The Long-Term Impact of Last-Minute Pension System Changes on Health and Labor Market Outcomes*

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Abstract

In this paper, we estimate how a decrease in private pension wealth affects workers' health and labor market outcomes by examining a Dutch pension reform that took place in 2005. Our findings indicate that the negative income effect resulting from a reduction in private pension wealth is more than compensated by a positive substitution effect − where leisure is replaced with labor − on individuals' yearly personal gross income. For men, we document effects of the pension reform on mental health related medication prescriptions in the short run, but these effects become smaller and statistically insignificant after the statutory retirement age. For women, we observe a small but statistically significant increase on the probability of death as a consequence of the new pension rules. Furthermore, this paper presents suggestive evidence that previously estimated causal effects of retirement on health derived from instruments such as pension reforms and/or minimum age discontinuities in the statutory retirement age (SRA) may (partly) consist of an income effect in addition to the labor supply effect.

Keywords: (early) retirement, mental health, chronic diseases, pension reform
JEL codes: J26, J32, I14

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1 Introduction

In this paper, we investigate how a reduction in pension wealth impacts individuals' health and labor market outcomes by analyzing a major Dutch pension reform. Because of concerns about aging and declining fertility rates, the Dutch government abolished a preferential tax treatment regarding early retirement (ER) schemes − which are retirement plans that facilitated workers to retire before the statutory retirement age (SRA) − on February 24, 2005. These rules substantially decreased private pension wealth for all individuals who were born in or after 1950 primarily by means of (i) stricter tax regulations for the accumulation of ER pension and (ii) the introduction of a (higher) penalty for retiring before the SRA. This paper studies the (long-run) effects of this pension reform on health, labor market participation, and income using rich administrative data.

As a consequence of the reform, individuals who were born before January 1, 1950, could retire from age 55 onwards while receiving generous early retirement benefits, whereas individuals who were born after January 1, 1950, did not have this option anymore (Baars, 2008). Our identification strategy relies on the discontinuity in pension treatment determined exclusively by an individual's birth date: we assign individuals to the treatment or control group based solely on whether they were born four months after or before January 1, 1950, respectively. We then employ a difference-in-differences research design in which we compare average outcomes of the treatment and control group across age and argue that both groups' average outcomes were comparable before age 55 and would move in parallel afterwards in absence of the reform.

There are several advantages of our research design. First, using rich administrative data from *Statistics Netherlands* on an individual's medication prescriptions and various income sources, we can track individuals' outcomes for both the treatment and control group starting from around age 50 (with some variation across outcomes) until age 70. This enables us to estimate both the short and long-run impacts of this reform while additionally accounting for unobserved age-invariant individual heterogeneity. Second, by leveraging the fact that this pension reform affects an individual's probability to retire at various ages, we are able to disaggregate the impact of this policy throughout the distribution of realized retirement ages. Specifically, we investigate the impact of reduced pension wealth on those who retire before age 55 or after the statutory retirement age (SRA) of 65 years and 3 months. This approach allows us to isolate and analyze health attributable to early or late retirement apart from diminished pension benefits, thereby providing insights into the relationship between financial security and health.

We document large positive labor participation effects between age 60 and the SRA as a consequence of the reform. Our results show that the probability of being retired at or before the SRA is approximately 12 percentage points (7 percentage points) lower for men (women) who were subject to the new pension rules, as individuals are incentivized to substitute leisure with labor due to lower replacement rates. Furthermore, we find that the stricter pension rules had a statistically and economically positive significant impact on personal gross income across nearly all age groups we examined. Following the SRA, this effect is equal to approximately 700 euros (500 euros) for men (women) per year, indicating that the new pension rules had a lasting positive impact on individuals' yearly personal income in old age.[1] All in all, our findings suggest that the negative income effect from reduced pension benefits due to the reform is more than offset by a positive substitution effect – of leisure with labor – on personal income.

We also document detrimental health effects as a consequence of the new pension rules. For men, we estimate an increase in medication prescriptions related to mental health conditions right after the reform got introduced at age 57. This result is in line with Grip et al. (2012) who study the short-run mental health effects of this reform for a subsample of male public sector workers. Furthermore, we find that the estimated effects on mental health become smaller and statistically insignificant after the SRA. For women, we observe a statistically significant but small increase in mortality outcomes as a result of the reform: women in the treatment group are, on average, approximately 0.46 percentage points more likely to have died by the age of 65 compared to those in the control group.

Supporting our research design, we do not find any economically significant differential pre-reform age trends in retirement and mortality. Furthermore, we conduct two placebo tests: one using individuals born in 1950, all of whom are subject to the new pension rules, and another using individuals born in 1949, who are all subject to the old pension rules. By assigning individuals in both cohorts to treatment or control groups based on a placebo birth-date threshold, we find no significant effects in either case. This suggests that there are no other confounding shocks to labor or health outcomes in the treatment and control group, further supporting the validity of our identification strategy.

We document heterogeneity of our main results. For the subsample of early retirees – defined as all individuals in both the treatment and control group who retire before age 55 – we report coefficient estimates that closely align with those reported in Grip et al. (2012). This result indicates that prescriptions for mental health-related medications between ages 57 and 60 are likely linked to the anticipation of significantly reduced pension benefits, rather than to leaving the labor force after age 55 as a consequence of the new pension rules. Moreover, we find larger effects on labor market outcomes for high-wage individuals compared individuals in the lower tail of the pre-retirement wage distribution. This is in line with the empirical fact that a relatively larger portion of high-wage workers' total pension wealth is derived from private pension wealth as opposed to public pension provision, thereby making high-wage workers relatively more affected by the new pension rules (Knoef et al., 2016). Our heterogeneity analysis also reveals that there exist substantial differences in the effects of the reform between private and non-private sector workers. For example, our results suggest that the mental health effects for men and the mortality effects for women hold for private sector workers only.

[1] For women, this approximately amounts to 41 euros per month extra, which is approximately equal to 2% of the monthly gross benefits received out of state pension for single individuals in the Netherlands, making this effect economically small.

This paper contributes to several strands of literature. First, we add to a large literature investigating the effect of a change in pension income or other types of social security on labor market outcomes. For labor participation, the literature documents a consistent positive effect of a decrease in pension benefits and/or other types of social security payments (Mastrobuoni, 2009; Manoli and Weber, 2016; Krueger and Pischke, 1992; Krueger and Meyer, 2002; Samwick, 1998; Fetter and Lockwood, 2018). Our results are consistent with this evidence. However, for income and earnings, the effect of this type of pension reform is both theoretically and empirically ambiguous – as the direct income effect and the substitution effect operate in opposite direction – and empirical evidence on which effect dominates is scant and mixed. For example, Gelber et al., 2016 show that a cut in Old Age and Survivors Insurance (OASI) benefits increased elderly earnings, which aligns with our findings. Fetter and Lockwood (2018) document large labor force participation effects as a consequence of the introduction of Old Age Assistance (OAA) programs in the US, in combination with small welfare costs to society as a consequence of implicit taxation on earnings. Recently, Börsch-Supan et al. (2024) show that in the German context – of the introduction of pension reform that decreased pension wealth – the substitution effect is economically small (but statistically significant), and the income effect tends to dominate.[2] One possible explanation for the discrepancy between our results and the latter two studies is that the reform we examine specifically targeted high-income individuals (Knoef et al., 2016).

Second, our results complement studies that estimate the causal effect of retirement on health.[3] Most of the existing studies estimate the short-term effect of retirement on health using minimum age eligibility to receive pension benefits as exogenous variation in a standard regression discontinuity design (Eibich, 2015; Gorry et al., 2018; Fitzpatrick and Moore, 2018; Bonsang et al., 2012; Neuman, 2008; Charles, 2002; Insler, 2014; Eibich, 2015).[4] Such thresholds that depend on age are a valid instrument for an individual's retirement status if all other (unobserved) determinants of health behave smoothly around this cutoff age. However, as pointed out by Heller-Sahlgren (2017), this potentially assumption is violated if receiving pension income is discontinuous around the considered age eligibility threshold. By examining a large pension reform that affects retirement-take up across a range of ages instead of a single eligibility threshold, we are able to estimate both the short and long-run effect of lower pension wealth for a subset of workers who all retire around the same age. Our findings suggest that a (foresight

[2] Börsch-Supan et al. (2024) call the substitution effect a "behavioral effect" and the income effect a "mechanical effect".

[3] The evidence on the sign of this effect is mixed: whereas some studies find a positive effect of retirement on health (Grip et al., 2012; Eibich, 2015; Bloemen et al., 2017), other find a negative effect (Mazzonna and Peracchi, 2017; Bonsang et al., 2012; Fitzpatrick and Moore, 2018; Behncke, 2012), or no effect at all (Coe and Zamarro, 2011).

[4] For example, Eibich (2015) uses a regression discontinuity design that exploits financial incentives to retire in Germany to estimate the local average treatment effect (LATE) of retirement on health. In the first stage, they find that crossing the minimum age for receiving pension (i.e., age 60) increases the probability of being retired by approximately 18 pp. They then inflate the difference in health outcomes near this age threshold by a factor of 5 and interpret this estimate as the LATE of retirement on health.

to a) reduction in pension wealth significantly affects mental health outcomes for men. This implies that previous studies estimating the health impact of retirement using the minimum eligibility age for receiving pension benefits may have overstated its true effect, possibly due to an income bias.

Third, we add to the literature on the role of income in explaining old-age health. Several studies suggest that a change in income after retirement explains health post-retirement (Bonsang and Klein, 2012; Bertoni and Brunello, 2017; Kesavayuth et al., 2016), and that exogenous negative shocks in income transfers or pension programs deteriorate health later in life (Case, 2004; Huang and Zhang, 2021; Jensen and Richter, 2004; Golberstein, 2015). Our heterogeneity analysis by retirement age is in line with this empirical evidence as our results indicate that (the prospect to) a decrease in pension wealth, in absence of labor force participation effects, deteriorates health.

Beyond the broader relevance of our paper for understanding the health and labor market effects of retirement and the provision of pension wealth in a general context, we are also the first to study the long-run consequences of the 2005 pension reform. Specifically, we utilize rich administrative data records, including both men and women, as well as public and private sector workers, whereas the previous literature has predominantly focused on male public sector workers (Grip et al., 2012; Lindeboom and Montizaan, 2020; Li et al., 2016; Montizaan et al., 2010).[5] We document substantial heterogeneity in the effect of the reform on health, labor force participation and gross personal income across sector, gender and pre-retirement wage level.

The rest of this paper is organized as follows. Section 2 presents the Dutch pension system and the pension reform. Section 3 describes the data, and Section 4 presents the empirical framework. Section 5 presents our main results. Section 6 presents the robustness checks, and Section 7 presents our heterogeneity analysis. Section 8 concludes.

2 Institutional setting

Section 2.1 provides a general background on the Dutch pension system. In Section 2.2, we describe the reform in 2005 that generated financial incentives in the collective pension system that made it unattractive to take up early retirement for individuals who were born in 1950 or later. Section 2.3 discusses the previous literature that exploits the same natural experiment as a consequence of the 2005 reform, which also forms the main source of exogenous variation in this paper.

[5] One recent study that also utilizes Dutch administrative data records on the full population to estimate social multiplier effects of this pension reform is Oral et al. (2024).

2.1 The Dutch pension system

The Dutch pension system consists of three pillars. The first pillar consists of a flat-rate old age pension. It provides a basic pension income (i.e., the benefit is related to the net minimum wage) starting from the statutory retirement age (SRA) for everyone who lived in the Netherlands for 50 years before reaching the SRA.[6] This pension is provided regardless of whether the person worked during their residency. In 2006, the SRA was equal to 65, and has been increased by the government as from 2013 (Rabaté et al., 2024). The first pillar operates on a system where the pension benefits are funded by income taxes collected from the working population (i.e., pay-as-you-go principle). For all individuals in our main sample (i.e., individuals born around January 1, 1950), the SRA is equal to 65 years and 3 months. It is not possible to claim pension benefits from the first pillar before the SRA. Most employment agreements automatically end at the SRA.

The second pillar involves pension plans that are linked to a person's earnings within specific industries or sectors. Accrual depends on the past employment duration, full-time wage, and the number of hours worked in terms of full-time hours. It is mandatory to participate in the second pillar for all employees in the Netherlands by the *Dutch Pensions and Savings Act*. Moreover, until 2005, private- and public sector schemes allowed employees to retire from age 55 onwards with a substantial pension benefit through early retirement (ER) schemes. From the 1990s onwards, this benefit comprised of two components for most sectors: a *flat-rate benefit* − which was unaffected by work history − and a so-called *accrued benefit* − which was contingent on an individual's work history. The total benefit amount was determined by adding these two components and multiplying them by a retirement age-specific factor. This factor served to penalize if an individual retires before the so-called *early retirement age* (ER age) − which was equal to 62 for the majority of sectors − or to incentivize working beyond the ER age. Employees could claim the ER benefit between the ages of 55 and the SRA through these ER schemes, otherwise they would forfeit the preferential tax treatment that generated pension benefits before the SRA.[7] Consequently, ER became the standard practice (Lindeboom and Montizaan, 2020; Euwals et al., 2006).[8]

The third pillar comprises savings that individuals choose to accumulate in addition to their public and occupational pensions. These pension products are provided by private insurance companies or banks and usually result in regular payments known as annuities at the SRA. Because the Netherlands has a strong first and second pillar, the third pillar is mostly used by

[6] If individuals have been living in the Netherlands for a shorter period, they receive a fraction of this basic pension income.

[7] For example, the system for contributing to public sector schemes allowed a public sector worker who had dedicated 40 years of service to retire at the age of 62 and 3 months. At that time, they would receive a pension equivalent to 70% of their average annual earnings before retirement (Euwals et al., 2006).

[8] Before 2005, around 80% of workers retired when they were 62 years old or even younger, while only 6% retired at the standard retirement age of 65 (Statistics Netherlands, 2009).

the self-employed.[9]

2.2 The history of the second pillar

Before the 1990s, the pension schemes in the sector pillar consisted of a flat rate component only (*Dutch:* VUT component). These schemes were actuarially unfair and very generous. In particular, if individuals stopped working before the ER age, they would not receive any benefits. Moreover, if persons continued to work after the ER age, they would lose the right to these benefits and would not be able to claim them later. For this reason, ER became standard practice. Because of concerns about the sustainability of the pension system due to demographic changes, the Dutch government, in collaboration with the Dutch labor unions, reached an agreement to reform the existing ER schemes in the early 1990s by including an accrued component (*Dutch:* pre-pensioenen) as described in Section 2.1 (Euwals et al., 2006). For public sector workers, this new scheme got introduced on April 1, 1997 (*Dutch:* Wet Flexibel Pensioen en Uittreden), followed by the health care sector on January 1, 1999 (Euwals et al., 2010).

To accelerate this transition to an actuarially fair pension system, the Dutch government introduced a reform on February 24, 2005, that substantially decreased second pillar pension wealth for all individuals who were born in or after 1950 through new tax rules (Baars, 2008).[10] These new pension rules work as follows. For all individuals, both before and after the reform took place, it holds that ER claims are subject to a standard income tax. For individuals born before 1950, the contributions paid by their employer to ER schemes are not subject to any tax and the employee contributions to the ER schemes are tax deductible. Both of these preferential tax treatments for ER schemes do not hold for individuals who were born in 1950 or later. All in all, this reform had five main implications for the younger cohorts: (i) a drop in pension benefits, (ii) an increase in the pension contribution rate (from 1.75% to 2% for public sector workers for example), (iii) an increase from the minimum eligibility age to claim pension from the second pillar from 55 to 60 years, (iv) the introduction of bonuses in case an individual starts claiming their pension after the SRA, and (v) the introduction of higher penalties on an individual's pension income when claiming pension benefits before the SRA.[11] As such, the latter two changes introduce an actuarially fair adjustment of pension benefits, thereby reducing individuals' monetary incentive to retire before the SRA.

For example, for public sector workers, the consequences of this reform were that individuals who were born before 1950 could benefit from a replacement rate of 70% of their average yearly earnings if they retire at age 62 and 3 months, whereas this replacement rate dropped

[9]For example, in 2007, it made up only 5% of the total retirement income on average (Bovenberg and Gradus, 2015).

[10]For ER plans that already existed on December 31, 2004, the new pension rules were implemented an January 1, 2006.

[11]Note that this age-specific factor also existed for individuals born in 1949, but then the so-called pivotal age for which an individual got penalized if claiming pension benefits before that age was 62 instead of 65 (Lindeboom and Montizaan, 2020).

between approximately 61% to 65% for individuals born after January 1, 1950, depending on their earnings (Grip et al., 2012; Euwals et al., 2006). The reform also affected private sector workers. For every age between 55 and 70, Lindeboom and Montizaan (2020) show that individuals' present pension wealth − which is a function of pension benefits, survival probabilities, pension contribution rates, and a discount factor − is lower for individuals born in 1950 versus 1949, and the drop is the largest for the highest income percentile.

Workers who were born around January 1, 1950, are approximately 55 years old at the time of the implementation of the new pension rules. Therefore, individuals born just after December 31, 1949, were faced with a substantial reduction in pension wealth after age 55 as compared to individuals born just before 1950. This strong differential treatment based on an individual's day of birth serves as our main source of exogenous variation in an individual's retirement age, (post-retirement) income, and long-run health outcomes.[12]

2.3 Previous literature

We are not the first to study the consequences of this pension reform on health and labor market outcomes. For example, Lindeboom and Montizaan (2020) show an increase in the labor supply for a subsample of male public sector workers, especially those who did not earn a wage that was high enough to compensate for the consequences of this reform via additional savings. Moreover, Montizaan et al. (2010) show that this exogenous shock to less generous pension rights significantly increased participation of older employees in training courses in large organizations, as the gains on investment in human capital are larger for individuals with lower prospects to early retirement. The results documented in Li et al. (2016) show that affected workers were less likely to transition into self-employment. We contribute to this literature by examining the labor market consequences of a reduction in pension wealth on labor force participation and personal income for the entire Dutch population. We therefore can examine the trade-off between substitution effects and income effects as a consequence of lower prospective pension wealth.

For health outcomes, Grip et al. (2012) document worsening mental health conditions for a sample of male public sector workers affected by the reform when they were approximately 58 and 59 years old (i.e., three years after the reform got implemented). In this paper, we inves-

[12]One potential threat to our identification strategy is the possible presence of other reforms that would differentially affect the treatment and control group. There exists a couple of other social security reforms that took place around the same time as the reform we study. First, the SRA was gradually increased from 65 to 67 years (Rabaté et al., 2024). Like the reform we study, the new SRA also depends on an individuals' date of birth. However, none of these thresholds co-occur at January 1, 1950. In fact, we construct our treatment and control group such that all individuals face the same SRA. Second, at the same time of the 2005 pension reform, the Dutch government also introduced a tax-facilitated savings plan (*Dutch:* Levensloopregeling) that offered a significant savings subsidy for individuals in both the treatment and control group until 2015. Lastly, the government introduced tax benefits for working at old age between 2012 and 2018, which impacted both individuals in the treatment and control group in our study. As such, we are confident that our results are not caused by other changes in the social security system (Oral et al., 2024).

tigate the long-term effects of this reform on various (mental) health medication prescriptions and mortality using rich administrative data on men and women.

