Health and labour market effects of an abrupt and unanticipated rise in women retirement age. Evidence from the 2012 Italian pension reform.

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Abstract

Population ageing is prompting governments around the world to increase the retirement age. However, not all workers may be equally able to extend their working lives as they may face adverse health consequences. In this article, we examine the health and labour market effects of an Italian pension reform that suddenly increased the normal retirement age for women by three to seven years. To do this, we use linked labour and healthcare administrative data, jointly with survey data and difference-in-difference methods. Our results show that the reform was effective in postponing retirement, as pension claiming dropped by 25 percentage points (pp) while the probability of working increased by around 11 pp during the ages 60 to 63. However, there were side effects as the reform also pushed a relevant fraction of women out of the labour market, into unemployment and disability pension, while increasing sick leaves among those who continued to work. The reform also increased hospitalization related to mental health and injuries among affected women. These side-effects were concentrated in the short-term and driven by those with previously low health status. Our results suggest that undifferentiated increases in pension age, independently of the health condition of the worker, might harm the health and the working capacity of more vulnerable workers.

Keywords: pension reform, health effects, population ageing

Classification-JEL: I10, J26

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1. Introduction

Population ageing is prompting governments around the world to increase the retirement age to guarantee the sustainability of pension systems. However, these policies might also affect the health of workers who are forced to prolong their working lives. This could be especially true for workers in precarious or physically demanding occupations, as in Italy prolonged exposure to potentially harmful working conditions and precarious contractual arrangements are particularly diffused among older workers, concerning more than one third of 60-64 years manual workers in 2013 (d'Errico et al. 2022). Moreover, raising the retirement age may force workers with health problems to continue working in jobs characterized by adverse conditions, which may further deteriorate their health. This may result in a higher incidence of sick leave, professional diseases, work-related injuries, and inflow into programs other than old-age pension. If this is the case, savings in the public pension system might be partially offset by higher social security and healthcare costs. Still, the literature on the health effects of increasing the retirement age is far from conclusive and largely depends on the context and the health outcomes under study (Pilipec et al 2021, Filomena and Picchio 2021, Odone et al. 2021, Vigezzi et al. 2021)

In this article we study the health and labour market effects of the 2011 Italian pension reform that suddenly increased the normal retirement age for some cohorts of women by up to seven years (from 60 to 67). Our implementation strategy compares neighbouring birth cohorts and age groups who were affected by the reform with those who could retire right before the reform, using several difference-in-difference specifications. We utilize administrative social security data to look at the effect of the reform on the labour market, assessing the employment-retirement transition and the uptake of working pensions, disability benefits and sick leaves. We further link this data with the national register of hospitalizations to look at the effect of the reform on morbidity measurable through hospital admissions using information on the main diagnosis. This data allows us to carefully examine which women, with respect to previous socioeconomic and health status, are the most affected by the reform. Additionally, we use survey data to look at the effect of the reform on a set of both physical and mental self-reported health variables, health behaviours and healthcare use.

Our results show that the reform was effective in postponing retirement, as pension claims dropped by about 25 percentage points (pp), and in increasing working rates between 10 to 12 pp among the ages 60 to 63, although it also increased the probability of unemployment or inactivity in a similar magnitude. Additionally, we report a significant substitution effect towards disability and an increase in sick leaves among those who continue to work by around 17-24% with respect to the pre-reform mean. The reform also increased the probability of hospitalizations caused by injuries and mental health conditions, but only in the short-term at the ages of 61 or 62, where the reform raised employment rates the most. However, it did not have any detectable immediate effect on physical and mental self-reported outcomes, nor on

cognitive decline. Actually, cohorts who were forced to extend their working lives had a decrease in body mass index (BMI).

Our heterogeneity analysis shows that workers engaged in manual labor, with lower wages, and in worst health were the most influenced by the retirement age increase. Among them, we find the largest delay in pension claiming, work-life extension and inflow into disability and unemployment. Turning to the effects of the reform on hospitalizations, we find no significant differences by occupation (blue-collar vs white-collar) or salary or education; however, a clear gradient with respect to previous health appears, something that has been overlooked in previous literature. Our results show that those with worse health status, as measured by the prevalence of hospitalizations prior to the reform, are driving the increase in injuries and mental health hospitalizations. The reform increased the probability of mental health hospitalization by 0.8 pp for the group with the poorest health, whereas it had no effect on the group with better health. Similarly, the effect on injuries hospitalizations was higher for women with more pre-reform hospitalizations. This health gradient is also present in the substitution effects toward disability. The increase in the probability of disability was 1.1 pp for those with no pre-reform hospitalization, 2.9 pp for those with at least one hospitalization and 4.3 pp for those with at least two hospitalizations. These results show that the pension reform adversely affected the health and working ability of women who already had a poorer health status.

Previous research on the effect of retirement on health summarized in a recent meta-analysis is inconclusive (Filomena and Picchio, 2021). Most of the studies included in the meta-analysis (59%) found no statistically significant effect, while 28% found that retirement improved health. These studies however employ different health outcomes (mental health, physical health, health care utilization, mortality, cognitive abilities, etc.) and retirement may have exert different effects on distinct dimensions of health. For example, while previous literature quite unanimously tends to find that retirement is detrimental to cognitive abilities (for example Mazzonna & Peracchi, 2017; Celidoni et al., 2017), when mental health outcomes are considered, retirement is generally found to be beneficial, as shown by a recent meta-analysis on the subject (Odone et al., 2021).

The literature that directly focuses on the effect of pension reforms increasing retirement age also provides mixed evidence (Pilipiec et al., 2021), showing a lack of understanding of the causal effect of pension reforms on health of older workers. Still, a growing number of causal studies suggest that increasing retirement age may have detrimental effects on the health of workers exposed to the worst working conditions (Ardito et al., 2020; Belloni et al., 2016; Blake & Garrouste, 2019; Carrino et al., 2020; Eibich 2015, Shai 2008). On the contrary, null effects are found on those working in white-collar occupations and in general among workers exposed to better working conditions. For example, Carrino et al. (2020), exploiting the 2010 UK pension reform, showed that rising the State Pension age reduces

physical and mental health among women from routine-manual occupations only. Similarly, Ardito et al. (2020) found that postponing retirement increases the risk of hospitalizations for cardiovascular diseases (CVDs) only among low socioeconomic status workers.

Some of the studies that look at the effect of increasing the retirement age examine pension reforms that are announced several years in advance allowing workers to adapt their expectations and work careers (Carrino et al., 2020). In this sense, Bertoni et al. (2018) showed that the announcement of a pension reform in Italy aimed at postponing the minimum retirement age improved the health behaviours of workers who had their potential work horizon extended, even before retirement. As a consequence, some of the findings of the pension reforms that were announced can be affected by the anticipation of health behaviours. This could mask some potentially harmful effects of postponing retirement. Other pension reforms, although announced with a limited lead-in period, increased the pension age only gradually. This meant increasing the pension age of cohorts closer to retirement just by a few months, while only drastically increasing pension ages for younger cohorts who are still far from retirement and can therefore also adapt their expectations and work careers (Shai 2018, Frimmel and Pruckner 2020).

Our article provides four key contributions to the literature. First, unlike previous studies, we examine a pension reform that was at the same time both drastic and implemented immediately after its announcement—therefore, we can discard any anticipation effects. Second, our administrative healthcare data by diagnosis allow us to focus on hospitalizations which according to well-established aetiologies are found to be associated to work-related exposures (EuroSafe 2014; Theorell et al., 2015; Hulshof et al., 2021). In particular, we focused on hospitalizations from injuries, mental health, stroke and myocardial infarction and musculoskeletal hospitalizations. Third, we carefully examine which women are most affected by pension age rises with a particular focus on the health gradient in the response to the reform. Additionally, following previous literature we also inspect heterogeneous effects with respect to the type of job and socioeconomic status. This provides important policy implications for future pension reforms who might differentiate retirement age by these individual characteristics in order to avoid unintended negative effects. Fourth, whereas the 2011 Italian reform has been previously studied vis-à-vis some labour market outcomes such as sick leave (Moscarola et al., 2016), old-young worker substitution (Bertoni and Brunello 202) and spillovers on co-workers (Bianchi et al., 2021), this is the first study to look at the health, employment and program substitution effects of the reform, focusing on a rich set of previously unexplored outcomes.

Our article is organized as follows. Section 2 describes the Italian pension system and the 2011 reform in detail. Section 3 describes the datasets we use. Section 4 details the methods and econometric specifications. Section 5 reports the main results for both labour and market outcomes. Section 6 provides a general discussion of the results. Lastly, in section 6 we conclude with policy implications.

2. Institutional setting

The reform was approved on 22 December 2011 after a "technocratic" government unexpectedly came into power in November 2011 to manage a fiscal and political crisis. The reform became effective on 1 January 2012 and therefore could not be anticipated. The reform induced both women and men to retire later by increasing age thresholds for both old age pensions and minimum years of contributions for seniority pensions. Importantly, individuals who acquired the right to a public pension before the reform could still retire after it under the pre-reform rules (grandfathering clause) (Carta and De Philippis, 2021). In this article, we focus on the case of women because they were the most affected by the reform. The reform established that a statutory pension age of 67 for both men and women should be reached by 2019. This implied an abrupt increase of 7 years for women (60 to 67) and a smaller and more gradual increase of 2 years for men (65 to 67).

The Italian system has two main public pension schemes: old-age and seniority. In Table 1, Column (1) we show the statutory pension age (old-age pension) per year and job sector, as set before and after the reform. The reform especially affected the women working in the private sector for whom retirement age increased from 60 in 2011, to 62 in 2012 and progressively to 67 in 2019. The minimum contribution years to receive an old-age pension remained unchanged at 20. Additionally, workers who had 5 or more years of contribution could retire at 70 (Carta et al., 2021).

Seniority pension required 40 years of contributions prior to the reform. This increased to 41 years and 1 month in 2012, and up to 42 years in 2019 (Table 1, column 3). Another mechanism to access a seniority pension was through the "quota" system, which required a combination of age and years of contribution. For women in the private sector, the quota system required 35 years of contribution and being 57 years old in the period 2004-2007, and being 59 years old in 2008-2009 (Table 1, Column 2). This system was abolished with the reform.

Early retirement was possible before reaching the old-age and seniority pension requirements through the so-called "women's option" (*opzione donna*), which allowed women to retire around 4 years before the statutory age (Bovini and Paradisi, 2019). Still, this option was hardly used due to a substantial reduction in the pension amount (Bovini and Paradisi, 2019). Although the take-up of "women's option" increased with the reform, only 20% of the eligible women took it in the peak year 2015 (Bovini and Paradisi, 2019).

Our implementation strategy relies on comparing neighbouring cohorts who were differently affected by the reform due to the increase in the statutory pension age (old-age pension). In Table 2, we report the age of each cohort by year, signalling (in dark grey) whether each cohort could retire at a particular year, based on old-age pension requirements for women working in the private sector. We also signal (in light grey) whether each cohort could retire under the quota system (i.e., conditional on having more than 35 years of contribution). We chose to use the private sector retirement requirements for two reasons. First,

the age threshold for women working in the private sector is always the lowest as compared to public sector and self-employed women. Therefore, the lowest normal retirement age for each cohort can be considered that of the private sector workers. Second, 79% of employed women work in the private sector (ISTAT 2012 and 2017); hence, most women in each cohort will be exposed to the private sector retirement rules. Therefore, Table 2 essentially shows the first year in which each cohort could access a public pension if the worker has less than 40 years of contribution (i.e., having not reached the contribution threshold necessary for a seniority pension).

The reform created sharp differences in retirement ages among neighbouring cohorts. Whereas women born in 1951 were eligible for retirement in 2011 by the age of 60 (or in 2010 under the quota system), women born in 1952 were only eligible for retirement from 2015 at the earliest, when turning 63[1]. Note that this is a result of the sudden increase in retirement ages of the reform, since prior to the reform the 1952 cohort were expecting to access a public pension as early as 2012, when turning 60 years old (Table 1). Therefore, their access to public pension was suddenly postponed by 3 or more years. In our main specification, we compare labour market trajectories and hospital outcomes of these two cohorts over time in a difference-in-difference setting (See Section 4).

[1] Only those born between January-March 1952 were able to retire from 2015 when turning 63, those born from April to December 1952 could retire between the ages 64 to 67 (Table 2).

