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MONETARY INCENTIVES VS. MONITORING IN ADDRESSING ABSENTEEISM: EXPERIMENTAL EVIDENCE

by Francesco D'Amuri*

Abstract

Exploiting two unexpected variations in sickness absence policy for civil servants in Italy, this paper assesses the relative importance of monitoring and monetary incentives in determining a basic measure of effort: presence at work. When stricter monitoring was introduced together with an average 20% cut in replacement rates for civil servants on short sick leave, sickness absence decreased by 26.4%, eliminating the wedge in absence rates with comparable private sector workers. The impact substantially decreased when a subsequent policy change brought back monitoring to the pre-reform level, while leaving monetary incentives untouched. Results are confirmed by a variety of robustness checks and are not driven by the presence of attenuation bias. No shift is detected in other types of absence as a consequence of the reforms. 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.

JEL Classification: J32, J38, J45.
Keywords: monetary incentives, monitoring, effort, sickness absence.

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1 Introduction

Exploiting two unexpected variations in sickness absence policy for civil servants, this article assesses the relative importance of monetary disincentives vs. monitoring in addressing absenteeism.[1]

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). A number of articles have investigated the existence of a such a relationship exploiting between firms variation in employment relations (Cappelli and Chauvin, 1991; Groshen and Krueger, 1990; Rebitzer, 1995). They find a negative association between stricter supervision and monetary incentives, suggesting that the two are substitutes rather than complements in determining workers' effort. Nevertheless, the identifying variation employed is hardly exogenous (Prendergast, 1999), since the empirical analysis relies on equilibrium quantities resulting from firms' and workers' optimization problem. Imprecise measurement of monitoring intensity (commonly defined as the ratio of supervisory to non supervisory personnel) constitutes another potential threat to estimation. Both problems can hardly be overcome with observational data, and further work has tried to tackle them in a laboratory setting (Dittrich and Kocher, 2006), at the cost of a loss in results' generality. Recently, a growing literature has turned to analyse separately the effects of incentives and monitoring on workers' behavior,[2] while their relative importance remains an unexplored issue.

The goal of this paper is to fill this gap looking at a basic measure of effort, presence at work, and how this was affected by two subsequent variations in sickness absence policy

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[2]The positive impact of performance pay on productivity is widely documented (Bandiera et al., 2009; Gielen et al., 2010; Lazear, 2000; Lavy, 2009), while evidence on monitoring is more scarce. Nagin et al. (2002) show that a relevant fraction of call center operators shirk more when perceived monitoring levels decrease. Evidence is more mixed in laboratory studies: Dickinson and Villeval (2008) find a positive impact of monitoring on effort up to a certain threshold, above which motivation can be crowded out (Frey and Jegen, 2001).

entailing a changing mix of incentives and monitoring for Italian civil servants. 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,[3] a specific dimension of shirking. The incidence of such an opportunistic behavior depends on the worker's surplus at the current job (Barmby et al., 1994), on her outside options (Askilden et al., 2005; Kaivanto, 1997) and on the likelihood associated with the frequently absent worker being fired when caught, mainly determined by the monitoring level and 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). 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.[4] 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.[5] In 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 (on average 20% of total wage) for the first ten days of sickness absence.[6] 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. The strategy of the policy was thus twofold: increase the relative price of absence compared to going to work and enhance monitoring effectiveness. After exactly one year, the provision was

[3]For a review of the literature see Brown and Sessions (1996). Markussen et al. (2010) provide extensive evidence on the relevance of moral hazard issues in determining sickness absence levels.

[4]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.

[5]Author's calculations on Italian Labour Force Data, following the same definition of footnote 4; for details see section 4.

[6]Henrekson and Persson (2004); Johansson and Palme (2005, 2002); Ziebarth (2009), among others, analyse the impact of a change in replacement rates on the probability of reporting sick.

partially amended for six months, with monitoring time intervals reduced to the pre-reform period, while leaving unchanged sickness absence wage cuts. Compared to previous empirical literature on incentives' effectiveness, this experimental setting has two major advantages. The double unexpected variation in sickness absence policy provides clean evidence on the importance of incentives relative to monitoring in determining workers' effort, while 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 that year.

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 0.66 percentage points (-26.4%) on average, eliminating the wedge with the private services sector conditional on observables. The subsequent change in the policy mix sheds light on the relative importance of monitoring compared to wage cuts 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. Evidence survives a variety of robustness checks and is not driven by attenuation bias. Moreover, no shift is detected to other types of absence as a consequence of the reform. 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.

This article is organised as follows. Section 2 discusses the experimental setting, while a simple theoretical framework is introduced in section 3 to identify the drivers of sickness absence rates' changes. Sections 4 and 5 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 6, while section 7 compares main results with evidence obtained from different datasets and previous papers analysing the reform. Section 8 concludes.

2 Experimental setting

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 inspections, aimed at ascertaining their presence at home and their real health conditions.

In the public sector, the treatment group, workers were entitled to receive the full wage during sickness leave of any length before the reform at study was introduced.[7] 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 two subsequent changes, that will be used to identify the relative importance of monitoring and incentives in determining absence levels.

Phase 1 *(July 2008 - June 2009): monetary disincentives and increase in 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.[8] 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. 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. The strategy of the government was thus twofold: increase the relative price of

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

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

absence and enhance monitoring effectiveness.

Phase 2 *(July 2009 - December 2009): monetary disincentives only, monitoring at the 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).

