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The Effect of Removing Early Retirement on Mortality

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The Effect of Removing Early Retirement on Mortality*

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Abstract

This paper studies the mortality effects of delaying retirement by leveraging the 1967 Spanish pension reform, which exogenously increased the earliest voluntary claiming age from 60 to 65 based on individuals' date of first contribution. Using Spanish administrative data, we find that removing access to early retirement delays age at last employment by 4 months and increases the probability of death between ages 60 and 69 by 11 percent. The mortality effects are concentrated among workers in physically demanding, high-psychosocial-burden, and low-skilled occupations, while men and women are affected similarly. Access to flexible retirement mitigates the adverse effects of delaying retirement.

JEL Codes: I10, I12, J14, J26
Keywords: Delaying retirement, Early retirement, Mortality, Heterogeneity, Work conditions

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1 Introduction

Many countries have reformed their public pension systems to cope with population aging and to maintain financial solvency. One common policy tool is to restrict access to early retirement schemes by increasing the minimum pension eligibility age. While there has been extensive literature studying the labor supply responses to such pension reforms,[1] there are relatively few studies about the impact of delaying retirement on mortality.[2] Moreover, the trend of unifying pension rules across occupations and sectors has led many OECD countries to phase out or tighten access to special early retirement schemes for workers in hazardous or arduous jobs (OECD, 2023). Understanding the mortality consequences of removing access to early retirement, therefore, has first-order policy relevance. This paper aims to fill this gap by providing causal evidence of the effect of removing early retirement schemes on mortality, offering insights to facilitate policy debates on the necessity of special pension provisions for hazardous or arduous work.

This paper provides novel empirical evidence on this important issue by investigating a Spanish pension reform enacted in 1967. This reform exogenously changed the early retirement age depending on the date individuals began contributing to the Social Security system. Individuals who began contributing to the pension system before 1967 could voluntarily claim a pension from age 60. In contrast, individuals who began contributing in or after 1967 could only voluntarily claim a pension at age 65.[3]

This reform offers two key advantages for addressing the research question. First, the early retirement age changes discontinuously based on the year in which individuals began contributing to the Social Security system, enabling us to credibly identify causal effects. Second, unlike previous studies that often focus on specific groups such as military personnel or civil servants (see, e.g., Hallberg et al. (2015); Bloemen et al. (2017); Hagen (2018)), this reform affects a broader and more general population. This feature allows us to capture mortality responses in the general population and examine the heterogeneous responses across subgroups.

We use a novel version of Spanish administrative Social Security panel data covering 10% of individuals born between 1940 and 1949 who were registered with the Social Security system at

[1] For example, see Coile and Gruber (2007), García-Pérez et al. (2013), Atalay and Barrett (2015), Manoli and Weber (2016), Blundell et al. (2016) and Geyer and Welteke (2021) for recent evidence on the direct effects of raising retirement ages.

[2] Existing empirical evidence largely draws lessons from policy experiments that allow for earlier retirement (Coe and Lindeboom, 2008; Hernaes et al., 2013; Hallberg et al., 2015; Bloemen et al., 2017; Kuhn et al., 2020). The mortality effects of advancing versus postponing retirement need not be symmetric because, for instance, individuals affected by reforms allowing early retirement may have different characteristics from those affected by reforms postponing retirement. Consequently, estimates from previous literature may not generalize to today's policy environment, in which most reforms seek to extend working lives.

[3] Individuals of certain cohorts could retire at age 61 through involuntary early retirement under certain conditions. See Section 2 for more details on the institutional setting.

any point in time until September 2023. This data contains both work biographies and detailed time-invariant information, including the exact date of death. We compare individuals who began contributing in 1966 with those who began contributing in 1967. Using a within-cohort fixed-effects model, we find that the reform delays the age of last employment by about four months (0.328 years). Those who began contributing in 1967 are also less likely to claim a regular pension and more likely to claim partial and occupational/partial disability pensions. This indicates that individuals resort to alternative pathways to exit the labor market earlier when early retirement schemes are not available.

To assess the causal validity of our estimates, we conduct placebo analyses by assigning alternative start years as placebo treatment years. We find no significant impacts on retirement decisions and mortality outcomes. To rule out the possibility of selection into treatment, we also show that treatment status has no effect on a set of predetermined early-career covariates, including individuals' first occupations and labor market outcomes in their 30s. Lastly, our estimates are robust to a series of robustness tests, including the use of age-at-first-contribution fixed effects to account for the potential impact of education on mortality, as well as variations in sample restrictions and control variables. These tests help rule out the possibility that our reduced-form estimates are driven by other confounding factors.

What is the impact of removing early retirement on mortality? We find that individuals who first contributed in 1967 have a 0.9 percentage point ($\sim 4.6\%$) higher probability of dying between the ages of 50 and 74.[4] When examining the reform's effect on mortality across different age brackets, we observe that the strongest responses occur between the ages of 60 and 69, when public pensions are either not yet accessible or only recently available. Removing the early retirement option increases mortality in this age bracket by 0.9 percentage points ($\sim 11.3\%$). We also report the impact of delaying the age at last employment by one year on mortality, using an instrumental variable (IV) approach. Using the year individuals began contributing as an instrument for age at last employment, we find that delaying labor market exit by one year increases the probability of dying between the ages of 60 and 69 by 2.5 percentage points (a relative increase of 32%). Compared with the reduced-form estimates, the IV results are more than doubled, consistent with the reduced-form estimate of a four-month increase in the age of last employment. This suggests that the adverse mortality effect of losing access to early retirement is primarily driven by the postponement of labor market exit.

To understand whether the reform has differential impacts across subgroups, we examine heterogeneity by labor-market conditions before retirement, eligibility for flexible retirement, and gender.

[4]Because the dataset ends in 2023, deaths for the 1940–1949 cohorts are observed only through age 74; later deaths are right-censored. We therefore measure mortality over ages 50–74 and report age at death censored at 74.

First, we show that the characteristics of individuals' last job play a significant role. Because jobs are multi-dimensional, we examine three aspects: physical burden (proxied by industry workplace-accident incidence), psychosocial burden (based on Job Exposure Matrices from Kroll, 2011), and occupational skill level. Mortality effects are larger in high-accident and high-psychosocial-burden sectors, and among low-skilled workers, even though these groups, in fact, extend their working lives to a lesser extent. This indicates that individuals in more demanding and strenuous jobs exhibit a revealed preference for earlier retirement. Our finding that these groups experience a larger increase in mortality due to the reform suggests that removing access to early retirement may exacerbate social disparities in mortality and reduce the progressivity of the pension system, as poor working conditions often coincide with low wages (OECD, 2019). If policymakers aim to close the widening gap in life expectancy across income groups (Tarkiainen et al., 2012), they should be aware of the distributional consequences of similar pension reforms. Our findings also suggest that advocating for different pension provisions based on working conditions could serve as an effective policy measure.

Furthermore, we consider the role of flexible and gradual retirement, which may mitigate the adverse effects of delayed labor market exit. In Spain, workers can opt for partial retirement, which allows them to work part-time while claiming a partial pension, provided they have contributed to the Social Security system for at least 33 years.[5] Comparing workers around this eligibility threshold, we find that those lacking access to partial retirement experience a large increase in mortality for each additional year of delayed retirement ($\sim$ 44%), whereas the effect for those with potential access is close to zero and not statistically significant. This finding underscores the importance of offering gradual retirement options to mitigate the detrimental mortality impacts of delayed retirement. It also contributes to current policy discussions on flexible retirement and reduced working hours for older workers.

Lastly, we show that the reform affects men and women in similar ways. Both women and men delay labor market exit by approximately four months (4.2 months for women; 3.5 months for men), and the probability of claiming a regular pension decreases by about 4 percentage points ($\sim$ 5–6%). The increase in mortality between ages 60 and 69 is also similar for both groups. After age 70, however, the effect fades for women but persists for men. This pattern is likely due to women's deaths after age 70 shifting more toward cerebrovascular and neurodegenerative causes, which are less linked to work-related exposures, whereas men's mortality remains more cardiometabolic, allowing the effect to persist.

Beyond contributing to studies on the impact of pension reforms on retirement decisions (e.g.,

[5] Eligibility for partial retirement additionally requires at least six years of tenure with the same employer and the employer's consent.

Mastrobuoni (2009); García-Gómez et al. (2012); Manoli and Weber (2016); Geyer and Welteke (2021)), our paper also relates to and complements research examining the mortality effects of retirement. The existing well-identified empirical literature finds mixed results and explores three types of policy experiments: allowing earlier retirement (Coe and Lindeboom, 2008; Hernaes et al., 2013; Hallberg et al., 2015; Bloemen et al., 2017; Kuhn et al., 2020), promoting later retirement (Zulkarnain and Rutledge, 2018; Hagen, 2018; Bozio et al., 2021) and switching to retirement at the statutory retirement age (Fitzpatrick and Moore, 2018).[6]

Studies on the impact of earlier retirement find either no significant effects on mortality or a reduction in mortality. For example, Hernaes et al. (2013) find that accessing a pension two to five years earlier has no effect on the probability of dying by ages 67, 70, 74, and 77 for the entire population of Norway. Evidence from specific population groups shows a decrease in mortality due to earlier retirement. For example, Hallberg et al. (2015) show that five-year early access to a retirement pension reduces mortality among male army officers in Sweden. Bloemen et al. (2017) find that male civil servants in the Netherlands who are entitled to claim a pension around eight years earlier have a lower mortality rate. The only study we are aware of that finds increased mortality due to earlier pension access is Kuhn et al. (2020). Using Austrian register data, they estimate the (very) short-run effect of advancing eligibility by three years and document a rise in male mortality before age 67. This is likely driven by the scarring effects of unemployment.

Evidence on the impacts of later retirement is scarcer. Our paper directly contributes to this smaller body of literature by providing a precise estimate of the effect of later retirement on mortality resulting from the elimination of early retirement access. To the best of our knowledge, only four studies examine the effect of delayed retirement on mortality. While Hagen (2018) studies the mortality effect of a two-year increase in the statutory retirement age, they find an imprecisely measured no effect on mortality by age 69.[7] The other three papers are Zulkarnain and Rutledge (2018), Bozio et al. (2021) and Saporta-Eksten et al. (2021). While these studies provide precise estimates of the effects of later retirement on mortality, they explore pension reforms that change early retirement financial incentives, rather than eliminating early retirement access altogether, as in our setting. Zulkarnain and Rutledge (2018) find that delaying retirement reduces the probability of dying within five years for men aged 62 to 65 by 32%, based on a tax reduction policy

[6] The empirical evidence on the mortality effects of switching to retirement is mixed. While Fitzpatrick and Moore (2018) find an immediate and sharp increase in mortality rates when becoming eligible for early retirement at age 62 in the U.S., both Nielsen (2019) and Rose (2020) find no impacts on mortality, instead noting improvements in health after switching to retirement. Specifically, Nielsen (2019) finds that switching to early retirement at age 60 leads to a reduction in hospitalizations in Denmark. Similarly, Rose (2020) shows an improvement in reported health when switching to retirement at age 65 in England.

[7] Hagen (2018) examines the mortality effect of a two-year increase in the statutory retirement age for female local government workers in Sweden and finds that the reform had no impact on mortality and/or health care utilization. See Section 5 for a more detailed comparison with the existing literature.

for continued employment at older ages in the Netherlands. Bozio et al. (2021) find that delaying retirement in France has no effect on the probability of dying between the ages of 61 and 79 by examining a pension reform that changed a variety of parameters of the pension system. Saporta-Eksten et al. (2021) explore an exogenous reduction in the implicit tax on continued work in Israel and find that later retirement increases mortality between the ages of 75 and 85, but has no effect on mortality between the ages of 65 and 74. Our paper differs from Zulkarnain and Rutledge (2018), Bozio et al. (2021) and Saporta-Eksten et al. (2021), as we expect the behavioral response to pension reforms that rely on financial incentives to differ from that under reforms that shut down early retirement schemes.[8]

When examining the literature on the health effects of delayed retirement, it is not surprising to find suggestions that delayed retirement may reduce life expectancy. Many studies on the health impacts of delayed retirement find retirement is beneficial (e.g., Grip et al. (2012); Shai (2018); Salvati (2020); see also Pilipiec et al. (2021) for a recent review.) through relief from work-related stress, increased sleep, and greater engagement in physical activity (Atalay and Barrett, 2014; Eibich, 2015; Barschkett et al., 2022). Studies also find positive effects of retirement on health outcomes, driven by the adoption of healthier lifestyles (Insler, 2014; Celidoni and Rebba, 2017; Gorry et al., 2018). Therefore, it is reasonable to expect that delaying retirement could increase mortality risk.

The remainder of the paper is organized as follows. Section 2 describes the Spanish pension system and the 1967 reform. Section 3 outlines the data and empirical strategy. Section 4 presents descriptive evidence, reduced-form impacts on retirement behavior and mortality outcomes, and the effect of age at last employment on mortality using the IV method. Section 5 discusses the implications, and Section 6 concludes.

2 Institutional Setting

The key elements of the current Spanish pension system were established in 1967, with subsequent reforms in 1985 and 1997 establishing the foundational rules applicable to all individuals in our sample, regardless of the year they began contributing. See Appendix B for further details on the various reforms the Spanish pension system has undergone since 1967.

Spain's current old-age pension system operates on a pay-as-you-go basis, with an average re-

[8] The behavioral response to reforms that incentivize later retirement through financial benefits is expected to differ from those that eliminate early retirement options. Reforms that change financial incentives for later retirement allow individuals to make gradual adjustments in retirement timing. Under such reforms, claiming a pension at age 60 remains possible, though less financially attractive. In contrast, eliminating early retirement options may be perceived as punitive, as it delays the earliest age at which individuals can access a pension, making retirement at age 60 no longer feasible.

placement rate of approximately 80% (one of the highest in the European Union). For the sample of individuals studied, the statutory retirement age is 65, and a minimum of 15 years of contributions is required to qualify for a pension.[9] Full pension benefits are granted to individuals with at least 35 years of contributions, while shorter contribution histories result in a 2 percent penalty for each missing year. Pension amounts are determined based on average contributions during the 15 years preceding the claim.

The retirement rules described above apply to all individuals in our sample, except for the earliest age of pension eligibility. Individuals who began contributing before or after 1 January 1967 faced different early retirement ages. Specifically, those who began contributing before 1967 retained a permanent right to claim early retirement at age 60.[10] These workers retained this right because they began contributing before the current old-age pension system was established in 1967.[11] The law consolidated multiple pre-existing insurance schemes (Mutual Societies) into a unified Social Security system, only preserving the voluntary retirement entitlement for earlier contributors. These individuals could retire voluntarily and claim early retirement from age 60, though with some financial penalties. Among individuals who began contributing in 1966, around 22% claimed a regular pension at age 60.

On the other hand, individuals who began contributing after 1 January 1967 could only claim a pension voluntarily at age 65, the statutory retirement age. Since 2002, involuntary early retirement has been available at age 61 under restrictive conditions, including involuntary unemployment for a minimum of six months and at least 33 years of contributions. However, due to these stringent requirements, only a small proportion of individuals opted for this pathway. The financial penalty for early claiming was the same regardless of when individuals began contributing. Before 2002, the financial penalty was 8 percent for each year of early claiming. Following the 2002 reform, the penalty became dependent on years of contributions and could be as low as 6 percent per year.

Although the foundational regulation for the new Social Security system was published on 21 April 1966, the detailed guidelines for its implementation (including the 1 January 1967 cutoff date for the initial Social Security contribution) were issued on 18 January 1967,[12] eliminating the possibility of manipulating the date of the first Social Security contribution. Consequently,

[9]Initially, only eight years of contributions were required, but this was gradually increased to 15 years following the 1997 reform. Importantly, this incremental adjustment uniformly affected all individuals in our sample, regardless of when they began contributing.

[10]The January 1967 cutoff was set at a later date for workers in specific regimes, such as the mining and maritime sectors, which represent only a small share of our sample. For these sectors, our analysis compares individuals whose contributions began within 12 months of the later, industry-specific dates rather than the general 1967 threshold.

[11]The 1967 Social Security Law came into effect on 1 January 1967 under the Franco regime. It unified and integrated various social insurance schemes. After Franco's death in 1975, the democratic government took over the management of the old-age pension system.

[12]See the official legal document on the Spanish Ministry of Labor website here.

manipulation of the contribution start date is not feasible, allowing us to causally estimate the reform's impact by comparing individuals who began contributing before and after 1 January 1967. As illustrated in Figure 1, individuals who began contributing before 1967 (regardless of their birth year) could voluntarily claim early retirement at age 60. For those who began contributing after 1967, the only way to access early retirement was through involuntary early retirement at age 61 (only available since 2002); otherwise, the earliest age at which an individual can voluntarily claim a pension is 65. Therefore, we expect individuals who began contributing after 1967 to delay labor market exit considerably.

Two alternative pathways exist for exiting the labor market earlier: disability and partial retirement pensions. Disability pensions have been widely used in Spain as an early retirement scheme (Boldrin et al., 1999; García-Gómez et al., 2012). Additionally, since 2002, partial retirement has allowed individuals to combine old-age pension income with employment earnings. Under this scheme, workers aged 60 or older with at least 33 years of contributions and six years of tenure with the same employer could claim up to 85% of their pension,[13] while continuing to work at least 15% of their usual hours. In later sections, we investigate the impact of the reform on the probabilities of individuals choosing these alternative labor market exit routes.

