Effects of the one-day waiting period for sick leave on absenteeism in the French central civil service[§]

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Abstract

Modulation of sick leave reimbursement scheme has often been used to combat health-related absenteeism. We study the effects of the presence of a one-day waiting period for sick leave. This less generous policy was introduced in the French central civil service in January 2012 and repealed in January 2014, whereas the private sector was not affected. We employ a difference-in-differences strategy with individual fixed effects, using the French Labour Force Survey. We find that the total prevalence of sick leave spells is not affected by the policy, but that its length distribution is. The prevalence of some short-term spells decreases, while the prevalence of some long-term spells increases. Effects are heterogeneous across gender: the total prevalence is not affected when it comes to women, whereas it increases for men. The effect on short-term spells is stronger for young and low-income employees. Effects are also heterogeneous across seasons: short-terms spells decreases in both winter and summer, but they are offset by more long-term spells in winter only. Overall, we conclude that the policy failed to combat absenteeism.

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The question of how health-related absenteeism reacts to the generosity of the reimbursement pattern remains an empirical concern, due to the social cost of absence from work. In a simple theoretical framework with unidimensional effort choice and unidimensional coverage level, the classical implication is that a lower coverage leads to a higher effort. The effort choice related to sick leave is however bidimensional, as the worker may decide of both the start and the length of his absence. The coverage is itself multidimensional, since the replacement rate may vary over the days of sick leave. In accordance with the unidimensional model, several papers on sick pay reforms across Europe find that the prevalence of absence decreases when the generosity of sick pay decreased (Ziebarth and Karlsson, 2014; Chemin and Wasmer, 2009; Henrekson and Persson, 2004). But subtler results have been recently found in settings that are more distant from the unidimensional reimbursement framework (Davezies and Toulemon, 2015; Paola, Scoppa and Pupo, 2014; Pettersson-Lidbom and Thoursie, 2013; Johansson and Palme, 2005). Studying the implementation of a waiting period or similar measures, most papers find that a lower generosity during the first days of sick leave induces a decrease in short-term spells. But it can also induce an increase in the length of long-term spells. As a result, it does not necessarily lead to a decrease in total prevalence. For instance, after the abolishment of a waiting period, it was found that total prevalence had not significantly increased (Davezies and Toulemon, 2015) and sometimes significantly decreased (Pettersson-Lidbom and Thoursie, 2013)[1]. Consequently, the reaction of health-related absenteeism to a change in the generosity of the reimbursement pattern remains an open research field.

This paper evaluates the effects of the presence of a one-day waiting period on the prevalence of sick leave, which is the proportion of employees on sick leave. We also differentiate the effects between the sick leave lengths. For that purpose, we exploit two exogenous changes in sick leave pay in the French civil service. On 1 January 2012, the French government introduced a one-day waiting period for all workers in the French civil service to combat absenteeism. On 1 January 2014, exactly 2 years later, the following government repealed the measure so as to keep its elec-

[1]See Pollak (2015) for a review of this literature.

tion promise. Both the exogenous introduction of this one-day waiting period and its exogenous repeal provide an ideal setting to assess this component of a sick pay scheme.

We apply a difference-in-differences strategy between the wage earners of the central civil service and those of the private sector. We choose to focus on the central civil service, the main of the three parts of the French civil service, for three reasons. First, in the local and territorial civil services, the two other parts of the French civil service, other monetary incentives on work attendance exist. The characteristics and the timing of implementation of these other incentives vary greatly between public institutions and over time. Second, in the local civil service, the timing of implementation of the one-day waiting period also vary greatly between public institutions and over time. Third, the hospital civil service is not comparable regarding sick leave trends with the private sector as a whole or with the private hospital sector.

We use panel data from the French Labour Force Survey from 2010 to 2014. We include individual fixed effects to control for unobserved individual heterogeneity. This quasi-natural experiment enables us to escape the selection concerns that may erupt in comparative studies due to the possible self-sorting of both employers and employees regarding their preferred pay sick scheme (Lanfranchi and Treble, 2010).

We obtain four different results. First, we do not find that the one-day waiting period decreases health-related absenteeism. If anything, it increases it (this increase is significant on the one hand for men and on the other hand for spring). Second, it leads however to a change in the length distribution of sick leave. We find that there is a significant decrease of 50 % in the prevalence of short-term 2 days spells, and a significant increase of 25 % in the prevalence of long-term 1 week to 3 months spells. Third, reactions differ across sociodemographic characteristics. Men react by significantly increasing long-term 1 week to 3 months spells, whereas women react by significantly decreasing short-term 2 days spells. Reactions on short-term 2 days spells are stronger when the employees are young and with low income. Fourth, reactions also differ across seasons: short-term spells decreases in both winter and summer, but they are offset by more long-term spells in winter only.

Our study has five main advantages over previous papers focusing on day waiting periods or similar schemes. First, the use of survey data allows us to properly focus on health-related absence. On the contrary, administrative data[2] focus on absence for which a medical certificate is provided. In both cases, these absences may be subject to prevention efforts to avoid to get sick, which is *ex ante* moral hazard, and to several layers of hidden actions after the employee knows she is sick, which is *ex post* moral hazard. These layers potentially include the decisions to go to work, to choose a doctor, to consult her and to influence her decision regarding the medical diagnosis and the medical certificate. But the difference is that there is an extra layer of *ex post* moral hazard with the administrative data, which is the reason stated by the employee to her employer for her absence. For short-term absence, employees are likely to use days off in order to avoid a wage penalty. This may lead to higher estimates with administrative data, as the employee has an extra degree of freedom. Estimates based on both kinds of data are of interest. While sickness insurers may be particularly interested in estimates based on administrative data to control their expenditure, our estimates based on survey data are better measures of the real impact on health-related absenteeism. Second, we study the impact of a change of one single parameter of the sick pay pattern, the replacement rate of the first day of the sick leave, whereas Pettersson-Lidbom and Thoursie (2013) get for instance a mixed effect of the abolishment of a one-day waiting period and of an increase in the replacement rate for spells up to 14 days. This enables us to recover the specific impact of a one-day waiting period on sick leave. Third, we include individual fixed effect to control for unobserved individual heterogeneity, which is rarely done in works that use survey data. Fourth, we are able to interact the studied policy with seasons and we find it matters. As far as we know, it had not been observed so far that a change of generosity of pay sick could lead to different effects across seasons. Fifth, the introduction and the repeal of the measure allows us to assess the symmetry of the corresponding effects and to conduct robustness checks. Eventually, this is the first research paper that assesses the effects of this controversial policy in France.

The remainder of the paper is organized as follows. In section 1, we present the institutional framework, the data and the descriptive statistics. In section 2, we present the empirical strategy. Our results regarding the responses of the prevalence of sick leave and the prevalence by length categories are presented in section 3, as well as those regarding the seasonality and the heterogeneity of these responses. Some robustness tests follow in section 4. Section 5 concludes.

[2]Examples of papers using administrative data include Pettersson-Lidbom and Thoursie (2013) and Paola, Scoppa and Pupo (2014).

1 Institutional framework, data, and descriptive statistics

1.1 The 1 January 2012 introduction and the 1 January 2014 repeal of the one-day waiting period in the French civil service

Until the end of 2011, employees of the civil service benefited from a 100 % replacement rate of their wage during the first 89 days of their sick leave. After that threshold, the replacement rate fell to 50 % of their wages. Hence they enjoyed a full coverage for sick leave before that threshold, and a partial coverage after.

In late 2011, the French government announced that it would implement a one-day waiting period in the civil service. The measure was taken in the 2012 Budget Act (law number 2011-1977 of 28 December 2011) by the right-wing ruling party for reasons of equity with respect to the private sector and also to combat absenteeism. This monetary incentive is strong since it makes the replacement rate fall on the first day from 100 % to 0 %. The measure was effective on the 1 January 2012. This policy applied to the whole civil service, that is all civil servants, soldiers, and employees with a private contract in the civil service[3].

Implementation details were specified by a circular dated 24 February 2012. The policy concerns neither work-related leave, neither the so called "long length" and "long sickness" leave (both cover severe diseases such as cancers), neither maternity leave, nor parental leave. Since the implementing circular was signed only in February 2012, and since many difficulties to adapt the pay information systems to the policy were reported, it is likely that the first deduction of earnings started with some delay. However, the circular clearly states that it applied to all sick leave starting from 1 January 2012 and the measure was highly publicized (notably by civil service unions). Hence most employees in the central civil service and in the hospital civil service had heard of the change, and probably knew it applied as soon as 1 January 2012. It differed in the local civil service where there was a huge heterogeneity in the policy implementation.

The possibility to cover the one-day waiting period by a collective health insurance plan also

[3]There was a doubt whether previously state-owned companies with still many civil servants had to apply it, but it appeared it was not necessarily the case after the French public transport company in Paris and suburbs (RATP) was successfully sued because it had started applying it. Other similar firms, such as the French Post, considered they were not required to apply it to their civil servants.

differed between on the one hand the central and hospital civil services and on the other hand the local civil service. While a coverage may have been available in some territorial collectivities of the local civil service, we are unaware of such a coverage in the central and hospital civil services (see Sénat (2013*a*)).

During the presidential campaign, the left-wing contender promised to abolish the one-day waiting period if elected. He became president in May 2012. In the 2014 Budget Act (law number 2013-1278 of 29 December 2013), the left-wing ruling party removed the one-day waiting period for sick leave for all civil service employees[4]. The measure started as soon as 1 January 2014 and there is no reason to believe it was not effective immediately.

In the French private sector, the social security compensates sick leave by providing sick leave benefits equal to half of the wage after a three-day waiting period. After a seven-day waiting period, the employer is also obliged to contribute, so that benefits reach then at least 90 % of the wage for the following 30 days. But most employees benefit from more generous conditions than those strictly required by the law. This is very heterogeneous since it is due to conventions at the industry or employer level. Note that in July 2008, some of these rules were reformed (see Ménard and Pollak (2015) and Ben Halima, Elbaz and Koubi (2016) for a precise description of sick leave in the French private sector and an assessment of the July 2008 reform). We are not aware of any other change regarding sick leave rules in the private sector between July 2008 and 2014.

The introduction and the repeal of the one-day waiting period policy constitute two quasi-natural experiments. It affected only the civil service, and did not concern the private sector. We choose to focus on the central civil service, the main of the three parts of the French civil service, for three reasons. First, in the local and territorial civil services, the two other parts of the French civil service, other monetary incentives aiming at combating absenteeism exist. They include for example semiannual or annual bonuses, calculated from professional value and work attendance. The characteristics and the timing of implementation of these other incentives vary

[4] A reinforcement of monitoring was announced at the same time. If the physician certificate was not sent within 48 hours after the drawing up of the sick leave, civil servants may lose half of their benefit between the date of prescription and the date of transmission of the physician certificate. Note that the corresponding decree was published in October 2014 and the corresponding circular was released in April 2015. It was also announced that controls of the relevance of their sick leave would be increased.

greatly between public institutions and over time. Second, in the local civil service, the timing of implementation of the one-day waiting period also varies greatly between territorial collectivity and over time[5]. Third, the hospital civil service is not comparable in terms of sick leave trend with the private sector as a whole or with the private hospital sector.

