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ABSTRACT: This paper studies the mortality effect of delaying retirement by investigating the impacts of the 1967 Spanish pension reform, which affected the general population and exogenously changed the early retirement age, depending on the date individuals started contributing to the pension system. Using the Spanish administrative data, we find that delaying retirement by one year increases the hazard of dying between the ages of 60 and 69 by 38 percent. We show that the reform leads to higher mortality in all subgroups, and the effects are statistically stronger for those employed in sectors with the highest workplace accidents and for those with low selfvalue jobs. Moreover, we show that allowing flexible retirement mitigates the adverse effects of delaying retirement.

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1 Introduction

Many countries have reformed their public pension systems to cope with population ageing and to maintain financial solvency. One main policy tool is restricting 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. Moreover, the existing empirical evidence mostly draws lessons from policy experiments that allow for earlier retirement (**?????**). Because the effects on mortality from preponing and postponing the retirement age are not necessarily symmetric, these estimates might not generalize to today's policy world, where most policymakers aim to incentivize prolonged working lives. Therefore, it is policy-relevant to understand the impact of delaying retirement on mortality, particularly the effect of closing the early retirement options on mortality.

This paper provides novel empirical evidence on this important issue by investigating a Spanish pension reform. This 1967 reform exogenously changed the early retirement age depending on the date individuals started contributing to the Social Security system. Individuals who contributed to the pension system before 1 January 1967 could voluntarily claim a pension as early as 60 years of age. On the other hand, individuals who started contributing after 1967 could only voluntarily claim a pension at age 65.[2]

This reform has several advantages in answering our research question. First, the discontinuity change in retirement age, based on the year the individuals started contributing to the Social Security system, allows us to credibly identify causal effects. Second, in contrast to most of the previous literature, this reform creates a substantial increase (approximately four years) in the early retirement age and leads to a considerable delay in the exit time of the labor market. Third, the reform affects a more general population compared to existing studies (see, e.g., **???**, all of which

[1] For example, see **?**, **?**, **?**, **?**, **?** and **?** for recent evidence on the direct effects of raising retirement ages.

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

study specific subsets of the population, such as military personnel and civil servants). This feature allows us to capture the mortality responses in the general population and examine the heterogeneous responses of subgroups. Lastly, the treatment was determined at the early stage of a worker's career, which provides a long-term horizon for the expected retirement age to impact mortality if there are some anticipatory responses.

We use a novel version of the Spanish administrative Social Security panel data covering 10% of the cohorts born between 1938 and 1949 who are registered with the Social Security system at any point in time until September 2023. We compare individuals who started contributing one year before 1 January 1967 with those who started one year after that date. Using within-cohort first-difference regression and controlling for a broad list of fixed effects, we find that the reform delays the age at last employment by around half a year.[3] Those who contributed in 1967 are also less likely to claim a regular pension and more likely to claim partial and disability pensions. This indicates that individuals have utilized other ways to leave the labor market earlier when the early retirement schemes are not available anymore. We also show that they have a higher probability of not claiming any pension, driven mainly by premature mortality. More specifically, individuals who started contributing after 1 January 1967 are 2.8 percentage points more likely to die before claiming any pension. To test the causality of our estimates, we use placebo cutoff dates and find no significant impacts on these placebo dates both before and after 1967.

What is the impact of removing early retirement on mortality? We find that individuals who contributed in 1967 have a 3.1 percentage point ($\sim$8%) higher probability of dying between the ages of 50 and 86. When we examine the reform's impact on mortality at different age brackets, we observe that the mortality responses are the strongest between ages when public pensions are not accessible (between the ages of 60 and 64). Removing the early retirement option increases mortality in that age bracket by 2.2 percentage points ($\sim$34%). This result underscores that the adverse impact of postponing retirement on mortality primarily stems from the short-term consequence of

[3] We show our results are robust using several robustness tests, including regression-based tests of the differences in covariates, and using within-age start contributing fixed effects analysis. For more details, see Section **??**.

losing access to early retirement schemes.

Furthermore, we shed some light on the possible mechanisms behind the detrimental effect of delaying retirement on mortality. To better compare the magnitudes, we perform an instrumental variable (IV) estimation, using the year individuals started contributing to the Social Security system as the instrument for the age at last employment. Initially, we examine the impact of age at last employment on the hazard of dying between different age brackets for the entire population. We find that delaying labor market exit by one year increases the hazard of dying between the ages of 60 and 69 by 4.4 percentage points (equivalent to a relative increase of 38%). In our heterogeneity analysis, we concentrate on the repercussions of delaying labor market exit by the individuals' labor market conditions preceding retirement. As the parameters of most jobs are multi-dimensional, we examine four dimensions of individuals' labor environments: the physical burden, psychosocial burden, self-value at work, and occupational skill level.

First, using registered workplace accidents at the industry level as a proxy for physical burden, we show that the increase in mortality is stronger for those who have worked in sectors with a very high share of workplace accidents. This finding is consistent with previous literature establishing that physically demanding occupations lead to adverse health effects (see **?** for a summary). We also find that the mortality effect is stronger for individuals in high psychosocial burden jobs (with a high level of mental and social stress). We measure the psychosocial exposure in a job following the Job Exposure Matrices constructed by **?**. Furthermore, we measure an individual's sense of achievement and recognition at their last job using the Occupational Information Network data (O*NET). We show that only individuals who work in low self-value industries are more likely to die when facing a one-year delay in the labor market exit. This result indicates that individuals who 'feel recognized' and have a sense of achievement in their work do not experience a negative mortality effect due to a delay in the labor market exit. Lastly, similar to previous literature, we find that the mortality effect is concentrated among blue-collar workers. Combined with the heterogeneous effects on age at last employment, this finding indicates that for people with a 'worse' job, working longer has a greater impact on life expectancy, even though they extend

their working lives to a lesser extent. Moreover, the heterogeneous results suggest that advocating for different ages to exit employment, depending on the working conditions of each individual's occupation, can be a potential policy tool to mitigate the detrimental impacts of delayed retirement.

Our findings imply that losing access to early retirement can decrease life expectancy. One proposal to incentivize individuals to stay longer in the labor force without having such a negative impact on their health is to allow them to gradually reduce their working time towards the end of their careers. In Spain, some workers can access partial retirement by working part-time while claiming a partial pension. One of the eligible conditions is to have contributed to the Social Security system for at least 33 years. Comparing individuals with and without access to partial retirement, we find that individuals who have no access to partial pension experience higher mortality rates when the retirement age is delayed. This finding highlights the importance of providing the opportunity for gradual retirement, which can smooth the adverse effects of delaying retirement.

Apart from contributing to studies on the impact of pension reforms on retirement decisions (e.g., **????**), our paper relates to and completes papers studying the mortality effects of retirement. The existing well-identified empirical literature finds mixed results and explores three types of policy experiments: allowing earlier retirement (**?????**), promoting later retirement (**???**) and switching to retirement at the statutory retirement age (**?**).

The studies of earlier retirement overall find no significant impacts on mortality or a reduction in mortality. For example, **?** find that accessing a pension two to five years earlier has no effect on the probability of dying by the ages of 67, 70, 74, and 77 for the entire population of Norway. Looking at some particular population groups, **?** and **?** find a decrease in mortality due to earlier retirement. **?** show that five-year early access to a retirement pension reduces the mortality of male army officers in Sweden. **?** 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 paper that finds (earlier) access to a pension increases mortality is **?**. Using Austrian register data, they estimate the (very) short-term impact of three-year early access to pension on mortality. **?** find that early retirement increases male deaths before the age of 67.

Evidence on the impacts of later retirement is more scarce. Our paper directly contributes to this literature and is the first paper that provides a precisely estimated impact of later retirement induced by a delay in the statutory early retirement age. To the best of our knowledge, only three papers study the effect of delayed retirement. While **?** studies the mortality effect of a two-year increase in the statutory retirement age, they find an imprecisely measured no effect on mortality by the age of 69.[4] The other two papers are **?** and **?**. While they both show precisely estimated impacts of later retirement on mortality, they explore pension reforms that change early retirement financial incentives rather than only removing the early retirement option, as in our setting. **?** find that delaying retirement in France has a zero effect on the probability of dying between the ages of 61 and 79. **?** explore an exogenous decrease in the implicit working tax in Israel and show the impact of work on longevity. They find that later retirement increases mortality between the ages of 75 and 85 but that it has no impact on mortality between the ages of 65 and 74. Our paper differs from **?** and **?**, as we expect the response to pension reforms that incentivize retirement via financial incentives is different from reforms that shut down early retirement schemes.

Our paper is the first one to provide empirical evidence that removing early retirement access increases mortality. When we look at the literature on the health impacts of delayed retirement, it is not surprising to find that delayed retirement reduces life expectancy. Many studies on the health impacts of delayed retirement find retirement is beneficial (e.g., **???** and see, **?** for a recent survey.) through relief from work-related stress and increased sleep and physical activities (**???**). Studies also find a positive impact of retirement on health outcomes due to the adoption of a healthier lifestyle (**???**). Therefore, it is reasonable to expect that later retirement might increase mortality rates.

Our findings have important policy implications. First, we show a large heterogeneity in the effect of delayed retirement on mortality, depending on the characteristics of jobs that the individ-

[4] **?** studies the mortality effect of a two-year increase in the statutory retirement age of local government female workers in Sweden and finds that the reform had no impact on mortality and/or health care utilization. See Section **??** for a more detailed comparison with the existing literature.

uals held before retirement. Going beyond distinguishing between blue- and white-collar jobs, we show that other job dimensions (such as physical, psycho-social, and self-value) also matter. This finding implies that policies that remove access to early retirement for the general population can exacerbate the socioeconomic disparities in life expectancy. Second, we show the option of a gradual transition to retirement is beneficial. Allowing older workers to gradually reduce their working time at the end of their careers can mitigate the adverse effects on mortality. Such mitigating effects can be made possible by promoting gradual retirement options. The results also speak to the recent public discussions on flexible retirement. This insight is also relevant for public policy and budgetary considerations, particularly when policymakers in many countries face long-term solvency challenges in both the pension and public healthcare systems.

This article proceeds as follows: Section **??** presents a brief description of the institutional setting in Spain and the 1967 pension reform. Section **??** describes the data, and Section **??** the empirical strategy. Section **??** and **??** present results on the labor supply responses, plus the instrumental variable estimates of the impact of age at last employment on mortality. We also discuss the heterogeneity and potential mechanisms that may explain the impact on mortality. Section **??** presents all the robustness checks and placebo of our main results. We provide a discussion of the findings in Section **??**. Section **??** concludes.

2 Institutional Setting

The key elements of the existing Spanish pension system were established in 1967, and the relevant rules for our sample were set in the 1985 reform. 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% (one of the highest in the European Union). The statutory retirement age is 65 years of age, and individuals need a minimum of 15 years of contributions to gain access to the pension. Full benefits are given to individuals with more than 35 contribution years, and the penalty for insufficient years of contributions is 2 percent per year. The pension benefits are calculated based on the average

contributions during the 15 years preceding a claim. See Appendix **??** for more details on the different reforms the Spanish pension system has experienced since 1967.

The pension of all individuals born in the same year is regulated by the same pension law. However, individuals who contributed before 1 January 1967,[5] even by one day, had an indefinite right to claim early retirement at the age of 60. These workers have this right because they began contributing before the current old-age pension system was established in 1967. The 1967 law (published on 30 December 1966) that created the new Social Security system maintained this benefit from the old Spanish pension system for only these workers. These individuals could freely claim early retirement from age 60, though with some financial penalties. The penalty for early retirement is 8 percent (or up to 6 percent as a function of the years contributed after the 2002 reform) per year of early claim. Around 13% of the individuals who started contributing in 1966 claimed a regular pension at the age of 60.

On the other hand, individuals who contributed after 1 January 1967 faced a statutory retirement age of 65. They can only claim early retirement under the involuntary early retirement scheme, set in the 2002 law, which allows individuals to claim early retirement at age 61 (again with some financial penalties, between 6 and 8 percent, depending on the years of contribution, per year of advancement) under certain conditions. These individuals need to have been unemployed (involuntarily) for at least six months and have contributed to the Social Security system for at least 30 years. Due to these stringent requirements, a very small proportion of workers have taken up this involuntary early retirement option.

Because the law was published on 30 December 1966, there is little room left to manipulate the date of the first social security contribution. This feature, therefore, allows us to compare individuals who started contributing before and after 1 January 1967. As we can see in Figure **??**, individuals who contributed before 1967 (independently of their birth year) could voluntarily

[5] The January 1967 deadline was set at a later date for workers in specific sectors, such as construction, mining, fishing, and the railway. For workers in these sectors (which constitutes a very small sample), we compare individuals that started contributing 12 months before and after that later date instead of 1967.

claim early retirement at the age of 60. For those who contributed after 1967, the only other way to receive early retirement was to claim involuntary early retirement at the age of 61; otherwise, the earliest an individual can voluntarily claim a pension is at the age of 65. Therefore, we expect individuals who started contributing after 1967 to increase their retirement age considerably.

In addition to the regular retirement pathway, there are two alternative pathways: permanent disability and partial retirement pensions. Permanent disability benefits have been used extensively in Spain as an early retirement mechanism (**??**). Additionally, in 2002, partial retirement options became available, allowing the combination of income from work with old-age pension benefits. The partial retirement option enables individuals aged 60 years and older, with at least 33 years of contribution and six years of tenure in the same company, to claim up to 85% pension while working 15% of the time (up to 75% of benefits after the 2011 reform). In this paper, we investigate the impact of the reform on the age of the individual when claiming disability, partial and regular retirement pensions, and the probabilities of choosing these alternative exit routes from the labor market.

3 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 1938 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.[6]

Therefore, we use a non-publicly available version of the MCVL provided by the Spanish Social Security system, which allows us to observe contributive workers and pensioners prior to 2005 (the starting time of the publicly available version). This data advantage makes it possible to explore

[6] Reliable data on contributions to the Social Security and date of death starts being recorded from 1981 on. These limitations do not restrict our sample, as individuals born in 1938 were 47 years old in 1981.

a representative sample of workers affiliated with the Spanish Social Security at any point in their working lives and examine their mortality responses. See Appendix **??** for more details about the novelty of our dataset and how to obtain it.

The MCVL includes time-invariant information, such as gender, birth month, and birth year. It also contains detailed labor market biographies from the date individuals started contributing to the Social Security system until their death.[7] Moreover, we observe their lifetime employment and unemployment spells, occupations, industry, and monthly contributions till 2023. The pension records from the MCVL contain accurate information on an individual's age at the time of claiming a pension, pension benefits, the type of pension they receive at each point in time, and the total number of contributive years before retirement. When individuals exit from the dataset due to death, we observe the exact date of their death, which helps us measure mortality accurately.

3.1 Sample

Our main sample covers Spanish individuals born between 1938 and 1949 who started contributing to the Social Security system 12 months before and after 1 January 1967. We further restrict our sample to individuals attached to the labor market by including those who still contribute at the age of 50, have not claimed a disability pension before 50, and have at least eight years of contribution.We also drop individuals who have claimed a SOVI pension. A SOVI pension is a residual pension from the old system for individuals who, at the age of 65, are not entitled to a pension from the current Social Security system but can prove that they have contributed at least 1,800 days to the previous system. These individuals could not claim early retirement even though they started contributing before 1967. We drop 20% of the observations with these restrictions.

In Table **??**, we verify that selection into the sample is not affected by the treatment status.

[7] Note that the date that individuals started contributing to the Social Security system coincides with the date at which they started their first formal job. It is important to emphasize that, for some individuals, this date does not correspond to the date they started working (for example, for those who switch from the informal sector to the formal sector).

First, we check whether the reform affects the probability of not being in the main sample, and we find no significant differences. Moreover, we also show no significant differences in mortality between 60 and 64 among those who were dropped from the main sample. In Table **??**, we perform robustness checks by modifying the definition of individuals not attached to the labor market. We show that including individuals who have claimed disability before age 50, have less than eight years of activity during their working life, or who have received a SOVI pension does not impact our results. The final sample contains 26,102 individuals, of whom 27% are female. See Appendix **??** for more details.

3.2 Treatment Status

To identify the treatment status, we need information on the exact date when individuals started contributing. One caveat of the dataset is that the exact date of the first contribution is poorly recorded for some individuals, especially those who started contributing around 1967, as the administrative dataset started to be constructed at the end of the 1960s. The top graph in Figure **??** shows the distribution of years the individuals in our sample started contributing, as recorded in the original dataset. We can observe that there are bunchings in the years 1966 and 1967. Moreover, Figure **??** shows that the bunchings are driven by people who are more likely to be recorded as starting contributing in January 1965, 1966, and December 1966, which are likely due to administrative practices.[8] The monthly distribution appears smooth in the other years, indicating that the "administrative" bunching problem is limited to 1965, 1996, and 1967. In fact, Figure **??**a) suggests that some individuals recorded as starting these years probably contributed to the Social Security system before these dates. We observe that some people who originally were recorded as having started contributing in 1967 claim a regular pension at age 60, which is legally impossible. This limitation is the reason we cannot use a Regression Discontinuity Design.

Fortunately, we can partially correct this measure as we have excellent information on the number of years contributed and the exact date individuals claim a regular pension. To correct the

[8] See Appendix **??** for further discussions of the administrative practices.

reported date of the first contribution, we subtract the total number of years contributed from the year they claim a regular pension. If the corrected year of starting contributions is before the reported date of the first contribution, we make this correction. This correction is only possible for individuals who have claimed a regular pension, as only for them the total number of years contributed is reported. We perform this correction for the years 1965, 1966, and 1967. However, for our main sample, which only includes individuals who started contributing in 1966 or 1967, the correction of 1965 will not matter. This correction changes the treatment status of 3,720 individuals, which corresponds to 14% of our main sample of individuals who started contributing in 1966 or 1967. After this correction, we see in Figure **??** that the bunching has been greatly reduced. Figure **??**b) also shows that, after the correction, fewer individuals started contributing in 1967 and retired early at 60. This means that the number of mistakes in the first year of contribution in 1966 and 1967 has been dramatically reduced with our correction. We also perform several robustness checks by varying the correction methods. We show that the results are similar when we use the reported date without any correction or remove only the months where the bunching is most pronounced (month 12 of 1966 and month 1 of 1967). See Section **??** and Appendix **??** for more discussion about the correction of this variable.

4 Empirical Strategy

We first provide causal estimates of the pension reform on retirement and mortality outcomes using a within-cohort OLS regression with a list of fixed effects and controls. The following equation estimates the reduced form reform impacts, where $Treated_i$ is a dummy that takes the value of one for individuals who started contributing to the Social Security system in 1967 and zero for those who started contributing in 1966. The treated group can claim regular pensions voluntarily at age 65 (involuntarily at 61), while the control group can claim them as early as 60 years of age.

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

R_{icm} represents the labor supply outcomes of individual *i* born in year *c* and month *m* who started contributing in year 1966 or 1967. The outcome variables include the age at last employment, the probability, and the age at which individuals claim the different pensions. α_3 measures the average treatment effect of the reform on labor supply and pension claiming decisions.

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

M_{icm} represents the probability of dying of individual *i* born in year *c* and month *m*. β_3 measures the average treatment effect of the reform on the mortality. δ_c is the year of birth, and μ_m is the month of birth fixed effects. X_{imc} includes a list of fixed effects, such as the highest level of occupation and industry sector between the ages of 30 and 40, and a list of other predetermined covariates, including individuals' mean monthly contribution to the Social Security system, the fraction of days active and employed, and the fraction of time self-employed between the ages of 30 and 40. We cluster the standard errors at the birth year level and report the wild-bootstrap p-values in brackets in all tables.

The critical assumption for estimating the causal impact of pension reform is that the year individuals started contributing to the Social Security system is independent of unobserved characteristics that affect the age at last employment and mortality. The following steps support the causality of our estimates.

First, we restrict our sample to those who started contributing in 1966 and 1967. The treated and control group individuals had similar labor market conditions when they began working: they were born in the same year and started working only one year apart. Second, we include their highest occupation, industry, birth year, and month of birth fixed effects, which allows us to estimate variations within occupation, industry, and birth year.

Furthermore, we check whether the characteristics of the treated and control groups are similar when they are between 30 and 40 years old.[9] Table **??** shows the impact of the treatment on a list

[9] Ideally, we would like to check whether the characteristics of individuals in the treatment and control groups differ at the beginning of their careers (before the age of 30). However, the data quality was not particularly good

of predetermined variables, including the fraction of time spent in employment, activity, and self-employment between the ages of 30 and 40; the probability of working in a blue-collar occupation and industry sectors; and average monthly contributions between the ages of 30 and 40. The estimates are obtained from estimating Equation **??**. Except for the fraction of time spent in self-employment and mean contribution, there are no significant impacts. In addition, the p-value of the joint significance test for all the covariates is 0.305, indicating that all these covariates do not predict treatment status.[10] This suggests that there is no manipulation of the treatment status and that our control and treatment groups are very similar.

To further establish the causality, we perform placebo tests using other years to define treatment status and a robustness test using age at first contribution fixed effect instead of birth month fixed effects. These tests rule out the possibility that other confounding factors drive our reduced form estimates. For more details, see Section **??**.

Then, to quantify the effect of age at last employment on mortality, we also report the instrumental variable (IV) estimate. Estimating the causal effect of the retirement age on mortality is difficult because many unobserved factors can influence both retirement age and mortality. To deal with the endogeneity in retirement behavior, we exploit the exogenous variation in age at least employment provided by the 1967 Spanish pension reform. The IV estimate helps us to understand to what extent and through which channels delaying retirement affects mortality and to compare the estimates across subgroups. The causal effect of age at last employment on mortality is estimated by the following equation. Here R_{icm} is age at last employment and is replaced by the predicted value ($\widehat{R_{icm}}$) in Equation **??**:

when our individuals were that young, so the labor market characteristics during the first years of their careers might have been wrongly recorded for some individuals. We, therefore, look at their characteristics between the ages of 30 and 40.

[10] In Section **??**, we show that the effect of the reform on our main outcomes is robust to excluding from our sample individuals in one of the self-employed pension regimes.

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

We include the same list of controls used in Equations **??** and **??** (δ_c, μ_m, and X_{icm}). The coefficient θ_3 captures the local average treatment effect of age at last employment on mortality among individuals who delayed their retirement because they could not claim a regular pension at age 60.

Three conditions are necessary to interpret the IV estimate showing the impact of delayed retirement on mortality. First, the treatment status is strongly associated with age at last employment. We show the validity and magnitude of the first-stage relationship in Section **??**.