Figure 1 provides evidence on the unexpectedness of the two policy changes, a crucial assumption underlying identification of its causal effects. The figure reports the incidence of queries for the keyword "sickness absence" and "sickness absence checks"[9] over total queries performed through the Google search engine,[10] with values normalized to 100 for the week in which the incidence was the highest. It is evident that the interest for the keywords reaches local maxima exactly in the weeks in which sickness insurance policy changed, starting phase 1 and 2 of the reform, a clear sign that the reforms were unanticipated. Another peak occurs at the end of October 2009, following anticipations made of a third phase (not analysed here), in which monitoring time intervals would have changed again.[11] The eventual effect this announcement had on sickness absence dynamics in the last quarter of 2009 will directly be tested in the empirical analysis.

The introduction of the new policy regarding approximately 3.5 million of workers in 2007 (RGS, 2008), or slightly more than 20 per cent of employees, and its partial amendment, provide an ideal experimental setting for evaluating the relative importance of monitoring and monetary disincentives in determining absence behaviour. The next section introduces a simple model, based on each worker utility maximization, clarifying the drivers of changes in absence rates determined by the different sick pay policies analised here.

[9]In Italian, "assenza per malattia" and "visite fiscali" respectively.

[10]This indicator is publicly available at http://www.google.com/insights/search/.

[11]Colombo, D. "Brunetta lancia l'allarme: torna l'assenteismo", *il Sole 24 Ore*, October 30, 2009.

3 Theoretical background

Conditional on actual health conditions, and assuming the participation constraint is always satisfied, the utility maximization problem for the risk-neutral worker is:

$$U(\tilde{s}|s) = s\{(1-\tilde{s})(w_0-\beta_0)+\tilde{s}(w_1-\beta_1)\}+(1-s)\{\tilde{s}(w_1+\gamma-pK-a)+(1-\tilde{s})w_0\} \quad (1)$$

where actual and reported health status are defined respectively by s, $\tilde{s} \in \{0,1\}$ (one if sick, zero otherwise). Actual health status (s) is assumed to be equal to one and zero respectively with probability x and $1-x$, while reported health status is determined by the maximization of equation 1 conditional on s. The term w_0 denotes daily wage paid when working and w_1 is income transfer for employees absent from work because of sickness, with $w_0 \geq w_1$. The terms β_0 and β_1 identify the utility loss related to sickness respectively when working or staying at home (with $\beta_0 > \beta_1$), γ is leisure utility when not working and not sick, p is monitoring effectiveness, that is the probability of detecting workers cheating over their health status, and K is the associated penalty. Finally, the term a identifies the psychological cost of cheating. The share of workers declaring to be sick will then be:

$$E(\tilde{s}) = Pr(s=1)\{Pr(w_1-\beta_1 > w_0-\beta_0)\}+(1-Pr(s=1))\{Pr(w_1+\gamma-pK-a) > w_0)\} \quad (2)$$

It is straightforward to see that, conditional on actual health conditions, the fraction of workers declaring to be sick is decreasing with the magnitude of the monetary disincentive for sickness absence, with monitoring effectiveness, with the cost sustained when opportunistic behaviour is detected and with the cheater's psychological cost. Before the reform at study (superscript B), the worker received the same payment irrespective of sickness absence ($w_1^B = w_0^B$), while p^B is monitoring effectiveness. During *phase one* of the reform (superscript A_1), sickness related payments were reduced ($w_1^{A_1} \leq w_0^{A_1} = w_0^B = w_1^B$) and monitoring effectiveness was increased ($p^{A_1} > p^B$). As a consequence, assuming the probability that a worker is actually sick remains constant across periods, the change in the share of workers declaring to be sick during the phase 1 of the reform will be:

$$E(\tilde{s}|A_1) - E(\tilde{s}|B) =$$
$$Pr(s=1)\{Pr(\beta_0 - \beta_1 > w_0^B - w_1^{A_1}) - Pr(\beta_0 - \beta_1 > 0)\} +$$
$$(1 - Pr(s=1))\{Pr(\gamma > w_0^B - w_1^{A_1} + p^{A_1}K + a) - Pr(\gamma > p^B K + a)\} \quad (3)$$

in which the first addend in the right hand side characterises the contribution of the increase in presenteeism due to the reform (i.e. sick workers going to work in order not to incur in the penalty $w_0^B - w_1^A > 0$ introduced by the new regulations) to the change in overall incidence of sickness absence. The second term characterises instead the contribution of the decrease in absenteeism among cheaters, due to both the wage penalty and the increase in monitoring effectiveness. Given the setting of the reform in *phase one*, it is then not possible to tell whether the eventual change in sickness absence is due to an increase in presenteeism or a decrease in opportunism. Nevertheless, in phase 2 (superscript A_2), monitoring effectiveness was reduced to the pre-reform level ($p^{A_2} = p^B$), while the payment for workers reporting sick remained intact during the whole post-reform period (superscript A, with $A = [A_1, A_2]$): $w_1^A = w_1^{A_1} = w_1^{A_2}$. As such, any change in the share of workers declaring to be sick between *phase 1* and *phase 2* will be driven by the variation in monitoring effectiveness, being an expression of the second term only of equation 3:

$$E(\tilde{s}|A_2) - E(\tilde{s}|A_1) =$$
$$(1 - Pr(s=1))\{Pr(\gamma > w_0^A - w_1^A + p^{A_2}K + a) - Pr(\gamma > w_0^A - w_1^A + p^{A_1}K + a)\}. \quad (4)$$

The relative size of absolute changes in average workers' sickness rates expressed in equations 3 and 4 provides a straightforward assessment of the relative importance of monitoring vs. monetary incentives in determining absence rates. Under the assumption that monitoring effectiveness is non-relevant for genuinely sick individuals, this formalization makes clear that, while changes taking place between the period B and A_1 could be driven both by an increase in presenteeism and a decrease in opportunism, variations between A_1 and A_2 can only be determined by opportunistic behaviour.

