Discussion Paper Series – CRC TR 224

Discussion Paper No. 410
Project A 02, C 01

The Effect of Removing Early Retirement on Mortality

Cristina Bellés-Obrero [1]
Sergi Jiménez-Martín [2]
Han Ye [3]

July 2024
(*First version: April 2023)*

[1]Universitat de Barcelona, IEB, IZA, Email: cristina.belles@ub.edu
[2]Universitat Pompeu Fabra, Barcelona School of Economics, Email: sergi.jimenez@upf.edu
[3]University of Mannheim, IZA, ZEW, Email: han.ye@uni-mannheim.de

Support by the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) through CRC TR 224 is gratefully acknowledged.

Collaborative Research Center Transregio 224 - www.crctr224.de
Rheinische Friedrich-Wilhelms-Universität Bonn - Universität Mannheim

The Effect of Removing Early Retirement on Mortality*

Cristina Bellés-Obrero†
Universitat de Barcelona, IEB and IZA

Sergi Jiménez-Martín‡
Universitat Pompeu Fabra and BSE

Han Ye §
University of Mannheim, IZA and ZEW

May 2024

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 self-value jobs. Moreover, we show that allowing flexible retirement mitigates the adverse effects of delaying retirement.

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

*We thank Christian Dustmann, Libertad González, Andrea Ichino, Eckhard Janaba, Julian Vedeler Johnsen, Jonathan Kolstad, Rafael Lalive, Adam Leive, Fabrizio Mazzonna, Todd Morris, Daniel Rees, Uta Schönberg for their helpful insights and suggestions. We are thankful to seminar and conference participants at Pompeu Fabra University, University of Mannheim, Boston University, University of Barcelona, University of Bergen, University of Bonn, University of Oxford, University of Verona, IRLE at UC Berkeley, the CRC annual meeting, ESPE2022, Max Planck/NBER workshop 2022 and IAAE 2023 for comments. We gratefully acknowledge funding from the German Research Foundation (DFG) through CRC TR 224 (Project C01) and the Spanish Ministry of Science, Innovation, and Universities (MICINN) through PID2020-114231RB-I00. We thank the Social Security Administration for giving us access to the data through the Barcelona Research Room. Sergi Jiménez also acknowledges financial support from the Spanish National Research Agency (AEI) through the Severo Ochoa Programme for Centres of Excellence in R&D (Barcelona School of Economics CEX2019-000915-S). Cristina Bellés also acknowledges financial support from AGAUR (Beatriu de Pinós). The views expressed here are solely those of the authors. All errors are our own.
†Universitat de Barcelona, Department of Economics, Spain *Email:* cristina.belles@ub.edu
‡Universitat Pompeu Fabra, Department of Economics, Spain. *Email:* sergi.jimenez@upf.edu
§University of Mannheim, Department of Economics, Germany. *Email:* han.ye@uni-mannheim.de

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. The existing empirical evidence mostly draws lessons from policy experiments that allow for earlier retirement (Coe and Lindeboom, 2008; Hernaes et al., 2013; Hallberg et al., 2015; Bloemen et al., 2017; Kuhn et al., 2020). 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. In addition, the trend of unifying pension rules across occupations and sectors has led many OECD countries to phase out or tighten access to special early retirement schemes for workers in hazardous or arduous jobs (OECD, 2023). Therefore, understanding the mortality consequences of removing early retirement access has important policy implications. This paper aims to fill this gap by providing causal evidence on mortality, offering insights to facilitate policy debates on the existence of special pension provisions for hazardous or arduous work.

This paper provides novel empirical evidence on this important issue by investigating a Spanish pension reform in 1967. This 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 for answering the research question. First, the early retirement age changes discontinuously based on the year when the individuals started contributing to the Social Security system, which enables us to credibly identify causal effects. Second, compare to most of the previous literature, this reform leads to a substantial increase (approximately four years) in the early retirement age and results in a considerable delay in retirement. Third, unlike previous studies that often focus on specific groups such as military personnel or civil servants (see, e.g., Hallberg et al. (2015); Bloemen et al. (2017); Hagen (2018)), our reform impacts a broader, more general population. This feature allows us to capture the mortality responses in the

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

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

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 were 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. 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%). To better compare the magnitudes, we perform an instrumental variable (IV) estimation, using the year individuals started contributing as the instrument for the age at last employment. 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%). This result underscores that the adverse impact of postponing retirement on mortality primarily stems from the short-term consequence of losing access to early retirement schemes.

Our results are robust using several robustness tests, including regression-based tests of the differences in covariates, using within-age start contributing fixed effects, varying sample restrictions and set of controls. Moreover, we verify that there are no impacts on retirement decisions and mortality outcomes when using placebo cutoff dates at first contribution.

Furthermore, we shed light on the possible mechanisms behind the detrimental effect of delaying retirement on mortality. We show a large heterogeneity in the effect of delayed retirement on mortality, depending on the characteristics of jobs that the individuals held before retirement.

As the parameters of most jobs are multi-dimensional, we show that other job dimensions also matter, beyond distinguishing between blue- and white-collar jobs. Specifically, we examine four dimensions of a job: 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 and hazardousness of a job, 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 Ravesteijn et al. (2013) 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 Kroll (2011). 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 demanding (hazardous or arduousness) job, working longer has a greater impact on life expectancy, even though they extend their working lives to a lesser extent.

Our findings suggest that removing access to early retirement could exacerbate the social disparity in mortality and diminish the progressivity of the pension system, as poor working conditions often coincide with low wage (OECD, 2019). On one hand, individuals in more demanding jobs see a smaller increase in pension benefits because they extend their working lives to a lesser extent, potentially due to lower employability. On the other hand, these individuals experience a decline in life expectancy due to the reform. In some cases, they may even die before being able to claim any pension. Consequently, removing early retirement access disproportionately reduces the duration of pension claims and the lifetime total pension income for those in demanding jobs. If policy makers aim to close the widening gap in life expectancy across income groups (Tarkiainen et al., 2012), they should be aware of the distributional consequences of similar pension reforms. Our findings also suggest that advocating for different pension provisions based on the working conditions could be an effective policy measure.

Furthermore, we find that allowing workers to gradually reduce their working time can mitigate the negative mortality effects of delayed retirement. In Spain, workers can opt for partial retirement, which combines working part-time with claiming a partial pension, provided they have contributed to the Social Security system for at least 33 years. Comparing individuals with and without access to partial retirement, we reveal that those lacking this option experience a greater increase in mortality for each additional year of delayed retirement. This finding underscores the importance of offering gradual retirement option to mitigate the detrimental mortality impacts of delayed retirement. It also speaks to the recent policy discussions on flexible retirement and reducing working hours at older ages.

Apart from contributing to studies on the impact of pension reforms on retirement decisions (e.g., Mastrobuoni (2009); García-Gómez et al. (2012); Manoli and Weber (2016); Geyer and Welteke (2021)), 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 (Coe and Lindeboom, 2008; Hernaes et al., 2013; Hallberg et al., 2015; Bloemen et al., 2017; Kuhn et al., 2020), promoting later retirement (Zulkarnain and Rutledge, 2018; Hagen, 2018; Bozio et al., 2021) and switching to retirement at the statutory retirement age (Fitzpatrick and Moore, 2018).[3]

The studies of earlier retirement overall find no significant impacts on mortality or a reduction in mortality. For example, Hernaes et al. (2013) find that accessing a pension two to five years earlier has no effect on the probability of dying by the ages of 67, 70, 74, and 77 for the entire population of Norway. Looking at some particular population groups, Hallberg et al. (2015) and Bloemen et al. (2017) find a decrease in mortality due to earlier retirement. Hallberg et al. (2015) show that five-year early access to a retirement pension reduces the mortality of male army officers in Sweden. Bloemen et al. (2017) find that male civil servants in the Netherlands who are entitled to claim a pension around eight years earlier have a lower mortality rate. The only paper that finds (earlier) access to a pension increases mortality is Kuhn et al. (2020). Using Austrian register data, they estimate the (very) short-term impact of three-year early access to pension on mortality. Kuhn et al. (2020) 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 smaller body of literature and is the first paper to provide a precise estimate of the impact of later retirement on mortality due to the elimination of access to early retirement. To the best of our knowledge, only four papers study the effect of delayed retirement. While Hagen (2018) studies the mortality effect of a two-year increase in the statutory retirement age, they find an imprecisely measured no effect on mortality by the age of 69.[4] The other three papers are Zulkarnain and

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

[4]Hagen (2018) studies the mortality effect of a two-year increase in the statutory retirement age of local government

Rutledge (2018), Bozio et al. (2021) and Saporta-Eksten et al. (2021). While they 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. Zulkarnain and Rutledge (2018) find that delaying retirement reduces the probability of dying within five years for men between age 62 ad 65 by 32 % by exploring a tax reduction for employment at older ages in the Netherlands. Bozio et al. (2021) find that delaying retirement in France has a zero effect on the probability of dying between the ages of 61 and 79 by examining a pension reform changed a variety of parameters of the pension system. Saporta-Eksten et al. (2021) 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 Zulkarnain and Rutledge (2018), Bozio et al. (2021) and Saporta-Eksten et al. (2021), as we expect the response to pension reforms that incentivize retirement via financial incentives to be 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., Grip et al. (2012); Shai (2018); Salvati (2020) and see, Pilipiec et al. (2021) for a recent survey.) through relief from work-related stress and increased sleep and physical activities (Atalay and Barrett, 2014; Eibich, 2015; Barschkett et al., 2022). Studies also find a positive impact of retirement on health outcomes due to the adoption of a healthier lifestyle (Insler, 2014; Celidoni and Rebba, 2017; Gorry et al., 2018). Therefore, it is reasonable to expect that later retirement might increase mortality rates.

This article proceeds as follows: Section 2 describes the Spanish pension system and the 1967 pension reform. Section 3 describes the data, and Section 4 the empirical strategy. Sections 5 and 6 present the reform impact on retirement decisions and mortality, and also the potential mechanisms driving the mortality responses. Section 7 presents placebo tests and robustness checks. Finally, Section 8 discusses and Section 9 concludes.

2 Institutional Setting

The key elements of the existing Spanish pension system were established in 1967, and the relevant rules for our whole sample (independently on the year they started contributing to the system) were set in the 1985 and 1997 reforms. The current old-pension system for the elderly in Spain is a

female workers in Sweden and finds that the reform had no impact on mortality and/or health care utilization. See Section 8.1 for a more detailed comparison with the existing literature.

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.[5] 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 B for more details on the different reforms the Spanish pension system has experienced since 1967.

The above retirement rules apply to all individuals in our sample, except for the earliest age at pension eligibility. Individuals who contributed before and after 1 January 1967 face different early retirement ages. Specifically, individuals who contributed before 1 January 1967, even by one day, had an indefinite right to claim early retirement at the age of 60.[6] These workers have this right because they began contributing before the current old-age pension system was established in 1967.[7] It created the new Social Security system, while maintained the pension rights of the old regime for these workers. These individuals could retire voluntarily and claim early retirement from age 60, though with some financial penalties.[8] 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 can only claim a pension voluntarily at age 65 when they reach the statutory retirement age. Since 2002, they could claim early at age 61 under the involuntary early retirement scheme (again with some financial penalties, between 6 and 8 percent, depending on the years of contribution, per year of advancement), however under some stringent conditions. They need to have been unemployed involuntarily for at least six months and have contributed to the Social Security system for at least 33 years. Due to these stringent requirements, a very small proportion of workers have taken up this involuntary early retirement option.

Although the foundational regulation for the new social security system was published on 21 April 1966, the detailed guidelines for its implementation (including the 1 January 1967 deadline for the initial social security contribution), were issued on 18 January 1967,[9] eliminating any pos-

[5]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. This change affected in the same way all individuals in our sample, independently on the year they started contributing.

[6]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 share of sample), we compare individuals that started contributing 12 months before and after that later date instead of 1967.

[7]The 1967 Social Security Law came into force on 1 January 1967 under the Francoist dictatorship. It unify and integrate various of social insurances. After his death in 1975, the democratic government took over the management of the old age pensions.

[8]The financial penalty for early claiming is 8 percent (or up to 6 percent as a function of the years contributed after the 2002 reform) per year of early claim.

[9]See the exact legal document on the the Spanish Ministry of Labor website.

sibility of manipulating the date of the first social security contribution. Consequently, this feature enables us to causally estimate the reform impact by comparing individuals who began contributing before and after 1 January 1967. As we can see in Figure 1, individuals who contributed before 1967 (independently of their birth year) could voluntarily 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 (only available since 2002); 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 delay labor market exit considerably.

There are two alternative pathways to exit earlier: disability and partial retirement pensions. Disability benefits have been used extensively in Spain as an early retirement scheme (Boldrin et al., 1999; García-Gómez et al., 2012). Additionally, in 2002, the partial retirement option became available, allowing the combination of income from work with old-age pension benefits. The partial pension 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 later sections, we investigate the reform impact on the probabilities of choosing these alternative exit routes to exit 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.[10]

We use a non-public version of the MCVL, which allows us to observe contributive workers and pensioners prior to 2005 (the starting time of the publicly available version). This data advantage allows us to study 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 C.1 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

[10]Reliable data on monthly contribution 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. All other variables are accurately recorded throughout the lifetime of our individuals, except for age at first contribution, which we will explain in detail in Section 3.2.

the Social Security system until their death.[11] 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 when claiming a pension, pension benefits, the type of pension they receive at each point in time, and the total number of contribution years at 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 choose eight years because eight years of contribution is the minimum requirement individuals in our sample needed to gain access to the pension.[12] 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 pre-1967 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 A1, we verify that selection into the sample is not affected by the treatment status. 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 A2, 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 C.2 for more details.

[11] 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, especially for those who switch from the informal sector to the formal sector.

[12] It was eight years before the 1997 reform and extended to 15 years thereafter.

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 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 A1 shows the distribution of years started contributing, as recorded in the original dataset. We observe some bunchings in the years 1966 and 1967. Moreover, Figure A2 shows that the bunchings are concentrated in January 1965, 1966, and December 1966, which are likely due to administrative practices.[13] This limitation is the reason we cannot use a Regression Discontinuity Design. The monthly distribution appears smooth in the other years, indicating that this "administrative" bunching problem is limited to 1965, 1966, and 1967.

Wrongly assigned starting dates can lead to two consequences. First, the treated and control groups become less comparable. Those bunch in January 1967 or December 1966 could have, in fact, started working in different years and could have different characteristics. We partially address this concern by controlling for an extensive list of fixed effects and covariates. Second, these mistakes could affect our treatment assignment. Although the nature of "administrative" bunching lessens our concern about misassigned treatment status, there are still around 25 percent of people who originally recorded as having started contributing in 1967 claim a regular pension at age 60, which is legally impossible (Figure A3 a)).

Fortunately, we can partially correct this measure as we have excellent and accurate information on the number of years contributed and the exact date individuals claim a regular pension. First, we create a new variable "calculated date of first contribution" by subtracting the total number of years contributed from the calendar date they claim a regular pension. Then, if the "calculated date of first contribution" is before the "reported date of first contribution", we make the correction. Note that the correction is only possible for individuals who have claimed a regular pension, because only for them the total number of years contributed at retirement is reported. We perform this correction for the years 1965, 1966, and 1967. 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. Figure A1 shows that the bunching has been greatly reduced after the correction. Figure A3b) also shows that, after the correction, very few individuals started contributing in 1967 and retired early at 60, suggesting is effective at correcting mistakes.