Table 1 – Requirements to receive a public pension before and after the reform

	A- Before reform					
	(1) Old-age pension, contribution years>=20 (*minimum age*)			(2) Quota, contribution years >=35 (*minimum age*)		(3) Seniority pension (*contribution years*)
Year t	Private sector	Public sector	Self-employed	Private and public sector	Self-employed	All
2004-2005	60	60	60	57	58	38 [b]
2006-2007	60	60	60	57	58	39 [b]
2008	60	60	60	59	60	40
2009	60	60	60	59 [c]	60	40
2010	60	61 [a]	60	60 [d]	61 [d]	40
2011	60	61 [a]	60	61 [d]	62 [d]	40
2012	60	65	60	61 [d]	62 [d]	40
2013	60Y 3M	65Y 3M	60Y 4M	62Y 3M [d]	63Y 3M [d]	40
2014	60Y 4M	65Y 3M	60Y 4M	62Y 3M [d]	63Y 3M [d]	40
2015	60Y 6M	65Y 3M	60Y 4M	62Y 3M [d]	63Y 3M [d]	40
	B- After reform					
2012	62	66	63Y 6M			41Y 1M
2013	62Y 3M	66Y 3M	63Y 9M			41Y 5M
2014	63Y 9M	66Y 3M	64Y 9M			41Y 6M
2015	63Y 9M	66Y 3M	64Y 9M	*Quota system abolished*		41Y 6M
2016-2017	65Y 7M [e]	66Y 7M	66Y 1M			41Y 10M
2018	66Y 7M [e]	66Y 7M	66Y 7M			41Y 10M
2019-2020	67Y [e]	67Y	67Y			42Y 3M

Notes: Y: Years, M: Months. This table is based on Bertoni and Brunello (2021) and Centro Studi e Ricerche di Itinerari Previdenziali (2019). It reports the conditions (age and contribution years) required to access a public pension for each year, pre and post Fornero reform. Rows in grey colour display the conditions required to get a public pension as it was expected in 2011, before the Fornero reform was announced. Rows in black colour display the actual requirements in place at each year. Column (1) displays the minimum statutory age of retirement, conditional on having accrued 20 years of contribution (i.e.: old-age pension). Column (2) displays the minimum age required to retire for those who had 35 or more years of contributions ("quota" system pension). Column (3) displays the minimum contribution years required to retire under the seniority pension rules.

[a] Set by the law n. 122/2010 (Source: Centro Studi e Ricerche di Itinerari Previdenziali (2019). [b] Self-employed needed already 40 years of contribution to get a seniority pension. [c] From 7/2009 had to be 60 years old or have at least 36 contribution years. [d] or one year younger if contribution years >=36. [e] women with at least 60 years old and 20 contribution years by 2012, could also retire upon turning 64Y 7M from 2016 onward (Marno, 2012, p. 41)

Table 2 – Normal and Early Retirement age per cohort and year for women who did not reach the seniority pension (< 40 years of contribution)

	Year																
Cohort	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020
NRA [a]	60	60	60	60	60	60	60	60	62	62,3	63,9	63,9	65,7	65,7	66,7	67	67
ERA [b]	57	57	57	57	59	59	59	60	no	no	no	no	no	no	no	no	no
1948	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72
1949	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71
1950	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70
1951	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69
1952	52	53	54	55	56	57	58	59	60	61	62	63 [c]	64 [d]	65 [e]	66 [e]	67	68
1953	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67
1954	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66
1955	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65

Notes: This table is based on the retirement requirements for women in the private sector in place at each year as summarised in Table 1, Column (1). ERA: Early Retirement Age

Cells in dark grey indicate that they have reached the statutory age for old-age pension at that particular year. Cells in light grey indicate if they could retire under the quota system (i.e.: more than 35- or 36-years contribution) in that particular year. Letter superscripts clarify when only a partial

[a] "NRA": Normal Retirement Age

[b] "ERA": Early Retirement Age

[c] Only those who were 63 years 9 months by 2015 (i.e.: born before March 1952) could retire.

[d] Only those who were 64 years and 7 months old in 2016 (i.e.: born before May 1952) if they had at least 20 years of contribution by 2012 (Bianchi et al 2021, appendix B)

[e] Only women who were 65 years 7 months by 2017 (i.e.: born before May 1952) could retire, and all women born in 1952 if they had at least 20 years of contribution by 2012 (Bianchi et al 2021, appendix B)

3. Data

We use two different datasets:

3.1. The Work Histories Italian Panel linked with healthcare data (WHIP – Health)

WHIP – Health is based on the Work Histories Italian Panel (WHIP), a database of individual work histories derived from a 7% random sample of individuals insured by the Italian social security administration (INPS). WHIP provides information on the main episodes of subjects' working lives: employment and unemployment, private employee contracts, atypical contracts, self-employment, and retirement between the years 1985 and 2015. This database has been linked with data on hospital discharges from all public and private hospitals in Italy, from the National Archive of Hospital Discharges. It includes information on each hospital admission between the years 2005 and 2015, including the main diagnosis based on the International Classification of Diseases (ICD-9). For more details on the WHIP-Health dataset see Bena et al (2012).

Since our main identification strategy compares the first affected cohort (born in 1952) with a neighbouring unaffected cohort (born in 1951), our WHIP sample is formed by 9,677 women born in 1951 or 1952 who were employed at 53, followed from age 53 to age 63. Our final panel has 105,122 observations after excluding missing values due to mortality; hence the final panel is not perfectly balanced since 272 women (2.8%) died before the age of 63[2].

We use WHIP-Health to explore the following main labour market outcomes: "employment", "receiving a work pension", "receiving a disability pension", and as a residual category, "unemployment or inactivity". Each outcome is defined as a binary variable that switches on when the person is observed in the labour market state at any moment of the year[3]. As far as for the "employment" outcome, at least one-week job spell should be observed in the year. The outcome "receiving a work pension" includes all types of pensions, i.e., old-age and any early retirement options. The outcome "receiving a disability pension" includes all types of pensions or benefits granted to persons whose work capacity is severely reduced by health. For both work and disability pension, we know the exact start and termination dates. We define a residual category as "unemployed or inactive" to include women not observed in employment nor receiving any pension for the entire year. Hence, they include inactive and unemployed individuals, with or

[2] Our results hold if we use instead a balanced panel data (i.e.: including only the 9405 women observed during the whole age range from 60 to 63, making a total sample of n = 103,455). This suggests that differential mortality is not explaining our results. These results are available upon request.

[3] Note that these four labour market outcomes are not mutually exclusive, although 95% of person-year observations fall in one and only one labour market state. For instance, 0.1% of our sample is simultaneously working and receiving disability benefits. See Table A1 of Appendix A for descriptive statistics of each labour market outcome.

without unemployment benefits. In addition, we look at the probability of taking at least a week of sick leave[4] in the subsample of employed in a blue-collar job, since we lack reliable information on sick leaves taken by white-collar or executives. This already serves as a health outcome and indirectly tests for the health consequences of extending the working life.

We then focus on four groups of hospitalizations related to work, identified through the ICD-9 of the main cause for hospitalization, and based on previous epidemiological literature. In particular, we focus on hospitalizations for: 1) injuries (ICD-9: 800-959), a substantial part of which would occur at work (EuroSafe 2014); 2) mental disorders (ICD-9: 290-319), whose risk can be increased by exposure to psychosocial factors at work (Theorell et al., 2015; Stansfeld et al., 2006); 3) stroke and myocardial infarction (ICD-9: 410-414, 430-438) as working conditions may play a role in the acceleration of stress pathogenic mechanisms leading to an increase in blood pressure and triggering the adoption of unhealthy behaviours (Brunner 1997; Marmot et al., 1997; Pollitt, Rose, and Kaufman 2005; Siegrist 1996). Finally, we include 4) musculoskeletal disorders potentially associated to mechanical overloading at work, including damaging of the joints at work from lifting heavy weights or awkward postures (Hulshof et al., 2021) such as spinal disorders and osteoarthritis, nerve compression from repetitive movements, such as carpal or radial tunnel syndrome (Newington et al. 2015).

3.2. Survey of Health Ageing and Retirement (SHARE)

We use the Italian sample of SHARE, a multidisciplinary microdataset with information on health and socioeconomic conditions of the 50+ population. Our initial sample includes 2,952 women aged between 60 and 67 years old for the waves 1 (2004), 2 (2007), 4 (2011), 5 (2013) and 6 (2015). From those, we exclude 1,048 who were homemakers since they are not affected by the reform. Then, our final sample is formed by 1,904 observations.

As outcome variables we utilize the following health variables: i) a dummy variable indicating a fair or poor self-reported health, ii) EURO-D scale of mental health (from 0 best mental health status, to 12 worst mental health status), iii) CASP-12 quality of life scale(QoL) (from 12 worst QoL, to 48 best QoL), iv) a mobility index, being the sum of 4 basic activities of daily living carried out with difficulties (from 0 best mobility, to 4 worst mobility), v) a recall variable which contains the number of words recalled from a list (from 0 worst memory to 10 best memory), vi) Body Mass Index (BMI), and vii) a dummy indicating whether the individual currently smokes.

[4] Sick leaves lasting less than one week are not available in the data.

We also look at healthcare use within the last 12 months with the following dependent variables i) the number of doctor visits, ii) a dummy indicating if the respondent was hospitalized, iii) the number of hospitalizations and iv) the number of days in the hospital.

4. Methods

Our identification strategy relies on exploiting discontinuities in pension eligibility conditions among cohorts and age groups. In particular, we specify two models depending on the dataset we use.

4.1. Specification 1 –Difference-in-Difference based on birth cohort over age (WHIP-Health data)

In order to analyse the effects of the reform on the labour market and on hospitalizations, we employ a difference-in-difference model comparing birth cohorts 1951 and 1952 over the same ages within the WHIP-Health dataset. Women born in 1952 are our "treatment" group since they had to postpone retirement after 60 because of the reform, whereas women born in 1951 serve as the control group since at 60 they could already claim an old age pension. We run the following regression

$$y_{i,t} = \beta_0 + \sum_{\substack{a=53 \\ a\neq 58,59}}^{63} \theta_a \gamma_a Cohort1952_i + \sum_{\substack{a=53 \\ a\neq 58,59}}^{63} \gamma_a + \delta_t + \alpha_i + \mu_{i,t} \quad (1)$$

where $y_{i,t}$ can be either the probability of being in a certain labour market status (retired, employed, unemployed/inactive or disability pension) or the probability of hospitalization at each of the four work-related diagnosis groups (musculoskeletal, stroke and myocardial infarction, injuries or mental health as defined in section 3.1). $Cohort1952_i$ is a binary variable equal to one if the woman i is born in 1952, and zero for women born in 1951. The model includes individual fixed effects (α_i), age fixed effects (γ_a) and a linear yearly time trend (δ_t).

Standard difference-in-difference models normally interact the treatment variable with a time variable measuring the period after the treatment. We instead use the age of respondents as our "time" variable. In doing so, we compare differences in outcomes between the treatment and control groups normalizing at the same ages, before and after the reform. Then, the set of interaction terms between age fixed effects and the cohort dummy ($\gamma_a Cohort1952_i$) measure the effect of postponing access to pension due to the reform. We set ages 58-59 as baseline[5], which correspond to ages when neither those born in 1951 nor in 1952 could retire. Cohort 1951 could

[5] Note that we need to keep two ages (58 and 59) as the baseline category to avoid multicollinearity between the age fixed effects, the cohort dummy and the linear yearly time trend.

retire at the age of 60, whereas cohort 1952 could not retire at least until 63 due to the reform. If the reform had an effect on the labour or health outcomes, the coefficient (θ_a) of the interaction between Cohort 1952 and ages should come out significant from age 60 ($a \geq 60$).

The identifying assumption is that, absent the reform, the change in the outcome variable would have been similar between the treatment cohort who had to postpone their retirement (1952) and the control cohort (1951), conditional on the linear year trend.

As robustness, we run a similar set of regressions specifying the DID model based on year rather than age, i.e., with year-fixed effects and interaction between the 1952 treated cohort and year dummies. In this alternative specification, we set as the baseline year 2009-2010, which corresponds to years when neither those born in 1951 nor 1952 could retire. Results are qualitatively equivalent to those obtained using the DID based on age.

4.2. Specification 2 – Pooled Difference-in-differences based on age groups over time (SHARE data)

For the SHARE analysis, we cannot use the same empirical strategy as with WHIP because we have fewer observations of cohorts 1951-52. To maximize our sample size, we include instead the sample of women aged 60-67 from waves 1 (2004), waves 2 (2006), waves 4 (2011), 5 (2013) and 6 (2015). We exploit the fact that we observe the same age groups before and after the reform, whereas the reform only affected some of them (ages 60-63), and not others (ages 64-67). In particular, we run the following regression:

$$y_{i,t} = \sigma_0 + \sigma_1 BelowNRA + X_{it}\Omega + \delta_t + \gamma_a + \lambda_c + e_{i,t} \quad (2)$$

Where $BelowNRA$ is a dummy indicating whether the individual is and has always been below the normal retirement age (i.e., old-age pension). This depends on the age at response and year of birth[6]. Before the reform, in waves 1 to 4, all ages from 60 to 67 could access to an old age pension (i.e.: belowNRA=0). In wave 5, belowNRA equals 1 if born 1952 or after (i.e., age 61-62 or younger, depending on interview and birth month), zero otherwise. In wave 5, below NRA equals 1 if born in 1952 or after and age is lower than 63 and 9 months, zero otherwise.

