Monitoring, monetary incentives and workers' rents in determining absenteeism

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Abstract

Exploiting three variations in sickness absence policy for civil servants in Italy, this paper assesses the effectiveness of monitoring and monetary incentives in addressing absenteeism, both on average and for different levels of workers' rents. Sickness absence is sensitive to monitoring intervals for random inspections, while moderate monetary incentives are relatively less effective. Results are not driven by attenuation bias, and are confirmed by a falsification test. Monitoring is particularly effective for low-skilled workers that, thanks to public sector wage compression, enjoy higher economic rents from working for the government. Given that sickness absence rates are higher in the public than in the private sector in the US and Western Europe as well, these results provide useful insights on how to draw a successful strategy for addressing absenteeism.

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1 Introduction

Exploiting three variations in sickness absence policy for civil servants in Italy, this article assesses the importance of monetary incentives and monitoring in addressing absenteeism, both on average and for different levels of workers' rents.

Economic theory postulates that, for given outside options, there is a trade off between monetary incentives and stricter monitoring in determining workers' effort levels (Shapiro and Stiglitz, 1984). Given asymmetric information on actual health conditions, workers might try to reduce the amount of work supplied by deciding to report sick even when their physical conditions are compatible with work,[1] a specific dimension of shirking. The incidence of such an opportunistic behaviour depends on the worker's surplus at the current job (Barmby et al., 1994), on her outside options (Askilden et al., 2005; Kaivanto, 1997), on sick leave replacement rates (Henrekson and Persson, 2004; Johansson and Palme, 2005; Puhani and Sonderhof, 2010; Ziebarth, 2009; Ziebarth and Karlsson, 2010) and on the likelihood associated with the worker being fired when cheating. This last element is determined by the degree of Employment Protection Legislation (EPL) enjoyed (Arai and Thoursie, 2005; Ichino and Riphahn, 2005; Johansson and Palme, 2005; Lindbeck et al., 2006; Riphahn, 2004; Scoppa, 2010) and by monitoring effectiveness (Banerjee et al., 2007; Duflo et al., 2010). In this framework, higher absence rates are expected for civil servants, given that they are less exposed to market forces and enjoy a higher level of effective EPL compared to their private sector peers.

In the United States, 2.8 per cent of public sector workers reported to have worked less than usual because of illness in the fourth quarter of 2007, 41.2 per cent higher an incidence than in the private services sector. In Western Europe, this difference was equal to 20.2 per cent.[2] Italy is no exception, and in the same period sickness absence incidence was 49.1 per cent higher in the public than in the private services sector.[3] In

[1] For a review of the literature on the determinants of absenteeism see Brown and Sessions (1996), while Banerjee and Duflo (2006) summarise the results of different attempts to curb absenteeism in the public sector in India and Kenya. Markussen et al. (2010) provide extensive evidence on the relevance of moral hazard issues in determining sickness absence levels.

[2] Author's calculations based on Current Population Survey data for the US (US-Census-Bureau, 2008) and on EULFS data for Western Europe (Eurostat, 2008). Absence rates are equal to the incidence of employees working less than usual in the reference week because of illness. Workers not working in the reference week for reasons outside their will (labor dispute, bad weather, technical reasons, reduced activity) are not included.

[3] Author's calculations on Italian Labour Force Data, following the same definition of footnote 2; for details see section 3.

order to reduce this wedge, the just installed Italian government introduced a new, more restrictive, sickness absence policy for civil servants at the end of June 2008. The new provision stayed in place for a full year and introduced monetary disincentives with the loss of any allowance or bonus (20% of total wage for the average worker) for the first ten days of sickness absence. At the same time, the law increased monitoring effectiveness, changing from 4 to 11 hours the time interval in which physicians' random inspections are carried out in order to check whether the worker reporting to be sick is at home and to ascertain her real health conditions. Both in the private and the services sector, a worker caught cheating is liable to disciplinary action leading to job loss. Penalties did not change with the reform. After exactly one year the provision was partially amended for six months, with monitoring time intervals reduced to the pre-reform period, while sickness absence wage cuts were left unchanged. Finally, in a third phase, inspections' time intervals were increased again from 4 to 7 hours.

Using Italian Labour Force Survey data, a large dataset with more than 150 thousand quarterly observations, the causal effect of the new policies on public sector workers' absenteeism is identified by means of a regression differences in differences approach using white collar private sector workers as the control group. When stricter monitoring was introduced together with monetary disincentives, sickness-related absence rates in the public sector fell by 0.64 percentage points (-26%) on average, eliminating the wedge with the private services sector conditional on observables. The subsequent change in the policy mix sheds light on the effectiveness of monitoring in determining workers' presence. When time intervals for monitoring were reset to the pre-reform level, sickness absence rates rebounded, meaning that stricter monitoring is the driving force in determining workers' attendance. This result is not driven by the attenuation of reform effects over time: when time intervals for inspections were increased again to 7 hours, a new significant drop in absence rates took place. Evidence survives a number of robustness checks, while no shift is detected to other types of absence as a consequence of the reform. Moreover, a falsification test shows that, out of the 13 semesters observed in this study, the only significant changes in the public/private sector relative absence rates were observed in the three semesters in which monetary incentives were coupled with stricter monitoring.

Assuming that genuinely sick individuals are indifferent to monitoring levels, this high sensitivity of absence rates to the length of the time intervals for medical inspections is

a clear sign of opportunistic behaviour. The threat of disciplinary action for cheaters is expected to be more effective in reducing absence among low-skilled workers that, thanks to public sector wage compression, enjoy higher economic rents from working for the government (Gregory and Borland, 1999). Empirical analysis confirms this intuition, showing that increased monitoring completely erased the absence rate differential with comparable private sector workers for civil servants at the lower end of the skill distribution, while for those in managerial positions absence rates remained higher than in the private sector.

Compared to previous empirical literature on incentives' effectiveness, this work has two major advantages. The triple variation in sickness absence policy provides clean evidence on the importance of incentives and monitoring in determining workers' effort, while most previous papers based on an experimental setting focussed on only one of the two possible dimensions. Moreover, such a clear identification is not obtained in a lab experiment, or limited field study, but comes from a real-world employment relationship involving 3.5 million of workers in 2007 (RGS, 2008), slightly more than one out of five employees in Italy in that year.

The results obtained in this work clearly underline the importance of monitoring associated with effective penalties in determining workers' effort, in particular for employees enjoying higher economic rents at the current job. Evidence is more mixed in laboratory studies: Dickinson and Villeval (2008) found a positive impact of monitoring on effort up to a certain threshold, above which motivation can be crowded out (Benabou and Tirole, 2003; Frey and Jegen, 2001). On the other side, evidence in favor of the relevance of effective monitoring is not absent in previous work: analysing absence rates for government nurses in Rajastan, Banerjee et al. (2007) found that monitoring was successful in addressing absenteeism only when it was combined with effective penalties; Duflo et al. (2010) found that monitoring coupled with financial rewards improved teachers' attendance in rural India. Nagin et al. (2002) confirmed the importance of monitoring showing that a relevant fraction of call center operators shirk more when perceived monitoring levels decrease. While a proposal aiming at the introduction of universal sick pay (the *Healthy Families Act*) is before the US congress, this paper underlines the importance of effective monitoring in drawing a successful policy for sick leave abuse prevention. The arousal of opportunistic behaviour is indeed one of the concerns of the critics of the pro-

posed legislation, that explicitly denies to employers the possibility of introducing any absence control policy.[4] Effective monitoring would also have the advantage of targeting cheating workers only, while replacement rates' cuts can reduce sickness absence not only by reducing absenteeism, but also by increasing presenteeism. Of course these advantages have to be weighted against the fact that non-discretionary cuts in replacement rates lower labour costs for given absence rates and do not entail the costs related to the management of ambitious monitoring plans.

This article is organised as follows. Section 2 introduces the institutional setting while sections 3 and 4 respectively describe the data and the identification strategy underlying the estimation of the causal effects of the reforms at study. Main results, together with a number of robustness checks, are reported in section 5, while section 6 analyses the differential impact of the reform across workers enjoying different rents from being in the public sector. Section 7 concludes.

2 Institutional setting

Private sector - control group. During the period analysed here, sickness absence policy remained constant in the private services sector, which will serve as the control group in the empirical analysis. The insurance system is funded by both firms and the Social Security Agency (SSA). For the first three days of continuous absence, sick leave payments have to be made by the employer, and their replacement rate is defined by each contract. Starting with the fourth day and until the twentieth day of absence, SSA pays 50 per cent of the worker's wage, a payment that is usually matched by the employer in order to reach full coverage (but the actual level of coverage can be different according to the contract). For absence spells longer than 20 days, SSA contribution increases to 67 per cent of the wage. Sick workers are required to produce medical certificates justifying their absence and to be at home 4 hours a day (10 to 12 am and 5 to 7 pm) in order to receive random medical checks, aimed at ascertaining their presence at home and their real health conditions (see below for details on the inspections and the related penalties).