Recently, Oral et al. (2024) have exploited this pension reform and similar data sources to investigate the role of family members, neighbors and coworkers on individuals' realized retirement age using Dutch administrative data on the entire population. Their results show large social multiplier effects in retirement, primarily as a consequence of women reacting to the retirement status of their partner. To address potential treatment heterogeneity on the basis on marital status, we perform separate analyses on the subsamples of unmarried and married individuals.

3 Data

We use administrative data from *Statistics Netherlands*. All monetary values are measured in 2015 euros, adjusted for inflation using the consumer price index (CPI).[13]

3.1 Sample selection

We construct a panel data set for individuals born four months before and after January 1, 1950. We track individuals' labor market participation from 1999 onwards. All individuals born in this period face the same SRA of 65 years and 3 months (Rabaté et al., 2024). We further restrict our sample to individuals who have a received income from employment for at least one month from 1999 onwards, as these individuals were able to participate in the collective pension system and therefore could receive pension benefits before the SRA at a reduced rate.[14] We use this sample to estimate the effect of the reform on mortality, which consists of 115,770 individuals. To analyze labor market outcomes and medication prescriptions, we further restrict our sample to individuals who are still alive at age 70. This subsample consists of 95,930 individuals.[15] Throughout the rest of the paper, we assign individuals who were born after December 31, 1949, to the treatment group and individuals who were born before 1950 to the control group.

3.2 Variable definitions

Retirement. To measure an individual's retirement age, we make use of the beginning and end date of various income sources. We construct a binary indicator variable that is equal to one if an individual does not receive any income from (self-)employment after a certain age, and zero otherwise. This follows a common definition of being retired, which entails the state of not actively participating in the labor force (Insler, 2014; Lazear, 1986; Bonsang and Klein, 2012).

[13]The CPIs used in this paper can be found here: `https://www.cbs.nl/nl-nl/reeksen/tijd/consumentenprijzen`.

[14]This implies that we restrict our sample to persons who had a job when they were at least 50 years old.

[15]A large part of the literature that examines the long-run effects of old-age labor supply on health outcomes restricts the sample to individuals who are alive at the time health outcomes are observed as often researchers make use of survey data (Fé and Hollingsworth, 2016; Heller-Sahlgren, 2017).

For robustness, we also present our main results in case we define retirees as individuals who receive pension benefits (i.e., receiving pension income from the first, second, or third pillar and pension benefits from abroad).[16]

Income. We measure yearly total gross income at the personal level between ages 54 and 70 for every individual.[17] A person's gross income consists of primary income received from employment, including unemployment benefits, and other social security benefits (i.e., pension benefits, disability insurance benefits, unemployment insurance benefits). We drop individuals whose personal gross income is strictly negative, as these individuals are generally associated to be relative wealthy and therefore their yearly personal income is generally perceived as uninformative. For robustness, we also transform the level of income x with the monotone transformation $\log(1 + x)$, $\arcsin(x)$, $\log(100 + x)$, and $\log(1000 + x)$. For our heterogeneity analysis, we also measure an individual's average yearly income from employment for the period between 1999 and 2020 during which individuals are employed.[18]

Health. We use yearly data on medication prescription provided by the *National Health Care Institute* from 2006 onwards, and classify these medications into (chronic) diseases according to the Anatomic Therapeutic Chemical (ATC) code.[19] We use the mapping of ATC4 codes to (chronical) diseases as presented by Van Ooijen et al. (2015), Lamers and van Vliet (2004), and Chini et al. (2011), leading to ten different classifications of diseases and medical conditions (see Table A.1 in the Appendix). For mental health, drugs with the ATC4 code "N05B" (anxiolytics), "N06A" (antidepressants), "N05A" (antipsychotics), and "N06B" (nootropics) belong to medical conditions prescribed in case of anxiety and/or depression. At every age, we construct a binary indicator variable that is equal to one in case an individual has at least one medical condition at that age, and zero otherwise. To ensure consistency with the existing literature on the relationship between old-age labor market outcomes and health, we analyze chronic (physical) diseases (i.e., having at least a medication prescription for coronary or cardiac dis-

[16] In general, there exist three definitions of being retired in the literature, as discussed by Insler (2014): (i) receiving pension benefits, (ii) self-reported retirement status, and (iii) not being active in the labor force. In this paper, we adopt the first and latter definition as we use administrative data, precluding access to an individual's self-reported retirement status.

[17] We are only able to measure an individual's personal income at a yearly basis from 2003 onwards. Since our empirical strategy exploits variation across individual over age, we determine an individual's personal income at a certain age by calculating a weighted average of personal income given their month of birth. For example, for individuals who were born in October, 1949, we determine the individual's personal income at age 54 as $\frac{2}{12}$ times the observed income in 2003, and $\frac{10}{12}$ times the observed income in 2004.

[18] This implies that if an individual is unemployed for a total of three years during the period 1999 and 2020, and therefore only works for 9 years in total, we calculate the average yearly wage over 9 years, instead of 12.

[19] The ATC classification system is a medical drug classification system that categorizes medications according to the organ/system they act upon, and their chemical/pharmacological/therapeutic effects. The ATC code consists of five levels. Our data only includes the ATC4 codes, meaning that we cannot distinguish medications at the fifth level, which implies that we cannot distinguish between two drugs that have the same ATC4 code (i.e., drugs that belong to the same anatomical main group, and therapeutic, pharmacological, and chemical subgroup), but have different chemical substances. However, the ATC4 code provides sufficient information to match medications to medical conditions.

eases, hypertension, rheumatic conditions, high blood cholesterol, glaucoma/cataract, peptic ulcers, bronchitis/asthma, or osteoporosis) and mental conditions separately. For mortality, we observe the day of death for every individual and use this, in combination with their month of birth, to determine the age of death.[20] Lastly, we observe whether an individual was hospitalized (inpatient stays) between 2006 and 2012 from the *Hospital Discharge Register* (*Dutch:* Landelijke Medische Registratie).[21]

Other characteristics. We determine individuals' marital status (or whether they have a registered civil partnership) at age 55 (i.e., before potential early retirement) for both the treatment and control group (Oral et al., 2024). Furthermore, we observe whether individuals are working in the private or public sector before they retire. Lastly, we observe whether an individual was born in the Netherlands.

3.3 Descriptive statistics

Table 1 presents the descriptive statistics for the treatment group (i.e., individuals born in 1950) and the control group (i.e., individuals born in 1949). The first four columns present the mean and standard deviation for the treatment group and control group respectively, and the fifth column presents the p-value of a birth threshold indicator variable − which is equal to one in case an individual is born after December 31, 1949 − from a regression of the variable of interest on this birth threshold indicator.

Table 1 shows that gender, employment sector, pre-retirement wage, and medication prescriptions (i.e., mental health related or another chronic condition) after age 65 are statistically similar across both cohorts. For all individuals who passed away before age 70, we find that the average age of death was equal to 65 years in the control group and 11 months and 65 years and 4 months in the treatment group.

The average retirement age for individuals in the control group is approximately equal to 62 years and 10 months, whereas the average retirement age for individuals in the treatment group is approximately equal to 63 years and 2 months. This difference of approximately 4 months in the average retirement age is similar to the results presented by Oral et al. (2024). In case we define retirees as individuals who receive pension benefits, the average retirement age is

[20] Note that for privacy reasons, we do not observe an individual's day of birth. Therefore, we are only able to determine an individual's age at death (and age at retirement) at the monthly level. Additionally, medication prescriptions are only observed at the yearly level. For example, for all individuals in our sample, we create a binary indicator at age 56 that is equal to one in case an individual has a medication prescription for a specific condition in 2006, and zero otherwise. Note that in 2006 all individuals in our sample are (almost) 56. Our reasoning behind the construction of this variable is that the data on medication prescriptions are less suitable for making trend analyses because the coverage area of the basic insurance can change significantly from year to year. As a result, certain groups of medicinal products are only observed in one (specific) year, whereas in other years they are not available (or to a lesser extent).

[21] The LMR covers all university, general hospitals, and most specialized hospitals in the Netherlands. It covers approximately 88% of all inpatient hospital stays (van der Laan, 2013).

Table 1: Descriptive Statistics

	Control group Born before Jan. 1 1950		Treatment group Born after Jan 1. 1950		
	Mean (1)	Std. dev. (2)	Mean (3)	Std. dev. (4)	p-value (5)
	Panel A: Subsample of individuals still alive at age 70				
Demographics					
Sector: private	0.5660	0.4956	0.5600	0.4964	0.0560
Sector: other (judicial sector, utilities)	0.1752	0.3802	0.1797	0.3839	0.0955
Sector: public	0.0670	0.2499	0.0665	0.2492	0.8965
Sector: education	0.0046	0.0678	0.0054	0.0730	0.0708
Sector: police/military	0.0090	0.0942	0.0102	0.1004	0.0842
Male	0.5705	0.4950	0.5691	0.4952	0.9656
Married (age 55)	0.7647	0.4242	0.7648	0.4242	0.6800
Dutch	0.8625	0.3444	0.8579	0.3492	0.0377
Labor market outcomes					
Average yearly wage (euro)	36,892	29,155	37,251	31,036	0.4351
Average yearly pension income (euro)	30,638	20,867	30,277	21,388	0.0052
Personal income at age 63 (euro)	38,757	36,986	41,788	43,909	0.0000
Personal income at age 66 (euro)	30,937	25,954	31,520	29,402	0.0004
Retirement age (out of labor force)	62.8226	4.9611	63.2033	4.9625	0.0000
Retirement age (receiving pension benefits)	61.5860	3.4845	62.2465	3.5805	0.0000
Retired at age 65 (out of labor force)	0.6619	0.4731	0.5589	0.4965	0.0000
Health outcomes					
Having mental condition after age 65	0.1527	0.3597	0.1555	0.3624	0.2015
Having other chronic condition after age 65	0.8701	0.3362	0.8694	0.3369	0.6956
Num. of obs.	44,854		47,994		
	Panel B: Subsample of individuals who died before age 70				
Health outcomes					
Age of death	65.9251	6.4900	65.4520	6.6048	0.0000
Num. of obs.	9,704		10,136		

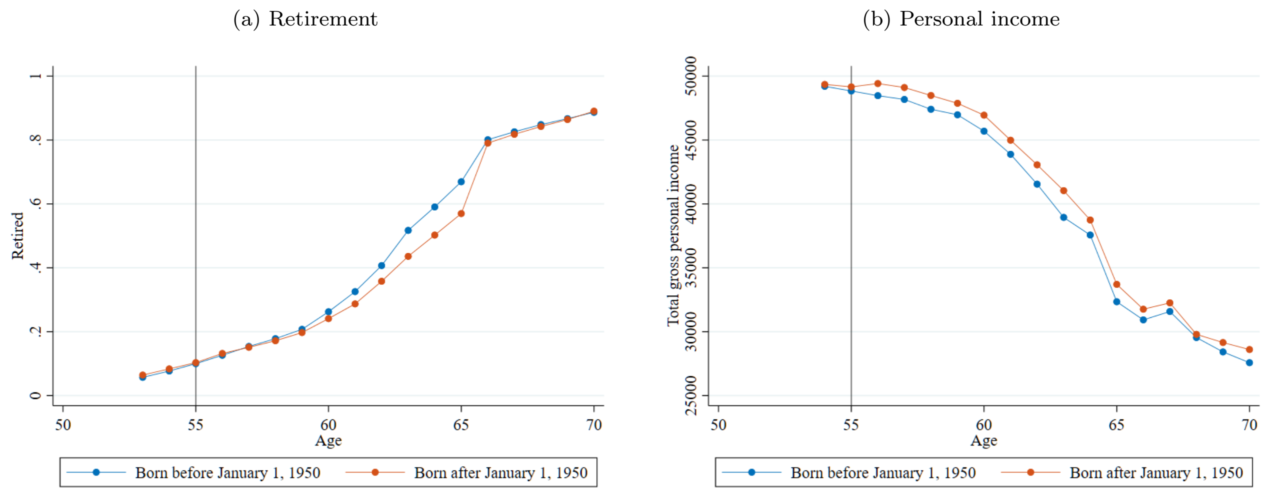
Notes. This table shows the descriptive statistics for our main sample. The main sample consists of individuals who were born between August 1, 1949, and May 1, 1950, and who have received income out of employment from January 1, 1999, onwards. Column (5) presents the p-value of the slope coefficient for regressing the variable on a dummy indicator indicating whether an individual was born after or on January 1, 1950. For more information on the variables, see Section 3.2.

lower for both groups: 61 years and 7 months for the control group, and 62 years and 3 months for the treatment group. This difference between the two definitions of retirement could point towards the observation that individuals first partially retire (i.e., receiving partial retirement benefits while still working part-time) before they permanently leave the labor force. At age

65, approximately 66 percent of the treatment group is retired, as opposed to 56 percent of the treatment group. All of these differences are statistically significant for a 1% significance level, implying that many workers in the treatment group postpone retirement.

Panel (A) in Figure 1 plots the mean retirement rate across age for the treatment and control group in our main sample. It shows a difference in the retirement take-up between age 60 and 65 for both groups. After the SRA of 65 years and 3 months, we do not observe a large difference in an individual's probability to permanently leave the labor force between the two groups. We document a similar pattern in case we define retirement as the state of receiving pension benefits (see Panel (A) in Figure A.1 in the Appendix). Panel (B) in Figure 1 plots the average yearly gross personal income as a function of age for both groups. Two notable patterns arise. First, it shows that the average income level between the treatment and control group are very similar before the reform could potentially affect the treatment group at age 54. After that age, the average income level of treated individuals is systematically higher than the one of untreated individuals throughout all ages we observe. Second, the average income declines with age for both groups. This observation is in line with the previous literature on old-age labor supply documenting that (partial) retirement is associated with a reduction in income (Knoef et al., 2016).

Figure 1: Labor market outcomes



Notes. These figures plot the mean retirement rate (Panel (A)) and yearly personal gross income (Panel (B)) across age for the treatment group (i.e., individuals born at or after January 1, 1950) and the control group (i.e., individuals born before January 1, 1950). We define an individual's retirement age as the age at which they no longer receive any income from (self-)employment. A person's gross income consists of income received from employment, including unemployment benefits, and other social security income sources (i.e., pension, disability insurance, and unemployment insurance). The black vertical line indicates the age at which individuals in the treatment group become subject to the new pension rules.

4 Empirical specification

The key empirical challenge in this paper is to estimate individuals' (long-run) counterfactual health and labor market outcomes in absence of the pension reform. To do so, we exploit exogenous variation in pension wealth across individuals born just before and after January 1, 1950, and identify counterfactual outcomes using a difference-in-differences design. Our main source of identification therefore comes from the discontinuity in pension treatment solely based on an individual's day of birth, thereby assuming that both groups are sufficiently similar prior to the reform.[22]

Our regression specification compares individual i's outcome y_{ia} at age $a \in \{53, \ldots, 70\}$ between individuals born before and after January 1, 1950, and is given by:

$$y_{ia} = \beta_0 + \nu_a + \alpha_i + \sum_{k=53}^{70} \beta_k \mathbb{1}\{a = k\} Z_i + \varepsilon_{ia}, \tag{1}$$

where $\mathbb{E}(\varepsilon_{ia}|Z_i, \nu_a, \alpha_i) = 0$. The indicator variable $\mathbb{1}\{a = k\}$ is equal to one in case age a is equal to k, and zero otherwise. As the reform affected individuals' pension wealth in the treatment group from age 55 onwards − as the reform got introduced on February 24, 2005 − we normalize β_{54} to zero (i.e., the age before treatment). The dummy variable Z_i is equal to one in case individual i belongs to the treatment group, and zero otherwise. Our specification controls for age and individual fixed effects, ν_a and α_i, respectively. This allows us to control for age-specific factors and unobserved individual heterogeneity in health and labor market outcomes across both the treatment and control birth cohort. Furthermore, we cluster our standard errors at the individual level. We therefore account for the possibility that health and labor market outcomes are correlated within individuals over age.

We consider four main outcomes in this paper. For health, we consider the binary indicator y_{ia} that is equal to one in case an individual i has a specific medication prescription (i.e., mental health related or another chronic condition) at age a, and zero otherwise. Similarly, for mortality, we let y_{ia} be equal to one in case individual i has passed away before or at age a, and zero otherwise. For labor participation, we let y_{ia} be equal to one in case individual i is retired (i.e., left the labor force) at age a, and zero otherwise. For income, we let y_{ia} be equal to an individual i's yearly gross personal income (in euros) at age a. For robustness, we also check logarithmic transformations of income in Section 6.4.

The coefficients of interest $\{\beta_k\}$ for $k = 53, \ldots, 70$ represent the effect of the reform at age k with respect to the baseline age 54 under the identifying assumption that an individual's health and labor market outcomes in the treatment and control group would move in parallel in absence of the pension reform. As we control for age-invariant unobserved heterogeneity α_i,

[22]The descriptive evidence presented in Figure 1 shows that the average retirement rate and gross personal income between the treatment and control group were very similar before age 55. The same holds for the health outcomes we consider (see Figure A.1 in the Appendix).

we perform a within-group estimator to estimate the causal effect of the new pension rules. This suggests that we estimate the extent to which the pension reform influences the outcome trajectory of individuals. As common in studies that use a difference-in-differences research design, one could test for this parallel trends assumption using leading health and labor market outcomes before age 54. For retirement and mortality, we can test this hypothesis (i.e., $H_0 : \beta_{53} = 0$).

On the contrary, for medication prescriptions, we only observe individuals' outcomes from age 56 onwards, resulting in the necessity to additionally set β_{53}, β_{54}, β_{55}, and β_{56} to zero as we are not able to estimate these coefficients. Furthermore, we observe individuals' personal income from age 54 onwards, implying that for this outcome we set β_{53} and β_{54} to zero. Therefore, we are not able to test the common trend assumption using an individual's leading income levels and medication prescriptions. As we exploit the discontinuous assignment in birth date for individuals born in a narrow time window, we expect the treatment and control group to be very similar in these outcomes at ages prior to the reform. Furthermore, as we exploit variation in health and labor market outcomes across age, we additionally capture any potential differences in unobserved age-invariant heterogeneity affecting individuals' outcomes later in life (Heller-Sahlgren, 2017). To the best of our knowledge, there are not other (institutional) reforms that differently affected individuals in the treatment and control cohort (Grip et al., 2012; Lee et al., 2022; Lindeboom and Montizaan, 2020; Montizaan et al., 2010).[23]

5 Results

5.1 Labor market

Retirement. The orange estimates in Panel (A) and Panel (B) in Figure 2 plot the coefficients estimates of $\{\beta_k\}$ relative to the difference in labor market outcomes between the two cohorts prior to the reform (i.e., age 54) for men and women, respectively. Our results show that the reform had a clear effect on individuals' probability to leave the labor force. We document (almost) no difference in retirement take-up before age 56, but a large effect between ages 60 and 65, as expected by the visual evidence presented in Panel (A) in Figure 1. This effect becomes substantially smaller after the SRA − which is equal to 65 years and 3 months for both the treatment and control group − but remains statistically significant until age 69. The latter observation can be explained by the fact the new pension rules also stimulated delayed retirement after the SRA, as workers would receive a bonus in case they only start claiming second pillar pension benefits after the SRA.

