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IZA DP No. 14150

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ABSTRACT

Increasing the Effective Retirement Age: Key Factors and Interaction Effects*

We study the effects of the recent increase in the statutory retirement age (SRA) in the Netherlands, using RDD and rich administrative data on the universe of the Dutch population. We find large interaction effects with a preceding early retirement reform. The employment effect of the SRA reform is much larger for cohorts receiving less generous early retirement benefits. Indeed, the level of employment before the SRA, together with the retirement hazard at the SRA, is key to understanding the effects of retirement age reforms. Our results further point to a big role for automatic job termination in the Netherlands.

JEL Classification: J14, J26

Keywords: statutory retirement age, employment, social insurance, bunching, Netherlands

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1 Introduction

The sustainability of public finances is a major concern in many countries, due to aging populations and lower fertility rates.[1] To alleviate the financial pressure such developments exert on the pension system, many countries have implemented reforms to increase the effective retirement age. Most prominent are changes in the minimum eligibility age for early retirement and the age at which individuals become eligible to a full pension (statutory retirement age, SRA), which are deemed effictive levers to extend the working life of older workers. However, estimates of the employment effect of such reforms vary widely in the literature, ranging from 6.3 percentage points in Cribb et al. (2016) to 20.9 percentage points in Rabaté & Rochut (2019).

In this paper, we show that a simple yet novel framework can reconcile much of the different findings in the empirical literature on retirement age reforms. We focus on SRA reforms, but a similar line of reasoning can be applied to ERA reforms. The effect mainly depends on how much the SRA is shaping retirement behavior: if a large share of the population retires before or after the SRA, increasing the SRA is not likely to have a strong impact on the average retirement age. The overall effect of the reform can then largely be predicted by the share of individuals retiring in the vicinity of the SRA. This bunching at the SRA can in turn be decomposed into two components: (i) the share of individuals still employed when they reach the SRA and (ii) the retirement hazard rate at the SRA. Two other mechanisms can also influence the effect of the reform. The effect can be smaller if individuals are not willing or able to delay retirement, which may result in substitution towards social insurance schemes. However, it can also be boosted if there are 'upstream' effects, where employment also increases at ages before the old SRA (Hairault et al., 2010). Overall, the employment effect of an increase in the SRA then depends on the employment rate before the SRA, the hazard rate at the SRA and potential active substitution and upstream effects.

In this paper we first illustrate the relevance of this framework for a recent reform in the Netherlands, which increased the SRA from 65 to 66 years and 4 months between 2013 and 2019. This was the final chapter of a series of reforms aiming at cutting old-age related spending, including a massive

[1] And more recently due to the vast expansion of support programs for the economy following the COVID-19 pandemic.

reduction in early retirement provision in the second pillar pensions. We leverage the sharp cohort-based shifts in the SRA in a regression discontinuity (RD) setting, to provide well-identified and comprehensive causal effects of the reform. To do so, we rely on administrative data on various types of income, on wealth and job characteristics for the universe of the Dutch population for the period 2007–2019. We analyze the effect of the reform on employment close to the SRA, as well as substitution effects toward other insurance schemes and upstream effects. We then use our local RD estimates to compute the global effect on the average retirement age. Furthermore, as we observe subsequent jumps in the SRA, we can study how the effects interact with the changes in the early retirement (ER) scheme. We compare outcomes for earlier cohorts that were still eligible for the more generous ER scheme, and later cohorts that were eligible for a much less generous one. Because of the ER reform, the employment rate before the SRA was much higher for the later cohorts, which in turn resulted in much larger effects of the SRA reforms for later cohorts. Finally, we study which mechanisms play a key role in the high level of bunching or retirement at the SRA in the Netherlands. By comparing bunching of retirement at the SRA for different subgroups of the population, we explore the relative importance of the main potential channels: financial incentives, credit constraints, employer effects and norm effects.

Our main findings are as follows. First, we find relatively strong employment effects of the reform at the vicinity of the SRA for the later cohorts (+20pp). We find no evidence of upstream effects before the SRA, neither for the earlier cohorts nor for the later cohorts (who had more time to adapt their behavior to the new SRA). Although we find a strong increase in the share of individuals in disability insurance, unemployment insurance and welfare benefits following an increase in the SRA, we find that this is mostly so-called passive substitution, where individuals persist longer in the state they were in before the old SRA. We do find statistically significant evidence for active substitution towards social insurance from employment before the SRA, but the effects are relatively small, much smaller than the passive substitution. Second, we find strong interaction effects of the SRA reform with the ER reform. Specifically, there is a modest drop in the retirement rate (12–15%-points) and rise in the employment rate (4–5%-points) for cohorts that were still eligible for the generous ER regime. However, this drop in the retirement rate and the rise in the employment rate become much larger for the earlier cohorts that faced a much less generous ER regime, 57–59%-points and 19–20% points, respectively. Indeed, the ER reform had a large positive effect on the employment rate before the SRA, resulting in large effects for the SRA reform for the later cohorts as well, in line with our framework. Third, we study the determinants of bunching at the SRA. Comparing bunching of retirement at the SRA of employees and self-employed, we find three times as much bunching for employees, consistent with an important role for automatic job termination and the end of employment protection in the retirement of older workers in the Netherlands. The bunching of self-employed is still substantial, suggesting that social norms also play a role. Furthermore, we find slightly higher bunching for individiuals with relatively low liquid (non-housing) wealth, consistent with some role for credit constraints. Finally, we observe similar bunching for sectors with different second-pillar pension incentives, which suggests a minor role for (other) financial incentives in the observed bunching.

Our analysis relates to the rich body of literature analyzing the effects of reforms of the early retirement age and normal retirement age. As pension claiming at the SRA in the Netherlands is universal and automatic, with a fixed amount that is unrelated to labor supply decisions, it shares characteristics with both types of retirement ages. Evaluations of shifts in the ERA, pioneered by Staubli & Zweimüller (2013) for Austria,[2] find strong effects on retirement and employment, as well as important (though mostly passive) substitution effects towards other social insurance schemes. Our analysis also relates to studies that consider changes in the SRA on the average retirement age, pioneered by Mastrobuoni (2009) for the US.[3] These studies also show that the effects are largely driven by shifts in the bunching of retirement at the NRA age, and consider the underlying mechanisms (see also Behaghel & Blau, 2012, Brown, 2013, Lalive et al., 2020, Seibold, 2021).

Aside from providing a clean evaluation for the Dutch context,[4] our paper makes the following contributions to this literature. First, we tie together the literature that considers the local effect of ERA and SRA reforms on retire-

[2]Other studies include Atalay & Barrett (2015) for Australia, Cribb et al. (2016) for the UK, Seibold (2017) and Geyer & Welteke (2019) for Germany, Manoli & Weber (2018) for Austria and Rabaté & Rochut (2019) for France.

[3]See also Manoli & Weber (2018) for Austria and Lalive et al. (2020) for Switzerland.

[4]This recent reform has not been studied extensively before, though a preliminary differences-in-differences analysis of the employment effects using the Labor Force Survey for the first cohorts affected by the reform can be found in De Vos et al. (2018).

ment and employment between the old and the new ERA and SRA respectively (Staubli & Zweimüller, 2013, Atalay & Barrett, 2015, Cribb et al., 2016, Geyer & Welteke, 2019, Rabaté & Rochut, 2019) with the literature that considers the effect on the average retirement age, including potential upstream effects on retirement and employment before the pre-reform ERA or SRA (Mastrobuoni, 2009, Manoli & Weber, 2018, Lalive et al., 2020). Specifically, we formally show how to use the local RD estimates to calculate the effect on the average retirement age. This approach could be easily implemented for other reforms in other countries. Second, we uncover important interaction effects between the generosity of the ERA and SRA. The sharp cohort-based differential shifts in the ERA and the SRA allows for a clean analysis of this interaction effect, comparing the RD estimates of the earlier cohorts that were still eligible to the generous ERA provision, and the later cohorts that were not. Our results show that from the perspective of employment and public finances, increasing the ERA generates additional employment and budgetary gains when the SRA is shifted upwards. This interaction effect also highlights a third element, that differences in employment (and retirement) effects across studies in different countries are largely driven by differences in employment (and retirement) rates just before the SRA. We establish this point with an extensive overview of existing studies on ERA and SRA reforms.

Along with the initial employment rate, the other essential determinant of the magnitude of SRA reforms is the retirement hazard rate at these ages. The product of the two numbers – the pre-SRA employment rate and hazard rate into retirement – determines the amount of bunching at the SRA, which in turns directly determines the employment effect of the reform in the absence of active substitution and upstream effects. Our paper then also touches upon the determinants of bunching at key ages of the pension system, an old puzzle of the retirement literature (Lumsdaine et al., 1996). We provide novel evidence on the mechanisms that play a key role in the bunching of retirement at the SRA in three ways. First, comparing the retirement hazard rates of employees and self-employed, we find that employees are much more likely to bunch at the SRA than the self-employed. We argue that this is closely related to mandatory retirement at the SRA, as the relatively strict employment protection of open-ended contracts in the Netherlands ends at the SRA. This points towards an important role of the employer side in bunching at the SRA, a determinant largely overlooked in the literature, except for Rabaté

(2019) for the French case. Although bunching at the SRA is much smaller for the self-employed, it is still substantial. This is in line with 'behavioral' effects playing an important role in the bunching of retirement at the SRA, consistent with the findings of Behaghel & Blau (2012), Lalive et al. (2020) and Seibold (2021). These papers however depict reference-dependent preferences with loss aversion as the main driver of bunching at the SRA, whereas in this case norms effects are more likely. Second, using administrative data on liquid household wealth (excluding housing wealth), we find that bunching is somewhat larger for individuals with low liquid household wealth than for individuals with high liquid household wealth. This is consistent with some role for liquidity constraints in the bunching into retirement at the SRA. This contrasts with the findings of Cribb et al. (2016), who do not find that wealth affects the effect of the increase inthe ERA in the UK using data on (typically less liquid) housing wealth. Lastly, we find hardly any differences between employees that work in different sectors, facing different second-pillar pension incentives around the SRA. This suggest that kinks in the budget constraint at the SRA arising from differences in sector-specific second-pillar incentives play a limited role in bunching at the SRA. This is in line with the results found by Behaghel & Blau (2012) and Seibold (2021), who show that financial incentives are not a major determinant of bunching at the focal ages of the pension system, and Brown (2013) and Manoli & Weber (2016) who find small elasticities of the retirement age to financial incentives.

The outline of the paper is as follows. Section 2 gives an overview of the institutional context, the SRA reforms and reforms in ERA and social insurance schemes that may interact with the SRA reforms. Section 3 outlines the empirical methodology and datasets used. Section 4 presents graphical evidence on the effects of the reforms, regression results and robustness checks. Section 5 unifies the related literature in our framework and considers the role of the different mechanisms. Section 6 concludes. Additional results are given in the Appendix. An online appendix contains supplementary material.

2 Institutional setup and reforms

The Dutch pension system consists of three pillars, which together allow workers to accumulate pension rights in the order of 70% of their average gross wage for retirement (Knoef et al., 2017).

The first pillar consists of pay-as-you-go old age pension benefits (AOW, *Algemene Ouderdomswet*). Individuals accumulate 2 percent of the full first pillar pension per year of residence in the Netherlands (up to a maximum of 100% of the full benefit). The benefits are linked to the social minimum and also depend on partnership status (a retired single person gets 70% of the social minimum, a retired couple gets 100% of the social minimum).[5] Individuals start receiving the first pillar pension once they have reached their birth-cohort specific 'AOW age' or SRA (in months). Individuals cannot bring any first pillar pension benefits forward when they retire earlier. Furthermore, at the SRA, employment contracts end by law and need to be renewed if an individual worker wants to continue to work. Also, beyond the SRA individuals are no longer eligible for unemployment insurance benefits or disability insurance benefits.