Second, the treatment status only affects mortality through its impact on age at last employment. The exclusion restriction could be violated if contributing after 1967 affects mortality through channels other than retirement age. Two possible additional channels are changes in labor market outcomes close to retirement and changes in pension benefits. We argue that the exclusion restriction assumption is reasonable by showing that controlling for the individuals' proxy of their mean pension[11] or their labor market decisions before retirement (between 45 and 55 years of age) [12]

[11] 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. Moreover, in Table **??**, we show the effect of the reform on this proxy.

[12] Table **??** shows that the reform had an impact on the labor market outcomes of individuals between the ages of 45 and 55. We observe that individuals who started contributing in 1967 spent 2.01 percent more time employed during these years and are 0.6 percent more likely to be active in the labor market. We also find that the individuals affected by the reform have a 1.6 percentage point higher probability of having a blue-collar occupation. They also have a 1.9 percentage point lower probability of working in the trade or transportation sector, 2.6 percentage points of working in the public, health, or education sectors, and 0.9 percentage points of working in the services, hotel, and housekeeping sectors.

does not change the size of our IV estimates to any great extent (Table **??**).

Third, the monotonicity condition requires that starting contributing to the Social Security system in 1967 instead of 1966 always induce people to delay labor market exit or maintain the same age at last employment. Given the nature of the pension reform, this condition is likely to be satisfied. Moreover, Figure **??** shows the cumulative distribution function of the age at last employment for individuals who started contributing in 1966 and those who began contributing in 1967. We can see that the distribution for the treatment group (contributed in 1967) stochastically dominates the distribution of the control group (contributed in 1966). Furthermore, the reform only influenced the probability of labor market exit after 60, with negligible and statistically insignificant effects for ages below 60 (Table **??**). Lastly, Tables **??** and **??** demonstrate that the effect on age at last employment is non-negative for various subgroups in the population. With all this evidence, we are confident that the monotonicity assumption is satisfied in our context.

5 The Reform Effect on Retirement Outcomes

5.1 Descriptive Evidence

Table **??** provides summary statistics for the main outcomes used in our analysis. There are three different pensions that individuals can claim. Table **??** shows that 49% of individuals claim a regular pension (old-age pension), while 31% claim a disability pension, and 4% of individuals choose a partial pension. Some individuals in our sample never claim any pension due to reasons such as a period of prolonged inactivity ($\sim 6\%$), dying before a claim can be made ($\sim 8\%$), and still being active in the labor market in 2023 ($\sim 0.3\%$). Figure **??** compares the share of different types of pensions by treatment status. Compared with those who started contributing in 1966 (control, light green bars), individuals who started contributing in 1967 (treated, darker green bars) have a lower likelihood of claiming a regular pension and are more likely to claim a disability pension, a partial pension, or claim no pension.

On average, individuals leave the labor market at 59.67 years old and claim regular pensions at

63.25 years of age. Figure **??** shows the age distribution at last employment for individuals who started contributing in 1966 and 1967. As expected, we see a distinct difference. Figure **??** shows that around 8% of individuals who started contributing in 1966 (control group, solid red line) leave the labor market at the age of 60, while this percentage is almost zero for those individuals that started contributing in 1967 (treated group, green dashed line). More than 23% of the treated individuals exit the labor market at the age of 65, while this number is only 17% for the control group. We see the same pattern regarding the age of claiming a regular pension. Figure **??**a) shows that 25% of individuals who started contributing in 1966 (control group, solid red line) claim a regular pension at the age of 60, and 32% of them claim at the age of 65. We also see some claims at the ages of 61 to 64. However, for those individuals who started contributing in 1967 (treated group, green dashed line), almost no one claims a regular pension at any age other than 65 years, whilst almost 70% claim a regular pension at 65 years of age. These figures provide visual evidence that the reform is binding and that individuals affected by it delayed their retirement.

In our sample, individuals, on average, claim a disability pension at the age of 57.36 and a partial pension at 61.1 years of age. Figure **??**b) and Figure **??**c) show the distribution of these ages by treatment status. We observe that individuals who started contributing in 1967 (green dashed line) claim more disability insurance between the ages of 60 and 65 than those who started contributing in 1966. Moreover, individuals who started contributing in 1967 (green dashed line) claimed partial pensions at slightly earlier ages.

5.2 Regression Results

Table **??** examines the impact of the reform on the different types of pensions that individuals have claimed. We find that individuals who started contributing to the Social Security in 1967 are less likely to claim a regular pension by 10.5 percentage points ($\sim$ 18.8%), yet their probability of claiming disability insurance increases by 5.8 percentage points ($\sim$ 20%). In Table **??**, we further show that the reform equally impacted the probability of claiming a severe or absolute disability and a partial or professional disability pension (by a 3.1 and 2.7 percentage point increase, respec-

tively).[13] They are also 1.9 percentage points ($\sim$ 54 %) more likely to claim a partial pension. These results indicate that individuals did not fully comply with the rise in statutory retirement age and have utilized other ways to leave the labor market before claiming a regular pension, by either claiming disability insurance or a partial pension.

We also observe that individuals contributing to the pension system in 1967 are 2.8 percentage points ($\sim$ 21%) more likely to leave the labor market without any pension. In Table **??**, we further explore three reasons why individuals might not claim any pension: first, they were still working in 2023; second, they became inactive; third, they died before claiming any pension. Table **??** indicates that the reform has no impact at all on the probability of continuing to work or becoming inactive. Interestingly, individuals who started contributing in 1967 have a 1.9 percentage point ($\sim$ 25%) higher probability of dying before claiming any pension. This finding implies that premature death is the main driver for not claiming any pension. We further explore this effect in Section **??**.

Table **??** examines the impact of the reform on the ages at which individuals leave the labor market and claim different types of pensions. The 1967 reform resulted in the treated individuals delaying their labor market exit by almost half a year and delaying claiming their first pension (regardless of the type) by 0.248 years (four months). Table **??** shows the reform's effect on the probability of exiting the labor market in different age brackets. The reform decreases the probability of leaving the labor market between the ages of 55 and 63. As expected, the reform has the most impact on reducing the probability of exiting the labor market at the age of 60, with a decrease of 4.3 percentage points or 38%. Individuals who started contributing in 1967 also have a higher probability of exiting the labor market after the age of 64. Once again, the reform has the

[13] There are four types of disability pensions. First, *partial disability* pensions are for individuals who have seen their functional capacity reduced by at least 33 percent. These individuals can continue working, even in the same jobs they had before applying for the pension. Second, *professional disability* is assigned to those workers who cannot resume their work activity but could carry out a different occupation. Third, *absolute pensions* are thought for individuals who cannot carry out any type of work due to physical or mental deterioration. Finally, *severe disability* occurs when the worker needs the support of another person to carry out their daily subsistence tasks.

most notable effect on increasing the probability of exiting the labor market at the age of 65, with an increase of 7.1 percentage points or 42%.

We find that early retirement is reduced by one year and two months for individuals who claim a regular pension.[14] The ages at claiming a disability pension and a partial pension are also affected. Individuals who contributed after 1967 delay claiming disability by around two and a half months but anticipate claiming a partial pension by around two months. Table **??** shows that the reform mainly increased the probability of claiming a disability pension between the ages of 60 and 65. This result suggests that individuals affected by the reform use disability pensions as an early retirement scheme between the ages of 60 and 65, ages at which these individuals would have been able to retire with a regular pension if they had contributed in 1966. As the reform influenced the probability of individuals claiming regular, disability, or partial pensions, the resulting impact on the age at which individuals choose to claim these pensions is selective. Therefore, we should interpret these findings with caution.

In Table **??**, we examine the reform impact on the pension benefit amount. We expect the pension benefits to be affected because the reform incentivizes individuals to work longer (as shown in Table **??**), which increases the pension base and decreases the penalty for early retirement. Moreover, as more individuals claim disability insurance due to the reform, we expect the overall pension benefits to be lower as disability pension benefits are typically less generous. We find that the total pension benefit of individuals who started contributing in 1967 increased by 52€ ($\sim$ 4.7%). The increase in the pension benefit is driven by an increment in the base pension (without any financial adjustments) of 22€ ($\sim$ 1.9%) and an increase in the pension adjustment (due to later claiming) of 5.3 percentage points ($\sim$ 6%). It is important to note that the positive effect on pension benefits that we observe for the sample where individuals claimed any pension is driven mainly by

[14] We observe in Table **??** that the reform decreased by 10 percentage points ($\sim$ 67 %) the probability of claiming a regular pension at age 60 and between 2 and 3.2 percentage points ($\sim$ 48 % to 76%) the probability of claiming it between the ages of 61 and 64. On the other hand, the reform increased the probability of claiming a regular pension at age 65 by 9.3 percentage points ($\sim$ 49 %) and 3.6 percentage points after the age of 65 ($\sim$ 32 %).

those individuals who claimed a regular pension, as Table **??** shows. In particular, individuals who claimed a regular pension and started contributing in 1967 received, on average, a monthly pension benefit that was 73.3€ higher. This increase is driven by a rise of 25.8€ in the pension base and a 9.2 percentage point increase in the pension adjustments. Furthermore, we observe that the mean monthly pension benefit decreases by 24€ for individuals who claimed a disability pension, while the reform does not significantly affect partial pension benefit.

6 Removing Early Retirement and Mortality

6.1 The Effect on Mortality

What are the implications of removing early retirement on mortality? The graphical evidence and regression analysis show that delayed retirement is harmful to life expectancy. Conditional on being alive at the age of 50, 42% of our sample died between the ages of 50 and 86. The hazard rate of dying between the ages of 50 and 59 years and the hazard rate of dying between the ages of 80 and 86 are low, at 8% and 7%, respectively. The highest mortality occurs between 60 and 79 years of age. The hazard of dying between the ages of 60 and 69 is 13%, and the hazard of dying between the ages of 70 and 79 is 20%. Figure **??** shows that the density of age at death exhibits a shift from dying after age 70 to dying between 60 and 65 for the treated group (green dashed line), compared to individuals who started contributing in 1966 (control group, red solid line).

The regression results confirm the graphical pattern. The upper panel of Table **??** shows the reduced form reform impact on mortality at different age brackets (conditional on having survived until that age). We find that individuals who contributed in 1967 have a 3.1 percentage point ($\sim$ 8%) higher probability of dying between the ages of 50 and 86. When we examine the reform's impact on mortality at different age brackets, we observe that the increase in mortality is concentrated between the ages of 60 and 69. In particular, individuals who contributed in 1967 die between those ages (that is, ages 60 and 69) with a 2.5 percentage points higher probability ($\sim$ 21%). Moreover, the reform reduces the age at death censored at age 74 by 0.46 years. We censor

the age of death to be 74 years old for those individuals still alive at that age (as the younger cohort, born in 1949, will be 74 years old at the end of our database in 2023).[15] This measure captures both the extensive margin (the effect on premature death) and the intensive margin (the length of life).

The upper panel of Table **??** further reports the effect of age at last employment on mortality in five-year age brackets. We observe that the mortality responses are the strongest between ages when public pensions are not accessible (between the ages of 60 and 64). Removing the early retirement option increases mortality in that age bracket by 2.2 percentage points ($\sim$ 34%). This result indicates that the negative effect of delaying retirement on mortality is driven mainly by the short-term effect of losing access to early retirement schemes.

Figures **??** and **??** show the estimates from placebo tests where we assign placebo treatment status to the individuals using other dates at first contribution. We compare individuals who started making contributions in the years indicated on the y-axis (from 1959 to 1976). The placebo estimates are labelled in black, while our baseline estimates are in red. We can observe that almost all placebo estimates are insignificant or close to zero. See Section **??** for a more detailed discussion of the placebo tests.

The bottom panel of Table **??** qualifies the impact of delaying age at last employment by one year on mortality. The simple OLS estimates show that delaying retirement is negatively correlated with mortality. This correlation likely captures that less healthy workers tend to retire early. The IV estimates indicate that delaying the age at last employment by one year increases the probability of dying between the ages of 60 and 69 by 4.4 percentage points ($\sim$ 38%) and reduces the age at death by around one year. All F-statistics are above the rule-of-thumb threshold of 10. Compared with the reduced form estimates, the IV results are more than double. This is consistent with the

[15] In our analysis, we examine individuals from 12 cohorts from 1938 to 1949, with observations extending until 2023. Consequently, we track the mortality of individuals from various cohorts up to different ages. Importantly, for mortality up to age 74, the analysis encompasses all cohorts in our dataset. Therefore, caution is advised when interpreting results related to the probability of death beyond age 75, as these regressions involve only specific cohorts within our sample.

almost half a year increase in age at last employment (as estimated in Table **??**).

To interpret the IV results, it is important to understand who the compliers are. As compliers cannot be identified individually, we follow **?** and calculate the fraction of compliers in different subsamples to recover their characteristics. We define treatment as retiring after the age of 61. In Table **??**, we characterize the compliers based on a list of predetermined characteristics. While the sample consists of more men, the compliers are much more likely to be women. This is consistent with Table **??**, which shows that women delay the age at last employment by 1.1 years, while men delay only by around four months. Yet, we find similar reduced-form impact on the probability of dying between 60 and 69 by gender, suggesting that men suffer more from working longer. Moreover, workers with fewer years of contribution and blue-collar workers are slightly over-represented among the compliers. Not surprisingly, the compliers are much less likely to be self-employed, as the self-employed are much less likely to be affected by changes in public pensions.

6.2 Mechanisms

This section attempts to shed light on some of the potential mechanisms explaining why losing access to early retirement increases mortality. We focus on two types of heterogeneities: labor market conditions before retirement and the possibility of flexible retirement. To better compare the magnitudes, we focus on discussing the IV estimates in this section. The reduced form estimates are included in the tables. They are consistent with the IV estimates and about half the size.

6.2.1 Labor Market Conditions Prior to Retirement

Delaying retirement can have very different effects on an individual's life expectancy, depending on the working conditions experienced by the individuals during their last years of employment

(**?**).[16] In this paper, we acknowledge that the burden of a job may be multi-dimensional. Therefore, we examine four characteristics of the individuals' labor environment before retirement: physical burden, psychosocial burden, self-value at work, and the skill level of their last occupation before retirement. The correlation between the first three measures (physical burden, psychosocial burden, and self-value at work) is not very high, indicating that they capture different characteristics of the individuals' labor environment. Specifically, the correlation between physical and psychosocial burden is 0.15, -0.09 between physical burden and self-value at work, and -0.33 between psychosocial burden and self-value at work.

Table **??** reports the heterogeneity results for the probability of dying between the ages of 60 and 69 (conditional on surviving to age 60)[17] based on all four measures. In the first panel, we report the reform's effect on the age at last employment for each subgroup, which serves as the first stage of the IV estimation. In the second and third panels, we report the effect of delaying retirement on mortality between the ages of 60 and 69. First, we report the reduced form effect of the reform and then the IV estimates, which capture the effect of delaying the exit of the labor market on mortality by one year. We also report p-values testing the hypothesis that the IV coefficients by subgroups are equal in the last row. Importantly, Table **??** shows that the reform has not affected

[16] One of the reasons we expect to see heterogeneity in mortality by labor market conditions is because harsher working conditions are more likely to trigger mortality due to specific causes, which are predominant during the ages of 60 to 69. For instance, the medical literature has long established that circulatory system diseases can often be correlated to work-related stress (**?**). In fact, both **?** and **?** report that retirement reduces the risk of heart-related mortality. For the cohort considered in our sample, circulatory diseases are the second cause of mortality (after tumors) between the ages of 60 and 69. Moreover, the third cause of mortality for our cohort of individuals is due to respiratory diseases. Significant risks for respiratory conditions include smoking and lack of physical activity (**??**). Both factors can be affected by working status and, ultimately, retirement (**????**). **?** find that mortality due to two lung-related conditions (COPD and lung cancer) statistically increases immediately after retirement at the age of 62.

[17] As the reform has no impact on the probability of dying before age 60, the sample used in this regression is not selected.

the probability of being in each subgroup, except for the probability of working in blue-collar jobs before retirement.

Physical and Psychosocial Burden Retirement enables individuals to enjoy more leisure time and eliminates work-related stress and exposure to job-specific accidents, potentially positively impacting individuals' mental and physical health and well-being. Thus, retirement may be particularly beneficial for those who work in strenuous occupations, either physically or mentally. Indeed, labor unions have used this argument heavily in their opposition to increases in the statutory retirement age. Therefore, we first classify individuals' last industry depending on their physical and psychosocial burden to analyze if the adverse effects of delaying retirement on mortality differ by these characteristics.

Previous literature has already established that physically demanding occupations lead to adverse health effects (see **?** and **?** for a summary). To measure physical burden at work, we use the Spanish Register of Workplace Accidents between 2003 and 2019, which has information on the total number of workplace accidents that individuals in our sample (cohorts born between 1938 and 1949) experienced in different industry sectors. Figure **??** shows the distribution of industry sectors depending on their incidence of workplace accidents per 1,000 workers. We link individuals' last industry to this aggregate industry-level data and divide our sample by the median of the workplace incidence. After this division, the water and sanitation sector, the extractive industry, the administrative sector, the energy sector, the health sector, the manufacturing industry, the public administration, and defense are considered to have a high incidence of workplace accidents, and the rest are included in the low-incidence group.

Columns 1 and 2 of Table **??** show that while the impact on age at last employment is similar, the increase in mortality is stronger for those individuals who worked in sectors with a higher incidence of workplace accidents before retirement. Delaying the age at last employment by one year increases the probability of dying between the ages of 60 and 69 by 4.3 percentage points ($\sim$ 34%) in sectors with a high incidence of workplace accidents. At the same time, the effect is 3

percentage points ($\sim 27\%$) in sectors with a low incidence of workplace accidents. However, the p-value of the difference between these two groups is 0.125, indicating that the difference is not statistically significant.

We further divide our sample into three groups (see Table **??**) and find that the impact of delaying retirement significantly differs between the highest and middle groups. It is worth noting that the group working in sectors with the highest workplace accidents delay retirement less. Although people who work in physically strenuous jobs delay retirement less due to the reform, the impact on mortality is greater for them. We find that the probability of dying between age 60 and 69 increases by 7.5 percentage points ($\sim 62\%$) for those in sectors with a very high physical burden. This is also in line with our finding of a higher probability of dying before claiming any pension, implying their health suffers directly at work.

Next, we examine the heterogeneous effect of delaying retirement on mortality by the mental and social stress that individuals have experienced before retirement. Unfortunately, we do not have data in Spain that provide a good measure of psychosocial burden. Thus, we measure psychosocial exposure by adopting occupational indexes based on the Job Exposure Matrices constructed by **?**, which uses a large-scale representative survey of the working conditions of about 20,000 employees in Germany. Their measure of 'psychosocial burden' is based on mental stress, social stress, and temporal loads.[18] Figure **??** shows a distribution of industry sectors by this psychosocial exposure index. We link individuals' last industry with this aggregate occupation-level data and divide our sample by the median of this index.

Columns 3 and 4 of Table **??** report that delaying retirement by one year increases the probability

[18] The psychosocial burden occupational index elaborated by **?** 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.

of dying by 5.3 percentage points ($\sim$ 44%) for individuals with occupations in industries with a high psychosocial burden. In contrast, the increase is smaller (2.7 percentage points) for those with occupations in industries with fewer psychosocial burdens. However, the p-value indicates that the difference is not statistically significant. We again find that the group that delays retirement less suffers more from delaying.

We further divide our sample into three groups (see Table **??**) and find a similar pattern. While mortality increases by 5.4 percentage points for those in sectors with a very high psychosocial burden, we find no effect on mortality for individuals working in sectors with a very low psychosocial burden. These results imply that losing the right to claim early retirement can lead to the death of people who not only had physically demanding jobs but were also exposed to high levels of psychosocial stress at work.

Self-value at Work Previous literature has pointed out that retirement can negatively impact individuals' well-being, as they often lose the social network of their co-workers and may feel less valuable to society (**?**). Therefore, we want to test this hypothesis by looking at the heterogeneous effect of delaying retirement on mortality based on how and whether individuals felt useful and recognized at work before retirement.

We utilize the data from the Occupational Information Network (O*NET) collected by the U.S. Department of Labor. We use the work value classification to measure self-value at the workplace, which measures a sense of achievement and recognition within the workplace. Figure **??** shows the distribution of industry sectors by this self-value index. In our sample, we link individuals' last industry with this aggregate occupational-level data,[19] and divide the sample by the index's median.

In columns 5 and 6 of Table **??**, we find strong evidence that the mortality effects between the ages of 60 and 69 are driven by individuals working in low self-value industries, even though they delay retirement less. People with a low self-value job delay retirement by 0.5 years, while people

[19] We link the occupational index of self-value with individuals' last industry following the same steps as for the psychosocial burden index.

with a high self-value job delay retirement by 0.93 years. The IV estimates indicate that delaying the labor market exit by one year increases the probability of dying between the ages of 60 and 69 by 6.1 percentage points ($\sim 51\%$) for individuals with low self-value jobs, while the impact is small and insignificant for individuals working in sectors with high self-value. The difference is statistically significant, with a p-value of 0.02. Therefore, this result suggests that individuals who feel a sense of achievement and recognition at work do not experience a negative mortality effect from delaying retirement.

Skill Level Finally, previous literature has relied heavily on heterogeneity differentiating between blue- and white-collar jobs, typically based on each occupation's assumed skill level (**?**). Following this literature, we also look at the differential effect for individuals working in white- and blue-collar occupations in columns 7 and 8 of Table **??**. Contrary to **?**, we find this heterogeneity is similar to that based on the physical burden. Delaying retirement by one year increases the probability of dying between the ages of 60 and 69 by 6.3 percentage points ($\sim 55\%$) for individuals with a blue-collar job, while it is only 2 percentage points and not significant for the rest. The difference is statistically different with a p-value of 0.05, 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 **??** shows that the reform had a significant negative effect on the probability of working in blue-collar jobs before retirement.

6.2.2 Possibility of Gradual Retirement

Reducing the possibility of early retirement is effective at prolonging the working careers of older workers. However, we have shown that this type of policy leads to serious adverse effects on individuals' life expectancy. A potential solution to incentivize workers to stay longer in the labor force without negatively affecting their health is to allow these workers to gradually reduce their working time at the end of their careers.

