How does higher full retirement age affect careers? Evidence from an increase in the full retirement age in Belgium

Bart Cockx[1,2,3,4], Muriel Dejemeppe[2], Corinna Ghirelli[1,2,5], Yannick Thuy[1], and Bruno Van der Linden[2,3,4,6]

[1]*SHERPPA - Ghent University (Ghent, Belgium)*
[2]*IRES - Université Catholique de Louvain (Louvain-La-Neuve, Belgium)*
[3]*IZA (Bonn, Germany)*
[4]*CESIfo (Munich, Germany)*
[5]*European Commission, DG Joint Research Centre*
[6]*FNRS (Belgium)*

January 3, 2017
Preliminary version, please do not quote

Abstract

We study the impact of an increase in the full retirement age on the retirement decisions of older workers in Belgium, a country with an abundance of early retirement possibilities. We examine a reform that increased the full retirement age for women from 63 to 64 as from January 2006 for specific birth cohorts. We use the resulting quasi-experimental setting to implement a regression discontinuity design based on women's birth dates. Our data is drawn from social security administrative data and the National Register containing all Belgian inhabitants. Overall the reform does not affect the proportion of women who work until retirement, nor the proportion of women who withdraw prematurely from the labour market through early retirement schemes. Therefore, the reform does not affect the pathway into retirement. However, the women who are still employed at the age of 63 prolong their career until the new full retirement age. Therefore, the reform strongly affects the retirement decision of the women who survive in employment, thereby increasing the employment rate in 2006. We find no evidence of anticipation effects, since women do not reduce the working hours prior to the reform so as to cope with longer careers.

1 Introduction

Many developed countries face enormous pressure on their social security provisions due to ageing populations. An important contributing factor to this problem is the low labour force participation of older workers, especially women, and the corresponding low effective retirement ages. In the EU-15, the average effective retirement age over the period 2009-2014 was 62.6 for men and 61.7 for women, while most countries have their full retirement age set at 65 (OECD, 2015). In Belgium, the country analysed in this paper, the difference between the full retirement age and the average effective retirement age is even bigger. Since the 1980s the average effective retirement age has not been above 60, and was in 2013 59.8 for men and 59.1 for women (OECD, 2015). Part of the explanation for this can be found in the abundance of early retirement options available to older workers in Belgium.

One strategy introduced by governments to cope with population ageing has been to prolong the working career by tightening the eligibility conditions for full retirement. The idea behind this is that if people have to work more years before obtaining full pension rights, they would do so in order to keep the same pension allowances (OECD, 2006). Hence, it is important to study how old-age workers react to increases in the full retirement age, in order to assess the effectiveness of such strategy. In addition, the labour supply of old-age women has not yet been extensively investigated, as opposed to men. The former are likely to reach differently to changes in the pension system. As a matter of fact, women are more likely to have discontinuous careers. On the one hand, they may be more responsive to financial incentives. On the other hand, they may be less attached to the labour market.

In this paper we study the effect of increasing the female full retirement age (FRA) on women's retirement decisions and labour supply. We exploit a reform that was implemented in Belgium and raised the female FRA from 60 to 65 between 1997 and 2009, with increases of one year of age each three calendar years. Due to data availability we are able to examine the causal effects of the last two policy changes, the increase in the FRA from 63 to 64 as from January 1, 2006 and the subsequent increase in the FRA from 64 to 65 as from January 1, 2009. In what follows, we will focus on the former policy change. Women born in November 1942 or before are not affected by the 2006 reform since they reach the old FRA before January 2006. By contrast, women born in December 1942 or later are affected by the policy. The latter can (i) comply with the reform, (ii) withdraw from the labour market by entering one of the available early retirement schemes, or (iii) claim the anticipated pension, i.e. enter retirement before reaching the new FRA. In this case the pension benefit is proportionally reduced due to the short career. Importantly, this proportional reduction in the pension is not coupled by any additional actuarial adjustment to the benefit so as to penalise early claims.[1] This is in contrast with the pension schemes of many other countries, where the choice of early claims have bigger consequences in terms of the pension level. Such difference

[1]These actuarial adjustments were present until 1991 and were abolished since 1992. In 2006, these actuarial reductions are still in place for the self-employment pension regime: the pension benefit is cut by 5% for each year of anticipation.

makes it interesting to investigate the impact of increasing the FRA in a context where actuarial adjustments are absent.

We exploit very rich administrative data on labour market histories (for private sector employment from as early as 1957), detailed information about the labour market status and social security provisions at the end of the quarter, firm and household characteristics. Based on these data we can estimate the impact of the pension reform on the employment rate, the (early) retirement rate and the participation rate in the social security schemes that act as early exit-routes out of the labour market. The reform entails an increase in the eligibility conditions for full pension for women born before or later than December 1942. Women born in November 1942 or earlier are eligible to full retirement once they reach age 63. By contrast, women born in December 1942 or later become eligible to full pension once they reach age 64, according to the new FRA. This allows us to exploit a sharp Regression Discontinuity Design (RDD) where the causal impact of increasing the FRA is identified by comparing the outcomes of women born just before and after the birth date threshold of December 1, 1942.[2]

Our findings can be summarized as follows. Overall, the reform does not change the pathway into retirement. That is, the reform does not affect the proportion of workers who enter retirement through early retirement options. At the same time, it does not affect the proportion of "virtuous" workers who survive in employment until the FRA - measured by direct transitions from employment to full retirement. However, it induces the workers who survive in employment to remain employed longer, so as to comply with the new eligibility rules. As a consequence, the employment rate increases by 2.5-3.8 percentage points in 2006. This effect is small in absolute terms but becomes economically important relative to the low female employment rate close to the FRA, amounting to a relative increase of 31-47%.

The literature on the effect of pension reforms is rich. One strand of this literature studies the impact of pension reforms in a quasi-experimental setting, exploiting exogenous changes in the eligibility conditions to (early) retirement to identify the effects of the pension treatment. A second strand simulate the effects of pension reforms by means of structural models. Our study is mostly related to this first strand. The results of this literature suggest that increasing the eligibility age to (early) retirement can be successful in prolonging careers. Comparable studies to our own by Lalive and Staubli (2015) for Switzerland and Manoli and Weber (2016) for Austria show that a one-year-increase in the (early) retirement age postpones entry into retirement by at least 6 months.

We contribute to the literature in a number of ways. While most of the literature focuses on gradual increases in the FRA, we focus on a reform that mandates a discontinuous increase of one year in the FRA as in Switzerland (Lalive and Staubli, 2015). This allows to study the causal impact of tightening the eligibility rules for full pension with a very credible identification strategy.

Second, many of the reforms studied in the literature, including the Swiss reform, combine the increase in the FRA with actuarial adjustments so as to penalise early claims. By contrast, the Belgian pension system does not foresee additional penalties in case of early retirement claims, on

[2] Lalive and Staubli (2015) use the same approach to evaluate the increase in the FRA in Switzerland. Montizaan et al. (2010) and de Grip et al. (2012) apply this approach to study a similar reform in the Netherlands.

top of the proportional reduction in the pension benefit given by the shorter career duration.[3] This setting allows us to assess whether the financial incentives embedded in the Belgian pension system are sufficient to induce compliance with the new FRA, without having to rely on further pension cuts.

Last, we complement a very recent line of empirical research on the Belgian pension reforms based on structural models and micro-simulations. This literature suggests that partial reforms that only modify the access to some retirement routes have limited effects (Lefebvre and Orsini, 2011; Jousten et al., 2012). This paper is the first ex-post evaluation of the impact of the increase in the female FRA mandated in Belgium.[4] We contribute to the Belgian policy debate by providing evidence that the increase in the FRA is successful in raising the employment rate of women. Nevertheless, the overall effectiveness of the reform is limited due to the generosity of the early-exit options.

The paper is structured as follows. Section 2 reviews the literature. In Section 3 we describe the institutional context and the reform of interest. Section 4 describes the sampling scheme and the data. Section 5 outlines the identification strategy, while in Section 6 we present our findings. In Section 7 we discuss anticipation effects and Section 8 concludes.

2 Literature Overview

A large strand of literature evaluates the impact of reforms that increase the eligibility conditions for full pension in a number of countries. The most similar reform to ours is the reform of the Swiss pension system, where the female retirement age was increased from 62 to 63 in 2001 and from 63 to 64 in 2005, as opposed to gradual increases.[5] This reform has been evaluated in two studies, Lalive and Staubli (2015) and Hanel and Riphahn (2012). Lalive and Staubli (2015) exploit the sharp discontinuity introduced by the policy change and use a RDD based on administrative data. They find that increasing the FRA by one year delays the labour market exit by 7.9 months and the claiming of retirement benefits by 6.6 months. In contrast to our study, Lalive and Staubli (2015) focus on labour market attached women and thus obtain a result that is more closely related to an average treatment effect on the treated than to an average treatment effect.[6] The same reform has been evaluated by Hanel and Riphahn (2012) by means of a difference-in-differences (DiD) approach based on Swiss Labour Force Survey data. One-year increase in the FRA induces a decline in the age-specific annual retirement probability by over 50%. Additionally, the response to changes in financial retirement benefits varies with the educational background with less educated women responding more strongly to the reform. In line with Lalive and Staubli (2015), and again in contrast

[3] Actuarial adjustments in the private sector pension regime were abolished since 1992.

[4] Tarantchenko (2016) is currently evaluating this reform using the same data we use, but exploiting a different empirical strategy. This study is discussed at the end of Section 2.

[5] However, an important difference with the Belgian reform is the introduction of actuarial adjustments that penalise early claims with benefit reductions.

[6] They restrict their sample by not considering women never employed between ages 50 and 53, women claiming a disability pension before age 53, and women never claiming an old-age pension.

to our study, Hanel and Riphahn (2012) only consider a sub-sample of the full population.[7]

Another extensively studied pension reform is the Austrian one that increases the early retirement age in 2000 and 2004 through incremental two-months increases for each quarter for men and women. Based on the Austrian Social Security database Manoli and Weber (2016) exploit bunching methods to study the effects of retirement benefits on labor force participation. They find that one-year increase in the ERA decreases retirement by 6 to 9 months. This impact is comparable in size to the effects found for the Swiss reform discussed above. In addition, affected workers do not substitute the pension with other social insurance programs. Based on the same data, Staubli and Zweimüller (2013) used a DiD approach and find that one-year increase in the ERA increases the employment rate by 9.75 percentage points (*pp*) among affected men, and 11 *pp* among affected women. In addition, they find large spillover effects on the unemployment insurance programme (12.5 *pp* for men, 11.8 *pp* for women) but small effects on disability insurance claims. Healthy high-wage workers respond by working longer until they reach the new ERA, while low-wage workers in bad health retire through the disability insurance or leave the labour market through unemployment.

Two Dutch studies based on matched survey and administrative data for male employees in the public sector are worth mentioning. First, de Grip et al. (2012) assess the mental health effects of the Dutch pension reform in 2006, when the government abolished the favourable tax treatment of early retirement schemes in the second pension pillar for all workers born after 1949. Based on a RDD approach they find a strong deterioration in mental health for workers affected by the reform.

Second, de Grip et al. (2013) analyse the effects of the announcement of a future increase in the eligibility age for the Dutch public old-age pension on individuals' retirement expectations. The announced reform foresees an increase in the FRA from 65 to 66 in 2020 for all inhabitants born after 1954, and an increase from age 66 to 67 in 2025 for all inhabitants born after 1959. Based on a RDD approach, they find a positive impact of the announced reform on the expected retirement age. Affected employees born between 1954-1959 expect to retire 3.6 months later, those born after 1959 expect to retire 10.8 months later. The size of the effect is comparable to the effects found in the previous studies, which could indicate that expectations on retirement are a relatively good indicator for actual retirement behaviour. Furthermore, under the assumption that expected behaviour is predictive of actual behaviour, it suggests that (employed) workers anticipate policy reforms when announced well in advance.

The US have also reformed the pension system in 1983 by increasing the FRA from 64 to 67 through two-months increases every calendar year.[8] Based on CPS monthly data from 1989 to 2007, Mastrobuoni (2009) uses a DiD approach and shows that the reform increases the average retirement age by half of the increase in the FRA. This result is comparable in size to those in the studies mentioned above. Second, Duggan et al. (2007) study whether there are spillover effects on the participation in the disability insurance (DI) programme. As the 1983 reform entails a decrease in pension benefits maintaining the DI invariant, the latter becomes relative more generous. The authors find that the reform can explain one-third and one-fourth of the increase in DI enrolments of

[7]The sample is restricted to women at risk of retirement, i.e. aged 60-65 in the labour force at the first interview.

[8]Actuarial adjustments are in place so as to discourage early claims.

men and women, respectively, since 1983. This shows that the 1983 reform has important spillover effects on DI participation.

Based on UK Labour Force Survey data, Cribb et al. (2013) analyse the increase in the female state pension age from 60 to 61 in the UK, implemented between April 2010 and March 2011.[9] Based on a DiD approach, they find an increase in the female employment rate at age 60 of 7.3 *pp*, which corresponds to about one month. Additionally, they find a positive effect on unemployment (1.3 *pp*) and employment of their male partners (4.2 *pp*). The evidence is generally in line with the evidence discussed above, although the magnitude of the employment effect appears to be lower.

Finally, Hernæs et al. (2015) study the 2011 reform of the Norwegian early retirement system. The reform takes place in two ways. Some people face an increase in the earliest access age (previously set to 62), leaving pension benefits untouched (the "prescription approach", similar to the reforms in the other countries discussed above). For others, all disincentives to work while claiming pension benefits are removed and the earnings test is replaced by an actuarially fair system (the "flexibility" approach). They find that the flexibility approach appears to be more successful in raising the employment rate. Additionally, the prescription approach increases disability inflow, which is in line with the findings of Staubli and Zweimüller (2013) and Duggan et al. (2007). Overall, most impact evaluation studies, with the exception of Hernæs et al. (2015), find a positive impact of increasing the FRA on the employment rate and age of labour market exit.

Next to this strand of literature, another type of studies analyse the effects of pension reforms by means of structural models and micro-simulation techniques. Based on administrative data from the Netherlands, Bernal and Vermeulen (2014) show that an immediate increase in the retirement age from 65 to 67 increases the effective retirement age more than increasing the FRA by the same amount gradually over time.[10] Next, Fehr et al. (2012) evaluate the impact of introducing different forms of financial incentives to the benchmark reform that is implemented in Germany in 2007.[11] They show that increasing the actuarial adjustment factor would achieve stronger effects on postponing retirement. In addition, Sánchez-Martin et al. (2014) study the effects of the 2011 pension reform in Spain, which raises the FRA from 65 to 67 years old and delays early retirement from age 61 to 63. They find that the reform increases the labour supply and reduces pension expenses but it generates large welfare losses among unemployed workers. They suggest to leave the early retirement age unaffected while penalising early claims by means of reduction in generosity of the pension benefits. By contrast, Manoli et al. (2015) simulate the effects of several social security reforms in Austria based on administrative social security data. They find that retirement decisions are more sensitive to changes in their effective wages that arise through changes in eligibility requirements for benefits rather than to changes in benefits conditional on being eligible. This suggests that changes in the eligibility requirement may be very effective in raising the employment

[9] The reform is announced in 1995 and increases the earliest claiming age from 60 to 65 with increases of one month every two months over the period 2010-2020.

[10] The gradual increase in the retirement age from 65 to 67 is actually implemented in July 2012 in the Netherlands.

[11] In Germany the FRA is gradually increased from 65 to 67 as from 2012. Three reforms are considered: (i) increasing the actuarial adjustment from 3.6 to 6%; (ii) making the adjusting factor dependent on life expectancy; (iii) reducing the contribution rates for older workers.

rate. Moreover, workers may be less responsive to financial incentives in generous pension systems where early retirement options are available, such as in the Austrian case. Taken together these findings allows to speculate that introducing or refining actuarial adjustments to pension benefits may be more or less effective in delaying the entry into retirement depending on the generosity of the pension system and the availability of early retirement options.

Moreover, this paper is related to the literature on the Belgian pension system, which is mostly composed of structural models that simulate hypothetical reforms (e.g. Dellis et al., 2004; Jousten et al., 2007). In line with Manoli et al. (2015), Lefebvre and Orsini (2011) find that a reform of the eligibility rules for early retirement has a larger impact on the retirement decision than a reform of the benefit generosity. Based on micro-simulations, Jousten et al. (2012) point out that partial reforms targeting old-age pension have limited effects on the retirement behaviour as they are likely to be offset by other retirement channels unaffected by the reform.

Finally, it is worth mentioning the study by Tarantchenko (2016), which is an ongoing ex-post impact evaluation of the same reform we are evaluating in this paper. The analysis is conducted on the same administrative database we use, restricting the sample to women in salaried private sector employment. The study finds that pension claims were reduced due to the reform. There is only a modest impact on employment.[12]

3 Institutional Context

The Belgian pension system is composed of three pillars. The first pillar is the state pension, available to all workers and organised as a pay-as-you-go system. This means that today's workers finance today's pensions via social security contributions on their wages. The pension benefit depends on career duration and earned income. The second and the third pillar instead provide complementary pensions on a voluntarily basis. As opposed to the first pillar, these schemes are fully-funded, where today's workers finance their own future pension benefits. The second pillar is a supplementary pension for private sector workers organised at the company or sector level.[13] Finally, the third pillar consists of personal pension savings which qualify for tax reductions.

The focus of this paper is on the first pillar. The latter is composed of three types of pensions: the survivor pension and the guaranteed minimum income for older people, and the old-age pension. The survivor pension is granted under certain conditions to the spouse of a deceased person. The

[12]She uses a DiD approach, comparing the difference in the outcomes of the group aged 63 (treated) before and after the reform with the corresponding difference in the outcomes of the group aged 62 (controls). Several remarks should be made regarding the set-up of this study. (1) The *before*-control group becomes the *after*-treated group. If there are any anticipation effects, this could bias the results. (2) The *before*-treated group has already reached age 63 in 2005 while the *before*-control group has not yet reached this age. This implies that, before the reform, the treated group has a lower employment rate than the control group. The positive employment effect obtained in the analysis is thus almost mechanical, as the employment rate after the reform will be very similar, as neither control or treated group after the reform has reached the (new) retirement age.

[13]Self-employed workers can also set up a second pillar pension. Sectoral pension systems are built up by collective agreements between the management and the workforce of a sector of activity.

allowance is equal to 80% of the pension paid or accrued to the deceased. The guaranteed minimum income for older persons is a means-tested safety-net income that covers elderly people above the FRA who have no pension rights based on a professional activity or whose pension rights are very low.[14] Lastly, the old-age pension is the component affected by the reform of interest. It is organised in three separated regimes, one for the private sector, one for the public sector, and one for self-employment. In terms of generosity, the public sector has the most generous regime, followed in order by the private sector and the self-employment regime. The reform of interest changes the eligibility conditions of the private sector and self-employment regime, leaving the eligibility conditions of the public sector regime unaffected.

Aside to the old-age pension, there are numerous early retirement possibilities that allow workers to exit from the labor force well before the FRA while receiving a replacement income, e.g. unemployment insurance, conventional pre-retirement, interruption of work schemes, and disability insurance. Two things are worth noting. First, the time spent on replacement income is fully considered as working time for the computation of the pension benefit. Second, individuals covered by these schemes are automatically rolled over into the retirement scheme at the FRA.

The rest of the section is organised as follows. First, in Section 3.1 we describe the reform of interest. Second, in section 3.2 we illustrate in detail the old-age pension regime. We mostly refer to the private sector pension regime for two reasons. First, the private sector regime by far the largest one both in terms of the number of individuals affiliated with the programme, but also in terms of amount of contributions and benefits payments.[15] Second, it is also the sector for which the richest data is collected.[16] Finally, Section 3.3 briefly overviews the aforementioned early retirement schemes.

