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

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Abstract

As retirement ages increase around the world, not all workers may be equally able to extend their working lives. In this article, we examine the health and labor market effects of an Italian pension reform that suddenly increased the normal retirement age up to 7 years for women and up to 2 years for men. To do this, we use linked labor and healthcare administrative data, jointly with survey data and difference-in-difference methods. Our results show that the reform was effective in postponing retirement among both genders, as pension claiming dropped substantially for older workers. However, there were also side effects as the reform significantly pushed previously employed men and women into unemployment and disability pension. Among women only, the reform also increased sick leave and hospitalizations related to mental health and injuries. These effects were driven by women with previously low health status, suggesting that undifferentiated and abrupt increases in pension age might harm more vulnerable workers. Coherently with the milder tightening of retirement age experienced by men, labor market responses were smaller in size, and they did not suffer any significant health effects.

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1 | INTRODUCTION

Population aging 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 (Filomena & Picchio, 2021; Odone et al., 2021; Pilipiec et al., 2021; Vigezzi et al., 2021).

In this article we study the health and labor market effects of the 2011 Italian pension reform that suddenly increased the normal retirement age for some cohorts of women by up to 7 years (from 60 to 67). Our identification strategy compares neighboring 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 labor 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 behaviors and healthcare use.

Our results show that the reform was effective in postponing retirement of women, as pension claims dropped by about 25% points (pp), and in increasing working rates between 10 and 12 pp among the ages 60–63, although it also increased the probability of unemployment or inactivity in a similar magnitude. Additionally, we report a significant substitution effect toward 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). We also carried out an analysis of the effect of the reform on men whose retirement age was raised from 65 to 67, a smaller and more gradual increase than that of women. While the results on labor market outcomes were similar to that of women with respect to the direction of the effect, they were smaller in magnitude and last for shorter time, disappearing by age 67 (the new normal retirement age). Furthermore, we did not find compelling evidence of any significant effect on the health of men.

Our heterogeneity analysis shows that women 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. On the contrary, we did not find any significant gradient on health for the case of men.

Previous research on the effect of retirement on health summarized in a recent meta-analysis is inconclusive (Filomena & 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 exerted different effects on distinct dimensions of health. For example, while previous literature generally finds that retirement is detrimental to cognitive abilities (e.g., Mazzonna & Peracchi, 2017; Celidoni et al., 2017), when mental health outcomes are considered, retirement is found to be beneficial, as shown by a recent meta-analysis (Odone et al., 2021). The literature that directly focuses on the health effects of pension reforms increasing retirement age also provides mixed evidence (Pilipiec et al., 2021), thus suggesting incomplete knowledge 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 only 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, 2018). 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 men with low socioeconomic status.

Our paper speaks also to the literature on program substitution and complementarity between welfare programs. It is common to find contradictory findings in this strand too, driven by specific institutional settings in place in different countries. For instance, an evaluation of an earlier Italian pension reform restricting the access to normal retirement showed that male employees substituted old age pension with disability and unemployment benefits (Ardito, 2021). On the contrary, Geyer and Welteke (2019) and Oguzoglu et al. (2020) found no evidence of active program substitution for Germany and Australia, respectively.

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 career plannings (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 behaviors of workers who had their potential work horizon extended, even before retirement. Carta and De Philippis (2021) pointed out to adjustments in labor supply among mid aged individuals in response to expected future longer working horizon. As a consequence, some of the findings obtained by evaluations of pension reforms that were announced in advance, can be affected by the anticipation of health and labor market participation behaviors. This could mask some potentially harmful effects of raising the retirement age.[1]

Our article provides four key contributions to the literature. First, unlike previous studies, we examine which workers are most affected by pension age rises with a particular focus on subjects' response to the reform based on their health status. This provides important policy implications for future interventions, suggesting that designing pension reforms that target individuals based on their health status might avoid unintended negative effects of a higher retirement age. Second, our administrative healthcare data by diagnosis allow us to focus on hospitalizations for diseases known to be associated to work-related exposures (EuroSafe, 2014; Hulshof et al., 2021; Theorell et al., 2015). We combine these outcomes with other self-reported health data, providing a comprehensive analysis on different set of health outcomes, something uncommon in previous literature. Third, we focus our main analysis on women (who experience a drastic and unexpected increase in pension age), while also analyzing the effect on men (who experience a smaller but also unexpected increase in pension age). In this way, we provide insights on the effect of both drastic and more gradual increases of pension age on health, while discarding any anticipation effects. Fourth, while the 2011 Italian reform has been previously studied vis-à-vis some labor market outcomes such as sick leave (Brunello et al., 2023; Moscarola et al., 2016), old-young worker substitution (Bertoni & Brunello, 2021; Carta et al., 2021) and spillovers on co-workers (Bianchi et al., 2021; Carta & De Philippis, 2021), this is the first study to look at the health, employment and program substitution effects of the Italian reform on the workers directly affected by the reform, investigating a rich set of previously unexplored outcomes.

Our article is organized as follows. Section 2 describes the Italian pension system and the 2012 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 labor market and health outcomes. Section 6 provides a general discussion of the results. Lastly, in Section 7 we conclude with policy implications.

2 | INSTITUTIONAL SETTING

2.1 | The Italian pension system

The Italian pension system is public and based on a pay-as-you-go (PAYGO) mechanism, where pension benefits are financed by contributions paid by both employees and employers. Pension formula was originally based on a very generous defined benefit (DB) scheme. Since the 1990s, the Italian pension system underwent a series of reforms, implying a long transition from a DB to a Notional Defined Contribution (NDC) system, while maintaining the PAYGO financing mechanism. Still, the system remain relatively generous. From 1995 to 2019, replacement rates remained approximately equal to 77% for both men and women (Mazzaferro, 2023), higher with respect to international comparisons (OECD, 2023). The Italian system has two main tracks of access to public pension schemes, both giving access to full pension benefit: old-age and seniority. The two schemes are equivalent with respect to the way pension benefit is defined, that is, they both adopt the same pension formula. The only difference is related to the eligibility condition required to accrue the benefit right. Whereas old-age pension benefits accrue when individuals reach a certain age, with a mild requirement on contribution years, seniority pension eligibility condition is based on long contribution history, combined with no or a mild minimum age requirement. Therefore, seniority pensions are typically paid in advance of old age pensions and their replacement rates is also higher (78,6% vs. 74,1%), since on average they are granted to workers with longer contribution years (Brugiavini et al., 2021; Mazzaferro, 2023). For workers whose work ability is severely limited due to a medical condition, short- or long-term disability benefit arrangements are in place. In general disability benefit is below 300 euro per month for those without contribution histories, while the amount is defined applying the same DB/NDC formula used for old age public pensions for those with at least 5 years of contributions (MISSOC, 2021).

2.2 | The 2012 pension reform

The reform under analysis in this paper was approved on December 22, 2011 after a "technocratic" government unexpectedly came into power in November 2011 to manage a fiscal and political crisis. The reform became effective on January 1, 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 & De Philippis, 2021). 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 (60–67) and a smaller and more gradual increase of 2 years for men (65–67). In our main analysis we focus more on the case of women because they were the most affected by the reform. We also provide an analysis on men in Section 5.6.

In Table A1 of the Appendix A we summarize the retirement requirements for both sexes, before and after the 2012 pension reform, for both the old age and seniority pension tracks. The reform especially affected the women working in the private sector for whom minimum old-age pension 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 & De Philippis, 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 A1 in Appendix A, 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 workers 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 A1, 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. Still, this option was hardly used due to a substantial reduction in the pension amount. 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 & Paradisi, 2019).

