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Impact of Later Retirement on Mortality: Evidence from France*

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Abstract

This paper investigates the impact of delaying retirement on mortality among the French population. We take advantage of the 1993 pension reform in the private sector to identify the causal effect of an increase in claiming age on mortality. We use administrative data which provide detailed information on career characteristics, dates of birth and death. Our results, precisely estimated, show that an exogenous increase of one year in the claiming age has no significant impact on the probability to die. To test the power of our sample to detect statistically significant effects for rare events like death, we compute minimum detectable effects (MDE). Our MDE estimates suggest that, if an impact of later retirement on mortality would be detectable, it would remain very small in magnitude.

JEL Codes: I10, J14, J26, H55
Keywords: *pension reform, health, mortality*

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Introduction

In a context of demographic ageing, most developed countries have been carrying out reforms in order to maintain the financial sustainability of pension systems. Most of these reforms have consisted in increasing incentives for delayed retirement. These policies have been widely regarded as successful in so far as labour market participation of older workers has increased in nearly every country which implemented a reform. However, the impacts of a longer working life on other outcomes, like health, have been harder to establish.

As of today, there is no consensus in the literature on the causal impact of later retirement on health outcomes. Three dimensions of health have attracted most research: self-reported health[1]; physical health[2]; and mental health – which is usually observed through depression and cognitive functioning[3]. The most consistent relationship established by the literature is that self-reported health is improved by retirement, but this falls short of a causal impact on objective measures of health – the key parameter of interest, as it could capture increased well-being associated with retirement.

There are few studies looking at the impact of later retirement on mortality. The expected results are not necessarily obvious. One may think work preserves health, through maintaining physical activities and social interactions. In that case, we may expect a positive impact of delaying retirement on health and a negative impact on mortality. On the contrary, one may think work is detrimental to health because of strain and stress. In that case, we may expect an increase in mortality consecutive to an increase in retirement age. Retirement may also affect mortality through income effects.[4]

[1]Coe and Lindeboom (2008); Coe and Zamarro (2011); Eibich (2015) show that retirement has a positive effect on self-report health. Blake and Garrouste (2019) find a negative effect of the 1993 reform on perceived and physical health, concentrated on the less-educated, while Messe and Wolff (2019) find non-significant impact of early retirement on health.

[2]These studies used activity daily living (ADL), instrumental activity daily living (IADL), and mobility index (walking ability, strength, climbing stairs). Bound and Waidmann (2007) find a positive, albeit temporary, effect on male (but not female) physical health. Neuman (2008) find no significant effect on muscle function and mobility.

[3]Studies on mental health show mixed results: Bingley and Martinello (2013); Bonsang et al. (2012); Coe and Zamarro (2011); Rohwedder and Willis (2010) show that retirement has either a negative or a non significant impact on cognitive functioning (memory test and verbal fluency). Mazzonna and Peracchi (2017) find heterogeneous effects on cognitive abilities across occupational groups. Bradford (1979); Carp (1967); Sheppard (1985) show retirement may be stressful and associated with a feeling of ageing and loneliness. Delaying retirement is associated with stress and strains (Ekerdt et al., 1983; Atalay and Barrett, 2014).

[4]Roger et al. (2005) show that doubling income leads to a decrease of mortality by 10%. On the opposite, Snyder and Evans (2006) show higher income groups are significantly associated with higher

Mortality is an interesting health outcome for several reasons. First, mortality is an objective health measure, available in most datasets, in particular panel data and administrative data. Second, it conjugates various health problems an individual may have experienced during his life. Third, it has the advantage of being easier to interpret – contrary to self-reported health which could simply capture well-being. Fourth, mortality measurement does not vary across different countries, so it is easy to draw up international comparisons. Self-reported health is known to vary across countries, even conditioning on objective measures of health, as cultural differences in the way respondents rank their own health vary.

Only a limited set of studies estimate the causal impact of retirement on mortality, with contrasting results. Coe and Lindeboom (2008) find no significant impact of early retirement on mortality using Health and Retirement Study (HRS) data. Hernaes et al. (2013) exploit a reduction of the early retirement age (ERA) in Norway from 67 to 62, for a subset of the population, and find no significant effect of early retirement on mortality. Hallberg et al. (2015) focus on Swedish military officers, and find that an ERA at 55 instead of 60 is associated with a decline in mortality. Bloemen et al. (2017) find a decrease of mortality within five years by 2.6 percentage points among Dutch male civil servants. In contrast, Kuhn et al. (2018) find that early retirement leads to an increase by 2.4 percentage point of the death probability before age 67 among blue-collar men workers in Austria. Hagen (2018) estimates the effect of an increase in retirement age due to the Swedish pension system reform on women mortality. The results show no evidence that the reform affects mortality or health care use for Swedish women. However, Zulkarnain and Rutledge (2018) find that delaying retirement reduces death probability within five years for men aged 62-65 in the Netherlands. Finally, using a regression discontinuity design, Fitzpatrick and Moore (2018) find a two percent increase of death counts for American men at the ERA – i.e., at age 62 –, but no effect for women.

Our paper contributes to this small literature by exploiting the 1993 French pension reform which was the first to reverse the trend towards earlier retirement in that country. The reform consisted in increasing the contribution length required for a full-rate pension progressively by cohort of birth. The reform impacted differently individuals born in the

mortality rate.

same year according to the contribution length they had acquired at the ERA, i.e., age 60 at the time. We use the change in retirement incentives as an instrumental variable in a two-stage-least-square (2SLS) model to measure the impact on mortality. We use administrative data encompassing the universe of private sector wage earners in France born between 1930 and 1950 – the 2017 data from *Caisse Nationale d'Assurance Vieillesse* (CNAV) – which gathers more than 10 million observations, from 450,000 to 650,000 retirees per cohort.

The first stage of the 2SLS regression shows a strong and significant effect of the 1993 reform on claiming age, both for the youth cohorts deeply affected by the reform and for the old cohorts slightly affected. The second stage of the 2SLS shows that an exogenous increase of claiming age by one quarter has no significant impact neither on the probability to die between ages 65 and 72, nor between ages 72 and 77. This result is also not significant for men and women separately.

Contrary to a large share of the literature our results are precisely estimated, i.e., we find very precise effect around 0. We discuss in the paper the sample size necessary to estimate significant effects of such small size, and review previous literature in that light. We also discuss the interpretation of different studies which focus on specific subset of the population.

This paper is structured as follows: Section 1 presents the institutional framework and the 1993 French pension reform while Section 2 presents the data, the sample and the method, Section 3 the results and Section 4 a discussion of the results.

1 Institutional Framework

The French pension system is a mandatory pay-as-you-go pension scheme. There are several pensions schemes, and individuals contribute to the one associated with their professional occupation group (private sector, public sector, etc.). The 1993 French pension reform only affected wage earners in the private sector. Hence, we focus in this section on pension rules in the private sector before and after the 1993 reform.

1.1 Private Sector Pensions Before the 1993 Reform

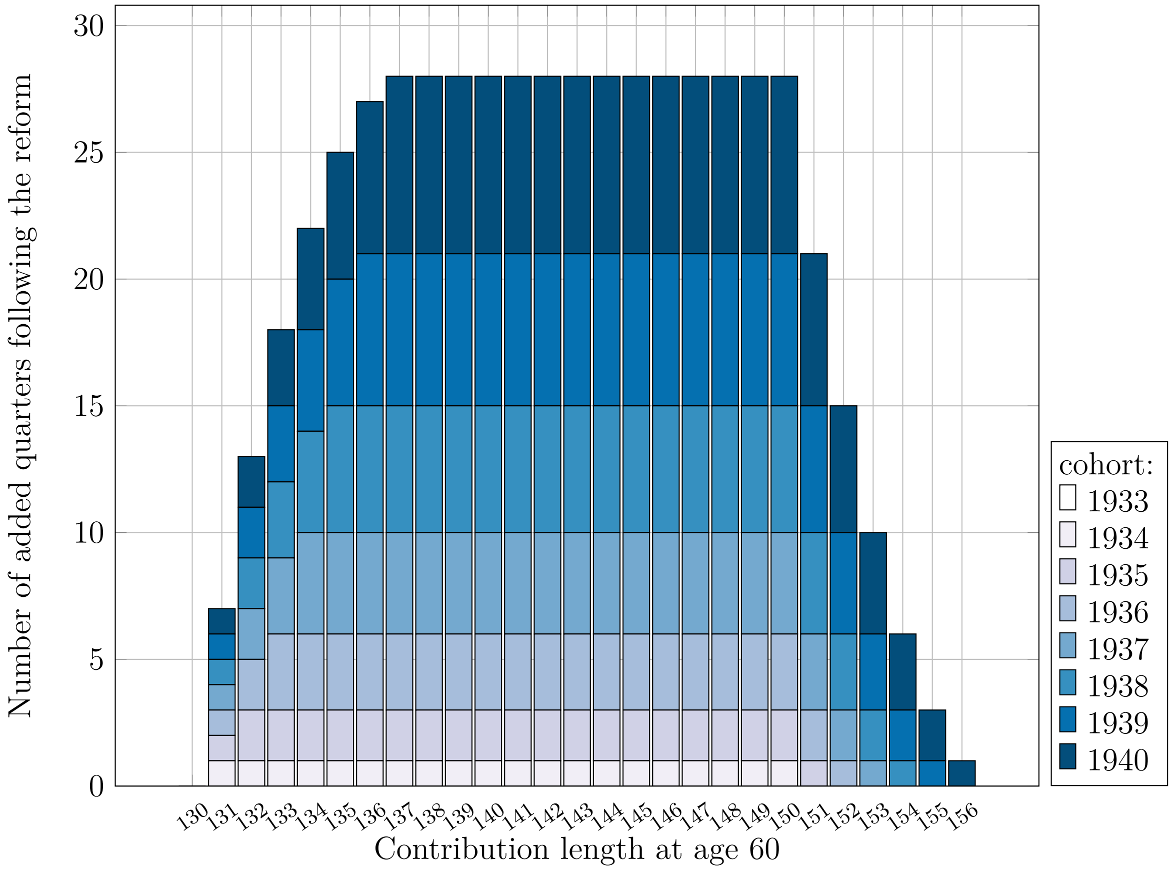
In the private sector, benefits depend on (i) the pension rate; (ii) the reference wage (equal to the mean of individuals' 10 best earnings years); (iii) the share of career an individual has done within the private sector scheme.

Early retirement age (ERA) is set at age 60, and a full-rate pension can be claimed either at age 65, or at an earlier age provided that the wage-earner has contributed more than the required contribution length – set at 37.5 years before the reform (or 150 quarters). There was, at the time, no actuarial adjustment of pension benefits after reaching the full replacement rate. The full replacement rate was 50%, and a penalty of 10% – higher than actuarial fairness – was applied for each year of early retirement or missing contribution length before conditions for the full-rate were reached. Hence, the financial incentives, as well as the reference norms, coincided largely with claiming a pension at the age of the full-rate.

1.2 The 1993 Pension Reform

In 1993, the Balladur government reformed the pension system for private sector employees (see Section A in the Appendix 1 for more details on the 1993 reform). This reform changed three parameters. First, it changed the indexation rules for pension, from wage growth to consumption price inflation. This change simultaneously affected all the cohorts in the same way, by reducing the level and dynamics of pensions. Second, the number of years considered for computing the reference earnings increased from the best 10 years to the best 25 best years. This change was phased-in progressively, affecting younger cohorts more intensively. Lastly, the reform changed the required contribution length for a full pension. It was gradually increased, cohort by cohort, from 37.5 years to 40 years (or from 150 to 160 quarters), starting with the 1934 cohort. As shown in Table A1, cohort 1934 had to contribute 151 quarters for a full pension, cohort 1935 had to contribute 152 quarters, and so on. All individuals in the same cohort were not affected in the same way, as shown in Figure 1. Using the change in the required contribution length, we exploit the variation between cohorts and within cohort to identify the causal effect of later retirement on mortality.

Figure 1: Impact of the 1993 Reform on Contribution Years Necessary to Get the Full-rate



Notes: This figure presents the impact of the 1993 reform on the number of quarters of contribution required to reach the full-rate by cohort and contribution length at the ERA, i.e., at age 60. Whatever the contribution length at age 60, a wage-earner born in 1933 is not impacted by the reform (zero added quarter required). Cohort born in 1934 who had contributed at age 60 between 130 and 150 quarters need to delay retirement by one quarter in order to qualify for the full-rate. Cohort born in 1935 who had contributed at age 60 between 131 and 150 quarters need to delay retirement by two quarters to reach the full replacement rate. Those from the same cohort who had contributed 151 quarters at age 60 had to delay retirement by one quarter.

