Monitoring vs. monetary incentives in addressing absenteeism: experimental evidence

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Abstract

Exploiting two unexpected variations in sickness absence policy, this article assesses the relative importance of monetary disincentives vs. monitoring in determining absence levels. According to the evidence, monitoring has a crucial role in reducing absence rates, while monetary incentives are relatively less effective, in particular for men.

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1 Introduction

In the fourth quarter of 2007, 2.8 per cent of public sector workers in the United States reported to have worked less than usual because of illness, an incidence 41.2 higher than in the private services sector. In Western Europe, this difference was equal to 20.2 per cent.[1]

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.[2] In the spirit of Shapiro and Stiglitz (1984), the expected economic loss of such an opportunistic behavior depends on the worker's surplus at the current job (Barmby et al., 1994), on the business cycle (Askilden et al., 2005; Kaivanto, 1997) and on the likelihood associated with the frequently absent worker being fired when caught, mainly determined by the degree of employment protection legislation 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 usually enjoy open ended contracts, less exposure to market forces and a higher level of effective Employment Protection Legislation compared to their private sector peers.

Italy is no exception, and in the period 2004Q1-2008Q2, sickness absence incidence was 62.5 per cent higher in the public (2.6 per cent) than in the private services sector.[3] At the end of June 2008, the just installed Italian government established a new, more restrictive, sick pay policy for civil servants, which stayed in place for a full year (2008Q3-2009Q2). The new provision 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. At the same time, the law increased monitoring effectiveness, changing from 4 to 11 hours the time interval in which the worker reporting to be sick had to be at home in order to be able to receive physicians' control visits. The strategy of the government was thus twofold: increase the relative price of absence and enhance monitoring effectiveness. After exactly one year, the norm was partially amended for six months (2009Q3-2009Q4), with the time interval for controls going back to the pre-reform period, while wage cuts for sickness absence remained unchanged.

The literature on effort and incentives broadly refers to "rewards and punishments" and their effects on motivation (Benabou and Tirole, 2003; Frey and Jegen, 2001), warning that incentives might actually backfire reducing intrinsic motivation. Applied work focusses either on the role

[1] 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 out of their will (labor dispute, bad weather, technical reasons, reduced activity) are not included.

[2] For a review of the literature see Brown and Sessions (1996). Skogman Thoursie (2004) provides evidence of a significant increase in the number of men relative to women reporting sick during a popular sport event.

[3] Author's calculation on Italian Labour Force Data, for details see section 2.

of monitoring (Dickinson and Villeval, 2008; Falk and Kosfeld, 2006; Nagin et al., 2002) or price incentives (Prendergast, 1999) in determining workers' effort. In the case of workers' attendance, most articles have analysed the impact of a variation in the sickness insurance replacement level on absence rates (Henrekson and Persson, 2004; Johansson and Palme, 2005, 2002; Ziebarth, 2009). The two subsequent unanticipated variations in sickness absence policy at study provide a clean and unprecedented experimental setting for evaluating the relative importance of monitoring vs. monetary incentives in determining workers' attendance.

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 (excluding education and health care)[4] fell by 24.7 per cent on average, eliminating the wedge with the private services sector conditional on observables. No shift to other types of absence as a consequence of the reform is detected. In principle, the sudden drop in absence rates could be due to a decrease in opportunism (non sick individuals reporting sick) or an increase in presenteeism (sick workers going to work not to incur in the wage cuts). The subsequent change in the policy mix sheds light on this aspect. When time intervals for monitoring were reset on the pre-reform level, so did sickness absence rates, meaning that stricter monitoring is relatively more important than monetary incentives in determining workers' attendance. Assuming that an enhance in monitoring is irrelevant to genuinely sick individuals, this result provides evidence in support of the idea that opportunism rather than presenteeism is driving changes in absence behaviour. Gender based analysis reveals additional patterns. Average effects are driven by men, the most responsive to monitoring levels. On the contrary, wage cuts seem to be relevant for women, a result in line with their greater labour supply elasticity.

This article is organized as follows. Section 2 introduces the dataset and data refinements, while section 3 discusses the experimental setting and the identification strategy underlying the estimation of the causal effects of the reform in changing absence behavior in the public sector. Main results are reported in section 4, together with a number of robustness checks, while section 5 concludes.

[4]It is not possible to distinguish people working in public/private schools and hospitals in this dataset, see section 2 for details.

2 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 five year interval 2004Q1-2009Q4, 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 developments in Italy. Two questions are used for constructing the variables analyzed in this article, 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.[5] 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 out of his 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 out of his 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.[6] After this sample selection, 273,207 observations are left, or 25.2 per cent of the total number of employees in the sample.

Figure 1 shows sickness absence incidence for the 2004Q1-2009Q4 period, separately for the private services and the public sector. Public sector workers show a constantly higher incidence in

[5] The other possible answers 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.

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

absence rates, while the seasonal pattern is similar in the two subgroups. The first and the second vertical lines identify subsequent changes in the sickness absence insurance system.[7]

Table 1 reports descriptive statistics for the private services sector *(control group)* and the public sector *(treatment group)*, for the period before (2004Q1-2008Q2) and after (2008Q3-2009Q4) 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 intermediate education being the mode (more than 60%) and a share of individuals with high education 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 41%). This might be 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 behaviour, table 2 shows the results of a logit 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 odds of being absent are 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 Not Self Sufficient Relative (NSSR) 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 into contracts requiring long 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 32.7% higher probability of being absent from work in the reference week than otherwise observationally equivalent private sector white collar workers.[8] Column 2 shows the results of an additional check, in order to test whether

[7] The particularly high incidence of sickness absence in winter 2005 could be due to the bird flu season.