Our identification strategy relies on comparing neighboring cohorts who were differently affected by the reform due to the increase in the statutory pension age (old-age pension). In Table 1, we report the age of each cohort by year, signaling (in dark gray) whether each cohort could retire at a particular year, based on old-age pension requirements for women working in the private sector (Panel A). We also signal (in light gray) 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 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, 2017); hence, most women in each cohort will be exposed to the private sector retirement rules. Therefore, Table 1 (Panel A) essentially shows the first year in which each cohort could access a public pension if the woman 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 neighboring cohorts 1951 and 1952. On the one hand, women born in 1951 were eligible for retirement under the statutory pension age in 2011 by the age of 60. Importantly, a 1-year waiting window was in place[2] for those reaching eligibility in 2011. As a result, individuals who reached pension eligibility

TABLE 1 Normal and early retirement age per cohort and year for women who did not reach the seniority pension (i.e., <40 years of contribution). [Colour Table can be viewed at wileyonlinelibrary.com]

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
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**[f]	**61**	**62**	**63**	**64**	**65**	**66**	**67**	**68**	**69**
1952	**52**	**53**	**54**	**55**	**56**	**57**	**58**	**59**	**60**	**61**	**62**	**63**[c]	**64**[c]	**65**[d]	**66**[e]	**67**	**68**
1953	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67

Note: This table is based on Bertoni and Brunello (2021) and Centro Studi e Ricerche di Itinerari Previdenziali (2019). It shows retirement requirements for women working in the private sector in place at each year as summarized in Table A1 in Appendix A. The cells in yellow indicate the NRA and ERA in place at each year. The cells in dark gray indicate that the cohort has reached the statutory age for old-age pension at that particular year. The cells in light gray indicate if the cohort could retire under the quota system (i.e., more than 35 years of contribution) in that particular year. Bold values signal the cohorts (treated and control) included in the sample.

[a]"NRA": Normal retirement age.

[b]"ERA": Early retirement age allows to retire under the quota system with at least 35 years of contribution.

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

[d]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).

[e]Only women who were 66 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).

[f]In the year 2011 the so-called waiting window was in place, requiring workers who reached the statutory pension age to wait for 12 months before receiving their first pension payment.

at 60 in 2011 only commenced receiving their pension at the age of 61. On the other hand, women born in 1952 were only eligible for claiming a pension from 2015 at the earliest, when turning 63.[3] 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 A1 in Appendix A). Therefore, their eligibility to retirement was suddenly postponed by 3–7 years. In our main specification, we compare labor market trajectories and hospital outcomes of these two cohorts over time in a difference-in-difference setting (See Section 4). Importantly, these two cohorts were not differently affected by any of the previous pension reforms implemented in Italy.[4]

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 neighboring unaffected cohort (born in 1951), our WHIP sample is formed by 9677 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[5].

We use WHIP-Health to explore the following main labor 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 labor market state at any moment of the year. As far as for the "employment" outcome, at least 1-week job spell should be observed in the year. The outcome "receiving a work pension" includes all types of pensions, that is, 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 without unemployment benefits. In addition, we look at the probability of taking at least a week of sick leave[6] 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 & Candy, 2006, p. (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 behaviors (Brunner, 1997; Marmot et al., 1997; Pollitt et al., 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) (ICD-9 codes are reported in Table A5).

3.2 | Survey of Health Ageing and Retirement (SHARE)

We use the Italian sample of SHARE (Börsch-Supan et al., 2013), a multidisciplinary microdataset with information on health and socioeconomic conditions of the 50+ population. Our initial sample includes 2952 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 1048 who were homemakers since they are not affected by the reform. Then, our final sample is formed by 1904 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.

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 The final SHARE sample is described in Table A8.

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 analyze the effects of the reform on the labor 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.[7] 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

$$y_{i,t} = \beta_0 + \sum_{\substack{a=53 \\ a\neq 58,59}}^{63} \theta_a \mathrm{Age}_a \mathrm{Cohort1952}_i + \sum_{\substack{a=53 \\ a\neq 58,59}}^{63} \mathrm{Age}_a + \delta_t + \alpha_i + \mu_{i,t} \tag{1}$$

where $y_{i,t}$ can be either the probability of being in a certain labor market status (retired, employed, unemployed/inactive or disability pension) at year t, or the probability of hospitalization at year t for each of the four work-related diagnosis groups (musculoskeletal, stroke and myocardial infarction, injuries or mental health as defined in Section 3.1). Specifically, for labor market outcomes, the dependent variable $y_{i,t}$ equals one if the individual has been at the corresponding labor market status at any time during year t. Note that the four labor market outcomes are not mutually exclusive, although 95% of person-year observations fall in one and only one labor market state. For instance, 0.1% of our sample is simultaneously working and receiving disability benefits at the same year (see Table A2 of Appendix A for descriptive statistics of each labor market outcome). For the probability of hospitalization, the dependent variable $y_{i,t}$, equals one if the individual has had one or more hospitalizations of the corresponding diagnosis group during year t. 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 dummy variables (Age$_a$) 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, before and after the reform, at the same age (measured by the end of year t). Then, the set of interaction terms between age dummies and the cohort dummy (Age$_a$Cohort1952$_i$) measure the effect of postponing access to pension due to the reform. Specifically, these estimates measure the effect of the immediate lack of access to pension at a specific age due to the reform (vs. being able to retire with pension at age 60). From now on, we will always refer to these estimates as the "effect of the reform" or the "effect of postponing retirement". As anticipated, we set ages 58–59 as baseline,[8] which correspond to ages when neither those born in 1951 nor in 1952 could retire. Cohort 1951 could retire at the age of 60 (or at 61 if taking into account the waiting window), whereas cohort 1952 could not retire at least until 63 due to the reform. If the reform had an effect on the labor or health outcomes, the coefficients (θ_a) of the interaction between Cohort 1952 and ages should come out significant from age 60 (a≥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.

To solve the standard age-period-cohort underidentification issue (De Ree & Alessie, 2011), we impose a parametric restriction grouping together two pre-reform ages groups (i.e., 58 and 59) using these as baseline. However, this could in principle be problematic since depending on the restriction imposed, results might change (De Ree & Alessie, 2011). We then test for the robustness of our results in two ways. First, we leverage within cohort variation induced by the reform by running a triple DiD model, getting rid of the age-period-cohort simultaneous identification. Second, we estimate our standard DiD model imposing different groups restrictions. Our results are confirmed in both robustness checks (see Section 5.5 for more details).

As a further robustness test, we run a similar set of regressions specifying the DiD model based on year rather than age, that is, 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. We have adjusted our results for multiple hypothesis testing across labor and health outcomes using the Sidak correction (Abdi, 2007). We provide along the manuscript both adjusted and unadjusted results. Additionally, to avoid the risk of over-adjustment we group all hospitalizations into one category (any work-related hospitalizations), following common practice of creating a summary index as an alternative to multiple hypothesis testing when the multiple outcomes are related (Viviano et al., 2021).

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:

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

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.[9] Before the reform, in waves 1–4, 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 6, below NRA equals 1 if born in 1952 or after and age is lower than 63 and 9 months, zero otherwise. Table A3 in Appendix A illustrates the normal retirement access requirement by age and SHARE wave, which serve as a base for this specification.

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 labor 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 total number of children (living at home or elsewhere).

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 labor market outcomes (Cribb et al., 2016; Morris, 2022; Soosaar et al., 2021; Staubli & Zweimüller, 2013) 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 labor market outcomes: (i) the probability of retirement, (ii) the probability of employment.

5 | RESULTS

5.1 | Effect on the labor 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. Note that the large increase in the probability of retirement for cohort 1951 occurs at the age 61, and not at the age of 60 (statutory pension age) because of the 1-year waiting window discussed in Section 2. 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 61–63 (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). These significant labor market effects are robust to multiple hypothesis testing correction (Table A4 in Appendix A). 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.[10] Results show an increase in the probability of having at least a weekly sick leave by around 5–7 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%–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 A6 in Appendix A). Importantly, before the age of 60, when still none of the cohorts could claim the old age pension, all the labor 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 60–63, although it also increased the probability of unemployment or inactivity in a similar magnitude. Additionally, we report a significant substitution effect toward 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.

