The Health-Consumption Effects of Increasing Retirement Age Late in the Game

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Abstract – Using the differentiated increase in retirement age across cohorts introduced by the 2010 French pension reform, we estimate the health-consumption effects of a 4-month increase in retirement age. We focus on individuals who were close to retirement age but had not yet reached statutory retirement age by the time the reform was passed. Using administrative data on individual sick-leave claims and health-care expenses, we show that the probability of having at least one sickness absence increases for all treated groups, while the overall number of sick days remains unchanged, conditional on having a sick leave. Delaying retirement does not increase the probability of seeing a general practitioner, except for men in the younger cohorts. In contrast, it raises the probability of seeing a specialist physician for all individuals, except men in the older cohorts. Delaying retirement also increases the probability of seeing a physiotherapist among women from the older cohorts. Overall, it increases health expenditures, in particular in the lower part of the expenditure distribution.

JEL: I10, J14, J18, J26
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Population ageing is a major challenge for societies and, in particular, for the viability of social protection systems. Over the past decades, most OECD countries have introduced pension reforms aiming at the financial sustainability of their pension system (OECD, 2017). These reforms are typically multidimensional, but they often include an increase in the statutory and/or ordinary retirement age[1] based on the assumption that delaying retirement creates an incentive for older workers to stay in employment. This should mechanically generate an increase in contributions and reduce pension expenditure on the short term, thereby contributing to the financial balance of the pension and, more generally, the social security systems.

However, this virtuous circle could be broken if postponing retirement negatively affects individual health (L'Haridon *et al.*, 2018). The literature has extensively studied the effect of moving from employment to retirement on old-age physical, mental and cognitive health, often using statutory retirement ages as an instrument and pension reforms as an exogenous shock to these ages. The results are overall ambiguous. The meta-analysis conducted by Filomena & Picchio (2022) on 275 observations from 85 articles published between 2000 and 2021 shows that 28% of them find positive effects of retirement on health outcomes, while 13% find negative effects, but even more important, almost 60% of the observations do not provide any statistically significant results. Another strand of literature has focused on the effects of delaying retirement on post-retirement health. In their survey, Garrouste & Perdrix (2021) conclude that later retirement has no effect on mortality, decreases healthcare consumption, and has a negative or non-significant impact on self-reported health at old age.

Nevertheless, pension reforms increasing retirement age are also likely to affect *pre-retirement* health outcomes. To the extent that they increase individuals' residual working horizon, they likely affect the expected value of investments in health which may, in turn, modify individual health conditions (Bertoni *et al.*, 2018). At the same time, following changes in the retirement rules, individuals may feel that they are forced into a new situation in which they have little control over their retirement decision. Moreover, if the new rules are perceived as unfair and/or affect individuals close to the retirement age, this may lead to severe disappointment (De Grip *et al.*, 2012). Both mechanisms may generate an upsurge in stress that may negatively affect both physical and mental health. If the health conditions of employees affected by the reform are modified, this may improve or hamper their ability to work and hence affect the potential savings expected from an increase in retirement ages. This unintended effect of pension reforms has been much less studied in the literature.

This paper investigates the health-consumption effects of a pension reform that raised statutory and ordinary retirement ages in France in 2010, on individuals who were close to retirement age but had not yet reached statutory retirement age by the time the reform was passed. By mid-July 2010, the French government announced that the statutory retirement age (SRA) – respectively the ordinary retirement age (ORA) – would increase by 4 months for all individuals born between July and December 1951, and by four additional months for each cohort born in the following years until 1956. Since SRA and ORA were initially 60 and 65 respectively, the reform eventually raised them to 62 and 67 for individuals born in 1956 and later. We take advantage of this design to provide a first-difference estimate of the impact of a 4-month increase in retirement age on the sickness absences, physicians' and physiotherapists' visits as well as health-care expenditure of individuals who were at most 5.5 years away from statutory retirement age before the reform was passed.

More specifically, we consider two different samples composed of individuals who were closer to and further away from statutory retirement age – at most 2.5 years for the older ones, and between 4.5 and 6.5 years for the younger ones, after the reform. For each of them, we estimate the effect of a 4-month increase in retirement age across individuals born in two adjacent months, so as to net out the potential confounding effect of age on health conditions. In addition, as a placebo experiment, we check that we find no difference in health consumption across individuals born in two adjacent months who face the same retirement age after the reform. To do so, we leverage administrative data on individual non-hospital health-care and sick leave claims, available for all wage and salaried workers employed in the private sector and contract personnel working in the civil service. We consider health consumption over the period ranging from July 15th 2010 – the day after the reform was announced – to

1. *The Statutory Retirement Age (SRA) is the earliest age at which retirement benefits can be claimed conditional on a given number of years of contribution to the pension system. The Ordinary Retirement Age (ORA) is the age at which workers are eligible for full old-age pension independent of the number of years they have contributed.*

May 31st 2011 – the day before the older cohort started retiring.

Our results suggest that increasing statutory and ordinary retirement ages by four months raises the probability of having at least one sickness absence over the period we study, by 11.8% for men and 10.3% for women in the older cohorts, and by 6.7 and 3.9% for men and women respectively in the younger ones. In contrast, we do not find any effect of the reform on the number of days of sick leave, conditional on having one, when estimating a zero-inflated negative binomial model. As regards physicians' visits, we show that delaying retirement does not increase the probability of seeing a general practitioner (hereafter GP), except for men in the younger cohorts. In contrast, it raises the probability of having at least one visit with a specialist physician for all individuals, except men in the older cohorts,[2] although the effect is moderate (about 1.5%). Moreover, delaying retirement increases the probability of seeing a physiotherapist by 3.4% among women from the older cohorts. Finally, when estimating unconditional quantile regressions for health-care expenditure, we find increases in expense claims consistent with the above findings, in particular in the lower part of the expenditure distribution. The same holds for drug expenditure of men in the younger cohorts. This suggests that expense claims increase when retirement age is raised, in particular among individuals who initially had low levels of health-care expenditure.

We interpret our findings as suggesting that individuals affected by changes in the pension system late in their career experience psychological, and even physical, health troubles, at least over the year following the announcement of the reform.

Our paper speaks to two strands of literature. The first one is quite small and considers the effect of changes in retirement age on pre-retirement health and health behaviour of workers who were late in their career when the reform was passed. Bauer & Eichenberger (2021) examine a policy change that lowered retirement age from 65 to 60 in Switzerland. They show that, while the reform was intended to improve workers' health, it resulted in the opposite outcome. Sickness absences increased by 33% among 56-60-year-old construction workers when working until 60 instead of 65, and the probability that they report health problems increased by 54%. This is, to some extent, in line with Bertoni *et al*. (2018) who find that an increase in minimum retirement age – induced by a pension reform affecting eligibility conditions in Italy in 2004 – improved health behaviours among middle-aged men. A one-year increase in the residual working horizon increased the likelihood of exercising regularly, the probability of having a body mass index below the level indicating obesity and the probability of reporting a high satisfaction with one's own health. In contrast, in their seminal work on the subject, De Grip *et al*. (2012) find that delaying retirement deteriorates mental health. They assess the effect of a change in the Dutch pension system that raised the minimum retirement age by 1 year and 1 month for public-sector workers to be eligible to full-pension benefit. They find that, two years after the policy change, the depression rates were about 40% higher in the treated group than among control individuals. Our results complement De Grip *et al*. (2012) results'. We show that a modest 4-month increase in retirement age substantially increases the probability of sickness absence also among private-sector workers aged 54 and above, in France. This finding is in line with d'Albis *et al*. (2020) who also find that the French 2010 pension reform increased sickness absences among a smaller sample of public-sector high-school teachers. In addition, we show that delaying retirement increases the probability of seeing a specialist physician, and correspondingly raises health expenditure, in particular among workers with low initial health-care expenses.

