The Effect of Raising the Full Retirement Age on Old Workers' Labor Market Outcomes

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Abstract

This article examines the effect of raising the Full Retirement Age (FRA) on employment and social insurance schemes. We exploit rich French administrative data on (i) claiming behavior, (ii) employment, and (iii) unemployment insurance to assess the consequences of the reform on labor market outcomes after 62. Based on a cohort difference-in-differences method, we find that raising the FRA decreases the probability of having claimed a pension at 65-66 by 8 pp. The effect is mostly driven by individuals who are eligible to a minimum pension and who have financial incentives to claim at that age. The impact of the reform on employment is small, about +1 pp. Meanwhile, the probability of being unemployed (resp. inactive) increases by +3 pp (resp. +4 pp). At least half of fiscal gains were offset by higher unemployment benefits, and they were primarily made off the bottom 25% of labor earnings.

Keywords: Pension reforms, Retirement age, Cohort difference-in-differences.

Classification JEL: H55, I3, J14, J26

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Introduction

Like many OECD countries faced with aging, France has implemented several pension reforms over the past 30 years in order to enhance the long-run financial sustainability of its pay-as-you-go (PAYG) pension system. These reforms mostly focused on increasing the retirement age. For instance, the 2010 (resp. 2023) reform increased the Early Retirement Age (ERA) from 60 (resp. 62) to 62 (resp. 64). However, 41% of newly retirees claim pension benefits after the ERA, either because they do not have the insurance duration required for full-rate pension benefits, or because they prefer higher pensions with a delayed retirement credit (Drees, 2024). While the effect of postponing the ERA from 60 to 62 on labor market outcomes around 60 has been the object of numerous studies, including in France (Dubois and Koubi, 2017; Rabaté and Rochut, 2020; Aubert et al., 2025), little is known about factors that lead to retire after that age. In particular, late retirees have peculiarities (e.g., their careers may be discontinuous, their entry on the labor market was delayed due to higher education, not claiming to be exhaustive) that may trigger specific responses to legislative changes as regards statutory retirement ages. Interestingly, the 2010 pension reform provides an opportunity to better understand labor market outcomes at old ages, which helps predicting the impact of reforms that consist in shifting such ages. In addition to delaying the ERA from 60 to 62, this reform also raised the Full Retirement Age (FRA) from 65 to 67. Reaching the FRA allows individuals to claim full-rate pension benefits even if criteria related to insurance duration are not fulfilled.

In this article, we empirically assess the causal effect of increasing the FRA on labor market outcomes at old ages (around 65-66) based on the 2010 pension reform. In particular, we quantify the role played by substitution effects between retirement and other social insurance schemes, especially unemployment insurance. We use French administrative data that provide us with relevant information about pension claiming, employment and unemployment. We rely on a cohort difference-in-differences design to compare cohorts affected by the reform (late 1951 to 1953) to those unaffected (1950 and early 1951).

Our results are as follows. First, we find that shifting the FRA from 65 to 66 led to a decrease in the probability of having claimed a pension at 65-66 by 8 pp. Yet the effect on employment is small (about +1 pp, namely 12.5% of the observed drop in retirement only), while substitution effects on unemployment (+3 pp) and inactivity (+4 pp) are substantial.[1] We further show evidence that the impact on retirement at 65-66 is especially high for individuals who benefit from a minimum pension benefit, which should be related to financial incentives that corresponding eligibility rules to this safe net scheme induce. Nevertheless, individuals who do not benefit from any minimum pension do postpone their retirement following the reform, which leaves room for alternative explanations than financial incentives. Second, these results are mostly driven by individuals remaining longer in their pre-FRA labor

[1] By contrast, the rise of employment due to the shift of the ERA represents 40% of the decrease of retirement (Rabaté and Rochut, 2020).

market state, and not by active substitution with other states, consistent with many previous studies (e.g., Rabaté et al., 2024 and Geyer and Welteke, 2021). Third, we estimate a higher bound of the annual net fiscal gain of the reform, namely 451 million euros. This modest estimate arises from the fact that at least one half of the fiscal gain generated by the decrease in pension benefits is offset by higher unemployment benefits. We view this estimate as a higher bound because potential negative externalities on health expenditures are not taken into account. Furthermore, we show that this fiscal gain is borne almost entirely by low-income individuals, indicating that the increase in the FRA was highly regressive.

Literature Our work contributes to a better understanding of the linkage between old-age insurance (statutory retirement ages, in particular) and retirement behavior. An empirical literature (e.g., Seibold, 2021) has indeed found evidence of bunching in the retirement hazard at statutory retirement ages, although the latter are not associated with real economic incentives. Moreover, these spikes tend to move along shifts in those ages following corresponding pension reforms. While several studies (including Staubli and Zweimüller, 2013; Geyer and Welteke, 2021; Rabaté et al., 2024, not claiming to be exhaustive) have focused on changes in the ERA, our focus here is whether and how policy-induced shifts of the FRA trigger changes in retirement behavior. A seminal contribution in the domain is Mastrobuoni (2009), who provided the first suggestive evidence of labor supply increasing after an increase in the FRA. In that vein, Behaghel and Blau (2012) study a quasi-experimental design based on a gradual shift in the FRA from 65 to 66, following the Social Security Amendments of 1983 in the U.S. Their findings point out to a spike in benefit claiming hazard at 65 that moves in lockstep along with the FRA. Their results based on survey data also indicate that people with higher cognitive skills have a higher responsiveness, which rather claims for a behavioral explanation related to reference dependence with loss aversion than to alternate mechanisms based on social norms or an endorsement effect, since responsiveness does not seem to be due to unsophisticated decision making. Still relying on the same 1983 reform, Deshpande et al. (2024) complement previous analysis with empirical evidence issued from administrative panel database, which enables them to finely disentangle the benefit-claiming age from the retirement age. They find that while claiming ages strongly and immediately shift in response to increases in the FRA, retirement ages exhibit persistent stickiness at the old FRA (65), which they interpret as evidence in favor of employers playing a role in workers responses to the FRA. On top of reference dependence with loss aversion in pension claiming, Lalive et al. (2023) emphasize the role played by joint retirement decisions within couples, and suggest that government savings from pension reforms that increase the FRA may be larger than expected due to the amplifying role of such spillovers.[2] Other recent evidence from the UK suggests that raising the pension age may have large impacts on employment even at older ages (Cribb et al., 2022). By contrast, Ardito et al. (2021) insists on unin-

[2]Dolls and Krolage (2023) exploit an unusual decrease in the German FRA. Their results suggest that the responses to the reform are driven both by the FRA serving as a reference point and by financial incentives.

tended adverse impacts on the Italian labor market, whereby the most vulnerable apply for disability and unemployment benefits. These countervailing effects weaken the effectiveness of the reform, and they are reminiscent of Duggan et al. (2007) focusing on the impact of the 1983 Social Security Amendments on the sole disability insurance. Our results rather concur with such a diagnosis in the French case.

The rest of this article is organized as follows. Section 1 presents the French retirement system as well as the 2010 reform. Section 2 is devoted to the data. Section 3 shows how the distribution of retirement ages varies across cohorts. Section 4 describes our cohort difference-in-differences method. Our results are exposed in section 5, and robustness checks in section 6. Section 7 concludes.

1 Institutional background

1.1 General overview of the French pension system

1.1.1 Retirement age and pension benefits

The French pension system is quite complex since it is an aggregation of 42 specific schemes related to various occupations and statuses on the labor market. We here focus on the main pension system prevailing in the private sector, which is ruled by the *Caisse Nationale d'Assurance Vieillesse* (hereafter CNAV), and which covers about 70% of all pensioners (Drees, 2024).

Workers' pension is made of two pillars: a base one, analogous to the US Social Security, and a supplementary one. The eligibility to full-rate pension benefits from the first pillar (*régime de base CNAV*) depends on three main parameters:

- The Early Retirement Age (ERA), which corresponds to the minimum age legally required for retirement.[3]
- Insurance duration, namely a reference contributory period which corresponds to a minimal number of quarters with positive social security contributions. This criterion should be met in order to retire with full-rate pension benefits. Some quarters with no contribution, corresponding to unemployment spells or sick leaves, may be included, though.
- The Full Retirement Age (FRA), after which retirement with full-rate pension benefits is granted regardless of whether previous criterion as regards insurance duration is met.

As a result, the age at which individuals are entitled to claim for full-rate pension benefits, the Normal Retirement Age (NRA), is specific to each individual depending on their career.

[3]Exceptions include a so-called "long-careers" scheme that is targeted at individuals entering early on the labor market and with sufficient insurance duration.

The NRA can be reached either by fulfilling the criterion related to insurance duration, or by reaching the FRA.

When retiring at the NRA, pension benefits P are computed according to the following formula:

$$P = \tau \times W \times \rho$$

where :

- τ is the *pension rate*, also called *full rate* when retiring at the NRA, and equal to 50%.[4]
- W corresponds to the *reference wage*, namely the average gross wage over the 25 best years, with past earnings being revalued according to the Consumer Price Index (CPI).[5]
- ρ is the ratio of actual over reference insurance duration, and this ratio cannot exceed one. Pension benefits are therefore almost proportional to the number of quarters with positive social security contributions. Individuals retiring at the FRA with a lower insurance duration than the reference thus see their pension benefits accordingly reduced, i.e. through this ρ parameter.

It is not mandatory to retire at the NRA, however. Individuals may retire at any age after the ERA, even though the pension rate τ is then reduced by some penalty, an actuarial reduction factor called *décote*, based on the minimum between the number of missing quarters of insurance duration and the number of missing quarters to reach the FRA. Conversely, the pension rate may also be increased for individuals who retire after the NRA through a delayed retirement credit called *surcote*.

The first pillar is augmented by a mandatory second pillar, a supplementary old-age insurance scheme called *régime complémentaire Agirc-Arrco*. This pension system is point-based. Points are purchased based on workers' contributions and a price called *valeur d'achat*, and they are credited to an individual retirement account. Upon retirement, corresponding pension benefits amount to the total number of points accumulated on that account multiplied by the nominal value of a point, a price called *valeur de service*. Both prices are set annually by employers' and employees' unions in charge of that system. These supplementary pension benefits may also be reduced for individuals retiring before the NRA.

1.1.2 Minimum pension and old-age welfare benefits

The *Minimum Contributif* (hereafter MICO) is a means-tested, *contributory* pension supplement designed to ensure that low-income workers with a complete career receive at least a minimal amount of pension. Eligibility conditions to claim for the MICO include that the

[4] This rate is not uniform across the 42 schemes, though. For instance, it amounts to 75% in the public sector.

[5] Again, this reference wage is not computed uniformly across the 42 schemes. For instance, bonuses are excluded from the perimeter of that wage in the public sector.

NRA is reached and that pension benefits do not exceed some threshold.[6] The amount of the supplement depends on the contributory period.

By contrast, the *Allocation de solidarité aux personnes âgées* (hereafter ASPA) is a *non-contributory* old-age welfare benefit.[7] This scheme belongs to the safe net part of the social insurance system, it aims at preventing poverty at old ages by guaranteeing a decent standard of living after 65. Eligibility to ASPA depends on household income, not on the insurance duration. At the beneficiary's death, the heirs are required to reimburse perceived benefits provided that their wealth exceeds 100,000 euros.

1.2 The 2010 reform

As made clear by Table 1, the 2010 pension reform jointly shifted the ERA and the FRA, respectively, by 2 years, gradually increasing them by 4 or 5 months between 1950 and 1955 cohorts. Due to policy design, 1950 and early 1951 cohorts were unaffected while late 1951 and younger cohorts were subject to higher statutory retirement ages than 60 and 65, respectively. In the current article, we focus on 1950-1953 cohorts only because the data is not yet available for younger cohorts.

Table 1: Statutory retirement rules

Cohort	ERA	FRA	Insurance duration (quarters)	Penalty (per missing quarter)
1950	60 years	65 years	162	-1.625%
early 1951	60 years	65 years	163	$-1,5\%$
late 1951	60 years and 4 months	65 years and 4 months	163	-1.5%
1952	60 years and 9 months	65 years and 9 months	164	-1.375%
1953	61 years and 2 months	66 years and 2 months	165	-1.25%

Sample. CNAV pension scheme.
Note: Early 1951 cohort corresponds to individuals who were born before July 1951.

Other parameters related to the computation of pension benefits were altered at the same time for the concerned cohorts, though. Both 2003 and 2010 reforms shifted the reference insurance duration, from 150 quarters for the 1933 cohort to 165 quarters for the 1953 cohort. The 2003 reform also decreased the *décote*, in absolute, from -2.5% per missing quarter for the 1944 cohort to -1.25% for the 1953 cohort.

The age at which individuals may claim for old-age welfare benefits (ASPA) was unchanged by the 2010 reform.

[6] From 2012, pensions from other schemes than CNAV are also taken into account to determine eligibility to MICO. If the sum of all pensions entitled to the individual exceeds a given ceiling, the supplement is reduced accordingly.

[7] The ASPA was created in 2007, but similar benefits referred as "minimum vieillesse" existed before, some of them being still active. In the following, we consider all these benefits as part of the same design.

2 Data

2.1 Sources

We mainly use four administrative databases: (i) the *Échantillon Interrégimes de Retraités 2020 (EIR)*, which provides information on retirement behavior, (ii) the *Panel Tous Actifs (PTA)*, which provides information on employment, (iii) the *Fichier National des Allocataires* database, which provides information on unemployment spells, and (iv) the *Échantillon national interrégimes d'allocataires de compléments de revenus d'activité et de minima sociaux (Eniacrams)*.

Échantillon Interrégimes de Retraités The 2020 *Échantillon Interrégimes de Retraités* (EIR) contains detailed information on pensions claims and benefits at the individual level for a representative sample on December 31st 2020.[8] Our main variables of interest are (i) the main pension scheme, (ii) the retirement age, (iii) the type of claim (including, e.g. the *décote* penalty and the *surcote* premium), (iv) the amount of pension benefits, (v) a dummy equal to 1 for individuals receiving the minimum contributory pension benefit (MICO), and (vi) a dummy for being granted the ASPA old-age welfare benefit.

40% of individuals in this sample can be matched to the *Panel tous actifs*, which provides us with precise data on employment and unemployment spells at the end of their career.

Panel Tous Actifs The 2020 *Panel Tous Actifs* (PTA) indeed includes information on both salaried and non-salaried employment at the individual × year × job level for a representative sample of workers.[9] The PTA is composed of two datasets:

- the *Panel tous salariés* (PTS): this dataset relates to salaried employment in the private sector from 1976 to 2020 (N.B. it also includes the public sector from 1988 onward). Each observation provides detailed information on some employment spell within a firm. Due to possibly multiple employment spells per worker, there might be multiple observations per individual × year. Our main variables of interest are (i) the period of employment, and (ii) wages.
- the *Panel non salariés* (PNS): this dataset relates to self-employment from 2006 to 2020. Each observation provides detailed information on the type of self-employment at the individual × year level. Our main variable of interest is the period of employment, but other variables related to revenues might be considered as well.

[8] The sampling rule is based on the day of birth.

[9] The sampling rule is also based on the day of birth, but it is more restrictive than in the EIR, which explains why 40% of individuals in the EIR only can be found in the PTA. In the salaried part of the panel, the PTS, sampled individuals were born in October of an even year for observations before 2002, while from 2002 onward they were born in October or on January 2nd-5th, April 1st-4th, and July 1st-4th. The same sampling rule prevails in the non-salaried part of the panel, the PNS, though from 2006 onward only.

Fichier National des Allocataires The 2020 *Fichier National des Allocataires* database provides information on unemployment at the individual × unemployment spell level. An unemployment spell is defined as a period during which an individual is enrolled as a job seeker at *France Travail*, a public employment agency in charge of fostering job search and firm-worker matching. All individuals in both the EIR and the PTA are also part of this database provided that they experienced an unemployment spell at some point.

Échantillon national interrégimes d'allocataires de compléments de revenus d'activité et de minima sociaux We also use a fourth administrative database, the *Eniacrams*, on other welfare benefits. The *Eniacrams* contains detailed information on several welfare benefits - including disability allowance (*Allocation adulte handicapé*, hereafter AAH), minimum income benefit (*Revenu de solidarité active*, hereafter RSA) and housing benefits (*Aide personalisée au logement*, hereafter APL) - at the individual × year level. Unfortunately, for some cohorts (including those unaffected by the reform), the data is censored at 65 so it is not possible to assess the impact of the reform on this margin. We provide descriptive evidence on the potential effects of the reform on these welfare benefits in section A in Appendix.

2.2 Working sample

In what follows, we focus on 1950 to 1953 cohorts. To understand why, the 1950 cohort is the youngest cohort that was left completely unaffected by the reform, while the 1953 cohort is the oldest cohort for which one avails of required data to evaluate the effect of the reform.[10] To prevent selection effects due to the right-censoring of our data, we restrict our sample to individuals who claimed pension benefits at 67.[11]

The shift of the FRA has mostly affected pensioners affiliated to the CNAV pension scheme (workers in the public sector experienced a more gradual shift).

We successively consider two samples:

- (1) Pensioners whose main affiliation scheme is the CNAV.
- (2) Pensioners whose main affiliation scheme is the CNAV and who experienced at least one salaried employment spell from 52.

Sample (1) is used to assess the effect of the reform on the sole retirement behavior, while sample (2) is used to examine effects on employment and unemployment. Note that because the sampling of our employment and unemployment data is more restrictive than those of our retirement data, we need to select pensioners who are present in the PTA to avoid interpreting

[10] Due to the timeline of the reform, we need to observe the labor market status between 66 and 67, hence we are currently unable to consider younger cohorts than 1953 because the data posterior to 2020 is not yet available.