The second pillar consists of firm- and sector-specific funded pension schemes. The benefits from the second pillar supplement the first pillar benefits. Pension savings in the second pillar depend on an individual's wage income and the pension arrangement that is provided by the firm or sector. Employees and employers pay monthly premiums to the pension fund of the respective firm or sector. These premiums are paid over a certain income threshold and exempt from income taxation up to a maximum income threshold (EUR 112,189 in 2021), and there is no wealth tax on second pillar pension savings. The second pillar pension benefits are indexed to average wages, although indexation may be stalled, or benefits may even be reduced, when the assets of the pension fund drops below a certain percentage of its projected future obligations. Individuals can decide to retire before (or after) the SRA, and bring part of the second pillar pension benefits forward, with an actuarial fair reduction (increase) in the monthly benefits (De Vos et al., 2018).

The third pillar consists of individual savings for retirement. Individuals can accumulate 1.875% of their average wage income for the expected retirement period per year tax free, via earmarked personal savings or life insurance schemes. Over a working life of 40 years this amounts to 75% of the average wage income.

Knoef et al. (2017) calculate replacement rates for a representative sam-

[5]On the 1st of January 2021, the (gross) montly AOW benefit for a single person was EUR 1,293 and for a couple it was EUR 1,767 (www.svb.nl).

Table 1: Reforms in the SRA in the Netherlands

Year	2011 reform	2012 reform	SRA	Affected birth cohort
2012	-	-	65	Before 01-01-1948
2013	1 month	1 month	65+1 month	After 31-12-1947 and before 01-12-1948
2014	1 month	1 month	65+2 months	After 30-11-1948 and before 01-11-1949
2015	1 month	1 month	65+3 months	After 31-10-1949 and before 01-10-1950
2016	2 months	3 months	65+6 months	After 30-09-1950 and before 01-07-1951
2017	2 months	3 months	65+9 months	After 30-06-1951 and before 01-04-1952
2018	2 months	3 months	66	After 31-04-1952 and before 01-01-1953
2019	3 months	4 months	66+4 months	After 31-12-1952 and before 01-09-1953

ple of the Dutch population, combining data on first, second and third pillar pension in the Income Panel dataset of Statistics Netherlands. The median replacement rate of expected retirement income from first and second pillar pensions for individuals 60–65 years of age when they turn 67 is 68 percent.[6] 39 percentage points come from the first pillar and 29 percentage points come from the second pillar. Adding third pillar pension savings and other assets (including housing wealth), raises the median replacement rate to 82 percentage points.[7] There is substantial variation in the replacement rate, ranging from 62 percent at the 25th percentile of the distribution to 106 percent at the 75th percentile of the distribution (Knoef et al., 2017, Table 4). The replacement rate is higher for individuals with a relatively low household income, and for employees when compared to self-employed (Knoef et al., 2017, Table 11).

At the introduction of the first pillar pension in the Netherlands in 1957, the SRA was set at 65. This continued to be the SRA all the way up to the end of 2012. In 2011, faced with public finances that were no longer sustainable in the long run, the Dutch government adopted a reform package that included

[6]They calculate an annuity based on all income and assets projected to be available to the individual at the age of 67, and divide this by gross primary income observed at the age the individual is observed.

[7]The median net replacement rate is 100 percent, as retired individuals pay less taxes than working age individuals at the same gross income level.

an increase in the SRA from 2013 onwards (see Table 1). The second column shows the planned increase in the SRA for the different birth cohorts of the reform announced in 2011. In 2012 this reform was amended to allow the SRA to increase at a faster pace from 2015 onward (third column of Table 1).[8] These reforms in the SRA are the focus of our analysis.

There are a number of related earlier reforms in early retirement schemes and the second pillar pension system that will turn out to be important for the SRA reforms as well, see Table 2. These reforms generate large upward 'jumps' in the birth-cohort specific employment rates between the ERA and SRA. Until 2006, workers could opt for an early retirement scheme several years before the SRA, which was financed via a sectoral or firm-specific pay-as-you-go system. This scheme was abolished in 2006, although individuals that would reach the official retirement age before 2015 could use a compensation scheme called the Life Course Saving scheme (*Levensloopregeling*).[9] Consequently, cohorts affected by changes in the SRA before 2015 are not directly comparable to cohorts affected by changes in the SRA from 2015 onwards, as we will see in the next section.[10]

Individuals can also exit the labor force before the SRA using so-called alternative pathways, most importantly unemployment insurance (UI) and disability insurance (DI).[11] A change in the SRA may, therefore, lead to increased substitution towards other social insurance programs (OECD, 2019). It is important to take these spillover effects into account when assessing the effectiveness of such a reform. Unemployed individuals are entitled to UI if they did not quit their job and worked at least 26 weeks in the last 36 weeks of employment. The minimal duration of the UI benefits is three months. The maximum duration of UI benefits was cut from 5 years to 3 years and 2 months in 2006, and in 2016 it was cut to a maximum of 2 years. The individual receives a benefit that is based on previous wage earnings. The replacement rate is 75 percent in the first two months, after which it drops to 70 percent for the remainder of the entitlement to UI. Individuals may

[8] Furthermore, from 2021 onwards, the increase in the SRA is linked to the increase in life expectancy.

[9] The Life Course Saving scheme, offered tax free savings for, amongst other things, retirement before the SRA. Saving into this scheme was abolished in 2012, but individuals could still use the accumulated savings to retire early in years beyond 2012.

[10] See e.g. Lindeboom & Montizaan (2018) for an analysis of this earlier reform.

[11] See CPB (2020) for an overview of the system of social insurance in the Netherlands.

Table 2: Overview of related reforms

Year	First pillar	Second pillar and early retirement	Unemployment insurance	Disability insurance
2006		ER tax exemptions abolished, Life Course Saving Scheme introduced	Reduction of max. benefit duration	Stricter distinction between partially, fully and permanently disabled
2008				Experience rating abolished
2009		Deferred Pension Bonus introduced		
2012		Life Course Saving Scheme abolished		
2013	Gradual increase SRA		New calculation of employment period	
2015	Accelerated gradual increase SRA			
2016			Gradual shortening of benefit period	

Source: De Vos et al. (2018) appended.

also exit the labour force via DI. An individual is eligible for DI of 75% of the previous wage when he or she is fully and permanently disabled. When the individual is partially and/or temporarily disabled, benefits are less generous and depend on the previous wage, number of weeks worked before, the current wage (if applicable) and the 'remaining earnings capability' of the individual.[12] The last major reform of disability insurance was in 2006, when the system became much more strict, as a distinction was made between fully and permanently disabled persons and partially and/or temporarily disabled persons (see Koning & Lindeboom, 2015). After this reform the inflow into DI dropped significantly. Previous studies have shown that these reforms in UI and DI have reduced participation in these schemes, see e.g. De Groot & Van der Klaauw (2019) and Koning & Lindeboom (2015), respectively. However, the different cohorts we focus on in the empirical analysis below would

[12]See e.g. CPB (2020) for further details.

be generally affected in much the same way by these reforms, and hence they are unlikely to interfere with our results.

3 Data and empirical strategy

3.1 Data and descriptive statistics

We use administrative data on the universe of the Dutch elderly population for the period 2007–2019. We use information on individual characteristics – such as migration background, income and wealth – as well as firm level information (sector).[13] We then construct a monthly panel for the whole population between ages 57 and 66 years and 6 months.

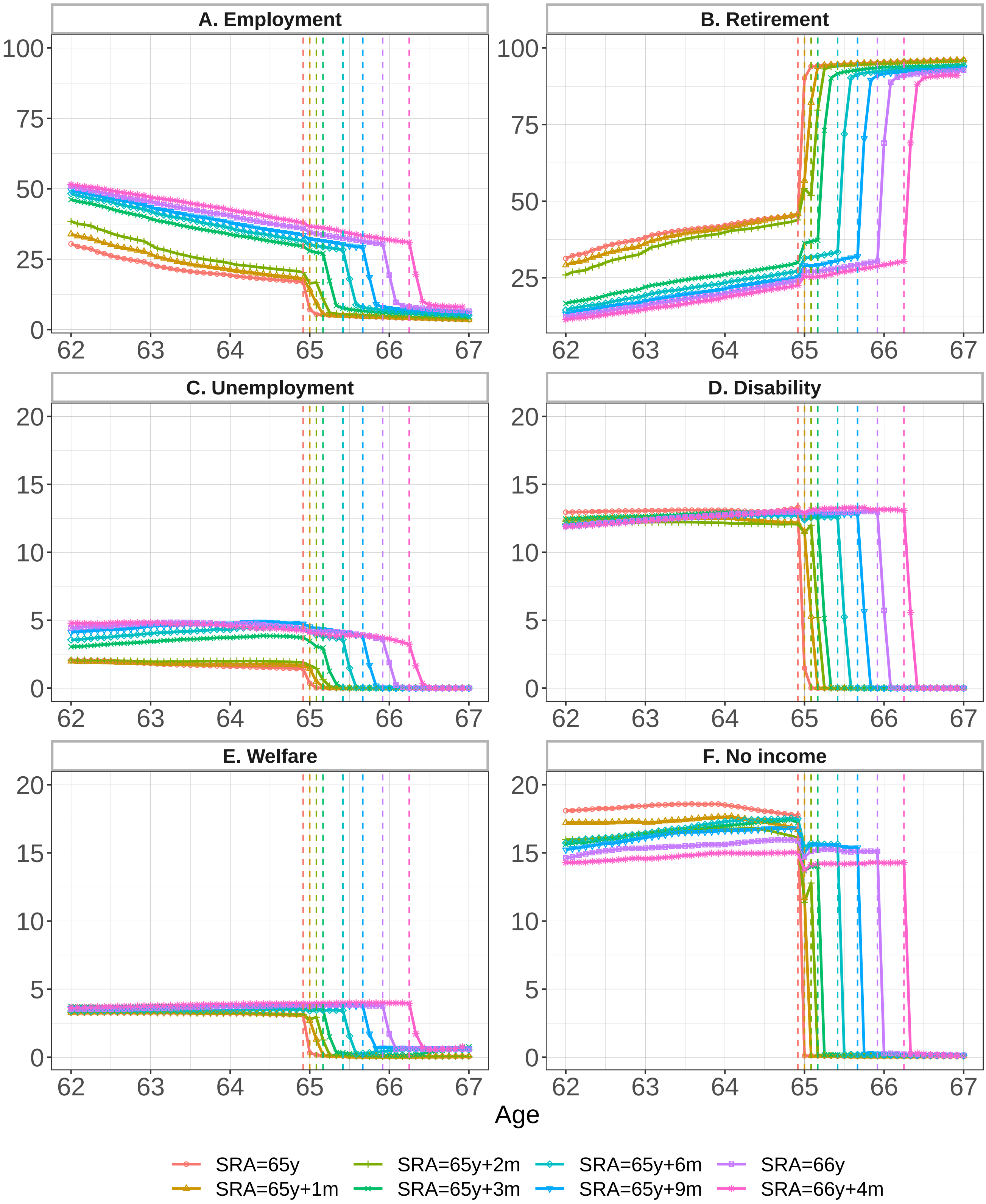
Our main outcome variables are the different states individuals can be in on and off the labor market. Specifically, individuals are classified according to their main source of personal income, e.g. wage income (employees), profit income (self-employed), disability insurance benefits, unemployment insurance benefits, welfare benefits, pension benefits, other benefits or no income (typically women in couples). Demographic variables include month of birth (to select individuals into treatment and control groups), gender (male/female), migration background (with/without) and household position (single/couple). Furthermore, we use information on sector of employment (public/private) for the individual at age 60.

In the analysis we focus on cohorts born between January 1947 and December 1952. We consider the full Dutch population and have approximately 1.4 million individuals. Table 3 presents summary statistics for the different SRA birth cohorts we consider.

Figure 1 presents the share of the population in different labor market states at different ages, for SRA-cohorts impacted by the gradual increase from 65 to 66 and 4 months. We observe the following patterns. First, the share of employed individuals decreases progressively over the age profile, until the SRA is reached and employment drops close to zero. Retirement follows a roughly symmetric pattern, while other workstates (generally) exhibit a flat profile (slightly increasing for unemployment), and all drop to zero beyond

[13]The datasets we use are linked and remotely accessed through a secured environment provided by Statistics Netherlands. In Section 6 in the online appendix we present a detailed list of the datasets used.