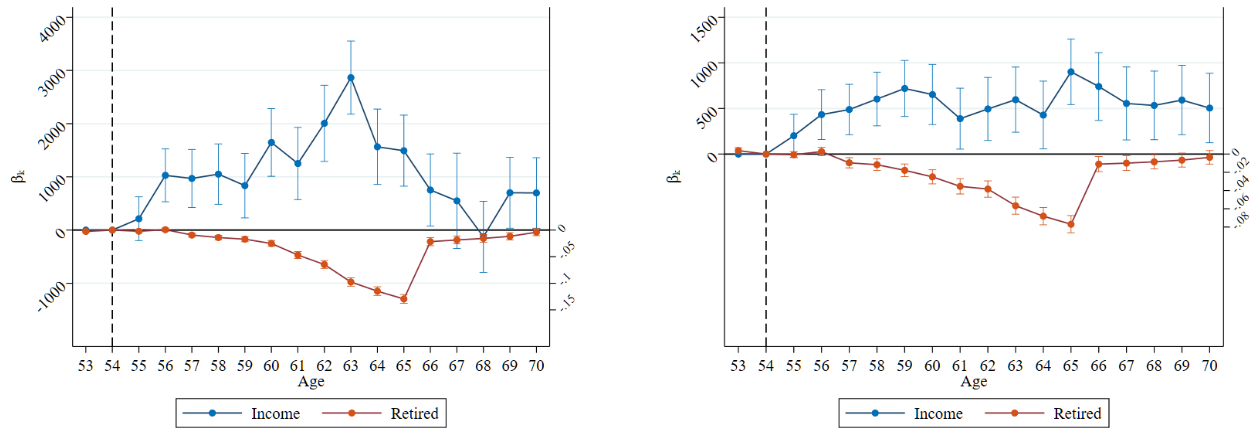
For men, the probability of retirement before (or at) age 65 is 13.0 percentage points (SE: 0.4

[23] Moreover, after the government introduced the new pension rules on February 24, 2005, the public sector announced the termination of the existing early retirement schemes on July 5, 2005 (Grip et al., 2012). The strict differential treatment rule of individuals born around January 1, 1950, was very unexpected, and therefore we expect anticipation effects to be negligible.

Figure 2: THE EFFECT OF THE REFORM ON LABOR MARKET OUTCOMES

(a) Male

(b) Female



Notes. This figure presents the effect of the pension reform on labor market outcomes for men (Panel (A)) and women (Panel (B)). The blue estimates (values on the left vertical axis) represent the estimated coefficients of $\{\beta_k\}$ for $k = 54, \ldots, 70$ with $\beta_{54} = 0$ in case the dependent variable is equal to an individual's gross personal income (in euros) in specification (1). The orange estimates (values on the right vertical axis) represent the estimated coefficients of $\{\beta_k\}$ for $k = 53, \ldots, 70$ with $\beta_{54} = 0$ in case the dependent variable is equal to a binary indicator that is equal to one in case an individual is retired (i.e., left the labor force), and zero otherwise, in specification (1). We plot the 95% confidence intervals using clustered standard errors at the individual level. For more information on the definition of these variables, see Section 3.2.

pp) higher for individuals who were born before 1950 as compared to individuals born in 1950. This difference becomes statistically indifferent from zero at age 70. To compare, the effect of reaching the minimum age eligibility for receiving Social Security on retirement take-up in the US is approximately equal to 14 percentage points (Gorry et al., 2018; Neuman, 2008; Fitzpatrick and Moore, 2018). For women, we find this effect to be half of the effect for men: being born before 1950 increases the probability of retirement before or at age 65 by 7.7 percentage points (SE: 0.5 pp). We document a similar pattern when we measure retirement as receiving pension benefits (see Figure A.2 in the Appendix). Our results are in line with the extensive literature documenting large substitution effects – as a consequence of a decrease pension benefits and other sources of retirement income – from leisure to labor (Lindeboom and Montizaan, 2020; Mastrobuoni, 2009; Manoli and Weber, 2016; Krueger and Pischke, 1992; Krueger and Meyer, 2002; Samwick, 1998; Euwals et al., 2006; Euwals et al., 2010).

To investigate the parallel trends assumption, we test the null hypothesis H_0: $\beta_{53} = 0$. Panel A in Table 2 presents the corresponding Wald test result and the corresponding p-value. For men (women), we reject the null hypothesis that the leading ages in an individual's retirement status are equal to zero at the 1% (5%) significance level. Although these leading terms are jointly statistically different from zero, they are economically small compared to post-reform ages. Moreover, as our main source of identification solely stems from a discontinuity based on an individual's day of birth, we are confident that the treatment and control group's evolution of old-age labor market participation would behave similarly in absence of the new pension rules.

Table 2: Test for a common pre-trend

	Male		**Female**	
	Test statistic	p-value	Test statistic	p-value
	(1)	(2)	(3)	(4)
Panel A.				
Retirement	7.8471	0.0051	5.3297	0.0210
Num. of obs.	56,036		42,386	
Panel B.				
Mortality	0.3529	0.5525	0.1970	0.6571
Num. of obs.	65,130		46,497	
Panel C.				
Mortality	1.25	0.2646	0.49	0.8643
Num. of obs.	87,269		83,846	

Notes. Panel A and Panel B in this table present the outcome of the Wald test (Column (1) and Column (3)) and its corresponding p-value (Column (2) and Column (4)) when testing the null hypothesis $H_0 : \beta_{53} = 0$ in specification (1) for the dependent variables retirement (Panel A) and mortality (Panel B). Panel C presents the results for the Wald test (for mortality) when testing the null hypothesis $H_0 : \beta_{46} = \ldots \beta_{53} = 0$ on the total population of individuals who were born between August 1, 1949 and May 1, 1950, irrespective of their working history. We define an individual's retirement age as the age when an individual does not receive income from (self-)employment anymore. The standard errors are clustered at the individual level.

Gross personal income. The impact of the new pension rules on an individual's total personal income is theoretically ambiguous and largely contingent on realized labor supply decisions at old age (Gelber et al., 2016; Blundell and MaCurdy, 1999). First, as an *income effect*, a decrease in pension income as a consequence of lower replacement rates results in a lower personal income. On the other hand, as the marginal costs of leisure become more expensive relative to labor, this creates an incentive to stay in the labor market (i.e., a so-called *substitution effect* of leisure by labor). The latter leads to an increase in an individual's gross personal income, as the number of contribution years increases.

The blue estimates in Figure 2 plot the coefficient estimates of $\{\beta_k\}$ for $k = 54, \ldots, 70$ for specification (1) where the dependent variable is equal to an individual's total gross personal income at the yearly level (in euros). For both men and women, we find that this pension reform had a statistically and economically significant effect on personal income throughout (almost) all ages we consider. For men, we find that the new pension rules increased an individual's personal income in the treatment group by approximately 2970 euros (SE: 330 euros) when they are 63 years old, as compared to the control group. After the SRA, this effect decreases to approximately 700 euros but remains statistically different from zero, suggesting that the new pension rules had a permanent positive effect on men's income at old age. Through the lens of the model developed by Blundell and MaCurdy (1999), our result implies that the negative *income effect* of this reform is overcompensated by a positive *substitution effect* on total personal income. This is in line with the results documented in Gelber et al. (2016) who document large positive income effects as a consequence of a decrease in Old Age and Survivors Insurance (OASI) in the US. For women, we also find that the new pension rules had a permanent positive effect on their (post-retirement) personal income: at age 66, the average personal income in the treatment group is approximately 500 euros higher than in the control group, again suggesting that the large substitution effects we documented earlier lead to a permanent higher income for women at old age.

Although these estimated average effects on individuals' gross personal income as a consequence of the reform are statistically different from zero, they are economically small. For example, for women, the monthly increase in personal income after the SRA is approximately equal to $\frac{500}{12} \approx 41$ euros, which amounts to approximately 2% of the monthly gross benefits received out of the first pillar (i.e., state pension) for single individuals.

5.2 Health

Mortality. For mortality, we estimate the regression specification (1) where the dependent variable y_{ia} is equal to a binary indicator indicating whether individual i has passed away before or at age a. The use of this measure of mortality follows the corresponding literature (Bozio et al., 2021; Hernaes et al., 2013; Rose, 2020; Hagen, 2018).[24] Figure 3 presents our results for men (Panel (C)) and women (Panel (F)). Note again that all the estimates are expressed as changes from the age prior to treatment (i.e., age 54) which is normalized to zero.

For women, we document that individuals in the treatment group are, on average, approximately 0.46 percentage points (SE: 0.19 pp) more likely to have died by the age of 65 as compared to untreated individuals. This difference is statistically significant and remains unchanged throughout all ages we can observe. For men, we do not find any statistically significant difference between treated and untreated individuals at any age we consider. It is worth noting, however, that the average effect of the reform on mortality for men is negative, which is in line with the results documented in Snyder and Evans (2006) and Fitzpatrick and Moore (2018) in the US. Particularly, until age 63, we do not observe differences in the probability of death. After that age, men in the treatment group are on average 0.31 percentage points (SE: 0.26 pp) less likely to have died before or at age 70. Furthermore, for both men and women, we do not find evidence that the pre-reform age (i.e., age 53) is statistically different from zero for any

[24] Other papers use hazard models to model the effect of (early) retirement on mortality (Brockmann et al., 2009; Hult et al., 2010). We follow this literature and estimate a Cox proportional hazard model as a robustness check in Section 6.3.

reasonable significance level, which supports our difference-in-differences research design (see Panel B in Table 2).

As we restrict our sample to individuals who had a job when they were at least 50 years old, we are unable to test for a common pre-trend for ages below 50 by construction. One potential criticism is that there exists selection into the main sample as a function of differential mortality rates between the treatment and control group that biases our main results. To overcome this potential threat to identification, we re-estimate our main regression specification (1) on the *total* population of individuals who were born between August 1, 1949 and May 1, 1950, irrespective of their employment history. Reassuringly, our results suggest that there exists no differential pre-trend for pre-intervention ages between the treatment and control group for men and women (Panel C in Table 2).

Medication prescriptions. Panel (A) and Panel (D) in Figure 3 show our results from estimating the specification presented in equation (1) in case the dependent variable is equal to a dummy variable indicating whether an individual had a medication prescription at a given age. In order to compare our results to Grip et al. (2012) for the men in our sample, we plot their main coefficients at age 58 and 59 by the gray diamonds in Panel (A).[25] For men, we estimate a statistically significant increase in the medication prescriptions related to mental health right after the reform got introduced at age 58. Men in the treatment group are on average 0.61 percentage point (SE: 0.25 pp) more likely to have a medication prescription for depression and/or anxiety symptoms at age 58 as compared to individuals who are not affected by the reform. However, this effect becomes smaller and statistically insignificant after the SRA, suggesting that this pension reform did not cause persistent effects in medication prescriptions for mental health conditions.

Comparing our estimates to Grip et al. (2012), we find that our effects are substantially smaller. We provide two explanations for this difference. First, note that Grip et al. (2012) make use of a subjective mental health measure based on survey questions. As individuals who report a mental health condition in a survey do not necessarily take antidepressants, this could explain the differences between our estimates and theirs. Second, since our estimates are based on the entire Dutch population and not all workers seem to postpone retirement as a result of the reform, our estimates should be interpreted as an intended-to-treat (ITT) effect. This is in contrast to Grip et al. (2012), who use a sharp regression discontinuity design as individuals in both the treatment and control group retire around the age of 63 (i.e., before the SRA) on average. For women, we do not find a statistically significant effect on mental health related

[25] Grip et al. (2012) employ a sharp regression discontinuity design using survey data on a sample of male public sector workers to estimate the effect of this pension reform on self-assessed mental health at age 58 and 59 only. They operate a simple linear regression model for whether an individual is feeling depressed on an indicator variable that is equal to one in case an individual is born after December 31, 1949. Additionally, they control for a linear birth date trend on both sides of this birth date threshold. As our specification in equation (1) controls for individual fixed effects α_i, we also capture any difference in mental health status that can be explained by an individual's day of birth (among other (unobserved) determinants).

prescriptions for any age we consider.[26]

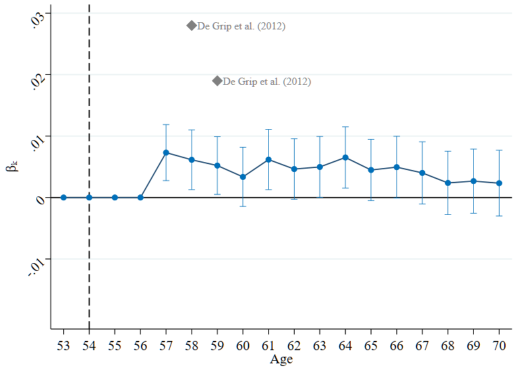
Lastly, we do not document any statistically significant difference in medication prescriptions related to other chronic conditions (for example, coronary, cardiac, rheumatic diseases, etc.) in Panel (B) and Panel (E) in Figure 3 for both men and women, respectively. For men, this suggests that the short-run effects of mental health conditions do not translate into the development of long-run chronic diseases.[27]

Hospitalizations. We re-estimate the regression specification (1) where the dependent variable y_{ia} is equal to a binary indicator indicating whether individual i has been hospitalized at least once at age a. Figure A.3 in the Appendix presents our results for men (Panel (A)) and women (Panel (B)), where β_{56} is normalized to zero. Our results show that the new pension rules did not significantly affect individuals' probability of being hospitalized.

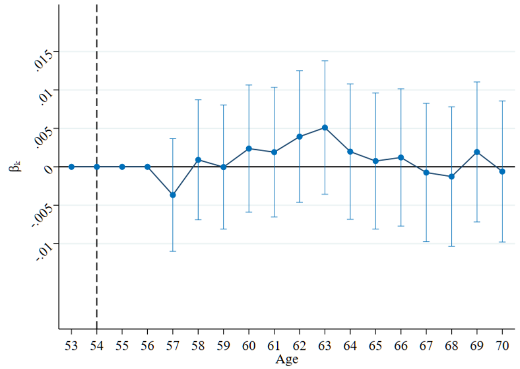
[26] In our analysis, by focusing on individuals who are still alive at age 70, it is possible that some of the observed effects may be influenced by attrition or selection bias among survivors. This concern is particularly relevant given our findings of significant mortality effects among women. To address this concern, we re-estimate our model using the entire sample including all individuals. Specifically, we construct a binary indicator that equals one if an individual develops a mental health condition at a given age or, if deceased, has received a mental health prescription prior to their death. The results for mental health related prescriptions are shown in Panel (A) and Panel (B) in Figure A.4 in the Appendix. These results are similar to our baseline estimates.

[27] This result remains robust when we include individuals who died before the age of 70 (see Panel (C) and Panel (D) in Figure A.4 in the Appendix). Again, in this analysis, we construct a binary indicator that equals one if an individual either received a medication prescription or died in the subsequent year after receiving such a prescription.

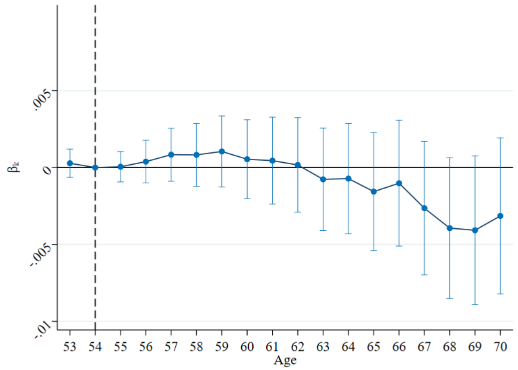
Figure 3: The effect of the reform on health outcomes



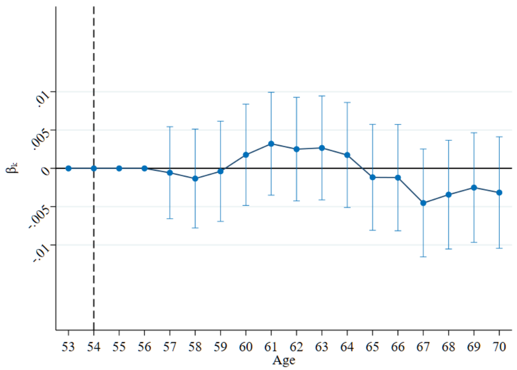
(a) Men – medication prescription for depression and/or anxiety



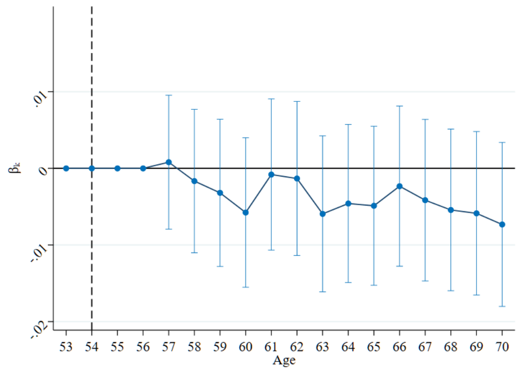
(b) Men – medication prescription for other chronic condition



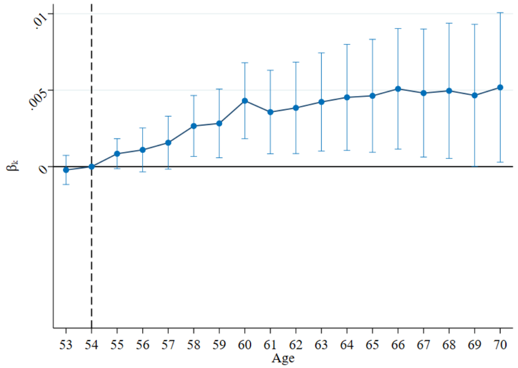
(c) Men – probability of death



(d) Women – medication prescription for depression and/or anxiety



(e) Women – medication prescription for other chronic condition



(f) Women – probability of death

Notes. These figures present the estimated coefficients $\{\beta_k\}$ on health outcomes for men (Panel (A), Panel (B), and Panel (C)) and women (Panel (D), Panel (E), and Panel (F)) using specification (1). For Panel (A), Panel (B), Panel (D), and Panel (E) we present $\{\beta_k\}$ for $k = 56, \ldots, 70$ with $\beta_{56} = 0$. For Panel (C) and Panel (F) we present $\{\beta_k\}$ for $k = 53, \ldots, 70$ with $\beta_{54} = 0$. Panel (A) and Panel (D) represent the results for the binary indicator that is equal to one in case an individual has a medication prescription for depression and/or anxiety symptoms. Panel (B) and Panel (E) present the results for the binary indicator that is equal to one in case an individual has a prescription for another chronic condition (see Table A.1 in the Appendix for an overview). Panel (C) and Panel (F) present the results for the binary indicator that is equal to one in case individual has passed away before or at age k, and zero otherwise. We also plot the main coefficient estimates for ages 58 and 59 as documented in Table 1 in Grip et al. (2012) by the gray diamond markers in Panel (A). We plot the 95% confidence intervals using clustered standard errors at the individual level.