Figure 2 shows that individuals in cohort non affected by the reform bunched at 150 quarters, the requirement for the full rate. From cohort 1934 (the first cohort affected by the reform), bunching at the full rate moves to the right for each cohort affected. It highlights significant behavioral responses to the 1993 reform.

Workers in a same cohort are differently affected by the change in required contribution length (but affected in the same way by the other part of the reform): individuals with very long career, having contributed at age 60 more than the required contribution length, were unaffected by the reform – they would qualify for the full rate at age 60 regardless of the reform. Conversely, individuals with short career, i.e., less than 130 quarters of contribution at age 60, were not affected by the change in required contribution length as the full-rate was obtained at age 65 anyhow.

Figure 1 illustrates the progressive increase in incentives to delay retirement across cohorts, and how this phasing-in of the reform impacted differently wage earners with different career length at age 60. Within each cohort only wage earners with a specific contribution length at age 60 were really impacted (those between 131 and 160 quarters of contribution) and the intensity of the reform was higher for younger cohorts.

2 Data and Empirical Strategy

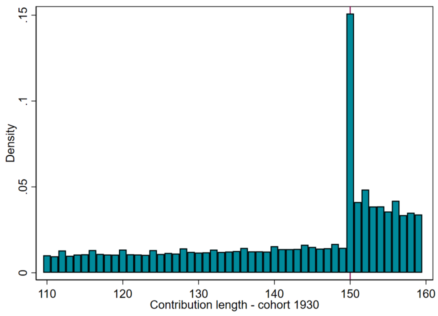
2.1 Data

In this study, we take advantage of the 1993 pension reform as an instrumental variable to estimate the causal impact of later retirement on mortality. We use exhaustive administrative data from the main pension scheme of the private sector, the *Caisse Nationale d'Assurance Vieillesse* (CNAV).[5] This data contains all the retirees born between 1930 and 1950 who have contributed at least one quarter in the Cnav pension scheme during their careers. We observe all retirees still alive, and all those who died between 2003 and 2017. These data are exhaustive for the cohorts we are interested in, with 500 000 observations per cohort on average.

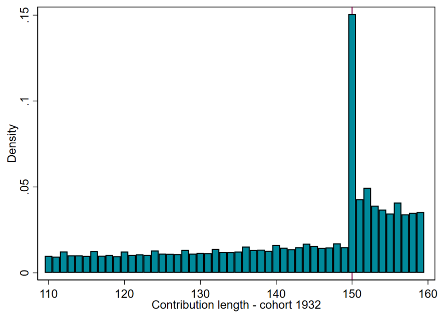
[5]The Cnav is the main pension scheme. It covers all the private sector wage earners. In France, 85% of the labor force contribute at least once in this pension scheme (source: EIR 2004). 90% of those affected by the 1993 reform had mainly contributed to the Cnav pension scheme.

Figure 2: Distribution of Claimants by Contribution Length.

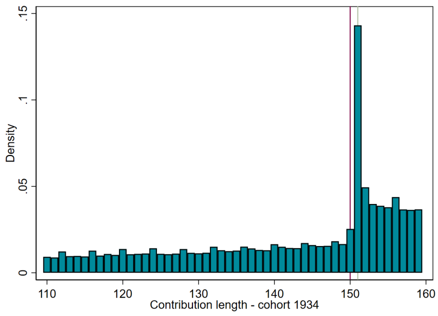
(a) Cohort 1930



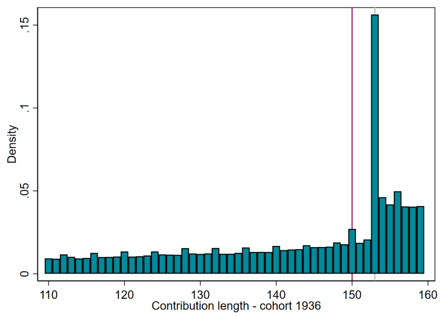
(b) Cohort 1932



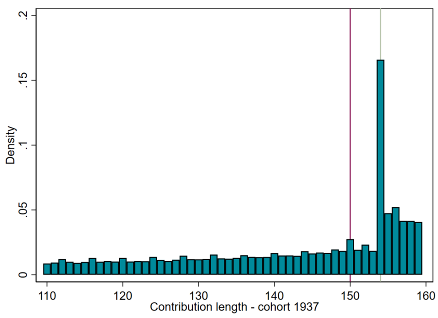
(c) Cohort 1934



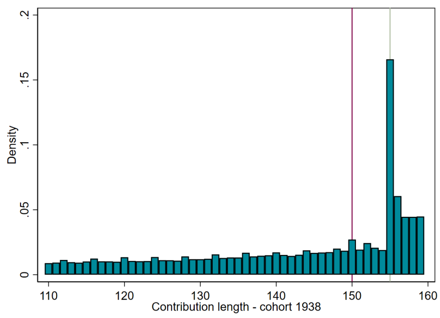
(d) Cohort 1936



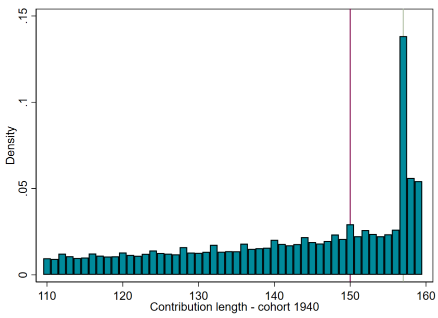
(e) Cohort 1937



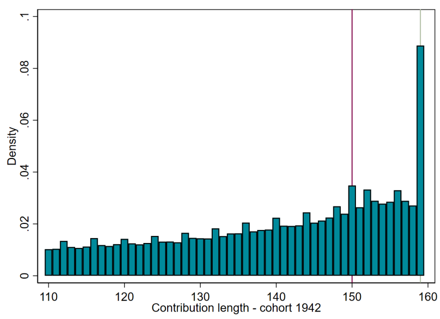
(f) Cohort 1938



(g) Cohort 1940



(h) Cohort 1942



Notes: This is the density by contribution length at retirement by cohort, for individuals who have contributed between 110 and 160 quarters. The red line shows the 150 quarters contribution (the required contribution before the reform). For cohorts 1930 and 1932 (cohorts not affected by the reform), there is bunching at 150 quarters, which corresponds to the required contribution length to retire with a full replacement rate. For cohorts 1934 and older (affected by the reform), bunching moves to the right, showing individuals seem to respond to the reform's changed incentives.
Sample: Individuals born between 1930 and 1942.
Source: Cnav 2017.

Sample Selection. The 1993 reform affects all individuals from cohort 1934 onwards. For our study, we select individuals born between 1933 and 1943. One cohort (born in 1933) is unaffected by the reform, while cohorts 1934 to 1943 are progressively more impacted by the change in incentives. Cohort 1943 is the first cohort fully impacted by the reform, and the last cohort not affected by the following French pension reform[6]. Thus, our sample is composed by individuals who (i) are born between 1933 and 1943; (ii) have contributed between 80 and 180 quarters at age 60.[7]

Given we observe mortality outcomes between 2004 and 2017 we do not observe mortality outcomes for the same ages for all the cohorts affected. As a result, we split our sample into two panels including individuals alive at the same age. In the first panel (Panel A), we observe the probability to die between 65 and 72 for individuals born between 1938 and 1943.[8] In the second panel (Panel B), we observe probability to die between 72 and 77 for individuals born between 1933 and 1938.

This enables us to have a global view of the impact of later retirement on mortality. As the effects on mortality could appear a long time after retirement, time is needed to observe the health consequences of later retirement. Panel A shows the impact in the short term (just after retirement), whereas Panel B gives us the effect in the long term, conditional on being alive at age 72.

Note that Panel A and B concern different cohorts, which might not be fully comparable. In particular, we may suspect that cohorts born during World War II (Panel A) could have specific health conditions.[9] As we use variations within cohorts to identify the impact of the pension reform, these differences should not threat the internal validity of the estimation.

Descriptive Statistics. Table 1 presents descriptive statistics of our main variable of interest for our two samples. The average number of quarters contributed is 156 quarters in Panel A, compared to 153 in Panel B. This difference between the two samples is expected

[6]The 2004 reform affects cohorts born in 1944 and later.

[7]As a robustness check, we change this restriction to individuals who contribute at age 60. Results are very close whatever the specifications (see Figure 6).

[8]The choice of age 65 rather that claiming age enables to avoid a selection bias on mortality between individuals with different claiming age.

[9]Stress due to the war, malnutrition during childhood, due to the Second World War II affect health during the whole life (Kesternich et al., 2014; Lindeboom et al., 2010; Van den Berg et al., 2006).

since individuals in Panel A are more intensively affected by the reform, and thus, have to contribute more to benefit from a full-rate pension. Consistently, the additional years of contribution required by the reform to obtain a full-rate is higher in Panel A than in Panel B (1.29 versus 0.41). Apart from the fact that the two sample are affected differently by the reform, they remain very close to national averages. For instance, mean claiming age in our data is 61.4 for Panel A (resp. 61.2 for Panel B), very close to the national mean claiming age of those who benefit from a pension (61.9 in 2004 according to Benallah and Mette (2009)). Reference earnings are also similar in our sample and in these national statistics. The death probability and mean age of death is higher in Panel B since we observe individual at older ages. In Panel A, individuals are observed between ages 65 to 72; and in Panel B, between ages 72 and 77. The death probability and the average age at death is different in these two panels. However, it is two different populations observed at different age range.

We have also conducted a comparison of our sample characteristics to the national statistics from INSEE, the French institute of national statistics, whose detailed results are presented in Appendix. A number of differences needs to be noted, as they reflect the selection of our sample on private sector workers. First, the share of women is slightly lower than those of men (Table B2). Second, the death probability between ages 65 and 77 is different from the national statistics for each cohort we are interested in (see Tables B3 and B4).

2.2 Empirical Strategy

The main challenge to measure the impact of later retirement on health is reverse causality.[10] Less healthy people may be inclined to leave employment at earlier age, whereas healthier people tend to stay on the labour market, which would create a positive correlation between retirement age and health status. Health has a strong effect on work choices. Previous studies show that health problems influence retirement plans, more generally the labor force behavior of older workers (Bound et al., 1999; Dwyer and Mitchell, 1999; Au et al., 2005; McGarry, 2004; Disney et al., 2006).

[10]Health and retirement are endogenously related (Kerkhofs et al., 1999; Llena-Nozal Ana et al., 2004; Lindeboom and Kerkhofs, 2009).

Table 1: Descriptive Statistics of the Variable of Interest

Variable	Mean	Std. Dev.	Min.	Max.	N
Panel A – Cohort 1938 to 1943					
Contribution length (in quarters)	155.69	22.27	80	206	2,198,258
Contribution length at age 60	150.38	25.70	80	180	2,198,258
Claiming age	61.41	2.03	60	66.5	2,198,258
Reference earnings (in euros)	14,704.78	7,246.37	0	1,816,800	2,198,258
ΔRCL	1.29	2.66	0	10	2,198,258
Disability pension	0.18	0.62	0	1	2,198,258
Age of death	71.87	3.69	65	79.92	393,049
Death probability between 65 and 72 yo.	0.0899	.	0	1	2,198,258
Panel B – Cohort 1933 to 1938					
Contribution length (in quarters)	152.94	23.27	80	206	1,900,893
Contribution length at age 60	148.31	26.489	80	180	1,900,893
Claiming age	61.24	1.913	60	67	1,900,893
Reference earnings (in euros)	13,695.08	6,763.97	0	1,989,700	1,900,893
ΔRCL	0.41	1.12	0	5	1,900,893
Disability pension	0.18	0.62	0	1	1,900,893
Age of death	77.67	3.18	72	84.92	478,666
Death probability between 72 and 75 yo.	0.1091	.	0	1	1,900,893

Notes: This table shows descriptive statistics of our samples. Individuals selected are those who had contributed at age 60 between 80 and 180 quarters, at least once in the private sector, and retire between ages 59 and 67. Moreover, Panel A selects only individuals born between 1938 and 1943, and alive at retirement; Panel B selects only individuals born between 1933 and 1938, alive at age 72. In Panel A (resp. panel B), the mean contribution length is 155.69 quarters (resp. 152.936).
Source: Cnav 2003-2017.