Public sector - pre reform. In the public sector, the treatment group, workers were

[4]The bill HR 2460, proposed at the House of Representatives (May 18, 2009) with the aim "to allow Americans to earn paid sick time so that they can address their own health needs and the health needs of their families" restricts "any absence control policy" (Section 7 - Prohibited acts).

entitled to receive the full wage during sick leave of any length before the reform at study was introduced.[5] They were also required, exactly as their private sector peers, to produce medical certificates and to be at home 4 hours a day to receive inspections. This policy had three subsequent changes, that will be used to identify the importance of monitoring and incentives in determining absence levels.

Public sector - Phase 1 of the reform *(July 2008 - June 2009): monetary disincentives and 11 hours monitoring.* At the end of June 2008, the just installed Italian government established a new, more restrictive, sickness absence policy, which stayed in place for a full year.[6] The new provision established that, for the first ten days of continuous absence, the worker on sick leave receives the base salary only. Any allowance or bonus, 20% of total wage on average according to RGS (2008), is thus lost until the 11th day of absence, when the worker reporting sick starts to receive the full wage again.[7] Few exceptions, confined to the most serious cases of illness, were warranted. At the same time, the law increased monitoring effectiveness, changing the time interval in which the worker reporting to be sick had to be at home in order to be able to receive random medical inspections (identical to those set for the private sector) from 4 to 11 hours.

Public sector - Phase 2 of the reform *(July 2009 - January 2010): monetary disincentives only, 4 hours monitoring (pre reform level).* Exactly one year later (Decree No. 78 of July 1st, 2009) the government partially amended the sickness absence policy. While monetary disincentives were not modified, the time intervals for medical inspections returned to the pre-reform setting: 4 hours (10 to 12 am and 5 to 7 pm).

Public sector - Phase 3 of the reform *(February 2010 - June 2010): monetary disincentives, 7 hours monitoring.* After seven months, a new 7 hours time interval for medical inspections was introduced (9am to 1pm and 3pm to 6 pm).

Inspections and penalties. Medical inspections aim at ascertaining sick workers' real health conditions. These inspections can take place at predetermined intervals, during which the worker is supposed to be at home unless she is involved in medical checks elsewhere. There are little official data for the total number of medical inspections carried out. According to treasury data covering 379 thousand civil servants (more than 10% of the total), employed at slightly less than 5 thousand municipalities (RGS, 2011), during

[5]Contractual arrangements could be different in subsectors of the civil service.

[6]Decree No. 112 of June 25th, 2008; converted in Law 133/2008 the 6th of August, 2008.

[7]The incidence of the bonuses on total wages can differ among the sub-sectors or occupations of the civil service, and is defined on a yearly basis.

2009 each employee reported sick at work for 9.1 days on average, while the probability of receiving an inspection for each day lost for sickness was equal to 5.4%. On average, each worker thus had a 49% probability of receiving an inspection in that year. Unfortunately, no data are available on the results of such inspections. Workers are required to interrupt their sick leave when their physical conditions are found to be compatible with work. A worker caught cheating, or found absent, loses the sick-leave payment and is liable to disciplinary action leading to job loss. The total, economy-wide, number of medical inspections carried out is not known to the employees. Given that penalties are high (both in terms of stigma and monetary costs), medical inspections constitute a credible threat, forcing the worker to be at home during the predetermined intervals and thus effectively reducing the amount of outdoor leisure time she can obtain when cheating.

The introduction of the new policy regarding approximately 3.5 million of workers in 2007 (RGS, 2008), or slightly more than one out of five employees, and its partial amendments, provide an ideal setting for evaluating the relative importance of monitoring and monetary disincentives in determining absence behaviour. The next section introduces the dataset employed to evaluate the effects of these policies on civil servants' absence rates.

3 Data and descriptive statistics

The Italian Labour Force Survey (ILFS) is the quarterly dataset used in this study, providing full information on the labour market status and other socio-economic characteristics of a sample representative of the Italian population (for a description, see Ceccarelli et al. (2007)). It is a short panel in which individuals are interviewed in two subsequent quarters and re-interviewed again after one year in the same quarters, for a total of four times. In this article more than 4 million observations are used, spanning the six and a half year interval January 2004-June 2010. These data report respondents' current labour market status and main socio-economic characteristics, constituting the main source for monitoring labor market dynamics in Italy. Two questions are used for constructing the main dependent variable, asking the reason why the respondent did not work at all during the reference week (question B3), or worked less than usual during the reference week (question C34). Sickness is one of the possible answers. The others are:

Subsidised work sharing, Reduced activity for economic or technical reasons, Strike, Bad weather, Annual leave, Bank holidays, Flexible time schedule, Part-time, Study, Compulsory maternity leave, Voluntary parental leave, Leave for family reasons, Reduced activity for other reasons, New job or job change during the week, Work contract just expired. The main binary dependent variable is defined as follows:

- *missing*, thus not used for estimation, if the individual did not work (or worked less than usual) for reasons outside her control (Subsidised work sharing at the firm, Reduced activity for economic or technical reasons, Strike, Bad weather, Bank holidays);
- *zero* if the worker worked as much as usual or if she worked less than usual for reasons other than sickness;
- *one* if the worker worked less than usual (or did not work at all) because of sickness.

A symmetric indicator for other kinds of absence is equal to one if the individual worked less than usual for reasons other than sickness, zero otherwise and missing if the worker worked less than usual for reasons outside her control. Only white collar employees are used for estimation, since there are almost no blue collar workers in the public sector. Furthermore, the final sample does not include workers in the army, workers employed in agriculture and manufacturing and those working in the education or health care sector. This last selection rule is determined by the fact that it is not possible to discern whether the worker is employed or not in the public sector, given the existence of private schools and hospitals.[8] After this sample selection, 295,561 observations are left, a quarter of the total number of employees in the sample.

Figure 1 shows seasonally adjusted sickness absence incidence for the pre reform period (2004:S1-2008:S1), separately for the private services and the public sector. Public sector workers show constantly higher absence rates but similar dynamics when compared to private sector workers. The vertical lines identify the three subsequent changes in the civil servants' sickness absence insurance system introduced in section 2. Graphical evidence clearly shows that the difference in absence rates between the public and the private sector is almost eliminated during *phase 1* of the reform, increases during *phase 2* and decreases again in *phase 3*.

Table 1 reports descriptive statistics for the private services sector *(control group)* and the public sector *(treatment group)*, for the period before (2004:S1-2008:S1) and

[8]The rest of the public sector is identified by individuals working for the Public Administration.

after (2008:S2-2010:S1) the introduction of the new sick pay policies. The distribution of workers across educational levels is similar for the treatment and the control group, with the share of highly educated individuals being around 20% in the private services sector (3 to 4 percentage points higher in the public sector). The share of women is higher in the private services (around 51%) than in the public sector (around 42%). This might seem surprising, but it is widely expected since education and health care are not included in the public sector. Moreover, while public sector workers are evenly distributed across the country, the private sector is concentrated in the North, the area where female employment rates are the highest. Distribution by age is different in the two sectors, with civil servants being over-represented among older (45-64) workers and under-represented among younger ones (15-34).

The incidence of workers reporting to have worked less than usual because of sickness is equal to 2.6% in the public sector in the pre-reform period, 1 percentage point higher than in the private sector. In the post reform period this incidence falls to 1.9%, still 0.6 percentage points higher than in the control group. Overall absence incidence (i.e. including also absence for reasons other than sickness) is similar in the two groups. Simple average comparisons thus highlight a much higher incidence of sickness absence among civil servants, partially offset by a lower incidence of absence for other reasons.

In order to better describe the patterns underlying absence, Table 2 shows the results of a Linear Probability Model (LPM) regression for the probability of the individual worker working less than usual for sickness during the reference week, estimated on the *pre-reform* period. The likelihood of being absent is positively associated with worker's age, tenure and firm size measured as the number of employees at the local unit. Higher probability of reporting sick is found for females and where a Dependent Relative (DR)[9] is present in the household (column 1). Longer contractual hours are associated with less frequent sickness absence, an opposite pattern compared to the rest of the literature on absence, that can be explained by a positive selection of workers into contracts requiring longer hours of work. The higher incidence of sickness absence in the public sector is confirmed when controlling for composition effects, with civil servants having 0.58 percentage points higher probability of being absent from work in the reference week than otherwise observationally equivalent private sector white collar workers.[10] Column

[9] A Dependent Relative is defined as a child below the age of 6 or an elderly above the age of 75.

