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Les effets d'un jour de carence pour arrêt maladie sur les absences pour raison de santé dans la fonction publique de l'État française

Résumé

La modulation du remboursement des congés maladie a souvent été utilisée pour réduire les absences pour raison de santé. Nous étudions les effets de la présence d'un jour de carence pour arrêt maladie. Cette politique moins généreuse a été introduite dans la fonction publique de l'État en France en janvier 2012 et abrogée en janvier 2014, alors que le secteur privé n'a pas été affecté. Nous employons une stratégie de différence de différences avec effets fixes individuels, en utilisant l'enquête Emploi. Nous constatons que cette politique ne modifie pas la prévalence totale des absences pour raison de santé.

Elle affecte en revanche la distribution par durée de ces arrêts. La prévalence d'arrêts courts diminue, alors que la prévalence d'arrêts longs augmente. La diminution des absences de courte durée est plus élevée parmi les femmes, les employés jeunes, et ceux travaillant seulement quelques jours par semaine. Les effets sont hétérogènes selon les saisons : les arrêts courts diminuent et les arrêts longs augmentent tant en hiver qu'en été, mais pas au printemps ni à l'automne.

Mots-clés : Absentéisme, congé maladie, jour de carence, incitations monétaires, différence de différences, secteur public

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Effects of the one-day waiting period for sick leave on health-related absences in the French central civil service

Abstract

Modulation of sick leave reimbursement scheme has often been used in attempt to reduce 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 This less generous policy was introduced in the French central civil service in January 2012 and repealed in January 2014, whereas the private sector was not affected.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 health-related absences is not affected by the policy.

However, its duration distribution is. The prevalence of short-term absences decreases, while the prevalence of long-term absences increases. Decrease in short-term absences is higher for women, young employees and those working few days per week. Effects are also heterogeneous across seasons: the effects on both short- and long-term absences are significant in winter and summer, but neither in spring nor in autumn.

Keywords: Absenteeism, Monetary incentives, Sickness pay, Difference-in-differences, Public sector

Classification JEL : D82, I18, J22, J45

The question of how health-related absenteeism reacts to the generosity of reimbursement patterns remains an empirical concern, due to the social costs 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 have some latitude regarding both whether she starts a sick leave and how long this leave lasts. 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 decreases (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 the number of short-term spells. But it can also induce an increase in the duration 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 health-related absences, defined as the proportion of employees on leave for health-related reasons. We also differentiate the effects between the leave durations. For that purpose, we exploit two exogenous changes in sick leave pay in the French civil service. On 1st January 2012, the French government introduced a one-day waiting period for all workers in the French civil service to combat absenteeism. On 1st January 2014, exactly 2 years later, the following government repealed the measure. Both the exogenous introduction of this one-day waiting period and its exogenous repeal create an ideal quasi-natural experiment to assess this component of a sick pay scheme.

We apply a difference-in-differences strategy between the employees of the French central

[1] See Pollak (2017) for a review of this literature.

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 hospital and territorial civil services, the two other parts of the French civil service, other monetary incentives related to work attendance exist. The characteristics and the timing of implementation of these other incentives differ greatly between public institutions and over time. Second, in the local civil service, the timing of implementation of the one-day waiting period also varied 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 a rotating panel: the French Labour Force Survey, from 2010 to 2014. Transitions between the treatment and control groups are rare. These ensures that there is no self-selection into groups based on treatment variation. We also include individual fixed effects. This makes the estimation consistency more plausible in the case where non-response to the survey affects the common trend assumption, as shown by Lechner, Rodriguez-Planas and Kranz (2016).

We obtain four different results. First, we do not find that the one-day waiting period decreases health-related absences. If anything, it increases them (this increase is significant for employees aged 45-55). Second, it leads however to a change in the duration distribution of these absences. Short-term absenteeism decreases while long-term absenteeism increases. More precisely, we find that there is a significant decrease of more than 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, effects differ across sociodemographic characteristics and working conditions. The decrease in short-term absences is higher for women, young employees and those working few days per week. In addition, considering each gender group separately leads to opposite results regarding the significance of short- and long-term effects: only the decrease in short-term absenteeism is significant for women, while only the increase in long-term absenteeism is significant for men. Fourth, effects also differ across seasons. The effects on both short- and long-term spells is significant in winter and summer. No effect is found in intermediate seasons.

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 absences. On the contrary, administrative data[2] focus on absences for which a medical certificate is provided. In both cases, these absences may be subject to prevention efforts to avoid getting sick, which corresponds to *ex ante* moral hazard, and to several layers of hidden actions after the employee knows she is sick, which corresponds to *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 absences, 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 in order to control their expenditure, our estimates based on survey data offer a more precise measure of the real impact on health-related absences. 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 for instance Pettersson-Lidbom and Thoursie (2013) get 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 health-related absences. Third, we include individual fixed effects 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 sick pay 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 duration 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 1st January 2012 introduction and the 1st 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 3 months of their sick leave[3]. After that threshold, the replacement rate fell to 50 % of their wages (except if they took out an optional additional coverage). Hence they enjoyed a full coverage for sick leave before that threshold, and a partial coverage after (except optional full coverage).

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 28th December 2011) by the right-wing ruling party for reasons of equity with respect to the private sector and also to reduce absenteeism. This monetary incentive was strong since it makes the replacement rate fall on the first day from 100 % to 0 %[4]. The measure was effective on the 1st 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[5].

Implementation details were specified by a circular dated 24th February 2012. The policy concerns neither work accident leave, neither the so called "long duration" 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

[3]This applies to all civil servants of the central civil service, and to all employees with a private contract in the central civil service and whose seniority is above 3 years. Both base salary and bonus are subject to a 100 % replacement rate during 3 months. For employees with a private contract in the civil service and whose seniority is below 3 years, the duration with no wage loss is at most 2 months. For employees of the civil service outside the central civil service, the same rules applies, except that only the replacement rate of the base salary is defined by the law. The replacement rate of the bonus may follow different rules.

[4]Technically, the wage penalty is equal to 1/30 of the usual monthly wage, whatever the calendar month and the number of working days per week.

[5]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.

of earnings started with some delay. However, the circular clearly states that it applied to all sick leave starting from 1st January 2012 and the measure was highly publicized (notably by labor 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 1st 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 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 units of the territorial 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[6]. 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 (2017) 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.

[6]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 sick leave would be increased.

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 reducing absenteeism exist. They include for example semiannual or annual bonuses, calculated from professional value and work attendance. The characteristics and the timing of the implementation of these other incentives vary greatly between public institutions and over time. Second, in the local civil service, the timing of the implementation of the one-day waiting period also varies greatly between territorial collectivity and over time[7]. 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. Responding to the survey is legally mandatory. It is a rotating 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. Most questions of each quarterly interrogation focus on the reference week, defined as the week just before the interrogation. This reference week is randomly sampled within the calendar quarter of the first interrogation. Each following interrogation then occurs with a time interval of precisely one quarter.

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. In the first case,

[7] 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*).

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[8] 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 answer is chosen, the individual is then asked the expected total duration of the leave. More details and extracts of the questionnaire are available in appendix A.

The two durations that correspond to the two sequences of questions have consequently a different meaning. In the first case, it is a realized value, but the duration 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 duration of the sick leave.

As a result of these two different intrinsic meanings of the duration 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 duration of the former cannot exceed 7 days, and the duration of the latter is very rarely under 7 days[9]. We then break sick leave spells into precise

[8]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).

[9]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.

duration categories. We consider the short-term spells with duration 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 duration is strictly superior to 1 day, and inferior to 2 days. We also consider the long-term spells with duration 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 duration under 3 months, we use the expression "1 week to 3 months" for the sake of simplicity, even if the duration may be under 7 days in rare occasions. We hence have a partition of sick leave spells into 6 categories[10]: each spell is in one and only one category.

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, we cap the value of the highest weights (1 %). Note that we use cross section weights since longitudinal weights are not currently available for the French Labour Force Survey[11]. 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[12].

We limit ourselves to individuals aged between 15 and 75, 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 only keep wage earners, and exclude self-employed workers. We also exclude survey respondents without information on whether they have been absent from work during the

[10]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 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".