3 Data and Empirical Methods

3.1 Data

This paper uses novel administrative data of an extended sample of the Continuous Sample of Working Histories (*Muestra Continua de Vidas Laborales* (MCVL)) provided by the Spanish Social Security Administration. The dataset contains a 10% random sample of individuals born between 1940 and 1949[14] who have been registered with the Social Security system (either as contributors or as pension recipients) at any point in their lives up to September 2023.[15]

We use a non-public version of the MCVL, which enables us to observe contributory workers and pensioners prior to 2005 (the earliest available date in the publicly accessible version of the dataset). This data advantage allows us to study a representative sample of workers affiliated with the Spanish Social Security system at any point during their working lives and to examine their

[13]This maximum share was reduced to 75% following the 2011 reform.

[14]These cohorts are likely the most affected by the reform: individuals born in these years were 18 to 27 years old in 1967, placing them at the typical ages for labor market entry.

[15]Monthly contributions and death dates are reliably recorded from 1981 onward, which does not limit our analysis, as even the oldest individuals in our sample (born in 1940) were only 41 years old by then. All other key variables are consistently documented throughout our observation period, except for the year of initial contribution, which we address in Section 4.1.

mortality outcomes. Further details on the dataset novelty and access procedures are provided in Appendix C.

The MCVL includes time-invariant information such as gender, birth month, and birth year. It also contains detailed labor market biographies spanning from the date individuals began contributing to the Social Security system until their death.[16] Moreover, the dataset records detailed lifetime employment and unemployment spells, occupations, industry, and monthly contributions up to 2023. The pension records in the MCVL provide accurate information on individuals' age at the time of pension claiming, pension benefit amounts, the type of pension received at each point in time, and the total number of contribution years at retirement. When individuals exit the dataset due to death, we observe their exact date of death, which allows us to measure mortality with precision.

Sample Our main sample includes Spanish individuals born between 1940 and 1949 who began contributing to the Social Security system within 12 months before or after 1 January 1967 and maintained affiliation with the system beyond age 50, either through employment, unemployment, or receipt of any other Social Security benefit. The age at first contribution ranges from 17 to 27, with a mean of 23.[17] We further restrict our sample to individuals with sustained labor market attachment by including only those who had not claimed a disability pension before age 50. We also exclude individuals who had claimed a SOVI pension. A SOVI pension is a residual pension from the old system, granted to individuals who, at age 65, are not entitled to a pension under the current Social Security system but can demonstrate at least 1,800 days of contributions to the pre-1967 system. These individuals were not eligible for early retirement, even though they began contributing before 1967. These restrictions result in the exclusion of approximately 4.5% of observations.

In Table A1, we verify that sample selection does not differ between individuals whose first contribution occurred before or after 1967. First, we examine whether the reform affects the probability of exclusion from the sample and find no significant impact. Moreover, we show no significant impact on mortality across different age brackets among those excluded from the main sample. In Table A2, we conduct robustness checks by modifying the definition of individuals not considered attached to the labor market. We show that including individuals who claimed disability before age 50 or received a SOVI pension does not affect our results. The final sample contains 40,737

[16]Note that the date individuals began contributing to the Social Security system coincides with the start of their first formal job. However, for some individuals, particularly those who transitioned from the informal to the formal sector, this date may not reflect their actual labor market entry.

[17]Note that the age at first contribution does not necessarily coincide with the age at first employment, as individuals may have been working informally before beginning contributions to the Social Security system.

individuals, of whom 23.26% are female.[18]

3.2 Empirical Strategy

Our empirical analysis proceeds in two steps. First, we estimate the reduced-form effect of the reform on retirement and mortality outcomes using a within-cohort OLS framework with fixed effects and controls. Second, we assess the causal effect of age at last employment on mortality using an instrumental variables (IV) approach that exploits exogenous variation in age at last employment induced by the reform. The main objective of the IV analysis is to compare effect magnitudes across subgroups.

3.2.1 Reduced-form Reform Impact: Within-cohort Fixed-Effect Model

We estimate the reduced-form effect of the reform on a range of labor supply and retirement outcomes (for example, age of retirement and last employment, the probability and the age at which individuals claim the different pensions) as well as on mortality outcomes, using the following specifications:

$$R_{icm} = \alpha_0 + \alpha_1 \delta_c + \alpha_2 \mu_m + \alpha_3 Treated_i + \gamma^R X_{icm} + U^R_{icm} \tag{1}$$

$$M_{icm} = \beta_0 + \beta_1 \delta_c + \beta_2 \mu_m + \beta_3 Treated_i + \gamma^M X_{icm} + U^M_{icm} \tag{2}$$

R_{icm} denotes the labor supply outcomes, and M_{icm} denotes the mortality outcomes of individual *i*, born in year *c* and month *m*. $Treated_i$ is a dummy variable equal to one for individuals who began contributing to the Social Security system in 1967, and zero for those who started in 1966. Treated individuals can claim regular pensions voluntarily at age 65 (involuntarily at 61), while control individuals are eligible to claim as early as age 60. δ_c is the year-of-birth, and μ_m is the month-of-birth fixed effects. X_{icm} includes gender, a list of fixed effects, such as the individual's first occupation and industry sector, and a list of other predetermined covariates, including individuals' mean monthly contribution, the fraction of days active and employed, and the fraction of time self-employed (all measured between ages 30 and 40).[19] We cluster the standard errors at the birth-year level, and report the wild-bootstrap p-values in brackets throughout. The coefficients α_3 and β_3 capture the average treatment effects of the reform on labor supply and mortality, respectively.

[18] The lower number of women in the sample is primarily due to the fact that relatively few women began contributing to the Social Security system in 1966 and 1967, which reflects the low female labor market force participation during this period.

[19] We measure these variables during the initial years following Social Security affiliation, as the vast majority of individuals are active and employed at the time of their first registration.

Causality. The key identifying assumption for estimating the causal impact of the pension reform is that the year in which individuals began contributing to the Social Security system is independent of unobserved characteristics that affect labor supply and mortality. We employ several strategies to support this identifying assumption.

First, we restricted the sample to individuals who began contributing in 1966 and 1967. Treated and control individuals faced similar labor market conditions at labor market entry: they were born in the same year and began working only one year apart. Second, we include fixed effects for first occupation, first industry, birth year, and birth month, which allows us to estimate variation within occupation, industry, and birth cohorts.

Third, we examine whether treated and control individuals display similar characteristics at the beginning of their careers. Table A3 reports the impact of the reform on a set of predetermined variables, including the fraction of time spent in activity, employment, and self-employment, as well as average monthly contributions, all measured between ages 30 and 40. Regression estimates indicate that treated and control individuals do not differ in their labor market activities during their 30s. The only exception is a small difference in self-employment rates. We also examine whether the probability of working in a low-skilled occupation or in different industry sectors at the time of first Social Security contribution is balanced across groups. Although treated individuals are slightly more likely to begin their careers in the construction sector, the difference is economically negligible. In addition, the p-value from the joint significance test of all covariates is 0.336.[20] These findings suggest that there is no manipulation of treatment status and that the control and treatment groups are very similar.

Finally, to further strengthen the causal interpretation of the results, we conduct placebo tests using alternative years to define treatment status, along with a battery of robustness tests, including the use of age-at-first-contribution fixed effect instead of birth-year fixed effects, as detailed in Section 4.2. These tests help rule out the possibility that confounding factors are driving our reduced-form estimates.

3.2.2 The Effect of Age at Last Employment on Mortality: Instrumental Variable Method

To quantify the reform's impact on mortality channeled through the age at last employment and to enable subgroup comparisons, we estimate the effect of age at last employment on mortality using an Instrumental Variable (IV) approach. Leveraging the reform-induced exogenous variation in age at last employment, we estimate the following equation:

[20] In Section 4.4, we show that the effect of the reform on our main outcomes is robust to the exclusion of individuals in self-employed pension regimes.

$$M_{icm} = \theta_0 + \theta_1 \delta_c + \theta_2 \mu_m + \theta_3 \widehat{R^a_{icm}} + \gamma^{IV} X_{icm} + \eta_{icm} \quad (3)$$

We include the same set of controls as in Equations 1 and 2 (δ_c, μ_m, and X_{icm}). $\widehat{R^a_{icm}}$ denotes the predicted value of age at last employment obtained from Equation 1. The coefficient θ_3 captures the local average treatment effect (LATE) of age at last employment on mortality for compliers (individuals who delayed their retirement because they were no longer eligible to claim a regular pension at age 60).

Assumptions. Three conditions must hold to interpret the IV estimate as the causal effect of delayed retirement on mortality. First, treatment status must be strongly correlated with age at last employment. We demonstrate the strength and validity of the first-stage relationship in Section 4.2.

Second, treatment status affects mortality solely through its impact on age at last employment. The exclusion restriction could be violated if contributing in 1967 affects mortality through channels other than retirement age. Two possible additional channels are changes in pension benefits and shifts in labor market outcomes close to retirement. We argue that the exclusion restriction assumption is reasonably met by showing that controlling for a proxy of individuals' pension benefits or their labor market decisions before retirement (between ages 45 and 55) does not change the magnitude of our IV estimates to any great extent. Further details are provided in Section 4.3.

Third, the monotonicity condition requires that beginning Social Security contributions in 1967 rather than 1966 always induce people to delay labor market exit or at least maintain the same age at last employment. Given the nature of the pension reform, this condition is likely to be satisfied. Moreover, Figure A1 presents the cumulative distribution function (CDF) of the age at last employment for individuals who began contributing in 1966 and 1967. We can see that the distribution for the treatment group stochastically dominates that of the control group. Furthermore, Tables 4 to 5 further confirm that the effect of the reform on age at last employment is non-negative across all subgroups. Taken together, this evidence supports the plausibility of the monotonicity assumption in our context.

4 Results

4.1 Descriptive Evidence

We begin by documenting descriptively whether the 1967 reform was binding and effectively delayed labor market exit. Figure 2 plots the distribution of age at last employment for individuals whose first contribution was before 1967 (solid red line) versus in 1967 or later (dashed green line). The figure reveals a marked shift in retirement behavior following the reform. Among individuals who began contributing before 1967, 14% exited the labor market at age 60, compared to a substantially lower share among those who began contributing after 1967. Conversely, nearly 30% of those who began contributing in 1967 retired at age 65, compared with 25% among those who contributed before 1967. A similar pattern emerges for the age of regular pension claiming: 25% of those who began contributing before 1967 claimed at age 60 and 37% at age 65, with a non-negligible share claiming between ages 61 and 64. In contrast, among those who began contributing after 1967, fewer than 12% claimed before age 65, while nearly 53% claimed precisely at age 65. These patterns provide visual evidence that the reform was binding and led to delayed labor market exit and pension claiming. It is worth noting that some individuals recorded as beginning contributions after 1967 are observed retiring at age 60, which was legally impossible. This likely reflects misreporting of the first contribution date in the administrative records. Such errors are plausibly random and unrelated to individual characteristics. Accordingly, we do not correct for these cases in our main specification, as these errors would bias our estimates toward zero, yielding conservative results. In Section 4.4.2, we implement a correction for these administrative errors and find that the estimated effect of age at last employment on mortality remains similar, further supporting the assumption that the misreporting is random.

Focusing on our baseline estimation sample (individuals who began contributing in 1966 or in 1967), Table A4 shows that individuals who started contributing in 1966 (control group) retire, on average, at age 60.89 and claim a regular pension at age 62.87. For individuals who begin contributing in 1967 (treatment group), these averages increase to 61.20 and 63.25, respectively, representing delays of approximately 0.31 and 0.38 years.

The magnitude of these differences is moderated by the availability of alternative exit routes from the labor market. In addition to regular old-age pensions, individuals may claim disability or partial pensions. Table A4 reports that 70% of the sample claim a regular old-age pension, 14% claim a disability pension, and 7% claim a partial pension. The remaining 6% do not claim a pension due to prolonged inactivity, death prior to eligibility, or continued activity as of 2023. Compared to the control group, individuals who began contributing in 1967 are less likely to claim a regular old-age pension and more likely to claim a disability pension, a partial pension, or no pension at all. This shift in claiming behavior suggests that some treated individuals sought to exit the labor market earlier by pursuing alternative pension pathways.

Finally, Table A4 also reports the mortality hazards for our estimation sample. Conditional on survival to age 50, 20% of the sample died between ages 50 and 74. The hazard rate of dying between ages 50 and 59 years is relatively low, at 3%. The highest mortality rates occur between ages 60 and 74. The hazards of death between ages 60 and 69 and 70 and 74 are both 8%. We also observe that individuals who began contributing in 1967 exhibit a higher hazard of death between ages 60 and 69 compared to those who began contributing in 1966, a pattern we quantify in the reduced-form and IV analyses below.

4.2 The Reform Effect on Retirement Outcomes

In this section, we present the reduced-form effects of the 1967 reform on retirement behavior, comparing individuals who first contributed in 1966 and 1967.

Column 1 of Table 1 shows that individuals who began contributing in 1967 delay their labor market exit by nearly four months. Panel A of Table A5 further unpacks the reform's impact on the probability of exiting the labor market at different ages. The reform has no impact on the probability of exiting the labor market between ages 50 and 54. However, we see a decline in the probability of exiting the labor market between ages 55 and 59 by 2.5 percentage points ($\sim$ 9%), at age 60 by 1.5 percentage points ($\sim$ 13%), and at age 63 by 1 percentage point ($\sim$ 14%). The most pronounced shift occurs at age 65, where the probability of exit increases by 6.5 percentage points ($\sim$ 29.5%). These patterns indicate binding compliance with the higher statutory retirement age.

Turning to exit pathways, Columns 2 to 4 of Table 1 document the reform's impact on the types of pensions claimed. Individuals who began contributing in 1967 are less likely to claim a regular pension by 3.7 percentage points ($\sim$ 5%), while the probability of claiming a partial pension increases by 2.6 percentage points ($\sim$ 40%). Although overall disability claiming remains unchanged, Panel A of Table A6 shows that partial or occupational disability claims increase by 0.5 percentage points ($\sim$ 6.4%).[21] Column 5 of Table 1 indicates a small increase in exits without any pension among individuals who lose access to early retirement (0.4 percentage points, $\sim$ 6%). Panel B of Table A6[22] suggests that the increase is mainly accounted for by deaths prior to claiming a pension, which has increased by 0.3 percentage points ($\sim$ 8%). However, this estimate is not statistically significant. This pattern is consistent with premature mortality contributing to the increase in exits without a pension and aligns with our mortality findings. These findings indicate that individuals did not fully comply with the rise in statutory retirement age and instead utilized alternative routes to exit the labor market before claiming a regular pension, either by claiming

[21] There are four types of disability pensions. First, partial disability applies to individuals whose functional capacity has fallen by at least 33 percent; these individuals may continue working, including in the same jobs as before applying. Second, occupational disability is assigned to those workers who cannot resume their current occupation but can perform a different one. Third, absolute disability is intended for individuals who cannot carry out any type of work due to physical or mental deterioration. Finally, severe disability applies when a worker needs assistance from another person to perform basic daily tasks.

[22] In this table, we explore three reasons why individuals do not claim any pension: first, the individuals were still working in 2023; second, they became inactive; third, they died before claiming any pension.

partial/occupational disability benefits or by opting for a partial pension.

Panel C of Table A6 shows that the age at which individuals claim their first pension increases by approximately three and a half months, while the age at claiming a regular pension rises by about five and a half months. The ages at which individuals claim disability and partial pensions are also affected. Individuals who began contributing in 1967 delay claiming disability by around three and a half months but claim partial pensions earlier, by roughly two months. Panel B of Table A5 shows that most of the delay in pension claiming occurs between ages 60 and 64, consistent with the elimination of early retirement for individuals who began contributing after 1967. Because the reform alters the composition of pension types claimed, type-specific claiming ages should be interpreted with caution. Nevertheless, these patterns help identify the adjustment margins underlying the observed increase in the age at labor market exit.

Finally, we examine whether the reform affected labor market outcomes in the years leading up to retirement. Table A6 reports the effects on labor market outcomes for individuals between ages 45 and 55. We do not observe any significant differences in the fraction of individuals active, employed, or self-employed between those who began contributing in 1966 and those in 1967. However, individuals who began contributing in 1967 exhibit slightly lower monthly contributions (by approximately 59 €, which is roughly a 1.6% decrease) and are 3.8 percentage points more likely to hold a low-skill occupation. These late-career shifts toward a lower contribution base and lower-skill occupations are modest in magnitude and suggest only modest changes in earnings and coverage trajectories.

Consistent with these muted adjustments between ages 45 and 55, effects on pension benefits are also limited. Although delayed pension claiming should mechanically increase benefits by reducing early-claiming penalties, Table A6 shows a slight decline in average monthly pensions of 19 € (about 1.6%) for individuals who began contributing in 1967. This reduction is driven by a 43 € (about 3%) decrease in the base component, partially offset by a 1 percentage point increase in the adjustment factor, consistent with fewer penalties from delayed claiming. Overall, the modest decline in late-career earnings slightly outweighs the financial gains from postponing retirement, resulting in a small reduction in realized pensions and a pattern consistent with the previous documented substitution into partial or disability pathways.

All in all, we find that the reform delayed labor market exit by about four months and induced pathway substitution away from regular pensions toward partial and partial/occupational disability pensions. In the next section, we analyze whether, and for whom, this delay affected mortality.