6 Robustness

Our main results can be interpreted as a causal effect of the pension reform in 2005 under the key identifying assumption that health and labor market outcomes for persons born in 1949 and 1950 would move in parallel across age in absence of the new pension rules. Although we do not find (economically) significant differential pre-reform age trends in retirement and mortality, we perform other robustness checks to test this assumption in this section. In Section 6.1 we conduct a placebo test using individuals who were all born in 1950 – all of whom are subject to the new pension rules – and assign individuals to the treatment or control group on the basis of a placebo birth-date threshold. Furthermore, we perform a donut hole regression in Section 6.2 to account for severe disappointment effects for individuals who were born early in 1950. We check for the robustness of our results on mortality by estimating a proportional hazard model in Section 6.3. Lastly, we assess the robustness of the income variable by applying logarithmic transformations in Section 6.4.

6.1 Placebo tests

In order to test whether there exist any other shocks – apart from the pension reform – that affect medication prescriptions and labor market outcomes in the treatment group separately, we perform a placebo test. We restrict the analysis to workers who were born between January 1, 1950, and April 30, 1950, and assign workers who were born before March 1, 1950, to the treatment group (i.e., $Z_i = 1$) and workers who were born after this birth date placebo threshold to the control group (i.e., $Z_i = 0$) and re-estimate equation (1). Note that all individuals in this subsample are subject to the new pension rules and therefore could not retire before the SRA while receiving pension benefits according to the old generous system. Figure A.5 in the Appendix presents the results of this placebo test for our main outcomes. Reassuringly, in sharp contrast to our main results, these results do not show any significant effect at any age we consider for both men and women.

Performing a similar placebo test for all individuals born in 1949 instead of 1950 – and therefore were all subject to the old generous pension rules – also does not produce a significant long-term effect (see Figure A.6 in the Appendix). Note that if there had been a general trend in labor market and health outcomes, the estimated effects resulting from the placebo tests would show a similar trend as in our main results (i.e., (part of) the estimated coefficients $\{\beta_k\}$ would have been significantly different from zero).

6.2 Donut hole

As pointed out by Grip et al. (2012), the abrupt policy change could have caused severe disappointment, especially among those born just after the cutoff date in the beginning of January. The pension reform was announced only a few years before these workers were planning to retire, giving them too little time to adjust for the loss in pension wealth. This sudden shift left workers with little control over their retirement decisions, which may have negatively affected their mental health.[28]

In order to check whether our results are robust to comparing individuals who were born later in 1950, we re-estimate our main specification (1) and disregard all individuals who were born just after January 1, 1950. That is, we construct a sample of individuals who were born between August 1, 1949 and December 31, 1949 (i.e., the control group), and a sample of individuals who were born between August 1, 1950, and December 31, 1950 (i.e., the treatment group). Our results in Figure A.7 in the Appendix mostly show a similar pattern as compared to our main results, suggesting that the reform had an impact for later generations too. An exception to this is female mortality, for which we find no statistically significant effect.

6.3 Proportional hazard model for mortality

Some papers make use of survival models to assess mortality (Brockmann et al., 2009; Hult et al., 2010). To check whether our results on mortality are robust to such alternative specifications, we consider a Cox proportional hazard model (Cox, 1972). To formally present this model, we let A denote a random variable that represents the time until death in years (i.e., failure time). We are interested in estimating the so-called hazard rate $h(a)$ at age a, which is defined as follows:

$$h(a) = \lim_{\Delta a \to 0+} \frac{Pr(a \leq A < a + \Delta \mid a \leq A)}{\Delta a}, \tag{2}$$

where the operator $\lim_{\Delta a \to 0+}$ indicates that the magnitude of the age interval approaches 0, but remains strictly positive. Following Cox (1972), individual i's hazard function for the Cox proportional hazards model is equal to:

$$h(a \mid Z_i, \boldsymbol{X_i}) = h_0(a) \times \exp(\beta_z Z_i + \boldsymbol{\beta_x X_i}), \tag{3}$$

where $h_0(a)$ is the unknown baseline hazard function at age a that is constant across all individuals. Our parameter of interest is β_z. The vector $\boldsymbol{X_i}$ consists of individuals' observable characteristics such as marital status, private sector job, and pre-retirement wage rank.[29] Given this specification in expression (3), the so-called hazard ratio between the treatment and control group is given by:

$$\frac{h(a \mid Z_i = 1, \boldsymbol{X_i})}{h(a \mid Z_i = 0, \boldsymbol{X_i})} = \exp(\beta_z). \tag{4}$$

Note that the hazard ratio does not depend on the baseline hazard function $h_0(a)$. Therefore, the additional advantage of such proportional hazard models is that the researcher does not

[28] It could be the case that some individuals who were born early 1950 and are therefore subject to the new less generous rules decided to retire at age 55 just before the reform was announced in February 2005. For example, consider individuals who were born January 2, 1950 and decided to retire when they turned 55 years old (i.e., on January 2, 2005). These individuals would receive pension benefits under the flexible scheme for a couple of months, whereas they would not be able to receive any generous second pillar pension benefits from 2006 onwards anymore. By focusing on individuals who were born later in 1950 by means of a donut hole framework, we take into account such unanticipated reform effects.

[29] As explained later in Section 7.2, we construct a wage rank between the 0 and 100 denoting individuals' pre-retirement wage rank with respect to all workers with the same gender in our sample.

need to specify the functional form of $h_0(\cdot)$.

To define the likelihood function, let the distinct ages at death be ordered as follows:

$$a(1) < a(2) < \ldots < a(n),$$

where n denotes the number of individuals that pass away in our sample (i.e., thereby excluding right-censored individuals). Furthermore, let $\mathcal{R}(a(i))$ denote the set of individuals who are at risk of dying at age $a(i)$. This set includes all individuals whose age of death or censoring age is at least equal to $a(i)$. The partial log-likelihood function is then given by:

$$\log \mathcal{L} = \sum_{i=1}^{n} Z_i \beta_z + \boldsymbol{X_i} \boldsymbol{\beta_x} - \sum_{i=1}^{n} \log \Big[\sum_{l \in \mathcal{R}(a(i))} \exp(Z_l \beta_z + \boldsymbol{X_l} \boldsymbol{\beta_x}) \Big]. \tag{5}$$

We maximize (5) with respect to the coefficients of interest β_z and $\boldsymbol{\beta_x}$. Column (1) and Column (3) in Table 3 present our results on the estimated coefficient of β_z for both men and women in case we do not control for any other observables. In line with our main results, we find a statistically significant increase in mortality for women in the treatment group as compared to the control group, with a hazard ratio equal to $\exp(0.0826) = 1.0861$. We report an insignificant decrease in mortality for men. Column (2) and Column (4) in Table 3 show the coefficient estimates for β_z in case we control for individuals' marital status, whether they work in the private sector, their wage rank, and a binary indicator that is equal to one in case they were born in the Netherlands. The estimated coefficient β_z is similar across the two specifications, thereby indicating that unobserved systematic heterogeneity on the basis of these observed characteristics does not bias our results.

Table 3: Results for Cox proportional hazard model

	Male		**Female**	
	(1)	(2)	(3)	(4)
Z_i	−0.0163 (0.0195)	−0.0152 (0.0195)	0.0826 (0.0306)	0.0841 (0.0306)
Married		−0.5697 (0.0211)		−0.4938 (0.0322)
Private sector		−0.0606 (0.0225)		0.0308 (0.0311)
Wage rank		−0.6273 (0.0344)		−0.3146 (0.0548)
Dutch		0.1284 (0.0278)		0.1265 (0.0437)
Num. of obs.	53,907	53,907	34,775	34,775

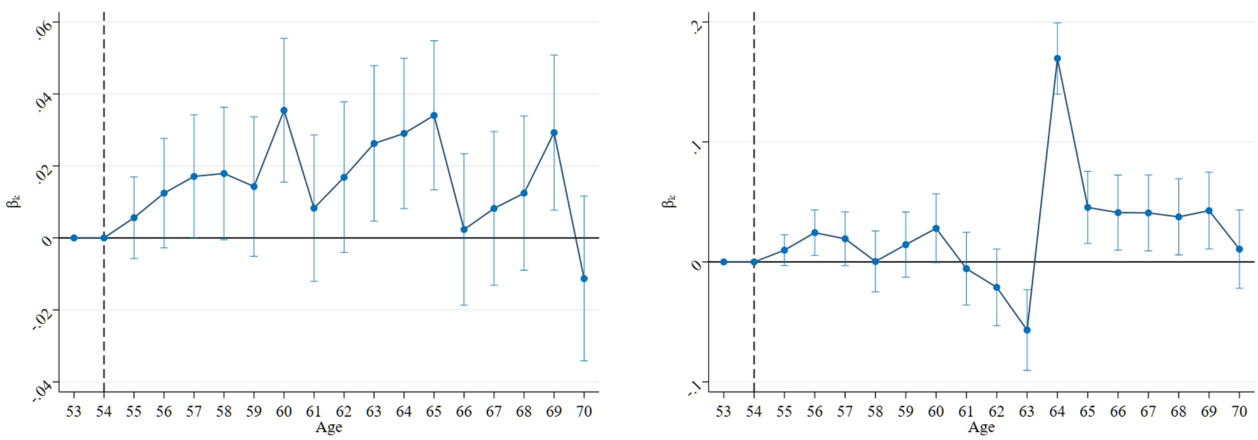
Notes. This table presents the coefficient estimate of β_z for when maximizing the partial log-likelihood function as presented in expression (5) with respect to β_z and $\boldsymbol{\beta_x}$. Column (1) and Column (3) present the estimated coefficient β_z in case we do not include additional covariates in the log likelihood specification (i.e., setting $\boldsymbol{\beta_x} = 0$ in (5). Column (2) and Column (4) present the estimated coefficients β_z and $\boldsymbol{\beta_x}$.

6.4 Income transformation

In this section, we evaluate the robustness of our baseline findings regarding the income outcome variable. Specifically, rather than utilizing income levels as the dependent variable y_{ia}, we re-estimate equation (1) using alternative specifications, such as $\log(1 + y_{ia})$, $\log(100 + y_{ia})$, $\log(1000+y_{ia})$, and $\text{arcsinh}(y_{ia})$ (i.e., the inverse hyperbolic sine transformation). The logarithm operator accounts for the fact that the distribution of personal income is skewed. For that reason, most of the empirical literature places more weight on treatment effects for individuals with lower initial income or earnings by considering a logarithmic transformation. We add one, 100, or 1000 inside the logarithm operator to take into account that some individuals have zero personal income (for some ages).

Figure 4: Robustness: the effect of the pension reform on the logarithm of personal income

(a) Men (b) Women



Notes. This figure plots the estimated coefficients of $\{\beta_k\}$ for $k = 54, \ldots, 70$ (with $\beta_{54} = 0$) on the logarithm of total gross personal income plus 100 euros (i.e., $\log(100 + y_{ia})$) for men (Panel (A)) and women (Panel (B)) using specification (1). We plot the 95% confidence intervals using clustered standard errors at the individual level.

Figure 4 presents our results for $\log(100 + y_{ia})$. For women, the sign of some of our baseline estimates changes if we transform the income level by a logarithmic transformation. This can be attributed to the fact that log specifications (or inverse hyperbolic sine transformations) put a greater emphasis on the incomes of individuals at the lower end of the income distribution. More specifically, according to Chen and Roth (2024), these results mainly capture the effect of the 2005 pension reform on the extensive margin (i.e., women who switch from positive earnings to zero earnings as a consequence of the new pension rules), rather than the intensive margin. Consequently, the large positive income effects as we document in our main results – in which we put equal weight on all observations – can mainly be explained by positive effects of the pension reform at the upper part of the income distribution. We document similar findings in Figure A.8 in the Appendix in case we consider other logarithmic and inverse hyperbolic sine transformations.

7 Heterogeneity

In this section, we study the heterogeneity of our results by studying the effect of the reform on various subsamples. In Section 7.1, we re-estimate our main specification on a subsample of individuals who all retired before or after a targeted age. Furthermore, we study heterogeneity of our results across income rank, employment sector, and marital status.

7.1 Retirement age

Utilizing that this reform influences an individual's likelihood of retiring at various ages, we can disaggregate the policy's impact across the distribution of actual retirement ages as we observe in the data. This concept is informed by the research on minimum wage effects as developed by Cengiz et al. (2019) and offers further insights into the mechanisms underlying the health effects documented in our main findings, specifically income or retirement effects.[30] First, we look at the lower tail of this distribution (i.e., all individuals who left the labor force before age 55). By doing so, we assess the effect of the 2005 pension reform on health outcomes by excluding one potential channel explaining our main results: the differential retirement take-up after age 55. Second, we use the upper part of the retirement age distribution (i.e., all individuals who retire after the SRA) to rule out any early retirement effects. In other words, because the difference in health and labor market outcomes between the treatment and control group for persons who all retire after the SRA cannot reflect the causal effect of early retirement by construction, we can use this subsample of late retirees to test for the presence of income shocks, or delayed retirement at older ages, that differentially affect the treatment and control birth cohort in our sample.

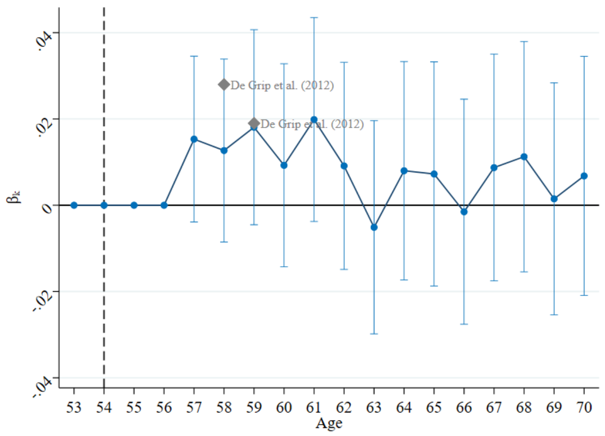
Early retirees (before age 55). Panel (A) in Figure 5 presents the estimated coefficients of $\{\beta_k\}$ in specification (1) for mental health related medication prescriptions for the subsample of early male retirees (i.e., all men who retired before age 55). For these workers, our estimates closely match the estimates as presented in Grip et al. (2012). This result suggests that mental health related medication prescriptions at ages 57 till 60 are likely attributable to (a foresight to) reduced pension benefits (i.e., personal income) for the treatment group, rather than the act (or prospect) of leaving the labor force after age 55. It is important to clarify that we are not claiming that analyzing heterogeneity by an individual's retirement age is the sole way to reconcile our findings with Grip et al. (2012), but rather suggesting it as one possible explanation.

We draw a similar conclusion for our main result documenting the increase in female mortality. Panel (B) in Figure 5 presents the estimated coefficients $\{\beta_k\}$ for the subsample of early female retirees. Although the effects are not statistically significant − potentially due to the

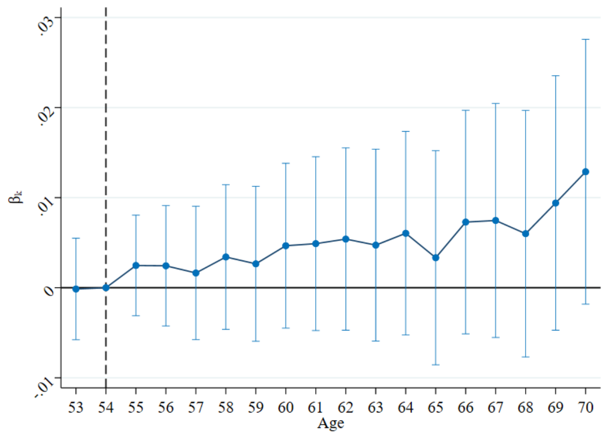
[30] Cengiz et al. (2019) estimate the effect of state-level minimum wage changes on employment throughout the hourly wage distribution using a difference-in-differences research design. They argue that they can use their estimates for higher wage bins as a falsification test for their research design, as the minimum wage is unlikely to cause employment changes in the upper part of the wage distribution.

Figure 5: HETEROGENEITY: THE EFFECT OF THE REFORM ON HEALTH OUTCOMES – EARLY RETIREES

(a) Men – medication prescription for depression and/or anxiety



(b) Women – probability of death



Notes. These figures present the estimated coefficients $\{\beta_k\}$ on mental health related outcomes for men (Panel (A)) and female mortality (Panel (B)) using specification (1). Panel (A) presents the estimated coefficients $\{\beta_k\}$ for $k = 56, \ldots, 70$ (with $\beta_{56} = 0$) for specification (1) in case the dependent variable is equal to a binary indicator that is equal to one in case a men has a medication prescription for a mental health related condition, and zero otherwise. Panel (B) presents the estimated coefficients $\{\beta_k\}$ for $k = 53, \ldots, 70$ (with $\beta_{54} = 0$) in case the dependent variable is equal to a binary indicator that is equal to one in case a woman has deceased, and zero otherwise. We estimate this equation using a subsample of early retirees only (i.e., all individuals who are retired before the age of 55). We also plot the main coefficient estimates at ages 58 and 59 as documented in Table 1 in Grip et al. (2012) by the gray diamond markers in Panel (A). We plot the 95% confidence intervals using clustered standard errors at the individual level.

smaller sample size – the direction of the effect is consistent with our main findings. This further supports the notion that the observed increase in average mortality among women in the full sample may be partially attributed to reduced income foresight.[31]

Note that in order to interpret these coefficient estimates as a causal effect of the pension reform for this specific subgroup of workers, we again need to assume that the likelihood of having medication prescriptions would evolve in parallel in both groups. This assumption is more challenging to defend compared to our main results, as the treatment and control groups are now less comparable due to the endogeneity of the retirement decision. We partly address this issue by controlling for age-invariant heterogeneity. Panel A in Table 4 presents the descriptive statistics for this subsample of early retirees with respect to their observable characteristics. The fifth column presents the p-value of a birth threshold indicator variable – which is equal to one in case an individual was born after December 31, 1949 – from a regression of the observable characteristic on this birth threshold indicator. We find that a higher fraction of early retirees in the treatment group are private sector workers as opposed to early retirees in the control group. Early retirees in the treatment group were less likely born

[31]Recall that the early retirees in the treatment group face a lower pension wealth as compared to the control group as the financial penalty for retiring at age 55 is higher in the treatment group.

in the Netherlands. We do not find any statistically significant differences in the pre-retirement wage, gender, and marital status between early retirees in the treatment and control group. Thus, only if there exists systematic unobserved *age-varying* heterogeneity for private sector employees and/or individuals with a migration background that explains differences in health outcomes as presented in Figure 5, we are unable to interpret these effects as a causal effect of (a foresight to) lower income.

