Public Pensions and Private Savings*

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Abstract

How does the provision of public pension benefits impact private savings? We answer this question in the context of a Danish reform that increased social security eligibility ages. Using administrative data and a regression discontinuity design, we identify the causal effects of the policy on savings throughout the financial portfolio. We find increases in contributions to personal and employer-sponsored retirement accounts when delayed benefit eligibility induces extended employment. We argue that inertia—the continuation of previous savings behaviors—is a key mechanism, and we highlight how firm default contribution rate policies can mediate savings responses to social security reform.

Keywords: social security, private savings, pension reform
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1 Introduction

A long-standing question in public finance asks how publicly-provided pension benefits impact private savings. Understanding the relationship between these two forms of retirement wealth is important for the optimal design of social security systems, which are some of the largest social insurance programs in the world. Classical work emphasizes that pension benefits should crowd out savings. Yet the effect of social security on savings is actually theoretically ambiguous after accounting for the effect of benefits on retirement decisions, since social security may induce earlier retirement and increase the time horizon over which assets are needed to finance consumption (Feldstein 1974). A principal task for empirical research is hence to investigate how public pension benefit schemes impact savings in practice, accounting for potential labor supply responses.

Establishing convincing causal evidence on this question is difficult, due largely to two significant challenges. First, data availability is a major obstacle. A thorough analysis requires data that contain information on employment, earnings, and benefit receipt, as well as information on private savings, assets, and liabilities. In most countries, these demands necessitate the use of survey data, which can suffer from small sample sizes and a lack of reliable and detailed information on wealth. Second, identification requires a compelling source of exogenous variation in pension benefit payout structures.

In this paper, we overcome these challenges using administrative register data from Denmark and a regression discontinuity (RD) design. The context of our study is a major reform to the Danish retirement system announced in 2011 and implemented in 2014 that created a six-month discontinuous increase in pension eligibility ages for those born on or after January 1, 1954. Those born just after this cutoff date are similar in all aspects to those born just earlier, yet differ sharply in the ages at which they become eligible for public pension benefits. We exploit this policy change to identify causal effects by estimating discontinuities in outcome variables by birthdate, and we exploit the breadth of our data to study the effect of the reform on labor supply and on separate measures of savings throughout the entire financial portfolio.

Leveraging the timing of the policy, we distinguish between anticipatory responses (after the reform is announced but before it is implemented) and responses after implementation (when individuals navigate retirement years facing different eligibility ages). In Denmark, there are three critical pension eligibility ages. The Early Retirement Age (ERA) stipulates the age at which individuals first become eligible for early retirement benefits, two years later is an incentivized retirement age, and the Full Retirement Age (FRA) denotes the age at which individuals can transition to standard old-age benefits. These ages used to be 60, 62, and 65, respectively. The policy reform that we study initiated step-wise increases in each of these eligibility ages by birth cohort. We focus on the first phase of the reform, which creates the cleanest quasi-experiment. Those born on or just after January 1, 1954 learn in 2011, at age 57, that their pension eligibility ages are increasing to $60\frac{1}{2}$, $62\frac{1}{2}$, and $65\frac{1}{2}$ and constitute the treatment group. Those born just earlier experience no such change and constitute the control group. Our RD estimates over the years 2011 to 2013 capture the causal effects of *future* differences in pension eligibility. Our RD estimates over the years 2014 to 2018 capture the causal effects of *current* differences in pension eligibility following the anticipation period, as it is during these years that our sample navigates through the early retirement program. Note the data are not yet available to study behaviors around the FRA, as the birth cohorts we study are age 65 in 2019, and our data extend through 2018.

We begin with an analysis of how retirement behavior changes in response to the reform. In the Danish setting, pension accrual incentives and high implicit taxes on work create strong incentives to retire either right at the ERA or right at the incentivized retirement age two years later. We show large corresponding spikes in retirement right at ages 60 and 62 for the control group. We then show how the reform causes the spikes in retirement to shift to the new eligibility ages in lockstep. The distribution of retirement ages for the treatment group contains large spikes in retirement right at $60\frac{1}{2}$ and $62\frac{1}{2}$, consistent with delayed retirement due to the reform-induced incentives.

We then turn to our RD design to quantify the effects of the reform on savings. We study as our main outcomes contributions to employer-sponsored retirement plans (analogous to 401(k)s) and contributions to personal retirement plans (analogous to IRAs), as well as other savings measures capturing bank accounts, stock market accounts, liabilities, and property wealth. Our first set of RD results corresponds to the three-year anticipation period, as our analysis sample approaches age 60. We do not find any statistically significant savings responses in any savings vehicle in anticipation of reaching pension eligibility ages. However, our standard errors prevent us from ruling out a priori plausible effect sizes.

Our second set of RD results corresponds to the early retirement period, as our analysis sample ages from 60 to 64 and differences in benefit eligibility manifest themselves. During the first critical year of 2014, when the analysis sample is age 60 and the treatment group works longer in order to retire at the new ERA of $60\frac{1}{2}$, we document an increase in aggregate average earnings of \$6,325 (a 14% increase when compared to the mean). We find concurrent and large increases in contributions to employer-sponsored retirement accounts that accompany this increase in earnings. Contributions to employer accounts rise by \$800 (16%). We also find significant impacts on personal retirement accounts. Contributions to these plans increase by \$84 (21%). During the second critical year of 2016, when treated individuals work longer to retire at the new incentivized age of $62\frac{1}{2}$, we find similar responses. In this year, we find an increase in earnings and an increase in contributions to employer retirement plans, with some evidence of an increase in savings in personal retirement plans.

In contrast, during the non-critical years of 2015, 2017, and 2018, when the strong incentives for delayed retirement are not present, we find mostly muted responses in earnings and retirement savings. Moreover, we consistently find little to no evidence of savings responses through any other financial vehicle in any year. That is, overall our results indicate the clearest savings responses precisely when the treatment group is induced to delay retirement to comply with the new pension eligibility ages and concentrated within traditional retirement accounts, which are earmarked for consumption in retirement.

What can explain our findings? To investigate mechanisms, we conduct a series of additional analyses and argue that the overall body of evidence points to inertia, by which we mean the continuation of previous savings behaviors. Most notable is our ability to unpack the increases in retirement savings during the years of extended employment. Consistent with the continuation of previous savings behaviors, we show that the increases in contributions to personal retirement plans are entirely driven by those who were making contributions in the past and continue to do so. Furthermore, we leverage our linked employee-employer data to show that the increases in employer-sponsored retirement plans are largely driven by continued contributions at employer default contribution rates during the policy-induced periods of extended employment. Employer contribution policies have been shown to be key drivers of savings in employer-sponsored retirement accounts (Madrian and Shea 2001, Choi et al. 2002, Beshears et al. 2009, Choi 2015), especially in Denmark (Chetty et al. 2014, Fadlon, Laird and Nielsen 2016) where unions, employer associations, and firms have a major influence in setting quasi-mandatory default contribution rates. We show how these types of policies can dictate responses to a national reform.

Our paper relates to two literatures. Most directly, we relate to the important literature that studies how private savings respond to the provision of public pension benefits. Traditionally, papers aim to provide explicit estimates of the elasticity between public pension wealth and private savings. Earlier papers laid theoretical groundwork and provided empirical evidence mostly correlational in nature (e.g., Feldstein 1974, Feldstein and Pellechio 1979, Kotlikoff 1979, King and Dicks-Mireaux 1982, Diamond and Hausman 1984, Hubbard 1986, Pozo and Woodbury 1986, and Bernheim 1987). More recent papers have used difference-in-differences style estimators applied to survey datasets to study reforms and have produced a wide range of elasticity estimates from several different countries (e.g., Attanasio and Brugiavini 2003, Attanasio and Rohwedder 2003, Bottazzi, Jappelli and Padula 2006, Aguila 2011, Feng, He and Sato 2011, Lachowska and Myck 2018, and Slavov et al. 2019). Furthermore, others have provided cross-country analyses on the question (Kapteyn and Panis 2005, Disney 2006, Hurd, Michaud and Rohwedder 2012, and Alessie, Angelini and van Santen 2013). Finally, using an RD design, Lindeboom and Montizaan (2020) study how retirement expectations, retirement realizations, and savings decisions respond to a composite reform in the Netherlands that reduced pension wealth. Broadly speaking, this literature tends to find that public pension wealth crowds out private savings to some extent, with results naturally varying across contexts.

We also relate to the literature that studies labor supply responses to changes in pension eligibility ages (Mastrobuoni 2009, Behaghel and Blau 2012, Staubli and Zweimüller 2013, Manoli and Weber 2016, Lalive, Magesan and Staubli 2017, Geyer and Welteke 2019, Haller 2019, Deshpande, Fadlon and Gray 2021, Geyer et al. 2020, Nakazawa 2022, and Seibold 2021).[1] This literature generally finds that individuals extend employment in response to increases in official retirement ages.

Our approach connects these two literatures, as we hone in on the prominent type of pension reform that increases social security eligibility ages and unpack the causal effects of this policy on savings through the lens of a standard lifecycle framework.[2] In doing so, we make three main contributions to the literature on social security and savings. First, we provide novel evidence on how savings respond to increases in social security eligibility ages using a compelling RD design and population-wide administrative data. Second, we leverage our data to analyze separate measures of third-party reported assets and liabilities throughout the financial portfolio, whereas the literature has typically been restricted to using survey measures of total savings such as self-reported income minus self-reported

[1] We also link to papers on pensions and retirement more generally, reviewed by Krueger and Meyer (2002) and Blundell, French and Tetlow (2016). For example, Burtless and Moffitt (1985), Asch, Haider and Zissimopoulos (2005), Coile and Gruber (2007), Liebman, Luttmer and Seif (2009), Brown (2013), and Manoli and Weber (2016) also analyze nonlinear budget sets from pensions.

[2] Two contemporaneous papers use approaches similar to ours. Etgeton et al. (2021) study anticipatory savings responses to a reform that increased the early retirement age of women using survey data from Germany. Nakazawa (2022) studies primarily how increasing pension eligibility ages impacts labor supply but also investigates savings, consumption, and physical and mental health using survey data from Japan.

consumption. We view this as an important step forward, as different types of vehicles for savings may differ in the extent to which they serve as substitutes for public pension wealth. Our findings highlight in particular a distinction between retirement accounts and other savings, itself the subject of a related strand of literature.[3] Third, we exploit our setting to provide a more thorough exploration into mechanisms. We are able to uncover evidence suggesting inertia as an operative channel through our ability to study both anticipation and post-implementation time periods, through the panel structure of our data (which allows us to explore heterogeneous responses by previous savings behaviors), and through the employer-employee linkages in our data (which allow us to explore the role of firm default contribution rates).

Overall, our results have broad implications for policy. Importantly, we find that in our setting the often-pulled policy lever of raising eligibility ages for public pensions leads to more savings set aside in retirement accounts for shorter retirement time horizons. Our findings therefore show how this policy instrument can increase and improve the adequacy of retirement savings when individuals are covered by employer-sponsored retirement plans. Here it is worth emphasizing that savings responses to public pension reforms are likely to be highly context specific, so it is important to understand the features that give rise to responses. In our case, our findings come about through inertia and quasi-mandatory employer-sponsored savings. Our findings thus build on and are consistent with those of Chetty et al. (2014), who show that the majority of Danish savers are passive.[4] That is, in a setting where individuals tend to accumulate savings passively, we find that savings responses to a social security reform come about primarily through delayed retirement and the continued accumulation of retirement savings. More generally, our results lend support to models that give rise to inertia in savings behaviors and underscore a tight link between employment and savings. They also emphasize that policy makers should consider interactions with firm policies when designing public policies.

The rest of this paper is organized as follows. Section 2 provides an overview of the institutional background. Section 3 grounds our analysis with a conceptual framework. Section 4 describes the data. Section 5 lays out our identification strategy. Section 6 documents the causal effects of the reform. Section 7 investigates mechanisms. We conclude in Section 8.

[3]For work on how retirement savings affect total savings, see, e.g., Poterba, Venti and Wise (1996), Engen, Gale and Scholz (1996), Bernheim (2002), Gelber (2011), Chetty et al. (2014), and Andersen (2018).

[4]The influential paper by Chetty et al. (2014) presents many key findings to show evidence of passive savings behavior; most related to our results on how savings respond to a social security reform, they show that a temporary government mandatory savings program did not crowd out private savings of low-income individuals.

2 Institutional Background

The Danish retirement system is broadly typical of other OECD countries. Primary sources of retirement income include private retirement savings accounts and public pension benefits. In this section, we first discuss the central features of the retirement system, and then we discuss the policy reform. More background information can be found in Appendix C.

2.1 Private Retirement Savings Accounts

As is typical of other modern economies, defined-contribution private retirement savings accounts in Denmark constitute a key source of income in older age. Retirement savings plans can be either employer-sponsored accounts, analogous to 401(k)s in the U.S., or personal accounts, analogous to Individual Retirement Accounts (IRAs). The treatment of these savings accounts in the tax code is similar to the U.S setting: contributions are tax-deductible, returns are tax-advantaged, distributions from the accounts are taxed upon withdrawal, and penalties exist on early withdrawals before age 60.[5]

Broadly speaking, participation in employer retirement savings plans in Denmark is often quasi-mandatory. Collective bargaining agreements between labor market unions and employer associations cover the majority of workers. These agreements frequently stipulate a minimum percentage of wages that are to be contributed to employer-sponsored retirement savings accounts, with employees being able to contribute more, but not less, than the mandated amount. Typically most of the contributions come directly from employers with the rest coming from employees via payroll deductions. These features mean that contribution rates to employer-sponsored accounts tend to be similar for workers under the same agreement. For workers not covered by these agreements, firms often set their own default contribution rates. Note that the limited flexibility within employer plans is an important feature of our context that should be kept in mind and that we will revisit later. In contrast to employer plans, contributing to personal retirement savings plans is completely voluntary.

[5] Our analysis mostly focuses on these traditional retirement accounts, but in 2013, Denmark introduced "Roth-style" plans. Contributions to these plans are not tax deductible, but distributions are tax-free. For completeness, we study Roth-style plans as well, though they likely make up a much smaller fraction of the asset portfolio for the birth cohorts that we study, who were 59 years old when the accounts were first introduced.

2.2 Public Pension Benefits

Public old-age retirement benefits come from two main sources. The Old Age Pension (OAP) provides basic retirement income security, and the Voluntary Early Retirement Pension (VERP) provides early retirement benefits for those who choose to participate in the program. Participation in VERP requires making modest contributions to qualified Unemployment Insurance (UI) funds during working life. While originally aimed at workers with physically demanding jobs upon its introduction in 1979, it became a more general and common route to retirement (OECD 2005). The majority of workers—about 70% of the individuals in the birth cohorts that we study—choose to participate. We focus our study on those participating in VERP, as it has historically played a major role in determining retirement patterns of the Danish population. The two programs are closely connected; however, the provision of benefits from each program is governed by different rules and regulations.

2.2.1 Voluntary Early Retirement Pension

The VERP program provides up to five years of early retirement benefits, starting at the Early Retirement Age (ERA) of 60 and ending at the Full Retirement Age (FRA) of 65. The most important idea for our study is that the features of the VERP program produce strong incentives to concurrently claim benefits and retire either right at the ERA or right at the incentivized age two years later. The following details explain why this is the case.

Workers claim into the VERP program, at which point they lock in their annual base benefits. Benefits are roughly $27,000 (in 2010 U.S. dollars), which are then subject to strict means testing.[6] First, base benefits for the duration of the program are reduced against wealth held in private retirement accounts right before reaching age 60.[7] Second, benefit payouts are reduced against drawdown income from retirement accounts. Third, benefit payouts are additionally reduced against hours worked at a rate of 100%, which creates high implicit taxes on continued work after claiming. Even more, there are no actuarial adjustments for delaying claiming; deferring claiming simply forfeits benefits. For example, claiming at 61 results in only four years of benefits instead of five.

Two key rules drive the incentives to claim and retire either right at the ERA of 60, or the incentivized age of 62. First, the "transition rule" requires workers to be available to

[6]Benefit amounts are determined through a formula linked to the UI system, but are capped at 91% of the maximum amount of UI benefits, which leads to base benefits that are in practice largely flat-rate.

[7]The government collects information on retirement account balances around age $59\frac{1}{2}$, and the base benefits are reduced using this information. The means testing rules depend on many factors, but roughly call for base benefits to be reduced by 60% of could-be annuitized income from retirement accounts.

the labor force in order to be eligible to claim. An important implication of this rule is that retiring and dropping out of the workforce before reaching the ERA results in forgoing the entire five years of VERP eligibility. This rule creates strong incentives for workers to wait to retire until at least reaching the ERA (whereas the high implicit taxes and lack of adjustments for deferring claiming discourage working after the ERA). Second, the "two-year rule" creates financial incentives for some to claim VERP and retire at age 62. Most importantly, working and deferring claiming until age 62 results in the elimination of the means testing of VERP base benefits against private retirement account balances. Some additional but smaller financial incentives exist as well, though the means testing of benefit payouts against drawdown income and hours worked remain.[8] This relaxation of means testing after age 62 can create strong financial incentives to wait to retire until age 62, especially for those with significant assets in private retirement accounts.

2.2.2 Old Age Pension

Upon reaching the FRA of 65, retirees transition from VERP to the OAP, which provides flat-rate, old-age benefits until death. The key idea for our study is that OAP wealth largely does not depend on retirement age. Annual benefits are roughly $15,000 for married individuals and $20,000 for single individuals, but are reduced proportionally for those who have not lived in Denmark for at least 40 years. OAP benefits are means-tested against income, subject to an income test, but those wishing to continue to work can take advantage of approximately actuarially-fair adjustments for deferring claiming.

2.3 The 2011 Reform on Later Retirement

In response to population aging and budgetary concerns, the Danish government announced in May of 2011 a major reform to the retirement system. A key component of the reform phased in stepwise 6-month increases in pension eligibility ages, contingent on birthdate. Figure 1 illustrates how the reform indexed each of the three eligibility ages to birthdate in a discontinuous fashion. We focus our entire analysis on the first birthdate discontinuity generated by the reform, which forms the cleanest quasi-experiment by creating a treatment and control group who differ only in their pension eligibility ages. The rules and regulations governing benefit amounts and means testing did not change for the sample we study.[9]

[8]Satisfying the two-year rule results in a modest increase in base benefit amounts as well, to about $29,600, as benefits become linked to 100% (rather than 91%) of maximum UI benefits. See Appendix C for details.

[9]The later phases of the reform continued to increase eligibility ages as illustrated in the figure, but also made more changes to the VERP program. The reform created more stringent VERP participation rules,

We exploit the fact that those born on January 1, 1954 learn in 2011 that their ERA has increased to $60\frac{1}{2}$, their incentivized retirement age has increased to $62\frac{1}{2}$, and their FRA has increased to $65\frac{1}{2}$. In contrast, those born one day earlier experience no change in their pension eligibility ages, which remain at 60, 62, and 65. Our identification strategy exploits the discontinuous nature of the policy change, as those born right around the birthdate cutoff should be similar in all aspects, yet face different incentives due to the reform.

3 Economic Framework

We use a simple lifecycle framework to model the key features of the pension system and to illustrate the changes in incentives brought on by the reform. Building directly on Laitner and Silverman (2007) and Hurd, Michaud and Rohwedder (2012), we write down a standard dynamic model of consumption with an endogenous retirement decision and no uncertainty. We have two goals. First, we aim to ground our study in baseline theory to aid in the interpretation of our results. Second, we aim to provide benchmark predictions that can be mapped to our empirical analysis.