[13]To prove eligibility for the "old regime", individuals can show a pre-1967 payslip to the pension office, which leads the office to typically record "December 1966" as the starting date. If unable to convince the office of an earlier start date, "January 1967" is recorded as the first contribution date. Given the nature of practice, we are not too worried that treatment status are wrongly assigned due to this type of administrative practice. See Appendix C.3 for further discussions of the administrative practices.

Another concern could be that the new pension system in 1967 may have attracted new contributors. However, there is no obvious institutional reason for this and the country is under the same Franco dictatorship. Moreover, we show that the characteristics of people contributed in 1966 and 1967 are similar when they are between 30 and 40 years old (Table A4).

We also perform several robustness checks by varying the correction methods. We show that baseline results are similar when without any correction, without correction and/or only dropping people bunch in December 1966 and Jan 1967. See Section 7.2 and Appendix C.3 for more discussion about the correction of this variable.

4 Empirical Strategy

In this section 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. Next, we analyse the relationship between labor supply (at older ages) choices and mortality under the assumption that any causal effect of the reform on mortality is channeled trough its effect in labor supply and retirement choices (for example, age of last employment).

4.1 Reduced-form Analysis: Within-cohort Fixed-Effect Model

The following equation estimates the reduced-form reform impacts of treatment on a variety of outcomes (age of retirement and last employment,the probability and the age at which individuals claim the different pensions, mortality)

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

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

R_{icm} represents the labor supply and M_{icm} represents the mortality outcomes of individual *i* born in year *c* and month *m* who started contributing in year 1966 or 1967, and $Treated_i$ 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 age 60). δ_c is the year of birth, and μ_m is the month of birth fixed effects. α_3 measures the average treatment effect of the reform on either labor supply and pension claiming decisions. β_3 measures the average treatment effect of the reform on mortality. X_{icm} 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 gender, individuals' mean monthly contribution, 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.

Causality

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 reduced form 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 occupation, industry, birth year, and 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.[14] Table A3 compares a list of predetermined variables for people started contributing in 1966 and 1967, 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. Table A4 shows the reform impact on these variables. Both summary statistics and regression estimates suggest that these covariates do not predict treatment status. 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.[15] 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 battery of robustness test, including using age at first contribution fixed effect instead of birth month fixed effects in Section 5. These tests rule out the possibility that other confounding factors drive our reduced-form estimates.

[14] Ideally, we would like to check whether the characteristics of individuals in the treatment and control groups differ before they start contributing to the pension system or at least at the beginning of their careers. However, we don't have information before they start contributing and the data quality around 1967 was not particularly good. Therefore, 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.

[15] In Section 7, we show that the effect of the reform on our main outcomes is robust to excluding individuals in one of the self-employed pension regimes.

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

To quantify the effect of labor supply and retirement choices on mortality and compare across subgroups, we estimate the impact of age at last employment on mortality using an Instrumental Variable (IV) method. Exploring the reform-induced exogenous variation in age at last employment, we estimate the causal effect on mortality using following equation,

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

We include the same list of controls used in Equations 1 and 2 (δ_c, μ_m, and X_{icm}). $\widehat{R^a_{icm}}$ is the predicted value of age at last employment obtained in Equation 1. 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 5.

Second, the treatment status only affects mortality through its impact on age at last employment. The exclusion restriction could be violated if contributing in 1967 affects mortality through channels other than retirement age. Two possible additional channels are changes in pension benefits and changes in labor market outcomes close to retirement. We argue that the exclusion restriction assumption is reasonable by showing that controlling for a proxy of the individuals' pension benefits or their labor market decisions does not change the magnitude of our IV estimates to any great extent. See Section 6 for more details.

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 at least maintain the same age at last employment. Given the nature of the pension reform, this condition is likely to be satisfied. Moreover, Figure A4 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 A7). Lastly, Tables 5 and 6 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

There are three different types of pensions that individuals claim. Table A3 shows that among people started contributing in 1966, 55% of individuals claim a regular old-age pension, while 27% claim a disability pension, 3% of individuals choose a partial pension and 13% of 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 ($\sim$ 7%), and still being active in the labor market in 2023 ($\sim$ 0.2%). Compared with those who started contributing in 1966, individuals who started contributing in 1967 have a statistically significant 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 who began contributing in 1966 retire at age 59.5 and claim a regular pension at age 62.7. For those who started contributing in 1967, these ages are are approximately 0.5 to 1.5 year later (Table A3). Figure 2 compares the distribution of age at last employment for individuals who started contributing in 1966 and 1967. As expected, we see a distinct difference. Figure 2 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 3a) 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.

Figure 3b) and Figure 3c) compares the distribution of age at claiming a partial pension and age at claiming a disability pension by treatment status.[16] 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.

[16] The age at claiming a partial pension is the age at which a person receives the work reduction and claims part of the pension benefit. Note that age at last employment is the age at which people leave the labour market permanently. For people who have claimed a partial pension, the age at last employment is later than the age at claiming a partial pension.

5.2 Regression Results

Table 1 examines the reform impact on the types of pensions that individuals claimed. We find that individuals who started contributing 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%). We further show that the reform affected the use of disability insurance equally by the severity of the disability. Table A5 shows that the reform increased the probability of claiming a severe or total disability and a partial or occupational disability pension by 3.1 and 2.7 percentage points, respectively.[17] Moreover, treated individuals are 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 who lose access to early retirement are 2.8 percentage points ($\sim$ 21%) more likely to leave the labor market without any pension. In Table A6, 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 A6 indicates that the reform has no impact at all on the probability of continuing to work or becoming inactive. In fact, the change is driven by individuals dying before claiming any pension, which has increased by 1.9 percentage points ($\sim$ 25%). This result implies that premature death before pension claiming is the main driver for not claiming any pension, which is consistent with the mortality effects in later sections.

Column 5 of Table 1 also 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 A7 further unpacks the reform impact on the probability of exiting the labor market in different age brackets starting at age 50. We find the reform has no impact on the probability of exiting the labor market between the ages 50 and 59. Moreover, the delay in the age at exiting is most pronounced at ages 60 and age 65. We find that the the reform reduces the probability of exiting the labor market at the age of 60 by 4.3 percentage points (38%) and increases the probability of exiting the labor market at the age of 65 by 7.1 percentage points (42%).

[17] 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.

When examining age at claiming pensions, Table A8 shows that the age at claiming first pension is increased by 3 months and the age at claiming a regular pension is increased by one year and two months. The ages at claiming a disability pension and a partial pension are also affected. Individuals who contributed in 1967 delay claiming disability by around two and a half months but claim a partial pension earlier by around two months. Because the reform influenced the probability of individuals claiming different types of pensions, we should interpret the impact on age at claiming these pensions with caution. Nevertheless, these analysis could help us to understand the adjustment margins behind the increase in age at exiting employment. For more details, see Appendix Section D.1.

We further 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 1), which both increases the pension base and decreases the penalty for early retirement. On the other hand, as more individuals claim disability insurance due to the reform, we expect the overall pension benefits decreases as disability pension benefits are typically less generous than old age pension. Table 2 indicates that the total pension benefit increases by 15€ ($\sim$ 1.5%) for those who started contributing in 1967. The increase is not driven by a higher base pension but from an increase in adjustable part of the pension benefits (less financial penalty due to delayed claiming) of 2.3 percentage points ($\sim$ 3.3%). It is important to note that the positive effect on pension benefits is driven mainly by those who claimed a regular pension, as Table A9 shows. Individuals who claimed a regular pension and started contributing in 1967 received, on average, 73.3€ more monthly pension benefit. 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.

Lastly, we investigate whether the reform has an impact on the labor market outcomes before retirement. Table A10 shows the effects 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 ages and are 0.6 percent more likely to be active in the labor market. One possible reason that the individuals started contributing in 1967 are more attached to the labor market is that they expect to retire later. 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.

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 4 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 3 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).[18] 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 4 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%). Moreover, Table A11 shows the impact on probability of dying before different ages. We also find that the impacts are driven by responses between age 60 and 65. Taken together,these results indicate that the negative effect of delaying retirement on mortality is driven mainly by the short-term effect

[18] 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.

of losing access to early retirement schemes.

Figures 5 and 6 show the estimates from placebo tests when we assign placebo treatment status to the individuals using other dates at first contribution. We compare individuals who started contributing in the years from 1959 to 1976 (indicated on the y-axis). 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 7 for a more detailed discussion of the placebo tests.

The bottom panel of Table 3 qualifies the impact of delaying age at last employment by one year on mortality using the IV method. 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 almost half a year increase in age at last employment (as estimated in Table 1). Moreover, Table A12 shows that the estimates are similar across specifications with varying controls. Controlling for labor market activities between age 30 and 40 (controls), a proxy of the individuals' pension benefits[19] and labor market decisions before retirement (between 45 and 55 years of age) does not change the magnitude of our IV estimates to any great extent. This also offers evidence that the treatment's effect on mortality is primarily through its influence on age at last employment, suggesting that the exclusion restriction is reasonably met.

To interpret the IV results, it is important to understand who the compliers are. As compliers cannot be identified individually, we follow Abadie (2003) 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 A13, 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 A14, 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

[19] 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.

to be self-employed, as the self-employed are much less likely to be affected by changes in public pensions.

Putting together the pension claiming and mortality responses, we show that removing early retirement access leads to a reduction in the duration of pension receipt on average. Table 2 shows that although the reform led to an increase in annual pension benefits, the duration of pension claiming decreased by 0.76 years (equivalent to 9 months), resulting in a reduction of lifetime pension benefits by 3,228€, approximately 2.4%. This analysis highlights the loss in total pension income for individuals who started contributing in 1967 compared to those who began in 1966.

6.2 Mechanisms

This section aims to shed light on potential mechanisms explaining the increase in mortality caused by losing access to early retirement. 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 also reported. 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 (Mazzonna and Peracchi, 2017).[20] 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. These four dimensions help capture jobs that are dangerous, arduous, or have low recognition. 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

[20] 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 (Kivimäki et al., 2002). In fact, both Bloemen et al. (2017) and Hallberg et al. (2015) report that retirement reduces the risk of heart-related mortality. 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 (Godtfredsen et al., 2008; Lee et al., 1999). Both factors can be affected by working status and, ultimately, retirement (Falba et al., 2005; Black et al., 2015; Evenson et al., 2002; Barnett et al., 2014). Fitzpatrick and Moore (2018) find that mortality due to two lung-related conditions (COPD and lung cancer) statistically increases immediately after retirement at the age of 62.

at work, and -0.33 between psychosocial burden and self-value at work.

Table 5 reports the heterogeneity results for the probability of dying between the ages of 60 and 69 (conditional on surviving to age 60)[21] 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 panel, we report the impact on mortality between the ages of 60 and 69, including both the reduced-form and the IV estimates. P-values testing the hypothesis that the IV coefficients by subgroups are equal are reported in the last row. Importantly, Table A15 shows that the reform has not affected 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, understanding whether the adverse effects of delaying retirement on mortality differ differ by these characteristics has important policy relevance. We do so by classifying individuals' last industry depending on the physical and psychosocial burden.

Previous literature has already established that physically demanding occupations is associated with adverse health effects (see Case et al. (2005) and Ravesteijn et al. (2013) for a summary). To measure physical burden at work, we use 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 A5 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 accident rate. 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 5 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

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

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 A16) 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 Kroll (2011), 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.[22] Figure A6 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 5 report that delaying retirement by one year increases the probability 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 A17) 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

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

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 (Szinovacz et al., 1992). 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 A7 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,[23] and divide the sample by the index's median.

In columns 5 and 6 of Table 5, 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 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 (Coe et al., 2012). 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 5. Contrary to Mazzonna and Peracchi (2017), 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

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

sectors. However, this result should be taken with caution as Table A15 shows that the reform had a significant negative effect on the probability of working in blue-collar jobs before retirement.

In summary, Table 5 shows that individuals with strenuous employment (both 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 of workers with worse working conditions plays an important role in explaining the shortened pension claiming duration on average.

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.[24]

As explained in Section 2, 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 A8 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 6 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

[24]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 years since they started contributing to the Social Security that they have been employed.

(not significant) higher probability compared with the control group.

Table 6 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 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 A18 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 A8.

To further convey the heterogeneous impact is a result of partial pension eligibility, we perform a "placebo" test by using a sample of ineligible individuals. Columns (7) and (8) of Table A18 display the estimates when we use a sample of individuals with 20 to 32 years of contribution. We find a similar impact on age at last employment and mortality regardless of whether individuals contributed more or less than 28 years. This placebo analysis further supports the conclusion that the availability of gradual retirement can help mitigate the adverse effects of delayed retirement on the life expectancy.

7 Placebo Tests and Robustness

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).[25] Figures 5 and 6 plot the estimated coefficients of the different placebo tests for our baseline sample. 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 birth 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 7 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.[26] 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.

7.2 Robustness to Correction Methods

As discussed in Section 3.2, 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 A1 and Figure A3b) 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

[25] 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 C.3, 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.

[26] 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.

claimed a pension; thus, it cannot be adjusted for those who died before claiming a pension.

We provide 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 4), suggesting the correction does by construction select people with different mortality trend into our treatment. Second, we show that without correction, the mortality effects remain significant but are attenuated. As suggested by Figure A1, without the correction, some treated individuals might be incorrectly classified in the control group, potentially biasing our estimates downward. Third, Table 7 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) or only correct those months with the highest bunching. Moreover, when we use both years of inactivates and contribution years for the correction (columns 9 and 10 of Table 7), the estimates are similar, suggesting that the estimates are not biased by correcting only people with uninterpreted careers. Finally, we show robustness of our findings by varying sample composition, i.e. by adding those who began contributing in 1965 and 1968 to our sample (Tables A19 and A20). Our estimates are robust to these exercises.

7.3 Additional Robustness Tests

Our estimates are robust to varying model specifications, such using age at first contribution fixed effect and varying controls, and they are also robust to varying sample restrictions, including keeping only younger cohorts and dropping the self-employed.

Within-Age at First Contribution Fixed Effects Model These individuals started contributing in 1967 and born in the same year were one year older when they started contributing. One potential confounding factor could be educational attainment. Unfortunately, we do not have information on the education. Therefore, to test whether the reform effect captures the differences in educational attainment, we show robustness by using age at first contribution fixed effect instead of birth year fixed effects in panel A of Table A19 and column (2) of Table A20. Here, we estimate the impact of losing access to early retirement for individuals who start working at the same age but were born one year apart. Compared with the baseline results, these estimates using starting age fixed effects are similar.

No Controls Because we use labor market activities between the ages of 30 and 40 to proxy for characteristics at the beginning of their contribution history, we might worry that these controls will bias our estimates. Panel B of Table A19 and column (3) of Table A20 show that the magnitudes of our estimates when removing any of these controls are similar to the baseline estimates.

These robustness checks suggest that these covariates betwen age 30 and 40 are not likely to be endogenous.

