Joint Retirement of Couples: Evidence from Discontinuities in Denmark*

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Abstract

We study joint retirement and its underlying determinants. First, we use full-population data from Denmark and a discontinuity design to document joint retirement at the early pension eligibility age. For every 100 individuals who retire when they reach pension eligibility, around 8 of their spouses adjust their behavior to retire at the same time. Next, we investigate mechanisms. We show that our estimates are explained primarily by leisure complementarities, and not other channels. We then explore pathways couples take to retire together and find that age differences are a fundamental determinant of joint retirement, which is driven by older spouses waiting to retire and claim pension benefits until their younger partners reach pension eligibility as well. Accounting for age differences and relative earnings reveals a gender gap; females are more likely to adjust their retirement to coincide with their partner's retirement. Finally, we show that a reform increasing eligibility ages induced similar joint retirement spillovers.

Keywords: Joint Retirement, Pension Eligibility Age, Couples Labor Supply
JEL Classification: J26, H55, D10

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1 Introduction

Why, and how, do couples retire together? The answers to these questions are important for designing and evaluating social security policies and can more generally inform models of household labor supply and retirement. For instance, aging populations are leading to widespread pension reforms. These reforms are often designed at the individual level, but the existence of coordinated retirement between spouses could have sizable implications for associated projections of labor supply, budgetary estimations, and welfare analyses. It is therefore crucial to quantify and explain joint retirement behavior of couples. While a large body of work has documented the existence of coordinated retirement behavior (e.g. Hurd, 1990; Gustman and Steinmeier, 2000; Coile, 2004), empirical evidence on the underlying mechanisms that give rise to joint retirement is comparatively scarce.

In this paper, we study joint retirement and its determinants. We leverage full-population administrative data from Denmark and a discontinuity design to provide clean and precise estimates of joint retirement at the early pension eligibility age. We show that many couples retire together right when one spouse reaches pension eligibility and first becomes eligible for retirement benefits. Our main contribution is to then provide new evidence on the mechanisms that underlie this joint retirement behavior. Specifically, we use the Danish institutional setting, our discontinuity-based identification strategy, and our detailed data on wealth to argue that our estimates are explained by leisure complementarities, and not other channels. Moreover, we exploit the breadth of our data to conduct a rich exploration of heterogeneity and provide a more comprehensive analysis on the pathways and labor market exit routes that couples take in order to achieve joint retirement.

Overall, our empirical analysis proceeds in three steps. In the first step, we document the existence of joint retirement behavior at the early pension eligibility age of 60. Using over two decades of data on the entire population and an age discontinuity design, we first show a sharp increase in own retirement when individuals reach their own pension eligibility age: they are 20 percentage points more likely to retire right when they become eligible for early retirement benefits. We then estimate a sizable spillover effect to their spouses: we document a sharp 1.5 percentage point increase in spousal retirement right when their partners reach the pension eligibility age. This amounts to a scaled spillover effect on spouses of 7.5%. For every 100 individuals who retire upon reaching the early pension eligibility age, around 8 of their spouses adjust their behavior to retire at the same time. We also document spousal spillovers to claiming of pension benefits and to labor market earnings.

In the second step, we investigate mechanisms. First, we explore the source of joint retirement in our setting. The literature generally discusses five reasons couples may retire together: (i) correlated preferences for leisure, (ii) common shocks, (iii) financial incentives, such as spousal pension benefits, (iv) the shared household budget constraint, and (v) complementarities in leisure. Our discontinuity design rules out correlated preferences for leisure and common shocks as leading explanations, as these two factors can influence retirement, but should do so smoothly through the pension eligibility age. The Danish setting rules out financial incentives such as spousal benefits, as the pension system and taxation operate at the individual level. Two plausible explanations remain: leisure complementarities and the shared budget constraint. If there are leisure complementarities between spouses, then one member of a couple reaching pension eligibility age and retiring could induce retirement of the other member, as we have found. Alternatively, our results could be explained by the shared household budget constraint. If one cannot borrow against future pension wealth, then reaching pension eligibility can unlock liquidity for the household and lead both spouses to retire. To sort out these two possibilities, we leverage our data on wealth. We study a sample of couples who are unlikely to be liquidity constrained and find a joint retirement estimate that is similar to our main estimate. This result supports the view that complementarities in leisure, and not the shared budget constraint, explain our findings.

Next, we uncover the pathways that couples take to retire together. We document three relevant sets of results. Most striking are the results on age differences. We find age differences between couples to be a crucial determinant of joint retirement behavior. Joint retirement in our context is primarily driven by older spouses working past their own eligibility age while waiting for their younger partner to reach pension eligibility. This is in contrast to the less prevalent pathway to joint retirement where younger spouses retire before reaching their own eligibility age in order to retire right when their older partner reaches pension eligibility. To investigate what explains these different paths, we study labor market exit routes and show that the ability to replace earnings with some form of benefits plays a key role. We show that older spouses who retire when their younger partner reaches pension eligibility do so by claiming their own pension benefits at the same time. Unlike older spouses, younger spouses cannot yet claim their own pension benefits when their older partner reaches pension eligibility. We show that a large share of younger spouses retire jointly by claiming unemployment benefits instead.

We also uncover a strong gender gap. We find that female spouses are more likely to adjust their own labor supply to time their retirement with their male partners. Importantly, this result is only revealed after we control for the age difference within the couple, since older partners are disproportionately males, which confounds results from a simple comparison of males and females. The gender gap prevails even after controlling for relative earnings within the couple, suggesting that the gap is not explained by traditional economic incentives and that gender norms may be playing a role.

We then study the role of relative earnings. First, we document patterns consistent with the collective model of household behavior; couples in which the primary earner values joint leisure more are more likely to retire jointly. Structural work from Gustman and Steinmeier (2000) and Browning et al. (2021) show that males value joint leisure more. Consistent with this, we find that couples in which male spouses are the primary earner are more likely to retire jointly. Second, we find patterns suggesting that couples consider the opportunity cost of retirement, seen as foregone earnings. After controlling for gender, we find that younger spouses who are secondary earners are more likely to retire jointly by retiring earlier, while older spouses who are primary earners are more likely to retire jointly by retiring later.

Our third and last step, after having documented and explained joint retirement at the early pension eligibility age, is to explore joint retirement in the context of social security reform. One common reform involves raising pension eligibility ages. Our setting allows us to directly study this policy variation of interest. We study a 2011 reform that discontinuously increased the early pension eligibility age by six months, contingent on exact birth date. We use monthly administrative records for the entire population and a local difference-in-differences design to study the causal effects of the reform. We find that individuals affected by the reform delay their retirement, and their spouses respond by delaying their retirement as well. We estimate a scaled spillover effect from this design of 8.8%, which is similar to our main estimate and indicates that for every 100 individuals who postpone their retirement due to the reform, around 9 of their spouses delay their retirement as well.

Our paper relates generally to the rich literature that studies the coordinated retirement behavior of couples.[1] A large and important body of work has shown the existence of joint retirement across various settings. Earlier work in this area documented evidence of coordinated retirement behavior (e.g. Hurd, 1990; Coile, 2004) and has been followed by detailed analyses of structural models of household retirement (e.g. Zweimüller et al., 1996; Blau, 1998; Gustman and Steinmeier, 2000, 2004; An et al., 2004; Jia, 2005; Bingley and Lanot, 2007; Van der Klaauw and Wolpin, 2008; Casanova, 2010; Michaud and Vermeulen,

[1] We also naturally relate to the literature studying the impact of own pension eligibility on own retirement (e.g. Mastrobuoni, 2009; Behaghel and Blau, 2012; Staubli and Zweimüller, 2013; Cribb et al., 2016; Manoli and Weber, 2016; Geyer and Welteke, 2019; Haller, 2019; Nakazawa, 2019; Deshpande et al., 2020).

2011; Honoré et al., 2020; Michaud et al., 2020). Most related to our paper, a growing number of reduced-form studies have estimated joint retirement spillovers around pension eligibility ages (Banks et al., 2010; Hospido and Zamarro, 2014; Selin, 2017; Lalive and Parrotta, 2017; Atalay et al., 2019; Bloemen et al., 2019; Johnsen et al., 2022).[2] Two of these papers stand out as the closest to our empirical strategy. Lalive and Parrotta (2017) use a discontinuity design and census data to document spousal spillover effects at the pension eligibility age in Switzerland. Johnsen et al. (2022) use administrative data to study a pension reform in Norway that lowered the retirement age for workers in some firms but not in others. They document spillover effects across public programs and across spouses, although their setting prevents them from studying spillovers to older spouses.

Our main contribution to these papers and the literature more generally is to advance our understanding of mechanisms. We improve upon existing empirical work by using our research design and administrative wealth data to disentangle the role of leisure complementarities from the shared budget constraint. In addition, we put forth a more comprehensive analysis of pathways to joint retirement, highlighting in particular the importance of age differences between spouses. We show that age differences matter because they can dictate the exit routes available for spouses to retire together.[3] We also show how failing to account for age differences can lead to erroneous conclusions when studying gender, an area where previous papers have found mixed results.[4]

In addition to our main contribution on mechanisms, we also contribute by providing results derived from a representative and modern pension system and from a representative population. Our context is representative of modern pension systems in that the pension eligibility age of males and females is the same.[5] Our population is representative in that it includes male and female spouses as well as spouses who are either younger or older than their partners reaching pension eligibility age. Previous work has been restricted to either studying specific groups of the population or studying pension systems where the pension

[2] Another strand of the literature studies financial features of the pension system that indirectly affect retirement (e.g. Baker, 2002; Stancanelli, 2017; Kruse, 2020). A different approach is followed by Goux et al. (2014), who study how an exogenous reduction in hours worked affects spousal labor supply.

[3] To our knowledge, the only other paper to provide evidence on exit routes is Johnsen et al. (2022), who show that younger spouses claim disability insurance to retire with their older partners. We complement and build on their analysis, as we are able to study exit routes for both younger and older spouses.

[4] Some studies find spillover effects to female spouses only (e.g. Hospido and Zamarro, 2014; Lalive and Parrotta, 2017; Bloemen et al., 2019; Johnsen et al., 2022), while others find spillover effects to male spouses only (e.g. Banks et al., 2010; Atalay et al., 2019). Also, Selin (2017) does not find any significant effects when exploring only spillovers to male spouses.

[5] In the past, many pension systems had different pension eligibility ages for males and females, but most developed countries are converging to have the same pension eligibility age for both genders (OECD, 2015).

eligibility age of women was lower than that of men. It is important to have estimates from a modern setting, because men tend to be older than their female partners, and therefore previous systems with later eligibility ages for men may facilitate greater amounts of joint retirement, as spouses will naturally reach pension eligibility age around the same time.

Taken together, our findings have two main implications for policy and models of household retirement. First, our results on the existence and size of joint retirement at the early pension eligibility age can be incorporated into policy design and evaluation to avoid systematic deviations. For instance, our main estimates can inform models of household retirement looking to incorporate complementarities in leisure. Moreover, our analysis of the reform provides policy makers with evidence that changes to pension eligibility ages can generate similarly sized spillover effects. Second, our results on pathways to joint retirement indicate that policy makers should consider the age composition of couples, and the corresponding economic incentives they face, when designing and predicting the effects of social security reforms. This is especially the case for countries that are still increasing eligibility ages of females to converge to those of men, as females tend to be the younger member of couples, who we show generate larger spillovers.

The rest of the paper is structured as follows. Section 2 describes the institutional background. Section 3 presents the data and the samples of analysis. Section 4 lays out our empirical strategy for estimating the effect of reaching pension eligibility age and reports the results. Section 5 explores the mechanisms that underlie joint retirement behaviors. Section 6 analyzes the reform that increased pension eligibility ages. Section 7 concludes.

2 Institutional Background

The Danish retirement system is broadly typical of other developed countries. The two primary sources of retirement income are benefit payments from public pensions and savings in private retirement accounts, with the latter coming from personal or employer contributions during working life. Employer-sponsored retirement savings programs are common in Denmark, with roughly 85% of the workforce participating in employer defined contribution savings schemes (OECD, 2019).

There are two sources of public pension benefits. The Old Age Pension (OAP) provides universal retirement income security at old ages, and the Voluntary Early Retirement Pension (VERP) provides early retirement benefits for those who choose to participate in the program. The majority of workers participate in the VERP program, about 80% of the birth cohorts we study. As VERP plays a major role in determining labor supply and retirement patterns of the Danish population, we focus our analysis on the VERP eligibility age.

Voluntary Early Retirement Pension. The VERP program, introduced in 1979, provides access to early retirement benefits, traditionally from age 60. Participating in VERP requires making modest contributions to qualified unemployment insurance funds during working life. Benefits are in practice flat-rate and typically result in payments equal to roughly $27,000 annually (in 2010 USD).[6]

The decision to claim VERP benefits is tightly linked to retirement, although they are technically separate decisions. The reason for this tight link is that the design of VERP produces strong incentives to retire at the same time as claiming. First, individuals must be "available to the labor market" in order to transition to VERP. That is, they must be employed or actively searching for jobs or on a special transition pension (*delpension*). Hence, if individuals choose to leave the labor market before reaching VERP eligibility age, they will potentially forgo 5 years of benefits. Second, there are no actuarial adjustments for deferring claiming, so delaying claiming by one year amounts to a foregone year of benefits. Third, benefits are also subject to substantial means testing against labor market earnings at essentially 100%, which creates strong disincentives to keep working after VERP benefits are claimed, and against private retirement accounts.

The VERP program has remained fairly stable over time. Importantly, during our main analysis period, 1991–2013, the VERP eligibility age remained constant at age 60. A few changes to other aspects of VERP occurred during this period that are worth mentioning. First, the number of years that an individual must contribute to an unemployment fund to qualify for VERP increased over time.[7] Second, a reform in 1992 made it such that waiting to claim until age 63 led to a modest increase in benefits. Third, a reform in 1999 changed the incentives and encouraged individuals to delay claiming of benefits until age 62. Specifically, for those born after 1939, postponing claiming to age 62 results in slightly increased benefits (from approximately $27,000 to $29,600) and the elimination of means-testing against private pension accounts. We show later that our estimates of joint retirement spillovers from the full sample period are similar to estimates from only the later time horizon, when everyone faces the same incentives to delay claiming due to the 1999 reform.[8]

[6]VERP benefits correspond to unemployment insurance (UI) benefits, but are capped at 91% of the maximum UI benefits.

[7]From 1985, individuals had to contribute for 15 years out of the last 20 years. In 1990 the number of years increased to 20 out of the last 25, and in 1995 it increased to 25 out of the last 30.

[8]While not a reform of VERP, between 1992 and 1996 a transitional program allowed long-term unem-

In 2011 the Danish government announced a pension reform increasing pension eligibility ages in 6 month steps contingent on birthdate. Both the VERP and OAP ages increased, as well as the incentivized VERP age. In Section 6 we describe this reform in detail, and we exploit the first discontinuity created by the reform to study the effect on joint retirement. We focus on the first cohort affected, those born after the cutoff date of January 1, 1954, whose VERP eligibility age was raised from 60 to $60\frac{1}{2}$, and who are first impacted in 2014 when they turn 60.

Two features of the VERP program make it ideal to study joint retirement behavior. First, the pension benefits are independent between spouses. The decision to claim or retire does not have any direct effect on the pension benefits of the spouse. Therefore, we can rule out direct effects on the pension benefits of spouses as a mechanism for joint retirement in our analyses.[9] Second, the pension eligibility age is the same for men and women over the entire period considered, which has two advantages. First, our setting is representative of modern systems in most OECD countries that have eliminated the gender gap in statutory pension eligibility ages over the last decades (OECD, 2015, 2017). Second, we can study heterogeneous effects by gender, age composition, and income shares within the couple that are not affected by differential pension eligibility ages.[10]

Old Age Pension. The OAP provides universal old-age benefits. The eligibility age was traditionally 67, and it was lowered to 65 by the 1999 reform. Less than 5% of the spouses in our samples of analysis are therefore old enough to be eligible for OAP. Benefits are roughly \$15,000 for married or cohabiting individuals and \$20,000 for single individuals. Individuals are eligible for full OAP benefits if they have resided in Denmark for at least 40 years, and benefits are reduced proportionally if individuals have resided for a shorter period. Claiming benefits is an active choice, and the decision to claim is separate from the decision to cease working. From 2004, individuals can defer claiming OAP benefits and receive (approximately) actuarially-fair increases in benefits.

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ployed above age 55 (and above age 50 from 1994) to retire with similar conditions as the VERP program.

[9]This is in contrast to Baker (2002), who studies exactly these direct links between spouses' pension benefits, and also to the second empirical design of Atalay et al. (2019) which is based on the characteristics of Vietnam veterans' pension system.

[10]Note that this is in contrast to Lalive and Parrotta (2017), who study a period where men and women face different statutory retirement ages, and to Atalay et al. (2019), who study a reform that raises women's pension eligibility ages to converge to that of men's.

3 Data

We use administrative data covering the entire population of Denmark over the period 1986–2014. Using personal identifiers for each individual, we combine different registers with information on labor market outcomes, wealth, pension benefits, socio-demographics, and family linkages. Variables are third-party reported on an annual basis and contain a large degree of disaggregation. Individuals cannot select themselves out of the registers, and they only exit the registers if they migrate out of the country or die.