1.2 Data: the French Labour Force Survey

This work uses a survey data source, namely the French Labour Force Survey. Since 2003, around 100,000 individuals are interviewed quarterly. They are sampled from the housing-tax registers and from the census in order to be representative of the individuals aged over 15 or more and living in France. It is a panel. Each individual is followed during 6 subsequent quarters and 1/6 of the sample is renewed each quarter. It contains full information on the labour market status, in addition to other socio-economic characteristics.

In the survey, two different sequences of questions can be used to determine if the survey respondent was on sick leave. The use of one or the other sequence of questions depends on whether the individual worked at least one hour during the reference week (defined as the week just before the interrogation). In the first case, when the individual worked at least one hour during the reference week, she is asked whether she took a sick leave or a leave related to a work accident[6] and how many days during the reference week this leave lasted. In the second case, when the individual did not work at all during the reference week, she is asked why she did not work. One of the possible answers is sick leave or a leave related to a work accident. When this

[5] In a response to an oral question in the French Senate published 27 March 2013 and related to the non-application of the one-day waiting period in a territorial collectivity, the French Minister of civil service states that she could "understand that [the one-day waiting period] would not necessarily be applied the following months of its existence. Each territorial collectivity executive should decide what to do" (translation). See Sénat (2013*b*).

[6] The regulation on sick leave and on leave related to work accident are distinct. Especially, no waiting period for leave related to work accident was implemented in 2012 in the civil service. This would tend to attenuate our estimates. Note however that over a reference day the prevalence of absence for work accident is ten time lower than the prevalence of absence for sick leave in the central civil service (DGAFP, 2015). In the private sector, a decree changed in 2010 the way contribution rates of companies are estimated and took effect from 2012 on. The official role of occupational doctors was also modified in July 2012 (Safon, 2015*a*,*b*). But this firstly applies to the employer side of the sick leave scheme. Secondly, considering expenditures, they are also more than ten times lower for work accidents than for sick leave in the private sector (Drees, 2014).

answer is chosen, the individual is then asked the expected total length of the leave. More details and extracts of questionnaire in French are available in appendix A.

The two lengths that correspond to the two sequences of questions have consequently a different meaning. In the first case, it is a realized value, but the length may be left-censored or right-censored, as the sick leave may have begun before or may continue after the reference week. In the second case, it is an expected value, but which is related to the total length of the sick leave.

As a result of these two different intrinsic meanings of the length in the two sequences of questions, a duration model analysis cannot be conducted. To go beyond the study of total prevalence of sick leave, we break sick leave spells into different categories. We then study the reaction of each category of sick leave spells to the policy.

We call "short-term" sick leave spells those for which the interviewed person worked at least one hour during the reference week (which corresponds to the first sequence of questions mentioned above). We call "long-term" sick leave spells those for which the interviewed person did not work at all during reference week (which corresponds to the second sequence of questions mentioned above). By construction, the length of the former cannot exceed 7 days, and the length of the latter is very rarely under 7 days[7]. We then break sick leave spells into precise length categories. We consider the short-term spells with length of 1 day, 2 days, 3 days, and 4 to 7 days. For instance, a short-term 2 days spell is a short-term spell whose length is strictly superior to 1 day, and inferior to 2 days. We also consider the long-term spells with length under 3 months, and over 3 months. This 3 months threshold was chosen because employees of the civil service without additional coverage start losing half of their benefits after that threshold. Specifically for long-term spells with length under 3 months, we use the expression "1 week to 3 months" for the sake of simplicity, even if the length may be under 7 days in rare occasions. We hence have a partition of sick leave spells into 6 categories: each spell is in one and only one category.

For the sake of brevity, we will omit the mention of "short-term" or "long-term" when we refer to each of these 6 spells categories. For example, a "short-term 2 days sick leave spell" will

[7]It can happen in case of part time job for instance: the person has been absent only two days, but these two days were her working days. This rarely occurs.

be referred to as "2 days spell" and a "long-term 1 week to 3 months sick leave spell" will be referred to as "1 week to 3 months spell".

We define the prevalence as the proportion of individuals who are on sick leave during the reference week. It differs from incidence, which is the proportion of individuals who begin a sick leave during that week. Both are of interest, but our data do not enable us to get access to the incidence, since we do not know when the sick leave begins. We hence focus on the prevalence. We consider the prevalence of all sick leave spells but also the prevalence of each category of spells.

For the descriptive statistics as well as the regressions, we use weights produced by INSEE. Due to the limited sample size of the central civil service, observations with the highest weights (1 %) are excluded. Note that we use cross section weights since longitudinal weights are not currently available for the French Labour Force Survey[8]. In order to use the weighted regression with individual fixed effects, we need a unique weight per individual. Thus, we attribute to each individual a unique weight equal to the mean of its weights over the periods of observation[9].

We limit ourselves to individuals aged under 75 (and over 15 by design of the French Labour Force Survey). In order for the private sector to be a convincing counterfactual to the central civil service, we keep only the wage earners, and exclude the self-employed workers. We also exclude survey respondents without information on whether they have been absent at work during the reference week.

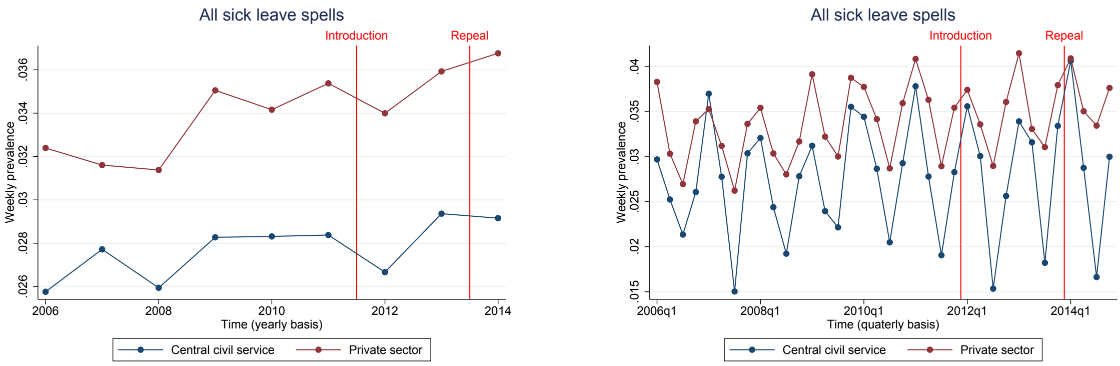
1.3 Descriptive statistics

We begin by looking at the total sick leave prevalence over time and by sector (Figure 1), at both a yearly and quarterly basis. Over the period the total sick leave prevalence is roughly 3 % in the central civil service and 3.5 % in the private sector (see Inan (2013) for a detailed comparison on the determinants of absenteeism based on the survey). There seems to be a slightly rising trend in both sectors. It also depicts a high change between the year 2008 and the year

[8] Even though some works are currently carried out at INSEE on the issue (Jauneau and Nouel de Buzonniere, 2011; Biausque, Juillard and Lebrère, 2016).

[9] The average mean of individual weights is 560. Regarding the dispersion of the weights of an individual across time (within dispersion), the average standard error of individual weights is 50.

2009 in the private sector. At that same period, two disrupting events that might explain this change occurred. First, in July 2008, National Inter-professional Agreements (ANI) increased generosity of the sick benefit system in the private sector. It increased absenteeism according to Ben Halima, Elbaz and Koubi (2016). Second, the 2008 crisis erupted and caused an increase of 2 points of percentages in the unemployment rate[10] between mid-2008 and late 2009. These two events may have affected differently the central civil service and the private sector between 2008 and 2009. As a consequence, we restrict the econometric analysis to years 2010-2014.



Source: French Labour Force Survey 2006-2014.

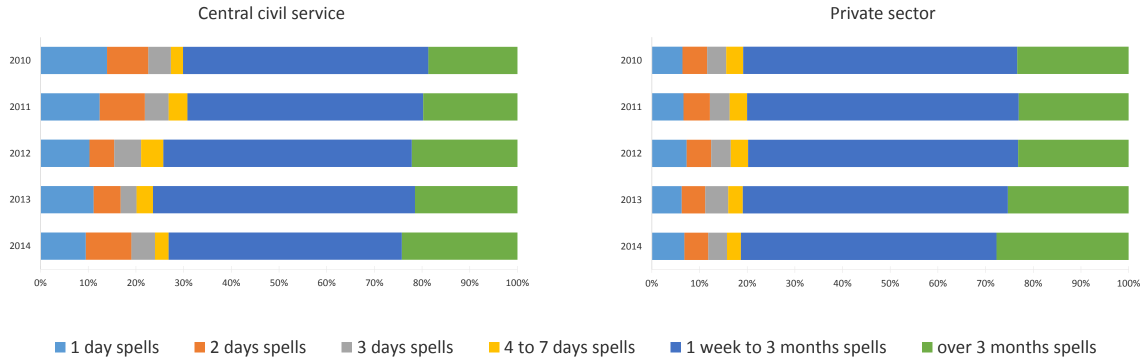
Lecture note: During the year 2006, the average weekly prevalence of all sick leave spells is 2.6 % in the central civil service and 3.2 % in the private sector.

Figure 1: Prevalence of all sick leave by sector, at a yearly (left) and quarterly (right) basis.

The two sectors seem to evolve in a very similar way over the period regarding the prevalence of all sick leave spells, except between 2008 and 2009 for the two reasons mentioned above. It is the case until 2011, before the introduction of the policy, but also in 2012-2013, during the time of implementation of the policy, and in 2014, after the removal of the policy. At this stage, we have no clue of an effect of the one-day waiting period.