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 involuntarily at the age of 61. Panel C of Table A19 and column 4 of Table A20 show the estimates when we only look at cohorts born between 1941 and 1949, essentially dropping the cohorts who were partially affected by the law of 2002. Although younger cohorts have the option to retire at 61, the estimates are similar, suggesting that the reform effects are not only driven by the older cohorts, who have to wait longer to claim.

Dropping Self-Employed Individuals Table A4 shows the reform has a small but significant impact on the fraction of time spent in self-employment between ages 30 and 40. It could be that the self-employed might have more flexibility in deciding when 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. Panel D of Table A19 and column 5 of Table A20 show that the estimates when dropping individuals who received a pension under the self-employed regime are similar to the baseline estimates.

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 ($\sim$ 38%). This may seem quite a large effect; however, our estimates are comparable in magnitude with studies showing that early retirement reduces mortality (Hallberg et al., 2015; Bloemen et al., 2017).

Hallberg et al. (2015) 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, Hallberg et al. (2015) 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. Bloemen et al. (2017) 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).[27] 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 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. Bozio et al. (2021) and Saporta-Eksten et al. (2021) are the only two papers we know of that have the statistical power to conclusively estimate the mortality impacts, and they find precisely zero effects of delaying retirement on morality before age 75.[28] 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. Bozio et al. (2021) 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. Saporta-Eksten et al. (2021) 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.[29] 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 6). 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., Jensen and Richter (2004); Snyder and Evans (2006); Malavasi and Ye (2023)). In particular, Snyder and Evans (2006) examine a variation in social security wealth for the U.S. "notch" cohort

[27] Although Hallberg et al. (2015) study male military officers in Sweden and Bloemen et al. (2017) 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.

[28] Hagen (2018) 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).

[29] It is important to note that Saporta-Eksten et al. (2021) 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.

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 early retirement access, leads to higher mortality, despite inducing higher pension benefits (as shown in Table 2). Our IV analysis, which controls for pension income, suggests that the adverse mortality impact is mainly due to delayed employment. While both Snyder and Evans (2006) and our paper show that the employment impact dominates, Snyder and Evans (2006) 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 (Sullivan and Von Wachter, 2009). Finally, Snyder and Evans (2006) points out that the "notch" cohort is working longer, mostly through an increase in part-time employment while still receiving pension benefits. As a result, their results are more comparable to our findings for people who are eligible for the partial retirement scheme. These institutional details may explain why we 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, Black et al. (2018) show that access to DI reduces mortality for sicker, inframarginal beneficiaries aged 55 to 64 due to DI income and Medicare eligibility.[30] Similarly, Gelber et al. (2023) 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, Black et al. (2018) and Gelber et al. (2023) 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 different from Black et al. (2018) 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.

[30] Black et al. (2018) 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.

8.2 Policy Discussion

We performed a simple back-of-the-envelope calculation to conduct cost-benefit analysis of the 1967 Spanish pension reform which removed the early retirement option. We show that the adverse impact on life expectancy outweighs the fiscal gains. On the one hand, using the value of a quality-adjusted life year at age 60 in Spain implied by Vallejo-Torres et al. (2018), we show that a 0.46-year increase in the age at death translates to a cost of 8,564 € per affected individual. On the other hand, the reform results in a delayed labor market exit of 0.44 years, which generates additional contribution to the pension system and tax revenue, amounting to 1,925 €. [31] Moreover, because the reform leads to early mortality, it decreased the average duration of pension payments by 0.76 year, thus saving the government 3,228 € per retiree in pension benefits (Table 2). As a result, the government realizes a fiscal gain of 5,213 € due to the reform, which is offset by the monetary costs of reduced life expectancy of 8,564€. In conclusion, the fiscal savings from delayed retirement and reduced duration of pension payments do not compensate for the higher costs of reduced life expectancy, suggesting that the reform is not economically beneficial in the broader context of societal well-being.

Furthermore, 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 (OECD, 2016).[32] 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 (Sanchez-Romero et al., 2020). 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. One possible policy recommendation would be to consider policy tools that link retirement age to heterogeneous life

[31] An average wage earner earns 9,310 € at age 60. The contribution rate to the pension system from employer and employee is around 28%. A 0.44-year of extra working life translates to an increase of contribution of 1,147 € per individual. this additional 0.44-year of work also increases tax revenues by 778€, calculated at an income tax rate of 19%.

[32] 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 A14, we examine whether the reform had differential effects by gender. We show that delaying retirement by one year increases mortality in absolute terms more for men than for women. 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, behavioral and environmental factors. One behavioral factor that explains part of the mortality gender gap is that women and men select into occupations with different degrees of health burdens, which may partly account for the differential effect of delaying retirement on mortality by gender (DeLeire and Levy, 2001).

expectancy. It might be worthwhile to consider well-targeted early retirement options for people with specific working conditions, 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. Comparing individuals who started paying contributions 12 months before and after January 1, 1967, we find that individuals who started paying contributions in 1967 delay their exit from the labor market by almost half a year. They are also more likely to claim disability and partial pensions in order to leave the labor market before the regular pension becomes available. Most strikingly, we find that they are also more likely to claim no pension, driven mainly by death before claiming a pension.

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. This suggests that the increase in mortality is driven mainly by the immediate effect of losing access to early retirement schemes. Further analysis shows that individuals who worked in hazardous, arduous and low self-value jobs before retirement suffer more in terms of mortality, although they delay retirement to a lesser extent. Moreover, we show that allowing workers to gradually reduce their workload can incentivize workers to stay longer in the labor force without negatively affecting their life expectancy.

The applicability and relevance of our findings extend further than the Spanish context. Delaying statutory retirement and closing early retirement options are pertinent policy agendas in many countries. However, 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.

References

Abadie, Alberto, "Semiparametric instrumental variable estimation of treatment response models," *Journal of econometrics*, 2003, *113* (2), 231–263.

Atalay, Kadir and Garry F Barrett, "The causal effect of retirement on health: New evidence from Australian pension reform," *Economics Letters*, 2014, *125* (3), 392–395.

— and — , "The impact of age pension eligibility age on retirement and program dependence: Evidence from an Australian experiment," *Review of Economics and Statistics*, 2015, *97* (1), 71–87.

Barnett, Inka, Esther van Sluijs, David Ogilvie, and Nicholas J Wareham, "Changes in household, transport and recreational physical activity and television viewing time across the transition to retirement: longitudinal evidence from the EPIC-Norfolk cohort," *J Epidemiol Community Health*, 2014, *68* (8), 747–753.

Barschkett, Mara, Johannes Geyer, Peter Haan, and Anna Hammerschmid, "The effects of an increase in the retirement age on health—Evidence from administrative data," *The Journal of the Economics of Ageing*, 2022, *23*, 100403.

Black, Bernard, Eric French, Jeremy McCauley, and Jae Song, "The effect of disability insurance receipt on mortality," in "Memorandum 19/2012," University College London, 2018.

Black, Sandra E, Paul J Devereux, and Kjell G Salvanes, "Losing heart? The effect of job displacement on health," *ILR Review*, 2015, *68* (4), 833–861.

Bloemen, Hans, Stefan Hochguertel, and Jochem Zweerink, "The causal effect of retirement on mortality: Evidence from targeted incentives to retire early," *Health economics*, 2017, *26* (12), e204–e218.

Blundell, Richard, Eric French, and Gemma Tetlow, "Retirement incentives and labor supply," in "Handbook of the economics of population aging," Vol. 1, Elsevier, 2016, pp. 457–566.

Boldrin, Michele, Sergi Jiménez-Martín, and Franco Peracchi, "Micro-modeling of retirement behavior in Spain," *NBER Chapters*, 2004, pp. 499–578.

— , — , — et al., "Social security and retirement in Spain," *Social Security and Retirement around the world*, 1999, pp. 305–53.

Bozio, Antoine, Clémentine Garrouste, and Elsa Perdrix, "Impact of later retirement on mortality: Evidence from France," *Health economics*, 2021, *30* (5), 1178–1199.

Case, Anne, Angus Deaton et al., "Broken down by work and sex: How our health declines," *Analyses in the Economics of Aging*, 2005, *1*, 185–214.

Celidoni, Martina and Vincenzo Rebba, "Healthier lifestyles after retirement in Europe? Evidence from SHARE," *The European Journal of Health Economics*, 2017, *18* (7), 805–830.

Coe, Norma and Maarten Lindeboom, "Does retirement kill you? Evidence from early retirement windows," 2008.

Coe, Norma B, Hans-Martin von Gaudecker, Maarten Lindeboom, and Jürgen Maurer, "The effect of retirement on cognitive functioning," *Health economics*, 2012, *21* (8), 913–927.

Coile, Courtney and Jonathan Gruber, "Future social security entitlements and the retirement decision," *The review of Economics and Statistics*, 2007, *89* (2), 234–246.

de Grip, Andries, Maarten Lindeboom, and Raymond Montizaan, "Shattered dreams: the effects of changing the pension system late in the game," *The Economic Journal*, 2012, *122* (559), 1–25.

DeLeire, Thomas and Helen G Levy, "Gender, occupation choice and the risk of death at work," 2001.

Eibich, Peter, "Understanding the effect of retirement on health: Mechanisms and heterogeneity," *Journal of health economics*, 2015, *43*, 1–12.

Evenson, Kelly R, Wayne D Rosamond, Jianwen Cai, Ana V Diez-Roux, and Frederick L Brancati for the Atherosclerosis Risk in Communities Study Investigators, "Influence of retirement on leisure-time physical activity: the atherosclerosis risk in communities study," *American journal of epidemiology*, 2002, *155* (8), 692–699.

Falba, Tracy, Hsun-Mei Teng, Jody L Sindelar, and William T Gallo, "The effect of involuntary job loss on smoking intensity and relapse," *Addiction*, 2005, *100* (9), 1330–1339.

Fitzpatrick, Maria D and Timothy J Moore, "The mortality effects of retirement: Evidence from Social Security eligibility at age 62," *Journal of Public Economics*, 2018, *157*, 121–137.

García-Gómez, Pilar, Sergi Jiménez-Martín, and J Vall Castelló, "Health, disability, and pathways into retirement in Spain," *Social security programs and retirement around the world*, 2012, pp. 127–174.

García-Pérez, J Ignacio, Sergi Jiménez-Martín, and Alfonso R Sánchez-Martín, "Retirement incentives, individual heterogeneity and labor transitions of employed and unemployed workers," *Labour Economics*, 2013, *20*, 106–120.

Gelber, Alexander, Timothy Moore, Zhuan Pei, and Alexander Strand, "Disability insurance income saves lives," *Journal of Political Economy*, 2023, *131* (11), 000–000.

Geyer, Johannes and Clara Welteke, "Closing Routes to Retirement for Women How Do They Respond?," *Journal of Human Resources*, 2021, *56* (1), 311–341.

Godtfredsen, Nina S, Tai H Lam, Trevor T Hansel, ME Leon, N Gray, C Dresler, DM Burns, E Prescott, and J Vestbo, "COPD-related morbidity and mortality after smoking cessation: status of the evidence," *European Respiratory Journal*, 2008, *32* (4), 844–853.

Gorry, Aspen, Devon Gorry, and Sita Nataraj Slavov, "Does retirement improve health and life satisfaction?," *Health economics*, 2018, *27* (12), 2067–2086.

Hagen, Johannes, "The effects of increasing the normal retirement age on health care utilization and mortality," *Journal of Population Economics*, 2018, *31* (1), 193–234.

Hallberg, Daniel, Per Johansson, and Malin Josephson, "Is an early retirement offer good for your health? Quasi-experimental evidence from the army," *Journal of health economics*, 2015, *44*, 274–285.

Hernaes, Erik, Simen Markussen, John Piggott, and Ola L Vestad, "Does retirement age impact mortality?," *Journal of health economics*, 2013, *32* (3), 586–598.

Insler, Michael, "The health consequences of retirement," *Journal of Human Resources*, 2014, *49* (1), 195–233.

Jensen, Robert T and Kaspar Richter, "The health implications of social security failure: evidence from the Russian pension crisis," *Journal of Public Economics*, 2004, *88* (1-2), 209–236.

Kivimäki, Mika, Päivi Leino-Arjas, Ritva Luukkonen, Hilkka Riihimäi, Jussi Vahtera, and Juhani Kirjonen, "Work stress and risk of cardiovascular mortality: prospective cohort study of industrial employees," *Bmj*, 2002, *325* (7369), 857.

Kroll, Lars Eric, "Construction and validation of a general index for job demands in occupations based on ISCO-88 and KldB-92," *methods, data, analyses*, 2011, *5* (1), 28.

Kuhn, Andreas, Stefan Staubli, Jean-Philippe Wuellrich, and Josef Zweimüller, "Fatal attraction? Extended unemployment benefits, labor force exits, and mortality," *Journal of Public Economics*, 2020, *191*, 104087.

Lee, I-Min, Howard D Sesso, and RS Paffenbarger Jr, "Physical activity and risk of lung cancer.," *International Journal of Epidemiology*, 1999, *28* (4), 620–625.

Malavasi, Chiara and Han Ye, "Pension Income and Mortality: Evidence from Germany," *Working paper*, 2023.

Manoli, Day and Andrea Weber, "Nonparametric Evidence on the Effects of Financial Incentives on Retirement Decisions," *American Economic Journal: Economic Policy*, November 2016, *8* (4), 160–82.

Mastrobuoni, Giovanni, "Labor supply effects of the recent social security benefit cuts: Empirical estimates using cohort discontinuities," *Journal of Public Economics*, dec 2009, *93* (11-12), 1224–1233.

Mazzonna, Fabrizio and Franco Peracchi, "Unhealthy retirement?," *Journal of Human Resources*, 2017, *52* (1), 128–151.

Nielsen, Nick Fabrin, "Sick of retirement?," *Journal of health economics*, 2019, *65*, 133–152.

OECD, "Fragmentation of retirement markets due to differences in life expectancy," 2016.

—, *OECD Employment Outlook 2019* 2019.

—, *Pensions at a Glance 2023* 2023.

Pilipiec, Patrick, Wim Groot, and Milena Pavlova, "The effect of an increase of the retirement age on the health, well-being, and labor force participation of older workers: a systematic literature review," *Journal of Population Ageing*, 2021, *14*, 271–315.

Ravesteijn, Bastian, Hans van Kippersluis, and Eddy van Doorslaer, "The contribution of occupation to health inequality," in "Health and inequality," Emerald Group Publishing Limited, 2013.

Rose, Liam, "Retirement and health: Evidence from England," *Journal of Health Economics*, 2020, *73*, 102352.

Salvati, Francesca, "Health Inequality, Labor Supply and Retirement Policies," Technical Report, Mimeo 2020.

Sanchez-Romero, Miguel, Ronald D Lee, and Alexia Prskawetz, "Redistributive effects of different pension systems when longevity varies by socioeconomic status," *The Journal of the Economics of Ageing*, 2020, *17*, 100259.

Saporta-Eksten, Itay, Ity Shurtz, and Sarit Weisburd, "Social Security, Labor Supply, and Health of Older Workers: Quasi-Experimental Evidence from a Large Reform," *Journal of the European Economic Association*, 2021, *19* (4), 2168–2208.

