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 begin by arguing that our estimates are explained primarily by leisure complementarities. We then explore heterogeneity and pathways couples take to retire together. We 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. We also show that females respond more than males and that secondary earners respond more than primary earners. 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 systems and can more generally inform models of household labor supply and retirement. Facing aging populations, many countries are reforming their pension systems, and coordinated retirement between spouses can factor into associated labor supply projections, 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 et al., 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, leveraging our detailed data to conduct a rich exploration of heterogeneity that informs on the pathways and 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 for our spillover estimates, as these two factors should influence joint retirement 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 explanations remain: the shared budget constraint and leisure complementarities. If reaching pension eligibility unlocks liquidity for liquidity constrained households, then the shared budget constraint could lead both spouses to retire. However, we use data on wealth to study couples unlikely to be liquidity constrained and find results similar to our main results. We view these findings as suggestive evidence that leisure complementarities between spouses drive our joint retirement estimates.

Next, we unpack our joint retirement estimates through a detailed subsample analysis that allows us to explore the pathways that couples take to retire together. We investigate three potentially important dimensions of heterogeneity: age differences, gender, and relative earnings. Crucially, when conducting subsample analyses based on each of these dimensions, we use reweighting methods to control for the other two dimensions, which allows us to make more meaningful comparisons across the different groups of interest.

We document three sets of results, each corresponding to a different dimension of heterogeneity. Most striking are the results on age differences. We find age differences between couples to be a fundamental 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 route is in contrast to the less prevalent path 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.

What might explain this asymmetry between younger and older spouses? The evidence suggests that differences in economic incentives are at play. In Denmark, pension accrual incentives within the early retirement scheme disincentivize retirement before reaching pension eligibility age, which makes it more costly for younger spouses to retire when their older partners reach pension eligibility. Moreover, as is the case in most pension systems, younger spouses cannot yet claim their own pension benefits when their older partners reach the early pension eligibility age. We show explicitly that the vast majority of older spouses who adjust their behavior to retire jointly do so by claiming their own pension benefits at the same time. We then show that a significant share of younger spouses retire jointly by claiming unemployment benefits instead. Further underscoring the importance of these economic incentives, we conduct a supplementary analysis that studies joint retirement at a later pension eligibility age, where these differences in incentives are not as stark, and we find no differences between the behaviors of older and younger spouses.

We also uncover a strong gender gap at the early pension eligibility age. 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 when we use our reweighting methods to control for age differences (and relative earnings), as males tend to be the older member of couples, which confounds results from a simple comparison of males and females. As the gender gap exists after controlling for other dimensions, our findings suggest that gender norms may be playing a role.

We end our investigation of heterogeneity by exploring the role of relative earnings. We find that spouses who are secondary earners respond more to their primary earner partner reaching pension eligibility. In a deeper investigation into this difference, we find evidence that is consistent with (i) the literature on collective models of household behavior where members with more negotiation power have more weight in the decision-making process and (ii) couples considering the opportunity cost of retirement, seen as foregone earnings.

Our third and last step, after having documented and unpacked 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 6 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 et al., 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

[1]We also relate to the literature on 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, 2022; Deshpande et al., 2020; Seibold, 2021).

Lanot, 2007; Van der Klaauw and Wolpin, 2008; Casanova, 2010; Michaud and Vermeulen, 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 provide a more comprehensive heterogeneity analysis that informs on mechanisms and on the pathways that couples take to retire together. Most importantly, our analysis sheds new light on the role of age differences by showing that age differences can influence the labor market exit routes that spouses take to retire together. To our knowledge, we are one of only two papers to study exit routes, the other being Johnsen et al. (2022), who study spillovers to younger spouses only and show that some claim disability insurance to retire with their older partners. We build on their work by studying exit routes for both younger and older spouses. Moreover, our reweighting analysis emphasizes the importance of controlling for other dimensions when studying heterogeneity in joint retirement, such as by gender, an area where previous papers have found mixed results, or by relative earnings, a less-explored dimension in the recent literature.[3] Overall, we view our detailed heterogeneity analyses as altogether providing a more concrete understanding of how joint retirement plays out at early pension eligibility ages.

We make two additional contributions. First, the combination of our research design

[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. Finally, Stancanelli and van Soest (2012) and Stancanelli and van Soest (2016) study the effect of retirement on household consumption and home production, as well as on hours of leisure, respectively.

[3]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. Johnson et al., 2006 investigates the role of relative earnings in explaining gender differences but does not show differences in joint retirement distinguishing between primary and secondary earners.

and population-wide data allows us to advance estimates that are particularly precise. We have the precision to detect, in our full-sample analysis and across several of our subsample analyses, spousal spillover estimates that correspond to 1 to 2 percentage point increases in the likelihood of retirement, which are meaningful magnitudes when scaled by the direct effects on their partners. In contrast, some estimates from important papers in the literature are similar in magnitude to ours, but are less precise and not statistically significant.[4] Second, we provide estimates from a representative 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. 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 eligibility age of women was lower than that of men, a common feature of many systems in the past that has changed in recent years (OECD, 2015).

Taken together, our findings have two main implications for policy and models of household retirement. First, our results on the size of joint retirement at the early pension eligibility age can be incorporated into policy design and evaluation. 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, suggesting an absence of significant frictions that could have prevented couples from adapting their joint retirement behaviors to the new eligibility age introduced by the reform. 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

[4]For example, both Lalive and Parrotta (2017) and Johnsen et al. (2022), who each report separate estimates for male and female spouses, find interesting evidence of spillovers to female spouses, but neither paper finds spillover effects to male spouses that are statistically significant at conventional levels. However, the magnitude of the spillover estimate for male spouses in Lalive and Parrotta (2017) amounts to a 1 percentage point decline in labor force participation and that in Johnsen et al. (2022) amounts to a 3.9 percentage point decline in employment.

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.

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).[5]

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

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

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. 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. 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 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.[6]

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.[7] Note that taxation is also independent between spouses in Denmark. 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.[8]

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 main sample 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.

[6] While not a reform of VERP, between 1992 and 1996 a transitional program allowed long-term unemployed above age 55 (and above age 50 from 1994) to retire with similar conditions as the VERP program.

[7] 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.

[8] 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.

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.

Private Retirement Savings. Private savings in defined contribution retirement savings accounts make up another key source of retirement income. Similar to the U.S. setting, retirement savings accounts in Denmark can be either employer-sponsored or personal accounts. Employer sponsored plans are quite common, with roughly 85% of the workforce participating in quasi-mandatory workplace savings schemes (OECD, 2019). Contributions to retirement savings accounts are generally tax-deductible. Distributions from the accounts, which are often paid out as annuities or over fixed time periods, are taxed as income, with additional tax penalties on withdrawals of funds before age 60. Whereas the VERP program facilitates early retirements and the OAP program provides a baseline level of income support throughout the later stages of the life cycle, private retirement savings accounts finance consumption above and beyond what is provided by the public pension programs.

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.

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.

Our main focus is on retirement. We define retirement as ceasing to earn labor market income. We use our annual data to define retirement as the last year during which an individual has positive earned income, before earnings permanently fall close to zero.[9] 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), and we note that recent evidence from Sweden indicates that retirees do not reenter the workforce even after experiencing a meaningful negative income shock (Johnsen and Willén, 2022).

We also study benefit claiming and annual earnings. We define claiming as an indicator equal to one if an individual receives any pension income, either VERP or OAP, in a given year. We define earnings as total taxable labor market earnings. Our ability to study earnings is useful in that it reflects both extensive margin and intensive margin labor supply adjustments. We winsorize earnings at the 1^{st} and 99^{th} percentile to reduce the influence of outliers, we adjust for inflation using 2010 as a baseline, and we convert Danish kroner to U.S. dollars using the exchange rate 1 USD = 5.56 DKK.

We additionally use 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 Sample of Analysis

To arrive at our analysis sample, 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.1, and we show that our results are robust to relaxing this restriction in Section 4.4.

[9] 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.

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. We note that there are four cohorts of spouses that we cannot observe before age 60 to impose this 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.

In our main analysis, 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. For reference, Appendix Table A.1 shows how each of the sample restrictions impact the size of our analysis sample.

Appendix Table A.2 presents summary statistics for the analysis sample and for the unrestricted population. The first four columns correspond to the main sample, whereas the last four columns correspond to an analysis sample discussed later in the paper, when we analyze the effects of a recent reform. Note that both reference individuals and spouses in the analysis sample have higher earnings, have more education, and are less likely to be retired before age 60 than their counterparts in the unrestricted population. 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.

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. The existing literature finds that early pension eligibility ages greatly impact own retirement (e.g. Manoli and Weber, 2016; Seibold, 2021), making this context suitable for studying spousal spillovers. 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, gender and relative earnings, 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 for individuals who turn 60 at any point during the year, captured by β_2, and a sharp change in trends stemming from individuals born earlier in the year who are increasingly exposed to a longer period of pension eligibility by the time the annual outcomes are measured, captured by the slope parameter β_3.

The crucial identifying assumption is that, in the absence of pension eligibility, the outcomes that we study would have evolved smoothly across monthly ages of individuals through the calendar year that they turn 60. Our regression framework compares the evolution of outcome variables to counterfactuals based on linear extrapolations of behaviors observed just before age 60, which we illustrate graphically below. In Section 4.4, we show the robustness of our results to using a non-linear counterfactual and to estimating a model that does not rely on differential trends at all, because it is restricted to individuals born in January, who are fully exposed by the time their outcomes are measured in December.

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. It is important to emphasize that the dots for a given year (e.g. from 60 to 61) do not correspond to the same individuals observed over time with monthly frequency, but rather to different individuals born in different months but observed at the same point in time (December) at different monthly ages. Therefore, differential trends observed after monthly age 60 are not due to the effects of simply growing older, as this is already captured by the counterfactual, but rather due to the increased time elapsed between the date when an individual turns exactly 60 (and becomes eligible) and the measurement of the outcome at the end of the year.[10]

Panels (a) and (b) illustrate retirement and benefit-claiming responses. They show visually clear discontinuous changes both in levels and trends in the likelihood of retiring and the likelihood of claiming pension benefits right as individuals reach pension eligibility. Panel (c) illustrates annual earnings responses. It shows a sharp change in the trend in earnings right at age 60, which highlights how earnings rapidly decline for individuals increasingly exposed to pension eligibility.[11]

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

[10]Consider individuals who turn 60 in December. Their outcomes are measured at a maximum of 30 days after the day they become eligible for benefits, and for some as little as 1 day after they become eligible. These individuals 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 the robustness section we show that the results are largely unaffected if these individuals are kept.

[11]Note that if all individuals responded to pension eligibility by claiming benefits and retiring exactly on the day that they become eligible for benefits, then binary outcomes such as retirement and claiming would only exhibit discontinuous changes in levels, and not in trends, as there would be no difference between partially and fully exposed individuals by the time their outcome is measured in December. However, as long as there is some natural delay between becoming eligible and actual responses, or if there is some lag in measurement (i.e. if individuals still receive some earnings a few weeks or months after they have retired, or if pension benefits take some time to be paid out), then the differential trend will arise.