4.3 Removing Early Retirement and Mortality

4.3.1 The Reform Effect on Mortality

We now examine how the elimination of access to early retirement affects mortality and age at death. The upper panel of Table 2 reports the reduced-form effects of the reform on mortality across different age brackets (conditional on having survived until that age). Individuals who first contributed in 1967 exhibit a 0.9 percentage point ($\sim 4.6\%$) higher probability of dying between ages 50 and 74 compared to those who first contributed in 1966.[23] The increase is primarily driven by higher mortality between ages 60 and 69, where individuals who first contributed in 1967 have a 0.9 percentage points ($\sim 11\%$) higher probability of death. Column (5) of Table 2 examines the effect of the reform on age at death. Age of death is top-coded at 74 for individuals still alive at that age (as the 1949 cohort turns 74 in 2023). This measure captures both the extensive margin (the effect on premature death) and the intensive margin (length of life). We find that the reform reduced age at death by almost one and a half months.

We also zoom in to examine the impact of the reform on mortality across five-year age intervals in Table 3. We find that mortality responses are concentrated between ages 60 and 64, when public pensions are not accessible, and ages 65 to 69, immediately following the statutory retirement age. Removing the early retirement option increases mortality between ages 60 and 64 by 0.4 percentage points ($\sim 10\%$) and by 0.7 percentage points ($\sim 13\%$) between ages 65 and 69. These findings are consistent with the observed increases in the probability of dying before different age thresholds, with the largest effects occurring before age 70 (Table A7). Taken together, these results indicate that the adverse mortality effects are concentrated in the short run, around the conventional retirement threshold, consistent with the loss of early retirement options.

We corroborate these findings in Section 4.4 using placebo cutoffs, which yield estimates close to zero, and alternative specifications that produce effects of similar magnitudes, thereby supporting a causal interpretation of the reduced-form results.

4.3.2 The Effect of Age at Last Employment on Mortality

To compare magnitudes across groups, we also quantify the impact of a one-year delay in age at last employment on mortality. We use an instrumental-variables (IV) strategy, using the reform as an instrument. Simple ordinary least squares (OLS) estimates suggest a negative correlation between later labor market exit and mortality, consistent with the notion that less healthy workers tend to retire earlier. In contrast, the IV estimates in the bottom panel of Table 2 indicate that a

[23] We follow ten birth cohorts (1940–1949) through 2023 and thus observe mortality from age 50 up to at least age 74 for all cohorts.

one-year increase in age at last employment raises the probability of dying between ages 50 and 74 by 2.9 percentage points ($\sim$ 14%). Mirroring the reduced-form results, the effects concentrate between the ages 60 and 69: a one-year delay in labor market exit increases the probability of death by 2.5 percentage points ($\sim$ 32%). Moreover, we also find that a one-year increase in age at last employment reduces age at death by roughly four months.

Importantly, all F-statistics are above the conventional rule-of-thumb threshold of 10. Moreover, the IV estimates are more than twice as large as the reduced-form estimates, consistent with the estimated four-month increase in age at last employment (as estimated in Table 1). Table A8 shows that the IV estimates are similar across specifications with varying controls. Controlling for predetermined labor market characteristics, a proxy for individuals' pension benefits[24] and labor market outcomes prior to retirement (between ages 45 and 55) does not substantially change the magnitude of our IV estimates.[25] These results further support the claim that the treatment's effect on mortality operates primarily through its influence on age at last employment, suggesting that the exclusion restriction is reasonably met.

To interpret the IV results, it is important to understand who the compliers are. Since compliers cannot be identified at the individual level, we follow Abadie (2003) and estimate the share of compliers in different subsamples to infer their characteristics. In this analysis, we define treatment as retiring after age 61. In Table A9, we characterize the compliers based on a set of predetermined characteristics. Compliers are slightly more likely to be women. They are also more likely to be active in the labor market, employed, and to have lower monthly earnings between the ages of 30 and 40. Compliers are disproportionately employed in sectors where extending employment near retirement is less costly, such as sectors that exhibit lower incidences of workplace accidents and reduced psychosocial burden.

4.4 Placebo Tests and Robustness

In this section, we assess the causal interpretation of our estimates by using placebo cutoffs after 1967 and conducting a series of robustness checks on the reduced-form labor market effects as well as on both the IV and reduced-form mortality estimates.

[24] We do not have information on pension benefits for individuals who have never claimed a pension. Therefore, for all individuals in our sample, we construct a proxy of the mean pension benefit using monthly contributions and years of contribution (or years of employment and unemployment) using the Social Security formula to calculate pension benefits. The correlation between this proxy and the actual mean pension benefit is 0.93 for individuals who claim a regular pension, indicating that it is a good proxy.

[25] While controlling for labor market outcomes before retirement and a proxy of individuals' pension benefits helps support the "exclusion restriction" assumption, we are aware that they are bad controls because they are affected by the reform. Therefore, our preferred main specification is the one without adding these controls.

4.4.1 Placebos

A potential threat to causality is that our results could be potentially biased by unobserved characteristics that simultaneously affect the timing of first contribution and the outcome variables. To test this possibility, we conduct several placebo tests by assigning treatment based on alternative first-contribution dates. We use two approaches to perform these placebo tests. The first approach compares individuals who began contributing around hypothetical cutoffs (after 1967) within our baseline sample (cohorts born between 1940 and 1949).[26] Figure 3 plots the estimated coefficients. The placebo estimates are labeled in black, while our baseline estimates are in red. As shown, nearly all placebo estimates are either statistically indistinguishable from zero or economically negligible.

A limitation of this exercise is that, by holding cohorts fixed, each placebo comparison includes individuals with varying ages at first contribution. For example, comparing individuals who began contributing in 1970 versus 1971 within the 1940–1949 cohorts yields an entry-age range of 22–31, whereas the baseline comparison (1966 vs. 1967) involves an entry-age range of 17–27. To address this issue, we adopt a second approach that holds the age-at-entry bracket constant while varying the cohorts: we restrict the sample to individuals whose first contribution occurred between ages 17 and 27 and implement placebo cutoffs in the same manner. Figure 4 reports the estimated coefficients for the placebo cutoffs between 1967 and 1972.[27] As before, the placebo estimates are labeled in black, while our baseline estimates are in red. Consistent with the previous results, nearly all placebo estimates are insignificant or close to zero.

Taken together, the two placebo exercises (holding cohorts fixed and holding entry ages fixed) yield consistent null results and reinforce the interpretation that our baseline estimates are driven by the exogenous increase in the early retirement age rather than by other confounding factors.

4.4.2 Robustness Tests

Next, we show that our results are robust to a range of alternative specifications and sample definitions. Specifically, we (i) address potential misreporting in the date of first contribution, (ii) replace birth-year fixed effects with age-at-first-contribution fixed effects, (iii) remove baseline controls, (iv) exclude self-employed individuals, and (v) expand the sample to include adjacent entry cohorts.

[26] For both placebo exercises, we exclude workers in special regimes (mining, maritime), for whom the January 1967 deadline was set later, as well as individuals whose first-contribution date is clearly misclassified (see Section 4.4.2). Including these observations leaves the placebo results essentially unchanged.

[27] Because data are available up to 2023, we observe cohorts born between 1933 and 1954 up to age 69, which limits feasible placebo years to the range 1967–1972.

Date of First Contribution. As previously explained in Section 4.1, an important caveat is that some individuals recorded as having started contributions after 1967 are observed retiring at age 60, which is legally impossible. This pattern likely reflects misreporting of the first contribution date in the administrative records. These errors are plausibly random and unrelated to individual characteristics. Accordingly, we do not correct for these cases in our main specification, as these errors would bias our estimates toward zero and thus yield conservative results.

In this section, we implement a correction by excluding individuals whose recorded contribution start date is after 1966 but who are flagged as having contributed to the pre-1967 system, known as Mutual Societies ("Mutualidades Laborales"). Panel A of Table A10 shows that the reform's effect on age at last employment (first stage) is nearly twice as large as in the baseline specification, indicating a stronger delay once misreporting is addressed. At the same time, Column 1 of Table A11 also finds that the reduced-form effect of the reform on mortality is larger than in the baseline estimation. Specifically, the reform increased the probability of dying between ages 60 and 69 by 1.8 percentage points for individuals who began contributing in 1967. As a result, the IV estimates remain very similar to those from the baseline specification. The increase in the reduced-form effects implies that our baseline reduced-form estimates are conservative, while the stability of the IV estimates suggests that misreporting is random with respect to the instrument, leaving the IV estimates unbiased.

Within-Age at First Contribution Fixed Effects Model. Within a given birth cohort, individuals who first contributed in 1967 are, by construction, one year older at entry than those who first contributed in 1966. Therefore, differences in educational attainment could confound estimates that rely on birth-year fixed-effects. Unfortunately, the dataset does not contain information on individuals' educational attainment. To assess whether the reform effect captures differences in education, we re-estimate the models using age-at-entry fixed effects instead of birth-year fixed effects. In this model, we estimate the impact of losing access to early retirement among individuals who enter the labor market at the same age but were born one year apart. Panel B of Table A10 and column 2 of Table A11 show that, compared with the baseline results, these estimates using starting age fixed effects are comparable.

No Controls. Our baseline specification includes controls for labor market activity at the time of entry and between ages 30 and 40, intended to proxy early-career characteristics. To assess the sensitivity of our estimates to the inclusion of these controls, we estimate the model without them. Panel C of Table A10 and Column 3 of Table A11 report estimates that are similar in magnitude to the baseline, suggesting limited endogeneity of these covariates.

Dropping Self-Employed Individuals. Table A3 shows a small but significant effect of the reform on the share of time in self-employment between ages 30 and 40. This raises the possibility that self-employed individuals may have more flexibility in determining when to begin contributing to the Social Security system. Panel D of Table A10 and column 4 of Table A11 show that the estimates are robust to excluding individuals who received a pension under the self-employed regime, suggesting that our baseline results are not driven by this group.

Augmented Sample. Finally, we expand the sample to include individuals who began contributing in 1965 and 1968. Panel E of Table A10 and Column 5 of Table A11 show that the findings remain robust when using this broader window.

4.5 Heterogeneous Effects

What are the distributional impacts of the reform? In particular, are individuals with hazardous or arduous occupations disproportionately affected by the removal of early retirement options? Could policies that facilitate gradual retirement mitigate the harmful impacts on mortality? Do men and women respond differently? To address these questions, in this section, we examine heterogeneity along three dimensions: labor-market conditions before retirement, access to flexible retirement, and gender. To facilitate comparability of magnitudes across subgroups, we emphasize the IV estimates (using the reform as an instrument for age at last employment) in this section, although the corresponding reduced-form estimates are also reported alongside in the tables.

4.5.1 Labor Market Conditions Prior to Retirement

Delaying retirement may have very different effects on individuals' life expectancy depending on the working conditions they experienced during the final years of employment (Mazzonna and Peracchi, 2017).[28] In this paper, we acknowledge that job burden is multidimensional. Therefore, we examine three characteristics of the individuals' labor environment before retirement: physical burden, psychosocial burden, and the skill level of their last occupation. The correlation between

[28] One reason we expect heterogeneity in mortality by labor market conditions is that harsher working environments are more likely to trigger mortality through specific causes that are predominant between the ages of 60 to 69. For instance, the medical literature links circulatory diseases to work-related stress (Kivimäki et al., 2002). In fact, both Bloemen et al. (2017) and Hallberg et al. (2015) report that retirement reduces the risk of heart-related mortality. Among the cohorts considered in our sample, circulatory diseases represent the second leading cause of death (after tumors) at ages 60-69. Moreover, respiratory diseases, the third leading cause, are associated with smoking and lack of physical activity (Godtfredsen et al., 2008; Lee et al., 1999), both of which can be affected by employment status and retirement (Falba et al., 2005; Black et al., 2015; Evenson et al., 2002; Barnett et al., 2014). Fitzpatrick and Moore (2018) also find that mortality due to two lung-related conditions (COPD and lung cancer) statistically increases immediately after retirement at age 62.

the first two measures (physical and psychosocial burden) is modest (0.15), indicating that they capture distinct aspects of the work environment.

Table 4 reports heterogeneity in mortality between ages 60 and 69, conditional on survival to age 60.[29] The upper panel presents the reform's effect on age at last employment across subgroups, which constitutes the first stage of the IV estimation. The lower panel reports, for the same subgroups, the reduced-form effects on mortality between ages 60 and 69 and the corresponding IV estimates; the last row reports p-values testing the equality of IV coefficients across groups. Importantly, Panel F of Table A6 finds that the reform did not affect the probability of belonging to each subgroup, except for the probability of being employed in low-skilled occupations before retirement, which we take into account when interpreting the skill-based heterogeneity.

Physical and Psychosocial Burden. Retirement enables individuals to enjoy more leisure time and eliminates work-related stress and exposure to job-specific accidents, potentially improving both mental and physical health and overall well-being. These gains are likely to be larger for workers in strenuous occupations, whether physically or mentally demanding. Indeed, labor unions have used this argument heavily in their opposition to increases in the statutory retirement age. Accordingly, it is policy-relevant to examine whether the adverse mortality effects of delayed retirement differ by these characteristics. To do so, we classify individuals' last industry according to its physical and psychosocial burden.

Previous literature has already established that physically demanding occupations are associated with adverse health effects (see Case et al. (2005) and Ravesteijn et al. (2013) for a summary). To measure physical burden at work, we use data from the Spanish Register of Workplace Accidents between 2003 and 2019, which records the total number of accidents experienced by workers in our study cohorts (1940–1949) across industry sectors. Figure A2 a) shows the distribution of industry sectors by their incidence of workplace accidents per 1,000 workers. We link each individual's last industry to this aggregate measure and divide the sample at the median accident rate.

Columns 1 and 2 of Table 4 show that treated individuals in low-incidence sectors delay retirement by nearly twice as much, yet their mortality rates are unaffected. In contrast, among individuals in high-incidence sectors, a one-year delay in age at last employment increases the probability of dying between ages 60 and 69 by 4.3 percentage points ($\sim 34\%$). The p-value for the difference across groups is 0.020.

Next, we examine the heterogeneous effects of delaying retirement on mortality based on the level of mental and social stress experienced before retirement. Due to lack of comparable Spanish

[29] Since the reform does not affect mortality before age 60, conditioning on survival to age 60 does not introduce selection bias.

measures, we adopt occupational indices from the Job Exposure Matrices developed by Kroll (2011), which are based on a large representative survey of working conditions in Germany and have been used in prior studies (e.g., Eibich, 2015; Mazzonna and Peracchi, 2017; Alacevich and Nicodemo, 2024). The index aggregates mental stress, social stress, and temporal loads.[30] Figure A2 b) shows a distribution of industry sectors according to this psychosocial exposure index. We link individuals' last industry to this aggregate occupation-level data and divide our sample by the median of this index.

Columns 3 and 4 of Table 4 indicate that, although first-stage delays are similar across psychosocial groups, a one-year delay increases mortality by 2.8 percentage points ($\sim$ 35%) in high-psychosocial-burden sectors, compared to 1.4 percentage points ($\sim$ 17%) in low-burden sectors. While the difference is in the expected direction (more than twice as large in high-exposure industries), it is not statistically significant.

Skill Level. Finally, previous literature has emphasized heterogeneity based on differences between high- and low-skilled occupations (Coe et al., 2012). Following this literature, we examine differential effects for individuals working in high- and low-skilled occupations, as shown in columns 5 and 6 of Table 4. Contrary to the findings of Mazzonna and Peracchi (2017), we find that this heterogeneity is similar to that based on the physical burden. Among individuals in low-skilled occupations, delaying retirement by one year increases the probability of dying between the ages of 60 and 69 by 4.2 percentage points ($\sim$ 50%), while the effect is only 0.4 percentage points, and not statistically significant, for the rest. The difference is statistically significant, with a p-value of 0.02, indicating that skill level is likely to capture a large part of the differences in physical burden across sectors. However, this result should be taken with caution as Table A6 shows that the reform had a significant positive effect on the probability of working in low-skilled jobs before retirement.

In summary, Table 4 shows that individuals with strenuous employment (both physically and psychosocially demanding) and those in low-skilled occupations experience a greater increase in mortality between ages 60 and 69 as a result of the reform. In contrast, the reform's impact on the age at last employment is relatively similar across subgroups. If anything, individuals with better jobs tend to delay labor market exit longer, indicating a revealed preference among those in more demanding occupations to retire earlier. These findings suggest that removing access to

[30]The psychosocial burden occupational index elaborated by Kroll (2011) is linked to individuals' last industry in our sample following these steps. First, we group all the industries defined in CNAE09 into 21 groups. Using the Labor Force Survey 2011, we observe which occupations (defined by CNO11) are most often performed in each of the 21 industry groups and with what frequency. Finally, we link the psychosocial index with each industry depending on which occupations are usually performed within each industry, using the frequencies as weights to calculate the mean psychosocial burden in each sector.

early retirement may exacerbate social disparities in mortality and reduce the progressivity of the pension system, given that poor working conditions often coincide with low wages (OECD, 2019).

4.5.2 Possibility of Gradual Retirement

While removing access to early retirement effectively prolongs careers, our findings indicate that it can also adversely affect life expectancy. A natural policy response is to allow older workers to reduce their working hours toward the end of their careers. We therefore examine whether access to partial retirement mitigates the mortality effects of delayed labor market exit.