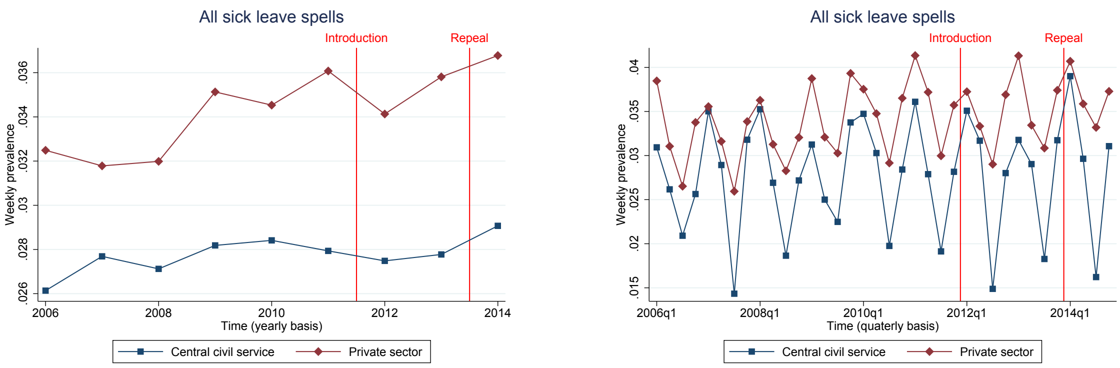
[11]Even though some works are currently carried out at INSEE on the issue (Jauneau and Nouël de Buzonnière, 2011; Biausque, Juillard and Lebrère, 2016).

[12]The average mean of individual weights is 579. Regarding the dispersion of the weights of an individual across time (within dispersion), the average standard error of individual weights is 53.

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. The total sick leave prevalence is roughly 2.8 % in the central civil service and 3.4 % in the private sector over the period (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 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 the generosity of the sick benefit system in the private sector. It increased absenteeism according to Ben Halima, Elbaz and Koubi (2017). Second, the 2008 crisis erupted and was associated with an increase of 2 percentage points in the unemployment rate[13] 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.



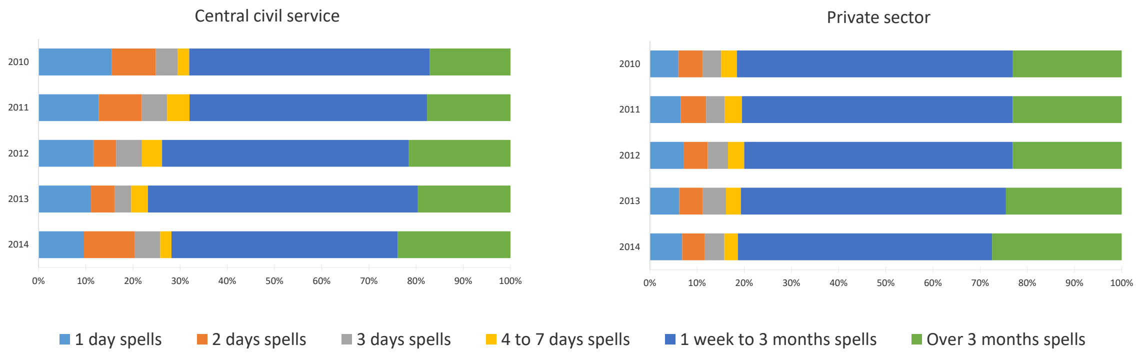
Source: French Labour Force Survey 2006-2014.

Lecture note: During the year 2010, the average weekly prevalence of all sick leave spells is 2.84 % in the central civil service and 3.45 % in the private sector.

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

[13]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).

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.

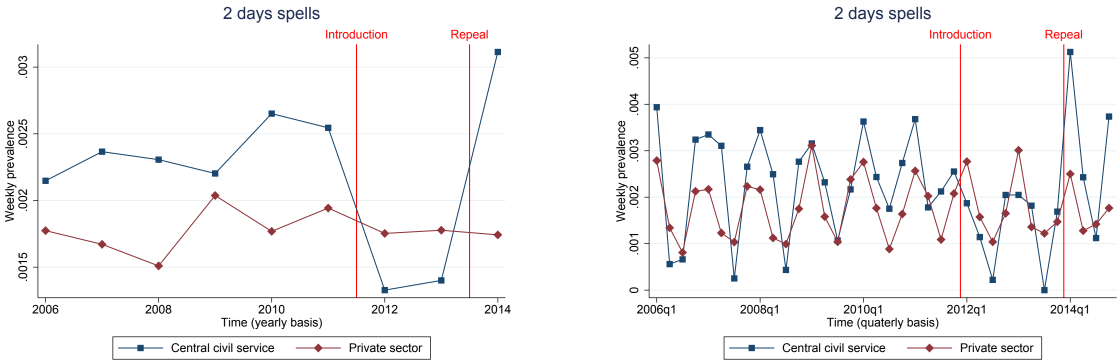


Source: French Labour Force Survey 2010-2014.

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

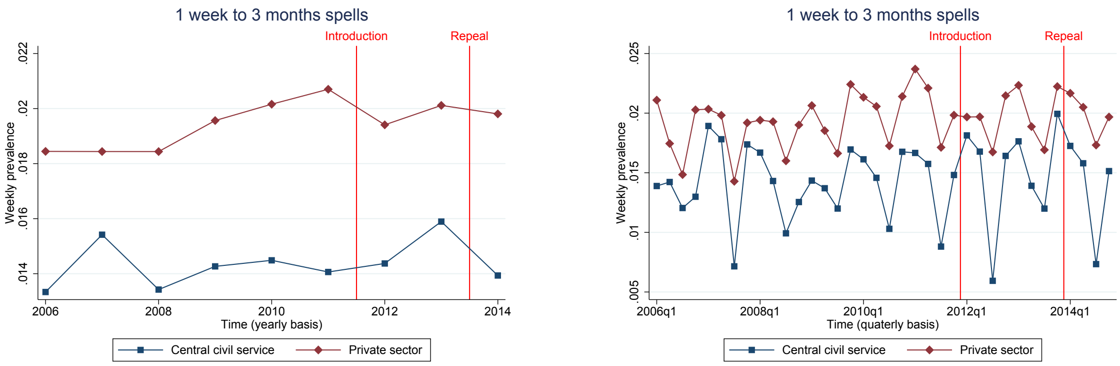
We go beyond the total prevalence by breaking sick leave spells into the 6 previously described duration categories. We consider the distribution of observations of sick leave spells between these 6 categories for each year between 2010 and 2014 (Figure 2). Contrary to the total prevalence, a clear change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

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



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 duration 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 duration (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 be not reported to the employer or reported as leave for another reason, like days off[14]. 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 duration of spells.

This is consistent with what was found by Pettersson-Lidbom and Thoursie (2013) in a similar context in Sweden 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 quantitatively 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.

[14]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).

We consider only the years 2010 to 2014 as explained above. From a cross section perspective, we have 704,000 observations and among them 25,000 for which the respondent is on sick leave. From a panel perspective, we have 186,000 individuals. It implies that we have on average 3.79 observations per individuals. The fact that this number is strictly below 6 is partially due to the definition of the sample. First we focus on years 2010 to 2014, which leads to left- and right-censoring. Second we impose a few restrictions on covariates, as explained in subsection 1.2. Considering only individuals whose first interrogation is at the earliest on 2010 Q1 and at the latest on 2013 Q3 (so that they can possibly be observed 6 times between 2010 Q1 et 2014 Q4) and not imposing any restriction on the covariates, the average number of observations per individual rises to 5.36. It suggests that once an individual begins to participate to the survey, she answers to most of the 6 interrogations.

Among individuals of the sample, 168,000 are never on sick leave, 14,000 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 169 individuals are at least twice on short-term sick leave.

Simple statistics regarding health-related absences and sociodemographic characteristics for each sector are presented in Table 1. Descriptive statistics confirm that the weekly prevalence of all health-related absences is lower in the central civil service than in the private sector. Distinguishing between short-term and long-term absences, the prevalence of very short-term absences (1 and 2 days spells) is higher in the central civil service, while the prevalence of all the other longer categories is lower. Most observables variables related to sociodemographic characteristics and working conditions are very close. Employees of the central civil service are slightly more likely to be woman, slightly older, slightly more in a couple and with a child under 6 years old. They are better paid and work slightly less days per week. The percentage of permanent contract or civil servant is slightly lower in the central civil service. The two main differences concern education and teachers. Concerning education, employees of the central civil service are much more likely to hold a graduate degree. The reason is that joining the central civil service require in most cases to pass a competitive exam, whose inscription makes necessary to hold a degree of various levels. Concerning teachers, almost all of them belong to the central civil service in France.