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 presented in Section 6. We compute standard errors for these scaled estimates by bootstrapping (Andrews and Buchinsky, 2000; MacKinnon, 2006).[12]

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, which capture both extensive margin and intensive margin responses, the

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

scaled effect is 9.8%. This estimate is larger than the scaled effect for retirement, which suggests that spouses might be adjusting along the intensive margin as well.[13] Of course, as indicated by the claiming responses, some of the decline in earnings is offset by an increase in pension benefits. We thus look at the sum of pension benefits and earnings as an additional outcome. Table 2 displays the full-exposure effects for reference individuals and their spouses. For reference individuals, the sum of earnings and pension income decreases by about $2,000, much less than the decline in earnings alone that amounts to roughly $8,600, which means that pension benefits largely offset the decline in earnings. In contrast, for spouses, the drop in earnings (about $850) does not appear to be offset much by an increase in pension benefits, as the sum of the two outcomes decreases by about $700. This analysis highlights how couples forgo income to retire together.

4.4 Threats to Identification and Robustness

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 between 1 and 3 years younger or older to the other half of the reference individuals.[14] 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.3 and Appendix Figure A.2.

Alternative specifications. We also carry out several robustness checks. First, in the spirit of the literature that studies the implementation of standard regression discontinuity designs (e.g. Cattaneo et al. 2020), we analyze the sensitivity of our results to varying two key elements of our regression specification: the estimation window and the degree of the age polynomial. We intend for our baseline specification to roughly approximate the local linear approach that is popular in regression discontinuity designs. That is, we use a linear

[13] Note that the earnings scaled effect could be larger than that for retirement even in the absence of intensive margin adjustments, if the extensive margin retirement spillover is larger for primary earner spouses. However, we show later, when analyzing heterogeneity by relative earnings, that this is not the case.

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

regression specification and zoom in close to the age 60 cutoff for our preferred estimation window of individuals with annual ages between 57 and 60.

We assess the sensitivity of our results to these choices in Figure 3. The left-hand side graphs plot, for each outcome, the estimated joint retirement scaled effects as we vary the estimation window, using the linear specification. For instance, consider graph (a), which plots estimates for the scaled effect on the retirement outcome. The horizontal axis indicates the number of monthly ages to the left of age 60 that we use to estimate counterfactual behaviors. The vertical line denotes our preferred estimation window of 36 months before age 60 (so, starting at age 57). The graph shows 42 estimates, each corresponding to a different monthly estimation window. The estimates are quite stable around 7.5% across the entire range of estimation windows considered, as well as highly statistically significant. Graphs (c) and (e) correspond to the scaled effects for claiming and earnings, respectively. The claiming and earnings estimates also do not seem to fluctuate much. Overall, when using the leading linear specification, our results do not appear sensitive to our choice of the estimation window. We then add to this robustness check by also investigating the sensitivity of our results to using a quadratic polynomial in age, instead of a linear polynomial. The right-hand side graphs report the results. Across all outcomes, the quadratic specification leads to less precise estimates that also vary more as we change the estimation window. For instance, the retirement estimate is roughly 5% around the leading window but then converges to closer to 7.5% at larger windows. The leading estimates are still statistically significant, but we note that this is not the case for the retirement and earnings outcomes at the smallest estimation windows.

Second, we analyze the sensitivity of our results to choices we made regarding the analysis sample and other specification choices. Table 3 presents the results for the scaled effects and Appendix Table A.4 presents results for the full exposure effects. 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 relax our restriction that drops couples with partners who are more than 8 years apart from each other. In row D we add additional controls, predetermined region of residence and education for both reference individuals and their spouses, to the regression. In row E, 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 F, we report the scaled effect for retirement defined as a flow variable, which allows individuals to retire multiple times (see Appendix Figure A.3

for the corresponding graphs). In row G, we report the scaled effect for earnings if we do not winsorize the variable. In row H we estimate the effects over the period 2008–2013, which is almost the same time period considered in the reform we present later in Section 6. In row I 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.

Our estimates remain statistically significant across all robustness tests presented in Table 3, and the magnitudes are mostly stable. The retirement estimates range from 6.7% to 7.7%, except for the estimate from the flow definition of retirement (5.7%). The flow definition allows for multiple retirements, rather than capturing absorbing state retirements, which leads to a weaker spillover. The claiming estimates range from 3.1% to 4.0%, except for when we restrict the sample to the most recent period, 2008–2013 (where the estimates are around 4.5%). Finally, the earnings estimates range from 9.1% to 10.4%.

5 Explaining Joint Retirement: Mechanisms and Heterogeneity

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 that leads them to make similar, although independent, choices about retirement. 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 so that utility from retirement depends on the retirement status of their partner, possibly leading to joint retirement. All of these mechanisms can be important determinants of joint retirement. Our goal is to consider which mechanism is most likely to be driving our joint retirement scaled effect estimates at the early pension eligibility age in Denmark.

First, we note that our identification strategy effectively rules out correlated preferences and common shocks as key explanatory channels. 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 spillovers that we document.[15] Next, we note that 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 note that the lifetime budget constraint should not be impacted by reaching pension eligibility age. If couples are aware of the financial effects of retiring and claiming pension benefits at various ages, we should not observe discontinuous spousal spillovers due to lifetime budget constraints. However, in the presence of liquidity constraints, reaching pension eligibility could impact joint retirement through the shared budget constraint, and it could do so in both directions. One possibility is that the shared budget constraint leads to a decrease in spousal retirement at the pension eligibility age through the impact on earnings. Specifically, we have shown that individuals retire when they reach pension eligibility. This drop in an individual's 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, indicating this channel is not driving our results.

Alternatively, the shared budget constraint could lead to an increase in spousal retirement at the pension eligibility age. If households cannot borrow against future pension wealth, then one individual reaching pension eligibility can unlock liquidity for the household, which could induce retirement of their spouse as well. To test this hypothesis, we leverage our wealth data to produce estimates of joint retirement spillovers for a subsample of couples who are not liquidity constrained. Specifically, we follow Leth-Petersen (2010) and define couples 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 4 reports the results. The sample size is smaller than our main analysis sample because our wealth data is only recorded consistently in the registers starting in 1995, so for this exercise we exclude couples where the reference individual was born before 1938, who turn 57 before 1995. Column (1) shows that the joint retirement estimate for the sample of couples unlikely to be liquidity constrained is 7.0%, which is not statistically different from

[15] 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.

our baseline estimate of 7.5%. We see this result as providing suggestive evidence against the hypothesis that the shared budget constraint is driving our joint retirement estimates, as the shared budget constraint itself should not lead to joint retirement right at the pension eligibility age for couples who do not face liquidity constraints. The evidence thus points to leisure complementarities as the key driver of our joint retirement estimates in our context.[16]

What do our findings thus imply about the relative importance of leisure complementarities compared to other mechanisms? To gain some insight, we can compare our main scaled effect estimate to the overall observed probability of joint retirement at the pension eligibility age. Our scaled effect estimate captures the discontinuous change in couples' retirement behaviors that occurs when one partner reaches pension eligibility. However, there is non-discontinuous joint retirement around the pension eligibility age as well, which our estimate does not quantify, and which can be explained by age correlations or any and all of the underlying mechanisms. Out of all reference individuals who retire at the early pension eligibility age (that is, at age 60), 13.0% have a spouse who also retires at the same time. Our scaled effect (which is driven by complementarities in leisure) thus explains roughly $\frac{7.5}{13.0} = 57.7\%$ of the total amount of joint retirement at the early pension eligibility age. We view this as a rough estimate, and perhaps a lower bound, on the explanatory power of leisure complementarities. The remaining joint retirement that is not picked up by our discontinuity design could still be explained by leisure complementarities as well, but it could also be influenced by the other channels.

5.2 Heterogeneity: The Roles of Age Differences, Gender, and Relative Earnings

We have explored why couples retire together in our setting. We now study how couples achieve joint retirement at the early eligibility age and which couples ultimately retire together. To do this, we investigate three key sources of potential response heterogeneity: age differences, gender, and relative earnings. We estimate joint retirement scaled effects for subsamples based on each of these dimensions. Importantly, when splitting the sample based on one of these dimensions, we control for the other two dimensions using reweighting methods. This facilitates more meaningful comparisons across subsamples. For example, we will be interested in comparing older spouses to younger spouses, but we need to account

[16]Here we make three points. First, we do not dismiss the role of liquidity constraints in general, but rather interpret the evidence as indicating that leisure complementarities are the primary driver of our estimates. Second, for completeness, column (2) of the table reports results for liquidity constrained households. Third, we vary the asset requirement used to define liquidity constraints and report corresponding results in Appendix Table A.5. The main conclusion—that the scaled effect for unconstrained households is similar to the scaled effect for the full sample—is not sensitive to the definition used.

for the fact that older spouses are more likely to be males and more likely to be primary earners.

Age Differences and Pathways to 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. Specifically, we split the sample into older and younger spouses, and we find that spillovers to older spouses are much stronger. Table 5 shows the results for retirement and Appendix Table A.6 presents results for claiming and earnings for completeness. Column (1) of Table 5 reports the estimates for the subsample of older spouses, that is, those who are older than their partner reaching age 60. The scaled effect for older spouses is 9.9%, which is larger than our full-sample estimate. Column (2) reports the estimates for the subsample of younger spouses, where we reweight this subsample to have the same joint distribution of gender and relative earnings as the older spouses. The scaled effect for younger spouses is still statistically significant and meaningful, but it is smaller at 4.1%. 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. Figure 4 reports the results. The largest spillovers are for spouses who are older, but not too far apart from the age of their partners. For younger spouses, we do not find evidence of differential spillovers in joint retirement as the difference in age between partners increases.

Why might older and younger spouses behave differently? On the one hand, it could be that relative age per se has its own direct effect on the joint retirement behavior of couples. On the other hand, in our setting there are important differences in the economic incentives faced by younger and older spouses when their partner reaches early pension eligibility. First, it could be that older spouses drive our estimates because they can claim their own pension benefits to replace foregone earnings when their younger partners reach the early pension eligibility age. This is in contrast to younger spouses who cannot yet claim pension benefits when their older partners reach early pension eligibility age. Second, recall that VERP disincentivizes retirements before reaching pension eligibility age by requiring claimants to be “available to the labor market” in order to transition to the early retirement program.

As leaving the labor force before age 60 can then result in the inability to claim into VERP, this regulation makes it quite costly for younger spouses who are also participating in VERP to retire right when their older partners reach pension eligibility age.

We find two pieces of evidence indicating that the differences in economic incentives are driving the differences in behaviors between older and younger spouses. First, we show that the ability to claim benefits plays a role. We show in Table 6 that a large majority of older spouses who retire jointly do so by claiming pension benefits at the same time. We estimate the full exposure effect on an indicator variable that takes the value one if an individual is retired and claims pension benefits. This estimate for older spouses is 0.022, which is 85% of the estimate on retirement alone (0.026, reported in Table 5). In contrast to older spouses, younger spouses cannot yet claim their own pension benefits when their older partner reaches age 60.