As described in Section 2, since 2002, the Spanish system has permitted partial retirement from age 60, enabling individuals to combine work and old-age benefits. Eligible individuals can claim up to 85% of their pension while reducing their working time to 15–85% of the original contract, subject to employer agreement, provided they have at least 33 years of contributions and six years of tenure in the same firm.

Since the reform directly affects the probability of claiming a partial pension, a simple comparison of claimants and non-claimants would yield biased estimates. Instead, we exploit the institutional eligibility rule, which restricts partial retirement to workers with at least 33 years of contributions. Because we only observe the number of contribution years for individuals who claim a regular or partial pension, we proxy contribution years using the number of years employed since their first Social Security contribution. Figure A3 shows that the probability of claiming a partial pension rises sharply at 33 years of employment and is close to zero below this threshold.

To ensure comparability around the eligibility threshold, Table 5 compares individuals with 23–32 years of employment to those with 33–42 years. The first row confirms that eligibility matters: individuals with more than 33 years of employment are substantially more likely to claim a partial pension. In particular, treated individuals with more than 33 years of employment are 4.1 percentage points more likely to claim a partial pension ($\sim$ 47%), whereas those with less than 33 years of employment show only a 0.1 percentage points increase, which is not statistically significant.

Table 5 further shows that a one-year increase in age at last employment raises mortality between ages 60 and 69 by 4.4 percentage points ($\sim$ 44%) among workers with fewer than 33 years of employment (ineligible for partial retirement). In contrast, the corresponding effect for those eligible for partial retirement is considerably smaller, only 0.6 percentage points, and not statistically significant. The difference in effects between the two groups is statistically significant, with a p-value of 0.068.

To verify that these differences are driven by eligibility, rather than unobserved differences in employment histories, we conduct two placebo exercises using subsamples consisting entirely of either ineligible or eligible workers. Columns 1 and 2 of Table A12 compare individuals with 26–32 years of employment to those with 17–25 years, both of whom are ineligible for partial retirement. As expected, we find no effect on partial pension take-up in either subsample. At the same time, a one-year increase in age at last employment raises mortality by 4.2 and 5 percentage points, respectively, in these two ineligible groups. Although these estimates are imprecise (due to the limited number of individuals with short employment histories; see Figure A3), the differences between the two ineligible groups are not statistically significant, and the magnitudes are comparable to those observed for the 23–32 group in Table 5. Columns 3 and 4 compare individuals with 38–42 years of employment to those with 33–37 years, both of whom are eligible for partial retirement. In both groups, partial pension take-up increases with the reform, while the mortality effect of a one-year delay is near zero and statistically insignificant. These placebo analyses support the interpretation that the subgroup estimates in Table 5 reflect differential effects attributable to access to partial retirement, rather than heterogeneity arising from unobserved employment histories.

Taken together, the heterogeneity results indicate that the mortality costs of delayed retirement are borne disproportionately by individuals in physically demanding, low-skill, and high-psychosocial-burden occupations. The attenuation observed among workers eligible for partial retirement suggests that access to a gradual exit from the labor market may offer protective health benefits and represent an effective policy tool for extending working lives without exacerbating health risks.

4.5.3 Gender Difference

Table 6 reports gender-specific estimates. Conditional on survival to age 60, both men and women delay labor market exit by similar amounts in response to the reform, and the associated mortality effects are also comparable. Table A13 examines in greater detail the reform's effect on the labor market outcomes by gender. The delay is slightly larger for women (4.2 months versus 3.5 months for men) and is accompanied by a larger decline in the probability of claiming a regular pension (4.3 percentage points for women and 3.5 percentage points for men). However, due to women's higher baseline probability of claiming a regular pension, the relative reductions as a consequence of the reform are similar (5.6% for women versus 5.0% for men).

Turning to mortality outcomes, Table 6 shows that the reform's effect on the probability of death between ages 60 and 69 is similar for men and women, and Table A14 confirms comparable effects up to age 70. Beyond age 70, however, the patterns diverge: by age 74, the effect for women attenuates to zero, whereas the effect for men persists.

A plausible explanation for the concentration of the reform's mortality effects among women at earlier ages is the age–sex distribution of causes of death in Spain.[31] Between ages 60 and 69, neoplasms and circulatory diseases account for the majority of deaths in both sexes (approximately 41–46% and 23–26%, respectively). If the reform primarily raises cardiometabolic risk (e.g., via prolonged work stress or physical strain), then a similar mortality response across genders during this age window is expected. After age 70, however, women's mortality profile shifts more rapidly toward cerebrovascular and neurodegenerative causes (11% and 10–11% at ages 70–74)–outcomes less tightly linked to short-run work exposure, so the cardiometabolic channel weakens. As a result, the difference in mortality between women who started contributing in 1966 and in 1967 attenuates between ages 70 and 74. In contrast, men's cause-of-death distribution remains comparatively more cardiometabolic-heavy, which may explain the persistence of the reform's mortality effects among men.

5 Discussion

We find that individuals who first contributed in 1967 (faced a delay of five years in the statutory retirement age) have a 0.9-percentage point higher probability of dying between ages 60 and 69 (an 11% increase). The IV estimates indicate that a one-year increase in age at last employment raises mortality at ages 60-69 by 2.5 percentage points ($\sim$ 32%). While these effects may appear large, they are smaller in magnitude than estimates from studies showing that earlier retirement reduces mortality (Hallberg et al., 2015; Bloemen et al., 2017).

Hallberg et al. (2015) find that offering a five-year reduction in the statutory retirement age (from 65 to 60) reduces the probability of dying by age 70 by 26%. Using the same mortality measure, we find that a five-year increase in the statutory retirement age (from 60 to 65) raises the probability of dying before 70 by 1.2 percentage points ($\sim$ 10%). They also show larger effects for workers more exposed to workplace hazards; that is, those with low pre-retirement incomes and without a college education. Their finding is consistent with our heterogeneous results. Bloemen et al. (2017) find that a temporary decrease in the retirement eligibility age (from 65 to 61-62) for Dutch male civil servants decreased the probability of dying within five years by 2.6 percentage points ($\sim$ 47%).[32]

[31] According to Spain's official 2024 cause-of-death statistics (Instituto Nacional de Estadística , INE), there are marked age–sex differences. Between the ages of 60 and 69, neoplasms and circulatory diseases account for around two-thirds of deaths in both sexes. Specifically, neoplasms account for 41.5% of male deaths and 44% of female deaths; circulatory diseases for 25.5% and 24%; respiratory for 11% and 6.5%; cerebrovascular for 6.1% and 8%; neurodegenerative for 3.9% and 5%; external for 5% and 2%; with other causes comprising 7% and 10.5%. At ages 70–74, women's deaths shift more toward cerebrovascular and neurodegenerative causes, while men's remain more cardiometabolic. Specifically, the corresponding shares for men and women are 36.5% and 35% for neoplasms, 29.5% and 26% for circulatory, 12% and 8% for respiratory, 8% and 11% for cerebrovascular, 6% and 11% for neurodegenerative, 5% and 3% for external, and 3% and 6% for other causes.

[32] Although Hallberg et al. (2015) study Swedish male military officers and Bloemen et al. (2017) focus on Dutch male civil servants, both papers argue that these groups' working environments are not more demanding than those of the

Our prior was that the effect of delaying retirement need not be symmetric to the effect of early retirement. And, indeed, when the nature of the reform and affected age ranges are comparable, our estimates suggest that the effect on mortality is smaller magnitude.

To our knowledge, we are the first to document higher mortality from delaying retirement using a quasi-experimental variation that shuts down early-retirement options. Prior studies generally find no effect of delaying retirement on mortality when reforms change financial incentives but leave the statutory age unchanged. Bozio et al. (2021) and Saporta-Eksten et al. (2021) are the only two papers we know of that have the statistical power to conclusively estimate the mortality impact.[33] Bozio et al. (2021) estimate precisely zero effects on mortality at ages 61–79 for French private-sector workers, and Saporta-Eksten et al. (2021) find no effect at ages 65–74 from reducing the implicit tax on work for married men in Israel.[34] One interpretation is that working longer is more harmful when early retirement is removed than when retiring early is simply made less financially attractive. Consistent with this, we find that eligibility for gradual (partial) retirement attenuates the mortality effect (Table 5), suggesting that flexible pathways mitigate the health costs of longer careers.

Related literature study the impact of old-age income on mortality (e.g., Jensen and Richter, 2004; Snyder and Evans, 2006; Malavasi and Ye, 2023). Snyder and Evans (2006) examine a variation in Social Security wealth for a U.S. "notch" cohort and find that lower pension wealth reduced mortality, which they attribute to beneficial effects of employment. In contrast, we show that the reform, which removes early retirement access, reduces pension benefits and increases mortality. However, in our IV analysis, when we control for pension income, our estimates remain robust suggesting that the adverse mortality impact is mainly due to delayed employment. Both papers highlight the dominance of the employment margin; however, Snyder and Evans (2006) find working longer beneficial, whereas we find it detrimental. A plausible reconciliation is that working longer due to the loss of early-retirement rights differs from working longer due to less generous pensions. Moreover, our setting does not require displacement, which itself can raise mortality (Sullivan and Von Wachter, 2009). Finally, Snyder and Evans (2006) points out that the "notch" cohort is working longer, mostly through an increase in part-time employment while still receiving pension benefits. As a result, their results are more comparable to our findings for people who are eligible for the partial retirement scheme. These institutional details may explain why we

general population.

[33] Hagen (2018) explores a reform that increases the statutory retirement age from age 63 to 65 for Swedish public sector workers born since 1938. They find an imprecisely measured no effect on mortality by the age of 69. Their IV estimates show that a one-year increase in retirement age results in a 0.34% increase in morality by the age of 69 (insignificant).

[34] Saporta-Eksten et al. (2021) also document lower survival at ages 75–85 among affected men and estimate that one additional year of employment reduces longevity by 9–12 months, with stronger effects for blue-collar workers.

find that later retirement leads to higher mortality.

Finally, our findings relate to recent evidence on disability insurance (DI) and mortality. Black et al. (2018) show that DI access reduces mortality for sicker, inframarginal beneficiaries ages 55-64, due to income and Medicare eligibility.[35] Similarly, Gelber et al. (2023) find that more generous DI benefits reduce mortality among low-income recipients. Both papers underscore the protective role of income for vulnerable individuals. Analogously, our evidence suggests that access to a pension (via early retirement or partial retirement) is protective: removing it raises mortality, and the groups driving our heterogeneity (physically demanding, low-skill, and high-psychosocial-burden jobs) are those for whom continued exposure near retirement appears most costly.

6 Conclusion

This paper studies the effect of delaying retirement on mortality. We exploit the 1967 Spanish pension reform that eliminated access to voluntary early retirement for individuals who had not begun contributing to the Social Security system by that year. Comparing individuals who began contributing within 12 months before and after January 1, 1967, we find that those who started contributing in 1967 delayed their labor market exit by approximately four months. They are also more likely to claim partial pensions and occupational or partial disability pensions in order to leave the labor market before the regular pension becomes available.

We find considerable effects on mortality. A one-year delay in labor market exit increases the probability of dying between ages 60 and 69 by 2.5 percentage points (32%). These mortality effects are most pronounced at ages when early retirement is no longer available or shortly after the statutory retirement age. This suggests that the increase in mortality is concentrated in the short term around the conventional retirement threshold, consistent with the elimination of early-retirement options. Further analysis shows that individuals who worked in physically demanding, low-skill, and high-psychosocial-burden jobs before retirement suffer more in terms of mortality, although they delay retirement to a lesser extent. Moreover, we show that allowing workers to gradually reduce their workload can incentivize workers to stay longer in the labor force without negatively affecting their life expectancy.

The applicability and relevance of our findings extend further than the Spanish context. Delaying statutory retirement and closing early retirement options are pertinent policy agendas in many countries. However, existing empirical evidence on the mortality effects of retirement is based almost exclusively on the estimates of policy experiments that have allowed for earlier retire-

[35] Black et al. (2018) also show that for conditions requiring low medical expenditures (e.g., musculoskeletal disorders), DI receipt can increase mortality because continued work is beneficial for that group.

ment. There is not necessarily a symmetry in the effects on health and mortality from preponing and postponing retirement age, as the individuals impacted by these policies may differ greatly. Given the existence of heterogeneous effects on mortality across groups of individuals who respond differently to becoming a retiree, our findings on the mortality effect of delaying retirement are particularly relevant.

The heterogeneous mortality impacts of delaying retirement points on the potential distributional consequences of raising the statutory retirement age. Although beyond the scope of our paper, we believe that examining the distributional effects of pension reforms while taking into account the health and mortality consequences is a fruitful avenue for future research.

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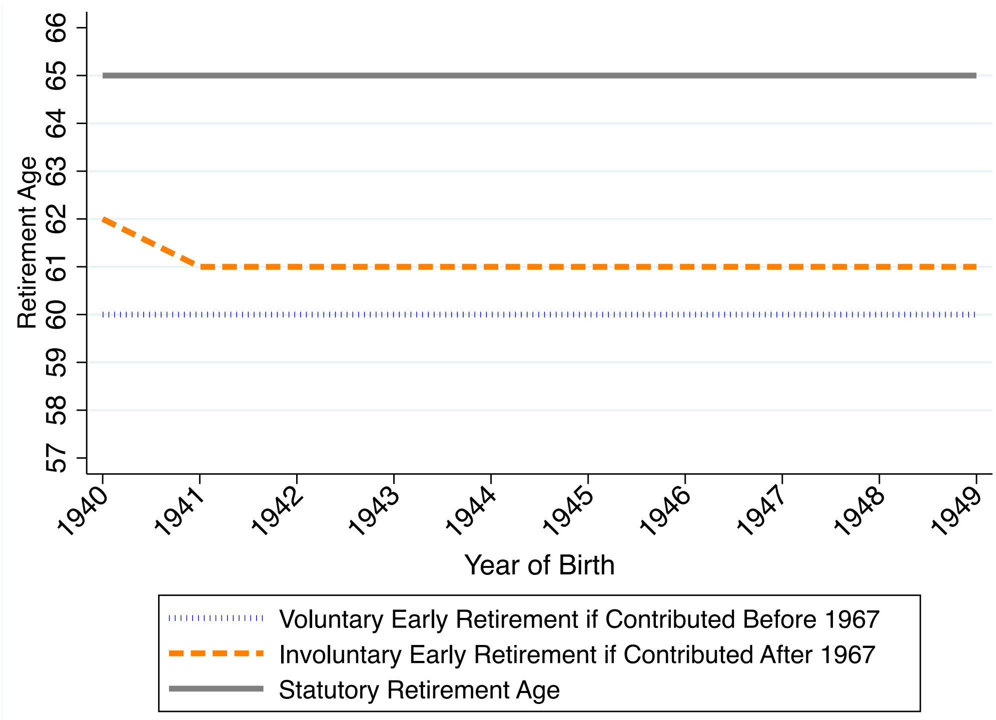
7 Figures and Tables

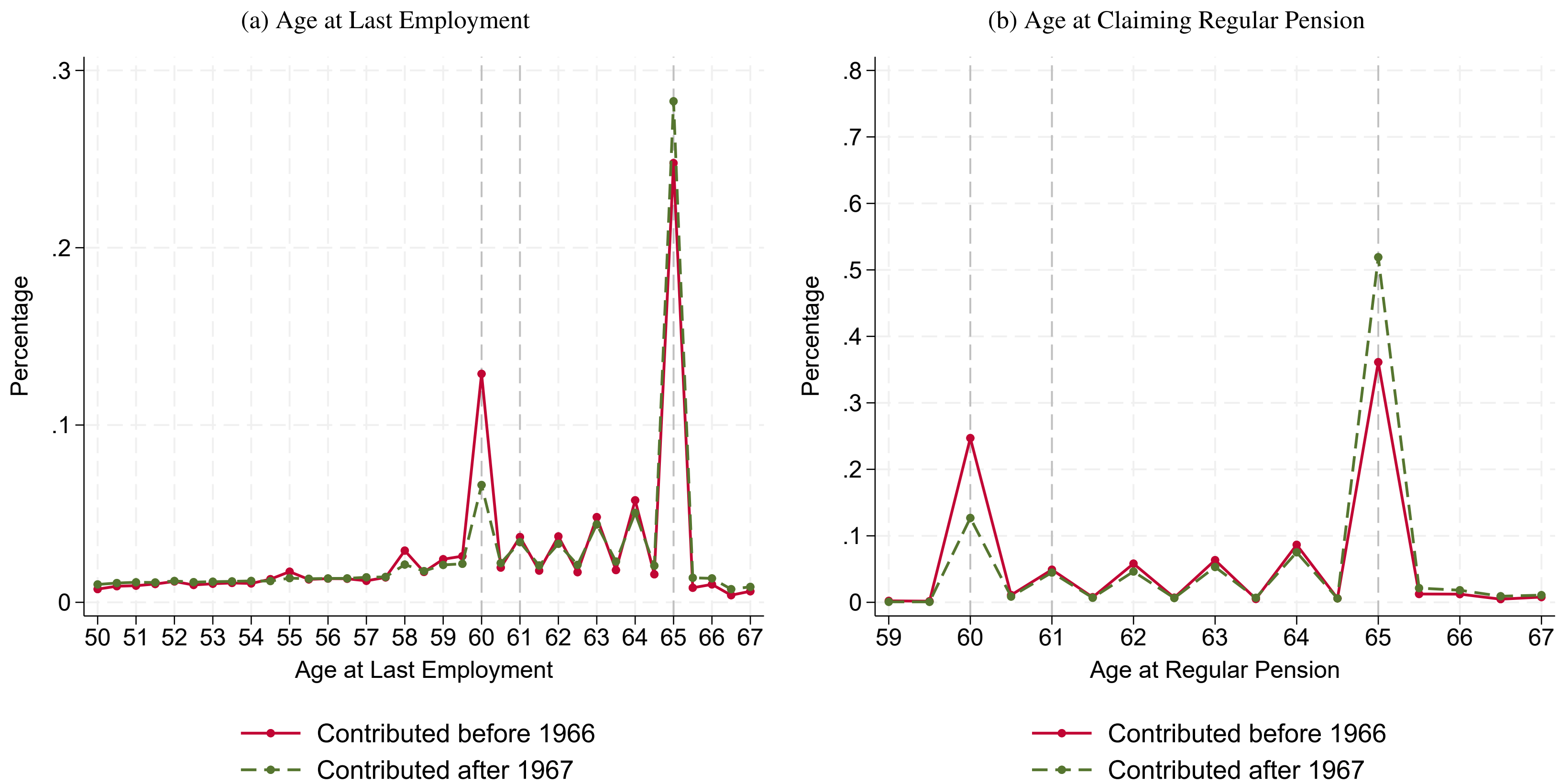
Figure 1: Retirement Age by First Year of Contribution and Cohort



Source: Authors' elaboration based on national pension legislation.

