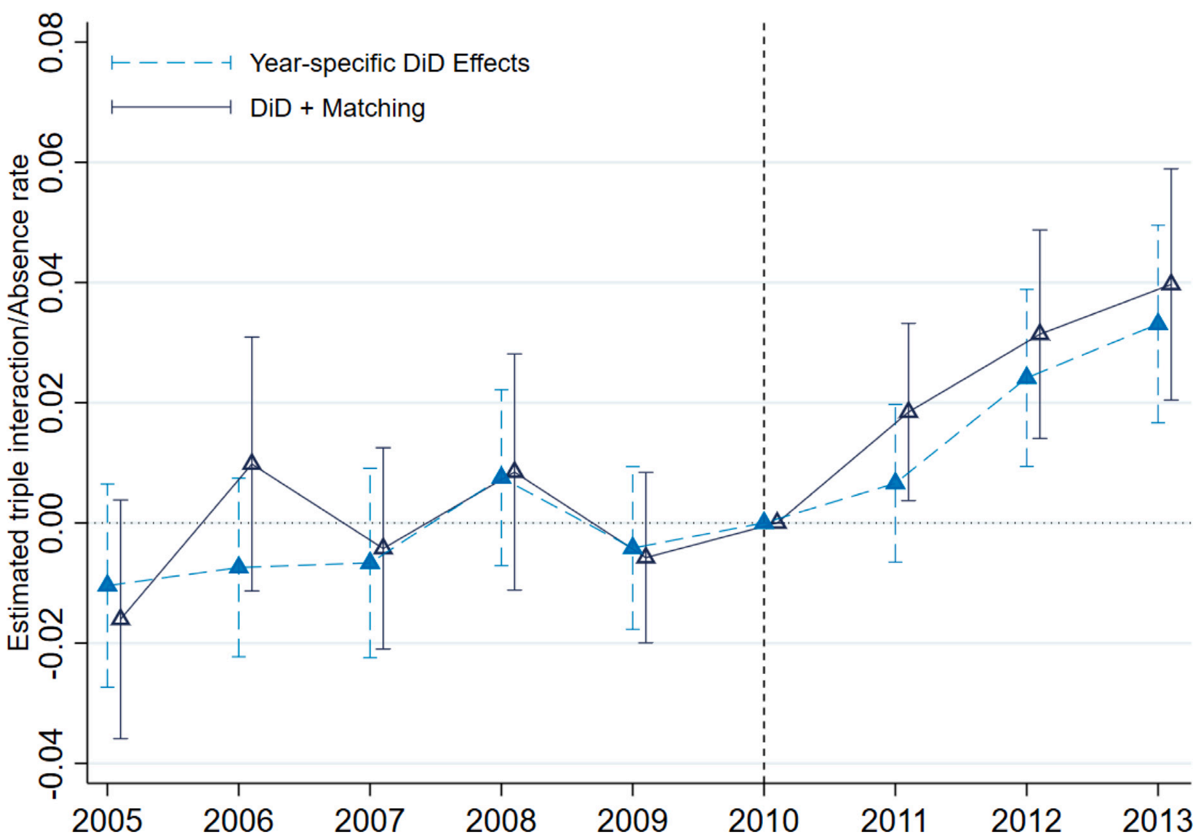

Fig. 7. Event-study analysis: incidence of sickness-related absence.
Notes: The figure shows event studies based on a DiD model as in Eq. (2). Dependent variable: indicator for whether individual i experienced a sickness absence spell (lasting at least four days) in month t. The graph displays the estimated 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). "Matching" refers to a re-weighted DiD estimation of a coarsened exact-matched sample of individuals employed in worker cooperatives and conventional firms. The standard errors are clustered at the individual level and the dash bars depict 90% confidence intervals.

estimates combining our basic DiD specification with a non-parametric coarsened exact matching. This procedure is aimed at improving the comparability between individuals employed in cooperatives and conventional firms in terms of observable characteristics (Iacus et al., 2012). Specifically, we first match individuals using pre-reform year characteristics (2010) and determine the matching weights, which are then used to estimate the DiD model. The pre-reform characteristics used for matching are: treatment status, age, gender, firm size, sector, employer's location (Montevideo). Results are similar to our baseline estimates.