However, younger spouses may be able to replace earnings in some other way. Here we consider unemployment insurance (UI) and disability insurance (DI) as potential channels, and we note that VERP eligibility is tied to previous contributions to UI funds. While it may be difficult to claim UI or DI in order to jointly retire, there is some evidence that in the past, a significant fraction of those on UI in Denmark may not have been involuntary unemployed (Kreiner and Svarer 2022, Pedersen and Smith 1995).[17] 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 0.003, which is 43% of the full exposure effect on their retirement probability (0.007). In contrast, we find no evidence of joint retirement facilitated through claiming of disability insurance, which has been shown to be a key exit route for spouses in the context of Norway (Johnsen et al., 2022). We note that Johnsen et al. (2022) describe DI in Norway as relatively lenient and flexible compared to other programs. One suggestive interpretation of the differences between our findings in Denmark and those in Norway could be that spousal retirement spillovers are likely to operate through whichever benefit-providing program might be more lenient within a country, which could differ across contexts.

Second, we explore joint retirement at a different pension eligibility age, where the dif-

[17]Currently, the UI program is characterized by generous benefits subject to eligibility and job search requirements. See Svarer (2011) and Kreiner and Svarer (2022) for more details and discussion. Some job search criteria include meeting with case workers and maintaining a resume. We note that during our sample period Denmark began implementing reforms to arrive at these job search requirements, leading to a progressive decline in the unemployment rate (Kreiner and Svarer 2022).

ferences in economic incentives faced by younger and older spouses are not nearly as stark. Specifically, we use our same design to investigate responses of reference individuals and spouses around the Old Age Pension eligibility age of 65. The key idea is that in this setting, when reference individuals reach the OAP age, many of their younger spouses will be old enough to (i) claim their own VERP benefits and (ii) avoid retiring before reaching the early pension eligibility age, which is disincentivized by VERP. To study couples for whom this OAP age is relevant and the most likely to generate a strong first stage, we focus the analysis on couples where the reference individuals did not receive VERP benefits by age 65, and we exclude those born before July 1, 1939 (who faced a different OAP age of 67). Appendix Table A.7 presents the results. Column (1) shows that there is substantial joint retirement occurring around age 65 as well. Columns (2) and (3) show that, in contrast to our main results centered around the early pension eligibility age, the responses of older and younger spouses around the OAP age are similar (the scaled effects are 8.2% and 9.6% respectively). Overall, these two additional pieces of evidence indicate that differences in economic incentives play a key role in explaining the differences in joint retirement behaviors between older and younger spouses at the early pension eligibility age.

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 confounders between gender and other correlated dimensions, such as age differences. 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 the statistical power to estimate gender differences controlling for other factors.

Column (3) of Table 5 presents results for the subsample of male spouses. The scaled effect is 7.5% for this subsample. Column (4) presents the results for female spouses, reweighted to match the joint distribution of age-differences and relative earnings of male spouses. The scaled effect for female spouses is a much larger 13.9%, indicating that female spouses are more likely to adjust their own behavior to retire jointly with their partners than are male spouses. 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.8 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.

It is worth noting that we would have failed to uncover this gender gap if we were unable to account for other factors using our reweighting strategy. As in most countries, Danish men tend to be the older members in couples. In our sample, males are around two years older than females, as illustrated in panel (b) of Appendix Figure A.1.[18] Because we have shown that older spouses are much more likely to retire jointly, a simple comparison of male spouses versus females spouses could have led to erroneous conclusions. In fact, in Appendix Table A.9, we show estimates comparing the simple male versus female sample splits without controlling for age differences (and relative earnings), and the estimates incorrectly suggest that both genders are equally likely to adjust their behaviors.

Overall, as our reweighting strategy ensures that we are accounting for age differences and relative earnings, we view the gender gap that we find as one that suggests a role for some other factor, such as gender norms.[19] 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. 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.[20]

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

[19] Interestingly, the gender gap remains even after further reweighting based on occupation (using ISCO-08 codes). The scaled effect for females after this additional reweighting is a statistically significant 0.142, which is very similar to our baseline scaled effect for female spouses. In addition, 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. While we only observe VERP contributions for the most recent period of time and can thus only perform this test for 2008–2013, we find scaled spillover effects of 7% for males and 11.1% for females.

[20] We show this result in Appendix Table A.10 where we split the sample in four, by gender and by relative

Relative Earnings and Household Behaviors. We now focus on the role of relative earnings. To define the relative earnings of each member, 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.1. 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.

Columns (5) and (6) of Table 5 present the results, with the subsample in column (6) reweighed to match column (5). We find that secondary earners respond more to their partner reaching pension eligibility age. The scaled effect for spouses who are secondary earners is 10.7%, whereas for spouses who are primary earners, it is 7.5%. To further unpack these estimates and the role of relative earnings, we consider two additional analyses.

First, we explore relative earnings using insights from the literature on collective models of household behaviors (Chiappori, 1992; Browning et al., 1994; Browning and Chiappori, 1998; Donni and Chiappori, 2011), where members with more negotiation power have more weight in the decision-making process. Gustman and Steinmeier (2000) and Browning et al. (2021) find that males value joint leisure more than females.[21] We thus might expect more joint retirement in couples where males are the primary earner, the member with more negotiation power. We investigate this in panel A of Table 7, where we report results from estimating retirement spillovers over four different subsamples, distinguishing by whether the spouse is the primary or secondary earner and by gender (where 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).

Indeed, we find that couples where males are the primary earner are more likely to retire jointly. 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

age between partners.

[21] 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.

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.

Second, we explore relative earnings considering the opportunity cost of retirement as forgone labor market earnings. Because the cost of a primary earner retiring is greater than a secondary earner retiring, we might expect older members who are primary earners to be more likely to extend employment, and we might expect younger members who are secondary earners to be more likely to retire earlier. We investigate this in panel B of Table 7, where we report results from estimating retirement spillovers 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 (where we reweight each primary earner subsamples so that it matches the distribution of the secondary earner subsample in terms of age differences and gender).

The key differences in point estimates here are not statistically significant and are perhaps less clear, but the patterns of the estimates at face value are consistent with the opportunity cost of retirement. 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 own eligibility age while waiting for their younger partner to reach pension eligibility. 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 eligibility age to retire when their older partner reaches pension eligibility. 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 some spouses retire when their partners reach pension eligibility, and we have investigated mechanisms and heterogeneity. We now directly study a policy question of interest: what happens to joint retirement decisions when pension eligibility ages rise? In 2011, Denmark announced a reform that increased pension eligibility ages starting in 2014. A key reform component raised the early eligibility age from 60 to $60\frac{1}{2}$ for those born on or right after January 1, 1954.[22]

[22]The reform also increased the incentivized early retirement age to $62\frac{1}{2}$ and the OAP age to $65\frac{1}{2}$, but we maintain our focus on the prominent early pension eligibility age. Also, later cohorts experienced further increases in their eligibility ages (see Appendix Figure B.1). For more details on the reform and an analysis

6.1 Data and Sample of Analysis

The reform incentivizes delayed retirement by 6 months, less than one year. Therefore, we incorporate into our analysis monthly-frequency register data on earnings for all employees and on pension benefits for all recipients.[23] These data are available from 2008 and allow us to define retirements more finely than in the annual data. To measure reform-induced delays in retirement, we study a dummy variable that takes the value one if an individual works past the first half of a given year (past July 1). This accommodates the fact that those unaffected by the reform become eligible for benefits at the beginning of 2014, when they turn 60, whereas those affected by the reform become eligible 6 months later, when they turn $60\frac{1}{2}$. We study benefit claiming using a dummy that takes the value one if an individual receives pension income before July 1, and we continue to study annual earnings.

We consider the period 2008 to 2014. To focus on those more likely to be impacted by the reform, we restrict our sample to reference individuals who made qualifying contributions to VERP at least once between ages 50 and 59.[24] We also balance the sample, keeping only couples observed every year between 2008 and 2014. Our sample contains 10,321 couples and 73,395 couple-year observations. Appendix Table A.2 displays summary statistics.

6.2 Discontinuity-Based Difference-in-Differences Design

To identify the causal effects of the reform, we use a local difference-in-differences framework. The treatment group is composed of individuals born on or just after January 1, 1954, whose eligibility age increases by 6 months. The control group is composed of individuals born just before January 1, 1954, whose eligibility age is unchanged. Our baseline specification includes those born within 3-months on either side of the cutoff. We compare outcomes for the treatment group to those of the control group before and after the reform. We focus on the effect during the implementation year, 2014, as we are interested in estimating spousal spillovers that are comparable to those from the age-discontinuity design, which captures joint retirement right at the pension eligibility age.

The identifying assumption is that in the absence of the reform, spousal outcomes of treated and control individuals would have moved in parallel. We provide evidence sup-

of how it impacts savings of individuals directly affected, see García-Miralles and Leganza (2023).

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

[24]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.

porting this assumption below. Moreover, causal interpretation of the estimates relies on the assumption that spousal behaviors differ after the reform only because their partners are differentially affected by the reform. A violation of this assumption occurs if spouses themselves are directly and *differentially* impacted by the reform. By construction, treated individuals are 3 months older on average than control individuals, and so are their spouses. Therefore, because the reform is based on birth date, older spouses are more likely to be directly impacted themselves. To account for this, we control in all our specifications for spouses' own exposure to the reform, although this turns out to have no effect on the results.[25]

We first estimate a dynamic difference-in-differences model 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 reference individuals (y_{it}) or 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.

We then estimate a pooled model to quantify magnitudes. The time horizon can be split into a pre-announcement period (2008–2010), an anticipation period after announcement (2011–2013), and a post-period implementation year (2014). We thus estimate:

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

where $y_{it}^{(s)}$ is the outcome variable of interest, either for reference individuals (y_{it}) or 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 reference individuals, the coefficient β_5 identifies the causal effect of the reform on reference individuals (the first stage). When the equation is estimated for spouses, the coefficient β_5 identifies the causal effect on spouses (the reduced-form).

[25] In Appendix B.1, we discuss this issue in more detail, demonstrate that it does not have a substantive impact on our results, and carry out additional robustness checks on the regression specification.

Finally, to obtain scaled effects, we estimate a 2SLS model where retirement of the reference individual is instrumented by $treat_i \cdot post_t$. The first stage corresponds to equation (5), estimated for reference individuals. The second-stage equation is the following:

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

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

We begin by analyzing the direct effects on reference individuals. Panel (a) of Figure 5 shows the results from estimating the dynamic difference-in-differences model on retirement, and Appendix Figure B.2 shows results for claiming and earnings. The behaviors of the treatment and control groups are similar before announcement (2008–2010) as well as before implementation (2011–2013), but during the implementation year of 2014, the treatment group responds by delaying retirement. The first row of Table 8 reports estimates from the pooled difference-in-differences model and shows that the reform caused an 18.9 percentage point decline in the likelihood of being retired during the first half of the year.

Next, we study spillovers to spouses. Panel (b) of Figure 5 shows the dynamic effects for spousal retirement. Before the announcement of the reform, spouses of treatment and control individuals behave similarly, supporting the parallel trends assumption. Similarly, after announcement and before implementation, no coefficient is significantly different from zero.[26] In 2014 though, spouses of individuals affected by the reform delay their retirement, consistent with extending employment in order to retire jointly with their partner. The point estimates for claiming and earnings, shown in Appendix Figure B.2, suggest that spouses might adjust along these other margins as well, although the estimates are imprecise.

The second row of Table 8 reports the difference-in-differences estimates that quantify effects on spouses, and the third row reports scaled effects from the 2SLS model. The scaled effect on retirement is 8.8%, indicating that for every 100 individuals who postpone their retirement due to the reform, around 9 of their spouses delay their retirement as well. The spillover in claiming is 4% and the spillover in earnings is a not-statistically-significant 8.8%.