[6] Age determines whether the woman is below NRA at that time, whereas year of birth determines whether the woman had acquired the right to retire before the reform. Women above the new minimum pension age could still retire if they had acquired the right to retire before the reform (grandfathering clause). For instance, a women born in July 1951, and responding at wave 5 in May 2013, would be 61 and therefore under the new normal retirement age in place (62 years and 3 months). However, she could still retire because she acquired the right to access an old age pension in July 2011 when she turned 60, right before the reform.

δ_t are year fixed effects. γ_a are age fixed effects. λ_c is a cohort linear trend that controls for cohort effects. Younger cohorts may have different labour force participation trends, education or health trends. The vector X_{it} includes control variables affecting retirement status such as education level (none, primary, secondary or tertiary), marital status (married or not) and number of children.

This specification resembles a difference-in-difference analysis based on age at each wave. Before the reform, all ages are above the retirement age, whereas after the reform some age groups have remained below the retirement age and therefore cannot retire anymore. The same specification has been previously used to study the effect of national pension reforms on labour market outcomes (Staubli and Zweimüller 2013, Cribb et al., 2016, Soosaar et al., 2021, Morris 2022) and health (Carrino et al., 2020; Shai 2018).

The identifying assumption is that, absent the reform, the change in the outcome variable would have been similar between the age groups who remain below the retirement age and therefore ineligible, and the age groups who have been above the retirement age and therefore are eligible for retirement, conditional on the control variables. Then, σ_1 measures the effect of being below the normal retirement age and therefore not being able to retire due to the reform of 2012.

$y_{i,t}$ may represent any of the health variables explained in section 3.2. We also validate our model by using as dependent variable the following labour market outcomes: i) the probability of retirement, ii) the probability of employment.

5. Results

5.1. Effect on the labour market (based on WHIP-Health)

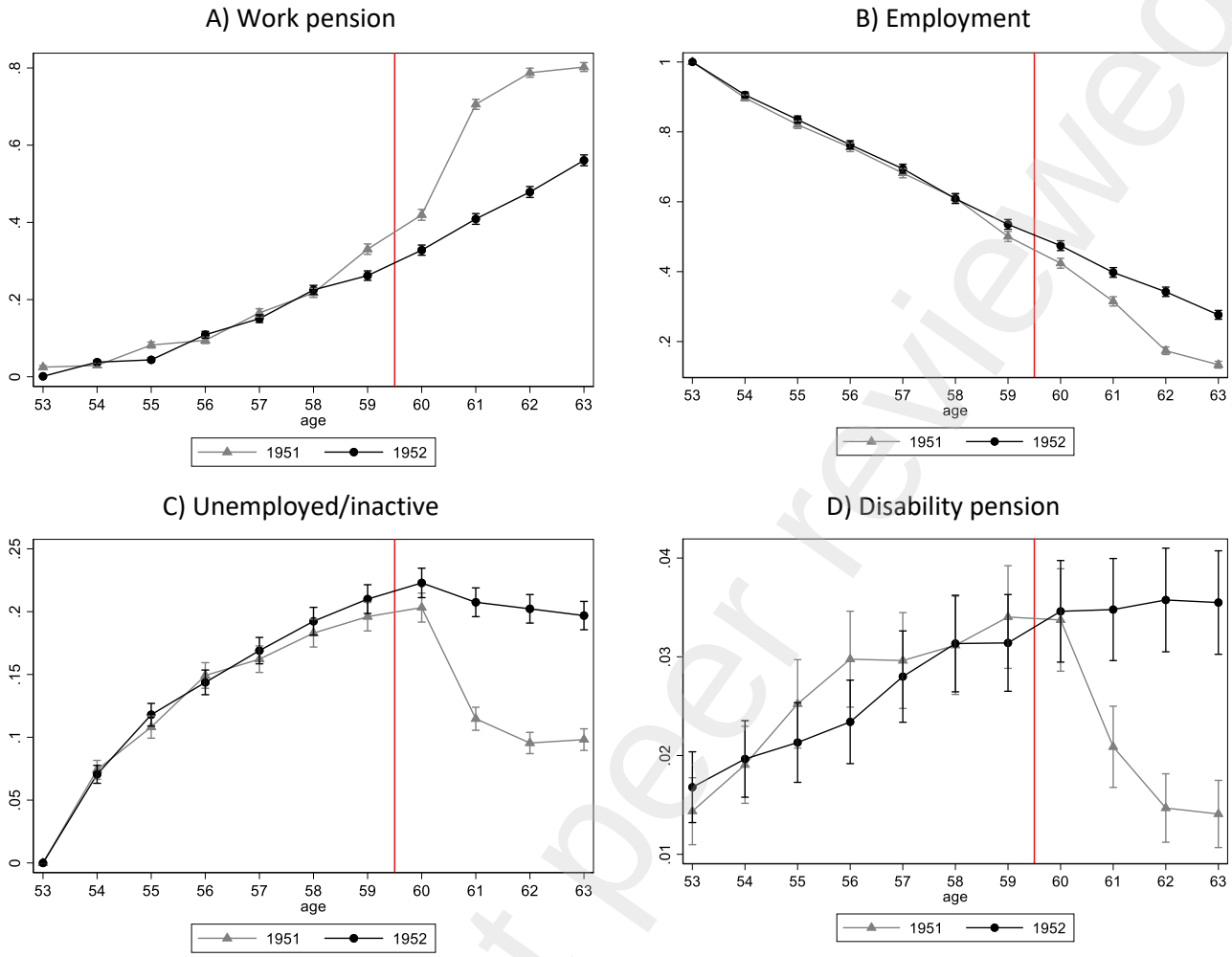
In Figure 1, we show the probability of retirement via old-age or seniority pension (panel A) and of employment (panel B) for the cohorts 1951 and 1952. Both cohorts, as they age, increase their probability of retirement while decreasing the probability of working following a parallel trend. However, after the age of 60 there is big jump in the probability of retirement (and a drop in the probability of working) for the cohort 1951, and not for the cohort 1952, whose retirement age was postponed due to the reform. By the latest age available at 63, women from the cohort 1952 were still around 20 pp less likely to be retired than women in the cohort 1951 when they were 61 years old. Moving to the "unemployed/inactivity" and the "disability benefit" outcomes (panels C and D), it is possible to observe that the proportion of individuals falling in these two categories raises with age in both cohorts and falls just in correspondence with the eligibility age for claiming a pension for the 1951 cohort.

In Figure 2 we plot the coefficients of the DiD model of specification 1 in event-study-like graphs. The results show that the reform indeed decreased the probability of retirement of the 1952 cohort by around 20-25 pp in the ages 61 to 63 (panel A). Conversely, there is an increase in the probability of employment of around 10-12 pp (panel B). We also found a significant increase of similar size in the probability of unemployment or inactivity, raising concerns about the risk of possible social exclusion and poverty risk among the elderly (panel C). On the other hand, we found a substitution effect with disability benefits, with the affected cohort increasing the probability of taking up disability benefits by around 2 pp in the ages 60-63 (panel D). In Figure A1 of Appendix A, we further test whether the reform had an effect on the probability of sick leave, among the subsample of women that continued to work[7]. Results show an increase in the probability of having at least a weekly sick leave by around 5 to 7 pp for women affected by the reform (born in 1952), as compared to women of the comparison cohort (born in 1951). This corresponds to a 17 to 24% increase with respect to the pre-reform mean, defined as the mean observed among the control cohort in the 58-59 ages (i.e.: 0.286, see Table A3 in Appendix A). Importantly, before the age of 60, when still none of the cohorts could claim the old age pension, all the labour market outcomes follow a parallel trend with only small fluctuations around zero.

Overall, the analysis of the employment and retirement trajectories shows that the reform was effective in postponing retirement and in increasing working rates among the ages 60 to 63, although it also increased the probability of unemployment or inactivity in a similar magnitude. Additionally, we report a significant substitution effect towards disability and an increase in sick leaves among those who continue to work. Both factors already suggest a plausible health deterioration among women who were forced to prolong their working life. We move to further test this hypothesis in the following subsections using health data.

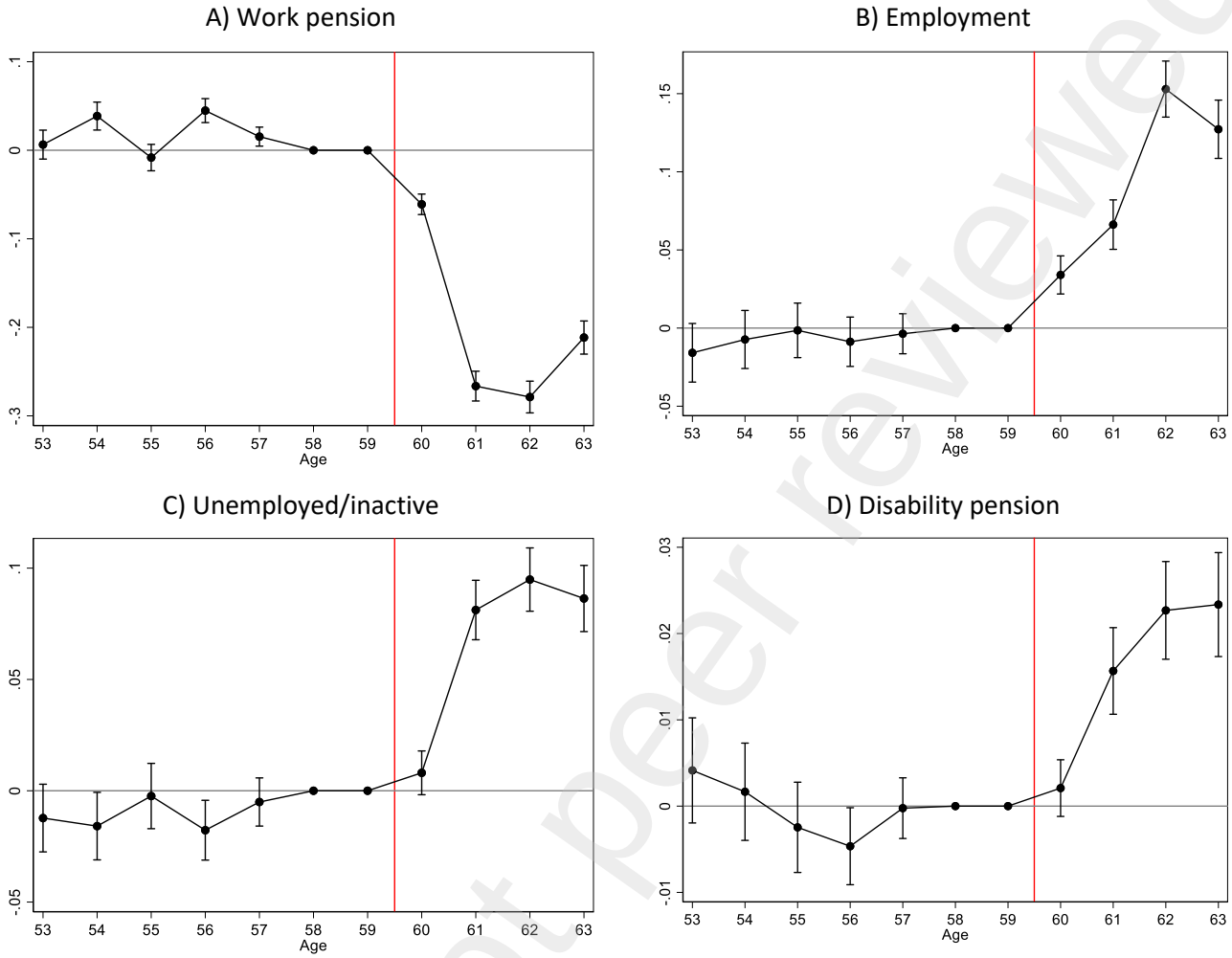
[7] We restricted this analysis to women working in blue-collar occupations (4,705 corresponding to 49% of the final sample of analysis of women employed at 53 years), since information about sick leaves for white-collar occupations is not fully available.