3.1 The reform of interest

Until 1997 the female FRA for self-employed and salaried private sector workers is 60, as opposed to 65 for men. To obtain a full pension, men have to work 45 years whereas women only have to work 40 years. As this is deemed discriminatory according to the European Court and the European Commission, the Belgian government decides to increase the female FRA to the level of men. From July 1997 until January 2009 the female FRA is increased from 60 to 65 in a stepwise fashion, with increases of one year of age each three calendar years, as shown in Table 1. These changes are all announced in 1996. For public sector workers the female FRA is already at 65 so there are no changes in that sector. Due to data availability, we are able to study the last two policy changes of January 2006 and of January 2009. In this version of the study we focus on the former reform, which brings the female FRA from 63 to 64 and the necessary full career from 43 to 44 years.

[14]It is financed by general taxation, and hence it a social assistance benefit and not a security benefit.

[15]The self-employment regime cannot be pooled with other regimes as it is very peculiar. For instance, self-employed cannot access the aforementioned early retirement possibilities. Therefore, non-employment spells are not assimilated to working time in the computation of pension benefits. Nevertheless, the size of self-employment sector is too small to be analysed alone.

[16]The information on registered self-employed is limited and we do not observe self-employment histories.

It is important to note that for individuals with a career length equal to or above the career eligibility threshold it is possible to claim a full pension before the FRA. However, many older women do not satisfy the career length condition. Moreover, individuals on replacement income are transferred automatically to the full pension scheme at the FRA.

Table 1: Changes in the FRA for women working in the private sector or as self-employed

	FRA	Career requirement	Affected birth cohorts
Until June 30, 1997	60	40	Born before June 1, 1937
From July 1, 1997	61	41	Born June 1, 1937 or later
From January 1, 2000	62	42	Born December 1, 1938 or later
From January 1, 2003	63	43	Born December 1, 1940 or later
From January 1, 2006	64	44	Born December 1, 1942 or later
From January 1, 2009	65	45	Born December 1, 1944 or later

Pensions can only be claimed one month after reaching the FRA. This table does not contain the changes to this scheme introduced since 2012, such as the decision to increase the FRA further to 66 in 2025 and 67 in 2030.

At each moment in the left column of Table 1, the increase in the FRA determines an exogenous change in the eligibility to old-age pension benefits which affects specific birth cohorts. As individuals with a career length below the career eligibility threshold can only claim a full pension one month after reaching the FRA, the first women to be affected by a change of the rules in January 2006 are those born in December 1942. While older women can still retire at the FRA of 63, the affected younger women with a career length below 44 years have to wait until age 64. This translates to retiring one calendar year later, as from January 2007. Women born before June 1937 are the only cohorts unaffected by any of these reforms, as they reach the FRA of 60 in June 1997, before the first increase in the pension age.

3.2 The old-age pension

The old-age pension can be claimed in the period between age 60 and the FRA. In particular, workers accrue the eligibility to anticipated old-age pension at age 60 or with 35 years of paid contributions. The full pension can be claimed either the month after reaching the FRA or after reaching a career duration requirement.

The pension benefits of the private sector regime are calculated according to Eq.(1):

$$benefit = \frac{n}{N} \times average\_wage \times k \tag{1}$$

where n is the number of years with pension contributions, N is the full career length, and k is the replacement rate that is equal to 60% for single women and 75% for women with a dependent spouse. The average wage is computed over the years with pension contributions (at most N). The yearly wages are subject to certain minima and maxima and adjusted for inflation.[17]

[17]Spells on replacement income are also included. Yearly wages are also subject to welfare adjustments. In addition,

In case of early claims, the pension benefit is proportionally reduced according to Eq. (1), due to the shorter career relative to the career length requirement (i.e. $n < N$). However, since 1992 there is no additional actuarial adjustment to the pension benefit in case of early claims. This means that the choice of claiming the anticipated pension is not penalised with a further reduction in the pension benefit, as in many other countries.[18]

In addition, spells spent on replacement income are fully considered as working spells. To these spells, a fictive wage is imputed which corresponds to the last wage earned by the worker before entering these schemes (see Table 10 for more information). Hence, the financial incentive to move back from replacement income to employment is very low. It amounts to the higher benefit resulting from the wage increase due to seniority if one did not leave employment compared to the imputed wage in case of spells in replacement income.

Combining work and pension claims is subjects to limits. All these features point out that the Belgian pension system provides low financial incentives to continue working or re-enter employment once it is left.

Recall that the reform increases the full career length from 43 to 44 according to Eq. (1). This implies that the same career before and after the reform (i.e. the same number of years with pension contributions) results in a lower monthly pension benefit after the reform, as the lifetime earnings are divided by a higher number.

3.3 Early retirement schemes and other policies aimed at older workers

This section briefly describes the main early retirement schemes that allow workers to exit from the labor force well before the FRA while receiving a replacement income.[19] These mechanisms are the unemployment insurance, the conventional pre-retirement, the career break scheme, and the sickness and disability insurance.

The conventional pre-retirement is granted to older workers who lose their job and are aged at least 58 with 25 years of contributions.[20] It consists of the unemployment benefit plus an additional allowance paid by the employer. The former corresponds to 60% of the last gross capped salary, while the latter is half of the difference between the last capped salary and the unemployment benefit. The recipients are not expected to take up a new position. Moreover, specific eligibility conditions are defined at the sectoral level by collective agreements. Similar to the conventional pre-retirement is the Canada Dry, alternatively called pseudo pre-retirement, according to which the worker can pre-retire by receiving the unemployment benefit plus a supplement from the employer. The difference with the conventional pre-retirement is that the conditions (the amount and the

benefits cannot go below a certain minimum, to allow women with a sufficiently long contribution period (15 years) but very low earnings to have a sufficiently high pension.

[18] An exception is the self-employment pension regime where the actuarial adjustment is still in place in 2006 and the benefit is reduced by 5% per year of anticipation.

[19] For more information see Table 10 in the Appendix.

[20] The age requirement is subject to exceptions for heavy occupations, jobs requiring night operations, and firms in financial difficulties or in restructuring.

duration of the supplement, and the notice period) are defined by a private agrement between the employer and the worker, rather than by law. The worker needs to be at least 45 years old.

Next, the unemployed with at least 50 years old and 20 years of career are subject to favourable conditions compared to younger unemployed. This makes unemployment insurance an alternative early retirement scheme. Old-age unemployed are exempted from job search obligations. In addition, their unemployment benefit is complemented by a top-up that is proportional to their age and the family situation.

An additional channel for early retirement is the disability insurance system (DI) to enter DI. One has to prove an earnings loss in their usual job of at least 66% for at least 12 months, during which one is covered by sickness benefits. DI benefits are subject to periodic medical controls and can be open-ended. Workers need to satisfy 5 years of paid contributions, three of which paid in the last 5 years. The benefits amounts from 40% to 65% of the last wage preceding the injury depending on the family situation.

Lastly, the career break scheme has been conceived to allow older workers to gradually reduce their working duties until they reach the FRA. Workers with at least 50 years old and at least 5 years of seniority in the current firm can reduce or interrupt their working time while receiving a replacement allowance. The aforementioned early retirement options provide generous allowances to participants and, except for the anticipated pension, keep accumulating (assimilated) career years for the pension calculation. This results in high participation rates in these schemes and a low average effective retirement age.[21]

Next to the policies that facilitate early retirement, the government has put in place several mechanisms that aim to keep workers employed until the FRA. Table 9 in the Appendix gives a summary of the most important programmes around 2006. In general the incentives provided by these mechanisms are not very strong. Anticipating our estimation strategy, where we compare women *just* affected with women *just not* affected by the 2006 reform, we should ensure that the increase in the FRA is the only factor that plays a role. It is important to rule out the possibility that other factors do not simultaneously affect the treated group *differently* than the non-treated group. In this case, any difference in the outcomes between treated and not treated women can be credibly attributed to the increase in the FRA. As Tables 9 and 10 in the Appendix show, there are not many other reforms around 2006.

The most important reform is the pension bonus starting in January 2007.[22] This reform targets the same women as those affected by the reform of interest, since it concerns pensions started as from January 2007 and refers to periods worked since January 2006. However, the incentives given by this policy are rather weak, as working one day extra after age 62 results in €2 extra pension benefits *per year*. Working a full additional year (around 200 working days) then results in a €400 pension increase per year or €33 per month. In addition, a structural wage subsidy is introduced

[21]In more recent years, beyond the scope of our analysis, the government has tried to make early retirement less appealing and working longer more so.

[22]It is clearly announced at the end of 2005 and thus workers do not have much time to anticipate it. Moreover, Smith et al. (2014) shows that the pension bonus has no impact before 2008.

that lowers employer social security contributions for older workers, thereby making these older workers more employable. At the same time an outplacement mechanism is designed to give laid-off older workers better employability prospects. Other reforms try to discourage early exits through levying social security contributions on employer-paid allowances in the (pseudo) early retirement and time credit schemes. All these reforms aim at keeping older workers employed longer, either by promoting employment or by making early labour market exits less profitable. However, most of these policies provide very small incentives, and thus are unlikely to drive the results of our analysis.

4 Data and Sample Selection

We use the same administrative data as in Albanese et al. (2015) and follow the description found there. This database is obtained by merging registers of the diverse social security institutions and of the National Register containing all Belgian inhabitants. The data becomes more comprehensive over time. For the period 1957-1998 it provides yearly information on earnings, the number of working days and hours (in case of part-time work) and the worker type (blue or white collar) for employees in the private sector. From 1998 onwards this information is available on a quarterly basis, not only for employees in the private sector, but also in the public sector. In addition, since 1998 it also contains information on the firm (size and sector), the industrial committee to which the worker belongs to[23], the timing of self-employment spells, unemployment benefit (UB) receipt, and participation in the career break and early retirement schemes. Since 2003 the data provides information on the receipt of statutory (possibly early) retirement benefits, sick leave and replacement income in case of disability, occupational diseases or accidents. Finally, since 1998 the National Register provides yearly information on December 31 on individual and household characteristics, such as age, gender, nationality, district of residence, and household composition. The data is available until the last quarter of 2011.

The rest of the section is organised as follows. First, we explain the sampling mechanism and discuss the weighting scheme that used to account for endogenous sampling. Second, we describe the sample used in the estimation.

4.1 Endogenous Sampling Mechanism

As for the paragraph above, we refer again to Albanese et al. (2015). The sample on which we base our analysis is drawn to evaluate the effect of a wage cost subsidy for employees in the private sector aged 58 years or more. The Belgian government introduces this subsidy in 2002 to enhance the employment of older workers (Albanese and Cockx, 2015). A representative sample is drawn of 243,655 individuals born between the 1st of April 1941 and the 31st of March 1950, i.e. aged between 52 and 61 in 2002. Because in Belgium many individuals are already inactive in that age bracket, the sample is not only stratified according to gender, but also into 9 birth cohorts c

[23] Industrial committees are organized for each type of worker at the sectoral level. In these committees trade unions and employer organizations negotiate the collective agreements. These agreements are binding for all workers belonging to this industrial committee, irrespectively of whether they are unionised and, hence, represented in the negotiation.

(=1,2,...,9) and 5 strata r (=1,2,...,5). These strata are defined according to employment status in the private sector and the earned wage in the period around the 2002 reform. This stratification aims at over-representing groups that are relatively rare in that age bracket and more responsive to the labour market policy reform: low-wage employees in the private sector and individuals transiting in and out of employment during this period. In Appendix B more details on this stratification can be found. Because the stratification is endogenous, consistent estimation requires to appropriately weight the data in these strata (e.g. Manski and Lerman, 1977; Cameron and Trivedi, 2005). If we denote the sampling weight for individual observation i belonging to birth cohort c and to substratum r by $W_{cr,i}$, then

$$W_{cr,i} = \frac{N_{cr}}{N} * \frac{n}{n_{cr}}, \tag{2}$$

where N_{cr} denotes the size of the population in substratum cr[24], n_{cr} the corresponding sample size, $N \equiv \sum_{c=1}^{9} \sum_{r=1}^{5} N_{cr}$ the total population size and n the corresponding sample size. As to avoid cumbersome notation, gender is not explicitly referred to. The weighting formula comes from a double re-weighing, within and between cohorts.[25] In the remainder of this paper we refer to $W_{cr,i}$ as W_i.

4.2 Sample

In this study we evaluate the impact of an increase in the FRA for women from 63 to 64 in January 1, 2006. The pension reform creates a sharp discontinuity in the eligibility conditions to full retirement. Women born in November 1942 or before are not affected by the reform since they reach the old FRA before January 2006. By contrast, women born in December 1942 or later are affected by the change in the eligibility criteria. We exploit this in a RDD approach and identify the impact of the change in the FRA by comparing women born just before December 1942 (control group) and just after December 1942 (treated group), as in Lalive and Staubli (2015); Montizaan et al. (2010); de Grip et al. (2012).

The analysis is based on a sample representing the full population of women born between April 1941 and July 1944. This results in a sample of 24,571 women representing 165,428 women in the population, as shown in Table 2. We include a set of covariates to improve the precision of the estimation. As described above, our sample consists of women born within a three year period (April 1941-July1944). Consequently, we expect these women to be relatively homogeneous in their characteristics. Moreover, while characteristics could gradually change over the birth cohorts, we do not expect a discontinuity at the threshold birth month of December 1942. We consider two subsets of covariates, namely personal characteristics and labour market characteristics. A summary of the descriptive statistics can be found in Table 3 and in Figures 9 and 10 in Appendix C. As expected, there is little difference in the covariate values at the cutoff.

[24] We have information on the population sizes in each substratum, i.e. on N_{cr}.

[25] First, to restore the representativeness within the cohorts we reweigh the units within each cohort by $W_{cr}^{c} = \frac{N_{cr}}{N_c} * \frac{n_c}{n_{cr}}$ (where N_c and n_c are the size of the cohort in the population and in the sample). To make the cohorts in the sample representative for the population, we weight each cohort a second time: $SW_{cr} = W_{cr}^{c} * \frac{N_c}{N} * \frac{n}{n_c}$, so that $W_{cr} = \frac{N_{cr}}{N} * \frac{n}{n_{cr}}$.

Table 2: Sample size by birth month

Control group			Treated group		
birth month	unweighted	weighted[§]	birth month	unweighted	weighted[§]
Apr 1941	1792	13955.7	Dec 1942	2320	16403.2
May 1941	2040	18293.3	Jan 1943	2664	18521.4
Jun 1941	1812	14395.2	Feb 1943	2304	16941.6
Jul 1941	1976	15607.8	Mar 1943	2820	20673.4
Aug 1941	1768	14560.9	Apr 1943	2940	20131.6
Sep 1941	1696	14384.0	May 1943	3044	17951.4
Oct 1941	1676	13210.6	Jun 1943	2768	15751.9
Nov 1941	1696	12811.1	Jul 1943	2688	17114.6
Dec 1941	1776	13520.3	Aug 1943	2904	17931.7
Jan 1942	2416	18879.0	Sep 1943	2952	16176.4
Feb 1942	1964	14022.9	Oct 1943	3116	19466.1
Mar 1942	1952	16003.2	Nov 1943	3000	18147.9
Apr 1942	1900	13321.8	Dec 1943	2952	14012.4
May 1942	2092	14622.7	Jan 1944	3384	20215.0
Jun 1942	1988	16847.2	Feb 1944	3224	18131.5
Jul 1942	1984	14514.6	Mar 1944	3552	21825.6
Aug 1942	2000	13976.2	Apr 1944	3192	17959.5
Sep 1942	2144	16005.3	May 1944	3552	19906.1
Oct 1942	2056	14364.9	Jun 1944	2912	14506.3
Nov 1942	2108	16523.7	Jul 1944	3160	20124.3
Total	38,836	299,820	Total	59,448	361,892

[§] Weighted using the endogenous sampling weights W_i.

First, the list of personal characteristics includes the region of residence, nationality, marital status, and the presence of children in the household. In our database, these variables are available on a yearly basis from 1998q4 onwards. We use the values at the earliest moment, or the first moment thereafter at which information is available for the individual.[26] An overview of the summary statistics for the personal characteristics by treatment group can be found in the top half of Table 3 and in Figure 9 in Appendix C.

Second, the list of labour market characteristics includes indicators for blue collar work, part-time work, the full-time equivalent time worked, the number of jobs, the number of years experience and the average monthly earnings. All variables refer to periods before the policy is announced to avoid endogeneity issues. Most variables are defined for the period 1990-1996, with the exception of the number of years experience, which refers to all years between age 14 and 1996. The data is obtained from yearly indicators and only refers to the private sector. Individuals who do not work

[26]The variables are defined after the announcement of the reform. However, this is not an issue as (i) they are measured still eight years before the reform, and (ii) they are relatively stable over the lifetime and not directly linked to the reform. We impute 3.11-3.26% of the observations who have missing information. This corresponds to around 10% of the population due to oversampling of inactive individuals in our database.

in the private sector in that period (other type of employment, unemployment or inactivity) have value equal to *zero*. An overview of the summary statistics for the labour market characteristics by treatment group can be found in the bottom half of Table 3 below and in Figure 10 in Appendix C.

Table 3: Descriptive statistics by treatment group

	Control group				Treated group			
	Mean	St. Dev	Min	Max	Mean	St. Dev	Min	Max
Personal Characteristics								
Reside in Brussels	0.0855	0.2797	0	1	0.0869	0.2817	0	1
Reside in Flanders	0.6091	0.4880	0	1	0.6092	0.4879	0	1
Reside in Wallonia	0.3053	0.4606	0	1	0.3039	0.4599	0	1
Belgian nationality	0.9353	0.2459	0	1	0.9352	0.2463	0	1
Married	0.7627	0.4255	0	1	0.7459	0.4353	0	1
Have children	0.3443	0.4751	0	1	0.3899	0.4877	0	1
Labour Market Characteristics§								
Worked as blue collar	0.1368	0.3436	0	1	0.1548	0.3617	0	1
Worked part-time	0.1785	0.3829	0	1	0.1967	0.3975	0	1
Full-time equivalent time worked (%)	13.889	28.589	0	100	16.179	30.652	0	100
# of firms where worked	0.4567	0.9243	0	23	0.5567	1.0449	0	26
# Years experience	11.602	11.570	0	39	12.686	11.622	0	39
Average monthly earnings (€)	363.84	811.56	0	12,956	417.56	858.12	0	13,396
Sample size (unweighted)	9,709				14,862			
Represented pop. size	74,955				90,473			

Data are weighted by W_i.

§ Each variable explained below refers to periods worked in the private sector between 1990-1996, except for #years experience which refers to all years worked in the private sector between labour market entry and 1996. The blue collar indicator reports whether the woman has worked at least one working day in a blue collar job. The part-time indicator reports whether the woman has worked at least one working day on a part-time basis. The full-time equivalent working time indicator reports the percentage of time the woman has worked on a full-time equivalent basis. The number of jobs indicator denotes the number of firms the woman has been affiliated. The indicator for the years of experience sums the years in which the woman had a positive number of working days and salary from age 14 until 1996. The average monthly earnings indicator reports the average monthly salary. Only periods with positive working days and remuneration are considered.

5 Empirical Strategy

We study whether women's retirement decisions are affected by changes in the eligibility conditions for full pension. The exogenous increase in the female FRA is the source of identification. The FRA increases from 63 to 64 for women born in December 1, 1942 or later, while it remains at 63 for women born before this date. We use a RDD and estimate the causal effects of interest by comparing women born before December 1, 1942 (control group) with women born afterwards (treated group). Such a comparison results in unbiased estimates of the treatment effect if the women on both sides of the cutoff show similar observable and unobservable characteristics (Lalive and Staubli, 2015). In addition, because the treatment is inevitable over time, all women born after the cutoff birth date will eventually age into the programme.[27] We are therefore in a sharp design.

The validity of RDD crucially depends on whether individuals are able to manipulate the treatment assignment D_i (Lee and Lemieux, 2010), which would invalidate the assumption of local randomisation of the treatment around the cutoff. In our framework manipulation would mean that the parents of the individuals in the sample decided the month of birth in anticipation of the reform, which cannot happen. Therefore, manipulation can be ruled out. However, seasonal effects, events during World War II or other policies could have an impact on the birth dates. We argue that even if this is the case, changes in the density of the running variable would be exogenous to the reform of interest. In addition, in Appendix C we show that the distribution of the covariates is smooth at the threshold, which indicates that individuals around the cutoff are similar to each other. In addition, in Appendix E we run a battery of falsification tests as suggested by (Lee and Lemieux, 2010). Such evidence validates the RDD approach.