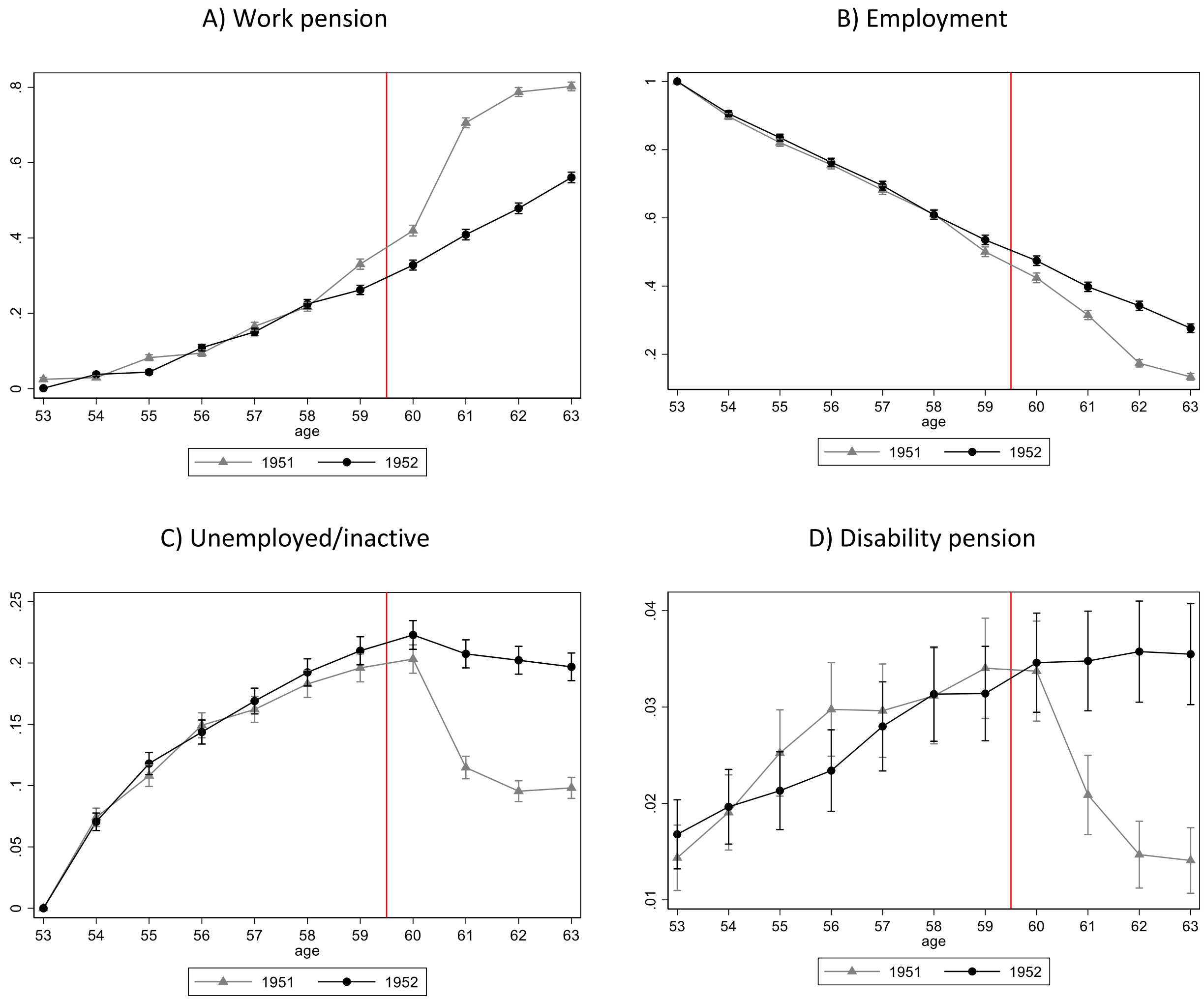

FIGURE 1 Descriptive evidence on labor market responses to the pension reform—Women (WHIP-Health data). These figures report the proportion of women in each employment status by cohort of birth and age. Sample of 9677 women born in 1951–1952 who were employed at 53 followed from age 53 to age 63 (n = 105,122). [Colour figure can be viewed at wileyonlinelibrary.com]

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 (even after adjusting for multiple hypothesis, Table A7 in Appendix A) in the probability of hospitalizations due to mental health diagnosis at the age of 61 and due to injuries at the age of 62. These significant effects occur right at the time when the effect of the reform on labor 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 imprecize. Lastly, we find a null effect for myocardial infarction hospitalizations. As in the case of the labor 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.

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% points (Table 2, 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

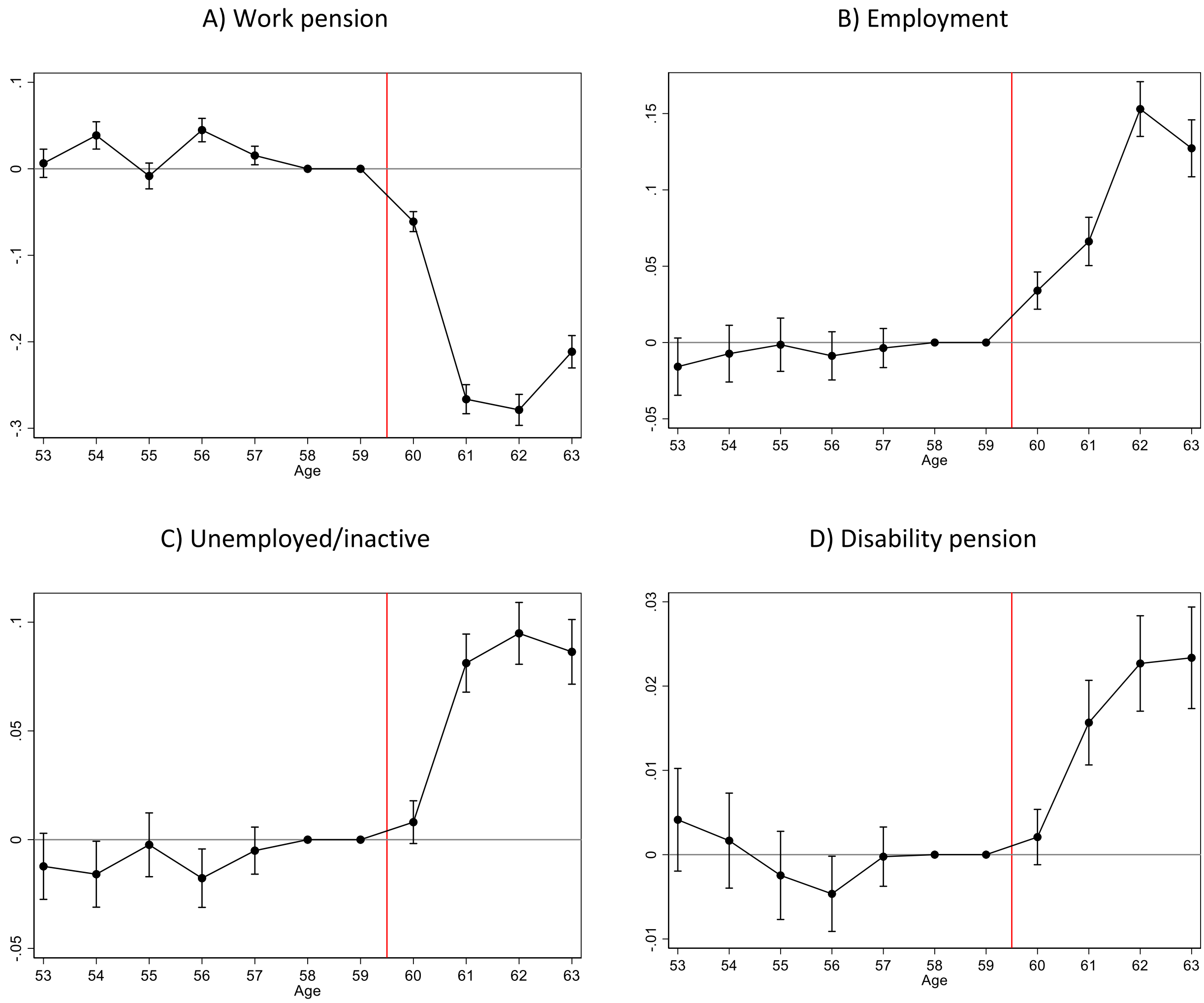

FIGURE 2 Effect of the pension reform on labor market outcomes—Women (DiD estimates on WHIP-Health data). These figures report the effect of the pension reform on the probability of being in each labor market outcome derived from the DiD model specified in Equation (1). Each figure plots the coefficients 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 9677 women born in 1951–1952 who were employed at 53 followed from age 53 to age 63 (n = 105,122). The p-values adjusted for multiple hypothesis testing are reported in Table A4 of Appendix A. [Colour figure can be viewed at wileyonlinelibrary.com]

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 behavior. Then, using the same specification, we exploit this effect on retirement behavior to test for possible second-order effects on health (Table 2, Panel B and C).