Our research also complements the literature on the health impact of retirement in France. Using household data, L'Haridon *et al*. (2018) and Messe & Wolff (2019) compare the health trajectories of individuals who retire and those of individuals who stay in employment, after balancing their pre-retirement characteristics. They find that transition into retirement has a short-term beneficial effect on respondents' self-assessed health. Consistent with these findings, Blake & Garrouste (2019) show that the increase in the required number of years of contribution to be eligible to full-pension benefit and the reduction of pension levels, imposed on private-sector employees by the 1993 pension reform, had a negative effect on perceived and physical health of low-educated retirees. Nonetheless, Bozio *et al*. (2021) do not find any significant effect of this reform on mortality rates between ages 61 and 79. We complement this literature by investigating

2. These results are not inconsistent with what we find for GPs since, in France, specialist physicians can be accessed directly, without being referred by a GP.

the effects of a more recent pension reform on individuals who are still in employment at the time of the reform. We show that postponing retirement increases their health consumption, thus suggesting that working longer is not only detrimental at old ages but can have negative health effects on active individuals, at least when introduced late in their career. Finally, Ben Halima *et al*. (2022) estimate the effect of being above or below the statutory retirement age on sickness absences of cohorts affected in a different way by the 2010 pension reform. We improve on their methodology by proposing an empirical set up which allows identifying the causal effect of the reform on sick leaves and health-care expenditure, while being immune to strong assumptions on parallel trends.

The remainder of the paper is organised as follows. Section 1 presents the institutional context. Section 2 develops our empirical strategy. Section 3 describes the data. Section 4 presents the results and we conclude.

1. The Institutional Context

France has a variety of pension and health insurance schemes to which individuals contribute based on their occupation and/or on the sector in which they are employed. In this paper, we consider a pension reform passed in 2010 that increased the statutory and ordinary retirement ages for all salaried workers. However, we restrict our analysis to wage and salaried workers employed in the private sector and contract personnel working in the civil service, since our health data do not cover civil servants nor self-employed workers.

Although the reform was definitely adopted by parliament on October 27th 2010, the need to rebalance the accounts of the French public pension system had been in the public debate since 1993. However, during this period, the option favoured by policy makers had been an increase in the number of years of contribution required to be eligible to full-pension benefit (d'Albis *et al*., 2020). This was actually progressively raised from 37.5 years for cohorts born in 1933 and before, to 40 years for cohorts born in 1944 and later, starting as of 1993 in the private sector and 2003 in the public sector. The number of years of contribution was then further raised to 41 in 2009. The idea of increasing the statutory and ordinary retirement ages came up later in the public debate, in the course of Spring 2010. On May 16th, a Government Policy Paper on pension reform was handed to social partners. It mentioned that the only solution to ensure the financial sustainability of the pension system without affecting the standard of living of retirees and employed workers would be to increase the statutory retirement age. On June 16th, the Minister of Labour, Eric Woerth, presented the main orientations of his pension-reform project: The statutory retirement age would be progressively raised from 60 to 62 – while the ordinary retirement age would be raised from 65 to 67 – and the reform would not affect cohorts born before 1951. On July 13th, the bill was finally presented to the Council of Ministers. It made it clear that statutory and ordinary retirement ages would increase in a differentiated way across cohorts, according to the schedule shown in Table 1.

In this first stage of the reform, the statutory and ordinary retirement ages were therefore increased by four months for the first cohort (born between July and December 1951) and by four additional months for each cohort born in the following years until 1956. For all individuals born in 1956 and later, SRA and ORA increased by 2 years as compared to what they used to be prior to the reform, to 62 and 67 respectively. The reform was then accelerated on January 1st 2012: for cohorts born after January 1st 1952, the increase in the statutory and ordinary retirement ages across cohorts was raised from four to five months until the two age limits reached 62 and 67 respectively, which occurred for individuals born in 1955 and later.

It has to be noted that the 2010 reform did not apply to individuals who had started working before 18 years old: the statutory retirement age remained 60 for those of them who had

Table 1 – **Statutory and ordinary retirement ages by date of birth**

Birth date	Statutory retirement age	Ordinary retirement age
Before July 1951	60	65
From July 1st to December 31st 1951	60 + 4 months	65 + 4 months
1952	60 + 8 months	65 + 8 months
1953	61	66
1954	61 + 4 months	66 + 4 months
1955	61 + 8 months	66 + 8 months
1956 and later	62	67

contributed at least 43.5 years to the pension system. Since 2010, they could even retire at 58 if they had started working before the age of 16.

An important feature of the reform is that who exactly would have been affected and to what extent, among individuals born in 1951 and later, was unknown until July 13th, when the Minister of Labour presented the details of the reform to the Council of Ministers. The fact that even individuals who were only one year away from the current statutory retirement age were affected by the reform came as a big surprise since previous pension reforms had been more gradual. We build upon the unexpectedness of the exact content of the reform to estimate the effect of a 4-month increase in minimum retirement age on the health-care consumption of individuals who were still in the labour force at the time the reform was announced.

2. Empirical Strategy

2.1. Empirical Set-Up

Our data (see Section 3) do not contain information on the employment status of individuals. Since we do not know whether they are still in the labour force, we estimate an intention-to-treat model.

Our identification strategy relies on the comparison of sickness absences, physicians' and physiotherapists' visits as well as health-care expense claims of individuals whose statutory (and ordinary) retirement ages are 4 months apart because of the reform: 60 years and 4 months *vs* 60 years old; 60 years and 8 months *vs* 60 and 4 months, etc.[3] Since the increase in SRA (and ORA) scheduled by the reform is indexed on the individual date of birth and since age strongly affects health conditions, we compare individuals whose age is as similar as possible. To do so, we define our treatment and control groups so that, across both groups, individuals' birth dates are, at most, 2 months apart.

More specifically, we consider five cohorts. Cohort C1 includes individuals born in June and July 1951. Cohort C2 pools individuals born in December 1951 and January 1952. Similarly, cohort C3 pools individuals born in December 1952 and January 1953; cohort C4, individuals born in December 1953 and January 1954; and cohort C5, individuals born in December 1954 and January 1955.[4] Within each cohort, we then compare the individuals born in the two different months, i.e. individuals born in June *vs* July 1951 for cohort C1, and individuals born in December of one year (1951 to 1954) *vs* individuals born in January of the following year (1952 to 1955) for cohorts C2, C3, C4 and C5 respectively. Thus doing, treated and control individuals all face the same gap in their minimum retirement age due to the reform (four months before the acceleration of the reform) and are all born, at most, 2 months apart.

In this set up, non-compliers are individuals for whom the increase in the statutory and ordinary retirement ages does not modify the age and conditions at which they retire. This is the case of people who have already retired by the time the reform is passed. This is also the case of individuals (in particular women) who have never worked in their entire life, and of individuals who were entitled full-pension benefits before the reform at an age which happened to be exactly the statutory retirement age after the reform (this was the case of workers who had started working at a very young age and hence benefited from the so-called long-career scheme). All other individuals are compliers. This is the case of people who planned to retire as soon as possible. This is also the case of people who planned to retire later anyway, since the reform modifies the age at which they are entitled higher pension benefits than normal – the so-called *surcote*. Of course, individuals who were planning to retire at the ordinary age because they had not contributed enough to be entitled full pension benefit before that age are also compliers since the ORA is increased by the reform.