Table 4: DESCRIPTIVE STATISTICS: LATE AND EARLY RETIREES

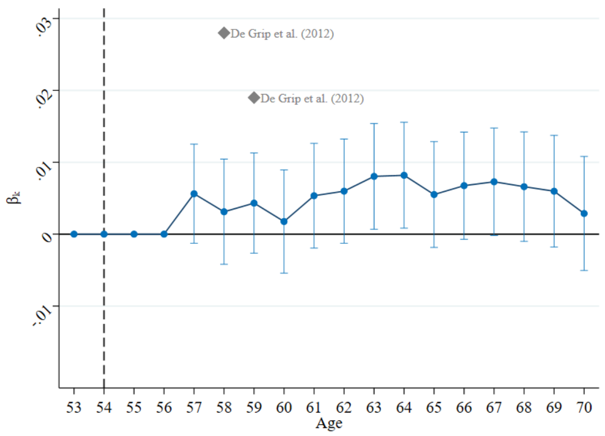
	Control group Born before Jan. 1 1950		**Treatment group** Born after Jan 1. 1950		
	Mean (1)	Std. dev. (2)	Mean (3)	Std. dev. (4)	p-value (5)
Panel A. Early retirees					
Private sector	0.2488	0.4323	0.2821	0.4501	0.0000
Male	0.4309	0.4953	0.4345	0.4957	0.8566
Married	0.5897	0.4919	0.5816	0.4933	0.2856
Born in the Netherlands	0.7935	0.4048	0.7651	0.4240	0.0002
Average yearly wage (euro)	28,862	62,356	27,573	31,993	0.1443
Num. of obs.	5,632		6,508		
Panel B. Late retirees					
Private sector	0.6030	0.4893	0.5527	0.4972	0.0000
Male	0.6377	0.4807	0.6542	0.4757	0.0005
Married	0.7528	0.4314	0.7542	0.4306	0.7795
Born in the Netherlands	0.8474	0.3596	0.8561	0.3510	0.0201
Average yearly wage (euro)	38,700	33,565	40,356	34,706	0.0000
Num. of obs.	16,200		22,664		

Notes. This table presents the descriptive statistics on time-invariant characteristics of early retirees and late retirees in our main sample. The main sample consists of individuals who were born between August 1, 1949 and May 1, 1950, and who have received income out of employment from January 1, 1999 onwards. The subsample of early retirees are individuals who are retired before age 55. The subsample of late retirees are individuals who retired after the SRA of 65 years and 3 months. Column (5) presents the p-value of the slope coefficient for regressing the variable on a dummy indicator that is equal to one in case an individual is born after or on January 1, 1950. For more information on the variables, see Section 3.2.

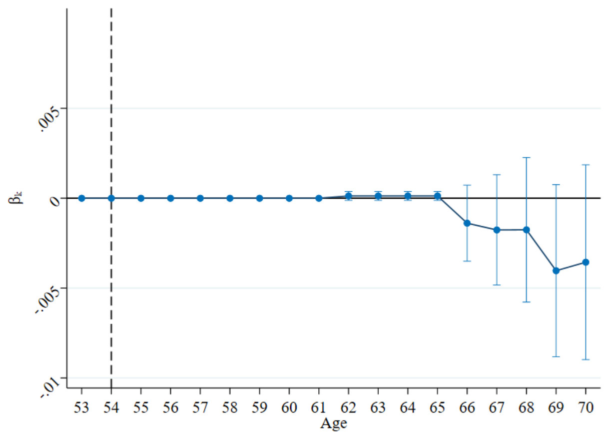
Late retirees (i.e., after the SRA). Figure 6 presents our coefficient estimates of $\{\beta_k\}$ when we re-estimate equation (1) for the subsample of individuals who all permanently leave the labor force after the SRA of 65 years and 3 months. For women, we do not document any significant effect on old-age mortality, suggesting that our main results are not driven by potential income effects for this subgroup of late retirees (see Panel (B) in Figure 6). For men, we document a statistically significant increase in medication prescriptions related to mental health related conditions around age 63, but these differences become statistically indifferent from zero after the SRA (Panel (A) in Figure 6). One potential explanation for the temporary increase in mental health care prescriptions for men in the treatment group could be the prospect of a lower yearly income after they retire. This may result from the lower accumulation of second pillar pension benefits compared to the control group, whose group members were still able to build their pensions under the generous tax rules.

Figure 6: The effect of the reform on health outcomes – heterogeneity by retirement age

(a) Men – medication prescription for depression and/or anxiety



(b) Women – probability of death



Notes. These figures present the estimated coefficients $\{\beta_k\}$ on mental health related outcomes for men (Panel (A)) and female mortality (Panel (B)) using specification (1). Panel (A) presents the coefficient estimates for $k = 56, \ldots, 70$ with $\beta_{56} = 0$ and Panel (B) presents the coefficient estimates for $k = 53, \ldots, 70$ with $\beta_{54} = 0$. We estimate this equation using a subsample of late retirees only (i.e., all individuals who are retired after the age of 65 years and 3 months). We also plot the main coefficient estimates at age 58 and 59 as documented in Table 1 in Grip et al. (2012) by the gray diamond markers in Panel (A). We plot the 95% confidence intervals using clustered standard errors at the individual level.

Similar to the results presented for the subsample of early retirees, we can only interpret these estimated coefficients as a causal effect of the 2005 pension reform if there exist no differences in the age-invariant unobserved (mental) health heterogeneity between the treatment and control group. To gain more insight into this potential thread to identification for the subsample of late retirees, Panel B in Table 4 presents the descriptive statistics for this subsample with respect to its observable age-invariant characteristics. We document that a lower fraction of late retirees in the treatment group are private sector workers and are female. Moreover, late retirees appear to have a higher pre-retirement wage than late retirees in the control group. Again, only if there exist systematic differences in the unobserved age-varying heterogeneity in mental health for private sector employees and/or high-wage workers between the treatment and control group, we are unable to interpret these effects as a causal effect of the new pension rules for men who retired after the SRA.

We contend that these results offer significant insights for the extensive body of literature examining the *causal* impact of (early) retirement on health outcomes. The majority of the literature on the effect of retirement on health outcomes uses (minimum) age eligibility criteria for receiving pension benefits (or other types of social security) as an instrument for retirement (Gorry et al., 2018; Bonsang and Klein, 2012; Neuman, 2008; Insler, 2014; Rose, 2020; Eibich, 2015), although pension reforms are also considered as quasi-experimental variation for leaving the labor force (Nielsen, 2019; Bertoni and Brunello, 2017). Particularly, our findings indicate that the observed discontinuities in health outcomes around critical ages are also evident among individuals who are already retired. This suggests that factors other than retirement itself − such as (the prospect to) less income − may be contributing to these estimated health effects, potentially making a similar pension reform or minimum age discontinuity an invalid instrument for retirement alone.

7.2 Pre-retirement wage

For individuals with a relatively high wage, a larger fraction of their pension wealth is derived from the second pillar compared to those who earned lower wages prior to retirement (Knoef et al., 2016). In this section, we separately estimate our main specification in equation (1) for low and high-wage workers. To assign individuals to these two wage groups, we construct a wage rank $w_i \in [0, 100]$ denoting individual i's average pre-retirement wage rank with respect to all workers with the same gender in our sample. Thus, a men with $w_i = 0$ ($w_i = 100$) belongs to the lowest (highest) earners of all men in our sample.

Figure A.9 in the Appendix presents our results for all individuals below the median wage (i.e., $w_i < 50$), and equal to or above the median wage (i.e., $w_i \geq 50$) for labor market outcomes. For both men and women, we find larger positive effects of the new pension rules on personal income for individuals with a higher wage pre-retirement as compared to workers who belong to the left tail of the wage distribution. For example, for high-wage women we document a permanent increase in yearly personal income equal to approximately 1,000 euros after the SRA as a consequence of the new pension rules. We also observe significant differences in retirement take-up rates. Specifically, the substitution effects are more pronounced for high-wage workers compared to low-wage workers, with a notable disparity among women. For instance, high-wage women born in 1950 are 10.6 percentage points (SE: 0.67 pp) less likely to be retired at age 65 due to the reform, while this difference is only 4.5 percentage points (SE: 0.69 pp) for low-wage women.

For health outcomes, a couple of notable differences arise as well. In Figure A.10 in the Appendix, we document that the probability of having a medication prescription related to anxiety and/or depression symptoms is only statistically significant for the high-wage male workers. This result is in line with the heterogeneity analysis conducted by Grip et al. (2012), who show that mental health effects appear to be larger for men who experience a larger absolute income loss as a consequence of the new pension rules. Moreover, for low-wage women in the treatment group the probability of death is approximately 0.9 probability points (SE: 0.36

pp) higher as compared to the control group. This effect is statistically significant. Conversely, for high-wage women, we observe a temporary increase in mortality between ages 60 and 65. However, this difference diminishes to 0.16 percentage points (SE: 0.34 pp) by age 70, indicating initial disparities in mortality rates that eventually converge.

7.3 Sector

The pension reform in 2005 affected both private and public sector workers' pension wealth. Using matched survey data and administrative data for male public sector workers only, the previous literature investigates the effects of this reform on health and labor market outcomes for a subset of the total Dutch population (Grip et al., 2012; Montizaan et al., 2010; Lindeboom and Montizaan, 2020). In addition to examining the effects of this pension reform on both men and women, we can also analyze its impact by employment sector (i.e., private- or public sector).

We document substantial differences between private and public sector workers for labor market outcomes, as presented in Figure A.11 in the Appendix. For both men and women in the private sector, the pension reform mostly affected workers' probability of leaving the labor force between ages 60 and 65. For men in the public sector, we document relatively small effects of the new pension rules on retirement take-up before age 62, which is in line with the evidence documented by Euwals et al. (2006). This effect becomes economically large later: at age 65, the difference in the probability of retirement between the control and treatment group is equal to 19.3 percentage points (SE: 0.70 pp). For income, we find notable differences between sectors. For private sector workers, the pension reform only systematically increased personal income for men. On the contrary, we document long-lasting effects of the reform on personal income for female public sector workers: after the SRA, yearly gross personal income is approximately 900 euros higher for women in the treatment group, as compared to women in the control group.

For health outcomes, these heterogeneity analyses reveal that most of our main results hold for the private sector workers only (see Figure A.12 in the Appendix). For men in the private sector, we document a similar impact of the reform on mental health related medication prescriptions at age 57 as documented in our main results. On the other hand, we find an economically small and statistically insignificant effect of the reform for male public sector workers. In both cases, the effects of the reform seem to be temporary. For the private sector, we find that women are subject to a 0.94 percentage points (SE: 0.38 pp) higher probability of death at or before age 70 as a consequence of the pension reform. For public sector women, this effect is smaller and statistically insignificant: 0.21 percentage points (SE: 0.43 pp).

Note that the absence of a statistically significant effect on women's mortality in sectors other than the private sector reinforces the validity of our research design. It is highly unlikely that unobserved health shocks – that are unrelated to the reform – would selectively impact the treatment and control groups in the private sector alone. One potential explanation for why we only find health effects for the private sector is that the non-private sectors are unique, with workers being generally more educated, earning higher incomes, and experiencing different working conditions compared to those in other sectors (Lindeboom and Montizaan, 2020; Grip et al., 2012; Montizaan et al., 2010).

7.4 Marital status

Recently, Oral et al. (2024) show that women react strongly to the retirement status of their partners, partly resulting in a social multiplier of the 2005 pension reform of approximately 40%. Given that women's retirement decisions partly depend on their partners' labor market status, (long-term) labor market and health effects may depend on individuals' marital status. To assess this, we re-estimate our main specification (1) for married and unmarried individuals separately.

As documented in Figure A.13 in the Appendix, we find that, on average, unmarried women have an 1.8 percentage point higher probability to retire as a consequence of the new pension rules, opposed to married women. Moreover, we document income effects that are more than three times larger for unmarried women. That is, an unmarried women who is affected by the new pension rules has, on average, a 1000 euros higher gross personal income per year than an unmarried women who was not affected by the reform. This difference is equal to approximately 300 euros for married women. Similarly, we document persistent income effects for unmarried men (see Panel (C) in Figure A.13 in the Appendix) of about 2000 euros per year. We do not document such long-lasting income effects for married men (Panel (A) in Figure A.13 in the Appendix).

For mortality outcomes, we do not find differences in the probability of death between married and unmarried women (see Figure A.14 in the Appendix). However, we do observe heterogeneity in the estimated effect of the 2005 pension reform depending on an individual's marital status for the prescription of mental health related medications. For men, we only document a statistically significant positive effect on prescriptions for unmarried individuals in the short-run (see Panel (A) and Panel (B) in Figure A.14 in the Appendix). This finding is in line with Grip et al. (2012).

8 Conclusion

This paper studies the short-and long run effects of a Dutch pension reform that got introduced in 2005 on labor participation, income, and health outcomes, using rich administrative data from *Statistics Netherlands*. This reform substantially decreased individuals' private pension wealth due to the termination of tax advantages of early retirement schemes. As we observe individuals over time, we are able to control for unobserved individual heterogeneity that is constant across an individual's lifetime.

Looking at labor market outcomes, our results suggest that the decline in income resulting from reduced pension benefits due to the reform is more than outweighed by a positive substitution effect, where labor replaces leisure, leading to an overall increase in personal income. For health outcomes, we provide consistent evidence of temporary detrimental mental health effects as a consequence of this reform for men. For women, we find a statistically significant increase in the likelihood of dying before age 70 as a consequence of the reform. Moreover, our identification approach allows us to disentangle the effect of old-age labor participation from diminished pension benefits. We show that (minimum) age eligibility discontinuities in the SRA and other social security systems, or pension reforms, could serve as an invalid instrument for individuals' retirement status, as (prospects to) changes in income may contribute to the estimated causal effects documented in the existing literature.

Our findings have important implications for public policy. As many developed countries have been reducing both the generosity of pension benefits, in combination with increasing the statutory retirement age, it is important to understand the underlying forces driving potential health differences later in life for individuals affected by such reforms.

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Appendices

A Supplementary material for Chapter 3

A.1 Additional tables

Table A.1: Mapping prescription drugs to chronic conditions

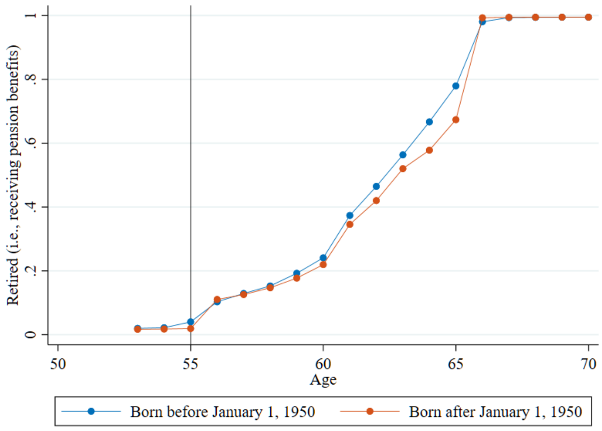
Disease	**ATC4-code**
Coronary disease	B01A, C04A
Cardiac disease	C01, C03C
Hypertension	C02, C03A, C07, C08, C09A, C09B
Rheumatic conditions	H02, M01, M02
High blood cholesterol	C01A
Glaucoma/cataract	S01E
Peptic ulcers	A02A, A02B
Chronic bronchitis/asthma	R03
Anxiety/depression	N05B, N06A, N06B, N05A
Osteoporosis	M05

Notes. This Table shows how the ATC4 codes are mapped into chronic diseases, based on the classifications used in Van Ooijen et al. (2015).

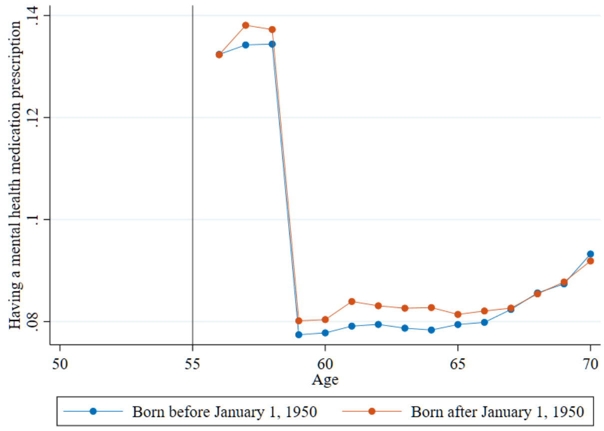
A.2 Additional figures

Figure A.1: Descriptive statistics

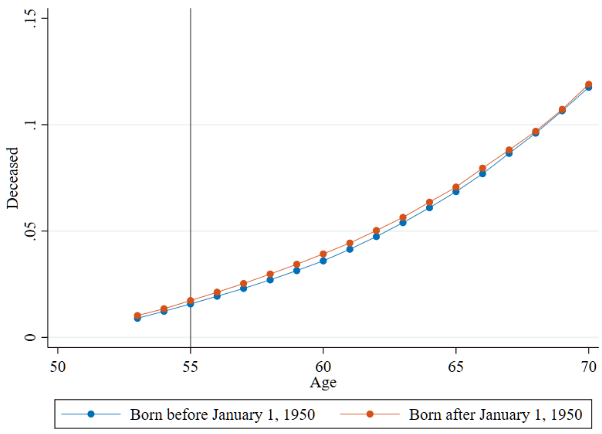
(a) Retirement (i.e., receiving pension benefits)



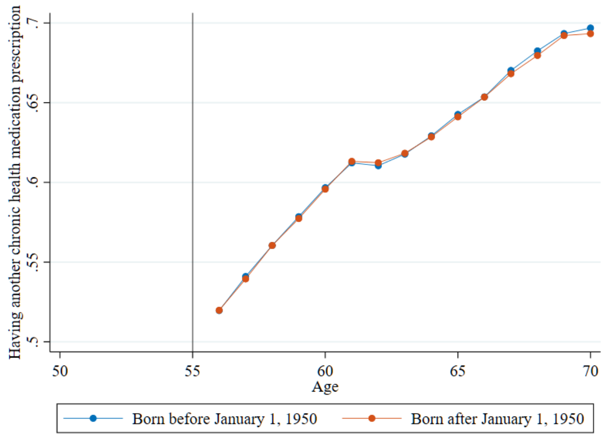
(b) Having a medication prescription for anxiety and/or depression



(c) Deceased



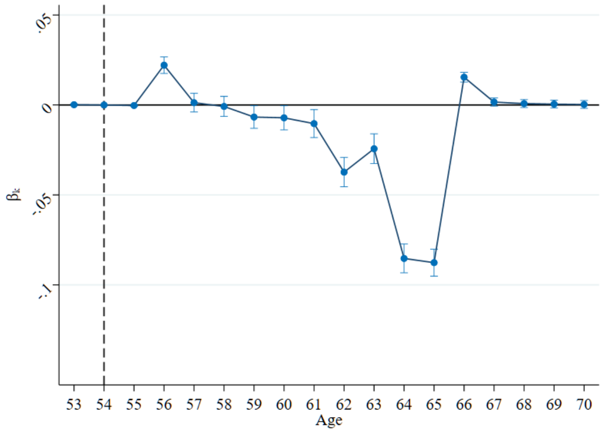
(d) Having another chronic condition



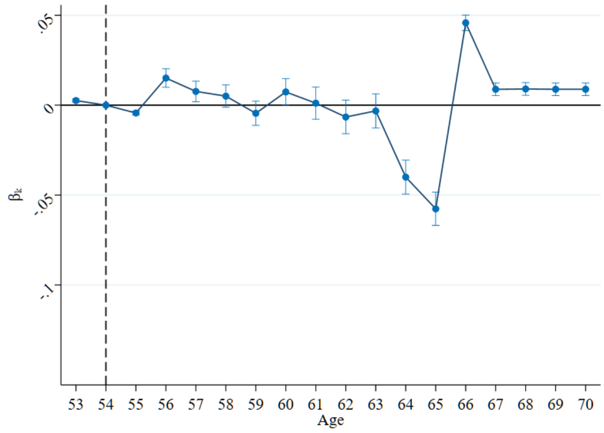
Notes. These figures plot mean outcomes (Panel (B)) across age for the treatment group (i.e., individuals born at or after January 1, 1950) and the control group (i.e., individuals born before January 1, 1950). Panel (A) shows the average retirement rate in case we define retirement as the state of receiving pension benefits. Panel (B) shows the fraction of individuals who have a medication prescriptions for mental health related symptoms. Panel (C) shows the fraction of individuals who deceased. Panel (D) shows the fraction of individuals who have a medication prescription for another chronic condition as indicated in Table A.1 in the Appendix.