3.1 Model Setup and Solution

We borrow the model from Hurd, Michaud and Rohwedder (2012). Consider people making decisions throughout continuous time $t \in [0, T]$. They choose consumption, c_t, and when to retire, $t = R$. Wages are constant while working so that $y_t = y$. Pension benefits received after retirement, $b_t(R)$, depend on the retirement age, and the present value of pension wealth is given by $B(R) = \int_R^T e^{-rt} b_t(R) dt$, where r is the interest rate. Utility during working life is given by $u(c_t)$, and utility in retirement is given by $u(c_t) + \Gamma$, where Γ is the utility gain from leisure. For simplicity, assume the rate of time preference, ρ, equals the interest rate r.

increased the standard base benefit amounts, and implemented stricter means testing policies against assets in retirement accounts. Importantly, these changes were phased in to impact later birth cohorts, and do not affect the individuals at the discontinuity that we study. However, due to the reform, VERP members were able to opt out of the program in 2012. While this option was likely targeted towards later cohorts that experience the more strict changes to VERP, in the appendix we investigate its relevance for our analysis and find no evidence of a change in participation in our sample.

Formally, people solve the following optimization problem:

$$\begin{aligned} \max_{R,\{c_s\}_{s=0}^{R}} \quad & \int_0^R e^{-\rho t} u(c_t) dt + \Psi(a_R + B(R), R) \\ \text{s.t.} \quad & \dot{a}_t = r a_t + y_t - c_t \\ & a_0 = 0, \end{aligned} \tag{1}$$

where $\Psi(a_R + B(R), R)$ is the post-retirement indirect utility given by

$$\begin{aligned} \Psi(a_R + B(R), R) = \max_{\{c_s\}_{s=R}^{T}} \quad & \int_R^T e^{-\rho t} \left(u(c_t) + \Gamma \right) dt \\ \text{s.t.} \quad & \dot{a}_t = r a_t - c_t \\ & a_T = 0. \end{aligned} \tag{2}$$

For any given retirement age R, this formal problem has a familiar solution for consumption. After deriving first-order conditions, one can write:

$$\frac{u''(c_t)}{u'(c_t)} \dot{c}_t = \rho - r. \tag{3}$$

Individuals should perfectly smooth consumption, as it is assumed that the utility discount rate equals the interest rate and that the marginal utility of consumption does not depend on retirement status (or leisure). Consumption in each period thus depends on lifetime resources, which depend on the timing of retirement:

$$c_t = c(Y(R), B(R)) = \frac{C^L}{T}, \tag{4}$$

where C^L is lifetime consumption and $Y(R) = y \int_0^R e^{-rs} ds$ is the present discounted value of lifetime earnings. The following first-order condition describes the optimal age of retirement:

$$(y + B'(R)) \cdot u'(c_R) = \Gamma. \tag{5}$$

The left-hand side is the marginal benefit of retiring later—the financial return to working longer converted to utility units using the marginal utility of consumption—and the right-hand side is the marginal cost of retiring later—foregone utility of leisure.

3.2 Retirement Incentives Before the Reform

This simple model offers insight into retirement decisions in our setting. Assume that heterogeneous preferences for leisure are smoothly distributed. If individuals face a linear budget constraint, that is, if the financial return to work, $y + B'(R)$, is constant, then the distribution of optimal retirement ages would be governed by some smooth density function.

However, in our setting, pension wealth $B(R)$ is highly non-linear in retirement age R. The black line in Figure 2 illustrates this notion graphically by plotting public pension wealth against retirement age for a worker from the pre-reform birth cohort. We can see that the key features of the system create two large spikes in pension wealth. The first spike occurs right at the ERA of 60. Retiring before this age results in a failure to satisfy the transition rule, and thus the inability to claim VERP benefits, which means public pension wealth is given by only the OAP. Retiring right at 60 discontinuously increases pension wealth by the entire 5 years of VERP benefits. The second spike occurs right at age 62, the age at which means testing of VERP benefits against private retirement account balances is eliminated. Retiring one day before age 62 locks in three years of standard VERP benefits, whereas retiring one day later increases benefit payouts in each year due to reduced means testing.

The spikes in pension wealth at the critical ages translate to discontinuities in lifetime budget constraints. The black line in Figure 3 plots lifetime consumption C^L against retirement age, for the same worker from the pre-reform cohort. The discontinuities at 60 and 62 should induce bunching in the claiming and retirement distributions, as those who would have otherwise retired just before or just after these ages find it optimal to claim and retire right at the critical ages.[10]

We let the data speak to the strength of these bunching incentives in our setting. Figure 4 plots the empirical distributions of VERP claiming ages and retirement ages. The black lines in each graph depict the distributions for those born before the January 1, 1954 birthdate cutoff, who are not affected by the reform.[11] Graph (a) shows the distribution of VERP claiming ages. We observe a large spike at each of the VERP critical ages. Graph (b) shows

[10]Note that incentive-induced bunching in retirement is not unique to Denmark. For example, Brown (2013) analyzes bunching in retirement at both kink and notch points created by incentives in the pension system for California teachers in the United States, Manoli and Weber (2016) study bunching at the early retirement age in Austria, and Seibold (2021) studies bunching at numerous benefit discontinuities in Germany. For a general review of the bunching literature, see Kleven (2016).

[11]Details on the monthly data underlying this graph can be found in Section 4. The sample consists of workers born within six months of January 1, 1954. We define monthly claiming age as the age of the individual upon initial receipt of VERP benefits and we define monthly retirement age as the age of the individual in the last month during which earnings are positive, before permanently falling to zero.

the distribution of retirement ages. There are few retirements before age 60, and the spikes in retirement at the critical ages are large, indicating that the strong financial incentives to retire at either exactly the ERA or exactly two years after the ERA shape labor supply decisions of older workers.

3.3 Reform-Induced Incentives and Benchmark Predictions

The 2011 reform increased social security eligibility ages. In our framework, the major change is a shift in the location of the spikes in public pension wealth, $B(R)$, to $60\frac{1}{2}$ and $62\frac{1}{2}$. This change is depicted by the maroon line in Figure 2, which then changes the lifetime budget constraint as depicted by the maroon lines in Figure 3. How should we expect individuals to respond to the reform? To ultimately provide benchmark predictions for savings, we first discuss changes in retirement incentives due to the reform. We then turn to the data to observe how the reform actually changed the claiming and retirement distributions. Finally, guided by these retirement responses borne out in the data, we use our framework to assess how savings should respond.

Given the strong retirement incentives attached to VERP eligibility ages, we expect the dominant forces at play to essentially shift the bunching masses at 60 and 62 to $60\frac{1}{2}$ and $62\frac{1}{2}$, respectively. We expect the influence of any other incentives to be minor. To examine whether this is the case, and to make headway on our predictions for savings, we directly evaluate the impact of the reform on claiming and retirement ages in the data.

Graph (a) of Figure 4 shows how the empirical distribution of claiming ages shifts after the reform. The maroon line depicts the behavior of those born after the birthdate cutoff, who are affected by the reform and face budget constraints corresponding to the maroon lines in Figure 3. We see that there is a shift of the spikes in the distribution of claiming ages to the new critical ages. Turning to retirement, the maroon line in graph (b) of Figure 4 shows that the reform also induces a clear shift in bunching to the new pension eligibility ages and thus induced later retirement for many individuals.

Given these reform-induced labor supply responses, we can provide benchmark predictions for savings. A key feature of the lifecycle framework is that future pension benefits and wages impact current consumption and savings, since individuals consider lifetime resources when determining optimal consumption paths. The reform induces later retirement, which represents an increase in lifetime income. This extra income should be spread over the lifecycle in the form of increased consumption in every period. This change in the consumption profile thus yields three implications for savings (income less consumption), that can be directly mapped to our empirical analysis. First, during the anticipation period, after the announcement of the reform but before it is implemented, savings should *decrease* on average, as income during this period is unchanged but consumption should increase. Second, during the reform-induced periods of extended employment (e.g., between ages 60 and $60\frac{1}{2}$), savings should *increase* on average. The predicted increase in consumption remains, but income is higher due to extended employment, and the increase in consumption cannot be greater than the increase in income, as some of the extra income should be saved to finance increased consumption throughout later stages of the lifecycle. Third, in periods after retirement, savings should decrease on average, as in the anticipation period, as income should be unchanged but consumption should increase.

These benchmark predictions help us to structure the empirical analysis and provide guidance on how we might expect total savings to evolve. Here we note two features of our context that can help set expectations regarding specific savings vehicles. First, because of the means testing of VERP benefits against retirement savings, those looking to increase savings during periods of policy-induced extended employment might have incentives to do so in non-retirement savings vehicles (although this should be weighed against the tax advantages of retirement accounts). Second, because many are limited in their scope to adjust quasi-mandatory employer retirement accounts, we might expect any declines in savings over the anticipation period to occur in other vehicles; further, we might expect mechanical increases in savings in employer accounts during periods of extended employment, with individuals then either increasing or decreasing other savings to reach their desired level of total savings. In Section 7, after documenting the main results, we conduct additional analyses to assess the role of employer contribution rate policies in explaining our findings.

4 Data

To study empirically how raising pension eligibility ages impacts savings, we use primarily annual administrative register data that cover the entire population of Denmark from 1985 to 2018. We use unique personal identifiers for individuals to link together population registers, which contain information on demographics (importantly including exact date of birth), with labor-market registers, which contain detailed information on income and savings, in order to create a rich annual panel dataset. We use these data to conduct the bulk of our analyses.

We also use a complementary, monthly-level administrative dataset that spans the years 2008 to 2017 and that contains employment information on all employees in Denmark as well as information on government transfers for all individuals.[12] We use these data to more finely track exits from the labor force and to conduct the bunching analysis discussed above.

4.1 Key Variables

Our data constitute some of the highest quality data available on savings. They contain third-party reported variables on assets that essentially capture the entire financial portfolio.[13] This allows us to avoid potential problems associated with studying self-reported measures of savings, and it allows us to study separately several types of savings.

Our first set of outcomes capture savings in defined contribution retirement accounts, which might naturally be considered the closest substitutes for public pension wealth. We observe flow variables in the data for these savings accounts. We study as our main outcomes contributions to traditional employer-sponsored accounts and personal accounts. We also study indicator variables for making positive contributions to personal retirement accounts, which are informative in their own right, as contributions to these plans are voluntary and much less common than contributions to employer-sponsored accounts. For completeness, we study contributions to Roth-style accounts as well as annuitized distributions from retirement accounts, although we are unable to distinguish between distributions from employer plans and personal plans. We winsorize contribution amounts at the 95th percentile, by year, in order to reduce the influence of outliers in our analyses.

Our second set of outcomes capture other types of savings. Specifically, we study bank accounts, stock market accounts, property values, and liabilities. For these types of savings, we do not observe flow variables, but rather stock variables. We observe in the data measures of bank account balances and stock market account balances that correspond to the value of assets held at the end of the calendar year, reported to tax authorities by financial institutions. Our measure of property values corresponds to year-end model-based appraisal values of properties as computed by the Danish tax authorities (SKAT), and it is updated at least every other year, with additional updates upon significant changes to the property or by request of the owner. Our measure of liabilities captures the year-end value of all debts except private debt. Among other types of debt, it includes debt in financial institutions, credit card debt, and mortgage debt. We use these measures to compute flow variables of

[12] This dataset, known as the *eIncome* register, contains information on earnings that firms report to tax authorities at a monthly frequency and on pension benefits paid by the government to all recipients. See Kreiner, Leth-Petersen and Skov (2016) and Kreiner, Leth-Petersen and Skov (2017) for more discussion on this relatively new dataset.

[13] Omissions from the data include cash holdings outside of bank accounts, private debt to individuals, and the value of consumer durable goods.

savings in year t by subtracting year-end balances in year t with those from year $t-1$. We thus study as our main outcomes changes in bank account balances, changes in stock market accounts, changes in property values, and changes in liabilities. We winsorize these outcome variables (which unlike contributions to retirement accounts are not naturally bounded below by zero) at the 5th and 95th percentile in each year.

We also study three aggregate measures of the various savings components to provide direct evidence on the total effect on savings. First, we study total (net) retirement savings, defined as the sum of contributions to all retirement accounts minus the distributions from retirement accounts. Second, we study total non-retirement savings, defined as the sum of changes in bank account balances, changes in stock market account balances, and changes in property values, minus changes in liabilities. Third, we study a complete measure of total savings, defined as the sum of retirement savings and non-retirement savings.

Finally, we study as our main measure of labor supply pre-tax earnings, as defined by the amount of income on which individuals pay an 8% labor market tax. We also winsorize this variable by year at the 95th percentile. To define retirement ages, we use our monthly-level data. We use an absorbing state measure for retirement. We define monthly retirement age as the age of the individual in the last month during which earnings are positive, before permanently falling to zero. We study as our measure of benefit claiming annual VERP benefit payments. We deflate all monetary values to 2010 levels and convert Danish kroner (DKK) to U.S. dollars. The exchange rate in 2010 was approximately 5.56 DKK to 1 USD.

4.2 Analysis Sample

Our analysis sample focuses on individuals participating in VERP who are born right around the first birthdate discontinuity generated by the 2011 reform. Starting with our data on the entire Danish population from 1985 to 2018, we carry out four sample restrictions. First, we include only Danes born within six months of the cutoff date, January 1, 1954. Second, we keep only individuals who made regular participatory contributions to the VERP scheme before the reform was announced. Specifically, we keep those who made contributions in at least 70% of the pre-announcement years between 2001 and 2010.[14] Third, we balance the sample between 2006 and 2018; that is, we exclude individuals who are not in the data for all of these years. Fourth, we exclude the self-employed (defined during the pre-announcement period), who are subject to different rules and regulations concerning their early retirement

[14] We do not require contributions in 100% of the pre-announcement years in order to allow for short lapses in contributions, for which the program allows, as individuals in our analysis sample are required to contribute in 25 out of the last 30 years to be eligible for VERP.

options through the VERP scheme. Appendix Table A.1 documents how each of these four sample restrictions impacts the size of the analysis sample.

Table 1 presents summary statistics for calendar year 2010, before the reform is announced. Columns (1) and (2) display means and standard deviations of key variables for the 40,042 individuals remaining after applying the restrictions.[15] These are the individuals who make up the bunching analysis described above. Columns (3) and (4) display statistics for the 15,789 individuals who ultimately make up the RD estimation sample, those born within 74 days of the birthdate cutoff. Note that this more local sample looks remarkably similar to the full analysis sample.

Overall, we see that our sample contains active older workers, most of whom are married. Average earnings in 2010 amount to approximately \$59,000. Most individuals (89%) make contributions to employer-sponsored retirement accounts, likely due to quasi-mandatory participation for many, and 41% of individuals contribute to personal retirement accounts. Average bank account balances amount to roughly \$26,000, whereas stock market account balances are smaller on average at about \$7,000.

Finally, columns (5) and (6) provide more context on our sample selection criteria and display statistics for an unrestricted version of our analysis sample, without imposing that individuals were making participatory contributions to the VERP scheme. We see that this restriction results in an analysis sample of individuals that are slightly more likely to be married, that earn more income, and that save more. However, the samples are not dramatically different, as most individuals in the population make participatory contributions.

5 Identification Strategy

5.1 Regression Discontinuity Design

To identify the causal effects of increasing pension eligibility ages on savings and labor market outcomes, we employ a regression discontinuity (RD) design. We derive identification from the discontinuous change in eligibility ages contingent on birthdate. Due to the 2011 reform, individuals born on or after January 1, 1954 face pension eligibility ages of $60\frac{1}{2}$, $62\frac{1}{2}$, and $65\frac{1}{2}$, whereas those born just before face the previous eligibility ages of 60, 62, and 65.

We use our RD design to estimate discontinuous changes in outcome variables at the

[15] We conduct our analysis at the individual level because Denmark maintains individual-level tax and pension systems. See García-Miralles and Leganza (2021) for a study on the retirement behavior of couples in Denmark.

birthdate cutoff. Specifically, we estimate equations of the following form:

$$y_i = \alpha + \beta \cdot 1[x_i \geq c] + \gamma \cdot (x_i - c) + \delta \cdot 1[x_i \geq c] \cdot (x_i - c) + Z_i\theta + \varepsilon_i, \tag{6}$$

where y_i is an outcome variable for individual i (such as contributions to retirement savings accounts over some specified time period), x_i is birthdate, the running variable, c is the birthdate cutoff, Z_i is a vector of pre-determined control variables, and ε_i is an error term. The coefficient of interest is β, which captures the average impact on the outcome of the six-month increase in pension eligibility ages for those born right around the birthdate cutoff.

In our baseline regression specification, we use triangular weights and include as controls a dummy for gender, a dummy for being married in the pre-announcement year of 2010, and dummies for the five pre-announcement year administrative regions of residence. To choose our bandwidth, we use the procedure from Calonico, Cattaneo and Titiunik (2014) to select the optimal bandwidth for labor market earnings during the first critical year of 2014, which turns out to be 74 days, and we use this bandwidth throughout the main analysis to keep the underlying sample constant. We probe the robustness of our results to these specification choices in Section 6.4. Specifically, we show results for a wide range of bandwidths (between 28 days and 180 days), and we assess the sensitivity of our results to not using triangular weights and to excluding controls.

5.2 Threats to Identification and Assessment of Validity

The identifying assumption in our RD design is that other factors that could influence outcome variables do so smoothly in birthdate through the cutoff. In implementing our design, we estimate sharp jumps in outcomes right at the cutoff; causal interpretation of our results relies on the assumption that, in the absence of the policy-induced discontinuity in pension eligibility ages, outcome variables would have evolved smoothly through the cutoff.

The classical threat to identification in RD designs is manipulation, which would typically generate a non-smooth density of the running variable. Manipulation in the usual sense is unlikely to be a potential problem in our setting, because our running variable is birthdate, which for our analysis group is determined decades before the policy is announced. The other main threat is the presence of some other policy that changes discontinuously around the January 1, 1954 birthdate cutoff. We are not aware of other reforms that specifically targeted this birthdate discontinuity, but education policy in Denmark can lead to discontinuities in school starting age for those born in January compared to December (Landersø, Nielsen and Simonsen 2017). This could be a problem if school starting age influences the later-life outcomes that we study, however two placebo exercises (discussed in more detail later) lead us to believe that this is unlikely to be the case. First, we find no evidence of discontinuities at the January 1, 1954 cutoff for outcome variables defined over a pre-period, before the reform that we study is announced. Second, we find no evidence of discontinuities at January 1 cutoffs for other cohorts that are unaffected by the policy that we study when they reach their key retirement ages of 60 and 62.

Another threat to our design is the possibility of differential attrition by birthdate, as we ultimately balance our sample, selecting on being alive and in Denmark. If the reform impacts the propensity to drop out of the data (either due to death or leaving the country) in a way that is not as good as random as it relates to the outcome variables that we study, then balancing the sample as we do could bias our estimates.

We first note that while the literature on the mortality effects of social security income and pension eligibility ages across contexts is generally mixed (e.g., Snyder and Evans 2006, Kuhn et al. 2020, Hernaes et al. 2013, Fitzpatrick and Moore 2018), a recent paper finds no evidence that early retirement in Denmark impacts mortality (Nielsen 2019). Nonetheless, to more directly investigate the possibility of differential attrition in our study, we examine the density of our running variable in the spirit of McCrary (2008). Appendix Figure A.1 plots a simple histogram of the running variable, birthdate, for the entire analysis sample. We also superimpose on top of the histogram smoothed values and confidence intervals from local polynomial regressions of the number of individuals on birthdate. A formal density test as proposed by Cattaneo, Jansson and Ma (2019) using our baseline bandwidth results in a p-value of 0.125, so we fail to reject the null hypothesis of a smooth density.