In addition, we also use monthly-frequency register data on earnings for all employees in Denmark and on pension benefits for all recipients, which are available from 2008. We combine these data with the annual-frequency registers using the same individual identifiers. This allows us to define retirement ages with more precision, which is crucial for the analysis of the 2011 reform that increased the pension eligibility age by 6 months.[11]

3.1 Key Variables

One advantage of our data is that we can measure three different margins of labor supply and retirement behavior: retirement, claiming, and earnings. Another advantage is that we can measure wealth.

Retirement. We define retirement as ceasing to earn labor market income. For the age-based design we use the annual data to define retirement as the last year during which an individual has positive earned income, before earnings permanently fall close to zero.[12] Therefore, we define retirement as an absorbing state. In the robustness section we show that the results are robust to using a flow definition of retirement that allows individuals to retire multiple times. These definitions are standard in the retirement literature (Coile and Gruber, 2007; Deshpande et al., 2020). For the reform-based design, we use the monthly data to define a dummy that takes the value one if an individual works past the first half of the year (that is, past July 1) in a given year. This accommodates the fact that individuals unaffected by the reform become eligible for benefits at the beginning of the reform year (2014) when they turn 60, whereas individuals affected by the reform become eligible at least 6 months later, when they reach age $60\frac{1}{2}$.

Claiming. We define claiming as receiving pension income, either VERP or OAP. For the

[11]This new dataset, often referred to as *eIncome*, is described in more detail in Kreiner et al. (2016).

[12]We allow for some small positive income, equivalent to 1 month of average earnings, to accommodate the fact that individuals can receive some labor income after they have retired, such as holidays payments or delayed wages.

age-based design we define an indicator equal to one if an individual receives any pension income in a given year. For the reform-based design we define an indicator that takes the value one if an individual receives pension income before July 1 in a given year.

Earnings. In both research designs we use taxable annual labor market earnings from the annual registers. The ability to study this additional outcome is useful in that it reflects both extensive margin and intensive margin labor supply adjustments. We winsorize this variable at the 1^{st} and 99^{th} percentile to reduce the influence of outliers. We adjust this variable for inflation using 2010 as a baseline and convert Danish kroner to U.S. Dollars using the exchange rate 1 USD = 5.56 DKK.

Wealth. We have detailed information on assets that are reported to the government mostly by third parties and recorded at the end of each calendar year. The wealth data are reliable and of high quality (Leth-Petersen 2010, Boserup et al. 2018, Jakobsen et al. 2020). We use these data later to categorize households as either likely to be liquidity constrained or unlikely to be liquidity constrained, which helps us to disentangle potential mechanisms that underlie our main estimates.

3.2 Samples of Analysis

We define two samples of analysis, one for each research design. We start with the full population of Danish couples who reside in Denmark between 1991 and 2014. We define couples as those who are either married, in a registered partnership, or cohabiting. To avoid endogenous changes in marital status around the time of pension eligibility, we identify couples when they are both below age 60 and observe them for as long as they remain together. We restrict the analysis to couples who are not more than 8 years apart from each other, which excludes around 5% of the sample on each side of the distribution. We illustrate the distribution of age differences within couples in panel (a) of Appendix Figure A.3, and we show that our results are robust to relaxing this restriction in Section 4.4.

We focus the analysis on dual-earner couples. First, we restrict the sample to couples where the reference individual (that is, the focal partner who reaches their own pension eligibility age) has earned labor income at least once between ages 55 and 59. All cohorts in our sample of analysis are observed back to age 55 since we have data from 1986. We also exclude reference individuals who are self-employed or on disability benefits at least once between ages 50 and 59, as they are subject to different rules and regulations of the VERP scheme. Second, we restrict the sample to couples where the spouse has earned labor income at least once between ages 50 and 59. We use this longer period for spouses to ensure that our sample does not exclude younger spouses who retire in their early 50s, as they can potentially retire jointly with their older partners.[13]

Age-based sample. For our age-based design, we consider the period 1991–2013, where the early pension eligibility age remained stable at age 60. This provides us with more than two decades of observations from individuals who faced the same pension eligibility age. We focus the analysis on couples where the reference individual is 57 to 60 years old, which leads to a sample size of 367,585 couples and 2,206,044 couple-year observations.

Reform-based sample. For our reform-based design, we consider the period 2008–2014, starting in 2008 because the monthly-frequency data is only recorded from that year. To focus on individuals who are more likely to be impacted by the reform, we restrict this sample to reference individuals who have made qualifying contributions to the VERP program at least once between ages 50 and 59.[14] In our baseline specification, we focus on individuals born within a 3-month window on either side of the January 1, 1954 cutoff. We also balance the sample, keeping only individuals and their spouses who are observed every year between 2008 and 2014, which leads to a sample of 10,321 couples and 73,395 couple-year observations.

Appendix Table A.1 presents summary statistics for the two samples of analysis and for their corresponding unrestricted populations. The first four columns correspond to the age-based period of analysis (1991–2013) and the last four columns correspond to the reform-based period of analysis (2008–2014). First, we can compare the analysis samples to their corresponding unrestricted population samples. We note that both reference individuals and spouses in the analysis samples have higher earnings, higher education, and are less likely to be retired before age 60. This is mainly a consequence of restricting the analysis to dual-earner couples and to those who did not receive disability benefits in the past. Also note that the age difference between spouses is similar between the analysis sample and the population, but the standard deviation is smaller due to the restriction that drops spouses who are more than 8 years apart. Second, we can compare the two analysis samples to each other. Overall the two samples are similar. The reform-based sample has a smaller share of males (47% against 52%), has higher earnings ($64,156 against $60,289) and is more likely

[13]There are four cohorts of spouses that we cannot observe before age 60 to impose the restriction, therefore we keep all those spouses, who represent 0.4% of the sample. Similarly, there are nine cohorts of spouses that we cannot observe during the entire period between ages 50 to 59. In this case, we impose the restriction based on the years we observe. This affects 12% of spouses, of which 80% are observed for five or more years.

[14]Note that we cannot impose this restriction on the full age-based sample because we do not observe contributions far back in time, but in the robustness section we show that our age-based results are robust to imposing this restriction for the subsample of observations between 2008 and 2013, for whom we can observe past contributions. We also show that the reform-based results are robust to not imposing the restriction.

to be retired before age 60 (16% against 14%), but these differences are not statistically significant. These differences are in line with restricting the reform-based sample to VERP contributors, as females are more likely to contribute to the program. The age difference between partners in both analysis samples is similar, and so are the standard deviations.

4 The Effect of Reaching Pension Eligibility Age

4.1 Age-Based Discontinuity Design

To identify causal estimates of joint retirement, we exploit discontinuities that occur at the early pension eligibility age. Specifically, we study retirement behaviors of reference individuals and their spouses right around the eligibility age of the reference individuals, that is around age 60, and we estimate discontinuous changes in key outcome variables. Importantly, when analyzing spouses' retirement outcomes we control flexibly for the effect of their age on their retirement behavior.

Note that in our analysis each member of a couple can potentially appear both as the reference individual and as the spouse, as long as they are observed at ages 57–60 during the period considered. This reflects the dual nature of the couple's decision, and our design allows us to study their retirement behavior from both sides; we observe them as reference individuals when they reach their pension eligibility age, and as spouses, when their partners reach their own pension eligibility age. In the heterogeneity analysis we will, nevertheless, split the sample by age differences and gender, and each member of the couple will appear only as either the reference individual or the spouse.

4.2 The Effect of Reaching Pension Eligibility on Own Retirement

We begin by analyzing the retirement behavior of reference individuals around their own pension eligibility age. We pool individuals for the period 1991–2013, and we leverage the fact that we have exact birth date to plot the raw means of outcome variables against monthly age. Monthly age is defined as the age of an individual, in months, at the end of the calendar year. Because our data is annual, the key outcome variables are also measured at the end of each calendar year. As a result of this, the annual outcomes of individuals who turn 60 early in the year capture behaviors due to being eligible for benefits for a longer period of time than do the annual outcomes of individuals who turn 60 later in the year. For instance, for those who turn 60 in January, the annual outcomes capture responses due to being eligible for a full year of pension benefits. For those who turn 60 in December, the annual outcomes capture responses due to being eligible for only one month of pension benefits.

We are interested in the "full-exposure" effect of being eligible for one entire calendar year. To estimate the effect of being fully exposed to pension benefit eligibility, we follow the methodology in Fadlon et al. (2019), who address the same issue when using annual outcomes and monthly age in U.S. administrative tax data. Specifically, we estimate the following piecewise linear regression, which is closely guided by our graphical analysis and which uses information from all individuals, including those only partially exposed:

$$y_{it} = \alpha + \beta_1 (age_{it} - 60) + \beta_2 \{age_{it} \geq 60\} + \beta_3 \{age_{it} \geq 60\} \cdot (age_{it} - 60) + \sum_{c=1991}^{2013} \kappa_c D_c + \epsilon_{it}, \quad (1)$$

where y_{it} is the outcome of interest for reference individual i at time t, age_{it} is monthly age of the reference individual at the end of the calendar year, and $\{age_{it} \geq 60\}$ is an indicator variable that takes the value one if the monthly age of the reference individual is 60 or above and zero otherwise. The model therefore estimates a discontinuous jump at monthly age 60 and a differential trend thereafter. D_c are calendar year dummies. We estimate this regression for individuals between monthly ages 57 and just below 61.

The full-exposure effect is given by $\beta_2 + \frac{11}{12} \cdot \beta_3$. This estimator captures the treatment effect of being eligible for early pension benefits for one full calendar year. It is composed of a sharp change in levels at the eligibility-age cutoff, captured by β_2, and a sharp change in trends, captured by the slope parameter β_3.

Figure 1 illustrates the methodology and the results for the reference individuals. Each panel plots the raw means of a different outcome variable of interest against the monthly age of the reference individuals. The solid lines plot the parametric fit estimated with the piecewise linear regression model (1). The dashed lines are a linear extrapolation from the observed outcomes before age 60, and they illustrate the counterfactual evolution of the outcome variables in the absence of pension eligibility. The full-exposure effect of being eligible for early retirement pension benefits for a full year is represented by the vertical distance between the solid and the dashed lines, just below monthly age 61.[15]

Panels (a) and (b) illustrate retirement and benefit-claiming responses. They show visu-

[15] Because the outcome variables are measured in December, individuals who turn 60 in December often do not have time to receive pension income until the next year. This is clearly seen in Figure 1, panel (b), where the dot for December is much lower. To prevent this from biasing our estimates, we exclude these individuals by adding a dummy variable that takes the value one if their monthly age is exactly 60. In Table 2 of the robustness section we show that the results are largely unaffected if these individuals are kept.

ally clear discontinuous changes both in levels and trends in the likelihood of retiring and the likelihood of claiming pension benefits, right as the reference individuals reach age 60 and then become fully exposed to benefits. Panel (c) illustrates annual earnings responses. It shows a sharp change in the trend in earnings right at the eligibility age of 60, and highlights how earnings rapidly decline as individuals age from partial to full exposure.

The first row of Table 1 reports the full-exposure estimates. The first column reports the full-exposure effect on retirement. The estimate is 0.2034, which means that reaching pension eligibility increases the share of retired individuals by 20 percentage points. The second row reports the full-exposure effect on claiming. The point estimate is 0.35, so around 35% of individuals claim VERP benefits by the end of their first year of eligibility. The effect for claiming is larger than for retirement for two reasons. First, it is not possible to claim VERP benefits before age 60, as illustrated in panel (b) of Figure 1, and second, individuals who claim can still have positive earnings in the same year. Finally, the third column reports the full-exposure effect on annual labor market earnings, which can potentially reflect responses both on the extensive margin and on the intensive margin. We estimate a decrease of $8,642 in annual earnings after one year of exposure to pension eligibility.

Overall our results show that reaching pension eligibility leads to a strong first stage. Individuals are discontinuously more likely to retire after age 60. We now turn to estimate the causal effects of pension eligibility on spousal retirement behavior.

4.3 The Effect of Reaching Pension Eligibility on Spouses

For the spillover effect on spouses, we follow a similar empirical strategy as for reference individuals. The main difference is that we need to control for the effect of spouse's own age on their retirement behavior so that we can isolate the causal effect of their partner's pension eligibility.

We begin the analysis with a nonparametric illustration of spouse retirement patterns around their partners' age, after accounting for the effect of the spouses' own age. Specifically, we plot residuals from the following regression:

$$y_{it}^{s} = \alpha + \sum_{a=49}^{69} \delta_a \cdot D_a^s + \sum_{a=49}^{69} \gamma_a \cdot D_a^s \cdot D_g + \sum_{c=1991}^{2013} \kappa_c \cdot D_c + \varepsilon_{it}, \tag{2}$$

where y_{it}^{s} is the outcome variable of interest for spouse s of individual i at time t, D_a^s are dummy variables for spouses' monthly age, and D_g is a gender dummy. The residuals $\hat{\varepsilon}_{st}$ therefore capture spousal retirement behavior not explained by their own age and gender.

The dots in Figure 2 plot spousal residuals $\hat{\varepsilon}_{it}$ binned over the monthly age of reference individuals. This illustrates the spouses' retirement patterns that are driven by their partner's age. We observe that spousal residuals change discontinuously right when their partner becomes eligible for early pension at age 60, resembling the same pattern we observed for the reference individuals themselves.

Guided by this graphical analysis, we estimate a parametric model that quantifies the causal effect of one partner reaching pension eligibility age on the retirement behavior of their spouse. The estimating equation is similar to equation (1) for the reference individual, but with spouses' outcomes as the dependent variables and additional controls for spouses' age and gender that do not impose any functional form. The estimating equation is:

$$\begin{aligned} y_{it}^{s} = \alpha + \beta_1 \left(age_{it} - 60\right) + \beta_2 \left\{age_{it} \geq 60\right\} + \beta_3 \left\{age_{it} \geq 60\right\} \cdot \left(age_{it} - 60\right) + \\ \sum_{a=49}^{69} \delta_a \cdot D_a^s + \sum_{a=49}^{69} \gamma_a \cdot D_a^s \cdot D_g + \sum_{c=1991}^{2013} \kappa_c \cdot D_c + \epsilon_{it}, \end{aligned} \tag{3}$$

where y_{it}^{s} is the outcome of interest for spouse s of individual i, age_{it} is monthly age of the reference individual, and $\{age \geq 60\}$ is an indicator variable that takes the value one if the reference individual is 60 or older (in terms of monthly age) and zero otherwise. D_a^s are dummy variables for spouses' monthly age, and D_g is a gender dummy. We estimate this regression for the same sample of reference individuals, between ages 57 to 61, as before.

The full-exposure effect is again given by $\beta_2 + \frac{11}{12} \cdot \beta_3$. For illustrative purposes, Figure 2 superimposes the parametric fit of the model estimated in equation (3) over the residuals from equation (2). The full-exposure effect corresponds to the vertical distance between the solid and dashed lines just below age 61. The second row of Table 1 reports the full-exposure effect on spouses from their partner reaching pension eligibility age. The effects on all three spousal outcomes are statistically significant at the 1% level. These point estimates can be viewed as the reduced-form effects on spouses.

To judge the size of joint retirement behaviors, we report "scaled effects" in the last row of Table 1, defined as the full-exposure effect on the spouse divided by the full-exposure effect on the reference individual. Scaled effects are our preferred measure for reporting and interpreting joint retirement spillovers, as they are comparable across different outcomes, samples of analysis, and empirical strategies, including our reform-based design. We compute standard errors for these scaled estimates by bootstrapping (Andrews and Buchinsky, 2000; MacKinnon, 2006).[16]

[16]Note that these scaled effects are conceptually similar to the estimates from an instrumental variables

The scaled effect on the retirement outcome is 7.5%. This is a sizable spillover. For every 100 individuals who retire right when they reach their early pension eligibility age, about 8 of their spouses are induced to retire as well. This is after controlling for the effect of the spouses' age on their own retirement behavior.

The scaled effect for claiming benefits is 3.4%. This effect is smaller than the one for retirement for two reasons. First, the denominator is larger. The full-exposure effect on the reference individual is larger for claiming than for retirement as discussed earlier. Second, the numerator is slightly smaller. The full-exposure effect on the spouses is smaller because of spouses who retire but do not claim. Knowing the spillover effect on claiming is important for policy and fiscal estimations, but for the reasons mentioned above it does not fully capture joint retirement behavior.

For earnings, the scaled effect is 9.8%. This outcome potentially captures both extensive margin responses and intensive margin adjustments. However, we cannot conclude that there are significant intensive margin responses based on the larger size of the scaled effect for earnings compared to retirement. Note that the size of the scaled effect for earnings depends on the relative earnings within each couple, and even if adjustments occur only through the extensive margin, the scaled effect on earnings will be larger than the scaled effect on retirement if the spouses who adjust tend to be the primary earner. This in turn depends on the response heterogeneity and mechanisms underlying joint retirement, which we analyze in Section 5.