We analyze whether having the option to claim a partial pension can mitigate the negative impact

of delaying labor market exit on mortality. As the reform affected the probability of people claiming a partial pension, we cannot simply compare the mortality outcomes of those who claimed a partial pension and those who did not. To study this question, we take advantage of the fact that only individuals with at least 33 years of contribution can access this scheme.[20]

As explained in Section **??**, in 2002, the Spanish pension system introduced the possibility of individuals partially retiring after the age of 60, allowing them to combine income from work with old-age pension benefits. They were allowed to claim up to 85% of their pension while reducing employment time from 85% to 15% of the original contract. However, this option, also subject to the firm's agreement, was only available for workers with at least 33 years of contribution and six years of tenure in the same company. Figure **??** demonstrates that the probability of claiming partial pension increases exponentially after reaching 33 years of contribution and is almost zero before. The first row of Table **??** also confirms that those with more than 33 years of contribution respond to the reform by having a higher likelihood of claiming a partial pension. In particular, treated individuals who contributed more than 33 years have a 4.3 higher probability of claiming a partial pension, while those with less than 33 years of contribution only have 0.2 percentage points (not significant) higher probability compared with the control group.

Table **??** shows that a one-year increase in the age at last employment increases mortality between the ages of 60 and 69 by 5.4 percentage points ($\sim$ 42%) for individuals with less than 33 years of contributions, who could not access to partial retirement. On the other hand, the effect is much smaller (2.6 percentage points or 25%) for individuals with more than 33 years of contributions who could potentially access the partial retirement scheme. This differential reform impact on mortality is statistically different at 10 percent level.

Because having more years of contribution could be correlated with knowledge of the partial

[20] We only observe the number of contribution years for those that claim a regular pension or a partial pension. For those individuals who do not claim any pension or claim a disability pension, we proxy the number of contribution years with the number of active years (the total number of years since they started contributing to the Social Security system until they claim a disability pension, die, or the end of our data).

pension program and other unobserved characteristics, we test the robustness of this finding by using individuals closer to the 33-year cutoff. We take two samples: individuals with contribution years between 23 and 43 years and those between 28 and 38 years. Table **??** displays the results. Again, delaying retirement has almost four times less impact on mortality outcomes for those with more than 33 years of contributions. The estimates are significantly different, with a p-value of 0.06 for the sample of individuals who contributed between 23 and 43 years. The difference is smaller and not significantly different when we restrict our sample to individuals who contributed between 28 and 38 years. This is expected and is consistent with the gradual increase in the probability of claiming a partial pension after reaching 33 years of contribution, as illustrated in Figure **??**. This result indicates that introducing the possibility of partially reducing the working time at the end of their careers can help mitigate the adverse effects on the health of older workers who delay retirement.

7 Placebo and RobustnessTests

In this section, we test the causality of our estimates by using placebo cutoff dates from both before and after 1967. Moreover, we perform several robustness checks on the labor market reduced form effects of the reform, as well as both the IV and reduced form estimates of the mortality responses.

7.1 Placebos

A concern for causality is that our results could be potentially biased by unobserved characteristics that affect both the date of starting contributions and our outcome variables. To test this possibility, we perform several placebo tests where we assign placebo treatment status to the individuals using other dates at first contribution. We use two approaches to perform these placebo tests. First, we compare individuals who started contributions around hypothetical cutoff years (from 1960 to 1976, except for 1965, 1966, and 1968) in our baseline sample (cohorts born between 1938

and 1949).[21] Figures **??** and **??** plot the estimated coefficients of the different placebo tests. The placebo estimates are labeled in black, while our baseline estimates are in red. We can observe that almost all placebo estimates are insignificant or close to zero.

However, one shortcoming of these placebo exercises is that each placebo estimate represents the impact of starting contribution one year later for people of different starting ages. This is because we restrict the placebo samples to the same cohorts as our baseline sample (1938-1949). For example, the individuals who began contributing in 1970 and 1972 were between the ages 22 and 32 at first contribution, as opposed to the range of ages 18 and 29 in our baseline sample.

Therefore, we use a second approach. Instead of holding the birth cohorts constant, we fixed the age bracket at first contribution while varying the cohorts in the placebo samples. Specifically, we compare individuals who started contribution around hypothetical cutoff years using samples of cohorts who were in the same age bracket at first contribution (18 to 29 years old) as those in our baseline sample. Figure **??** plots the estimated coefficients using this alternative approach. Here, we only show placebo cutoffs between the years 1963 and 1972 (except for 1965, 1966, and 1968) due to data availability.[22] The placebo estimates are labeled in black, while our baseline estimates are in red. We can observe that almost all placebo estimates are insignificant or close to zero. Both placebo exercises are complementary and suggest that the estimated changes in our baseline analysis result from the exogenous increase in early retirement age rather than from other confounding factors.

[21] We do not perform the placebo test on years too close to the actual treatment years, including 1964 vs. 1965, 1965 vs. 1966, and 1967 vs. 1968. As explained in Appendix **??**, we adjusted the years 1965, 1966, and 1967 of the first contribution by using the total number of years contributed and the date that individuals claimed a pension. Therefore, if we used placebos for 1964 vs. 1965, 1965 vs. 1966, or 1967 vs. 1968, we would compare a corrected year of the first contribution with a year that has not been corrected.

[22] Note that the data is only available up to 2023; we can only observe cohorts born between 1933 and 1954 till age 69, limiting our analysis from 1962 to 1972.

7.2 Within-Age at First Contribution Fixed Effects Model

The baseline analysis compares individuals born in the same year (along with their highest occupation level and industry sector fixed effects) who started contributing to the system one year apart (1966 vs. 1967). One potential confounding factor of this specification is the age at which individuals started contributing. These individuals were born in the same year but started contributing in 1966 and 1967 and were at different ages when they started contributing. One reason for starting at different ages could be differences in educational attainment. Unfortunately, we do not have information on the education level of individuals in our database. Therefore, to test that the reform is not capturing differences in educational attainment, we use age at first contribution fixed effect instead of birth year fixed effects in Table **??**. This robustness check estimates the impact of losing access to early retirement for individuals who start working at the same age but were born one year apart. These estimates should be similar to the main estimates unless the different starting age is a confounding factor. Compared with the baseline results in Tables **??** and **??**, the magnitudes of the estimates in Table **??** are very similar.

Table **??** also shows that our mortality estimates are not sensitive to using age at first contribution fixed effects instead of month and year of birth fixed effects (Column 2). This robustness check indicates that the impact of losing access to early retirement on mortality is similar if we consider individuals who start working at the same age but were born one year apart.

7.3 No Controls

In our baseline estimation, we control for a list of fixed effects, such as the highest level of occupation and industry sector between the ages of 30 and 40, and a list of other predetermined covariates, including individuals' mean monthly contribution to the Social Security system, the fraction of days active and employed, and the fraction of time self-employed between the ages of 30 and 40. Ideally, we would like to control for the characteristics of the individuals at the beginning of their careers. However, the data quality was not particularly good when our individuals were that young, so the labor market characteristics during the first years of their careers might

have been wrongly recorded for some individuals. We, therefore, control for their characteristics between the ages of 30 and 40.

In order to make sure that these controls are not endogenous, in Table **??**, we check if the treatment had any significant impact on these variables and, except for the fraction of time spent in self-employment, we confirm that these variables are not affected by the treatment. We also performed another robustness check of our main results, not controlling for any of these controls. Table **??** shows that the magnitude of the reduced form labor market estimates is quite similar to the baseline results in Tables **??** and **??**. Column 3 of Table **??** also estimates the mortality effect without controlling for any labor market variable when the individuals were between 30 and 40 years old, and the estimates are quite similar to the baseline estimates. These robustness checks suggest that these covariates are not likely to be endogenous.

7.4 Cohorts Born between 1941 and 1949

In the baseline sample, we consider individuals born between 1939 and 1949. A law in 2002 introduced the possibility of retiring early via the involuntary pathway. As a result, cohorts born from 1938 to 1940 can claim a pension at the ages of 64, 63, and 62, respectively, while cohorts born after 1941 can claim at the age of 61 (see Figure **??**).

Therefore, we performed a robustness check, dropping the cohorts that were only partially affected by the law of 2002. Table **??** and column 4 of Table **??** report the main labor market and mortality results for the cohorts born between 1941 and 1949. If we compare them with the baseline results, we can see that the magnitude of the estimates is quite similar. These results demonstrate that our reduced form effects are not driven by the older cohort of individuals with later access to involuntary early retirement.

7.5 Dropping Self-Employed Individuals

Table **??** shows the impact of the treatment on a list of labor market variables when the individuals were between 30 and 40 years old. Except for the fraction of time spent in self-employment, we

do not find significant impacts, suggesting that there is no manipulation of the treatment status. A potential reason for finding significant effects on individuals' fraction of time spent in self-employment is that self-employed individuals might have more flexibility in deciding when they want to start contributing to the Social Security system. In this robustness check, we want to ensure that our main baseline results are not driven by these individuals.

Therefore, we perform a robustness check dropping those individuals who received a pension under the self-employed regime (see Table **??** and column 5 of Table **??**). If we compare them with the baseline results, we can see that the magnitude of the estimates is quite similar, indicating that our baseline reduced form effects are not driven by those individuals who were self-employed.

7.6 Correction for the Starting Year

As discussed in Section **??**, one caveat of the administrative dataset is that the exact date of the first contribution is poorly recorded for some individuals, especially those who started contributing around 1967. We partially correct this measure by using the accurately measured number of contribution years. Figure **??** and Figure **??**b) indicate that the correction is effective in reducing the bunching and correcting mistakes. However, we are conscious that this correction has some limitations. For instance, we can only correct the date of the first contribution for individuals who claimed a pension; thus, it cannot be adjusted for those who died before claiming a pension.

Here, we provide some suggestive evidence that this correction does not introduce bias into our results. First, we show that the probability of dying between 50 and 54 or between 55 and 59 is not significantly affected by the reform (Table **??**). Second, we show that without correction, the mortality effects remain significant but are attenuated. As suggested by Figure **??**, without the correction, some treated individuals might be incorrectly classified in the control group, potentially biasing our estimates downward. Third, Table **??** shows that the effect is very similar to the baseline estimation if we remove from the sample those months with the highest bunching (month 12 of 1966 and month 1 of 1967). Finally, we show robustness of our findings by including those who began contributing in 1965 and 1968 in Table **??**. We also consider the same augmented sample

but exclude individuals who began contributing in 1966 and 1967 in Table **??**. Our estimates are robust to these exercises.

8 Discussion

8.1 Comparison with Existing Studies

We find that individuals who contributed in 1967 (a delay of five years in statutory retirement age) have a 2.5-percentage point higher probability of dying between the ages of 60 and 69 (21% increase). The IV estimates indicate that delaying the age at last employment by one year increases the probability of dying between the ages of 60 and 69 by 4.4 percentage points (38%). This may seem quite a large effect; however, our estimates are comparable in magnitude with studies showing that early retirement reduces mortality (**??**).

? find that offering a five-year reduction of the statutory retirement age from the age of 65 to 60 reduces the probability of dying by the age of 70 by 26 percent. Using the same measure of mortality, we find that a five-year increase in the statutory retirement age from the age of 60 to 65 increases the probability of dying before age 70 by 3 percentage points, which is equivalent to a 17 percent increase. Additionally, **?** show that the mortality effects are driven by those who are more exposed to workplace hazards; that is, those with low pre-retirement incomes and those without a college education. Their finding is consistent with our heterogeneous results. **?** also find estimates of a similar magnitude. They find that retirement induced by a temporary decrease in the retirement eligibility age (from the age of 65 to 61 or 62) for male Dutch civil servants decreased the probability of dying within five years by 47 percent (2.6 percentage points).[23] Although our prior is that the effect of delaying retirement is not necessarily symmetric with the impact of early retirement, our estimates suggest that the effect on mortality has a similar magnitude when the

[23] Although **?** study male military officers in Sweden and **?** focus on Dutch male civil servants, both papers point out that the working environment of these subgroups of males is not more demanding than that for the general population.

nature of the reform and affected age ranges are comparable.

Our paper is the first to find that retirement reduces mortality by exploring quasi-experiments that shut down early retirement options. Existing papers find no effect of delaying retirement on mortality. **?** and **?** are the only two papers we know of that have the statistical power to conclusively estimate the mortality impacts, and they find precisely zero effects of delaying retirement on morality before age 75.[24] One common feature of these two papers is that they explore reforms that increase the financial incentives to delay retirement while keeping the statutory retirement age unchanged. **?** find a precisely zero impact of delaying retirement on the probability of dying between the ages of 61 and 79 for private-sector workers in France. **?** find no effect of delaying retirement on mortality between the ages of 65 and 74 by exploring a reform that reduced the implied tax of working for married males in Israel.[25] One possible reason could be that working longer has a more harmful impact on mortality when the early retirement option is removed than when early retirement is financially less attractive. Particularly, we find that workers entitled to gradual retirement suffer less from the reform (see Table **??**). This finding indicates that delaying retirement is less harmful when pension reform provides a flexible choice rather than a paternalistic policy that prohibits workers from retiring earlier.

Furthermore, we compare our paper with studies on the impact of old-age income on mortality (e.g., **???**). In particular, **?** examine a variation in social security wealth for the U.S. "notch" cohort and show that reductions in pension benefits led to lower mortality, which they attribute to beneficial effects of employment. In contrast, our paper shows that the reform, which removes

[24] **?** explores a reform that increases the statutory retirement age from the age of 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).

[25] It is important to note that **?** show a decline in the probability of survival of the affected men between the ages of 75 and 85 due to later retirement. Overall, they find that one additional year of employment decreases longevity by 9 to 12 months. They also find the mortality effect is stronger among blue-collar workers, who are more likely to be performing manual, physical tasks. Our results are in line with their findings.

early retirement access, leads to higher mortality, despite inducing higher pension benefits (as shown in Table **??**). Our IV analysis, which controls for pension income, suggests that the adverse mortality impact is mainly due to delayed employment. While both **?** and our paper show that the employment impact dominates, **?** suggest that working longer is beneficial, whereas we find working longer is detrimental. One possible explanation for this difference is that working longer caused by abolishing early retirement option is different from working longer induced by less generous pension income. Additionally, in our setting, people entitled to earlier retirement do not necessarily experience the pain of being displaced, which can lead to an increase in mortality (**?**). Finally, **?** 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 find that later retirement leads to higher mortality.

Finally, we discuss our findings in comparison to recent literature examining the impact of disability insurance (DI) on mortality. For example, **?** show that access to DI reduces mortality for sicker, inframarginal beneficiaries aged 55 to 64 due to DI income and Medicare eligibility.[26] Similarly, **?** find that generous DI benefits reduce mortality among low-income DI recipients. Both papers show the beneficial effects of access to income for vulnerable people. Similarly, our paper finds that access to a pension is beneficial because the loss of early retirement leads to an increase in mortality. Moreover, **?** and **?** show that the beneficiaries who benefit from DI receipt are those whose labor earnings do not respond to DI eligibility/DI income. Our heterogeneity analysis suggests that the mortality responses are driven by people who are less likely to comply with the reform. In other words, although these people have managed to mitigate the impact of the reform by delaying their exit from the labor market to a lesser extent, they are the ones who suffer most from working longer. This is also supported by our finding of an increased likelihood of dying before claiming a pension, implying that many people's health suffers directly at work. What's

[26] **?** also show that among applicants with conditions that require lower medical expenditures, such as musculoskeletal disorders, DI receipt increases mortality because working is beneficial for this group.

different from **?** is that the mortality responses in our paper are not driven by those in the worst health. This is because we show that the pension reform leads to an increase in the probability of claiming DI, suggesting that those in the worst health could use DI to exit the labor market when the old-age pension is not available.

8.2 Policy Discussion

The heterogeneous mortality impacts of delayed retirement suggest important distributional consequences of raising the statutory retirement age. In particular, the socio-economic disparities in lifespans are large and have increased in recent decades (**?**).[27] One possible contributing factor might be the heterogeneous mortality responses to pension reforms, which could exacerbate the disparity. Moreover, individuals who survive longer receive more years of pension. The resulting gaps in life expectancy will affect the actuarial fairness and progressivity of public pension systems (**?**). Specifically, individuals from lower socio-economic groups (typically those who are more exposed to workplace hazards) spend fewer years in retirement than the rest of the population due to the pension reform.

A reduction in the duration of claiming a pension is composed of two factors: delayed pension claiming and an earlier death. While the welfare impact of a delayed labor market exit can be positive, an earlier death is harmful. Table **??** shows that individuals with strenuous employment (both

[27] We acknowledge that life expectancy also differs largely by gender. In Spain, in 2021, men live on average until age 80.2, while women live on average until age 85.8 (Spanish National Institute of Statistics). In Table **??**, we examine if the reform had differential effects across gender. We show that a year of delaying retirement increases mortality more for men than women[28]. In particular, a one-year delay in the age at which men exit the labor market increases by 7.7 percentage points ($\sim$ 50%) the probability of dying between the ages of 60 and 69. The same delay for women increases mortality by 2 percentage points ($\sim$ 45%). Factors influencing gender differences in mortality include biological factors (genetics and hormones) and behavioral and environmental factors. One behavioral factor that explains part of the mortality gender gap is that women and men select into different occupations. Therefore, the differential effect of delaying retirement on mortality by gender may be partly driven by men and women selecting occupations and sectors with different degrees of health burden (**?**).

physically and psychosocially), low self-valued jobs, and who work in blue-collar jobs experience a greater increase in mortality between the ages of 60 and 69 due to the reform. In comparison, the reform impact on the age at last employment is relatively similar across different subgroups. If anything, individuals with better jobs delay their exit from the workplace for a longer period. This comparison implies that the mortality impact plays an important role in explaining the shortened pension claiming duration for workers with worse working conditions. One possible policy recommendation would be to consider reforms that link retirement age to changes in life expectancy. It might be worthwhile to consider a target retirement age based on the years a person is expected to claim a pension, rather than a uniform nationwide retirement age.

9 Conclusion

This paper studies the effect of delaying retirement on mortality. We exploit the 1967 Spanish reform that removed access to voluntary early retirement for individuals who had not contributed since that year. Individuals who started contributing to the pension system before 1 January 1967 maintained the right to claim early retirement at the age of 60. However, individuals who have not contributed by that date can only claim retirement voluntarily at the statutory retirement age of 65 (although, under certain circumstances, some individuals can involuntarily claim early retirement at the age of 61).

Focusing on cohorts born between 1938 and 1949, we use Spanish administrative Social Security data and compare individuals who started contributing 12 months before and after 1 January 1967. We find that individuals who started contributing after 1967 delayed their labor market exit by almost half a year. The reform not only modified the age at last employment but also decreased the probability of claiming a regular pension. Instead, people are more likely to claim a partial pension and disability insurance. This indicates that people utilized other ways to leave the labor market before claiming a regular pension. Moreover, the results suggest that treated individuals are more likely to claim no pension, driven mainly by premature death.

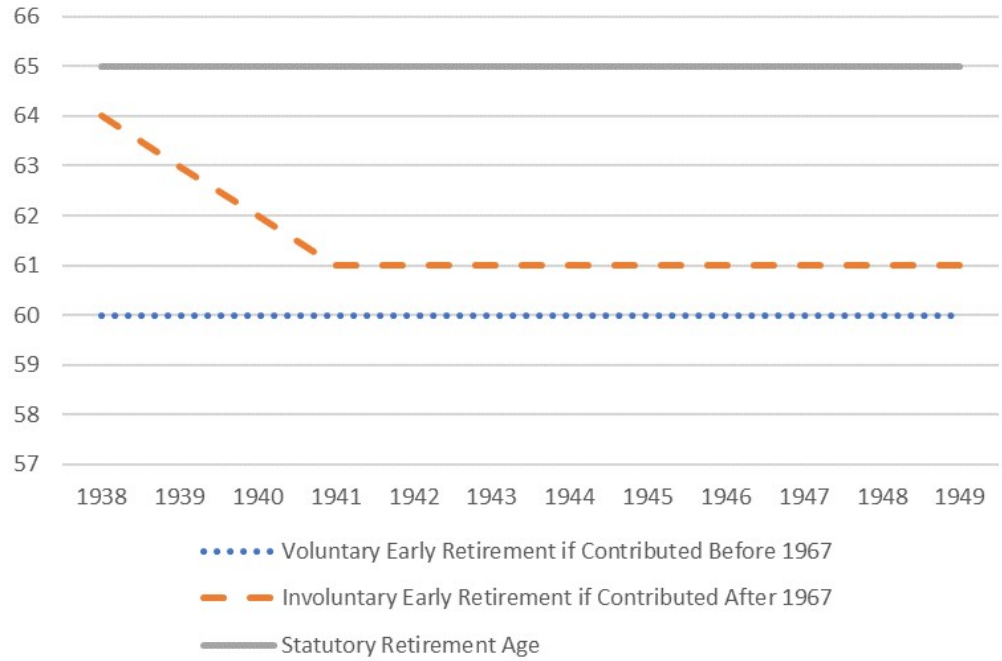
We find considerable mortality responses. Delaying labor market exit by one year increases the hazard of dying between the ages of 60 and 69 by 4.4 percentage points (38%). The mortality responses are the strongest between the ages of 60 and 64 (60%) when public pensions are no longer accessible for individuals who started contributing after 1967. This suggests that the effect of delaying retirement on mortality is driven mainly by the immediate effect of losing access to early retirement schemes. We explore several mechanisms to explain the detrimental effects of delaying retirement on health. First, we show that individuals working in more physically strenuous and less self-valued jobs before retirement suffer the most in terms of mortality, even though they delay retirement to a lesser extent. Moreover, we show that allowing workers to gradually reduce their working time towards the end of their careers can incentivize workers to stay longer in the labor force without negatively affecting their health. This finding suggests that it is crucial to provide options for gradual and flexible retirement while raising the age of statutory retirement.

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, the existing empirical evidence on the mortality effects of retirement rests almost exclusively on the estimates of policy experiments that have allowed for earlier retirement. As it is unclear whether there is a symmetry effect between preponing and postponing retirement age, 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.