Figure 1: Shares in different labor market states, by age and SRA cohorts



NOTES: This Figure presents the average share of the population in different workstates, by age and SRA-cohort.

Table 3: Sample description

	SRA							
	65 y	65 y + 1 m	65 y + 2 m	65 y + 3 m	65 y + 6 m	65 y + 9 m	66 y	66 y + 4 m
Demographic variables								
Age	64.48	64.48	64.48	64.48	64.48	64.48	64.48	64.30
Share single	0.50	0.50	0.50	0.50	0.50	0.50	0.51	0.51
Share foreigners	0.20	0.20	0.20	0.20	0.21	0.21	0.21	0.21
Share female	0.08	0.09	0.10	0.11	0.09	0.10	0.10	0.10
Nb of obs. (in millions)	13.64	11.87	11.58	9.33	9.04	9.19	9.34	7.88

the SRA, which is a mechanical effect of the first pillar pension being automatically claimed and replacing all other existing benefits.[14] Second, we can observe a progressive increase of the employment rate over time, with a large jump in the 'middle' of the cohorts we consider. The former evolution can be attributed to the progressive increase in education and labor force participation of women (CPB, 2018). The second one is the consequence of the 2006 second pillar reform of early retirement, which had a strong impact of the average retirement age (e.g. Lindeboom & Montizaan, 2018). Lastly, we also observe a clear effect of the reform, as the patterns observed at the SRA (increase in retirement, drop in other outcomes) appear to shift to the right with each cohort, suggesting that it moves with the SRA. This can be considered as direct evidence of a causal effect of the SRA change on employment and retirement profiles.

3.2 Empirical strategy

To measure the causal effect of the reform over employment and other outcomes, we take advantage of the cohort-based implementation of the reform to implement a regression discontinuity (RD) approach, as in Geyer & Welteke (2019). Intuitively, we will compare the labor market outcomes of individuals born around the SRA discontinuities, which are likely to be very similar in every dimension except for the SRA they face. Formally, we will estimate the

[14] With the exception of welfare benefits for some individuals that have not lived for 50 years in the Netherlands, and do not receive the full first-pillar pension.

following models, separately for each SRA jump j and for different ages t:

$$y_{it}^{j} = \alpha_j t + \beta_{jt} T_i + \gamma_{jt} f(Z_i - c_j) + \delta_j t f(Z_i - c_j) T_i + \eta X_{it} + \epsilon_{it}, \qquad (1)$$

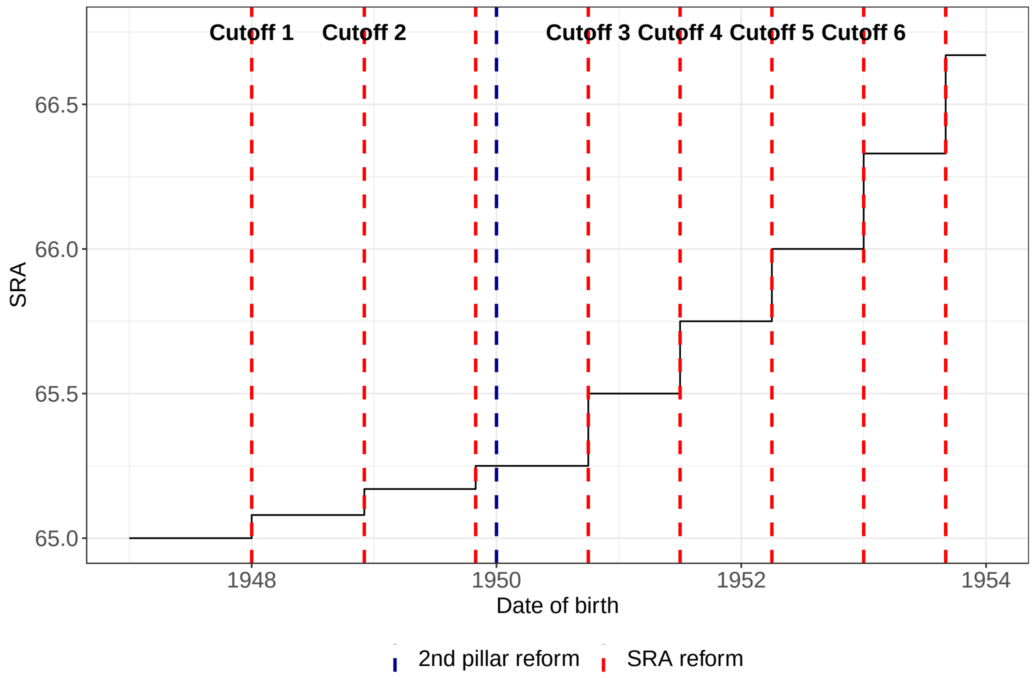
where Z_i is the birth month of the individual and c_j the first cohort impacted by the reform. $f(Z_{ij} - c_j)$ is the running variable, and represents the distance in months between the month of birth of individual i to the cutoff that applies to the individuals that are part of the RD analysis for j. This distance variable takes on value zero at the cutoff. For values of the distance variable greater than or equal to zero, the treatment indicator T_i takes on value 1, indicating the treated individuals. Lastly, X_{it} is a vector of individual level control variables and the ϵ_{ijt} indicates the error term. We expect the β_{jt} to be positive for employment and negative for retirement at the ages impacted by the reform (e.g at 65 years and 6, 7 and 8 months for an increase from 65.5 to 65.75 of the SRA). Other ages should not exhibit any discontinuity.

We estimate equation 1, using a two degree polynomial specification for the $f()$ functions, and keeping only observations with an SRA equals to 65 year and 3 months (resp. 65 year and 6 months) in the control (resp. treatment) group. We present alternative specifications in Section 4.2.

The identifying assumption of this approach is that individuals around the cutoff are similar in all dimensions except for their SRA. This implies in particular that individuals should not be able to manipulate the running variable, which is likely to be the case with month of birth. Figure A.1a of the online appendix presents the number of births for different birth years. We observe some spikes at round numbers, which are likely to be driven by administrative decisions, in particular for individuals born outside of the Netherlands. As those dates sometimes coincide with SRA change, this may affect the estimates when e.g. migrants differ in their labor market outcomes from natives. However, there is no systematic discontinuity in the number of births when we pool all the cutoffs and center it to 0 (Figure A.1b). Moreover, we also study the effects of so-called 'donut' RD regressions models in Section 4.2, where we leave out observations right next to the cutoff points.

Another identifying assumption for the RD is that the SRA must be the only varying factor at the vicinity of the cutoff. In particular, no other reforms impacting employment trajectories should interfere with the SRA reform. As the third jump (from 65 and 2 months to 65 and 3 months) occurs almost

Figure 2: Summary of the empirical strategy



at the same moment as the second pillar reform of early retirement schemes (November 1949 vs. January 1950), we do not estimate our RD model for this SRA reform.

Figure 2 summarizes the different jumps in the SRA used for the estimation of the effects. Among the eight jumps observed, we discard the last one, as we do not have enough data to study it, as well as the third one, which occurs almost at the same time as the second pillar reform (see above). We end up with six cutoffs/reforms, for which we estimate equation 1. As the right of the discontinuity for one cutoff is also the left of the discontinuity for the next one, some observations will be used alternatively as treatment and control groups.

4 Results

4.1 Main results

Graphical evidence of the effect of the reforms and the validity of our identification strategy is presented in Figure 3. We first focus on a given cutoff (cutoff$_4$, at 65 and 6 months), and show the effect of the increase in the SRA

Table 4: Effect of the SRA reform (all workstates, cutoff 4)

	Employment	Retirement	Unemployment	Disability	No income	Welfare	Others
Treatment effect	18.943	−58.945	3.962	12.679	16.403	4.098	2.873
	(0.446)	(0.459)	(0.162)	(0.279)	(0.303)	(0.164)	(0.156)
No. obs.	295989	295989	295989	295989	295989	295989	295989

NOTES: This Table presents the estimated β_{jt} coefficient from equation 1, for different workstates as outcome variable and for age $t = 65$ and 6 months and cutoff $j = 4$, corresponding to an increase from 65 and 6 months to 65 and 9 months in the SRA, for individuals born before and after July 1951. We use a two-degree polynomial for the control functions and consider all the observations from both sides of the cutoff in the estimation.

on the probability of being in a particular labor market state. Each panel exhibits a large change in the rate when the SRA jumps, and relatively smooth patterns on both sides of the cutoff, confirming the direct effect of the reform on employment outcomes. We observe the direct effect of the SRA reform: a large drop in the probability of being retired, and an increase in the other outcomes.

Table 4 presents the corresponding estimation results. These confirm the graphical evidence of the effect of the reforms, with strong effects and estimates that are statistically significant at the 0.1 percent level. We estimate a steep drop in the share of individuals that are retired of 59 percentage points. The employment rate increases by 19 percentage points (32% of the decrease in retirement). The share of individuals with as their main source of income disability benefits, unemployment benefits, welfare benefits and other benefits increases by 13, 4, 3 and 4 percentage points, respectively. Hence, in total, the share of individuals on (other) social insurance increases by 20 percentage points (39% of the decrease in retirement). Moreover, the share of individuals that have no other income increases by 16 percentage points (30% of the decrease in retirement). The reform hence generated important employment effect, but also large substitution effects towards other social insurance schemes.

Those effects are the sum of two different mechanisms: passive substitution – individuals in social insurance stay longer in their state instead of retiring – and active substitution – individuals change their labor force participation due to the reform and enter those schemes. We disentangle those two dimensions by estimating equation 1 on a subpopulation of individuals employed at age