[11] This is a mild restriction: almost all retirees of these cohorts have already claimed some pension benefit at this age. For the 1950 cohort, only 1.6% of individuals claiming a pension before 71 claimed their first pension benefits between 67 and 71.

a missing value as non-employment.[12] Moreover, because the PTA sampling changed in 2002 when cohort 1950 reached 52, we consider only pensioners who had at least one job spell from 52 to make cohorts comparable. Our main results will therefore evaluate the effect of the reform on the subpopulation of pensioners who were employed at least once from 52.[13]

2.3 Outcomes

To get a precise view on late-career trajectories, we aggregate our data and corresponding information on labor market statuses, labor earnings, and various benefits at the individual $\times$ half-year level. An individual aged a is assigned to a labor market state if they spent more than 50% of the corresponding age interval in that state. For sample (1), we consider only two states: claiming (if the individual has claimed all or part of their pension) and not claiming. For sample (2), we consider four states: claiming, employment, unemployment, and inactivity (the residual category). These categories are built from administrative records, and do not correspond to the ILO definitions based on labor force surveys. In particular, an individual is considered as unemployed if they are registered at the Public employment service. Regarding the employment state, we mainly focus on salaried employment, but we show in figure C.9.1 in Appendix that including the self-employed does not change our results. Note that these labor market states are not necessarily exclusive: working during retirement is possible, being enrolled at *France Travail* does not preclude from working part-time, etc.

As regards earnings, we pay attention to four types of (gross) income: pension benefits (including MICO but not survivor benefits), wages, unemployment benefits, and old-age welfare benefits, all of them being measured during half-year periods.

3 Descriptive evidence

We first describe our working sample in Table 2. The control group, which will be defined in section 4 below as the union of 1950 and early 1951 cohorts, looks quite similar to the treated group, namely the set of late 1951 to 1953 cohorts. The share of women is slightly lower in the control group, as well as the share of singles. Note that the insurance duration with Social Security contributions (resp. the total insurance duration) increases by 1.4 (resp. 3.4) quarter(s) from 139.7 quarters for the 1950 cohort to 141.1 quarters for the 1953 cohort, consistent with the shift of the statutory retirement ages induced by pension reforms.

The magnitude of this shift is most apparent in Figure 1 that depicts how the claiming age

[12] We could select all individuals that are potentially in the PTA given its sampling. Unfortunately, the day of birth is not available in the data, hence we cannot define any individual-level sampling status.

[13] As a robustness check, we also consider a broader definition of employment that includes self-employment. In that case, our sampling criterion is slightly relaxed since we include all pensioners who experienced some salaried employment spell from 52 or some self-employment spell from 56 (because the PNS has been available from 2006 only, when cohort 1950 reached 56).

evolves depending on the cohort.[14] First, the distribution of that age exhibits two spikes: one at the ERA and the other at the FRA. For instance, as regards the 1950 cohort, which is completely unaffected by the reform, more than 40% of individuals bunch at the ERA and more than 10% at the FRA. Bunching at the ERA can easily be explained by the fact that most individuals are not allowed to retire earlier than this age. However, the observed bunching at the FRA is more puzzling because, in general, there is no change in financial incentives at that age. In particular, the pension benefit formula exhibits no discontinuity around the FRA: remember that the *décote* penalty is based on the minimum between the number of missing quarters of insurance duration and the number of missing quarters to reach the FRA.[15] Furthermore, contributed quarters beyond the FRA provide a premium of the insurance duration of a similar percentage as the *surcote* when individuals retire beyond the NRA. If one views retirement as the result of an optimization in order to meet a target replacement rate that would be continuously distributed across individuals, there is therefore no reason why one should expect some bunching at the FRA. That being said, there are in fact two other discontinuities occurring at the FRA related to two distinct eligibility rules as regards: (i) the MICO pension benefit (only individuals who have reached their NRA are eligible to the MICO), and (ii) unemployment benefits (only individuals who have not reached their NRA can claim unemployment benefits). Interestingly, equipped with our comprehensive database, we are able to determine whether individuals are subject to these conditions. Figure C.1.1 in Appendix shows that bunching at the FRA is indeed much higher for individuals who receive the MICO. Nevertheless, we also see some bunching for those who do not benefit from the MICO, which is a puzzling feature that we discuss in section 5.5.1.

Second, as these statutory retirement ages (viewed as parameters of the pension system) increase due to the reform, we observe that the corresponding spikes accordingly shift to the right. Note that some changes in the distribution before 60 are related to the evolution of pension rules in the long-career scheme. We are confident that these changes do not affect our estimates since they occur at early ages and concern individuals who would have retired before the FRA even in the absence of the reform.

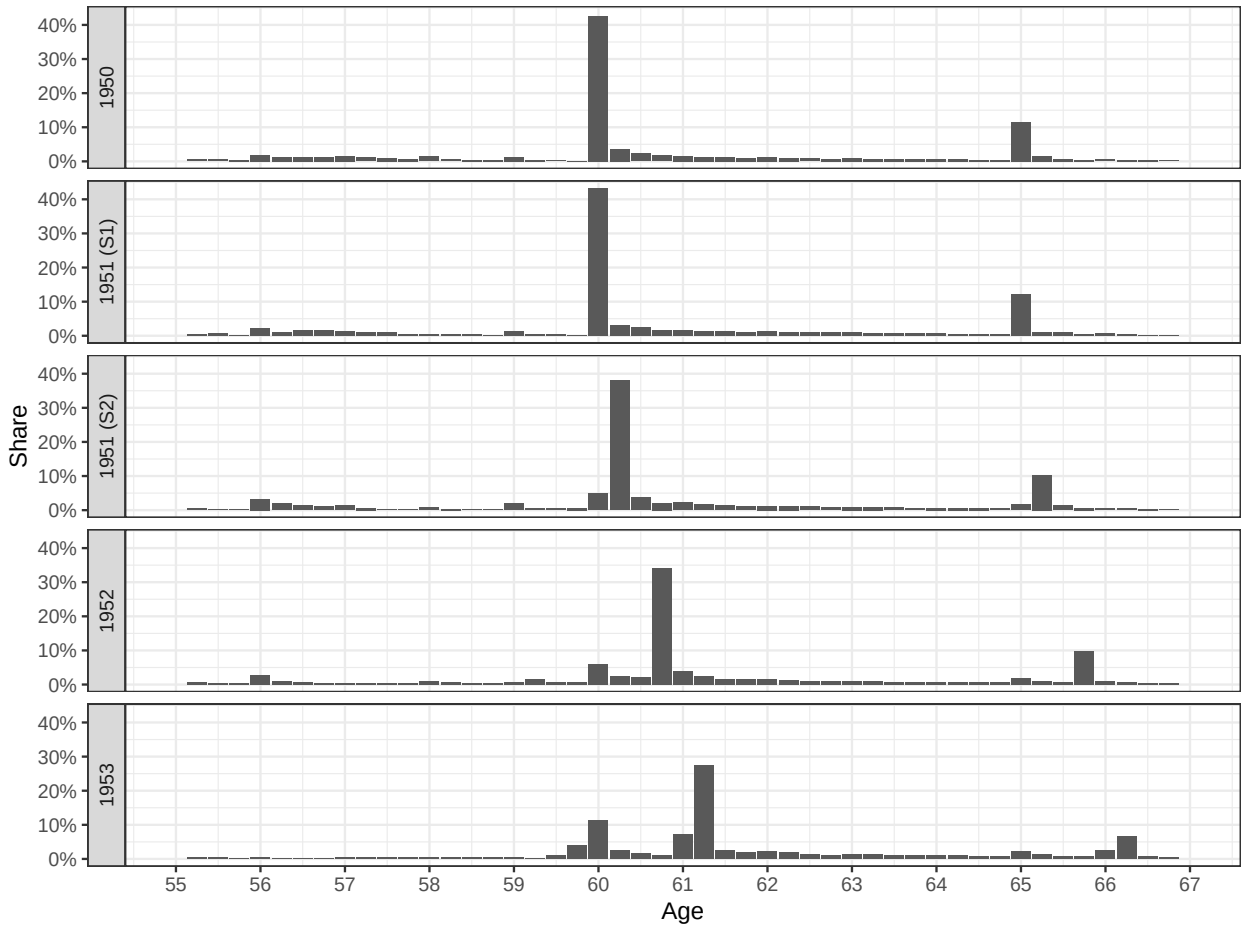
[14] The distribution of that age is here computed from all pensioners, i.e. from sample 1.

[15] Assume for instance that some retiree of the 1953 cohort obtains their full-rate pension benefit by reaching the FRA, rather than by fulfilling the criterion related to insurance duration (i.e. reaching the reference insurance duration). When retiring one quarter before the FRA, their penalty is -1.25% regardless of the (total) insurance duration.

Table 2: Summary statistics

	1950	Early 1951	Late 1951	1952	1953
Women	48.6%	49.2%	50.8%	50.0%	50.0%
Insurance duration (with SSC)	139.7	139.8	140.0	140.7	141.1
Total insurance duration	164.4	164.8	165.9	166.9	167.8
Resident in France	98.8%	98.8%	98.9%	99.1%	99.0%
Number of children	2.1	2.1	2.1	2.1	2.1
Number of pension schemes per retiree	2.6	2.6	2.6	2.6	2.6
Marital status					
Single	12.6%	13.5%	13.5%	14.5%	15.7%
Married	62.6%	62.2%	61.2%	61.4%	60.5%
Widowed	10.1%	9.5%	9.6%	8.7%	8.0%
Separated	14.2%	14.2%	14.9%	14.4%	14.2%
Civil union (PACS)	0.2%	0.2%	0.3%	0.4%	0.6%
Cohabitee	0.4%	0.5%	0.6%	0.7%	1.0%
Number of observations					
All	75,341	51,991	60,190	113,974	110,765
Sample (1): CNAV	53,550	36,899	42,879	81,152	77,526
Sample (2): CNAV x PTS	42,204	9,592	16,295	26,337	26,015

Notes: Descriptive statistics based on pensioners whose main pension scheme is Cnav and with at least one salaried employment spell from 52 (sample 2). SSC: Social Security contributions. Insurance duration: in quarters.
Source: EIR 2020.



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: all pensioners.
Source: EIR 2020.

Figure 1: Distribution of claiming age over cohorts 1950 to 1953

4 Identification strategy

In this section we present our empirical strategy to evaluate the effect of the shift of the FRA on labor market outcomes. As in Vestad (2013) and Rabaté and Rochut (2020), we rely on a cohort difference-in-differences design where the dynamic dimension of the treatment is based on age (rather than time). It means that we do not directly compare retirement/employment rates across cohorts, but rather that we compare the evolution of these rates *between ages* across cohorts.

We estimate the following specification:

$$Y_{aci} = \sum_{a'=62}^{67} \lambda_{a'} A_{ai}^{a'} + \sum_{c'=1950}^{1953} \delta_{c'} C_{ci}^{c'} + \sum_{a'=62}^{67} \sum_{c'=1951}^{1953} \beta_{a'c'} \left(A_{ai}^{a'} \times C_{ci}^{c'}\right) + \alpha X_i + \epsilon_{aci} \qquad (1)$$

where indices a refer to age (from 62 to 67, the baseline being 64.5) and c to cohort (from the baseline, 1950, to 1953, yet carefully distinguishing between early and late 1951) of

individual i. The outcome Y is either the labor market state (retired, employed, unemployed, or inactive) or some component of income. $A_{ai}^{a'}$ is an age dummy equal to 1 when $a = a'$ and $C_{ci}^{c'}$ is a cohort dummy equal to 1 when $c = c'$: they both account for age- and cohort-specific fixed effects. Individual-specific control variables X include gender, location of birth, marital status, number of children, age at first employment, number of days worked between 52 and 59, and wages earned between 52 and 59. The coefficients of interest β_{ac} measure the difference between (i) the probability gap for cohort c of being, say, in some state s on the labor market at age a rather than at 64.5, and (ii) the probability gap for cohort 1950 of being in state s at age a rather than at 64.5. They can be interpreted as the causal effect of the reform under the common trend assumption, namely that, absent the reform, age profiles would have been similar across cohorts. To assess the credibility of this assumption, we can look at coefficients before 64.5. If these coefficients are close to 0, then the common trend assumption cannot be rejected. As a result, the coefficients related to ages 65 and 66 of the treated cohorts can be interpreted as the causal effect of the increase of the FRA. We use sample weights in the regression to ensure that the results are representative of the whole population.

The main threats to identification are the existence of other policy changes that concomitantly affected 1950-1953 cohorts and the existence of period effects. Regarding the first concern, the most important other policy change is obviously the shift of the ERA, which has been shown to have a major impact on labor market outcomes at ages 55-62 (Dubois and Koubi, 2017; Rabaté and Rochut, 2020). Another substantial policy-induced change has trait to early retirement for workers with a so-called long career, as previously mentioned. However, these two major changes had likely little impact at old ages (62-67) because the new ERA for cohort 1953 still remains below 62 (more precisely, 61 and 2 months), and because early-retirement for workers in the long-career scheme occurs even earlier.

Remembering Table 1, two other potential confounders are (i) the rise of the reference insurance duration required to get full-rate pension benefits, and (ii) the decrease of the *décote* penalty for claiming pension benefits before the NRA. Note that confounder (i) would lead us to *overestimate* the effect of the shift of the FRA on labor market outcomes: part of the decrease (resp. increase) of retirement (resp. employment) rate that we observe at 65 could in fact be due to the increase of the reference insurance duration rather than to the shift of the FRA.[16] Conversely, confounder (ii) would lead us to *underestimate* the effect: for a given reference insurance duration and FRA, the decrease of the *décote* penalty when claiming pension benefits before the NRA creates an incentive to retire *earlier*. Though the simultaneity of confounders (i) and (ii) does not enable us to identify each of them separately, we are able to recover their aggregated effect: this is due to early 1951 cohort facing both policy changes without experiencing any change in the FRA. Any difference between 1950

[16] Although the pension replacement rate τ is left unchanged, ρ depends on the reference insurance duration. As a result, an increase in this required insurance duration mechanically leads to a decrease in pension benefits, regardless of whether the FRA is shifted or not.

and early 1951 cohorts at ages 62-67 should therefore reflect contemporaneous changes that are not due to the shift of the FRA. To further limit concerns about the spurious role played by possible confounders (i) and (ii), we also compare early to late 1951 cohorts only as a robustness check: Table 1 makes it clear that these factors should not intervene in such a comparison which, in spirit, combines our cohort difference-in-differences approach with a regression discontinuity design around July 1st, 1951. The results of this robustness check are available in section 6.1.

The second concern relates to the presence of period-effects. In theory, the analysis should account for the fact that two different cohorts reach the same age at different points in time. For instance, our control cohort born in 1950 reaches age 65 in 2015, whereas cohort 1953 reaches age 65 in 2018. If labor market conditions were more favorable in 2018 than in 2015 (as was indeed the case), we would overestimate the effect of the reform. However, in practice, it is not possible to separately identify cohort, age and period effects because of perfect collinearity. As a robustness check, we follow Deaton (1997) and Rabaté and Rochut (2020) by estimating a specification that includes constrained period-effects.[17] Moreover, the robustness check based on the 1951 cohort discussed above also helps to account for period effects, as early- and late-born individuals within the 1951 cohort reached the same ages at nearly the same points in time.

5 Results

5.1 Effect of the reform on retirement

We first assess the impact of the reform on retirement behavior based on sample (1), *i.e.* all pensioners whose main pension scheme is the CNAV. The evolution of the age-specific share of pensioners over cohorts is displayed by Figure 2. Although 1950 to 1952 cohorts have different shares of retirees at 62, they gradually converge towards the same proportion at 64.5. By contrast, the 1953 cohort has a share of retirees that is 3 pp higher than the other cohorts at 64.5.

The common trend assumption does not seem to hold between 62 and 63, yet shares of retirees evolve quite similarly across cohorts between 63.5 to 64.5. We will therefore rely on 64.5 as our baseline age in our cohort difference-in-differences (DiD) estimations.

Figure 3 plots the DiD coefficients obtained with our estimating equation (1) as regards two outcomes: claiming as a binary state on the labor market, and the amount of pension benefits, in €. Consistent with Figure 2, both 1950 and early 1951 cohorts in the control group displayed similar retirement behavior. It suggests that confounders (i) and (ii), possibly

[17] Specifically, we normalize the sum of the period effects to zero and we force them to be orthogonal to a time trend, so that they capture deviations of the business cycle around a zero trend.

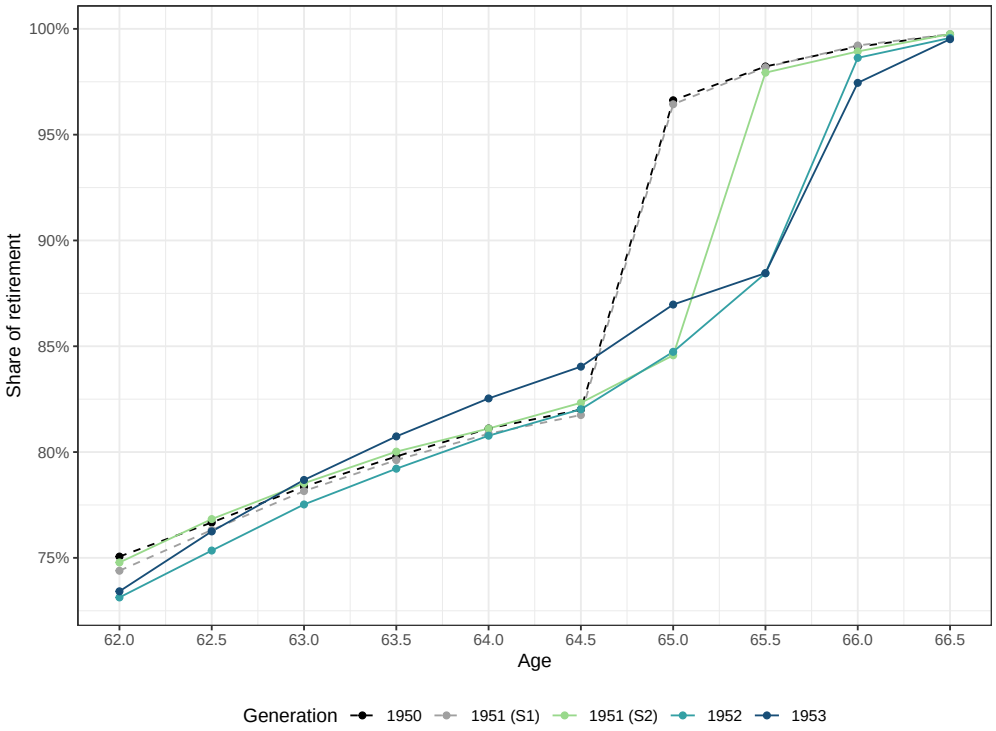
affecting the early 1951 cohort but not the 1950 cohort, does not bias much our results.[18] However, pre-trends at ages 62-63 among these cohorts substantially differ from the one that prevails among the 1952 cohort, which could be related to the increase in the reference insurance duration (confounder (i)) or to a downstream effect of the shift of the ERA.[19] Reassuringly, at age 64, differences across cohorts become very small, which suggests that the common trend assumption is verified from age 64 onward, hence allowing for a proper identification of the impact of the shift of the FRA.