Figure 1 – Descriptive evidence on labour market responses to the pension reform (WHIP-Health data)



Notes: These figure reports the proportion of women in each employment status by cohort of birth and age. Sample of 9,677 women born in 1951-1952 who were employed at 53 followed from age 53 to age 63 (n=105,122)

Figure 2 – Effect of the pension reform on the labour market outcomes (DID estimates on WHIP-Health data)



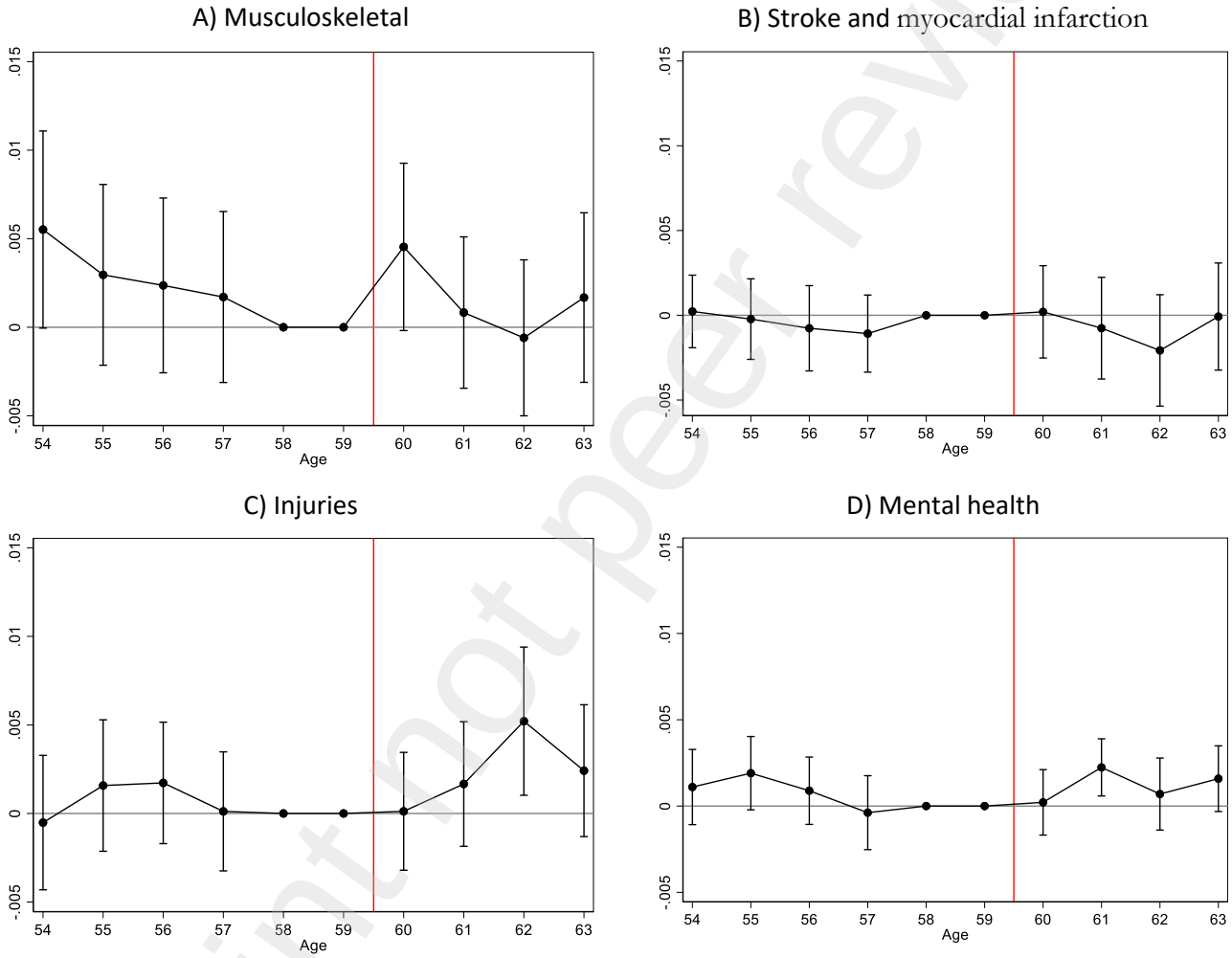
Notes: These figures report the effect of the pension reform on the probability of being in each labour market outcome derived from the DiD model specified in Equation (1). Each figure plots the coefficients and the 95% confidence intervals of the interactions between the Cohort 1952 and the age dummies, leaving ages 58 and 59 as the base category. Note that the y scale is not the same for each graph. Sample of 9,677 women born in 1951-1952 who were employed at 53 followed from age 53 to age 63 (n=105,122).

5.2. Effects on work-related hospitalizations (based on WHIP health)

In Figure 3, we report the DiD results for the probability of work-related hospitalizations based on specification 1. We find a significant increase in the probability of hospitalizations due to mental health diagnosis at the age of 61 and due to injuries at the age of 62, right at the time when the effect of the reform on labour market outcomes was at the highest (panel C and D). More concretely, the probability of mental health hospitalization increased by 0.2 pp, which corresponds to a 69% increase with respect to the pre-reform mean (Table A3 in Appendix A), whereas the probability of hospitalizations due to injuries increased by 0.5 pp, an 88% raise with respect to the mean. However, the significance does not hold after that. On the other hand, musculoskeletal hospitalizations seem to marginally increase by the age of 60 among the treated compared to the

control cohort, although the estimate is quite imprecise. Lastly, we find a null effect for myocardial infarction hospitalizations. As in the case of the labour markets, the pre-reform parallel trend assumption holds also for the health outcomes since none of the coefficients prior to the age of 60 is significantly different from zero.

Figure 3 – Effect of the pension reform on work-related hospitalizations



Notes: These figures report the effect of the pension reform on the probability of hospitalization by diagnosis group, derived from the DiD model specified in Equation (1). Each figure plots the coefficients and the 95% confidence intervals of the interactions between the Cohort 1952 and the age dummies, leaving ages 58 and 59 as base category. Sample of 9,656 women born in 1951-1952 who were employed at 53 followed from age 54 to age 63 (n=95,445)

5.3. Effects on self-reported health outcomes (based on SHARE)

SHARE results relying on specification 2 show that the reform decreased the probability of retirement by 32 points and raised the employment rate by about 31 percentage points (Table 3, Panel A). Results are fairly like what emerged in WHIP, especially regarding the probability of receiving a pension, although we found a larger effect with respect to employment here. These results are reassuring as they clearly show that also in the SHARE sample, despite a smaller sample size, we can detect a strong effect of the reform on actual retirement behaviour. Then, using the same specification, we exploit this effect on retirement behaviour to test for possible second-order effects on health (Table 3, Panel B and C).

SHARE results also show that the reform affected healthcare use (Table 3, Panel C), particularly with respect to secondary care. Age groups affected by the reform increased hospital care use as detected by different measures: an increase in the number of hospitalizations by 0.13 in the last 12 months, in the number of hospital days by 1.2 and in the probability of hospitalizations (although the latter only significant at the 10% level). This is in line with the increase in some work-related hospitalizations found in the WHIP-health dataset, mainly injuries and mental health. Unfortunately, SHARE does not offer the cause of admissions to test whether these hospitalizations are also driving these results.

However, we do not find significant effects in most of the self-reported health outcomes (Table 3, Panel B). Self-reported health, CASP quality of life score, EURO-D scale of mental health, or cognitive decline (as measured by the number of words remembered) do not seem to be affected by the reform. We only detect a reduction in the BMI by 1.4 for those affected by the reform, which corresponds to a 5% with respect to the mean (Table A4 in Appendix A).

It is worth noting that SHARE has a much lower sample size than WHIP-health, therefore there might be small health effects that we are not able to capture due to lack of statistical power. Still, by looking at the 95% Confidence Interval (CI) of our estimates we can rule out an increase in the probability of bad health of 14 pp (36% with respect to the mean), a decrease in CASP quality of life of 1.1 points (3% with respect to the mean), an increase in the depression scale of 0.28 (9% with respect to the mean), an increase in mobility index of 0.12 (23% with respect to the mean) and a cognitive deterioration of -0.09 in the recall index (2% with respect to the mean). This suggests that we have enough power to detect relatively small effects, particularly with respect to quality of life, mental health, and cognitive status, although somehow higher for physical health as measured by the mobility index.

Table 3 - Effect of the pension reform on self-reported health outcomes. SHARE data

Panel A - Labour market outcome		
	(1)	(2)
VARIABLES	retired	employed
belowNRA	-0.328***	0.313***
	(0.052)	(0.049)
95% CI	[-0.430; -0.226]	[0.218; 0.408]
Observations	1,860	1,860
Mean Y	0.390	0.134

Panel B - Health outcomes and health behaviours							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Bad self-reported health	CASP quality of life	Euro-d mental health	Mobility index	Recall (number of words)	Body Mass Index (BMI)	Smoking [a]
belowNRA	0.0326	0.418	-0.290	-0.0588	-0.0981	-1.435***	-0.527*
	(0.057)	(0.784)	(0.294)	(0.093)	(0.180)	(0.491)	(0.313)
95% CI	[-0.079; 0.145]	[-1.120; 1.956]	[-0.867; 0.287]	[-0.241; 0.124]	[-0.450; 0.254]	[-2.399; -0.471]	[-1.141; 0.087]
Mean Y	0.390	34.65	2.931	0.521	5.029	26.06	4.370
Observations	1,863	1,726	1,849	1,864	1,839	1,847	1,374

Panel C - Healthcare use				
	(1)	(2)	(3)	(4)
VARIABLES	Doctor visits	Any hospitalization	Number of hospitalizations (NB)	Hospital days (NB)
belowNRA	-0.222	0.0513*	0.135**	1.222**
	(1.210)	(0.031)	(0.057)	(0.547)
95% CI	[-2.596; 2.151]	[-0.009; 0.112]	[0.022; 0.247]	[0.149; 2.294]
Mean Y	8.637	0.0909	0.149	0.966
Observations	1,853	1,862	1,861	1,860

Notes: belowNRA reports the effect of being below the Normal Retirement Age on each corresponding outcome derived from the specification 2. Each column and panel reports results from a different regression. Robust standard errors clustered at individual level in parenthesis *** p<0.01, ** p<0.05, * p<0.1. All regressions are estimated by linear models except those from the columns (3) and (4) of panel C that are estimated through a negative binomial (NB) and with results reported in marginal effects. [a] Smoking was not included in wave 6 of SHARE and so we have a lower number of observations for this outcome.

5.3.1. Identifying assumptions checks for specification 2 (SHARE data)

In Appendix B we discuss in detail the identifying assumptions needed for our identification strategy with SHARE to be valid. First, the age groups affected and non-affected by the reform should follow similar trends before the reform as seems plausible from Figures B1-B4 (section B1 in Appendix B). Second, in section B2 of Appendix B, we demonstrate that treated and control cohorts did not have significant differences in the relevant health outcomes nor in the labour outcomes before the reform, when they were the same age (Table B1). Additionally, we show that they followed the same retirement trajectories up to the age of 60, just prior to the reform (Figure B5).

5.4. Mechanisms

In this subsection, we perform additional analyses to shed light on the potential mechanisms that could explain our results, including heterogeneity analyses by socioeconomic status (SES) and pre-reform health status, as well as examining the effect of the reform on income.

5.4.1 Heterogeneity analysis by socioeconomic status (SES): occupation, wage and education

In Table A5 of Appendix A, we explore whether the effect of the reform was different by SES as measured by occupation (blue-collar vs white-collar) and by wage (below vs above median) in the WHIP-Health database. Those with lower SES experienced the highest drop in working pensions due to the reform. However, they were not more likely to work than those with higher SES, but rather more likely to be unemployed or take disability benefits. This finding points to lower employability of the lower SES women affected by the reform. The low generosity of disability benefits and the lack of income support in the case of unemployment without benefits raises concerns about the risk of old-age poverty in this group of women. On hospitalizations, however, we find no heterogeneous effects by occupation or by wage. Results from SHARE are also inconclusive and show no clear pattern of the effect of the reform by SES, neither as measured by occupation nor by education (Table A6 in Appendix A). Overall, these results suggest that there was no differential effect of the pension reform on health by SES, although they raise questions about the employability and financial well-being of lower SES women affected by the reform.

5.4.2. Heterogeneity analysis by health condition.

In Table A7 of Appendix A, we look at the effect of the reform by pre-reform health status of the workers, as proxied by the number of hospitalizations during the ages 49 to 57. Results show that the poorer the health, the larger the reaction to the tightening of minimum pension age. For instance, those with 2 or more pre-reform hospitalizations reduced their probability of pension by

24 pp and increased the probability of work by 13 pp, whereas these figures were 21 and 9 pp respectively for those without any hospitalization. It also emerges a clear gradient for the program substitution towards disability pension. The increase in the probability of disability pension was 1.1 pp for those with no pre-reform hospitalization, 2.9 pp for those with at least one hospitalization and 4.3 pp for those with at least two hospitalizations. This health gradient is also clear for mental health hospitalizations. The reform increased the probability of these hospitalizations by 0.08 pp for the group with the poorest health, whereas it had no effect on the group with better health. Similarly, the coefficient for injuries increases as the pre-reform health status deteriorates. Furthermore, the effect of the reform on sick leaves also seems to be driven by those with poorer health as the probability of sick leave increases by 7 pp for those with at least one hospitalization, whereas it had no effect for those with no hospitalizations. Results from SHARE (Table A8 in Appendix A) also point out this differential effect by health status, with the effect on hospitalizations only found among those with poor self-reported health status. Overall, these results show a clear pattern: the reform had the most pernicious health effects among those women who already had worse health conditions before the reform.

