Increasing the retirement age: policy effects and underlying mechanisms

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By Simon Rabaté, Egbert Jongen, and Tilbe Atav*

We study the effects of increasing the statutory retirement age (SRA) in the Netherlands, using RDD and administrative data on the universe of the population. We find clear and large employment effects of the reform. A simple model in which individuals stay longer in their pre-SRA labor market state predicts the treatment effects well. The employment level before the SRA and the retirement hazard at the SRA are the key determinants of the effects of the policy change. Exploring potential explanations for the high hazard rate observed in the Netherlands, our results point to an important role of employers' effects.
JEL: J14, J26
Keywords: Statutory retirement age, employment, social insurance, bunching, Netherlands

The sustainability of public finances is a major concern in many countries, due to aging populations and declining fertility rates. 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 effective levers to extend the working life of older workers. The impact of the increase in the early or statutory retirement age on public finances depends crucially on how this affects labor market outcomes. On the one hand, next to the direct savings on retirement benefits, the government may benefit from additional tax receipts from continued employment beyond the old retirement age. On the other hand, the government may spend more on other types of social insurance like unemployment insurance (UI) and disability insurance (DI), which may act as 'alternative pathways' to retirement. In part, these additional government receipts and expenditures will be mechanical, as individuals simply remain in their pre-SRA labor market state longer (Staubli and Zweimüller, 2013,

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Manoli and Weber, 2018, Oguzoglu, Polidano and Vu, 2020, Geyer and Welteke, 2021). But the reform may also change the behavior of individuals between the old and the new SRA, resulting in active substitution from e.g. employment to UI or DI, driving up the additional government expenditures. Furthermore, the reform may also change the behavior of individuals before the old SRA, resulting in so-called upstream or horizon effects (Hairault, Sopraseuth and Langot, 2010, Jacobs, 2010), or after the new SRA, which we may call downstream effects. Determining the empirical relevance of the mechanical and behavioral effects on labor market outcomes is therefore key to studying the overall effect of changes in the retirement age on public finances.

In this paper we consider the mechanical and behavioral effects of recent reforms in the SRA in the Netherlands, which led to a step-wise increase in the SRA from 65 years in 2012 to 66 years and 4 months in 2019. We provide a comprehensive assessment of the different effects and also consider the underlying mechanisms that shape and shift the retirement behavior.

We leverage the sharp cohort-based shifts in the SRA in a regression discontinuity (RD) design, to present well-identified and comprehensive causal effects of the reform. To do so, we rely on administrative data on various types of income, wealth and job characteristics for the universe of the Dutch population for the period 2007–2020. We analyze the effect on a set of labor market outcomes, including retirement, employment and the use of different types of social insurance, and on public expenditures and receipts related to these labor market outcomes. We consider the empirical relevance of mechanical and behavioral effects, by relating the estimated outcomes to the predictions of a simple mechanical model. We also study how the (local) treatment effects at a given age relative to the SRA translate into a treatment effect on the (global) average retirement age. Furthermore, we consider how the mechanical model can be helpful in understanding the wide range of estimates in the quasi-experimental literature on the effects of changes in the (early) retirement age. Finally, by comparing bunching of retirement at the SRA for different subgroups in the population, we explore the relative importance of the following channels that potentially play a role in this: kinks in the budget constraint, credit constraints, the demand side and social norms.

Our main findings are as follows. First, we find substantial effects on the employment rate (+21pp) and substantial effects on the participation in social insurance (+22pp, DI in particular), between the old and the new SRA. Furthermore, despite substantial additional costs on social insurance, the savings on retirement benefits and the additional tax receipts on labor and profit income lead to a substantial net gain for the government of about 65 million euro per month between the old and the new SRA. Second, we find no evidence of upstream effects before the old SRA or downstream effects after the new SRA, and substitution towards social insurance between the old and the new SRA is almost completely mechanical. Indeed, a simple mechanical model predicts the estimated treatment effects well. Converting the local effect on retirement to an effect on the average retirement age, we find that per month increase in the SRA, the average retirement age increases by about 0.2 months. We further show that the mechanical model is also helpful in understanding the smaller effects for cohorts that were born before 1950, many of which could still use the generous early retirement scheme, resulting in lower pre-SRA employment rates than cohorts born later. The mechanical model is also consistent with the typical findings in the quasi-experimental literature on (early) retirement reforms, and we show that the pre-SRA employment rate and the hazard rate into retirement at the retirement age are key to understanding the different estimates for different contexts. Finally, regarding the underlying mechanisms that determine the bunching into retirement at the SRA in the Netherlands, we find three times as much bunching for employees when compared to the self-employed. This is consistent with an important role for employment protection and automatic job termination in the Netherlands. We also find that bunching is higher in sectors that have relatively steep wage profiles and in sectors that were hit particularly hard by the Great Recession, again consistent with a role for the demand side in shaping retirement patterns. We further find that the bunching of self-employed is still substantial, suggesting that social norms also play an important role next to demand side factors. Furthermore, we find slightly higher bunching for individuals with relatively low 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 kinks in the budget constraint in the observed bunching in the Netherlands.

Our analysis relates to the rich body of literature analyzing the effects of reforms of the early retirement age (ERA), the normal retirement age (NRA) and the SRA. Evaluations of shifts in the ERA, pioneered by Staubli and Zweimüller (2013) for Austria[1], find strong effects on retirement and employment, as well as important (though mostly mechanical or 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.[2] 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 of this bunching.[3]

Aside from providing a clean evaluation for the Dutch context[4], our paper makes the following contributions to this literature. First, we provide a simple

[1]Other studies include Manoli and Weber (2018) for Austria, Atalay and Barrett (2015) and Oguzoglu, Polidano and Vu (2020) for Australia, Rabaté and Rochut (2019) for France, Geyer and Welteke (2021) and Seibold (2021) for Germany and Cribb, Emmerson and Tetlow (2016) for the UK.

[2]See also Manoli and Weber (2018) for Austria and Lalive, Magesan and Staubli (2020) for Switzerland.

[3]See also Behaghel and Blau (2012), Brown (2013), Lalive, Magesan and Staubli (2020) and Seibold (2021).

[4]A preliminary differences-in-differences analysis of the employment effects using the Labor Force Survey for the first cohorts affected by the SRA reforms can be found in De Vos, Kapteyn and Kalwij (2019). Note that we also contribute to the empirical literature on the ERA reform in the Netherlands in 2006.

yet insightful framework for the analysis of SRA reforms, which can be easily transposed to other reforms (ERA increase) or countries. In our context, we find that a simple mechanical model – where most individuals continue in the state they were in before the old retirement age – predicts the estimated treatment effects for the different labor market states well. In such settings, treatment effects are determined by two key parameters: i) the pre-SRA employment rate and ii) the hazard rate into retirement at the SRA. We illustrate this by showing that the effects of shifts in the SRA on the employment rate are much larger for cohorts that faced less generous ER schemes, because more individuals remain in the labor force until the SRA. More generally, it emphasizes the important role of other policies that affect the pre-SRA employment rate, like early retirement schemes but also the generosity and entry conditions of UI and DI. Higher pre-retirement employment rates increase the effectiveness of shifts in the SRA.

Second, we provide key insights that tie together the wide range of effects found in the empirical literature on pension reforms. Specifically, we tie together the literature that considers the local effect of ERA and SRA reforms on retirement and employment between the old and the new ERA and SRA respectively (Staubli and Zweimüller, 2013, Atalay and Barrett, 2015, Cribb, Emmerson and Tetlow, 2016, Rabaté and Rochut, 2019, Geyer and Welteke, 2021) with the literature that considers the effect on the average retirement age, including potential upstream and downstream effects (Mastrobuoni, 2009, Manoli and Weber, 2018, Lalive, Magesan and Staubli, 2020). We formally show how to use the local RD estimates to calculate the effect on the average retirement age. Furthermore, we show that the wide range of point estimates found in the literature – with the employment effect ranging from 6.3 percentage points in Cribb, Emmerson and Tetlow (2016) to 20.9 percentage points in Rabaté and Rochut (2019) – are in fact qualitatively quite similar. Differences across studies in different countries are largely driven by differences in the two key factors we put forward, employment rates just before the ERA/SRA and the hazard rate into retirement at the ERA/SRA.

Finally, our paper contributes to the literature that considers the mechanisms that cause bunching at key ages of the pension system, an old puzzle in the literature on retirement patterns (Lumsdaine, Stock and Wise, 1996). Our findings point towards an important role for the demand side in bunching at the SRA in the Netherlands, a determinant largely overlooked in the literature, except for Rabaté (2019) for the French case. We also find suggestive evidence that social norms and reference-dependent preferences play an important role, consistent with the findings of Behaghel and Blau (2012), Lalive, Magesan and Staubli (2020) and Seibold (2021). We find that bunching is somewhat larger for individuals with relatively low wealth, which is consistent with some role for liquidity constraints in the bunching into retirement at the SRA. This contrasts with the findings of Cribb, Emmerson and Tetlow (2016), who do not find differences by wealth level for the effect of the increase in the ERA in the UK, using data on housing wealth (although this is typically less liquid than other forms of wealth). Finally, we find hardly any differences between employees in different sectors that face 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 only a limited role in bunching at the SRA. This is in line with the results found by Behaghel and 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 and Weber (2016) who find small elasticities of the retirement age to financial incentives.

The outline of the paper is as follows. Section 2 discusses the institutional context, the SRA reforms and reforms in ER and social insurance that may interact with the SRA reforms. This section also presents the simple mechanical model. 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 different mechanisms in bunching at the SRA. Section 6 concludes. Additional results are given in an appendix. An online appendix contains supplementary material.

I. Institutional background and potential reform effects

A. *The pension system and reforms*

THE DUTCH PENSION SYSTEM. — 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 before 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). Individuals start receiving the first-pillar pension once they have reached their birth-cohort specific 'AOW age' or SRA. Individuals cannot bring any first-pillar pension benefits forward when they retire earlier. Furthermore, for most employees there is mandatory retirement at the SRA, employment contracts end by law. When an individual worker wants to continue to work beyond the SRA, the employer and the worker have to draw up a new contract.[5] Also, individuals are no longer entitled to unemployment insurance benefits or disability insurance benefits beyond the SRA.

The second pillar consists of firm- and sector-specific funded pension schemes. The benefits from the second pillar supplement the first-pillar benefits. Pension

[5] According to the OECD (2014, p. 94), 92% of open-ended labor contracts in the Netherlands end when the SRA is reached.

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. 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, Kapteyn and Kalwij, 2019).