Figure 3: Local linear regression plots at 65 and 6 months, all workstates

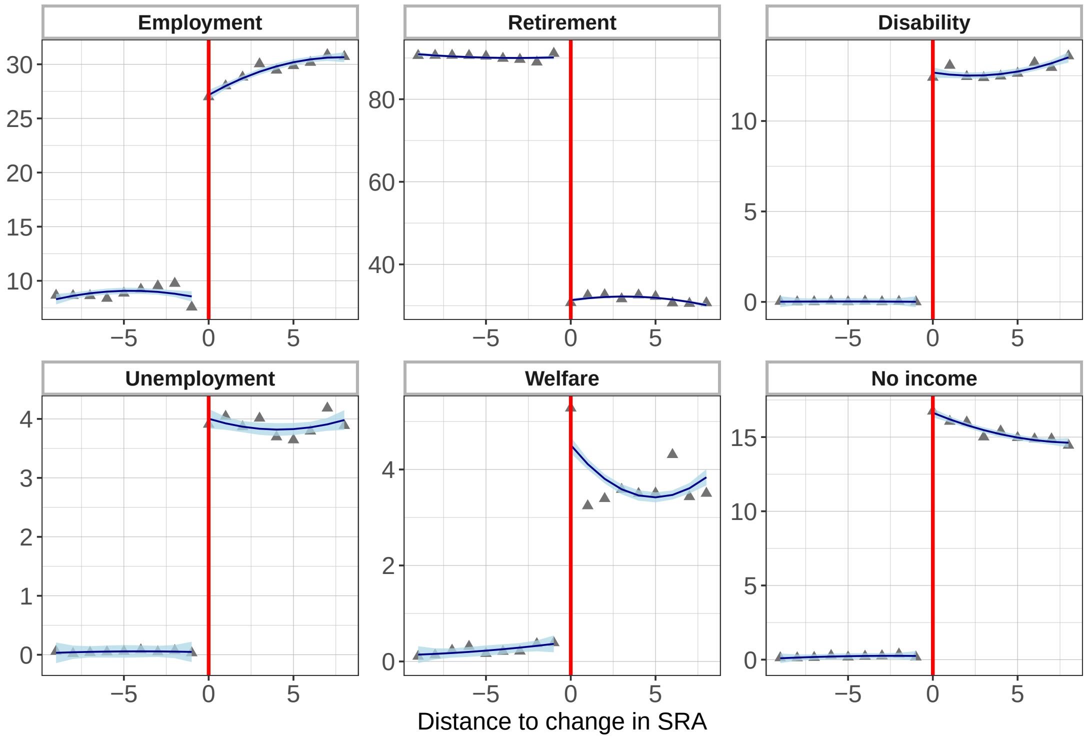


Figure 4: Local linear regression plots for employment and retirement

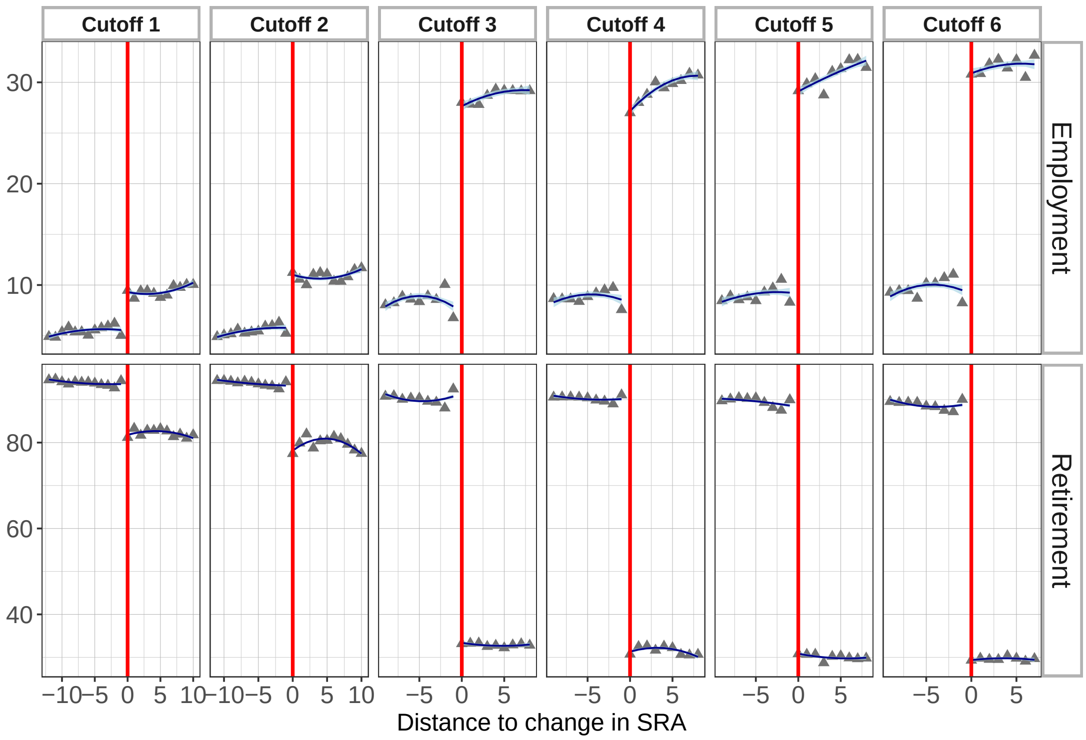


Table 5: Substitution effect of the SRA reform (all workstates, cutoff 4)

	Employment	Retirement	Unemployment	Disability	No income	Welfare	Others
Treatment effect	53.096	−60.363	2.710	1.148	3.798	0.056	−0.418
	(0.841)	(0.795)	(0.217)	(0.142)	(0.239)	(0.040)	(0.095)
No. obs.	103664	103664	103664	103664	103664	103664	103664

NOTES: See Table 4. We further restrict the sample of estimation to individuals still employed at age 65.

65, in order to isolate active substitution out of employment. Table 5 shows that a large share of the retirement effect is translated into higher employment: 88% (53.1/60.4) of individuals employed at age 65 keep on working when the SRA is increased from 65.5 to 66.75. We find statistically significant active substitution towards unemployment insurance, disability insurance and the state of no income. But the effects are rather small in magnitude, much smaller than the passive substitution.

We then compare the effect of the reform for successive increases in the SRA. Figure 4 present the RD plots for employment and retirement for all the cutoffs we consider, for the first age impacted by the different reforms. We observe a similar pattern for the different cutoffs, with a drop in retirement and an increase in employment. There are, however, large differences in the magnitude of the effects. The effects are much stronger for the last four SRA increases than for the first two. This is confirmed by the point estimates presented in Table 6: the effect of the SRA increase is about four times bigger for the later increases, both for retirement (60pp vs. 15pp) and employment (20pp vs. 5pp).

This difference is not primarily due the fact that the first increases were 'smaller' (1 month vs. 3 months), as we consider the effect for one particular age in this case. The difference can be explained by the early retirement scheme reform (see Section 2), which substantially increased employment rates before the SRA. As a result, the effect of the increase in the SRA is also much bigger. As we discuss in more detail in Section 5, the employment rate before the old SRA is an important explanatory factor when it comes to the different effect estimates in the literature on related reforms.

Table 6: Effect of the increase in the SRA, all reforms

	Cutoff 1	Cutoff 2	Cutoff 3	Cutoff 4	Cutoff 5	Cutoff 6
Y = Employment	3.810	5.251	20.351	18.943	19.942	21.793
	(0.249)	(0.281)	(0.444)	(0.446)	(0.444)	(0.458)
Y = Retirement	−11.879	−15.083	−58.067	−58.945	−57.457	−59.814
	(0.302)	(0.341)	(0.465)	(0.459)	(0.451)	(0.462)
No. obs.	416129	382190	299082	295989	300325	288292

NOTES: This Table presents the estimated β_{jt} coefficient from equation 1, for two outcomes (employment and retirement) and for six different cutoffs j corresponding to consecutive increases in the SRA, as presented in Figure 2. Models are estimated for the first age impacted by the reform (e.g at 65 and 6 months for an increase in SRA from 65 and 6 months to 65 and 9 months). We use a two-degree polynomial for the control functions and consider all the observations from both sides of the cutoff in the estimation.

4.2 Robustness and heterogeneity

In this subsection, we focus on the employment effect and present different types of robustness and heterogeneity analyses. For the sake of brevity, we focus on one SRA reform, the increase from 65.5 to 65.75 years (cutoff 4).

Figure 5 tests the sensitivity of our results to alternative specifications for the estimation of equation 1.[15] We first use different values for the degree of the polynomials for the control functions $f()$, one in the RT1a specification, three in the RT1b specification. We then cluster the standard errors at the level of the month and year of birth (RT2a and RT2b models), to account for the fact that individuals born at a specific time are subjected to similar macroeconomic developments over time. RT3a and RT3b change the bandwidth used for the estimation compared to the reference (9 on each side), respectively to a smaller (6 on each side) and larger (18 and 26 months) window. In order to deal with potential biases related to mass points in the distribution of month of birth (cf. Section 3.2), RT4a and RT4 present the results of a 'donut-RD' estimation, removing observations around the threshold (at the cutoff in RT4a, and at the cutoff and just before in RT4b). Finally, RT5a and RT5b implement the bias-corrected and robust bias-corrected estimation proposed by Calonico et al. (2014), respectively, including an optimal choice for the estimation bandwidth. Figure 5 shows that the results are stable

[15] The corresponding estimation table is presented in figure B.2 on the online appendix.

across the different specifications.

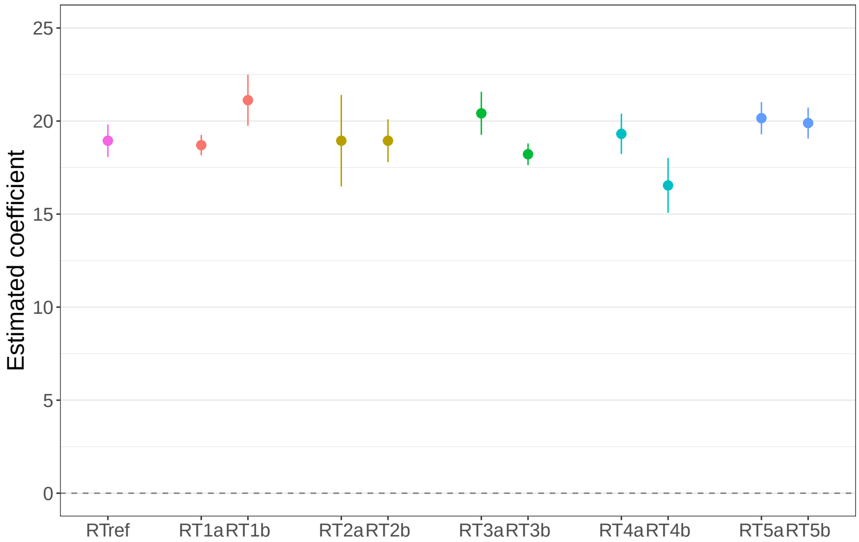
Figure 6 presents the results for different subpopulations and different definitions of employment. We first present the results separately for men and women. As the effect of the reform largely depends on the employment rate before the SRA, we expect some differences reflecting the lower employment rate of women. We indeed find a bigger employment effect for men (around 25pp) than for women (around 15pp). Second, we consider an alternative measure of employment that is commonly used in the literature. Instead of defining employment when income from work is the main source of income for a given month (definition 1), we use the less restrictive definition of having positive income from work (definition 2). The estimated effect of the reform is smaller with this second definition, because some individuals combine pension (as a main source of income) and labor income after the SRA, and hence are not impacted by the reform when using this definition of employment.

A further robustness check is provided in Figure 7, which presents the estimated RD coefficients for all ages, from 63 onward. We can then verify that the successive reforms only have a significant impact at the ages impacted by the SRA changes. We can also hereby assess the differential effect of the large or small increase in the pension age (1 month vs. 3 months). We observe the expected pattern, with no significant difference in the employment rate between the different cohorts except for the ages for which the SRA differs. We also verify that the effect of the 3 months increases is more important, not only because the effect for one given age is bigger (see discussion above), but also because it impacts a larger part of the employment trajectory.

Note that this approach is also a check for possible 'upstream' effects, where people respond to the new SRA also before the old SRA (also known as horizon or distance-to-retirement effects). The coefficients are small and statistically insignificant before the age of 65 and 3 months (the SRA for the control cohort), and positive and statistically significant after this age. Hence, we cannot reject that the cohorts with different SRA share common time effects and that the upstream effects are limited for these cohorts.[16] The absence of an upstream effect is hard to reconcile with a standard economic model with a trade-off between income and leisure (see e.g. Hairault et al., 2010), but it is consistent with the results found in similar settings (see e.g.

[16]We also estimated a fully interacted difference-in-difference model, which yields similar results. They are presented in Table A.2 of the online appendix.

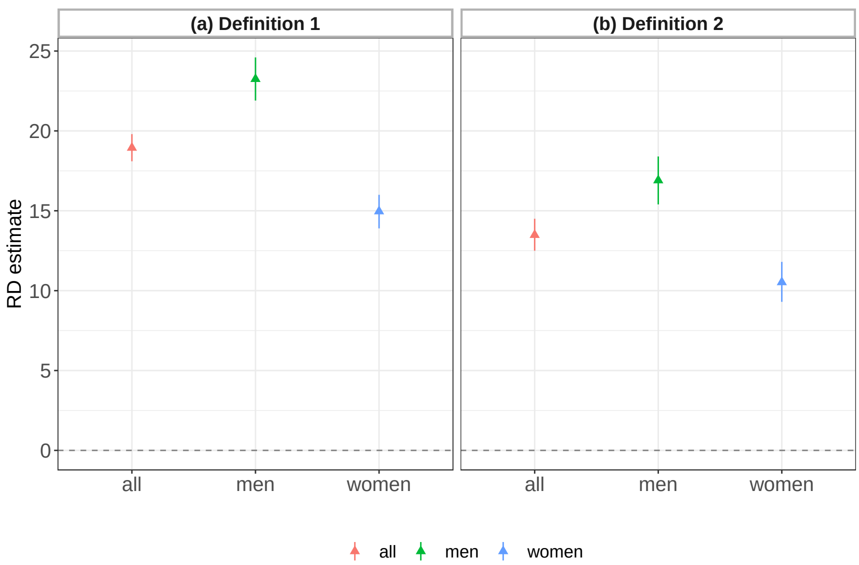
Figure 5: Employment effect of the reform: robustness



NOTES: The RTref points corresponds to the specification used in Table 3. The next points correspond to the five series of robustness tests (see the text for details and online appendix table B.2 for the full table):

