Couples' Retirement under Individual Pension Design: a Regression Discontinuity Study for France

Elena Stancanelli[*†]

Abstract

Retirement policies are individually designed, but the majority of older workers are partnered, and are likely to coordinate their employment decisions with their spouse. The goal of this study is to estimate the direct and indirect (via the spouse) effects of a pioneer French pension reform on both spouses' retirement decision. The extent of the reform varies by birth year, which enables us to identify its retirement effects on both spouses, since the husband is, on average, two years older than the wife. We use labor-force survey data to implement a sharp regression-discontinuity framework, in which the running variable is the distance of the individual birth month to a certain reference month, as well as an incremental differences-in-differences approach. We find a significant drop in each spouse's probability of retirement. The husband's retirement probability also drops immediately by 2 percentage points if the wife is affected by the reform, while her retirement probability does not respond immediately if he is affected.

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[*] Paris Jourdan School of Economics, CNRS, and IZA. Adress: 48 Boulevard Jourdan, 75014 Paris.
E-mail: elena.stancanelli.fr@gmail.com

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1. Introduction

Prolonging individual working lives to counter population ageing and public pension deficits is high on the agenda of all OECD governments. Earlier work has established, theoretically and empirically, that married couples have incentives to retire from work at close to the same time, often within a year of each other (Michael Hurd, 1990; David Blau, 1998; Alan Gustman and Thomas Steinmeier, 2000). It follows that economic policies targeted at older individuals may also affect their spouse's employment behavior (Michael Baker, 2002; Courtney Coile, 2004; Kanika Kapur and Jeannette Rogowski, 2007; James Banks, Richard Blundell, Maria Casanova, 2010; Rafael Lalive and Stefan Staubli, 2014). This issue is important for policy purposes, especially when most OECD countries are changing their pension schemes. This paper contributes to the literature by studying the effects of a pioneer pension reform in France on both spouses' retirement decisions.

The reform we analyze here became law in July 1993, and took effect from January 1994, thus leaving older workers little time to adjust their plans in anticipation of the reform. Individuals born after December 1933 were required to contribute additional months to the social security fund (i.e. to work more months) to be able to retire with maximum pension benefits. The reform was stronger for younger cohorts of workers, with those born in 1934 having to contribute three more months, those born in 1935, six more months, and so on, up to 30 additional months for individuals born as late as 1943. Thus, thanks to the average age difference between spouses, which is about two years, it is possible to identify the own and cross (or indirect, via the spouse) effects of the reform on spouses' retirement decisions.[1]

[1] Earlier literature studied the effects of the reform at the individual level (Antoine Bozio, 2008), taking an incremental differences-in-differences approach, to find quite small effects. Jean-Olivier Hairault, Francois Langot and Thepthida Sopraseuth (2010) modelled the employment effect of the distance in time to legal retirement age in France, in a theoretical job-search framework, to conclude that increasing the legal retirement age is likely to increase the employment rates of older workers. Luc Behagel, Didier Blanchet and Muriel Roger (2014) provide a comprehensive picture of individual retirement patterns in France. Beatrice Sedillot (2002) gathers descriptive evidence of interactions in spouses' retirement decisions in France.

In particular, we can identify the effects of the reform on spouses' retirement decisions, as there are no spousal pension benefits in France, which may create significant interactions in spouses' employment decisions (Michael Baker, 2002; Marcus Dillender, 2016; Mauro Mastrogiacomo, Rob Alessie, and Maarten Lindeboom, 2004), thus complicating the analysis of the effects of pension reform. Another element of our empirical design is that about 80% of French retirees only claim a public (first pillar) pension (Lans Bovenberg, 2011). Moreover, health insurance, which can also affect older spouses' employment decisions (Donna Gilleskie and David Blau, 2006; Kanika Kapur and Jeannette Rogowski, 2007), is universally public and entitlement does not vary with (old) age in France. Finally, retirement is an almost total condition in France, in that our sample shows that less than 1 percent of men and only about 0.3 percent of women who report being retired from work also report positive hours of work. Therefore, the context is such that the identification of the effects of the reform on spouses' retirement decisions is quite straightforward and there are a priori no other confounding factors.

Female participation rates in France have traditionally been very high, which is another feature of our study, as we can estimate the effects of the reform on both spouses, while considering a sample which is highly representative of the population of older couples in France –in contrast to studies in other continental European countries, in which fewer older women were employed such as, for example, the Netherlands (Mauro Mastrogiacomo, Rob Alessie, and Maarten Lindeboom, 2004; Hans Bloemen, Stefan Hochguertel and Jochem Zweerink, 2015). Indeed, the labour-force participation rates of French women and men aged 45 to 54 have been slightly above those of their American counterparts (Francine Blau and Lawrence Kahn, 2013) since, respectively, the 1990s and the 2000s (see Figure 1).

We allow the reform to affect each spouse both directly and indirectly (via the spouse). We take a regression discontinuity approach, using as the running variable the distance in months between the relevant policy threshold (e.g. 1934) and the individual birth month. We also estimate the employment effect of each policy threshold separately (by taking a twelve-month bandwidth) in the year in which individuals were aged 60 (the early pension eligibility age in France) or across a decade of post-reform years pooled together. Next, we estimate an incremental differences-in-differences model, in which individuals born in 1933 serve as the control group and 1993 is the control year. We distinguish the employment effect of the reform in the first year of implementation (in 1994) or, alternatively, pool across a decade of post-reform years.

All the specifications lead to qualitatively similar conclusions. The reform induced both spouses to retire later. In particular, the reform significantly and dramatically reduced each spouse's probability of retirement in the year of their 60th birthday, by between 10 and 24 percentage points. Pooling over all thresholds, and considering a decade of policy years, the average effect is equal to a drop of between 2 and 4 percentage points in the own retirement probability. We also conclude that the husband's retirement probability immediately drops by about 2 percentage points when the wife is affected by the reform, while her own retirement probability drops, in the longer run, by about 8 percentage points when the husband is affected by the reform.

The structure of this paper is as follows. Section 2 provides a brief overview of the economic literature, Section 3 describes the institutional background, and our empirical approach is presented in Section 4. Section 5 presents the data, and the graphical analysis and the results of our estimations are discussed in Section 6. Our conclusions are detailed in Section 7.

2. Spouses' interactions in retirement decisions

In a standard set-up, the household utility function can be seen as a weighted average of the utility of each spouse, with the weights representing the bargaining power of each individual (Olivier Donni, 2008). So-called "distribution" factors, such as changes in public policies, may affect the weight of each spouse and thus, their say in the household decision process. Spouses maximize the household utility function subject to a budget constraint that depends on each spouse's (expected) labor and non-labor income, including future pension income. Each spouse retires if the household expected utility in retirement is larger than the household expected utility if they do not retire.

A scant literature has concluded that spouses take "joint-retirement" decisions and retire close in time, often within a year of each other. In particular, leisure complementarities are generally considered as among the main drivers of joint retirement strategies of spouses. Within this literature, most studies to date relate to the United States (Michael Hurd, 1990; David Blau, 1998; Alan Gustman and Thomas Steinmeier, 2000; Tammy Schirle, 2008; Michaud, Pierre-Carl and Frederic Vermeulen, 2011). The joint labor supply decisions of older couples have also been documented for Denmark (Mark An, Bent Jesper Christensen and Nabanita Datta Gupta, 2004), Australia (Mavromaras, Kostas, and Rong Zhu, 2015), Canada (Tammy Schirle, 2008), and the UK (Maria Casanova, 2010).

Recent work highlights asymmetries in spouses' retirement strategies. Robert A Pollak (2013) argues that spouses may have conflicting interests over the timing of retirement because of age differences and gender differentials in life expectancy, as well as the design of social security regimes. Age differences between spouses may not be exogenous to household decision-making, as they may capture 'marriage mismatch' (Pierre-Andre Chiappori, Sonia Oreffice and Climent Quintana Domeque, 2012), and the older spouse may, for example, compensate by working longer hours in the market (Hans Bloemen and Elena Stancanelli, 2015). Gustman and Steinmeier (2009) incorporate partial retirement strategies in a discrete choice model of spouses' retirement, to conclude that individual members of a couple may decide to retire only if their spouse does not retire. Using data drawn from the Health and Retirement Study (HRS), these authors find that the increased labour force participation of American women has actually contributed to lower husbands' hours of market work. Using data on actual leisure time spent together by French spouses, Elena Stancanelli and Arthur van Soest (2016) conclude that partnered retirees spend only 20 more minutes per day in leisure activities performed together with their spouse than do partnered workers.

Other North-American studies have exploited changes in social security design or health insurance policy to estimate older spouses' interactions in employment decisions, with contrasting findings on the direction of these interactions. Courtney Coile (2004) finds that both spouses have similar participation responses to own financial and social security incentives, although while the husband reacts to the wife's (cross) incentives, the opposite is not true. Donna Gilleskie and David Blau (2006) propose a dynamic model of couples' retirement decisions accounting for health insurance, to find differential responses of husbands and wives. Kanika Kapur and Jeannette Rogowski (2007), investigating the effect of employer-provided retiree health insurance on the retirement behaviour of dual-earners in the USA, find evidence of asymmetric effects for partners: the wife's health insurance increases joint retirement, while the husband's does not. David Blau (1998) concludes that eliminating dual entitlement to social security benefits has a significantly positive effect on the labour supply of married women and a negative one on husbands' labour supply, though both effects are small. Michael Baker (2002) finds somewhat more symmetric responses of partners, finding a negative effect of a new allowance for dependent spouses on the participation rates of eligible Canadian women and their husbands. Mauro Mastrogiacomo, Rob Alessie, and Maarten Lindeboom (2004) also find unintended perverse effects, in the Netherlands, of old-age pensions on the employment decisions of married couples, which are especially tangible for means-tested public pension benefits.