SHARE results also show that the reform affected healthcare use (Table 2, 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 loses significance when adjusting for multiple hypothesis). 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 2, 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 more than 14 pp (36% with respect to the mean), a decrease in CASP quality of life of more than 1.1 points (3% with respect to the mean), an increase in the depression scale of

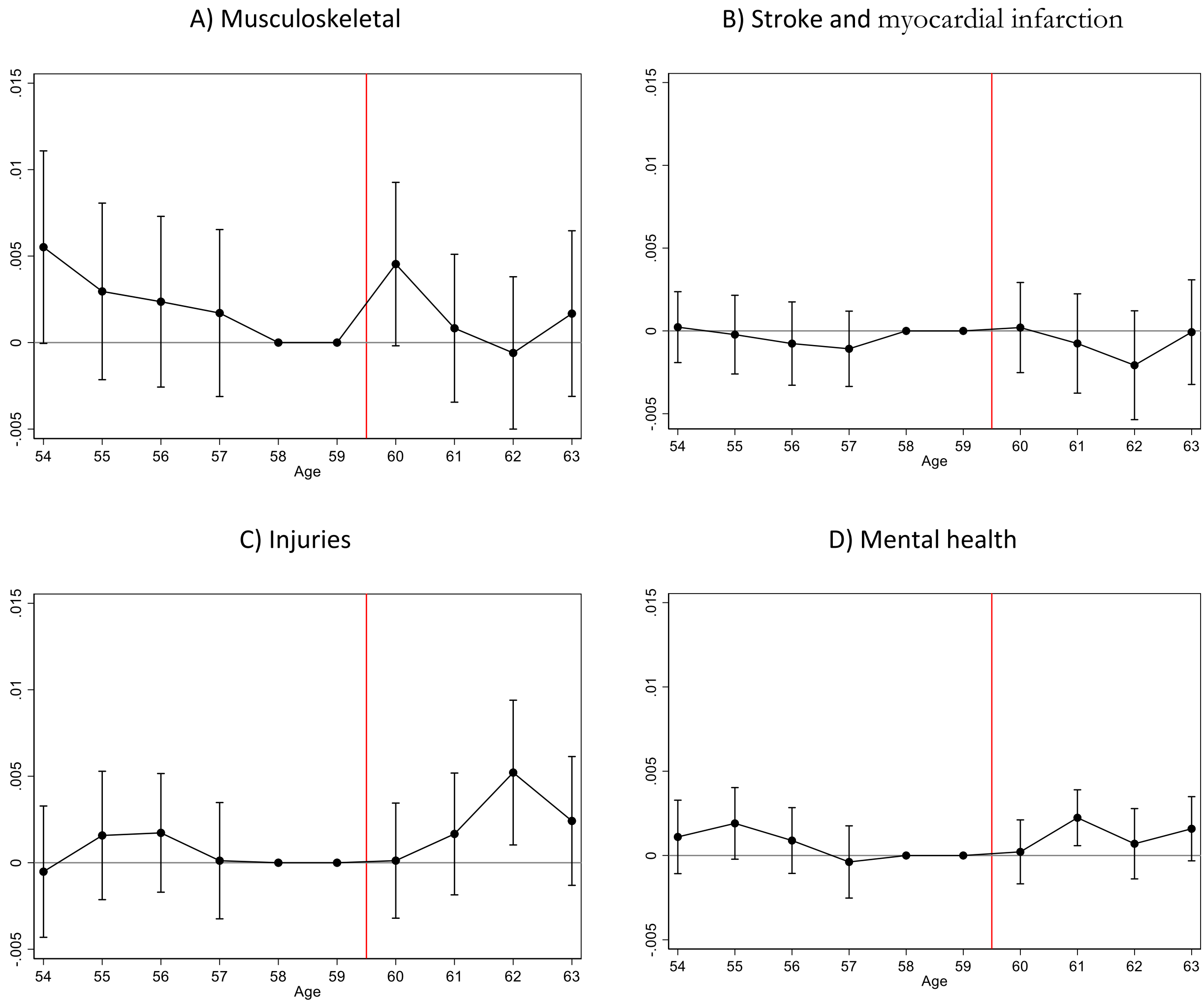

FIGURE 3 Effect of the pension reform on work-related hospitalizations—Women. These figures report the effect of the pension reform on the probability of hospitalization by diagnosis group, derived from the DiD model specified in Equation (1). Each figure plots the coefficients and the 95% confidence intervals of the interactions between the Cohort 1952 and the age dummies, leaving ages 58 and 59 as base category. Sample of 9656 women born in 1951–1952 who were employed at 53 followed from age 54 to age 63 (n = 95,445). The p-values adjusted for multiple hypothesis testing are reported in Table A7 of Appendix A. [Colour figure can be viewed at wileyonlinelibrary.com]

more than 0.28 (9% with respect to the mean), an increase in mobility index of more than 0.12 (23% with respect to the mean) and a cognitive deterioration of more than −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.

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 labor 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).

TABLE 2 Effect of the pension reform on self-reported health outcomes—Women (SHARE data).

Panel A—Labor market outcomes

Variables	(1) Retired	(2) Employed
BelowNRA	−0.328***	0.313***
	(0.052)	(0.049)
95% CI	[−0.430; −0.226]	[0.218; 0.408]
p-value	0.001	0.001
Sidak p-value	0.001	0.001
Mean Y	0.390	0.134
Observations	1860	1860

Panel B—Health outcomes and health behaviors

Variables	(1) Bad self-reported health	(2) CASP quality of life	(3) Euro-d mental health	(4) Mobility index	(5) Recall (number of words)	(6) Body mass index (BMI)	(7) 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]
p-value	0.568	0.594	0.324	0.527	0.585	0.004	0.093
Sidak p-value	0.997	0.998	0.935	0.995	0.998	0.025	0.493
Mean Y	0.390	34.65	2.931	0.521	5.029	26.06	4.370
Observations	1863	1726	1849	1864	1839	1847	1374

Panel C—Healthcare use outcomes

Variables	(1) Doctor visits	(2) Any hospitalization	(3) Number of hospitalizations (NB)	(4) 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]
p-value	0.854	0.096	0.019	0.026
Sidak p-value	1.000	0.331	0.074	0.099
Mean Y	8.637	0.0909	0.149	0.966
Observations	1853	1862	1861	1860

Note: 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. 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. Sidak p-values use the Sidak correction (Abdi, 2007) for multiple hypothesis testing across outcomes (i.e., 2 outcomes for labor market, 7 outcomes for health outcomes, and 4 outcomes for healthcare use).

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

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 A9 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 A10 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 A11 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 54–57. 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 toward 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 significantly increased the probability of these hospitalizations by 0.8 pp for the group with the poorest health (Sidak corrected p-value<0.10), whereas it had no effect on the group with better health. Similarly, the coefficient for injuries increases as the pre-reform health status deteriorates. After grouping all work-related hospitalization into one group, we also found a clear gradient on the effect of the reform, as it seemed to increase the probability of any work-related hospitalization by 1.5 pp (p-value < 0.10) on those with very bad health, while it had no effect on those with good health. 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 A12 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 A13 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 67% (OECD, 2023)). 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.

5.5 | Robustness checks

5.5.1 | Triple difference model exploiting contribution years

Since cohort, age and year effects cannot be jointly identified, models like our DiD model 1 can suffer from the age-period-cohort underidentification issue. While in model 1 we grouped age 58–59 to try to solve this issue, the later may be also problematic (De Ree & Alessie, 2011). As discussed by Carta and De Philippis (2021) the 2012 Italian reform also generated within cohort variation that could be exploited to solve the under-identification issue. This is what we exploit in this section. In particular, we leverage within cohort variation in contribution years in a triple difference model for a subsample of women for whom we can observe full contribution histories. More concretely we exploit the fact that only women below 40 years of contribution by 2011 were affected by the reform since with 40 years of contribution they could already access to a seniority pension. Our triple difference model (DDD) is an expansion of model (1) as follows:

$$y_{i,t} = \beta_0 + \sum_{\substack{a=53 \\ a\neq 59}}^{63} \Omega_a \text{Age}_a \text{Cohort1952}_i \text{below40}_i + \sum_{\substack{a=53 \\ a\neq 59}}^{63} \theta_a \text{Age}_a \text{Cohort1952}_i + \sum_{\substack{a=53 \\ a\neq 59}}^{63} \text{Age}_a + \alpha_i + \mu_{i,t} \tag{3}$$

where below40_i equals one if the woman was expected to have a maximum of 40 years of contribution by age 60. The rest of the variables are equal to those of model (1). We calculate expected years of contribution by age 60 based of the contribution years by age 53, and assuming no discontinuity in the labor market. Note that we could only retrieve information of the contribution years for the subsample of those who were retired by 2016 (6675 of 9677 women of our main sample). The set of triple interaction terms $\text{Age}_a\text{Cohort1952}_i\text{below40}_i$ now measure the effect of the reform.