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. Figs. 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 normalised to zero. We do not find evidence of differential trends in workplace absences before

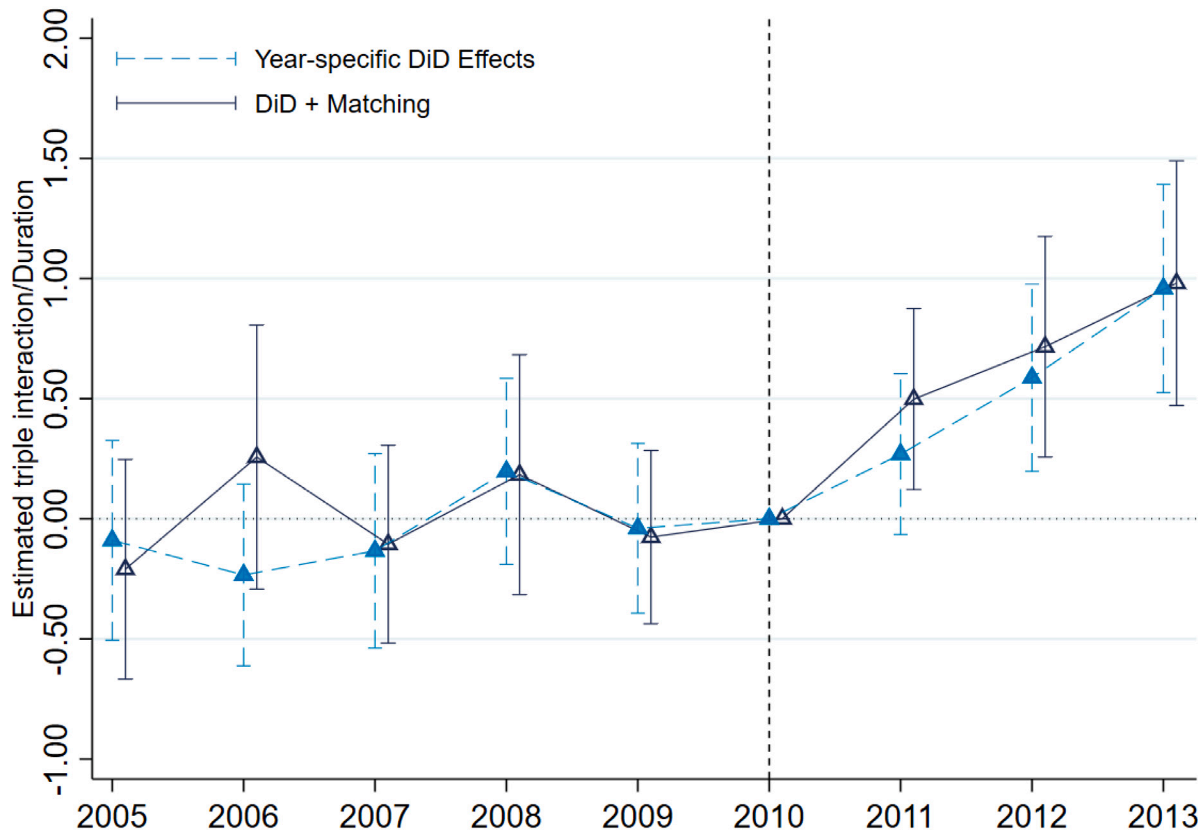

Fig. 8. Event-study analysis: duration of sickness-related absence.
Notes: The figure shows event studies based on a DiD model as in Eq. (2). Dependent variable: number of days of sickness absence individual i took in month t. The graph displays the estimated ϕ coefficient associated with the triple 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). "Matching" refers to a re-weighted DiD estimation of a coarsened exact-matched sample of individuals employed in worker cooperatives and conventional firms. The standard errors are clustered at the individual level and the dash bars depict 90% confidence intervals.