To address this endogeneity issue, we exploit the exogenous variation in retirement age created by the 1993 reform, as an instrument for assessing the causal impact on mortality. The 1993 reform affected differently individuals of the same cohort depending on the exact number of quarters of contribution at the ERA. For example, the reform consisted in an incentive to retire one quarter later for individuals born in 1934 and who had contributed 150 quarters at age 60. With 151 quarter of contributions, individuals of the same cohort were not affected by the reform. Individuals born in 1933 with either 150 or 151 quarters of contributions were not affected in neither cases. Within cohorts 1933 and 1934, we could estimate the impact of the reform in a difference in differences setting, following equation (1):

$$A_i = \delta_0 + \delta_1 \mathbb{1}_{\{yob_i=1934\}} \times \mathbb{1}_{\{CL_{60_i}=150\}} + \delta_2 \mathbb{1}_{\{yob_i=1934\}} + \delta_3 \mathbb{1}_{\{CL_{60_i}=150\}} + \varepsilon_i \qquad (1)$$

with A_i (claiming age, in quarter of years), $\mathbb{1}_{\{yob_i=1934\}}$ a dummy equal one if individual i is born in 1934, $\mathbb{1}_{\{CL_{60_i}=150\}}$ a dummy variable equal one if contribution length of individual i equal 150 at age 60, ε_i the error term. The interaction term $\mathbb{1}_{\{yob_i=1934\}} \times \mathbb{1}_{\{CL_{60_i}=150\}}$ captures the causal impact of the reform on retirement age within cohort.

With the progressive phasing-in of the reform we can exploit all the different impacts of the reforms on different cohorts, in the spirit of a generalised difference in differences model (with cohorts and quarters of contributions dummies). We go one step further by exploiting the intensity of the reform, by computing the number of quarters of contribution needed to reach the full-rate ΔRCL, which captures the full impact of the reform taking into account cohorts.

The first-stage in our two stage least square estimation represents the impact of being affected by the reform on the claiming age, and can be written as follows:

$$A_i = \alpha_0 + \alpha_1 \Delta RCL_i + \sum_g \alpha_{2,g} \mathbb{1}_{\{yob_i=g\}} + \sum_t \alpha_{3,t} \mathbb{1}_{\{CL_{60_i}=t\}} + \alpha_4 X_i + \zeta_i \tag{2}$$

with A_i, the claiming age; ΔRCL_i, the additional quarters required to get a full pension due to the reform[11]; $\mathbb{1}_{\{yob_i=g\}}$, dummies for cohort; $\mathbb{1}_{\{CL_{60_i}=t\}}$, dummies for the contribution length at age 60; X_i, the pensioners' individual characteristics (gender, annual reference wage and a dummy for being invalidity pension recipient); ζ_i, the error term.

The second-stage equation is the causal impact of later retirement due to the reform on mortality between 65 and age 72 (Panel A), and between ages 72 and 77 (Panel B). It can be written as follows:

$$q_i = \beta_0 + \beta_1 \hat{A}_i + \sum_g \beta_{2,g} \mathbb{1}_{\{yob_i=g\}} + \sum_t \beta_{3,t} \mathbb{1}_{\{CL_{60_i}=t\}} + \beta_4 X_i + \tau_i \tag{3}$$

with q_i equal to zero if individual i is alive at age 72 (respectively at age 77), and equal to one if individual i died between ages 65 and 72 (respectively between ages 72 and 77), $\hat{A}_i$, the variation in claiming age due to the reform, and τ_i, the error term.[12] Notice that this method allows to have the average impact within cohorts.

[11] Thus, ΔRCL_i varies according to birth year, and contribution length at age 60.

[12] We assume that the number of contributed quarters at age 60 is independent from the reform. We check this assumption by testing the impact of the reform on quarters of contribution at ERA.

3 Results

We first present reduced-form results with graphical evidence, before detailing the 2SLS results for each panel.

3.1 Impact of the Reform on Claiming Age and Mortality

Impact of the Reform on Retirement. Figure 3 presents the impact of the 1993 reform on claiming age for different cohorts and according to the contribution length at age 60. Figure 3b compares two affected cohorts (1936 and 1938) with an unaffected cohort (1933). Cohort 1936 had to delay retirement by three quarters to get the full rate if contribution length was below 151, while cohort 1938 had to report retirement by five quarters if contribution length was below 155 quarters. We observe strong effects of the reform on claiming age for those individuals affected. The increasing intensity of the reform is also evident in the stronger impact for the younger cohorts. For contribution length above 155 quarters at 60 no cohort is affected and we do not detect any difference in claiming behavior. Figure 3a presents similar effects for younger cohorts (1940 and 1942) compared with cohort 1938. Figure 3c presents the results for three unaffected cohorts (1931 and 1932 versus 1933). No difference in claiming age is detected.

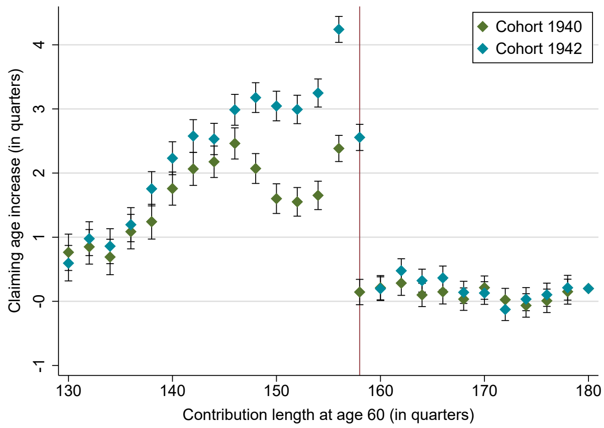
Figure 4 presents the graphical results of the first stage estimate of our main specification, i.e., the impact on claiming age of the variable ΔRCL capturing the intensity of the reform. The impact is strong, proportional to the intensity of the treatment.

These graphical results are confirmed by the OLS regression (see Table 2). It shows a large impact of an increase in the required contribution length on claiming age. An increase in the contribution length by one quarter implies a 0.696 (resp. 0.672) additional quarter in claiming age for men of Panel A (resp. Panel B), and 0.589 (resp. 0.425) for women, both significant at 1%. This result confirms that the 1993 reform can be used as an instrumental variable to estimate the causal impact of claiming age on mortality.

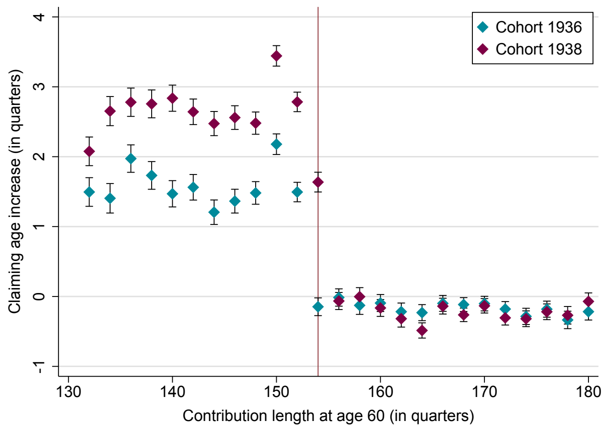
Individuals postpone almost the entire additional required contributions to obtain a full pension, meaning that they respond to the incentives to work longer. An increase of the required contribution length by one quarter (three months) induces a deferral of 2 months (resp. 1.68 for Panel B) in the claiming age for younger cohorts (resp. older

Figure 3: Impact of the 1993 Reform on Claiming Age

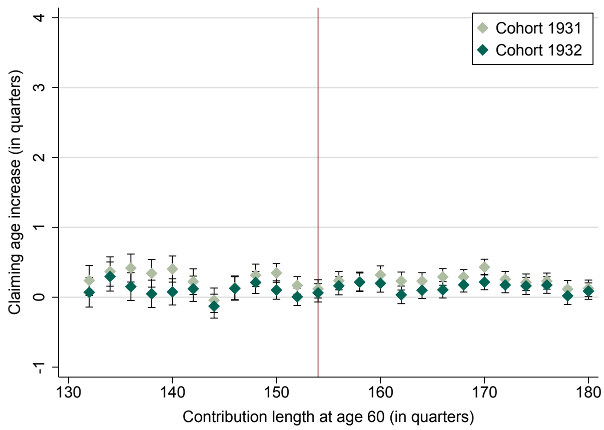
(a) Treated Cohorts (1940 and 1942 vs 1938)



(b) Treated Cohorts (1938 and 1936) vs Controls (1933)



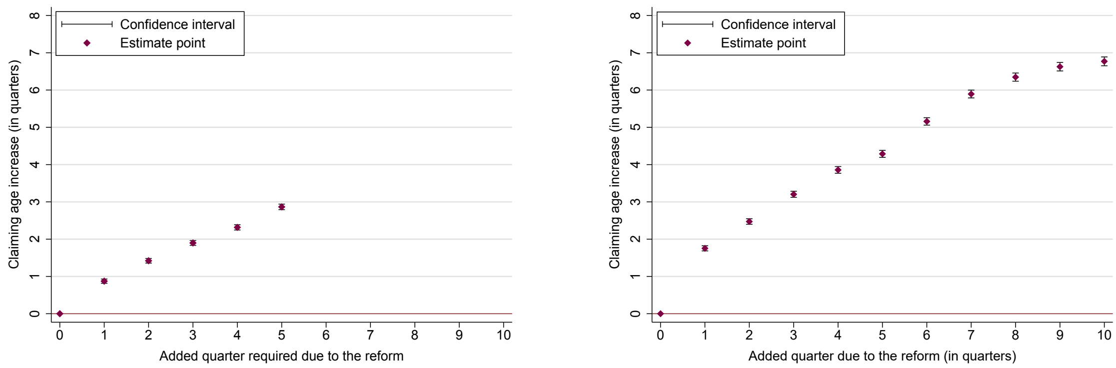
(c) Placebo Test: Cohorts 1931 and 1932 vs 1933



Lecture: Average impact of the contribution length at age 60 on the claiming age for 1940 and 1942 cohorts, taking 1938 cohort as reference, for treated cohorts (1938 and 1936), taking 1933 cohort as reference and for untreated cohorts (1931 and 1932), taking cohort 1933 (untreated) as reference. Confidence Intervals at 95%.
Sample: Individuals from Panel A and B.
Source: Cnav 2017.

Figure 4: Impact of the Reform on Claiming Age

(a) Cohort 1933 to 1938 (b) Cohort 1938 to 1943



Lecture: Average impact of the number of added quarter an individual experience due to the reform on the claiming for cohorts 1933 to 1938, and for cohorts 1938 to 1943. Confidence Intervals at 95%.
Sample: Individuals from Panel A and B.
Source: Cnav 2017.

cohorts). The effect is slightly lower for women, who postpone the claiming age by close to two months.