Looking now at what happens after the FRA, Figure 3 indicates that the 2010 reform reduced the share of retirees among treated cohorts by 12 pp at 65. This estimation is very stable across cohorts. Likewise, the reform diminished the share of retirees among treated cohorts by 10 pp to 12 pp at 65.5, and still decreased that share by 4 pp at 66, which resulted in a reduction in the average half-year pension of about 350-400€ at 65-65.5 and of about 200€ at 66. The lower effect measured at 66 is probably due to the fact that the FRA of the 1953 cohort is 66 years and 2 months only, which attenuates our point estimates at 66.[20]

[18] A careful look at pension benefits further indicates that early 1951 cohort receives slightly lower benefits than the 1950 cohort at ages 62-62.5, though, which may be due to the increase of the reference insurance duration (confounder (i)).

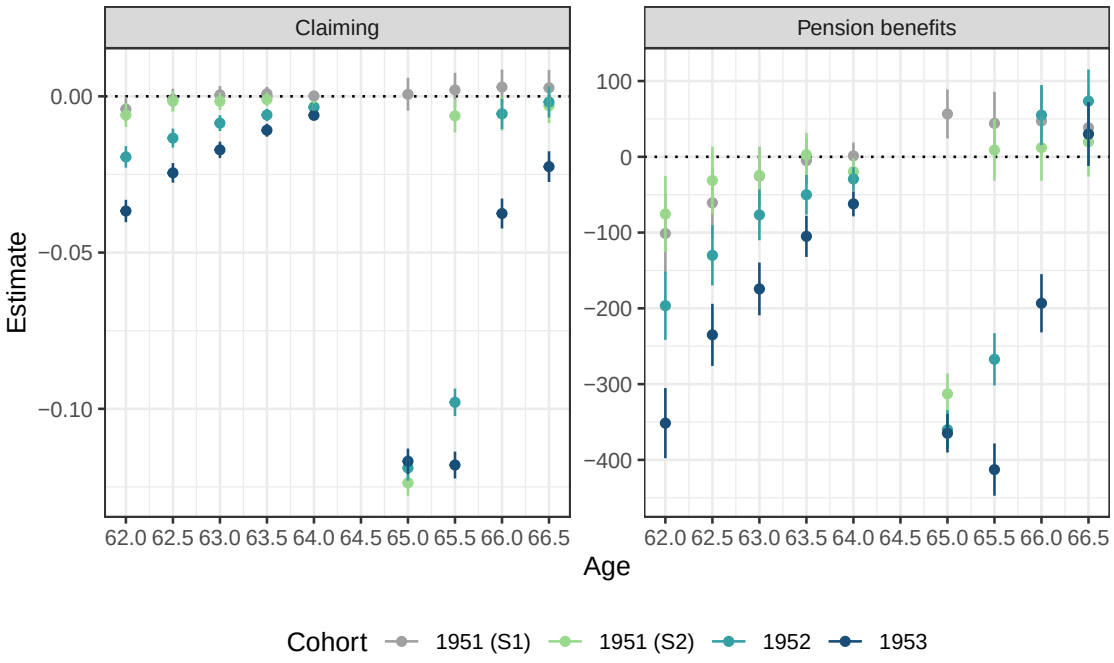
[19] Results on early 1951 cohort suggest that an increase of +1 quarter in the reference insurance duration, combined with a decrease of 0.125 pp of the *décote* penalty, does not have any significant effect on retirement behavior. It cannot be excluded, though, that a larger increase (say, by +2 or +3 quarters) might have an effect.

[20] The estimated effect at age a is based on the main state on the labor market between age a and age $a+1$ at the next semester. Because the FRA of the 1953 cohort is 66 and 2 months, the major part of the $[66, 66.5]$ interval falls after the new FRA, which thus attenuates the estimate at 66 for this cohort.



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: Pensioners whose main pension scheme is CNAV.
Source: EIR 2020.

Figure 2: Age-specific share of retirees

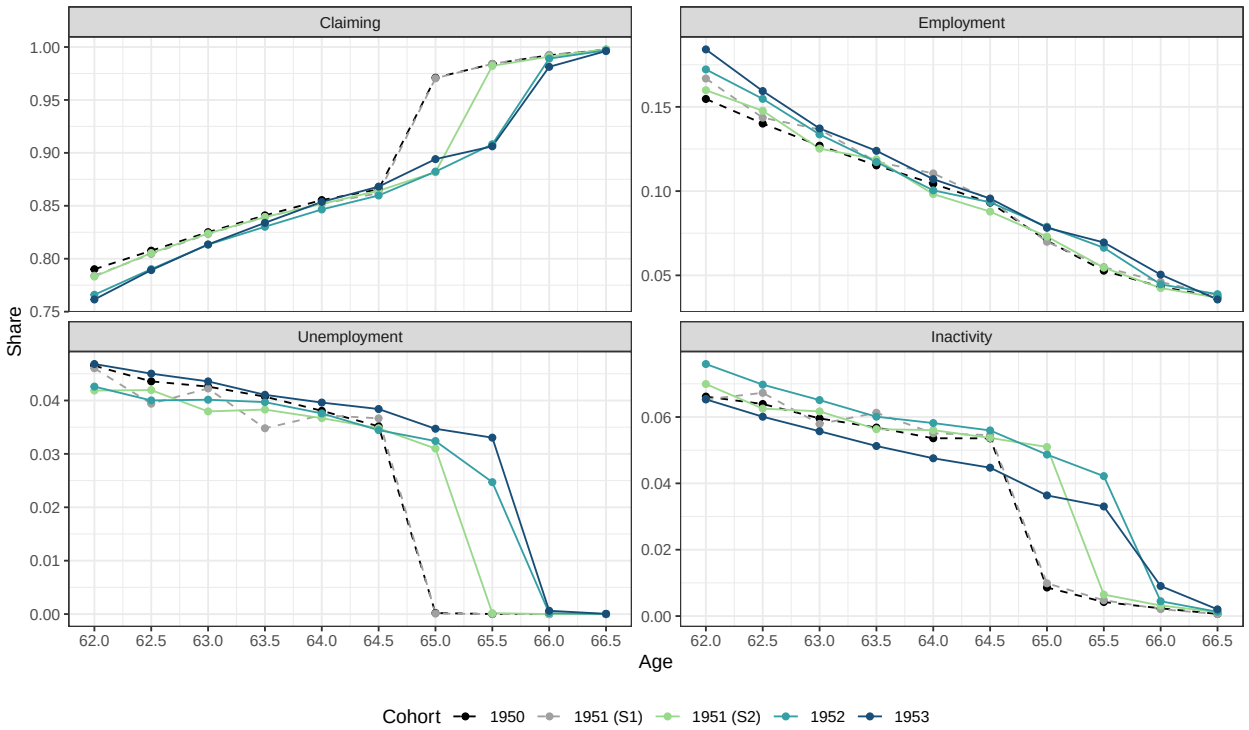


Note: The retirement pension corresponds to the gross half-year pension received.
Sample: Sample (1) (pensioners whose main affiliation scheme is the CNAV).
Source: EIR 2020.

Figure 3: Effect of the reform on retirement

5.2 Effect of the reform on employment and unemployment

In order to enlarge our assessment of the effects of the reform, and especially to detect potential substitution toward other social insurance schemes, we now restrict our analysis to sample (2), *i.e.* pensioners whose main pension scheme is the CNAV *and* who experienced at least one salaried employment spell from 52. The evolution of the age-specific share of individuals in each labor market state is displayed by Figure 4. Again, pre-trends look rather parallel between 63.5 and 64.5. Since the share of retirees increases with age, the shares of employed and unemployed individuals follow downward trends. The share of unemployed mechanically vanishes when the FRA is reached since unemployment benefits are no longer granted from that age onward.



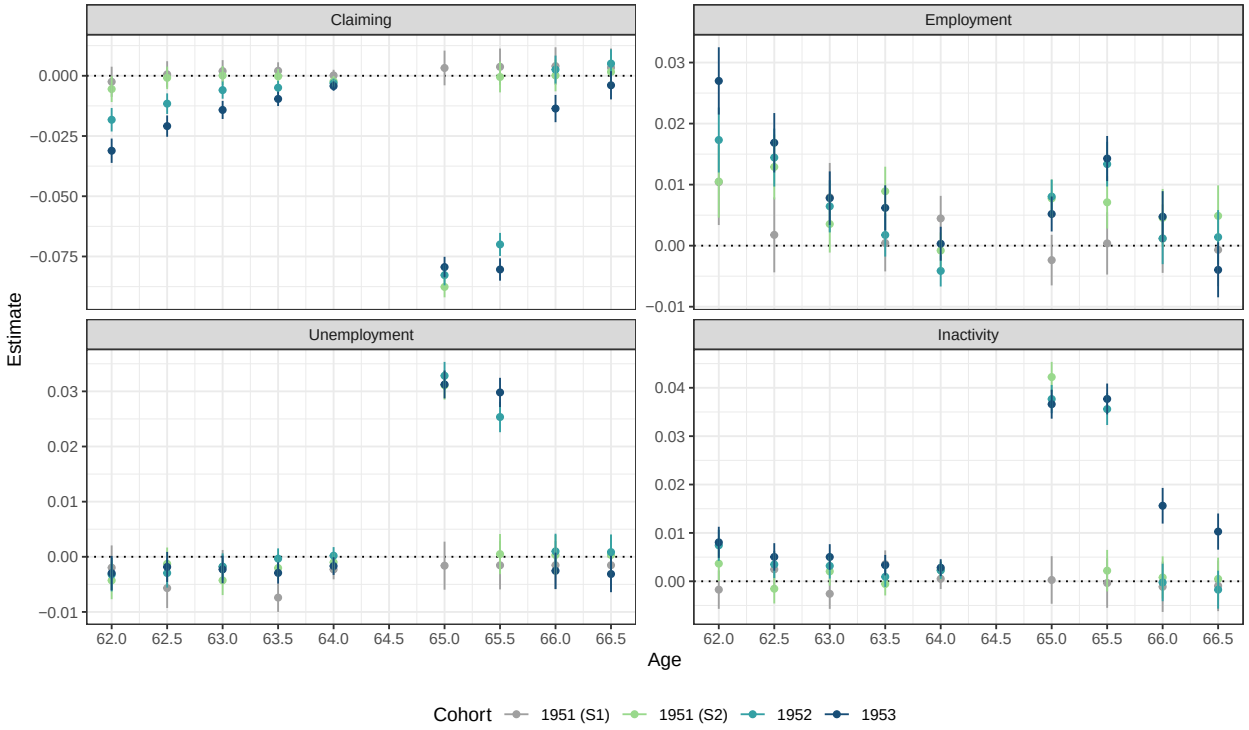
Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: Pensioners whose main pension scheme is CNAV *and* who experienced at least one salaried employment spell from 52.
Source: EIR 2020.

Figure 4: Age-specific shares of various labor market states

Figure 5 summarizes the estimated effects of the reform on each labor market state. Regarding pre-trends (before 64.5), the diagnosis is similar to that discussed above: significant differences across cohorts emerge between ages 62 and 63 (particularly for the claiming and employment states), but they look similar between 63 and 64. The presence of such pre-trends between 62 and 63 could bias our estimates at ages 65-66. Reassuringly, the robustness check restricted to cohort 1951 reveals virtually no pre-trends before age 64.5 while yielding very similar estimates at age 65 (Figure 11). Moreover, as detailed in section 5.3, the analysis of labor market transitions across states shows similar patterns across cohorts at ages 62-63

(except for employment-to-employment transitions, that are more frequent among younger cohorts).

Turning to the results after 64.5, we show that in sample (2), the reform led to a drop in retirement by 8 pp at 65. The effect is admittedly smaller than in sample (1) due to the selection criterion (namely experiencing some salaried employment spell after 52, which selects individuals that are more likely to be already retired at 64.5: 87% vs. 82% for the 1950 cohort). This 8 pp decrease in the share of retirees translates to an increase of employment at 65 of about 1 pp.[21] By contrast, unemployment increases by 3 pp at the same age. Last, inactivity increases by about +4 pp, *i.e.* half of the drop in the share of retirees.



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: Pensioners whose main pension scheme is CNAV *and* who experienced at least one salaried employment spell from 52.
Source: EIR 2020.

Figure 5: Effect of the reform on various labor market states

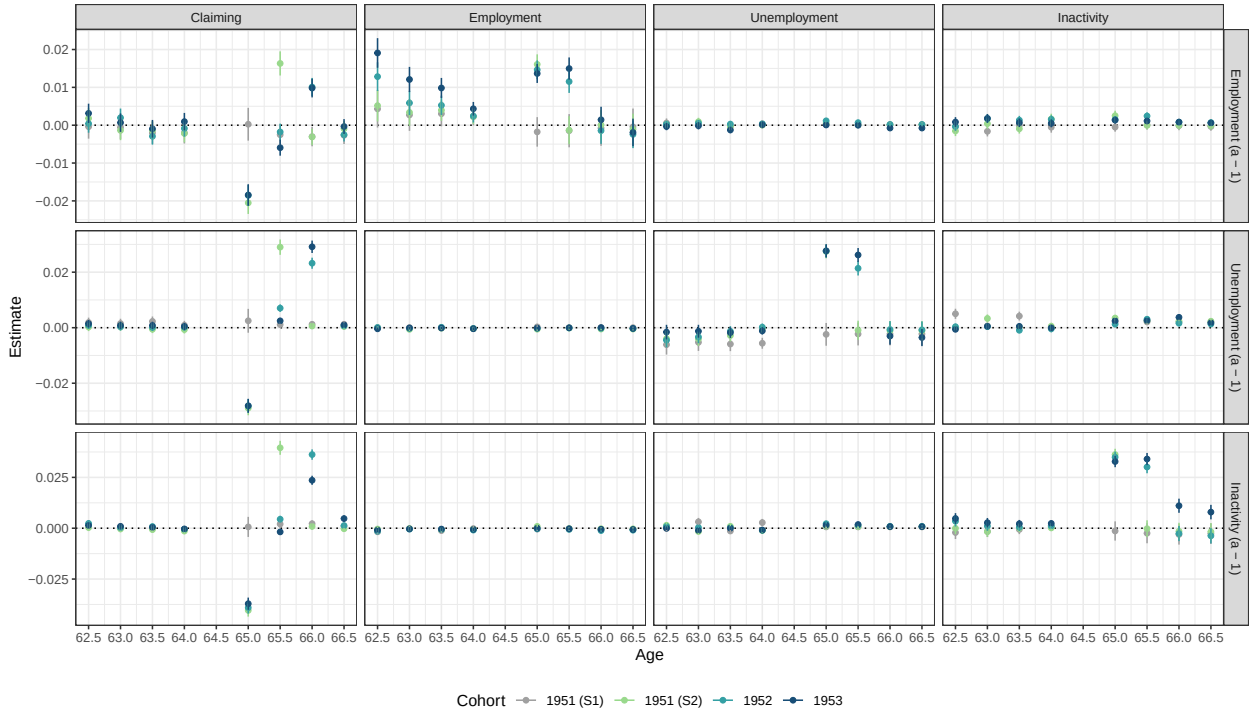
5.3 Effect of the reform on labor market transitions

To dig further into the explanatory mechanisms driving previous results, we next examine whether these effects reflect active transitions across states or individuals remaining longer in their last state on the labor market. We therefore estimate the same specification (1) as before, outcomes being now indicators of a transition on the labor market.[22] All estimations are reported by Figure 6. First, the parallel trend assumption seems to hold since most

[21] Figure C.9.1 in Appendix shows that including self-employment leads to very similar results.
[22] We here focus on transitions across (mutually exclusive) main states: retirement, inactivity, employment, and unemployment.

coefficients before 64.5 are insignificant at 5%. Transitions towards retirement aside, the sole significant coefficients are those corresponding to inertia at various FRAs, which indicates that state dependence is at stake, consistent with previous findings in the literature (Behaghel and Blau, 2012; Deshpande et al., 2024; Rabaté et al., 2024).

It is therefore likely that most unemployed or inactive individuals at 65 either failed to find a job or did not try to find one, but rather remained longer in their last state on the labor market until the new FRA. The reform thus induced a substitution of transitions from unemployment towards retirement with a higher inertia in unemployment, increasing the probability of the latter at 65 by approximately 2 pp. In the same vein, the effect of the reform on the probability of remaining inactive at 65 is +3 pp. Likewise, the sole employed individuals remained employed at 65 (now waiting to reach the new FRA), hence transitions from employment towards retirement were substituted with a higher inertia in employment. The probability of the latter increased at 65 by approximately 1.5 pp.



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Lecture: The reform induced a drop of the probability of transition from employment at age 64.5 to retirement at age 65 by 15 pp for late 1951 cohort compared to Early 1951 cohort.
Sample: Pensioners whose main pension scheme is CNAV *and* who experienced at least one salaried employment spell from 52.
Source: EIR 2020.