4.4 Threats to Identification and Robustness

Our identification strategy relies on the assumption that, once we control flexibly for the spouses' age on their own retirement, the discontinuous behavior that occurs when their partner reaches pension eligibility age is caused by that event, and nothing else. In this section we provide a number of tests to assess the validity of our design.

Placebo test. To be assured that we successfully control for the effect of the spouses' age, we carry out a placebo test. We repeat the analysis for the same sample of reference individuals, but we randomly assign them fake spouses of similar age. Specifically, we assign a spouse of the same age to half of the reference individuals, and we assign spouses who are

approach. We use scaled effects because they allow for a more flexible estimation of the second stage (the spouses' full-exposure effect) by estimating the jump at 60 and the differential trend separately. An instrumental variables approach, instead, imposes that the functional form of the jump and the differential trend estimated in the first stage is maintained in the estimation of the second stage.

between 1 and 3 years younger or older to the other half of the reference individuals.[17] In this sample, these fake spouses appear to be retiring at the same time because their ages are by construction highly correlated and most of them reach pension eligibility age at the same time. However, we should not observe any joint retirement behavior beyond the one due to this age correlation between spouses, given that fake spouses cannot influence each other. If our empirical strategy successfully controls for the effect of age correlations, then we should not find any evidence of joint retirement in this placebo sample. Reassuringly we do not find any, as reported in Appendix Table A.6 and Appendix Figure A.1.

Alternative specifications. In Table 2 we report the results from a wide variety of robustness and specification checks. Row A reports the baseline estimates for comparison.

First, we assess the sensitivity of our estimates to choices we made regarding the analysis sample and the regression specification. In row B we restrict our sample to individuals born in January, who are fully exposed in the year they turn 60. These estimates are therefore solely identified from the discontinuous jump in levels of the outcome variables. In row C we extend the sample of analysis to include reference individuals of ages 55 and 56. In row D we relax our restriction that drops couples with partners who are more than 8 years apart from each other. In row E we add additional controls, predetermined region of residence and education for both reference individuals and their spouses, to the regression. In row F, we drop the dummy variable that identifies reference individuals who turn 60 in December, so that they are included in the estimation of the jump at 60 and the differential trend afterwards. In row G, we report the scaled effect for retirement defined as a flow variable, which allows individuals to retire multiple times (see Appendix Figure A.2 for the corresponding graphs).

Next, we vary our approach to estimating the counterfactual behavior and extrapolating outcomes observed before age 60. In row H we use a more flexible nonlinear counterfactual extrapolation by adding a second order polynomial of the age of the reference individuals to the regression model. This specification reduces our point estimates (the scaled effect on retirement becomes 5%), but note that we are fitting a second order polynomial over a short period of three years (ages 57 to 60). In row I we account for this by increasing the age range to include reference individuals of ages 55 and 56 (as in row C) and fitting a second order polynomial (as in row H), and we obtain spillover effects similar to our baseline estimates.

Finally, we provide estimates that are more closely comparable with the estimates from the reform-based design that we present in Section 6. In row J we estimate the effects over the period 2008–2013, which is almost the same time period considered in the reform-based

[17] We do not use only spouses of the same age to avoid collinearity between the age of both partners.

design. In row K we further restrict the sample to reference individuals who have made contributions to qualify for VERP at least once between ages 50 and 59. Note that we can only impose this restriction for the 2008–2013 period as we do not observe contributions far back in the past. Overall, we conclude that our results are quite stable and robust to these alternative specifications, sample definitions, and strategies.[18]

5 Explaining Joint Retirement: Heterogeneity and Mechanisms

We have documented the existence of sizable joint retirement spillovers. In this section, we exploit the breadth of our data and the statistical precision of our empirical design to take the next step and investigate mechanisms and pathways that underlie our estimates.

5.1 Leisure Complementarities as the Key Explanatory Channel

The literature generally discusses five main underlying reasons that could lead couples to retire together. First, spouses may have correlated preferences for leisure. Second, spouses may face common shocks that induce joint retirement, such as adverse health events. Third, spouses might have financial incentives to retire together, due to features in some retirement systems such as the existence of spousal benefits. Fourth, spouses in a household share a budget constraint, which could impact joint retirement in different ways. Fifth, spouses might experience leisure complementarities that can lead to joint retirement.

Our identification strategy effectively rules out correlated preferences and common shocks as underlying sources of the joint retirement in our setting. Preferences and shocks can certainly impact retirement, but they should do so smoothly through the pension eligibility age, and thus should not be driving the spousal spillovers that we document.[19] Moreover, the Danish system generally rules out direct financial incentives such as spousal benefits, as both the pension system and taxation are defined at the individual level. We are thus left with two potential leading explanations: the shared budget constraint and leisure complementarities.

We rely on our research design and in particular our data on wealth to disentangle the relative importance of these two underlying reasons for joint retirement. First, we note that the *lifetime* budget constraint should not be impacted by simply reaching pension eligibility

[18] Appendix Table A.2 presents results from the same robustness analyses, but where we break down the scaled effects and report separate estimates for the full exposure effects on reference individuals and the spouses.

[19] We note the possibility that reaching pension eligibility age could cause an adverse health event that then impacts own and spousal retirement. However, in a recent paper also set in Denmark, Nielsen (2019) finds no evidence of worse health at retirement when studying the same age discontinuity that we study.

age. Thus if couples are aware of the financial effects of retiring and claiming pension benefits at various ages, we should not observe any discontinuous spousal spillovers due to the lifetime budget constraint. However, in the presence of liquidity constraints, reaching pension eligibility could impact the propensity to jointly retire through the shared budget constraint, and it could do so in both directions. One possibility is that the shared budget constraint leads to a decreased likelihood of joint retirement at the pension eligibility age through the impact on earnings. Specifically, we have shown that individuals retire right when they reach pension eligibility age. This drop in an individual's labor market earnings impacts their spouse as well. If leisure is a normal good, then the decline in household income should lead to increases in spousal labor supply. We have found the opposite response, an increase in spousal retirement, which indicates that this is not driving our results.

Alternatively, the shared budget constraint could lead to an increased likelihood of joint retirement at the pension eligibility age. If households cannot borrow against future pension wealth, then one individual reaching pension eligibility can unlock liquidity and ease the shared budget constraint, which could induce retirement of their spouse as well. To directly test this hypothesis, we leverage our wealth data to produce estimates of joint retirement spillovers for a subsample of households who are not liquidity constrained. Specifically, we follow Leth-Petersen (2010) and define households unlikely to be liquidity constrained as those whose liquid wealth (total assets excluding housing and pension wealth) is larger than one month of their average labor market earnings, measured for the couple when the reference individual is age 57.

Table 3 reports the results. In column (1) we see that the joint retirement estimate for this sample of households unlikely to be liquidity constrained is 6.7%, which is not statistically different from our baseline estimate of 7.5%. We see this result as evidence against the hypothesis that the shared budget constraint is driving our joint retirement estimates, as the shared budget constraint itself should not lead spouses in couples who do not face liquidity constraints to discontinuously retire right when their partner reaches pension eligibility age. The evidence thus points to leisure complementarities as the key driver of joint retirement in our context.[20]

[20]Note that we do not dismiss the role of liquidity constraints in general, but rather interpret the evidence as indicating that leisure complementarities are the primary explanation underlying our estimates. Our estimate for liquidity constrained couples reported in column (2) indicates a spillover of 8.5%. This estimate, although also not statistically different from our leading estimate, is larger in magnitude, and we acknowledge that these households could be facing some liquidity constraints that are alleviated by accessing their public pension wealth, which could thus contribute to increased spousal joint retirement. But we also note that there are other reasons why these couples might retire jointly at a higher rate, as they are much less wealthy.

5.2 Age Differences and Pathways to Joint Retirement

We have explored *why* couples retire together and found evidence supporting leisure complementarities. We now study *how* couples achieve joint retirement. We begin by studying the effect that relative age within couples has on joint retirement and find that it plays a crucial role. We start by splitting our sample based on whether spouses are older or younger than their partners who reach age 60. For each of these subsamples we replicate the analysis and report the results on retirement in columns (1) and (2) of Table 4.[21] We observe that the scaled effect is 9.9% for older spouses and only 2.9% for younger spouses, while still highly significant. These results show that in our context, the preferred path to joint retirement is the one where the older spouse works longer, past their own pension eligibility age, while waiting for the younger partner to reach pension eligibility in order to retire together. This is in contrast to the alternative path to joint retirement where the younger spouse retires earlier, before reaching pension eligibility age, when the older partner first becomes eligible for early pension benefits.

To explore the effect of age differences in more detail, we define subsamples based on smaller intervals of age differences and estimate scaled effects for these subsamples. The results are reported in Figure 3. We observe that the largest spillovers are concentrated among spouses who are older, but not too far apart from the age of their partners. Focusing on the retirement outcome in panel (a), we see the largest spillover effect (above 10%) for spouses up to 2 years older, followed by spouses who are between 2 and 4 years older. The effect decreases for spouses who are more than 4 years older than their partners. For younger spouses, we do not find evidence of differential spillovers in joint retirement as the difference in age between partners increases.

Benefit claiming as an exit route. A natural hypothesis is that older spouses drive our joint retirement estimates because it is older spouses who can claim their own pension benefits when their younger partners reach pension eligibility age. Indeed, in Table 5 we report results that support this hypothesis. We show that a large majority of older spouses who retire jointly also claim their pension benefits at the same time. Specifically, we estimate the full exposure effect on an indicator variable that takes the value one if an individual is retired *and* claims pension benefits in a given year. The estimate for older spouses is 0.022, which is 85% of the estimate on retirement alone (0.026), reported in Table 4. In contrast to older spouses, younger spouses who retire jointly when their partners reach age 60 cannot yet

[21] For completeness, Appendix Table A.3 reports results for claiming and earnings as the outcomes.

claim their own pension benefits. However, they may be able to replace foregone earnings with another source of benefits: unemployment insurance. Interestingly, we find that a significant share of younger spouses who retire jointly also claim unemployment benefits. The full exposure effect for younger spouses on an indicator variable that takes the value one if they are retired *and* receive unemployment benefits is 0.0016, which is 40% of the full exposure effect on their retirement probability (0.004).

Taken together, our results on age differences have important implications for policy. Policies that aim to account for the joint retirement of couples should consider the economic incentives faced by each age group, and in particular for their ability to claim benefits that can replace foregone earnings. For instance, in our context, younger spouses cannot claim pension benefits of their own when their partner reaches the early pension eligibility age. However, in other contexts, such as those focused on a "full retirement age" preceded by an early retirement age, younger spouses may have an option to claim their own early retirement benefits when their older partner reaches the full retirement age. In these cases, one might expect larger joint retirement spillovers to younger spouses.

5.3 A Gender Gap in Joint Retirement

Next, we explore heterogeneity by gender, a dimension where previous studies have found mixed results. Some of the difficulties faced by the literature include pension systems where eligibility ages differ by gender, reforms that affected one gender only, lack of statistical power to estimate small effects, and not accounting for confounded effects between age differences and gender. Our analysis overcomes these challenges, as there are no gender differences in the Danish pension system, benefits and taxation are independent between spouses, and we have statistical power to estimate gender differences controlling for other factors.

We begin by replicating our analysis over a simple split by gender. Column (3) of Table 4 presents results for the subsample of male spouses and column (4) for female spouses. The scaled effects for both male and female spouses is 7.5%, which could erroneously lead us to conclude that both genders are equally likely to adjust their behavior to retire jointly with their partner.

However, this simple split by gender masks important differences in the composition of relative age between spouses among the two groups. As in most countries, Danish men tend to be the older member in couples.[22] In our sample, males are around two years older than

[22] Hospido and Zamarro (2014) and Coile (2004) consistently find similar age gaps, of around two years on average, for different European countries and for the U.S. respectively.

females, as illustrated in panel (b) of Appendix Figure A.3. We have shown that older spouses are much more likely to retire jointly, therefore the estimate for men confounds the fact that the subsample of male spouses is composed of a larger share of older spouses. Therefore, to explore gender differences in joint retirement, we must control for age differences. We address this by reweighting the subsample of female spouses to match the distribution of age differences from the subsample of male spouses, and then re-estimating the spillover effect.

The result is shown in column (5) of Table 4. The scaled effect for females rises from 7.5% to 13%. We can then compare this scaled effect to the scaled effect for male spouses, assured that the difference is not driven by the age-difference composition of both subsamples. We find that females clearly respond more, contrary to the conclusion that we could have reached from the simple split by gender.[23] The reweighting strategy assumes that couples where females are the older spouse are comparable to couples where females are the younger spouse. We explore this in Appendix Table A.5, and show that these two types of couples are remarkably similar along observable characteristics such as labor market earnings, educational attainment, retirement probability, or whether they live in the Copenhagen region, all measured before age 57. Female spouses are similar to each other regardless of whether they are the younger or the older member in the couple, and so are males.

A potential explanation for these gender differences in behavior is that relative earnings within couples confound joint retirement and gender. We study the role of relative earnings in detail in the next subsection, but regarding its impact on the gender gap, we show that the gender gap we find is robust to further reweighting the sample of female spouses to have the same distribution of earnings shares as male spouses. The results are reported in column (6) of Table 4, where the scaled effect estimate for retirement remains high at 13.2%.[24] Our results therefore unveil a gender gap that cannot be explained by age or relative earnings within couples, suggesting a role for gender norms. This result adds to recent findings of gender differences that cannot be explained by traditional economic incentives such as Daly and Groes (2017), Kleven et al. (2019), Gørtz et al. (2020), and Lassen (2020).

Our results also document a new source of gender differences in earnings and labor supply.

[23]Furthermore, we find this gender gap both for couples were the female partner is the younger member as well as for couples where the female partner is the older member. We show this in Appendix Table A.4 where, as an alternative to the reweighting strategy, we split the sample in four, by gender and by relative age between partners.

[24]The gender gap also remains when we further reweight the subsample of female spouses to ensure that the share who made contributions to qualify for VERP in the past is the same as in the subsample of male spouses. Note that we only observe VERP contributions for the most recent period of time and hence we perform this test for the period 2008–2013 only. The scaled spillover for retirement is 7% for males and 11.1% for females after reweighing.

Unlike previous studies that focus on childbearing and childcare, we show a gender gap that originates in the dynamics of family formation and its interaction with joint retirement behavior, manifesting itself at the end of working life. Because males tend to be older than their female partners, couples who retire together most often achieve this either by males retiring later or by females retiring earlier, therefore increasing males lifetime earnings relative to females. Note that the "grandchild penalty" found by Gørtz et al. (2020) could explain part of the gap we identify, as grandmothers retire earlier to take care of their grandchildren, but it does not explain it all, as we also find that older female spouses are more likely to retire later, waiting to retire together with their younger partners.

5.4 Relative Earnings and Household Behaviors

We now focus directly on the role of relative earnings within couples for joint retirement. To define the relative earnings of each member of the couple, we compute predetermined earnings shares based on the average labor market earnings of each partner between ages 55 and 57, reporting the distribution of these shares in panel (d) of Appendix Figure A.3. We define an indicator for who is the primary earner in the couple based on these shares, excluding couples with very similar earnings shares (those between 47.5% and 52.5%, which represent 14% of the sample), although the results are robust to keeping them.

Collective model and joint retirement. A growing literature studies the decision-making process of households through the lens of a collective model (Chiappori, 1992; Browning and Chiappori, 1998; Donni and Chiappori, 2011), where members with more negotiation power have more weight in the decision-making process of the household (Browning et al., 1994). If males and females differ in their preference for joint leisure, we would expect that the member with more power, the primary earner, will have a bigger influence in the joint retirement decision. We explore this in Table 6, where we replicate our analysis to estimate spillover effects over four different subsamples, distinguishing by whether the spouse is the primary or secondary earner and by gender. To avoid composition effects confounding our results, we reweight each primary-earner subsample so that it matches the distribution of the secondary-earner subsample of the same gender in terms of age differences. For simplicity, we report results only for the retirement outcome.

We find that couples where males are the primary earner are more likely to retire jointly, consistent with the findings of Gustman and Steinmeier (2000) and Browning et al. (2021)

that males value joint leisure more than women.[25] This constitutes one piece of evidence consistent with the collective model as an explanation of couples labor supply. Interestingly, we find that the increased joint retirement among couples where males are the primary earner is driven equally by either males or females in these households adjusting their behavior to retire together. Specifically, we find that male spouses who are primary earners, reported in column (2), are much more likely to adjust their behavior to retire jointly than male spouses who are secondary earners, as reported in column (1). The scaled effect is 9.1% against 4.3%. Correspondingly, female spouses who are secondary earners are much more likely to adjust their behavior to retire together than female spouses who are primary earners, as we see from comparing column (3) to column (4), with scaled effects of 8.2% and 2.3% respectively.

Opportunity cost and joint retirement. We now explore whether the interaction between relative earnings and age differences within couples affects their preferred path to joint retirement. Specifically, one might expect that older members who are primary earners are more likely to extend their employment while younger members who are secondary earners are more likely to retire earlier, consistent with the opportunity cost of retirement as foregone labor market earnings. We study this by replicating our analysis to estimate spillover effects over four different subsamples, distinguishing by whether the spouse is the primary or secondary earner and whether the spouse is the younger or the older member of the couple. To avoid composition effects confounding our results, we reweight the primary-earner subsamples so that they match the distribution of the secondary-earner subsamples in terms of age differences and gender.