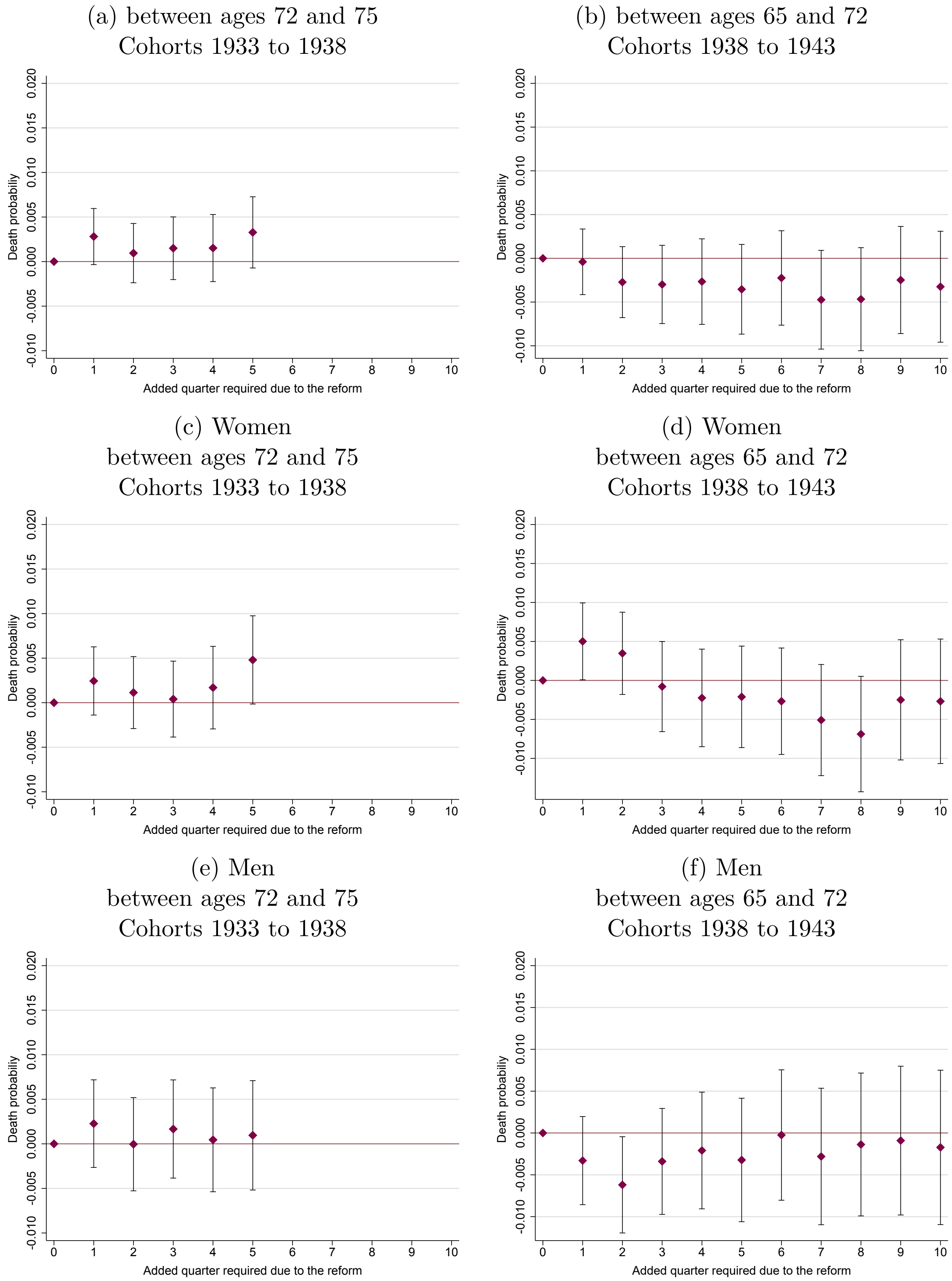
Impact of the Reform on Mortality. We show in Figure 5 similar graphical evidence with mortality outcome instead of claiming age. This is akin to the reduced-form estimation on mortality. The effects are never significant, whatever the cohort, gender, or the treatment intensity.

3.2 Impact of Claiming Age on Mortality – IV Estimates

Table 2 presents the main results of the analysis for the two samples (Panels A and B). In column (1) we report the coefficient of an OLS regression of claiming age on mortality. The correlation is negative and significant for all samples: -0.00099 for men born between 1938 and 1943 (resp. -0.00094 for those born between 1933 and 1938) and -0.00042 (resp. -0.00039) for women, meaning that a higher claiming age is associated with a lower probability to die. The correlation may be explained by a selection bias as workers in good health are likely to be those who retire later ("healthy worker effect").

In column (2) we report the coefficients of the impact of the pension reform on mortality (the reduced form estimation of equation (3)). The negative correlation turns

Figure 5: Impact on Mortality by Treatment Intensity



Lecture: Average impact of the number of added quarter an individual experience due to the reform on the probability to die, respectively between ages 72 and 75 for cohorts 1933 to 1938, and between ages 65 and 72 for cohorts 1938 to 1943. Confidence Intervals at 95%.
Sample: Individuals from Panel A and B.
Source: Cnav 2017.

Table 2: Main Estimates of the Impact of Delaying Retirement on Mortality.

	(1) OLS	(2) *Reduced Form*	(3) *1st stage*	(4) 2SLS	Obs.
Panel A: Cohorts 1938 to 1943, observed between ages 65 and 72					
All	-0.00049***	-0.00023	0.64607***	-0.00035	2,198,258
	(0.00003)	(0.00028)	(0.00603)	(0.00044)	
	0.00000	0.42299	0.00000	0.42293	
Male	-0.00099***	0.00004	0.69616***	0.00006	1,283,687
	(0.00005)	(0.00042)	(0.00788)	(0.00060)	
	0.00000	0.91704	0.00000	0.91703	
Female	-0.00042***	-0.00043	0.58855***	-0.00073	914,571
	(0.00004)	(0.00035)	(0.00941)	(0.00060)	
	0.00000	0.22495	0.00000	0.22486	
Panel B: Cohorts 1933 to 1938, observed between ages 72 and 77					
All	-0.00045***	0.00038	0.56020***	0.00068	1,900,893
	(0.00004)	(0.00035)	(0.00684)	(0.00062)	
	0.00000	0.27354	0.00000	0.27362	
Male	-0.00094***	0.00007	0.67153***	0.00011	1,081,343
	(0.00007)	(0.00054)	(0.00941)	(0.00081)	
	0.00000	0.89240	0.00000	0.89240	
Female	-0.00039***	0.00055	0.42517***	0.00128	819,550
	(0.00004)	(0.00043)	(0.01013)	(0.00101)	
	0.00000	0.20540	0.00000	0.20561	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Lecture: Column (1) presents the coefficients from an OLS regression of claiming age on mortality; column (2) the coefficient of the reduced form impact of the reform on mortality; column (3) the first stage impact, i.e. the impact of the reform on claiming age; and finally column (4) presents the coefficients of the 2SLS estimation. F-test are in italics for 1st stage.
Notes: Results are for samples of individuals who had contributed at age 60 between 80 and 180 quarters; retired between ages 59 and 67; contribute at least once during their career in the private sector and for Panel A who are born between 1938 and 1943, and alive at age 65; for Panel B, who are born between 1933 and 1938 and alive at age 72. The F-statistics of the first stage is systematically high enough to not worry about weak instrument issue. Thus, it is 11,477.23 for the whole panel A and 7,798.24 (resp. 3,915.25) for men (resp. women). For panel B, it is 6,705.05 and 5096.00 (resp. 1761.31) for men (resp. women).
Source: Cnav data 2017.

insignificant for Panel A and Panel B. In column (3) we report the first stage impact (i.e., the impact of the reform on claiming age) which exhibits strong and significant effects, while column (4) reports the 2SLS estimates.

The results from the IV estimation show that an exogenous increase in claiming age has no significant impact neither on the probability to die between ages 65 and 72 (Panel A) nor on the probability to die between ages 72 and 77 (Panel B). This non-significant effect is very close to zero. This result is also not significant for men and women separately. In each sub-sample, we would like to know if the non significant result can be interpreted as an absence of link between retirement and death or a lack of power. In that aim, we compute minimum detectable effect.

Detecting Small Effects with Rare Events Data. The minimum detectable analysis gives us the lowest detectable effect. Thus, a minimum detectable effect of x means that with an estimated coefficient non significant and lower than x, we could not conclude on the absence of association between the dependant variable and the treatment variable, i.e., we accept the null hypothesis with a risk of making a type II error higher than 20%, the usual threshold of statistical power (see Appendix C). We compute MDE estimates for each sample, for a two-side hypothesis test, at a 5% significance level, and a statistical power of 20%. There is not enough power to detect an effect when the MDE is above the confidence interval of the estimated beta. Panel A includes 2,198,258 observations, with a share of treated of 23.20%, a death probability of 8.99%. The minimum detectable effect is -0.00049, which is higher in absolute value than our estimated β (-0.00035). It means that if there were an effect lower than -0.00049, it could have been detected. Panel B includes 1,900,893 observations, with a share of treated of 15.14%, a death probability of 10.91%. The minimum detectable effect is 0.0017 in Panel B, which is higher than our estimated β (0.00068). It means that an impact higher than 0.0017 could have been detected if it had occur, or in other words that, if there is an effect on mortality, it is lower than 0.0017 in Panel B and lower in absolute value than -0.00035 in Panel A (see Table C2 in Appendix).

3.3 Robustness Checks

Sample Selection. We test several alternatives to our sample restrictions. Figure 6 shows the causal effect of later retirement on mortality for each panel depending on the sample selection. Our main specification is a selection of individuals who contribute between 80 and 180 quarters at age 60. Figure 6a, 6e and 6c (Panel A) shows the causal effect of later retirement on the probability to die between ages 65 and 72 considering various sample selection on contribution length. This effect is non-significant whatever the sample selection. When considering the effect on the probability to die between 72 and 77 (see Figure 6b,6f, 6d), the effect is positive, but non-significant whatever the specification. This shows that not affected individuals with short or long careers do not drive the results.

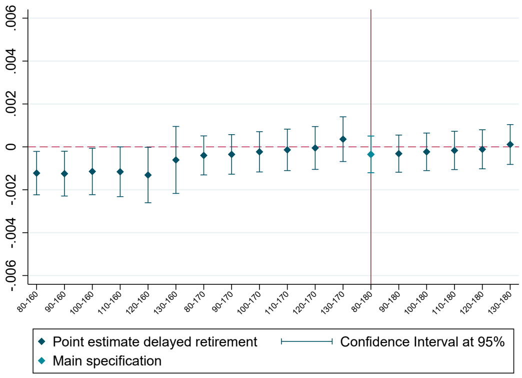
Additional Controls. Our data does not provide many information concerning individuals socio-economic characteristics. We use *Echantillon interrégime des retraités* (EIR) data, an administrative dataset of retirees born in early October of even years (details in Appendix D). This data is smaller than the CNAV data, but allows for controlling for individual characteristics such as having children, marital status, and socio-professional characteristics. We rerun our model on this data, without control, as in Cnav data, and with control for marital status, profession and children (see Table D1). With and without control, our results are very similar, showing that adding controls does not change the results.

Mortality Measures. We check alternative mortality definitions, e.g., death between ages 65 and 66, 66 and 67, 67 and 68, and so on (see results in Appendix, Table D2). An exogenous increase of claiming age by one quarter has a non significant impact at the conventional 5% threshold. This may be explained by the very low death rate within a year, which is statistically hard to capture.

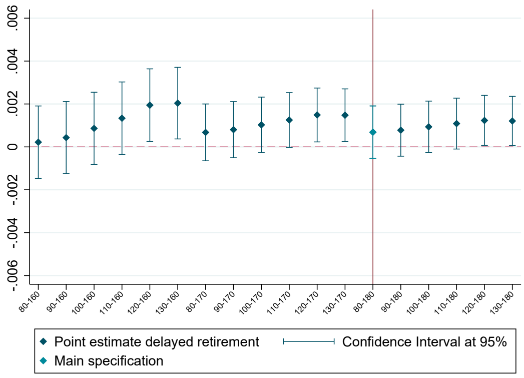
Heterogeneous Treatment. Our main model assumes linear impact of the reform on claiming age. As a robustness check, we allow for heterogeneous impact of the reform. Results are presented in Appendix, Table D3. An increase by one quarter of the required contribution length leads to a significant increase of claiming age by 1.840 quarters in Panel A (resp. 0.873 in Panel B); an increase by two quarters leads to an increase of claiming age by 2.596 quarters in Panel A (resp. 1.420 in Panel B). Table D4 shows there is no significant impact on mortality when we allow heterogeneous effects.