We go beyond the total prevalence by breaking sick leave spells into the 6 previously described length categories. We consider the distribution of observations of sick leave spells between these 6 categories for each year between 2010 and 2014 (Figure 2). Contrary to the total prevalence, a clear change appears at first sight. During the 2 years of the implementation of the policy (2012

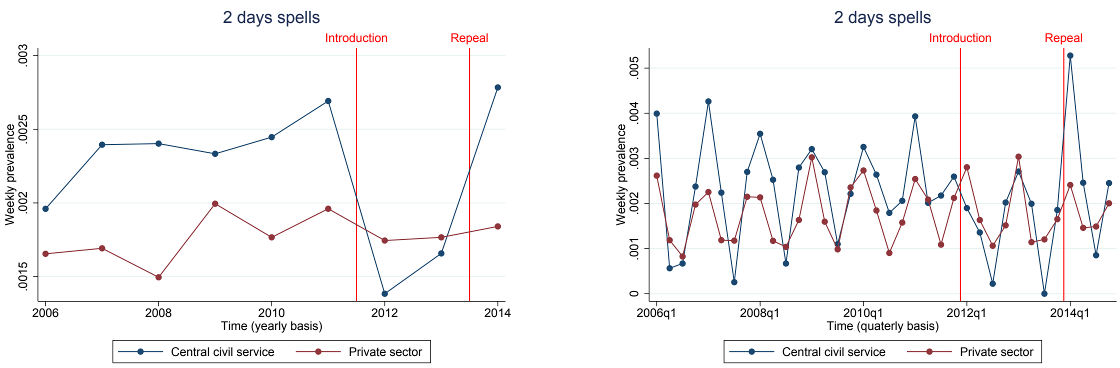
[10]Several studies have however found a negative correlation between unemployment and sick leave (Arai and Thoursie, 2005; Pichler, 2015), notably in France (Grignon and Renaud, 2007).



Source: French Labour Force Survey 2010-2014.

Figure 2: Length category distribution of observations of sick leave spells by sector, years 2010-2014.

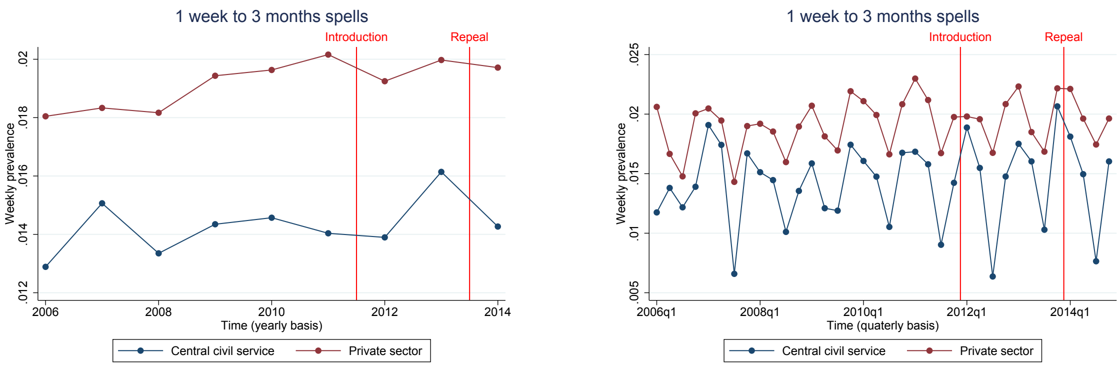
and 2013), we observe a clear shift to the left of the spells distribution, in the treated group only. Looking more precisely, the category which decreases the more is the 2 days spells, while the category which increases the more is the 1 week to 3 months spells. We hence continue by looking specifically at these two categories of sick leave spells, to check whether this distribution change also comes with a level change for each of these two categories.



Source: French Labour Force Survey 2006-2014.

Figure 3: Prevalence of 2 days spells by sector, at a yearly (left) and quarterly (right) basis.

We first examine the prevalence of 2 days spells over time (Figure 3). We observe both a strong decrease at the time of introduction of the policy and a strong increase back to a pre-policy level at the time of repeal. The one-day waiting period seems to have strongly decreased the prevalence of 2 days spells, with an immediate effect both at the introduction and repeal.



Source: French Labour Force Survey 2006-2014.

Figure 4: Prevalence of 1 week to 3 months spells by sector, at a yearly (left) and quarterly (right) basis.

Second we examine the prevalence of 1 week to 3 months spells over time (Figure 4). We observe both an increase between the years 2012 and 2013, which is one year after the introduction of the policy, and a decrease back to a pre-policy level at the time of removal. The one-day waiting period seems to have increased the prevalence of 1 week to 3 months spell, with a delayed effect at the introduction and an immediate effect at the repeal.

Due to the one-day waiting period, employees may be reluctant to begin a sick leave. However, when the sick leave is taken, its length may increase costlessly. For example, a sick leave which would last 2 days lasts 3 days. Similarly, a sick leave which would last 3 days lasts 4 days, and so on. The impact on the prevalence of 3 days spells is hence unclear. This may explain why we do not see a clear-cut effect on spells of intermediate length (see Appendix B).

Regarding the two extreme spells categories, 1 day spells and over 3 months spells, they are both specific cases. Due to their shortness, 1 day spells can more easily not be reported to the employer or reported as leave for another reason, like days off[11]. As for over 3 months spells, they imply a wage loss usually much more significant than the one-day waiting period (without additional coverage) and they concern mostly severe diseases. As a result, a change for this type of spell is not likely. This may explain why we do not observe a clear effect for these two categories (see also Appendix B).

The global picture is hence a decreased prevalence of some short-term spells, an increased prevalence of some long-term spells, and an unchanged total prevalence. The underlying mechanism is likely to be a decreased incidence of sick leave and an increased length of spells.

This is consistent with what was found by Pettersson-Lidbom and Thoursie (2013) in a similar context where a one-day waiting period was repealed. Their "*major point [...] is that the reform made individuals start new spells to a larger extent but that ongoing spells became shorter*".

Although these descriptive statistics are preliminary evidence of effects of the policy, we identify the causal effects on each category of spells by carrying out an econometric analysis on our panel data. It enables to take into account time-variant observed and time-invariant unobserved individual heterogeneity regarding sick leave. In particular, the ability to control for the unobserved individual risk level allows to conclude that the change in the prevalence is due to a state dependence and not to an unobserved heterogeneity.

We consider only the years 2010 to 2014 as explained above. From a cross section perspective, we have 742,000 observations and among them 26,000 for which the respondent is on sick leave. From a panel perspective, we have 206,000 individuals. Among them, 187,000 are never on sick leave, 15,000 are only once on sick leave and 4,000 are at least twice on sick leave. When the individual is at least twice on sick leave, it implies most of the time at least one long-term spell (if the long-term spell lasts more than 3 months, it may be observed several times, as interrogations are conducted quarterly). Only 178 individuals are at least twice on short-term sick leave.

Simple statistics regarding absenteeism and sociodemographic characteristics for each sector

[11] In some administrations, such as customs, there even exists leave for very short indispositions. In the particular case of customs, the French highest Court of Administrative Justice (Conseil d'État) stated that the one-day waiting period did not apply to this leave for very short indispositions (Conseil d'État, 2013).

Table 1: **Absenteeism and sociodemographic characteristics per sector, years 2010-2014**

Sector	Central civil service	Private Sector
Total prevalence of sick leave spells	2.8 %	3.5 %
Women	55 %	46 %
Mean age	42	40
Highly educated (degree level)	47 %	12 %
Teachers	44 %	1 %
Permanent contract or civil servants	89 %	87 %
Mean monthly wage	2200 €	1800 €
Being in a couple	72 %	69 %
Having a child	55 %	51 %
Observations	82,246	659,298

Note: Information on monthly wage is missing for 5.4 % of observations.

Source: French Labour Force Survey 2010-2014.

are presented in Table 1. The main difference concerns teachers, who nearly all belong to the central civil service. A degree level is usually a requirement for this profession, which explains the difference between the two sectors. Other characteristics are quite close. Employees of the central civil service are slightly more likely to be women, slightly older, have slightly more a permanent contract, are better paid, are slightly more in a couple and with a child. On average, they are less on sick leave than in the private sector.

Total prevalence of sick leave spells by sociodemographic characteristics and sector is presented in Table 2. We already observed in Table 1 that the employees of the central civil service are on average less on sick leave than their counterparts of the private sector. This remains true for almost all categories of the studied sociodemographic characteristics. The opposite holds only for the high wage earners (by 0.1 %), the teachers (by 0.5 %) and the highly educated (by 0.6 %).

By comparing the prevalences between the categories of sociodemographic characteristics, we observe that women are more on sick leave than men. Older workers and less-paid workers are respectively more on sick leave than younger workers and better-paid workers. From these rough statistics, being in a couple does not seem to matter much for health absenteeism and having

Table 2: **Total prevalence of sick leave spells by sociodemographic characteristics and sector, years 2010-2014**

Sector	Central civil service	Private Sector
Women	3.4 %	4.0 %
Men	2.2 %	3.1 %
Under the age of 35	2.6 %	3.0 %
Between the ages of 35 and 45	2.6 %	3.1 %
Between the ages of 45 and 55	2.9 %	3.9 %
Over the age of 55	3.5 %	4.9 %
Highly educated (degree level)	2.6 %	2.0 %
Not highly educated	3.1 %	3.7 %
Teachers	2.8 %	2.3 %
Non-teachers	2.9 %	3.5 %
Permanent contract or civil servants	2.9 %	3.7 %
Fixed-term contract	2.0 %	2.3 %
Monthly wage under 1500 €	3.2 %	4.3 %
Monthly wage between 1500 € and 2000 €	3.2 %	3.7 %
Monthly wage between 2000 € and 2500 €	2.8 %	2.7 %
Monthly wage over 2500 €	2.0 %	1.9 %
Being in a couple	2.8 %	3.5 %
Single	2.8 %	3.5 %
Having a child	2.8 %	3.5 %
No child	2.9 %	3.6 %

Note: Information on monthly wage is missing for 5.4 % of observations.

Source: French Labour Force Survey 2010-2014.

a child is associated with slightly less health absenteeism than having no child. There is less absenteeism amongst teachers and highly educated people than amongst non-teachers and not highly educated people, and more absenteeism among employees with a permanent contract (or being civil servants) than among those with a fixed-term contract. All these differences of total prevalence by sociodemographic characteristics are observed in both the central civil service and the private sector. They are usually of a higher magnitude in the private sector compared to the central civil service.

We also run simple regressions with a linear and two non-linear (logit and probit) specifications to enlighten the correlations between the total sick leave prevalence and observable characteristics (see Table 12 in Appendix C). This calls for three remarks. First, the choice of specification hardly has any impact on the results. Second, most observable individual characteristics[12] matter for absenteeism in the same in the regressions (conditionally on other observables) and in the rough statistics of Table 2. The two notable exceptions are being a teacher, which is not very significant in the regressions (it is significant at a 5 % level for the probit specification, but neither for the linear model nor for the logit model) and being in a couple, which significantly decreases the probability to be absent in the regressions. Third, year and quarter also matter for absenteeism in the same in the regressions and in the rough prevalence levels presented on Figure 1. Particularly, the winter quarter is strongly associated with more sick leave.

Working conditions also matter for absenteeism, as shown by Afsa and Givord (2014) and Pollak and Ricroch (2016). Detailed information related to working conditions is available in our survey only in year 2007. We will hence use the panel dimension of our data to encompass the time invariant effects of all variables which are not available.