However, the identification strategy could be violated if the reform of interest interfered with other policies that set the same age thresholds and that affect the outcome variables. As shown in Section 3, at the exact moment of the policy change there are no other policy changes. There are a few minor policy changes around this moment though, but we do not expect them to influence the results much as it introduced rather weak incentives.

We apply the RDD by estimating the following regression:

$$\sqrt{W_i}\ y_i = \sqrt{W_i}\ *\ (\alpha + \beta\ D_i + \gamma\ f(Z_i - c) + \delta\ D_i * f(Z_i - c)\ + X_i^{'}\ \zeta + \epsilon_i), \tag{3}$$

where W_i represents the endogenous sampling weights (see Section 4.1) and

- i denotes the individual.
- c is the cutoff that is set at December 1942.
- y_i denotes the realisation of the outcome of interest measured at the end of the quarter.
- D_i is the treatment indicator equal to one if a woman is born after December 1, 1942 and zero otherwise. β is the effect of interest.
- $Z_i - c$ is the birth month, i.e. the running variable, normalised at the cutoff. At most it can range between [-20, 19], with -20 corresponding to April 1941 and 19 corresponding to the birth month July 1944. $f(\cdot)$ is a function of the distance between the birth month and December 1942, that is assumed to be linear, quadratic or cubic.
- $D_i * f(Z_i - c)$ represents the interaction(s) between the treatment indicator and the term(s) describing the function in the running variable. In particular, the interaction between D_i and $Z_i - c$ allows the polynomial $f(Z_i - c)$ to have different slopes at both sides of the cutoff. Note that for each parametric specification $f(\cdot)$ we also estimate a simpler version of Eq. (3) dropping the interaction(s) $D_i * f(Z_i - c)$, so that the polynomial in $Z_i - c$ is restricted to have the same slope at both sides of the cutoff.

[27]All women affiliated to the private sector or self-employment regime.

- X_i is the set of covariates listed in Table 3, centered at their sample mean.
- ϵ_i the individual error terms. We cluster the standard errors by birth month in order to account for the correlation in the outcomes of individuals born in the same month, induced by the fact that the running variable is discrete (Lee and Card, 2008).

In the case of RDD with discrete running variable, the treatment effect is identified by comparing the (observed) average outcome at the cutoff with the counterfactual average outcome that is extrapolated based on the observations below the cutoff assuming a specific functional form for the relationship between the outcome and the running variable (Lee and Card, 2008). Such extrapolation introduces a random mis-specification error that is common to all units with the same value of the running variable. Hence, correct inference requires to cluster the standard errors at the level of the running variable (Lee and Card, 2008). However, in our case this may not be sufficient since we rely on at most 40 clusters, depending on the window width considered. In case of few clusters, cluster-robust standard errors may be under-estimated. Hence, we correct the inference with wild bootstrap methods suggested by Cameron and Miller (2015); Cameron et al. (2008). This procedure allows us to account for the correlation in the error terms of individuals born in the same month with few clusters. In the tables of results we provide the P-values resulting from wild bootstrap (see the next section and Appendix D).[28]

First, we study the impact of the reform on the following outcomes measured at the end of the quarters of 2006:

- *Employment rate*: the share of employed women compared to the total number of women in our sample. It includes salaried private sector employment, salaried public sector employment (both statutory and contractual workers), and self-employment.[29]
- *Pension rate*: the share of women receiving the pension compared to the total number of women in our sample. Note that the pension can be claimed before the FRA, either the full amount if the career requirement is met, or a proportionally lower amount through the anticipated pension if it is not (see Section 3).

To improve the precision of the estimates, we pool all quarters of 2006 and include quarter-specific effects. This amounts to assume that the policy has the same impact over the four quarters, and to measure the mean impact of the policy in 2006. This assumption is reasonable since the results of the estimations at the end of each quarter are very similar. In addition, we test whether the coefficient of the policy variable can be assumed to be the same for all quarters in 2006. To do so, we add the interactions between D_i and the quarter-specific effects to each specification and

[28] Wild bootstrap P-values are obtained with the post-estimation command *boottest* by Roodman (2015), using Webb weights, assuming the null hypothesis and setting replications to 10,000.

[29] Results are very similar if we consider the private sector employment rate (not reported). However, the private sector employment rate may be selective if the reform induces changes in the composition across private, public sector and self-employment. Nevertheless, selectivity should be unlikely since mobility between jobs in the public sector, the private sector, and in self-employment is limited (Jousten et al., 2007).

test for every outcome whether these interactions are jointly significant. We cannot reject the null hypothesis for both outcomes (results of this test are not reported, and available upon request).

Next, we evaluate the impact of the reform on the "pathway into retirement". The latter is defined as the last state before entering in full pension at or after the FRA.[30] The reason for this is that we would like to distinguish between direct transitions from employment to full pension from the (indirect) transitions from employment to full pension via early exit-routes. The former transitions corresponds to "virtuous" pathways, i.e. surviving in employment until retirement. Therefore we define the following outcomes:

- *Rate of withdrawals from the labour market*: it denotes direct transitions from employment to full pension.

- *Rate of early withdrawals from the labour market*: it considers transitions from inactivity or replacement income into full pensions. These individuals are automatically rolled over into the retirement scheme at the FRA.

These outcomes are measured at the end of the observation period (2011q4), so as to observe the entry into retirement for most of individuals in the sample. Table 4 provides summary statistics of all aforementioned outcomes.

Table 4: Summary statistics of the outcome variables

	Observations		Statistics			
Outcomes	unweighted	weighted	Mean	St. Dev.	Min	Max
Participation rates in 2006§						
Employment rate	98284	661,712.2	0.0863	0.2809	0	1
Pension rate	98284	661,712.2	0.4688	0.4990	0	1
Pathway into retirement†						
Rate of withdrawals from labour market	24,571	165,428	0.0518	0.2216	0	1
Rate of early withdrawals from labour market	24,571	165,428	0.9216	0.2689	0	1

The sample contains 24,571 individuals representing 165,428 women in the population. Reported statistics are weighted using the endogenous sampling weights W_i.
§ Statistics for the participation rates in 2006 are computed by pooling all quarters of 2006.
† Statistics for the pathway into retirement are measured in 2011q4.

For each outcome, we estimate Eq.(3) assuming a linear, quadratic and cubic specification, and with or without the interaction $D_i * f(Z_i - c)$, and by varying the window width.

[30] It is an absorbing state: first we identify the quarter of entry in full pension at or after the FRA; then the pathway is defined as the state occupied in the quarter before entry.

6 Results

This section reports the results of the impact of increasing the FRA from 63 to 64 on the outcomes of interest. First, we examine the impact of the reform on the participation rates in 2006 and on the pathway into retirement. Second, in Section 6.1 we relate our findings to the literature and discuss anticipation effects. Figures 1 and 2 show the graphical analysis for the participation rates

Graphical analysis on participation rates in 2006

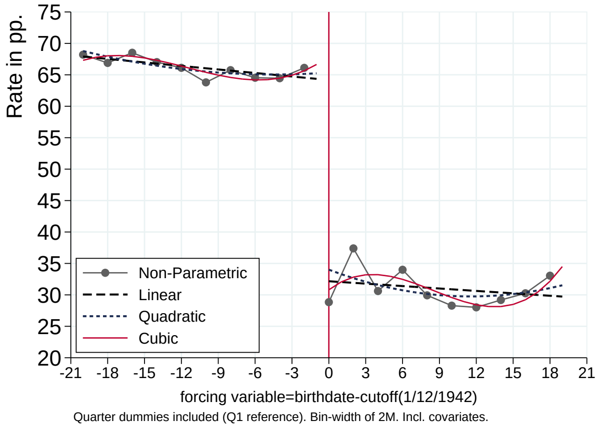

Figure 1: Pension rate

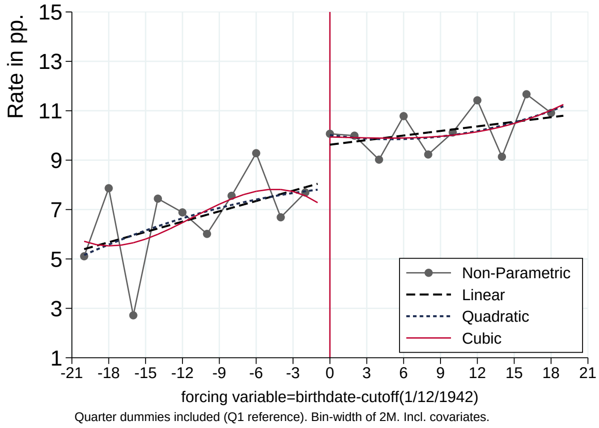

Figure 2: Employment rate

measured in 2006, considering the largest window width. The horizontal axis plots the month of birth normalised at the cutoff of December 1942. All quarters of 2006 are pooled. Each graph shows the two-months bin averages as well as linear, quadratic and cubic splines. In all estimations covariates are accounted for and data are weighted by W_i.

Figure 1 shows the pension take-up rate in 2006: this rate amounts to around 65% for the control group, and 32% for the treated one. The discontinuity at the cutoff is around 33 *pp*. The rate is higher for the control units because they become eligible to full pension since 2005 according to the old eligibility conditions.[31] By contrast, treated individuals are not yet eligible to full pension due to the reform: still, 32% of them claim the pension earlier than the FRA, either through the anticipated pension, or because they satisfy the years of career requirement. The latter possibility is however unlikely since women have discontinuous careers. Overall, the Figure suggests that the reform is effective in delaying the entry into retirement.

Figure 2 shows the employment rate in 2006. The rate of the treated group is higher by almost around 2.5 *pp* due to the reform. However, this provides a partial picture of the effect of interest. In particular, this does not allow us to understand if the increase in the employment rate is driven by increased transitions from non-employment to employment or by longer spells in employment. The latter channel is however unlikely because the Belgian pension system provides very low financial

[31] All treated are older than the FRA. However, their pension take-up rate is not 100%, since some of them cannot claim a full pension, if they were never employed.

incentives to re-enter employment.

Next, we present the results on the pathway into retirement. As anticipated in the previous section, this analysis allows us to disentangle whether the increase in the employment rate is due to increased duration in employment or to increased transitions from non-employment to employment. Figure 3 shows the proportion of direct transitions from employment to full retirement that occur

Graphical analysis on the pathway into retirement

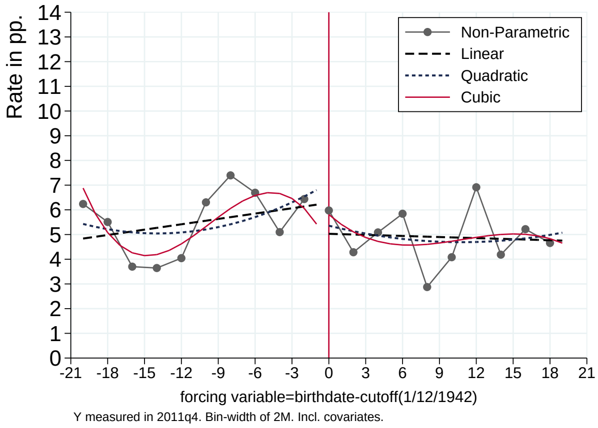

Figure 3: Rate of withdrawals from LM

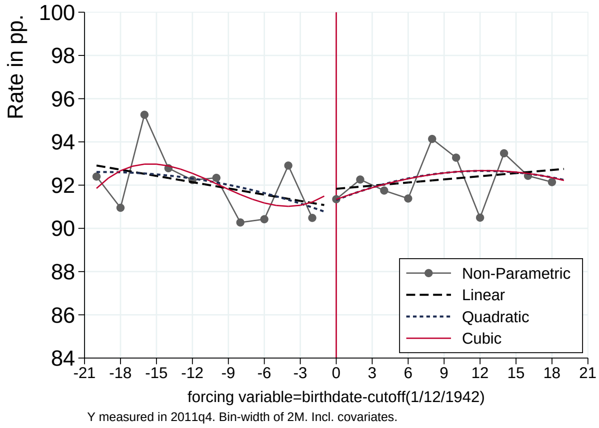

Figure 4: Rate of early withdrawals from LM

since the FRA until 2011q4. The absence of discontinuity at the cut-off suggests that overall the reform does not affect the proportion of women who survive in employment until retirement. Figure 4 represents the proportion of transitions from inactivity or replacement income to full pensions. This rate represents the proportion of women who exit out of the labour market prematurely (mostly) via the available early exit-routes. Once they reach the FRA, these women are automatically rolled over into the old-age pension scheme. Also this proportion is not affected by the reform.

Hence, the reform does not change the pathway into retirement. Namely, neither the proportion of women surviving in employment until the FRA, nor the proportion of women who withdraw prematurely from the labour market are affected. Taken together, both results allow to discard the possibility that the increase in the employment rate shown in Figure 2 is due to transitions from non-employment to employment (since otherwise we would have observed an increase in direct transitions from employment to retirement, measured by the rate of withdrawals from the labour market). By contrast, women who survive in employment at an age close to retirement prolong their spell in employment, so as to comply with the new eligibility rule. Hence, the reform affects the career duration of women surviving in employment until the FRA. This increases the employment rate in 2006.

In the main analysis, we estimate the effect of the reform on the aforementioned outcomes for different window widths (ranging from 10 to 40 months of birth) and by assuming different specifications, as discussed in Section 5 (complete results are reported in Appendix D). For simplicity, Table 5 below shows the effect of increasing the FRA from 63 to 64 for selected window widths

(10, 20, 30, and 40 months) and assuming a linear specification. We choose to focus on the linear specification for two reasons: first, it is the simplest specification. Second, it passes the specification test suggested by Lee and Lemieux (2010) in the RDD context. This means that according to this test, the linear function is sufficient to depict the relationship between the running variable and all outcomes of interest. A description of the test and the results are reported in Appendix F.[32]

Table 5: Overview of the main results by selected window width assuming linear specification

	(1)	(2)	(3)	(4)
Panel A§: Participation rates in 2006				
Pension rate				
T	-0.3493***	-0.3268***	-0.2983***	-0.3208***
se	(0.0369)	(0.0322)	(0.0285)	(0.0238)
P-val	0.008	0.001	0.000	0.000
Employment rate				
T	0.0381**	0.0249**	0.0090	0.0155
se	(0.0139)	(0.0097)	(0.0116)	(0.0099)
P-val	0.0315	0.0269	0.4618	0.1397
Window width	10	20	30	40
Unweighted Obs.	23,340	47,592	72,528	98,284
Weighted Obs.	168,055.9	327,799.6	490,577.5	661,712.2
Panel B†: Pathway into retirement				
Rate of early withdrawals from the labour market				
T	0.0001	-0.0046	0.0153	0.0066
se	(0.0145)	(0.0103)	(0.0108)	(0.0091)
P-val	0.9931	0.6626	0.1785	0.4854
Rate of withdrawals from the labour market				
T	-0.0018	0.0017	-0.0199*	-0.0114
se	(0.0121)	(0.0094)	(0.0097)	(0.0079)
P-val	0.8869	0.8567	0.0522	0.1700
Window width	10	20	30	40
Unweighted Obs.	5,835	11,898	18,132	24,571
Weighted Obs.	42,014.0	81,949.9	122,644.4	165,428.0

T is the treatment effect resulting from estimating Eq. (3) with linear specification without spline for different window widths. Standard errors (se) clustered by month of birth shown in parenthesis. $P-val$ indicates the wild bootstrap P-value (10,000 replications and Webb weights). *** $P-val$ <0.01, ** $P-val$ <0.05, * $P-val$ <0.1. Estimations include the following covariates centered at the sample mean: region of residence, Belgian nationality, married, having children, work part-time in private sector between 1990-1996, work as blue-collar in private sector between 1990-1996, full-time equivalent time worked in private sector between 1990-1996, number of firms where one has worked between 1990-1996, average monthly salary in private sector between 1990-1996, years of experience in private sector from labour market entry until 1996 (see Table 3).

§ Eq. (3) is estimated pooling all quarters of 2006 controlling for quarterly-specific effects centered at the mean computed over 2006.

† Eq. (3) is estimated on 2011q4.

Panel A of Table 5 shows the effects of the reform on the participation rates in 2006. Column

[32] To deal with the small number of clusters, the specification tests are computed based on the wild bootstrap methods.

(1) of the first row shows that the increasing the FRA from 63 to 64 decreases the pension take up rate by 35 *pp* based on a window width of 10 months. By increasing the window width, the effect decreases slightly to 32 and 30 *pp*. This strongly supports the findings that women delay pension claims due to the increase in the FRA. The second row reports the impact on the employment rate in 2006. For window widths of 10 and 20 months, the effect is significant and varies between 3.8 and 2.5 *pp*. For window widths of 30 and 40 months the effect is likely to be under-estimated because the slope of the linear fit is biased downwards due to the outliers at the extreme of the window (see Fig.2). Panel B of Table 5 provides an overview of the effects the reform on the pathway into retirement. The effects are all close to zero and not significant. This suggests that the reform does not affect neither the proportion of "virtuous" women who survive in employment until the FRA, nor the proportion of early withdrawers.

Appendix D reports the complete results of the treatment effect considering all window widths (ranging from 40 to 10 months of birth) and different specifications. Overall, lower order polynomials perform better in small window sizes, because they are less influenced by outliers than more flexible functions. Therefore, our preferred estimations are the linear specifications (as suggested by the specification tests in Appendix F) for smaller window widths. However,the tables in Appendix D suggest that results are robust across specifications, although the treatment effect estimated with quadratic or cubic functions becomes less precise for the employment rate. As discussed in Section 5, we ensure correct inference by clustering standard errors by month of birth and by applying wild bootstrap methods as suggested by Cameron and Miller (2015); Cameron et al. (2008). This procedure allows to account for the correlation in the error terms of individuals born in the same month when clusters are too few.[33] The problem of over-rejection of the null hypothesis gets exacerbated the smaller the window width. For the employment rate and the pension rate, we show this graphically in Figure 12 and 11 of Appendix D, respectively. In these graphs, the horizontal axis represents the window width (from 10 to 40 months). Each plot then shows the point estimates of the treatment effect for different window widths, together with the corresponding 95% level confidence intervals resulting from the cluster-robust standard errors and wild bootstrap methods. These plots are provided separately for the linear and quadratic specifications, with and without spline. Figure 12 and 11 allow to draw the following conclusions: first, the point estimates of the treatment effect are quite stable across window widths and specifications. Second, linear parametric specifications provide more precise estimates than higher polynomial orders. Third, the precision of the estimates worsens as the window width shrinks (this is true for both types of confidence intervals). Fourth, cluster-robust standard errors perform well with 40 clusters but lead to over-rejection as the window width gets smaller.

There are a number of reasons why these women may decide to comply with the reform. A first possibility is that they remain employed longer because they are liquidity constrained. Alternatively, they could comply with the reform because had discontinuous careers in the past and hence are

[33] All tables of the main results show the P-values resulting from wild bootstrap procedure, in addition to the treatment effect and the corresponding cluster-robust standard errors (which are likely to be under-estimated in case of few clusters).

willing to improve the benefit level of the pension by adding an extra year of contribution exploiting the financial incentives embedded in the formula for the benefit computation. If the main reason for compliance was liquidity constraints, we would expect that treated women at the left of the cutoff earn lower salaries than women at the left of the cutoff. We plan to further investigate this by studying the impact of the reform on work intensity and earnings.[34]

6.1 Discussion

In this section we discuss our main results and compare them with the results found in the literature.