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

this higher propensity to report sick for civil servants can be explained by the contribution of particular subgroups of workers. In particular, the model includes a set of interactions between gender and presence of a NSSR in the family, a control for workers having a second job and higher level interactions of these controls with the PUB dummy. There is also an interaction of PUB with the educational level. Men and women have a similarly higher propensity to report sick when employed in the public sector. Presence of a NSSR 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*NSSR interactions, this average effect is not different for public sector females. The same applies when 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 particular subgroups of workers, but can be summarized by a higher intercept for both men and women, with the difference between the two being statistically non significant at standard confidence levels. As a consequence, the focus of the rest of the analysis will be on average rather than group specific effects.

In the next section the experimental setting introduced by the reform at study will be discussed together with the identification issues faced when evaluating its impact on absenteeism.

3 Experimental setting

During the period analyzed 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 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 12am and 5 to 7 pm) in order to eventually receive medical inspections, aimed at ascertaining their real health conditions.

In the public sector, the treatment group, workers were fully insured against sickness absence

compositional 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.

before the reform at study was introduced.[9] 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.

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, sick pay policy, which stayed in place for a full year.[10] The new provision established that, for the first ten days of absence, the worker on sick leave receives the base salary only, thus losing any allowance or bonus (on average, 20% of total wage). 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 has to be at home in order to be able to receive medical inspection from 4 to 11 hours. The strategy of the government was thus twofold: increase the relative price of 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 12am and 5 to 7pm).

Figure 2 provides evidence on the unexpectedness of two changes in the policy, a crucial assumption underlying identification of its causal effects. The figure reports the incidence of queries for the keyword "sickness absence" and "sickness absence controls"[11] over total queries performed through the Google search engine,[12] 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.[13] The eventual effect this announcement had on sickness absence dynamics in the last quarter of 2009 will be indirectly 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.

[9] Slightly different arrangements could be present in subsectors.
[10] Decree No. 112 of June 25th, 2008; converted in Law 133/2008 the 6th of August, 2008.
[11] In italian, "assenza per malattia" and "visite fiscali" respectively.
[12] This indicator is publicly available at http://www.google.com/insights/search/.
[13] Colombo, D. "Brunetta lancia l'allarme: torna l'assenteismo", *il Sole 24 Ore*, October 30, 2009.

3.1 Theoretical background

Actual and reported health status are defined respectively by s,$\tilde{s} \in \{0,1\}$ (one if sick, zero otherwise), while w_0 denotes daily wage paid when working and w_1 is income transfer for employees absent from work because of sickness, with $w_0 \geq w_1$. 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 β_0 and β_1 identify the cost of sickness respectively when working or staying at home, 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, monitoring effectiveness, the cost sustained when opportunistic behaviour is detected and the cheater's psychological cost. Before the reform at study, $w_1^B = w_0^B$, that is, the worker receives the same payment irrespective of sickness absence and p^B is monitoring effectiveness. In phase one, the reform reduced sickness related payments ($w_1^A \leq w_0^A = w_0^B = w_1^B$) and increased monitoring effectiveness ($p^{A_1} > p^B$). As a consequence, assuming that actual probability for a worker to be sick remains constant, the change in the share of workers declaring to be sick during the phase 1 of the reform will be:

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

in which the first term of the expression characterizes 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 $w_0^A - w_1^A > 0$ introduced by the new norms) to the change in overall incidence of sickness absence.

The second term characterizes instead the contribution of the decrease in opportunistic behaviour 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, monitoring effectiveness was reduced to p^B, the pre-reform level, while the wage penalty remained intact. As such, the change in the share of workers declaring to be sick between phase 1 and phase 2 will only be an expression of the second term of equation 3:

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

providing clean evidence on the importance of opportunistic behaviour in determining sickness absence. At the same time, a significant change in workers' behaviour between *phase 1* and *phase 2* of the reform, provides an assessment of the relative importance of monitoring vs. monetary disincentives in determining absence rates.

3.2 Identification

We will evaluate the effects of the reform using a Regression Differences in Differences (RDID) approach. In particular, we will estimate:

$$y_{it} = \alpha + \beta X_{it} + \gamma PUB_{it} + \lambda PUB_{it}^{133/2008} + \lambda_2 PUB_{it}^{133/2008_2} + q_t + \varepsilon_{it} \quad (5)$$

where the binary variable y_{it}[14] is equal to one if individual i worked less than usual due to sickness during the reference week of quarter t, q_t are quarter dummies, X_{it} is a vector of socio-demographic and job related controls including age, education, marital status, presence of NSSR in the household, working region, tenure, type of contract, contractual hours 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 dummy $PUB^{133/2008}$ is the interaction between PUB and a dummy variable ($\mathrm{d}_{133/2008}$) equal to one when the new policy was enforced (2008Q3-2009Q4). Its coefficient λ is supposed to

[14] See section 2 for details.

capture the average causal effect of *phase 1* policy on the change on sickness absence

$$\lambda = E[y_{it}|PUB = 1, d_{133/2008} = 1] - E[y_{it}|PUB = 1, d_{133/2008} = 0] - E[y_{it}|PUB = 0, d_{133/2008} = 1] - E[y_{it}|PUB = 0, d_{133/2008} = 0] \quad (6)$$

expressed by equation 3. The coefficient of an additional dummy equal to one for public sector workers in *phase 2* of the reform (λ_2), identifies any differential impact of the changes to sickness policy introduced in summer 2009 (equation 4).