10 Figures and Tables

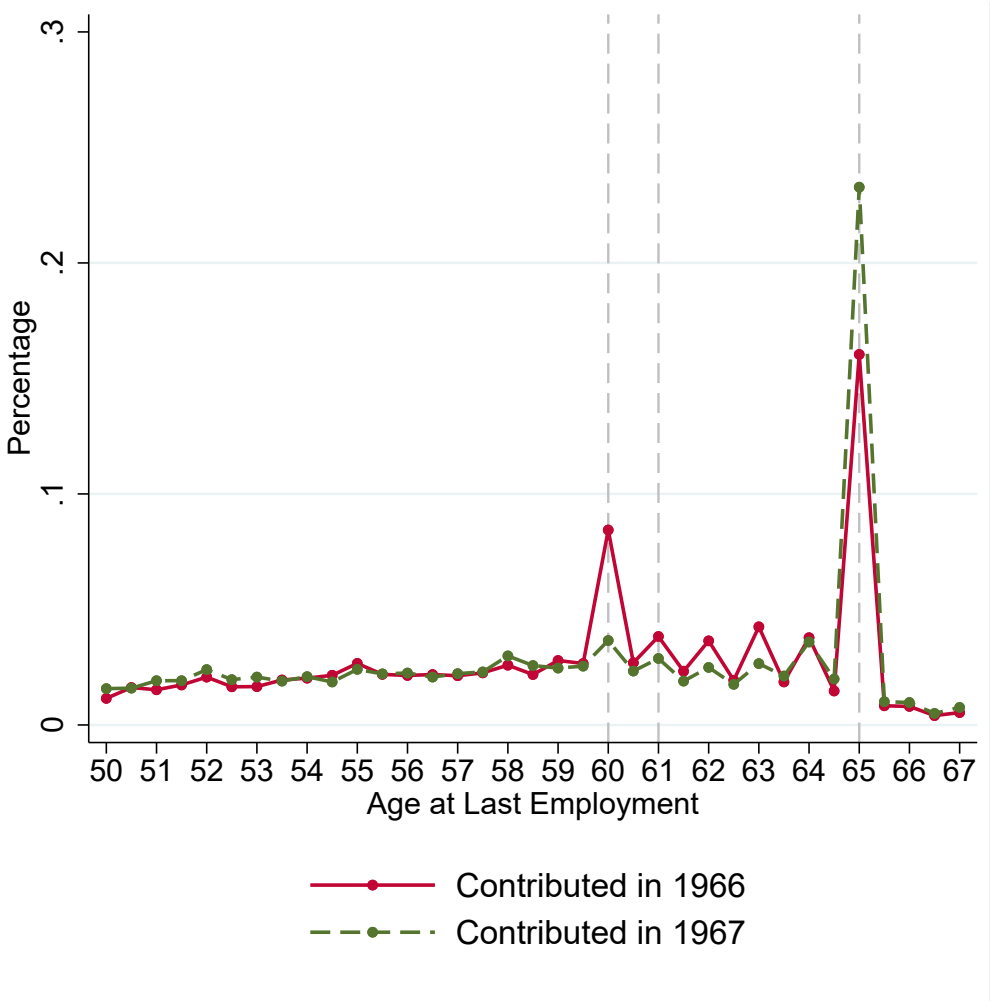
Figure 1: Retirement Age by First Year of Contribution and Cohort



Source: Authors' own construction according to the pension laws.

Notes: This figure plots the statutory retirement age and the earliest possible early retirement age for individuals that contributed before and after 1 January 1967 as a function of their birth year. The blue line shows that individuals who start contributing before 1 January 1967 can voluntarily retire after age 60, independently of their birth year. The orange line shows that those who start contributing after 1967 can only involuntary retire after 64 to 61, depending on their birth year. The grey line shows that the statutory retirement age remains at age 65 for all cohorts independently from the moment they started contributing.

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

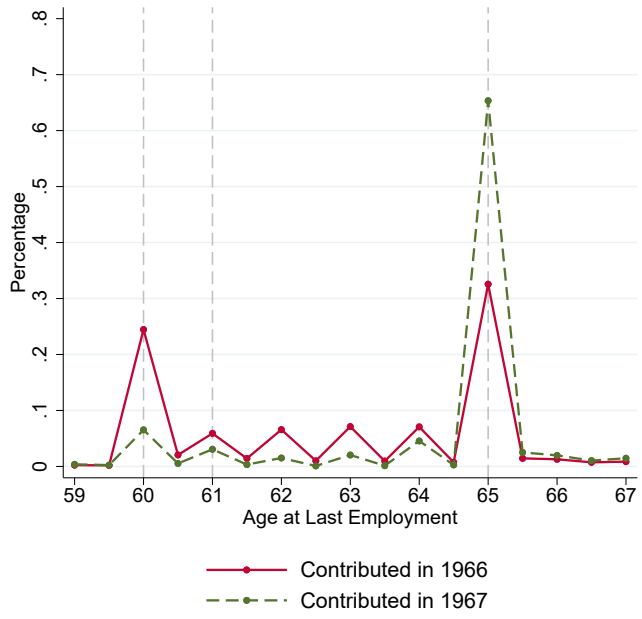


Source: MCVL, cohorts 1938-1949.

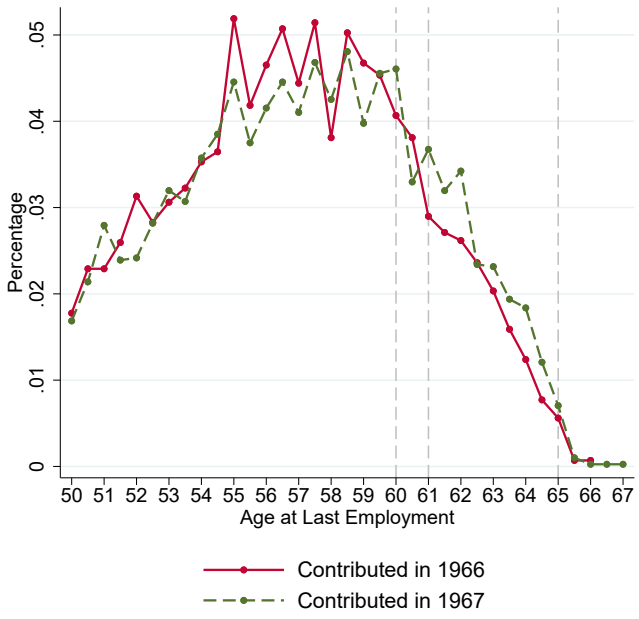
Notes: This figure plots the percentage of individuals by the age at which they finished their last employment. The solid red line shows the density for individuals who started contributing in 1966, while the green dashed line shows those who started contributing in 1967.

Figure 3: Density of Pension Ages by Treatment Status

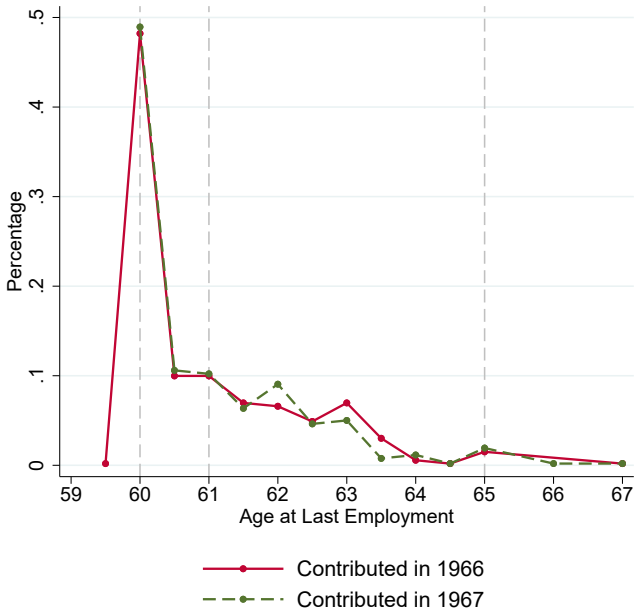
(a) Age at Regular Pension



(b) Age at Disability Pension



(c) Age at Partial Pension



Source: MCVL, cohorts 1938-1949.

Notes: This figure plots the percentage of individuals by the age at claiming regular pension (Graph a), age at claiming disability pension (Graph b), and age at claiming partial pension (Graph c). The solid red lines show the density for individuals who started contributing in 1966, while the green dashed lines show those who started contributing in 1967.

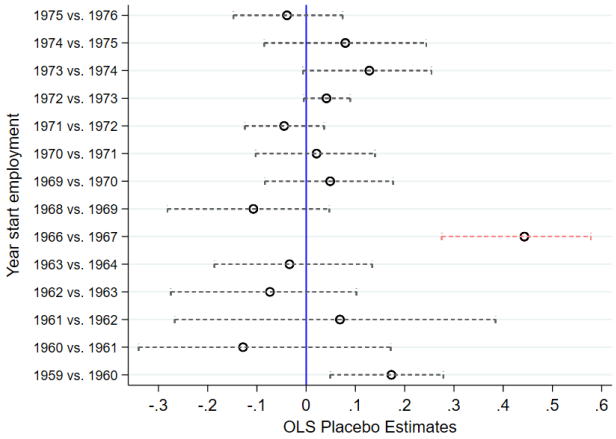
Figure 4: Density of Age at Death by Treatment Status

Source: MCVL, cohorts 1938-1949.

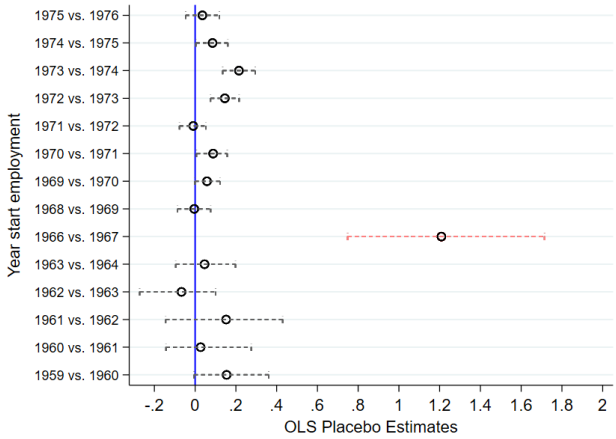
Notes: This figure plots the percentage of individuals by the age at which they died. The solid red line shows the density for individuals who started contributing in 1966, while the green dashed line shows those who started contributing in 1967.

Figure 5: Placebo Tests: Using Other Cutoffs

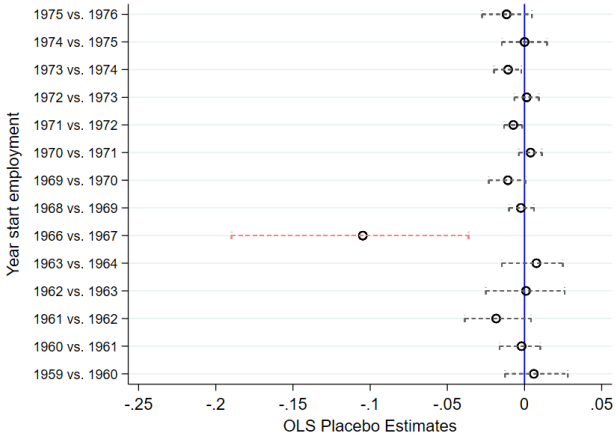
(a) Age at Last Employment



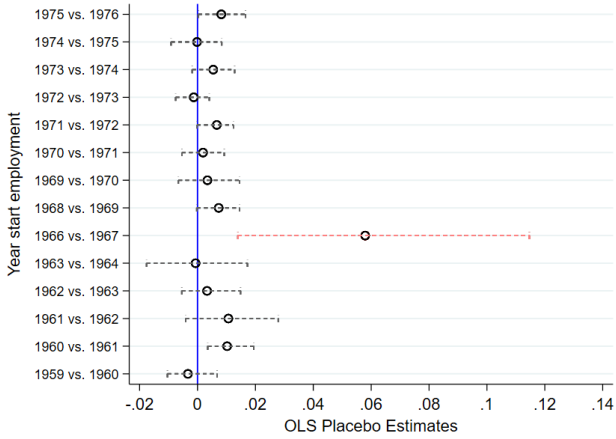
(b) Age at Claiming Regular Pension



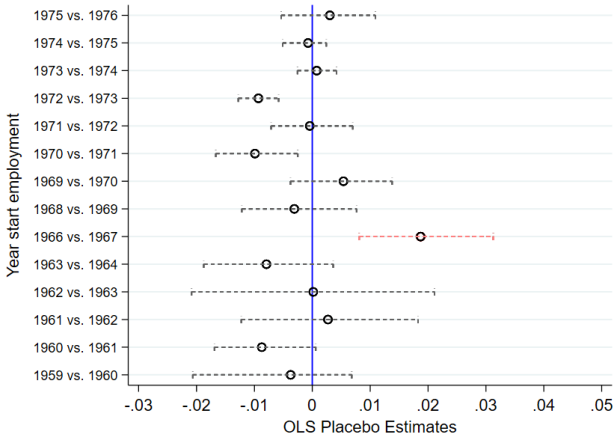
(c) Probability of Claiming Regular Pension



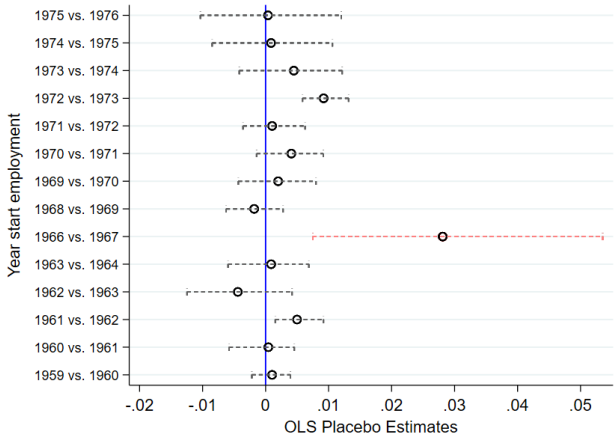
(d) Probability of Claiming Disability Pension



(e) Probability of Claiming Partial Pension



(f) Probability of Not Claiming Any Pension

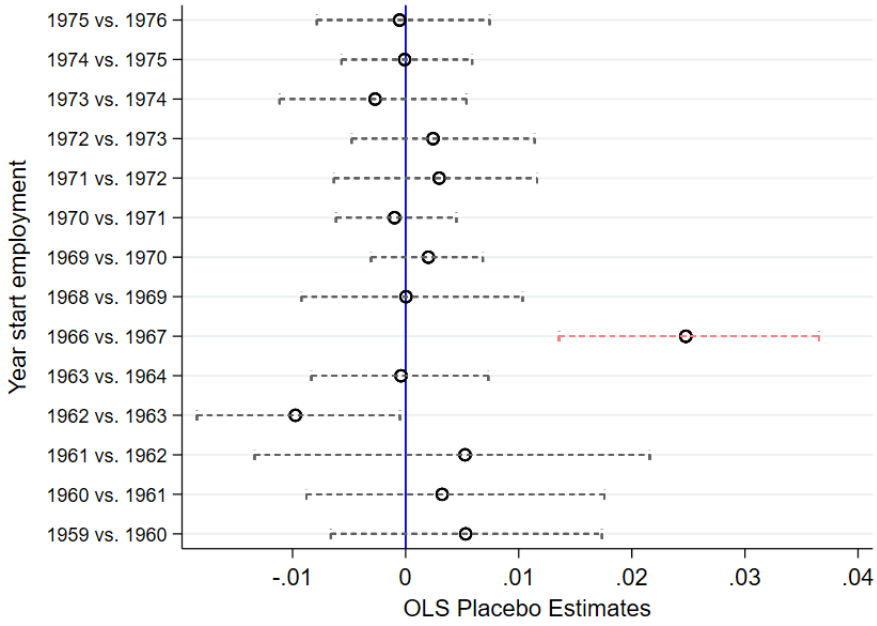


Source: MCVL, cohorts 1938-1949.

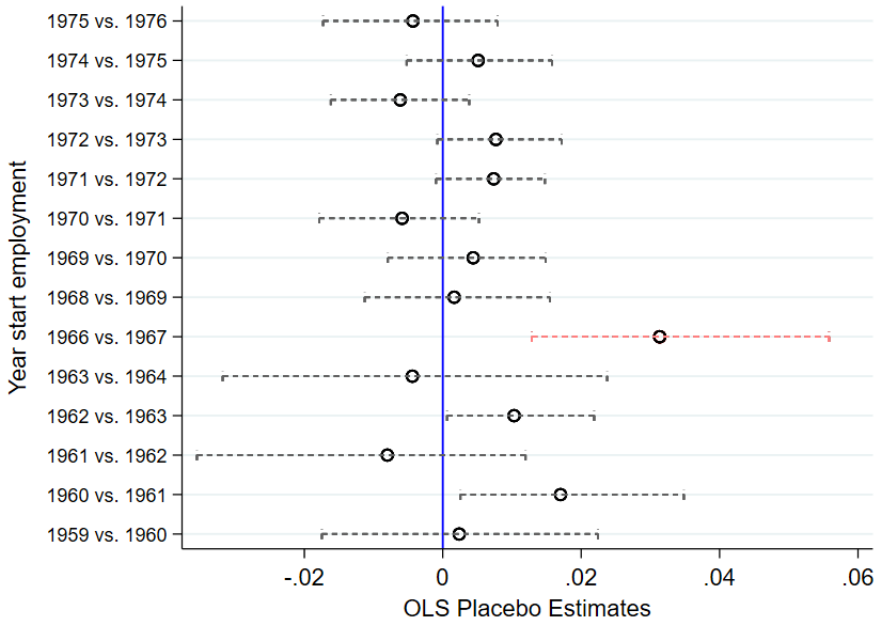
Notes: This figure shows the estimates and the 95 percent confidence intervals of a list of placebos, estimating regression **??** comparing individuals that starting contributing in the years of the y-axis. The red estimate corresponds to the estimation of the regression **??** on the real cutoff: 1966 vs.1967. The outcomes considered are displayed on top of each figure.

Figure 6: Placebo Tests for Mortality: Using Other Cutoffs

(a) Dying between 60 and 69 Years Old



(b) Dying between 50 and 86 Years Old

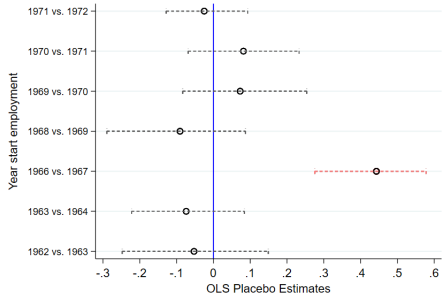


Source: MCVL, cohorts 1938-1949.

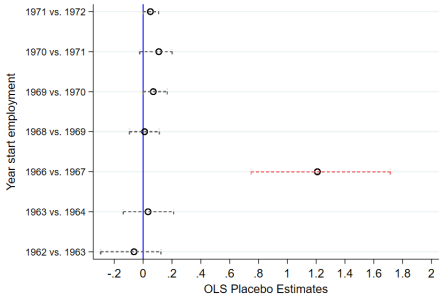
*Notes:*This figure shows the estimates and the 95 percent confidence intervals of a list of placebos, estimating regression **??** comparing individuals that starting contributing in the years of the y-axis. The red estimate corresponds to the estimation of the regression **??** on the real cutoff: 1966 vs.1967. The outcomes considered are displayed on top of each figure.

Figure 7: Alternative Placebo Tests: Keeping Age Started Contributing Constant

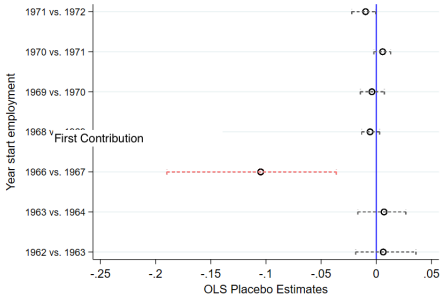
(a) Age at Last Employment



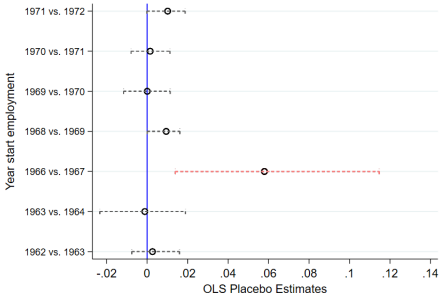
(b) Age at Regular Pension



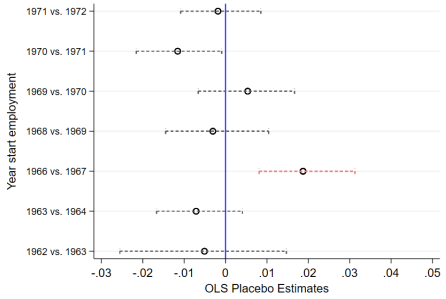
(c) Regular Pension



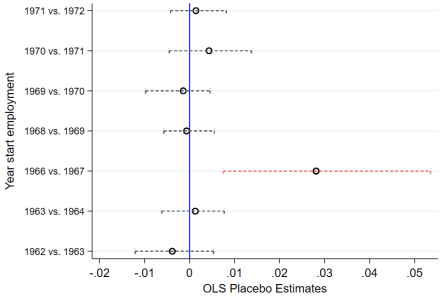
(d) Disability Pension



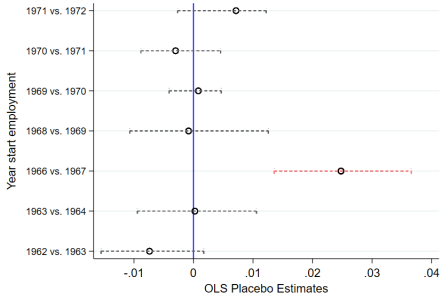
(e) Partial Pension



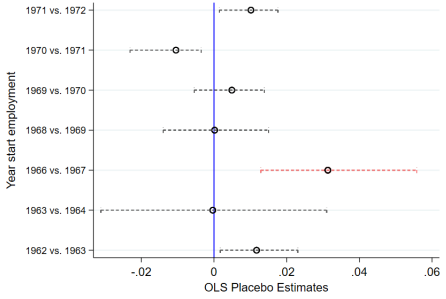
(f) Not Claiming Any Pension



(g) Dying 60-69 Years Old



(h) Dying 50-86 Years Old



Source: MCVL, cohorts 1932-1954.

Notes: This figure shows the estimates and the 95 percent confidence intervals of a list of placebos, estimating regressions **??** and **??** comparing individuals that started contributing in the years of the y-axis between 18 and 29 years old. The red estimate corresponds to the estimation of the regressions **??** and **??** on the real cutoff: 1966 vs.1967. The outcomes considered are displayed on top of each figure.