Overall, we find that the reform led to spousal spillovers that are similar to those from

[26] We note the coefficient moves slightly in 2013, perhaps suggesting a mild, though not statistically significant, anticipatory response. This is why we include an indicator variable for the anticipation years in our specifications.

the stable setting, where eligibility ages did not change and were know by couples well in advance.[27] This finding—that estimates from a stable setting can carry over well to a reform setting—is relevant for policy makers interested in predicting short-run responses to increases in pension eligibility ages. The estimates across settings did not have to be similar. For instance, labor market frictions could have prevented couples who intended to retire jointly at the previous eligibility age from retiring together at the new eligibility age.

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 underlying mechanisms and heterogeneity. We show that our discontinuous joint retirement estimates at the pension eligibility age are explained primarily by leisure complementarities, and we highlight how older spouses, females, and secondary earners respond more to their partner reaching pension eligibility than do younger spouses, males, and primary earners, respectively. We emphasize that, in our context, the particularly prominent asymmetry between the responses of older and younger spouses is likely driven by differences in economic incentives and the ability to replace earnings with one's own pension benefits.

Our results are relevant for policy makers considering reforms to pension eligibility ages. First, we note that our main estimates, which are based on a period when the early pension eligibility age remained constant, seem to carry over to a period when the pension eligibility age changed. This finding could be useful for policy makers interested in predicting responses to potential reforms that increase a long-standing pension eligibility age, as it suggests that couples do not face frictions that prevent them from retiring jointly as they would have in the absence of a reform. More generally, our heterogeneity analyses show how the demographic characteristics of individuals within couples can influence economic incentives

[27] We note that the lack of statistical precision limits our ability to deeply investigate heterogeneous responses to the reform, but Appendix Table B.1 shows that results from simple sample splits tend to go in the same direction as the results from the analogous unweighted sample splits in our main analysis.

and propensities to retire jointly at pension eligibility ages. In honing in on age differences and gender, we provide evidence on two dimensions that may be particularly relevant for countries that are still increasing pension eligibility ages of females, who also tend to be the younger member of couples, to converge to those of men.

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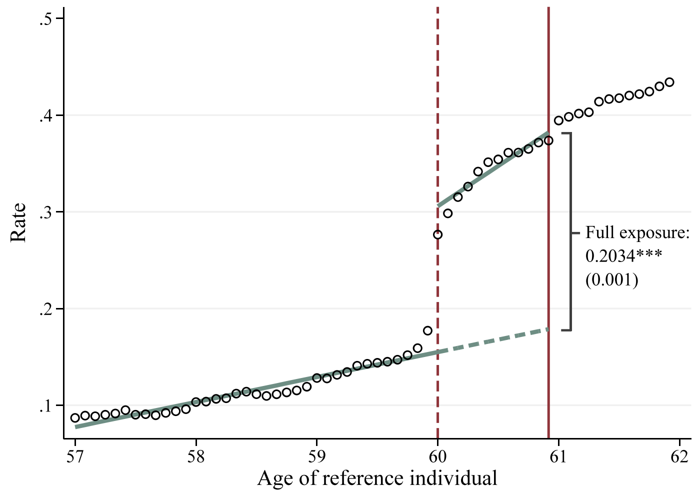
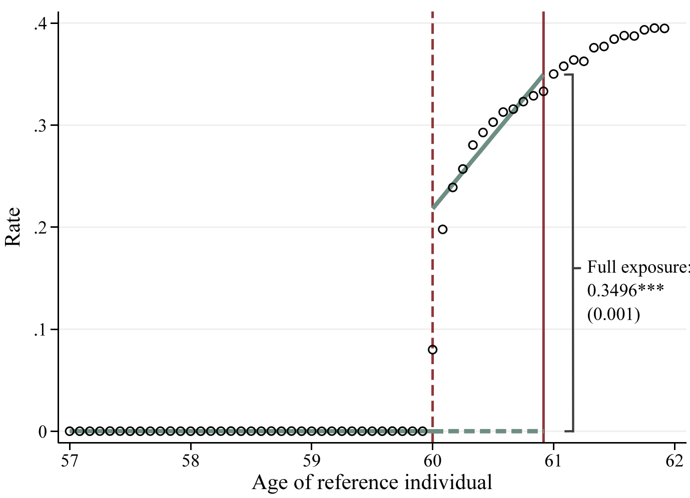
Figure 1: The Effect of Reaching Pension Eligibility Age on Own Retirement

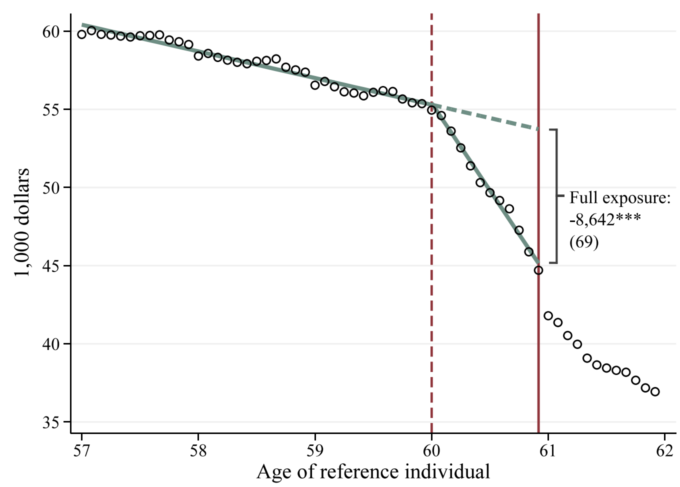
(a) Retirement



(b) Claiming



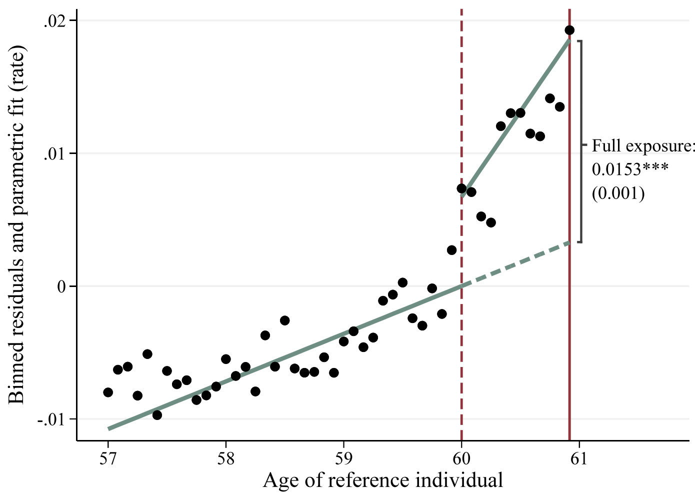
(c) Earnings



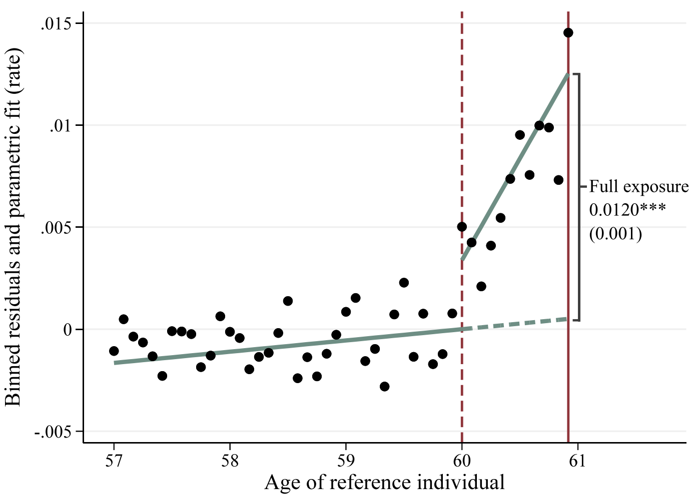
Notes: These figures plot different outcomes for individuals around their own pension eligibility age of 60, pooling individuals over the period 1991–2013. The hollow circles are raw means of the outcome variable measured at the end of each calendar year, grouped in monthly age bins. The solid lines 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