2 The empirical strategy

To assess the effect of the presence of the one-day waiting period, we adopt a difference-in-differences strategy. The central civil service is the treated group and the private sector is the control group. Descriptive statistics above indeed showed that trends on the prevalence of sick leave in each sector were similar between the two groups before the introduction of the policy in January 2012.

Our main specification is the following:

$$y_{i,t} = \alpha.T_{i,t} + \beta.x_{i,t} + \mu_i + \nu_t + \epsilon_{i,t}$$

Where:

- The dependent variable $y_{i,t}$ is the dummy of employee i taking sick leave during its reference

[12] For a more detailed analysis of the determinants of absenteeism using also the French Labour Force Survey, see Inan (2013).

week of quarter t. It is a prevalence. Regressions are run for all sick leave spells, but also for spells of each of the 6 length categories, as described in subsection 1.2.

- The treatment dummy $T_{i,t}$ is the presence dummy of the one-day waiting period in the central civil service. Let us note $C_{i,t}$ the dummy of employee i belonging to the central civil service at time t. $T_{i,t}$ stands for the belonging to the central civil service which is interacted with years 2012 and 2013:

$$T_{i,t} = C_{i,t} \times 1[2012 \text{ Q1} \leq t \leq 2013 \text{ Q4}]$$

- $x_{i,t}$ stands for the socio-demographic controls that may explain absenteeism and that are available in our data set: the belonging to the central civil service or the private sector, gender and age (through a second order polynomial of age with a cross effect with gender), the educational level, the socio-professional category, the sector of activity, the number of employees in the facility, the type of contract, the fact to be in a couple and to have a child.

- μ_i is an individual (employee) fixed effect. They indeed control for unobserved time-invariant individual heterogeneity. Such fixed effects enable to assess the effect of the reform using only the within variations.

- ν_t is the time effect of the quarter.

- $\epsilon_{i,t}$ is the error term. In all regressions, we report heteroskedasticity robust standard errors. We also cluster at the employee level to address eventual downward bias in the standard errors due to within correlation over time.

We use OLS regression with individual fixed effects to control for the unobserved heterogeneity. This is almost never done in studies that use survey data that are related to absenteeism. For instance, neither Ziebarth and Karlsson (2010, 2014), nor Goerke and Pannenberg (2015), nor D'Amuri (2011) use such individual fixed effects. Puhani and Sonderhof (2010) use them in robustness tests that most often lose significance compared to their preferred specification.

The difference-in-differences strategy assumes that the studied policy did not entail any self-selection between the two groups at the time of the incentive changes. There is no clue of the policy driving less entrance in (or more exit from) the central civil service. Studying quarterly transitions between the central civil service and the private sector, we found these events to be quite rare, and very stable over the period. Roughly 0.5 % of employees in the central civil service leave it every quarter to enter the private sector, whereas 0.06 % of employees of the private sectors leave it to enter the central civil service.

3 Results

3.1 Average treatment effects for spells of different lengths

Table 3: **Average treatment effect on the prevalence of spells of each length category.**

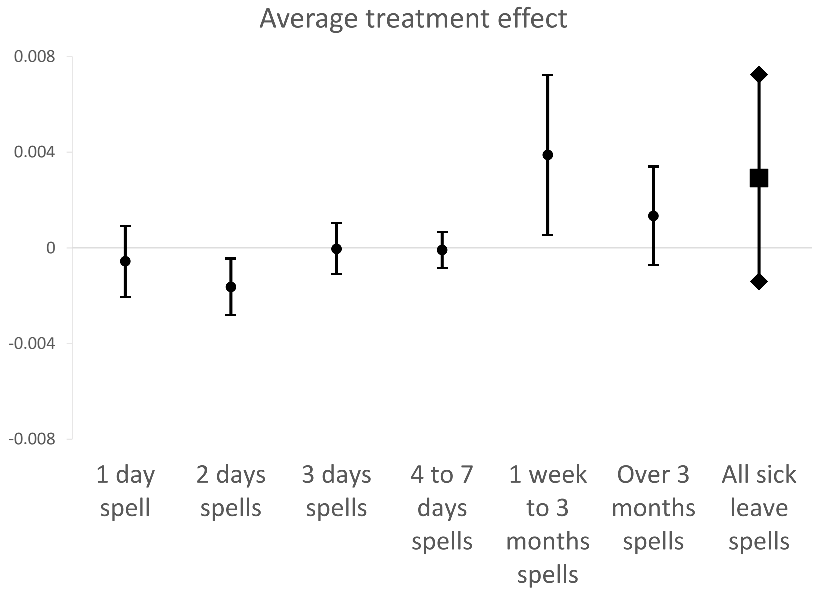
Spell category	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	over 3 m.	All
T	-0.000561	-0.00163**	-0.0000386	-0.0000902	0.00389*	0.00134	0.00292
	(0.000754)	(0.000598)	(0.000544)	(0.000387)	(0.00171)	(0.00105)	(0.00221)
Observations	741,544	741,544	741,544	741,544	741,544	741,544	741,544
R^2	0.0005	0.0004	0.0004	0.0004	0.0009	0.0010	0.0017

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3 presents the results for the most general specification. Figure 5 presents them in a graphical way with confidence intervals at 95 %. The regressions yield results that confirm what was suggested by the descriptive statistics: the impact of the policy on the prevalence for all sick leave spells is not significant.

Regarding the prevalence of short-term spells, the coefficient is negative, meaning that there are less sick leave of short length for the time of the policy. It is highly significant for 2 days spells, but neither for 1 day spells, nor for 3 days spells, nor for 4 to 7 days spells. The coefficient for 2 days spells has to be compared to the mean value of 2 days spells in a reference week before the introduction of the policy, which is 0.25 %: it implies a reduction by more than half of 2 days spells due to the one-day waiting period. For 1 day spells, the fact that the coefficient is not significant might come from substitution behaviors when reported to the employer (with day offs or other kinds of absence) as detailed above.



Source: French Labour Force Survey 2010-2014.

Figure 5: Average treatment effect on the prevalence of spells of each length category (graphical view with 95 percent confidence interval).

Regarding the prevalence of long-term spells, the coefficient is positive. It is significant only for 1 week to 3 months spells, as suggested by the descriptive statistics. The prevalence of 1 week to 3 months spells is 1.4 % before the introduction of the policy, which implies an increase by roughly a quarter due to the one-day waiting period. Since the prevalence of long-term spells is higher than the prevalence of short-term ones, the prevalence of all sick leave spells is more driven by the long-term spells, which explains why the coefficient for all sick leave spells is positive.

To gather further insight, we interact the treatment dummy with years 2012 and 2013. Results are presented in Table 4. This more detailed table yields overall the same results as the previous one. The two coefficients for 2 days spells are still significant (though at 5 % and no longer at 1 %, but there are less observations for which the interaction term is equal to one). The effects are also of the same order of magnitude each year. The main difference with the previous table lies in the coefficient for 1 week to 3 months spells, that is lower in 2012 than in 2013. It is not significant in 2012, whereas it remains significant in 2013 only. This latter point might indicate that the increase in the prevalence of long sick leave does not take place immediately

Table 4: **Average treatment effect on the prevalence of spells of each length category, for years 2012 and 2013.**

Spell category	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	over 3 m.	All
T × Year 2012	-0.000996	-0.00170*	0.000474	-0.000204	0.00268	0.00194	0.00219
	(0.000889)	(0.000683)	(0.000643)	(0.000433)	(0.00187)	(0.00117)	(0.00250)
T × Year 2013	-0.0000782	-0.00154*	-0.000607	0.0000362	0.00523*	0.000676	0.00372
	(0.000914)	(0.000675)	(0.000647)	(0.000464)	(0.00208)	(0.00118)	(0.00260)
Observations	741,544	741,544	741,544	741,544	741,544	741,544	741,544
R^2	0.0005	0.0004	0.0004	0.0004	0.0009	0.0010	0.0017

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

after the implementation of the policy, but required some time to reach its full effect, as seen in the descriptive statistics.

We also present in Appendix D the interactions of the treatment dummy with each quarter of 2012 and 2013 (see Table 13). The coefficients for 2 days spells are always negative and often significant. The coefficients for 1 week to 3 months spells are almost always positive and sometimes significant. Whereas the effect for 1 week to 3 months spells is significant for year 2013 and not for year 2012 for the yearly interaction, for the interaction with each quarter of 2012 and 2013 it is significant only for the first quarter of 2012. And a positive significant effect also appears at that quarter for over 3 months spells. This is suggestive evidence that some individuals who had started a long-term spell at the end of 2011 preferred extending it during the first quarter of 2012 rather than risking getting sick again and enduring a one-day waiting period.

The global picture put forward by the descriptive statics is hence confirmed by the econometric analysis. The presence of the one-day waiting period induces a decreased prevalence of some short-term spells, an increased prevalence of some long-term spells, and an unchanged total prevalence. The underlying mechanism suggested by these results is a decreased incidence of sick leave and an increased length of spells.

Intuitively, we have in mind three theoretical behavioral mechanisms that may simultaneously explain all or part of the observed phenomena. First, a static explanation. The one-day waiting period may be seen as a deductible: the employee pays the cost of the first day, but nothing else until 3 months in our setting. This deductible may be seen as unfair and encourages the employee to shirk, in the sense that the length of the sick leave increases. This is in line with a small body of empirical literature focusing on deductible in car insurance (Dionne and Gagné, 2001; Miyazaki, 2009; Lammers and Schiller, 2010), which finds that a higher deductible leads to a higher reported cost of car crashes[13]. Regarding the absence of effect on total prevalence of sick leave, it may come from the monetary deterrent effect of the one-day waiting period on starting a sick leave. Second, a dynamic explanation. Starting a new sick leave spell implies paying a fixed cost. Once a spell is started and the first fixed cost is paid, a forward-looking employee prefers to stay longer on sick leave so that the probability of getting sick again decreases, in order to avoid paying a second time this fixed cost. This explains our main result regarding the total prevalence and the sick leave distribution, but also why we find a slightly higher prevalence of long-term spells just after the policy is introduced: employees who started a sick leave spell just before the policy is introduced, without paying the fixed cost, are disincentived to go back to work. This explanation is moreover put forward by Johansson and Palme (2005) and Paola, Scoppa and Pupo (2014). Third, a health capital explanation. The one-day waiting period deters the employee from starting a sick leave. Consequently, the policy would induce a degradation of health capital. After a certain delay, employees are forced to stop. Their sick leave spells are longer, due to a worst heath state. This would explain[14] why the increase in 1 week to 3 months spells is observed only with a delay, contrary to the decrease in 2 days spells which is observed instantaneously.