4 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 24 quarters of data are used, spanning the six year interval 2004:Q1-2009:Q4, with a total of more than 4 million observations. 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.[12] After this sample selection, 268,544 observations are left, or 25.2 per cent of the total number of employees in the sample.

Figure 2 shows sickness absence incidence for the 2004:Q1-2009:Q4 period, separately for the private services and the public sector. Public sector workers show constantly higher absence rates, while the seasonal pattern is similar in the two subgroups. The first and the second vertical lines identify the two subsequent changes in the civil servants' sickness absence insurance system introduced in section 2. Graphical evidence shows that the difference in absence rates between the public and the private sector seems to decrease during the *phase 1* of the reform, while increasing again thereafter.

Table 1 reports descriptive statistics for the private services sector *(control group)* and the public sector *(treatment group)*, for the period before (2004:Q1-2008:Q2) and after (2008:Q3-2009:Q4) 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% (3 to 4 percentage points higher in the public sector). The share of females 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 2%, still 0.7 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*

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

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)[13] 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 the civil servants having 0.6 percentage points higher probability of being absent from work in the reference week than otherwise observationally equivalent private sector white collar workers.[14] Column 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 Female*PUB*DR 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 summarized 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.

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

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

5 Identification

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

$$y_{it} = \alpha + \beta X_{it} + \gamma PUB_{it} + \lambda_1 PUB_{it}^{A_1} + \lambda_2 PUB_{it}^{A_2} + q_t + \varepsilon_{it} \tag{5}$$

where the binary variable y_{it}[15] is equal to one if individual i worked less than usual due to sickness during the reference week of quarter t and zero otherwise, q_t are quarter by year interactions, X_{it} is a vector of socio-demographic and job related controls including 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 quarter 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} and PUB^{A_2} are interactions between PUB and two dummy variables equal to one during phase 1 (2008:Q3-2009:Q2) and phase 2 (2009:Q3-2009:Q4) of the reform. As a consequence, coefficients $\lambda_{1,2}$ capture any systematic variation in absence rates taking place during phase 1 and 2 of the reform at study compared to pre-reform levels:

$$\begin{aligned} \lambda_x = E[y_{it}|PUB = 1, d_{A_x} = 1] - E[y_{it}|PUB = 1, d_B = 1] - \\ E[y_{it}|PUB = 0, d_{A_x} = 1] - E[y_{it}|PUB = 0, d_B = 1]; x = 1, 2 \end{aligned} \tag{6}$$

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 q_t, the treatment ($PUB = 1$) and the control ($PUB = 0$) group have a similar trend in sickness absence *before* the introduction of the new policy;

[15] See section 4 for details.

Condition 2. Conditional on the controls X_{it} and q_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.

5.1 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 the dummy 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 $PUB * q_t$ interactions for each of the 18 quarters 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 quarter differences in absence rates between the control and the treatment group is constant conditional on the controls and on the (common) quarter by year fixed effects. Note that this is also equivalent to a falsification exercise on the *pre-reform* period, given that systematic changes in the relative public/private sector absence rates in the *after reform* period will quantify the effects of the reform on average absence rates (section 6). 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 18 quarters. An F-test of all the interactions being jointly equal to zero does not reject the null (p value=0.556). Given that we cannot reject the hypothesis of a common trend in the control and the treatment group in the 18 quarters preceding introduction of the new provisions, for computational ease we will restrict the policy evaluation sample to the 6 quarters preceding the new law (2007:Q1-2008:Q2) and the 6 quarters following it (2008:Q3-2009:Q4).

5.2 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 services) 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:

$$y_{it|y_{i,t-4=0}} = \alpha + \beta X_{i,t-4} + \gamma SICKABS_{i,t-4} + \gamma_{A_1} SICKABS^{A_1}_{i,t-4} + \gamma_{A_2} SICKABS^{A_2}_{i,t-4} + q_t + \varepsilon_{it} \quad (7)$$

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 quarter by year dummies q_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 report sick in $t-4$. The variables of main interests here are $SICKABS^{A_1}_{i,t-4}$ and $SICKABS^{A_2}_{i,t-4}$, respectively the interaction between the dummy $SICKABS_{i,t-4}$ and the dummies d_{A_1} (equal to one during *phase 1* of the reform) and d_{A_2} (equal to one during *phase 2* of the reform). These two variables would detect any differential mobility pattern taking place during the two *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 ($t-4$) 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 do not have a significantly higher probability of changing sector or leaving employment in t, and no differential pattern is detected during the two phases of the reform at study. For workers employed in the private services sector in $t-4$, the probability of experiencing a transition to any other state is on average not significantly different for individuals who reported to be sick in $t-4$ (column 3), but it increases significantly by 7.4 and 9.4 percentage points respectively during *phase 1* and *phase 2* compared to the pre-reform period. A higher propensity to leave the private services sector (the control group) during the $t, t-4$ interval for individuals who reported to be sick in $t-4$ might introduce (if anything) a downward bias in the policy evaluation exercise if the propensity to be sick is assumed to be correlated over time. 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 the *phase 1* of the reform, the likelihood of experiencing this transition significantly increased by 0.5 percentage points for individuals who reported sickness absence in $t-4$. The coefficient increases to 0.95

percentage points during *phase 2*. 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 6.3. 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.