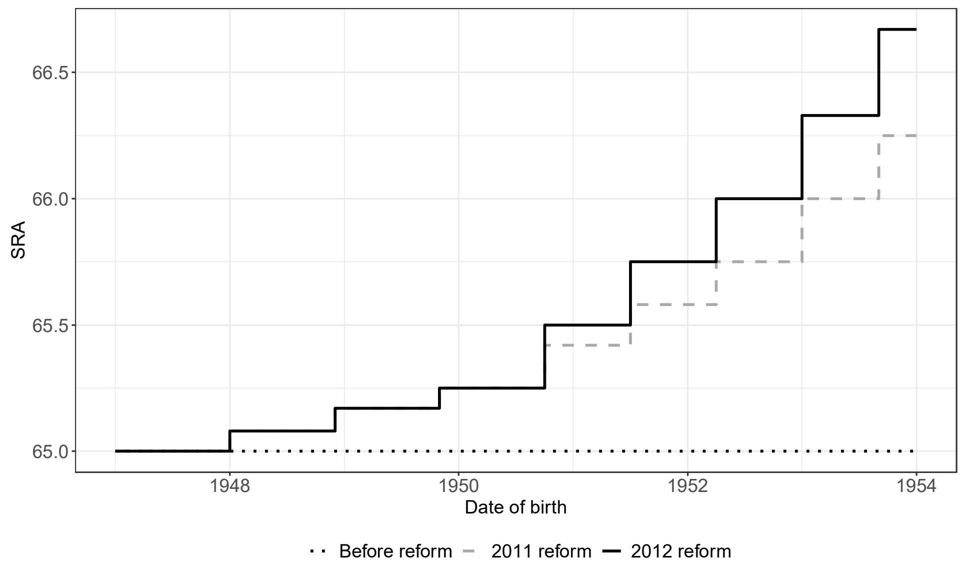
The third pillar consists of individual savings for retirement. Individuals can accumulate 1.875% of their average wage income for the expected retirement period per year tax free, via earmarked personal savings or life insurance schemes. Over a working life of 40 years this amounts to 75% of the average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

Knoef et al. (2017) calculate replacement rates for a representative sample of the Dutch population, combining data on first, second and third pillar pensions 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. On average, 39 percentage points come from the first pillar and 29 percentage points come from the second pillar. Adding income from third-pillar pension savings and other assets (including housing wealth), raises the median replacement rate to 82 percent.

Pension reforms. — 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 until 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 an increase in the SRA from 2013 onwards. The dashed line in Figure 1 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, the solid line in Figure 1. These step-wise increases in the SRA are the focus of our analysis.

There are a number of reforms in early retirement schemes and the second-pillar pension system that are important for our analysis of the SRA reforms (see also Table 1). First, in 2006, there was a major reform of the early retirement schemes. The reform package resulted in lower early retirement benefits and early retirement benefits that were more actuarially fair for cohorts born after December 1949. Early retirement benefits for cohorts before January 1950 were unaffected. Financial incentives to postpone early retirement increased substantially for cohorts born after December 1949. This reform substantially increased employment rates before the SRA for cohorts born after 1949, see Lindeboom and Montizaan (2020) and appendix B, where we discuss this reform and its effects in more detail. Second, in 2012, the *Doorwerkbonus* (Deferred Pension Bonus) was introduced. This was an age-dependent tax credit for working individuals in the age range 62-67. The tax credit was particularly high for individuals 63 and 64 (up to 4,600 euro). The Deferred Pension Bonus was reformed somewhat in 2013, becoming the *Werkbonus* (Workbonus), but was then phased out between 2015 and 2018.

Figure 1. : Reforms in the SRA in the Netherlands



NOTES: This figure presents the evolution of the SRA implemented by the 2011 and 2012 reforms. SRA increases gradually, based on the month of birth of the individuals. The initial pace of the increase decided upon in 2011 (dashed grey line) was accelerated in 2012, as depicted in the figure.

However, we expect that this reform hardly affects our results, since it mostly targets individuals a few years before the SRA, the available evidence suggests the effect on this group was limited (CPB, 2020*b*), and control and treatment groups in our RDD analysis of the SRA are affected in very similar ways by this reform. Finally, there was a reduction in the maximum accrual rate of tax-favored savings for second-pillar pensions in 2015 ('Witteveenkader'). Tax-favored savings were restricted to earnings up to 100 thousand euro. This may have affected second-pillar savings and total wealth accumulation. However, wealth effects on employment and retirement are generally found to be small (see e.g. Van Erp, Vermeer and van Vuuren, 2014), and the control and treatment groups in our analysis of the SRA are affected in very similar ways by this reform.

ALTERNATIVE PATHWAYS. — Individuals can also exit the labor force before the SRA using so-called alternative pathways, most importantly UI and DI.[6] A change in the SRA may lead to increased substitution towards social insurance programs.

Unemployed individuals are entitled to UI if they did not quit their job and worked at least 26 weeks in the last 36 weeks. The individual receives a benefit

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

Table 1—: 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 from 60 to 38 months	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	Deferred Pension Bonus becomes Workbonus		
2015	Accelerated gradual increase SRA	Phase out of Workbonus, reduction in tax favored savings		
2016			Gradual shortening of max. benefit duration from 38 to 24 months	

Source: De Vos, Kapteyn and Kalwij (2019) appended.

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. The minimal duration of the UI benefits is three months. The maximum duration of UI benefits was first cut from 5 years to 3 years and 2 months in 2006. Subsequently, over the years 2016 to 2019 it was gradually cut further from 3 years and 2 months to 2 years. The reduction in the maximum duration of UI benefits will reduce the share of individuals in UI and is also likely to increase employment.[7] The most important reform for our analysis is the reduction from 3 years and 2 months to 2 years. Since this reform was gradual (spread out over the years 2016-2019), and affects our treatment and control groups in our SRA analysis in a very similar way, we expect this reform to have a limited effect on our estimates of the SRA reforms.

Individuals may 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. 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

[7]See De Groot and Van der Klaauw (2019) for an analysis of the reduction in the maximum UI duration from 5 years to 3 years and 2 months.

persons. This reform led to a reduction in the inflow into DI (see e.g. Koning and Lindeboom, 2015). However, since our treatment and control groups in our SRA analysis will be affected in a very similar way by this reform, we do not expect this reform to affect the results for the SRA reform, apart from starting from a lower level of DI and a higher level of employment. A reform in 2008 abolished experience rating in disability insurance for large firms (individual employers' premiums for disability insurance increased with the number of workers that entered DI from a given employer). This reform is likely to have increased the inflow into DI (and reduced the outflow of DI), as suggested by the analysis of De Groot and Koning (2016). However, this reform again affects our treatment and control groups in our SRA analysis in almost the same way, and we expect only a level effect on the employment rate before the SRA.

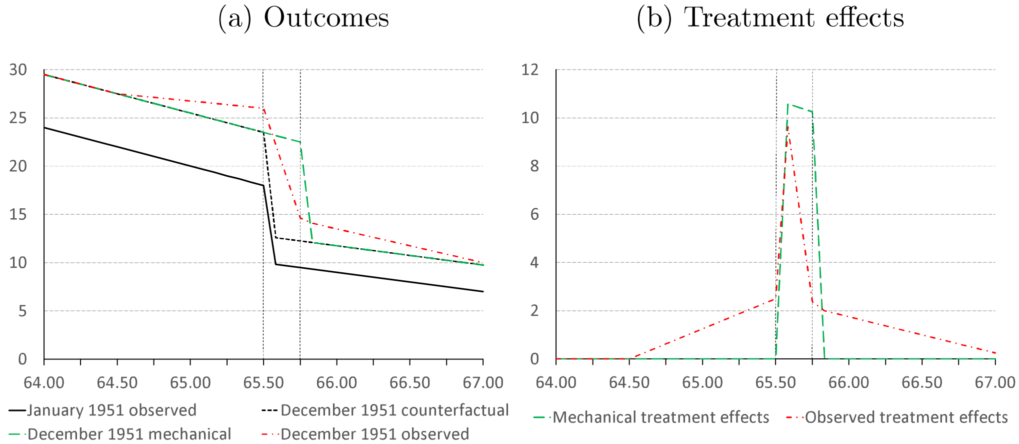
B. Mechanical and behavioral effects of the SRA reforms

When considering the effects of the reforms in the analysis below, we will compare the estimated treatment effects with the treatment effects predicted by a simple 'mechanical' model. Specifically, in the mechanical model, we predict individuals to simply remain longer in the state they were in before the old SRA and there are no treatment effects before the old SRA or after the new SRA. The actual treatment effects may differ from these mechanical effects due to behavioral responses. This is illustrated below.

Figure 2 illustrates the hypothetical predictions of the mechanical model for the employment rate – which typically plays a central role in the analysis of shifts in the (early) retirement age – and how it relates to a set of hypothetical estimated treatment estimates which may include upstream (or horizon) effects before the old SRA of the control cohorts, downstream effects after the new SRA of the treatment cohorts, and active substitution effects between the old and the new SRA. We consider the hypothetical outcomes for two cohorts born in 1951. The control cohort is born in January 1951 and has an SRA of 65 years and 6 months, and the treatment cohort is born in December 1951 and has an SRA of 65 years and 9 months. Panel (a) shows a series of hypothetical outcomes for the employment rate for each cohort. The solid black line is the observed employment rate for the control cohort born in January 1951, where we see a decline up to the SRA (due to e.g. deteriorating health conditions), then a steep drop off at the SRA, and a more gradual decline after the SRA. The dashed black line gives the counterfactual employment rate profile for the cohort born in December 1951 if they would have had the same SRA as the control cohort from January 1951, with a constant cohort effect before the old SRA and after the old SRA, with the cohort effect being smaller after the SRA than before the SRA (consistent with the descriptive statistics in Section II below).

Next, in panel (a) we also have two hypothetical employment rate profiles for the cohort born in December 1951 under the new SRA: i) mechanical (dashed green lines) and ii) observed (dashed red lines). The corresponding mechanical and

Figure 2. : Hypothetical effects of the reform



observed treatment effects are given in panel (b). In the mechanical profile, both cohorts move in parallel before the old SRA and after the new SRA – no upstream or downstream effects – and between the old and the new SRA individuals (on average) simply remain employed, apart from an increasing share of individuals exiting employment (on net) due to e.g. deteriorating health conditions.[8]

The profile we actually observe may deviate from this simple mechanical model, as illustrated in Figure 2. The hypothetical case shown here has positive upstream and downstream effects, where employment rises in the periods before the old SRA age and also remains higher for some periods after the new SRA age. These may result from a wealth effect following the reduction in pension wealth (Gustman and Steinmeijer, 1986, Hairault, Sopraseuth and Langot, 2010, Van Erp, Vermeer and van Vuuren, 2014) and/or more investment in human capital due to longer working lives (Jacobs, 2010). In between the old and the new SRA age, the employment rate may actually be lower than predicted by the mechanical model, because individuals actively move from employment to alternative pathways to retirement like UI or DI, resulting in lower treatment effects between the old and new SRA, as illustrated in panel (b) in Figure 2. In the empirical analysis below we will study the empirical relevance of upstream and downstream effects, and active substitution towards social insurance.