More recently, a growing literature (most of which is still in progress) focuses on pension reforms in Europe to estimate the joint-retirement patterns of married couples (Francois Gerard and Lena Nekby, 2012; Rafael Lalive and Stefan Staubli, 2014; Jonathan Cribb, Carl Emmerson and Gemma Tetlow, 2014; Bloemen, Hans, Stefan Hochguertel and Jochem Zweerink, 2015), producing new evidence on patterns of spouses' retirement decisions across Europe. This current paper fits in well with this branch of the literature.

3. Institutional Background

The French pension system is essentially a public pension system with defined benefit pension payments that depend on individual past earnings and work history. According to recent estimates, about 79 per cent of French retirees claim only a public (first pillar) pension, while 6 per cent also receive an occupational (employer-provided) pension and 18 per cent a private pension. The corresponding American figures are, respectively, 45, 13 and 42 per cent (Lans Bovenberg, 2011).

The age of early eligibility to a public retirement pension is 60 in France, and we do see a large increase (equal to about 0.30) in both spouses' retirement probability at age 60 (the top panel in Figure A in the Appendix).[2] Indeed, age 60 is the 'effective' retirement age in France, according to recent OECD estimates (OECD, 2014). Specific sectoral agreements

[2] In 2010, this age threshold was raised from 60 to 62, but it does not take effect until 2018. The age 60 threshold thus still currently applies. Focusing on the legal retirement age, Figure 2 traces out the retirement probability of spouses as a function of own and spousal age. We estimate (by means of a local polynomial method, applying a triangular kernel distribution and an optimal bandwidth of 48 months) the jump in the retirement probability of each spouse at the legal retirement age of 60 (the top panel of Figure 2); the legal retirement age of 65 (the bottom panel in Figure 3); and at 55 (the top panel in Figure 3), the age at which people typically enter sector-specific early-retirement schemes.

enable some workers to retire as of age 55, but these apply to only a minority of workers, and there is no change in the retirement probability at age 55 for either the husband or the wife (top panel of Figure B in the Appendix). By age 65, retirement becomes mandatory[3], although the majority of French workers retire long before 65. The probability of retirement does not increase at age 65 for the husband, and the increase in the odds of retirement at age 65 for the wife is much smaller than that at age 60 (bottom panel in Figure B).

The public pension payments are calculated using the following formula:

1) Retirement benefit$_i$= $\frac{\sum_{n=1}^{Z} W_{ni}}{Z}$ * S * Q_i/Q^m

Where W are the annual earnings of individual *i* that are averaged over the past Z years, with Z varying by employment sector, and (since the 1993 reform) by year of birth; S is the replacement rate of pension benefits to earnings, which varies by sector of employment (for example, it is equal to 50% in the private sector and 75% in the public sector); and Q_i is the social security contribution period of the individual *i* (for example, 40 years) while Q^m is the "optimal" pension contribution period for individual *i*, which depends on the sector of employment and (since the 1993 reform) the birth-year. In particular, the ratio Q_i/Q^m cannot exceed one, which implies that there is no financial incentive to continue to work past the optimal contribution period. The statutory retirement age must also be considered though, since individuals cannot retire before the minimum pension eligibility age of 60, regardless of the length of their social security contribution period, Qi. We have seen that most French workers retire at age 60 (Figure A, Appendix), due to the fact that they entered the labor market early (as compulsory education age was 14 for individuals born before 1953 and 16 for those born thereafter) but also retirement benefits do not increase if individuals continue

[3] The 2010 reform also raised this age 65 threshold with effect from 2018.

to work, once they have worked long enough to receive the maximum pension benefits (Q^m).[4] Periods of inactivity (maternity leave, sickness, and unemployment) are all fully insured with pension rights, as long as they are linked to a previous spell of gainful employment (for example, the unemployment spell of a new entrant to the labour market does not include pension rights, but that following a job loss does).

The 1993 retirement reform - which was enacted in the summer of 1993 and came into force in January 1994 - increased the length of the contribution period required to retire with maximum pension benefits (Q^m) for workers born after December 1933. The number of extra contribution months varied according to the distance between the birth date and the "cutoff" point of being born in 1934. While those born in 1934 needed an extra three months of work history, those born in 1935 needed six more months, and those born in 1943 or later needed 30 extra months. In line with the formula above, not contributing these additional months would entail an income penalty, as workers retiring earlier would not be able to receive the maximum level of pension benefits. The 1993 reform also increased the reference period for the earnings that would enter the calculation of pension benefits ("n" in the formula above), which was increased, in the private sector, from the last 10 years of earnings to the last 25 years. By lengthening the reference period, less weight is given to the highest earnings. Therefore, the reform provided incentives for individuals to retire later, although individuals were not compensated with more generous pension benefits for doing so.

For example, a private sector worker born in 1943, with average yearly earnings of €14,000 (approximately the minimum wage) over the past 25 years of earnings, and with 41.25 years (Q_i) of work experience (which is exactly the optimal contribution period, Q^m) would receive a pension benefit of €7,200 per year (14 000 * 0.50 *(165/165)). If retired with only 39 years

[4] This has now been changed and continuing to work (past the Q^m threshold) leads to slightly more generous pension benefits.

(152 quarters) of work experience, that same worker would receive a pension benefit of €6,448 per year (14 000 * 0.50 *(152/165)). This pension benefit (adjusted for inflation) would be received every month from retirement until death. Thus, the drop in retirement benefits for individuals who retire with less than Q^m periods of social security contributions is generally quite sizable.

4. The data

Our data comes from the French Labour Force Survey (LFS). Social security data in France is only available at the individual level to date, and do not allow us to identify or link same-household members. The census data is cross-sectional and only collected every ten years. The LFS considered in this study was collected yearly, with a third of each year sample being re-interviewed for three consecutive years (three-year rotating panel). The interviews were carried out in person at the respondent's home and the response rate was almost 90%. In 2003, the LFS series was broken and made quarterly, but this coincided with another pension reform, and so we only consider the LFS from 1993 to 2002 (and 1992, which serves for a placebo test). We construct a sample of couples as follows:

- Individuals are matched to their partner, if any, and singles are dropped from the sample.[5]
- Multi-couple households are dropped.
- Records from different survey years are then pooled together.

Cohabitation was rare at the time of the survey, but we include both cohabiting and formally-married partners, denoting them, for the sake of simplicity, "husband" and "wife" throughout this paper. To give an order of magnitude for the overall sample, pooling all the 1990 to 2002

[5] In this survey, it is not possible to distinguish same-sex couples from singles sharing housing, as same sex individuals are automatically coded as singles.

surveys, the data includes 148,395 couples, in which spouses were aged between 50 and 70. Table 1 provides descriptive statistics for this large sample, to show that, in fact, the average age difference between spouses is about 2 years. The sample size for the empirical analysis varies according to the empirical specification chosen.

Attrition does not seem to be a major problem, as it only concerns 5% of the sample. Some of this attrition could be associated with the couple changing their address on (joint) retirement (as the survey does not follow households that move), but the McCrary test performs well, suggesting that this is not a systematic problem (Elena Stancanelli, 2012).

The LFS collects the month and year of birth of the respondent, together with the day, month and year of the interview. We can therefore construct a measure of the distance in months from being born in January 1934 (or January 1935, and so forth). The retirement status is subjectively assessed by the individual and measured on the interview date. In particular, the individual reports whether his/her main economic status was employment, unemployment, full-time education, military service, retirement, being a housewife, or other inactive. Retirement is an absorbing state in France: less than 1 percent of men and only about 0.3 percent of women who report being retired from work also report positive hours of work in our sample.

There is no information in the survey on the age at which individuals retire. Thus, our measure of retirement is a stock measure of retirement. Our focus is the impact of the reform on the individual probability to have retired from work, for workers of a given age (and not how the reform affected the age at which individual retired). This is a drawback of our analysis. On the other hand, administrative pension and retirement data for France are only available at the individual level to date.

Pension income is not collected in the survey either. Other explanatory variables that we control for in some of the empirical analysis include completed years of education, the (lagged) local unemployment rate, and the number of children.

5. The method

Our aim is to estimate the effect of the 1993 retirement reform on spouses' employment decisions. Because spouses are likely to take joint retirement decisions, the policy reform is likely to affect each partnered individual, directly and indirectly (via their spouse). The reform we consider here affected individuals born after December 1933, who were required to work longer to be able to retire with maximum pension benefits (i.e. Q^m in the retirement benefit formula, Equation 1 of Section 3, was increased). Specifically, those born in 1934 had to work 3 more months, those born in 1935, 6 more months, and so on, with those born in 1943 having to work 30 more months. Therefore, the intensity of the "treatment" (or the policy change) varied by birth cohort, while at the same time being clearly proportional to the distance from the first policy threshold (being born in 1934). In particular, workers born in 1934 would be 60 precisely in 1994. The reform is likely to have reduced individual incentives to retire at the early pension eligibility age (i.e. at age 60). In our sample, the husband is, on average, 2 years older than the wife and we therefore expect to be able to identify the direct and indirect effects of the reform on each spouse.