[13]Dionne and Gagné (2001) do not mention how the cost of car accidents per individual is affected by a higher deductible, but only that the cost per car accident increases. In our case, we have more detailed results. Not only our findings are consistent with a longer spells length (whose equivalent in car insurance is an increased cost per car crash). But we also focus on the total prevalence (whose equivalent in car insurance is the average cost per individual) and we do not find a significant decrease. Hence, the classical deterrent effect of the deductible on the incidence of a sick leave (whose equivalent in car insurance is the probability of claims) would be fully offset by the increased length of sick leave spells.

[14]Note that this explanation is not in line with some studies regarding the 1997 German reform. If health capital mattered at that time in Germany too, the two opposite changes in the distribution for short and long spells would arguably be also present in this simpler reform with a uniform decrease in the replacement rate. Yet, no health degradation was observed by the studies that carefully examined it (Ziebarth and Karlsson, 2014; Puhani and Sonderhof, 2010).

3.2 Seasonal effects

Table 5: **Average treatment effect on the prevalence of spells of each length category, by calendar quarters**

Spell category	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	over 3 m.	All
T × Quarter 1	0.000204	-0.00222**	-0.00121	0.00108	0.00560**	0.00179	0.00524
	(0.00106)	(0.000799)	(0.000661)	(0.000703)	(0.00217)	(0.00119)	(0.00281)
T × Quarter 2	0.000412	-0.00116	-0.000170	-0.000159	0.00477*	0.00266	0.00636*
	(0.00103)	(0.000760)	(0.000595)	(0.000566)	(0.00223)	(0.00137)	(0.00288)
T × Quarter 3	-0.00167*	-0.00269***	0.000558	-0.000701	0.000325	0.000623	-0.00355
	(0.000811)	(0.000648)	(0.000674)	(0.000410)	(0.00200)	(0.00124)	(0.00253)
T × Quarter 4	-0.00130	-0.000591	0.000800	-0.000745	0.00419	0.000334	0.00269
	(0.00103)	(0.000747)	(0.000761)	(0.000431)	(0.00231)	(0.00116)	(0.00283)
Observations	741,544	741,544	741,544	741,544	741,544	741,544	741,544
R^2	0.0005	0.0004	0.0004	0.0004	0.0009	0.0010	0.0017

Standard errors in parentheses

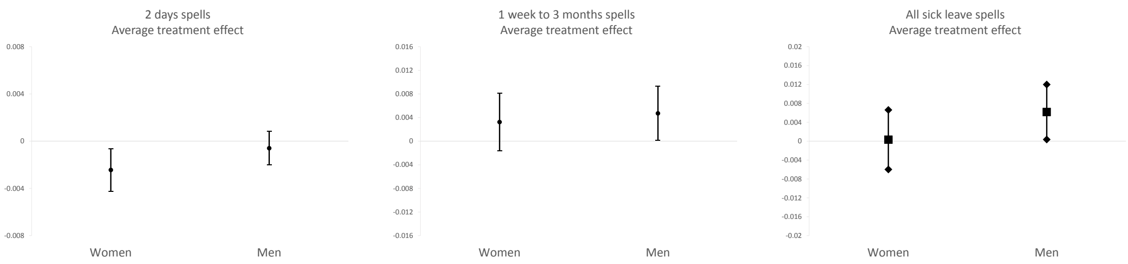
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

We also interact the treatment dummy with quarterly dummies in Table 5 to investigate for possible seasonal pattern of the impact of the policy. These regressions highlight different reactions in winter (Quarter 1) and summer (Quarter 3). In the winter, we both observe less 2 days spells and more 1 week to 3 months spells. Since total prevalence is not significantly changed, it might imply that due to the policy there is less sick leave incidence at that season, but that the duration of spells increases. In the summer, 2 days spells decreases without long-term spells increasing. Since the prevalence of long-term spells does not increase significantly, the coefficient for all sick leave spells becomes negative there. On the contrary, in spring (Quarter 2) the decrease in prevalence of short-term spells is no longer significant, whereas the increase in prevalence of 1 week to 3 months spells is still significant. This latter effect might come from spells of this category that began in Quarter 1. It leads the total prevalence of sick leave spells to become significantly positive. No effect at all is found in fall (Quarter 4).

Hence there is a clear difference between winter, where both 2 days spells and 1 week to 3 months spells react, and summer, where only 2 days spells reacts. The explanation may come from general health, which is usually better in summer. It might consequently be easier to work with the symptoms of short benign diseases in summer.

3.3 Heterogeneous effects

In the following, we explore possible heterogeneous effects by gender, age and wage. We present in Figure 6 the effects by gender for 2 days and 1 week to 3 months spells (the two categories than react the most for the full sample) and for all sick leave spells. Exhaustive results for all lengths categories are presented in Appendix E.



Source: French Labour Force Survey 2010-2014.

Figure 6: Average treatment effect on women and men, for 2 days spells (left), 1 week to 3 months spells (middle) and total prevalence (right), with 95 percent confidence interval.

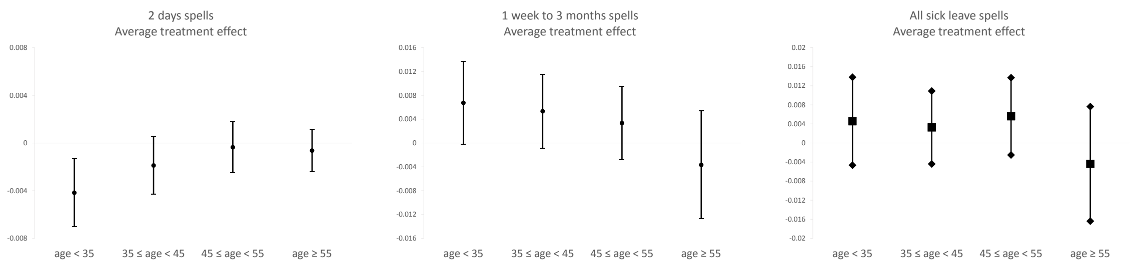
The coefficients have the same sign when they are estimated separately on men and women. The policy would decrease the prevalence of 2 days spells, and would increase the prevalence of 1 week to 3 months spells. The effect on short-term spells is stronger for women than for men (and significant only for women), whereas the effect on long-term spells is higher for men (and significant only for men)[15]. Moreover, for men the policy leads to a significant increase (at the 5 % level) in the total prevalence of sick leave spells whereas this total increase is much weaker and not significant for women.

Comparing this result with Pettersson-Lidbom and Thoursie (2013) whose studied change includes a removal of a one-day waiting period, they had the opposite picture. Swedish men increased their number of short-term spells more than their female counterparts, and those latter

[15]Additional regressions in which the treatment dummy is interacted with gender (not represented) however show that these differences are not statistically significant.

decreased more their long-term spells. They made the hypothesis that this could be attributed to the fact that women were more present in the hospital sector, where there is almost no flexibility regarding short-term spells. Since we are focusing on the central civil service, it is not the case in our sample. Moreover, they studied both a removal of a one-day waiting period and an increase in replacement rates. More generally, results on gender differences regarding behavioral responses to monetary incentives for sickness absence are far from being unanimous in the literature. Some authors find than men react more strongly than women (Johansson and Palme, 2005; Ziebarth and Karlsson, 2014; Ben Halima, Elbaz and Koubi, 2016) whereas others find the opposite (Paola, Scoppa and Pupo, 2014) or find no difference (Puhani and Sonderhof, 2010).

We then present in Figure 7 the same effects estimated on each age groups. As found in Puhani and Sonderhof (2010), the response is much stronger in the younger part of the sample. For both 2 days and 1 week to 3 months spells, we find that the response is all the more stronger as the age group decreases[16], with responses being highly significant only for employees under 35. Since these two effects go in the opposite direction, there is no clear pattern for all sick leave spells. The effect on total prevalence is never significant.



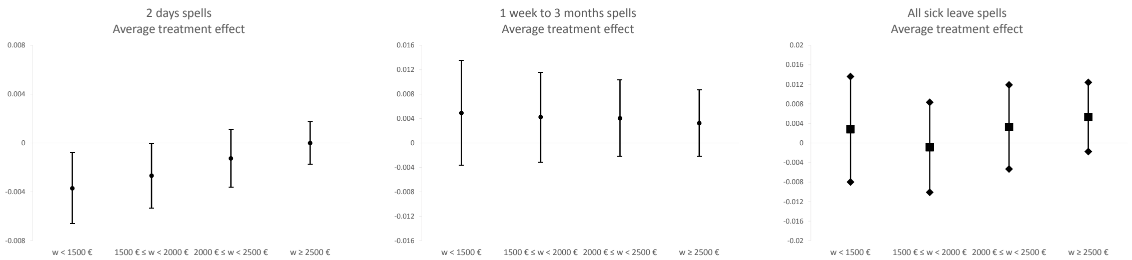
Source: French Labour Force Survey 2010-2014.

Figure 7: Average treatment effect on four age groups, for 2 days spells (left), 1 week to 3 months spells (middle) and total prevalence (right), with 95 percent confidence interval.

By presenting the same effects over four wage groups in Figure 8, we get a similar picture for

[16]Additionnal regressions in which the treatment dummy is interacted with age groups (not represented) show these differences are statistically significant for 2 days spells when comparing the first and last group.

2 days spells. The lower the wage, the higher the effect on the prevalence of 2 days spells[17]. The effect on total prevalence is also here never significant.



Source: French Labour Force Survey 2010-2014.

Figure 8: Average treatment effect on four wage groups, for 2 days spells (left), 1 week to 3 months spells (middle) and total prevalence (right), with 95 percent confidence interval.

Finally, since teachers account for 44 % of the central civil service, we also investigated for a possible heterogeneous effect on teachers and non-teachers of the central civil service (see Appendix E)[18]. Coefficients are hardly more important for teachers than for non-teachers.

In sum, we find that the reaction on 2 days spells is significant for women whereas the reaction on 1 week to 3 months spells is significant for men. As a whole, the policy has a significant positive impact on men's total prevalence of sick leave. The reaction on both 2 days and 1 week to 3 months spells is decreasing with age. The reaction on 2 days spells is decreasing with wage, but not the reaction on 1 week to 3 months spells.

Note that groups considered may partially overlap. Table 6 shows the correlations between the groups. They are all quite weak, which suggests that all observed heterogeneous reactions exist independently from each other.

Several reasons might explain why responses to the one-day waiting period policy along with

[17] Additional regressions in which the treatment dummy is interacted with wage groups (not represented) show these differences are statistically significant for 2 days spells when comparing the first and last group.

[18] Note that due to the limited size of the sample of teachers in the private sector, we took here the whole private sector as a counterfactual for both the teachers and non-teachers of the central civil service.