6 Results

6.1 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 5 on the full sample (column 1 of Table 5).

Conditional on observables, civil servants have 0.65 percentage points higher 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 *phase 1* of the reform, when monetary incentives were coupled with increased monitoring, sickness absence incidence decreased exactly by 0.66 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 -0.15 percentage points, statistically non significant 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. 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.[16] 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, 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. Nevertheless, they are compatible with the presence of *attenuation bias* in the reform effects, an eventuality that is explicitly addressed in section 6.2, while section 6.3 assesses the effects of another potential source of bias, the presence of *spillover* effects of the reform on the control group.

6.2 Attenuation bias

A potential explanation for the evidence presented in the previous section would be the presence of attenuation bias, assuming that the reform might have had a strong impact on sickness absence rates at its introduction, then decreasing over time. This could determine previous results, implying that the dynamics detected between *phase 1* and *phase 2* of the reform are not a genuine response to a change in the incentives' scheme, but merely the result of workers adjusting over time to the stricter policy. In order to detect this possibility, equation 5 is estimated with a slightly different specification. Instead of estimating the two step dummies interacting PUB with d_{A_1} and d_{A_2}, and capturing the average effect of the reform during the two subsequent phases of the reform, the PUB variable is interacted with a full set of quarter dummies for the whole evaluation period. Had the reforms' effect been fading over time, this should be evident in the estimates, slightly decreasing quarter after quarter. Results reported in column 1 of Table 6 show that this is not the case. According to coefficients estimated for the quarter by quarter interactions, the impact of the reform was negative (-0.3 percentage points in sickness absence incidence), but not significant, in the first quarter since its introduction. It then increased substantially in the second quarter (-0.61 percentage points, a result significant at the 10% level), to reach its maximum in the last two quarters of *phase 1*, when the decrease in absence rates, compared to the

[16] See section 4 for a definition of the variable.

pre-reform values, was equal to 0.85 and 0.86 percentage points, respectively significant at the 5 and 1% level. During *phase 2* of the reform, when monitoring was set back to the *pre-reform* period, the quarter by PUB interactions are no longer significant, and point estimates become lower in absolute value (-0.10 and -0.19 respectively in the first and the second quarter). A formal test of the effects of the reform being equal in the last quarter of *phase 1* and in the first quarter of *phase 2* rejects the null at the 5% confidence level. The drastic change in coefficient estimates between the last quarter of *phase 1* and the first one of *phase 2* suggests that the results are genuinely due to the change in the incentive scheme and not to reform's effects fading over time. These results rule out also the possibility of announcement effects driving the results. In the last quarter of *phase 2*, the announcement of stricter rules to take place in the following year (outside the evaluation period),[17] might have pushed workers to increase absence rates when monitoring was at a comparatively low levels. If this were the case, the decrease in workers' attendance found during *phase 2* could be due to these announcement effects and not to the change in the incentive system. Nevertheless, the coefficient estimate does not change significantly between the first and the second quarter of *phase 2*, providing evidence for the fact that the rebound in absence rates taking place when monitoring was reduced to *pre-reform* levels is not driven by workers anticipating future stricter rules.

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).

6.3 Spillovers

Finally, the eventual existence of spillovers, preventing correct identification, is taken into consideration.[18] 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

[17]See section 2. Figure 1 shows that the announcement was widely followed by the public.

[18]See condition 3 of section 5.

partner to the private sector one are *a priori* unclear and determined by three different elements:

- *between partner substitution in absence behavior*, determined for example by the necessity of staying at home for taking care of Dependent Relatives. The increase in relative price of absence for the 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;
- *between partner complementarities in absence behavior*, 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 5.

In order to check the robustness of the section 6.1 results to this kind of bias, equation 5 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 0.77 percentage points higher in the public than in the private services sector (column 1 of Table 7). During *phase 1* of the reform, this difference is eliminated with a 0.78 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 decreased by only 0.26 percentage points compared to the pre-reform period, a result that is statistically non-significant. 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. Results are confirmed when dropping from the sample all workers with tenure shorter than a year (column 2). Finally, we do not find any significant variation in absences for reasons other than sickness during the reform period (columns 3 and 4).

7 External validation

Results of the econometric analysis entail strong reform effects, providing clean evidence for the fact that, at least for Italian civil servants, the main determinant of presence at work is monitoring rather than monetary incentives. 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,[19] 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. Administrative data on their own employees collected by the Social Security Agencies and the Fiscal Agencies,[20] subsectors of the Public Administration employing around 30 thousand people each, convey a similar picture.

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 data (30 thousand employees). They find a 20 per cent decrease in 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.

[19]Ceci and Giungato (2010).
[20]See Fioravanti et al. (2010) and Dongiovanni and Pisani (2010).

8 Conclusions

This paper provides evidence on the relative importance of monitoring vs. monetary incentives in determining a basic measure of workers' effort: presence at work.