In order to address the eventual downward bias in the standard errors due to within individual correlation, 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;

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 spell in a systematic way;

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

In order to test empirically *Condition 1* a regression is run similar to the one reported in table 2, but adding a linear and a quadratic trend interacted with the dummy PUB. This 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 being not statistically 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 the 18 quarters prior 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 quarter differences in absence behavior between the control and the treatment group are constant conditional on the controls and on the (common) quarter 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 after the reform will provide the empirical evaluation of the relevance of its effects (section 4). The estimated values for the interactions reported in table 3 show that the hypothesis of the presence of a common trend in absence rates cannot be

rejected, with parameter estimates never statistically different from zero, in all but one (2006Q1) of 18 quarters, significant only at the 10 per cent level. Note that such an event falls within the probability of a type 1 error. An F-test of all the interactions being jointly equal to zero does not reject the null ($pvalue = 0.144$). 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, we will restrict the policy evaluation sample to the 6 quarters preceding the new law (2007Q1-2008Q2) and the 6 quarters following it (2008Q3-2009Q4).

Condition 2 will now be tested detecting the possibility of sorting effects across sectors and labour market states triggered by the reform.

The longitudinal dimension of the dataset at hand is exploited to provide an assessment, restricting the analysis to individuals who have been interviewed at least twice, at one year intervals (75 per cent of the whole sample). For these individuals, sector of employment in *t-4* together with eventual sickness absence in the same period is observed. Conditional on being employed in the public (private services) sector in $t-4$, a logit is estimated for the probability for leaving that sector to any other labour market status or any other sector during the $[t-4, t]$ interval:

$$y_{it|y_{i,t-4=0}} = \alpha + \beta X_{it-4} + \gamma SICKABS_{it-4} + \gamma_{133/2008} SICKABS_{it-4}^{133/2008} + q_t + \varepsilon_{it} \qquad (7)$$

where $y_{it} = 0$ if the individual was employed in the public (private services) sector in $t-4$ and is still in the same sector in t, while it is equal to one if the individual left that sector to any other labour market status or sector; X_{it-4} are usual socio-demographic and job related characteristics and q_t are quarter dummies. Econometric results show that the probabilities of experiencing this type of transition are higher for part-time and temporary workers, while decrease with tenure. The coefficients of interest are here γ and $\gamma_{133/2008}$. The former detects any impact of experiencing a sickness absence spell in $t-4$ on the probability of leaving the $t-4$ sector, while the latter identifies any departure from this average effect during the period in which the 133/2008 was in force. A significant coefficient for this variable would entail a systematic change in the probability of changing sector or labour market status for workers more exposed to sickness absence during the period in which the reform at study was in place. On average, workers who report to have worked less than usual in $t-4$ because of sickness have indeed a higher probability of changing sector or leaving employment, both in the public and in the private sector, but no significant change of this pattern is found during the period in which law 133/2008 was introduced. This means that, conditional on the controls, the law did not alter the composition of workers of the control and the treatment group in terms of propensity of experiencing sickness absence.

The effects of eventual departures from *Condition 3* will be assessed in section 4.

As a final caveat, it is likely that the total incidence of sickness absence is affected by underreporting of short sickness spells, given that the data have low frequency. Nevertheless, there is no reason to expect that the extent of underreporting is systematically different between the control and the treatment group, or in the pre/post reform period. This is confirmed when looking at the relative incidences of short to long spells in the two groups (table 5), with 48.96 (50.52) short spells for 100 long spells in the private (public) sector. If anything, since the wage penalty introduced by the new policy is the highest for absence spells below 10 days, the presence of underreporting of short spells is expected to introduce a downward bias in the policy evaluation exercise.

4 Results

4.1 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 (table 6).

Two different specifications are adopted: specification 1 including the full sample as defined in section 2, spec. 2 excluding individuals with tenure shorter than a year. The exclusion of workers with tenure shorter than a year 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.
The coefficient attracted by the variable PUB*133/2008, identifying the average effect of the reform in its *phase 1* setting, is negative, significant at 1% level and consistent across the two specifications, ranging between -0.289 and -0.288, implying a strong decline in sickness absence in public sector workers when monetary incentives were coupled with increased monitoring. On the contrary, during *phase 2* of the reform, in which only monetary incentives were in place, there was a significant rebound in absence rates, providing evidence of stronger effects of monitoring compared to monetary incentives in determining workers' attendance. A similar set of regressions, having absence for reasons other than sickness as the dependent variable, does not show any significant spill-over effect between absences of different typology.

As an additional robustness check, we test for the possibility of the reform having declining effects over time. This would justify previous results, implying that dynamics detected between *phase 1* and *phase 2* of the reform are not a genuine response to a change in the incentive scheme, but merely the result of workers adjusting over time to the stricter policy. In order to detect this possibility, we drop data relative to the first two quarters in which the reform was in place, leaving

an evaluation period that includes the last two quarters of *phase 1* and the two quarters of *phase 2*. We do not find any sign of the reform having a declining effect over time. The parameter estimated for the effects of the reform in the last two quarters of *phase 1* is still clustering around -0.4, statistically significant at the 1% level, while in the next two quarters, during *phase 2*, the estimate is reversed, being equal to +0.48 at the same significance level (table 7). The sudden change in coefficient estimates between the last two quarters of *phase 1* and the two subsequent quarters 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.

Finally we test for the possibility of announcement effects driving the rebound in absence rates during *phase 2*. Indeed, during the fourth quarter of 2009[15], the government announced a new increase in monitoring time intervals to take place in 2010. The announcement of stricter rules might have pushed workers to increase absence rates when monitoring was at a comparatively low level. 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 incentives' system. To test for this possibility it is possible to interact the dummy capturing the differential impact of *phase 2* of the reform on absence rates with each of the two quarters, one before the announcement was made and one including it. The estimated coefficients (table 8) for the two interactions are ranging between 0.34 and 0.4 and are not statistically significantly different from each other, meaning that the rebound in absence rates was in place before the announcement of stricter rules was made.