First, we find a significant negative effect on the pension take-up rate of around 32 *pp*. This indicates that the increase in the FRA is very effective in delaying the pension claiming. This result is not surprising, as many Belgian women do not meet the career requirement.[35] As a consequence, to avoid a lower pension they should enter retirement at the FRA. We find that treated women comply with the new FRA, rather than exiting from the labour market through early retirement schemes. In addition, this result is in line with the literature that finds positive effects on the effective age of pension claiming.[36] For instance, Lalive and Staubli (2015) and Manoli and Weber (2016) consider a sample of labour market attached women, and show positive effects on the age of exit from employment and the age of claiming a full pension. Manoli and Weber (2016) explain that this result is obtained because treated individuals remain longer in their pre-retirement jobs. For Belgium, we find that this is the case for the sub-sample of women who survive in employment until close to the FRA.

Second, we find a positive effect on the employment rate of 2.5 to 3.8 *pp*. Recall that this effect is estimated on the overall population of interest. The small effect on employment could be due to the fact that the reform does not change the abundant early retirement options for Belgian workers. While the effect appears to be small in absolute size, it is big in relative terms. Non-treated women right before the cut-off show an employment rate of just 8% (see Fig. 2), which results in a relative increase in the employment rate of 31 to 47%. The latter is in line with the increase of 30 *pp* in the employment rate at the old FRA estimated by Lalive and Staubli (2015) for the sub-sample of Swiss labour market attached women.[37] As opposed to the Swiss pension system, in Belgium there are no actuarial reductions in the pension benefits in case of early claims. Therefore, our positive and sizeable effect on the employment rate suggests that women are very much reactive to changes

[34]We will examine the impact of the reform on two measures of the quality of private sector employment: average monthly earnings and full-time equivalent time worked.

[35]Table 3 shows that the average career length is only 12.7 years in 1996 for treated women, such that even with 10 additional years of experience most women would still have a career length well below the required 44 years to claim an anticipated full pension.

[36]We cannot study directly the age of retirement due to data limitation. On the one hand, not all workers in our sample are employed during the period of analysis. On the other hand not all workers exit employment during this period. We can draw conclusions on the effects of the reform on retirement decisions based on the aforementioned indicators for the pathway into retirement.

[37]For Austria, Staubli and Zweimüller (2013) find that one-year increase in the early retirement age leads to 11 *pp* increase in the female employment. However, this result is hardly comparable to ours since the effect refers to the increase in the eligibility conditions of the early retirement scheme.

in the eligibility conditions. This is in line with the policy simulations by Manoli et al. (2015) for Austria, a country with a generous pension system and where people show strong preferences for early retirement, as in Belgium. They conclude that changes in eligibility for pension are more effective in delaying retirement rather than policies that introduce or modify actuarial adjustments to benefits.

In addition, the ex-post evaluation literature finds spillover effects on other social security schemes. For Austria, Staubli and Zweimüller (2013) find large positive effects on the participation rate in unemployment insurance of 12 *pp*, although small positive effects for disability insurance. Lalive and Staubli (2015) also find a positive effect on unemployment (0.5 *pp*) and disability insurance (1-2 *pp*) inflow in their Swiss study. Finally, Duggan et al. (2007) find spillover effects on disability insurance for the US. We do not find increased transitions from employment to inactivity or replacement income, since individuals do not change their preferences with respect to the pathway into retirement. The absence of spillover effects is reasonable in the Belgian context, since the decision of early withdrawal from the labour market entails very small consequences in terms of the generosity of the future pension.

7 Anticipation effect

Last, we examine anticipation effects. Since the reform is announced in 1996 and implemented in 2006, anticipation effects may be at work.

There are at least two reasons that could trigger anticipation. First, in anticipation to the policy change, individuals may start reducing their workload before the age of retirement as to be able to cope with longer careers. This should be reflected in a reduced number of working hours. Second, individuals may stop working before the age of retirement due to the stress generated by the expectation of an extra year of work or due to preferences for leisure. If individuals would be induced to enter early retirement by anticipation of the policy reform, then one could also expect some impact on the pension take up (through anticipated pensions) in addition to the other early retirement routes.

Therefore we consider the following outcomes: the employment rate, the pension rate (which before the attainment of the FRA measures the anticipated pension), and a residual category, which sums up the take up in early retirement, unemployment and disability insurance, and inactivity. In addition, we consider also the number of hours worked in the private sector. The latter is set equal to zero if one is not employed in the private sector. Accordingly, the composition of the treated and control group remains fixed, so that the results cannot be driven by compositional changes. If this type of anticipation occurs its maximal impact should be measured just prior to the reform year in 2006. So, in order to measure anticipation effects we examine the impacts on the aforementioned outcomes in the period 2004-2005.

To carry out the RDD estimation on outcomes measured in 2005, we have to deal with the fact that in 2005 the control group attains the pre-reform mandatory retirement age of 63 which introduces a discontinuity that confounds the anticipation effect. For instance, the last quarter of 2005 cannot be used since the whole control group is eligible to full pension as opposed to the treated group: therefore, differences in the outcomes between the two groups should be attributed to differences in terms of eligibility rather than anticipation. Therefore, for the remaining quarters of 2005 we select the birth cohorts that are not yet eligible to full pension, so that any difference at the cutoff can be attributed to the anticipation of the policy. For instance, women born in December 1941 - February 1942 are eligible to full pension in 2005q1, as they have reached the old FRA. By contrast, women born in March 1942 - November 1942 have not yet reached the FRA, and can be considered to study anticipation at the end of that quarter. By symmetry, the treated group should be selected so as to consider the first 9 birth months to the right of the cut-off. The same reasoning is applied to the other quarters of 2005 and 2004, so as to increase precision. The selection of the birth months is reported in Table 6.

Note that in 2004q1 two eligibility conditions could confound the anticipation effects: on the one side, the eligibility for full pension (age 63) for the control group; on the other side, the eligibility for anticipated pension (age 60) for the treated group. Accordingly, we select control units born in September 1941 - November 1942 and treated units born in December 1942 - February 1944, since the treated units born in March 1944 - July 1944 are not yet entitled to anticipated pension in 2004q1.

Table 6: Anticipation effect: selection of birth months

	Control group	Treated group
2004q1	09/1941-11/1942	12/1942-02/1943
2004q2	06/1941-11/1942	12/1942-05/1943
2004q3	09/1941-11/1942	12/1942-02/1943
2004q4	12/1941-11/1942	12/1942-11/1943
2005q1	03/1942-11/1942	12/1942-08/1943
2005q2	06/1942-11/1942	12/1942-05/1943
2005q3	09/1942-11/1942	12/1942-02/1943

Based on this selection, the final sample used for the analysis of anticipation effect is reported in Table 7 by treatment group.

The graphical analysis is shown in Figures 5 to 8. Table 8 below reports the estimated treatment effects for a selection of window widths (of 10, 20, 30 and 36 months) and for the linear and quadratic specification. There is no effect on the employment rate, pension rate or rate on the residual category. This suggests that individuals do not change their preferences vis-a-vis working in anticipation of the policy change. As for the fraction of time worked, some specifications do display significant effects with the quadratic specifications (see Table 8). However, based on Figure 8, the significance of the treatment effect seems to be driven by the non-linear parametric fit rather than indicating a real discontinuity at the cutoff. Hence, overall there is no evidence of anticipation. This result is reasonable, since overall we find that the reform did not change the pathway into retirement.

Table 7: Anticipation effects: sample size by birth month

Control group			Treated group		
birth month	unweighted	weighted[§]	birth month	unweighted	weighted[§]
Jun 1941	453	3598.79	Dec 1942	4060	28705.52
Jul 1941	494	3901.94	Jan 1943	4662	32412.50
Aug 1941	442	3640.22	Feb 1943	4032	29647.76
Sep 1941	1272	10788.03	Mar 1943	4230	31010.07
Oct 1941	1257	9907.93	Apr 1943	4410	30197.36
Nov 1941	1272	9608.36	May 1943	4566	26927.10
Dec 1941	1776	13520.30	Jun 1943	3460	19689.86
Jan 1942	2416	18878.99	Jul 1943	3360	21393.19
Feb 1942	1964	14022.88	Aug 1943	3630	22414.63
Mar-1942	2440	20004.02	Sep 1943	2952	16176.37
Apr 1942	2375	16652.22	Oct 1943	3116	19466.07
May 1942	2615	18278.41	Nov 1943	3000	18147.89
Jun 1942	2982	25270.80	Dec 1943	2214	10509.33
Jul 1942	2976	21771.95	Jan 1944	2538	15161.26
Aug 1942	3000	20964.37	Feb 1944	2418	13598.59
Sep 1942	3752	28009.27	Mar 1944	888	5456.41
Oct 1942	3598	25138.57	Apr 1944	798	4489.87
Nov 1942	3689	28916.47	May 1944	888	4976.52
Total	38773	292873.50	Total	55222	350380.30

This table reports the number of observations present in the sample by month of birth, pooling quarters 2004Q1-2005Q3.
[§] Weighted using the endogenous sampling weights W_i.

8 Conclusion

This paper evaluates the effect on employment and labour market exit of an exogenous increase in the FRA for Belgian women in 2006, from 63 to 64. In recent years many EU countries have introduced similar measures with the aim to raise the average effective retirement age. The idea is that the basic pension value remains the same but that people have to work longer to obtain it. Retirement at the previous retirement age results in a reduced pension (OECD, 2006). Evidence on the effectiveness of these changes suggests that increasing the eligibility age to (early) retirement can be successful in prolonging careers. Comparable studies to our own by among others Lalive and Staubli (2015), Manoli and Weber (2016), or Staubli and Zweimüller (2013) show a positive impact on the age of exit out of the labour market or on the employment rate.

We find a significant negative effect of 32 *pp* on the pension take-up rate in 2006. Thus, the policy has the expected effect: Belgian women start to claim their full pension later. However, women do not change their pathway into retirement due to the reform. That is, the proportion of women who withdraw prematurely from the labour market does not change. Similarly, the proportion of women who survive in employment until retirement is not affected. What changes is the length of the working spells for the women who survive in employment. These women remain employed

Anticipation effect: graphical evidence for 2004q1-2005q3

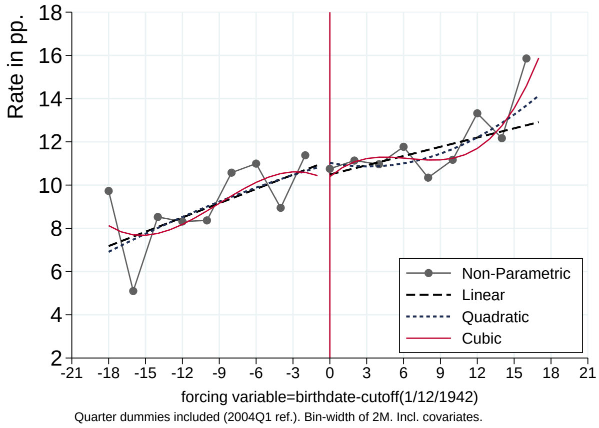

Figure 5: Employment rate

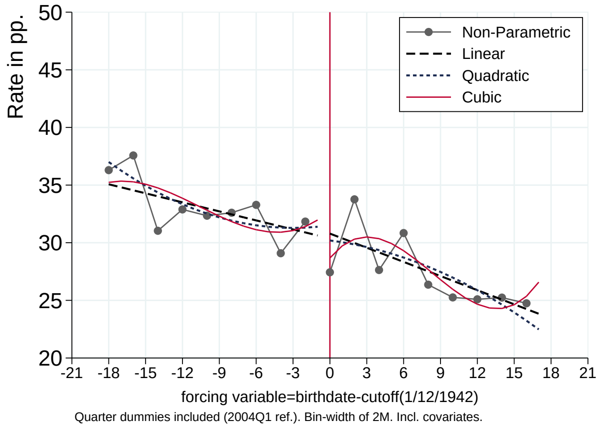

Figure 6: Pension rate

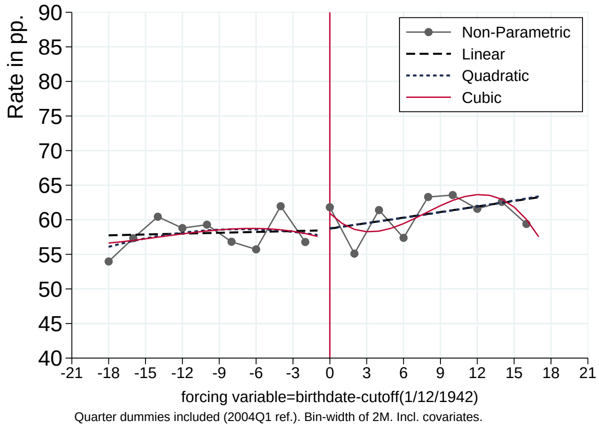

Figure 7: Rate in residual category

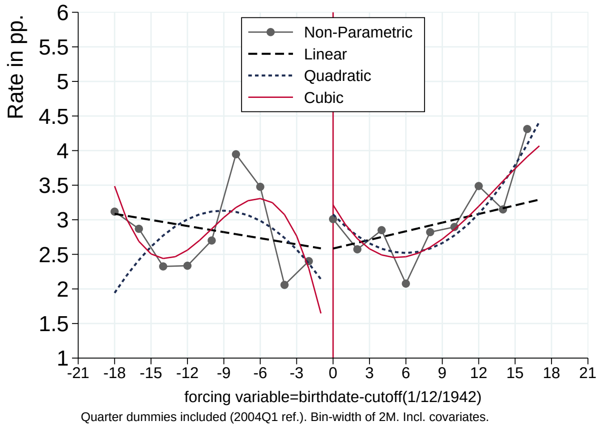

Figure 8: Full-time equivalent time worked

longer, and this increases the employment rate in 2006. Relative to the low female employment rate close to the FRA, the increase in the employment rate is economically important and ranges between 31 to 47%. These results are in line with those of previous studies.

We contribute to the literature on pension reforms by studying a country with many early retirement options and where the pension system does not entail any actuarial adjustment to pension so as to penalise early claims. This is in contrast with other countries studied in the literature, where the increase in the FRA is combined with actuarial reductions in the pension benefit in case of early pension claiming (e.g. Lalive and Staubli (2015) for Switzerland). Despite the absence of these financial incentives, our positive and sizeable effects on the employment rate suggest that Belgian women are very much reactive to changes in the eligibility conditions. This is in favour of the working hypothesis that, in countries with generous pension systems and with many early retirement options, changes in the eligibility for (early) retirement may be sufficient to delay retirement, and

Table 8: Anticipation effects: overview of the effects for selected window widths.

	Linear specification				Quadratic specification			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employment rate	-0.0058	-0.0073	0.0025	-0.0007	0.0030	0.0097	-0.0080	-0.0120
	(0.0105)	(0.0107)	(0.0125)	(0.0165)	(0.0135)	(0.0159)	(0.0160)	(0.0150)
Pension rate	0.0057	0.0077	0.0009	-0.0314	-0.0085	-0.0172	-0.0408	-0.0837
	(0.0271)	(0.029)	(0.0355)	(0.0452)	(0.0379)	(0.0391)	(0.044)	(0.0715)
Rate in residual	0.0001	-0.0004	-0.0034	0.0321	0.0055	0.0075	0.0488	0.0958
category	(0.0294)	(0.0308)	(0.0380)	(0.0452)	(0.0390)	(0.0407)	(0.0417)	(0.0669)
Full-time equivalent	-0.0004	0.0001	0.0071	0.0159**	0.0100*	0.0130**	0.0152**	0.0154***
time worked	(0.0052)	(0.0053)	(0.0057)	(0.0056)	(0.0058)	(0.0056)	(0.0061)	(0.0033)
Window width	36	30	20	10	36	30	20	10
Obs. (unweighted)	93,995	90,032	68,753	38,409	93,995	90,032	68,753	38,409
Obs. (weighted)	643,253.8	617,190.1	477,603.3	276,773.8	643,253.8	617,190.1	477,603.3	276,773.8

*** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by month of birth shown in parenthesis. Column 1-4 (5-8) report the treatment effect resulting from estimating Eq.3 with linear (quadratic) specification without spline on different window widths. All specifications control for quarter-specific effects and the following covariates centered at the sample mean (not reported): region of residence, Belgian nationality, being married, having children, working part-time in private sector between 1990-1996, working as blue collar in private sector between 1990-1996, full-time equivalent time worked in private sector between 1990-1996, number of firms to which one has been affiliated between 1990-1996, average monthly salary in private sector between 1990-1996, and years of experience in private sector from labour market entry until 1996 (see Table 3 for more information).

thus reforms that reduce the generosity of the pension may not be necessary. Since benefit cuts decrease the welfare of the pensioners, this has strong policy implications from a social welfare perspective. This conjecture is also consistent with the Austrian study by Manoli et al. (2015) which show that, in a country with generous pensions and high early retirement rates, reforms affecting the eligibility for pension are more effective in postponing retirement than changes in actuarial adjustments to benefits.

Although the reform is very effective for the sub-group of women who survive in employment until age 63, the overall impact of the reform is limited. As a matter of fact, these women represent a minority in the Belgian labour force, as most of women exit the labour market through the early retirement possibilities. This is because the early withdrawals are not punished, and the time spent in most early retirement regimes is fully assimilated to working time in the pension calculation. In line with Lefebvre and Orsini (2011) and Jousten et al. (2012), this suggests that in countries with many early retirement options it is important to consider the universe of (early) retirement policies at once to give correct incentives.

Our analysis has a few limitations. First, in line with the comparable studies mentioned above, we only consider the effects of the policy on the extensive margin of labour force participation. However, there might be other effects of the policy as well. de Grip et al. (2012) find an adverse effect on mental health of treated older workers.[38] In turn, this could influence the decision to

[38] The authors note that mental health could also have been affected by the late announcement of the policy change and the perceived unfairness towards younger generations, as both factors are beyond the control of the worker.

continue working after the policy change and limit the effectiveness of the policy change (Hanel and Riphahn, 2012). Second, retirement decisions are likely to be taken jointly within the couple. Hence, it could be interesting to assess how the reform affects the spouses' retirement decisions (e.g. Lalive and Parrotta, 2016). Unfortunately, we cannot investigate this research question based on our data. In addition, this study ignores the impact of the reform on the supply side. This may be relevant, since it could be costly for firms to hire or keep old-age workers in the firm until the new FRA, as the latter earn higher salaries and yet are less productive than younger workers. Finally, the increased employment rate of older workers (the aim of the reform) may crowd out the labour supply of younger workers.

We can think of several avenues for improving the current research. First, providing a characterisation of the compliers. A second possibility is to incorporate the 2009 reform in the study. Results for the 2009 reform could reinforce the findings related to the 2006 one. Note that the 2009 increase in the FRA follows the introduction of the Pension Bonus for delaying pension claiming beyond age 62. Although this incentive is small, it would be interesting to compare the effect of increasing the FRA without any financial incentive (as the 2006 reform) versus the effect of the same increase in the FRA in presence of the Pension Bonus.

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Appendix

A Policies related to early exit from the labour market

Table 9: Policies **preventing** early exits in Belgium around January 2006

Name	Short Description
1. Pension bonus	Pensions starting 1/1/2007 or later are increased with a lump sum allowance for every day worked after age 62 or the eligibility career length (44). These days have to be worked January 1, 2006 or later and up to 30 non-working days can be assimilated under certain conditions.
2. Outplacement	Professional job search assistance for laid-off workers aged at least 45, to prevent early LM exit. Exists since July 1, 2002 for workers with $\geq$ 1y seniority in the firm. Since April 1, 2006 this scheme is more intense in sectors under restructuring or firms in difficulties. Participants receive their normal salary from the previous employer. The scheme is not aimed at workers above 58 as they are already exempted from job search (see Table 10).
3. Late retirement	It is possible to work beyond the FRA, for example to improve the pension allowance, as this only covers the last 43 (44) career years. However, incentives to work after the FRA are in general weak, as earnings above certain limits result in a withdrawal of the pension allowances for as long as the earnings are too high.
4. Structural wage subsidy	Employer social security contributions for employees aged 58 are decreased since 1/4/2002. This age has been decreased to 57 since 2004 and 50 since 1/4/07, but irrelevant for our sample).