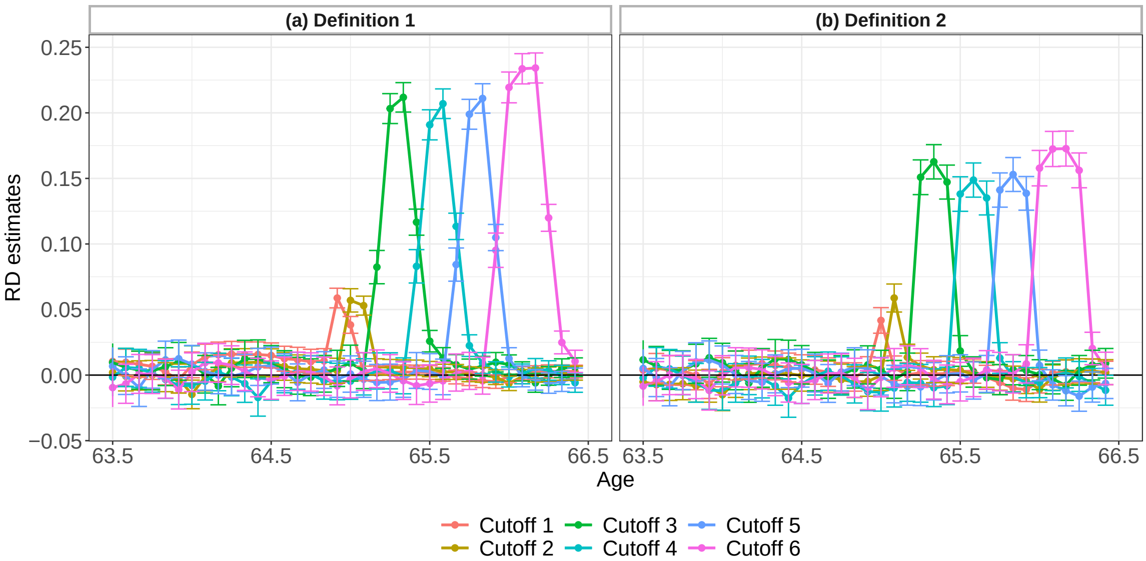
RT1: Alternative specification for the degree of the polynomials in equation 1 (1 in RT1a, 3 in RT3a).
RT2: Clustering of standard errors at the monthly (RT2a) and yearly (RT2b) levels.
RT3: Alternative bandwidth used for the estimation compared to the reference (9 on each side), respectively to a smaller (RT3a, 6 on each side) and larger (RT3b, 18 and 26 months) window
RT4: Donut RD estimation, removing the observation at the cutoff (RT4a) and the -1 and 0 observations (RT5a).
RT5: Bias-corrected (RT5a) and robust bias-corrected (RT5b) estimation proposed by Calonico et al. (2014)

Figure 6: Employment effect of the reform: heterogeneity



NOTES: This Figure present the estimation of equation 1, estimated for cutoff$_4$, for age 65.5, for different groups and for two different outcomes variable. Individuals are employed according to definition 1 if labour income is their main source of income, and according to definition 2 if they have a positive labour income. See online appendix for the full table.

Figure 7: Employment effect of SRA increase by age in months



NOTE: This Figure presents the estimated β coefficients of equation (1) for all ages from 63.5 and all cutoffs we consider. Results are presented for two different definition of employment: labour income as main source of income (definition 1, Panel (a)) and labour income above 0 (definition 2, Panel (b)).

Staubli & Zweimüller, 2013). One explanation to the absence of upstream effects could be that we only measure the short-run effects of the reform, and that the mechanisms underlying the distance to retirement have effects on younger ages only in the longer run. However, also for the longer run effects, the evidence on upstream effects is mixed: Geyer & Welteke (2019) find no upstream effect of a German reform of the early retirement age announced 10 years in advance,[17] whereas Carta & De Philippis (2019) find significant labor market effects for middle-aged women of an Italian reform of the early retirement age.

4.3 Effect on the average retirement age

One limitation of the RD estimates provided so far is that they only give the 'local' effect of the SRA-reform on the probability of being employed, retired, unemployed, etc. They do not yield the effect of the reform on the effective retirement age, which may be a more relevant elasticity parameter for the evaluation of the effect of pension reforms. We remedy this shortcoming by deriving an effect of the reform on the average retirement age from our

[17] Studing the average retirement age in the US, Mastrobuoni (2009) also finds hardly any upstream effects before the old NRA.

estimates.

Under some assumptions for the effect of the reform at older ages, we can use the age-specific estimates to compute the effects of the reform on the average retirement age for our two definitions of the retirement age (employment as main source of income and non-zero labor income). The methodology is described in detail in Appendix 6. The effect of the reform on the average retirement age can be computed as the sum of the coefficients when using employment as the outcome. The intuition behind the result is the following: the RD estimates can be interpreted as the difference between the cumulative distribution of retirement age caused by the change in the SRA, from which we can retrieve the impact on the employment rates (see also Mastrobuoni, 2009).

Table 7 presents the results obtained using this computation, for the different SRA reforms we consider and for the two different definitions of retirement we use. As expected, the increases in the retirement age are larger for the later jumps in the SRA (cutoffs 3 to 6), due to the combination of a larger at a given age (due to the higher employment rates before the SRA) and a larger age span impacted by the reform. For the most recent reforms, we find an increase of around 0.62 months for a 3 months increase in the SRA, translating in an elasticity of 0.2 for the average retirement age for a one year increase in the SRA. When using the alternative definition of employment, the elasticity is slightly lower, around 0.15. As expected, elasticities are much higher for SRA reforms occurring after the ER reform.

Our estimates of the effect of the reform on the average retirement are smaller in terms of magnitude to the ones found by Manoli & Weber (2018) for Austria (who use a regression kink design). They find an elasticity of 0.4 for a one year increase in the early retirement age. However, they restrict their sample to individuals working at age 53, for whom they can observe a transition from work to retirement. We expect the effect to be stronger for this subpopulation, since the pre-retirement employment level is higher. Panel (a) of Figure 8 presents the RD estimates and the associated average retirement age we obtain for a more comparable population to Manoli & Weber (2018), individuals employed at age 57. We obtain a 1.1 months response to a 3 months increase, i.e an elasticity of 0.36, consistent with the findings of Manoli & Weber (2018).

The other advantage of using this subsample is that we can then observe

Table 7: Effect on the average retirement age

	Cutoff 1	Cutoff 2	Cutoff 3	Cutoff 4	Cutoff 5	Cutoff 6
Definition 1						
Increase in retirement age	0.12	0.11	0.68	0.63	0.61	0.94
Increase in SRA	1	1	3	3	3	4
Elasticity	0.12	0.11	0.23	0.21	0.2	0.23
Definition 2						
Increase in retirement age	0.02	0.06	0.48	0.42	0.4	0.68
Increase in SRA	1	1	3	3	3	4
Elasticity	0.02	0.06	0.16	0.14	0.13	0.17

NOTES: The increase in retirement age are computed using the coefficient presented in Figure 7, following the methodology depicted in Appendix A.

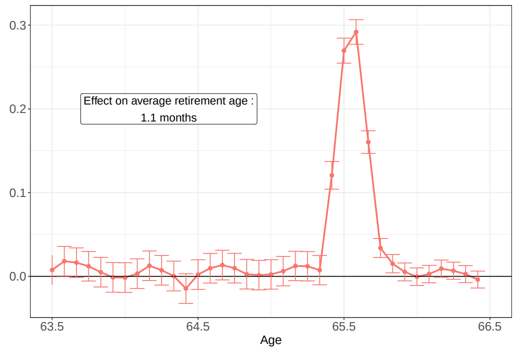
individuals transitioning from work to retirement and define an individual retirement age. In order to assess the validity of our estimation of the effect of the reform on the average retirement, we compare our result to a RD estimation on the average retirement age. More precisely, we estimate the following model:

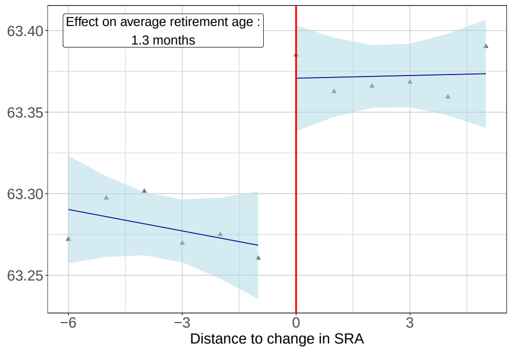
$$y_i^j = \alpha_j + \beta_j T_i + \gamma_j f(Z_i - c_j) + \delta_j f(Z_i - c_j) T_i + \eta X_{it} + \epsilon_i, \tag{2}$$

where y_i is the individual retirement age, defined as the maximum age of employment (and we correct for seasonality in retirement behavior by month of birth). Panel (b) of Figure 8 presents the RD plot for the average retirement age, using the same reform and the same population as in Panel (a). We find a point estimate of 1.3 months (significant at the 1% level), which is slightly bigger but of similar magnitude as the one obtained using the RD estimates for the employment rates.[18]

[18]Note that the results for the retirement age are generally more sensitive to the specifications, see online appendix Table B.4.

Figure 8: Effect on average retirement age: comparison of two approaches





NOTE: Panel (a) presents the same results as Figure 7 for only one cutoff and for a subsample of individuals working at age 57. The label shows the sum of significant coefficients and is interpreted as the effect on the average retirement age (see text for details). Panel (b) present a local linear plot with polynomials of degree 1. The label presents the β_j parameters of equation (2). The full table is available in Table B.4 the online appendix.

5 Channels

Reconciling the findings of the literature

Several papers have evaluated the impact of reforms of the retirement age – which can be either the early retirement age (ERA) or the normal retirement age (NRA) – see Table B.1 in the Appendix. On the one hand, there is a wide range of estimates of the employment effects found in the literature, ranging from 6.3 percentage points in Cribb et al. (2016) to 20.9 percentage points in Rabaté & Rochut (2019). On the other hand, the results are qualitatively very similar throughout the different papers: there is limited active substitution and upstream effects, most workers merely stay longer in the state they were in before the old SRA. In fact, the mechanism behind the effects found is essentially the same: the employment effects are directly proportional to the share of individuals retiring in the vicinity of the retirement age, *i.e* to the bunching at the retirement age. This is a rather intuitive statement – as individuals retiring before as well as after the retirement age will not be impacted by such a reform – but it is key to understanding the different results found in the literature. As illustrated in Table B.1 in the appendix, the estimated effect increases almost linearly with the magnitude of the pre-reform bunching at the retirement age targeted by the reform. The different findings in the related literature then mainly reflects differences in bunching at the retirement age.

Another important element can be learned from the comparison of the different papers. It appears that the magnitude of the bunching at the retirement age is the combination of two effects: i) the share of individuals still employed before the retirement age, and ii) the hazard rate into retirement at the retirement age for those individuals. This is shown for example by the results for men and women in Staubli & Zweimüller (2013). They find a similar effect of approximately 10 percentage points for men and women, but this consists of a relatively high pre-ERA employment rate and a relatively low hazard rate for women (where the ERA is lower for women than for men) and a relatively low employment rate for men but combined with a high hazard rate into retirement. This shows that a similar point estimate for the effect of the reform can hide very different underlying mechanisms.

How do our results fit in this comparison? The estimated employment effect for the later cohorts in the Netherlands is relatively high (19–22 percentage points) compared to the literature, whereas the employment effect for the earlier cohorts is relatively low (4–5 percentage points). Interestingly, the effect for the latter cohorts is largely driven by a high hazard rate out of employment at the SRA, 70%, which is the highest in the related literature. The employment rate before the SRA on the other hand is relatively low, which is likely due to the fact that we consider a reform that targets individuals at a relatively old age (beyond 65) when compared to most of the other papers (beyond 60). The high hazard rate is all the more surprising since the institutional features of the pension system do not provide a (strong) incentive to retire exactly at the SRA. Recall that the SRA pension is claimed automatically, that the amount is flat, and that there is no constraint in the combination with other types of income. Given those characteristics, there should be no kink in the individuals' lifetime budget constraint related to the first pillar pension,[19] hence there are no (strong) financial incentives to retire exactly at the SRA. But the Netherlands still has the highest retirement hazard rate at the retirement age found in the literature. Below we discuss and illustrate potential explanations for this paradox.