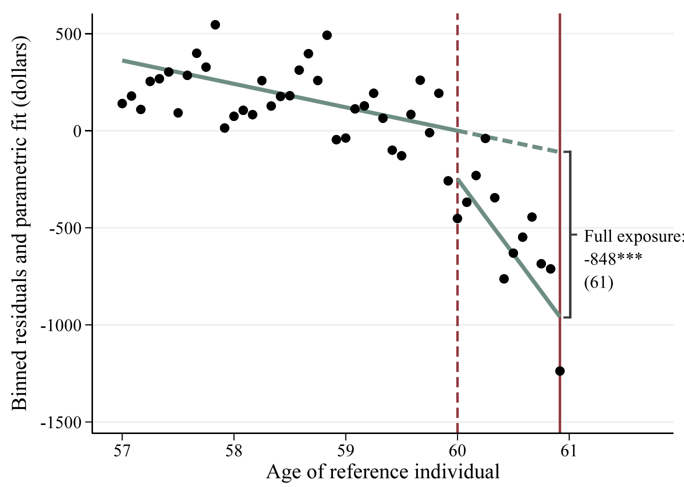
(a) Spouse Retirement



(b) Spouse Claiming

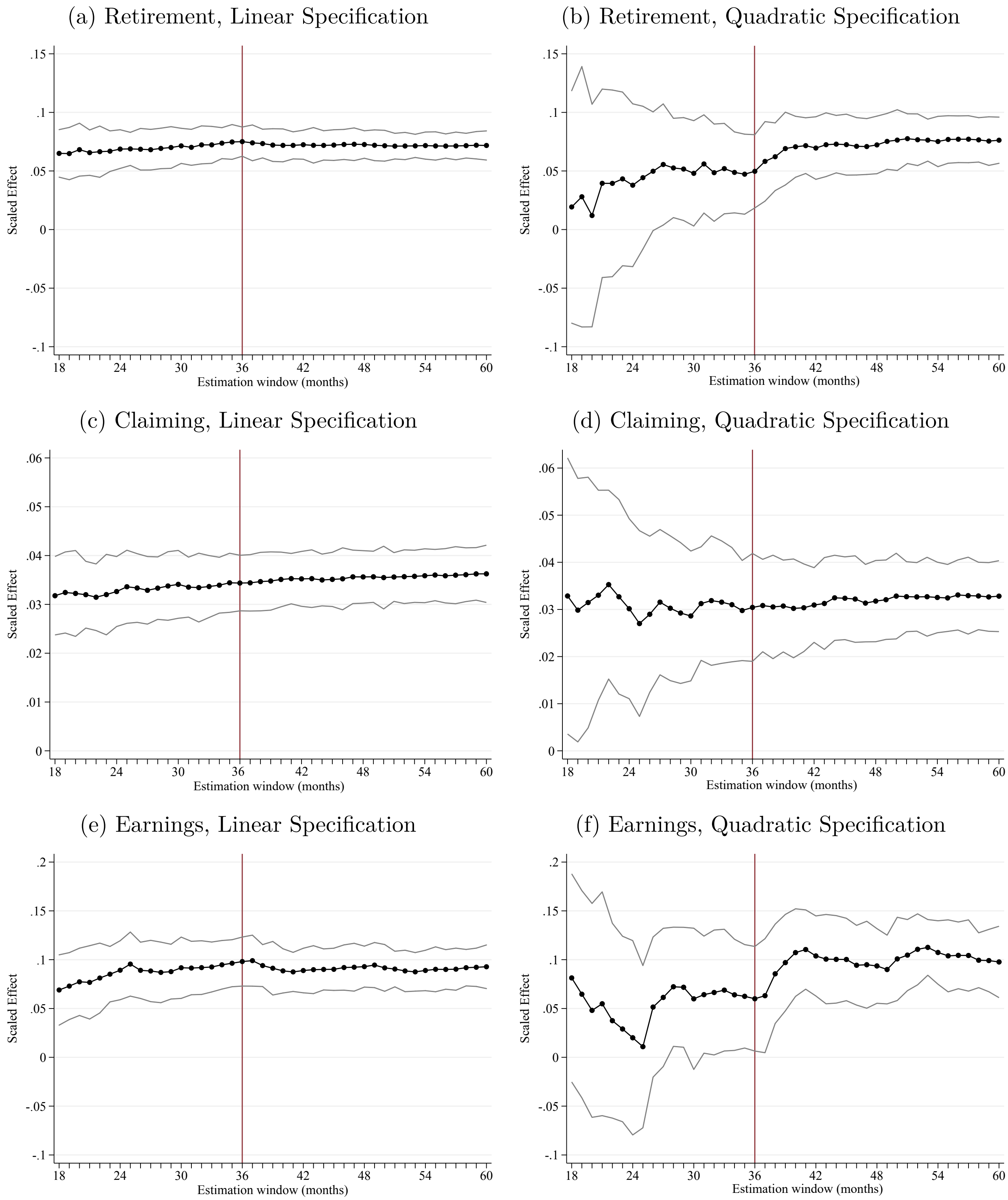


(c) Spouse Earnings



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.

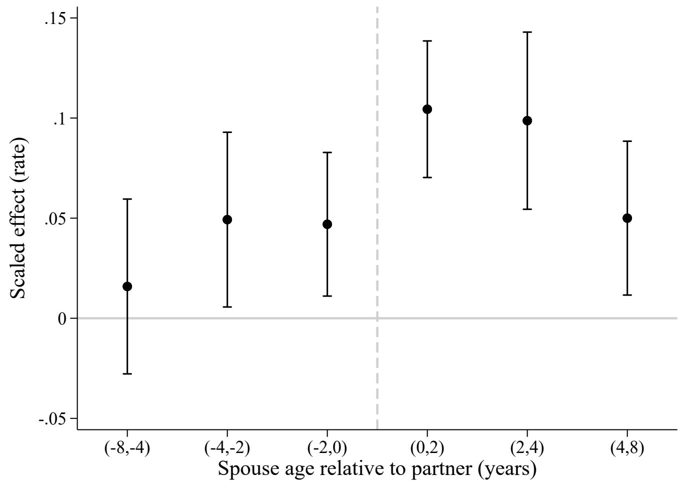
Figure 3: Robustness to Estimation Window and Choice of Polynomial



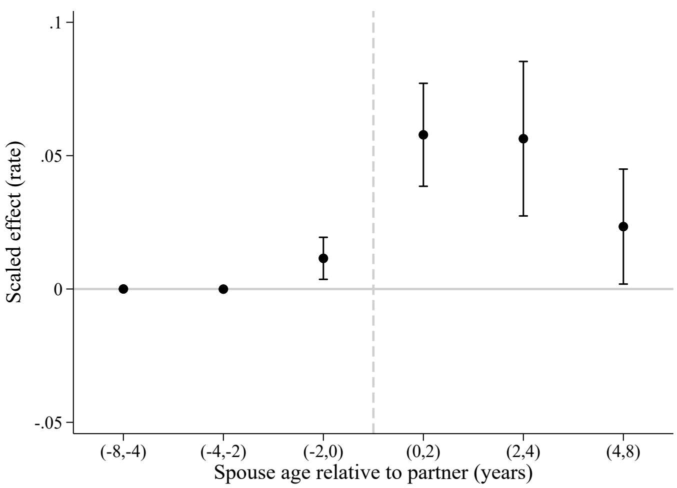
Notes: These figures assess the sensitivity of our estimates to the choice of estimation window, which is the range of ages we use to estimate counterfactual behaviors through the pension eligibility age. The left-hand-side graphs plot, for each outcome, the estimated joint retirement scaled effects and 95-percent confidence intervals as we vary the estimation window, using the leading linear specification. The right-hand side graphs use a quadratic specification. The vertical lines denote the baseline estimation window, which corresponds to individuals up to 36 monthly ages away from age 60, so those between ages 57 and 60.

Figure 4: Joint Retirement Behavior by Age Differences Within Couples

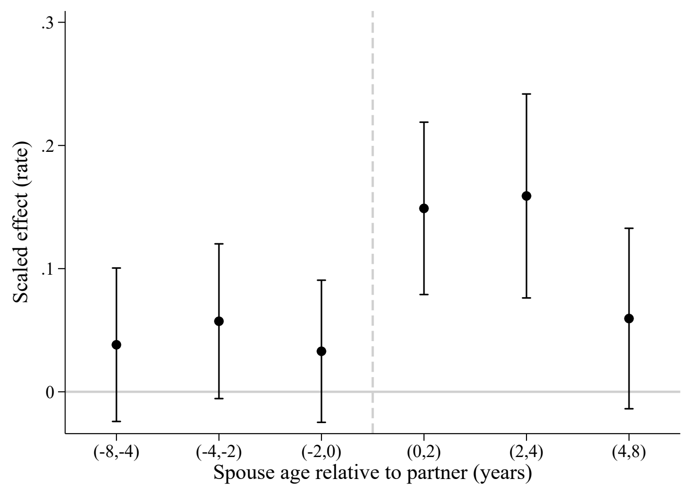
(a) Retirement



(b) Claiming

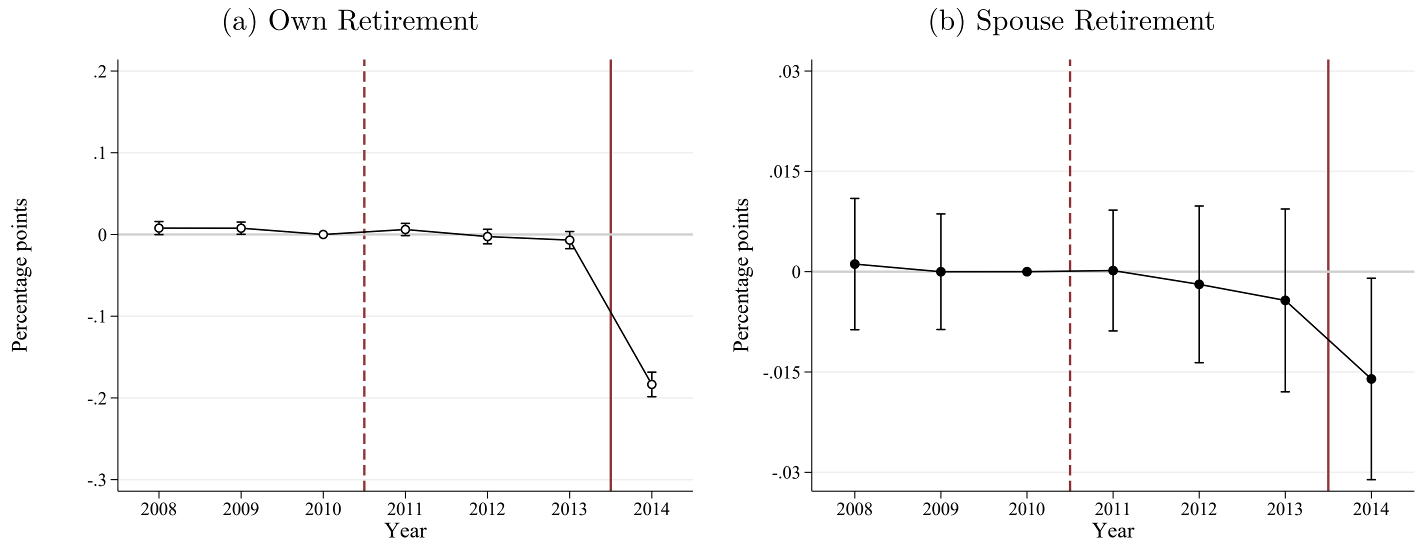


(c) Earnings



Notes: These figures plot the scaled spillover estimates of joint retirement for different subsamples of couples based on the age difference between spouses. Each granular age difference subsample is reweighted to have the same joint distribution of gender and relative income as the sample of spouses who are 0 to 2 years older than their partners, that is, the sample in the (0,2) bin in the graphs. We report 95% confidence intervals calculated from bootstrapped standard errors.

Figure 5: 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)
Spouse	0.0153***	0.0120***	-848***
	(0.001)	(0.001)	(61)
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: The Effect of Reaching Pension Eligibility Age on the Sum of Pension Benefits and Earnings

	Sum of Pension Benefits and Earnings (1)
Reference Individual Full-Exposure Effect	-2,021***
	(62)
Spouse Full-Exposure Effect	-699***
	(55)
N. of clusters	367,585
Observations	2,206,044

Notes: This table reports the effect of reference individuals reaching pension eligibility age on the sum of their pension benefits and earnings, as well as on that of their spouses. 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). 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 3: 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. Unrestricted Age Difference	0.0690***	0.0311***	0.0905***
	(0.007)	(0.0029)	(0.012)
D. Adding Controls	0.0749***	0.0340***	0.0922***
	(0.007)	(0.0031)	(0.012)
E. No Donut December	0.0736***	0.0327***	0.0987***
	(0.007)	(0.0030)	(0.012)
F. Retirement Flow Definition	0.0573***	–	–
	(0.005)		
G. Without Winsorizing Earnings	–	–	0.0999***
			(0.014)
H. Period 2008-2013	0.0758***	0.0459***	0.104***
	(0.012)	(0.0068)	(0.025)
I. 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 couples with partners that are more than 8 years apart from each other. Row D controls for predetermined region and education of reference individuals and spouses. Row E drops the dummy variable that identifies individuals who turn 60 in December. Row F reports the estimate for retirement defined as a flow. Row G winsorizes the earnings variable. Row H estimates the effect over the period 2008–2013. Row I 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 4: Heterogeneity in the Effect of Reaching Pension Eligibility Age on Retirement by Household Liquidity Constraints

	Unconstrained Households (1)	Constrained Households (w) (2)
Reference Individual	0.2108***	0.1742***
	(0.001)	(0.005)
Spouse	0.0147***	0.012***
	(0.001)	(0.003)
Scaled Effect	0.070***	0.066**
	(0.010)	(0.027)
N. of clusters	288,890	51,042
Observations	1,644,063	222,562

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, age differences and relative earnings as column (1). Robust standard errors in parentheses, clustered at the couple level. $^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.1$

Table 5: Heterogeneity in the Effect of Reaching Pension Eligibility Age on Retirement by Age Difference, Gender, and Relative Earnings