Figure 6: Robustness Checks for Sample Selection

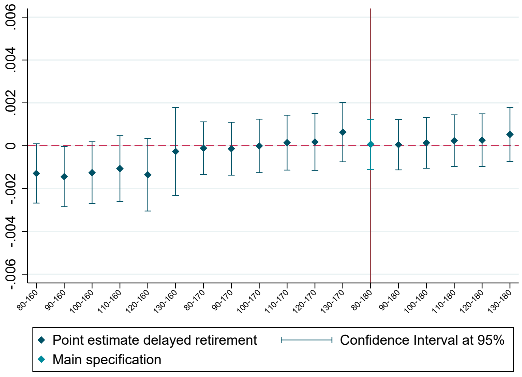
(a) Effect on the Probability to Die between 65 and 72 (Panel A)



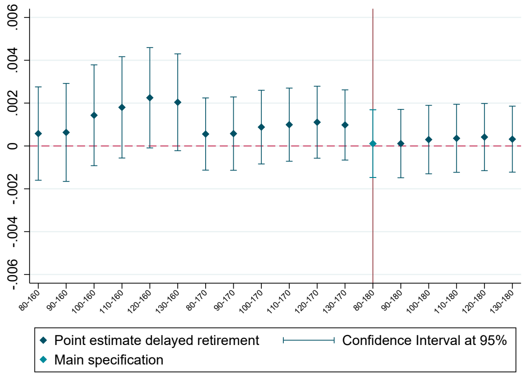
(b) Effect on the Probability to Die between 72 and 77 (Panel B)



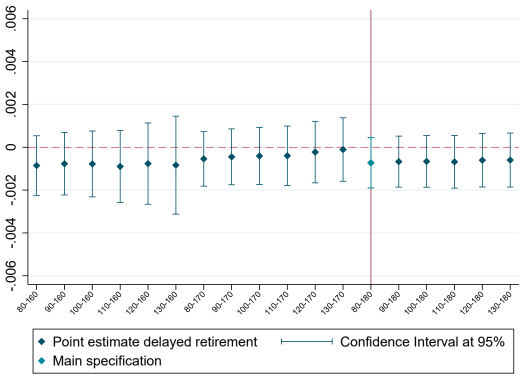
(c) Effect on the Probability to Die between 65 and 72 (Men Panel A)



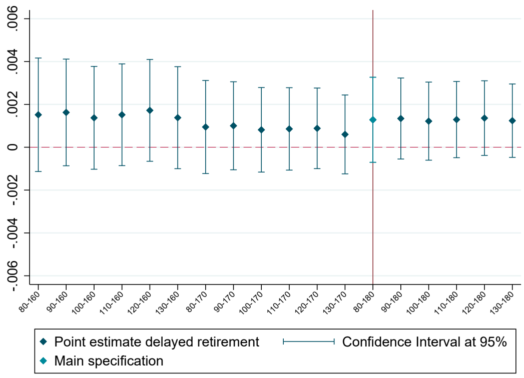
(d) Effect on the Probability to Die between 72 and 77 (Men Panel B)



(e) Effect on the Probability to Die between 65 and 72 (Women Panel A)



(f) Effect on the Probability to Die between 72 and 77 (Women Panel B)



Lecture: We test several alternatives to our sample restrictions. Our main specification is a selection of individuals who contribute between 80 and 180 quarters at age 60.
Source: Cnav 2017.

4 Discussion

We have found that an exogenous increase in the retirement age in France led to no impact on mortality of individuals affected. In order to interpret the implications of such results, two issues need to be discussed: i) in what respect the French reform carries information for other reforms, i.e., assessing the external validity of the study, and ii) what is the economic significance of the results.

External Validity. All the studies exploiting exogenous change of retirement age to assess its causal impact on health outcomes have the drawbacks to be local results, for which generalisation to other settings is problematic. Our study faces similar limits. First, the reform does not affect individuals with very long or very short careers, which means that our results concern only a subset of individuals with average career length. Individuals with such careers have particular socio-economic characteristics, which can be endogenous with health status. In particular, it is possible that detrimental impact of retirement on mortality could be found for individuals with very long careers. Second, this reform does not affect individuals eligible for disability pension. Individuals in poor health affected by the reform have been able to retire with disability pensions, and thus to retire without postponing their claiming age.[13]

On the other hand, our study show the impact of increased retirement age for the population effectively affected by the reform, which carries out implications for a large part of the wage earners with average career length, representing a large share of the population.

Meta-analysis of the Literature. In order to better assess the external validity of our results, we carry out a meta-analysis of previously published studies. We compare our results to those obtained in the literature on the long-term effects of later retirement on mortality. We also compute the MDE estimates to assess whether each study had the statistical power to estimate the possible impact.

The effect of postponing retirement is not necessarily symmetric to the impact of

[13] Bozio (2011) shows there is a very small share of the affected that ask for a disability pension due to the reform.

early retirement, and most of the studies focus on the causal impact of early retirement (Coe and Lindeboom, 2008; Hernaes et al., 2013; Hallberg et al., 2015; Bloemen et al., 2017; Kuhn et al., 2018). We therefore split the sample by making separate comparisons between studies exploiting increase or decrease in retirement ages.

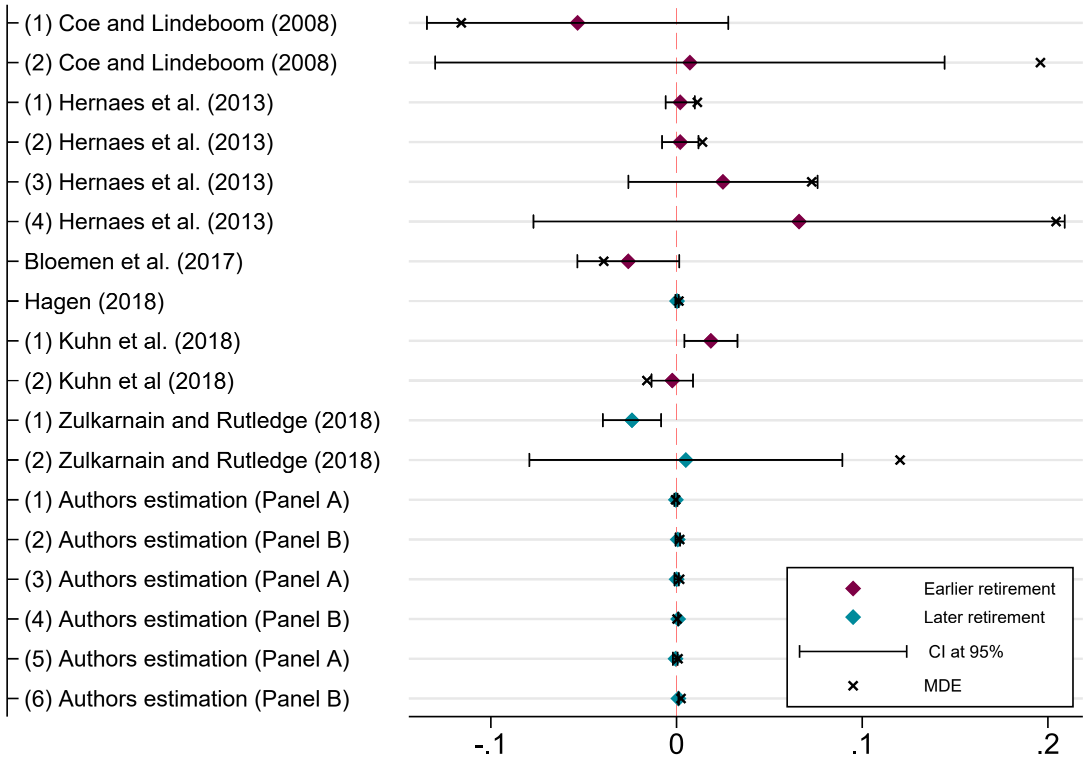
Figure 7 shows our point estimates and confidence intervals at 95% and those obtained in the previous studies. It relies on estimates of papers presented in detail in Table 3.

Two results stand out from this meta-analysis. First, few studies have enough statistical power to conclusively estimate impact on mortality of retirement age changes. Apart from our study, only Hernaes et al. (2013), Hagen (2018) and Kuhn et al. (2018) have enough precision to draw inference on the likely impact. In those three cases, estimated impact are very close to zero. Second, even if one takes seriously the point estimates of all these studies, the average impacts remain very small: for all studies together, the average estimate is slightly positive, around 0.0011, for studies focusing on later retirement the impact is slightly negative around -0,0020. And if one compare our results with the most precisely estimated effects of reforms delaying retirement, we find very similar results: the baseline estimates of Hagen (2018) is a non-significant positive point estimate of 0.00028 compared to our estimates of -0.00035 and 0.00068 in our two samples.

Economic Significance. We have found impact estimates that are both non significant, of opposite sign, and relatively small in magnitude, even for MDE estimates. What is important to discuss is the economic significance of such results.

The minimum detectable effect is small in magnitude: a one quarter increase in claiming age, if having an impact on death probability, is lower than a probability to die by 0.0017 (Panel B). It is equivalent to an increase of the probability to die between 72 and 77 by 1.56%. This variation is lower than the variation of the death probability between ages 72 and 77 between cohorts 1932 and 1933 (1.59%); lower than the variation of the death rate at age 74 between cohorts 1933 and 1938 (13.24%). It means that if an effect on mortality occurs due to the reform that affect the young cohort, who benefit from a higher life expectancy, this impact is lower than the mortality gain due to their cohort. Another comparison point could be with the impact of education on mortality. The variation we find is lower than the 3.6% decrease in 10 years mortality due to an additional

Figure 7: Meta-analysis of the Literature



Note: This Figure presents a meta-analysis of the literature regarding the causal effect of later vs earlier retirement on mortality. For each row, we show point estimates, confidence intervals at 95% and MDE for non-significant effects.

Lecture: Coe and Lindeboom (2008) measure the impact of early retirement on the probability to die within 4 years – see row (1), within 6 years – see row (2). See Table 3 for details on each point-estimate. The 6 last lines show our point estimates and confidence intervals at 95% for each panel, for men and women. Rows (1) and (2) show the estimation on the all sample for Panel A and resp. B, rows (3) and (4) for men (resp. Panels A and B), rows (5) and (6) for women, resp. Panels A and B.

year of education, according to Lleras-Muney (2005).

In light of these comparisons, our estimates suggest that increasing retirement age, around the age of 60, and for a population excluding those with very long or very short career length, has no detrimental impact on mortality.

Table 3: Literature Review on the Impact of Retirement on Mortality

Authors (year)	**Country Population**	**Pension rules or reform**	**Method**	**Outcome**	**Point estimates**
Coe and Lindeboom (2008)	USA HRS, blue- and white-collar workers, men	Age specific retirement incentives of the US Social Security system	IV	Mortality (1) within 4 years : (2) within 6 years :	 -0.0533 (0.0414) Table 9 (column 2) 0.0072 (0.07) Table 9 (column 4)
Hernaes et al. (2013)	Norway register data	Introduction of early retirement scheme	DD and IV	Mortality (1) by age 67 (2) by age 70 (3) by age 74 (4) by age 77	 0.002 (0.004) Table 4 (column 2) 0.002 (0.005) Table 4 (column 4) 0.025 (0.026) Table 4 (column 6) 0.066 (0.073) Table 4 (column 8)
Hallberg et al. (2015)	Sweden Military	Introduction of early retirement scheme	DD	Causes of death ages 56–70	Early retirement offer reduces mortality
Bloemen et al. (2017)	The Netherlands civil servant, men	Early retirement reform	IV	Mortality within 5 years	 -0.026 (0.014) Table 2 (column 3)
Hagen (2018)	Sweden local government workers, women	Reform increasing incentives to retire later	DD	Mortality by age 69	 0.000283 (0.000454) Table 9 (column 7)
Kuhn et al. (2018)	Austria blue-collar	Extension of early retirement scheme	DD and IV	Mortality (1) by age 73 for men (2) by age 73 for women	 0.0185 (0.0073) Table 3 (column 1, IV) -0.0023 (0.0057) Table 3 (column 4, IV)
Zulkarnain and Rutledge (2018)	The Netherlands people born btw 1943 and 1954	Reform that induced delayed retirement	IV	5-year mortality rate (1) for men aged 62-65 (2) for women aged 62-65	 -0.024 (0.008) Tables 4b and 4c (resp. columns 2 and 1) 0.005 (0.043) Table 4b (column 5)

Notes: We report the point-estimates for studies measuring the causal impact of later or earlier retirement on mortality. IV: instrumental variables. RDD: regression discontinuity design DD: difference-in-differences.

Conclusion

This paper investigates the impact of delaying retirement on mortality among the French population. We take advantage of the 1993 pension reform in the private sector to identify the causal effect of an increase in claiming age on mortality. We use administrative data which provide detailed information on career characteristics, dates of birth and death. Our results show that an exogenous increase of the claiming age has no significant impact on the probability to die.

This effect is precisely estimated, contrary to a large part of the literature which is not able to distinguish power issues from really small effects. On a more methodological note, we suggest that using minimal detectable effect procedure more systematically could be a way to identify the ability to estimate small effects with rare event data.

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Online Appendix

The following Appendix is in four parts. The first contains details on the institutional framework concerning the French pension system and the 1993 reform. The second contains details on the sample. The third contains details about the minimum detectable effect methodology. The last contains robustness checks, based on the construction of alternatives specifications.

A The Institutional Framework

In France, the pension system is a mandatory pay-as-you-go system. Pension amounts depend on the time workers contribute to this system and their best-earning years.