[8]To determine the mechanical treatment effect we need to construct the employment rate profiles rate between the old and the new SRA for the treatment cohort, both for the new SRA and under the counterfactual old SRA. For the employment profile under the new SRA, we can extrapolate the employment rate for the treated cohorts up to 3 months after the old SRA, using the observations before the old SRA. Mutatis mutandis, we can use the observations after the new SRA to extrapolate the employment rate under the counterfactual backwards up to the old SRA. In the special case that the cohort and age effects are the same before and after the SRA, the treatment effect between the old SRA and new SRA for each month is simply the drop in the employment rate of the control group at the old SRA.

II. Data and empirical strategy

A. Data and descriptive statistics

We use administrative data on the universe of the Dutch elderly population for the period 2007–2020.[9] We construct a monthly panel for the whole population between ages 57 and 67 years old, where we focus on cohorts born between January 1947 and December 1953. We have approximately 1.4 million individuals in our sample. 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) – which together make up the state of employment – 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 control and treatment groups), gender (male/female), migration background (with/without) and household type (single/couple). Furthermore, we use information on sector of employment (public/private) for the individual at age 60.

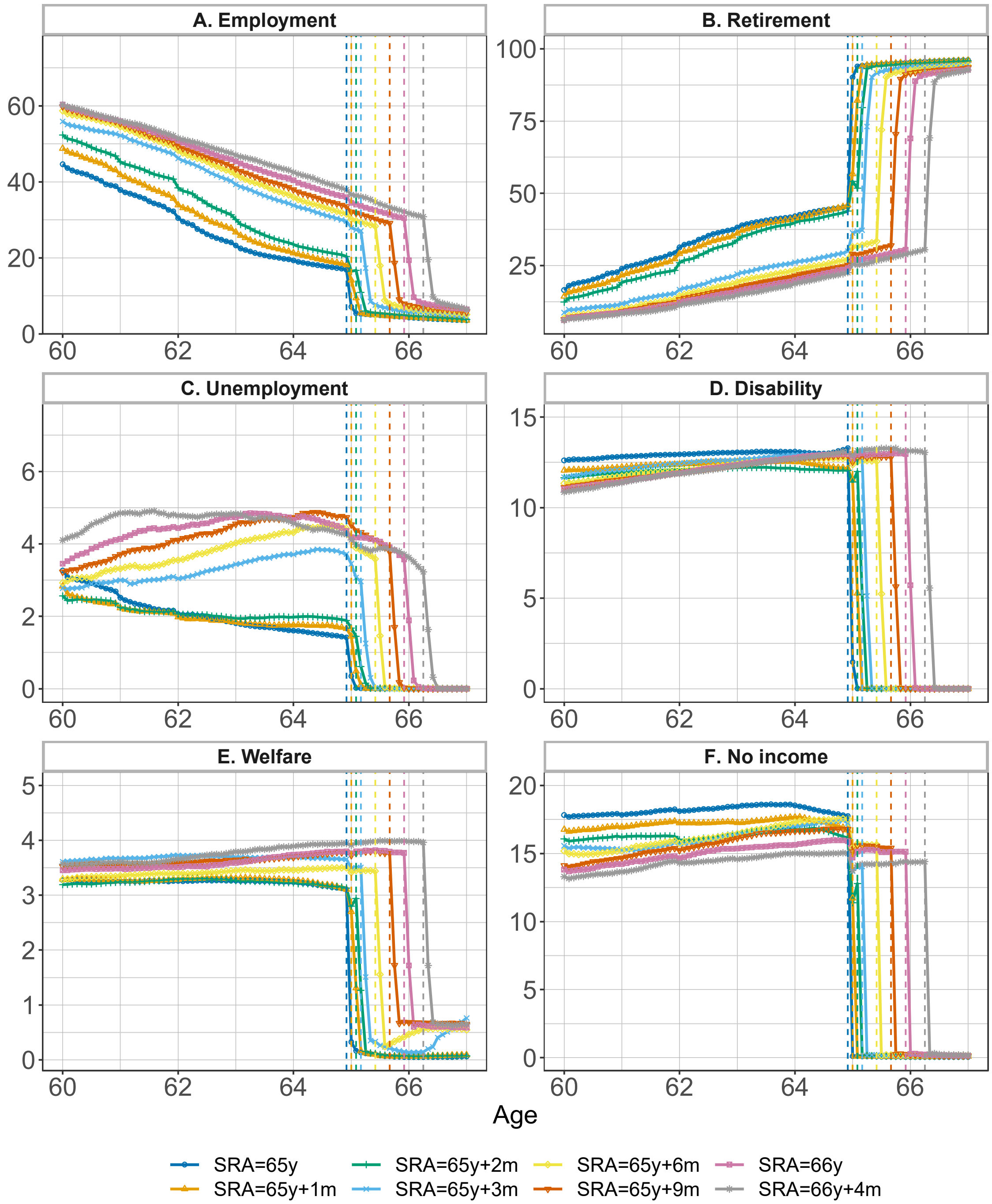
Figure 3 presents the share of the population in different labor market states at different ages, by SRA-cohorts impacted by the gradual increase from 65 to 66 and 4 months. We observe the following patterns. First, the share of employed individuals decreases steadily with age, until the SRA is reached and the share drops close to zero. Retirement follows a roughly similar but upward-sloping pattern. The other labor market states (generally) exhibit a flat profile (slightly increasing for unemployment), and drop to zero beyond the SRA, which is a mechanical effect of the first-pillar pension being automatically claimed and replacing all other existing benefits. Second, we observe a progressive increase of the employment rate over cohorts, with a large jump in the 'middle' of the cohorts. The former evolution can be attributed to the progressive increase in education and labor force participation, in particular 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 (see Lindeboom and Montizaan (2020) and appendix B). Lastly, we also observe a clear effect of the reform, as the patterns observed at the SRA (increase in retirement, drop in other outcomes) appears to shift to the right with the SRA of each cohort. This can be considered direct evidence of a causal effect of the SRA reforms on labor market outcomes.

B. Empirical strategy

To measure the causal effect of the reform on employment and other outcomes, we take advantage of the cohort-based implementation of the reform to implement

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

Figure 3. : Shares in different labor market states, by age and SRA cohorts



Notes: This figure presents the average share of the population in different labor market states, by age and SRA-cohort. Labor market status is defined as the main source of income between each possible categories.

a RD design, as in Geyer and Welteke (2021). We estimate models of the following form:

$$y_i = \alpha_j + \beta_j T_i + \gamma_j f(Z_i - c_j) + \delta_j f(Z_i - c_j) T_i + \eta X_i + \epsilon_i, \tag{1}$$

with y_i a given labor market outcome of individual i, and j a given discontinuous increase in the SRA generated by the reform. Z_i is the month of birth of the individual and c_j the cutoff point for increase j, the first cohort impacted by the reform. $f(Z_i - c_j)$ 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 increase 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 the value 1, indicating individuals in the treated cohorts. Lastly, X_i is a vector of individual level control variables, and the ϵ_i indicates the error term.

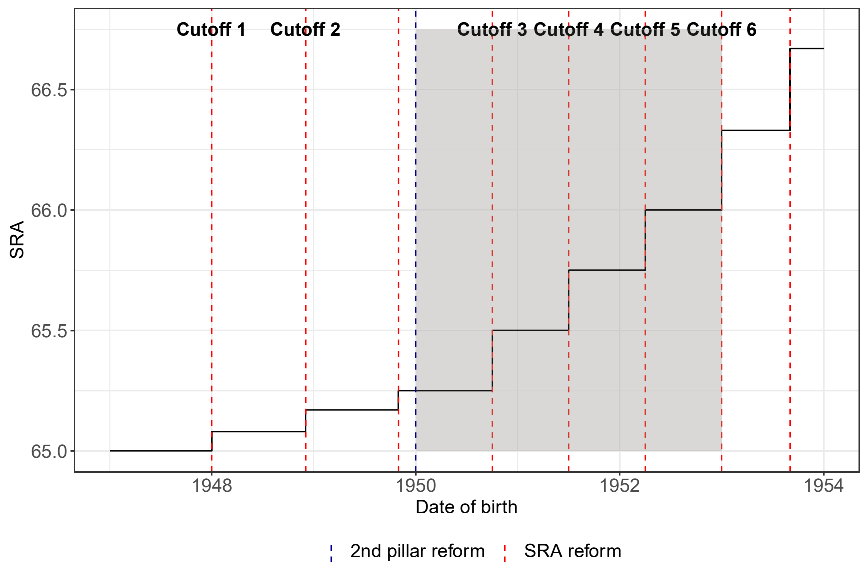
In the empirical analyses, we sometimes use time invariant variables as y (e.g retirement age), but in our main specification we estimate the effect of the reform on labor force status separately for each monthly age t, for a given cutoff j.

$$y_{ijt} = \alpha_{jt} + \beta_{jt} T_i + \gamma_{jt} f(Z_i - c_j) + \delta_{jt} f(Z_i - c_j) T_i + \eta X_{it} \tag{2}$$

With t expressed as the distance to the previous SRA, e.g equal to 0 when an individual is aged 65.5 when considering the SRA increase form 65.5 to 65.75. We expect the β_j to be positive for employment and negative for retirement at the ages impacted by the reform (e.g at 0, 1 and 2 for a three-month increase in the SRA). Other ages should not exhibit any discontinuity when there are no upstream or downstream effects. We estimate equation (2) using a second-degree polynomial for the $f()$ functions. For each cutoff c, we select all the observations with the corresponding old SRA (resp. new SRA) in the control (resp. treatment) group, and only them. We present alternative specifications for the bandwidth in the robustness checks in subsection III.A.

In the analysis we use different SRA increases j, as summarized in Figure 4, denoting the different jumps in the SRA used as sources of identification. Among the eight increases in SRA that we observe, we discard the last one, as we do not have enough data to study it. We also discard the third jump, for the following reason. An important identifying assumption for the RD is that the SRA must be the only discontinuous factor in 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 years and 2 months to 65 years and 3 months) occurs almost 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. We end up with six cutoffs/reforms, for which we estimate

Figure 4. : Sources of variation used in the empirical analyses



Notes: This figure presents the sources of variation used in the empirical analysis. Among the eight jumps in the SRA that we observe, we remove the last one (not enough observations after the cutoff) and the third one (simultaneous 2nd pillar reform). We end up with six different cutoffs for which we estimate equation 2. In our main specification, we regroup cutoffs 3 to 5 (highlighted by the shaded area) in a pooled RD estimation.

equation (2).