Table 1: Health-related absences and observables characteristics by sector, years 2010-2014

Sector	Central civil service	Private Sector
Weekly prevalences of sick leave (dependent variables)		
All spells	2.815%	3.547%
1 day spells	0.340%	0.232%
2 days spells	0.222%	0.180%
3 days spells	0.137%	0.151%
4 to 7 days spells	0.097%	0.117%
1 week to 3 months spells	1.455%	2.004%
Over 3 months spells	0.564%	0.863%
Sociodemographic characteristics and working conditions (covariates)		
Women	54.9%	46.2%
Age	42.5%	40.7%
Highly educated (graduate degree level)	51.5%	16.5%
Being in a couple	73.0%	70.5%
Having a child under 6	21.2%	19.6%
Permanent contract or civil servants	89.2%	90.2%
Wage (euros per month)	2187	1833
Teachers	46.0%	0.69%
Working days per week	4.66	4.85
Observations	83,595	620,413

Source: French Labour Force Survey 2010-2014.

Determinant of health-related absences are presented in Table 14 in Appendix C. These pooled OLS regressions show that sociodemographic characteristics, working conditions and season matter for explaining the overall prevalence and the prevalences by spell categories. Some determinants affect spells in the same direction for both short-term and long-term absences, or only for long-term absences. Other determinant affects in opposite direction short-term and long-term absences. Determinants that affect in the same direction short-term and long-term absence are women (more absences), being in a couple (less absences), having a child under 6 (more absences), being more paid (less absences) and seasons different from winter (less absences). Determinants that affect in the same direction spells related only to long-term absences are the contract (more long-term absences for permanents contract or civil servant), the number of working days during a usual week[15] (more long-term absences as the number of days increases). Determinants that affect short-term and long-term absences in opposite direction are age (more long-term absences but less short-term absences as age increases), education (more 1 day spells for graduate employees but less spells of longer duration), working in the central civil service (slight more short-term absences but less long-term absences), being a teacher (more 1 and 2 days spells but less 3 days spells). Regarding the effect on overall prevalence, it is usually driven only by the effect on long-term absences, due to their level importance compared to short-term absences.

Additional working conditions may also matter for health-related absences, as shown by Afsa and Givord (2014) and Pollak and Ricroch (2016). More detailed information related to working conditions is available in our survey only in year 2007. Fortunately, the panel dimension of our data enables us 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

[15]We focus on working conditions available in our survey and related to a usual week, and not to the reference week. The reason is that working conditions during the reference week can be directly affected by a health-related absence.

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 a health-related absence during her reference week of quarter t. It is a prevalence. Regressions are run for the dummy of all spells, but also for dummies of each of the 6 duration category spells, as described in Section 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:

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

- α is the coefficient of interest. It captures the causal effect of the treatment, which is the presence of the one-day waiting period.

- $x_{i,t}$ stands for the socio-demographic and working conditions controls that may explain health-related absences and that are available in our data set. We include the belonging to the central civil service or the private sector, gender and age (through a spline function of age interacted with gender), a triple interaction of being in a couple, having a child under 6 and gender, the educational level and diploma, the professional category, the sector of activity, the type of contract, the number of working days during a usual week, the categorized paid vacation time per year, the housing occupation status and an interaction of the calendar quarter of the year with the sector[16].

- μ_i is an individual (employee) fixed effect. It controls for unobserved time-invariant individual-specific heterogeneity. Such fixed effects enable to assess the effect of the policy

[16]Wage is not in the covariates, as we a keep a unique value per employee, which is hence time-invariant.

using only the within variations. The inclusion of fixed effects increases the plausibility of estimation consistency in the case non-response is affecting the common-trend assumption, as shown by Lechner, Rodriguez-Planas and Kranz (2016). The impact of the inclusion of fixed effects is studied in subsection 4.3, and a brief discussion follows.

- ν_t is the time effect of the quarter, from 2010 Quarter 1 to 2014 Quarter 4.
- $\epsilon_{i,t}$ is the error term. In all regressions, we report heteroskedasticity robust standard errors. The reason for which the error term is heteroskedastic is that its variance depends on the explanatory variables, as we use a linear probability model. We also cluster at the employee level. It allows to address eventual downward bias in the standard errors due to serial correlation, as enlightened by Bertrand, Duflo and Mullainathan (2004).

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

The difference-in-differences strategy requires that the studied policy did not entail any self-selection between the two groups at the time of the policy 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. 0.42 % of employees in the central civil service leave it every quarter to enter the private sector, whereas 0.12 % of employees of the private sectors leave it to enter the central civil service.

3 Results

3.1 Treatment effects for spells of different durations

Table 2 presents the results for the main specification. The regressions yield results that confirm what was suggested by the descriptive statistics. Regarding the prevalence of all sick leave spells, no effect is found.

Table 2: Treatment effects on the prevalence of spells

	Spell category						All spells
	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	Over 3 m.	
T	-0.0000747	-0.00172^{***}	-0.000367	-0.0000521	0.00356^{**}	0.000652	0.00199
	(0.000800)	(0.000581)	(0.000533)	(0.000391)	(0.00171)	(0.00119)	(0.00228)
Observations	704,008	704,008	704,008	704,008	704,008	704,008	704,008
R^2	0.00216	0.00125	0.00156	0.00251	0.00246	0.00311	0.00387

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Regarding the prevalence of short-term spells, the four coefficients are all negative. The coefficient is highly significant for 2 days spells, but not for the three other categories. The coefficient for 2 days spells has to be compared to the mean value of 2 days spells in the central civil service when the policy is not in place, which is 0.278 %. It implies a decrease of 62 % 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 days off or other kinds of absence) as detailed above.

Regarding the prevalence of long-term spells, the two coefficients are 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 1.416 % in the central civil service when the policy is not in place. It implies an increase of 25 % 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 3. This more detailed table yields overall the same results as the previous one. The two coefficients for 2 days spells are still very significant. The effects are also of the same 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 after the implementation of

Table 3: Treatment effects on the prevalence of spells, by year of implementation of the policy

	Spell category						All spells
	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	Over 3 m.	
T × Year 2012	0.0000340	−0.00177***	0.000250	−0.0000897	0.00241	0.00154	0.00238
	(0.000990)	(0.000671)	(0.000665)	(0.000482)	(0.00193)	(0.00121)	(0.00262)
T × Year 2013	−0.000183	−0.00168**	−0.000980	−0.0000147	0.00469**	−0.000237	0.00160
	(0.000886)	(0.000666)	(0.000620)	(0.000424)	(0.00210)	(0.00144)	(0.00272)
Observations	704,008	704,008	704,008	704,008	704,008	704,008	704,008
R^2	0.00216	0.00125	0.00157	0.00251	0.00247	0.00311	0.00387

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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 15). The coefficients for 2 days spells are always negative and significant each winter and summer. The coefficients for other short-term absences, 1 day, 3 days and 4 to 7 days are mostly negative. When significant, they are negative, save one (in summer 2012, during the first year of implementation of the policy, 2 days spells might have been substituted by 3 days spells, the nearest upper category). The coefficients for long-term absences are mostly positive. One of them, regarding 1 week to 3 months spells, is significantly positive, in summer 2013, during the second year of implementation of the policy. These results corroborated what was found in Table 3.

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

The explanation of the decreased incidence is straightforward: the cost on starting a spell has a deterring effect on doing so. Explanations of the increased duration of spells are subtler. Intuitively, we have in mind three theoretical behavioral mechanisms. 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. Consequently, the employee may be tempted to compensate for this perceived unfairness by increasing the duration of her sick leave spell[17]. This explanation is all the more likely that the measure was continuously and unanimously criticized as unfair by labor unions. This suggests that this feeling of unfairness was widespread and might have been sustained over the two years of implementation of the measure. The effects we found are also in line with a small body of empirical literature focusing on deductible in car insurance (Dionne and Gagné, 2001; Miyazaki, 2009; von Bieberstein and Schiller, 2017), which finds that a higher deductible may lead to a higher reported cost of car crashes[18]. 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 results in an increased duration of spells. This explanation is 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 worse health state. This would explain[19] 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.

[17]Voss, Floderus and Diderichsen (2001) refers to a Danish study from Holm et al. (1986) which have similar results to ours and puts forward this explanation: "In Denmark, the introduction of a [qualifying day] in 1983 was followed by a clear decrease in short-term sick-leave events (1-3 days) and to some extent an increase in longer sick-leave events (>4 days). One explanation from the authors was that some people might compensate with an extra day of sick-leave if the [qualifying day] was experienced as unreasonable.".