Following the estimates of the specification including all workers irrespective of their tenure (first column of table 6), the Average Treatment Effect on the Treated (ATET), calculated as the marginal effect of the reform on the public sector, entails a decrease of 24.5 per cent in sickness absence rates for public sector workers with the *phase 1* setting. Interestingly, this would be exactly the wedge between the public and private services sector during *phase 1* period simulating absence rates had the reform not taken place. This strong effect is completely reversed during *phase 2*, in which absence rates go back to the pre-reform period. Assuming that genuinely sick individuals' choice whether to report sick is not affected by monitoring effectiveness (equation 5), this results highlights the relevance of opportunistic behaviour rather than presenteeism in driving changes in absence rates.

4.2 Average treatment effects: gender differences

We will now focus on gender differences in the reaction to the reform (table 9). For men and women, *phase 1* of the reform significantly reduced absence rates in a similar fashion (coefficient

[15]See section 3. Figure 2 shows that the announcement was widely followed by the public.

estimates are ranging between -0.3 for men and -0.27 for women, statistically non different from each other). The change in absence rates when monitoring time intervals were brought back to the pre-reform level reveals instead a different pattern, with a strong and statistically significant increase in absence rates detected only for men (the coefficient estimate being slightly above 0.5). Such a pattern is not present for women, for which the change in monitoring level does not alter absence behaviour. Such a differential impact of the amendment of longer time interval for sickness absence controls introduced in *phase 2* of the reform entails a higher incidence of opportunistic behaviour among men. This is clear from equation 4, showing that any change in absence rates due to a change in monitoring is relevant only to individuals cheating on actual health conditions. On the other side, women seem to be more sensitive to wage cuts in place both in *phase 1* and *phase 2*, confirming women's higher elasticity of labour supply, an element widely present in the literature (Alesina et al., 2007). In the case of women, an appraisal of reform's effect is thus less straightforward since it is not clear whether the decrease in absence is due to an increase in presenteeism or a decrease in absenteeism (see equation 5). This result is not due to a higher incidence of women in public administration subsectors suffering higher wage cuts. If anything, women tend to be overrepresented in sectors where the share of non-base salary on total salary is lower: according to the results of a weighted OLS for the pre-reform year 2007, an increase in 10 per cent in the share of female employment in a given public administration's subsector is associated with a decrease of the share of non-base salary over total wage of more than 3% (fig. 3).

4.3 Spillovers

We finally take into consideration the possibility of indirect effects triggered by the reform in the private sector, eventually preventing the correct identification of its effects.[16] An increasing media-pressure on absenteeists' opportunistic behaviour, or a decrease in absenteeism-related opportunism triggered by the reform might for example have put a downward pressure on private services workers (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 behavior*, determined for example by the necessity of staying at home for taking care of dependent relatives. The increase in relative price of absence

[16] See condition 3 of subsection 3.2.

for the public sector might have induced substitution in absence between partners if one of them works in the private sector. In this case we expect an increase in absence rates in the private sector as a result of the reform and an *upward bias* in the reform effects' estimates;
- *between partner complementarities in absence behavior*, if partners prefer to spend together their time absent from work. 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 general 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 3.2.

In order to check the robustness of the section 4.1 to this kind of bias, equation 5 is re-estimated dropping all the observations for couples in which one partner works in the private and one in the public sector. Dropping mixed couples increases marginal effects of *phase 1* setting of the reform in absolute value (the consequent reduction in absence rates being equal to -24.8 instead of -24.5 per cent), meaning that within couple interactions introduce a downward bias in the estimates. The understanding of the channel through which indirect effects determine a reduction in absence rates in the control group is an interesting topic for further research, but is out of the scope of the present paper.

4.4 External validation

Results of the econometric analysis entail strong reform effects and interesting gender differences in the reaction to incentives. Such a study can be performed in Italy only using the dataset at hand, a unique source providing homogeneous information on sickness absence both for the private and the public sector. Nevertheless, it is useful to look for descriptive evidence able to confirm or contradict the main empirical results obtained in this paper. According to government's official data[17], 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

[17]Ceci and Giungato (2010).

Security Agencies and the Fiscal Agencies[18], each employing around 30 thousand people, convey a similar picture. According to SSA, the only one providing gender based results, after falling by 40 per cent during *phase 1* of the reform, days of absence increased more for men (19 per cent) than for women (15 per cent) during *phase 2*, a result not too dissimilar from those of section 4.2.

5 Conclusions

Compared to private service sector workers, civil servants have higher sickness absence rates both in the United States and Europe. In an effort to reduce this wedge, a reform took place in Italy, reducing sickness insurance replacement rates in the first ten days of absence and increasing controls on individuals reporting sick. After exactly one year, a partial amendment to the reform brought monitoring level back to the pre-reform setting, while the replacement rates' cut remained unchanged. The two subsequent and unexpected changes in sick insurance policy concerning about 20 per cent total employees provide an ideal experimental setting for evaluating the relative importance of monitoring vs. monetary incentives in determining workers' absence. According to our (conservative) estimates, based on a regression difference in difference approach with the private services sector as the control group, absence rates dropped on average by 24.7 per cent when the reform introduced both monetary disincentives and stricter monitoring. This drop was completely 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 limiting absence. Moreover, given that monitoring levels should be rather irrelevant to genuinely sick individuals, it can be inferred that opportunism, rather than changes in presenteeism due to the cut in replacement rates, is the driving force behind changes in absence levels. Average results are driven by men, more sensitive to monitoring levels, while monetary incentives seem to be relatively more effective for women.