Table 1: Impact of the Reform on the Type of Pension

	First Pension Claimed			
	Regular Pension	Partial Pension	Disability Insurance	No Pension
	(1)	(2)	(3)	(4)
Contributed in 1967	-0.105***	0.019***	0.058**	0.028***
	(0.033)	(0.005)	(0.021)	(0.009)
	[0.006]	[0.003]	[0.020]	[0.006]
Month-Year Birth FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓
Observations	26,102	26,102	26,102	26,102
R^2	0.129	0.066	0.077	0.030
Mean Dep. Variable (Treated)	0.417	0.048	0.368	0.168
Mean Dep. Variable (Control)	0.557	0.035	0.280	0.129

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the probability of leaving the labour market through regular pension (Column 1), partial pension (Column 2), disability pension (Column 3), and not claiming any pension (Column 4), obtained from the estimation of regression **??**. The estimation sample includes individuals that started contributing 12 months before and after January 1st, 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table 2: Impact of the Reform on the Age at Claiming Pension

	Age of the Individual at				
	Last Employment (1)	First Pension (2)	Regular Pension (3)	Disability Pension (4)	Partial Pension (5)
Contributed in 1967	0.443***	0.248**	1.208***	0.195**	-0.152**
	(0.066)	(0.090)	(0.205)	(0.063)	(0.053)
	[0.003]	[0.031]	[0.001]	[0.031]	[0.032]
Month-Year Birth FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓
Observations	26,102	22,319	13,018	8,251	1,050
R^2	0.087	0.110	0.244	0.035	0.248
Mean Dep. Variable (Treated)	59.948	61.051	64.174	57.509	61.077
Mean Dep. Variable (Control)	59.478	60.924	62.765	57.233	61.126

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the age at last employment (Column 1), at claiming first pension (any type) (Column 2), regular pension (Column 3), disability pension (Column 4), and partial pension (Column 5), obtained from the estimation of regression **??**. The estimation sample includes individuals that started contributing 12 months before and after January 1st, 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table 3: Impact of the Reform on Pension Benefit

	Pension Benefit (1)	Base Pension (2)	Percent of Base Pension (3)
Contributed in 1967	52.106***	22.020***	5.315***
	(10.129)	(5.927)	(1.023)
	[0.001]	[0.008]	[0.001]
Month-Year Birth FE	✓	✓	✓
Controls	✓	✓	✓
Contributed 1966-1967	✓	✓	✓
Observations	22,318	22,318	22,318
R^2	0.388	0.424	0.151
Mean Dep. Variable (Treated)	1199.245	1216.949	86.995
Mean Dep. Variable (Control)	1094.632	1152.355	79.381

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on monthly pension benefit (Column 1), pension base (Column 2), and pension adjustment factor (Column 3), obtained from the estimation of regression **??** for those individuals in our sample that claim any pension. The estimation sample includes individuals that started contributing 12 months before and after January 1st, 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table 4: Impact of Age at Last Employment on Mortality

	Probability of Dying between the Ages					
	50-86 (1)	50-59 (2)	60-69 (3)	70-79 (4)	80-86 (5)	Age at Death Censored 74 (6)
Reduced Form:						
Contributed in 1967	0.031***	0.012	0.025***	0.002	0.000	-0.464***
	(0.009)	(0.007)	(0.005)	(0.007)	(0.002)	(0.151)
	[0.001]	[0.152]	[0.001]	[0.740]	[0.944]	[0.008]
OLS:						
Impact of Age at Last Employment	-0.015***	-0.013***	-0.005***	-0.005***	-0.001***	0.289***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)	(0.018)
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
IV:						
Impact of Age at Last Employment	0.071**	0.027	0.044***	0.003	0.000	-1.059**
	(0.028)	(0.018)	(0.009)	(0.010)	(0.003)	(0.409)
	[0.017]	[0.168]	[0.000]	[0.743]	[0.945]	[0.029]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓
Observations	26,102	26,102	23,922	20,442	16,273	26,102
Mean Dep. Variable (Treated)	0.470	0.097	0.155	0.225	0.086	70.680
Mean Dep. Variable (Control)	0.386	0.074	0.115	0.190	0.063	71.484
F-stat FS	45.338	45.338	77.336	68.340	55.039	52.797

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 50-86 (Column 1), 50-59 (Column 2), 60-69 conditional on surviving until age 59 (Column 3), 70-79 conditional on surviving until age 69 (Column 4), and 80-86 conditional on surviving until age 79 (Column 5). Column 6 reports the impact of age at last employment on age at death censored at 74 years old. The first panel reports the correlation of age at last employment on mortality (OLS), and the second panel shows the effect of the reform on mortality (reduced form effect using regression **??**). The IV estimates, obtained from the estimation of regression **??**, are reported in the third panel. The estimation sample includes individuals that started contributing 12 months before and after January 1st, 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

Table 5: Impact of Age at Last Employment on Mortality at Five-year Intervals

	Probability of Dying between the Ages							
	50-86	50-54	55-59	60-64	65-69	70-74	75-79	80-86
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reduced Form:								
Contributed in 1967	0.031***	0.003	0.009	0.022***	0.009***	0.001	0.001	0.000
	(0.009)	(0.003)	(0.006)	(0.004)	(0.003)	(0.007)	(0.005)	(0.002)
	[0.001]	[0.280]	[0.176]	[0.003]	[0.007]	[0.820]	[0.891]	[0.944]
OLS:								
Impact of Age at Last Employment	-0.015***	-0.008***	-0.007***	-0.003***	-0.003***	-0.003***	-0.003***	-0.001***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
IV:								
Impact of Age at Last Employment	0.071**	0.007	0.018	0.039***	0.015***	0.002	0.001	0.000
	(0.028)	(0.006)	(0.012)	(0.008)	(0.005)	(0.010)	(0.007)	(0.003)
	[0.017]	[0.296]	[0.187]	[0.002]	[0.009]	[0.827]	[0.894]	[0.945]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓	✓	✓
Observations	26,102	26,102	25,316	23,922	22,073	20,442	18,405	16,273
Mean Dep. Variable (Treated)	0.470	0.035	0.065	0.096	0.085	0.108	0.131	0.086
Mean Dep. Variable (Control)	0.386	0.027	0.048	0.065	0.066	0.094	0.106	0.063
F-stat FS	45.338	45.338	58.675	77.336	64.802	68.340	57.175	55.039

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 50-86 (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), 70-74 conditional on surviving until age 69 (Column 6), 75-79 conditional on surviving until age 74 (Column 7), and 80-86 conditional on surviving until age 79 (Column 8). The first panel reports the correlation of age at last employment on mortality (OLS), and the second panel shows the effect of the reform on mortality (reduced form effect using regression **??**). The IV estimates, obtained from the estimation of regression **??**, are reported in the third panel. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table 6: Impact on Mortality by Labour Market Conditions Before Retirement

	Last Industry						Last Occupation	
	Workplace Accidents		Psychosocial Expousure		Self-value		Blue-collar	
	High	Low	High	Low	High	Low	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Age at Last Employment							
First Stage:	0.690***	0.634***	0.568***	0.706***	0.934***	0.500***	0.703***	0.486***
Contributed in 1967	(0.122)	(0.093)	(0.092)	(0.136)	(0.221)	(0.077)	(0.062)	(0.081)
	[0.001]	[0.001]	[0.001]	[0.004]	[0.005]	[0.001]	[0.001]	[0.003]
	Probability of Dying between 60 and 69							
Reduced Form:	0.030***	0.019***	0.030***	0.019**	0.011	0.030***	0.014	0.031***
Contributed in 1967	(0.008)	(0.005)	(0.008)	(0.007)	(0.009)	(0.007)	(0.009)	(0.006)
	[0.002]	[0.006]	[0.003]	[0.016]	[0.237]	[0.004]	[0.136]	[0.001]
IV:	0.043***	0.030***	0.053***	0.027**	0.011	0.061***	0.020	0.063***
Impact of Age at Last Employment	(0.014)	(0.006)	(0.017)	(0.010)	(0.010)	(0.015)	(0.012)	(0.014)
	[0.007]	[0.000]	[0.007]	[0.012]	[0.277]	[0.005]	[0.157]	[0.001]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓	✓	✓
Observations	10,385	10,691	10,344	10,732	6,544	14,532	8,410	15,512
Mean Dep. Variable (Treated)	0.173	0.140	0.168	0.145	0.136	0.165	0.146	0.160
Mean Dep. Variable (Control)	0.123	0.111	0.119	0.115	0.113	0.119	0.118	0.113
F-stat FS	32.204	46.670	38.153	27.008	17.832	42.272	127.511	35.627
P-value Difference (IV Est.)	0.125		0.273		0.019		0.055	

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-and 69 (conditional on surviving until age 59) by the labour market conditions experienced by the individual just before retirement. Individual's last industry is classified depending on their share of workplace accident incidence for our cohorts between 2003 and 2019 (Columns 1 and 2), by the psychosocial exposure (mental stress, social stress, and temporal load) following **?** (Columns 3 and 4), and by their self-value index (sense of achievement and recognition) constructed using O*NET (Columns 5 and 6). We also differentiate if individuals' last occupation pertains to a white or a blue-collar occupation (Columns 7 and 8). The first panel reports the first stage of the IV estimation (the reform's effect on the age at last employment, using **??**). The second panel shows the second stage; the effect on the probability of dying between 60 and 69 years old. First, we report the reduced form effect of the reform on mortality using regression **??**. After that, we report the IV estimates obtained from the estimation of regression **??**. The estimation sample includes individuals that started contributing 12 months before and after January 1st, 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. At the bottom, we report the First Stage F-statistic and the p-value of the differences between groups in the IV estimation. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table 7: Impact on Mortality by Availability of Flexible Retirement

	More 33 Years of Contribution (1)	Less 33 Years of Contribution (2)
	Partial Retirement	
Contributed in 1967	0.043***	0.002
	(0.011)	(0.002)
	[0.004]	[0.115]
	Age at Last Employment	
First Stage:	0.802***	0.460***
Contributed in 1967	(0.141)	(0.070)
	[0.002]	[0.003]
	Probability of Dying between 60 and 69	
Reduced Form:	0.021**	0.025***
Contributed in 1967	(0.007)	(0.005)
	[0.020]	[0.001]
IV:	0.026***	0.054***
Impact of Age at Last Employment	(0.007)	(0.012)
	[0.003]	[0.000]
Month-Year Birth FE	✓	✓
Controls	✓	✓
Contributed 1966-1967	✓	✓
Observations	12,532	11,390
Mean Dep. Variable (Treated)	0.129	0.185
Mean Dep. Variable (Control)	0.103	0.128
F-stat FS	32.272	43.193
P-value Difference (IV Est.)	0.084	

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-and 69 (conditional on surviving until age 59) for individuals with less (Column 1) or more than 33 years of contribution (Column 2). Only individuals with more than 33 years of contribution when claiming a pension can access the partial retirement scheme. The first panel reports the reform's effect on the probability of claiming a partial pension, using **??**). The second panel reports the first stage of the IV estimation (the reform's effect on the age at last employment, using **??**). The third panel shows the second stage; the effect on the probability of dying between 60 and 69 years old. First, we report the reduced form effect of the reform on mortality using regression **??**. After that, we report the IV estimates obtained from the estimation of regression **??**. The estimation sample includes individuals that started contributing 12 months before and after January 1st, 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. At the bottom, we report the First Stage F-statistic and the p-value of the differences between groups in the IV estimation. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported 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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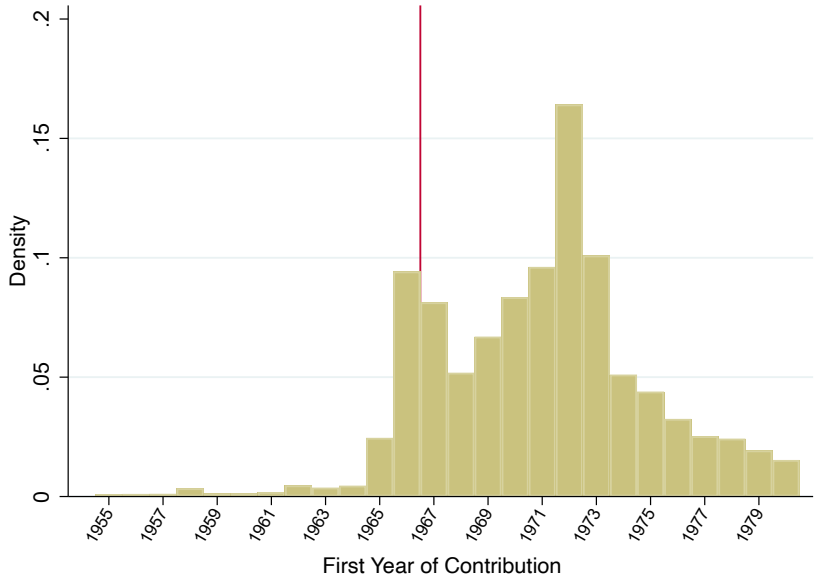
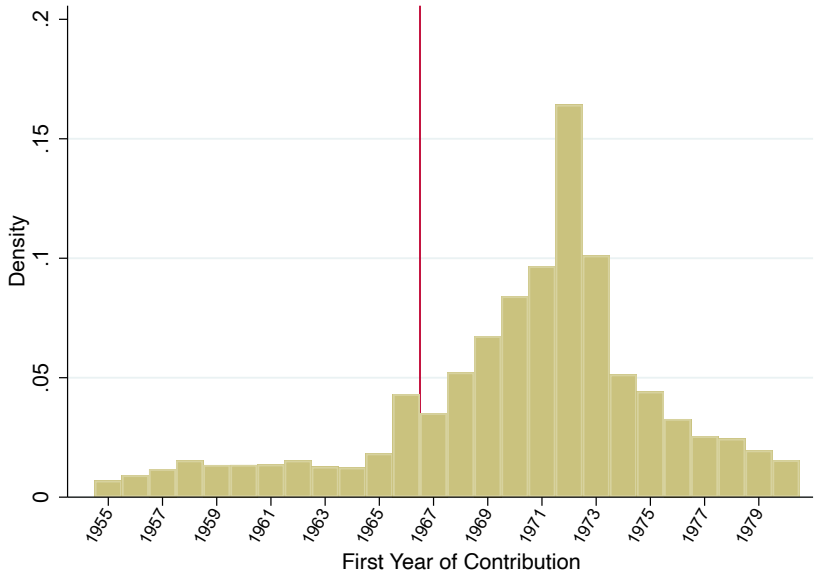
A Appendix Tables and Figures

Figure A1: Correction of Year Started Contributing



(a) Without Correction



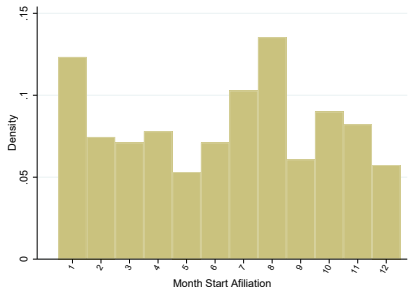
(b) With Correction

Source: MCVL, cohorts 1938-1949.

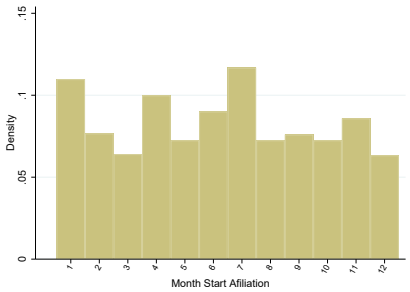
Notes: This figure plots the density of date started contributing without correction (Graph a) and with correction (Graph b). The correction uses the number of years of contribution and the date starting a regular or partial pension (years of contribution are not available for individuals that claim a disability pension) to correct for the date of starting contributing for those whose year of started contributing was between 1965 and 1967.

Figure A2: Distribution of month started contribution by year

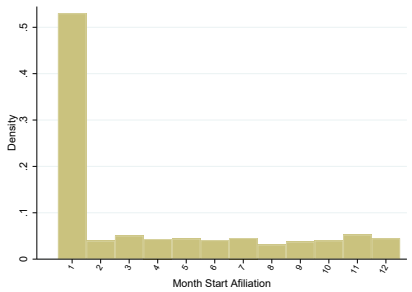
(a) 1963



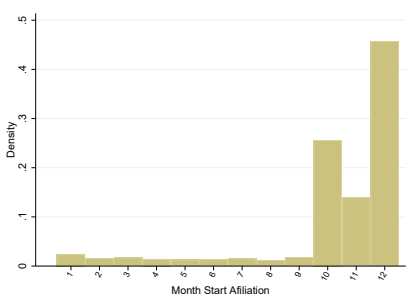
(b) 1964



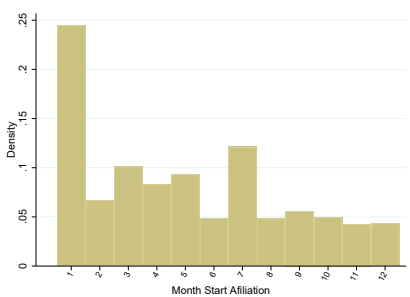
(c) 1965



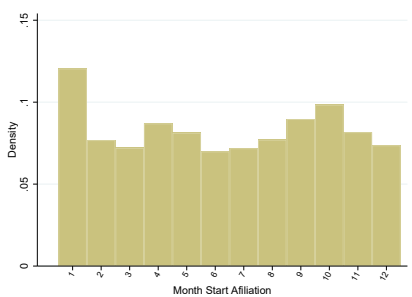
(d) 1966



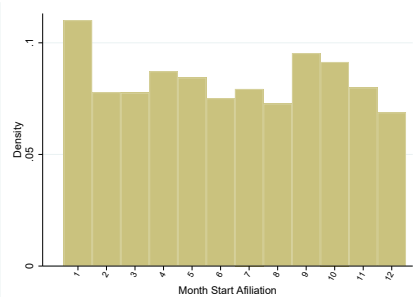
(e) 1967



(f) 1968



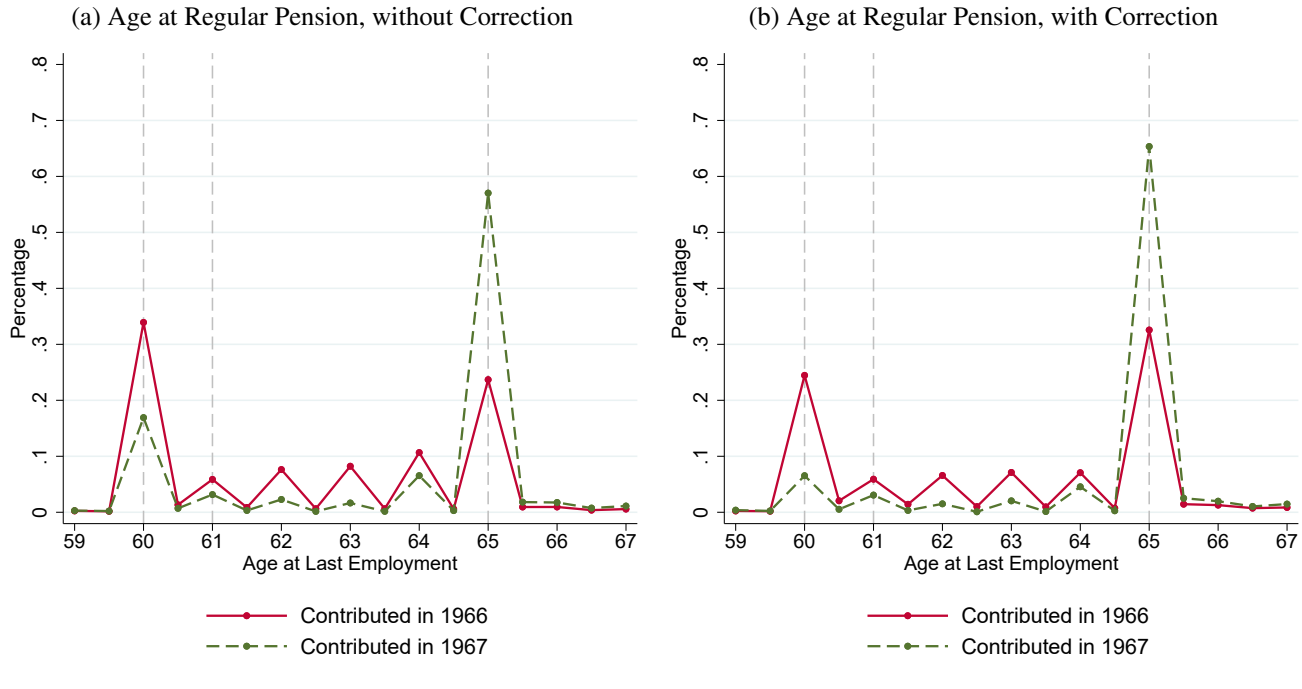
(g) 1969



Source: MCVL, cohorts 1938-1949.

Notes: These figures plot the distribution of individuals by the month they started contributing to the Social Security system for years 1963 to 1969.

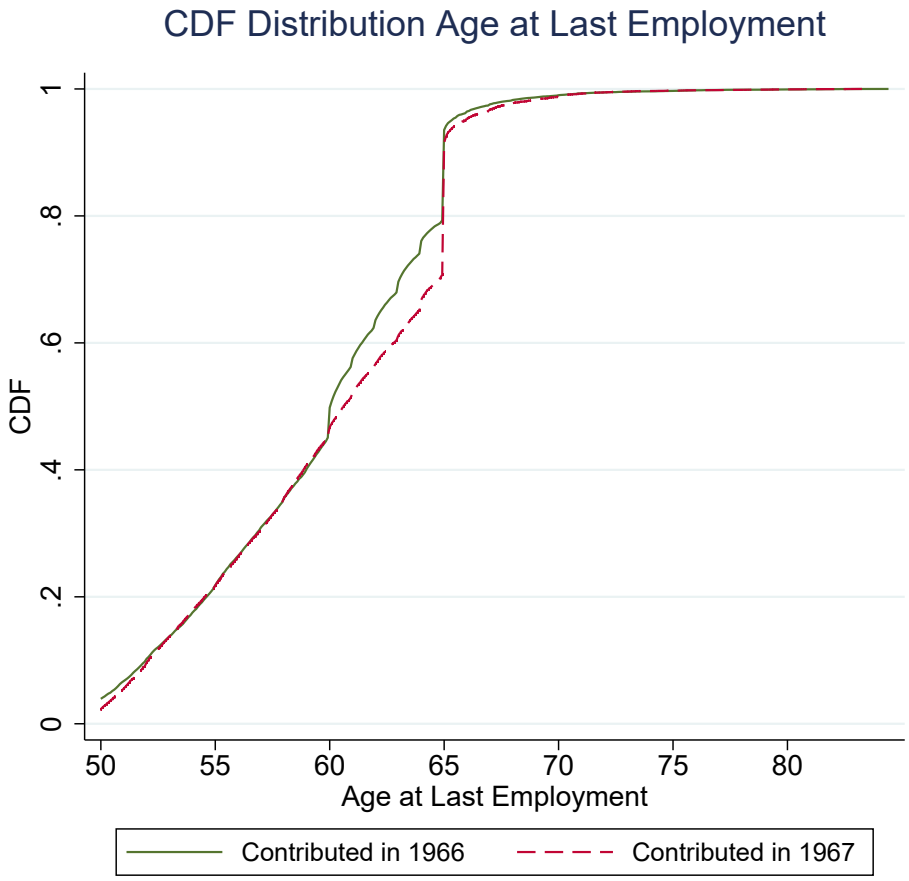
Figure A3: Density of Age at Regular Pension by Treatment Status with and without Correcting Age at First Contribution



Source: MCVL, cohorts 1938-1949.