[18]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 duration (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 duration of sick leave spells.

[19]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 4: Treatment effects on the prevalence of spells, by calendar quarter

	Spell category						All spells
	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	Over 3 m.	
T × Quarter 1	−0.000816	−0.00252**	−0.00214***	0.00106	0.00469*	−0.000739	−0.000453
	(0.00132)	(0.00101)	(0.000819)	(0.000756)	(0.00250)	(0.00143)	(0.00331)
T × Quarter 2	0.000681	−0.000794	−0.000344	−0.000319	0.00319	0.00113	0.00354
	(0.00128)	(0.000840)	(0.000657)	(0.000747)	(0.00258)	(0.00162)	(0.00339)
T × Quarter 3	0.000293	−0.00248***	0.000601	−0.00113**	0.00361*	0.00109	0.00199
	(0.000860)	(0.000722)	(0.000736)	(0.000496)	(0.00219)	(0.00150)	(0.00286)
T × Quarter 4	−0.000269	−0.00113	0.000555	−0.0000624	0.00273	0.00125	0.00306
	(0.00119)	(0.000874)	(0.000832)	(0.000425)	(0.00258)	(0.00133)	(0.00324)
Observations	704,008	704,008	704,008	704,008	704,008	704,008	704,008
R^2	0.00216	0.00126	0.00159	0.00252	0.00246	0.00311	0.00387

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

We also interact the treatment dummy with quarterly dummies in Table 4 to investigate for possible seasonal pattern of the impact of the policy. These regressions highlight more pronounced effects during the winter (Quarter 1) and the summer (Quarter 3) than during intermediate seasons (Quarter 2 et 4). Significant coefficients are indeed found only in the winter and summer. Both the decrease in 2 days spells and the increase in 1 week to 3 months spells are significant during these two extreme seasons. In addition, 3 days spells decrease in winter only, while 4 to 7 day spells decrease in summer only. During intermediate seasons, coefficients are in absolute value a little bit lower for 1 week to 3 months spells and much lower for 2 days spells.

The pronounced effects in the winter and summer might have two different explanations for these two seasons. In the winter, the prevalence of all categories of spells save the last one reaches its yearly maximum (see Table 14). The possibility of an absolute drop in the prevalence of short-term spells is hence higher. Besides, the general health may be worse during this season. Consequently, the lack of a short recovery time when needed may more likely result in a long recovery time in the end. In the summer, the utility of a unit of leisure time may be higher. Consequently, the incentive to extend the duration of a spell may also be higher.

3.3 Heterogeneous effects

In the following, we explore possible heterogeneous effects by covariate. We hence interact the treatment dummy with various covariates related to socio-demographic characteristics and working conditions. Results are presented in Table 5. Each column summarizes a unique regression. A coefficient can hence be interpreted as the effect of the associated covariate on the intensity of the reaction, the effect of other covariates on the intensity being held constant, even if there is a correlation between these covariates.

Table 5: Heterogeneous treatment effects

	Spell category						All spells
	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	Over 3 m.	
T	0.00278	−0.0105***	0.00177	−0.000983	0.0225**	0.00360	0.0192
	(0.00457)	(0.00345)	(0.00354)	(0.00166)	(0.00999)	(0.00599)	(0.0129)
T × Women	−0.00000908	−0.00182*	−0.000612	0.000107	−0.00192	0.0000273	−0.00423
	(0.00156)	(0.000959)	(0.00102)	(0.000704)	(0.00316)	(0.00196)	(0.00410)
T × Age	−0.0000848	0.000103**	−0.000113**	0.0000389	−0.000212	−0.0000936	−0.000361
	(0.0000696)	(0.0000521)	(0.0000545)	(0.0000312)	(0.000169)	(0.000131)	(0.000230)
T × Working days per week	0.000457	0.00105*	0.000353	−0.0000495	−0.00275	−0.000231	−0.00117
	(0.000829)	(0.000542)	(0.000499)	(0.000249)	(0.00175)	(0.000961)	(0.00220)
T × Wage	−0.000414	0.000313	0.000597	−0.000219	0.00140	0.00128	0.00296
	(0.000484)	(0.000305)	(0.000473)	(0.000438)	(0.00180)	(0.00103)	(0.00222)
T × Teacher	−0.000838	−0.000549	0.000176	−0.000186	0.00187	−0.00145	−0.000978
	(0.00161)	(0.00102)	(0.00100)	(0.000736)	(0.00321)	(0.00205)	(0.00426)
Observations	704,008	704,008	704,008	704,008	704,008	704,008	704,008
R^2	0.00217	0.00129	0.00158	0.00251	0.00247	0.00311	0.00388

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Wage is expressed in thousand euros per month.

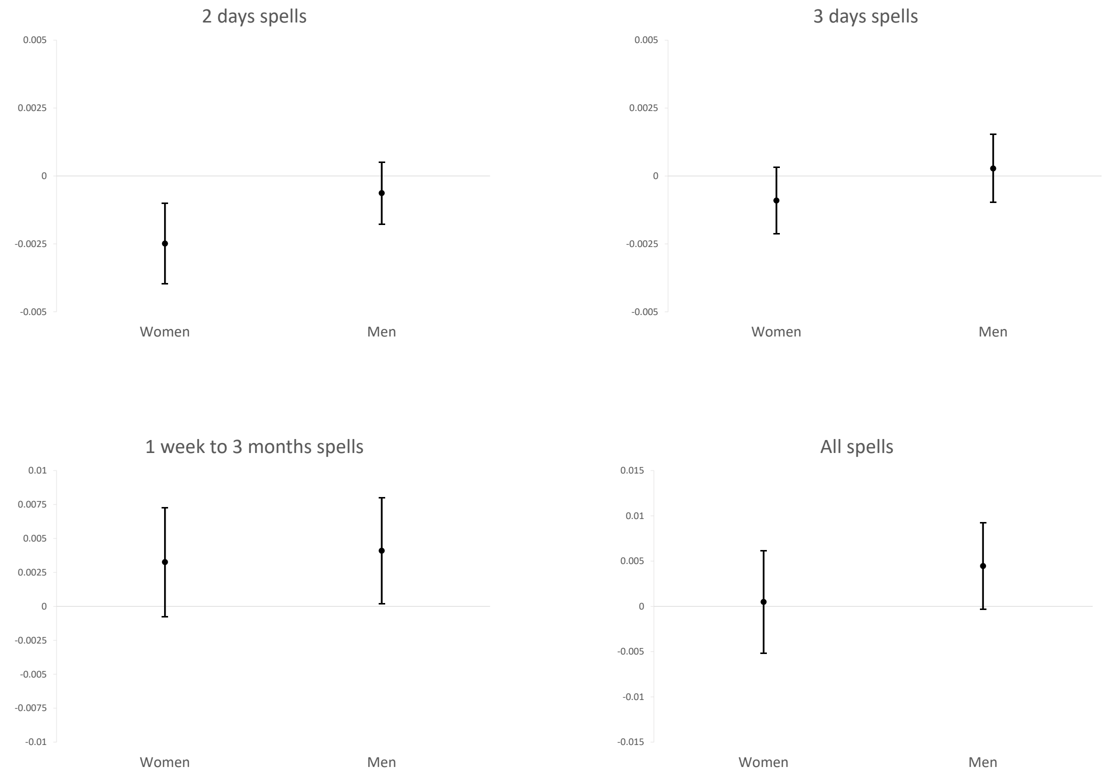
Three covariates change significantly the intensity of the reaction: two related to sociodemographic characteristics (gender and age) and one related to working conditions (the number of working days during a usual week). These three covariates change the intensity of the decrease in 2 days spells: being a woman, being younger or working less days per week implies independently a higher decrease. In addition, being younger also affects the intensity of the effect on 3 days spells, in an opposite way to its impact on the effect on 2 days spells. As no effect was found overall on 3 days spells (Table 2), it suggests that the policy slightly increases 3 days spells for young employees and slightly decreases them for old employees. As 2 days spells always decrease (more for young employees, less for old employees), a rationale is that the pivot point, between absences categories which decrease and increase, increases with age. Young employees are more likely to substitute 2 days spells for 3 days and longer spells, while old employees are more likely to substitute 2 days and 3 days spells for longer spells.

