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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 not retired yet by the time the reform was passed. Using administrative data on individual sick-leave claims and non-hospital health-care expenses, we show that the probability of having at least one sickness absence increases for all treated groups, while the duration of sick leaves remains unchanged. Delaying 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 a visit with 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 expense claims, in particular in the lower part of the expenditure distribution.

Keywords: pension reform, retirement age, health, health-care consumption

JEL codes: I10, J14, J18, J26

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Introduction

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 age¹ 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, thereby contributing to the financial balance of the pension and, more generally, the social security systems.

This virtuous circle could, however, break down 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 and Picchio (2021) 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 and 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.

However, pension reforms increasing retirement age are also likely to affect *pre-retirement* health outcomes. If the health conditions of employees affected by the reform tend to deteriorate, this may hamper their ability to work and hence reduce 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 retired by the time the reform was passed. By mid-July 2010, the French

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.

government announced that the statutory retirement age (SRA) – resp. the ordinary retirement age (ORA) – would increase by four 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 an RDD estimate of the impact of a 4-month increase in retirement age on the sickness absences, doctors' and physiotherapists' visits as well as health-care expenditure of individuals who were at most 5.5 years away from retirement before the reform was passed.

More specifically, we consider two different samples composed of individuals who were closer to and further away from 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. 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 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 length of sick leaves, conditional on having one, when estimating a zero-inflated negative binomial model. As regards doctors' visits, we show that delaying 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,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

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.

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 and 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 potential 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. One mechanism explaining these findings may be that, by changing the retirement horizon, the reforms also modified the incentives for employers and employees to invest in 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)'s. 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. Our findings are 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 and 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 and 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 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 announced 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 Section 5 concludes.

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 – see 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 following schedule:

| 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 |

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 1956 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

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

Empirical set-up

Our data (see below) do not contain information on the employment status of individuals. Since we do not know whether they are still in the labour force nor at what age they would have liked to retire, we estimate an intention-to-treat model.

Our identification strategy relies on a regression-discontinuity design comparing sickness absences, doctors' and physiotherapists' visits as well as health-care expense claims of individuals whose statutory and ordinary retirement ages are four 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. 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 compare individuals born in June to individuals born in July 1951 – who belong to cohort C1 –, and individuals born in December of one year (1951 to 1954) to individuals born in January of the following year (1952 to 1955) – who belong to cohorts C2, C3, C4 and C5 respectively.³ Thus

³ Cohort C1 contains 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; C4, individuals born in December 1953 and January 1954; and C5, individuals born in December 1954 and January 1955. 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.

doing, they 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.

We exclude from our analysis cohort C3, born in December 1952 and January 1953. Our identifying assumption is indeed that the only difference in the health-care outcomes of the treated and control groups is 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 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 education affects health outcomes, our identifying assumption 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. Another difference between the two samples is that 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.

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 regression-discontinuity models in which we compare the frequency and length of sick leaves, the probability of seeing a doctor

or a physiotherapist and the amount of health-care expenses claimed between July 15th,⁴ 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.

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:

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

where SA 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 is a dummy variable equal to 1 if the individual 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 is a dummy variable equal to 1 if the individual belongs to cohorts C2 or C4, according to the sample, and 0 otherwise. ε_i is an error term.

As a second step, we consider the impact of the reform announcement on the number of days of sickness absence. 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:

$$\begin{cases} Pr(NSD_i = 0) = \Phi(\alpha_s T_i + \beta_s D_i) + (1 - \Phi(\alpha_s T_i + \beta_s D_i))g(0) \\ Pr(NSD_i) = (1 - \Phi(\alpha_s T_i + \beta_s D_i))g(NSD_i) \end{cases} \quad (2)$$

⁴ 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 doctors and pharmacies are closed.

where NSD 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 $g(\cdot)$ is the negative binomial distribution modelled as a function of T_i and D_i .

Impact of the reform on doctors' 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:

$$V_i = \gamma T_i + \delta D_i + \vartheta_i \quad (3)$$

where V 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.