[19]For example, in the model of Brown (2013) or Manoli & Weber (2018) who adapt the labor supply model developed in Saez (2010) to the case of retirement.

Determinants of the bunching at the SRA

In the absence of (strong) financial incentives to retire exactly a the SRA, we consider alternative determinants of the observed bunching at the SRA. To explore those channels, we compare the hazard rate of different subgroups that are differently impacted by those determinants.

First, we consider potential financial incentives to retire at the SRA resulting from incentives in the second pillar pension's schedule. As it can represent a large share of the total pension, kinks in the second pillar pension could potentially be an important driver of the bunching we observe. To test for this potential channel, we focus on the health care sector, for which we know that there is no financial incentives to retire exactly at the SRA from Kantarcı & Zweerink (2020). As a result, if bunching were primarily driven by financial incentives in the second pillar pension, we would observe no bunching in the healthcare sector. Figure 9a compares the hazard rate by age for individuals working in this sector (measured at age 60), to the harzard rate of individual working in other sectors. We do not see any difference between the two groups, if anything, bunching is stronger in the healthcare sector. This suggests that second pillar incentives are not the main driver of bunching at the SRA.

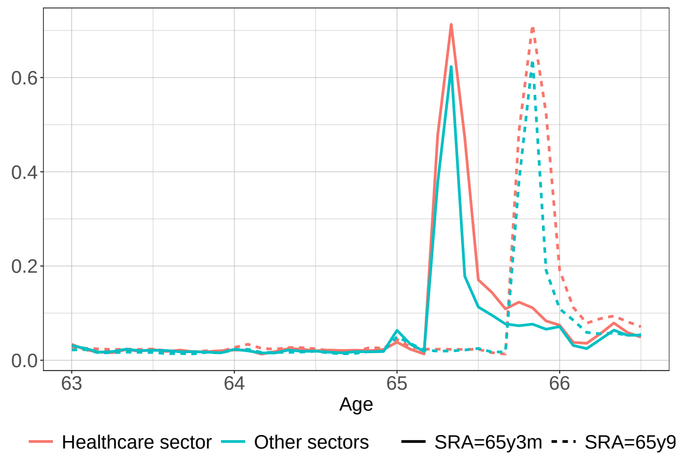
Second, another potentially important mechanism might be credit constraints. As individuals cannot borrow against their first-pillar pension wealth, they may be constrained in their consumption smoothing and may be forced to work until the moment they can get their first pillar pension. This would generate bunching at the SRA. We directly observe liquid household wealth in our data.[20] Figure 9b then compares the hazard rate for individuals in the lowest and the highest wealth quartiles. We do observe a somewhat larger hazard rate at the SRA for individuals with relatively low (liquid) wealth. However, we also observe a large hazard rate for the individuals with relatively high (liquid) wealth, suggesting that credit constraint are only a part of the explanation.

Next, we consider the importance of demand side factors, in particular employment protection. There is evidence that changes in employment protection at key ages of the pension system can be an important driver of bunching (Rabaté, 2019). We expect this effect to be relatively strong in

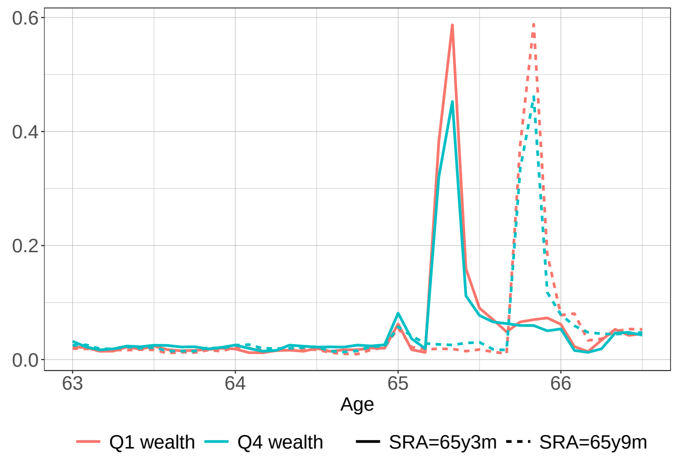
[20] Contrary to Cribb et al. (2016), who use (relatively illiquid) home ownership as a proxy for credit constraints.

Figure 9: Determinants of bunching at the SRA

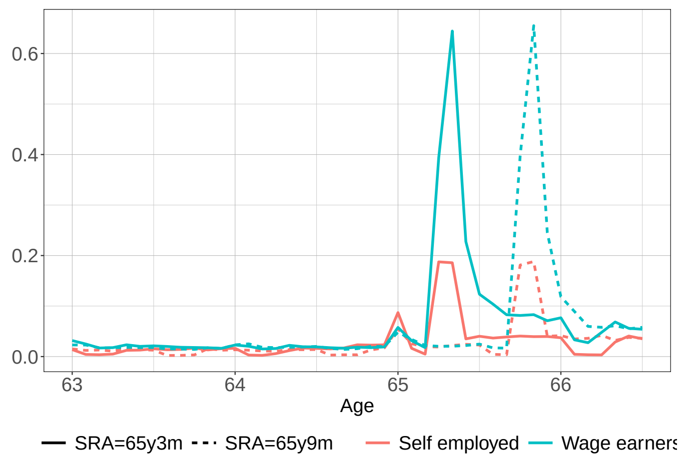
(a) Financial incentives



(b) Credit constraints



(c) Employer effects and norms



NOTE: These panels present the retirement hazard rate by quarterly age (probability of retire at this age conditional on not being retired before), for different subgroups. Panel (a) compares individuals working in the healthcare sector at age 60 to individuals working in other sectors. Panel (b) compares individuals in the first and last quartile of wealth (measured at age 60, for the whole population). Panel (c) compares wage earners to self-employed (defined by the situation at age 60).

the Netherlands, as the discontinuity in terms of job protection is important: job protection is strong for permanent contracts in general, but most contracts automatically end at the SRA. As it may be difficult for older workers to find a job beyond age 65, in particular at the previous wage level, this is likely to induce retirement. We explore the importance of this channel by comparing the hazard rate of wage earners and self-employed (defined by their income status at age 60) in Figure 9c. Employment protection is not binding for the self-employed, hence we expect smaller bunching for this group. We indeed observe that the hazard rate is three times bigger for the wage earners than for the self-employed, suggesting that automatic job termination and the end of employment protection may be a important drivers of bunching at the SRA.

Finally, norms or framing potentially play an important role in the bunching of retirement at the SRA (Behaghel & Blau, 2012, Lalive et al., 2020, Seibold, 2021). In our setting, the residual bunching we observe for the self-employed suggests that norm effects are also important. If we consider that all the norm effects are measured by the bunching observed for self-employed, we can conclude that they are not a big driver of bunching, compared to employers' effects working via automatic job termination and employment protection. However, we cannot directly interpret the difference in bunching between the two groups as a pure employer side effect, as it can also be due to group-specific norms or framing effects. The employers' effect discussed above can indeed be a mix of employer driven retirement effects and workplace norms effects.

6 Discussion and conclusion

In this paper we have analyzed the effects of the increase in the Dutch retirement age on employment and the use of social insurance of older workers. We used an RD approach and rich administrative data on the universe of the Dutch population. We find that the reform decreased the share of individuals retirement by 57 percentage points. Close to one third (16 percentage points) of these individuals are employed between the old and new retirement age, whereas more than one third (20 percentage points) are in social insurance (disability insurance in particular). We statistically significant active substitution, but the magnitude is small compared to passive substitution. Most individuals merely persist in the state they were in before the old retirement age. We do find substantial complementarity between the SRA and ER reforms, the effect of the former being four times larger after the ER reform than before the ER reform.

Pre-SRA employment rates then largely determine the effect of such reform. The other important determinant is the retirement hazard rate at the SRA (or ERA). This simple framework makes it possible to reconcile the findings of the literature on policy evaluation of ERA or SRA reforms. The wide range of estimates found are in fact qualitatively consistent: the effect of such reforms is largely driven by the amount of bunching in retirement behavior. The relatively strong effect we find in the Dutch case results mostly from a large hazard rate at the SRA, which appears mainly driven by employer and norm effects.

Several policy implications can be derived from these results. So far, it seems that the increases in the SRA have been beneficial in terms of the sustainability of public finances. However, we should note that these results may only hold true up until a certain age. Even though life expectancy of individuals is increasing, after a certain point individuals may simply not be able to work due to, for example, health related reasons. On the other hand, increasing the SRA could also have longer term upstream effect on employment rate before 65, which would in turn have positive budgetary effects in the future.

The effects of further increases in the SRA will also depend on the role of this age in shaping retirement behavior in the future. The different potential determinants of the bunching at the SRA – liquidity constraints, norms, employers' effects and second-pillar financial incentives – may not be constant over time. A better understanding of the relative importance of these channels remains an interesting direction for future research.

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Appendices

A. Computation of the effect on the average retirement age

This appendix describes the computation of the effect of the reform on the average retirement age. We use the coefficients estimated in the regression discontinuity models presented in section 3.2:

$$y_{ia}^{c} = \alpha_{ca} + \beta_{ca}T_i + \gamma_j af(Z_i - c_c) + \delta_c af(Z_i - c_c)T_i + \eta X_{ia} + \epsilon_{ia} \tag{A.1}$$

The RD coefficients we are interested in are the β_{ca} coefficients. They give, a given outcome y, the effect of the increase in the SRA for a given monthly age a and for a given cutoff c, for the treated group (with SRA increase) relative to the control group (no SRA increase)

Using employment as the y variable β_{ca} coefficients measure the effect of the reform on the probability to be employed, and can be interpreted as follows: with the reform, the probability be employed at age a, i.e to retire later than age a, is β_{ca} bigger. Formally, if we note X_R the random variable of the observed retirement age for the control cohort (on the left-side of the cutoff j) and X_R^{cf} the counterfactual one absent the reform:

$$P[X_R > a] = P[X_R^{cf} > a] + \beta_a \tag{A.2}$$

The effect of the reform on the average retirement age can be defined as the difference between the observed average retirement age and the counterfactual one, absent the reform [21], using monthly age in the sum.

[21] The following calculation are inspired by Mastrobuoni (2009) (eq (4) in p. 1229)).

$$
\begin{aligned}
\Delta_c &= \sum_{a=720}^{798} aP[X_C = a] - \sum_{a=720}^{798} aP[X_C^{cf} = a] \\
&= \sum_{a=720}^{798} a(P[X_R = a] - P[X_R^{cf} = a]) \\
&= \sum_{a=720}^{798} a(P[X_R > a-1] - P[X_R^{cf} > a-1] - P[X_R > a] + P[X_R^{cf} > a]) \\
&= \sum_{a=720}^{798} a(\beta_{a-1} - \beta_a)
\end{aligned}
$$

The third step of the computation is obtained from the following property of the CDF : $P[X = x] = P[X > a-1] - P[X > a]$. The last steps directly comes from equation A.2. This expression can be simplified if there is an age a_{min} (resp. a_{max}) below (resp. above) which there is no effect of the reform (i.e $\beta_{a,c} = 0$ for $a \leq a_{min}$ or $a \geq a_{max}$)

$$
\begin{aligned}
\Delta_c &= \sum_{a=a_{min}}^{a_{max}} a(\beta_a - \beta_{a-1}) \\
&= a_{min}(0 - \beta_{a_{min}}) + (a_{min}+1)(\beta_{a_{min}} - \beta_{a_{min}+1}) + ... + a_{max}(\beta_{a_{max}-1} - 0) \\
&= \sum_{a=a_{min}}^{a_{max}-1} \beta_a
\end{aligned}
$$

We can than compute the effect on the reform on the average retirement age as the sum of the β coefficients estimated for a given cutoff.