We exclude from our analysis cohort C3, born in December 1952 and January 1953. Our identifying assumption is indeed that the difference in the health-care outcomes of the treated and control groups is only due to the reform. This is plausible for all cohorts since both groups are almost the same age and are observed over the same period of time. However, this assumption is likely violated for individuals born in December 1952 and January 1953. In fact, the Berthoin reform, passed in 1959, increased the minimum school-leaving age from 14 to 16 for children born from January 1st 1953 onward. To the extent that this school-leaving age affects careers and pension rights and may affect health outcomes (Kemptner *et al.*, 2011),

3. In contrast to what is usually done in the literature, we do not estimate the effect of retirement on health outcomes using a pension reform as an instrument of retirement age. Since we do not know whether individuals are retired or not, our model is a reduced form where the pension reform directly affects health outcomes.

4. We currently do not have access to the health-consumption data of individuals born in 1956, which prevents us from extending the analysis to the cohort born in December 1955 and January 1956.

our identifying assumption likely does not hold for this cohort.

We group individuals in two different samples. The first one contains individuals from cohorts C1 and C2 who were close to retirement when the reform was passed. For them, the statutory minimum age was raised by 8 months at most (for those born in January 1952) and statutory retirement age was still on a relatively short horizon after the reform – less than 2.5 years. The second sample pools individuals from cohorts C4 and C5 who were much younger at the time of the reform. For these cohorts, retirement was delayed by a more substantial amount – from one year to one year and eight months – but, more importantly, the time horizon of retirement was distant – at least 3.5 years before the reform and 4.5 years afterwards. We group the cohorts into a younger and an older sample for several reasons. First, since the Berthoin reform potentially delayed entry on the labour market by 2 years for all individuals entering at the school leaving age in the younger group, pension entitlements were completely different across both groups of cohorts. Second, the overall increase in the retirement age was much larger in the younger than in the older sample which may have affected the way they responded to the treatment we study, i.e. an additional increase in retirement age by 4 months. As a matter of fact, 4 additional months may be considered a more marginal difference when the overall increase in the retirement age is larger than when it is smaller. Third, individuals in both samples were at different time distances from retirement before – and even more so after – the reform, which may also have affected their reactions to the reform (Bertoni *et al*, 2018). Last, while it was expected that the younger cohorts would be affected by the reform, as already mentioned, this came as a surprise for the older individuals since it amounted to changing the rules (very) late in the game. This difference may also have determined different psychological reactions to the reform, which may have, in turn, affected individuals' health in a different way. For these reasons, we choose to study the younger and older samples separately. However, as a robustness check, we re-estimate our models on a sample in which we pool the four cohorts and include cohort dummies.

To avoid considering treatments of different intensity, we focus on the period in which SRA and ORA were raised by four months for each successive cohort, i.e. before the acceleration of the reform in January 2012. This restriction is actually not binding since we want to estimate the impact of the reform on health-consumption outcomes of individuals who are not retired yet. Since the oldest individuals in our control groups may retire from June 1st 2011 – i.e. when they reach 60 years old – we consider health-care consumption over the period extending from the day following the announcement of the reform – made on July 13th 2010 – to the day before the oldest individuals in the control group reached the statutory retirement age – i.e. May 31st 2011.

To sum up, our empirical strategy consists in estimating first-difference models in which we compare the frequency and the overall number of days of sick leaves, the probability of seeing a physician or a physiotherapist and the amount of health-care expenses claimed between July 15th,[5] 2010 and May 31st, 2011, across individuals born in June and July 1951 and across individuals born in December 1951 and January 1952 (i.e. cohorts C1 and C2) , on the one hand; and across individuals born in December 1953 and January 1954 and across individuals born in December 1954 and January 1955 (i.e. cohorts C4 and C5), on the other hand. Thus doing, the treated groups face statutory and ordinary retirement ages four months higher than the control groups. We also present placebo estimates comparing individuals born in April *vs* May 1951 and individuals born in October *vs* November 1951, 1953 and 1954. For them, the statutory and ordinary retirement ages are indeed the same across placebo treatment and control groups.

2.2. Impact of the Reform on Sickness Absences

We first estimate the effect of increasing retirement age by four months on the probability of having at least one sickness absence, in our two samples, using a linear probability model:

$$SA_i = \alpha T_i + \beta D_i + \varepsilon_i \qquad (1)$$

where SA_i is a dummy variable equal to 1 if individual i had at least one sickness absence starting between July 15th 2010 and May 31st 2011, and 0 otherwise. T_i is a dummy variable equal to 1 if individual i belongs to the treated group – i.e. was born in July 1951 or January 1952 in the first sample and in January 1954 or 1955 in the second sample – and 0 otherwise. D_i is a dummy variable equal to 1 if individual i belongs to cohorts C2 or C5, according to the sample, and 0 otherwise. ε_i is an error term.

5. *We use July 15th instead of July 14th as our start date since July 14th is a public holiday in France so that people do not work on that day and most medical practices and pharmacies are closed.*

As a second step, we consider the impact of the reform on the total number of sick days cumulated over the period ranging from July 15th 2010 to May 31st 2011. Since the latter is a highly skewed count variable with excess zero observations (about 93% of the C1+C2 sample and 89% of the C4+C5 sample) and overdispersion – the conditional variance exceeds the conditional mean – we estimate a zero-inflated negative binomial model. The model is a mixture distribution model combining two processes: the first one generates the zero counts and the second one generates counts from a binomial model:

$$\begin{cases} Pr(NSD_i = 0 \mid T_i, D_i) = \Phi\left(\vartheta T_i + \varphi D_i\right) + \\ \qquad \left(1 - \Phi\left(\vartheta T_i + \varphi D_i\right)\right) g(0 \mid T_i, D_i) \\ Pr(NSD_i \mid T_i, D_i) = \left(1 - \Phi\left(\vartheta T_i + \varphi D_i\right)\right) \\ \qquad g(NSD_i \mid T_i, D_i) \end{cases} \tag{2}$$

where NSD_i denotes the number of days individual i was on sick leave between July 15th 2010 and May 31st 2011 for sick leaves starting during this period. Φ is the normal link function and $g(.)$ is the negative binomial distribution.

2.3. Impact of the Reform on Physicians' and Physiotherapists' Visits

We then estimate the effect of increasing retirement age by four months on the probability of seeing a GP, a specialist physician or, alternatively, a physiotherapist. The corresponding linear probability model is:

$$V_i = \gamma T_i + \delta D_i + \vartheta_i \tag{3}$$

where V_i is a dummy variable equal to 1 if individual i saw a GP – or alternatively a specialist physician or a physiotherapist – at least once between July 15th 2010 and May 31st 2011, and 0 otherwise.

2.4. Impact of the Reform on Health-Care Expenditure

Finally, we investigate the impact of the increase in minimum retirement ages on health-care expenditure. Since the effect may be different according to individuals' initial health conditions, and hence health-care expenditure, we allow it to vary along the distribution of expenditure. To do so, we estimate unconditional quantile regressions. We consider the distribution of health-care expenditure of both treated and control individuals in our two samples, separately. Following Dube (2019), we denote by $Y_{i,v}$ a binary indicator equal to 1 when individual i has health-care expenditure greater than semi-decile – i.e. ventile – υ, and 0 otherwise. We then estimate the following linear probability model:[6]

$$Y_{i,v} = \theta_v T_i + \mu D_i + \tau_i \tag{4}$$

The set of estimated coefficients on the treatment variable, $\hat{\theta}_v$, are estimated in nineteen separate regressions. For each regression, the coefficient shows how postponing retirement by four months shifts individuals at the margin above or below the corresponding ventile of the distribution of health-care expenditure. We use these coefficients to compute the percentage change in the probability that health-care expenditure claimed by individuals be larger than each ventile of the distribution, following the announcement of the reform.