We implement a Regression Discontinuity Design (Guido Imbens and Thomas Lemieux, 2007; Wilbert Van der Klaauw, 2008; David Lee and Thomas Lemieux, 2010),[6] in which the cut-off is being born in 1934 (pooling over the first few thresholds) and the running variable is the number of months elapsed between the cutoff and the individual birth month. As individuals born within a few months of each other, on the opposite sides of this cut-off, are

[6] Erich Battistin, Agar Brugiavini, Enrico Rettore and Guglielmo Weber (2009) were the first to apply an RD design to study the effect of the household head's retirement on household consumption.

likely to be very similar, a regression discontinuity is very close to an experimental design. There are no other policies that specifically affected individuals born after December 1933. Regarding anticipation (David Lee and Thomas Lemieux, 2010), while birthdate certainly cannot be manipulated, individuals do know their birthday and could therefore behave differently in anticipation of the reform, which would invalidate the natural experiment design. However, since the policy was proposed and enacted only 6 months before its introduction, this seems unlikely. We use the McCrary approach (Justin McCrary, 2008) to test for the continuity of the running variable (see the graphs in Elena Stancanelli, 2012).

Let R denote the outcome variable which is here the probability of retirement. The treatment is given by the 1993 policy reform, which affected individuals born after December 1933 (labeled by the subscript 1, while 0 denotes those born just before December 1933). Our goal is to estimate the average impact of the treatment on individual j's retirement decision:

2) $\gamma = E[R_{j1} - R_{j0}]$

We assume that any difference in the outcome across individuals born before or after December 1933 is due to the reform, knowing that we may only observe the outcome R for the same individual j, either before or after the treatment. Assuming the continuity of E[R] on either side of the cut-off, and defining the running variable, M, as the difference in months between the individual birth month and the 1st January 1934, the RD estimator γ_{RD} can be rewritten as:

3) $\gamma_{RD} = \lim^{-}_{M\to 0} E[R_{j1} \mid M_j=0] - \lim^{+}_{M\to 0} E[R_{j0} \mid M_j=0]$

which can be approximated (Hahn, Jinyong; Petra Todd; Wilbert Van der Klaauw, 2001; Imbens, Guido and Thomas Lemieux, 2007) by taking the difference of the mean outcomes of the respondents born in the months close to (before or after) the treatment (here, the cutoff point of being born in January 1934). Assuming a linear regression model for the outcome, we can also write:

$$4) \quad R_j = \gamma_{RD}\ T_j + \lambda\ M_j\ T_j + \beta\ M_j\ (1\text{-}T_j) + u_j$$

where T is the treatment (which equals 1 for individuals born after December 1933 and zero for those born before), M_j is the difference in months between the individual birth month and January 1934 (i.e. a linear function), which is interacted with the treatment dummy T to allow for different effects on either side of the cutoff. This model provides us with an estimate of the Intent-to-Treat (ITT) impact of the reform on the retirement decision of spouses. Moreover, in some of the RDD specifications, we also allow for multiple discontinuities (Matias Cattaneo, Luke Keel, Rocio Titunik and Gonzalo Vazquez-Bare, 2015), specifying a discontinuity for the husband being born after December 1933 and an additional discontinuity for the wife being born after December 1933. Thus, we explicitly allow individuals to be treated twice, when they are hit by the reform (the direct effect) and also when their spouse is hit (the indirect effect). Within this framework, we allow for the same bandwidth in both spouses' birth dates (and select couples in which both spouses were born within 48 months of January 1934), as follows:

$$5) \quad R_j = \gamma_{RD}\ T_j + \lambda\ M_j\ T_j + \beta\ M_j\ (1\text{-}T_j) + \gamma_{RDs}\ T_{js} + \lambda_s\ M_{js}\ T_{js} + \beta_s\ M_{js}\ (1\text{-}T_{js}) + u_j$$

Where j denotes the own variables and parameters, while the subscript "s" denotes those of the spouse.

We apply the procedure as in Sebastian Calonico, Matias D. Cattaneo and Rocio Titiunik (2014) to determine the optimal bandwidth, which produces an optimal bandwidth of 48 months for the RDD impact of the reform on the wife's retirement probability, and a slightly different figure (36 or 41) for the other RDD specifications. We choose to present results using the same bandwidth for all the RDD specifications. The results are, in general, robust to using different bandwidths. We also apply a 12-month bandwidth to estimate the separate impact of each policy threshold, i.e. being born in 1935, 1936, and so on, until 1943, distinguishing the effect of the reform in the year in which each spouse was aged 60 (e.g. 1995 for those born in 1935); or pooling over different policy years. When pooling the data over different survey years, we cluster the errors at the individual level,[7] to account for the rotating sample structure (households are re-interviewed for three consecutive years). In addition, to control for individual unobserved characteristics, we also estimate a variant of these models that allows for individual random effects. We estimate γ_{RD} using a fully non-parametric approach (specifying local polynomials with a triangular kernel, as in Austin Nichols, 2014), as well as linear regression models. We use the same bandwidth for both models.

As an alternative empirical strategy, we estimate a differences-in-differences model of each spouse's (j) retirement probability (R), taking individuals born in 1933 and observed in 1993, as the control group, as follows:

6) $R_{jt} = \alpha + \sum_{k=1}^{10} \beta_k \text{birthyear}_{kj} * \text{year} + \varrho Z_{j+\xi} \text{year} + \mu_j$

where birthyear is a dummy equal to zero if spouse j was born in 1933 and to 1 if j was born, respectively, in 1934 (corresponding to the index k=1), 1935 (k=2), 1936 (k=3), …and 1943 (k=10); while year stands for the policy year/s,[8] with year 1993 serving as a counterfactual.[9] The μ_j are the individual random disturbances that are assumed to be distributed normally.

[7] We do not cluster the standard errors at the level of the running variable, in line with the procedure implemented to calculate the optimal bandwidth (see Ottavio Bartalotti and Quentin Brumet, 2017, for a detailed discussion of this econometric issue).

[8] In some specifications, we only consider the first policy year, 1994, while in others we pool the data over all policy years from 1994 to 2002 (as the LFS series was broken in 2003).

[9] Equation 6 can also be written as $R_{jt} = \alpha + \beta_1\, b34_j * \text{year} + \beta_2\, b35_j * \text{year} + \beta_3\, b36_j * \text{year} + \beta_4\, b37_j * \text{year} + \beta_5\, b38_j * \text{year} + \beta_6\, b39_j * \text{year} + \beta_7\, b40_j * \text{year} + \beta_8\, b41_j * \text{year} + \beta_9\, b42_j * \text{year} + \beta_{10}\, b43_j * \text{year} + \kappa Z_{j+\xi} \text{year} + \mu_j$.

The standard errors are corrected using robust standard errors. When pooling the data over the 1993-2002 survey years, we also cluster the standard errors at the individual level. This differences-in-differences model is estimated separately for husband and wife and, alternatively, including both husband's and wife's birth-year dummies.

6. Estimation Results

Our aim is to estimate the own and cross (via the spouse) effects of the 1993 French retirement reform on spouses' retirement decisions. We expect the reform to have reduced spouses' retirement probability. Specifically, the probability of retiring at the early pension eligibility age of 60 (e.g. 1994 for the cohort born in 1934) is likely to have fallen. We first graphically inspect these effects, and then we present the results of the estimations of, respectively, RDD models and differences-in-differences models.

Graphical analysis

A number of insights into the validity of the empirical design and the effects of the treatment can be obtained by simply plotting the data (Guido Imbens and Thomas Lemieux, 2007; Wilbert Van der Klaauw, 2008; David Lee and Thomas Lemieux, 2010). Figure 2 illustrates husband's and wife's retirement probability as a function of the own (left panel in Figure 2) and the spouse's birth month (right panel in Figure 2), after the 1993 reform. We plot the raw means of the outcome variable (grouped by bins of two months) together with the kernel triangular estimates (using the same bandwidth as in the empirical model) and the 5% confidence intervals around these estimates, against the running variable (the distance in months from being born in January 1934). To understand these graphs, it is important to keep in mind that spouses born to the left of the cut-off point (the vertical line at zero, which corresponds to being born in January 1934) are older while those born to the right are younger. After the reform, the husband's and the wife's retirement probability falls significantly, as expected. We find no detectable indirect effects: the retirement probability of the husband (wife) is a smooth function of the birth date of the wife (husband).

Next, we run a "placebo" test, in which we (fictitiously) assume that generations born in 1932 and later were affected by a reform implemented in1992 (Figure 3).[10] The graphs show no significant drop in the retirement probability at the 1932 birth month (cut-off) for either the husband or the wife. The placebo test thus validates our RDD design: the effects we see in Figure 2 are not driven by a spurious combination of birth years and policy years.