Table 10: Policies **allowing** early exits in Belgium around January 2006

Name	Short Description	Replacement Income	Age Elig.[1]	Career requirement[1]	Impact on Pension
1. Anticipated Full Pension	Anticipate start of the full pension with same characteristics as full pension after FRA.	Same calculation as full pension	60	35	Fewer years of pension contribution imply lower pension. No actuarial reduction in full pension.[2]
2. Pre-retirement (bridge pension)	Unemployment benefits (UB) in combination with allowance from previous employer. Eligibility requirements can differ by sector. Recipients are not expected to take up a new position.	UB + 1/2(last net salary-UB)[3]	58	25	Need to stay in pre-retirement until FRA, anticipated full pension not possible. Period is assimilated to working period.
3. Pseudo Pre-Retirement (Canada Dry)	Same as normal Pre-Retirement, but based on an agreement between the employer and the employee.[4]	UB + lump sum payment from employer[3]	45	none	Need to stay in pseudo pre-retirement until FRA, anticipated full pension not possible. Period is assimilated to working period.
4. Involuntary unemployment of older workers	Removal of job search requirements, including the need to stay available on the labour market or be registered as job seeker.	UB + old age supplement[3]	58	none	Period is assimilated to working period.
5. Full-time time credit	Allows workers to fully interrupt career in return for an allowance from the government. Number of years in the scheme is limited.[4]	Lump sum allowance	none	none	Up to 3y assimilated to working time.
6. Part-time time credit for older workers	Allows workers aged 50+ to reduce working time by 20 or 50% of a full-time job until reaching the FRA. Participation can be interrupted. Part of wage loss compensated by government.[4]	Remaining salary + lump sum allowance	50	5y tenure + 20y career	Period is assimilated to working period.
7. Sickness and Disability scheme	Provides benefits for individuals that suffer from a loss of their earnings capacity in their usual job of at least 66% due to sickness or disability for more than 12 months. The first 12 months are covered by the sickness insurance.	65% of earnings before injury (lower if living alone).	none	5y paid contributions[5]	Period is assimilated to working period.

[1] Age eligibility and the career length requirement do not change around 2006.

[2] For self-employed workers there is 5% reduction in pension benefit per anticipated year.

[3] Unemployment benefits (UB) are usually calculated at 60% of the last gross salary, subject to certain maxima.

[4] From April 1, 2006 onwards social security contributions are levied on the (extra) allowances paid by the employer for the allowances started in 2006. This excludes 20% reductions in working time of the old-age part-time time credit scheme.

[5] At least three of those five years must have been during the last five years. There is no career requirement for professional diseases or accidents at work.

B Endogenous sampling procedure

The population on which our sample is based is stratified by gender in the nine birth cohorts defined in Table 11. The reference periods by birth cohort were chosen in function of Albanese and Cockx (2015), as they had to observe sufficient transitions in and out of private sector employment for both their treated and control group to be able to evaluate the policy discussed in that paper. Each of these 18 strata is subsequently endogenously stratified in five substrata:

1. The population exiting salaried employment in the private sector within the reference period;
2. The population entering salaried employment in the private sector within the reference period and not contained in substratum 1;
3. The population employed throughout the reference period as salaried worker in the private sector and earning a gross wage lower than €100 per day at the start of this period;
4. The population employed throughout the reference period as salaried worker in the private sector and earning at least €100 per day at the start of this period;
5. The population that was not employed as salaried worker in the private sector during the reference period, i.e. individuals who were out of the labour force, unemployed, self-employed or working in the public sector.

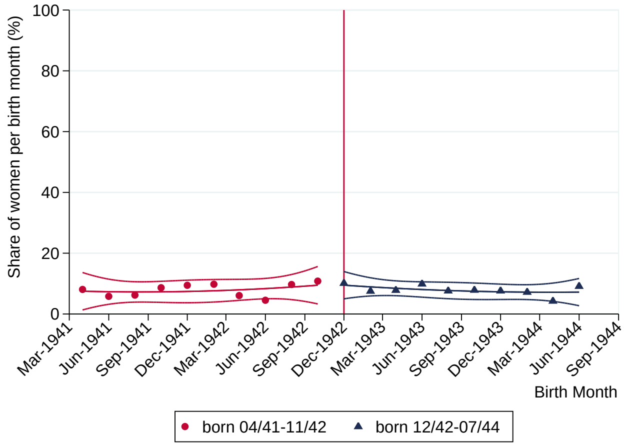
Table 11: Retained Birth Cohorts and Corresponding Reference Periods

	Cohort (quarter/year)	Reference Period (quarter/year)
1	2/41-1/42	[2/99-1/02]
2	2/42-1/43	[2/99-4/01]
3	2/43-1/44	[2/99-4/03]
4	2/44-1/45	[2/00-1/05]
5	2/45-1/46	[2/99-4/03]
6	2/46-1/47	[2/00-4/04]
7	2/47-1/48	[2/00-3/05]
8	2/48-1/49	[2/02-3/05]
9	2/49-1/50	[2/02-3/05]

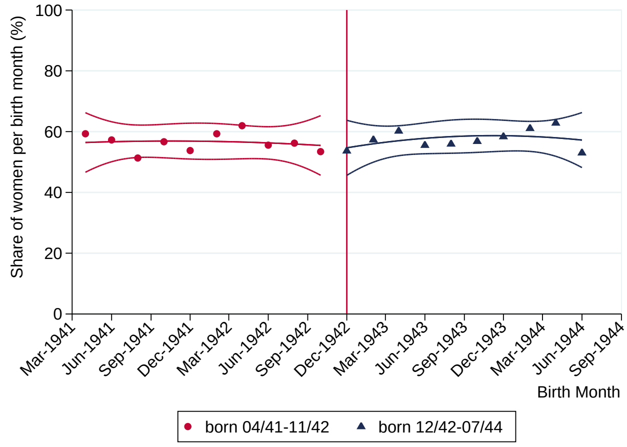
In each of the 18 strata a random sample of 2,000 individuals is drawn in this substratum, while the sample size was 1,500 for substratum 4 and 5. In cases that the population of the substratum was smaller than the population, the complete population was sampled. The size of the population is known for each substratum, so that it was straightforward to construct the appropriate weights to make inference on the population.

C Distribution Covariates around Cut-Off

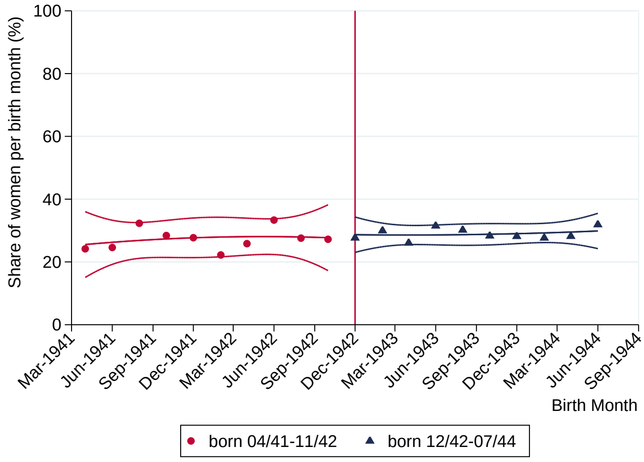
Figure 9: Distribution of personal characteristics per birth month (weighted by W_i)



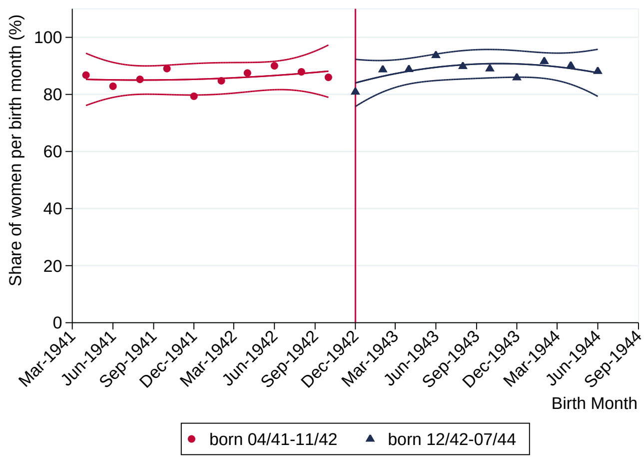
A: Share of women living in Brussels



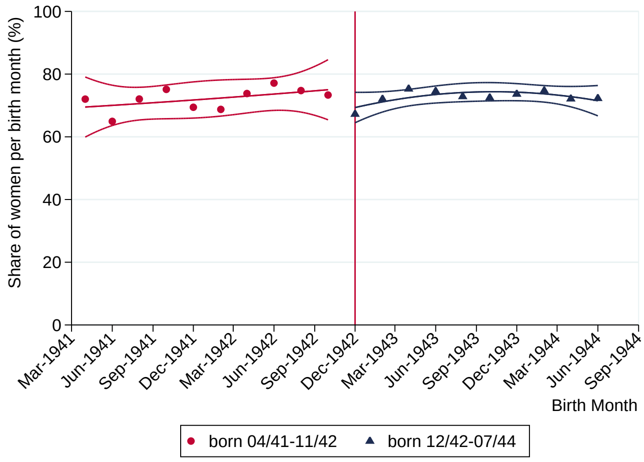
B: Share of women living in Flanders



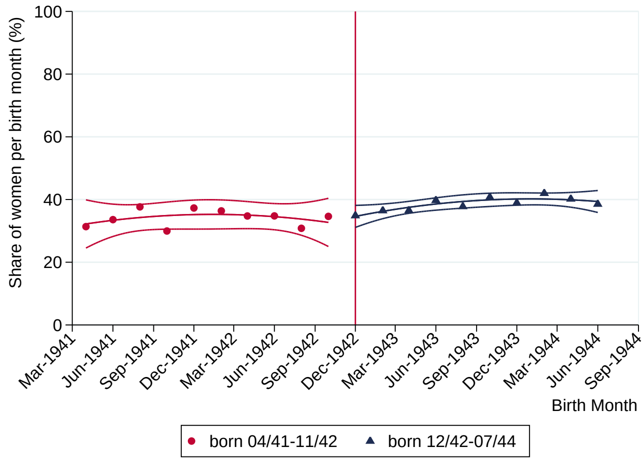
C: Share of women living in Wallonia



D: Share of women with Belgian nationality

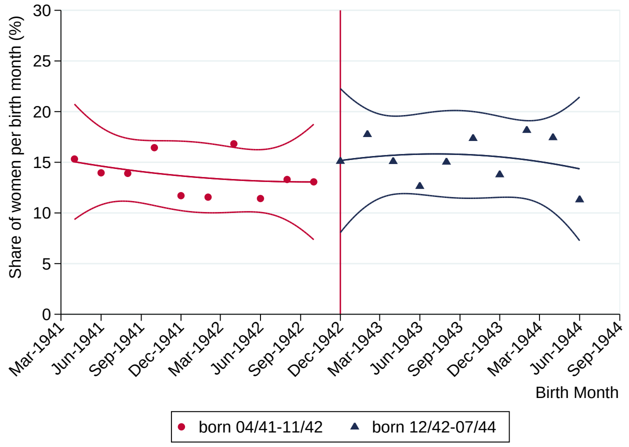


E: Share of women with a spouse

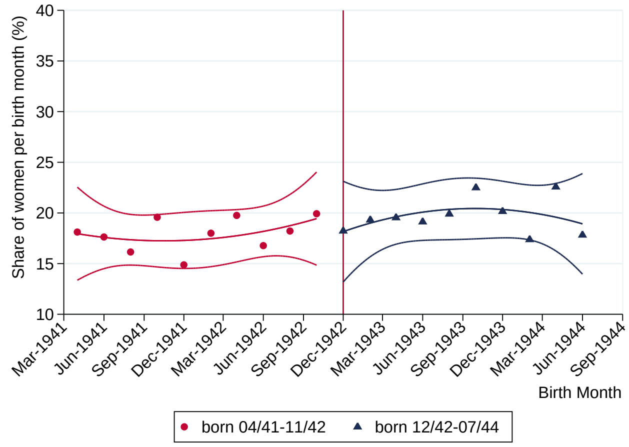


F: Share of women with children

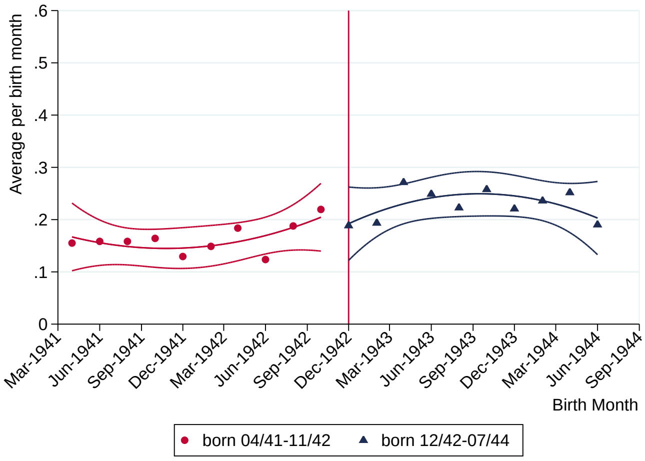
Figure 10: Distribution of private sector labour market characteristics per birth month (weighted by W_i)



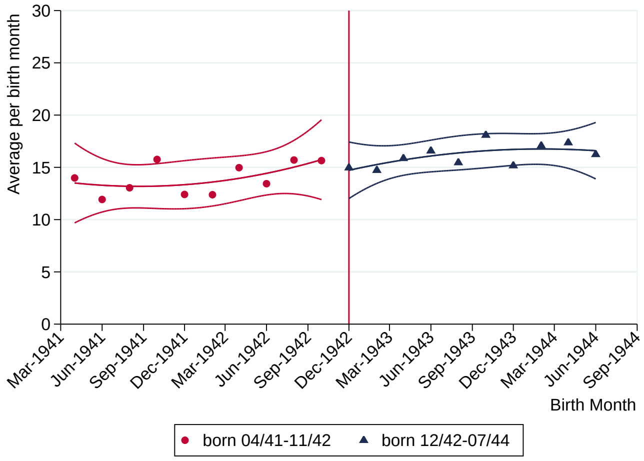
A: Share of women with blue collar experience ('90-'96)



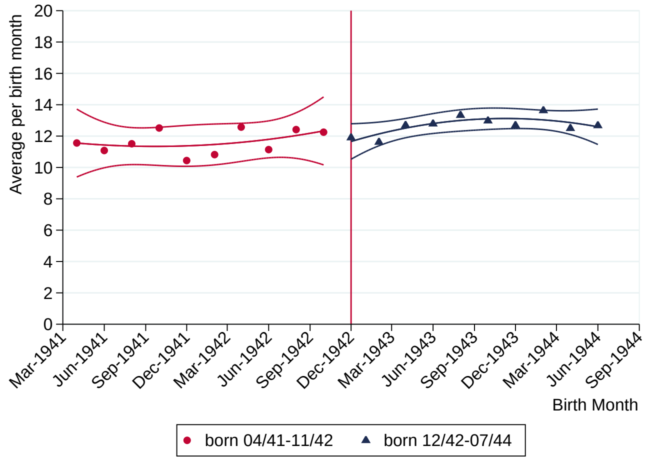
B: Share of women with part-time experience ('90-'96)



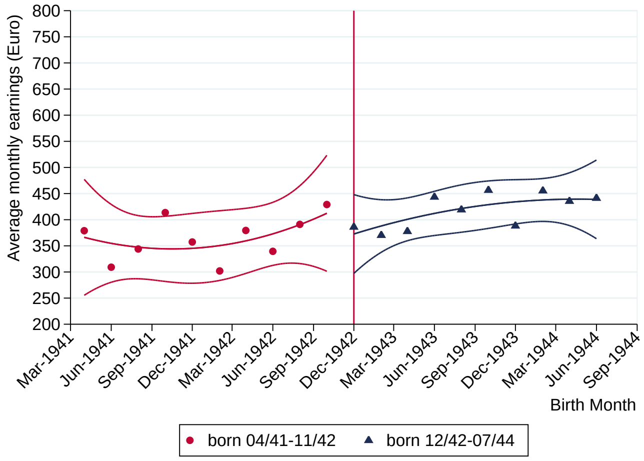
C: Number of jobs ('90-'96)



D: Full-time equivalent working time ('90-'96)



E: Number of years experience (age 14-'96)



F: Average earnings ('90-'96)

D Robustness analysis

Table 12: Pooled estimations - pension rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
linear	pL20	pL19	pL18	pL17	pL16	pL15	pL14	pL13	pL12	pL11	pL10	pL9	pL8	pL7	pL6	pL5
T	-0.3208***	-0.3145***	-0.3123***	-0.3118***	-0.3060***	-0.2983***	-0.3057***	-0.3091***	-0.3116***	-0.3149***	-0.3268***	-0.3348***	-0.3366***	-0.3595***	-0.3478***	-0.3493***
se	(0.0238)	(0.0242)	(0.0252)	(0.0263)	(0.0273)	(0.0285)	(0.0293)	(0.0307)	(0.0322)	(0.0333)	(0.0322)	(0.0334)	(0.0361)	(0.0355)	(0.0336)	(0.0369)
P-val	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0001	0.0001	0.0002	0.0004	0.0019	0.0080
linear spline	pL_i20	pL_i19	pL_i18	pL_i17	pL_i16	pL_i15	pL_i14	pL_i13	pL_i12	pL_i11	pL_i10	pL_i9	pL_i8	pL_i7	pL_i6	pL_i5
T	-0.3200***	-0.3151***	-0.3135***	-0.3120***	-0.3078***	-0.2997***	-0.3088***	-0.3124***	-0.3143***	-0.3215***	-0.3291***	-0.3368***	-0.3357***	-0.3549***	-0.3489***	-0.3421**
se	(0.0226)	(0.0231)	(0.0238)	(0.0250)	(0.0261)	(0.0274)	(0.0277)	(0.0293)	(0.0313)	(0.0308)	(0.0306)	(0.0313)	(0.0327)	(0.0313)	(0.0317)	(0.0354)
P-val	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0001	0.0001	0.0002	0.0007	0.0031	0.0218
quadratic	pQ20	pQ19	pQ18	pQ17	pQ16	pQ15	pQ14	pQ13	pQ12	pQ11	pQ10	pQ9	pQ8	pQ7	pQ6	pQ5
T	-0.3096***	-0.3153***	-0.3173***	-0.3217***	-0.3281***	-0.3463***	-0.3427***	-0.3470***	-0.3537***	-0.3459***	-0.3624***	-0.3625**	-0.3823**	-0.3794*	-0.3794	-0.4194
se	(0.0361)	(0.0360)	(0.0370)	(0.0379)	(0.0385)	(0.0365)	(0.0376)	(0.0386)	(0.0398)	(0.0429)	(0.0411)	(0.0423)	(0.0434)	(0.0433)	(0.0472)	(0.0600)
P-val	0.0000	0.0001	0.0001	0.0002	0.0002	0.0003	0.0003	0.0005	0.0026	0.0043	0.0076	0.0162	0.0309	0.0700	0.1214	0.1301
quadratic spline	pQ_i20	pQ_i19	pQ_i18	pQ_i17	pQ_i16	pQ_i15	pQ_i14	pQ_i13	pQ_i12	pQ_i11	pQ_i10	pQ_i9	pQ_i8	pQ_i7	pQ_i6	pQ_i5
T	-0.3136***	-0.3208***	-0.3240***	-0.3292***	-0.3378***	-0.3590***	-0.3556***	-0.3610***	-0.3681***	-0.3602***	-0.3673***	-0.3625***	-0.3816***	-0.3686***	-0.3788**	-0.4426*
se	(0.0333)	(0.0328)	(0.0333)	(0.0339)	(0.0340)	(0.0310)	(0.0307)	(0.0309)	(0.0337)	(0.0350)	(0.0344)	(0.0374)	(0.0371)	(0.0352)	(0.0479)	(0.0405)
P-val	0.0001	0.0001	0.0001	0.0002	0.0002	0.0001	0.0001	0.0001	0.0009	0.0016	0.0042	0.0081	0.0083	0.0075	0.0157	0.0590
cubic	pC20	pC19	pC18	pC17	pC16	pC15	pC14	pC13	pC12	pC11	pC10	pC9	pC8	pC7	pC6	pC5
T	-0.3591***	-0.3613***	-0.3687***	-0.3753***	-0.3763**	-0.3617**	-0.3726**	-0.3770*	-0.3760*	-0.3913	-0.3914	-0.4010	-0.3907	-0.4056	-0.4173	-0.3827
se	(0.0384)	(0.0390)	(0.0402)	(0.0426)	(0.0441)	(0.0434)	(0.0469)	(0.0477)	(0.0471)	(0.0553)	(0.0527)	(0.0582)	(0.0514)	(0.0526)	(0.0699)	(0.0445)
P-val	0.0013	0.0022	0.0037	0.0077	0.0159	0.0251	0.0402	0.0569	0.0840	0.1216	0.1440	0.1475	0.1215	0.1106	0.2188	0.2090
cubic spline	pC_i20	pC_i19	pC_i18	pC_i17	pC_i16	pC_i15	pC_i14	pC_i13	pC_i12	pC_i11	pC_i10	pC_i9	pC_i8	pC_i7	pC_i6	pC_i5
T	-0.3716***	-0.3764***	-0.3846***	-0.3887***	-0.3882***	-0.3639***	-0.3738***	-0.3733***	-0.3591***	-0.3873***	-0.3826***	-0.4085**	-0.3950**	-0.4315*	-0.5237*	-0.4776
se	(0.0306)	(0.0307)	(0.0313)	(0.0339)	(0.0362)	(0.0396)	(0.0411)	(0.0423)	(0.0487)	(0.0370)	(0.0429)	(0.0395)	(0.0447)	(0.0574)	(0.0418)	(0.0408)
P-val	0.0001	0.0001	0.0005	0.0007	0.0024	0.0060	0.0058	0.0077	0.0091	0.0018	0.0035	0.0124	0.0330	0.0912	0.0598	0.1028
window width	40	38	36	34	32	30	28	26	24	22	20	18	16	14	12	10
Unweighted Obs.	98,284	93,332	88,380	83,016	77,848	72,528	67,608	62,548	57,900	53,124	47,592	42,676	37,820	33,232	28,372	23,340
Weighted Obs.	661,712.2	627,632.1	594,832.6	560,531.3	526,964.1	490,577.5	458,062.0	424,636.4	397,812.8	366,144.7	327,799.6	297,600.3	263,665.4	233,229.1	202,854.5	168,055.9

The table reports the treatment effect from estimating Eq. (3). The columns refer to different window widths. The rows refer to different specifications. Standard errors (se) clustered by month of birth shown in parenthesis. $P-val$ indicates the wild bootstrap P-value (10,000 replications and Webb weights). *** $P-val$ <0.01, ** $P-val$ <0.05, * $P-val$ <0.1.