B. Literature review

Table B.1: Comparison with results of related studies on effects near ERA, NRA or SRA[a]

Study	Country	Reform	Method	Results		At ERA, NRA or SRA		
				Employm. rate pp	Retirem. rate pp	Empl. rate[b] %	Hazard rate[c] Level	Bunching[d] %
Staubli &	AUT	♂: ERA 60 → 62	DID	+9.8	–24.8	28	0.50	14
Zweimüller (2013)		♀: ERA 55 → 58+3m		+11.0	–25.4	57	0.25	14
Vestad (2013)	NOR	ERA 64 → 62	DID	–33.2	×	65	0.46	30
Atalay & Barrett (2015)	AUS	♀: NRA 60 → 65	DID	+7.7	×	30–50	×	×
Cribb et al. (2016)	UK	♀: ERA 60 → 62	DID	+6.3	–11.5	55	0.25	14
De Vos et al. (2018)	NLD	SRA 65 → 65+6m	DID	+10	×	43	0.35	15
Rabaté & Rochut (2019)	FRA	NRA 60 → 61	DID	+20.9	–47.8	45	0.50	23
Geyer & Welteke (2019)	GER	♀: ERA 60 → 63	RDD	+13.5	–27.6	62	0.19	12
This paper	NLD	More generous ER[e]						
		SRA 65 → 65+2m	RDD	+4.5	–13.5	15	0.50	8
		Less generous ER[f]						
		SRA 65+3m → 66+4m	RDD	+20.3	–58.6	29	0.70	20

NOTES: [a]Exact references for the values reported in this table can be found in Table C.1 in the online appendix. [b]The employment rate just before the ERA, NRA or SRA. [c]The drop in the share of employed persons at the SRA over the share of employed persons just before the ERA, NRA or SRA. [d]The share of employed persons retiring at the ERA, NRA or SRA. [e]Averages for cutoff 1 and 2. [f]Averages for cutoffs 3 to 6.

Table B.2: Comparison with related studies looking at average retirement age[a]

Study	Country	Reform	Method	Results	
				Ave. retirement age per month ERA/NRA/SRA[b]	Ave. claiming age per month ERA/NRA/SRA[c]
Mastrobuoni (2009)	USA	NRA 62 → 65	RKD	♂: +0.8, ♀: +0.6	×
Manoli & Weber (2018)	AUT	♂: ERA 60 → 62.5	RDD,	+0.20 to +0.36	+0.49 to +0.54
		♀: ERA 55 → 58.5	RKD	+0.39 to +0.55	+0.54 to +1.03
Lalive et al. (2019)	SWI	♀: FRA 62 → 64	RDD	+0.41 to +0.65	+0.69 to +0.72
This paper	NLD	More generous ER[d]			
		SRA 65 → 65+2m	RDD	+0.02 to +0.06	+0.11 to +0.12
		Less generous ER[e]			
		SRA 65+3m → 66+4m	RDD	+0.13 to +0.17	+0.21 to +0.23

NOTES: [a]Exact references for the values reported in this table can be found in Table C.2 in the online appendix. [b]The increase in the average retirement age per month increase in the ERA, NRA or SRA. [c]The increase in the average claiming age of retirement benefits per month increase in the ERA, NRA or SRA. [d]Min and max for cutoff 1 and 2 in Table 7. [e]Min and max for cutoff 3 and 6 in Table 7.

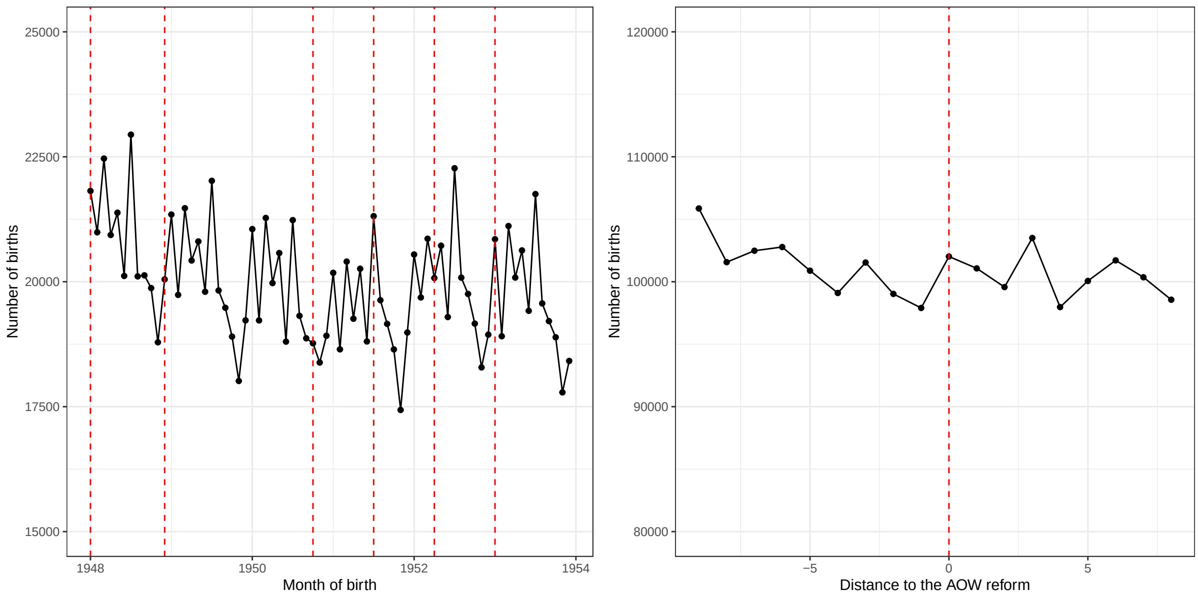
Supplementary material for online appendix

A. Additional results

Number of births by date of birth

Figure A.1: Distribution of the number of births data by year

(a) Births by month of birth (b) Births by distance to the AOW reform



Differences-in-differences results

Following Staubli and Zweimüller (2013), we estimate the effect of the increase in the SRA by comparing the trajectories for e.g. retirement and employment of different cohorts facing different AOW ages. Specifically, we use the following baseline specification in the differences-in-differences (DID) analysis:

$$y_{iact} = \beta_0 + \delta_c + \theta_a + \beta_1 \mathrm{I}(\text{age} < \text{AOW})_{iact} + X'_{iat}\beta_2 + \epsilon_{iact}. \tag{A.1}$$

In this specification, δ_c are AOW cohort dummies and θ_a are age dummies (in months). X_{iat} represent demographic and macroeconomic controls. In our main specification, we include two cohorts effects, before and after the AOW age, as the cohort effects exhibit different patterns from both sides of this age.

The parameter of interest is β_1, which indicates the difference in the outcome variable before and after individuals reach the AOW age, between different cohorts. This parameter can be estimated by including a dummy variable that indicates the interaction between the age of an individual and the SRA cohort that he or she belongs to. In equation (A.1), the $\mathrm{I}(\text{age} < \text{AOW})_{iact}$ variable represents this interaction. This dummy variable equals one for individuals below the SRA that is applicable within their cohort, and zero for individuals that have reached this age. As individuals are affected by the reform at different ages depending on the cohort, the value of this variable changes over cohorts as well as over time. Note that the outcomes are binary variables, and we estimate linear probability model. As a result, β_1 can be interpreted as the percentage point difference in the probability that an individual is in a particular state for a cohort for which the age is below the SRA compared to a cohort for which the age is above or equal to the SRA.

In the analysis we focus on cohorts born between January 1950 and April 1953. Indeed, Figure 1 shows a clear discontinuity in the trend by age before and after the cohorts born in 1950, driven by the 2006 early retirement reform. This implies that the parallel trend assumption does not hold for cohorts born in 1950 or after when compared to cohorts born before 1950. Therefore, in our main analysis we focus on cohorts born in 1950 or thereafter. For those cohorts, we expect to have valid pre-trends according to Figure 1. This can be tested formally by estimating the following fully interacted differences-in-differences specification:

$$y_{iact} = \alpha_0 + \delta_c + \theta_a + \alpha_1(\delta_c \times \theta_a) + X_{iat}\alpha_2 + \epsilon_{iact}, \tag{A.2}$$

where we expect the α_1 coefficients to be small and statistically insignificant before the age of the change in the SRA, and significant for the ages a for which different cohorts c face a different SRA.

Note that this test for the absence of effect before the SRA is also a test for the existence of upstream or horizon effects. As discussed in the results section below, an increase in the SRA can also have employment effects before this age, as increasing the horizon of retirement can induce both labor demand and supply changes that can affect e.g. the employment rate (Hairault et al., 2010). We expect some of the SRA α_1 coefficients for employment to be significant in the presence of (positive) upstream effects.

Results of the estimation of equation A.1 and A.2 are presented below in Tables A.1 and A.2 and Figure A.2.

Table A.1: DID result: main analysis

	Ret.	Emp.	Unemp.	Dis.	Welf.	Oth.	Noinc
Under AOW	-52.42	16.19	2.609	11.89	2.733	3.211	15.78
	(0.466)	(0.321)	(0.0801)	(0.108)	(0.0834)	(0.346)	(0.187)
Observations	35611989	35611989	35611989	35611989	35611989	35611989	35611989
Pre-reform mean	19.46	41.29	4.4	12.51	2.69	3.55	16.11

NOTES: Cluster-robust standard errors in parentheses (clustered by month of birth), * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Pre-reform means correspond to the average computed for the treatment cohorts for age group 62–65.

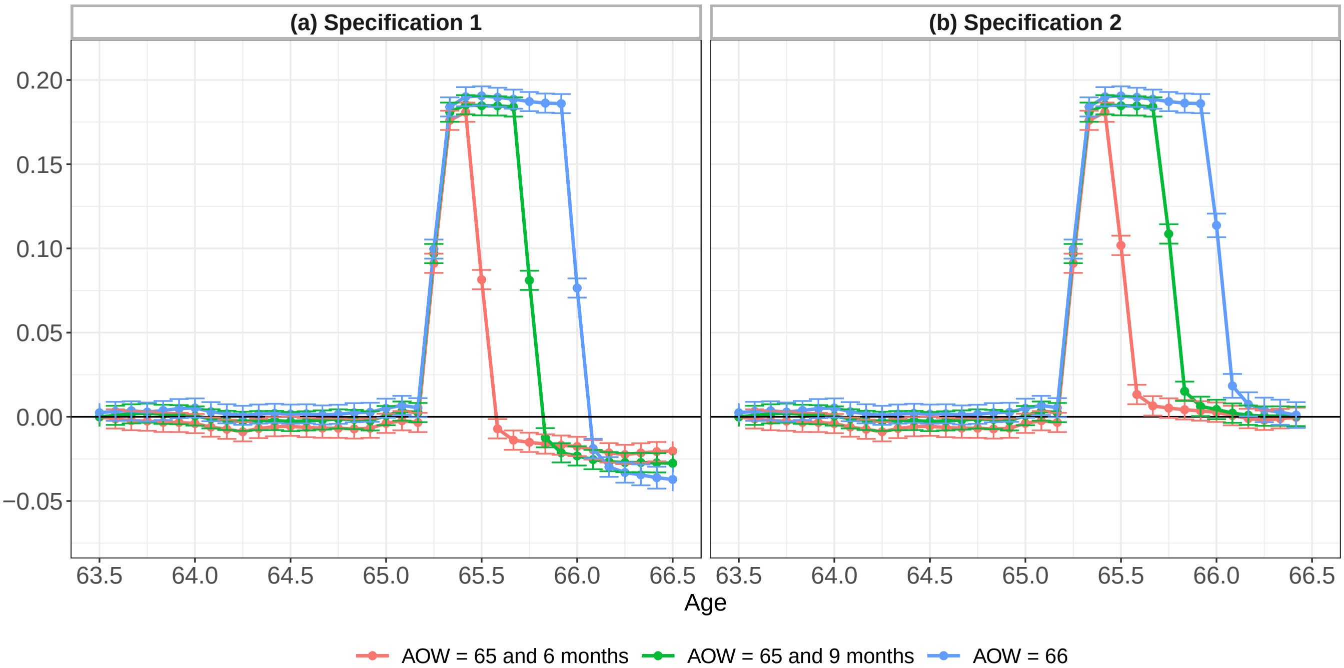
Table A.2: DID result: robustness

	Ref	RT1	RT2	RT3	RT4	RT5	RT6	RT7
Under AOW	16.29	17.31	17.31	17.33	17.04	17.01	17.21	15.53
	(0.315)	(0.0446)	(0.160)	(0.160)	(0.182)	(0.171)	(0.160)	(0.295)
Observations	33014061	33354485	33354485	33354475	33014061	33014061	33014061	16605667

NOTES: Cluster-robust standard errors in parentheses (clustered by month of birth), * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. First column corresponds to the specification used in Table A.1. The next columns correspond to the seven robustness tests:

Column (1): Reference model, with demographic and time effect proxy with unemployment rate, different cohort effects before and after 65 and clustering at the month of birth level.
Column (2): Estimation without controls without clustering
Column (3): Column (2) + clustering.
Column (4): Column (3) + demographic controls.
Column (5): Column (4) + time effect proxy with unemployment rate.
Column (6): Column (5) + quarter dummies.
Column (7): Column (6) + year dummies.
Column (8): Reference model with sample restriction for the first increase in the AOW only.