Shai, Ori, "Is retirement good for men's health? Evidence using a change in the retirement age in Israel," *Journal of health economics*, 2018, *57*, 15–30.

Snyder, Stephen E and William N Evans, "The effect of income on mortality: evidence from the social security notch," *The Review of Economics and Statistics*, 2006, *88* (3), 482–495.

Sullivan, Daniel and Till Von Wachter, "Job displacement and mortality: An analysis using administrative data," *The Quarterly Journal of Economics*, 2009, *124* (3), 1265–1306.

Szinovacz, Maximiliane, Maximiliane Szinovacz, David J Ekerdt, and Barbara H Vinick, *Families and retirement*, Sage, 1992.

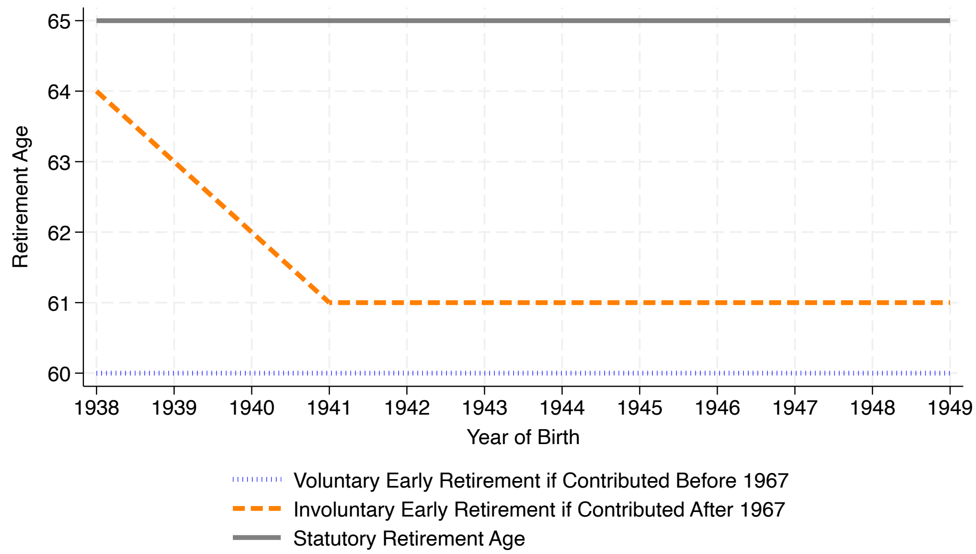
Tarkiainen, Lasse, Pekka Martikainen, Mikko Laaksonen, and Tapani Valkonen, "Trends in life expectancy by income from 1988 to 2007: decomposition by age and cause of death," *J Epidemiol Community Health*, 2012, *66* (7), 573–578.

Vallejo-Torres, Laura, Borja García-Lorenzo, and Pedro Serrano-Aguilar, "Estimating a cost-effectiveness threshold for the Spanish NHS," *Health economics*, 2018, *27* (4), 746–761.

Zulkarnain, Alice and Matthew S Rutledge, "How does delayed retirement affect mortality and health?," *Center for retirement research at Boston College, CRR WP*, 2018, *11.*

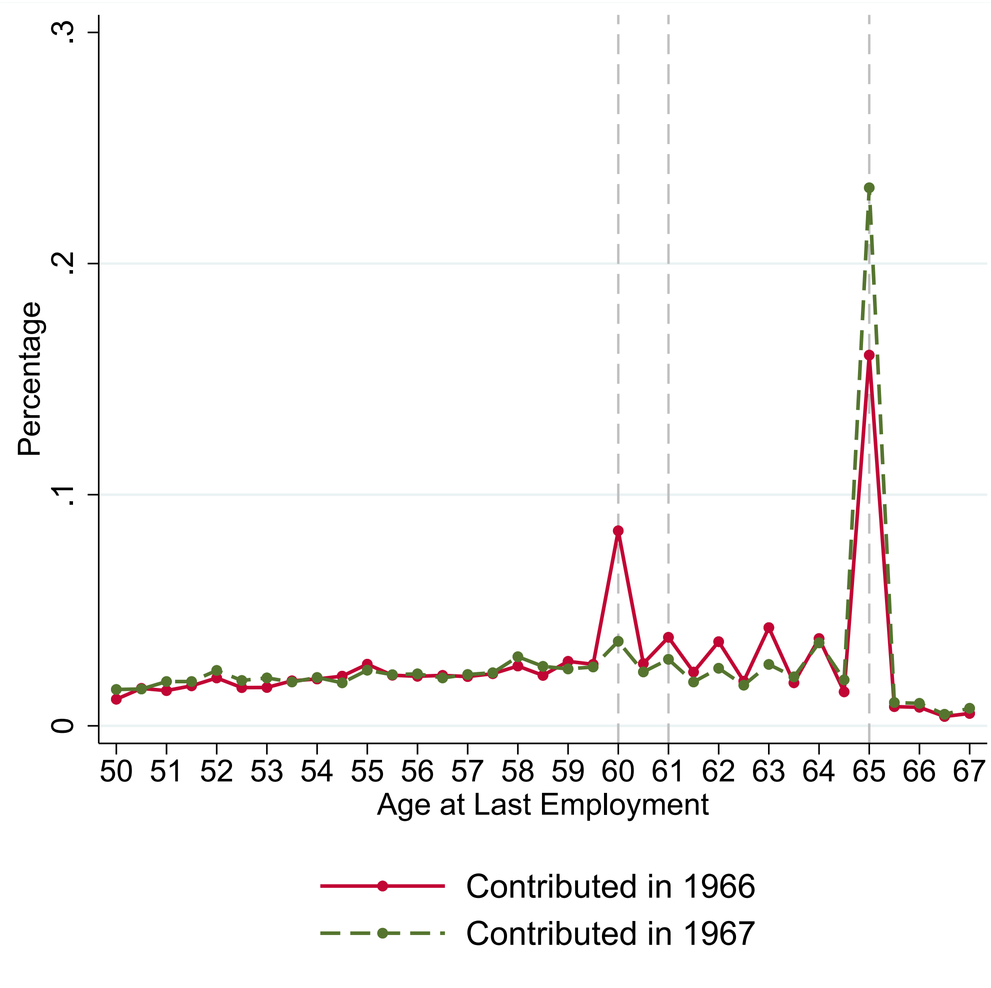
10 Figures and Tables

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



Source: Authors' own construction according to the pension laws.
Notes: This figure illustrates the statutory retirement age and the earliest possible early retirement age for individuals who contributed before and after 1 January 1967 as a function of their birth year. The blue dotted line shows that individuals who began contributing before 1 January 1967 can voluntarily retire after age reaching 60, independently of their birth year. The orange dashed line shows that those who started contributing after 1967 can only involuntary retire between ages 64 and 61, depending on their birth year. The solid grey line shows that the statutory retirement age remains at age 65 for all cohorts, regardless of the timing of their initial contributions.

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

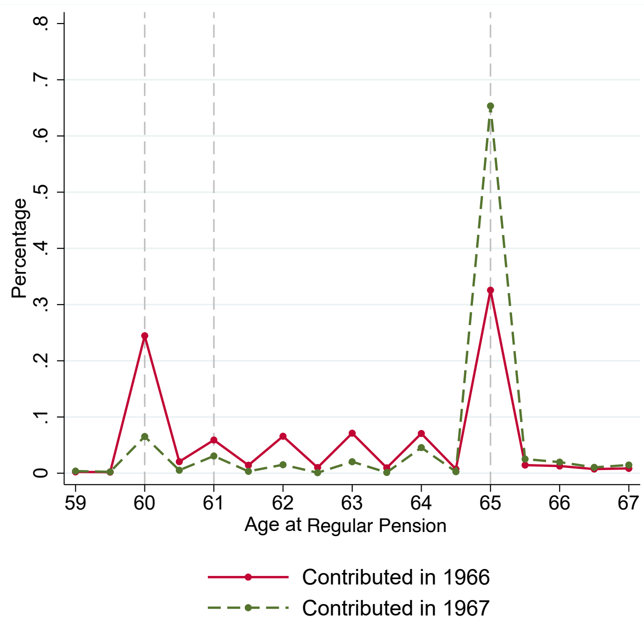


Source: MCVL, cohorts 1938-1949.

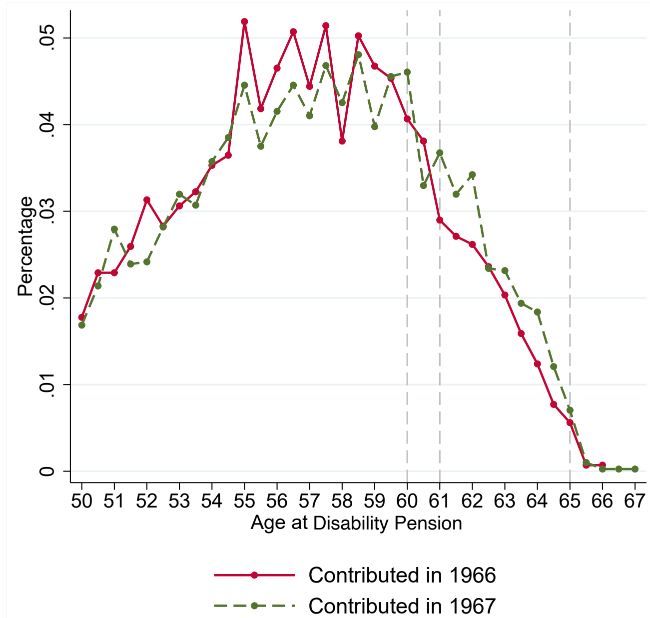
Notes: This figure plots the percentage of individuals by the age at which they finished their last employment. The solid red line shows the percentage of individuals who started contributing in 1966, while the green dashed line shows it for those who started contributing in 1967.

Figure 3: Density of Pension Ages by Treatment Status

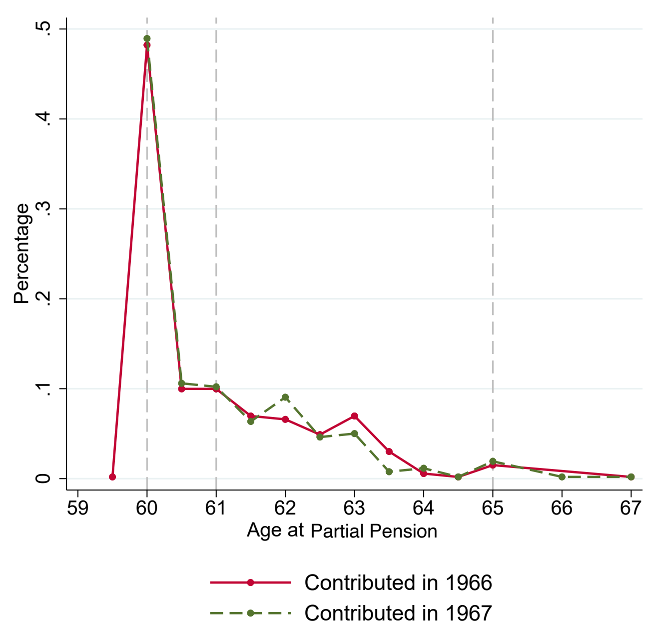
(a) Age at Regular Pension



(b) Age at Disability Pension



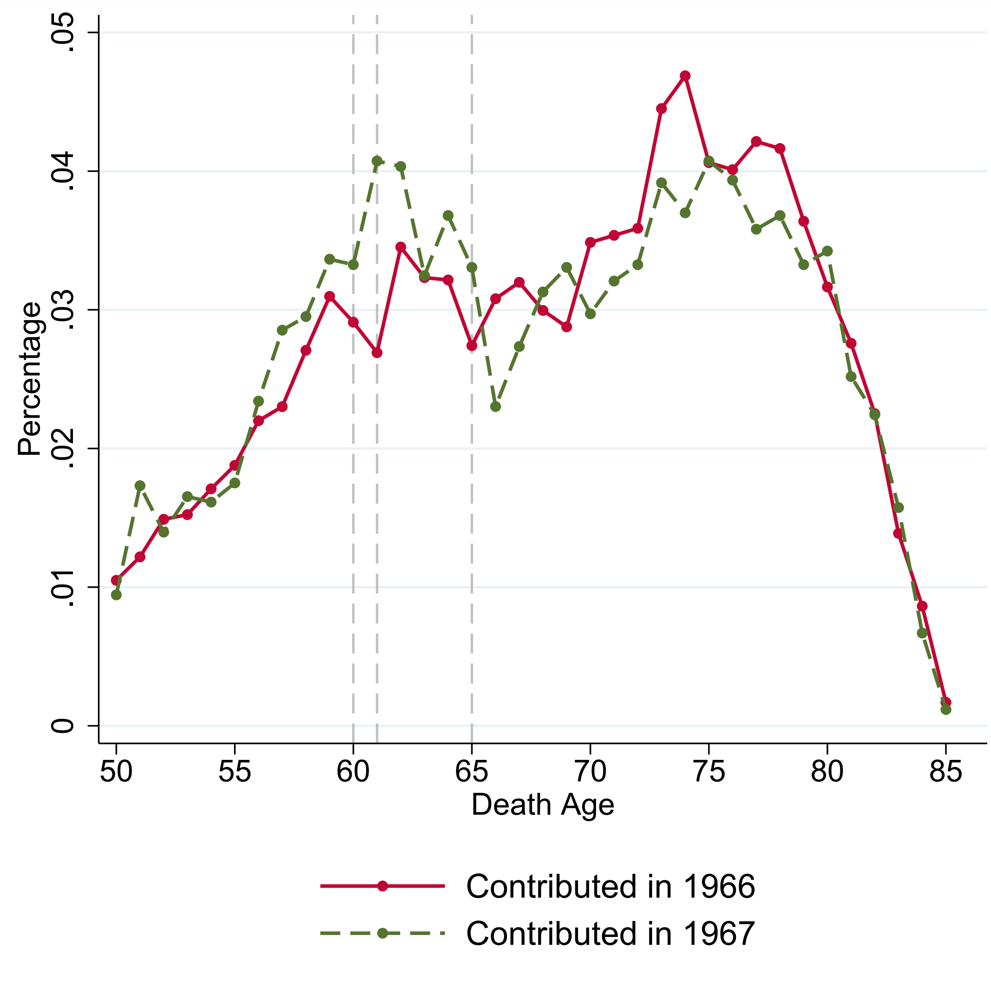
(c) Age at Partial Pension



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 percentage for individuals who started contributing in 1966, while the green dashed lines show it for those who started contributing in 1967.

Figure 4: Density of Age at Death by Treatment Status

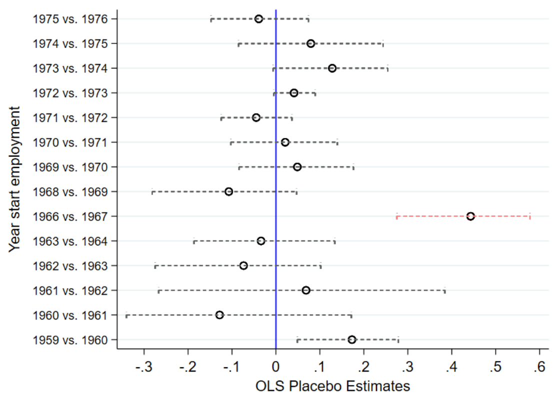


Source: MCVL, cohorts 1938-1949.