Italian civil servants have higher sickness absence rates compared to private sector workers with comparable characteristics. In an effort to reduce this wedge, a reform took place in June 2008, lowering by an average 20% sickness insurance replacement rates for the first ten days of continuous absence and intensifying monitoring on individuals reporting sick. After exactly one year, a partial amendment to the reform brought monitoring levels back to the pre-reform setting, while the cut in replacement rates remained unchanged. The two subsequent and unanticipated changes in sick insurance policy concerning about 20 per cent total employees provide a clear experimental setting. According to our (conservative) estimates, based on a regression differences in differences approach with the private services sector as the control group, absence rates dropped on average by 26.4 per cent when the reform introduced both monetary disincentives and stricter monitoring. This drop was reversed when, a year later, monitoring levels were reduced to the pre-reform level, suggesting this last element of the reform was the effective one in addressing absence. These findings are not driven by attenuation bias and survive a variety of robustness checks, while no shift to other types of absence as a consequence of the reforms is detected.

These results are relevant for the literature on incentives. Previous studies focussing on the relative importance of monitoring and incentives in determining workers' effort are limited either by endogeneity, when based on equilibrium quantities resulting from workers' and firms' optimization problems, or by a lack of generality, when exploiting results of laboratory experiments. Since US and Western European civil servants seem to share with their Italian colleagues a higher propensity to report sick compared to the private sector, this policy evaluation exercise provides insights on how to draw a successful strategy for reducing absenteeism.

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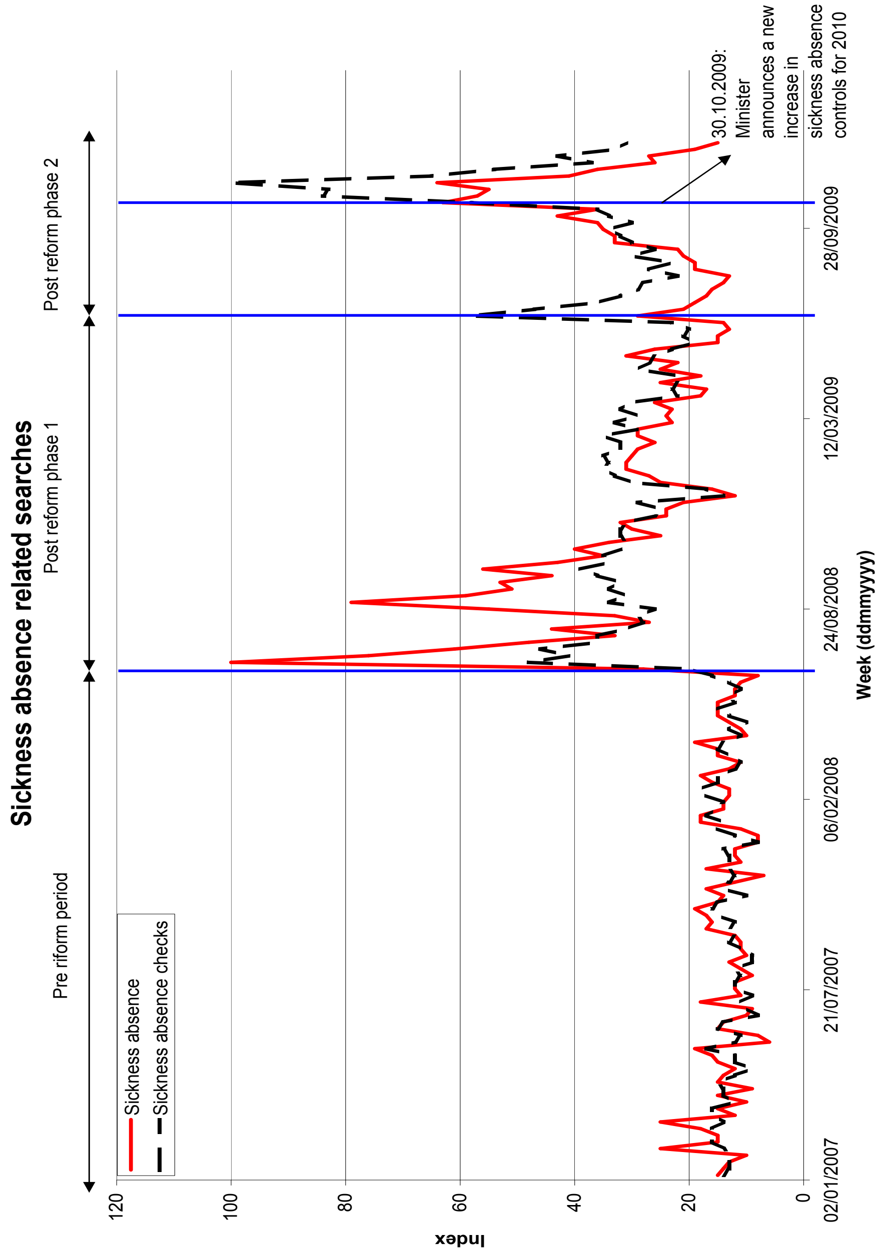
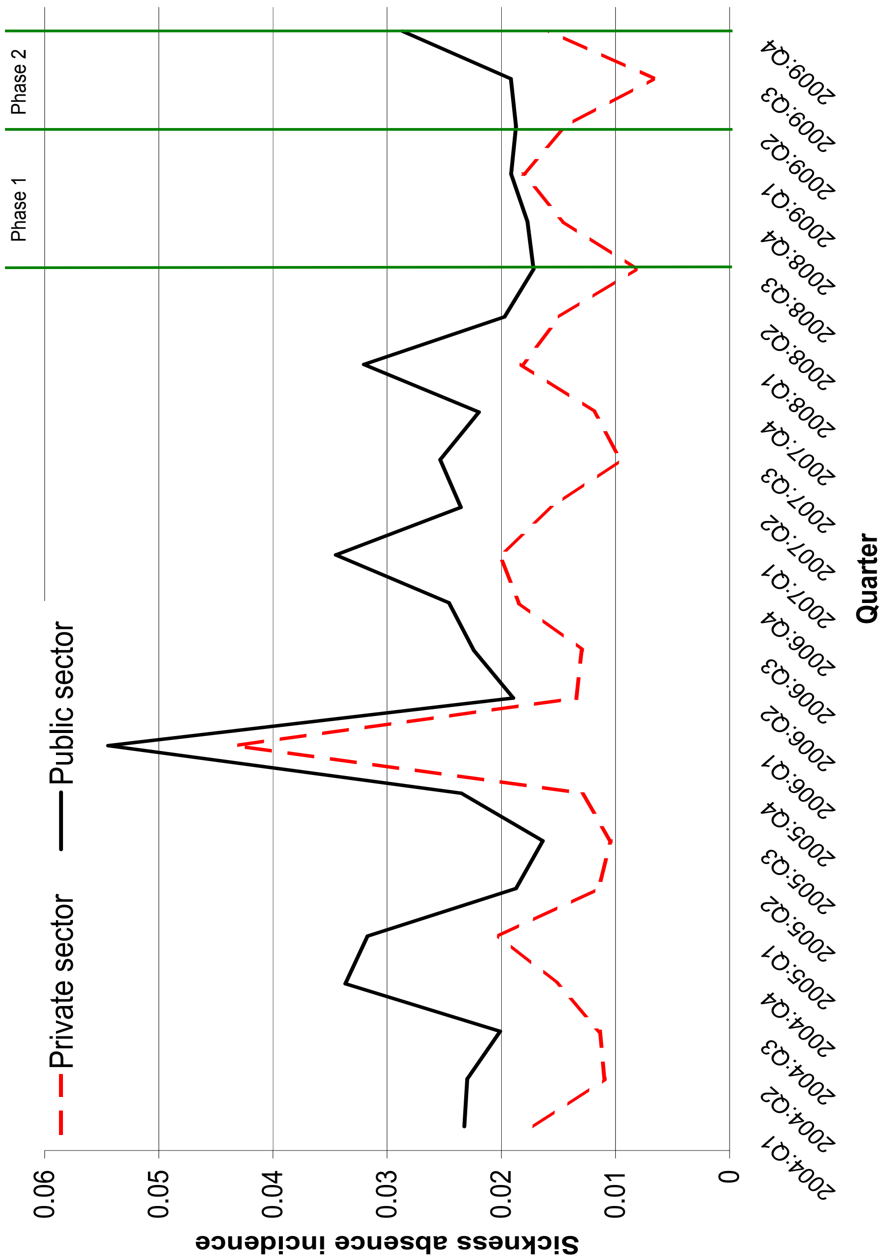
Figure 1: **Incidence of sickness absence related Google searches**