[10] An epidemiological study using a 2005 cross-section of Italian workers (Costa et al., 2010) shows

2 shows the results of an additional estimate, checking whether the higher propensity to report sick varies across subgroups of civil servants. In particular, the model includes a set of interactions between gender and presence of a DR in the family, a control for workers having a second job and higher level interactions of these controls with the PUB dummy, equal to one if the worker is a civil servant and zero otherwise. An interaction of PUB with the educational level is also included. Men and women both show a similarly higher propensity to report sick when employed in the public sector, the difference between the two being statistically not significant. Presence of a DR in the household increases significantly the probability of a woman reporting sick at work, while such an effect is not found for men. According to the non-significance of the Woman*DR*PUB interactions, this average effect is not statistically different for public sector females. The same applies when higher education is taken into consideration, while workers having second jobs do not display a higher propensity to report sick, both on average and in the public sector.

According to these results, civil servants show an average higher propensity to sickness absence than private sector ones, and this higher propensity is not due to the contribution of particular subgroups of civil servants, but can be summarised by a higher intercept.

In the next section, the identification issues faced when evaluating the impact of the two reforms at study will be discussed.

4 Identification

The effects of the three subsequent reforms for civil servants will be evaluated using a Regression Differences in Differences approach. In particular, the following equation will be estimated:

$$y_{it} = \alpha + \beta X_{it} + \gamma PUB_{it} + \lambda_1 PUB_{it}^{A_1} + \lambda_2 PUB_{it}^{A_2} + \lambda_3 PUB_{it}^{A_3} + s_t + \varepsilon_{it} \qquad (1)$$

where the binary variable y_{it}[11] is equal to one if individual i worked less than usual due to sickness during the reference week of semester t and zero otherwise, s_t are semester by year interactions, X_{it} is a vector of socio-demographic and job related controls including

that, net of composition effects, civil servants are more likely to experience sickness absence spells even after controlling for several health-related variables, suggesting higher absence rates in the public sector are not due to epidemiological factors.

[11] See section 3 for details.

age, education, marital status, presence of a DR in the household, working region, tenure (linear and quadratic), type of contract, contractual hours (linear and quadratic) and firm size. The average effect of belonging to the public sector in semester t is captured by the parameter γ, coefficient of the PUB_{it} dichotomic variable equal to one if the employee works for the public sector and zero otherwise. The dummies PUB^{A_1}, PUB^{A_2} and PUB^{A_3} are interactions between PUB and respectively three dummy variables equal to one during phase 1 (2008:S2-2009:S1), phase 2 (2009:S2) and phase 3 (2010:S1) of the reform. As a consequence, coefficients $\lambda_{1,2,3}$ capture any systematic variation in absence rates taking place during phase 1, 2, and 3 of the reform at study compared to pre-reform levels:

$$\lambda_x = E[y_{it}|PUB = 1, d_{A_x} = 1] - E[y_{it}|PUB = 1, d_B = 1] - E[y_{it}|PUB = 0, d_{A_x} = 1] - E[y_{it}|PUB = 0, d_B = 1]; x = 1, 2, 3 \quad (2)$$

where d_B is equal to one during the pre-reform period and zero otherwise.

In order to address the eventual downward bias in the standard errors due to within individual correlation over time, throughout the analysis standard errors are clustered at the individual level following White (1980), as suggested by Bertrand et al. (2004). For the causal interpretation of the results, three identifying conditions have to be met (Blundell and Macurdy, 1999; Cameron and Trivedi, 2005):

Condition 1. Conditional on the controls X_{it} and s_t, the treatment ($PUB = 1$) and the control ($PUB = 0$) group have common time effects;

Condition 2. Conditional on the controls X_{it} and s_t, the introduction of the policy under evaluation does not alter the treatment and the control group composition in terms of propensity to experience sickness absence in a systematic way;

Condition 3. The reform does not trigger spill-over effects between the *treatment* and the *control* group.

Conditions 1 and 2 will be respectively assessed in subsections 4.1 and 4.2, while the eventual bias due to departures from condition 3 will be discussed in section 5.2.

4.1 Common trend

In order to empirically test *Condition 1*, a regression on the pre-reform period is run identical to the one reported in Table 2, but adding a linear and a quadratic trend interacted with a dummy equal to one for civil servants (abbreviated, PUB). These controls should capture any systematic change in relative public/private absence rates taking place over time before the reform. Point estimates for both coefficients are very close to zero and are statistically not-significant, providing no evidence of the existence of a trend in relative public/private sector absence rates. As an additional robustness check, a more flexible specification is adopted, substituting the linear and the quadratic trends with a full set of $PUB * s_t$ interactions for each of the 9 semesters prior to the introduction of the 133/2008 law. The hypothesis of a common trend in absence rates cannot be rejected if the interactions are not significantly different from zero, that is, each of the semester differences in absence rates between the control and the treatment group is constant conditional on the controls and on the (common) semester by year fixed effects. The estimated values for the interactions, reported in Table 3, show that the hypothesis of the presence of a common trend in absence rates before the reform cannot be rejected, with parameter estimates never statistically different from zero in any of the 9 semesters. Point estimates range around zero, and a formal F-test of all the interactions being jointly equal to zero does not reject the null (p value=0.84).

4.2 Sorting effects

Condition 2 will now be tested detecting the possibility of systematic sorting effects across sectors and labour market states triggered by the reform. Conditional on labor market state in $t-4$, four equations are estimated through LPM (Table 4). The first two of them estimate the probability of leaving the public (private services) sector to any other state during the $[t-4, t]$ interval, and detect any systematic variation in these transitions for individuals who reported sick in $t-4$. The aim is to test whether the probability of quitting the control or treatment group increased during the reforms for workers with a systematically different propensity to report sick. As discussed in the previous section, if this were the case, there would be non random attrition, a potential source of bias.

The other two equations estimate the probability of being in the public (private ser-

vices) sector in t, conditional on being employed, but not in that sector, in $t-4$. Note that the last two equations are not symmetric with respect to the first ones, since sickness absence in $t-4$ can be observed for employed individuals only. This is the reason why the estimating sample is restricted to individuals employed in $t-4$. This set of equations complements the previous one, checking whether the probability of entering the control (treatment) group changed during the reforms for individuals with a systematically different propensity to report sick.

The longitudinal dimension of the dataset at hand is exploited, restricting the analysis to individuals who have been interviewed at least twice in a one year interval (75 per cent of the whole sample). For these individuals, employment status in $t-4$ together with eventual sickness absence in the same period is observed. Formally, the following equation is estimated:

$$y_{it|y_{i,t-4=0}} = \alpha + \beta X_{i,t-4} + \gamma SICKABS_{i,t-4} + \gamma_{A_1} SICKABS^{A_1}_{i,t-4} + \gamma_{A_2} SICKABS^{A_2}_{i,t-4} + \gamma_{A_3} SICKABS^{A_3}_{i,t-4} + s_t + \varepsilon_{it} \quad (3)$$

where $y_{i,t} = 0$ defines the four different transitions at study. In the public (private services) sector to other state transitions it is equal to zero if the individual was employed in the public (private services) sector in $t-4$ and is still employed in the same sector in t, while it is equal to one if the individual left that sector to any other status. Viceversa, in the two opposite transition equations it is equal to one if the individual moved from any other sector in $t-4$ to the public (private services) sector in t, while it is equal to zero if the individual did not experience this transition and was not employed in the public (private services) sector in $t-4$. The right hand side of the equation includes the usual socio-demographic and job related characteristics $X_{i,t-4}$, and semester by year dummies s_t. $SICKABS_{i,t-4}$ is a dummy variable equal to one if the worker experienced sickness absence in $t-4$ and zero otherwise. This variable captures any differential mobility pattern for individuals who reported to be sick in $t-4$. The coefficients of interests are here $SICKABS^{A_{1,2,3}}_{i,t-4}$, respectively the interaction between the dummy $SICKABS_{i,t-4}$ and the dummies $d_{A_{1,2,3}}$ (equal to one respectively during phases 1, 2 and 3 of the reform). These three variables would detect any differential mobility pattern taking place during the three *post reform* phases for individuals who were sick in $t-4$. A significant coefficient

for these variables would entail a systematic change in the probability of changing sector or labour market status during the reform period for workers more exposed to sickness absence. This would provide evidence of workers' sorting as a result of the reforms.