Notes: This figure plots the percentage of individuals by the age at claiming regular pension without correcting for age start contributing (graph a), and with correcting for age start contributing (graph b). The solid red lines show the density of individuals who started contributing in 1966, while the green dashed lines show those who started contributing in 1967.

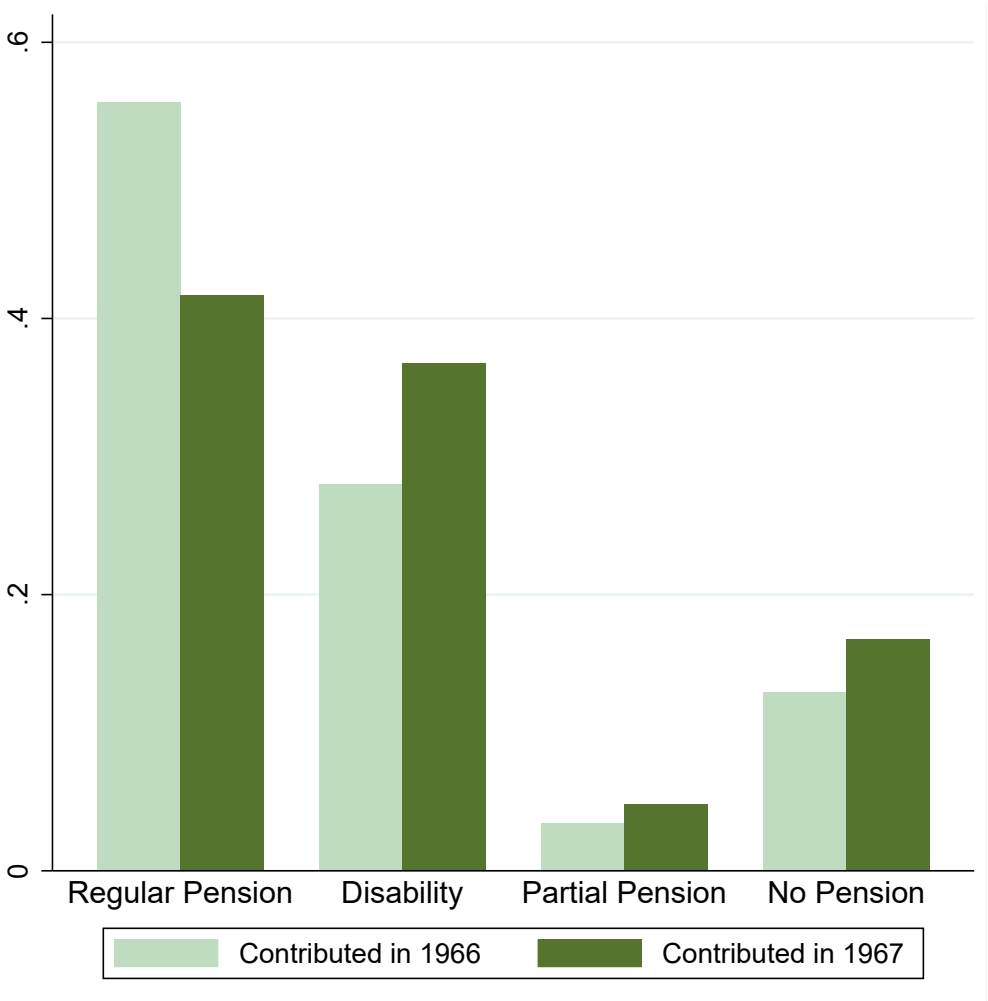
Figure A4: Cumulative Distribution Function of Age at Last Employment by Treatment Status



Source: MCVL, cohorts 1938-1949.

Notes: This figure plots the cumulative distribution function of the age at last employment by treatment status. The solid green line shows the distribution of individuals who started contributing in 1966, while the red dashed line shows those who started contributing in 1967.

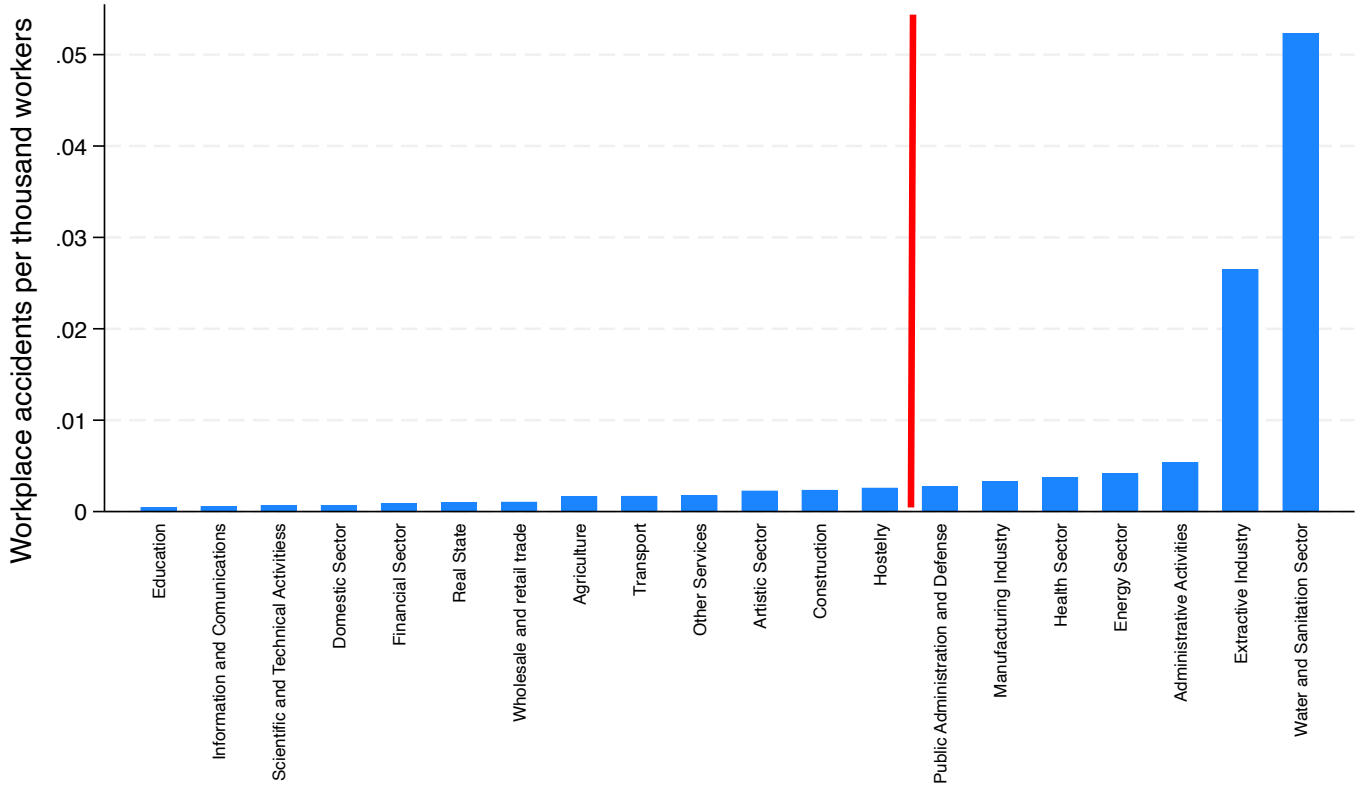
Figure A5: Types of Pension by Treatment Status



Source: MCVL, cohorts 1938-1949.

Notes: This figure plots the percentage of individuals by the different types of pension claimed (regular pension, disability insurance, partial pension, or no pension). The light green bars show the density for individuals that started contributing in 1966, while the dark green bars show the density for those who started contributing in 1967.

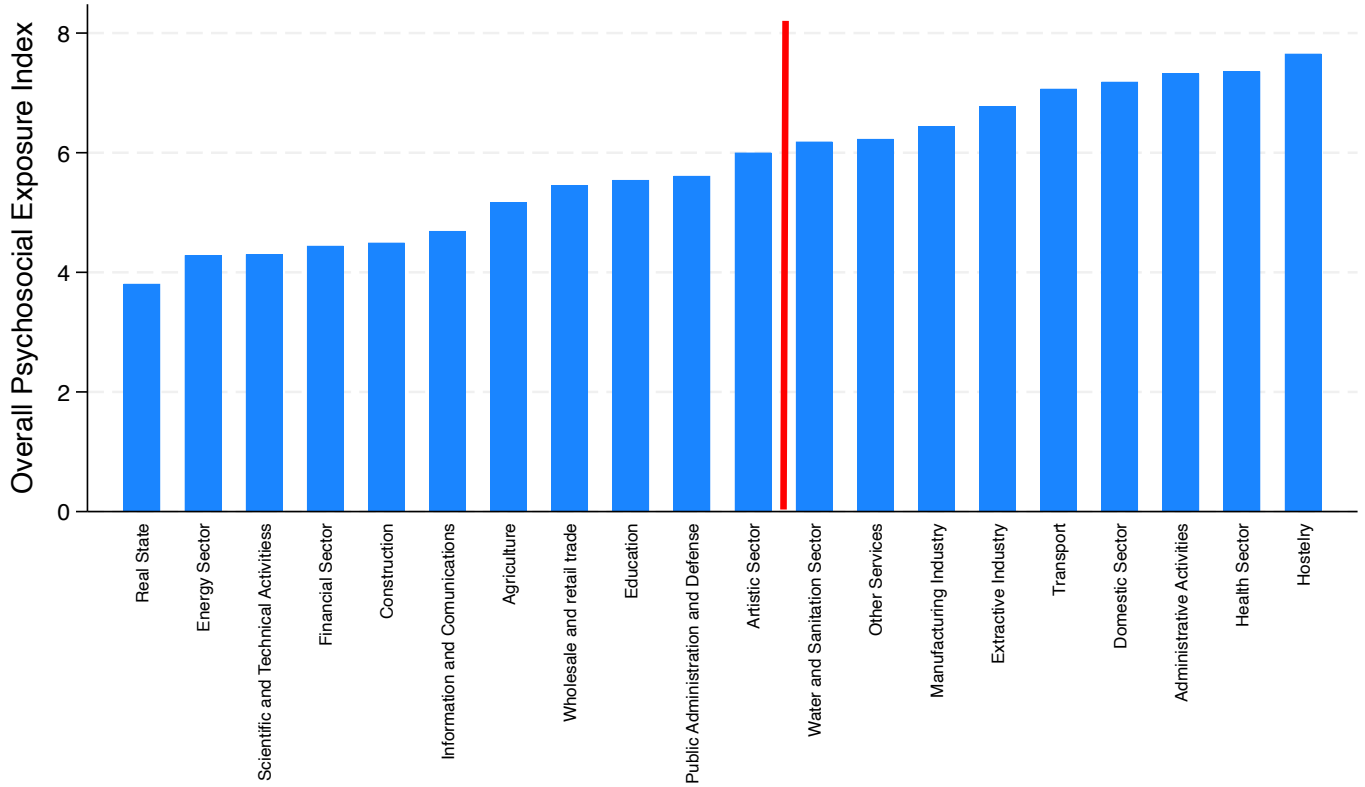
Figure A6: Classification of Industries by Incidence of Workplace Accidents



Source: Register of Workplace Accidents 2003-2019, cohorts 1938-1949.

Notes: This figure plots the share of workplace accidents per 1,000 workers between 2003 and 2019 for workers born between 1938 and 1949 in the industry sector the workers were working at the moment of the accident.

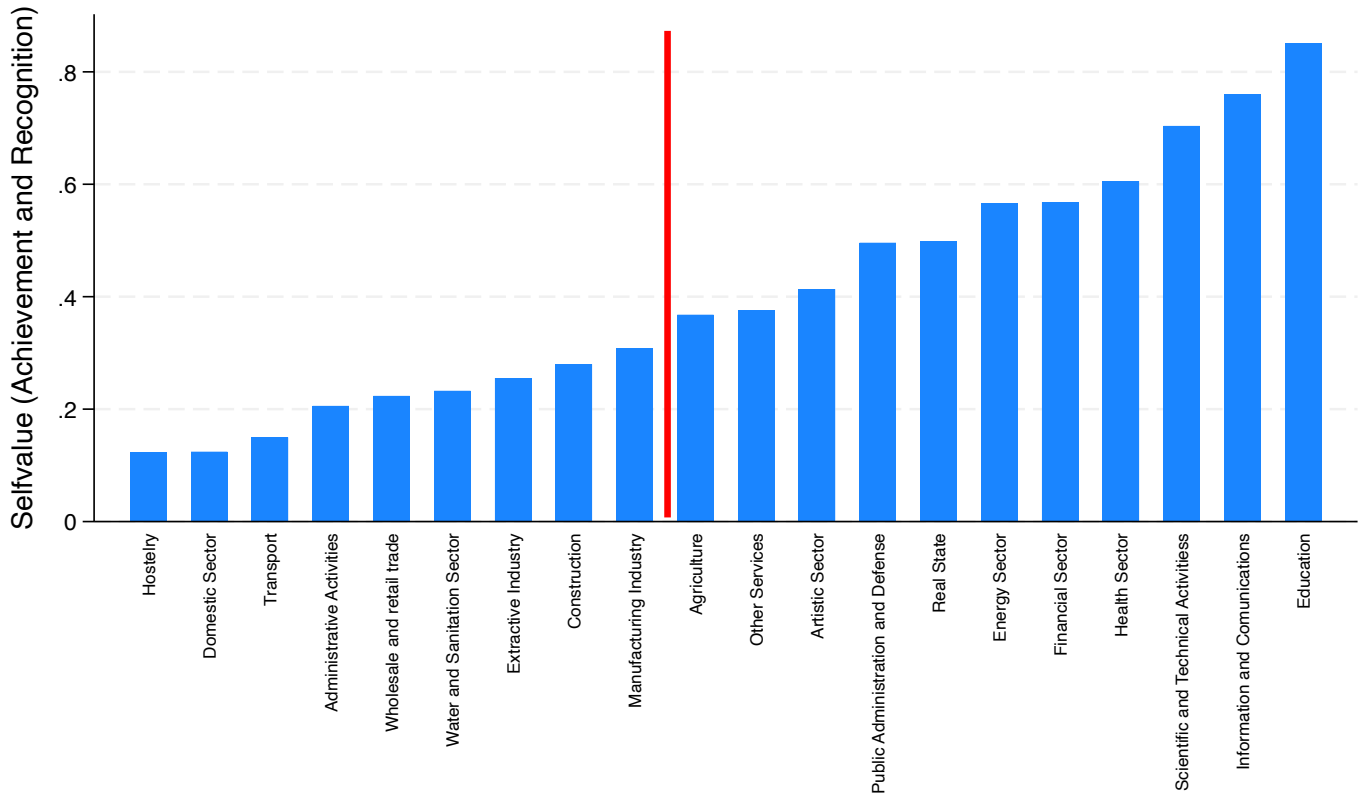
Figure A7: Classification of Industries by Psychosocial Exposure



Source: MCVL, cohorts 1938-1949.

Notes: This figure plots the different industry sectors classified by the degree of psychosocial pressure (mental, social stress, and temporal load) individuals working in these sectors are exposed to. We follow **?** for the definition of psychosocial exposure.

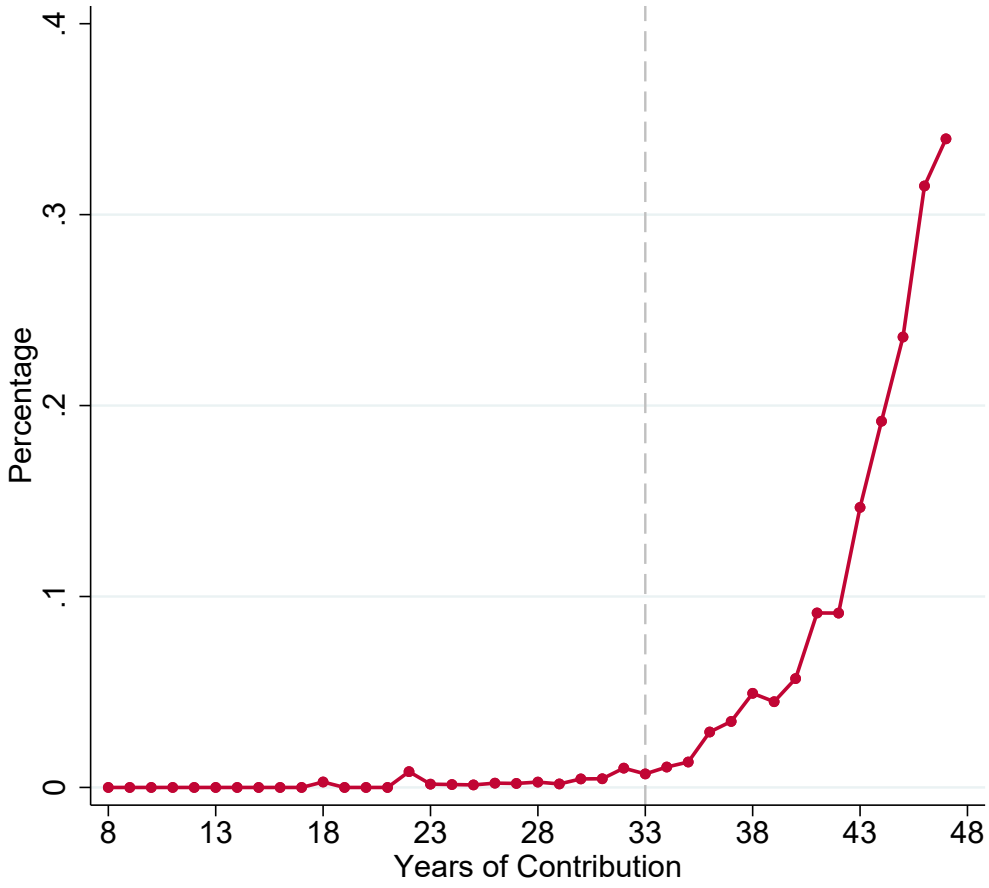
Figure A8: Classification of Industries by Self-value Index



Source: MCVL, cohorts 1938-1949.

Notes: This figure plots the different industry sectors classified by the degree of self-value (sense of achievement and recognition) that individuals working in these sectors are exposed to. We follow the O*NET for the definition of the self-value index.

Figure A9: Probability of Claiming Partial Pension by Years of Contribution



Source: MCVL, cohorts 1938-1949.

Notes: This figure plots the percentage of individuals that claim partial pension depending on the number of years of contribution.

Table A1: Sample Selection

	Sample Selection	
	Observations Dropped (1)	Mortality 60-64 in Obs. Dropped (2)
Contributed in 1967	-0.011	0.011
	(0.012)	(0.010)
	[0.397]	[0.287]
Month-Year Birth FE	✓	✓
Controls	✓	✓
Contributed 1966-1967	✓	✓
Observations	32,743	5,099
R^2	0.031	0.030
Mean Dep. Variable (Treated)	0.190	0.091
Mean Dep. Variable (Control)	0.212	0.060

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the probability of not being in the main sample due to having stopped contributing to the Social Security system before age 50, having claimed a disability pension before 50, not having at least 8 years of contribution, or having claimed the residual SOVI pension (Column 1). Column 2 reports the effect of the reform on mortality between the age 60 and 64 (conditional on surviving until age 59) for the sample of individuals dropped from the main sample, obtained from the estimation of regression **??**. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. All standard errors are clustered at the birth year level, and wild-bootstrap p-values are reported in brackets.

Table A2: Robustness: Dropping Individuals Not Attached to the Labour Market

	Baseline Sample		Drop Active < 50 y.o. Disability < 50 y.o. Active < 8 years SOVI		Drop Active < 50 y.o. Disability < 50 y.o. SOVI		Drop Active < 50 y.o. Active < 8 years SOVI	
	Age Last Employment	Mortality Age 60-69	Age Last Employment	Mortality Age 60-69	Age Last Employment	Mortality Age 60-69	Age Last Employment	Mortality Age 60-69
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Contributed in 1967	0.443***	0.025***	0.363***	0.022***	0.363***	0.024***	0.422***	0.023***
	(0.066)	(0.005)	(0.052)	(0.004)	(0.089)	(0.005)	(0.066)	(0.004)
	[0.003]	[0.000]	[0.000]	[0.000]	[0.010]	[0.000]	[0.004]	[0.000]
IV: Age at Last Employment		0.045***		0.046***		0.052***		0.046***
		(0.009)		(0.010)		(0.013)		(0.010)
		[0.000]		[0.000]		[0.000]		[0.000]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓	✓	✓
Observations	26,102	23,922	26,670	24,530	26,417	24,223	26,569	24,296
Mean Dep. Variable (Treated)	59.948	0.155	60.518	0.148	59.864	0.154	59.888	0.155
Mean Dep. Variable (Control)	59.478	0.115	60.291	0.110	59.440	0.114	59.441	0.115
F-stat FS		92.849		105.265		36.786		95.068

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the age at last employment (Columns 1, 3, 5, and 7) and the probability of dying between the ages 60 and 69 (conditional on surviving until age 59) (Panel A of Columns 2, 4, and 6), obtained from the estimation of regressions **??** and **??** modifying the definition of individuals not attached to the labor market. Panel B of Columns 2, 4, 6, and 8 reports the IV estimates of 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), obtained from the estimation of regression **??** modifying the definition of individuals not attached to the labor market. Columns 1 and 2 drop those individuals that became inactive before the age of 50, got a disability pension before the age of 50, have less than 8 years of activity during her/his working life, or received a SOVI pension. Columns 3 and 4 drop those individuals that became inactive before the age of 50, got a disability pension before the age of 50, or had less than 8 years of activity during her/his working life. Columns 5 and 6 drop those individuals that became inactive before the age of 50, got a disability pension before the age of 50, or received a SOVI pension. Columns 7 and 8 drop those individuals that became inactive before the age of 50, have less than 8 years of activity during her/his working life, or receive a SOVI pension. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A3: Smoothness of the Covariates

	Labor Market between the Ages of 30 and 40				
	Fraction Active	Fraction Employed	Blue-collar Occupation	Av. Monthly Contribution	Fraction Self-employed
	(1)	(2)	(3)	(4)	(5)
Contributed in 1967	0.205	0.667	-0.008	35.557*	2.468***
	(0.334)	(0.503)	(0.009)	(16.567)	(0.524)
	[0.574]	[0.266]	[0.380]	[0.061]	[0.002]
Year Birth FE	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓
Observations	26,102	26,102	26,102	26,102	26,102
R^2	0.164	0.186	0.064	0.212	0.006
Mean Dep. Variable (Treated)	95.140	91.906	0.521	614.608	11.436
Mean Dep. Variable (Control)	91.483	87.378	0.490	581.348	8.608

	Industries between the Ages of 30 and 40					
	Agriculture Minery Construction	Manufacturing	Trade Transportation	Public Health Education	Science Administrative	Services Housekeeping Hostelry
	(6)	(7)	(8)	(9)	(10)	(11)
Contributed in 1967	0.002	-0.005	-0.003	-0.002	0.000	0.000
	(0.007)	(0.003)	(0.003)	(0.006)	(0.001)	(0.002)
	[0.765]	[0.143]	[0.280]	[0.769]	[0.830]	[0.856]
Year Birth FE	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓
Observations	26,102	26,102	26,102	26,102	26,102	26,102
R^2	0.045	0.013	0.008	0.043	0.004	0.004
Mean Dep. Variable (Treated)	0.121	0.098	0.069	0.166	0.024	0.019
Mean Dep. Variable (Control)	0.105	0.097	0.068	0.175	0.025	0.019

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on a list of predetermined variables: fraction of time spent active (Column 1), the fraction of time spent employed (Column 2), probability of having been employed in a blue-collar occupation (Column 3), average monthly contribution (Column 4), the fraction of time self-employed (Column 5), and probability of being employed in the agriculture, minery or construction sectors (Column 6), manufacturing sector (Column 7), trade or transportation sectors (Column 8), public, health or educational sectors (Column 9), scientific or administrative sectors (Column 10), or services, hostelry or housekeeping sectors (Column 11). The p-value of the joint significance test for all the covariates is 0.305. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. All standard errors are clustered at the birth year level, and wild-bootstrap p-values are reported in brackets.