5.4.3. Effect of the reform on income

The pension reform may affect health and healthcare use not only through exposure to work-related hazards but also through an effect on disposable income. Pensions may lower purchasing power and therefore reduce private healthcare demand or might more generally affect health through a lower standard of living. SHARE results however show no effect of the reform on household income nor on making ends meet (Table A9 in Appendix A). Two factors may explain this. First, for the cohorts under analysis, the Italian pension system offers a relatively high replacement rate with respect to previous salaries (82% for women as compared to an OECD average of 61% (OECD 2022)). Second, since women's earnings represent a smaller share of the total household income as compared to that of their men partners (Segato 2021), the drop in women's income induced by retirement alter the total household income only marginally. These results suggest that income is not confounding our results.

6. Discussion

The 2012 Italian pension reform increased the normal retirement age for women from 60 to 62 in 2012 and progressively to 67 in 2019. As a consequence, it suddenly postponed access to an old age pension for the affected cohorts of women from three to seven years. In this article, we studied

the labour market and health effects of this abrupt and not anticipated increase in normal retirement age using administrative data on social security and hospitalizations records and SHARE survey data allowing us to investigate a wide array of health dimensions (subjective, physical, mental, cognitive, behaviours).

Our results first show that the reform effectively decreased the probability of retirement by around 20 to 25 pp during the ages 61 to 63 for the affected cohort. Conversely, their probability of working increased by around 10 to 12 pp, whereas they also increased the probability of unemployment or inactivity by a similar magnitude. Additionally, we report a significant substitution effect towards disability and an increase in sick leaves among those who continue to work by around 17-24% compared to the pre-reform mean. Overall, these results show that the reform effectively increased the probability of working at older ages. Although it also pushed some of the workers out of employment and towards disability insurance, while increasing sick leaves of those who remained employed, casting doubts about the employability of some of the affected women.

Results from registered data on hospital use show some increases in the probability of work-related hospitalizations (injuries and due to mental health) around the ages 61-62 for those who were forced to prolong their working life, although only temporal and not lasting in the longer term. We do not detect any effect on self-reported health status or any other self-reported health outcomes of physical health (mobility index), mental health (EURO-D scale), quality of life (CASP) or cognitive decline. On the other hand, there is a decrease in BMI for those affected by the reform, consistently with what was found by Bertoni et al. (2018), assessing among the other outcomes also BMI and obesity. This can be a consequence of the extension of the working life, since working may increase physical activity, or even just commuting from home to work may force individuals to walk daily.

We find heterogeneous effects of the reform on labour market outcomes across socio-economic position. Blue-collar and low-paid female workers were the most constrained by the reform experiencing the largest delay in pension take-up and at the same time the highest increase in the probability of leaving the labour market through disability or non-employment. These results are consistent with previous studies showing that the labour market effects of pension reforms are stronger among disadvantaged workers (Mastrobuoni, 2009; Hanel and Riphahn, 2012; Ardito 2021). However, this does not translate into heterogeneous effects on health outcomes. Contrary to previous literature assessing the health effect of raising pension age pointing to striking heterogeneity across manual and non-manual workers (on women Carrino et al 2020; on men

Ardito et al. 2020, Shai 2018), we do not find differences between blue-collar and white-collar. One possible reason for this inconsistency might be that in a country like Italy, where female participation is still extremely low compared to European average, women in the labour market are relatively more selected and homogenous with respect to health than men (d'Errico et al. 2022) and differential health vulnerabilities along occupational gradients (i.e., wage, occupational grade) are diluted.

We do find, however, a clear pattern of the heterogeneous effects of the reform by the prior health status of the women, which is persistent across all the different health outcomes analysed. Our results show that those in worse health conditions, as measured by the prevalence of hospitalizations prior to the reform, are driving the observed increase in injuries and mental health hospitalizations. Moreover, the same pattern is also observed by looking at the effect of the reform on self-reported health, on pension disability uptake and on sick leaves, where the effects are concentrated among those who had poorer health prior to the reform. This is a heterogeneity analysis that has been overlooked in previous literature, which is rather focused on the effect by occupation or other measures of socioeconomic status.

7. Conclusions and policy implications

Summing up, our results show that the pension reform was successful in extending the working horizon for the affected women, who on average postponed retirement and increased employment at older age. However, there were side effects as tightening the minimum pension age pushed a fraction of previously employed women out of the labour market, into unemployment, inactivity, and disability pensions, particularly those with previously worse health and those employed as blue-collar and with lower pay. Women affected by the reform also experienced some short-term negative health effects, mainly hospitalizations due to mental health or injuries, concentrated among those with previously worse health. Despite the abrupt increase in retirement age, we did not find any negative effects on the self-reported outcomes under study assessing physical, mental and behavioural health, expect from an increase in self-reported number of hospitalizations and hospitalized days, consistently with results obtained on our administrative data. It is important to note that our results are driven by increasing retirement ages from a relatively younger age of 60 and particularly from increasing employment rates around 61 to 63. Hence, our general results should not be extrapolated to anticipate the health effects of other more recent pension reforms that predict to increase retirement ages over 70 (OECD Pension at a glance 2021). The clear signs of health deterioration among those with previously worse health conditions may give

policymakers a better hint of what could happen if the pension age continues to rise towards older ages when workers are expected to have worse physical and cognitive health conditions.

These results contribute to the debate about pension reforms and whether there should be differentiated eligibility conditions depending on workers' characteristics, helping to exactly pinpoint who are the workers less able to extend their working life and more likely to suffer from a pension age rise. Whereas for women their own occupation and education do not seem to be a determinant factor in the health effects of the reform, their previous health status clearly is. This suggests that equal rises in pension ages might be harmful for the health and the working capacity of the previously worse-off. Then, if policymakers are keen to apply differentiated eligibility conditions to prevent unintended consequences of rising pension age, they may want to focus not only on occupation but also on workers' health conditions to avoid that pension reform hit disproportionately more on the most vulnerable.

Credit authorship contribution statement

Manuel Serrano-Alarcón: Conceptualization, Data curation, Formal analysis: Methodology, Roles/Writing - original draft. **Chiara Ardito**: Conceptualization, Data curation, Formal analysis: Methodology, Roles/Writing - original draft. **Roberto Leombruni**: Conceptualization, Data curation, Writing - review & editing. **Alexander Kentikelenis**: Conceptualization, Writing - review & editing. **Angelo d'Errico**: Conceptualization, Writing - review & editing. **Giuseppe Costa**: Writing - review & editing. **Anna Odone**: Writing - review & editing. **David Stuckler**: Conceptualization, Writing - review & editing

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Appendix A. Additional results

Tables

Table A1 - Descriptive statistics on labour market outcomes' exclusive and simultaneous occurrence in WHIP-Health data

	N	%
Exclusively in one labour market state		
Employed	57,915	55.09%
Receiving a work pension	25,671	24.42%
Receiving a disability pension	1,383	1.32%
Unemployed or inactive	14,894	14.17%
Simultaneously in more than one labour market state		
Employed and receiving a work pension	3,873	3.68%
Employed and receiving a disability pension	1,305	1.24%
Receiving both work and disability pensions	71	0.07%
Employed and receiving both work and disability pensions	10	0.01%
Total	105,122	1.00

Notes: WHIP final sample of analysis, cohorts 1951-52, aged 53-63.

Table A2 - ICD-9 codes used to identify hospitalizations for musculoskeletal disorders associated with work-exposures in previous literature

Main diagnosis ICD-9 codes	
Musculoskeletal disorders	3540-3543, 7150, 7151, 7158, 7159, 7210-7216, 7219, 7220-7227, 7229, 7230-7234, 7240-7245, 7260-7269, 7270-7273, 7286
Herniated cervical intervertebral disc without myelopathy	7220
Herniated thoracic or lumbar intervertebral disc without myelopathy	7221
Intraspongiose Schmorl's hernias	7223
Discopathy with myelopathy	7227
Carpal tunnel syndrome	3540
Procedures ICD-9 Codes	
Intervertebral disc removal or demolition	805
Intervertebral disc removal or demolition, unspecified	8050
Intervertebral disc removal	8051
Intervertebral chemonucleolysis	8052
Other intervertebral disc destruction	8059
Carpal tunnel release	0443
Total hip replacement	8151
Partial hip replacement	8152
Total knee replacement	8154

Notes: When a hospitalization displays one of the above procedure codes but it has a main diagnosis "Injury" (ICD-9: 800-959), the hospitalization is recorded as "Injury".

Table A3- Summary statistics of WHIP-Health data before the reform (at ages 58-59)

	Born 1951		Born 1952	
	Mean	SE	Mean	SE
Labour market outcome probability				
Work Pension	0.274	0.20	0.244	0.18
Employed	0.556	0.25	0.572	0.25
Disability Pension	0.033	0.03	0.031	0.03
Non employment	0.189	0.15	0.201	0.16
Number of women	4735		4952	
Sick leave (among those employed in blue-collar)	0.286 (N=2338)	0.20	0.290 (N=2367)	0.21
Probability of hospitalization				
Musculoskeletal (ICD-9: see Table A2)	0.011	0.01	0.008	0.01
Injuries (ICD-9: 800-959)	0.006	0.01	0.005	0.01
Mental health (ICD-9: 290-319)	0.003	0.00	0.001	0.00
Myocardial infarction and Stroke (ICD-9: 410-414, 430-438)	0.003	0.00	0.003	0.00
Number of women	4735		4942	

Notes: This table reports the mean and standard errors (SE) per cohort at ages 58-59 of our sample of interest. That is, women born in 1951-52 who were employed at 53 years old.

Table A4- Summary statistics of SHARE data

	(1) All sample		Before reform (Waves 1-4) (2) Age 60-63		(3) Age 64 – 67		Difference in means[a]
	Mean	SE	Mean	SE	Mean	SE	(2) – (3)
Labour market outcomes							
retired	0.802	0.399	0.831	0.375	0.934	0.248	-0.103***
employed	0.134	0.341	0.106	0.309	0.039	0.193	0.068***
Health outcomes							
Bad SRH	0.379	0.485	0.349	0.477	0.441	0.497	-0.092**
CASP QoL	34.652	6.305	34.732	6.238	34.243	5.854	0.489
eurod	2.931	2.472	2.897	2.438	3.007	2.424	-0.11
Mobility index	0.521	0.894	0.504	0.868	0.604	0.883	-0.1
Recall of words	5.029	1.63	5.032	1.542	4.661	1.658	0.371***
Body mass index	26.057	4.354	26.331	4.398	26.313	4.156	0.017
Doctor visits	8.637	12.358	8.093	11.53	9.269	11.236	-1.176
Any hospitalization	0.091	0.287	0.096	0.295	0.098	0.297	-0.001
Hospitalization	0.149	0.605	0.133	0.46	0.139	0.515	-0.006
Hospital days	0.966	5.351	1.189	6.313	0.852	4.514	0.337
Controls							
Age	63.59	2.274	61.52	1.131	65.516	1.141	-3.996***
Education: none	0.02	0.141	0.002	0.045	0.014	0.116	-0.012*
Education: primary	0.422	0.494	0.47	0.5	0.571	0.495	-0.102**
Education: secondary	0.41	0.492	0.405	0.491	0.315	0.465	0.090**
Education: tertiary	0.148	0.355	0.123	0.329	0.1	0.3	0.024
married	0.775	0.418	0.814	0.389	0.744	0.437	0.070*
children	1.813	1.147	1.81	1.057	1.943	1.241	-0.134
Observations	1904		502		442		944

NOTES: [a] p-values of the independent sample t-test for the difference in means: *** p<0.01, ** p<0.05, * p<0.1.

Table A5 – Effect of the pension reform by socioeconomic group with WHIP-Health data. Simple difference in difference model.