The results are reported in Table 6. Overall, the primary-earner status does not seem to be a major determinant of joint retirement, as the differences between primary and secondary earner spouses are small and not statistically significant. However, interpreting the estimates at face value, we observe patterns consistent with the opportunity cost of retirement. We see that among older spouses, shown in columns (1) and (2), primary earners are 1.1 percentage points more likely to retire jointly. That is, they are more likely to work past their retirement age waiting for their younger spouses to reach their own pension eligibility age. On the contrary, among younger spouses, shown in columns (3) and (4), secondary earners are 2.7 percentage points more likely to retire jointly. That is, they are more likely to stop working before they reach their own pension eligibility age to retire when their older partner becomes eligible. These results are consistent with the opportunity cost of retirement seen

[25] As noted by Gustman and Steinmeier (2000), an interpretation of this result is that males might dislike having more time to take care of the household while their partners continue to work.

as foregone earnings. The returns to continued employment are higher for primary earners, who therefore are more likely to work longer, while the foregone earnings from secondary earners are smaller, making it less costly for them to stop working earlier.

6 Impact of Increasing Retirement Ages

We have shown that spouses are more likely to retire right when their partners reach pension eligibility age. What happens to the joint retirement of couples when the pension eligibility age of one partner changes? In this section we study a major reform that discontinuously increased the early pension eligibility age of selected cohorts.

6.1 The 2011 Pension Reform

In May of 2011 the Danish government announced a pension reform that discontinuously increased the early pension eligibility age from 60 to $60\frac{1}{2}$ for those born right after January 1, 1954, while it remained at 60 for those born right before.[26] Other characteristics of the VERP program remained the same, including the pension benefits and its independence between spouses.

The design of the VERP program, which we described in Section 2, creates strong incentives to retire right at the VERP eligibility age. Hence, the reform induced strong shifts in claiming and retirement ages of the affected individuals that we can use as a first stage to study spillover effects to their spouses. For more details on this reform and an analysis of how it affects savings of individuals directly affected see García-Miralles and Leganza (2021).

6.2 Reform-Based Discontinuity Design

To identify the causal effects of increasing the pension eligibility age, we use a local difference-in-differences framework. The treatment group is composed of individuals born on January 1, 1954 or soon after, whose pension eligibility age increases by 6 months due to the reform. The control group is composed of individuals born right before January 1, 1954, whose pension eligibility age remains the same. In our main analysis we use a bandwidth of three months around the cutoff, and we present results for different bandwidths in the robustness section. The reform was announced in 2011 and implemented in 2014, which is when those

[26] The reform also introduced six-month increments in the incentivized early retirement age of 62 and in the OAP age of 65, but we maintain our focus on the prominent early pension eligibility age. Later cohorts experienced further increases in their eligibility ages, illustrated in Appendix Figure B.1.

born after the cutoff reach age 60 but cannot claim benefits until age $60\frac{1}{2}$. Our difference-in-differences approach compares the evolution of outcomes for the treatment group to that of the control group around the announcement and implementation of the reform. We focus on the effect of the reform during the implementation year, as we are interested in estimating spousal spillovers from the reform that are comparable to those from the age-discontinuity design, which are centered on joint retirement right at the pension eligibility age.

The validity of our empirical approach relies on the assumption that in the absence of the reform, spousal outcomes of the treated and control individuals would move in parallel across time. We provide evidence supporting this parallel trends assumption below. Moreover, interpreting our results on the spillover effect on spouses as causal relies on the assumption that spouse behaviors differ after the reform only because their partners are differentially affected by the reform. A violation of this assumption occurs if the spouses themselves are directly, and *differentially*, impacted by the same reform. By construction, treated individuals are 3 months older on average than control individuals, and so are their spouses. Therefore, because the reform affects individuals based on their birth date, older spouses are more likely to be directly impacted by the reform themselves. To account for this we control in all our specifications for spouses' own exposure to the reform, although this turns out to have virtually no effect on the results.

We asses the parallel trends assumption and the dynamics around the announcement and implementation of the reform by estimating a dynamic difference-in-differences model over the period 2008–2014 of the form:

$$y_{it}^{(s)} = \alpha + \delta \cdot treat_i + \sum_{c \neq 2010} \kappa_c \cdot D_c + \sum_{c \neq 2010} \beta_c \cdot D_c \cdot treat_i + X_{it}' \cdot \psi + \epsilon_{it}, \tag{4}$$

where $y_{it}^{(s)}$ is the outcome variable of interest, either for the reference individuals (y_{it}) or for their spouses (y_{it}^{s}). D_c are calendar year dummies, and $treat_i$ is an indicator for individuals in the treatment group. The matrix X_{it}' is a set of controls that includes spousal age rounded to quarters interacted with gender, as well as a dummy indicating whether a spouse was born after January 1, 1954 and its interaction with calendar year dummies.

The results from these dynamic difference-in-differences (Figure 4) are discussed in detail in the next section. Note that to assess the parallel trends assumption we must consider the pre-announcement period (2008–2010), for which we find no evidence of differential trends. During the period between announcement and implementation (2011–2013), treated individuals and their spouses could adjust behaviors in anticipation of reaching increased pension eligibility ages. However, we find no evidence of anticipatory responses for the reference individuals, nor for the spouses despite a slight change in the coefficient for 2013, the year just before implementation. Nevertheless, to be on the safe side we quantify the effects of the reform with respect to the pre-announcement period only, and show that our results are robust to including the anticipation period in the pre-period. Specifically, we estimate the following model to quantify the causal effects of the reform:

$$y_{it}^{(s)} = \beta_0 + \beta_1 \cdot treat_i + \beta_2 \cdot ant_t + \beta_3 \cdot post_t + \beta_4 \cdot treat_i \cdot ant_t + \beta_5 \cdot treat_i \cdot post_t + X_{it}' \cdot \psi + \epsilon_{it}, \quad (5)$$

where $y_{it}^{(s)}$ is the outcome variable of interest, either for the reference individual (y_{it}) or for their spouses (y_{it}^s), $treat_i$ is an indicator for individuals in the treatment group, ant_t is an indicator for years in the anticipation period (2011-2013), $post_t$ is an indicator for implementation year 2014, and X_{it}' is a set of controls that includes spousal age rounded to quarters interacted with spousal gender, as well as a dummy indicating whether a spouse was born after January 1, 1954, and its interaction with the anticipation and implementation periods separately. When this equation is estimated for the reference individual, the coefficient β_5 identifies the causal effect of the reform on the reference individuals (the first stage). When the equation is estimated for the spousal outcomes, the coefficient β_5 identifies the causal effect on the spouses (the reduced-form).

To obtain scaled spillover effects of the reform, which can be compared to the scaled effects estimated in Section 4, we estimate a 2SLS model where the retirement outcomes of the reference individual are instrumented by their treatment status interacted with the calendar year during which the reform directly affects them ($treat_i \cdot post_t$). The first stage of the 2SLS model corresponds to equation (5) when it is estimated for the reference individual's outcomes. The second-stage equation is the following:

$$y_{it}^s = \beta_0 + \beta_1 \cdot \hat{y}_{it} + \beta_2 \cdot treat_i + \beta_3 \cdot ant_t + \beta_4 \cdot post_t + \beta_5 \cdot treat_i \cdot ant_t + X_{it}'\psi + u_{it}, \quad (6)$$

where $\hat{y}_{it}$ is the predicted outcome for the reference individual estimated in the first-stage and the coefficient β_1 identifies the scaled spillover effect.

6.3 The Effect of Increasing the Pension Eligibility Age on Own Retirement

Panel (a) of Figure 4 shows the results of the dynamic difference-in-differences model on the retirement outcome of individuals directly affected by the reform, and Appendix Figure B.2 shows the results for claiming and earnings in panels (a) and (c). We confirm that the behavior of the treated and control groups along the three outcomes considered is similar during the period before announcement (2008–2010) as well as before implementation of the reform (2011–2013). The trends of both groups move in parallel, and we can rule out any significant anticipatory response.

During the implementation year of 2014, individuals in the treatment group respond to the reform by delaying retirement, consistent with the incentives built into the VERP program. Panel (a) of Figure 4 shows that individuals in the treatment group are around 19 percentage points less likely to retire during the first half of the year. Note that the reform increased the pension eligibility age by 6 months and hence we define retirement as stopping to work during the first half of the year, as explained in Section 3. In the first row of Table 7, we report estimates from the pooled difference-in-differences model, which quantify the effects of the reform on retirement, claiming, and earnings for individuals directly affected. We have a strong first stage to analyze spillover effects to spouses.

6.4 The Effect of Increasing the Pension Eligibility Age on Spouses

We now study the effect of the reform on spousal retirement behavior. Panel (b) of Figure 4 reports the dynamic effects for the retirement outcome of spouses. In the period preceding the announcement of the reform (2008–2010), spouses of both treatment and control individuals behave similarly, providing evidence in support of the parallel trends assumption. After announcement and before implementation (2011–2013), no coefficient is significantly different from zero, suggesting that spouses do not respond differentially in anticipation of their partners' increased pension eligibility age, in line with the lack of anticipation of the reference individuals who are themselves affected directly by the reform.[27] In the implementation year, 2014, we see that spouses of individuals who are affected by the reform delay their retirement, consistent with extending employment in order to retire jointly with their partner. The pattern of the point estimates for claiming and earnings, shown in Appendix Figure B.2 panels (b) and (d), suggests that spouses might adjust their behavior along these other two margins as well, although the estimates are less precise.

The second row of Table 7 reports the difference-in-differences estimates that quantify the spousal effects. The estimates are statistically significant for retirement and claiming, but not for earnings. We report scaled effects from the 2SLS model in the third row of

[27] Although we do not find evidence of anticipatory responses from spouses, we do observe that in 2013, the year just before implementation, the coefficient moves slightly, perhaps suggesting a mild, and not significant, anticipatory response by spouses. This is why in our main specifications (equations 5 and 6) we include an indicator variable for the years between announcement and implementation of the reform.

Table 7. The scaled effect on retirement is 8.8%, indicating that for every 100 individuals who postpone their retirement due to the reform, around 9 spouses will delay their own retirement to make it coincide with that of their partner. The spillover in claiming is 4% and the spillover in earnings is 8.8%, although the latter is not statistically significant.

Overall, our findings show that the reform induced similar spillover effects as the ones we estimated in a stable context where pension eligibility ages did not change and were known by the couples well in advance. These results are consistent with a lack of significant frictions that could prevent couples from adjusting their behavior to retire jointly. This may be of particular interest to policy makers trying to predict short-run responses of social security reforms based on estimates from stable settings. Conversely, it helps with interpreting other reform-based estimates in the literature, as it shows that couples' joint retirement behavior can adjust relatively quickly to changes that affect the retirement age of one partner.

We also explore heterogeneity in responses to the reform. Despite the relatively large sample size of our reform-based design (a panel of 10,321 individuals), we are unable to explore mechanisms in as much depth as in our age-based design, where we estimated effects on reweighted samples and from more granular sample splits. However, we are able to replicate our reform-based analysis on a simple sample split by age differences and gender, and the results, reported in Appendix Table B.1, go in the same direction as our age-based analysis. We find that in response to the reform, older spouses respond the most, with a 10.7% spillover in retirement against 3.2% for the younger spouses. A simple gender split returns similar spillover estimates for male and female spouses (8.4% and 8.8% respectively) as was the case in the age-based design. This suggest again that female spouses respond more once we account for the fact that females are most often the younger member of the couple (around 1.8 years younger in this reform-based analysis sample).

6.5 Threads to Identification and Robustness

Spousal exposure to the reform. As discussed above, treated individuals are 3 months older on average than control individuals, and so are their spouses. This leads to spouses being differentially affected by the reform. Across our analyses we control for whether spouses are affected by the reform to account for the differential exposure.

In this section we show that the differential impact of the reform on spouses is small, and we demonstrate that our results are not driven by this differential impact. First, note that only spouses born during the first 6 months of 1954 are affected by the part of the reform that increases eligibility ages from 60 to $60\frac{1}{2}$ and impacts them in 2014. In Appendix

Figure B.3 we plot the distribution of spouses' birth dates and show that spouses of treated individuals are only 1.3 percentage points more likely to be born during those 6 months than spouses of control individuals (6.5% against 5.2%). To ensure that our results are not driven by these spouses, we replicate the analysis excluding individuals whose spouses are born in the first half of 1954, both from the treatment and control groups. The results, reported in row B of Table B.2, are very similar to the baseline results.

We also note that spouses born after July 1, 1954 are affected by the reform by experiencing larger increases in their pension eligibility ages (Appendix Figure B.1). However, these increases only affect them directly after 2014, and we do not include those years in our analysis. Spouses in the control group are 2.2 percentage points more likely to be born after July 1, 1954 (44.3% against 42.1%). Importantly, this differential impact of the reform on the spouses would only affect our results if the reference individuals or their spouses responded in anticipation to future changes in their pension eligibility age. Across our analyses, we do not find evidence of significant anticipatory responses. Nevertheless, we replicate the analysis for the subsample of individuals whose spouses are more than 3 months older, which ensures that all spouses are born before January 1, 1954 and therefore are completely unaffected by the reform. The results, reported in Appendix Table B.4, show even larger spillover effects. This is to be expected, as we have shown that older spouses are the ones that respond the most. Overall, these tests show that the share of spouses who are differentially impacted by the reform do not have a substantive impact on our results.

Robustness. We perform a series of robustness tests that we report in Appendix Table B.2. Row A reports the baseline estimates for comparison and Row B was introduced earlier.

First, we explore the sensitivity of our results to alternative sample definitions. Row C extends the analysis sample to include reference individuals who did not contribute to the VERP program between ages 50-59. Rows D through G report results from decreasing and increasing the bandwidth around the cutoff date of January 1, 1954 by one and two weeks. Row H shows the results when we do not balance the sample of analysis. We also test the sensitivity of our results to alternative model specifications. Row I adds controls for region and education of the reference individuals and their spouses, defined when they are 57 years old. Row J specifies the model without the anticipation variable.

The magnitudes of the estimates are broadly similar across specifications. The retirement spillover is statistically significant across all specifications other than the one using the largest bandwidth. The statistical significance of the claiming spillover is more sensitive to the specification. The estimates for earnings continue to be insignificant in most cases.

7 Conclusion

Understanding the extent to which couples retire together, why they do so, and how they do so, is important for public pension policy. In this paper, we show how social security systems, and in particular pension eligibility ages, influence the joint retirement behavior of couples. Using high-quality administrative data from Denmark, we estimate sizable spillover effects from individuals reaching early pension eligibility age to their spouses of roughly 8%. This means that for every 100 individuals who retire upon reaching pension eligibility age, 8 of their spouses adjust their behavior to retire at the same time. We also find spillover effects to claiming of pension benefits and to labor market earnings.

We then analyze the underlying mechanisms and pathways that lead to joint retirement in our setting. First, we show that our results are explained by leisure complementarities. We use our empirical design and data on wealth to rule out other traditional explanations for joint retirement, such as common shocks or the shared budget constraint. Next, we study how couples achieve joint retirement. We find that joint retirement at the early pension eligibility age is driven by older spouses who work past their own pension eligibility age while waiting for their younger partner to become eligible for pension benefits as well. In contrast, we estimate smaller spillover effects for younger spouses, and we highlight how this is likely explained by the limited availability of exit routes, as younger spouses cannot yet claim their own pension benefits if they retire when their older partner reaches the early pension eligibility age. We also demonstrate that accounting for age differences is important when studying other determinants of joint retirement. After accounting for age differences, we uncover a strong gender gap, which is not explained by relative earnings, suggesting that gender norms play a role. After accounting for both age differences and gender, we provide evidence on the role of relative earnings that lends support to collective models of household behavior and that is consistent with couples considering the opportunity cost of retirement.

Our results have implications for policy. The existence of sizable joint retirement spillovers driven primarily by complementarities in leisure can inform policy makers, who should account for these spillovers when designing policies and projecting their effects. Moreover, policy makers should account for the demographic characteristics of individuals affected by social security policies, in particular the age composition of couples and the corresponding economic incentives they face. This is particularly relevant for countries that are still increasing eligibility ages of females to converge to those of men, as females tend to be the younger member of couples, and therefore these policies are likely to induce the largest spillovers.