Figure 6: Effect of the reform on transitions

The magnitudes of the effects are similar when comparing cohorts 1952 and 1953 with late 1950 cohorts at 66. Inertia in both employment and unemployment seems quite easy to understand because individuals can postpone pension claiming - and thus avoid the penalty - while increasing their insurance duration without facing liquidity constraints (either by earning wages or, if eligible, unemployment benefits). Inertia in inactivity is less clear since

most of the concerned individuals do not experience any increase in their insurance duration, presumably. It could correspond to individuals who want to minimize the penalty by claiming their pension at the new FRA (with a supplementary cost due to foregone pension benefits before the FRA, though), or to potential beneficiaries of minimum pension benefits who need to wait for the FRA if they do not meet the required insurance duration (see section 5.5.1 for further details on this issue). Finally, a small minority could correspond to individuals who were still expecting to find a job between 65 and the FRA, or who considered that higher future pension benefits at FRA outweighed the cost of not earning a pension before.[23] Note that not all inactive individuals necessarily face liquidity constraints, as they may continue receiving welfare benefits of active ages (e.g., RSA or AAH) until they reach their new pension claiming age.[24] In any case, it would suggest that substantial labor market frictions are at stake and prevent inactive individuals from finding a job.

5.4 Fiscal assessment of the reform

We now estimate equation (1) on earnings outcomes, including pension benefits, wages, unemployment benefits, and old-age welfare benefits. Our goal is here to quantify the fiscal effect of the reform. Figure 7 shows that wages do not substantially vary after 65. The drop in retirement at 65 implies a reduction in pension benefits of about €350 per semester, but it is also associated with supplementary unemployment benefits (about €175 per semester).

Previous estimates enable us to derive the short-term financial effect of the reform. We compute it for the sole 1953 cohort, which was the most affected one.[25] The net fiscal gains per individual are obtained by summing previous DiD estimates at 65-66, and one can then multiply by the cohort size to retrieve aggregate fiscal gains. We estimate that the government saves €942 of pension benefits per retiree[26] and €25 of old-age welfare benefits (ASPA), on the one hand. On the other hand, the government spends €438 more on unemployment benefits. Assuming a uniform tax rate of 8.8% (Drees, 2025),[27] the government would then

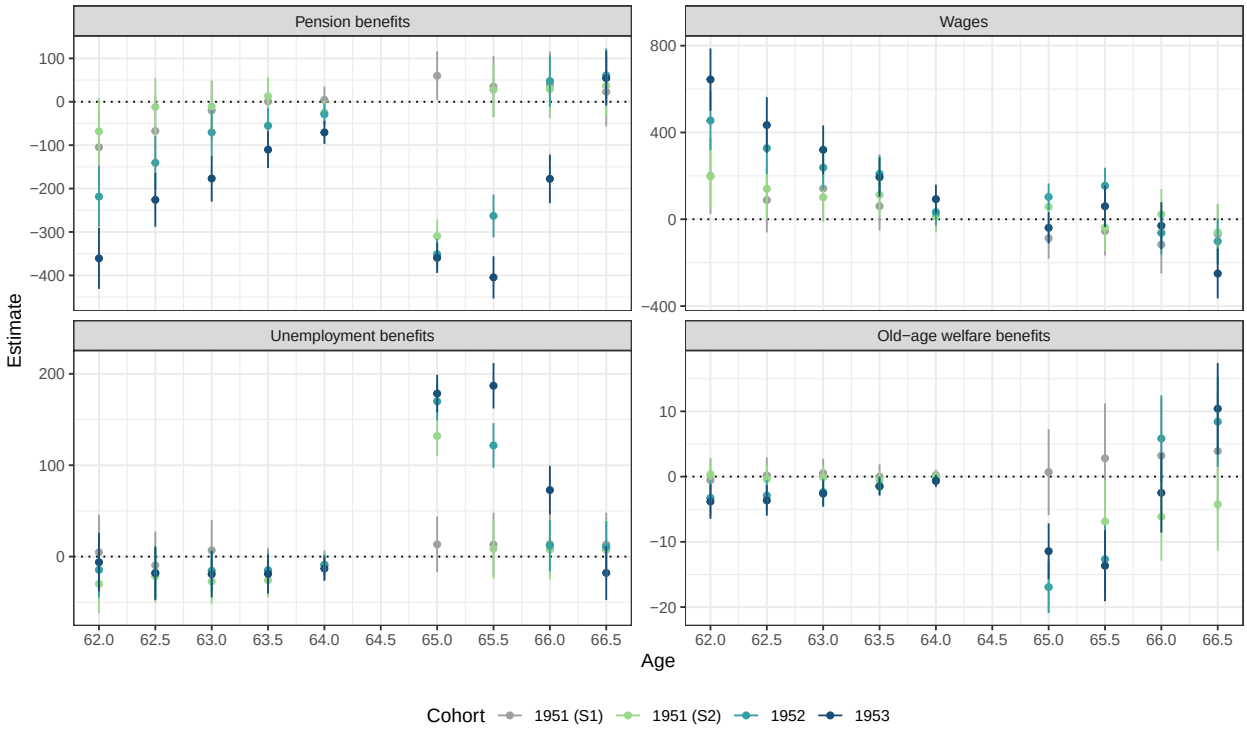
[23] In 2021, the number of unemployed aged 65 and over - as measured by the Labor Force Survey, thus corresponding to individuals actively looking for a job - is only 19,000 (representing less than 0.5% of all individuals aged between 65 and 69).

[24] Figure A.0.1 in Appendix seems to rule out such behavior for the AAH beneficiaries, but does not allow to conclude for RSA beneficiaries.

[25] The reform postponed the FRA by 2 years (65 to 67), but we can only estimate the effect of the shift from 65 to 66 and 2 months. Under the assumption of homogeneous treatment effects over age, the total effect of the reform is obtained by inflating previous estimates by a factor equal to $\frac{24}{14} \approx 1.71$.

[26] This estimate includes second-pillar pension benefits, which are not *stricto sensu* part of the government budget. We choose to include them in our fiscal assessment of the reform because they are part of the French pay-as-you-go (PAYG) system.

[27] We here consider the sole *Contribution Sociale Généralisée* (CSG) and *Contribution au Remboursement de la Dette Sociale* (CRDS) social contributions viewed as income taxes with 8.3% and 0.5% respective tax rates, assuming a 0% labor income tax rate as individuals retiring at the FRA are poorer than the average, due to more fragmented careers (in France, only 45% households are effective labor income tax payers, see Péquignot et al., 2025). Note that this simplifying assumption leads us to overestimate the fiscal gain of the reform, because we omit (and thus neglect) the loss of income tax that is induced by a lower provision of pension benefits.



Note: Early 1951 cohort is composed of individuals born between January and June 1951. Various earnings correspond to gross half-year earnings.
Sample: Pensioners whose main pension scheme is CNAV *and* who experienced at least one salaried employment spell from 52.
Source: EIR 2020.

Figure 7: Effect of the reform on earnings

have saved $(1 - 0.088) \times (942 + 25 - 438) = 482$ euros per person. Because we are unable to detect any significant effect on (gross) wages, we assume that the reform did not allow any fiscal gain resulting from labor income taxation. Overall, because the 1953 cohort is composed of 555,397 individuals, we conclude that the annual net fiscal gain of the reform is €268 m. Section A in Appendix suggests that taking into account all other welfare benefits is unlikely to change much this estimate. The only potential additional expenditures induced by the reform that we do not measure are additional health expenditures due to sick leave (Caroli et al., 2023; Ben Halima et al., 2022), but we expect little effect on this margin given the low employment rates at those ages.

5.5 Heterogeneity of effects

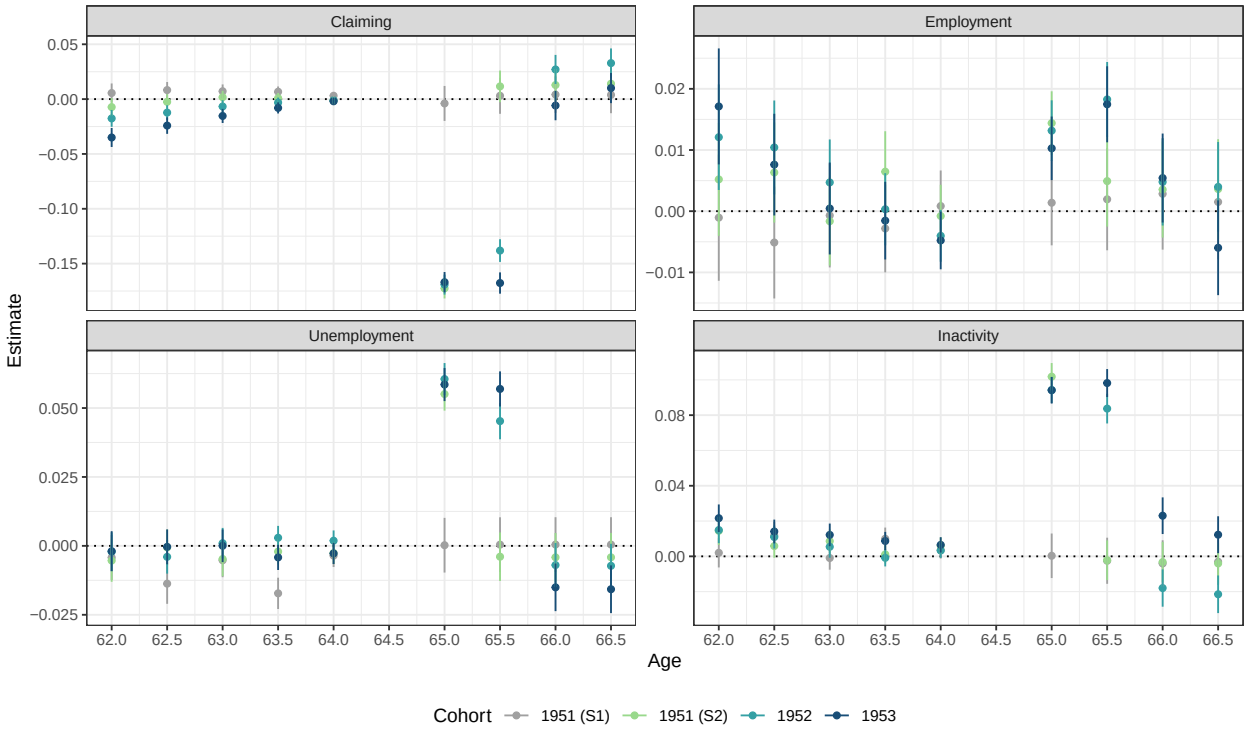
To investigate which mechanisms possibly drive the previous findings, we look at various dimensions of heterogeneity. Among the typical explanations proposed in the literature including financial incentives, joint retirement decisions within the household, employer-side behavior, social norms, we find that financial incentives do play a role, but also that this role is somehow modest in the sense that other explanations seem to prevail. We find a limited role for the spousal channel, possibly due to the fact that the shift in the FRA does not prevent individuals from retiring before that age, contrary to a shift in the ERA.

5.5.1 Minimum pension benefit

Previous empirical evidence suggests that the shift of the FRA had large effects on retirement behavior, which is interesting *per se*: contrary to what happens with the shift of the ERA that made it legally impossible to claim pension benefits before the new age, individuals may still retire before the FRA. As discussed in section 3, (strong) financial incentives to retire at the FRA for individuals eligible to the MICO minimum pension benefit may explain (part of) our findings.[28] To investigate this channel in more details, we further estimate equation (1) separately for individuals who receive the MICO and for individuals who do not.

Figure 8 unambiguously shows that the effects on retirement, unemployment, and inactivity are much more pronounced for those who benefit from the MICO. The probability of being retired (resp. unemployed and inactive) at 65-65.5 is about 17 pp lower (resp. 6 pp and 10 pp higher) for treated cohorts than for control cohorts. As was the case before, the probability of being employed is not much higher, though: the estimated coefficient related to employment turns out to be very close to the one obtained on the whole sample. Indeed, MICO beneficiaries are much less likely to be employed at ages 62-64.5 (the employment rate at these ages being 8.9% only) than the rest of the population (that employment rate amounts to 14.5%). MICO beneficiaries are more often women with shorter careers, including many non-employment spells, which makes them less prone to participate to the labor market at older ages (Drees, 2024). To sum up, even if the reform markedly increased their financial incentives to postpone retirement, it mostly resulted in a transition toward other social insurance schemes rather than to employment.

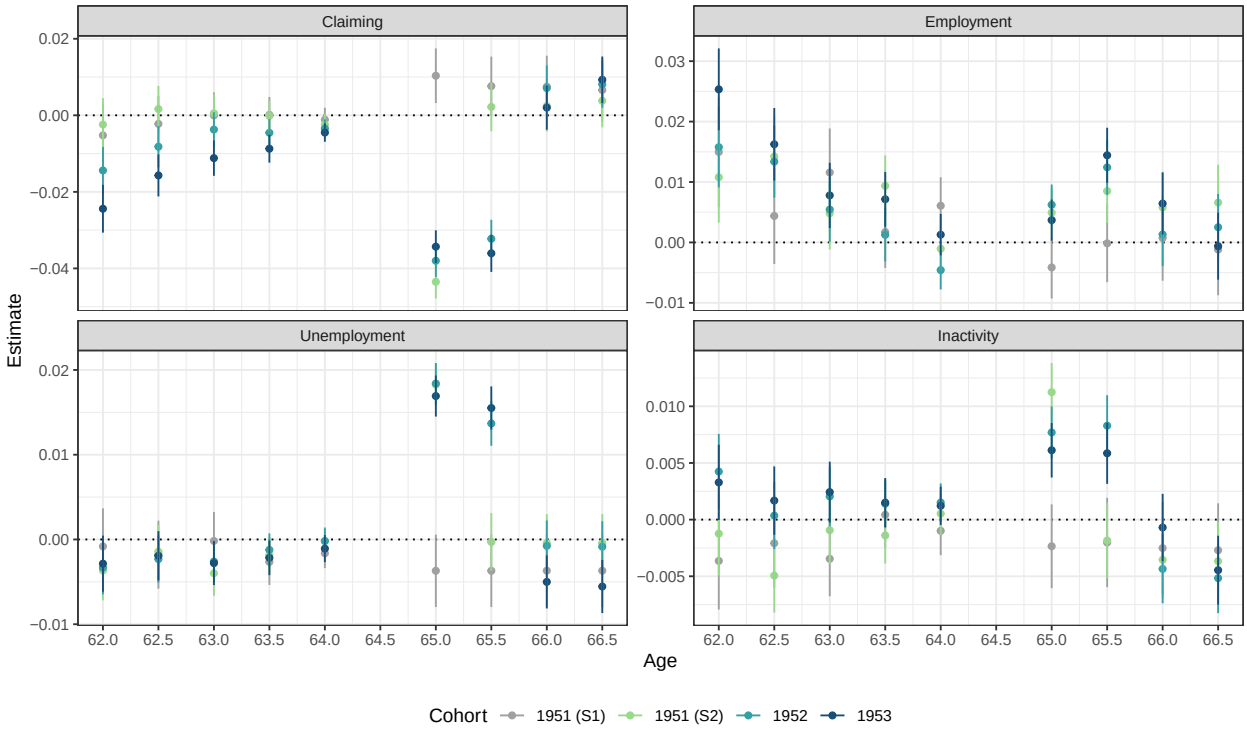
[28] Section B in Appendix illustrates the different incentives of MICO vs. non-MICO pensioners using representative individuals from each group.



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: MICO pensioners whose main pension scheme is CNAV *and* who experienced at least one salaried employment spell from 52.
Source: EIR 2020.

Figure 8: Effect of the reform on the labor market state (MICO pensioners only)

However, substantial effects of the shift of the FRA are also observed in the subsample of pensioners who do not benefit from the MICO (Figure 9). The share of retirees falls by around 3.5 pp, the share of employed individuals increases by nearly 1 pp, and the share of unemployed (resp. inactive) increases by more than 1.5 pp (resp. 0.5 pp). On this subsample excluding MICO beneficiaries, the sole discontinuity that may arise at the FRA is the loss of eligibility to unemployment benefits. However, the share of individuals retiring at the FRA following some unemployment spell represents only one third of observed retirements at the FRA (see Figure C.2.1 in Appendix). Interestingly, this empirical evidence suggests that financial incentives are not the sole driving mechanism behind retirement at the FRA, which leaves room for other explanations (including behavioral ones such as reference-dependence (Behaghel and Blau, 2012; Seibold, 2021) or peer-effects (Oral et al., 2024)).



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: Pensioners whose main pension scheme is CNAV *and* who experienced at least one salaried employment spell from 52, at the exclusion of MICO beneficiaries.
Source: EIR 2020.

Figure 9: Effect of the reform on the labor market state (excluding MICO beneficiaries)

Overall, the heterogeneous effects observed between MICO and non-MICO beneficiaries have important implications for the distributional impact of the reform. Indeed, by definition, MICO beneficiaries are the poorest retirees.[29] To provide a broader assessment of the distributional consequences of the reform, we estimate our cohort event-study separately for each quartile of reference wage, used as a proxy for lifetime earnings. Figure C.7.2 in Appendix shows that the effects of the reform are concentrated in the bottom quartile of reference wage, while they are negligible in the top 50%. Table D.0.3 in Appendix shows that most of the fiscal effect of the reform (both the decrease of pension benefits and the increase of unemployment benefits) can be attributed to the bottom 50%. These results suggest that the reform generated modest fiscal savings borne almost entirely by lower-income individuals. Finally, Figure C.6.1 in Appendix show that retirees who experienced unemployment spells between ages 50-56 were more affected by the reform compared to those who did not experience unemployment spell.