	Age Difference		Gender		Relative Earnings	
Spouse	Old (1)	Young (w) (2)	Male (3)	Female (w) (4)	Primary (5)	Secondary (w) (6)
Reference Individual	0.2562***	0.1893***	0.2668***	0.1788***	0.2423***	0.1975***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
Spouse	0.026***	0.007***	0.020***	0.025***	0.020***	0.021***
	(0.002)	(0.001)	(0.002)	(0.003)	(0.001)	(0.002)
Scaled Effect	0.0994***	0.0414***	0.0745***	0.139***	0.0747***	0.107***
	(0.010)	(0.010)	(0.009)	(0.018)	(0.009)	(0.014)
N. of clusters	297,686	334,966	302,589	330,172	300,312	332,422
Observations	1,038,096	1,167,948	1,054,359	1,151,685	1,056,592	1,149,452

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 age differences within the couple, by gender, and by relative earnings between partners. For each heterogeneity cut, the columns marked with (w) are reweighted to be comparable to the remaining sample. For example, columns (1) and (2) show a split by age difference within the couple, and column (2) is reweighted to have the same joint distribution of gender and relative income as column (1). Likewise, column (4) is reweighted to have the same joint distribution of age difference and relative income as coumn (3), and column (6) is reweighted to have the same joint distribution of age differences and gender as column (5). 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 6: The Effect of Reaching Pension Eligibility Age on Concurrent Benefit Claiming and Retirement

	Claiming & Retired		Unemployed & Retired		On Disability & Retired	
Spouse	Old (1)	Young (w) (2)	Old (3)	Young (w) (4)	Old (5)	Young (w) (6)
Spouse Full-Exposure Effects	0.022^{***} (0.002)	0.001 (0.000)	-0.0003 (0.001)	0.003^{***} (0.001)	0.0002 (0.000)	0.0004 (0.000)
N. of clusters	297,686	334,966	297,686	334,966	297,686	334,966
Observations	1,038,096	1,167,948	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. Columns (6) and (7) show the probability of spouses receiving disability insurance and being retired. Columns marked with (w) are reweighted to be comparable to the remaining sample in terms of gender and relative earnings. Robust standard errors in parentheses, clustered at the couple level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 7: 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

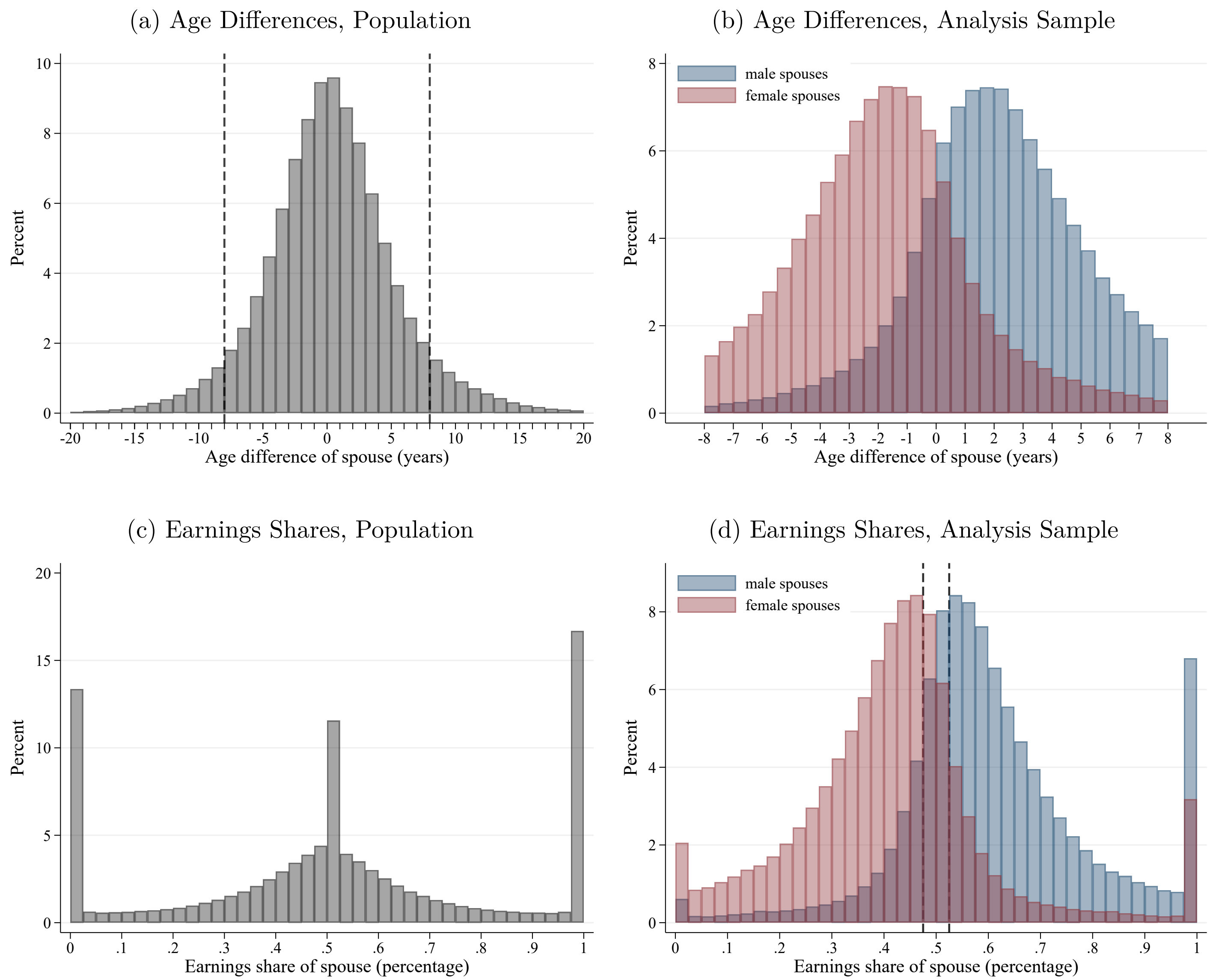
Table 8: The Effect of the Reform Increasing the Pension Eligibility Age

	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)
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

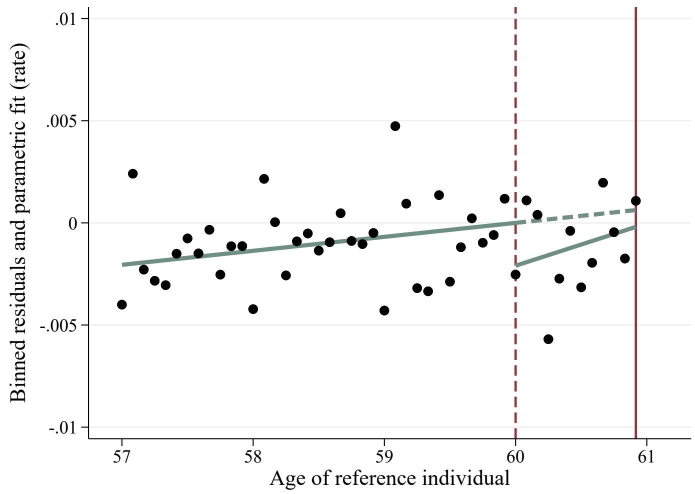
Figure A.1: Distribution of Spouses' Age Differences and Earnings Shares



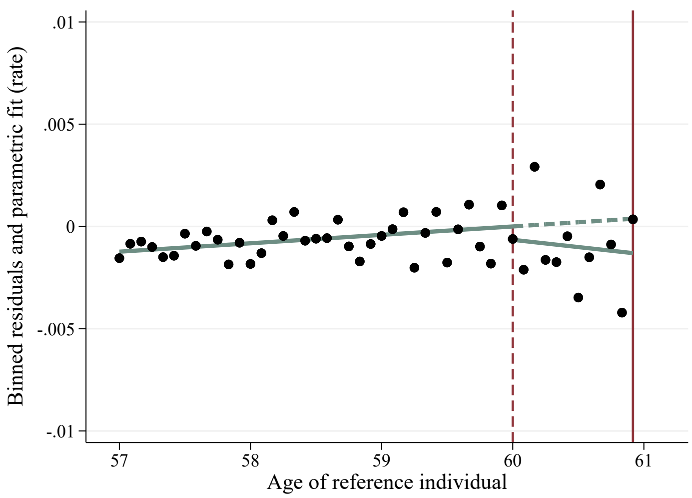
Notes: Panel (a) plots the distribution of age differences within spouses for the population of Danish couples between 1991 and 2013, before applying any sample restrictions. The vertical dashed lines mark the tails that are excluded from the sample of analysis, corresponding to couples with more than 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.

Figure A.2: Placebo Test Assigning Fake Spouses of Similar Age
for the Effect of Reaching Pension Eligibility Age

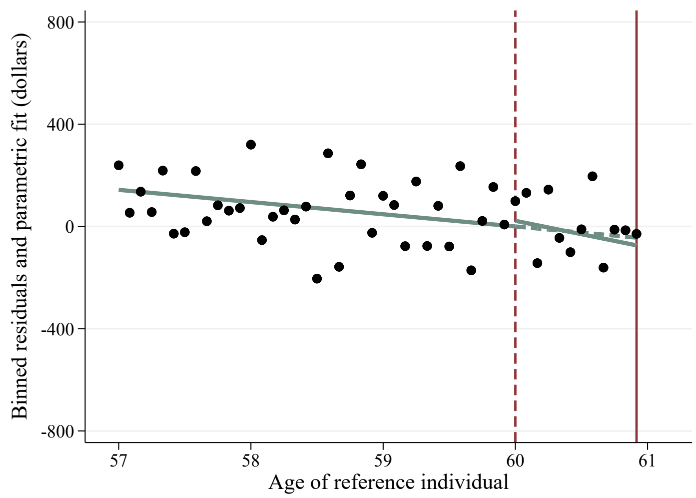
(a) Spouse Retirement



(b) Spouse Claiming



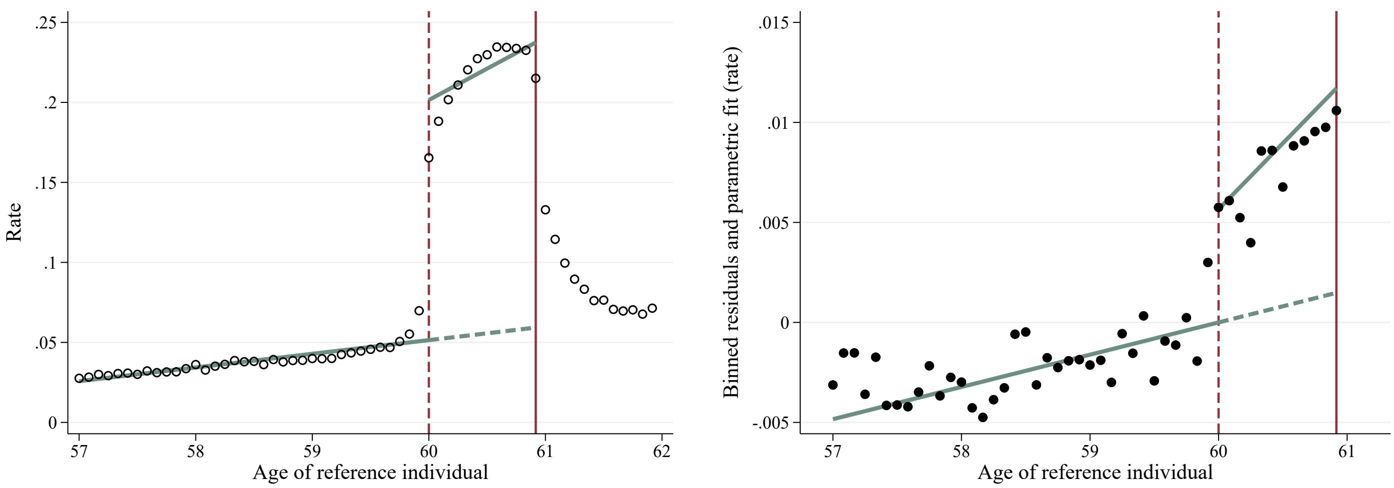
(c) Spouse Earnings



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.3 for the placebo point estimates.