Figure A.2: The Effect of Pension Reforms on Retirement

(a) Male



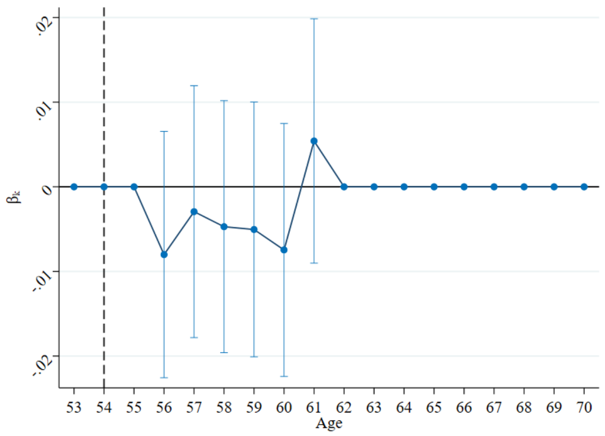
(b) Female



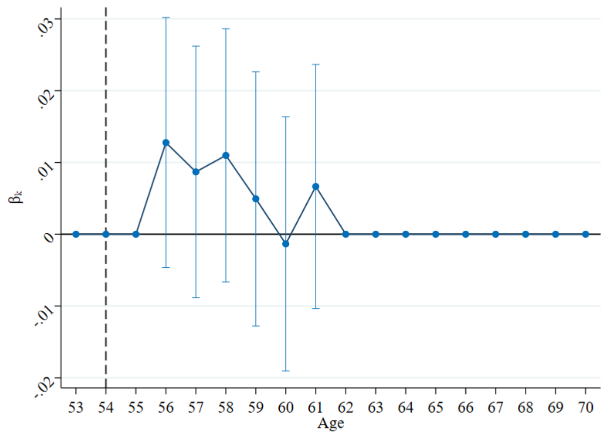
Notes. This figure presents the estimated coefficients β_k for $k = 53, \ldots, 70$ of the pension reform on an individual's probability of retirement (i.e., receiving pension benefits) for men (Panel (A)) and women (Panel (B)) using specification (1), with $\beta_{54} = 0$. We plot the 95% confidence intervals using clustered standard errors at the individual level. For more information on the construction of this definition of retirement, see Section 3.2.

Figure A.3: The Effect of the Pension Reform on Hospitalizations

(a) Male



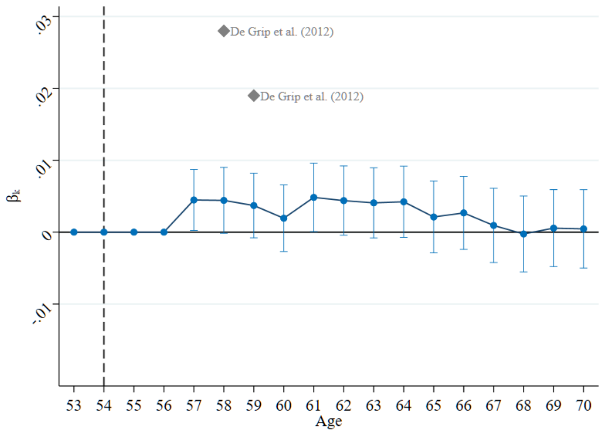
(b) Female



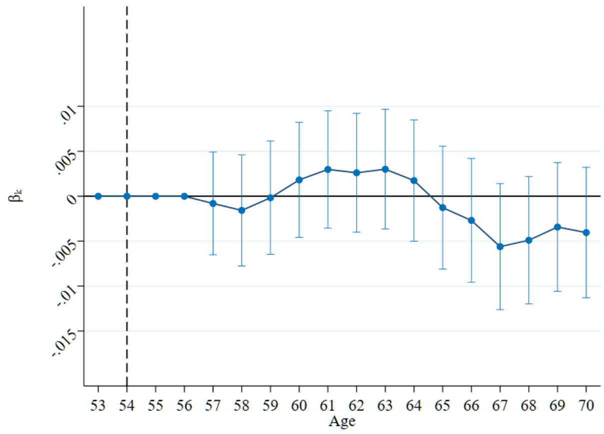
Notes. This figure presents the estimated coefficients β_k for $k = 56, \ldots, 61$ of the pension reform on an individual's probability of being hospitalized for men (Panel (A)) and women (Panel (B)) using specification (1), with $\beta_{56} = 0$. We plot the 95% confidence intervals using clustered standard errors at the individual level. For more information on the construction of the dependent variable, see Section 3.2.

Figure A.4: The effect of the reform on medication prescriptions – including individuals who deceased before or at age 70

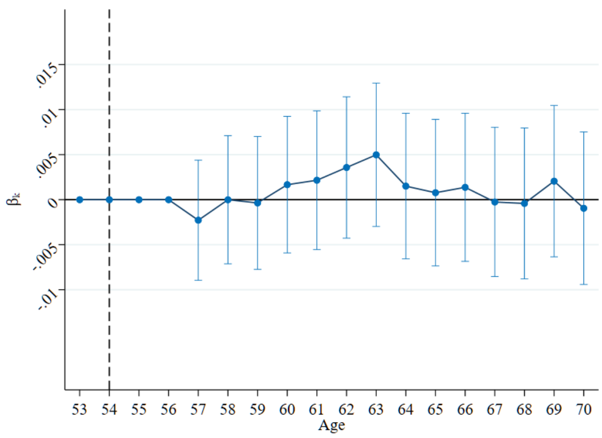
(a) Men – medication prescription for depression and/or anxiety



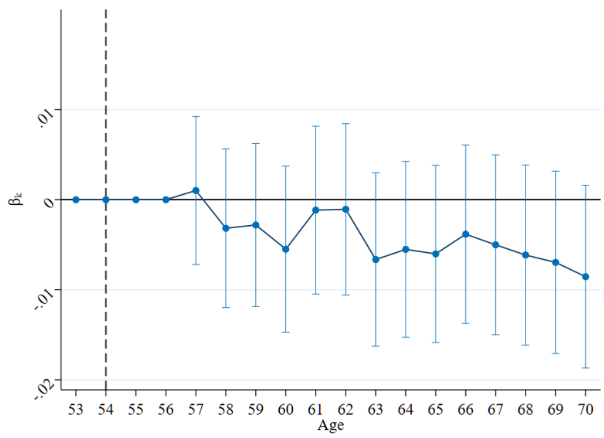
(b) Women – medication prescription for depression and/or anxiety



(c) Men – medication prescription for other chronic condition



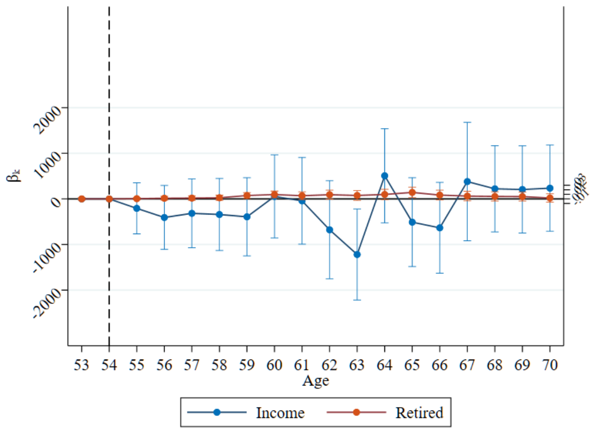
(d) Women – medication prescription for other chronic condition



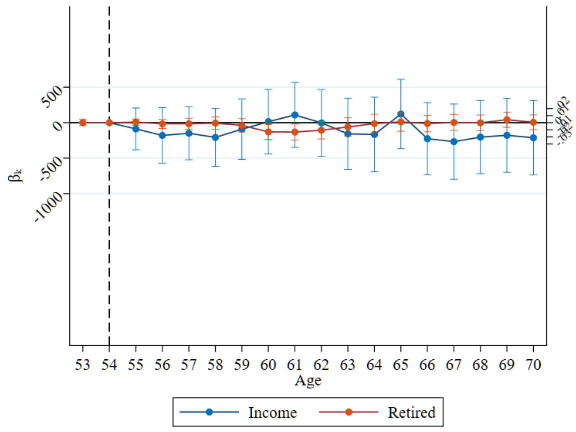
Notes. These figures present the estimated coefficients $\{\beta_k\}$ on medication prescriptions for men (Panel (A) and Panel (C)) and women (Panel (B) and Panel (D)) using specification (1) for $k = 56, \ldots, 70$ (with $\beta_{56} = 0$). Panel (A) and Panel (B) present the results for the binary indicator that is equal to one in case an individual has a medication prescription for depression and/or anxiety symptoms. Panel (C) and Panel (D) present the results for the binary indicator that is equal to one in case an individual has a medication prescription for another chronic condition (see Table A.1 in the Appendix for an overview). We also plot the main coefficient estimates for ages 58 and 59 as documented in Table 1 in Grip et al. (2012) by the gray diamond markers in Panel (A). We plot the 95% confidence interval using clustered standard errors at the individual level.

Figure A.5: PLACEBO TEST IN 1950

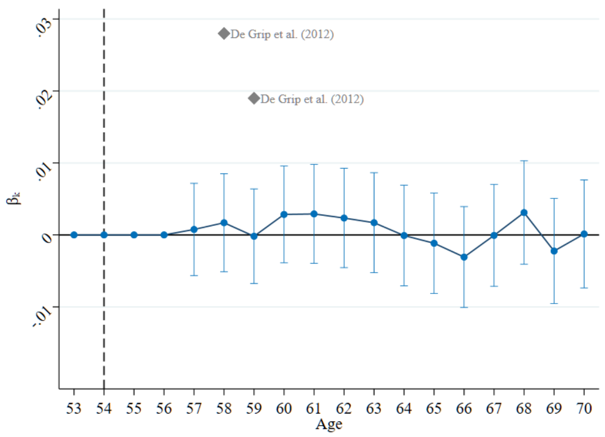
(a) Men – labor market outcomes



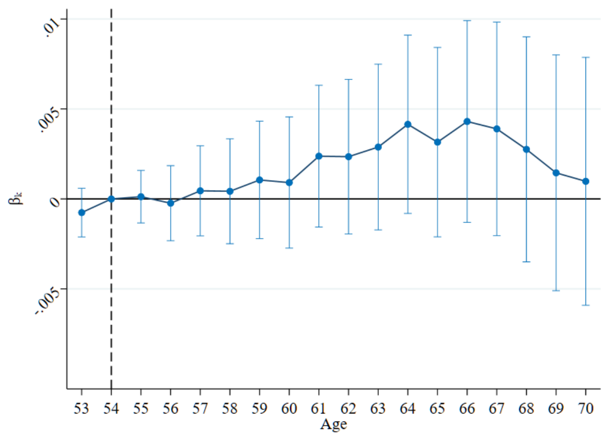
(b) Women – labor market outcomes



(c) Men – medication prescription for depression and/or anxiety



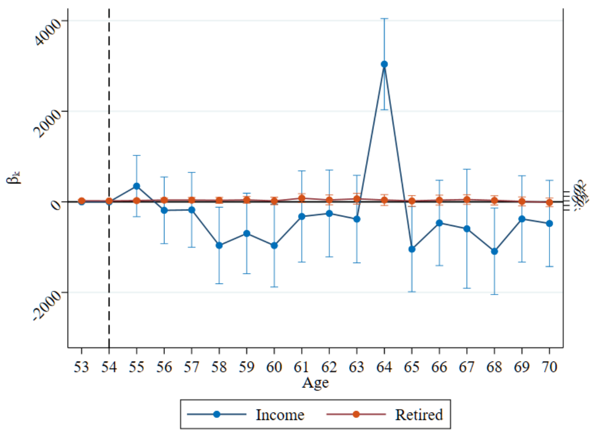
(d) Women – probability of death



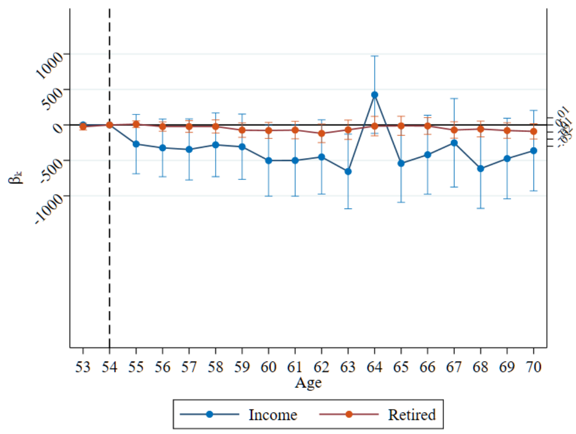
Notes. Panel (A) and Panel (B) in this figure presents the 1950 placebo test on labor market outcomes using specification (1). We let $Z_i = 1$ for all workers who are born between January 1, 1950 and March 1, 1950. We let $Z_i = 0$ for all workers between March 2, 1950 and April 30, 1950. The blue estimates represent the estimated coefficients of $\{\beta_k\}$ for $k = 54, \ldots, 70$ (with $\beta_{54} = 0$) in case the dependent variable is equal to an individual's gross personal income (in euros) in specification (1). The orange estimates represent the estimated coefficients of $\{\beta_k\}$ for $k = 53, \ldots, 70$ (with $\beta_{54} = 0$) in case the dependent variable is equal to a binary indicator that is equal to one in case an individual is retired (i.e., left the labor force), and zero otherwise, in specification (1). Panel (C) presents the estimated coefficients $\{\beta_k\}$ for $k = 56, \ldots, 70$ (with $\beta_{56} = 0$) where the dependent variable is equal to a binary indicator that is equal to one in case a men has a medication prescription for depression and/or anxiety symptoms, and zero otherwise. Panel (D) presents the estimated coefficients $\{\beta_k\}$ for $k = 53, \ldots, 70$ (with $\beta_{54} = 0$) where the dependent variable is equal to a binary indicator that is equal to one in case a women has deceased, and zero otherwise. We plot the 95% confidence intervals using clustered standard errors at the individual level. We also plot the main coefficient estimates for ages 58 and 59 as documented in Table 1 in Grip et al. (2012) by the gray diamonds in Panel (C).

Figure A.6: Placebo test in 1949

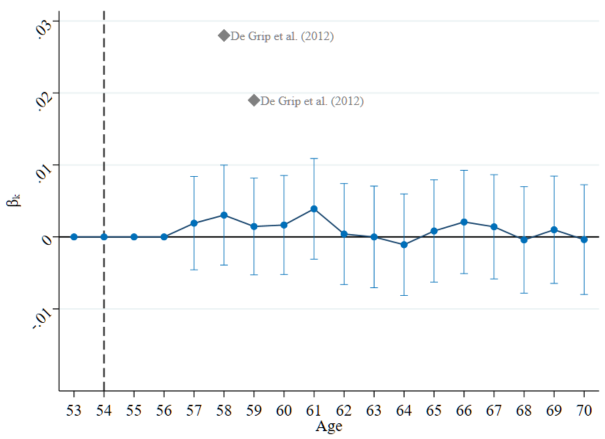
(a) Men – labor market outcomes



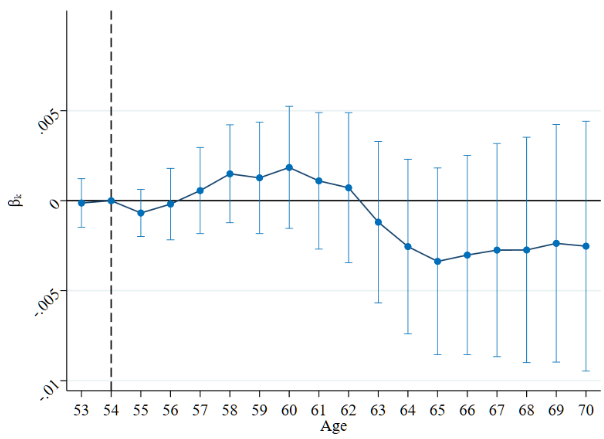
(b) Women – labor market outcomes



(c) Men – medication prescription for depression and/or anxiety



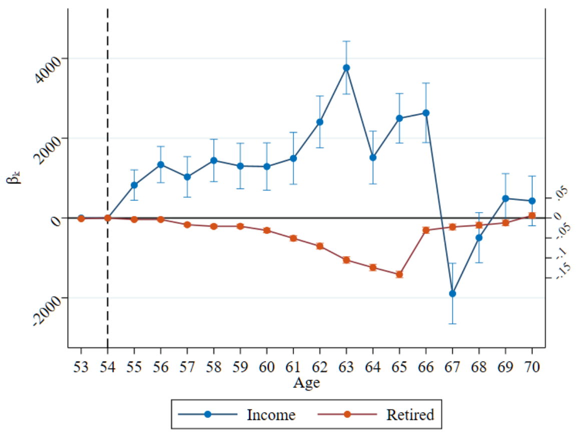
(d) Women – probability of death



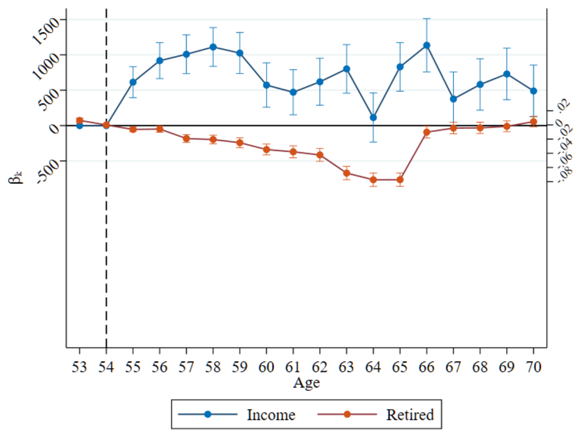
Notes. Panel (A) and Panel (B) in this figure presents the 1949 placebo test on labor market outcomes using specification (1). We let $Z_i = 1$ for all workers who are born between September 1, 1949 and before November 1, 1949. We let $Z_i = 0$ for all workers between November 1, 1949, and December 31, 1949. The blue estimates (values on the left vertical axis) represent the estimated coefficients of $\{\beta_k\}$ for $k = 54, \ldots, 70$ (with $\beta_{54} = 0$) in case the dependent variable is equal to an individual's gross personal income (in euros) in specification (1). The orange estimates (values on the right vertical axis) represent the estimated coefficients of $\{\beta_k\}$ for $k = 53, \ldots, 70$ (with $\beta_{54} = 0$) in case the dependent variable is equal to a binary indicator that is equal to one in case an individual is retired (i.e., left the labor force), and zero otherwise, in specification (1). Panel (C) presents the estimated coefficients $\{\beta_k\}$ for $k = 56, \ldots, 70$ (with $\beta_{56} = 0$) where the dependent variable is equal to a binary indicator that is equal to one in case a men has a medication prescription for depression and/or anxiety symptoms, and zero otherwise. Panel (D) presents the estimated coefficients $\{\beta_k\}$ for $k = 53, \ldots, 70$ (with $\beta_{54} = 0$) where the dependent variable is equal to a binary indicator that is equal to one in case a women has deceased, and zero otherwise. We plot the 95% confidence intervals using clustered standard errors at the individual level. We also plot the main coefficient estimates for ages 58 and 59 as documented in Table 1 in Grip et al. (2012) by the gray diamonds in Panel (C).