5.5.2 Old-age welfare benefits and stickiness at 65

Beyond the shift of bunching in retirement behavior from 65 to 66 and 2 months between 1950 and 1953 cohorts, Figure 1 shows that the share of individuals retiring at the FRA is

[29] At least at the individual level, since we do not have data at the household level.

slightly decreasing over cohorts from 11.6% for the 1950 cohort to 9.1% for the 1953 cohort.[30] Conversely, we observe some stickiness in the retirement behavior at 65, which is mostly driven by ASPA old-age welfare beneficiaries whose eligibility starts at 65 (see Figure C.8.1). Surprisingly, such a stickiness at 65 is *increasing* over cohorts. The retirement age of ASPA old-age welfare beneficiaries is first shifting almost perfectly with the FRA, with very few retirements observed at 65 (2.2% for the late 1951 cohort). Yet it is rising again: 4.3% for the 1952 and 8.1% for the 1953 cohort. This pattern could reflect some misunderstanding of the eligibility age for ASPA a few years after the reform was implemented, or an increase in the relative value of ASPA for younger cohorts following the upward revision of its amount in 2018-2020.[31] Indeed, the behavior of old-age welfare beneficiaries of 1952-1953 cohorts suggests that they might have incentives to retire earlier than FRA and to apply for ASPA at 65, rather than to wait until the FRA to get the MICO minimum pension benefit. For a single person retiring at 65 in 2017 (hence belonging to the 1952 cohort), the ASPA is worth €803 monthly, which is higher than the MICO (€694 monthly[32] for a complete career). This could explain why 85% of old-age welfare beneficiaries from the 1950 cohort who retire at 65 or after also claim for a MICO minimum pension benefit, against 52% only for the 1953 cohort.[33] Even if these conflicting financial incentives are likely to lower the effect of the shift of the FRA, we believe that their impact is somehow modest because less than 5% of a cohort receives some old-age welfare benefit.

5.5.3 Gender and marital status

Departing now from the role played by financial incentives, we next seek to examine whether previous effects differ according to individual sociodemographic characteristics like gender and marital status.

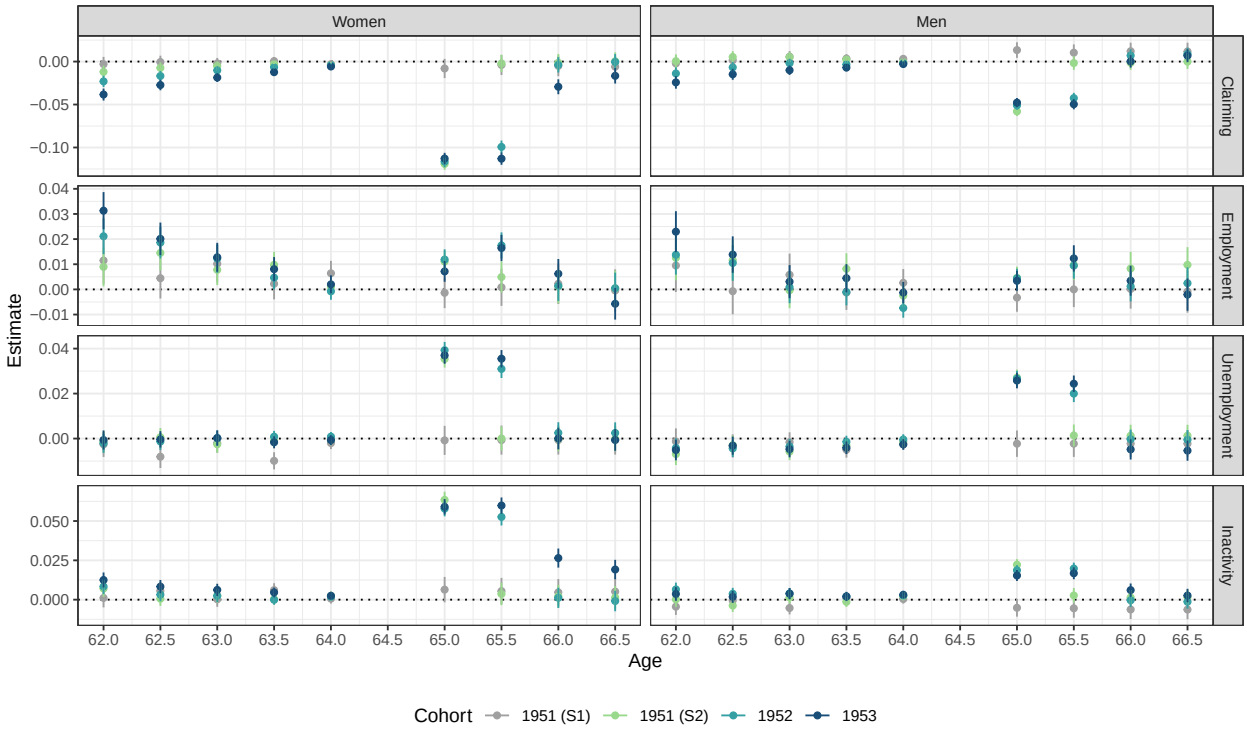
Men and women experience different trajectories on the labor market, and might thus be differently affected by the reform. Figure 10 displays our estimates obtained separately for men and women, and indicates that the reform had a higher impact on women. While the probability of retirement at 65 drops by 5 pp for men, it drops by about 12 pp for women. This result is somehow expected from Figure C.4.1 which shows that the distribution of the retirement age is more concentrated around the FRA for women, most likely because women have more often experienced incomplete careers. This more substantial drop in the probability of retiring at 65 is also accompanied with a higher increase in the probability of being unemployed (+3.5 pp vs. +2.5 pp for men) or inactive (+6 pp vs. +2 pp for men) at that age. The increase in the probability of employment at 65 is less substantial: nearly

[30] Because the FRA of the 1953 cohort is 66 and 2 months, we aggregate retirements at 66 (2.5%) and 66.25 (6.6%) for this cohort.

[31] The amount of ASPA was increased progressively by +100€ (+12.5%) between April 2017 and January 2020.

[32] This amount does not include the second-pillar pension benefit.

[33] Note that this is not driven by the stricter MICO eligibility rules enforced from 2012 onward. These rules already applied to individuals from the 1950 cohort who were retiring at 65.



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: Pensioners whose main pension scheme is CNAV *and* who experienced at least one salaried employment spell from 52.
Source: EIR 2020.

Figure 10: Effect of the reform on the labor market state according to gender

+1 pp for women vs. +0.5 pp for men (+1.7 pp vs. +1 pp at 65.5). These results concur with the descriptive evidence of Figure C.5.1, which shows that women are more likely to be unemployed or inactive at these ages, and slightly less likely of being employed. Inertia in the last state on the labor market seems thus to be more detrimental for women since a higher share of them remains inactive while waiting to reach the FRA.

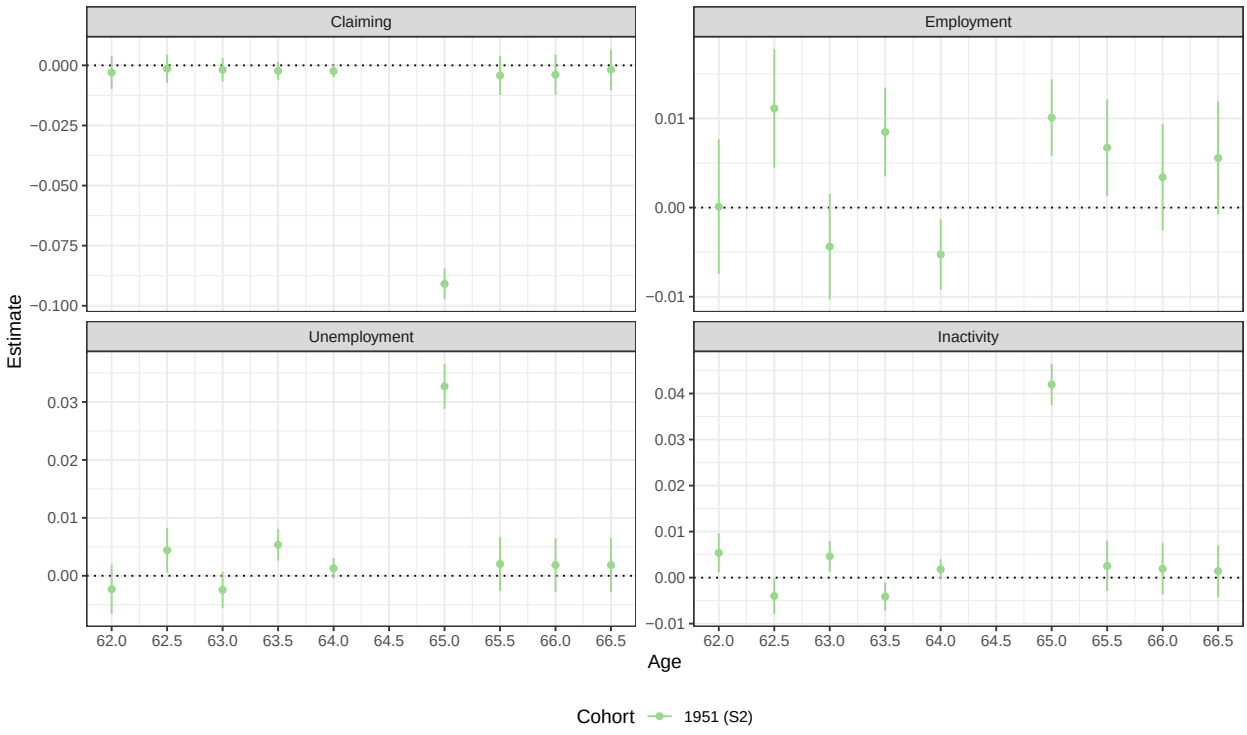
We have also investigated whether previous effects differ according to the marital status, namely the fact of being in a couple (being married or engaged in some union like the *PActe Civil de Solidarité* (PACS) or another informal union) as opposed to being single, widowed, or separated. We do not find substantial heterogeneity along this dimension, though. Couples tend to retire more often before 62, if anything, and are thus less impacted by the reform. They incur a slightly lower effect of -8 pp on their probability of retiring at 65, to be compared with -9 pp for individuals who are not in a couple at that age (corresponding estimates are available upon request). Overall, we fail to detect any conclusive evidence as regards the role of joint retirement decision within couples, partly because we lack more granular data matching individuals within households.

6 Robustness checks

We here provide with three robustness checks related to (i) the interval validity of the identification strategy, (ii) the sensitivity to period-effects and (iii) statistical issues.

6.1 Internal validity

A threat of identification could be related to the presence of confounders (i) and (ii) and to period effects. To alleviate this concern, we focus on a simple comparison between early and late 1951 cohort (who are presumably affected by the same period effects), the former being viewed as the control group and the latter as the treated group. Note that this alternative identification strategy resembles to a regression discontinuity design around July 1st, 1951. Under the continuity assumption, a direct comparison between these (semi-)cohorts yields the desired causal effect. Implementing this empirical strategy, Figure 11 actually confirms that the previous estimated effects of the reform on labor market states are close to those obtained here: -8 pp for the probability of retiring at 65, +1 pp (resp. +3 pp, +4 pp) for the probability of being employed (resp. unemployed, inactive) at this age.



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: Pensioners whose main pension scheme is CNAV *and* who experienced at least one salaried employment spell from 52.
Source: EIR 2020.

Figure 11: Effect of the reform on the labor market state (early vs. late 1951 cohort)

6.2 Period-effects

Another way to control for period-effects would be to directly include period dummies in equation 1. However, as mentioned earlier, age, period and cohort (APC) effects cannot be separately identified because of perfect collinearity. A solution proposed by Deaton (1997) consists of replacing the period dummies with transformed period dummies whose coefficients sum to zero and are orthogonal to a linear time trend, breaking the perfect collinearity with age and cohort dummies. This solution allows to capture cyclical fluctuations that average zero in the long-term, and is better suited for studies with a longer time window. In our setting, the analysis follows cohorts born between 1950 and 1953 from ages 62 to 67, corresponding to calendar years 2012–2020, which provides only limited variation to identify period effects. Moreover, our specification includes age-by-cohort interaction dummies rather than additive age, period, and cohort effects, absorbing substantially more variation than the standard APC model and making the separate identification of period effects even more challenging.

Figures C.3.1 and C.3.2 in Appendix report the results obtained using the Deaton (1997) method. We see that coefficients before 64.5 are less precisely estimated and that pre-trends between 62 and 63 become more pronounced. Reassuringly, however, the estimated effects from age 65 onward are very similar to those obtained in our baseline specification, although they are estimated less precisely.

6.3 Statistical issues

Though our main specification weighs observations with sample weights, we also assess whether the absence of weighting (meaning that each observation receives the same weight) alters our conclusions. Reassuringly, previous results hardly vary without weighting (see Figure C.11.1 in Appendix).

7 Conclusion

In this article, we have shown that the increase of the FRA implemented by the 2010 reform led to a 8 pp drop in the probability to having claimed a pension at 65-66, but also that it had little impact on employment at these ages (around +1 pp), while increasing the probability of being unemployed (+3 pp) or inactive (+4 pp). We have hereby documented that substitution toward other social insurance schemes than pensions limits the cost-effectiveness of such a reform. We estimate that half of the fiscal gain arising from fewer and smaller pension benefits is offset by more unemployment benefits, in particular. This crowding-out effect is *underestimated* since potential additional costs related to supplementary health expenditures are ignored.

Although the design of the reform does not allow us to fully disentangle the effect of the in-

crease in the FRA from simultaneous changes to the pension system and period effects across all affected cohorts, the stability of our results when restricting the analysis to the 1951 cohort lends credibility to our identification strategy. Further research should concentrate to better identify the underlying mechanisms that are the source of the observed findings, including a proper disentangling of financial incentives, social norms, employer-side explanations, joint retirement decisions, etc.

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Appendix

A Additional welfare benefits

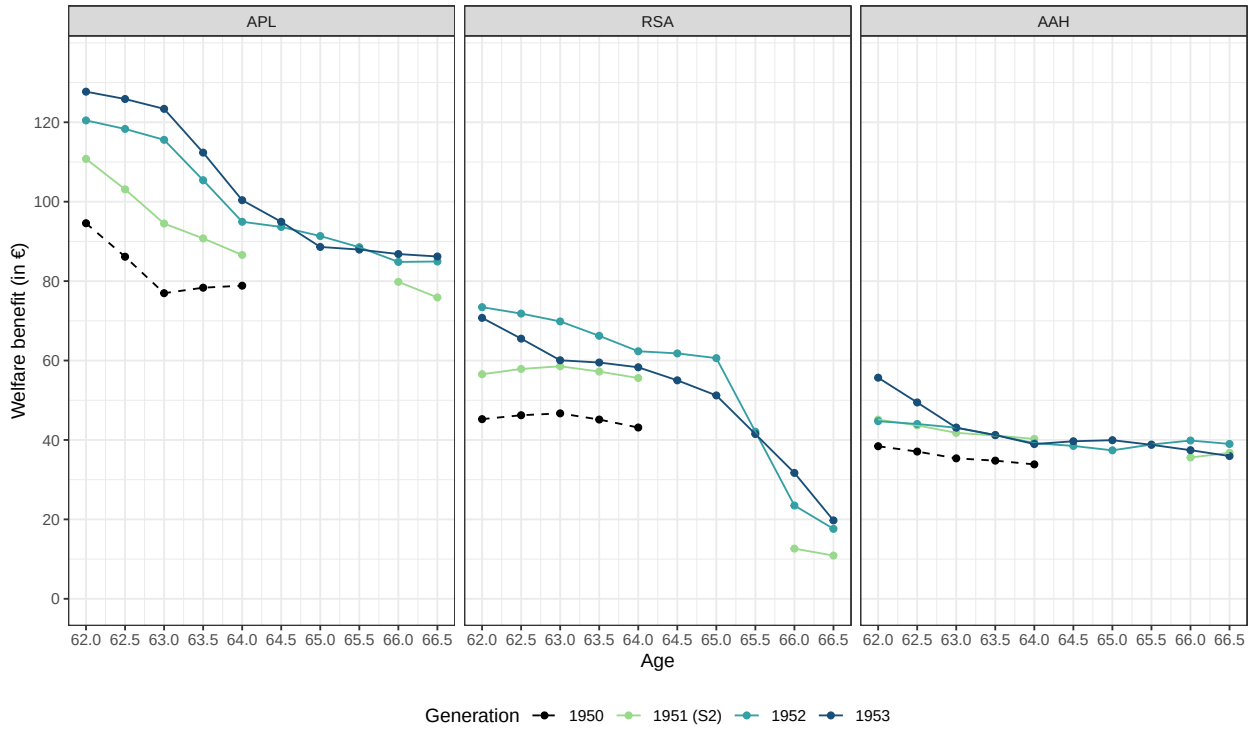
As mentioned before, welfare benefits specifically targeted to old-age people (*ASPA*) are already observed in the *EIR*. The *Eniacrams* database complements the *EIR* by providing information on welfare benefits of "active ages" (AAH, RSA, APL). However, these benefits can also be granted at older ages, so it is necessary to take them into account to assess the overall fiscal effect of the reform.

One difficulty arising with this data is that beneficiaries are not observed at ages greater than 65 until 2017. It means that for cohort 1950 (resp. 1951), we lack data at 65-66 (resp. 65).[34] We are therefore not able to identify the causal effect of the reform on these welfare benefits because our control and early-treated cohorts are censored at treated ages.

Nevertheless, we can still compare cohorts 1952 and 1953 (and cohort 1951 from 66) to get a sense of how welfare benefits' amounts differ between cohorts experiencing different FRA shifts. Figure A.0.1 shows that there is almost no difference between cohorts 1952 and 1953 from 64 to 66.5 for disability allowance (AAH) and housing benefits (APL), and only small differences at 64-65 for minimum income (RSA). The comparison with late 1951 cohort, though censored for the most relevant ages (64.5-65.5), also suggests that AAH and APL trends were unaffected by the reform. The only benefit whose trend could have been shifted by the reform would be the RSA, even though this is somewhat unlikely given that the minimum age for applying to old-age welfare benefits (ASPA) - that is supposed to replace the RSA - was kept at 65. Taken together, these results suggest that the fiscal estimate computed in section 5.4 by ignoring these additional benefits is likely to be reasonably accurate.[35]

[34] More precisely, because the *Eniacrams* only provides information on the welfare benefit status on December 31st of each year, we assume that the benefit period lasts one year and is centered on this date. It means that the data is (at least partially) censored between 64.5 and 66.5 for cohort 1950 and between 64.5 to 65.5 for cohort 1951.