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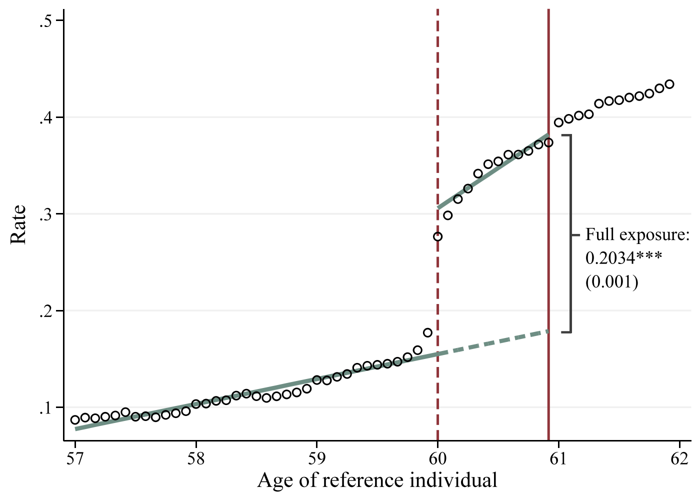
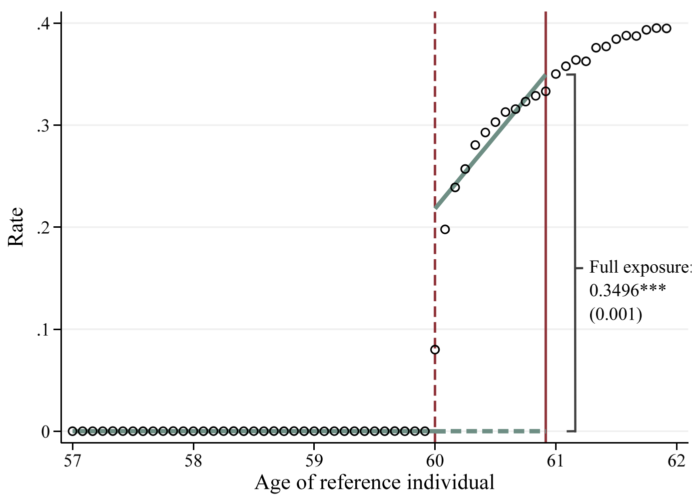
Figure 1: The Effect of Reaching Pension Eligibility Age on Own Retirement

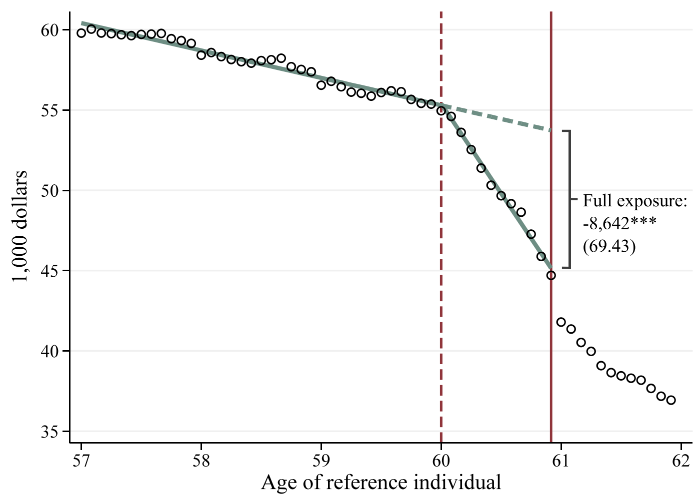
(a) Retirement



(b) Claiming



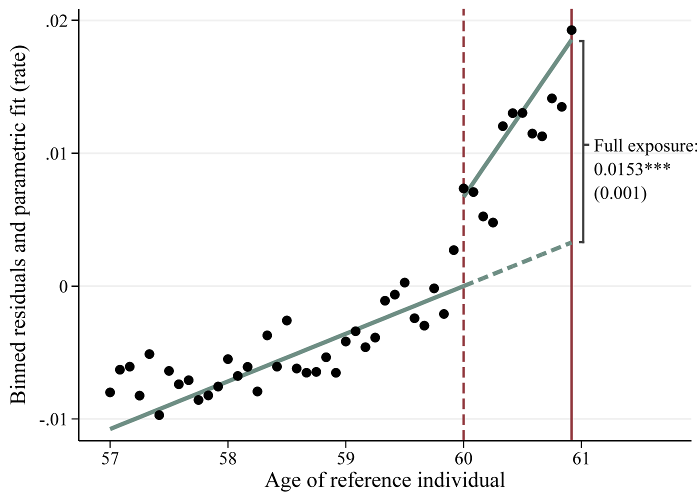
(c) Earnings



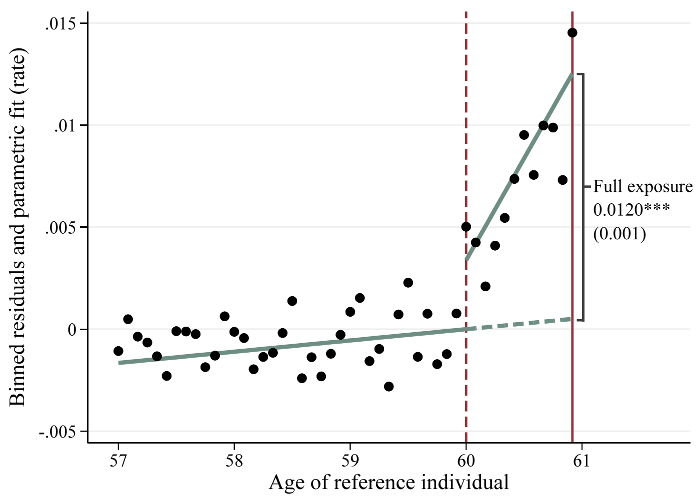
Notes: These figures plot different outcomes for individuals around their own pension eligibility age of 60, pooling individuals over the period 1991–2013. The hollow circles are raw means of the outcome variable measured at the end of each calendar year, grouped in monthly age bins. The solid lines plot the parametric fit estimated with the piecewise linear regression model (1). The dashed line represents the counterfactual behavior in the absence of pension eligibility, based on a linear extrapolation from the observed outcome before age 60. The full-exposure effect of being eligible for early retirement pension during an entire year is represented by the vertical distance between the solid and dashed lines just below age 61.

Figure 2: The Effect of Reaching Pension Eligibility Age on Spouses

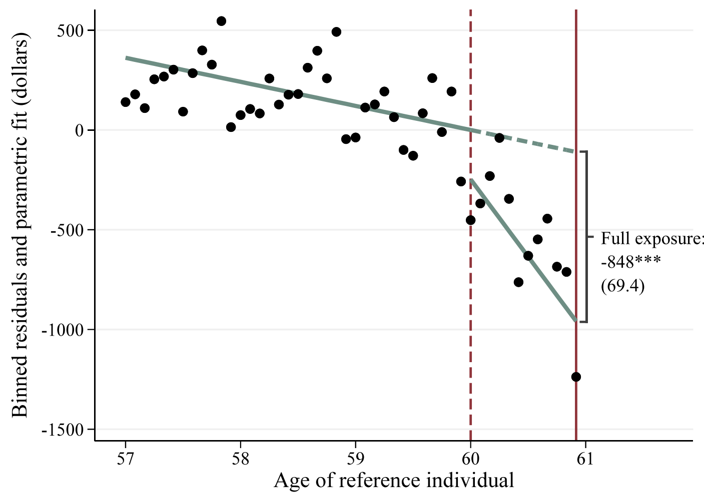
(a) Spouse Retirement



(b) Spouse Claiming

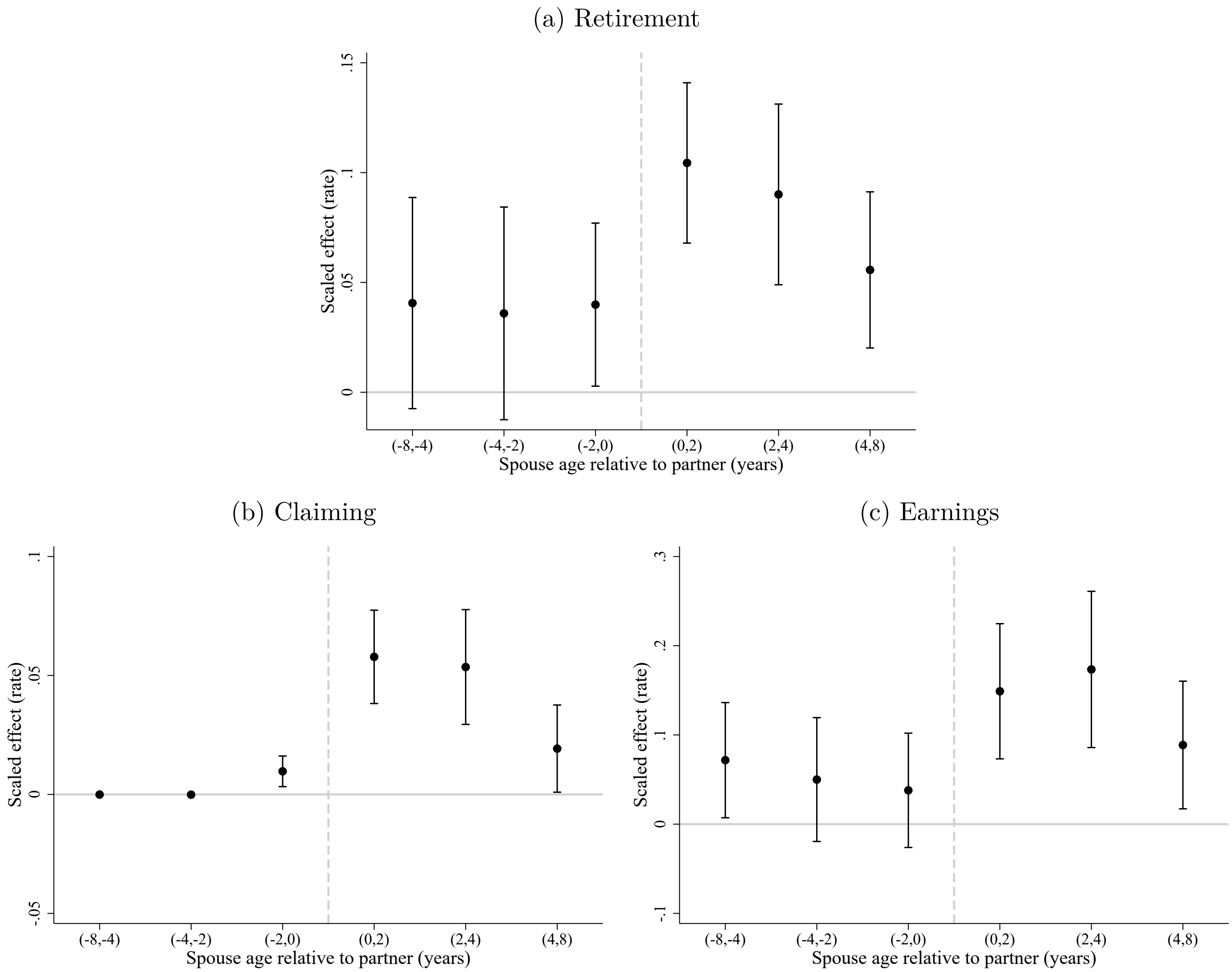


(c) Spouse Earnings



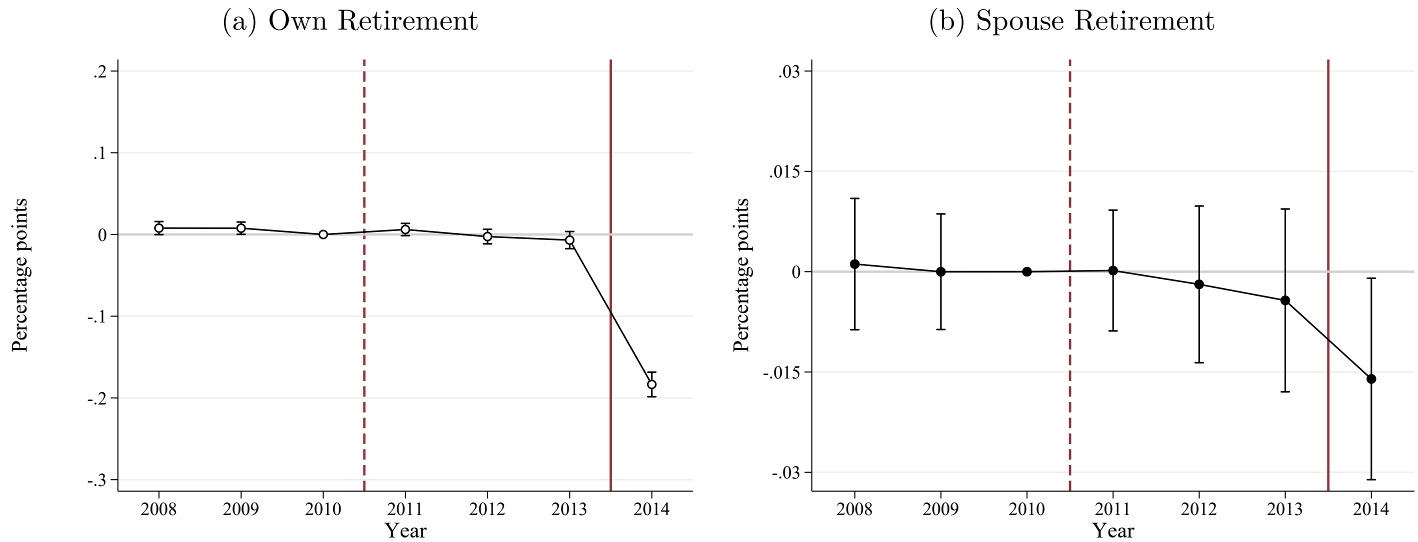
Notes: These figures plot different outcomes for spouses around the pension eligibility age of their partner. The dots are the residuals estimated in equation (2) where spousal outcomes are regressed on spousal age and gender. The residuals are grouped in monthly bins of the reference individual's age. The solid lines plot the parametric fit estimated with the piecewise linear regression model (3). The dashed line represents the counterfactual behavior in the absence of pension eligibility, based on a linear extrapolation from the observed outcome before age 60. The full-exposure effect on the spouses of their partners being eligible for early retirement pension during an entire year is represented by the vertical distance between the solid and dashed lines just below age 61.

Figure 3: Joint Retirement Behavior by Age Differences Within Couples



Notes: These figures plot the scaled spillover estimates of joint retirement for different subsamples of couples based on the age difference between spouses. These scaled effects are estimated using the same methodology as for the full sample: first estimating models (1) and (3) to obtain full-exposure effects and then dividing the full-exposure effect on spouses by the full-exposure effect on reference individuals. We report 95% confidence intervals calculated from bootstrapped standard errors.

Figure 4: The Effect of the Reform Increasing the Pension Eligibility Age on Retirement



Notes: These figures plot the β_c coefficients from the dynamic difference-in-differences model (4), estimated on retirement outcomes. Panel (a) plots results for own retirement, and panel (b) plots results for spouse retirement. Each coefficient shows the difference between the treated group (whose pension eligibility age increases by 6 months, to age $60\frac{1}{2}$) and the control group (whose pension eligibility age remains at age 60), relative to the difference in 2010. The coefficient for 2014 identifies the causal effect of the reform during the implementation year. We report confidence intervals at the 95% level, calculated from robust standard errors clustered at the couple level.

Table 1: The Effect of Reaching Pension Eligibility Age

	Retirement	Claiming	Earnings
Reference Individual	0.2034***	0.3496***	-8,642***
	(0.001)	(0.001)	(69.43)
Spouse	0.0153***	0.0120***	-848***
	(0.001)	(0.001)	(61.16)
Scaled Effect	0.0750***	0.0344***	0.0981***
	(0.007)	(0.003)	(0.012)
N. of clusters	367,585	367,585	367,585
Observations	2,206,044	2,206,044	2,206,044

Notes: This table reports the effect of reference individuals reaching pension eligibility age on their own retirement and on their spouses' retirement. Each column reports the results for a different outcome. The first row reports the full-exposure effect to pension eligibility on own retirement estimated in equation (1). The second row reports the full-exposure effect on the spouses from their partners becoming eligible for pension, estimated in equation (3). The third row reports the scaled effect resulting from diving the spouse full-exposure effect by the reference individual full-exposure effect. Robust standard errors in parentheses, clustered at the couple level. Bootstrapped standard errors for scaled effects. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.1$

Table 2: Robustness to Alternative Specifications
for the Effect of Reaching Pension Eligibility Age

	Retirement	Claiming	Earnings
A. Baseline	0.0750***	0.0344***	0.0981***
	(0.007)	(0.0031)	(0.012)
B. January-born	0.0771***	0.0404***	0.0966***
	(0.022)	(0.0084)	(0.033)
C. Including Younger Ages	0.0718***	0.0363***	0.0928***
	(0.006)	(0.0028)	(0.010)
D. Unrestricted Age Difference	0.0690***	0.0311***	0.0905***
	(0.007)	(0.0029)	(0.012)
E. Adding Controls	0.0749***	0.0340***	0.0922***
	(0.007)	(0.0031)	(0.012)
F. No Donut December	0.0736***	0.0327***	0.0987***
	(0.007)	(0.0030)	(0.012)
G. Retirement Flow Definition	0.0573***	–	–
	(0.005)		
H. Nonlinear Counterfactual	0.0498***	0.0300***	0.0580**
	(0.016)	(0.0055)	(0.026)
I. Nonlinear & Incl. Younger	0.0761***	0.0328***	0.0972***
	(0.010)	(0.0037)	(0.017)
J. Period 2008?-2013	0.0758***	0.0459***	0.104***
	(0.012)	(0.0068)	(0.025)
K. 2008–2013 & VERP Eligible	0.0674***	0.0434***	0.104***
	(0.011)	(0.0063)	(0.024)

Notes: This table reports the scaled effect estimates from replicating our main analysis over different sample definitions and different specifications of the estimation models presented in equations (1) and (3). Row A reproduces results from our baseline specification, introduced in Table 1. Row B restricts the analysis sample to individuals born in January. Row C extends the analysis sample to include reference individuals of ages 55 and 56. Row D extends the analysis sample to include couples with partners that are more than 8 years apart from each other. Row E controls for predetermined region and education of reference individuals and spouses. Row F drops the dummy variable that identifies individuals who turn 60 in December. Row G reports the estimate for retirement defined as a flow. Row H allows for a nonlinear counterfactual by adding a second order polynomial. Row I implements the changes applied in C and H. Row J estimates the effect over the period 2008–2013. Row K estimates the effect over the same period as K and restricts the sample to reference individuals who contributed to VERP. Bootstrapped standard errors in parentheses. $^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.1$.