Notes: This figure presents the statutory retirement age and the earliest available early retirement age for individuals based on whether they initiated contributions before or after 1 January 1967, as a function of their birth cohort. The blue dotted line indicates that individuals who started contributing before 1 January 1967 are eligible for voluntary retirement from age 60, irrespective of their birth year. The orange dashed line denotes that individuals who began contributing after this date may only access involuntary early retirement between the ages of 61 and 62, contingent upon their specific birth cohort. The solid grey line represents the statutory retirement age, fixed at 65 years for all cohorts, regardless of the timing of their initial contributions.

Figure 2: Density of Age at Last Employment and Age at Regular Pension by Treatment Status

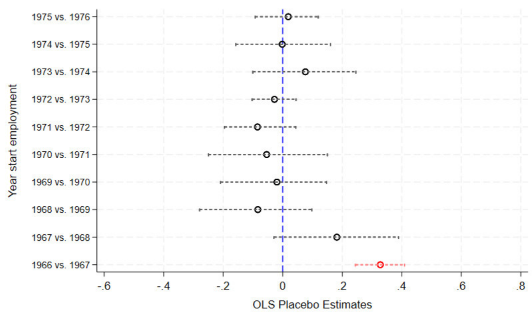


Source: MCVL, cohorts 1940-1949.

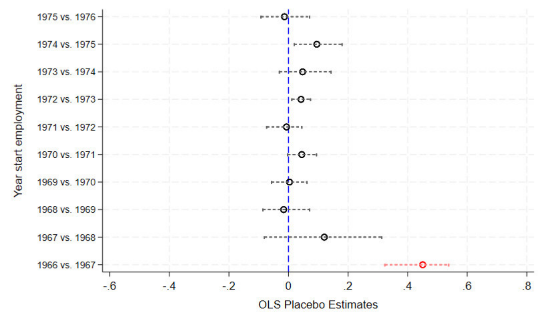
Notes: This figure displays the distribution of individuals according to the age at which they exited their last employment (a) and the age at which they initiated receipt of a regular pension (b). Solid red lines represent individuals who began contributing before 1 January 1967, while dashed green lines represent those who started contributing after this date.

Figure 3: Placebo Tests: Using Other Cutoffs

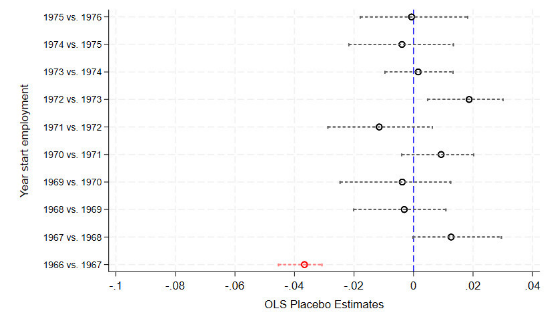
(a) Age at Last Employment



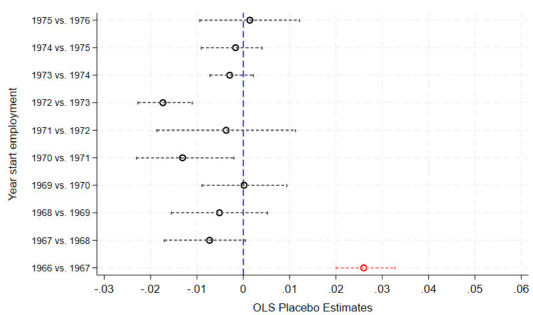
(b) Age at Claiming Regular Pension



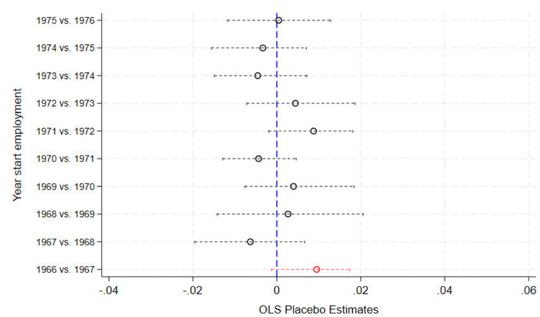
(c) Probability of Claiming Regular Pension



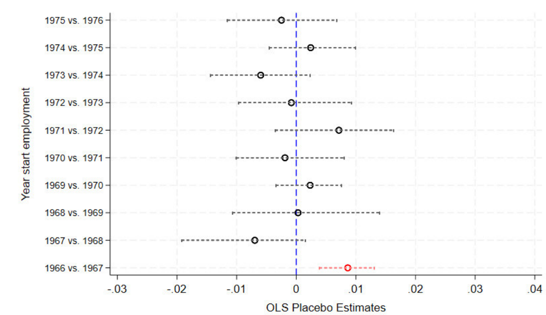
(d) Probability of Claiming Partial Pension



(e) Probability of Dying between 50 and 74



(f) Probability of Dying between 60 and 69

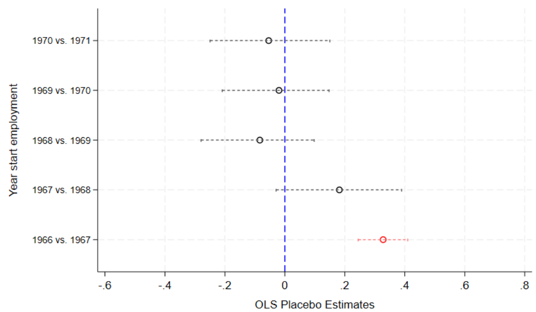


Source: MCVL, cohorts 1940-1949.

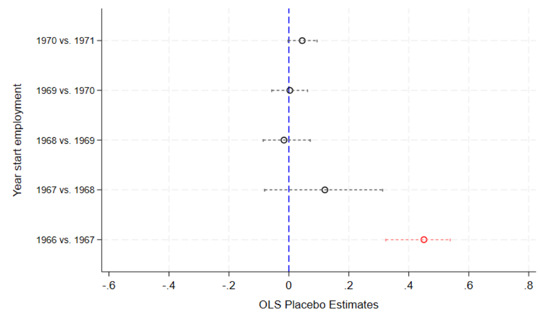
Notes: These figures present point estimates and their corresponding 95% confidence intervals from a series of placebo regressions based on equations 1 and 2. Each placebo estimate compares cohorts beginning contributions in adjacent years as indicated on the y-axis. The highlighted estimate in red corresponds to the actual policy cutoff analysis comparing individuals who began contributing in 1966 versus 1967. The outcomes analyzed are indicated at the top of each figure.

Figure 4: Alternative Placebo Tests: Keeping Age Started Contributing Constant

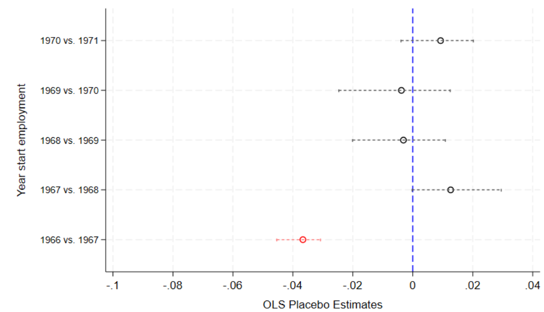
(a) Age at Last Employment



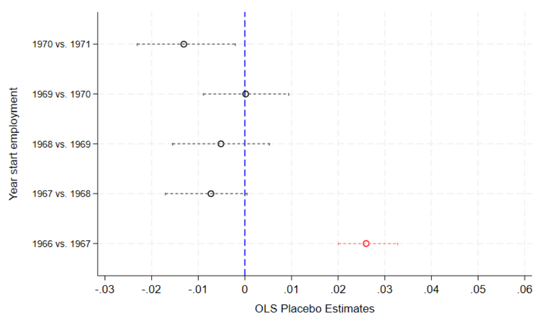
(b) Age at Claiming Regular Pension



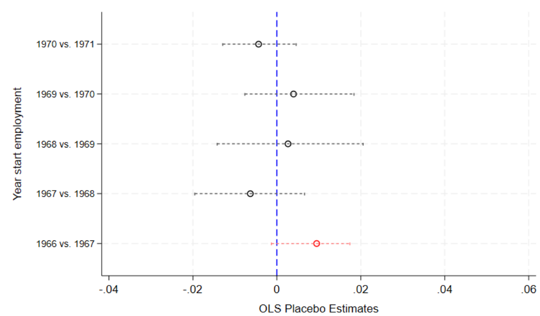
(c) Probability of Claiming Regular Pension



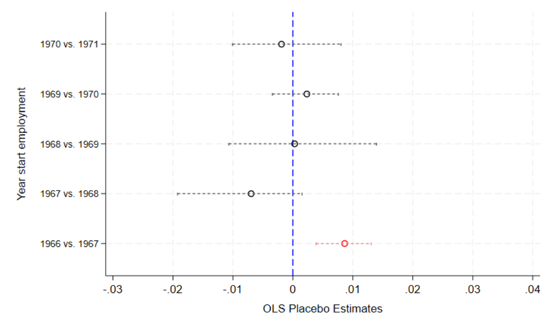
(d) Probability of Claiming Partial Pension



(e) Probability of Dying between 50 and 74



(f) Probability of Dying between 60 and 69



Source: MCVL, cohorts 1941-1954.

Notes: These figures present point estimates and their corresponding 95% confidence intervals from a series of placebo regressions based on equations 1 and 2. Each placebo estimate compares cohorts beginning contributions in adjacent years as indicated on the y-axis. The analyses vary the cohorts included but consistently restrict the sample to individuals who started contributing between the ages of 17 and 27. The highlighted estimate in red corresponds to the actual policy cutoff analysis comparing individuals who began contributing in 1966 versus 1967. The outcomes analyzed are indicated at the top of each figure.

Table 1: Impact of the Reform on Age at Last Employment and Type of Pension

	Age at	First Pension Claimed			
	Last Employment	Regular Pension	Partial Pension	Disability Insurance	No Pension
	(1)	(2)	(3)	(4)	(5)
Contributed in 1967	0.328***	-0.037***	0.026***	0.006	0.004**
	(0.035)	(0.003)	(0.003)	(0.003)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.110]	[0.010]
Month-Year Birth FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓
Observations	40,737	40,737	40,737	40,737	40,737
R^2	0.057	0.030	0.058	0.019	0.020
Mean Dep. Variable (Treated)	61.062	0.683	0.094	0.152	0.070
Mean Dep. Variable (Control)	60.753	0.723	0.065	0.144	0.069

Source: MCVL, cohorts 1940-1949.
Notes: This table presents the impact of the reform based on regression 1. The outcomes analyzed include the age at which individuals exited their last employment (Column 1) and the probabilities of exiting the labor market through a regular pension (Column 2), a partial pension (Column 3), a disability pension (Column 4), or without claiming any pension (Column 5). The estimation sample includes individuals who started contributing within 12 months before or after 1 January 1967. All regressions include controls for gender, birth year, and birth month fixed effects. Additional controls include average monthly contributions, fraction of time employed, fraction of time active, fraction of time in self-employment (all measured between ages 30 and 40), and fixed effects for initial occupation and industry. Standard errors are clustered at the birth-year level, and wild-bootstrap p-values are provided in brackets.
*** p<0.01, ** p<0.05, * p<0.1.

Table 2: Impact of Age at Last Employment on Mortality

	Probability of Dying between the Ages				Age at Death
	50-74	50-59	60-69	70-74	Censored at 74
	(1)	(2)	(3)	(4)	(5)
Reduced Form:					
Contributed in 1967	0.009*	0.001	0.009***	-0.001	-0.112***
	(0.004)	(0.002)	(0.002)	(0.005)	(0.029)
	[0.068]	[0.605]	[0.008]	[0.829]	[0.009]
OLS:					
Impact of Age at Last Employment	-0.018***	-0.010***	-0.007***	-0.004***	0.272***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.016)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
IV:					
Impact of Age at Last Employment	0.029*	0.004	0.025***	-0.003	-0.341***
	(0.012)	(0.006)	(0.006)	(0.013)	(0.074)
	[0.097]	[0.576]	[0.002]	[0.821]	[0.001]
Month-Year Birth FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓
Observations	40,737	40,737	39,181	35,416	40,737
Mean Dep. Variable (Treated)	0.213	0.040	0.090	0.086	73.122
Mean Dep. Variable (Control)	0.196	0.037	0.079	0.084	73.302
F-stat FS	88.790	88.790	83.200	65.984	88.790

Source: MCVL, cohorts 1940-1949

Notes: This table reports the impact of age at last employment on mortality outcomes. Columns 1 to 4 show effects on the probability of dying between ages 50–74 (Column 1), 50–59 (Column 2), 60–69 conditional on surviving until age 59 (Column 3), and 70–74 conditional on surviving until age 69 (Column 4). Column 5 reports effects on age at death, censored at 74 for individuals alive at that age. Panel A presents the reduced-form estimates of the reform's direct effect on mortality (based on regression 2). Panel B shows the ordinary least squares correlation between age at last employment and mortality. Panel C reports the instrumental variable estimates, derived from regression 3. The estimation sample includes individuals who started contributing within 12 months before or after 1 January 1967. All regressions include controls for gender, birth year, and birth month fixed effects. Additional controls include average monthly contributions, fraction of time employed, fraction of time active, fraction of time in self-employment (all measured between ages 30 and 40), and fixed effects for initial occupation and industry. Standard errors are clustered at the birth-year level, and wild-bootstrap p-values are provided in brackets.

Table 3: Impact of Age at Last Employment on Mortality at Five-year Intervals

	Probability of Dying between the Ages					
	50-74	50-54	55-59	60-64	65-69	70-74
	(1)	(2)	(3)	(4)	(5)	(6)
Reduced Form:						
Contributed in 1967	0.009*	0.000	0.001	0.004**	0.007***	-0.001
	(0.004)	(0.001)	(0.003)	(0.002)	(0.001)	(0.005)
	[0.068]	[0.962]	[0.691]	[0.036]	[0.001]	[0.829]
OLS:						
Impact of Age at Last Employment	-0.018***	-0.005***	-0.006***	-0.005***	-0.003***	-0.004***
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]
IV:						
Impact of Age at Last Employment	0.029*	0.000	0.003	0.012**	0.019***	-0.003
	(0.012)	(0.002)	(0.007)	(0.005)	(0.003)	(0.013)
	[0.097]	[0.951]	[0.656]	[0.028]	[0.001]	[0.821]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓
Observations	40,737	40,737	40,216	39,181	37,547	35,416
Mean Dep. Variable (Treated)	0.213	0.013	0.027	0.045	0.062	0.086
Mean Dep. Variable (Control)	0.196	0.013	0.025	0.039	0.052	0.084
F-stat FS	88.790	88.790	109.246	83.200	67.200	65.984

Source: MCVL, cohorts 1940-1949.

Notes: This table reports the impact of age at last employment on mortality risk, measured as the probability of dying between ages 50–74 (Column 1), 50-54 (Column 2), 55-59 conditional on surviving until age 54 (Column 3),60-64 conditional on surviving until age 59 (Column 4), 65-69 conditional on surviving until age 64 (Column 5), and 70-74 conditional on surviving until age 69 (Column 6). Panel A presents the reduced-form estimates of the reform's direct effect on mortality (based on regression 2). Panel B shows the ordinary least squares correlation between age at last employment and mortality. Panel C reports the instrumental variable estimates, derived from regression 3. The estimation sample includes individuals who started contributing within 12 months before or after 1 January 1967. All regressions include controls for gender, birth year, and birth month fixed effects. Additional controls include average monthly contributions, fraction of time employed, fraction of time active, fraction of time in self-employment (all measured between ages 30 and 40), and fixed effects for initial occupation and industry. Standard errors are clustered at the birth-year level, and wild-bootstrap p-values are provided in brackets.