Estimates show that the probabilities of moving from the public sector in $t-4$ to any other state in t (column 1 of Table 4) are lower in the South of Italy and are higher for part-time and temporary workers, while decrease with tenure. On average, civil servants who report to have worked less than usual in $t-4$ because of sickness have higher probabilities of changing sector or leaving employment in t, but the only differential pattern taking place during the reform is a significant decrease of this probability during *phase 3*. If anything, a lower probability of leaving the public sector for individuals with a higher propensity to be sick should introduce a downward bias in our estimates of the reforms' effects, if the propensity to be sick is assumed to be correlated over time. Also for workers employed in the private services sector in $t-4$, the probability of experiencing a transition to any other state is on average higher for individuals who reported to be sick in $t-4$ (column 3), and the point estimate is similar to the one found for civil servants. This probability increases significantly by 5.3 percentage points during *phase 1*. Also in this case, a higher propensity to leave the private services sector (the control group) during the $t-4, t$ interval for individuals who reported to be sick in $t-4$ might introduce (if anything) a downward bias in the policy evaluation exercise. Finally, the transitions into the treatment and the control groups are analysed (columns 2 and 4). The only significant change in transitions that is relevant for identification is detected with respect to workers moving into the public sector. During *phase 2* of the reform, the likelihood of experiencing this transition significantly increased by 0.8 percentage points for individuals who reported sickness absence in $t-4$. Also in this case, this result might introduce, if anything, a downward bias in the policy evaluation exercise.

The effects of eventual departures from *Condition 3* will be assessed in section 5.2. As a final caveat, it is likely that the total incidence of sickness absence is affected by truncation of short sickness spells, given that the data at hand have low frequency (weeks) compared to the event at study (days). Nevertheless, there is no reason to expect that the extent of truncation changes systematically because of the reform. If anything, since the wage penalty introduced by the new policy is the highest for absence spells below 10 days, the presence of truncation of short spells is expected to introduce a downward bias

in the policy evaluation exercise.

5 Results

5.1 Average treatment effects

Having discussed the conditions underlying the causal interpretation of the reform's effects it is now possible to present the results obtained estimating equation 1 on the full sample (column 1 of Table 5).

Conditional on observables, civil servants have 0.63 percentage points higher average probability of reporting sick at work. The coefficient of the variable $PUB*A_1$, identifying the average effect of the reform in its *phase 1* setting, is negative and significant at 1% level. According to the estimate, during this phase of the reform, when monetary incentives were coupled with increased monitoring, sickness absence incidence decreased exactly by 0.63 percentage points, eliminating the difference with private services sector workers conditional on observables. On the contrary, during *phase 2* of the reform, in which only monetary incentives were in place and monitoring went back to the *pre-reform* period, there was a neat rebound in absence rates. In this case, the variation compared to the pre-reform period drops to -0.13 percentage points, statistically non significant an estimate at standard confidence levels. A formal test of the variation in absence rates taking place in *phase 2* being equal to the one occurred during *phase 1* rejects the null at the 5% level. Finally, when during *phase 3* monitoring intervals increased again to 7 hours, a new significant drop in absence rates is observed (-0.63 percentage points). These patterns are confirmed when estimation is performed on a sub-sample excluding individuals with tenure shorter than a year (column 2). This robustness check is meant to test the robustness of the results restricting the sample to individuals who have terminated their probation period, thus enjoying higher EPL levels.

In order to test for the presence of substitution between sickness absence and other types of absence, an identical set of regressions is run where the dependent variable is absence for reasons other than sickness.[12] No significant shift to other types of absences as a response to the sickness absence policy reforms is found, both on the full sample and on the sample including only workers with tenure longer than a year (respectively,

[12]See section 3 for a definition of the variable.

columns 3 and 4 of Table 5).

These results point unambiguously to the fact that monitoring effectiveness is the driving force in determining presence at work. The new, significant, reduction in absence rates observed during phase 3 of the policy shows that the results are not driven by the attenuation of the reform effects over time *(attenuation bias)*. In the next three subsections, the effects of other potential sources of bias will be addressed. In particular, subsection 5.2 will assess the effects of the presence of *spillover* effects of the reform on the control group (a violation of condition 3 for identification), while subsection 5.3 will address the potential bias coming from heterogenous time effects due to differences in predetermined characteristics between the control and the treatment group. Finally, subsection 5.4 will present the results of a *falsification test* showing that, out of the 13 semesters observed in this study, the only significant drops in the public/private sector absence rates were observed in the three semesters in which monetary incentives were coupled with stricter monitoring.

5.2 Spillovers

In this section, the eventual existence of spillovers preventing correct identification is taken into consideration.[13] An increasing media-pressure on absenteeism triggered by the reform might for example have put a downward pressure on private services workers' absence rates (the control group) during the evaluation period. These indirect interactions are very difficult to disentangle empirically. Nevertheless, if present, indirect effects of this kind would introduce a downward bias in the magnitude of the estimates of the reform at study. Implications could be less clear at the household level, where the sign of spillovers from the civil servant partner to the private sector one are *a priori* unclear and determined by three different elements:

- *between partner substitution in absence behaviour*, determined for example by the necessity of staying at home for taking care of Dependent Relatives. The increase in relative price of absence for public sector workers might have induced substitution in absence between partners if one of them works in the private sector. In this case, an increase in absence rates in the private sector is expected as a result of the reform, determining an *upward bias* in the policy evaluation estimates;

[13] See condition 3 of section 4.

- *between partner complementarities in absence behaviour*, if partners prefer to spend their time absent from work together. In this case a decrease in the private sector absence rates is expected, implying a *downward bias* in the reform effects' estimates;
- *changes in absence behaviour in the reference group:* the stricter policy on absenteeism might have increased the psychological cost of opportunistic behaviour within the household, decreasing the propensity to be absent for both partners, irrespective of sector of employment, when one of them works for the public sector, implying a *downward bias* in the reform effects' estimates.

Negative (positive) spillover effects of the reform on absence rates of private sector workers, the control group in the policy evaluation exercise, would induce a downward (upward) bias in the estimates of the relevant policy parameter, violating *Condition 3* for identification, as outlined in section 4.

In order to check the robustness of section 5.1 results to this kind of bias, equation 1 is re-estimated dropping all the observations regarding so called mixed couples, in which one partner works in the private and one in the public sector. In this case, average absence rates are 0.66 percentage points higher in the public than in the private services sector (column 1 of Table 6). During *phase 1* of the reform, this difference is eliminated with a 0.67 percentage points decrease in absence rates, a result significant at the 1% level. Again we find a neat rebound in sickness absence during *phase 2* of the reform, when the probability for a civil servant to report sick is 0.16 percentage points lower than in the pre reform period, a difference not statistically different from zero. A formal test of this variation in absence rates being equal to the one estimated for *phase 1* rejects the null at the 5% level. Absence rates significantly drop again (-0.62 p. p.) during *phase 3* of the reform. This reduction is not statistically different from the one observed in *phase 1*. Results are confirmed when dropping from the sample all workers with tenure shorter than a year (column 2). Finally, no significant variation in absences for reasons other than sickness during the reform period is found (columns 3 and 4).

5.3 Heterogeneous time effects

Another potential source of bias could come from the presence of heterogeneous time effects, due to pre-determined *observed* characteristics. For example, given that civil servants are concentrated in the South of the country compared to private sector workers, a

higher incidence of the epidemiological season in the North during some semesters of the post-reform period could determine a decline in relative public/private average absence rates. This is actually an extreme event, given that the common trend test presented in subsection 4.1 shows that, conditional on observables and semester by year fixed effects, relative public/private absence rates did not show any significant shift in any of the 9 semesters of the pre-reform period. Nevertheless, we check whether this potential source of bias could be driving the results obtained so far estimating equation 1 with matched differences in differences regressions. In particular, using the algorithm developed by Leuven and Sianesi (2005), in each quarter a subsample of the control group is created including, without replacement, all private sector workers whose probability of being in the public sector lies within a caliper of 1 percentage point compared with the one estimated for a civil servant. The propensity to be in the public sector is estimated as a function of the following pre-determined characteristics: gender, age, area of work, education.[14] Given the fact that the control group constitutes 72.7 per cent of the sample, it was possible to match each civil servant to one private sector worker. The resulting subsample includes all the public sector workers and a control group including private services sector employees having a propensity to be in the public sector similar to the one estimated for the civil servants themselves. The two groups of the subsample are balanced with respect to the above-mentioned explanatory variables in each of the quarters analysed in this study.[15]

Results obtained with this specification are not dissimilar from the standard estimates (Table 7): in this case, civil servants show absence rates that are on average 0.58 percentage point higher than in the private services sector, a coefficient that is significant at the 1 per cent level. This difference is almost eliminated during *phase 1* of the reform, when a significant drop in absence rates equal to 0.53 p.p. is observed, while absence rates observed during *phase 2* are not statistically different from the pre-reform ones. Finally, a new drop in absence rates is observed under *phase 3* of the policy. Similar results are found when workers with tenure lower than a year are not included in the sample, while

[14]Main results are not sensitive to the specification employed for the estimation of the matching equation. Independent variables are defined as in the main equations (age: 5 dummies spanning 5 ten year intervals; area of work: north, centre, south; dummy for high education).

[15]The test rejects the null of equality in the means at the 5 per cent level in 4 out of 208 times (208=8 independent variables*26 quarters). Descriptive statistics and results of the tests of the balancing properties are available upon request.

no spill-over effect is found on other types of absence.