Table 3: Heterogeneity in the Effect of Reaching Pension Eligibility Age on Retirement by Household Liquidity Constraints

	Unconstrained Households (1)	Constrained Households (2)
Reference Individual	0.2065***	0.1963***
	(0.001)	(0.003)
Spouse	0.0144***	0.0154***
	(0.001)	(0.002)
Scaled Effect	0.0671***	0.0850***
	(0.009)	(0.018)
N. of clusters	257,194	85,664
Observations	1,438,946	411,495

Notes: This table reports the effect of the reference individuals reaching pension eligibility age on their spouses behavior, distinguishing heterogeneous responses by household liquidity constraints. Each column contains results for a subsample of the population. Column (1) shows the result for the subsample of households who are not liquidity constrained (their liquid wealth is larger than the average monthly labor market income). Column (2) shows the result for the subsample of liquidity constrained households, reweighted to have the same distribution of gender and age differences as column (1). Robust standard errors in parentheses, clustered at the couple level. $^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.1$

Table 4: Heterogeneity in the Effect of Reaching Pension Eligibility Age on Retirement by Age Difference and Gender

Reference Individual	Young	Old	Female	Male	Male (w)	Male (w)
Spouse	Old	Young	Male	Female	Female (w)	Female (w)
	(1)	(2)	(3)	(4)	(5)	(6)
Reference Individual	0.2562***	0.1588***	0.2668***	0.1479***	0.1616***	0.1795***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)
Spouse	0.026***	0.004***	0.020***	0.011***	0.021***	0.024***
	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)
Scaled Effect	0.0994***	0.0287***	0.0745***	0.0751***	0.130***	0.132***
	(0.010)	(0.011)	(0.008)	(0.013)	(0.017)	(0.019)
N. of clusters	297,686	334,966	302,589	330,172	330,172	330,172
Observations	1,038,096	1,167,948	1,054,359	1,151,685	1,151,685	1,151,685

Notes: This table reports the effect of the reference individuals reaching pension eligibility age on their own retirement and on their spouses' retirement, distinguishing heterogeneous responses by gender and age differences within the couple. Each column shows results for a different subsample. The subsample in column (5) is reweighted to have the same distribution of age differences as the subsample from column (3) and the subsample in column (6) is further reweighted to have the same distribution of earnings shares as (3). The first row reports the full-exposure effect of pension eligibility on own retirement. The second row reports the full-exposure effect on spouses of their partners being eligible for retirement pension. The third row reports the scaled effect resulting from diving the spouse full-exposure effect by the reference individual full-exposure effect. Robust standard errors in parentheses, clustered at the couple level. Bootstrapped standard errors for scaled effects. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 5: The Effect of Reaching Pension Eligibility Age on Concurrent Benefit Claiming and Retirement

	Claiming & Retired		Unemployed & Retired	
Reference Individual	Young	Old	Young	Old
Spouse	Old	Young	Old	Young
	(1)	(2)	(3)	(4)
Spouse Full-Exposure Effects	0.022***	0.000	-0.0003	0.0016**
	(0.002)	(0.000)	(0.001)	(0.001)
N. of clusters	297,686	334,966	297,686	334,966
Observations	1,038,096	1,167,948	1,038,096	1,167,948

Notes: This table reports the effect of the reference individuals reaching pension eligibility age on their spouses behavior, distinguishing heterogeneous responses by age differences within the couple. Each estimate reports the full exposure effect on the spouses. Columns (1) and (2) show the probability of spouses claiming pension benefits and being retired. Columns (5) and (6) show the probability of spouses receiving unemployment and being retired. Robust standard errors in parentheses, clustered at the couple level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 6: Heterogeneity in the Effect of Reaching Pension Eligibility Age on Retirement by Relative Earnings

A. By Gender

Reference Individual	Female Primary	Female Sec. (w)	Male Primary	Male Secondary (w)
Spouse	Male Secondary	Male Primary (w)	Female Secondary	Female Primary (w)
	(1)	(2)	(3)	(4)
Reference Individual	0.2475***	0.2745***	0.1434***	0.1426***
	(0.004)	(0.002)	(0.002)	(0.004)
Spouse	0.0111***	0.025***	0.0117***	0.003
	(0.003)	(0.002)	(0.001)	(0.003)
Scaled Effect	0.0434**	0.0909***	0.0816***	0.0225
	(0.021)	(0.011)	(0.016)	(0.028)
N. of clusters	58,311	201,541	229,321	53,949
Observations	191,681	713,870	800,843	185,860

B. By Age Differences

Reference Individual	Young Primary	Young Sec. (w)	Old Primary	Old Second. (w)
Spouse	Old Secondary	Old Prim. (w)	Young Secondary	Young Prim. (w)
	(1)	(2)	(3)	(4)
Reference Individual	0.2096***	0.2651***	0.1413***	0.1541***
	(0.003)	(0.003)	(0.002)	(0.004)
Spouse	0.0198***	0.028***	0.0072***	0.004
	(0.003)	(0.002)	(0.002)	(0.002)
Scaled Effect	0.0931***	0.104***	0.0535***	0.0265
	(0.022)	(0.012)	(0.018)	(0.024)
N. of clusters	94,735	161,573	193,106	93,917
Observations	321,527	571,978	670,997	327,752

Notes: The table reports the effect of the reference individuals reaching pension eligibility age on their own retirement and on their spouses' retirement, distinguishing heterogeneous responses by primary earner status within the couple. Panel A further distinguish by gender and Panel B by age differences. Each column contains results for a subsample of the population. In Panel A, the subsamples in columns (2) and (4) are reweighed to have the same distribution of age differences as columns (1) and (3), respectively. In Panel B the subsamples in columns (2) and (4) are reweighed to have the same distribution of gender and age differences as columns (1) and (3), respectively. Within each panel, the first row reports the full-exposure effect of pension eligibility on own retirement. The second row reports the full exposure-effect on spouses of their partners being eligible for retirement pension. The third row reports the scaled effect resulting from diving the spouse full-exposure effect by the reference individual full-exposure effect. Robust standard errors in parentheses, clustered at the couple level. $^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.1$

Table 7: The Effect of the Reform Increasing the Pension Eligibility Age

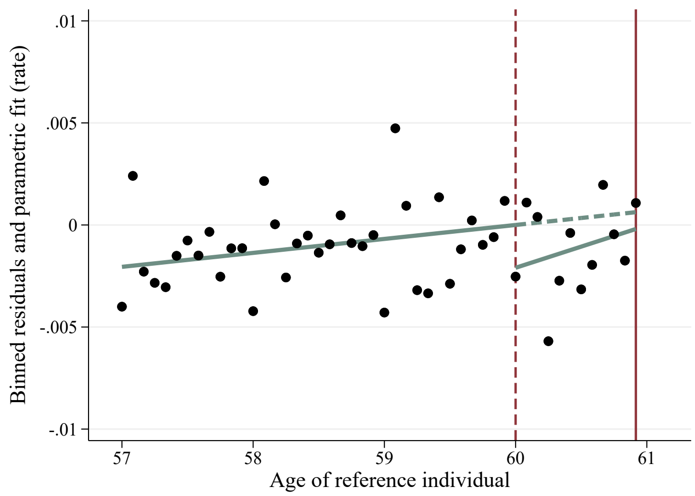
	Retirement	Claiming	Earnings
Reference Individual	-0.189***	-0.263***	8,111***
	(0.007)	(0.007)	(477.1)
Spouse	-0.0165**	-0.0105	710
	(0.007)	(0.006)	(531.2)
Scaled Effect	0.0876**	0.0399*	0.0875
	(0.039)	(0.024)	(0.065)
F-test instr.	657.7	1,644.8	289.0
N. of clusters	10,321	10,321	10,321
Observations	73,395	73,395	73,395

Notes: This table reports the effect of the 2011 reform, which increased the pension eligibility age. Each column reports results for a different outcome. The first row reports the effect on the individuals affected by the reform (the first stage) and the second row reports the spillover effect to their spouses (the reduced-form effect), which are estimated using equation (5). The third row reports the scaled effect resulting from the 2SLS model estimated in equation (6). Robust standard errors in parentheses, clustered at the couple level. $^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.1$

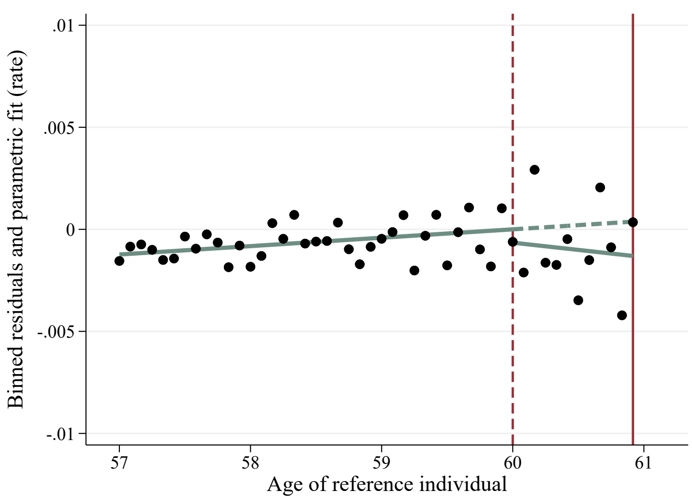
Appendix A Age Discontinuity Design

Figure A.1: Placebo Test Assigning Fake Spouses of Similar Age
for the Effect of Reaching Pension Eligibility Age

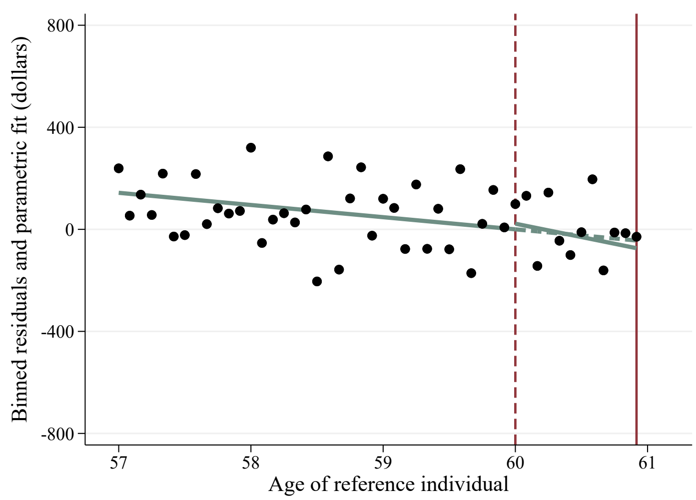
(a) Spouse Retirement



(b) Spouse Claiming



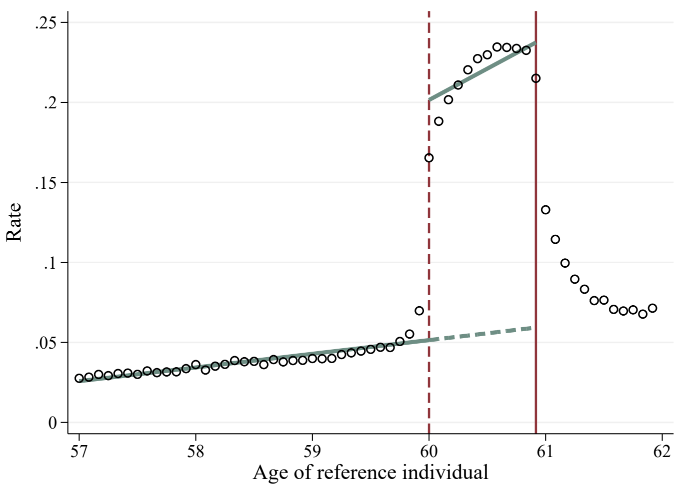
(c) Spouse Earnings



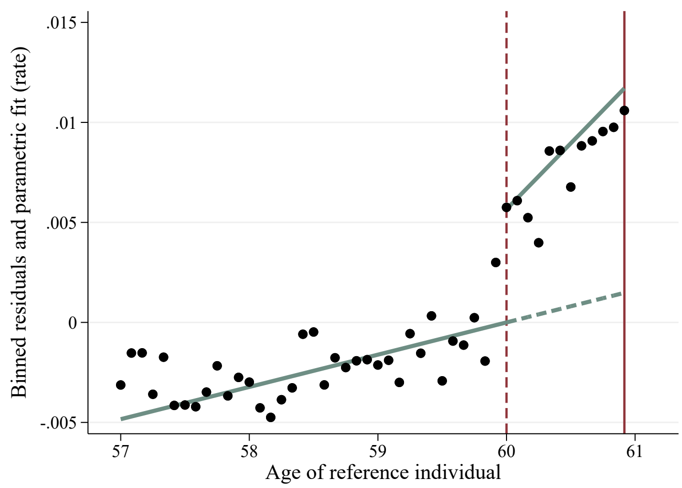
Notes: These figures plot results from replicating the analysis over a placebo sample where the reference individuals are the same as in the main analysis, but they are matched to fake spouses of similar age. The figures show no evidence of joint retirement, as is expected if the research design is valid: fake spouses cannot affect each other's retirement behavior, and the effect coming from the correlation between their ages is controlled for by the empirical design. For more details on the construction of this figure, see the notes of Figure 2. See Appendix Table A.6 for the placebo point estimates.

Figure A.2: The Effect of Reaching Pension Eligibility Age on Retirement Defined as Flow

(a) Retirement Flow

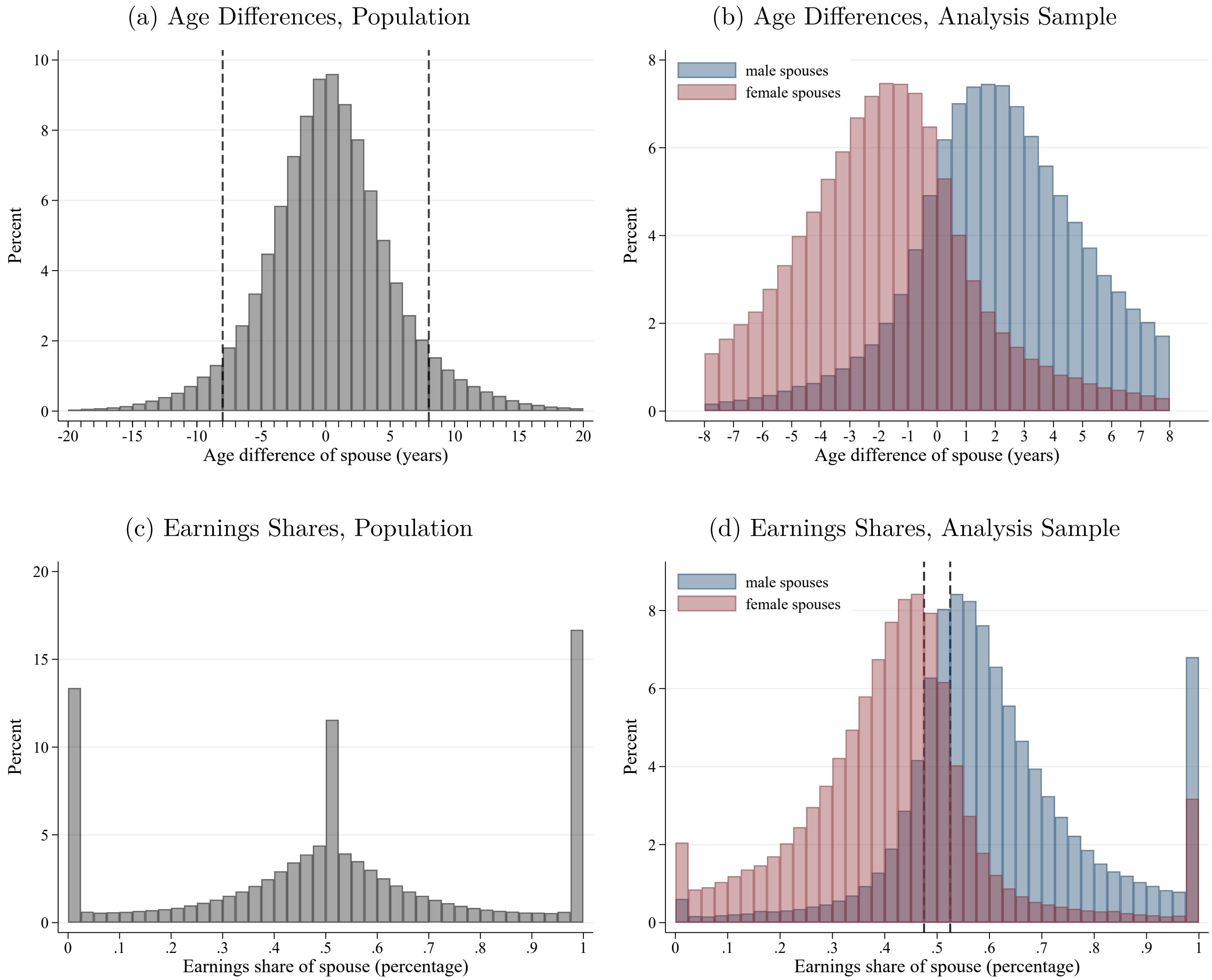


(b) Spouse Retirement Flow



Notes: These figures plot an alternative definition of the retirement outcome, defined as a flow variable that takes the value one in the year in which an individual retires and zero otherwise. For more details on the construction of these figures see notes of Figures 1 and 2. The scaled effect estimate resulting from this outcome is reported in Table 2.