We run separate regressions on different gender and age category that comfort these results. Figures 5 and 6 represent the coefficients of interest for categories for which significance occurs at least once, while the whole set of results is presented in Appendix E (Tables 16, 17, 18, 19, 20 and 21). Regarding gender, only the decrease in 2 days spells is significant for women, while only the increase in 1 week to 3 days spells is significant for men[20]. Regarding age, only the decrease in 2 days spells is significant for employees under 35. No significant effect is found for employees aged 35-45. And the increase in 1 week to 3 months spells is significant for employees aged 45-55, resulting in an increase in total prevalence. Employees over 55 may appear specific with respect to the three previous regressions, as it is only the decrease in 3 days spells that is significant, with no significant increase in long-term absences[21].

Regarding gender differences, Pettersson-Lidbom and Thoursie (2013) have different results. In their study, which examines the effect of the removal of a one-day waiting period and of a increase in sick pay, the reaction was higher for women: the decrease in total prevalence was higher

[20]For 2 days spells, these results are consistent with the significance of the interacted term in Table 5. For 1 week to 3 months spells, closeness of coefficients for the two gender regressions is also consistent with the absence of significance for the interacted term.

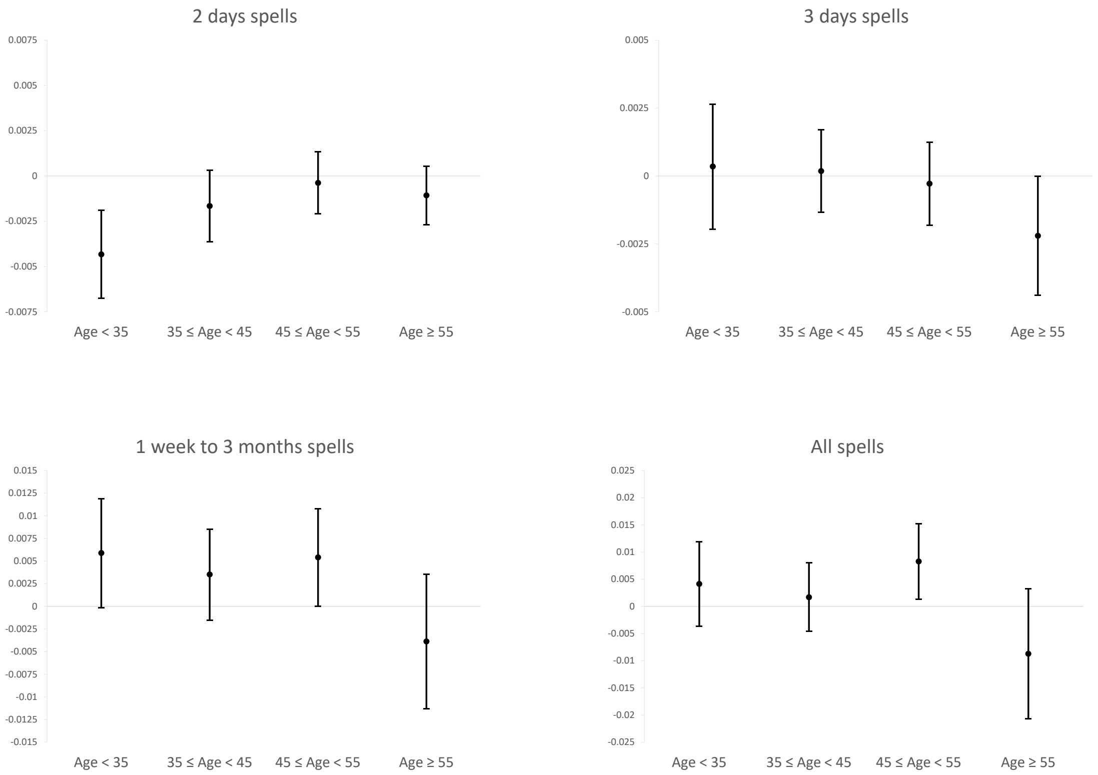
[21]It may be explain by selection: employees over 55 are more likely to have decided not to retire yet or are less likely to have begun to work at an early age. In both case, this could suggest a better health state or better unobservable working conditions, lowering the propensity to increase long-term absences when the policy is introduced.



Source: French Labour Force Survey 2010-2014.

Figure 5: Treatment effects by dividing the sample into gender groups, for 2 days spells (top left), 3 days spells (top right), 1 week to 3 months spells (bottom left) and all spells (bottom right) with 90 percent confidence interval.

for women than for men. Here, the coefficient of the interacted term in Table 5 is not significant, and its sign, as well as the comparison of the two corresponding coefficients of Figure 5, go in the sense of a higher reaction for men. They made the hypothesis that this could be attributed to the fact that women were more present in the care sector, where it is not allowed to work even with benign symptoms. In the presence of a waiting period, employees of the care sector would be more likely than employees of other sectors to take a long-term spell in order not to bear the costs of multiple 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



Source: French Labour Force Survey 2010-2014.

Figure 6: Treatment effects by dividing the sample into four age groups, for 2 days spells (top left), 3 days spells (top right), 1 week to 3 months spells (bottom left) and all spells (bottom right) with 90 percent confidence interval.

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) whereas others find the opposite (Paola, Scoppa and Pupo, 2014) or find no difference (Puhani and Sonderhof, 2010).

Several reasons might explain why responses to the one-day waiting period policy along with age, gender and number of days per week 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 gender, we find that the policy affected the prevalence of short-term spells for women, and the prevalence of long-term spells for men. In other contexts, different conclusions were drawn for gender, which calls for being cautious with the external validity of these results. For the number of working days per week, the explanation may be that a shorter working week increases the degrees of freedom of the employee regarding her weekly schedule. It is easier for her to shift the day dedicated to illness recovery to a non-working day.

Wage is not a determinant of the intensity of the reaction. Although the one-day waiting period is a monetary incentive, the penalty is proportional to the wage. The relative loss is hence identical whatever the wage. It may explain why employees with different wages do not react differently, other observables characteristics being held constant[22].

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 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.

[22]When the treatment dummy interacted with the wage is the sole interacted term, a lower wage implies a significantly stronger decrease in 2 days spells. This effects is however due to the correlation of wage with other covariates, as age.

Table 6: Treatment effects of the switch on

	2 days spells		1 week to 3 months spells	
	Age under 45	Whole	Age under 45	Whole
Switch on (2012 *versus* 2011)	-0.00225^{*}	-0.00108	0.00175	0.00159
	(0.00133)	(0.000944)	(0.00330)	(0.00243)
Observations	166,291	290,835	166,291	290,835
R^2	0.00482	0.00278	0.00660	0.00418

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Treatment effects of the switch off

	2 days spells		1 week to 3 months spells	
	Age under 45	Whole	Age under 45	Whole
Switch off (2014 *versus* 2013)	0.00262^{**}	0.00208^{**}	-0.00676^{*}	-0.00553^{**}
	(0.00127)	(0.000818)	(0.00381)	(0.00280)
Observations	151,819	273,116	151,819	273,116
R^2	0.00405	0.00238	0.00720	0.00455

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Using two successive years, effects estimated separately on the switch off and the switch on are respectively presented in Tables 6 and 7, for 2 days and 1 week to 3 months spells, the two more reactive categories. They are presented for the whole population and employees under the age of 45, as young employees were found to be the more reactive for both short-term and long-term absences in the previous section. At first glance, all coefficients have the expected sign. For 2 days spells, the coefficients are negative between 2011 and 2012 and positive between 2013 and 2014. On the whole population, only the switch off is significant. But considering employees under 45, more reactive, both the switch on and the switch off are significant. For 1 week to 3 months spells, the coefficients are positive between 2011 and 2012 and negative between 2013 and 2014. The switch off is significant for the whole population and for employees under 45, but this not the case for the switch on. This is consistent with Table 3, where we found an effect only in 2013 and not in 2012. Reaction on long-term spells may require some time to reach his steady state effect 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 tests are 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.

Results are presented in Table 8. Regarding 2 days and 1 week to 3 months spells, coefficients are either positive or negative and they are never significant.