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

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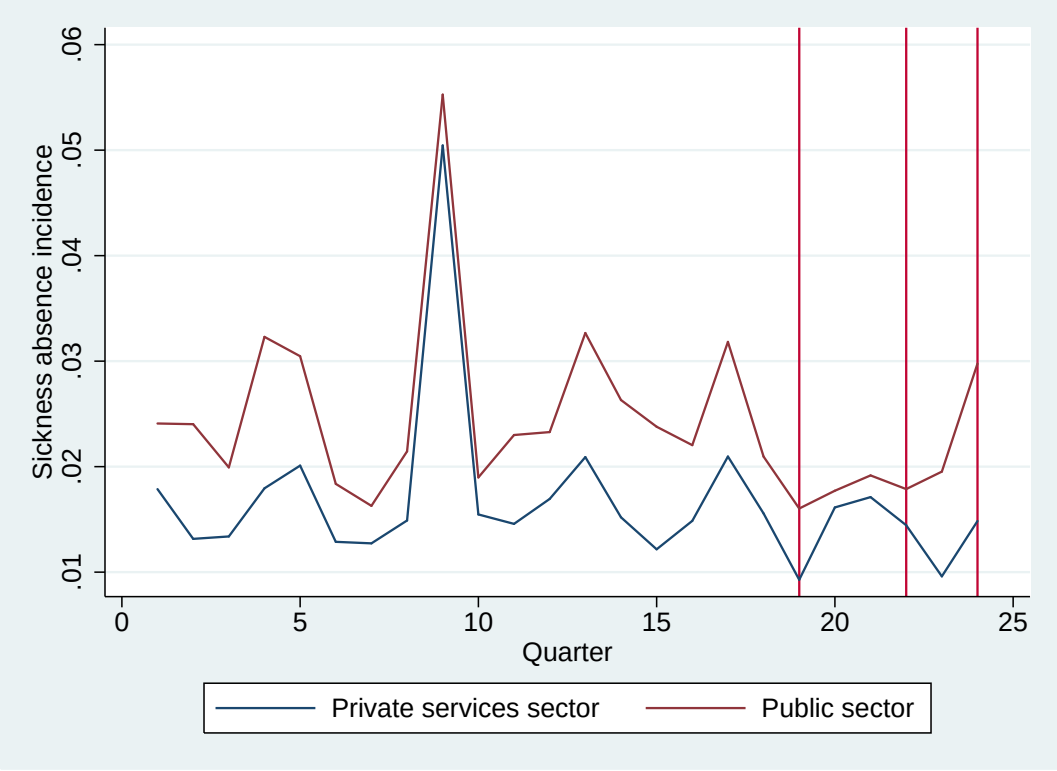
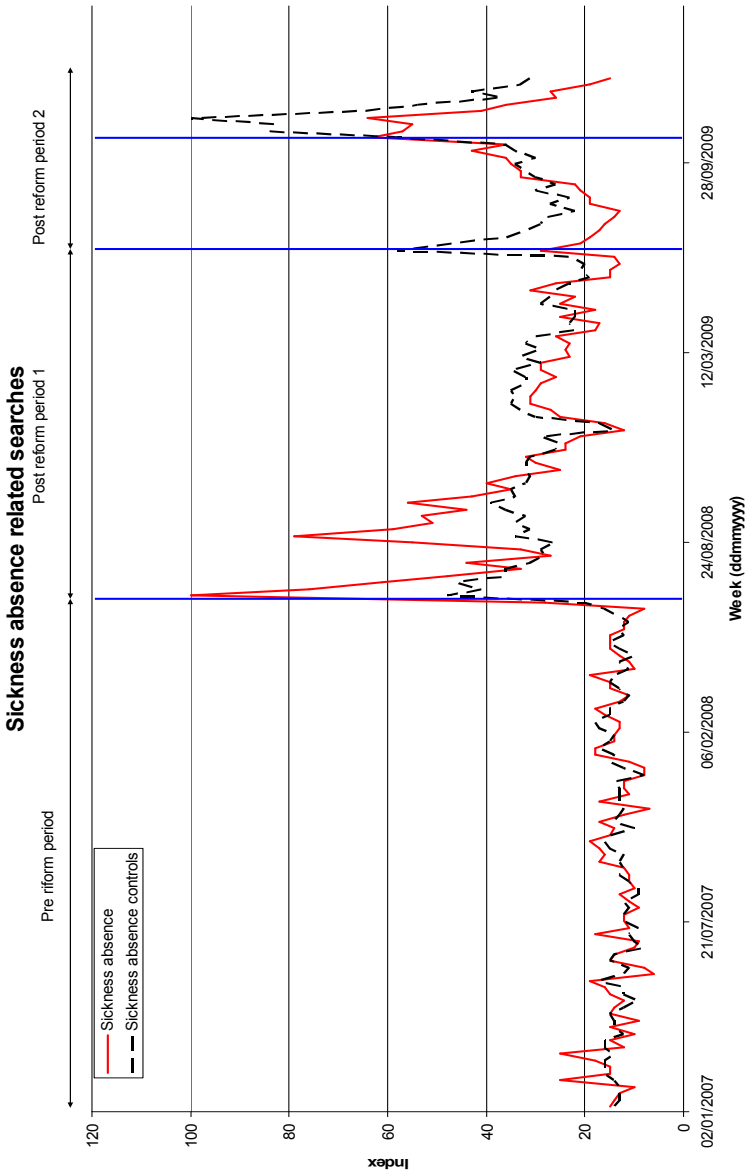
Figure 1: Sickness absence rates



Notes: 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 red vertical line identify respectively the quarter of introduction of the new sickness absence policy (2008Q3) and of its partial amendment (2009Q3).