5.4 Falsification test

As a final robustness check, a falsification test is carried out (Table 8). In particular, we add to the basic specification a full set of Public sector*semester*year interactions. These interactions would capture any significant change in relative public/private absence rates in any semester of the estimation interval. In the 9 semesters of the pre-reform period, point values for these interactions range between -0.36 and 0.13 and are never statistically different from zero at any standard confidence level. The point estimates for these interactions are instead equal to -0.56 and -0.97 in the first and the second semester of *phase 1* of the post-reform period, respectively significant at the 10 and the 1 per cent level. The coefficient for *phase 2* interaction is equal to -0.26, not statistically significant at standard confidence levels, while during *phase 3* its value again increases in absolute value to -0.78 percentage points, significant an estimate at 1 per cent level.

The overall pattern does not change when restricting the sample to workers with tenure shorter than a year (column 2). Also with this specification, no shift to other types of absence is detected (columns 3 and 4).

5.5 Evidence from other data sources

Results of the econometric analysis entail strong reform effects, providing clean evidence for the fact that, at least for Italian civil servants, effective monitoring is a very useful mean for reducing absenteeism. Such a study can be performed only using the dataset at hand, a unique source providing homogeneous information on sickness absence both for the private services and the public sector. Nevertheless, it is useful to use alternative datasets to look for evidence able to confirm or contradict the main empirical results obtained in this paper. According to government's official data,[16] during *phase 1* of the reform at study, days of sickness absence diminished on average by 38 per cent compared to a year earlier. During the first 5 months of *phase 2* (July to November), there was instead an average 30 per cent increase on the same period of the previous year, slowing to +8 per cent in December. A new drop in absence rates compared to the pre reform period is observed in *phase 3* of the reform. Administrative data on their

[16]Ceci and Giungato (2011).

own employees collected by the Social Security Agencies and the Fiscal Agencies,[17] sub-sectors of the Public Administration employing around 30 thousand people each, convey a similar picture for the first two phases of the reform, while results for *phase 3* period are not available.

Also results presented in Del Boca and Parisi (2010) and De Paola et al. (2009), two articles evaluating the effects of the reform on different datasets, are coherent with the main findings of this paper. These articles have the advantage of relying on administrative datasets. Nevertheless, the analysis carried out here is more general since it uses a sample with homogenous and broadly representative information on the control and the treatment groups. Del Boca and Parisi (2010) make use of two personnel datasets coming respectively from a security company (control group) employing slightly less than 3 thousand workers and from the Fiscal Agencies (30 thousand employees). They find a 20 per cent decrease in relative absence rates during *phase 1* of the reform, and a reversal when monitoring was loosened. De Paola et al. (2009) use instead time series variation in absence rates for a local branch of the public administration employing 860 workers to identify the effects of the *phase 1* of the reform, finding a 50% decrease in absence.

6 Differences by workers' rents

Previous literature (Henrekson and Persson, 2004; Johansson and Palme, 2005; Ziebarth, 2009; Ziebarth and Karlsson, 2010) has found a noticeable impact of sick pay insurance levels on the number of days lost because of sickness. According to the evidence discussed so far, the increase in strictness of the spot, medical inspection system had a crucial role in reducing absence rates in the public sector, while moderate monetary incentives were relatively less effective. The drop in absence rates taking place during the first phase of the reform could be driven both by an increase in presenteeism driven by pay cuts, or by a decrease in opportunistic behaviour determined by stricter monitoring. On the contrary, the great changes in absence rates taking place in the following phases, when only monitoring levels changed, have to be determined by variations in opportunistic behaviour. In this section, we will further investigate this aspect checking whether the threat of disciplinary action for cheaters was more effective in reducing opportunistic

[17] See Fioravanti et al. (2010) and Dongiovanni and Pisani (2010).

behaviour among workers enjoying higher rents from being in the public sector.

The fact that public sector wages are compressed compared to private sector ones is unquestioned by economic literature (see Gregory and Borland (1999) for a survey). In particular, low-qualified civil servants enjoy higher wages compared to their private sector peers, while the opposite is true for highly qualified workers. Recent work has confirmed such a tendency for the Italian public sector as well (Lucifora and Meurs, 2006). This peculiar feature of public sector labour markets entails that opportunistic behaviour should be more sensitive to monitoring for low skill government workers compared to high skilled ones: for the former a job loss related to disciplinary action is more costly.

In order to investigate this possibility, the effects of the reform at study are estimated separately for civil servants with different skill levels, following the specification of equation 1. In a first estimate (table 9), we look for heterogeneous effects of the reform based on workers' occupation (clerk or managers). On average, clerks tend to have a 0.36 percentage point higher probability of reporting sick at work compared to managers, the reference group. Clerks in the public sector have an additional 0.72 p.p. probability of reporting sick,[18] the wedge is instead equal to 0.57 p.p. for civil servants working as managers compared to the average manager (all coefficients are significant at 1% level). During the first phase of the reform, clerks' absence rates dropped by 0.74 p.p. (significant an estimate at the 1% level), converging to the average clerk's absence rate. The drop for managers was equal to -0.37 p.p., statistically not different from zero. Even looking at point values, during phase one of the reform, public sector clerks' converge on the average clerk absence rate, while managers working for the government reduce their wedge by 65% only. During phase 2 of the reform, both clerks and managers do not show significant drops in absence rates, while in phase 3 (when monitoring levels increased again), a new, strongly significant drop in absence rates (-0.72 p.p.) is observed for clerks, while coefficient estimates for managers are again lower in magnitude (-0.37 p.p.) and statistically not-significant. Main results are unchanged when dropping from the sample workers with tenure lower than a year (column 2). These estimates are coherent with the results obtained in the previous section, and suggest that workers enjoying highest economic rents are also the most sensitive to changes in monitoring levels, given that a disciplinary action potentially leading to job loss is more costly for them.

[18] Compared to the average manager, the difference is thus equal to 1.08 p.p.=0.36 p.p. + 0.72 p.p.

As a robustness check for previous results we also estimate equation 1 for workers with different educational levels (table 10), finding very similar patterns: the drop in absence rates taking place during phase 1 and phase 3 of the reforms was much more pronounced for civil servants with low education than for highly educated ones.

7 Conclusions

Results presented in this paper are relevant for the literature on incentives and absenteeism, showing that well targeted monitoring combined with effective penalties can be a way to deter sick leave abuse. The threat of disciplinary action for cheaters is more effective in reducing opportunistic behaviour among low-skilled workers that, thanks to public sector wage compression, enjoy higher economic rents from working for the government. Guaranteeing sick leave to all workers without reducing incentives to work is a major issue in the US, where the *Healthy Families Act* is currently before Congress. The proposal[19] would introduce universal paid sick leave, that is currently part of the contractual agreement: during 2009, paid sick leave was available only for 61% of private sector workers (89% in the public sector, for details see BLS (2010)). The arousal of opportunistic behaviour is one of the main concerns linked to such a legislative provision, given that the bill denies employers the possibility of introducing "any absence control policy".[20] Most Western European countries have been implementing measures to curb absenteeism, such as cutting replacement rates (Germany, Sweden) and increasing controls on sick leave claims (France); some companies have introduced interviews after sick leave (Germany, United Kingdom).[21] Based on the evidence provided by a sequence of sick pay reforms introduced for the Italian public sector and affecting more than 3.5 million workers, this paper shows that an effective way to deter opportunistic behaviour can be to increase monitoring on workers reporting sick, in particular on those who enjoy the highest rents from the current job. This strategy has the advantage of targeting cheating individuals only, while replacement rates' cuts can reduce sick absence not only by reducing absenteeism but also by increasing presenteeism. Of course these advantages have to

[19]Bill HR 2460, proposed at the House of Representatives (May 18, 2009) with the aim "to allow Americans to earn paid sick time so that they can address their own health needs ant the health needs of their families".

[20]Section 7 - Prohibited acts.

[21]For a comparative perspective on sick leave policies across developed countries, see Edwards and Greasley (2010) and Heymann et al. (2009).

be weighted against the fact that non-discretionary cuts in replacement rates lower labour costs for given absence rates and do not entail the costs related to the management of ambitious monitoring plans.