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 – v , and 0 otherwise. We then estimate the following linear probability model:

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

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 DCIR database 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 DCIR also collects gender and birth dates but does not contain information on individuals' socio-demographic or career-related characteristics.⁵

For each individual in our database, we compute the number of sickness absences⁶ 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 number of days of sick leave between July 15th 2010 and May 31st 2011. Regarding doctors' 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 expenses⁷ 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

⁵ 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 an RDD model comparing individuals born at 2 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.

⁶ 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.

⁷ 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.

that, although older individuals are less likely to have a sickness absence, their sick leaves are on average longer than those of younger individuals (38.6 days for the former as compared to 37.1 days for the latter, conditional on having a sickness absence).⁸ 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 doctor 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 doctor 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

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 1. 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 respectively. 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

⁸ Another 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 a 4-month increase in retirement age.

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 1, all treated individuals have a lower probability of not having any sickness absence starting between July 15th 2010 and May 31st 2011 – see Table 2, first row. In contrast, conditional on having a sick leave, we do not find any evidence that delaying retirement increases the length of sickness absences: 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 – see Table 2, second row.

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.3 and A.4.

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 average duration of sick leaves.

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 doctor, 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) 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 between the two explanations by considering the impact of increasing statutory and ordinary retirement ages on the probability of seeing a doctor or a physiotherapist and on individual health-care expense claims.

Doctors' 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 doctor's visit in the months following the announcement of the reform. The results, shown in Table 3, 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.⁹

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 doctors 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 doctors' 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.

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 1 to 6. 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

⁹ Placebo tests for these estimates are presented in Appendix Table A.5.

Table 4, delaying retirement increases their GP's expense claims significantly. This is particularly so in the lower part of the distribution – see Figure 1 –: 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) – see Figures 2 and 3. For younger women, the probability that specialist doctors' 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 – see Figure 4. 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 percentile¹⁰ and even more so, in the upper part of the distribution – see Figure 5. Finally, drug expenditure of treated men in the younger cohorts also increase 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 – see Figure 6.

Overall, our findings suggest that increasing minimum retirement ages by four months has a non-negligible effect on health-care expenditure generated by doctors' 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.

5. Conclusion

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

¹⁰ All percentiles being equal up to the 85th, the effect of the treatment is the same up to that level.

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 length of sick leaves. 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. If sick leaves were merely a form of protest, we would expect to see a higher probability of visiting a GP since family doctors 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 that individuals affected by adverse changes to the pension system late in their career experience 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. All 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, 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 for the few 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 problems (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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Tables

Table 1 – 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) |
| Cohort dummy | yes | yes | yes | yes |
| Observations | 114767 | 132928 | 122282 | 144371 |

Note: cohorts C1 and C2 pool individuals born in June, July and December 1951 as well as January 1952. Cohorts C4 and C5 pool individuals born in December of 1953 and 1954 and January of 1954 and 1955. Treated individuals are born either in July or in January. **p<0.05; ***p<0.01.

Table 2 – 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) | -0.065*** (0.012) | -0.054*** (0.011) | -0.045*** (0.010) | -0.025** (0.010) |
| Duration equation (number of days) | 0.005 (0.035) | 0.062 (0.033) | -0.013 (0.027) | -0.033 (0.027) |
| Cohort dummy | yes | yes | yes | yes |
| Observations | 114767 | 132928 | 122282 | 144371 |

Note: cohorts C1 and C2 pool individuals born in June, July and December 1951 as well as January 1952. Cohorts C4 and C5 pool individuals born in December of 1953 and 1954 and January of 1954 and 1955. Treated individuals are born either in July or in January. **p<0.05; ***p<0.01.

Table 3 – Impact of a 4-month increase in retirement age on the probability of having at least one doctor's visit – Linear probability model.

| Dep. Var At least one visit | Cohorts C1 and C2 | | Cohorts C4 and C5 | |
|---------------------------------|----------------------|---------------------|----------------------|---------------------|
| | Men | Women | Men | Women |
| Impact of the Treatment on: | | | | |
| 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 | 114767 | 132928 | 122282 | 144371 |

Note: cohorts C1 and C2 pool individuals born in June, July and December 1951 as well as January 1952. Cohorts C4 and C5 pool individuals born in December of 1953 and 1954 and January of 1954 and 1955. Treated individuals are born either in July or in January. **p<0.05; ***p<0.01.