	(1) employed	(2) work pension	(3) disability pension	(4) Non-employed	(5) sick-leave	(6) Musculoskeletal	(7) Mental Health	(8) Stroke and MI	(9) Injuries
					Panel A – blue-collar				
post60	-0.042***	0.207***	-0.017***	-0.100***		0.002	-0.001	0.002**	-0.002
	(0.007)	(0.007)	(0.003)	(0.006)		(0.002)	(0.001)	(0.001)	(0.001)
post60x1952	**0.102*** **	**-0.247*** **	**0.023*** **	**0.085*** **		**-0.001**	**0**	**0**	**0.002**
	(0.009)	(0.009)	(0.004)	(0.008)		(0.002)	(0.001)	(0.001)	(0.001)
N	50,212	50,212	50,212	50,212		50,212	50,212	50,212	50,212
					Panel B – white-collar				
post60	-0.090***	0.216***	-0.011***	-0.090***		0.001	-0.001	0	0.002
	(0.008)	(0.009)	(0.002)	(0.006)		(0.002)	(0.001)	(0.001)	(0.002)
post60x1952	**0.098*** **	**-0.193*** **	**0.010*** **	**0.064*** **		**0**	**0.001**	**-0.001**	**0.002**
	(0.010)	(0.010)	(0.003)	(0.007)		(0.002)	(0.001)	(0.001)	(0.002)
N	45,233	45,233	45,233	45,233		45,233	45,233	45,233	45,233
					Panel C – low-wage (below median)				
post60	-0.056***	0.232***	-0.018***	-0.104***	-0.016	0.002	-0.001	0.003**	-0.003*
	(0.008)	(0.007)	(0.003)	(0.007)	(0.013)	(0.002)	(0.001)	(0.001)	(0.001)
post60x1952	**0.101*** **	**-0.264*** **	**0.020*** **	**0.102*** **	**0.017**	**-0.002**	**0**	**-0.001**	**0.002**
	(0.010)	(0.010)	(0.004)	(0.009)	(0.014)	(0.002)	(0.001)	(0.001)	(0.001)
N	48,026	48,026	48,026	48,026	21,849	48,026	48,026	48,026	48,026
					Panel D – high-wage (above median)				
post60	-0.073***	0.191***	-0.011***	-0.086***	-0.027	0.001	-0.001	0	0.002
	(0.008)	(0.008)	(0.002)	(0.005)	(0.028)	(0.002)	(0.001)	(0.001)	(0.001)
post60x1952	**0.098*** **	**-0.179*** **	**0.014*** **	**0.049*** **	**0.051**	**0.001**	**0.001**	**0**	**0.001**
	(0.010)	(0.010)	(0.003)	(0.006)	(0.033)	(0.002)	(0.001)	(0.001)	(0.002)
N	47,419	47,419	47,419	47,419	5,707	47,419	47,419	47,419	47,419

NOTES: This table reports the results of a simple Difference in Difference model: $y_{i,t} = \beta_0 + \beta_1 1952_i x\, post60 + \beta_2 post60 + \delta_t + \alpha_i + \mu_{i,t}$, where 1952 equals 1 if born in 1952, post60 equals one if age 60 or older, δ_t is a linear yearly time trend and α_i are individual fixed effects. Each column and panel reports results from a different regression. Panels A and B stratify the sample based on the most prevalent type of job (blue-collar or white-collar) during the ages 49 and 57. Panels C and D stratify the sample based on the individual average weekly wage adjusted for inflation and part time during the ages 49 and 57 and compared to the median salary (below median or above median). Columns (1) to (4) report the effect of the reform on the probability of being in each corresponding labour market outcome. Column (5) reports the effect of the reform on the probability of having a weekly or longer sick leave among the subsample of women working in a blue-collar occupation. Columns (6) to (9) report the effect of the reform on the probability of hospitalization by each diagnosis group. Robust standard errors clustered at individual level in parenthesis *** p<0.01, ** p<0.05, * p<0.1

Table A6 – The effect of the pension reform by socioeconomic group with SHARE data.

VARIABLES	(1) retired	(2) employed	(3) Bad self-reported health	(4) CASP quality of life	(5) Euro-d mental health	(6) Mobility index	(7) Recall (number of words)	(8) Body Mass Index (BMI)	(9) smoking [a]	(10) doctor visits	(11) Any hospitalization	(12) nbreg hospitalizations	(13) nbreg hospital days
Panel A – blue-collar													
belowNRA	-0.392***	0.302***	0.0767	-0.901	0.0218	0.0545	0.0195	-1.650	-0.266	-0.391	0.0462	0.0853	0.128
	(0.104)	(0.102)	(0.123)	(1.582)	(0.624)	(0.193)	(0.348)	(1.052)	(0.588)	(2.202)	(0.045)	(0.077)	(0.838)
n	733	733	734	666	730	734	729	728	589	730	732	731	731
Panel B – white-collar													
belowNRA	-0.352***	0.307***	0.0251	0.582	-0.211	-0.0106	-0.206	-1.535**	-0.588	0.820	0.0321	0.120*	1.003*
	(0.064)	(0.064)	(0.068)	(0.976)	(0.331)	(0.093)	(0.216)	(0.604)	(0.385)	(1.501)	(0.041)	(0.066)	(0.573)
n	966	966	967	908	961	967	953	958	678	961	967	967	966
Panel C – low education (lower secondary or lower)													
belowNRA	-0.257***	0.230***	0.0786	-0.0601	0.0731	-0.109	0.0223	-1.192	-0.690	0.0964	0.0682	0.139*	2.115*
	(0.076)	(0.063)	(0.081)	(0.458)	(1.167)	(0.148)	(0.267)	(0.744)	(0.457)	(1.617)	(0.042)	(0.083)	(1.200)
n	1,196	1,196	1,197	1,185	1,088	1,197	1,181	1,188	919	1,190	1,195	1,194	1,193
Panel D – high education (upper secondary or lower)													
belowNRA	-0.385***	0.384***	-0.0147	-0.469	0.939	0.0144	-0.228	-1.702***	-0.360	-0.493	0.0324	0.109	0.483
	(0.074)	(0.077)	(0.084)	(0.367)	(1.098)	(0.096)	(0.239)	(0.636)	(0.425)	(1.917)	(0.046)	(0.070)	(0.491)
n	664	664	666	664	638	667	658	659	455	663	667	667	667

NOTES: belowNRA reports the effect of being below the Normal Retirement Age on each corresponding outcome derived from the specification 2. Robust standard errors clustered at individual level in parenthesis *** p<0.01, ** p<0.05, * p<0.1. All regressions are estimated by linear models except those from the columns (12) and (13) that are estimated through a negative binomial and with results reported in marginal effects. The blue/white collar classification is based on the International Standard Classification of Occupations (ISCO-88) of the current job for those working or the last job for those retired or unemployed. We follow Eurofound classification to determine whether and ISCO-88 occupation is blue or white collar. Those who were not employed were asked their last occupation (Source: https://www.eurofound.europa.eu/surveys/ewcs/2005/classification). Education categories are based on the International Standard Classification of Education (ISCED-97) classification. Low education corresponds to ISCED-97 = [0, 2]. High education corresponds to ISCED-97 = [3,6].. [a] Smoking was not included in wave 6 of SHARE and so we have a lower number of observations for this outcome.

Table A 7- Effect of the pension reform by health status with WHIP-Health data. Simple difference in difference model.

	(1) employed	(2) work pension	(3) disability pension	(4) Non-employed	(5) sick-leave	(6) Musculoskeletal	(7) Mental Health	(8) Stroke and MI	(9) Injuries
					Panel A – Good Health (0 hospitalizations)				
post60	-0.063***	0.209***	-0.006***	-0.098***	-0.017	-0.001	0	0.001	-0.001
	[0.007]	[0.007]	[0.001]	[0.005]	[0.013]	[0.001]	[0.000]	[0.001]	[0.001]
post60x1952	**0.095*** **	**-0.219*** **	**0.011*** **	**0.079*** **	**0.012**	**-0.001**	**0**	**0.000**	**0.002***
	[0.008]	[0.008]	[0.002]	[0.006]	[0.015]	[0.001]	[0.000]	[0.001]	[0.001]
N	64,913	64,913	64,913	64,913	18,839	64,913	64,913	64,913	64,913
					Panel B – Poor Health (≥1 hospitalizations)				
post60	-0.068***	0.213***	-0.032***	-0.087***	-0.036	0.006*	-0.004*	0.002	0
	[0.010]	[0.010]	[0.004]	[0.008]	[0.022]	[0.004]	[0.002]	[0.002]	[0.002]
post60x1952	**0.107*** **	**-0.219*** **	**0.029*** **	**0.065*** **	**0.071*** **	**0.004**	**0.003***	**0.000**	**0.004**
	[0.012]	[0.013]	[0.006]	[0.009]	[0.025]	[0.003]	[0.002]	[0.002]	[0.002]
N	30,532	30,532	30,532	30,532	8,717	30,532	30,532	30,532	30,532
					Panel C – Very Poor Health (≥2 hospitalizations)				
post60	-0.075***	0.249***	-0.064***	-0.101***	-0.06	0.019***	-0.007*	0.004	0
	[0.016]	[0.016]	[0.009]	[0.013]	[0.038]	[0.006]	[0.004]	[0.004]	[0.004]
post60x1952	**0.134*** **	**-0.246*** **	**0.043*** **	**0.064*** **	**0.056**	**-0.001**	**0.008** **	**0.002**	**0.008***
	[0.019]	[0.020]	[0.011]	[0.014]	[0.041]	[0.006]	[0.003]	[0.004]	[0.004]
N	12,489	12,489	12,489	12,489	3,466	12,489	12,489	12,489	12,489

NOTES: This table reports the results of a simple Difference in Difference model: $y_{i,t} = \beta_0 + \beta_1 1952_i x\, post60 + \beta_2 post60 + \delta_t + \alpha_i + \mu_{i,t}$, where 1952 equals 1 if born in 1952, post60 equals one if age 60 or older, δ_t is a linear yearly time trend and α_i are individual fixed effects. Each column and panel reports results from a different regression. Panel A include women who had no hospitalizations during the ages 49 to 57. Panel B includes women who had one or more hospitalizations during the ages 49 to 57. Panel C includes women who had two or more hospitalizations during the ages 49 to 57. Columns (1) to (4) report the effect of the reform on the probability of being in each corresponding labour market outcome. Column (5) reports the effect of the reform on the probability of having a weekly or longer sick leave among the subsample of women working in a blue-collar occupation. Columns (6) to (9) report the effect of the reform on the probability of hospitalization by each diagnosis group. Robust standard errors clustered at individual level in parenthesis *** p<0.01, ** p<0.05, * p<0.1

Table A8 – The effect of the pension reform by health status with SHARE data.

VARIABLES	(1) retired	(2) employed	(3) Bad self-reported health	(4) CASP quality of life	(5) Euro-d mental health	(6) Mobility index	(7) Recall (number of words)	(8) Body Mass Index (BMI)	(9) smoking [a]	(10) doctor visits	(11) Any hospitalization	(12) Hospitalizations (NB)	(13) hospital days (NB)
Panel A – Good, very good or excellent self-reported health													
belowNRA	-0.373***	0.384***	0	1.088	-0.528*	-0.0149	-0.0378	-1.086*	-0.300	1.513	-0.0228	-0.0242	-0.393
	(0.063)	(0.061)	(0.000)	(0.829)	(0.270)	(0.060)	(0.189)	(0.584)	(0.321)	(1.246)	(0.022)	(0.045)	(0.346)
n	1,152	1,152	1,155	1,081	1,150	1,155	1,144	1,145	847	1,151	1,155	1,155	1,155
Panel B – Fair or poor self-reported health													
belowNRA	-0.259***	0.187***	0	-0.456	-0.0571	-0.178	-0.0950	-2.272***	-0.976	-3.346	0.170**	0.405***	4.699***
	(0.088)	(0.070)	(0.000)	(1.035)	(0.502)	(0.201)	(0.402)	(0.825)	(0.605)	(2.329)	(0.074)	(0.138)	(1.799)
n	707	707	708	644	698	708	694	701	527	700	706	705	704

NOTES: belowNRA reports the effect of being below the Normal Retirement Age on each corresponding outcome derived from the specification 2. Robust standard errors clustered at individual level in parenthesis *** p<0.01, ** p<0.05, * p<0.1. All regressions are estimated by linear models except those from the columns (12) and (13) that are estimated through a negative binomial (NB) and with results reported in marginal effects. Panel A and B stratify the sample by self-reported health status (SRH). [a] Smoking was not included in wave 6 of SHARE and so we have a lower number of observations for this outcome.