Replacement Rate The full replacement rate is 0.5. If neither the required contribution length (D) nor the required age (i.e. 65) is reached, the replacement rate decreases by a δ factor for each missing quarter. Therefore, the replacement rate is computed as follows:

$$\tau = 0.5 - \delta \times \max[0, \min(4 \times (65 - a), D - d)] \quad (4)$$

where δ is the minimization coefficient, equals 1.25% per missing contributions quarter, a is the claiming age, d the number of contribution quarters and D, the needed quarters required for a full pension. Before 1993, parameter D was equal to 150 quarters (i.e. 37.5 years) and the pension amount paid was proportional to the average wages of the ten best-earning years. In 1993, the government led by Prime Minister E. Balladur chose to reform the pension system. Following the 1993 reform, D goes gradually from 150 to 160 depending on the cohort. This reform concerned only the private sector. The rules didn't change in other sectors.

The Political Context of the 1993 Pension Reform The 1993 reform was the first one of the French pension system which aims at increasing the claiming age. Individuals were not expected this. The reform was adopted 22nd July 1993 during the summer holiday. The decree was published one month after the vote, i.e. 28th of August. The application was scheduled for the 1st January 1994. As there has not been any communication beforehand, individuals could absolutely not anticipate the reform and the ensuing consequences.

The Details of the 1993 Pension Reform

First, following the 1993 reform, the number of years of contributions required for a full pension was gradually raised from 37.5 to 40 years, cohort by cohort, starting with the 1934 generation. The number of contribution quarters required for a full pension increased by one quarter per year: 151 for the 1934 generation (in 1994) and so on, through to 160 for the 1943 generation (in 2003). Second, the reform raised gradually the number of years required for the pension amount calculation for each generation from 10 to 20 years. This last parameter does not vary within cohort. Third, the reference wage was indexed on prices starting from 1993, but this last measure does not vary by cohort. We exploit the variation between cohorts and within cohort to identify the causal effect of later retirement on mortality, thus we focus only on the first measure (see Table A2).

Table A1: Progressive Increase in Required Contribution Length in Private Sector, due to the 1993 Reform.

Birth year	**Nb of contr. quarters** ***(to get a full pension)***
1933 and before	150
1934	151
1935	152
1936	153
...	...
1942	159
1943 and after	160

Lecture: Individuals born in 1933 or before, have to contribute 150 quarters to benefit for a full replacement rate, those born in 1934 have to contribute 151 quarters, and so on.

Table A1 presents the progressive increase in required contribution duration (D) following the reform, starting from 1934 cohort. Individuals born in 1933 or before, have to contribute 150 quarters to benefit from a full replacement rate. Cohort 1934 have to contribute 151 quarters to get a full pension, cohort 1935 have to contribute 152 quarters and so on, and so forth. Table A2 shows the number of additional quarters individuals have to contribute to get a full pension of each even cohort, depending on their contribution duration at 60. Individuals born in 1934 and who contribute between 131 and 150 quarters at age 60 are required to contribute one additional quarter following the reform ($\Delta RCL = 1$). Individuals born in 1936 and who contribute 131 or 152 quarters at age 60 are also required to contribute one additional quarter following the reform.

Table A2: Variation of Required Contribution Length due to the Reform

Variation of required contribution length ΔRCL	**Cohort**	**Contribution length at age 60**
0	1930	All
	1932	All
	1934	$\in [0; 130] \cup [151; +\infty[$
	1936	$\in [0; 130] \cup [153; +\infty[$
	1938	$\in [0; 130] \cup [155; +\infty[$
	1940	$\in [0; 130] \cup [157; +\infty[$
	1942	$\in [0; 130] \cup [159; +\infty[$
1	1934	$\in [131; 151[$
	1936	$\in (\{131\}; \{152\})$
	1938	$\in (\{131\}; \{154\})$
	1940	$\in (\{131\}; \{156\})$
	1942	$\in (\{131\}; \{158\})$
2	1936	$\in (\{132\}; \{151\})$
	1938	$\in (\{132\}; \{153\})$
	1940	$\in (\{132\}; \{155\})$
	1942	$\in (\{132\}; \{157\})$
3	1936	$\in [133; 151[$
	1938	$\in (\{133\}; \{152\})$
	1940	$\in (\{133\}; \{154\})$
	1942	$\in (\{133\}; \{156\})$
4	1938	$\in [134; 151[$
	1940	$\in (\{134\}; \{153\})$
	1942	$\in (\{134\}; \{155\})$
5	1938	$\in [135; 151[$
	1940	$\in (\{135\}; \{152\})$
	1942	$\in (\{135\}; \{154\})$
6	1940	$\in (\{136\}; \{151\})$
	1942	$\in (\{136\}; \{153\})$
7	1940	$\in [137; 151[$
	1942	$\in (\{137\}; \{152\})$
8	1942	$\in (\{138\}; \{151\})$
9	1942	$\in [139; 151[$

Lecture: Individuals born in 1934 and who contribute between 131 and 150 quarters at age 60 are required to contribute one additional quarter following the reform ($\Delta RCL = 1$). Individuals born in 1936 and who contribute 131 or 152 quarters at age 60 are also required to contribute one additional quarter following the reform.

B Data Details

This section presents the description of the Cnav data. We observe all retirees still alive, and all those who died between 2003 and 2017. Table B1 describes cohorts 1933 to 1943 observed in the data. For cohort 1933, Cnav data includes information about death between ages 71 and 84; for cohort 1934, information about death between ages 70 and 83. Given we observe mortality outcomes between 2004 and 2017 we do not observe mortality outcomes for the same ages for all the cohorts affected. As a result, we split our sample into two panels including individuals alive at the same age. In the first panel (Panel A), we observe the probability to die between 65 and 72 for individuals born between 1938 and 1943 (see Table B1). In the second panel (Panel B), we observe probability to die between 72 and 77 for individuals born between 1933 and 1938 (see Table B1).

We compare the sample characteristics to the national statistics from INSEE (the French institute of national statistics). Table B2 shows that the share of women is lower in our sample than in the INSEE data for Panel A and B respectively. Tables B3 and B4 shows the death probabilities per cohort.

Table B1: Description of Cohorts

Year of birth	**Death observed**	
	from age	**to age**
1933	71	84
1934	70	83
1935	69	82
1936	68	81
1937	67	80
1938	66	79
1940	65	78
1941	64	77
1942	63	76
1943	62	75

Lecture: For cohort 1933, Cnav data includes information about death between ages 71 and 84; for cohort 1934, information about death between ages 70 and 83.

Table B2: Share of the Sample per Cohort and Gender

Year of birth	Men			Women			Total
	N	Share	% INSEE	N	Share	% INSEE	
Panel A : Cohort 1938 to 1943, alive at age 65							
1938	212,603	59.07	47.23	147,290	40.93	52.77	359,893
1939	218,937	58.89	47.57	152,834	41.11	52.42	371,771
1940	211,437	59.15	48.22	146,003	40.85	51.78	357,440
1941	197,403	58.32	48.59	141,102	41.68	51.41	338,505
1942	214,805	57.86	48.96	156,451	42.14	51.04	371,256
1943	228,502	57.21	49.01	170,891	42.79	50.99	399,393
Total	1,094,476	58.40	48.27	914,571	41.60	51.73	2,198,258
Panel B : Cohort 1933 to 1938, alive at age 72							
1933	169,199	55.78	44.87	134,125	44.22	55.13	303,324
1934	177,871	56.50	45.32	136,967	43.50	54.68	314,838
1935	179,575	57.10	45.74	134,926	42.90	54.26	314,501
1936	183,216	57.26	46.41	136,769	42.74	53.59	319,985
1937	183,191	57.20	46.90	137,047	42.80	53.10	320,238
1938	188,291	57.40	47.23	139,716	42.60	52.77	328,007
Total	1,081,343	56.89	45.28	819,550	43.11	54.72	1,900,893

Lecture: This table shows the share of men and women in each cohort of our study. Individuals selected are those who contribute between 80 and 180 quarters, at least once in the private sector, and retire between ages 59 and 67. Moreover, panel A selects only individuals born between 1938 and 1943, and alive at age 65; panel B selects only individuals born between 1933 and 1938, alive at age 72. This table also shows national statistics from INSEE (the French institute of national statistics). Cohort 1933 in Panel B includes 303,324 individuals, and 56.89 % of them are men.
Source: Cnav Data and Insee data.

Table B3: Death Rate per Cohort – Cohorts 1938 to 1943

year of birth	Between age 65 and 66		66 and 67		67 and 68		68 and 69		69 and 70		70 and 71		71 and 72	
	Panel A	Insee	Panel A	Insee	Panel A	Insee	Panel A	Insee	Panel A	Insee	Panel A	Insee	Panel A	Insee
1938	0.60	1.11	1.17	1.15	1.28	1.21	1.35	1.28	1.42	1.36	1.50	1.45	1.54	1.52
1939	1.05	1.07	1.16	1.11	1.25	1.18	1.36	1.30	1.44	1.36	1.43	1.41	1.53	1.51
1940	1.05	1.04	1.17	1.08	1.21	1.13	1.29	1.24	1.32	1.27	1.44	1.40	1.49	1.49
1941	1.04	1.02	1.14	1.09	1.19	1.14	1.29	1.24	1.33	1.31	1.37	1.40	1.46	1.48
1942	1.07	1.02	1.16	1.09	1.19	1.14	1.19	1.18	1.29	1.26	1.38	1.39	1.46	1.46
1943	1.05	1.03	1.11	1.07	1.19	1.15	1.23	1.21	1.29	1.30	1.33	1.37	1.44	1.47
Total	0.798		1.15		1.22		1.28		1.35		1.41		1.48	

Lecture: This table shows the death rate per cohort for individuals selected in Panel A, and compared to death rate in the whole French population. Individuals selected are those who contribute between 80 and 180 quarters, at least once in the private sector, and retire between ages 59 and 67, alive at age 65. 0.60% individuals born in 1938 in Panel A died between ages 65 an 66, which is lower than the French death rate for this cohort (1.11% for the cohort 1938).
Source: Cnav 2017 and Insee life table by cohort data

Table B4: Death Rate by Cohort – Cohorts 1933 to 1938

	Between ages									
year of birth	72 and 73		73 and 74		74 and 75		75 and 76		76 and 77	
	Panel B	Insee	Panel B	Insee	Panel B	Insee	Panel B	Insee	Panel B	Insee
1933	2.00	1.87	2.15	1.20	2.24	2.15	2.40	2.39	2.54	2.59
1934	1.97	1.82	2.10	1.96	2.19	2.11	2.30	2.25	2.42	2.43
1935	1.94	1.78	2.05	1.92	2.18	2.05	2.31	2.22	2.44	2.41
1936	1.97	1.78	1.94	1.83	2.05	1.95	2.21	2.15	2.38	2.38
1937	1.88	1.71	1.95	1.83	2.07	1.95	2.23	2.15	2.29	2.28
1938	1.80	1.67	1.90	1.79	2.02	1.91	2.11	2.06	2.29	2.31
Total	1.92		2.01		2.12		2.26		2.39	

Lecture: This table shows the death rate by cohort for individuals selected in panel B, and compared to death rate in the whole French population. Individuals selected are those who contribute between 80 and 180 quarters, at least once in the private sector, and retire between ages 59 and 67, alive at age 72. 2% individuals born in 1933 in panel B died between ages 72 an 73, which is higher than the french death rate for this cohort (1.87% for the cohort 1933).
Source: Cnav 2017 and Insee life table by cohort data

Table B5 shows the number of individuals affected by the reform per number of additional quarters they had to contribute following the reform. Individuals selected are those who contribute between 80 and 180 quarters, at least once in the private sector and who retire between ages 59 and 67. In Panel A, there are 288,625 individuals born in 1938 who do not have to contribute more following the reform and 46,140 individuals who have to contribute five additional quarters to get a full pension. In Panel B, there are 51,219 individuals born in 1934 who have to contribute one additional quarter to get a full pension.