Figure A.2: Robustness check: fully interacted DID, effect on share employed



NOTES: Specification 1 (resp. 2) : without (resp. with) different cohort effects before and after the SRA)

B. Full results tables

Table B.1: Full table for main analysis

	Cutoff 1	Cutoff 2	Cutoff 3	Cutoff 4	Cutoff 5	Cutoff 6
Employment	3.810	5.251	20.351	18.943	19.942	21.793
	(0.249)	(0.281)	(0.444)	(0.446)	(0.444)	(0.458)
Retirement	−11.879	−15.083	−58.067	−58.945	−57.457	−59.814
	(0.302)	(0.341)	(0.465)	(0.459)	(0.451)	(0.462)
Unemployment	0.405	0.432	3.627	3.962	3.805	3.778
	(0.047)	(0.062)	(0.163)	(0.162)	(0.158)	(0.156)
Disability	6.092	6.309	12.293	12.679	13.474	13.314
	(0.147)	(0.162)	(0.276)	(0.279)	(0.276)	(0.277)
No income	−0.066	0.030	16.121	16.403	15.143	14.208
	(0.031)	(0.038)	(0.304)	(0.303)	(0.297)	(0.290)
Welfare	1.145	1.109	3.022	4.098	2.645	3.814
	(0.080)	(0.086)	(0.160)	(0.164)	(0.170)	(0.173)
Others	0.497	1.973	2.647	2.873	2.458	2.921
	(0.083)	(0.113)	(0.153)	(0.156)	(0.161)	(0.167)
No. obs.	416129	382190	299082	295989	300325	288292

Table B.2: Full table for robustness analysis

	RTref	RT1a	RT1b	RT2a	RT2b	RT3a	RT3b	RT4a	RT4b	RT5a	RT5b
I_emp	18.943	18.705	21.121	18.943	18.943	20.415	18.217	19.312	16.541	20.155	19.891
	(0.446)	(0.281)	(0.700)	(1.166)	(0.267)	(0.590)	(0.297)	(0.553)	(0.753)	(0.440)	(0.424)
Cluster	No	No	No	Month	Year	No	No	No	No	No	No
Polynomial degree	2	1	3	2	2	2	2	2	2	2	2
Left bandwith	9	9	9	9	9	6	18	9	8	3	3
Right bandwith	9	9	9	9	9	6	26	8	8	3	3
No. obs.	295989	295989	295989	295989	295989	197025	599271	278101	262173	83890	83890

Table B.3: Full table for heterogeneity analysis

	all	men	women
Employment definition 1	18.943	23.247	14.963
	(0.446)	(0.690)	(0.552)
Employment definition 2	13.485	16.904	10.525
	(0.513)	(0.778)	(0.643)
No. obs.	295989	146614	149375

Table B.4: Full table for RD on the average retirement age

	(1)	(2)	(3)	(4)
Treatment effect	1.281	1.830	0.631	0.898
	(0.327)	(0.560)	(0.331)	(0.566)
Retirement definition	Definition 1	Definition 1	Definition 2	Definition 2
Polynomial degree	1	2	1	2
No. obs.	153295	153295	159528	159528

Table C.1: Exact references outcomes related studies in Table B.1

Study	Results		At ERA, NRA or SRA		
	Employment rate	Retirement rate	Employment rate	Hazard rate	Bunching
Staubli &	♂: Table 3, column (2)	Table 3, column (2)	Figure 4 (A)		
Zweimüller (2013)	♀: Table 3, column (6)	Table 3, column (6)	Figure 5 (A)		
Vestad (2013)	Table 2, column 'DD estimate'	×	Figure 2		
Atalay & Barrett (2015)	Table 3, column 'Full Sample'	×	♂: Figure 2, at age 60		
Cribb et al. (2016)	Table 4	Table 5, "Retired"	Figure 2, age 59		
De Vos et al. (2018)	Page 22	×	Figure 12		
Rabaté & Rochut (2019)	Table 5, column 'Employment'	Table 5, column 'Retirement'	Figure 2 (a)		
Geyer & Welteke (2019)	Table 1, column (1)	Table 1, column (5)	Figure 2 (a)	Figure 4	Figure 4
This paper	More generous ER				
	Table 6	Table 6	Figure 1	Figure 1	Figure 1
	Less generous ER				
	Table 6	Table 6	Figure 1	Figure 1	Figure 1

Table C.2: Exact references outcomes related studies in Table B.2

Study	**Results**	
	Ave. retirement age	Ave. claiming age
Mastorbuoni (2009)	♂: Table 5, column (2), ♀: Table 5, column (2)	×
Manoli & Weber (2018)	♂: Table 4, 'Short contribution years'	Table 4, 'Short contribution years'
	♀: Table 4, 'Short contribution years'	Table 4, 'Short contribution years'
Lalive et al. (2020)	Table 4, columns (1) and (3), panel C	Table 4, columns (1) and (3), panel A
This paper	More generous ER	
	Table 7	Table 7
	Less generous ER	
	Table 7	Table 7

D. Datasets

We hereby present the different dataset we used in this analyses. Table D.1 below present the version of the files we use.

gbapersoontab[22]

It contains demographic background data (e.g. gender, year of birth, migration background) for the universe of the Dutch population, that is all persons who appear in the registered in the population register (Basic Register of Persons, BRP) since 1 October 1994.

gbaoverlijdentab[23]

Contains the date of death of all persons who have died since 1 October 1994 and were registered in the population register (Basic Register of Persons, BRP) at the time of death. It also contains the date of death of persons who are not residents but were once residents of the Netherlands since 1 October 1994 and whose information about the death is received in the Register of Non-Residents (RNI). The main source of information for this dataset is the municipal registries (Gemeentelijke Basisadministratie Persoonsgegevens, GBA).

gbamigratiebus[24]

It contains all migration spells for the full universe of the Dutch population (as defined in the gbapersoontab). For each immigration (resp. emigration) spell, a date of beginning and end is registered, as well as the country of origin (resp. destination). For each individual, we have as many spells as migration events occurring since 1994. The main source of information for this dataset is the municipal registries (Gemeentelijke Basisadministratie Persoonsgegevens, GBA).

gbahuishoudensbus[25]

For the full universe of the Dutch population (as defined in the gbapersoontab), it contains information about the household composition: their place in the household, and the details of the household they belong to (e.g couple or not, married or not, with or without children, etc.). Retrospective information is available, as the data is presented as spells (one additional line when one characteristic of the household changes). The main source of

[22] Link to gbapersoontab documentation in Dutch
[23] Link to gbaoverlijdentab documentation in Dutch
[24] Link to gbamigratiebus documentation in Dutch
[25] Link to gbahuishoudensbus documentation in Dutch

information for this dataset is the municipal registries (Gemeentelijke Basisadministratie Persoonsgegevens, GBA).

polisbus[26] and spolisbus[27]

It contains information on the full universe of job in the Netherlands, available from year 2006. There is one line by employment spells, with information on both the individual (wage, hours worked, contributions, etc) and the firm (sector, collective agreement, etc).

secm datasets[28]

The secm datasets contain monthly information on the income receive each month from year 1999 for different types of incomes: employment wage (SECMWERKNDGAMNBEDRABUS), profit (SECMZLFMNDBEDRAGBUS), other activities (SECMOVACTMNDBEDRAG-BUS), unemployment benefits (SECMWERKLMNDBEDRAGBUS), disability benefits (SECMZIEKTAOMNDBEDRAGBUS), other benefits (SECMSOCVOORZOVMNDBEDRAG-BUS) welfare (SECMBIJSTMNDBEDRAGBUS) and pension income (SECMPENSIOEN-MNDBEDRAGBUS).

These datasets are constructed by Statistic Netherlands using different administrative data sources (taxes, social security, pension funds). The initial form of the dataset is spell data, and contains a date of beginning, a date of end and an associated monthly amount. A new line is added for a given individual everytime the monthly amount she perceives changes. The secmbus dataset combines the different sources mentioned above in a single dataset containing the main source of income and associated amount for each spell.

vehtab[29]

The vehtab data provide information about the wealth of the full universe of the Dutch household. It is available from year 2006, and contains on a yearly basis the value of asset and debt owned, for different types of wealth (e.g financial assets, business assets, housing). The vehtab data do not cover all wealth in the national accounts, as pension wealth is not included. Depending on the type of wealth, the value is either observed (from tax data) or computed by Statistic Netherlands.

[26] Link to polis documentation in Dutch
[27] Link to spolis documentation in Dutch
[28] Link to secm documentation in Dutch
[29] Link to vehtab documentation in Dutch

Table D.1: Versions of the datasets used in the analyses

Content	Name of dataset	Source
Date of birth and gender	GBAPERSOON2019TAB (V1)	Population registers
Death	GBAOVERLIJDENTAB2019TAB (V1)	Death records
Migration	GBAMIGRATIE2019BUSV2 (V1)	Migration records
Households characteristics	GBAHUISHOUDENS2019BUS (V1)	SSB
AOW benefits	AOWUITKERING1ATAB2007 ()	SSB
	AOWUITKERING1ATAB2008 (V1)	SSB
	AOWUITKERING1ATAB2009 (V1)	SSB
	AOWUITKERING1ATAB2010 (V1)	SSB
	AOWUITKERING1ATAB2011 (V1)	SSB
	AOWUITKERING1ATAB2012 (V1)	SSB
	AOWUITKERING1ATAB2013 (V1)	SSB
	AOWUITKERING1ATAB2014 (V1)	SSB
	AOWUITKERING1ATAB2015 (V1)	SSB
	AOWUITKERING1ATAB2016 (V1)	SSB
	AOWUITKERING1ATAB2017 (V1)	SSB
	AOWUITKERING1ATAB2018 (V1)	SSB
	AOWUITKERING1ATAB2019 (V1)	SSB
Individual income		
Wage income	SECMWERKNDGAMNBEDRABUSV20191	SSB
Profits from self-employment	SECMZLFMNDBEDRAGBUSV20191	SSB
Income from other activity	SECMOVACTMNDBEDRAGBUSV20191	SSB
Social welfare benefits	SECMBIJSTMNDBEDRAGBUSV20191	SSB
UI benefits	SECMWERKLMNDBEDRAGBUSV20191	SSB
DI and sickness benefits	SECMZIEKTAOMNDBEDRAGBUSV20191	SSB
Other social security benefits	SECMSOCVOORZOVMNDBEDRAGBUSV20191	SSB
Pension income	SECMPENSIOENMNDBEDRAGBUSV20191	SSB
Activity sector	POLISBUS 2007 (V1)	SSB
	POLISBUS 2008 (V1)	SSB
	POLISBUS 2009 (V1)	SSB
	SPOLISBUS 2010 (V1)	SSB
	SPOLISBUS 2011 (V1)	SSB
	SPOLISBUS 2012 (V1)	SSB
	SPOLISBUS 2013 (V2)	SSB
	SPOLISBUS 2014 (V1)	SSB
	SPOLISBUS 2015 (V3)	SSB
	SPOLISBUS 2016 (V3)	SSB
	SPOLISBUS 2017 (V2)	SSB
	SPOLISBUS 2018 (V5)	SSB
	SPOLISBUS 2019 (V5)	SSB