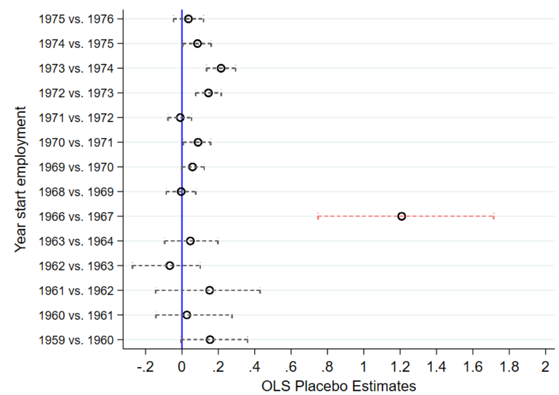
Notes: This figure plots the percentage of individuals by the age at which they died. The solid red line shows the percentage for individuals who started contributing in 1966, while the green dashed line shows it for those who started contributing in 1967.

Figure 5: Placebo Tests for Retirement Outcomes: Using Other Cutoffs

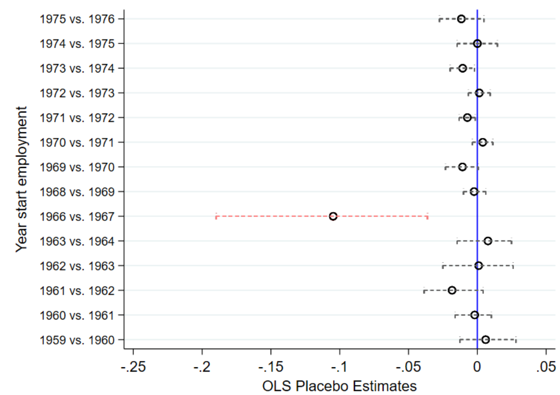
(a) Age at Last Employment



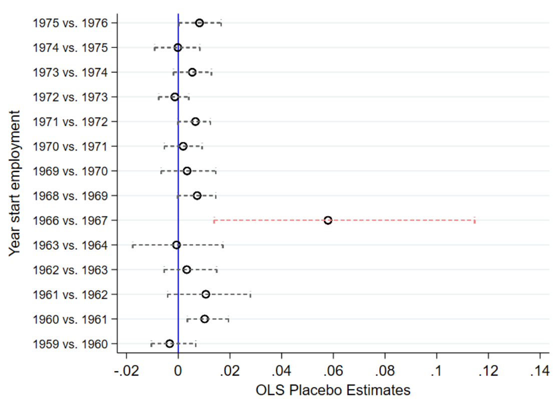
(b) Age at Claiming Regular Pension



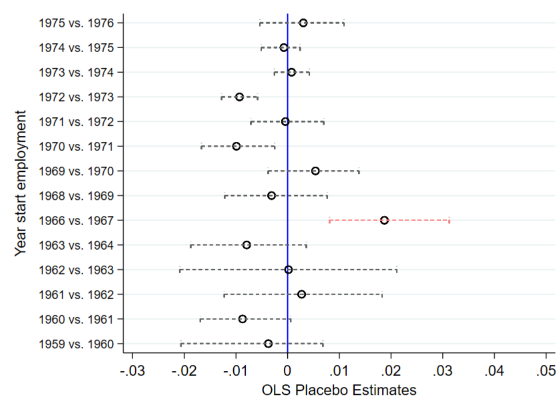
(c) Probability of Claiming Regular Pension



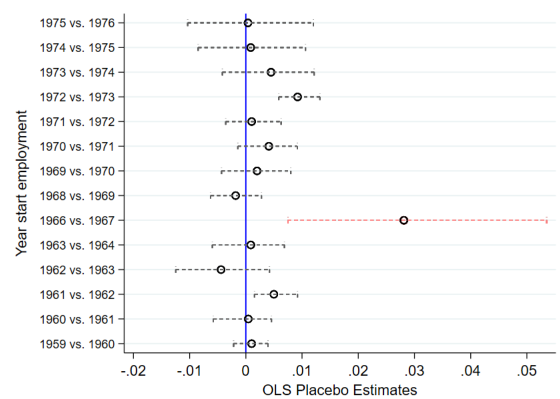
(d) Probability of Claiming Disability Pension



(e) Probability of Claiming Partial Pension



(f) Probability of Not Claiming Any Pension

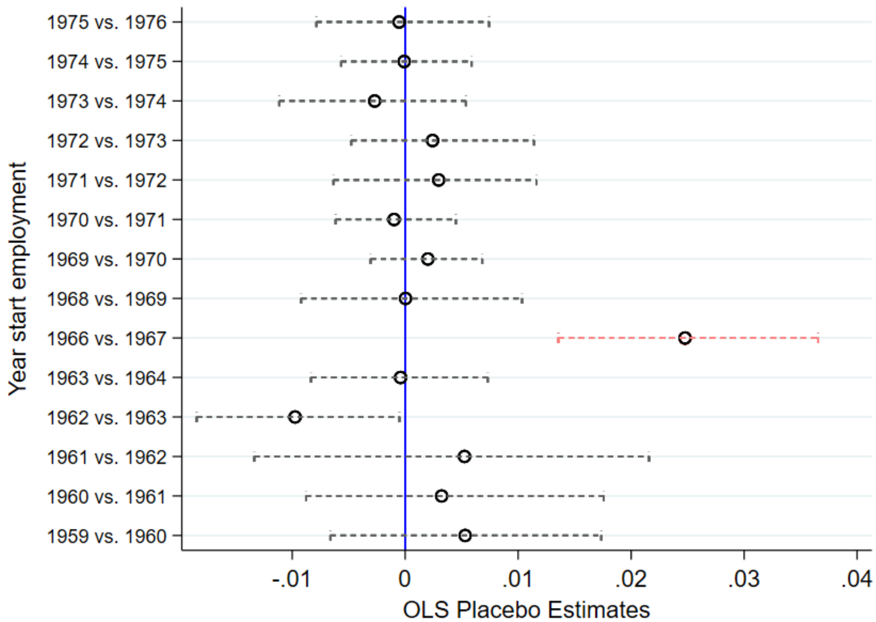


Source: MCVL, cohorts 1938-1949.

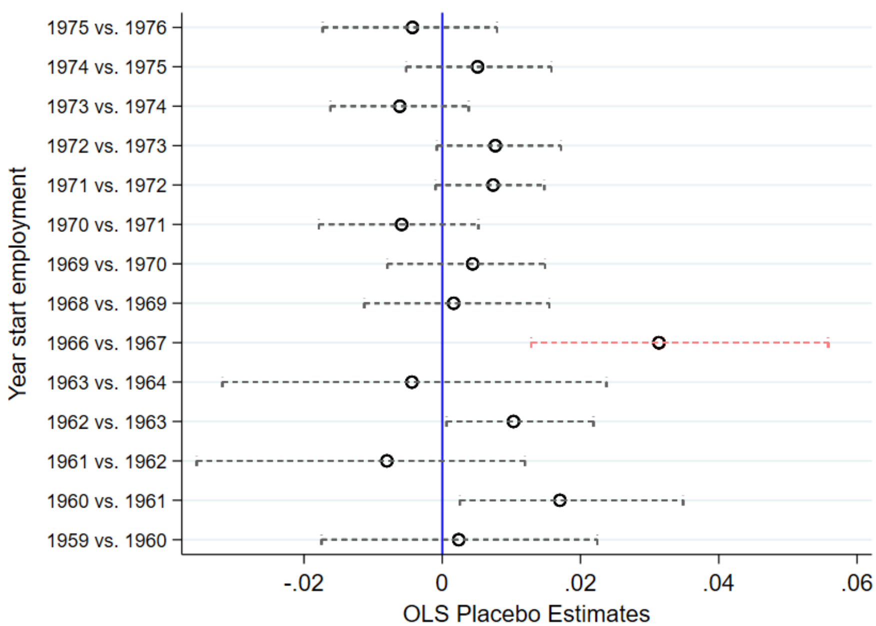
Notes: These figures show the estimates and the 95 percent confidence intervals of a list of placebos, estimating regression 1 comparing individuals that starting contributing in the years of the y-axis. The red estimate corresponds to the estimation of the regression 1 on the real cutoff: 1966 vs. 1967. 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



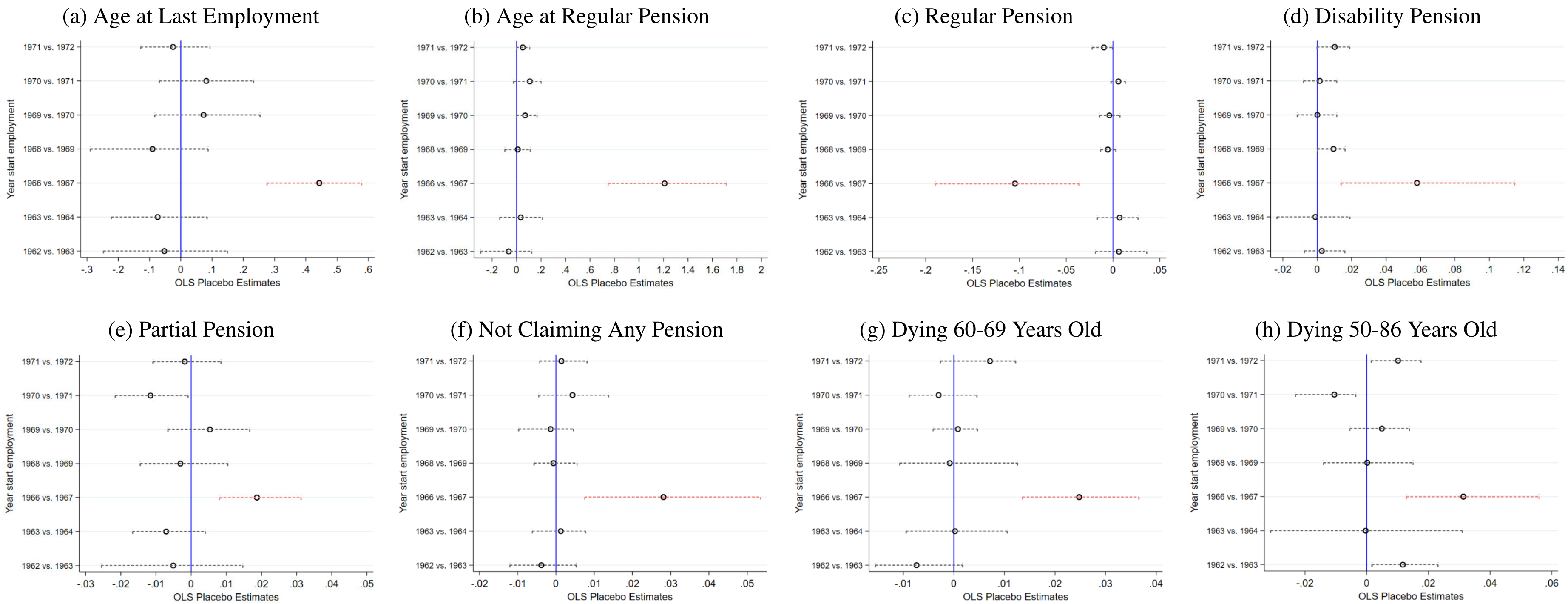
(b) Dying between 50 and 86 Years Old



Source: MCVL, cohorts 1938-1949.

Notes: These figures show the estimates and the 95 percent confidence intervals of a list of placebos, estimating regression 2 comparing individuals that starting contributing in the years of the y-axis. The red estimate corresponds to the estimation of the regression 2 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



Source: MCVL, cohorts 1932-1954.

Notes: These figures show the estimates and the 95 percent confidence intervals of a list of placebos, estimating regressions 1 and 2 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 1 and 2 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 and Age at Last Employment

	First Pension Claimed				Age at
	Regular Pension	Partial Pension	Disability Insurance	No Pension	Last Employment
	(1)	(2)	(3)	(4)	(5)
Contributed in 1967	-0.105***	0.019***	0.058**	0.028***	0.443***
	(0.033)	(0.005)	(0.021)	(0.009)	(0.066)
	[0.006]	[0.003]	[0.020]	[0.006]	[0.003]
Month-Year Birth FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓
Observations	26,102	26,102	26,102	26,102	26,102
R^2	0.129	0.066	0.077	0.030	0.087
Mean Dep. Variable (Treated)	0.417	0.048	0.368	0.168	59.948
Mean Dep. Variable (Control)	0.557	0.035	0.280	0.129	59.478

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the probability of leaving the labour market through a regular pension (Column 1), a partial pension (Column 2), a disability pension (Column 3) or not claiming any pension (Column 4), and on the age at which individuals finished their last employment (Column 5), obtained from the estimation of regression 1. 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 Pension Benefit

	Pension Benefit (1)	Base Pension (2)	Percent of Base Pension (3)	Annual Pension Benefit (4)	Lifetime Pension Benefit (5)	Duration Pension (6)
Contributed in 1967	14.955*	-12.816	2.308***	179.455*	-3228.213*	-0.764***
	(7.122)	(11.346)	(0.408)	(85.458)	(1650.506)	(0.157)
	[0.075]	[0.287]	[0.003]	[0.075]	[0.093]	[0.001]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓
Observations	26,102	26,102	26,102	26,102	26,102	26,102
R^2	0.197	0.230	0.039	0.197	0.157	0.035
Mean Dep. Variable (Treated)	998.205	1012.942	72.411	11978.458	134587.040	9.178
Mean Dep. Variable (Control)	953.465	1003.744	69.144	11441.586	134015.630	10.140

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the monthly pension benefit (Column 1), the pension base (Column 2), the pension adjustment factor (Column 3), the annual pension benefit (Column 4), lifetime pension benefit (Column 5), and pension duration (Column 6), obtained from the estimation of regression 1. For individuals who do not claim any pension, all of these variables are set to zero. 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 Age at Last Employment on Mortality

	Probability of Dying between the Ages					Age at Death Censored 74
	50-86	50-59	60-69	70-79	80-86	
	(1)	(2)	(3)	(4)	(5)	(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 effect of the reform on mortality (reduced form effect using regression 2), and the second panel shows the correlation of age at last employment on mortality (OLS). The IV estimates, obtained from the estimation of regression 3, 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 4: 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 effect of the reform on mortality (reduced form effect using regression 2), and the second panel shows the correlation of age at last employment on mortality (OLS). The IV estimates, obtained from the estimation of regression 3, 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 5: 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 Kroll (2011) (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 1). 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 2. After that, we report the IV estimates obtained from the estimation of regression 3. 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 6: 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 1. The second panel reports the first stage of the IV estimation (the reform's effect on the age at last employment, using 1). 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 2. After that, we report the IV estimates obtained from the estimation of regression 3. 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: Robustness: Correction of the Year Start Contributing

	Baseline Correction 1966-1967		No Correction		Correction 1966-1967 Removing Months 12-1966 and 1-1967		Only correct Months 12-1966 and 1-1967		Use Years of Inactivity for Correction	
	Age Last Employment (1)	Mortality Age 60-69 (2)	Age Last Employment (3)	Mortality Age 60-69 (4)	Age Last Employment (5)	Mortality Age 60-69 (6)	Age Last Employment (7)	Mortality Age 60-69 (8)	Age Last Employment (9)	Mortality Age 60-69 (10)
Reduced Form:	0.443***	0.025***	0.552**	0.017***	0.259***	0.024***	0.670***	0.016***	0.305**	0.027***
Contributed in 1967	(0.066)	(0.005)	(0.210)	(0.003)	(0.074)	(0.008)	(0.175)	(0.004)	(0.105)	(0.006)
	[0.003]	[0.000]	[0.009]	[0.000]	[0.008]	[0.004]	[0.002]	[0.006]	[0.031]	[0.000]
IV:		0.045***		0.022***		0.065***		0.018***		0.064**
Age at Last Employment		(0.009)		(0.006)		(0.024)		(0.004)		(0.023)
		[0.000]		[0.000]		[0.006]		[0.000]		[0.013]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	26,102	23,922	56,152	53,877	18,124	16,691	45,101	42,827	21,050	18,870
Mean Dep. Variable (Treated)	59.948	0.155	61.261	0.103	60.192	0.146	61.119	0.111	59.435	0.173
Mean Dep. Variable (Control)	59.478	0.115	60.583	0.080	59.900	0.112	60.34	0.089	59.037	0.138
F-stat FS		92.849		9.874		31.732		16.961		19.812

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of the reform on the age at last employment (Columns 1, 3, 5, 7, and 9) and the probability of dying between the ages 60 and 69 (conditional on surviving until age 59) (Panel A of Columns 2, 4, 6, 8, and 10), obtained from the estimation of regressions 1 and 2 using different corrections for the years that individuals started contributing reported in the affiliation data. Panel B of Columns 2, 4, 6, 8, and 10 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 3 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 dropping the last month of 1966 and the first month of 1967. Columns 7 and 8 makes the same correction of Columns 1 and 2 but only to the last month of 1966 and the first month of 1967. Finally, Columns 9 and 10 uses not only the number of years of contribution but the number of inactive years to do the correction. 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.