[35] The only potential additional expenditures that we cannot measure are those related to sick leave, but we expect them to be small given that the employment rate is low at those ages.



Note: All pensioners belonging to the *Eniacrams* sample are born in October, so early 1951 cohort is not observed. Cohort 1950 is not observed from age 64.5 onwards. Cohort late 1951 is not observed between age 64.5 and 65.5.
"APL" are housing benefits; "RSA" are minimum income benefits; AAH are disability allowance.
Sample: Pensioners belonging to the *EIR* and *Eniacrams* sample.
Source: EIR-Eniacrams 2020.

Figure A.0.1: Additional welfare benefits

B Incentives to retire at FRA between MICO and non-MICO beneficiaries

In order to make the financial incentives associated with the shift of the FRA more concrete, we consider two typical cases of cohort 1953, representing both MICO and non-MICO beneficiaries. Among each group (MICO and non-MICO retirees claiming at the FRA), we consider a representative individual who would have the career of the "median retiree" (i.e. the median number of validated quarters D and the median reference wage W). We then compute two fictitious pensions: the pension that would be granted if the individual was claiming at 65 (i.e. at the old FRA, reflecting a sticky behavior) and the pension that would be granted if the individual was claiming at 66 and 2 months (i.e. at the new FRA, reflecting full adjustment). We assume that no additional quarter is validated between 65 and 66, meaning that we estimate a lower bound of the financial incentives to postpone claiming. Table B.0.1 shows that the MICO representative individual has a huge incentive to postpone claiming, because it would allow to increase their pension by +186€ (+60%). Conversely, for the non-MICO representative individual, the incentive is modest (+42€, representing +5%).

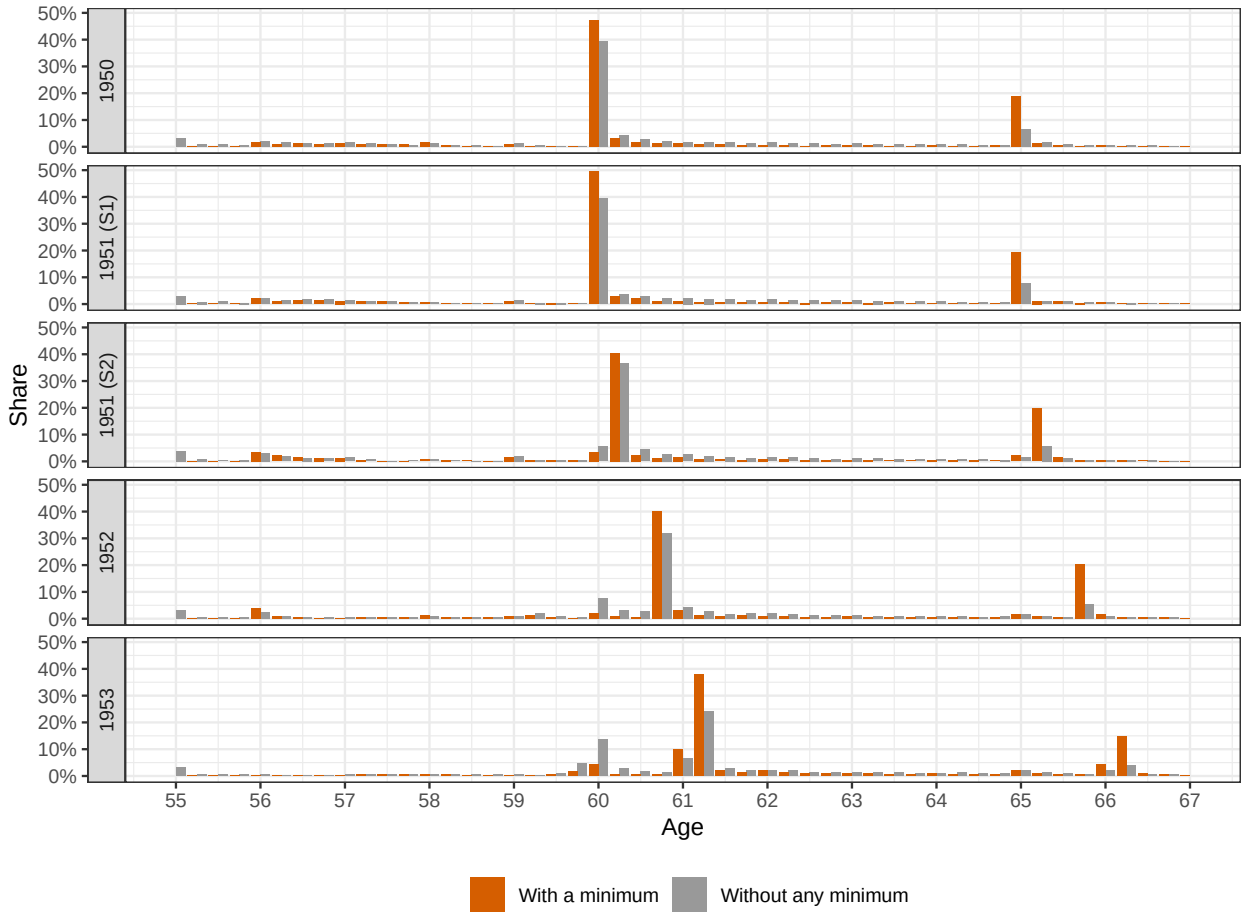
	MICO	Non-MICO
Number of contributed quarters	75	125
Number of validated quarters (D)	128	153
Yearly reference wage (W)	€10,000	€21,200
Yearly pension benefits at 65	$0.475 \times W \times (D/165) = €3,690$	$0.475 \times W \times (D/165) = €9,350$
Yearly pension benefits at 66 and 2 months	$B_{\text{MICO}} \times (D/165) = €5,930$	$0.5 \times W \times (D/165) = €9,840$
Monthly pension benefits at 65	€308	€779
Monthly pension benefits at 66 and 2 months	€494	€821
Number of observations	1,003	843

Note: The descriptive statistics were computed on a subsample of sample (2) (pensioners whose main affiliation scheme is CNAV *and* having experienced at least one wage employment spell from their 52), where indivudals are from the 1953 cohort and claimed their pension at FRA. Individuals for which the reference wage was not declared were removed. The number of contributed quarters, validated quarters and the yearly reference wage are medians computed on the sample.
Source: EIR 2020.

Table B.0.1: Estimated pension benefits for MICO and non-MICO retirees

C Additional figures

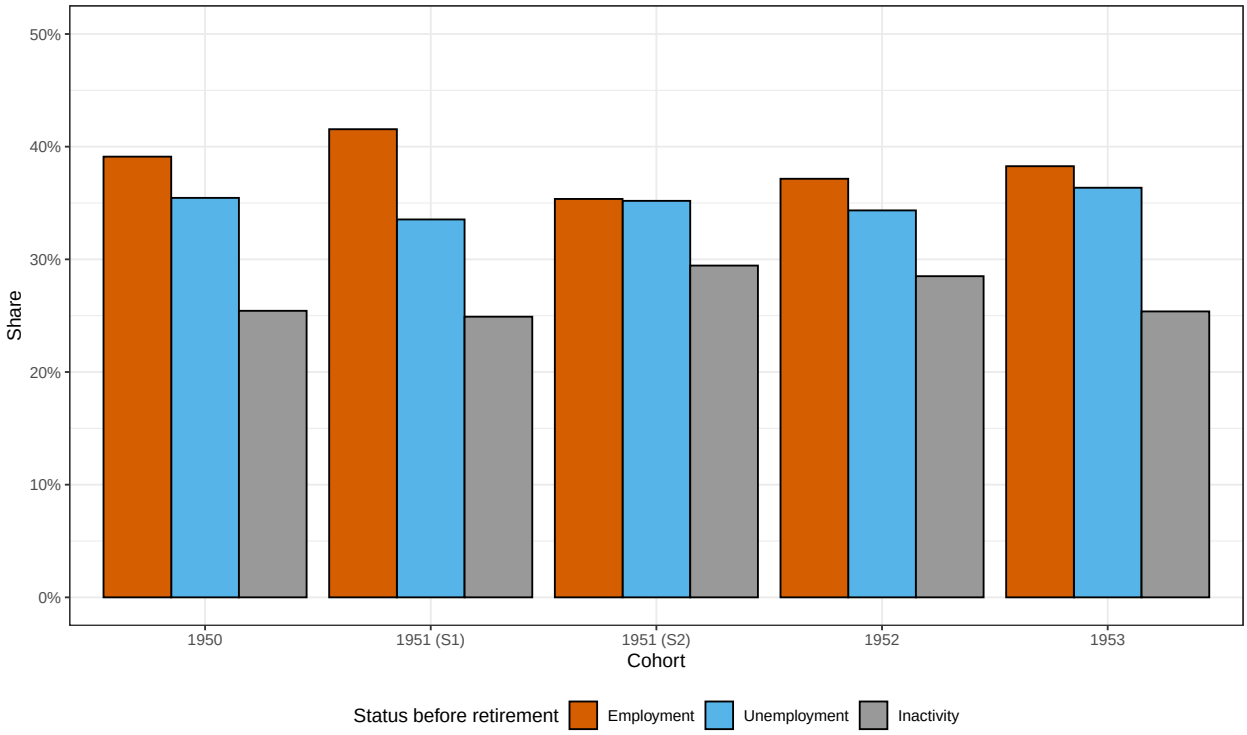
C.1 Heterogeneity of retirement age: contributory minimum pension



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: all pensioners.
Source: EIR 2020.

Figure C.1.1: Heterogeneity of retirement age according to being beneficiary of a minimum pension

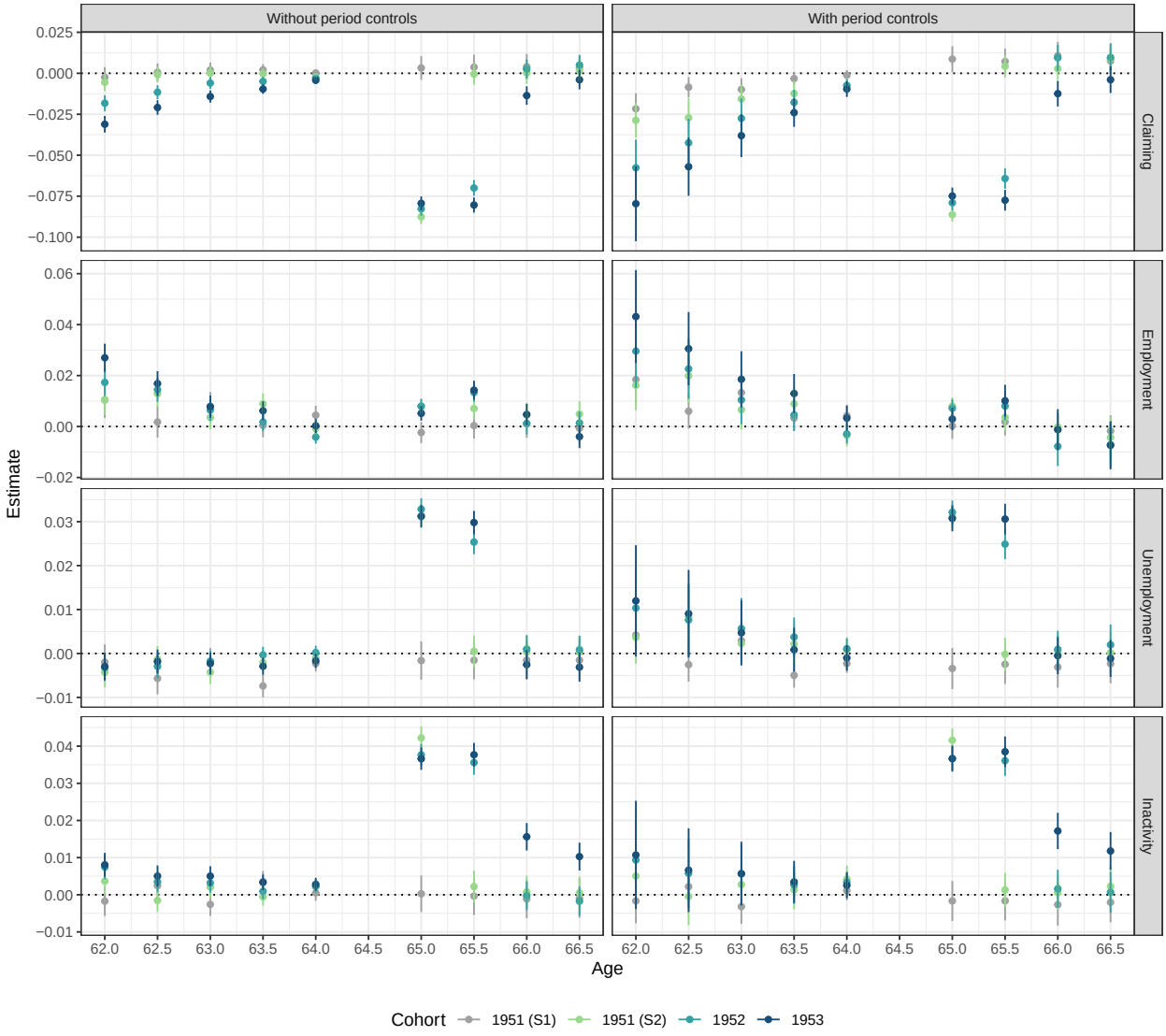
C.2 Retirement at the FRA for non-MICO pensioners according to the last status



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
The "status before retirement" corresponds to the observed main status during the six-months age preceeding retirement.
Sample: Non-MICO pensioners whose main affiliation scheme is the CNAV, who have experienced at least one wage employment spell from their 52 and who retire at the FRA.
Source: EIR 2020.

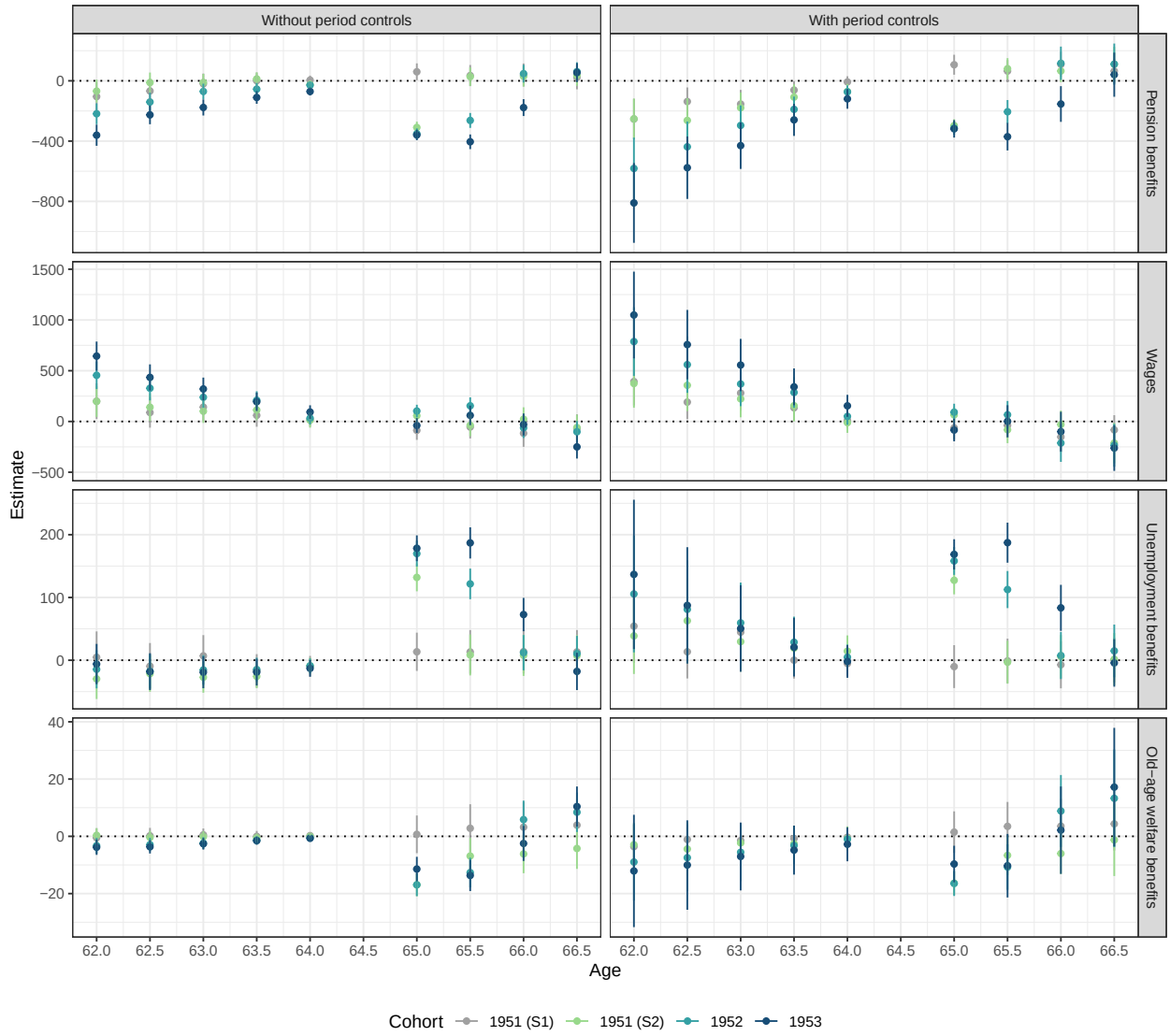
Figure C.2.1: Distribution of last status for non-MICO pensioners retiring at the FRA

C.3 Adding period controls



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: Pensioners whose main affiliation scheme is the CNAV *and* who have experienced at least one wage employment spell from their 52 *or* a self-employment spell after 56.
Source: EIR 2020.