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

Table 4: Impact on Mortality by Labour Market Conditions Before Retirement

	Last Industry				Last Occupation	
	Workplace Accidents		Psychosocial Exposure		Low-skilled	
	High	Low	High	Low	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)
	Age at Last Employment					
First Stage:						
Contributed in 1967	0.259***	0.400***	0.355***	0.355**	0.279***	0.357***
	(0.037)	(0.070)	(0.055)	(0.097)	(0.066)	(0.069)
	[0.008]	[0.002]	[0.003]	[0.012]	[0.005]	[0.010]
	Probability of Dying between 60 and 69					
Reduced Form:						
Contributed in 1967	0.011**	0.004	0.010*	0.005*	0.001	0.015**
	(0.003)	(0.003)	(0.005)	(0.002)	(0.002)	(0.004)
	[0.011]	[0.252]	[0.055]	[0.051]	[0.674]	[0.015]
IV:						
Impact of Age at Last Employment	0.043***	0.009	0.028**	0.014**	0.004	0.042***
	(0.011)	(0.007)	(0.013)	(0.007)	(0.008)	(0.009)
	[0.008]	[0.202]	[0.043]	[0.038]	[0.684]	[0.006]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓
Observations	18,967	16,763	18,030	17,700	17,098	19,650
Mean Dep. Variable (Treated)	0.094	0.085	0.093	0.087	0.078	0.101
Mean Dep. Variable (Control)	0.079	0.079	0.079	0.080	0.076	0.083
F-stat FS	50.088	32.707	41.684	13.382	17.796	26.594
P-value Difference (IV Est.)	0.020		0.417		0.019	

Source: MCVL, cohorts 1940-1949.
Notes: This table reports the impact of age at last employment on the probability of dying between ages 60 and 69 (conditional on survival until age 59), disaggregated by labor-market conditions experienced immediately prior to retirement. Specifically, columns 1 and 2 classify individuals by their industry's workplace accident incidence rate for our cohorts during 2003–2019. Columns 3 and 4 categorize individuals by psychosocial exposure levels (mental stress, social stress, and temporal load), following the classification by Kroll (2011). Finally, columns 5 and 6 distinguish between individuals whose last occupation was high-skilled versus low-skilled. Panel A presents first-stage IV estimates, capturing the effect of the reform on age at last employment (equation 1). Panel B shows second-stage results regarding the probability of dying between ages 60 and 69. First, the reduced-form estimates illustrate the direct effect of the reform on mortality (equation 2), followed by the instrumental variable estimates derived from equation 3. The estimation sample includes individuals who started contributing within 12 months before or after 1 January 1967. All regressions include controls for gender, birth year, and birth month fixed effects. Additional controls include average monthly contributions, fraction of time employed, fraction of time active, fraction of time in self-employment (all measured between ages 30 and 40), and fixed effects for initial occupation and industry. Standard errors are clustered at the birth-year level, and wild-bootstrap p-values are provided in brackets.
*** p<0.01, ** p<0.05, * p<0.1.

Table 5: Impact on Mortality by Availability of Flexible Retirement

	23-32 Years of Employment (1)	33-42 Years of Employment (2)
	Partial Retirement	
Contributed in 1967	0.001	0.041***
	(0.003)	(0.005)
	[0.683]	[0.002]
	Age at Last Employment	
First Stage:	0.328*	0.363***
Contributed in 1967	(0.100)	(0.054)
	[0.076]	[0.002]
	Probability of Dying between 60 and 69	
Reduced Form:	0.014**	0.002
Contributed in 1967	(0.007)	(0.002)
	[0.049]	[0.306]
IV:	0.044**	0.006
Impact of Age at Last Employment	(0.020)	(0.006)
	[0.040]	[0.328]
Month-Year Birth FE	✓	✓
Controls	✓	✓
Contributed 1966-1967	✓	✓
Observations	10,480	21,216
Mean Dep. Variable (Treated)	0.123	0.075
Mean Dep. Variable (Control)	0.101	0.071
F-stat FS	10.732	45.523
P-value Difference (IV Est.)	0.068	

Source: MCVL, cohorts 1940-1949.
Notes: This table presents the impact of age at last employment on the probability of dying between ages 60 and 69 (conditional on survival until age 59), separately for individuals with 23–32 years (Column 1) and 33–42 years (Column 2) of employment by the end of our observation period. Panel A shows the reform's effect on the probability of claiming a partial pension (based on regression 1). Panel B presents first-stage IV estimates, capturing the effect of the reform on age at last employment (equation 1). Panel C shows second-stage results regarding the probability of dying between ages 60 and 69. First, the reduced-form estimates illustrate the direct effect of the reform on mortality (equation 2), followed by the instrumental variable estimates derived from equation 3. The estimation sample includes individuals who started contributing within 12 months before or after 1 January 1967. All regressions include controls for gender, birth year, and birth month fixed effects. Additional controls include average monthly contributions, fraction of time employed, fraction of time active, fraction of time in self-employment (all measured between ages 30 and 40), and fixed effects for initial occupation and industry. Standard errors are clustered at the birth-year level, and wild-bootstrap p-values are provided in brackets.
*** p<0.01, ** p<0.05, * p<0.1.

Table 6: Impact on Mortality by Gender

	Age at Last Employment	
	Men	Women
	(1)	(2)
First Stage:	0.306***	0.367**
Contributed in 1967	(0.060)	(0.158)
	[0.001]	[0.038]
	Probability of Dying between 60 and 69	
Reduced Form:	0.009**	0.009***
Contributed in 1967	(0.002)	(0.003)
	[0.028]	[0.002]
IV:	0.028**	0.026*
Impact of Age at Last Employment	(0.008)	(0.015)
	[0.015]	[0.082]
Month-Year Birth FE	✓	✓
Controls	✓	✓
Contributed 1966-1967	✓	✓
Observations	29,894	9,287
Mean Dep. Variable (Treated)	0.101	0.048
Mean Dep. Variable (Control)	0.093	0.039
F-stat FS	25.891	5.410
P-value Difference (IV Est.)	0.894	

Source: MCVL, cohorts 1940-1949.
Notes: This table reports the impact of age at last employment on the probability of dying between ages 60 and 69 (conditional on survival until age 59) for men (Column 1) and women (Column 2) separately. Panel A presents first-stage IV estimates, capturing the effect of the reform on age at last employment (equation 1). Panel B shows second-stage results regarding the probability of dying between ages 60 and 69. First, the reduced-form estimates illustrate the direct effect of the reform on mortality (equation 2), followed by the instrumental variable estimates derived from equation 3. The estimation sample includes individuals who started contributing within 12 months before or after 1 January 1967. All regressions include controls for birth year, and birth month fixed effects. Additional controls include average monthly contributions, fraction of time employed, fraction of time active, fraction of time in self-employment (all measured between ages 30 and 40), and fixed effects for initial occupation and industry. Standard errors are clustered at the birth-year level, and wild-bootstrap p-values are provided in brackets.
*** p<0.01, ** p<0.05, * p<0.1.

Online Appendix

The Effect of Removing Early Retirement on Mortality

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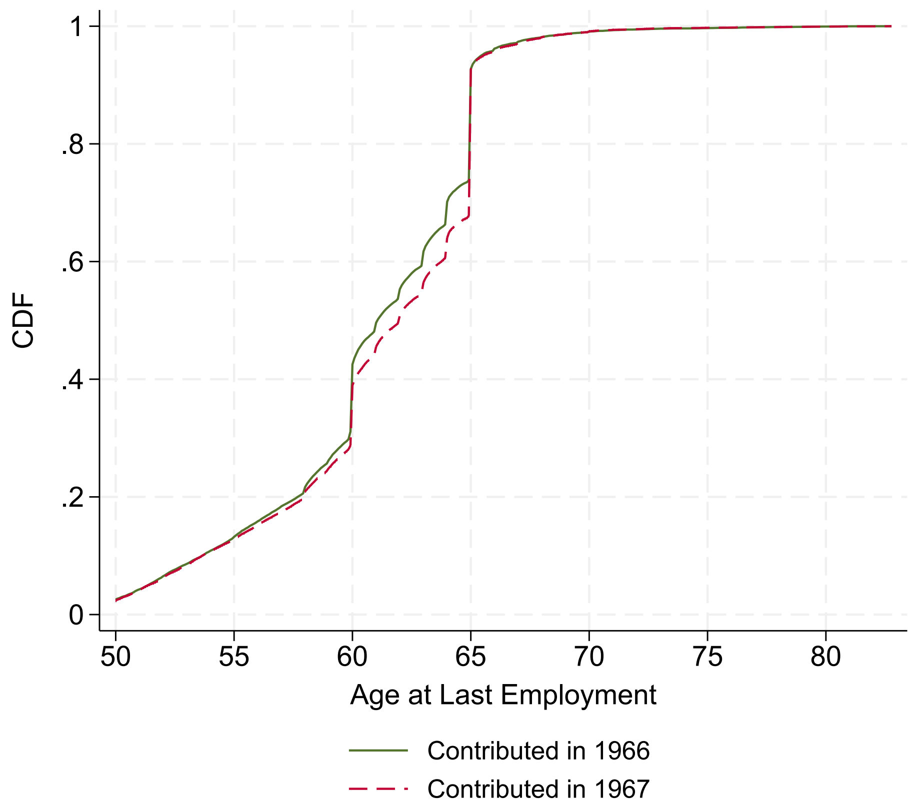
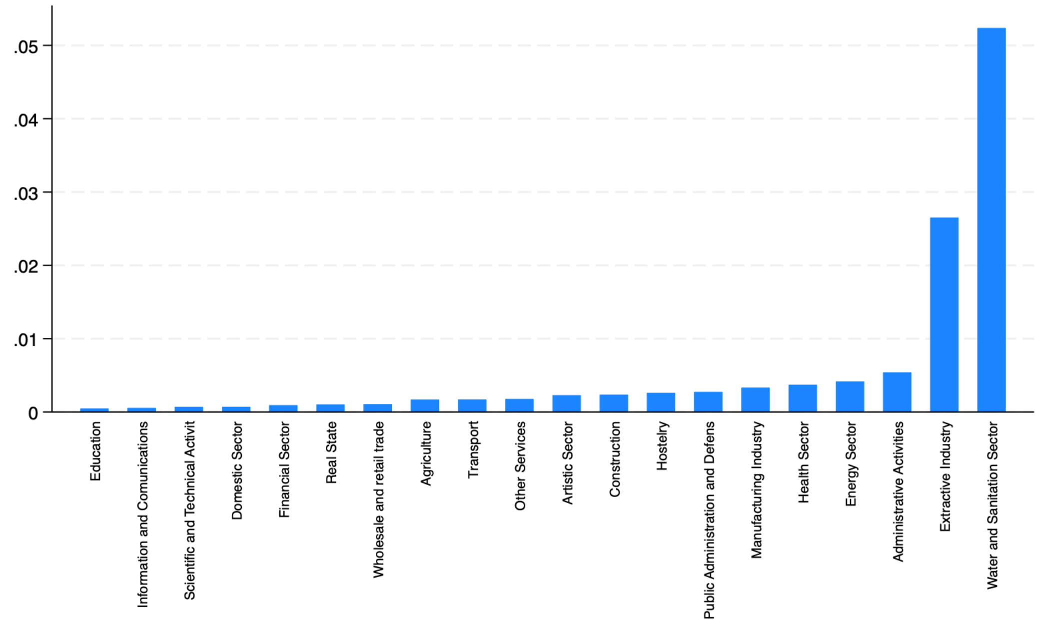
A Appendix Tables and Figures

Figure A1: Cumulative Distribution Function of Age at Last Employment by Treatment Status

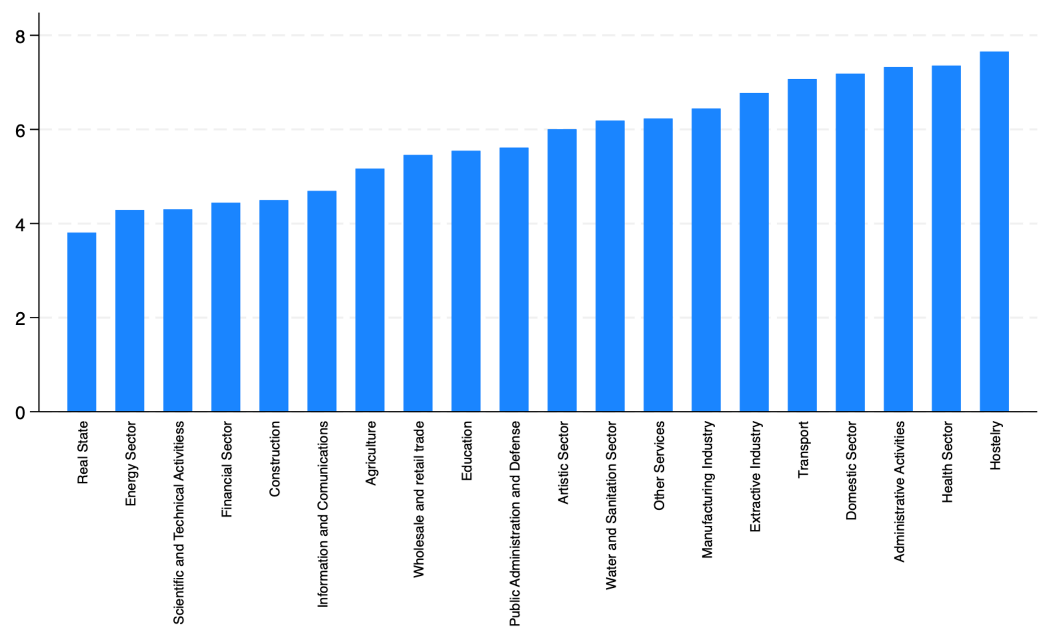


Source: MCVL, cohorts 1940-1949.
Notes: This figure plots the cumulative distribution function (CDF) of age at last employment, separated by treatment status. The solid green line represents individuals who started contributing in 1966, and the dashed red line represents those who began contributing in 1967.

Figure A2: Classification of Industries



(a) By Incidence of Workplace Accidents

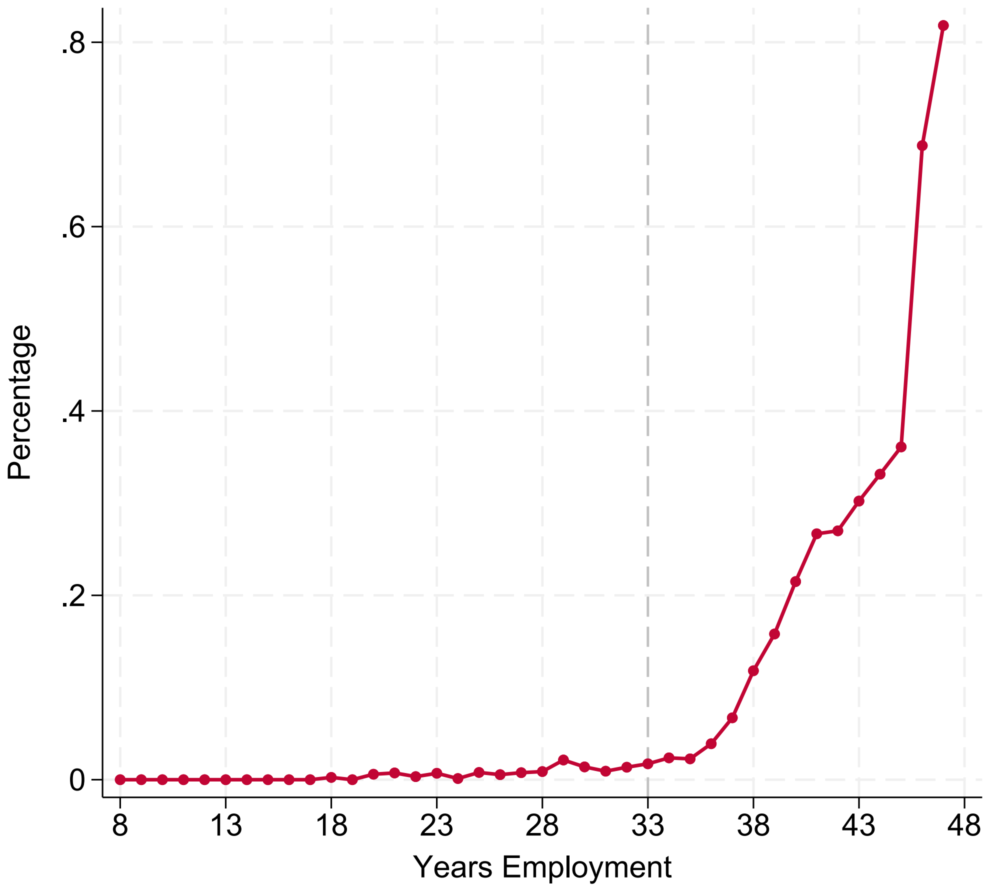


(b) By Psychosocial Exposure

Source: Register of Workplace Accidents 2003-2019 or MCVL, cohorts 1940-1949

Notes: Figure (a) displays the incidence of workplace accidents per 1,000 workers between 2003 and 2019, classified by industry sector based on individuals born between 1940 and 1949. Figure (b) shows industry sectors categorized by psychosocial exposure, which includes mental stress, social stress, and temporal load, following definitions provided by Kroll (2011).

Figure A3: Probability of Claiming Partial Pension by Years of Employment



Source: MCVL, cohorts 1940-1949.