Figure A.7: Donut hole

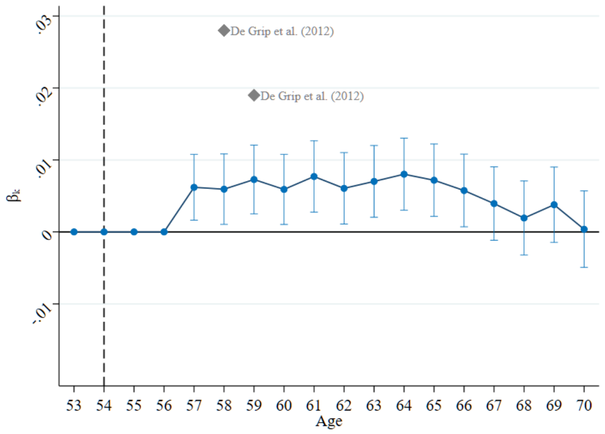
(a) Men – labor market outcomes



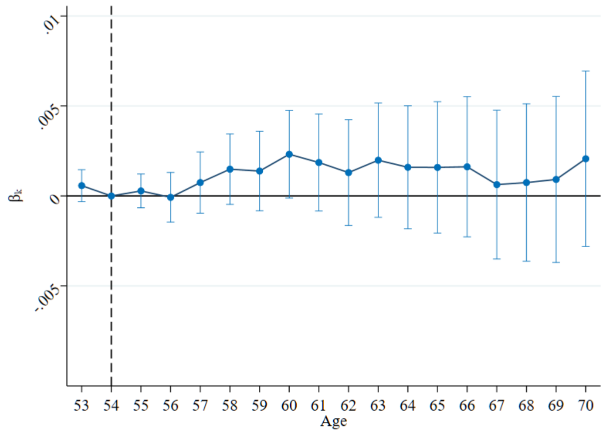
(b) Women – labor market outcomes



(c) Men – medication prescription for depression and/or anxiety



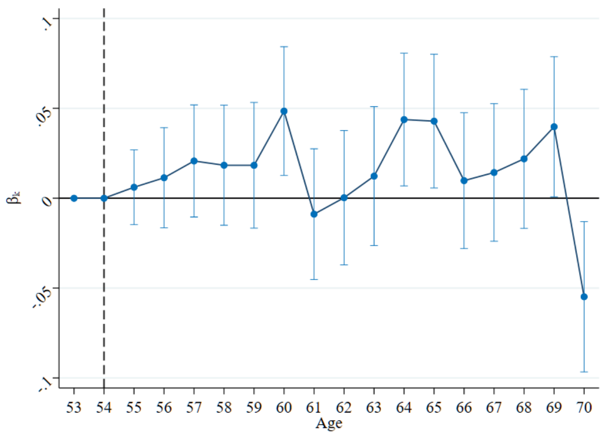
(d) Women – probability of death



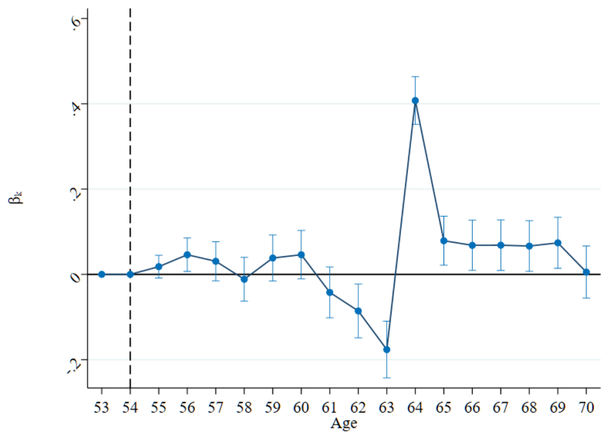
Notes. Panel (A) and Panel (B) in this figure presents the donut hole regression estimates on labor market outcomes using specification (1). We let $Z_i = 0$ for individuals who were born between August 1, 1949 and December 31, 1949, and $Z_i = 1$ for individuals who were born between August 1, 1950, and December 31, 1950. The blue estimates (values on the left vertical axis) represent the estimated coefficients of $\{\beta_k\}$ for $k = 54, \ldots, 70$ (with $\beta_{54} = 0$) in case the dependent variable is equal to an individual's gross personal income (in euros) in specification (1). The orange estimates (values on the right vertical axis) represent the estimated coefficients of $\{\beta_k\}$ for $k = 53, \ldots, 70$ (with $\beta_{54} = 0$) in case the dependent variable is equal to a binary indicator that is equal to one in case an individual is retired (i.e., left the labor force), and zero otherwise, in specification (1). Panel (C) presents the estimated coefficients $\{\beta_k\}$ for $k = 56, \ldots, 70$ (with $\beta_{56} = 0$) where the dependent variable is equal to a binary indicator that is equal to one in case a men has a medication prescription for depression and/or anxiety symptoms, and zero otherwise. Panel (D) presents the estimated coefficients $\{\beta_k\}$ for $k = 53, \ldots, 70$ (with $\beta_{54} = 0$) where the dependent variable is equal to a binary indicator that is equal to one in case a woman has deceased, and zero otherwise. We plot the 95% confidence intervals using clustered standard errors at the individual level. We also plot the main coefficient estimates for ages 58 and 59 as documented in Table 1 in Grip et al. (2012) by the gray diamonds in Panel (C).

Figure A.8: Robustness: the effect of the pension reform on the logarithm of personal income

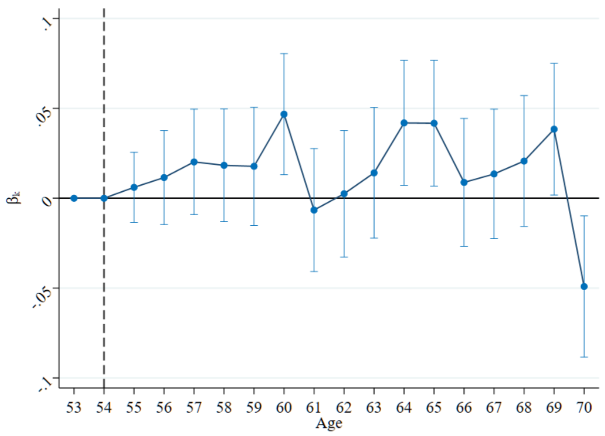
(a) Men – $\text{arcsinh}(y_{ia})$



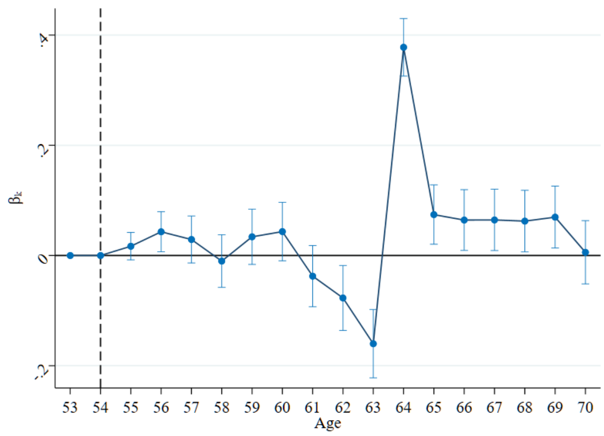
(b) Women – $\text{arcsinh}(y_{ia})$



(c) Men – $\log(1 + y_{ia})$

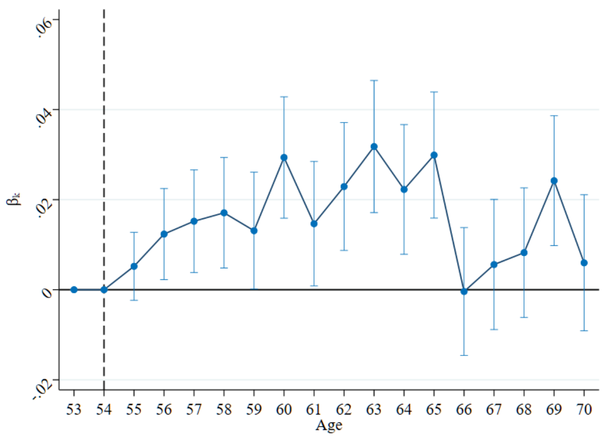


(d) Women – $\log(1 + y_{ia})$

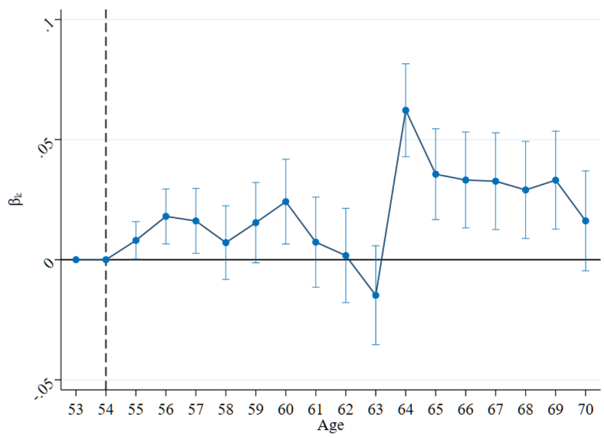


Notes.

(e) Men – $\log(1000 + y_{ia})$



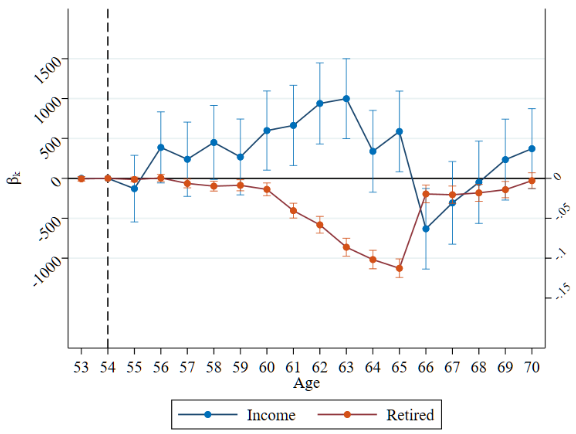
(f) Women – $\log(1000 + y_{ia})$



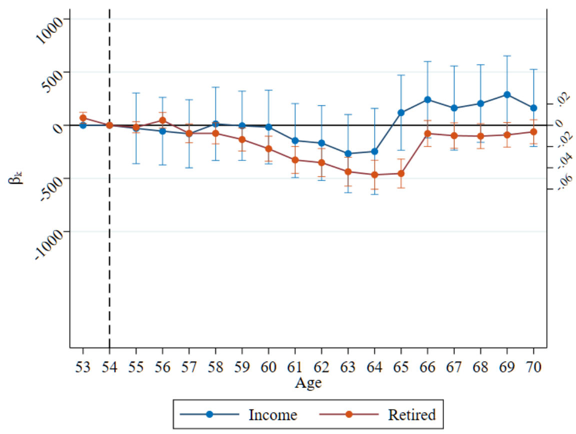
Notes. This figure illustrates the estimated coefficients $\{\beta_k\}$ for $k = 54, \ldots, 70$ (with $\beta_{54} = 0$) for different transformation of the income dependent variable. We plot the 95% confidence intervals using clustered standard errors at the individual level.

Figure A.9: Heterogeneity by pre-retirement wage: The effect of the pension reform on income

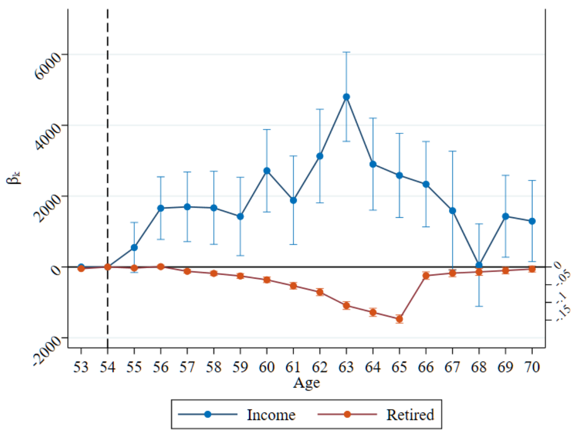
(a) Men $w_i < 50$



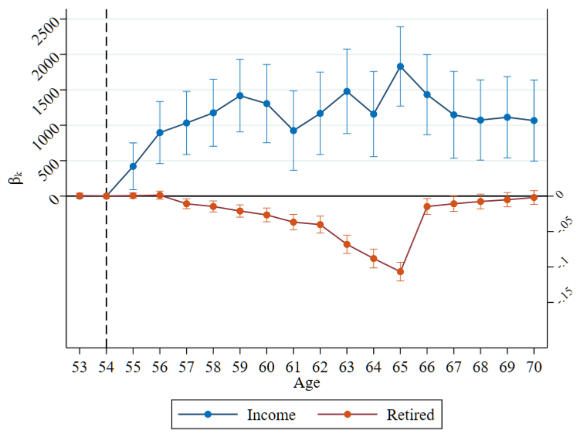
(b) Women $w_i < 50$



(c) Men $w_i \geq 50$



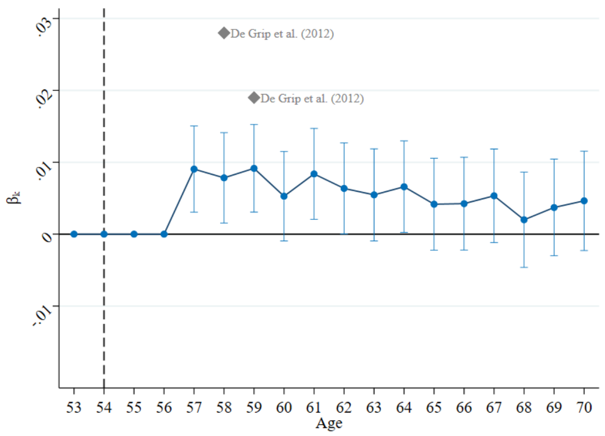
(d) Women $w_i \geq 50$



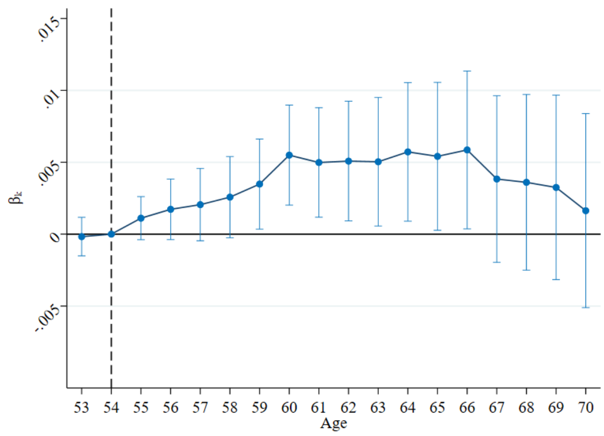
Notes. These figures present the effect of the pension reform on labor market outcomes for low wage individuals (i.e., with a wage rank below 50) in Panel (A) and Panel (D) and high wage individuals (i.e., with a wage rank above 50) in Panel (C) and Panel (D). The blue estimates (values on the left vertical axis) represent the estimated coefficients of $\{\beta_k\}$ for $k = 54, \ldots, 70$ (with $\beta_{54} = 0$) in case the dependent variable is equal to an individual's gross personal income (in euros). The orange estimates (values on the right vertical axis) represent the estimated coefficients of $\{\beta_k\}$ for $k = 53, \ldots, 70$ (with $\beta_{54} = 0$) in case the dependent variable is equal to a binary indicator that is equal to one in case an individual is retired (i.e., has left the labor force), and zero otherwise. We plot the 95% confidence intervals using clustered standard errors at the individual level.