Table B5: Share of Each Cohort Affected by the Reform

	Added quarter required due to the reform										
Year of birth	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
Panel A											
1938	288,625	6,325	6,539	6,478	5,786	46,140	0	0	0	0	
1939	293,831	6,742	6,883	6,435	6,620	6,177	45,083	0	0	0	0
1940	276,442	6,911	6,941	6,318	6,377	6,590	6,560	41,301	0	0	0
1941	258,179	6,515	7,149	6,229	6,336	5,943	6,386	5,701	36,067	0	0
1942	278,258	7,345	7,884	7,210	6,812	6,599	7,243	6,800	6,267	36,838	0
1943	292,844	8,243	8,559	7,909	7,867	7,469	7,847	7,341	7,534	6,903	36,877
Total	1,688,179	42,081	43,955	40,579	39,798	78,918	73,119	61,143	49,868	43,741	36,877
% Total	76.80	1.91	2.00	1.85	1.81	3.59	3.33	2.78	2.27	1.99	1.68
Panel B											
1933	303,324	0	0	0	0	0	0	0	0	0	0
1934	263,619	51,219	0	0	0	0	0	0	0	0	0
1935	261,162	5,407	47,932	0	0	0	0	0	0	0	0
1936	262,654	5,869	5,764	45,698	0	0	0	0	0	0	0
1937	259,601	5,515	6,027	5,318	43,777	0	0	0	0	0	0
1938	262,686	5,770	5,999	5,913	5,294	42,345	0	0	0	0	0
Total	1,613,046	73,780	65,722	56,929	49,071	42,345	0	0	0	0	0
% Total	84.86	3.88	3.46	2.99	2.58	2.23	0	0	0	0	0

Lecture: This table shows the number of individuals affected by the reform, by number of added quarter they had to contribute following the reform. Individuals selected are those who contribute between 80 and 180 quarters, at least once in the private sector, and retire between ages 59 and 67. Panel A selects only individuals born between 1938 and 1943 and alive at retirement; Panel B selects only individuals born between 1933 and 1938, alive at age 72. In Panel A, there are 288,625 individuals born in 1938 who do not have to contribute more following the reform and 46,140 individuals who have to contribute five additional quarters following the reform if they want a full replacement rate.
Source: Cnav data

C Minimum Detectable Effect Analysis

In statistics, there are two types of error when testing if hypothesis H_0, "the result is zero" against H_1, "The result is different from zero" (see Table C1):

- The error type I, which is the probability of rejecting the null hypothesis whereas it is true;
- the error II type which is the probability of accepting the null hypothesis whereas it is false.

Table C1: The two types of error when testing H_0

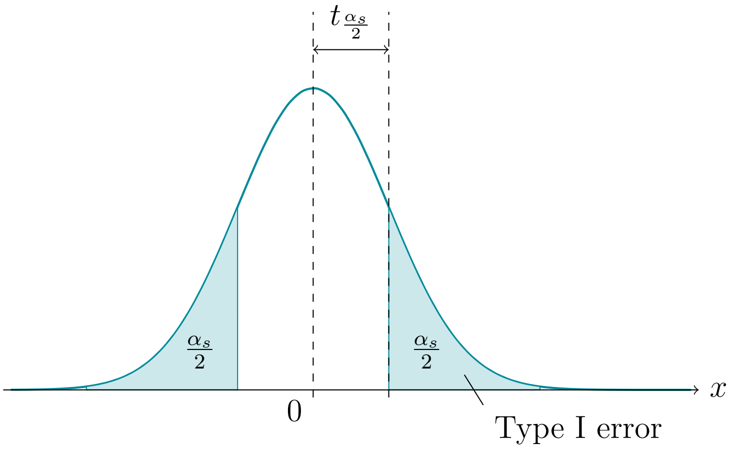
		True Value	
		H_0 is true	H_1 is true
Measured value	$= 0 \Leftrightarrow H_0$ is accepted	OK	Error type II
	$\neq 0 \Leftrightarrow H_0$ is rejected	Error type I	OK

The error type I is always tested through the p-value computation. Thus, a significant result at the 5% level means that the probability making a mistake when assuming H_1 : "$\beta \neq 0$" is lower that 5%. We use the p-value to test the probability to make type I error:

$$P\left(|\frac{\hat{\beta}}{\sigma_{\hat{\beta}}}| < t_{\frac{\alpha}{2}}\right) = 1 - \alpha \Leftrightarrow P\left(-t_{\frac{\alpha}{2}} < \frac{\hat{\beta}}{\sigma_{\hat{\beta}}} < t_{\frac{\alpha}{2}}\right) = 1 - \alpha$$

If $|\frac{\hat{\beta}}{\sigma_{\hat{\beta}}}| > t_{\frac{\alpha}{2}}$, we reject the null hypothesis at the α level. In other words, if $\frac{\hat{\beta}}{\sigma_{\hat{\beta}}} \geq t_{\frac{\alpha}{2}} \cup \frac{\hat{\beta}}{\sigma_{\hat{\beta}}} \leq -t_{\frac{\alpha}{2}}$, the probability to make a mistake rejecting H_0 hypothesis is greater than 5% (type I error).

Figure C1: Graphical Representation of Type I Error – 2 Tailed-test



Lecture: This is the distribution of Y under the H_0 hypothesis. Blue areas are the probability of making type I error (ie. accepting H_1 wheareas it is false).

When a result is non significant, we face a risk of making a type II error, a much more forgotten type of error in economics studies. The error type II is the probability of accepting H_0: $\beta = 0$ while it is false. Usually, we use a 20% power threshold.

The power analysis test for this type of error is:

$$P\Big(\big(|\frac{\hat{\beta}}{\sigma_{\hat{\beta}}}| \geq t_{\frac{\alpha}{2}}\big)|\beta\Big) = \kappa \Leftrightarrow P\Big(\big(\frac{\hat{\beta}}{\sigma_{\hat{\beta}}} \geq t_{\frac{\alpha}{2}} \cup \frac{\hat{\beta}}{\sigma_{\hat{\beta}}} \leq -t_{\frac{\alpha}{2}}\big)|\beta\Big) = \kappa$$

This formula can be simplify while the statistical power is compute either under the assumption of beta positive or negative but never both. As a proof, consider A, the event "$\frac{\hat{\beta}}{\sigma_{\hat{\beta}}} \geq t_{\frac{\alpha}{2}}$" and B the event "$\frac{\hat{\beta}}{\sigma_{\hat{\beta}}} \leq -t_{\frac{\alpha}{2}}$"

$$\Leftrightarrow P(A \cup B|\beta) = \kappa$$

knowing that $A \cap B = \emptyset$, $P(A \cup B|\beta) = \kappa \Rightarrow P(A|\beta) + P(B|\beta) = \kappa$. Moreover, this two probabilities are conditional to β. Consequently, $P(A|\beta) \neq 0 \Rightarrow P(B|\beta) = 0$ and $P(B|\beta) \neq 0 \Rightarrow P(A|\beta) = 0$. Graphically, that is equivalent to assume the H_1 distribution is either on the right or on the left to the H_0 distribution, but cannot be on both sides (see Figure C2).

So that, if $\hat{\beta} > 0$ but not significant, $P(A \cup B|\beta) = \kappa \Rightarrow P(A|\beta) = \kappa$ and if $\hat{\beta} < 0$ but not significant, $P(A \cup B|\beta) = \kappa \Rightarrow P(B|\beta) = \kappa$.

The following details are considering the case $\hat{\beta} > 0$ but not significant.

$$P(A|\beta) = \kappa \Leftrightarrow P\bigg(\big(\frac{\hat{\beta}}{\sigma_{\hat{\beta}}} \geq t_{\frac{\alpha}{2}}\big)|\beta\bigg) = k$$

$$\Leftrightarrow P\bigg(\big(\frac{\hat{\beta}-\beta}{\sigma_{\hat{\beta}}} \geq t_{\frac{\alpha}{2}} - \frac{\beta}{\sigma_{\hat{\beta}}}\big)|\beta\bigg) = k$$

$$\Leftrightarrow \Phi\bigg(\frac{\beta}{\sigma_{\hat{\beta}}} - t_{\frac{\alpha}{2}}\bigg) = 1 - k$$

$$\Rightarrow \frac{\beta}{\sigma_{\hat{\beta}}} - t_{\frac{\alpha}{2}} = t_{1-\kappa} \Leftrightarrow \frac{\beta}{\sigma_{\hat{\beta}}} = t_{1-\kappa} + t_{\frac{\alpha}{2}}$$

$$\Leftrightarrow \beta = \bigg(t_{1-\kappa} + t_{\frac{\alpha}{2}}\bigg)\sigma_{\hat{\beta}}$$

If $\hat{\beta} > 0$, The minimum detectable effect is $\bigg(t_{1-\kappa} + t_{\frac{\alpha}{2}}\bigg)\sigma_{\hat{\beta}}$.

The following details are considering the case $\hat{\beta} < 0$ but not significant.

$$P(B|\beta) = \kappa \Leftrightarrow P\bigg(\big(\frac{\hat{\beta}}{\sigma_{\hat{\beta}}} \leq -t_{\frac{\alpha}{2}}\big)|\beta\bigg) = k$$

$$\Leftrightarrow P\bigg(\big(\frac{\hat{\beta}-\beta}{\sigma_{\hat{\beta}}} \leq -t_{\frac{\alpha}{2}} - \frac{\beta}{\sigma_{\hat{\beta}}}\big)|\beta\bigg) = k$$

$$\Leftrightarrow \Phi\bigg(\frac{\beta}{\sigma_{\hat{\beta}}} + t_{\frac{\alpha}{2}}\bigg) = 1 - k$$

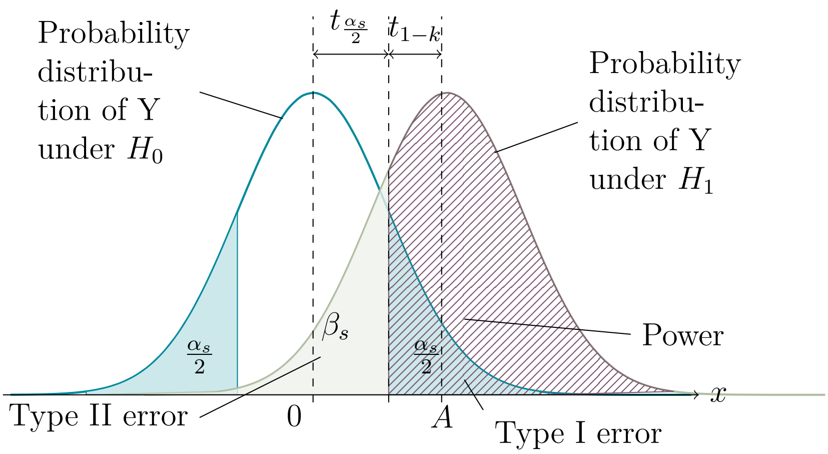
$$\Rightarrow \frac{\beta}{\sigma_{\hat{\beta}}} + t_{\frac{\alpha}{2}} = t_{1-\kappa} \Leftrightarrow \frac{\beta}{\sigma_{\hat{\beta}}} = t_{1-\kappa} - t_{\frac{\alpha}{2}}$$

$$\Leftrightarrow \beta = \bigg(t_{1-\kappa} - t_{\frac{\alpha}{2}}\bigg)\sigma_{\hat{\beta}}$$

If $\hat{\beta} < 0$, the minimum detectable effect is $\bigg(t_{1-\kappa} - t_{\frac{\alpha}{2}}\bigg)\sigma_{\hat{\beta}}$.

The minimum detectable effect (MDE) is the smallest effect we could detect taking into account the probability of being in the treatment group, the size and the variance of

Figure C2: Graphical Representation of Statistical Power



the sample. The higher the MDE, the lower the power.

The green curve in Figure C1 is the β distribution under the assumption H_1 is true ($\beta \neq 0$). For a β_s significance level, H_1 will be rejected if the distribution is in the green area (type II error). Consequently, the power of our test is the red dashed area.

In this paper, we want to test the hypothesis H_0: "the effect of delaying retirement due to the reform on mortality is equal to 0" against the alternative hypothesis H_1 "the effect of delaying retirement due to the reform is different from 0". This computation is interesting for the following sample: first for the estimation of the impact of delaying retirement on mortality for cohort 1938 to 1943, because of the non significant negative impact we found (-0.00035, with a standard error of 0.00044) and second for the impact of delaying retirement on mortality respectively for men and women born between 1933 to 1938 with non significant impacts of resp. 0.000884 (standard error: 0.00011), and 0.00102 (standard error: 0.000858). In each case, we would like to know if the non significant result is due to a lack power or can be interpreted as a null effect. Thus, we compute the MDE for each sub-sample.

$$\beta_1 > \underbrace{(t_{1-k} + t_{\alpha/2})SE(\hat{\beta}_1)}_{\textit{Minimum Detectable Effect}} \tag{5}$$

where $t_{1-k} + t_{\alpha/2} = 0.84 + 1.96 = 2.80$ (or $t_{1-k} - t_{\alpha/2} = 0.84 - 1.96 = -1.12$), according to student table[14], for a two-tailed test at the 5% level and a power of 20%.