As an additional check on the validity of our RD design, we investigate the smoothness of the (pre-determined) control variables through the birthdate cutoff. We estimate equation (6) without any covariates on the right-hand side, instead using each control variable as a left-hand side outcome variable. Appendix Table A.2 presents these results. There are no statistically significant discontinuities in any of the control variables at the cutoff.

6 Main Results: Impact of Increasing Pension Eligibility Ages

In this section, we present our main results, which document the causal effects of increasing pension eligibility ages. We often lead with standard graphical analyses, which offer non-parametric representations of the causal effects of the reform. Specifically, we plot means of key outcome variables in one-week date-of-birth bins for those born around the birthdate cutoff, and we superimpose on these plots regression lines from estimating separate linear trends in the running variable for observations within our bandwidth on either side of the cutoff. We then use regression estimates to quantify magnitudes and assess the statistical significance of our findings.

6.1 Responses During the Anticipation Period

We begin by documenting the impacts of the reform over the anticipation period. Recall that this period captures responses after the announcement, but before the implementation, of the reform. The individuals we study are 57 years old when the reform is announced, giving them time to make consumption and savings adjustments before they reach age 60, at which point differences in pension eligibility from the reform manifest themselves. The benchmark prediction laid out in Section 3 suggests a negative impact on savings over the anticipation period, as treated individuals should increase current consumption due to the net increase in lifetime income that will come from delayed retirement.

We find no evidence of anticipatory savings responses, although we show that our estimates over this period suffer from a lack of statistical precision. We note that recent work that focuses on labor supply and earnings in the context of pension reform also finds little support for forward-looking responses (Gelber, Isen and Song 2016 and Haller 2019). Figure 5 illustrates our results graphically. Each graph corresponds to a different key outcome, where the outcome variables are averaged over the anticipation time period. Graph (a) illustrates the RD estimate of the reform on average annual contributions to employer-sponsored retirement accounts between 2011 and 2013 and shows no evidence of a discontinuous change in this outcome at the birthdate cutoff. Similarly, graphs (b), (c), and (d) show no evidence of changes in contributions to personal retirement accounts, non-retirement savings, or total savings, respectively.[16]

Table 2 presents the corresponding regression analyses. We report RD estimates of β from estimating equation (6) using our baseline specification. All point estimates are statistically indistinguishable from zero, and our estimate for total savings is a statistically insignificant \$301 (s.e. 400).[17]

[16] Appendix Figure A.2 shows graphical results for each of the savings components (bank accounts, stock market accounts, property values, and liabilities) that make up the total non-retirement savings variable.

[17] We draw attention to a relevant robustness exercise regarding these estimates. Because capital gains from selling property can make it particularly difficult to interpret results on financial account flows, we perform a robustness analysis where we look at anticipatory savings responses while excluding individuals

How does this estimate compare in magnitude to what we might have expected? To provide more context, we carry out a stylized back-of-the-envelope calculation detailed in Appendix B. Given the observed retirement responses, we calculate the expected increase in lifetime income resulting from extended employment and the predicted decline in savings if individuals perfectly smooth consumption, as in our lifecycle framework. Our calculations suggest that aggregate average savings should decline by around \$220 annually over the anticipation time period. While our estimate for total savings is of the opposite sign of the benchmark prediction, our confidence interval does not allow us to rule out responses within the range of the back-of-the-envelope calculation.

In Section 7, where we explore mechanisms, we further investigate the anticipation period, but we first document the causal effects of the reform over the early retirement period, which allows us to then assess and discuss the overall body of evidence as a whole.

6.2 Responses During the Early Retirement Period

We now estimate the impact of the reform over the years 2014 to 2018. Discontinuities in these years reflect responses due to the implementation of the reform. Recall from Figure 4 that the reform induces extended employment, in order to comply with the strong incentives now attached to the new pension eligibility ages. In our RD framework, we expect the shift in the spike in retirement at age 60 to age $60\frac{1}{2}$ to manifest itself as increases in earnings during 2014, the year during which our treatment and control group are both age 60, but when those in the treatment group retiring right at the ERA work six more months than their control group counterparts. Likewise, we expect the shift in the spike in retirement at age 62 to age $62\frac{1}{2}$ to be captured by the RD estimates in 2016. We call years 2014 and 2016 "critical years," and we call age 60 and age 62 "critical ages," as this is when individuals reach the two eligibility ages in the VERP scheme. Recall also that the benchmark lifecycle framework predicts increases in savings during these critical years, as individuals consume some of the extra income from continued work, but save some for future consumption.

Calendar year 2014 corresponds to the first critical year of the early retirement period, when individuals are age 60 and differences in public pension eligibility present themselves. Figure 6 graphically depicts responses to the reform during this year. Graph (a) shows that the treatment group receives less VERP benefits during the year, almost exactly half of the average amount received by the control group, consistent with early retirees claiming right

with real estate transactions. The results, displayed in Appendix Table A.5, are quite similar to our leading estimates.

at $60\frac{1}{2}$, now that they are no longer eligible to claim at 60. Graph (b) shows a visually clear and large discontinuous increase in earnings amounting to \$6,325, which is a 14% increase off of a mean of \$44,359. These results are consistent with the delayed retirement documented in Figure 4.[18]

Graph (c) of Figure 6 illustrates the effect of the reform on contributions to employer-sponsored retirement savings accounts. The RD estimate indicates an increase of \$800 to these retirement plans, which represents a meaningful 16% increase off of the control group mean of \$4,940. This result is perhaps unsurprising, given the quasi-mandatory nature of employer-sponsored retirement savings. That is, here we find increased savings in employer retirement accounts precisely when we find postponed retirement and increased earnings. Indeed, in Section 7 we unpack this increase in savings in employer retirement plans and provide empirical evidence that a good deal of this response is driven by continued contributions at employer-set default contribution rates.

Graph (d) shows a discontinuous increase in contributions to personal retirement plans as well. The average increase in contributions to these completely voluntary accounts is \$84 dollars, a 21% increase off of the mean. These plans are much less common than employer-sponsored plans, and average contribution amounts are thus heavily influenced by a large number of individuals contributing zero dollars, so we also study indicator variables for making contributions to personal retirement plans. We find a large, visually clear jump in the likelihood of making a contribution; those in the treatment group are 4.8 percentage points more likely to contribute, a 34% increase off of a small mean of 14% (see Appendix Figure A.3 and Appendix Table A.3).

Graph (e) illustrates the effect on total non-retirement savings and graph (f) illustrates the effect on total savings. The data underlying graph (e) come from combining the more noisy measures of changes in stock variables, and there appears to be an increasing trend in savings for those born to the left of the cutoff, but the graph also shows no evidence of a discontinuous change right at the cutoff. Graph (f) shows an increase in total savings. The overall pattern of the dots is clearly influenced by the non-retirement savings outcomes, but the graph indicates that total savings increases at the cutoff by \$939 (s.e. 552).

We present regression-based results for all outcomes in column (1) of Table 3. Panel B shows that the reform not only results in greater contributions to both employer and personal

[18]Note though that the earnings response is larger than what we might have expected based on a rough calculation. Approximately 15.8% of workers retire right around the first eligibility age, and these workers earned on average \$47,806 in the pre-period (see Appendix B); if these individuals work an additional 6 months, we would expect earnings to increase by about (0.158)(47,806)(.5) = \$3,777.

retirement accounts, it also leads to a decrease in annuitized distributions from retirement accounts. Treatment individuals receive payments from retirement accounts that are about \$233 less on average. Panel C reports RD estimates for each of the components of other savings. None of the estimates are statistically distinguishable from zero. Panel D shows how the statistically significant (at the 10% level) increase in total savings is driven by a large and highly statistically significant increase in total retirement savings (which combines all retirement savings components presented in panel B), whereas there is no evidence of a change in non-retirement savings. Overall, results from the first critical year show that in response to increases in pension eligibility ages, individuals earn more from continuing to work and save more in retirement accounts.

Calendar year 2015 is not a critical year; in this year our analysis sample individuals are 61 years old. Those retiring right at the ERA have already done so, and those waiting to retire until the incentivized age must continue working until either age 62 or $62\frac{1}{2}$. The first column of Table 4 reports muted labor supply and savings responses during this time.

In 2016, the second VERP critical year, our analysis sample individuals are 62 years old. Those who have continued to work in order to claim into VERP right when the means testing is relaxed retire during this year, either at age 62 for the control group or age $62\frac{1}{2}$ for the treatment group. Key results are graphically illustrated in Figure 7, and regression estimates for this year are reported in column (3) of Table 3.

We find responses similar to those during the first critical year. Treated individuals receive less VERP benefits and experience a \$5,299 increase in earnings, which is a 16% increase off of the mean. Again we see that extended employment leads to more savings in retirement accounts. Contributions to employer-sponsored retirement plans increase by a statistically significant \$721. The point estimate for contributions to personal plans in dollars is positive but small and statistically insignificant; however, Appendix Figure A.3 and Appendix Table A.3 show clear and strong evidence of a meaningful increase in the likelihood of making any contributions to personal retirement plans. In contrast to the results on retirement savings, we do not find any statistically significant evidence of responses in other savings vehicles, although the point estimate on total non-retirement savings at this second critical age is not small. Combining all of our measures, the point estimate for total savings indicates that on average those in the treatment group experience increases in savings in 2016 amounting to \$1,510 dollars, driven by a just-under \$900 increase in total retirement savings.

Finally, in columns (3) and (5) of Table 4, we report RD estimates for calendar years 2017 and 2018, which are not critical years. During these years, individuals in our analysis sample are 63 and 64 years old. The majority of those retiring through the VERP scheme have already done so. Our RD estimates reported in the table show how responses in general have mostly dissipated during this time frame. There is no clear pattern in the point estimates, most of which are statistically insignificant, although the estimate on total savings at age 63 is large and statistically significant, driven by an imprecisely-estimated increase in non-retirement savings. We note that these point estimates tend to shrink with additional winsorization (see Appendix Table A.6).

6.3 Cumulative Responses Over the Entire Period

Thus far, our estimates have traced out the effects of the reform over time. To tie all the results together, we now estimate the cumulative effects over the entire period, from 2011 to 2018, as households age from 57 to 64. We focus on cumulative earnings and cumulative total savings, decomposed into retirement and non-retirement savings.

Table 5 reports the results. We find an increase in cumulative earnings over the entire time horizon that amounts to \$18,980, and we find a corresponding increase in cumulative total savings of \$5,242, which is driven primarily by savings in retirement accounts. We present graphical evidence and highlight the sensitivity of the results to bandwidth selection in Figure 8. While the evidence on earnings in panel (a) is rather clear, the evidence on total savings in panel (b) is less clear. The leading estimate is statistically significant, but estimates at most other bandwidths are insignificant. Panels (c) and (d) show results on retirement and non-retirement savings, which make up the total savings measure. We see that the evidence for cumulative retirement savings is much more clear and robust, whereas the estimates for non-retirement savings fluctuate considerably and are consistently statistically insignificant. Overall, we view these cumulative results as generally confirming the major takeaway from our main year-by-year estimates: the reform increased earnings and retirement savings.

Before moving on to investigate mechanisms, we first conduct a series of robustness checks, sensitivity analyses, and placebo exercises to further establish the validity of our main results. The upshot of these analyses is that our estimates are robust to standard RD specification checks, while several placebo tests provide reassuring evidence that our RD estimates indeed capture the causal effects of the policy reform.

6.4 Robustness Checks and Placebo Exercises

We probe the robustness of our results along several dimensions, focusing on our main outcomes for simplicity. First, we assess the sensitivity of our results to the choice of bandwidth by plotting RD estimates for a wide range of bandwidths, from 28 days to 180 days. Appendix Figures A.4, A.5, and A.6 show results for the anticipation period, the first critical age, and the second critical age, respectively. Overall, our results do not appear sensitive to the choice of bandwidth, though smaller bandwidths tend to increase standard errors. Perhaps the most sensitive result is that on total savings during the first critical year; estimates using smaller bandwidths are statistically indistinguishable from zero, whereas estimates using larger bandwidths are positive, increase in magnitude, and become highly statistically significant.

Second, we assess the sensitivity of our results to specification choices, namely the inclusion of control variables and the use of triangular weights. Appendix Table A.4 shows estimates for the anticipation time period and each critical age. The point estimates for anticipatory responses are broadly similar to one another and none are statistically distinguishable from zero at the 5% level, although a few estimates are statistically significant at the 10% level in the alternative specifications. Results are similar for the critical ages as well. Again the most sensitive results are for total savings during the first critical year, with the alternative specifications yielding larger estimates.

Third, to attempt to gain more precision, we follow Chetty et al. (2014) and winsorize our outcomes more aggressively, at the 10th and 90th percentiles. Appendix Table A.6 shows the results. While the point estimates are generally similar to our leading estimates, we do experience some gains in precision. For example, the point estimate for total savings during the first critical year is very similar to the leading estimate but the standard error is much smaller.

We additionally conduct three placebo exercises. First, we estimate our RD over a placebo time period. We test for discontinuous jumps in outcomes during the pre-announcement period from 2008 to 2010. There should be no discontinuities in outcomes due to the reform during this period, as the policy had not yet been announced. Indeed, Appendix Table A.7 shows no statistically significant effects on any of the outcomes analyzed.

Second, we estimate our RD using placebo cutoffs around the true cutoff date. Appendix Figures A.7 and A.8 show how our RD estimates for key outcome variables during each critical year tend to shrink and become statistically insignificant as we use cutoffs further away from the true cutoff. We note that since we consistently use a bandwidth equal to 74 days on either side of the cutoff, the RD estimates corresponding to placebo cutoffs more than 74 days away from the true cutoff provide placebo estimates as proposed by Imbens and Lemieux (2008), since these estimates do not come from underlying data that contain a known discontinuity.

Finally, we replicate our entire analysis using placebo January 1 birthdate cutoffs for earlier birth cohorts who were not impacted by the reform. Specifically, we implement our RD design first as if the cutoff was January 1, 1951, and then again as if the cutoff was January 1, 1952, testing for discontinuities in outcomes during the years these individuals reach their critical retirement ages of 60 and 62.[19] Appendix Table A.8 reports the results; we find no evidence that being born just after these placebo January 1 cutoff dates impacts key outcomes at age 60 or 62.

7 Mechanisms

Taken together, our main results may point to inertial behavior as an underlying channel. We find the clearest evidence of savings responses to the increase in eligibility ages when the reform directly induces extended employment and within retirement savings accounts. To explore mechanisms and assess the extent to which inertia might be driving the results, we first investigate the anticipation time period, and then we unpack the increases in contributions to retirement savings accounts during the two critical years.

7.1 Investigating the Anticipation Time Period

While we lack the statistical precision to rule out anticipatory savings responses predicted by our benchmark model, the sign of the point estimate for total savings at face value might suggest a disconnect between our prediction and what is borne out in the data. Here we investigate two natural explanations that could potentially be underlying our estimates. First, one might consider a lack of awareness; if those impacted by the reform are not aware of the changes to their pension eligibility ages until they reach age 60, then they would not decrease savings. We consider this unlikely, because the reform was well-publicized and a matter of political discourse.[20] For some reference, Appendix Figure A.9 plots a Google

[19] We do not use the January 1, 1953 birthdate as a placebo since a change in unemployment insurance policy for older individuals differentially impacted those born in 1953 compared to 1952 (OECD 2015).

[20] The later phases of the reform impact essentially all Danes younger than those that form our control group, and the reform is regarded as an initial push towards the gradual elimination of the VERP program. Later phases of the reform make the scheme less financially attractive, and so in 2012 individuals could opt out and withdraw from VERP. While likely a more attractive option for those younger than our sample, we nonetheless investigate whether the reform impacted VERP participation at the birthdate cutoff we study. Appendix Table A.9 reports results from estimating our RD on the likelihood of making participatory contributions to VERP and shows no evidence of responses along this potential margin.

search intensity index for "efterløn," which is the Danish word for the VERP program, and shows large spikes in searches throughout the anticipation time period.

Second, one might consider the inability to respond as a factor underlying our estimates. If "hand-to-mouth" or "wealthy hand-to-mouth" (Kaplan and Violante 2014, Kaplan, Violante and Weidner 2014) behavior is prevalent and individuals have little liquid assets, then it could be that they had little scope to reduce savings in anticipation. To investigate this, we study a subsample of individuals who had been using personal retirement plans before the announcement of the reform. These individuals have a natural way to respond—by adjusting their voluntary contributions to personal retirement plans—and also have higher bank account balances on average and may be more financially sophisticated. We find no evidence of anticipatory savings responses for this subsample, although as is the case in our main analysis, we lack the statistical precision to rule out meaningful responses (see Appendix Table A.10 for details).

7.2 Unpacking the Increased Savings in Personal Retirement Accounts

We next investigate the increase in contributions to personal retirement plans at the critical ages. We study response heterogeneity by pre-announcement usage of these accounts. The goal is to assess whether the policy increases contributions for those using the accounts less regularly, or whether the average effect is mostly the result of continued contributions by those already using the accounts. To this end, we split the estimating sample into two groups: frequent contributors to personal plans (who contributed in either 2 or 3 years between 2008 and 2010) and infrequent contributors (who contributed in either 0 or 1 year between 2008 and 2010). We then estimate our RD on contributions to personal plans in each critical year separately for each group.

We report results in Table 6. The first two rows correspond to the first critical age. Consistent with the continuation of previous savings behaviors, we find that the average response documented earlier is driven entirely by frequent contributors. In response to the reform, these individuals increase contributions. In contrast, there is no evidence of a change in behavior for the infrequent contributors. The next two rows correspond to the second critical age. A similar pattern emerges, although only the estimate on the indicator for contributing for frequent contributors is statistically significant. Overall, this exercise suggests that the policy results in continued contributions during periods of policy-induced extended employment for those who had been contributing before the announcement of the reform, whereas there is no evidence the reform spurs infrequent contributors to save more in personal retirement plans.

7.3 Unpacking the Increased Savings in Employer Retirement Accounts

We next examine the increase in contributions to employer-sponsored retirement plans. The literature on retirement savings has shown firm policies such as firm default contribution rates to strongly influence wealth accumulation within retirement accounts (e.g., Madrian and Shea 2001, Beshears et al. 2009). This is especially true in Denmark (Fadlon, Laird and Nielsen 2016), where there is additional evidence that individuals save passively and that quasi-mandatory employer retirement plans can play a key role in driving overall wealth accumulation (Chetty et al. 2014). Recall that many workers are covered by collective bargaining agreements between unions and employer associations that typically stipulate minimum default contribution rates, and firms outside of these agreements often set their own default contribution rates.

In the light of these institutional practices and the influential literature on firm savings policies, our findings of large increases in savings through employer retirement plans in response to the reform inspires a natural question: to what extent do employers mediate savings responses to national reforms of social security systems? We exploit our linked employer-employee data to conduct two exercises that directly investigate this question.