To simplify the exposition of the main results, we group the three consecutive three-months increases in the SRA (cutoffs 3, 4 and 5) in a pooled estimation, for which we stack the estimation samples for the different cutoffs. In this setting, some observations are used twice, as they appear on both sides of the discontinuity for different cutoff samples. As a robustness check, we use an alternative approach where individuals are randomly assigned to be used in the treatment or the control group, so that they appear only once in the pooled estimation sample. Table A.1 in the online appendix presents summary statistics for the different estimation samples used in the empirical analysis.

One identifying assumption of the RD 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. Figure A.1a in the online appendix presents the number of births for different birth years. We actually observe some spikes at round numbers for individuals born outside the Netherlands. These are the result of administrative decisions on the date of birth at registration when this information is missing. As those dates sometimes coincide with SRA changes, this may affect the estimates when e.g. migrants differ in their labor market outcomes from natives. For this reason, we remove migrants from our main samples. Figure A.1b shows the resulting number of births by date of birth for the pooled sample, which exhibits no systematic discontinuity at the cutoff. Moreover, we additionally study the effects of so-called 'donut' RD regressions in the robustness tests in subsection III.A, where we leave out observations just to the left and right of the cutoff points.

We also verify that the estimation samples are similar from both sides of the cutoff in terms of observable characteristics that should not be impacted by the SRA increases, e.g socio-demographic characteristics (gender, household type, migration background, and labor force status, all measured at age 58). The results of those balancing tests are presented in Figure A.2 in the online appendix, which shows the β coefficients from the estimation of equation (1) for different outcomes. As expected, for almost all variables and estimation samples we observe no discontinuity at the cutoff. We observe some significant differences in terms of household types for some cutoffs, but these are very small in terms of magnitude when compared to the sample mean.

III. Results

A. *Main results*

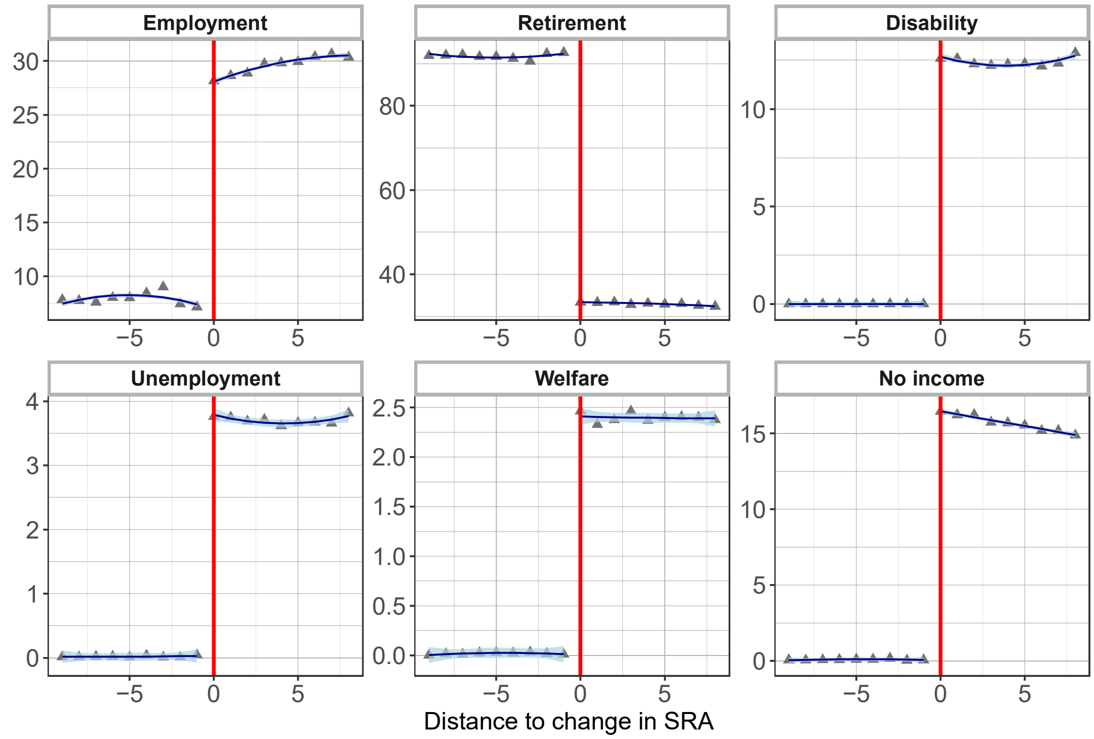
In this subsection and the next, we focus on the general effects of the SRA increases and consider the results obtained with our pooled estimation sample (for cutoffs 3 to 5). We consider the results for each specific SRA increase in subsection III.C (including cutoffs 1 and 2).

Base specification. — Graphical evidence on the effect of the reforms and the validity of our identification strategy is presented in Figure 5. This figure shows the average share of individuals in different states on the labor market one month after the old SRA.[10] Each panel displays a large change in these shares for the birth cohorts where the SRA jumps, and relatively smooth patterns on both sides of the cutoff, consistent with a direct effect of the reform on labor market outcomes. Indeed, we observe a large drop in the share of individuals who are retired, and substantial increases in the shares in the other labor market states.

Table 2 presents the corresponding estimation results. These confirm the graphical evidence on the effect of the SRA 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 who are retired of 59.5 percentage points. The employment rate increases by 21.2 percentage points (36% of the decrease in the share in retirement). The share of individuals on DI, UI, welfare benefits and other benefits (not shown in Figure 5) increases, by 12.7, 3.8, 2.4 and 3.1 percentage points, respectively. In total, the share of individuals on social insurance

[10]For example, at 65 years and 7 months for the increase in the SRA from 65 years and 6 months to 65 years and 9 months.

Figure 5. : Local linear regression plots one month after SRA of control cohorts



Notes: This figure presents local linear regression plots of the shares of individuals in each labor market state for different birth cohorts at the recentered age $t = 1$ after the SRA age of the control cohorts, for the pooled estimation sample. The SRA jumps up for the birth cohorts at 0.

Table 2—: Effect of the SRA reform one month after SRA of control cohorts

	Retirement	Employ-ment	Unemploy-ment	Disability	Welfare	Other	No income
Treatment effect	−59.5 (0.3)	21.2 (0.3)	3.8 (0.1)	12.7 (0.2)	2.4 (0.1)	3.1 (0.1)	16.4 (0.2)
No. obs.	807484	807484	807484	807484	807484	807484	807484

Notes: This table presents the estimated β_{jt} coefficient from equation (2), for different labor market states as outcome variable and for the recentered age $t = 1$ after the SRA age of the control cohorts, using the pooled estimation sample. We use a second-degree polynomial for the control functions and consider all the observations from both sides of the cutoff in the estimation.

benefits increases by 22.0 percentage points (37% of the decrease in the share in retirement). Hence, the reform generated large employment effects, but also large substitution effects towards other social insurance schemes. Finally, the share of individuals who have no personal income also increases, by 16.4 percentage points.

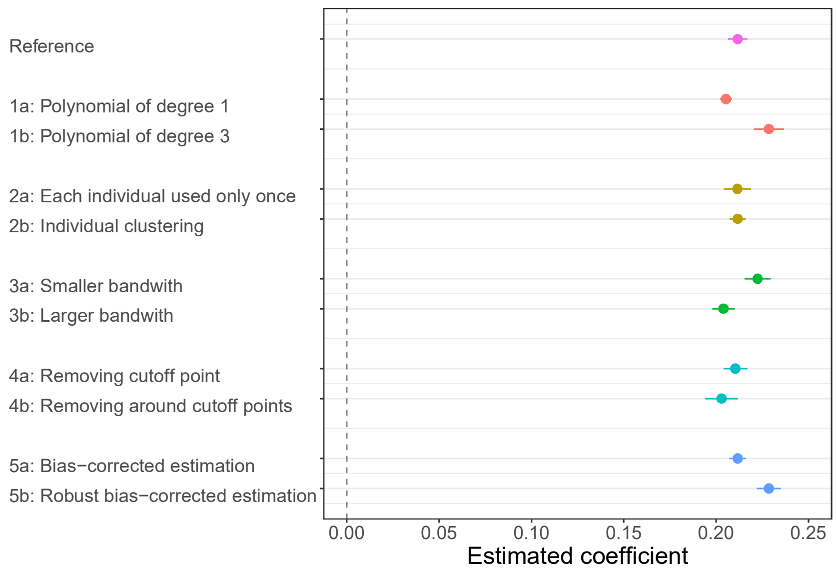
Robustness checks. — Figure 6 tests the sensitivity of our results to alternative specifications of equation (2), focusing on the employment effects.[11] The reference row is the estimated effect from our base specification. Rows '1a' and '1b' are the estimated effects using a first-degree or a third-degree polynomials for the control functions $f()$. In row '2a' we randomly assign individuals to the sample in the pooled RD, so that they never appear more than once in the estimation (so not once in the treatment group for one discontinuity and then again in the control group for the next discontinuity). In '2b' we cluster the standard errors at the individual level. In rows '3a' and '3b' we make the bandwidth used in the estimations smaller (6 months on each side) or larger (18 months on each side), respectively. In order to deal with potential biases related to mass points in the distribution of month of birth (see subsection II.B), rows '4a' and '4b' present the results of a 'donut-RD' estimation, removing observations around the threshold (at the cutoff in '4a', and at the cutoff and one month before in '4b'). Finally, rows '5a' and '5b' implement the bias-corrected and robust bias-corrected estimation proposed by Calonico, Cattaneo and Titiunik (2014), respectively, including the corresponding optimal choice for the estimation bandwidth. The estimation results are very stable across these different specifications.

Upstream and downstream effects. — Next, we consider potential upstream and downstream effects. Specifically, we consider RD estimates for each month from 36 months before the SRA of the control cohorts, to 12 months after. Figure 7 presents the resulting set of RD estimates per labor market state. This figure shows that the SRA reforms have a statistically significant and large impact between the old and new SRA.[12] Estimated coefficients are typically small and insignificant for the months before the (old) SRA of the control cohorts and for the months after the (new) SRA of the treatment cohorts. However, sometimes we observe some small upstream and downstream effects. These may in part result from our inability to perfectly control for business cycle and/or cohort effects. Indeed, there is some variation in the small up- and downstream effects depending on the number of polynomials we include to control for smooth time and cohort effects, see Figure A.5 in the online appendix. Yet, we should note that

[11]Table A.2 in the online appendix gives the regression results of the robustness checks for the employment rate in a table.

[12]Note that we find positive and significant effects for four months in a row, even though we consider a three-months increase in the SRA. This is due to the definition of work-state that we use – main source of income – and to the fact that different types of income are typically combined at the month of retirement. Pension benefits become the main source of income in either t = 0 or t = 1, depending on this 'rounding' effect.