Figure 2: Incidence of Google searches for "Sickness absence"



Notes: The figure reports the incidence of queries for the keyword "sickness absence" and "sickness absence controls" (In italian, "assenza per malattia" and "visite di controllo", respectively) over total queries performed through the Google search engine with values 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 bar identifies introduction of *Phase 2* of the policy, while the third bar 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 3: Incidence of non base salary on total wage

Notes: The figure reports on the y-axis the share of non-base salary on total salary and, on the x-axis the share of women on total employment in each of the subsectors of Italian public employment (RGS, 2008). These include: Research, Local administrations, Ministries, Prime minister's office, State monopolies, Fire brigades, Police, Judiciary, Prefects, Diplomats, Prison guards. OLS estimates having the share of nonbase salary on total salary as the dependent variable give a coefficient equal to -0.314 significant at the 1% for the share of women on total employment in the sub-sector.

	Private pre-reform				Private post-reform				Public pre-reform				Public post-reform			
	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Low edu	0.16				0.14				0.17							
Int edu	0.65				0.64				0.61				0.60			
High edu	0.19				0.22				0.23				0.26			
Female	0.51				0.52				0.41				0.42			
Married	0.55				0.54				0.71				0.71			
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			
North	0.56				0.55				0.35				0.37			
Center	0.22				0.23				0.26				0.25			
South	0.22				0.22				0.39				0.38			
Part time	0.16				0.18				0.07				0.07			
Temps	0.09				0.10				0.07				0.07			
Firm's 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				0.39			
$x >= 250$	0.12				0.12				0.23				0.22			
$x < 10$	0.05				0.03				0.06				0.03			
Tenure	10.51	9.51	0	47	10.65	9.69	0	48	16.50	9.69	0	44	17.70	10.03	0	46
$(\text{Tenure}/100)^2$	2.01	2.98	0	22.09	2.07	3.10	0	23.04	3.66	3.47	0	19.36	4.14	3.77	0	21.16
Sick. Abs.	0.016	0.13	0	1	0.013	0.11	0	1	0.026	0.16	0	1	0.020	0.14	0	1
Other Abs.	0.019	0.13	0	1	0.017	0.13	0	1	0.011	0.11	0	1	0.010	0.10	0	1
Obs.	149474				48897				57272				17564			

Table 1: Descriptive statistics, weighted sample.

Notes: Author's calculation on ILFS data. Weighted values. The pre-reform period is 2004Q1-2008.2; the post reform period is 2008.3-2009.2. NSSR stands for Not Self Sufficient Relative(s). The table include only white collar employees not employed in the army, the health care or education sector and those individuals absent from work for reasons out of their control.

Table 2: Logit regression for the incidence of absence, pre-reform period (2004.Q1-2008.2)

Contractual hours	-0.042	-0.041
	[2.96]***	[2.88]***
(Contractual hours/100)^2	0.042	0.042
	[2.41]**	[2.41]**
Int edu	-0.295	-0.235
	[6.55]***	[4.04]***
High edu	-0.476	-0.444
	[7.81]***	[5.50]***
Female	0.328	0.325
	[8.43]***	[6.04]***
Married	-0.064	-0.067
	[1.42]	[1.47]
Age 25-34	0.289	0.288
	[2.54]**	[2.52]**
Age 35-44	0.369	0.389
	[3.08]***	[3.26]***
Age 45-54	0.452	0.478
	[3.64]***	[3.85]***
Age 55-64	0.587	0.607
	[4.40]***	[4.55]***
NSSR	0.2	
	[4.15]***	
Centre	0.259	0.258
	[5.19]***	[5.18]***
South	0.008	0.008
	[0.08]	[0.08]
Part time	-0.34	-0.364
	[3.38]***	[3.61]***
Temps	-0.153	-0.148
	[1.86]*	[1.80]*
Tenure	0.021	0.021
	[2.98]***	[2.90]***
(Tenure/100)^2	-0.026	-0.026
	[1.29]	[1.27]
11 to 15 empl	0.236	0.24
	[3.45]***	[3.51]***
16 to 19 empl	0.299	0.305
	[3.33]***	[3.39]***
20 to 49 empl	0.297	0.305
	[5.17]***	[5.29]***
50 to 249 empl	0.349	0.358
	[6.42]***	[6.56]***
250 or more empl	0.442	0.451
	[7.17]***	[7.29]***
10 or more empl^a	-0.357	-0.349
	[3.31]***	[3.24]***
PUB	0.291	
	[6.92]***	
Man*PUB		0.424
		[4.72]***
Woman*PUB		0.333
		[3.52]***
Int edu*PUB		-0.148
		[1.62]
High edu*PUB		-0.098
		[0.81]
Man*NSSR		-0.031
		[0.35]
M*NSSR*PUB		0.21
		[1.61]
W*NSSR		0.327
		[4.42]***
W*NSSR*PUB		-0.022
		[0.17]
Second job		0.258
		[1.34]
Second job*PUB		0.193
		[0.70]
Observations	202863	202863

Notes: Author's calculation on ILFS data. Logit regression for the probability of being absent. Robust z statistics in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. NSSR stands for Not Self Sufficient Relative(s). ^{a} the worker is not able to recall exact firm size. Includes only white collar employees not employed in the army, the health care or education sector and those individuals absent from work for reasons out of their control. Includes a constant. Standard errors clustered at the individual level following White (1980).

Table 3: Test of common trend

		Pub vs. Prv	
Column		1	2
2004	Q2	0.158	
		[0.77]	
2004	Q3	0.106	
		[0.49]	
2004	Q4	0.078	
		[0.40]	
2005	Q1	-0.123	
		[0.66]	
2005	Q2	0.012	
		[0.06]	
2005	Q3	0.174	
		[0.81]	
2005	Q4	-0.093	
		[0.45]	
2006	Q1	-0.298	
		[1.74]*	
2006	Q2	-0.162	
		[0.75]	
2006	Q3	0.05	
		[0.23]	
2006	Q4	-0.149	
		[0.75]	
2007	Q1	0.069	
		[0.37]	
2007	Q2	-0.033	
		[0.16]	
2007	Q3	0.284	
		[1.30]	
2007	Q4	-0.041	
		[0.19]	
2008	Q1	-0.061	
		[0.32]	
2008	Q2	-0.326	
		[1.53]	
Ftest: all int.= 0			
Pvalue		0.18	
Trend			-0.03
			[1.06]
$Trend^2$			0.001
			[0.84]
Observations		206746	206746

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. Logit regression for the probability of being absent.