Figures

Figure 1 – Impact of a 4-month increase in retirement age on the distribution of expense claims for GP visits – Men (Cohorts C4 and C5)

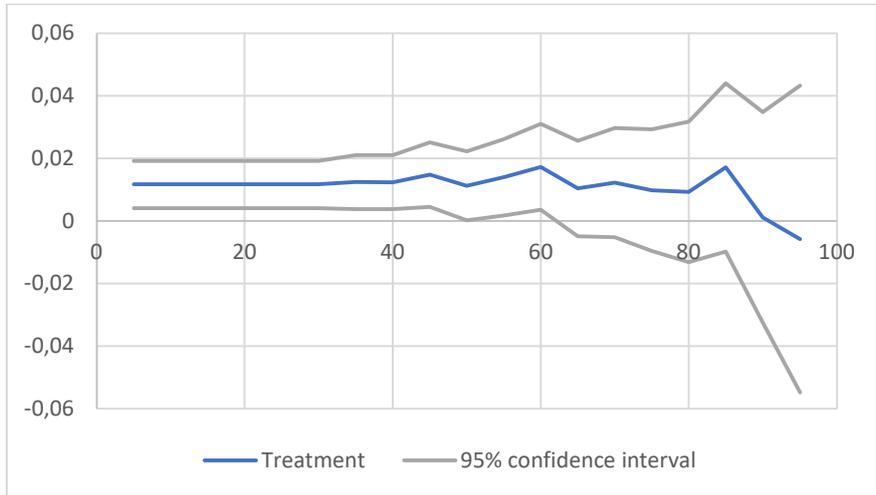


Figure 2 – Impact of a 4-month increase in retirement age on the distribution of expense claims for specialist physician visits – Women (Cohorts C1 and C2)

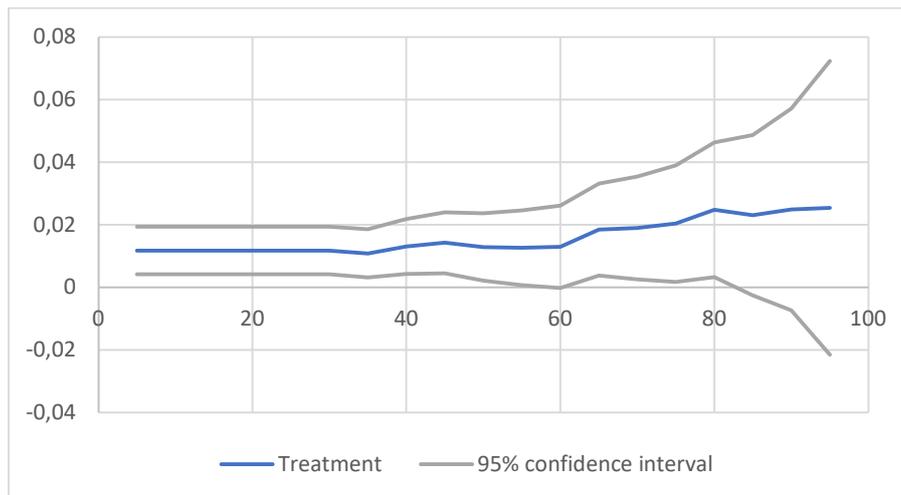


Figure 3 – Impact of a 4-month increase in retirement age on the distribution of expense claims for specialist physician visits – Men (Cohorts C4 and C5)