Figure 6. : Employment effect of the reform: robustness

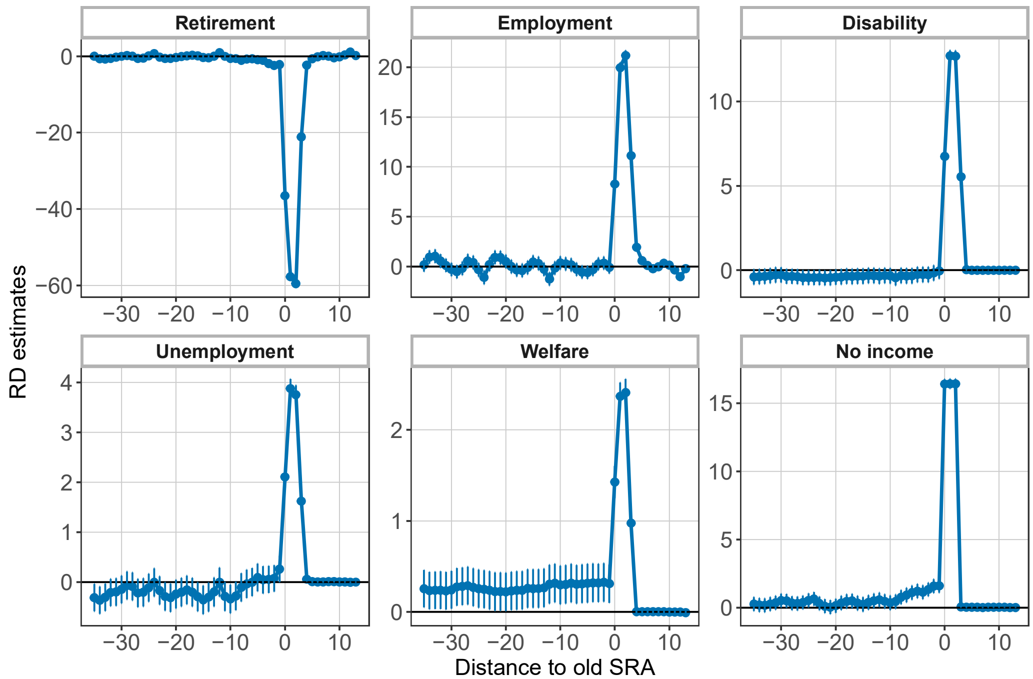


Notes: The reference points correspond to the specification used in Table 5. The next points correspond to the five series of robustness tests (see the text for details and online appendix table A.2 for the full table). Rows 1: Alternative specification for the degree of the polynomials in equation 2 (one in 1a, three in 1b). Rows 2: Alternative specification for pooled RD, using only once each individual (2a) and clustering at the individual level (2b). Rows 3: Alternative bandwidth used for the estimation compared to the reference (9 on each side), respectively to a smaller (3a, 6 on each side) and larger (3b, 17 and 12 months) window. Rows 4: Donut RD estimation, removing the observation at the cutoff (4a) and the -1 and 0 observations (4b). Rows 5: Bias-corrected (5a) and robust bias-corrected (5b) estimation proposed by Calonico, Cattaneo and Titiunik (2014).

we do observe a slight increase in the no-income category a few months before the SRA increase, which seems to be related to a similar decrease in the retirement category. This can be attributed to some degree of stickiness in the retirement behavior at the old SRA when the SRA increases, as has been documented for the US by Deshpande, Fadlon and Gray (2020). Figure A.6 in the online appendix presents separated estimation for the different SRA reforms of the estimation sample, and indeed shows that the small pre-SRA effects start at age 65 for each cutoff.

This finding of hardly any upstream effects is consistent with the results found in similar settings (see e.g. Staubli and Zweimüller, 2013). One explanation for the lack of upstream effects in our setting 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

Figure 7. : Estimated coefficient for all ages and work status



NOTES: This figure presents the estimated β coefficients of equation (2) for the pooled RD specification, estimated separately for each month, from 36 months before the SRA of the control cohorts to 12 months after.

in the long run, it is not obvious that upstream effects will arise. Mastrobuoni (2009) finds no upstream effects for a reform of the normal retirement age in the US announced almost 20 years in advance, Geyer and Welteke (2021) find no upstream effects for a reform of the early retirement age in Germany announced 10 years in advance. However, Carta and De Philippis (2021) do find significant upstream effects on labor supply for middle-aged women (and their partners) of a reform of the early retirement age in Italy.

SUBSTITUTION EFFECTS. — As noted above, the SRA reforms resulted in substantial substitution to social insurance schemes between the SRA of the control cohorts and the SRA of the treatment cohorts. These substitution effects are the sum of two effects: i) mechanical substitution, because people remain longer in their pre-SRA state or aging, and ii) active substitution, where individuals actively switch from e.g. employment to UI or DI now that the SRA has increased.

To study mechanical versus active substitution, we compare the estimated treatment effects to the predictions from a simple mechanical model, as outlined in subsection I.B. In the mechanical model, the shares of individuals in the treatment

Table 3—: Comparison substitution effects of the SRA reform

	Retirement	Employ-ment	Unemploy-ment	Disability	Welfare	Others	No income
Estimated treatment effect	−59.5 (0.3)	21.2 (0.3)	3.8 (0.1)	12.7 (0.2)	16.4 (0.2)	2.4 (0.1)	3.1 (0.1)
Predicted treatment effect mechanical model	−59.0	20.9	4.0	12.7	15.1	3.2	3.1

NOTES: This table first presents the estimated β_{jt} coefficient from equation 2, for different work states as outcome variable and for the recentered age t = 1 after the SRA age of the control cohorts, using the pooled estimation sample (same as in Table 2). Subsequently, we show the treatment effects that result from a linear extrapolation of the share of the different states from -14 months to -2 months before the SRA of the control cohorts, extrapolated to t = 1 after the SRA age of the control cohorts, minus a linear extrapolation of the share of the different states from +2 months to +14 months (up to a maximum of age 67), extrapolated (backwards) also to t = 1 after the SRA age of the control cohorts ('mechanical model').

cohorts in the different labor market states follow a linear extrapolation between the SRA of the control cohorts and their higher SRA from the ages before the SRA of the control cohorts.[13] This also allows for potential aging effects for the treatment cohorts beyond the SRA of the control cohorts (e.g due to deteriorating health conditions). To get at the treatment effect in the mechanical model, we then subtract the counterfactual[14] shares of individuals in the treatment cohorts in the different labor market states using a (backwards) linear extrapolation from the ages beyond the SRA of the treatment cohorts.[15] This also deals with aging effects beyond the SRA. The results are given in the last row in Table 3. We see that the mechanical model predicts the estimated treatment effects (very) well, suggesting that active substitution into other social insurance schemes between the SRA of the control cohorts and the SRA of the treatment cohorts is (very) limited.

FISCAL COSTS. — Mechanical or otherwise, substitution effects have important consequences for the effects of the SRA reforms on expenditures and receipts of the government. To determine the overall effect on the government budget, we

[13]We extrapolate the shares of the treatment cohorts beyond the SRA of the control cohorts using the shares observed for the treatment cohorts from 14 months before the SRA of the control cohorts to 2 months before the SRA of the control cohorts. The analysis of upstream effects suggests that these outcomes are (virtually) not affected by the SRA reform.

[14]The counterfactual that the SRA had not moved up for the treatment cohorts.

[15]We extrapolate the shares of the treatment cohorts beyond the SRA of the control cohorts using the shares observed for the treatment cohorts from 2 months after the SRA of treatment cohorts to 14 months after the SRA of treatment cohorts (up to age of 67, which implies less than 14 months for the latest cohorts). The analysis of downstream effects suggests that these outcomes are (virtually) not affected by the SRA reform.

first estimate equation (2) using data on monthly gross income from the different income sources for each individual (including the zeros). In this way we also account for individuals that have multiple sources of income in a given month, and changes therein.[16] Because second-pillar pension benefits are approximately actuarially fair (De Vos, Kapteyn and Kalwij, 2019), we focus on first-pillar pension benefits when we consider the effect of pension income on the government budget. Furthermore, as we find essentially no upstream or downstream effects[17], we again focus on the effects between the SRA of the control cohorts and the SRA of the treatment cohorts. Specifically, we consider again the effects at $t = 1$ month after the SRA of the control cohorts.

The results are given in Table 4. The government saves a total of 874 euro per person on average on gross first-pillar pension benefits. Furthermore, income from wages and profits rise, by 682 euro per person on average. Assuming a marginal tax rate of 45% on these additional wages and profits (Quist, 2015), income tax receipts rise by 307 euro per person on average. However, expenditures on gross UI benefits, DI benefits, social assistance and other types of social insurance rise by 99, 285, 24 and 24 euro per person on average, respectively. In total, on average 432 euro of gross benefits per person is lost on additional social insurance expenditures (49% of the savings on gross first-pillar pensions). Assuming a marginal tax rate of 45% on these changes in gross benefits as well, the government saves $(1-0.45)\cdot(874-432) = 243$ euro per person on net benefit payments. Hence, the net fiscal gain to the government is $307+243 = 550$ euro per month per person on average. For a cohort size of about 120 thousand individuals, this amounts to 66 million euro per month between the SRA of the control cohorts and the SRA of the treatment cohorts.

B. *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 retired or employed. They do not yield the effect of the reform on the effective retirement age, which may be a more comprehensive parameter for the evaluation of the effect of pension reforms. We remedy this limitation by deriving the effect of the reform on the average retirement age from our estimates.

Under some assumptions for the effect of the reform at older ages, we can use the age-specific estimates for employment to compute the effects of the reform on

[16]Descriptive statistics for the shares of individuals having income by income source (not just the share with the main income source) are given in Figure A.3 in the online appendix. The patterns are very similar to Figure 3. One notable exception is the small peaks we observe for welfare benefits just before the new SRA, which is likely to reflect the exhaustion of UI benefits for some individuals just before the first-pillar pension benefits start.

[17]Also not for each income source separately, see Figure A.4 in the online appendix. Again, the patterns are very similar to Figure 3. We do see some peaks and troughs for some benefits though, which are likely to reflect composition effects, as a select group of individuals in the control group persists in the respective labor market states after their SRA.

Table 4—: Effect on average monthly gross income by income source

	First-pillar pension	Wages and profits	Unemploy. insurance	Disability insurance	Welfare	Other social insurance
Estimated treatment effect	−873.7 (0.9)	681.6 (16.3)	99.4 (2.5)	285.0 (3.6)	24.3 (0.9)	23.6 (0.4)

NOTES: This table presents the estimated β_{jt} coefficient from equation (2), for each income source using individual gross incomes (including the zero's) for the recentered age $t = 1$ after the SRA age of the control cohorts. We use a second-degree polynomial for the control functions and consider all the observations from both sides of the cutoff in the estimation. We present outcomes for the pooled RDD model.

the average retirement age. The methodology is described in detail in appendix A. 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). With this approach, we find that the three-month increase in the SRA leads to a 0.61 months increase in the average retirement age, as shown in the panel (a) of Figure 8. This corresponds to a 0.21 elasticity of the average retirement age to an SRA increase.