Online Appendix

The Effect of Removing Early Retirement on Mortality

Cristina Bellés-Obrero
Universitat de Barcelona, IEB, IZA

Sergi Jiménez-Martín
Universitat Pompeu Fabra, and Barcelona School of Economics

Han Ye
University of Mannheim, IZA, ZEW

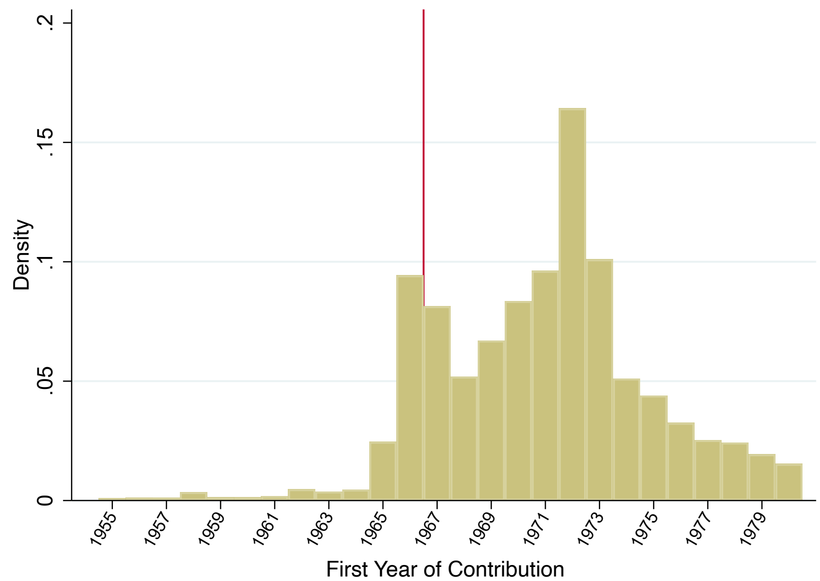
Contents

List of Figures

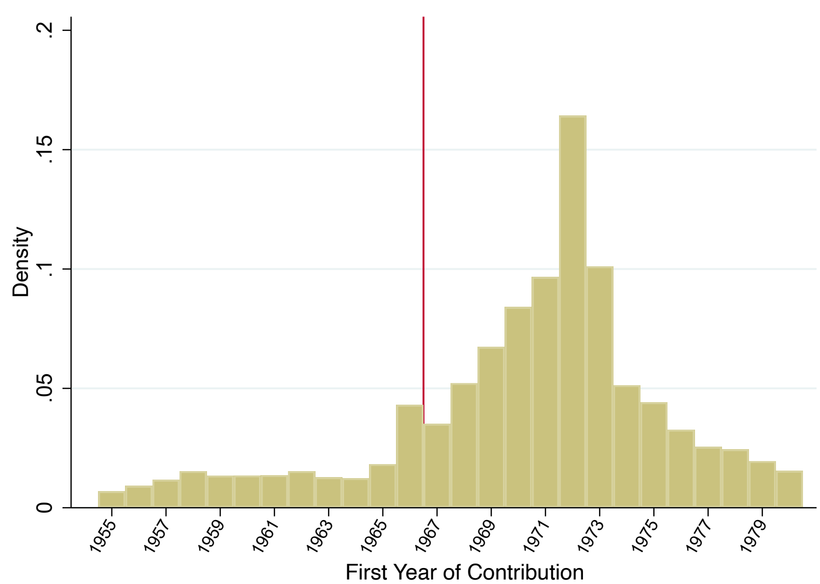
List of Tables

A Appendix Tables and Figures

Figure A1: Correction of Year Started Contributing



(a) Without Correction

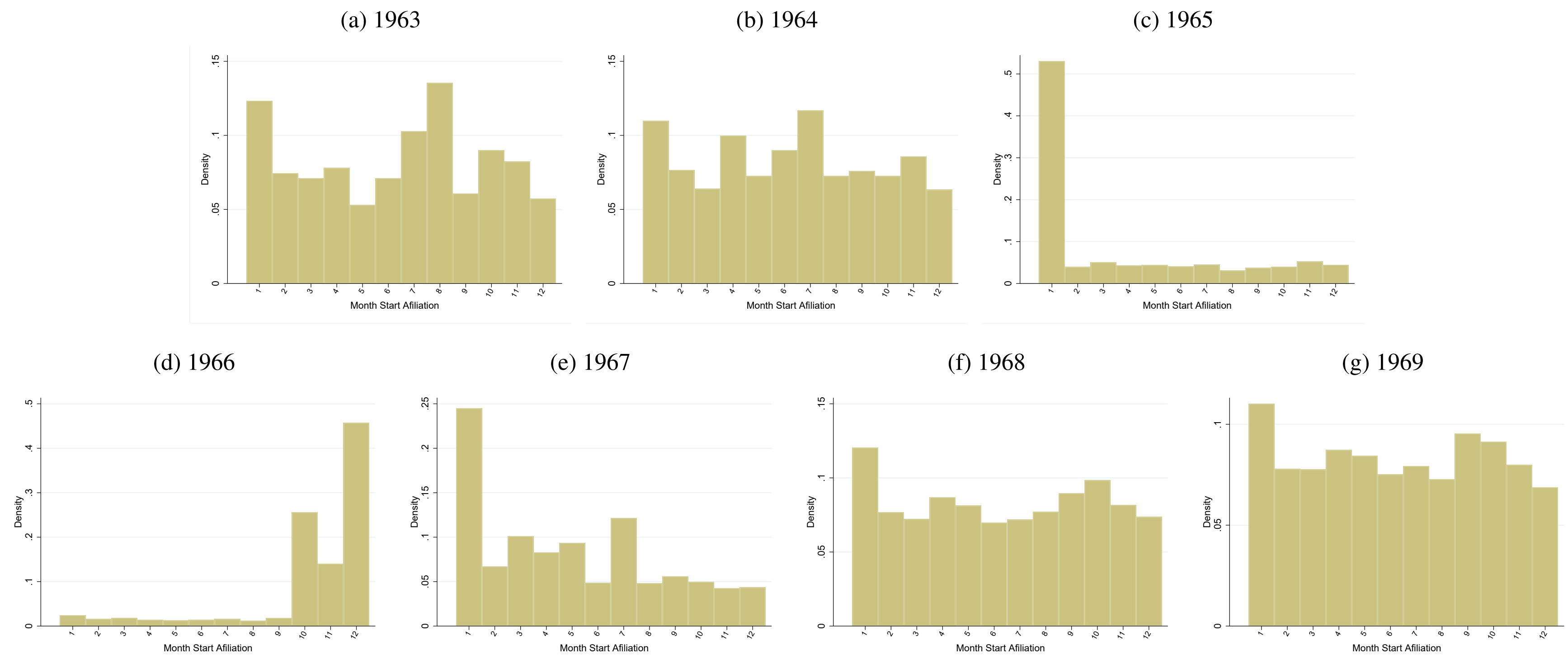


(b) With Correction

Source: MCVL, cohorts 1938-1949.

Notes: These figures plot the density of the first year of contribution without correction (Graph a) and with correction (Graph b). The correction involves using the number of years of contribution and the date of starting a regular or partial pension (years of contribution are not available for individuals who claim a disability pension) to adjust the starting contribution date for those whose initial year of contribution was between 1965 and 1967.

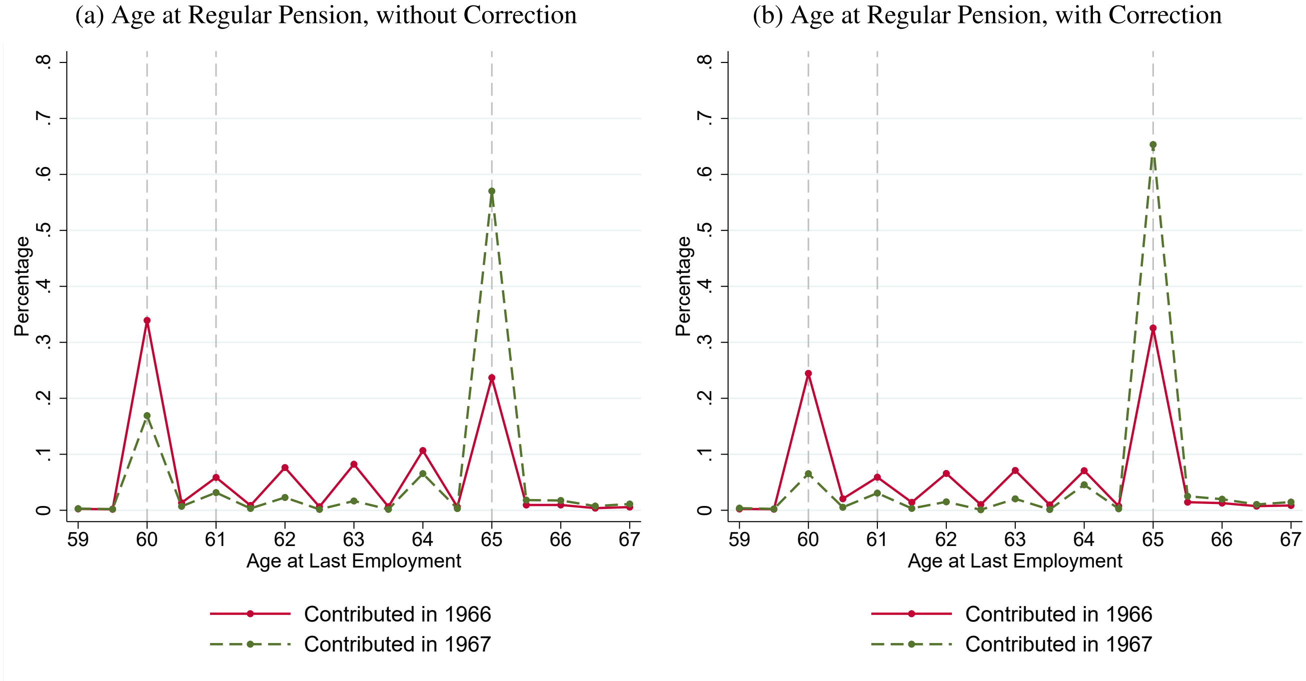
Figure A2: Distribution of Month Started Contribution by Year of First Contribution



Source: MCVL, cohorts 1938-1949.

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

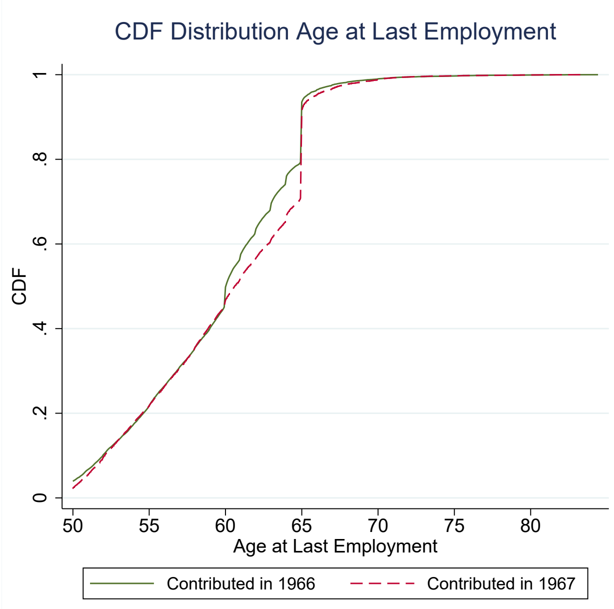
Figure A3: Density of Age at Regular Pension by Treatment Status with and without Correcting for Year of First Contribution



Source: MCVL, cohorts 1938-1949.

Notes: These figures plot the percentage of individuals by the age at claiming a regular pension without correction (Graph a) and with correction for the first year of contribution (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.