RDD estimates of the direct and indirect effects of the 1993 policy reform

First, we estimate the effect of the 1993 pension reform on own and spousal retirement, applying a sharp RDD design and focusing on the first policy threshold, i.e., being born in 1934. Pooling over thresholds and policy years, the optimal bandwidth is 48 months, which we apply, respectively, to both spouses (specifications 1 and 2 of Table 2), or only to the husband (specifications 3 and 4 of Table 2), or only to the wife (specifications 5 and 6 of Table 2). For the generations born shortly before the reform parameter (from July to December 1933), the mean retirement was 0.86 for the husband and 0.53 for the wife (over 30% of the wives were housewives). The reform significantly reduced both spouses' own retirement probability by about 0.02, or roughly, two percentage points (Table 2, specifications 1 and 2). The drop in the retirement probability is larger, equal to about 0.04 (or four percentage points), when controlling for individual unobserved random effects (Table 2, specifications 1a and 2a). Therefore, the reform worked as expected, inducing spouses to retire later.[11]

[10] We drop from the estimation sample couples who answered the survey in 1994 and later years.
[11] Earlier studies of the effect of the reform, using an incremental differences-in-differences strategy, also found very small reform effects on the individual retirement probability (Antoine Bozio, 2008).

The estimates of the indirect effect of the reform on spousal retirement are negative but not statistically significant (Table 2, specifications 1 and 2), except for the effect of the wife's treatment on the husband's retirement probability, which is equal to minus 0.01 and weakly significant (at the ten per cent significance level), when controlling for individual random effects (Table 2, specification 1a).

As a robustness check, specifications 3 and 4 (3a and 4a, when allowing for individual random effects) of Table 2 present the RDD estimates, setting the bandwidth only with respect to the husband's birth month and estimating separately the own and cross (or indirect, via the spouse) effects of the reform. Similarly, specifications 5 and 6 (5a and 6a, when allowing for individual random effects) of Table 2 illustrate the RDD estimates, setting the bandwidth only with respect to the wife's birth month and estimating separately the own and the cross effects of the reform. The conclusions are qualitatively the same, although the cross-effects become statistically non-significant, which is perhaps explained by the fact that the spouse's birth month is not bounded in these specifications, and thus spouses very far from (or well past) pensionable age are included in the estimation sample, which may blur the estimates of the indirect effects. These conclusions are robust to estimating non-parametric local polynomials, or varying the bandwidth (Table A in the Appendix).

Then, for the sake of completeness, we allow each threshold to have a different effect on spouses' decisions to retire (Table 3), allowing for a 12-month bandwidth. We distinguish these effects in the year in which each cohort of spouses is 60 (first two columns of Table 3) from the effects pooling over different policy years (last two columns of Table 3). In Table 3, the own and cross effects are estimated separately, not imposing the same 12-month bandwidth for both spouses, as it would then be impossible to disentangle the own from the cross effect of the reform. First, it is striking that restricting attention to one policy threshold at a time, in the year in which the "treated" turns 60, reveals significant and large drops in the probability to retire for the treated (respectively, Specification 7 of Table 3 for the husband, and Specification 12 of Table 3 for the wife). For example, we estimate that, due to the reform, the probability to retire drops in 1994 by 0.21 for husbands born in 1934 and by almost 0.20 for wives born in 1934. The size of these negative effects varies from one policy threshold to the next, but it is, overall, statistically significant and large. This is not surprising, as we saw that the French retirement system provides strong incentives for individuals to retire at the early eligibility age (as pension benefits do not increase if individuals continue to work past Q^m periods). On the other hand, the estimates of the own retirement effect of the policy become much smaller and often lose significance when pooling over a decade of policy years (respectively, specifications 9 for the husband, and 14 for the wife, in Table 3), except for the first policy threshold estimate (and a few more thresholds for the husband). This could be explained by the fact that, under this set-up, the estimates are averaged over treated individuals of different ages, some of whom are too young to retire (if aged less than 60) and others are too old to keep working (if aged over 65).

The cross-effects are given, respectively, in specification 8 (or 10 when pooling over policy years) of Table 3, for the effect of the husband's treatment on the wife's retirement probability, and in specification 11 (or 13 when pooling over policy years) of Table 3, for the effect of the wife's treatment on the husband's retirement probability. They are almost all statistically non-significant, although all negative, as expected. This is likely due to the fact that the spouse's birth month is not bounded in these specifications and thus, some of the spouses may be very far from (or well past) retirement age - which regardless of spousal preferences for the timing of retirement may prevent them from retiring at more or less the same time.

Differences-in-differences estimates

As an alternative specification, we take an incremental differences-in-differences approach, defining spouses born in 1933 as the control group and 1993 as the control year. We consider first, couples in which the husband was born between 1933 and 1943, regardless of the wife's birth year (specifications A, B, C, and D of Table 4) and then vice-versa, couples in which the wife was born between 1933 and 1943, regardless of the husband's birth year (specifications E, F, G, and H of Table 4). In contrast, the last two columns of Table 4 (specifications I and J) only include couples in which both spouses were born between 1933 and 1943. We distinguish the effects of the reform in the first year of implementation (first two columns of Table 4) from the effects in the first decade of implementation (last four columns of Table 4). It should be stressed that the first row of estimates in each of the two blocks of Table 4 (the coefficient of the dummy for the husband, or respectively, the wife, being born after December 1933, interacted with the policy years) captures the overall effect of the reform and the remaining estimates (for example, the estimated coefficient of the dummy for whether the husband was born in 1935, interacted with the policy year dummy) tell us whether the effects are significantly different across different policy thresholds (or birth cohorts).

The estimates in Table 4 are somewhat comparable to those in Table 3 that focused on the effect of each policy threshold on spouses' retirement probabilities. We find a large and significant drop in the own retirement probability of each spouse in the first year of implementation of the policy (specifications A and F of Table 4). The own effects become statistically non-significant for the first policy thresholds (being born in 1934 or 1935 for the husband, and being born in 1934, for the wife), when pooling over a decade of policy years, but they remain significant and sizable for the other policy thresholds (specifications C, D, H, I, J of Table 4). Coming to the cross-effects, few of them appear significant in the first year of implementation of the policy. Specifically, the retirement probability of women partnered with men born between 1938 and 1943 drops by about 0.04 in 1994 (specification B of Table 4). The retirement probability of men partnered with women born in 1937 drops (weakly) by 0.047 and that of men partnered with women born in 1943 drops by 0.066 in 1994 (specification E of Table 4). However, when controlling for both spouses birth years (specifications I and J of Table 4), only the drop in retirement for women partnered with men born in 1943 remains statistically significant, and is equal to 0.039.

To better elicit couples' interactions and obtain estimates comparable to those of our (preferred) RDD specification of Table 2, we pool all the policy thresholds and policy years together, and control for individual random (or fixed) effects (see Table 5). We estimate these models for couples in which either spouse was born between 1933 and 1943 (first block of Table 5) or, alternatively, dropping couples in which spouses were born in the same year (about 15% of the couples in which either spouse was born between 1933 and 1943; second block of Table 5). Perhaps reassuringly, the estimates are not much affected from dropping the latter, and so we concentrate on discussing the first block of results. We find a significant and sizable drop in the own retirement probability of each spouse due to the reform and these effects are larger when controlling for individual unobserved effects. The OLS estimates of the drop in the husband's own retirement probability following the policy change are much smaller than the panel model estimates, while the size of the drop in the wife's own retirement probability is quite stable across the three specifications (OLS, random effects, and fixed effects) and equal to -0.22 to -0.23. This perhaps suggests that there is more heterogeneity in the husband's unobservables than in those of the wife. As far as the cross-effects go, the husband's retirement probability is not sensitive to the wife's treatment under any of the specifications, while the wife's retirement probability responds significantly to the husband's treatment under all specifications, with the estimates ranging from -0.11 (OLS) to -0.08 (RE) and -0.05 (FE).

It should be stressed that these differences-in-differences estimates gather the long-term effect of the policy, while the RDD estimates only capture the "immediate" effect of the policy. This could perhaps explain why the cross-effects are significant for the husband but not for the wife under the RDD specification, and vice-versa for the differences-in-differences specification, as perhaps the husband responds only in the very short-term to his wife's treatment, while the wife responds only in the long-term. We have seen that partnered men tend to retire as soon as they reach the early eligibility age of 60 (see Figure A in the Appendix) while many partnered women may only retire at age 65 (see Figure B in the Appendix). There is no significant change in the husband's retirement probability at age 65 (see Figure B in the Appendix) while the wife's retirement probability spikes up at age 65 (see Figure B in the Appendix), though by a lesser extent than at age 60 (see Figure A in the Appendix), which is likely due to women having more uninsured spells of inactivity than men. Thus, women may possibly only be able to adjust their retirement to their husband's retirement over a longer time horizon, while men may react more quickly (or never, as they are often older, and perhaps, already retired).

7. Summary and Conclusions

Population ageing and increasing budgetary pressure have led most OECD countries to introduce policies to extend individual working lives. Over two-thirds of individuals of retirement age live as couples and the vast majority of older couples are dual-earners. It is therefore of great importance for policy purposes to understand the retirement strategies of partnered workers. We here exploit a pioneering 1993 retirement reform that provided incentives for workers to postpone retirement, to identify the direct and indirect effects of the reform on spouses' retirement outcomes.