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 direct RD estimation of the effect on the average retirement age. We estimate equation (1) with the individual retirement age – defined as the maximum age for which employment is the main source of income – as outcome variable y_i. Panel (b) of Figure 8 presents the RD plot for the average retirement age, using the same reforms and the same sample as in panel (a). We find a point estimate of 0.59 months (statistically significant at the 1% level), which is slightly lower but close to the one obtained using the RD estimates for the employment rates.[18]

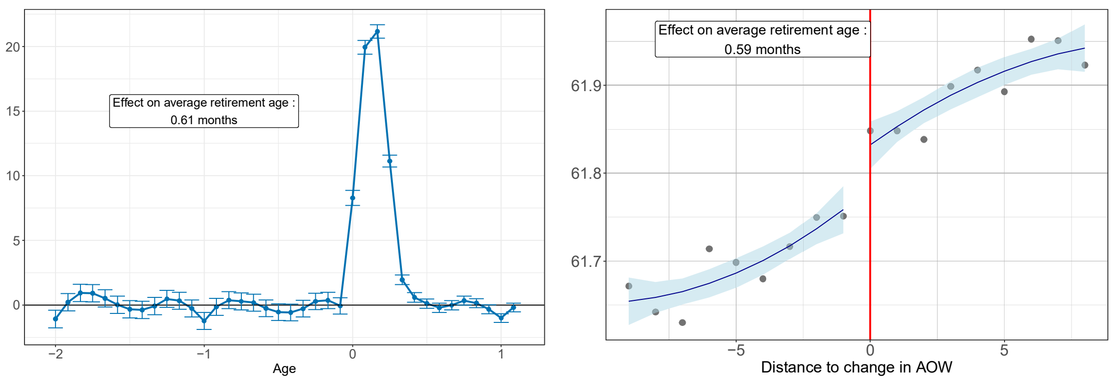
C. *Effect by SRA reform and interaction with ERA reform*

The effects presented above were for the pooled sample around cutoffs 3 to 5. Here we consider each SRA increase separately and compare their effects. Figure 9 presents the full RD plots for employment, for all the cutoffs we consider. We observe a similar pattern for the different cutoffs, with a large increase in employment rate at ages impacted by the SRA reform, and limited effects before or after. However, there are large differences in the magnitude of the effects. The effects are much stronger for the last four SRA increases than for the first two. The

[18] Note that the results for the retirement age are generally more noisy and sensitive to the specification, see online appendix Table A.3.

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

(a) RD on employment rates (b) RD on retirement age



NOTES: Panel (a) presents reproduces the employment panel of Figure 7. 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) presents a local linear plot with polynomials of degree 2. The label presents the β_j parameters of equation (1). Table A.3 in the online appendix gives the estimation results.

Table 5—: Effect on the average retirement age

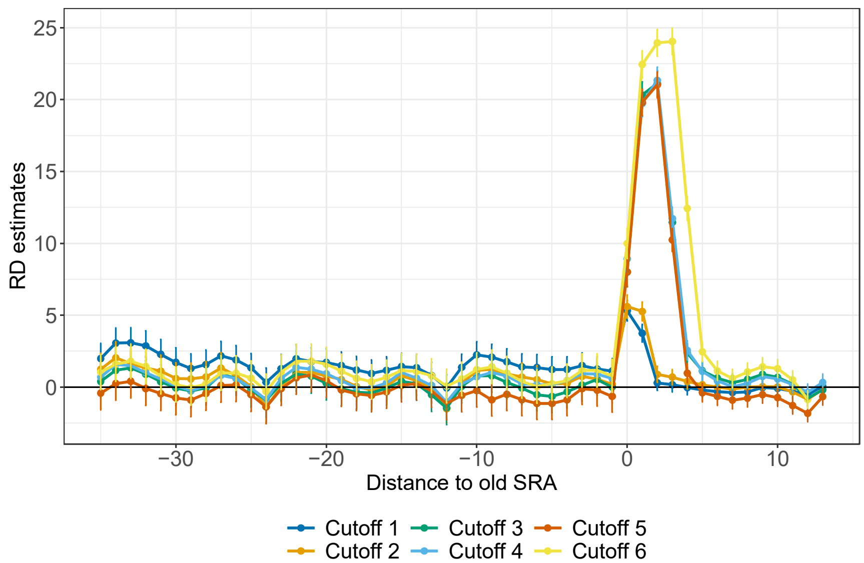
	Cutoff 1	Cutoff 2	Cutoff 3	Cutoff 4	Cutoff 5	Cutoff 6
Increase in retirement age	14.13	12.43	64.41	65.36	60.04	97.63
Increase in SRA	1.00	1.00	3.00	3.00	3.00	4.00
Elasticity	14.13	12.43	21.47	21.79	20.01	24.41

NOTES: The increase in retirement age are computed using the coefficients presented in Figure 9, following the methodology outlined in appendix A.

employment effect of the SRA increase – one month after the old SRA – is about four times bigger for the later increases (approximately 20pp vs. approximately 5pp). Interestingly, we observe that the effect of the 3 or 4 months increases is more important, not only because it impacts a larger part of the employment trajectory, but also because the effect for a given age is much bigger. As a result, not only the estimated effect of the SRA increase on the average retirement age is bigger, but also the corresponding elasticity. The effect of a one month SRA increase almost doubles from the first two reforms to the last four.

This difference is not primarily due the fact that the first increases were 'smaller' (1 month vs. 3 or 4 months), and can be explained by two reasons. First, we observe some stickiness to retirement at 65 for the first SRA increases (see also

Figure 9. : RD employment effect by cutoff

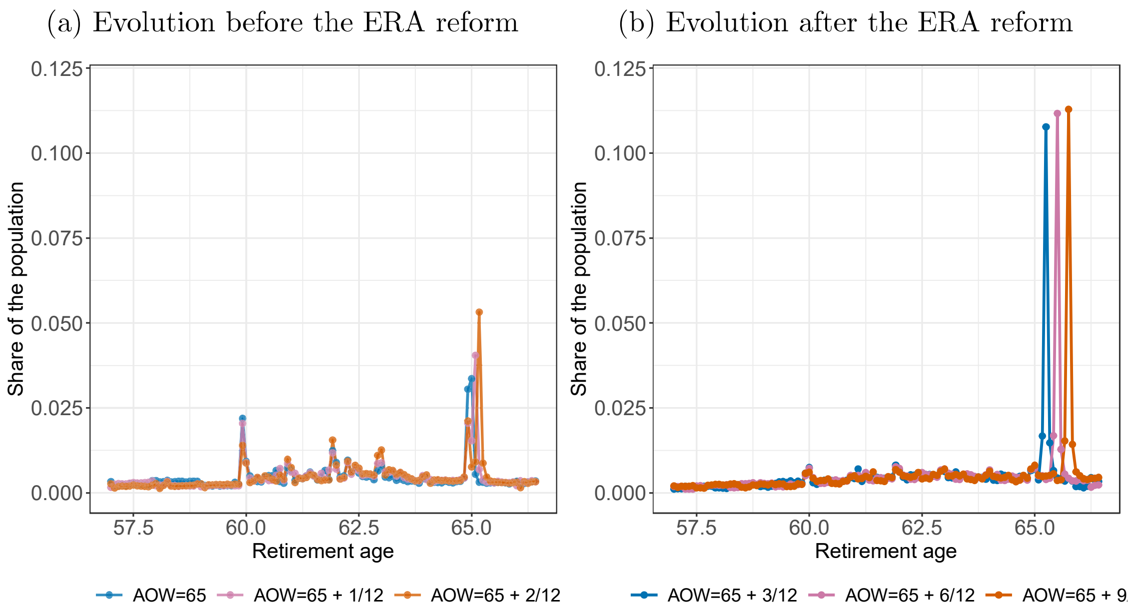


Note: This figure presents the estimated β coefficients of equation (2) for each increase in the SRA that we consider, for each age in months, from three years before the old SRA to one year after.

Figure 10a), which delays the effect of the reforms. Second, more importantly, the interaction of the effect of the SRA increases with the early retirement age (ERA) scheme reform. As explained in more detail in appendix B, the ERA reform occurred between the second and third SRA jump under study, and had a major impact on retirement and employment patterns between ages 60 and 65. As early retirement schemes were made much less generous, the distribution of retirement was shifted to a large extent from pre-SRA ages to the SRA (see also Figure B.3). The share of individuals retiring at the SRA almost doubles, from around 5% to 10%. This shift in the distribution of the retirement age strongly interacts with the SRA reforms. This is illustrated in Figure 10, which compares the effect of the SRA increases on the retirement distribution before and after the ERA reform. As much more individuals retire at the SRA after the ERA reform, shifting the SRA also has a much bigger effect on employment around the SRA and on the average retirement age.

This interaction between the two types of reforms, therefore, is the key reason behind the different elasticities measured for the SRA increase before (cutoffs 1 and 2) and after (cutoffs 3-6) the ERA reform. The result we establish here is more general: the share of individuals retiring at the old SRA before the reform is one of the main driving factors of the effect of a SRA increase. This is also a key dimension to understand the different estimates found in the literature on

Figure 10. : Effect of SRA reform on the distribution of retirement age



Note: This figure presents the distribution of monthly retirement age for different groups of SRA cohorts. Retirement is defined as the last observed age with positive labor income. Panel (a) (resp. panel (b)) presents the distributions for SRA-cohorts born before (resp. after) January 1950.

related reforms, as we show in the next section.

IV. Key determinants of the effect of retirement policies

The mechanical model seems to predict the estimated treatment effects in the Netherlands (very) well. In this section we show that the mechanical model is also helpful in understanding the different results found on (early) retirement reforms in other contexts. Furthermore, we consider the role of key factors that potentially play a role in the substantial bunching of retirement at the SRA in the Netherlands.

A. *Reconciling the findings in the literature*

For the Netherlands, there are no apparent upstream and downstream effects, and substitution towards other types of social insurance after the SRA of the control cohorts is nearly all mechanical. This is consistent with the typical findings of the quasi-experimental literature on ERA and SRA (and NRA) reforms in other contexts, see e.g. Mastrobuoni (2009), Staubli and Zweimüller (2013) and Geyer and Welteke (2021) on the limited role of upstream effects[19] and downstream effects, and e.g. Manoli and Weber (2018), Oguzoglu, Polidano and Vu (2020) and Geyer and Welteke (2021) on the limited role of active substitution.