Figure A.3: Distribution of Spouses' Age Differences and Earnings Shares



Notes: Panel (a) plots the distribution of age differences within spouses for the population of Danish couples between 1991 and 2013, before applying any sample restrictions. The vertical dashed lines mark the tails that are excluded from the sample of analysis, corresponding to couples with more than 8 years difference in age. Panel (b) plots the distribution of age differences for the age-based sample of analysis resulting from imposing the restrictions described in Section 3.2. Panel (c) plots the distribution of earnings shares within the couple, based on average annual labor market earnings of each partner between ages 55 and 57, for the full Danish population between 1991 and 2013. Panel (d) plots earnings shares for the age-based sample of analysis. The vertical dashed lines mark the interval of couples with very similar earnings shares (between 0.475 and 0.525) who are excluded in the heterogeneity analysis that defines an indicator variable to identify which member of the couple is the primary earner.

Table A.1: Summary Statistics

	Age-Based Design Period (1991–2013)				Reform-Based Design Period (2008–2014)			
	Population		Analysis Sample		Population		Analysis Sample	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)	Mean (7)	SD (8)
A: Reference Individuals								
Age	58.45	1.12	58.44	1.12	57.45	2.04	57.47	2.06
Male	0.51	0.50	0.52	0.50	0.50	0.50	0.47	0.50
Dane	0.98	0.15	1.00	0.00	0.97	0.18	1.00	0.00
Copenhagen region	0.26	0.44	0.27	0.44	0.25	0.43	0.22	0.41
Educ. Primary	0.37	0.48	0.29	0.45	0.30	0.46	0.25	0.43
Educ. Secondary	0.41	0.49	0.45	0.50	0.41	0.49	0.45	0.50
Educ. Tertiary	0.03	0.18	0.04	0.19	0.04	0.20	0.04	0.20
Educ. Bachelor	0.14	0.34	0.17	0.37	0.18	0.39	0.20	0.40
Educ. Master	0.05	0.22	0.05	0.22	0.07	0.26	0.05	0.23
Earnings age 55-57	45,268	41,165	60,289	35,186	55,582	41,780	64,156	32,218
Retired by age 57	0.20	0.40	0.09	0.29	0.25	0.43	0.12	0.32
Retired by age 58	0.22	0.41	0.11	0.31	0.26	0.44	0.13	0.34
Retired by age 59	0.24	0.43	0.14	0.35	0.29	0.45	0.16	0.37
Retired by age 60	0.39	0.49	0.34	0.47	0.43	0.49	0.35	0.48
B: Spouses								
Age difference (years)	0.34	5.23	0.25	3.46	0.19	5.26	-0.10	3.50
Age	58.12	5.36	58.19	3.64	57.26	5.62	57.57	4.04
Male	0.49	0.50	0.48	0.50	0.50	0.50	0.53	0.50
Dane	0.99	0.08	1.00	0.06	0.98	0.12	0.99	0.08
Copenhagen region	0.26	0.44	0.27	0.44	0.25	0.43	0.22	0.41
Educ. Primary	0.37	0.48	0.29	0.46	0.28	0.45	0.23	0.42
Educ. Secondary	0.41	0.49	0.44	0.50	0.42	0.49	0.45	0.50
Educ. Tertiary	0.03	0.18	0.04	0.19	0.04	0.21	0.05	0.23
Educ. Bachelor	0.14	0.35	0.17	0.38	0.18	0.39	0.20	0.40
Educ. Master	0.05	0.22	0.05	0.22	0.07	0.26	0.06	0.24
Earnings age 55-57	43,510	40,250	57,770	35,070	52,069	44,725	66,224	34,921
Retired by age 57	0.20	0.40	0.12	0.33	0.26	0.44	0.15	0.36
Retired by age 58	0.21	0.41	0.13	0.34	0.26	0.44	0.14	0.35
Retired by age 59	0.22	0.42	0.15	0.35	0.26	0.44	0.15	0.35
Retired by age 60	0.34	0.48	0.30	0.46	0.35	0.48	0.27	0.44
Observations	4,366,996		2,206,044		166,554		73,395	

Notes: This table reports means and standard deviations of relevant variables for different samples of interest. The first four columns correspond to the age-based period of analysis (1991–2013) where the pension eligibility age remained stable, and it includes individuals of age 57 to 60. The last four columns correspond to the reform-based period of analysis (2008–2014) where the pension eligibility age was increased starting in 2014, and it includes individuals born between July 1, 1953 and June 30, 1954. Columns denoted "Population" correspond to the full population without applying any sample restriction. Columns denoted "Analysis sample" correspond to our baseline samples of analysis, after applying the restrictions described in subsection 3.2.

Table A.2: Robustness of Full-Exposure Effects to Alternative Specifications.
for the Effect of Reaching Pension Eligibility Age

	Reference Individuals			Spouses		
	Retirement	Claiming	Earnings	Retirement	Claiming	Earnings
A. Baseline	0.2034***	0.3496***	-8,642***	0.0153***	0.0120***	-848***
	(0.001)	(0.001)	(69.43)	(0.001)	(0.001)	(61.17)
B. January-born	0.1623***	0.3332***	-8,843***	0.0125***	0.0135***	-854***
	(0.002)	(0.002)	(126.99)	(0.002)	(0.002)	(119.85)
C. Including Younger Ages	0.2232***	0.3495***	-9,136***	0.0160***	0.0127***	-847***
	(0.001)	(0.001)	(77.78)	(0.001)	(0.001)	(69.99)
D. Unrestricted Age Difference	0.1997***	0.3462***	-8,558***	0.0138***	0.0108***	-774***
	(0.001)	(0.001)	(66.63)	(0.001)	(0.001)	(56.30)
E. Adding Controls	0.2035***	0.3497***	-8,704***	0.0152***	0.0119***	-803***
	(0.001)	(0.001)	(66.11)	(0.001)	(0.001)	(58.75)
F. No Donut December	0.2072***	0.3673***	-8,589***	0.0153***	0.0120***	-848***
	(0.001)	(0.001)	(64.12)	(0.001)	(0.001)	(61.17)
G. Retirement Flow Definition	0.1784***	–	–	0.0102***	–	–
	(0.001)			(0.001)		
H. Nonlinear Counterfactual	0.1622***	0.3521***	-7,672***	0.0081***	0.0106***	-445***
	(0.001)	(0.001)	(94.11)	(0.001)	(0.001)	(93.97)
I. Nonlinear & Incl. Younger	0.1780***	0.3500***	-7,905***	0.0135***	0.0115***	-768***
	(0.001)	(0.001)	(79.02)	(0.001)	(0.001)	(70.88)
J. Period 2008–2013	0.2216***	0.2849***	-8,125***	0.0168***	0.0131***	-845***
	(0.001)	(0.001)	(140.60)	(0.001)	(0.001)	(132.17)
K. 2008–2013 & VERP Eligible	0.2468***	0.3286***	-8,910***	0.0166***	0.0143***	-929***
	(0.001)	(0.001)	(145.35)	(0.001)	(0.001)	(140.61)

Notes: This table reports the full-exposure effect estimates from replicating our main analysis over different sample definitions and over different specifications of the estimation models (equations 1 and 3). For an explanation of each row see Table 2. Standard errors in parentheses. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.1$.

Table A.3: Heterogeneity in the Effect of Reaching Pension Eligibility Age on Claiming and Earnings by Age Difference and Gender

Reference Individual Spouse	Young Old (1)	Old Young (2)	Female Male (3)	Male Female (4)	Male (w) Female (w) (5)	Male (w) Female (w) (6)
A. Claiming						
Reference Individual	0.4307***	0.28***	0.4567***	0.2544***	0.2632***	0.295***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Spouse	0.021***	0.000	0.017***	0.008***	0.018***	0.020***
	(0.002)	(0.000)	(0.001)	(0.001)	(0.002)	(0.003)
Scaled Effect	0.0495***	0.0035***	0.0374***	0.0301***	0.0674***	0.0683***
	(0.006)	(0.001)	(0.004)	(0.004)	(0.009)	(0.011)
B. Earnings						
Reference Individual	-9,558***	-7,970***	-9,081***	-8,408***	-9,035***	-9,160***
	(93.66)	(97.97)	(81.42)	(104.02)	(140.99)	(162.51)
Spouse	-1,763***	-490***	-1,105***	-570***	-589***	-726***
	(111.11)	(75.16)	(106.72)	(64.96)	(97.23)	(150.95)
Scaled Effect	0.186***	0.0633***	0.122***	0.0678***	0.0661***	0.0802***
	(0.020)	(0.017)	(0.020)	(0.014)	(0.018)	(0.026)
N. of clusters	297,686	334,966	302,589	330,172	330,172	330,172
Observations	1,038,096	1,167,948	1,054,359	1,151,685	1,151,685	1,151,685

Notes: This table reports the effect of the reference individuals reaching pension eligibility age on their own retirement and on their spouses' retirement, distinguishing heterogeneous responses by gender and age differences within the couple. Each column shows results for a different subsample. The subsample in column (5) is reweighted to have the same distribution of age differences as the subsample from column (3) and the subsample in column (6) is further reweighted to have the same distribution of earnings shares as (3). Panel A reports results for claiming as the outcome variable. Panel B reports results for earnings as the outcome variable. Within each panel, the first row reports the full-exposure effect of pension eligibility on own retirement. The second row reports the full-exposure effect on spouses of their partners being eligible for retirement pension. The third row reports the scaled effect resulting from diving the spouse full-exposure effect by the reference individual full-exposure effect. Robust standard errors in parentheses, clustered at the couple level. Bootstrapped standard errors for scaled effects. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.1$

Table A.4: Heterogeneity in the Effect of Reaching Pension Eligibility Age on Retirement Alternative to Reweighting: Split by Age Differences and Gender

Reference Indiv.	Young Female	Young Male	Old Female	Old Male
Spouse	Old Male	Old Female	Young Male	Young Female
	(1)	(2)	(3)	(4)
A. Retirement				
Reference Indiv.	0.2801***	0.1765***	0.2257***	0.1409***
	(0.002)	(0.004)	(0.004)	(0.002)
Spouse	0.025***	0.030***	0.002	0.005***
	(0.002)	(0.003)	(0.002)	(0.001)
Scaled Effect	0.0872***	0.167***	0.00954	0.0359***
	(0.010)	(0.030)	(0.016)	(0.015)
B. Claiming				
Reference Indiv.	0.4758***	0.2793***	0.3975***	0.2482***
	(0.002)	(0.004)	(0.004)	(0.002)
Spouse	0.020***	0.025***	0.000	0.000
	(0.002)	(0.004)	(0.001)	(0.000)
Scaled Effect	0.0428***	0.0878***	0.0041*	0.0033***
	(0.0055)	(0.017)	(0.0023)	(0.0013)
C. Earnings				
Reference Indiv.	-9,579***	-9,740***	-7,571***	-8,076***
	(93.05)	(248.05)	(166.76)	(114.30)
Spouse	-1,881***	-1,200***	-284	-583***
	(131.71)	(191.19)	(226.94)	(75.48)
Scaled Effect	0.197***	0.127***	0.0450	0.0725***
	(0.023)	(0.033)	(0.051)	(0.016)
N. of clusters	228,199	69,596	74,390	260,576
Observations	797,667	240,429	256,692	911,256

Notes: This table reports the effect of the reference individuals reaching pension eligibility age on their own retirement and on their spouses' retirement, distinguishing heterogeneous responses by gender and age composition of the couple. Each column contains results for a different subsample. Each panel reports results for a different outcome variable. Within each panel, the first row reports the full exposure effect of pension eligibility on own retirement as estimated in equation (1). The second row reports the full exposure effect on the spouses of their partners being eligible for retirement pension estimated in equation (3). The third row reports the scaled effect resulting from diving the spouse effect by the own effect. Robust standard errors in parentheses, clustered at the couple level. Bootstrapped standard errors for scaled effects. $^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.1$

Table A.5: Descriptive Statistics by Gender and Age Differences

	Female				Male			
	Younger		Older		Younger		Older	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)	Mean (7)	SD (8)
Earnings age 55-57	48,213	23,886	50,393	24,445	74,823	45,510	72,220	39,940
College education	0.22	0.42	0.28	0.45	0.25	0.43	0.21	0.41
Retired by age 57	0.11	0.32	0.12	0.32	0.08	0.27	0.07	0.25
Copenhagen region	0.26	0.44	0.30	0.46	0.30	0.46	0.26	0.44
Observations	213,862		69,661		65,431		240,733	

Notes: This table reports means and standard deviations of relevant variables for all reference individuals in the sample of analysis used for the age-based empirical design. Column (1) corresponds to females who are younger than their partner, whereas column (2) corresponds to females that are older than their partners. Columns (3) and (4) do the same for males. Labor market earnings are computed as the average between ages 55 and 57. Retirement, education, and whether they live in the capital region, are measured at age 57.

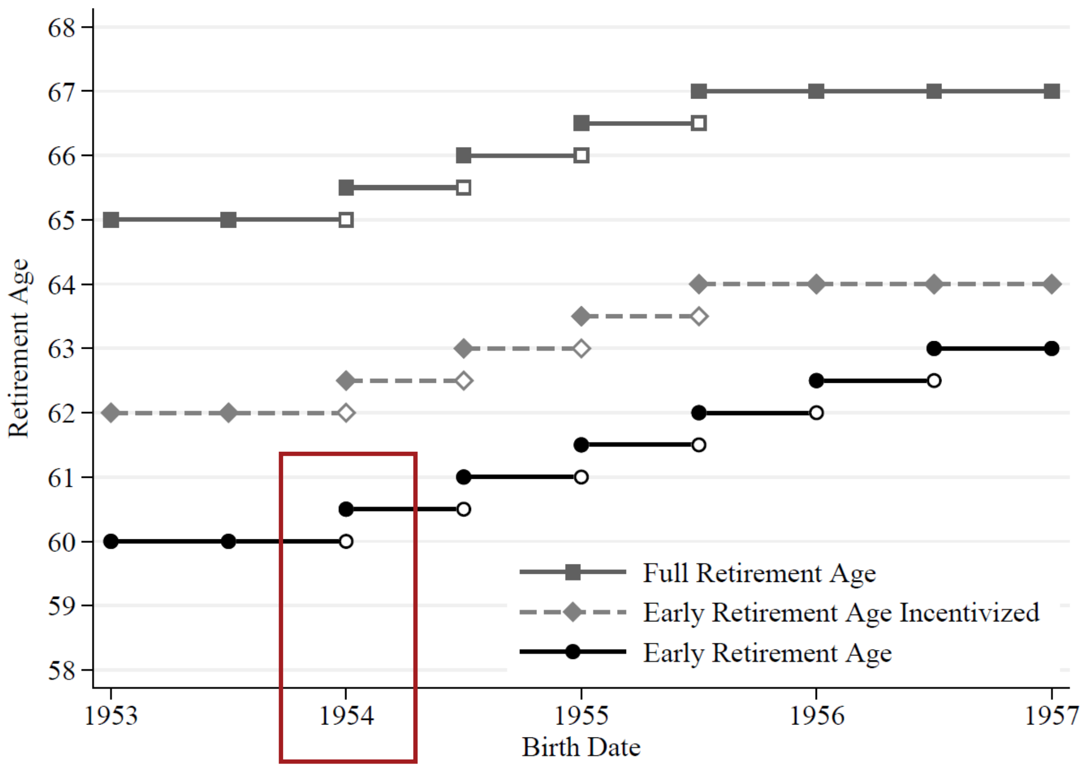
Table A.6: Placebo Test with Fake Spouses
for the Effect of Reaching Pension Eligibility Age