Main results for labor market are displayed in Figure 4 below. Our results are robust to this identification. Coefficients are of larger magnitude than in our main model, as expected given that this specification is able to better identify the group of workers "actually" bound by the reform. Results are robust also when we correct for multiple hypothesis (Table A14 in Appendix A). In Figure 5 we summarize the main hospitalization results (i.e., for the full sample, and by previous health status), using a simple triple difference model grouping the effect on all post-reform ages into a single coefficient (i.e., a triple interaction post60 × Cohort1952 × below40). Coherently with our main model, the mental health effect seems to be driven by those in very bad health, although it does reach significance levels. They also suffer a significant increase of 2.1% points for those in injuries, although p-value increases to 0.14 once we adjust multiple hypothesis. It is still worth noting that this model has a lower statistical power as we use a smaller selected sample of those with information on contribution years. After we group all hospitalizations into one category (any work-related hospitalizations), the health gradient clearly shows up again, with those with very bad health showing a significant increase in hospitalizations by 5.4 pp (Sidak p-value = 0.051, Table A15 in Appendix A), while those with good health having a zero effect. Overall, these results suggest that our results are not affected by the above explained age-period-cohort underidentification issue.

5.5.2 | Other robustness checks

We further carry out the following robustness checks: (a) expand the treatment group by including also cohort 1953, (b) excluding age 60 since at this age the treatment cohort 1951 was affected by the 1-year waiting window and therefore had an incentive to also postpone retirement to 61 (see Section 2.2 for a discussion on the waiting window), (c) not including a time trend while including a 58 age dummy, (d) combining the three previous robustness check in a model. Both labor market and hospitalization results are qualitatively unaltered and often also quantitatively almost the same (See Figures A2 and A3 in Appendix A). The health gradient observed in the effect on the pension reform also seems to be confirmed with these robustness checks (Table A16 in Appendix A) with a larger substitution effect toward disability among those with worse health as well as a stronger effect on sick leave. Additionally, the significant effect among mental health injuries continues to be concentrated among those with very bad health, with a zero effect among those with good health. The gradient seems also to be apparent on injuries and more in general on any work-related hospitalizations although the coefficients fail to reach statistical significance.

5.6 | Effect of the reform on men

The 2011 pension reform also restricted access to normal retirement for men. However, ss explained in Section 2, the increase in the retirement age for men was smaller and more gradual (65–67) than that of women (60–67). Still, in this subsection we examine the potential labor market and health effects of the reform on men. Our identification strategy

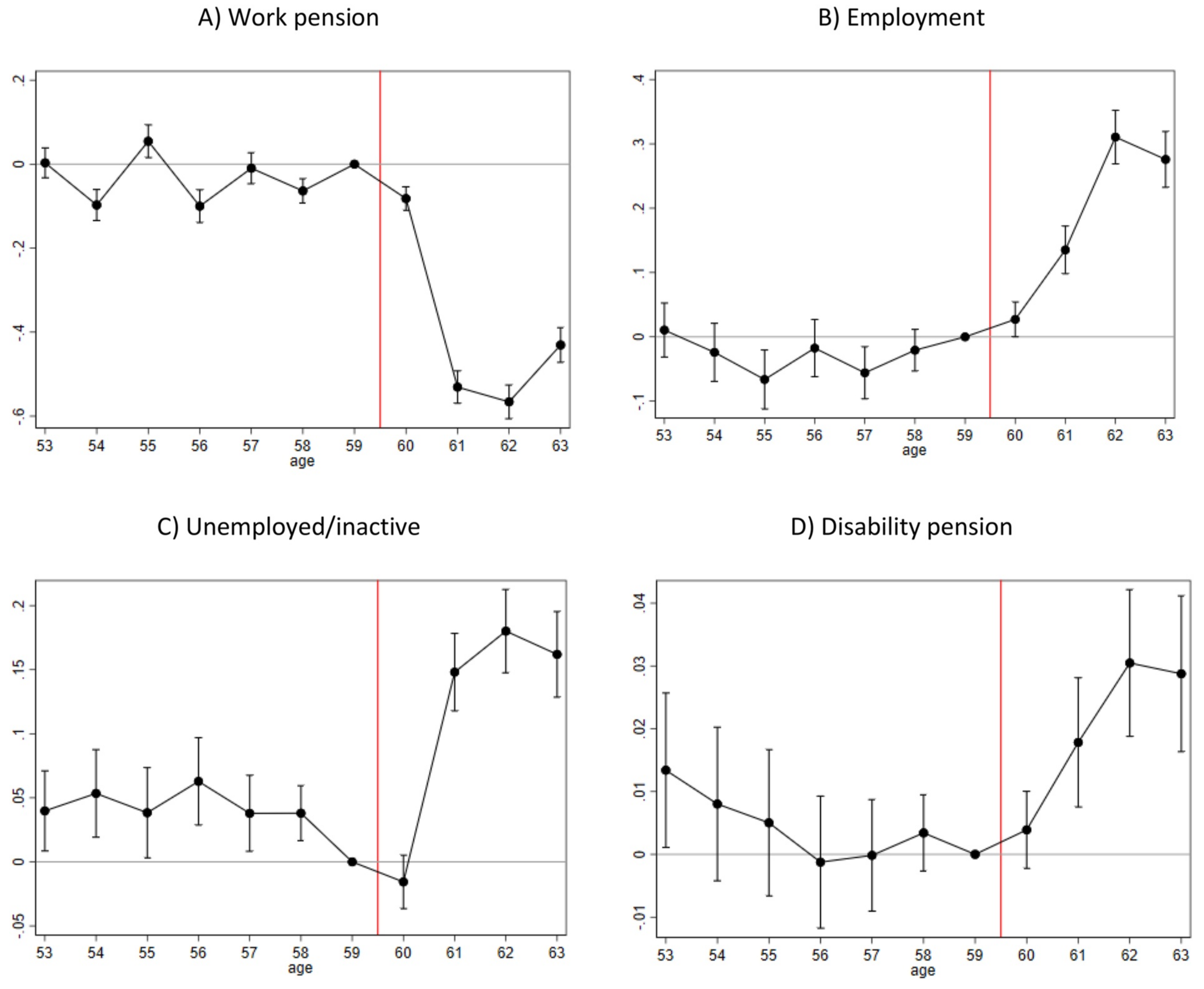

FIGURE 4 Effect of the pension reform on labor market outcomes—Women (DDD estimates on WHIP-Health). These figures report the effect of the pension reform on the probability of being in each labor market outcome derived from the DDD model specified in Equation (3). Each figure plots the coefficients and the 95% confidence intervals of the interactions between the Cohort 1952, the low contribution group, and the age dummies, leaving ages 59 as the base category. Note that the y scale is not the same for each graph. Sample of 6675 women born in 1951–1952 who were employed at 53, not yet receiving any pension and who received it by 2016 ($n = 74{,}439$). The p-values adjusted for multiple hypothesis testing are reported in Table A14 of Appendix A. [Colour figure can be viewed at wileyonlinelibrary.com]

(summarized in Table 3 below) is similar to that for women as we also compare birth cohorts over time. On the one hand, men born in 1945 were eligible for retirement under the statutory pension age when turning 65. On the other hand, men born in 1947 were only eligible when turning 66 or 67 (depending on the month of birth). Then, men from cohort 1947 form our "treatment" group since they had to postpone retirement from 1 to 2 years due to the reform, whereas cohort 1945 is our control group. Note that we did not use the closest neighbor cohort 1946 because although they were formally able to retire at 65 by 2011, they had to wait for 1-year for the first pension payment (due to the so-called 1-year waiting window) (Figure A4 in Appendix A).[11]