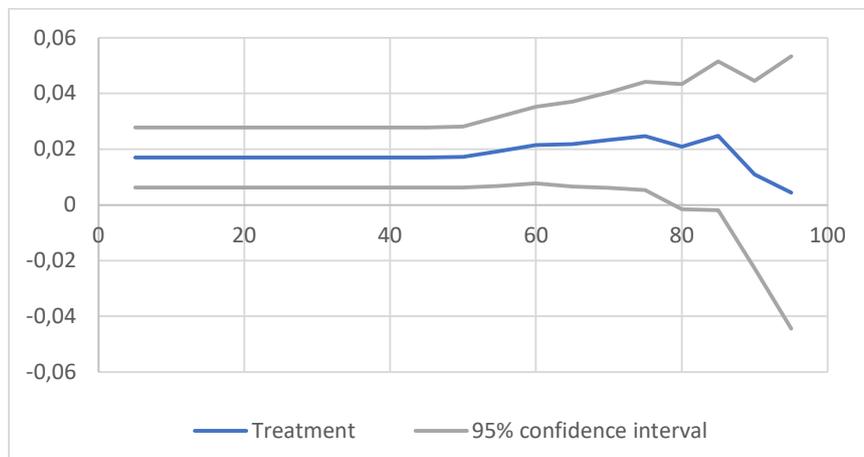


Figure 4 – Impact of a 4-month increase in retirement age on the distribution of expense claims for specialist physician visits – Women (Cohorts C4 and C5)

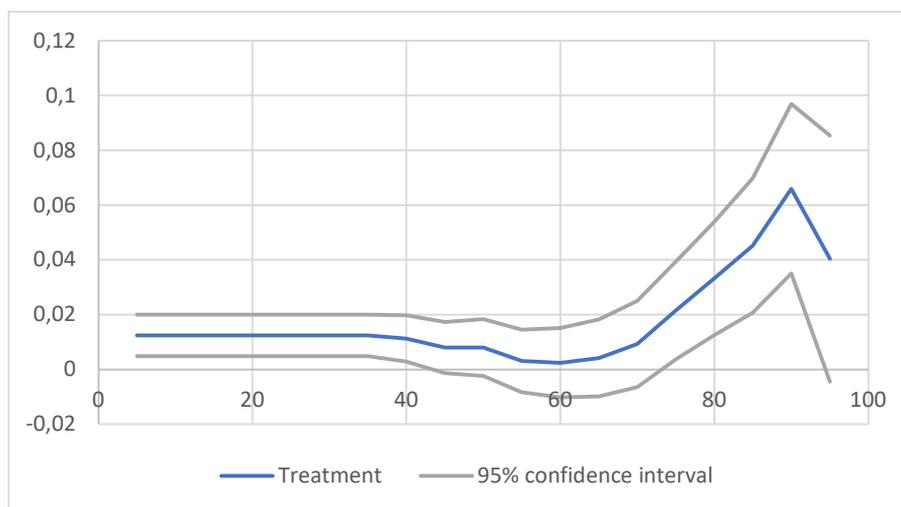


Figure 5 – Impact of a 4-month increase in retirement age on the distribution of expense claims for physiotherapy – Women (Cohorts C1 and C2)