2011. The differential increase in sickness-related absences for treated workers employed in worker cooperatives becomes significant in 2012 and 2013. Importantly, this holds for both unmatched and matched DiD estimates (Iacus et al., 2012).

Switchers. An important concern is that the announcement of the reform may have induced sorting of individuals into cooperatives. These individuals may anticipate the possibility of taking more advantage of the new sick pay regime if they are employed in a worker cooperative. Moreover, they may have unobserved attributes 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 Appendix Table A4. Treated workers in cooperatives increased their likelihood of being absent from work in a given month by 1.3 percentage points in comparison to treated individuals employed in conventional firms. The effect is significant at the 10% level. Duration increased by 0.316 days relative to the other groups, albeit the effect is imprecisely estimated (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.[24]

Compositional Changes. We perform additional DiD estimates using the balanced panel in order to control for workforce compositional changes. Estimates reported in Appendix Columns (1)–(2) of Table A5 restrict the sample to individuals observed for 36 consecutive months before and after the reform. Results are similar to baseline estimates. We find a 1.3 percentage point increase in absenteeism among treated workers in cooperatives relative to other groups, although the effect is imprecisely estimated (SE 0.008). According to results reported in column (2), duration rose by 0.5 days in a given month.[25]

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 (3)–(4) of Table A5 indicate that the incidence and duration of sickness absence increased differentially among treated workers employed in cooperatives in relation to the other group. Effect sizes are comparable to our baseline estimates.[26]

Continuous Treatment. Our binary treatment indicator masks the fact that the increase in the generosity of paid sick leave after January

[24] In Appendix Table A8, we conduct a probit regression in which we assess whether individuals who experienced a more frequent sick leave use in 2005–2007 were more likely to enter into a worker cooperative in the period 2008–2013, controlling for other personal characteristics (age, gender, initial tenure, firm size, industry and region). The regressions provide no support for the idea that absence-prone individuals self-selected into worker cooperatives.

[25] Results for the balanced panel are qualitatively similar if we exclude switchers. 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 Columns 3–4 of Table A4).

[26] 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.

2011 did not affect individuals in the treatment group uniformly. As shown in Fig. 1, the pre-reform benefit cap (3 BPC) gradually increased by 1 BPC per year from January 2011, reaching 6 BPC by January 2013. Given the sick pay formula described by Eq. (1), workers earning less than (3/0.7) BPC just before January 2011 were not intended to be affected by the reform (never treated control group). Instead, all individuals for whom $w > (3/0.7)$ 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 (3/0.7)–(4/0.7) BPC *only* benefited from the initial sick pay cap rise in January 2011; (2) individuals earning (4/0.7)–(5/0.7) BPC *also* benefited from the second cap rise in January 2012; (3) finally, individuals earning (5/0.7)–(6/0.7) 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 (5)–(6) of Table A5. Consistent with our previous results using a discrete treatment indicator, the behavioural response to treatment intensity for workers employed in cooperatives is significantly stronger relative to other groups.

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 (3/0.7)–(4/0.7) BPCs just before January 2011, who only benefited from the first sick pay cap hike. Results reported in Appendix Table A4 are qualitatively similar to our baseline estimates.

5. Mechanisms

Small vs. Large Firms. It has been argued, albeit controversially, 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*).[27] By contrast, in small teams, the dilution of incentives may be less severe and shirking could be mitigated through mutual monitoring among members without relying on specialised supervisors. To check for this mechanism, in Table 7 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. This appears to be broadly consistent with survey evidence on managerial perceptions of work ethics in large cooperatives reported in Section 3.2.[28]

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.[29]

In columns (3) and (5) of Table 3, 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. The insignificant estimated coefficients on the triple difference term in the estimates the includes hired coop workers (columns (4) and (6)) stand in sharp qualitative contrast with the positive, highly significant coefficients for the corresponding estimates including only cooperative members (columns (3) and (5)). This indicates that 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.[30]

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 3, we display estimates of Eq. (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. Similarly, long-term absences increased by 1 percentage point. 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.