Table 13: Pooled estimations - employment rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
linear	pL20	pL19	pL18	pL17	pL16	pL15	pL14	pL13	pL12	pL11	pL10	pL9	pL8	pL7	pL6	pL5
T	0.0155	0.0188*	0.0176	0.0178	0.0166	0.0090	0.0187*	0.0196*	0.0219*	0.0274**	0.0249**	0.0209*	0.0286**	0.0218*	0.0376***	0.0381**
se	(0.0099)	(0.0103)	(0.0111)	(0.0123)	(0.0140)	(0.0116)	(0.0100)	(0.0100)	(0.0100)	(0.0094)	(0.0097)	(0.0100)	(0.0097)	(0.0108)	(0.0119)	(0.0139)
P-val	0.1397	0.0843	0.1342	0.1750	0.2806	0.4618	0.0903	0.0781	0.0547	0.0144	0.0269	0.0643	0.0144	0.0769	0.0087	0.0315
linear spline	pL_i20	pL_i19	pL_i18	pL_i17	pL_i16	pL_i15	pL_i14	pL_i13	pL_i12	pL_i11	pL_i10	pL_i9	pL_i8	pL_i7	pL_i6	pL_i5
T	0.0144	0.0179*	0.0167	0.0158	0.0135	0.0086	0.0188*	0.0191*	0.0208*	0.0266**	0.0236**	0.0201*	0.0295**	0.0241*	0.0390**	0.0325*
se	(0.0096)	(0.0099)	(0.0107)	(0.0115)	(0.0125)	(0.0118)	(0.0096)	(0.0097)	(0.0098)	(0.0090)	(0.0090)	(0.0096)	(0.0092)	(0.0107)	(0.0119)	(0.0112)
P-val	0.1551	0.0921	0.1466	0.2042	0.3213	0.4981	0.0804	0.0791	0.0642	0.0173	0.0341	0.0771	0.0126	0.0636	0.0168	0.0751
quadratic	pQ20	pQ19	pQ18	pQ17	pQ16	pQ15	pQ14	pQ13	pQ12	pQ11	pQ10	pQ9	pQ8	pQ7	pQ6	pQ5
T	0.0244*	0.0210*	0.0230*	0.0267**	0.0319**	0.0402***	0.0307**	0.0333**	0.0340**	0.0271*	0.0296**	0.0351**	0.0261**	0.0311**	0.0213	0.0321
se	(0.0115)	(0.0111)	(0.0111)	(0.0110)	(0.0130)	(0.0127)	(0.0109)	(0.0117)	(0.0130)	(0.0121)	(0.0134)	(0.0162)	(0.0128)	(0.0161)	(0.0134)	(0.0142)
P-val	0.0614	0.0967	0.0702	0.0472	0.0463	0.0065	0.0181	0.0113	0.0194	0.0518	0.0414	0.0357	0.0437	0.0399	0.2296	0.1989
quadratic spline	pQ_i20	pQ_i19	pQ_i18	pQ_i17	pQ_i16	pQ_i15	pQ_i14	pQ_i13	pQ_i12	pQ_i11	pQ_i10	pQ_i9	pQ_i8	pQ_i7	pQ_i6	pQ_i5
T	0.0216*	0.0178	0.0201*	0.0238*	0.0307**	0.0413**	0.0301**	0.0328**	0.0335**	0.0250*	0.0297*	0.0394*	0.0278*	0.0357	0.0038	0.0072
se	(0.0102)	(0.0099)	(0.0101)	(0.0107)	(0.0123)	(0.0126)	(0.0101)	(0.0111)	(0.0122)	(0.0116)	(0.0130)	(0.0161)	(0.0126)	(0.0175)	(0.0138)	(0.0060)
P-val	0.0635	0.1056	0.0891	0.0533	0.0331	0.0122	0.0224	0.0177	0.0267	0.0731	0.0521	0.0527	0.0954	0.2523	0.8684	0.4941
cubic	pC20	pC19	pC18	pC17	pC16	pC15	pC14	pC13	pC12	pC11	pC10	pC9	pC8	pC7	pC6	pC5
T	0.0299*	0.0374**	0.0391**	0.0388**	0.0348*	0.0219	0.0318**	0.0270*	0.0241	0.0317*	0.0271	0.0169	0.0251	0.0138	0.0243	0.0159
se	(0.0138)	(0.0153)	(0.0158)	(0.0155)	(0.0165)	(0.0127)	(0.0154)	(0.0143)	(0.0137)	(0.0153)	(0.0144)	(0.0141)	(0.0177)	(0.0154)	(0.0135)	(0.0137)
P-val	0.0570	0.0237	0.0182	0.0189	0.0609	0.1022	0.0421	0.0780	0.1309	0.0703	0.1100	0.2830	0.2014	0.4020	0.2371	0.6351
cubic spline	pC_i20	pC_i19	pC_i18	pC_i17	pC_i16	pC_i15	pC_i14	pC_i13	pC_i12	pC_i11	pC_i10	pC_i9	pC_i8	pC_i7	pC_i6	pC_i5
T	0.0305*	0.0402**	0.0441**	0.0422**	0.0327	0.0190	0.0343*	0.0285	0.0238	0.0357	0.0251	-0.0031	0.0039	-0.0311	0.0225***	0.0166
se	(0.0140)	(0.0155)	(0.0168)	(0.0168)	(0.0163)	(0.0136)	(0.0159)	(0.0156)	(0.0151)	(0.0178)	(0.0162)	(0.0171)	(0.0221)	(0.0230)	(0.0019)	(0.0071)
P-val	0.0567	0.0242	0.0239	0.0445	0.1512	0.2464	0.0804	0.1173	0.1776	0.1373	0.2604	0.8875	0.9019	0.4170	0.0052	0.1644
window width	40	38	36	34	32	30	28	26	24	22	20	18	16	14	12	10
Unweighted Obs.	98,284	93,332	88,380	83,016	77,848	72,528	67,608	62,548	57,900	53,124	47,592	42,676	37,820	33,232	28,372	23,340
Weighted Obs.	661,712.2	627,632.1	594,832.6	560,531.3	526,964.1	490,577.5	458,062.0	424,636.4	397,812.8	366,144.7	327,799.6	297,600.3	263,665.4	233,229.1	202,854.5	168,055.9

The table reports the treatment effect from estimating Eq. (3). The columns refer to different window widths. The rows refer to different specifications. Standard errors (*se*) clustered by month of birth shown in parenthesis. $P - val$ indicates the wild bootstrap P-value (10,000 replications and Webb weights). *** $P - val$ <0.01, ** $P - val$ <0.05, * $P - val$ <0.1.

Table 14: Pooled estimations - Rate of early withdrawals from the LM

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
linear	pL20	pL19	pL18	pL17	pL16	pL15	pL14	pL13	pL12	pL11	pL10	pL9	pL8	pL7	pL6	pL5
T	0.0066	0.0036	0.0049	0.0071	0.0085	0.0153	0.0078	0.0036	-0.0028	-0.0032	-0.0046	-0.0048	-0.0043	0.0075	-0.0019	0.0001
se	(0.0091)	(0.0093)	(0.0099)	(0.0109)	(0.0124)	(0.0108)	(0.0098)	(0.0102)	(0.0098)	(0.0094)	(0.0103)	(0.0103)	(0.0104)	(0.0099)	(0.0107)	(0.0145)
P-val	0.4854	0.7067	0.6279	0.5306	0.5215	0.1785	0.4461	0.7294	0.7816	0.7376	0.6626	0.6466	0.6877	0.4715	0.8711	0.9931
linear spline	pL_i20	pL_i19	pL_i18	pL_i17	pL_i16	pL_i15	pL_i14	pL_i13	pL_i12	pL_i11	pL_i10	pL_i9	pL_i8	pL_i7	pL_i6	pL_i5
T	0.0085	0.0055	0.0074	0.0100	0.0126	0.0177	0.0095	0.0063	0.0001	-0.0013	-0.0025	-0.0038	-0.0055	0.0069	-0.0019	0.0073
se	(0.0085)	(0.0086)	(0.0093)	(0.0100)	(0.0105)	(0.0106)	(0.0094)	(0.0090)	(0.0085)	(0.0085)	(0.0089)	(0.0099)	(0.0123)	(0.0106)	(0.0115)	(0.0083)
P-val	0.3403	0.5372	0.4372	0.3407	0.2540	0.1146	0.3354	0.5019	0.9905	0.8821	0.7803	0.7132	0.6927	0.5734	0.8955	0.4962
quadratic	pQ20	pQ19	pQ18	pQ17	pQ16	pQ15	pQ14	pQ13	pQ12	pQ11	pQ10	pQ9	pQ8	pQ7	pQ6	pQ5
T	0.0002	0.0030	-0.0002	-0.0052	-0.0113	-0.0217*	-0.0154	-0.0162	-0.0101	-0.0067	-0.0016	0.0043	0.0123	-0.0021	0.0017	-0.0168
se	(0.0117)	(0.0119)	(0.0117)	(0.0120)	(0.0130)	(0.0114)	(0.0105)	(0.0114)	(0.0124)	(0.0118)	(0.0134)	(0.0145)	(0.0161)	(0.0087)	(0.0159)	(0.0096)
P-val	0.9887	0.8117	0.9893	0.6809	0.4296	0.1144	0.2050	0.2333	0.4722	0.6124	0.9136	0.7938	0.5134	0.8190	0.9328	0.4177
quadratic spline	pQ_i20	pQ_i19	pQ_i18	pQ_i17	pQ_i16	pQ_i15	pQ_i14	pQ_i13	pQ_i12	pQ_i11	pQ_i10	pQ_i9	pQ_i8	pQ_i7	pQ_i6	pQ_i5
T	0.0074	0.0108	0.0074	0.0022	-0.0052	-0.0177	-0.0104	-0.0107	-0.0041	-0.0008	0.0038	0.0102	0.0228	0.0054	0.0269	0.0125
se	(0.0098)	(0.0097)	(0.0096)	(0.0105)	(0.0117)	(0.0118)	(0.0106)	(0.0119)	(0.0122)	(0.0123)	(0.0135)	(0.0137)	(0.0137)	(0.0117)	(0.0177)	(0.0131)
P-val	0.4656	0.2715	0.4509	0.8488	0.6835	0.1133	0.3253	0.3813	0.7638	0.9552	0.8059	0.5089	0.0469	0.6605	0.1307	0.3866
cubic	pC20	pC19	pC18	pC17	pC16	pC15	pC14	pC13	pC12	pC11	pC10	pC9	pC8	pC7	pC6	pC5
T	-0.0102	-0.0184	-0.0203	-0.0183	-0.0131	0.0026	0.0001	0.0071	0.0043	0.0050	0.0056	-0.0003	-0.0183	-0.0113	-0.0289***	-0.0250**
se	(0.0150)	(0.0149)	(0.0146)	(0.0145)	(0.0159)	(0.0158)	(0.0152)	(0.0190)	(0.0193)	(0.0190)	(0.0198)	(0.0160)	(0.0117)	(0.0114)	(0.0090)	(0.0077)
P-val	0.5470	0.3121	0.2738	0.3257	0.5020	0.8883	0.9948	0.7566	0.8624	0.8451	0.8345	0.9900	0.2977	0.4921	0.1214	0.2638
cubic spline	pC_i20	pC_i19	pC_i18	pC_i17	pC_i16	pC_i15	pC_i14	pC_i13	pC_i12	pC_i11	pC_i10	pC_i9	pC_i8	pC_i7	pC_i6	pC_i5
T	-0.0056	-0.0159	-0.0190	-0.0165	-0.0068	0.0119	0.0066	0.0180	0.0178	0.0217	0.0317	0.0333	0.0052	0.0353	-0.0344*	-0.0587***
se	(0.0142)	(0.0150)	(0.0158)	(0.0156)	(0.0134)	(0.0131)	(0.0141)	(0.0139)	(0.0142)	(0.0159)	(0.0197)	(0.0235)	(0.0238)	(0.0305)	(0.0164)	(0.0151)
P-val	0.7189	0.2854	0.2132	0.2906	0.6262	0.3792	0.6758	0.2215	0.1931	0.1697	0.1222	0.2085	0.8610	0.3904	0.4116	0.2066
window width	40	38	36	34	32	30	28	26	24	22	20	18	16	14	12	10
Unweighted Obs.	24,571	23,333	22,095	20,754	19,462	18,132	16,902	15,637	14,475	3,281	11,898	10,669	9,455	8,308	7,093	5,835
Weighted Obs.	165,428.0	156,908.0	148,708.1	140,132.8	131,741.0	122,644.4	114,515.5	106,159.1	99,453.2	91,536.2	81,949.9	74,400.1	65,916.4	58,307.3	50,713.6	42,014.0

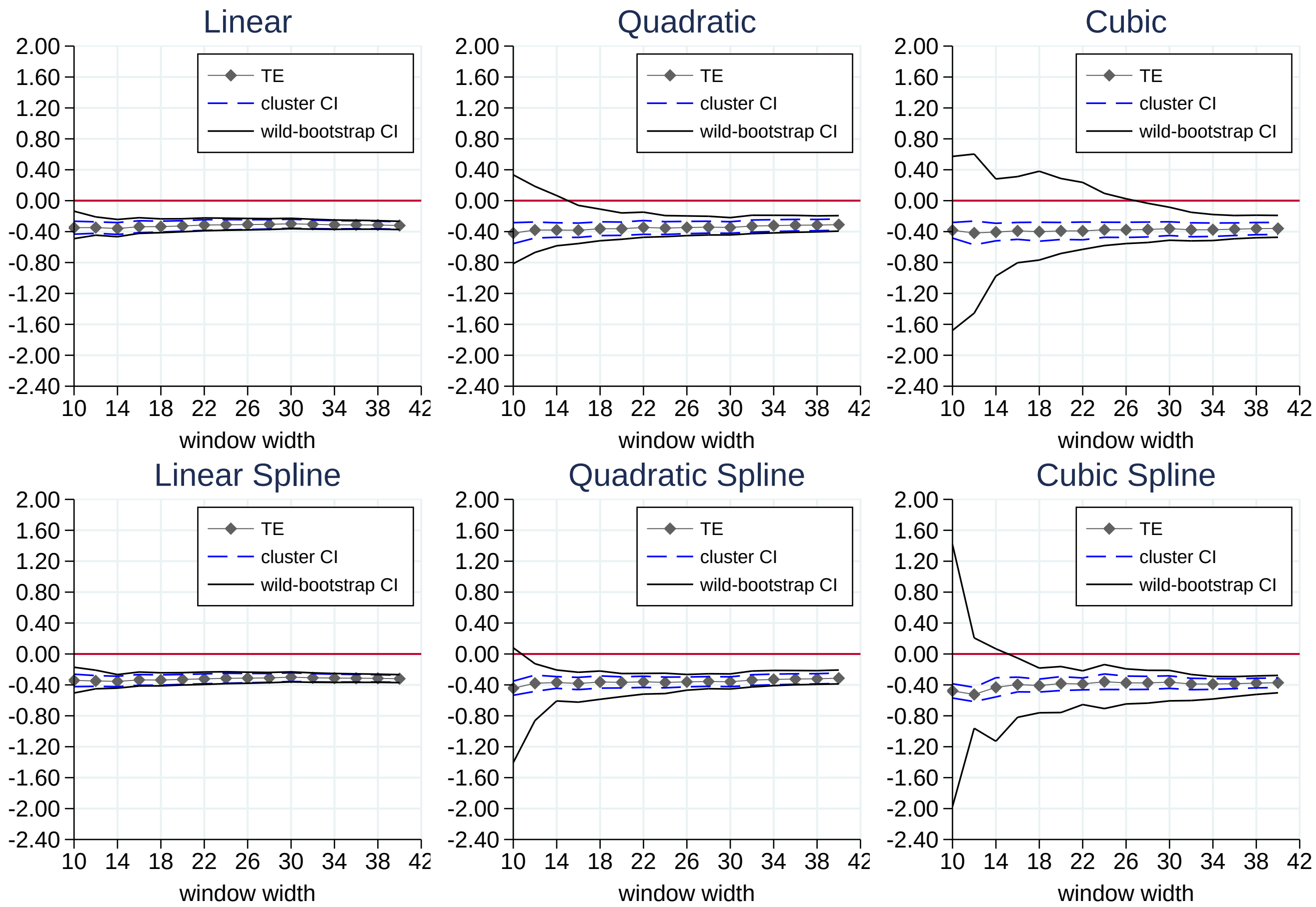
The table reports the treatment effect from estimating Eq. (3). The columns refer to different window widths. The rows refer to different specifications. Standard errors (se) clustered by month of birth shown in parenthesis. $P-val$ indicates the wild bootstrap P-value (10,000 replications and Webb weights). *** $P-val$ <0.01, ** $P-val$ <0.05, * $P-val$ <0.1.