Notes: This figure shows the proportion of individuals claiming a partial pension, as a function of their total number of employment years contributing to the Social Security system.

Table A1: Sample Selection

	Sample Selection				
	Dropped From	Mortality in Observations Dropped between			
	Main Sample	50-74	50-59	60-69	70-74
	(1)	(2)	(3)	(4)	(5)
Contributed in 1966	0.001	-0.014	-0.007	0.009	-0.016
	(0.004)	(0.015)	(0.008)	(0.009)	(0.015)
	[0.818]	[0.357]	[0.416]	[0.350]	[0.407]
Month-Year Birth FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓
Observations	42,675	1,938	1,938	1,877	1,793
R^2	0.104	0.110	0.108	0.048	0.052
Mean Dep. Variable (Treated)	0.045	0.209	0.039	0.088	0.084
Mean Dep. Variable (Control)	0.046	0.193	0.037	0.076	0.083

Source: MCVL, cohorts 1940-1949.
Notes: This table presents the impact of the reform based on regression 1. Column 1 presents the impact on the probability of exclusion from the main sample due to having claimed either a disability pension before age 50 or the residual SOVI pension. Columns 2 to 5 show the reform's impact on mortality risk among individuals excluded from the main sample, specifically for ages: 50–74 (Column 2), 50–59 (Column 3), 60–69 conditional on survival until age 59 (Column 4), and 70–74 conditional on survival until age 69 (Column 5).The estimation sample includes individuals who started contributing within 12 months before or after 1 January 1967. All regressions include controls for gender, birth year, and birth month fixed effects. Additional controls include average monthly contributions, fraction of time employed, fraction of time active, fraction of time in self-employment (all measured between ages 30 and 40), and fixed effects for initial occupation and industry. Standard errors are clustered at the birth-year level, and wild-bootstrap p-values are provided in brackets.

Table A2: Robustness on Sample Selection

	Baseline Sample Drop Disability < 50 y.o. SOVI		Drop Disability < 50 y.o.		Drop SOVI		No Drop	
	Age Last Employment (1)	Mortality Age 60-69 (2)	Age Last Employment (3)	Mortality Age 60-69 (4)	Age Last Employment (5)	Mortality Age 60-69 (6)	Age Last Employment (7)	Mortality Age 60-69 (8)
Contributed in 1967	0.328***	0.009***	0.330***	0.009***	0.344***	0.008**	0.345***	0.008***
	(0.035)	(0.002)	(0.032)	(0.002)	(0.039)	(0.002)	(0.035)	(0.002)
	[0.000]	[0.008]	[0.000]	[0.007]	[0.000]	[0.011]	[0.000]	[0.008]
IV: Age at Last Employment		0.025***		0.025***		0.023***		0.024***
		(0.006)		(0.005)		(0.005)		(0.005)
		[0.002]		[0.001]		[0.004]		[0.000]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓	✓	✓
Observations	40,737	39,181	41,109	39,493	42,290	40,733	42,675	41,058
Mean Dep. Variable (Treated)	61.062	0.090	61.035	0.091	61.264	0.088	61.238	0.088
Mean Dep. Variable (Control)	60.753	0.079	60.723	0.079	60.941	0.076	60.913	0.076
F-stat FS		83.200		99.989		74.157		91.269

Source: MCVL, cohorts 1940-1949.

Notes: This table presents estimates of the reform's impact on the age at last employment (Columns 1, 3, 5, and 7), and the probability of dying between ages 60 and 69 conditional on survival until age 59 (Panel A of Columns 2, 4, 6, and 8), derived from regressions 1 and 2. Panel B of Columns 2, 4, 6, and 8 reports instrumental variable estimates of the effect of age at last employment on mortality risk (ages 60–69, conditional on survival until age 59), based on regression 3. Columns 1 and 2 exclude individuals who claimed either a disability pension before age 50 or a residual SOVI pension (baseline sample). Columns 3 and 4 exclude only individuals who claimed a disability pension before age 50. Columns 5 and 6 exclude only individuals who received a SOVI pension. Columns 7 and 8 include all individuals without exclusions. The estimation sample includes individuals who started contributing within 12 months before or after 1 January 1967. All regressions include controls for gender, birth year, and birth month fixed effects. Additional controls include average monthly contributions, fraction of time employed, fraction of time active, fraction of time in self-employment (all measured between ages 30 and 40), and fixed effects for initial occupation and industry. Standard errors are clustered at the birth-year level, and wild-bootstrap p-values are provided in brackets.

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

Table A3: Impact of the Reform on Predetermined Covariances

	Reduced-form Impact of Contributed in 1967			
	Coefficient	S.E.	Wild-bootstrap p-value	Mean Dep. Variable (Control)
Labor Market Outcomes between the Ages of 30 and 40				
Fraction Active	0.022	(0.284)	[0.941]	94.742
Fraction Employed	-0.034	(0.416)	[0.940]	92.104
Fraction Self-employed	1.395^{***}	(0.274)	[0.002]	5.506
Average Monthly Contribution	-19.261	(24.669)	[0.482]	856.922
First Occupation				
Low Skilled	0.003	(0.011)	[0.798]	0.623
First Industry				
Primary Sector	-0.005	(0.003)	[0.113]	0.017
Secondary Sector	0.019	(0.016)	[0.113]	0.330
Construction Sector	0.010^{**}	(0.002)	[0.010]	0.060
Tertiary Sector	0.004	(0.008)	[0.550]	0.295

Source: MCVL, cohorts 1940-1949.
Notes: This table reports the impact of the reform on a list of predetermined variables obtained from the estimation of regression 1. These variables include various labor market outcomes measured between ages 30 and 40, as well as individuals' occupation and industry at the time of their first social security contribution. The estimation sample includes individuals who started contributing within 12 months before or after 1 January 1967. Standard errors are clustered at the birth-year level, and wild-bootstrap p-values are provided in brackets. The p-value of the joint significance test for all the covariates is 0.336.
*** p<0.01, ** p<0.05, * p<0.1.

Table A4: Descriptive Statistics

	All Sample		Control Group		Treated Group	
Dependent Variable	N	Mean	N	Mean	N	Mean
Regular Pension	40,737	0.70	21,778	0.72	18,959	0.68
Partial Pension	40,737	0.07	21,778	0.06	18,959	0.09
Disability Pension	40,737	0.14	21,778	0.14	18,959	0.15
No Pension	40,737	0.06	21,778	0.06	18,959	0.07
Age at Last Employment	40,737	60.89	21,778	60.75	18,959	61.06
Age at First Pension	37,902	61.86	20,278	61.76	17,624	61.97
Age at Regular Pension	28,695	62.87	15,740	62.70	12,955	63.08
Age at Disability Pension	6,017	57.38	3,127	57.23	2,890	57.54
Age at Partial Pension	3,190	61.17	1,411	61.3	1,779	61.08
Dying between 50-74	40,737	0.20	21,778	0.19	18,959	0.21
Dying between 50-59	40,737	0.03	21,778	0.03	18,959	0.03
Dying between 60-69	39,181	0.08	20,975	0.07	18,206	0.09
Dying between 70-74	35,416	0.08	19,100	0.08	16,316	0.08
Age at Death	8,318	66.27	4,042	66.35	4,042	66.19

Source: MCVL, cohorts 1940-1949.
Notes: This table reports summary statistics for the main outcome variables. The sample corresponds to individuals born between 1940 and 1949, registered in the Social Security (contributive workers and pensioners) at any point of their lives till September 2023. We further restrict the same to individuals that do not receive a pension before the age of 50, and do not receive a SOVI pension. The analysis sample includes individuals whose first contribution occurred within 12 months before or after January 1, 1967. The control group comprises individuals who first contributed in 1966; the treated group comprises those who first contributed in 1967.

Table A5: Impact of the Reform on Other Outcomes

	Reduced-form Impact of Contributed in 1967			
	Coefficient	S.E.	Wild-bootstrap p-value	Mean Dep. Variable (Control)
Probability of Leaving the Labor Market				
between Ages 50–54	-0.002	(0.002)	[0.263]	0.104
between Ages 55–59	-0.025^{***}	(0.003)	[0.002]	0.182
at Age 60	-0.015^{**}	(0.007)	[0.043]	0.170
at Age 61	0.000	(0.003)	[0.984]	0.055
at Age 62	-0.006^{*}	(0.003)	[0.077]	0.056
at Age 63	-0.010^{**}	(0.003)	[0.015]	0.070
at Age 64	-0.007	(0.004)	[0.118]	0.075
at Age 65	0.065^{***}	(0.003)	[0.000]	0.220
after Age 65	-0.001	(0.002)	[0.648]	0.073
Probability of Claiming a Disability Pension				
between Ages 50–54	-0.001	(0.002)	[0.650]	0.041
between Ages 55–59	-0.001	(0.002)	[0.707]	0.065
between Ages 60–64	0.008^{**}	(0.002)	[0.012]	0.037
after Age 65	0.000	(0.000)	[0.227]	0.000
Probability of Claiming a Regular Pension				
at Age 60	-0.045^{***}	(0.006)	[0.000]	0.224
at Age 61	-0.003	(0.002)	[0.138]	0.045
at Age 62	-0.010^{***}	(0.003)	[0.004]	0.053
at Age 63	-0.013^{***}	(0.002)	[0.000]	0.056
at Age 64	-0.015^{***}	(0.003)	[0.003]	0.075
at Age 65	0.062^{***}	(0.004)	[0.000]	0.225
after Age 65	0.019^{***}	(0.006)	[0.002]	0.116

Source: MCVL, cohorts 1940-1949.
Notes: This table reports the impact of the reform on the listed outcomes, obtained from the estimation of regression 1. The estimation sample includes individuals who started contributing within 12 months before or after 1 January 1967. All regressions include controls for gender, birth year, and birth month fixed effects. Additional controls include average monthly contributions, fraction of time employed, fraction of time active, fraction of time in self-employment (all measured between ages 30 and 40), and fixed effects for initial occupation and industry. Standard errors are clustered at the birth-year level, and wild-bootstrap p-values are provided in brackets.
*** p<0.01, ** p<0.05, * p<0.1.

Table A6: Impact of the Reform on Other Outcomes (continued)

	Reduced-form Impact of Contributed in 1967			
	Coefficient	S.E.	Wild-bootstrap p-value	Mean Dep. Variable (Control)
First Pension Benefit				
Pension Benefit	−19.453***	(4.490)	[0.004]	1177.740
Base Pension	−43.586***	(8.503)	[0.004]	1394.547
Percent of Base Pension	1.071***	(0.57)	[0.004]	76.112
Type of Disability Pension				
Severe or Absolute Disability	0.001	(0.002)	[0.648]	0.066
Partial or Occupational disability	0.005**	(0.002)	[0.013]	0.078
Reasons for Claiming No Pension				
Still Working	0.000	(0.000)	[0.674]	0.001
Became Inactive	0.001	(0.002)	[0.445]	0.031
Dies before Pension	0.003	(0.002)	[0.134]	0.037
Ages at Claiming Different Pensions				
Age at Claiming First Pension	0.296***	(0.022)	[0.000]	61.769
Age at Claiming Regular Pension	0.450***	(0.048)	[0.000]	62.711
Age at Claiming Disability Pension	0.304**	(0.095)	[0.039]	57.243
Age at Claiming Partial Pension	−0.175**	(0.075)	[0.048]	61.300
Labor Market Outcomes between the Ages of 45 and 55				
Fraction Active	0.056	(0.123)	[0.649]	96.800
Fraction Employed	0.243	(0.212)	[0.289]	88.770
Fraction Self-employed	0.016	(0.316)	[0.972]	11.482
Average Monthly Contribution	−59.482***	(9.435)	[0.002]	1365.231
Low-skilled Occupation	0.038***	(0.004)	[0.001]	0.470
Labour Market Conditions Before Retirement				
High Workplace Accident Industry	0.002	(0.005)	[0.804]	0.538
High Psychosocial Burden Industry	0.006	(0.008)	[0.599]	0.509
High-skilled Occupation	−0.036***	(0.005)	[0.003]	0.445
Employed more than 33 Years	0.014	(0.010)	[0.191]	0.665

Source: MCVL, cohorts 1940-1949.

Notes: This table reports the impact of the reform on the listed outcomes, obtained from the estimation of regression 1. The estimation sample includes individuals who started contributing within 12 months before or after 1 January 1967. All regressions include controls for gender, birth year, and birth month fixed effects. Additional controls include average monthly contributions, fraction of time employed, fraction of time active, fraction of time in self-employment (all measured between ages 30 and 40), and fixed effects for initial occupation and industry. Standard errors are clustered at the birth-year level, and wild-bootstrap p-values are provided in brackets.

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

Table A7: Impact of the Reform on Alternative Measures of Mortality

	Probability of Dying Before				
	55	60	65	70	74
	(1)	(2)	(3)	(4)	(5)
Reduced Form:					
Contributed in 1967	0.000	0.001	0.005**	0.012***	0.009*
	(0.001)	(0.002)	(0.002)	(0.002)	(0.004)
	[0.951]	[0.581]	[0.042]	[0.002]	[0.080]
OLS:					
Impact of Age at Last Employment	-0.005***	-0.010***	-0.014***	-0.016***	-0.018***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
IV:					
Impact of Age at Last Employment	0.000	0.004	0.016**	0.035***	0.029*
	(0.002)	(0.006)	(0.005)	(0.004)	(0.012)
	[0.951]	[0.576]	[0.027]	[0.000]	[0.097]
Observations	40,737	40,737	40,737	40,737	40,737
Mean Dep. Variable (Treated)	0.013	0.040	0.083	0.139	0.198
Mean Dep. Variable (Control)	0.013	0.037	0.074	0.123	0.180
F-stat FS	88.790	88.790	88.790	88.790	88.790

Source: MCVL, cohorts 1940-1949.
Notes: This table reports the impact of age at last employment on mortality risk, measured as the probability of dying before age 55 (Column 1), 60 (Column 2), 65 (Column 3), 70 (Column 4), and 74 (Column 5). Panel A presents the reduced-form estimates of the reform's direct effect on mortality (based on regression 2). Panel B shows the ordinary least squares correlation between age at last employment and mortality. Panel C reports the instrumental variable estimates, derived from regression 3. The estimation sample includes individuals who started contributing within 12 months before or after 1 January 1967. All regressions include controls for gender, birth year, and birth month fixed effects. Additional controls include average monthly contributions, fraction of time employed, fraction of time active, fraction of time in self-employment (all measured between ages 30 and 40), and fixed effects for initial occupation and industry. Standard errors are clustered at the birth-year level, and wild-bootstrap p-values are provided in brackets.
*** p<0.01, ** p<0.05, * p<0.1.

Table A8: Impact of Age at Last Employment on Mortality with Different Controls

	Probability of Dying between the Ages 60 and 69			
	(1)	(2)	(3)	(4)
IV: Age at Last Employment	0.039***	0.025***	0.020***	0.024***
	(0.009)	(0.006)	(0.005)	(0.005)
	[0.002]	[0.002]	[0.003]	[0.003]
Contributed 1966-1967	✓	✓	✓	✓
Month-Year Birth FE	✓	✓	✓	✓
Controls		✓	✓	✓
Proxy Pension Base			✓	✓
LM Controls 45-55				✓
Observations	39,181	39,181	39,181	39,181
Mean Dep. Variable (Treated)	0.090	0.090	0.090	0.090
Mean Dep. Variable (Control)	0.079	0.079	0.079	0.079
F-stat FS	29.185	83.200	106.683	129.840

Source: MCVL, cohorts 1940-1949.
Notes: This table reports the impact of age at last employment on the probability of dying between the ages of 60 and 69 (conditional on surviving until age 59) with no controls (Column 1), with controls (average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, measured when the individuals were between 30 and 40 years old, and first occupation and industry fixed effects) (Column 2), controlling also for the proxy of the base of the pension benefit (Column 3), and adding as controls also the labour market outcomes when the individuals were between 45 and 55 years old (Column 4), obtained from the estimation of regression 3. The estimation sample includes individuals who started contributing within 12 months before or after 1 January 1967. All regressions include controls for gender, birth year, and birth month fixed effects. Standard errors are clustered at the birth-year level, and wild-bootstrap p-values are provided in brackets.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A9: Characteristics of Compliers

Characteristics	$P[X=x]$	$P[X=x \vert complier]$	$\frac{P[X=x \vert complier]}{P[X=x]}$
Men	0.767	0.608	0.792
Women	0.232	0.294	1.26
Labor Market Outcomes between the Ages of 30 and 40			
Fraction of Being Active below Median	0.052	-0.020	-0.384
Fraction of Being Active above Median	0.947	1.01	1.07
Prob. of Being Employed below Median	0.199	0.026	0.130
Prob. of being employed above Median	0.800	0.966	1.208
Probability of Being Self-employed below Median	0.902	0.841	0.933
Probability of Being Self-employed above Median	0.097	0.056	0.576
Prob. of Having an Avg. Contribution below Median	0.499	0.617	1.235
Prob. of Having an Avg. Contribution above Median	0.500	0.324	0.648
First Occupation			
Low-skilled	0.621	0.623	1.002
High-skilled	0.288	0.356	1.235
Labour Market Conditions Before Retirement			
High Workplace Accident Industry	0.573	0.499	0.870
Low Workplace Accident Industry	0.426	0.555	1.302
High Psychosocial Burden Industry	0.548	0.506	0.924
Low Psychosocial Burden Industry	0.451	0.518	1.146

Source: MCVL, cohorts 1940-1949.
Notes: This table shows the first stage, marginal distribution, complier distribution and relative likelihood for different subgroups. Compliance is defined as exiting the labor market after age 61.
*** p<0.01, ** p<0.05, * p<0.1.