Table 6: **Correlation matrix of socio-demographic characteristics for the employees of the central civil service**

Characteristics	Women	Age under 45	Wage under 2000 €	Teachers
Women	1	-0.01	0.19	0.20
Age under 45	-0.01	1	0.28	0.06
Wage under 2000 €	0.19	0.28	1	-0.05
Teachers	0.20	0.06	-0.05	1

Source: French Labour Force Survey 2010-2014.

age, wage and gender are heterogeneous. For age, many explanations may account for such heterogeneity: either that the young have intrinsically more latitude to choose between work and sick leave when confronted with a disease, because they can more easily work with its symptoms, or that reputation costs that might be related to absenteeism could be higher for them, or that there are perhaps generation effects, or that old employees may have some inertia to adapt to new incentive schemes. For wage, although the absolute loss due to the one-day waiting period is proportional to the wage[19], monetary incentives are likely to be more effective on individuals with more liquidity constraints (that would be correlated with lower wages). Note that the young usually have lower wages than older employees, and that this correlation might mutually reinforce these effects. We eventually find that the policy affected the short-term spells prevalence for women, and the long-term spells prevalence for men (even resulting in the significant increase in total prevalence for men). However, these gender differences should be taken with caution since cross-effects with gender are not significant in our econometric specifications and that in other contexts the opposite conclusion was drawn.

3.4 Switch on and switch off

A notable feature of the policy under study is its short time of implementation, since it was repealed exactly two years after its implementation. As noticed by Puhani and Sonderhof (2010), such a feature is of particular interest in a difference-in-differences approach. Indeed, this approach relies on the common trend assumption. Without treatment, the two groups are assumed to evolve similarly. With a single switch, it can be difficult to test whether any estimated effect

[19]The relative wage loss is independent of the wage level.

could come from a violation of this assumption. For instance, even if the studied policy had no real effect, a significant estimated effect might come from another simultaneous event going in the same direction, and of which the researchers would be unaware. When the studied policy implies both a switch on and a switch off, we can be more confident in the fact that we have effectively assessed the causal impact of the policy, if we find that both estimated effects have opposite signs and are of similar magnitude. In the latter example, it is indeed more unlikely that for both the switch on and the switch off, there would be two simultaneous events going in opposite direction, and of which the researchers would be unaware.

Table 7: **Average treatment effects for the switch on and the switch off**

	2 days spells				
	Women	Men	Age < 45	w < 2000 €	Whole
Switch on (2012 *v.* 2011)	-0.00191	-0.000903	-0.00267*	-0.00346**	-0.00144
	(0.00122)	(0.00117)	(0.00121)	(0.00128)	(0.000845)
Switch off (2014 *v.* 2013)	0.00310*	0.000198	0.00279*	0.00262	0.00187*
	(0.00143)	(0.000686)	(0.00139)	(0.00159)	(0.000849)
	1 week to 3 months spells				
	Women	Men	Age < 45	w < 2000 €	Whole
Switch on (2012 *v.* 2011)	0.00228	0.00140	0.00293	0.00311	0.00197
	(0.00308)	(0.00296)	(0.00291)	(0.00329)	(0.00214)
Switch off (2014 *v.* 2013)	-0.00424	-0.00792*	-0.00868*	-0.00766	-0.00574*
	(0.00396)	(0.00372)	(0.00384)	(0.00493)	(0.00275)
	All sick leave spells				
	Women	Men	Age < 45	w < 2000 €	Whole
Switch on (2012 *v.* 2011)	0.000471	0.00417	0.00139	-0.000989	0.00226
	(0.00421)	(0.00410)	(0.00399)	(0.00457)	(0.00294)
Switch off (2014 *v.* 2013)	-0.000453	-0.00736	-0.00660	-0.00344	-0.00335
	(0.00491)	(0.00432)	(0.00439)	(0.00565)	(0.00333)

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Using two successive years, effects estimated separately on the switch off and the switch on are presented in Table 7. They are presented for the whole population and for the various classes that were found to be the most reactive in the previous section. At first glance, all coefficients have the expected sign. For 2 days spells, the coefficient is negative between 2011 and 2012 and positive between 2014 and 2013. On the whole population, the two coefficients are only significant for the switch off (between 2013 and 2014) and are not significant for the switch on (between 2011 and 2012). But they are of similar magnitude (and significant) on some classes such as the young and the low-income for the switch on. The fact that no coefficient of the switch on is significant for 1 week to 3 months spells is consistent with Table 4, where we found an effect only in 2013 and not in 2012. Reaction on long-term spells may require some time after the implementation of the policy.

4 Robustness tests

4.1 Placebo test

Similarly to what was conducted regarding the switch on and the switch off, an additional robustness check is to test whether the time pattern is similar in the two sectors during the pre-reform period (between 2010 and 2011). Such test is often used to dismiss the existence of diverging trends that may bias the results. Under the common trend assumption, we expect to find no significant results in these tests.

Table 8: **Average treatment effects in placebo tests**

	Before the policy is in place (2011 *v.* 2010)				
	Women	Men	Age < 45	w < 2000 €	Whole
2 days spells	-0.000566	0.0000204	0.000421	0.000167	-0.000302
	(0.00153)	(0.000950)	(0.00125)	(0.00162)	(0.000942)
1 week to 3 months spells	-0.00225	0.00249	0.00497	0.000934	0.00000846
	(0.00332)	(0.00311)	(0.00309)	(0.00360)	(0.00230)
All sick leave spells	-0.00386	0.00321	0.00712	0.00357	-0.000470
	(0.00469)	(0.00380)	(0.00403)	(0.00466)	(0.00309)

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Results are presented in Table 8. Regarding all sick leave spells but also 2 days and 1 week to 3 months spells, coefficients are sometimes positive and sometimes negative depending on the category of employees, and they are never significant.

4.2 Speed of reaction

Proceeding similarly to sections 3.4 and 4.1, we can test whether there is a delayed effect of the policy. We test if there are any significant differences between the two successive years during which the one-day waiting period applies (between 2012 and 2013). Notably, we want to check if the delayed effect on 1 week to 3 months spells found in 4 is also found by considering only the years during which the policy applies.

Table 9: **Average treatment effects in speed of reaction tests**

	While the policy is in place (2013 *v.* 2012)				
	Women	Men	Age < 45	w < 2000 €	Whole
2 days spells	0.000672	0.0000662	0.000656	0.000743	0.000337
	(0.000990)	(0.00118)	(0.00102)	(0.00116)	(0.000758)
1 week to 3 months spells	0.00404	-0.000340	0.00423	0.0106^{**}	0.00194
	(0.00332)	(0.00338)	(0.00329)	(0.00364)	(0.00237)
All sick leave spells	0.00343	-0.00213	0.00209	0.00709	0.000757
	(0.00450)	(0.00432)	(0.00443)	(0.00477)	(0.00313)

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Results are presented in Table 9. Regarding 2 days spells, coefficients are never significant. Hence, there is no clue of a delayed effect of the one-day waiting period on 2 days spells.

Regarding 1 week to 3 months spells, we first find that the coefficient for the whole population is not significant. But one of the coefficients is significant (the one regarding wages under 2000 €). We can note that the category of employees who experience this significant increase between 2012 and 2013 are those who react the more to the introduction of the policy in term of a decrease of 2 days spells. This might be an additional suggestive evidence that part of the effect regarding long-term spells might require some time to take effect, and that it goes hand in hand with the effect regarding short-term sick leave (which, in contrast, takes effect immediately).

4.3 Alternative specifications

As robustness tests, we consider alternative specifications for our two main results, the effect of the policy for 2 days and 1 week to 3 months spells. Having only pooled OLS or fixed effects without controls does not change the sign of the effect (see Table 10). Including or not our various controls with employees fixed effects has almost no impact: time variant controls are not likely to bring much information in addition to the time invariant employee fixed effect, as we observe individuals during at most 6 quarters.

Table 10: **Average treatment effects with different econometric specifications**

	2 days spells			1 week to 3 months spells		
Specification	Pooled OLS	FE	FE + controls	Pooled OLS	FE	FE + controls
T	-0.00102**	-0.00162**	-0.00163**	0.000949	0.00394*	0.00389*
	(0.000346)	(0.000598)	(0.000598)	(0.000994)	(0.00171)	(0.00171)
Robust SE	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects		Yes	Yes		Yes	Yes
Clustering		Yes	Yes		Yes	Yes
Controls			Yes			Yes
Observations	741,544	741,544	741,544	741,544	741,544	741,544
R^2	0.0002	0.0003	0.0004	0.0004	0.0004	0.0009

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

4.4 Weights

In all the previous regressions, weights were used. Though the use of weights is clearly needed to get proper descriptive statistics from a survey, there is a debate within statisticians whether they should be used for regressions (see Davezies and D'Haultfœuille (2009), Solon, Haider and Wooldridge (2015)). One way to deal with this issue consists in comparing regressions with and without weights, in order to see how much it matters. In our case, results are not very sensitive to the inclusion of the weights (see Table 11).

Table 11: **Average treatment effects with and without using the weights**

	2 days spells		1 week to 3 months spells	
Weighting	No	Yes	No	Yes
T	-0.00171**	-0.00163**	0.00284	0.00389*
	(0.000606)	(0.000598)	(0.00159)	(0.00171)
Observations	741,544	741,544	741,544	741,544
R^2	0.0004	0.0004	0.0008	0.0009

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

5 Conclusion

Our results provide further support to the thesis that the pattern of sick pay matters for health-related absence. We hence conclude to the presence of moral hazard. The length distribution of sick leave is impacted, not the total prevalence.

Whereas the usual theoretical prediction in a static framework is that there is a trade-off between coverage and incentive, we do not find that the one-day waiting period, which represents a less generous coverage, conducts to a decrease in the total prevalence of sick leave. If anything, it increases total prevalence, even if this increase is not significant in most regressions (it is significant on the one hand for men and on the other hand for spring). Despite this lack of significant effect on total prevalence, we find that this monetary incentive alters the length distribution of sick leave spells. The policy leads to a significant increase in the prevalence of 2 days spells and to a significantly decrease in the prevalence of 1 week to 3 months spells. The corresponding changes are of a large magnitude. We find a decrease by more than half in 2 days spells and an increase by roughly a quarter in 1 week to 3 months spells. These two effects of the one-day waiting period go into opposite directions, which results in the stability of the total prevalence level. Whether the one-day waiting period may or may not have any impact on productivity is unclear, since the partition of a same level of absence between short-term absence and long-term absence could also matter to this respect.

In addition to these findings, we also document heterogeneous effects of the one-day waiting period along with gender, age, and wage and across seasons. Men significantly increase their 1 week to 3 months spells, whereas women significantly decrease their 2 days spells. Younger employees decrease more their 2 days spells and increase more their 1 week to 3 months spells than older ones. Low-wage employees decreased more their 2 days spells than high-wage employees. Short-term spells decreased in both winter and summer but are offset by more long-term spells in winter only. This might suggest to make the sick pay pattern vary with some selected individual characteristics or even seasons.