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

Table A4: Impact of the Reform on Pension Benefit by Type of Pension

	Regular Pensions				Disability Pensions				Partial Pensions				No Pension
	Mean Benefit (1)	Base Benefit (2)	Perc Base (3)	Proxy Base (4)	Mean Benefit (5)	Base Benefit (6)	Perc Base (7)	Proxy Base (8)	Mean Benefit (9)	Base Benefit (10)	Perc Base (11)	Proxy Base (12)	Proxy Base (13)
Contributed in 1967	73.353***	25.875**	9.221***	41.753**	-24.547**	-12.882	0.340	-30.795*	-4.953	-12.882	0.416	-38.564	-41.402
	(13.003)	(10.783)	(1.558)	(11.774)	(6.707)	(22.500)	(0.473)	(13.028)	(20.290)	(22.500)	(0.362)	(22.580)	(25.196)
	[0.001]	[0.048]	[0.001]	[0.014]	[0.012]	[0.581]	[0.529]	[0.063]	[0.791]	[0.581]	[0.296]	[0.117]	[0.159]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	12,233	12,233	12,232	12,367	8,630	1,038	8,630	8,630	1,038	1,038	1,038	1,038	3,863
R^2	0.362	0.386	0.311	0.355	0.378	0.496	0.043	0.467	0.476	0.496	0.280	0.387	0.485
Mean Dep. (Treated)	1049.106	1063.049	88.605	1089.888	1255.342	1852.118	84.176	1235.933	1545.399	1852.118	81.723	1684.927	1231.647
Mean Dep. (Control)	952.156	1029.814	75.750	1028.816	1280.379	1856.496	84.164	1277.680	1544.278	1856.496	81.199	1712.329	1186.478

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on monthly pension benefit (Columns 1, 5, and 9), pension base (Column 2, 6, and 10), the pension adjustment factor (Column 3, 7 and 11), and the proxy of the pension base (calculated using years of contribution for those individuals that claimed regular pension and total years of activity for the rest) by type of pension claimed by the individual, obtained from the estimation of regression **??**. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Impact of the Reform on Labour Market Outcomes between the Ages of 45 and 55

	Labor Market between the Ages 45 and 55				
	Fraction Active	Fraction Employed	Blue-collar Occ	Av. Monthly Contribution	Fraction Self-employed
	(1)	(2)	(3)	(4)	(5)
Contributed in 1967	0.754***	2.328***	0.017***	7.783	-0.816**
	(0.224)	(0.611)	(0.005)	(12.921)	(0.334)
	[0.007]	[0.001]	[0.008]	[0.574]	[0.039]
Year Birth FE	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Observations	26,102	26,102	26,102	26,102	26,102
R^2	0.123	0.133	0.414	0.430	0.304
Mean Dep. Variable (Treated)	97.047	88.310	0.463	1179.532	17.326
Mean Dep. Variable (Control)	94.137	83.117	0.431	1109.528	15.899

	Industry between the Ages of 45 and 55					
	Agriculture Minery Construction	Manufacturing	Trade Hostelry	Public Health Education	Science Administrative	Services Housekeeping
	(6)	(7)	(8)	(9)	(10)	(11)
Contributed in 1967	0.000	0.000	-0.019***	-0.026**	-0.006	-0.009**
	(0.005)	(0.004)	(0.004)	(0.009)	(0.004)	(0.003)
	[0.917]	[0.937]	[0.004]	[0.025]	[0.129]	[0.017]
Year Birth FE	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	26,102	26,102	26,102	26,102	26,102	26,102
R^2	0.247	0.079	0.045	0.091	0.041	0.069
Mean Dep. Variable (Treated)	0.132	0.140	0.087	0.319	0.059	0.030
Mean Dep. Variable (Control)	0.122	0.135	0.110	0.348	0.077	0.044

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on a list of labour market outcomes when the individual is between 45 and 55 years old: fraction of time spent active (Column 1), the fraction of time spent employed (Column 2), probability of having been employed in a blue-collar occupation (Column 3), average monthly contribution (Column 4), the fraction of time self-employed (Column 5), and probability of being employed in the agriculture, minery or construction sectors (Column 6), manufacturing sector (Column 7), trade or transportation sectors (Column 8), public, health or educational sectors (Column 9), scientific or administrative sectors (Column 10), or services, hostelry or housekeeping sectors (Column 11), obtained from the estimation of regression **??**. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A6: 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	0.036***	0.044***	0.045***	0.050***
	(0.009)	(0.009)	(0.009)	(0.014)
	[0.006]	[0.000]	[0.001]	[0.008]
Contributed 1966-1967	✓	✓	✓	✓
Month-Year Birth FE	✓	✓	✓	✓
Controls		✓	✓	✓
Proxy Pension Base			✓	✓
LM Controls 45-55				✓
Observations	23,922	23,922	23,922	23,922
Mean Dep. Variable (Treated)	0.155	0.155	0.155	0.155
Mean Dep. Variable (Control)	0.115	0.115	0.115	0.115
F-stat FS	90.753	77.336	92.849	42.828

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 and 69 (conditional on surviving until age 59) with no controls (Column 1), with controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector 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 **??**. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A7: Descriptive Statistics

Dependent Variable	N	Mean	SD	Min	Max
Prob (Regular Pension)	26,102	0.49	0.50	0	1
Prob (Partial Pension)	26,102	0.04	0.19	0	1
Prob (Disability Pension)	26,102	0.31	0.46	0	1
Prob (No Pension)	26,102	0.14	0.35	0	1
Age at Last Employment	26,102	59.67	5.40	18.75	84.41
Age at First Pension	22,319	60.97	4.21	41.41	76.66
Age at Regular Pension	13,018	63.25	2.81	41.41	74
Age at Disability Pension	8,251	57.36	3.74	50	76.66
Age at Partial Pension	1,050	61.10	1.37	56.75	70.58
Prob (Dying between 50-86.)	26,102	0.42	0.49	0	1
Prob (Dying between 50-59)	26,102	0.08	0.27	0	1
Prob (Dying between 60-69)	23,922	0.13	0.33	0	1
Prob (Dying between 70-79)	20,442	0.20	0.40	0	1
Prob (Dying between 80-86)	16,273	0.07	0.25	0	1
Age at Death (censored at 74)					

Source: MCVL, cohorts 1938-1949.

Notes: This table reports summary statistics for the main outcome variables. The sample corresponds to individuals born between 1938 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 contributing to the Social Security system at age 50 with at least 8 years of employment.

Table A8: Impact of the Reform on the Type of Disability

	Type of Disability	
	Severe or Absolute (1)	Partial or Professional (2)
Contributed in 1967	0.031**	0.027**
	(0.012)	(0.011)
	[0.016]	[0.025]
Month-Year Birth FE	✓	✓
Controls	✓	✓
Contributed 1966-1967	✓	✓
Observations	26,102	26,102
R^2	0.039	0.042
Mean Dep. Variable (Treated)	0.176	0.192
Mean Dep. Variable (Control)	0.131	0.149

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the probability of claiming absolute or severe disability (Column 1) and partial or professional disability (Column 2), obtained from the estimation of regression **??**. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A9: Impact of the Reform on Reason for No Pension

		Reason for No Pension		
	No Pension	Still Working	Became Inactive	Died before Pension
	(1)	(2)	(3)	(4)
Contributed in 1967	0.028**	0.000	0.009	0.019***
	(0.009)	(0.001)	(0.006)	(0.005)
	[0.006]	[0.473]	[0.144]	[0.004]
Month-Year Birth FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓
Observations	26,102	26,102	26,102	26,102
R^2	0.030	0.006	0.015	0.032
Mean Dep. Variable (Treated)	0.168	0.002	0.062	0.104
Mean Dep. Variable (Control)	0.129	0.002	0.054	0.074

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the probability of leaving the labour market without claiming any pension (Column 1), continuing working (Column 2), becoming inactive (Column 3), and dying before claiming a pension (Column 4), obtained from the estimation of regression **??**. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A10: Impact of the Reform on Age at Last Employment (in Brackets)

	Last Employmennt at Age								
	50-54	55-59	60	61	62	63	64	65	After 65
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Contributed in 1967	0.005	-0.002	-0.043***	-0.011**	-0.012***	-0.011***	0.004	0.071***	0.016***
	(0.009)	(0.005)	(0.014)	(0.005)	(0.004)	(0.003)	(0.003)	(0.012)	(0.004)
	[0.573]	[0.776]	[0.009]	[0.026]	[0.006]	[0.009]	[0.103]	[0.001]	[0.003]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	26,102	26,102	26,102	26,102	26,102	26,102	26,102	26,102	26,102
R^2	0.038	0.036	0.020	0.007	0.008	0.012	0.009	0.076	0.050
Mean Dep. Variable (Treated)	0.193	0.240	0.060	0.048	0.042	0.048	0.056	0.243	0.085
Mean Dep. Variable (Control)	0.175	0.238	0.111	0.062	0.056	0.061	0.052	0.169	0.065

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the probability of leaving the labour market between the ages of 50-54 (Column 1), 55-59 (Column 2), at 60 (Column 3), at 61 (Column 4), at 62 (Column 5), at 63 (Column 6), at 64 (Column 7), at 65 (Column 8), and after age 65 (Column 9), obtained from the estimation of regression **??**. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A11: Impact of the Reform on Age at Regular Pension (in Brackets)

	Regular Pension at Age						
	60	61	62	63	64	65	After 65
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Contributed in 1967	-0.099***	-0.020***	-0.032***	-0.032***	-0.021***	0.093***	0.036***
	(0.024)	(0.006)	(0.005)	(0.006)	(0.005)	(0.014)	(0.008)
	[0.001]	[0.005]	[0.001]	[0.001]	[0.001]	[0.001]	[0.003]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓	✓
Observations	26,102	26,102	26,102	26,102	26,102	26,102	26,102
R^2	0.087	0.029	0.023	0.028	0.019	0.105	0.139
Mean Dep. Variable (Treated)	0.029	0.014	0.007	0.009	0.020	0.283	0.159
Mean Dep. Variable (Control)	0.148	0.041	0.042	0.045	0.043	0.189	0.112

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the probability of claiming a regular pension between the ages of 50-54 (Column 1), 55-59 (Column 2), at 60 (Column 3), at 61 (Column 4), at 62 (Column 5), at 63 (Column 6), at 64 (Column 7), at 65 (Column 8), and after age 65 (Column 9), obtained from the estimation of regression **??**. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A12: Impact of the Reform on Age at Disability Pension (in Brackets)

	Disability at Age			
	50-54	55-59	60-64	After 65
	(1)	(2)	(3)	(4)
Contributed in 1967	0.015**	0.015	0.026***	0.001
	(0.007)	(0.012)	(0.004)	(0.001)
	[0.048]	[0.218]	[0.001]	[0.193]
Month-Year Birth FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓
Observations	26,102	26,102	26,102	26,102
R^2	0.021	0.041	0.016	0.003
Mean Dep. Variable (Treated)	0.103	0.159	0.102	0.004
Mean Dep. Variable (Control)	0.079	0.131	0.067	0.002

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the probability of claiming a disability pension between the ages of 50-54 (Column 1), 55-59 (Column 2), 60-56 (Column 3), and after age 65 (Column 4), obtained from the estimation of regression **??**. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A13: Characteristics of compliers

Characteristics	$P[X = x]$	$P[X = x\|complier]$	$\frac{P[X=x\|complier]}{P[X=x]}$
Men	0.727	0.543	0.745
Women	0.273	0.548	2.01
Characteristics between age 30 and 40			
Probability of being employed below median	0.254	0.257	1.012
Probability of being employed above median	0.746	0.751	1.001
Probability of being self-employed below median	0.859	0.830	0.967
Probability of being self-employed above median	0.141	0.062	0.441
Lower than average years of contribution	0.505	0.619	1.225
Higher than average years of contribution	0.495	0.416	0.840
Blue collar	0.502	0.562	1.119
White collar	0.498	0.469	0.942
Characteristics of last job			
High workplace accidents	0.560	0.311	0.556
Low workplace accidents	0.440	0.732	1.664
High psychosocial exposure	0.552	0.365	0.661
Low psychosocial exposure	0.448	0.629	1.404
Low self-value	0.605	0.549	0.908
High self-value	0.394	0.451	1.144

Source: MCVL, cohorts 1938-1949.
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. replications.
*** p<0.01, ** p<0.05, * p<0.1.

Table A14: Impact on Mortality by Gender

	Age at Last Employment	
	Men	Women
	(1)	(2)
First Stage:	0.349**	1.109***
Contributed in 1967	(0.097)	(0.117)
	[0.017]	[0.001]
	Probability of Dying between 60 and 69	
Reduced Form:	0.027***	0.022***
Contributed in 1967	(0.006)	(0.005)
	[0.004]	[0.005]
IV:	0.077***	0.020**
Impact of Age at Last Employment	(0.023)	(0.006)
	[0.001]	[0.011]
Month-Year Birth FE	✓	✓
Controls	✓	✓
Contributed 1966-1967	✓	✓
Observations	17,037	6,885
Mean Dep. Variable (Treated)	0.178	0.067
Mean Dep. Variable (Control)	0.152	0.044
F-stat FS	12.876	89.647
P-value Difference (IV Est.)	0.014	

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-and 69(conditional on surviving until age 59) for men (Column 1) and women (Column 2). The first panel reports the first stage of the IV estimation (the reform's effect on the age at last employment, using **??**). The second panel shows the second stage; the effect on the probability of dying between 60 and 69 years old. First, we report the reduced form effect of the reform on mortality using regression **??**. After that, we report the IV estimates obtained from the estimation of regression **??**. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. At the bottom, we report the First Stage F-statistic and the p-value of the differences between groups in the IV estimation. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A15: Impact on Labour Market Conditions Before Retirement

	Last Industry			Last Occupation
	High Workplace Accidents	High Psychosocial	High Selfvalue	Blue collar
	(1)	(2)	(3)	(4)
Contributed in 1967	0.010	-0.001	0.016	0.016***
	(0.007)	(0.006)	(0.009)	(0.004)
	[0.190]	[0.851]	[0.103]	[0.004]
Observations	22,956	22,956	22,956	26,102
R^2	0.132	0.093	0.088	0.454

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the labour market conditions experienced by the individual just before retirement: the probability of working in an industry with high share of workplace accidents per 1,000 workers (Column 1), with high psychosocial exposure (Column 2), high self-value index (Column 3), and if individuals' last occupation pertains to a white-collar occupation (Column 4). The estimation sample includes individuals that started contributing 12 months before and after January 1st, 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table A16: Impact on Mortality by Workplace Accidents: Division by 3 Groups

	Last Industry		
	Workplace Accidents		
	High	Medium	Low
	(1)	(2)	(3)
	Age at Last Employment		
First Stage:	0.484***	0.748***	0.762***
Contributed in 1967	(0.097)	(0.218)	(0.113)
	[0.004]	[0.019]	[0.001]
	Probability of Dying between 60 and 69		
Reduced Form:	0.036***	0.012	0.023***
Contributed in 1967	(0.010)	(0.007)	(0.005)
	[0.004]	[0.106]	[0.003]
IV:	0.075**	0.016*	0.030***
Impact of Age at Last Employment	(0.028)	(0.009)	(0.008)
	[0.016]	[0.090]	[0.006]
Month-Year Birth FE	✓	✓	✓
Controls	✓	✓	✓
Contributed 1966-1967	✓	✓	✓
Observations	7,451	6,695	6,930
Mean Dep. Variable (Treated)	0.179	0.155	0.133
Mean Dep. Variable (Control)	0.120	0.129	0.102
F-stat FS	24.926	11.758	45.218
P-value Difference (Low-Medium)		0.256	
P-value Difference (Medium-High)		0.044	
P-value Difference (Low-High)		0.139	

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-and 69 (conditional on surviving until age 59) by the share of workplace accident incidence for our cohorts between 2003 and 2019 experienced by the individual just before retirement. The first panel reports the first stage of the IV estimation (the reform's effect on the age at last employment, using **??**). The second panel shows the second stage; the effect on the probability of dying between 60 and 69 years old. First, we report the reduced form effect of the reform on mortality using regression **??**. After that, we report the IV estimates obtained from the estimation of regression **??**. The estimation sample includes individuals that started contributing 12 months before and after January 1st, 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. At the bottom, we report the First Stage F-statistic and the p-value of the differences between groups in the IV estimation. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A17: Impact on Mortality by Psychosocial Exposure: Division by 3 Groups

	Last Industry		
	Psychosocial Expousure		
	High	Medium	Low
	(1)	(2)	(3)
	Age at Last Employment		
First Stage:	0.559***	0.811***	0.496***
Contributed in 1967	(0.098)	(0.145)	(0.122)
	[0.001]	[0.004]	[0.007]
	Probability of Dying between 60 and 69		
Reduced Form:	0.030***	0.031***	0.005
Contributed in 1967	(0.008)	(0.007)	(0.012)
	[0.004]	[0.002]	[0.681]
IV:	0.054***	0.038***	0.011
Impact of Age at Last Employment	(0.018)	(0.011)	(0.021)
	[0.012]	[0.010]	[0.658]
Month-Year Birth FE	✓	✓	✓
Controls	✓	✓	✓
Contributed 1966-1967	✓	✓	✓
Observations	9,773	6,852	4,451
Mean Dep. Variable (Treated)	0.170	0.149	0.139
Mean Dep. Variable (Control)	0.119	0.107	0.126
F-stat FS	32.470	31.343	16.380
P-value Difference (Low-Medium)		0.281	
P-value Difference (Medium-High)		0.519	
P-value Difference (Low-High)		0.176	

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-and 69 (conditional on surviving until age 59) by the psychosocial exposure (mental stress, social stress, and temporal load) experienced by the individual just before retirement, measured following **?**. The first panel reports the first stage of the IV estimation (the reform's effect on the age at last employment, using **??**). The second panel shows the second stage; the effect on the probability of dying between 60 and 69 years old. First, we report the reduced form effect of the reform on mortality using regression **??**. After that, we report the IV estimates obtained from the estimation of regression **??**. The estimation sample includes individuals that started contributing 12 months before and after January 1st, 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. At the bottom, we report the First Stage F-statistic and the p-value of the differences between groups in the IV estimation. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table A18: Robustness: Impact by Availability of Flexible Retirement

	Baseline Sample		23 to 43 Years of Contribution		28 to 38 Years of Contribution	
	< 33	≥ 33	< 33	≥ 33	< 33	≥ 33
	(1)	(2)	(3)	(4)	(5)	(6)
	Partial Retirement					
Contributed in 1967	0.002	0.043***	0.004**	0.027**	0.001	0.017**
	(0.002)	(0.011)	(0.002)	(0.009)	(0.002)	(0.007)
	[0.115]	[0.004]	[0.017]	[0.005]	[0.647]	[0.029]
	Age at Last Employment					
First Stage:	0.460***	0.802**	0.432**	0.788***	0.298	0.427**
Contributed in 1967	(0.070)	(0.141)	(0.099)	(0.138)	(0.137)	(0.110)
	[0.003]	[0.002]	[0.036]	[0.004]	[0.104]	[0.021]
	Probability of Dying between 60 and 69					
Reduced Form:	0.025***	0.021**	0.033***	0.024***	0.033	0.007
Contributed in 1967	(0.005)	(0.007)	(0.009)	(0.006)	(0.019)	(0.018)
	[0.001]	[0.020]	[0.006]	[0.007]	[0.121]	[0.685]
IV:	0.054***	0.026***	0.077***	0.030***	0.110*	0.017
Impact of Age at Last Employment	(0.012)	(0.007)	(0.022)	(0.008)	(0.071)	(0.040)
	[0.000]	[0.003]	[0.006]	[0.004]	[0.079]	[0.681]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓
Observations	11,390	12,532	5,851	8,930	4.691	3,495
Mean Dep. Variable (Treated)	0.185	0.129	0.218	0.155	0.243	0.222
Mean Dep. Variable (Control)	0.128	0.103	0.172	0.125	0.210	0.198
F-stat FS	43.193	32.272	18.988	32.609	4.691	15.162
P-value Difference (IV Est.)	0.084		0.063		0.248	

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 and 69 (conditional on surviving until age 59) for individuals with less (Columns 1, 3, and 5) or more than 33 years of contribution (Columns 2, 4, and 6), using different samples. Columns 1 and 2 report our baseline estimates from Table **??**. Columns 3 and 4 reduce the sample to individuals that have between 23 and 43 years of contribution, while Columns 5 and 6 to individuals that contributed between 28 and 38 years. Only individuals with more than 33 years of contribution when claiming a pension can access the partial retirement scheme. The first panel reports the reform's effect on the probability of claiming a partial pension, using **??**. The second panel reports the first stage of the IV estimation (the reform's effect on the age at last employment, using **??**). The third panel shows the second stage; the effect on the probability of dying between 60 and 69 years old. First, we report the reduced form effect of the reform on mortality using regression **??**. After that, we report the IV estimates obtained from the estimation of regression **??**. The estimation sample includes individuals that started contributing 12 months before and after January 1st, 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. At the bottom, we report the First Stage F-statistic and the p-value of the differences between groups in the IV estimation. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A19: Robustness: Age Start FE

	Type of Pension				Age at			
	Regular Pension	Partial Pension	Disability Pension	No Pension	Last Employment	Regular Pension	Disability Pension	Partial Pension
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Contributed in 1967	-0.098**	0.025***	0.052**	0.020**	0.525**	1.362***	0.331**	-0.244**
	(0.032)	(0.006)	(0.021)	(0.009)	(0.163)	(0.156)	(0.109)	(0.104)
	[0.014]	[0.005]	[0.023]	[0.036]	[0.022]	[0.001]	[0.040]	[0.048]
Age Start Contributing FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓	✓	✓
Observations	25,764	25,764	25,764	25,764	25,764	12,233	8,630	1,038
R^2	0.135	0.069	0.086	0.054	0.083	0.219	0.033	0.228
Mean Dep. (Treated)	0.394	0.048	0.390	0.168	59.830	64.636	57.350	61.097
Mean Dep. (Control)	0.531	0.035	0.297	0.137	59.386	63.032	56.986	61.131

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the probability of leaving the labour market through regular pension (Column 1), partial pension (Column 2), disability pension (Column 3), not claiming any pension (Column 4), age at last employment (Column 5), age at claiming regular pension (Column 6), age at claiming disability pension (Column 7), and age at claiming partial pension (Column 8), obtained from the estimation of regression **??** using age at first contribution fixed effects instead of month and year of birth fixed effects. This robustness check estimates the impact of losing access to early retirement for people that start working at the same age. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender and age at first contribution fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the age of the first contribution, and wild-bootstrap p-values are reported in brackets.