3. Data

We use the French national health insurance information system (SNIIRAM) and, more specifically, the database containing information on individual non-hospital health-care expenditure (DCIR). We focus on the general scheme which covers the universe of wage and salaried workers in the private sector as well as contract personnel working in the civil service.

The key advantage of the DCIR database is that it contains exhaustive individualised and anonymous health-care claims reimbursed by the French National Health Insurance. These claims include, in particular, sick pay, visits to a GP, a specialist physician or a physiotherapist, as well as dispensed drugs. The main drawback of these data is that, being based on Social Security files, they do not contain any socio-demographic information except gender and age. Other data sources – e.g. Hygie or EDP-Santé – contain information on both health-care consumption and individual socio-demographic and/or professional characteristics. However, they are all based on samples containing a limited number of individuals. Since our empirical strategy relies on estimating a first-difference model comparing individuals born at two different months, we need data for the entire population in order to have enough observations in the treated and control groups. This is why we use the DCIR database rather than richer but smaller datasets.

A consequence of this choice is that we have information on physicians' visits and health-care expenditure for all individuals in our population but we do not observe whether those individuals are active or inactive. We also have exhaustive

6. We have written the corresponding code using the SAS software.

information on sick leaves even if only individuals in employment and on unemployment benefits are eligible to such leaves.

For each individual in our database, we compute the number of sickness absences[7] which start date was strictly after July 14th 2010 and strictly before June 1st 2011. We then define a dummy variable equal to 1 if the individual had at least one sick leave, and 0 otherwise. For all individuals in our data, we also compute the total number of days of sick leave between July 15th 2010 and May 31st 2011. Regarding physicians' visits, we define two dummy variables equal to 1 when the individual saw a GP – or, alternatively, a medical specialist – at least once between July 15th 2010 and May 31st 2011, and 0 otherwise; we do the same for visits to a physiotherapist's. We also consider health-care expenditure. We aggregate all expenses[8] claimed by each individual between July 15th 2010 and May 31st 2011, separately for GPs', medical specialists' and physiotherapists' visits, as well as for drug dispensation. Expenditure is expressed in nominal euros.

Descriptive statistics of our samples are presented in Appendix Tables A-1 and A-2. The size of our cohorts ranges from 118,000 individuals in the older group (C1) to 134,000 individuals in the younger one (C5) (see Appendix Table A-2). On average, 7.1% of individuals in the C1+C2 sample had at least one sickness absence starting between July 15th 2010 and May 31st 2011 as compared to 10.7% in the C4+C5 sample (see Appendix Table A-1). It has to be noted that, although older individuals are less likely to have a sickness absence, their total number of sick days is larger than for younger individuals (38.6 days for the former as compared to 37.1 days for the latter, conditional on having a sickness absence).[9] Consistent with the fact that health conditions deteriorate with age, individuals in the older cohorts (C1 and C2) have a higher probability of seeing a GP, a specialised physician or a physiotherapist. They also spend more on these items, as well as on drugs. Whatever the cohort, men have a higher probability than women to start a sickness absence over the period we study: 7.3% *vs* 7% in the C1+C2 sample as compared to 11.7% *vs* 9.8% in the C4+C5 sample. In contrast, women are more likely to see a physician or a physiotherapist and, correspondingly, have higher expense claims on these items. Finally, drug expenditure is slightly higher for men than for women, whatever the cohort we consider.

4. Results

4.1. Sickness Absence

We first estimate the impact of postponing retirement on the probability of having at least one sickness absence starting between July 15th 2010 and May 31st 2011. The results are presented in Table 2.

For the older individuals – cohorts C1 and C2 – increasing the statutory and ordinary retirement ages by four months raises the probability of having at least one sickness absence by 0.86 and 0.72 percentage points for men and women respectively, significant at the 1% level. This represents a 11.8% increase in the probability of sick leave for men – 10.3% for women – when computed at sample average. Results are similar, although of smaller magnitude, for younger individuals (from cohorts C4 and C5): increasing minimum retirement ages by four months increases the probability of sickness absence by 6.7% and 3.9% for men and women

7. To the extent that we rely on Social Security data, we only have information on sickness absences compensated by the Social Security. These are typically absences longer than 3 days.
8. We consider health-care expenses at the rate covered by the National Health insurance. Thus doing, we exclude extra statutory fees charged to the patient, as these vary greatly across medical specialties and location.
9. A reason why older individuals are less likely to have a sick leave may be that they are more selected than younger ones if those with particularly bad health status have already left the labour market. If this is the case, our estimates are likely lower bounds since individuals in better health conditions are less likely to be strongly affected by a 4-month increase in retirement age.

Table 2 – **Impact of a 4-month increase in retirement age on the probability of having at least one sickness absence – Linear probability model**

Dep. Var At least one sickness absence	Cohorts C1 and C2		Cohorts C4 and C5	
	Men	Women	Men	Women
Treatment	0.0086***(0.0015)	0.0072***(0.0014)	0.0078***(0.0018)	0.0038** (0.0016)
Intercept	0.0622***(0.0014)	0.0644***(0.0014)	0.1127***(0.0016)	0.0933***(0.0014)
Cohort dummy	Yes	Yes	Yes	Yes
Observations	114,767	132,928	122,282	144,371

**p<0.05. ***p<0.01.
Note: Cohort C1 contains individuals born in June and July 1951. Cohorts C2, C4 and C5 contain individuals born in December 1951 and January 1952, December 1953 and January 1954, and December 1954 and January 1955, respectively. Treated individuals are born in July in cohort C1 and in January in cohorts C2, C4 and C5.

respectively.[10] The reason why the effects we estimate are smaller in the C4+C5 sample than in the C1+C2 sample may be twofold. First, individuals may be more sensitive to an increase in retirement age when they are closer to their retirement date e.g. because they had already made leisure plans and are therefore more strongly disappointed. In contrast, individuals who are further away from retirement may consider that a 4-month delay does not make much of a difference for them. Alternatively, the effect of a 4-month delay in retirement may have a decreasing effect as the overall increase in retirement ages gets larger. Since retirement is postponed by 1 year and 4 months to 1 year and 8 months for individuals in the C4 and C5 cohorts – as compared to only 4 to 8 months for individuals in the C1 and C2 cohorts – the former may be less sensitive to a marginal 4-month increase than the latter. Our data do not allow disentangling the two effects – which could also combine. However, it is worth noticing that, in all cases, the effects we estimate are surprisingly large in view of the fact that the increase in statutory and ordinary retirement ages we are considering is only four months.

We then turn to the intensive margin and consider the impact of a 4-month increase in statutory and ordinary retirement ages on the number of sick days, as estimated using a zero-inflated negative binomial model. Consistent with the results presented in Table 2, all treated individuals have a lower probability of not having any sickness absence starting between July 15th 2010 and May 31st 2011 (Table 3, first row). In contrast, conditional on having a sick leave, we do not find any evidence that delaying retirement increases the number of sick days: whether we consider men or women and younger or older cohorts, the point estimate on the treatment variable is never significant in the duration equation (Table 3, second row).[11]

To make sure that our results are due to the change in retirement ages induced by the reform, we run placebo tests for each of the preceding estimates. As regards cohorts C1 and C2, we compare individuals born in April *vs* May and October *vs* November 1951. For cohorts C4 and C5, we compare individuals born in October *vs* November 1953 on the one hand, and October *vs* November 1954, on the other hand. Whatever model we estimate, none of the results we obtain are ever significant (see Appendix Tables A-5 and A-6).