Extensive Margin Responses by Disease-Categories. In this section, we further investigate extensive margin responses of sickness

[27] For critiques and experimental evidence against this hypothesis, see for instance (Putterman, 2006; Jossa, 2009; Grosse et al., 2011; Dow, 2018). 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 et al., 2018).

[28] In Appendix A.2, 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.

[29] 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 et al., 2019, p. 592).

[30] We also analyse tenure-based heterogeneous effects using two measures. First, we classify members as high- or low-tenure based on whether their seniority exceeds the firm's median. Second, we identify founders by matching firm creation dates with employment start dates. Tenure's effect is theoretically mixed—longer tenure may increase firm-specific skills, raising moral hazard risks, but may also foster commitment and reciprocity, reducing such risks, especially among founders. Additional DiD estimates reveal that differential increases in sickness-related absences among treated cooperative members holds regardless of tenure or founder status.

Table 3
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
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. 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 4
Difference-in-differences estimates: incidence of sickness absence by disease categories.

	(1)	(2)	(3)	(4)	(5)	(6)
	Musculoskeletal	Infectious	Respiratory	Mental	Poisoning	Pregnancy complications
2013 × 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)
2013 × 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
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 5
Relapses, incidence.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	General	Musculoskeletal	Infectious	Respiratory	Mental	Poisoning	Pregnancy complications
2013 × Treatment	0.00052**	0.00032*	0.00004	0.00011	0.00000	−0.00002	0.00008
	(0.00025)	(0.00019)	(0.00003)	(0.00011)	(0.00008)	(0.00004)	(0.00008)
2013 × Treatment × Coop	0.00141*	0.00080	0.00015	−0.00003	0.00052	−0.00008	0.00005
	(0.00084)	(0.00061)	(0.00016)	(0.00038)	(0.00038)	(0.00009)	(0.00015)
Observations	879,880	879,880	879,880	879,880	879,880	879,880	879,880
Individual's controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-specific time trends	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. As in Table 4, treatment and control groups are redefined using the increase in the benefit cap that came into force in January 2013. The post-reform variable equals 1 for 2013 (policy-on period) and 0 for 2011–2012 (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"). The dependent variable is a dummy that takes the value 1 if the individual has 2 or more spells of the same disease category in the last 6 months. Standard errors clustered at the individual level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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,[31] 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).

Information on disease categories for each sickness spell is only available from 2010 onward. Hence, we redefine our treatment and control groups and compare 2011–2012 versus 2013, exploiting the

[31] Respiratory diseases are part of a mixed category including both contagious and noncontagious diseases.

Table 6
Difference-in-differences estimates: day of first report.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	Monday	Tuesday	Wednesday	Thursday	Friday	Mon-Fri	Tue-Wed-Thu
Post-Reform × 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 × Treatment × 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
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 7
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
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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

increase in the benefit cap that came into force in January 2013. Table 4 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.

Health Relapses. We further exploit information on absence spells by disease type by analysing the probability of relapses. Following Marie and Vall-Castello (2022), we define a health relapse as a dummy variable that takes the value one if the individual has two or more spells due to the same diagnosed illness in the last six months. Our analysis of relapses follows the same structure as our DiD estimates by disease type, using available information from 2010 onward. Table 5 displays additional DiD estimates with the relapse binary indicator as the dependent variable. The first column in Table 5 reports a significant increase in the probability of relapse. We also observe a differential increase in the probability of relapse for treated individuals in worker cooperatives relative to the other group. In columns (2)–(7) of Table 5, we report the analysis of relapses by disease type. The differential increase in relapses appears to be driven by relapses from musculoskeletal illnesses, albeit effects are imprecisely estimated. As mentioned, this category includes conditions that are more likely to be associated with labour supply adjustments driven by moral hazard.