Table A10: Robustness: Retirement Outcomes

	Type of Pension				Age at
	Regular Pension	Partial Pension	Disability Pension	No Pension	Last Employment
	(1)	(2)	(3)	(4)	(5)
Panel A: Revision Date First Contribution					
Contributed in 1967	-0.142***	0.062***	0.052***	0.028***	0.652***
	(0.020)	(0.008)	(0.009)	(0.005)	(0.167)
	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]
Observations	35,876	35,876	35,876	35,876	35,876
Mean Dep. Variable (Control)	0.723	0.065	0.144	0.069	60.753
Panel B: Age Start FE					
Contributed in 1967	-0.052***	0.038***	0.010**	0.005	0.378**
	(0.005)	(0.003)	(0.003)	(0.003)	(0.120)
	[0.001]	[0.000]	[0.018]	[0.239]	[0.042]
Observations	40,737	40,737	40,737	40,737	40,737
Mean Dep. Variable (Control)	0.723	0.065	0.144	0.069	60.753
Panel C: No Controls					
Contributed in 1967	-0.033***	0.023***	0.006*	0.003	0.216***
	(0.002)	(0.003)	(0.003)	(0.001)	(0.043)
	[0.000]	[0.000]	[0.067]	[0.121]	[0.004]
Observations	40,737	40,737	40,737	40,737	40,737
Mean Dep. Variable (Control)	0.723	0.065	0.144	0.069	60.753
Panel D: Dropping the self-employed					
Contributed in 1967	-0.046***	0.032***	0.004	0.011***	0.249***
	(0.004)	(0.003)	(0.004)	(0.002)	(0.040)
	[0.001]	[0.000]	[0.366]	[0.002]	[0.003]
Observations	35,861	35,861	35,861	35,861	35,861
Mean Dep. Variable (Control)	0.715	0.072	0.136	0.077	60.438
Panel E: Augmented sample 1965-1968					
Contributed after 1967	-0.037***	0.022***	0.005*	0.010***	0.342***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.049)
	[0.001]	[0.000]	[0.078]	[0.001]	[0.003]
Observations	59,746	59,746	59,746	59,746	59,746
Mean Dep. Variable (Control)	0.723	0.065	0.144	0.069	60.753

Source: MCVL, cohorts 1940-1949.

Notes: This table presents the estimated impact on retirement decisions under alternative model specifications and sample selections. Panel A excludes individuals whose recorded start date is post-1966 but who are flagged as having contributed to the pre-1967 system. Panel B replaces the month and year of birth fixed effects with fixed effects for the age at first contribution. Panel C excludes the set of predetermined control variables. Panel D restricts the sample by excluding individuals affiliated with self-employed pension regimes. While Panels A to D include individuals who began contributing in 1966 or 1967, Panel E expands the baseline sample to include individuals who started contributing between 1965 and 1968. All regressions control for gender, month and year of birth fixed effects (except Panel A), and predetermined variables (except Panel B). Standard errors are clustered by age at first contribution in Panel B, and by year of birth in Panels A and C to E. Wild-bootstrap p-values are reported in brackets.

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

Table A11: Robustness: Mortality Outcomes

	Probability of Dying between the Ages 60 and 69				
	Revision Date First Contribution	Age Start FE	No Controls	Drop Self-employed	Contributed 1965-1968
	(1)	(2)	(3)	(4)	(5)
Reduced Form: Contributed in 1967	0.018***	0.008***	0.009**	0.011***	0.011***
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
	[0.000]	[0.008]	[0.011]	[0.002]	[0.002]
IV: Age at Last Employment	0.023***	0.021**	0.039***	0.042***	0.017**
	(0.005)	(0.010)	(0.009)	(0.009)	(0.007)
	[0.001]	[0.041]	[0.002]	[0.000]	[0.049]
Month-Year Birth FE	✓		✓	✓	✓
Age Start Contributing FE		✓			
Controls	✓	✓		✓	✓
Contributed 1966-1967	✓	✓	✓	✓	
Contributed 1965-1968					✓
Observations	34,320	39,181	39,181	34,371	57,432
Mean Dep. Variable (Treated)	0.101	0.090	0.090	0.094	0.090
Mean Dep. Variable (Control)	0.079	0.079	0.079	0.081	0.081
F-stat FS	17.138	11.155	29.185	43.104	48.368

Source: MCVL, cohorts 1938-1949.
Notes: This table reports the impact of age at last employment on the probability of dying between the ages of 60-69 (conditional on surviving until age 59) under alternative model specifications and sample selections. Column 1 excludes individuals whose recorded start date is post-1966 but who are flagged as having contributed to the pre-1967 system. Column 2 replaces the month and year of birth fixed effects with fixed effects for the age at first contribution. Column 3 excludes the set of predetermined control variables. Column 4 restricts the sample by excluding individuals affiliated with self-employed pension regimes. While Columns 1 to 4 include individuals who began contributing in 1966 or 1967, Column 5 expands the baseline sample to include individuals who started contributing between 1965 and 1968. All regressions control for gender, month and year of birth fixed effects (except column 2), and predetermined variables (except column 3). Standard errors are clustered by age at first contribution in column 2, and by year of birth in columns 1 and 3 to 5. Wild-bootstrap p-values are
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A12: Placebo: Impact by Availability of Flexible Retirement

	Non-eligible for Partial Pension		Eligible for Partial Pension	
	18-25 Years of Employment	26-32 Years of Employment	33-37 Years of Employment	38-42 Years of Employment
	(1)	(2)	(3)	(4)
	Partial Retirement			
Contributed in 1967	-0.001	0.001	0.017**	0.059***
	(0.002)	(0.003)	(0.004)	(0.007)
	[0.697]	[0.711]	[0.037]	[0.002]
	Age at Last Employment			
First Stage:	0.518**	0.225*	0.302**	0.349***
Contributed in 1967	(0.145)	(0.087)	(0.066)	(0.077)
	[0.036]	[0.078]	[0.014]	[0.002]
	Probability of Dying between 60 and 69			
Reduced Form:	0.022*	0.011	0.005	0.002
Contributed in 1967	(0.009)	(0.009)	(0.005)	(0.003)
	[0.070]	[0.244]	[0.416]	[0.438]
IV:	0.042*	0.050	0.017	0.006
Impact of Age at Last Employment	(0.018)	(0.039)	(0.016)	(0.008)
	[0.059]	[0.176]	[0.444]	[0.444]
Month-Year Birth FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓
Observations	4,830	8,287	12,863	10,026
Mean Dep. Variable (Treated)	0.129	0.120	0.094	0.057
Mean Dep. Variable (Control)	0.096	0.103	0.086	0.054
F-stat FS	12.824	6.712	21.166	20.824
P-value Difference (IV Est.)	0.883		0.870	

Source: MCVL, cohorts 1938-1949.
Notes: This table presents the impact of age at last employment on the probability of dying between ages 60 and 69 (conditional on survival until age 59), separately for individuals with 18-25 (Column 1), 26-32 (Column 2), 33-37 (Column 3), and 38-42 (Column 4) years of employment by the end of our observation period. Panel A shows the reform's effect on the probability of claiming a partial pension (based on regression 1). Panel B presents first-stage IV estimates, capturing the effect of the reform on age at last employment (equation 1). Panel C shows second-stage results regarding the probability of dying between ages 60 and 69. First, the reduced-form estimates illustrate the direct effect of the reform on mortality (equation 2), followed by the instrumental variable estimates derived from equation 3. The estimation sample includes individuals who started contributing within 12 months before or after 1 January 1967. All regressions include controls for gender, birth year, and birth month fixed effects. Additional controls include average monthly contributions, fraction of time employed, fraction of time active, fraction of time in self-employment (all measured between ages 30 and 40), and fixed effects for initial occupation and industry. Standard errors are clustered at the birth-year level, and wild-bootstrap p-values are provided in brackets.
*** p<0.01, ** p<0.05, * p<0.1.

Table A13: Impact of the Reform on Age at Last Employment and Type of Pension by Gender

	Age at	First Pension Claimed			
	Last Employment	Regular Pension	Partial Pension	Disability Insurance	No Pension
	(1)	(2)	(3)	(4)	(5)
Panel A: Men					
Contributed in 1967	0.292***	-0.035***	0.026***	0.007	0.002
	(0.054)	(0.004)	(0.003)	(0.005)	(0.001)
	[0.000]	[0.000]	[0.000]	[0.175]	[0.239]
Observations	31,260	31,260	31,260	31,260	31,260
R^2	0.049	0.032	0.058	0.018	0.006
Mean Dep. Variable (Treated)	61.254	0.673	0.098	0.163	0.067
Mean Dep. Variable (Control)	61.035	0.709	0.071	0.154	0.065
Panel B: Women					
Contributed in 1967	0.349**	-0.043***	0.026***	0.007	0.010
	(0.157)	(0.011)	(0.007)	(0.007)	(0.007)
	[0.036]	[0.004]	[0.008]	[0.346]	[0.143]
Observations	9,477	9,477	9,477	9,477	9,477
R^2	0.069	0.035	0.076	0.021	0.062
Mean Dep. Variable (Treated)	60.290	0.725	0.077	0.112	0.086
Mean Dep. Variable (Control)	59.956	0.761	0.046	0.114	0.079
Month-Year Birth FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓

Source: MCVL, cohorts 1940-1949.
Notes: This table presents the impact of the reform based on regression 1 separately for men (Panel A) and women (Panel B). The outcomes analyzed include the age at which individuals exited their last employment (Column 1) and the probabilities of exiting the labor market through a regular pension (Column 2), a partial pension (Column 3), a disability pension (Column 4), or without claiming any pension (Column 5). The estimation sample includes individuals who started contributing within 12 months before or after 1 January 1967. All regressions include controls for birth year, and birth month fixed effects. Additional controls include average monthly contributions, fraction of time employed, fraction of time active, fraction of time in self-employment (all measured between ages 30 and 40), and fixed effects for initial occupation and industry. Standard errors are clustered at the birth-year level, and wild-bootstrap p-values are provided in brackets.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A14: Impact of the Reform on Mortality by Gender

	Probability of Dying Before				
	55	60	65	70	74
	(1)	(2)	(3)	(4)	(5)
Panel A: Men					
Reduced Form:					
Contributed in 1967	0.000	0.001	0.004**	0.011***	0.011**
	(0.001)	(0.002)	(0.002)	(0.002)	(0.004)
	[0.881]	[0.806]	[0.045]	[0.003]	[0.063]
IV:					
Impact of Age last employment	0.000	0.002	0.015**	0.037***	0.039*
	(0.004)	(0.008)	(0.004)	(0.004)	(0.015)
	[0.885]	[0.805]	[0.015]	[0.000]	[0.074]
Observations	31,260	31,260	31,260	31,260	31,260
Mean Dep. Variable (Treated)	0.014	0.044	0.092	0.155	0.239
Mean Dep. Variable (Control)	0.014	0.043	0.087	0.145	0.228
F-stat FS	29.595	29.595	29.595	29.595	29.595
Panel A: Women					
Reduced Form:					
Contributed in 1967	0.000	0.003	0.007	0.014***	0.004
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)
	[0.938]	[0.368]	[0.125]	[0.006]	[0.467]
IV:					
Impact of Age last employment	0.001	0.009	0.021	0.040*	0.010
	(0.007)	(0.011)	(0.019)	(0.023)	(0.015)
	[0.933]	[0.389]	[0.213]	[0.059]	[0.474]
Observations	9,477	9,477	9,477	9,477	9,477
Mean Dep. Variable (Treated)	0.007	0.022	0.046	0.076	0.111
Mean Dep. Variable (Control)	0.008	0.019	0.039	0.062	0.108
F-stat FS	4.960	4.960	4.960	4.960	4.960
Month-Year Birth FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓

Source: MCVL, cohorts 1940-1949.
Notes: This table reports the impact of age at last employment on mortality risk, measured as the probability of dying before age 55 (Column 1), 60 (Column 2), 65 (Column 3), 70 (Column 4), and 74 (Column 5), separately for men (Panel A) and women (Panel B). Panel A presents the reduced-form estimates of the reform's direct effect on mortality (based on regression 2). Panel B reports the instrumental variable estimates, derived from regression 3. The estimation sample includes individuals who started contributing within 12 months before or after 1 January 1967. All regressions include controls for gender, birth year, and birth month fixed effects. Additional controls include average monthly contributions, fraction of time employed, fraction of time active, fraction of time in self-employment (all measured between ages 30 and 40), and fixed effects for initial occupation and industry. Standard errors are clustered at the birth-year level, and wild-bootstrap p-values are provided in brackets.
*** p<0.01, ** p<0.05, * p<0.1.

B Additional Details on the Spanish Pension System

The current old-pension system for the elderly in Spain is a pay-as-you-go system, with an average replacement rate of around 80%. The key elements of the existing Spanish pension system were established in 1967.[1] Prior to 1967, a fixed-amount pension financed by employers and the state was available for low-income or disabled workers. This pension, which was basic and insufficient, was complemented by the Mutual societies (*Mutualidades Laborales*), which were specific to each occupation/sector.

In 1967, the General Social Security Law (*Ley General de Seguridad Social*) unified the pre-existing insurance systems into a single institution, called 'Social Security'. In the new system, further modified by the 1985, 1997, and the 2002 reforms,[2] the statutory retirement age became 65 years of age. Initially, individuals needed a minimum of eight years of contributions to gain access to the pension, which gradually increased to 15 years after the 1997 reform. The pension benefits were calculated based on the average contributions during the 15 years preceding a claim. In addition, full benefits are given to individuals with 35 contribution years. Finally, the penalty for insufficient years of contributions is 2 percent per year.[3]

It is important to note that in many cases, the claim of a regular retirement pensions is preceded by a period of unemployment that can last for a considerable time. To assist older workers in long unemployment spells, since 1989, those unemployed at age 52 or above who have exhausted their contributive benefits have been allowed to receive unemployment assistance benefits until their pension-claiming age. The only prerequisite is to reach the minimum contribution years to become eligible for an old-age pension. This unemployment assistance paid 75% of the minimum wage. Moreover, a reform in 2002 also created the possibility of combining unemployment insurance claims with labor earnings. Older workers could receive 50% of their unemployment insurance entitlement and work simultaneously, with the employer paying the remaining wages.

All of these rules apply uniformly across our treatment and control groups, with one exception:

[1] It was then further developed in the 1970s. In the last four decades, the system has experienced six important reforms, in 1985, 1997, 2002, 2007, 2011, and 2013. See Boldrin et al. (1999), Boldrin et al. (2004) and García-Gómez et al. (2012) for a detailed explanation of all the reforms of the old-age pension system in Spain.

[2] Ley 26/1985, de 31 de julio, de medidas urgentes para la racionalización de la estructura y de la acción protectora de la Seguridad Social, Ley 24/1997, de 15 de julio, de Consolidación y Racionalización del Sistema de Seguridad Social, and Ley 35/2002, de 12 de julio, de medidas para el establecimiento de un sistema de jubilación gradual y flexible.

[3] The 2011 reform further increased the statutory retirement age and the accrual rules for pension benefits. Starting in 2013, the statutory retirement age was to increase gradually from 65 to 67 in 2027. The reference period for calculating pensions was extended from 15 to 25 years between 2013 and 2022. In 2024, the pension will be calculated on the basis of the average contribution over the last 25 years. Finally, the accrual rate reaches 100% after 35 years for people in the sample, but this number is now 37 as of 2016. Since the youngest cohort is 66 in 2013 and 68 in 2016, individuals in our sample are not affected by the 2011 reform.

the earliest age of pension eligibility. Both groups face the same statutory age (65) and the same minimum contribution requirements (15). The only difference relevant for our analysis is the early-retirement age.

C Additional Details on Data

This paper uses novel administrative data of an extended sample from the Continuous Sample of Working Histories (Muestra Continua de Vidas Laborales (MCVL)) provided by the Spanish Social Security system. The dataset contains a 10% random sample of individuals born between 1940 and 1949 who have registered with the Social Security (such as contributive workers and pensioners) at any point in their lives up to September of 2023.

Therefore, we use a non-publicly available version of the MCVL provided by the Spanish Social Security administration. Access to this data is, however, possible after submitting a formal request via email (solicitudes.sala-investigacion@seg-social.es). A Committee of Experts will evaluate the application. In case it is approved, the necessary data will be prepared, access to which will be allowed through one of their three Safe Data Rooms in Madrid, Barcelona, or Albacete.

There are two main differences between the dataset we use in this paper and the publicly available one. First, the publicly available MCVL is only available from 2005, but it contains all the employment histories of the individuals who had some contact with the Social Security administration since then. Therefore, it is not possible to observe individuals who died or became inactive before that date. The dataset that we use in this paper allows us to observe contributive workers and pensioners prior to 2005. This data advantage makes it possible to explore a representative sample of workers affiliated with the Spanish Social Security at any point in their working lives and examine their mortality responses. Secondly, the reform we examine impacted only individuals born in certain cohorts. Therefore, we asked for a 10% random sample of individuals born in those cohorts, 1938 and 1949. The publicly available MCVL only contains a 4% random sample of all the individuals affiliated with the Social Security administration.