4.2 Alternative control group

The difference-in-differences strategy relies on the assumption that the control group is a good counterfactual for the treated group. Table 1 has shown that most covariates means are of the same order between the two groups, but that a few observable characteristics, such as the educa-

Table 8: Treatment effects in placebo tests

	2 days spells		1 week to 3 months spells	
	Age under 45	Whole	Age under 45	Whole
Placebo (2011 *versus* 2010)	0.000432	−0.000266	0.00289	−0.0000139
	(0.00136)	(0.00100)	(0.00386)	(0.00268)
Observations	166,018	286,013	166,018	286,013
R^2	0.00396	0.00351	0.00684	0.00416

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Treatment effects with alternative control group

	2 days spells		1 week to 3 months spells	
Control group	Private service sector	Whole private sector (Baseline)	Private service sector	Whole private sector (Baseline)
T	-0.00166^{***}	-0.00172^{***}	0.00415^{**}	0.00356^{**}
	(0.000600)	(0.000581)	(0.00175)	(0.00171)
Observations	517,489	704,008	517,489	704,008
R^2	0.00136	0.00125	0.00303	0.00246

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

tional level, differ. To get a more similar control group in terms of socioeconomic characteristic and working conditions without losing too many observations, we consider the private service sector as an alternative control groups. Results are presented in Table 9. They show results of similar magnitude. Standard errors are higher, which is consistent with the use of a lower number of observations.

4.3 Covariates and fixed effects

Table 10: Treatment effects with different econometric specifications

	2 days spells			1 week to 3 months spells		
Specification	Pooled OLS with covariates	FE without covariates	FE with covariates (Baseline)	Pooled OLS with covariates	FE without covariates	FE with covariates (Baseline)
T	-0.00137^{***}	-0.00176^{***}	-0.00172^{***}	0.00178^{*}	0.00372^{**}	0.00356^{**}
	(0.000354)	(0.000577)	(0.000581)	(0.00105)	(0.00172)	(0.00171)
Observations	704,008	704,008	704,008	704,008	704,008	704,008
R^2	0.00152	0.00030	0.00125	0.00688	0.00044	0.00246

Cluster-robust standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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. The results of those specifications are presented in Table 10. We remove on the one hand the individuals fixed effects, and on the other hand the covariates.

Removing the fixed effects decreases the magnitude of the two effects in absolute value. However, both remains significant. When OLS and FE estimations differ on an unbalanced panel, Lechner, Rodriguez-Planas and Kranz (2016) suggest that it "should be considered as evidence that non-response is not ignorable for the differences-in-differences estimation". In the survey, we saw in subsection 1.3, that once an individual begins to answer to the survey, she answers to most of the six interrogations. Yet responding to the survey is mandatory, it might be suggested that the propensity to begin to answer to the survey is decreasing with the health status. If the one-day waiting period have a negative impact of the health status, affected employees would choose not to begin to answer to the survey. This would explain why the increase in 1 week to 3 months spells is lower when considerering the OLS estimation instead of the FE one. If non-response is not ignorable, then the FE estimation is to be preferred to the OLS one, as only the former may be consistent. On the contrary, not including the covariate has almost no impact on the results. 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.

4.4 Weights

Table 11: Treatment effects without and with using the weights

	2 days spells		1 week to 3 months spells	
Weighting	No	Yes (Baseline)	No	Yes (Baseline)
T	-0.00186^{***}	-0.00172^{***}	0.00270^{*}	0.00356^{**}
	(0.000595)	(0.000581)	(0.00156)	(0.00171)
Observations	704,008	704,008	704,008	704,008
R^2	0.00119	0.00125	0.00194	0.00246

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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).

4.5 Nonlinear specifications

We use a linear specification with a binary variable as dependent variable. The use of a linear probability model has many advantage. We can cite the ability to interpret directly coefficients as treatment effects, the ability to interact the treatment dummy with covariates and to interpret easily the coefficients of the interacted terms, computational ease (no numerical problem of convergence as for likelihood maximization), the ability to cluster standard error to avoid downward biases of the standard errors due to serial correlation.

However, a first concern is that it is not fully adapted to situations in which the dummy variable is close to zero, as it is the case here. A more natural model is a logit model. Compared to the linear probability model, the two may have similar behavior in the linear zone of the logistic function, when the dependent variable is close to 0.5, but not when the dependent variable it close to 0 or 1.

We hence perform a robustness check with a logit model instead of a linear probability model. For computational ease, covariates are not included, and neither are weights. However, we keep fixed effects, as they allow to capture unobserved heterogeneity. Using fixed effects in a logit model makes the estimation subject to the incidental parameter problem. When the number of period is small, estimators are inconsistent. A solution lies in considering a conditional fixed effect logit model (Chamberlain, 1980). The computation of average treatment effects is not straightforward, as fixed effects are not estimated. We can however interpret the sign of the coefficient of the interaction term as the sign of the treatment effect[23].

We present the results of the conditional fixed effect logit model in Table 12. We do not present the raw coefficients but their exponential, as the latter can be interpreted as relative changes. A first sight, we have smooth decreasing (from 1 day to 2 day spells) coefficients and then an increasing pattern. The two main coefficients, regarding 2 days spells and 1 week to 3 months spells, have the same significance level than in the linear probability model. Numerically, relative changes are straightforward: 2 days spells decrease by 56 %, while 1 week to 3 months spells increase by 24 %. These relative changes are close to those computed in Section 3.

A second concern is related to the relationship between the different probabilities we consider. We partition the possible events into different categories and we consider separately the probabilities of each event, without taking into account the fact that the sum of these probabilities, including the one of not reporting a sick leave, is equal to one. To investigate whether

[23]See Puhani (2012).

Table 12: Binary logit with fixed effects

	Spell category						All spells
	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	Over 3 m.	
T	0.743	0.441***	0.771	0.936	1.237**	1.255	1.028
	(0.172)	(0.127)	(0.259)	(0.390)	(0.133)	(0.255)	(0.0860)
	[0.508,1.087]	[0.275,0.708]	[0.444,1.339]	[0.472,1.858]	[1.036,1.476]	[0.899,1.752]	[0.895,1.179]
Observations	7,582	5,783	4,766	3,597	52,591	14,256	74,301
Pseudo R^2	0.03294	0.03745	0.03535	0.03759	0.00626	0.03658	0.01066

Odds ratios are displayed. Standard errors in parentheses. 90% confidence interval below.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Conditional fixed effect logit model only uses employees who experience a change in the dependent variable, which explains why the number of observations is low compared to the linear probability model.

imposing this condition matters, we consider a multinomial conditional fixed effect logit model. Each modality is one of the 6 considered duration categories, and the reference modality is not having a sick leave. Numerical implementation is made possible by Pforr (2014). As for the binary logit model, covariates and weights are not included. Results are presented in Table 13. We find results which are similar both to the binary conditional fixed effect logit model and to the linear probability model.

Table 13: Multinomial logit with fixed effects

	Odd ratio	Standard error	90% confidence interval
1 day spells	0.746	(0.173)	[0.510,1.092]
2 days spells	0.444***	(0.128)	[0.276,0.714]
3 days spells	0.768	(0.258)	[0.442,1.335]
4 to 7 days spells	0.944	(0.395)	[0.474,1.880]
1 week to 3 months spells	1.259**	(0.137)	[1.052,1.505]
Over 3 months spells	1.332	(0.274)	[0.950,1.867]
Observations		76,066	
Pseudo R^2		0.02034	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5 Conclusion

Our results provide further support to the thesis that the pattern of sick pay matters for health-related absences. We hence conclude to the presence of moral hazard. But it is only the duration distribution of sick leave that 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 only for employees aged 45-55). We find that this monetary incentive alters the duration distribution of sick leave spells. The policy leads to a significant decrease in the prevalence of 2 days spells and to a significantly increase in the prevalence of 1 week to 3 months spells. The corresponding changes are of a large magnitude. We find a decrease of more than half in 2 days spells and an increase of 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 age, gender, number of working days per week and across seasons. Being a young employee, a woman or working less days per week implies a higher decrease in 2 days sick leave. Considering separately women, the sole decrease in 2 days spells is significant. Considering separately men, the sole increase in 1 week to 3 months spells is significant. Both the decrease in short-term spells and the increase in long-term spells are significant in winter or summer. No effect is found in spring or fall. This might suggest to make the sick pay pattern vary with some sociodemographic characteristics, working conditions 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 duration of sick leave, that is a locally increasing marginal coverage. On the contrary, these opposite effects are not observed when the coverage remains monotonically decreasing with the duration 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 aged between 15 and 75 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 duration 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 durations 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 duration of this leave during the reference week. Information may be consequently left-censored or right-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 left- or right-censored, but the duration is an estimated duration 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 duration 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 7: 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