The reform required individuals to work for more quarters to be able to retire with the maximum level of retirement benefits. The extent of the reform was greater for younger cohorts of workers: individuals born in 1934 had to work three more months; those born in 1935, 6 more months; those born in 1936, 9 more months, and so on, with those born in 1943, having to work 30 more months to be able to retire with maximum pension benefits. Therefore, the extent of the reform was proportional to the distance from being born in 1934. Because spouses are, on average, 2 years apart in age, it is possible to identify the effect of the reform on both spouses. Earlier studies that exploited retirement reforms (often reforms of female retirement age) to identify spousal retirement effects could often only instrument the retirement of one of the two spouses, and took as exogenous the retirement decision of the other spouse. Because spouses tend to take the retirement decision together, it is important to be able to identify both spouses' decisions.

Another advantage of our study is that the institutional context in France is such that we can neatly identify the effects of the retirement reform. The earlier literature considers the employment decisions of dual-earners in North-American, British, and North-European, countries, in which private-pension schemes and spousal pension benefits are widespread. As in Central European or Mediterranean countries, around 80 per cent of French retirees receive only first pillar (public) pension benefits, which are individually designed. There are no spousal benefits (only survivor benefits). Health insurance is public and entitlement does not vary close to retirement age. The labor force participation of older women in France was very high at the time of the reform (above that of their American counterparts, for example). The reform we consider was enacted in the summer of 1993 and came into force as from January 1994, which left individuals little time to adjust in anticipation of it. Moreover, individuals were not financially compensated for having to work longer but, on the contrary, pension benefits were made slightly less generous, thus providing additional incentives to comply with the reform.

Our empirical construct is twofold. We estimate the immediate effect of the reform on spouses' retirement decisions, implementing an RDD in which the running variable is given by the distance of the individual birth month to being born in January 1934, pooling over different threshold and policy years. We also estimate models with multiple cut-offs, allowing for both spouses' birth month to affect each other's retirement probability; and separate RDD models for each policy threshold. To capture the longer horizon effects of the policy, we also estimate an incremental differences-in-differences model in which individuals born in 1933 serve as the control group and 1993 as the control year.

We use Labor Force data for France to estimate these models and exploit the rotating structure of the survey, which is such that a third of the sample is retained for three consecutive years, to also control for individual unobserved heterogeneity. Although the size of the estimates tends to vary from one specification to the other, the conclusions are qualitatively similar. The reform induced both spouses to retire later. As far as the indirect (or cross, via the spouse) effects go, the reform immediately reduced by about 2 percentage points the husband's probability of retirement if the wife was affected by the reform. The wife's retirement probability was not significantly impacted if the husband was affected by the reform. Therefore, we conclude that the reform affected the retirement decisions of spouses both directly and indirectly. Moreover, both direct and indirect effects were negative, leading individuals to postpone retirement. Neglecting spousal interactions would underestimate the total effect of the reform on retirement decisions. It follows that when designing policy reforms, spousal interactions may also have to be considered.

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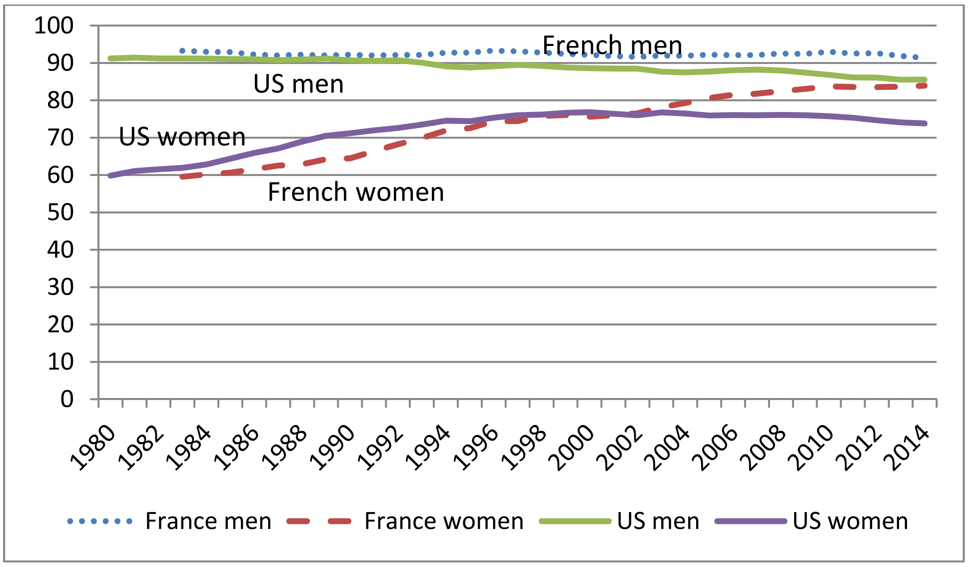
Figure 1. Labor-Force Participation Rates of Men and Women Aged 45 to 54.



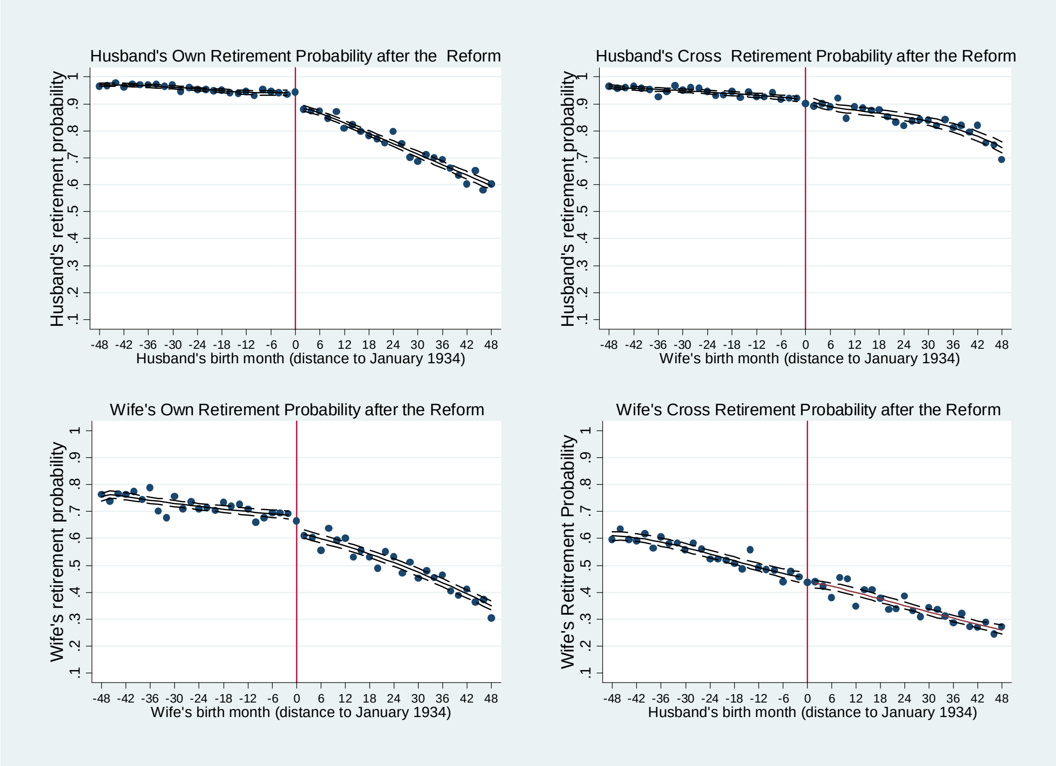
Source: OECD (2017), Employment rate (indicator). doi: 10.1787/1de68a9b-en

Table 1. Descriptive statistics: couples with spouses aged 50 to 70, LFS 1990-2002

	Husband		Wife	
	Mean	Standard dev.	Mean	St. dev.
Age	60.776	5.293	58.617	5.239
Age 60 and above	.553	.497	.403	.490
Elementary School	0.531	0.499	0.605	0.488
Middle School	0.292	0.454	0.252	0.434
High School	0.065	0.247	0.075	0.264
College	0.109	0.312	0.063	0.244
French	0.949	0.217	0.957	0.201
Retired	.598	.490	.308	.461
Employed	0.337	0.472	0.317	0.465
Other Inactive	0.063	0.244	0.373	0.483
Usual Hours	41.707	11.950	33.837	13.692
		Couple's characteristics		
		Mean	Standard dev.	
Married		0.970	0.169	
Children number		0.393	0.773	
Local Unemployment rate		9.368	2.429	
Observations	*148395*			