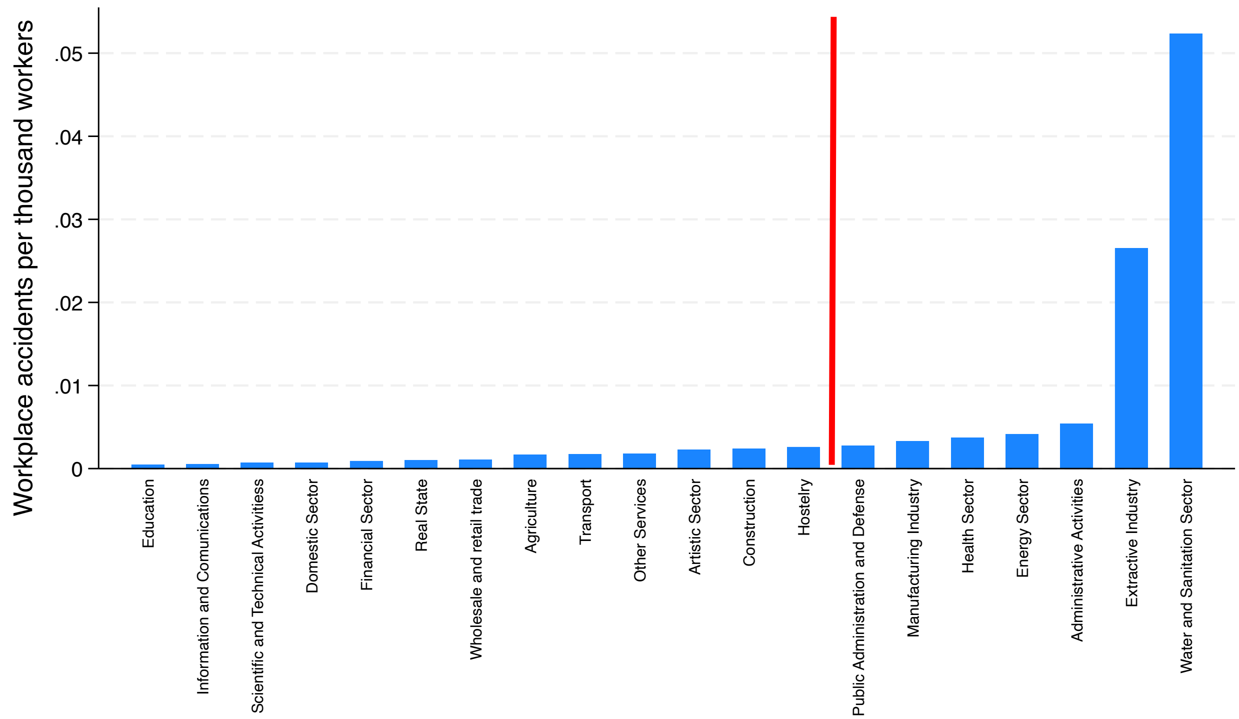
Figure A4: Cumulative Distribution Function of Age at Last Employment by Treatment Status



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.

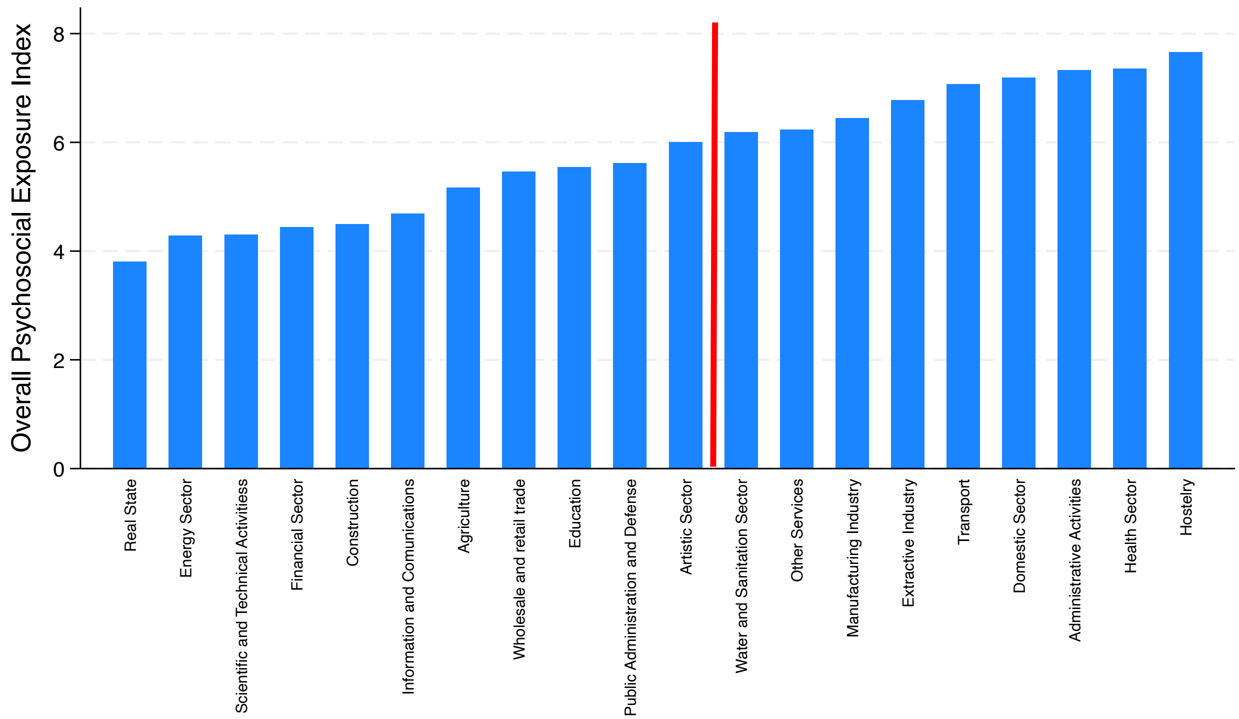
Figure A5: Classification of Industries by Incidence of Workplace Accidents



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, categorized by the industry sector in which the workers were employed at the time of the accident.

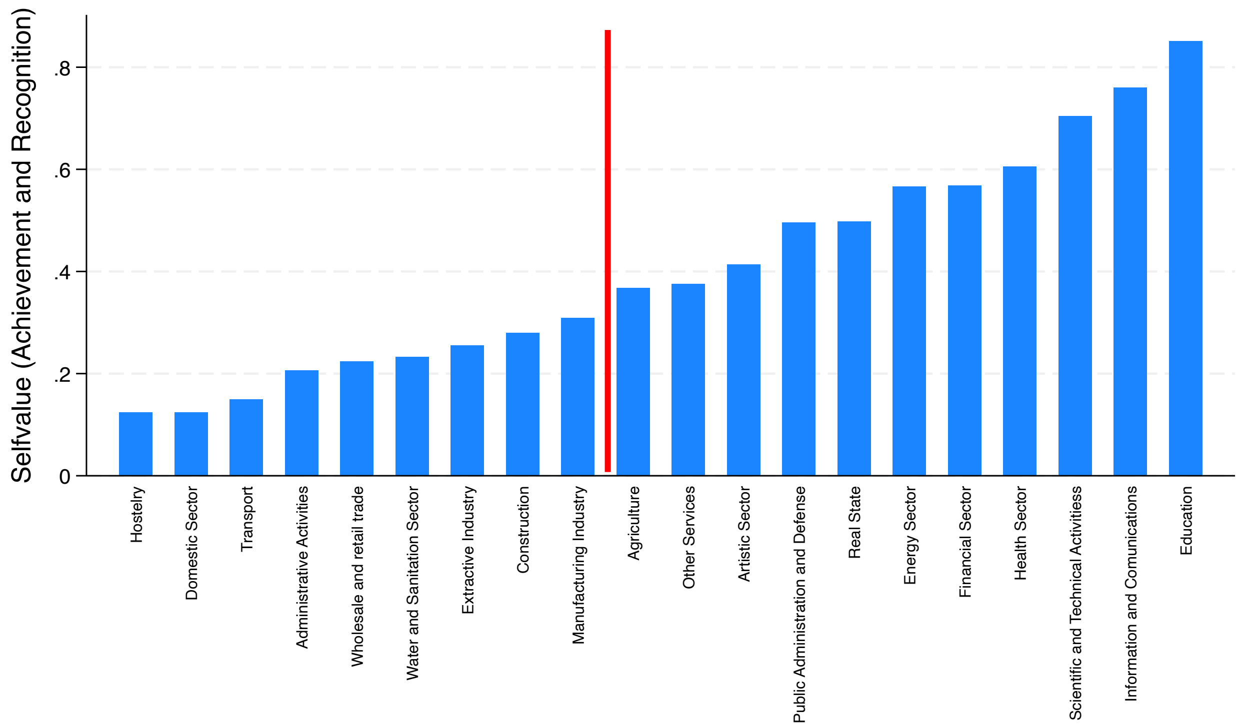
Figure A6: Classification of Industries by Psychosocial Exposure



Source: MCVL, cohorts 1938-1949.

Notes: This figure plots the different industry sectors categorized by the degree of psychosocial pressure (mental, social stress, and temporal load) to which individuals working in these sectors are exposed. We adopt the definition of psychosocial exposure as used by Kroll (2011)."

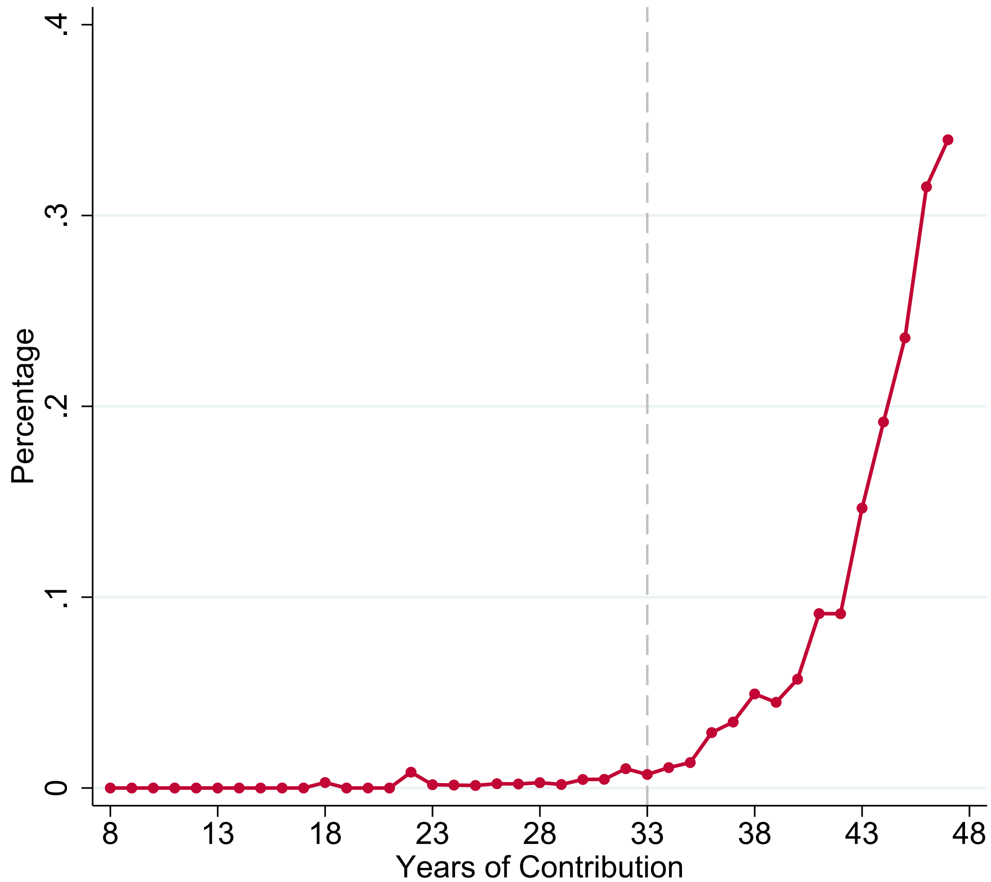
Figure A7: Classification of Industries by Self-value Index



Source: MCVL, cohorts 1938-1949.

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

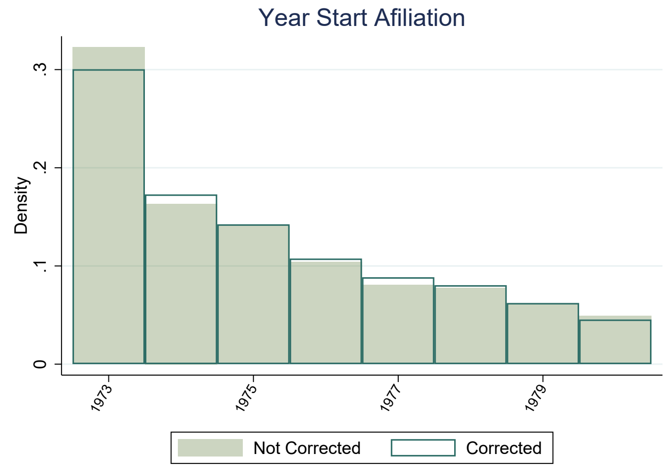
Figure A8: Probability of Claiming Partial Pension by Years of Contribution



Source: MCVL, cohorts 1938-1949.

Notes: This figure plots the percentage of individuals that claim a partial pension based on the number of years they have contributed to the Social Security system.

Figure A9: Distribution by First Year of Contribution with and without Correction for Years 1973 to 1980



Source: MCVL, cohorts 1938-1949.

Notes: This figure compares the distribution of individuals based on the year they started contributing to the Social Security system for the years 1973 to 1980 in our sample, both with and without correction.

Table A1: Sample Selection

	Sample Selection	
	Observations Dropped	Mortality 60-64 in Obs. Dropped
	(1)	(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 1. 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. Active < 8 years		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, 6, and 8), obtained from the estimation of regressions 1 and 2 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 3 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: Summary Statistics by Treatment Status

	Started contributing in						P-value
	1966			1967			
	Mean	SD	N	Mean	SD	N	
Covariates							
Male	0.67	0.47	15,299	0.80	0.39	10,803	0.000
Labor market activities between age 30 and 40							
Frac. Active.	91.48	27.91	15,299	95.14	21.50	10,803	0.574
Frac. Employed	87.37	29.10	15,299	91.90	22.95	10,803	0.266
Blue-collar Occ.	0.49	0.50	15,299	0.52	0.49	10,803	0.380
Av. Monthly Contribution	581.34	668.18	15,299	614.60	678.29	10,803	0.061
Industry between age 30 and 40							
Frac. Self-employed	8.61	25.64	15,299	11.43	29.74	10,803	0.002
Agriculture/Minery/Construction	0.10	0.30	15,299	0.12	0.32	10,803	0.765
Manufacturing	0.09	0.29	15,299	0.09	0.29	10,803	0.143
Trade/Transportation	0.07	0.25	15,299	0.07	0.25	10,803	0.208
Public/Health/Education	0.17	0.38	15,299	0.16	0.37	10,803	0.769
Science/Administrative	0.03	0.15	15,299	0.02	0.15	10,803	0.830
Services/Housekeeping/Hostelry	0.02	0.13	15,299	0.01	0.13	10,803	0.856
Retirement outcomes							
Regular Pension	0.55	0.49	15,299	0.42	0.49	10,803	0.009
Partial Pension	0.03	0.18	15,299	0.05	0.21	10,803	0.000
Disability Pension	0.27	0.44	15,299	0.36	0.48	10,803	0.032
No Pension	0.13	0.33	15,299	0.17	0.37	10,803	0.018
Age at Last Employment	59.47	5.37	15,299	59.94	5.44	10,803	0.03
Age at Regular Pension	62.76	2.65	8,517	64.17	2.86	4,501	0.000
Ageat Disability Pension	57.23	3.67	4,278	57.50	3.823	3,973	0.028
Age at Partial Pension	61.12	1.38	531	61.07	1.37	519	0.02
Mortality outcomes							
Dying 50-86 y.o.	0.38	0.48	15,299	0.47	0.49	10,803	0.000
Dying 50-59 y.o.	0.07	0.26	15,299	0.097	0.29	10,803	0.221
Dying 60-69 y.o.	0.11	0.32	14,166	0.15	0.36	9,756	0.000
Dying 70-79 y.o.	0.19	0.39	12,370	0.22	0.41	8,072	0.995
Dying 80-86 y.o.	0.06	0.24	10,016	0.08	0.28	6,257	0.859

Source: MCVL, cohorts 1938-1949.