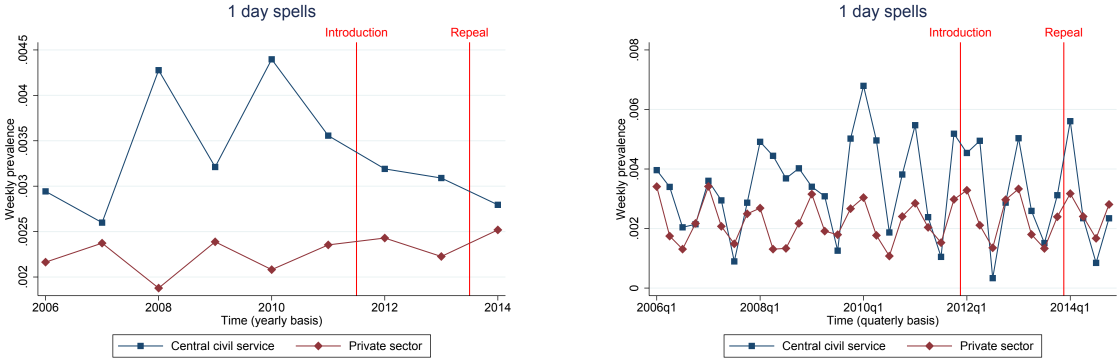
Pour ceux qui ont été absents pour maladie ou accident du travail la semaine de référence
EMPABS=1

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

.... nombre de jours (0,5 à 7)

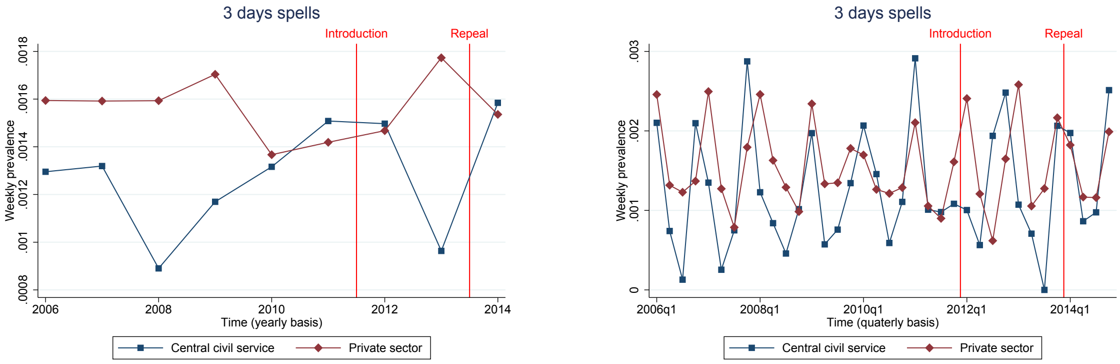
Figure 8: French Labour Force Survey 2013, extract of the questionnaire related to short-term sick leave spells.

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



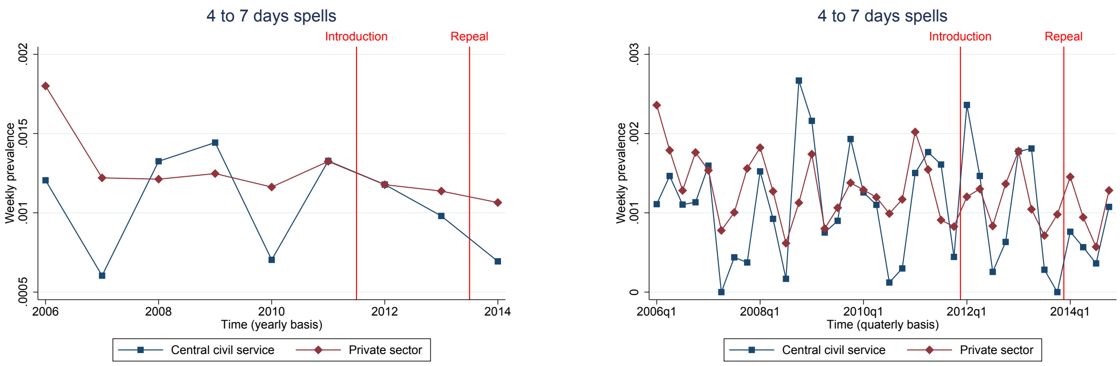
Source: French Labour Force Survey 2006-2014.

Figure 9: Prevalence of 1 day spells by sector, at a yearly (left) and quarterly (right) basis.



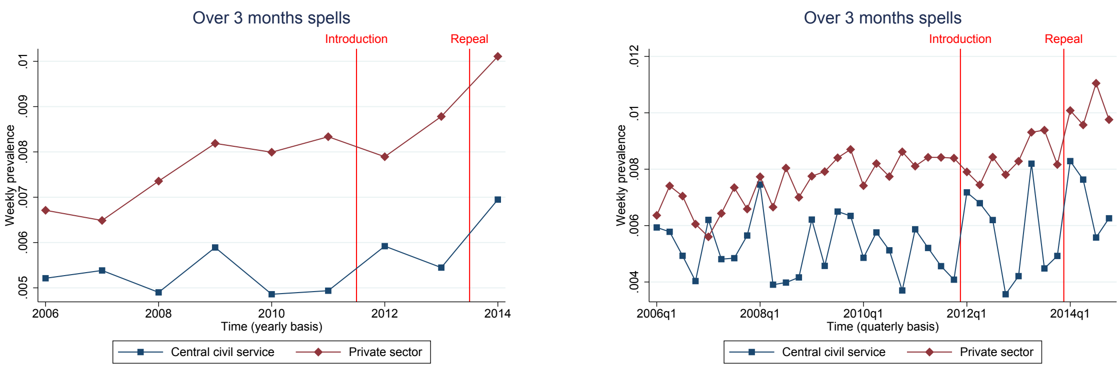
Source: French Labour Force Survey 2006-2014.

Figure 10: Prevalence of 3 days spells by sector, at a yearly (left) and quarterly (right) basis.



Source: French Labour Force Survey 2006-2014.

Figure 11: 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 12: Prevalence of over 3 months spells by sector, at a yearly (left) and quarterly (right) basis.