Figure A.3: 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 3.

Table A.1: Construction of Analysis Sample

	Unique Couples (1)	Couple-Year Observations (2)
All couples observed between 1991 and 2014	881,911	13,798,750
Keep couples not more than 8 years apart in age	741,478	12,302,350
Require reference individuals to be employees with earned income	627,878	8,969,471
Require spouses to be employees with earned income	429,942	7,115,704
Drop year 2014, when the eligibility age increased	428,150	6,750,837
Focus on reference individuals between ages 57 and 60	367,585	2,206,044

Notes: This table documents how we construct our main analysis sample and how the restrictions that we apply impact the number of unique couples and the number of couple-year observations that we study. Each of the sample restrictions are discussed in more detail in Section 3.2. Starting with all Danish couples observed between 1991 and 2014, we impose sample restrictions to focus on dual-earner couples who are not too far apart in age, and our main analysis studies the behaviors of these couples when the reference individuals are between ages 57 and 60.

Table A.2: 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.3: 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)
Spouse	-0.001	-0.002	-30
	(0.001)	(0.001)	(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. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.1$

Table A.4: 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)	(0.001)	(0.001)	(61)
B. January-born	0.1623***	0.3332***	-8,843***	0.0125***	0.0135***	-854***
	(0.002)	(0.002)	(127)	(0.002)	(0.002)	(120)
C. Unrestricted Age Difference	0.1997***	0.3462***	-8,558***	0.0138***	0.0108***	-774***
	(0.001)	(0.001)	(67)	(0.001)	(0.001)	(56)
D. Adding Controls	0.2035***	0.3497***	-8,704***	0.0152***	0.0119***	-803***
	(0.001)	(0.001)	(66)	(0.001)	(0.001)	(59)
E. No Donut December	0.2072***	0.3673***	-8,589***	0.0153***	0.0120***	-848***
	(0.001)	(0.001)	(64)	(0.001)	(0.001)	(61)
F. Retirement Flow Definition	0.1784***	–	–	0.0102***	–	–
	(0.001)			(0.001)		
G. Without winsorizing earnings	–	–	-8,705***	–	–	-870***
			(86)			(73)
H. Period 2008–2013	0.2216***	0.2849***	-8,125***	0.0168***	0.0131***	-845***
	(0.001)	(0.001)	(141)	(0.001)	(0.001)	(132)
I. 2008–2013 & VERP Eligible	0.2468***	0.3286***	-8,910***	0.0166***	0.0143***	-929***
	(0.001)	(0.001)	(145)	(0.001)	(0.001)	(141)

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 3. Standard errors in parentheses. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.1$.

Table A.5: Robustness of Results to Varying the Definition of Liquidity Constrained

	Unconstrained Households (1)	Constrained Households (w) (2)
Liquid Wealth $\geq$ 1 month of earnings	0.070***	0.066**
	(0.008)	(0.027)
Liquid Wealth $\geq$ 2 month of earnings	0.071***	0.082***
	(0.009)	(0.019)
Liquid Wealth $\geq$ 3 month of earnings	0.066***	0.092***
	(0.009)	(0.016)
Liquid Wealth $\geq$ 4 month of earnings	0.065***	0.092***
	(0.010)	(0.016)
Liquid Wealth $\geq$ 5 month of earnings	0.064***	0.086***
	(0.010)	(0.013)
Liquid Wealth $\geq$ 6 month of earnings	0.066***	0.078***
	(0.010)	(0.012)

Notes: This table shows how joint retirement scaled effects for liquidity constrained and unconstrained households vary as we increase the asset requirement that categorizes households. The subsample of liquidity constrained households is reweighted to have the same distribution of gender, age differences and relative income as the unconstrained subsample. The first row reproduces our leading estimates, which categorizes households based on whether or not they have at least one month of labor market earnings in liquid wealth. The subsequent rows incrementally increase the asset requirement as indicated. Robust standard errors in parentheses, clustered at the couple level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.6: Heterogeneity in the Effect of Reaching Pension Eligibility Age on Retirement by Age Difference, Gender, and Relative Earnings

	Age Difference		Gender		Relative Earnings	
Spouse	Old (1)	Young (w) (2)	Male (3)	Female (w) (4)	Primary (5)	Secondary (w) (6)
A. Claiming						
Reference Individual	0.4307***	0.2993***	0.4567***	0.2835***	0.4552***	0.289***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Spouse	0.021***	0.001	0.017***	0.021***	0.019***	0.014***
	(0.002)	(0.000)	(0.001)	(0.002)	(0.001)	(0.002)
Scaled Effect	0.0495***	0.00773***	0.0374***	0.0753***	0.0369***	0.0482***
	(0.006)	(0.002)	(0.004)	(0.010)	(0.004)	(0.008)
B. Earnings						
Reference Individual	-9,558***	-7,829***	-9,081***	-9,137***	-9,103***	-8,904***
	(94)	(107)	(81)	(155)	(80)	(120)
Spouse	-1,763***	-1185***	-1,105***	-722***	-664***	-807***
	(111)	(106)	(107)	(65)	(136)	(99)
Scaled Effect	0.186***	0.154***	0.122***	0.0801***	0.117***	0.0913***
	(0.020)	(0.022)	(0.020)	(0.023)	(0.019)	(0.018)
N. of clusters	297,686	334,966	302,589	330,172	300,312	332,422
Observations	1,038,096	1,167,948	1,054,359	1,151,685	1,056,592	1,149,452

Notes: This table reports the effect of the reference individuals reaching pension eligibility age on their own benefit claiming and on their spouses' benefit claiming (panel A) as well as on their earnings (Panel B), distinguishing heterogeneous responses by age differences within the couple, by gender, and by relative earnings between partners. For each heterogeneity cut, the columns marked with (w) are reweighted to be comparable to the remaining sample. For example, columns (1) and (2) show a split by age difference within the couple, and column (2) is reweighted to have the same joint distribution of gender and relative income as column (1). Likewise, column (4) is reweighted to have the same joint distribution of age difference and relative income as column (3), and column (6) is reweighted to have the same joint distribution of age differences and gender as column (5). 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.7: The Effect of Reaching the Old Age Pension Eligibility Age

	Full Sample (1)	Older Spouse (2)	Younger Spouse (w) (3)
Reference Individual	0.2054^{***}	0.2089^{***}	0.2125^{***}
	(0.004)	(0.004)	(0.005)
Spouse	0.0197^{***}	0.018^{***}	0.021^{***}
	(0.003)	(0.004)	(0.004)
Scaled Effect	0.096^{***}	0.0821^{**}	0.096^{***}
	(0.022)	(0.033)	(0.030)
N. of clusters	91,810	91,810	91,810
Observations	346,282	346,282	346,282

Notes: This table reports the effect of reference individuals reaching the Old Age Pension eligibility age of 65 on their own retirement and on their spouses' retirement. Column (1) shows results for the full sample, where the full sample consists of couples for whom the reference individual faces an Old Age Pension eligibility age equal to 65 and for whom the reference individual did not previously make contributions to the VERP scheme. Column (2) shows results for the subsample of older spouses. Column (3) shows results for the subsample of younger spouses, reweighted to have the same joint distribution of gender and relative income as column (2). Robust standard errors in parentheses, clustered at the couple level. Boostrapped standard errors for scaled effects. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.1$

Table A.8: 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.9: Heterogeneity in the Effect of Reaching Pension Eligibility Age on Retirement by Age Difference, Gender, and Relative Earnings. Without Reweighting

	Age Difference		Gender		Relative Earnings	
Spouse	Old (1)	Young (2)	Male (3)	Female (4)	Primary (5)	Secondary (6)
A. Retirement						
Reference Individual	0.2562***	0.1588***	0.2668***	0.1479***	0.2423***	0.1710***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Spouse	0.026***	0.004***	0.020***	0.011***	0.020***	0.016***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Scaled Effect	0.0994***	0.02387***	0.0745***	0.0751***	0.0747***	0.0727***
	(0.010)	(0.011)	(0.009)	(0.013)	(0.009)	(0.012)
B. Claiming						
Reference Individual	0.4307***	0.28***	0.4567***	0.2544***	0.4552***	0.256***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Spouse	0.021***	0.000	0.017***	0.008***	0.019***	0.011***
	(0.002)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Scaled Effect	0.0495***	0.0035***	0.0374***	0.0301***	0.0369***	0.0319***
	(0.006)	(0.001)	(0.004)	(0.004)	(0.004)	(0.005)
C. Earnings						
Reference Individual	-9,558***	-7,970***	-9,081***	-8,408***	-9,103***	-8,504***
	(94)	(98)	(81)	(104)	(80)	(103)
Spouse	-1,763***	-490***	-1,105***	-570***	-664***	-685***
	(111)	(75)	(107)	(65)	(102)	(67)
Scaled Effect	0.186***	0.0633***	0.122***	0.0678***	0.117***	0.0607***
	(0.020)	(0.017)	(0.020)	(0.014)	(0.019)	(0.014)
N. of clusters	297,686	334,966	302,589	330,172	300,312	332,422
Observations	1,038,096	1,167,948	1,054,359	1,151,685	1,056,592	1,149,452

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 age differences within the couple, by gender, and by relative earnings between partners. None of the columns are reweighted. 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.10: Heterogeneity in the Effect of Reaching Pension Eligibility Age on Retirement Alternative to Reweighting: Split by Age Differences and Gender

Spouse	Old Male (1)	Old Female (2)	Young Male (3)	Young Female (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)	(248)	(167)	(114)
Spouse	-1,881***	-1,200***	-284	-583***
	(132)	(191)	(227)	(76)
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. $^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.1$

Appendix B Reform-Based Discontinuity Design

B.1 Threads to Identification and Robustness for the Reform Analysis

Spousal exposure to the reform. As discussed in Section 6, 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.

Here 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 Appendix 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.3, 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.