Notes: This table presents summary statistics of our baseline sample, divided into individuals who commenced contributing in 1966 (Columns 1 to 3) and those who started in 1967 (Columns 4 to 5). Column 6 displays the wild-bootstrap p-value derived from regressing each variable on a dummy variable equal to 1 if the individual began contributing in 1967. These regressions include controls for year of birth, month of birth fixed effects, and gender (except in the case where gender is the dependent variable).

Table A4: 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 A5: Impact of the Reform on the Type of Disability

	Type of Disability	
	Severe or Absolute	Partial or Professional
	(1)	(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 1. 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 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), and the probability of not claiming any pension because they are still working (Column 2), they became inactive (Column 3), and they died before claiming a pension (Column 4), obtained from the estimation of regression 1. 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 A7: 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 1. 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 A8: Impact of the Reform on the Age at Claiming Pension

	Age of the Individual at			
	First Pension	Regular Pension	Disability Pension	Partial Pension
	(1)	(2)	(3)	(4)
Contributed in 1967	0.248**	1.208***	0.195**	-0.152**
	(0.090)	(0.205)	(0.063)	(0.053)
	[0.031]	[0.001]	[0.031]	[0.032]
Month-Year Birth FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓
Observations	22,319	13,018	8,251	1,050
R^2	0.110	0.244	0.035	0.248
Mean Dep. Variable (Treated)	61.051	64.174	57.509	61.077
Mean Dep. Variable (Control)	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 which individuals claimed their first pension (any type) (Column 1), claimed a regular pension (Column 2), claimed a disability pension (Column 3), and claimed a partial pension (Column 4), obtained from the estimation of regression 1. 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 A9: 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 1. 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 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 Transportation	Public Health Education	Science Administrative	Services Housekeeping Hostelry
	(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 1. 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 Alternative Measures of Mortality

	Probability of Dying before				
	55	60	65	70	75
	(1)	(2)	(3)	(4)	(5)
Reduced Form:					
Contributed in 1967	0.003	0.012	0.031***	0.037***	0.034***
	(0.003)	(0.007)	(0.009)	(0.010)	(0.010)
	[0.280]	[0.152]	[0.003]	[0.002]	[0.005]
OLS:					
Impact of Age at Last Employment	-0.008***	-0.013***	-0.015***	-0.016***	-0.016***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
IV:					
Impact of Age at Last Employment	0.007	0.027	0.070**	0.082**	0.077**
	(0.006)	(0.018)	(0.024)	(0.027)	(0.029)
	[0.296]	[0.168]	[0.016]	[0.013]	[0.019]
Observations	26,102	26,102	26,102	26,102	26,102
Mean Dep. Variable (Treated)	0.035	0.097	0.183	0.253	0.333
Mean Dep. Variable (Control)	0.027	0.074	0.134	0.191	0.268
F-stat FS	45.338	45.338	45.338	45.338	45.338

Source: MCVL, cohorts 1938-1949.

Notes: This table reports the impact of age at last employment on the probability of dying before age 55 (Column 1), 60 (Column 2), 65 (Column 3), 70 (Column 4), and 75 (Column 5). The first panel reports the effect of the reform on mortality (reduced form effect using regression 2), and the second panel shows the correlation of age at last employment on mortality (OLS). The IV estimates, obtained from the estimation of regression 3, 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.

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

Table A12: 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 3. 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 A13: Characteristics of compliers

Characteristics	$P[X = x]$	$P[X = x \vert complier]$	$\frac{P[X=x \vert complier]}{P[X=x]}$
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 1). 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 2. After that, we report the IV estimates obtained from the estimation of regression 3. 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]
Month-Year Birth FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓
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 1). 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 2. After that, we report the IV estimates obtained from the estimation of regression 3. 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 Kroll (2011). The first panel reports the first stage of the IV estimation (the reform's effect on the age at last employment, using 1). 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 2. After that, we report the IV estimates obtained from the estimation of regression 3. 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 and Placebo: Impact by Availability of Flexible Retirement

	Baseline Sample		Robustness				Placebo	
			10 years around 33		5 years around 33		Less than 33	
Years of Contribution	< 33	≥ 33	[23,33)	[33,43]	[28,33)	[33,38]	[20,28)	[28,32]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Partial Retirement							
Contributed in 1967	0.002	0.043***	0.004**	0.027**	0.001	0.017**	-0.002	0.008**
	(0.002)	(0.011)	(0.002)	(0.009)	(0.002)	(0.007)	(0.003)	(0.003)
	[0.115]	[0.004]	[0.017]	[0.005]	[0.647]	[0.029]	[0.603]	[0.009]
	Age at Last Employment							
First Stage:	0.460***	0.802**	0.432**	0.788***	0.298	0.427**	0.336**	0.378***
Contributed in 1967	(0.070)	(0.141)	(0.099)	(0.138)	(0.137)	(0.110)	(0.113)	(0.101)
	[0.003]	[0.002]	[0.036]	[0.004]	[0.104]	[0.021]	[0.041]	[0.007]
	Probability of Dying between 60 and 69							
Reduced Form:	0.025***	0.021**	0.033***	0.024***	0.033	0.007	0.031**	0.030*
Contributed in 1967	(0.005)	(0.007)	(0.009)	(0.006)	(0.019)	(0.018)	(0.011)	(0.015)
	[0.001]	[0.020]	[0.006]	[0.007]	[0.121]	[0.685]	[0.010]	[0.074]
IV:	0.054***	0.026***	0.077***	0.030***	0.110*	0.017	0.092**	0.079**
Impact of Age at Last Employment	(0.012)	(0.007)	(0.022)	(0.008)	(0.071)	(0.040)	(0.044)	(0.037)
	[0.000]	[0.003]	[0.006]	[0.004]	[0.079]	[0.681]	[0.016]	[0.048]
Month-Year Birth FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	✓	✓	✓
Observations	11,390	12,532	5,851	8,930	4.691	3,495	3,462	3,739
Mean Dep. Variable (Treated)	0.185	0.129	0.218	0.155	0.243	0.222	0.210	0.206
Mean Dep. Variable (Control)	0.128	0.103	0.172	0.125	0.210	0.198	0.166	0.159
F-stat FS	43.193	32.272	18.988	32.609	4.691	15.162	8.781	14.021
P-value Difference (IV Est.)	0.084		0.063		0.248		0.863	

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 6. 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. In Columns 7 and 8, we perform a placebo considering only individuals that have between 20 and 32 years of contribution and comparing individuals with less or more than 28 years of contribution. The first panel reports the reform's effect on the probability of claiming a partial pension, using 1. The second panel reports the first stage of the IV estimation (the reform's effect on the age at last employment, using 1). 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 2. After that, we report the IV estimates obtained from the estimation of regression 3. 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: Retirement Outcomes

	Type of Pension				Age at
	Regular Pension	Partial Pension	Disability Pension	No Pension	Last Employment
	(1)	(2)	(3)	(4)	(5)
Panel A: Age Start FE					
Contributed in 1967	-0.098**	0.025***	0.052**	0.020**	0.525**
	(0.032)	(0.006)	(0.021)	(0.009)	(0.163)
	[0.014]	[0.005]	[0.023]	[0.036]	[0.022]
Observations	25,764	25,764	25,764	25,764	25,764
Mean Dep. (Control)	0.531	0.035	0.297	0.137	59.386
Panel B: No Controls					
Contributed in 1967	-0.103***	0.019***	0.057***	0.027***	0.533***
	(0.030)	(0.005)	(0.020)	(0.008)	(0.100)
	[0.004]	[0.001]	[0.008]	[0.006]	[0.000]
Observations	26,102	26,102	26,102	26,102	26,102
Mean Dep. Variable (Control)	0.557	0.035	0.280	0.129	59.478
Panel C: Cohorts 1941 to 1949					
Contributed in 1967	-0.160***	0.025***	0.091***	0.044***	0.418**
	(0.020)	(0.006)	(0.015)	(0.005)	(0.134)
	[0.001]	[0.001]	[0.001]	[0.000]	[0.011]
Observations	18,491	18,491	18,491	18,491	18,491
Mean Dep. Variable (Control)	0.624	0.046	0.230	0.100	59.727
Panel D: Dropping the self-employed					
Contributed in 1967	-0.112***	0.023***	0.049**	0.040***	0.529***
	(0.030)	(0.006)	(0.019)	(0.009)	(0.097)
	[0.004]	[0.001]	[0.014]	[0.003]	[0.001]
Observations	21,664	21,664	21,664	21,664	21,664
Mean Dep. Variable (Control)	0.527	0.041	0.281	0.152	58.886
Panel E: Augmented sample 1965-1968					
Contributed after 1967	-0.385***	0.106***	0.187***	0.091***	1.436***
	(0.026)	(0.016)	(0.013)	(0.006)	(0.218)
	[0.000]	[0.001]	[0.000]	[0.000]	[0.002]
Observations	48,477	48,477	48,477	48,477	48,477
Mean Dep. Variable (Control)	0.621	0.039	0.238	0.102	59.919

Source: MCVL, cohorts 1938-1949.
Notes: This table reports the estimated impact on retirement decisions by varying the model specifications and sample selection. Panel A uses the age at first contribution fixed effects instead of month and year of birth fixed effects. Panel B does not controlling for the labor market activities between ages 30 and 40. Panel C restricts the sample to cohorts born between 1941 and 1949. Panel D drops individuals that are in one of the self-employed pension regimes. Panels A to D contain individuals who started contributing in 1966 and in 1967. Panel E augments the baseline sample and includes people who started contributing in 1965 to 1968. All specifications control for gender and year of birth fixed effects (except for Panel A, which instead includes age at first contribution fixed effects) and labor market activities between ages 30 and 40 (except for Panel B). 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: Mortality Outcomes

	Probability of Dying between the Ages 60 and 69					
	Baseline	Age Start FE	No Controls	Cohorts 1941-1949	Drop Self-employed	Augmented 1965-1968
	(1)	(2)	(3)	(4)	(5)	(6)
Reduced Form Contributed since 1967:	0.025***	0.021***	0.029**	0.031***	0.028***	0.024***
	(0.005)	(0.005)	(0.010)	(0.006)	(0.007)	(0.003)
	[0.001]	[0.000]	[0.001]	[0.003]	[0.000]	[0.000]
IV: Age at Last Employment	0.044***	0.030***	0.036***	0.058***	0.043***	0.014***
	(0.009)	(0.010)	(0.009)	(0.011)	(0.012)	(0.002)
	[0.000]	[0.002]	[0.006]	[0.002]	[0.001]	[0.000]
Month-Year Birth FE	✓		✓	✓	✓	✓
Age Start Contributing FE		✓				
Controls	✓	✓		✓	✓	✓
Contributed 1966-1967	✓	✓	✓	✓	✓	
Contributed 1965-1968						✓
Observations	23,922	23,922	23,922	17,184	19,579	45,306
Mean Dep. Variable (Treated)	0.155	0.155	0.155	0.145	0.172	0.123
Mean Dep. Variable (Control)	0.115	0.115	0.115	0.098	0.123	0.107
F-stat FS	77.336	17.681	90.753	57.011	73.376	48.845

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), restringing the sample to individuals that are not in one of the self-employed pension regimes (Column 5), and augmenting the baseline sample and includes people who started contributing in 1965 to 1968 (Column 6). The first panel reports the effect of the reform on mortality (reduced form effect using regression 1). The IV estimates, obtained from the estimation of regression 2, 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 A21: 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 1. 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: 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 1. 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.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

B Additional Details on the Spanish Pension System

The current old-pension system for the elderly in Spain is a pay-as-you-go system, with an average replacement rate of around 80% (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.

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

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

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

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

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

The above retirement rules apply to all individuals, except for the earliest age at pension eligibility. People in the control and treatment group face the same statutory retirement age of 65 years, with a minimum contribution period of eight years (further modified to 15 years after the 1997 reform). The only difference is early retirement age.

C Additional 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 A2, 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 A1, 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 A1 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. This limitation is also the reason we cannot use a Regression Discontinuity Design.

When we further zoom in and look at monthly distribution. Figure 2 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.

After consulting with workers at pension office, we believe that these are "administrative bunchings" as a result of administrative practices. At the time of retirement, individuals need to prove that their first contribution was before 1967 to the pension office if not recorded as such, to gain eligibility for the "old regime". Typically, if they can provide a payslip that occurred before 1967, the pension office is likely to simply record "December 1966" as their first date of the contribution, even though they may actually have started working many years before 1967. However, if they choose to dispute but fail to convince the pension office that they started working before 1967, the pension office tends to record "January 1967" as their first contribution date. The bunching in January 1965 seems to be due to similar reasons.

The "administrative bunching" due to correction made by pension offices do not endanger the assignment of treatment status, however, wrongly assigned starting dates can make our treated and control groups less comparable. Those bunch in January 1967 or December 1966 could have, in fact, started working in different years and could have different characteristics. We partially addresses this concern by controlling for an extensive list of fixed effects and covariates.

However, this is the not the only story. There are still mistakes in the records. In fact, in Figure A3 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.

To deal with those mistakes, we correct the "reported date of first contribution" by using the number of years of contribution and the date individuals claim a regular pension, which are accurately recorded. First, we create a new variable "calculated date of first contribution" by subtracting the total number of years contributed from the calendar date they claim a regular pension. Then, if the "calculated date of first contribution" is before the "reported date of first contribution", we make this correction. Note that the correction is only possible for individuals who have claimed a regular pension, because 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 A1 that the bunching has been greatly reduced. Figure A3 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 A3 a) and b) shows that our correction does a decent job reassigning year start contribution.

To test the "out-of-sample" validity of the correction, we perform the correction for people in our sample who started contributing after 1973. Because the variable "year started contribution" should be accurately recorded, we expect our correction procedure will not make too many alternations. Figure A9 confirms it.

To test the robustness of our results to the correction method, we also perform two alternative methods. First, in addition to correcting for individuals with reported year start contributing in 1966 and 1967, we drop the individuals who reported starting contributing in December 1966 and January 1967. Second, we only correct these who reported starting contributing in December 1966 and January 1967. Table 7 reports the robustness of our main results to the correction methods. All estimates are robust to this alternative correction method. Moreover, Table 7 shows that estimates when using a sample without any correction are also not too different.

D Details of Additional Analysis

D.1 Labor supply responses

Because the reform influenced the probability of individuals claiming different types of pensions, we should interpret the impact on age at claiming these pensions with caution. Nevertheless, these analysis could help us to understand the adjustment margins behind the increase in age at exiting employment.

We find that the age at claiming first pension is increased by 3 months and the age at claiming a regular pension is increased by one year and two months. Similar as the impact on age at last employment, we observe the impact on age at claiming regular pension is most pronounced at age 60 and 65. Table A21 shows that the reform reduces the probability of claiming a regular pension at age 60 by 10 percentage points ($\sim$ 67 %) and increases the probability of claiming a regular pension at age 65 by 9.3 percentage points ($\sim$ 49 %).

The ages at claiming a disability pension and a partial pension are also affected. Individuals who contributed in 1967 delay claiming disability by around two and a half months but claim a partial pension earlier by around two months. Table A22 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 and partial pensions as early retirement schemes when regular pension is not available.