Note: The figure reports the incidence of queries for the keywords "sickness absence" and "sickness absence checks" (In Italian, "assenza per malattia" and "visite di controllo", respectively) over total queries performed in Italy through the Google search engine. Values are normalized to 100 for the week in which the incidence was the highest (Google data available at http://www.google.com/insights/search/.). The first vertical line identifies the week in which the *Phase 1* of new sickness absence policy was introduced, the second line identifies introduction of *Phase 2* of the policy, while the third line marks the week in which the government announced a new change in sickness absence policy to be introduced in 2010 (not in the time interval analysed here).

Figure 2: **Sickness absence rates**



Note: Author's calculations on Istat, Labour Force Survey. The figure reports average sickness absence rates in the public and the private services sectors. The first and the second green vertical lines identify respectively the quarter of introduction of the new sickness absence policy (2008:Q3) and of its partial amendment (2009:Q3).

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

	Private pre-reform				Private post-reform				Public pre-reform				Public post-reform			
Variable	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Woman	0.51		0	1	0.52		0	1	0.42		0	1	0.42		0	1
High education	0.19		0	1	0.22		0	1	0.22		0	1	0.25		0	1
North	0.56		0	1	0.56		0	1	0.35		0	1	0.38		0	1
Center	0.22		0	1	0.23		0	1	0.26		0	1	0.25		0	1
South	0.22		0	1	0.22		0	1	0.39		0	1	0.38		0	1
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	0.33				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				0.17			
Tenure	10.53	9.51	0	47	10.66	9.69	0	48	16.53	9.68	0	44	17.74	10.02	0	46
Temp	0.09		0	1	0.10		0	1	0.07		0	1	0.06		0	1
Firm size (x)																
$x <= 10$	0.36		0	1	0.34		0	1	0.08		0	1	0.07		0	1
$11 <= x <= 15$	0.10		0	1	0.11		0	1	0.05		0	1	0.05		0	1
$16 <= x <= 19$	0.04		0	1	0.06		0	1	0.03		0	1	0.04		0	1
$20 <= x <= 49$	0.15		0	1	0.16		0	1	0.18		0	1	0.20		0	1
$50 <= x <= 249$	0.18		0	1	0.18		0	1	0.36		0	1	0.39		0	1
$x >= 250$	0.12		0	1	0.12		0	1	0.23		0	1	0.22		0	1
$x < 10$	0.05		0	1	0.03		0	1	0.06		0	1	0.03		0	1
Sick. Abs.	0.016		0	1	0.013	0	0	1	0.026		0	1	0.021		0	1
Other Abs.	0.019		0	1	0.017		0	1	0.012		0	1	0.009		0	1
Obs.	146483				48290				56380				17391			

Notes: Author's calculations on ILFS data. Weighted values. The pre-reform period is 2004:Q1-2008:Q2; the post reform period is 2008:Q3-2009:Q4. DR stands for Dependent Relative(s). 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.