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.[32]

We investigate the existence of a "Monday effect," bearing in mind that data on sick leave spells lasting fewer than four days is unavailable prior to 2010. Additionally, as discussed in Section 2, the Uruguayan sick leave system includes a three-day nonpayable period. To maintain consistency with the rest of the analysis, we run separate estimates by day of first report for sick leave spells of at least four days.[33]

With this caveat in mind, Figure A2 presents the distribution of sickness spells by the day of first report.[34] 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 6, we report additional DiD estimates of the incidence of sickness spells by the day of first report. As there are individuals with multiple absence spells in a given month, these estimates consider the day of first report of each absence spell in a given month. We find no evidence of a differential increase in extended weekend absences (Monday/Friday) for treated individuals employed in cooperatives.

Labour Discipline. Finally, we investigate whether documented differences in absence behaviour between individuals employed in cooperatives and conventional firms could be explained by the use of

[32] 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).

[33] It is worth noting that this approach may result in a mismatch between our measure and the conventional "Monday effect", which typically focuses on single-day absences.

[34] In Appendix Figures A3 and A4, we provide the distribution of sickness spells by disease category and day of first report.

Table 8
Difference-in-differences estimates: probability of being dismissed.

	(1)	(2)	(3)	(4)
			Small Firms	Large Firms
Post-Reform × Treatment	−0.005***	−0.006***	−0.002	−0.014***
	(0.001)	(0.001)	(0.001)	(0.004)
Post-Reform × Treatment × Conventional	0.007***	0.008***	0.002	0.020***
	(0.001)	(0.001)	(0.001)	(0.004)
Observations	2,362,933	2,362,933	1,169,451	616,779
Individual's controls	Yes	Yes	Yes	Yes
Industry-specific time trends	No	Yes	No	No
Region-specific time trends	No	Yes	No	No

Notes: DiD estimates comparing treatment and control individuals. Dependent variable: indicator for whether individual i experienced an involuntary job termination in month t. Conventional equals 1 for individuals employed in a conventional firm 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.

more punitive labour discipline strategies in conventional firms (Bowles and Gintis, 1993). It is well established that worker cooperatives maintain more stable employment than conventional firms when faced with negative demand shocks (Craig and Pencavel, 1992; Pencavel et al., 2006; Burdín and Dean, 2009). 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 a worker cooperative.

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 involuntary job separations, excluding other types of separations (quits, retirement, etc.).

Table 8 shows estimates of Eq. (2) in which the dependent variable is a dummy indicating that the individual has experienced an involuntary job separation in the corresponding month. In this specification, we use a dummy $ConventionalFirm_{it}$ indicating whether the individual is employed in a conventional firm. Involuntary job separations seem to affect treated workers in the two types of firms asymmetrically. Our preferred estimates reported in column (2) indicate that the probability of being dismissed is 0.8 percentage points higher among treated workers employed in conventional firms relative to those employed in cooperatives. Considering the average pre-reform dismissal rate (1%), the magnitude of the effect is large. In columns (3)–(4), we show that differences in involuntary job terminations are driven by individuals employed in large firms.

Fig. 9 reports the results from an event-study analysis in which we track differences in dismissal rates before and after the paid sick leave reform. Each estimated coefficient corresponds to the interaction between $T_i \times ConventionalFirm_{it}$ and a full set of year dummies, where the coefficient for 2010 is normalised 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.[35] 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 involuntary job terminations 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.[36]

6. Conclusions

In this paper, we aim to examine individuals' absence behaviour, an extensive-margin proxy for work effort, 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 control 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 underutilise sick leave insurance, leading to potential problems

[35] Estimates presented in Table 8 also reveal a reduction in dismissals among treated individuals employed in worker cooperatives. However, it is important to highlight that the observed differences in dismissals following the reform cannot be fully attributed to this reduction and partially reflect the stricter labour discipline enforced in conventional firms.

[36] If firms could fully enforce the threat of dismissal, workers would avoid taking sick days, and actual dismissals would rarely occur. However, dismissals serve to reinforce an imperfect threat—occasionally being carried out to maintain credibility. From a measurement perspective, a high threat level would result in few observed dismissals, as the mere risk of job loss (due to low alternative wages, prolonged job searches, or minimal unemployment benefits) would be enough to deter absenteeism. We thank an anonymous referee for making this point.