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

Table A20: Robustness: No Controls

	Type of Pension				Age at			
	Regular Pension (1)	Partial Pension (2)	Disability Pension (3)	No Pension (4)	Last Employment (5)	Regular Pension (6)	Partial Pension (7)	Disability Pension (8)
Contributed in 1967	-0.103***	0.019***	0.057***	0.027***	0.533***	1.278***	0.256*	-0.193***
	(0.030)	(0.005)	(0.020)	(0.008)	(0.100)	(0.160)	(0.080)	(0.058)
	[0.004]	[0.001]	[0.008]	[0.006]	[0.000]	[0.000]	[0.095]	[0.008]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls								
Contributed 1966-1967	✓	✓	✓	✓	✓	✓	✓	✓
Observations	26,102	26,102	26,102	26,102	26,102	13,018	8,251	1,050
R^2	0.079	0.035	0.053	0.021	0.013	0.084	0.005	0.157
Mean Dep. Variable (Treated)	0.417	0.048	0.368	0.168	59.948	64.174	57.509	61.077
Mean Dep. Variable (Control)	0.557	0.035	0.280	0.129	59.478	62.765	57.233	61.126

Source: MCVL, cohorts 1941-1949.

Notes: This table reports the impact of the reform on the probability of leaving the labour market through regular pension (Column 1), partial pension (Column 2), disability pension (Column 3), not claiming any pension (Column 4), age at last employment (Column 5), age at claiming regular pension (Column 6), age at claiming disability pension (Column 7), and age at claiming partial pension (Column 8), obtained from the estimation of regression **??** not controlling for any of the labor market outcomes when the individuals were between 30 and 40 years old. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A21: Robustness: Cohorts 1941 to 1949

	Type of Pension				Age at			
	Regular Pension (1)	Partial Pension (2)	Disability Pension (3)	No Pension (4)	Last Employment (5)	Regular Pension (6)	Partial Pension (7)	Disability Pension (8)
Contributed in 1967	-0.160***	0.025***	0.091***	0.044***	0.418**	1.563***	0.146	-0.153**
	(0.020)	(0.006)	(0.015)	(0.005)	(0.134)	(0.057)	(0.111)	(0.050)
	[0.001]	[0.001]	[0.001]	[0.000]	[0.011]	[0.000]	[0.501]	[0.034]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓	✓	✓
Observations	18,491	18,491	18,491	18,491	18,491	10,093	5,159	1,005
R^2	0.126	0.060	0.075	0.023	0.076	0.278	0.043	0.202
Mean Dep. Variable (Treated)	0.427	0.068	0.354	0.152	60.208	64.323	57.477	60.998
Mean Dep. Variable (Control)	0.624	0.046	0.230	0.100	59.727	62.614	57.262	61.070

Source: MCVL, cohorts 1941-1949.

Notes: This table reports the impact of the reform on the probability of leaving the labour market through regular pension (Column 1), partial pension (Column 2), disability pension (Column 3), not claiming any pension (Column 4), age at last employment (Column 5), age at claiming regular pension (Column 6), age at claiming disability pension (Column 7), and age at claiming partial pension (Column 8), obtained from the estimation of regression **??** restringing the sample to cohorts born between 1941 and 1949. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A22: Robustness: No Self-employed

	Type of Pension				Age at			
	Regular Pension (1)	Partial Pension (2)	Disability Pension (3)	No Pension (4)	Last Employment (5)	Regular Pension (6)	Partial Pension (7)	Disability Pension (8)
Contributed in 1967	-0.112***	0.023***	0.049**	0.040***	0.529***	1.400***	0.220	-0.150**
	(0.030)	(0.006)	(0.019)	(0.009)	(0.097)	(0.165)	(0.076)	(0.048)
	[0.004]	[0.001]	[0.014]	[0.003]	[0.001]	[0.000]	[0.119]	[0.023]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓	✓	✓
Observations	21,664	21,664	21,664	21,664	21,664	9,980	6,854	1,047
R^2	0.150	0.066	0.096	0.046	0.066	0.217	0.035	0.248
Mean Dep. Variable (Treated)	0.362	0.060	0.370	0.208	59.267	63.698	57.307	61.075
Mean Dep. Variable (Control)	0.527	0.041	0.281	0.152	58.886	62.305	57.048	61.122

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the probability of leaving the labour market through regular pension (Column 1), partial pension (Column 2), disability pension (Column 3), not claiming any pension (Column 4), age at last employment (Column 5), age at claiming regular pension (Column 6), age at claiming disability pension (Column 7), and age at claiming partial pension (Column 8), obtained from the estimation of regression **??** restringing the sample to individuals that are not in one of the self-employed pension regimes. The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A23: Robustness: Mortality Outcomes

	Probability of Dying between the Ages 60 and 69				
	Baseline	Age Start FE	No Controls	Cohorts 1941-1949	Drop Self-employed
	(1)	(2)	(3)	(4)	(5)
Contributed in 1967	0.025***	0.021***	0.029**	0.031***	0.028***
	(0.005)	(0.005)	(0.010)	(0.006)	(0.007)
	[0.001]	[0.000]	[0.001]	[0.003]	[0.000]
IV: Age at Last Employment	0.044***	0.030***	0.036***	0.058***	0.043***
	(0.009)	(0.010)	(0.009)	(0.011)	(0.012)
	[0.000]	[0.002]	[0.006]	[0.002]	[0.001]
Month-Year Birth FE	✓		✓	✓	✓
Age Start Contributing FE		✓			
Controls	✓	✓		✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓
Observations	23,922	23,922	23,922	17,184	19,579
Mean Dep. Variable (Treated)	0.155	0.155	0.155	0.145	0.172
Mean Dep. Variable (Control)	0.115	0.115	0.115	0.098	0.123
F-stat FS	77.336	17.681	90.753	57.011	73.376

textitSource: 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) using the baseline specification (Column 1), using age at first contribution fixed effects instead of month and year of birth fixed effects (Column 2), no controlling for any labor market outcome of the individuals were between 30 and 40 years old (Column 3),restringing the sample to cohorts born between 1941 and 1949 (Column 4), and restringing the sample to individuals that are not in one of the self-employed pension regimes (Column 5). The first panel reports the effect of the reform on mortality (reduced form effect using regression **??**). The IV estimates, obtained from the estimation of regression **??**, are reported in the second panel. The estimation sample includes individuals that started contributing 12 months before and after January 1st, 1967. All specifications control for gender fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A24: Robustness: Correction of the Year Start Contributing

	Baseline Correction 1966-1967		No Correction		Correction 1966-1967 Removing Months 12-1966 and 1-1967	
	Age Last Employment	Mortality Age 60-69	Age Last Employment	Mortality Age 60-69	Age Last Employment	Mortality Age 60-69
	(1)	(2)	(3)	(4)	(5)	(6)
Contributed in 1967	0.443***	0.025***	0.552**	0.017***	0.259***	0.024***
	(0.066)	(0.005)	(0.210)	(0.003)	(0.074)	(0.008)
	[0.003]	[0.000]	[0.009]	[0.000]	[0.008]	[0.004]
IV: Age at Last Employment		0.045***		0.022***		0.065***
		(0.009)		(0.006)		(0.024)
		[0.000]		[0.000]		[0.006]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓
Observations	26,102	23,922	56,152	53,877	18,124	16,691
Mean Dep. Variable (Treated)	59.948	0.155	61.261	0.103	60.192	0.146
Mean Dep. Variable (Control)	59.478	0.115	60.583	0.080	59.900	0.112
F-stat FS		92.849		9.874		31.732

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the age at last employment (Columns 1, 3, 5 and 7) and the probability of dying between the ages 60 and 69 (conditional on surviving until age 59) (Panel A of Columns 2, 4, 6 and 8), obtained from the estimation of regressions **??** and **??** using different corrections for the years that individuals started contributing reported in the affiliation data. Panel B of Columns 2, 4, 6 and 8 reports the IV estimates of 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), obtained from the estimation of regression **??** using different corrections for the years that individuals started contributing reported in the affiliation data.The estimation sample includes individuals that started contributing 12 months before and after 1 January 1967. Columns 1 and 2 correct the reported date of the first contribution by subtracting the total number of years of contribution from the date they claimed a pension for those who reported having started contributing in 1966 and 1967. If the corrected year of starting contributions is before the reported date of the first contribution, we make this correction. Columns 3 and 4 do not make any correction. Columns 5 and 6 make the same correction of Columns 1 and 2 but for all years. Finally, Columns 7 and 8 makes the same correction of Columns 1 and 2 but dropping the last month of 1966 and the first month of 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A25: Robustness: Augmented Sample 1965-1968

	Type of Pension				Age at				Mortality
	Regular Pension (1)	Partial Pension (2)	Disability Pension (3)	No Pension (4)	Last Employment (5)	Regular Pension (6)	Partial Pension (7)	Disability Pension (8)	between Age 60-69 (9)
Contributed in 1967	-0.385***	0.106***	0.187***	0.091***	1.436***	2.530***	0.383	0.316	0.024***
	(0.026)	(0.016)	(0.013)	(0.006)	(0.218)	(0.249)	(0.252)	(0.141)	(0.003)
	[0.000]	[0.001]	[0.000]	[0.000]	[0.002]	[0.000]	[0.155]	[0.205]	[0.000]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Contributed 1965-1968	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	48,477	48,477	48,477	48,477	48,477	27,725	12,456	2,789	45,306
R^2	0.155	0.099	0.099	0.037	0.104	0.340	0.037	0.220	0.027
Mean Dep. Variable (Treated)	0.522	0.077	0.276	0.126	60.838	63.999	57.688	61.091	0.123
Mean Dep. Variable (Control)	0.621	0.039	0.238	0.102	59.919	62.634	57.455	61.062	0.107

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the probability of leaving the labor market through regular pension (Column 1), partial pension (Column 2), disability pension (Column 3), not claiming any pension (Column 4), age at last employment (Column 5), age at claiming regular pension (Column 6), age at claiming disability pension (Column 7), age at claiming partial pension (Column 8), and the probability of dying between the ages of 60 and 69 (conditional on surviving until age 59) (Column 9), obtained from the estimation of regressions **??** and **??** augmenting the sample to individuals that started contributing from 1965 to 1967. The estimation sample includes individuals that started contributing 24 months before and after 1 January 1967. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

Table A26: Robustness: Hole 1965 vs. 1968

	Type of Pension				Age at				Mortality
	Regular Pension	Partial Pension	Disability Pension	No Pension	Last Employment	Regular Pension	Partial Pension	Disability Pension	between Age 60-69
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Contributed in 1967	-0.371***	0.140***	0.155***	0.077***	1.578***	2.336***	-1.081**	0.517**	0.016***
	(0.034)	(0.022)	(0.013)	(0.004)	(0.242)	(0.287)	(0.413)	(0.101)	(0.004)
	[0.000]	[0.001]	[0.000]	[0.000]	[0.002]	[0.000]	[0.042]	[0.020]	[0.009]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Contributed 1965 or 1968	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	22,375	22,375	22,375	22,375	22,375	14,707	4,205	1,739	21,384
R^2	0.108	0.119	0.071	0.023	0.092	0.356	0.043	0.236	0.015
Mean Dep. Variable (Treated)	0.608	0.100	0.200	0.091	61.568	63.900	57.959	61.097	0.097
Mean Dep. Variable (Control)	0.727	0.046	0.170	0.057	60.650	62.468	58.058	60.982	0.093

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the probability of leaving the labor market through regular pension (Column 1), partial pension (Column 2), disability pension (Column 3), not claiming any pension (Column 4), age at last employment (Column 5), age at claiming regular pension (Column 6), age at claiming disability pension (Column 7), age at claiming partial pension (Column 8), and the probability of dying between the ages of 60 and 69 (conditional on surviving until age 59) (Column 9), obtained from the estimation of regressions **??** and **??** comparing individuals that started contributing in 1965 with those that started contributing in 1968. All specifications control for gender, year of birth, and month of birth fixed effects. Each regression also includes the following controls measured when the individuals were between 30 and 40 years old: average monthly contribution, fraction of time employed, fraction of time active, fraction of time in self-employment, and highest occupation and industry sector fixed effects. All standard errors are clustered at the year of birth, and wild-bootstrap p-values are reported in brackets.

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

B More 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% (one of the highest in the European Union). 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]

The pension of all the individuals considered in our sample is regulated by the same pension law and face a statutory retirement age of 65 years, with a minimum contribution period of eight years (further modified to 15 years after the 1997 reform). However, individuals from the selected cohort (1938 - 1949) who contributed before 1 January 1967,[4] even by one day, maintained an indefinite right to early retirement from the old-age pension system. These individuals could freely retire early from age 60, though with some financial penalties.[5] Around 13% of the individuals who started contributing in 1966 claimed a regular pension at the age of 60.

[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 **?**, **?** and **?** 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] 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.

[4] The January 1967 deadline was set at a later date for workers in specific sectors, such as construction, mining, fishing, and the railway. We control for these specific deadlines for workers in these sectors.

[5] The penalty for early retirement is 8 percent per year of early claim. After the 2001 reform, the yearly penalty for early retirement was reduced (up to 6 percent per year) as a function of the years contributed.

On the other hand, individuals from the selected cohorts (1938 - 1949) who contributed after 1 January 1967 faced a statutory retirement age of 65. They can now only retire early under the involuntary early retirement scheme, set in the 2001 law, which allows individuals to retire as early as age 61 (again with some financial penalties, between 6 and 8 percent, depending on the years of contribution, per year of advancement) under certain conditions. These individuals need to have been unemployed (involuntarily) for at least six months and have contributed to the Social Security system for at least 30 years. Due to these stringent requirements, a very small proportion of workers have taken up this involuntary early retirement option.

Because the law was published on 30 December 1966, there is little room left to manipulate the date of the first social security contribution. This feature, therefore, allows us to compare individuals who started contributing before and after 1 January 1967. As we can see in Figure **??**, individuals who contributed before 1967 (independently of their birth year) could voluntarily retire early at the age of 60. For those who contributed after 1967, the only other way to receive early retirement was to claim involuntary early retirement at the age of 61; otherwise, the earliest an individual can voluntarily claim a pension is at the age of 65. Therefore, we expect individuals who started contributing after 1967 to increase their retirement age considerably.

In addition to the regular retirement pathway, there are two alternative pathways: permanent disability and partial retirement pensions. Permanent disability benefits have been used extensively in Spain as an early retirement mechanism (**??**). This option has thus prompted several reforms since 1985 that have tightened the eligibility criteria in order to maintain a steady level of applications into the disability system henceforth. Nevertheless, disability insurance is an important way by which to exit the labor market. Additionally, from 2002, partial retirement options became available, allowing the combination of income from work with old-age pension benefits. The partial retirement option enables individuals aged 60 years and older, with at least 33 years of contribution and six years of tenure in the same company, to claim up to 85% pension while working 15% of the time (up to 75% of benefits after the 2011 reform). The partial retirement option requires the firm's agreement because the worker must be replaced with a new employee. In this paper, we investigate the impact of the reform on the age of the individual when claiming disability, partial and regular retirement pensions, and the probabilities of choosing these alternative exit routes from the labor market.

C More Details on Data and Sample

C.1 A novel data source

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 1938 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 contains all the employment histories of the individuals that had some contact with the Social Security administration since then. Therefore, it is not possible to observe individuals that 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.

C.2 Sample construction

Our main sample covers Spanish individuals born between 1938 and 1949 who started contributing to the Social Security system 12 months before and after 1 January 1967. We drop individuals who are unlikely to be affected by pension reform, i.e., people who have weak labor market attachment and who do not fulfill the pension access requirement. Specifically, we drop people who are not active in the labor market at age 50, people who have claimed a disability pension before age 50, and people who have less than 8 years of contribution (the minimum requirement to gain access to a pension). We further drop people who have claimed a SOVI pension (Seguro Obligatorio de Vejez e Invalidez or Compulsory Old Age and Disability Insurance). A SOVI pension is a residual pension from the old system for individuals that, at the age of 65, are not entitled to a pension from the

current contribution-based Spanish pension but can prove that they contributed at least 1,800 days to the previous system. A SOVI pension is a means-tested pension available to all Spanish citizens aged over 65, or 60 in the case of disabilities, earning below a threshold (€5,164.60 per year in 2018). We drop SOVI claimants because, regardless of their year of starting contributions, they are not eligible for the contribution-based old-age pension. In total, we drop 20% of observations due to these restrictions. The final sample contains 25,764 individuals, of whom 27% are female.

In Table **??**, we perform robustness checks by modifying the definition of individuals who are unlikely to be affected by pension reform. We present three alternative selections: removing the "claimed SOVI" criterion, removing the "less than 8 years of contribution" criterion, and removing the "claimed disability before age 50" criterion. The reduced-form impacts of contributing before 1967 on age at last employment and mortality rate between ages 60 and 69 are similar to the estimates in the baseline sample. Moreover, the IV estimates of the impact of age at last employment on mortality are robust to the sample selection.

In Table **??**, we verify that our sample is not selected. First, we check if the reform has impacted the probability of not being in the main sample, and we find no significant differences. Moreover, we also show no significant mortality differences among individuals not included in the main sample.

C.3 Correction of the variable "year started contribution"

The variable "year started contribution" is poorly recorded for some individuals, especially those who started contributing around 1967, as the administrative dataset started to be constructed at the end of the 60s. The top graph in Figure **??** shows the distribution of years the individuals in our sample started contributing, as recorded in the original dataset. We can observe that there is bunching in the years 1966 and 1967.

Moreover, we observe some "administrative bunching" as a result of administrative practices. Figure **??** shows that the monthly distribution in the starting year is normal in the years before 1965 and after 1967, indicating that the bunching problem is limited to the years 1965, 1966, and 1967. We see people are more likely to report to start contributing in January 1965, January 1967, and December 1966. The distribution is smooth in other years. This is likely caused by administrative bunching. At the time of retirement, individuals need to prove that their first contribution was before 1967 to the pension office in order to gain eligibility for the "old regime". If they manage to show a payslip made before 1967, the pension office is likely to simply record "December 1966" as their first date of the contribution, even though they might have shown that they started working many years before 1967. If they fail to convince the pension office they started working before 1967, the pension office tends to record "January 1967" as their first date of contribution. The

bunching in January 1965 seems to be due to similar reasons. These wrongly assigned starting dates can make our treated and control groups less comparable. This is because those bunch in January 1967 or December 1966 could have, in fact, started working in different years and could have different characteristics. In fact, in Figure **??** a), we can observe that a sizeable mass (around 20 percent) of individuals who originally were recorded as having started contributing in 1967 retiring at age 60, which is legally not possible. This limitation is the reason we cannot use a Regression Discontinuity Design.

To deal with the "administrative bunching", we correct the reported date of the first contribution by using the number of years of contribution and the date individuals claim a regular pension, which are accurately recorded. We subtract the total number of years contributed from the year they claim a regular pension. If the corrected year of starting contributions is before the reported date of the first contribution, we make this correction. This correction is only possible for individuals who have claimed a regular pension, as only for them the total number of years contributed is reported. We perform this correction for the years 1965, 1966, and 1967. However, in our main sample, which only includes individuals that started contributing in 1966 or 1967, the correction of 1965 does not matter. After this correction, we see in Figure **??** that the bunching has been greatly reduced. Figure **??** b) also shows that, after the correction, fewer individuals started contributing in 1967 and retired early at 60. The mass is much subdued and reduced to 8 percent. This comparison between Figure **??** a) and b) shows that our correction does a decent job reassigning year start contribution.

We also perform an alternative method of correction. In addition to correcting for individuals with reported year start contributing in 1966 and 1967, we also drop the individuals who are bunching due to administrative practices based on the density figures. Those are the ones who reported starting contributing in December 1966 and January 1967. Table **??** reports the robustness check for our main results to correction methods. We show the estimated reform impacts on age at last employment and mortality rate between ages 60 and 69 in the first row. The second row shows the IV estimates of the impact of age at last employment on the mortality rate between 60 and 69. All estimates are robust to this alternative correction method. Moreover, Table **??** shows that estimates when using a sample without any correction are also not too different.

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