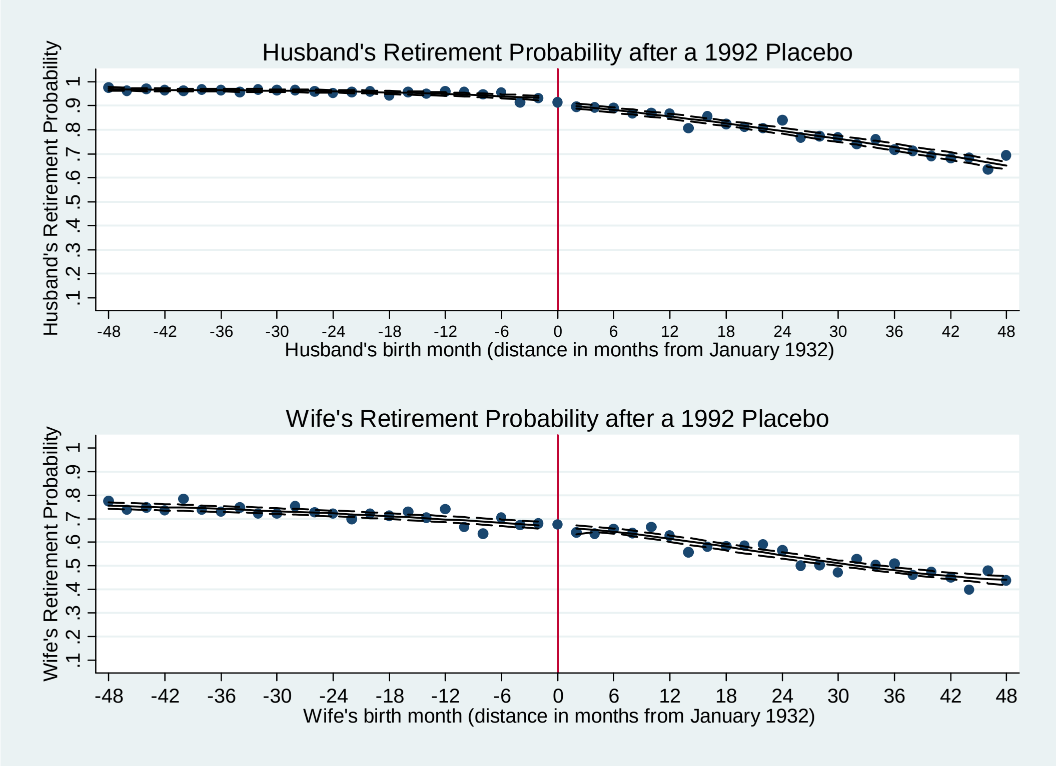
Note: The sample includes all active and inactive partners aged 50 to 70. It includes cohabiting couples. Hours are averaged over positive hours.

Figure 2. Husband's and Wife's Retirement Probability after the Retirement Reform



Note: The graphs show the retirement probability of the husband (top panel) and the wife (bottom panel) by own month of birth (left panel) and by spouse's month of birth (right panel), respectively, after the 1993 reform. The zero on the horizontal axis corresponds to being born in January 1934, as individuals born in 1934 and later were hit by the retirement reform as from 1994. The observations are grouped by bins of two months. The dots are the raw means of the outcome variable (the retirement probability), plotted against the running variable (distance in months from being born in January 1934). The solid line is the retirement probability non-parametrically fitted, using a triangular kernel with an optimal bandwidth of 48 months. The dotted lines are the 5 percent confidence bounds around the kernel estimates.

Figure 3. Placebo: Husband's and Wife's Retirement Probability after the "1992 fictitious reform"



Note: The graphs show the retirement probability of the husband (top panel) and the wife (bottom panel) by own month of birth before and after 1992. There was no reform in 1992; these graphs are a counterfactual of the 1993 reform. The zero on the horizontal axis corresponds to being born in January 1932. The observations are grouped by bins of two months. The dots are the raw means of the outcome variable (the retirement probability), plotted against the running variable (distance in months from being born in January 1932). The solid line is non-parametrically fitted using a triangular kernel with an optimal bandwidth of 48 months. The dotted lines are the 5 percent confidence bounds around the kernel estimates.

Table 2. Own and cross effects of the reform on spouses' retirement probability. Regression Discontinuity Design estimates, pooling over thresholds and years.

Outcome :	**H Retired**	**W Retired**	**H Retired**	**W Retired**	**H Retired**	**W Retired**
Mean if born 1933	*0.86*	*0.53*	*0.86*	*0.53*	*0.86*	*0.53*
Mean if spouse born 1933	*0.82*	*0.21*	*0.82*	*0.21*	*0.82*	*0.21*
Linear regressions, optimal bandwidth 48 months						
	(1)	***(2)***	***(3)***	***(4)***	***(5)***	***(6)***
Dummy H. Born ≥ 1934	**-0.018****	**0.003**	**-0.020****			**-0.008**
standard error	(0.009)	(0.016)	(0.007)			(0.012)
Dummy W. Born ≥ 1934	**-0.012**	**-0.025***		**-0.0329****	**-0.004**	
standard error	(0.008)	(0.015)		(0.012)	(0.007)	
R square	*0.10*	*0.07*	*0.117*	*0.065*	*0.036*	*0.047*
Observations	*28463*	*28463*	*51350*	*48284*	*48284*	*51350*
Linear regressions with individual random effects, optimal bandwidth 48 months						
	(1a)	**(2a)**	**(3a)**	**(4a)**	**(5a)**	**(6a)**
Dummy H. born ≥ 1934	**-0.04****	**0.004**	**-0.0449****			**-0.015**
standard error	(0.009)	(0.015)	(0.007)			(0.012)
Dummy W. born ≥ 1934	**-0.014***	**-0.039***		**-0.049****	**-0.008**	
standard error	(0.009)	(0.015)		(0.0117)	(0.007)	
R square	*0.10*	*0.07*	*0.116*	*0.065*	*0.036*	*0.047*
Observations	*28463*	*28463*	*51350*	*48284*	*48284*	*51350*

Note: The linear regression model includes linear polynomials in the distance from birth in 1934 and interaction of the dummy for being born after 1933 with this polynomial. Specifications 1), 2), 1a) and 2a) set the bandwidth at the couple's level, including in the estimation sample couples in which both spouses were born within 4 years of the cutoff. Specifications 3), 4), 3a) and 4a) set the bandwidth only considering the husband's birth month, regardless of the wife's birth month, and vice-versa, for specifications 5), 6), 5a) and 6a) that set the bandwidth only considering the wife's birth month, regardless of the husband's birth month. The standard errors are robust and are also clustered at the individual level. Standard errors appear in parentheses. In the table, ** indicates statistical significance at the 5% level and * indicates statistical significance at the 10% level.

Table 3. Own and cross effects of the reform on spouses' retirement probability.
Regression Discontinuity Design estimates for each separate policy threshold (bandwidth 12 months).

Data period:	Year when 60	Year when 60	1994-2002	1994-2002
Outcome :	H Retired	W Retired	H Retired	W Retired
	(7)	(8)	(9)	(10)
H born 1934	-0.210***	0.00857	-0.0214*	-0.0192
(obs. (7): 1547)	(0.0421)	(0.0420)	(0.0126)	(0.0260)
H born 1935	-0.167***	-0.0596	-0.0110	-0.0382
(obs. (7):1603)	(0.0452)	(0.0453)	(0.0169)	(0.0258)
H born 1936	-0.246***	-0.0168	0.0262	0.0153
(obs. (7):1463)	(0.0458)	(0.0436)	(0.0207)	(0.0249)
H born 1937	-0.0725	0.0493	0.0234	0.00195
(obs. (7):1386)	(0.0466)	(0.0445)	(0.0236)	(0.0244)
H born 1938	-0.158***	-0.00758	0.0349	0.00502
(obs. (7):1385)	(0.0463)	(0.0459)	(0.0254)	(0.0229)
H born 1939	-0.281***	-0.0265	-0.0551**	0.000371
(obs. (7):1399)	(0.0477)	(0.0482)	(0.0256)	(0.0212)
H born 1940	-0.0952**	-0.00471	-0.0414*	0.0125
(obs. (7):1394)	(0.0450)	(0.0461)	(0.0243)	(0.0186)
H born 1941	-0.179***	-0.0392	-0.0232	0.0184
(obs. (7):1233)	(0.0504)	(0.0500)	(0.0251)	(0.0184)
H born 1942	-0.139***	-0.0190	-0.0190	0.00820
(obs. (7):1305)	(0.0494)	(0.0464)	(0.0207)	(0.0159)
H born 1943	*(no comparable LFS for 2003)*		-0.0185	-0.0101
			(0.0170)	(0.0135)
Data period :	Year when 60	Year when 60	1994-2002	1994-2002
Outcome :	H Retired	W Retired	H Retired	W Retired
	(11)	(12)	(13)	(14)
W born 1934	-0.0627	-0.197***	-0.0117	-0.0493**
(obs. (12):1543)	(0.0423)	(0.0493)	(0.0146)	(0.0243)
W born 1935	0.0786*	-0.121**	0.0271	-0.000623
(obs. (12):1486)	(0.0442)	(0.0523)	(0.0177)	(0.0261)
W born 1936	-0.0625	-0.133**	-0.00350	0.00980
(obs. (12):1360)	(0.0435)	(0.0528)	(0.0193)	(0.0260)
W born 1937	-0.000572	-0.131**	-0.00386	-0.00521
(obs. (12): 1392)	(0.0426)	(0.0528)	(0.0204)	(0.0262)
W born 1938	-0.0306	-0.125**	-0.0267	-0.00438
(obs. (12):1335)	(0.0441)	(0.0551)	(0.0234)	(0.0254)
W born 1939	0.0179	-0.223***	0.00686	-0.0141
(obs. (12):1338)	(0.0432)	(0.0534)	(0.0247)	(0.0239)
W born 1940	-0.0121	-0.136**	-0.00740	-0.0403*
(obs. (12):1285)	(0.0440)	(0.0542)	(0.0258)	(0.0221)
W born 1941	-0.0269	-0.114*	-0.00664	-0.00621
(obs. (12):1162)	(0.0492)	(0.0609)	(0.0289)	(0.0209)
W born 1942	-0.00789	-0.180***	0.0144	-0.0462**
(obs. (12):1167)	(0.0495)	(0.0581)	(0.0283)	(0.0185)
W born 1943	*(no comparable LFS for 2003)*		-0.0185	-0.0101
			(0.0170)	(0.0135)