Table C2: Minimum Detectable Effect

Specification	$\hat{\beta}$	SE	N	MDE	Variation in mortality
Panel A - All	-0.00035	0.00044	2,198,258	-0.00049	-0.54%
Panel A - Men	0.00006	0.0006	1,283,687	0.00168	1.87%
Panel A - Women	-0.0007	0.0006	914,571	-0.000672	-0.75%
Panel B - All	0.00068	0.00062	1,900,893	0.001736	1.59%
Panel B - Men	0.00011	0.00081	1,081,343	-0.00413728	-3.79%
Panel B - Women	0.00128	0.00101	819,550	0.002828	2.59%

Lecture: In Panel A, composed by all individuals born between 1938 and 1943, considering the sample size and the share of treated, the smallest effect we could detect is -0.00049. So that, an effect non significant but higher than -0.00049 can lead to the conclusion of an absence of effect but we cannot conclude on a non significant effect lower than -0.00049. This -.00049 effect is equivalent to a variation of death probability by -0.54%.

[14]See, for example Bloom (1995) for Student table.

D Robustness Checks

We rerun the 2SLS regressions on the EIR data, which contains information on socio-demographics characteristics. EIR data includes individuals from all pension schemes, born in early October of each even years. We select individuals who contribute the major part of their career to the private sector, born in 1934 or 1938 and alive at 70. This data contains information on gender, year of birth, contribution length, reference wages, marital status, children or not and professions.

Table D1 shows the results are virtually unchanged whatever the specification, i.e. with or without socio-demographics controls.

Table D1: Effect of Claiming Age (2nd Stage) with EIR Data - With and Without control for Individual Characteristics

	(1) Without control	(2) Marital status	(3) Profession	(4) Children
Claiming age	0.00432	0.00424	0.00426	0.00426
	(0.00513)	(0.00515)	(0.00518)	(0.00519)
N	11,809	11,809	11,809	11,809

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Lecture: We select from EIR data individuals who have contributed the major part of their career to the private pension scheme, born in 1934 and 1938, have contributed between 80 and 180 quarters at age 60, are alive at age 70. Death probability is between ages 70 and 74. The first model "without control" control only for variables we have in CNAV data: gender, year of birth, contribution length at age 60 and reference wage. The second model "marital status" add controls for being widow and being married. The third model "Profession" add controls for being farmer and being an executive. The last model add control for having at least three children. It shows the estimated impact of an exogenous increase of claiming age on mortality does not change when adding controls for socio-economic characteristics.
Source: EIR data 2004, 2008 and 2012. This is a French administrative dataset, representative of French retirees. There has been one EIR wave every four years since 1988. Each EIR wave includes all retirees born in early October of an even year of birth. It contains all the information collected by pension schemes, necessary for benefit computation (contribution length, reference wage, claiming age, etc.) and some socio-demographics variables (marital status, number of children, being a past farmer, being a past executive)

Table D2 shows the causal effect of claiming age on mortality at one year, at two, three, four, five and six years respectively. An exogenous increase of claiming age by one quarter has no significant impact on mortality at one year, whatever the age. The effect is never significant at 5%, i.e. the conventional level.

Tables D3 and D4 present the results, controlling for heterogeneous impact of the reform.

Table D2: Effect of Claiming Age on the Probability to Die - 2SLS - by Age Range

	72 and 73	73 and 74	74 and 75	75 and 76	76 and 77	
Claiming age	0.000532*	0.0000482	-0.000336	0.000148	0.000361	
	(0.000276)	(0.000282)	(0.000288)	(0.000302)	(0.000308)	
N	1,900,893	1,900,893	1,900,893	1,900,893	1,900,893	
	65 and 66	66 and 67	67 and 68	68 and 69	69 and 70	70 and 71
Claiming age	-0.000163	0.000295*	0.000157	-0.000124	-0.0000399	-0.000316*
	(0.000164)	(0.000170)	(0.000173)	(0.000179)	(0.000185)	(0.000188)
N	2,198,258	2,198,258	2,198,258	2,198,258	2,198,258	2,198,258

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Lecture: This is the second stage of 2SLS. Individuals selected are those who have contributed between 80 and 180 quarters, at least once in the private sector, and retire between ages 59 and 67. Panel A includes individuals born between 1938 and 1943, and alive at retirement; Panel B includes individuals born between 1933 and 1938, alive at age 72.
An exogenous increase of claiming age by one quarter has a no significant impact on mortality between ages 73 and 74.
Source: Cnav data.

Table D3 presents the OLS regression of the impact of the 1993 pension reform on the claiming age, with control for heterogeneous impact of the reform, and with control for contribution length at age 60, cohort, gender, and reference wage. Individuals selected are those who contributed between 80 and 180 quarters, at least once in the private sector, and retire between ages 59 and 67. Panel A selects only individuals born between 1938 and 1943, and alive at age 65. Panel B selects only individuals born between 1933 and 1938, alive at age 72. It shows all cohorts affected, both in Panel A and B, answer to the incentive to retire later. The intensity of the reaction increases with the intensity of the incentive. Taking into account these heterogeneous treatment effects does not change the results (see Table D5).

Table D4 presents the OLS regression of the impact of the reform on mortality (reduced form), assuming non-linear association between the variation of required contribution length due to the reform and mortality. This regression controls for contribution length at age 60, cohort, gender, and reference wage. Individuals selected are those who had contributed between 80 and 180 quarters, at least once in the private sector, and retire between ages 59 and 67. Panel A (resp. B) selects only individuals born between 1938 and 1943, and alive at age 65 (resp. born between 1933 and 1938, alive at age 72). Table D4 shows there is no significant impact on mortality at 5% when we allow heterogeneous effects – see column "All" of each panel.

Table D3: Effect of the Reform on Claiming Age
– Control for Heterogeneous Treatment Effect

	Panel A: 1938 - 1943			**Panel B: 1933 - 1938**		
	All	Men	Women	All	Men	Women
$\Delta RCL = 0$	*Ref.*	.	.	*Ref.*	.	.
$\Delta RCL = 1$	1.840***	1.847***	1.856***	0.873***	1.004***	0.724***
	(0.0394)	(0.0470)	(0.0673)	(0.0304)	(0.0419)	(0.0449)
$\Delta RCL = 2$	2.596***	2.659***	2.545***	1.420***	1.695***	1.081***
	(0.0427)	(0.0513)	(0.0721)	(0.0325)	(0.0454)	(0.0476)
$\Delta RCL = 3$	3.359***	3.443***	3.292***	1.898***	2.196***	1.515***
	(0.0469)	(0.0561)	(0.0798)	(0.0349)	(0.0484)	(0.0511)
$\Delta RCL = 4$	4.022***	4.191***	3.824***	2.316***	2.790***	1.750***
	(0.0512)	(0.0615)	(0.0865)	(0.0377)	(0.0519)	(0.0557)
$\Delta RCL = 5$	4.480***	4.803***	4.118***	2.866***	3.421***	2.196***
	(0.0534)	(0.0646)	(0.0894)	(0.0407)	(0.0559)	(0.0603)
$\Delta RCL = 6$	5.344***	5.735***	4.878***			
	(0.0562)	(0.0681)	(0.0939)			
$\Delta RCL = 7$	6.085***	6.551***	5.489***			
	(0.0587)	(0.0713)	(0.0979)			
$\Delta RCL = 8$	6.580***	7.037***	6.017***			
	(0.0613)	(0.0746)	(0.102)			
$\Delta RCL = 9$	6.860***	7.311***	6.330***			
	(0.0636)	(0.0775)	(0.106)			
$\Delta RCL = 10$	6.995***	7.478***	6.476***			
	(0.0659)	(0.0805)	(0.109)			
N	2,198,258	1,283,687	914,571	1,900,893	1,081,343	819,550

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Lecture: This table presents the OLS regression of the impact of the 1993 pension reform on the claiming age, with control for heterogeneous impact of the reform, and with control for contribution length at age 60, cohort, gender, and reference wage. Individuals selected are those who contributed between 80 and 180 quarters, at least once in the private sector, and retire between ages 59 and 67. Panel A selects only individuals born between 1938 and 1943, and alive at age 65. Panel B selects only individuals born between 1933 and 1938, alive at age 72. It shows all cohorts affected, both in Panel A and B, answer to the incentive to retire later. The intensity of the reaction increases with the intensity of the incentive.
Source: Cnav data.

Table D4: Effect of the Reform on the Mortality - Reduced Form with Non-linear Effect

	Panel A: 1938 - 1943			**Panel B: 1933 - 1938**		
	All	Men	Women	All	Men	Women
$\Delta RCL = 0$	*Ref.*	.	.	*Ref.*	.	.
$\Delta RCL = 1$	-0.000401	-0.00330	0.00501**	0.00281*	0.00227	0.00244
	(0.00196)	(0.00282)	(0.00250)	(0.00161)	(0.00251)	(0.00195)
$\Delta RCL = 2$	-0.00272	-0.00619**	0.00348	0.000946	-0.0000422	0.00114
	(0.00213)	(0.00307)	(0.00268)	(0.00170)	(0.00267)	(0.00206)
$\Delta RCL = 3$	-0.00299	-0.00339	-0.000789	0.00150	0.00166	0.000404
	(0.00234)	(0.00336)	(0.00296)	(0.00179)	(0.00281)	(0.00217)
$\Delta RCL = 4$	-0.00266	-0.00209	-0.00224	0.00152	0.000451	0.00169
	(0.00255)	(0.00369)	(0.00321)	(0.00192)	(0.00297)	(0.00236)
$\Delta RCL = 5$	-0.00354	-0.00323	-0.00211	0.00328	0.000959	0.00480*
	(0.00266)	(0.00387)	(0.00332)	(0.00204)	(0.00313)	(0.00253)
$\Delta RCL = 6$	-0.00224	-0.000242	-0.00267			
	(0.00280)	(0.00408)	(0.00349)			
$\Delta RCL = 7$	-0.00473	-0.00281	-0.00508			
	(0.00293)	(0.00428)	(0.00364)			
$\Delta RCL = 8$	-0.00467	-0.00137	-0.00688*			
	(0.00306)	(0.00447)	(0.00379)			
$\Delta RCL = 9$	-0.00248	-0.000908	-0.00249			
	(0.00317)	(0.00464)	(0.00392)			
$\Delta RCL = 10$	-0.00325	-0.00172	-0.00268			
	(0.00329)	(0.00483)	(0.00405)			
N	2,198,258	1,283,687	914,571	1,900,893	1,081,343	819,550

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Lecture: This table presents the OLS regression of the impact of the reform on mortality (reduced form), assuming non-linear association between the variation of required contribution length due to the reform and mortality. This regression controls for contribution length at age 60, cohort, gender, and reference wage. Individuals selected are those who had contributed between 80 and 180 quarters, at least once in the private sector, and retire between ages 59 and 67. Moreover, Panel A selects only individuals born between 1938 and 1943, and alive at age 65. Panel B selects only individuals born between 1933 and 1938, alive at age 72.
Source: Cnav data.

Table D5: Effect of Later Retirement on Mortality – Control for Heterogeneous Treatment Effect

	All	Men	Women
Panel A: 1938 to 1943			
Claiming age	-0.000519	-0.000270	-0.000441
	(0.000397)	(0.000544)	(0.000535)
N	2,198,258	1,283,687	914,571
Panel B: 1933 to 1938, alive at age 72			
Claiming age	0.000759	0.000164	0.00131
	(0.000618)	(0.000800)	(0.000994)
N	1,900,893	1,081,343	819,550

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Lecture: This table presents the second stage of 2SLS regression of the impact of later retirement on mortality, with control for heterogeneous impact of the reform, and with control for contribution length at age 60, cohort, gender, and reference wage. Individuals selected are those who had contributed between 80 and 180 quarters, at least once in the private sector, and retire between ages 59 and 67. Panel A selects only individuals born between 1938 and 1943, and alive at age 65. Panel B includes only individuals born between 1933 and 1938, alive at age 72.
Source: Cnav data.