Figure A.10: Heterogeneity by pre-retirement wage: The effect of the pension reform on health outcomes

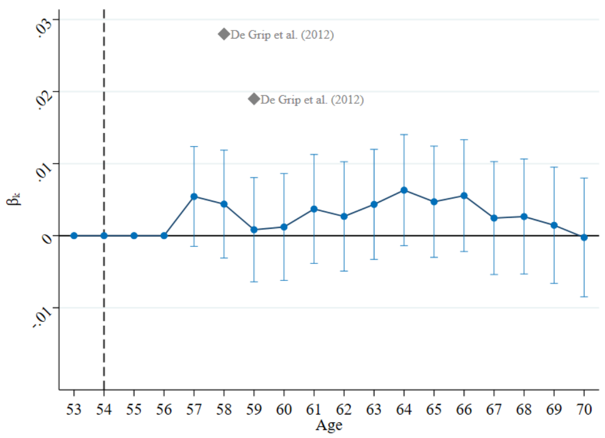
(a) Men $w_i \geq 50$: medication prescription for depression and/or anxiety



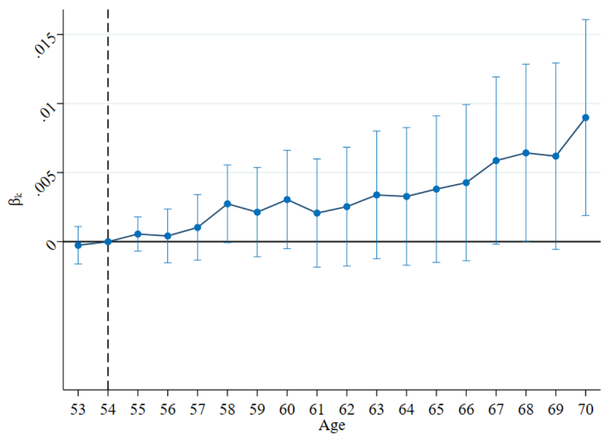
(b) Women $w_i \geq 50$: probability of death



(c) Men $w_i < 50$: medication prescription for depression and/or anxiety



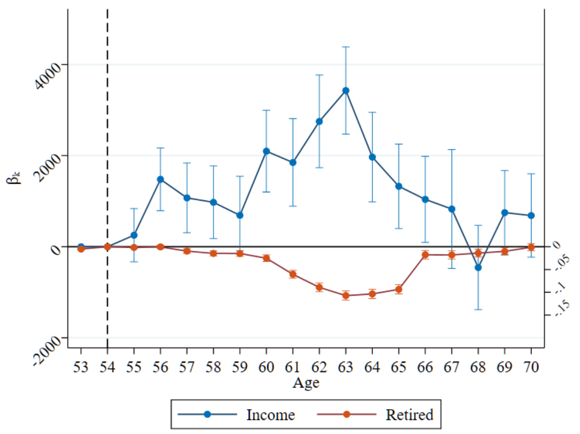
(d) Women $w_i < 50$: probability of death



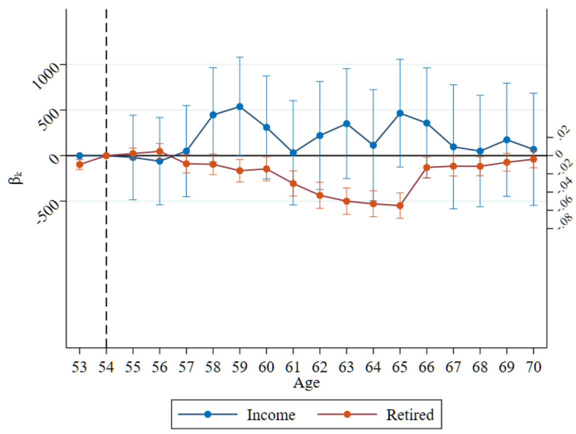
Notes. These figures present the estimated coefficients $\{\beta_k\}$ from specification (1) as documented in our main results for high wage individuals (i.e., with a wage rank above 50) in Panel (A) and Panel (B) and low wage individuals (i.e., with a wage rank below 50) in Panel (C) and Panel (D). Panel (A) and Panel (C) present the estimated coefficients $\{\beta_k\}$ for $k = 56, \ldots, 70$ (with $\beta_{56} = 0$) in case the dependent variable is equal to a binary indicator that is equal to one in case a men has a medication prescription for depression and/or anxiety related symptoms at a particular age, and zero otherwise. Panel (B) and Panel (D) present the estimated coefficients $\{\beta_k\}$ for $k = 53, \ldots, 70$ (with $\beta_{54} = 0$) in case the dependent variable is equal to a binary indicator that is equal to one in case a woman has deceased or before at a particular age, and zero otherwise. Panel (A) and Panel (C) show the main coefficient estimates for ages 58 and 59 as documented in Table 1 in Grip et al. (2012) by the gray diamond markers. We plot the 95% confidence intervals using clustered standard errors at the individual level.

Figure A.11: Heterogeneity by sector: The effect of the pension reform on income

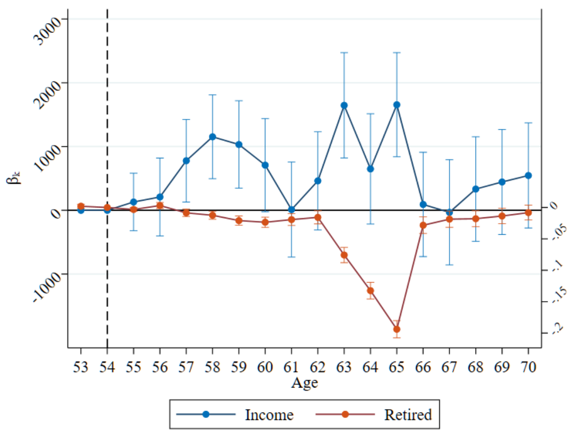
(a) Men – private sector



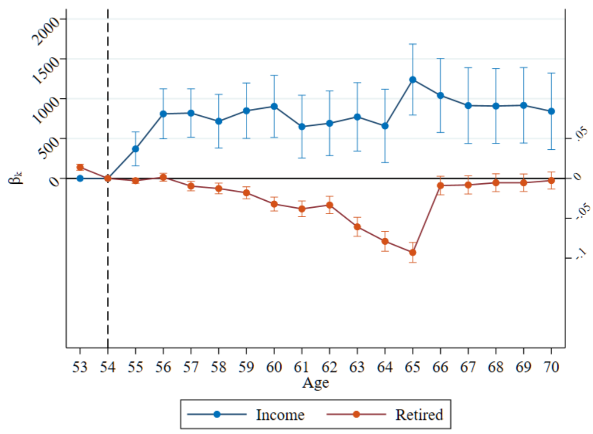
(b) Women – private sector



(c) Men – non-private sector



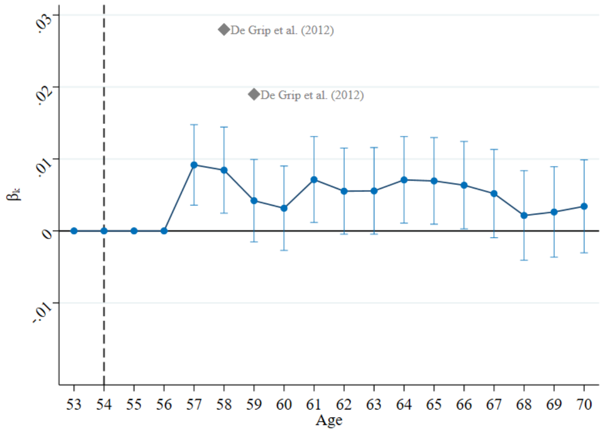
(d) Women – non-private sector



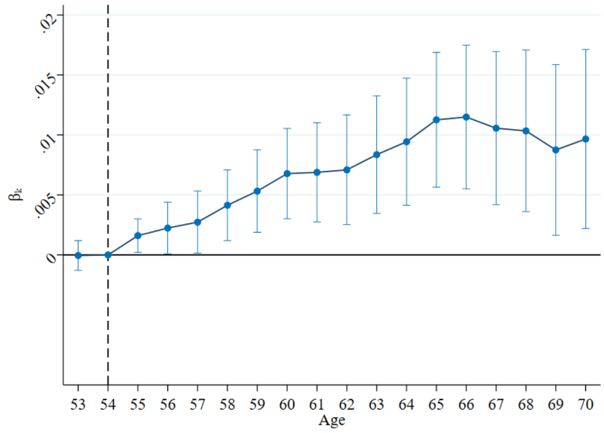
Notes. These figures present the effect of the pension reform on labor market outcomes for private sector workers in Panel (A) and Panel (D) and non-private sector workers in Panel (C) and Panel (D). The blue estimates (values on the left vertical axis) represent the estimated coefficients of $\{\beta_k\}$ for $k = 54, \ldots, 70$ (with $\beta_{54} = 0$) in case the dependent variable is equal to an individual's gross personal income (in euros) using specification (1). The orange estimates (values on the right vertical axis) represent the estimated coefficients of $\{\beta_k\}$ for $k = 53, \ldots, 70$ (with $\beta_{54} = 0$) using specification (1) in case the dependent variable is equal to a binary indicator that is equal to one in case an individual is retired (i.e., has left the labor force), and zero otherwise. We plot the 95% confidence intervals using clustered standard errors at the individual level.

Figure A.12: Heterogeneity by sector: The effect of the pension reform on health outcomes

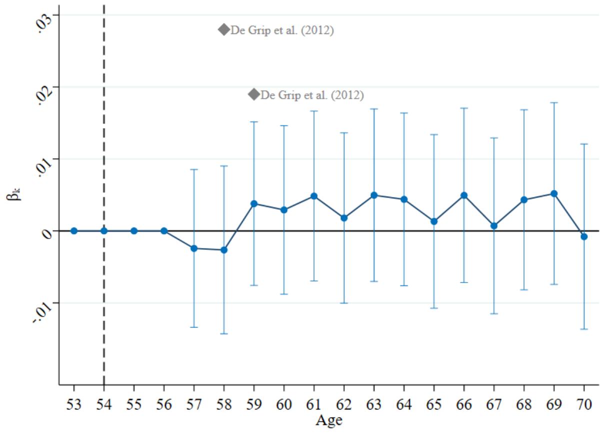
(a) Men, private sector: medication prescription for depression and/or anxiety



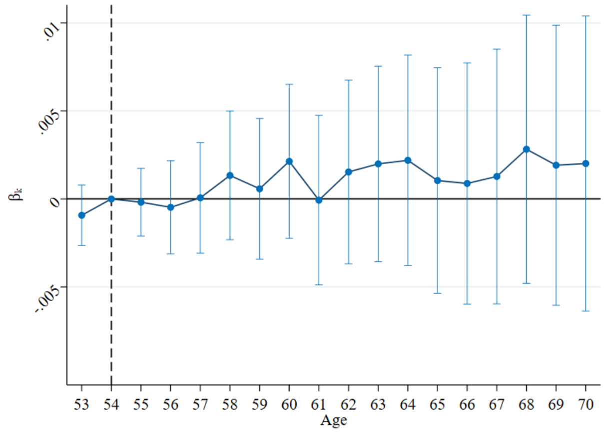
(b) Women, private sector: probability of death



(c) Men, non-private sector: medication prescription for depression and/or anxiety



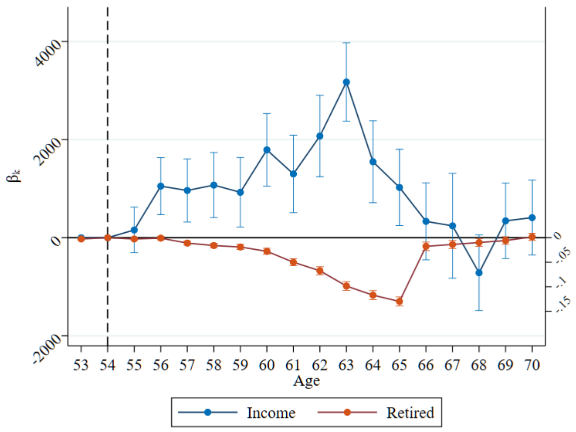
(d) Women, non-private sector: probability of death



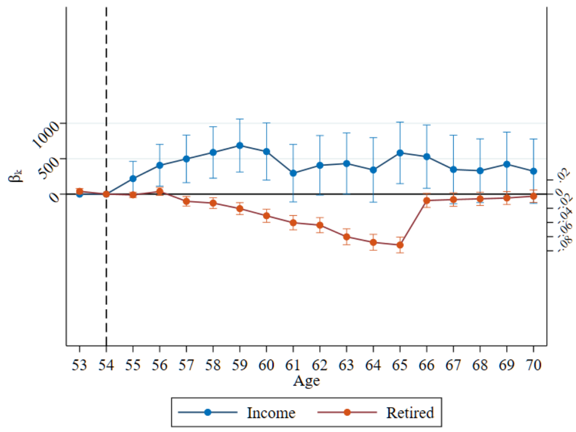
Notes. These figures present the estimated coefficients $\{\beta_k\}$ from specification (1) as documented in our main results for private sector workers in Panel (A) and Panel (B) and non-private workers in Panel (C) and Panel (D). Panel (A) and Panel (C) present the estimated coefficients $\{\beta_k\}$ for $k = 56, \ldots, 70$ (with $\beta_{56} = 0$) in specification (1) in case the dependent variable is equal to a binary indicator that is equal to one in case a men has a medication prescription for depression and/or anxiety related symptoms at a particular age, and zero otherwise. Panel (B) and Panel (D) present the estimated coefficients $\{\beta_k\}$ for $k = 53, \ldots, 70$ (with $\beta_{54} = 0$) in specification (1) in case the dependent variable is equal to a binary indicator that is equal to one in case a woman has deceased or before at a particular age, and zero otherwise. Panel (A) and Panel (C) show the main coefficient estimates for ages 58 and 59 as documented in Table 1 in Grip et al. (2012) by the gray diamond markers. We plot the 95% confidence intervals using clustered standard errors at the individual level.

Figure A.13: Heterogeneity by marital status: The effect of the pension reform on labor market outcomes

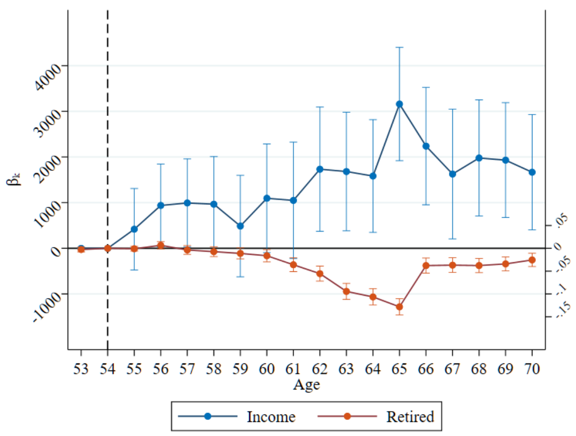
(a) Men – married



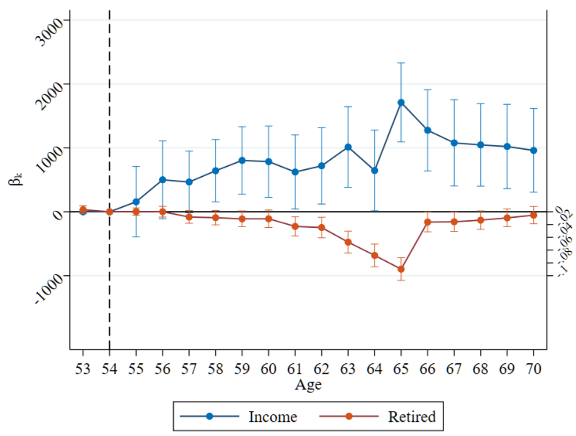
(b) Women – married



(c) Men – unmarried



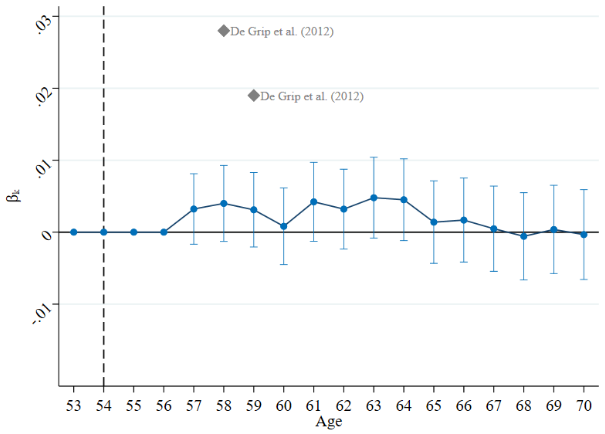
(d) Women – unmarried



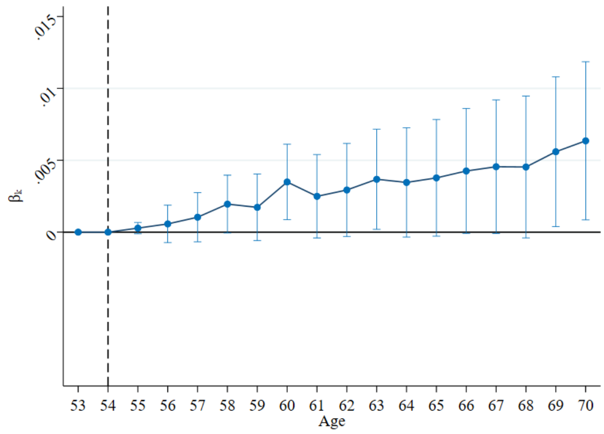
Notes. These figures present the effect of the pension reform on labor market outcomes for married workers in Panel (A) and Panel (D) and unmarried workers in Panel (C) and Panel (D). The blue estimates (values on the left vertical axis) represent the estimated coefficients of $\{\beta_k\}$ for $k = 54, \ldots, 70$ (with $\beta_{54} = 0$) in case the dependent variable is equal to an individual's gross personal income (in euros) using specification (1). The orange estimates (values on the right vertical axis) represent the estimated coefficients of $\{\beta_k\}$ for $k = 53, \ldots, 70$ (with $\beta_{54} = 0$) using specification (1) in case the dependent variable is equal to a binary indicator that is equal to one in case an individual is retired (i.e., has left the labor force), and zero otherwise. We plot the 95% confidence intervals using clustered standard errors at the individual level.

Figure A.14: Heterogeneity by marital status: The effect of the pension reform on health outcomes

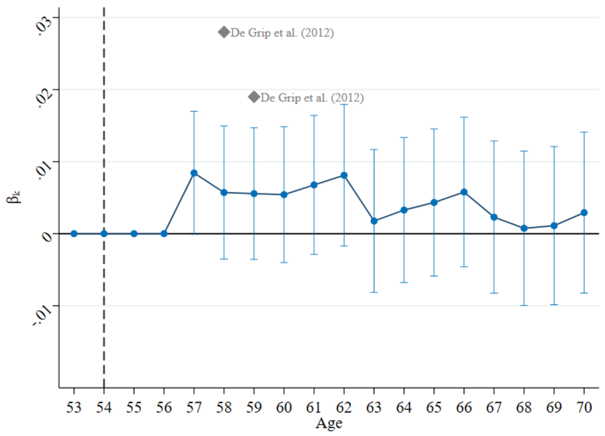
(a) Men, married: medication prescription for depression and/or anxiety



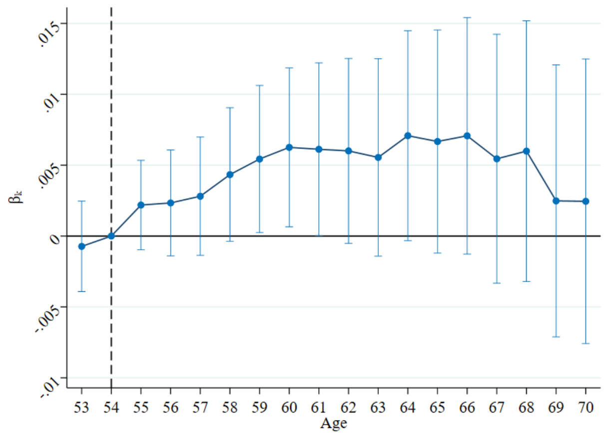
(b) Women, married: probability of death



(c) Men, unmarried: medication prescription for depression and/or anxiety



(d) Women, unmarried: probability of death



Notes. These figures present the estimated coefficients $\{\beta_k\}$ from specification (1) as documented in our main results for married workers in Panel (A) and Panel (B) and unmarried in Panel (C) and Panel (D). Panel (A) and Panel (C) present the estimated coefficients $\{\beta_k\}$ for $k = 56, \ldots, 70$ (with $\beta_{56} = 0$) in specification (1) in case the dependent variable is equal to a binary indicator that is equal to one in case a men has a medication prescription for depression and/or anxiety related symptoms at a particular age, and zero otherwise. Panel (B) and Panel (D) present the estimated coefficients $\{\beta_k\}$ for $k = 53, \ldots, 70$ (with $\beta_{54} = 0$) in specification (1) in case the dependent variable is equal to a binary indicator that is equal to one in case a woman has deceased or before at a particular age, and zero otherwise. Panel (A) and Panel (C) show the main coefficient estimates for ages 58 and 59 as documented in Table 1 in Grip et al. (2012) by the gray diamond markers. We plot the 95% confidence intervals using clustered standard errors at the individual level.