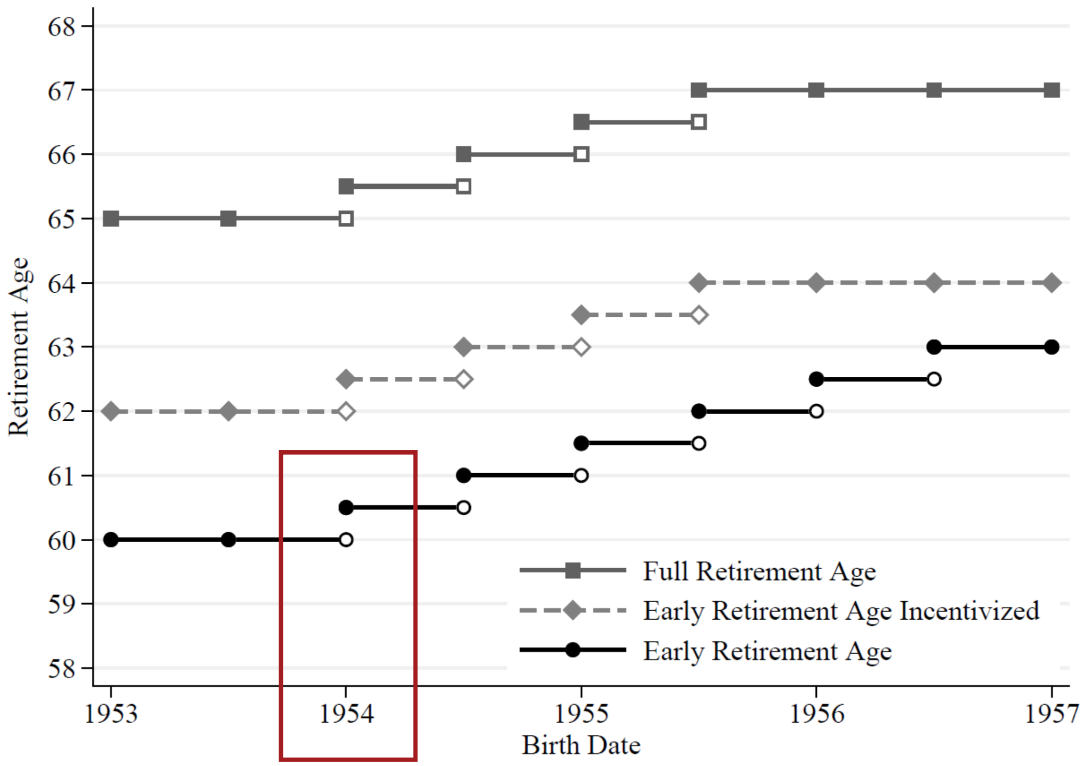
Robustness. We perform a series of robustness tests that we report in Appendix Table B.2. Row A reports the baseline estimates for comparison and Row B was introduced earlier.

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 not statistically significant in most cases.

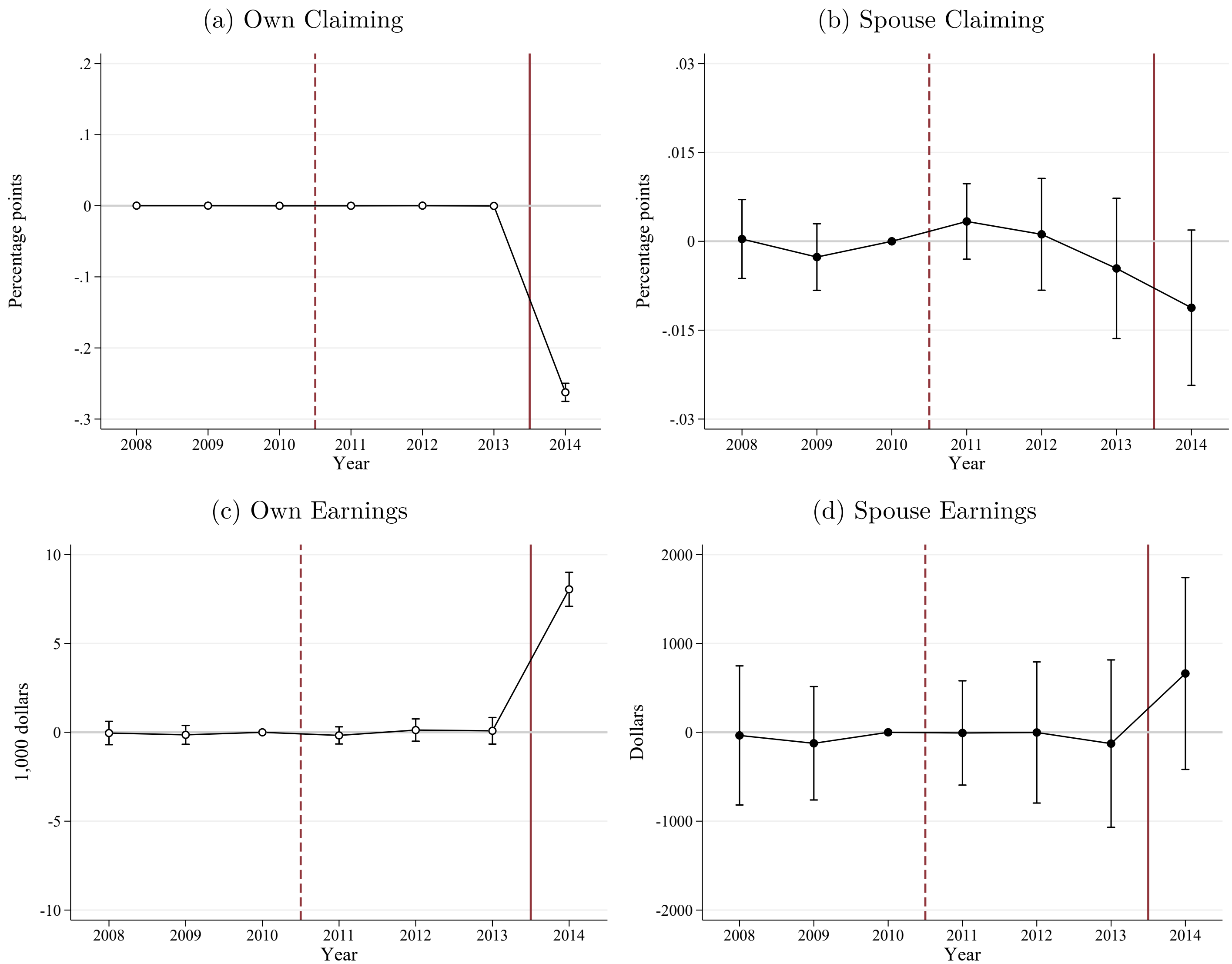
B.2 Additional Figures and Tables for the Reform Analysis

Figure B.1: Graphical Depiction of the 2011 Reform



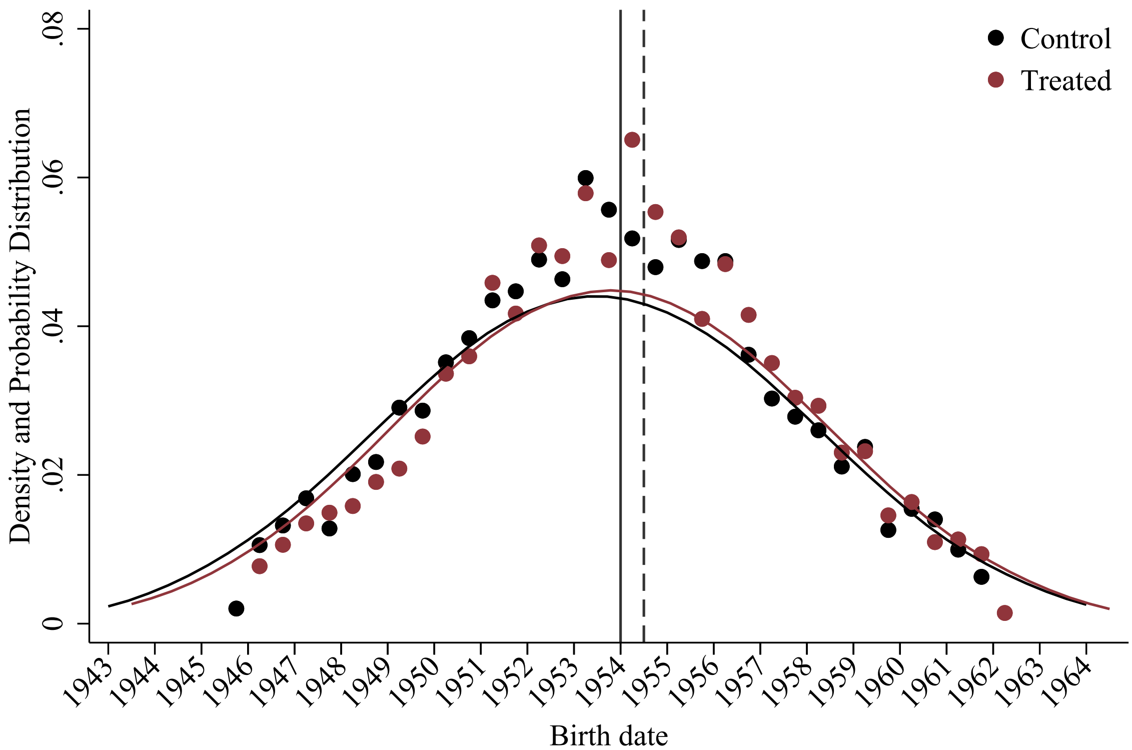
Notes: This figure depicts the 2011 reform that increased retirement ages in 6-month steps contingent on birth date. Cohorts born before January 1, 1954 were unaffected by the reform. Cohorts born between January 1, 1954 and July 1, 1954 experienced an increase of 6 months in their pension eligibility 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

Spouse	Old (1)	Young (2)	Male (3)	Female (4)	Primary (5)	Secondary (6)
A. Retirement						
Reference Individual	-0.259***	-0.118***	-0.257***	-0.117***	-0.235***	-0.141***
	(0.011)	(0.010)	(0.011)	(0.010)	(0.011)	(0.009)
Spouse	-0.0278**	-0.00370	-0.0215*	-0.0103	-0.0246**	-0.0084
	(0.013)	(0.007)	(0.011)	(0.009)	(0.010)	(0.010)
Scaled Effect	0.107**	0.0315	0.0838*	0.0877	0.104**	0.0593
	(0.049)	(0.058)	(0.044)	(0.077)	(0.043)	(0.073)
B. Claiming						
Reference Individual	-0.327***	-0.197***	-0.337***	-0.183***	-0.327***	-0.195***
	(0.010)	(0.009)	(0.010)	(0.008)	(0.010)	(0.008)
Spouse	-0.0174	-0.000219	-0.0171	-0.00276	-0.0154	-0.0052
	(0.013)	(0.001)	(0.011)	(0.007)	(0.0095)	(0.0083)
Scaled Effect	0.0534	0.00111	0.0508	0.0150	0.0472	0.0264
	(0.038)	(0.0034)	(0.031)	(0.036)	(0.029)	(0.042)
C. Earnings						
Reference Individual	10,804***	5,375***	10,580***	5,439***	8,631***	7,611***
	(668)	(690)	(610)	(740)	(621)	(722)
Spouse	1,174	75	871	369	297	981
	(912)	(540)	(870)	(568)	(831)	(626)
Scaled Effect	0.109	0.0140	0.0823	0.0678	0.0344	0.129
	(0.084)	(0.10)	(0.082)	(0.10)	(0.096)	(0.082)
N. of clusters	5,385	5,161	5,541	5,008	5,475	5,074
Observations	37,541	35,854	38,542	34,853	38,094	35,301

Notes: This table reports the effect of the 2011 reform, which increased the pension eligibility age, distinguishing heterogeneous responses by age composition, gender, and relative earnings. 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 8. 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 H does not balance the sample. Row I controls for region and education of reference individuals and their spouses. Row J 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: 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 8 for notes on the construction of this table.