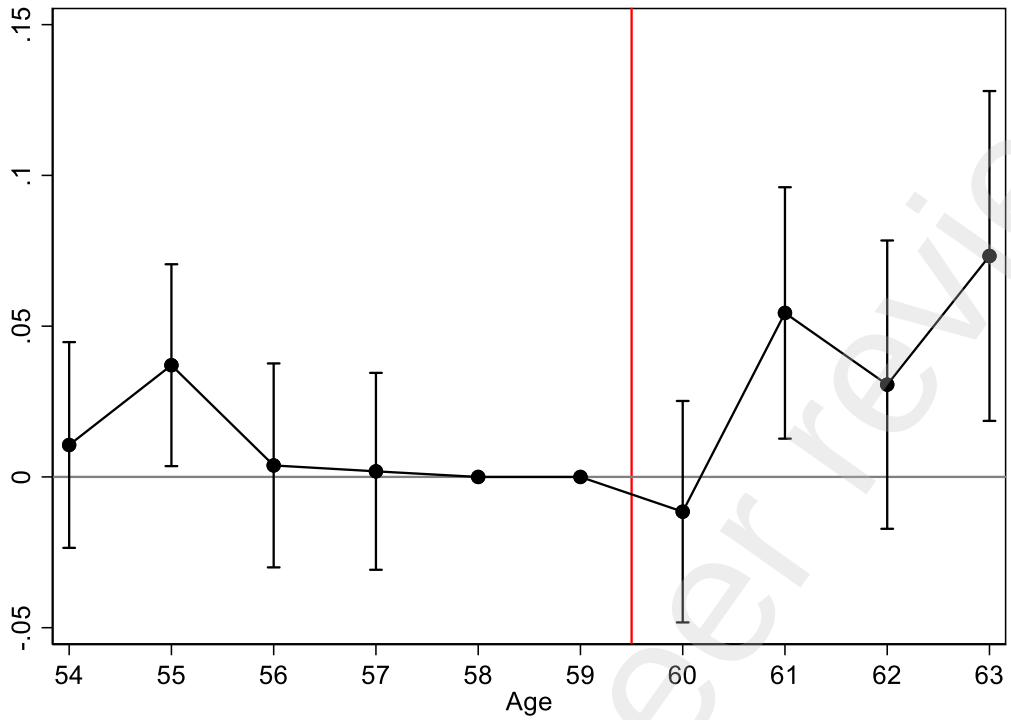
Table A9- SHARE results on household income and on making ends meet

VARIABLES	(1) Household income	(2) Difficulty making ends meet
belowNRA	1,285.0	0.016
	(1,958.1)	(0.055)
Wave 2	4,798.4	-0.105
	(3,555.8)	(0.084)
Wave 4	16,927.7**	-0.384*
	(8,096.4)	(0.196)
Wave 5	10,896.8	-0.341
	(10,406.0)	(0.253)
Wave 6	11,892.5	-0.464
	(12,473.5)	(0.307)
Linear cohort trend	-1,685.6	0.039
	(1,183.6)	(0.028)
Primary education	-5,985.6*	-0.052
	(3,449.2)	(0.075)
Secondary education	1,371.9	-0.255***
	(3,469.0)	(0.074)
Tertiary education	11,994.5***	-0.323***
	(3,930.8)	(0.081)
Married	17,285.9***	-0.193***
	(1,247.4)	(0.034)
Number of children	-710.1	0.046***
	(521.1)	(0.013)
Age FE	Yes	Yes
Observations	1,865	1,839

NOTES: Robust standard errors in parentheses clustered at individual level. *** p<0.01, ** p<0.05, * p<0.1. This table reports the results from running specification 2. Column (1) reports the effect of being below the normal retirement age (belowNRA) on household net income. Column (2) reports the effect of being below the minimum retirement age (belowNRA) on the probability of reporting having difficulties making ends meet. The dependent variable is a dummy variable derived from the following question: "Thinking of your household's total monthly income, would you say that your household is able to make ends meet..." Those who responded "with great difficulty" or "with some difficulty" have the dependent variable equal one. Those who responded "fairly easily" or "easily" have the dependent variable equal zero.

Figures

Figure A1- Effect of the pension reform on sick-leave (among those employed in blue-collar jobs) in WHIP-Health data



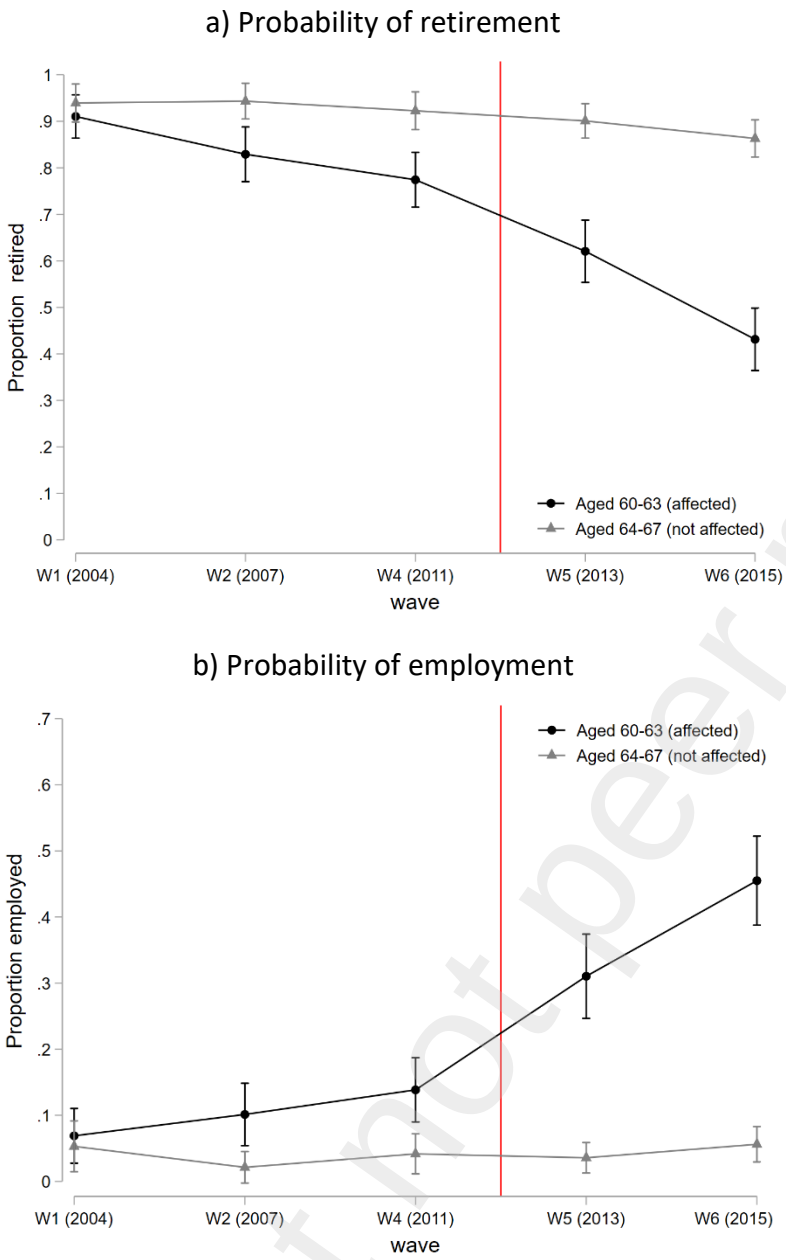
NOTES: This figure report the effect of the pension reform on the probability of having a sick leave of more than a week long duration, derived from the DiD model specified in specification (1). Each figure plots the coefficients and the 95% confidence intervals of the interactions between the Cohort 1952 (treated cohort) and the age dummies, leaving ages 58 and 59 as base category. We use the subsample of women who were working in a blue-collar job, since we lacked reliable information on sick-leaves among those working at white-collar job. Note that now the sample of women is not constant over age. At each age, we use the subsample of women who were employed in a blue-collar job, out of our initial sample of women born in 1951-52 who were employed at age 53 (n = 27,556, women = 4,705)

Appendix B. Checks for identification assumptions of specification 2 (SHARE data)

B.1. Parallel trends based on age-group

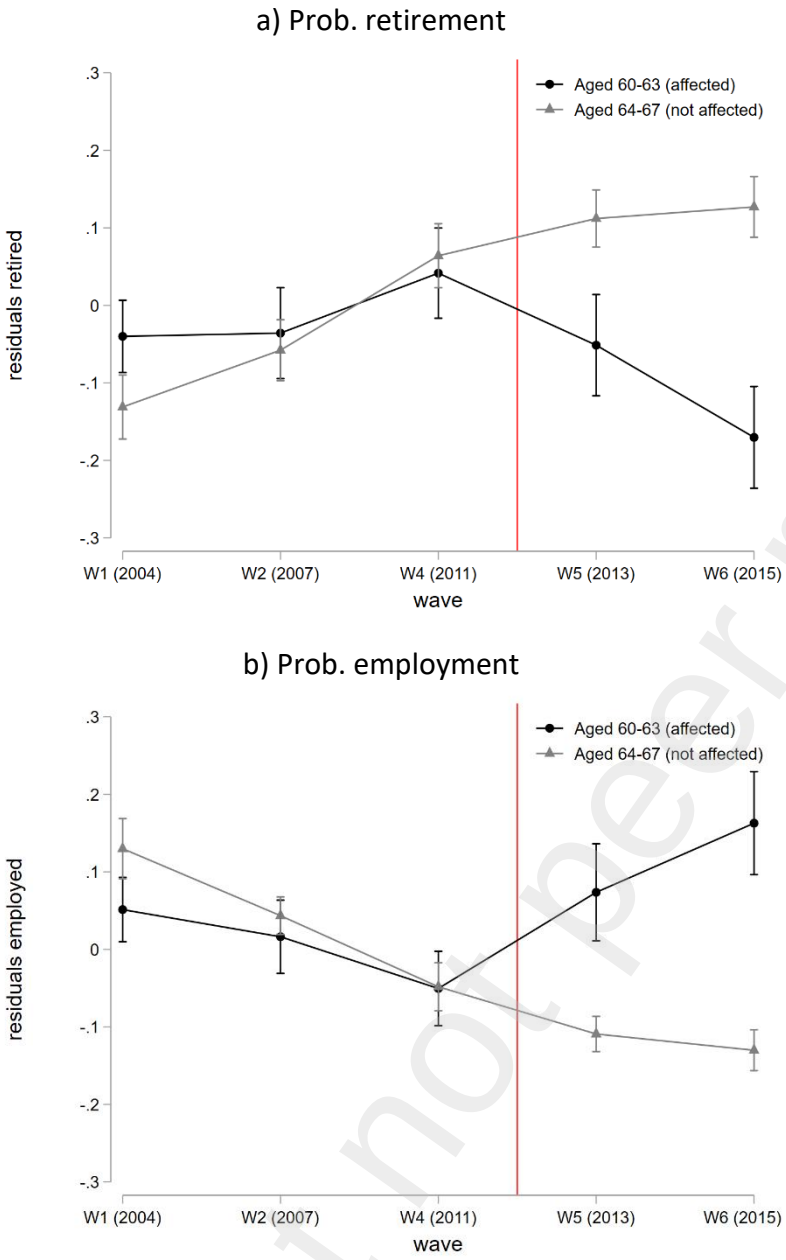
In order for our implementation strategy with SHARE to be valid, the age groups affected and non-affected by the reform should follow similar trends before the reform. In Figures B1 and B3 we plot the trends in employment status and in the health outcomes where we found significant effects for age group 60-63 (affected by the reform) vs age group 64-67 (not affected by the reform). Regarding the health outcomes, BMI and hospitalizations outcomes are not significantly different in the waves before the reform. Regarding labour market outcomes, those aged 60-63 seem to be already increasing their probability of work, before the reform, although the slope clearly becomes steeper after reform. These slight differences in pre-reform trends with respect to the group 64-67 can be a consequence of cohort of birth differences. As we go forward in time, women of the same age are from younger birth cohorts. The younger cohorts of women might have different work dynamics and be more attached to the labour force. In Figure B2 we further control for this by plotting the errors of regressing the labour market outcomes on a birth cohort linear trend. Note that we directly control for this trend in the specification 2. This figure reports the variation of labour market outcomes after controlling for cohort trend and shows no pre-reform differences in the labour market outcomes. The same occurs for health outcomes (Figure B4).