Additionally, we also leverage within cohort variation in contribution years. More concretely, we exploit the fact that men older than 60 with more than 35 years of contribution could access an early retirement pension before 2011 (Table A1 in Appendix A). Therefore, only men of cohorts 1947 with less than 35 years of contribution by 2011 were affected by the reform, as those with more years of contribution could access a pension through early retirement (seniority pension). Our model is then very similar to that of Equation (3):

$$y_{i,t} = \beta_0 + \sum_{\substack{a=58\\a\neq 64}}^{67} \Omega_a \mathrm{Age}_a \mathrm{Cohort1947}_i \mathrm{below35}_i + \sum_{\substack{a=58\\a\neq 64}}^{67} \theta_a \mathrm{Age}_a \mathrm{Cohort1947}_i + \sum_{\substack{a=58\\a\neq 64}}^{67} \mathrm{Age}_a + \alpha_i + \mu_{i,t} \tag{4}$$

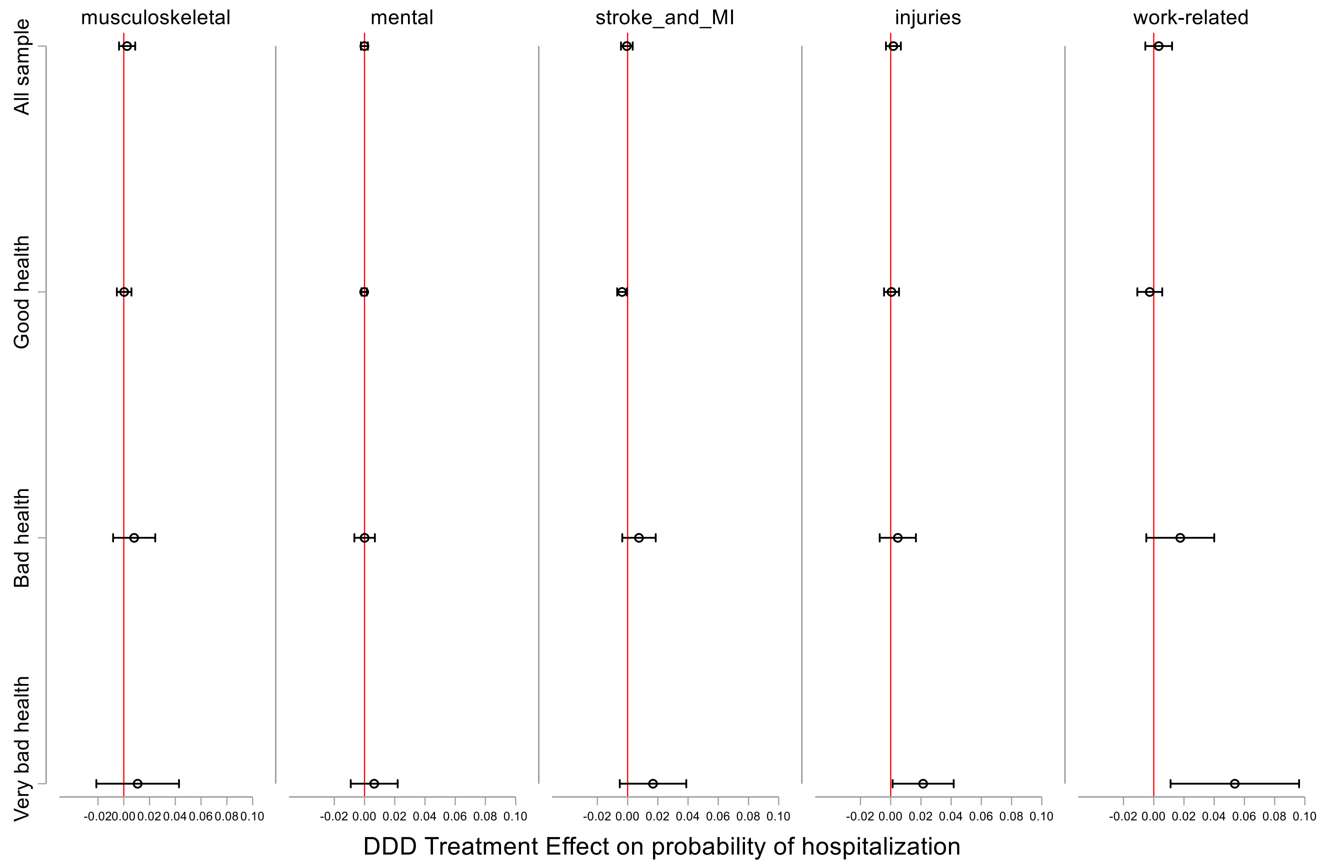

FIGURE 5 Effect of the pension reform on work-related hospitalizations by health status. Women. Simple triple difference (DDD) model. This figure reports the results of a simple triple difference model: $y_{i,t} = \beta_0 + \beta_1 1952_i \times \text{post60} \times \text{below40}_i + \beta_3 1952_i \times \text{post60} + \beta_3 \text{post60} \times \text{below40}_i + \beta_4 \text{post60} + \alpha_i + \mu_{i,t}$, where 1952 equals 1 if born in 1952, post60 equals one if age 60 or older, below40 equals 1 if the woman was expected to have a maximum of 40 years of contribution by age 60, and α_i are individual fixed effects. Each coefficient reports the estimate and the 95% Confidence Interval of the triple interaction ($1952_i \times \text{post60} \times \text{below40}_i$) from a different regression with a combination of outcomes (as labeled at the top of the graph) and samples (as labeled as the y axis). In particular, each graph from the first to the fourth report the effect of the reform on the probability by cause of hospitalization (musculoskeletal, mental, stroke and myocardial infarction [MI] or injuries). The graph at the right groups all hospitalizations into one category (i.e., reports the effect of the reform on the probability on any "work-related" hospitalization). "All Sample" include our main full sample of women. "Good health", "Bad health" or "Very bad health" include women who during the ages 54 to 57 had no hospitalization ($n = 4754$), one or more hospitalizations ($n = 1921$) or two or more hospitalizations ($n = 710$), respectively. Robust standard errors clustered at individual level in parenthesis $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. The p-values adjusted for multiple hypothesis testing are reported in Table A15 of Appendix A. [Colour figure can be viewed at wileyonlinelibrary.com]

TABLE 3 Retirement age per cohort and year for men who did not reach 35 years of contribution. [Colour Table can be viewed at wileyonlinelibrary.com]

Cohort	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020
NRA	65	65	65	65	65	65	65	65	66	66,3	66,3	66,3	66,7	66,7	66,7	67	67
1945	**59**	**60**	**61**	**62**	**63**	**64**	**65**	**66**	**67**	**68**	**69**	**70**	**71**	**72**	**73**	**74**	**75**
1946	58	59	60	61	62	63	64	65[a]	66	67	68	69	70	71	72	73	74
1947	**57**	**58**	**59**	**60**	**61**	**62**	**63**	**64**	**65**	**66**[b]	**67**	**68**	**69**	**70**	**71**	**72**	**73**
1948	56	57	58	59	60	61	62	63	64	65	66[b]	67	68	69	70	71	72

Note: This table is based on the retirement requirements for men working in the private sector in place at each year as summarized in Table A1, Appendix A. The yellow cells show the normal retirement age (NRA) and the early retirement age (ERA) in force in each year. Bold values indicate the cohorts (treated and control) included in our sample. The cells in dark gray indicate that the cohort has reached the statutory age for old-age pension at that particular year.

[a]In the year 2011 the so-called waiting window was in place, requiring workers who reached the statutory pension age to wait for 12 months before receiving their first pension payment.

[b]Only those who were 66 years 3 months years old (born in September or before).