Each exercise aims to compare the reform-induced savings behaviors within employer accounts that are borne out in the data to behaviors we would expect based on firm default contribution rate policies. While we do not have information on firm default rates, we are able to proxy for them. We use our population-wide data to identify employees who work at the same firms, and we proxy for firm default contribution rates using the median contribution rate at each firm. Our measure is based on data from 2010, the year preceding the announcement of the reform, to avoid defining firm characteristics of an individual based on their endogenous choice of workplace after the announcement of the reform.[21] Appendix Figure A.10 plots the distribution of our proxy for firm-specific default contribution rates, for our analysis sample. Most of our sample worked for a firm in 2010 for which we infer a default contribution rate of either 10 or 11 percent.

Graphical Anlaysis. First, we conduct a graphical analysis that compares the distribu-

[21] Our approach broadly follows Chetty et al. (2014) and Fadlon, Laird and Nielsen (2016). We construct firms by assigning all individuals over 18 years old to their workplaces. We compute contribution rates by dividing contributions to employer plans by earnings. We infer the default contribution rate of a firm as the median contribution rate among workers at the firm. Our sample decreases slightly due to our inability to define workplaces in 2010 for the roughly 6% of individuals who did not have positive earnings that year.

tions of deviations from employer default contribution rates, for our treatment and control group, before and after the reform. Figure 9 depicts the results. Consider graph (a), which corresponds to calendar year 2010, before the reform is announced, when our analysis sample is 56 years old, and when we define our proxy measure for the default rates. The large spikes around zero indicate that a large share of individuals contributed right around the rate that we have inferred as the default contribution rate. The mass further away from zero can reflect real adjustments away from the true default rate but also measurement error in our proxy.[22] The fact that the distributions for the treatment and control group lie on top of one another though indicates that the propensity to deviate from the default contribution rate, or our ability to approximate the true firm default contribution rate, did not differ by treatment group.

Graph (b) plots the distributions during 2012, after the reform is announced and when our sample is 58 years old. The graph shows no evidence that the behavior of the treatment and control group have diverged. Graph (c) then plots the distributions during 2014, the first critical year. The mass around zero has decreased more for the control group than for the treatment group, with a corresponding rise in mass around negative 10 and 11 percent (the more common default rates), consistent with the control group beginning to retire and ceasing to contribute. In contrast, the mass of the treatment group remains higher around zero, suggesting that they are more likely to continue contributing right around the default rate. The pattern continues in graph (d), for the second critical year. This graphical analysis thus points to continued contributions at employer default rates during extended employment.

Regression Analysis. Second, we conduct a regression-based analysis to attempt to quantify the fraction of the savings responses within employer retirement accounts that could be explained by firm default contribution rates. Specifically, we define a new outcome variable, predicted contributions, as earnings multiplied by the 2010 (pre-announcement) firm default contribution rate, and we estimate our RD using this outcome. The RD estimate for predicted contributions captures the change in contributions to employer-sponsored plans that would arise if individuals responded to the reform by continuing to work and contribute at the default rate. We then compare the discontinuity in predicted contributions with the discontinuity in actual contributions.

We report these results in Table 7. Column (1) reports the estimate for the impact of

[22]For instance, it could be that the median worker contributes more than the default rate, or it could be that some workers within a firm are covered by different collective bargaining agreements and thus could face different default rates.

the policy on actual contributions in 2014, but for the subsample of individuals for whom we can define firm default contribution rates in 2010. The subsample is 94% of our main sample, and the $852 point estimate is similar to our baseline estimate. Column (2) reports the estimate for the impact of the policy on predicted contributions in 2014, which is $606. Taking these RD estimates at face value, the results indicate that in 2014, roughly $\frac{606}{852} = 71\%$ (s.e. 12.4) of the increase in contributions to employer-sponsored retirement accounts can be explained by contributions at firm default rates. Similarly, in 2016, the discontinuity in predicted contributions amounts to $549, whereas the discontinuity in actual contributions is $771, and thus firm default contribution rates can explain approximately 71% (s.e. 13.5) of the actual response during the second critical year. In comparing these estimates in this way, we provide a rough quantitative gauge on the role of default contribution rates and limited flexibility in employer retirement accounts in explaining our estimates. This regression exercise does not, however, explain the differences in actual contributions from predicted contributions; for instance, some of the difference could be due to positive savings responses to the reform, but some could be due to workers who previously contributed above their default rates continuing to do so. Overall though, we view our results as indicating that employer policies can play an important role in shaping how savings ultimately respond to national social security reform.

8 Conclusion

In this paper, we provide new evidence on how the provision of public pension benefits impacts savings. Our context is a reform in Denmark that increased public pension eligibility ages, and we use a regression discontinuity design and administrative data to document the causal effects of the reform. We find that increasing pension eligibility ages leads to large and meaningful increases in retirement savings. Specifically, we find increases in contributions to both personal and employer-sponsored retirement accounts during the periods in which individuals continue to work in order to retire at the new eligibility ages. We then investigate mechanisms and argue that inertia—the continuation of previous savings behaviors—is a leading explanatory channel. Note though that this form of inertial behavior does not necessarily imply that individuals fail to optimize. The patterns we document could be consistent with rational people optimizing in the face of frictions. For instance, it could be that the gains from re-optimizing savings over a relatively short time period do not outweigh adjustment costs associated with changing savings decisions.

Altogether, our findings have implications for social security policy. We find that increasing social security eligibility ages leads to longer working lives and more retirement savings set aside for shorter retirement time horizons. From a policy standpoint, the goal of raising retirement ages is to encourage more years of work; our results on labor supply thus provide evidence that this type of policy can work as intended. Moreover, our main results on savings further highlight how this type of reform can also be a tool used by policy makers to improve the retirement income security of older persons; in our context, with roughly 90% of the sample contributing to quasi-mandatory employer retirement savings plans, the reform induces more work and more savings in these accounts. More generally, our analysis underscores the importance of accounting for interactions between employer policies and public policies. Our findings emphasize that when individuals make their labor supply decisions, they are often choosing a compensation bundle, which can include employer-sponsored retirement plans, employer-provided health insurance, or other benefits. Consequently, and as we have shown in our context, employer policies can ultimately mediate individual responses to national reforms. Policy makers should consider this interplay when designing and predicting the effects of public policies that intend to impact labor supply.

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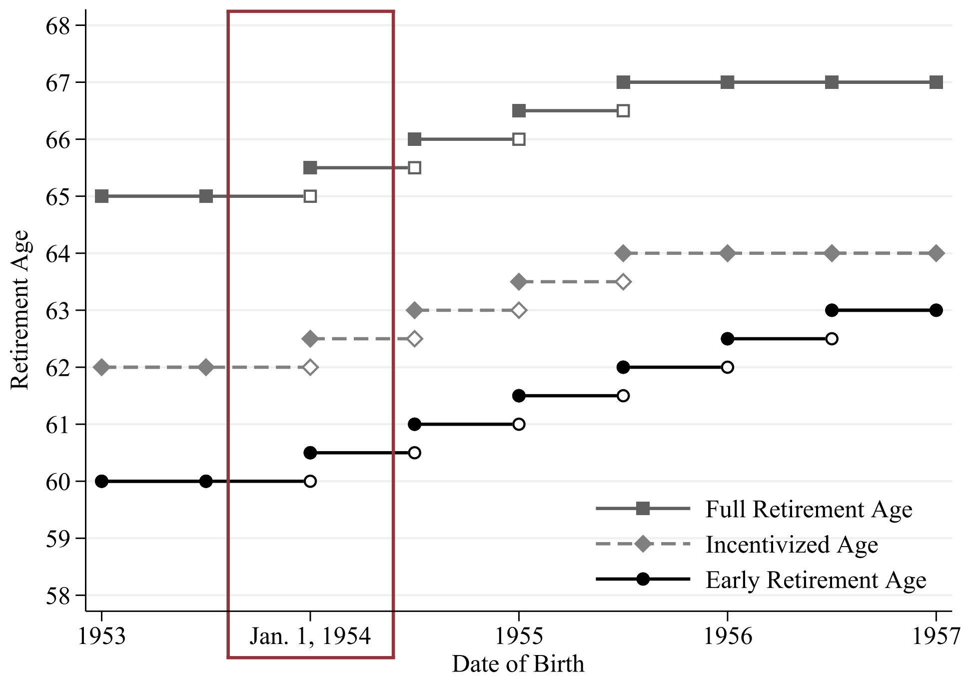
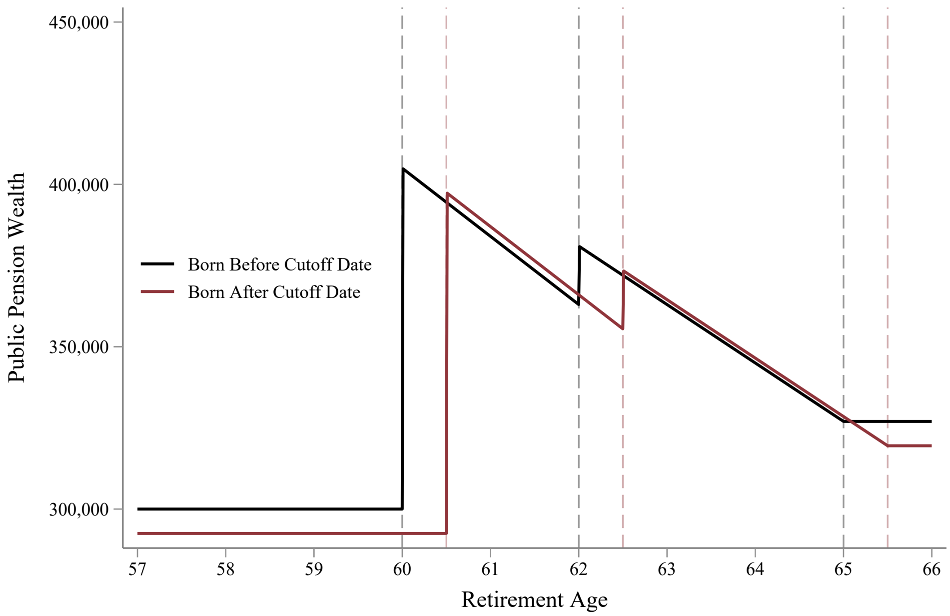
Figure 1: Pension Eligibility Ages by Birthdate



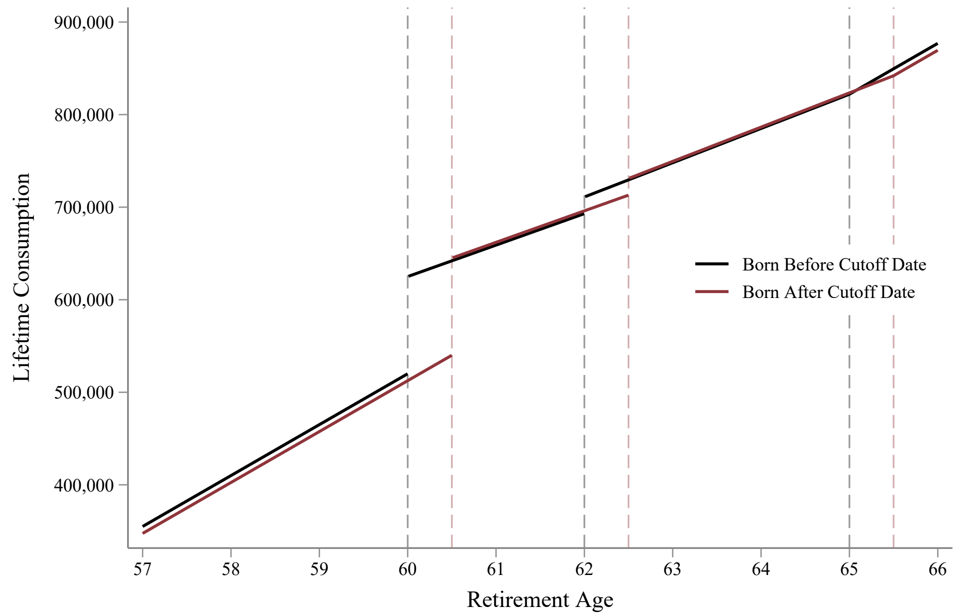
Notes: This figure graphically depicts the increases in pension eligibility ages due to the 2011 reform. Birth cohorts born before January 1, 1954 were unaffected by the reform. For these individuals, the key eligibility ages remained constant at 60, 62, and 65. Individuals born between January 1, 1954 and July 1, 1954 experience a six-month increase in each of the eligibility ages. Later phases of the reform introduced additional increases of eligibility ages as illustrated. The maroon rectangle highlights the birth cohorts relevant for our study.

Figure 2: Public Pension Wealth by Retirement Age



Notes: This figure plots public pension wealth against retirement age. The black line is for a worker born before the cutoff date. The maroon line is for a worker born after the cutoff date. For illustrative purposes, we abstract from discounting and plot benefit amounts for a worker who is married, who lives until age 85, and who has $250,000 in private retirement savings accounts at age 60. The y-intercept corresponds to standard OAP benefits. The first spike in pension wealth is due to the transition rule. Individuals retiring before the ERA are not eligible to claim into VERP and thus forfeit five years of early retirement benefits. The second spike in pension wealth is due to the two-year rule. Retiring two years after the ERA eliminates the means-testing of early retirement benefits against private retirement savings accounts and produces higher benefits over the remaining three years of VERP. The negative slopes after the spikes are due to the lack of actuarial adjustments. Pension wealth for those who retire after the FRA is greater than OAP wealth due to bonus payments for continuing to work after satisfying the two-year rule (see Appendix C).

Figure 3: Lifetime Budget Constraints

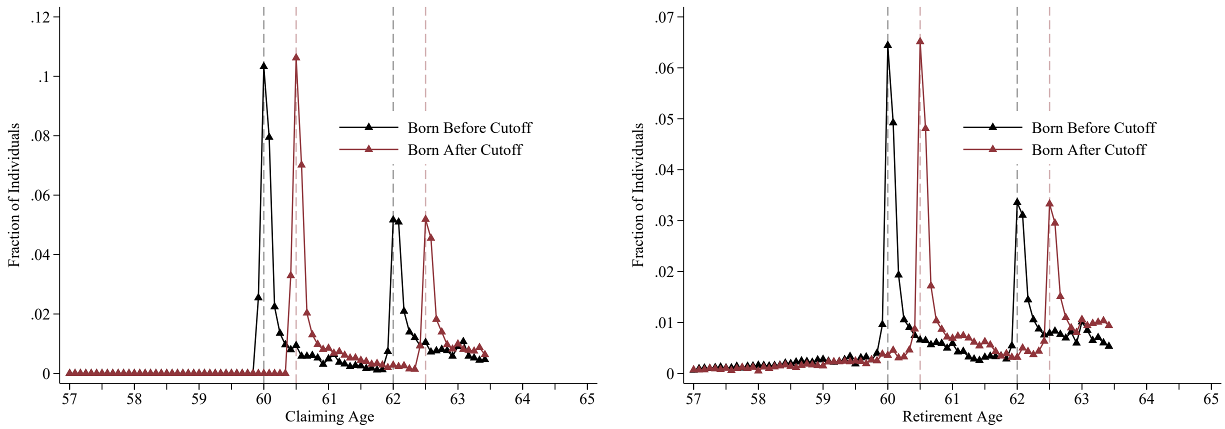


Notes: This figure plots lifetime consumption against retirement age for the same workers as in Figure 2. The black lines are for a worker born before the cutoff date. The maroon lines are for a worker born after the cutoff date. Lifetime consumption is the sum of public pension wealth and lifetime earnings. For illustrative purposes, annual earnings are assumed to be $55,000 and lifetime earnings are earnings after age 57, the age of our sample when the reform is announced. The spikes in pension wealth translate to discontinuities in the lifetime budget constraint. The key change from the reform is the shift in the location of the discontinuities in the budget constraint.

Figure 4: Empirical Distributions of VERP Benefit Claiming Ages and Retirement Ages

(a) VERP Benefit Claiming

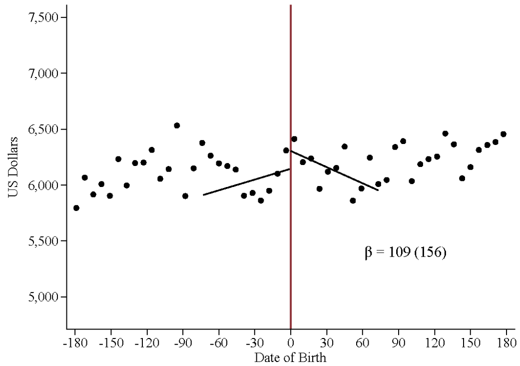
(b) Retirement



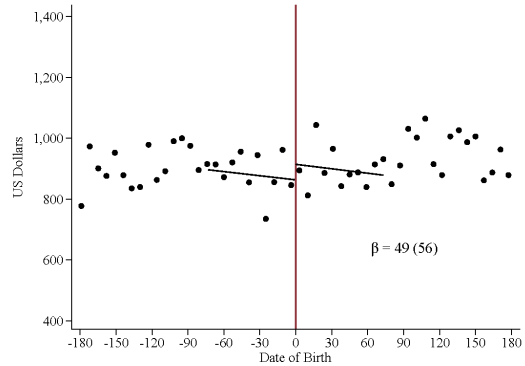
Notes: This figure plots the empirical distributions of claiming and retirement ages. Monthly claiming age is defined as the age of the individual upon initial receipt of VERP benefits. Monthly retirement age is defined as the age of the individual in the last month during which earnings are positive, before permanently falling to zero. Graph (a) shows how those born before the January 1, 1954 birthdate cutoff tend to either claim right around 60 or 62, while those born after the birthdate cutoff, who are impacted by the reform, tend to claim right around $60\frac{1}{2}$ or $62\frac{1}{2}$. Graph (b) shows how those born before the January 1, 1954 birthdate cutoff tend to retire right around 60 or 62, while those born after the birthdate cutoff, who are impacted by the reform, tend to retire right around $60\frac{1}{2}$ or $62\frac{1}{2}$.

Figure 5: Anticipatory Responses at Ages 57 to 59

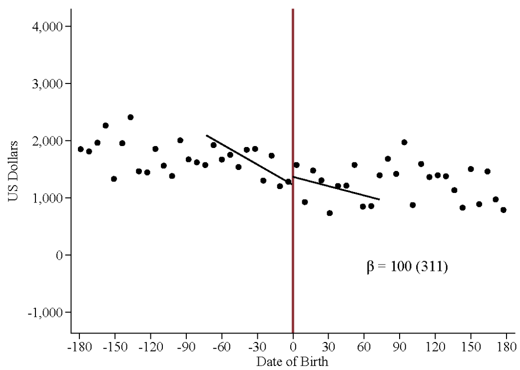
(a) Employer Retirement Plans



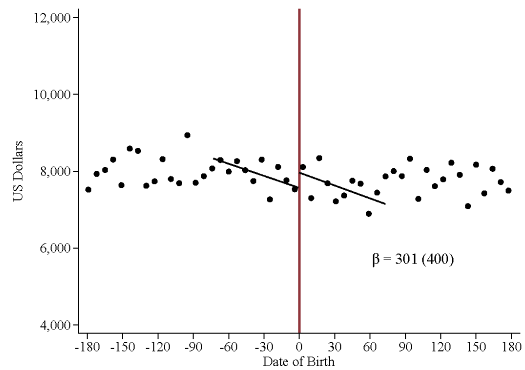
(b) Personal Retirement Plans



(c) Non-Retirement Savings



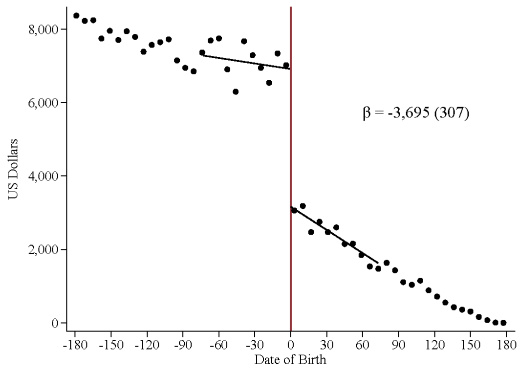
(d) Total Savings



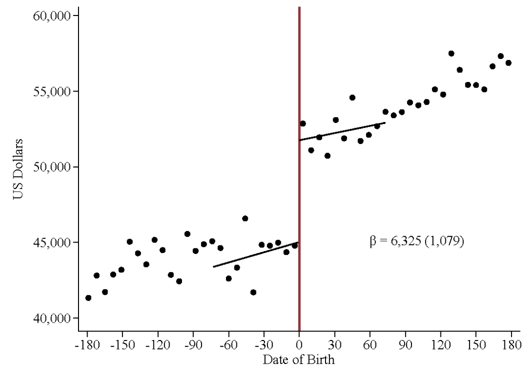
Notes: This figure illustrates the effect of the reform on key outcome variables over the anticipation time period. Each RD graph (a)–(d) corresponds to a separate outcome variable averaged over the three-year anticipation period, from 2011 to 2013. The graphs plot average outcomes in one-week date-of-birth bins. The maroon vertical lines designate the January 1, 1954 birthdate cutoff. The superimposed regression lines are based on the underlying unbinned data.