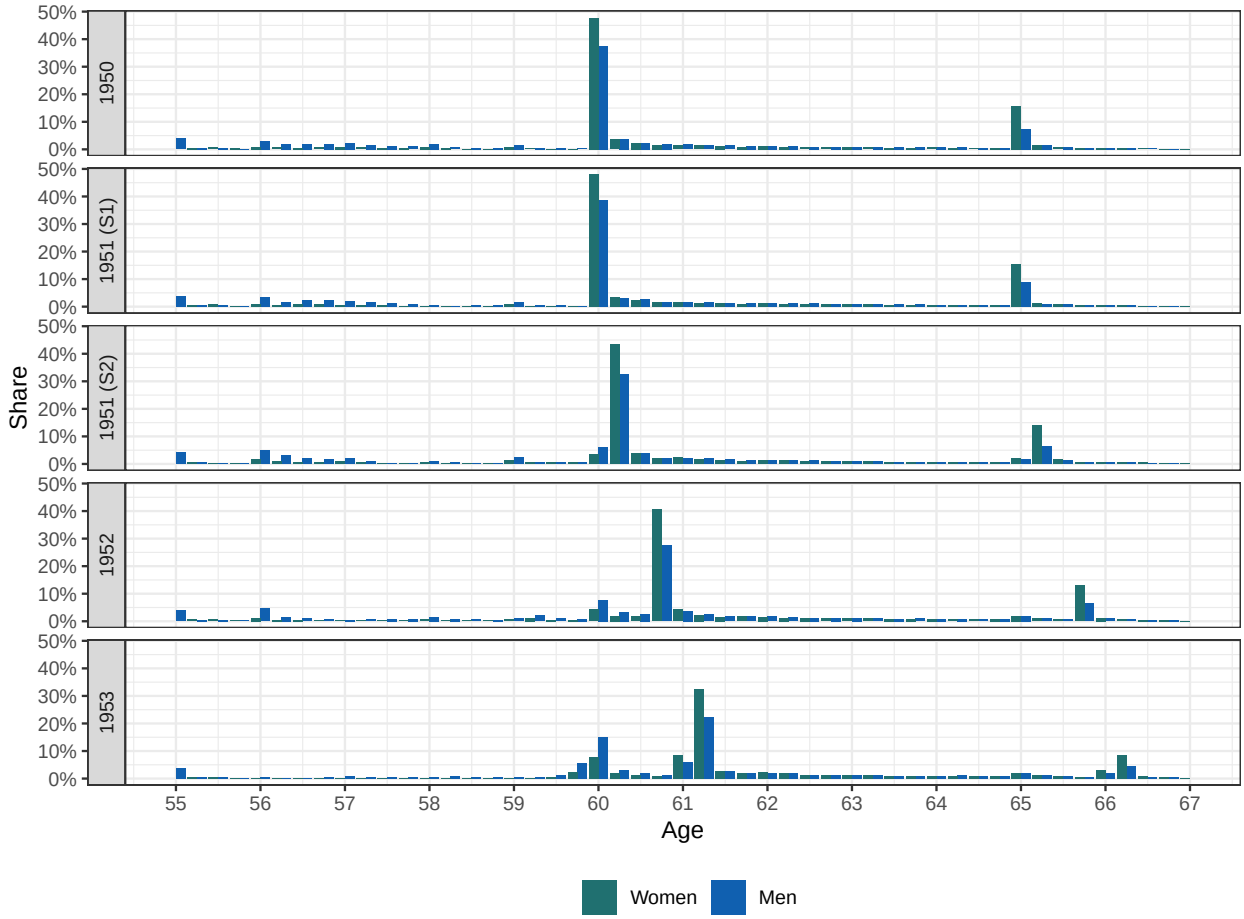
Figure C.3.1: Effect of the reform on various labor market states (including period controls)



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: Pensioners whose main affiliation scheme is the CNAV *and* who have experienced at least one wage employment spell from their 52 *or* a self-employment spell after 56.
Source: EIR 2020.

Figure C.3.2: Effect of the reform on earnings (including period controls)

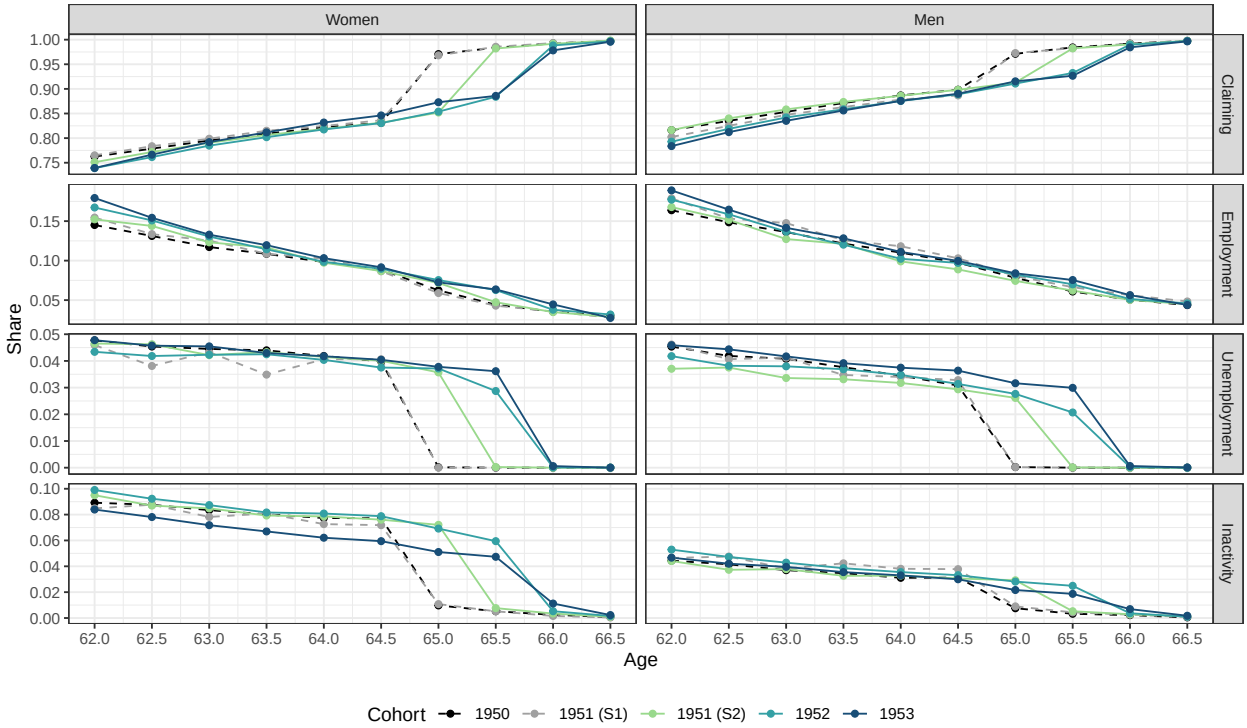
C.4 Heterogeneity of retirement age: gender



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: all pensioners.
Source: EIR 2020.

Figure C.4.1: Heterogeneity of retirement age according to gender

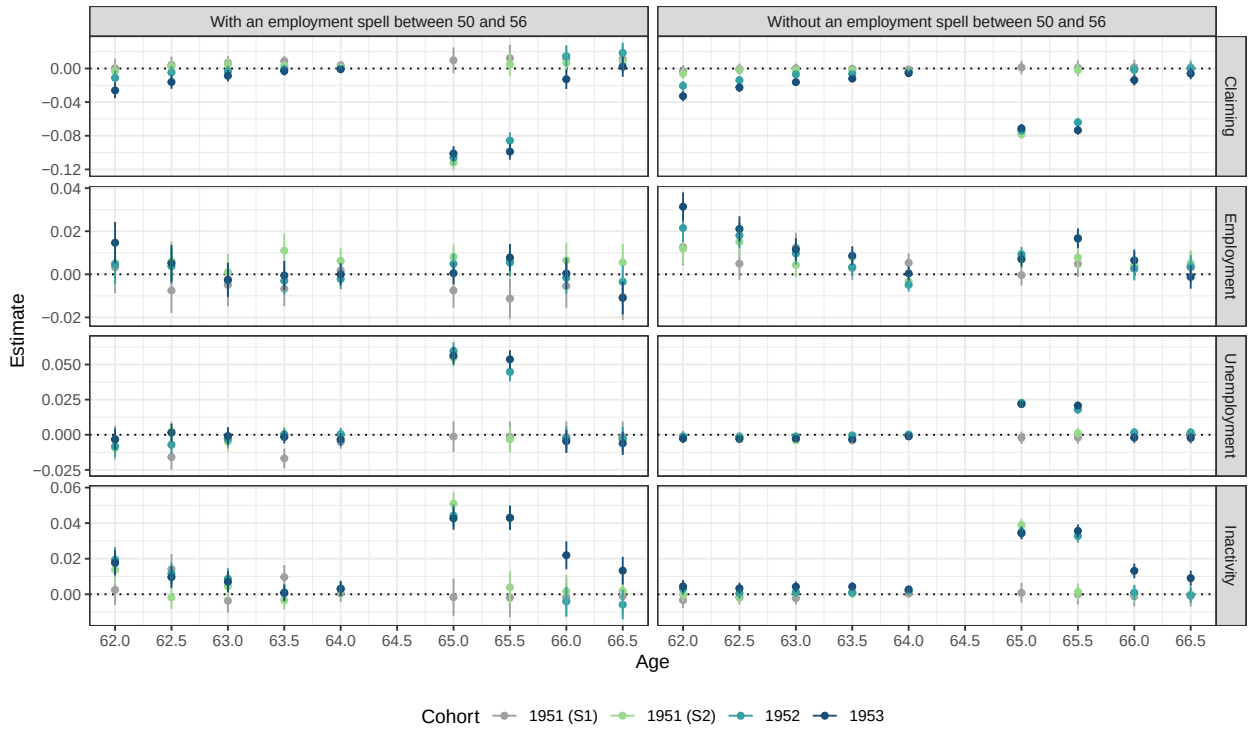
C.5 Heterogeneity of share of individuals at each state: gender



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: Pensioners whose main pension scheme is CNAV *and* who experienced at least one salaried employment spell from 52.
Source: EIR 2020.

Figure C.5.1: Share of individuals at each state by age and gender

C.6 Heterogeneity with respect to unemployment spell



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: Pensioners whose main pension scheme is CNAV *and* who experienced at least one salaried employment spell from 52.
Source: EIR 2020.

Figure C.6.1: Effect of the reform on the labor market state by unemployment status between 50 and 56

C.7 Heterogeneity with respect to the reference wage

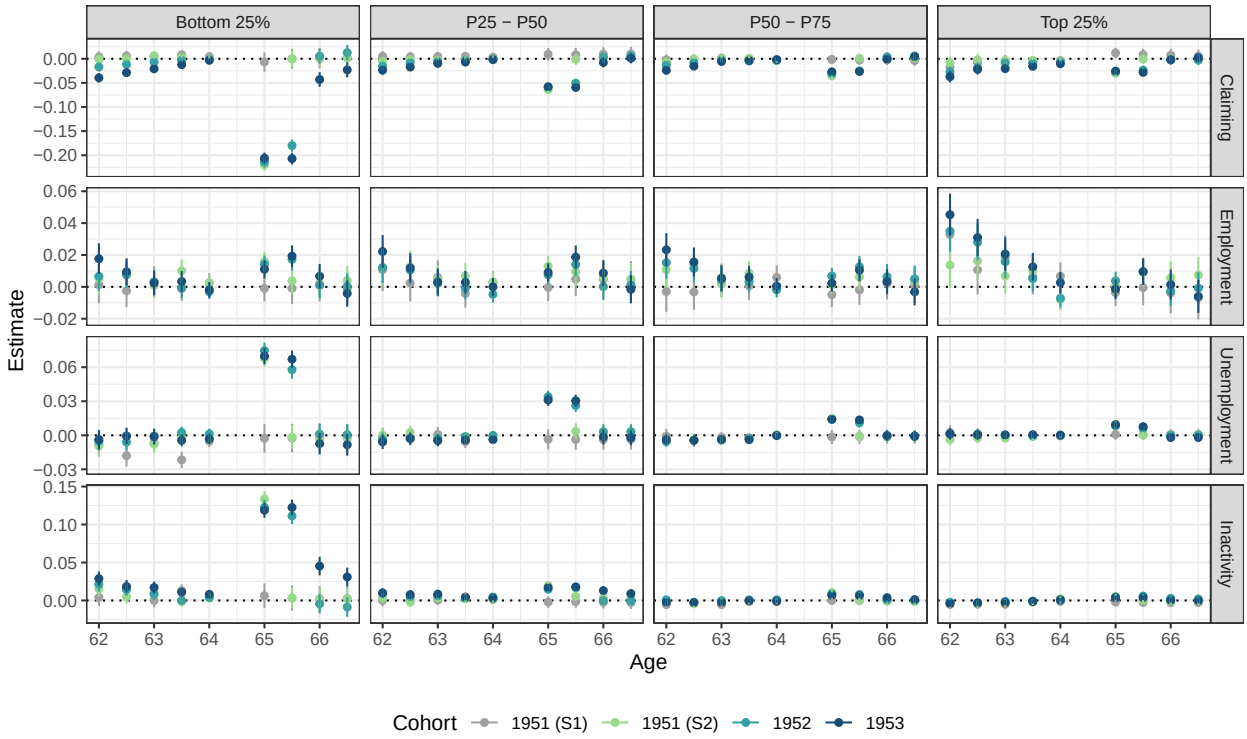


Figure C.7.1: Effect of the reform on the labor market state by quantiles of the reference wage

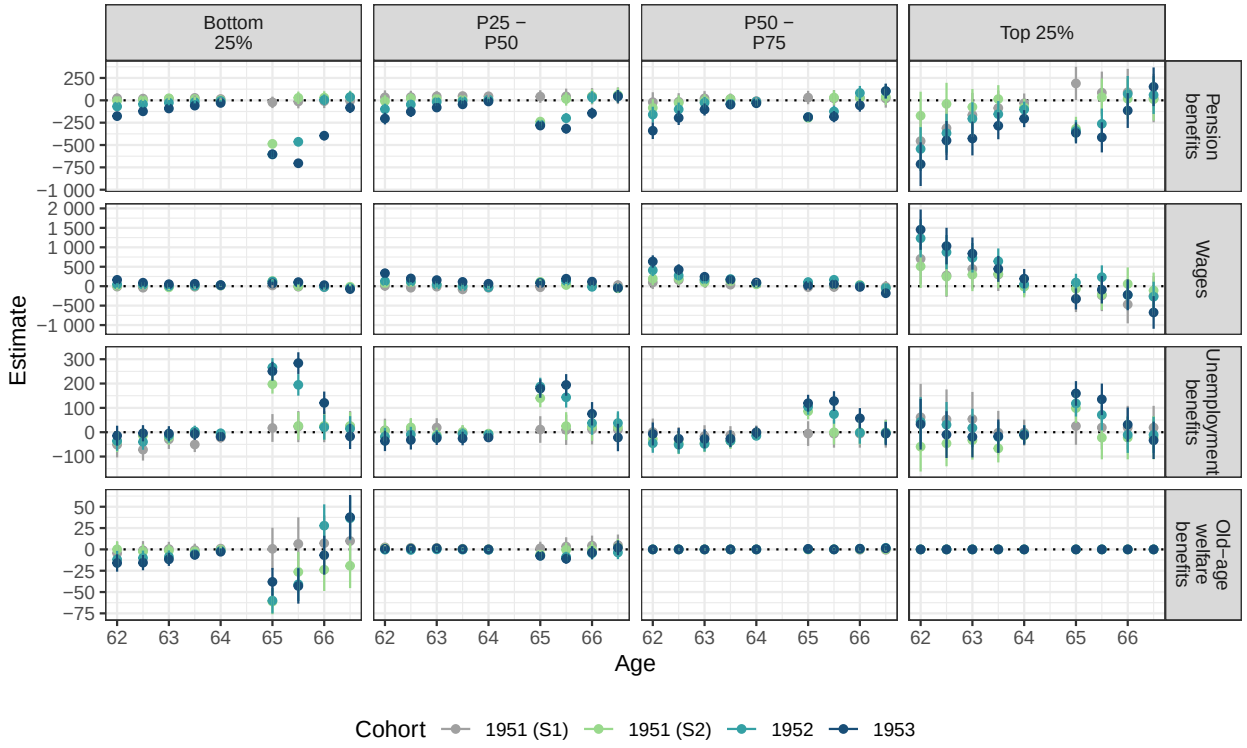


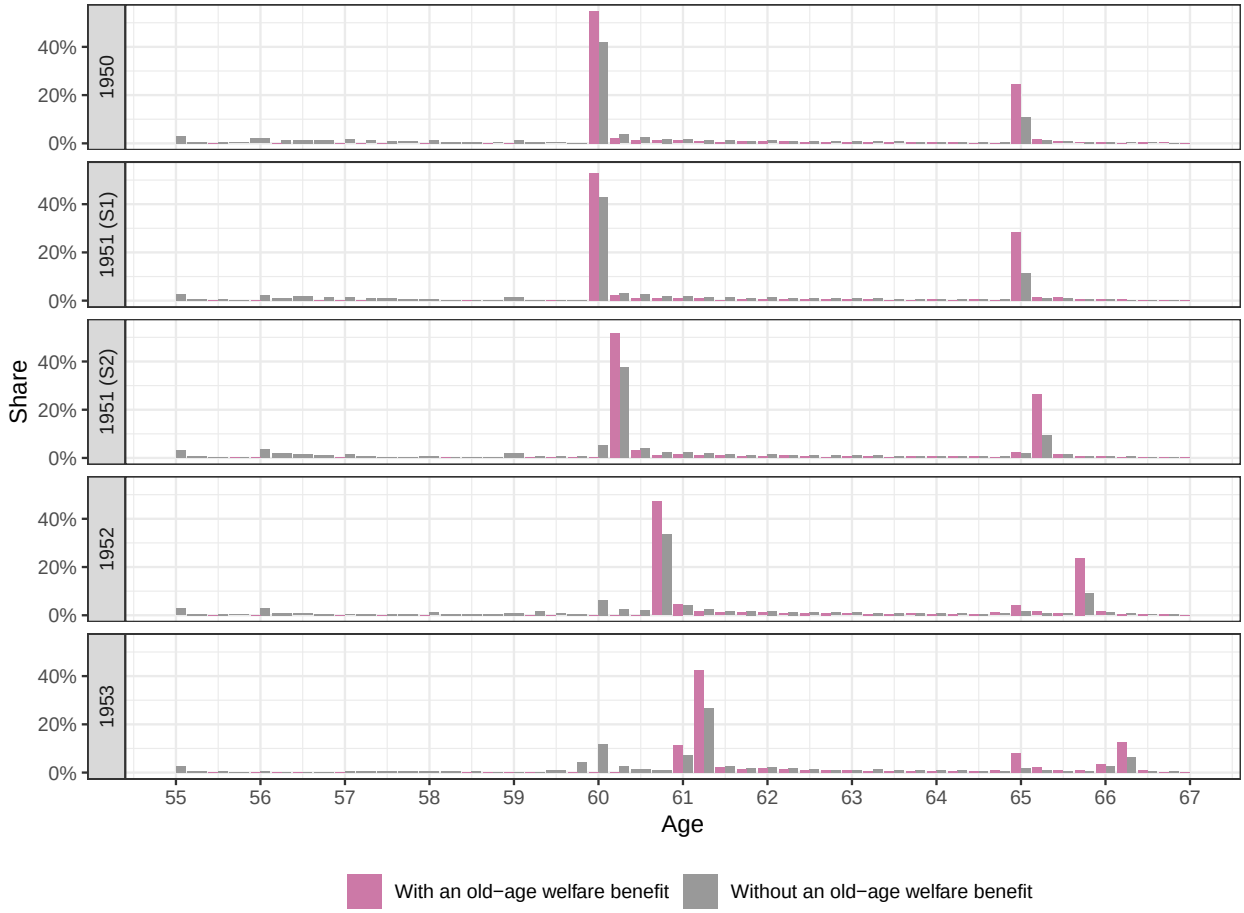
Figure C.7.2: Effect of the reform on earnings by quantiles of the reference wage

Note: Early 1951 cohort is composed of individuals born between January and June 1951.