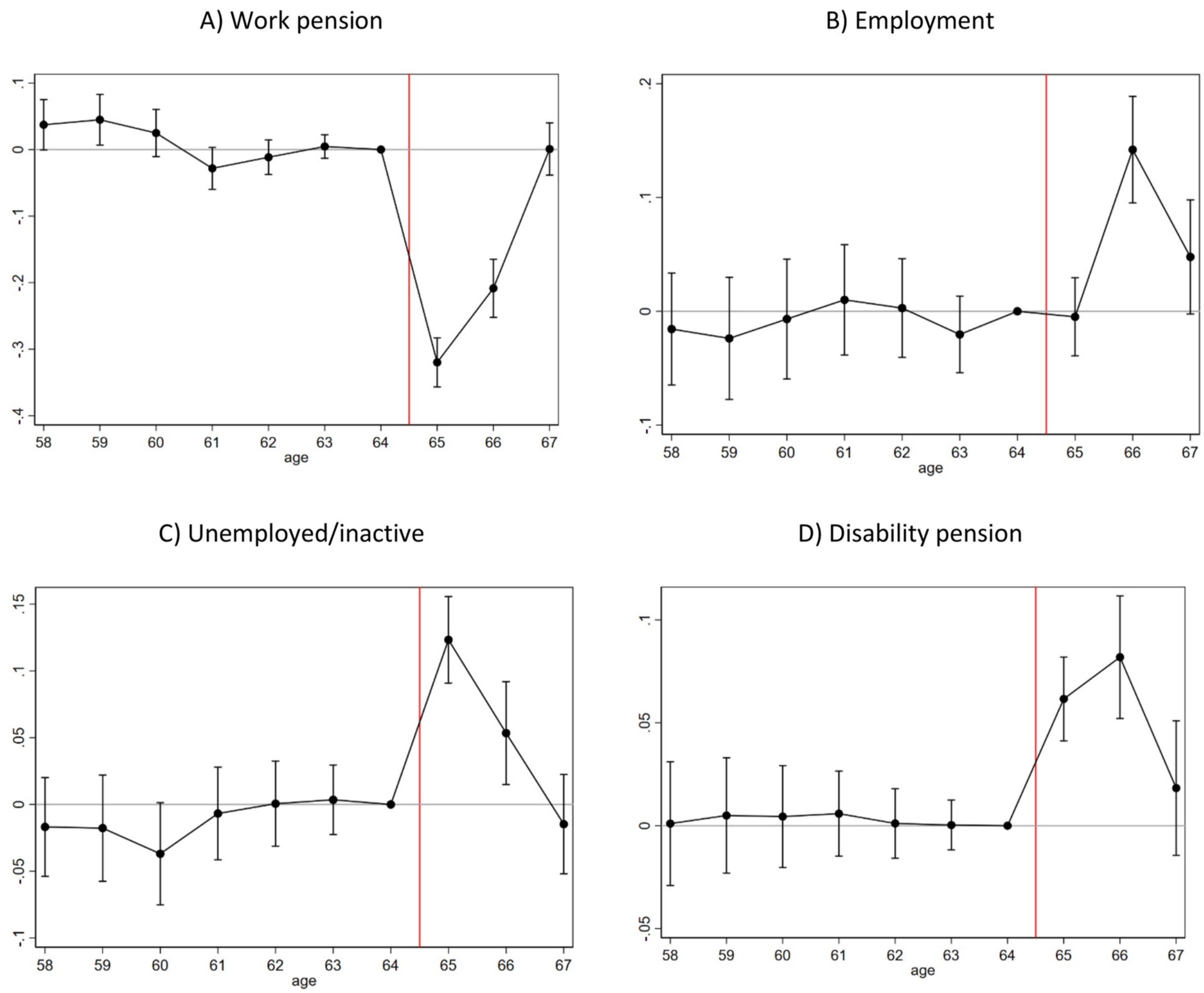

FIGURE 6 Effect of the pension reform on labor market outcomes—Men (DDD estimates on WHIP-Health). These figures report the effect of the pension reform on the probability of being in each labor market outcome derived from the DDD model specified in Equation (4). Each figure plots the coefficients and the 95% confidence intervals of the interactions between the Cohort 1947, the low contribution group, and the age dummies, leaving ages 64 as the base category. Note that the y scale is not the same for each graph. Sample of 6288 men born in 1945 and 1947 who were employed at 58, not yet receiving any pension and who received it by year 2016 ($n = 70{,}006$). The p-values adjusted for multiple hypothesis testing are reported in Table A17 of Appendix A. [Colour figure can be viewed at wileyonlinelibrary.com]

where below35_i equals one if the man was expected to have a maximum of 35 years of contribution by age 65. We calculate expected years of contribution by age 65 based of the contribution years by age 58, and assuming no discontinuity in the labor market. The set of triple interaction terms $\text{Age}_a\text{Cohort1947}_i\text{below35}_i$ measures the effect of the reform on men.

Our sample consists now of 6288 men born in 1945 or 1947 who were employed not receiving any pension at the age of 58, and who were receiving a work pension by year 2016.[12] Observations lost due to premature mortality correspond to 271 men (4,1%) who died before the age of 67.

Our results for the labor market outcomes (Figure 6) show that the reform also had an effect on male's work to retirement transition. The DDD specification demonstrates a nearly perfect parallel trend in pre-treated ages, with a highly significant decrease in pension claiming after the reform. However, the effect on pension claiming (30 pp at age 65) is of a smaller size than that for women and lasts for shorter time, disappearing by age 67. In contrast, similar to women, approximately half of the effect of lower pension claiming results in a higher probability of continued work, while the other half leads to an increased likelihood of exiting employment, either by entering inactivity or transitioning to disability retirement. These results remain robust even after applying the Sidak correction for multiple hypothesis testing (Table A17 in Appendix A).

However, we did not observe any significant health effects and there was no clear health gradient in the impact of hospitalizations (Figure 7), unlike the case of women (Figure 5). The only significant effect found was related to hospitalizations for cardiovascular disease (CVD) in the most populated group (good health), but this significance vanishes with the Sidak