Table 2: **LPM for the incidence of absence, pre-reform period (2004:Q1-2008:Q2)**

Column	1	2
PUB	0.631	
	[7.11]***	
DR	0.354	
	[3.84]***	
Contractual hours	-0.073	-0.07
	[2.57]**	[2.48]**
Contractual hours^2/100	0.072	0.07
	[2.09]**	[2.04]**
High education	-0.406	-0.34
	[5.14]***	[3.98]***
Woman	0.577	0.515
	[8.05]***	[6.35]***
Married	-0.16	-0.16
	[1.85]*	[1.85]*
Age 25-34	0.304	0.281
	[2.75]***	[2.53]**
Age 35-44	0.459	0.464
	[3.50]***	[3.53]***
Age 45-54	0.649	0.654
	[4.38]***	[4.41]***
Age 55-64	0.969	0.966
	[4.95]***	[4.95]***
Center	0.507	0.51
	[5.24]***	[5.26]***
South	0.172	0.175
	[2.34]**	[2.38]**
Part time	-0.612	-0.642
	[3.46]***	[3.62]***
Temps	-0.219	-0.222
	[2.11]**	[2.14]**
Tenure	0.023	0.023
	[1.80]*	[1.79]*
Tenure^2/100	0.003	0.002
	[0.07]	[0.05]
11 to 15 employees	0.335	0.335
	[3.04]***	[3.05]***
16 to 19 employees	0.456	0.457
	[2.81]***	[2.82]***
20 to 49 employees	0.461	0.462
	[4.74]***	[4.75]***
50 to 249 employees	0.581	0.584
	[6.19]***	[6.21]***
250 or more employees	0.797	0.801
	[6.61]***	[6.64]***
10 or more employees[a]	-0.383	-0.378
	[3.47]***	[3.43]***
Man*PUB		0.64
		[4.99]***
Woman*PUB		0.613
		[4.04]***
High edu*PUB		-0.259
		[1.42]
Man*DR		0.01
		[0.09]
Man*DR*PUB		0.369
		[1.42]
Woman*DR		0.582
		[4.06]***
Woman*DR*PUB		0.087
		[0.25]
Second job		0.415
		[1.29]
Second job*PUB		0.658
		[0.94]
Constant	1.646	1.604
	[2.67]***	[2.59]***
Observations	202863	202863

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). 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 4 for details on sample selection). Includes a full set of quarter 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**

Column	1	2
PUB*Trend	-0.001	
	[0.02]	
PUB*Trend^2/100	-0.001	
	[0.16]	
PUB*2004:Q2		0.114
		[0.28]
PUB*2004:Q3		-0.142
		[0.34]
PUB*2004:Q4		0.304
		[0.68]
PUB*2005:Q1		-0.028
		[0.06]
PUB*2005:Q2		-0.307
		[0.76]
PUB*2005:Q3		0.046
		[0.11]
PUB*2005:Q4		-0.286
		[0.68]
PUB*2006:Q1		0.117
		[0.21]
PUB*2006:Q2		-0.478
		[1.16]
PUB*2006:Q3		-0.115
		[0.27]
PUB*2006:Q4		-0.212
		[0.47]
PUB*2007:Q1		0.469
		[0.99]
PUB*2007:Q2		-0.132
		[0.30]
PUB*2007:Q3		0.194
		[0.45]
PUB*2007:Q4		-0.24
		[0.56]
PUB*2008:Q1		0.142
		[0.30]
PUB*2008:Q2		-0.68
		[1.63]
F test: all int.=0		
Pvalue		0.5563
Observations	202863	202863

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 PUB and a linear/quadratic trend and a full quarter by PUB 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 4 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**

	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	3.495	-0.256	1.455	-0.54
	[1.61]	[2.31]**	[0.86]	[2.20]**
$SICKABS_{A_1}$	1.358	0.517	7.384	0.088
	[0.41]	[1.96]**	[2.28]**	[0.23]
$SICKABS_{A_2}$	-3.121	0.954	9.46	0.313
	[0.82]	[2.02]**	[1.89]*	[0.63]
Woman	-0.403	0.004	0.932	0.03
	[0.85]	[0.10]	[2.45]**	[0.35]
Center	-0.678	0.143	0.56	0.012
	[1.11]	[2.85]***	[1.18]	[0.10]
South	-0.889	0.158	1.847	-0.448
	[1.84]*	[3.87]***	[4.28]***	[5.61]***
Contractual hours	-0.153		-0.515	
	[0.58]		[3.82]***	
Contrac. hours^2/100	0.001		0.005	
	[0.53]		[3.63]***	
High education	0.599	0.297	0.545	1.039
	[1.05]	[4.63]***	[1.14]	[8.53]***
Tenure	-1.191		-0.696	
	[10.10]***		[9.32]***	
Tenure^2/100	3.211		1.868	
	[9.66]***		[7.96]***	
Temp	4.608	0.045	6.796	0.164
	[3.61]***	[0.90]	[7.69]***	[1.27]
Part time	3.006	0.001	-1.235	0.807
	[1.91]*	[0.03]	[1.32]	[4.68]***
Observations	18368	187149	39422	132375

Notes: LPM for the probability of experiencing the transition specified in the header. Includes a constant and additional quarter by year fixed effects and firm size dummies. 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 4 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**