Our main findings are consistent with results previously found in few other papers based on quasi-natural experiments and focusing on the existence of a waiting period (Davezies and Toulemon, 2015; Pettersson-Lidbom and Thoursie, 2013; Voss, Floderus and Diderichsen, 2001) or on the implementation of similar schemes involving the replacement rate (Johansson and Palme, 2005; Paola, Scoppa and Pupo, 2014). We thus contribute to the assessment of the external validity of these results. It seems that such opposite effects regarding the prevalence of on the one hand short-term spells and on the other long-term spells are observed when the change introduces a locally decreasing marginal cost over the length of sick leave, that is a locally increasing marginal coverage. On the contrary, these opposite effects are not observed when the coverage remained monotonically decreasing with the length of sick leave (as in Ziebarth and Karlsson (2010, 2014); Puhani and Sonderhof (2010); Goerke and Pannenberg (2015)).

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Appendices

A Questionnaire of the French Labour Force Survey on sick leave

Individuals below 75 years old are first asked if during the reference week they have made at least one hour of paid work. If not, they are asked if they have nevertheless a job.

In case they have not worked but they have a job, they are asked why they have not worked on the reference week. Possible answers include:

- paid day-off -including days for the reduction of the working time ("RTT").
- sick leave (including sick child leave) or work accident
- maternity or paternity leave
- part time
- parental leave
- other unpaid day-off
- paid learning sessions under an apprentice contract
- part time unemployment
- dismissal or firing
- strike
- weather conditions
- not enough activity for seasonal jobs

If they are on sick leave or on leave related to work accident, they are also asked what is the total expected length of the leave.

In case they have worked during the reference week, they are asked if they have taken day-offs during the reference week. As for those who have not worked at all during the reference week but are employed, it is possible to determine if those day offs are:

- ordinary day offs,
- unusual day offs,
- bank holiday,
- extra day offs granted by the employer in relation to bank holiday,
- resting day offs,
- unpaid day offs (such as unpaid leave, parental leave, and so on).

Then, those that have worked during the reference week are also asked if they have been absent for sickness or work accident, and if yes, how many days during the reference week.

These two flows of questions related to sick leave provide two sick leave lengths of different meaning. For those who have worked at last one hour and who have been on sick leave during the reference week (we will say they are on short-term sick leave), it is the length of this leave during the reference week. Information may be consequently right-censored or left-censored. For those who have not work at all during the reference week and who are on sick leave (we will say that they are on long-term sick leave), the information is neither right- or left- censored, but the length is an estimated length of the current sick leave at the time of the interrogation.

This part of the questionnaire remains almost intact for the years 2006-2015. The most notable evolution in 2013 concerns how the length of the leave is measured (for those who have worked during the reference week). Up to 2012, it was measured in days or hours. After 2013, it is measured in days or half-days.

A2 **Nous allons parler de la semaine du lundi... au dimanche...** **TRAREF**
Pendant cette semaine-là, avez-vous effectué au moins une heure de travail rémunéré ?

1. Oui → ACTOP=1, ACTOPREM=1 puis A11
2. Non

Pour ceux qui n'ont pas effectué au moins une heure de travail rémunéré durant la semaine de référence
TRAREF=2

A3a **Avez-vous cependant un emploi rémunéré ?** **PASTRA**

1. Oui
2. Non → ACTOPREM=2 puis A8

Pour ceux qui n'ont pas travaillé durant la semaine de référence mais ont cependant un emploi rémunéré
TRAREF=2 et PASTRA=1

A3b **Pourquoi n'avez-vous pas travaillé cette semaine là ?** **RABS**

1. Congé rémunéré (y compris RTT ou repos compensateur) → ACTOP=1, ACTOPREM=1 puis A11
2. Congé maladie (y compris enfants malades) ou accident du travail → A4.
3. Congé de maternité / paternité → ACTOP=1, ACTOPREM=1 puis A11
4. Temps partiel → ACTOP=1, ACTOPREM=1 puis A11
5. Congé parental → A4.
6. Autres types de congés non rémunérés → A4.
7. Formation rémunérée par l'employeur ou dans le cadre d'un contrat en alternance ou en apprentissage → A4.
8. Chômage partiel (chômage technique) → A5.
9. Mise à pied, période de fin d'emploi → A4.
10. Grève → A5.
11. Période de morte saison dans le cadre d'une activité de saisonnier ou période précédant le début d'emploi
12. Intempéries → ACTOP=1, ACTOPREM=1 puis A11

Pour ceux qui étaient en congé maladie ou accident du travail, en congé parental, en congé non rémunéré, en formation rémunérée ou en période de fin d'emploi durant la semaine de référence.
RABS=2,5,6,7,9

A4 **Au total, combien de temps dure ce congé maladie / ce congé parental / ce congé non rémunéré / cette formation / cette période de fin d'emploi ?** **RABSPA**

Précisez l'unité de temps avec les initiales **A**nnées/**M**ois/**S**emaines/**J**ours → A6

Figure 9: French Labour Force Survey 2013, extract of the questionnaire related to long-term sick leave spells.

BC15a **La semaine du lundi... au dimanche..., avez-vous été absent pour maladie ou accident du travail ?** **EMPABS**

1. Oui
2. Non → BC16

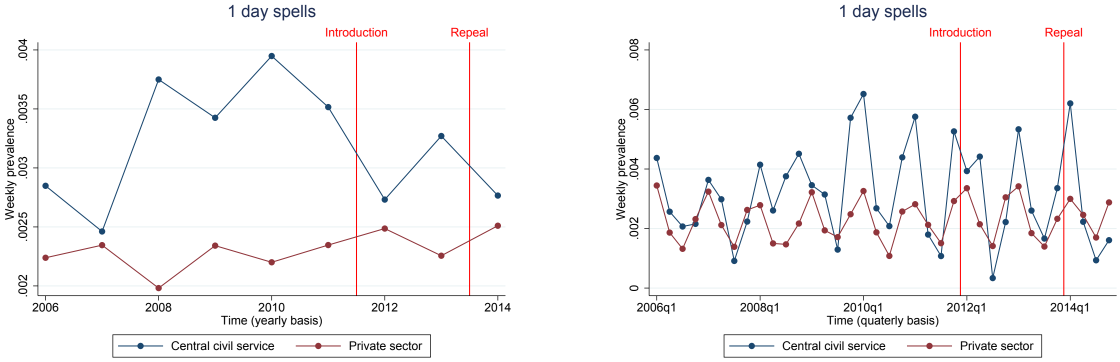
Pour ceux qui ont été absents pour maladie ou accident du travail la semaine de référence
EMPABS=1

BC15b **Combien de jours a duré cette absence ?** **EMPANH**

.... nombre de jours (0,5 à 7)

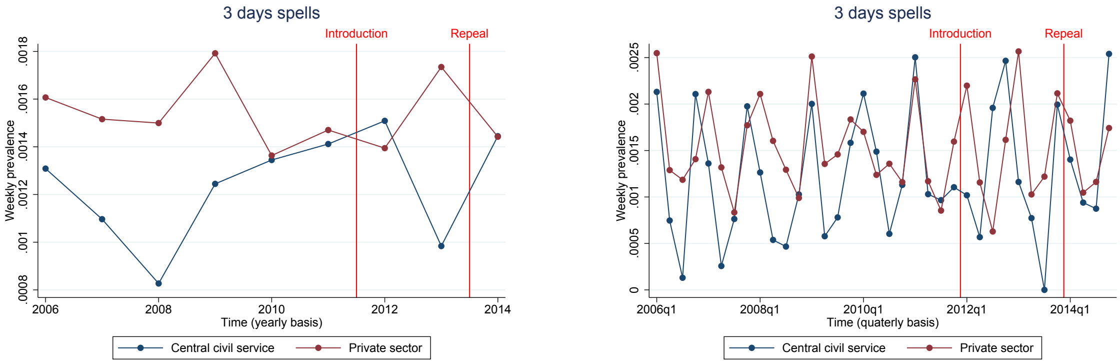
Figure 10: French Labour Force Survey 2013, extract of the questionnaire related to short-term sick leave spells.

B Prevalence of sick leave spells over time and sector, by length category



Source: French Labour Force Survey 2006-2014.

Figure 11: Prevalence of 1 day spells by sector, at a yearly (left) and quarterly (right) basis.

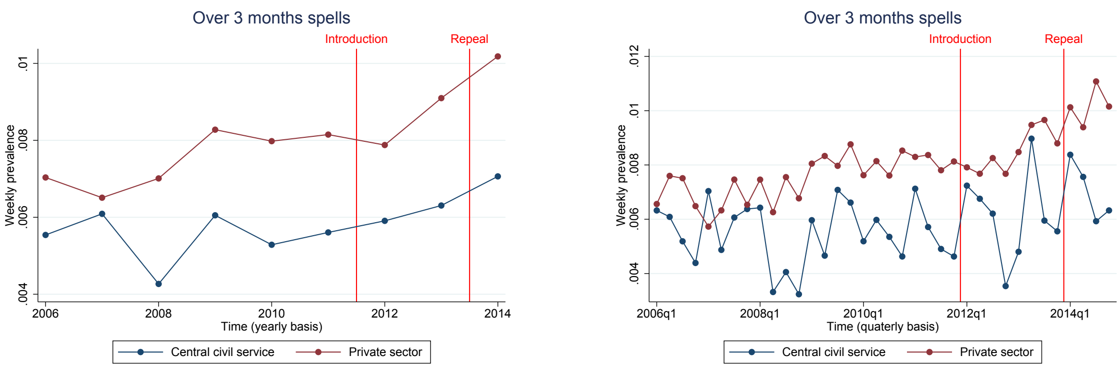


Source: French Labour Force Survey 2006-2014.

Figure 12: Prevalence of 3 days spells by sector, at a yearly (left) and quarterly (right) basis.

Source: French Labour Force Survey 2006-2014.

Figure 13: Prevalence of 4 to 7 days spells by sector, at a yearly (left) and quarterly (right) basis.



Source: French Labour Force Survey 2006-2014.

Figure 14: Prevalence of over 3 months spells by sector, at a yearly (left) and quarterly (right) basis.