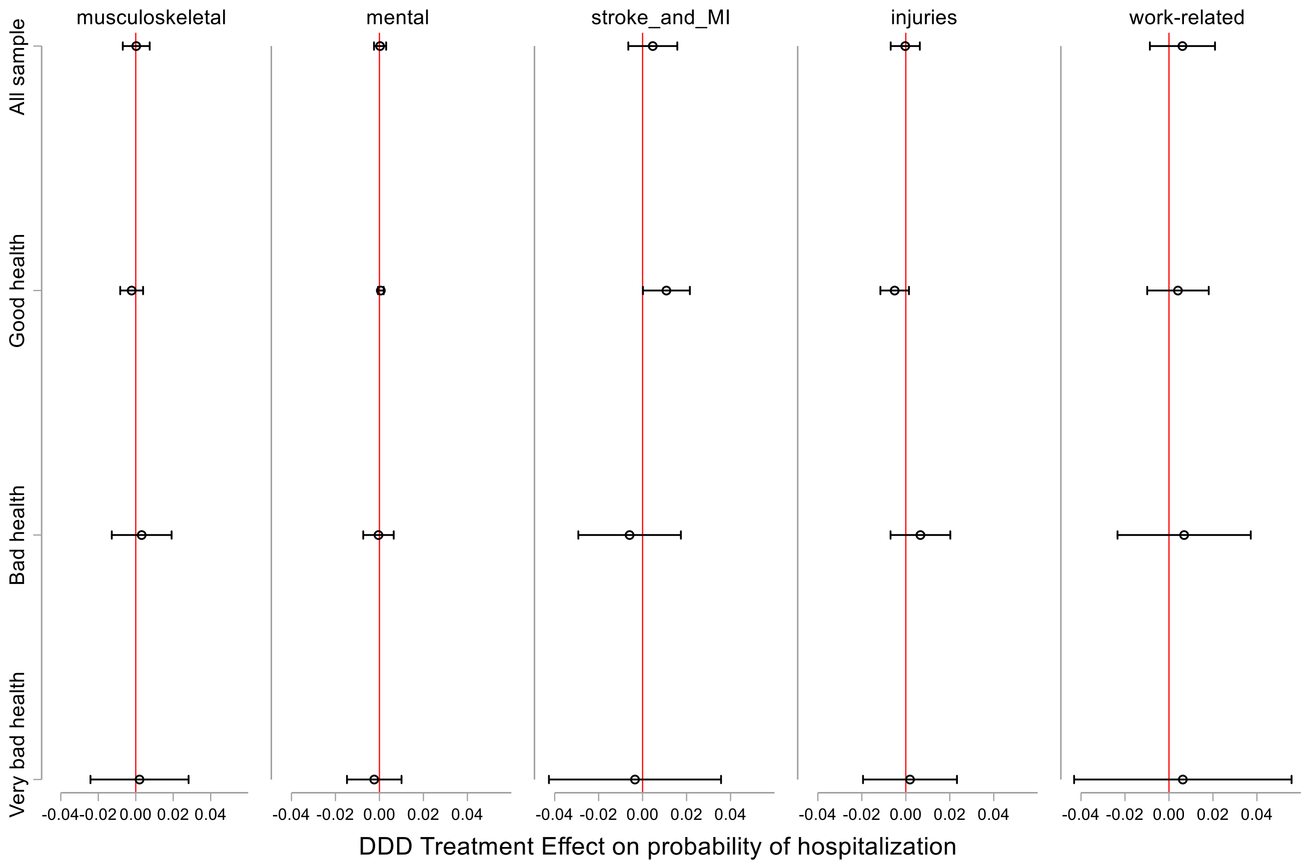

FIGURE 7 Effect of the pension reform on work-related hospitalizations by health status. Men.
Simple triple difference (DDD) model. This figure reports the results of a simple triple difference model: $y_{i,t} = \beta_0 + \beta_1 1947_i \times \text{post65} \times \text{below35}_i + \beta_3 1947_i \times \text{post65} + \beta_3 \text{post65} \times \text{below35}_i + \beta_4 \text{post65} + \alpha_i + \mu_{i,t}$, where 1947 equals one if born in 1947 and zero if born in 1945, post65 equals one if age 65 or older, below35 equals one if the man was expected to have a maximum of 35 years of contribution by age 65, and α_i are individual fixed effects. Each coefficient reports the estimate and the 95% Confidence Interval of the triple interaction ($1947_i \times \text{post65} \times \text{below35}_i$) from a different regression with a combination of outcomes (as labeled at the top of the graph) and samples (as labeled as the y axis). In particular, each graph from the first to the fourth report the effect of the reform on the probability by cause of hospitalization (musculoskeletal, mental, stroke and myocardial infarction [MI] or injuries). The graph at the right groups all hospitalizations into one category (i.e., reports the effect of the reform on the probability on any "work-related" hospitalization). "All Sample" include our main full sample of men. "Good health", "Bad health" and "Very bad health" include men who during the ages 60 to 62 had no hospitalization ($n = 3974$), one or more hospitalizations ($n = 2314$), two or more hospitalizations ($n = 1131$). Robust standard errors clustered at individual level in parenthesis ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. [Colour figure can be viewed at wileyonlinelibrary.com]

correction (Table A18 of Appendix A). Furthermore, we attempted to adapt the pooled Difference-in-Differences model using SHARE data (Specification 2) to examine the effects on self-reported health outcomes for men. However, although the coefficients for labor market outcomes showed the expected signs, the model did not yield statistically significant results (See Appendix C for an explanation of the model and the results). Therefore, we were unable to leverage this model to assess the effects on other health outcomes. This is likely due to the smaller increase in men pension age compared to women and the smaller sample size of the SHARE survey.

6 | DISCUSSION

This paper uses both administrative and survey data to estimate the impact of an Italian 2012 pension reform on employment and health outcomes of women and men whose pension eligibility age was increased by the reform. The reform increased abruptly the normal retirement age for women from 60 to 62 in 2012 and then progressively to 67 in 2019, while for men the increase was smaller (from 65 to 67) and more gradual (the 67 threshold to be reached by 2019). As a consequence, the reform suddenly restricted the access to old age pension for certain cohorts, resulting in a exogenous variation in access to pension between cohorts and over time/age that we exploit using several difference-in-differences strategies.

Regarding women, our results first show that the reform effectively decreased the probability of retirement by around 20–25 pp during the ages 61–63 for the affected cohort. Conversely, their probability of working increased by around 10–12 pp, whereas they also increased the probability of unemployment or inactivity by a similar magnitude. Additionally, we report a significant substitution effect toward 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 toward 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 substantial heterogeneous effects of the reform on labor 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 labor market through disability or non-employment. These results are consistent with previous studies showing that the labor market effects of pension reforms are stronger among disadvantaged workers (Ardito, 2021; Hanel & Riphahn, 2012; Mastrobuoni, 2009). 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 labor 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 analyzed. Our results show that women 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.

Among men, on the other hand, while we found a significant effect of the reform on labor market trajectories like women, although of smaller size, we did not find any significant effect on health. This may be expected as their rise in retirement age was much smaller and more gradual than that of women.

7 | CONCLUSIONS AND POLICY IMPLICATIONS

Summing up, our results show that the pension reform was successful in extending the working horizon for the affected workers, who on average postponed retirement and increased employment at older age. However, there were more evident side effects on women, who were those whose retirement age was more abruptly increased. Tightening the minimum pension age pushed a fraction of previously employed men and women out of the labor market, into unemployment, inactivity, and disability pensions. For women this was particularly salient for 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 higher sick-leave, and risk of hospitalizations due to mental health or injuries, concentrated among those with previously worse health. Despite the abrupt increase in retirement age, when analyzing survey SHARE data, we did not find any negative effects on the self-reported outcomes under study assessing physical, mental, and behavioral health, except from a significant 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 61–63. 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 women with previously worse health conditions may give policymakers a better hint of what could happen if the pension age continues to rise toward 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 differentiate 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 abrupt 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 target individuals not only based on their occupation but also on their health conditions to avoid that pension reform hit disproportionately more on the most vulnerable.

AUTHOR CONTRIBUTIONS

Manuel Serrano-Alarcón: Conceptualization; data curation; formal analysis; methodology; writing – original draft and revised manuscript. **Chiara Ardito:** Conceptualization; data curation; formal analysis; methodology; writing – original draft and revised manuscript. **Roberto Leombruni:** Conceptualization; data curation; writing – review and editing. **Alexander Kentikelenis:** Conceptualization; writing – review and editing. **Angelo d'Errico:** Conceptualization; writing – review and editing. **Giuseppe Costa:** Writing – review and editing. **Anna Odone:** Writing – review and editing. **David Stuckler:** Conceptualization; writing – review and editing.

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CONFLICT OF INTEREST STATEMENT

The authors declare that they have no competing interests.

DATA AVAILABILITY STATEMENT

This paper uses data from WHIP-Health, the Working Histories Italian Panel and SHARE, the Survey of Health, Ageing and Retirement in Europe. SHARE Waves 1, 2, 4, 5 and 6, were used (DOIs: 10.6103/SHARE.w1.800, 10.6103/SHARE.w2.800, 10.6103/SHARE.w4.800, 10.6103/SHARE.w5.800, 10.6103/SHARE.w6.800). See Börsch-Supan et al. (2013) for methodological details. Data are publicly available for registered users at www.share-project.org. WHIP-Health dataset is available from Italian Ministry of Health, but restrictions apply to the availability of these data, which were used under license for the current study, and so are not publicly available. The authors are available to guide through the procedures for accessing and to make available the computer programs used to generate the results.

DISCLAIMERS

The views and opinions expressed are only those of the authors and do not necessarily reflect those of the European Union or the European Commission. Neither the European Union nor the European Commission can be held responsible for them.

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ENDNOTES

[1] 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 (Frimmel & Pruckner, 2020; Shai, 2018).

[2] The 1-year waiting window rule was passed in 2010, and only implemented during 2011, as it was subsequently abolished by the 2012 pension reform.

[3] 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 64–67 (Table 2).

[4] The Maroni 2004 reform which planned to increase the ERA from 57 to 60 in 2008, and whose implementation was actually postponed by another reform in 2007, would have also similarly affected the 1951 and 1952 cohorts. See Angelini et al. (2009) for a comprehensive review of previous pension reforms.

[5] 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 53 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.

[6] Sick leaves lasting less than 1 week are not available in the data.

[7] Due to the nature of our labor market data, one could have thought to use a competing risk model instead. However, a classical difference in difference model allows us better to study the full labor market dynamics over time and relate them with health outcomes. On the contrary, with a competing risk model one could only examine the first exit from employment, but not the following events. Furthermore, it cannot be replicated with the health data since health outcomes are intrinsically not mutually exclusive.

[8] 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.

[9] 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.

[10] We restricted this analysis to women working in blue-collar occupations (4705 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.

[11] By looking at descriptive data (Figure A4 in Appendix A), cohort 1946 behaves very similarly to 1947 despite not being affected by the 2012 pension reform. They seem to postpone retirement to age 66 due to the waiting window.

[12] As for the case of women we could only retrieve information of the contribution years for the subsample of those who were retired by 2016 (6288 out of 10,160 men working at the age of 58 and not receiving any pension).

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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