Overall, our results suggest that increasing statutory and ordinary retirement ages – even by a small amount – increases the probability that individuals have a sickness absence, whatever their age distance to retirement. In contrast, this does not seem to affect the number of sick days, conditional on having a sick leave.

These findings leave open the question of why individuals are more likely to have a sick leave when retirement ages are raised. One possibility is that they consider the reform as unfair and, to some extent, retaliate by reducing their effort. Provided that they can collude with their physician, and in particular their GP, this may give rise to more frequent sick leaves as a form of protest. This moral hazard mechanism has been put forward by d'Albis *et al*. (2020)

10. When pooling the four cohorts and including cohort fixed effects, unsurprisingly, the point estimates we find are in between those found for C1+C2 and C4+C5 separately (see Appendix Table A-3).

11. The same holds when pooling the four cohorts and including cohort fixed effects (see Appendix Table A-4).

Table 3 – **Impact of a 4-month increase in retirement age on the number of days of sickness absence – Zero-Inflated negative binomial model**

	Cohorts C1 and C2		Cohorts C4 and C5	
	Men	Women	Men	Women
	Selection equation (probability of not having a sickness absence)			
Treatment	−0.065***(0.012)	−0.054***(0.011)	−0.045***(0.010)	−0.025** (0.010)
Intercept	1.405***(0.012)	1.395***(0.011)	1.047***(0.011)	1.177***(0.010)
	Duration equation (number of days)			
Treatment	0.006 (0.036)	0.062 (0.033)	−0.014 (0.027)	−0.033 (0.027)
Intercept	3.439***(0.034)	3.335***(0.032)	3.389***(0.026)	3.372***(0.026)
Dispersion parameter α	2.997***(0.094)	2.880***(0.085)	3.172***(0.081)	2.990***(0.074)
Cohort dummy	Yes	Yes	Yes	Yes
Observations	114,767	132,928	122,282	144,371

$^{**}p<0.05$; $^{***}p<0.01$.

Note: Cohort C1 contains individuals born in June and July 1951. Cohorts C2, C4 and C5 contain individuals born in December 1951 and January 1952, December 1953 and January 1954, and December 1954 and January 1955, respectively. Treated individuals are born in July in cohort C1 and in January in cohorts C2, C4 and C5.

regarding French teachers. A second possibility, however, is that when facing a change in retirement rules late in their career, individuals be subject to an acute stress episode generating, in turn, psychological or even physical health troubles. This is what De Grip *et al*. (2012) find for the Netherlands. In what follows, we try to disentangle the two explanations by considering the impact of increasing statutory and ordinary retirement ages on the probability of seeing a physician or a physiotherapist and on individual health-care expense claims.

4.2. Physicians' and Physiotherapists' Visits

We first estimate the effect of a 4-month increase in minimum retirement ages on the probability of having at least one visit with a physician in the months following the announcement of the reform. The results, shown in Table 4, suggest that postponing retirement does not increase the probability of seeing a GP, except for men in the younger cohorts. In contrast, it raises the probability of having at least one visit with a specialist physician for all individuals except men in the older cohorts, although the effect is moderate (about 1.5%). Similarly, we find that women in the older cohorts are more likely to see a physiotherapist when facing higher retirement ages, with an increase in the corresponding probability by 3.4% on average.[12]

This evidence is not quite consistent with an interpretation based on moral hazard since, in this case, we would expect treated individuals to have a greater probability of seeing their GP in order to be prescribed a sick leave. In contrast, we would not expect them to see a specialist physician more frequently since collusion with such physicians is quite unlikely. Moreover, treated women from older cohorts would have no reason to see a physiotherapist since the latter are not allowed to prescribe sick leaves. Overall, our set of results regarding physicians' and physiotherapists' visits are more in line with the idea that workers affected by changes in the pension system late in their career may suffer from psychological, or even physical, health troubles.

4.3. Health-Care Expenditure

As a second step, we estimate the effect of a 4-month increase in statutory and ordinary retirement ages on health expense claims using unconditional quantile regressions. Whenever significant, these effects are presented in Figures I to VI. For each ventile of the distribution of health-care expenditures, the graphs show how postponing retirement by four months changes the probability that expenditures claimed by individuals affected by this increase be larger than this ventile. Regarding GPs, consistent with what we find for men in the younger cohorts in Table 4, delaying retirement increases their GP's expense claims significantly. This is particularly so in the lower part of the distribution (Figure I): until the 55th percentile, the probability that expenses be higher than any given ventile increases by about 1.2% following a 4-month increase in retirement ages.

Although the effect remains positive in the upper part of the distribution, it is no longer significant since confidence intervals get larger. Regarding expense claims for specialist physicians, the effect of postponing retirement is positive for all groups except men in the older cohorts. For women in the older cohorts and men in the younger ones, the increase is modest, although significant over most of the distribution – i.e. until the 80th percentile (Figures II and III).

12. When pooling the four cohorts and including cohort fixed effects, the results we obtain are essentially unchanged: a 4-month increase in minimum retirement age has no effect on the probability of seeing a GP while it increases the probability of seeing a specialist physician for both men and women, and the probability of seeing physiotherapist though for women only (see Appendix Table A-7). Placebo tests are presented in Appendix Table A-8.

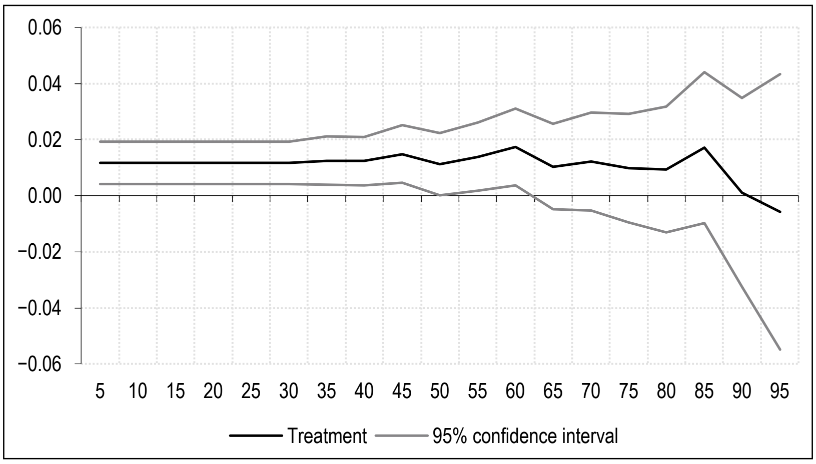
Table 4 – **Impact of a 4-month increase in retirement age on the probability of having at least one physician's visit – Linear probability model**

Dep. Var At least one visit	Cohorts C1 and C2		Cohorts C4 and C5	
Impact of the treatment on:	Men	Women	Men	Women
Visit to a GP	−0.003(0.002)	0.001 (0.002)	0.008***(0.002)	0.001 (0.002)
Visit to a specialist physician	0.002(0.003)	0.008***(0.003)	0.008***(0.003)	0.008***(0.002)
Visit to a physiotherapist	−0.001(0.002)	0.005** (0.002)	0.003 (0.002)	0.003 (0.002)
Cohort dummy	Yes	Yes	Yes	Yes
Observations	114,767	132,928	122,282	144,371