Figure 6: Responses at Age 60 – the First Critical Age

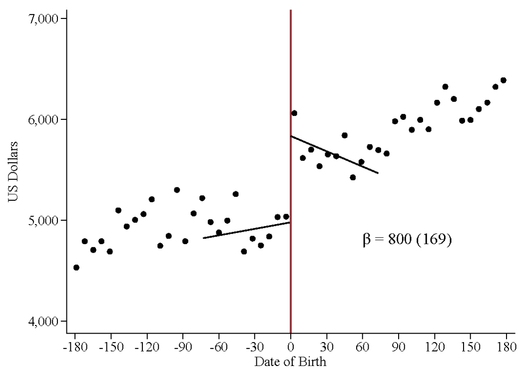
(a) VERP Benefits



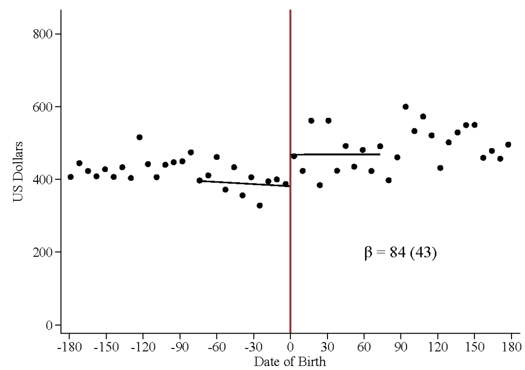
(b) Labor Market Earnings



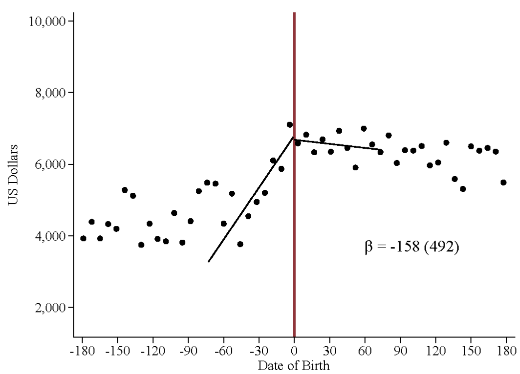
(c) Employer Retirement Plans



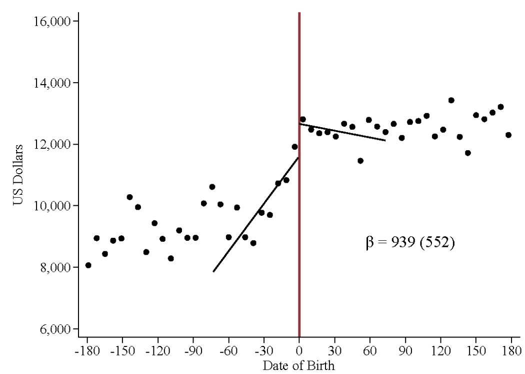
(d) Personal Retirement Plans



(e) Non-Retirement Savings



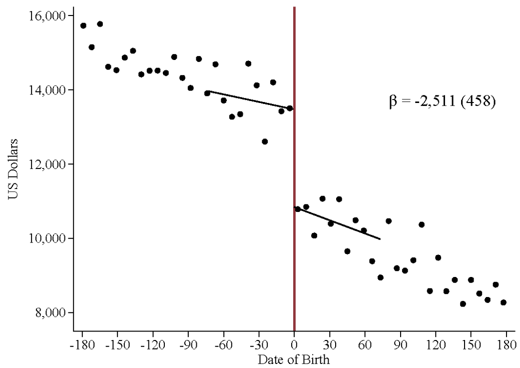
(f) Total Savings



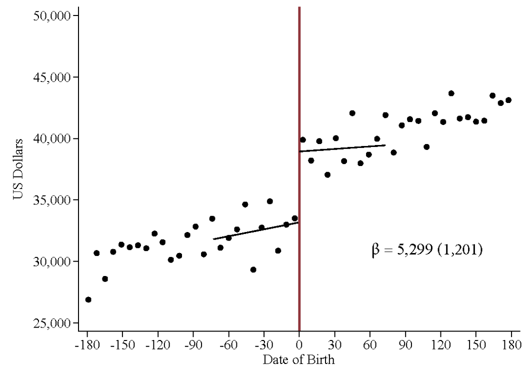
Notes: This figure illustrates the effect of the reform on key outcome variables during the first critical year, when individuals born at the cutoff date are age 60. Each RD graph (a)–(f) plots average outcomes during 2014 in one-week date-of-birth bins. The maroon vertical lines indicate the January 1, 1954 birthdate cutoff. The superimposed regression lines are based on the underlying unbinned data. The RD estimates reported in the figures correspond to those in Table 3, and come from estimating equation (6).

Figure 7: Responses at Age 62 – the Second Critical Age

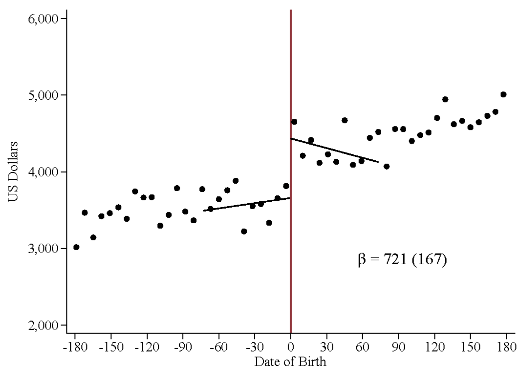
(a) VERP Benefits



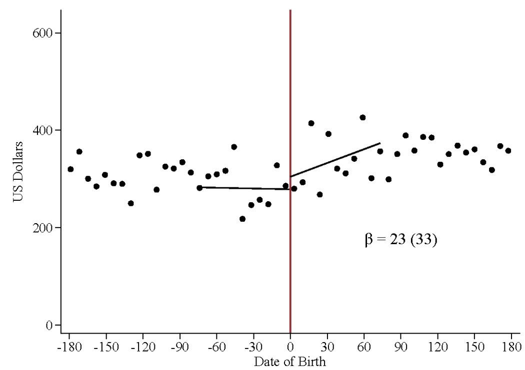
(b) Labor Market Earnings



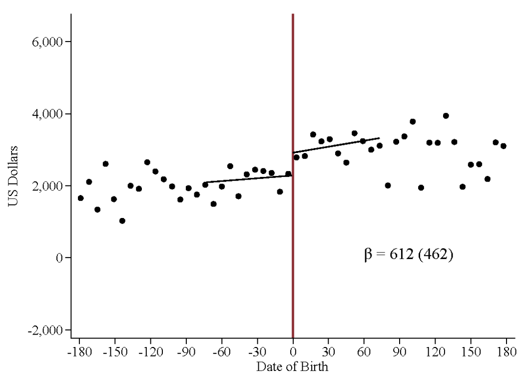
(c) Employer Retirement Plans



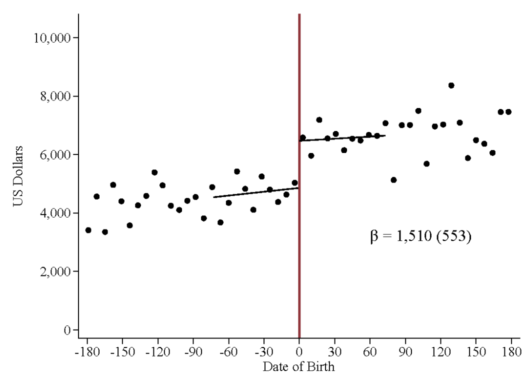
(d) Personal Retirement Plans



(e) Non-Retirement Savings



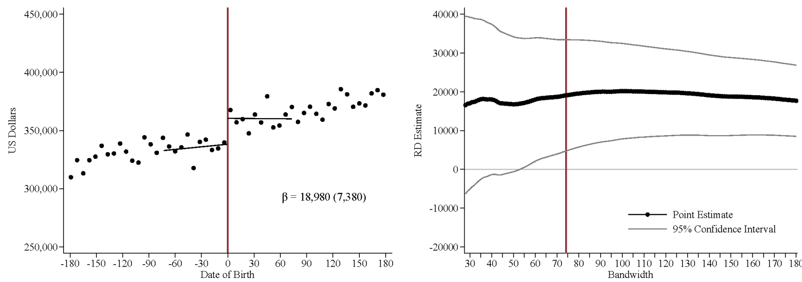
(f) Total Savings



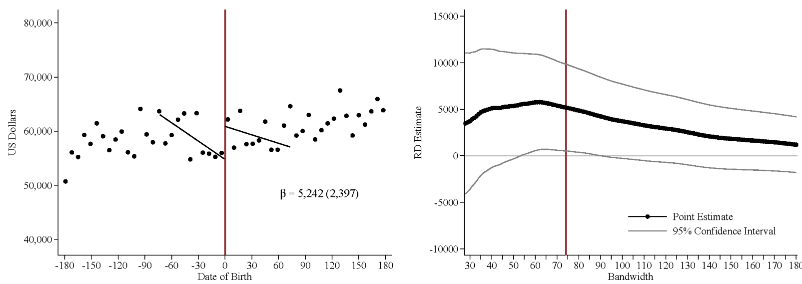
Notes: This figure illustrates the effect of the reform on key outcome variables during the second critical year, when individuals born at the cutoff date are age 62. Each RD graph (a)–(f) plots average outcomes during 2016 in one-week date-of-birth bins. The maroon vertical lines indicate the January 1, 1954 birthdate cutoff. The superimposed regression lines are based on the underlying unbinned data. The RD estimates reported in the figures correspond to those in Table 3, and come from estimating equation (6).

Figure 8: Cumulative Responses: RD Graphs and Bandwidth Sensitivity

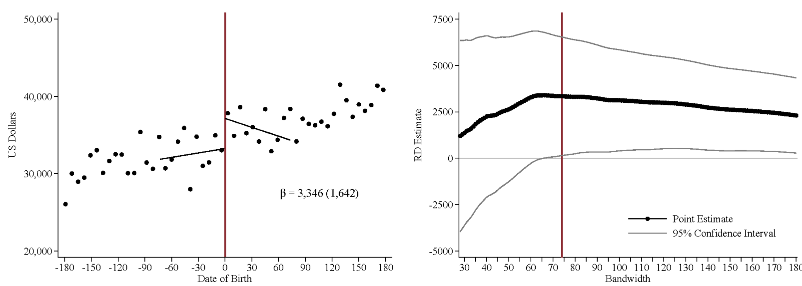
(a) Cumulative Earnings



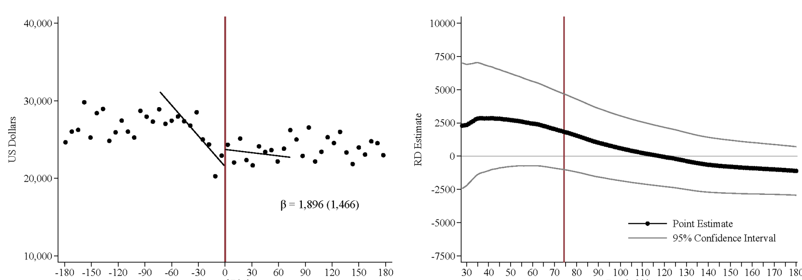
(b) Cumulative Total Savings



(c) Cumulative Retirement Savings

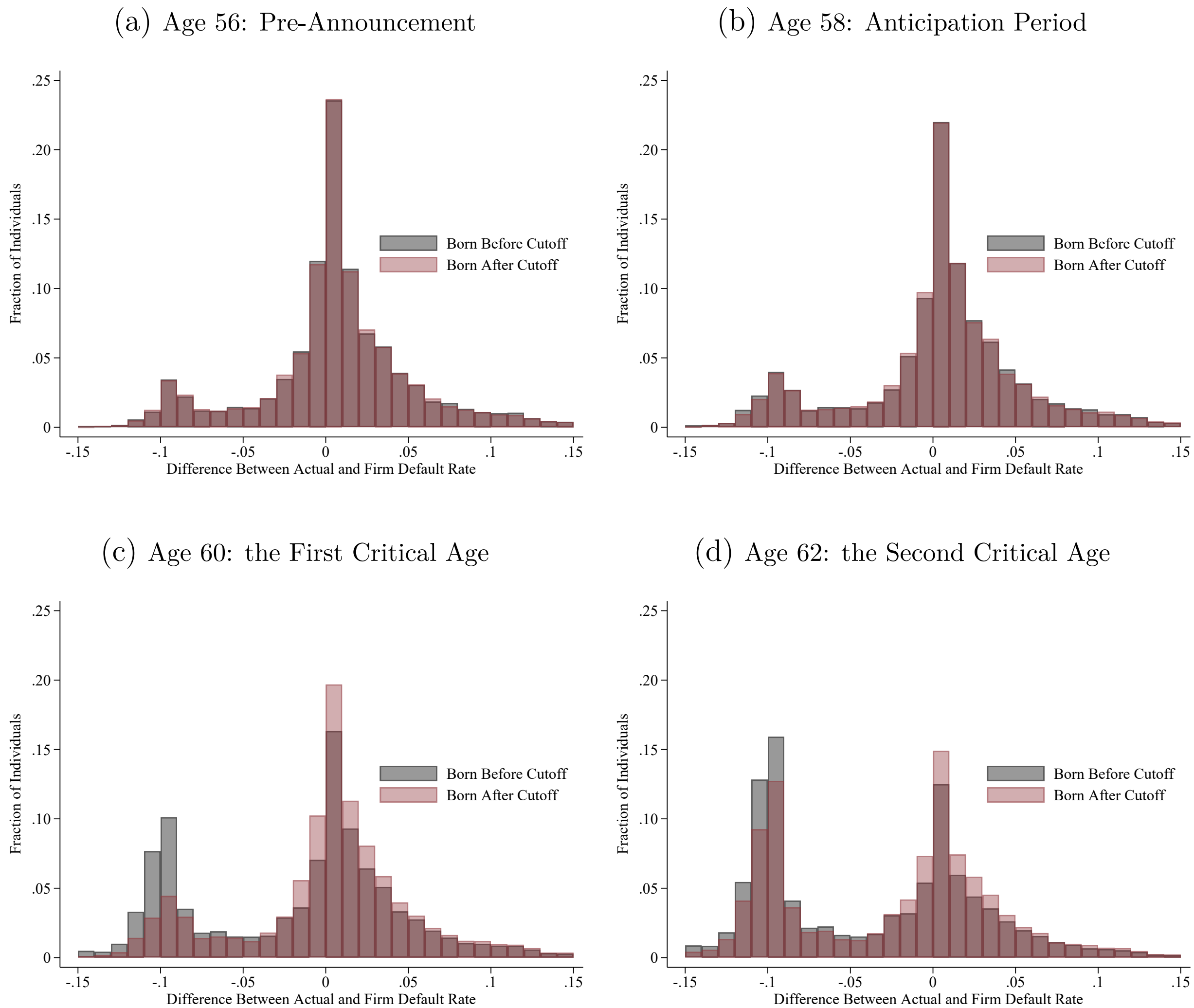


(d) Cumulative Non-Retirement Savings



Notes: This figure plots RD graphs (on the left-hand side) and bandwidth sensitivity graphs (on the right-hand side). Each panel corresponds to a different outcome. The RD graphs plot average outcomes in one-week date-of-birth bins. The maroon vertical lines designate the January 1, 1954 birthdate cutoff. The superimposed regression lines are based on the underlying unbinned data. The bandwidth sensitivity graphs plot RD estimates and confidence intervals as we vary the bandwidth from 28 days to 180 days; the maroon vertical lines in these graphs correspond to the baseline bandwidth of 74 days.

Figure 9: Differences Between Actual and Firm Default Contribution Rates



Notes: This figure illustrates how actual contribution rates to employer-sponsored retirement plans deviate from firm default contribution rates, over time, for both the treatment and control groups. Firm default contribution rates are inferred as the median contribution rate among individuals working at the same firm, as described in Section 7.3. Each graph (a)-(d) captures the distributions of deviations from firm default rates during a different year.

Table 1: Summary Statistics

	Analysis Sample		RD Sample		Unrestricted Sample	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)
A: Demographics						
Age	56.99	0.29	56.99	0.12	56.99	0.29
Male	0.46	0.50	0.46	0.50	0.47	0.50
Married	0.72	0.45	0.72	0.45	0.68	0.47
Treated	0.52	0.50	0.53	0.50	0.52	0.50
B: Labor Market Earnings						
Any Earnings	0.94	0.23	0.94	0.24	0.84	0.37
Earnings	59,264	26,521	58,955	26,549	52,332	32,522
C: Retirement Savings (Flow Variables)						
Any Contributions to Employer Plans	0.89	0.32	0.89	0.32	0.80	0.40
Contributions to Employer Plans	6,508	4,951	6,437	4,883	5,533	5,143
Any Contributions to Personal Plans	0.41	0.49	0.41	0.49	0.36	0.48
Contributions to Personal Plans	1,192	2,130	1,168	2,117	1,008	1,955
Any Distributions from Retirement Plans	0.03	0.17	0.03	0.17	0.05	0.22
Distributions from Retirement Plans	509	3,870	490	3,659	850	5,143
D: Non-Retirement Savings (Stock Variables)						
Bank Account Balances	26,505	46,790	26,480	46,111	24,694	64,587
Stock Market Account Balances	7,240	44,006	6,982	43,132	7,974	83,264
Property Wealth	152,541	189,923	151,969	193,151	144,187	224,791
Liabilities	102,252	138,501	101,837	136,739	103,012	208,931
Number of Individuals	40,042		15,789		57,178	

Notes: This table reports means and standard deviations of key variables for relevant samples, in 2010, the year before the announcement of the reform. We deflate all monetary values to 2010 levels and convert Danish kroner to U.S. dollars. Columns (1) and (2) correspond to the analysis sample, which consists of a balanced panel of individuals born within six months of the January 1, 1954 birthdate cutoff who were making participatory contributions to the early retirement scheme and who were not self-employed. Columns (3) and (4) correspond to the RD estimation sample, which consists of the subset of individuals from the analysis sample who were born within 74 days of the birthdate cutoff. Columns (5) and (6) correspond to an unrestricted sample, where we do not impose the restriction that individuals were making participatory contributions to the early retirement scheme.