C Linear and non-linear specifications

Table 12: **Comparison between linear and non-linear specifications**

	Dependant variable: all sick leave spells		
	OLS	Logit	Probit
Central civil service	-0.00389***	-0.128***	-0.0547***
	(0.000795)	(0.0269)	(0.0117)
Woman	0.00914***	0.270***	0.121***
	(0.000460)	(0.0137)	(0.00602)
Age	0.000362***	0.0104***	0.00455***
	(0.0000218)	(0.000642)	(0.000280)
Highly educated (degree level)	-0.0149***	-0.539***	-0.233***
	(0.000565)	(0.0243)	(0.0102)
Teacher	0.00173	0.0743	0.0351*
	(0.00106)	(0.0384)	(0.0167)
Permanent contract or civil servant	0.0112***	0.406***	0.171***
	(0.000626)	(0.0255)	(0.0107)
Being in a couple	-0.00220***	-0.0646***	-0.0261***
	(0.000590)	(0.0168)	(0.00737)
Having a child	-0.00319***	-0.0825***	-0.0367***
	(0.000529)	(0.0152)	(0.00665)
Year 2010	ref	ref	ref
Year 2011	0.00121	0.0364	0.0157
	(0.000703)	(0.0215)	(0.00940)
Year 2012	-0.000178	-0.00581	-0.00251
	(0.000696)	(0.0216)	(0.00946)
Year 2013	0.00175*	0.0522*	0.0229*
	(0.000726)	(0.0220)	(0.00963)
Year 2014	0.00251***	0.0741***	0.0329***
	(0.000736)	(0.0220)	(0.00968)
Quarter 1	ref	ref	ref
Quarter 2	-0.00535***	-0.152***	-0.0675***
	(0.000655)	(0.0187)	(0.00826)
Quarter 3	-0.0103***	-0.315***	-0.139***
	(0.000637)	(0.0196)	(0.00856)
Quarter 4	-0.00348***	-0.0966***	-0.0429***
	(0.000665)	(0.0185)	(0.00821)
Observations	741,544	741,544	741,544
Goodness-of-fit	$R^2 = 0.0032$	Pseudo $R^2 = 0.0111$	Pseudo $R^2 = 0.0111$

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

D Average Treatment Effects for quarters of years 2012 and 2013

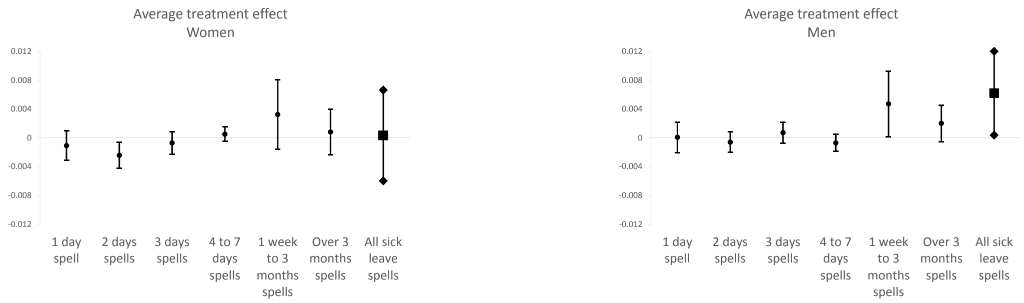
Table 13: **Average treatment effect on the prevalence of spells of each length category, for quarters of years 2012 and 2013**

Spell category	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	over 3 m.	All
T × 2012 Q1	-0.000458	-0.00222*	-0.000976	0.00129	0.00593*	0.00304*	0.00661
	(0.00135)	(0.000997)	(0.000797)	(0.000907)	(0.00262)	(0.00139)	(0.00348)
T × 2012 Q2	0.00133	-0.00165	-0.0000533	-0.000182	0.00275	0.00307	0.00527
	(0.00146)	(0.000935)	(0.000661)	(0.000727)	(0.00269)	(0.00158)	(0.00357)
T × 2012 Q3	-0.00272**	-0.00270***	0.00182	-0.00126*	-0.00223	0.00161	-0.00547
	(0.000899)	(0.000753)	(0.00104)	(0.000559)	(0.00227)	(0.00163)	(0.00309)
T × 2012 Q4	-0.00274*	-0.000775	0.00135	-0.000468	0.00163	0.0000504	-0.000952
	(0.00127)	(0.000982)	(0.00105)	(0.000633)	(0.00281)	(0.00157)	(0.00366)
T × 2013 Q1	0.000213	-0.00229*	-0.00104	0.000838	0.00378	0.000422	0.00192
	(0.00152)	(0.00111)	(0.000883)	(0.00107)	(0.00309)	(0.00165)	(0.00400)
T × 2013 Q2	-0.00121	-0.000669	0.0000577	-0.000162	0.00551	0.00189	0.00542
	(0.00119)	(0.000998)	(0.000832)	(0.000821)	(0.00307)	(0.00190)	(0.00394)
T × 2013 Q3	-0.00113	-0.00268***	-0.000525	-0.000134	0.00188	-0.000963	-0.00355
	(0.00112)	(0.000731)	(0.000590)	(0.000544)	(0.00271)	(0.00157)	(0.00332)
T × 2013 Q4	-0.000200	-0.000344	0.000343	-0.00108*	0.00621	-0.0000538	0.00487
	(0.00137)	(0.000903)	(0.000949)	(0.000486)	(0.00317)	(0.00137)	(0.00371)
Observations	741,544	741,544	741,544	741,544	741,544	741,544	741544
R^2	0.0005	0.0004	0.0004	0.0004	0.0009	0.0010	0.0017

Standard errors in parentheses

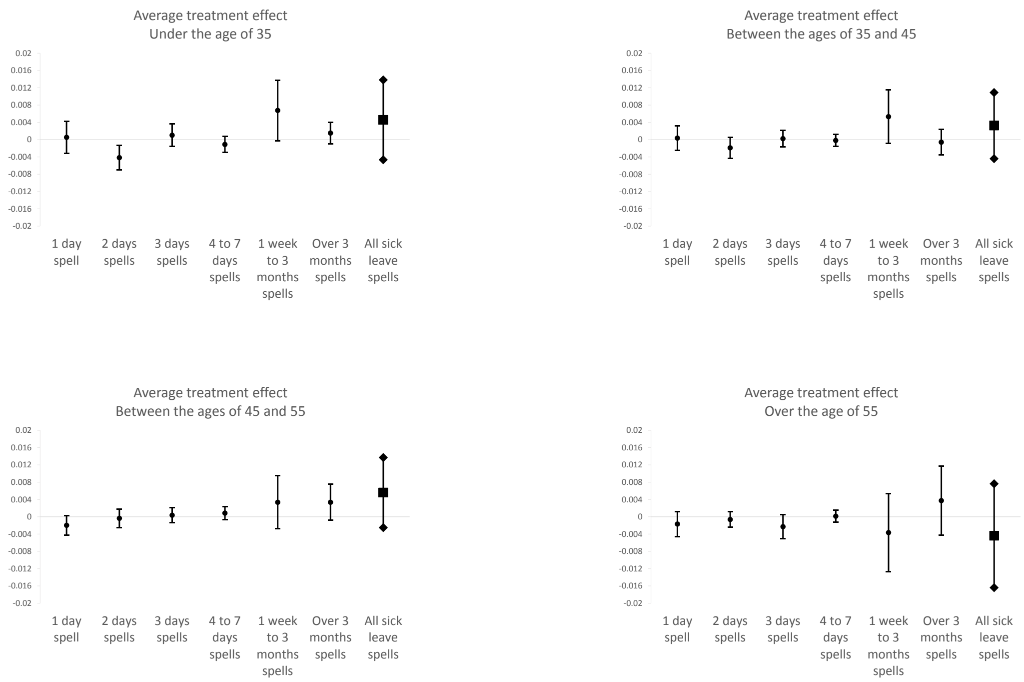
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

E Average treatment effects for various sub-populations



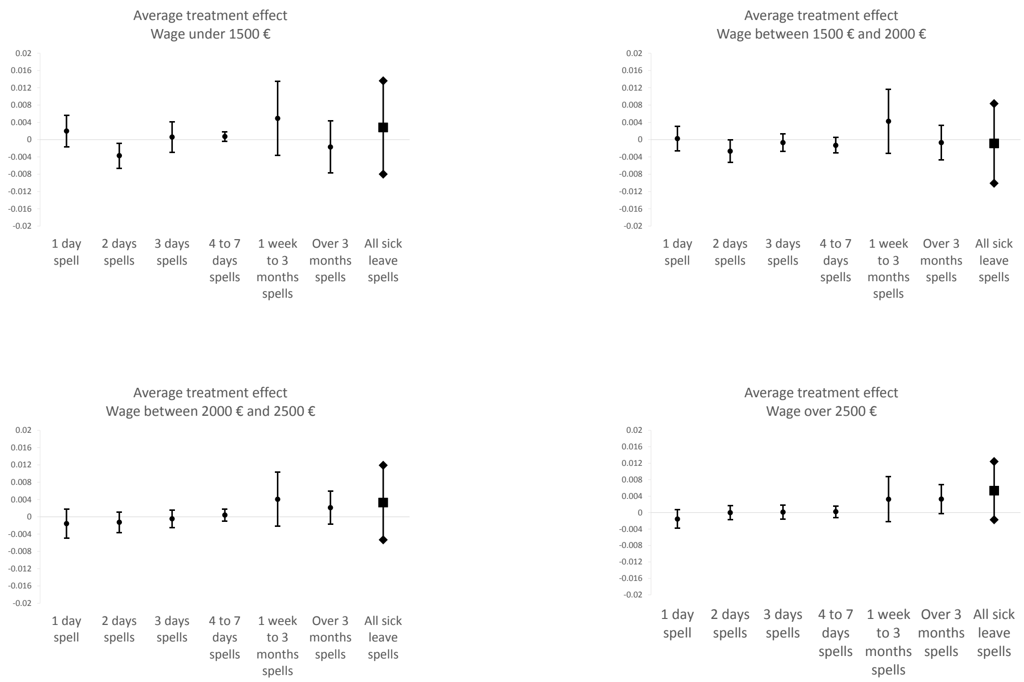
Source: French Labour Force Survey 2010-2014.

Figure 15: Average treatment effects on women and men



Source: French Labour Force Survey 2010-2014.

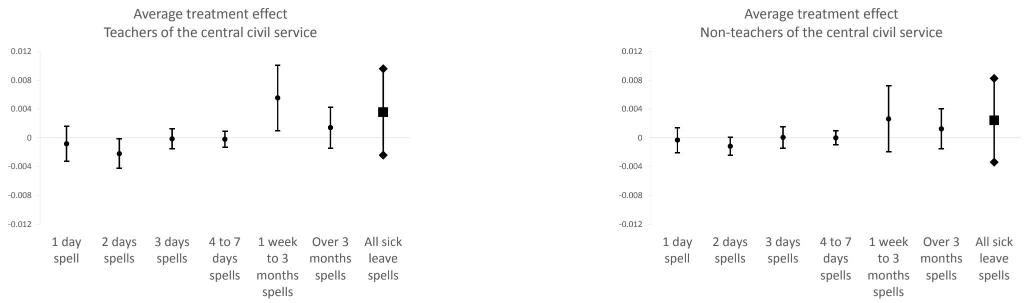
Figure 16: Average treatment effects on different age groups



Source: French Labour Force Survey 2010-2014.

Figure 17: Average treatment effects on different wage groups

F Average treatment effects for teachers and non-teachers



Source: French Labour Force Survey 2010-2014.

Figure 18: Average treatment effects on teachers and non-teachers