Table 15: Rate of withdrawals from the LM

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
linear	pL20	pL19	pL18	pL17	pL16	pL15	pL14	pL13	pL12	pL11	pL10	pL9	pL8	pL7	pL6	pL5
T	-0.0114	-0.0121	-0.0143	-0.0152	-0.0160	-0.0199*	-0.0180*	-0.0125	-0.0060	-0.0023	0.0017	0.0021	-0.0019	-0.0103	-0.0027	-0.0018
se	(0.0079)	(0.0083)	(0.0086)	(0.0092)	(0.0099)	(0.0097)	(0.0098)	(0.0100)	(0.0096)	(0.0096)	(0.0094)	(0.0104)	(0.0108)	(0.0107)	(0.0106)	(0.0121)
P-val	0.1700	0.1641	0.1164	0.1161	0.1262	0.0522	0.0838	0.2332	0.5453	0.8156	0.8567	0.8541	0.8696	0.3512	0.8108	0.8869
linear spline	pL_i20	pL_i19	pL_i18	pL_i17	pL_i16	pL_i15	pL_i14	pL_i13	pL_i12	pL_i11	pL_i10	pL_i9	pL_i8	pL_i7	pL_i6	pL_i5
T	-0.0125	-0.0134	-0.0163*	-0.0175*	-0.0190**	-0.0222**	-0.0199**	-0.0147	-0.0085	-0.0041	0.0010	0.0018	0.0001	-0.0098	-0.0028	-0.0071
se	(0.0077)	(0.0080)	(0.0082)	(0.0086)	(0.0089)	(0.0092)	(0.0094)	(0.0093)	(0.0088)	(0.0089)	(0.0091)	(0.0101)	(0.0119)	(0.0105)	(0.0103)	(0.0107)
P-val	0.1250	0.1110	0.0595	0.0520	0.0418	0.0213	0.0459	0.1354	0.3583	0.6713	0.9130	0.8686	0.9958	0.3833	0.8035	0.5238
quadratic	pQ20	pQ19	pQ18	pQ17	pQ16	pQ15	pQ14	pQ13	pQ12	pQ11	pQ10	pQ9	pQ8	pQ7	pQ6	pQ5
T	-0.0116	-0.0093	-0.0040	-0.0004	0.0043	0.0128	0.0156	0.0152	0.0113	0.0061	-0.0050	-0.0087	-0.0131	0.0009	0.0006	0.0156
se	(0.0111)	(0.0113)	(0.0108)	(0.0110)	(0.0111)	(0.0100)	(0.0102)	(0.0109)	(0.0119)	(0.0121)	(0.0118)	(0.0134)	(0.0144)	(0.0093)	(0.0111)	(0.0104)
P-val	0.3185	0.4383	0.7282	0.9701	0.7168	0.2397	0.1486	0.1878	0.3815	0.6593	0.6793	0.5218	0.4075	0.9292	0.9625	0.1415
quadratic spline	pQ_i20	pQ_i19	pQ_i18	pQ_i17	pQ_i16	pQ_i15	pQ_i14	pQ_i13	pQ_i12	pQ_i11	pQ_i10	pQ_i9	pQ_i8	pQ_i7	pQ_i6	pQ_i5
T	-0.0173	-0.0152	-0.0096	-0.0060	-0.0003	0.0098	0.0133	0.0128	0.0089	0.0038	-0.0076	-0.0122	-0.0194	-0.0022	-0.0139	0.0011
se	(0.0103)	(0.0105)	(0.0099)	(0.0102)	(0.0103)	(0.0098)	(0.0104)	(0.0110)	(0.0115)	(0.0118)	(0.0117)	(0.0129)	(0.0158)	(0.0141)	(0.0196)	(0.0150)
P-val	0.1011	0.1624	0.3500	0.5846	0.9814	0.3315	0.2167	0.2598	0.4562	0.7631	0.5175	0.3265	0.2092	0.8944	0.5785	0.9563
cubic	pC20	pC19	pC18	pC17	pC16	pC15	pC14	pC13	pC12	pC11	pC10	pC9	pC8	pC7	pC6	pC5
T	0.0136	0.0170	0.0167	0.0165	0.0138	0.0036	-0.0029	-0.0088	-0.0101	-0.0097	-0.0027	0.0040	0.0188	0.0116	0.0242	0.0192
se	(0.0114)	(0.0119)	(0.0121)	(0.0125)	(0.0127)	(0.0118)	(0.0123)	(0.0141)	(0.0153)	(0.0152)	(0.0123)	(0.0109)	(0.0161)	(0.0125)	(0.0153)	(0.0131)
P-val	0.2717	0.1957	0.2157	0.2336	0.3233	0.7800	0.8244	0.5446	0.5224	0.5386	0.8343	0.7383	0.1710	0.3161	0.1306	0.2380
cubic spline	pC_i20	pC_i19	pC_i18	pC_i17	pC_i16	pC_i15	pC_i14	pC_i13	pC_i12	pC_i11	pC_i10	pC_i9	pC_i8	pC_i7	pC_i6	pC_i5
T	0.0128	0.0174	0.0179	0.0181	0.0139	0.0018	-0.0060	-0.0150	-0.0209	-0.0246	-0.0179	-0.0144	0.0140	-0.0072	0.0517	0.0843
se	(0.0111)	(0.0116)	(0.0119)	(0.0123)	(0.0120)	(0.0114)	(0.0123)	(0.0136)	(0.0157)	(0.0189)	(0.0220)	(0.0252)	(0.0211)	(0.0245)	(0.0145)	(0.0146)
P-val	0.2627	0.1404	0.1391	0.1480	0.2702	0.8802	0.6357	0.2280	0.1205	0.1688	0.4795	0.6496	0.6790	0.8395	0.2206	0.2003
window width	40	38	36	34	32	30	28	26	24	22	20	18	16	14	12	10
Unweighted Obs.	24,571	23,333	22,095	20,754	19,462	18,132	16,902	15,637	14,475	3,281	11,898	10,669	9,455	8,308	7,093	5,835
Weighted Obs.	165,428.0	156,908.0	148,708.1	140,132.8	131,741.0	122,644.4	114,515.5	106,159.1	99,453.2	91,536.2	81,949.9	74,400.1	65,916.4	58,307.3	50,713.6	42,014.0

The table reports the treatment effect from estimating Eq. (3). The columns refer to different window widths. The rows refer to different specifications. Standard errors (se) clustered by month of birth shown in parenthesis. $P-val$ indicates the wild bootstrap P-value (10,000 replications and Webb weights). *** $P-val$ <0.01, ** $P-val$ <0.05, * $P-val$ <0.1.

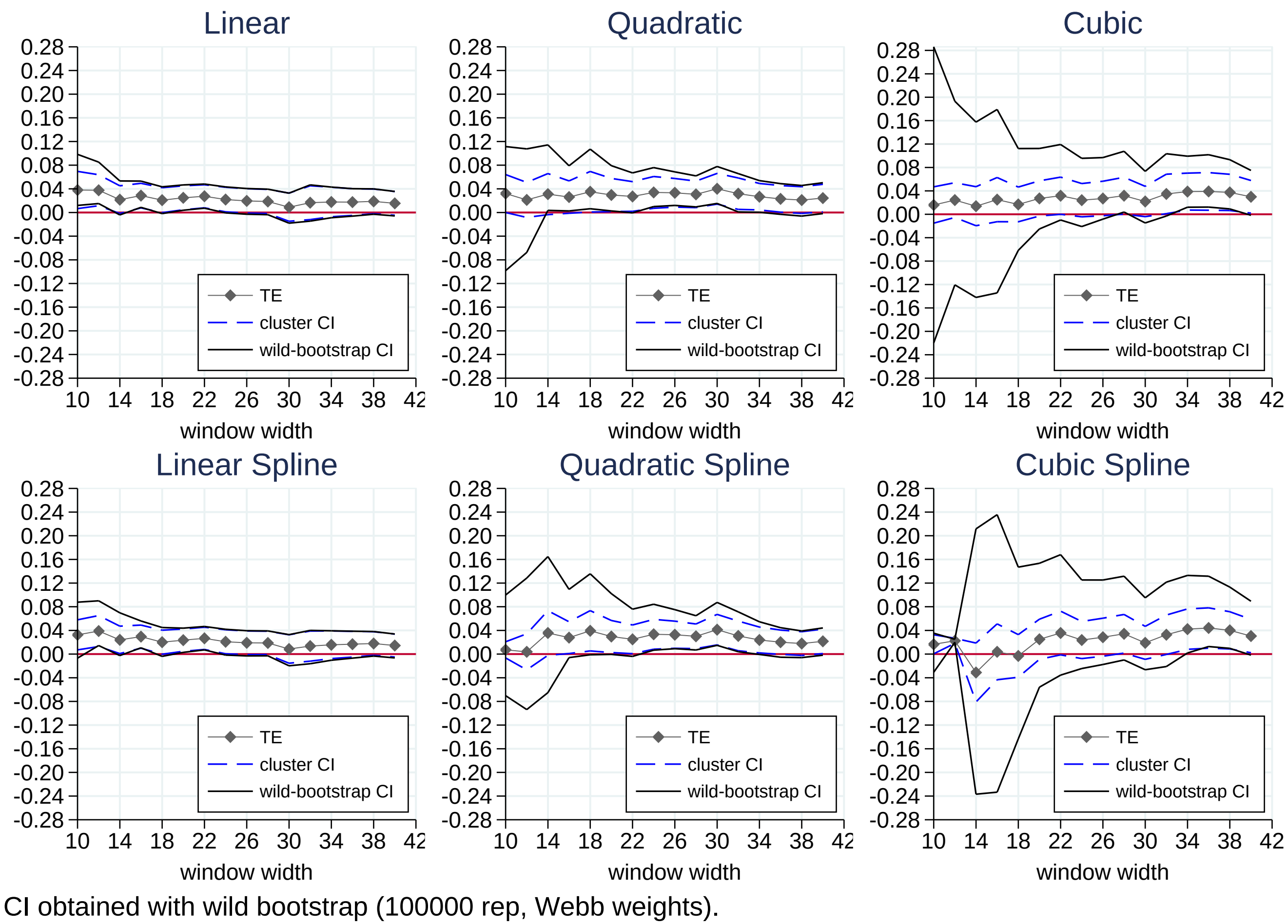
Figure 11: Pension rate - CI



CI obtained with wild bootstrap (100000 rep, Webb weights).

The graph shows the point estimate of the treatment effect by varying window width together with the corresponding confidence intervals at 95% level resulting from cluster-robust standard errors and wild bootstrap methods, for different specifications.

Figure 12: Employment rate - CI



The graph shows the point estimate of the treatment effect by varying window width together with the corresponding confidence intervals at 95% level resulting from cluster-robust standard errors and wild bootstrap methods, for different specifications.

E Falsification test

This section provides the results of a number of falsification tests of the RDD (Lee and Lemieux, 2010). The aim of these tests is to validate the identification strategy of the RDD and hence strengthen the internal validity of the results. As suggested by Lee and Lemieux (2010), we test for the presence of other unexpected discontinuities in the regression function at points other than the cutoff point. To do this, we assume that policy affected different birth cohorts and shift the cutoff accordingly along the running variable. Then we run the analysis for each outcome of interest and show the treatment effect is not significant away from the true cutoff. In detail, we follow the procedure below:

- Consider window width of 20 bins, 10 months in each side of the true cutoff corresponding to $Z_i - c = 0$ in Eq. (3).
- Start from the left-had side: set the cutoff at $Z_i - c = -10$ and consider the window width ranging from $Z_i - c = -20$ and $Z_i - c = -1$ and finally estimate Eq. (3) where $D_i = 1$ if $Z_i - c \geqq -10$ and the corresponding running variable is normalised at -10.
- Shift the cutoff to $Z_i - c = -9$ with window width ranging from $Z_i - c = -19$ and $Z_i - c = 0$; estimate Eq. (3) where $D_i = 1$ if $Z_i - c \geqq -9$ and the corresponding running variable is normalised at -9.
- Shift the cutoff by one month of birth until reaching the other extreme, where the cutoff is set at $Z_i - c = 10$ and the window width ranges from $Z_i - c = 0$ and $Z_i - c = 19$; estimate Eq. (3) where $D_i = 1$ if $Z_i - c \geqq 10$ and the corresponding running variable is normalised at 10.

Results are reported in tables 16 to 19 below, for each outcome separately. Overall, the treatment effects are not significant (or if they are, the significance is not robust across all specifications). This strengthens the internal validity of the RDD. Note that, in this exercise the standard errors are clustered by month of birth (20 clusters). Clustered standard errors are likely to be under-estimated due to the small number of clusters. Hence, correct inference should be based on P-values resulting from wild-bootstrap methods (not reported yet). However, since the problem of clustering with few clusters leads to the over-rejection of the null hypothesis and the tables broadly suggest no systematic jumps in the regression line away from the cutoff, the falsification tests can be considered successful.

Table 16: Falsification test - Pension rate in 2006

(1) Window width	(2) Treatment	(3) Linear	(4) Linear Spline	(5) Quadratic	(6) Quadratic spline	(7) Cubic	(8) Cubic spline	(9) Unw. Obs
bdate=[-20,-1]	$bdate \geqq -10$	-0.0240	-0.0230	-0.0241	0.0021	-0.0087	0.0642*	38836
		(0.0301)	(0.0299)	(0.0292)	(0.0439)	(0.0381)	(0.0348)	
bdate=[-19,0]	$bdate \geqq -9$	0.0345	0.0273	0.0353	-0.0399	-0.0671	0.0538	39364
		(0.0561)	(0.0500)	(0.0534)	(0.0424)	(0.0524)	(0.0548)	
bdate=[-18,1]	$bdate \geqq -8$	0.1112	0.0963*	0.1110**	-0.0029	-0.0240	0.0518	39988
		(0.0658)	(0.0526)	(0.0507)	(0.0496)	(0.0657)	(0.0448)	
bdate=[-17,2]	$bdate \geqq -7$	0.1224*	0.0982*	0.1171**	0.0128	-0.0073	-0.0620	40480
		(0.0612)	(0.0477)	(0.0469)	(0.0444)	(0.0546)	(0.0620)	
bdate=[-16,3]	$bdate \geqq -6$	0.1081	0.0769	0.0962**	0.0532	0.0097	-0.1117**	41324
		(0.0646)	(0.0462)	(0.0434)	(0.0571)	(0.0489)	(0.0431)	
bdate=[-15,4]	$bdate \geqq -5$	0.1208	0.0960**	0.1133***	0.0947*	0.0340	0.0523	42496
		(0.0734)	(0.0356)	(0.0378)	(0.0492)	(0.0353)	(0.0388)	
bdate=[-14,5]	$bdate \geqq -4$	0.0535	0.0235	0.0437	0.0569	-0.0049	-0.0177	43844
		(0.0943)	(0.0493)	(0.0583)	(0.0528)	(0.0512)	(0.0509)	
bdate=[-13,6]	$bdate \geqq -3$	-0.0380	-0.0643	-0.0453	0.0108	-0.0592	-0.0232	44936
		(0.1056)	(0.0701)	(0.0841)	(0.0695)	(0.0664)	(0.0608)	
bdate=[-12,7]	$bdate \geqq -2$	-0.0909	-0.1080	-0.0946	-0.0199	-0.0191	0.1392	45928
		(0.1153)	(0.0886)	(0.1019)	(0.0771)	(0.0880)	(0.0831)	
bdate=[-11,8]	$bdate \geqq -1$	-0.1915	-0.2069**	-0.1958*	-0.1121	-0.0511	-0.0278	47056
		(0.1170)	(0.0953)	(0.1079)	(0.1161)	(0.1513)	(0.0781)	
bdate=[-9,10]	$bdate \geqq 1$	-0.1905*	-0.1815	-0.1893	-0.0554	0.0482	0.1445	48744
		(0.1073)	(0.1107)	(0.1096)	(0.1463)	(0.2103)	(0.1271)	
bdate=[-8,11]	$bdate \geqq 2$	-0.0332	-0.0093	-0.0248	0.2433***	0.1906***	0.4311***	49792
		(0.1158)	(0.1011)	(0.1047)	(0.0617)	(0.0634)	(0.0803)	
bdate=[-7,12]	$bdate \geqq 3$	0.0140	0.0406	0.0206	0.2300*	0.1130	0.1414	50844
		(0.0980)	(0.0785)	(0.0793)	(0.1182)	(0.0781)	(0.1768)	
bdate=[-6,13]	$bdate \geqq 4$	0.0201	0.0414	0.0230	0.1006	0.0357	-0.2052**	52136
		(0.0848)	(0.0658)	(0.0631)	(0.1032)	(0.0697)	(0.0936)	
bdate=[-5,14]	$bdate \geqq 5$	0.0854	0.1026*	0.0847*	0.0785	0.0671	-0.1356	53372
		(0.0726)	(0.0506)	(0.0470)	(0.0760)	(0.0573)	(0.1159)	
bdate=[-4,15]	$bdate \geqq 6$	0.1033	0.1226**	0.1058**	0.0492	0.0801	-0.0574	54940
		(0.0600)	(0.0454)	(0.0373)	(0.0735)	(0.0601)	(0.1021)	
bdate=[-3,16]	$bdate \geqq 7$	0.0313	0.0547	0.0360	-0.1362**	-0.0549	-0.2157**	56132
		(0.0633)	(0.0571)	(0.0466)	(0.0633)	(0.0672)	(0.1013)	
bdate=[-2,17]	$bdate \geqq 8$	0.0414	0.0631	0.0488	-0.1295	-0.0596	0.0954	57540
		(0.0614)	(0.0559)	(0.0468)	(0.0924)	(0.0617)	(0.0785)	
bdate=[-1,18]	$bdate \geqq 9$	0.0105	0.0208	0.0105	-0.1198	-0.0811*	0.1583	58396
		(0.0635)	(0.0601)	(0.0513)	(0.0713)	(0.0465)	(0.0926)	
bdate=[0,19]	$bdate \geqq 10$	-0.0483	-0.0436	-0.0473	0.0134	-0.0108	-0.0103	59448
		(0.0327)	(0.0284)	(0.0300)	(0.0346)	(0.0422)	(0.0513)	

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered by month of birth shown in parenthesis. The table reports the treatment effect from estimating Eq. (3) by shifting the cutoff according to column (2). In all estimations, the window width is set to 20 months, i.e. 10 months to each side of the ad-hoc cutoff defined in Col (2), as reported in Column (1).