Table 2: Anticipatory Responses

	Ages 57–59	
	RD Estimate (1)	Mean (2)
A: Labor Supply		
Average Earnings	648	55,804
	(873)	
B: Retirement Savings		
Average Contributions to Employer Plans	109	6,088
	(156)	
Average Contributions to Personal Plans	49	884
	(56)	
Average Contributions to Roth-Style Plans	1	25
	(3)	
Average Distributions from Retirement Plans	-43	675
	(145)	
C: Non-Retirement Savings		
Average Change in Bank Accounts	-60	1,560
	(186)	
Average Change in Stock Market Accounts	37	966
	(94)	
Average Change in Property Wealth	-141	-3,555
	(197)	
Average Change in Liabilities	-263	-2,627
	(170)	
D: Total Savings		
Average Total Savings	301	7,919
	(400)	
Average Total Retirement Savings	201	6,322
	(232)	
Average Total Non-Retirement Savings	100	1,597
	(311)	
Obs.	15,789	

Notes: This table reports RD estimates for the impact of the reform on outcomes over the anticipation period. Outcome variables are averaged over 2011 to 2013. Panel A presents results on labor supply. Panel B presents results on retirement savings. Panel C presents results on non-retirement savings. Panel D presents results on total savings. The RD estimates come from estimating equation (6). The regressions use separate linear polynomials in the running variable on either side of the birthdate cutoff, employ triangular weights, and include as controls gender, marital status as of 2010, and indicators for region of residence as of 2010. Robust standard errors are in parentheses. $^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.1$

Table 3: Responses at Early Retirement Period Critical Ages

	Critical Age 60		Critical Age 62	
	RD Estimate (1)	Mean (2)	RD Estimate (3)	Mean (4)
A: Labor Supply				
VERP Benefits	-3,695***	7,080	-2,511***	13,679
	(307)		(458)	
Earnings	6,325***	44,359	5,299***	32,608
	(1,079)		(1,201)	
B: Retirement Savings				
Contributions to Employer Plans	800***	4,940	721***	3,609
	(169)		(167)	
Contributions to Personal Plans	84*	392	23	287
	(43)		(33)	
Contributions to Roth-Style Plans	-21	372	-39	535
	(42)		(52)	
Distributions from Retirement Plans	-233***	971	-194	1,901
	(77)		(143)	
C: Non-Retirement Savings				
Change in Bank Accounts	-105	1,757	391	763
	(412)		(410)	
Change in Stock Market Accounts	-191	1,873	57	336
	(184)		(75)	
Change in Property Wealth	-19	-528	-7	-652
	(19)		(24)	
Change in Liabilities	-157	-2,189	-171	-1,710
	(257)		(235)	
D: Total Savings				
Total Savings	939*	10,025	1,510***	4,689
	(552)		(553)	
Total Retirement Savings	1,097***	4,734	898***	2,531
	(215)		(258)	
Total Non-Retirement Savings	-158	5,291	612	2,157
	(492)		(462)	
Obs.	15,789		15,789	

Notes: This table reports RD estimates for the impact of the reform on outcomes during the early retirement period critical years. Column (1) displays results during 2014, when individuals born at the cutoff date are age 60. Column (3) displays results during 2016, when individuals born at the cutoff date are age 62. Panel A presents results on labor supply. Panel B presents results on retirement savings. Panel C presents results on non-retirement savings. Panel D presents results on total savings. The RD estimates come from estimating equation (6). The regressions use separate linear polynomials in the running variable on either side of the birthdate cutoff, employ triangular weights, and include as controls gender, marital status as of 2010, and indicators for region of residence as of 2010. Robust standard errors are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 4: Responses at Early Retirement Period Non-Critical Ages

	Age 61		Age 63		Age 64	
	RD Estimate (1)	Mean (2)	RD Estimate (3)	Mean (4)	RD Estimate (5)	Mean (6)
A. Labor Supply						
VERP Benefits	-437	8,377	-944*	16,922	-935*	16,859
	(423)		(513)		(513)	
Earnings	1,908	41,135	2,701**	26,998	804	24,080
	(1,217)		(1,189)		(1,166)	
B: Retirement Savings						
Contributions to Employer Plans	349**	4,569	281*	3,024	55	2,461
	(174)		(160)		(149)	
Contributions to Personal Plans	16	351	13	222	4	191
	(40)		(27)		(24)	
Contributions to Roth-Style Plans	-17	493	27	619	-63	1,021
	(48)		(57)		(83)	
Distributions from Retirement Plans	-98	1,421	-39	2,279	54	2,757
	(117)		(171)		(187)	
C: Non-Retirement Savings						
Change in Bank Accounts	-464	1,114	467	6	356	4,850
	(415)		(411)		(490)	
Change in Stock Market Accounts	80	1,714	-36	1,216	-80	-1,737
	(206)		(144)		(161)	
Change in Property Wealth	7	-957	-35	-1,321	-65	-1,045
	(37)		(50)		(40)	
Change in Liabilities	-267	-3,123	-407	3,269	-240	-3,340
	(284)		(276)		(251)	
D: Total Savings						
Total Savings	335	8,986	1,162**	4,756	393	6,325
	(603)		(584)		(642)	
Total Retirement Savings	446*	3,993	360	1,587	-59	917
	(250)		(271)		(283)	
Total Non-Retirement Savings	-111	4,993	803	3,170	451	5,408
	(502)		(490)		(558)	
Obs.	15,789		15,789		15,789	

Notes: This table reports RD estimates for the impact of the reform on outcomes during the early retirement period non-critical years. Column (1) displays results during 2015, when individuals born at the cutoff date are age 61. Column (3) displays results during 2017, when individuals born at the cutoff date are age 63. Column (5) displays results during 2018, when individuals born at the cutoff date are age 64. Panel A presents results on labor supply. Panel B presents results on retirement savings. Panel C presents results on non-retirement savings. Panel D presents results on total savings. The RD estimates come from estimating equation (6). The regressions use separate linear polynomials in the running variable on either side of the birthdate cutoff, employ triangular weights, and include as controls gender, marital status as of 2010, and indicators for region of residence as of 2010. Robust standard errors are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 5: Cumulative Responses

	Ages 57–64	
	RD Estimate (1)	Mean (2)
Cumulative Earnings	18,980**	336,592
	(7,380)	
Cumulative Total Savings	5,242**	58,538
	(2,397)	
Cumulative Total Retirement Savings	3,346**	32,728
	(1,642)	
Cumulative Total Non-Retirement Savings	1,896	25,810
	(1,466)	
Obs.	15,789	

Notes: This table reports RD estimates for the impact of the reform on cumulative outcomes over the entire analysis time period. Outcome variables totaled over 2011 to 2018. The RD estimates come from estimating equation (6). The regressions use separate linear polynomials in the running variable on either side of the birthdate cutoff, employ triangular weights, and include as controls gender, marital status as of 2010, and indicators for region of residence as of 2010. Robust standard errors are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 6: Contributions to Personal Retirement Plans by Previous Use

	Frequent Contributors		Infrequent Contributors	
	RD Estimate (1)	Mean (2)	RD Estimate (3)	Mean (4)
Contributions at Age 60	185**	799	6	103
	(89)		(29)	
Indicator for Contributing at Age 60	0.110***	0.28	0.003	0.04
	(0.025)		(0.009)	
Contributions at Age 62	61	538	-9	108
	(68)		(27)	
Indicator for Contributing at Age 62	0.067***	0.21	0.005	0.05
	(0.023)		(0.009)	
Obs.	6,596		9,193	

Notes: This table reports RD estimates for the impact of the reform on contributions to personal retirement plans during critical years 2014 and 2016, by previous use of the accounts. Panel A reports results for the subsample of individuals who made contributions to personal plans in either two or three of the years between 2008 and 2010. Panel B reports results for the subsample of individuals who made contributions in either 0 or 1 year between 2008 and 2010. The RD estimates come from estimating equation (6). The regressions use separate linear polynomials in the running variable on either side of the birthdate cutoff, employ triangular weights, and include as controls gender, marital status as of 2010, and indicators for region of residence as of 2010. Robust standard errors are in parentheses. $^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.1$

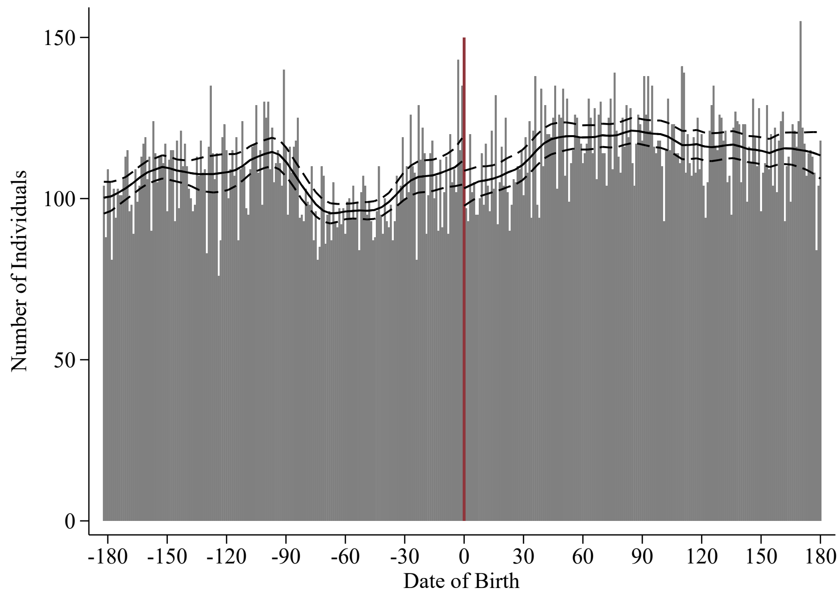
Table 7: Actual vs. Predicted Contributions to Employer Retirement Plans

	RD Estimates	
	Actual Contributions (1)	Predicted Contributions (2)
Contributions at Age 60	852***	606***
	(174)	(123)
Contributions at Age 62	771***	549***
	(174)	(132)
Obs.	14,869	14,869

Notes: This table reports RD estimates for the impact of the reform on actual contributions to employer-sponsored retirement plans as well as predicted contributions to employer-sponsored retirement plans, during both critical years 2014 and 2016. Predicted contributions are defined as current earnings multiplied by the 2010 inferred firm default contribution rate. Firm default contribution rates are inferred as the median contribution rate among individuals working at the same firm, as described in Section 7.3. The RD estimates come from estimating equation (6). The regressions use separate linear polynomials in the running variable on either side of the birthdate cutoff, employ triangular weights, and include as controls gender, marital status as of 2010, and indicators for region of residence as of 2010. Robust standard errors are in parentheses. $^{***}\,p < 0.01$, $^{**}\,p < 0.05$, $^{*}\,p < 0.1$

Appendix A Additional Figures and Tables

Figure A.1: Histogram of the Running Variable



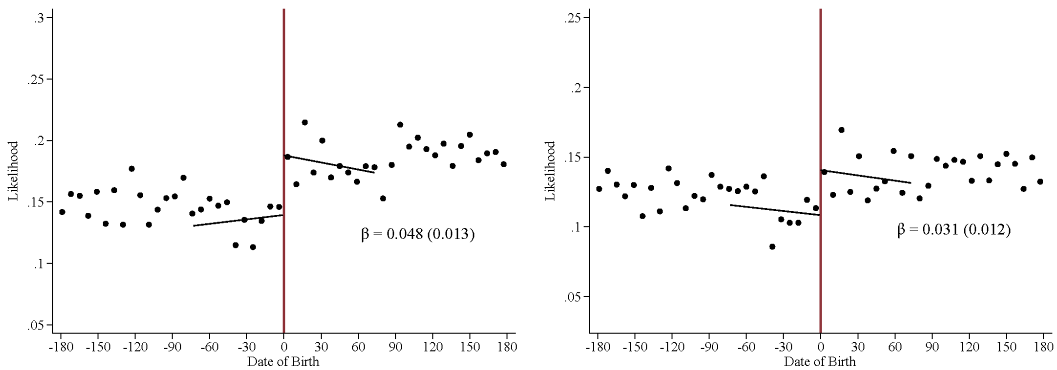
Notes: This figure depicts the density of the running variable, birthdate. The graph plots a histogram of the running variable for the entire analysis sample. Superimposed on top of the histogram are smoothed values and confidence intervals from local polynomial regressions of the number of individuals on birthdate. A formal density test as proposed by Cattaneo, Jansson and Ma (2019) using our baseline RD bandwidth of 74 days results in a p-value of 0.125.

Figure A.2: Anticipatory Savings Responses for Non-Retirement Savings Components

Notes: This figure illustrates the effect of the reform on key outcome variables over the anticipation time period. Each RD graph (a)–(d) corresponds to a separate outcome variable averaged over the three-year anticipation period, from 2011 to 2013. The graphs plot average outcomes in one-week date-of-birth bins. The maroon vertical lines designate the January 1, 1954 birthdate cutoff. The superimposed regression lines are based on the underlying unbinned data.

Figure A.3: Impact on Indicator Variables for Contributing to Personal Retirement Plans

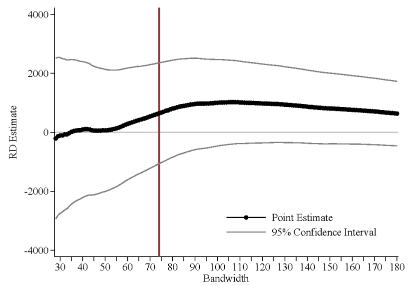
(a) Any Contributions at Age 60 (b) Any Contributions at Age 62



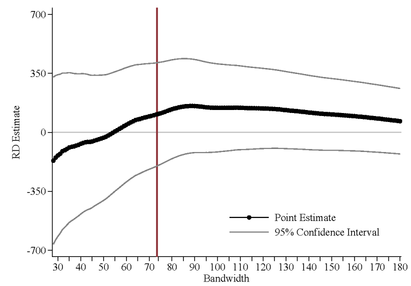
Notes: This figure illustrates the effect of the reform on indicators for contributing to personal retirement plans during the critical years. Graph (a) corresponds to 2014, the first critical year, when individuals born at the cutoff date are age 60. Graph (b) corresponds to 2016, the second critical year, when individuals born at the cutoff date are age 62. The graphs plot average outcomes in one-week date-of-birth bins. The maroon vertical lines designate the January 1, 1954 birthdate cutoff. The superimposed regression lines are based on the underlying unbinned data.