Note. The bandwidth is 12 months. The models include controls for the distance in months between the spouse's birth month and the relevant policy threshold and interactions with the policy threshold dummy.
They are estimated by OLS with robust standard errors (clustered at the individual level for the 1993-2002 models). The year when 60 denotes, for example, 1995 for those born in 1935. The sample size for specifications 7) and 12), is reported in brackets in the first column of the Table. The standard errors are robust (and are also clustered at the individual level in Specifications 9) and 10)). Standard errors appear in parentheses. In the table, ** indicates statistical significance at the 5% level and * indicates statistical significance at the 10% level.

Table 4. Incremental differences-in-differences estimates. The control group includes those born in 1933.

Data period:	1993-94	1993-94	93-02	93-02	93-02	93-02
Outcome :	H Retired	W Retired	H Retired	W Retired	H Retired	W Retired
	(A)	*(B)*	*(C)*	*(D)*	*(I)*	*(J)*
H born≥34*ypolicy	-0.328***	-0.0216	0.0143	-0.0185	0.0231	-0.0179
	(0.0268)	(0.0209)	(0.0247)	(0.0205)	(0.0282)	(0.0181)
H born 35*ypolicy	-0.0496**	-0.0113	0.000536	-0.0203	0.00279	-0.0114
	(0.0242)	(0.0178)	(0.0229)	(0.0190)	(0.0253)	(0.0174)
H born 36*ypolicy	-0.0155	-0.00150	-0.0406*	-0.0323*	-0.0586**	0.0193
	(0.0243)	(0.0182)	(0.0225)	(0.0182)	(0.0256)	(0.0169)
H born 37*ypolicy	-0.0326	-0.00958	-0.0502**	-0.0723***	-0.0588**	-0.00469
	(0.0237)	(0.0178)	(0.0221)	(0.0182)	(0.0255)	(0.0174)
H born 38*ypolicy	-0.00292	-0.0447***	-0.0377*	-0.131***	-0.0318	0.00110
	(0.0231)	(0.0172)	(0.0202)	(0.0180)	(0.0236)	(0.0174)
H born 39*ypolicy	-0.0799***	-0.0481***	-0.139***	-0.155***	-0.130***	-0.00436
	(0.0203)	(0.0157)	(0.0197)	(0.0167)	(0.0238)	(0.0173)
H born 40*ypolicy	-0.109***	-0.0285*	-0.220***	-0.188***	-0.212***	-0.0191
	(0.0199)	(0.0164)	(0.0195)	(0.0166)	(0.0246)	(0.0188)
H born 41*ypolicy	-0.0972***	-0.0318**	-0.310***	-0.200***	-0.296***	0.0111
	(0.0195)	(0.0156)	(0.0190)	(0.0159)	(0.0252)	(0.0182)
H born 42*ypolicy	-0.115***	-0.0433***	-0.399***	-0.231***	-0.386***	0.0100
	(0.0191)	(0.0153)	(0.0189)	(0.0160)	(0.0253)	(0.0210)
H born 43*ypolicy	-0.0948***	-0.0442***	-0.414***	-0.243***	-0.409***	0.0399**
	(0.0187)	(0.0150)	(0.0180)	(0.0154)	(0.0251)	(0.0195)
Observations	*16490*	*16,490*	*77,840*	*77,840*		
Rsquared	*0.274*	*0.062*	*0.493*	*0.203*		
Data period:	1993-94	1993-94	93-02	93-02		
Outcome :	H Retired	W Retired	H Retired	W Retired		
	(E)	*(F)*	*(G)*	*(H)*		
W born≥ 34*ypol.	-0.0148	-0.201***	0.0590***	0.00533	0.00555	0.0336
	(0.0226)	(0.0256)	(0.0227)	(0.0216)	(0.0405)	(0.0359)
W born 35*ypolicy	0.0239	-0.0487**	0.0494**	-0.0579***	0.00283	-0.0876***
	(0.0240)	(0.0206)	(0.0244)	(0.0197)	(0.0338)	(0.0273)
W born 36*ypolicy	0.00697	-0.0554***	0.0519**	-0.111***	-0.00814	-0.129***
	(0.0249)	(0.0199)	(0.0244)	(0.0190)	(0.0327)	(0.0253)
W born 37*ypolicy	0.0476*	-0.0565***	0.130***	-0.168***	0.0203	-0.208***
	(0.0249)	(0.0194)	(0.0248)	(0.0185)	(0.0316)	(0.0245)
W born 38*ypolicy	-0.00327	-0.0541***	0.121***	-0.188***	0.0281	-0.223***
	(0.0237)	(0.0183)	(0.0236)	(0.0169)	(0.0302)	(0.0227)
W born 39*ypolicy	0.00278	-0.0811***	0.138***	-0.226***	0.0356	-0.263***
	(0.0236)	(0.0170)	(0.0239)	(0.0167)	(0.0302)	(0.0223)
W born 40*ypolicy	0.00547	-0.0842***	0.123***	-0.293***	0.0185	-0.323***
	(0.0241)	(0.0169)	(0.0236)	(0.0163)	(0.0301)	(0.0221)
W born 41*ypolicy	-0.0351	-0.0871***	0.0831***	-0.340***	0.0102	-0.383***
	(0.0225)	(0.0165)	(0.0234)	(0.0163)	(0.0301)	(0.0225)
W born 42*ypolicy	-0.0357	-0.0883***	0.0414*	-0.388***	0.00617	-0.425***
	(0.0219)	(0.0159)	(0.0221)	(0.0156)	(0.0297)	(0.0221)
W born 43*ypolicy	-0.0660***	-0.0886***	-0.0235	-0.417***	-0.0129	-0.461***
	(0.0203)	(0.0159)	(0.0214)	(0.0153)	(0.0297)	(0.0222)
Observations	*15,899*	*15,899*	*74,158*	*74,158*	*48,335*	*48,335*
Rsquared	*0.212*	*0.147*	*0.293*	*0.327*	*0.475*	*0.334*

Note. The table shows selected estimates (standard errors in brackets). All the regressions include controls for the lagged local unemployment rate (a year before), dummies for individual completed education level, French nationality, number of children, birth-year dummies and survey-year fixed effects. The models are estimated by OLS with robust standard errors, clustered at the individual level.
** indicates statistical significance at the 5% level and * indicates statistical significance at the 10% level.

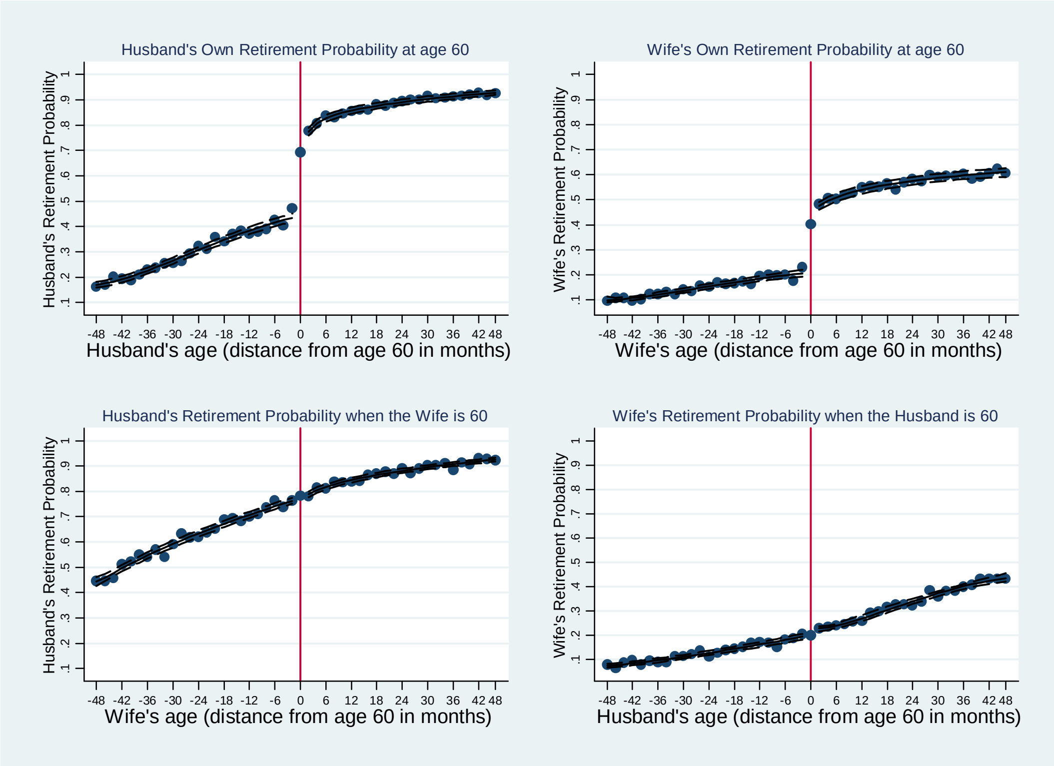
Table 5. Differences-in-differences estimates, pooling over all birth-year thresholds and survey years. The control group includes those born in 1933 and control year is 1993.