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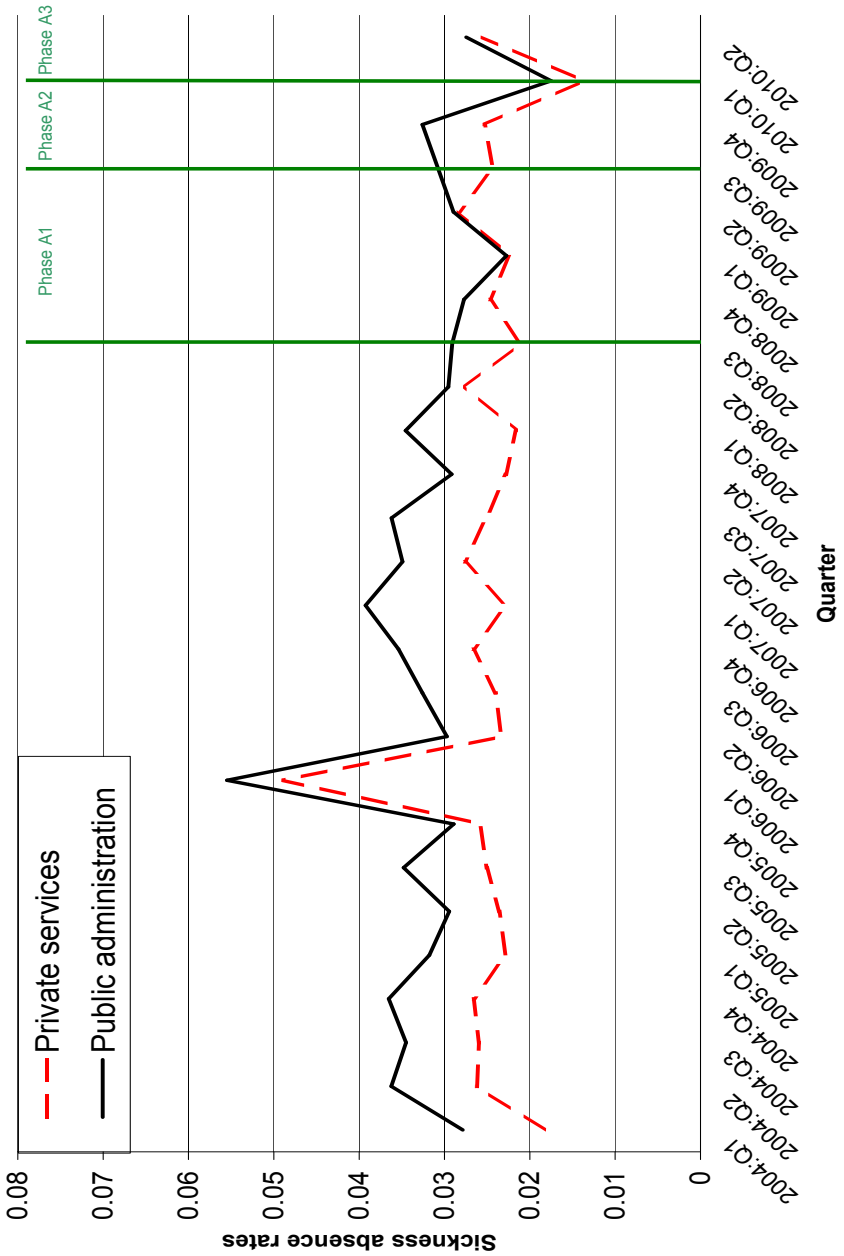
Figure 1: **Seasonally adjusted sickness absence rates**



Note: Author's calculations on Istat, Labour Force Survey. The figure reports seasonally adjusted average sickness absence rates in the public and the private services sectors. The first, second and third green vertical lines identify respectively the semester of start of the phase 1 (2008:S2), phase 2 (2009:S2) and phase 3 (2010:S1) of the new sickness absence policies.

Table 1: **Descriptive statistics, weighted sample**

Binary variables means

	Private sector		*Public sector*	
	Pre-reform	*Post-reform*	*Pre-reform*	*Post-reform*
Woman	0.51	0.52	0.41	0.42
High education	0.19	0.22	0.23	0.25
North	0.56	0.55	0.35	0.38
Center	0.22	0.23	0.26	0.25
South	0.22	0.22	0.39	0.38
Aged 15-24	0.07	0.06	0.01	0.01
Aged 25-34	0.33	0.30	0.13	0.10
Aged 35-44	0.32	0.33	0.35	0.32
Aged 45-54	0.22	0.24	0.37	0.40
Aged 55-64	0.06	0.07	0.14	0.18
Temp	0.09	0.10	0.07	0.07
Firm size (x)				
$x <= 10$	0.36	0.34	0.08	0.07
$11 <= x <= 15$	0.10	0.11	0.05	0.05
$16 <= x <= 19$	0.04	0.06	0.03	0.04
$20 <= x <= 49$	0.15	0.16	0.18	0.19
$50 <= x <= 249$	0.18	0.18	0.36	0.38
$x >= 250$	0.12	0.12	0.23	0.23
$x < 10^a$	0.05	0.03	0.06	0.03
Obs.	149474	65436	57272	23379
Sickness absence	0.016	0.013	0.026	0.019
Other absence	0.019	0.017	0.011	0.010

Discrete variables

	Mean	Std. Dev.	Min	Max
Private sector, pre reform				
Years of tenure	10.51	9.51	0	47
Contractual hours	37.17	9.76	0	110
Private sector, post reform				
Years of tenure	10.79	9.69	0	49
Contractual hours	37.00	9.22	0	105
Public sector, pre reform				
Years of tenure	16.50	9.69	0	44
Contractual hours	35.97	7.16	0	105
Public sector, post reform				
Years of tenure	17.80	10.01	0	46
Contractual hours	36.06	6.47	0	100

Notes: Author's calculations on ILFS data. Weighted values. The pre-reform period is 2004:S1-2008:S1; the post reform period is 2008:S2-2010:S1. The Table includes only white collar employees not employed in the army, the health care or education sector and those individuals absent from work for reasons outside their control. [a] the worker is not able to recall exact firm size.

Table 2: **LPM for the incidence of sickness absence, pre-reform period (2004:S1-2008:S1)**

Column	1	2
	*Coefficients*100*	
Public sector (PUB)	0.585	
	[7.45]***	
Dependent Relative (DR)	0.405	
	[4.93]***	
Contractual hours	-0.021	-0.021
	[1.98]**	[2.01]**
Contractual hours^2/100	0	0
	[0.85]	[0.95]
High education	-0.38	-0.323
	[5.53]***	[4.35]***
Woman	0.53	0.484
	[8.45]***	[6.79]***
Married	-0.149	-0.148
	[1.99]**	[1.97]**
Age 25-34	0.291	0.272
	[2.98]***	[2.79]***
Age 35-44	0.457	0.467
	[3.96]***	[4.04]***
Age 45-54	0.66	0.674
	[5.07]***	[5.18]***
Age 55-64	1.006	1.013
	[5.91]***	[5.97]***
Center	0.481	0.482
	[5.66]***	[5.68]***
South	0.141	0.143
	[2.20]**	[2.23]**
Part time	-0.333	-0.376
	[3.21]***	[3.61]***
Temps	-0.255	-0.256
	[2.85]***	[2.87]***
Tenure	0.03	0.03
	[2.68]***	[2.64]***
Tenure^2/100	-0.032	-0.032
	[0.91]	[0.90]
11 to 15 employees	0.281	0.285
	[2.90]***	[2.94]***
16 to 19 employees	0.395	0.4
	[2.87]***	[2.90]***
20 to 49 employees	0.425	0.43
	[5.01]***	[5.07]***
50 to 249 employees	0.545	0.551
	[6.58]***	[6.65]***
250 or more employees	0.754	0.761
	[7.11]***	[7.17]***
10 or more employees[a]	-0.406	-0.398
	[4.05]***	[3.96]***
Man*PUB		0.585
		[5.16]***
Woman*PUB		0.505
		[3.81]***
High edu*PUB		-0.219
		[1.37]
Man*DR		0.006
		[0.05]
Man*DR*PUB		0.573
		[2.39]**
Woman*DR		0.615
		[4.83]***
Woman*DR*PUB		0.134
		[0.43]
Second job		0.327
		[1.19]
Second job*PUB		0.609
		[0.99]
Observations	250927	250927

Notes: Author's calculations on ILFS data. LPM regression for the probability of being absent. Robust t statistics in brackets based on standard errors clustered at the individual level following White (1980). PUB stands for public sector; DR stands for Dependent Relative(s). Includes only white collar employees not employed in the army or manufacturing; individuals working in the health care or education sector, or otherwise absent from work for reasons outside their control are excluded (see section 3 for details on sample selection). Includes a full set of semester by year interactions. [a] the worker is not able to recall exact firm size. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: **Test of common trend, pre-reform period (2004:S1-2008:S1)**

	*Coefficients*100*	
Column	1	2
Public sector*Trend	-0.001	
	[0.02]	
Public sector*Trend^2/100	-0.001	
	[0.33]	
Public sector*2004:S2		0.02
		[0.07]
Public sector*2005:S1		-0.213
		[0.70]
Public sector*2005:S2		-0.203
		[0.68]
Public sector*2006:S1		-0.227
		[0.65]
Public sector*2006:S2		-0.257
		[0.82]
Public sector*2007:S1		0.12
		[0.36]
Public sector*2007:S2		-0.117
		[0.38]
Public sector*2008:S1		-0.376
		[1.20]
F test: all int.=0		
Pvalue		0.84
Observations	206746	206746