	Retirement	Claiming	Earnings
Reference Individual	0.2034***	0.3496***	-8,642***
	(0.001)	(0.001)	(69.43)
Spouse	-0.001	-0.002	-30
	(0.001)	(0.001)	(78.79)
Scaled Effect	-0.00405	-0.00481	0.00349
	(0.008)	(0.004)	(0.017)
N. of clusters	367,585	367,585	367,585
Observations	2,206,044	2,206,044	2,206,044

Notes: This table reports the results of replicating the analysis over a placebo sample where the reference individuals are the same as in the main analysis, but they are matched to fake spouses of similar age. The placebo test finds no evidence of joint retirement, as should be expected if the empirical strategy is valid. Fake spouses cannot affect each other's retirement behavior, and the effect coming from the correlation between their ages is controlled for by the empirical design. See the notes of Table 1 for a detailed explanation of the content of the table. Robust standard errors in parentheses, clustered at the couple level. Bootstrapped standard errors for scaled effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

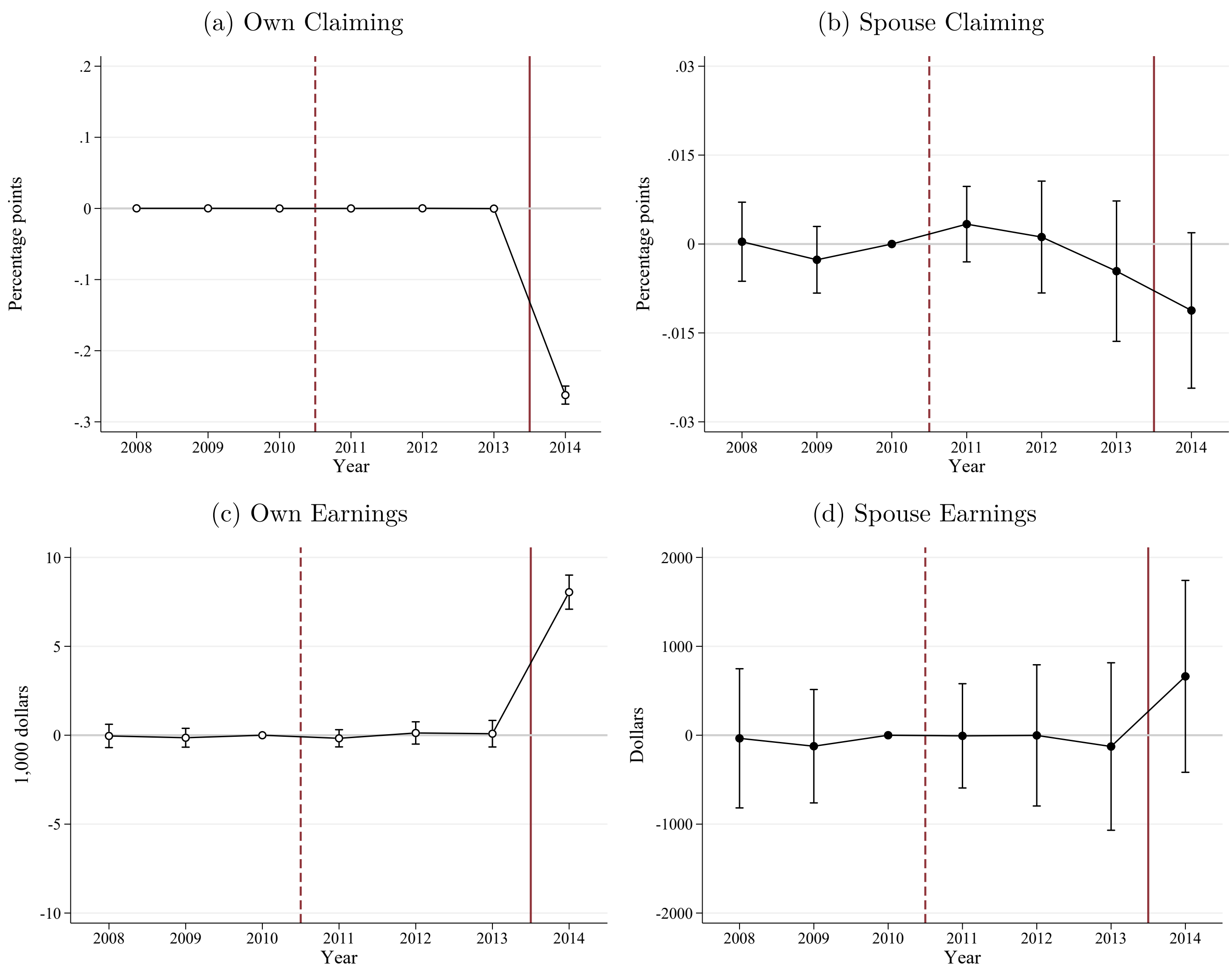
Appendix B Reform-Based Discontinuity Design

Figure B.1: Graphical Depiction of the 2011 Reform



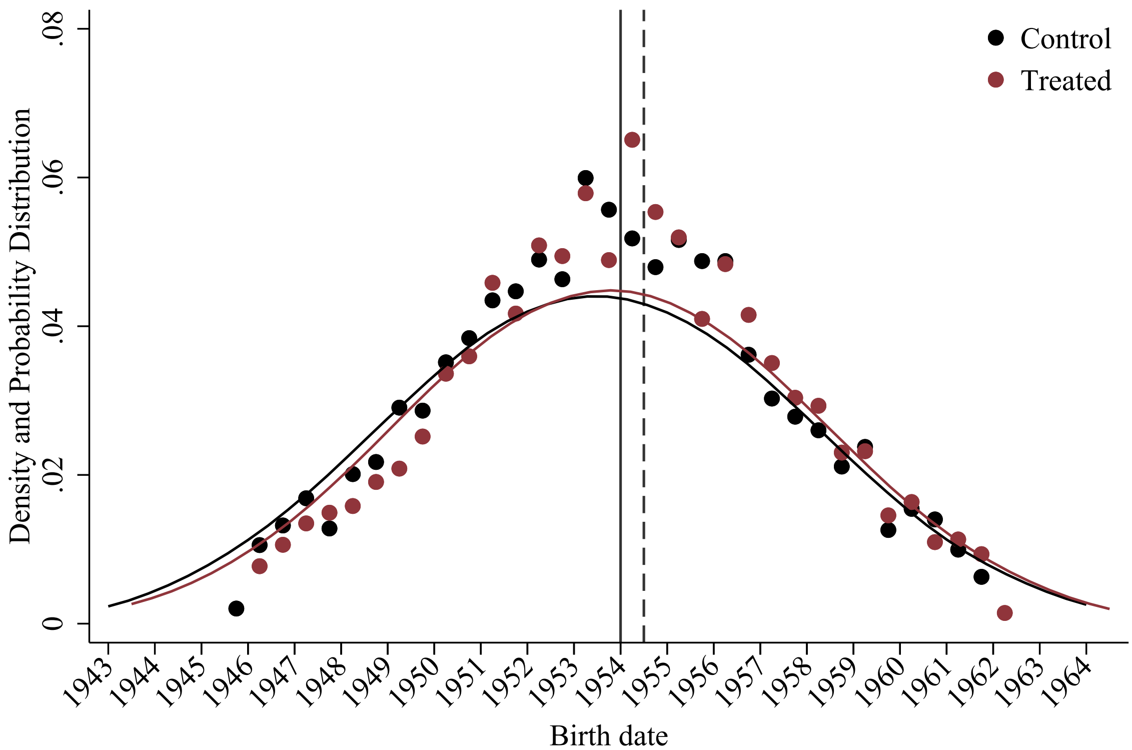
Notes: This figure depicts the 2011 reform that increased retirement ages in 6-month steps contingent on birth date. Cohorts born before January 1, 1954 were unaffected by the reform. Cohorts born between January 1, 1954 and July 1, 1954 experienced an increase of 6 months in their pension eligibility ages. Their early pension eligibility age increased from 60 to $60\frac{1}{2}$, their incentivized early pension eligibility age increased from 62 to $62\frac{1}{2}$ and their full retirement pension increased from 65 to $65\frac{1}{2}$. The red square marks the discontinuity that we exploit in our reform-based research design, where we study the effect of increasing pension eligibility ages. Later cohorts experienced larger increases.

Figure B.2: The Effect of the Reform Increasing the Pension Eligibility Age on Claiming and Earnings



Notes: These figures plot the β_c coefficients from the dynamic difference-in-differences model (4), estimated on claiming and earnings outcomes. Panel (a) plots results for own claiming, panel (b) plots results for spouse claiming, panel (c) plots results for own earnings, and panel (d) plots results for spouse earnings. Each coefficient shows the difference between the treated group (whose pension eligibility age increases by 6 months, to age $60\frac{1}{2}$) and the control group (whose pension eligibility age remains at age 60), relative to the difference in 2010. The coefficient for 2014 identifies the causal effect of the reform during the implementation year. We report confidence intervals at the 95% level, calculated from robust standard errors clustered at the couple level.

Figure B.3: Birth Date of Spouses by Treatment Group for the Reform Sample



Notes: This graph plots the kernel density function and the probability distribution of the birth date of spouses in the treatment and control groups. Spouses in the treatment group are slightly younger than those in the control group, as a consequence of defining the treatment and control groups based on whether the reference individual was born, respectively, after or before January 1, 1954. Spouses that are born between January 1 and June 30, 1954 (indicated by the solid and dashed vertical lines) are directly impacted by the reform in 2014. We can see from the probability distribution, which is depicted by the dots, that spouses in the treatment group are 1.3 percentage points more likely to be born within those dates than the spouses from the control group (6.5% against 5.2%). Spouses born after June 30, 1954 (dashed vertical line) are impacted by the reform only after 2014. Spouses in the treatment group are 2.2 percentage points more likely to be born after June 30, 1954 (44.3% against 42.1%).

Table B.1: Heterogeneity in the Effect of the Reform Increasing Pension Eligibility Age

Reference Individual	Young	Old	Female	Male
Spouse	Old	Young	Male	Female
	(1)	(2)	(3)	(4)
A. Retirement				
Reference Individual	-0.259***	-0.118***	-0.257***	-0.117***
	(0.011)	(0.010)	(0.011)	(0.010)
Spouse	-0.0278**	-0.00370	-0.0215*	-0.0103
	(0.013)	(0.007)	(0.011)	(0.009)
Scaled Effect	0.107**	0.0315	0.0838*	0.0877
	(0.049)	(0.058)	(0.044)	(0.077)
B. Claiming				
Reference Individual	-0.327***	-0.197***	-0.337***	-0.183***
	(0.010)	(0.009)	(0.010)	(0.008)
Spouse	-0.0174	-0.000219	-0.0171	-0.00276
	(0.013)	(0.001)	(0.011)	(0.007)
Scaled Effect	0.0534	0.00111	0.0508	0.0150
	(0.038)	(0.0034)	(0.031)	(0.036)
C. Earnings				
Reference Individual	10,804***	5,375***	10,580***	5,439***
	(667.9)	(690.4)	(609.7)	(740.3)
Spouse	1,174	75	871	369
	(911.6)	(539.5)	(870.2)	(567.9)
Scaled Effect	0.109	0.0140	0.0823	0.0678
	(0.084)	(0.10)	(0.082)	(0.10)
N. of clusters	5,385	5,161	5,541	5,008
Observations	37,541	35,854	38,542	34,853

Notes: This table reports the effect of the 2011 reform, which increased the pension eligibility age, distinguishing heterogeneous responses by age composition and gender of the couple. Each column contains results for a different subsample. Each panel reports results for a different outcome variable. Within each panel, the first row reports the effect on the individuals affected by the reform and the second row reports the spillover effect on their spouses, which are both estimated in equation (5). The third row reports the scaled effect resulting from the 2SLS model estimated in equation (6). F-tests for the strength of the instruments are all well above 10. Robust standard errors in parentheses, clustered at the couple level. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.1$

Table B.2: Robustness to Alternative Specifications
for the Effect of the Reform Increasing Pension Eligibility Age

	Retirement	Claiming	Earnings
A. Baseline	0.0876**	0.0399*	0.0875
	(0.039)	(0.024)	(0.065)
B. Donut Affected Spouses	0.0945**	0.0378	0.0932
	(0.040)	(0.025)	(0.068)
C. No VERP restriction	0.102***	0.0300	0.109**
	(0.035)	(0.022)	(0.055)
D. Smallest Bandwidth	0.0994**	0.0477*	0.0946
	(0.043)	(0.027)	(0.073)
E. Smaller Bandwidth	0.104**	0.0539**	0.109
	(0.040)	(0.025)	(0.068)
F. Larger Bandwidth	0.0645*	0.0376	0.0875
	(0.037)	(0.023)	(0.065)
G. Largest Bandwidth	0.0558	0.0299	0.0875
	(0.036)	(0.022)	(0.065)
H. Not Balancing	0.0907**	0.0448*	0.0701
	(0.039)	(0.024)	(0.070)
I. Adding Controls	0.0870**	0.0395	0.0824
	(0.039)	(0.024)	(0.066)
J. Without Anticipation	0.0828**	0.0413**	0.0870
	(0.033)	(0.021)	(0.053)

Notes: This table reports the scaled effect estimates (2SLS estimates) from replicating our main analysis using different sample definitions and different specifications of the estimation model (equation 6). Row A reproduces results from our baseline specification, which correspond to those reported in Table 7. Row B excludes spouses born in the first half of 1954. Row C extends the sample to include individuals who did not contribute to the VERP program between ages 50-59. Row D reduces the bandwidth by 2 weeks. Row E reduces the bandwidth by 1 week. Row F extends the bandwidth by 1 weeks. Row G extends the bandwidth by 2 weeks. Row F does not balance the sample. Row H controls for region and education of reference individuals and their spouses. Row I does not estimate the anticipation period separately. Robust standard errors in parentheses, clustered at the couple level. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.1$

Table B.3: Robustness of the Effects of Increasing Pension Eligibility Age on Reference Individuals and on Spouses

	Reference Individuals			Spouses		
	Retirement	Claiming	Earnings	Retirement	Claiming	Earnings
A. Baseline	-0.189***	-0.263***	8,111***	-0.0165**	-0.0105*	710
	(0.007)	(0.007)	(471.1)	(0.007)	(0.006)	(531.2)
B. Donut Affected Spouses	-0.193***	-0.265***	8,144***	-0.0182**	-0.0100	759
	(0.0076)	(0.0067)	(491.4)	(0.0077)	(0.0068)	(553.3)
C. No VERP restriction	-0.159***	-0.222***	7,206***	-0.0162***	-0.0067	782**
	(0.0062)	(0.0056)	(391.3)	(0.0055)	(0.0048)	(394.6)
D. Smallest Bandwidth	-0.187***	-0.259***	7,953***	-0.0186**	-0.0123*	752
	(0.0080)	(0.0071)	(522.7)	(0.0081)	(0.0070)	(581.6)
E. Smaller Bandwidth	-0.189***	-0.262***	8,071***	-0.0197***	-0.0141**	877
	(0.0077)	(0.0068)	(497.6)	(0.0077)	(0.0067)	(552.9)
F. Larger Bandwidth	-0.188***	-0.262***	8,111***	-0.0121*	-0.00984	710
	(0.0070)	(0.0062)	(477.1)	(0.0070)	(0.0061)	(531.2)
G. Largest Bandwidth	-0.187***	-0.263***	8,111***	-0.0104	-0.0079	710
	(0.0067)	(0.0059)	(477.1)	(0.0067)	(0.0059)	(531.2)
H. Not Balancing	-0.186***	-0.262***	7,761***	-0.0169**	-0.0117*	544
	(0.0074)	(0.0064)	(493.8)	(0.0073)	(0.0063)	(543.2)
I. Adding Controls	-0.189***	-0.262***	8,034***	-0.0164**	-0.0104	662
	(0.0074)	(0.0065)	(475.8)	(0.0073)	(0.0064)	(529.7)
J. Without Anticipation	-0.186***	-0.262***	8,086***	-0.0154**	-0.0108**	703
	(0.0068)	(0.0065)	(415.1)	(0.0061)	(0.0054)	(433.7)

Notes: This table reports the first and econd stage estimates from replicating our main analysis over different sample definitions and over different specifications of the estimation model (equation 5). For an explanation of each row see Appendix Table B.2. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.4: The Effect of the Reform Increasing Pension Eligibility Age. Replication Over Sample of Spouses At Least 3 Months Older

	Retirement	Claiming	Earnings
Reference Individual	-0.258***	-0.326***	10,718***
	(0.011)	(0.010)	(680.8)
Spouse	-0.0280**	-0.0179	1,480
	(0.013)	(0.013)	(938.1)
Scaled Effect	0.109**	0.0550	0.138
	(0.051)	(0.039)	(0.087)
F-test instr.	523.4	1,078.6	247.8
N of clusters	5,096	5,096	5,096
Observations	35,511	35,511	35,511

Notes: This table replicates the analysis for a subsample where spouses are at least 3 months older than their partners. This ensures that all spouses are born before January 1, 1954, and therefore are totally unaffected by the 2011 reform. This rules out the possibility that the spillover effect to spouses is driven by spouses in the treated and control groups being diferentially impacted by the reform. See Table 7 for notes on the construction of this table.