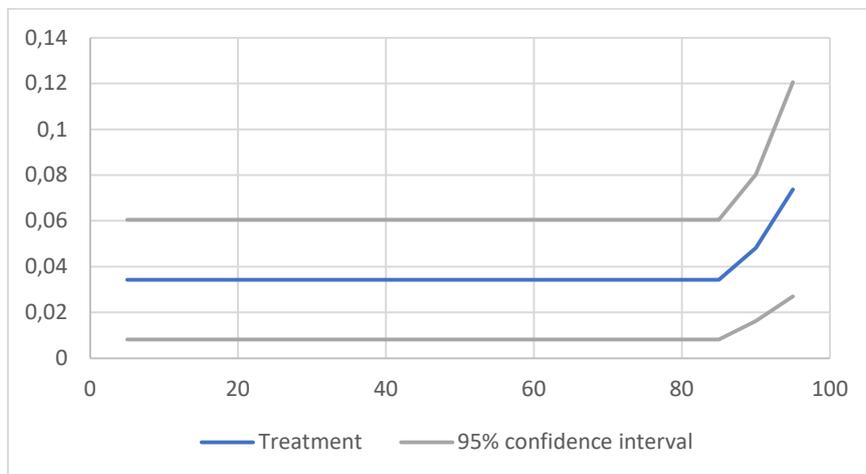
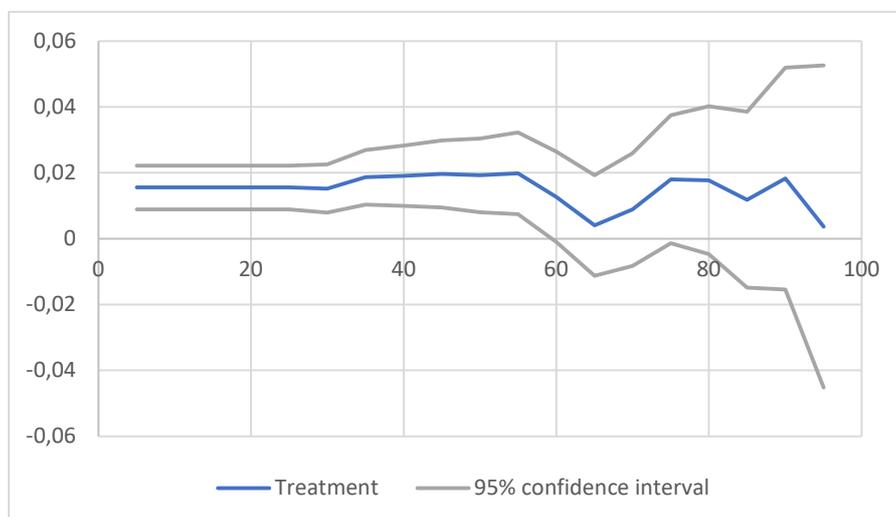


Figure 6 – Impact of a 4-month increase in retirement age on the distribution of expense claims for drug dispensation– Men (Cohorts C4 and C5)



Appendix Tables

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 | 57 568 | 26 527 | 31 041 |
| July 1951 | 60 129 | 27 557 | 32 572 |
| October 1951 | 54 787 | 25 030 | 29 757 |
| November 1951 | 50 670 | 23 187 | 27 483 |
| December 1951 | 61 540 | 28 378 | 33 162 |
| January 1952 | 68 329 | 32 305 | 36 153 |
| October 1952 | 56 777 | 26 156 | 30 621 |
| November 1952 | 54 390 | 24 761 | 29 629 |
| December 1952 | 64 025 | 29 312 | 34 713 |
| January 1953 | 68 329 | 31 746 | 36 583 |
| October 1953 | 52 452 | 25 513 | 29 911 |
| November 1953 | 55 424 | 24 244 | 28 208 |
| December 1953 | 64 095 | 29 310 | 34 785 |
| January 1954 | 68 641 | 31 641 | 37 000 |
| October 1954 | 58 391 | 27 015 | 31 376 |
| November 1954 | 54 915 | 25 145 | 29 770 |
| December 1954 | 65 210 | 29 794 | 35 424 |
| January 1955 | 68 699 | 31 537 | 37 162 |

Table A.3 – 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) |
| Cohort dummy Observations | yes 102614 | yes 121513 | yes 101917 | yes 119265 |

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

Table A.4 – 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) | 0.011 (0.012) | -0.021 (0.012) | -0.020 (0.011) | -0.017 (0.011) |
| Duration equation (number of days) | 0.040 (0.039) | 0.019 (0.034) | 0.002 (0.029) | -0.017 (0.029) |
| Cohort dummy | yes | yes | yes | yes |
| Observations | 102614 | 121513 | 101917 | 119265 |

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

Table A.5 – Placebo test: impact of a 4-month increase in retirement age on the probability of having at least one doctor's visit – Linear probability model.

| Dep. Var At least one visit | Cohorts C1 and C2 | | Cohorts C4 and C5 | |
|---------------------------------|----------------------|-------------------|----------------------|------------------|
| | Men | Women | Men | Women |
| Impact of the Treatment on: | | | | |
| 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 | 102614 | 121513 | 101917 | 119265 |

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