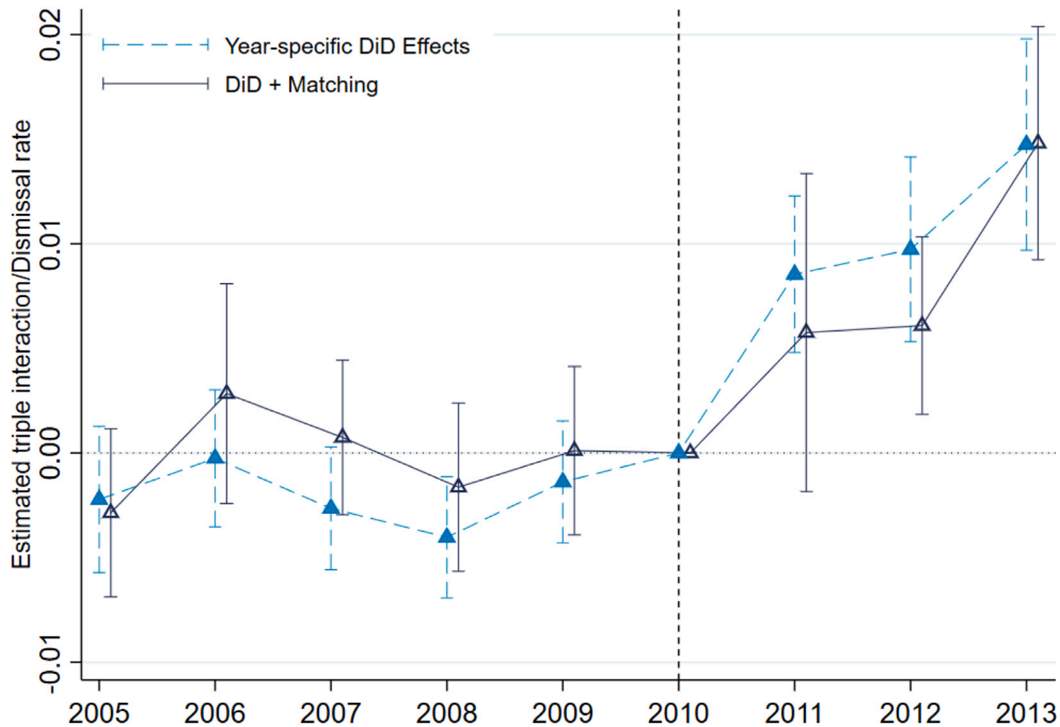

Fig. 9. Event-study analysis: dismissals.
Notes: The figure shows event studies based on a DiD model as specified in Eq. (2), but replaces the dummy variable $Coop_{it}$ with $ConventionalFirm_{it}$, which equals 1 for individuals employed in conventional firms. Dependent variable: indicator for whether individual i experienced an involuntary job termination 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). "Matching" refers to a re-weighted DiD estimation of a coarsened exact-matched sample of individuals employed in worker cooperatives and conventional firms. The standard errors are clustered at the individual level and the dash bars depict 90% confidence intervals. $t = 0$ corresponds to January 2011, when the reform came into effect.

of contagious presenteeism, reduced productivity, and additional costs to public health services. Indeed, we cannot rule out the theoretical possibility that higher levels of sick leave are efficient. In other words, given the very low pre-reform benefit cap, sickness-related absence levels may have been in the presenteeism range. If this is true, caution is needed in interpreting an expansion of sick leave as a reduction in work effort. Further research could analyse compensatory behaviour among peers (e.g., quits) or leadership changes in response to absenteeism within cooperatives. Moreover, it would be important to investigate 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.

CRediT authorship contribution statement

Pablo Blanchard: Methodology, Formal analysis, Data curation, Conceptualization. **Gabriel Burdin:** Writing – original draft, Supervision, Methodology, Formal analysis, Data curation, Conceptualization. **Andrés Dean:** Supervision, Project administration, Methodology, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jdeveco.2025.103533.

Data availability

The data used is confidential.

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