Data period:	1993-2002	1993-2002	1993-2002	1993-2002	1993-2002	1993-2002
Outcome variable:	H Retired	W Retired	H Retired	W Retired	H Retired	W Retired
	OLS	OLS	Random Effects	Random Effects	Fixed effects	Fixed effects
Sample: both spouses born between 1933 and 1943						
H Born >=1934 * year>=1994	-0.0860***	-0.116***	-0.265***	-0.0758***	-0.374***	-0.0480***
	(0.0224)	(0.0140)	(0.0227)	(0.0117)	(0.0268)	(0.0151)
W Born >=1934 * year>=1994	-0.0546	-0.224***	-0.0236	-0.229***	-0.00525	-0.234***
	(0.0340)	(0.0305)	(0.0326)	(0.0314)	(0.0405)	(0.0412)
Observations	*48,335*	*48,335*	48,335	48,335	48,335	48,335
(panel observations)			*(21,189)*	*(21,189)*	*(21,189)*	*(21,189)*
R squared	*0.316*	*0.226*	*0.121*	*0.079*	*0.124*	*0.082*
Sample: both spouses born between 1933 and 1943: Couple's age difference >12 months						
H Born >=1934 * year>=1994	-0.0845***	-0.157***	-0.268***	-0.0867***	-0.379***	-0.0395***
	(0.0234)	(0.0148)	(0.0235)	(0.0122)	(0.0281)	(0.0149)
W Born >=1934 * year>=1994	-0.00513	-0.150***	0.0400	-0.232***	0.0846	-0.296***
	(0.0520)	(0.0452)	(0.0555)	(0.0461)	(0.0687)	(0.0598)
Observations	*40,776*	*40,776*	*40,776*	*40,776*	*40,776*	*40,776*
(panel observations)			*(17,883)*	*(17,883)*	*(17,883)*	*(17,883)*
R squared	*0.183*	*0.098*	*0.042*	*0.012*	*0.048*	*0.019*

Note. The table shows selected estimates (standard errors in brackets). All the regressions include controls for the lagged local unemployment rate (a year before), dummies for individual completed education level, French nationality, number of children, birth-year dummies and survey-year fixed effects. The models are estimated by OLS with robust standard errors, clustered at the individual level. ** indicates statistical significance at the 5% level and * indicates statistical significance at the 10% level.

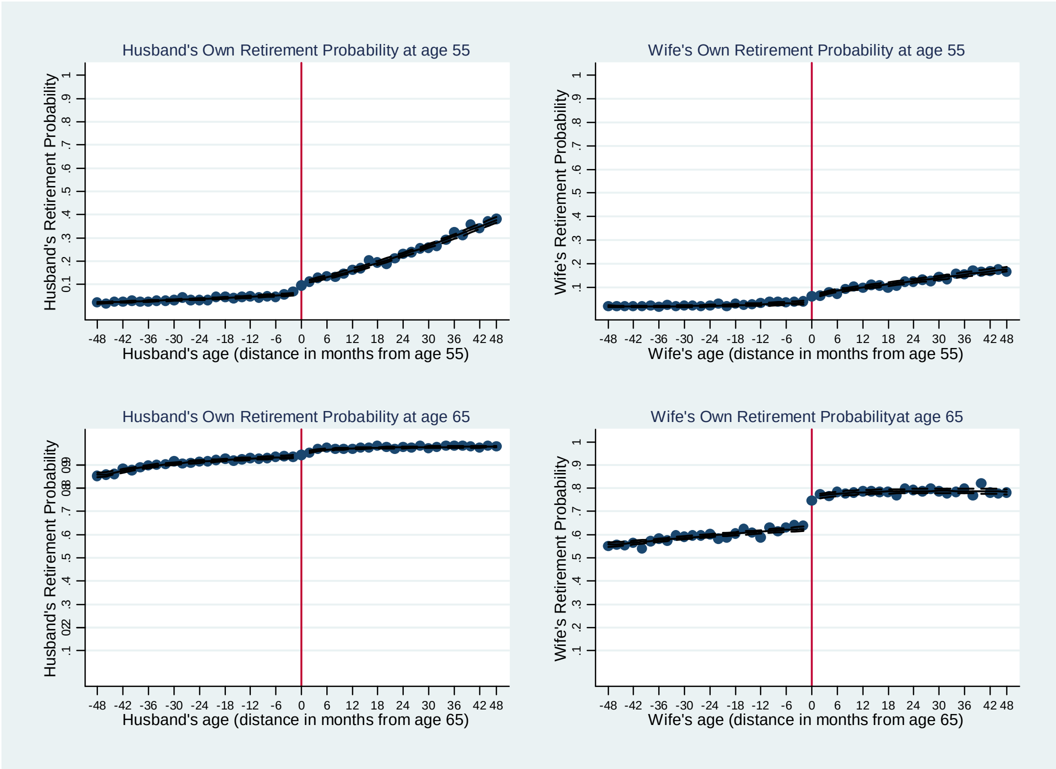
Appendix

Figure A. Husband's and wife's own and cross retirement probability at age 60.



Note: The graphs show the retirement probability of the husband (top panel) and the wife (bottom panel) by own and spouse's age. The zero on the horizontal axis corresponds to being aged 60, which is the minimum pension eligibility age for most workers in France. The observations are grouped by bins of two months. The dots are the raw means of the retirement probability, which is plotted against the distance in months from being aged 60. Assuming a Fuzzy RDD framework with cut-off at age 60, the solid line is the retirement probability non-parametrically fitted, using a triangular kernel with an optimal bandwidth of 48 months, while the dotted lines are the 5 percent confidence bounds around these estimates (see also Elena Stancanelli, 2012).

Figure B. Husband's and wife's own retirement probability at age 55 or 65.



Note: The graphs show the retirement probability of the husband (left panel) and the wife (right panel) by own age. The zero on the horizontal axis corresponds, respectively, to the age of 55 (top panel) or to the age of 65 (bottom panel). While 60 is the early eligibility age for most workers in France, 55 is the typical age at which special early-retirement programs may apply and 65 is the age by which most workers are obliged by law to retire, if they have not yet done so. The observations are grouped by bins of two months. The dots are the raw means of the retirement probability, which is plotted against the distance in months from being aged, respectively, 55 or 65. Assuming a Fuzzy RDD framework with cut-off at, respectively, age 55 or 65; the solid line is the retirement probability non-parametrically fitted, using a triangular kernel with an optimal bandwidth of 48 months, while the dotted lines are the 5 percent confidence bounds around these estimates (see also Elena Stancanelli, 2012).

Table A. Robustness checks: non-parametric estimates and different bandwidths. Regression Discontinuity Design estimates, pooling over thresholds and years.

Outcome	**H Retired**	**W Retired**	**H Retired**	**W Retired**
Mean if born 1933	*0.86*	*0.53*	*0.86*	*0.53*
Mean if spouse born 1933	*0.82*	*0.21*	*0.82*	*0.21*
Local Polynomial model, bandwidth 48 months				
	(3b)	***(4b)***	***(5b)***	***(6b)***
Dummy H. Born ≥ 1934	**-0.0197****			**-0.0048**
standard error	(0.007)			(0.014)
Dummy W. Born ≥ 1934		**-0.039****	**-0.0010**	
standard error		(0.013)	(0.008)	
Local Polynomial model, bandwidth 24 months				
	(3c)	***(4c)***	***(5c)***	***(6c)***
Dummy H. born ≥ 1934	**-0.0169****			**-0.007**
standard error	(0.009)			(0.0198)
Dummy W. born ≥ 1934		**-0.036****	**-0.014**	
standard error		(0.0188)	(0.011)	
Local Polynomial model, bandwidth 96 months				
	(3d)	***(4d)***	***(5d)***	***(6d)***
Dummy H. born ≥ 1934	**-0.0145****			**-0.0118**
standard error	(0.005)			(0.0098)
Dummy W. born ≥ 1934		**-0.0436****	**0.009**	
standard error		(0.009)	(0.006)	

Note: The local linear polynomials are estimated using a non-parametric triangular kernel. Specifications 3b), 4b), 3c), 4c), 3d) and 4d) set the bandwidth only considering the husband's birth month, regardless of the wife's birth month, and vice-versa, for specifications 5b), 6b), 5c), 6c), 5d) and 6d) that set the bandwidth only considering the wife's birth month, regardless of the husband's birth month. The standard errors are robust and are also clustered at the individual level. Standard errors appear in parentheses. In the table, ** indicates statistical significance at the 5% level and * indicates statistical significance at the 10% level.