**p<0.05; ***p<0.01.

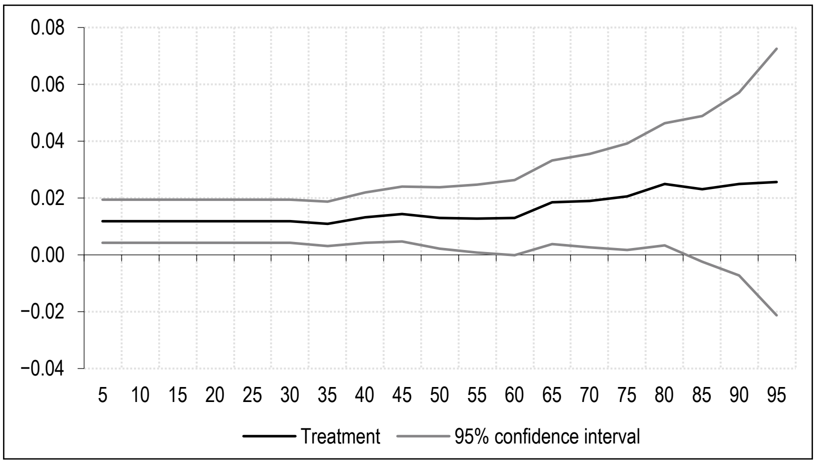
Note: Cohort C1 contains individuals born in June and July 1951. Cohorts C2, C4 and C5 contain individuals born in December 1951 and January 1952, December 1953 and January 1954, and December 1954 and January 1955, respectively. Treated individuals are born in July in cohort C1 and in January in cohorts C2, C4 and C5.

Figure I – **Impact of a 4-month increase in retirement age on the distribution of expense claims for GP visits – Men (Cohorts C4 and C5)**



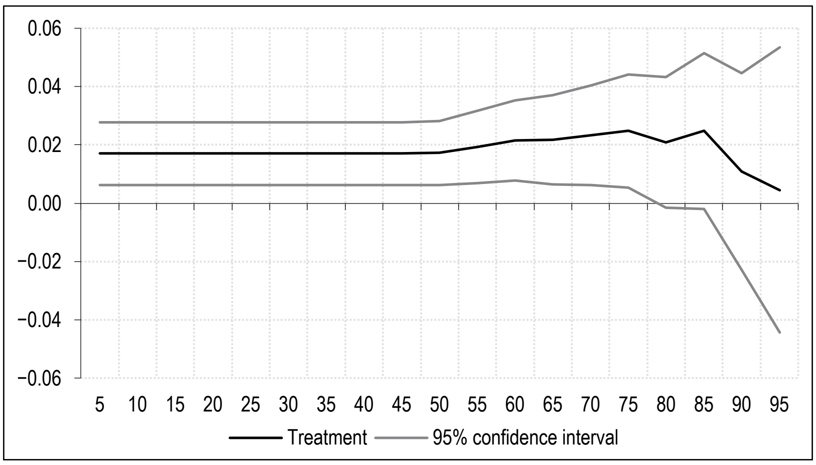
Source: Authors' calculation, based on DCIR (SNIIRAM).

Figure II – **Impact of a 4-month increase in retirement age on the distribution of expense claims for specialist physician visits – Women (Cohorts C1 and C2)**



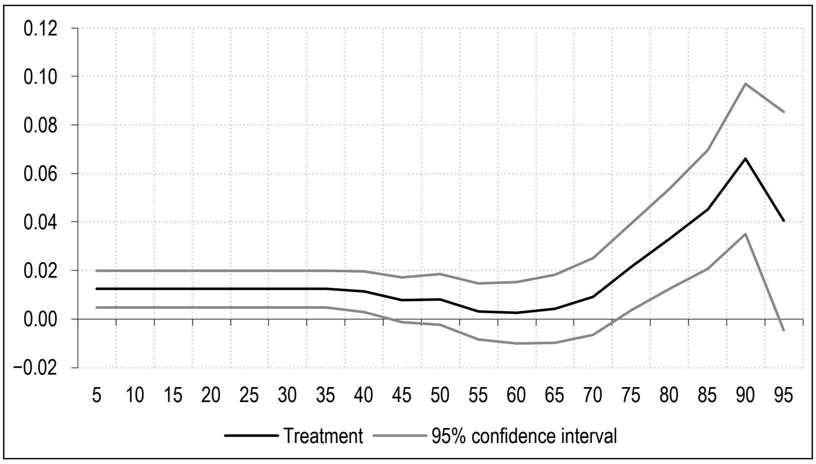
Source: Authors' calculation, based on DCIR (SNIIRAM).

Figure III – **Impact of a 4-month increase in retirement age on the distribution of expense claims for specialist physician visits – Men (Cohorts C4 and C5)**



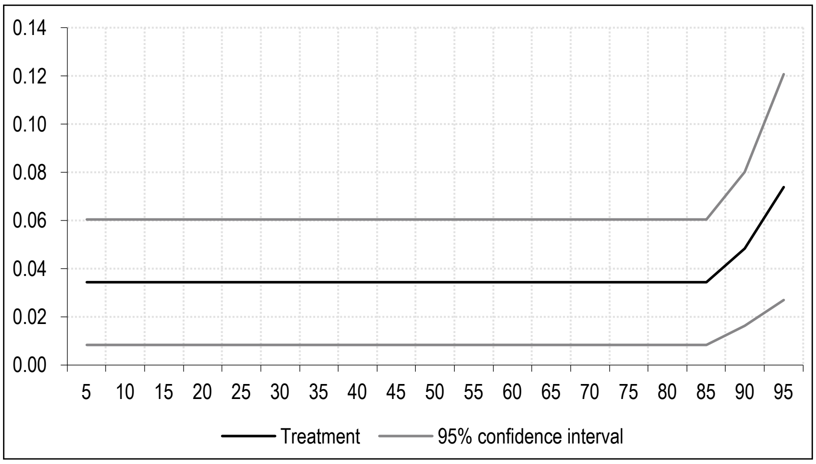
Source: Authors' calculation, based on DCIR (SNIIRAM).

Figure IV – **Impact of a 4-month increase in retirement age on the distribution of expense claims for specialist physician visits – Women (Cohorts C4 and C5)**



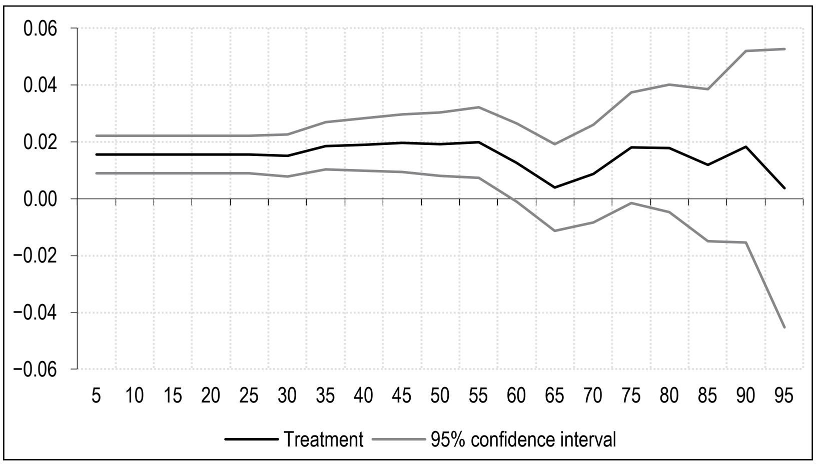
Source: Authors' calculation, based on DCIR (SNIIRAM).