Even so, there is still a wide range in the estimates of, e.g., the employment effects found in the related literature, see Table 6.[20] Indeed, the effect on the employment rate ranges from 6.3 percentage points in Cribb, Emmerson and Tetlow (2016) to 20.9 percentage points in Rabaté and Rochut (2019). However, the key elements behind these effects are qualitatively the same and follow from the mechanical model as outlined in subsection I.B. We do not have the data from the other studies, but if we ignore a potential different cohort and age effect before and after the SRA, the mechanical model predicts that the employment effect between the SRA of the control cohorts and the SRA of the treatment cohorts should be very close to the drop in the employment rate at the SRA of the control cohorts, or the 'bunching' of retirement at the SRA as we denote it in Table 6. Indeed, from Table 6 we see that the estimated treatment effects on the employment rate are closely related to the bunching at the relevant ERA, NRA or SRA of the control cohorts. This is a rather intuitive statement – as individuals retiring before as well as after the retirement age are apparently not impacted by the reform – but it is key to understanding the different results found in the literature. The different findings in the literature then mainly reflect differences in bunching at the retirement age.

Related to this, Table 6 is also informative about the two key elements that make up the bunching at the (early) retirement age: i) the share of individuals still employed just before the retirement age, and ii) the hazard rate into retirement at the retirement age for those individuals. There are substantial differences across contexts and groups. This is shown for example by the results for men and women in Staubli and 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.

For the Netherlands, the employment rate before the retirement age that we consider is relatively low, which is likely to be due to the fact that we consider a reform that targets individuals at a relatively old age. However, the hazard rate out of employment is relatively high at the retirement age that we consider,

[19]The recent analysis of Carta and De Philippis (2021) being a notable exception, they do find significant upstream effects on labor supply for middle-aged women (and their partners) of a reform of the early retirement age in Italy.

[20]Table A.4 gives a brief overview of related studies on the effect on the average retirement age. The elasticity of the average retirement age with respect to the shift in the retirement age is relatively low compared to other studies. This is due to the relatively low employment rate at the SRA.

Table 6—: Results related studies on effects near ERA, NRA or SRA[a]

Study	**Country**	**Reform**	**Method**	**Results**		**At ERA, NRA or SRA**		
				Employment rate pp	Retirement rate pp	Employment 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. (2019)	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 (2021)	GER	♀: ERA 60 → 63	RDD	+13.5	–27.6	62	0.19	12
This paper	NLD	SRA 65+3m → 66	RDD	+21.2	–59.5	29	0.72	21

NOTES: [a]Exact references for the values reported in this table can be found in Table A.5 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.

especially for the later cohorts. Below, we consider potential channels at work in the relatively high hazard rate out of employment at the SRA in the Netherlands.

B. Potential channels behind retirement at the SRA

We consider the potential channels behind the bunching at the SRA in the Netherlands by comparing the hazard rates of subgroups of the elderly that are impacted to a different degree by these channels.

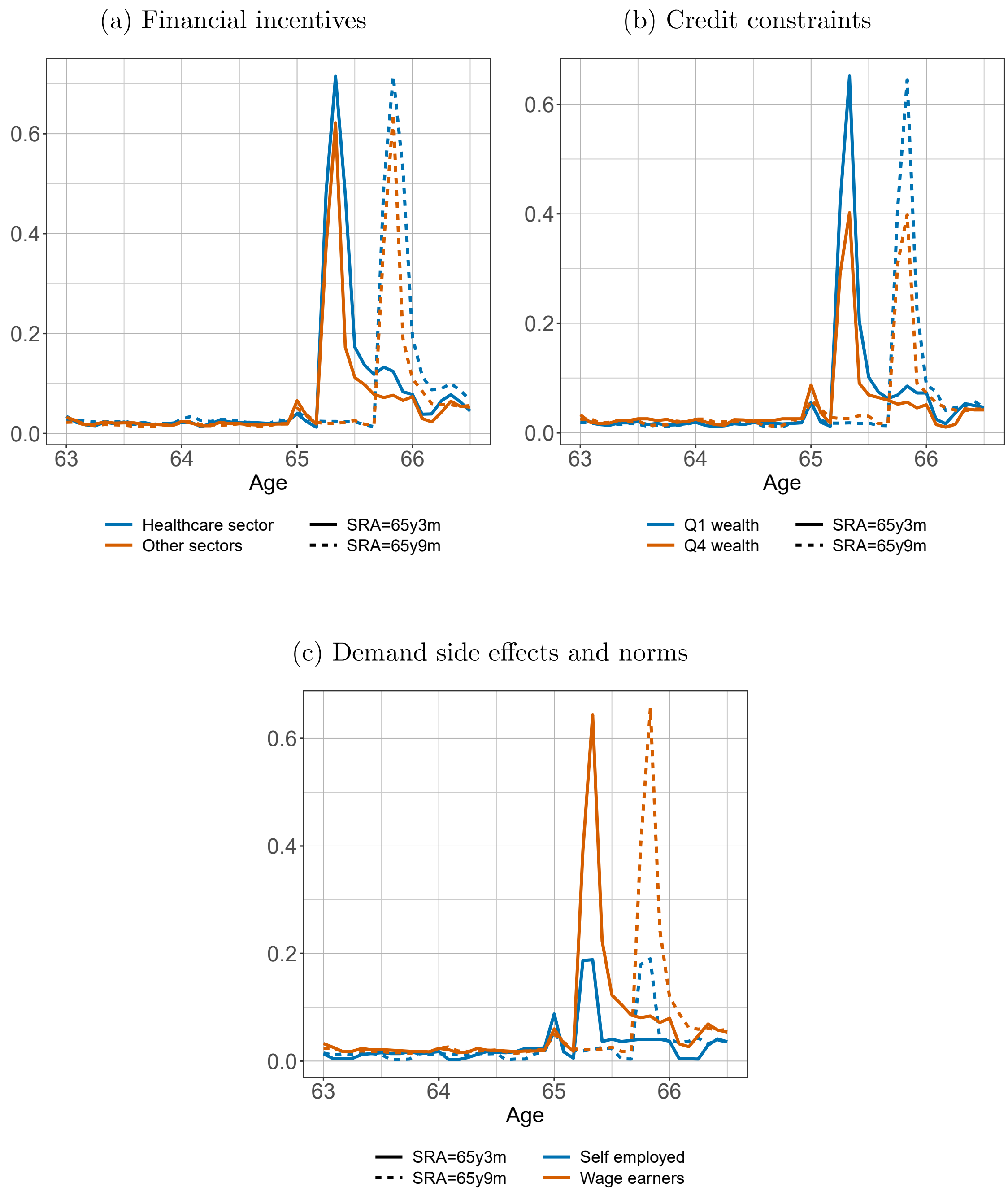
Kinks in the budget set. — Second-pillar pensions can represent a large share of the total pension. Hence, kinks in the second-pillar pension could potentially be an important driver of the bunching we observe (as in e.g. Brown, 2013). To test whether this channel is important, we focus on the health care sector, for which we know that there is no financial incentive to retire exactly at the SRA from Kantarcı and 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 11a compares the hazard rate (into retirement) by age for individuals working in the health care sector (measured at age 60), to the hazard rate of individuals 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 kinks in the second-pillar pension are not the main driver of bunching at the SRA.

Credit constraints. — Since 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 observe total household wealth in our data.[21] Figure 11b then compares the hazard rate for individuals in the lowest and the highest wealth quartiles (again measured at age 60). We do observe a somewhat larger hazard rate at the SRA for individuals with relatively low wealth. However, we also observe a large hazard rate for the individuals with relatively high wealth, suggesting that credit constraints are only part of the explanation.

Demand side factors. — Demand side factors are likely to play a key role in the large bunching at the SRA in the Netherlands. 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 the Netherlands. Employment protection in the Netherlands was and is one of the highest in the OECD (OECD, 2020). Related to this, wage profiles in the

[21] Contrary to Cribb, Emmerson and Tetlow (2016), who use (relatively illiquid) home ownership as a proxy for wealth and the role of credit constraints.

Figure 11. : Determinants of bunching at the SRA



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

Netherlands are relatively steep, as the ratio of wages of older workers to the wages of younger workers is particularly high in the Netherlands (OECD, 2014). At the SRA, employment protection ends, and hence there is an important discontinuity there. Furthermore, more than 90 percent of open-ended labor contracts have mandatory retirement at the SRA (92% in 2014, according to the OECD, 2014). When workers want to continue working beyond the SRA, their employer has to draft a new contract under new conditions, with transaction costs being another barrier to continued employment.

We explore the importance of demand side factors by comparing the hazard rate of wage earners to the hazard rate of self-employed (defined by their income status at age 60) in Figure 11c. Since employment protection does not cover the self-employed, we expect smaller bunching for the self-employed. We indeed observe that the hazard rate is three times bigger for wage earners than for self-employed, suggesting that the combination of strict employment protection and mandatory retirement may be a driver of the bunching at the SRA in the Netherlands.

Figure A.7 in the online appendix provides additional results suggestive of an important role for the demand side in shaping the hazard rate at the SRA. Figure A.7a shows that hazard rates at the SRA are higher for workers in sectors where the pre-retirement wage profiles are steeper, and hence there is a stronger incentive for employers to send workers into retirement.[22] Figure A.7b further shows that the hazard rate increased relatively more in sectors that were more severely hit during the Great Recession in the late 2000s/early 2010s, consistent with employers being more strict in applying mandatory retirement – or more reluctant in drafting a new contract after the SRA – when they need to downsize their workforce.

Norms. — The final channel that we consider are norms and framing effects, which also potentially play an important role in the bunching of retirement at the SRA (Behaghel and Blau, 2012, Lalive, Magesan and Staubli, 2020, Seibold, 2021). In our setting, the residual bunching we observe for the self-employed suggests that norm effects are indeed 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 demand side effect, as it can also be due to group-specific norms or framing effects. The demand side effects discussed above can indeed be a mix of employer driven retirement effects and workplace norms effects.

[22]Note that this is also consistent with the model of Lazear (1979) that can explain why we need mandatory retirement. There is an ongoing debate in the Netherlands on the pros and cons of abolishing mandatory retirement.

V. Conclusion

In this paper we study the effects of the increase in the Dutch statutory retirement age on employment and the use of social insurance of older workers. We use an RD design and rich administrative data on the universe of the Dutch population. We find that the reform decreases the share of individuals in retirement between the old and the new retirement age by 60 percentage points. Close to one third (21 percentage points) of these individuals are employed, whereas also close to one third (22 percentage points) are in social insurance (disability insurance in particular). We find virtually no evidence of upstream effects before the old SRA, or downstream effects after the new SRA. Furthermore, we also find hardly any active substitution into social insurance between the old and the new SRA. Indeed, a mechanical model that simply extrapolates the shares of individuals in the different labor market states to the ages between the old and the new SRA predicts the estimated treatment effects well. We further show that the reform led to substantial savings for the Dutch government, also after accounting for (mostly mechanical) substitution into social insurance.