The table reports the estimated coefficients for all the quarter by PUB interactions in the pre-reform period. The interactions augment the basic specification reported in table 2. Standard errors clustered at the individual level following White (1980).

Table 4: Test for sorting effects

	Public sector *(Treatment group)*	Private services sector *(Control group)*
Age 25-34	-0.778	-0.115
	[7.13]***	[2.63]***
Age 35-44	-0.979	-0.245
	[8.96]***	[5.28]***
Age 45-54	-0.829	-0.278
	[7.38]***	[5.52]***
Age 55-64	-0.327	0.381
	[2.80]***	[6.67]***
Woman	0.009	0.058
	[0.23]	[2.61]***
Center	-0.067	0.037
	[1.35]	[1.34]
South	-0.066	0.117
	[1.72]*	[4.92]***
Int edu	-0.104	-0.095
	[2.34]**	[3.40]***
High edu	-0.03	-0.043
	[0.55]	[1.25]
SICKABS	0.261	0.165
	[2.47]**	[2.21]**
$SICKABS_{133/2008}$	-0.052	0.171
	[0.27]	[1.28]
Temp	0.219	0.288
	[3.56]***	[8.30]***
Part time	0.271	0.103
	[4.28]***	[3.65]***
Tenure	-0.079	-0.038
	[11.74]***	[9.70]***
$(Tenure/100)^2$	0.2	0.098
	[11.33]***	[8.53]***
Observations	18644	40031

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. Robust z statistics in brackets. The table reports the estimated coefficients for a logit regression explaining the probability of moving from the public (private services) sector in $t-4$ to another labor market status or sector in t. Includes a constant and additional quarter fixed effects and firm size dummies. Standard errors clustered at the individual level following White (1980).

	Private sector	Public sector
Long absence sickness spells	100	100
Short absence sickness spells	48.96	50.62

Table 5: Relative short/long sickness absence spells incidence

Notes: Includes only white collar employees not employed in the army, the health care or education sector and those individuals absent from work for reasons out of their control. Long sickness absence spells last a week or more. Short absence sickness spells last less than a week.

Table 6: The causal effect of the 133/2008 law on public sector absenteeism: whole sample

	Sickness absence		Other absence	
PUB	0.283	0.285	-0.125	-0.142
	[4.25]***	[4.24]***	[1.17]	[1.31]
PUB*(133/2008)	-0.289	-0.288	-0.027	-0.002
	[3.13]***	[3.11]***	[0.19]	[0.01]
PUB*(133/2008_2)	0.364	0.321	0.289	0.26
	[2.95]***	[2.56]**	[1.50]	[1.34]
NSSR	0.39	0.398	2.121	2.153
	[6.46]***	[6.51]***	[28.39]***	[28.41]***
Part	-0.293	-0.286	-0.526	-0.518
	[2.46]**	[2.35]**	[4.37]***	[4.29]***
Temps	-0.134	-0.046	-0.692	-0.579
	[1.33]	[0.43]	[5.33]***	[4.16]***
Female	0.329	0.319	2.882	2.898
	[6.56]***	[6.25]***	[26.32]***	[26.14]***
Int edu	-0.317	-0.323	0.105	0.095
	[5.13]***	[5.16]***	[0.92]	[0.82]
High edu	-0.417	-0.4	0.185	0.156
	[5.36]***	[5.10]***	[1.50]	[1.25]
Tenure	0.035	0.034	0.03	0
	[3.88]***	[3.52]***	[2.22]**	[0.01]
$(\text{Tenure}/100)^2$	-0.079	-0.074	-0.13	-0.032
	[3.08]***	[2.79]***	[2.38]**	[0.60]
Observations	133521	126623	131346	124502
Firm size dummies	Yes	Yes	Yes	Yes
Family composition dummies	Yes	Yes	Yes	Yes
Quarter dummies	Yes	Yes	Yes	Yes
Tenure<= 1	Yes	No	Yes	No

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. Logit regression for the probability of being absent. Robust z statistics in brackets. Includes only white collar employees not employed in the army, the health care or education sector and those individuals absent from work for reasons out of their control. Includes a constant. Standard errors clustered at the individual level following White (1980).

Table 7: The causal effect of the 133/2008 law on public sector absenteeism: first two quarters of post-reform period not included

PUB	0.287	0.288	-0.126	-0.143
	[4.29]***	[4.27]***	[1.17]	[1.31]
PUB*(133/2008)	-0.413	-0.421	-0.133	-0.106
	[3.50]***	[3.55]***	[0.65]	[0.52]
PUB*$(133/2008)_2$	0.481	0.445	0.397	0.364
	[3.37]***	[3.07]***	[1.68]*	[1.53]
NSSR	0.388	0.396	2.137	2.171
	[6.33]***	[6.39]***	[28.10]***	[28.11]***
Part	-0.302	-0.295	-0.534	-0.524
	[2.51]**	[2.40]**	[4.39]***	[4.28]***
Temps	-0.132	-0.043	-0.687	-0.572
	[1.28]	[0.39]	[5.29]***	[4.12]***
Female	0.334	0.324	2.901	2.919
	[6.51]***	[6.20]***	[25.62]***	[25.43]***
Int edu	-0.298	-0.304	0.104	0.093
	[4.73]***	[4.77]***	[0.90]	[0.80]
High edu	-0.399	-0.382	0.191	0.162
	[5.04]***	[4.77]***	[1.53]	[1.28]
Tenure	0.032	0.031	0.029	-0.001
	[3.49]***	[3.19]***	[2.07]**	[0.10]
$(\text{Tenure}/100)^2$	-0.068	-0.064	-0.122	-0.023
	[2.64]***	[2.39]**	[2.15]**	[0.42]
Firm size dummies	Yes	Yes	Yes	Yes
Family composition dummies	Yes	Yes	Yes	Yes
Quarter dummies	Yes	Yes	Yes	Yes
Tenure<= 1	Yes	No	Yes	No