Figure V – **Impact of a 4-month increase in retirement age on the distribution of expense claims for physiotherapy – Women (Cohorts C1 and C2)**



Source: Authors' calculation, based on DCIR (SNIIRAM).

Figure VI – **Impact of a 4-month increase in retirement age on the distribution of expense claims for drug dispensation – Men (Cohorts C4 and C5)**



Source: Authors' calculation, based on DCIR (SNIIRAM).

For younger women, the probability that specialist physicians' expenses be higher than any given ventile increases by about 1.2% until the 40th percentile; it remains unchanged in the middle of the distribution but increases again – by 2 to 6% – between the 75th and 85th percentiles (Figure IV).

As regards physiotherapists' expenditure, consistent with the results presented in Table 4, we find that delaying retirement increases expense claims for women in the older cohorts: the probability that expenses be higher than any given ventile increases by about 3.4% until the 85th percentile[13] and even more so, in the upper part of the distribution (Figure V).

Finally, drug expenditure of treated men in the younger cohorts also increases following a 4-month increase in retirement age: the probability that expense claims be higher than any given ventile increases by 1.5% to 2% until the 60th percentile. The effect remains stable higher up in the distribution but confidence intervals get larger so that it is no longer significant at the 5% level (Figure VI).

Overall, our findings suggest that increasing minimum retirement ages by four months has a non-negligible effect on health-care expenditure generated by physicians' and physiotherapists' visits, as well as drug dispensation. This pattern of results is consistent with a deterioration of the health conditions of individuals affected by the reform and supports the idea that the increase in the probability of sick leave that we observe in reaction to the reform is not only generated by moral hazard.

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In this paper, we have investigated the health-consumption effects of a 4-month increase in retirement age, on individuals who were close to retirement age but had not yet retired by the time the reform was passed. We show that the probability of having at least one sickness absence increases by 11.8% for men and 10.3% for women in the older cohorts, and by 6.7 and 3.9% respectively in the younger ones. These effects are surprisingly large in view of the fact that the increase in retirement age we are considering is only one third of a year. In contrast, we do not find any effect of the reform on the overall number of sick days, conditional on having a sick leave. In addition, increasing retirement age by four months does not raise the probability of seeing a GP, except for men in the younger cohorts. In contrast, it increases the probability of having at least one visit with a specialist physician for all individuals except men in the older cohorts, although the effect is limited in size (about 1.5%). Delaying retirement also increases the probability of seeing a physiotherapist by 3.4% among women in the older cohorts. Consistent with these results, we find increases in expense claims, in particular for treated individuals with low initial levels of health expenditure, in reaction to the reform.

Our findings are not quite consistent with an interpretation of the increase in the frequency of sickness absences as driven by moral hazard such as the one put forward by d'Albis *et al.* (2020). If sick leaves were merely a form of protest, we would expect to see a higher probability of visiting a GP since family physicians are more likely to collude with workers than specialist physicians. Moreover, physiotherapists' visits and drug consumption would have no reason increase. The moral hazard interpretation is not supported by our data since we observe an increase in specialists' and physiotherapists' visits, along with higher drug expenditure, in particular at the bottom of the distribution. In contrast, these findings are consistent with the idea put forward by De Grip *et al.* (2012) according to which individuals affected by adverse changes to the pension system late in their career experience a severe disappointment. A plausible mechanism is that this generates an upsurge in stress which eventually induces psychological, and even physical, health troubles.

To the extent that we estimate an intention-to-treat model, our results are likely to represent a lower bound. Individuals who have already left the labour market by the time we observe their health expense claims are indeed non compliers. Since the literature has shown that the latter tend to be in poorer health than individuals who are still in employment (Kuhn, 2018), our effects are estimated on a selected sample of individuals whose health is likely more resistant to external shocks than average.

One caveat though is that the period we study spans the ten months and a half following the announcement of the reform. As a consequence, the effects we estimate are mechanically short run and we do not know whether they may persist in the medium and long run or not. Nonetheless,

13. All percentiles being equal up to the 85th, the effect of the treatment is the same up to that level.

we can safely conclude that increasing retirement age by four months has a large effect on the probability of sick leave and a non-negligible impact on health-care expenditure, at least in the months following the announcement of the reform.

One may wonder how a 4-month increase in retirement age may have such a substantial effect on health-care consumption. A first mechanism may be that individuals who were close to retirement had made leisure plans which are deceived by the reform. Bitter disappointment may generate psychological distress which may, in turn, affect physical wellbeing. This will likely affect more strongly older cohorts who were closer to retirement age than younger ones who were already quite far away from the end of their career before the reform was passed. A second – potentially complementary – mechanism is that individuals who were suffering from psychological or physical disorders (e.g. musculoskeletal disorders, pain, etc.), and who used to cope with it as best they could, decide that they have to seek medical help since they will have to work longer. This potentially affects all cohorts since older ones suffer probably more from health troubles to start with, but younger ones face a larger increase in retirement age overall, which may affect the way they react to the marginal 4-month increase we consider here.

Overall, our results suggest that delaying retirement may have negative health effects, not only at old ages – as suggested in the literature – but also earlier on, when individuals are still in employment. This may thwart the financial gains expected from an increase in retirement ages and must be taken into account when designing reforms aiming at the sustainability of pension, and more generally, social systems. ☐

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Table A-1 – **Descriptive statistics**

Variable	All		Men		Women	
	C1+C2	C4+C5	C1+C2	C4+C5	C1+C2	C4+C5
Men						
Mean	0.463	0.458	-	-	-	-
Standard Deviation	0.498	0.498	-	-	-	-
At least 1 sickness absence						
Mean	0.071	0.107	0.073	0.117	0.070	0.098
Standard Deviation	0.026	0.309	0.260	0.321	0.255	0.297
Number of sick days (if >0)						
Mean	38.63	37.14	40.02	37.38	37.99	36.28
Standard Deviation	55.53	54.71	57.33	53.82	55.84	53.52
At least 1 visit to the GP's						
Mean	0.733	0.707	0.723	0.688	0.741	0.723
Standard Deviation	0.442	0.455	0.447	0.463	0.438	0.447
At least 1 specialist visit						
Mean	0.619	0.593	0.561	0.523	0.669	0.652
Standard Deviation	0.485	0.491	0.496	0.499	0.470	0.476
At least 1 visit to the physiotherapist's						
Mean	0.127	0.123	0.106	0.103	0.145	0.140
Standard Deviation	0.333	0.328	0.308	0.303	0.352	0.347
Expenditure on GP visits						
Mean	72.99	68.89	68.92	63.09	76.52	73.81
Standard Deviation	97.98	97.55	96.62	94.28	99.00	99.97
Expenditure on specialists' visits						
Mean	98.50	90.58	83.63	73.58	111.3	104.9
Standard Deviation	190.5	181.2	181.6	169.6	196.9	189.3
Expenditure on physiotherapy						
Mean	32.27	30.68	28.61	27.67	35.43	33.23
Standard Deviation	145.5	140.4	143.2	141.2	147.4	139.6
Drug expenditure						
Mean	275.6	239.5	303.1	255.7	251.8	225.7
Standard Deviation	537.8	217.3	575.3	551.1	502.0	486.4

Note: Individuals belonging to C1 and C2 are born in June, July and December 1951, as well as January 1952. Individuals belonging to C4 and C5 are born in December 1953, January 1954, December 1954 and January 1955.