Figure A.4: Robustness: Bandwidth Selection for the Anticipation Period

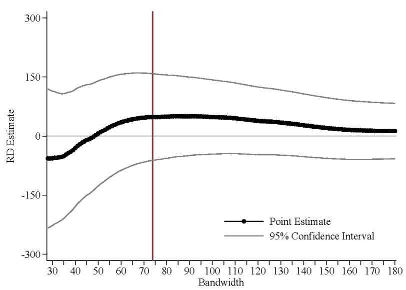
(a) Labor Market Earnings



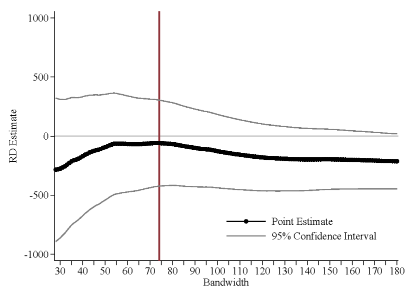
(b) Employer Retirement Plans



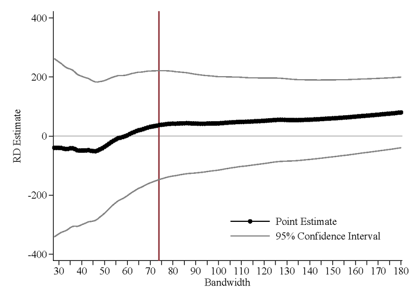
(c) Personal Retirement Plans



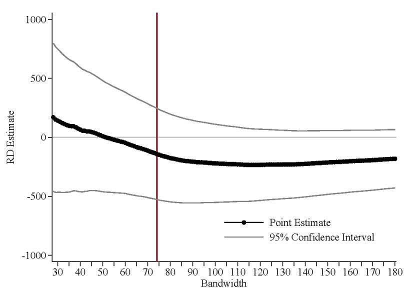
(d) Bank Accounts



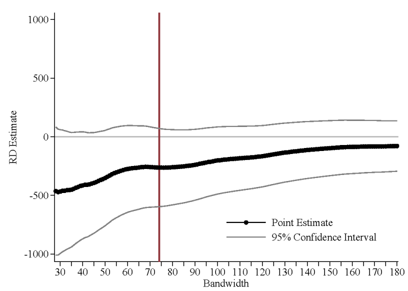
(e) Stock Market Accounts



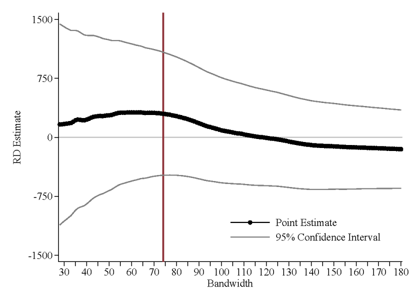
(f) Property Wealth



(g) Liabilities

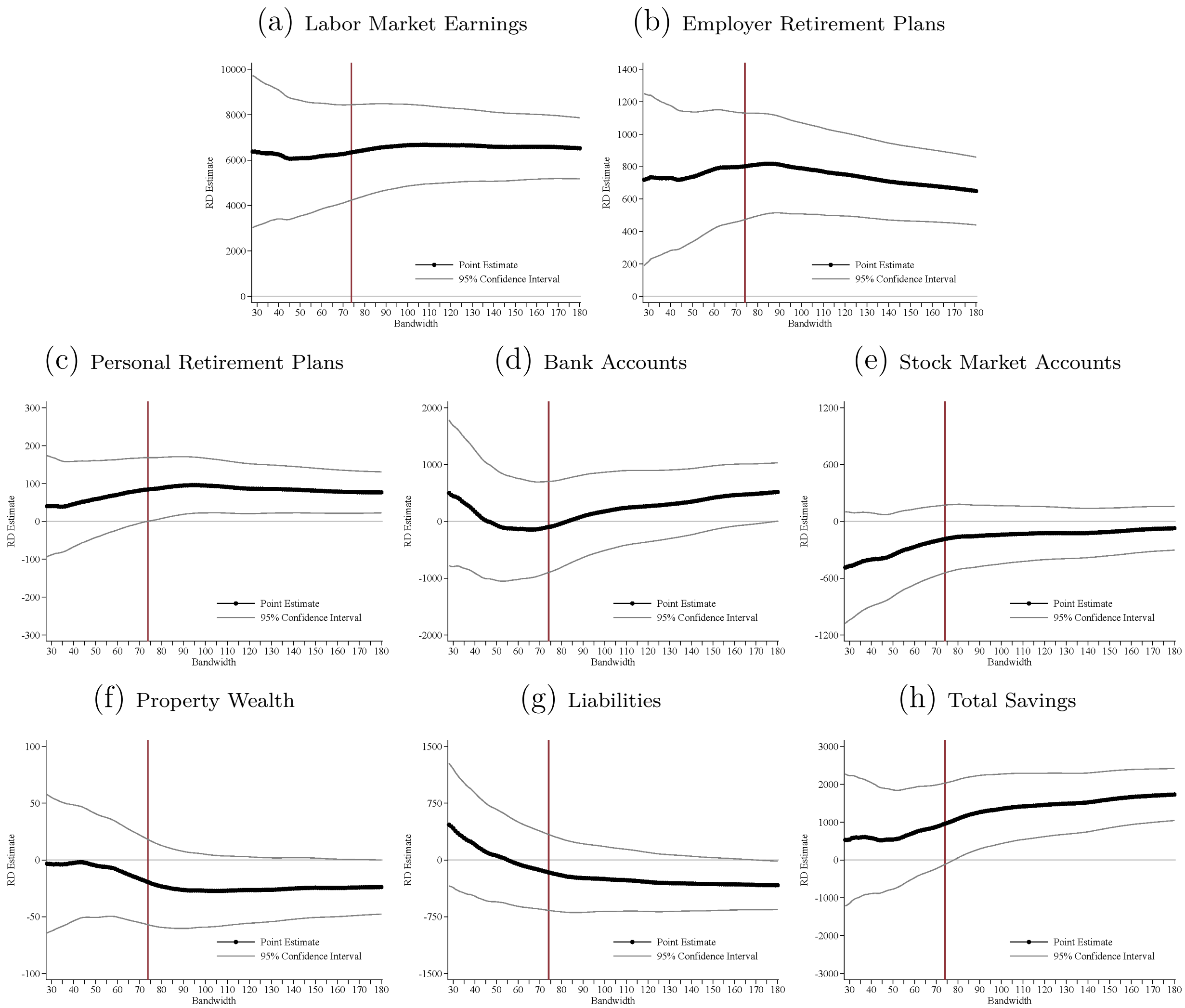


(h) Total Savings



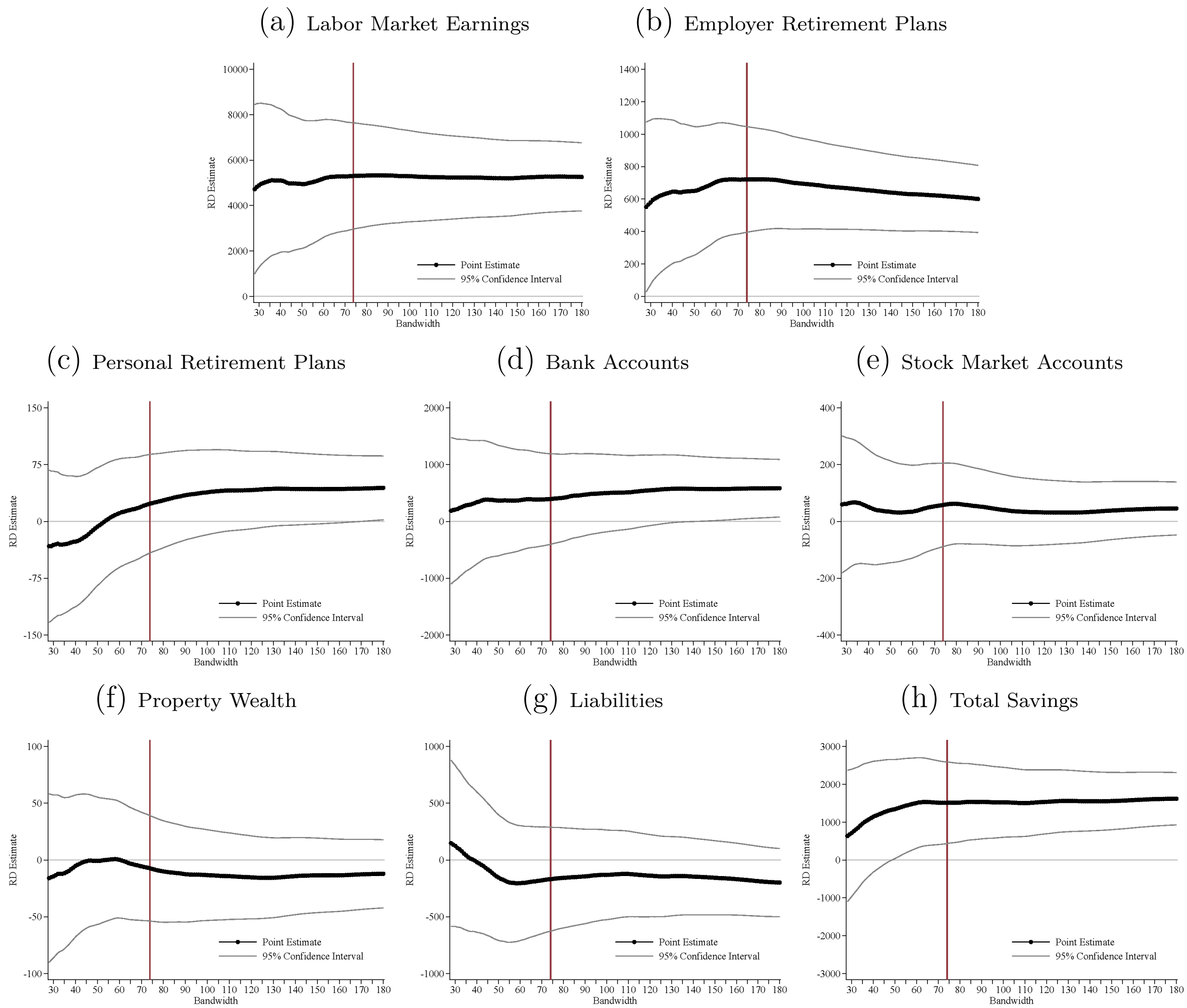
Notes: This figure illustrates how our RD estimates over the anticipation time period change with the bandwidth. Each graph (a)–(h) corresponds to a different key outcome variable and plots RD estimates and 95-percent confidence intervals as we vary the bandwidth from 28 days to 180 days.

Figure A.5: Robustness: Bandwidth Selection for the First Critical Age



Notes: This figure illustrates how our RD estimates during the first critical year change with the bandwidth. Each graph (a)–(h) corresponds to a different key outcome variable and plots RD estimates and 95-percent confidence intervals as we vary the bandwidth from 28 days to 180 days.

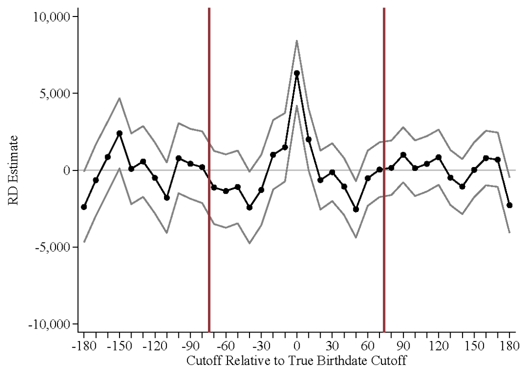
Figure A.6: Robustness: Bandwidth Selection for the Second Critical Age



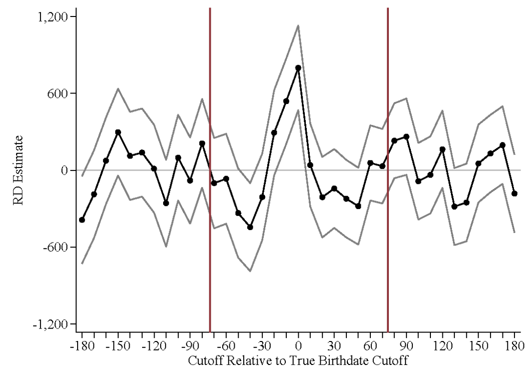
Notes: This figure illustrates how our RD estimates during the second critical year change with the bandwidth. Each graph (a)–(h) corresponds to a different key outcome variable and plots RD estimates and 95-percent confidence intervals as we vary the bandwidth from 28 days to 180 days.

Figure A.7: Placebo Exercise: Pseudo Birthdate Cutoffs at Age 60

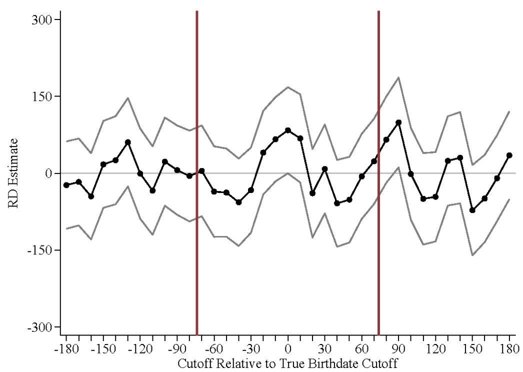
(a) Labor Market Earnings



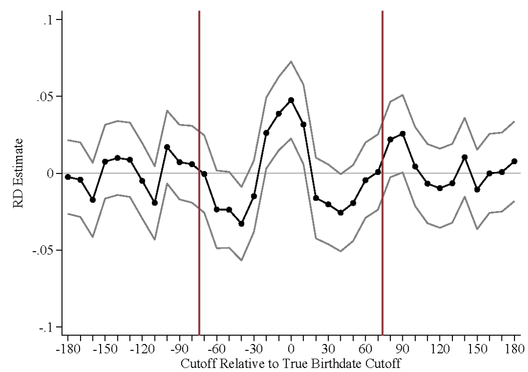
(b) Employer Retirement Plans



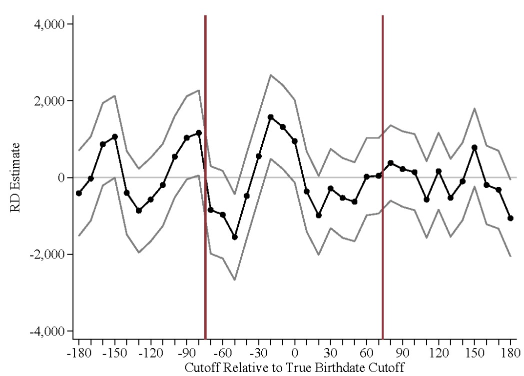
(c) Personal Retirement Plans



(d) Personal Plan Indicator

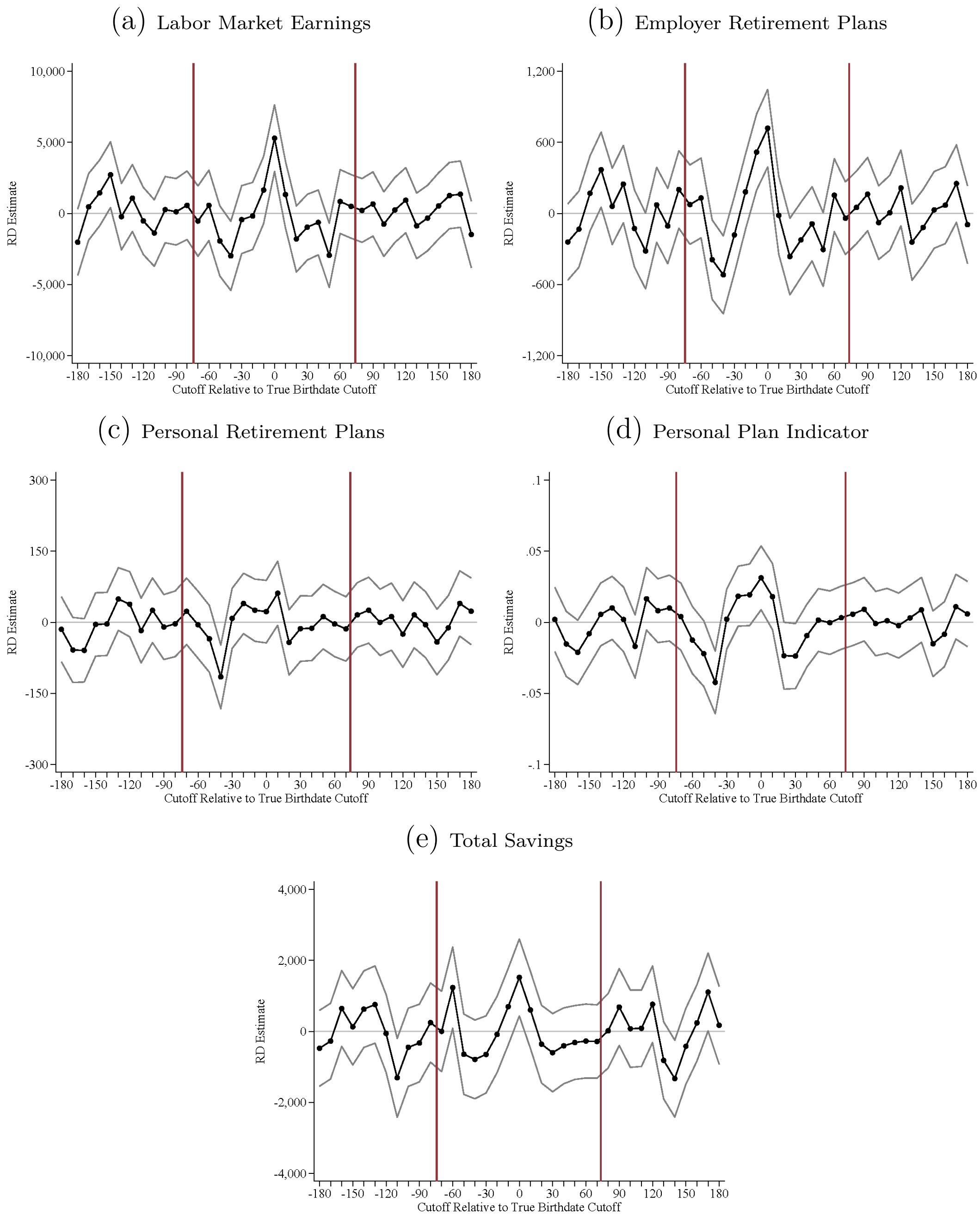


(e) Total Savings



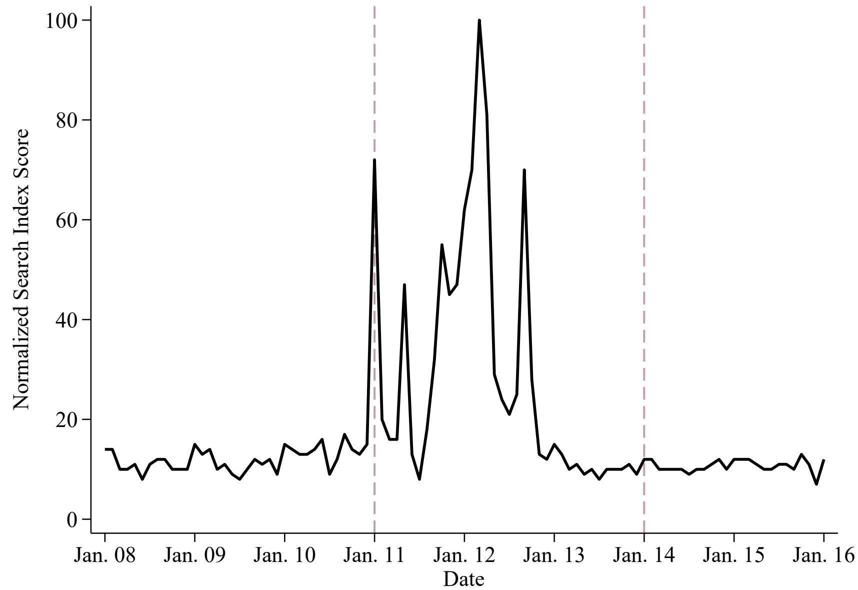
Notes: This figure illustrates how key RD estimates during the first critical year change when placebo cutoffs are used rather than the true cutoff. Each graph (a)–(e) plots RD estimates and 95-percent confidence intervals using the baseline RD estimating specification at various pseudo cutoffs. Because we use a bandwidth equal to 74 days on either side of the cutoff, the estimates between the maroon vertical lines include the true discontinuity.

Figure A.8: Placebo Exercise: Pseudo Birthdate Cutoffs at Age 62



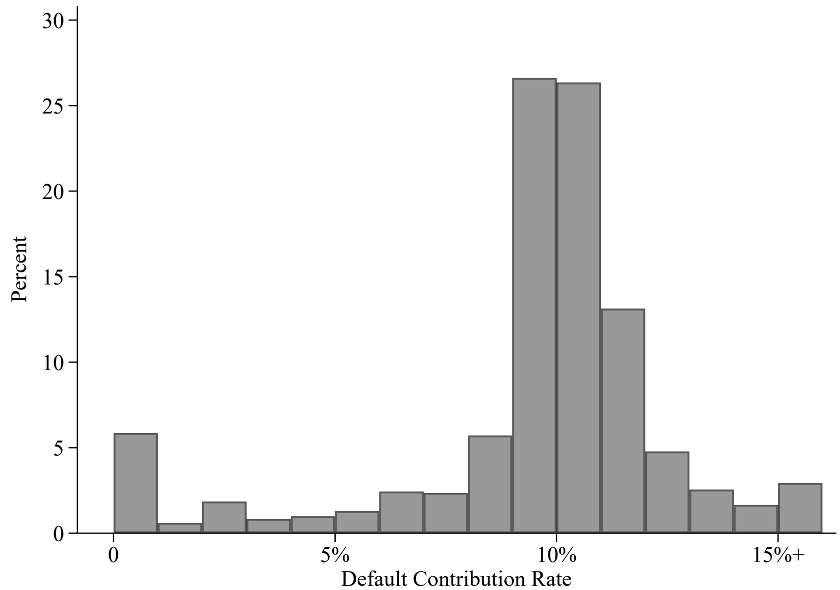
Notes: This figure illustrates how key RD estimates during the second critical year change when placebo cutoffs are used rather than the true cutoff. Each graph (a)–(e) plots RD estimates and 95-percent confidence intervals using the baseline RD estimating specification at various pseudo cutoffs. Because we use a bandwidth equal to 74 days on either side of the cutoff, the estimates between the maroon vertical lines include the true discontinuity.

Figure A.9: Google Searches for Efterløn



Notes: This figure plots a Google Trends search intensity index for "efterløn," which is the Danish word for the VERP program, between January 1, 2008 and January 1, 2016. The first vertical line marks the announcement of the reform, and the second vertical line marks the implementation.

Figure A.10: Distribution of Firm Default Contribution Rates



Notes: This figure plots the distribution of our proxy measure for firm default contribution rates for the individuals in our analysis sample, defined during the year 2010.

Table A.1: Analysis Sample Construction

	Number of Individuals (1)
Restriction 1: Born around the cutoff date	71,095
Restriction 2: Regular participatory contributions to VERP	47,047
Restriction 3: Balanced sample	43,348
Restriction 4: Exclude the self-employed	40,042

Notes: This table documents the impact of our four main sample restrictions, as detailed in Section 4. Our first restriction is to include only Danes born within six months of the cutoff date, January 1, 1954. This leaves us with 71,095 individuals. Our second restriction is to include only those making regular participatory contributions to the VERP scheme, defined as making contributions in 70% of pre-announcement years between 2001 and 2010, which leaves us with 47,047 individuals. Our third restriction is to balance the sample by excluding individuals not in the data for each of the years between 2006 and 2018, which leaves us with 43,348 individuals. Our fourth restriction is to drop the self-employed, defined over the pre-announcement period using Statistics Denmark's definition of an individual's main source of income, which leaves us with our analysis sample of 40,042 individuals.