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. Logit regression for the probability of being absent. Robust z statistics in brackets. Includes only white collar employees not employed in the army, the health care or education sector and those individuals absent from work for reasons out of their control. Includes a constant. Standard errors clustered at the individual level following White (1980).

Table 8: The causal effect of the 133/2008 law on public sector absenteeism: announcement effects

PUB	0.283	0.285	-0.125	-0.142
	[4.25]***	[4.23]***	[1.17]	[1.31]
PUB*133/2008	-0.289	-0.288	-0.027	-0.002
	[3.13]***	[3.11]***	[0.18]	[0.01]
PUB*133/2008$_{Beforeannouncenment}$	0.403	0.366	0.342	0.293
	[2.44]**	[2.20]**	[1.48]	[1.25]
PUB*133/2008$_{Announcement}$	0.338	0.289	0.236	0.227
	[2.29]**	[1.91]*	[1.00]	[0.95]
NSSR	0.39	0.398	2.12	2.153
	[6.45]***	[6.51]***	[28.39]***	[28.41]***
Part	-0.293	-0.286	-0.525	-0.518
	[2.46]**	[2.35]**	[4.37]***	[4.28]***
Temps	-0.134	-0.046	-0.692	-0.579
	[1.33]	[0.43]	[5.33]***	[4.16]***
Female	0.329	0.319	2.882	2.898
	[6.56]***	[6.25]***	[26.32]***	[26.14]***
Int edu	-0.317	-0.323	0.105	0.095
	[5.13]***	[5.16]***	[0.92]	[0.82]
High edu	-0.417	-0.4	0.185	0.156
	[5.36]***	[5.10]***	[1.50]	[1.25]
Tenure	0.035	0.034	0.03	0
	[3.88]***	[3.52]***	[2.22]**	[0.01]
(Tenure/100)^2	-0.079	-0.074	-0.13	-0.032
	[3.08]***	[2.79]***	[2.38]**	[0.59]
Obs	133521	126623	131346	124502
Firm size dummies	Yes	Yes	Yes	Yes
Family composition dummies	Yes	Yes	Yes	Yes
Quarter dummies	Yes	Yes	Yes	Yes
Tenure<= 1	Yes	No	Yes	No

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. Logit regression for the probability of being absent. Robust z statistics in brackets. Includes only white collar employees not employed in the army, the health care or education sector and those individuals absent from work for reasons out of their control. Includes a constant. Standard errors clustered at the individual level following White (1980).

Table 9: The causal effect of the 133/2008 law on public sector absenteeism: gender based analysis

	Sickness abs.		Other abs.	
M*PUB	0.414	0.41	0.417	0.46
	[4.72]***	[4.64]***	[1.53]	[1.68]*
M*PUB*133/2008	-0.301	-0.299	0.223	0.218
	[2.45]**	[2.43]**	[0.63]	[0.61]
M*PUB*133/2008$_2$	0.542	0.528	0.461	0.467
	[3.48]***	[3.37]***	[1.11]	[1.12]
F*PUB	0.147	0.153	-0.181	-0.205
	[1.63]	[1.68]*	[1.58]	[1.76]*
F*PUB*133/2008	-0.27	-0.271	-0.063	-0.034
	[2.06]**	[2.06]**	[0.40]	[0.21]
F*PUB*133/2008$_2$	0.124	0.031	0.24	0.203
	[0.67]	[0.16]	[1.14]	[0.95]
Part	-0.304	-0.297	-0.52	-0.513
	[2.54]**	[2.44]**	[4.32]***	[4.23]***
Temps	-0.121	-0.028	-0.686	-0.569
	[1.19]	[0.26]	[5.28]***	[4.10]***
Female	0.456	0.448	3.14	3.177
	[7.18]***	[6.91]***	[22.09]***	[21.93]***
Intermediate edu	-0.307	-0.312	0.114	0.105
	[4.96]***	[4.99]***	[0.99]	[0.91]
High edu	-0.403	-0.387	0.2	0.172
	[5.18]***	[4.92]***	[1.62]	[1.38]
Tenure	0.035	0.034	0.03	-0.001
	[3.87]***	[3.51]***	[2.15]**	[0.09]
(Tenure/100)^2	-0.079	-0.074	-0.13	-0.031
	[3.09]***	[2.80]***	[2.37]**	[0.58]
NSSR	0.39	0.398	2.121	2.154
	[6.45]***	[6.51]***	[28.41]***	[28.43]***
Observations	133521	126623	131346	124502
Firm size dummies	Yes	Yes	Yes	Yes
Family composition dummies	Yes	Yes	Yes	Yes
Quarter dummies	Yes	Yes	Yes	Yes
Tenure<= 1	Yes	No	Yes	No

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. Logit regression for the probability of being absent. Robust z statistics in brackets. Includes only white collar employees not employed in the army, the health care or education sector and those individuals absent from work for reasons out of their control. Includes a constant. Standard errors clustered at the individual level following White (1980).