Table 17: Falsification test - Employment rate in 2006

(1) Window width	(2) Treatment	(3) Linear	(4) Linear Spline	(5) Quadratic	(6) Quadratic spline	(7) Cubic	(8) Cubic spline	(9) Unw. Obs
bdate=[-20,-1]	$bdate \geqq -10$	0.0018	0.0016	0.0018	-0.0139	-0.0083	-0.0522*	38836
		(0.0144)	(0.0143)	(0.0143)	(0.0195)	(0.0161)	(0.0270)	
bdate=[-19,0]	$bdate \geqq -9$	-0.0011	-0.0007	-0.0011	-0.0268	-0.0155	-0.0374*	39364
		(0.0151)	(0.0146)	(0.0150)	(0.0201)	(0.0163)	(0.0207)	
bdate=[-18,1]	$bdate \geqq -8$	0.0132	0.0140	0.0132	0.0168	0.0182	0.0722***	39988
		(0.0158)	(0.0154)	(0.0153)	(0.0267)	(0.0188)	(0.0158)	
bdate=[-17,2]	$bdate \geqq -7$	-0.0053	-0.0045	-0.0050	0.0074	0.0086	0.0332	40480
		(0.0171)	(0.0172)	(0.0172)	(0.0263)	(0.0244)	(0.0390)	
bdate=[-16,3]	$bdate \geqq -6$	-0.0008	-0.0021	-0.0012	0.0318*	0.0355*	0.0346*	41324
		(0.0178)	(0.0164)	(0.0171)	(0.0172)	(0.0189)	(0.0178)	
bdate=[-15,4]	$bdate \geqq -5$	-0.0204	-0.0218	-0.0210	-0.0326	-0.0070	-0.0742***	42496
		(0.0145)	(0.0140)	(0.0135)	(0.0241)	(0.0236)	(0.0162)	
bdate=[-14,5]	$bdate \geqq -4$	-0.0135	-0.0126	-0.0132	-0.0381*	-0.0185	-0.0246	43844
		(0.0126)	(0.0130)	(0.0125)	(0.0215)	(0.0167)	(0.0256)	
bdate=[-13,6]	$bdate \geqq -3$	-0.0126	-0.0105	-0.0119	-0.0057	-0.0144	0.0043	44936
		(0.0140)	(0.0127)	(0.0128)	(0.0164)	(0.0147)	(0.0209)	
bdate=[-12,7]	$bdate \geqq -2$	0.0061	0.0066	0.0060	0.0151	0.0049	0.0254	45928
		(0.0139)	(0.0135)	(0.0139)	(0.0112)	(0.0111)	(0.0207)	
bdate=[-11,8]	$bdate \geqq -1$	0.0164	0.0173	0.0166	0.0166	0.0101	0.0345	47056
		(0.0114)	(0.0105)	(0.0110)	(0.0136)	(0.0130)	(0.0257)	
bdate=[-9,10]	$bdate \geqq 1$	0.0212	0.0183	0.0199*	0.0240	0.0098	-0.0195	48744
		(0.0131)	(0.0107)	(0.0112)	(0.0176)	(0.0205)	(0.0141)	
bdate=[-8,11]	$bdate \geqq 2$	0.0071	0.0055	0.0067	-0.0177	-0.0135	-0.0457**	49792
		(0.0158)	(0.0146)	(0.0153)	(0.0152)	(0.0103)	(0.0197)	
bdate=[-7,12]	$bdate \geqq 3$	-0.0155	-0.0170	-0.0158	-0.0482***	-0.0231**	-0.0757***	50844
		(0.0136)	(0.0131)	(0.0132)	(0.0107)	(0.0109)	(0.0183)	
bdate=[-6,13]	$bdate \geqq 4$	-0.0083	-0.0084	-0.0082	-0.0231	0.0006	0.0280*	52136
		(0.0167)	(0.0168)	(0.0166)	(0.0292)	(0.0209)	(0.0138)	
bdate=[-5,14]	$bdate \geqq 5$	-0.0004	-0.0007	-0.0004	0.0407**	0.0284	0.0935***	53372
		(0.0178)	(0.0178)	(0.0178)	(0.0183)	(0.0166)	(0.0145)	
bdate=[-4,15]	$bdate \geqq 6$	0.0030	0.0008	0.0027	0.0145	-0.0067	0.0455	54940
		(0.0215)	(0.0187)	(0.0197)	(0.0309)	(0.0302)	(0.0314)	
bdate=[-3,16]	$bdate \geqq 7$	-0.0234	-0.0255	-0.0239	-0.0372	-0.0439**	-0.0841***	56132
		(0.0165)	(0.0154)	(0.0157)	(0.0223)	(0.0159)	(0.0196)	
bdate=[-2,17]	$bdate \geqq 8$	-0.0091	-0.0096	-0.0094	0.0029	-0.0037	-0.0132	57540
		(0.0156)	(0.0166)	(0.0160)	(0.0246)	(0.0243)	(0.0380)	
bdate=[-1,18]	$bdate \geqq 9$	-0.0052	-0.0054	-0.0052	-0.0072	-0.0097	-0.0302	58396
		(0.0138)	(0.0142)	(0.0140)	(0.0176)	(0.0205)	(0.0215)	
bdate=[0,19]	$bdate \geqq 10$	0.0082	0.0093	0.0085	0.0170	0.0151	0.0254	59448
		(0.0137)	(0.0132)	(0.0132)	(0.0165)	(0.0164)	(0.0248)	

*** p <0.01, ** p <0.05, * p <0.1. Standard errors clustered by month of birth shown in parenthesis. The table reports the treatment effect from estimating Eq. (3) by shifting the cutoff according to column (2). In all estimations, the window width is set to 20 months, i.e. 10 months to each side of the ad-hoc cutoff defined in Col (2), as reported in Column (1).

Table 18: Falsification test - Rate of early withdrawals from the LM

(1) Window width	(2) Treatment	(3) Linear	(4) Linear Spline	(5) Quadratic	(6) Quadratic spline	(7) Cubic	(8) Cubic spline	(9) Unw. Obs
bdate=[-20,-1]	bdate¿=-10	-0.0158	-0.0162	-0.0158	-0.0039	-0.0117	0.0490**	9709
		(0.0136)	(0.0135)	(0.0136)	(0.0155)	(0.0146)	(0.0197)	
bdate=[-19,0]	bdate¿=-9	-0.0221	-0.0225	-0.0221	-0.0021	-0.0136	0.0024	9841
		(0.0145)	(0.0142)	(0.0148)	(0.0215)	(0.0181)	(0.0218)	
bdate=[-18,1]	bdate¿=-8	-0.0233	-0.0227	-0.0233	-0.0093	-0.0166	-0.0512***	9997
		(0.0155)	(0.0143)	(0.0154)	(0.0200)	(0.0233)	(0.0127)	
bdate=[-17,2]	bdate¿=-7	0.0072	0.0093	0.0074	0.0466**	0.0335	0.0266	10120
		(0.0188)	(0.0177)	(0.0179)	(0.0209)	(0.0237)	(0.0338)	
bdate=[-16,3]	bdate¿=-6	0.0024	0.0072	0.0042	0.0014	0.0004	-0.0326	10331
		(0.0200)	(0.0175)	(0.0176)	(0.0272)	(0.0250)	(0.0255)	
bdate=[-15,4]	bdate¿=-5	0.0243	0.0282*	0.0257*	0.0319*	0.0227	0.0520**	10624
		(0.0153)	(0.0135)	(0.0125)	(0.0170)	(0.0156)	(0.0193)	
bdate=[-14,5]	bdate¿=-4	0.0156	0.0174	0.0164	0.0172	0.0196	-0.0033	10961
		(0.0133)	(0.0145)	(0.0138)	(0.0197)	(0.0173)	(0.0219)	
bdate=[-13,6]	bdate¿=-3	0.0070	0.0079	0.0075	-0.0309*	0.0007	-0.0379*	11234
		(0.0140)	(0.0152)	(0.0145)	(0.0164)	(0.0154)	(0.0197)	
bdate=[-12,7]	bdate¿=-2	-0.0098	-0.0091	-0.0095	-0.0409***	-0.0164*	-0.0489**	11482
		(0.0108)	(0.0110)	(0.0106)	(0.0128)	(0.0091)	(0.0194)	
bdate=[-11,8]	bdate¿=-1	-0.0053	-0.0038	-0.0047	-0.0050	-0.0076	0.0151	11764
		(0.0115)	(0.0109)	(0.0107)	(0.0205)	(0.0103)	(0.0235)	
bdate=[-9,10]	bdate¿=1	-0.0009	0.0006	-0.0002	0.0143	0.0255**	0.0369*	12186
		(0.0084)	(0.0083)	(0.0081)	(0.0107)	(0.0110)	(0.0210)	
bdate=[-8,11]	bdate¿=2	-0.0031	-0.0021	-0.0027	0.0241*	0.0115	0.0030	12448
		(0.0089)	(0.0092)	(0.0089)	(0.0116)	(0.0086)	(0.0159)	
bdate=[-7,12]	bdate¿=3	0.0047	0.0058	0.0050	0.0017	0.0072	0.0012	12711
		(0.0093)	(0.0093)	(0.0092)	(0.0116)	(0.0099)	(0.0130)	
bdate=[-6,13]	bdate¿=4	0.0030	0.0022	0.0028	-0.0123	-0.0108	-0.0381***	13034
		(0.0119)	(0.0109)	(0.0111)	(0.0174)	(0.0130)	(0.0120)	
bdate=[-5,14]	bdate¿=5	0.0033	0.0028	0.0033	-0.0416***	-0.0200	-0.0479***	13343
		(0.0130)	(0.0126)	(0.0132)	(0.0093)	(0.0118)	(0.0130)	
bdate=[-4,15]	bdate¿=6	0.0062	0.0068	0.0063	-0.0020	0.0202	0.0089	13735
		(0.0159)	(0.0145)	(0.0152)	(0.0253)	(0.0279)	(0.0096)	
bdate=[-3,16]	bdate¿=7	0.0236**	0.0230*	0.0235**	0.0444***	0.0489***	0.0807***	14033
		(0.0110)	(0.0112)	(0.0110)	(0.0138)	(0.0144)	(0.0101)	
bdate=[-2,17]	bdate¿=8	0.0118	0.0101	0.0114	0.0332	0.0339*	0.0153	14385
		(0.0129)	(0.0120)	(0.0122)	(0.0199)	(0.0188)	(0.0230)	
bdate=[-1,18]	bdate¿=9	-0.0009	-0.0018	-0.0009	0.0045	0.0096	-0.0182	14599
		(0.0145)	(0.0132)	(0.0140)	(0.0141)	(0.0205)	(0.0128)	
bdate=[0,19]	bdate¿=10	-0.0117	-0.0134	-0.0120	-0.0272**	-0.0204	-0.0500**	14862
		(0.0142)	(0.0120)	(0.0128)	(0.0113)	(0.0143)	(0.0205)	

*** p <0.01, ** p <0.05, * p <0.1. Standard errors clustered by month of birth shown in parenthesis. The table reports the treatment effect from estimating Eq. (3) by shifting the cutoff according to column (2). In all estimations, the window width is set to 20 months, i.e. 10 months to each side of the ad-hoc cutoff defined in Col (2), as reported in Column (1).

Table 19: Falsification test - Rate of withdrawals from the LM

(1) Window width	(2) Treatment	(3) Linear	(4) Linear Spline	(5) Quadratic	(6) Quadratic spline	(7) Cubic	(8) Cubic spline	(9) Unw. Obs
bdate=[-20,-1]	$bdate \geqq -10$	0.0380***	0.0387***	0.0379***	0.0281***	0.0338***	0.0057	9709
		(0.0097)	(0.0094)	(0.0096)	(0.0088)	(0.0102)	(0.0071)	
bdate=[-19,0]	$bdate \geqq -9$	0.0297**	0.0296**	0.0297**	0.0041	0.0181	-0.0164	9841
		(0.0135)	(0.0134)	(0.0138)	(0.0145)	(0.0176)	(0.0131)	
bdate=[-18,1]	$bdate \geqq -8$	0.0197	0.0185	0.0197	0.0005	0.0119	0.0089	9997
		(0.0159)	(0.0135)	(0.0152)	(0.0152)	(0.0240)	(0.0139)	
bdate=[-17,2]	$bdate \geqq -7$	-0.0073	-0.0105	-0.0079	-0.0598***	-0.0421**	-0.0518***	10120
		(0.0183)	(0.0149)	(0.0157)	(0.0120)	(0.0181)	(0.0152)	
bdate=[-16,3]	$bdate \geqq -6$	-0.0036	-0.0088	-0.0055	-0.0200	-0.0181	0.0291	10331
		(0.0182)	(0.0154)	(0.0152)	(0.0287)	(0.0247)	(0.0209)	
bdate=[-15,4]	$bdate \geqq -5$	-0.0195	-0.0239*	-0.0210*	-0.0293**	-0.0261*	-0.0097	10624
		(0.0142)	(0.0121)	(0.0112)	(0.0135)	(0.0135)	(0.0185)	
bdate=[-14,5]	$bdate \geqq -4$	-0.0184	-0.0220	-0.0199	-0.0062	-0.0149	0.0113	10961
		(0.0126)	(0.0132)	(0.0122)	(0.0159)	(0.0147)	(0.0179)	
bdate=[-13,6]	$bdate \geqq -3$	-0.0042	-0.0065	-0.0051	0.0446***	0.0062	0.0593***	11234
		(0.0128)	(0.0143)	(0.0131)	(0.0114)	(0.0136)	(0.0167)	
bdate=[-12,7]	$bdate \geqq -2$	-0.0032	-0.0045	-0.0037	0.0343	0.0061	0.0124	11482
		(0.0118)	(0.0116)	(0.0113)	(0.0199)	(0.0095)	(0.0183)	
bdate=[-11,8]	$bdate \geqq -1$	-0.0042	-0.0059	-0.0049	0.0005	-0.0026	-0.0382**	11764
		(0.0116)	(0.0104)	(0.0101)	(0.0173)	(0.0094)	(0.0142)	
bdate=[-9,10]	$bdate \geqq 1$	0.0017	0.0011	0.0014	-0.0094	-0.0077	-0.0122	12186
		(0.0090)	(0.0087)	(0.0089)	(0.0130)	(0.0132)	(0.0198)	
bdate=[-8,11]	$bdate \geqq 2$	-0.0066	-0.0064	-0.0064	-0.0325***	-0.0176**	-0.0207	12448
		(0.0086)	(0.0090)	(0.0088)	(0.0097)	(0.0078)	(0.0134)	
bdate=[-7,12]	$bdate \geqq 3$	-0.0038	-0.0034	-0.0036	0.0131	-0.0003	0.0202	12711
		(0.0103)	(0.0105)	(0.0102)	(0.0157)	(0.0123)	(0.0154)	
bdate=[-6,13]	$bdate \geqq 4$	-0.0042	-0.0026	-0.0039	0.0155	0.0094	0.0276*	13034
		(0.0115)	(0.0100)	(0.0101)	(0.0145)	(0.0106)	(0.0156)	
bdate=[-5,14]	$bdate \geqq 5$	-0.0002	0.0006	-0.0003	0.0335***	0.0136	0.0345**	13343
		(0.0111)	(0.0110)	(0.0112)	(0.0079)	(0.0100)	(0.0149)	
bdate=[-4,15]	$bdate \geqq 6$	-0.0041	-0.0036	-0.0040	0.0042	-0.0140	-0.0133	13735
		(0.0122)	(0.0124)	(0.0124)	(0.0200)	(0.0220)	(0.0105)	
bdate=[-3,16]	$bdate \geqq 7$	-0.0145	-0.0128	-0.0142	-0.0377**	-0.0369**	-0.0545***	14033
		(0.0105)	(0.0106)	(0.0101)	(0.0149)	(0.0172)	(0.0112)	
bdate=[-2,17]	$bdate \geqq 8$	-0.0148	-0.0125	-0.0142	-0.0407***	-0.0385***	-0.0347**	14385
		(0.0110)	(0.0088)	(0.0093)	(0.0128)	(0.0118)	(0.0151)	
bdate=[-1,18]	$bdate \geqq 9$	0.0034	0.0045	0.0034	-0.0001	-0.0058	0.0238	14599
		(0.0138)	(0.0119)	(0.0128)	(0.0153)	(0.0193)	(0.0157)	
bdate=[0,19]	$bdate \geqq 10$	0.0141	0.0154	0.0143	0.0199*	0.0138	0.0424**	14862
		(0.0124)	(0.0107)	(0.0116)	(0.0104)	(0.0142)	(0.0190)	

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors clustered by month of birth shown in parenthesis. The table reports the treatment effect from estimating Eq. (3) by shifting the cutoff according to column (2). In all estimations, the window width is set to 20 months, i.e. 10 months to each side of the ad-hoc cutoff defined in Col (2), as reported in Column (1).

F Specification tests

The functional form to depict the relationship between the running variable and the outcome of interest in Eq. (3) can be chosen according to the following test, as suggested by Lee and Lemieux (2010). The idea is as follows: (i) start from the simplest specification and estimate Eq. (3) including $K-2$ bin dummies (two dummies must be omitted because of collinearity with the constant and with the treatment dummy); (ii) test the null hypothesis that all bin dummies are zero; (iii) add polynomial terms until the null hypothesis cannot longer be rejected. Rejection of the null hypothesis suggests that the polynomial function does not capture all variation in the data. In our case, since standard errors are clustered by month of birth, this test cannot be performed by adding one-month bin dummies since the model is fully saturated once they are included in the regression. By contrast we can perform the test by adding two-months bin dummies.

With the maximum window width of 40 months, the test requires adding 18 bins of size equal to two-months each, excluding the two extreme bins at each side of the cutoff (e.g. add $bi\_bin2-bi\_bin19$). Restricting the window width, the test follows the scheme reported in the table below.

Table 20: Specification test: number of bin dummies to add by window width

window width=40	bi_bin2-bi_bin19
window width=38	bi_bin2-bi_bin19
window width=36	bi_bin3-bi_bin18
window width=34	bi_bin3-bi_bin18
window width=32	bi_bin4-bi_bin17
window width=30	bi_bin4-bi_bin17
window width=28	bi_bin5-bi_bin16
window width=26	bi_bin5-bi_bin16
window width=24	bi_bin6-bi_bin15
window width=22	bi_bin6-bi_bin15
window width=20	bi_bin7-bi_bin14
window width=18	bi_bin7-bi_bin14
window width=16	bi_bin8-bi_bin13
window width=14	bi_bin8-bi_bin13
window width=12	bi_bin9-bi_bin12
window width=10	bi_bin9-bi_bin12

Results of the tests are reported in Table 21. We report both the conventional P-values resulting from cluster-robust standard errors and the P-values computed with wild bootstrap methods. Given the small number of clusters, the tests based on cluster-robust standard errors are likely to over-reject the null hypothesis. Over-rejection is addressed by wild bootstrap methods.[39] Based on the wild bootstrap P-values, the linear specification is sufficient to depict the data.

[39] Joint hypothesis testing is provided by the *boottest* Stata command (Roodman, 2015), using Webb weights, imposing the null hypothesis and setting the number of replications to 10,000.

Table 21: Specification tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Pension rate																
Conv. P-val	0.000	0.000	0.000	0.000	0.000	0.000	0.002	0.000	0.002	0.000	0.002	0.000	0.004	0.000	0.000	0.003
WB P-val	0.911	0.879	0.918	0.527	0.868	0.649	0.865	0.650	0.766	0.506	0.649	0.625	0.581	0.066	0.150	0.474
Employment rate																
Conv. P-val	0.000	0.000	0.000	0.000	0.000	0.000	0.001	0.003	0.131	0.019	0.032	0.000	0.145	0.000	0.040	0.005
WB P-val	0.348	0.295	0.121	0.135	0.091	0.417	0.831	0.955	0.938	0.879	0.754	0.832	0.842	0.754	0.474	0.504
Window width	40	38	36	34	32	30	28	26	24	22	20	18	16	14	12	10
Unweighted Obs.	98,284	93,332	88,380	83,016	77,848	72,528	67,608	62,548	57,900	53,124	47,592	42,676	37,820	33,232	28,372	23,340
Weighted Obs.	661,712.2	627,632.1	594,832.6	560,531.3	526,964.1	490,577.5	458,062.0	424,636.4	397,812.8	366,144.7	327,799.6	297,600.3	263,665.4	233,229.1	202,854.5	168,055.9
Rate of early withdrawals from the labour market																
Conv. P-val	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.020	0.000	0.002	0.000
WB P-val	0.305	0.402	0.167	0.397	0.113	0.637	0.108	0.294	0.058	0.222	0.130	0.218	0.604	0.434	0.127	0.239
Rate of withdrawals from the labour market																
Conv. P-val	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.011	0.000	0.215	0.000	0.007	0.002
WB P-val	0.086	0.147	0.132	0.265	0.105	0.302	0.059	0.247	0.297	0.403	0.550	0.615	0.803	0.337	0.182	0.272
Window width	40	38	36	34	32	30	28	26	24	22	20	18	16	14	12	10
Unweighted Obs.	24,571	23,333	22,095	20,754	19,462	18,132	16,902	15,637	14,475	3,281	11,898	10,669	9,455	8,308	7,093	5,835
Weighted Obs.	165,428.0	156,908.0	148,708.1	140,132.8	131,741.0	122,644.4	114,515.5	106,159.1	99,453.2	91,536.2	81,949.9	74,400.1	65,916.4	58,307.3	50,713.6	42,014.0

The table reports the P-value of the joint test described above for different window widths. Standard errors are clustered by month of birth.
$Conv.P-val$ denotes the conventional P-value resulting from cluster-robust standard errors.
$WBP-val$ reports the P-value computed according to wild bootstrap methods.
The tests on pension rate and employment rate are estimated by pooling all quarters of 2006.
The tests on the rate of early withdrawals from the labour market and on the rate of withdrawals from the labour market are estimated in 2011q4.