Sample: Pensioners whose main pension scheme is CNAV *and* who experienced at least one salaried employment spell from 52.

Source: EIR 2020.

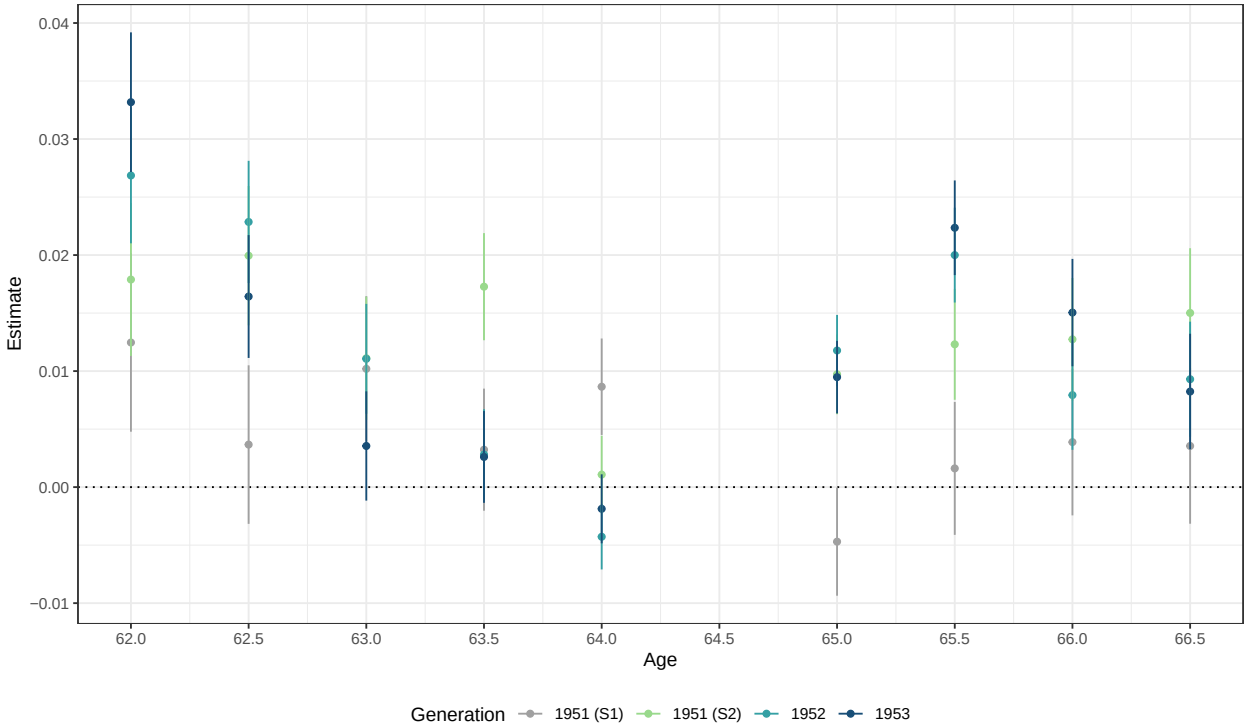
C.8 Heterogeneity of retirement age: old-age welfare benefit



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: all pensioners.
Source: EIR 2020.

Figure C.8.1: Heterogeneity of retirement age according to being beneficiary of an old-age welfare benefit

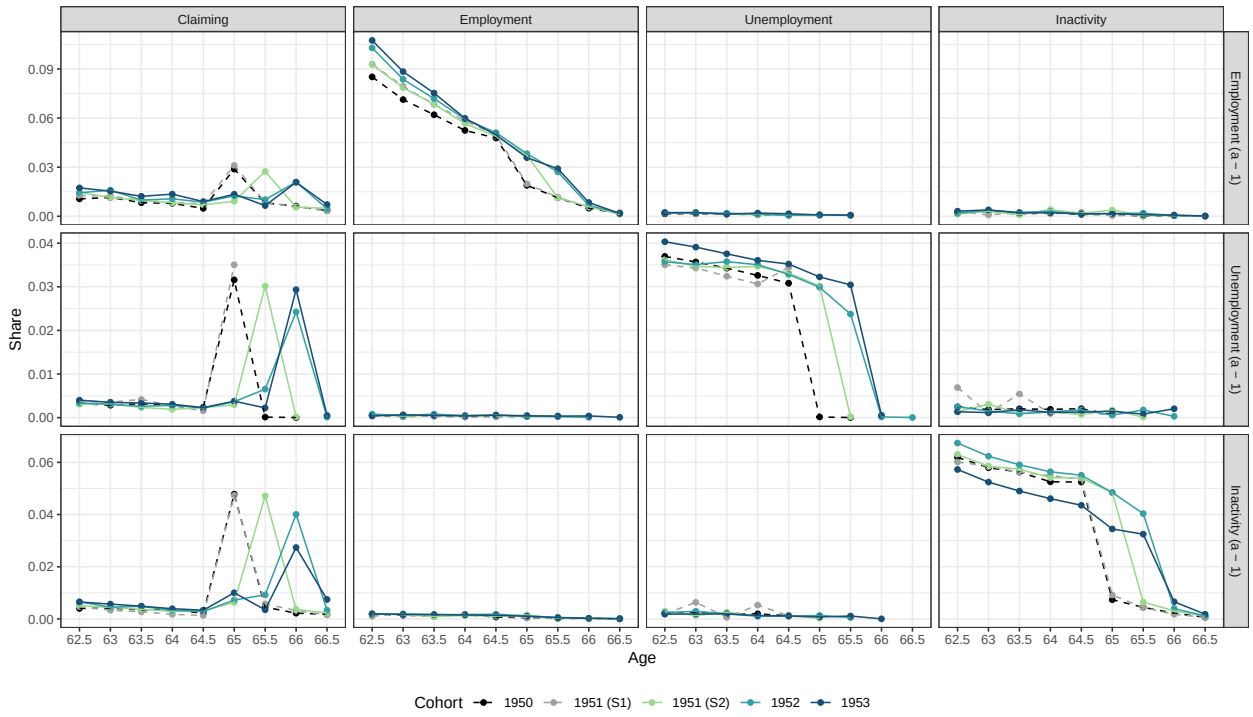
C.9 Adding self-employed individuals



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: Pensioners whose main affiliation scheme is the CNAV *and* who have experienced at least one wage employment spell from their 52 *or* a self-employment spell after 56.
Source: EIR 2020.

Figure C.9.1: Effect of the reform on employment (including self-employment)

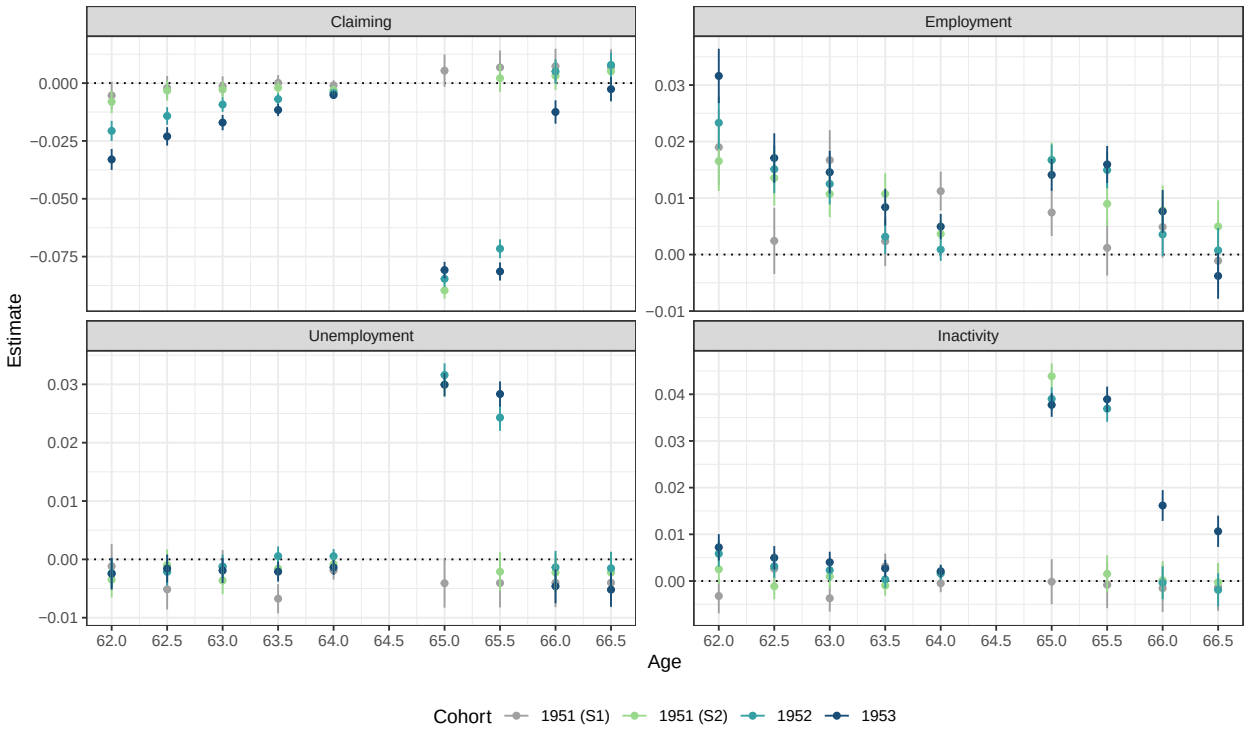
C.10 Evolution of transition probabilities with age and cohort



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: Pensioners whose main affiliation scheme is the CNAV *and* who have experienced at least one wage employment spell from their 52 *or* a self-employment spell after 56.
Source: EIR 2020.

Figure C.10.1: Unconditional probabilities of transition by age and cohort

C.11 Without sample weights



Note: Early 1951 cohort is composed of individuals born between January and June 1951.
Sample: Pensioners whose main pension scheme is CNAV *and* who experienced at least one salaried employment spell from 52.
Source: EIR 2020.

Figure C.11.1: Effect of the reform on the labor market state (without sample weights)

D Additional Tables

Table D.0.1: Descriptive statistics comparing MICO pensioners against non-MICO pensioners

	1950		Early 1951		Late 1951		1952		1953	
	MICO	Non-MICO	MICO	Non-MICO	MICO	Non-MICO	MICO	Non-MICO	MICO	Non-MICO
Women	64.5%	40.4%	64.5%	41.9%	69.2%	43.0%	71.6%	42.3%	73.5%	43.0%
Number of contributed quarters	114.2	153.0	113.5	152.2	111.2	152.3	107.6	152.4	104.4	152.2
Number of validated quarters	155.4	169.0	156.2	168.9	156.8	169.8	156.4	170.7	157.6	170.8
Resident in France	99.2%	98.6%	99.1%	98.7%	99.0%	98.8%	99.4%	99.0%	99.4%	98.9%
Number of children	1.5	0.7	1.5	0.8	1.6	0.8	1.7	0.8	1.7	0.8
Number of affiliated scheme	2.9	2.5	2.9	2.5	2.8	2.5	2.8	2.5	2.7	2.5
Marital status										
Single	12.6%	12.5%	13.7%	13.4%	13.5%	13.5%	14.9%	14.3%	17.3%	15.2%
Married	56.5%	65.7%	55.6%	65.3%	54.7%	64.0%	55.0%	63.7%	51.9%	63.1%
Widowed	13.6%	8.3%	12.9%	7.8%	13.1%	8.1%	12.3%	7.4%	11.7%	6.9%
Separated	16.2%	13.1%	16.9%	12.9%	17.7%	13.7%	16.6%	13.6%	17.3%	13.3%
PACS	0.2%	0.2%	0.3%	0.2%	0.2%	0.3%	0.3%	0.4%	0.4%	0.7%
Cohabitee	0.8%	0.2%	0.6%	0.4%	0.8%	0.4%	1.0%	0.5%	1.4%	0.9%
Number of observations	14,290	27,914	3,072	6,520	4,790	11,505	6,904	19,433	5,940	20,075

Note: The descriptive statistics were computed on sample (2) (pensioners whose main affiliation scheme is CNAV *and* having experienced at least one wage employment spell from their 52).
Source: EIR 2020.

Table D.0.2: Descriptive statistics comparing ASPA pensioners against non-ASPA pensioners

	1950		Early 1951		Late 1951		1952		1953	
	ASPA	Non-ASPA	ASPA	Non-ASPA	ASPA	Non-ASPA	ASPA	Non-ASPA	ASPA	Non-ASPA
Women	45.3%	48.7%	44.7%	49.4%	48.1%	50.9%	46.8%	50.1%	48.2%	50.1%
Number of contributed quarters	72.0	142.4	70.5	142.5	66.1	142.7	67.5	143.6	66.7	144.3
Number of validated quarters	107.4	166.6	107.6	167.1	105.4	168.1	105.8	169.4	109.9	170.2
Resident in France	100.0%	98.8%	99.7%	98.8%	100.0%	98.8%	100.0%	99.0%	99.7%	99.0%
Number of children	1.0	1.0	0.9	1.0	1.0	1.0	1.0	1.0	1.1	1.0
Number of affiliated scheme	2.5	2.6	2.4	2.6	2.4	2.6	2.5	2.6	2.3	2.6
Marital status										
Single	29.0%	11.9%	31.5%	12.8%	36.2%	12.7%	30.6%	13.8%	35.2%	14.9%
Married	26.2%	64.0%	27.7%	63.5%	21.8%	62.7%	27.2%	62.8%	21.6%	62.1%
Widowed	6.2%	10.2%	4.4%	9.7%	4.8%	9.7%	5.3%	8.8%	5.4%	8.1%
Separated	36.3%	13.3%	34.1%	13.4%	36.3%	14.1%	35.8%	13.6%	36.4%	13.3%
PACS	0.2%	0.2%	0.3%	0.2%	0.0%	0.3%	0.0%	0.4%	0.1%	0.7%
Cohabitee	2.2%	0.3%	2.0%	0.4%	0.9%	0.5%	1.1%	0.6%	1.2%	1.0%
Number of observations	1,524	40,680	354	9,238	553	15,742	991	25,346	1,020	24,995

Note: The descriptive statistics were computed on sample (2) (pensioners whose main affiliation scheme is Cnav *and* having experienced at least one wage employment spell from their 52).
Source: EIR 2020.

Table D.0.3: Fiscal effect of the reform, by quantiles of the reference wage

		Amount (Individual level)	Amount (Cohort level)	Total social security contribution base	Total amount over salary mass
Pension benefits	Bottom 25%	-1,700	-236,000,000	1,540,000,000	-15.3%
	P25 - P50	-744	-103,000,000	2,620,000,000	-3.9%
	P50 - P75	-427	-59,200,000	3,610,000,000	-1.6%
	Top 25%	-893	-124,000,000	4,540,000,000	-2.7%
Wages	Bottom 25%	203	28,200,000	1,540,000,000	1.8%
	P25 - P50	386	53,600,000	2,620,000,000	2.0%
	P50 - P75	43.9	6,090,000	3,610,000,000	0.2%
	Top 25%	-639	-88,700,000	4,540,000,000	-2.0%
Unemployment benefits	Bottom 25%	655	90,800,000	1,540,000,000	5.9%
	P25 - P50	449	62,300,000	2,620,000,000	2.4%
	P50 - P75	304	42,200,000	3,610,000,000	1.2%
	Top 25%	326	45,200,000	4,540,000,000	1.0%
Old-age welfare benefits	Bottom 25%	-87.8	-12,200,000	1,540,000,000	-0.8%
	P25 - P50	-22.2	-3,080,000	2,620,000,000	-0.1%
	P50 - P75	1.50	208,000	3,610,000,000	0.0%
	Top 25%	0	0	4,540,000,000	0.0%

Note: The descriptive statistics were computed on sample (2) (pensioners whose main affiliation scheme is CNAV *and* having experienced at least one wage employment spell from their 52).
Source: EIR 2020.