Table A.2: RD Estimates for Control Variables as Outcomes

	RD Estimate (1)	Mean (2)
Male	0.019	0.46
	(0.0173)	
Married	0.013	0.72
	(0.0156)	
Residence in Hovedstaden	-0.001	0.11
	(0.011)	
Residence in Sjælland	-0.014	0.25
	(0.015)	
Residence in Syddanmark	-0.003	0.24
	(0.015)	
Residence in Midtjylland	0.020	0.24
	(0.015)	
Residence in Nordjylland	-0.003	0.15
	(0.013)	
Obs.	15,789	

Notes: This table reports RD estimates for the impact of the reform on (pre-determined) control variables. Control variables include an indicator for being male, an indicator for being married in 2010, and indicators for residing in each of the five regions of Denmark in 2010. The five regions are Hovedstaden (the capital region containing Copenhagen), Sjælland, Syddanmark, Midtjylland (containing Aarhus), and Nordjylland. The RD estimates come from estimating equation (6), except without any control variables on the right-hand side, but rather control variables on the left-hand side as outcomes. The regressions use separate linear polynomials in the running variable on either side of the birthdate cutoff and employ triangular weights. Robust standard errors are in parentheses. $^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.1$

Table A.3: RD Estimates on Indicator Variables for Contributing to Personal Retirement Plans

	RD Estimate (1)	Mean (2)
Fraction of Years Contributing between ages 57 and 59	0.012	0.33
	(0.014)	
Any Contributions at age 60	0.048***	0.14
	(0.013)	
Any Contributions at age 61	0.024**	0.13
	(0.012)	
Any Contributions at age 62	0.031***	0.11
	(0.012)	
Any Contributions at age 63	0.013	0.10
	(0.011)	
Any Contributions at age 64	0.008	0.10
	(0.011)	
Obs.	15,789	

Notes: This table reports RD estimates for the impact of the reform on indicators for contributing to personal retirement plans. The RD estimates come from estimating equation (6). The regressions use separate linear polynomials in the running variable on either side of the birthdate cutoff, employ triangular weights, and include as controls gender, marital status as of 2010, and indicators for region of residence as of 2010. Robust standard errors are in parentheses. $^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.1$

Table A.4: Robustness: Specification Checks

	Earnings (1)	Employer Plans (2)	Personal Plans (3)	Bank Accounts (4)	Stocks (5)	Property (6)	Liabilities (7)	Total Savings (8)
A. Anticipatory Responses								
A1. Baseline	648	109	49	-60	37	-141	-263	301
	(873)	(156)	(56)	(186)	(94)	(197)	(170)	(400)
A2. No Controls	1,005	159	51	-56	43	-136	-290*	399
	(894)	(158)	(56)	(186)	(94)	(203)	(171)	(406)
A3. No Tri. Weights	1,308	211	55	-75	79	-325*	-280*	197
	(801)	(142)	(52)	(169)	(87)	(181)	(157)	(364)
B. Critical Age 60								
B1. Baseline	6,325***	800***	84*	-105	-191	-19	-157	939*
	(1,079)	(169)	(43)	(412)	(184)	(19)	(257)	(552)
B2. No Controls	6,734***	855***	87**	-104	-173	-25	-184	1,046*
	(1,103)	(172)	(43)	(412)	(185)	(20)	(258)	(556)
B3. No Tri. Weights	6,777***	838***	102**	127	-53	-38**	-347	1,505***
	(986)	(153)	(40)	(377)	(168)	(18)	(235)	(503)
C. Critical Age 62								
C1. Baseline	5,299***	721***	23	391	57	-7	-171	1,510***
	(1,201)	(167)	(33)	(410)	(75)	(24)	(235)	(553)
C2. No Controls	5,769***	776***	26	408	59	-14	-177	1,611***
	(1,237)	(171)	(33)	(410)	(76)	(24)	(235)	(557)
C3. No Tri. Weights	5,433***	725***	47	439	85	-19	-121	1,518***
	(1,101)	(152)	(31)	(372)	(69)	(22)	(218)	(507)

Notes: This table reports results from assessing the sensitivity of the RD estimates to various specification checks. The panels indicate the time period to which the estimates correspond. Each column corresponds to a different main outcome variable. Each row indicates the regression specification used. The first row within each panel reproduces baseline estimates for ease of comparison, whereas the second row drops control variables from the regressions, and the thrid row does not use triangular weights. Robust standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.5: Robustness: Excluding Those with Real Estate Transactions

	Ages 57–59	
	RD Estimate (1)	Mean (2)
A: Labor Supply		
Average Earnings	590	55,790
	(883)	
B: Retirement Accounts		
Average Contributions to Employer Plans	101	6,080
	(158)	
Average Contributions to Personal Plans	48	884
	(57)	
Average Contributions to Roth-Style Plans	0	25
	(3)	
Average Distributions from Retirement Plans	6	636
	(149)	
C: Non-Retirement Savings		
Average Change in Bank Accounts	-4	1,609
	(190)	
Average Change in Stock Market Accounts	27	964
	(96)	
Average Change in Property Wealth	-218	-3,488
	(198)	
Average Change in Liabilities	-265	-2,641
	(170)	
D: Total Savings		
Average Total Savings	214	8,078
	(405)	
Average Total Retirement Savings	144	6,352
	(237)	
Average Total Non-Retirement Savings	70	1,726
	(313)	
Obs.	15,145	

Notes: This table reports RD estimates on outcomes over the anticipation time period for a subsample that excludes individuals with real estate transactions. Outcome variables are averaged over 2011 to 2013. Panel A presents results on labor supply. Panel B presents results on retirement savings. Panel C presents results on non-retirement savings. Panel D presents results on total savings. The RD estimates come from estimating equation (6). The regressions use separate linear polynomials in the running variable on either side of the birthdate cutoff, employ triangular weights, and include as controls gender, marital status as of 2010, and indicators for region of residence as of 2010. Robust standard errors are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.6: Robustness: Additional Winsorization of Outcome Variables

	Earnings (1)	Employer Plans (2)	Personal Plans (3)	Bank Accounts (4)	Stocks (5)	Property (6)	Liabilities (7)	Total Savings (8)
Anticipation (Ages 57–59)	625	99	41	41	-48	-3	-129	173
	(807)	(133)	(28)	(28)	(132)	(29)	(153)	(307)
Age 60	6,172***	754***	55***	-16	-23	-6	-128	951**
	(1,016)	(148)	(18)	(290)	(51)	(16)	(152)	(375)
Age 61	1,840	298*	23	-292	31	8	-217	322
	(1,150)	(154)	(14)	(288)	(47)	(29)	(156)	(411)
Age 62	5,167***	673***	17*	405	14	7	-100	1,254***
	(1,135)	(147)	(9)	(287)	(18)	(19)	(134)	(395)
Age 63	2,620**	254*	6	349	6	-12	-271*	872**
	(1,118)	(139)	(4)	(288)	(32)	(39)	(144)	(398)
Age 64	810	23	2	83	-79	-24	-138	-17
	(1,086)	(124)	(2)	(359)	(79)	(30)	(135)	(464)
Obs.	15,789							

Notes: This table reports RD results for outcomes that are more stringently winsorized, at the 10th and 90th percentiles. Each column corresponds to a different main outcome variable, and each row indicates the time period. The RD estimates come from estimating equation (6). The regressions use separate linear polynomials in the running variable on either side of the birthdate cutoff, employ triangular weights, and include as controls gender, marital status as of 2010, and indicators for region of residence as of 2010. Robust standard errors are in parentheses. $^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.1$

Table A.7: Placebo Exercise: Pre-Announcement Period

	Ages 54–56	
	RD Estimate (1)	Mean (2)
A: Labor Supply		
Average Earnings	829	59,917
	(784)	
B: Retirement Accounts		
Average Contributions to Employer Plans	69	6,639
	(171)	
Average Contributions to Personal Plans	88	1,280
	(75)	
Average Contributions to Roth-Style Plans	0	0
	(.)	
Average Distributions from Retirement Plans	-60	381
	(121)	
C: Non-Retirement Savings		
Average Change in Bank Accounts	-36	1,489
	(183)	
Average Change in Stock Market Accounts	-23	-182
	(39)	
Average Change in Property Wealth	-472	-12,727
	(542)	
Average Change in Liabilities	31	-632
	(217)	
D: Total Savings		
Average Total Savings	-346	-3,251
	(601)	
Average Total Retirement Savings	216	7,538
	(226)	
Average Total Non-Retirement Savings	-562	-10,789
	(590)	
Obs.	15,789	

Notes: This table reports RD estimates on outcomes over the pre-announcement placebo time period. Outcome variables are averaged over 2008 to 2010. Panel A presents results on labor supply. Panel B presents results on retirement savings. Panel C presents results on non-retirement savings. Panel D presents results on total savings. The RD estimates come from estimating equation (6). The regressions use separate linear polynomials in the running variable on either side of the birthdate cutoff, employ triangular weights, and include as controls gender, marital status as of 2010, and indicators for region of residence as of 2010. Robust standard errors are in parentheses. $^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.1$

Table A.8: Placebo Exercise: Previous Birth Cohorts

	Age 60	Age 62
	RD Estimate	RD Estimate
	(1)	(2)
A: 1950/1951 Birth Cohorts		
Earnings	-105	-602
	(1,120)	(1,162)
Contributions to Employer Plans	-129	-79
	(178)	(157)
Contributions to Personal Plans	-3	-25
	(71)	(36)
Total Savings	-569	-367
	(773)	(618)
Obs.	15,621	15,621
B: 1951/1952 Birth Cohorts		
Earnings	950	1,439
	(1,129)	(1,177)
Contributions to Employer Plans	175	129
	(173)	(162)
Contributions to Personal Plans	1	13
	(65)	(34)
Total Savings	821	151
	(635)	(562)
Obs.	15,620	15,620

Notes: This table reports RD estimates during "critical years" for placebo birth cohorts. Panel A presents results for earnings and contributions to retirement savings accounts using January 1, 1951 as a placebo birthdate cutoff. Column (1) presents results for the year that individuals born on this placebo birthdate cutoff are age 60. Column (2) presents results for the year that individuals born on this placebo birthdate cutoff are age 62. Panel B presents results when using January 1, 1952 as a placebo birthdate cutoff. The RD estimates come from estimating equation (6). The regressions use separate linear polynomials in the running variable on either side of the birthdate cutoff, employ triangular weights, and include as controls gender, (pre-determined) marital status, and (pre-determined) indicators for region of residence. Robust standard errors are in parentheses. $^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.1$

Table A.9: RD Estimates for VERP Participation

	RD Estimate (1)	Mean (2)
Participate in 2011	-0.004	0.94
	(0.008)	
Participate in 2012	0.002	0.93
	(0.009)	
Participate in 2013	-0.008	0.92
	(0.009)	
Obs.	15,789	

Notes: This table reports RD estimates for the impact of the reform on participatory VERP contributions. The outcome variables are indicators for making qualified contributions to UI funds in each of the three years leading up to the implementation of the reform. The RD estimates come from estimating equation (6). The regressions use separate linear polynomials in the running variable on either side of the birthdate cutoff, employ triangular weights, and include as controls gender, marital status as of 2010, and indicators for region of residence as of 2010. Robust standard errors are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.10: Anticipatory Responses for Users of Personal Retirement Plans

	Ages 57–59	
	RD Estimate (1)	Mean (2)
A: Labor Supply		
Average Earnings	238 (1,309)	57,029
B: Retirement Accounts		
Average Contributions to Employer Plans	319 (233)	6,012
Average Contributions to Personal Plans	101 (105)	1,926
Average Contributions to Roth-Style Plans	3 (7)	48
Average Distributions from Retirement Plans	167 (247)	729
C: Non-Retirement Savings		
Average Change in Bank Accounts	140 (301)	1,561
Average Change in Stock Market Accounts	53 (154)	1,170
Average Change in Property Wealth	-105 (303)	-3,780
Average Change in Liabilities	-339 (263)	-2,569
D: Total Savings		
Average Total Savings	681 (632)	8,777
Average Total Retirement Savings	255 (376)	7,257
Average Total Non-Retirement Savings	426 (490)	1,520
Obs.	6,596	

Notes: This table reports RD estimates for the impact of the reform on outcomes over the anticipation time period for the subsample of individuals who had been using personal retirement plans before the announcement of the reform. The subsample is defined as those who made contributions to personal plans in either two or three of the years between 2008 and 2010. Outcome variables are averaged over 2011 to 2013. Panel A presents results on labor supply. Panel B presents results on retirement savings. Panel C presents results on non-retirement savings. Panel D presents results on total savings. The RD estimates come from estimating equation (6). The regressions use separate linear polynomials in the running variable on either side of the birthdate cutoff, employ triangular weights, and include as controls gender, marital status as of 2010, and indicators for region of residence as of 2010. Robust standard errors are in parentheses. $^{***}\, p < 0.01$, $^{**}\, p < 0.05$, $^{*}\, p < 0.1$

Appendix B Back-of-the-Envelope Calculation for Anticipatory Savings Responses

Here we carry out the back-of-the-envelope calculation that informs on the expected savings responses over the anticipation time period. We take the retirement response as given. A six-month delay in retirement should give rise to an increase in earnings and a decrease in Old Age Pension benefits. Using pre-period data to guide our calculations, we estimate a positive net effect on income that results from delaying retirement. We then treat this extra income as a pure lifetime income "shock" and calculate the expected declines in savings over the anticipation time period, assuming individuals perfectly smooth consumption.

Our predictions are centered on those induced to retire later due to the reform, namely those who retire right at the key pension eligibility ages. We therefore start by defining individuals who retired at either the first or the second retirement spike, for the treatment and the control group. Specifically, for the control group, we define those retiring at the first spike as those with retirement ages between 60 and 60 and 5 months, and we define those retiring at the second spike as those with retirement ages between 62 and 62 and 5 months. For the treatment group, we define those retiring at the first spike as those with retirement ages between $60\frac{1}{2}$ and 60 and 11 months, and we define those retiring at the second spike as those with retirement ages between $62\frac{1}{2}$ and 62 and 11 months.

Those who would have retired at the previous eligibility ages are induced by the reform to work longer. Average pre-period (i.e. 2010) earnings of individuals who ultimately retire at the first spike are \$47,806. Average pre-period earnings of those who ultimately retire at the second spike are \$62,352. Working six more months for each of these groups thus amounts to an expected increase in earnings of \$23,903 and \$31,176, respectively. But retiring later also results in receiving 6 months less of OAP benefits over their lifetime, which amounts to roughly \$7,500. This yields a total increase in income of \$16,403 for those retiring at the first spike and \$23,676 for those retiring at the second spike.

Assuming individuals live until they are 80 years old, they should spread this extra income over the 23 years of life remaining after the announcement of the reform. For those retiring at the first spike, this amounts to an increase in annual consumption of \$713. For those retiring at the second spike, this amounts to an increase in annual consumption of \$1,029.

These calculations are for a subsample of the individuals that we study, but our RD estimates capture aggregate responses, for the entire analysis sample. That is, our analysis sample includes those retiring at the relevant spikes as well as those who retire at other ages. The fraction of people retiring at the first spike as we have defined it amounts to 15.8%, and the fraction of people retiring at the second spike as we have defined it amounts to 10.6%. Thus, the consumption response that we expect to see when studying the population as a whole is: (0.158)(\$703) + (0.106)(\$1,028) = \$220. Since savings is income less consumption, we roughly expect to see a savings response of −\$220 per year in anticipation.

Appendix C Additional Institutional Details

This section provides additional institutional details that pertain to our analysis time period and the birth cohorts relevant for our study.

C.1 Additional Information on Retirement Savings Accounts

Traditional defined contribution retirement savings plans in Denmark can be either employer-sponsored plans or personal plans. Within each type of plan, there are also three main types of accounts, which differ in the way that they are paid out. Life annuity accounts pay out as annuities for the rest of the account holder's life. Fixed-term annuity accounts pay out as income streams for a designated time period, typically either ten or twenty-five years. Capital accounts pay out as lump sum distributions.

Similar to the U.S. setting, the accounts are tax-advantaged. Contributions to the accounts are tax-deductible. Capital gains in the accounts are taxed upon accrual at approximately 15%, which is typically favorable compared to taxation of capital gains on savings outside of retirement accounts. Payments from life annuity and fixed-term annuity accounts are taxed as regular income, whereas distributions from capital accounts are taxed at approximately 40%.

In 2013, Denmark introduced "Roth-style" retirement plans. Contributions to these accounts are not tax-deductible, but lump sum distributions from the accounts are tax-free. These accounts aimed to replace the traditional capital accounts, as starting in 2013 contributions to capital accounts are no longer tax-deductible.

C.2 Additional Information on the Voluntary Early Retirement Pension

Participating in VERP requires making fixed contributions to qualified unemployment insurance (UI) funds during working life. These contributions amount to roughly $1,000 per year. To be eligible to claim, individuals must have contributed in 25 out of the previous 30 years.

VERP benefits are linked to the UI benefit schedule, but are typically viewed as flat-rate in practice, since they are capped at 91% of the maximum UI benefits. Typically benefit amounts are calculated using the highest twelve months of earnings over the previous two years. Monthly benefits correspond to 90% of these earnings divided by 12. Base benefits are then the minimum of either this amount or 91% of the maximum UI benefits. The maximum VERP benefits amount to roughly $27,000 per year, in 2010 USD.

Benefits are then subject to means testing, first against assets held in private retirement accounts, which determines base payments for the duration of the program. The government collects information on account balances from banking and financial institutions, usually when workers are around age $59\frac{1}{2}$. This information is used to compute base benefits depending on claiming age. Benefits are reduced against assets in retirement accounts at approximately 60% of "could-be annuitized" payments.

In addition to this means testing, benefit payouts are further means tested against income after claiming. Benefits are means tested against drawdown from private retirement accounts, at a rate of around 50%. Benefits are also means tested against hours worked at a rate of 100%. VERP benefits are linked to an hourly rate per month, and each hour of work while on the program reduces VERP benefits by one hour.

Two key rules serve as defining features of the VERP program. The "transition rule" stipulates conditions under which individuals can transition to the VERP program. The regulation states that, to be eligible to claim VERP benefits, one must be "available to the labor force." Individuals can transition to VERP either from employment or from formal unemployment, which involves meeting UI requirements such as searching for jobs. An important implication of this rule is that an individual who retires and exits the labor force before reaching VERP eligibility age will not satisfy the transition rule and will not be eligible for benefits.

The "two-year rule" provides incentives for individuals to retire and transition to the VERP program two years after the earliest eligibility age. To satisfy the rule, individuals must work through the first two years of the VERP program. It is not enough to simply delay claiming of benefits. Satisfying the rule leads to three financial bonuses. First, base benefits for the duration of the VERP program are no longer means-tested against wealth held in private retirement accounts. Second, benefit amounts are weakly increased, as benefits become tied to 100% of the maximum UI benefits, rather than 91%. Third, every additional quarter worked after satisfying the two-year rule results in a tax-free lump sum payment equal to approximately $2,250.

C.3 Additional Information on the Old Age Pension

The OAP provides near-universal old-age benefits for Danes. Benefits are proportionally reduced for individuals that have lived in Denmark fewer than forty years. Benefit amounts are comprised of three main components. First, a base benefit of approximately $10,000 per year is provided to all individuals. This amount is subject to an earnings test where benefits are reduced at a rate of 30% against earnings above roughly $40,000. Second, a pension allowance is provided. The allowance is approximately $10,000 per year for single individuals and $5,000 for married individuals. This amount is subject to an income test where benefits are reduced at a rate of roughly 30% against earnings above $9,500. Third, there is a pension supplement available for the poorest pensioners. This amounts to about $1,000 per year and is delivered to only those with low levels of assets. In general, due to a 2004 reform, OAP benefits can be deferred with adjustments that are approximately actuarially fair.