	Sickness absence		Other absence	
Column	1	2	3	4
PUB	0.651	0.657	-0.255	-0.261
	[4.51]***	[4.46]***	[2.41]**	[2.42]**
PUB*A_1	-0.662	-0.66	0.068	0.09
	[3.34]***	[3.26]***	[0.46]	[0.59]
PUB*A_2	-0.146	-0.23	0.131	0.119
	[0.57]	[0.88]	[0.66]	[0.57]
DR	0.642	0.671	4.484	4.646
	[5.80]***	[5.85]***	[26.92]***	[26.87]***
Part time	-0.491	-0.496	-0.615	-0.664
	[2.52]**	[2.43]**	[2.87]***	[2.91]***
Temp	-0.154	-0.081	-0.888	-0.842
	[1.34]	[0.59]	[7.79]***	[6.04]***
Woman	0.511	0.51	2.43	2.535
	[6.32]***	[6.01]***	[29.72]***	[29.39]***
High edu	-0.25	-0.229	0.208	0.215
	[2.76]***	[2.40]**	[2.01]**	[1.97]**
Tenure	0.047	0.047	0.048	0.02
	[3.31]***	[3.02]***	[3.56]***	[1.36]
$\text{Tenure}^2/100$	-0.1	-0.099	-0.135	-0.058
	[2.26]**	[2.12]**	[3.96]***	[1.57]
Constant	2.515	2.411	-0.464	-0.476
	[3.41]***	[3.13]***	[0.78]	[0.72]
Observations	133521	126623	133521	126623
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
Quarter*Year interactions	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. 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 4 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: quarterly interactions**

	Sickness absence		Other absence	
Column	1	2	3	4
PUB	0.652	0.657	-0.254	-0.261
	[4.51]***	[4.46]***	[2.40]**	[2.41]**
PUB*2008:Q3 (A_1)	-0.326	-0.32	0.19	0.186
	[1.16]	[1.10]	[0.81]	[0.76]
PUB*2008:Q4 (A_1)	-0.608	-0.594	0.083	0.104
	[1.91]*	[1.81]*	[0.35]	[0.42]
PUB*2009:Q1 (A_1)	-0.852	-0.872	0.198	0.238
	[2.54]**	[2.57]**	[0.87]	[1.03]
PUB*2009:Q2 (A_1)	-0.861	-0.838	-0.198	-0.17
	[2.84]***	[2.72]***	[0.94]	[0.79]
PUB*2009:Q3 (A_2)	-0.101	-0.108	0.061	0.025
	[0.32]	[0.34]	[0.24]	[0.10]
PUB*2009:Q4 (A_2)	-0.188	-0.346	0.197	0.207
	[0.55]	[0.99]	[0.82]	[0.83]
DR	0.641	0.669	4.484	4.647
	[5.79]***	[5.84]***	[26.92]***	[26.87]***
Part time	-0.491	-0.495	-0.614	-0.663
	[2.52]**	[2.43]**	[2.87]***	[2.91]***
Temp	-0.154	-0.083	-0.888	-0.843
	[1.35]	[0.60]	[7.79]***	[6.05]***
Woman	0.512	0.51	2.43	2.535
	[6.32]***	[6.02]***	[29.72]***	[29.39]***
High edu	-0.25	-0.229	0.207	0.214
	[2.76]***	[2.40]**	[2.01]**	[1.96]**
Tenure	0.047	0.046	0.048	0.02
	[3.29]***	[3.02]***	[3.56]***	[1.36]
$Tenure^2/100$	-0.1	-0.099	-0.134	-0.058
	[2.25]**	[2.11]**	[3.95]***	[1.57]
Constant	2.513	2.407	-0.465	-0.478
	[3.41]***	[3.12]***	[0.78]	[0.72]
Observations	133521	126623	133521	126623
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
Quarter*Year interactions	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. 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 4 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: indirect test for spillover effects**

	Sickness absence		Other absence	
Column	1	2	3	4
PUB	0.769	0.777	-0.212	-0.216
	[4.98]***	[4.93]***	[2.00]**	[1.99]**
PUB*A_1	-0.777	-0.777	0.022	0.03
	[3.65]***	[3.58]***	[0.15]	[0.20]
PUB*A_2	-0.26	-0.333	0.06	0.042
	[0.96]	[1.20]	[0.30]	[0.20]
DR	0.619	0.645	4.464	4.635
	[5.37]***	[5.41]***	[25.75]***	[25.72]***
Part	-0.411	-0.407	-0.533	-0.582
	[2.03]**	[1.92]*	[2.41]**	[2.46]**
Temp	-0.161	-0.096	-0.782	-0.732
	[1.40]	[0.69]	[6.87]***	[5.22]***
Woman	0.509	0.507	2.391	2.501
	[6.11]***	[5.80]***	[28.38]***	[28.07]***
High edu	-0.263	-0.242	0.156	0.165
	[2.83]***	[2.48]**	[1.49]	[1.50]
Tenure	0.055	0.058	0.055	0.028
	[3.82]***	[3.70]***	[3.97]***	[1.82]*
$\text{Tenure}^2/100$	-0.127	-0.134	-0.148	-0.075
	[2.80]***	[2.78]***	[4.29]***	[1.97]**
Constant	2.341	2.21	-0.649	-0.663
	[3.11]***	[2.81]***	[1.07]	[0.98]
Observations	125634	118931	125634	118931
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
Quarter*Year interactions	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. 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 4 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%.

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