Notes: LPM regression for the probability of being absent. Columns one and two report parameter estimates for a model equal to the one of Table 2, column 1, augmented respectively with an interaction between the Public sector and a linear/quadratic trend and a full semester by year by Public sector interactions. Includes only white collar employees not employed in the army or manufacturing; individuals working in the health care or education sector, or otherwise absent from work for reasons outside their control are excluded (see section 3 for details on sample selection). Robust T statistics in squared brackets based on standard errors clustered at the individual level following White (1980). * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: **Test for sorting effects**

*Coefficients*100*

	Treatment group		Control group	
Column	1	2	3	4
Transition	Public (t-4) to Other state (t)	Other state (t-4) to to Public (t)	Private (t-4) to to Other state (t)	Other state (t-4) to to Private (t)
SICKABS	4.764	-0.087	3.766	-0.492
	[3.46]***	[0.95]	[2.89]***	[3.01]***
SICKABS$*A_1$	-0.407	0.36	5.347	0.032
	[0.15]	[1.40]	[1.75]*	[0.10]
SICKABS$*A_2$	-4.171	0.792	7.11	0.255
	[1.22]	[1.69]*	[1.45]	[0.55]
SICKABS$*A_3$	-6.472	0.029	0.939	0.133
	[2.17]**	[0.14]	[0.28]	[0.32]
Woman	0.288	0.003	0.801	0.036
	[0.85]	[0.09]	[2.88]***	[0.61]
Center	-0.753	0.107	0.141	-0.062
	[1.73]*	[3.05]***	[0.41]	[0.78]
South	-0.779	0.146	1.965	-0.355
	[2.25]**	[5.07]***	[6.26]***	[6.16]***
Contractual hrs	-0.197		-0.631	
	[1.01]		[6.26]***	
Contrac. hrs^2/100	0.002		0.006	
	[1.06]		[5.70]***	
High education	0.384	0.302	0.433	0.944
	[0.94]	[6.49]***	[1.24]	[10.82]***
Tenure	-1.105		-0.74	
	[13.06]***		[13.61]***	
Tenure^2/100	3.042		1.961	
	[12.54]***		[11.26]***	
Part time	5.139	0.055	6.785	0.07
	[5.30]***	[1.44]	[10.42]***	[0.75]
Temp	1.957	0.018	-2.103	0.906
	[1.73]*	[0.48]	[3.12]***	[7.08]***
Observations	34329	347058	72263	248391
Age dummies	Yes	Yes	Yes	Yes
Firm size dumm.	Yes	Yes	Yes	Yes
Family comp. dumm.	Yes	Yes	Yes	Yes
Semester*Year interact.	Yes	Yes	Yes	Yes

Notes: LPM for the probability of experiencing the transition specified in the header (see section 4.2 for details). Includes a constant. Includes only white collar employees not employed in the army or manufacturing; individuals working in the health care or education sector, or otherwise absent from work for reasons outside their control are excluded (see section 3 for details on sample selection). Robust T statistics in squared brackets based on standard errors clustered at the individual level following White (1980). * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: **The causal effect of the 133/2008 law on public sector absenteeism: whole sample**

	*Coefficients*100*			
	Sickness absence		Other absence	
Column	1	2	3	4
Public sector	0.631	0.642	-0.099	-0.105
	[7.39]***	[7.33]***	[1.40]	[1.45]
Public sector $*A_1$	-0.638	-0.63	-0.048	-0.02
	[3.89]***	[3.76]***	[0.36]	[0.15]
Public sector $*A_2$	-0.129	-0.204	0.01	-0.002
	[0.56]	[0.87]	[0.05]	[0.01]
Public sector $*A_3$	-0.652	-0.676	0.164	0.199
	[3.06]***	[3.14]***	[0.86]	[1.03]
Dependent Relative	0.407	0.428	4.377	4.526
	[5.42]***	[5.51]***	[38.94]***	[38.84]***
Part time	-0.501	-0.507	-0.614	-0.64
	[3.75]***	[3.56]***	[3.95]***	[3.85]***
Temp	-0.241	-0.205	-0.825	-0.74
	[2.98]***	[2.11]**	[9.82]***	[7.20]***
Woman	0.515	0.525	2.533	2.636
	[9.02]***	[8.79]***	[43.51]***	[43.03]***
High edu	-0.335	-0.312	0.322	0.336
	[5.35]***	[4.72]***	[4.37]***	[4.31]***
Tenure	0.035	0.03	0.039	0.009
	[3.45]***	[2.71]***	[4.01]***	[0.83]
Tenure $^2/100$	-0.052	-0.039	-0.12	-0.038
	[1.64]	[1.17]	[4.90]***	[1.40]
Constant	1.579	1.566	-0.299	-0.281
	[3.43]***	[3.16]***	[0.66]	[0.56]
Observations	295561	280447	295561	280447
Age dummies	Yes	Yes	Yes	Yes
Region of work dummies	Yes	Yes	Yes	Yes
Firm size dummies	Yes	Yes	Yes	Yes
Family composition dummies	Yes	Yes	Yes	Yes
Semester*Year interactions	Yes	Yes	Yes	Yes
Contract. hrs. (linear and quad.)	Yes	Yes	Yes	Yes
Tenure<= 1	Included	Not Included	Included	Not Included

Notes: LPM for the probability of experiencing the absence specified in the header (see section 5.1 for details). Includes only white collar employees not employed in the army or manufacturing; individuals working in the health care or education sector, or otherwise absent from work for reasons outside their control are excluded (see section 3 for details on sample selection). Robust T statistics in squared brackets based on standard errors clustered at the individual level following White (1980). * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: **The causal effect of the 133/2008 law on public sector absenteeism: indirect test for spillover effects**

	*Coefficients*100*			
	Sickness absence		Other absence	
Column	1	2	3	4
Public sector	0.66	0.671	-0.087	-0.089
	[7.31]***	[7.26]***	[1.21]	[1.21]
Public sector$*A_1$	-0.674	-0.67	-0.064	-0.047
	[3.85]***	[3.74]***	[0.47]	[0.33]
Public sector$*A_2$	-0.165	-0.228	-0.032	-0.047
	[0.68]	[0.93]	[0.17]	[0.24]
Public sector$*A_3$	-0.623	-0.653	0.066	0.094
	[2.71]***	[2.81]***	[0.35]	[0.48]
Dependent Relative	0.383	0.403	4.334	4.487
	[4.90]***	[4.99]***	[37.14]***	[37.03]***
Part time	-0.458	-0.466	-0.561	-0.589
	[3.30]***	[3.13]***	[3.51]***	[3.43]***
Temp	-0.227	-0.196	-0.73	-0.639
	[2.79]***	[1.99]**	[8.72]***	[6.18]***
Woman	0.531	0.544	2.495	2.606
	[9.04]***	[8.84]***	[41.62]***	[41.19]***
High edu	-0.344	-0.32	0.259	0.273
	[5.34]***	[4.71]***	[3.47]***	[3.46]***
Tenure	0.04	0.037	0.047	0.017
	[3.92]***	[3.32]***	[4.77]***	[1.54]
Tenure^2/100	-0.067	-0.059	-0.138	-0.055
	[2.07]**	[1.72]*	[5.55]***	[2.03]**
Constant	1.437	1.411	-0.464	-0.447
	[3.05]***	[2.78]***	[1.00]	[0.87]
Observations	278084	263376	278084	263376
Age dummies	Yes	Yes	Yes	Yes
Region of work dummies	Yes	Yes	Yes	Yes
Firm size dummies	Yes	Yes	Yes	Yes
Family composition dummies	Yes	Yes	Yes	Yes
Semester*Year interactions	Yes	Yes	Yes	Yes
Contract. hrs. (linear and quad.)	Yes	Yes	Yes	Yes
Tenure<= 1	Included	Not Included	Included	Not Included

Notes: LPM for the probability of experiencing the absence specified in the header (see section 5.2 for details). Includes only white collar employees not employed in the army or manufacturing. The health care or education sector and those individuals absent from work for reasons outside their control are excluded (see section 3 for details on sample selection). Robust T statistics in squared brackets based on standard errors clustered at the individual level following White (1980). * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: **The causal effect of the 133/2008 law on public sector absenteeism: matched control group**