The bunching into retirement appears to have shifted almost one-for-one with the SRA. We show that the two key elements that determine the bunching at the SRA are: i) the pre-SRA employment rate, and ii) the retirement hazard rate at the SRA. Decomposing the bunching into these two elements helps to reconcile the wide range of findings in the quasi-experimental literature on (early) retirement reforms. The differences in the estimated treatment effects are driven by the amount of bunching in retirement behavior. The relatively strong effect we find in the Dutch case – for the cohorts born after 1949 – results mostly from a large hazard rate at the SRA (the pre-SRA employment rate is actually relatively low). Considering the potential role of different channels that may cause this high hazard rate, our results suggest that demand side factors related to strict employment protection and mandatory retirement in combination with norms and framing effects play a key role in the hazard into retirement.

Several policy implications can be derived from these results. So far, the increases in the SRA have been beneficial in terms of the sustainability of public finances. We also see that the effectiveness of raising the SRA increased after early retirement became less generous. Hence, there appear to be important interaction effects of early retirement reforms and statutory retirement reforms. More generally, the effectiveness of retirement policies largely depends on their interaction with other determinants of the employment of older workers, including e.g. the entry conditions and generosity of unemployment insurance and disability insurance. However, we should note that these results may only hold true up until a certain age. Even though the 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.

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 – kinks in the budget constraint, liquidity constraints, demand side factors and norms – may not be constant over time. A better understanding of the relative importance of these channels remains an interesting direction for future research.

Appendix

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 subsection II.B:

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

The RD coefficients we are interested in are the β_{ca} coefficients. They give, for 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, the 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 to be employed at age a, i.e to retire later than age a, is β_{ca} bigger. Formally, if we denote by X_R the random variable of the observed retirement age for the control cohort (on the left-hand side of the cutoff j) and denote by X_R^{cf} the counterfactual one absent the reform, we have:

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

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, using monthly age in the sum:[23]

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

The third step of the computation is obtained from the following property of the

[23]The following derivation is inspired by Mastrobuoni (2009) (equation (4) on page 1229).

CDF: $P[X = x] = P[X > a-1] - P[X > a]$. The last steps directly follow 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 then compute the effect on the reform on the average retirement age as the sum of the β coefficients estimated for a given cutoff.

B. The ERA reform

The focus of the main text is on the effects of the SRA reforms. We find that the effects of the SRA reforms are much larger for cohorts that were directly affected by the ERA reform of 2006 than for cohorts that were (largely[24]) unaffected by the ERA reform of 2006. In this appendix we briefly consider the main elements of the ERA reform of 2006, and show that this reform had a large effect on retirement and employment after the age of 60 (for earlier analyses of this reform see e.g. De Grip, Lindeboom and Montizaan, 2012, Lindeboom and Montizaan, 2020).

Description of the 2006 ERA reforms. — The ERA reform package was announced in 2005 and came into effect on January 1, 2006. The reform package resulted in lower early retirement benefits and early retirement benefits that were more actuarially fair for cohorts born after December 1949. Early retirement benefits for cohorts before January 1950 were unaffected.[25] In the same reform, the government also introduced the *Levensloopregeling* (Life Course Savings scheme), which allows for tax-free saving up to 12% of annual earnings, which can be used to retire early (or to take leave for raising children or a sabbatical). Individuals could use this scheme to partly offset the reduction in early retirement benefits. However, all cohorts could participate in this scheme, though cohorts born in

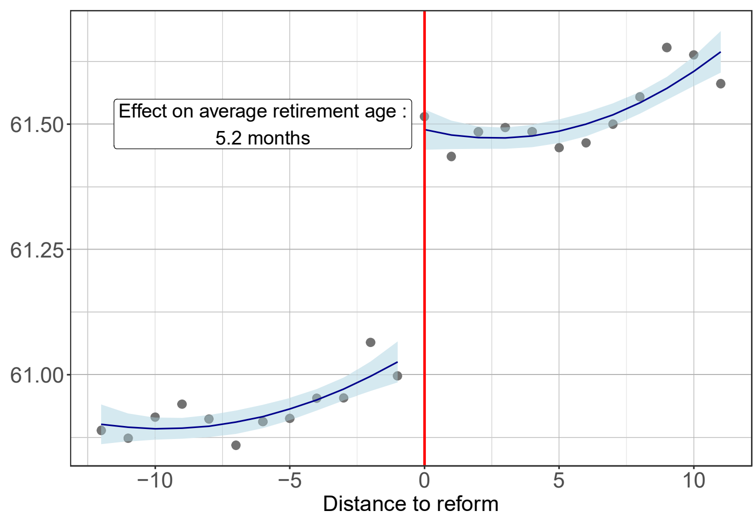
[24]They were not directly affected by the ERA reform, but they were to some extent affected by the introduction of the Life Course Savings scheme, as discussed below (also see Lindeboom and Montizaan, 2020).

[25]Lindeboom and Montizaan (2020) provide examples of public pension wealth for cohorts born in 1949 and 1950 at different potential retirement ages.

1950–1954 were allowed to save more than 12% into this scheme (up to a maximum of 210% of annual earnings for all cohorts). Overall, financial incentives to postpone retirement were substantially increased for cohorts born after December 1949, though tax-favored savings scheme may have promoted some earlier retirement for cohorts born before January 1950 (see also Lindeboom and Montizaan, 2020).

Effects of the ERA reforms. — The ERA reforms had an important impact on employment and retirement behaviors. Figure B.1 first presents the effect of the reform on the average retirement age. We see a large jump at the 1950 discontinuity, with an estimated increase of 5.2 months in the average retirement age.[26] This is a much larger effect than the SRA jumps we study (maximum of 1 month increase in the average retirement age for the 4 months increase in the SRA).

Figure B.1. : Effect of ERA increase on the average retirement age



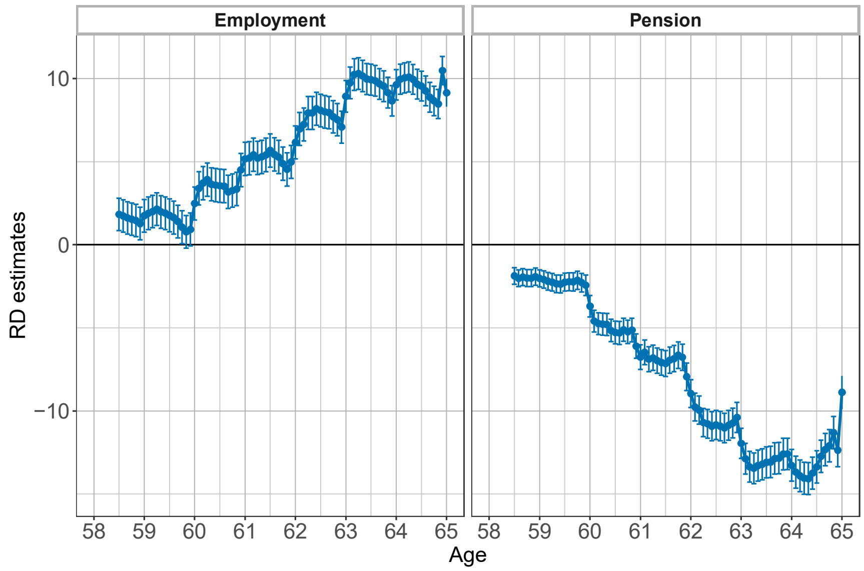
Note: This figure presents a local linear plot with polynomials of degree 2 of the average retirement age by date of birth (centered around the 1950 cutoff). The label presents the β_j parameters of equation (1), estimated with the individual retirement age as outcome variable and January 1950 as cutoff.

Figure B.2 shows the results from RDD analyses at different age levels for cohorts born in 1949 (control cohorts) and 1950 (treated cohorts).[27] Panel (a)

[26]We estimate equation (1) with January 1950 as a cutoff and the retirement age as left-handside variable.

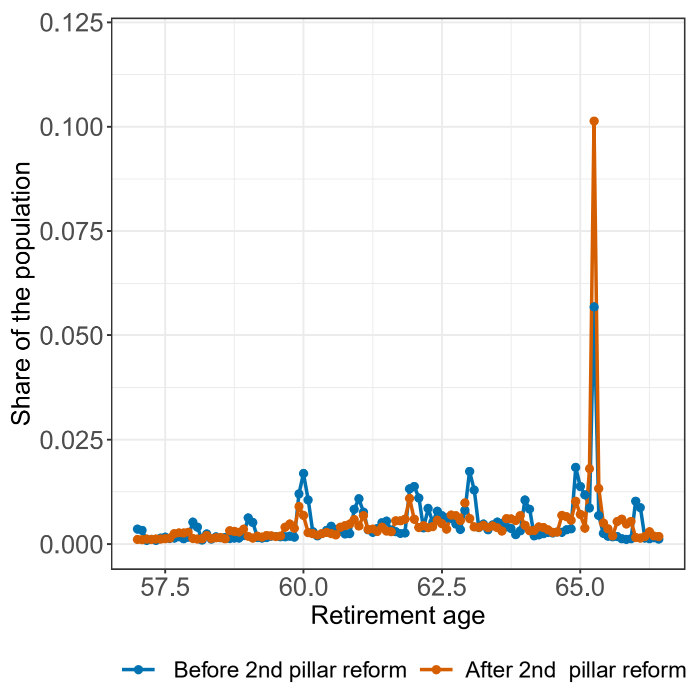
[27]We estimate equation (2) with January 1950 as a cutoff and labor force status as the dependent variable.

Figure B.2. : Effect of ERA increase on employment and retirement rate, by age



Note: This figure presents the estimated β coefficients of equation (2) for the ERA reform cutoff, estimated separately for each age (in month) between 58 and 65.

Figure B.3. : Effect of ERA increase on the distribution of retirement age



Notes: This figure presents the distribution of monthly retirement age for individuals born between September and December 1949 (before reform, in blue) and individuals born between January and March 1950 (after reform, in red). Retirement is defined as the last observed age with positive labor income.

shows that cohorts born after 1949 are much more likely to work between the age of 60 and the SRA, and the share in employment increases by about 10pp at the age of 64. Conversely, Panel (a) shows that cohorts born after 1949 are much less likely to retire between the age of 60 and the SRA than cohorts born before 1950, and the difference increases between the age of 60 and 64, with the share in retirement dropping by some 12pp at the age of 64. This is roughly consistent with the findings of Lindeboom and Montizaan (2020) for public sector workers.

The reform thus largely increased employment and decreased retirement before age 65. More precisely, it induced many workers who previously retired before the SRA to retire exactly at the SRA. This is illustrated in Figure B.3, which presents the distribution of retirement age for the control and treatment group. We observe a large decrease in all the retirement mass points before 65, while the bunching in retirement at the SRA almost doubles.

*

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