Table A-2 – **Number of individuals per month of birth**

Month of birth	All	Men	Women
April 1951	58,180	26,575	31,605
May 1951	60,490	27,822	32,668
June 1951 (Cohort C1)	57,568	26,527	31,041
July 1951 (Cohort C1)	60,129	27,557	32,572
October 1951	54,787	25,030	29,757
November 1951	50,670	23,187	27,483
December 1951 (Cohort C2)	61,540	28,378	33,162
January 1952 (Cohort C2)	68,329	32,305	36,153
October 1952	56,777	26,156	30,621
November 1952	54,390	24,761	29,629
December 1952 (Cohort C3)	64,025	29,312	34,713
January 1953 (Cohort C3)	68,329	31,746	36,583
October 1953	52,452	25,513	29,911
November 1953	55,424	24,244	28,208
December 1953 (Cohort C4)	64,095	29,310	34,785
January 1954 (Cohort C4)	68,641	31,641	37,000
October 1954	58,391	27,015	31,376
November 1954	54,915	25,145	29,770
December 1954 (Cohort C5)	65,210	29,794	35,424
January 1955 (Cohort C5)	68,699	31,537	37,162

Table A-3 – **Impact of a 4-month increase in retirement age on the probability of having at least one sickness absence – Linear probability model – Pooled sample**

Dep. Var At least one sickness absence	Men	Women
Treatment	0.0082***(0.0015)	0.0054***(0.0011)
Intercept	0.1134***(0.0013)	0.0987***(0.0012)
Cohort C1	−0.051*** (0.0012)	−0.033*** (0.0015)
Cohort C2	−0.039*** (0.0017)	−0.029*** (0.0014)
Cohort C4	−0.00009 (0.0017)	−0.006*** (0.0014)
Cohort C5	*Ref.*	*Ref.*
Observations	237,049	277,299

**p<0.05; ***p<0.01.
Note: Cohort C1 contains individuals born in June and July 1951. Cohorts C2, C4 and C5 contain individuals born in December 1951 and January 1952, December 1953 and January 1954, and December 1954 and January 1955, respectively. Treated individuals are born in July in cohort C1 and in January in cohorts C2, C4 and C5.

Table A-4 – **Impact of a 4-month increase in retirement age on the number of days of sickness absence – Zero-inflated negative binomial model – Pooled sample**

	Men	Women
	Selection equation (probability of not having a sickness absence)	
Treatment	−0.054***(0.008)	−0.037***(0.007)
Intercept	1.049***(0.009)	1.146***(0.009)
Cohort C1	0.342***(0.011)	0.235***(0.011)
Cohort C2	0.247***(0.011)	0.205***(0.011)
Cohort C4	0.007 (0.010)	0.041***(0.010)
Cohort C5	*Ref.*	*Ref.*
	Duration equation (number of days)	
Treatment	−0.007 (0.022)	0.004 (0.021)
Intercept	3.356***(0.023)	3.318***(0.022)
Cohort C1	0.076** (0.033)	0.040 (0.031)
Cohort C2	0.077***(0.030)	0.063** (0.029)
Cohort C4	0.037 (0.027)	0.040 (0.027)
Cohort C5	*Ref.*	*Ref.*
Dispersion parameter α	3.105***(0.061)	2.947***(0.056)
Observations	237,049	277,299

**p<0.05; ***p<0.01.
Note: Cohort C1 contains individuals born in June and July 1951. Cohorts C2, C4 and C5 contain individuals born in December 1951 and January 1952, December 1953 and January 1954, and December 1954 and January 1955, respectively. Treated individuals are born in July in cohort C1 and in January in cohorts C2, C4 and C5.

Table A-5 – **Placebo Test: impact of a 4-month increase in retirement age on the probability of having at least one sickness absence – Linear probability model**

Dep. Var At least one sickness absence	Cohorts C1 and C2		Cohorts C4 and C5	
	Men	Women	Men	Women
Treatment	−0.0012 (0.0015)	0.0027 (0.0015)	0.0038 (0.0021)	0.0029 (0.0018)
Intercept	0.0633***(0.0013)	0.0639***(0.0012)	0.1191***(0.0018)	0.1025***(0.0015)
Cohort dummy	Yes	Yes	Yes	Yes
Observations	102,614	121,513	101,917	119,265

**p<0.05; ***p<0.01.
Note: Cohorts C1 and C2 pool individuals born in April, May, October and November 1951. Cohorts C4 and C5 pool individuals born in October and November 1953 and 1954. “Treated” individuals are born either in May or in November.

Table A-6 – **Placebo Test: impact of a 4-month increase in retirement age on the number of days of sickness absence – Zero-inflated negative binomial model**

Dep. Var Number of days of sickness absence	Cohorts C1 and C2		Cohorts C4 and C5	
	Men	Women	Men	Women
	Selection equation (probability of not having a sickness absence)			
Treatment	0.011 (0.013)	−0.021 (0.012)	−0.020 (0.011)	−0.017 (0.011)
Intercept	1.396***(0.013)	1.401***(0.035)	1.000***(0.012)	1.107***(0.011)
	Duration equation (number of days)			
Treatment	0.039 (0.039)	0.013 (0.035)	0.002 (0.030)	−0.017 (0.029)
Intercept	3.444***(0.037)	3.407***(0.032)	3.354***(0.028)	3.321***(0.027)
Dispersion parameter α	3.041***(0.107)	2.876***(0.090)	3.283***(0.092)	3.133***(0.085)
Cohort dummy	Yes	Yes	Yes	Yes
Observations	102,614	121,513	101,917	119,265

**p<0.05; ***p<0.01.
Note: Cohorts C1 and C2 pool individuals born in April, May, October and November 1951. Cohorts C4 and C5 pool individuals born in October and November 1953 and 1954. “Treated” individuals are born either in May or in November.

Table A-7 – **Impact of a 4-month increase in retirement age on the probability of having at least one physician's visit – Linear probability model – Pooled sample**

Impact of the treatment on:	Men	Women
Visit to a GP	0.003 (0.002)	0.0005 (0.002)
Visit to a specialist physician	0.005***(0.002)	0.007***(0.002)
Visit to a physiotherapist	0.001 (0.001)	0.004***(0.001)
Cohort dummies	Yes	Yes
Observations	237,049	277,299

**p<0.05; ***p<0.01.
Note: Cohort C1 contains individuals born in June and July 1951. Cohorts C2, C4 and C5 contain individuals born in December 1951 and January 1952, December 1953 and January 1954, and December 1954 and January 1955, respectively. Treated individuals are born in July in cohort C1 and in January in cohorts C2, C4 and C5. Our model controls for 4 cohort dummies; cohort C5 is the reference.

Table A-8 – **Placebo test: impact of a 4-month increase in retirement age on the probability of having at least one physician's visit – Linear probability model**

Dep. Var At least one visit	Cohorts C1 and C2		Cohorts C4 and C5	
Impact of the treatment on:	Men	Women	Men	Women
Visit to a GP	−0.001(0.003)	0.001(0.002)	0.001(0.003)	0.004(0.002)
Visit to a specialist physician	0.001(0.003)	−0.001(0.003)	0.005(0.003)	0.002(0.003)
Visit to a physiotherapist	−0.003(0.002)	0.002(0.002)	0.001(0.002)	0.001(0.002)
Cohort dummy	Yes	Yes	Yes	Yes
Observations	102,614	121,513	101,917	119,265

**p<0.05; ***p<0.01.
Note: Cohorts C1 and C2 pool individuals born in April, May, October and November 1951. Cohorts C4 and C5 pool individuals born in October and November 1953 and 1954. "Treated" individuals are born either in May or in November.