Figure B1 - Parallel trends check- labour market outcomes in SHARE data



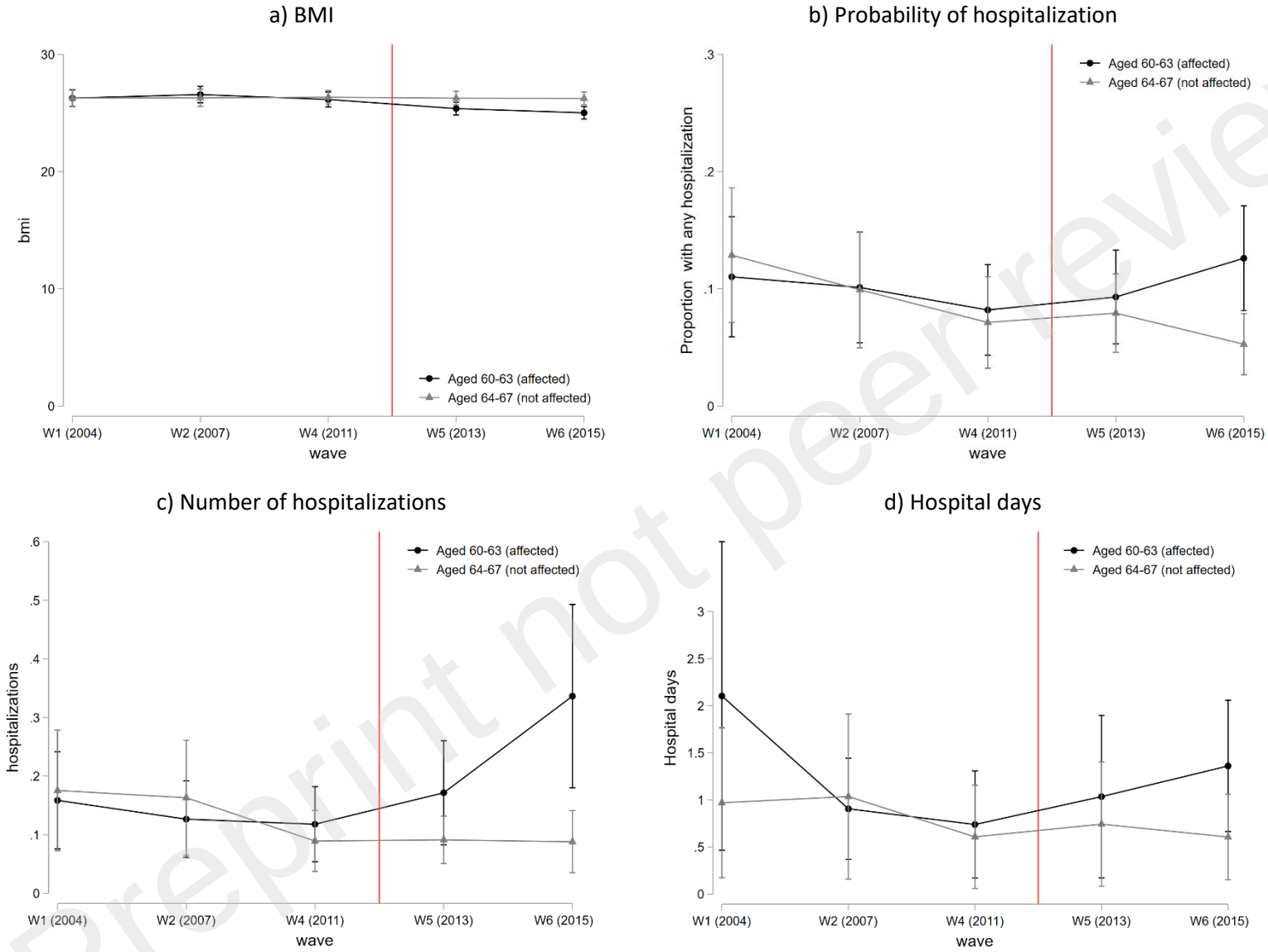
NOTES: On these figures we report the average probability of retirement and employment per age group and wave (n=1,860).

Figure B2 - Parallel trends check (adjusting for cohort of birth trend)- labour market outcomes in SHARE data



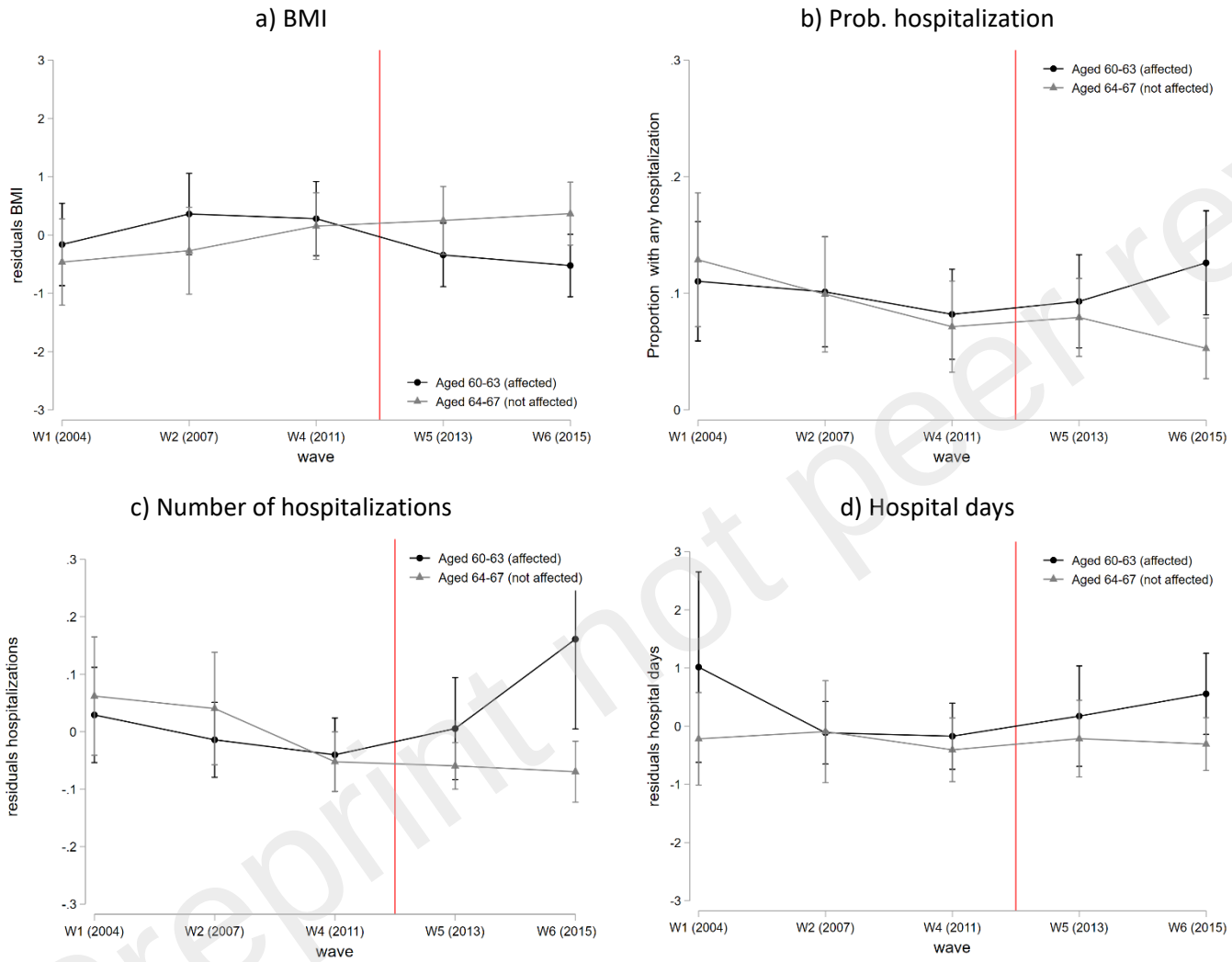
NOTES: On these figures we report the average probability per age group and wave, after controlling for a birth cohort linear trend. That is we plot the errors of regressing the dependent variable on a linear cohort of birth trend, for each age group and wave. (n=1,860).

Figure B3- Parallel trends check – Health outcomes in SHARE data



NOTES: On these figures we report the averages per age group and wave for each corresponding outcome.

Figure B4 - Parallel trends check (adjusting for cohort of birth trend) - Health outcomes in SHARE data



NOTES: On these figures we report the averages per age group and wave for each corresponding outcome, after controlling for a cohort of birth linear trend. That is, we plot the errors of regressing the dependent variable on a linear cohort of birth trend, for each age group and wave.

B.2. Pre-reform birth cohort differences

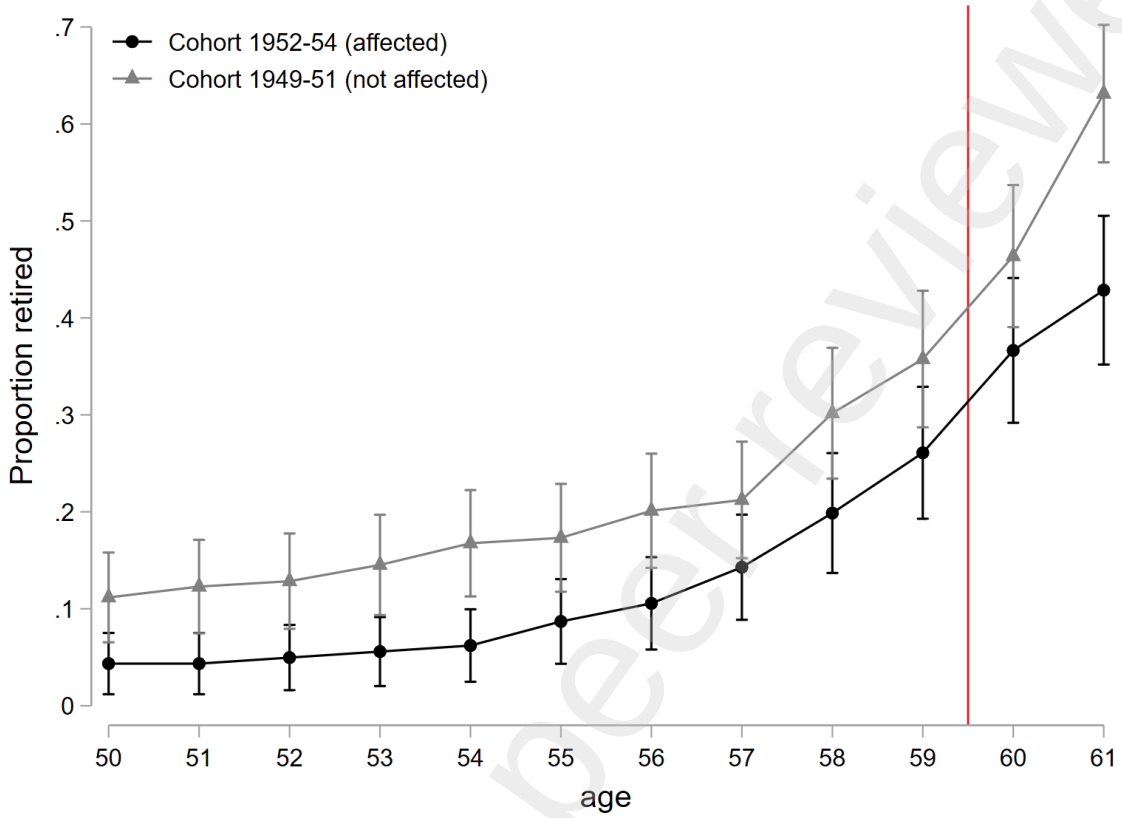
Even if there is a pre-reform parallel trend based on age group, it could be the case that the affected individuals already had different health outcomes before the reform. In Table B1, we look at pre-reform differences from a cohort of birth perspective. Those affected by the reform and aged 61-63 in 2015 are from the birth cohort 1952-54, whereas those not affected by the reform and aged 64-66 in 2015 are from the birth cohort 1949-51. We then compare these two birth cohorts (1952-54 vs 1949-51) before the reform, when they were at the same age (53-55). This comparison shows that these two birth cohorts did not have significant differences in the relevant health outcomes nor in the labour outcomes before the reform. Ideally, one would follow longitudinally these two birth cohorts over the same ages prior to the reform. Based on retrospective information at wave 6, we could create the retirement trajectories for these two cohorts from the age of 50 to age of 61. **(**Figure B5). Both birth cohorts follow a parallel trend up to 60 years old when only the cohort 1952-54 was affected by the reform. Note that despite that these checks suggest that birth cohorts were similar before the reform, we still control in our main regression for a birth cohort linear trend.

Table B1 - Check for birth cohort differences before the reform when aged 53-55 of SHARE data

	(1) Affected cohort Born in 1952-54 by 2007		(2) non-affected cohort Born in 1949-51 by 2004		Difference in means[a] (2) – (1)
	Mean	SE	Mean	SE	
Employment status					
Retired	0.071	0.259	0.176	0.383	0.104
Employed	0.829	0.38	0.743	0.44	-0.085
Labour market	0.886	0.32	0.824	0.383	-0.061
Health variables					
Body mass index	25.534	4.064	24.984	3.649	-0.55
Prob (hosp)	0.086	0.282	0.095	0.295	0.009
Hospitalizations	0.114	0.435	0.108	0.354	-0.006
Hospital days	0.643	3.909	0.784	3.163	0.141
Observations	70		75		145

NOTES: [a] p-values of the independent sample t-test for the difference in means: *** p<0.01, ** p<0.05, * p<0.1.

Figure B5 - Probability of retirement by birth cohort over age in SHARE data (based on retrospective information at wave 6)



NOTES: This figure plots the probability of being retired by birth cohort over age, based on retrospective information of respondents (not homeworkers) at wave 6, n= 340. To build the retirement trajectories we use a variable that indicated the year of retirement, for those retired in wave 6. From that variable we calculated the age at retirement, and then created a dummy variable equal to one if the individual was retired for each age. Women who were employed or unemployed at wave 6 had the retirement dummy variable equal zero at all ages.