C Determinants of absenteeism

Table 14: Determinants of absenteeism as shown by pooled OLS regressions

	Spell category						All spells
	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	Over 3 m.	
Women	0.000921***	0.000620***	0.000490***	0.000169*	0.00411***	0.000596	0.00691***
	(0.000139)	(0.000118)	(0.000108)	(0.0000942)	(0.000446)	(0.000366)	(0.000706)
Age	−0.0000469***	−0.0000369***	−0.0000156***	−0.0000155***	0.000211***	0.000356***	0.000452***
	(0.00000661)	(0.00000558)	(0.00000510)	(0.00000468)	(0.0000217)	(0.0000182)	(0.0000339)
Highly educated (degree level)	0.000634***	−0.000548***	−0.000367***	−0.000498***	−0.00851***	−0.00298***	−0.0123***
	(0.000207)	(0.000154)	(0.000133)	(0.000113)	(0.000533)	(0.000415)	(0.000895)
Being in a couple	−0.000371**	−0.000379***	−0.000245*	−0.000112	−0.00185***	−0.00108***	−0.00404***
	(0.000162)	(0.000142)	(0.000128)	(0.000111)	(0.000500)	(0.000403)	(0.000766)
Having a child under 6	0.000857***	0.000245	0.000405***	0.0000114	0.00161***	0.000772*	0.00390***
	(0.000208)	(0.000169)	(0.000155)	(0.000125)	(0.000539)	(0.000410)	(0.000816)
Permanent contract or civil servants	0.0000719	0.000122	0.000210	0.000257*	0.00760***	0.00448***	0.0127***
	(0.000248)	(0.000211)	(0.000178)	(0.000152)	(0.000621)	(0.000390)	(0.000903)
Wage (thousand euros per month)	−0.0000872*	−0.0000265	−0.000117***	−0.000105***	−0.00266***	−0.00212***	−0.00512***
	(0.0000480)	(0.0000358)	(0.0000298)	(0.0000301)	(0.000310)	(0.000247)	(0.000568)
Teachers	0.00129***	0.00124***	−0.000696***	0.00000365	0.000121	0.000316	0.00227
	(0.000432)	(0.000329)	(0.000235)	(0.000213)	(0.000912)	(0.000743)	(0.00145)
Working days per week	0.00000781	0.0000667	0.000285***	0.000288***	0.00156***	0.00142***	0.00363***
	(0.0000828)	(0.0000602)	(0.0000542)	(0.0000529)	(0.000270)	(0.000222)	(0.000411)
Central civil service	0.000301	0.0000971	0.000385*	0.0000809	−0.00198***	−0.00173***	−0.00284***
	(0.000287)	(0.000198)	(0.000208)	(0.000170)	(0.000668)	(0.000534)	(0.00104)
Year 2010	ref	ref	ref	ref	ref	ref	ref
Year 2011	0.000150	0.000159	0.0000828	0.000233	0.000600	0.000358	0.00158*
	(0.000206)	(0.000183)	(0.000153)	(0.000144)	(0.000587)	(0.000431)	(0.000827)
Year 2012	0.000189	−0.000125	0.000144	0.000104	−0.000346	0.0000905	0.0000578
	(0.000208)	(0.000176)	(0.000156)	(0.000135)	(0.000600)	(0.000452)	(0.000875)
Year 2013	−0.0000141	−0.000110	0.000342**	0.0000431	0.000506	0.000849*	0.00161*
	(0.000209)	(0.000182)	(0.000164)	(0.000139)	(0.000613)	(0.000467)	(0.000900)
Year 2014	0.000207	0.0000796	0.000214	−0.0000454	0.0000479	0.00217***	0.00268***
	(0.000216)	(0.000187)	(0.000161)	(0.000137)	(0.000615)	(0.000507)	(0.000923)
Calendar quarter 1	ref	ref	ref	ref	ref	ref	ref
Calendar quarter 2	−0.00123***	−0.00115***	−0.000961***	−0.000323**	−0.00140***	0.000284	−0.00478***
	(0.000194)	(0.000176)	(0.000145)	(0.000136)	(0.000503)	(0.000257)	(0.000615)
Calendar quarter 3	−0.00205***	−0.00166***	−0.00107***	−0.000774***	−0.00508***	0.000425	−0.0102***
	(0.000182)	(0.000163)	(0.000142)	(0.000125)	(0.000483)	(0.000289)	(0.000615)
Calendar quarter 4	−0.000618***	−0.000957***	−0.000329**	−0.000487***	−0.000742	−0.0000296	−0.00316***
	(0.000205)	(0.000178)	(0.000162)	(0.000130)	(0.000500)	(0.000277)	(0.000631)
Constant	0.00470***	0.00386***	0.00116***	0.000774**	0.00404**	−0.0129***	0.00159
	(0.000540)	(0.000427)	(0.000380)	(0.000335)	(0.00159)	(0.00131)	(0.00241)
Observations	704,008	704,008	704,008	704,008	704,008	704,008	704,008
R^2	0.00068	0.00043	0.00031	0.00021	0.00271	0.00345	0.00465

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

D Treatment effects for the 8 quarters of implementation of the policy

Table 15: Treatment effects on the prevalence of spells, for the 8 quarters of implementation of the policy

	Spell category						All spells
	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	Over 3 m.	
T × 2012 Q1	−0.000874	−0.00249**	−0.00169*	0.00157	0.00445	0.000491	0.00146
	(0.00172)	(0.00120)	(0.000974)	(0.00100)	(0.00293)	(0.00154)	(0.00400)
T × 2012 Q2	0.00203	−0.00125	−0.000216	−0.000637	0.00238	0.00189	0.00419
	(0.00175)	(0.00101)	(0.000769)	(0.000923)	(0.00315)	(0.00170)	(0.00414)
T × 2012 Q3	−0.000699	−0.00234***	0.00191*	−0.00160***	−0.000216	0.00222	−0.000737
	(0.000945)	(0.000828)	(0.00110)	(0.000614)	(0.00244)	(0.00175)	(0.00333)
T × 2012 Q4	−0.00118	−0.00122	0.00104	0.00000313	0.00105	0.000808	0.000495
	(0.00154)	(0.00108)	(0.00110)	(0.000634)	(0.00319)	(0.00166)	(0.00410)
T × 2013 Q1	−0.00119	−0.00259**	−0.00212**	0.000463	0.00345	−0.00180	−0.00379
	(0.00154)	(0.00123)	(0.000969)	(0.000988)	(0.00330)	(0.00183)	(0.00425)
T × 2013 Q2	−0.00118	−0.000356	−0.000145	−0.000160	0.00272	0.000155	0.00103
	(0.00130)	(0.00104)	(0.000833)	(0.000945)	(0.00316)	(0.00217)	(0.00422)
T × 2013 Q3	0.000671	−0.00258***	−0.000510	−0.000832	0.00646**	−0.000536	0.00268
	(0.00110)	(0.000793)	(0.000672)	(0.000665)	(0.00288)	(0.00189)	(0.00367)
T × 2013 Q4	0.0000578	−0.000979	0.0000441	−0.000273	0.00404	0.000906	0.00379
	(0.00140)	(0.00101)	(0.00102)	(0.000433)	(0.00335)	(0.00162)	(0.00403)
Observations	704,008	704,008	704,008	704,008	704,008	704,008	704,008
R^2	0.00218	0.00126	0.00160	0.00253	0.00247	0.00312	0.00388

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E Treatment effects for various sub-populations

Table 16: Treatment effects on the prevalence of spells for women

	Spell category						All spells
	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	Over 3 m.	
T	−0.000280	−0.00249***	−0.000901	0.000313	0.00326	0.000589	0.000495
	(0.00120)	(0.000904)	(0.000742)	(0.000575)	(0.00244)	(0.00191)	(0.00343)
Observations	336,831	336,831	336,831	336,831	336,831	336,831	336,831
R^2	0.00206	0.00179	0.00252	0.00811	0.00388	0.00445	0.00610

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 17: Treatment effects on the prevalence of spells for men

	Spell category						All spells
	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	Over 3 m.	
T	0.000255	−0.000631	0.000277	−0.000371	0.00410*	0.000833	0.00446
	(0.00103)	(0.000691)	(0.000760)	(0.000518)	(0.00237)	(0.00128)	(0.00290)
Observations	367,177	367,177	367,177	367,177	367,177	367,177	367,177
R^2	0.00442	0.00248	0.00312	0.00264	0.00243	0.00403	0.00369

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 18: Treatment effects on the prevalence of spells for individuals under the age of 35

	Spell category						All spells
	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	Over 3 m.	
T	0.00138	−0.00433***	0.000352	−0.000430	0.00589	0.00127	0.00413
	(0.00185)	(0.00148)	(0.00140)	(0.000825)	(0.00367)	(0.00114)	(0.00475)
Observations	205,510	205,510	205,510	205,510	205,510	205,510	205,510
R^2	0.00557	0.00280	0.00322	0.00743	0.00733	0.00625	0.00941

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 19: Treatment effects on the prevalence of spells for individuals between the ages of 35 and 45

	Spell category						All spells
	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	Over 3 m.	
T	−0.000169	−0.00166	0.000181	−0.000700	0.00352	0.000505	0.00167
	(0.00140)	(0.00119)	(0.000922)	(0.000860)	(0.00306)	(0.00141)	(0.00383)
Observations	194,551	194,551	194,551	194,551	194,551	194,551	194,551
R^2	0.00569	0.00352	0.00711	0.00708	0.00446	0.00600	0.00640

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 20: Treatment effects on the prevalence of spells for individuals between the ages of 45 and 55

	Spell category						All spells
	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	Over 3 m.	
T	−0.000650	−0.000381	−0.000280	0.000803	0.00541*	0.00337	0.00827**
	(0.00152)	(0.00104)	(0.000928)	(0.000709)	(0.00327)	(0.00209)	(0.00421)
Observations	203,024	203,024	203,024	203,024	203,024	203,024	203,024
R^2	0.00255	0.00806	0.00291	0.00557	0.00451	0.00780	0.00642

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 21: Treatment effects on the prevalence of spells for individuals over the age of 55

	Spell category						All spells
	1 day	2 days	3 days	4 to 7 d.	1 w. to 3 m.	Over 3 m.	
T	−0.000984	−0.00107	−0.00220*	0.000133	−0.00388	−0.000709	−0.00872
	(0.00150)	(0.000984)	(0.00133)	(0.000691)	(0.00450)	(0.00575)	(0.00727)
Observations	100,923	100,923	100,923	100,923	100,923	100,923	100,923
R^2	0.00377	0.00390	0.00113	0.01477	0.00447	0.01098	0.00890

Cluster-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$