	*Coefficients*100*			
	Sickness absence		Other absence	
Column	1	2	3	4
Public sector	0.585	0.603	0.007	-0.004
	[5.90]***	[5.96]***	[0.09]	[0.05]
Public sector$*A_1$	-0.527	-0.535	-0.046	-0.034
	[2.69]***	[2.67]***	[0.32]	[0.23]
Public sector$*A_2$	0.028	-0.034	0.04	0.035
	[0.10]	[0.13]	[0.20]	[0.17]
Public sector$*A_3$	-0.426	-0.463	0.19	0.211
	[1.70]*	[1.82]*	[0.94]	[1.03]
Dependent Relative	0.444	0.459	2.95	2.989
	[4.01]***	[4.06]***	[23.29]***	[23.14]***
Part time	-0.483	-0.486	-0.117	-0.123
	[2.22]**	[2.13]**	[0.60]	[0.61]
Temp	-0.345	-0.362	-0.884	-0.789
	[2.33]**	[2.14]**	[6.79]***	[5.29]***
Woman	0.454	0.464	1.945	1.969
	[5.29]***	[5.28]***	[26.62]***	[26.29]***
High edu	-0.339	-0.329	0.524	0.546
	[3.78]***	[3.55]***	[6.31]***	[6.40]***
Tenure	0.049	0.045	-0.004	-0.017
	[3.40]***	[2.90]***	[0.37]	[1.43]
Tenure^2/100	-0.081	-0.072	0.001	0.036
	[1.92]*	[1.61]	[0.04]	[1.25]
Constant	1.768	1.866	0.292	0.493
	[2.37]**	[2.32]**	[0.49]	[0.76]
Observations	157908	153542	157908	153542
Age dummies	Yes	Yes	Yes	Yes
Region of work dummies	Yes	Yes	Yes	Yes
Firm size dummies	Yes	Yes	Yes	Yes
Family composition dummies	Yes	Yes	Yes	Yes
Semester*Year interactions	Yes	Yes	Yes	Yes
Contract. hrs. (linear and quad.)	Yes	Yes	Yes	Yes
Tenure<= 1	Included	Not Included	Included	Not Included

Notes: LPM for the probability of experiencing the absence specified in the header (see section 5.3 for details). Includes only white collar employees not employed in the army or manufacturing; individuals working in the health care or education sector, or otherwise absent from work for reasons outside their control are excluded (see section 3 for details on sample selection). In each quarter, only private services sector workers matched with a public sector worker are included in the control group. Matched private sector workers have a probability to be in the public sector (explained by gender, age, geographical area, education) which lies within a caliper of one percentage point of the same probability estimated for the civil servant. Robust T statistics in squared brackets based on standard errors clustered at the individual level following White (1980). * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8: **The causal effect of the 133/2008 law on public sector absenteeism: falsification test**

	*Coefficients*100*			
	Sickness absence		Other absence	
Column	1	2	3	4
Public sector	0.763	0.758	-0.012	-0.006
	[3.44]***	[3.37]***	[0.07]	[0.03]
Public sector∗2004 : $S2(B)$	0.025	0.06	-0.108	-0.185
	[0.08]	[0.19]	[0.44]	[0.74]
Public sector∗2005 : $S1(B)$	-0.21	-0.199	-0.055	-0.045
	[0.69]	[0.64]	[0.22]	[0.18]
Public sector∗2005 : $S2(B)$	-0.198	-0.179	-0.034	-0.054
	[0.66]	[0.58]	[0.14]	[0.21]
Public sector∗2006 : $S1(B)$	-0.216	-0.191	0.062	0.061
	[0.62]	[0.54]	[0.23]	[0.22]
Public sector∗2006 : $S2(B)$	-0.251	-0.253	-0.063	-0.066
	[0.80]	[0.79]	[0.25]	[0.25]
Public sector∗2007 : $S1(B)$	0.134	0.144	-0.01	0.005
	[0.41]	[0.43]	[0.04]	[0.02]
Public sector∗2007 : $S2(B)$	-0.11	-0.054	-0.198	-0.266
	[0.36]	[0.17]	[0.80]	[1.04]
Public sector∗2008 : $S1(B)$	-0.361	-0.375	-0.387	-0.359
	[1.15]	[1.18]	[1.64]	[1.49]
Public sector∗2008 : $S2(A_1)$	-0.562	-0.529	-0.072	-0.072
	[1.91]*	[1.75]*	[0.28]	[0.28]
Public sector∗2009 : $S1(A_1)$	-0.975	-0.957	-0.199	-0.167
	[3.23]***	[3.13]***	[0.82]	[0.68]
Public sector∗2009 : $S2(A_2)$	-0.26	-0.32	-0.078	-0.101
	[0.84]	[1.02]	[0.31]	[0.39]
Public sector∗2010 : $S1(A_3)$	-0.783	-0.793	0.077	0.1
	[2.65]***	[2.65]***	[0.30]	[0.38]
Observations	295561	280447	295561	280447
Tenure<= 1	Included	Not Included	Included	Not Included

Notes: LPM for the probability of experiencing the absence specified in the header. This specification is the same as the one presented in column 1 of Table 2, plus the full set of semester*year*Public sector interactions. See section 5.4 for details. Includes only white collar employees not employed in the army or manufacturing. The health care or education sector and those individuals absent from work for reasons outside their control are excluded (see section 3 for details on sample selection). Robust T statistics in squared brackets based on standard errors clustered at the individual level following White (1980). * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9: **The causal effect of the 133/2008 law on public sector absenteeism: effects by occupation**

	*Coefficients*100*			
	Sickness absence		Other absence	
Column	1	2	3	4
Clerk*Public sector	0.722	0.736	-0.179	-0.183
	[7.68]***	[7.63]***	[2.33]**	[2.32]**
Clerk*Public sector*A_1	-0.74	-0.736	0.035	0.067
	[4.06]***	[3.94]***	[0.24]	[0.44]
Clerk*Public sector*A_2	-0.235	-0.313	0.015	0.002
	[0.92]	[1.21]	[0.08]	[0.01]
Clerk*Public sector*A_3	-0.724	-0.747	0.1	0.127
	[4.09]***	[4.15]***	[0.65]	[0.81]
Manager*Public sector	0.567	0.566	0.21	0.193
	[3.50]***	[3.45]***	[1.69]*	[1.53]
Manager*Public sector*A_1	-0.37	-0.36	-0.348	-0.319
	[1.21]	[1.16]	[1.74]*	[1.57]
Manager*Public sector*A_2	0.159	0.088	-0.049	-0.038
	[0.34]	[0.19]	[0.15]	[0.12]
Manager*Public sector*A_3	-0.372	-0.372	0.106	0.135
	[1.13]	[1.12]	[0.40]	[0.50]
Clerk	0.364	0.369	0.223	0.223
	[4.45]***	[4.42]***	[3.04]***	[2.95]***
Observations	295561	280447	295561	280447
Tenure<= 1	Included	Not Included	Included	Not Included

Notes: LPM for the probability of experiencing the absence specified in the header. This specification is the same as the one presented in column 1 of Table 2, plus the full set of semester*year*Public sector interactions. See section 6 for details. Includes only white collar employees not employed in the army or manufacturing. The health care or education sector and those individuals absent from work for reasons outside their control are excluded (see section 3 for details on sample selection). Robust T statistics in squared brackets based on standard errors clustered at the individual level following White (1980). * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 10: **The causal effect of the 133/2008 law on public sector absenteeism: effects by education**

	*Coefficients*100*			
	Sickness absence		Other absence	
Column	1	2	3	4
Low edu*Public sector	0.752	0.764	-0.104	-0.1
	[7.90]***	[7.87]***	[1.45]	[1.37]
Low edu*Public sector*A_1	-0.776	-0.768	-0.032	-0.005
	[4.19]***	[4.07]***	[0.24]	[0.04]
Low edu*Public sector*A_2	-0.255	-0.315	0.128	0.112
	[0.97]	[1.18]	[0.64]	[0.55]
Low edu*Public sector*A_3	-0.734	-0.743	0.096	0.117
	[3.97]***	[3.97]***	[0.66]	[0.79]
High edu*Public sector	0.438	0.438	-0.106	-0.148
	[2.90]***	[2.79]***	[0.66]	[0.89]
High edu*Public sector*A_1	-0.261	-0.253	-0.076	-0.028
	[0.89]	[0.84]	[0.26]	[0.09]
High edu*Public sector*A_2	0.216	0.095	-0.409	-0.4
	[0.52]	[0.22]	[1.22]	[1.14]
High edu*Public sector*A_3	-0.331	-0.368	0.107	0.159
	[1.17]	[1.28]	[0.34]	[0.49]
High edu	-0.303	-0.278	0.341	0.367
	[4.70]***	[4.04]***	[3.80]***	[3.81]***
Observations	295561	280447	295561	280447
Tenure<= 1	Included	Not Included	Included	Not Included

Notes: LPM for the probability of experiencing the absence specified in the header. This specification is the same as the one presented in column 1 of Table 2, plus the full set of semester*year*Public sector interactions. See section 6 for details. Includes only white collar employees not employed in the army or manufacturing. The health care or education sector and those individuals absent from work for reasons outside their control are excluded (see section 3 for details on sample selection). Robust T statistics in squared brackets based on standard errors clustered at the individual level following White (1980). * significant at 10%; ** significant at 5%; *** significant at 1%.